

CONVERSATIONS IN SOIL TAXONOMY
(ORIGINAL TRANSCRIPTIONS OF TAPED CONVERSATIONS)

by

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Cornell University for the
Soil Management Support Service

USDA - SCS

Ithaca, New York

1986

Addendum to:

THE GUY SMITH INTERVIEWS:

RATIONALE FOR CONCEPTS

IN SOIL TAXONOMY

by Guy D. Smith

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1986

SMES Technical Monograph No. 11

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Preface

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

The one person who was fully conversant with the system and who co-ordinated its design was the late Dr. Guy D. Smith. In 1976, Dr. M. Leamy and staff of the Soil Bureau of New Zealand conducted a series of interviews with Dr. Smith. These interviews were published in the *Newsletter* of the New Zealand Soil Science Society and later reprinted in *Soil Survey Horizons*. The considerable interest shown in these interviews was the impetus necessary for the Soil Management Support Services (SMSS), established in October 1979, to continue this effort.

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

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

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

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

Dr. M. Leamy (New Zealand); Dr. R. Tavernier (Belgium); Dr. R. Rust (Minnesota); Dr. B. Allen (Texas); Dr. A. Van Wambeke and Dr. M. G. Cline (Cornell); Dr. L. Wilding (Texas); Dr. J. Comerms (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
at University of Minnesota**

1981

St. Paul, Minnesota

Question 1

Rust:

This discussion for the rationale background for *Soil Taxonomy* began about late '78 or '79. At that time Guy was in New Zealand. We wrote to him and asked him if he would be willing to participate in such an effort. Somebody else had the same idea. Dr. Mike Leamy of New Zealand had already begun to think about this idea of putting down the rationale and background for Soil Taxonomy. So Guy wrote and said yes he would be willing to assist in any way he could. We then went to the International meetings at Edmonton and suggested the idea to the international group assembled. It got a favorable reception there. So using the mailing list of the International Soil Science Society we wrote to people and asked them to submit kinds of questions they might have with respect to *Soil Taxonomy*. Over the next year we assembled these and tried to edit them and have put them together in some 50 to 55 questions. These then have been some sort of basis for these interviews with Guy. In July, of this past year, Mike Leamy went to Ghent and visited with Guy and talked with him for several days and included some of the questions that are on the list. I will pass out this list of questions. Some of you may have them from something I've sent to you earlier. On this list I have indicated with an "L" the questions that were discussed by Guy and Mike Leamy in Ghent in July. Guy arrived in the States about November 20, 21 and has had some conversation with Washington folk. The 15th of December he visited the Cornell group with Arnold and Marlin Cline. I have the questions which were discussed at Cornell, I only have one copy. There are a few duplications in the additional questions that were covered at Cornell. Last week Guy was at Texas Tech visiting with B.L. Allen and group and some additional questions were discussed there. I thought it might be helpful if Guy would briefly overview what was discussed at Texas Tech. Perhaps there is a lot of duplication in the things that we might be concerned with in this group. With those of you that are here, we thought that we would have a little more emphasis on questions that might relate to the Mollisols, to the Alfisols, to the Histosols, and since we have our folk from the higher latitudes maybe some questions relating to the soils of the cold regions and their classification. Also some questions that Fred Peterson will bring in respect to the Aridisols of the west.

Guy, would you want to say a little bit about the conversations at Lubbock just briefly so that we have a little idea of what we might talk about here?

Guy Smith:

Yes, I would be glad to but I would like to make a couple of introductory comments first. I think it would be extremely useful in editing this material if I had a written list of the people who are here with their present affiliations or former. And if each person asking a question would identify himself into the microphone so that there will be a record as to who asked what. I would also like to say I wish I were nearing the end of this but unhappily I go from here to Venezuela and then to Trinidad and then when I get back to Ghent Dr. Frank Moormann is going to visit me. So there are three more weeks of questions at least after this one.

At Lubbock we were concerned more with the soils with either ustic or aridic moisture regimes. There were a good many questions about these. There were a good many questions about soils in pale-great groups because we don't have the same criteria for the Paleargids or the Paleorthids that we have for the Paleustolls or the Paleudalfs and so on. The criteria vary from one great group to another and the questions were really concerned with the intent of the pale-great groups and how the definitions we prepared had actually met that intent. There were a number of questions about calcic horizons, about salic horizons, diagnostic horizons that

we use particularly in Aridisols. There were quite a few questions that concerned the moisture regime, their use in *Soil Taxonomy*; methods and means of estimating them, and likewise, about soil temperature. Some of the questions that concerned soil temperature, it seemed to me, were very speculative in that they were talking about mesic temperatures at very high altitudes in the Rockies. Difficult for me to see how the soil temperature is going to get that warm at high altitude. A number of studies have been made and most of them tend to indicate that soil temperature, mean annual temperature, can be predicted very closely from latitude and altitude. Over quite a wide range of latitudes and altitudes. There were very few questions, but there were some, about the place of soil series in *Soil Taxonomy*. Should the soil series, like the soil type, be taken out of Taxonomy itself and allowed to "float" like the phases which we took out of the Taxonomic system. We defined the phases that are essential to the anticipated uses of the soil in the particular soil survey area. There seems to be some concern about the soil series - that perhaps it might also be freed from any rigid limits that are in *Soil Taxonomy* - and used where the series is necessary for the intended uses of the soil survey. There were questions about the possibility of the use of higher categories for soil surveys, the families, the subgroups, with appropriate phases. I think this was the main impact, although there were questions about whether or not *Soil Taxonomy* was useful in showing landscape relations of soils, shape and size of the polypedons and questions about what we were classifying, a pedon or the polypedon.

Question 2

Rust:

The organization of these questions might be improved a great deal. And I don't think I will go through the unanswered questions at this point, because I believe it might be more profitable for those of you that have come with some notions and questions of your own. You can see what is here and if it is similar to the question that you would like to raise, that is well and good. Some of you could probably rephrase these questions better, probably could provide a little bit more background in terms of what might be underlying in the question. Perhaps, it would be appropriate, that we think in terms of organizing this ultimate effort, more or less parallel to *Soil Taxonomy*. We can talk briefly to Guy about this and then go into the discussion of the orders. So I think we could begin with anyone who wishes to offer a question in the general area of philosophy that you don't feel has been spoken to in the questioning heretofore. And I'm sure that as someone opens up the questions others will think of a related item. As Guy has said, would you simply identify yourself as you begin your question so that the record would show from whence it came and, in case there is some follow-up because of some other intent of the question, we will know with whom to relate.

Guy Smith:

I should also ask you to speak up when you ask questions, because, while I have a hearing aid, there are many improvements possible in it and without it my hearing is very bad.

Question 3

Rieger:

I have one that has been discussed and it might just be in that category. This concerns the definition of soil that you've seen, Guy? Would you care to discuss that any further?

Guy Smith:

The only question that I can recall answering on that is what do you do, how deep does the water have to get before it ceases to be soil? This concerns coastal plains soil. When I was asked to cooperate on this book I said I would be happy to but I didn't know what kinds of questions people were concerned with and I was asked to find that out before I could answer them. So I think your question should be fairly specific now.

Question 4

Rieger:

O.K. Well, I was concerned with the definition of soil - the thing that we are trying to classify which is defined rather vaguely in Taxonomy, particularly with respect to its lower boundary. There are two problems that come up with respect to cold soils, especially. One, if soil is defined as the lower limit of extensive rooting, in cold soils the roots are quite shallow and we end up with soils that may, by that definition, be confined to the O horizon and yet in our classification, we use the control section as we are doing in any other soil. The second part concerns organic soils which mostly, in the natural state, have very shallow rooting and yet were defined on the properties below the root zone. And the third part to this question concerns soils that are not vegetated. By definition then, not soil. Any loose material on the earth's surface, vegetated or not, at least in its natural condition, such as the polar desert type of soil or the soils in Antarctica desert salt flats, shifting sand dunes - all of these are not soil as presently defined. And I wonder if they should be defined as soils?

Guy Smith:

You've got three questions. I'll try to remember them. First, your first question was brought up in a previous meeting, and my answer was that this required a great deal more work. That there were many soils where the rooting was in the O horizon and yet we classify the soil on the basis of the mineral part where the soil has virtually no roots. The answer to the second question would have about the same answer as the first. I replied that this was an unresolved question so far as the O horizon was concerned and would require considerable thought on the part of the people who knew something about these soils. In most of the U.S., the Soil Conservation Service staff, the Experiment Station staff are not concerned with such soils. They don't have them other than in the forest. So the lower boundary of soil in that situation, as in Histosols, has got to be somewhat arbitrary. We pointed this out in *Soil Taxonomy*, that the lower boundary was a very difficult one and that in many instances, in many kinds of soils, the lower boundary could only be an arbitrary limit. In *Soil Taxonomy* we have treated two meters as its arbitrary limit, this limit being taken on the basis that it is impractical in most soil surveys to examine the soil frequently enough below two meters to have any reliability in our observations. With respect to your third question, regarding unvegetated soils, I'm going to have to draw a line somewhere between the field of pedology and the field of geology. Normally we left the barren areas to the geologists although they concern

themselves generally more with the bedrock than with what's above it. There is a question where the regolith is thick and the soil scientist stops at two meters and the geologist starts at 50 meters - who's field is this one in between? In some instances, as where we are irrigating a new project, we need to know what is going to happen to the leaching water and it is necessary for our interpretations to make rather deep observations in the regolith to figure where that water is going to surface. This requires power drilling equipment and is only practical for very intensive uses, such as that under irrigation. The salt flats in some cases do carry vegetation, in which case they become a soil and then there is a problem - is the salt flat a salty parent material or is it a saline horizon - and this was gone into in considerable detail at Lubbock. In general, however the purpose of *Soil Taxonomy* is to facilitate soil surveys and their interpretations. It is inconceivable to me that we are going to spend very much money studying these unvegetated areas; they are going to be left to the geologist rather than brought into the classification. There are some soils in Antarctica but there are very few. There is no particular reason to make very many soil surveys in Antarctica except to get at the history of the area. And that's not a good reason for most soil surveys. I think that most that are going to be made, probably have been made already by the people in New Zealand.

Question 5

Rieger:

I was discussing this with Dr. Tarnocai; here and there are some soils in the Canadian North that are unvegetated or essentially unvegetated that aren't being studied by soils people.

Tarnocai:

In a Canadian scene I think we don't want to discriminate between soils which are unvegetated and vegetated because we carry our soil surveys in the Arctic which is largely unvegetated. There is a certain amount of biologic activity but not necessarily forest vegetation and grass vegetation. So, it is difficult to us to sort out the problem this way - this is for the geologist and this for the pedologist - because this is unvegetated.

Guy Smith:

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

Question 6

Franzmeier:

You mentioned that one of the main purposes of Soil Taxonomy is to facilitate soil surveys and the interpretation of them. It appears that there are two main aspects of soil, the profile aspects and landscape aspects. In our soil surveys and through *Soil Taxonomy* we are emphasizing more and more the profile aspects. I am not sure if this would go along with your observation but in comparing older surveys with those we now produce it seems to me to be the trend. Yet in working with users of soil surveys, they seem to relate more to the landscape aspects. Do you recognize this as a dilemma? If so, what might we do about it?

Guy Smith:

I'm not sure I understand precisely your meaning of landscape aspects.

Franzmeier:

Well, the geomorphic aspects -- the slope position, the slope shape, where it stands in the landscape relative to other land forms. Just as certain aspects of the soil profile have implications relative to the genesis of the profile some landscape aspects of the soil would imply a certain genesis of the landscape -- whether it got there by a glacier or wind erosion; this type of thing.

Guy Smith:

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

Question 7

Tarnocai:

First question is, why is it stated in the soil temperature regime section, *Soil Taxonomy*, page 57, that "below freezing point water no longer moves as a liquid and unless there is frost heaving, time stands still for the soil." It has been well demonstrated that water moves in liquid form in a frozen soil well below zero centigrade and that frost heaving and ice formation is caused by this movement of liquid water. I quoted several references here that these ice-lenses are able to grow because liquid water moves from warmer to colder areas through the frozen soil system.

Guy Smith:

You are making the assumption here that below the freezing point, which is zero normally. Let me start again, you are making the assumption that zero is the freezing point of all water. This is not true. At temperatures far below zero some of the water is still in a liquid form rather than solid and it is this liquid water that does move in the soil where most of the water is in a solid state. Professor Miller at Cornell has been doing considerable work on this and he finds two things. One, if he suspends a piece of mineral soil in ice, this mineral particle will move upward through the ice and emerge at the surface. Now it is moving because the water at the top of the mineral grain liquefies, moves around the side, and solidifies at the bottom and pushes the mineral particle up. If the mineral particle is fixed and cannot move, then the water moves. If the water moves from underneath the mineral particle to on top of it, it seems then to sink in the ice. Actually the water is moving from below to above the fixed mineral particle. So there is no one freezing point for water in soil. But for the most part, except for this small unfrozen part of the water, zero is the freezing point. But around every mineral grain there is a bit of unfrozen water which is held at temperatures that cannot freeze at zero and freezing point may be far below zero for some of the water. This (explanation) might have been (stated) better but this is the way modern soil physics looks at soil temperature and water.

Tarnocai:

In reading the material here it states that water no longer moves as a liquid.

Guy Smith:

It can move as a vapor. But below freezing point I say that nowadays evidence has been established at this point on the nature of the water. And, as I say, this could have been better stated perhaps. It does move as a liquid or solid. Even according to the most modern soil science.

Question 8

Tarnocai:

My second question is: Why is permafrost defined in *Soil Taxonomy*, page 50, "as a layer in which the temperature is perennially at or below zero centigrade"? Now in a Canadian definition, which is similar to Alaskan definition, permafrost is defined as a *thermal condition*, (not a layer) having a temperature below zero (not at or below zero centigrade).

Guy Smith:

Weil, that's another definition, I guess. Zero can be frozen or unfrozen.

Tarnocai:

Excuse me, sir. Zero centigrade, regardless of water, is a term of condition. See, this is the point.

Guy Smith:

But it is a layer also, although it may go far below the soil. As you go deep you will always come to a temperature above zero. It may be a hundred feet down or two hundred or it may be relatively shallow, but I don't see any serious conflict between these two statements other than that this Canadian definition says it has to be below zero. But it can be at or below as far as I am concerned because zero may be frozen or unfrozen, either one depending on which way your heat flow is affecting the soil. You can bring the ice up to zero and then with additional heat it melts but the temperature is unchanged.

Tarnocai:

Can I rephrase my question? Why did you choose to change the definition which is internationally accepted?

Guy Smith:

I didn't change it. This comes from 1960.

Tarnocai:

For the future you would like to stay with the definition?

Guy Smith:

Normally we would take an internationally accepted definition in preference to one of our own but it has to be in existence first.

Tarnocai:

The Alaskan definition is 1969. So definitions were filed formally, to my knowledge in Canada and I think in other northern countries by sometime in the second part of the '60s.

Guy Smith:

But by the end of the first half of the '60s this was all finished. We couldn't keep everything up to date all the way through.

Question 9

Tarnocai:

My next question relates to the "pergelic soil temperature regime" as defined in Taxonomy, page 62, as soil having mean annual soil temperature lower than zero centigrade. Does the

projected soil temperature regime indicate that permafrost occurs within a control section, let's say one meter, or that it occurs at any depth (either within or below the control section)?

Based on some of our preliminary soil temperature data, a MAST (mean annual soil temperature) below 0 degree C does not necessarily indicate that permafrost occurs at the particular depth to which the 0 degree C MAST refers. MAST values for two sites located in the Inuvik area, N.W.T. (based on weekly measurements between Feb. 15, 1979 and Feb. 21, 1980) are as follows:

Depth cm	Site I-3	Site I-4
20	-2.4	-0.8
50	-2.9	-1.6
100	-3.1	02.1

Soil I-3 has permafrost within the control section (at a depth of 55 cm) whereas soil I-4 has no permafrost within the control section. Permafrost occurs, however, in soil I-4 below a depth of 120 cm.

Guy Smith:

On this third question I should comment first that the mean annual soil temperature below zero centigrade may indicate that there is a permanently frozen horizon or layer beginning at a rather shallow depth or beginning at a very deep depth but it should be present based on what we know about the mean annual soil temperature relation to permanently frozen ground. It was not our intent that the control section should stop at one meter, normally we prefer to think the control section will stop at two meters given a low category. At a higher category, the Inceptisols' control section does stop at one meter but that's at the family level not at the series level. So that I think the Russians have some soils with mean annual temperatures below zero during the summer to depths of something like two meters. Below that there is no further change and the original intent was that there was permafrost at some depth not necessarily within the family control section.

Question 10

Tarnocai:

The fourth question concerns the pergelic subgroups having a mean annual soil temperature below zero centigrade. To determine the mean annual soil temperature, measurements are required for periods of at least one year. Are there any factors, other than the mean annual soil temperature, which may play a role in determining the pergelic soil temperature regime? To explain in a little bit more detail, the pergelic subgroups described in the Exploratory Survey of Alaska gave no indication of actual mean annual soil temperature values. The depth of permafrost is variously described as: The permafrost table is usually deep but ice-rich permafrost may exist at depths of 60 to 150 cm "(page 30). . permafrost table is quite deep" (page 44). . "The permafrost table is commonly many feet deep" (page 29). These pergelic soils probably have a variety of mean annual soil temperatures, some of them may even be above zero centigrade as we found in the Discontinuous Permafrost Zone in Manitoba. For example, the Kiski series, situated in the Discontinuous Permafrost Zone in Manitoba, has a

permafrost table at the 104 centimeter depth and the mean annual soil temperature value is 0.2 degrees C at the 50 centimeter depth.

Guy Smith:

First, the mean annual temperature is not necessarily measured at 50 centimeters. I should point out that we have below the solum a zone of constant temperature which does not vary even from year to year, let alone season to season and this, within the limits of reliability of measurement, is the mean annual temperature of that soil. When you get significant differences between different depths in a given soil, I'm convinced there is a systematic error in your measurement, I don't know necessarily what it is but commonly it is because you made your measurements at a particular time of day, day after day and this does not necessarily represent the average temperature for that day or even for that half day. I don't know that your reliability of measurement of mean annual soil temperature is within 0.2 of a degree. There are errors of measurement that are due either to instrumentation or to systematic recording of temperatures. I would think make your mean annual soil temperature is subject to errors more than 0.2 of a degree. So this isn't going to concern me. You do the best you can when you are measuring the clay content of the soil or the base saturation and etc. You know there is a possibility of error of measurement of any particular property of the soil. And when you are within the limits of that error you disregard your measurements. In this case if you have actually got permafrost, I would say the likelihood is that your error is in the measurement at 50 centimeter depth rather than in the temperature being above zero. That would be my judgement. Now I have to ask Dr. Reiger to explain all these statements about the Exploratory Soil of Alaska that hadn't been written when *Soil Taxonomy* was written.

Rieger:

I didn't quite get the point of the question. Oh, well I give no indication of actual mean soil temperature values basically because in most of the soils it's simply not known, we just don't have that data especially out in the wilderness area. However, where we do have information on soil temperatures we have also found the same situation of temperatures at 50 cm being slightly above the zero centigrade mark, these soils where permafrost is rather deep. In fact, it's happened in one soil that we studied fairly thoroughly, (that) the permafrost table under the natural vegetation of black spruce forest is at about 50 cm or even less. When it's cleared the table drops to four or five meters. Temperatures were measured quite carefully and the mean annual temperature in the upper meter was slightly above zero within the error range, as you suggested, perhaps 0.1 or 0.2, and they found the same situation in an alpine soil with deep permafrost. I can't say whether the mean annual temperature throughout the soil column is exactly at zero or below in soils with permafrost but perhaps we need to allow a slight range. The statement that it is essentially the same throughout the column, I am quite sure holds.

There is relict, permafrost around, certainly, but is usually very deep. It's not within the upper 5 meters anyway.

Does permafrost re-form in those cleared fields you are talking about?

Yes, if you allow the (soil to be) under permanent grass it will reform and, certainly, if you allow the native vegetation to come back, it will.

The soil temperature in these cold climates is not as well related to the air temperature as it is in climates where there is little snow. You have an O horizon on these soils that forms a layer of insulation during the warmest weather. And you have a snow horizon that forms a layer of insulation during the coldest weather. Now if you cleared the soil and removed the O horizon you are removing the insulation that is effective during the warmest weather, but you are not able to do much about the snow insulation during the cold weather, so the soil temperature will change if you disturb the native vegetation in the O horizon. It may be that (you) have gotten your measurement at the time when this change is in progress.

These particular measurements were made in a rather elaborate set-up by the people at the University; analysis is of a continuous type of measurement.

I remember the measurements.

I think information we have on soil temperatures indicates that the deviation from the mean at any level is extremely slight. That the mean is the same. I know we don't agree on that point.

Well, I would like you to bring the soil physicist in and explain how it can be different at different layers. Where does the heat come from? I can not imagine any mechanism where soils can have different temperatures at different layers and in the absence of a thermal source of heat.

Question 11

The whole German system, is our new Histosol classification. I think in the organics, John Day has done a good job. They don't call it fibric, hemic, sapric, but it's almost the same thing.

We use those names.

You have humic. We even played with that term ourselves, and you pulled it out because the temperature is wrong. We had mesic for it at one time. John Day and Walter Ehrlich took over and said no, we'll use the mesic for the in between - for the hemic - and Guy says we can't do it because of the temperature class. Can't have the same word for a different category.

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

You say there are two orders they want to add?

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

You are saying they have nothing comparable to our family or series categories?

The Russians, well no, they have.

(incoherent?)

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

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

What about Australia?

Australians rejected the classification of Stevens and they put Northcote to develop a new classification and he did that when he made his map of Australia. Then while I was in New Zealand they advertised for a man to come to Australia to develop a new system of soil classification. They hired a soil chemist from Aberdeen whose experience in classification has been lacking. What they will come out with I don't know but I'm dubious about what they'll accomplish. When we had our meeting in Malaysia, he had an opportunity to come to learn something about *Soil Taxonomy*. But he didn't show up at all. We had Australians there but not him.

It sounds like they are not even serious?

I don't have any notion but I know they are in trouble in Australia. New Zealand is trying *Soil Taxonomy*. This guy isn't a student of Fitzpatrick? That's where he had his position. His one position was with Fitzpatrick.

He spent summer in Alaska.

Has he been in Alaska?

Yes, all summer.

That's a kind of a different tundra than Australia, isn't it?

The Soils Bureau we found in New Zealand decided they would use *Soil Taxonomy*. Some of the old timers are opposed to it, it's natural. Present, younger people at the Soil Bureau are just going to have to work at it. There is no way around it. Tedrow is never going to accept it in New Jersey. At least he no longer has any responsibility for soil surveys so the state college is using *Soil Taxonomy*. When Sam Obenchain retired, his successor immediately adopted *Soil Taxonomy* for teaching. Sam never would mention it.

Have you ever seen the work in Britain of the sort of statistical approach to Soil Survey and spatial distribution of soils?

Reading about it I have a hard time seeing how it might fit into practical use. I cannot imagine how it's going to work.

Webster's work is picking up. Webster mainly, probably some others.

You must remember that this was the Soil Survey of England and Wales, located at Rothamsted. The emphasis was on pure science, pure research. And at least one director of that survey retired because they would not allow him to make interpretations of the soil survey. That wasn't pure research, that was applied research, if he made interpretations. So if you are not trying to make interpretations you can make soil surveys.

Question 12

Tarnocai:

What we are hoping in Canada is that the *Soil Taxonomy* will develop as an international system. We would like to see a system we could use for international communication in soil classification. We did just that in 1978 during the 11th I.S.S.S. Congress.

Guy Smith:

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

Tarnocai:

It's too bad because I think we are looking for it. That's why, when this offer came from Guthrie, I think it was very well received. Everybody looked at this as the type of cooperation we needed for the future. We would like to see an international system because the Canadian system is very narrow, just Canadian, that's all. We would like to have a system where we can use the same terminology.

I think we are much happier with U.S. Taxonomy than with the FAO system.

Question 13

Guy Smith:

You have a number of teams working around the world in survey projects, haven't you?

Tarnocai:

We have the CIDA arrangement for the international soil survey.

Guy Smith:

I ran into some Canadian assistance in the West Indies but not with soil survey. But the Canadian government was making contributions.

Tarnocai:

Soil survey, as such, is making a contribution. In most cases they are using the already available local classification or the U.S. classification. Our system is not adapted to the tropical regions.

Question 14

You are going to have a conference in New Zealand? Next month.

That has nothing to do with it (AID involvement). That's only New Zealand soil scientists. But we have at the moment six international committees at work and AID funds them at least to the extent of one meeting a year. In an area where there are extensive soils of the sort they are working on. Most of their work is by correspondence. But once a year, they are able to get together. The problem is getting the money from AID. And so they generally have about three weeks, one week of discussion of something like this, and two weeks out in the field where they can look at the actual soil and discuss things so that they can realize whether or not they are using the same language.

Question 15

What areas are they working in then?

Guy Smith:

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

Kenya.

North of there.

North of Kenya? Ethiopia?

South of Ethiopia.

Sudan?

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

Question 16

Now the study of Brazil in the tropics would expand the Alfisols and Ultisols at the expense of the Oxisols, isn't that the thrust of the committee's work there?

Guy Smith:

I don't know. Brazil surely has large areas that are not involved in this argument amongst the committee on lower activity clays about what is an argillic horizon. That's been their principal problem. To distinguish these soils from the Oxisols.

Half the soil's composition is quartz, paleonite (?), probably lacking a lot of gibbsite, iron oxide. There is some fairly good structure development in the B horizon but really no identifiable clay. Composition-wise it would be like Alfisols. Morphologically they might represent - at least macromorphologically - something like an Alfisol or an Ultisol.

Mostly Alfisols in Africa, mostly Ultisols in South America.

Question 17

Moormann is working with which committee?

Guy Smith:

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

Question 18

Who will make those decisions whether to change or not?

Guy Smith:

I think mostly they will rely on the chairmen of the International Committees. There isn't anybody in Washington competent to consider whether or not to adopt except as he relies on the Committee itself. But these are truly international committees with representatives from all over the world where there are such soils. The Canadians don't get in on this low activity clay business because they don't have any.

Unless you have some people working in those parts of the world.

Guy Smith:

Weil, if they are working there, they may get involved.

Rust:

The problem of West African soils is a little perplexing. I had a lot of difficulty seeing Alfisols there.

Guy Smith:

These committees always have a number of members with axes to grind. They want to reorganize *Soil Taxonomy* completely and make it to fit their own prejudices instead of a compromise. Every committee has quite a number of these people.

Rust:

That will probably get Taxonomy into international acceptance as much as anything we can do, that is, to have international committees working at developing it.

(What is the Benchmark soils project?)

The one that is based in Hawaii and Puerto Rico.

Guy Smith:

They have laid out experimental fields on the basis of the soil family to see whether or not results within the one family are consistent enough that research experience can be transferred at the family level. For all the fine details, we have series, but still the general management of a family is supposed to be very similar. The Benchmark soil project is based at the Universities of Hawaii and of Puerto Rico. The Soils Science Department in Hawaii has a newsletter that reports the news on this about four times a year, I think.

They apparently have a number of sites in the Far East now. Benchmark sites.

Guy Smith:

And they have some in Africa. They tried desperately to establish at least one in Venezuela but Comerma was away and the people that were there refused to do a thing about it. They have some very nice places to set up stations in Venezuela but they just didn't respond to Beinroth's influence and so nothing is in Venezuela that I know of. But Puerto Rico University has some fields in Africa.

Rust:

I would think FAO would be very interested in this project's success.

Guy Smith:

The Director of Soil and Water in FAO is Rudy Dudal and he is violently opposed to *Soil Taxonomy*. He told me a couple of years ago, "You can not transfer experience on the basis of the far ... Well now, the FAO view on soil and water has reversed that statement. But I don't think ... dal wrote it.

Tarnocai:

That's surprising, Dudal was up our way a couple of years ago, and in talking with him he seemed to be quite supportive of the U.S. *Soil Taxonomy*.

Guy Smith:

Well, he's refused to accept diagnostic horizons but he used to recognize soil temperature/moisture as diagnostic criteria. Where he needs it most is on his small-scale maps. On large-scale maps we have it as a family criteria with finer breakdowns. On the large-scale maps you need it to make any interpretations whatever of his twenty-three orders.

Rust:

Admittedly he's playing quite a political ball game in the sense of having to merge many concepts. I'm surprised that he won't accept the idea of technology transfer.

Guy Smith:

He wouldn't two years ago.

Question 19

On soils of low activity clay, what is your opinion? Some I have seen seem to be a lot more like other Oxisols than they are to the concept of Alfisols as I envision the concept of Alfisols from the midwestern U.S.

Guy Smith:

They are not like those in the U.S., they are more like the central concept of the Paleudalfs. But still composition-wise they would be Paleudalfs, probably have a lot of kaolinite, but still aren't oxidized as much. Don't have as many of the oxidic minerals as some of these in question.

We don't have too many Paleudalfs in the U.S. to judge by. Soils in the valleys and south from Pennsylvania range from Alfisols in Pennsylvania to Ultisols in Alabama. There are a lot of them, and certainly they are very red. Now many of them are very dark red and have acidic mineralogy rather than kaolinitic. They have no ideal place for sure. They are very thick with very fine texture. Those in Africa are derived from more acidic rocks and much more quartz sand in the limestone valleys. You get soils from limestone there and they will be very similar soils. There is not much limestone in Africa.

Question 20

Which soils are you talking about?

I thought that they would have kaolinite and some 2 to 1 minerals like aluminum-layered silicates and that type of thing. You would have more silicate minerals and oxidic minerals in most of those soils. Now that is not the case.

In Africa?

No, in the U.S.

Guy Smith:

In the U.S. I don't know what data they have now. When I retired there were very few data. We had CEC but not many studies on mineralogy. In the southeast this also involves soils like Norfolk and Ruston. Stan has been having a lot of trouble with the Southern Work Planning Conference who rejected at one time this idea of clay activity because it was going to split Norfolk and Ruston in the Mississippi Valley. There had been enough loess, enough montmorillonite blowing around that the clay activity there is well above the limit for Oxisols but Norfolk and Ruston on the coastal plains, in North Carolina and South Carolina and Georgia, are well within the range of Oxisols. It is around eight or nine meq. per hundred grams of clay.

Rust:

The Norfolk and Ruston concepts must have been about as wide as Miami was once.

Guy Smith:

They still are. Stan has been working with the Southern Regional Work Planning Conference. They have to divide these series. But it isn't the sort of thing that they are going to accept the first time it is proposed. Stan is working with the Soil Testing program that AID sponsored in South America. The management practices are vastly different in the Ruston in the Mississippi Valley and the Ruston in North Carolina.

I would think so, with that much difference.

As you well know loess goes darn near to Georgia. I mean, from the Mississippi Valley. When I got out of school I thought I knew everything about soils. My first job was in the mountains bordering Georgia, right in northeast Alabama, right by Chattanooga, Tennessee. You go off the mountain there. The first trip I took there was to see all these silty soils. "Well, where is the silt from?" I said. I kept asking the old-timers and they said "Oh, hell, this is from siltstone." I thought I knew something about geology, in fact I did know something about bedrock geology. "Well, you show me the rock from which the soil formed." They couldn't do it because they weren't that fine-grained. Coarse grains were close to clay. They weren't in the silt range. Not in the sand range. Now, everybody accepts the fact that loess from the Mississippi Valley went all the way over to the northeastern, northwest Georgia. On top of the mountains. There is sandstone before you hit the limestone. The soils are not formed from that rock. As a young guy I'd taken a lot of geology and mineralogy and stuff and I said, "You mean to tell me that sandstone weathered through to that silt?" I couldn't believe that. I was stubborn on that one.

Question 21

Tarnocat

Many pergelic soils are described in the Exploratory Soil Survey of Alaska as having a deep permafrost table although it is indicated that they have a mean annual soil temperature of lower than 0 degrees C. What is the critical depth for the active layer, that is, the layer which thaws and freezes annually, to start affecting the soil forming processes?

Guy Smith:

I have no information really that there is an answer to that question. When we wrote *Soil Taxonomy* we were just thinking about the Exploratory Study of Alaska, which is virtually the only place in the U.S. where we have permafrost. There is a bit in Montana. But that is about all. So, I can not answer that. It's going to take much more knowledge than I have.

I would say the permafrost table is high enough so that water perches above it. Seems to me that would be a critical depth. Within the zone of processes that depend on the surface situation. So that if your perched water is not as high as, say the depth of the spodic horizon, then below is critical.

Question 22

Tarnocai:

Is the mean annual soil temperature the best single value indicating the thermal regime of the permafrost soil, especially as related to soil-forming processes, soil properties, and utilization of the soil? Just very briefly I want to explain that in the Canadian system we rely more on the presence of the permafrost than on the soil temperature, which is also a thermal indicator indicating a certain thermal regime.

Guy Smith:

Again, I don't know. Normally I discussed the single value, but prefer to use combinations of values. Which would be the best single value, I would not know, but I generally do not like to use single values. I like to use limits of one sort in combination with one set of properties and another sort in combination with another set of properties. I seriously doubt anyone who wants to explain everything by single values.

Question 23

Tarnocai:

Now still with the terminology point: Could you explain the term "ruptic" as it applies to cryoturbated or permafrost soils? I would like to explain that the problem is that we feel the U.S. term ruptic is similar to the Canadian term 'turbic' or 'cryoturbic'. In the report "Exploratory Soil Survey of Alaska", a relatively small area was designated as ruptic soil. During the northern tour of the Eleventh Congress of ISSS along the Yukon and Alaska border region this question arose and it was indicated by the Americans that the areas adjacent to the Yukon in Alaska were not considered to be 'ruptic' but on the Canadian side, however, the soils were 'turbic'. So my question is, could you explain the term 'ruptic' as it applies to cryoturbated or permafrost soils?

Guy Smith:

The term 'ruptic' indicates that the horizons within the soil are not continuous over the area of a pedon. The discontinuous nature of the horizons may be due to one of at least three things, you may have a horizon that is just forming and it forms in spots rather than uniformly over the whole area, this is not uncommon but perhaps it is more common when one starts with a uniform parent material (and) horizon development proceeds uniformly over a large lateral area. But it may also indicate the destruction of horizons where the horizons when destroyed, are destroyed in spots, tongues, what have you, rather than uniformly over large areas. This is the normal destructive process. The third is the soil movement which we get in at least two kinds of soil; one is in the Vertisols, where the soil shrinks and swells. There is considerable movement in Vertisols the underlying material is often pushed up in the centers of the polygons, polyhedrons perhaps, and emerges at the surface in Vertisols. Exactly the same thing can happen in the presence of a pergelic temperature regime where you get frostboils sometimes in the centers of your polygons, but the horizons are not continuous anymore. If you have a frostboil in the center of your pedon and (if) you have thick organic material on the edge of your polyhedron, the ruptic merely means that the horizons are discontinuous on a very small scale which is repetitive. This disagreement on the International Society tour reminds me that when we made the soil map of North America for FAO and UNESCO, this difference of opinion existed already. And we had a lot of trouble in drawing a boundary that roughly parallels that border. Apparently there were differences of opinion and nobody has done a great deal of work on either side of that border.

Rieger:

Ruptic, technically, is not the equivalent of cryoturbated. These are two different concepts. A good many nonruptic soils in the Latin and American classification are classified as ruptic soils. It is a different concept. For example, take the pergelic Cryochrepts. Some of them have, as Guy has just pointed out, a thicker histic O horizon in the troughs between polygons, (than in) the centers. Well, you see the histic epipedon is not continuous throughout the pedon, therefore, it is ruptic. However, you will find other soils where the histic epipedon is continuous, also polygonal, also strongly cryoturbic, but they are not ruptic.

Question 24Tarnocai:

This leads into my next question: Do you think a term other than "ruptic" should be used to indicate the presence of cryoturbation in the soil?

Guy Smith:

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

Rieger:

In the case of the wet soils, yes, but not all pergelic soils are in cryoturbation. There are soils with rather deep perma-frost in rather dry climates, like the interior of Alaska where there is very little, if any, cryoturbation

Guy Smith:

Well, that reflects the ignorance of the Washington staff at the time that *Soil Taxonomy* was written. And if there is a need for another term, I think it should be proposed.

Reiger:

If there is a need for a term other than ruptic?

