CONVERSATIONS IN SOIL TAXONOMY
(ORIGINAL TRANSCRIPTIONS OF TAPED CONVERSATIONS)

by

Guy D Smith

Compiled by an editorial committee at

The Agronomy Department of

Cornell University for the

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Addendum to:

THE GUY SMITH INTERVIEWS: RATIONALE FOR CONCEPTS IN SOIL TAXONOMY

by Guy D. Smith

Edited by

T.R. Forbes

Reviewed by

N. Ahmad
J. Comerma
H. Eswaran
K. Flach
T.R. Forbes
B. Hajek
W. Johnson
J. McClelland
F.T. Miller
J. Nichols
J. Rourke
R. Rust
A. Van Wambeke
J. Witty

Soil Management Support Services
Soil Conservation Service
U. S. Department of Agriculture

New York State College of Agriculture and Life Sciences
Cornell University
Department of Agronomy

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Preface

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

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

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

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

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

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

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

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

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

Interview by H. Eswaran

December 1986

Ghent, Belgium

Interview of Guy D. Smith with H. Eswaran, December 4, 1980

Question 1 from Dr. Eswaran is:

As a historical introduction, when did the concept of Oxisols emerge? It was probably the outcome of the earlier Latosols and what were the compelling reasons for the big change in concepts?

Guy Smith:

The concepts of Oxisols emerged rather gradually in the earlier approximations. At one time, we separated the soils in the highest category according to whether or not they had no horizons, they had an horizon that was very distinct or they had an horizon and a B horizon, or currently a cambic horizon, or they had a B horizon of accumulation of clay or of amorphous materials. In the *Sixth Approximation*, we adopted the concepts of the diagnostic horizons rather than the A, B, C horizons. The very strongly weathered soil horizons that we have in Oxisols were recognized as a special kind of B horizon, using the concept of the Latosols of Dr. Kellogg, (reference to be added). This concept was very similar to that of the present concept of the oxic horizon. There was no big change in concepts, there was only a change in the application of the concepts. The soils with high variable charges developed from pyroclastic materials in Hawaii, were called Latosols. The only general common feature of soils that were called Latosols that I could discover, seemed to be that they occurred in intertropical regions. Nearly everything was called a Latosol in the soil survey of Hawaii published in (reference to be added). These included the present Andepts or proposed Andisols as well as the Oxisols and Ultisols. At the time that we were working on the development of the *Seventh Approximation*, we had virtually no data on the chemical properties of the soils of Hawaii other than their total analyses. The first concepts we had of the Oxisols, then, were a mixture of what we now call Oxisols and Andepts.

It took some years to straighten out these differences. We recognized early that we needed an order for a kind of soil such as the Nipe of Puerto Rico, soils that consisted of completely weathered materials. The Nipe would be a good example, I think, of Dr. Kellogg's concept of a Latosol, although his concept was broader than that. He included soils such as Nipe along with soils that have distinct argillic horizons. The original definition was in descriptive terms, not in quantitative terms. Dr. Kellogg spoke of low activity of the clay, but did not specify what that was, and what is low to one person may be high to another; it depends on their experience and training. In developing *Soil Taxonomy*, it was recognized early that definitions could not be uniformly interpreted if they were written in qualitative terms rather than in quantitative terms. Qualitatively what is high and what is low in any property depends entirely on the experience of the individual who is trying to classify a particular soil, and it was our goal that the definitions would be such that competent pedologists, using the same information, would arrive at the same classification of the soil.

Question 2 was answered in the discussion of Question 1.

Question 3

Laterites fared very highly in the early approximations. With successive approximations, they nearly faded into oblivion. Today, some kinds of laterites are considered in some subgroups. Can you discuss this evolution and give your personal feelings?

