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

Soil Management Support Service

USDA - SCS

Ithaca, New York

1986

Addendum to:

THE GUY SMITH INTERVIEWS:

RATIONALE FOR CONCEPTS

IN SOIL TAXONOMY

by Guy D. Smith

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

1986

SMSS Technical Monograph No. 11

Table of Contents

Preface	ii
Interview by Mike L. Leamy	1
Interview by J. Witty & K. Guthrie	37
Interview at the Agronomy Department at Cornell University	48
Interview at the Agronomy Department at University of Minnesota	149
Interview by H. Eswaran	312
Lecture Given at the University of the West Indies	322
Interview at the Agronomy Department at Texas A & M University	328
Interviews by Coplanar staff & J. Comerma, Venezuela	441

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*.

Lecture Given at the University of the West Indies

February 6, 1981

St. Augustine, Trinidad

Text of a seminar delivered at the Department of Soil Science, University of the West Indies, St. Augustine, Trinidad, West Indies on February 6, 1981.

Ladies and gentlemen, I want to discuss with you this morning a little bit about how we got to the point where we are in *Soil Taxonomy* and what it looks like as we go into the future - a little bit about the past and a few words about what is going to come. To understand where we are in the classifications of soils we have to go back into the history of soil classification in the U.S. to about 1895, when Dr. Milton Whitney started the soil survey. Dr. Whitney, when he was a Professor of Soils at the University of South Carolina, wrote a paper on what the soil survey could achieve. He eventually was able to move into the Department of Agriculture and get started with the job of making the soil survey of the United States. In his introduction or his propaganda to get funds for the soil survey, he wrote a statement about what the purpose would be of the soil survey, if we were to try to write that same justification for the soil survey today we could not do better than to copy precisely what he wrote. He wanted to make the soil survey so that he could transfer results of experience or of research in one place to the other parts of the United States where that particular knowledge would have applicability.

The first map units for the soil survey were soil types; the type as it was envisaged at that time was determined by the texture or particle-size of the entire soil profile. Whitney emphasized the importance of the texture of the soil. He thought that was its most important property. Within a period of about two years afterwards the concept of the soil series was introduced. The series were a group of types that varied in texture but derived from approximately the same parent material laid down at about the same time. There was therefore an enormous geologic bias in the definitions of the early soil series. The soil series were named after the place where they were first found; the names were abstract and they included a very wide range in textures from gravels on one extreme to very fine clays on the other extreme, all in a single series.

In a few years the field scientists began to appreciate that there were soil properties rather than geologic properties and they had to sort out which of the soil properties were important to the use of the soil. No man had ever attempted this before; there was no group of trained men, so all of the early field men were trained as geologists and in soil surveying to exercise or use their training in geology. In the beginning the soil series were not grouped into any higher category; it was simply a grouping of types and there was no arrangement of the series into any discernible grouping or orders other than that the series were restricted to a particular geologic formation.

About 1912 Dr. Marbut took over the direction of the soil survey and he wrote in the U.S.D.A. Bulletin and he expressed the view that perhaps the most important property of a soil was the mode of deposition of its parent material. About 1914 Professor Glinka from Russia was persuaded to publish a book in the German language explaining the Russian concepts of soil not related to the American soil types, he began to think about the possibility of arranging the soil series into the Russian Soil Type or Great Soil Group as it was eventually called in the United States. Marbut translated Glinkas book into English and the Russian concepts were almost immediately accepted by the American universities. The classification of U.S. soils into the Great Soil Groups resulted. However, the classification of soils into series was never changed and the series continued to be the map units used. It was found impractical to relate the series to the Great Soil Group. Nobody could define or could say specifically which series belonged in which Great Soil Group. We therefore, had in effect, two classifications of soil in the U.S. starting about 1927 when Marbut published his first paper on a scheme of soil classification.

