Eberhardt Rechtin

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ORAL HISTORIES

Interviewed by: Finn Aaserud

Location: The Aerospace Corporation

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▼ ABSTRACT

Exposure to JASON and scientific advising. Early JASON projects; SEESAW work on lasers; director of Advanced Research Project Agency (ARPA). Projects Sanguine and AGILE; war issues. Management philosophy; assessment of JASON’s function.

Transcript

Aaserud:

I will ask a few questions, mainly, for a little time, on your JASON experience and that kind of thing. But first of all, I would like to ask you if you have any papers or that kind of sources relating to this, or if you know of anything.

Rechtin:

I have almost no papers on that subject any more. Most of that kind of thing I left at ARPA when I left ARPA, because I was in a completely different part of the Office of the Secretary of Defense, and whatever papers are around would be either in ARPA files or Lukasik's files.

Aaserud:

OK. See, I was here a year ago, and I spoke to Daniel Whitcraft. We had a nice conversation, and he was going to look into the possibility of finding some JASON material at the Aerospace Corporation Library or Archives — you have some kind of archive — and I haven't come back with him on that. You haven't discussed that with him?

Rechtin:

He hasn't brought that up. He's presently in the hospital or just out of the hospital with a heart valve concern.

Aaserud:

It was successful?

Rechtin:

It seems to be.

Aaserud:

Good. You don't have much in terms of personal papers or things like that either?

Rechtin:

No. My principal contact with the JASONs was through Project SEESAW.

Aaserud:

I'm asking that not only because of my JASON interest, but also because the Center I work with represents an interest in retaining the history of physics materials, and the universities are better at that, until now at least, than other institutions. So we're trying to learn a little more about other sides of the history of physics and what kinds of information are available in that respect. But let's get on with the questions here. What was your first experience with JASON, your first knowledge of JASON? Of course, JASON was established in 1959, or 1960 — around there — when you were at the JPL. [Note: I had no direct contact with JASON at that time as far as I can remember. ER]

Rechtin:

Right.

Aaserud:

And ARPA too of course was involved in JPL, certainly during the early years.

Rechtin:

As a matter of fact, ARPA Order No. 1 went to JPL. And that was to launch what we later called Pioneers to the moon.

Aaserud:

Was there any connection between the JASON connection with ARPA and the JPL connection?

Rechtin:

I don't recall any, no, although we knew that there were various scientific groups that were formed during that particular time, all of whom had ideas and impressions on what the space business was going to be or should be or something. There was PSAC, it got in the act, and Wiesner got in the act himself personally, and a variety of astronomers got in the act. And we were the guys doing the work.

Aaserud:

Did you have any personal contacts with JASON members, either as JASONs or as people involved in these matters from different points of view? At that early time, I mean?

Rechtin:

Well, we had various scientific groups which would show up to talk to us at JPL, as we began in those early years. One of them, I remember, was a very prestigious group. It happened to have three Nobel Prize winners on it [added by ER in proof: "JASON?"]. And they came out to tell us that we couldn't conceivably navigate through the solar system the way we were proposing to do it, because of the limitations on the velocity of light. And so we had to prove to them how wrong they

were, in as few lines as possible, which we did. That was one of the scientific encounters we got into. Their mistake was, they didn't appreciate that the way we were going to navigate was not in meters but in microseconds, and we converted the navigation problem to a time problem, and we found out how far the planets were by the elementary technique of sending out a radar pulse and finding out how long it took to get back.

With that, we then determined the astronomical unit, which was average distance from the sun to the earth, and improved it by a factor of 100, right off the bat. We then said, "Well, since we know how far Venus is in microseconds, we can figure out how to get there, if we keep in the same units." OK? It then didn't make any difference what the scaling factor was to meters. And as long as the space between here and the planets was reasonably uniform, we could have been going through molasses, for all we cared. So we said, "We propose to navigate in microseconds, and as clocks get better and better, our accuracy will go up proportionately. The thing which degrades the accuracy is the geometric situation that you're in, what distances you have, what motions you are taking, the gravitational fields, and whether you reasonably know them or not." And we figured we could easily navigate to a part in 10 to the 6th and maybe a part in 10 to the 8th, if we chose to do so, which was far more precision than anybody knew the velocity of light. And proceeded to do so.

Aaserud:

That was a pretty fundamental mistake on the part of the experts.

Rechtin:

Well, we were often told we couldn't do things, in those early days.

Aaserud:

That was a regular mode of doing things, that you had these prominent people looking over your shoulder?

