CONVERSATIONS IN SOIL TAXONOMY
(ORIGINAL TRANSCRIPTIONS OF TAPED CONVERSATIONS)

by

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Addendum to:

THE GUY SMITH INTERVIEWS: RATIONALE FOR CONCEPTS IN SOIL TAXONOMY

by Guy D. Smith

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Preface

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

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

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

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

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

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

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

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

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

Interviews by COPLANAR staff and J. Comerma

Maracaibo, Venezuela

January 1981

Coplanar Interview - Venezuela, January 27, 1981

Question 1

Guy Smith:

The first question has a rather clear statement of the problem involved and proposes an addition to the words in *Soil Taxonomy* to clarify or to help solve the particular problem which is a much more extensive problem perhaps than we realize. It is not only very common in South America amongst the Ultisols but the identical problem exists in Africa amongst the Alfisols. You asked my opinion and I can say only this that we have recognized this problem for a number of years. We have now two international committees working on a solution to the problems. The Agency for International Development has become interested in the use of *Soil Taxonomy* as a tool for transfer of experience between developing countries to increase food production, one of the main problems that they face in these countries. They have contracted now with the Soil Conservation Service of the U. S. Department of Agriculture to furnish financial assistance to pedologists, from any country, who are concerned with the problems of improving the definitions and the classification that is proposed by *Soil Taxonomy*. There have been six of these committees established so far and AID provides funds through SCS and through the University of Puerto Rico for the members of these committees to meet once a year in a country where the particular problems that they are concerned with exist. This particular problem was the one faced by the first of these international committees, under the chairmanship of Professor Frank Moormann of the University of Utrecht in Holland. They had a meeting in the field in Brazil two years ago. They had a meeting in the field in Thailand one year ago at which time there was a discussion in a conference room followed by about two weeks of studying in the field of the soils with which the committee was concerned. The field study is important because as yet there is still considerable differences of opinion amongst pedologists about the meanings of various technical words and the committee members cannot be sure they understand each other unless they can examine a number of the same profiles in the field together and discuss between themselves in person about the impressions that they get from these particular soils. This committee has been working for about seven years now so the problem is not a simple one. And there has been at one time or another something like 40 different members from virtually every continent in the world where such soils exist. They're due to report, to make their final report, this coming June in London where the committee on the classification of Oxisols is meeting with them. The committee on Oxisols and the committee on the classifications of these soils with low activity clay in Ultisols and Oxisols have a common problem, the boundaries between the argillic horizon and the oxic horizon and they must have a joint meeting of the two committees. The first joint meeting of the two committees took place in mid-Asia just preceding the meeting in Thailand last year. A final meeting will be in Rwanda in June of this year after which the committee will submit a joint proposal to the Soil Conservation Service for circulation to anyone anywhere in the world who expresses an interest in it. My opinion is of very little importance in this and it is a difficult problem and needs the international consideration and debate that it has been getting.

Question 2

The second question from number one from Coplanar is why does not *Soil Taxonomy* include the aquic moisture regime at the suborder level for differentiating Vertisols located in hilly areas from Vertisols in lowlands?

Guy Smith:

The intent of the definitions in *Soil Taxonomy* was to provide operational definitions which could be applied uniformly by pedologists with very diverse backgrounds and experience and that would permit the classification of the same soil in the same place by these pedologists working independently and with varying backgrounds. To define the aquic moisture regime, we found it finally necessary to provide an operational definition which involves a borehole or an observational well in which one could observe the position of the water table in the soil by the depth at which the water stood in the borehole. We could not find any other definition which would be simple enough for field men to use. It would be possible to have written a definition in terms of zero tension but this would require that samples be collected and transported to the laboratory and would have been much more costly and time-consuming than the drilling of a borehole. However, in the Vertisols the hydraulic conductivity is so low that you can put a borehole in a Vertisol where the moisture content is virtually zero, that it is saturated, but no water will come from the soil into the borehole. As a consequence the operational definitions which work in most kinds of soil cannot be applied to the Vertisols and eventually then we had to drop the originally proposed suborder of Aquerts until which time as we could find some operational definition to define that situation. At the time of the printing of *Soil Taxonomy* we had not found such a definition and in the hopes that one could be found we created a third international committee on the classification of Vertisols. The chairman of that committee is Dr. Comerma, and we hope that he will find a solution for these soils. We were not able to.

The intent of the pellic great groups and chromic great groups in the Usterts and Uderts and Xererts was to make the separation that might have been made by the aquic moisture regime but was the defined in terms of chroma rather than in terms of the soil moisture regime. The attempt did not work when we began to apply the definitions in the West Indies and in Venezuela. We realized that we would have to find a substitute eventually for the definitions of the these great groups in terms of chromas.

This problem exists in a number of countries where there are Vertisols and not just Venezuela and the West Indies. It also exists in North Africa and India and to a considerable extent in Australia. Although the Australians don't use *Soil Taxonomy*, the problem is there. The French classification as used by ORSTOM has adopted the concept of Vertisols but have simply said that the two classes - the pellic and the chromic great groups are... Well the pellic great groups are those that cannot be drained with surface drainage and the chromic great groups are those for which surface drainage can be provided, making it an engineering application but trying to solve the same problem of classification. It is the most important one amongst the Vertisols.

Question 3

The next question is why the cambic horizon was not considered in the classification of the Vertisols, although there has been adequate alteration in many of them to produce a cambic horizon.

Guy Smith:

The cambic horizon was not used as a diagnostic in the Vertisols because the arrangements of horizons are commonly so complex that it was considered undesirable to try to distinguish a Vertisol with a cambic horizon from one without. It is very common that a Vertisol will be developed in a calcareous parent material but the churning processes that go on from the shrinking and swelling may push this calcareous parent material to the surface in parts of each pedon, while in other parts of each pedon the carbonates are leached rather deeply. We try then to distinguish between that part of the pedon that has a cambic horizon and that part of the pedon which does not have a cambic horizon. We are in effect complicating our classification of the soil. It is the intent of the pedon to permit soils to have intermittent horizons that do not occur everywhere in the pedon and the Vertisols are the most common group of soils that have these intermittent horizons. Some of them actually have natric horizons and have albic horizons and have argillic horizons and yet we don't recognize any of those in the Vertisols, yet they are telling us that a single plowing will obliterate them. In the case of the cambic horizons, it would be possible to make a distinction between the Vertisols with and without the horizons but then we complicate our nomenclature at the subgroup level and our series in the U.S. which have these intermittent horizons are split so we require complexes of series rather than single series with intermittent horizons.

You may have noticed that the question is... The question is whether it is incorrect to give the elimination of the B horizon to an area within a Vertisol, a pedon that is a Vertisol that has been leached in carbonates.

It would be perfectly correct in writing a description where you are using the ABC horizon terminology to label such a horizon as a B. You will notice, however, that we have not in *Soil Taxonomy* used the ABC horizon terminology. We have deliberately tried to substitute diagnostic horizons for that terminology.

Question 4

Guy Smith:

I don't see any particular contradiction here. It is necessary in the Vertisols where the surface granular mulch falls down a crack to permit an irregular decrease in carbon with depth at any point in the pedon. I have seen Vertisols in which the black granular clay has penetrated to depths of well over two meters down these cracks and at the base of the crack there would commonly be a rather large bulbless protrusion of that black material into the sides of the crack so you get what looks like a thermometer with a big bulb at the bottom. And eventually then one can find that black material pushing its way upward at an angle of about 60 degrees to either side of the crack. The churning process can be quite pronounced in the Vertisols. In the vertic subgroup of the Fluvaquent, we have a soil that probably would be a Vertisol were it not for the failure to have one or more of the other requirements of the Vertisol in addition to the cracks. One of those is the presence of slickensides. Another is the requirement that there be at least 30 percent clay in all horizons after the surface 18 centimeters has been mixed as by plowing. Now one can have then a soil on the flood plain which has very fine texture, montmorillonitic mineralogy if you please but which has no slickensides perhaps because it is too humid throughout the year or which has a strata that has less than 30 percent clay, either one of which would keep it from being a Vertisol, but neither one of which would keep it from having many of the characteristics of Vertisols and this was the intent then of the vertic subgroup of the Fluvaquent.