Guy Smith:

Laterite is a name which has been in use for years (citation to be added later). Over the many, many years, the meaning of the word laterite took on a very wide variety of meanings according to the individual who used the word. Laterites included what we now call plinthite, sesquioxide sheet, an acric horizon, and the literature about laterites is an extremely confusing one to read. As a consequence, we decided not to use the term in the later approximations, and we introduced the term plinthite and sesquioxide sheets in the *Fifth Approximation*, as substitutes. The first definition of plinthite included the domains in the soil that would harden on repeated wetting and drying and exposure, and the hardened relicts of that material. Subsequently, the term was restricted to the material that had not yet hardened irreversibly. At present, the plinthite name has been used as a formative element in two additional kinds of material: one, the nodular, hard ironstone, which has been called petroplinthite. This usually is a transported material, and occurs in the soil as stone lines. The other proposal for using plinthite as a formative element is for litho-plinthite, which is a material which has hardened irreversibly, in place, with a tubular structure which permits it to transmit water, and permits roots to penetrate through it. Plinthite has been used as a formative element for several great groups, in which the plinthite forms an interconnected matrix, or forms more than half of the matrix of some subsurface horizon. It is also used as a formative element in a number of subgroups, in which it is present in smaller amounts than in the plinthic great groups. The desirability of retaining the plinthic great groups has been receiving considerable discussion in the international committee on Ultisols and Alfisols that have clays of low activity. At this time it is impossible to predict what recommendations the committee will make on the use of plinthite in the classification. The plinthic great groups were established because we had little information about them in the United States, and the importance of laterite had been stressed so much in the literature. The plinthic subgroups were recognized in the United States because they are brittle when moist, and are slowly permeable to water, and nearly impermeable to roots. They behave much as does a fragipan. Plinthic great groups in intertropical regions apparently do not have this particular property, and there is no question in my mind but that some changes in *Soil Taxonomy* will be required to reflect these differences.

Question 4

As has been pointed out in *Soil Taxonomy*, some consider the oxic horizon as B, and others as a C horizon. In the recent Segalen's French approach, they prefer to consider it as a diagnostic material, which perhaps is also correct, as we are in the process of making changes, we would like to know the reasons for considering it as a diagnostic horizon, and would there be any advantages for considering it as a diagnostic material? I might add that in soils developed in situ on rocks, it is a horizon, but as in most Oxisols, which are developed on preweathered and transported materials, it is more a material.

Guy Smith:

We should, note in answer, that *Soil Taxonomy* has used the concept of the nature of the material of the soil at the order level, in some soils, as in Vertisols, and in some suborders, as in

Andepts. However, the general philosophy of *Soil Taxonomy* has used horizons to arrange and define the orders and has used other features, such as moisture regimes, nature of the material, and so on, at the suborder level. One could, as Segalen has proposed, use the nature of the material which forms the horizons, rather than the nature of the horizons themselves. This, however, has not been done. We have used the spodic horizon to identify the Spodosols. We have used the mollic epipedon as one identifying horizon of Mollisols. It is of no material consequence whether one considers the oxic horizon as a horizon or as material, because the horizon is defined in terms of the materials that compose it.

It would be very difficult for me to assert that most Oxisols are developed on preweathered and transported materials. It is true that the material must be physically weathered before it can be transported, but whether or not the oxic horizon has formed in materials which were weathered physically, or both physically and chemically, is currently only a matter of speculation, and cannot in any way be used as a part of a definition.

Question 5

In the humid tropics, particularly in the nonaquic soils, it is extremely difficult to differentiate an oxic horizon from a cambic. The only criteria that separates the two is "traces of weatherable minerals." The Picacho series of Puerto Rico, Dystropept, has more gibbsite and a lower charge than the Matanzas, a Tropeptic Haplorthox. The Picacho is an Inceptisol because it has some feldspars. Is this potential to supply nutrient elements so critical as to separate the soils at order level? Do you see the need for special kinds of cambic horizons for the tropics?

