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. Cornerma (Venezuela), and Dr. N. Ahmad (Trinidad). Staff of the Soil Conservation Service, particularly Dr. R. Arnold, R. Guthrie (formerly SCS) and J. Witty (Washington, D.C.); J. Nichols (Texas); S. Riegen (Alaska) and F. Gilbert (New York) also contributed to the interviews.

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

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

Interview by J. Witty and R. Guthrie

December 1980

Ghent, Belgium

Question 1

When should a new subgroup be recognized, versus expanding a related taxa to include the soils in question? Should there be some minimum extent requirement or guideline, and should this be tied to similarity or dissimilarity between the soils, especially in terms of interpretation? An example is the implied subgroup of Arenic Mollic Albaqualfs which was proposed recently. In this case it was decided to expand the limits of Arenic Albaqualfs to allow them to have a thin, dark surface rather than establishing the proposed subgroup. It was believed the interpretations between Arenic Albaqualfs and the proposed subgroup were so similar that a new subgroup was not warranted.

Guy Smith:

The answer to that in my judgement would be as follows: that if at the family level phases of family interpretation, there are no significant differences between the proposed implied subgroup and the established subgroup, then the definition of the present subgroup should be expanded to include both. If, however, there are significant differences of interpretation of phases of family of the proposed subgroup and of the established subgroup, then I think that we should recognize a new subgroup rather than expand the definition of the old one. The whole thing hinges on the interpretation of the family. If you need two families because the interpretations differ, then you must have two subgroups in order to be sure that you have the two families. Now, as for the extent minimum acreage, you are going to have some difficult decisions to make from abroad at least, where the man who proposes the subgroup has been working in an area without a soil survey, without a detailed soil survey then he will not know what the acreage might be. And if the acreage is very minor you can handle this distinction by phases. But if the area is considerable and the differences are important, you may want to establish the new subgroup, even for a smaller acreage, because the phases can get too complicated for the user of the survey to understand. You may not have a dozen phases, different kinds of modifiers to the series name as a phase or the family name as a phase, and understand what has been done, because one of the reasons that we introduced moisture and temperature into the taxonomy was to simplify the matter of naming of phases. Too many phases are very bad in your legend. It can get too long and too complicated for the understanding of the nature of the map unit.

Question 2

Oxidic mineralogy. What would be lost if oxidic mineralogy were deleted from *Soil Taxonomy*? Currently about 65 percent of the soils in the New England States meet the requirements for oxidic mineralogy, although they are officially listed as having mixed mineralogy. I believe a high percentage of the coarse A-textured soils in New York, Michigan, Wisconsin, and Minnesota also meet the requirements for oxidic mineralogy. We are also finding that many soils in Virginia and the southeastern States, that were thought to have either mixed or kaolinitic mineralogy, really have oxidic mineralogy. The current limits for oxidic mineralogy do not seem to be very meaningful for most of these soils. Attempts to change the limits have not been very satisfactory either.

Guy Smith:

The original intent of the oxidic mineralogy was to separate the soils that have enough free oxides to form a more or less complete coating of the oxides on the clay. These soils have an appreciable variable charge or pH dependent charge, and it was thought that there were the two reasons for the separation: (1) that the variable charge would be more or less distinguished from the soils with a permanent charge, and (2) that in general, there are many fewer problems of soil structure in soils that have oxidic mineralogy. The normal Alfisol or Ultisol will form a crust after cultivation, as a result of the first heavy rain. The soils then with oxidic mineralogy have a much more stable structure in the plow layer, and we wanted to make this distinction. The definitions of taxa of higher categories is for the rhodic great groups, and subgroups were made because of the distinct difference in the tendency to crust when cultivated, and the oxidic mineralogy then makes some break within these rhodic great groups and subgroups; most of them are oxidic; a few turn out not to be. Rather than drop the oxidic mineralogy, I would think it better to put some sort of a limit on the minimum clay content at which the oxidic mineralogy is used. For example, require that the particle size class be loamy or clayey rather than permitting sandy soils to be included in the oxidic mineralogy, or you may have only 3 or 4 percent clay, then the significance of the iron is greatly reduced. I should also say, I do not think I know enough at this point to have a very firm opinion on the utilities of the oxidic mineralogy in loamy and clay soils. There should be some examination of your data in the U.S. to see where, if you restrict oxidic mineralogy to finer textures, where that restriction should be placed.

