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Evening Lecture: TDR Reflections: My Thoughts and Experiences on TDR

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TDR Reflections: My Thoughts and Experiences on TDR

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Introduction

What is TDR? Are we sure we have not been led astray? Topp's Dowsing Rods - Ty Ferré introduced this term a decade ago, being convinced that he had been led astray by his colleagues. Just to be sure of the meaning of the term I leaned on Google and look what I found:
TDR = Tropical Disease Research (A UNICEF Special Program)
TDR = Turbo Diesel Register
TDR = TriUniversity Data Resources (U of Guelph, U of Waterloo, Wilfrid Laurier U)
TDR = The Drama Review (A social science journal on theatre and performance)
TDR = **Time Domain Reflectometers**

Since Vince gave me a title with the word **Reflections** in the title, I am led to believe the fifth Google find is the topic for this evening.

The Topp, Davis, Annan Team

How did I get into TDR? Purely by accident! Geophysicists, Les Davis and Peter Annan, with the Geological Survey of Canada were using ground penetrating radar (GPR) in the Canadian Arctic along with TDR. They needed an independent measure of soil water content because the varying water content affected the precision of their GPR reflection depth to the permafrost. My being a specialist in soil water, led them to me. But in the mid 70s, all I could offer was neutron probe and gravimetric sampling, neither of which was of interest for work in seasonal permafrost. Les and Peter were already convinced that TDR and GPR could be used to measure soil water content, a concept that piqued my interest to be sure. We, in agriculture, needed field water content instrumentation very badly. What these young scientists were saying was of great interest, especially when they said it would measure on a volume basis. Thus began an enduring friendship and a mutually beneficial professional alliance.

What made this team so enduring? Professionally we needed each other as our expertise specialties were and are mutually complementary. In essence, I knew little of electromagnetic theory applied to earth materials and they knew little about the physical behavior of water in soil. Another important ingredient of intra-personal "chemistry" was our commitment to good science. None of us would permit any compromise on the quality of the work. No short cuts would be tolerated. We were very hard on each other as well as on our technicians, but as our TDR efforts unfolded all of us came to enjoy the results and the three of us soon developed a lot of enthusiasm for TDR. Amazingly this became infectious as others felt our enthusiasm and shared in it with us - even our summer students were smitten by the bug. In essence we knew we were onto a good thing and we lived that out with an increasing excitement. Thus we were not at all ready for the reactions to the early results that we received when we tried to share our carefully designed and executed experiments at professional meetings.

Reaction to our Early Data

In 1976, Les Davis presented a progress report on our TDR application to measurement of soil water content at a workshop on *Remote Sensing of Soil Moisture and Groundwater* sponsored by the Canadian Aeronautics and Space Institute. The reception accorded this paper was largely unfavorable as respected researchers from the University of Kansas Microwave Laboratory severely challenged Les when he presented our findings that soils of varied texture (clay loam to sandy loam) could have the same water content versus relative permittivity relationship. The microwave professionals carried the audience in their favor because of their longer experience, i.e. we were "the new kids on the block" and had no credibility deposit. This unexpected experience did not daunt our enthusiasm but it did send us "back to the drawing board" with questions like what more do we need to do? We soon realized that we had to get data from additional media to show the limits of what we had found. Surely data from more soils and more media should go a long way to convince the skeptics, or so we thought! As it turned out, this was only the beginning as we have faced skeptics for over 20 years from this first public presentation in 1976. In 1979, when I presented the additional soils data that we were so certain were going to convince the skeptics, the same microwave scientists from U. of Kansas were present and publicly accused me of presenting "fabricated data", "as it was well-known that sandy loam and clay-textured soils would not exhibit similar permittivity vs water content relationships". In three years while our data became better substantiated, our crittics became harsher in their criticism.

We were also faced with doubt and skepticism in soil science. Our kind of study and experiment were new to soil physics in the 70s. Not much had been done with EM waves or signals at the TDR frequency range. As one of my colleagues was fond of saying "that's the spectral region of black magic, Clarke, don't go there".

These were deflating experiences but at the base of our effort was our conviction that we had solid data which were following very consistent patterns where the differences could be largely explained from particle surface area effects. Here we were - a triumvirate full of excitement about our science and our professional colleagues were accusing us of "fabricating data" or engaging in witchcraft. Undaunted, we began referring to our work as witchcraft and carried on.

Very soon after the fabricating data accusation, I received word from Water Resources Research Journal that they were publishing our first TDR paper which has since become the Topp et al. (1980) paper to which many of you have referred. Now, I refer back to the introductory question “Are we sure we are not being led astray?” Many of you have referenced a paper whose basis was “fabricated data” if you listen to the critics!!

