

## ARNOLD ARONS TALK AT AOPE DEPARTMENT

10/11/00

**INTRO:** ... familiar name for the physical oceanographers. I would say that you can't go to three physical oceanography lectures consecutively before someone mentions Dr. Arnold Arons (Inaudible) circulation. And that work dates back to the 50s. And I was surprised to find out that you did acoustics earlier than that here, apparently in this building. Dr. Arons is a Professor Emeritus at the University of Washington, and previously had taught at Amherst College and Stevens Institute, and has received numerous awards in both physics teaching and research from many societies. He's a fellow of a couple of societies. So it's a great honor to have you, and we're eager to hear what you have to say.

**AA:** Since I'm going to have the temerity to talk about shallow water acoustics (Laughter), I feel intimidated by the presence of so many experts. As you obsolesce as a scientist, if you live long enough, you turn into a historian. I guess that's what's happening to me.

So let me start with a few remarks about what transpired here during the Second World War, before most of you were born,

I guess. That's a preliminary to the work that I will be talking about, in fact. I assume most of you know that, prior to the Second World War, this institution was essentially a summer operation. It had virtually nothing in the way of a permanent staff. Bigelow was director, out of his real seat at Harvard. Columbus Iselin was a full-time employee, and Bill Schroeder(?), the ichthyologist, was sort of permanent caretaker. He took care of the place when it was locked up during the winter months. And the summer operation was very much the way MBL used to be -- namely, people would come from the academic institutions. Iselin essentially conducted whatever physical oceanography was going on. I believe the ... I'm not very well versed of the details of what went on then because that was before my own time, but one of the prominent biological enterprises was microbiology under Selman Waksman. He got the Nobel Prize for the discovery of streptomycin(?). His headquarters was Rutgers. And he came here with his students, who became a rather distinguished coterie all around the country. So that was the general atmosphere of the institution prior to 1941.

Now, with the advent of the Second World War, beginning a little bit before Pearl Harbor, but then developing rapidly

after December 41, there were three principle operations that came into being on a full-time, year-round basis here. One was in biology, and incidentally, at that time, the only building that the institution had was this one here. None of the other buildings existed. Out on the dock, there was a little brick house we called the Pump House, which had the pumping machinery that supplied the salt-water tanks for biological work in the building. That's all there was. Now, the group in biology was a substantial one under Alfred Redfield and Buck Ketchum and a number of other people whose names I don't recall offhand, although they come back after an interval. The problem was bottom-fouling in ships, which was a serious wartime problem, incapacitation of ships that should be active because of biological growth on the bottom. And that group developed the copper-based anti-fouling paints that still persist in the maritime practice. They occupied the second floor.

The first floor was occupied by Maurice Ewing and his group, who were concentrating principally on underwater sound. Al Vine was then a young graduate, one of the leading members of the group, Joe Worzell, who left here with Ewing subsequently, but they were the key members. They discovered the SOFAR channel and set up the system for triangulating boats in

distress after sinking, or merchant shipping and so forth. And they greatly refined and improved the bathythermograph, which had been started by Avelstan Spilkhaus(?) at MIT, and they did extensive work on ray tracing, ray propagation. Al Vine spent a good bit of those war years visiting active submarines around the war circuit and instructing them on how to use shadow zones to hide from sonar detection based on the ray tracing insights and so forth that they were gaining here. So that was the enterprise of that group.

And there was a sprinkling of smaller projects that I won't dwell on. I'm picking out the big ones. The third group was the one that I was involved in, and we were the last of the big three groups to come to the institution. Our program was started at Harvard. I was then a graduate student of E. Bright Wilson, Jr.'s. Wilson, who had been doing infrared and raman(?) spectras(?) structure of polyatomic molecules, had been commissioned from Washington. The big enterprises set up in Washington were the OSRD, the Office of Scientific Research and Development, under Vanavar Bush, which had overall purview of the scientific, engineering, technological development in the war effort; and then there was a science operation, the NDRC, the National Defense Research Committee, under James Conant, who

was president of Harvard. And the NDRC had a number of divisions: acoustics, explosion phenomena, electricity and magnetism, mine warfare -- all those problems of magnetic mines and whatnot. The explosive division was headed by George Kistiakowski, a physical chemist from Harvard, and he entangled his colleague, Bright Wilson, who was then a junior member in physical chemistry in that department, to abandon infrared and raman spectra and to turn his attention to explosion phenomena, in water and air. And I was then a beginning graduate student of Wilson's and had started to modify the infrared spectrometer for the purpose of measuring absolute dipole moments of polyatomic molecules. Well, that went by the boards in a hurry, as Wilson called in all his graduate students and announced that he was switching to explosives research (Laughter) and invited those who were willing to join him to stay and told the others who didn't want to do that kind of thing to find another thesis advisor. Well, actually, all of us stayed. This was, to pinpoint it, three months before Pearl Harbor.