Guy Smith:

Yes, and pergelic.

Reiger:

Ruptic is used in a lot of places other than permafrost.

Guy Smith:

But ruptic in combination with pergelic would make that distinction.

Question 25

Tarnocai:

Why are soils associated with near surface permafrost recognized only at the subgroup level in the U.S. Soil Taxonomy? Their unique properties, reflecting the cold environment, stand out as the basis for an order level split much more than is the case, for example, with the Vertisols or Aridisols. I am asking this question because this is the question we hear very often in Canada.

Guy Smith:

Would you combine this thought with your next question? Just combine the two. Basically they relate to the same problem.

Tarnocai:

Properties of pergelic soils occurring in different orders (Entisols, Histosols, Inceptisols) are much more closely related to each other than to other non-permafrost soils within the same order. Do you think this causes a discrepancy in *Soil Taxonomy*? Some of the similarities of permafrost or Cryosolic soils, in the Canadian sense, are: 1) presence of near surface permafrost; 2) cryoturbated soil pedon (in the Canadian North, cryoturbated soils are the dominant soils); 3) unique (thermal) properties; 4) presence of ground ice in the soil, often in the form of pure ice layers; 5) associated with patterned surface, or patterned ground; 6) unique micro-morphology; 7) the utilization of these soils, (either mineral or organic) requires similar methods as concerns engineering, (construction of roads, etc.) and sensitivity towards use.

Guy Smith:

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

Question 26Tarnocai:

I just want to turn this around and ask: What do you see as a disadvantage of separating permafrost soils at the order level as in the Canadian system of soil classification? The disadvantage of recognizing a separate order.

Guy Smith:

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

Question 27Rust:

Thank you, Dr. Tarnocai and your contributions to this same subject, Dr. Rieger. Are there any other questions from the group that relate to this same cold topic?

Rieger:

I do have one again, a general sort of thing. The Russians, as you know, in their classification use vegetation as a guide to classification, though I understand from conversations this morning that is changing. In connection with pergelic soils we can have Pergelic Cryochrepts under forest and, Pergelic Cryochrepts under tundra vegetation. Permafrost can be

as much as two or even more meters deep or in the case of the forest, (it) can be quite shallow. All of these soils are lumped under Pergelic Cryochrepts. At the moment we really have no way of separating them for any number of purposes. For interpretation, certainly the soils with tundra should be separated from the soils with birch forest. We use the phase, of course, but that is not very good, I think. What can be done, short of very exhaustive soil temperature difference between these forested soils and the tundra soils that can be used to separate them?

Guy Smith:

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

Rieger:

Well, it is at a consistent elevation. This matter of a tree line, for example, this is definitely temperature related.

Guy Smith:

Yes, if it is a matter of the timberline.

Rieger:

Well, this is where the situation occurs.

Guy Smith:

We often have seen a soil that is normally above timberline lying well below one that was below timberline because of frost pockets. Now this, again, is hardly a soil feature. It is a matter of the length of growing season. The length of the growing season can be treated. If it can be related to soil temperature (it) can be treated at a series level. If it is unrelated to temperature, I wouldn't know how to do it. If we take the soil series that starts around here, the Clarion, and carry it south to Des Moines, productivity is considerably greater in Des Moines than it is here because the growing season is longer. Now it is conceivable that one could use this, say, at the series level, because the soil is colder here than at Des Moines, or it can be used as a phase. The minute you build it into your taxonomy as a series the plant breeders are going to come along and change all this and you will find your taxonomy is tied to an agriculture that no longer exists. For this sort of thing I would prefer a phase. I can give an example in Canada where you made an interpretive map for wheat production in the prairie provinces and before you could get it printed the plant breeders came along and pushed the wheat line many miles to the north. The map was made doubtful because it had been made as an interpretation rather than based on soil properties. So for this sort of thing, I much prefer phases to putting it (in) small, say one or two degree, increments of temperature as series limits.

Rieger:

These are such profound differences that occur over such wide areas that it would really be desirable to have something in the classification system to account for them.

Guy Smith:

It may be very difficult. It may only be the growing season because you have willows in your tundra and they are one of your dominant vegetation (types).

Rieger:

The reason given by the ecologists is (that) the July temperature is less than 40 degrees (5 degrees centigrade) above tree line. Whether that is reflected in soil temperatures or not, I don't know.

Guy Smith:

Well, it might be reflected in temperature. It might be a very small difference. I don't know enough to really give a good answer only to explain what I see would be the principles involved. But you have lots of Salix in your tundra. They may not be greatly different from your birch. These are very small trees, you know.

Question 28

Rust:

Well, then maybe we can move to a warmer topic. Any other questions that we could say relate to the general concepts?

Hall:

There is an overall background feeling that I get here about change in the system and I don't know whether you want to test this or not. You spent ten, fifteen years developing this system and as you developed it you went through the approximations and presented it. What was your feeling about future changes, what kind of a structure did you visualize to implement changes; should we set up for these changes? Could you expand on that topic?

Guy Smith:

Well, when I retired we had worked out a provisional soils memorandum outlining procedures for making changes. We know changes are going to be essential for at least one or two reasons. We find soils whose existence we never suspected or we learn more about soils and we find that for our interpretations we must use parameters that did not occur to us at the time that we were developing *Soil Taxonomy*. I am personally of the opinion, I think I have already expressed here, that these changes should be considered very broadly before they are accepted by a group of people or groups of people who have some familiarity with the soils that are under discussions or the changes that are under discussion. This is why we have these international committees working on necessary changes in kinds of soil that we don't have in the United States. Where the kinds of soils are well represented in the U.S. and in other countries (and) do not significantly differ from ours, I think that international committees are unwarranted. But for the kinds of changes we've been discussing on cryo-soils, I think it would be advisable if we could, have an international committee. I think this is off the record, but I'll delete when it comes. Because of the changes in the Russian attitude within the last couple of years, it is not inconceivable that they would be willing to cooperate on this, given one of two or three things. First, that they could travel to countries outside of the U.S.S.R. or that they could arrange for travel within the U.S.S.R. for these committees. It's quite likely that they have a great deal of experience that would be useful to us in north Canada and northern U.S. They do cultivate rather extensive areas with perma-frost in the Soviet Union but this is not common in North America. And from the publications I have been able to find, I don't see how they can do it when we can't. It may be they have techniques we don't know about, it may be that things are very different, that they have much hotter summers than we do. Very difficult to read the translations of their literature and figure this out. I have tried.

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

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

Question 29

Hall:

Who are the main people in this group? You were the leader of it. Dr. Kellogg, Dr. Cline - who were the other main contributors, would you say, to the main overall effort?

Guy Smith:

Well, principally, the principal correlators and the state correlators. Dr. Kellogg had very little time for this, Dr. Simonson, none.

Hall:

What kind of response did you get from universities? Any?

Guy Smith:

Quite a bit, yes, at the work planning conferences, a great deal. Every state was represented except New Jersey and Virginia. They were represented, but there was no cooperation.

Hall:

There is a difference between being represented and having a strong input in response.

Guy Smith:

I think we had very good input from 48 of the 50 states. Well, Alaska didn't do anything. I don't know whether they had anybody in soil science in Alaska prior to 1960 or 1965. I don't remember anyone from the Experiment Station in Alaska at any of the western meetings.

Rieger:

There is now Drew, of course, formerly of Nebraska, now head of the Alaska Experiment Station, for some time. To the best of my knowledge he has never contributed anything for classification or studies of soil temperature in Alaska. He or anyone else.

Rieger:

Well, there was the old fashioned type of SCS, you know, code survey going on, beginning in 1948. Some of Nick's people were involved in that. But that all stopped abruptly in 1955.

Guy Smith:

And this may be why we didn't do much (with the) cold soils. Sam Rieger wasn't up there yet and the Alaskan people were doing nothing and we simply knew very little about them.

Question 30

Farnham:

Did you ever have any input, Guy, from Nick Holowaychuk's group on the tundra? At the same time Tedrow was up at Barrow, Alaska. I was there for a summer, and Finney was there for two summers, and K. Everett from Ohio was there, a lot of people were there. A lot of that information is published, Guy.

Guy Smith:

Holowaychuk, I think, worked on mapping some areas on the coast, Cape Thompson. Certainly all the information he had was in our hands.

Farnham:

That's what I wondered. That was about '61 or '62.

Guy Smith:

We have the descriptions, probably have what data was available to him.

Stout:

Now, Tedrow, of course, went on his own way, on his own classification and wouldn't participate at all in the new taxonomy.

Guy Smith:

He explains why in his book on polar soils. He says he distrusts any classification made by committees. So he evidently wants some one mind to understand all the world soils. That's the alternative. I am quoting almost verbatim.

Farnham:

He wasn't even receptive to any group, Sam. I think Nick tried to make an appointment with him to go from Cape Thompson over to Barrow and never got anywhere because Tedrow didn't want to see anybody. I (would have) taken him out in the field. I don't know if Sam had the same trouble.

Guy Smith:

This will all be off the record. Don't send it to the New Jersey Agricultural Experiment Station. I think he's retiring this year.

Rust:

Any other comments or questions relating to our general background?

Stout:

I think that Guy has brought out some good points here and I just want to come back again and hit them. It sort of explains a little bit why Taxonomy, some parts of Taxonomy, seem to be in a state that people are questioning. But, Guy, Taxonomy was made on the basis of the knowledge that we had during the period that we were working on the thing. Second, it was based primarily on our knowledge (of) the soils of the U.S. These are the kinds of proofs that we made. I think that it is very important to do that. In the last few years, particularly I think, Guy, you have been all over the world and various places, spreading the 'gospel' of Taxonomy. You talked about the classification system that we have. The merit that it has been recognized by others and is being adapted and adopted by other people. When other people get into this, as you point out, bringing other kinds of soils that we do not have and other situations (then) we (can) go back and start taking another look at it. I think that is about where we are.

Guy Smith:

That's all we can do.

Stout:

It is a dynamic thing and it is set up to accommodate change. I was very pleased to hear you say that it isn't the end. You didn't say it in exactly these words but it's a means to the end, that it's a pretty good one.

Guy Smith:

And we won't stop changing it until we stop learning things about soils.

Question 31

Hall:

Somewhere in your writing you suggested, I believe, that this was a U.S. system. When you started to develop this, did you visualize an international system or what? You were primarily with USDA and with a production oriented system. I have often wondered if in the grand scope of things you visualized the whole world accepting this system some time or whether you had a lesser vision at that time?

Guy Smith:

Well I did not visualize that the whole world would accept *Soil Taxonomy* and use it as such, but I did visualize that the best system for the U.S. was one that would accommodate all soils of the world. So that we could transfer knowledge to (or from) anywhere in the world if the soils have been studied enough to place them in our system. We spent a good deal of time when we first began to develop this system in studying the soil classification systems and the soils of various developed countries, particularly western Europe, that had on-going soil surveys. I could see no reason to visit a country where the soil classification was a theoretical sort of thing. I tried it and I found that it was useless. They had nothing to tell me. I could use only what I could see about their soils myself. The justification for spending so much time in Europe with countries with soil surveys was that we could potentially benefit the American people if we could uncover some soils information in these countries that could be transferred to the U.S. This was all we could do according to law. Now AID has the opposite restriction, but it is supposed to spend it's money for the benefit of these other countries, increasing food production, what have you, rather than for the benefit of the U.S. directly. The cooperation now of AID with SCS permits us to work on a world-wide basis in countries that will admit us.

Question 32

Rust:

One question, Guy, that comes to us from some of our people working in developing areas trying to introduce *Soil Taxonomy* is the difficulty they have in applying it because of the lack of quantitative data on their soils. Therefore, they sometimes hesitate. Do you feel that this is going to continue to be some problem in extending *Soil Taxonomy* to the developing countries?

Guy Smith:

I have answered this one before, but I will give you a brief answer now. When we started to map soils in the U.S. we started with soil types and series. We had the Miami series that ran from North Dakota to Maine because it was developed in glacial drift and loess and what have you. Any glacial deposit was Miami, at one time. We learned very shortly that that wasn't satisfactory. I don't have any notion how many series have been cut out of Miami but it must run in the hundreds. These developing countries are in the same situation with respect to series. They don't know enough to begin to define them. When you go to a higher categoric level, one above the family at least, relatively much less information is required about the soil, in terms of quantitative laboratory information. One map at the subgroup level (can be made) with relatively little quantitative information. That (which) is required above the subgroup level can generally be inferred in some very simple measurements that can be made in the field, or, if one requires something more sophisticated, I hope we still have this portable laboratory, about the size of my briefcase. It can be taken to the field and will make most of the measurements that are required for a classification at the subgroup level at least.

Question 33

Guy Smith:

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

Rust:

I think that essentially answers question 4 on my list. As you say, Guy, you had discussed this earlier somewhere.

Question 34

Rust:

I believe question 1 is in regard to the rationale for establishing limits in the definition of class in several categories. I believe, Guy, that you have made quite an answer to that question sometime back already in a paper which you presented in Venezuela. In June '76, you presented a paper entitled "Reasons for Limits Selected for the Definitions of Soil Taxonomy". I guess that would be your answer to our question 1.

Guy Smith:

I would have interpreted 1 a little differently. Because you speak of limits rather than *the* limits. I thought you were getting at the matter of definitions by limits rather than by type. When we started on the development of Soil Taxonomy, a good many of the correlation staff thought that we should, as the botanists do, define our great groups or other taxa by type. Ruston and Norfolk were typed Red-Yellow Podzolic soils; Miami was typed Gray-Brown Podzolic soils, and Marshall, I suppose, the type Prairie Soils and so on. Then we would analyze these type theories and there would be no limits between the taxa, they would be the ones most closely related to that particular type that would be grouped in that taxonomy. This is an appealing way to define things but it leads to enormous difficulties of application unless you are going to run all of your decisions through one person. We found this was impossible. Our correlation process failed to keep up with our mapping when everything had to go through the office of the principal director of soil correlation. We had many arguments, between Marlin Cline and myself, about what he called "building fences". He says, in his Agronomy Monograph article, or in some of his papers, that a class is formed by ties from within not by limits from without. Now I used several times the illustration of Gray-Brown and Red-Yellow Podzolic soils prior to Soil Taxonomy. We had a field correlation trip between the northeastern and the southern states in Virginia and Maryland and we came on the Chester series in Maryland. The

Chester series resembles the type Red-Yellow Podzolic soils in mineralogy, in base saturation. It resembled the Gray-Brown Podzolic soils in the thickness of the solum, and the colors. The correlators from the south said "that's a Gray-Brown Podzolic soil", the correlators from the north said "that's a Red-Yellow Podzolic soil" and we never could resolve the issue. It resembled one in one respect and the other in another respect. Which one to give priority? So it seemed to me that I preferred the logic of Bridgeman, and that's the logic of modern physics, how to write definitions. He was the first one to propose what became the "operational definition". You write your definition in terms of the operations you go through to reach your decision. This could be, then, something that could be applied uniformly by a great many people instead of going through a single mind. This was the rationale behind using limits to taxa instead of the central concept. That's the answer I would have given.

Whiteside:

That's a rather fundamental change from earlier classification of taxonomy.

Guy Smith:

This taxa specimen in botany - I am going into this on my seminar - gets me in just as much trouble as taxa specimen in pedology. I'll give an example on Thursday.

Question 35

Peterson:

I am sure you are tired of this question, Guy, but it seems to keep coming up; it is one of those aggravating arguments. Is the pedon a sampling device or a real individual?

Guy Smith:

Well, I get asked that question by everyone. I'd say very briefly that the pedon has no natural boundaries. Its boundaries are almost completely arbitrary depending on where you start your examination. You can have an infinite number of pedons in most soils in a few acres and so I don't see how it can be considered anything but an arbitrary sampling device.

Question 36

Rieger:

In the Taxonomy there are a number of references to a biological zero at 5 degrees Celsius. And yet there is all sorts of evidence published that there is considerable biological activity at temperatures well below five degrees. I am wondering if this concept of biological zero, at 5 degrees, is valid?

Guy Smith:

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

Rieger:

For example, the tundra plants where we do make soil surveys do grow at very low temperatures. The reference to biological zero, which I am looking at here on page 55, is in the discussion of the aquic moisture regime. It says, "it is implicit in the concept that the soil temperature is above biological zero at some time while the soil or the horizon is saturated" - which I interpret to mean that the soil is not aquic unless it has a temperature higher than 5 degrees centigrade. Am I correct in that?

Guy Smith:

That would be correct for everything but the tundra in Alaska. We discussed at some length yesterday that at the time Taxonomy was written we had no source of reliable information about those soils. There was general information in the literature but we had no one in Alaska who would cooperate with us and the Canadians also decided not to cooperate so we went ahead with the information we had.

Rieger:

Will it be changed now?

Guy Smith:

When we change Taxonomy, if we have the information. You can only do what is possible.

Question 37

Franzmeier:

I'd like to hear comments on the fragipan, on the origin of the term, and the kinds of horizons that are called fragipans, especially in other parts of the country and other parts of the world. One of the questions would be - are different kinds of horizons now included under the term 'fragipan'?

Guy Smith:

Well the origin of the term was Latin *fragilis*, for brittle, because, in some parts of the country, (this was in the '50's) called a brittle pan. I think there is little question that the definition is completely inadequate. There is no operational definition possible at this moment. There is probably no diagnostic horizon that has been the subject of so many doctorate theses and we still don't know very much about it. It is quite possible, and that is implied in Taxonomy, that there is a 'cement' of some sort in the fragipan. But it's not necessarily the

same in all fragipans. The studies using agents to remove silica or aluminum or iron show that one pan is aggregated by one treatment and another by another but a single reagent does not 'cement' all fragipans. So it seems likely to me that there is more than one kind of cement in different fragipans and yet we have no general theory whatever to account for this. The distinction between fragipans and compact basal till such as one gets on a drumlin, was discussed at some length at Cornell and the same problem would exist here in Wisconsin and Minnesota. The very compact tills are as much a barrier to water and root development as is the pan, and yet one can not blame all pans on compaction by glaciers when one sees them in loess in Mississippi and Louisiana. Those have never been glaciated and, so far as we know, have never been frozen at any time. Fragipans in Belgium and Scotland are commonly attributed to permafrost but this is speculative at this moment so far as I am concerned. If permafrost forms fragipans, Dr. Reiger should have been finding some in Alaska. There are many who do in Scotland and Belgium.

Franzmeier:

How about those in New Zealand, in loess; are they similar?

Guy Smith:

They are very similar to the ones here, in general. They have one genetic difference in that they are in a much drier climate. They are largely confined to soils with ustic moisture regimes instead of udic. But they occur in ustic moisture regimes so rarely that when the New Zealand people find one in a udic moisture regime they group it with those that have ustic moisture regimes even though their classification is supposed to distinguish those.

Question 38

Franzmeier:

Were you the first to use the term 'fragipan'? Did you coin it?

Guy Smith:

I coined it, yes.

Franzmeier:

When was that, do you remember?

Guy Smith:

That would have been about 1948, I think. It was when we were trying to improve the '38 classification and I was chairman of the committee on Planosols and I realized there were at least three kinds of Planosols in the U.S. - those with clay pans, those with fragipans and those with duripans.

Question 39

Hall:

You say that there is no acceptable definition of a fragipan from your experience now. What morphological and physical characteristics would you emphasize in a definition of a fragipan?

Guy Smith:

The only thing I would know would be the brittleness when the soil is moist or wet. The brittleness is weakened compared to the dry pan but still the brittleness remains when the soil is moist or wet. In a weaker form but detectable by the fingers on a sample that hasn't been disturbed by an auger.

Hall:

What about size of the units and thickness of the pan, what kind of minimums would you have on those?

Guy Smith:

The size of the units, we have pretty well standardized throughout Taxonomy at ten centimeters or more that are free of roots. Thickness of a fragipan is very difficult to determine because it generally is quite thick in its lower boundary, something that no two people would agree upon. It's a very diffuse boundary unless, of course, there is rock or something underneath.

Hall:

We had a situation where we had a lithologic discontinuity and we felt we had about four inches of fragipan over this underlying material. We had some discussion as to whether that was really a pan because of thickness? It certainly did present problems for root penetration because of thickness.

Guy Smith:

Well I have not seen so thin a pan but I suppose it could form, say, on top of sand or something.

Peterson:

When you mention brittleness, I was writing and I don't know if I got exactly the context. Are you saying that brittleness is the sole common characteristic between the things that are being called fragipans in terms of operational definition?

Guy Smith:

That's all that I know of.

Peterson:

Does that tie together?

Guy Smith:

The absence of roots.

Peterson:

Would it be fairer to say that the brittleness goes together with slakability. Would this be a way to put it - that if you drop the dry material in water it will slake? Where does that work?

Guy Smith:

That works so far as I know. It fractures into gravel-size fragments for the most part. It does not slake like a densipan which simply becomes a fluid mixture of water and silt and sand-size particles and slakes, forms an angle of repose of less than 15 degrees as a mud. The fragipan does not slake in that manner but it does fracture. The duripan is cemented to the point where the dry fragment will not fracture when put in water. That is an operational distinction between the fragipan and duripan where one leads into the other.

Question 40

Peterson:

Do fragipans slake into sand-sized material, as fine-gravel sized? Is there a range in the sizes?

Guy Smith:

They are mostly gravel-sized particles. I don't know whether an individual sand grain will fall off or not; they probably will.

Peterson:

Well, I was trying to get an idea as to the size one would want to ask for in terms of maximum size of chunks that result from slaking.

Guy Smith:

They are largely gravel-size or some will be, I think, the upper limit of gravel. What is the upper limit of gravel? Mostly they will be less than 7 1/2 centimeters. You understand it depends a little on the operations you use when you are slaking. If you put in a large chunk, the sides slake off but they compress the interior of your large chunks. The fractures may form there but it doesn't fall apart because it is held by the fragments around it.

Peterson:

It also depends on how dry it is, too, doesn't it? Shouldn't chunks be completely air dried?

Guy Smith:

They should be air-dried or oven-dried. Air-dried is the normal procedure because we can do that in the field and we don't have an oven in the field.

Peterson:

Has anybody ever tried sequential wetting and drying, in other words, drying and re-wetting the fractured material? Would it continue to break as it is wetted and dried and wetted and dried again?

Guy Smith:

I do not know, I haven't read of such trials. Many of the theses on fragipans are unpublished and only some are in the literature. I don't know the answer there. On the densipan we did try this approach. We slaked both disturbed and undisturbed material and the bulk density of the dried slurry, in either case, was the same and it was 1.7 g/cc.

Question 41

Aandahl:

This densipan, that's a new one on me, Guy. Where did that concept come from?

Guy Smith:

Well, that is an albic horizon above either an argillic or a spodic horizon. In the West Indies, in South America, the only densipans I know are above an argillic horizon in an Aqualf. They're unique to some extent in that they are pans but they are so close to the surface, a matter of 15 centimeters, that your rooting, your water storage is restricted to that very thin layer. The densipans occur in temperate climates in New Zealand above a spodic horizon and they've also been reported above a spodic horizon in inter tropical regions as in Sarawak. I've seen them in Australia and Queensland just near the margin between tropical and non-tropical areas. With a bulk density of about 2 when wet, saturated, permeability near zero, roots are absent. It is impossible to dig with a spade or an auger in a saturated albic horizon. One has to have a bar or a pick to break a small hole through the pan. Then you can break out large chunks which come away cleanly from the underlying argillic or spodic horizon with an abrupt boundary.

Question 42

Franzmeier:

Is the coarse prismatic structure common in most fragipans you've seen?

Guy Smith:

Most fragipans, yes. In regions with a perudic climate the polyhedrons become larger and larger until they are virtually discontinuous. There are cracks but they do not completely surround the polyhedron. Cracks are bleached in those soils as well. It's part of the argument,

I suppose, that originated in New England about the distinction between a fragipan and compact till.

Peterson:

You mean the moister the climate, generally the larger the structural units until you find just randomly oriented cracks?

Guy Smith:

Yes, to the extent that the frequency of drying is a factor in the development of a polyhedron that is completely surrounded by the leached gray non-brittle material that permits water and roots to enter.

Question 43

Hall:

In our operational definition in the field, there are sometimes problems in determining whether roots are present or not depending upon the crop that is there. Occasionally we'll find the sides of the polygon free of roots. This is probably one of our biggest problems particularly if you are out in a bluegrass pasture or some other field, as contrasted to alfalfa or forest situations. Do you have any experience or comments on that?

Guy Smith:

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

Question 44

Hall:

In the classification system, we have the fragic subgroup in the Ultisols but not in the Alfisols. Is there a reason why you went this direction when you developed the system?

Guy Smith:

It's only that, when we provided the subgroups in Soil Taxonomy, we listed only the ones for which we had series in the U.S. Now it's my judgement that such soils exist in the Alfisols but the correlation staff, the state representatives, did not suggest anything along this line for Alfisols. I'm sure they exist in Belgium but it was not our principle to include subgroups for other countries unless they requested them.

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

Hall:

I can say it is difficult to map these fragipans in some cultivated areas because they seem so inconsistent. It seems they could be discontinuous close to the surface as a result of freezing. I don't remember that we discussed this with the people in Michigan. It is an interesting idea.

Guy Smith:

I tried to get them to study it and our administrative people were not interested.

Whiteside:

The McBride series is one, for example, that Yassoglou studied in his doctoral thesis* and yet going back to the areas of McBride we map it is very difficult to find fragipans. Maybe this is a documentation, it is an observation.

Guy Smith:

It could be documented if one had a fence line with a fragipan under the forest and then see what we have in the cultivated fields.

Peterson:

That certainly should be very easy in Central Lower Peninsula.

Guy Smith:

We have in Belgium in the loess in the cultivated fields the color pattern of the fragipan (with the polyhedrons with the model brown colors and the gray fillings between the polyhedrons) but the roots go all the way through everything and this stops at the fence line; under the forest is a fragipan. I think it would be good evidence the fragipan has been destroyed by something.

* Summarized in SSSAP V. 24, No. 5, pp 396-407, 1960.

Question 45

Peterson:

This question goes back to general philosophy, but we are on the diagnostic horizons. In one of your earlier answers there was a suggestion that the concept of diagnostic horizons grew out of the problems of use of the ABC notation for communication. Is that all there is to it, to the history of the diagnostic horizon? I've wondered if at some time they appeared to be a particularly nice device for characterizing different kinds of profiles and drawing distinctions of difference or similarities between different kinds of soils. Now that we have the diagnostic horizons to work with, thinking seems to be much sharper than it was, say, in 1950.

Guy Smith:

Well, in the early approximations we began very quickly to distinguish soils according to the kind of B horizons or the kind of A horizons. We talked about textural B horizons and podzol B horizons and chernozemic A horizons. It was not until we got into the development of the concept of Oxisols that we could get no agreement whatever amongst the people from different countries as to whether the Oxisols have or do not have, a B horizon. They argued so much about whether that should be called a B that they forgot to look to see how the definition grouped their soils. That was the last straw so far as I was concerned and so we shifted completely to diagnostic horizons instead of A and B and C horizons. There were other problems between the U.S. and Canada. If there is a horizon of lime accumulation, that is a B horizon to the Canadians but not to the Americans. Both sides had rather firm opinions on that subject. There seemed to be no compromise except occasionally, but one can agree on a calcic horizon when you are not arguing about A, B, C horizon nomenclature.

Question 46

Fenton:

As you know in the Midwest, we have a problem with eroded Mollisols. In the definition color and organic carbon content of the mollic epipedon are specified in terms of thickness requirements. In the development of criteria was consideration given to waiving the color requirement, if the organic carbon content was 0.6% for the required thickness?

Guy Smith:

I do not recall any such discussion. I am quite aware of your problem of Mollisols that have lost most of their mollic epipedon. It is not unique to the U.S., this problem. It occurs in other parts of the world also. Here again, I tried to get some hard core information about these eroded areas, what was actually present. I could never find out what the problem was so I made no attempt to solve it without knowing what was there. I thought that, since we are classifying the polypedon and, in the eroded areas that I knew in Iowa, there would surely be a higher percentage of any particular polypedon that retained its mollic epipedon. I thought that potentially it would be possible to derive a definition that would keep the whole polypedon as a Mollisol even though it has eroded spots. But I could not get the hard information I needed and finally the time came I had to write the book.

Question 47

Fenton:

In the Inceptisols there is no provision for a mollic subgroup which would handle some of those kinds of soils without argillic horizons. Presently in the Inceptisols, we include a wide range of color and thickness of the surface horizon from a very light colored surface, 5/4, all the way down to one with 9 inches of 2/1 color. Were any of those kinds of soils known?

Guy Smith:

We knew that the Inceptisols that had a high base status normally had a somewhat darker epipedon than those of low base status. That is a generalization, probably there are exceptions. There seemed to be no desire on the part of the people who had these darker colored Inceptisols to put them into a mollic subgroup so it was not done. It could be done.

Question 48

Holzhey:

You mentioned the chernozemic A horizon a while ago, would you care to comment on the role of the chernozemic A in the development of the concept of the mollic epipedon?

Guy Smith:

It was the only horizon that I could find that was common to the soils of the 1938 classification and suborder of dark-colored soils of the subhumid, humid climates, that is, the old Chestnut, Chernozem and Prairie Soils. I could find no other common feature they had. When combining that with a high base status we were able to arrive at the concept of some diagnostic horizon that would tie those soils together in the Taxonomy. This was where they traditionally had been, tied together but without a definition. When you examine the data, the descriptions of the soils that had this range of moisture from the Chestnut to the Prairie, it was immediately obvious that the drier the soil became the thinner was this dark-colored A horizon, which we began to call chernozemic A to distinguish from the more acid ones of the humid forested region or not necessarily forested, particularly under the heather in Europe. If we put a limit of 25 centimeters of thickness as the minimum that we would recognize, then we excluded the drier range of the soils in the Great Plains. If we develop a sliding scale, based on the depth to secondary lime, with the maximum thickness of 25 centimeters then we could tie them all together. This was what we tried to do in defining the mollic epipedon. In so doing we included some of the former intrazonal soils like the Rendzina. There seemed to be no good way to exclude them. At the time that we were developing the Taxonomy, these soils on limestone were commonly called Rendzinas even though they would have been called Chernozems in the absence of the limestone. This was the reason that we restricted the Rendzinas to soils that have udic moisture regimes. The Rendolls are restricted to udic moisture regimes because there they are only truly what was considered intrazonal.

Question 49

Fenton:

In the concept of cumulic - was the intent to imply genetic process? The reason I ask is that, in some of the categories, the organic carbon content is specified as being irregular with depth and in others it could either be regular or irregular with depth.

Guy Smith:

The concept of the cumulic was one in which there was slow addition to the surface of a Mollisol resulting in the greatly over thickened mollic epipedon. Depending on the stability of the slope from which the sediments came there could be pauses in deposition. We have very little data on that particular point. I don't know of any studies that have sampled in the cumulic soils in small enough increments to detect an irregular pachic but potentially it could be. I can't recall where in Taxonomy we require an irregular decrease with depth. It's one of two things, normally. It is an irregular decrease in carbon with depth or a high carbon content at depth that defines the concept of cumulic subgroups and fluventics. It is acquired in cumulic Hapludolls, to a large extent, but I don't know studies that prove that is correct.

Fenton:

In a soil like Webster in Iowa on a stable, or relatively stable, upland position - that was not the concept of cumulic?

Guy Smith:

I would like to correct that. I was reading the definition of the Typic Hapludalf which has a regular decrease with depth to a content of .3% or less within a depth of 1.25 meters. The cumulic, then, can have an irregular decrease or it can have more than 3/10 percent at a depth of 1.25 meters. It is not required to be a regular decrease in the cumulic subgroup, but it can be, if the content of carbon remains higher than .3 percent at 1.25 meters.

Question 50

Stout:

The Ustolls have both pachic and cumulic subgroups. The Udolls only the cumulic subgroups. We feel that often times the udolls have soils with a uniform, smooth carbon curve (pachic) included with those in which we have an irregular carbon curve. Can you give us some idea as to why the pachic subgroup was put in the Ustolls and not in the Udolls?

Guy Smith:

We have, in the Ustolls, soils that have a much thicker mollic epipedon than their neighbors. As the Ustolls get drier we normally expect the mollic epipedon to thin but in the regions where normally the mollic epipedon is thin, there are Ustolls with a rather thick mollic epipedon. The reasons for this, at the time we were working on Taxonomy, were unknown. As far as I know they are still unknown. The correlation staff felt that these should be separated from the soils with the thinner mollic epipedons. Soils with thickened mollic epipedons were recognized at the series level and the correlation staff wanted to carry this to a higher categoric

level so the pachic subgroup was introduced. I'm told, at Lubbock, that these pachic soils are more productive than the others. Although they receive so far as anyone knows, the same precipitation and the precipitation is one of the controlling factors on productivity in the Ustolls.

In the Udolls we don't have this variability in thickness of the mollic epipedon within the U.S. except where it is presumably the result of erosion, post-cultural erosion. In some Udolls of the world we now have to think a little bit about Borolls instead of Udolls. There are Udolls in Ecuador with a two meter mollic epipedon that runs from sideslope across the ridge and down the other side so it is not due to accumulation of materials as a result of erosion, natural or cultural.

Question 51

Stout:

I have one other question, Guy, concerning the thickness of mollic epipedons and the Ustolls. It always seems to me that they're reversed. You would think the colder climate would be more efficient; that's where we have 40 cm as break between pachic and typic subgroups. The Ustolls have the break at 60 cm. Could you give us any insight on the selection at those depths?

Guy Smith:

No, except that was done pretty much by the correlation staff and Dr. Aandahl was in on that. We'd be interested in his recollection.

Aandahl:

Two things, on that particular question, Mike. Pachic ran into the Calciquoll and therefore we had to choose the minimum or the maximum depth to the calcic horizon for the mollic epipedon. Back to the concept of the calcium carbonate Solonchaks. At one time, in North Dakota particularly, we had what was called a calcic Solonchak. There wasn't any distinction as to the amount of lime. We set up and used in the field the depth of fifteen inches where you got a definite increase in lime so it changed the color. It went from a dark color to a light color. That was carried over into the thickness of the pachic in the Mollisols where we have all these Calciquolls. We changed the fifteen to sixteen inches.

Guy Smith:

Sixteen comes out better than fifteen in the metric system.

Aandahl:

It was purely arbitrary. Again we had to do something to pull these soils apart, get some meaning to calcium Solonchaks. One other aspect relative to this pachic-cumulic situation out in the plains. We had a mollic epipedon and we couldn't use the term cumulic, as Guy pointed out, by accumulation. We argued it on the basis of more water for soil development. Now as we went from the Webster to Marcus and to the Primghar you had these changes. As we got out to the West, we didn't have the moisture. You may or may not have it as in the Aastad so we put the emphasis on the thickness and darkness and introduced the term pachic which had no indication of a cumulative process.

Guy Smith:

We had the general principle that we would not use cumulic in soils with argillic horizons. If the landscape was stable enough, you had an argillic horizon. That we thought indicated too much stability for a cumulic subgroup. In the Ustolls the pachic subgroups, I think, are in the soils that have argillic horizons. You may have both. Well, you may have cumulic or pachic in the haplic great groups and only pachic in the argic great groups. Argiustolls can be pachic, Haplustolls can be cumulic or pachic.

Aandahl:

You get out to the area of Arnegard or the Goshen and you frequently have a little run in drainage that goes on through which is temporary.

Question 52

Collins:

To get back to what we were talking about before we went on break; Cumulic Haplaquolls and the stage of development. We've had some discussion in Iowa about if a B horizon is present or not?

Guy Smith:

I'm not sure I understand the question. Can you rephrase it?

Fenton:

The question dealt with Cumulic Haplaquolls and the horizonation or the presence of the B horizon. We have a long history in the midwest. We had B horizons in some of those soils like Colo where they have been called AC profiles and ABC profiles. Is that your question?

Collins:

Well, the term cumulic gives the indication of sediments on a relatively young landscape position. Is that so or can the landscape be older than that?

Guy Smith:

We have provided a cumulic subgroup in the Haplaquolls. We have not provided a cumulic subgroup in the Argiaquolls. Now I presume this goes back to our general decision that we would not recognize cumulic subgroups, even though the mollic epipedon was thick, if the soil had an argillic horizon. This was on the theoretical grounds that the presence of an argillic horizon indicated more stability than the presence of a cambic horizon or the absence even of a cambic horizon. I think most of the Haplaquolls in Iowa would qualify as having a cambic horizon. But not all. The cumulic ones, probably not. The Typic Haplaquoll, I think, would have a cambic horizon. That would be something like Webster.