Guy Smith:

In considering the importance of a critical limit between orders, we must always keep in mind that, soils form a continuum, that there are intergrades between most kinds of soils that may go through other orders. In order to have a clear cut definition that defines the limits of a taxon, whether it is an order or a subgroup, we have to put the limit at a point which will divide the soils on either side of that point into different taxa. Thus the two soils which are very similar, one on each side of that limit, are separated. They are more like each other than they are like the other soils in the taxon. The gradational change from one soil to another is reflected in the names. The Picacho is an Oxic Dystropept, and the Matanzas is a Tropeptic Haplorthox, indicating that these are gradational between the two orders. If one were to change the limit of the percentage of feldspars, it would only shift the subgroup nomenclature to another series, and would not eliminate any problem whatever. I do not at the moment foresee the need for special kinds of cambic horizons in intertropical soils.

Question 6

Wet oxic horizons are frequently mistaken for cambic horizons, primarily because of a color difference with the "C", and an apparent better structure. This may be one explanation for the lack of Aquox descriptions. Do you see this as a real problem, and how can we rectify it?

Guy Smith:

I think, perhaps, the principle reason for the lack of Aquox descriptions is, for the small areas that they occupy in the world. The Aquox that I have seen have normally been small, polypedons, a matter of a few hectares at the most, and they are generally far apart in the landscape. They do exist, and a lack of description probably reflects the facts that their area is extremely small compared to the areas of the other kinds of Oxisols. I think that one would not have much trouble in identification of the Aquox if one finds a wet soil surrounded by other kinds of Oxisols. Its position in the landscape should be enough to guide the pedologist in his classification, even in the absence of any laboratory data.

Question 7

There are some numbers given in the definition of the oxic horizon. It will be useful to have some remarks on the origin of these numbers.

(a) 30 cm thickness.

(b) ECEC of less than 10.

(c) CEC 7 of less than 16.

(d) More than 15% clay (why not 18).

(e) 5% rock structure.

Guy Smith:

(a) 30 cm thickness

The minimum thickness of an oxic horizon was set with the notion that the oxic horizon was resting on some sort of saprolitic material. We have prohibited in *Soil Taxonomy*, a cambic horizon that overlies an argillic horizon because, it is really a transition between the epipedon and the argillic horizon. We had the same thought that a material that is transitional between the epipedon and the argillic horizon would not be called an oxic horizon; even though it has the properties of an oxic horizon, it is a transitional horizon, and so we put the 30 cm limit of thickness on the oxic horizon with the notion that it would not be a transitional horizon between an epipedon and an argillic horizon. We also thought that if the thin oxic horizon rested on saprolite, which either retains weatherable minerals or has rock structure, some minimum thickness was required. Otherwise, people would begin to find an oxic horizon that was one cm thick or a half cm thick, and the 30 cm comes from the notion that the oxic horizon should be thick enough to have some significant effect on plant roots.

(b) ECEC of less than 10.

The ECEC, which is the sum of bases extractable by ammonium acetate and the sum of aluminum extractable by KCl, was used in the definition of the oxic horizon because we felt it was easier to determine with precision than the CEC by ammonium acetate buffered at pH 7. Different laboratories frequently get what appear to be significantly different CEC's of the same horizon by ammonium acetate. The sum of bases plus the KCl-extractable aluminum can be measured, we think, with more precision than the ammonium acetate CEC.

The limit of 10 was selected because in the limited data that we had for soils of the United States, this was about the maximum that we could find in the soil that we thought belonged with the Oxisols. With more data from other parts of the world, it may be desirable to modify this number. While it was proposed for criticism, no criticism was ever received, nor were there ever any suggestions for changing the numbers. Therefore, what was proposed for criticism became a number that appeared in *Soil Taxonomy*.

(c) CEC 7 of less than 16.

The CEC by ammonium acetate of less than 16 was proposed again for criticism and was never criticized. The reason for the 16 was precisely the same reason as the 10 for the ECEC.

(d) More than 15% clay (why not 18).