Now, we started working on the explosion phenomena immediately. There was very little known about the shock wave generated by an explosion. You see, it took obviously rapid instrumentation in order to detect and see what was going on.

Attempts had been made during the First World War, which didn't really get very far, and so here in 1941, the question was, what is the shape of the shock wave? We knew it was approximately exponential, but what was the decay constant? How sharp is a shock front? How would the amplitudes scale? And then, of course, the phenomenon goes beyond that because once you have a detonation take place, the gas globe expands and overshoots and comes back, and you get another pulse, and there's the hydrodynamics of all that stuff -- all of those phenomena.

Now, in 1941, what we had available for instrumentation was the Dumont 208 oscilloscope. It was the first oscilloscope in which you could hope to get ... or you had to modify the electronics because the sweep on it was non-linear. But if you developed a better electronics, you could get a linear sweep and the amplitude electronics were reasonably linear. They needed calibration, etc., but it was a device with which you could begin to do quantitative work. And I found myself, rather than monkeying with an infrared spectrometer over in the mineralogy lab at Harvard, learning how to use a diamond saw and cutting crystals of tourmaline. Now, we selected tourmaline as the piezo-electric device for getting a signal for a number of reasons. The obvious thing to most individuals would have been

quartz, but quartz is not hydrostatically sensitive. In order to get a piezo-electric ... if you subject quartz to hydrostatic pressure, you get no piezo-electric signal, no charge. Quartz is sensitive along the principal axis. It's also sensitive along another, but only unilaterally. So if you wanted to use quartz, you'd have to put it into a housing, and if you put it into a housing and subjected it to a shock-wave pulse, you were going to get serious ringing. And so what was indicated was something hydrostatically sensitive, and tourmaline is hydrostatically sensitive, and just about the same sensitivity as quartz. There's no real distinction between them. And so I was assigned the task of creating a piezo-electric gauge that we could attach to the oscilloscope.

Bob Cole, who subsequently became trustee here, he was senior to me. He took on the ... he was a very skilled electronics man, as well as high-powered theoretically, and he undertook the task of generating the electronic equipment. I was assigned the task of creating the gauge. And I won't go into the details, but what was necessary was to cut tourmaline slabs, plate them so you had a conductive coating. One slab was of limited sensitivity; what we did was pile up four slabs in parallel, so you got more charge for the smallest possible

radium, but you had to provide a conductive coating, cement them together, provide a waterproof coating that would stand up under shock-wave pressures attached to a cable. So that was my principal enterprise personally, with a lot of other stuff going on.

We started verifying the techniques in a tank about five or six feet in diameter and about six feet high in one of the laboratories at Harvard, and we shot blasting caps and got a lot of complaints. (Laughter) And it was obvious that, to go beyond blasting caps, this kind of work could not be done in the middle of Cambridge. So Bright was searching around for somewhere to go where we would have access to open water, and it had to be more than just a pond kind of thing because there loomed in the future the necessity of testing full-scale depth charges and things of that variety. So Bright discovered the Woods Hole Oceanographic Institution and approached Columbus Iselin on whether a set-up could be brought here, and Columbus said, "Why, of course, come along." And the operation was moved here. And our group was increasingly rapidly in size, and we occupied the third floor. My original office was up in that extreme corner of the third floor.

I vividly remember, before we actually moved, Bright and I



came down here by car. Bob Cole, who had been setting up the electronic system out at Brewston(?), Pennsylvania, moved here with the electronic equipment. Bright and I came down from Harvard with the gauges. We put the equipment out there in the Pump House and rigged a ... we had a steel ring on cables, which we lowered off the dock and shot our first charges of maybe 100 grams of explosive instead of a blasting cap. What I remember about that time was that it was about the middle of February, 1942, and there was a week of weather in which the temperature did not come above zero Fahrenheit. It stayed around zero. Now, I've never been so cold in my life, working out there on the dock. When we raised that rig out of the water, icicles formed from the salt water. Well, we learned to dress more practically on subsequent occasions. But anyway, the group moved here, and we operated throughout the Second World War.

My assignment was the physics of the explosion: the scaling of the shock wave, the pressure, amplitude. You know, acoustically it should be  $1/R$ , but with finite amplitude you get dissipation, and the radio dependence is 1.13 rather than 1.10. And that, incidentally, extends over a phenomenally large range, down to pressures of only a few pounds per square inch, almost, acoustic levels. But it continues as a 1.13 straight line semi-

lock plot(?), as far as you can go -- as far as I was able to go. One of the things we were looking for eventually was, does it change, the slope? And we never found any change in slope.