Collins:

Some of the soil scientists in Iowa see a Cumulic Haplaquoll and right away they say the soil has an AC profile even though there is some indication of a B horizon being present. They

say, well, the landscape position indicates a young soil so this is the way we are going to interpret it.

Guy Smith:

Soil Taxonomy does not deal with A B and C.

Collins:

Yes, but I am talking about the soil scientists' mental model of a Cumulic Haplaquoll.

Guy Smith:

Well, I am not concerned, myself, about the implications. These are A B and C concepts not well enough agreed upon, not well enough defined, that I don't even want to talk about them anymore.

Question 53

Turner:

I might make one comment for clarification. As I understand, most Cumulic Haplaquolls in the position in the solum where one might think of there being a cambic B it would be excluded from cambic B because it still qualifies for a mollic epipedon. Is that part of your question?

Guy Smith:

Do you mean that part of the cambic horizon would be part of the mollic epipedon?

Turner:

Not cambic horizon. If it qualifies for a mollic epipedon, it can't be a cambic horizon; they are mutually exclusive. You go from the dark colors into what most people have identified as the C horizon. The typical B horizon position has colors and organic matter content that would be part of the mollic epipedon. As I understand the definition of a cambic horizon, such properties are excluded. The horizon might have blocky structure and going back to the A B C nomenclature, we might choose to put a B horizon designation on it, but it would not be a cambic horizon.

Question 54

Franzmeier:

Relative to the moisture and temperature regimes we are faced with dividing a continuum up into certain classes. Perhaps you discussed this earlier but, what were the reasons for

drawing the lines where they are currently placed? Does it relate to natural vegetation, changes in cropping practices, that type of thing?

Guy Smith:

You are speaking now about the udic and ustic Mollisols?

Franzmeier:

I think mainly Mollisols and also the temperature of mesic and other temperature regimes.

Guy Smith:

If you started from Ames and went across the Great Plains you would be starting in a tall grass prairie. Somewhere around Lincoln the native vegetation becomes mixed. Somewhere west of Lincoln (you know where these lines are) we have a short grass prairie.

Aandahl:

Very far west where you get into the short grasses. Sideoats gramma will replace some of the switchgrasses.

Guy Smith:

But the vegetation, the native vegetation, did change as moisture decreased. We don't have much native vegetation anymore. It's mostly cropland today so when we examine the pattern of cultivation on this same traverse we find we are going from a region of the corn and soybeans. West of Lincoln you will begin to get an appreciable amount of winter wheat and sorghum. You come into the western part of the state of Nebraska and you find that to get maximum crop production they fallow one year and plant the next, so that they plant only half of the land instead of all of it. And the correlation staff and the states people on the Great Plains decided that we should subdivide the soils with ustic moisture regimes into udic, typic, and aridic subgroups. They plotted on a map where they wanted these boundaries to be, thus, classifications of these soils was predetermined. Then Franklin Newhall, using his model, calculated the moisture regimes of the major weather stations across the Great Plains. Unhappily, we made a serious mistake on the definition of the udic subgroup, which has got to be corrected one of these days. You start with a noncalcareous parent material, and any Mollisol will come out in the udic subgroup even though it is marginal to an Aridisol.

Question 55

Franzmeier:

Did the reasoning for the temperature regimes follow a similar pattern?

Guy Smith:

No, it did not. When we started to develop *Soil Taxonomy* and the first approximations came out, the correlators and the state people complained bitterly that this approximation was splitting series wholesale. They wanted to keep the series as nearly as possible as they had been conceived. They were willing to split a series if, when they examined the split, they saw that it would improve their interpretations, but otherwise they wanted to retain the series uniquely. The series had been used for sixty years or more and people had become familiar with them.

Highway engineers were using them; tax assessors were using them. When you saw an advertisement for a farm for sale in the Des Moines paper, it generally said, a hundred and sixty acres Carrington loam identifying the soils incorrectly, in most cases, and, sometimes rightly according to the public soil survey at least. The problem was then to split the temperature continuum without splitting series and for a time it was acceptable in correlation to the Director of Classification and Correlation to change series when you crossed a major land use boundary. From one major land use area to another you could have very similar series but you were making different interpretations. The limit across the Great Plains and across to the Atlantic coast - the limit between the corn belt and the cotton belt was fifteen degrees centigrade. So then we could choose that limit between thermic and mesic without splitting any series. There was the odd soil that overlapped slightly but this did not seem to make a problem. When we went on to the Aridisols the fifteen degrees centigrade boundary drew a line between what had been called Reddish Desert and Gray Desert soils so that was based on vegetation. The creosote bush was on the Reddish Desert and not on the Gray Desert.

The eight degree limit was picked because it seemed to run across North Dakota and to the Atlantic coast. On the Great Plains it was the limit, pretty much, between winter wheat and spring wheat. The crops changed at about 8 degrees, across Wisconsin and across Michigan. The Great Soil Groups changed. Michigan had Podzols below 8 degrees and Gray-Brown Podzolic soils above. That was approximately the boundary there and, in Wisconsin, it was a mixture of Podzols and Gray-Wooded soils or just Gray-Brown Podzolic soils. Minnesota was pretty much the same, Gray-Brown Podzolic and Gray-Wooded in the eastern part of Minnesota. In the western part the prairie soils did not change much at eight degrees. I'm not sure about the farming system there but that continuum had to be split somehow and so we picked limits that would split the fewest possible series.

Question 56

Turner:

Back to that udic, ustic line. Newhall's model of predicting from climatic data tends to indicate that, in the mesic temperature zone, at least, the moisture regime predicted from his equation for the Udic Ustolls is also a udic moisture regime. Some questions have been raised about methods of calculating PE. Has there been any question or plan to run those calculations using some other more restrictive model say, for instance, Eagleman? Would you care to comment on that at all?

Guy Smith:

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

temperate regions, so that committee is going to debate the problems in the moisture regimes and will come up in a few years with some recommendations. What they will be, at this moment, I do not know.

Turner:

As far as the secondary lime is concerned we have reports of secondary lime extending on into the Udoll areas so we have been trying to make the definitions exclusive of the secondary lime. Even in soils like Miami, in Illinois and Indiana, people have recorded secondary lime.

Guy Smith:

There is surely secondary lime in the Mollisols around Champaign/Urbana. But it's not soft powdery lime; it's hard lime concretions. They're excluded from the definitions.

Question 57

Peterson:

The *Soil Taxonomy* -- at least certain discussions of it -- made a very great point of *operational definition* in the use of soil properties and it seems that the soil moisture regime, and for some time the soil temperature regimes, were the places where this demand for operational definition was at least closely applied. I'm thinking particularly of the soil moisture regimes. We don't seem to be collecting very much soil base data and we keep coming back to climatic-calculation type approaches. I was wondering what your feeling is about the possibilities for picking up soil morphological or chemical criteria to help us with operational definition of the soil moisture regimes?

Guy Smith:

We looked for these in the various approximations. We looked at conductivity. The conductivity limit, unhappily, came into the distinction between Aridisols and Inceptisols. An irrigated Inceptisol can be converted into an Aridisol by the definition we have. That was a mistake. We could not make the conductivity work with the Mollisols. We could not make the accumulation of monovalent cations at depth work to distinguish Aridisols and Mollisols or to distinguish ustic from udic moisture regimes. Conductivity distinguished Udolls and Aridisols by and large, although there may still be exceptions. If someone can come up with something, perhaps a computer, someday when we get enough data stored, perhaps we can come up with relations that would suggest something that no one has thought of. We tried everything we could think of before we went directly to the moisture regime. We did want the line between the sown and the unsown soils to appear in our classification at some high categoric level. That is why we have used aridic moisture regime as a part of the definition of Aridisols. This was discussed at some length at Lubbock and very few actual studies are in progress that I know of. In Indiana and Ohio they are not concerned with it because they know it is udic to begin with. When you get out to the inter-mountain states, it becomes a serious problem when you have a rain shadow and you have melting snow accumulating here and there in the mountains and blowing away from other parts of the mountains. It is a serious problem and we discussed the use of vegetation as indicator of the moisture regime at some length, since these soils mostly have their natural vegetation. I suppose we spent at least an hour on it at Lubbock.

Question 58

Peterson:

I am particularly interested in that. What was your conclusion about using natural vegetation as an indicator of soil moisture regimes? May I ask a second, connected question? Were the temperature and moisture regimes defined to reflect vegetation changes? If so, many of these vegetation changes do not seem to be for the inter-mountain areas. Some do. When we display our vegetation against the present classes of temperature-moisture regimes, the class breaks seem to cut through natural vegetation where we don't want them to. Have you any suggestions as to how one might think ahead to handle recognizing vegetation?

Guy Smith:

No, I do not have. I recollect, for example, for the Cryoborolls of the inter-mountain region, that if I collected all the series descriptions of the soils in a given family, some were under forest, some were under grass. Forest types might be one thing or another, ponderosa pine or what-have-you. The vegetation and land use as described for the series varied appreciably from one series to another. I was not happy with what had been done but I got no proposals for anything from anybody. I thought the best we could do was to start a study of morphology of some of these cryic soils in the west, but I found I had nobody to do it and I retired. It is for those of you who have to work with these soils to come up with some suggestion.

Peterson:

You just answered another question I got from Ed Naphan: why didn't you set up other projects besides the Desert Project?

Guy Smith:

Well, I did. I started a study on the High Plains for the reason that when I collected all of the descriptions and the data on the Paleustolls, not one of them fitted the definition. I thought something must be wrong there. We should have had at least one sample of a pedon that fitted the definition of a Paleustoll, we had lots of series classified that way. It seemed logical to move from the desert to the margin of the desert on the High Plains because much of the information we got from the Desert Project was pertinent to the High Plains.

Question 59

Collins:

Some Histosols, especially in Florida, because they are being formed in Oxisols, are losing the criteria for a histic epipedon. Now they are having some problems. Genetically it's an organic soil. But you have to classify it as a mineral soil because it's lost that criteria. Could you comment on that?

Guy Smith:

Well, under cultivation the organic soil materials do oxidize and disappear. Not just in Florida, many of the soils mapped around 1912 and 1915 in Illinois were described as peat

whereas they are now mineral soils. It hasn't worried me that when the diagnostic horizon disappears the classification can change. Eventually even the very thick peats in Florida are going to disappear. They may last for some hundreds of years but not forever. It's only in the Histosols. Again the 8 degree C temperature works out pretty well. The European studies show that you can maintain a Histosol by careful management if the temperature is less than 8 degrees C, but that when the temperature goes much above that the Histosol is going to disappear no matter how carefully it is managed. Rouse, you probably know that literature better than I.

Farnham:

Yes, there are some rather complex things involved. For example, in the Everglades, talking about Florida, it depends on what you have underlying the peat as to whether or not this is going to become dehydrated to the extent that the subsidence is greatly increased. But there is a way around that, as you well know. The Everglades, the whole of Florida, was drained to the Atlantic ocean, natural drainage flowed south and southwest to the Gulf of Mexico. That really fouled the whole hydraulics. That was the first thing they did wrong in Florida. The Corp of Engineers built these canals, deep canals, to go down to the Atlantic. They cut off the water that flows down the Everglades and comes out in the Keys over on the Gulf side. That's one thing they never have been able to manage. The agronomists and soil scientists never could convince the farmers down there that they should have water level control. Everybody thinks about getting rid of water but nobody thinks about years when it is going to be dry. Even in Minnesota, as long as I have been working on organics, we've had two to three or four years out of the twenty-five that have been dry, where the water tables have been really deep. But subsidence can be controlled. The European experience, and the Polish experience is - if you keep it in grass and you keep your water levels monitored while they are fairly high - you have minimal subsidence. I'm not saying you won't have some, you'll have some oxidation anyway, but a lot of this is shrinkage. A lot of data from the Florida Everglades and from the San Joaquin in California is ridiculous if you really think about it. I mean, you've got to know what the original density was and what the density after, is. You can take something that is five pounds per cubic foot and compress it down to something that is twenty pounds. Is this oxidation? No. So all that is involved, it's very complicated. Now Israel is the best example, there is a peat bog in the Jordan River Valley-twenty to thirty feet of peat. It was overdrained, that's the first mistake the engineers made in a place that had six months of moist climate and six months of dry. In Israel they had trouble because they put two ditches there where they should have put one. They told us this themselves. They did this partly because of the border to Jordan. The International Water Commission had said that they had to share the water with the Jordanians, to my understanding. They had a little Tennessee Valley project. So they did this but Israel overdrained part of theirs. They are trying to rectify that by damming water. They are utilizing that bog for agriculture. They made that decision despite the fact that subsidence was going to take place. They said it was too valuable a piece of land not to use, so they are growing all kinds of crops there - on the peat. I've never seen melons growing beside cotton anywhere else in the world, and alfalfa in the next field. What a variety of crops. If you want to see a variety of crops on peatland go to Israel. They really have it.

Question 60

Fenton:

I have a question from our friends in civil engineering, especially. One of the most common questions we get concerns the family textural classification. Why was the 35 percent clay break, which separates fine from fine-silty and fine-loamy selected? Secondly, why is clay mineralogy not specified in the fine-loamy and fine-silty families?

Guy Smith:

We studied the relation between clay percentage and the Atterburg limits and the engineering classification. We had potentially a break at 35 percent which we used in some surveys and a break of 40 percent which was used in others. We compared these two percentages, primarily, and it seems that when we made our analyses of the data, either by all soils or by separate orders, that we got a little better relation with the 35 percent break than we did with the 40 percent clay. We were pretty much limited by the series definitions to these two alternatives. Obviously, the more clay you have, the more important the clay mineralogy becomes. When you get to five percent clay the clay mineralogy is not as important as is the mineralogy of the silt and sand as a general rule. There are places where we have used this with 5 percent clay, and we've used the clay mineralogy at the subgroup level rather than the family. If experience shows that in the fine loamy and fine silty fraction the clay mineralogy is as, or more, important than the silt and sand mineralogy we would probably be inclined to change the family definition for the U.S. as a whole. When we were doing this work the correlation with a kind of clay, at least, 1:1 and 2:1 lattice clays, was quite good. With the identification of a subgroup of Mollisols, you had 2:1 lattice clays. As a subgroup of Alfisols you had a subgroup of 1:1 lattice clays. The correlation was not perfect but it was pretty good. Now with the engineers concerned with the difference between illite and montmorillonite, they'll get that at the series level, they don't have to use the family level. We don't make all our interpretations at the family level; we make only the major interpretations.

Question 61

Peterson:

For the family particle-size class and the mineralogy control sections, depth criteria are used that give noncomparable depths for soils that occur contiguously in a landscape. I'm thinking of Argids up against some sort of Orthents, or Camborthids. In particular, the control sections that are keyed to the argillic horizon have variable depths to the bottom.

Guy Smith:

I thought that we were consistent throughout. If there is an argillic horizon we use its upper 50 centimeters. If there is no argillic horizon, we used about the closest equivalent depths we could where there are no real morphological benchmarks that you can tie to within the soil. For example the distinction between an ochric epipedon and a cambic horizon is not a very clear thing. We use an arbitrary 25 cm to one meter for the control section.

Peterson:

We do go as deep as one meter in some cases and not in others. I guess my question really is: why did you tie into the argillic horizon to give both upper control section boundary and lower in some cases, but use arbitrary depths in other situations?

Guy Smith:

I suppose this is due to the prejudices of some of the correlators. For example, in the Ultisols, specifically the Paleudults, which were the type Red-Yellow Podzolic soils at one time, the upper part of the argillic horizon normally has less clay than the middle or lower parts. Some correlators working with Ultisols wanted to tie the control section, at the Family level, to the upper part of the argillic horizon rather than to the lower part, which has very little rooting. In the Midwest, the upper part of the Alfisol argillic horizon is the part that has the most clay.

In the younger soils, the Alfisols, the Mollisols, if you find you have the maximum clay in the upper part of the argillic horizon, the lower part will show a considerable decrease in the percentage of clay. It is that maximum part, the maximum amount of clay, that controls permeability and other things in the younger soils. For both the old soils and young soils, there were reasons why the correlators preferred to use that upper 50 centimeters of the argillic horizon because no two pedologists could agree on where the argillic horizon stops. It had to be an arbitrary thickness in the upper part of the argillic horizon. It is operationally possible, as we pointed out in Taxonomy, to agree on the upper limits of the argillic horizon.

Peterson:

I would question that. At least in the field, I'm not user people can agree on where the top of the argillic horizon is in many soils.

Guy Smith:

Well, working in the field that's quite possible. The method that we pointed out requires laboratory analyses, requires drawing a smooth curve for the percentage of clay and the point at which the ratio reaches 1.2 times the clay content of the epipedon is the top of your argillic horizon. This is the method we proposed; it does require laboratory analyses but it is possible to do it.

Question 62

Franzmeier:

The families are designed for practical purposes, such as plant growth and for engineering properties. It seems that, for plant growth, for example, the plant essentially sees a certain depth of soil, to some extent, regardless of the genesis of the horizons. Was some thought given to define the families on a uniform depth across the board rather than changing with the soil orders, which reflect changes in soil genesis? This would also apply to engineers. I think we could explain the concept better to engineers if we said the family represents the same depth for all soils.

Guy Smith:

If you have the time or somebody has the time to make an analysis of the alternative systems, you can make a better decision on which is the thing to do. At the time we worked on this there was really not much opportunity. In fact it was very difficult to get the correlation staff and the state people to check the families versus the capability classification. They were supposed to have done that several times but in fact I think they took a bunch of data and looked at them and, 'yeah, that's O.K.' without actually going into details. They were very pressed for time because they were very busy making maps and correlating the completed surveys. There is more time now to go back and reexamine what was done and whether it was done properly.

Question 63

Reiger:

Were you aware of the proposed changes in the chemical criteria for recognition of a spodic horizon?

Guy Smith:

No, I am not. We did the best we could at the time. There were difficulties that we were quite aware of, mainly that many of best Spodosols have a spodic horizon that does not meet the chemical requirements and so we put in the field identification of the spodic horizon to permit their identification. The best developed of the Spodosols, generally miss the chemical requirements. The chemical requirements were actually based on a study of the intergradation between Spodosols and Dystrichrepts. They represent the properties of the spodic horizon as it is just beginning to form.

Holzhey:

I might comment that there is a field kit being used in four states right now with a different kind of extractant which is designed to measure the aluminum in the spodic horizon. The first results have come in from New England and the people there are quite pleased with it. It will be a while before we get all the results in. Where you have sufficient aluminum to measure with this technique, I imagine it should work quite well. The question is, in some of those Aquods with the low aluminum contents. Then there is a question if it will work. That's not a change in criteria, that's simply a change in the tools available to be standardized to the local condition and standardized to the lab technician.

Guy Smith:

The Europeans commonly use fluoride and phenolphthalein to identify the spodic horizon whereas we developed from a cambic horizon.

Question 64

Collins:

The Department of Energy is interested in mining peat deposits in America. They want to know the quality and the quantity of these deposits. The Europeans, I know have been mining the peat and burning it for fuel. The Department of Energy is interested in their experience. We would like to try it, do you think it is feasible?

Guy Smith:

Certainly it has been an important source of energy in the past and it still is, particularly in Ireland, where many of the electric generating plants burn peat. This is the major country where I have seen important harvesting of *Sphagnum* peat. You are getting rid of something that is agriculturally worthless. When you get the *Sphagnum* off you'll have productive farmland remaining. There may be, in some of the Communist countries, mining of *Sphagnum* for energy. I do see, traveling by rail, very commonly *Sphagnum* is being harvested for heating homes and cooking but this is on a small scale.

Farnham:

Presently the Irish and the Finnish and the Swedish, more recently, are people working on using peat for energy. They are very careful to preserve the *Sphagnum* for the simple reason that *Sphagnum* has a world-wide market to the horticultural people. Germany made the mistake of getting rid of theirs. The last vestiges of German production from *Sphagnum* is now carried out by the Dutch Activated Carbon Company. They have tied up all the big bogs in West Germany for that production, so Germany no longer produces for export *Sphagnum* peat. England gets peat from Ireland because England has long since used most of their *Sphagnum*. The countries of Ireland, Finland and Sweden are saying now that horticultural peat should be flagged and not used for energy. The more decomposed peat is used for energy, or, in the case of Ireland, they prefer to take the *Sphagnum* off first and get down to the more decomposed peat which is a better fuel peat in the first place. Ireland is actually expanding peat production. You'd think they would just about run out but they have not.

Question 65

Peterson:

The feature of *abrupt textural changes* is given prominence in *Soil Taxonomy* by being discussed in Chapter 3. Yet there are three more very similar abrupt textural boundaries used in other places, plus the fact that many natric horizons have an upper abrupt textural boundary. I was wondering why this was done? Why the other three abrupt changes in texture from A to B more-or-less got hidden back in the intricacies of the Taxonomy, whereas the abrupt textural change that is used with certain groups came out in Chapter 3?

Guy Smith:

I'm not sure I can identify the other abrupt changes to which you refer.

Peterson:

Well, I was so interested in it, because of our local situations where it controls vegetation, that I worked up a handout for class use. The first type, the *abrupt textural change* of Chapter 3, is used in the Albaqualfs, the Argialbolls, the Argiaquolls, and the Albaquults. The second type, which I called an *abrupt textural boundary* is in the boreal soils, the Argialbolls, Cryoborolls and Paleborolls. The third type, which I also call an *abrupt textural boundary*, is used in the Durixerolls. Then there is another *abrupt textural boundary* used in some ustic, xeric, and aridic soils, in Durargids, Paleargids, Palexerolls, Paleustolls, Palexeralfs, Durixeralfs, and Paleustalfs. That makes a total of four kinds of abrupt changes of clay content from A to B that are used somewhere in the Taxonomy. Then we also have all the soils with natric horizons that have columnar structure; they have the very prominent abrupt textural boundary.

Guy Smith:

The emphasis was placed in the Udolls, the Aquolls, the Udalfs, the Aqualfs, because in these soils this abrupt textural change results in a perched water table when the soil re-moistens in the fall and winter. This produces a serious problem for the plant and for the highway designer. Soils of drier climates may have this abrupt textural change but don't have the perched water table above the argillic horizon. Above the natric horizon you normally expect some perched water. You have the evidence of the perched water in the presence of an albic horizon above the argillic or the natric horizon. As the soil gets drier you have a similar interpretation that this abrupt textural change indicates considerably greater age than a soil

without such a feature. And yet it does not have the same significance for the use of the soil. An abrupt textural boundary was also used to define some of the pale-great groups in the drier soils, but since these do not have the albic horizon above the argillic horizon we did not use the abrupt textural change at quite the same categoric level as we did, say in the Albolls.

Peterson:

I want to comment that these drier soils do perch water in the winter but it's for a relatively short time. That's about the only place I can find a horizon to illustrate an 'A2' horizon, in other words, there is a graying over this abrupt textural boundary, a very prominent gray. It does have, in many cases, a major effect on the natural vegetation that occurs if it is shallow. If it is deeper than about 6 inches, then it doesn't make much difference for natural vegetation.

Guy Smith:

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

Question 66

Hall:

When I was in Hawaii I had a chance to see some Histosols. I felt they were very contrasting to many of our midwestern Histosols and felt that they weren't enough alike to be grouped together. I know Dr. Cline did a lot of work in Hawaii. What is the background on those Histosols? In some cases graded-up volcanic cinders are mapped as Histosols. They had organic matter on the surface at one time, but looking at them now you really can't tell them from a volcanic flow material.

Guy Smith:

We have in Hawaii these two kinds, two suborders of Histosols. They probably are fibrous on the island of Hawaii, rain-fed Histosols. Very few examinations have ever been made of them because they have up to six hundred inches of rain a year. The other kind is extensive on the island of Hawaii where you have lava. It's a Folist. There is nothing between the chunks of lava except organic materials. The concept of the Folists really comes from those soils in Hawaii. If you don't have a place for them you'll have to call them 'not soil' but they support a fairly good forest. Under the forest I suppose there is a thin O horizon which decomposes in that warm climate fairly rapidly but between the chunks of lava there are just organic materials. These are obviously very different from what you have in midwestern states here. I had a lot of trouble with Farnham. The committee on organic soils was trying to develop a classification of the Histosols. He wasn't interested in these soils. He was interested in the thick organic materials that are so typical of Minnesota but I finally got that through for

a classification because I've seen enough of them in the world. The O horizon rests on hard rock and yet supports quite a good forest.

Question 67

Hall:

I'd like to understand more of the background for the reason you put all the wet soils together? Some classifications would separate those out at the order level. I assume that option was looked at along the way. Could you give us a little more background on that?

Guy Smith:

We didn't put them all together, we divided them up and we put them at the suborder level not at the order level. Most other taxonomies have an all wet soils group. Background on that started when I first mapped soils in Illinois in the northeastern part of the state where all the soils are Udolls and Aquolls. My first year's experience was restricted to those two kinds of soil and they were entirely different to me. I then took the soil maps of the various experiment stations and located the plots that were all Udolls and the plots that were all Aquolls and compared the yields on the two sets of plots. They were identical, which shook me badly. I puzzled over that until finally I realized that on these plots the Aquolls had been drained so that when you drained the Aquolls, the Mollisol properties became important as well as the udic properties that we get from the summers in Illinois. Then if I compared say, the Red-yellow Podzolic soils with the Low Humic Gleys soils in the southeast I compared what would happen. If you drained the Low Humic Gley soils, you would have a soil with the same properties as the Red-Yellow Podzolic soils. There was a zonality to the soils with aquic moisture regimes and this would be best reflected if the aquic soils with aquic moisture regimes were separated below the order level. I argued in some of the conferences that the separation should be made at the Great Group level so that we would have an aquic correlative of the Xeroll and an aquic correlative of the Ustoll and one of the Udoll. It was too big a leap for the people of that time to do that. I could get no support whatever for that treatment of soils with aquic moisture regimes. We are coming around now to somewhat the same thing in that the committee on inter-tropical soil moisture and temperature regimes is considering making subgroups for the soils with aquic moisture regimes, such as the Aquoll in Venezuela where you have six months of heavy rain and six months with no rain. These soils do not behave as do the Aquolls in Illinois because they require drainage at one season and irrigation at another. The same thing holds for the Aquolls in the Willamette Valley in Oregon. They can not grow corn or soybeans on Aquolls without irrigation because it is a pronounced wet-dry climate. They must drain in winter and then irrigate in the summer or else about the only crop they can grow is grass for seed. That's what you see lots of there. These soils have wonderful chemical and physical properties. They lack only the evenness of the moisture distribution that we get in Illinois and Iowa where they are the most productive soils. We are coming around to it but instead of making the subdivision at the great group level, I think probably we will wind up and make it at the subgroup level.

Question 68

Hall:

What has been the foreign response to this? Do most of the foreigners, Europeans particularly, and New Zealanders agree with this philosophy now or is there still some resistance?

Guy Smith:

The Europeans are mostly stuck with their former prejudices about it. They want one order for all the wet soils.

Peterson:

You say that the international group wants to put the "aquic" division in at the subgroup level?

Guy Smith:

They put in an ustic subgroup of an Aquoll.

Peterson:

An ustic subgroup of the Aquoll?

Guy Smith:

That's what's being discussed.

Peterson:

Would you consider a similar approach for a xeric group.

Guy Smith:

Yes, ustic, xeric, and udic subgroups of all of the aquic great groups except that one of those will be set as typic, probably the udic will become the typic. Then they will have ustic, xeric subgroups. The chairman of that committee is Professor Van Wambeke of Cornell. If you have an interest in it he would be happy to have you on his committee, I am sure.

Peterson:

Did they consider an aridic subgroup also?

Guy Smith:

I don't think that has been mentioned yet. There is no aquic suborder of Aridisols.

Peterson:

Well, I was thinking of an aridic Aquoll. In other words, we map Aquolls down from the mountains into the desert along the stream floodplains quite a way and that might be an interesting question, as to what those things are called.

Guy Smith:

One of the big problems is the manner of definition.

Question 69

Fenton:

In the midwest, we don't have any pale-great groups for soils like the Yarmouth Paleosol. Paleosols are important stratigraphic markers. The criteria as presently written or, as originally written, do not recognize that type of development as pale, at least, in my experience here in the midwest. I was wondering if the bias is towards the red color and what other criteria were considered?

Guy Smith:

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

Question 70

Franzmeier:

This question probably belongs in an earlier session; it might be more philosophical than answerable. Do you think we are getting to the point, I think Mark Twain was concerned with it, that we have more names than things. The more detail with which we look at a soil, the more reason we have for taking it out of some class whether it is a family or a series. This may relate to where taxadjuncts are very involved in the classification system. It appears that if we have a short profile description we can classify the soil quite well without any hesitation into some class. If we have a more detailed one there will be more reason for removing it from that class. When we start adding more and more laboratory data, there is more and more reason that it doesn't fit into that class. Then, when we look at a natural soil landscape unit, something that seems to fit together logically on the landscape, we find that it falls in several classes, whether those be family or series or whatever; sometimes it might cross boundaries at the order level. Do you think we might be to the point where taxonomic units are dividing things up more finely than nature has made them?

Guy Smith:

It is quite likely that nature in building a landscape unit didn't pay much attention to our definition. I remember Professor R.S. Smith at Illinois University. He always said, "If I had the world to make over I could do it a lot better". Because some of the soils were 'stubborn' and didn't fit anywhere into any series that we had in Illinois or that we could map. Sometimes there were real complexes that surely crossed family and subgroup definitions in the glacial till. In the loss it was much simpler to make a map that would contain relatively homogeneous properties throughout the delineation. The present family is somewhat at the level of generalization, that the soil series was in much of the U.S., say in the late '20's and early '30's. The number of soil series is now approaching 14,000 or 15,000. At that time the number was

much lower, somewhere above the number of series of Marbut's Atlas. It may have been slightly fewer than we have families today but the same order of magnitude. There is nothing we can do to unravel some of nature's complexities except to map associations and complexes which we use depending on the scale we are mapping. The taxadjuncts, I think, have been over-used because of people's failure to recognize the limits of significance of their observation and of the laboratory. There is an appreciable sampling error involved in the collection of samples. The soil survey laboratory has always insisted on matching samples, that is, sample 2 pedons in 2 different polypedons and you map them as closely as possible. So the differences between these two matching pedons can be used now with a little statistical analysis to determine the order magnitude of the sampling error. So far as I know this really has not been done yet. It is an appreciable error. The laboratory technician who runs duplicate samples from time to time to check on himself has some notion of the magnitude of laboratory error. Particle-size analysis is a measurement that is made in the laboratory. But in the classification of the soil itself, you have these two sources of error, and I think that when you call a soil a taxadjunct because it has 5 percent too much silt, that you are ignoring the reliability of the laboratory measurements, which in turn are subject to the sampling error. I think it has been over-used, if the soil fits a given family except for 5 percent too much silt, then I don't think we should bother our users with calling it a taxadjunct. I think we should use a series name, but I am not responsible for correlation and for the nomenclature that's used in the soil surveys. The user of a soil survey is not concerned with whether that is a taxadjunct of Clarion, he is concerned with what we have to say about that soil in terms of its responses in use and management and he is not misled if we use the wrong name provided we make the proper interpretations for it. He couldn't care less if it is Clarion or Clarion taxadjunct as long as our interpretations are the same. That has nothing to do with taxonomy. This is how we use it, it's the application to soil surveys.

Franzmeier:

He might be concerned whether it is Clarion or Clarion taxadjunct, if he is also using another survey where that soil has been named Clarion or another name. That's where the name comes in, where it confuses the user if he sees the same soil named differently in different surveys.

Holzhey:

I was going to comment that we have gotten a start on looking at the variability among the polypedons and a paper by Mausbach and others was published in the SSSA Journal just in the last year or two. The paper talks about clay content, cation exchange capacity, variability between polypedons and it is appreciable. But it does point out that we can characterize the 'center'. We can identify and sample the 'center' of our concepts with sufficient accuracy that we are within our class limits, that is, our class limits are probably not too narrow so that we can hold our sampling and analytical error to less than the distance between the class limits.

Guy Smith:

As I look at the SSIR from the Riverside Laboratory for California soils, there are a number of taxadjuncts in there.

Peterson:

When the pedons are classified rigorously then, I think, in that SSIR, in general about half of them do come out a few percentage points outside some limit. Of course there are so many properties, that it is still probably 95 percent accurate and in the one property it may miss by only a few percent but those are all indicated as taxadjuncts.

Guy Smith:

I would have preferred to list them under the name of a series under which they were sampled if they were close and put in a footnote without calling it a taxadjunct. This is within the range of sampling.

Question 71

Peterson:

Along these same lines as your question, you correctly say that there is a very considerable -- I don't know if you would call it an error or variability -- involved in sampling polypedons. We could look at our data, especially our laboratory data, but also our profile descriptions that are collected during a survey, as having + or - values because of variability. I have wondered several times if one should also look at the class limits in the Taxonomy as having + - values. I think we already do it in as much as when mapping we will include similar soils without fussing about it very much. Naphan calls this a *mapping decision*, which is an euphemism for ignoring slight taxonomic variations. In terms of procedure, would you want people, or do you think we would be safe to apply + - to those class limits when applying Taxonomy? Taxadjuncts come up for that reason; they are a little bit over or a little bit under a certain limit.

Guy Smith:

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

Fenton:

That use, the use of a word to mean things, is an integral part of language. I don't think there is anything wrong with it. I think it is the way the language is built. Aren't you saying in effect that, yes, those class limits during mapping are allowed to stretch?

Guy Smith:

During mapping, yes, but I wouldn't want to allow a variable limit to the conceptual unit that is Miami silt loam in the Taxonomy.

Question 72

Fenton:

I would like to follow up on a question I asked this morning. A point of clarification. In discussing eroded Mollisols, you said that, if you could satisfy yourself that within that polypedon there was enough surface horizon to qualify in terms of properties for a mollic epipedon, then you would still consider that soil a Mollisol. I don't think you said exactly if that would be one point within that body that would satisfy you or if it would have to be a certain percentage of that body?

Guy Smith:

I would still like to see some data on those to make up my mind about that point. I doubt that one point, one pedon would satisfy me but I have a feeling you will find a great many if you take a look. This came up at Lubbock relative to some soils in Central America. They did have some numbers and it was something like 60 percent where the mollic epipedon remains and 40 percent where it was gone. In the case of your eroded Mollisols in Iowa, certainly if you have something like (60 percent) you should classify it as a Mollisol and allow these eroded areas to remain because their behavior is not greatly different from that of the uneroded Mollisols. It's very, very similar and it's a matter of a difference of a few centimeters, maybe 8, between the soil that is properly a Mollisol in Taxonomy and one that is not. It may be only 5 cm.

Question 73

Fenton:

In all of the soils that we have data on that we call Mollisols, the organic carbon matter is always greater than 1% down to the 10-inch depth but it may not meet the color requirements for a mollic epipedon. When you get to that point, would you state which property we are most interested in or can't we separate those? Does it have to be both of these properties or could it be only one?

Guy Smith:

I suspect that you'll find very little difference in the percentage of carbon in the eroded one and the uneroded one. One percent is an extraordinarily low limit for a Mollisol and we simply lack the data to develop a sliding scale for a relation between carbon and clay and silt in the mollic epipedons. The one percent limit was established for some soils from the western part of the Great Plains that were fine sands. In cultivation they get winnowed and a good bit of the clay and carbon are blown away but the color remains that of the Mollisol, the uneroded member of the series. The correlators on the Great Plains wanted to keep the series together and one percent was about the lowest level that we could get for the winnowed sand.

Whiteside:

I just want to follow up, talking of polypedons. You're saying if the contiguous portions of series on landscape have these recognizable characteristics, the polypedon, you are talking about a natural entity, not simply delineations on the map, right? In other words the eroded places might be separated as separate segments of that polypedon.

Guy Smith:

They often are, yes. According to the degree of erosion.

Peterson:

But you would consider this acceptable philosophically.

Guy Smith:

Philosophically, I would accept it although I would reword some paragraphs in *Soil Taxonomy*.

