CSEG interviews Art Weglein


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Arthur B. Weglein received his PhD in physics from the City University of New York in 1975 and then spent two years as a Robert Welsh Postdoctoral Fellow at the University of Texas at Dallas. He entered seismic petroleum research in 1978, first at Cities Service Oil Company Research Laboratory in Tulsa (1978-81) and Sohio Petroleum Company Research Laboratory in Dallas (1981-85). Weglein spent the next 15 years as a member of ARCO's research staff. He spent a sabbatical year (1989-90) as visiting professor at the Federal University of Bahia, in Brazil, and three years (1990-94) as scientific advisor at Schlumberger Cambridge Research in Cambridge, England. In 2000, Weglein joined the University of Houston as the Margaret S. and Robert E. Sheriff Endowed Faculty Chair in Applied Seismology.

Weglein started the Mission-Oriented Seismic Research Program and industry consortium in January 2001. The goal is to address highest prioritized problems whose solutions would produce the biggest positive step-change in the ability to locate and produce hydrocarbons. This program is designed to meet the highest standards and aligned interests of university education and research, seismology, and the petroleum industry. In 2002, Weglein was promoted to a university-wide chair, the Hugh Roy and Lillie Cranz Cullen Distinguished Professorship in Physics, with a joint professorship in the Department of Physics and the Department of Geosciences.

The following interview was conducted by Bill Goodway, CSEG president, and *Recorder* editors Satinder Chopra and Jason Noble on 28 April 2003, on the occasion of Weglein's Distinguished Lecture in Calgary. The DL, "A perspective on the evolution of processing seismic primaries and multiples for a complex multidimensional earth," was ultimately presented in 25 SEG Sections in six countries. The following article (edited for *TLE* and published with kind permission of the Canadian Society of Exploration Geophysicists) first appeared in the September 2003 *Recorder*.

Your work has been unique, beyond even the best minds in our business. It's hard to look back at this point and see who would doubt the conclusions you've come up with, but I can see at certain points you must have been seen as something of a heretic.

Many thanks for those kind words. Oh yes, a heretic. Early on I was naïve and surprised by how much opposition to new thinking there would be from both academic and industry geoscientists, mathematicians, and physicists. Human nature, inertia, egos, fear of being superseded, and collisions of ambition account for some of the resistance and negativism. Now, I've come to believe that new ideas will have a concomitant and expected resistance, and to understand, predict, and anticipate it. If you want most people to think well of your efforts, go into mild variation of current thinking research, which by the way is a useful and necessary enterprise, but different from developing fundamental new concepts and step change capability.

In hindsight, I'm even more impressed when I think about the amount of support we did (and do) receive, in particular the depth, extent and duration of support we received from management—not necessarily universal support, but all you need is enough to carry forward the program. Among those we are grateful to are Hamid Al-Hakeem, Jack Golden, Jamie Robertson, Jim O'Connell, Dodd DeCamp, Mike Wiley, Phil Christie, David Campbell, Marlan Downey, Bill Clement, James Martin, Ken Tubman, Reid Smith, Bee Bednar, Craig Cooper, Andre Romanelli Rosa, Lincoln Guardado, Vandemir Oliveira, Paulo Siston, Edoardo Faria, and Jurandyr Schmidt for providing the support and environment necessary to carry out long-range high impact prioritized research.

In our experiences these enlightened and courageous managers shared a certain scale of inner character, imagination, confidence, a fascination (rather than fear) of new visions of what might be possible, and those traits often propelled their own careers. They were often geologists or geologically oriented. They just had a certain personal openness and orientation because they had this kind of courage and flexibility and willingness to support those ready to be "the first to walk into a dark room."

Many mathematicians and physicists are not necessarily better suited than others to do creative things. It surprised me to come to this way of thinking. They are often attracted to their discipline because things seem reasonable and ordered. One can move away from the trouble of the real world to a world where if you add one to both sides of the equation and integrate it all works, it's all fair. The real world is often not fair, people get hurt. Math can be an escape to a world of fairness. However, when you do really creative things, it doesn't come from math and physics alone. Most mathematicians and physicists, like most people in general, want comfort in the current format, in the current framework. To do creative things you have to jump outside that framework, where logic cannot take you, and trust your intuition, to attempt to reach a new, broader superseding vision, and predictive capability. It's your humanness that separates you from merely manipulating formulas and it's intuition and feeling that are both the source of inertia and creativity. In my view, math–physics capability and knowledge of geosciences is a necessary but far from sufficient condition for significant creative contribution.

