AOPE Seminar
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Bob Frosch

Intro- Today I have the pleasure of introducing you to Robert Frosch. Actually it's amazing, the most information you can find about anyone, on the planet, in the case of Robert, the number of hits on Google was huge. So I took one particular reference, that I enjoyed the most, which is Wikipedia. So for sure I checked these entries with the NASA webpage, but correct me if I'm wrong in what I'm going to say. Robert is actually Robert Allen Frosch. He's a New Yorker and has had a very busy life. So much so, that he got his undergraduate and graduate degrees in theoretical physics at Columbia, and if I'm right, you got your PHD by the age of 24. Then he moved to Hudson Laboratories, at Columbia University as research scientist and then later as Director of Research. At the time you were project manager of a project called ARTEMIS, developing an active sonar system for the Navy. In '63, then Robert moved to Washington DC to what became the ARPA program at the US Department of Defense. Here the commission says you were working as Director for Nuclear Test Detection. After that, in '66, he became the assistant secretary of the Navy, and in charge of all the research and development at the US Navy. In 1973 to 1975 he was Assistant Executive Director of the United Nations Environmental Program. In 1977 he moved to NASA to become its fifth administrator, staying there until 1981, when the current administration was finished, and then he moved to General Motors, becoming the vice president of research, and developing in research labs. In 1993, he decided to retire, finally. So he went to the Kennedy School of Government and he's been involved in policy and research.

As you can see, this is a retread of a talk, no good deed goes unpunished. Jim Lynch and I were talking about a talk he was going to give, and I said—oh I have some slides on that—and I sent him the slides, and as a result, I'm giving this talk. It's partly history and partly institutional arrangements for doing applied research. It's fairly deep history. The slide sequence begins in an odd way, I need to explain what these talks were constructed for. I was working with a group at the Kennedy school that was interested in the use science and engineering for international development, development in developing countries. It started with the question—how come we know so much and so little of it seems to do any good? So the theme was, what is it that one can say about the use of knowledge to do something for some job somebody wants done and I kept sitting there, saying—some of us think we kind of know how to go about doing that. So I was finally challenged to put together a couple talks on it. This is really two of those, compressed together, and a third one was given by Dick Pittenger, who filled in a whole lot of other stuff. So let me start with this.

Now, this was a group of people who are mostly not scientist or engineers, although some of them are. Most of them are political scientists and social scientists and what they wanted to start with was the political science and social science theory of how military R&D gets done. So I'm going to run through several things. A guy named Owen Cote wrote a whole book on the political science of the military R&D. From his theoretical point of view, that the reason that things change is that because the civilian executives at the top of the Navy intervene and there are internal struggles for power and inter-service rivalry, and inter-service competition. Please note this theory contains nothing in it about anyone actually wanting to do anything; it only has to do with people arguing. But all of it is part of the process.

Then the second part was that people don't like the change systems and so they are resistant to R&D. Here's a whole list of reasons for conservatism. You already own a lot of ships and airplanes; you don't want to spend more money. Also, everybody already has a career, and they don't want to change. Nobody quite trusts new stuff because you don't know how to test it in a real way. Then of course, you have large, coordinated systems, and the instance that you breathe on them with something new, they're likely to come unglued. Please remember most of the period of time I'm going to be talking about is back deep in the past, post World War Two up through maybe fifteen, twenty years ago, when there were no computer capabilities to simplify coordination. So everything was done by very tedious means.

So the conclusion from this particular set of theories is that R&D is presumably a byproduct of all these kinds of political battles, internal ones and external ones.

Then there was the Macnamara Theory, planning, programming and budgeting system, where you would start with— what are the foreign policy objectives of the United States— and— what is the national strategy. Eventually you'd work your way down to the fact that you needed a new fuse for a torpedo. So, in that case, R&D is presumably a result of military requirements. Somebody in the military says—Gee guys we need a new fuse for the torpedo. However, they didn't leave out this part, it was in the budget. But it made some of them very, very uncomfortable that there were people who were doing things for which there wasn't a military requirement, yet. That got to be a paradoxical problem, it wasn't unreasonable, but you couldn't do it. By the time you did this year's analysis of foreign policy and so on and so on, and so on, the budget process was finished. It was over, it was gone, and you never got out of it. Instead of being PPBS, it became "Please Pass the BS". We were sort of rationalizing what it was that you really decided on, it never worked and of course it produced this muddle. You shouldn't do serious R&D unless there's a military requirement. But how do you get a military requirement for something that somebody hasn't already thought about? So you kept going round and round.