Question 74

Peterson:

Another clarification of Gene's question: When you talk about accepting eroded spots, that is, sampling points or pedons within a polypedon that are in the minority, that do not meet all the qualifications for the class that you would want to put the entire polypedon in, are you considering this again as another "mapping decision", or are you thinking in terms of something philosophical within the concepts of the Taxonomy? Because the definition of the pedon and the polypedon takes these eroded spots out, doesn't it? Won't they become a different soil?

Guy Smith:

The become conceptually a different soil if, as in the case of the Hapludolls in Iowa, the dark colored plow layer is too thin to qualify as a mollic epipedon. We simply do not have another subsurface diagnostic horizon at the order level to use to keep them as Mollisols. If I am going to Ohio with the Miami I can lose all of the A in the plow layer and B entirely in the argillic horizon, but I still have the diagnostic horizon that keeps it in the Alfisols and by inference in the Miami series.

Question 75

Peterson:

It seems to me that there is still some sort of a problem and I think it is bothering you too, Gene, at least at the philosophical level. It has to do with the definition of the polypedon as being within the limits that are established by the limits of the soil series. This is somehow circular, it is a nonoperational definition and perhaps it was written purposely so.

Guy Smith:

I told you if I did this, I would want to modify some paragraphs in *Soil Taxonomy*. I won't tell you which ones or how until I have some factual data that I can inspect.

Question 76

Rust:

I believe, Guy, you have been asked the question somewhere, you have commented on whether or not a taxonomic name should be the same as the mapping unit name. I take it from your answer now that you would say we should continue to use the same name.

Guy Smith:

I am not disturbed by it, some others are. It is very common, we use the same word with two meanings and infer the meaning from the context of the usage.

Whiteside:

We have allowed the name to carry too much inference that the actual bodies do agree with the name. Once the facts are actually verified by tests, I could live with it too, but it seems to me it would be well worth considering divorcing the names from the map unit names, but this also becomes confusing.

Guy Smith:

I think it has greater confusions but I am not responsible for this anymore and if they want to do that I can't stop them.

Peterson:

Gene, you are just suffering from the eastern bias. They have never distinguished between the concept and the map unit. A lot of Easterners never realized you could map at other than the series level.

Question 77

Peterson:

I would like to continue the same general discussion on naming with a question I am a bit embarrassed to ask because the answer is very obvious -- or at least there is not much you can do about it. But, the *Soil Taxonomy's* connotative nomenclature -- which is rather special to *Soil Taxonomy*, even including the biological taxonomies because few of us understand those Latin elements -- is one of its strongest features, especially for teaching it or using it in working with other people. At the same time, the connotative names are potentially very misleading because I find both students and myself not paying attention to properties that don't appear in the names, which is a very serious error for many soils. For example, many of the soils that have abrupt textural boundaries do not appear as an abruptic subgroup. Was there any particular policy in choosing those properties that would appear in the connotative name, or did the formative elements come out almost by accident in terms of the demands to separate this soil from that soil? As soon as they were distinguished by name was that the end?

Guy Smith:

The things that come out in the name are the properties that the people within the state or region where the soil occurred thought were important and they're spelled out in the definition of the typic subgroup for that great group and it was decided by the correlators and the state people that for some kinds of soil -- commonly in pale-great groups -- the one that has a very thick clay argillic horizon would be considered typic with gradual boundaries. Where there is an abrupt upper boundary to the argillic horizon it was no longer considered typic. These people could have said that they are both typic and then there would have been no distinction between the abruptic and the typic subgroup but they felt that this was important to something and it is impossible for me to know all that went on at all these work planning conferences and why they decided that one kind of assemblage of properties was typical of the great groups and another was not. I do not know the answers to these questions.

Peterson:

One thing that has bothered me is that we have Paleargids and then we have Petrocalcic Paleargids, which are quite different to my mind, and then we have the Durargids which aren't called Paleargids and yet they certainly are Paleargids. The 'pale' name is only used for some of the very old soils. That to me, is rather misleading for the novice reader; they probably would think of the 'pale' soils as the old and the others -- by the lack of "pale" -- as not old. That would be one illustration.

Guy Smith:

By and large we followed the principle throughout taxonomy of identifying the presence of a pan and the kind of pan at the great group level. This gives us all of our "duri" great groups and our "fragic" great groups. We didn't have a set of great groups under a common name. I have had several questions about why the petrocalcic horizon wasn't treated as a pan and I don't have a good answer for that, except that it was traditional that the petrocalcic horizon is just hard caliche and is no different from the soft caliche. This was the traditional thinking. I had no experience with the soils. It's quite likely that petrocalcic horizons could have been considered to be another kind of pan, just like the duripan, the fragipan, and the densipan.

Question 78

Collins:

Some of my students who study *Soil Taxonomy* know the implications of the names such as fine-silty, mixed, mesic Typic Hapludoll. But at the series level you lose all the information from the higher categories unless you know the series. They ask - why the series is set apart in the classification of the soil? I really don't know why, other than that's the way it had been in the past. Any reason for it?

Guy Smith:

The series were inviolable. They could not be changed. The pressures on me were enormous not to disturb the soil series because people knew these. They knew the local names of the series, the soils they were concerned with, they didn't want the names changed.

Collins:

Some of my students can get something out of family names. But when you talk about a series they are not familiar with, they have no idea what the soil is. It is easier to ask "what is the classification of the soil" because then they have some concept what the soil is. The other question I would like to ask is about laboratory data. Is there any specific laboratory analyses that you think should be included in Taxonomy? If so, how much data are you talking about, how many pedons do you need for something to be established?

Guy Smith:

You don't need very many data to establish the importance of some particular property but you need quite a bit of data to decide whether or not you can use that in Taxonomy and, if you can, at what categorical level you can use it. You must know how the introduction of another property will affect the Taxonomy. For example, they are talking about the point of zero charge as a soil property that should be considered in Taxonomy. At the moment the measurement of the point of zero charge is enormously time-consuming, and so expensive that it has rarely been done. It may be decided that that is more important than any other property and should be introduced into Taxonomy. Let's make the assumption that they find a method to do it that is practical. Then they decide to use it. You must have data on a great many soils in order to know how the introduction of that into your definitions is going to affect the classification. The whole principle of classification was expressed very clearly by John Stuart Mill when he said that the best classification is the one which permits you to make the greatest number of the most important statements about the objects that are grouped in the classification. That is our final test. If the only thing we can say about classes of the point of zero charge (you'll have to have classes of this), is whether the point of zero charge is 3.5, 4.5, 5.5 and so on - if that is all you can say about the soil, that the point of zero charge is between 4.5 and 5.5, it's not important. You have to examine what soils have been grouped by the classes of point of zero charge and then examine the statements that you can make about those soils in contrast to others. So it takes quite a few data to decide how to use one of these properties.

Collins:

How can we incorporate new data and techniques into *Soil Taxonomy*?

Guy Smith:

If it is something that we haven't used then it requires a proposal to be used.

Collins:

Back to Dr. Fenton's question. There were a lot of eroded Mollisols, and there's a lot of data in Iowa about eroded Mollisols that I think might be included.

Guy Smith:

I tried when we were developing *Soil Taxonomy* to get some data on that. I tried to get a project set up to collect the data, I didn't have any. You say it is available but it wasn't available anywhere I could find.

Question 79

Fenton:

I have a letter from Joe Fehrenbacher. He has a question which I am interested in also. In this letter he reminds us that in 1939 F. Riecken and Guy Smith and he spent the summer in Cass County in Illinois. That survey is starting over again in 1981 so I thought maybe Dr. Smith would like to know that. Anyway, the question of interest would be your ideas or preference on the use of soil series vs. soil phases. You know in Illinois they have used a lot of soil series that, I think, in other states might have been put in phase categories. That is one of the questions that Joe had some interest in.

Guy Smith:

I think what Dr. Fehrenbacher had in mind is the division of the soil continuum into series on the basis of slope. That is one parameter, whereas other states commonly use slope as a phase. Now, Dr. Whiteside, you can verify for me, there are consistently associated soil differences with slope. This may be important enough to recognize as another series in another state. You may have exactly the same differences but he may feel that these are better shown as slope phases than as series. Of course, I have a bias in this respect. I feel that the differences in drainage, for example, in Illinois associated with slopes are quite significant and are well worth serious differences. The differences in soil drainage associated with slope in another landscape, say, in central Nebraska maybe quite unlike differences that you find in Illinois. There the slope is important because, perhaps, of erosion problems but not because of other problems of soil use. The correlators have occupied themselves since the start of the soil survey with series. And yet there has never been a really good discussion of what should be a soil series and what should be a phase. Ever since WWII every European who came to the U.S. came with one burning question, when do you establish a phase and when do you establish a series? I don't think any of them ever got an answer to that. I can give you my philosophy but I can't say that correlators are going to agree with me. They are the ones that are doing it. Take as an example our soil temperature classes in Venezuela. The coffee is grown in the warmer part of the isothermic soils. In the cooler part of the isohyperthermic soils, they straddle that isothermic temperature. You get to the cooler part of the isothermic and the coffee grows well but doesn't bear fruit. What can one do about this? You can change the limit from 22 to 18 C degrees for the isothermic. That would fit with coffee but then another crop comes along and it doesn't pay any attention to that limit. It has its own preferences for temperature. The Venezuelans have been talking and will discuss this at some length, I suspect, about dividing some of the isothermic classes and changing the limits of some. This has been mentioned somewhere earlier this week. If you set up series on the basis of temperature alone, then somebody over in plant breeding is going to come along with a new variety. You tied your Taxonomy to an old variety and it is out of date and it is very difficult to correct, but if you do this at the phase then the plant breeders can do all they please and you can still adapt your Taxonomy to the new varieties. So it would be possible. We used to discuss whether or not Clarion should run from Ames to St. Paul because the yield is different and we did finally decide that this should not be a series difference but a phase difference. The Canadians made a wheat suitability map for the prairie provinces. When they got finished and, before they could publish it, the plant breeder had new varieties of wheat that made their map useless and they never printed it. This sort of thing suggests that one should not tie his Taxonomy to a particular variety of a particular crop. It will get you into trouble. Other decisions are not so obviously clear. It is time for the correlators to put up some guidelines as to what they are doing in the first place. We don't know that. Discussion of what should be done is in the handbook or the manual that was supposed to be in preparation all through the time Taxonomy was in preparation but never appeared. Reminds me of another example of what should be a phase, or should be a series. You will have to use your own judgement on this one. The sugar cane in the inter tropical regions is ideally adapted to the fine-textured soils, to either Vertisols or vertic-subgroups of various kinds of soil. I looked at a soil at Maracaibo which was a Vertic Haplaquept and, after examining it, I said this should be ideal for sugar cane. From a pedologic point of view it was, but they told me immediately, they said no, you can not grow

sugar cane here because there is not enough difference in the air temperature between night and day. Sugar cane, to produce sugar, requires a cool night relative to the day temperature. Now this is a climatic factor and so far as I know is not reflected in the soil itself. What should that be, a phase or a series? How about our correlator?

Stout:

We have the same kind of a deal around the Great Lakes. We get into the same thing, Guy. In most cases we tried to attack it, not by series but mostly by climatic phases and we say that this is a climatic phase in which the series occurs and in this area we have this kind of crops that are common to this area. I would hate to go the other route and change the series. I would have to know where I was.

Question 80

Franzmeier:

If there is a temperature difference it would be reflected in the soil only, just in the surface cm or so of the soil. What would be the difference between that situation and between hyperthermic and isohyperthermic, the seasonal difference? The only difference, as I see it, would be the depth at which you are looking. One, you would be looking at a depth where you would be looking for seasonal variation and, the other, you would be recording temperature and would be looking for diurnal differences but they both would be reflected in soil properties.

Guy Smith:

They would both be reflected in the actual soil temperature but in the Maracaibo Haplaquept the differences would not extend to the depth that the differences do in the hyperthermic soils of Florida. However, I have proposed that, so far as Taxonomy goes, they should use the air temperature with the isothermatures. There is an agronomic difference in that the hyperthermic ones may, at times, have air frost. This is not a soil property but it is related to the property of seasonal temperature fluctuations. If they are combined one will have to use air frost phases.

Whiteside:

I think there was a similar case related to sugar cane production in Hawaii where they seemed to be controlled by elevation but at higher elevations they couldn't grow sugar cane because of extreme lack of sunshine in that case.

Guy Smith:

Yes, they are in a cloud bank. But there they have other series.

Whiteside:

Do they? I wasn't clear on that.

Question 81

Fenton:

I have another question concerning your philosophy on the classification of disturbed lands and perhaps it was discussed before. In terms of man-modified soil materials, and the definition of soil in Taxonomy should disturbed areas be placed in existing categories or should there be another category?

Guy Smith:

They have proposed a suborder of Spolents. This was discussed at some length in Washington and at Lubbock. I had to plead ignorance and disclaim any knowledge that was adequate to have a valid opinion.

Question 82

Peterson:

Going back to the moisture regimes, was there ever consideration given to splitting the Typic Aridisols into those that have the "ustic" moisture distribution vs. the "xeric" distribution? We have the ustollic and xerollic subgroups, but within the typical Aridisols we still find vegetation differences depending upon seasonality of precipitation and temperature. Was there ever any consideration given to further splits?

Guy Smith:

Not to my recollection. We wanted to get those soils whose lack of moisture was extreme into the Typic Aridisols. These would run into inter tropical regions where the season in which it rains is immaterial. You can't define season in terms of summer and winter in Venezuela, but they do have two short rainy seasons there. There's been discussion of subdivisions of moisture regimes on the basis of one or two rain seasons. In Aridisols these are not severe rainy seasons, you understand, but the soils that have two rainy seasons can occur under very low or very high rainfall and in the latter the two rainy seasons are important. Such soils are much to be preferred to soils with only one rainy season because you have a relatively dry season during which you can harvest one crop and plant the second. In Venezuela, with only one rainy season, they are only able, at the moment, to grow one crop per year although the growing season is long enough for two crops. The maturing of the first crop comes at the height of the rainy season when they can't harvest it. They cannot plant the second crop except with hand labor. This is one of the things the committee on moisture and temperature regimes will undoubtedly discuss. Whether they will get out into the Aridisols with this discussion, I don't know.

Peterson:

It might not seem so important to them.

Guy Smith:

I'm sure it won't to them because they are oriented to the more humid tropics, ustic and udic.

Question 83

Hall:

In Taxonomy, we have the perudic moisture regime. It seems to have been introduced and then not really used very much. Was there thinking at one time that the perudic would become an important moisture consideration?

Guy Smith:

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

Question 84

Hall:

Talking about argillic horizons, in some of your discussions you said you came up with a 1.2 ratio because that was what you could identify in the field in Iowa. It was one of the criteria at least.

Guy Smith:

In the Mollisols.

Hall:

And you also indicate that some of the other countries have different ratios that they would like to use. In light of the kind of deposition and the kinds of or lack of uniformity of parent material, how useful do you think that 1.2 is in a lot of areas? I see that value quoted many times and yet there is always a lingering doubt about what has been deposited on that landscape or what has been eroded away. Both deposition and erosion raise some questions as to the value of the ratio.

Guy Smith:

This was discussed at length in Lubbock. We, of course, can not use the ratio or the difference in clay percentages in soils that have been eroded and in which the plow layer's base is in the argillic horizon, there is no possibility of using any ratio there. Where there is a distinct difference in the parent materials as on some of the late Pleistocene and early Holocene

terraces in the middle west, one can find a lacustrine clay that is capped by a silty alluvium or colluvium and one can get very similar clay distribution in those soils as in soils with argillic horizons. Along the Mississippi floodplain when the levee bursts, you get sand on a clay. There is an enormous change in the clay percentage but it doesn't bother anybody. Nobody I have ever met has wanted to say that that was an argillic horizon although it does have some of properties in that it does perch water in the sand on top of clay. The usefulness then would be restricted to soils in which there has been an appreciable clay movement or an observable clay movement as indicated by clay skins in the subsurface horizon. How far does that have to go in an untruncated soil before we want to say this is an argillic horizon? It is generally, I think, more useful on soils in loess than it is on soils in glacial till. The French use the ratio of 1.4 but they're concerned with soil such as you have in Ohio, the Hapludalfs, and the Paleudalfs and so on. The 1.4 would work well there but it doesn't work in the Mollisols.

Hall:

In Ohio some of us have had a feeling that the glacial till was there and then there has been a mixture of maybe six inches of loess that has been incorporated. That brings up the problem of uniformity of the parent material and you run into that 1:2 ratio.

Guy Smith:

Your ratio is so much above 1.2.

Hall:

Not always, it is the borderline (that) gets us into trouble.

Guy Smith:

The ratio of fine and coarse clay, fine clay over coarse clay seems to be useful in Ohio. But there are parts of the world where that doesn't seem to help.

Question 85

Peterson:

I have a question concerning the epipedon-argillic horizon clay ratio. The Taxonomy talks about minimum thicknesses of argillic horizons with respect to the thickness of the A horizon. But I don't remember anywhere in it where it either talks about a minimum eluvial horizon thickness or where there is an A horizon (here I will use "A" rather than the epipedon or "A2") that has a progressive clay increase towards its base, and then something under it that you would want to call both the B horizon and an argillic horizon. If you are going to calculate ratios, what subhorizon of the A horizon do you calculate against. The lowest clay content or the average clay content of the A horizon? Is there a minimum thickness for an A horizon or eluvial horizons that we can use as a guide?

Guy Smith:

It's difficult to answer that question without reference to the book.

Question 86

Fenton:

I think you said before that no one could agree on the lower boundary of the argillic horizon. In our medium-textured soils, if 20% increase is a significant increase, would it be reasonable to assume that when the clay content drops below that figure that would automatically become the lower boundary?

Guy Smith:

One would have to have some considerable discussion on that with the correlators.

Whiteside:

I had a note in my Taxonomy that that sentence had been added to the paragraph as a footnote subsequently, that the lower boundary would be where it dropped below the 1.2 ratio on a smooth clay-depth.

Guy Smith:

I don't recall that. We used the lower boundary of a horizon in which there is accumulation of carbonates on the Great Plains.

Whiteside:

I think this must have been removed on those first revisions.

Guy Smith:

There was a memorandum issued that listed a few changes to *Soil Taxonomy* and the Washington staff has had second thoughts of approving any. All I am told, there have been a couple of approved memoranda issuing amendments. They have come so recently that they were mailed after I left Belgium. I've not seen them. A correlator from Fort Worth said he had a copy of a couple but no one else had seen them. He said they were sitting on his desk.

Whiteside:

I happen to have the memo here. It refers to page 385, control section, footnote 4. Insert the new sentence before the last sentence. "The lower boundary is determined using the same curve and is the depth at which the clay content is less than that of the minimum requirement for an argillic horizon."

Stout:

Guy, I have copies of those amendments, too. There are two of them and I don't recall what they are now.

Guy Smith:

I understand that there are only two. That memorandum was premature. They are going to withdraw it or they have withdrawn it.

Stout:

I don't think it has been withdrawn but it is in review.

Guy Smith:

I would not word that amendment in precisely those terms. As I understood it, I would, if I wanted to define the lower boundary in a soil that had no secondary carbonates or no lithic contact or paralithic contact, I would certainly specify that the decrease of 1.2 would be from, the maximum clay content in the argillic horizon. I didn't catch that as you read that. If you have an argillic horizon with a maximum clay content of 35%, then to get to the base of the argillic horizon you would use that 35% as your starting point or your reference point ($35/1.2=29.2$).

Question 87

Franzmeier:

A point of clarification. This list that is entitled "Accrued Changes in Soil Taxonomy" - is it necessarily approved at this point? Is that what the discussion is about? It is not approved?

Stout:

The May 5, 1978 list? That did come out. It was premature. Many of those are right but I can't tell you which ones are and which ones aren't. We will be getting announcements that will be sent out on these things. They will become attached to the handbook notices. This is one of the approved amendments to allow Arenic Albaqualfs in *Soil Taxonomy* to have a dark colored surface horizon. This is one on temperature requirements for Vertisols. This one is to establish the Great Group Fragixeralfs, the subgroups of typic mollic and ochreptic.

Guy Smith:

What Xeralfs?

Stout:

Fragixeralfs. So there are three of them. They are dated November 17, and we received them January 5th.

Franzmeier:

Incidentally, those are called inside, 2, 3, and 4 so there must be one floating around someplace that hasn't arrived yet.

Question 88

Peterson:

Are you saying when the clay content drops off to 80% of what it was at the maximum?

Guy Smith:

That's what would be implied. If I had agreed to anything like this, I would have insisted on something like that. But I might even insist on something greater. I certainly wouldn't word it this way. It would come at a lithic or paralithic contact. It could come at the top of the horizon of accumulation of calcium carbonate. I know the original intent as well as anyone and this wasn't it.

Stout:

We have talked about this, Guy at the National Work Planning Conference in 1972, I think. If you go back and look at the minutes it includes a schematic drawing of this thing. We also talked about it one time when Guy was at Lincoln.

Question 89

Peterson:

A question of clarification. When you say draw a smooth curve through the data points, there are two different ways that people draw a smooth curve. One is to literally draw it through the points. The other is to consider that we sample by depths and mix; then we should plot bar graphs for each sample layer. Then if you draw a smooth curve through the bars, it should include and exclude equal areas of each bar. You can get a rather different looking curve for some soils, depending on which sort of "smooth curve" is drawn.

Guy Smith:

The latter is what I thought was a smooth curve.

Peterson:

That's what I want. The bar graph and then a smooth curve drawn to enclose an equal area.

Guy Smith:

So your maximum is determined by several of the sub horizons of your argillic horizon, more than the measured maximum of any one of the individual sub horizons.

Peterson:

Not everyone does it that way. They should but they don't.

Question 90

Hall:

A point of clarification on one of your responses. You were talking about clay films and the necessity for identification. Then you went on to say that if it is down within the profile and it's on coarse fragments you considered it as passing through and not as an argillic horizon. I think I understand the logic. The operation bothers me.

Guy Smith:

We have lots of very skeletal and even fragmental soils in New Zealand and they also occur in many parts of the world and the clay skins are defined either as being on peds or on pores, they are not defined as being on pebbles or rock. Those are irrelevant to the identification of an argillic horizon. I will cite one example of a soil in this case, in Maine (or the same thing in Norway) from a marine shale that has been uplifted and dried and fractured so that it is just really a skeletal or even fragmental assemblage of more or less blocky fragments of siltstone. In these soils the clay seems to be quite mobile in the surface horizons. We will get a soil such as a Dystrochrept. When we hit the fragmented shale we begin to pick up coatings of oriented clays on these fragments of shale. You can trace them down to the depth at which you can examine the soil. We had a cut through these soils for an interstate highway. You could examine it for thirty feet down and it was just as thick at the base of the thirty feet as at the top. I wouldn't want to call this a thirty foot argillic horizon just because I had these clay coatings on the marine shale.

Question 91

Hall:

We have allowed a bridging in the sands. We assume that in the genesis of an argillic horizon the processes should take about the same amount of time. In fact they don't. Sometimes we find sands that are bridged that we feel or know are very young. Could you give us a little background on the thinking that went into this bridging in sand?

Guy Smith:

Does this bridging occur in the form of laminae?

Hall:

Yes, quite often.

Guy Smith:

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

Hall:

The concern, I think, is the length of time. We recognize that some of those can form very rapidly. I think in our study of genesis we think of an argillic horizon taking a substantial amount of time. I guess my question is basically, what thinking went into determining the thickness that is allowed, the thickness required for an argillic horizon resulting from sand bridging.

Guy Smith:

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

Question 92

Rust:

There is a question 35. I don't know if it is appropriate in this context of discussion. In some soils of high density, lack of structure in the C horizon gives rise to a root zone limitation which in the opinion of this person is a severe or more severe limitation than some diagnostic subsurface horizons such as fragipans which are currently recognized in *Soil Taxonomy*. The question is, why has the C horizon in some shallow soils not been recognized as a restricted layer in *Taxonomy* as have some diagnostic subsurface horizons, such as fragipans?

Guy Smith:

Well if it becomes that restrictive it probably would constitute a paralithic contact. In general we try to use properties that were the result of genesis or that control genesis, in our definitions. Now this seems to be something other than that. It has been used according to its depth as a depth class at the family level but not at any higher categoric level because it is virtually unrelated to soil genesis.

Franzmeier:

This could be, like we mentioned, compact glacial till that would be considered a genetic horizon.

Guy Smith:

That's right. I think that's what they are talking about. This came up at Cornell too, and went through considerable discussion.

Question 93

Franzmeier:

If the compact glacial till has a density approaching 2 would this be related to the densipan that was referred to earlier, or is that a shallower kind of horizon, closer to the surface?

Guy Smith:

A densipan is an albic horizon, to begin with.

Franzmeier:

It must be an albic horizon?

Guy Smith:

It has the 'powers' of an albic horizon and it has the position of an albic horizon. It is more shallower and I would say a density of 2 would be rather rare when you restrict yourself to the fine earth fraction, even in drumlins. If you include the gravels it's not too difficult to get up to 2, but not if you take the fine earth fraction.

Franzmeier:

I think we have measured some that would be easily about 1.9 in the fine earth fraction. In the sandier things it could get up to very close to 2.0.

Question 94

Hall:

You brought up the term, paralithic. We've been having a little bit of problem in Ohio with that; in identifying it, and in being consistent about its identification. There's a suggestion, and this will be in a paper that will come out in the Soil Science Society of America Journal, that this should not be a contact but should be an actual horizon which goes from unconsolidated to paralithic. Give us a little bit of background. Where did the term come from and what areas were looked at to identify this? What was your feeling on this when the paralithic was set up?

Guy Smith:

We first had the lithic contact which was a contact through some sort of bedrock that was of significance to the use of the soil and which reflected a shortening of the soil itself from the bottom. In other words, the soil just hadn't developed into this sort of material. It was a clear base of the horizons that were genetic horizons where we had it. The lithic contact created a problem for a time before the concept was proposed as a property because we said we would not classify soils on the basis of anything other than their own properties. When you get below the lithic contact you're out of soil and into the problems of geologists in the rock. But having devised the concept of the lithic contact then comes a question of the salt rocks? They are just as effective in stopping roots and engineering. They're a different sort of material because

they're more easily moved with power machinery whereas we wanted to restrict the lithic contact to materials that required blasting for engineering construction work. That was the goal. Whether we achieved it or not, I don't know. The proposals were made, they were criticized a little bit by the laboratory and modified in accordance with their suggestions. I don't recall suggestions for modification from any other source. But it's a horizon in the sense that you can see it in the field, you can sample it separately and so on, but it's hard to call it genetic, the result of soil genesis.

Question 95

Peterson:

I would like a clarification of that. I don't know whether I am misreading the paralithic contact definition or if I am just afraid to use it. But I keep worrying about using it when one finds "decomposed granite" (i.e., early stage saprolite from granitic rocks) which excludes roots; it breaks out in chunks of gravel size, so to speak, and it will disperse when you shake it in water. I would presume this to be paralithic-contact material.

Guy Smith:

That was the intent.

Peterson:

How about so-called "compact glacial till" does that also fall in there?

Guy Smith:

That would also fall in there.

Peterson:

Quite a few things would, actually. Do soils on decomposed granite in southern California have paralithic contacts described for most of them?

Guy Smith:

I thought they did. It's paralithic. It would be a shallow family, but only if the paralithic contact is shallow enough.

Question 96

Holzhey:

I would like to bring up the topic of densipan one more time. Yesterday you mentioned that densipan occurs in albic horizons and in the U.S. We have a number of things that have

been identified or described as x horizons. I was not quite clear, but are those similar to, or identical to, the densipan as you conceived it?

Guy Smith:

The albic horizon in a soil as an Alboll or an Albaqualf in Illinois and Iowa, like Putnam and Cisue, when dry, has some of the characteristics of the densipan in that you spend five or ten minutes in getting an auger through it, you grind at it. But it differs distinctly when the soil is wet; there is no resistance to an auger, in those albic horizons. I don't know of measures of the bulk density from these albic horizons and I don't know the series in which the albic horizon is identified as an A2x.

Holzhey:

The one that comes to mind is in the McBride series which has been described as having a weak somewhat intermittent spodic horizon over an 'A prime 2x' over a strongly expressed argillic horizon, I mean over an 'A prime 2' horizon. It is brittle and firm but I have not dug in it when it is wet so I don't know about the strength.

Guy Smith:

I saw one profile in northern Michigan in which I was a little puzzled about the nature of the albic horizon. It was quite difficult to get through with the auger. When I got through, the water ran down the auger hole and disappeared, the surface water.

Question 97

Rust:

In the questions that we have put together - number 6 on the latest compilation (this would be number 9 in an earlier listing) - the question or thought was raised that, as a result of the work of Kubiena and Brewer and others, we have another way of describing the morphology or an additional way of describing morphology of this natural body of soils. I suppose the question, Guy, is, had you given any thought to this kind of micromorphology description of soils to bring it anyway into Taxonomy. Had you, or would you, consider it something to be looked at for the future? Where do you place this kind of work?

Guy Smith:

We did try to describe the micromorphology of the cambic horizon, the argillic horizon and the spodic horizon. There had been enough, I think, studies of those that we could have some confidence in the micromorphology there. I have been concerning myself with the possibility that the micromorphology of the oxic horizon might be more diagnostic than that of the cambic horizon. But I am unable to find very many thin sections with descriptions of the oxic horizons. It does seem to me to offer considerable potential in the definition of the oxic horizon. There, even in the field, the morphology seems rather distinctive in that, in the fresh pit, it appears to have no structure and yet when you examine it you find that you have a very strong granular structure but the granules are so small that they are not visible to the naked eye. We've not used micromorphology of the epipedons because this is so subject to change by soil management. We have some descriptions of the micromorphology of fragipans and of duripans, particularly the studies made at Riverside on the duripans. This has not been generally available, I suppose. Steve, your thin sections on the duripans, did those get published?

Holzhey:

Only as a summary sort of paper and I don't believe they were published as a detailed study.

Guy Smith:

You did send me copies of your slides but I didn't remember seeing anything that would be generally available to the public yet. The micromorphology is an expensive thing to study and I don't imagine that we will ever have many studies of micromorphology compared to the descriptions that we get of soils that are written in a pit somewhere in the field. Data, I suspect, are always going to be limited because of cost.

Question 98

Rust:

In that thought or connection, if you had an alternative of a micromorphology examination and a laboratory determination, it's possible they could be of equal cost. Laboratory determinations, as you pointed out are expensive. Looking ahead, or looking perhaps, as you say, to the oxic horizon, are you suggesting that it would be just as well to pursue it in this vein as to pursue it chemically?

Guy Smith:

Well, I use the example of the point of zero charge as an expensive laboratory measurement, and I would not actually want to bring that into the Taxonomy. Until we have some sort of substitute that we can make readily, I would prefer to keep them both out. They are working in Hawaii on a relatively simple laboratory method that approximates the point of zero charge. It is correlated with it but it is not that.

Question 99

Peterson:

I am looking at the questions from your Cornell conversations. There is one in here in which they ask about the effect of geomorphic concepts on *Soil Taxonomy*. I'd like to turn that around. I've got sort of a private point of view that the infusion of soil concepts into geomorphology did more for that science in twenty years than conversely. Many of the basic operational ideas on geomorphic age were already described in papers written about the Great Plains in the '30's and in the 1927 or '28 papers out in California. Do you have any comments on the effects you have seen within the geological world of the soils-geomorphology work?

Guy Smith:

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

Peterson:

How about Shaw's California studies? The paper Shaw published on the different families of soils in California (1st International Soil Science Society Conference 4:291-317 1928) was essentially a geomorphic study. Did that not get into some of the thinking in soils until much later? Also, Thorpe has a paper in about 1940 again describing surface ages.

Guy Smith:

Well, Shaw's classification of soils according to stages of development probably had a distinct impact in California. But not as much as it probably should have had because when I first visited California to study the non-calcic Brown soils I was shown the same series with and without a duripan. It was treated as a phase rather than as a series differentiae. In the middle west the studies of development of the argillic horizon in soils formed in loess led to very much the same sort of concepts that Shaw had, namely, that you have a continuum between the Hapludolls, the Argiudolls and the Albolls, and that this was split into segments in the middle west. It was this study, actually, that led to the use of the various properties that intergrade between one Great Group and another at the Subgroup level. It could not be shown at the Great Group level but it seemed important to show this at some categoric level and the subgroup took over for this function. But I don't recall if Shaw had anything of this nature in his classification, he merely used the presence or absence of these overly developed horizons on the higher terraces and their absence on the lower terraces to show that these differences were due to the time factor, but the idea of an intergradation, I don't believe you can find in Shaw's papers.

Question 100Rust:

Guy, I think you are, to some extent, speaking to questions 2 and 4 in your list. Is there any additional background on the subgroup category? Is it fair to say that it forms a junction between what some would call the high classification and the low classification? I think it is not fair to say that, but what do you feel is the role of the subgroup category?

Guy Smith:

Well, it is the lowest category in which we consider genesis in forming our definitions. When we go below the subgroup into the family and the series we find that the distinctions are largely pragmatic, that we want one series or two series because of some differences important to our interpretations and this has been the basis for justifying, establishing series. And the

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

Question 101

Peterson:

Following along on your statement about building a classification from the bottom up versus from the top down, there has been quite a bit of philosophical discussion of classification in various places. What are your reactions to the possibility of using numerical taxonomy to alter or improve or create soil classifications?

Guy Smith:

It has potentials, but we are not yet ready to explore numerical taxonomy. The studies that have been published have been very discouraging for the use of numerical taxonomy for a number of reasons. It is quite common, for example, that one starts with multiple correlation between particle size and organic carbon and so on. If you find a high correlation between two properties you use only one of those properties for your classification, you eliminate the other. The advantage that the proponents claim for numerical classification is that it is not biased by judgement because each property is given an equal weight whereas in *Soil Taxonomy* we weight some properties more highly than others. This advantage is a fictitious one because in assigning an equal weight to each property you are still weighing it, the only difference is that you are weighting them the same. And it seems absurd to me to say that the color hue of a soil has the same importance as any other property of the soil. I do recall one such numerical taxonomy classification in which the Haplaquolls of Iowa were most closely related to the Calciorthids of the arid region. Now this seemed incomprehensible, and, of course, it comes from the selection of the wrong properties. That's the central problem of numerical taxonomy and it seems to me that we will not get anywhere with numerical taxonomy until we quit eliminating properties because they are correlated with other properties. This correlation is imperfect and in one part of the world it may hold and in another part of the world it may break down completely.

Question 102

Fenton:

I have a question pertaining to what, in the past, you called drainage classes, especially among biosequences here in the Midwest. Most of the same what poorly drained Mollisols are classified as Aquic Hapludolls, but the transition and forested members of a biosequence are classified as Ochraqualfs. Was it your intent to reflect that those soils with forest influence have a different moisture regime than those soils formed under prairie vegetation or is it just the way the criteria were selected?

Guy Smith:

No, I don't think that was the intent. We had a problem with the drainage classes in that there were five of them and we're limited in Taxonomy to showing four, at the moment. We haven't figured out a good way to show the fifth one except at the series level. The drainage classes are very ill-defined. I'm to blame for that I suppose. We had it in the Soil Survey Manual and we had the committee working on that with the correlation staff who finally submitted a report which tried to define the various drainage classes. Dr. Kellogg revised this report slightly and assembled it in the draft manual and went off to Europe and gave it to me to criticize. I read the definitions and discovered that the very poorly drained class included all Aridisols because they said, no water goes through the soil. That's very poorly drained. I pointed this out to Dr. Kellogg as he was about to leave and he said fix it up. So I had a couple of weeks to do over again the work of the committee on drainage classes and any imperfections there are due to me. But the interpretations of what was said in the manual can differ very greatly. I went with Andy and some others on an excursion to compare the classifications on the Great Plains with those in the Middle West. We stopped on the Fargo clay along the Red River. On the North Dakota side, Fargo clay was classed as a well drained soil and on the Minnesota, just across this little channel in the Red River, the same soil was considered poorly drained. Between well drained and poorly drained, that's a rather extreme example perhaps, but that is the way that the North Dakotans and the Minnesotans considered these soils. So it's very difficult to use these concepts that are so ill-defined, better to try to devise some definitions and then check those out to see how they group the soil series. There was quite a bit of discussion at this point, at the moment, when we discovered the difference in drainage classes across a twenty foot channel.

