Post Guy Smith with Larry Wilding.

Ken Scheffe interviewer.

With respect to the National Cooperative Soil Survey, did you observe a significant change in the involvement of land-grant universities during your career?

I would say a significant change occurred with regard to the land-grant university involvement in the National Cooperative Soil Survey Program (NCSSP) during my career. During most of the years of the Dr. Charles E. Kellogg administration and shortly thereafter, the state/federal relationships among the land-grant institutions and the Soil Conservation Service (SCS) were quite formal, structured and strained. This was especially true for some states conducting their own soil survey programs, either in concert with or independent of the SCS. Disharmony often diluted the spirit of cooperation among NCSSP constituencies. It was often a philosophy of "they" versus "us" with true cooperative initiatives limited. However, during early phases of *7th Approximation* development (and earlier *Approximations)* which served as the forerunner to *Soil Taxonomy*, enhanced harmony and enrichment among NCSSP partners began to bear fruit. A number of state representatives to the NCSSP and international scientists, became active and productive contributors to the NCSSP. While it is always dangerous to single out individuals, early contributions were made by Drs. Marlin G. Cline (Cornell University), Frank F. Riecken (Iowa State University) and Frederick C. Westin (South Dakota State University), among others. Likewise, Dr. R. Tavernier (University of Ghent, Belgium) was an early pioneer in development of *Soil Taxonomy* and scientific contributions to NCSSP. As a side light, when I was a graduate student at South Dakota State University in the late 50's under Dr. Frederick C. Westin, he was quite involved in the development of the *4th and 5th Approximations*, well before the 7th *Approximation* and *Soil Taxonomy* were completed. His Sabbatical Leave to Venezuela working with Dr. Juan Comerma certainly augmented those efforts. In summary, I would say there were isolated cases of engagement of academic faculty involvement early in the NCSSP, but it certainly was not universal. More recently following the Dr. Guy D. Smith's Interviews, and progressive state and federal leaders in the USDA-NRCS, the NCSSP has been molded into a dynamic partnership which is the envy of many countries.

Do you think that the international committees, such as ICOMID, were effective in their effort to improve Soil Taxonomy? What would be your thoughts on continuing this?

Well, I thought the Soil Management Support Services (SMSS) program was an ingenious concept to foster international scientific interactions and collaborations among leading pedologists domestically and abroad. It did three or four things to enhance these relationships. For example, it provided an opportunity for land-grant professors/academicians to more closely interact with field soil scientists. As such it fostered working partnerships and recognition of mutual contributions to the NCSSP. There were multiple SMSS projects spanning diverse environments and ecosystems from the tropics to the artic, and from deserts to humid regions. I probably was involved in four or five of these and always found them well organized, efficient, productive, informative, creative, and motivating. This speaks volumes for the quality of participants and leaders engaged in the SMSS, and especially for Drs. John Kimble and Hari Eswaran's efforts to lead most of these ventures. In addition to strengthening the interaction between academia and field soil scientists, it enhanced the international protocol of a mostly nationalistic NCSSP before SMSS. At that time (the 60's, '70s and '80s,) we were looking for a way of testing *Soil Taxonomy*, including

the *1st to 7th Approximations* in an international arena. And in many cases, the only way we could do this effectively was to go to other countries to see for ourselves the management history, soil/landscape patterns, diagnostic horizons, presumed pedogenesis, environmental interactions, and develop a rapport with their scientists, to develop the protocol that enhanced the transformation of the *Soil Taxonomy* into a more international product. The third thing it did, it was serve as a teaching tool for those of us in academic institutions. Knowledge gained from SMSS paper presentations, field trips, and subsequent SMSS publications were incorporated into our student lectures and educational materials. Further, such information helped focus future research efforts of the NCSSP.

Finally, in some ways the SMSS program served as the forerunner to help develop the World Reference Base (WRB). For example, through *Soil Taxonomy* and SMSS important diagnostic horizons and properties were identified and quantified. These were used as *Soil Taxonomy* differentiae and many of these diagnostic features have subsequently been used in WRB as an international correlation tool. So while it is in the best interests of international community to continue the development of these two systems collaboratively, we need to vigilantly preserve caretaker rights of *Soil Taxonomy*.

