Transcript of Doug Webb's OASL Seminar on April 13, 2011

Tim Duda:
Thanks for coming and welcome to Doug Webb,polyurethane? who 's visiting us as part of our distinguished acousticians series and engineers. I've known Doug for about 20 years since i came here. Doug originally is from Ontario, is that correct?

Doug Webb:
Correct.

Tim Duda:
He told me when we were working in the field that he was from Howen Sound and I never could find that and it took me a while to figure out where that is, which is Owen Sound. Not being a linguistics expert I didn't quite get that. And then after college in Manchester and in Queen's University, Doug went to the (Baronte?) Electric Company, allso located in Manchester, England, and then from there to the (Olivetti?) Company, and I don't actually know where it is located but he worked at (Olivetti?) for 6 years before coming to Woods Hole in 1962, to WHOI, and he was on the technical staff of the research branch until approxiamately 1982 when he founded the Webb Research Corporation, which is a Falmouth company and one of the sturdy legs holding up the teledine marine conglomerate. Anyway, we've invited him here because of his vast experiences and we will try to learn from some of his adventures. Thanks for coming.

Doug Webb:
Ok. Well, it's great to be invited and thank you for having me. I don't quite know how the conversation here should go, but I am ready to share a few stories about adventures and inventions and learning about the world and glad to entertain any kind of questions. This discussion will be primarily about engineering and I thought I would try to trace a current about how we have depended so much on outside technologies becoming available to us. The thread of the story that I think of really starts with the work of John Swallow in the mid 50's. John made some rather simple, neutrally buoyant floats to look at the movement of water deep in the interior of the ocean. And that turned out to be a very important thing to do. At the time, in the mid 50's, the model of the deep ocean was that there was a slow, almost imperceptible motion of water southward from the polar region towards temperate and tropics. He found that wasn't the case. He put floats in the water and they would move off somewhere at a higher speed than he could reasonably track them for very long. Come back a few weeks later, he was working from a Woods Hole sailing ship out of Bermuda. The ship is called the Ares and it is often called the Ares Experiment. Back in a few weeks, put some more floats in, they would go off in a new direction. He wasn't able to really do a great deal more than that, but it presented a mystery that the ocean was behaving entirely differently than people thought and, we've worked to, he had discovered the mesoscale. Technically, it was kind of an admirable effort. He was using, technically, a nickel scroll for the sound transducer, a scroll about this large with an external winding on it. The electronics were a 300 volt battery and a single gas tube, discharge tube inside that was just oscillating and sending a pulse every few seconds. Well, following that work, in the early 60's, I had started working here at the Woods Hole Oceanographic and worked at evoloving those short range neutrally buoyant floats. We introduced, for the first time, the then fairly new PZT (------) transducers. It was a ring transducer opperating at 4 kHz. We introduced accurate timing, and it was as far as I know the first instrumentation to come from here using transistors. Not yet integrated circuit chips but just individual transistors. Well, I don't think that that group of short range floats really improved our knowledge of the ocean a great deal, but there was a kind of an offshoot of that which was kind of interesting. Part of what I thought was interesting to say is just how thinks get to happen. I had in my notebook some ideas about measuring vertical current in the ocean. No one thought much about vertical currents at the time. I'm not sure that it's a big question right now. But I thought, well, if we take a neutrally buoyant float and put veins or propellor-like eveins on it, it will rotate if water is moving up and down past it and we can measure the rotation with a compass. I didn't think it would go anywhere because at the time, the physical oceanographers were really very conservative, very distrustful of new electronic instruments and they weren't entirely wrong, the (------). As I said, we had just managed to introduce transistors and the stuff before that people had tried to use vacuum tubes, you could see there was a really big problem with that. However, by whatever fortune, the ship Crawford was in the Carribean and was supposed to do an air-sea experiement, the equipment wasn't ready, there was a rush to find someone to use the ship, so Val Worthington arranged that we would take the ship. In 6 weeks, I designed and put together and shipped four of these new vertical current meters, and they worked very well there. That experiement, we learned some things, but where it really payed off was when we then took the vertical current meters to the Medditerranean in the winter for a first look at convection when the surface water becomes sufficiently dense to sink through to depths, forming new bottom water. We did that in '69 and '70, and for the first time, we saw steady vertical flows of 10 cm/s, hour after hour. Hadn't ever seen or measured that and it would be very difficult to approach it with anything other than a vertical current meter. The usual density measurements don't tell you anything except that overturning would be possible, but it doesn't tell you how much or how fast. So that was a nice accomplishment out of those short range floats.