In practice, what actually happened was actually very straight forward. 6.1 and 6.2 didn't require a military requirement, that was agreed. In the Navy, as Assistant Secretary of the Navy, I owned that budget, my signature, not the military's side, curiously enough. I think it's still that way by law; the army and the air force are different. But you couldn't get the 6.3 or the 6.5, that is, you couldn't start to build a real thing that would go test in the field without a military. What you mostly did was you ignored it. If someone said—where's the military requirement—you'd find a convenient admiral who would invent one with you. I'll come back to that idea of the process. Then of course, there's the ever-present military-industrial complex theory, the reason things change is that someone wants to peddle something and the Lockheed guy comes in and peddles it, and that's what you get. The question is, it's partly true, but the assumption is that the arms merchant can always convince the naval officer and is that good or bad? Just a thought.

Now this is sort of my theory, of what really happens, as you get down to the bottom. Many naval officers think. Many scientists think. Sometimes they think together-able to have a conversation. All navy officers, no. All scientists, no. By the way the Arms Merchants also think, they have ideas and that might be a good thing as well. When I say which naval officers and which scientists, at GM, I was perpetually being told—all of your scientists and engineers ought to go over and spend time in the manufacturing division. To which the answer was—hey, I got some people who are really valuable, and I wouldn't dream of subjecting a manufacturing division to some of them or them to some of the manufacturing divisions. Those of you that knew him, might think a moment of what it would be like to send Al Vine off to fix a problem at a manufacturing place. Brilliant guy, national asset, but don't send him to fix a production line.

Now some additional theory, the customer for R&D is always wrong. What do I mean? How is a customer for R&D possibly going to know what is is possible to get out of R&D, unless the customer is actually an R&D person. Customers are usually thinking what they need now, what they need next year, almost never thinking of what they may need in five years or ten years, they're just not good at it. So that's a problem. Then there's a second thing, they usually ask the wrong question, and I'll get back to an example of that. Then of course, you have to think about System Engineering. You have to think about where this fuse is going to be in this torpedo, in this torpedo tube, in this submarine, and what is it going to be doing and so on. Otherwise you build junk. I'll come back to matrices of knowledge. Of course bureaucracy is the ever-present enemy. That is, it's trying to explain to you, you shouldn't do this because it wasn't in last year's published plan or it wasn't in the budget and so on. So you find loopholes and you go around saying—gee, I didn't know there was such a rule—or—gee, send me the form—or whatever. It's harder to do now-a-days.

OK, now what does the customer really want? Well, I'll give an example. The admiral comes and says—Bob, you have to give us faster fighter aircrafts. OK, why do you need faster fighter aircrafts? Oh so when we get in a dog fight we can maneuver faster that the other guy. Ya, but the first thing that happens in a dog fight is you turn and when you turn you go to velocity zero. Oh it's really so we can get to the dogfight faster and be prepared when the other guy is there. Oh, why do you want to get to the dogfight faster. So we have a better chance of killing the other guy. So what you really want is to get the weapon to the dogfight faster. So at the end of the discussion, no, you don't really need a faster fighter aircraft, what you need is a longer range, fast weapon. So, that kind of thing. How do they know what the possibilities are? Another example, nobody ever asked for, I don't know if it's a good idea, but nobody in the military ever asked for beam weapon. Hadn't occurred to them there could be such a thing as a laser weapon. Then of course, we all have a tendency to say—that wonderful thing I did in the lab last week is just what you need. So, there's a flip-side. By the way, the other problem is not only are they likely to tell us what they want us to build for them, they'll tell us how to do it. In fact, usually, their idea of what they want is their answer to what the problem is, build me an aircraft with the following kind of engine. Then of course there is a whole set of questions about time relationships. R&D takes a long time, if you want something in two years you better not start the 6.1 research today, you ain't going to get it in two years. So there is a whole set of interrelationship problems that you have to think about.

