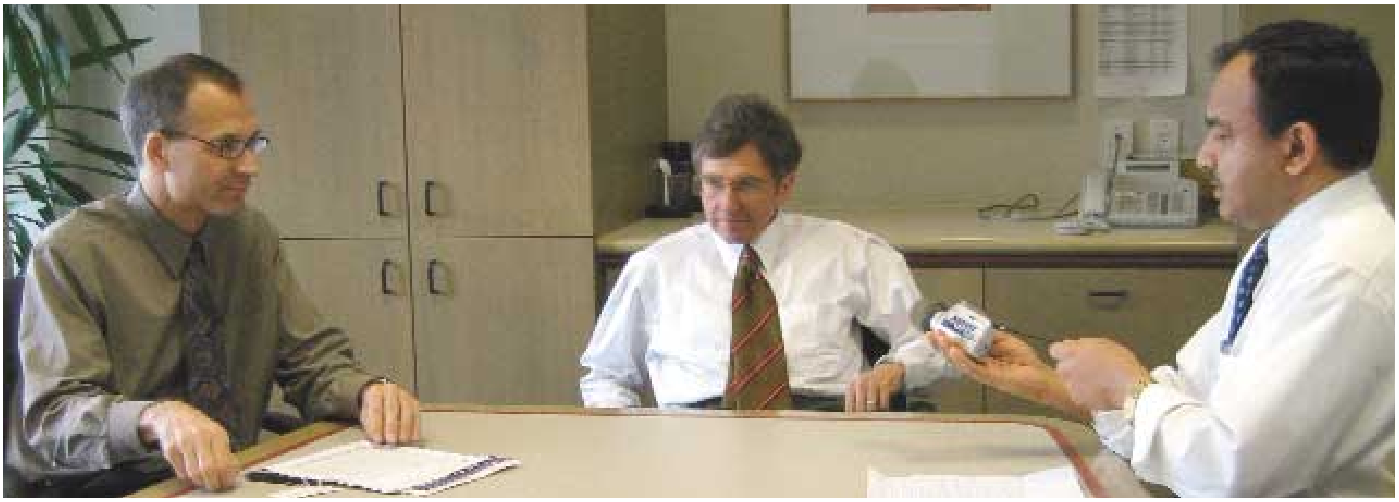
"If you study whatever you are studying in great depth, that will pay a dividend….."

— An interview with Art Weglein



Bill and Satinder preparing to interview Art (Photos courtesy: Vince Law)

Arthur Weglein delivered the Spring 2003 SEG Distinguished Lecture on 'A Perspective on the Evolution of Processing Seismic Primaries and Multiples for a Complex Multidimensional Earth', at Calgary on April 28. After his lecture, Art gladly agreed to spare some time for this RECORDER interview. Bill Goodway (B), CSEG President, Satinder Chopra (S) and Jason Noble, RECORDER Editors, sat with Art to get his views on a wide range of topics. Following are excerpts from the interview.

S. Art let us begin by asking you about your educational background and work experience.

A. I went to City University in New York, and received a Bachelors, Masters, and a Ph.D. in mathematics and physics. Then UT Dallas for two years of post-doc work, still in physics. After this I joined the petroleum industry, working at City Service in Ohio, then I was at Arco for 15 years before joining the University of Houston in August 2000.