Rechtin:

Yes. Either because they liked what we were doing, but didn't know it was possible, or because they didn't like what we were doing and wanted to show that it was impossible, one of the two. We got them fairly routinely. People who said we could never communicate to the edge of the solar system with only 10 watts. That was clearly impossible. Well, that's now routine. We said how we would do it and proceeded to do it. So I got used to scientific advisors at a very early stage.

Aaserud:

Maybe a negative experience. That's another matter. So were those people appointed by any agency, or did they come independently?

Rechtin:

Oh, I frankly didn't pay particularly much attention to the auspices. That would come through the director of JPL, and he'd say, "This group is coming down and would you guys talk to them." We'd talk to them.

Aaserud:

So you didn't learn about JASON or have any experience with them at that early stage?

Rechtin:

That didn't ring a bell. We weren't in the nuclear business. That was the one that they were specializing in, and which they were very well known for. We weren't in that game.

Aaserud:

So was it only when you came to ARPA that you had some contact with them?

Rechtin:

Yes.

Aaserud:

Because ARPA was the main supporting agency.

Rechtin:

I guess I really should say that there were three things. There was SEESAW. There was the Sanguine ELF system. And there was the Vietnam War and its aspects.

Aaserud:

So should we take one at a time?

Rechtin:

OK.

Aaserud:

Why not start with SEESAW, since you mentioned that first.

Rechtin:

OK. That was the wildest of the three. I thought it was particularly aptly named, because it seesawed each year from "it's practical" to "it isn't practical." And sometimes it was the same scientist each year proving the opposite of what he had proved the previous year. The problem, obviously, was to produce a lightning bolt, in a sense, out to destroy missiles, and that's led to the charged beams and the neutral particle beams and a lot of the other things subsequently. But at the time, you had to launch this beam from the surface of the earth, rather than from space. Nobody was talking about doing it from space at that time. And so, how would you ever get a particle beam, charged or not, out of the atmosphere? They found very early that if you're going to direct it, and do much of anything at short range, it has to be a charged particle beam in the atmosphere, and it obviously had to be neutral if it was in outer space.

Now, whether or not you can get a beam of electrons or whatever to go through the atmosphere was questionable. As a matter of fact, it probably still is questionable as to just how far you can get and why. As I remember, the first time they came in

and said, "We've got a water hose problem," and we had to figure out what they meant by that. And obviously there was a loose water hose with water coming out, and you were seeing the spray going in all different directions as the hose twists under the forces in the water. Well, in this case, the same problem was going to occur if you tried to shoot electrons into the atmosphere, and it was going to go and spiral out, thrash its way out and then get stopped. So then they tried to concoct schemes where you would make yourself your own channel.

That is, you'd blast away at the atmosphere, heating it in such a way as to create a channel which presumably then the other charged particles could stream down. To do that, you had to keep constructing the channel. And when they first started, they wondered how to get it out the nozzle alone, and then they figured out how to get it a few meters. The equations then said it wouldn't work any further than that. They tried to think of ways, and got into ideas of pulsing it, rather than doing it continuously. Each of these ideas took about a year to try to figure out, so about once a year they would run into an obstacle, analytically if not physically, which they'd have to overcome next. I thought it was a fascinating idea, quite appropriate for ARPA, because the way I felt ARPA should be run was that we should be working on things with high payoff and high risk. There's no point in high payoff, low risk for ARPA. Somebody else can do that. There's no point in low payoff, high risk for ARPA. That's just bad business. So we wanted to do high risk, high payoff, with the idea that 50 percent of the time, we might find something useful, and the other 50 percent of the time, if we were successful, we would know why we couldn't do something. We thought that was equally important. We could then have essentially a 100 percent success ratio by failing half the time if we knew why.

So I thought the SEESAW was quite appropriate. I supported it the whole time, through all of its vicissitudes, including all of the strange contracting that seemed to go on, and how the same people always seemed to be able to come back with another year's worth of work to do on the opposite premise of the preceding year. But it didn't bother me. Our contracts shop kept things straight.

Aaserud:

You inherited the project, right?

Rechtin:

I inherited it. It had been going on for, I don't know, four or five years, I guess, by that time. It was a typical ARPA program, of the kind that ought to be part of the mix. Steve Lukasik and I didn't agree on this. I'm not sure but that he would have thought of it as quite a scam, but he clearly didn't want any more of it than he absolutely had to have. I think he finally canceled it (after he became director), and it drifted off into other things, and finally reappeared in the Navy as an effort to see whether they could do defense of ships, short range. Heritage, I think, was the name of the Navy project, if I recall it right.

Aaserud:

So it died in the early seventies?