Now this I'm not going to spend much time on it, you guys are basically system engineers you're used to going back and saying what is the purpose of this thing really? Where does it fit? How does it fit in the system? So I don't think I have to spend much time. But for the audience at the Kennedy School, this was a blinding flash of something new. It literally had not occurred to many of them that it doesn't do any good to ship grain to a starving country in Africa, unless you actually have a pier for the ship, and trucks, and roads—ship the grain, they'll eat. That's an exaggeration, but not to much of a characature. There are many people, Jeff Saks is a perfect example, send money and somehow they'll eat, well it doesn't work that way. OK, now I mentioned knowledge matrices, and this is something thing you also understand, but lots of non-technical people don't understand. If you say, I want a more efficient, lower admissions auto engine. There's a lot of science in that, and a lot of engineering in that. You can't just say—I'm going to put a better molecule in there to burn—because as soon as you change the combustion chemistry, you've changed the combustion, you have to change the ignition, it flows differently, the gases flow differently, the heat transfer has a different set of rates, the mechanical systems are pulsed in a different way. The structure has a different pressure. You may need different materials. There was an interesting problem with substituting the older floro-carbons in air conditioners and automobiles with 134A. Of course it turned out 134A was not compatible with the greasers that were used in the compressors with the older floro-carbons and if you put the grease that was compatible with 134A in as a drop wind, it ate away all the seals. So you had to start all over again with that. So that's the first level of the matrix. But then there's a second level that breaks up all of these and so on. It's a process you guys all know about, but not so widely understood.

Then of course the individual theories. In the real world you've got a whole lot going on, so you have to worry about the politics, along with the rest of it. OK, then this was turned into a second lecture, that I will now proceed to.

So now I'm going to go back to the history, and how some of this played out in practice of the days long gone by. Submarine warfare in WWII, the Navy came out of WWII with the clear perception that it had survived the submarine war by the skin of its teeth and it was a great deal of luck. They were really scared that if they had to play a more modern submarine war with the Soviets, they'd be in deep trouble. Now, why did they survive? Well, sonar, on the whole the US sonar and the British asdek (sic) were better than the German sonars, but it's not really very clear. We had periscope detecting Radar before anyone else did and of course that led to a whole game of measures, and countermeasures, and so on. But Admiral Dönitz in the Atlantic made a terrible mistake. He wanted to be in command of all his submarines, that meant he had to know where everybody was, and what they were doing. So every single day, every submarine in the German fleet had to come up to periscope depth, at least once and send a high frequency radio message to Berlin. We had high frequency direction finding services. So roughly speaking, everyday we knew where the Germans were. Now, not well enough to do the job, but well enough so that it was a major advantage. He didn't catch on, until too late. So if the next group of guys caught on, and didn't play that silly game, then it might have been a very close shave. So they were very frightened.

This was the set of worries: They won the submarine war, but it was close. The German submarines had developments, they had snorkels, we didn't have snorkels, this meant that the submarine could have very long, effective battery life, without actually surfacing. Nobody knew what nuclear submarines would mean. Nobody knew what the Soviets were going to do. What about quiet submarines, maybe somebody will figure out how we're going to do this and how are we going to deal with that. Then of course this story I think most of you, especially the acoustics guys know, the noon effect and the BT from here Ewing and Worzel really low, long range frequency stuff. Then of course the key thing was that the wartime research happened with cooperation. That is the military and the civilian research people were all inside each other's pockets. Nobody worried about bureaucracy, nobody worried about whether bills were paid, and so on. I once asked a senior guy from ONR, who'd been there through the war—what did everybody do about contracts, everyone now is dominated by contracts. He said—as far as I can tell they were all signed the day after VE Day. There's a story about General Motors going into the air plane business. Whoever was counting airplanes decided that we weren't building B17s fast enough during WWII. So the appropriate guy called up the president of General Motors and said—you guys are going to build B17s. The president of General Motors said—we don't know anything about air planes! We can build cars, but we can't build air planes. The answer was—you are going to build airplanes, I want B17s to roll out of your factory in less than a year. In fact, they actually went out and bought Willow Run airport, built a factory, and in six months were producing flyable B17s, I don't know what the first ones looked like. But it worked, it was sort of a-don't bother me with the details, just go and do it, and if you're worried about whether to take path A or path B, send someone on both paths and we'll sort it out later.