So that was the kind of thing we had to work on, from a wartime practical standpoint. The physics was obviously an underpinning. The questions were, what are optimum compositions for explosives? TNT was becoming old hat. There were new explosive compounds: HBX, pentalyte(?), various other things. You enhanced the pressure pulse by adding ammonium nitrate, aluminum, etc. So ultimately, for manufacturing purposes, the questions were, what mixtures are optimum? And this was obviously a serious economic problem. You were determining the national wartime effort on optimum explosive compositions.

We had a casting house over on Nanomasset(?) Island, plus magazines where we stored explosives. We leased the northeast end of the Nanomasset, all the way down to where the woods begin, the end of the meadow. We had a leasehold from the Forbesees. So the delicate work was done over there. And in the meadow, between the house that you still see in the woods, they would deliver full-scale depth charges to us, you know, bounce them on the dock out here, put them on a boat, take them over, and bury them in the meadow till we took them out. We set up a

larger boat, a Gloucester fisherman, to shoot larger charges out in the Sound. We did the small-scale work right here off the dock; the experimental compositions were poured over on the island, etc. We studied the pulsation phenomena, we studied the surface effect. We had a group under Paul Fye studying the collapse of cylinders under explosive impact -- in other words, models of submarine hulls -- and the scaling problems that went with that. That was the nature of the enterprise.

And those were the three big operations that went on here during the war. For some reason or other, it was out of the ... well, Alfred Redfield and Buck Ketchum were permanent staff members here. They were here all the way through. But out of the explosive group, there came six individuals who entered the trustee structure of the institution. Bright Wilson was the first one. He was a long-time trustee, a member of the executive committee. Spike Coles, who was junior to Wilson, was one of the research supervisors. He supervised the full-scale work out on in the Sound. He went to Brown as a physical chemist; he became dean there, and quickly became president of Bowdoin College and subsequently president of a research corporation. And he was a long-time trustee. Spike chaired the committee that created the joint graduate program with MIT.

Bright Wilson was taken away from here; Kistiakowski left Division 2 of NDRC to head the explosives work at Los Alamos, and Wilson was made head of Division 2, the explosives division, at NDRC. So he was succeeded here by Paul Cross, who was professor of physical chemistry at Brown, and Cross subsequently became a trustee. Cross left Brown and became head of the chemistry department at my place, the University of Washington, then he became the head of the Mellon Institute of Pittsburgh, and he sat on the combination of Carnegie Institute of Technology and the Mellon Institute that became Carnegie-Mellon University. He was a trustee. Bob Cole went to Brown, made a career in di-electric(?) phenomena, and was a trustee and member of the executive committee. Paul Fye became a director, and I am the sole survivor of that crowd. I became a trustee in 64, and I sat with Spike on the committee that generated the graduate program with MIT. So that explosion phenomena group had extensive connections with the institution, as you can see.

Now let's get down to some science. These are just qualitative stories. I got into oceanography by staying on here, coming back ... I went back to academic work, but I came here summers, with students, and I knew nothing whatsoever about oceanography. I had hardly heard about it before. But what I

did initially was simply to continue work that we had started during the war years. There were questions left over, and I had funding for that -- there was money left over, actually, from the war years -- and so what I proceeded to do was to continue looking at some questions that had puzzled me from the previous period. One was that scaling business that I've already referred to. How far does that pressure of scaling law(?) go? And, as I said, it went as far as I was able to follow. Then, another question that I had in mind was, what about the shock front? High frequencies are attenuated more strongly, as you know, than lower frequencies, and quite significantly more strongly. If you think of a pulse like that as having a fluorease(?) spectrum, the high frequencies are associated with the abrupt rise, and if one goes out far enough, one ought to begin to see some rounding of the shock front. And so I thought I'd start having a look at that.

And the ship available to me at the time was the *Chain*, which some of you remember, and a small boat. And what we did was to use the techniques that we had developed back during the war -- the tourmaline gauges, cables, oscilloscopes -- but to go out in the Sound and start increasing range, looking at what happened to the lowest peak pressures we could go to. Let me

show you some of the kinds of records that we developed. These are oscilloscope traces. What we have here is the shock pulse, the leading edge. Typical shape. That shape is very well represented by an exponential decay, until you get down about one time constant of the exponential, and beyond one time constant, it's slower than the exponential decay. But the decay continues, and mathematically we treat it as an exponential shape with an infinitely steep front. But what I was looking for in going out to long range was visible rounding off of that front, which in fact we never did see.