Participant:

A cambic is not allowed as you have said, and should have organic matter with depth at a level of 0.2 or higher. Then when you have cracks with a cambic you may have a Vertic Fluvaquent.

Guy Smith:

The Tropaquept is supposed to have a cambic horizon. The Fluvaquent is not supposed to have a cambic horizon. The definition of alteration adequate for recognition of a cambic horizon in these wet soils was not particularly satisfactory. You recall that there was considerable criticism of that here in Venezuela when I was here. We did propose that restriction against irregular decrease of organic matter or a high content of organic carbon at depth be removed from the definition of the cambic horizon and that in its place we would substitute the presence of iron-manganese concretions as an evidence of alteration adequate for the recognition of the cambic horizon in these wet soils. The proposal was made about two and half years ago and it has been sitting in Washington waiting for someone who had the time to pay attention to the proposals for the changes in taxonomy and to approve or disapprove of those changes after the necessary consultation. In New Zealand the presence or absence of the iron-manganese concretions was found to be adequate to separate the very recent wet alluvium from the older, say early Holocene alluvium, that had to be called an Entisol with the previous definition. And I think I wrote you here in Venezuela asking you to see whether or not the same situation would hold in Venezuela.

(unintelligible comment or request for an explanation)

Well, I can not...it's an Entisol - it cannot be a Vertisol. I think those definitions are reasonably mutually exclusive.

Question 5

The next question comes from the floor, and asks what would be the difference between a Vertic Tropaquept and a Vertic Fluvaquent?

Guy Smith:

The difference would be primarily one of those listed in the definitions of the orders of Inceptisols and Entisols. The most common distinction that I would visualize would be that the Vertic Tropaquept would have a histic epipedon, a mollic epipedon or an umbric epipedon and the Fluvaquent would not have any one of these. The presence or absence of the cambic horizon as a distinction would be very exceptional, in my opinion. One would rather expect that the soils would be very similar below the thickness or the depth to which the umbric, the normal epipedon would extend.

The histic, mollic or umbric epipedon is not required if presence of a cambic horizon can be demonstrated. And this can be the situation if the sediments are old enough that the organic carbon has disappeared. By the present definition, the carbon must decrease regularly and reach low levels less than 0.2 percent at a depth of 1 and 1/4 meters.

The restriction against a hard or very hard consistence and the massive structure in a mollic epipedon was introduced to keep out of Mollisols certain soils that have a xeric moisture regime in southern California. These soils have what the Australians call a hard-setting A horizon such that if one wants to sample a soil in the summer he starts with an air-drill such as they use to break concrete up in the pavement. Once you're through the epipedon, digging by shovel is possible. These soils have a color and a carbon content that is just marginally adequate for a mollic epipedon and we wanted to keep them fairly out of the Mollisols and keep them together whether or not there was just a little more carbon or a little less or whether the color value was closer to three and four but lay between. The Mollisols that we know in the U.S. do not present these same problems with sampling or plowing. They are structured enough that they may be plowed when dry. Whereas, the ones we wanted to keep out are very difficult. The British groundnut scheme failed because of this nature of the epipedon that they tried to work the soils with big tractors and heavy plows. The plows were destroyed like trying to plow up a concrete pavement. This is what we wanted to keep out of the Mollisols. There are problems if we introduce the moist consistence that are Mollisols that we would like to have be Mollisols that have consistence that is firm when moist. It is the dry consistence, that hard-setting epipedons that we want to keep out of the Mollisols.

Question 6

In Colombia there are soils that have the color, organic carbon requirements for a mollic epipedon and that under excessive use with heavy machinery, the structure has been damaged and when dried these soils have a hard and massive epipedon. The question is should *Soil Taxonomy* favor the soils in California or Colombia?

Guy Smith:

When the Colombian soils are moist, they are very friable and have a favorable chemical condition. In reply, the first thing is that the southern California soils are exactly the same. When moist one would never suspect that they were going to become so hard when they have dried. But yet they do. I think there is a micromorphologic distinction that permits one to recognize these hard-setting A horizons when they are moist, but it has never been made quantitative. You must keep in mind, always, the conditions under which *Soil Taxonomy* was developed, that we had a large body of established soil series which had been tested by use of the public soil surveys and the pressures were enormous not to split these series with minor criteria unless the splitting of the series would improve the interpretations that were appropriate. Many of the complications of the definitions in *Soil Taxonomy* are due to this strong bias by the soil survey staff against changes in the definitions of soil series. And in an effort to avoid splitting the series, we have introduced what looked like inconsistencies in many places but really are consistently in favor of one reason, namely that we want to keep the soils together in the taxonomy if they really belong together because of their genesis and their behavior. We could not, of course, know everything about all the soils of the world when we developed *Soil Taxonomy* and so we disregarded those that we knew nothing about and paid attention to the soil surveys that we had already established and known to the general public through the published soil surveys.

If in the Colombian soils the moisture regime is udic and the epipedon is rarely ever dry, then the importance of the cementing properties is at a minimum and if the definition creates problems, then it is important that it be brought the attention of the correlation staff especially the staff leader for soil classification in the Soil Conservation Service so that the appropriate steps can be taken to correct the errors which I indicated in the original definition.

Of course, we can put in exceptions. Instead of saying that structure is strong enough that the epipedon is not both hard and massive when dry, we can say the epipedon is moist at all times or the soil has a humid moisture regime or the epipedon has a structure strong enough that it doesn't need to have any further definition.

Question 7

The lower depth limit of the cambic horizon in well drained soils must be at least below 25 centimeters depth. The upper depth limit has not been established. What is the maximum allowed depth of the upper horizon? Why in the definition of the cambic horizon is this depth not defined?

Guy Smith:

The limit of 25 centimeters to the lower boundary of the cambic horizon was set to avoid changing the classification of a soil by plowing a very thin cambic horizon so that it becomes mixed in the plow layer and ceases to be identifiable. We have tried to help taxonomy to keep cultivated and uncultivated soils together as long as it remains possible. If, of course, under cultivation, new horizons form then the classification needs to reflect this, but the transfer of experience from a cultivated soil to virgin areas of the same soil is complicated if we changed the classification as a result of a few plowings. This is the reason for the definition of the lower limits of the cambic horizon which normally would have to be on lithic or paralithic contact or then above the horizon of accumulation of carbonates, things which are excluded from the cambic horizon and which would have a fairly clear boundary. The upper limit of the cambic horizon did not seem important. Normally, in a cultivated soil it would be at least at the base of the plow layer if the epipedon is ochric or it would be at the base of mollic or umbric epipedons and does not become critical to the classification of the soil so that it did not seem important to specify where the cambic horizon begins. This is a difficult problem in soils that have an ochric epipedon. It is not particularly difficult if the epipedon is umbric or mollic. It is the presence of the cambic horizon that is relevant to the classification, not the thickness. The lower limit is relevant to the classification if plowing is going to obliterate it.

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

Question 8

The next question is number 6 from Coplanar. There are Haplustalfs with a high sodium content in the argillic horizon but not enough to meet the requirements for a natric horizon. Would it be interesting to show this characteristic at the subgroup level by creating a subgroup of Natric Haplustalfs?

Guy Smith:

This was done at one time in one of the various approximations or rather one of the supplements to the *Seventh Approximation*. The subgroup was eliminated on the grounds that it was very difficult to estimate the sodium saturation or the SAR in the field. There was not always adequate visible clues to the presence or absence of sodium. And when the interpretations were checked against the data that we had from the laboratory on the sodium saturation we could find no evidence in the U.S. at least, that a sodium saturation say of 10 or

12% was significant to the behavior of the soil. So we had two factors working against this natric subgroup: 1) the difficulty of its recognition in the field and 2) the similarity of interpretations for soils with and without the significant but smaller amounts of sodium. If there is evidence that suggest that the behavior of the soils in Venezuela with say 10 or 12% saturation with sodium is significantly different from the others, then a proposal should be made or a modification of the definition of Typic Haplustalfs.

