THE SEARCH FOR EXTRATERRESTRIAL INTELLIGENCE

Proceedings of an NRAO Workshop held at the National Radio Astronomy Observatory Green Bank, West Virginia May 20, 21, 22, 1985

PALOMAR OBSERVATORY PHOTOGRAPH

Honoring the 25th Anniversary of Project OZMA

Edited by K. I. Kellermann and G. A. Seielstad

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PREFACE

1985 marked the Silver Anniversary of Project Ozma, the first search for extraterrestrial intelligent life. Conducted in Green Bank in April and June 1960, shortly after the National Radio Astronomy Observatory had been established, the project was a longshot gamble for a young scientific organization and a young scientist. The gamble paid off; not that it found any extraterrestrials but rather that it opened an entirely new field of scientific investigation. Although many radio searches for extraterrestrial intelligence have been made since 1960, each with increasing sensitivity and sophistication, the biggest step was the one made by Project Ozma. Frank Drake's experiment was unique; it was the first!

To commemorate this occasion, a Workshop was held in Green Bank from May 20 to 22, 1985. The field is still young enough that many of the original participants in Project Ozma could attend. Their presence and the fact that the workshop was held during the same season as Project Ozma 25 years earlier, coupled with the timelessness of Green Bank and the looming presence of the Tatel Telescope, all helped to create the proper atmosphere for the gathering.

One purpose of the Workshop was to document the history of the original investigation, not in a dry archival sense only, but in a way that captured the creative, human dimension of the undertaking. The appropriate talks have been organized under the heading "Historical Perspective." Programs that are underway or about to become so have been collected under "Current Programs." The health of the field is documented by the number of talks identified as "Search Strategies." Clearly the number of ideas exceeds the resources of investigators to carry them out immediately. Talks collected under "A Glimpse of the Future" show that human ingenuity is unlikely ever to be exhausted. In a field such as SETI, this characteristic will remain an essential; it is good to know it is available in such abundant supply.

We attempted to retain the informality and vitality of the oral presentations by providing, in essence, written transcriptions lightly edited by the speakers, however, to achieve some conformity with the rules of written English. The discussions following each talk and the summary Panel Discussion likewise attempt to record the spontaneity of informal exchanges among participants. The editors took the liberty of rearranging the order of presentation of the talks. Occasionally, then, reference will be made to a "previous speaker" whose contribution has not yet appeared in the written Proceedings. We ask the reader's forbearance.

The occasion of the Workshop also honored another SETI pioneer, Sebastian von Hoerner, upon his retirement from NRAO and return to his native Germany. Sebastian and his family will be sorely missed by his friends in Green Bank. We hope they return often. The paper he presented at the Workshop justifies by itself the publication of these Proceedings. Sebastian's contributions to astronomy and the respect of his friends and colleagues have been eloquently described by John Kraus, another pioneer of SETI and radio astronomy, as follows:

Astronomer, engineer, philosopher, scientist, cosmologist, Sebastian von Hoerner's interests are diverse and his insights run deep. From homologous antennas to galactic cultures, from an expanding universe to gravitational collapse, his thoughts encompass the infinite and address the ultimate question. Sebastian von Hoerner is our friend, our co-explorer and our Elder Statesman of the Cosmos.

Shortly before the Workshop, we learned of the death of Joseph Shklovsky, who had been expected to attend. Shklovsky was one of the first scientists to consider the question of extraterrestrial intelligence. His impact on astronomy and SETI was broad and far reaching. His contributions are so numerous and enduring that his influence was evident at the Workshop. But the investigations he supported and encouraged will have to be carried out by his scientific successors. We hope these Proceedings demonstrate a commitment to accept this inherited responsibility and to pass it on to succeeding generations. Few fields of human endeavor are likely to require as enduring a commitment as SETI.

Carl Sagan, who was unable to participate in the Workshop, prepared a short statement in memory of Joseph Shklovsky which was signed by all of the conference participants and sent to his widow and associates in Moscow. We dedicate these Proceedings of the Green Bank SETI Workshop to the memory of our friend and colleague, Joseph Shklovsky.

No workshop is successful without the contributions of many individuals. We would like particularly to thank Mrs. Beaty Sheets for her support during the conference and in preparing this volume for publication, Wally Oref for his audio-visual assistance, Mrs. Becky Warner and many others for arrangements, and Associated Universities, Inc. for financial support. Jill Tarter provided the cover illustration.

May the Golden Anniversary of Project Ozma, to be held in Green Bank in 2010, be as stimulating and successful. Perhaps, if we are lucky, that event will concentrate on preparation of an appropriate return message.

Ken Kellermann and George Seielstad
Green Bank, West Virginia
March, 1986

In memory of

I. S. SHKLOVSKII

As astronomers celebrating the twenty-fifth anniversary of the first radio search for extraterrestrial intelligence, we mourn the passing of I. S. Shklovskii, one of the pioneers in this promising but still unfulfilled subject. We send our deep sympathies to his family, friends, and colleagues. Through his writings, his students, and his influence on the world scientific community, this brilliant and courageous man lives on.

Green Bank. West Virginia
May 22, 1985

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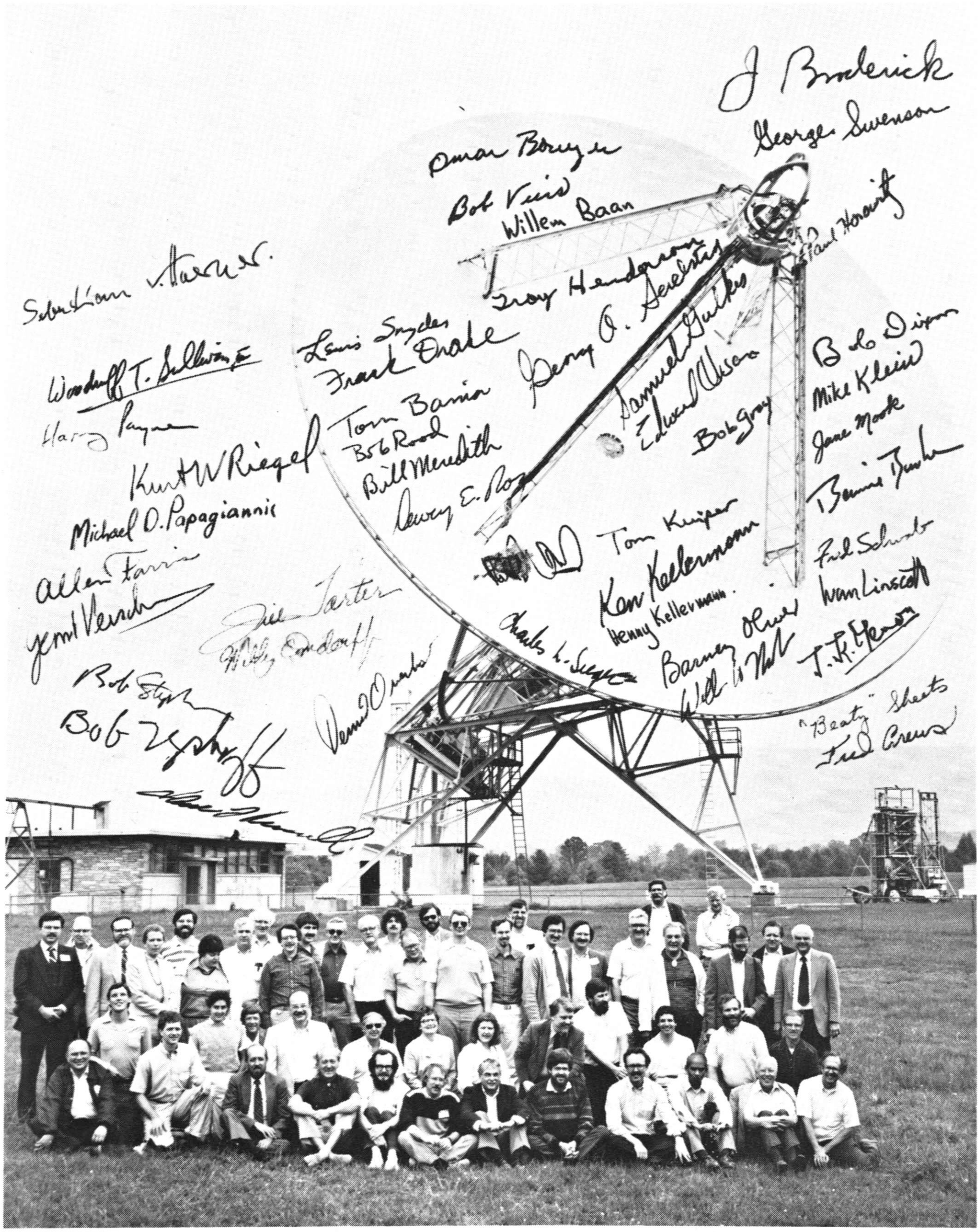

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I.

KEYNOTE ADDRESS

LIFE IN SPACE AND HUMANITY ON EARTH

Sebastian von Hoerner
National Radio Astronomy Observatory*

During longer flights it may be nice to have a talk with your neighbor, and one of the standard questions is: "What kind of job do you have, what do you do for a living?" So I say, "Well, I'm an astronomer, I work at an observatory". And in about 1/4 of the cases the reply is, "Oh, that's interesting. So you make horoscopes!" And this comes from engineers, medical doctors, all sorts of people.

In some of the other 3/4 of the cases there isn't much interest, but in some cases there is a serious interest with many questions, and so we talk for a while about stars and galaxies. And it doesn't take long until the question comes: "Do you think that there is life way out in space?" Then I describe the various ways we think about it. I explain SETI, the Search for Extraterrestrial Intelligence, and I tell that I had the good luck to be nearby when Frank Drake did his "Project Ozma", the first search for radio signals from other sun-like stars, in 1960 at Green Bank. I also may talk about telescopes and receivers, and some more about astronomy in general. And then normally there is a pause, and I see how it works in the fellow to find a polite way of asking: "What the heck is all that good for, what do we get out of it?"

And behind that question is the feeling, expressed or not, that we have a great deal of more serious problems, that humanity on Earth is truly in bad shape, and that <u>these</u> are the problems we should worry about. And not about the stars and other life in space, things which are extremely remote, and not helpful to any of our urgent problems. So what should one answer? I think it is a good and true question, and it deserves a serious honest answer.

There are some quick answers, for example, that curiosity is just part of mankind and that we couldn't give up curiosity and still stay human. All intelligent animals are curious: just look at the difference between a cat and a cow and, given the choice, I'd rather be a cat.

On a different level, I would answer that progress is mostly counted in terms of gadgets, but that in the long run it is <u>ideas</u> that matter, not gadgets. And some of the most important ideas for mankind came from astronomy. Astronomy really was the first science, for which I even found an explanation years ago: astronomy became the first exact science because the sky has no friction. No, don't laugh, I mean that seriously. Think about it for a while!

As any other field of work, astronomy yields direct results, facts and theories; but also some more indirect results, regarding things I will call "background", and some I like to call "fringe benefits" (I don't have a better word). For example, in astronomy as a whole, the direct results are data about our solar system, about other stars, that we live in a galaxy, that there are many other galaxies. And all that is very interesting data.

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But to my own (private) feeling, the most important result from astronomy is the general background, not the single data. It is the fact, awe-inspiring as well as pleasing and certainly not trivial, that as far out as we can look into space, and as far back into the past, we always see the same atoms and molecules, obeying the same laws of physics and chemistry as we have here and now on Earth. What I mean is this background feeling which it gives us: The universe is a cosmos, a thing of beauty, of order, and not just some chaos. This is the main result for me.

Also, we have just started to move out into space ourselves, we had some people on the moon. And again, to my feeling the most important things they brought back are not the rocks and dust which now are investigated. I think much more important are the photos of Earth taken from a distance, although they are only fringe benefits of the projects, not the main purpose. These pictures should hang in all offices of politicians and men of power. They should show us what a nice beautiful planet we live on, and how easily this beauty can be destroyed. We should consider our Earth with more thanks, and treat her with more care.

So much about astronomy in general. Now we come to life in space and to SETI, and again we may have direct results and fringe benefits. First, the direct ones. I seriously think that trying to take up contact with other beings in the universe is the next great task of mankind. And if we ever have success, this will be a tremendous impact, comparable only with our development of language about a million years ago. To explain this statement I would like to remind you that the decisive milestones of our evolution on Earth have all been set by new methods of data handling. All life, self-reproducing life, is based on the genetic code; this is an extremely ingenious way to store the information how to reproduce, multiply and store this very information, including the information how to build up a complicated organism. Later on, higher life needed a brain, which we may call a central switchboard, for turning incoming information into proper outgoing action. Then I think it is fair to say that all our human culture is based on language, that the development of speech was the turning point in our history, from apes to humans. And script, the storing of information, was another great step, too.

We may be now at a new turning point, without knowing where it will lead us. It is called cybernetics, or artificial intelligence. What we have so far in terms of computers and programs, great as that is, it is only a tiny part of what actually is possible, only a timid beginning of what may be done in the future. Thus, if we think along this line: From genetic code, to brain, to language, to cybernetics, then I feel that it is very natural to think that the next big step is interstellar communication. Trying to make contact with other beings, if there are any. The next milestone then is Ron Bracewell's "Galactic Club." And our puzzling question is whether there actually is such a club, and if so, how could we enter.

Today I will not talk about all the many technical (and moral) difficulties and objections involved. But I think we all could agree that if SETI ever had any success it would be a tremendous impact, overwhelming, maybe frightening. (I don't mean the short-lived headlines, but a period of a few hundred years, say.) Mutual talk creates common culture. And keep in mind: the sun is not an old star, most stars are twice as old, and so will be their civilizations. Meeting the Galactic Club would mean the end of our own culture, which would gradually merge into the much older galactic one.

Our present anniversary at Green Bank means that SETI is now 25 years old. It has steadily gained interest and respectability (even a bit of money). But so far, there has been no success at all. Is this a reason for disappointment? Certainly not. SETI is an astronomical project, with astronomical distances, and it may take astronomical times as well. Maybe this is one of the test-requirements for entering the Galactic Club: the ability to think and to plan in terms of astronomical times (not just our four years between elections).

Could we make an estimate about the time needed? I cannot make one for us as late-comers to an existing club (who may shorten the time by sending strong signals to us, or lengthen the time by deciding to leave us alone). But I can give a nice estimate for the minimum time t, during which a civilization must transmit signals and listen for answers until they have a good chance of success, if they assume to be among the first ones searching for contact. Omitting technical details, I only want to mention that the longer the search time t, the more numerous are the searchers at any time, thus the shorter is the distance between them. Demanding that the search time is just twice this distance divided by the speed of light (signal plus answer), we derive t = 5000 years. The worst uncertainty entering this estimate is the fraction p of all stars which have planets where searchers ever have tried, but fortunately this uncertainty enters only with the power 1/4. I have used a fairly optimistic but not impossible value of $p = 0.005$, half a percent. Since p cannot be much larger, the 5000 years are considered a good lower value. And maybe not very low either, since increasing t by a factor 10 needs decreasing p by a factor 10,000. But in any case, t is the minimum time assuming a perfect search mode.

The good thing is that anybody else in the Galaxy can make the same estimate with the same answer. The bad thing is that the Galactic Club may have set these 5000 years as the waiting time between application and grant of entrance, just because everybody will get this same answer, knowing that it took that long for the original founders of the Club. Good is also that 5000 years are very short for an astronomical time, but bad is how long they are as compared to human times. In any case, no success after 25 years is no reason for disappointment.

I also want to say that if we make a dedicated search, for a long time and with sufficient equipment, but still don't get an answer, or if from some theory it follows that there never will be an answer because nobody else can exist, even this negative result is just as dramatic as a positive one, if you really think about it.

So much about the direct results, and now I want to talk about what I called the fringe benefits. I mentioned already the photos of Earth taken from far away, and how they should influence our thoughts. And now, on a larger scale and with broader application, I think that meditating about life in space may give us a proper distance and better perspective, to look back to Earth and our human activity there.

With regard to our serious problems we have on Earth, I think it may well be the most important thing to turn people's mind out to the stars and to the question of life in space. We are so deep in old ruts here, in ruts of thought and tradition that are tens of thousands of years old, most behavior even got bred during hundreds of thousands of years and was developed for circumstances of the past: small groups in wide surroundings, with lots of natural resources, short life span, small weapons, little organization and power. All this has drastically changed, by extreme and truly dangerous amounts, but we still keep going in the old ruts. We must get out of these ruts, must start to think in different new terms, must find new ways in better directions. Maybe the best we can do is: stand still for a moment, lift our view up from the rutted ground, and look up to the stars for better orientation, and think about life in other places. And then, with our mind still "out there", turn our view back to Earth with a fresh look, to see what must be done.

Meditating about life in space leads to intriguing questions. For example, "Where is Everybody?", first asked by Fermi, 1950 at Los Alamos. Since our Galaxy has 200,000 million stars, 1000 million of which are very similar to our sun but much older, should we not see lots of signs of highly advanced civilizations? Why don't we? Which makes you think about possible dangers to life and intelligence. Do only very aggressive species get dominant on a planet? Do technical civilizations always kill themselves the very moment their weapons have enough power? Does medical progress lead to serious biological degeneration?

Another healthy thing would be to make our political and military leaders meditate: What will happen if the Little Green Men land on Earth? They sure will be very curious what to find here, and now let our leaders think about it, how to explain our present state on Earth to our visitors from space! They may find this extremely difficult.

If we had gone to some other planet, coming down in our flying saucer, our most important question would be: "Is there intelligent life on that planet?" And this again is the Little Green Men's great question when they come here: "IS THERE INTELLIGENT LIFE ON EARTH?" Actually, it was Frank Drake who asked that question first, with a big and doubtful question mark behind it. We pride ourselves to be rational intelligent beings, we even call ourselves the crown of creation, but a lot of strong evidence speaks against it.

The worst example is our present arms race. Let me make the general statement: The largest organized human efforts so far have always been self-destructive. The only exception I can think of are the Egyptian pyramids, any other really large project, large as a part of the gross national product, was military. Which is no sign of intelligence. And our arms race has acquired a life of its own, beyond rational control, it has become completely senseless even in the military sense.

In 1983, Physics Today had several articles about it, with good numerical estimates, showing that the United States had a destructive power of 4000 Megatons while Russia had 6000, which adds up to a total of 10,000 Megatons. On the other side, it had been estimated how much destructive power is needed for a second strike capability, or Mutual Assured Destruction (MAD), which means that each country, after having been fully attacked by the enemy, still can strike back and successfully destroy this enemy. And that estimate gave 400 Megatons. So this says that in 1983 the United States had 10 times the actually needed power, Russia 15 times. Yet we were tremendously worried that they had more than we did. Now both sides are still increasing and improving their arsenals, and all our great efforts go to the military. (And hopefully down the drain, never used.) By the way, this total destructive power is about three tons of dynamite for every living person on Earth, babies, grandmothers, black yellow and white. And, for comparison: all of the shells, missiles and bombs (including Hiroshima and Nagasaki) used in World War II had a total of only 3 Megatons.

A statement, worth remembering, I saw in these 1983 publications: "The USA now devotes over 200 billion dollars per year to military defense against foreign enemies, but 45% of the Americans are afraid to go out alone at night within one mile of their homes." (Try to explain this to Little Green Men!) More recently, a terrible result has been discovered, called "Nuclear Winter", a very cold and dark month-long period after a nuclear war, which may endanger most life on Earth.

I have seen the movie "The Day After", and I got extremely upset. This was supposed to show the day after a nuclear war between America and Russia. And it was so unbelievably weak and meaningless. My wife and I have been (1945) in the air raid on Dresden, and, believe me, on the day after this one we have seen pictures much more gruesome than those in that movie. And in Dresden it was only the day after a normal attack without any nuclear bombs. Furthermore, the worst is not the day after a nuclear war, it is the year after, when all the food has been eaten up and when the thin skin of our civilization comes peeling off in large chunks. But the most saddening is left for the generation after, when our culture breaks down, and large mutations occur. How could we ever explain to Little Green Men: That people, intelligent enough to design nuclear bombs, can be stupid enough to build them?

The arms race is not the only evidence against intelligent life on Earth. Just look at our standard of living: 1/4 of the human population is dying from overfeeding, 1/2 lives in a rather poor state, and 1/4 is dying from starvation. And look at all this in connection with unemployment and population explosion (70 million more people each year).

And then consider our attitude toward abortion and contraception. There is a huge outcry against abortion because human life is sacred and cannot be destroyed, and one leading religion goes a step further and even forbids contraception. But most of these dedicated fighters for the human life have nothing against sending their soldiers to war. Just think about it and try to fit those two things together: objecting against abortion, and training grown-up men to kill other men and get killed.

A nice remark, regarding intelligence on Earth, was made by Konrad Lorenz, the famous old biologist, when he was young. There was one of these serious discussions about the "missing link" (between apes and humans, in our evolution). Suddenly Lorenz called out: "The missing link? That's us!" Well, it seems he is right, we haven't quite made it to the true humans, we are still stuck in between.