Now, quantitatively, we needed to know the range -- how far we were from the charge. We knew the depth of the water (Inaudible); we measured that. The bottom out there is slowly undulating, unfortunately, so you get differences in level of five or six feet. We treated it as a plane(?) surface, as we did (Inaudible). Now, the kind of thing that we observed as we went out to longer and longer ranges is shown here. This is the angle of incidence at the bottom is indicated there. It's 57 degrees, the angle of incidence for a ray, and this thing here is the surface reflection, the rarefaction (Inaudible) phase, and that's the bottom reflection, which is pretty much like the original shape lower in amplitude. But then, as we went to

larger range, here's the incident shock, and the bottom reflection looked like this.

**Q:** What depth was (Inaudible)?

**AA:** The receiver and the charge were placed at mid-depth. Always at mid-depth. Now, here was the bottom reflection, considerably bigger than it is there, altering in shape at least the amplitude of the incident pulse. See, these are increasing angles of incidence. This goes on. That peak gets sharper and sharper, and it frequently exceeded the amplitude of the incident peak. Now, the first thing, when we first saw that effect, we started tearing the equipment apart and worrying that there was something wrong with the instrumentation. But that turned out not to be the case. This was for real. So the question came, what's the physics? What's going on there? And I'm not(?) quite positive about it, but the one thing that I learned while I was in that game(?) was that you could find practically anything in Raley(?). (Laughter)

**Q:** (Inaudible) really part of everything?

**AA:** That's right. So I went back to Raley, and I examined reflections. And, of course, everything in Raley is in terms of sinusoidal wave trains. He never thought about pulses(?). But the story is there. And the story is as follows. I won't go

through the mathematics; I'm going to talk about the physics and the phenomenology, and if you want the full dress treatment, I will give you the references. But I'll talk about the physics.

If you look at Rayleigh on a reflection from (Inaudible), with the usual boundary conditions -- continuity of pressure, continuity of particle velocity(?) (Inaudible) -- you find that when you have a reflection from something like the seabed, namely, an interface with higher velocity of propagation, there's a critical angle for total reflection. And associated with that critical angle for total reflection is the phase shift of the reflected wave relative to the incident. And the phase shift depends on the acoustic parameters -- on the densities and the sound (Inaudible) -- only.

Now, I had been thinking in terms of pulses and occasionally in terms of fluorease spectrum, and what I realized was that that phase shift was bound to have serious consequences. In ordinary electronics, we have a phase shift for input signals, but the phase shifts are proportionate to the frequency. When you describe your sinusoid at  $E$  to the  $I$ - $\Omega$ - $T$ , and then you have a phase shift which is also  $E$  to the  $I$ - $\Phi$ (?), but  $\Phi$  is proportional to  $\omega$ , you see either the  $I$ - $\Omega$ - $T$  and  $E$  to the  $I$ - $\Omega$  with a phase shift, the  $T$  and the



shift couple, you get the same time delay for every frequency component if the phase shift is proportional to the frequency. Then you'd have no distortion. But in this physics, the phase shift turns out not to be frequency-dependent at all. It's a function of the angle of incidence. It is zero at the critical angle for total reflection, and it's 180 degrees for grazing incidence. The phase shift varies from zero to 180 degrees as you go beyond the critical angle for total reflection. And so for every different angle of incidence beyond the critical, you have a different phase shift which you would have to apply to the spectrum (Inaudible). And the question was, what would that look like?

Now, what we did was to take the exponential pulse, with an infinitely steep rise, zero time interval, the shock front actually, of course, (Inaudible) time it. The time of rise of the shock can't be zero, but it is fast(?). It's a matter of a few (Inaudible) paths(?) in the liquid, as was subsequently determined. So it is plenty steep without being zero.

Fluorease spectrum the exponential, and applied the phase shift and recombined. Now, when you do that, you encounter a strange kind of function -- it was strange to me, although it was well tabulated in Yonta(?) and Emblow(?) with all the other

functions, like Bessel(?) functions and (Inaudible) and whatnot -- called the exponential integral. EI -- exponential integral function. That's what turns up in this case. Now that Yonta and Embrow is dead, I'm sure, (Inaudible) mathematical and whatnot. But we had to go to the tables.

Now, when we go to the tables, what we get are these graphs. That's for an angle of incidence before the critical. You get a reflection that is the same shape as your incident pulse. Now you get beyond the critical angle. This is for a phase shift of 15 degrees, angle of incidence of 71. And now, with that exponential integral in there, applying the phase shift, you see you begin to get this kind of thing, and there it is. And now we're at a phase shift of 30 degrees, and here is the bottom reflection; there is the shape that you would expect. And that amplitude can easily be higher than the amplitude of the incident pulse because the exponential integral at zero has a logarithmic infinity. For a mathematical of this continuity, you have an infinity. And if it's not really discontinuous, then you don't go to infinity, but you can get plenty high pulses. I was a little worried about that from the standpoint of conservation, but you see the energy transport in the pulse goes at  $P^2$ , and the exponential integral is integrable(?)

in the square. So its P-squared is integrable, energy is conserved, but you can get very high amplitude pulses.