We have a precedent for natric subgroups in the Alfisols in that there is a subgroup of Natric Haploxeralfs and the subgroup of Natric Palexeralfs. In these, the sodium is high but high at considerable depth. In the definitions of these subgroups, the sodium exceeds 15% within one meter of the surface. Similar provisions could be inserted for the Ustalfs if it is felt to be important. If these soils are to be irrigated, then as the Xeralfs are commonly in California and in Spain and in north Africa and so on, the sodium becomes potentially important because if too much water is applied you will create a groundwater that will come up by capillary rise bringing the sodium up into the active rooting zone where it becomes an important factor in soil management.

Question 9

In the definition of Humults, there is required either 0.9% organic carbon in the upper 15 centimeters of the argillic horizon or 12 kilograms of carbon per square meter to a depth of one meter. The range here appears to be very great.

Guy Smith:

It was always our desire to keep together in the classification the soils that were virgin, the cultivated soils, and also, the eroded soils, so that the experience of the use of one could be extrapolated to the other. In reviewing the data for the Humults that we have in the United States, it seemed that the soils that had 12 kilograms of carbon in a cubic meter also had 0.9% carbon in the upper part of the argillic horizon. We have a number of these soils in the U.S., some of which have been eroded to the point where the present content of carbon is less than 12 kilos per cubic meter but where the carbon is at or above 0.9% in the part of the argillic horizon that remains. So that these soils can remain as Humults, even though rather severely eroded, is only when a major part of the argillic horizon has been lost that they get changed from Humults to some other suborder. The range does look great and yet when we examine the data there was a relation in the virgin soils between the two numbers.

If some Humults are so classified because of the carbon in the argillic horizon and others because of the total carbon in the upper meter and there are differences between the two kinds of Humults that are not due to erosion, then it seems likely that some sort of separation at the subgroup level would be surely warranted. *Soil Taxonomy* provides for ustic subgroups of the Tropohumults because these were known to exist in Zaire by the Belgian pedologist who has worked there. Is it possible that the differences between the Humults in Venezuela can be associated with differences in the moisture regime? There's difference between the udic and the ustic regimes. Can someone answer that?

If the differences are not associated with the moisture regime, either ustic or epiaquic and it is felt important that the kinds of soils be separated because of their differences in behavior, then it is necessary that those who know the soils make some more or less specific proposals for modification of the definitions. The definitions do provide for the typic, the epiaquic, the ustic subgroups, but perhaps these are inadequate for conditions that were unknown to us in Puerto Rico and Hawaii. These are the only places where we have experience with the Tropohumult.

Question 10

It points out that in the Central Llanos, there are soils having an aeolian mantle of coarse sand between 50 centimeters and one meter thick, lying on a buried soil which may be, for example, a Tropaqualf. The question is how to classify these soils?

Guy Smith:

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

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

Question 11

Are there field criteria to recognize the family mineralogy of fine-textured soil?

Guy Smith:

Most pedologists are able to distinguish the soils in which the dominant clay mineral consists of kaolin with accessory oxides of iron and aluminum. The knowledge of soil genesis for example would tell us that the family mineralogy of an Oxisol was either kaolinitic or oxidic or ferridic, but would not tell us which amongst those three. The geologic knowledge of the bedrocks of the geomorphology gives us some clues as to the nature of the clays in the soil, but is hardly adequate to let us say definitely which one it is. Now, when you have a measure of the cation exchange capacity one can infer from that a good deal about the nature of the clay if the CEC by ammonium acetate is 60 milliequivalents per 100 grams of clay, one can infer either montmorillonite or vermiculite. And with some background information from the laboratory, one can infer which of those it is. Now the CEC can be estimated in the field. With the help of a small portable laboratory about the size of my briefcase, you can measure the CEC. You can estimate the clay with your fingers and from those two, you can get an estimate of the nature of the clay. Montmorillonitic; if it's below 24 milliequivalents or below 16 it certainly is kaolinitic. Somewhere between 24 and 45, its going to be mixed. This, however, requires the use of the field laboratory kit to get at the CEC per hundred grams of soil. Without that, it's very difficult. In working in the West Indies, we did use the field kit and we arrived at kaolinitic mineralogy on some Paleudults which they should have been, of course, but we had to check it out and it came out about 16 milliequivalents per hundred grams of clay so I assigned the soil a kaolinitic mineralogy.

Question 12

Why is there no subgroup of Plinthic Tropaqualfs? There exists such soils as Tropaqualfs with plinthite and without plinthite in Venezuela that have geographic extent.

Guy Smith:

It must be remembered that the subgroups that are listed in *Soil Taxonomy* are those that were known to exist in the United States or that were specifically requested in other countries. So that the failures to list such a subgroup only means that no one asked for it and it was not known in the U.S. Had we had such a soil in the U.S. we surely would have created a plinthic subgroup of Tropaqualfs because it would have been consistent with the recognition of plinthite in other great groups and in other orders.

Comerma Interview - Venezuela, January 28, 1981

Having finished the questions from Coplanar last night, we resume this morning, Wednesday, January 28, with questions from Dr. Juan Comerma and his associates.

Question 13

What is the origin of the 25 millimeters and 75 millimeters of water penetrating during 24 and 48 hours respectively to determine the moisture control section?

Guy Smith:

If one is going to use the concept of soil climate, the periodicity of dryness and availability of moisture in the soil must be determined relative to some fixed part of the soil. And the moisture control section was devised to permit the estimation of the soil moisture condition from climatological data. The 25 millimeter limit was so that the period of dryness would not be interrupted by a brief, light shower during the dry season. The 75 millimeter lower boundary of the moisture control section was set to give some arbitrary limit for reference when calculating the soil climate. The moisture control section itself, its content of available water was calculated from the measured moisture contents of the dryland stations where records have been kept for up to about 30 years. A model was devised for estimating recharge following rains and withdrawal between rains and the periods of time during which the moisture control section was dry in some parts or dry in all parts or moist in all parts was calculated for these dryland stations. This was not perfect because the correlation observed between calculated moisture conditions and measured moisture conditions had a coefficient of correlation of about 0.8 leaving nearly 1/3 of the differences unaccounted for. Then the classification of the soils of the great plains was predetermined by the correlation staff and the boundaries drawn between soils that were desired to be classified as Ustolls, Udolls and Aridisols. Amongst the Ustolls, boundaries were drawn between the udic, typic and aridic subgroups. Based on the prevailing knowledge of the seriousness of moisture availability or moisture shortage and the means that were available to adapt the farming systems to the prevailing climate. Having drawn these boundaries, the calculated moisture conditions were determined for a large number of stations on the Great Plains and the periods of dryness were fitted to the calculated conditions within the limits that had been predetermined.

Question 14

The next question is, how is the classification of the soils determined? Is it by calculation of the moisture regime from meteorological data or is it by actual measurements of soil moisture?

Guy Smith:

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

Question 15

Were the temperature limits determined by consideration of soil genesis, soil utilization, or both? Which had the greater importance?

Guy Smith:

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

The taxon of Entisols established for the Psamments require textures coarser than loamy very fine sand between 25 centimeters and one meter. Nevertheless, if below one meter there is some other diagnostic horizon, the soils fall out of the Entisols.

Question 16

Should not the control section for the sandy soils extend to two meters?

Guy Smith:

The definition of the Entisols prohibits a horizon such as an argillic horizon unless it is a buried horizon and provided that its upper boundary is within 2 meters of the surface. A spodic horizon or an argillic horizon deeper than two meters to the upper boundary is considered to be a soil without a diagnostic horizon and therefore falls into the Entisols and these soils with such thick epipedons are almost always a sandy soil.

So the control section for the Psamments that distinguishes them from other Entisols such as Orthents and Fluvents extends to one meter, but the control section for defining the order to which a soil belongs extends to two meters in the sandy soils. The limit of two meters was taken because the difficulty of making observations at depths greater than two meters in sand is enormous and some limit must be set that will permit the mapper to determine in the field without specialized drilling equipment whether or not there is a diagnostic horizon. If the diagnostic horizon is present but deeper than two meters it was believed that its influence on the use of the soil would be minimal.