Rechtin:

It died in the early seventies, and was resurrected again about in the mid-seventies. Then the Congress decided it was the wrong thing for the Navy to do, so they handed it back to ARPA again, and in due course it got picked up with the SDI, but as neutral particles in space. I don't know what the status is of the charged particles in the atmosphere, whether anybody is still working on it. One of the services, I would guess, but I just don't know. I haven't kept track of it. Anyway, typically it seesawed. And JASON was square in the middle of it. Indeed, some of the members were personally involved in it. It then got built into a project under Ruth Davis — if you haven't talked to her, you should. OK, it's Dr. Ruth Davis. We can give you her phone number, name and address. She's now a member of the Aerospace Board, so you can ask about that, but she was the one who probably had more to do with beam weapons, in her position as Assistant Director of DDR&E for Research and Advanced Technology. Brilliant woman, you'll have a great time interviewing her. She's in Washington. And she pursued it to the point where the limited accelerator, I forget the exact name, was to be built by one of the national labs — I don't know if it's Livermore or Los Alamos but I think it was Livermore.

Aaserud:

When is this, now?

Rechtin:

That would have been late seventies, when the program began to get more formal. So that device began to be built there. And then she also was deeply involved in high energy lasers, which was another thing the JASONs looked at every once in a while. In a sense, lasers are also an up-and-down kind of program. I remember when they first started to work on the lasers. Hey, that's another one JASON got involved in. I forgot that. The more I think about this, the more damned things they're involved in. That had its own curious history. Why don't I do that one next?

Aaserud:

I just have a few more questions on the SEESAW thing. What did JASON do and what did ARPA do on that, or is it meaningful to distinguish?

Rechtin:

Well, ARPA would have been the leader, the funder and everything else. JASON was funded by ARPA as an advisory group. So JASON as such didn't do the SEESAW program. Individuals there might have contracts with ARPA to do it. So JASON has always been advisory, and the money that it gets is enough to pay expenses for the group.

Aaserud:

To what extent did JASON do most of the work, or a smaller part of the work?

Rechtin:

Oh, I'd say that the activity of JASON as a whole in that area was relatively small. They were advisors. They would meet, oh, I don't know, once every couple of months or so, then all sit around and have some subject that they were going to go over. I would guess that the different ARPA directors used them in different ways.

Aaserud:

What was the nature of your contact [with JASON]?

Rechtin:

ARPA was the then supporter of JASON. I don't know whether we did it through IDA or directly, but it was the principal support. That was another thing that Steve and I didn't quite agree on. I thought it was fine to have that gang, they were interesting and they certainly knew things I didn't know. Steve was quite unsure about that. So in due course, he managed to divorce JASON from its direct tie to ARPA, and I suggested that JASON instead be carried under a MITRE banner, that MITRE could probably act as sort of the administrator and general support easily enough. They'd asked Aerospace whether we wanted to do it, and at the particular time [added by ER in proof: "late 1970s?"], I suggested it wasn't too appropriate for us — we were backing out of sponsoring consultants — but that MITRE being in Washington might be fine anyway. MITRE seemed interested, and I think that's where it went, so it continued to get support.

Aaserud:

This was in the seventies?

Rechtin:

Yes, this was in the late seventies, or early eighties, yes.

Aaserud:

While you still were ARPA director?

Rechtin:

Oh no, no, no, no. As long as I was there (1967-70), JASON was under ARPA, and the shift was either Lukasik or post-Lukasik.

Aaserud:

JASON transferred from IDA to SRI in 1973, and Lukasik was director then.

Rechtin:

Then it got shifted again.

Aaserud:

Yes, that was in the early eighties.

Rechtin:

Early eighties? OK.

Aaserud:

To MITRE. Did that affect the relationship with ARPA?

Rechtin:

It should have, because they no longer had the direct... OK, so Steve would have moved it away from IDA, while he was there. I was not then at ARPA. I ended ARPA in 1970. And so he would have gotten it away from IDA, because that was our direct support — we went through IDA, that was just an administrator. He obviously moved it to SRI, one step further removed, and treated it as though it were no longer FCRC-affiliated. Because IDA-JASON then looked like a combine. He broke that combine and said, no, I'm going to have SRI administer, or something like that. And I guess SRI got disinterested or something and then that was about the time, after 1977 — you say it was 1983 or so — MITRE came in or whatever.

Aaserud:

Yes, a little bit before. I think it was a little less than ten years roughly in SRI.

Rechtin:

OK, they managed to keep getting funded by one route or another.

Aaserud:

Did you have any hands-on involvement with JASON, or was that lower down with the program managers?

Rechtin:

The program managers had more contact with them than I did. Another guy you may want to talk to, if you haven't already, is Sy [Seymour] Deitschmann. Have you had a chance to talk to Sy?