The limit of 15% clay as a minimum for an oxic horizon, was proposed because we were concerned with a limit between Oxisols and Quartzipsamments, which may also be completely weathered. The intergrades then, or the limit was to separate an Oxic Quartzipsamment from a Psammentic Oxisol. We chose 15% clay on the assumption that material so completely weathered would have virtually no silt. In Venezuela, we have soils that have less than 15% clay, but have too much silt and clay to become Quartzipsamments. They, therefore, come out from the key as Entisols, although they are completely weathered, and may be very stable in the landscape. I do not like the idea of having an Entisol that represents really an intergrade between an Oxisol and a Quartzipsamment. This seems to me to be unreasonable, so I have proposed that, that limit be dropped completely, and that the limit between the Oxisol and the Quartzipsamment be set at the limit between loamy sand and sandy loam particle-size classes.

A limit of 18% would be a change in the wrong direction, because it would increase the area of soils of Entisols that lie between the Oxisols and the Quartzipsamments.

(e) 5% rock structure.

The limit of 5% by volume of rock structure in the oxic horizon was set to exclude from the oxic horizon, materials that were completely weathered chemically, but were not yet physically weathered. We want to restrict the Oxisol to the oxic horizon to a material that was completely weathered, or nearly so, both physically and chemically.

This limit was proposed for criticism, and never received any. Therefore, it has come on over into *Soil Taxonomy*. One can find a weathered basic igneous rock that has been completely altered, mineralogically. The primary minerals have all been altered, and yet it may be so hard that one must use a hammer to break it. We did not think that this material should constitute a part of the oxic horizon. It is not in any sense a part of the soil; it is, rather, the bedrock.

Question 8

The definition of Oxisols has created problems, especially with people from LDCs who go by the letter, as they frequently are not aware of the intent. The first problem is the classical question "Where does an argillic horizon end and an oxic horizon begin, or vice verse?" I like to take the classical situation in Malaysia. The pedon has an A1 of about 10 cm. A B1, which meets all the requirements of an oxic horizon and 40 cm thick, and this is underlain by B21t, B22t, etc. We happily called this pedon a Tropeptic Haplorthox until, during a recent Workshop, some experienced pedologist classified it as a Typic Paleudult. As the soils also

shows the clay increase for the argillic horizon with clay skins in the major part of the B. The "Kandi" concept of ICOMLAC will not solve this problem.

Guy Smith:

We have two precedents in *Soil Taxonomy* for handling this particular question, where the transition horizon overlies the argillic horizon, and has all the characteristics of an oxic horizon. The first precedent is that of the cambic horizon, which by definition may not overlie an argillic horizon, unless it is separated from it by an albic horizon. The other precedent is where we have a spodic horizon that overlies an argillic horizon. In this case, the horizon is not transitional, and the order is determined by the overlying surficial horizon, on the assumption that that represents best the present processes going on in the soil. In dealing with the material horizon that has the properties of an oxic horizon, but rests on an argillic horizon, it is possible to use either of these precedents. The limit of 30 cm thickness, mentioned under question 7, was set without thought that this would be a transitional horizon. In the discussions of ICOMLAC, I proposed that this limit be increased to 50 cm on the grounds that if it is that thick, the soil would behave more like an Oxisol than like an Ultisol. In this situation then, one could establish an ultic subgroup of Oxisols to separate soils with this horizon sequence at the subgroup level rather than at the order level.

Question 9

According to the definition, a spodic horizon is not permitted to lie over the oxic horizon. Can such a situation occur in nature? If not, why does the statement appear in the definition?

Guy Smith:

I have seen in the Amazon, soils that have a spodic horizon overlying what appears to be an oxic horizon, though I have no data on the soils in question other than my visual and manual observations. The soil in question probably at one time was an Ultisol, with a rather thick epipedon of a loamy sand or sand texture. With great age, the argillic horizon seems to have been degraded into an oxic horizon, but the thick sandy epipedon was favorable for the formation of a distinct spodic horizon above the oxic horizon. In accordance with the other taxa in which we have a spodic horizon overlying another horizon, we would assume that the current processes probably are those that lead to the strength of that spodic horizon, and therefore we would put it into order of Spodosols, and establish a subgroup of oxic Spodosols, to distinguish these soils from the alfic and ultic Spodosols.