Question 17

The next question is what were the changes in the concept of the aquic moisture regime between the *Seventh Approximation* and *Soil Taxonomy* so that a soil that earlier would have been classified as an Aquert cannot be so classified now?

Guy Smith:

In the *Seventh Approximation* the Aquerts were not defined on their moisture regime but rather on the colors and depths to mottling. Aquic suborders in other great groups were defined on being saturated with water at some season or artificially drained and then in addition having certain specified colors. The concepts of the moisture regimes were not fully developed at the time of the *Seventh Approximation* because the ustic moisture regime had not yet been introduced and the concept of the aquic moisture regime had not been yet defined. In the 1967 supplement, the definition of saturated with water was put on an operational basis so that a borehole was required to determine the height at which the water table stood. The distinction between the xeric and the ustic moisture regimes was also introduced in the supplements to the *Seventh Approximation.* The same problem persisted with the Aquerts that existed in the *Seventh Approximation* because the definition of saturation with water on an operational basis could not be applied to Vertisols, since the borehole measurements were unreliable in such slowly permeable soils.

The concept of the Aquerts was transferred from the suborder to the great group level in the supplements where the pellic great groups were supposed to represent what had previously been considered the Aquerts.

The distinction between a Pellustert and a Chromustert then was based not on the moisture regime being aquic but rather on the chroma of the soil within the specified depths. This distinction does not seem to work as was intended and soils that should have low chromas do not always do so. For example, in Jamaica, the wettest Vertisol on the island has a chroma of 3 to 4 throughout in 10YR hues. This soil is frequently flooded for considerable periods of time and in addition to being very wet is quite saline. As a consequence, there has been very little vegetation on these soils during their development and apparently there has not been enough energy for the iron-reducing micro-organisms to produce even faint models in this very wet soil. An international committee has been organized to reexamine the distinctions between the various great groups of the Vertisols.

Question 18

Why is plinthite near the surface with an aquic moisture regime included with Oxisols without regard to the presence or absence of an oxic horizon?

Guy Smith:

We know very little about the soils that were intended to be included in the superic subgroup of Plinthaquox. These are the soils that are reported to have the plinthite at the surface. They occur normally at the base of a slope where there is an outcrop of petrolitho-plinthite above. They receive seepage rich in iron and the plinthite reforms and recements the petro-plinthite that has been transported down slope. If cleared, these soils form an iron crust at the surface and are permanently useless. The intent was to keep all these soils together because the hazard of removing the forest from these soils is enormous and we do not know the kinds of horizons that we find in them. There are no studies reported of these soils in the literature, only reports from pedologists who have seen them in passing. It is simply a matter of keeping together the soils that have this over-riding problem that precludes the clearing of the forest without permanently destroying the productivity of the soil.

Question 19

What is the concept of the epiaquic regime? Can this concept be applied to soils that are flooded occasionally as the lowlands along the floodplains of Venezuela?

Guy Smith:

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

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

The soils used for paddy rice are not treated in *Soil Taxonomy* for lack of enough description to be able to define such a group of soils. They have been studied rather extensively in Japan and there is a small literature concerning their classification. The need for an international committee to develop the classification of these soils is obvious and such a committee will doubtlessly form in the not too distant future.

Question 20

Soil Taxonomy appears to be constructed so that if one desires to have a considerable number of implications concerning cultural practices, the application to the....The soils must be classified at least to the family level. In part this constitutes a limit on the application in countries that are developing. What could be the solution to this?

Guy Smith:

One of the principles followed in the construction of *Soil Taxonomy* is that we should be able to make the largest number of the most important statements about the soils that are grouped in any taxon at any categoric level. For the most detailed interpretations, one does have to go to the family or even to the series level. However, if there are known factors and one is mapping at a small scale so that application at the family level is impossible, it is still practical to use phases of subgroups or great groups to increase the number of interpretations that can be made. The phases may include family criteria that are pertinent to the foreseeable uses of the soil. Thus, if the reaction is known but the clay mineralogy is unknown, one can use a phase at the subgroup level to indicate a non-acid reaction provided that this seems important to foreseeable uses of the soil.

Question 21

The next question is whether there has been experience with soil correlation at categoric levels higher than the series.

Guy Smith:

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

Question 22

The question is raised that where the mapping units are in terms of phases of families or subgroups, the soils represented by the delineations on the map may have only a part of the range of properties for those particular taxa. Another survey area might have the same taxa in the namc, but have another part of the range of the particular taxa. Has there been experience in correlation with this?

<u>Guy Smith:</u>

The answer is that I have not been closely associated with the correlation process for many years and I do not know.

Question 23

What are the criteria that guide the formation of the key of the taxa at different categoric levels? Are they genetic or pragmatic?

<u>Guy Smith:</u>

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

Question 24

The Fluvents and the fluventic subgroups may have an irregular distribution of organic matter with depth. How much difference in organic carbon with depth constitutes an irregular distribution? Is it less than 1/10 %, more than 1/10 %, less than 2/10 %? Is there some figure in relation to the determination of carbon?

Guy Smith:

There's no fixed number that we have had in mind other than that the difference should be significant. If the difference is less than the reliability of the laboratory determination, it must be disregarded. If the difference is greater than the reliability of the laboratory measurement and greater than the probable error of sampling it is considered to be significant and irregular. Normally, the laboratory people know the difference in measurement of organic carbon between duplicate samples. This is the probable laboratory error. The laboratory people do not understand the probable sampling error for measurement of organic carbon. For example, if one takes two samples from a pedon, one from each side of a pit, the difference may be vastly greater than the laboratory error. It may amount, in some soils, to a difference of 3% carbon, perhaps. If one is sampling a pit in an Aridisol, the sample taken from the pit may have a value perhaps of 3/10 % carbon, but if one then takes a composite sample at a distance of 5 meters from the sample collected in the pit, the value may be something like 8/10 % of the composite sample. This is because within the Aridisols, the organic carbon varies enormously according to the position of the vegetation. The pits are normally dug in barren areas between the plants and so they have a bias toward a low carbon value whereas if one takes and draws a circle around that pit and samples every few meters and composites the samples, one gets a number of samples from under the plants and these are generally higher so that one must consider not only the laboratory error but the possibility of the sampling error. Now the sampling error is much greater in the surface correlations than it is the deeper correlations. And for the identification of a Fluvent or a fluventic subgroup, I think the sampling error would normally be very small, if there were no disturbance or animal activity that was visible in the soil.

The irregularity in carbon is normally associated more closely with the particle-size class or the percentage of clay than with any other one thing. It was assumed in the definition that the Fluvents and fluventic subgroups would be stratified in any instances, and if so the stratification would be reflected in the content of organic parts.

Question 25

The soil of moderate and medium subangular blocky structure that contains carbonates only in the upper 50 to 60 centimeters could possibly be produced by a process of recalcification. Should this soil be considered to have a cambic horizon or on the contrary should the soil be considered rejuvenated and lacking a cambic horizon?

Guy Smith:

The recalcification of a soil which has been leached of its carbonates would normally be due to addition of carbonates at the surface either by wind or water action. If the recalcification is the result of flooding, the calcium carbonate that is present normally would be accompanied by fresh alluvial sediments. If the carbonates are brought in by wind, there is no necessary addition of other mineral sediments than the calcium carbonate. In the first case where the recalcification might be due to flooding, I would be inclined to consider that the leached horizons were part of a buried soil and classify the soil accordingly. If the carbonates had been brought in through aeolian action, it is generally common to find the secondary carbonates on the surfaces of the peds and absent in the interiors of the peds. In this case, I would be inclined to consider the soil to have a cambic horizon and that it has been rejuvenated by the addition of carbonates from the atmosphere.

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

Question 26

The question is raised that there may be other methods of recalcification than the two previously mentioned: one from capillary rise from a carbonate-rich ground water and the other seepage from a higher-lying area where the soils are highly calcareous.