Meditating about SETI can also lead to other interesting thoughts. For example: Is intelligence really an asset, or just some freak of nature? On the one side, intelligence opens, it even creates, a tremendously large and manyfold biological niche. On the other side, one might say that the real successful animals are not intelligent, they just lay a million eggs. Who is in danger of extinction? The big intelligent ones are, the elephants, tigers and foxes; certainly not the dumb little ones with their million eggs, in spite of our efforts to extinguish some of them! And Homo sapiens, the crown of intelligence, is in danger of self-extinction! Funny, how much intelligence it takes to become so stupid. Well, maybe intelligence, technical intelligence, acts as kind of a filter: letting only those technical species pass into the future who have developed wisdom as well.

Another hopefully fruitful fringe benefit is thinking in parallel the two thoughts: (1) What might be the activities of further advanced civilizations? (2) What would or should we do in the future? These two questions, if thought and answered in parallel, may on the one side lead us to develop reasonable search strategies for SETI, suggesting probable types of evidence to look for. On the other side, they may lead us to make proper plans for our own next century or two.

So far, however, they have led us into a dilemma. What we soon and first should start doing is: stop the arms race (freeze all, or destroy a lot, but keep MAD or twice it); but then the tremendous quantity of people, skill and money, taken from weapons production and development, must get redirected to other tasks; if possible to similar ones in technical nature and size, reasonable, and hopefully with future profit. And I think that the exploration and exploitation of our solar system offers itself as an exciting new task: building colonies on Moon and Mars, mining the asteroids, having huge factories in space using these large resources and lots of solar power, with orbiting self-sufficient colonies living in their "mobile homes" for generations. Some of those, after a Declaration of Independence, may leave for good and travel to other stars, which may take many generations but would not greatly change their life-style. They may stop at some nice planet, settle and multiply, and send off further mobile homes. All this, and many of its details, has been already worked out by scientists as Gerald O'Neill, E. M. Jones, M. Hart, M. D. Papagiannis, and many others. The result is that the whole Galaxy, from one end to the other and with every nice planet in it, can be colonized by us within about ten million years. Now, if we will or at least could do all this, and if we were typical, then at least some of the many much older civilizations should have done this very long ago (of which we see no evidence), and we should be the descendants of early settlers which we certainly are not. Thus: If we were typical, we should not exist.

Let me give a short SUMMARY and CONCLUSION. If SETI ever has success, it will be a tremendous impact, a change of culture. After a long dedicated search, even no success is a highly dramatic answer. Thus: try to get good ideas and some money and keep searching! But be patient, it may well take a long time.

Maybe most important for our present sad state on Earth is what I called the fringe benefits: Meditating about life in space may give us the needed distance and better perspective to look at our own problems. Thus:

make SETI more public, don't be shy or hesitant. Talk about it, use television and radio, write books, public articles. Make people <u>think</u> about it!

Let me finish with a little poem, having some relevance to our question "Where is Everybody?" (if you don't object to letting poetry include a limerick):

With a flying saucer Type B
Came a man from star cluster M3.
But when he saw with distress
Our terrestrial mess,
He went back and said: "No, not for me!"

<u>K. Kellermann:</u> Thank you very much, Sebastian, that was a lot of food for thought. You seem to think that it would be difficult to explain our society to an ET. I wonder if that is really true. What really worries me is whether it is just a terrestrial phenomena of our society, or is it a universal phenomena?

If it is really universal, there there are no ETI's. They have all killed each other. They have blown up their planet. So let's hope that we're not too universal!

<u>B. Burke:</u> I just wanted to call up your philosophical point by a question. You are distressed by trying to tell whether we have intelligence here on Earth. The terrestrial mess actually was generated by the best and the brightest. Don't blame it on the politicians; there was a letter written by Einstein and Compton and dictated by Szilard, and built by a constellation of our predecessors. What would be your reaction to this compounding of the question?

Well, I would say, unfortunately, you are right. It isn't to be blamed on the politicians only. The scientists have done all the actual work.

<u>K. Kellermann:</u> The politicians would not be able to build nuclear weapons without the scientists and engineers.

<u>C. Seeger:</u> I'd just like to introduce a totally opposite point of view. Mankind has used whatever knowledge came to hand for whatever purposes he had in mind. That goes for societies, too. Knowledge is something that is a result of our genetics, our curiosity; it is unavoidable. It increases as long as we exist. What mankind does with it has to do with his irrational, that is, non-scientific, aptitudes for his fellow-man and society. I don't think there was any sense, when you look at it in retrospect, to the self-grieving which so many scientists went through that they had invented and constructed a bomb. Those who lived through those times knew the motivation of it; they may repeat in the future. I hope not! The main fact of the matter is society in one form or another, in this degree of organization reaches motivations and decisions about solutions to problems that they envision, and acts on them, using whatever is at hand to do it. This whole business of pinning down responsibilities of scientists versus politicians is completely beside the point--this is a human problem, and if we spend our time trying to isolate the blame, we're going to be in trouble.

Yes, the arms race problem concerns all mankind, I agree. I think part of this problem is that we have been bred for being aggressive, for trying to be on top of some enemy, and it is very difficult to do something about it.

Voice: I'm not arguing that.

Our whole tradition. The way we have been taught at school is full of hero worship and being proud of wars which we have won, and so on. Unfortunately, we have now reached a state where we cannot continue this way. We must replace tradition by reason!

C. Seeger: I agree. I was replying to my colleague across the way here.

Voice: I think that actually both sides of this question are again addressed by the point that Sebastian made, and which I think should be stressed. We must be open to the public, and to our very own colleagues. Even within the discipline of astronomy, I would say probably less than a quarter have that perspective. Most people are simply too busy worrying about binary stars or planetary nebulae to actually think of the implications of a universe on our society here on Earth. So I would say that that was the single most important point that you made. That's really what SETI is all about; getting that perspective out into the rest of our community, into the scientific community, and into society.

Yes, and it is something which we all could work for right now, while the direct success of SETI, as I think most of us would agree, might come only after a very long time.

Voice: I think I would agree. With a cosmic perspective, with a different type of perspective, your problems which you identify may be much different than if you have a much narrower perspective.

C. Seeger: I think it would help if we recognize that the definition of intelligence is essentially what we are. Of any other intelligence we know nothing! What we have been arguing about, is various points of view about what is intelligence in terms of motivation. I couldn't agree more with some of the sentiments here. We have visions, and have had them for thousands of years, of a better way for man to treat man. (Excuse the sexism of that remark.)

Voice: I think we enter some of the same problems with artificial intelligence. We are not very successful at it yet, and the problems are with some of our assumptions. Assuming that competition is worse than cooperation in all instances, we are making certain assumptions that aren't necessarily always true. For example, the medical people that did medical work in India, Africa and South America did not perceive the population explosion problem they generated; they were not working on world-perfect knowledge. They might have thought they were working on world-perfect knowledge, but when you start trying to build AI systems that are self-error correcting and work in a fuzzy world rather than an ideal world, then you start seeing behavior patterns that tend to have competition, such as voting systems and things like this. Perhaps a lot of our competition is inbred, *it was made for a different type of world and might be misapplied. But unless we have perfect knowledge of what is the purpose of intelligence in the cosmos et cetera, we can't really make valued judgements.*

B. Oliver: *I think the very term "artificial intelligence" is such that I can't take it seriously. (Laughter) I think John Pierce says artificial intelligence is real stupidity. (Laughter)*

Voice: *It is like SETI is a reflection of our view of ourselves, I think that the search for automating it just reflects how stupid we are in regards to intelligence.*

Ed Olsen: *I want to make a point that in the last year or two my opinion of what L in the Drake equation is has increased in terms of its magnitude, based very largely on the nuclear winter concept. But here is this indirect fallout again from the NASA planetary exploration program.*

Voice: *Unfunded!*

Ed Olsen: *Sorry about that! It was just some programs and model atmospheres that were set up to do something else and some one said, "Well, why don't we crank in our own planet?" We may have shown, and I hope that it bears up, that we can't even have a limited strike capability. Because if it can be shown conclusively that you can't even use nuclear weapons in a limited manner, I think we will have overcome a further barrier. We will really realize that the concept of a limited nuclear war is totally insane, and that our civilization now has a new lease on life that we did not have before, that L may be greater for us, and therefore there may be other civilizations out there too that have realized the same thing.*

If we behave in a rational way.

G. Verschuur: *The tragedy is that no one has ever shown anything conclusively theoretically.*

Voice: *Right.*

G. Verschuur: *And the only way you can show this conclusively is to do the experiment. (Laughter) I think it is a real tragedy.*

K. Kellermann: *You know, Sebastian, that aside from this spring being the 25th anniversary of Project Ozma, and the 22nd or 23rd anniversary of your arrival in Green Bank, it is also the 40th anniversary of the first nuclear explosion. Phil Morrison, who is a pioneer in both areas, wasn't able to be with us this week because he is involved in some activities in the area in which you were so concerned about.*

T. K. Menon: *Most societies until very recently had never given much thought to the future at all, and religions talked about the future after life had left the body and never about the living. Even now, most western societies at least still think of the future only in an extremely limited manner in the sense that it is almost considered immoral to talk about planning for the future. Politicians and non-politicians have talked about this immoral work of planning for the future. The thinking about the future* *is a recent phenomenon, thinking about the future of humanity has been an extremely recent phenomenon, so we should not despair that we haven't come to any real conclusions as to the consequences of our actions, because its a very new type of activity for human society.*

Voice: We'd better hurry up.

J. Tarter: On good days, I think one of the most wonderful things about SETI is that it's an example, I think the first example of a project which may need astronomical kinds of time, and that our society is willing to contemplate undertaking for reasons of curiosity, basic curiosity. In the past you mentioned the pyramids, and there have been the European cathedrals also which have been built over times that are long compared to the human time scale, but there has always been a single power, either a church or a ruler involved.

Here's something that may take generations and generations, but we are beginning to contemplate doing it.

B. Oliver: It is not sold on that, Jill. It is sold on a shorter term basis. (Laughter)

J. Tarter: On the good days I see it that way, so then we go to NASA and say, "Can we have a little money to do it?"

My question is really is our society mature enough? Or are we premature in trying to solve it?

Let's try and find out!

B. Oliver: Sebastian, I tend to be a little bit more optimistic than you, I think.

Or otherwise you wouldn't have gone to work at Ames. (Laughter).

B. Oliver: I think that if you really think about it, the world has changed enormously over the past four decades and one of the reasons for that is the jet engine. The world has been knit together enormously by the advent of relatively cheap and quick travel.

And communication!

B. Oliver: But this traffic has permitted all the multi national corporations to grow. I know that these are not fashionable in certain circles, but I would say that I think they knit the world together a lot more effectively than treaties do! Boards of Directors hate to declare war on their party's division. (laughter)

C. Seeger: I think that's a very real thing. Also, I think that while our attention has grown of late over the threat of nuclear war, and I can't deny that it exists, I think perhaps the Russian tension has decreased. In a way I'd rather have the Russian generals complacent and ours worried than the other way around. I think, it may have reduced their tension. Also, I think the increase in the number of years in which nothing has happened has *put a little credence to this. Finally, on the nuclear winter, let me say that the nuclear winter would be about 10^{-4} Alvarez asteroids. That was clearly a global death, it did wipe out half of the things on the earth. But although it would be a catastrophe for civilization, and I don't want to test the hypothesis, I don't think that it would wipe out mankind.*

Well, certainly our cultures.

With respect to the second strike capability, it has been estimated how much physical destruction in a country will destroy its integrity and culture. It is not necessary to kill all the people; one-fourth of the people and half the industry is a good estimate.

B. Oliver: Well, I'm sure that disaster is in all our minds, but I think it is even possible to exaggerate it.

Yes, it will give us a second chance.

B. Burke: I just want to reinforce that optimism, that is, I have to say I disagree completely with what Kochu said. I think that there are many evidences in the past of people worrying about more than in the immediate future; it probably started with Plato's Republic, and whether you like Plato's Republic or not ...

T. K. Menon: No, I was talking about society, not people!

B. Burke: Well, I'm going by the written record, I don't know about societies. Societies don't sign their documents "Society". Societies are the people who are in those societies, and if you want societies, the closest I can come to it are structures like the English Common Law, which certainly have to do with rights of property and social responsibility that stretch on into the future, that is, the lawgivers have the same quality of looking toward the future.

C. Seeger: The main lawgivers historically have been the religions.

B. Burke: No, Solon was not religious. Anglo Saxon law did not come from religious considerations.

Voice: The Magna Carta is all about inheritances. Bernie is right. If you read Magna Carta, it's all about inheritances.

B. Burke: No, Roman law was not religious. I just disagree, I think religions have had input into laws, but fundamentally that the lawgivers who are famous in history did not do it for religious purposes.

Voice: I was drawing a distinction between what Kochu said, which applied to whole societies, and what you said, which has to do with a small, very small element of society.

K. Kellermann: Sebastian's talk was supposed to be an introduction to SETI and not the beginning of a discourse on history!

II.

HISTORICAL PERSPECTIVE

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

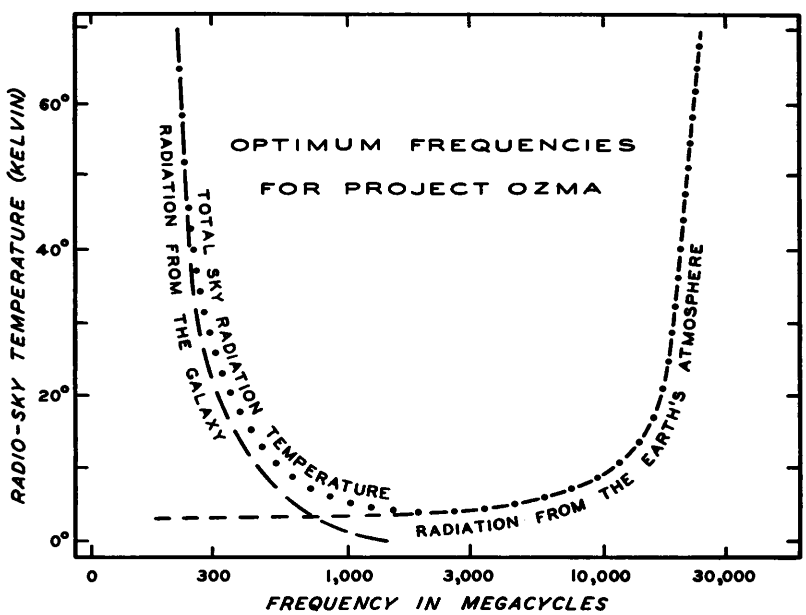

Figure 1.

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.)

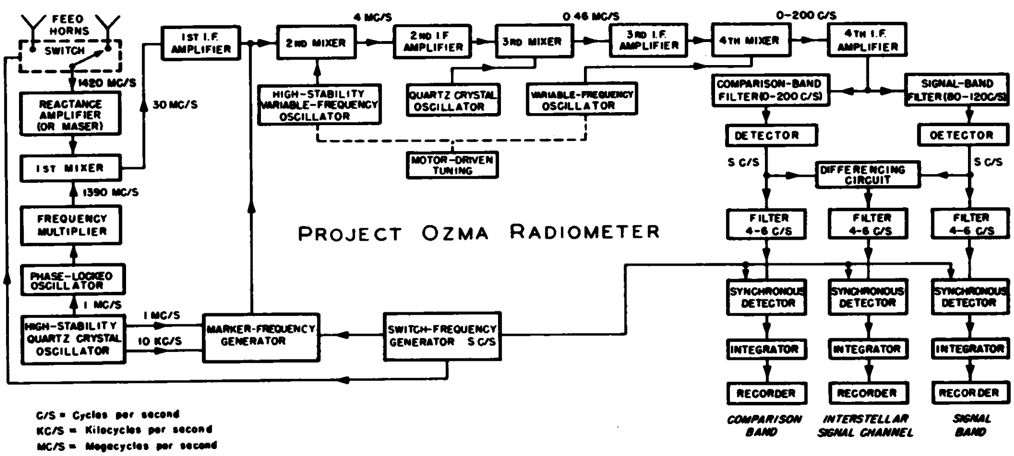

Figure 2.

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?

PROJECT OZMA - HOW IT REALLY WAS

Fred Crews
National Radio Astronomy Observatory

I would like to go back to the beginning of the Observatory some two to two and a half years earlier than the two and a half decades that we have been talking about. I would like to tell things as they were at the 85-foot Tatel telescope, not necessarily how things were at the Observatory. I think that most people have been acknowledged this afternoon with the possible exception of Sid Smith who was and is an engineer here at the Observatory--right here he is. He is the guy who taught me how to wear a hard hat and shortly after that I began bumping my head on steel.

Frank mentioned the scientific offices and laboratories--such as they were. I don't know if you are aware that they were named the Nut Bin by John Findlay--because that was where the nuts stayed. Kochu Menon was here and he stayed there, too.

Bill Meredith was the first telescope operator. He came in 1958, he's here today, and works in Charlottesville. I came on board about three weeks after Bill.

Frank mentioned the 85-1, as we call it, and the fact that it became operational in April of 1959. That was on Friday the 13th, by the way. In those days Dave Heeschen and Bill Meredith as an observing team, and Frank Drake and myself as an observing team went on these rather long competitive observing binges, sometimes for four and five days at a time, working twelve-hour shifts to do the pointing and otherwise evaluate the performance of the telescope. At the end of these observing stands we would go to the Hill House, which was the bachelor quarters, to compete again--playing pool. This was the really tough part. Part of the time Dave Heeschen and Frank Drake teamed up against Bill and me, and I don't want to be too critical of Dave's and Frank's pool playing, but sometimes it was darned hard to lose. On the other hand, Heeschen and Meredith teamed up against Drake and Crews meant that you had to be a super pool shot.

Initials of the observers were commonly used on the logs, and so when they weren't around we called them by the little nicknames that the initials inspired. Dave Heeschen was DSH or dish, CRL for Roger Lynds was charley roger lynds, TKM for T. K. Menon we couldn't do much with, and FDD for Frank Drake was foxy dog dog.

Bill and I sort of found ourselves as go-betweens or explainers between the astronomers and the electricians and the mechanics and the other craft persons who put the telescope together, and sometimes this was nearly impossible. I recall that the telescope control panel was built by the J. W. Fecker Co. of Pittsburgh, a well-known name at that time. One of the dials was the telescope clock. Beside it, the legend "sidereal time" was marked--S I D E R E A L--no problem for us, because that's what Drake and Heeschen said it was, but we could never convince anyone that it shouldn't be two words--side real.

You may have wondered why, when Frank mentioned the names of the people who were the telescope operators, there were so many of them. I kind of wondered about that, too, when I looked at the logs. I think that we can only say that some of us had already worked up to our level of incompetency and had moved part-time to doing some other things.

In trying to put together something for this meeting, I talked at some length to all these people to see what they remembered about the project. At first everyone was dumbfounded that they couldn't remember anything. Some said they were busy being trained and Ozma was just another experiment to them. I have to confess to that a little bit myself because even after two years everything was still so exciting that I don't think anything would have surprised me. As we began to share and discuss the project we began to remember but, unfortunately, almost nobody remembered the same things. Part of this was, of course, due to the fact that if it didn't happen on your shift you might not know about it anyway. But slowly things began to fall in place although there still appears to be some haze.

The telescope log is a book in which we expected the telescope operator to make entries during and at the end of each shift. These are still around; we have never microfilmed anything of this nature. Some of the entries are kind of interesting to read. Some of them were at times unbearable. Here's one. "Shut off 440 volts in dog house to check telescope drift." Another--"Dr. Drake did not show up to observe from 1300 to 1600." June 9th from 1430 to 1600 "set up for visit of Governor Underwood" who at that time was governor of West Virginia. From 1600 to 1645--"A visit from the Honorable Governor Underwood" and in parenthesis--"Republican fool." Arnold Davidson, who wrote that, was a very staunch Republican.

Frank talked about the first day of Project Ozma, and here is where I get into some trouble because Bob Viers remembers it a little differently from Frank, or maybe it's because I misinterpreted Bob Viers. Bob recalls that something happened that afternoon after people had gone home. The last minute flurry of activity was over after the installation of the Ozma equipment, and he and Frank were checking things out. Suddenly there was this voice and that was a long time before "10-4 good buddy." Bob doesn't remember what they heard, but both Frank and Bob moved quickly by reaction to the windows of the building to look out, and when they came to themselves they realized they had been standing there looking at the telescope in awe. It turned out to be terrestrial, of course.

There were two observing runs, as Frank mentioned, one starting on April 11th and ending on April 19th--when that first paramp blew its lid and had to be removed.

The observing was simple; track the source applying telescope and clock corrections; start the receiver to scanning as Frank explained; and about once an hour terminate a scan to correct the telescope position. You watched a voltmeter and chart recorder and, Frank, I don't know how the chart recorder was connected to that block diagram that you showed a while ago. You also listened to the output of the receiver and you ran an audio magnetic tape. If you were to get an audio message like "you have a pick-up at 10th and Main," you weren't supposed to do anything. Something garbled, pulses on the chart recorder, or something similar, was important to look into. As Frank has said, the feed system was a cross polarized on-axis feed and an off-axis feed. The beam separation was something like five and a half minutes of time at the equator. If you got a signal in the main beam the chart recorder was driven to the right. Then you moved the off-axis beam onto the source position and if you now had a chart recorder deflection to the left, you had an event. If you got signals in both beams without moving the telescope or while completely off source, forget it. During the time that I observed, I don't recall a single event that indicated signals from the source position itself. But, in the event that we did get signals of any sort that were not easily explained, we were to repeat all these tests for verification and contact Dr. Drake by yelling out the window toward the Nut Bin.