Aaserud:

I haven't talked with him, no.

Rechtin:

OK. He's still at IDA. And he had a lot of contact with them. His contact was largely the part of the Vietnam War and counter-insurgency.

Aaserud:

So that was during your tenure.

Rechtin:

That was during my tenure, right. And so he also has a very good memory and probably could comment in more detail than I could as to exactly what JASON did and which people in JASON. So I guess I would meet with JASON once every six months to a year, something like that, but the day-to-day contacts were often at what's called the director level.

Aaserud:

I think it's important to have your perspective on JASON too, or ARPA directors' perspectives, because until now I've been interviewing mostly the JASONs themselves, and it's a very different perspective they have of themselves.

Rechtin:

Of course!

Aaserud:

So that's why I've come to Lukasik and come to you and I should also interview the early ARPA directors, DDR & Es, John Foster, all that.

Rechtin:

Well, the government people probably thought the JASONs were arrogant, and the JASONs probably thought the government was either stupid or authoritarian, one of the two. I think I understood both sides well enough.

Aaserud:

So you communicated with both sides also then. So that was SEESAW.

Rechtin:

Yes.

Aaserud:

You remembered lasers along the way. There was Vietnam and then there was Sanguine.

Rechtin:

Sanguine. The ELF system. The laser business started early in my time there, when Johnny Foster...

Aaserud:

...it started during your time?

Rechtin:

Yes. Johnny Foster felt that the work that [Arthur] Kantrowitz was doing was so important and was going to lead to such vast things that ARPA should go out and sponsor something. ARPA then could go sole source to anybody with a good idea, and proceed to develop it. In my cursory look at the subject, I concluded that the laser schemes as then proposed were vastly overblown. At the time, they had what

was called gas dynamic lasers, which were essentially rocket engines that you seduced into putting out coherent light. It was extraordinarily inefficient, and it looked like a large rocket engine. Well, I'd been in JPL and rocket engines, and I knew what that stuff looked like. I knew how far it was from that to coherent light, because we'd also worked with Charles Townes when he was doing his laser work, and I thought, well, that's interesting but it's inefficient. It's not clear that that's going to go very far. Indeed, I made a bet with the guy who was working at the time, Dave Mann — you might want to talk to him too, David Mann, he's in Washington also, runs his own shop. I would bet him, as I recall, one dollar to be doubled every three months until they produced a megawatt. When last seen, he owed me the DOD budget! They never did get there from here.

Aaserud:

When will you collect it?

Rechtin:

I'll write it off on my income tax as a bad debt, right? That'll take care of my future income tax for a long long time. And JASON and its people turned out to be very helpful there, because as that gas dynamic CO_2 laser proceeded, it impressed me as really not going anywhere, that it was far too large, too cumbersome, too spread a beam, OK, not a good tight beam, ever to be made into a weapons system.

Aaserud:

More so than SEESAW as you saw it then?

Rechtin:

Well, for that particular CO_2 gas dynamic laser, you put this enormous amount of front end energy out of a rocket engine, no less, chewing up fuel like mad, and you have big fuel containers all over the place. It's a massive installation. And even a megawatt isn't very much, when you get around to how much damage you can do. Sounds like a big number, but, you know, what the hell. And you use up all this fuel and in a few minutes it's all done. And I said, "We're not going to put that thing in orbit. We're not going to carry it around in a tank. That doesn't seem to me to make very much sense." But I'm not a good enough physicist to know whether the fundamentals are against me in this game. So I got JASON together, down in

Florida, and the group down there took a look at it, and they concluded, it doesn't look as though this one's going to go very far. If they're ever going to get anywhere, they're going to have to extract laser light out of chemicals much more directly.

Aaserud:

Now we're in 1971?

Rechtin:

That would be before 1970.

Aaserud:

OK.

Rechtin:

Relatively early on. The project was then called Project Eighth Card. And it was a limited access by name. It wasn't what we now call "compartmented," but it was obviously controlled. We got these people together, as I recall a subset of JASON. We all went down to Florida to visit the United Technology laser installation there, and we looked at what they had there. We also talked about this thing. It seemed pretty evident to me that it wasn't going to go anywhere, so I asked the collected group, "Is this going to go anywhere?" And the guy who knew most about it — I don't recall his name, if somebody told it to me I'd say, yes, that's who it was, Steve may remember or Dave Mann may remember, or Professor Hal Lewis of U.C. Santa Barbara, it was a French name: LeLevier? I believe so — said, "It doesn't look as though it's going anywhere." I said, "Fine, I'll cancel it," which we proceeded to do, right then and there. That's the glory of being an ARPA director. You can do that. You don't have to ask anybody. You didn't have a captive lab you were trying to support. You can decide, nuts to that. If it's going to have a prayer, it's going to have to have the efficiency of a chemical laser. At the time, chemical lasers were on the distant horizon. By a curious quirk of fate, it was The Aerospace Corporation, here, that came up with the solution of how you make a chemical laser. And so the big lasers you hear about now are chemical lasers, essentially originated here, then expanded by industry and still going in SDI and various places as chemical lasers. We spotted that there was no point in putting further research work into gas

dynamic CO_2 lasers. It was a dead end. And so JASON and its people indeed did have a very influential effect, at just the right time, enabling us to stop something which otherwise would have grown.