Question 10

Why was plinthite at shallow depth made a defining criteria for some kind of Aquox? The way the order is defined makes it possible for a soil to be an Aquox by having plinthite, and without an oxic horizon. This might prompt a paper on Oxisols without oxic horizons. We are now downgrading plinthite, and so would like to know the reason for using it at a high level.

Guy Smith:

The soils that have plinthite at a shallow depth were included with Oxisols in an attempt to keep them all in one part of the taxonomy, irrespective of what underlay the surficial plinthite. These soils were thought to be of extremely small extent. They have been described to me from Africa, but I have never seen them myself. They lie, for the most part, on a colluvial slope below an escarpment that is protected from retreat by petroplinthite or some other form of hardened ironstone. They contain large amounts of ironstone, but they receive seepwaters from the soil above, and are thus kept wet. If cleared, the plinthite hardens at the surface and the soil is destroyed for the growth of plants for an almost unlimited time. Our feeling was, then, this characteristic overshadowed all others, and they should be kept together in the taxonomy in one order or another, and since they commonly are associated with Oxisols, we put them in the order of Oxisols.

Question 11

The Torrox presents a conceptual problem which needs your remarks. Conceptually, Aridisols are soils with aridic soil moisture regimes, and with a diagnostic subsurface horizon. If they are recent soils with no diagnostic subsurface horizons, they go into Entisols - Torriorthents and so on. But if they have an oxic horizon and an aridic moisture regime, they go into Oxisols. Why not Oxids instead of Torrox? If they have andic soil materials and an aridic soils moisture regime, they cannot go into the new Andisols, but instead go into Aridisols. Do we have a conceptual hiatus?

Guy Smith:

It would be possible to put the soils that have an oxic horizon and aridic soil moisture regime into either Oxisols or Aridisols. They were put into Oxisols rather than Aridisols on the assumption that if irrigated, they would behave more like Oxisols than like any other Aridisol. They do differ enormously in their properties from the vast bulk of the Aridisols.

Question 12

The Humox was created to separate the mountain or high level or low latitude Oxisols. Use of a temperature criterion at the suborder level has been criticized due to a duplication at the family level. As the definition of the Humox also requires that these soils have a low base saturation, high elevation, organic-rich, base-rich soils are excluded, and so defeats the objective of the suborder. Comments? The latter soils are present in Rwanda.

Guy Smith:

The use of temperature as a diagnostic at a high categoric level is duplicated at the family level, but at the suborder level, the intent was to provide for broad temperature groups to be used for small-scale maps, and the much more refined temperature classes used at the family level, were intended for use in making large-scale maps. It was assumed that an Oxisol, being strongly weathered in a cool, humid climate, would always have a low base saturation. The soils

being referred to in the question, in Rwanda, were unknown to us at that time, and I have not yet seen the data on these soils. It seems difficult to imagine that in such an environment, a soil as highly weathered as an Oxisol would have retained its bases against leaching. With the evidence that such soils do exist, the definition of Humox will need to be reexamined. It was our general principle in the development of *Soil Taxonomy* to provide for soils that were known to exist, but not necessarily to provide for all possible kinds of soil that might or might not exist.

Question 13

An Ultic Haplorthox is frequently misclassified as an Oxic Tropudult. People ignore the sentence (page 329 *Soil Taxonomy*) "An appreciable increase in the percentage clay with depth is a property shared with Ultisols, and defines the ultic subgroups (in Haplorthox etc.)." The subgroup and the explanatory sentence emphasizes the fact that the clay increase by itself is insufficient to identify an argillic horizon. If the subsurface horizon has oxic properties, it is an oxic horizon, and so will be keyed out as an Oxisol. Indirectly it implies that the oxic horizon has priority over the argillic horizon. Is this the intent?