And so it goes on. This is the phase shift at 45 degrees, angle of incidence 74; phase shift of 60 degrees, an angle of incidence 83; 75 degrees, angle of incidence 84. And this is the shape for glancing(?) incidents: phase shift of 180 degrees, you simply have the mirror-image rarefaction (Inaudible) pulse. And you can see these positive reflective pulses here, but over here, they're out at very long range. You see angle of incidence is 87 degrees. This is the surface reflection, this one here, and that is the bottom reflection as a rarefaction. So this is what's going on in your shallow-water acoustics if you start thinking about pulses. And you see the region that we're in is the region between where we were concerned with pressures of 100 more pounds per square inch damaging things, and the region where you begin to think in terms of (Inaudible). This is the in-between. This is the connection(?).

But I suggest that there are some bothersome things that you might ... and I don't know to what extent you've thought about them (Inaudible) and broach some of the kinds of things that kept arising back in the days when we were doing this and

discussing it with others. See, people always want to think in terms of reflection coefficients. So you have an incident amplitude, you have a reflection amplitude, you have a reflection coefficient. If you have a reflection amplitude higher than your incident amplitude, you (Laughs). What about the meaning of reflection coefficients? You've got some ambiguity, or paradox, or whatever you like.

Incidentally, as far as quantitative aspects of (Inaudible), I failed to mention that we took the ratio of acoustic velocities in the free water and the bottom, to be 1.15, and we picked that pretty much on the basis of establishing the critical angle for total reflection. And that looked like 1.15, and that's what we used in making these calculations. We consulted Harry Stetson, who was then the world's leading expert on bottom sediments and their properties, and he told us that he took the density ratio around here to be 2.7. So we used Harry Stetson's value for the ratio of the densities, and those were the numbers that went into this.

Now, of course, at long range, this begins to show all of these effects that are going on. The surface reflection gets reflected from the bottom, the bottom reflection gets reflected from the surface, (Inaudible) -- you're heading toward normal

modes(?). When you extend the mathematics, the formulas, it was very easily done. You have the same shape, but you keep reapplying the same mathematical result, but different phase shifts because, as you go out, you have different angles of incidence. But that's very easily applied. And here is the kind of thing that we would get. There was the experimental trace. This is angle of incidence somewhere in the 80-degree range, but here is the theoretical curve, and here's the shape that we actually observed. And it goes on ... this is an even greater angle of incidence. You can see the correspondence. And that was, in fact, astonishing for me, that kind of (Inaudible).

**Q:** I assume there was a wave(?) guide, or did you just have a constant(?) background (Inaudible)?

**AA:** Just constant (Inaudible). (Overlapping Voices) And there are deviations and discrepancies, you know, a little bit of sea state(?) and some undulation on the bottom and whatnot, but it was astonishing. So that was the nature of the physics. It was a matter of recognizing the existence of the phase shift and applying it to the fluourescence spectrum.

**Q:** What was the critical angle(?)?

**AA:** Fifty-eight degrees.

Q: So that first (Inaudible).

AA: Right. That was deliberately close(?) to the (Inaudible). Fifty-eight degrees was very close to the (Inaudible). Now, let me wind up with a brief look at the rest of the story because, astonishing as this seems to be, it's not quite right. Let me show you why it was not quite right. Now, there's nothing that seriously modifies those shapes that we see, but there is something else in the physics. And I'll be interested in the extent of awareness of this. So let me go back to fundamentals.

The wave equation, which we take for granted (Inaudible). Suppose we integrate that equation with respect to time. You see, what we're driving at in doing that is looking at what happens to the impulse in our pulse. Is the impulse delivered by an acoustic pulse, a pressure pulse? Is (Inaudible) the integral of  $PDT(?)$ , the area under your pulse (Inaudible)? So let's integrate both sides of that equation with respect to time, from zero to infinity. Over here, with civilized(?) functions, we have the integral from zero to infinity for the pressure-time curve, the area under the curve -- and I'll simply denote that by the impulse, (Inaudible) under the impulse -- ...

**SIDE 2**

AA: ... just plain(?) waves. So that solution which I've been showing is a plain-wave solution. The source was always at infinity. And that didn't bother us at all. We had that agreement between the observed traces(?) and the theoretical curves. But then along the line in there, I had a young undergraduate physics major working in my group here. His name was Kenneth Wilson. He was the son of Bright Wilson, my old thesis advisor. And Kenneth Wilson, incidentally, was the 1982 Nobel laureate in physics for his discovery of the renormalization(?) group. At this point, somewhere in the early 60s, it must have been, or late 50s, he was in my group here, and he called my attention to this: that the impulse has to obey the process(?) equation and, incidentally, the spatial distribution of the velocity of sound is (Inaudible).