With the inverse scattering multiple attenuation research, we didn't start with rigor; it largely began as a loose set of guesses and then we sought a mathematical expression for that thinking, and refinement of concepts by testing and evaluation. "If this is sort of how the internal multiple might start to be created in a forward scattering series maybe this is how it starts to be removed in the inverse … now can we express that thought as a guessed formula and evaluate its validity, and refine and rethink the concept if necessary," that's how it started. It became clear that the inverse scattering series had the potential to remove multiples and image/invert primaries at depth without the traditional need for a velocity model. Mining that potential into stable and practical algorithms is far from trivial. The entire series promises to input all your data at once and output the earth … unfortunately the entire series doesn't have favorable con-

*Editor's note: In "CSEG interviews Olivier Dubrule" (*TLE*, September 2003) the name of one of the interviewers, Oliver Kuhn, assistant technical editor of the* Recorder*, was inadvertently omitted in the credits.* TLE *regrets this oversight.*

vergence properties, without a good starting model that is not available in practice … how to cajole and finesse useful information from that overall series gave rise to a staged and task specific subseries strategy: Locate where free surface multiple removal, internal multiple removal, imaging primaries, inverting primaries are located within the series … that strategy has been useful and provides significant practical benefit and added-value.

When you take a step where there is no framework, you trust in your humanness. It's your humanness that gives you your edge. There are computer programs now that can do mathematics better than we can, it can arrange equations, and it can solve equations. Our edge is first of all understanding what it means, interpreting it, and going where that manipulation can't go because there is no logic step yet. I think we need to educate and require that our students have strong capabilities in the tools, but they should also trust their intuition. Gut feel—that's what gives you the edge. Roger Penrose, a professor at Oxford, wrote *The Emperor's New Mind*, a book which demonstrates that it is impossible for a computer to match the human mind now because the current state of physics is fundamentally inadequate to describe the human mind. There's a part of creativity that has to do with taking a step that you can't explain. In our field there is a big component of nonrigor at the beginning. Then others come and fancy it up and make themselves happy; that often takes years. There's an obvious important place for rigorous mathematics to clearly understand the assumptions and conditions behind your algorithm, but if you're trying to take a step that will provide a significant new concept, and potential capability, I know of no step that first came from only deriving things.

> "If you study whatever you are studying in great depth, that will pay a dividend."

Art, I was looking at your research interests, and it says, "Research and development of new seismic technology that enables exploration and production of hydrocarbons, a prioritized list of problems is identified which is felt will have the highest impact." So what is your prioritized list and what kind of problems are you working on?

In general, imaging at depth and delineating large contrast targets, with complex geometry, beneath a complex overburden such as basalt, salt, karsted sediments, and volcanics are outstanding and highest priority issues for E&P and hence, are at the top of our list. For example, we hear from oil company operations people that imaging beneath salt was, and is, their biggest obstacle to current effectiveness in deep water Gulf of Mexico. That's something we didn't have particular knowledge of; we certainly didn't have knowledge of velocity analysis. We were involved in development of migration-inversion theory, which along with all current depth-accurate imaging techniques assumes an adequate velocity is achievable. We were spending a lot of time on multiples, 10 or 12 years, we could have continued, that would be an easy road. We chose not to do that. That would be boring. It's not dangerous anymore. There's a bit of excitement in hearing today at the Distinguished Lecture, "You're going to get the accurate depth image when you haven't got the velocity, before or after, give me a break!" Those same people thought it impossible to attenuate all multiples without knowing the subsurface … now it's an industry standard. It was hard to fathom that that could be possible back then. Now we are pursuing imaging and inversion at the correct depth beneath complex media, without the detailed velocity. It's intriguing, it is relevant, and most important it is very highest priority for E&P and it deserves our attention. In exploration and production you want more effective capability, not solving insignificant problems that publish useless papers, with low priority issues/parameters. If you are solving the most significant science, stepping out, then you are having a step improvement in prediction and a step reduction in risk. There is an alignment between the interests of science and the petroleum industry; because they are both best served by solving prioritized problems. A drill is empirical, it's experiment. From a scientific perspective, mathematics and models don't mean anything until you experiment; it isn't science until then. To paraphrase Francis Bacon and Albert Einstein, all the chicken scratching on the blackboard is philosophy until you experiment and, for us, the experiment is when we drill into the earth. There's a very high bar for exploration and production—it has to be effective. It has to move the drill from a less to a more reliable location.