How Data Collection has Changed!

Let us reflect back 30 years to the mid 70s and think about data collected from instruments for field use before digitization came in and data analyses were done without spreadsheets. For TDR field work you had one choice only, the Tektronix TDR cable tester which was battery powered. Some short-term experiments used a generator and extension cord but the model 1502 cable tester was the usual choice. To have a record of your data, Tektronix offered a 4 cm wide thermal activated y vs t strip chart where each TDR wave form was on paper strips 4 cm by 30 cm. To say the least, these were not a convenient or useful form of collection for storage and retrieval of data. My colleagues Davis and Annan had used Polaroid photos of cathode ray tube images in the GPR development. Very early in our TDR program Polaroid photos of the oscilloscope traces were our most efficient and effective data collection method, particularly in the field. We wrote date, time and other key data on the photo edges. Polaroid photos were a very good tool for teaching colleagues and assistants some of the basics of data collection protocols. I can recall several instances where mislabeled, unlabeled or misplaced photos became the centre of dispute among us. I once chastised a colleague when I requested to see some photos to do some “anomaly checking” and I was advised the photos had been discarded when the data had been transcribed to data sheets. That was unacceptable to me and the message was received! All of our three children have been involved with coating photos, drawing tangent lines or transcribing travel-times to data sheets. I regularly brought boxes of photos from the day’s fieldwork to our home for after dinner “entertainment” with the children.

In the laboratory we used a t-y recorder which used a capillary ink pen on letter-size paper. It took about 15 sec to record each curve and 90 sec to dry the ink or sequential traces would smear each other. While running infiltration experiments, two minutes between readings was far too long as we would miss too much data. So out of necessity we had to find a way to speed up the process. All soils labs have sample drying ovens and ours was right beside the infiltration experiment. Our summer student, Rob, soon realized that 30 seconds in 105 ^{o}C oven would achieve the drying of ink. Each curve was recorded, the sheet was labeled with time, date and identification; the sheet was removed from the recorder and popped into the oven for 30 seconds during the next recording. This process of recorded data popping in and out of the oven caught a visiting soil physicist by surprise. So he asked Rob what was the purpose of the oven treatment interval. Rob’s reply was even more surprising to the visitor, “That’s how we cook our data. Doesn’t everybody cook their data?” The advent of digital data acquisition and storage improved the science undoubtedly but it did replace some experiences of differing value.

Employer Support for our Efforts

Our TDR work was undertaken in the Government of Canada research laboratories in the Departments of Agriculture (my employing agency) and the Geological Survey of Canada (employing agency for Les Davis and Peter Annan). Initially both Agriculture and Geological Survey were very supportive and encouraging for our effort. Soon the Geological Survey personnel could not see much coming from our work which would pertain to their mandate. Les and Peter began to get suggestions that they should phase out of this work which pertained only to agriculture. Les, Peter and I saw things very differently. We were operating as a wonderfully collaborative and complimentary team from which results benefiting well beyond the bounds of either department were coming. I lobbied Agriculture Canada headquarters to petition Geological Survey to continue the support for Les and Peter to work with me on TDR. It worked, but very soon the mood for this work soured in both departments. By the late 70s no scientific paper had been published and no design for an instrument was yet available from us, so both Agriculture and Geological Survey were getting cold feet at having supported us and allowing us to go into such deep, cold water from which they thought we would not achieve success.

Although disappointed by this apparent lack of vision on the part of our bosses, we were not deterred. We may have spent longer time in the after-hours because we were less-hassled. Very shortly thereafter, Les and I set about to write a description and develop specifications for instrument development.

Instrument Development

Les and I wrote up a request for proposal (RFP) for development of an instrument in the context of the Canadian Government in collaboration with an industry. Both departments supported this principle of partnership with industry and Geological Survey had earlier instrument development experience. However, they were not prepared to give financial support to an agricultural instrument. Agriculture Canada had no experience in such partnerships and they had great fear of the unknown. I pestered my bosses and finally broke down their resistance. OR did I? While I was convinced of the viability of what we were proposing, my bosses became equally convinced that nobody would write a proposal and submit it in response to our RFP. Hence, they saw no great risk in sending out the RFP and that would get me off their back.

To their surprise, five proposals were received in response to the RFP, three of which seemed viable but embodying very different approaches. With viable proposals Agriculture had to come up with the funds! Needless to say, I had to do some tall talking but finally made it for a contract from the following budget year, over two years after starting the process.