S. Your background is basically in Math and Physics?

A. Yes.

S. So how come you decided to switch over to geophysics?

A. I needed to earn a living. (Laughter.) There were no jobs. In the late 70s there were no or very few jobs for physicists. And, people like Stolt and myself – I didn't know him then – faced a similar problem: is there a way to do interesting work and earn a living. We had two children then, and my wife said one day, "I hate to upset you but eating regularly is an interesting idea." She said "I know you love physics but you're earning $11000 a year as a post doc". I was happy, I was just doing science. I have been very fortunate; I have been allowed the freedom to work on even more interesting problems than I did in physics. In physics it was constrained by how much the government was willing to fund the person for whom I was working as a post doc. Here we can choose to prioritize what problems are most significant; the challenges which, if you could address them, would have the most impact. The fact that oil companies would give that kind of freedom says a lot. I'm very grateful to all the managers who allowed that to happen over the years. They took a bigger risk than I did because we hardly ever knew what we were talking about and they knew less and they had to trust that we meant well and we would give it our best shot. Managers are under pressure to produce things, quarterly returns for example, and when they give some allocation of a part of their resources for no return in sight, it takes courage and vision on their part. They meet opposition. I don't think any company ever had a uniform view of management or technical staff that the support of our activities was the right thing to do. It always was a very delicate balance of having enough support to keep yourself going. It wasn't like, "wow Arco supported this", there were certain courageous managers who looked out for this activity. There were others who if they had their way would have shut us down in a flash.

B. Your work has been unique, beyond even the best minds in our business. The management you're talking about obviously saw the uniqueness, as opposed to just the standard repetition. 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.

A. Oh yes. I think people like ….. Jim O'Connell, who is the head of exploration at ConocoPhillips now, said, "What you represent is a vision of what might be possible," and that's why he supported it. It's not something he could have next week. It takes

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a certain scale of imagination, of confidence, on their part to be able to support work without instant results. It really isn't a person's background. Over the course of my career, I have had more problems with people from math and physics backgrounds. These people I'm mentioning are mostly geologists or more geologically oriented, they had a certain imagination, and they usually moved to the top of companies because they had this kind of flexibility. However, they had very different attitudes when they looked at processing; it was how many lines did you process? Very tough guys. They had different attitudes toward different activities. They were flexible, there were certain activities they had under their responsibility of which they had a certain expectation of deliverable. Our expectation was given a longer lead time.

Mathematicians and physicists are often the last people to do creative things. It surprised me to come to think this way. They like things to be ordered. Most mathematicians 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's not fair, people get hurt. Math is an escape to a world of fairness. When you do really creative things, it doesn't come from math and physics. Most mathematicians and physicists want comfort in the current format, in the current framework. To do creative things you have to trust your intuition. It's a humanness that separates you. With all the multiples and all that, we didn't have any rigor to that, it was like a guess. If this is sort of how it's created maybe this is how it's removed. The mathematicians are very critical. When we were in Cambridge, the mathematicians said "where is your derivation". I said "we need to get the multiples out", they'd ask "where's your algorithm". I had no algorithm. When you take a step there is no framework, you trust in the humanness. It's your humanness that gives you your edge. There are computer algorithms now that can do mathematics better than we can; arrange equations, and solve equations. Our edge is first of all understanding what it means, interpreting it and going where that can't go because there is no logic step yet. *I think we need to teach our students to have strong capabilities in the tools but they should also trust their intuition.* Gut feel, that's what gives you the edge. Roger Pemrose, 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 our physics is fundamentally inadequate to describe the human mind. There's a part of creativity that has to do with taking a step you can't explain. In our field there is a big component of non-rigor to it at the beginning, then others come and fancy it up and make themselves happy – that often takes years. There's a place for rigorous mathematics, but if you're trying to take a step that will have a significant impact, I know of no step that came from just deriving.

S. Art, I was looking at your research interests, and somewhere 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?

A. We hear from Oil Company operations people that imaging beneath salt was, and is, their biggest obstacle to current effectiveness. That's something we didn't have particular knowledge of; we certainly didn't have knowledge of velocity analysis. We had some involvement in migration inversion theory. 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 the excitement in solving "you're going to get the depth image when you haven't got the velocity, give me a break". It's hard to fathom that that could be possible. It's interesting, it deserves our attention. What is not understood by people in academia is that a lot of non-exploration earth scientists look down at exploration and production earth science. They couldn't be more confused in my view. In exploration and production you want more effective capability, not solving insignificant problems that publish useless papers, with parameters that, who knows what they mean. 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's an alignment between that, and at the end of the day it's science because they put a drill in the ground. A drill is empirical, it's experimental. All your mathematics and all your models don't mean anything until you do your experiment; it isn't science until then. Einstein said, *all the chicken scratching on the blackboard is philosophy until you experiment*, and for us, the experiment is the drill. People in the whole earth world with the Moho or Soho or whatever, they tend to look down on exploration and production. There's a very high bar for exploration and production, it has to be effective. It has to move the drill from less to more reliable. Bob Stolt had an interesting idea; he once said to me, why don't we only write something when we have something to say. I said what do you want to do that for? I had been hurt early on; I only published one paper when I was a post doc. I went to look for a job, they said you've only got one paper. I got into oil, I started publishing. Later when I bumped into Stolt, we went to only writing something every five years.