Publishing papers is a walk in the park. Producing truly new and impactful methods and algorithms is another thing altogether.

You spent a lot of time with Bob Stolt, whose f-k *algorithm came at a very opportune time to help people to migrate data quickly. What is Stolt working on now?*

After his *f-k* paper, Stolt made pioneering contributions to migration-inversion and inverse scattering multiple attenuation. He published a paper just last year on data reconstruction, extrapolating, and interpolating data with a better model. Stolt is very deep, very capable, enormously productive and creative, and very quiet. I often have a need to explain to people what we do. When someone doesn't understand I feel it necessary to try to make it clearer. Stolt has a confidence that, he understands, and if you don't understand perhaps that's your problem. It's refreshing to see. Working together with Stolt is a special privilege; it also provides a good balance to that issue for me.

I have followed your interaction with Guus Berkhout and the controversy over the need for the background velocity field for attenuating internal multiples. I was impressed with your reply when he acknowledged that you were correct.

The statements that Berkhout made that you are referring to speak to his personal and professional integrity, and I stated that at the time in reply to his statement.

In our personal history, Guus and I had a bit of an adversarial relationship early on. People found it amusing and entertaining, but then we sat down and decided to collaborate. He was a very gracious and warm host when an opportunity was arranged for me to be a visiting professor at Delft University. What impressed me most working one on one with Guus was his physical intuition and creativity. He has made numerous significant theoretical contributions, and he makes sure they are applied and have impact.

We learned things from Berkhout and his group, and vice versa, about the nature of multiples. Each group got stronger. We cooperated but we also were competing. And we both drove harder to the internal multiple on field data because we knew the other one was after it.

The main controversy, as you indicate, Bill, was about the need for an accurate background velocity field for attenuating internal multiples, which Guus defended and I said we didn't need. His agreement on that point at the SEG and EAGE meetings some years back speaks to his technical acumen and strength and character. So now we are at a similar juncture on that point, but there are still substantive differences between our actual methods/approaches for free surface and internal multiples. When you can isolate the reflector in your data where an internal multiple has its downward reflection, there are computational advantages to his approach …but when you are interested in attenuating all internal multiples without any interpretive intervention, or picking reflectors or anything else, the inverse scattering internal demultiple algorithm is the method of choice. Both methods belong in your toolbox.

In our current research, we're saying you don't need the background velocity detail to get a detailed image in depth. As you could see there were quite a few people in the DL audience today who were not comfortable with that assertion. At the least, we know it's new when there is that degree of discomfort.

I think the discomfort today at the lecture was in the lack of background field for the depth image.

Right. Because that's new. Twelve years ago if you said you were going to predict the multiples without any information about the earth—in other words, you were going to have a section with the data and a section with the ringing from the salt predicted without knowing top or bottom of salt—they would have thought you were insane. Fifteen years ago processing primaries were conceptually more advanced than processing multiples; now that situation is reversed. Previously multiples were treated as deriving from a 1D earth with a known 1D velocity, whereas primaries were viewed as deriving from a multi-D earth with a known multi-D velocity. Free surface and internal multiples can be attenuated today with absolutely no subsurface information whatsoever about the heterogeneous and/or anisotropic multi-D earth by arranging certain distinct additive and multiplicative communications between the entire recorded wavefield of primaries and multiples. Multiplicative communication is the key difference that allowed multiple attenuation to leap-frog processing primaries in concept and efficacy.