Aaserud:

That's one function of them, yes.

Rechtin:

So I told Johnny Foster we were no longer going to support his friend Kantrowitz, which is one of the things that also I was entitled to do as director of ARPA. [Note added by ER in proof: "Johnny agreed. He subsequently became disenchanted with laser weapons in general."] We're not going to do that anymore and we're going to put our money instead into chemical lasers, wherever they were, and to support whoever has some ideas. So that was the early laser study. In the very beginning, I had been skeptical of that approach, more in my gut than anything else. So the first funding that ARPA got was not in the ARPA budget. Johnny Foster used to have a DDR & E separate little budget that he could use. I knew that. I said, "Johnny, if you want to start this thing, it's got to be on your money." I don't know how much risk I was taking with my job at the time, but I insisted that he pay for it to start. It was three million dollars starting. So he anted up the first year. Nothing much happened, OK, and we picked up some more in ARPA until we finally said, that's the end of that. Now, another interesting twist. As long as we had it pseudo-compartmented, the Russians, as far as we were ever able to tell, were not doing anything in high energy lasers. We decided to kill the program and reduce Eighth Card to a standard secret program, because although we were killing it off, we wanted to maintain the techniques at a secret level anyway. And apparently, the best information I ever got, within weeks after we set it up as secret, the Soviets started their high energy laser program. Because they'd already targeted the various organizations working in this program. They didn't know what it was. But as soon as we broke it back to secret, within weeks it had leaked, and within weeks they said, there must be something there, we'll go after lasers too. So if anybody wants to say, when was the start-up date of the Soviet laser or SDI program, you can peg it almost to the week. That triggered it.

Aaserud:

There's a cause-effect there.

Rechtin:

Yes, they spotted what we'd done. Now, later we recognized that the Soviets do indeed have what I call a "draft strategy." A draft strategy is a very good one for an organization that doesn't particularly want to take the lead in technology, but wants to be able to exploit it, when and wherever they choose. And the word "draft" is taken from an automobile race, where you stick behind the leader in draft position. You can stay there almost indefinitely, and then pull out with a little extra energy when you feel you need to, and get to the combat line or the finish line at the same time, if not first. And so they would wait for the US to demonstrate something. They wouldn't need to do that. Our research is generally unclassified, so we hand all that to them anyway, and they draft behind us technologically. That's been borne out over the years by the fact that the Soviets have been consistently five to ten years behind us, regardless of the total amount of money they spend. And so the Eighth Card was my first, my real experience in that game, as to how that draft strategy might work. So that was Eighth Card and that was the lasers. Afterwards that expanded further in the direction of chemical and free electron lasers and so on. Now, let's see, the next one, probably Sanguine. That was this marvelous scheme where you radiated at about a 50 Hertz radio frequency, to submarines. It was invented by Nick Christofilos, that astounding guy at Livermore.

Aaserud:

Yes, he was in SEESAW too, wasn't he?

Rechtin:

Probably was, yes, he was involved in all of these things. We were all "convinced" that he'd come back here from 200 years hence, come back here to try to get us going on the right track so that 200 years hence his project would finally get done. And that's where I got a lesson in advanced politics, because on the technical facets of it, he was pretty close to right. But we had to find the right kind of a place where you could build an antenna that would indeed radiate at such a low frequency, because the wavelength is so very long (about 4000 miles), you're going to interact with the earth. It therefore had to be on a portion of the earth which is the right kind of dielectric rather than a conductor.

Aaserud:

You were involved in that problem too, were you, or how was that?