(Laughter) Have you people ever thought of that one? At first I thought that it was Kenneth who had discovered this, but then I was prowling around in treatises on waves and layered media and I found this in a Russian monograph on waves and layered media. But that's the only other ...

Q: But what if  $C(?)$  is a function of  $T$  (Inaudible)?

AA: That's a different ball of wax, right. But if you have a stationary distribution, (Inaudible). Now, this is a serious

constraint, and in the situation that we were talking about, where you have a point source(?), if you're going to apply this, you can use the image method and you'll apply the boundary condition, and you get an impulse out here, let's say. But according to this theorem, the impulse is never zero -- that's total impulse -- combining the incident and the reflective wave at this point. But the solution that I showed, where glancing incidents you get a mirror image, a rarefaction, of the incident wave, that says that the incident impulse is zero. But this says it can't be zero. And this is what convinced us in the final analysis that the plain-wave solution, although it's neat(?) and gives you an insight, is incomplete, and that the real solution resides in solving the problem with the point source. Now, that's a different kettle of fish. It's a much more complicated problem than the one we solved. But it has been solved, and I didn't have the mathematical horsepower to (Inaudible), and I threw that to my Amherst colleague, Dudley Towne, who was a mathematical physicist trained at Harvard. And we picked up an approach using Immafloss(?) transfer(?), which had been developed for seismic studies by a Frenchman by the name of Conyar(?).

**Q:** (Inaudible)?



AA: That's right, treating it as a transfer, Immafloss transfer. And when you do that, you get a complete solution. It gives all those shapes, but what is missing -- what completes the solution that the plain wave doesn't give -- is the fact that you get a precursor (Inaudible) the higher velocity in the seabed. You get a very low amplitude precursor that contains all of the impulse that is missing in the plain wave solution. That's where it resides. So the complete solution has to be with a point source at a finite location, but the physics of the phase shift idea is perfectly valid. And this is the kind of thing that transpired. So that's the insight that we got out of this game. We'd been looking for something entirely different, failed completely in that respect (Laughter), and ended up with this, which was, in many ways, gratifying. So that's the nature of the story. If you want the references on this, I have them. You can pursue that you heart's content.

Q: What journal is that in?

AA: JASA, the Journal of the Acoustics Society. The first paper is 1950, and the second one is '68. There was quite an interval. But we had to come to the realization that the plain-wave solution was incomplete. Though we do have the whole story now, at the acoustic level. None of this is finite. And of

course, no time variation in the sound velocity pattern, etc.

(Background Conversation)

AA: To come back to what was going on here at Woods Hole, for example, in the immediate post-war years, of course the institution remained open full-time and work continued in all the areas. And the acoustics ... there was a big acoustics group under the leadership of Brackett(?) Hersey. He had Bureau of Ships money, and all of their work was in terms of frequencies. They didn't think in terms of pulses at all. Everything was essentially monochromatic. But the real world is not monochromatic, and (Laughter) ...

Q: You get very different results.

AA: I kept urging Brackett to have a look at ... to begin to have a look at pulses and see whether ... I don't know what can be drawn from that. I'm not expert in that field at all. But none of that ever happened in the acoustics group at that time. They continued everything in terms of frequencies.

Q: Much of it was driven by passive ASW considerations, I think.

AA: Absolutely, yeah. (Overlapping Voices) That was the driving factor, but they tried to convey the idea that they were doing fundamental physics as well, that it wasn't just all ASW.

Q: We're still seeing the same thing. I think part of it is the ability to model ... to create acoustic models. The only "truly" wide-band acoustic model we have is ray tracing. We do four-ray(?) synthesis on the results of a bunch of narrow-band models, but this data that we took down in San Diego, the surf-zone data, doesn't show us the precursor arriving to the main arrival. But if we do the four-ray synthesis -- I mean, this is something we're trying to track down ...

Q: You see it there.

Q: ... but it's not there in our real data right now. The only thing we have to use, a truly wide-band model, is ray tracing, and that really doesn't capture ... so you have this one model which is really deficient in the physics (Overlapping Voices), and you have the narrow-band models, which can capture more of the (Overlapping Voices) ...

(Background Conversation)

Q: I guess the other thing is both the attenuation and dispersion were kind of unknown at that point, too. That's what you were trying to capture with seeing the pulse shape (Inaudible).