Someone Offscreen:
Doug, I have a question. Were those tracked? The VCM? Acoustically tracked?

Doug Webb:
Yes, they were acoustically tracked, but we had to recover the data from them. I think running through this story from here on, it's going to be several decades of just being really severely handicapped by any kind of data storage . The VCMs introduced for the first time data telemetry, acoustic data telemetry. Primative but it did the job, worked well, and continued to be used that way for several decades. But the successful - to give you an idea of this whole business - the successful experiments on convection, we carried a paper strip chart recorder inside the instrument, and not because we were backward, there was really no other way to do it. Well, all of those together, although the John Swallow experiment had been very important, I don't think we learned a great deal except about convection with those short range floats, but by the late 60's I had started to get interested in whether we could make neutrally buoyant floats which would have a long life and would be able to signal to land based stations and this is really what was the beginnning of the notion of SOFAR floats. I did this work with Tom Rosby who was then a grauate student at MIT and we put together 3 floats. Sandy would remember some of this stuff going on. We used a large sphere for flotation and the sound projectors were large cermaic rings, 30 inches in diameter by 15 inches tall resonating at 550 Hz. An interesting little adventure there. The first 2 that we deployed, the signals did not get through. Now, I'll talk in a moment about the recievers we were using, ocean hydrophones cable connected to shore, that we had to do to get the long endurance, I didn't want to use ship tracking at all. So I had some hints that perhaps - it's very difficult to find out the performance of these (foams/phones?), but I had some hints that we might be suffering from attenuation of the high frequencies in the cable. So, we dropped the frequency to 300 Hz, operated the rings at off resonance at a much lower output, and the third one worked. Well, that was kind of helpful because the next thing that happened was going to be the possibility of initiating what became the MODE I Experiement. And in terms of luck and things that have surprisingly good results, that was an interesting kind of thing. In the early 70's, there was in the United Nations a great deal of discussion about the law of the sea. (Dom Mentov?), the representative from Malta was in charge of these discussions, and, as I understand it, the American ambassador made a very inept quote and it upset a lot of people, upset them badly in these law of the sea discussions. So without much review or discussion, President Johnson made a television announcement that the United States was going to sponsor the international decade of ocean exploration and with some substantial sums to do it. An interesting statement as well is that it was an international decade of having nations join together to do this work. Well, Hank Stommel had for some time been thinking of - I'm sure he wasn't alone - but, he had been thinking hard about resolving the mystery that Swallow had unlocked in '55 and came forward with the notion of the MODE I Experiement, try to see what was really going on in the interior of the ocean. It was rather an exciting possibiltiy. For the first time, it required a considerable group of scientists working together to a common goal. That was not really the MO for most investigators and it was quite a challenge. It worked out very well, but there was a lot of discussion, we really brought together the resources of a large number of people and occupied a piece of ocean and looked at it intensively. I apologize for many of you, many of you have probably read about MODE I and the results, but it opened the door to walk right through with the SOFAR technology. And that was in a way a substantial adventure. Start with ocean data, what did we know about noise and attenuation in the ocean? There was remarkably little data and what there was was often in conflict. We had to master the use of these hydrophones which were - there was another piece of very good fortune. At the time, acoustic surveillance of the ocean for subermarines was a large effort and a very secretive affair. You could get into trouble just by talking about it. However, by very good fortune, (Gordon Hamilton?), who was in charge of the SOFAR station in Bermuda, was one of those rare people who kept in his mind clearly what was classified and what wasn't and helped us to get helped up with what were some deep hydrophones of the missile impact system. The early missile tests that they landed in the atlantic dropped a SOFAR charge and the impact was located by where these hydrophones could triangulate the position of that sound. So we gained access to those. That was crucial. It took a substantial effort, but it was just a piece of very good luck. We had to sort out a signaling system, but the real problem was about a projector for these floats. It seemed pretty clear that we weren't going to move ahead with these 30 inch rings. They performed very well, they are still a proper technology today, but very costly, a little bit fragile but mostly costly. So I set out to try and decide how to solve that little problem. It was a big problem. We wanted a frequency of order 250 Hz. It had to operate deep in the ocean at the channel axis. And it had to be of good efficiency because it had to operate for months at a time on battery power. Well, there was really, as far as I could tell, nothing available that would meet those needs, so I made - I talked to everyone I could find about that. It's interesting looking back, some of the people are now probably known to many of you. I got a lot of help from (Ralph Roult?) who was at the underwater sound laboratory and was very well known at that time. And I had an interesting story talking to (Fred Humpt?) who was in charge of the Harvard underwater sound laboratory, which was a big activity during the Second World War. I visited (Humpt) in his office on a day that he was cleaning up his office for retirement and he was sitting there, shuffling his papers as we talked, and he reached into a draw, pulled out a letter he showed me and said this was a letter that his father had recieved from Lord Raleigh. Well, all of that didn't really advance us much about the projector, so I essentially talked to myself about it, I said you're at a fork in the road, either you have to