Rechtin:

It had been going on for maybe four or five years. It was first thought up in the early sixties. And our role was to somehow try to keep it going. Later on, I went from ARPA to head all telecommunications, and that of course was part of telecommunications, so I continued some more. I had six years worth of Sanguine politics, and saw what I considered the underside of politics in that thing. I've never seen anything played quite so dirty, or so irrelevant. It was at the time of the Vietnam War and the anti-Defense thing and whatever. But I watched the newspapers behave about as irresponsibly as I've ever seen anybody behave, and it just increased my general irritation at the time. I was already irritated at what they were doing with the Vietnam War. But to watch them try to block one of the few ways that we could keep control of our submarine force — which was a damned dangerous force, if you will, certainly if it's out of control –- I felt was just plain irresponsible. And the idea that power lines operating at far less power than the normal power lines overhead were somehow this terrible danger to everybody is very strange — first occurring in the North Central US and then down in Texas and so forth. I was just disgusted. That battle kept on going long after I left. And I guess there finally is an installation now, and as far as I know, they're using it as a bell ringer, whatever. That was a strange and wondrous thing.

The role of JASON was essentially to try to maintain some sort of scientific honesty to it anyway, to try to make sure that the Navy could get some help in the design of tests and so forth, all of which showed nothing serious is going to happen to anybody; although it's possible to measure effects, there's a long road between effects and danger or damage. If we're all surrounded by 60 cycles all the time, and we all drive under power lines all the time, then why this had to be such a big fuss, I don't know. The media or the public put us in the hopeless position of proving that nothing could happen. That's an almost impossible thing to do. One of the most difficult things in science is to prove that absolutely nothing can occur. You're always going to measure something. The question is, what does it mean? But nobody was interested in that. So that was a difficult time.

Aaserud:

Did the project die because of this?

Rechtin:

It struggled along. I think there are now two facilities, both of them relatively small. I think they're both on government land. To make them survivable, you'd like to make them a lot bigger, so that they'd be a much harder target to take out. They were even assuming that there were going to be nuclear blasts to take out a piece of power line, for instance. They had the people of the North Central states convinced that they were going to be a prime target in a nuclear war because this thing down around the landscape. Very strange. Now, the Vietnam War. There the interest was in counter-insurgency and in what the tactics and strategy might be. I don't know that JASON was any wiser or better equipped than anybody else to do that. But I think they tried to be as objective as a group could be in the very difficult political circumstances of the time. It was very hard to do.

Aaserud:

Were they asked to contribute, or was that something they took on by themselves? How is that question answered generally speaking?

Rechtin:

They were often self-initiating. They weren't a task group. You could task them if you had some problem. They were always glad to pick it up and see what they could do. But I think they were trying to philosophize, as many of us were. My impression is that they tended to agree with us that the problem was by no means solely military, and that at the heart of it was, did we really understand the population and its culture? If we didn't, what might we do to try to understand it better, so that we could be more effective? As such, we tended to run up against conventional military thought, and so it was civilian versus military, but with a different dimension. In this case, we were also trying to help rather than trying to stop. So our thoughts at times were orthogonal to that of the conventional military approach. I suppose you needed some of both, when all was said and done. My history of the Vietnam War says that we won the war and lost the peace. Unfortunately, the impression the American public has is that the American military was driven out of Vietnam, which is not the case. The American military

hadn't been there for two years when the North Vietnamese invaded in strength. Anyway, so they were trying to help there. They were also trying to help on some physics problems.

Aaserud:

You're still talking about Vietnam?

Rechtin:

On Vietnam, yes. If my memory serves me right, one of the problems was that the Marines were surrounded at Que Sanh, the North Vietnamese were thought to be tunneling in under the base, and the question was, can you detect the tunneling? And Lincoln Lab had come up with a radar, a strange kind, because you just put it on the ground and aim down, instead of aim up, to see whether you could detect voids, which would then tell you that there was tunneling going on. And in due course a machine was built. As I recall, it looked like a very large lawnmower, about a meter or so in diameter, which you drove around trying to see whether you could spot voids. I believe it did prove the ability to do some of that. The place was so noisy, with all the artillery going off, that you couldn't use sonic techniques very effectively. Be that as it may, no tunnels ever succeeded. How many came in at us, I'm not quite sure — you'd have to go back and look it up — but I think the JASON people at least looked at the feasibility and tended to agree. I don't know whether they were involved in the so-called — what did they call it? It wasn't the McNamara Line. The line of sensors that was supposed to go across, that Kistiakowsky got so embroiled in.

Aaserud:

The electronic barrier.

Rechtin:

Yes. The difficulty with that barrier is, the North Vietnamese came south of it before the barrier could ever be set up, so it didn't help, but we did use it in a different way. We sent a lot of this equipment over there — the sonar buoys and the sensors and so on, and all those things. But the original scientists' idea was, whenever one of these things would go off, you would shoot at wherever that sensor said something is happening. The military planners, particularly the Marines at

Que Sanh, learned a far better way; indeed, it was more conventional in that sense. They used it as essentially intelligence collectors, and by listening to what was going on, figured out what the troop movements would be, and then waited until there were concentrations, because you could predict that from listening on the roads and so forth. You would wait until all your indicators said they had massed in location X.

