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THE SEARCH FOR EXTRATERRESTRIAL INTELLIGENCE

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K. I. Kellermann and G. A. Seielstad

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PROJECT OZMA

Frank D. Drake
University of California, Santa Cruz

I am here to tell you, perhaps why we are all here today. It may seem strange, but this is the first time I have ever given a talk about Project OZMA. I was asked to tell you just what did happen back then and, as you will see, the story creates a context from which we can view all the things we have heard today and, in a way, feel good about it. In twenty-five years we have come a very very long way. Of course, that bodes well for near-term programs in SETI. If we can continue the rate of progress we have had in the last twenty-five years, twenty-five years from now it should be a leadpipe cinch to detect life in space.

The story begins about 1958 when the NRAO was founded. At that time there were two astronomers here, David Heeschen and myself. That fact influences what eventually happened because we were both from Harvard and both used the same particular instrumentation. The NRAO was then well funded and was building telescopes at a very great pace. The plans then were to start with the 140-foot telescope, which was considered to be a modest telescope at that time, and then called for the early construction of a 600-foot telescope. That size will ring a bell with some of you.

By early 1959, the first parts of the 140-foot telescope were arriving at the site, and one of the prime parts was an extremely massive polar axis. You may wonder what this has to do with Project OZMA, but it will become clear. This thing was made out of sheets of steel several feet thick, all welded together, all terribly massive in order to provide adequate rigidity in the equatorial design on which the 140-foot telescope was based. That, too, was a result of the influence of Harvard University, where equatorial telescopes reigned. Alt-azimuth designs were very foreign, so that equatorial philosophy infected NRAO for a long time.

Now at just about the time that polar axis was to be installed, a massive project, it was called to the attention of the observatory that the polar axis had been made of a type of steel which, when the temperature is below freezing, cracks and is the subject of "brittle fracture." This had been the cause of many ship sinkings during World War II. The temperature does get very cold here in Green Bank, and now we had a polar axis which was going to crack, and this was a terrible crisis in the history of the observatory.

The polar axis had to be abandoned, and one didn't even know what to do with it. I believe it is buried out behind he 140-foot telescope because it was too costly even to cut it up and take it away.

In any event, this catastrophe affected the NRAO very seriously. Wanting to become a national center serving users as we were supposed to, we looked for a rapid way to achieve some observing capability. The solution was to build an 85-foot telescope. There was one on the drawing boards designed by the Blaw-Knox Corporation for the University of Michigan. The

NRAO funds flowed freely then. We just went up to Blaw-Know and ordered an 85-foot telescope. It was erected here in record time and, in fact, was delivered before the one at the University of Michigan, which irritated Fred Haddock no end. In 1959 in the spring that telescope was in action here in Green Bank.

We were quickly looking for projects to do with it, and many things were done. It was a time when it was easy to make discoveries, such as the Jupiter radiation belts, structure of the galactic center; things like that just rolled out very easily.

One day I sat down and tried to calculate how far that telescope might detect the strongest radio transmissions then leaving the earth. It worked out that with that telescope with its huge collecting area and the new radio receivers which had just come on the scene, it could detect the strongest signals leaving earth from a distance of ten or twenty light years, which was pretty remarkable. That was the distance to the nearest stars. This result depended on these new receivers. The solid state maser had just been developed by Bloembergen at Harvard, and a new thing called a reactance amplifier, later called a parametric amplifier, was on the scene. These in particular offered the possibility of lowering the system temperatures from 1500 degrees (which we thought was pretty good) to about 350 degrees in the case of the paramps, and down close to zero in the case of the maser.

There was another device on the scene which probably all of us have forgotten, and that was called an Adler tube, which was an electron beam amplifier which also produced system temperatures of the order of 300 degrees. In any case, the great turning point came one day at lunch. There was Dave Heeschen and Otto Struve, who was then the Director, and myself and another person. I can't remember who that was. We went to lunch at Ryder's Diner, which you can see when you drive through Boyer on the way out. It is still there, still serving the same greasy hamburgers. But that's all there was folks! In those days the entire NRAO consisted of a farmhouse which was midway between where we are now and the 85-foot telescope, and that's where the offices were and the shops, and everything. There was no cafeteria. That was actually a convenient location because from that house, where there is now just a grove of trees, by the way, if you had a loud enough voice you could shout to the 85-foot telescope, and that's how Beaty Sheets told us we were wanted on the phone. She was the only one that could shout that loudly. That's how communications were done in the 1960's.