become a projector designer or abandon this line of research. So I decided to press on and the person who I had talked to, became a very important person, (Claude Sims?), who had worked at the Navy acoustic laboratory in Orlando and had started a small company of his own in Orlando, he didn't have an answer but I thought Claude talked the most sense about this. And we worked together for quite a period of time, and Claude guided us to essentially the organ pipe transducer, a 12 inch cylinder with a piece of ceramic bender face at one end, still in use today, simple and insensative to pressure, a reasonable efficiency, and that was really a crucial step. Of all those problems about making SOFAR work, that was probably the most challenging. We got 20 floats in the water for the MODE I Experiment. We had an interesting field adventure during that time. On the projectors, the ceramic on the bender is isolated from the sea water by caster oil contained by a polyurethane membrane. There were 2 projectors on each float, and we found during the experiement that the cementing of the membrane to the aluminum was breaking down and letting seawater into the face. By very good fortune, we had, during this experiment, people at three of the recieving stations and the main line of collecting data was a recording system which put it on a simple cassette recorder. That was the first time we really had a proper recording medium. It would be laughable today, I think we had 1 mega-bit of storage. We thought that was gigantic advance. But we had purchased some surplus Navy fax machines and converted them to record the signals in the stations and the people in the stations were all keen HAMs radio amateurs. They kept in touch with each other and kept in touch with the ship, we knew where these floats were that were giving trouble, they had a recovery system, we found them, called them up, took the face out, put it in the ships (laeth?), we made a 60 ton manually operated press and pressed in polyurethane faces and refilled them and reballasted them, got them back in the ocean, and they went on working for another year or so. And that was an adventure with that. Those became, I shouldn't say popular, but they became accepted as a part of the oceanographic toolkit, and probably on the order of 150 of those floats were made before we finished. It led to a request to make a self-contained recieving station. And we could start to think about that because we had for the first time the capabilities of microprocessors. We had gone from making things with individual transistors to integrated chips, which would just be a (flipflop?) and gates. By this time, we had the microprocessors availble. (El Bradley?) led that charge. We changed the signaling system from what we had used in MODE I to use an FM sweep, which was rather a novelty at the time, an 80 second sweep over 1.5 Hz with coherent summation at the reciever. And as that went ahead, Tom Rosby, who was very much still involved in all this work and was a major partner in all this work, created the RAFOS float system, which is still operational, where he essentially inverted the system and made more transmitters and drifting recievers, and that equipment is still being manufactured and used. That opening was made available to Tom because of the availability fro the first time of microprocessors. The 1802, it would seem pretty primative today, but it was a big and valuable deal there. Well, what comes next? A current of thinking that I was following, in any case, was kind of interesting. In a sense, it was a personal story, but it may be worth thinking about. I really was starting to think that the biggest handicap facing us about learning about the ocean, a common kind of thing that people had meetings about, I thought the biggest handicap was the size of the research fee. How are we going to really understand the oceans with 25 ships? The next 2 kind of developments was an effort to cope with that. So, in '82, as Tim has said, I started a company here in Falmouth and set about trying to make ocean profilers. Initially they were called (ALICE?) and they have other names now, but essentially they were neutrally buoyant devices which drift at depth in the ocean, ascend to the surface at intervals, report their position, go back down again, and keep on doing that for several years. It was an effort, compared to SOFAR or the RAFOS that Tom Rosby had been doing, to see us get global coverage, long-term coverage. They were intially aimed at providing trajectories of ocean currents. They bevolved to carry sensors. Initially, we were fortunate in that the Sea-Bird system was available and of adequate accurate and of adequate power, low power runs through all of these devices because they run for long periods of time on battery. It was only possible because, for the first time, we had available the ARGOS satellite system which could tell us their position and handle the data coming back. So again, we were just exploiting these new things as they came along, first the transistors, then ICs, then the microprocessors, now the ARGOS, and today using GPS and (iridium?) has made all that possible. That work on the profiliers was done together, because everything takes a lot of people, and I'm not at all acknowledging all the people involved, but the profiliers was a joint effort with (Ross Davis?). It seemed to me that the profiliers were pretty good, but they really worked well when you got a lot of them. Half a dozen profiliers are nice, and that's worked out very well, and many of you may have seen these kinds of plots, but that's where the world's profiliers were on Monday (shows a map of the world covered in dots). There's 3,214 of them. So, now that says we've really made some strides to being able to monitor the interior of the ocean. It is still grossly undersampled, but it's not outrageously undersampled like it was before. So, that worked ouut fairly well. Now, things like the profilers are only as useful as the sensors they are carrying, and they are carrying an increasingly wide range of sensors. Well, that got underway fairly well. And, following on that, we come to gliders. Well, I had thought about some long range vehicle ideas, and wrote up some notes in the 70's, but it wasn't going to hang together very well. In February of '86, I thought I could see a reasonably complete synthesis of a glider using thermal propulsion, that is harvesting the thermal energy in the ocean to keep it moving. And as part of the adventure story, it's interesting that I spent about 2 years talking about that to people who said, well, Doug, I don't think it will work, and if it does work, I don't think it's very useful anyways. However, once we demonstrated a glider working, it really took off, and, for the first time - the kind of conventional wisdom is that a new instrument in oceanography usually takes something like 10 years to go from a proof of concept to an accepted tool. The gliders, after the very skeptical period, once demonstrated, they took off really too fast, that people wanted them, and we were in the environment of a small company and had to keep moving, and were shipping prototypes and we hadn't really thought it out. For fairly complicated affairs or anything like that, it takes a lot of internal smarts to look after it, so. You know, you try to get things wet and show that they're working, but that's really only a start. The ideas about thermal propulsion were bypassed by going with battery power, that made it simpler and more straightforward, and to-date we have made 200 gliders and they are primarily battery powered. We've made a few thermal gliders, Tom (Patterson?) and I have put together 2 which worked well but failed prematurely and we're trying to overcome that. They ran for 200 plus days and we don't really know why they failed. Now, I'll go back to acoustics. I've spoken to you about making the SOFAR floats with their 250 Hz signaling. During that period, in the mid 70's, Bob (Spindell?), who was doing acoustics work here, said, well, what do you say we use something like the SOFAR float for the sound research that I'm doing. They were doing sound transmission research using a moored projector at Eluthera, and they were a little concerned that the Eluthera source was getting old and might not last. And os I told him what we could do, and the answer was, well that is an almost laughable sound pressure level, that will be almost never useful. But we decided to do it anyways. And they came back from the cruise, (Sentry?) said that's great, we've got it under our control, we can move it around and do these tests in different places where we really want to do them. That opened up that possibality and some Navy labs purchased sources for experiments that I was never able to find out quite what they were doing. But that changed, in 1978, Walter (Monk?) and Howard (Munch?) described their ideas about ocean acoustic tomography. And at (Scripps?), they had been doing some esssentially tomographic experiments with comparatively high frequency sources, and it was thought that really - they asked if they could get a greater range. And there was somewhat of a forced marriage between (Scripps) and Woods Hole. Here we had these lower frequency sources and we started on that. It was kind of interesting, in 1980, we took several sources to sea, the first built specifically for tomography, and it was not a great success. They were built, they followed the sort of general SOFAR technology, they were built with a phase code and a bandwidth of 16 Hz. When it came to looking at the results, it was clear, I was surprised at the time that it wasn't there before the experiment, that that was not enough bandwidth to resolve the individual arrivals. However, those 1980 sources are still around and still being used. They were fairly rugged. So that led to the next major experiment. (Karl Vunch?) commisioned the building of 5 sources, we started in 1986, which were to be 100 Hz bandwidth. Now, I thought there's an interesting story there. I was, myself, very concerned about the 100 Hz bandwidth, because we knew the organ pipe projectors did not have anything like that much bandwidth. We built it on a kind of a best-efforts basis, and there were 4 projectors on the source and they were all tuned to a slightly different frequency. And, from my perspective, the world divided into 2 groups. There was a group who looked, measured, and looked at the performance of the system and said this is hopeless, don't bother with it. And there was the group who put them in the ocean, did experiments, and got quite good results. And in a sense, you know, they are all smart people and they were all to a degree right. I don't know what others would say about it, but what I think really happened was we got quite narrow pulses, 10 millisecond, you could see a formal 10 millisecond band, but there was a long kind of sloping tail on the pulses, presuamably from energy rattling around inside the projectors as they tried to change phase. I mention that just because there were 2 kinds of reviews. We got some good experiments, and it was formally a lousy system, just what kind of lousiness is important, and that was not really sorted out. Does that make sense Tim? Would you say it that way?