Guy Smith:

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

Question 27

In the depression of the Lake of (?) it is common to find cartographic units of two soils closely associated of the great groups of Haplustolls and Ustochrepts. Nevertheless, the majority of the Ustochrepts of fine loamy or finer families meet all the requirements for Mollisols except that of color of the epipedon. In this way, soils developed under natural conditions that are very similar with the same use potential are separated by the taxonomy of the soil at the highest category. Would it be justifiable to modify the requirement of the mollic epipedon to allow the grouping of all the soils in the same order?

Guy Smith:

We have recognized while developing *Soil Taxonomy* that in intertropical regions the color value of the epipedon is not as well related to the carbon content as it is in temperate regions. We set up the suborder of Tropepts in order to avoid being tied by the distinction between umbric and ochric epipedons in the temperate soils. We have permitted a mollic epipedon in a number of the Tropepts if they have these characteristics of a vertic subgroup. It would be legitimate in my judgement to attempt to bypass the distinction between the mollic and the umbric epipedon under the conditions that are sited in the question. Precisely how to do it, I do not know. Some suggestions from those who are familiar with the soils in question would be essential in my judgement.

Question 28

The question is raised about the origin of the 0.6% carbon required for the mollic epipedons.

Guy Smith:

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

Question 29

Was the color value of the mollic epipedon used as a basis to distinguish the soils in the United States that formed under grass from the lighter-colored soils that formed under a forest vegetation?

Guy Smith:

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

Question 30

In light of the answer given to the preceding question, would it be useful in Venezuela to examine the colors of the epipedons in soils that are under forest as in contrast to those under savannah to see whether some adjustments can be made?

Guy Smith:

One never knows what studies are going to be useful until they are completed or at least well along. There certainly is no harm done to examine the available data from this point of view and whether or not the conclusions will prove useful will depend on what the data shows.

Question 31

Reference is made to page 271 of *Soil Taxonomy* in the chapter and definition of Mollisols to 0.4. This raises the question of whether the criteria listed for Mollisols having an isomesic or warmer iso temperature regime can have vertic properties as in the Vertic Haplustolls or Vertic Hapludalfs. The intent of the 0.4, was to exclude from Mollisols the vertic subgroups that would otherwise go into Mollisols or Inceptisols. They are allowed to have a mollic epipedon in Inceptisols, if they have vertic properties. This same requirement would exclude from Mollisols the soils having vertic properties and lacking in argillic horizons. A Vertic Argiustoll would be a possibility in intertropical regions, but a Vertic Haplustoll would be excluded and would be included with the Vertic Ustropepts.

The question is why these soils were included with the Tropepts rather than with the Mollisols if they had the vertic property?

Guy Smith:

It so happens that the Vertisols are permitted but not required to have a mollic epipedon and in Puerto Rico we have in many places a transition from an Inceptisol on a side slope of a hill to a Vertisol at the base of the hill. The epipedon in these soils are sometimes mollic and sometimes not, but they are always marginal to the limit between a mollic and an ochric epipedon. It seemed desirable to keep these soils together in the classification even at the series level so that if we were to do so we had to permit a mollic epipedon in the vertic subgroup of the Tropepts.

Soils that are classified in the great groups of Dystropepts may present, may have, cation exchange capacity less than 24 milliequivalents per hundred grams of clay. And also have characteristics as follows: a phreatic water table between 75 centimeters and one meter depth. A horizon with more than 5% plinthite within one and a half meters depth. The combination of these two characteristics. It is possible to classify within the subgroup aquoxic, plinthoxic and plinthaquoxic Dystropepts respectfully. No provision has been made in taxonomy for these soils. It is, of course, possible to have these three subgroups if the behavior of the soils is such that it seems desirable to have all three rather than two. No provision is made in taxonomy as we have mentioned earlier for soils that do not appear in the United States and soils for which no foreign request has been made for special subgroups.

Maracaibo Interview - January 29, 1981

Question 32

The fifth congress of the Venezuelan Soil Science Society concerns a statement concerning some unsolved taxonomic problems of Venezuelan soils. The summary is as follows. Frequently, the limnic materials are found in the layers of organic soil materials. For this reason the system of classification of the USDA *Soil Taxonomy* provides their inclusion in Histosols. The same system, on the contrary, does not offer any specific place to accommodate limnic materials with little organic matter. In the Lake of Valencia, soils are developed, in part, from lacustrine material with little organic matter, principally marl and secondarily diatoms. The soils of recent emersion are classified as aquic; those soils with a greater time of exposure in the Ustolls. Between these two extremes of the chronosequence there are found Fluvents, an important group of lacustrine soils. Because of the origin and the particular characteristics, low bulk density, high water with shrinking (?), very rapid infiltration and so on, this grouping is unsatisfactory.

Guy Smith:

To correct the deficiencies of the system that is found in the classification of limnic materials it is proposed to create a suborder of Limnents and a great group of Limnaquents.

It's also suggested to create a limnicsubgroup andfamilies ofmarly and diatomaceous mineral soils. The relationship of the soil system, soils more involved strongly calcareous and with a mollic epipedon classification more adequate for the (?) and not for the Ustolls. This would mean the creation of a new class of soils, that of the Limnic Ustirendolls. [The transcriber had a very difficult time understanding the question and response up to this point.]

The situation of the soils formed in the limnic sediments at Lake Valencia is not unique in the world, though, to the best of my knowledge the soils are not particularly extensive. I have seen somewhat similar soils in The Netherlands where the genesis may have been due to the cutting of the peat for fuel but at any rate the soil is composed of limnic sediments with too little organic matter to classify them with the Histosols.

There are procedures set forth for dealing with situations of this sort and these procedures involve very much what you have done in your resolution but involve also a little additional preparation. One thing that needs to be done is to submit this proposal to the Soil Conservation Service for, let us say, international consideration. There may be a few such soils in the United States, though I know of only a couple that would have a diatomaceous mineralogy. These may not have the low bulk densities of the soils around Lake Valencia. It should, therefore, be... let me start again. The society should, therefore, submit this resolution to the Soil Conservation Service together with some documents about the nature of these soils. It is specified here the bulk density is low but what is low? How low? There must be some measurements of the bulk density of the soil. I should also point out that you might, advisedly mention the presence of the cracks in the soil, even though they have been out of the bottom of the lake for an appreciable time. The original cracks which appeared at the family level are still present in at least some of the soils that I have been shown. The low bulk density is very apparent in field, but it is not apparent to someone reading the documents on the society unless some numbers are included to document how low this bulk density is.

You have made a second proposal in that you would like to modify the definition of Rendolls as given in *Soil Taxonomy.* This will be a more disputable proposal than the one about the limnic groups because the soil survey staff in the U.S. has gone through this particular argument before, where we have soils in ustic moisture regimes with very prominent segregations of secondary carbonates, soils that are now classified as Calciustolls. At one time, someone from Europe went through Texas and told the Texans that these were Rendzinas and so this was accepted by the Texans and they started the argument about whether a Rendzina could have a calcic horizon. Many of theirs do and those that do not have a calcic horizon have distinct accumulations of secondary carbonates. There is no harm in making this proposal to the Soil Conservation Service but it will be disputed more than the proposal for the classification of the soils that have the low bulk density, the high infiltration, the cracks and so on, soils that do not fit comfortably into any family that now exists in *Soil Taxonomy*.

Question 33

The following questions have been collected from the members of the Venezuelan Soil Science Society.

Questions 1, 2 and 3 are very closely related. If the horizon is both massive and hard, the mollic epipedon is excluded, meaning that any degree of structure development in combination with hard consistency can be mollic.

Number 2, that to be mollic, it has to have at least a moderate structure when dry regardless of consistency. And three, the most common interpretation is that there are epipedons that are both hard and massive or very hard when dry, regardless of structure, these can not be mollic, umbric, or anthropic.