AA: Exactly. There were data beginning to appear on attenuation as a function of frequency. And what I hoped I

might capitalize on was applying that frequency data to the pulse, but never got anywhere. That's exactly what I was after, because those data were just beginning to appear.

Q: Was some of that work collaborative with the Naval Ordinance Lab?

AA: We were funded by the Naval Ordinance Lab. It was Nav Ord money.

Q: Was Yenny(?) at (Inaudible)?

AA: He was my graduate student at the time that we did that work.

Q: And Paul Fye came from there, didn't he?

AA: Paul Fye was in the group here during the war.

Q: But didn't he come from the Naval Ordinance Lab?

AA: That's right. Paul left here, he spent a year at the University of Tennessee in Knoxville, and then he became director ... then he went to Bu Ord(?) in Silver Spring, and then he came here as director. Those were his steps. And the material that I showed you here is co-authored with Don Yenny, who was my graduate student at Stevens in the years immediately after the war. He went on to get a Ph.D. under Yukara(?) at Columbia, and eventually he became professor at Minnesota, and then professor at Cornell. But he went into particle physics.

(Background Conversation)

**Q:** You might be interested to know that we're still working on that reflection problem (Laughter), the reflection coefficient problem. (Overlapping Voices) scrambling the plain-wave reflection coefficient from the point-source measurement, and we've actually recently made some headway on that with a new student from Brazil. It's a tough problem.

**AA:** Actually, in deciding to talk about this to a group that is on the leading edge, as this one is, I was curious as to where I stood with this ancient history. (Laughter)

(Background Conversation)

**AA:** One aspect I intended to mention and didn't, but this brings it up, see, we were getting away with treating the bottom as fluid. And that bothered me. But at some point or other there, I talked with Ewing about it, and Ewing just laughed. He said that in the work they were doing, they found that treating limestone as a fluid was better than trying to treat it as a solid. (Laughter)

(Background Conversation)

**Q:** But if you're concerned about precursors, you would want to treat it as a solid, wouldn't you? Why would you treat limestone as a fluid?

AA: Well, you don't excite the sheermount(?). It remains essentially a longitudinal pulse. I suspect that's the principle.

(Background Conversation)

Q: How far did you look to try to see the blunting of the shock front? You mentioned that earlier.

AA: Oh, gosh. I'm not sure I remember. I think more in terms of pressure amplitudes rather than ranges. It would have been a few pounds per square inch amplitude, which, in range from the size charge we were using, few pound charges would be out in some hundreds of yards, something like that. Ranges of about 500 yards. By the time you get down to a few pounds per square inch amplitude, that feeding back is pretty small. And the viscous(?) attenuation ought to be taking over. I had actually ... there was other work that we did during the war. Among that work was a look at the effect of sharpening under finite amplitude. And I calculated shock propagation velocities using what are called the Rankin(?) (Inaudible) conditions. You satisfy the second law, and you don't do the dissipation in detail, but the results that you get for propagation velocity are the correct results taking dissipation into account. So that kind of thing was done and published. Do you want the

references on those? This is the Journal of the Acoustical Society. The first paper is this one.

(Background Conversation)

AA: The 44 one has the Conyar solution. Conyar was doing seismology. But he had a powerful approach, and it looks ... and of course, for the fluid situation, it's far more attractive than the seismic one (Inaudible).

Q: What was the bandwidth of the oscilloscope? How sharp a pulse could you perceive(?)?

AA: Well, the frequency response of the electronics, including the cables which (Inaudible), was up in the 30, 40 (Inaudible). So that did not determine our frequency response. Our frequency response was determined by the transit time across the gauge.

Q: From one end of the crystal to the other? Wow.

AA: That's the fundamental (Inaudible).

Q: And that was -- what? A quarter inch?

AA: The large gauges we had to use for the low amplitudes were about an inch.

Q: And that gives you some (Inaudible)?

AA: Yeah. The signal is directly proportional to the area of the crystal. And what we did was to pile up ... I had four plates in parallel. And so you have four times this area. But

the response was determined by the transit time across that  
(Inaudible).

Q: How did you (Inaudible) physical oceanography?