Interference or signals recorded on the audio magnetic tape was or is today called "blips" by Omar Bowyer and Bill Meredith. I don't know what they are talking about. I talked some to Dr. Drake about it and he's not too sure, I suspect. But when they played the tapes back they heard these "blips" and for some reason you were supposed to count these "blips" over some time frame. I still don't know what that was all about. I don't know whether it was one by land or two by sea or three by air or something like that. Maybe we can sort that out later.

We started observing again on the second of June and observed on every succeeding day until the end of June.

At that time the telephone system was still manual control--Frank touched on that a little bit--or I should say, manual central office. We didn't have to crank the phones here on the site but the community did. Whenever you wanted to call outside, you just picked up the phone and, no matter what time of the day or night, ultimately a female voice would come on the line and say "Cass," and you knew then that you were getting connected to the outside world. On-site calls did require a strong voice--it was worse than that, you sort of yelled your head off. Most of the time you were better off to get in a car and go see the person you wanted to talk to.

As Frank pointed out, there was a lot of advance publicity on this project. We received a lot of calls from the news services, and at night the only incoming telephone line was connected to the control building. We were under strict orders from Frank not to reveal anything about the project. As far as I know, that was not violated. Toward the end of the last Ozma run, the telescope was shut down for one and a half hours for a press tour. Bill Meredith remembers that Bob Abernathy, the television newscaster, called him aside and said something like, "If you had really heard something, you'd tell me, wouldn't you?" Omar Bowyer remembers Frank Drake being interviewed with these huge television cue cards out in front of him. George Grove still mutters about a bold news photographer who took his photograph complete with hard hat, telephone handset to mouth and ear, with the plug in his back pocket, and the telescope as a backdrop. I can remember being interviewed for radio up in the center of the dish--climbing up there--just for atmosphere.

I remember when Sam Harris showed up with the parametric amplifier and sitting on the back of the seat of his sports car, presented it to Frank Drake.

Finally, Troy Henderson (and this is the last story) recalls when Hein Hvatum, who was on the staff in electronics, introduced the Corvair to Green Bank. I'm sure there are people here who may not know what a Corvair was, or is. During the Ozma observations, Hein comes driving up to the telescope in this new Corvair station wagon. Troy was there; Omar, I think, was there, and possibly George Grove. The observing was going O.K. and everybody goes outside to examine this new General Motors latest adventure. Meanwhile, back in the control room, science was going on. The pen on the chart recorder was banging the stop to the right BANG BANG BANG--three times. Well, after some kicking of the tires, and raising and lowering the engine compartment door because of some squeaks and few other things, the fellows returned inside and, lo and behold, there it was. It was now, however, over. I have often thought about this story since Troy shared it with me a few days ago. You know what happens in the office all the time. Three rings, they hang up, and it's all over.

Let me say in conclusion, that I have seen a lot of experiments here at the Observatory after twenty-six years, and I think that considering the technology and the wherewithal available then that project Ozma was as good as they come and I attribute that to the excellent guidance and leadership of Frank Drake.

EVOLUTION OF OUR THOUGHTS ON THE BEST STRATEGY FOR SETI

Michael D. Papagiannis
Boston University

It is quite evident that our ideas on how to carry out an ambitious technological task are shaped by the existing state of the art. In the second century B.C., for example, Lucian of Samosata wrote about a voyage to the Moon with a ship with very large sails that was blown away from the Earth by cosmic winds. Similarly, in the seventeenth century Kepler in his book "Somnium," probably one of the earliest science fiction stories, imagined a trip to the Moon during a lunar eclipse carried by winged demons along the long shadow of the Earth. In 1865 Jules Verne described a round-trip to the moon using a spaceship shaped like a huge cannon shell that was launched into space by a colossal cannon. This of course reflected the state of the art a century ago when they did not know yet about rocket propulsion. Actually Verne was quite prophetic because he had the moon probe launched from Florida, it carried three passengers, and the splash-down occurred in the Pacific, exactly as it all happened a century later when in 1969 we went for the first time to the Moon.

The first scheme to try to communicate with extra-terrestrial intelligence was put forth by the famous German astronomer and mathematician, Karl Friedrich Gauss, and was again an exaggerated plan of what was already available. Gauss proposed to plant on Earth a colossal forest in the form of an orthogonal triangle to show to the astronomers of other, more advanced worlds, who might be observing the Earth with their powerful telescopes, that our planet is inhabited by intelligent beings who are familiar with the Pythagorean Theorem.

Soon afterwards, science and technology began their accelerated pace toward bigger and greater achievements. In the 1880's Heinrich Hertz discovered radio waves; in 1901 Guglielmo Marconi performed the first transatlantic radio communication; in 1932 Karl Jansky discovered radio astronomy; in 1951 H.I.Ewen and E.M. Purcell detected the first radio line, the line of atomic hydrogen at 21 cm; and in 1957 the first Sputnik was sent into space. Thus when in 1959 Cocconi and Morrison published in "Nature" their historic paper "Searching for Interstellar Communications," we had already entered the modern era of science and technology. In their paper, Cocconi and Morrison urged that a search for radio signals from other civilizations be undertaken in the vicinity of the 21cm hydrogen line, which was the only radio line known at the time. Frank Drake carried out the first radio search in the spring of 1960 (Project Ozma), using the 85 ft. Tatel radio telescope of NRAO, and spent 200 hours searching for radio signals at the hydrogen line from two near-by, sun-like stars, Epsilon Eridani (10.7 l.y.) and Tau Ceti (11.9 l.y.).

The rationale behind the proposal of Cocconi and Morrison and the search of Drake was that since it was practically impossible for us to search throughout the entire radio window of the Earth, the extraterrestrials would oblige (very convenient) by transmitting at a universally known frequency (a "magic frequency," as it is often called) to make contact easy. In 1963 the hydroxyl (OH) lines were discovered at 18 cm, and hundreds more were discovered in the 60's, 70's, and 80's. Some searches were conducted in these new magic frequencies (again following the state of the art), though the hydrogen line (primus inter pares) has continued to dominate the searches. In these past twenty-five years, we have accumulated about 120,000 hours of observations in about fifty different search projects, using major observations in seven advanced countries (USA, USSR, Australia, Canada, France, Germany, and Holland), with steadily improving sensitivity and frequency resolution.

The fact that all these searches, most of which were conducted at magic frequencies, produced no positive results, together with the tremendous progress in microelectronics and computers during the last twenty-five years, made NASA decide to take the leadership in this new field and begin to plan a new and far more comprehensive SETI program that will search with high spectral resolution over a wide frequency range in the microwave window of the Earth (1-10 GHz). The cornerstone of the NASA SETI Program is a revolutionary Multi-Channel Spectrum Analyzer (MCSA) with 8.25 million channels (an improvement of more than 100 times what is now available) and a frequency resolution as high as 1 Hz/channel, which is now being developed for NASA by Stanford University. The whole system, including highly sophisticated signal detection algorithms, is expected to become operational around 1990, and the bi-modal NASA SETI Program, which will consist of the Targeted Search that will emphasize high sensitivity and the Sky Survey that will emphasize sky coverage, will probably take about ten years, which will bring us to the end of the twentieth century.

An interesting philosophical, but also practical question that is often asked is the following: Since technology is advancing so rapidly, why conduct our searches with the primitive means of today and not wait for the more sophisticated devices of tomorrow? This question is indeed valid, because the 1,000 ft Arecibo radio telescope with its modern receivers could do the two hundred hours of Project Ozma in just a fraction of a second, and will become several orders of magnitude better when in five years it will become outfitted with the 8 million channel spectrum analyzer mentioned above. The answer, however, is not so simple because we simply do not know in advance the level of sophistication required to achieve a certain result, and therefore we do not know when our technology is good enough to try. The Wright brothers, for example, managed to fly in 1903 with a very primitive contraption and opened the doors of the new field of aviation; it would have been unfair if they had to wait for the development,say, of the jet engine before they could try to fly. Drake, therefore, was fully justified in trying SETI in 1960, because though unsuccessful, he too opened the doors of an exciting new field. It seems also that science and technology are like ladders that we must climb one rung at a time, starting from the lowest rungs. If we do not start climbing, we will never get anywhere. On the contrary, as we keep climbing the horizons begin to widen, leading to new discoveries and technological achievements,such as the jet engine and the 8 million channel MCSA, with important benefits for many other fields of human endeavor.

During the 1970's SETI went through a major crisis of identity and purpose, such as those that often do occur during adolescence. The climate in the scientific community was generally negative toward SETI, which was often wrapped with the same cold blanket as UFO's. Congress had repeatedly refused SETI funding in NASA's budget, and to top it all there was considerable internal strife within the small SETI community. It started in 1975 with a paper by Michael Hart entitled, "An Explanation for the Absence of Extraterrestrials on Earth." In it he claimed that the absence of any scientifically verifiable evidence of past or current presence of extraterrestrials on Earth, is evidence for the complete absence of advanced technological civilizations in our Galaxy. The argument was based on the concept of interstellar travel and galactic colonization, which many have come to consider as inevitable for advanced civilizations.This absence became known as "The Great Silence" or the "Fermi Paradox," after Enrico Fermi who, at a meeting in Los Alamos in 1950 is said to have asked the famous question, "Where are they?"

There were many heated debates, arguments, and counter-arguments in the late 1970's and early 80's on the inevitability or impossibility of interstellar travel and galactic colonization, which for a while threatened to wreck the entire effort. In the early 1980's, however, a sequence of events pulled SETI out of the doubts and frustrations of adolescence and brought it into the hard work and goal-setting period of young adulthood. The three most important events credited for this transition were the following: 1) The blue ribbon Committee of the NAS/NRC, under the chairmanship of George Field of Harvard, which was setting the goals and priorities for Astronomy and Astrophysics in the decade of the 80's, included SETI in these goals and proposed the allocation of about $20,000,000 for this effort in this decade; 2) Congress finally withdrew its objection and NASA began to fund SETI with about $1,500,000 per year. The bulk of these funds go to the development of the NASA/SETI Program, but they also support several SETI-related projects in universities; 3) The prestigious and generally conservative International Astronomical Union (IAU) endorsed the search for extraterrestrial life as a legitimate astronomical objective, and in 1982 established a new commission -- IAU Commission 51: Search for Extraterrestrial Life -- with Michael Papagiannis as President, and Frank Drake and Nikolai Kardashev as Vice Presidents. This Commission grew rapidly to over 270 members, and held with great success the first IAU Symposium on this subject in Boston on June 18-21, 1984, the Proceedings of which will be published in the fall of 1985.

An important development that emerged during the IAU Symposium was the resolution of our old arguments on the Fermi Paradox and galactic colonization. It was actually the realization that none of us can claim to know how civilizations far more advanced than ours are likely to behave. The conclusion, therefore, which was stated by several of the participants, can be summarized as follows: Debates, useful as they might be in sharpening our understanding of issues, will never answer our questions on the existence of other advanced civilizations in the Galaxy. The answers will come only through comprehensive scientific searches, which must be pursued vigorously. The search strategy, however, ought to be broad enough (it was also called flexible, mixed, multi-path and multi-pronged) to allow, besides the main-stream search projects, exemplified by the NASA SETI Program, the parallel experimental testing of other theoretical possibilities. With such a search strategy, we are likely to keep many more good people active in this young field, and improve our chances for success by not neglecting some of the possible alternatives. In summary, twenty-five years after Project Ozma, SETI stands strong and vigorous, having resolved its internal strifes, having gained the support of the international scientific community, developing ambitious plans for the next 15 years, and having many capable scientists seriously committed to this effort.

What about the future? In the next fifteen years we will be busy with the NASA SETI Program and several other searches with more specialized objectives. By the turn of the century we will have expanded immensely the volume of Search Space, the "Cosmic Haystack" as it is often called, that we will have explored. The unequivocal detection of an intelligent extraterrestrial signal will be the greatest discovery of all time. If after a concerted effort nothing has been found, we will probably stop for a while to take stock, and then we might begin to plan an even more sophisticated search with a far more advanced technology.

In the 21st century we can anticipate major observatories in space and/or on the Moon that will open new horizons. A Large Infra-Red Array (LIRA) in space will probably be able to detect earth-like planets around near-by stars, and even to establish spectroscopically whether these planets have liquid water on them. We will also have expanded our understanding of planetary formation, planetary evolution, and the ability of planets to retain water over cosmic periods to allow the biological evolution to run its long and arduous course to an advanced civilization. With the rate at which science and technology have been advancing in this century, it seems reasonable to assume that in the next century, if we manage to avoid self-destruction from a nuclear holocaust, we will have the answers to many fundamental questions on the origin, evolution, and prevalence of life -- especially intelligent life -- in the Universe. It is conceivable, though I think not very likely, that after long and concerted efforts we might have to admit that we are practically alone among the many billions of stars in our Galaxy. But even such an unexpected result will not constitute a failure on our part, because it will teach us how unique and precious our civilzation is, and how cosmically important it is to preserve it.

Our thoughts on the best strategy for SETI have followed a long growing process, as have our technological means to conduct these searches. It is immensely exciting to have lived in this era, and even more to have been part of this effort. We can look with great anticipation into the future as we learn more about how life and intelligence grace the entire creation. Thank you.

J. Findlay: I am afraid that the march of technology is closing the windows for SETI by using many of the frequency ranges that might be used for interstellar communications. There are now six systems in the sky operating perilously close to the protected bands reserved for radio astronomy.

I sympathize completely with you and Dr. Oliver and I think it is up to us to stand firm and use all of our means and all of our collective institutions to keep the radio window clear for radio astronomical research.

S. von Hoerner: It is possible that a message is being sent from a star so far away that we have not discovered it yet. That is true, but the same argument wil be true in five years and in a million years. So it is not necessary to wait for the best message, it is only necessary to have one message. So we should not wait.

This is why I also said that we never know what technology is adequate to make the first detection.

K. Kellermann: There are other spin-offs too in not waiting. Drake's experiment was unsuccessful by normal standards for experiments, but was very successful for having been the catalyst for the many experiments that followed, which have brought all of us here today.

III.

SEARCH STRATEGIES

THE SEARCH FOR BIOMOLECULES IN SPACE

Lewis E. Snyder
Astronomy Department
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My talk today, "The Search for Biomolecules in Space," concerns an area of research which is opening up - it is not a well defined area but it certainly is an area of interest. It is particularly appropriate that we discuss biomolecules at a meeting honoring both Sebastian von Hoerner and the beginning of SETI. As a guide to begin to discuss this topic, Table 1 gives a list of the 68 interstellar molecular species which have been reported as of this meeting. Individual isotopes and a few unconfirmed species have not been listed. Square brackets mean that further confirmation of the detection is needed. Lists of the transition frequencies have been prepared by Lovas,

Table 1. The 68 Reported Interstellar Molecules Listed in Order by Number of Atoms

2	3	4	5	6	7	8	9	10	11	13
H_2	H_2O	H_2CO	H_2C_2O	$HCONH_2$	HC_5N	$HCOOCH_3$	HC_7N	[CH_3C_5N]	HC_9N	$HC_{11}N$
CH	H_2S	H_2CS	H_2CNH	CH_3CN	$HCOCH_3$	CH_3C_3N	$(CH_3)_2O$			
CH+	HCN	$HCNH^+$	H_2NCN	[CH_3NC]	CH_3C_2H		CH_3CH_2CN			
C_2	HNC	HNCO	HC_3N	CH_3OH	CH_2CHCN		CH_3CH_2OH			
CN	HCO	HNCS	HCOOH	CH_3SH	NH_2CH_3		CH_3C_4H			
CO	HCO^+	$HOCO^+$	C_3H_2							
CS	[HOC^+]	C_3H	C_4H							
NO	HCS^+	C_3N								
NS	[HNO]	C_3O								
OH	N_2H^+	NH_3								
SiO	C_2H									
SiS	OCS									
SO	SO_2									
[HCl]	SiC_2									
[PN]	[NaOH]									
	[H_3^+]									

Snyder, and Johnson (1979), Mann and Williams (1980), and Lovas (1985). Many of the interstellar molecular species have also been found in circumstellar shells. Because of excitation and partition function arguments, it is generally expected that the smaller molecules will be found to predominate in the short millimeter and submillimeter range of the spectrum while the large species will be observed in the longer millimeter and centimeter range. There are exceptions, of course, such as the short millimeter wavelength emission of methanol, dimethyl ether, and methyl formate from dense, high temperature clumps.

Some interesting things to note from Table 1 are:

a) Two small rings, SiC_2 and C_3H_2, have been found (Thaddeus et al. 1984; 1985).
b) Protonated species are starting to appear, such as N_2H+ (protonated nitrogen) and HCNH+ (protonated HCN).
c) The large molecules are important because each complex molecule represents many other species that can't be seen.
d) There are tentative detections of molecules which contain sodium and chlorine–potential tracers of stellar processing.
e) Three molecules contain the amide group: H_2NCN (cyanamide), $HCONH_2$ (formamide), and NH_2CH_3 (methylamine).

These interesting facts are related to some of the most exciting astrophysical and astrochemical reasons for studying molecules in space but the question to be asked at this meeting is, could any of the molecules in Table 1 be classified as biomolecules? We have just heard a lecture from the president of IAU Commission 51, Professor Pagagiannis. Commission 51, also called Bioastronomy or the Search for Extraterrestrial Life, is the fastest growing group in the International Astronomical Union. Among the commission's stated areas of interest is, "the search for, and study of, biologically relevant interstellar molecules". Are there really biologically relevant molecules, i.e. biomolecules, in the interstellar clouds? If so, can they be found or have they already been found? To try to answer these questions, it is necessary to think about the building blocks of biological systems. Deevey (1970) pointed out that if minor elemental constituents can be neglected, an empirical formula for living matter would contain 49.84% hydrogen, 24.92% oxygen, 24.92% carbon, 0.27% nitrogen, 0.03% phosphorus and 0.02% sulfur. Interstellar searches have detected compounds containing all of the above elements including phosphorus, if the tentative detection of PN in Table 1 is verified. Even if interstellar PN is not confirmed, interstellar ionized phosphorus is easily detected via ultraviolet observations. Other authors have advocated sulfur as the fountainhead of life (based partly on descriptions of nucleosynthesis during explosive oxygen-burning in the supernova phase of stellar evolution which produces a high sulfur yield), or drawn "trees of life" with common interstellar atoms and molecules in the roots, RNA and DNA in the trunk, and advanced creatures such as horses and humans in the branches. All of these approaches are useful, but is there any hard evidence that the energy sources available in interstellar (or circumstellar) molecular clouds drive the synthesis of organic compounds into a biological chemistry? Is biomolecular ordering present in these clouds of gas and dust? Hoyle and Wickramasinghe (1984) have argued that the dust grains in interstellar clouds contain bacteria such as E coli. When the clouds condense to form stars and planets, the bacteria survive in comets. Those bacteria caught inside periodic comets are further processed into microorganisms via successive perihelion passages, trapped by the outer cometary crust which acts much like the lid on a pressure cooker. Hoyle and Wickramasinghe (1977) have suggested that a cometary impact on the Earth about 4 billion years ago could have led to the start of terrestrial life and even today the continuing influxes of cometary debris may be responsible for major pandemics, such as influenza, which sweep the planet.

Much of the experimental evidence for bacterial interstellar grains has centered on the comparison of the shapes of interstellar extinction curves with laboratory opacity curves for various preparations of E coli. It is enough to say that these comparisons have been controversial (Moore and Donn 1982), mostly because solid state spectra generally do not have any sharply localized spectral features like the well defined rotational or vibrational-rotational lines found in gas phase spectra. Therefore, it would be extremely useful if the bacterial grain controversy could be formulated in terms of gas phase spectroscopy, where well-defined patterns of spectral lines can uniquely identify interstellar compounds.