A contract was let to Foundation Instruments (FI) of Ottawa, a small telecommunication company which spun off from Bell Northern Research (BNR later became part of Nortel). Working hand-to-hand with company personnel provided me with a whole new learning experience. (Les Davis and Peter Annan had gone from Geological Survey into their own company to develop GPR.) FI personnel knew well the RF electronics and the necessary EM theory for the development of an instrument. The problem for them arose when they had to include in their circuit and calculation a medium which as we now know is a lossy or highly conductive component. Most of their calculations had assumed lossless components. They thought I should be able to look up the electrical properties of soil from texts or handbook so they could complete their design calculations including a lossy or conductive dielectric. Of course that was not available and we began to devise ways of getting approximations for the electrical properties of soil in the field. Working with industry taught me the art of practical compromise and how to break from scientific rigor without experiencing undue trauma.

Finally a prototype instrument came out after several faltering attempts, some of which were caused by me or Agriculture Canada and some by FI. This initial instrument worked well and I convinced FI to sponsor a booth at Soil Science Society of America convention the following November, after only a partial summer of use and evaluation in 1983. I had to go to the convention as if I was FI personnel. None of the FI staff was confident they could handle questions about soil. So to the surprise of many of my friends, I appeared at the SSSA convention as an exhibitor. Of course Soilmoisture Equipment Corp. was also an exhibitor and I was pleased to be able to invite Whitney Skaling to the FI booth to see the prototype instrument. But despite my enthusiasm, he went away that year not at all convinced that we had anything to watch.

The next year a revised instrument was launched with the name IRAMS (Instrument for Reflectometry Analysis of Moisture in Soil). The IRAMS was a much improved version and now there was more interest. Whitney Skaling, visionary that he is, could see the potential and Soilmoisture became the collaborating company which marketed IRAMS outside of Canada.

IRAMS was a good instrumentation start and was poised to go into upgrading and improvement which initial users were finding was needed. This phase of development was disrupted by a buy out of FI by a company who had no interest in TDR soil instruments. Thus FI made the distinctly mistaken decision to sell IRAMS to Campbell Pacific Nuclear (CPN). My advice to FI was to sell to Soilmoisture Equipment if they could. Guess why CPN was interested in IRAMS! Yes, they perceived TDR as competition for neutron probes and they wanted IRAMS off the market. It is thus that IRAMS fell to its demise. Having been so involved in the development and promotion of IRAMS, I became the person that people called when their IRAMS failed and CPN was even giving my name to people who were calling them for assistance. I was terribly angry to have the experience from these years of hard but innovative effort get so rudely ditched in the cause of industrial competition and marketing strategy.

Another dimension to the story is that Soilmoisture Equipment had become very interested in TDR from their limited experience with IRAMS. What was additionally annoying to me was that while I was being used by CPN, I was unable to exchange ideas with Whitney Skaling and contribute to the development of the TRASE. The story of development of the TRASE instrument has been documented and published by Whitney Skaling. Thus I was quite effectively held hostage by CPN in the way they were contacting me for any IRAMS complaints, but they were doing nothing to further develop the initial IRAMS instrument. I am not sure what I learned from all of this but I am not convinced that the business ethic is the optimal moral code for bringing publicly supported scientific innovation into instrumentation and practice.

Getting the TDR Story Out There (How TDR Came into its Own)

Having gone off into the instrument development arena for awhile let us return to the scientific area to see what transpired there. In the early 80s I remained convinced that TDR was working but not many were picking up the message. What does one do? Why was it so hard to get colleagues into trying TDR? In agricultural sciences, the capital outlay for a cable tester seemed exorbitant for a method that had not been evaluated widely. To overcome this prevailing reluctance I soon adopted the habit of packing a cable tester, or IRAMS and probe as part of my luggage for every scientific meeting or workshop I attended. With any available flower pot, lawn or parking lot edge I would offer people a hands-on opportunity to try TDR. On one occasion the hotel in which I was staying was surrounded by construction with barriers around the hotel. There was no soil to be found in that highly sanitized concrete environment! I looked out of the hotel and could see that subsoil was just on the other side of the barrier. Armed with waste basket from my room and bathroom cup I proceeded to find a way to get through, managing to get the soil both in the basket and on my clothes in the process. But science must go on! I had a pot of soil in which to demo TDR. I even dumped the waste basket before I left the convention!