AA: Well, by being here. And as time went by, going to colloquy ... that was the heyday, the flowering, of physical oceanography. And I had an office next to Hank Stommel(?) (Laughter), in the middle of the third floor, over on this side. And we became close personal friends, and he kind of pulled me in. (Laughs) It was very easy. Another close friend was Al Woodcock, whom we've just been talking about here. Al was working on sea-salt nuclei and the atmosphere. Do you know Woodcock's history? Al Woodcock came ... drifted ... he never finished high school in Georgia. He drifted in here somehow or other at the time Columbus was going to go over to Copenhagen and bring back the *Atlantis* in the early 30s. And he got himself hired as a deck hand on the *Atlantis*, and participated in bringing her over on the first voyage. And Al was always looking at what was going on around him. He began watching the soaring of seagulls and began to discern that they were using the Langmuir(?) circulation cells in their soaring patterns, and wrote a paper about that. And Columbus Iselin, who was very great on identifying people of that variety, among them Fritz



Fugelister and Val Worthington. Fritz came out of the drafting room(?), you know. Fritz came here during the war, an artist and musician. He had a young family and he wanted to stay out of the draft. He got hired in the drafting room. (Laughter) Fritz and Val both had some academic background. Woodcock had none whatsoever. But he did that kind of thing, and Columbus recognized a real naturalist, of the Agassi(?) variety, and that's what Woodcock is. And then, at the end of the war, Al decided to go into the sea-salt nuclei problem and the connection with weather, and proceeded to investigate sea-salt nuclei. I became friendly with him. Al was a wonderful guy. He has a facility that I don't possess. He'd look at things that I simply couldn't see.

But he needed certain kinds of help. He was looking at distribution of sea-salt nuclei sizes, so he had a kind of histogram in which he had nuclei of so many micrograms, nuclei this, nuclei that. And so I finally said to Al, "There aren't any nuclei that are 10 micrograms." And he looked at me and he said, "That's what Langmuir says." (Laughter) Well, I was never able to get him to understand the distribution function. That simply didn't ring. But he did understand a cumulative distribution, so he finally presented this result with a

cumulative distribution rather than a distribution function(?).  
(Laughter) In connection with that, I had one of my students ... we did two jobs. We did high-speed photography of bubbles bursting at a water surface and projecting nuclei. And we also did some accurate measurements on the vapor pressure of different concentrations of sea water, all the way down to very high evaporated concentrations. The reason for that was that we wanted to be able to calculate the growth rates, starting with a dry nucleus and going on to saturation. And I did that, among other things, and Charlie Keith did observations of nuclei growth under a microscope. So he had data, and I did a theoretical prediction of the growth rates that giped nicely with the observations. But we needed the accurate vapor pressures for that (Inaudible). So all of this kind of (Inaudible).

**Q:** What was Val Worthington's (Inaudible)?

**AA:** I don't remember in detail. He also was in the drafting room. Prior to that, I don't know. (Laughter)

**Q:** Do you think that sort of thing can still happen today?

**AA:** No. That was an artifact of the times. There wasn't anybody trained in oceanography. Hank Stommel had an undergraduate background in astronomy at Yale. He never got a

doctor's degree. (Laughter)

(Background Conversation)

**AA:** A few years after the end of the war, Hank was still around here, dabbling in various things. He went to Ray(?) Montgomery and asked him what would be a good problem in physical oceanography, and Ray said the Gulf Stream. (Laughter) So Hank did the Gulf Stream, with the insight that it was the sphericity of the earth, the variation in the coriolis(?) parameter, that produced the asymmetry. Prior to that, nobody had done that kind of thing. And Hank had a very low-powered mathematical background. The high-powered people -- Walter(?) Monk and George Carrier and those people -- came in after Hank's paper, and cleaned up the business with more high-powered mathematics, but the real insight was Hank Stommel's. But that was the climate, that was the nature of all this kind of thing going on in the hands of people completely new to the field, who nowadays wouldn't make the grade at all, wouldn't be able to get in. It was the flowering of post-war physical oceanography. It was an exhilarating time.

**Q:** Did you have much interaction with Walter Monk?

**AA:** Not much. I met him many times personally. He was never here. He was always at Scripps. He visited.

(Background Conversation)

Q: What was the funding climate like? Because that's an issue that we all have to deal with these days.

AA: During the war, there was the funding ... the explosives group was funded by Bu Ord. It had started with MBRC, but Bu Ord took it over.

Q: Was it a block of money? Did you have to write proposals yourself?

AA: The director ... I was a very junior member, remember, and precisely what the director did, I don't know. But he did not write long, detailed proposals. (Laughter)

Q: The National Science Foundation didn't even exist then.

AA: It didn't exist until 1950. Now, that was well after the war. And the Bu Ord funded us, and continued my work after the war. Bu Ships funded Brackett Hersey and the acoustics work, and ONR funded the physical oceanography. So Columbus had in his pocket a block grant from ONR, and if you wanted to do something, you came into this office, where Columbus sat -- right here, where I'm sitting (Laughter) -- and told him what you wanted to do, and he'd listen, and, Sure, go ahead."

(Laughter) If somebody wanted a month of *Atlantis* time

(Overlapping Voices). It was a golden age which could not

possibly last. It's interesting to have been part of it.

(Background Conversation)

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