Question 3

Slope or shape of soil - page 389

It appears that the sloping family class has not been used consistently. Currently only three sloping families are recognized, and all are Aquolls. It seems that most people prefer to recognize slope as phase criteria rather than family criteria.

Guy Smith:

It would be a little slow in accepting a proposal to eliminate the sloping families of the aquic great groups. The differences in normal sloping phases are not so much in the nature of the soil as in the hazards of erosion. The differences in these sloping families are not concerned with erosion, but are concerned with the difficulty of removing the surplus water, almost the impossibility of removing it, and the genetic differences in the ground water levels. The normal users of the soil surveys have associated sloping phases with the problems of soil management related to erosion. They could easily be confused by the use of the sloping phase where the problem is almost completely another problem, one of drainage. The differences in the genesis, of course, are related to the fact that the water in the sloping phases is coming from seepage, rather than from the rain that falls directly on the soil. The *Soil Taxonomy* states that they should not be used in Aquods where in many soils the wetness is due to a placic horizon, or in the Albaqualfs, where the intent was to keep the old clay pan Planosols together. I should also comment that I think it would be desirable in the case of the Histosols to use sloping families as well as in the Aquolls and the Aquults. Whether or not sloping phases of Aquults exist, I do not know at this moment, I have not myself seen such.

Question 4

The International Committee on Low Activity Clays is proposing a new diagnostic horizon, the fine-textured subsurface horizon. It seems that if acceptance is given for the new diagnostic horizon, that for some (and maybe many) soils there could be strong disagreement among soil scientists as to whether a soil has a fine-textured subsurface horizon or an argillic horizon. For classification purposes a distinction, apparently, is not needed.

Guy Smith:

The original proposal to recognize the fine-textured subsurface horizon as a basis for placing a soil in a Paleudalf or a Paleudult was the difficulty of getting agreement amongst different pedologists as to whether or not there was an argillic horizon. The proposal was to put into the definition, then, of Alfisols and Ultisols this distinction in texture with depth, as being the equivalent of an argillic horizon, so that no decision would be needed as to whether or not there was an argillic horizon in a particular soil. This reason is one that was suggested it should not be recognized as a diagnostic horizon, but as a diagnostic feature, perhaps, but certainly not a diagnostic horizon. So that a soil might have an argillic horizon and have this fine-textured subsurface horizon and no decision would be necessary then, as to whether or not that horizon was or was not an argillic horizon. This was only proposed for use in the low activity clay soils and nowhere else in *Soil Taxonomy*.

Question 5

Is there a good logical reason why the definition of the argillic horizon should not be expanded to include the concept of the fine-textured subsurface horizon?

Guy Smith:

Well, of course, there is. There are many soils with lithologic discontinuities where you have a coarser surface deposit on the finer subsurface layer. Mostly these occur in alluvium, but occasionally you find them in soils developed from rocks of very contrasting mineralogy and particle size distribution. If you extend this definition generally, then, to all soils, by including this concept in the definition of the argillic horizon, you will then put into argillic horizons all kinds of stratified parent materials, such as the alluvium along the Mississippi, (Fluvents if you please) where you have a layer of sand over backwater clays. And you don't want to do that; you don't want to make an argillic horizon out of a stratification of parent material. This proposal was restricted just to soils that could have an argillic horizon, but where there was a question about whether or not this subsurface horizon was an argillic horizon or was not.

Question 5b

I would like to make a comment on the item concerning including stratified material such as alluvium. I was thinking, in a case like this, that we would put disclaimers in to exclude those soils that have irregular decrease in organic matter.

Guy Smith:

I think that is going to give you some extremely complicated definitions. Many argillic horizons have more carbon than the horizons above, and so this disclaimer on the irregular decrease of organic matter is going to throw out all your Albaqualfs, because the argillic horizon normally has more organic carbon than the overlying albic horizon. Such a change would result in some very complicated definitions that are extremely difficult to understand.

Question 6

How much documentation should be required, and what should the procedure be for accepting amendments submitted by our foreign colleagues?