Then you hit them with everything you had, and the carnage was awful. The worst of the examples was, they would wait until the sensors out there told them that the North Vietnamese were gathering, let them come within artillery range, which they were always doing surreptitiously. Then what they'd do, the Marines would launch an artillery attack behind them, force them forward into the machine guns. Highly effective, but jeez! Anyway, that's how the sensors turned out to be used and turned out to be effective, rather than as a barrier. ARPA also was involved in trying to set an electronic barrier on the Korean DMZ, and there we learned that that's just a way of adding expense to the barrier. The simple way up there was cyclone fence, pop-up mines, South Koreans every hundred yards, and searchlights; you didn't need all those electronics and they didn't work very well. We worked closely with the Israelis at the time. I don't know whether they have found other schemes, electronics that were better or more efficient necessarily.

There's nothing like a well-illuminated fence with a machine gun every hundred yards. So far nobody's managed to figure out how to do much about that. But the North Vietnamese, to train their troops, had as their initiation ceremony, a guy had to go up to the fence, cut out a square yard of it and take it back with him. That was their initiation. And they did, from time to time. Boy. What a thing to be up against. So I guess those were the main ones where, as I recall, JASON did get involved. I always had a lot of respect for them. I don't know that they ever did a large amount of analytic work and reporting as such. But they were a good one for sanity checks. It was only when they got off into the philosophy of how do you deal with the Viet Cong, that I think they were kind of out of their range. But so were we.

Aaserud:

Do you mean, the counter-insurgency thing?

Rechtin:

Yes.

Aaserud:

Was that part of the behavioral sciences program or the AGILE program?

Rechtin:

AGILE. Yes.

Aaserud:

Right, so the JASONs were connected concretely to ARPA programs.

Rechtin:

Yes. This book on AGILE is extraordinarily biased against it. It's too bad. The author was in that program and he saw all the confusion, but he didn't quite understand what we were attempting to do, what the rationale was, and the struggle to have a group of Americans try to understand a very foreign war. Deitschmann has written a book on the subject of AGILE and others on defense strategy; I have a couple of them here. You can borrow them if you want to send them back, or you can talk to him when you get there.

Aaserud:

Yes, it would be interesting to see that.

Rechtin:

OK? Well, that's fairly close. What else can I do to help?

Aaserud:

Well, just what do you think the impact of JASON has been? I mean, has it made a difference generally, philosophically speaking?

Rechtin:

Well, I'm obviously biased. If I thought it didn't make a difference, I never would have supported it. I was known in ARPA for terminating things abruptly that were worthless, or that somebody was putting us on or trying to lead us down a daisy

chain, or that I felt I couldn't trust.

Aaserud:

The emphasis on transfer was introduced with you, more or less. Wasn't it?

Rechtin:

Yes. And it's had a checkered history, too. My principles for operating ARPA were characteristically mine, and one of the facts about ARPA history is that each director has had a different view of the world. I don't think that's bad. It says that every two, three, four years, you have a completely different view brought in. ARPA is one of the most amazing inventions of the government that's happened in a long time. It essentially recognized the weaknesses of the formal Defense structure, and was invented as a mechanism to take care of those weaknesses. I'm not sure that the originators understood its potential as completely as it then turned out. One of the prime weaknesses is between justifications and research. The dilemma in normal Defense Department work is that you can't do research unless you have a justification. Justification means you have to have an application. On the other hand, research often doesn't know where it's going.

So therefore you can't justify research and get the cycle started to produce something which has an application which justifies the research in the first place. So ARPA does not have to have a justification of a specific kind. All it really has to say is that this seems relevant to Defense somehow, but we aren't sure where. It's an ingenious bureaucratic invention. The next thing that they did — and this had to be determined after the first year or so — they decided not to have any laboratories of their own. There was a big battle about that. It was one of the wisest decisions ever made, because it meant that ARPA could drop out of anything it wanted at any time, and often did. It didn't have to support anything. It didn't have a built-in set of material science labs. It didn't have a built-in set of neutral particle beam labs. It didn't have any of that. So that was good. And then it was always run by transient civilians, so that you didn't have a permanent staff trying to justify its own particular line of research. If we wanted to cancel our job, we could cancel our job.