Question 103

Fenton:

In relationship to that subject in the Midwest a very high percentage of the Aquolls have been drained. Their water table is now at a greater depth than before the land was cultivated and there's a high probability that many of the morphological features that we see would be considered relict features, or at least in another hundred years will be considered relict features, related to that pre-settlement water table rather than the present water table. Do you have any thoughts or concerns on that?

Guy Smith:

No, I won't speak on that.

Rust:

Well, I would think, Guy, that you have thought about it because you have encouraged us to get real water table data.

Guy Smith:

If possible, in areas that have been undrained so that we will understand something of the genesis of the soils. Once drained, it is very difficult to estimate where the water tables stood before drainage so that in our definition, we say of the aquic suborder that it is saturated or has artificial drainage. We don't say that it was saturated because that is an inference, not a demonstrable fact. Now I have not considered what it will be like a hundred years from now.

Question 104

Peterson:

I can't help but back up and ask a question that I'm sure you have answered before. I was wondering when and for what reason did you shift from the class definitions from so-called modal-individual or type-individual thinking for defining classes to defining classes with limits, or "fences"? Just a little subsidiary question to that: if we no longer use the modal-individual thinking to define classes, why do our soil series descriptions still have a "typical pedon"? Wouldn't that be "typology" pedon classification?

Guy Smith:

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

Aandahl:

I mention that it was pretty much the same type of thinking that goes into breaking down the Great Group into the subgroup. We wanted to call the central concept of the series and then to relate it to the other series by pointing out how Barnes goes to Svea - where you draw the line. I think is quite similar to the type of thinking that goes into the Great Group and the Subgroups.

Peterson:

You may have a typic subgroup but it is not the type example that the botanist or zoologist at one time used to establish a class, literally, by a specimen in a museum. Nor is it the central concept that you mentioned for this typic subgroup. I also take it that this "central concept" is in no way a statistical mode. Am I correct?

Guy Smith:

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

Question 105

Peterson:

Just a comment about the ability to use the class name of the *Soil Taxonomy ad hoc*. This is one of the handiest, nicest things about it. I've also noticed a certain reticence to do it. I was displaying that the other night when I was reticent to speak of "xeralfic epipedons"; I got a little bit worried about manufacturing new ideas. I have noticed this reticence to use this device, and yet you have spoken strongly for it right in the Taxonomy.

Guy Smith:

It is pointed out.

Aandahl:

Just one comment on that, I have found out that doing that sort of thing was very convenient to group series when I was working on my map, like the Udic Ustalfs, Typic Ustalfs, Arid Ustalfs.

Question 106

Rust:

Somewhere it says in *Soil Taxonomy* that it is not a compendium for the beginning student of soils. The question that is somewhere in this list relates to the problem of reading some of the definitions. Many of the students are concerned about reading more exclusions than inclusions in some definitions. I am sure you must have wrestled with a lot of this writing. What can we say to the students in this regard?

Guy Smith:

Well, it is possible to simplify these definitions enormously if we're willing to forget about, say, 1 percent of our soils. Maybe less than 1 percent. The greater part of the complicated part of the definitions are due to the presence somewhere of a group of soils that belong together. They're very similar in all their properties but they overlap one of the limits at a higher category. I've used a number of times the Glossudalfs as an example. These soils have a rather narrow range of base saturation at the limit between Alfisols and Ultisols. They straddle that limit but they never get far from it. And they have so many similar properties that they needed, we thought, to be kept together. When writing the definition then to permit Alfisols to have a base saturation of less than 35 percent we introduced a serious complication into the definition of Alfisols and of Ultisols, both orders. One should say to the students that these definitions are written for people who are actually classifying soils for the Soil Survey. For the people who use the map, the use of Taxonomy for other purposes than these complicated definitions is unnecessary. And I think it can be done, too. The definitions can be greatly simplified by footnoting to a definition the presence of some exceptions. At one time I had thought to do this myself. I still may do it but this current book we are talking about seems to have a higher priority. And I received considerable discouragement when I discussed this possibility with the Washington correlation staff.

Rust:

Dr. Franzmeier isn't with us anymore but I had some feeling that he had thought seriously about the kind of 'student version'.

Question 107Holzhey:

Along this line of the degree of complication of the definition. The definitions were simplified greatly by the statements of class criteria and were greatly simplified by the definitions of the series, of diagnostic horizons, and features in the soil. The statements of the criteria themselves could be further simplified by adding a lot more definitions, say, for example, a definition of dominance by expanding lattice clay or something of that sort. Yet if you did that you could end up with such a long list that that would be unmanageable. Do you have any comments on the lines of reasoning or the things that influenced you in stopping where you did in the defining of things such as dominance by a morphous material and features of that sort and then describing things such as shrink-swell properties, repeatedly in the class definitions?

Guy Smith:

It's not an easy question to answer. It is certainly true that the definitions of the classes are greatly simplified by referring to the diagnostic horizons. If one had to repeat all of the characteristics of a particular diagnostic horizon any time you used it the definitions would be unmanageable completely.

Where it seemed critical to comprehension we did try to use, not necessarily horizons but features. We did try to define these, to simplify the definitions, well, I suppose, we didn't do any more of it because we didn't find it necessary. It's a very vague limit as to how far one should go in that direction.

Question 103

Peterson:

What would be your reaction if someone came out with a simplified *Soil Taxonomy* for the perplexed in which lengthy definitions of features were condensed? I suspect some them, for example, the mottling in the gley colors, are not all the same, are they?

Guy Smith:

Not at all.

Peterson:

Could one still go ahead and talk about "gley conditions". Then manufacture some name for all of these different evidences of wetness, but use that name instead of the actual characteristics for that group. How would you react to that?

Guy Smith:

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

Peterson:

One could also group the discussion of evidences of aquic soil moisture regimes in a separate section. That device would, I think, greatly increase the comprehension of, say, geographers, archaeologists, foresters, of many of the Taxonomy parts. Just the statements you made on the changes in the evidences of wetness with temperature changes has rationalized much of that detail for me.

Guy Smith:

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

Peterson:

Such comments on significance would increase the intelligibility of the soil of Taxonomy for the student.

Guy Smith:

Yes, that difference could be explained at some length in the discussions of, I hate to say, "aquic characteristics", but the kind of aquic characteristics that we use for the aquic subgroups.

Rust:

Does this suggest another formative element?

Guy Smith:

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

Question 109

Rust:

I would like to begin by reference to question 9 on one list. Rather on question 15. This question is talking about the matter of establishing the taxa of a system and says that theoretical taxa for which real pedons could reasonably be expected to exist were not created as long as a real pedon was not discovered somewhere to document the case. The next is the questioner's commentary. I suppose, that this strategy appeared to a result in a slow growth of the system and thus, changes are generated over long periods of time. Now the question may be what were the compelling reasons to adhere strictly to the rule of considering only pedons, perhaps more correctly, polypedons, which had been recognized in a field to create new taxa? Could some problems have been avoided if reasonably accepted taxa would have been introduced in the system at some early stage of development, particularly at the higher level of extraction? Perhaps this has some particular bearing on the Histosols but we can develop that as we go?

Guy Smith:

I think the Histosols would be one good example in which we did not insist on a pedon but we worked out a theoretical classification that provided for foreseeable contingencies. We had no alternatives with the Histosols because we had no well defined series of Histosols in the U.S. against which we could test our proposals. We have probably more subgroups amongst Histosols that are proposed than we will ever have soil series in the U.S. We will have to completely re-examine what has been done in Histosols. This does not suggest that providing for soils that we do not know would simplify anything. In fact it will require more changes. The general rule that we followed of not providing for a taxon until we had some knowledge of its existence was because we did not want to prejudice the classification of a soil that is currently unknown. We wanted to wait until we had a chance to study that soil and its behavior in order to decide how it should be classified. Classification is not just an arbitrary system of subdividing when you know nothing about what you are doing. You have a purpose for classifying and as an example that has been used in other discussions, I would like to take the definition of the typic subgroup in which Item A is something, Item B is something else. We provide for a subgroup for soils like the typic except for A and other subgroups, soils like the typic except for B. Suppose we find then a soil that is like the typic except for A and B. This is called an implied subgroup but to decide whether we want that subgroup we have to have an example that we can study. It may be that we will prefer to avoid that implied subgroup by saying that the soils are like typic except for A with or without B. We wouldn't say B with or without A because those are parallel definitions. We would however, not want to establish that subgroup in the absence of any knowledge about its behavior. I can not quite agree with your questioner that to provide for every contingency would reduce to many changes.

Tarnocai:

Basically we looked at the organic soils, and made some changes in our classification. For example, in the control section, since we now have only one control section, not two like we had before. And it's because we received more information and we thought that simplification was necessary and practical, too. Otherwise, we still have problems with the organic soils. We are in the process now of again reviewing the organic order, especially related to the Folisols. We have a proposal and in the next two or three years we will have some kind of a final answer to these proposals. I think we are not in good shape as far as the organic soils.

Question 110

Farnham:

What was said about the theoretical number of Histosols was certainly true, but, as Guy has mentioned, there were not very good series definitions of the Histosols. I remember, Guy, when we first did this, some of the state soil scientists and the correlators were scared to death that we were going to have thousands of new series. I tried to tell them that I just didn't believe that was the case. We went ahead with this theoretical classification. Actually there were 38 defined subgroups and the last time we took a count, only 37 of those have shown up in the world or in the U.S. anyway. I'm not sure we need all those 37, a lot of these are single series subgroups. What bothers me most is the highest category of the system, at the suborder level we use the criteria of decomposition stage. We thought using three systems, it would work very well, the Russians use five, and I didn't like five. It was hard enough to get three but this has gone over very well with the committee. Just recently, last year, the International Peat Society has taken the idea that there are three types of peat which correspond to our three types of Histosols. They are now using the terms, fibric, humic, hemic, and sapric. The difference in the European approach and Canadian and U.S. approach is that they (the Europeans) don't particularly map pedons or mapping units. They are more inclined to map landscapes, peat lands, not peat or organic soil. It's hard to get over this, although Walter Stanek is chair of a terminology committee, and I am on this committee to try to get the people together. There's a fellow in Germany, Dr. Schnerdtfege trying to coordinate the various systems. He gave a paper recently at our Int. Peat Society Conference in Duluth, in which he compared the U.S. system and the Canadian system, the German system and the Finish system, and so on. A very good paper which will be published very soon. You have there, Guy, on that abstract I gave you a summary of his paper. Anyway he is very interested. He even went to the trouble of coming ahead of time to this conference last summer and went in the field with several of our parties, our survey parties. He went out with the SCS group in St. Louis County, Minnesota and he came down and my boys took him out to Anoka County, Minnesota. He wanted to see the soils in the field. It's taken a long time. I have made lots of talks and I've gotten almost hoarse when I've gone to Europe when I start explaining to them our system. Not only our technique but what we are classifying, I had a hard time getting a three-dimensional pedon concept across. But the Canadians, of course, have been using this for years, our maps and our taxonomy. I do think in this business of fibric, hemic and sapric we were too restrictive on the two end members. Let's put it this way, looking back on it, there's a lot more Hemists than I'd like to see in this, not that it has to be a balance but I think that we were too restrictive with the Saprists and too restrictive with the Fibrists. You know we pulled out the *Sphagnum* types but nowhere except Florida and one place in Minnesota, do we have a fibrous - other than *Sphagnum* - that I've seen. There's about two series in existence so what I'm thinking is we should change a little bit of our concept for the description of these horizons. These organic soil materials, not horizons. We should broaden, we should include a little more sapric, take out of the Hemists that end of it that approaches the more decomposed. Take out some of the

Hemists that were relatively raw and put them in the Fibrists. I don't know how to do that, Guy.

Peterson:

When you say that, are you including the tidal marshes?

Farnham:

Yes, I've seen series. Florida has a Tropofibrist, Minnesota has a Borofibrist. I don't believe in the middle west there is a Medifibrist. Like Ohio. And Iowa, I don't believe so although I think it exists in Iowa. I'm not sure that it's ever been mapped and I think of the *Hypnum* mosses that do exist in these in-between temperature regimes. I know they exist and we have some here in Minnesota. They are few and far between but there is *Hypnum* moss over rock. It's in this in-between climatic regime. I noticed some in Michigan. It's an excellent peat, it's what the Germans used to export to the Long Island area of New York state as prime peat moss. They called it brown peat moss, as opposed to *Sphagnum* which is really light-colored. And those people there, the horticulturalist thought that was the greatest thing in peat moss. So called brown peat moss, it was *Hypnum* moss from Germany.

Guy Smith:

I should like to comment on the procedure we followed. If we had not made these proposals and focused people's attention on the possible combinations of characteristics, we would not have people studying the Histosols and writing descriptions that were more intelligible than the old ones in which we had woody peats. These were largely classified on the basis of what was growing on the bog rather than what was in it.

Farnham:

But that concept still exists in Europe. Just because it is *Pinus sylvestris*, (scotch pine) growing on the surface of the bog, they call it woody peat. That was a hard one to overcome, calling it woody peat just because there was black spruce in the bog.

Guy Smith:

That was very firmly entrenched in the U.S. series definitions.

Tarnocai:

Maybe I can add some more from our experience to that. In establishing soil series in organic soils we tend to pay more attention to peat material like *Sphagnum* peat, forest peat, or aquatic peat and so on. To form the soil series these are the components which are quite important. In our experience, if someone says, this is a Typic Fibrist soil in our classification where we have fibrous, it could be *Sphagnum*. Some of this feather moss peat would not go into the fibric end. So the classification did not necessarily indicate a situation that exists or the use for interpretations. It tends to put more emphasis on peat material.

Reiger:

It's a question with the boreal great groups which you've heard about. They were set up expressly to separate at a fairly high level those northern or cold peats that can be farmed successfully. But as it turns out a number of peats are included in the boreal great groups that have no prayer of ever being farmed. Which creates a real problem in our classification, in Alaska, particularly. It's rather illogical and I'm wondering if the boreal great groups should be abolished or be redefined so that only those peats that can be cultivated are included.

Farnham:

I think maybe Guy might have a comment on that. I remember we had some misgivings about putting a boreal group in there at one time because you do not have anything other than boreal and cryo groups. Do you?

Reiger:

That's right. Those two.

Farnham:

So it is a matter of where you're coming from. But in the case of the Canadians I might ask Dr. Tarnocai. Southern Ontario, to me, would be in the same zone as Ohio. It would be in the medi-temperature regime. I don't know if you make that distinction, do you make that distinction?

Tarnocai:

We don't have a boreal.

Farnham:

So I think it's logical. Maybe we should consider whether we need boreal. Although we did it sort of following the ecologist's idea of the boreal forest zone soil.

Guy Smith:

We will need eventually some sort of an international committee to re-examine the whole problem of the classification of Histosols and I think the formation of that committee should wait until we have actually accumulated more experience and more descriptions and analyses of the soils. At the moment I suspect we are still rather short in the U.S. at least of descriptions and analyses of Histosols. They have a very low priority for study. Partly because their extent is so limited.

Farnham:

There's a lot going on at the moment in the Department of Energy inventory of fuel-type peats. Minnesota is in on that but Minnesota has state money in it, therefore, we said that any survey, is going to take into account all peats, all organic soil and we don't want to go back to make an agricultural type survey later. In other words we said we don't want to just make an energy peat survey in Minnesota. In the state of South Carolina they have a charge there, with the money obtained from the federal government, Department of Energy, just to look at energy peat. I don't know how they went at this. They couldn't presume it was going to be a non-fuel type peat when they went out there. At any rate it was only a partial survey that they made. They are doing this in Alaska too, I might add, right now. Now I didn't like that approach, I like the multiple use approach. When you go out on a landscape you may never come back. You get all the information you can get. I was taught that the first day I was ever on soil survey. I've never forgotten that. Who is to say what that soil is going to be used for? I don't like that concept but that is the way they are operating. They have surveys going in Maine, Alaska, Minnesota, Michigan, Massachusetts, South Carolina, North Carolina, Florida, with Wisconsin being run by Eric Bourdo at Michigan Tech with help from Bartelli. Bartelli is retired but he is at Michigan Tech. They are doing the survey, sticking to the D.O.E. concept that they will only look at fuel peat. What is the purpose of the survey if you already know what it is, why make the survey? There's a lot of money being pumped into it, three hundred thousand dollars a year in Minnesota. There's a hundred and fifty thousand in Michigan and mostly it's in the Upper Peninsula. The South Carolina survey is completed. This was done by Dr. Cohen who is a geologist at the University of South Carolina but he really knows his peat. He's done an awful lot of work on microtome sectioning of peat in the Everglades and the Dismal Swamp. He has accepted our fibric, hemic, sapric terms in the U.S.D.A. system.

Tarnocai:

We have the same problem. There are separate surveys going on for locating fuel peat and it basically is in the southern part of Canada. The problem is, for example, in southern Quebec. These deposits which are prime fuel peat are the best agricultural organic soils. That is one thing. Now the question also came up why would we need these separate surveys, why don't we have a survey for everything? We re-examined our program as far as organic soil mapping and we found that the main criticism we received from these people was we are only considering the surface, 160 to 180 cm. That's where the classification was based. Secondly, our description as far as material concerns were not adequate for their use. Thirdly, the far north carried out analyses which serve their purposes in interpretation. Sulfur content. These were the comments we received, so in the future how we try to shape our organic soil mapping will be to introduce this test or analysis so that data can be used for interpretation or establishment for fuel peat. We introduced added information from our examining peat deposits not only within 180 cm but to the total depth. We are encouraging soil surveyors to do cross-sectioning so we can get some kind of a volume estimate of the peat deposit. That was the problem. We are hoping that in the future we will answer all these questions and all the information will be useful for finding fuel peat and interpreting peat deposits for fuel peat and other types of use.

Farnham:

One other thing to add is that in the survey some of the newer techniques, remote sensing satellite imagery, plus ground penetrating radar are being used in these surveys. Now we were not too happy with it in Minnesota. We did it at about 40 degrees below zero in Minnesota about this time last year and it didn't come out well. The company claimed that they could tell the difference in the density of peat. I doubted that but they said that. It didn't show that. They did it in Alaska, Sam. Did you see the report? I have a copy of the report. They did it in Florida. Ray Daniels did it quite a while ago. This company is located in Massachusetts. That particular survey, I was familiar with the bog they surveyed, had a previous survey with the old map. They compared the ground penetrating radar. There is a lot of work going on and the Macaulay Soil Research Institute in Scotland is into this. They presented a paper on ground penetrating radar plus the remote sensing of peatlands at the I.P.S. Congress last year. A lot of this is going on in Canada also.

Reiger:

It could be very difficult in northern areas because there are many soils with the histic peaty O horizon not thick enough to be a Histosol. Same vegetation, some about 20 inches deep on the surface. It would be almost impossible for any remote sensing to distinguish between the peat and Histic Cryochrepts in the huge areas there.

Farnham:

There are other indicators. You have to have a lot of ground truth for remote sensing. There's a lot of indicators, Sam, that are not just the heat sensitive ones you're talking about. If they are all wet it's going to show up like the same dry peat. But there are other things they are going by now and it looks pretty good. They take these observations monthly, and get this information processed in South Dakota. You do the same area, in the office, in the summer, spring and winter months. I was absolutely amazed, I could find every raised bog in Minnesota looking at a winter satellite photo. I could find every single one, even in the winter with snow cover. Amazing, I don't know why, it must be all that water in the raised bog, something affecting the heat sensitivity. That was no problem. The *Sphagnum*-raised bog is easy to find with an ordinary black and white photograph. The Macaulay Soil Research Institute is doing a lot of work on that. Also there are others. The Swedes have gotten into the ground penetrating radar business on their surveys. I might mention the Swedish soil scientists and geologists are working on an inventory of Swedish peat bogs for energy purposes. So a lot of this information is really coming forward. A year or two from now we're going to be a lot further along than we are on the techniques of survey as well as classification.

Question 111

Rust:

The comments that you have made raise a question, Guy, and I believe it's already proposed in some other way. Where is there a place in Taxonomy for this additional information below our present arbitrary or non-arbitrary depth of classification? Must it be handled totally differently? We really, as some people have called it, have a gray zone between the lower limit of our classification area and where the geologists pick up their interest.

Guy Smith:

Well, we could say generally that our control section is adequate for agricultural uses. Where we need interpretations that involve examination of the soil materials to a greater depth, that is, unconsolidated materials, I think we're fully justified. I do not think the nature of the materials below our present control section should be brought into the Taxonomy. I think it should be a matter of phases. It might require phases that include not only the criteria that we have used in the proposed classification of Histosols but phases according to the calorie content of the materials, sulfur content of the materials, the things that are critical to the use of the material for production of energy. This can be phased.

Question 112

Rust:

Would you use a comparable logic in the mineral soils also?

Guy Smith:

Primarily for irrigation projects in order to predict where the irrigation excess water is going to surface and salts are going to accumulate. For that we may need our drilling equipment, we may have to trace out the aquifers to find out where the excess irrigation water will surface.

Question 113

Holzhey:

There's a related question in question 12 which asks why the properties in the control section don't come to the surface? This is a pretty common question and you may have been asked this one before and may have discussed it. I am asking if you have any comments.

Guy Smith:

Well, I have discussed it in previous sessions, very briefly. Surface horizons, particularly the plow layer, have many properties that are drastically affected by the management system and for our interpretation we do not require much because our interpretations are made on the assumption of specified systems of management. If we built them into the Taxonomy you would find your classification changing when one land operator died and someone else came in with another idea about how the soils should be managed. Our pedologist simply can not examine every part of every field, for example, to find out which part got lime and which part didn't and which part received fertilizer and which part didn't. This would be a virtually impossible task for the man making soil maps. In Russia they do this on the state farms and the cooperative farms because the management is going to be controlled. It isn't going to change until there is a change in the direction from Moscow. So they have, the Russians, in their soil maps, have cultivated varieties of soils. Some category is cultivated and some is severely cultivated. I don't know the definitions of those terms but they show up again and again.

Question 114

Fenton:

In the family textural groupings, in many cases, when those boundaries are superimposed on our textural triangle, the boundaries do not coincide. I was wondering if you could briefly give us the background of the relative weight of the engineering influence versus the agronomic influence on the choosing of those boundaries for the family classifications?

Guy Smith:

I suppose it's about equal. Though I would hate to be very specific on that. We had to subdivide the loams and the silt loams somewhere in the neighborhood of 13 percent clay. That is an important limit in the engineering classification but it also has some considerable importance to the growth of plants. The silt loams, for example, in the old textural triangle ranged from zero to twenty-seven percent clay and when you are in a coarse silty family you have a number of problems with the growth of plants. Their structure is bad, your irrigation, your permeability is very slow, because of the poor structure it puddles rapidly and you don't get much penetration of your sprinkler water, it just runs off unless you apply it very slowly. So that there is an important agricultural difference between coarse silty or coarse loamy but particularly coarse silty and fine silty particle-size classes. We had a great deal of difficulty in deciding what to do about the very fine sand and Dr. Whiteside and I had much correspondence about this. We tried to get the engineers and the geologist and pedologists to agree on a common classification and each society basically said we are willing to have a common one if you choose ours. So that effort broke down after quite a few years, didn't solve our problem of what to do with very fine sands which, in general, behave more like silt than they do like sand. In terms of capillary rise, in terms of available moisture-holding capacity and so on. So I could see nothing to do but sort of let this distraction float in the particle-size distribution grouping so that if the soil was otherwise a sand, examination of summation curves showed that the bulk of the very fine sand was more than seventy-four microns in diameter but if it was otherwise a silt loam the bulk of the very fine sand was less than seventy-four microns. So we arrived at a grouping that is very similar to that of the engineers. The geologists used sixty-four, I believe, but this was not purely for engineering interpretations because these properties of capillary rise or moisture holding capacity are also important to the growth of plants. In general, I think one can say that most of the properties that are important for the growth of plants are also important for engineering uses. Or vice versa.

Question 115

Hall:

In relation to engineering during the development of this Taxonomy; was there any attempt or thought of putting engineering parameters such as Atterberg limits, etc. at the family level to make these separations? Sometimes I have gotten the feeling that engineering was kind of tacked on as an afterthought.

Guy Smith:

The problem (of putting) the Atterberg limits, for example, into the family is that we have so few determinations of Atterberg limits. If we used them we wouldn't know how to use them and we wouldn't know how they caused the groupings of our soils to be changed. It's simply a lack of data. The engineers have a large volume of data on Atterberg limits but not by kinds of soil.

Question 116

Rust:

Considering the increased use of our surveys for non-agricultural and non-forest uses would you suggest that we should seek to develop some special parameters that would be more appropriate or more useful for engineers and these other kinds of uses? If we did, where would we use them?

Guy Smith:

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

Rust:

Is it still continuing, Dr. Whiteside? Have you got the engineers convinced?

Whiteside:

This is continuing, I think the Illinois Engineer, T.H. Thornburn, actually had some experience in the Michigan State Highway Department (e.g., SSSAP 24:297-300, 1960). Two M.S. Theses at M.S.U. have dealt with this subject also; C.C. Wang, 1967 and G.C. Steinhardt, 1968.

Stout:

Yes, South Dakota took 22,000 samples and put them on the computer and then we went back to the field and matched up the identity of soils that were out there. When we got these things together, we had an excellent correlation. It does work and it is very good. South Dakota is presently designing highways based upon this. They estimated that they have reduced their sampling by 75 percent.

Rust:

Someone has said that the problem we have with engineers is that they look upon the soil as hamburger and we look upon it as steak. So that they are making an unnatural body out of our natural body. Does not this, in some respects, pose special problems in dealing with the engineers?

Guy Smith:

Well, yes, points of view are very unlike in some places. And there's a very large education job needed among the engineers, but it needs to be done by engineers.

Question 117

Rieger:

In view of your comments about the history of the family textural classification with respect to agriculture, does it make much sense any longer to continue to use our old textural triangle?

Guy Smith:

Well, it doesn't to me but there is a tradition here, I don't know how long it will take to change it. There were serious defects in the old textural triangle that required that we make some rather drastic changes. In the first place, a boulder of a meter in diameter was not part of the soil. How this idea originated, I don't know, but the larger stones were not considered as part of the soil although trees growing there and so on, noticed the stones. The fragmental family class had no place in the old textural triangle. A soil may be a hundred percent coarse fragments but if these are large fragments then there isn't any soil there despite the trees growing. So we could not use the old textural triangle for a variety of reasons. It lacked the break between fine and coarse loamy and silty which approximates the engineering break between plastic and non-plastic. It ignored the skeletal classes completely. The bulk of those could not be part of the soil. You can't bring them into your textural triangle. The fact that it is sixty or seventy percent by volume of boulders and stones comes out exactly the same as a soil in which there are no boulders or stones. If the boulder gets on the surface it is treated as a phase but otherwise it is ignored in the old textural triangle. They are revising the manual. I don't know what they are going to do about that. They maintained that we must have the two terms "texture" which relates to the old triangle and "particle size" class which relates to *Soil*

Taxonomy. I think it gets rather confusing at times but I'm not around to argue about this much.

Question 118

Collins:

In some subtropical areas there are argillic and spodic horizons several feet below the surface. Would you consider that to be a soil material or geologic material?

Guy Smith:

They can actually be considered deeper than that, well developed cemented spodic horizons with twelve to fifteen feet of quartz sand above. We pointed out specifically here that when the spodic horizon is more than two meters deep that it's presence or absence is not too important to the use of the soil above except perhaps as a source of sand. We draw the limit at two meters on that and we classify such soils as Quartzipsaments. The reason being that the difficulty of observation in two meters of sand is enormous. One commonly has to have drilling equipment and case the hole with his drill in order to get down to the spodic horizon. It didn't seem that this would be a good investment of money for the soil survey. The presence or absence may be of some importance; the occasional boring to find out whether or not the spodic horizon is there would be of some interest from a soil genesis point of view. I've been enormously puzzled on these as to where the aluminum in the spodic horizon can come from. I have no answer to that question yet except that because there's nothing but quartz overlying the spodic horizon the aluminum must come from some outside source, perhaps a moving ground water, in which you have the humus coming down from the surface and the aluminum coming in laterally and then the two can meet and precipitate. It's the only hypothesis I can think of, how it checks at the moment I don't know. You have a somewhat similar situation in North Carolina with Dr. Daniel's geomorphology study, when under some of the Paleudalfs, at some depth below the argillic horizon, one comes into sands that have every appearance of a spodic horizon. An argillic horizon above a spodic horizon you can see but it is so deep that we have only few observations of it.

Farnham:

In that same area, there's a lot of aluminum where have you organic soils, that same kind of problem you are talking about. Like in places where the Leon soil used to be mapped. I believe the Leon had to have the spodic in the upper two meters.

Guy Smith:

Yes, they have more than one series according to the depth to the spodic horizon. The Leon often has, in deep pits, multiple spodic horizons. And there was a long argument at one time about whether these represented different positions of the ground water or whether they were buried Spodosols. A radiocarbon date on the organic carbon in the spodic horizon of the Leon was around twelve hundred years and the first next lower spodic horizon was a bit over twenty thousand years. So I concluded that was enough investigation, that we would consider these as buried soils.

Question 119

Rust:

When you just mentioned or made the comment about the possibility of lateral movement of aluminum into a profile, or maybe out of, this suggests a landscape feature. The question is, (you have spoken to it at Cornell to some extent), are we able to establish, understand relationships between soils on the landscape with the assistance of Taxonomy or do we have to look elsewhere? That's poorly phrased but you probably sense the question.

Guy Smith:

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

Question 120

Fenton:

Perhaps, a follow-up on that question. In the Soil Survey Manual there is a statement "Soils are landscapes as well as profiles". I suppose in *Soil Taxonomy* the polypedon takes care of that landscape aspect. But there is, I think, a tendency to overemphasize, in some cases, morphology or diagnostic criteria at the expense of landscape. I think that has been somewhat corrected with the need to correlate interpretations across state lines or even within state. Do you consider the emphasis on morphology at the expense of landscape to be a problem in *Taxonomy*?

Guy Smith:

I think the basic answer is one that is related to another question that has come up repeatedly, what are we classifying, pedons or polypedons? The polypedon is a landscape in the sense that it has shape that the individual pedons do not have. It has transitional borders to other polypedons and it has natural borders. The pedon does not have a natural border, it's shape may be very different from that of the polypedon in which it belongs.

Question 121

Peterson:

Why was the old term "soil individual" dropped and apparently replaced with the term "polypedon", was there any really important difference here or was this just a preference?

Guy Smith:

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

Question 122

Hall:

In light of your comment about polypedons and the soil and the landscape relationships, would it be reasonable to require a description of the landscape as related to a soil on soil series descriptions; limiting that soil to a certain landscape?

Guy Smith:

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

Hall:

The use of a single soil on two or three landscape positions may suggest that we just haven't looked at it close enough or maybe not related it to the use as well as we should.

Guy Smith:

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

Question 123

Rust:

This question is in regard to the making of soil maps with soil identifications at a categorical level above the series. We don't see too many examples. Should this be encouraged? Where would you say we ought to strive to do this? I guess one can understand why in the U.S. we haven't done it much because we have so much series information but in terms of using *Soil Taxonomy*, should we not be encouraging the making of more maps at some higher categorical level?

Guy Smith:

Surely. In the lesser developed countries, where there is relatively little soils information, the use of series as the basis for map units of large scale maps is going to result in the same kinds of problems we have had in the U.S. Since the survey was started in the U.S., series have been split time and time again and the names changed -- at least a large proportion of the series have been split. In such a situation as the lesser developed countries, I would encourage the use of a higher category until such time as we develop information that will permit us to make different interpretations for different parts of a soil family. An additional situation that occurs to me is one such as Alaska. Dr. Rieger, I think, can explain why he didn't set up a lot of soil series in Alaska.

Rieger:

Actually, we did not and one of the reasons was because of the rule that requires two thousand acres to have been mapped before a series can be recognized. But to get back to the original question, when you have an area that is essentially unknown, access is difficult, and you want to get information in a hurry, it certainly pays to operate at a higher level than the series. I think this would apply to undeveloped countries and to many of the western states' rangeland area. Another situation is, for example, making a map in the tundra where reindeer grazing is important. On the average, it takes some three hundred acres to support a single reindeer. Now obviously you can't justify a detailed survey so there you work at the subgroup level, or as we did, phases of subgroups, to make a useful map in a reasonable amount of time.

Guy Smith:

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

Rieger:

Well, of course, as a preliminary to making a small-scale survey we tried to select spots, areas of ten square miles, or even less, and map them in detail so that we could get an idea of the composition of these larger units. When we did that, we used the standard series identifications for the map units that we set up on the detailed mapping blocks. Now, whether, after that's been done, it is worth while maintaining these series, keeping a record, making all the interpretations that are required if you are going to have an official series?

Guy Smith:

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

Peterson:

Just to bring you up to date, Guy, the extensive surveys being made in the western United States are not being made at the family anymore, unfortunately. Unfortunate decisions were made by the BLM, to my mind, for these surveys of large areas; one was to use phases of soil series rather than phases of families for soil identification; the second was to map at 1:24,000 scale rather than, say, 1:60,000 scale. Yet in terms of time and money they were talking in terms of an Order 3 survey; actually, I think they were thinking in terms of Order 4 survey. This demand for detail is the "eastern bias" I've been talking about. It's so deeply ingrained in people's minds that they think one can not interpret a family. If you phase a family to pick up thicknesses of horizons you can make most interpretations since it's the only particular thing that is needed to come up with the same kinds of interpretations. That just didn't seem a possibility in their minds. The result of this was 1) very slow progress in the mapping to start out; 2) an inordinate demand placed on the back of the correlation staff in the states involved; and 3) too many and too small delineations. The whole thing just slid towards an ordinary Order 2 survey. Then there is another thing involved when the soils are identified as slope and stoniness phases of series that I think is rather disconcerting. There is an implication of more knowledge about those series than there is either reasonable or desired the actual intensity of mapping is Order 3 or 4 level. So we may be deluding ourselves, whereas, you didn't, Sam, on your type of work. But it was just impossible to fight the "eastern bias". That the series is the only valid soil identification and that you can't interpret a phase of a family.

Guy Smith:

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

Peterson:

Surprisingly, they also begin to use the soil family or subgroup names. It's the same as dealing with people in terms of the Latin plant names. If they hear them often enough, pretty soon they begin to prefer them.

Guy Smith:

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

Question 124

Peterson:

I would like to add to that. Recently, correlation staff members involved in Order 3 soil surveys that weren't progressing rapidly enough -- perhaps in desperation -- got out old 1:60,000 scale photos and put those in the hands of the field surveyors. After mapping on the 1:60,000 photos with large delineations, they then traced the lines back onto the 1:24,000 maps the BLM and the Forest Service like to use. They can't seem to get away from detail, but this device at least cut down cartographic detail. I wish the Order 3 kind of soil surveys had been defined without series as a possible taxonomic unit, but that was too shocking to the traditional concepts of necessary taxonomic and cartographic detail for field soil mapping.

Guy Smith:

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

Rust:

A number of us have students from the developing countries. We are concerned about trying to offer them a scheme that would be useful in their preliminary work. We don't have enough examples from the contiguous forty-eight states and we won't look for one in Alaska that we can point to as examples.

Guy Smith:

Well, no, I don't suppose you're apt to get any examples of any importance in the U.S. unless it is for the extensive range soils where you're making virtually only one interpretation -- the production of edible forage. And yet I consistently advise people in the developing countries to avoid using soil series at any cost.

Peterson:

Dick, there are examples. Ed Naphan's group in Nevada started reconnaissance mapping of rangelands back in the early 60's, and they did do a lot of mapping at 1:250,000 scale using phases of families.

Guy Smith:

Yes, but are those published?

Peterson:

Yes, they are. A limited edition. And then we made three more surveys that were field mapped at 1:60,000 and published at 1:250,000. Two of them have been formally published (Railroad Valley Area and Dixie Valley Area, Nevada) and the third one is in preliminary form yet. Those are available, there are a few copies around yet. They were used for more than range interpretation. In fact, I don't know that they were used for range interpretation. Part of the problem of mapping the rangelands at a scale and at a level of categorical identification that seems appropriate to the soil scientist is that it seems quite inappropriate to the range scientist. We had a discussion the other night about the problems of mapping concepts originating in range science. These can be very serious problems. It's the range scientist, I think, that had more to do with the decision to stay at very detailed levels of mapping and then pretend that we could do it rapidly. There is a lack of understanding of what the soil map units are, and what the associated range interpretation map units would have to be. That problem has yet to

be logically analyzed and aired in discussions with enough data behind it that one can see what happened.