Guy Smith:

In general, I think that we should require a description of at least one pedon, a description of the extent of the soils that require separation, laboratory data on at least one pedon or on the critical parts of the diagnostic horizons that are used to propose new taxa. I think that there should be some discussion of the significance of the separation to the interpretations that might be made, and why a new taxon is required rather than a phase. The problem might be illustrated a little bit by the soils I found in New Zealand, which would have been Dystrochrepts except that they had a rather shallow placic horizon. No provision is made in taxonomy for such a soil, but the importance of the placic horizon to the use of the soil is very considerable, and its existence is indisputable either from the field description or from the laboratory data that have been acquired. The proposal, then, should include the data, the description, the differences in interpretations from other Dystrochrepts that do not have a placic horizon because those are freely drained, those with placic horizons are commonly quite wet, and the management of these soils, either for cultivation or grazing, is quite different for the soils with and without placic horizons. If the soils under discussion are not known to occur in the United States, I believe the approval could be given rather readily, perhaps following the discussion with the principal correlators to confirm the absence in the U.S. If they are willing to say they do not know of such soils, then I think the decision to approve or disapprove should be made by the Washington Office people working in soil classification. If the soils do occur in the United States, we originally proposed that the suggestion should be sent to the principal correlator and discussed at the regional work planning conference. The approval should wait on the discussion at the regional work planning conference.

Question 7

The definition of a buried soil is different in *Soil Taxonomy* than in the Soil Survey Manual. For many people, this is confusing. It seems that the definition of buried soils in *Soil Taxonomy* really defines a control section for the higher categories. Would it be appropriate, rather than defining a "buried soil" on page two of *Soil Taxonomy*, to define a control section for the higher categories?

Guy Smith:

It was assumed in the discussion of buried soils in *Soil Taxonomy*, that the buried soil was buried by a mantle of largely unaltered materials because we specify that it normally shows fine stratification; it would therefore be quite a recent deposit. We would find it on flood plains, say, where a dike has burst, or near volcanoes where there is a mantle of very recent ash or pumice, or in areas where dunes are moving across the landscape. These were the things we had in mind. It is certain that the discussion can be improved considerably to draw the line on what is largely unaltered. The presence of an argillic or a spodic horizon would seem to be clearly eliminated. The presence of a very weakly developed cambic horizon of course, could be tolerated as a part of a recent mantle, because we surely can develop the cambic horizon in places, given the proper environment, in something like a matter of a hundred years or so. The definition of a buried soil in the Soil Survey Manual is really a statement that the man who is describing the soil makes the assumption that the material at the surface is of another age than the underlying material, and that the horizon, then, in the underlying materials are indicated by the subscript little "b" in the horizon designation. This is completely undefined, stated clearly that this is the interpretation of the man describing the soil, and that the confirmation of his interpretation may later require laboratory analyses to validate his opinion at the moment that he describes the soil. The intent was to include only those mantles that had no diagnostic horizons other than a ochric epipedon, and many would hardly have that if they were finely stratified. It would have no epipedon, in fact. We had in mind materials that were that recent. The definitions, say of Inceptisols, Entisols, state that they have no argillic horizon unless it is a buried horizon. The thought was that the new material would be new enough, recent enough, that there would be no diagnostic horizon and that the buried soils would occur only amongst Entisols.

Question 8

Four series are classified in Arenic subgroups of Haplargids. They all supposedly have a sandy epipedon more than 20 inches thick with the argillic horizon below this depth. I am not aware, however, of an argillic horizon actually forming at such a great depth in Argids, but they could be buried by aeolian sand. What guidelines can be used to distinguish between Arenic Haplargids and Torriorthents with a thick sandy surface and a buried argillic horizon (buried soil), or is it an error to recognize Arenic Haplargids?

Guy Smith:

My experience with Aridisols is quite limited. I cannot be sure of any answer to this question at the moment. It is possible that one can have a genetic sequence of horizons of a thick sandy epipedon overlying an argillic horizon, and an aridic moisture regime, if the soils formed under a higher rainfall than they have today. They would not necessarily qualify as a Paleargid because there might be no petrocalcic horizon, and the sandy nature of the soil eliminates the soil from Paleargid because of the low clay content. Paleargids have either

petrocalcic horizons or 35 percent or more clay in the argillic horizon. so these could be polygenetic soils, although since I know none of these four series, I am not able to answer this question adequately.

Question 9

A paralithic contact in combination with vertic properties is subgroup or implied subgroup criteria for selected great groups. What is the significance of this combination of properties to recognize a paralithic contact at the subgroup level, when mostly it is recognized only at the family level if the contact is at a depth of less than 50 centimeters?