In any case, I mentioned to Struve that "Gee, this telescope could detect reasonable signals coming from some nearby solar-type stars, and maybe we ought to look, because for all we know, practically every star in the sky has a civilization that's transmitting." Now he thought that was a great idea, and for those of you who don't know Struve's history, he had in fact believed for many years that many many stars had planetary systems. That was an outgrowth on his work on stellar rotations, which had showed that in the late spectral types the stellar rotations are much lower than in the early types, and he attributed that to the presence of planetary systems as objects which soak up the angular momentum during star formation. So he thought this was a good idea, and without any hesitation said, "Go ahead." In those days you wrote no proposals, and there were no referees, no committees, no anything. That's how things were done. One wonders if maybe that wasn't the better system, because of lot of things happened that never would have happened today. For instance, the eleven meter telescope at Kitt Peak was never studied by a committee. The proposal to build it was added as a short paragraph in the budgetary request of the NRAO one year just because there was sort of half a page left blank. There were no studies. There was a cost estimate based on scaling laws, and that's how the money was granted. I know because I wrote the paragraph. (I'm a great expert on millimeter wavelengths.) Of course that telescope turned out pretty well.

In any case, Struve said, "Go to work," and we proceeded. Now if we recognize that this was a sensational subject, and that we might open the observatory to criticism, and that this young observatory was trying to establish the concept of national observatories as a good thing, then we had to be very careful not to do things which bring criticism. And so, very early on we made two decisions. One was that we would seek no publicity, that we would keep the project completely secret as much as possible; and secondly, we would build an instrument per se, but we would make sure that it was useful for some form of conventional astronomy.

Now just then there was a lot of excitement about the detection of the Zeeman effect in the 21 centimeter line, and so we decided we will build an instrument which will be useable to search for the Zeeman effect. We would need two channels, good frequency stability, narrow bandwidths, all very similar to the SETI requirement; and, in order that the system would be suitable for 21 centimeter Zeeman effect, we would build it and do the search at the 21 centimeter line. That is why the frequency was picked, not for any profound reason like magic frequencies or waterhole or anything else. It was a way to prevent criticism of the observatory, and in a way, kill two birds with one stone.

So the system was designed. About that time Kochu Menon came on board and was interested in observing the Zeeman effect, and so he got involved with the idea of helping build the equipment and then eventually using it to make the Zeeman effect observations.

Another person who was involved was Ross Meadows, who was here as a visitor from England. He built much of the electronics, with the help of the Green Bank electronics people, which at that time I am pretty sure were only two, Warren Wooddell who isn't here anymore, and Dewey Ross. In fact, Dewey spent a great deal of time working on what he thought was going to be the heart of the project, which was an Adler tube amplifier. He spent endless months trying to make that thing work. In fact it never did work right. It wasn't Dewey's fault. It just was not a good device.

It took quite a long time to build all the equipment. We were careful not to spend much money, again, to prevent any criticism. The total budget for the project was about $2,000, and that went essentially to buy the narrow band filters which were necessary. Those were a couple of Kronhite filters. Some of you will remember that name. They were a box of vacuum tubes, with which you could turn two knobs and adjust the lower and upper cutoff of the filter. You could get quite narrow bandwidths with them. They were really quite nice. We could still use them today but they no longer exist. That was about the only special equipment bought for the project; four Kronhite filters. Everything else was just conventional radio astronomy devices which you could use in any experiments and could be used for many other things.

When the receiver was about half built, a crisis developed. The Morrison-Cocconi paper was published. That didn't bother us in the project at all. We thought it was fine because it was a very inspiring paper, it supported the 21-cm choice, and made us feel that maybe we were doing the right thing. However, it upset Otto Struve very much. That was because, as an old-timer, he was well aware that obtaining credit for things and publicity was important to the welfare, financial and otherwise, of an institution and a person. So he got very upset because he thought Morrison and Cocconi were going to get credit for the project which had been going on at Green Bank for six months or so. He seized the opportunity to give a lecture at MIT the next week, and changed his whole lecture to talk about the project at Green Bank. He thereby tried to head off the publicity of past so to speak, and to arrange that Green Bank got credit for what was going on.