Marbut was never able to complete the groupings of the series into Great Soil Groups because he didn't have enough Great Soil Groups. He divided all soils into what he called pedocals (soil with a horizon of accumulation of iron and aluminum). There were some problems with his classification because some soils had both a horizon of accumulation of lime and of iron and aluminum, and some soils had neither. One could manage the soils that had

both by giving precedence to one over the other but the soils that did not have a horizon of accumulation could not be placed in his system.

In 1935 the then Secretary of Agriculture (Mr. Henry Wallace) decided that the Agricultural Year Book would deal with a subject matter - a disciplinary area; where previously it had been merely an assemblage of statistics and that the first book that had to be written in the new series was to be about soil. The Secretary of Agriculture appointed Dr. Charles Kellogg as chairman of a committee to arrange the outline for the book and to find the writers for the various chapters. Dr. Kellogg initially held the view that a yearbook could not be published until there was a recognized system for classifying soils. Secretary Wallace argued that this was reason enough for the first Yearbook to be about soils but the object was to expose the status of knowledge and if there were weaknesses, these should be made known. So rather than publishing a book without the classification system, Dr. Kellogg had one year to devise a system.

The Year Book was published in 1936 as expected there were some chapters about soil classification. A number of new Great Soil Groups like those that Marbut had were introduced and they were enlarged somewhat to allow for deficiencies. It allowed for the soils that did not have any horizon of accumulation by introducing new Great Soil Groups such as Alluvial Soils, Lithosols, and Regosols.

At the same time, the Soil Conservation Service was started and they were making soil surveys as a basis for planning and conservation programs, farm by farm, in virtually every county in the United States. Accordingly, the number of series increased very rapidly at this time. Whereas when Marbut took over there had been only a few hundred series, by the time that the 1936 Yearbook on Soils was published, there were quite a few thousands of series still without any plan or any relation to any of the Great Soil Groups.

So the series were tested, people knew the names of the series in their County or in their part of the state where they worked. Classification of the soils at the series level was successful since farmers identified their soils by series, the price of agricultural land was influenced by it, tax assessors used them to equalize taxes and the highway engineers used the soil survey to plan many of the highways that were being built. When a series was found to be too broad in its range of properties, it was gradually refined and re-defined, new series took over in the parts of the old series that differed in response to management.

Because Kellogg and his staff had only had one year to develop the Great Soils Groups, they were rather vague and imprecise, due not only to lack of time but also to defined nomenclature. We had to wait until the *Soil Survey Manual* of 1951 was published to understand the more commonly used works.

Immediately after the end of World War II, Kellogg put his staff at work to improve the definitions of the Great Soils Groups. They appointed special committees: one committee for the Red-Yellow Podzolic soils; one for the Grey Brown Podzolic soil; one for Planosols and so on. These committees struggled to improve the definitions and those definitions which these committees prepared were inclined to be very narrow definitions. As a result, there were huge gaps between the Great Soil Groups in some instances, and in other instances they overlapped considerably so that the same series could fit equally well into two or three different Great Soil Groups.

The committees struggled but the more they worked, the worse the situation became. We were finishing surveys of about 50 or 60 counties a year at that time but the correlation process was so time-consuming that we could never publish more than about 30 or these. In time we found ourselves with a ten-year backlog of completed soil surveys that could not be published because they could not be correlated. It was too time-consuming to compare the series in a particular soil survey with the six or seven thousand series so that these comparisons could be made systematically and efficiently.

About 1950 it was decided that things were going to get worse instead of better and we would need a system in which a soil series is clearly related to a Great Soil Group and this

would have to be devised. This necessitated a closer study of the system of classification we had and we found that the concept of Zonal, Intra-zonal, Azonal soils that had been borrowed from the Russians could not be defined in terms of soil properties. It was finally decided that to solve this dilemma, a new system of soil classification was needed. Because the three orders-Zonal and Intra-zonal and Azonal soil could not be defined in terms of soil properties. Soils were arranged into Great Soil Groups and into Soil Series. This meant that we had to virtually start all over again and devise a new system with orders that could actually be defined in terms of soil properties.