Rust:

Dr. Tarnocai, are there any lessons from Canada here?

Tarnocai:

Well, we faced the same problem and we carry out reconnaissance and exploratory surveys, level 4 or 5. Quite a large number right now. We had to go away from soil series because the information that we collected was fairly broad. We can not establish soil series on the basis of that. We cover such a large area we are not able to deal with the soil series so what we use is soil associations. In Canada the soil family is not very well accepted as useful or a category which is used for mapping. The soil association is widespread. We don't have to go too far north in British Columbia. They use it in forested regions very much.

Question 125

Rust:

Can you match it with any category of Taxonomy?

Tarnocai:

Basically the definition of the soil associations includes soils developed in a similar parent material. That's basically what it is. The toposequence. This is our way to solve this problem.

Whiteside:

It might be like the situation in Greece. They developed their mapping from experience of one of their leaders in this country in the late 50's. They have chosen at the present time to use *Soil Taxonomy*. Actually in their *agricultural areas* they're essentially using phases of subgroups. They refer to those phases as 'series'. They have not assigned geographic names to them but use symbols including the subgroup symbol plus the subdivisions of that. Unfortunately they haven't described those subdivisions of the subgroups anywhere in a standard series, such as our soil series description sheets, in all the survey areas. I asked whether they should be developing series names. I discouraged them from doing it. I think their present system has many advantages over what we are doing although it has also some risks. They need to more carefully control their legends. They tend to get more units than they really need. In their *forest land* they are just beginning to make their national inventory. I'm trying to encourage them to use a similar legend, such as phases of subgroups, which would give them at least a level at which they could compare their inventories throughout the country. Unfortunately the person in charge of that was intrigued by the world FAO-UNESCO soil map legend and has tried to use that with phases. You can arrive at similar kinds of groupings but that requires more coordination than we have yet been able to establish between the two groups. That's the kind of problem we are working with there to make all their efforts consistent with the national inventory, upcoming in ten to fifteen years. They are using, in the forest land, a scale of 1:50,000, and in the agricultural areas many are irrigated 1:5,000 to 1:20,000.

Guy Smith:

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

Question 126Peterson:

We've been talking about extensive areas so this leads to a question I've written up for myself. What role did the size or extent of different kinds of soils -- I guess you would call it geographic extensivity -- what role did this play in the construction of the *Soil Taxonomy* as compared with the role of differences in kind when one is thinking in terms of pedons?

Guy Smith:

I was violently opposed to considerations of geographic extent. I am just as strongly opposed to the rule they have that you must have mapped two thousand acres to establish a series. We have lost some information as a result of that rule for very contrasting kinds of soil for which we could not get a series name because the total area involved was less than two thousand acres. But because of the extreme differences in the nature of the soil and the information we could get about soil genesis if we could preserve the location of those small areas, I would have preferred to have had established series. The general principle was that area was not to be considered, except for this two thousand acre minimum for establishing a series. The FAO-UNESCO legend is admittedly biased toward soils that are extensive enough that they can be shown on a 1:5,000,000 scale map. There are many kinds of soils that are extensive on a given tract of land, but that can not be placed anywhere in the FAO-UNESCO legend because they can't be shown on a 1:5,000,000 map. To me that's one reason why that legend is ill-suited to large-scale maps and interpretation of large-scale maps. Even if they add additional categories, as is now proposed, add two more categories to that legend, they're not going to be able to correct that bias toward geographic extent of a particular kind of soil.

Question 127Peterson:

Didn't geographic extent come into decisions on defining, say the order, suborder, great group categorical levels? For instance, would the mollic epipedon have as great importance as it does if it didn't have its great extent?

Guy Smith:

The diagnostic horizon, not the Mollisols. Mollic epipedons are in a number of kinds of soils. Several orders. There's a bias that is inescapable, insofar as there is a probability that we will fail to study a kind of soil of very small geographic extent. That's inescapable. It has to be extensive enough that we're going to find it.

Question 128Collins:

What is the geographic extent of a soil. For example, a soil series which is mapped on floodplains. It's geographic extent is from just south of Minneapolis to just north of Kansas City and from about Lincoln, Nebraska to Champaign, Illinois. With the chemical and morphological analyses which have been done the soil seems to be similar. Should this soil be one series or would you try to separate two or three or more series from it?

Guy Smith:

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

Question 129Rust:

Has not the example which Dr. Collins cited - could one of your correlators speak to this - has not this been something that has been faced up to, if not in this region, in other regions?

Stout:

You mean on the extent or range of series? I think probably she's picked one for which there is some evidence of too wide a range. Guy is correct in pointing out the differences that you may have; regional climatic differences more than anything else. The soil moisture is not too much different and the morphology is very similar over the area used. I can't think of any other series which have quite that wide an extent except Sogn. It's range is an administrative decree rather than anything else.

Guy Smith:

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

Stout:

The Colo series is mostly used in Iowa, along the edge of eastern Nebraska, and extending into the western edge of Illinois. There's not quite as much difference as one might suspect.

Turner:

The series to which Mary is referring is also positioned on the floodplains, and is considered a poorly drained soil. You have that also compounding the moister climate.

Question 130

Rust:

On this climatic problem, Dr. Peterson, have you asked in your discussion about some of the kinds of climatic prefixes? How do we establish these?

Peterson:

Yes, I think Guy gave an answer to this. He said it would be very nice if we had some soil morphological diagnostics. But, I would like to add another question to that. Is the criteria by which we decide that a soil moisture regime class boundary is appropriate only vegetation? Is there any other purpose or any other reason why we should be worrying about soil moisture regimes other than vegetation?

Guy Smith:

You mean both cultivated and natural vegetation?

Peterson:

Yes. In fact, let me amend my question: wouldn't it be reasonable to use native vegetation to set class limits where that is the expected use of non-irrigated land and then use crop plants where that is the expected use of non-irrigated land?

Guy Smith:

In the estimates of moisture regimes, we surely are concerned with the cultivated plants, where that's the expected use. Where the cultivated plants are absent, as they are in many of

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

Peterson:

Pardon me, that wasn't my intention in the question. I'm talking about using the native vegetation to establish the class limits of the soil moisture regime, not to map them. The significance of different moisture regimes presumes that you have some way of determining them, either from soil morphology, or from calculations from climate data, or from moisture regimes. Obviously we have very little data for actual, measured soil moisture regimes.

Guy Smith:

We won't get many and in the mountains virtually nothing that is useful because it can vary so much in such short distances. You can't always have a network of meteorological stations or study the soil moisture over a ten-year period. We had quite a good discussion about this in Lubbock in which some of the men who were concerned with mapping of federal lands in so-called native vegetation said that a good man could just look at the assembled vegetation and give you an excellent idea of the soil moisture and temperature regime at that point. Their experience is extremely important and we've said in *Soil Taxonomy* that we shouldn't use properties that can not be measured or at least estimated from the combined knowledge of pedology and one or more other disciplines. For example, we estimate mineralogy for some of these soils from our knowledge of pedology and geology. We get at the ages from our combined knowledge of pedology and geomorphology. We get at the moisture regime from the combined knowledge of pedology and the experience of the range people, the foresters, the botanists. On the plains we have also the common knowledge of the cultivators which is probably better than our knowledge from the meteorological stations.

Question 131

Tarnoczi:

In Canada we are using the vegetation also to determine climatic and moisture regimes because of the lack of climatic data. In order to overcome the problem of disturbance, let's say fires, we would rather rely on a chronosequence of vegetation. That tells us what kind of a situation we are dealing with even if we start with the Aspen forest. There are indications that succession will lead this way or because we look at the whole picture. We can reestablish the picture from all of the known stages. Then we can use this to establish regions. We are working now on these eco-regions, which basically delineate large segments of the country, and are relatively uniform climatic areas based on the interpretation of the vegetation and the climatic data.

Guy Sraith:

It can happen. This disturbed vegetation before it is replaced can have a profound effect on the nature of the soil. In the Aspen areas of northern Minnesota, under pine I think I could demonstrate that we had a good Spodosol with a thin but well developed albic horizon. When the pine was cut and the O horizon burned, the Aspen came in. It was a much better food for earthworms than pine litter and the worms simply mixed the spodic horizon and the albic horizon over very large areas of northern Minnesota. They're still at it. In this case, of course, one has to look at other things and the fact that the stumps are pine rather than Aspen.

Farnham:

It's an old road and it's exposure is to the south in this particular country. It has a beautiful Spodosol exposure profile. If you jump over the fence into the Aspen grove you won't find any evidence of a Spodosol. I worked myself to 'death' in that aspen grove trying to find an A2 horizon. I took a tiling spade and away I went. I never did find it. Just like you say, what I think it is though, Guy, is a recirculating out of the nutrients of the leaves. The Aspen leaves are not taking nutrients out of the soil. In fact, I have some data. The pine did not have many nutrients, that is, the pine needles are not returning nutrients. The pine don't use as many nutrients as Aspen, so the Aspen is recirculating the nutrients. I think it is changing pH of the soil. That's the most spectacular thing I ever saw. Here was a preserved one because of the exposure of a profile. You couldn't find any evidence of it back in the Aspen.

Guy Smith:

I've had similar experiences in New Zealand where most of their type profiles have been sampled in roadcuts and they have shown me very nice Spodosols with A2 horizons. I always insisted on crossing the fences into the pasture. Under grass with fertilization you can not find that albic horizon anywhere. It's still a Spodosol in *Soil Taxonomy* because we don't emphasize the presence of the albic horizon as do the Zealanders or the Australians. I don't know about Canadians.

Tarnocai:

I have another example and we use these for an indicator when conditions are changing, especially thermal conditions in the soil after forest fires in the southern portion of the discontinuous permafrost zone. The permafrost, after forest fires could disappear completely. In other words permafrost moves out of the system. Of course, it could come back if the stable vegetation is re-established. Now, in these areas there is a problem of a discontinuous permanently frozen subsoil. For example, in Thompson, Manitoba, there is a rule that before urban development takes place the soils must be surveyed and have to be cleared and exposed for at least two years. If this is not carried out then, for example, half of the house could be on frozen soil and the other half on unfrozen soil and half on frozen soil is drastic when melting of the frozen soil takes place. As another example from the same area is the railroads running to the nickel mines, carrying the ore. When the railroad was built this wasn't considered and now the rails are hanging in the air about a meter or so because of the subsidence of the railroad bed.

Question 132

Guy Smith:

We have a similar problem with the Alaska railroad, don't we, Sam?

Rieger:

Alaska, most of it, has been burned especially during the growing stage but the soils in the cryic zone, apparently cooling off, don't survive. We found that in burned soils, in the succession after the fire, you first get aspen and birch, then white spruce and eventually black spruce if it's allowed to remain unburned for several hundred years, but during this whole vegetation sequence the soil is essentially unchanged. I suspect that it's the low temperatures that makes the difference. Do you find that?

Tarnocai:

Well, I think if we go further out of the southern limits of the discontinuous permafrost zone, the moderating effect of fire or any other disturbance is much less. So what we will have is increasing depths of the active layer so that the depth to the permafrost table increases. The active layer between is drier, warmer and that's about it. The permafrost is still there.

Rieger:

We can have the situation where a thin layer of permafrost disappears, as you say, after a fire. This poses a problem in classification. Presumably the soil temperature will remain the same -- below the freezing point. But no evidence of permafrost.

Question 133

Rust:

You have been giving examples of climatic vegetative relationships. But it seems that the kind of examples you've given are rather drastic changes in vegetation as a function of fire, cutover, whatever. Our botanists keep impressing upon us the idea of plant communities and the succession of things in plant communities. Doesn't this make it a little more difficult to establish a relationship between a climatic property of soil, if indeed, your plant community is changing throughout the season and really very gradually over the landscape in many respects? How do you deal with that problem?

Tarnocai:

Definitely. If you think about it in a micro-meso-scale, that's definitely a problem. These eco-region areas are very broad areas where change in vegetation takes place due to climate. When we look in this area on a meso-micro-scale there is a definite problem as far as change of vegetation, removing the vegetation cover, or removing the organic layer which is just as important as the vegetation cover. Then, in order to determine the climatic characteristics of the soil, we have to collect soil temperature information and establish the relationship to use. For example, in Manitoba, in the south half of the province, we have about ten years soil temperature data and it is very interesting information based on measured properties.

Question 134

Fenton:

I have a question on a different topic. This morning you expressed a principle that I am interested in following up on a bit. You said in the definition of a typic subgroup that the extent of that unit didn't matter, in other words you wanted a typic and the other ones you defined as varying from that by a certain property. One of our concerns here in the Midwest is that in an operational soil survey to use the same principle a little differently, the representative pedon of a soil series is to be located within the mapping unit of that series that has the largest acreage in the survey area. In the long run, in terms of relating our taxonomic system and soil genesis, doesn't the principle you espoused about the acreage limits not mattering, apply also in the terms of stability of the landscape from which those mapping units come?

Guy Smith:

There probably is a somewhat different guiding principle involved there in selecting the pedon that typifies or represents best the mapping unit. I shouldn't say 'typifies' probably because I think that 'representing' is perhaps a better term. If it is to be representative of all of the map delineations carrying that particular symbol in the particular survey of the county, I think the area is a matter of some consequence. You map a phase of a series in one county and you go two counties away and you map that same phase of that same series but it is not necessarily quite the same as in the first county. And for some uses of these soil surveys, for example, planning a secondary highway, the engineers at least are interested in what they are going to run into on that particular map unit most frequently. In splitting up the continuum of soils in Taxonomy as I said, we tried to avoid that but we had that one little inescapable bias that if the soil was so rare that we never saw it, it wouldn't get into the system.

Fenton:

Consider this problem. In County A, the representative profile has a C slope, another county the profile is on D slope and another county, on B slope. Over a long period of time if you're interested in going back and studying that soil series, say from a classification point of view, or soil genesis point of view, you have no reference point from which to work. The typical situation changes from county to county. Perhaps we should use a system whereby we describe two pedons, one that would be for a specific purpose related to genesis, and another for the specific purpose of describing the population in the county?

Guy Smith:

Presumably, if it is in the same series, the phase would have very little influence on the genesis. It could if you have the wrong phases. When we first started our cooperative work with the highway engineers, the Bureau of Public Roads, we took three samples per county. One that represented about the center or the middle of the range in properties in that particular series in that particular county. One that was marginal to some adjacent series but still within that same series. And another that was marginal to a third series but still within the range of the first. For some years we sampled our soils for the cooperative program with the Bureau of Roads. I don't know whether that's been continued under the present program of cooperating with the State Highway Department.

Stout:

Yes, we are still doing that, Guy. In most cases we are not sampling the minimum and maximum, we are sampling what they think is fairly representative in the survey area. It's working out, I think, fairly nicely. We have compared the three of them together and we found that, in most cases, we were getting three at about the same kind of status.

Guy Smith:

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

Question 135Fenton:

One of the concerns is that the more sensitive indicators of differences in soil genesis, such as depth to clay maximum, solum thickness, depth to neutral pH, and so forth, as you well know, are systematic functions related to landscape position. From a genetic point of view, when these properties are compared without landscape control, there is confusion in terms of explaining the genesis and depth distribution of properties of a particular soil series.

Guy Smith:

The depth to clay maximum, of course, is influenced by more than genesis, it's influenced by erosion that has taken place. These are fairly complex measurements.

Question 136Stout:

Guy, several years ago when we were working with the approximations we had frigid Ustolls, that were later changed to Borolls. We still have thermic and mesic subgroup adjectives and then we switch to the suborder level for frigid soils and use Borolls. Can you give us some idea of why that shift was made?

Guy Smith:

It originated in the Lincoln Regional Technical Service Center, and I don't recall being in on much of the discussion about it. The potential reason is that it is simpler to control the soil moisture than it is the soil temperature and by putting all the Borolls together you have then a group of soils where one very limiting element is soil temperature. On that is superimposed the moisture problem which we take care of at the subgroup level rather than the suborder level.

Stout:

We get quite a few questions from our younger soil scientists wanting to know why don't we have something similar in the thermic area?

Guy Smith:

When you get home, ask Andy about it.

Aandahl:

The Chernozems with frigid temperatures were considered a unique group of soils which should be recognized at a rather high level. In the *7th Approximation* they were called Altolls and defined as follows: "The Altolls have a mollic epipedon that, to depths of 15 cm, (6 inches) or more, has chromas of less than 1.5 when moist, and that has common bleached silt and soil grains.

The mean annual temperature is less than 8.5 degrees C (47 degrees F)... Also, in the *7th Approximation*, the Gray-Wooded soils were included in the Altolls which were Alfisols with mean annual temperatures of 8.3 degrees C (47 degrees F). In *Pedologie* (the lectures by Guy Smith presented at the State University of Ghent in 1964-1965) these names were changed to Borolls and Boralfs which place more emphasis on soil temperature.

The supposedly unique color of the Borolls which distinguished them from the frigid Typic Ustolls was frustrating to apply in the field. During 1965 it was abandoned and all Mollisols with mean annual temperatures less than 8 degrees C (except Xerolls) were called Borolls.

The possibility of recognizing the distinctions between mesic, thermic, and hyperthermic Ustolls at the suborder level was never given serious consideration.

Question 137

Farnham:

Couldn't this go back, Guy, to the old Southern Chernozem and Northern Chernozem?

Guy Smith:

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

Question 138

Hall:

Along the same lines, was there ever any consideration given to length of growing season in the classification; it's related very closely to temperature.

Guy Smith:

This is related quite closely to the soil temperature, the mean annual soil temperature and the mean summer soil temperature at 50 centimeters. There was discussion about using degree days and so on to get a better refinement of the length of growing season but this doesn't seem to be a soil property as such and I would prefer to use this as a phase if necessary to distinguish between the shorter and the moderately short growing seasons. In the isofrigid soils, certainly it is a factor but it's correlated again with soil temperature on a mean annual basis. In areas with 10 degrees of mean annual soil temperature, you have frost virtually every night. Your growing season is about fourteen or fifteen hours and that's the longest one you get. So you get there another 'march' boundary. If you get above 10,000 feet maybe up to 11,000, the frosts are rare or absent. You can stand on the mountainside and survey the mountains and you see cultivation comes up to a certain elevation and stops. So I stood on a mountain in Ecuador and I looked across the valley and I saw a field on the other side at about the same elevation. I asked the local people, isn't that maize over there? Yes, it is but it takes three years to mature it.

Question 139

Peterson:

Back to the Borolls: Why did you decide to let the Cryoborolls go out into the far-western mountain ranges, which are otherwise xeric?

Guy Smith:

The assumption was that they were not xeric at high elevations, that there it is so cold that the soils would probably be udic even though the bulk of the precipitation comes in the form of snow in winter. The growing season is short enough when evapotranspiration is important, that the soil shouldn't be dry long enough to get into xeric or ustic. That was an assumption.

Peterson:

I believe that assumption works at least above 8,000 feet in central Nevada. What if we find that we have a zone on the mountains that is cryic but does dry out so that it would go xeric?

Guy Smith:

I would say we'd have to find that before we would know what we wanted to do. We did specify that the cold dry soils were not accommodated in *Soil Taxonomy* for lack of knowledge.

Question 140

Rust:

We are, in this region, obviously, interested that a considerable extent of Mollisols, Alfisols are mapped -- and a few others. Are there some questions or concerns in the experience of classifying these particular orders that some of you want to raise?

Rieger:

Mine concerns Spodosols. This is in the Aquods. If a placic horizon in the Aquods is either above a spodic horizon or above a fragipan it is classified as a Cryic Placaquod; however, if the placic horizon is within the spodic horizon or below it, it's either a Placic Haplaquod or a Placic Humod. I am wondering what is the reasoning behind this, why so much importance is attached to the position of the placic horizon within the profile, whether it is above the spodic horizon or in the spodic horizon?

Guy Smith:

This comes from the study of the British Podzols with thin iron pans. They have there this very involuted horizon. If the placic horizon is separated by some depth from the fragipan that underlies it, there is a spodic horizon under the placic horizon. But in the deeper involutions of the placic horizon there is no spodic horizon because the placic horizon rests directly on the fragipan. This was a desire to keep this kind of soil from becoming a complex of a great many series. The definition was written in this way so we could have this ruptic spodic horizon in the thin iron pan soils of Great Britain.

Question 141

Rieger:

How about the situation where there is no fragipan?

Guy Smith:

That does not then apply, the fragipan is required in addition to the placic horizon, I think in order to ...

Rieger:

It can be either above a spodic or above a fragipan and it would be a Placic Aquod. If we go to the key to the suborders.

Guy Smith:

I was just looking at the definition of Placic Aquods. Aquods that have a placic horizon that rests on a spodic horizon, on a fragipan and/or on an albic horizon that is underlain by a fragipan. That leaves a little loophole there. Actually from a genetic point of view, this placic horizon has no spodic materials above it because for the most part we have strong lateral movement of the perched water downslope. When we get into the Humods where the placic horizon, say, is in the spodic horizon, this lateral movement is not adequate to prevent the

accumulation of spodic material above the placic horizon. There's been a lot of confusion between what has been called podzolization and the placic horizon. Many people feel that this represents translocated iron and aluminum and, therefore, represents podzolization. The placic horizon differs from the spodic horizon so far as I know, in only two respects, the thickness and the common presence of accumulations of manganese as well as iron and aluminum. We can not find in the normal spodic horizon an accumulation of manganese. This is an indication of ground water effect of some sort that we do not find in the Humods or the Orthods. And so far as I know, we don't find it in the Aquods other than the soil with the Placaquod where you may have manganese in the placic horizon but not in the spodic horizon that underlies it.

Rieger:

According to studies of the placic horizon, by McKeague for one, there are two distinct kinds of placic horizons. In the upland soils, there is a two-part horizon with the upper part relatively high in organic matter, the lower part high in iron. Under an upland or blanket peat you find a placic horizon where the upper part of the horizon is red, high in iron, and the lower part is black because of manganese rather than organic matter. I think, in the case of the well-drained soils with spodic horizons -- Placorthods, Placohumods -- the manganese is not that critical. I could be wrong. But in the Aquods the main problem that I have is that we have one series with a placic horizon in the albic horizon above the spodic horizon. The second series has a placic horizon within the spodic horizon. Yet we've got to call one of these very similar soils a Typic or Cryic Placaquod and the other a Placic Haplaquod based on a very minor difference.

Guy Smith:

Perhaps it was because we didn't know very much about these soils other than that they existed. If you hadn't gone to Alaska to study them, nobody in this country was studying them, and in Europe they were considered very unlike soils. So, without any way to test the matter, this appeared in fairly early approximations. And they really never got criticized in the U.S. or even in Canada, to the best of my recollection. This happens throughout *Soil Taxonomy*. Proposals are made that came through by default. Lack of criticism.

Question 142

Rust:

Any other problems with the Spodosols?

Rieger:

I have one other question. The FAO classification has a unit called Podzoluvisols. These are not the same as Boralfic Cryorthods. There is supposed to be a significant clay migration and organic matter - iron-aluminum complex migration simultaneously. Do these soils actually exist?

Guy Smith:

I haven't seen such a soil, no. I thought that their Podzoluvisols were more like our Borolls with an albic horizon rather than an argillic horizon.

Rieger:

No. Perhaps I misunderstood.

Guy Smith:

I have to go back to their definitions to find out just what they had in mind. But I think this is a carryover from the concept that a Podzol was a soil with an albic horizon. It could have any kind of B horizon, it could be argillic, could be spodic, it didn't even have to have any horizon of accumulation of anything. In the sands in Australia where the upper 50 centimeters of the sand was bleached and white and there was no horizon of accumulation of anything, these were considered strong Podzols in Australia. The Russians considered any soil with an albic horizon podzolic.

Whiteside:

I didn't come across the use of that soil in upper Michigan where we go from sandy to clayey materials, where the kinds of soils we find are bisqual. When get them with clay loam parent materials a little finer, this happens. The two illuvial horizons come together as coatings on top of the peds in the argillic horizon. I suppose that is the kind of thing they are talking about, I'm not sure.

Guy Smith:

That's the definition. We have those soils very well developed under the Kauri pine in New Zealand where the argillic horizon may have 60 to 80 percent clay and lying directly on top of that and tunneling into it we have spodic materials. But those spodic materials are very low in clay.

Rieger:

The point here is that if these soils actually do exist, we really have no place for them.

Guy Smith:

Not in Taxonomy, no. If the two are distinctly separated so that you can scrape that centimeter of black material off the top of the gray clay materials and dig it out of the tongues between the prisms, we proposed another kind of intergrade between Ultisols and Spodosols in New Zealand. The present intergrade is defined as having a horizon with all the properties of a spodic horizon except the accumulation index. This is quite different from the soils of New Zealand where the spodic materials are, perhaps, adequate. Even in some parts of the pedon, if you hit a tongue and sampled vertically, you will get an adequate index of accumulation. If you miss the tongue you won't. We can only view these things when we can study them. But so far I have yet to find what could be identified as a spodic horizon with much over 22-24% clay. There's an antagonism there of some sort. It's currently unknown.

Question 143

Rust:

We read about the Kauri pine profile. What is outside the sphere of the influence of the Kauri pine in terms of the landscape?

Guy Smith:

You get a variety of soils, very commonly Dystrochrepts. The Kauri pine can not make an albic horizon in a Vertisol. But it can in a material with a considerably coarser texture. They are mostly coarse to fine loamy Dystrochrepts.

Rust:

One tree does not a forest make but it does make a soil.

Guy Smith:

Yes, we have a tree there in New Zealand that makes a placic horizon. You find the placic horizon just where that tree stood and nowhere else. There are still things to learn.

Question 144

Rust:

Any other concerns with the Spodosols?

Guy Smith:

Steve, we got your note on the record that you are working on some of these things in the laboratory.

Holzhey:

Yes, I should expand a little on what I said yesterday. I mentioned that the field kit that is being tested, tests for aluminum. I should've said that there is a color test, of the extract which is related to the organic accumulation in the spodic horizon. So it may work in the Aquods low in iron-aluminum as well as in the soils that have a lot of aluminum tied up with the organic matter and that's one thing that's being tested. A strong extractant that hopefully pulls a low molecular weight organic into solution in a form that is still colored enough that the color of the solution can be used as a field tool in the identification of the spodic horizon.

Guy Smith:

Have you corresponded with Mr. Blakemore in Soils Bureau of New Zealand on this? He has this same color test that he's been working on in the laboratory rather than the field.

Holzhey:

We've looked at some of the material that he put together for you when you were in New Zealand and George Holmgren has. I don't know if he has corresponded with him. He was aware of Blakemore's interest in his activities but I'm not sure about the extent of correspondence.

Guy Smith:

He was still working on this when I left New Zealand. He was trying to compile a reasonable number of values for different densities of colors. We'd better make sure that we are in touch.

Question 145

Rust:

Another order?

Peterson:

One of the criteria used to distinguish the Xeralfs from the Aridisols when the Xeralf has an aridic soil moisture regime bordering on xeric is that the epipedon is both massive and hard or very hard when dry. In southern California, at least with the granodiorite parent materials, the uncultivated Xeralfs do not have a massive, hard, dry epipedon. It's weakly structured and slightly hard at most. So, it would seem that along the aridic soil moisture regime border this would result in very short-term anthropomorphic type changes of the identity of the soils. If cultivated a soil would be a Xeralf, whereas it would have been an Aridisol of some sort previously. That seems like quite a change in classification. I'm wondering if use of the massive and hard when dry criterion is an appropriate one for identifying Xeralfs?

Guy Smith:

This criterion came from the experience of looking at the noncalcareous brown soils in California and comparable soils in South Australia, mostly cultivated soils. Nobody really ever showed me a virgin soil, I think, in this environment. In South Australia the soil with a hard, massive epipedon was called a hard-setting stage and is comparable to the cultivated Xeralfs in the U.S. They disappear over a distance of only three or four miles. We went into more arid climates and there we found soils with argillic horizons, they had a very soft epipedon. It seemed to work on the basis of the soils that they showed me in Australia and in southern California. Ustalfs can do the same thing; they do in Venezuela, at least. As you go from the Ustalf or the Ustult to the Aridisol, the epipedon is first hard, massive and then soft. Experience generally can be utilized as a field criteria where you are just on the margins between ustic or xeric on one hand and aridic on the other. The intent was that it would avoid the necessity of forming judgements about which side of that boundary you were on. Focusing attention on it then causes people to make more observations. If I'd left it out, it wouldn't have been the subject of any studies whatever. Even though it is aridic. We did the same thing between the Aridisols and the Mollisols. We said that if you had a mollic epipedon, a Mollisol could have an aridic moisture regime. And in the marginal area between the ustic and udic moisture regimes we tried to use presence or absence of soft, powdery lime in the profile to put the soil in the Udalfs or Ustalfs. This was all done to avoid the necessity of actually determining the moisture regime. Now, certainly the presence or absence of soft, powdery lime is not a good marker between Udalfs and Ustalfs in non-calcareous parent materials, especially in regions where there is very little calcareous dust in the air. I suspect that several or most of these attempts are going to prove impractical once we've focused attention on them by putting them into Taxonomy and we may have to modify them. It's going to make it more difficult to map.

Question 146

Rust:

In the use of a consistence term such as "hard when dry" would we be encouraged to look for some field test to come up with a quantifiable number on this kind of determination?

Guy Smith:

I don't know. I think it would be interesting to see some studies of the micromorphology of these in that I think when I look at a soil that is moist I can identify the ones that will become hard and massive when dry, using just a ten power hand lens. Professor Tavernier also agrees. He thinks that's possible. He calls it a "ruined structure". But I haven't seen any thin-sections on any of these; somebody someday may undertake some. We've looked at them in many places in Italy and Spain in the xeric soils.

Peterson:

Does he have any more descriptive statements besides "ruined"?

Guy Smith:

No, r-u-i-n-e-d. I can not quantify it very well in the absence of good terminology. I only know that when I look at the soil in a hand lens I think I see a distinct difference. To quantify that would require work with thin sections first.

Question 147

Peterson:

When you get to the Aridisoi side of the boundary, from a Xeralf or an Ustalf, then you see a non-ruined structure?

Guy Smith:

Yes, it seems quite different under a hand lens.

Peterson:

Well, I've got to look at that.

Question 148

Holzhey:

I might just comment that one of the characteristics of these epipedons that are not both hard and massive in the uncultivated state but are in the cultivated state is the ease with which that uncultivated consistence can be destroyed. Or, that is, the ease with which the hard consistence can be created with manipulation when the soil is wet. It does seem as though there should be some relatively easy technique to do that and then look at it. Either index the strength of the structure or somehow destroy it with a simple technique where you would just wet it, destroy it and then look at it. We haven't devised that, I might ask, are you aware of any work of that sort?

Guy Smith:

No, I am not.

Guy Smith:

On this structure business I would like to come back to the densipan for a moment. In Venezuela we sampled one and took it to Maracaibo and dried it. We wanted to run tests on it to see if we could get some measures there. So we got the professor from the university to bring his penetrometer into my office and he studied the problem. We had a chunk that would've been 60 centimeters in diameter, something like that, and 15-20 centimeters thick. He looked at it. He had his little penetrometers. We went back to his office and he brought out a large penetrometer. About 3 feet long that you could almost stand on. So he applied pressure to that and he increased the pressure. Presently he broke the fragment and his penetrometer was bent. It did not give. We abandoned the penetrometer test because we had no more machines. We should have confined it.

Question 149

Collins:

I was just curious to know why, with the Alfisols, the base saturation is on sum of the bases. For a mollic epipedon the base saturation is determined by the ammonium acetate method. How was that decision made?

Guy Smith:

There were two reasons. One of which we didn't fully understand at the time but we knew that the difference existed. One was that we had regionalized our laboratories and in the eastern part of the U.S. where we had most of our Alfisols, the laboratory used the sum of cations to measure the base exchange capacity and base saturation. On the Great Plains where we had a lot of calcareous soils the laboratory at Lincoln used ammonium acetate extraction because the sum of cations doesn't work in the calcareous soils. Most of our data on the Mollisols were accumulated at the Lincoln lab where pH was measured and base saturation was measured by ammonium acetate at pH 7. Most of our data on Ultisols were from the Beltsville laboratory where these same measurements were made by the sum of cations. When we began to look at 35 percent or 50 percent or what have you, as a limit that would affect the classification of the series, we could not very well compare the two methods because we had only the sum of cations on the Ultisols and only ammonium acetate on the Mollisols and the Inceptisols. We had a few soils of which we had both. And one of those was the pedon I used in the *Seventh Approximation* as an example of an Ultisol. Now it just happened that that was quite rich in free oxides as well as kaolinite. It had a very considerable pH-dependent charge. So that it went as an Ultisol if we used sum of cations and it went as an Alfisol if we used ammonium acetate. Some of the best Ultisols were Red-Yellow Podzolic soils in the southeast at that moment. So without realizing what caused that pH-dependent charge at that moment we went ahead and said, well, this soil, a representative Red-Yellow Podzolic soil, is an Ultisol if we use sum of cations and 50 percent by ammonium acetate but where you have a large pH-dependent charge that breaks down and it just happens that that particular soil was one that had a large pH-dependent charge. That's how it happened.

Question 150

Holzhey:

You have the *Seventh Approximation* here?

Rust:

Yes.

Holzhey:

Sodium acetate. Don't you use the sodium acetate method? Didn't Riverside use it?

Peterson:

They did for quite a few years. There's been a lot of sodium acetate used in various places in the U.S.

Holzhey:

Is it about comparable to the sum of cations?

Peterson:

The sodium acetate? Well, it depends on the pH in which it is run, if you run it at pH 7 then it's closer to the ammonium acetate.

Holzhey:

But isn't it specified in your book as 8.2 in sodium acetate?

Peterson:

Sodium acetate? There is a procedure for the higher pH in our lab book but we haven't used it a great deal.

Holzhey:

You do have one listed in there?

Peterson:

There is one listed in there but it is one that was used years ago. We don't have a lot of data except some of the older data using that technique.

Guy Smith:

We didn't publish the data by both methods here. We only published the sum of cations.

Rust:

This is on the example of the Ultisol?

Guy Smith:

Yes.

Rust:

Profile 12 of the approximation?

Guy Smith:

This has an argillic horizon with a 5YR 5/6-5/8 color and it has the reticulate mottling below. These are things we don't really expect in the Ultisols as a general rule. However, the CEC per hundred grams of clay is a bit over 50 milliequivalents which would not be representative of most, or a great many at least, of our Ultisols. The clay mineralogy was not known at this moment but with that you certainly have a lot of 2:1 lattice clay as well as a lot of free iron.

Question 151

Rust:

Dr. Holzhey the recommended procedure nowadays is, for the CEC?

Holzhey:

Well, just the procedures that are specified in *Soil Taxonomy*, either the ammonium acetate or the sum of cations. I might ask if you have any comment on the philosophy of using procedures of this sort to classify soils as opposed to evaluating their performance. We hear a lot of discussion about kinds of cation exchange capacity measurements in order to get at the effective cation exchange capacity which would be at the pH of the soil and discussions about attempts to use CEC measurements and base saturation measurements closer to the effective cation exchange capacity as the soil occurs in the field. Of course, if you have different pHs and a high pH-dependent charge then you're not exactly comparing one thing to another. The comparison is more difficult than it would be if they were all run at one pH. Do you have any comments on the philosophy involved there for a taxonomic scheme?

Guy Smith:

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

Holzhey:

It is there but it turned out to be a little more difficult than we had thought so we aren't using it. In any case the procedure was developed out at Riverside and used for a while and then we stopped using it partly because the time requirement and partly because we weren't

getting what we thought it would do. Right now we don't have a standard procedure in our laboratory to get at the sum of exchangeable cations in the presence of carbonates.

Guy Smith:

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

Peterson:

If you use the dichromate method on soils with a high content of sodium chloride it breaks down. It doesn't break down, it explodes. With most wondrous crimson vapors boiling off. At the time I'm glad I didn't know chromium was bad. An old, old chemistry book that I'd kept described exactly what was happening. Don't try it on chloride-rich soils either. Quite interesting.

Rust:

Well, with that power of technique I presume we should close.