Now I don't know if you've seen this. In '43 Doc Ewing's idea of what to use long range transmission for was as a system of communication, not as a detection system, not as anything else, but some how or another, the original idea of deep sound transmission was that a downed aircraft could drop a small charge in the ocean and it could be located, so that they could be found, so that you would know that there was somebody there.

All of this you know, I came in at the beginning of this, fifty-five years ago I came into the business. Bill Nerrinburg recruited me, believe it or not. So this is what we sort of knew, we knew about convergence zones, and we had SOFAR, mainly locating things with explosives, and LOFAR, the reverse, you set off explosives in set places and somebody who hears them knows where they are. Then Ted Hunt, who was professor of applied physics at Harvard, sold the Navy on the concept of—we can in principal detect everything that is in the ocean in an hour. An hour is about the transmission time across the Atlantic and back. So his motto was an ocean an hour. Then there were all the problems you know about. They started to build a response to the worries.

The Navy invented ONR, and when the Navy invented ONR, what it did was inventing NSF and ONR. NSF is kind of a later copy of the ONR idea. They started SOSUS, which is a sound surveillance system of big arrays that you know about that were all over the coast of the Atlantic and Pacific. It was called Project Jezebel, it was run by Western Electric and AT&T. But the navy wanted someone else to worry about this acoustic stuff, so in addition to having Woods Hole worry about it and Scripts worry about it, and NRL and so on, they invented Hudson Labs, specifically to look at long range low frequency underwater detection. This is just a side item. Everyone knows about a sonobuoy, sonobuoys were always passive listening devices. Then someone thought up the idea of using a separate sound source, an explosion, and then using the sonobuoys as a set of detectors to get an echo off a submarine. It was called Project Julie because there was a very well known and rather voluptuous café singer in New York at the time, Julie, I don't remember her last name. So they called it Project Julie because it made passive buoys active. Then there was an Admiral Hartwell who did a report and that created project Michael, which was named after Michael Pupin who was a physics professor of a previous era at Columbia who'd do the oceanography. So we were chartered to be the scientific counter weight to AT&T and Bell Labs. That is, we were just supposed to be doing science, but the Navy was going to look to us and say—hey are these guys doing their part of the science right. In fact the first serious job I had in the business was to do ray tracing in order to figure out whether the SOSUS arrays should be deeper or shallower that the depth that they were being put, which was largely picked out of a hat, because it looked like it would be alright and it was convenient from an engineering point of view. It was a perfectly good depth and we ended up saying they should go somewhat deeper. It took a year to do one ray tracing, and the computer I assembled consisted of six young women, because it was women because they answered the add, in a room with electrified marshant (sic) and freedman (sic) calculators, and a spread sheet. Everything in little blocks and snail's law at the boundary of the blocks, after awhile you actually saw a ray, it was fascinating.

OK, so what was this ONR system? Well first off it started with Roger Bacon, 1620, not explicitly, but implicitly. "Nature, to be commanded, must be obeyed." That means you've got to find out how things work if you want to build machines that work. The scheme was, we have Navy questions, which we'll discuss with you—you figure out what to do about it. The program officers were mostly civilians, some of them were regular civil servants, usually out of the laboratories-particularly NRL. But a lot of them were people who recruited from universities and labs to come in for a couple of years and go out. So you were tending to be dealing with people that were colleagues. The naval officers who were assigned there were experience and educated, they generally had degrees in physics, or advanced degrees, so it was easy. Coming out of WWII, everyone around this place was comfortable with the Navy; they had been working with the Navy for six or seven years. A lot of them, Fred Schest (sic) was an ex-naval officer, were just comfortable, it was an easy social situation.

So the way that ONR went about doing things, I think about it as the investigators 'club'. The guys in ONR, the people in the fleet in the Navy, and the people in the university labs, and in the Navy labs, and in the contractor laboratories were all in this together trying to figure out how to solve the Navy's problems. Not very much, we're inside, you're outside with a customer, you're the supplier, but much more—hey we got this problem. This is a technical term I learned at the Kennedy school, and epistetic community is a community of people involved in the same body of knowledge. Oceanographers are an epistetic community and inside that, marine geologists are a smaller epistetic community and so on. There was a lot of what anthropologists would call mutual acculturation. Those who didn't know about the Navy got indoctrinated into it, and naval officers got indoctrinated into the odd ways of scientists and engineers behaved. There were no artificial lines drawn, so that you'd have the same room, the same ship, navy officers, university people, navy labs, industrial contractors—anyone who was assembled because they probably actually knew something or could do something that could do the job.