What mechanisms conventionally believed to be important for the evolution of biology on the early earth might instead be active in the interstellar clouds? The idea that life arose when self-replicating crystals of clay assembled organic molecules which in turn eventually learned to become self-replicating (see, for example, Cairns-Smith 1985) may be applicable to dense interstellar molecular clouds with clay-like grains. If there are certain molecules which play a central role in the formation of gene materials via this process, perhaps it would be possible to detect them astronomically. Another approach might be to ask if the molecules produced in the Miller-Urey type of experiments (see Dickerson 1978, for example) can be found in space. Sagan and Khare (1979) have advocated the possible astronomical importance of the tarry residues (or "tholins") produced in these types of experiments, but the gas phase components have not been emphasized at all. Of the 20 some molecules produced in quantity in the experiments of Miller and Urey, the fairly small molecules formic acid, glycine, acetic acid, and urea have assigned microwave spectra. Of these, only formic acid has been detected in the galactic clouds. Glycine, shown in Fig. 1, may be impossible to find with present telescopes

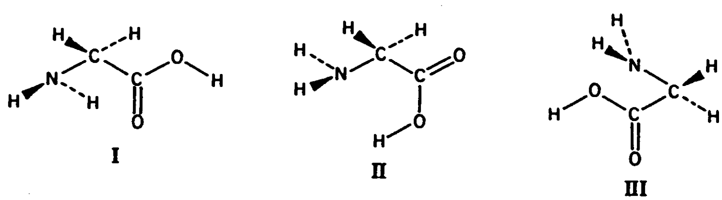


Fig. 1 - The three conformers of glycine (from Schäfer *et al.* 1980).

because of its three conformers (see Hollis *et al.* 1980; Snyder *et al.* 1983, and references therein) but clearly its 10 atom complexity has been surpassed already by several molecules in Table 1. This suggests that interstellar searches for acetic acid and urea could be pursued in order to obtain abundance comparisons with formic acid and thereby perhaps provide some insight about whether chemical processes analogous to Miller-Urey cycles are ongoing in the interstellar clouds. The problem with acetic acid (CH_3COOH) is that, although it has only 8 atoms, it has an intermediate barrier with respect to internal rotation of the methyl group (see; for example, Krisher and Saegebarth 1971). This barrier divides rotational transitions into A and E states (somewhat like the case for methanol), causes large frequency differences between A and E states, makes microwave frequency assignments more difficult, and causes interstellar excitation predictions to be unreliable. Bandscan observations of Orion A and IRC+10216 have produced negative results for acetic acid (Johansson *et al.* 1984). Interstellar urea

$[(NH_2)_2CO]$ is a molecule which we could hope to detect because, with only 8 atoms, its not very complicated. From an astronomical viewpoint, urea is very similar to the formaldehyde (H_2CO) molecule - it has simple structure with H, C and O atoms, C_{2v} symmetry, and ortho and para states. The interstellar detection is made more difficult by the pair of nitrogen atoms. Their double quadrupole interaction splits a simple rotational transition into multiple lines. Fig. 2 shows 17 hyperfine components resulting from the splitting of the $J_{K-K+} = 1_{10}-1_{01}$ rotational transition. It is quite likely that the

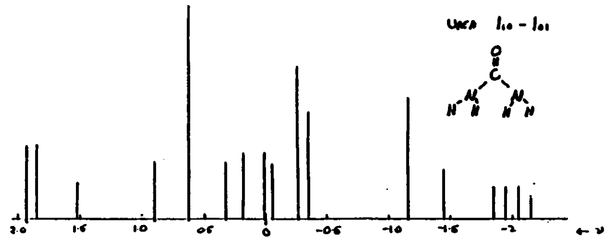
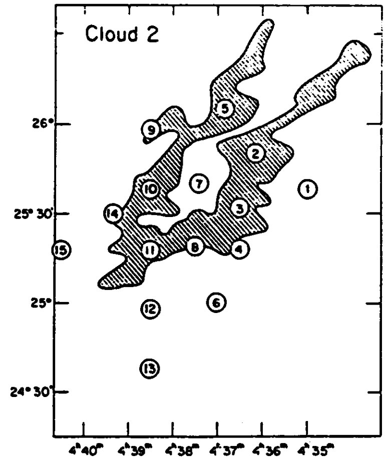


Fig. 2 - The expected quadrupole hyperfine pattern of interstellar urea (from Sutter 1984).

$1_{10}-1_{01}$ transition of interstellar urea would have been measured by now if the hyperfine lines in Fig. 2 were lumped together into one line. Even so, with a modern low noise telescope system, like George Seielstad assures me is available here in Green Bank on the 140-ft. telescope, it should be possible to detect urea. The careful measurement of the abundances of the gas phase molecular species which are also found in Miller-Urey type experiments would be an organized approach to the question of biomolecules which could be very fruitful.

There is another approach to the problem which is more instrumental and less chemical and has to do with things like interferometry. I brought this discussion along because I knew George Swenson would be here and he likes to hear about the results of interferometry and other measurements of high spatial resolution. Studies of star-forming regions have been aided greatly by molecular observations made with high spatial resolution. For example, the picture of the Orion infrared source IRc2, which is emerging from telescopes at Berkeley, Bonn, Onsala, and the VLA, is that IRc2 is surrounded by a dense disk or donut of gas which has a high-velocity bipolar outflow (Erickson et al. 1982; Plambeck et al. 1982). The outflow is traced in HCO+ and appears to originate either from the surface of the disk or from the central star itself. The diameter of activity is less than one arc min. and, within this region, IRc2 is believed to drive the flow. The disk is composed of molecules such as SO, HCN, SiO, and NH_3. The NH_3 seems to be central to the clumps or condensations in the disk (Pauls et al. 1983), the OH masers are scattered around inside the dense gas disk, and the inner edges seem to support the SiO masers, within 35 AU of the infrared source IRc2 (Wright and Plambeck 1983; Lane 1982; Wright et al. 1983). So in the disk - which is the material that the star IRc2 formed from - we find all of the major precursor molecules of a Miller-Urey experiment. In a very elementary sense, we are observing the biomolecular gas going into the very earliest stages of star formation. Clearly this is an area where both interferometry and molecular spectroscopy are starting to make a real contribution, not only to our understanding of star formation, but also possibly to our picture of the type of protostellar environment that might contain biomolecules.

A third approach involves careful measurements of molecules in dark dust clouds using single element antennas. There is a well-known, nearby dust complex called TMC 1 or Heiles Cloud 2 - a dark dust cloud believed to be a site for formation of low mass stars such as our Sun. The top part of Fig. 3 shows an outline of the TMC 1 obscuring region sketched from the Palomar prints by Patrick Palmer who was working here at Green Bank late one night (that's when he does his best work).



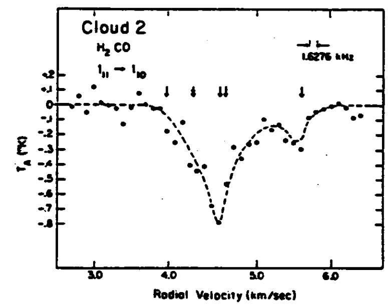


Fig. 3 - Upper. Obscuring region around TMC 1, with the positions where H_2CO observations were made indicated by circles whose diameter is that of the antenna beam. Lower. The 1_{11}-1_{10} H_2CO spectrum at point 11 in TMC 1 (from Palmer et al. 1969).

The bottom part of Fig. 3 shows formaldehyde (H_2CO) in absorption against the 3°K microwave background (a so-called DASAR). The TMC 1 complex has formaldehyde in many locations, which is fascinating in itself, and it also contains the largest molecules listed in Table 1: HC_3N, HC_5N, HC_7N, HC_9N, $HC_{11}N$; CH_3CN, CH_3C_3N, CH_3C_5N; CH_3C_2H, and CH_3C_4H. Each of these families of molecules differ in size only by an incremental pair of carbon atoms. We don't think that the TMC 1 cloud is unusual; it is just very cool, about 10°K, so the molecular partition functions are very favorable for detection, and it is fairly close, so it fills the beam for a typical observation. For quite some time, it appeared that the TMC 1 molecules were being detected almost at random and we could discuss only the obvious implications. For example, the essentially linear molecular structure gives a simple Hamiltonian which has relatively few energy levels of high degeneracy. This effectively concentrates radio photons into a few transitions which have enhanced detectability. Thus, these structurally special large molecules are similar to simple diatomics in their radiative transfer properties. We could easily believe that each structurally simple TMC 1 molecule represents a family of companion molecules, each with a different structural arrangement of exactly the same atoms, but with transitions weakened by photon drains to other levels as allowed by the more liberal selection rules which usually accompany increased structural complexity. Therefore each large m-atom molecule detected in TMC 1 should be regarded as the visible representative of many other undetectable m-atom molecules. At 10 atoms, the molecular complexity of TMC 1 is at the level of glycine.
Another interesting point is that originally many of the large TMC 1 molecules were reported to have column density values which had been computed using rough estimates for the dipole moments which turned out to be not very accurate. Therefore some fairly large errors in column density values can be found in the literature. Now the dipole moments of many of the large TMC 1 molecules have been computed very accurately and Fig. 4 shows the interesting

relationship between m, the number of atoms in a molecule, and the logarithm of column density in TMC 1 which emerges when the correct dipole moments are applied to the column density computation. The solid line in Fig. 4 shows the results of a least squares fit to log N for the revised column densities of HC_3N through $HC_{11}N$, which gives a decrement $N(HC_{2n+1}N)/N(HC_{2n+3}N) = 3.06$. The dashed line is a fit for HC_3N through HC_9N only, which gives a decrement of 2.51. The simplicity of the relationship in Fig. 4 is not predicted by straightforward modeling, but it strongly suggests that there may be key formation reactions which dominate the abundances of the long carbon chain molecules in quiescent interstellar dust clouds (Herbst 1985). One important point is that Fig. 4 may be a predictor diagram for the abundances of other

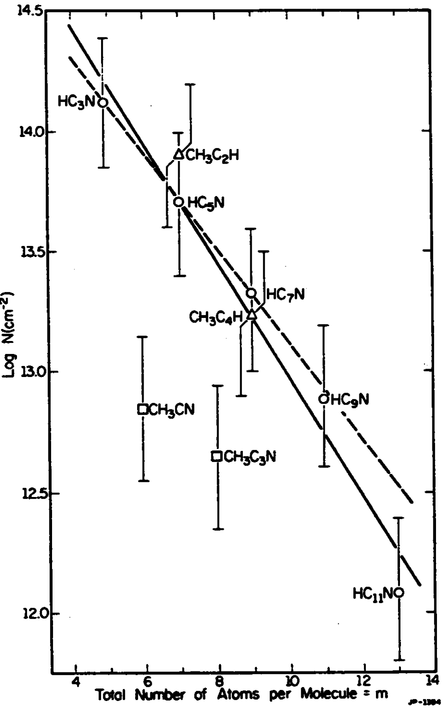
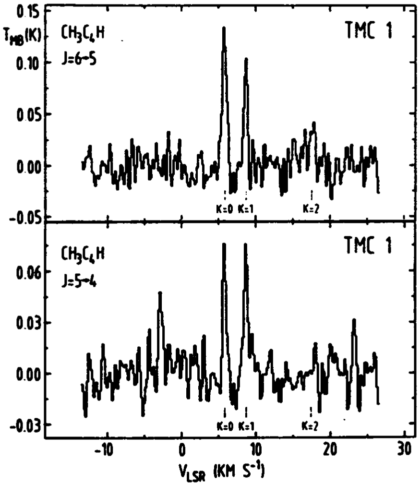


Fig. 4 - The logarithms of the revised column densities (ordinate) plotted against m, the total number of atoms per molecule (abscissa) for the dark dust cloud TMC 1 (from Snyder, Dykstra, and Bernholdt 1985a). Solid line-least squares fit for HC_3N through $HC_{11}N$ (circles). Dashed line-least squares fit for HC_3N through HC_9N only. The logarithms of the column densities of the CH_3CN-CH_3C_3N (squares) and CH_3C_2H-CH_3C_4H (triangles) families are plotted but not included in the least squares fitting.

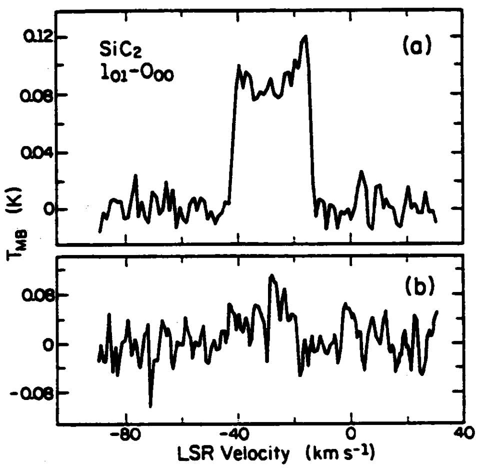
large TMC 1 molecules. For example, given a decrement of 2.51 and the projected density of a species like C_3O, it might be possible to predict C_5O, C_7O, etc. As an example of how useful this might be, consider Fig. 5 which shows the excellent signal-to-noise ratio found for the large interstellar molecule CH_3C_4H in TMC 1 using the Effelsberg 100-m telescope of the MPIfR. The excellent signal-to-noise ratio seen in this spectrum of an 8 atom molecule shows that there is a lot of margin left with receivers available today before signals from large molecular species go below the limits of detectability. If Fig. 4 is a prediction diagram, it can be used to make the first meaningful estimates of the abundances of undetected molecules of possible biological relevance in TMC 1.

Fig. 5 - The CH_3C_4H spectrum toward TMC 1 (from Walmsley et al. 1984).



One final point of interest that is difficult to evaluate is the contribution of the interesting molecular mix fed back into the interstellar medium by evolved stars. Thaddeus, Cummins, and Linke (1984) have identified the small ring SiC_2. Fig. 6 shows the spectrum of the $1_{01}-0_{00}$ transition of SiC_2 in the envelope of the evolved carbon star IRC+10216, observed with the Effelsberg

Fig. 6 - (a) Spectrum of the $1_{01}-0_{00}$ transition of SiC_2 toward IRC+10216 taken with the Effelsberg 100 m radio telescope of the MPIfR (from Snyder et al. 1985b). The ordinate is main beam brightness temperature and the abscissa is LSR radial velocity. (b) The average of all offset spectra (N, S, E, and W) taken from a five point cross map in R.A. and dec., centered on IRC+10216 with one beamwidth offsets.



100 m radio telescope of the MPIfR. If ring structure is important for biomolecular studies, the contribution from evolved stars must be evaluated. A second small ring, C_3H_2, has been detected and identified in many sources in the galaxy (Thaddeus et al. 1985).

B. Oliver: Is there a reason for the square shape of that line?

Yes, there is. If the radially flowing gas is observed with the Bonn telescope (as in Fig. 6) which has a beamwidth of 42" (FWHP) at this frequency, the gas shell starts to become spatially resolved. If the gas is optically thin, this allows us to estimate that the emission radius R_e of the IRC+10216 circumstellar envelope is approximately 29" in the SiC_2 $1_{01}-0_{00}$ transition. If the circumstellar envelope is not spatially resolved, the spectral line has a flat top. I can show you a spectrum of SiC_2 in IRC+10216, taken with the old NRAO 36 ft. telescope at higher frequency, which is not spatially resolved and it has a flat top.

B. Oliver: It's Doppler spread, then, is it?

Yes. It is Doppler spread (in Fig. 6) and the breadth of the line base is twice the outflow velocity of the SiC_2 gas leaving IRC+10216.

Let me finish by discussing comets - the delivery system which potentially connects the interstellar cloud to the solar system to the planet Earth. It is not new to talk about comets as a potential delivery system and Donn (1982) has summarized the historical background of the idea that comets may have played a role in biochemical evolution on the early Earth. Radio molecular observations of comets are important because they have the potential to probe deep into the coma and sample the potentially rich chemistry near the nucleus - which may contain many elements of the preserved interstellar chemistry. Molecular observations have been reviewed elsewhere by Snyder (1982) and Crovisier (1985) so it is sufficient to say that cometary radio signals from NH_3 and H_2O and hints of signals from HCN and CH_3CN have been reported, but these species have not been detected consistently in major comets. This lack of consistency has caused some to doubt that comets actually have a very complex chemistry. A most important clue that this viewpoint may be incorrect comes from radio observations of cometary OH - to date 16 comets have been found to have detectable radio lines of OH so it is safe to say that almost every major comet has observable OH (see Snyder 1986, for example). Fig. 7 shows OH spectra from two comets taken with the old 120 ft. radio telescope of

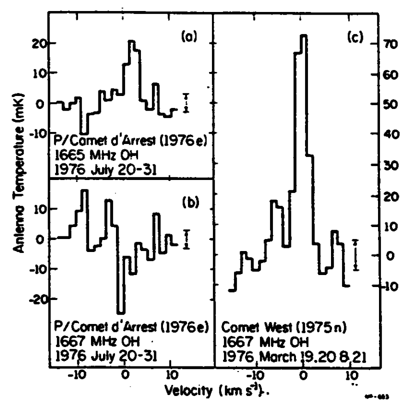


Fig. 7 - Cometary OH spectra observed at the Vermilion River Observatory for (a) 1665 MHz emission from P/Comet d'Arrest, (b) 1667 MHz absorption from P/Comet d'Arrest, and (c) 1667 MHz emission from Comet West 1975n. Spectra (a) and (b) were observed from 1976 July 20 through 31, and spectrum (c) was observed from 1976 March 19 through 21. Ordinates, antenna temperature in units of 10^{-3} K (mK) where 1 K = 5 Jy; abscissae, radial velocity with respect to the rest frame of the comet (from Webber and Snyder 1977).

the Vermilion River Observatory. Periodic comets such as d'Arrest (Figs. 7a and 7b) are central mechanisms for the delivery of cometary microorganisms to Earth in the model proposed by Hoyle and Wickramasinghe. The actual radio observation of cometary OH can be tricky because OH is pumped by solar ultraviolet radiation. Time variations in the OH pumping mechanism, often called the Swings effect, happen when cometary OH absorbs Doppler-shifted solar ultraviolet Fraunhofer bands. This causes steady-state fluorescent pumping of the $X^2\Pi$ ground state Λ doublet levels to the electronically excited $A^2\Sigma^+$ state. The OH molecules cascade back to the ground state, thereby establishing the relative populations of the ground state Λ doublets. Hence this mechanism determines whether cometary radio OH signals will be observed in absorption, emission, or not at all. Other cometary molecules with greater chemical complexity may also undergo radiative or collisional pumping which ultimately determines whether they are detectable with current radio observational approaches.

It appears that cometary studies are now in the phase that interstellar cloud studies were in around 1965 when OH was the key molecule observed by radio astronomers in interstellar molecular clouds, long before all of the other molecules in Table 1 (except CH, CH^+ and CN) were known. Its abundant presence in comets may signal the existence of a rich chemistry near the cometary nucleus, far beyond the simple dissociation of H_2O, which will be uncovered by new millimeter and submillimeter instruments which have both high sensitivity and good spatial resolution. Already Hasegawa et al. (1984) have used the unprecedented millimeter-wavelength sensitivity of the 45 m telescope of the Nobeyama Radio Observatory to detect a hint of the long carbon chain molecule HC_3N in Comet Sugano-Saigusa-Fujikawa (1983e), as shown in Fig. 8.

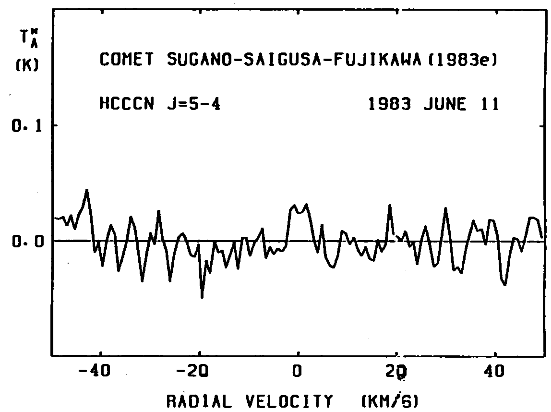


Fig. 8 - Possible detection of the J=5-4 transition of HC_3N in emission at 0 km s^{-1} radial velocity in the cometary rest frame (from Hasegawa et al. 1984).

So the Japanese have it all wrapped up coming and going - they have the comet and the telescope.

K. Kellermann: *They don't have the line, though.*

We actually can think of some experiments which will test quantitatively whether or not comets are a good delivery system. So let's conclude with just two points:

1. We need to agree on what the important biomolecules are, and then perform the gas phase spectroscopy, both in the laboratory and in space, in order to get good identifications and abundances.
2. We should use the best radio instrumentation that we have to observe comets in order to bring our knowledge of them past the OH stage and to allow us to learn how well comets preserve the interstellar chemistry.

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S. von Hoerner: I would like to make a comment about the implications of all that for the development of life. I think it's not only that these molecules might be brought down by comets, or something else, into the atmosphere of the planet. For me, it's very impressive that these very complicated molecules can develop in almost empty space, so how much more developed might they be in a nice warm pool on Earth?

One of Fred Hoyle's important points, which is probably not well known, is that the continuum emission from periodic comets doesn't behave as expected. It is too weak and consistent with the presence of a cometary crust. This crust, in the Hoyle-Wickramasinghe theory, could act as the "lid of the pressure cooker" which traps H_2O in liquid form during perihelion passage. It is not a bad point, at least until we understand what is happening with cometary crusts.

B. Oliver: Have any calculations been made on the survival efficiency of these components on cometary impact? It seems to me it would be awfully low.

I think that the answer to your question is yes, but I think that we also have to look at some of the interesting things that are going on that are possibly related to your question, and yet unexplained. For example, there is no longer any argument about the existence of amino acids in the Murchison meteorite. One interesting, but highly controversial interpretation of the small particules found by H.D. Pflug in the Murchison meteorite is that they look like shrunken, dehydrated bacteria. A lot of related studies of meteoritic material might have some bearing on your question.

J. Tarter: What's the mechanism for survivability for the large molecules passing through the accretion shock? Do they come in on grains?

You just need a lot of opacity. But before we get excited about survivability, I might pass on a remark that Bill Watson made to me several weeks ago. That is, if we strictly apply the theory we know concerning grains in 10 °K clouds, we shouldn't be observing any large interstellar molecules at all. I worry more about that than about survivability, but if the molecules are surrounded by high opacity, I believe they will survive.

T. Bania: Is the OH in comets really the tip of the biological iceberg or is it what's left over after the stuff that you are interested in has photodissociated? Is there really anything there to see?

The OH in a comet is very widespread and it flows outward from the nucleus, probably in a toroidal distribution if current models are correct. Water and OH coming off of the nucleus are photodissociated, leaving a distorted OH toroid. OH is a good radio signal molecule because it is widespread, giving very little beam dilution, and we understand the Swings pumping mechanism for OH. On the other hand, we don't know much about the Swings pump for radio CN, for example. This is not because radio CN is necessarily difficult to understand but rather because somebody has to take the time to model it and test the model observationally. A molecular pumping mechanism can be very critical for cometary detection of a given molecular species; in the case of OH, the Swings pump determines whether the OH is observed in emission, absorption, or not at all. So in the absence of very much hard information about the existence of other radio molecules and their pumping mechanisms, I think the existence of OH is a fair indicator of chemical activity.