Question 152

Rust:

As they say in the baseball world, "We may be approaching something we call cleanup position", in looking through the list of questions which I had assembled. It seems to me that we have covered most of them, here or elsewhere, with a few exceptions that I am not too sure about. I've asked a couple of you to look at some of them and see if there are some things yet to be covered. We left off yesterday afternoon discussing some observations on the Spodosols that are possibly Alfisols. Are there any additional questions in the area of Alfisol observations?

Whiteside:

In the argillic horizon definition it's suggested that the ratio of fine to total clay, in the illuvial compared to the eluvial horizon, should be about 1/3 greater.

Guy Smith:

Or the underlying horizon.

Whiteside:

Yes, or the underlying horizon. It seems to me that this is too high where we are intergrading to Inceptisols. In other words, it seems it just may be more of a central definition than a borderline definition. Would you agree with that or is that not agreeable?

Guy Smith:

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

Whiteside:

In the recently completed SSIR 36, for Michigan, 35 pedons of soil series with argillic horizons (sampled in Michigan from 1968-1975 inclusive), were analyzed by U.S.D.A. 32 of these pedons were analyzed for fine clay and total clay! Of these over 1/4 had fine-clay/clay in the Bt/Ap or B3 or C ratio of only 1.1 instead of over 1.3! Only 14 (44%) had ratios of 1.3. The average of the maximum ratio in each of these 32 pedons was 1.38.

Question 153

Featon:

The Russians have been doing a considerable amount of work on the composition of organic matter, breaking it down into different fractions and so forth. In your experience do you think that in the U.S. we should be looking to that type or those types of analyses to, perhaps, refine our classification. Do you think these are important characteristics or should just the total amount of organic matter be the primary criterion?

Guy Smith:

They use the ratio between humic and fluvic acid as diagnostic criteria. We don't have a lot of data in the U.S. on this subject. You have to go to other countries. For example, I have to go Canada for a moment, where they took a soil, I think it was in Saskatchewan, and with fertilization over a period of couple of decades the ratio reversed itself. It's a very unstable thing, I believe, in the soil. That was the reason, after having looked at what data I could find, I found this reversal of the ratios as a result of cultivation under reasonable fertilization in contrast to the soil under the natural vegetation. It may be that it has a good deal of genetic

significance in uncultivated soils. But if we're going to keep the cultivated and the uncultivated equivalents together it's a difficult thing to use. ORSTOM, the French overseas soils people, commonly make that analysis. They find, between the Mollisols developed in ash and the Andepts, there's a very large difference. Some of the Mollisols in ash have almost a hundred percent humic acid and there's virtually none in the Andepts of Ecuador. This certainly reflects something that has been going on in those soils. The Mollisols are cultivated in Ecuador and have been for some hundreds of years and the Andepts mostly are left alone and grazed. But there's an enormous difference in this ratio there. You find this in publications of *Le Pedologie* and in ORSTOM's *Cahier de Pedologie*.

Question 154

Rieger:

In cultivated soils isn't it primarily humic acid rather than fluvic?

Guy Smith:

Yes, but it's also a difference between Andepts and Mollisols. I've never seen such Mollisols as they have in the ash in Ecuador where the clay is pure halloysite. Those soils have been cultivated by the Incas for an unknown length of time but without fertilization. I talked with one cultivator who was about to harvest his corn and I estimated that his yield would be about 40 bu/ac. I asked him what fertilizer he used and he said he had never used any. It strengthened my desire to keep the Mollisols together.

Question 155

Peterson:

Why do they not cultivate the Andepts in Ecuador?

Guy Smith:

They didn't get satisfactory yields.

Peterson:

It wasn't worth it?

Guy Smith:

It wasn't entirely a matter of soil temperature, the Andepts you can find at any elevation. But the yields are so low that they're rarely cultivated. Remember they don't have access to fertilizers.

Rust:

Guy, I believe at one time in the development of the mollic epipedon concept you had the notion of using the carbon-nitrogen ratio as a part of the definition but I believe that was abandoned.

Guy Smith:

Yes, as a general rule the C/N ratio in the Mollisols will be 12, 11, 10 something in that range but we kept finding the exceptions for reasons that are unknown to me, where the C/N ratio went up to 15 or 16, particularly in the Aquolls. And so we thought if we had to go that high it wouldn't make any particular distinction from other kinds of soil and we dropped that ratio. As I recall the very wide ones were always in an Aquoll.

Question 156

Rust:

The question was asked, and you probably have responded to it elsewhere, of the importance that you have given to the argillic horizon. It would appear to some that the argillic horizon is "weighted" higher than other diagnostic subhorizons. Is that a fair statement?

Guy Smith:

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

Rust:

This observation would appear to be a warning to the soil chemists that makes "hamburger" out of the soil before they analyze it.

Guy Smith:

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

Question 157

Hanson:

I have some arguments with my colleagues doing greenhouse experiments where they will compare soils in greenhouse pots. I haven't really seriously debated it but I was hoping to convince them that I didn't think they should call a mutilated sample of soil a given soil name. That should be distinguished from a soil because the definition of a soil is a natural body.

Guy Smith:

I don't think they should use the series name. They should perhaps say where they got it from, what soil. But it is not a soil in the sense that we are classifying soils. A soil has many meanings and ...

Hanson:

The temperature is different, the moisture regime is different, it's not in it's environment.

Guy Smith:

People have written me that if they told their wives there wasn't any soil in the pot where she was growing her plants that they'd be thrown out of the house.

Hanson:

We need some terminology that would distinguish the difference between a natural soil and a soil that's been transplanted. I don't know if there is any suggested terminology.

Guy Smith:

I don't know of any common word that one would substitute. Soil has a number of meanings in the English language. If you look it up in the Oxford Dictionary of the English language it takes about two to three pages.

Question 158

Rust:

You have spoken of the Alfisols and the observations of the clay skins in the lower part of the argillic I presume. This brings the point of the question that has been asked and I guess you've probably responded to this also -- of the rule or the differentiation based on the 35% base saturation distinguishing between Ultisols and Alfisols. Was this criteria a long time brewing, as we say, or how did it develop?

Guy Smith:

It was a long time brewing. From the early data that we had when we began this work, it was obvious that in the Gray-Brown Podzolic soils the base saturation increased with depth, or was 100%, whereas in the Ultisols, the base saturation decreases with depth in the soil. At one stage we tried to make the distinction on the base saturation of the argillic horizon relative to

the underlying horizon. The base saturation was low and it decreased with further depth. I think we had a limit at that time of 35% and, in the *Sixth Approximation*, the order that became Ultisols was defined as having a textural B with base saturation less than 35% or base saturation which decreases with depth from B to C. After this *Sixth Approximation* came out, I believe we kept much the same definitions in the *Seventh*. This stimulated some studies particularly in Maryland, Pennsylvania, New Jersey where it had been a practice since the settlers first came to the U.S. to apply small amounts of burned lime to soil once a rotation. We had these soils that were on the coastal plain, very old soils in a humid climate that had been limed for upwards of about three hundred years. If we sampled in the forest areas that had not been cleared we had extremely low base saturation but if we sampled in the fields that had long been cultivated and limed, base saturation was commonly about 60% through the argillic horizon. We still had the problem of whether or not this was a large enough change to recognize new series for the woodlots as distinct from those of the fields on the farms in this area. Most of the people felt that it was not warranted to change the series because one was a woodlot and the other was cultivated but it would be useful to keep the same series so that the experience the people had from the cultivated field could be extended into the woodlots. To keep these soils as Ultisols instead of Alfisols we had to modify the definition and we set the depth at which the base saturation should be under 35% at, I think, one meter or 1.8 meters. If we did this then we could keep the soils together in a series. We have a complication in that definition, that comes from the soils from basalt in the southeast where the base saturation hangs just above or just below 35% at one meter eight. So there's a very complicated definition that is in there just to keep a few soils from basalt in the same series. And it is admittedly not an easy thing to map when the base saturation at that depth is unpredictable. You know it is going to be in the neighborhood of 35% but it may be 30, it may be 40. This is not a wide range but the soils that cause this complicated definition on depth were minor in extent in the U.S. but important in some countries.

Question 159

Ruse:

Any other questions related to Alfisols?

Hall:

Just a comment and maybe you have a reflection. Commonly, in operation, the attempt is made to correlate pH with base saturation and in eastern Ohio this becomes very difficult and causes a lot of problems. We're trying to 'uneducate' them to the fact that this doesn't work. Was pH considered at any time in the development of Taxonomy rather than base saturation?

Guy Smith:

No, not that I know of. It was considered but it didn't get written into any definition except pH appears in the definitions of the Subaquepts. At the family level we have some pH limits for Histosols and so on. But otherwise we have kept pH out. The pH is quite a variable thing with respect to base saturation and it varies quite a bit from one place to another. It depends on when you take your sample what the pH is going to be. It can have half a unit, or occasionally even a unit, variability with the season. Too bad Rouse Farnham isn't here because some of the most careful studies have been on Histosols in Finland where they found the pH varying practically one unit seasonally. I think Michigan has some studies of this sort.

Whiteside:

Yes, usually we get 3 to 6 tenths seasonal variation.

Rieger:

Just as a comment. Quite a few Alfisols in cryic and even pergelic regions are not now acknowledged in Taxonomy that do need to be added when the time comes.

Guy Smith:

We had no report of any soils with argillic horizons and pergelic temperatures in the U.S. The Russians do have such soils.

Rieger:

Not only Boralfs but Cryaqualfs. Even Natriborells. Natraqualfs are also in the cold area.

Hall:

In your Ultisol definition you originally had color in there, didn't you? Wasn't there a six chroma in there at one time?

Guy Smith:

That shows up in the *Fourth Approximation*, 1955. It disappeared in the *Fifth Approximation* dated 1956. It was tested and after a year apparently was dropped. I do not recall the details of that.

Question 160

Rust:

Is there anything else regarding Alfisols? Shall we turn to another order? Are there any other historical concerns about the development of the Mollisol order?

Fenton:

In an earlier publication, specifically, *Prairie Soils of the Upper Mississippi Valley*, there is a statement that I'm interested in. It states that, in terms of A horizon thickness, included in the *Prairie Soils* would be soils that had six inches or more of A horizon. I was wondering if that includes soils that we would now call Mollic Hapludalfs. Was the original thinking that some, assuming now that *Prairie Soils* and *Mollisols* are roughly equivalent, of those soils that would have been included in the *Prairie Soils* should be excluded from the *Mollisols*?

Guy Smith:

The thinking at that time did not include soils having, shall we say, a lighter colored eluvial horizon above the argillic horizon, even though the plow layer of the soil was six or seven inches thick and was dark in color. These were not considered *Prairie Soils* at that time. In that paper we were considering the various soils that had been called *Prairie Soils* but we knew nothing about those in the western states or on the southern plains. So we specifically titled the paper to eliminate those *Prairie Soils* from the discussion. Our thinking at that time

was those were Gray-Brown Podzolic soils and could be distinguished from the Prairie Soils by the presence of what we then called an A2 horizon. And those I think have remained as Alfisols not as Mollisols.

Question 161

Fenton:

It's in some of those that have the morphology or some of the morphology of an A2 horizon, we'll say the platy structure, but the color may be dark enough to qualify for a mollic epipedon. They don't dry to 6 value. I was wondering if the presence of the A2 horizon should be a diagnostic criterion? There's a grey area between the so-called incipient A2 horizon or even a well developed A2 horizon with platy structure that's dark colored in the albic horizon. I suppose that's one of those borderline cases that couldn't be covered?

Guy Smith:

We didn't try to cover that. If the colors, dry and moist, are dark enough for a mollic epipedon, the distinction of the platy structure was not brought into *Taxonomy*. I had long discussions in Iowa about whether or not, say in the loess in northeastern Iowa, we could identify three or four series. The one without any forest influence, the one without any grass influence showing in the profile, then a prairie soil intergrading to a forest soil and the forest soil intergrading to the prairie soil. And the general feeling in Iowa was that we could only recognize one intergrade, not two. And having had those long discussions when we got into the business of writing *Soil Taxonomy* we did not provide for both intergrades, only for one, the forest soil that still shows a prairie influence.

Question 162

Rust:

We have, Guy, a subgroup in the Mollisols called the Vermiborolls in which there's recognition given to the action of the earthworm. Are there other soil orders where we need to consider where this kind of faunal activity needs to be a matter of consideration, or perhaps put it another way, at what point do we have to consider the earthworm or faunal activity?

Guy Smith:

There's been considerable discussion about this. I can give you an example from Europe, not from the U.S. There it is possible to maintain a soil under grass for some hundreds of years particularly in some of the Dutch orchards. And if you have a pit you find the odd remnant of a blocky ped of an argillic horizon that has not been chewed up by worms as yet but I thought at one time, I still think, we probably need a 'Vermiorthent'. Professor Tavernier in the Near East has pointed out to me in conversation that the many of the long-term irrigation soils are extremely wormy and that they need to be distinguished from the soils that have been irrigated for short periods and do not have the faunal activity. The irrigation in those soils is commonly

with somewhat muddy water. You get fine stratifications that would make the soil an Entisol where there is no worm activity but the worms destroy that within a matter of some hundreds of years at least. Now, I have not seen these soils, nor have I seen a description of them but they came up for discussion at the International Correlation Conference that was held in Syria and Lebanon last summer. The proceedings of that conference will probably have something to say about these soils. In New Zealand I strongly considered the definition of a Vermic epipedon. There the agriculture is almost entirely pastoral on most of the two island and the worms can multiply. They were introduced and they have multiplied under the permanent grass with high fertilization. They make a problem for us in that the epipedon is dark enough for a mollic epipedon, base saturation is high enough and the dark colors extend to the depth at which the worms spend the winter. This is just in the neighborhood of the 25 centimeters that's required for a mollic epipedon. So we get these soils with an epipedon that is mollic to 26 centimeters, 27 centimeters. On the other side of the pit it's 24 centimeters thick. It's just on the 25 centimeter limit and it's causing a problem in the application of *Soil Taxonomy* in New Zealand. It's entirely due to worm activity but an activity that terminates at about 25 centimeters whereas the Vermiborolls of the steppes of Russia show intense worm mixing to depths of at least 2 meters. That's the thickness of the mollic epipedon in these soils of the Russian steppes. Those were the ones that caused us to establish the vermic great groups of Borolls, Udolls and Ustolls. We have all three in Europe.

Question 163

Peterson:

Why do you think they have the intense earthworm activity in the Russian steppes and we don't see it here in our grasslands to that degree?

Guy Smith:

In the first place, they have another species of worm. This is the so-called rain-worm of Europe, which we do not have in the U.S. They have been introduced here now, but they were native there and as long as those soils remain under grass, there are enormous populations of earthworms in those soils. When they are cultivated, the population drops, but the evidence of their activity persists. That's *Lumbricus terrestris*.

Question 164

Rust:

On the other kinds of faunal activity, and I guess you've observed them as well as most of us, in the African continent, the termitariums are a common feature of the landscape. Do we reach a point where we have to consider this also in the same way?

Guy Smith:

The way the definition of the vermic groups is written the disturbance is due to animals but not necessarily to worms. If we begin to find significant numbers of soils that have been disturbed by other kinds of animals then we might consider changing the formative element in the name from one suggesting worm to something else. What it would be I would not know. We have a few soils in the U.S. where the disturbance has been due mostly to the prairie dog. I forget where I have seen these, I think Montana. But it was in the northwest somewhere where we have a loess over basalt and everything has been mixed by burrowing mammals down to the basalt. Fred, I don't know whether you have something like that in Utah or not.

Peterson:

In Washington state. I remember seeing pedon-sized spots that are mixed down to the basalt where the loess is shallow over the basalt, some 30 or 40 inches deep. Within that mixed material, the upper 12 inches is largely non-calcareous, even though there are chips of carbonate in it that were brought up from a calcic horizon. But the matrix is non-calcareous, whereas toward the bottom of these mixed spots the entire soil is calcareous because of the mixing upward of the carbonate. In this situation there is considerable mixing. I might add that at least in the Great Basin, the harvester ants seem to create bare spots on Haplargids and Durargids. These bare spots are like slick spots. Many have a harvester ant nest in the center of them. If you dig through the crusted epipedon, you're apt to find an abrupt textural boundary in the spot. Perhaps the harvester ant moves into these spots, prefers them because perhaps they have a drier nesting volume under this abrupt textural boundary; I've never satisfied myself which way it goes, whether the ant comes first or it picks the spot, but it effects the epipedon most, rather than mixing to depth over a large area.

Guy Smith:

We had one soil sampled in Venezuela, an Aridisol, where, for some reason, they sampled in an ant mound. This particular ant carries organic matter underground. The description mentions the presence of holes that are filled with organic materials to a considerable depth. The pH of that soil was about 3.6. I went to look at the soil to see what was going on, but I couldn't recover the exact site although I could get close to it. I couldn't find the ant mound that they had sampled. Conductivity of the saturation extract was somewhere around 12 to 15. When we looked at the anions and cations that we normally determine, they wouldn't balance, so I asked the laboratory to dig the sample out and run nitrates. It had large amounts of nitrates in the saturation extract of the soil in that mound. Now that would be a significant difference, I suspect, but you'd only have one or two pedons of that. But only by accident did I find that this situation existed, because when I sampled a transect that I thought should cross the point where they had taken this sample, I couldn't find anything remotely resembling the nitrate contents. But 3.6 is not an uncommon pH for Aridisols in Venezuela. It's not necessarily due to the nitrates. It's due to the aluminum in the saturation extract.

Question 165Peterson:

Are those areas that were previously under a wet climate that formed highly weathered saprolite and are now dry?

Guy Smith:

I could not answer that. These are not Argids; they're Orthids.

Question 166

Rust:

Having considered the ant at length let us recess for a few moments...Dr. Fenton, you had another question on Mollisols?

Fenton:

For historical perspective if we put together in a working hypothesis what we think is known concerning past vegetational changes, landscape evolution and so forth, it would appear that, on our loess-derived soils in Iowa, you could build a case that some of the soils on the stable upland positions are really polygenetic in terms of their vegetative history; whereas in terms of landscape evolution the associated soils on slopes are younger and probably formed under only prairie vegetation. Based on the criteria of the mollic epipedon these soils are all classified the same way. If this hypothesis should prove to be true and there were other lines of evidences to support it, would you think that these differences should be reflected in their classification?

Guy Smith:

I don't have any firm opinion on that. We discussed that as early as 1930 in Illinois, the differences between the Tama in one part of Illinois versus another. We have the same differences in Iowa, in some of the Tama, the argillic horizons shows very distinct skeleton and, in other kinds of Tama, in other areas, do not. We began to discuss this at least in 1930 in Illinois. The work of Ruhe and Walker on the vegetative sequence in Iowa would suggest that at least some of the Tama at one time had a forest vegetation and these skeletons, the argillic horizons, the skeleton may date from that time. This was a boreal forest and the skeletons are much more distinct in the Boralfs now than in the Udalfs. So far as I can see, there is this genetic difference within the Tama series in both Illinois and Iowa. We never could make any different interpretations for one kind of Tama than we made for the other and while we have discussed both in Illinois and Iowa about the wisdom of making the separation nobody has ever seriously proposed that they separate them in mapping.

Question 167

Rust:

Then we will go backwards, alphabetically, from Mollisols to Histosols for some additional concerns.

Tarnocai:

We found in Canada that two control sections 130 and 160 cm, were not very useful and also complicated the classification. In the mid-70's we changed that and are now using only one control section, 160 cm; zero to forty cm, surface tier; 40 to 120 cm middle tier, and etc. What is your reaction to using only one control section for the classification of organic soils?

Guy Smith:

I have no distinct reaction for or against. The two control sections were provided on a theoretical ground, the whole classification that was proposed for Histosols was a theoretical one that we could not test in the U.S. because of lack of defined series. The theoretical basis, as I recall, was that, if we had a very low bulk density material before drainage, it would have about the same control section that the higher bulk density organic materials would have after drainage. Now if it isn't being drained, certainly it is not useful. But this was only a theoretical consideration and if it doesn't work in practice it surely should be abandoned.

Tarnocai:

My other question is also related to Histosols specifically to the use of the term "freely drained", in relation to the description of Folist. We use this term, too, and we have difficulty in defining what 'freely drained' means. Could you suggest a definition for freely drained?

Guy Smith:

It would have to be in terms of the absence of groundwater for certain periods either the year-round or so many months a year. I can not suggest what sort of limits you should use. The concept comes from the soils that we have on the island of Hawaii where we have a forest growing on lava and a litter which falls down the cracks between the blocks of the lava. On these soils there is never any groundwater, but if it doesn't rain today it's a drought.

Tarnocai:

Yes, basically, this is the situation that we are looking at when these soils occur in a high rainfall area, let's say rainfall precipitation is a hundred inches or sometimes more in the Pacific coast and I think, Alaska too. After a rain these soils are saturated, but if you have a rain-free period for a few days, they are freely drained.

Guy Smith:

They would fit our concept of well drained soils.

Tarnocai:

Well drained soils, but there is a period of time, I think, when they are saturated. This is a little bit confusing, when it is compared to definition of well drained. This is where we are having problems.

Guy Smith:

How long are these saturated periods?

Tarnocai:

Well, if you have a wet period of a few days they are saturated for a week or so, or if you have a longer one they are saturated for a longer period. It depends on how long the rainy period lasts.

Guy Smith:

Does the water flow into a bore hole?

Tarnocai:

Oh, yes, it just pours out, almost like a heavy groundwater discharge.

Guy Smith:

It's moving?

Tarnocai:

It's moving, yes. These soils are situated mainly on the slope position in Coastal British Columbia, Vancouver Island and the Queen Charlotte Islands and I think, they are also found in Alaska.

Guy Smith:

These moving groundwaters in general seem to carry oxygen. Where I've seen the soil with moving groundwater there was no evidence of mottling or reduction of iron, segregation. Sometimes there has been evidence of removal of iron from the soil, but not reduction and segregation. This was built into the definition of saturation with water.

Tarnocai:

It's on page 217.

Guy Smith:

This doesn't speak of free drainage. It's in the discussion. These are the more or less freely drained Histosols. But then the definition says they're never saturated with water for more than a few days following heavy rains and etc. That's page 217 where it starts. That's not the definition. That's the general concept. The definition is on the next page, 218 at the top, where we don't use the term. Saturated with water is discussed under the aquic moisture regime, page 54. Perhaps it could have been written better by saying we do not have an aquic moisture regime instead of not saturated with water.

Rieger:

Where we find these soils in southeastern Alaska, we don't have much trouble with them because the soil really consists of the same coarse litter that you find over the loess, except in this case they are directly over bedrock or in another case, over fragmental material. We had the usual fragmental sequence. When you get up above the forest, above treeline, you find organic soils again shallow over bedrock which are constantly saturated. Those would be the Histosols. We never had any difficulty with these we call the Folists. After the rain they dry out. There are some other areas further north along the Aleutian Islands where it's Folist or lithic Histosols.

Guy Smith:

We need considerable further discussion on classification of some of these soils of yours where you have quite a thick O horizon over a minimal soil which may be a Spodosol or Andisol or what have you. Virtually all the rooting is in the O horizon and these are considered mineral soils. Should they be? This needs discussion on the part of the people who know something about these soils. It's not outside of my experience. I've seen such soils in the Alps in Europe but to just see one pit does not suggest how we should classify them. So I think that when and if we have a committee to discuss the organization, re-organization of the Histosols

classification that they should consider this particular problem also. The definition of the Histosol.

Rieger:

Off the top of my head I strongly favor continuing to consider these mineral soils as long as there is a developed soil below this thick O horizon. They're really no different than other soils except that the O horizon is thicker and the rooting is shallower.

Tarnocai:

Relating to your question, we did that same thing. The criticism we received from the forestry people is that most of the rooting zone is in the O horizon and that's where most of the nutrients come from. The management of the forest is the management of the O horizon of the mineral soil. They said we can not look at these as mineral soils because they don't behave like mineral soils. The vegetation that they support is supported by the O horizon, not the mineral soil. This created the problem. This is why we are reviewing the whole Organic order in the Canadian Soil Classification. On the Pacific coast most of these soils produce high quality timber so it's not just a marginal type of timber growth on these soils.

Guy Smith:

These are soils where there is virtually no hazard of fire burning off the O horizon?

Rieger:

There are some fires occasionally, rarely.

Guy Smith:

Burn in the O?

Tarnocai:

As a student, I was a forest fighter. One of these areas in the Rivers Inlet, British Columbia burned down to the bedrock. After the burn, after the fire we went back to collect some of our equipment that we had left along the creek. All of the roots were up and sitting like tripods on the bedrock. We were walking between these roots and the trunks were way up above us, about a meter or a meter and a half, which was the depth of the O horizon.

Rieger:

In that area the drainage is such that if you have no rain for two or three weeks the fire hazard becomes almost extreme.

Question 168

Tarnocai:

I have another question relating to this problem. We have also found in these areas that peat material (wet organic material derived from wetland vegetation) and Folist material (which is a forest litter) could occur in the same soil profile. Now, we have these two materials which are morphologically very similar. It's a moderately to well decomposed organic material -- the

botanical component is not readily visible -- and that creates two problems for us. One of them is to identify and separate these two materials for mapping purposes, and the second is to classify such a soil which is composed of two contrasting organic materials, one being forest litter (produced by forest vegetation and a freely drained condition) and the other being peat (a poorly drained organic material produced by wetland vegetation). I wonder if you have any suggestion as to how to separate these two genetically different organic materials? And secondly, how would you classify such a soil?

Guy Smith:

First, I have no suggestions on how to separate them. You must know how to do it or you wouldn't recognize that they did.

Tarnocai:

We are looking for answers. We don't know them yet.

Guy Smith:

And for me to make a statement on how to classify these soils would be very rash. My knowledge in that is deficient.

Tarnocai:

This is a kind of an intergrade between the wet organic, wet Histosols and the Folist. I think Ugolini just published a paper from Alaska, just recently.

Question 169

Rust:

You have no similar situations, Dr. Reiger, quite like that?

Rieger:

No, I just can't bring any to mind. Now this is, I didn't quite get it straight, an O horizon, forest litter over peat substratum?

Tarnocai:

Yes. What really is happening is that we have a wetland and then, for some reason, forest invades this. So we have peat and the forest comes over. Of course, the wetland situation stops. Then you have an upland forest, mainly hemlock and red cedar, a heavy growth about 110 feet tall and several feet in diameter. We are talking about heavy timber. This situation produces litter which is a Folist. What Ugolini described in southern Alaska is just the opposite. You have a Folist developing first and then some kind of a natural drainage change. We have both situations. That's how the two materials arise.

Guy Smith:

The division for contrasting materials in the Histosol classification does not take care of that.

Tarnocai:

We have the same problem in the lower level of the classifications.

Rieger:

We have had a situation where a peat bog becomes drained naturally, the forest invades and then you do have basically an organic soil supporting the forest. I think the classification can handle that one. The wet substratum and the well drained overburden.

Rust:

Maybe we have to pass from wet to dry. Dr. Peterson?

Question 170

Peterson:

Guy, I'd like to go back to a question in Dr. Rust's list. I want to rephrase it. Question number 28 asks why the aridic soil moisture regime is introduced at the great group level in the Entisols whereas it is an order-level criteria for the Aridisols. I think you answered that earlier when you were describing how you chose levels of generalization. You chose to introduce the soil moisture regimes in the Entisols down one step from the usual suborder level because you had used the suborder level for other features. I'd like to rephrase that question in another way and ask you what was the background thinking for separating aridic soils with pedogenic horizons from those without pedogenic horizons? Earlier thinking seems to group everything that is dry together, regardless of the features of the profile. As the author of this question 28 says, Taxonomy produces a great group sitting next to an order in the same landscape and with a common boundary.

Guy Smith:

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

have to say that one may question the logic of all this, but the Taxonomy evolved slowly and some of the ideas from some of the earlier approximations carried over, presumably because no one criticized them.

Peterson:

I want to say that I am not criticizing it. Rather, it seems to me that this particular question of how the Entisols are treated in aridic climates is another reflection of people thinking about the Taxonomy as if it were a *key* rather than a hierarchy, and that diagnostics should appear at only one categorical level rather than having the possibility of appearing at various categorical levels. For example, the first time I saw any of the new taxonomy was at the *Third Approximation*. I tried to lay out what the diagnostics were for each category, and I found that they were jumping around. I went to Henry Smith and I told him this was an absolutely inane way to do it! I was upset because it was not constructed in nice key-like form. I thought, at that time, that a diagnostic should be used at only one categorical level, and that it should apply to every class at that level, even if at degree 0. I was wrong at that time. I wonder if some people are not still upset by the Taxonomy not working like a simple key.

Guy Smith:

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

Question 171

Peterson:

I'd like to ask you an accessory -- back to general philosophy for a while. When we are teaching the logic of classification, would it be fair to say that when attempting to define the different categories of a hierarchy, you can't effectively define them by telling what the diagnostics are, rather you have to tell what the purpose of the category is to distinguish between categories? I believe you've done that in the *Soil Taxonomy*. Approached it in that fashion.

Guy Smith:

We have tried to. Yes.

Peterson:

That would emphasize again that when we are trying to teach what classification is, we should emphasize the difference between the keys and hierarchies.

Guy Smith:

When you come to the logic of the classification, I think that there is one overriding principle of logic. If you follow it, you're going to avoid this business of using a given characteristic at only one categorical level and using it throughout the system at that categorical level. That overriding principle has been well stated by John Stuart Mill and quotations from him are in *Soil Taxonomy*. What we're trying to do is to organize our knowledge and develop classes of the objects about which we can make the greatest number of the most important statements. According to the purpose of our particular discipline we can have several classifications of the same objects. They can all be equally good. Those who wish to stick to what seems to me to be an illogical principle of logic, can make their own classification.

Question 172

Peterson:

I'd like to ask another question. I wonder if you would give an historical outline of how the concept of the duripan developed? You said before, that in Shanks classification of 1927, there was no recognition of the duripan, or at least a very vague recognition. He called the duripan an iron pan, which implies to me an iron-cemented pan. This "iron pan" term in the old literature, for the duripan bothers me, because it was used for what we now recognize to be an opal-cemented pan.

Guy Smith:

I think that Professor Shaw's experience was largely restricted to the soils of California and his classification was intended for them, not for a more general system of soils of the U.S. or any larger area than California. In California the duripans do contain appreciable amounts of iron, if one judges by the color, as well as opal. In some of them, at least, there are pretty well preserved clay skins with oriented clays that have been impregnated with silica. In the arid regions the accumulation of silica generally goes along with the accumulation of lime rather than of iron. Shaw, at his family level, distinguishes soils according to the kinds of root inhibiting layers: Clay pans and iron pans. The latter, I think, are included in the present duripan. Shaw's lime-iron pans may refer to the duripans, say, of Nevada. I do not know any lime-iron pans. What Shaw would have done with some of the duripans, such as those in the Durorthids and the Durargids, I do not know. but his principle of separating soils according to the kind of pan is consistent with what we have done in *Soil Taxonomy*. We, in our committee on Planisols, in attempting to reorganize and improve the 1938 classification, recognized the different kinds of pans also as different kinds of Planisols, one of which was the Noncalcic Brown soils which had the hardpan. It was distinctly different from the soils with fragipans of the mid-west and the eastern states or the soils with clay pans from the Midwestern states. We first called the duripan a silica pan or hardpan. But it's not necessarily the only kind of hardpan. We finally changed it to duripan using the Australian terminology for the same kind of horizon. In examining the arid soils, with very prominent hardpans particularly in Nevada,

we found some of the duripans are partly cemented with carbonates and grade to the petrocalcic horizon, and some have relatively small amounts of carbonates compared with the silica. We broadened our definition, or concept, of the duripan and in the discussion in *Taxonomy*, we point out that duripans have different appearances in different environments. The duripan under the Alfisols tends to consist of very large polyhedrons with silica coatings on the sides and, in some, across the tops of the polyhedrons and in others not. I guess in the U.S. we have no duripans in Ustalfs. They do occur in other countries but I think in the U.S. probably not. They are not known to occur in the U.S. according to the *Soil Taxonomy*. They do occur in the West Indies; they do occur in New Zealand. In the West Indies the Durustalf pan looks like the Durixeralf pan of California. In New Zealand it is more clayey, consisting of huge polyhedrons. It apparently can have either appearance in ustic regimes. The concept then varied with our knowledge of the moment and if anyone is studying it now I'm really unaware of it.

Question 173

Peterson:

Another question. Sometime in the past, I remember you discussing the concepts behind the definition of the calcic horizon and, particularly, the part of the definition that requires a total carbonate content of 15% vs. a 5% pedogenic carbonate content. If you hadn't already put that into the record of these meetings...

Guy Smith:

I have already.

Peterson:

Then we don't need to go into that. Did you consider the duric subgroups as being analogous, that is, a soil that fits into the duric subgroups as having an opalized horizon analogous to the calcic horizons? In other words, if you look from the petrocalcic down to the calcic and look at the duripan down to something less, was that in terms of an analogous horizon of opal accumulation?

Guy Smith:

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

Peterson:

But we don't have the calcic horizon *per se* at the time calcium carbonate accumulation has reached a level comparable to durinodes. A calcic horizon is quite a prominent horizon and when opal accumulation has reached, you might say, comparable levels, we have duripans. There really isn't a direct analogy between the two sequences of cementation.

Guy Smith:

It's not a good one, no.

Question 174

Peterson:

I did have an accessory question. As I remember there was a considerable reticence to recognize the petrocalcic as a pedogenic horizon. Did you find similar reticence for the duripan?

Guy Smith:

I do not recall any. There is still reticence to accept the petrocalcic horizon. Particularly in North Africa amongst the ORSTOM people.

Question 175

Collins:

I don't want to change the subject but I guess I will. I know how the new classification system developed after WWII (as far as what was written about it). I want to know what really happened behind closed doors. What discussions took place. Second question I would like to ask, is, was there anyone who really influenced you as far as your thoughts in soil science, what effect did that person have on you?

Guy Smith:

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

Collins:

You were influenced by John Stuart Mill's logic? That must have had an effect on you.

Guy Smith:

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

Question 176Collins:

Referring to the other question. Maybe you were in the field one day and you were talking and decided to start over again or was it just something that naturally happened? Did everyone suddenly come up with the idea or was it just one person?

Guy Smith:

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

Question 177Cooper:

I worked in California for a while and one of the things that puzzled me was that on the coastal mountains halfway between LA and San Francisco we had two soils that were being

mapped as a complex because they could not be separated, one was a Chromoxerert and the other one was a Argixeralf, a dark colored surface soil with an argillic horizon in the clay. It was a clayey montmorillonitic Argixeralf. These two soils were in close approximation, mainly on slopes of 5-15%. You could actually sometimes feel yourself stepping across the boundary and you knew that if you dug there you could find the argillic, if you dug here you wouldn't find it and you'd find cracking to the parent material. I never could figure out why we developed an argillic in one and a Vertisol in the other one. Do you have any speculation?

Guy Smith:

No, I'm afraid I wouldn't want to speculate. I've never seen those soils. It's not uncommon among Vertisols, where the cracking pattern is large, that, in the centers of the big polyhedrons, you'll find an argillic horizon. That's quite common in Australia. And to keep those all together we require the surface 18 centimeters to be mixed to ensure we had 30% clay because these albic horizons that get perched above the argillic horizon are normally quite thin. Once you plowed you would be hard put to be sure that they had ever been there.

Question 178

Cooper:

Do you think that Vertisols, in some cases, have developed from, say, soils that had argillics that then swallowed the surface?

Guy Smith:

That's the theory that the correlators were told. They set up a subgroup of Vertisols because they thought those soils started out as Paleustolls or became Paleustolls first before enough clay had been formed by weathering to cause the churning process to start. In the lower part of the soil you will find a clay skin and so on that suggest it was a very fine-textured argillic horizon at one time.

Question 179

Grigal:

It probably has been discussed sometime this week but I'm concerned about Spodosols, their presence or absence. I've been to a few spots in upstate New York and Michigan where they're deemed to be present and I've been to the very spots in Minnesota where they're deemed to be absent. To me it looks like I'm standing in the same place. The soils look very similar, morphologically, at least. Apparently the Minnesota soils don't make it chemically and the Michigan and New York ones, whenever I inquire, haven't been tested. They are still Orthods. I talked with the fellow who was in North Carolina from the Lincoln lab, he worked with Daniels, the geomorphologist, Erling Gamble. He said, in the context of North Carolina and the coastal Spodosols, we just aren't even approaching them up here. He thought it'd be a travesty

to have Spodosols in the north compared to those in the coast. I think it's sort of a travesty not to have Spodosols in Minnesota, at least somewhere.