Guy Smith:

The significance of this criteria is similar to most of the others that we have in *Soil Taxonomy*. Namely, we want groupings of soils that belong together because of similar behavior reflecting similar properties. We have this group of soils that we wanted to keep in a single taxon instead of splitting into two or more. It happens that some of these soils have a mollic epipedon, and some do not, but the epipedon in all of them is close to the margin between a mollic and an ochric epipedon. It is a natural unit in the landscape that should not be split because of a difference, perhaps, of one centimeter of thickness in the dark-colored part of the epipedon. Because the intergrades to Vertisols are intergrades to soils which very commonly have a mollic epipedon, it did not seem unreasonable to permit the mollic epipedon in the subgroup that intergrades to the Vertisol, and by this combination of characteristics, we keep all these soils together, even though one pedon has an ochric epipedon and the next one has a mollic epipedon.

Question 10

Why is the petrocalcic horizon recognized at the subgroup level when most other similar root-limiting features (pans) are recognized at the great group level?

Guy Smith:

In a sense, the petrocalcic horizon generally is a part of the definition of the great group. For example, in the Paleargids and the Paleorthids a Petrocalcic horizon is one criterion for classifying the soil in the Pale great group. we then had in the Paleargids, for example, two kinds of soils: one with an abrupt textural change between the A and the B and the other that had a Petrocalcic horizon. All these soils were classed as Paleargids because we thought they were all polygenetic in the sense that they had gone through one or more glacial and one or more interglacial periods. Therefore, since we were grouping these soils with and without petrocalcic horizons in a single great group, we had to separate those at the subgroup level rather than at the great group level. We avoided at least one additional great group in the Paleargids. The Paleorthids are defined in terms of having a petrocalcic horizon, although the name Petrocalcic does not appear as a formative element in the name of the great group.

It would, of course, be perfectly possible to define the Argids having a petrocalcic horizon as a separate great group from those that do not have one. At the time that we were writing *Soil Taxonomy*, this possibility either did not occur to us, or we were trying to be economical in the numbers of great groups that we established.

Question 11

Eroded Mollisols. Extensive areas in the Midwest have soils that are classified as "Mollisols" but have lost their mollic epipedon through erosion. There is great resistance to reclassify these soils as Alfisols, Inceptisols, or Entisols--depending on the diagnostic horizons that remain. Many years of effort have been put in, to try and develop criteria that could be used to keep these soils as Mollisols, but all efforts have failed. Keeping with the philosophy of *Soil Taxonomy*, is there any other realistic option than to establish new series for these eroded "Mollisols," and classify them based on the criteria in *Soil Taxonomy*? Most of these soils are now being correlated as taxadjuncts or a typical pedon is selected from a spot on the earth's surface that has a mollic epipedon.

Guy Smith:

The philosophy of *Soil Taxonomy* is that a soil should be classified on its own properties, and not on those that are presumed to have existed at some time in the past, and not on the properties of adjacent soils. The use of the mollic epipedon to group the grassland soils of the great plains was unavoidable with the knowledge that we had of those soils at the time we developed *Soil Taxonomy*. We did state that we preferred to use subsurface horizons for the definitions of the higher categories because these would be the last horizons to be removed by erosion. There was, however, no criterion that we could find to retain the grouping that existed in the previous classification which called these soils dark-colored soils of the subhumid and humid grasslands. The possible alternative would be to find some characteristic that was common to Mollisols and was not found in other orders besides the mollic epipedons. I do not know what this might be. An alternative approach might be to recall that we are not classifying pedons, but we are classifying polypedons. The pedon is merely a sampling unit of the polypedon. The vast bulk of the eroded areas of Mollisols will have a mollic epipedon as well as pedons that do not have a mollic epipedon. In classifying these soils as Mollisols, when the mollic epipedon has been removed in places, perhaps most places even, it might be possible to write definitions such that when applied to a polypedon the presence of these less eroded areas would be considered justification for putting the soil into the Mollisol order. This will require some study in the field, and there was no time to do this while *Soil Taxonomy* was being written. This question has been bothering the soil scientists of the Midwestern States for many years, and we attempted at one time to get a study in Iowa of these soils with statistical controls, and somehow or other we never were able to find funds and personnel to do it.

Question 12

Soils with frigid temperature regimes in California have winter precipitation in xeric patterns, but soil moisture calculations indicate a udic moisture regime. Is the udic moisture regime consistent with the intent of *Soil Taxonomy* for these soils?

Guy Smith:

At the time *Soil Taxonomy* was being developed, we had very little information about the soils that have a xeric moisture regime and a frigid temperature regime. We gave priority to temperature over moisture where the soils were cold enough that the temperature was a limiting factor. We thought that it was more simple to change the soil moisture through irrigation in dry soils than it was to change the soil temperature. We know of no way that the temperature can be altered appreciably. Therefore, the soils that were frigid or cryic were grouped into Boralfs. The Xeralfs that were frigid were left as Xeralfs because we actually have no knowledge of their use and managem .. This may have been a mistake, and it may well be that the definition of Boralfs should include soils that have winter precipitation, but that are cold enough that temperature is more significant than lack of rain in the summer. The soils in question now appear to have winter precipitation, but do not become dry throughout in the summer for a long enough period to be xeric.