Tim Duda:
Sure, would you like me to draw the pulse?

Doug Webb:
Sure, yes.

Tim Duda:
This is the famous pulse (draws picture on board and describes it). So they were half good sources. Half the energy's useful, half the energy is not, so both groups were right.

Doug Webb:
Welll this story goes on just one more chapter. How to cope with this? My own sense is that we then spent a very long time looking for a porjector that would do the job. And I've felt that the crucial problem was the statement that we wanted to see results of 10 milliseconds. Formally that means you need to have 100 Hz bandwidth and 250 Hz carrier, and at the time, I feel these are very personal comments about it that may not stand up to widespread review, but the field was dominated by the wonderful success of jamming information down copper wires for modems and fax machines and those were all poahse codes. I thought to myself, man, this isn't going to work. This is not signals going down a wire, if you've got some large mechanical structre trying to change the volume of the ocean and you ask it to suddenly change pahse 180 degreees, that's the last thing it wants to do. We'd been thinking of unconventional approaches. Walter (Monk) was kind of interested in unconventional approaches (like/light) sweep. Tim supported the notion of writing a nice paper about what could be done

with FM sweeps. And we finally got some traction in 2 ways that ONR gave us some money, a phase I SPIR to look at this and Andrey Morozov joined us at Webb Research and the two of us put together the sweeper, which the basic notion is to deal with the problems I have just been mentioning by taking a highly efficient narrow band projector and mechanically retuning it to be resonant across the band, changing the statement that to get 10 milliseconds resolution, don;t say you need 100 Hz bandwidth, say the signal must have energy across 100 Hz. Would you agree with that?

Tim Duda:
That sounds right.

Doug Webb:
Well, it changed the way of thinking about it.

Tim Duda:
So what you're saying, is you don't need 100 Hz bandwidth throughout the transmission. You need it to occupy 100 Hz, but not all the time.

Doug Webb:
Yes, you need to occupy with energy. So in the last few days, Andrey has shown me that 6 of those sweepers have been recovered in the Phillipine sea. As far as we know, they all work. There's the 50 year story folks.

Ken Foote:
Time for questions?

Tim Duda:
Yes, go ahead.

Ken Foote:
You mentioned the use of PZT at an early time, but for some of these large robust projectors, are you using any magnetostriction to drive these, or can you explain that?

Doug Webb:
Well, you know Andrey there? Yes, well Andrey and I talk about this from time to time. For a while, in the mid 80's, magnetostriction was a hot topic. That is, it had been improved. What John Swallow had used was nickel magnetostriction, but nickel doesn't have as big a strain, so the (turfanol?) materials came along at that time. They haven't resulted for us in many useful devices. Is that fair, Andrey? The Japanese made a low frequency projector. I understand it cost several million dollars.