Current leading-edge imaging methods, once you have settled on your velocity model, only allow additive communication between events that have experienced the same reflector. For this reason , an inadequate velocity model will result in an inadequate image in depth. Sums of the surface data produce an image in depth, and weighted sums of the imaged data over angle produce the AVO response. Our current research seeks to extend the vision and capability to processing primaries that we earlier provided to removing multiples. Allowing other specific types of distinct additive and multiplicative communication between all primaries at once, contains the conceptual possibility of imaging and inverting all primaries at their correct spatial locations, without knowing or determining an adequate velocity model. This provides a totally new vision for primaries aimed at directly addressing our most significant and intractable E&P problems. That some might find this unbelievable today and not deserving their attention and support is not surprising; what is truly amazing to me is the breadth and depth of industry support worldwide and the direct industry collaboration we have received. That's an extremely optimistic and positive note about the petroleum industry.

In our early history, migration-inversion—a form of migration before AVO—was received with the biggest negative blowback, both from people in migration and people in AVO. They made it clear in the strongest terms possible how crazy we were. Today it's just another industry common practice.

You're saying the migration people didn't really believe the amplitudes they came up with?

Many migration people stated "they were not interested in amplitudes" and the AVO mantra was, "We can ignore multi-D effects on propagation and reflection, because our 1D model is closer to the real 3D world (since both are odd numbers) than 2D is to 3D." The guys who are really doing new things more often than not can accept other new things. That's a big test of the mettle of a scientist. You first demonstrate your respect of science by trying to improve upon it. The toughest test is to know that you yourself will be superseded. Of course we don't try to make that easy, we try to keep moving, but it's going to happen. Everything we do has assumptions including all the things we said at the Distinguished Lecture today, and we make them, acknowledge them, and argue for their reasonableness. Today's reasonable assumption is invariably tomorrow's high obstacle to effectiveness.

I don't think anyone would accuse you of being an obstacle, but of testing their ability to think beyond the box.

Thank you, Bill. In real research you have to be prepared to fail. Stolt and I worked on something in the late 1970s and early 1980s that we eventually decided was intractable. If you're really in the new research business, that's something for managers to understand, and also in universities. If everyone were showing progress, I would say, "Where are the failures?" If you are never failing you're not taking chances with new thinking. You've got to have some rate of failing. Stolt and I, separately—he at Stanford, I at Cities Service Oil—were looking for a closed form solution to the simplest multi-D earth: Velocity varying in x and z. Just find an algorithm for that type of model, without small earth property changes or other linear assumptions, which didn't require a series. We worked very hard, looked all over the world, and tried to solve it ourselves, and couldn't find a solution. We shut it down; you've got to know when it's enough. When you do that it isn't a waste of time. We learned a lot of things that guided other approaches to these prioritized problems.

How do you in a tenure system, or an industry judging research, allow people who have potential big steps within them to have the flexibility to fail? If you're not allowing that flexibility for some portion of your research staff, all you're going to get are small, insignificant cosmetic forms of change. Counting the number of published papers is easy and quantitative but rarely a metric of real contribution. You've got to have the right people. You give some people too much freedom, they'll go sit on the beach and drink *piña coladas*. I had a great manager at ARCO, Al-Hakeem who said, "Why should we send you to Brazil? If I think of what's best for ARCO for next year, I shouldn't send you. But if I think what's best for ARCO for the next 10 years, it makes sense. It will give you a place to think." It was that, and I am indebted to him. All of the research on multiples had its origin there.

Is it your perspective that companies have all changed for the worse now?

I think the oil companies are moving in the wrong direction in general—too much short-term thinking on research and technology development, too much outsourcing. There are several hopeful exceptions, however, and reason for measured optimism. You don't outsource what you consider critical to your business success and competitive advantage. On the other hand, I have also been extremely encouraged, and frankly positively surprised, by the broad and extensive industry support of our efforts within the Mission-Oriented Seismic Research Program at University of Houston. It speaks volumes about the interest that exists within the industry for supporting fundamental research when it focuses on relevant issues that have the potential of providing significant increased effectiveness and reduced risk.