S. Gulkis: How good is the analogy between the molecular clouds and the Miller-Urey experiment? I would expect that the conditions are vastly different between the two - the temperatures, the densities, the lightning.

In general, the Miller-Urey experiment just requires energy input into a pool of simple species. If you have the energy input and a long enough time scale, you don't need an efficient process.

S. Gulkis: You're saying the densities don't matter?

Well, I'm not saying they don't matter, but I'm saying that it is something we can test quantitatively, and, unwittingly, these tests have already started. We are perhaps one-third to one-fourth of the way through these tests now and trying to reach half way. We need very careful measurements of abundances for the key Miller-Urey molecules. Once we have these measurements, we can ask whether or not these abundances are in agreement with what would be expected from a Miller-Urey type experiment. Certainly the interstellar measurements can be compared with the molecular yields from terrestrial experiments.

S. Gulkis: Are there any similar experiments going on today under the conditions that you would expect in the molecular clouds? Are (unintelligible) groups doing that?

Yes, not anybody here that I know of, but there are some experiments going on to try to generate species under "semi-molecular cloud" conditions. Mayo Greenberg's lab in Holland is doing some work of this type, the Winnewissers in Germany are doing some molecular generation work, and Sagan and Khare are doing experiments that are basically next generation Miller-Urey experiments, producing tholins which are the unanalyzed tarry residues from the Miller-Urey experiment.

Wm. Mook: You mentioned very briefly about carbon molecules. What wavelengths are you looking at?

Well, our best wavelengths has been the spectral region where the excellent JPL receivers operate, which is K band - around 1.2 centimeters - but we are not working at the peak intensities of the very long carbon chain molecules. For that, we should be lower in frequency - down around 15 GHz.

Wm. Mook: Concerning absorption, I've got a list from JPL of all the predicted occultations for Comet Halley. These sources aren't well studied at molecular frequencies. So measuring the continuum spectra of these sources at different frequencies might be important for molecular absorption experiments in comets.

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MUTUAL HELP IN SETI's

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Thank you for letting such an ignoramus speak to you on this subject. My knowledge of astronomy is that I never got quite through Russell, Dugan and Stewart. And, as for radio, when they took the variable coupler off the top of the Atwater-Kent, that lost me. But nobody here knows about that.

C. Seeger: What do you mean? I had one!

My work will be over with the couple of publications I am going to tell you about, so it is no longer current. And it is certainly only a suggestion rather than a program.

The suggestion is occasioned by listening as a layman to several talks by Drake and others in the field some years ago, and having an impression that where you guys are going--leaving out the ESP and the other stuff, and apart of course from eavesdropping on nearby stars--is radio wave detection from many, many distant stars. The impression I get of what it is going to be like is that when you are well-funded every civilization-bearing planet will be totally covered with radio receiving dishes (Fig. 1). At the console will be two senior scientists saying to each other, "Where is everybody?", as essentially in the legend, as said at the beginning by the only ET we know of when He or She walked into the Garden of Eden and called out for Adam and Eve - the very first message. For those into signal recognition, note that the vowels are missing.

Throughout the galaxy the others will all be saying to themselves, as I heard you say today: some supercivilization is going to be making a signal for us to detect, and/or we are going to evolve in some mystic way to a higher order of things; and/or we are going to get out in space; somehow it is all going to be different. That may well be and I don't want to argue against imagination in science. But what struck me as remarkable is that nobody took up the option of saying maybe they'll be like us, and/or maybe they'll go through only a short phase, such as we can now imagine only too easily. Maybe Drake's L is only 10 or 100 years.

If that's the case, what would it take to hear something? How many candidates would we have to be sending to, and with what "Golden Rule" assumption of responsibility for sending as well as receiving, in order to make a working system with (as an order of magnitude) unit probability of success? That is, how do you actually lay it out the way you would if it were, say, a particle physics experiment, where you say, "In order to get success in this experiment, we estimate we need the following."

Now this approach didn't start that way, but rather started confusedly, as always with me. My colleague in this work was Fulvio Melia, a bright young theoretical astrophysicist, just getting his degree on neutron star atmospheres with Paul Joss. We have three publications. The first paper, which I will not talk about, was "Siblings for SETI" (Icarus 55, 432, 1983), in which we looked through the SAO catalog to try to find what properties of

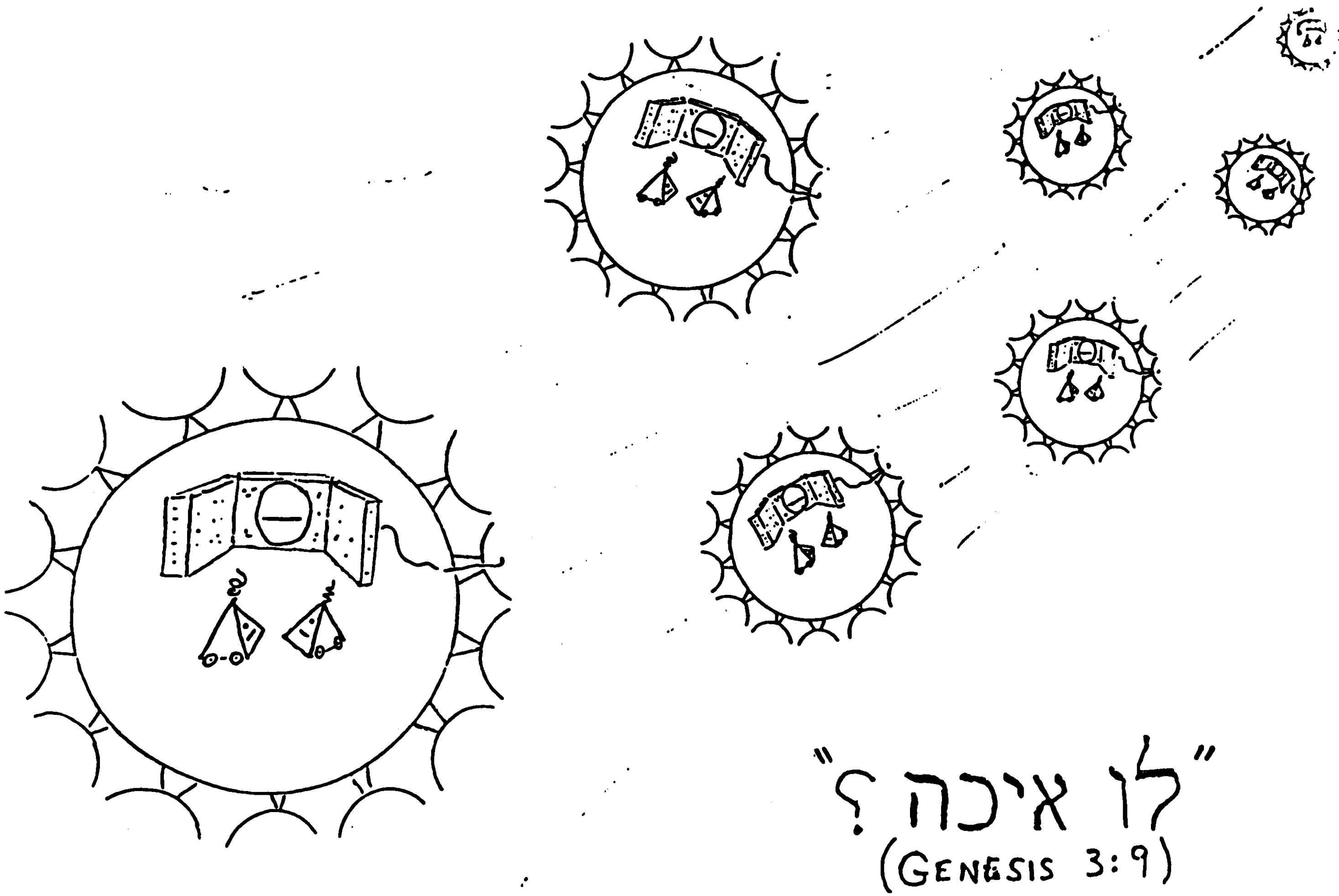


Fig. 1. Our Galaxy, full of maximally instrumented but totally disappointed listeners...since NO ONE IS SENDING.

other GV stars they would see similarly in us. For example, are we together with them in a line of stars, or are we and they stellar spectroscopic analogs, etc. We came up with what we thought was a pretty good scheme that suggested about a thousand mutual candidates. But we looked back at the numbers that we would put into the Drake equation and realized that a thousand stars is way too small a set to give [the order of] unit probability of success.

So the second publication, "Mutual Help in SETI's" (Quarterly Journal Royal Astronomical Society 26, 147, 1985), addresses the larger questions of what it takes for success. The third publication will be the erratum to the second publication, necessary because I screwed up the calculation in the second, as will be explained soon.

It's a very short story, and the components of it you all know. Equation 1 is the Drake equation in our own notation.

$$N \approx 10\, C\, g\, T_{send}\, N_{*}. \qquad (1)$$

On the left side is the number of stars N that are sending at a given time, among a set of GV candidates N_*. On the right is a constant C (for Civilization) which subsumes practically everything in the debates over the Drake equation. C is the rate of maturation to our state--the stage represented by this workshop--per star, per year, averaged over a typical GV star's lifetime. For us on earth C is 10^{-9} to 10^{-10}. Next, g is the "golden rule" factor, the fraction of listeners who also send; for us g has been zero in the systematic programs discussed here, and even episodically gT_{send} has been only $\approx 10^{-5}$. And N_* is the number of GV candidates in the set we will consider. The coefficient 10 is thrown in because the GV stars are in the Main Sequence together, so that candidates are all ripening at somewhat the same epoch. We are going to be sloppy about numbers.

T is Drake's L, except that here, so far, it is only for the sending time. Think of T as perhaps 10 or 100 years. That's not to say that we are necessarily short-L people; this is just an attempt to get at something practical by considering such short times. N_* is the number of candidates we are going to listen to. We will decide that N_* had better be at least 10^8 if we want to find at least one roughly GV star that is in the process of sending, and thus have a chance near unity of getting one contact. Therefore we must listen to all the candidates in pretty much the whole galaxy, or at least to those on the near side of the galaxy.

Having put in the parameters in the Drake Equation to determine the size of the set, and thence having determined the distances to be reached, we can ask what does it take for them to broadcast to us and for us to broadcast symmetrically to them, so that we can hear each other. (For first contact we will not hear the same ETs that will hear us; that does not affect the calculation of hearing someone.) We will assume maximum symmetry throughout this calculation: they are going to make their antennas the same as ours, and we and they are going to use the same antennas for receiving and broadcasting, although maybe not simultaneously; similarly, everyone will use roughly the same time $T = T_{send} = T_{receive}$ for sending and receiving.

To calculate how much power is needed we adapt Drake and Helou's equation for the minimum time, t_o, for detection by substituting the antenna gain $G = 4\pi A/\lambda^2$ into it; and also assume $A_{send} = A_{receive}$:

$$t_o = (k\theta_r)^2 B \frac{\lambda^4 G^4}{A^4 P^2} (SNR)^2. \quad (2)$$

Here θ_r is receiver temperature; B bandwidth (the minimum bandwidth is about 10^{-1} sec^{-1} in the range we will consider), λ wavelength; R distance from sender to receiver; A the (assumed equal) area of sending or receiving antenna; P the sending power, and SNR the signal to noise ratio to be achieved in simultaneous listening and sending on-time t_o. Note that the calculation is much more sensitive to area than to power because the area comes in for both sender and receiver, but the power is send only by the sender.

So now we have calculated the minimum time t_o necessary to send and listen to a single candidate, the result of a calculation assuming that those guys are beaming at you in the way you're beaming at them, symmetrically. Perhaps they are not sending, but if they are, we say they're bright enough to do it by calculating symmetrically, with the receiving time roughly the same as the sending time, given by the Drake equation estimate of the number of stars needed in the set.

Here is a pick of parameters to give something to think about. We ask to hear one ETI—though maybe if it turns out to be easy, we should design to see 10 or 100—so we take the number of senders on the left of the Drake Equation as 1. The rise time of civilization, $C^{-1} \approx 10^{10}$ years, is taken from our own history to be the standard for this galaxy. That's the best we can do—the same as measuring a half life from one event.

The probability g that people will be altruistic enough to send as well as listen--[among other costs] to reserve a place for SETI transmission in their radio spectrum—the altruism probability, is hard to judge. We could take a poll to find what you think; I took it as 1/10. We take 100 years as a reasonable time T for a technically ripe civilization to pay attention to SETIs; this T was T_{send} in the Drake Equation, and is also now $T_{receive}$. This short T is not to prejudice 10,000 years, but a short T is my idea of using our own history as best we can know.

If you take those numbers, Equation 1 is $1 \approx 10 \cdot 10^{-10} \cdot 10^{-1} \cdot 10^{2}\, N_*$, so the set must contain $N_* \approx 10^8$ stars. To estimate roughly the distance to these candidate stars, we take the galaxy as a uniform disc, with 10^{-4} GV-type stars per cubic light year. The volume is the whole height of the galaxy, 1000 light years, times the area πR^2. This requires a disc that extends half way into the center of the galaxy, some 15,000 light years.

They--you--have two extreme choices of sending/listening scanning schemes. One choice is to scan candidates in series, looking for a coincidence. You irradiate them, they irradiate you; and you listen to them, they listen to you. The total time T for sending to and receiving from the N_* candidates in your set is $T \equiv T_{send} \equiv T_{receive} \approx N_*^2\, t_o$ so as to get overlap between sender and receiver. (The erratum was that I put that as N_* in the paper rather than N_*^2.) But that N_*^2 is a killer, allowing only $t_o = 0.3$ microseconds for dwell-time when $N_* = 10^8$, and so requiring huge power and area, as we will see.

The other choice is to try to arrange to irradiate them in parallel and listen in parallel. If you can arrange to do that, N_* does not enter, and the allowed overlap time is $t_o \approx T = 3 \cdot 10^9$ seconds; you have a very cushy job indeed.

Whichever scanning scheme we choose, we now can equate its t_o with that necessary for signal recognition, as given by the Drake-Helou Equation.

The other factors in the Drake-Helou equation that we choose as typical are the wavelength, which we take as the 21 centimeter line, and the signal to noise ratio, which we take as 10. That's a very low signal to noise if you are going to canvas 10^8 things, and you are going to get many duds, but then you can later focus on them (the promising candidates) and find the true signal at a higher signal to noise. Also, Drake and Helou find that interstellar scattering from R_{ly} light years distance sets a minimum value for the term $(k\theta_2)^2$ B, to within a factor of 10 over the 1-400 GHz range, of about $10^{-47} R_{ly}^{1/2}$ in mks units.

Putting in these values, we get for the approximate minimum dwell-time between a sending-listening pair of searching civilizations

$$t_o = \frac{300}{A_H^4 \, P_{MW}^2} ,$$

where A_H is given in hectares (100×100 m^2 = 2.47 acres) and power P_{MW} in megawatts. A denominator of about unity would be reasonable with current technology. Scanning 10^8 candidates in series requires $A_H^4 P_{MW}^2 = 10^9$, a very big number. Simultaneous scanning requires only 10^{-7}, so that only a phased array of one thousand 1m^2 dishes at 30 watts each would do it.

Scanning in parallel assumes that these dishes focus on and listen to all the different stars simultaneously. How can you focus on all the stars simultaneously, each with the full intensity that the total antenna area and power would give if focused on only one star? Since each star subtends a much smaller angular patch than does the small central diffraction lobe of one large antenna, the energy is there to be used if it can be distributed among bright interference spots in the direction of the individual stars. This suggestion is to use the very large diffraction lobes of a [phased] array of small antennas to overlap to make a specified interference pattern of light spots. You have available for the location of the individual small antennas the surface of the earth in two dimensions, and also the up-down direction for depth, and the phases can be chosen arbitrarily. I am incompetent of formulating the mathematics--probably Diophantine Equations--to calculate the location and phasing of antennas necessary to give this sending/receiving pattern.

G. Verschuur: Are you saying that you have many, many beams which essentially follow many stars?

Not single beams; they are all one grand array.

G. Verschuur: Right, but a multiple array that produces many beams?

Yes, it produces many interference spots, so that each spot tracks a star.

S. Weinreb: Does each telescope look at all the stars, or is each one looking at one star?

Each telescope looks at all the stars. But, practically, it would probably be better to break the total target area of the sky into plaques, so that there would be several groups of antennas, and all the antennas within a group would be used to make bright spots at all the stars in that group only. It might turn out that the blunder I made in the published paper of using N instead of N^0 or N^2 is about the most practical compromise.

[A phased sending/receiving array for many different candidate stars could not scan frequencies, necessary over at least a 10 kHz band because of coordinate uncertainty. So my estimate above was quite wrong in using the same narrow (10^{-1} Hz) bandwidth for receiving as for sending, and there must be a separate, much larger receiving array to match the tightly phased sending array.]

Is it reasonable to assume full symmetry between independent civilizations? As a layman at least, I do not see a vast range of technical possibilities. For example, since structural limits are set by common metals, variation in the practical limits of the areas of individual antennas cannot be very great. It cannot be a big choice whether or not to use 21 centimeters. How could you not? [See below!] In general it seems to me that there is enough symmetry and simplicity in the problem that what I propose here would allow you to say "We have unit chance, we estimate, of making a detection in 30 years."

B. Oliver: Can you ever say that?

No. [You can say that your estimate of success using certain categorized assumptions (with probabilities attached), equipment and procedures, is greater than, e.g., 50%. This is the standard method of proposing research--idealized, of course. By modeling sending as well as receiving, one is forced to choose parameters, most crucially a time scale. The hope of such studies would be that a consensus would develop for a particular sending/receiving strategy with a particular characteristic time--say 10^2 years. It would then make sense to listen using that scheme for that time before sending. After that period of merely listening one should begin sending too. The more likely outcome of careful consideration of sending along with receiving is that mutual responsibility will seem too complex and contingent to be worth pursuing. Reliance on a Supercivilization to be sending may well have lost its appeal too, seeming to be an even more improbable development than mutuality.]

The only list of reasons I could find why we should not be broadcasting are shown in this box:

"- Even rather optimistic estimates of the number of civilizations in the Galaxy imply that the rate of emergence of new civilizations competent in radio astronomy could be less than one a decade. Therefore, it seems likely that few if any civilizations in the Galaxy could be simultaneously capable of interstellar radio communication and more backward than we. Accordingly, as the newest potentially communicative civilization in the galaxy, we should be listening rather than sending;

- Other civilizations considerably more advanced than we would have substantially greater energetic resources and more advanced technologies to devote to transmission;

- Our global long-term planning does not yet accommodate dialogues that could take many centuries for a single exchange;

- Some humans are concerned that transmissions even to nearby stars might "give our position away" (although this has effectively been accomplished by commercial television and military radar systems);

- And, at any rate, it is not yet clear that we have anything particularly interesting to say.

- For all these reasons the present SETI strategy of listening but not sending seems appropriate for our backward status."

Excerpts from C. Sagan, Science 220, 4/29/83,
explaining why we should not send signals to ETIs.

The first reason is that we should not broadcast because we are the newest boy on the block. Well, we are the newest boy on what block? Our estimate using the Drake Equation, including our estimate of g, says that if we listen for 30 years with the detection array just described, we ought to hear somebody. So after 30 years, it is presumably our turn. The trouble with that argument about being the newest boy on the block is that it doesn't have a cut-off date. Suppose you believe something different--say, that it will take a thousand years of listening to hear someone, are you then to wait a thousand years? I sense in all this a reluctance to talk about sending that is almost certainly well founded from a political, technical and so on standpoint, but remarkable in a group that is devoted to speculation.

The second reason is the "other civilizations are more advanced." I think that if you can make a calculation that shows that it doesn't take a super-civilization to give you a detectable signal, there is no need to wait for one.

The third reason is that "our global long-term planning does not yet accommodate dialogs that could take many centuries for a single exchange." That could be well taken as a summary of Dr. von Hoerner's talk, and the talks of others, about the sad state of the human condition. But I don't think the problem is an exchange at first--it's just getting some indication that there is someone there.

The fourth reason is that this will give our position away, and the fifth is that "at any rate it is not clear that we have anything particularly interesting to say." Well, that's throwing in the towel--but good! If someone came for support and said, "I want to listen because they may have something terribly interesting to say, but we don't have anything interesting to say," it somehow would sit very badly. We had better believe that our existence is of as much interest to them as theirs is to us [or else why would they go to the effort of sending to us].

B. Burke: Dave, if you think you are going to transmit at 21 centimeters, you have another think coming. We promise you that. You will not!

K. Kellermann: That is not the important thing. What about the extraterrestrials? Will they be allowed to transmit to anyone? Presumably not. It may be a waste of time to listen there at all.

B. Oliver: I think Kellermann's argument about 21 centimeters is valid but that is why we talk about a band rather than a single frequency.