In fact, I was involved in at least one formally chartered Navy committee, which had all those kinds of people on it officially, all together, quite illegal now—nobody would allow that, I mean they're different kinds of contractors and they're civil servants—but it actually worked. Everybody knew who everybody else was, so if somebody said—well you ought to build such and such a system—and you knew he was from company X that made that system, you've got to have the appropriate conversation, which was not a difficulty. So the program officers played a funny role in ONR, they probably still do at ONR, of playing communication nodes. That is, they talked to a whole string of people who were off doing research and the ideas would come together. You'd get a phone call saying—I was talking to so-and-so from the University of Iowa about a mathematical something and it reminded me about a question you asked me about such-and-such, here's his phone number, here's her phone number, call them up. So that was very useful. Furthermore, they acted as a recruiting system to find new people to get into this investigators 'club'. Then there was a lot of advisory oversight. When I did this ray tracing, I talked about the Navy wasn't quite sure they wanted this newly minted PHD theoretical physicist producing numbers that they were actually going to act on. So they gave me an advisory committee of three people. The three people were Harvey Brooks, who was the dean of applied physics and engineering at Harvard. A guy named Bracket Hurcy (sic), who was a senior guy here. A third guy named Warren Turo (sic) who was from Bell Labs and inside the SOSUS business. They put together ad hoc committees all the time, sometimes people would go to each other's labs just to go and see what was happening. All of this is familiar. There is by law of thing called the Naval Research Advisory Committee, which sometimes plays a useful function and sometimes just the ceremonial function, it depends on who's on it and whether or not the secretary wants to listen. Then there was an undersea warfare committee that later became the Naval Studies Board, some of this machinery is still there. So just my own experiences, this I mentioned. All of the sonar and under-water acoustics people who had come out of WWII had worked with sonars that were up in the multi kHz regions, where there wasn't any coherence. I mean if there was a ten wavelength coherence, you never found out about it. So they were telling us—this ocean is chaotic, it's turbulent, you guys are never going to see anything useful at long range, you aren't going to make very long arrays, and so on. So that was our first question, and we looked out at the ocean and said—what is it in the ocean that can possibly be changing fast enough, spatially and temporally, to effect an 100 Hz sound wave. We called them cycles per second in those days, Hz hadn't been the name yet. The answer was, we couldn't think of anything. Everything we could think of was slowly changing compared to that, or was big and was going to be averaged over. So we did an experiment: we borrowed four hydrophones, well we did two experiments, let me tell you the first. We had a guy named Dana Mitchell, who's a professor of physics at Columbia, who thought about making a very very loud, low frequency sound source. He took a device that was called an A Mark 6B Mindsweep. The A Mark 6B had a steel case which was about two inches thick and in it there was a ten horse power DC motor and a cranked shaft and at the edges at both sides there was a steel plate this big with a thick rubber gasket. So when the motor went around these plates in and out, it was a mono-pole source. Let me tell you, if you stood two feet away from it in air, it pumped your chest. But it wobbled, it was a DC motor. So he extended the case and the shaft and put a one horse power AC motor on it, which we ran from a tuning fork running a big thyrotron oscillator and we got a Q of eight thousand. That is we could really control this thing when you locked it into the tuning fork. So the first thing we did was put it on a ship and send the ship out, six hundred, eight hundred miles to sea and listened on a hydrophone. Of course we could, one of the earliest Hulett (sic) Packer audio-oscillators, one that came out of the garage, was used to beat this. Of course we could get the doppler shift, so we knew the radio velocity. We actually called the ship up one day and said—the kid who was on the wheel yesterday at 2 pm is on the wheel today again, isn't he? He's the one who writes his name on the water—we were able to make boxes actually and in travel time later we knew where it was going. We were pretty sure it was fairly coherent. But we then decided to make a big array. So we borrowed a SOSUS hydrophone from Eleuthera, Cape Hatteras, Cape May, and Sable Island and got the AT&T long lines department, they'd never done anything like this, to give us a connecting circuit, a dedicated circuit from Eleuthera, from those four places, into Hudson Labs and we ran the array. It was strictly speaking unnecessary, but it was perfectly clear that we had a coherent four-element array that was sort of as big as the ocean. That worked. Then we worked on the other questions that everyone is still working on. Except this probably changed now, one of our questions was how loud ship noise and I is think the answer is now a lot, but not quite so much then.