Guy Smith:

The intent of the exclusion of a horizon that is both hard and massive or very hard and massive from themollic epipedons was to exclude from Mollisols certain soils that have what people in the southern hemisphere commonly refer to as a hard-setting A horizon. These horizons are truly massive when dry, that is no discernible structure can be found. When moist, these horizons have at least a moderate, granular structure, or blocky structure depending on the management of the soil. We did not want to include these soils with Mollisols even though some of them are dark enough in the surface and have enough organic carbon to meet the requirements for Mollisols. Nevertheless, the problem of structure is so serious that these we preferred to include withAlfisols orUltisols, rather than with Mollisols, or Aridisols.

If the epipedon has an appreciable amount of clay, the individual peds are almost always hard or very hard, but these are not included within the meaning of both massive and hard or very hard when dry.

Question 34

What is the maximum depth at which the argillic horizon starts in Ultisols and Alfisols?

Guy Smith:

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

In a fewBoralfs, Paleboralfs, that are very stony and bouldery, the argillic horizon may not be discernible within this two meter depth, although the soil above is loamy-skeletal in its particle size. This is an unresolved problem in the classification of Paleboralfs.

Question 35

If the argillic horizon is lamellar, do we distinguish between pale- and haplic great groups using the same criteria as with a continuous argillic horizon?

Guy Smith:

In general the requirements for the pale- great groups are that the clay content does not decrease from its maximum by as much as 20% within the depth of 1.5 meters. If the argillic horizon consists of a series of lamellae the clay content in the inter-lamellar areas will almost always be 20% less than that of the lamellae themselves and we would interpret this generally to exclude the soil from a pale- great group and throw it into a haplic great group. While some semantic subgroups are provided in pale- great groups, the exclusion from the typic subgroup of the pale- great group requires that the particle size be finer than ... The typic subgroup is required to have an argillic horizon that is continuous horizontally, that is continuous vertically for at least the upper 20 centimeters and that has a texture finer than loamy fine sand. The soils getting into the pale- great groups then can be put into the semantic subgroups on the basis of the loamy, fine sand texture of the argillic horizon, rather than on the presence or absence of lamellae.

Question 36

How important is the ratio of fine clay to total clay in doubtful argillic horizons?

Guy Smith:

The definition of theargillic horizon stresses that the ratio of fine to total clay can be a guide to the accumulation of translocated located clay but it is not a requirement for an argillic horizon that there be any variation in this ratio between the argillic horizon and the underlying or overlying horizons. In my experience the ratios have been illuminating but the data are largely restricted to soil of late Wisconsin age. If one were to get more ratios on older soils, one might find that the ratio has little meaning. But in the absence of data, its impossible to make a definitive statement.

Question 37

Can we use the suffix "t" for subsoil horizons that show a large increment in clay and a high ratio of fine to total clay that have no clay skins.

Guy Smith:

The answer is that *Soil Taxonomy* does not use the suffix "t" as a diagnostic. A man describing a profile is permitted to put the "Bt" designation on a horizon if he believes there has been accumulation of translocated clay. This does not mean anargillic horizon, however, because the argillic horizon has other requirements than the judgement of the man who is looking at the particular soil. A single lamella in a sandy soil would logically be labeled a "Bt" but it would not substitute an argillic horizon.

Question 38

Why was the requirement for water-dispersable clay eliminated?

Guy Smith:

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

Question 39

The presence of soil structure or absence of rock structure in at least half of the volume of soil looked very drastic for the definition of acambic horizon. If this is compared with other requirements for the cambic horizon, do you not believe that in soils free of carbonates without reduction phenomena, it could be specified that the degree of structure development should be at least moderate?

Guy Smith:

If you read the various definitions in *Soil Taxonomy* you will notice that we have specified the absence of structure in some soils or the presence, but we have never specified a degree of structural development, weak, moderate and strong. We have not done this because

in traveling with other pedologists, we find that there are serious differences in opinion about the degree of structural development. It depends, first of all, on the moisture content at which you examined the soil and it also depends to a considerable extent on the background and experience of the men describing these soils. The Belgian pedologists who have worked mostly in the Congo, where structure is extremely weak in virtually all the soils, will put a moderate structure on any soil in which they can see any structure whatever. And one has to interpret their descriptions with great care
because......(end of tape)

Question 40

When can we consider the absence of rock structure? When can it be considered very weak or weak?

Guy Smith:

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

Aside: This is January 30 at Maracaibo. We are continuing with the questions asked by members of the Venezuelan Soil Science Society in 1974.

Question 41

The first question this morning is how can we explain the presence of plinthite in Alfisols and Inceptisols and so forth?

Guy Smith:

Plinthite is formed by the reduction, movement and segregation of iron oxides in a soil in the presence of a fluctuating water table. The iron can be mobilized and segregated much more quickly than many of the soil minerals can be destroyed by weathering or altered by weathering to kaolin and free oxides. We have plinthites in a number of parts of the world in which the mineral portion, in addition to the free iron consists ofweatherable minerals, even of calcium carbonate. In this situation we find the plinthite in the Inceptisols and the Alfisols. A new cycle of weathering can begin to remove the iron oxides from the plinthite leaving the matrix rather rich in weatherable minerals. It is a mistake to relate plinthite to the oxic horizon although by error in *Soil Taxonomy* we said that it is highly weathered. This was an error.

Professor Armand Van Wambeke has reported verbally to me that in his studies in the Amazon basin in Colombia, he found many Inceptisols with plinthite and even with petro-plinthite. The plinthite and petro-plinthite there were rich in weatherable minerals such as feldspars and micas. Professor Frank Moormann working in Southeastern Asia has reported to me verbally that he has found many areas with petro-plinthite which contain free carbonates in the interiors of the ironstone nodules.

Question 42

When can we consider that plinthite forms a continuous phase? Is it always necessary to consider several cycles of wetness and dryness to include it as plinthite?

Guy Smith:

Theplinthite is considered to form a continuous phase when the domains in the soil will harden on exposure of wetting and drying are interconnected, or that occupy more than half of the volume of the soil horizon. We do not know how many cycles of wetting and drying are essential to the identification of the red mottles as plinthite. We do know that in a number of instances the wetting and drying has hardened the plinthite into ironstone within a year's time. We do not know how many wetting and drying cycles occurred during this year but this has been observed in Trinidad and as far north as the state of Oregon in the United States. A pit, dug one year and refilled but leaving some of the plinthite at the surface, on reexamination a year later showed that the plinthite had hardened. In general the plinthite may be identified in areas where roads have been constructed because the grading for the road will leave some road banks in which the plinthite is exposed at the surface and in examination of an old road cut that shows no petro-plinthite or any hardening of the red mottles would indicate that plinthite was absent. There have been, since these questions were asked, some papers on field identification of plinthite in the *Soil Science Society of America Journal*, but I do not have these references in my head.

Question 43

What is the field difference between plinthite and laterite?

Guy Smith:

There's no necessary difference between plinthite and laterite. The later term is one that has been used by geologists for well over a hundred years and has acquired over that time many meanings according to the particular author of the paper that you are reviewing. The situation was so confused that we decided to abandon the term laterite and substitute plinthite by using Greek roots instead of Latin roots. Plinthite, then, is identical to some of the geologists' laterite. The "doughy" laterite, in particular, the petro-plinthite, the litho-plinthite are equivalent to other kinds of laterites as they are used by geologists and by many pedologists.

Question 44

Is exposure to the sun necessary for the hardening of plinthite or are there cases where the plinthite has hardened within the soil rather than at the surface?

Guy Smith:

There's a great deal more that I do not know about the hardening ofplinthite than that I do know. I can cite a few examples where the plinthite has hardened in a road cut that was facing the sun. In Brazil, this happens to be on a north-facing roadcut and the plinthite had hardened there but the south-facing roadcut it had not. So that is one suggestion but it's not really very good evidence. The other instances in which the plinthite has hardened have all been rather ambiguous. The hardening reported by Alexander and Cady in their bulletin on the hardening of laterites included a building in Africa in which the same building material, the same ironlike, plinthic material had been used for the wall of the house and for a sun-dial in the garden. Under the porch of the house, the plinthite had not hardened but the walls that were exposed had hardened and the sun-dial had hardened and this, they said then, meant that the plinthite required alternate wetting and drying, but it is also true that the plinthite under the porch which had not been wet and dried was also shaded from the sun so that one could ascribe the hardening to either the sun or the wetting or drying or a combination of the two.