I also think that there isn't an overabundance of new thinking from researchers overall, rather more complaining about lack of support. People who complain about industry not supporting research generally have it backwards: if they were in fact actually doing real research they would have support. In my own experience, I have never seen a single creative approach that could provide, if successful, an increased capability that would not find support within the industry. I have also found that industry by and large wants universities to be universities—to concentrate on the core educational and research responsibility, on fundamentals, to be aware of industry issues and to be responsive—but not too responsive—to industry needs.

I don't think there is much time allowed. The reliance is upon people like yourself and your students.

There is also a certain bit of responsibility to the turning off of research, that's the responsibility of the researchers. Even if researchers are actually working on relevant problems, they are still looked upon suspiciously by operations. Operations have pressure; they need something done now, and for them a great thing a week from now is useless. It is always easier to show an increased profit by cutting costs than by finding and producing more hydrocarbons; and, research can be an easy target. On the other hand, researchers that either oversell, hide assumptions and prerequisites, solve irrelevant and low priority or convenient problems, and publish useless but résumé expanding papers, or try to masquerade technical service as research, also give support to short-sighted near-term thinking that damages the development of relevant and differential capability.

Do you interact with the majors in terms of how you might change concepts of sampling in acquisition?

First we look at a new concept for intrinsic capability given perfect data conditions. Then we start to take away perfection to bring realism in. If it remains robust, then we ask what different kinds of acquisition would enhance effectiveness? Is it achievable, that is, does it have an added cost that makes it unrealistic, or is the added cost more than offset by increased effectiveness with increased reliability and reduced risk? We get involved in all aspects. We are dealing with all levels of the seismic experiment because these new methods put a higher bar on our expectations.

When you speak about multinationals, the majors, do they come directly to you with their problems?

We don't see their data. How we figure out what to work on is, for example, a company might say, "We have a problem with deghosting ocean bottom pressure measurements," so we check around to see if this is that company's problem or a universal problem. For us to look at it, it has to be a global high priority matter for the industry. The company might show us the data as an example of the problem, but they don't give us the data. We provide code, but it's research prototype code. We don't provide production strength code; it's research code that has been tested on field data. We don't do tech service. Many universities are doing tech service; that's generally a maltreatment of graduate students. If you have a code and you're a professor, and some company wants to get their data processed, the faculty will use the graduate students as essentially processors. Processing data once is an education, but processing it routinely is not an education and has little or no research value. The company saves money because graduate students are not as well paid as contractor processors. Further, the company can write it off as an educational expense, a tax write-off. The company saves money, the professor is a hero because he brings money in, and the only victims are the students, because they are not getting an education, nor performing research. Fortunately most companies, especially larger companies, avoid that practice. Universities lose their way when they only measure success by the number of papers published and how much money they're bringing in. We need to be vigilant to the educational mandate and role and responsibility of a university in our society.

How different is it lecturing as an SEG distinguished lecturer versus the normal lectures that you give?

The objective of the distinguished lecturer tour is to speak to the average SEG member, and that's a challenge when the activity is very technical. If you really know what you're doing you can explain it to an intelligent farmer. If you understand the machinery, there is a way of explaining what it is trying to do, without the math and the physics, just give some sense of why this new possibility is there. It's a wonderful part of the tour; you get to see people you haven't seen for years, meet new people, see wonderful places, and see places you haven't seen at all. The difficult part is you are away from your family a long time, weeks at a time. What I've done at most universities and would have done here in Calgary if there were more time is to follow the DL with a separate talk which is more technical.

What advice would you give young people who are just entering the geophysical profession?

That's a tough one. When I go and speak at universities on this DL tour, I tell them I don't really have a crystal ball about what their livelihood—you know, oil, environment, etc.—will be. The only thing I'm pretty sure of is that if they study whatever they are studying in great depth, that that will pay a dividend. They can transfer that to other things. We mentor students toward PhDs in seismic physics, while demonstrating and guiding them on how to choose and solve problems that haven't been solved. That's a transferable thing. Don't assume that they will be in the narrow place that we provide them, but make sure they understand that what we are doing exemplifies a process, rather than being the process. TLE