These relatively simple but personal contacts with individuals led to some highly significant advances in the state of TDR. Did anybody here get introduced to TDR by my invitations? Some of the examples - liquid water in frozen soil with Danny Patterson, solute transport with Gary Kachanoski and measuring electrical conductivity with Frank Dalton and Bill Herkelrath. There are probably more.

TDR took root with various people in various ways. I especially like how it went into Australia and New Zealand. From our field installation in a clay soil, we had a most interesting finding from a field observation. We had measured with vertically oriented TDR probes an infiltration wetting front that remained stable for over one week. After a dry August in 1980 we recorded rainfall of 36 mm between Sept. 15 and Sept. 23. Our weekly TDR readings of Sept 23 and Sept. 29 showed distinct wetting fronts at a depth of 200 mm. During the week the wetting front advanced an average of 15 mm but had not redistributed down the profile. That is exactly what I had found in soil columns when I was studying soil water hysteresis during my Ph.D. work but I had not observed it in the field from rainfall events. In 1983, a special conference on infiltration was sponsored by ASAE. Thinking our TDR field-measured wetting fronts pertained to infiltration, I offered a paper and presented it with enthusiasm. In presenting the paper, I sensed that I was not understood and felt fully out of place with my paper. Nobody seemed to care one bit about what I was finding in the field using TDR. In fact one of my colleagues suggested that such a paper did not pertain to an infiltration conference. In spite of this humiliating experience at the time, good things happened later. As Ian White (Canberra, Australia) and Brent Clothier (Palmerston North, New Zealand) discussed my paper on their flight together back to the southern hemisphere they became excited by the possibilities offered by TDR. Two years later, Steve Zegelin (colleague of Ian White) was sponsored to spend time with me to learn TDR and Brent Clothier bought the first IRAMS instrument that was sold outside Canada.

In 1986, I had an experience which would not be allowed to happen today. I was heading off to Hyderabad, India as part of my small consultation contribution to a Canadian International

Development project. I was to help them assess the feasibility of using TDR in an agricultural production program in Hyderabad, India. As was my habit, I proposed to take an instrument with me and demonstrate the use of TDR. This time I took an IRAMS instrument. When I got about to board a British Airways plane in London, the security personnel wanted me to demonstrate what the instrument did. The IRAMS instrument needs a probe attached or the display only showed an E for "error". The agent was not satisfied at all with my explanation and the probe was in my checked baggage, of course. He wanted me to check the instrument and I refused as I did not trust all airport personnel! So we came to a stand-off. Finally he blinked and said "The pilot will have to decide this one." Someone went to beckon the pilot who came and enjoyed my story and allowed me to proceed to Delhi with his wish for success in our International Development project. (It probably helped that I was traveling on a special Passport.)

TDR gained mature status in 1987, but skepticism still had the early word. At the Utah State University Centennial Conference on *Measurement of Soil and Plant Water Status*, Wilford Gardiner (U. of California, Berkeley) gave a keynote address on *Water Content: An Overview*. In that address he noted, "That the search for this perfect measurement [soil water content] is as doomed to failure as was the search for the city of gold, but that does not deter us from the hunt." More specifically of TDR he said "Today, we are once more within sight of our 'city of gold' ... TDR promises to give us measurements of water content and electrical conductivity with off-the-shelf electronics and disposable probes. For some of us it is hard to overcome the feeling of deja-vu, but we must remain hopeful". In contrast, later on that same day, before presenting my paper, I was introduced as the *Grandfather of TDR.* Mine was the first of five papers on TDR from four different countries - two from USA, and one each from Israel, Poland and Canada. This conference had three indicators of TDR having come of age: (i) The dominance of non-US papers at a US-based conference was an indication of how truly international was the TDR advance, (ii) Never before had a single conference been the venue for five papers on TDR, (iii) At this conference representatives from a large number of companies were in attendance, including Tektronix, who was closely following the progress of these TDR applications. This was the beginning of the much wider diversification of TDR applications which has been made evident by these subsequent TDR conferences.

In Closing

We had resistance in our path from the beginning which led the three of us to resolve to dig in and do a better job. We were convinced that we must press on in spite of the messages reaching us. We substantiated our findings more fully and the reception now accorded our 1980 paper demonstrates the wisdom of getting the story correct from the start. Just doing the research and publishing the results probably would not have been enough to ensure success for TDR. Cross-fertilization of ideas among many people from around the world as in this conference promoted the early growth of TDR. Now it is up to you to continue to foster the cross-fertilization of minds with new and challenging ideas for the refining and improving of applications for TDR, i.e. Time Domain Reflectometry.

Thank you.