B. Burke: Maybe spread spectrum...?

[Surely a sender--no matter how superior in resources--will pick universally appealing carrier and modulation frequencies if at all possible. As pointed out by Kuiper and Morris (Science 196, pp 620-621, 6 May 1977), the set

$$\left(\frac{e^2}{hc}\right)^n \cdot \left(\frac{c}{2\pi r_{Bohr}}\right)$$

gives the characteristic frequencies formed from the universal physical constants, and the carrier frequency 2.5568 GHz (n = 4) uniquely is in the water hole. As pointed out above by Ken Kellermann, it is presumably a waste of time to listen at 21 cm, and by implication at any wavelength that is naturally emitted, so 2.5568 GHz is a unique choice in the 10^{-1} - 10^{+2} GHz range. Similarly 993.56 Hz (n = 7) and 7.2504 Hz (n = 8) are the only modulation frequencies in the 10^{-1} - 10^{-5} sec. range worth listening to.]

B. Oliver: The big reason for listening first is because if you send you do so in the hope of evoking responses, which means ipso facto *that you have to wait out the round trip light time. Whereas, if there is any action going on right now, you can detect it. And there may be. I can give you other scenarios that will predict that there might be. So we want to test that hypothesis first.*

I'm afraid you missed my point; if everybody does this...?

B. Oliver: I'm not saying everybody is going to continue to do that.

Oh, then give me a characteristic time.

B. Oliver: All right. A decade or two.

I'm certainly not urging anybody to do this tomorrow--it's only in a decade or two.

B. Oliver: After having made a comprehensive search by listening and having been unable to obtain funding for any more collecting area than we already have, you might then resort to some sending.

B. Burke: You really mean a decade squared, don't you?

B. Oliver: That word comprehensive could occupy an entire meeting.

J. Broderick: I think the appropriate time at which it comes to send is when we have the capability. Right now we ignore sending because we don't have the capability. We are capable of listening, and if we were doing this conference a few years ago, before we had astronomy, the most appropriate strategy would be to wait for them to land here in space ships and we would ignore all the others.

What I am trying to show was that it is an open question whether we have the capability. I think it is quite possible that we have the capability. We know about interference; it should be taken as an interesting challenge in antenna design.

J. Broderick: I think we have the capability as well but I don't think all of us think we are going to wait until we have it before we do that. We do what we can.

R. Dixon: I think you will find that our society is not quite ready to talk about transmitting, because to transmit, in this country at least, you have to have a license with the FCC. I have conducted a small experiment in this regard, in that the FCC has certain forms you have to fill out to apply, and they don't have a category that you can check off on this. (Laughter) I contacted the FCC commissioner, as sort of an experimental process, to say I want a license to transmit to other civilizations. I really expected sort of an administrative brush-off. I was really surprised--I received a very encouraging letter from an engineer at the FCC saying that this is worth talking about. Your application belongs in the experimental radio service. That service is one in which you do not do something--you develop techniques for doing something. He was very encouraging that if I were to apply for a license to develop techniques for transmitting to other civilizations it would be granted. But the surprising thing is that nobody ever raised any questions about who am I to be sending things out there. That level of issue was never even discussed.

M. Papagiannis: If I understand you correctly, by the time we would send the signal, or we would receive a signal, the other civilization that sent it would have stopped being available, either extinct or disinterested or whatever.

Possibly, unless it is one of the super-civilizations you are counting on.

M. Papagiannis: But your program was based on short lifetimes.

The calculation was done on short lifetimes. If you get a hint, then you are in the search for super-civilizations.

M. Papagiannis: But when you needed planets covered with all these antennas you were really going for civilization that would only transmit for 10-100 years.

I covered the planets with antennas, saying that was the way you guys were going. What I would like to do instead is find a signal with antennas that are very small indeed, and then having gotten a hint try to see if it is still there.

M. Papagiannis: But am I wrong in assuming that your calculation is based on very short lives, which essentially means that by the time they would receive our signal we would not be here anymore; or by the time we would receive one of their signals, they would not be?

I don't assume that. I assume we may have lost interest in SETI.

M. Papagiannis: It would not be a dialog. It would only be one-step communication.

Yes, until you get the first hint. Then you wipe your feet on the mat and start again. I don't see that is any different from the present situation: what you are trying to do now is hear a single signal. But you won't hear a single signal if everybody reasons the present way. Therefore, to hear a signal you want to broadcast too. What happens thereafter is something else.

T. K. Menon: I have a question. Has anyone given some thought to the question of whether the lifetime of an intelligent being is in any way related to any other astronomical time scale?

A fellow at Harvard (A. Lightman, Am. J. Phys. 53 (3), 211-214, 1984) calculates from general considerations the length of the year on habitable planets, and points out that individual maturity very probably takes at least several years. But he gives no upper limit.

B. Oliver: There is some speculation about very long lives for extraterrestrials. But I would raise the issue that there won't be enough generations for evolution to take place if they are very long-lived. In other words, I think that the evolutionary process restricts you to a modest lifetime if you are going to have enough generations.

A SYMBIOTIC SETI SEARCH

T. M. Bania
Boston University and NRAO

Despite the title of my paper, what I'm really about to give is a symbiotic SETI speech, because compared to the other folks you are going to hear this afternoon, I'm not doing anything. What I am doing is using the Serendip II box. What I would like to share with you, quickly so we can keep to the schedule, are some aspects of a three-year project on which I am about to embark. So this is a coming attractions talk: not only am I not doing anything but I haven't even done it yet!

In a very real sense this talk is your fault: If there is any group of people in the world that ought to be suffering through me standing here right now its you. In 1966 I was a junior in high school. I had a summer job with a wonderful company that amassed thousands and thousands of volumes of books that they then proceeded to sell to high schools throughout our nation. There was a scam here. What they did to these books was to put nice little plastic covers on them, just like this, and they filled them with library cards for high schools that didn't have librarians. My job was to make the book labels with the Dewey decimal classification number on them.

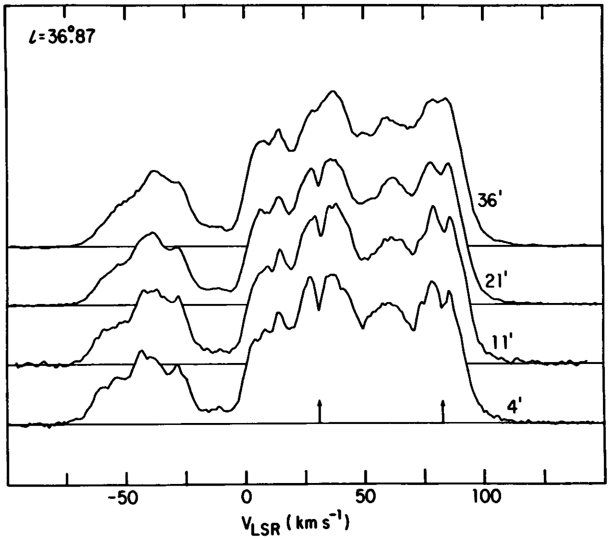
Bania holds up We are Not Alone: The Search for Intelligent Life On Other Worlds, Revised Edition, by Walter Sullivan, 1966 (McGraw-Hill: New York).

In 1966 this book came by, and after I made 5000 more of these labels I went to lunch and took it with me. I later put it under my coat and took it home and that's really why I'm here now.

G. Seielstad: Nail the stuff down!

So what is it that I'm doing? I'm surveying the Galaxy. I'll answer the question: why make another galactic HI survey? About five years ago, Jay Lockman and I started thinking about what we didn't know about the vertical structure of the 21 cm layer in our galaxy. The answer was we didn't know a whole lot. And so we went off and did a survey with Arecibo (Bania and Lockman 1983, Ap. J. Suppl. 54, 513) where we made latitude strips at constant longitudes which were strategic positions in the Galaxy geometrically chosen for galactic structure purposes. What was interesting was that we found a whole lot of HI features that were narrow in velocity space, and had small angular sizes. We were taking advantage of Arecibo's high angular resolution. Let me just remind you in a visual fashion what the 21 cm beams of the Arecibo antenna, the 300-foot telescope, and the 140-foot telescope look like. Clearly if you go to Arecibo, it takes a lot longer to map the sky, but you get many more independent data points.

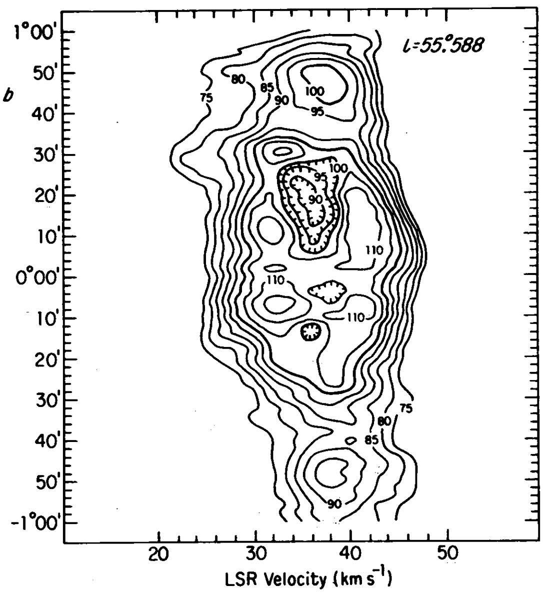
Now why is this important? It turns out that in our galaxy there are some very interesting things that you can see at narrow angular resolution. I am about to show you some HI brightness temperature line profiles. I am showing you spectra with brightness temperature plotted vertically and LSR velocity horizonally. The spectra were taken at the same direction in the sky with these four telescopes: Hat Creek (HPBW = 36 arcmin), NRAO 140-foot (21'), NRAO 300-foot (11') and Arecibo (4'). I call attention to the flagged features, these narrow absorption lines that clearly come into focus at higher and higher angular resolution.



It turns out you can do a whole lot of things with such sources because indeed there are a whole lot of them as that survey slide suggests. There are something like 20 per square degree detectable in the galaxy on average. Each one of these objects is of intrinsic interest for the following reasons: what we can get out of any detection is a galactic longitude and latitude and frequency, its LSR velocity. We can also get, in the cases where we have mapped in both galactic longitude and latitude, an angular scale for the features. Since they are absorption lines we can usually calculate their apparent optical depths.

But what's beautiful about these things is that they provide you with distances to objects whose distances have been previously unknown and unmeasurable by most standard techniques. The reason is that in the first quadrant of the Galaxy, the way in which the Galaxy rotates is such that as you march out in distance from the Sun, the frequency of the radiation is a doubled valued function for reasonable rotation curves. So for any given 21 cm frequency, gas emitting at two vastly different distances in the Galaxy produces this emission. The bottom line is that most of these clouds we are seeing are not absorbing against continuum sources, not HII regions, not known nonthermal supernova remnants, not extragalactic objects, but against background line radiation. That is to say, there is a hot 21 cm cloud in the far distance and there happens to be a cold cloud in the near distance. Since the near cloud is absorbing radiation from the hot background cloud we know its distance.

So what we did next was go out and do maps in 31 regions, square zones in the Galaxy, to see whether we could indeed convince ourselves that these things happen. In many cases you can see a lot of these self-absorbing clouds. Here is a subset of the data, this is a latitude velocity contour map of just a portion of the dataset. There is a clear absorption dip here and possibly two there. The kinematic distance that the argument I just described to you gives to this cloud is 3.25 kiloparsecs. This cloud happens to be, in fact, two clouds. For a clear case like this, one can fit the expected background profile. If you do that and then subtract this background profile you get two absorbing clouds. At the distance of these clouds, their angular sizes imply that they are about 12 parsecs across: they look like standard diffuse clouds.



There is some CO in them as well. There is also a star cluster of 18 OB stars in that direction of the sky with a measured spectroscopic parallax that agrees with the kinematic distance to within 5%. This is a new way of finding distances to clouds. Obviously what one wants to do is get as many clouds as possible, and that is the survey I am about to embark upon.

So really what I'm asking you folks here is not only what Serendip II can do for me, but what else I may be able to do with this dataset. I am going to go ahead and take 70 square degrees of data at 4 arcminute angular resolution with 2 arcminute spacings at 60 seconds per point over the next three years. Along the way, while we are doing the survey we are going to be running the Arecibo interferometer as well, to get for free a high sensitivity, high angular resolution continuum map of the Galaxy which should find a great many compact HII regions.

So that's really my talk, those are the parameters: 60 seconds per point, 70 square degrees, about 10,000 spectra over the next three years. If anyone can come up with a convenient way for me to help out the SETI program besides Serendip II, I am open to suggestions. In particular, one thing that occurred to me is that your box, Jill, only has 60 seconds before I move on. With the new laser disk technology coming on line, for projects like this where there is going to be a large homogeneous sample, why not put the whole IF bandwidth on the laser disk? Do your sampling afterwards.

I could now proceed to go on and tell the NSF what wonderful science I can do with these objects. Let me just say that these are a new sample of clouds. We already have 200 of them. The average distance to them is 3.6 kpc. That average is almost the optical horizon. The HI self-absorption clouds are about 8 arcminutes on average in size. They are correlated, but not one to one, with molecular clouds. In this zone of the galaxy you can obviously see absorption lines from both thermal and nonthermal continuum sources. I will close by showing you a Bonn continuum map of a small zone of the sky. This is one of the regions I have already mapped. This is a one-square degree zone comprising about 500 some odd spectra. There are large giant molecular cloud complexes here that can have their distances checked for the first time. I would like to point out that you can do a lot of galactic structure with the 1000 some odd clouds whose distances can be determined for the first time. This is going to be happy marriage of good astrophysics and SETI.

J. Tarter: Tom, we are very aware of your survey. In fact I've been using your old survey because it poses a most difficult problem for Serendip II. You're giving us data which intrinsically has extremely fine frequency features and we've got to develop the smarts in the Serendip II system to automatically ignore what you are looking for but find what we want to find. It's a wonderful challenge.

Because they are narrow; that's how I find them.

J. Tarter: Yes, that's right. So we can't usually use our normal caveats about astrophysical things being much broader than the narrow spikes we're searching for.

S. Weinreb: How narrow are your lines?

About 1 km s^{-1} or 5 kHz.

S. Weinreb: Isn't that a lot wider than your narrow things, Jill?

J. Tarter: Yes, but there is still a lot of structure in his spectra. When you try to use robust estimator theory to come up what you believe in that intrinsically very spikey, noisy data it's just not as clean as you would like it to be at all.

HI spectra do not have flat tops.

J. Tarter: Not even when you're looking at 2 Hz resolution.

G. Seielstad: Do we know why? Is that the question? Yes or no.

Yes; No. Every feature in this spectrum is real.

J. Broderick: If there was a big spike in there would you be able to see it or would you automatically get rid of it by some interference removing algorithm? If some big, strong 5 kHz wide SETI signal popped up in your data would you be able to recognize it or would you throw it away?

I would be guilty of what every other radio astronomer in my place would do: I'd throw it out and reobserve the position.

SHOULD THE SEARCH BE MADE OPTICALLY?

J. J. Broderick
VPI & SU

Orthodox SETI searches have been confined to the radio region of the spectrum. I propose that unorthodox searches in the optical part of the spectrum might make sense given the right conditions.

These conditions might be:

1. that life is earth-like and its host star is sun-like;
2. that life is bound to such a star, i.e. it doesn't travel;
3. that ETI is so desperate for communication that it would be willing to talk to an emerging society (like us). (Maybe this ETI society would itself be an emerging society, having not yet successfully communicated with anyone. As we ourselves may be in a few thousand years); and
4. that life in the Galaxy is common enough so as to be in sight of other life. Because of dust this means within one or a few kiloparsecs of each other.

If the ETI on a G star chose to make itself conspicuous to astronomers orbiting neighboring G stars, it could most effectively do so by contaminating its host star's spectrum with lasers tuned to some conspicuous optical wavelength (say, one of the lines in the many multiplets of the Fraunhofer spectrum, like <u>one</u> of the two sodium D lines or <u>one</u> of the Balmer series), and having strengths equal to the equivalent width of the absorption lines. This would be readily discovered by the astronomers on planets orbiting the target stars. Once discovered, this feature would not likely be dismissed as having some natural (i.e. unintelligent) cause, especially if the line were narrow, unnaturally complex (e.g. a comb), or were modulated in some intelligent way (e.g. π computed to $10^{32}-1$ decimal places in binary and repeated when necessary).

<u>*Audience*</u>*: Laughter*

The fact that the poisoned line was in the optical part of the spectrum would mean the signal would be conspicuous each time an astronomer with the target star took and studied an optical spectrogram of the ETI star. Perhaps this would happen every few generations of astronomers. It would not go long unnoticed.

Rather than broadcasting these signals, it would appear simpler to beam them to stars likely to have intelligent life orbiting them. So the ETI could target the F, G, K main sequence stars and their descendant red giants and white dwarfs (on the chance that life survives stellar evolution). A 50 meter dish operating at 5000 Å casts a 2 a.u. spot at 1 kpc. A 1 MW laser hooked to this dish can fill a 1 Å equivalent-width line of a G star at 1 kpc. Neither the size of the antenna nor the power of the laser are excessive.[1] A 50 m solar collector picks up a few megawatts of sunlight at earth orbit. Thus an ETI not evolved much beyond our Star Wars phase could probably produce the 10^6 or so pieces of equipment needed to signal all the sun-like stars in its visual range. We here on earth would be a little harder pressed to even send one such signal at the present time.

But we can receive these signals each time we take a spectrogram to classify a star. And whether we consciously make that a SETI search or do it as part of some other work (perhaps, ironically, in the process of finding suitable stars for a targeted SETI search), this search will be done over and over.

Perhaps as Prof. Frisch suggests, when we've been around for a few answer-back times, we'll be tempted to initiate the transmission phase of the search if we haven't found an ETI by then.

W. Sullivan: Do you or anybody else know if people like Nancy Houk who have been doing the big redoing of the HD catalog are looking for anomalies like this?

I hope so. But maybe she'll have some clever way that she can avoid looking at spectral lines to classify the stars. I have no idea.

W. Sullivan: No, she'll be classifying thousands of stars.

Is she going to use blue sensitive plates? The peak of the solar spectrum appears in a region that's not normally photographed by stellar spectroscopists, I understand. So, like, if they fill the sodium D lines, we wouldn't even know it.

W. Sullivan: I'm sure she's got the sodium D lines. I don't know exactly but...

Well, she'll probably discover it, then.

W. Sullivan: If it's important to G star classification.

G. Verschuur: The theorists will eventually explain it away, John.

It would be hard. I think it would be hard. If someone were to fill up the red line of the Balmer series and then try to find some mechanism, some natural mechanism, whereby that happens is a.....

J. Russell: Never underestimate astrophysicists when they have to explain away one data point!

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[1]Curiously, the amount of signal power required to "swamp" an absorption line varies inversely as the square of the distance, because the star's apparent luminosity does also, but the focussed signal doesn't. More power is needed to signal nearby stars!

A SEARCH FOR SETI TARGETS

Jane Russell
Space Telescope Science Institute

Currently the NASA/Ames SETI program has a list of 773 stars, taken out of the Woolley catalog of stars within twenty-five parsecs, as the finding list for their first targeted search. Our topic here is where or how they or someone else, ready to begin a much larger targeted search, can identify a list of more than a few hundred target stars. We presume that the minimum goal is a list of near-solar spectral type main sequence stars of known distance.

There are no parallax catalogs which are even approaching completeness beyond the Woolley catalog. We consider instead what lists might be available from stellar surveys that currently are in progress or planning and how they will contribute to identifying SETI targets. We will conclude with a brief description of the ideal stellar survey which would provide the information and completeness desired for a large targeted SETI program.

This section includes all of the major stellar surveys I know about which are currently in progress. It also includes descriptions of two other surveys in the planning stages.

The largest survey in progress is the the Guide Star Catalog. It is a survey of the sky complete to 14.5 visual magnitude to provide a complete finding list of possible guide stars, i.e., all stars with visual magnitude 9.0 to 14.5, for the Hubble Space Telescope. Actually, the depth of the catalog may vary from 15th magnitude in sparse regions--necessary for completeness because of photometric uncertainties--to 14th magnitude in crowded low latitude fields, but for planning purposes we assume the 14.5 limit.

The survey is based on Schmidt plates from the Palomar and the UK telescopes. The plates are digitized, the list of objects on them inventoried, then classified as stellar or non-stellar, and the positions, magnitudes, and classifications are cataloged. The relative positions of the stars will have an accuracy of 0.25 arcsec for stars from the same plate. On an absolute system, i.e., compared to radio positions, they can have systematic errors as large as 2.5 or 3 arcseconds for objects from the plate corners. The entry for each star is based on information from just one plate; that means only one magnitude and a position for one epoch, with no colors or proper motions. Stars which appear in the areas where plates overlap initially will have multiple entries with mean positions and magnitudes in the first published version of the catalog.

The total number of entries in the catalog is expected to be about 20 million objects. If we consider the near-solar type stars as dwarfs with spectral types F5 - K5, the Guide Star Catalog contains a complete sample of near-solar type stars out to about 280 pc. But the catalog does not contain enough information to sort out these stars from the millions of others.

However, the Guide Star Catalog makes a good finding list for any future survey. It will be especially helpful for any surveys which produce digitized images of the sky, where a lot of data processing can be eliminated if the approximate positions of the objects are known a priori.

The second survey in progress which we consider is the Hipparcos satellite. This is a satellite being launched by the European Space Agency, which will establish a fundamental reference net of stellar positions, as well as measure proper motions, parallaxes, and B and V colors for the stars in the survey. The satellite will orbit in a precessing pattern according to a preprogrammed path so that it scans the whole sky. Its scheduled launch date is mid-1988. It is a two and one-half year mission. The data reduction for the catalog of star positions, parallaxes, and proper motions will be done in one combined solution and the results published about 1992.