Ken Foote:
I would like to ask the following question. I should first say that it was jsut a marvelous presentationl, thank you, it was very interesting to hear your recollections about this history.

Doug Webb:
Thank you. There's actually an interesting sotry about PZT, (Lead zerkanate titanate?). It's to a degree an engineering and commercial story, but (Cleavite?) developed barium titanate for the Navy. Big advance because before that it was (roschell scholtz?) was the active material. But because it had been done for the navy, it wasn't available for the public use, so they went ahead on their own money and developed (lead zerkanate titantate) which was an advance over barium titanate. And it was thought that Cleavite now have this locked up, they've got it pattented under their own name, but I fell into working with Bob Carlson who had just started Channel Industries, and he had been a Second War naval officer, starting a little naval business, so he just marched up to Cleavite and said can I have a liscence for PZT and they said sure. Always pays to ask the question.

Ken Foote:
You mentioned (turfanol?) but there is now a gallium based equivalent, I'm sure you know about it, (Penn States involved in galfinol?), and this thing is strong under tension, not just pressure. Is this going to maybe influence what you're doing?

Andrey Morozov:
Yes, Doug, we discussed it, (------). We didn't see a big advantage for it, at least in our designs. And there are a lot of additional problems with making high efficiency models. It's not so easy. You get efficiency in one direction, you lose it in

another.

Jim Lynch:
Can I ask a quick question? One thing that's kind of fun, I mean after 50 years you see evolution, and you get all that in the process but on the other hand you get to (see/say?) a lot. But the thing that's cool is that you went from floats to drifters to gliders and tomography and the question is, are these going to interconnect in the future? What's the future of these, are they going to be individual, are they going to connect? Where do you see people going with these? And include AUVs. I mean, you've been looking in the future of a lot of technologies and the future of a lot of these seem to be linking, and where do you see these linking?

Doug Webb:
Jim, in my own head, I see it as kind of 2 paths, that - I've been mostly engaged in what I think is the problem of the size of the research fleet, which was the profilers and then the gliders. The recent work on the sweepers was really a leftover from SOFAR technology and a kind of frustration because I thought we were asking the wrong questions about how to create the sound for tomography, it's only a fraction of whats required in tomography. And that had a very long gestation period, and it got unlocked both by the support of Jen Lyndberg at ONR who gave us that crucial SPIR program and by Andrey coming, and I think the sweeper, neither one of us could do it by ourselves, but together we made it work. And that's now largely in Andrey's hands.

Tim Duda:
What about, to follow up on that, what about a mobile system I mean a -

Jim Lynch:
A combination system.

Tim Duda:
A tomographic mapping with gliders and floats. I mean, we've talked about that, of course, among the scientists for decades, let's put recievers on (Alice ---) at the time and put a source out and then we get a lot of paths and find structure. Does that - Is there a power constraint on that, or why haven't we seen that? Have you thought about that Doug?

Doug Webb:
I think we need to talk about the design of an experiment.

Tim Duda:
So you see it as possible, it's just the desire hasn't been there to build such a thing.

Jim Lynch:
A lot of these technologies seem like there could be a lot of nice interlinked synergy between them. I mean, they've all matured over the years to the point of they're all operational kind of on their own, but the combination seems to be an interesting question.

Doug Webb:
Well, running through what I've been saying, often it's just a fortunate set of circumstances brings together the outline of an experiement and the people and the technology.

Joe:
Doug has often mentioned using gliders as station keeping, keeping them in just one location but acoustically being able to talk to more instruments down deep and then having them use the glider to come up to the surface, transmitting data. Is that what you're talking about?

Jim Lynch:
No, no, I'm talking about - you know, the stuff Doug's been doing has been on a global scale, and now when you talk about this tomographic system that can communicate with all these floats or communicates with the glider system that's out there, just cross-wiring all these different systems to be in synergy with each other, it seems to me that - well, they're not separate technologies anymore, they're a bigger system and a better system. I was wondering if you are looking in those directions, wondering if there's anything going on there. It just seems to me that you've got 4 incredible scores right there, with, you know, the floats, drifters, gliders, and tomography, and nice interior ocean measurements. It just seems that a lot of the things you do after a while is you do cross-discipline or inter-disciplinary. So I was just wondering if you guys are kicking

around on any of that stuff. What you're talking about seems like more of a data communication, which is certainly part of something.