S. My next question is about Bob Stolt. You spent a lot of time with him. He came up with this FK algorithm at a very opportune time. It helped people to migrate data quickly. Could you tell us something more about Bob Stolt, what is he working on now?

A. He is working. He published a paper just last year on data reconstruction, extrapolating, and interpolating data with a better model. Not everything that Bob Stolt works on gets published; he

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knows that he works for a company. There is only a fraction that can be published, for example if you want to publish a paper you have to show them three papers you didn't publish. That adds value to them, otherwise why are they paying you a salary? Stolt is very deep, very capable, and very quiet. I sort of have a need to explain to people what we do, when someone doesn't understand I feel it incumbent on me to try to make it clearer. Stolt has a confidence that, he understands, if you don't understand that's your problem. It's refreshing to see. When I do derivations, I'll go through every step and detail; he writes the last step and says "what else could it be". He has a very deep understanding of the physics of wave theory, and has a tremendous intuition. Our personalities are very different, I go on blah-blah-blah-blah, like in this interview, and he'll say on the phone "OK". But when he says something, I pay a lot of attention. I remember practically every word from every sentence he has ever said.

B. I followed your interaction with Berkhout. I was very impressed with your reply after he acknowledged that you were correct.

A. It speaks to his integrity. Berkhout is an example of a person with a lot of physical intuition. In our personal history, we have had a bit of an adversarial relationship. People have found it amusing and entertaining. We sat down and decided to collaborate. He agreed with that and was very proactive. I had an opportunity to be a visiting professor at Delft. He was a very gracious and warm host to me and my family. The thing that impressed me the most was when I would work one on one with him alone he dropped all that sort of peacock thing he puts on in public. The sort of prima donna. He was just a regular guy struggling with some kind of concept he couldn't quite write down. He isn't really a math physics guy. He comes from more of an electrical engineering signal processing background. His entry into multi-dimensional wave fields is largely intuitive and sometimes he misses detail. It's hard to get the normal derivative of a Green's Function in Green's Theorem from intuition. You're lucky to explain it after you can derive it. It's pretty spectacular what he can come to given his background. He has a lot of creativity. He has made a lot of contributions. I learned things from their group, and vice versa, about the nature of multiples. Each group got stronger. First of all we were competing, even though it was cooperative, we were competing. And we both drove to the internal multiple on field data because we knew the other one was after it.

B. The main controversy was the need for the background field?

A. That's right, which he dropped. So now we are at a similar juncture. We're saying you don't need the background detail to get a detailed image. As you could see, there were quite a few people in the audience today (Luncheon lecture) who were not comfortable. At the least, we know it's new when there is that degree of discomfort.

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

A. Right. Because that's new. 12 years ago if you said you were going to predict the multiples without any information about the earth, you were going to have a section with the data and a section with the ringing from the salt predicted without knowing top and bottom of salt they would have thought you were insane. Migration inversion, just migrating before, was the biggest blowback, both from people in migration and people in AVO. They made it very clear, in the strongest terms possible, just how crazy we were.

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

A. I think the migration people were quicker. There are people who are sort of the real researchers, and then there are sort of the camp followers. The camp followers tend to be dogmatic, it's like it's a religion. There's a leader, then there's people who might get some business out of it, and they are not always of the same character, they're running a business. The followers tend to be more rigid than the person who pioneered the idea. They got this idea and implemented it and their career is depending on it. They themselves are not creatively capable, they are protégés in some way. 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 improve your respect of science from the past by trying to improve on it. The toughest test is to know that you yourself will be superceded. When that comes, boy is that a test. 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 say, and we make them and they sound reasonable. Today's reasonable assumption is invariably tomorrow's high obstacle to effectiveness. There are different obstacles to new science, you just have to make sure at the end of the day you're not one of them. That's not easy.