Yes, the SMSS projects (like ICOMID), were a truly valuable part of the NCSSP. They brought a nice combination of pedologists "to the table" to help enhance our knowledge of *Soil Taxonomy* as an international taxonomic system. They provided seed monies to help augment pedological research that coupled personnel with field and the laboratory expertise. I would strongly encourage the leadership within the NCSSP to explore possible ways to undertake a similar program as SMSS in the future. It is an excellent model to enhance the knowledge base of geoscientists nationally and internationally.

Do you feel that geostatistics should play a larger role in Soil Taxonomy?

As a latecomer to geostatistics, I have relatively little expertise to evaluate its possible role as a soil survey or soil taxonomy tool. I've done quite a bit of soil variability work and most of it was done within the context of polygonal mapping units, of which I'm still a very strong proponent. Part of the reason I'm a strong proponent is because I'm a believer in landscape models of soil patterns. Soil variability in these systems is often (at least partially) systematic and not random. Classic statistics assumes that observations are random and this goes against our best pedological knowledge. But I know geostatistics has some powerful applications. It can help in sampling strategies. It can help in determining where and what kind of separation distances are needed before observations are more or less independent of one another. It can help us determine how many samples need to be taken in a certain locale. And It can help in distinguishing how much of the variability is random and how much is systematic. While I am reluctant to say too much more about geostatistics and its soil survey applications, I feel that it has an important future role in soil survey applications. Geostatistics is much better equipped to help quantify soil variability in landscape models than classical statistics because it has the capability to capture systematic soil variability that may well be lost in random sampling schemes.

Is there more work that should be done with soil carbonates, especially with respect to carbon sequestration?

There are probably more questions than answers in understanding carbonate synthesis and its role in carbon sequestration. I have enjoyed the opportunity to work with other geoscientists on synthesis of pedogenic carbonates in humid, semi-arid and arid systems and certainly the processes have similarities and differences. Pedologists have done extensive work on pedogenic carbonate synthesis and conclude

that precipitation is triggered by biogenic (organic), chemical (inorganic), or mixed process mechanisms. The question remains as to which process or processes take precedence and under what environmental conditions. Personally, I would vote that in most soil systems both inorganic and biogenic processes are active, but the extent to which they function and under what conditions are still a mystery. Some geoscientists investigating ancient limestone systems in Texas claim that all of the carbonates in these bedrock systems are biogenic. This may be true but I am a bit a skeptic. In *humid* climates where pedogenic carbonates occur we commonly believe they have chemically precipitated closer to the surface by evaporative pumping, although we know this process is also active in some arid and semi-arid systems too with shallow ground waters. But, we are not sure of how much influence biogenic processes may impact this model. Chemical precipitation of pedogenic carbonates in semi-arid and arid regions is often believed associated with downward-moving water fronts, but that model too may need to be modified with biogenic synthesis of carbonates. How to put together inorganic and biogenic models of formation of pedogenic carbonates is still a challenge.

Likewise, carbon sequestration associated with pedogenic carbonate synthesis is also a question. Judged from equilibria chemistry, half of the carbon in chemical precipitation of carbonates would be from the atmosphere and half from lithogenic sources. However, in most soil systems, kinetics control reaction rates rather than chemical equilibria. As I understand it from geochemists, this favors the more energetic light carbon isotope which is enriched in atmospheric gases to participate preferentially in the chemical reaction compared to the heavier less energetic lithogenic carbon isotope. If this is true, then chemical equilibria models would underestimate the amount of atmospheric carbon sequestered by chemical precipitation. In other words, more than half of the carbon sequestered within the pedogenic carbonates would be of atmospheric source. Further, how might isotope geochemistry dynamics influence sequestration of carbon by biogenic processes? And what about soluble organics? How do they influence carbonate precipitation and sequestration of atmospheric carbon? Finally, what controls the limits of pedogenic carbonate synthesis in base-rich environments? Is it the source of soluble bases or some other limiting factor?

Clearly our understanding of carbonate synthesis and carbon sequestration is in early stages of gestation. This is rather interesting given the fact that carbonate precipitation chemically would seem to be a straight forward pedogenic process. But it is further complicated by biogenic carbonate synthesis and isotope geochemistry. This nicely illustrates that soils are very complex biogeochemical systems with few unmitigated answers to pedogenesis and functionality.