Andrey Morozov:
You should probably make a similar (---) about it.

Jim Lynch:
Ok Andrey, you're on it.

Ying-Tsong Lin:
I just wonder how difficult is it to put the source in the glider and operate from it?

Jim Lynch:
The source is too heavy for a glider. For an AUV maybe, depends on what source you use.

Doug Webb:
Yes, it would be difficult, but -

Andrey Morozov:
(We could do it?)

Ying-Tsong Lin:
So using like a resonance tube that transects.

Jim Lynch:
You think even in a glider you could do it?

Andrey Morozov:
Yes, yes I do.

Ying-Tsong Lin:
But how about power, (----) sufficient?

Andrey Morozov:
The (---) doesn't really need a lot of power, we are using reasonable power, 80 watts only, our sources are very efficient. 80 watts is a fraction of the power (-----) and we will transmit probably a few times a day, so I don't think it's a big deal, we have such resources on a glider.

Doug Webb:
But to put a reciever on a glider is quite simple.

Andrey Morozov:
Reciever definitely can be done.

Ying-Tsong Lin:
So we could have a mobile source, tomography source.

Jim Lynch:
But this is interesting thing because the (power vehicles?) you can put sources on quite nicely and we've done and Andrey's done things like that.

Andrey Morozov:
Well yes, but we have to (---), there's not a lot of power you have to think about, If you need to communicate with the glider once a day, on a longer mission you need only 30 transmissions in (one?), and we definitely can transmit (70?) times with the system we have.

Jim Lynch:
So it's not so bad. It just seems maybe we'll take Andrey up on doing a seminar.

Doug Webb:
Well the blessing that we got is that Andrey is at both places and we talk regularly.

Ken Foote:
When you were trying to achieve the large bandwidth at low frequency, did you ever consider parametric sources, just to have access to the bandwidth?

Doug Webb:
Yes, of course. Do you want to adress that Andrey?

Andrey Morozov:
No, I'll leave it to you.

Doug Webb:
It was not efficient enough.

Andrey Morozov:
Very low efficiency.

Ken Foote:
Well, yes, of course, it's low conversion efficiency, however the access to that bandwidth and the use of complicated signals allows you to use cross-correlation match filtering to recover -

Andrey Morozov:
But you can make the same bandwidth without -

Jim Lynch:
Correct, that's what they do. It's all cross-correlated, Ken. Either M sequences or sweep sequences.

Ken Foote:
You know, cause doing it linearly and -

Andrey Morozov:
Yes, and (---) your source is too broadband.

Doug Webb:
Well, you know, the parametric sources it always seems to me are definitely attractive because of their high directivity, that you can get them pointed at the right direction so that their efficiency isn't as big of a disadvantage. Therefore things like tomography generally we can't take advantage of the directivity.

Jim Lynch:
Or you can do it with an array if you have to, you can do it with a transciever.

Ying-Tsong Lin:
So they are very good for high frequency acoustics.

Jim Lynch:
Well, with the parametric array, you look at the bottom a lot,because you get a lot of low frequency that can penetrate the surface and it's directive so that's a better use than tomography

Someone Offscreen:
Different type of - different type of scientific goal.

Ken Foote:
I understand from some of the work currently in tomography there are still very much unresolved issues on timing, time of arrival, and speculation maybe about side-load involvement, that is extraneous interferrance, extraneous arrivals. Do you have any comments on that?

Doug Webb:
If I may, in this tomography business that Andrey and I have done together, I have primarily dealt with the engineering and Andrey with the acoustics and science. What do you say about that, Andrey?

Andrey Morozov:
Actually, can I get a little bit more information about your question? You're saying that timing is not really, I mean, it's like, disturbing by some kind of effects connected to the ocean, or what?

Ken Foote:
There are central problems with accounting for arrival times and differences due to multipath which are unexplained.