This, of course, created problems for us because then the press was all over us. We succeeded in keeping all of them away except John Lear from the Saturday Review, and I will get back to that. It took about eight months to build the equipment. Because of the pressure of publicity, there was pressure to get the project done, to get on to other things. Project Ozma finally went on the air on April 11, 1960. Here is a figure from that time, which gives you an idea of what the scientific thinking was. This supported

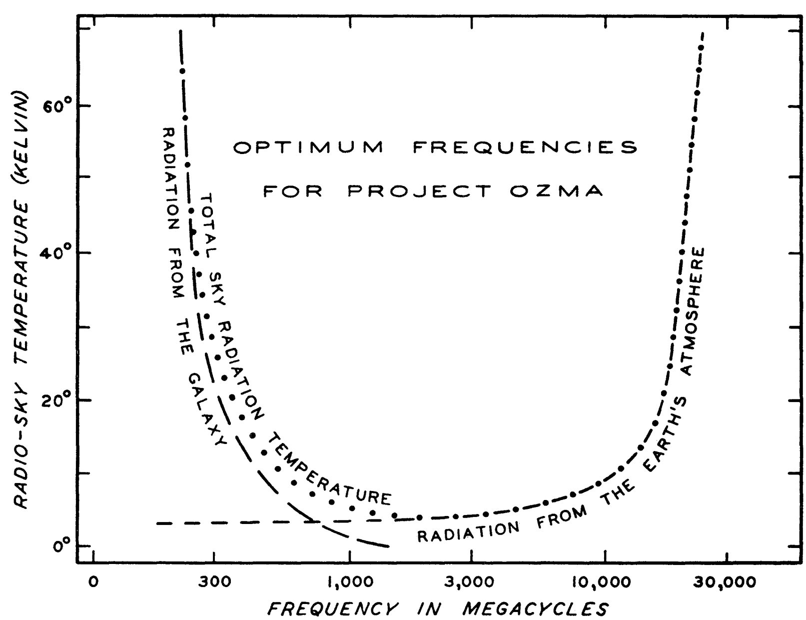

Figure 1.

a choice of frequencies, and it may look similar to the curve that Barney Oliver showed this morning of noise temperatures, and galaxy noise, and how they lead to the water hole as a good place to search. But if you look at this carefully, you will see that in fact we didn't really understand that situation well at all in 1959. We did realize that the lower frequencies were going to be poor candidates because of the galactic radiation, but nobody had thought then of the quantum noise effect which noises up the higher frequencies, and of course we didn't know about the three degree black body radiation. We did know about radiation from the earth's atmosphere and even that, as you can see, was known poorly.

Only many years later did we arrive at the concept of the water hole (Barney pointed out the quantum noise effect). In a way that intellectually legitimized what we had done long before. (The figure is published in a 1960 Sky and Telescope article on Project OZMA.)

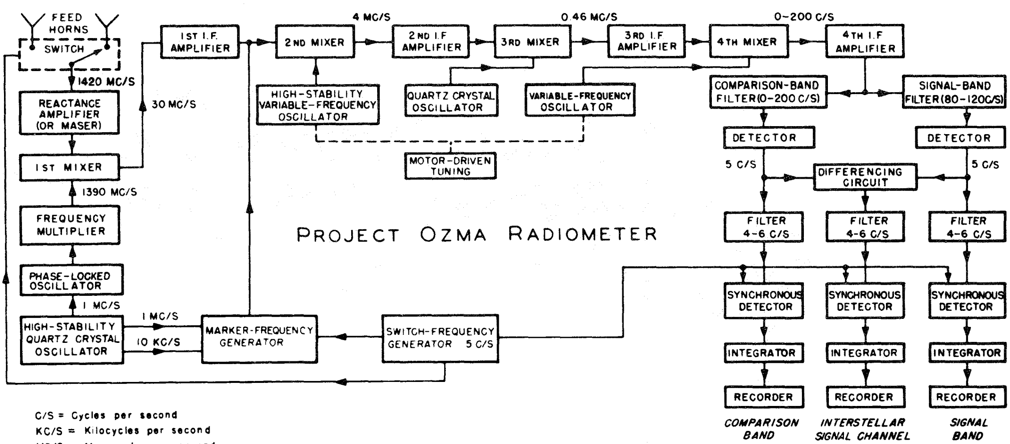

Figure 2.