Then here are all the things. The Navy came in one day and said—OK, you guys know so much, build us an 100mi sonar. We eventually translated to that saying—can we build a third convergence zone sonar. We wanted a name for the project, we wanted to call it Project Diana because she was the goddess of the hunt, after Ted Hunt who had started the ocean analysis thing, but the Airforce was already using that, so we made it Artemis, the roman goddess of the hunt, same as Diana but a Latin name. We started to build this thing. In the end, we had to take a 10,000 ton tanker, cut a sea chest through it and dedicate it to hang the sound source. The junction box on a reforved (sic) Bermuda was on an early Texas tower because we had 200 hydrophone towers, each with 20 hydrophones on it, erected on the bottom. In the end, I had left, they actually got third convergence zone echoes. But then you turned around and said—I can do it, now what do I do with a system of that scale because there wasn't a lot of excess signal to noise ratio in it. We also, by the way, had a terrible time figuring out how to do the signal processing, no computers, delay lines maybe. Ross did some neat stuff with optical processing, but that was optical processing where you did 4EA transforms by optical processing with actual signal of film. So it was a tricky business. Then the other interesting aspect of ONR, the other advantage of the time, was that there was a lot of money around. Cold war times, but you'd suddenly get a call—can you come to Washington Thursday, I have this funny problem I can't tell you about on the telephone and some of the guys are getting together. There would be this same heterogeneous group of people, trying to figure out what the latest funny signal from a Soviet submarine meant or what it was all about. Sometimes it was really nice, we're coming to the end of the fiscal year and we've got this $10 million we've got to get rid of, so let's have a little conference and decide what's the best thing to do with the marginal $10 million. Students say to me—how do I have a great career like yours? You have to be born into an expanding universe. The current students have been born into a contracting universe, very difficult.

Now there was a lot of other stuff, we went to sea. The Navy gave us a ship. They gave us an ATA, attack tug, 600 tons. There was a guy who must have been the model for Queege, he through me off the bridge because I suggested that maybe we weren't in the Puerto Rico trench, because the depth was only about 1,000 fathoms. His next job was a disciplinary officer at Portsmith Naval Prison. Then we did a thing called Medea in '55. It was another typical mix and match operation. We had a coast guard cable layer, we had the Hudson Lab ship, the Willard Gibbs which was a converted AVP, Aviation Personnel Ship from WWII. We had two EPCERs that belonged to Navy labs and a destroyer escort. The destroyer escort was used for throwing explosives over the side mostly. That was our fleet and it was manned by scientist from any place you could think of, we had a mix of people from all sorts of places, from here and so on. We went and surveyed the Norwegian Sea for SOWSUS. The question was, was the acoustics such that you could put a SOWSUS array in. The answer was yes, and I don't know if there ever was one or not. It was kind of a fun trip, we got into a storm northwest of Iceland, in which the Hydrographic Office professional wave observer on board said—we are now in 50 to 60 foot seas with the top 10 to 20 feet breaking, short crested sea, very interesting. OK, I mentioned the Undersea Warfare R&D Planning Council and it's worth noting this was the same mix of people. The Thresher went down the night of our annual meeting in D.C. The night we had the dinner with all the senior naval officers, I happened to be chairman at the time. So here we are with a room full of senior admirals, more gold braid and white uniforms than I can ever remember seeing. Half the Navy Scientific Civil Service from D.C. and so on. Suddenly the guys with their loafers looped, the aids start popping in and out and whispering to admirals and popping in and out. Finally the Senior Admiral tells us what happened, Thresher is down and so on. The Navy, at that point, said—we don't have anything much to look for her with, we've got nothing, we've got ordinary sonars, and so on. So the committee organized the search for the Thresher on the spot and we didn't have anything to look for the Thresher either. But everybody thought they had something that might be useful. So, at one point there were 5 ships out there, towing anything anyone thought was useful, magnetometers, cameras from here that went down and took film and came up, sonars, whatever anyone had. By the way, nobody had communication for such a fleet, voice communication. Hudson, for some reason, had the only single side band radios and the only single side band station. So we ran around to every electronics store in New York City, buying single side band transceivers for all the other ships. We put them on and hung the thing together. The Navy eventually, about a week after we were all at sea searching, they conjured up a poor devil to be the operations officer in charge of this search, but he didn't know anything about it. So he just kind of had this trail to go around. She was actually found from a magnetometer strike from somebody here. That gave us data so we could get pictures of stuff and then we were able to follow it up. But it was kind of an interesting adventure.