Hipparcos has a fixed observing list of 100,000 stars that includes the sky complete to at least 8th magnitude as well as stars suggested in response to a request for proposals. The task of choosing the observing list, the Hipparcos Input Catalog, was made difficult by the requirements that it include as many astrophysically interesting stars as possible, but that the final list be uniformly distributed across the sky as nearly as possible. The program includes a few objects as faint as 13th magnitude. The final list of 100,000 stars is nearly complete.

The expected accuracy of the parallaxes, 0.002 arcsec, means that about 10,000 stars out of the 100,000 will have parallaxes large enough that their errors are 10% or less of the parallax. This will not increase the SETI observing list by as much as an order of magnitude, but will probably provide some new targets. It will also provide improved data for most of those on the current list. And Hipparcos will provide an improved reference frame for any future surveys.

Most astronomers are not aware that part of the hardware for the Hipparcos satellite will also conduct the Tycho project, a survey of the sky in two colors to 12th magnitude or fainter. The Hipparcos satellite has two star mappers, which were planned to be used occasionally to observe bright stars to check the position of the spacecraft. Each of these sensors has in front of it a group of four slits, unevenly spaced, so that when a bright star is scanned it has a very distinct signal. The signal can be processed to update the spacecraft's position. Hoeg from Copenhagen realized that if the data from the star mappers were continually collected, instead of occasionally for just a few bright stars, then the whole sky could be scanned and cataloged. Allowing for recognizing the signatures of stars in the data stream and deconvolving, "pick out and fold," set the limiting magnitude for the Tycho survey to about 11 or 11.5 visual. However, if the Guide Star Catalog is used as a finding list, the magnitude limit can be extended to nearly 12.5 visual while the amount of data processing is reduced. The star mappers have two filters, B and V. Thus Tycho will produce positions, proper motions, magnitudes and colors for the sky complete to about 12.5 visual. The accuracy per star varies because the orbit of Hipparcos will give some stars in Tycho more observations than others. The worst case is an accuracy of about 0.1 arcsec in position and some of them be as accurate as 0.01 arcsec.

As with Hipparcos the Tycho data reduction will be done as one great simultaneous reduction and published as a catalog of approximately one million stars. Useful to SETI, it will include down through the K5 stars complete to 100 pc. The list can be assembled using the colors of the stars to identify the near-solar types. With the apparent magnitude and the color we can calculate the estimated distance to the star by assuming that it is a main sequence dwarf. This would trim the original list of one million stars to about 50 to 100 thousand stars. The only expected errors in the list would be the background giants, probably about 15% of the list for stars outside of the galactic plane.

The estimate of 15% is taken from the Bahcall and Soniera galaxy models, based on data of galactic latitude 20 degrees and higher. We have checked the Bahcall and Soniera predictions in a few selected test regions during the construction of the the Guide Star Catalog data for a few regions, admittedly a slightly different data set since it included stars of all spectral types and was complete to 14.5 magnitudes, and so far the results matched. The contamination by giants starts to be a serious problem at galactic latitude 20 degrees and will be worse at lower latitudes, but perhaps there a general survey would be more appropriate than a targeted search because of the star density.

In summary, Tycho, sometime in the first half of the 1990's, will enlarge the SETI observing list, with some contamination by giants, up to several tens of thousands of stars. I consider this an appropriate size program for a targeted SETI search beginning in the mid to late 1990's. This is the best result from the surveys which I know are in progress or in planning.

A more extensive target list than that from Tycho, but also with no way to recognize and remove the background giant stars, is a proposed survey which would be a joint project of California Institute of Technology and the U. S. Naval Observatory. The survey would be based on the new Palomar sky survey, which includes three Schmidt plates with different color sensitivities for each area of the sky reachable from Palomar Observatory. It would require a fourth, short exposure plate taken of each region. The short exposure would allow transfer of the 8th magnitude reference frame to 13th or 15th magnitude stars, which could be used as the reference to reduce the positions of everything visible on the standard survey plates, limiting visual magnitudes of about 22. This project would produce a catalog of the sky, including positions, magnitude and two colors for each entry. Monet at USNO has suggested a design of a measuring engine, which effectively digitizes the Schmidt plates by photographing them with a CCD camera and would be capable of handling such a massive measuring load.

This is still a proposed project; if it is approved and completed, it will produce a catalog of "billions and billions" of stars. It would include all of the F5-K5 type stars visible in the optical out to 5 kpc. I didn't even calculate how many stars would be obtainable as a SETI target list because the list would include so many stars that it is probably better to consider a whole sky survey rather than a targeted search.

The last survey discussed here is one proposed by McGraw and colleagues at the University of Arizona, using the CCD transit instrument, which they have prototyped. It uses a mosaic of CCDs in the focal plane of the telescope and lets the sky transit across them. By observing successive nights with different filters and in different declination strips, they build up an image of the sky in several bandpasses. Their interest is not in just surveying the sky, but in finding pre-maximum supernovae, since they especially will note any photometric variations between observations. Their current prototype uses one CCD and observes in two colors, B and V, at a fixed declination, which just happens to include the Coma cluster.

What I think is most important about the CCD transit instrument is it's implications for planning future surveys. When I asked McGraw about using his data to survey the sky to 15th magnitude (which would include near-solar type stars out to 360 pc), his concern was that 16th visual magnitude is almost too bright for their instrument. Thus, any survey method using this technology, for example using narrow band filters, will initially not need to worry about being photon limited.

The major limitation in the surveys described above is the lack of determination of the luminosity class of the stars. Without it, all of the observing lists of SETI targets will be contaminated by giant stars of the proper surface temperature, but well beyond the search volume. To determine the luminosity class of the survey stars requires that, in addition to determining broadband magnitudes of the stars, we make some specific intermediate and narrow band measurements. These would give not only a lot of information on the luminosity class and spectral type, but, if the system were carefully defined, also might provide other information such as an estimate of the composition and age of the star. The McGraw system provides a prototype for this type of survey, one which is not only automated and removed from the inefficiencies of photographic plates, but apparently can use narrow band filters easily. A serious program would require at least two dedicated telescopes, and would still not be able to cover the poles, but has incredible possibilities in the future searches for our neighbors in the solar neighborhood.

Sullivan: If you really have a million identified stars of a certain B-V value, then, if we think in radio astronomy terms, a list of a million corresponds to a 12 arcmin beam. That's the kind of thing we are dealing with in microwave with a typical dish. So that's the point at which you're looking at all-sky surveys instead of targeted surveys. The other point I wanted to make is if you're in the galactic plane, for which I mean plus or minus a few degrees, you can take any random 10 arcmin position and the nearest F, G, or K star will be about 40 pc away. So if you concentrate on the Milky Way, the catalogs aren't needed either in some sense unless you really are going to use the information about that star, for instance if you have fantastic radial velocity information and you can see that it has a planet going around it.

Voice: A quick calculation says that billions and billions is closer to 4.5 billion. Elaborating on that, it's about one star for every arcsec of sky.

Sullivan: That's well beyond the limit I was talking about.

Voice: That's great though. I think that if we ever find something, we'd like to know where it's coming from. We'd like to look at the candidates.

As part of constructing the Guide Star Catalog, we have essentially digitized the sky. In fact on the digitized tapes you can see almost all of the information on the Schmidt plate. We did the digitalization at 25 micron resolution and the limit on the plates is about 18 microns. That digital data we are hoping to someday make available on an optical disk. Our hope is to attach it to a trackball, so that when you get a detection you just dial it in and that part of the sky will roll by.

Voice: That would be sort of like—Paul Horowitz talked about the smart guys out there correcting for everything. This would be the first step toward this. We would be the smart guys and correct for ourselves.

Tom: I think the ecological limit of the galaxy will probably put beacons off planetary surfaces, far away from their stars. So maybe you should look in between the stars.

Tarter: But you do because your beam is so much larger than the image or the planetary system.

Voice: Look at the transport costs—cheap photons out there arbitrarily as large as you wish.

Seeger: That's interesting.

Oliver: Not if you're far away from the star.

Tarter: George, I have a statistical anomaly I would like to mention. Today I have seen two viewgraphs which have page numbers on them and David Frisch gave me another which also had a page number on it. Those are the only numbers I have seen. And they have all been 41. As all of you know, the answer to life the universe and everything is 42, so we're doing something wrong.

I'd say we're getting very close.

Woodruff T. Sullivan, III
Department of Astronomy
University of Washington

Dr. Sullivan's contribution was similar to his publication, with Kenneth J. Mighell, of "A Milky Way Search Strategy for Extraterrestrial Intelligence," Icarus 60, 675-684 (1984), to which the reader is referred.

Discussion of Dr. Sullivan's contribution follows.

G. Verschuur: The ambient cosmic ray background, the gamma ray background, and the X-ray background are all far greater in the central region of the galaxy, so it is my own belief that there is a zone of avoidance for life, which may be the inner eight kiloparsecs.

That is quite correct. If you believe in any kind of habitable zones or uninhabitable zones in the Milky Way, then clearly it greatly affects any search, but I don't think we really have any evidence to go on there.

G. Verschuur: We have the data on the background.

But suppose the emission is mainly coming from a ring. Then when you're looking in a certain direction only part of your line-of-sight column is not good, but there is a whole lot that is still good.

T. K. Menon: We don't know what role the background plays in the origin and maintenance of life.

G. Verschuur: Except to argue that the environment of other living organisms will have to be relatively similar to ours, whether that is their planetary, solar system, or galactic environment.

We still don't know whether the gamma rays there are really strong enough to inhibit [or perhaps even nurture] the evolution of life.

J. Tarter: You make the point that your analysis is good for anything that isn't subject to any kind of opacity in the intervening medium, but the thing that we've learned from looking at the millisecond pulsar is that there is a deep fading--I believe Don Backer's result is that about fifty percent of the time the source is reduced on the order of a factor two. Obviously it is a distance-dependent thing, and so there really needs to be some effective optical depth.

I haven't looked at the intensity scintillations, only at the bandwidth effect.

J. Tarter: But in fact it is fading. On the other hand, it gets coherent too, and you can gain from that.

J. Broderick: Did your calculations take into account differences between Population I and Population II, or did you treat them the same?

It turns out not to be too important because Population II is only ten percent of all the stars in the galaxy. But we put them all in properly.

J. Broderick: So that most of what you are doing is on Population I?

That's the way the Galaxy is, but we did put in Population II.

J. Broderick: What about globular clusters?

No, I didn't put in two hundred concentrations of Population II stars. Clearly that is an example of a class of target you might have a special interest in, although there's always the argument regarding the lack of heavy elements [needed for our type of life] in globular clusters.

S. von Hoerner: Someone once mentioned that the probability of finding interstellar communication may not be proportional to the density of stars, but maybe to the square of the density, because the nearer you are to each other the more likely you will get contact. So that maybe if there is a galactic club, they may have developed in the densest part. On the other side, it should not be too dense, otherwise stable planetary orbits don't last very long since the density of stars is too high.

Or maybe internecine warfare causes civilizations to be less common, as in the game called Life where too much density around a spot causes it to disappear. Of course if the probability of civilizations goes as a higher power of density, then the Milky Way search strategy is even more heavily favored.

IV.

CURRENT PROGRAMS

SETI Observations Worldwide

Jill C. Tarter
University of California at Berkeley
&
SETI Institute

Everybody who was at the IAU Commission 51 Symposium in Boston last summer can go get another cup of coffee. This is basically the same talk and I don't know whether to be discouraged or encouraged by the fact that things haven't changed within a year. Actually, there's one change to make on my viewgraph from last year and that's in this dedicated search column, I can now take out the question mark I had following Bob Stevens name because Bob has climb down from the poles on which he used to make telephone calls and has convinced us that, in fact, he has been able to surmount a large number of difficulties and has begun operations. I'm sure given any success in raising funds he will continue for some time. I've gotten a little ahead of myself with that. Nothing much has changed in a year, and that's why when George and Ken wrote asking if I wanted to give a talk, I gave them a choice of two topics. One is this one which I figured everybody knew about, the other one was, "What's a Nice Signal Like You Doing in a Noisy Place Like This?", and somehow that just turned them off.

K. Kellermann: That's okay, you can do that one if you want.

In 1978 in Montreal, Ben Zuckerman gave a very nice summary of SETI observations that had been done up until that date and so I'm going to take off from there because that proceeding has been published and I think you all have had a chance to read it. The problem with Ben's report then, the problem with my report last summer, and the problem that remains today with my report to you is that I can't fill you in on what the Russians are doing. The last data or hard information we have from them is the result of a meeting in Tallinn, Estonia in 1981. I don't know what has happened with the plans that we heard about then, so anyone who has been there and knows anything should help me out. Ah, Michael Papagiannis, do you know what Russia's doing?

M. Papagiannis: Yeah, I went to Moscow last summer after a meeting in Boston and I had several meetings with Kardashev, Troitsky, and Slysh. Troitsky, as a matter of fact, came down to Moscow just because I was there and I was giving a report on our Boston SETI symposium at the Space Observatory Academy. I was told that they had done nothing, so where you have parentheses in your table you might as well wipe it out.

Kelly Beatty from Sky Telescope is going to Moscow in two weeks and he has promised to try and follow up there as well.

M. Papagiannis: What has happened is that Troitsky was sick for a long time. This idea of setting up all these antennas never materialized and they don't have any plans of doing it and, in general, there doesn't seem to be any interest in radio search. Part of the problem as Slysh told me candidly (a little bit on the one-to-one bases) was that Shklovskii, who was *the head of the Astrophysics Division of the Institute, was very much against it and nobody dared to go against the boss, so therefore the only interest of the Russians has been essentially in infrared searches for super civilizations; I received two papers for the IAU proceedings, one from Kardashev and one from Slysh, and they both deal with this type of super civilization approach, so the radio searches are not anymore in vogue.*

But the infrared searches, are they theoretical or are they doing something?

M. Papagiannis: *Well, the one by Kardashev is kind of theoretical in some sense, what could you expect, but the one of Slysh has taken actual IRAS data and has tried to see if there are some cases that could be ETI's.*

Using the IRAS data base?

M. Papagiannis: *Yes.*

Could I get a copy of that?

M. Papagiannis: *Yes, I have them both getting ready.*

Voice: *Maybe they have found a signal and they're not telling us.*

Voice: *No, no.*

Because we have talked to our Russian colleagues and have seen how dispirited they are in their ability to proceed.

C. Seeger: *You can always pose conspiracies, but it's futile, absolutely futile.*

Well, now that Michael has told you what the situation in Russia is, let me tell you what I know about what's been going on in the rest of the world in the past six years. I routinely keep an archive of SETI searches that I'm aware of and at this moment there are 45 entries in that archive and 25 of them are in the last six-year period. I think that the rapid increase in the entries in the archive, which starts with Frank's first observation in 1960, indicates a growing public interest in SETI and a growing scientific legitimacy. Observers are now not quite so reluctant to put their reputation (or whatever) on the line and request telescope time or request funds to build SETI specific instrumentation, and so more is being done. I like to categorize the types of searches that are being made into three classes, DIRECTED, SHARED, and DEDICATED. Six years ago, with the exception of Ohio State University which is a dedicated SETI facility, there was only need for one column, that is DIRECTED: SETI observations made by requesting time on a facility and choosing the direction and the frequencies, and the mode of operation of the receiver for SETI purposes. Since then, there has been an increase in the number of dedicated facilities, and there has also been the rise of an activity which I call SHARED, sometimes called parasitic or piggy-back.[1] This results from the fact that people generally recognize that SETI is a long shot concept by saying, "Why don't we do something which is least costly, in the exchange currency of the astronomical community, that is, telescope time? Why don't we see if there's some way to use data already bought and paid for? Or data that is being gathered for another prime purpose, but which also makes sense for SETI." So a number of activities that come under this category of SHARED have sprung up. The other thing that's changed in the 6 years since Ben's report is that when Ben reported, he told you about observations made in the U.S.A. and Canada, and in the Soviet Union, and in these past few years, we've been able to add France and the Netherlands, Germany and Australia. This worldwide interest and willingness to allow observers to have telescope time is very important to SETI and in fact has been used for the benefit of SETI, as a real form of political currency. We've availed ourselves of the worldwide interest in promoting the NASA program; it's important to the politicians that this is an activity which can be demonstrated to have some international support.

M. Papagiannis: May I interrupt for a moment? I think that Australia is not now added on your own list; you have, I think, an earlier Australian work that was done by Ken Kellermann.

That's true, but Ben didn't report on it.

M. Papagiannis: So, Australia has been on the list for a long time.

Thank you, Michael. In fact, it is in my archive. I have about 15 copies.

M. Papagiannis: It is in the archive, that's where I picked it up.

I have about 15 copies in the archive if anybody wants it. Michael, you're absolutely correct. Ken pointed out to me that he did one of the very earliest SETI searches and it was done in Australia, but I was making a grammatical structure, which referred to what Ben Zuckerman reported, and he didn't mention it. Thank you. In fact, should we go over Ken's experiment since I wasn't otherwise planning to? We have a reference here for Kellermann in 1966 done at CSIRO using a 64 m telescope and many frequencies between 350 megahertz and 5 gigahertz. It was a continuum search looking for a notch in the spectrum of the galaxy 1934-63 which would have been an indication of extraterrestrial super civilization engineering such as Nick Kardashev had predicted. I'll change the wording of my opening statement, thank you, Michael. Australia has been on the books for longer than 6 years but we've at least brought in France, Germany and the Netherlands and additional searches in Australia.

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[1] Dr. John Billingham informs me that such searches should really be called "Commensal" because parasitic implies negative interaction between the host and the parasite.

M. Papagiannis: By the way, I wanted to ask you also, is it true that no radio telescope in England has ever been used for SETI work? Because I went through that list and I couldn't find England at all.

We did use the Jodrell Bank telescope for a SETI experiment but it was really part of another RFI experiment and I haven't included it on the list because we didn't actually search with it. We used the JPL spectrum analyzer in a van at Jodrell Bank but the particular program we were doing at the time (except for looking at Vega which had just been reported by IRAS as having a ring around it), was a series of horizon scans to look at what the 21 centimeter interference situation really is. It's something we can't do at the DSN because they don't have an L-band receiver there. I haven't included that activity in the archive because it wasn't really a legitimate SETI search and I'm not aware of any other use of British telescopes.

G. Verschuur: I grew up in the age when it was extremely non-U to do SETI in the 60's; whenever I was on the Jodrell Bank telescope I had always an audio monitor going, just in case. That's how I got involved. There's no data, except I didn't hear anything except one-way radar.

The name of the game has been and always will be (barring an infinite amount of money and an infinite amount of time) finding some way to define a search strategy that limits the very large problems that you're facing. The limitations that you need to place on your search. The limits may be as mild as saying that the microwave region of the spectrum is the quietest place and therefore most worthy of a systematic search for evidence of extraterrestrial technology. Or the limit may be as extreme as saying, "I've got to get the frequency right to within 3 one hundredths of a hertz. They're going to do it for me as long as I take care of my end." This is what Paul Horowitz has done and there's a whole spectrum of strategies in between those two which serve to define and delimit a doable project. The increase in the numbers of both the DEDICATED and the SHARED contributions in this archive in part has resulted from the fact that people are now also willing to put some effort in making SETI specific hardware, not just using bastardized radioastronomy receivers in some strange configuration. They're really confident enough of their particular strategy to invest whatever funding level is appropriate into making specialized hardware they need for SETI. It's time to stop the generalizations and get started on a brief description of the different search strategies.

I'll go through the DIRECTED searches first. The first observation that occurred that Ben didn't report on was done by Woody Sullivan and Steve Knowles in 1978. It was done at Arecibo using a Mark I VLBI tape recorder in a single station VLBI mode, basically using the tape as a fast bit bucket. If you're willing to do long Fourier transforms after the fact, you can get out of this one-bit sampled data a 5 hertz spectral resolution. The limiting assumption or hypothesis that Knowles and Sullivan made was that the interesting frequency range was 100-500 megahertz, which is the frequency range that is characteristic of our television transmission. They went eavesdropping on a couple of nearby stars attempting to find either the analogs of our TV usage for that civilization's own purposes or perhaps transponded signals that would be echoes of our own early TV transmission that were being sent back to us. "Hey, we know you're there!" The thing that they pointed out was that given the detection sensitivity of their equipment, a successful eavesdrop on extraterrestrial TV would have required a power associated with their TV transmissions that is quite a bit greater than we typically use. You can't really detect the megawatt or so of power in our typical TV transmissions to the distance of the nearest star. But Knowles and Sullivan were postulating perhaps either a transponded signal or a strong signal intended for usage of the other civilization which might be in that frequency range.

W. Sullivan: Jill, could I make a comment? I wouldn't put the emphasis quite so much on TV, the idea was just non-magic frequency, low frequency, vhf kind of thing that has been rather ignored, because, of course, it does have the disadvantage of the high galactic background, if you pick a couple of nearby stars out of the galactic plane, it's not as bad, nevertheless, you are sacrificing sensitivity because of that. That, I think, was the thing as opposed to it might be leaking. Or it might be a beacon also with some magic frequency that we hadn't thought of.

Okay. With respect to the transponding concept, there are currently, in this year, 121 stars that are close enough to our Sun to have picked up the earliest TV transmitters and have transponded a return signal that we should have heard by now; an interesting number.