Here is the block diagram of the Project OZMA receiver, Figure 2. Looking at it now it really was a pretty good scheme. Its core was a reflection of the Harvard background of the people at NRAO in that it was a DC comparison radiometer. Remember those terms. In DC comparison radiometers you had a broad band channel and then a narrow band signal channel and you differenced the two while scanning the narrow band channel. In this way you developed spectra. This was a way of overcoming gain instabilities and noise in the receivers. There was to be a comparison band and a signal band as the basis of the receiver. They were to be differenced and in this way the narrow band signals would be picked out. Only narrow band signals. This would reject continuum sources, that is, conventional cosmic radio sources. Thus the system was designed to reject the radiation of astronomical objects, such as galaxies, supernovae, and so forth.

The one other thing it had, which is of interest, was two feed horns. It used a Dicke switching system with the horns, and so it was a combination Dicke switching receiver and DC comparison radiometer. The idea behind the two feed horns was to reject terrestrial interference. Terrestrial interference would come in equally in each feed horn (which, of course, was a very bad assumption), so if you switched and then differenced, any terrestrial interference that was coming in both feed horns would be differenced out and would not appear. Of course the signal would be coming in one horn and not the other, and it would pass through the system and be detected. So there was a Dicke system with a switch frequency generator, which controlled all this, the controlling synchronous detectors, and at the same time use of the DC comparison system. We also recorded the comparison band, which was typically one kilohertz wide, and a signal band which, in the actual experiment, was 100 hertz wide. The difference, which is where the signal should be, was in the third channel.

The receiver system was a superheterodyne, but with many conversions, and that may look overly complicated. It wasn't, because there was actually one conversion in the usual way with a mixer at the front end up in the focal box of the telescope, and these others, which are all tuned, came in a Hammerlund Superpro receiver, which was the home of all the subsequent IFs and the ability to tune.

What was done was simply to convert in the first mixer from 1420 down to 30 megahertz, amplify, and then feed the Hammerlund Superpro, which was tuned to 30 megahertz. We then tuned the Hammerlund and in that way we tuned the signal frequency. The tuning device was a motor on the Hammerlund Superpro. The actual receiver is here at Green Bank, although the motor tuning device is not on it. The Hammerlund converted down, provided the tuning, and then put out a broad band of frequencies from which we filtered the required bands. That was the system.

In the feed system, the axes of the two feed horns were at right angles to one another. So they were in fact cross-polarized, one with respect to the other. That was necessary in order to fit the second horn in. We did not want to move the main horn from its central position, because the telescope had been optimized for pointing and sidelobes for conventional observations. The second feedhorn had to be added on the side, and the only way it could be fitted in was to cross-polarize it. Of course, this reduced the ability to reject interference. It didn't eliminate it, but it reduced it. In the end it didn't matter because when we did get interference, it was so powerful that even if the horns had been matched, they would have gotten slightly different signals and that would have been enough to leave a very strong interference signal in system.

The front end was crucial to the operation. As I mentioned, we had the Adler amplifier. It never worked, so a slight miracle was called for. The publicity about the project produced some good. We got a call from Dana Atchley, who was the president of Microwave Associates, a very successful company, saying that they had developed an extremely good and stable parametric amplifier. Before that none of them had been stable enough to use in radio astronomy. He claimed it was the best in existence. They had one copy and would like to loan it to us for Project OZMA. That was very good news, and we said, "Sure, please send it to us." He said, "I'll not only send you the amplifier, I'm going to send my chief engineer with it to show you how to make it operate." So very soon, about a week later, the paramp showed up in a remarkable way. A telephone call came to my office saying that the chief engineer of Microwave Associates was down at the desk here and had the parametric amplifier. Of course I expected to meet a very distinguished engineer. When I got downstairs, what I found parked outside the NRAO was a Morgan sports car within which was sitting a fairly rotund gentleman with a red beard, red hair, a tam-o'-shanter, and a bright red plaid shirt. Sitting next to him strapped in the other seat of the Morgan sports car was the parametric amplifier. He had driven all the way from Boston with this thing in the Morgan sports car. By now it was completely out of tune. If any of you have ever been in a Morgan sports car, you know they need springs.

It had an isolator and Dicke switch. There was a directional coupler to squirt calibration signals into the system. The first mixer is in the paramp; the old-timers will recognize the typical IF of those days. This equipment went up in the front end of the telescope and became the core of the project.