Question 13

In the absence of measurements of the number of days that cracks are open in Vertisols, what is the best guide to classification in the suborders and subgroups?

Guy Smith:

I should mention that in Venezuela, in trying to classify the Vertisols at the suborder level, there were no records or measurements of the length of time that the cracks were open to 50 cm depth. I solved the problem by discussing the presence of cracks with the cultivators, and they could give me the average date that these cracks appeared, and the average date at which they closed. There is much common knowledge among cultivators that is better than we're ever going to get in terms of actual measurements. Soil moisture regimes were not used in the classification of Vertisols because the moisture control section is relatively meaningless in a soil that cracks. We used as a substitute the period that the cracks were open and the number of times that the cracks opened and closed during the year. It was the intent to define the periods of cracking in such a way that we would have Usterts associated with Ustalfs and Ustults. Whether or not we succeeded with the periods will depend on measurements of at least a few soils to guide us in the classification at the suborder level.

Question 14

One of the requirements for Typic Haplorthods is that they (page 346, item c) "do not have distinct or prominent mottles of approximate spherical shape in the spodic horizon unless the variability in color is associated with differences in consistence in such a manner that the redder or darker parts are extremely firm or very firm, or, if the color is due to un coated sand grains, do not have the water table within 1 m of the soil surface for as many as 60 days cumulative in most years." My question is, what is the intent of the phrase "if the color is due to un coated sand grains?" It refers to the spodic horizon in which the sand grains are un coated.

Guy Smith:

In many Humods in Europe, the upper part of the spodic horizon consists of un coated sand grains surrounded by black fecal pellets. The sand grains in the lower part of the spodic horizon of the Humods are normally coated wherever there is a measurable amount of free iron. But in the upper part of the spodic horizon, the free iron is lacking, and the spodic material consists largely of fecal pellets which do not coat the sand grains, but merely surround them.

Question 15

Another requirement of Typic Haplorthods is that they (page 346, item i) "have 1.2% or more organic carbon in the Ap horizon if the Ap horizon extends into the upper part of the spodic horizon." What is the significance of this requirement? (Typic Fragiorthods have a similar requirement).

Guy Smith:

There are many Haplorthods in the U.S. in which the albic horizon was so thin that when they were plowed the albic horizon disappears, and the upper part of the spodic horizon is mixed into the plow layer. One of the other requirements of Typic Haplorthods is that there is a minimum content of organic carbon in the upper part of the spodic horizon. The intent of this item is to provide for the cultivated Spodosols that have had at least a part of the spodic horizon mixed into the plow layer.

Question 16

Many places in *Soil Taxonomy* there is reference to a specific depth. It is not always clear as to whether the measurement is made from the mineral soil surface, or from the soil surface which might include an O horizon. Examples follow:

1. For frigid or warmer temperature regimes, criteria for a spodic horizon must be met at a depth below 12.5 cm. Should this measurement be made from the "soil surface" or "mineral soil surface?"

2. On page 337, for Lithic Haplaquods it is specified that the measurement is from the "mineral soil surface."

3. On page 346 for Lithic Haplorthods the measurement is made from the "surface."

Was the intent to measure from the soil surface, including any O horizon, unless it was specifically stated to measure from the mineral soil surface?

Guy Smith:

The general intent was that the O horizon would not be included in the depth measurements. The O horizon is transient and may be destroyed by fire, which would then

change the classification of the soil overnight, even though the O horizon will reform within a few years. It is normally not feasible to include an O horizon in the definition for depth unless the O horizon is thick enough that the primary rooting zone in the soil is in the O horizon. In such soils the climate is normally so cold and humid that there is no particular hazard of fire destroying the O horizon. And one of the unresolved questions is what to do about an O horizon that is perhaps 50 cm or more thick overlying a mineral soil with normally rather well developed spodic horizons, when the rooting of the plants is almost entirely in the O horizon.

Witty:

I might mention that there is relatively extensive areas like that in the Adirondacks where there is a thick O horizon. Most of the rooting is in the organic layer, and I have seen them up to a meter thick up there, and then below you have what appears to be well developed spodic horizons with no roots.