Question 45

The question is what is the difference between litho-plinthite and petro-plinthite?

Guy Smith:

Litho-plinthite is a more or less continuous seam of iron-cemented material containing numerous tubes which are filled with clay material similar to those that underlie the litho-plinthic horizon. The water in the roots can penetrate through the tubes of the litho-plinthite and the type-section for that would be at the ecological station at Calabozo (?). The petro-plinthite is normally a gravelly material that has been transported. It consists of gravels that are cemented with iron and rounded by transport. It may occur at any depth in the soil, either one at Calabozo (?) the litho plinthite has, in places, been buried to various depths. In the literature throughout the intertropical regions, there are reports of stonelines that consist largely of rounded petro-plinthite. Now the petro-plinthite is then a gravel. The litho-plinthite is more like a rock.

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

We have relatively little plinthite in the United States and *Soil Taxonomy* is strongly biased by the soils of the U.S. because the appropriation for the Department of Agriculture are more or less precluded our doing any work in intertropical regions with USDA money. But within the U.S. where we have soils with relatively small amounts of plinthite in the subsoil, the horizon containing the plinthite acts like a pan in that it stops water. The water perches on top of that horizon, the roots do not enter it. It behaves just as a fragipan in the soil. The soils are not as well-drained as those without the plinthite and the trees growing on the soil tend to be quite shallow-rooted so that a strong wind will overturn the tree. Whereas in the soils without the plinthite, a hurricane that overturns the trees on the soils with plinthite will break the trees on soils without the plinthite, but does not blow them over. Because of this behavior of the soils that had small amounts of plinthite in the subsoil, we made the genetic assumption that larger amounts would be more important and the committees now, the international committee under Dr. Moormann, which is considering the classification of soils with low-activity clays has had to consider the relative importance of the plinthic great groups in the Alfisols and Ultisols. They have had considerable debate on this subject without reaching any real unanimity of opinion, but in the last circular letter, which is addressed in their report, the have retained the plinthic great groups. The plinthic subgroups in the intertropical regions that I have seen do not seem to have the same behavior as plinthic subgroups in the U.S. in that I do not find any evidence that the roots of plants are unable to enter the horizons with either small or large amounts of plinthite. This is a feature that so far, in my experience is restricted to the United States.

Question 46

Can you explain the difference between Aridisols and Alfisols when the moisture regime is aridic but marginal toward ustic and the epipedon is massive and hard or very hard when dry?

Guy Smith:

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

I said that we use the nature of the epipedon in an attempt to eliminate the need for the mapper to decide about the moisture regime and I did not say that this was entirely successful. The Australians have reported to me verbally somewhat similar situations where their Paleargids do not have a soft-structured epipedon. There's probably considerable need for reexamination for this criterion and there is now an international committee reexamining the classification of Aridisols. I would prefer that you should take this up with that committee and you will get some support from the Australians in trying to find another solution for the marginal cases, then. In this situation of yours and in the Australian situation the moisture regime is not marginal to ustic at the moment. It's clearly aridic, and I personally, never having seen these soils have no suggestion as to what modification in the definitions might be needed, but it seems clear from the verbal reports that I get that some modification is required in the definitions of the Alfisols, Ultisols, and Aridisols.

Question 47

Why were two millimhos, rather than 4, used to differentiate between Aridisols and Inceptisols?

Guy Smith:

This limit on conductivity was introduced in an effort to provide a field criterion that could be used for distinguishing theOrthids from theInceptisols? It has not worked, whether we use two millimhos or four millimhos the use of conductivity to make this distinction breaks down whenever the soil is irrigated. There are large areas in the middle east, in the Rio Grande Valley, in Texas and in southern California where Inceptisols are irrigated, the conductivity may become quite high and I proposed when I was here in Venezuela that we drop all reference to conductivity to distinguish between Inceptisols and Aridisols.

It is pointed out that there are some soils in which the conductivity is appreciably higher than 2 or 4 millimhos, up to 12 millimhos. In this situation, the salinity is a limitation for many crops and these still must be included in Inceptisols if the requirement for conductivity is dropped completely. This brings us in to the use of conductivity in the taxonomy which has avoided the use of conductivity everywhere except this one place. Elsewhere, the salinity is used as a phase rather than as a taxonomic differentiae. We have kept the use of salinity to the phase level outside of the taxonomy deliberately for two reasons. One was the precedent in the mapping in the United States in which salinity was used as a phase for soil series and if salinity were introduced as a taxonomic differentiae, theseseries would have been split and this was in general considered a very serious thing to split a series. The other is that the salinity under irrigation is quite variable according to several features. One is the quality of the irrigation water. One is the length of time that has passed since one has gone through a leaching cycle, and one is the overuse of water so that soils become water-logged and the salts come up by capillary rise. The conductivity of the irrigated soils is an extremely dynamic feature of the soil and is dependent on the water and the irrigation practices. If then we introduce absolute limits on conductivity into the taxonomic classification, we have soils that will shift with each leaching cycle from one taxon to another and so the theories will have changed every time the soil is leached and this seems to us to be irrational and this is why we have kept salinity out of the taxonomy itself, but it is extremely important and must be used as a phase. Our interpretations are always made for phases of taxa, not for the taxa.

Question 48

Why were the requirements of a mollic or umbric epipedon in the Eutrustox eliminated?

Guy Smith:

To the best of my recollection, this was eliminated because we found soils in Puerto Rico, in particular, which would be misclassified if we required the umbric or mollic epipedon. Some of them have chromas that are too high for either epipedon. And yet they have high base saturation and if we retained this provision we would have had to establish another great group for the soils that had the high base saturation. We did not want them with the Haplustox which have much lower base saturation and the general principle is that we want our definitions arranged to group the soils so that we can make the largest number of the most important statements and these, for the soil survey purposes, are our interpretations.

Question 49

Have you considered the possibility of grouping the former "lateritic sands", at present mostly in Psamments, separately at a higher level from the recently deposited sands with low weathering rates?

Guy Smith:

It was desired to keep together the loamy sands and sands without distinctive horizons such as spodic horizons or argillic horizons in one suborder because of the very common problems in the sands of low moisture-holding capacity, blowing, poor trafficability when dry and a number of other common properties. These are important properties to the uses of the soil and we thought that keeping them in similar taxa or closely related taxa would permit us to make the most important statements. We made provision we thought, at two categoric levels: the family and the subgroup, to distinguish these soils from others that were included with Psamments. This proposition or question includes the assumption that the coarser Psamments are recently deposited, but this is not the situation of the sands from the Kalahari desert in Africa have drifted far to the east and north in relatively ancient times, back in Pliocene times. These are not recent sands and so we provided for an oxic subgroup of the Psamments as well as a psammentic subgroup of theOxisols. That gives us the central concept of the Oxisols and of the Psamments and one intergrade in each direction which is the maximum we can get without establishing a new great group. If we consider the coarser Psamments that are in uncoated families at the moment, some of these are very ancient soils and some are very recent soils. The age seems immaterial in the coarser Psamments in so far as the possibility of weathering of the coarse, is virtually nil and they may be of recent origin as on the coastal dunes in Florida where the wind and the waves are bringing up coarse sands and depositing them as dunes along the coast. These would be our Typic Quartzipsamments with an uncoated family. Going inland a bit, into Florida, we have the older sands, some of which are uncoated and are considered Typic Quartzipsamments. The probabilities are that certainly many of these have a spodic horizon at depths greater than two meters. Where there is no spodic horizon it is quite common but not universal to find coated families and in these families, we for the most part have the subgroup of Oxic Quartzipsamments. This surely is the case in much of southern Zaire where sands are very extensive and where there is nothing weatherable except, well there is only quartz and free oxides, but they're coated and belong in the oxic subgroup. It seemed to us that this was a high enough categoric level to deal with the coated sands. If there are difficulties of interpretation, certainly then, we would want to consider the possibility of another taxon somewhere in the system.