Guy Smith:

If you have some.

Grigal:

We haven't been able to find any, have we Dick?

Rust:

They are little to find.

Grigal:

To meet all the criteria.

Guy Smith:

You must recall that the identification of a spodic horizon can be chemical and it can be morphologic, something you can identify in the field. We had a lot of trouble in drawing a boundary between the Spodosols and the Dystrichrepts of New York state. We asked Professor Cline to identify the soils in the field as a Spodosol or as a Dystrichrept and we sampled them. From these samples we worked out a proposed chemical definition of the weakly developed Spodosols. These are the Spodosols that intergrade to the Dystrichrepts. They're not really the most representative of the Spodosols of the world. We sent our proposed definition to the Canadians to be criticized and the people who worked there in the laboratory objected to the definition on the ground that we gave too much emphasis to field identification. The field people objected on the grounds that we gave too much emphasis to the chemical properties. That was, I thought, about the best we could get at the stage of our knowledge at that moment. Many of the most strongly developed Spodosols will not meet the chemical requirements. They don't worry me because when they're that strongly developed you don't need the laboratory analysis to identify them. I thought we might well get along without creating a big demand for laboratory work. There's no argument about some of the Spodosols in the Carolinas and Florida. These are mostly Aquods, when they get that far south. You do not find any laboratory data on them. But I have seen Spodosols in Minnesota. I have a photograph of one. Maybe in my notes, if I get home, maybe I'll have the location close enough that you can find one.

Bruns:

These soils have all the appearances of the Spodosols. They are often bisequal with the darker colors but they don't meet the chemical requirements.

Grigal:

So, in that case, don't pay any attention to the chemistry.

Rieger:

If it makes you feel better, people in Quebec have been complaining that their Spodosols don't meet the requirements.

Grigal:

That's what I say, traveling in New York and Michigan, to me, morphologically, it looks like the same soil but over there they are Spodosols because somebody has deemed it but here they are not.

Whiteside:

Michigan actually is very concerned about this. While we're still calling ours Spodosols, they don't meet the chemical requirements. I think there needs to be some changes.

Grigal:

That's my point.

Guy Smith:

May I call your attention to plate 7B which is a bisequum and which is from Minnesota, a Spodosol, and I don't think we have a chemical analysis on this one.

Rust:

Unless it was sampled this last year or two.

Cooper:

It would appear that a soil in the field was being mapped as a Spodosol because three or four soil scientists are agreeing that this is a Spodosol according to the definitions as defined without the chemical lab data. They're going through a whole mapping process because of land use and vegetation and characteristics that that soil has. Then all of a sudden the lab data is taken and it's going to kick it out of that particular classification.

Guy Smith:

It won't if the identification has been made in the field because the definition is written deliberately so that it can be identified in the field. We knew that a good many spodic horizons wouldn't have that particular set of chemical requirements. That is only valid for the Spodosol-Dystrachrept boundary.

Question 180

Whiteside:

On this particular illustration (Plate 7B, p. 103) I don't think that pedon will make it. Because when you plow there's not going to be a spodic horizon remaining.

Guy Smith:

I think that goes below 25 centimeters. I'll have to have a look at the photograph. There will be some left, I think, after you plow 18 centimeters. Well, I have a bisequum from Maine that I could use, too, as an illustration but again we have no laboratory analysis. The identification was made in the field.

Question 181

Hall:

This discussion brings up a whole area, Guy, that after five days, we haven't even touched on and that is the cambic diagnostic horizon and the Inceptisols. I don't think there's been a word said about those. I think in teaching this we find that we have a tendency to teach the Inceptisols and the cambic by exclusion. If it doesn't fit anywhere else, we'll let it fall into the Inceptisols. I wonder if you can give us a little bit of a background on this thinking. I know that you and Dr. Cline have said that all classifications have this kind of a catch-all category, but I would like to have you talk a little bit about the thinking that went into it. Were there any other diagnostic horizons that you tried to come up with to keep this from being such a wide ranging order?

Guy Smith:

The Inceptisol order is the wastebasket for certain. We have the concept from Europe of the B horizon. It was the only sort of B we had in the soils we now call Dystrachrepts. There was no accumulation of anything, it was purely a subsurface horizon that had been altered by weathering and by soil-forming processes, that is, mixing by roots and by animals to destroy the original rock structure. The very extensive soils in western Europe in the higher altitudes, such as the Black Forest, the Ardennes, the Central Massif in France. At one time, as the concept of diagnostic horizons was forming, we were talking about podzol B's and textural B's; which is our concept now of the cambic horizon. We tried in the various approximations to group these with the various other soils that had spodic horizons or argillic horizons, mostly. No one was ever happy with the groupings of series that resulted. They always objected to the inclusion of these soils in what's now the Alfisols and Ultisols. Originally, the cambic was defined primarily on color. We got into troubles with that because in some of the western European sands we had a distinct color difference in the sand in the position where we would normally look for a B horizon. Yet, when we made a laboratory analysis of these color B's in the sands you couldn't find a thing. Presumably it was some sort of translocated humus from the cultivation that had been practiced on the sands. So we excluded the sands from the cambic horizon on the ground that so little alteration is necessary to produce a color change. Dr. Simonson said it doesn't take much paint to make a barn red. And in this case it doesn't take much to color a sand grain. Having tried various combinations of the soils with cambic horizons and soils with other kinds of B horizons, the argillic in particular, and having had nothing but objections to these trials, we tried to group the soils with argillic horizons according to their base status and soils with spodic horizons and oxic horizons and then we had some soils left over. This was the original Brown Forest soil concept actually but some with high base status and some with very low base status. These being left over, after we had all our other orders defined, we threw together into the Inceptisols. We put too much in the Inceptisols in that we should have recognized a separate order for the Andepts. Those are young soils. I can not find one where the ash is dated as much as 20,000 years ago. Mostly the ash is dated considerably less than 20,000 years. Now when we get an ash that's dated 20,000 or more we're more apt to find there a soil with an argillic horizon. So they come out as Alfisols and Ultisols and Mollisols and Spodosols and what have you. So I have a lot of trouble with the cambic horizon in some of the wetter soils and in the supplement, I think, of 1964, we had a Fluventic Haplaquept. This was criticized primarily by the Dutch on the grounds that if they had, say, a silty parent material they would find the fine stratification in the soil. That kept it as an Entisol but, in the slack water deposits that have a clayey texture, the deposits did not originally show the fine stratification. They were absent and it took very little time after deposition before the soil could be considered to have a cambic horizon by our definition because it had soil structure. So we eliminated that subgroup by requiring that there be enough evidence of alteration in the cambic horizon to reduce the inherited organic matter to a low level. This then in turn was criticized by people in New Zealand and in Venezuela and other places on the grounds that if they had a well or moderately well drained soil, it would have a cambic horizon but the wet soil that was associated with the Fluventic Dystrachrepts, for example, would come out as an Entisol. Everyone objects to the mixing of orders in the same landscape and in parent materials of the same age. So I did

propose that we remove the limitation on organic carbon in the cambic horizon of the Aquepts and substitute for it, the presence of a significant amount of iron manganese concretions that were hard enough to withstand a normal dispersion process for mechanical analysis. This we tested in New Zealand and it has been tested now in other places. It seems as though it might work. It hadn't been approved and I don't know whether any tests have been made in the U.S. on this proposal. Probably not because I doubt that anyone but Dr. McClelland saw this proposal. But it would reestablish the Fluventic Haplaquepts if it were adopted. It would then put a Fluventic Haplaquept and a Fluventic Dystrochrept as association in one landscape on deposits of one geologic age.

Question 182

Rust:

In the development of the cambic horizon concept was there at any time a notion that it was an eluvial rather than illuvial kind of horizon?

Guy Smith:

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

Question 183

Rust:

Any other questions related to this concept of cambic horizon?

Cooper:

In coarser granitic material either from the parent material or that has been washed in on fans or terraces it seems that we can go from a soil that's an Entisol where we have no visible structure, no clay films, to a soil that has a few clay films. Then we go to a soil that has more clay films and has the 1.2 times more clay. All textures will be sandy loams and we can find a classification of three soils: Entisols, Inceptisols, and Alfisols, and mainly Xeralfs, again in California. The sequence doesn't seem to have any relationship. We come back and we wait for

the lab data to see if in fact there is 1.2 times more clay to separate the Alfisol from the Inceptisol. Do you see any problems with that kind of situation? Again, waiting for lab data to determine that?

Guy Smith:

We took that 1.2 ratio because we thought that was representing a large enough difference that the field man should be able to identify it consistently. That's where we got the ratio. When there is very little clay we took the 3% increase because we felt that could be identified in the field and the intent was that that would be a large enough difference that you wouldn't have to wait for the laboratory data. Admittedly the laboratory might come back with a 1.16 ratio. Round that, and you get 1.2. But these ratios seem to be taken as sacred. You must always remember that there are two sources of error and you must consider the magnitude of that error in making a decision. The one is in the laboratory and the laboratory people know pretty well what this amounts to because they can and have run duplicate samples a number of times. They know the variability that they get. What they don't realize is that there's also a sampling error. And you may not pick the best sample for them to study. They assume you did. When I was at CSC we always tried to have someone from the laboratory present if there were a major study involved but we permitted the field man to send in samples for dual analysis. In this case, you might ask comparison between two samples, A and the B. We do know that, in the studies we've made, where we have a laboratory man present that the sampling error is appreciable. Two samples from the same pit may differ by 3 or 4% carbon. In sampling Aridisols where the ratio of carbon is varying with respect to the sand/clay ratio, we've collected a number of satellite samples to find out something about the variability of organic carbon within short distances. It is very large. A difference of .1 in the pit against .3 or .4 on a composite sample collected at a distance of about 5 meters from that pit in a circle around it. If you relied exclusively on the sample that came from the pit you'd be neglecting the probability of a sampling error. It's quite common in the Aridisols, where much of the surface is exposed, that you will get under the plants very different conductivity, very different sodium adsorption ratios, than you get in the bare ground between the plants. It is a tendency of people to avoid sampling under the plants. It's more work to dig there and to sample there than it is on that beautiful bare ground between.

Question 184

Grigal:

Rattlesnakes will also hide there. It seems to me from just the little time I've been here it's my perception, that the 'lab tail' is starting to wag the 'dog' in terms of the morphology. In Taxonomy because the lab criteria are nice .1 or 3.5 values it becomes a very simple decision. Or it doesn't require a decision. You can run something in a lab. If it makes 3.5 or 1.2 you can say yes or no. So the path of least resistance is to use the lab data and forget about the morphological background of many of the criteria, the variability in the lab, the variability in the field. My perception is that we are setting up these criteria in discussion of the Spodosol. We talked about it a bit in some of these other discussions. The lab criteria were set up to reflect morphology but now we are finding out that we're using the lab in spite of the morphology.

Guy Smith:

Well, I think it's perhaps a normal tendency, one that should be resisted. It surely is characteristic that the laboratory men have full confidence in the field men. And the field men

have full confidence in the laboratory results and believe each other but one field man doesn't necessarily believe another, he knows the potential for error.

Grigal:

Accountants begin to run businesses after a while. When you get an 'accountant' in I wonder if the same thing is happening with Taxonomy. We have too many accountants that are looking at the third decimal point and they're failing to look at the whole system.

Question 185

Rieger:

I can understand the reasoning with respect to the brown sandy horizons that are excluded from cambic horizons. But this also extends to the soils with obvious aquic moisture regimes, strongly mottled sandy soils that must be classified as Aquents rather than Aquepts because of texture. Is that deliberate or does it just go along because of the exclusion of sands?

Guy Smith:

The wet sands?

Rieger:

Wet sands. Wet mottled sands.

Guy Smith:

The proposal I made to modify the cambic horizon definition only dealt with the loamy and finer-textured soils. It didn't concern itself with getting the sand in. We exclude the well-drained sands and so in a sandy-alluvial deposit they would all be Entisols. Whether they are wet or well drained.

Rieger:

The problem is not necessarily restricted to alluvial deposits. There are wet sands in other situations, too.

Rust:

Is your question, Dr. Rieger, one of how to decide to limit the cambic discussion to only sands and finer or why was there a line drawn in the textural grouping?

Rieger:

What's happened is that soils with brown sandy B horizons can not have cambic horizons. They can not be in the Inceptisols, in other words, because they are sandy. This same concept is extended to the wet sandy horizon. I just wondered if this was something that just tagged along behind that other decision or if it was deliberate?

Guy Smith:

It just tags along. But it does not need much alteration to produce mottles in the soil. I have gone out on the Missouri floodplains and when the water had just run out, here was this year's alluvium and it was already mottled. That was a matter of a few days. Of course, it could have gotten its mottles when it was still under water but when the water withdrew and we went out on it, the mottles were already there.

Rieger:

You've seen them then in fresh floodwash?

Guy Smith:

Put that into Inceptisols.

Question 186

Okusami:

Should mottling take precedence over, say, chroma of 2 in classification of aquic?

Guy Smith:

In some situations, yes. The sediments I looked at on the Missouri floodplain had 2 chroma and mottles after the floodwaters withdrew. I think the color of the deposit naturally had the 2 chroma. It will, I'm sure, by now have a 3 chroma or more. Once they've had a chance to be really oxidized.

Question 187

Okusami:

In wetland soils mottles seem to be predominant and chroma is most of the time above 3 but they are wet soils.

Guy Smith:

I think I know what you are talking about. I ran into this in the West Indies and Venezuela. In the Ultisols we do not require a chroma of 2 or less for the soil to be classified as an Aquult. We accept low chromas as evidence of wetness but we also accept a hue of 2.5Y or 5Y as evidence of wetness. In the intertropical regions I ran into this over and over again, very wet soil that had a 2.5Y hue and had prominent mottles. In every order in which I found these wet soils. I did propose then that we modify our evidence of wetness in the intertropical regions by adding to Alfisols, Mollisols, Oxisols, and Inceptisols, the same status that we have now for Ultisols. So that a mottled horizon with a 2.5Y hue and a chroma of 4 or 6 would be

considered to have evidence of wetness. It must be mottled, of course, before you can accept the hue as indicative of anything because there are plenty of sediments that start out with a 5Y hue and as they weather they may get a redder hue. There's plenty of Mollisols around here with a 2.5Y hue, too, but without the mottles.

Question 188

Okusami:

We started on tropical soils. Do you think of any modification with regard to classification? Take the oxic soils, for example. You are talking about morphology and chemistry. The chemistry seems to be more important. What are your ideas? With regard to argillic horizons, chemistry, what do we use, CEC? Which one describes the morphology of the soil? I just want your ideas.

Guy Smith:

The older soils of the intertropical regions in Africa are dominantly Alfisols if you have a very distinct dry season. In the absence of a dry season, dominantly Ultisols. Now the morphology of these as such is very similar between the Paleudults and the Paleudalfs. But they have this other property, that of the moisture regime, which seems to correlate very well with the base saturation in the studies that I have been told about in Africa. And they may still be Ultisols if the moisture regime is udic. There is still quite a bit to learn about South American soils. The committee on the classification of soils with low activity clays have been wrestling with this problem. They have a proposal that we should establish an order of soils with low activity clays. But the committee generally has been in favor of retaining these soils as Alfisols and Ultisols although they may remove them from Mollisols before they finish.

Question 189

Crum:

I'm working on mottles that have been seen in the well-drained soils. The water table is below 6 feet.

Guy Smith:

Mottles of an argillic horizon or Bt?

No. It's in till.

Guy Smith:

In sand?

Crum:

In glacial till. I was just wondering if you might have any ideas what might cause that to happen? These are well drained soils.

Guy Smith:

I don't think I could come up with any idea that would be worth having on instant notice.

Rieger:

There is one, if I can interject. Soils that freeze deeply in the thawing process, the middle part of the frozen zone remains frozen longest. It thaws both from the bottom up and from the surface down. Adjacent to this frozen zone that lingers the longest period in the spring or early summer there's water perched both above and below the frozen zone. It can be saturated and the Russians particularly claim that this situation creates mottling or gleying in those two zones.

Question 190

Crum:

Why wouldn't it become re-oxidized again?

Rieger:

That's a good question. Mottles that are created tend to persist even after drying. That is, a soil can be saturated for a month or so and become mottled and then after the free drainage is established the mottles seem to remain. Why that occurs perhaps Guy can explain.

Guy Smith:

The soil physicists have discovered what those of us who have walked across a field know - when the frost has gone out of the surface 6 inches but is still present below, we know that you're walking on frozen ground. It's saturated above and the day the frost goes out the water disappears, the excess water. It just goes away instantly. Hitler lost an army because his generals didn't know about this. They hadn't walked across a field when the ground was thawing or they never would have gone to Stalingrad. They went there when the ground was frozen and they got trapped there when the thaw came. Trafficability on these soils is about nil. When there's still frost at a moderate depth it surely does thaw from above and below. I'm aware that the ground is saturated above, I didn't know that it was saturated below.

Question 191

Rieger:

The water from below is drawn up towards the frozen zone. The problem is that after free drainage is established, after the frost comes out, why would mottles persist?

Guy Smith:

Once you form a mottle there is no way to get rid of it, except by mixing from animals or plant roots. Once the iron has gotten there in a segregated form there's no way to diffuse it.

Question 192

Bruns:

I think this is from Kandiyohi county that you are talking about?

Crum:

Yes.

Bruns:

There are two different substages of glacial material and, within 12 miles, we can have the loamy profile that is bright all the way through. In the older stage, still a loamy profile, but immediately below the A horizon you begin to get a number of mottles. What we are wondering, how long can those mottles be retained in a soil?

Guy Smith:

I don't know but I'm reasonably confident it's a matter of some millions of years unless you have some biologic mixing of the soil.

Question 193

Hall:

Then I take it that you don't really have any problem with inclusion of data in a soil survey that doesn't exactly fit the series; there may be one property that's outside the boundary of the series? That's been one of the rules that they would not publish anything that had any characteristic outside the series limit. In this way we lose an awful lot of data.

Guy Smith:

I've nothing to do with any of this anymore.

Rust:

Philosophically, I think we understand the answer.

Question 194

Bruns:

We were talking about some of the fragipans and the problems. In Minnesota we find that these characteristics are, from some studies, inherent in the till material and the fragic characteristics go on down into the C horizon to considerable depths. We do not have the polygonal structure that is required per the definition and it's being proposed now that we drop the classification fragipans and identify them as Alfisols?

Guy Smith:

I saw one soil or two on a non-calcareous till. I think the series was Nokay. I thought it had a fragipan. I have to go back to my notes but I remember telling Nygard that I thought it was a fragipan. Now, I don't know whether that's one of those that you're involved in here. I've also seen, on drumlins in this part of the world, an extremely compact till. They have the same sort of thing in New York State, particularly on drumlins, the till is extremely compact. They have been discussing in New York State and the New England states how these soils should be classified, as shallow families with a paralithic contact or as soil with a fragipan. The influence of the compact till is the same as that of the fragipan in stopping movement of water and preventing entrance of roots. I would say that, to the best of my recollection, there is no strict requirement of polyhedrons in the fragipan because in my experience as the climate becomes more humid the polyhedrons tend to become larger and larger until you get only discontinuous leached cracks that do not completely surround the polyhedron. Yet they have all the characteristics of fragipans except for this failure to form complete polyhedrons; they're incomplete. I think I pointed that out in the peradic regimes. They don't always have the complete polyhedrons. I don't think I would have forgotten that when I was writing.

Question 195

Bruns:

After a strong wind storm there is a great deal of wind throw in the thicker forest. Looking at some of the roots the way they went down, there were a few areas where it looked like there was a start of these cracks but they were never complete. There would be some places that the roots went down but then they would be disseminated in other areas.

Guy Smith:

If they're disseminated, of course. The spacing of the fine roots is in the definition of fragipan.

Rieger:

Page 44, midway down the first column.

Guy Smith:

That's where I'm looking.

Rieger:

Is it the pan that's saturated for long periods? If the texture is sandy, polygonal color pattern may be absent.

Guy Smith:

You notice that word normally in there? Some or all pedons normally are leached. Now that 'normally' is a weasel word, it means that it's not always present.

Rust:

I think, Guy, perhaps one of the more perplexing morphologic problems is the establishment of a lower limit of something we want to call a fragipan.

Guy Smith:

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

Question 196

Crum:

Dr. Guy Smith, I did some work in Purdue and George Hall might want to say something to this also. As you know, Franzmeier and also Hall, looked at fragipans. I think there are two differences. We might want to call them fragipans but in Indiana-Ohio at least, a fragipan is thought to be a soil formation, a pan developed by soil formation. In Minnesota they appear to be a parent material feature. Do you think those two different criteria, should be named something different?

Guy Smith:

Well, if the properties are not pedogenic, if they are properties of the basal till, I would not want to include it as a fragipan. There are so many that have formed in loess, they have an

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

Question 197

Hall:

A little bit of clarification on what Jim has said. In northeastern Ohio we did a study and found that the polygonal pattern that started out in the pan, there was a definite pan, carried right on down into the glacial till. You could just trace them continuously down, they became larger and larger as they went down.

Guy Smith:

Those gray cracks can go very deeply, as I was mentioning, going into the sand below the loess in Tennessee.

Hall:

Your comment the other day about the freezing made me wonder. We've had some problems in parts of Ohio with pans in one field and not in the other field. I wondered if this could possibly be related to an earlier clearing or different cover and, therefore, not freezing. I had never considered this possibility before. Maybe we've destroyed them in some fields and not others and that's why we are having a difficult time in our mapping.

Guy Smith:

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

Hall:

In the area where we are having problems, we do get freezing down to almost a meter on occasion, so this would be a possibility.

Question 198

Rieger:

Something entirely different; this has to do with the Fluvents. Most Fluvents are identified by the irregular organic matter distribution with depth. But alluvial soils with permafrost are excluded from the Fluvents -- they're called Pergelic Cryorthents. There is no such thing as a Pergelic Cryofluvent. The reason, of course, is that, in churning, the soil movement can create an irregular organic matter distribution in any soil, whether alluvial or not. It has nothing to do with the alluvial deposit. However, this creates a problem. In much of the north the major agricultural soils are on the alluvial plains along the major rivers. You can not distinguish, at least at the subgroup level, these soils from upland soils that are also Cryorthents. It occurred to me that one way we could identify areas that would flood, etc., would be to consider an alluvial moisture regime. Has that ever been done? Relatively short period of total saturation followed by long periods of non-saturation?

Guy Smith:

In other words, when it is flooded?

Rieger:

Yes, that's right.

Guy Smith:

It's wet. When the floods recede, it drains out. That has been discussed. To my knowledge, I see no reason for not considering it. Nobody ever proposed it. We thought in the areas without permafrost that this irregular decrease would make the difference we wanted, the distinction we wanted. But we realized, of course, as you point out, that the freezing and thawing can produce the same sort of irregular decrease and it is normal that there is an accumulation of organic materials just above the permafrost.

Rieger:

Yes, but not necessarily. In those alluvial soils with permafrost that I'm familiar with, the permafrost table is at depths between 2 and 3 meters. When it's that deep you don't get this organic layer.

Guy Smith:

If it's at that depth it wouldn't enter into the classification anyhow. We stop at two meters normally in our examination.

Rieger:

The soil temperature would be below zero.

Guy Smith:

Yes, it would be in a pergelic great group or a subgroup, I mean. One of the things we wanted to be able to say about the Fluvent was that it had the considerable possibility of flooding.

Question 199

Bruns:

It just occurred to me that this could be approached directly through the moisture regime, flooded soil moisture regime.

Guy Smith:

That could be applied to the pergelic subgroups. I just add here that if the temperature regime is pergelic then we could get them into Fluvents. We have some alluvial deposits, particularly in our most arid regions, where the organic matter is extremely low because the alluvium is coming from eroding soft rock in which there's no original organic matter. Some of the floodplains in the arid regions come out, not as Fluvents, but as Torriorthents and this hasn't particularly disturbed me. The major transport of the soft rocks probably doesn't alter them very much. It's possible that the transported material has a lower bulk density than the original rocks and yet when you look at these eroding soft shales they're not particularly compact.

Question 200

Cooper:

In teaching students and lay people the use that they can obtain from a soil survey where we put all our data, many times the only map that people have used is a road map which says that when you leave here and go from point A to point B at that point you will find this town. Then you give them a soil survey that also has the base map on an aerial photograph. We have drawn our lines but in many cases we have not told them explicitly enough that when you go from Soil A to Soil B you may find Soil C. This problem of really identifying what is included on our soil surveys within the mapping units, is one that I have difficulty with. They really think that line is as good as the road map. It's not. What do you see as ways to really get across what's in our soil surveys?

Guy Smith:

I have no thoughts on that, I've no experience of that sort. My teaching has been more or less unrelated to interpretations of soil surveys.

Question 201

Bruns:

I have another topic and this concerns the calcic horizon or accumulation of carbonates. Our Calciaquolls have the calcic horizon within 40 centimeters but then when we go to

Calciborolls they're free of mottles for 1 meter. We have some soils that have mottles within 1 meter that have a calcic horizon. There's no subgroup in Calciborolls, so it's been assumed that, automatically, we get Aeric Calciaquolls. I'm wondering whether they really have an aquic moisture regime? Was there a subgroup identified in Calciborolls? Is there a subgroup that has been inadvertently left out of this?

Guy Smith:

I could have been, I'm not familiar enough with the precise situation to say what you should or should not do other than that if you feel it's needed, you should propose a subgroup.

Rust:

Ed, is another way of stating the problem or question, that there is a difficulty in placing profiles that we once would have thought to be moderately well-drained profiles?

Burns:

Yes. The Glyndon series is an example of a moderately well-drained soil and you dominantly have 6/4 color and mottles in the C horizon, yet it has the calcic horizon within the 40 centimeters so it's called an Aeric Calciaquoll.

Guy Smith:

We thought that there was a distinction between the calcic horizon of the Calciaquolls from the calcic horizons, say, in your normal Borolls. The calcic horizon in the Calciaquolls, we thought, was due to capillary rise and evaporation from the surface. Whereas, in the Borolls, we thought the calcic horizon was due to downward-moving water and withdrawal of that water precipitating the carbonates. It's quite possible that you can have something that's halfway between. In theory that could happen, you could get precipitation from capillary rise of a ground water and you could also have downward movement at another season of the year of the carbonates stopping at about the same point. You could theoretically have a calcic horizon formed as a result of both processes instead of one or the other. But your problems would involve first a proposal of a subgroup if you think it is necessary that you should have that.

Bruns:

Some of our field soil scientists have indicated that we have two different types. Just based on the position in the landscape.

Guy Smith:

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

Rust:

We have added a complication to that genesis in the last few years, we now have found gypsic horizons associated with calcic horizons in certain situations.

Guy Smith:

I think that we took care of that in Taxonomy. It can happen and you then decided which one takes priority.

Rust:

Yes, on the basis of the percentage amount.

Question 202

Bruns:

Another problem with the accumulation of calcium. The definition of Udolls excludes any accumulations of calcium within 1.5 meters and we are finding that we are getting these accumulations into central Minnesota. They are going down into Iowa and these have always been considered to be Udolls in the past. We are recognizing these accumulations.

Guy Smith:

We only prohibit soft powdery lime, we don't prohibit accumulation of carbonates. I don't know in what form you find this carbonate. Plenty of Aquolls in Illinois with horizons of lime concretions, large amounts of lime concretions but they're always too hard for our definition of soft, powdery lime. We made the genetic assumption that in a human environment an accumulation of lime would be in the form of concretions. That assumption may not have been warranted.

Bruns:

It's our feeling that some of these are threads and soft masses of lime rather than concretions.

Guy Smith:

We're about to remove that distinction. It's been under discussion elsewhere.

Bruns:

Yes, I know Dr. Turner is working on that now.

Question 203

Rust:

I think you've commented on it in some places. The correlation between color and organic matter often isn't very good. Sometimes it seems quite good. In Mollisols we don't complain too much. Where does the correlation not work out very well?

Guy Smith:

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

Question 204

Robert:

I wish to change the subject and talk about computers. I don't use *Soil Taxonomy* very often. I just access it from time to time and each time I have to access it takes me a lot of time to read, assimilate. To see if something is an Argiudoll or whatever. Wouldn't it be possible to interactively access *Soil Taxonomy* in an easier way? Accessing it on a statewide system. What I mean is ask questions, first, related to diagnostic horizons and find out if your data is part of the diagnostic horizons (definition). To do that I have an example. I'm looking at the Argid and I selected some of the questions I would see on the CRT of the computer. First, how many horizons in the profile? The second question, is the profile truncated? Is there lithologic continuity? The fourth one would be - for each horizon - to enter data like thickness, texture, structure, the amount of clay and fine earth, and so on, whatever is needed for the selection. Or, is there 2:1 clay in the horizon? The last question would be which horizon is tested for argillic. The program, using those data could tell you if what you are looking at actually is an Argid, or if not, why it's not. I know that in *Soil Taxonomy* almost any word is important so I wonder if you think that such a system could be helpful or could be possible. Could be helpful not only to English-speaking users but I think it would be very easy to do this in French and Spanish. Then it would be much easier for non-soil survey people, the ones not working all the time with *Taxonomy* at accessing it from time to time. Wouldn't this be in some ways helpful?

Guy Smith:

Have you seen the pullcards that Blakemore and his associates have developed in New Zealand?

Rust:

No, we have not.

Guy Smith:

Well, before you do much work on this I think it's already been done and the best thing to do would be to write. These are developed for all the diagnostic horizons and it's always a yes or no proposition. This would go regularly into a computer. They're working now on the pullcards for the orders and suborders and so on. I don't remember whether I've seen one for an order yet or not. I have, however, seen one with all the diagnostics on it. Leslie Blakemore. That's not for the diagnostic horizon but it shows you what they had been doing.

Rust:

I think there are two approaches to this idea. One is, of course, that you read your morphologic description and whatever laboratory data you've got and answer these questions. The other is that you assume you have a bank of data and you simply apply an interrogating system to the data searching for kinds of soils which is a slightly different approach to the matter. I presume Blakemore's approach is the first one that I speak of.

Guy Smith:

There is an International Committee report circular letter on this matter.

Robert:

What I was thinking of doing is to show, display different 'menus', for example, do you want to go to a diagnostic horizon or order and suborder? You answer, I want diagnostic horizon. Now, the second thing would be which one. Say argillic, for example. Then display all the data required to test for argillic. If you don't have such and such data try to get it. When you have your data come back and start others. Eventually, have some kind of 'help' from time to time, -- a 'help' command to explain whatever it is required. Some additional explanations. I guess this would be easy to put in different languages because those questions are very simple. The processing would of course be in English but the questions coming on the screen would be easy questions.

Guy Smith:

I wonder what the Chinese do about computers with their language?

Peterson:

IBM made a typewriter with Japanese characters.

Robert:

There is one Apple computer in North Vietnam already.

Question 205

Rust:

Guy would appreciate any thoughts that some of you might have on how to put these discussions together in a format that would be useful. I guess we have biased these interviews in terms of teaching needs to some extent. And not only our own students but also the international audience. Do you have any thoughts on how this compilation might be assembled?

Guy Smith:

I might start by explaining what I had thought would perhaps be the most useful organization but I surely would welcome any comments on that from the people who are here. I had thought to arrange the questions and the answers by subject matter and the order in which the chapters are presented in *Soil Taxonomy* so that we start with Chapter 1 here, the definition of soil and what it is that we're classifying. Then on the logic of Taxonomy, Chapter 2, and then the diagnostic horizons in the same order in which they are presented in *Soil Taxonomy* and then go to Chapter 19, not beyond. That's the application to soil surveys. So in making the transcriptions from these tapes I've asked them to double space and to start a new page with each question so that I can examine the questions and answers and shuffle them and arrange them by the format of *Soil Taxonomy*.

Hall:

Two comments. In teaching this I find that it is very difficult at times to keep the diagnostic horizons separate from the orders because they are so intimately related. I think you may have that problem in trying to shuffle them because immediately when you start talking about a spodic horizon you're into the Spodosols very heavily. A second comment would be that I hope there is a chapter on history where you will perhaps expand some of your answers to give the future students an idea of why some of these decisions were made. I don't know how deeply you want to get involved in that but many times we are asked 'well why did he do that, who made up that idea?' I think some of that would be very, very useful for the future students in trying to understand Taxonomy. If they have the historic background, it becomes much clearer as to why things are so.

Guy Smith:

That would be the introduction. It would cover the same grounds as in the chapter I wrote for you. I had thought the introduction would cover that and I'll have to be careful to paraphrase that. That I can go over the same ground at least.

Whiteside:

It seems to me that a supplemental index would be very important too. Supplement what's in Taxonomy.

Guy Smith:

I had also thought when I finish these interviews and have it assembled that I would have a group again come together to see what further questions there might be and comments. I figure, I'm sure that's going to be in Ghent. I'm not coming here again to the U.S. for that. I thought to ask Professor Tavernier to organize that.

Cooper:

I think that that's most appropriate because the students will have the taxonomy text and if they're coming through an area that they're having difficulty getting through such as cambic horizon which as we mentioned here today really throws the students for a loop, but they

understand podics, they understand argillics, they understand when nothing is there and it's an Entisol and then they come to the cambic and it's kind of like the argillic but it's not the argillic and going back they get an idea, they can go to that index and say what were these guys thinking of when they actually started out with that. If they can then go to that particular section, that really might help in understanding that clarification. So keying to the index in Taxonomy, I think is a very good idea. I really would see no other way to go about it except in that fashion because when a student then is using the Taxonomy and is running across a problem in trying to grasp a concept of the cambic horizon which I think is really the way that we need to teach it. Not so much in trying to point it out but to teach it as a concept of an idea of what to expect that they can go back and really find that. I like to teach the concept of cambic from a standpoint of not looking at one for a long time but looking at things that aren't and then all of a sudden showing something and saying what do you think that is? They say it can't be that, although they do think that's a cambic. That's kind of a good way to do it because they've gone through the deductions themselves. I think, seems to be logical to me.

Hall:

It of course would be useful, and I'm not sure that the length will permit this, to have examples of profiles for some of these and maybe you can refer directly to Taxonomy.

Guy Smith:

I think I could refer to Taxonomy in a number of places. I made a serious mistake in writing here when I numbered the pedons in the order in which I referred to them in the text. The pedons should have been organized by the Taxonomy. You could have all the Oxisols in one place and alphabetical. When this is revised I'm sure that they will have to do so next time. For myself I've made up a list of the classification of 42 pedons. After that they are alphabetical by orders and suborders, great groups and so on but the first 42 are not. If I want to know whether or not I have an example of a certain kind of soil, I can refer first to that list. It's an index to the first 42 pedons and then the rest of them are all alphabetical.

Hall:

I had a secretary go through and list all the pedons and then the state they were from and the order. The state and the order so I can at least skim down and pick them out a little faster.

Rust:

I'm sure that this effort will be appreciated by many. I think it will help us too, in our international audience I think as much as anyone.

Guy Smith:

I'm very reluctant to try to say who proposed what. Because so many people have contributed and I haven't always been present. I don't know, I don't trust my memory on that at all. I propose mostly to keep this anonymous and speak of the soil survey staff instead of a particular individual.

Question 206

Hall:

Would it be possible to somewhere list the Principal Correlators that were in power at a particular time or would that be too much?

Guy Smith:

That would be possible, yes.

Hall:

You talked about the fact you've worked very closely with the Principal Correlators so if we could have somewhere in there the listing of who they were, I think that would help.

Rust:

Having been going through fifteen years of microfiche correspondence I can verify that observation.

Guy Smith:

You've got the microfiche about correspondence?

Rust:

Yes. I certainly would agree with you that so many concepts derived from a consensus, because you kept saying to these people, we want to fit to the soils that are out there. Things kept coming back and forth in the correspondence. It must fit what is there.

Whiteside:

We've certainly appreciated your central contribution to this, Dick.

Hall:

I second that, I certainly appreciate Guy's willingness to spend this, what must be terribly trying time sometimes, hour after hour, month after month. So we appreciate you really putting this effort in and I think it's very worth while. I also express my appreciation to Dick for organizing this and looking at all that microfiche.

Rieger:

I would like to express my appreciation too especially for being brought out of retirement.