If we wanted to cancel Eighth Card, we could cancel Eighth Card. Or whatever we chose to do. And people would go and leave. Also, I had my own views, and they were certainly tempered by the fact that we were at war. It was an ad hoc war, but we were at war, and people were getting killed, and it was our job to both try to win

and reduce the casualties, and I felt very strongly about that. My predecessor — several predecessors — felt that ARPA was something like a university whose job it was to find money to handle its professors. I didn't agree with that. I'm an engineer, not a scientist, and a very practical engineer who wants to see things done usefully for whomever the customer is, and felt that we should do something about that. This was also the time of relevance, which was all right, but I felt that mandating it had long term effects that were very bad. [Note added by ER in proof: "the infamous Mansfield amendment"]. It's made mission agencies, including Defense and all the rest, far too narrowly focused in their research. Fortunately there's still ARPA, but even ARPA was told to be closer to relevant. So I felt, the two criteria for success were, first of all, it had to be professionally done — either proved it could be done or proved it couldn't be done and why. And second was, if it could be done usefully, you had to transfer it, because we were not an operational agency, so therefore to make any difference on the combat line, you must transfer. And that had a lot of management consequences. Not everybody felt that way.

I don't think George Heilmeier — later director of ARPA — felt that way, and he started a number of projects, some of which we still have, which are non-transferable. They are of themselves dead-end demonstrations. And although a demonstration is a fine idea, unless it's built into something that's going to follow it, you question why you did it. I felt it was important to transfer, and how much got transferred is harder to measure, because unless that policy was in place for, I would guess, at least ten years, you wouldn't see a steady stream and a continuous flow-through. So I think the fraction that was transferred while I was there was probably not more than about 10 percent, but I figured there should be a complete turnover of ARPA projects every four to ten years, something like that.

Aaserud:

How does JASON fit into this change of policy? Did JASON become more relevant, more transferable? Did you have conflicts with JASONs on this?

Rechtin:

I never did, because I felt that was a completely different aspect. The idea of the criteria for success were essentially management criteria. How do you judge success of an ARPA effort, on the one side? And the other had to do with the technical soundness of something. In a sense, JASON was coupled to the criteria

which said it had to be professionally done, and therefore there was no conflict at all with us. The fact that it had to be transferred had nothing to do with JASON, and a great deal to do with how you worked with the services.

Aaserud:

Yes, so JASON was fairly independent.

Rechtin:

It was independent of that. Yes. And those policies, as far as I was concerned, were irrelevant to the user of JASON.

Aaserud:

OK, fine. I have seen some JASON steering committee minutes, and there was one that I thought was particularly interesting, from the point of view of our interview here. It reads, "E. Rechtin discussed the just concluded ARPA review of JASON. He stated that the most successful JASON efforts had been those which followed the long term involvement of JASON in the technical area concerned. He said that JASON was less successful in, 1) quick response in a new area, and 2) complex system studies. He also stated that in some cases the DOD was not interested in or not ready for the JASON output. Specific examples of the non-successes were not given, but the JASON work on 'unsound ASW' [the slang term for non-acoustic antisubmarine warfare] appeared through discussion to fall into the last category. He accented his strong interest in JASON as a 'technical scouting party' for new ideas, particularly those appropriate to ARPA." And further down, "Rechtin left with us two brief critiques of JASON prepared at his request, plus a list of possible future JASON problems. It was agreed H.W. Lewis [of U.C.S.B.] would prepare an informal description of the JASON modus operandi and possible future activities." [“Minutes of the JASON Steering Committee meeting, January 27, 1970,” Charles Townes Papers, Berkeley.] I'm very interested obviously in that kind of review that you're talking about.

Rechtin:

It sounds precisely like me, even the words they chose, so whoever wrote the minutes got not only the gist but the flavor of it.

Aaserud:

Good.

Rechtin:

That's typical Rechtin. I try to lay it on the line as best I can and as objectively as I can, and I would say the same thing again.

Aaserud:

What were the circumstances, background, for that review, and what did it say, and is it available, do you think? Is it declassified?

Rechtin:

Oh, I'm sure it's declassified, and if I had to guess, it would have been one of the semi-annual queries as to, "why do you have so many committees?" That question shows up frequently. The Packard Report for example just came out and said there were too many committees. Or the Congress will come out and say there are too many committees, or Mansfield will say it or Fulbright will say it, in those days, why do you have so many committees? And the gist of this is one of those typical ones where we had to review all our committees, and see whether or not we thought they were all right or not, and obviously the conclusion was, I thought it was just fine. I don't think it had anything to do with whether we were, as ARPA, specifically and personally concerned about the JASONs at the time.

Aaserud:

It was Joe Bengston, by the way, who wrote that, who's responsible for the good coverage. I guess I won't keep you any longer, unless there's something you want to add on your own, wrapping this up.

Rechtin:

Well, as a matter of fact, you trigger me into remembering far more about the JASONs than I thought I knew.

Aaserud:

OK, good. Great.

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