Now ARPA, ARPA was the same kind of operation, except smaller, intended to sponsor very advanced stuff. Never go into production, if you actually prove the science and rudimentary engineering you try to pass it off to one of the services to do the next thing. I went there to do the nuclear test detection job after the limited test ban treaty. The bureaucratic ideas of ARPA well exemplified this. The procurement officer said—if one of you guys comes in before noon and said I had an interesting phone call from so-and-so and I think we should do something about it, we'll have an authorization to that person to spend money by the end of the day. Not for everything, but a few a week-we can do that. A different time and spirit. I don't know it they do that anymore.

Then of course I could go through a lot of stuff here, but it's more bureaucratics. But I'll tell you two stories. One is that the way we found out about, other than that she was overdue- that Scorpion was lost in the middle of the Atlantic- was that Gordon Hamilton who was then sort of a one man ONR contractor in Bermuda at the Bermuda Biolgical Lab, running some hydrophones there, saw something funny as an acoustic signal on one the hydrophones. He said—I think that's something imploding. Then they conducted a search on all the SOSUS records and they were able to find the details and follow it. So you know about that, Alvin , Palomares you know all about. This is an interesting one about applied physics, applied science of any kind and whether people understand what's applicable and what isn't. I got a call from Tom Owen, who was then the chief of naval research. Tom said—boss, I got a funny problem—as the guy who ran the R&D budget and by agreement and law the chief of naval research reported to me and double had it to the CNO. So I said—what's the problem. He said—well, there's an add that just appeared in one of the mathematical journals—remember this is sort of '68, '69, Vietnam war time—signed by three eminent mathematicians saying—mathematicians do not take the dirty money-namely the dirty money from the department of defense—and thereby support this effort for this terrible war. I said—mathematicians are entitled to do that, what's the problem? He said—they're all contractors of ONR and two of them are contractors of the Army and the Air Force- what should I do? Well the first thing I said was—Tom, don't do anything, let's think about it. In those days I was host, because we had the best dining room, to a weekly meeting of the assistant secretaries of the services for R&D and the military chiefs for R&D for the chief of research, the Army and Air Force equivalent, sometimes the director of the CIA, sometimes the president's science advisor, and the director of defense, research, and engineering. It was just a general conversation—what's going on. So we raised this question, what should we do? Finally, after much discussion, somebody, maybe me, purposed—we're going to write these guys a letter. It's going to be a very simple letter that just says—we have noted your add in such-in-such a journal, and as we're now doing our preliminary budget planning for next year, we'd like to know whether we should pencil you in at about the usual amount. So then we got three telegrams and a letter and so on. One of them said—of course, because my pure mathematics, I know that it can not be really used for any military purposes. But nobody felt that we wanted to tell him that in fact his papers were being used to develop algorithms for figuring out ordinance loaning on bombers. Just an example, do you actually have to know what your stuff is used for-not really.

Finally, the main point of the thing is the reason that all of this worked was that you had people from different backgrounds, and fields and subjects working together. The ingredients for that: you've got to have enough mutual respect so you can really sit down and talk about what a problem means. You can't do it the way procurement people like to have it done, at arms length, that is a perfect recipe for failure, and we see it all the time. I mean clearly we're seeing it in the Big Dig in some ways. Conversation has to lead to that; you really have to agree on what you're trying to do. You have to have some kind of sense of working community. Then of course, money, but it may not be a lot of money. I had ,for a long time, a working hypothesis: never give a scientist money for the first thing they come in and ask for, if the third thing turns out to be the same as the first one, that's OK, but not the first one- make them go back and think about it. You've got to have enough bureaucratic support so you can run the place, but you don't want it to put a blanket over you. I think external advisory and oversight systems are good and always you have to listen, but like I've said you have to be careful to think about what you're hearing because maybe it shouldn't be taken by faith. That's it! Questions? Comments?