We realized that we could not call our staff together and sit them around a table and expect them to come up with any very good solution; rather, we had to prepare some sort of document that they could study and react to and they could say this is good or that is bad and so we started with the series of approximations, beginning in 1951 with the first approximation and the last one published was-the seventh in 1960.

Dr. Kellogg was insistent that the first job we had to do was what Marbut had been unable to do, i.e. classify the series into Great Soil Groups, and that had to come first. After that had been done then we could think about publishing a classification system which we finally got published in 1976. We began to use it in 1965 by printing supplements to the *Seventh Approximation* and we went through several supplements with modifications. Eventually we were able to publish a classification of soil series in terms of the Great Groups and of the *Seventh Approximation.*

During the various approximations there was one complaint that was consistent and this was that the various proposals were splitting the established soil series. This was considered undesirable unless the splitting of the series enabled us to make more precise statements about the behavior of the soils in one or another of the series. Therefore, in the process of developing definitions, the first over-riding principle was that the established soil series were considered the more stable of the various taxa.

There are some interesting comparisons between *Soil Taxonomy* and systems of classifications of living organisms. The soil series can be defined in terms of its own properties; it can be redefined if we like because as we learn more about any particular soil, it might become necessary for more attention to be given to some properties in its classification. We can re-define a series at any time that we please. In this respect, the taxonomy of soils differs from the taxonomy of plants and animals. A plant is put into a museum as a type-specimen for a species and you cannot change that type-specimen, as stated above, the definition of the series can be changed. The pedologist keeps stressing that soil forms a continuum in nature, going by imperceptible steps from one kind of soil to another whereas, according to the existing theories of living organisms there is a progression by discreet steps from one species to another species and there is no over-lap.

Haynes has written a little book about the concept of species, he is a zoologist and he used a British bird, the finch, as a case study. The British finch has a close relative living in France across the Channel with slightly different markings but if they cross breed, the offspring are fertile and they are considered to be varieties of one species. If one goes to Italy and Greece there is another finch again closely related but differing in marking and the off-spring of the Greek finch is cross-fertile with the French finch. It is another variety. In the Middle East there is another finch with other markings but it is cross-fertile with the Greek finch; and we go on into Southern Asia and we have other varieties and each one is cross-fertile with the one closest to it. And so going up the Pacific coast we come finally to Siberia, where there is another finch, differing somewhat in markings but cross-fertile with the Southern Asian finch. These are all then varieties of one species. But if you cross the Siberian finch with the British finch the off-spring is not fertile. This is therefore another species.

It depends, you see, on which way you go. You have two species if you take the two that are geographically far apart, but you have only one species if you take varieties that are geographically close by.

The taxonomists who have studied Linnaeus are quite unhappy with the system for classification of plants. The nomenclature is awkward since you cannot tell from the name what kind of an organism it is. It could be a microbe, an animal, a plant, you don't know. If you happen to know the genus you're all right since from this you may be able to know the related plants or animals. Linnaeus was of the view that a botanist had to know all the genes which was a fundamental assumption. This is utterly impossible, and it led to much of the current thinking that the taxonomy of plants and animals needs re-examination. It cannot accommodate all the observable variations that may be necessary in any particular case; one species may serve one purpose, other species to serve other purposes. Haynes from what he said, would like to re-organize the classification system and many other taxonomists agree with him, in this context, the principal priority of nomenclature is going to make it virtually impossible and they're searching it for a solution to their dilemma. A completely new approach may be necessary.

We think we have in *Soil Taxonomy*, a solution to our old dilemma of relating the unknown soil with our firmly established series.