B. I don't think anyone would ever accuse you of being an obstacle. I think people would accuse you of testing their ability to think outside the box.

A. You have to be prepared to fail. Stolt and I worked on something in the late 70s and early 80s that we gave up. If you're really into something new, that's something for managers to understand, and also universities. If everyone is showing progress, I would say "where are the failures?" If you're not failing you're not taking chances. You've got to have some rate of failing. Stolt and I, separately, he was at Stanford, I was at City Service, were looking for a closed form solution to the simplest multi-dimensional earth: velocities, bearing in x and z. Just find something which didn't require a series.

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We worked very hard, looked all over the world, 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 which were tools for other problems. How do you in a tenure system, or an industry judging of science, allow for people who have potentially big steps to take and have the flexibility to fail. If you're not allowing that flexibility, it's only small variations you are going to get. You've got to have the right people – you give some people too much freedom, they'll go sit on the beach and drink pina coladas. We went down to Brazil and worked ourselves to death down there; a lot of people never got off the beach. I had a great manager at Arco, he 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 to Brazil. But If I think what's best for Arco for the next ten years, it makes sense. It will give you a place to think." It was that, and I am indebted to him. All of the multiple stuff came from there.

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

A. I think the oil companies are moving in the wrong direction in general. I also think that there isn't an overabundance of new thinking.

B. I don't think there is much time allowed. The reliance is upon people like yourself, and the students you have.

A. 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. Heterogeneity is the biggest issue for P–P data, and most of our data is P–P. A bunch of people have run to anisotropy, P–P anisotropy is low priority. It's made a lot of people famous, they've solved anisotropy with a homogeneous media. The earth for P–P data is anisotropic but it's not as important as heterogeneity. Once you get shear waves anisotropy gets more important but most of our data is P–P. There is a bit of a sham. When you look at long offset data and you take the Dix equation, and you get t-squared and a constant and x-squared, x to the fourth, they call the coefficient in front of x to the fourth the anisotropic correction. You don't need

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anisotropy to get the x to the fourth term, all you need is a binomial expansion with one more term, but it's politically correct to call it anisotropy. We can't solve isotropic heterogeneity, so they solve anisotropic homogeneity. That's great, but it's not the problem. When I was in Cambridge, it was a hotbed of anisotropy, so I told them they were working on the wrong problem. They said yes, but your problem is hard.

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

A. First we look at a new concept as if given perfect conditions – does it work. Then we start to take away to bring the realism in. If it remains robust, what different kinds of acquisition would enhance it? Is it achievable, that is, does it have an added cost that makes it unrealistic? 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.

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

A. We don't see their data. How we figure out what to work on is, for example, Shell might say "we have a problem with deghosting ocean bottom pressure measurements", so we check around to see if this is Shell's problem – or if it is a universal problem. We are not looking at people's anecdotal problems or personal problems. It has to be a global problem for the industry. They don't show us the data, that is, they 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 been tested on field data. We don't do tech service. A lot of universities are doing tech service; that's a maltreatment of graduate students. If you have a code and you're a professor and some small company wants to get their data processed, the faculty will use the graduate students essentially as processors. Processing data once is an education but processing it routinely is not an education. The company that comes to the university to have this done can write it off as an educational expense, a tax write off. You go to a contractor, you pay for it. They do it to save money. The professor is a hero because he brings money in. The only victim is the student because the student is not getting an education. Universities lose their way when they only measure how much money they're bringing in.