Q- Can you relate a sea star array to the ARTIMUS project?

It was much lighter and not related. That was something that Bracket was trying to do. Well, it was related in the sense that it was an attempt to answer the question, could we do the engineering that would enable us to make a reasonably big three dimensional array in the deep ocean, and that was certainly one problem that had to be solved. In principal, one could think about doing an ARTIMUS in a region if you build a large three dimensional array. Then you could always drive the sound source around on a ship. But if you thought about the system certainly isn't not mobile, isn't very portable. How many things of these are you going to do? How many ships are you going to run around? The general conclusion was that that was not a good way to solve the problem. The other half of course was—this was all in the era when Soviet submarines were very, very loud. The Soviet Polaris boats were called; the term of art in the US Navy was boomers. The funny part was, there were lots of people are going around saying—well the Soviets don't actually know anything about the acoustics. Then I'd say—there's this guy Brehovsky who publishes on it, would you like to see his articles-he seems to know roughly what we know. So I never have found out, or found anybody that would tell me how come the Soviets decided that they didn't care that their submarines were noisy, I just don't know. My impression would be, that in spite of the fact that Brehovsky and others were both on the civilian and military side, that somehow the higher parts of the Navy didn't want to pay any attention to that. For a while, I had a theory that the Soviet boats were actually really very quiet, but that all the vibration isolators had wooden blocks in them with a sledge hammer hanging next to them. So if a signal was given, you knock out all the blocks, and everything goes down 20 dB. But as far as I know, that wasn't right. So I think there is sort of a connection, but the problem was, that I think the mooring and buoy engineering that Bracket was trying to work with at the time wasn't quite up to it. They had mechanical problems was really the difficulty.

Q-You mentioned the Navy had worries after the second World War, and their view that it was a close call. But there's another source of literature connected with operations research, which is really quite optimistic on the subject of anti-submarine warfare. I just wondered what your take is on that.

Well as it happens, even now, I'm in a running battle with trying to get the attention of somebody in the Navy over the analytical methods they're using for warfare analysis. They are using a system of methods they call standard practice. But which fundamentally makes very simple analytical assumptions. Essentially what they do is they do one weapon, one target analysis. Then they just do the probability of a large number of independent events. That's clearly too simple. There's another body of stuff called configural theory. Which is associated with one particular guy who was a contractor for a long time, that does it more correctly. I have a couple of neat, simple, probabilistic examples, that show that, and there are some very interesting statistical arguments. For example, the standard practice people assume that the law of large numbers always applies. So that you can take averages of averages, and even that isn't right because in non-linear systems, the average of a function of another function is not the same as the function of the average, so they make that error. But also, they assume this law of large numbers, and it actually doesn't apply if you make realistic assumptions about things like minefields and so on. It's not very complicated. After some trouble with my college, I worked out a five page example that someone had suggested the principal of that doesn't even require probability theory; it just requires making the table of possibilities. Then you can immediately see that the distribution does not converge to a mean. The farther you go into the system, the more it tends to flatten out. So there is a large issue there, I have been looking at this, and recently I said—maybe I should find out what the naval research people are up to. So I looked at a couple of text books, and they're pure applied mathematics, they don't have anything in them about how to set up a model. So I don't know what's going on exactly, but I think it's still an issue. I had a place on a slide, which was a place holder to talk about Macnamara's system's analysists of which the less said the better. But, crude analysis tends to produce bad results, is the only thing to say. The sophistication is not in doing the mathematics; the sophistication is in thinking up what it is worth doing the mathematics about. My prime example is Einstein, who always said that he was not much of a mathematician, which is correct; but he had the ideas that were worth doing the mathematics about. So he took a walk with Lady Chafita (sic) who said—oh yes, there is a mathematics for that, it's called tensor analysis, let me show you. So that's a key point. But I know there are all sorts of analyses that say that this is no problem, but in fact if you look at what's really going on, you find out that it is a problem. OK, thank you!