It is recognized that the present taxonomy is made for the U.S. service due to the financial constraints and we could study the designing of the systems of classification that accommodates their soil as well as ours and so would be able to transfer their experience directly to the United States. But we could not use that same justification for most of the less-developed countries because they did not have soil surveys. They have yet to accumulate knowledge about the behavior of their soils and there was nothing that we could transfer to the U.S. to help us. So the taxonomy that was developed accommodated the soils that we knew in the United States, we knew in Europe but not the soils in the tropical regions. We have only two examples in the tropical countries - they are Puerto Rico and Hawaii. In these two areas there is predominantly a basic rock. The major areas int he inter-tropical parts of the world are on South America and Africa, in Southern Asia too, where we have a wide variety of rocks but we had no one with the expertise and no chance to study those soils. Consequently, there are many parts of *Soil Taxonomy* that are very incompletely developed and these are primarily soils of inter-tropical regions. There was nothing we could do about it because of the limitations imposed by the language of our appropriation.

Recently, due to the involvement of the Agency for International Development (AID), we have been able to make more progress in studies on tropical soils. Their appropriation language specifies that their funds are to go for the benefit of the other countries, not for the United States. One of their most urgent problems is increasing food production in these less-developed countries, and for that purpose, they want to be able to transfer the experience that they have in one country to any other country where that experience is applicable. Their money is appropriated for that purpose so they have begun to spend some of it to complete the development of *Soil Taxonomy* in the inter-tropical areas of the world. They have a contract with the Soil Conservation Service to give technical assistance in soil science to any developing country where the AID mission decides it is needed. They have also provided for the funding of a number of international committees which are studying the changes needed for the completion of the classification of the inter-tropical soils. AID expects these committees to function primarily by correspondence but not entirely, because they realize that it is important for committee members to get together in the field and examine the same soils together and then discuss what they see and how they feel these soils should be classified in the field. So far, they have funded one meeting a year of one or more of these classification of soils with low activity clays. i.e. Oxisols and Ultisols. The second was in Malaysia and Thailand. It included field studies of the least complete order in taxonomy, the Oxisols. This complimented the meeting held in Brazil on soils of low-activity clays. There is a common border that had to be drawn between the committees of soils that are concerned with the low-activity clays and the committee concerned with the Oxisols. The third meeting was held last year in Syria and Lebanon but the situation - well, things were a bit disturbed at that moment - war or violence of all sorts was expected and so the AID did not pay the cost of travel or subsistence to that meeting. The one coming up in June of this year is scheduled for Rwanda for the following year the meeting is planned to be held in the Sudan. After that I'm not sure where it will be following the Sudan meeting.

AID has also been funding a test of the functioning of *Soil Taxonomy* and the transfer of experience. They have established, through the University of Hawaii and the University of Puerto Rico, common field experiments, they use the same treatments of soils of a particular family. And when they compare the results of all the stations in many parts of the world on a particular family to see whether or not they do get comparable experience results from these various experimental fields. This has been in progress now for about seven years. It is now being phased out at the University of Hawaii but it will have at least another five years, I think, to go at the University of Puerto Rico. The stations are on many islands and continents and the results have looked to be very good. As a result, people who said originally that you cannot transfer experience at the family level have now reversed their opinion.

The international committees include scientists from all over the world wherever the particular kind of soil that concerns the committee is known to exist. Some of them are quite large. There is a committee working presently on the reclassification of the Andepts, i.e. soils from volcanic ash which are quite important on a number of the West Indian islands. This committee has something like 75 members; so it's truly an international committee. There are only very few from the U.S. on any of these international committees. When they complete their work they will submit a report to the Soil Conservation Service for any further checking and then for international distribution through publication in international journals so as to be readily available around the world.

Because we deliberately designed the system to make it possible to change one part or another part of the system without interfering with the rest of it, we can, for example, redefine the suborder, make it into an order and it doesn't affect the 46 other suborders. There are 47 of them, if we take out one and raise it to an order or lower it to something less; we do not affect very much of the system at any one moment. This is the way that we are working at present. The taxonomy is going to change one section, a segment at a time, with the completion of the reports of these committees. There is something like a dozen that is foreseen and there will be a good many more before the job is finished. The system obviously is going to change as long as we continue to learn things about soils. We can stop learning indeed but we cannot avoid changes as long as we are observant!