I should like to add that we have had laboratory problems in applying the definition of the oxic subgroup of Quartzipsamments. We have in the soils in Zaire analyses of the clay fraction and we find there nothing but iron oxides, kaolin and quartz and yet the measured CEC's relative to the measured percentage of clay is 20, 25, 30 milliequivalents. This is a laboratory artifact of some sort, it's not the nature of the soil. Similarly with some of the more sandy Oxisols where we had a provision that required 16% or more clay in the oxic horizon we have another laboratory artifact. We assumed there was no silt in such soils of any consequence, but in the laboratory as a result of this version a good bit of the coarse sand is broken down to silt and so we come out with measured sandy loams that have less than 16% clay. The proposal was made to the Soil Conservation Service as a result of the Zaire data that we drop the reference to the cation exchange capacity of the clay fraction and substitute the mineralogy of the clay fraction in its place. And as a result of the Venezuelan data we proposed that we permit Oxisols to oxic horizons to have less than 16 % clay if they have a sandy loam texture. These proposals have accumulated in the Soil Conservation Service, but I believe now that they have one man who is responsible for soil classification that we will begin to see approvals of these proposals.

Question 50

What are the reasons to stress the word fragments of diagnostic horizons without discernible order and what are the requirements of Psamments?

Guy Smith:

We won't answer this question because it makes assumptions that are invalid.

Question 51

Asks for useful criteria to describe soil structure at different moisture conditions and determining the moisture regime in the transitional areas between udic and ustic or ustic or aridic.

Guy Smith:

I group these questions because they have the same answer. That is "no", I can not.

Question 52

When can we consider a lithological discontinuity taking into account the percentage of clay and silt?

Guy Smith:

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

Question 53

Question 23 concerns the recognition of a cambic horizon in a soil with anaquic moisture regime.

Guy Smith:

The present definition requires that the carbon decreased regularly and reached levels of less than 0.2% at a meter and a quarter before a cambic can be recognized. At one time in one of the earlier supplements to *Soil Taxonomy*, we had a Fluventic Aquept subgroup. This was objected to by particularly the Dutch and it was eliminated. They solved their problem by transferring it to Venezuela and if we reinstitute that subgroup and move the soils then from Entisols toInceptisols, we transfer the problem from Venezuela to The Netherlands. So in New Zealand we had the same problems that you have here in Venezuela with the Aquepts and the Aquents. In searching for an alternative to the use of carbon as an indication of alteration, I propose that we use the presence of 0.25% or more of iron manganese concretions that were cemented strongly enough that they would withstand normal laboratory dispersion or disaggregation. The distinction between a mottle and a concretion is a rather vague one unless one has some sort of an operational definition. This distinction seemed to solve the problem in New Zealand and I'm hopeful it will solve the problem in Venezuela, but I have not seen any data yet on the numbers of iron-manganese concretions in the soils that are here referred to.

Question 54

The question concerns Haplustalfs that have a moisture regime that is marginal to udic and that also have less than 24 milliequivalents exchange capacity per hundred grams of clay. Some have carbonates, secondary carbonates, and others are noncalcareous to very considerable depths. The question is to weight the presence or absence of carbonates versus the moisture regime that is marginal to udic. In *Soil Taxonomy* they are all called Oxic Haplustalfs.

Guy Smith:

There are two international committees presently working on the problems that have been brought up here. The one is the classification of theAlfisols with low-activity clay fractions. The other one is the committee ICCOMORT which is working on moisture and temperature regimes in intertropical areas. There is no question but that the present definitions which stress the presence or absence of secondary carbonates are not going to be applicable to the soils of Venezuela. When I was working in Venezuela, I made a proposal on the subdivision of the soils with usticmoisture regimes, with or without regard to the presence or absence of carbonates. Certainly the fact that the moisture regime is marginal to udic is much more important than the presence or absence of secondary carbonates. I proposed that we have subgroups of the ustic great groups in which we would have a central concept that would be used for typic subgroups, a udic subgroup and an aridic subgroup based on the length of the period in terms of consecutive days when the moisture control section was partly dry or wholly dry. Because this was a rather drastic change in the concept and really requires an additional soil moisture regime to distinguish the type of ustic regime that we have in Venezuela from the type of ustic regime we have in the United States. I made it as a proposal to be discussed, not one that was ready for adoption. The committee ICCOMORT under Professor Van Wambeke is considering this suggestion, I might say, rather than recommendation and they will eventually submit a report and recommendations on this. The other committee on the classification of Alfisols and Ultisols with low activity clays is probably going to propose some new great groups amongst the Alfisols and Ultisols, the Kandiustalfs and Kandiustults. Now if they have such a great group, then the nomenclature of Udic Kandiustalfs, Typic Kandiustalfs, Aridic Kandiustalfs will be greatly simplified. This seems to be about what they are proposing for the Ustalfs. The Kandiustalfs would have the clay activity less than 24 milliequivalents. The Kandiustults would have less than 16 milliequivalents. This committee, having worked for about 7 years is about to submit its final report in June of this year and I anticipate that their recommendations will be adopted. If they are adopted, then the use of carbonates to distinguish udic, ustic and aridic subgroups and ustic great groups will disappear completely. It has certainly little validity even in the United States we have udic, ustic, and aridic subgroups of Ustalfs are all in the same neighborhood and have all the same potentials for production of plants.

Question 55

The question concerns the classification of soils developed in the Andes to the west of Maracaibo with about 2400 millimeters of precipitation, a mean annual temperature of about 24 degrees Celsius, and about 4 to 600 meters above sea level, developed in or on limestone. The moisture regime is estimated to be udic. The country is hilly. The slopes range from about 20 to 25%. There are two soils classified as Argiustolls and Argiudolls respectively. The first occur generally on the higher parts of the slopes and the Argiudolls occur in the depressions with respective intergrades between these. The first question concerns the possibility that both soils occur in the same landscape with the characteristics of the precipitation mentioned earlier under tropical conditions.

Guy Smith:

It is always possible that the two soils may occur in the same general landscape with the more humid soils in the depressions and the drier soils on the hills. There is, normally, runoff and seepage from the soils on the hill to the soils on the footslopes and in the depressions so that normally we expect more humid conditions in the depressions than we do on the hills. The tropical conditions would seem to be irrelevant here because the same kinds of moisture differences occur in temperate regions as well.

Question 56

The second question, in *Soil Taxonomy* on page 299, the distinction between the great groups of Mollisols requires within the Calciustolls all the subhorizons above thecalcic or petro-calcic horizon are calcareous, after the surface 18 centimeters are mixed. At what value or percentage does one consider these horizons calcareous?

Guy Smith:

The percentage of carbonates to make us consider a horizon calcareous is not specified in *Soil Taxonomy* but rather we do specify that there must be effervescence when hydrochloric acid, cold hydrochloric acid is added to the soil. The data that one gets sometimes from the laboratory seem to contain a systematic error in that a few tenths to 5% carbonates are reported in the soils that have a pH well below 7 in KCl and that do not effervesce in hydrochloric acid.

These are noncalcareous. The laboratory data sometimes must be questioned by the field men. The field men are inclined to accept laboratory numbers without question but they may not do this.

Question 57

The third point is that there are transitional horizons between the mollic epipedon and the argillic horizon which meet all the color and carbon requirements of the mollic epipedon but also present some clay skins. Which of the diagnostic horizons represent this transitional horizon? Is it defined by the characteristics of both or by the one which predominates?

Guy Smith:

In the discussion of themollic epipedon it was pointed out that it is not mutually exclusive of the argillic horizon, yet the same subhorizon may be both a part of the mollic epipedon and of the argillic horizon. So that one does not have a choice of either one or the other, but one may have both in the same subhorizon. A mollic epipedon may extend into or completely through and below a very well defined argillic horizon and the same horizon, in this situation, would be part of the mollic epipedon and would constitute the argillic horizon.

(end of tape)