Andrey Morozov:
Ok, I got it now. Yes, there is of course gloabl scale, which is moving (----) to tomography scale, and there is small scale scattering due to internal waves. And this is the main scale. There are two areas. It is probably (---) scale also distorting a little bit, but this is most near the surface, and there are internal waves. And the (-----) developed a very good theory about internal wave disturbing of this time of arrival. And you can read our papers and we now don't need a (Monte Carle?), we can use this (----) statistical (---) approach, we can complete by one run, we are getting so close in our agreement with (Monte Carle?), it's amazing. So it's actually a very very studied effect now and we can predict - and we now are even talking about internal waves tomography - because for looking of this scattering of this separated arrivals, we can predict what was the intensity of the internal waves.

Tim Duda:
I have a question about bandwidth. I remember you were talking about the 223 or 224 Hz sources on the 16 Hz bandwidth, which we use in shallow water and then when we can't resolve the individual arrivals because of the echoing of the shallow water wave guy obscures that individual arrivals from a multipath ray model for example, but in the deep ocean Andrey and I did some simulations and it appears that you can see you only need 14 - that's the number we can up with, a 14 Hz bandwidth will allow you to see the individual arrivals at a long range in the ocean and then there's some cutoff where the individual arrivals get close near the end of the pulse and then no bandwidth is enough for that. But I would be interested in seeing what the problem was in the deep ocean with the 16 Hz because it seems to me that that should have been useable. I know that you were very successful with the 100 Hz bandwidth.

Doug Webb:
Well, of course, all is not lost with the 16 Hz badnwidth, but it's a smeared picture.

Tim Duda:
Correct, but if you know what you're looking at, you can deal with the smear. And I think perhaps we know more now about what we're looking at and with all internal waves, scattering research and so on, the appropriate averaging scales are known so that you can extract what you need from the lesser bandwidth. I think that's where we are now.

Jim Lynch:
Yes, we're better at it now. In the beginning, when they started it, if you didn't see a result (----) it was useless, right?

Tim Duda:
Right. But I think that you could do something with a 16 Hz badnwidth.

Jim Lynch:
In shallow water, that thing shows the modes beautifully.

Tim Duda:
Yes, yes, it's great for modes. Anyway, it's interesting, I just perked up when you said that that bandwidth wasn't enough because we've sort of analyzed the heck out of it and thought that it would be a useful bandwidth.

Ken Foote:
I'ld like to ask a very different sort of question. In your background, you spent some time at Manchester and did some things there that are maybe considered first in terms of programming. I'm just curious about the experieince being there. I guess in the 1950's, maybe still a postwar (embarrment?), and just curious what your observations might be on the climate in the university, in the city, and generally in the society. Well, that was a different experience, and perhaps we could chat about

that more, the two of us. There was an effort, especially at Manchester, where they created the first stored digital program computer. They were largely led by several people who had spent time, as it happened, in the U.S. who had worked on radar during the Second War. They really had some ideas about storage, which, you know, was really a major holdup, a terrible holdup if you were working with vacuum tubes, and they built a store and then decided they had to build a computer to test the store. It was kind of a backwards way around. They were able to take a box the size of like a small refrigerator and store about 250 bits. But high speed random-access, you could get at it. For myself and a fellow graduate student, we built the world's first computer system using transistors. The germanium point contact transistors. I may tell you my favoirte story about the importance of asking the right question. In the mid 30's, in England there was a big concern about aerial defence of the island and a civil servant of London was asked to write a report about what was to be done about air defence. I guess he was a consciencous man and he had heard about death rays that would destroy an aircraft or kill a pilot in the sky and he said, well I better include this in the report. So he sent off an inquiry to the National Physical Laboratories where it arrived on the desk of a Scotsman, Watson (Watt?). And (Watt) looked at this and sent back a replied, he said, you asked the wrong question, don't think about using radiation to destroy an aircraft, think of using radiation to detect an aircraft. And that was the start of radar. Watson Watt was a frugal Scotsman and it happened to be the end of the month and he tore off the sheet on his calendar and wrote on the back this historic repsonse and sent it back to London

Tim Duda:
Thank you very much.