S. You worked for Arco for a long time. What prompted you to join the University of Houston?

A. I always wanted to be a professor; it was my original interest, maybe a bit naïve. BP was taking over Arco so I was coming to a juncture of decision anyway. Once I'm at a juncture of decision, I usually consider options. At Arco we were all treated well, we had a good history, why look around. I had to decide where I was going to go with BP. BP was not Arco. They are almost the poster child of outsourcing. They feel you don't need to develop things inside; you can just buy things from outside. That's the diametric opposite view of Arco, Shell and others, which feel if you don't make it, if you don't develop it, you don't value it. I have never heard of a vice president outsourcing his own job. When all the guys are sitting around the table, they are not the expertise you can buy from the outside.

S. You've given a number of talks on the research work you've done, how different is it lecturing now as an SEG distinguished lecturer versus the regular/routine lectures you give?

A. I think 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 – someone I once worked with said that. 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, see wonderful places, and see places you haven't seen at all. You get to have an appreciation that there are a lot of unemployed people in geophysics. Even with the things we moan and groan about back at the office, we are fortunate to be working. You see audiences where significant fractions are clearly unemployed. The difficult part is you are away from your family a long time, weeks at a stretch. What I've done at universities and would have done here if there was more time, is I give the talk, then I have a separate talk which is technical. I go to the board and start from the beginning and anything you want to see in detail I can derive and show you. It complements it by giving it a foundation. I would like to return to Calgary to do just that.

S. What are your other interests, apart from geophysics?

A. Family. I think my happiest moments are when my whole family, all four boys and my wife and I, are around the table together. One is in New York, two are at A&M, one is at home, it gives me a good feeling. We like music, we like dancing, we like African and Brazilian music, we have drums in the house. We like to not think. When the music goes on, I don't know how to dance, I don't want to know. I just want to cut free, to dance to steps is like painting to numbers. I think it's important. We spend time outdoors, we like to run. If I thought of where we are now, how fortunate, I've got a good job, good salary, healthy family, 30 years ago I never would have imagined my life having this positive turn. I've been very fortunate.

S. In the next five years where do you think you'll be going?

A. Although I enjoy being the Director of the research program, it's not as much fun as doing science. I'm not doing enough; I'm watching people around me doing more science than I am. I had to do it since there was no other structure for it at U of H. Given that I'm the last guy in the world to be a manager or a director, I can't even manage myself. I'm going to be looking to hire a junior faculty who will be groomed quickly to take over that position, and I'll go

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into the background to just work. When I do work, which I haven't done for a while now, it's a very disturbing and wondrous thing. You get so confused, I bite my nails. Everything gets all mixed up and you think you don't know anything. The only good thing is sometimes when it comes together, if it does, you have a better understanding, and that's such a rush. You're the first to see that thing, it's such a wondrous thing. The thing I can't stand about universities is the arrogance of professors who can't sympathize with students who are struggling, and walk around in judgment. They have never lived in the real world; they have just done homework problems their whole life. If you are really struggling because you are doing research, you have sympathy with students who are struggling. There is no way you can feel they are stupid and don't get the point, because you don't get the point either. These people who are taken with their brilliance are people who stopped working, if they ever worked. When a student comes and says they don't understand something, and you don't understand what you are doing, you can say "I know it's hard just keep trying". You are not quick to say "stupid". If you are so smart then why are you struggling? I struggled when I was in school. I had a very bad background; I was a Puerto Rican street gang leader in New York City. I worked very hard to make up for things, I was very fortunate. I had a public education that allowed me to do that when I got my act together. It's always been a struggle, I worked very hard in undergraduate, very hard in graduate, and very hard now. Nothing comes easy to me. So when I see students working hard and struggling, how could I not empathize?

S. What advice would you give young people who are just entering their profession?

A. That's a tough one. When I go and speak at universities on this tour, I tell them I don't really have a crystal ball about what their livelihood, you know, oil, will be. It's hard to know. The only thing I'm pretty sure of, is if they study whatever they are studying in great depth that will pay a dividend. First off they'll learn how you do that. The simplest thing, if you think hard enough, is difficult. They can transfer that to other things. I think we should give them Ph.D.s in seismic physics, but also show them how to solve problems that haven't been solved. That's a transferable thing. 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.

S. Thank you very much for giving us your time Art. It's been a pleasure talking with you.

A. I appreciate it, thank you. R