The paramp did indeed work. It produced a system noise temperature of about 350 degrees and, once tuned up, was stable for a few hours. It would then slowly drift out of tune because of temperature changes and such. We all had to become experts at tuning it. The rest was straightforward. But the paramp was a work of art which needed an artist to make it function. And so the first week when the chief engineer of Microwave Associates was here, we spent the time trying to learn how to tune up. When we started, he was the only one in the world who knew how to do it. By the end of the first week there were two of us, him and I, and midway through the Project, the telescope operators could do it too.

It wasn't easy. It had four micrometer screws on it. These adjust the dimensions of some cavities which are in it, and all these cavities are interacting. There was an idler cavity, a signal cavity, a mixer cavity, and an L.O. cavity. They were all coupled together. To tune it, you had to twist these micrometer screws in sequence, one after the other. Every time you changed one it affected the tuning of another. The first time you did it, even under supervision, it would take at least an hour, and at best, it would take ten minutes or so after you really learned the thing and had a feel for which twist did what. The paramp was very important but also very time consuming. Each observing day started about four o'clock in the morning because that's when Tau Ceti rose at Green Bank. The first thing that happened was somebody, beginning with me, would have to climb up in the feedbox and sit there, very cold, and twiddle those knobs for about an hour, talking to somebody in the control room, until we had the paramp operating. Then you would tighten down those micrometer screws, come down and observe, and perhaps two or three hours later you would have to go up and adjust them again. When people ask me about Project OZMA, that's always what I remember.

Down below in the control room was the rest of the system, which was straightforward. In April 1960 the system went into operation and started observing our two target stars which were Tau Ceti and Epsilon Eridani which are both about ten light-years from the earth. They are solar type stars and both, as best we knew it then, single stars in the sky.

Every morning after tuning the paramp, we would start observing by turning on the motordrive of the Hammerlund receiver. The procedure was to tune one channel bandwidth every hundred seconds, with the strip chart recorder running all the time. The bandwidth was 100 hertz, so we scanned 100 hertz every hundred seconds. We ran about a week when the paramp failed, and we had to take it down. Kochu Menon made Zeeman experiments with the equipment. We started it up again in June and ran for about thirty days then, generally running about ten or twelve hours a day. In the end there was a total observing of about 200 hours, all of it on two stars.

All the data were taken on strip chart recordings, thousands of feet of strip chart recordings. Everyday we had to go through all these charts and look for anomalous events--pulse trains and all the things we heard about this morning. Of course it was then all analyzed by human eyes. This got to be relentlessly boring and led in time to the development of the first digital systems in astronomy, so maybe Project OZMA produced something.

That's an interesting history, which I think nobody's heard. The old-timers here will remember that, just about this time, "we really should have some way of recording data digitally." At the time this was considered a really far out and rebellious way to proceed. Everybody knew astronomers read charts.

The first digital data-taking device at the NRAO was a digital volt meter which came on the market at that time. It had shiny Nixie tubes, which was really spectacular because these numbers would light up on the Nixie tubes when you pressed a button. All we did was connect an RC circuit to the output of the receiver and connected the digital volt meter to it and the telescope operators sat there and at the appropriate times pressed the button on the digital volt meter and wrote down the numbers that appeared on the Nixies. That was the first introduction of digital data taking..

Well, the astronomers loved this. They got these columns of numbers. We decided we had to do better than that. Very soon there was a Mark II version which used a voltage-frequency variable oscillator. The output frequency was proportional to the voltage. It was attached to a counter. So the output of the radiometer would cause the frequency to change and the count of the total number of cycles over a certain period became the output of the receiver.

We improved the system by attaching an actual printer, which still had to be activated by a button. You didn't have to write the numbers down, but you still had to push buttons. The dramatic breakthrough that took place then was that Gart Westerhout came and recognized that he could make life much more pleasant for himself if he attached a string to the button so that he wouldn't have to stand by the button. He rigged a string across the ceiling of the telescope control room and down from the ceiling right over an easy chair, in which he could sit. He attached a pull to this string and then he sat in the easy chair and whenever he wanted to take a reading he would pull the chain. Pretty soon we were able to augment that system and in fact made it so it punched paper tape and that led to the introduction of, as far as I know, the first computer in radio astronomy which was an IBM 610. Some of you may remember it by plugging wires into a board, and it became the first digital system in astronomy. That was about 1960. Part of it was an outgrowth of the Project OZMA.