Guy Smith:

It was the intent that the oxic horizon has priority over the argillic horizon. In fact, on page 20 of *Soil Taxonomy*, we stated "The argillic horizon by itself has little importance to soil classification. It is the accessory properties that are important." The soils that have a finer-textured subsurface horizon appear to be giving considerable trouble in the field. The pedologists seem to be unable to agree generally, as to whether or not this subsurface horizon is an argillic horizon. The problem has received much discussion from ICOMLAC and it is quite likely that some changes in the definition of the Oxisol will be needed and will be proposed by ICOMOX.

Question 14

Some Gibbsiorthox have also acric properties, and from a management point of view, the latter is a more limiting factor. It appears desirable to key the Acrorthox earlier, and provide gibbsic subgroups. Any particular reason why the present key was preferred?

Guy Smith:

Only two series of Gibbsiorthox have been recognized to date in Hawaii. None have been recognized in Puerto Rico. They are known, however, to occur in other islands in the South Pacific. Both of the series of Gibbsiorthox in Hawaii have a higher pH in KCl than in water, and are considered to have a net positive charge. This is obviously important from a management point of view because of the relative inability of such materials to retain bases against leaching. However, from a management point of view, the Gibbsiorthox also have multiple sheets of gibbsite with root mats above the gibbsite sheets. These sheets behave as do the thin iron pans called placic horizons, and other kinds of pans, although we have not defined

them as a pan. This is, perhaps, the principle reason why the Gibbsiorthox were not included with the Acrorthox, which do not have these pans.

Question X

This is a new question which is not recorded in the list of questions on Oxisols. The question relates to the mechanisms or the procedures to be followed for receiving suggestions from individuals outside the United States for amendments to *Soil Taxonomy*. The suggestions or recommendations which we have received from the international committees have been well debated, and usually are in a position to be reviewed and submitted for testing. On the other hand, when we receive questions from individuals, usually with not too much supporting data, we have problems in receiving or considering these suggestions for amendments. So, I would like to have your suggestion on the possible procedures that we could follow, and the kinds of data that should be requested from individuals for their suggestions for amending *Soil Taxonomy*.

Guy Smith:

My first point would be that it should not be much simpler to establish a new subgroup, than it is to establish a new series. The SCS policy has been to require a limited number of descriptions of different pedons to support the proposal for the creation of a new series. I believe this number is something like ten. Interpretations are also required for new series, to show that the proposed series differs in some interpretation from some closely-related established series. It has also been the policy of SCS, when a proposal is made for laboratory analyses for a given kind of soil, that at least two pedons must be sampled and they must be from differing polypedons, and at least 1.6 kilometers apart. When one compares the data from the two pedons, one then gets some notion of the reliability of the judgement of the individuals who want the help in selecting pedons that are as nearly similar as possible. If one examines the results, one finds that the individuals who made the request are not always competent to evaluate the soils properties prior to the receipt of the analyses, and many of the pedon data that are supposed to be similar are rather widely divergent. It would seem to me, therefore, that one requirement should be that descriptions and laboratory analyses of at least two pedons, at least something like two kilometers apart in the landscape, should be submitted as part of the supporting data. If supporting laboratory data are presented from a laboratory in an LDC, I think it would be desirable to require the submission, along with the data, of at least one subsample of one horizon of each pedon that has been analyzed. Proposals for the creation of new taxa above the series level should be accompanied by estimates of the areal extent of the kind of soil, and by interpretations for the proposed kind of soil showing some significant difference in behavior from the most closely related taxa in *Soil Taxonomy*. The proposal should also include long-term climatic data as possible, if the soil is one in which there is the possibility of a Udic, Xeric, Ustic or Aridic moisture regime. Proposals for changes in definitions of diagnostic horizons or features, or of existing taxa should also be documented with descriptions of the soils that cause the proposal to be made, the interpretations of the soils with the present definitions, and with the proposed definitions.