Many people were involved, and many of them are still here. I mentioned Kochu's participation, Ross Meadows, Dewey Ross, and a lot of telescope operators, many of whom are still at NRAO. For instance, Fred Crews; Bill Meredith who is now in Charlottesville, and Bob Viers who runs the 300-foot; Omar Bowyer who went on to better things, Troy Henderson still holding the fort here with Dorsaline, George Grove, and Bob Uphoff, who has gone on to become an industrial magnate in the computer world.

For two months, we searched for signals from the Tau Ceti and Epsilon Eridani. The only exciting thing that happened occurred on the very first day. We had observed about six hours on Tau Ceti, seeing nothing. The system worked. We stopped to tune the parametric amplifier, and went on to Epsilon Eridani, pointed on it, turned the system on, and started the frequency scanning. In less than a minute we suddenly heard something remarkable from a loudspeaker connected to the system. We heard a very intense pulsed noise. It wasn't a pulsar. It was pulsing eight times a second, very loud; the recorder needles went off scale. It was all very spectacular; it lasted a few minutes and then disappeared.

When it happened, we were all dumbfounded. Could it be this easy? All you need to do is point to a random star and within one minute you get a signal that puts your receiver into overload? We were so surprised and so unprepared for it, we didn't know what to do. Everybody just looked at each other. I remember my reaction was that something's gone wrong with the equipment, so the first thing I did was run across the room and start checking all the connections and wires and try to figure out why the equipment was doing this. We didn't have the presence of mind to steer the telescope off the source, which of course is what we should have done. Instead we continued tracking the star and everybody was scrambling around trying to figure what piece of electronics had broken in order to make this event happen.

After a few minutes, before we had time to test the signal's origin, the signal disappeared. We were left with this very firm data on the presence of a signal which was very close to what everyone had been predicting, a pulsed signal coming from cosmic space. After that, each day we would go back to Epsilon Eridani and try that frequency again, hoping the signal would come back. After a few days we realized there was a good chance it was really some kind of interference, and so we actually added a completely separate receiver that had just a little horn stuck out the window. Sure enough, about ten days later, the signal came back and we saw it not only on the 85-foot telescope but also with equal intensity coming in the horn. Therefore it was clearly coming in the sidelobes and not from outer space. We have never known what that was, but it had all the earmarks of being an electronics countermeasure system, probably airborne based on the timescale during which we heard it.

In the end, we saw no signals. The total frequency coverage was very small, about 7,200 channels. That meant in 200 hours we had covered 360 kilohertz per star, with two stars, which was a coverage of about 76 kilometers per second in velocity range. We thought that was enough to cover all reasonable velocities. Remember we knew the radial velocity of these stars, and this range took care of possible orbital velocities and such in the systems.

The sensitivity with the 350 K system noise and the 100 Hz bandwidth and the 100 second time constant was about 10^{-22} w m^{-2}. Nowadays you get 10^5 times better sensitivity without much trouble.

What has happened in the 25 years since then? System temperatures have gotten much better still, going from 350 K to 35 K. There is a factor of 10 there. Dish sizes have gone up; the maximum, if we consider Arecibo, is 75 times greater in collecting area than the 85-foot telescope; antenna temperatures for a given signal have gone up a factor of 75. This means the required integration times to achieve the same sensitivities as in Project OZMA have gone from the 100 seconds of Project OZMA to a time required to duplicate Project OZMA at Arecibo of about two-tenths of a millisecond. If you use a system like Paul Horowitz's, you do not do just 7200 channels in a tenth of a millisecond or so, but you do hundreds of thousands so you get much better frequency coverage and you get the same sensitivity in a fraction of a millisecond.

I have said this same thing on occasion in the past and everyone said, "Well, why did you bother? Why didn't you just wait twenty years and you could have done the whole project in a few seconds somewhere?" I think the answer to that, which I think is relevant to all of us today, is that you have to climb a ladder one rung at a time. Nobody allows you to start at the top of the ladder. The fact that we could have SETI systems like the NASA system or Paul's system now depends to some extent on the fact that we have tried it before with lesser equipment and shown you could achieve good performance with lesser systems. It creates the underpinnings, rationality, and the support for building larger systems still. Today, we are doing the eight million channel systems with very high sensitivity and, sure enough, twenty years from now we will have a hundred million channel systems or so. But we only have them because we will have demonstrated that the eight million channel systems are possible and you can observe the sky successfully with such systems. So I don't think it was a waste of time. It laid the groundwork for what came later. What took 200 hours in 1960 now takes a millisecond or less. An interesting question: what will it be like twenty-five years from now?