The next magic frequency work that got done was by myself, Tom Clark, Jeff Cuzzi, and Bob Duquet. We had also previously done the same sort of VLBI bit bucket Mark I tape recorder project here at NRAO a few years before and thereby convinced Arecibo that they wanted to build us a one-bit sampler to do a more extensive project at Arecibo. In fact, they did that and we looked at 210 stars with 5 hertz resolution, 4 megahertz worth of frequency coverage around both the hydrogen line and hydroxyl lines. In all we collected about 3.4 million channels per star at 5 hertz resolution. Years of NASA-Ames computing time later we had gone through all of the data tapes and found that we could explain almost all of the 'birdy' signals that we detected on these tapes after the fact. There were two exceptions, in one of which my "off source" reference tape broke while on the computer and so I was never able to get that particular spectrum. In another case the quality of the tape initially was so poor that we didn't have a really reliable reference. These two sources plus a list of about 40 sources that Arecibo can see, but that we did not observe, remain to be done when and if we take the NASA prototype multi-channel spectrum analyzer to Arecibo.

Paul Horowitz told you yesterday, I think it was part 2 or maybe it was part 3, about "Suitcase SETI" which is the all-time winner in terms of the magic frequency hypothesis. In 1982 Horowitz, Teague, Linscott, Chen and Backus observed 250 stars at Arecibo with .03 Hz resolution in 128K channels. This is the narrowest frequency work that has been done and has incredibly good sensitivity. It's about the best job that you can do, with current technology, to test the hypothesis of a single magic frequency. As you know from Paul's report, the "Suitcase SETI" has been rephoenixed as Project Sentinel and is still alive and well at Oak Ridge. It's about to be outdone by the next generation of "META-SETI."

There is another piece of instrumentation that has been built in the past few years specifically for doing SETI by an observatory, and that's at Nancay. They built a 1024 channel correlator with 3-bits and 8 levels.

They allowed a number of frequency divisions in order to clock this device down, the last resolution of which they allowed to be defined for SETI use. A practical number came out to be 50 hertz to bin width. In 1981 I started a program there with Francis Biraud concentrating once again on the region between the 1665 and 1667 MHz OH line and, in addition, another 4 megahertz surrounding the HI line. We've been working away at a list of 320 stars that are solar types from the RGO catalog that are visible from Nancay with successive runs getting us better and better sensitivity. Since the equipment over there has been improved, we will continue this activity. Our most recent observing run was last August. It's provided us with some really very nice data for looking at the noise statistics of the observatory environment. We've been looking at what you really have to do in data processing in the real world when you're analyzing 6 million channels worth of data in order to get a false alarm rate that is as close as possible to the false alarm rate that you predict theoretically. It's a difficult job.

Another magic frequency choice, which was set, I think, not so much by philosophy as by technology, was a program done by Steve Lord and O'Dea at U. Mass. They chose the CO line as their magic frequency and they further delimited the search space by choosing a magic location. They scanned the north galactic rotation axis thinking that it was a rather unique place in the galaxy. Some super civilization might have located their transmitter along that axis and chosen CO as their choice of magic frequency. There is also a good receiver at U. Mass. and that influenced the observational strategy.

There's another special place in the galaxy, or rather there is one particular special place in the galaxy. Seth Shostak and myself used the Westerbork interferometer to investigate the center of the galaxy. The rationale being that this might be the one place where a beacon was erected. But again, it requires a super civilization and an advanced technology. We used the interferometer because the galactic center is an incredibly noisy place. There is an enormous amount of radiation coming from a very small volume and the interferometer helped as a filter for that background. We further argued that for any signal to be visible against that background, a pulsed format would enhance the detection probability. We looked for pulsed signals coming from the galactic center with repetition rates between 40 seconds and one hour. They had to be relatively narrow band pulses, less than 3 kilohertz in width, and their flux had to be greater than 20 milli-Janskys to have been detected. We detected nothing.

Another "special place" kind of observation refers to a special place very close to home. That's a series of observations conducted at optical frequencies using a 30-inch telescope at the Leuschner Observatory, Berkeley, California and the 24-inch at Kitt Peak. It was done by Freitas and Valdez in 1979 and 1983. Their special strategy was that they were going to examine the libration points of the earth-moon-Sun system. They were looking for artifacts that had been parked in these relatively stable orbits, requiring very little station keeping to keep them there for a long time, during which a probe might sit patiently and wait for evidence of an intelligent technology arising on the surface of our planet. They looked for reflected light on something like 137 photographic plates and were able to place limits on any kilometer size shiny bodies but could not do much better than that. You could still have many little dark balls out there if their microtechnology is good and the probes are camouflaged.

These same investigators also had another scheme, one based on magic frequencies now, not magic positions. This time it was a magic frequency of 1516 megahertz; that was a new one to me. It turns out to be the tritium equivalent of the 21 centimeter line. Their reasoning was as follows. Since the half life of the tritium isotope is so short, any detectable amount of tritium in the vicinity of a nearby solar type star probably can't be explained by natural astrophysical production. It therefore might be evidence of some civilization who was using nuclear fusion as an energy source. They failed to detect any tritium. The minimum detectable level of tritium concentration was set by the hardware at Hat Creek, and it's impossible to assess whether a non-detection means anything or not. It's almost impossible to predict what the relevant amount of tritium would be. This is one of the many experiments which, had it been successful, would have shown something, but the failure to detect anything really doesn't tell you much.

W. Sullivan: What frequency was that, Jill?

1516 megahertz.

The last search in the DIRECTED column is a Russian one called MANIA. It's an acronymn which actually starts out with "megacannaly." It refers to an optical system that's on a 6 meter optical telescope. The device is looking for very fast optical pulses and looking at some 21 "peculiar" objects, which as far as I can determine are BL Lac objects. Once again the search is looking for super civilizations.

Alright, that finishes the DEDICATED column. The SHARED column asks, "How do you use data that's already bought and paid for?" The Dutch at Westerbork are the only members of our profession that I can imagine would try to do this, but they have tried to save all of the dirty maps ever created by the Westerbork interferometer. If you go through that data base and remove fields that have strong sources in them will cause enormous grating rings. You can go through the remaining fields and you can ask a question, "Are there any excess noise peaks, that you could otherwise clean out and not be interested in, that happen to be coincident with the position of any solar type stars?" Frank Israel and I did a cross-correlation between stars pulled from the FGK 3 catalog and 130 of these Westerbork fields. For the several hundred stars within these fields, we search for a positional coincidence with a noise peak within a fraction of a beam width. We didn't find anything that we couldn't explain as being excess noise from a piece of a grating ring.

There is an old suggestion by Tom Gold that an ET civilization sitting at one side of a maser cloud might decide to take advantage of the free amplification in space they could get by transmitting through the maser. Anyone in the line of sight on the other side of the maser would then have a very detectable signal. Cohen, Malkan and Dickey used a big survey that they had done to look for correlations between globular clusters and OH and water masers. They argued that at any instant there were about 10^5 stars in their telescope beam. So why not go back and look at the velocity extremes where they would not expect to find natural maser features coming from the globular cluster and look for artificially generated signals being amplified by the masers in space. They found nothing.

You heard a little bit from Tom Bania about SERENDIP 2. Actually there's already been a SERENDIP 2; I think we should call the Arecibo system the Son of SERENDIP! SERENDIP is a black box, the prototype or the first copy of which sat on the back of the Hat Creek Radio Observatory Telescope and then was taken to JPL; Sam Gulkis and Ed Olsen were involved at that point using the 64 meter. Its first incarnation was as a hundred channel spectrum analyzer with 2.5 kilohertz resolution per channel. The idea here is that radio astronomers typically look in places that are of interest to SETI. Why not just let the radio astronomers choose the place where the telescope is pointed and a frequency at which they are observing and then piggyback a device capable of doing narrowband analysis of whatever's coming through the IF? The original activity was carried out for about three years. The real problem is that the processing time after you have collected the heavily thresholded data is many times realtime! The detective work that is involved to slough out what the signal was is quite a convoluted process. This task is one of the most difficult things in the new system that's being designed for Arecibo and will have 65,000 channels.

Wielebinski and Seiradakis have, on occasion, inserted into a standard pulsar searching routine the positions of stellar type stars. As far as the pulsar hardware is concerned, it would have responded to a pulse coming from the star as well as a pulsar. This really is just an add-on to a program that's already in progress.

Sometimes telescopes break and you can't use them for doing what they were intended to do, but you can still use them for SETI. In one instance, it was the 64 meter telescope at Tidbinbilla, Australia. It was shut down for work on the support pedestal for an extended period of time, but the receivers were still viable. The telescope could not be driven around the sky, but it could be parked at a different declination strip for many days at a time. Gulkis, Kuiper, Olsen, Jauncey and Peters used funding from the Planetary Society to develop a small 256 channel spectrum analyzer and made a drift scan sky survey of a limited declination range for quite a bit of time. In fact, that instrument is still in place and whenever the telescope is unavailable for normal observations or not busy doing other things, it can be used in this mode again.

Considering the column called DEDICATED, we heard about Ohio State yesterday and Sentinel, we heard about the small SETI Observatory, we heard about Bob Stevens work in CAnada. This entry called AM-SETI is an amateur group of radio hams who have become interested in using satellite receivers and their home computers as SETI detectors. They have been advised by people like Kent Cullers, Charlie Seeger, and John Wolfe at NASA-Ames on the best way to build cooled FET amplifiers and do FFT's on their home Apple. There was one group of people who actually put together some equipment, but now seem to have disbanded. Kent tells me there is new interest in this activity in the Silicon Valley. The question I personally have with respect to this amateur effort is how any kind of record of what has been done will ever be organized, and what kind of archiving and what kind of staying power will these individuals have? So the question mark remains on my list for good reason.

Coming up, what's on the horizon for the future? You heard about the NASA SETI program, the SERENDIP box to go on line at Arecibo sometime next year, and META-SETI due for dedication in the fall of this year. That's the sum total of everything I'm aware of and I'd be happy to hear about anything I haven't mentioned as yet. (Do you have an opinion about the Arecibo data, what fraction of the observing time which is allocated for other purposes altogether is fruitful for SETI?) I would say, I've been having this argument with Tom Bania about how to get all the data that he might provide given the fact that we can only take in 130 kilohertz of the IF at one time. I'd say I'd like to use Arecibo all the time it isn't doing atmospheric work or radar work and that's about 70% of the time.

K. Cullers: I just wanted to make a comment, Jill, about the amateur SETI effort. I've been selling it to the people who have talked to me as a learning experience and very little more. The good news is I haven't come across any nuts yet that have recorded one discovery a week. The bad news is that it is hard to keep them organized, and if they don't want to keep a record, they don't. They get interested for a few weeks, they put together some stuff and if it doesn't work out, they say the heck with it, they go do something else. It looks like there may be a few people a little more interested now, but we'll see.

D. Frisch: Jill, the repetition times again when you looked toward the galactic center.

Somewhere between 40 seconds and an hour.

D. Frisch: That falls in a natural gap, between 6 seconds and 800 seconds.

How many powers in 1/137 seconds?

D. Frisch: 9th and 10th but the second and third above the Drake-Helou limit.

Note in proof: Time precluded discussion of SHARED searches by Witteborn, Valle and Simard-Normandin and Damashek. Parameters for these and all other searches may be found in the archive attached as an appendix.

TABLE A.1

SETI SEARCHES

DIRECTED	SHARED	DEDICATED
KNOWLES & SULLIVAN (1978)	ISRAEL & DERUITER (1975-79) ISRAEL & TARTER (1981)	OSURO (1973 -)
FREITAS & VALDES (1979) SETA (1981-82)	COHEN et al (1978)	SENTINEL (1983 -)
TARTER et al (1979-81)	COLE & EKERS (1979)	SMALL SETI OBSERVATORY (1983 -)
LORD & O'DEA (1981)	SERENDIP (1976-79) SERENDIP II (1979)	AM-SETI (1983 - ?)
BIRAUD & TARTER (1981)	WIELEBINSKI & SEIRADAKIS (1977 -)	STEPHENS (1983 - ?)
SIGNAL (1981)	WITTEBORN (1980)	TROITSKIJ et al. (1983 - ?)
SUITCASE SETI (1982)	MERIDIAN CENTRAL GALACTIQUE (1982 -)	[SOVIET DIPOLE NETWORK]
VALDES & FREITAS (1983)	DAMASHEK (1983)	
[MANIA]	GULKIS (1983 -)	
	FUTURE PROGRAMS	
NASA's SETI Program SKY SURVEY & TARGET SEARCH (1988)	SERENDIP II (SON OF SERENDIP) (1986)	META-SETI (SEPTEMBER 1985)
	IRAS DATA BASE SEARCHES FOR DYSON SPHERES	

SUMMARY OF SETI OBSERVING PROGRAMS (JUNE 1984)

DATE	OBSERVER	SITE	INSTR. SIZE(m)	SEARCH FREQ. (MHz)	FREQ. RESOL. (Hz)	OBJECTS	FLUX LIMITS (W/m²)	TOTAL HOURS	COMMENTS	REF.
1960	DRAKE "OZMA"	NRAO	26	1420-1420.4	100	2 STARS	4.E-22 **	400	Single channel receiver.	1.
1963	KARDASHEV & SHOLOMITSKII	CRIMEA DEEP SPACE STATION		920	10 MHz	QUASAR			Reported detection of CTA102 as possible Type III civilization.	20.
1966	KELLERMANN	CSIRO	64	MANY, BETWEEN 350 & 5000	FULL BANDWIDTH FOR EACH FEED	1 GALAXY	± .5FU	---	No "notch" of ETI origin was detected in galaxy 1934-63.	2.
1968 & 1969	TROITSKII, GERSHTEIN, STARO-DUBTSEV, RAKHLIN	ZIMENKIE, USSR	13	926-928 & 1421-1423	13	12 STARS	2.E-21 **	11	25 Channels with F=13 Hz were spaced 4 KHz apart: coverage not continuous.	3.
1968 & ON	TROITSKII	GORKY	DIPOLE	21 cm 50 cm 1 m	---	ALL SKY SEARCH		CONT	Search over all sky visible by single dipole.	24.
1970 & ON	TROITSKII, BONDAR, STARO-DUBTSEV	GORKY, CRIMEA, MURMANSK, USSURI	DIPOLE	1863 & 927 600	---	ALL SKY SEARCH FOR SPORADIC PULSES	1.E+4 FU	700 & CONT. AT 50% TIME	Network of isotropic detectors: cross correlation from 2 or 4 sites over 8000 km.	4.
1970 TO 1972	SLYSH	NANCAY	40x240	1667	20 kHz	10 NEAREST STARS				

SUMMARY OF SETI OBSERVING PROGRAMS (JUNE 1984)

DATE	OBSERVER	SITE	INSTR. SIZE(m)	SEARCH FREQ. (MHz)	FREQ. RESOL. (Hz)	OBJECTS	FLUX LIMITS (W/m^2)	TOTAL HOURS	COMMENTS	REF.
1971 & 1972	VERSCHUUR "OZPA"	NRAO	91 & 43	1419.8 - 1421 & 1410-1430	490 & 6900	9 STARS	5.E-24 & 2.E-23	13	384 channel correlator on-line.	5.
1972	KARDASHEV & STEINBERG	CAUCASUS, PAMIR, KAMCHATKA, MARS PROBE		40 - 500	2 MHz	OMNI-DIRECTIONAL			"Eavesdropping" search for pulses.	
1972 TO 1976	PALMER, ZUCKERMAN "OZMA II"	NRAO	91	1413-1425 & 1420.1- 1420.7	$6.4x10^4$ & 4,000	674 STARS	1.E-23 **	500	384 channel correlator on-line.	6.
1972 & ON	KARDASHEV, GINDILIS	EURASIAN NETWORK, INST. FOR COSMIC RADIATION	DIPOLE	1337-1863	---	ALL SKY SEARCH FOR SPORADIC PULSES	1.E+4 FU	---	2 or more sites operating simultaneously.	7.
1973 & ON	DIXON, EHMAN, RAUB, KRAUS	OSURO	53	1420.4 REL. TO GAL. CEN. ± 250 kHz	10 & 1 kHz	ALL SKY SEARCH	1.5E-21 **	CONT.	Receiver is tuned to hydrogen rest frequency relative to Gal. Cen. (as a function of direction).	8.
1974 TO 1976	BRIDLE, FELDMAN "QUI APPELLE?"	ARO	46	22235.08 ± 5 MHz	$3x10^4$	70 STARS	1.E-22 **	140	70 solar type stars within 45 lt yrs have been observed to date.	

SUMMARY OF SETI OBSERVING PROGRAMS (JUNE 1984)

DATE	OBSERVER	SITE	INSTR. SIZE(m)	SEARCH FREQ. (MHz)	FREQ. RESOL. (Hz)	OBJECTS	FLUX LIMITS (W/m^2)	TOTAL HOURS	COMMENTS	REF.
1974	WISHNIA	"COPERNICUS" SATELLITE	1	3.E9	---	3 STARS	---	---	Search for UV laser lines.	9.
1974 & ON	SHVARTSMAN "MANIA"	ZELENCHUK-SKAYA	6	5500	$\Delta\lambda < 10^{-5}$ Å	21 PECULIAR OBJECTS			Optical search for short pulses of length 3×10^{-7} to 300 seconds, and narrow laser lines.	21.
1975 & 1976	DRAKE, SAGAN	NAIC	305	1420 & 1667 & 2380 B = 3 MHz	1,000	4 GALAXIES	3.E-25 **	100	Search for type II civilizations in local group galaxies.	10.
1975 TO 1979	ISRAEL, DE RUITER	WRST	1500 MAX BASELINE	1415	4×10^6	50 STAR FIELDS	2.E-23 **	400	Searches of "cleaned" maps prepared for the WSRT background survey, looked for positional coincidence between residual signals and AGK2 stars.	
1976 & ON	BOWYER et al U. C. BERKELEY "SERENDIP"	HCRO	26	1410-1430 & 1653-1673	2500	ALL SKY SURVEY	5.E-22 **	---	Automated survey parasitic to radio astronomical observations.	11.

SUMMARY OF SETI OBSERVING PROGRAMS (JUNE 1984)

DATE	OBSERVER	SITE	INSTR. SIZE(m)	SEARCH FREQ. (MHz)	FREQ. RESOL. (Hz)	OBJECTS	FLUX LIMITS (W/m^2)	TOTAL HOURS	COMMENTS	REF.
1976	CLARK, BLACK, CUZZI, TARTER	NRAO	43	8522-8523 *	5	4 STARS	2.E-24 **	7	VLBI High speed tape recorder combined with software direct Fourier transformation to produce extreme frequency resolution (non-real time).	
1977	BLACK, CLARK, CUZZI, TARTER	NRAO	91	1665-1667 *	5	200 STARS	1.E-24 **	100	VLBI High speed tape recorder combined with software direct Fourier transformation to produce extreme frequency resolution (non-real time).	12.
1977	DRAKE, STULL	NAIC	305	1664-1668 *	0.5	6 STARS	1.E-26 **	10	High speed tape recorder combined with optical processor to produce extreme frequency resolution (non-real time).	13.
1977 & ON	WIELEBINSKI, SEIRADAKIS	MPIFR	100	1420	20,000,000	3 STARS	4.E-23	2	Candidate stars are inserted into ongoing program which searches for pulsed signals with periods of 0.3 to 1.5 sec.	

SUMMARY OF SETI OBSERVING PROGRAMS (JUNE 1984)

DATE	OBSERVER	SITE	INSTR. SIZE(m)	SEARCH FREQ. (MHz)	FREQ. RESOL. (Hz)	OBJECTS	FLUX LIMITS (W/m^2)	TOTAL HOURS	COMMENTS	REF.
1978	HOROWITZ	NAIC	305	1420 +/- 500 Hz	0.015	185 STARS	8.E-28 **	80	Assumes that signal frequency was corrected at the source to arrive at rest in Heliocentric or barycentric laboratory frame.	14.
1978	COHEN, MALKAN, DICKEY	NAIC HRO CSIRO	305 36 63	1665+1667 22235.08 1612.231	9500 65000 4500	25 GLOBULAR CLUSTERS	1.8E-25 1.1E-22 1.5E-24 **	40 20 20	Passive search for Type II & III civilizations using astronomical data originally observed to detect H_2O and OH masers in globular clusters.	15.
1978	KNOWLES, SULLIVAN	NAIC	305	130-500 (SPOT) *	1	2 STARS	2.E-24 **	5	Attempted "eaves-dropping" using MKI VLBI tapes as in Black, et. al., 1977.	16.
1979	COLE, EKERS	CSIRO	64	5000 +/- 5 MHz and +/- 1 MHz	10^7 and 10^6	NEARBY F, G AND K STARS	$4x10^{-18}$ **	50	Simultaneous pulsed events in both 2 MHz and 10 MHz filters are sought in detectors having time resolution of 4 μ seconds.	17.
1979	FREITAS, VALDES	LEUSCHNER OBSERVATORY UCB	0.76	5500A°	____	STABLE "HALO ORBITS" ABOUT L4 AND L5 LIBRATION POINTS IN EARTH-MOON SYSTEM	M_V<14	30	Attempt to discover evidence of discrete objects (such as interstellar probes) in stable orbits about L4, L5 by study of 90 photographic plates.	18.

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DATE	OBSERVER	SITE	INSTR. SIZE(m)	SEARCH FREQ. (MHz)	FREQ. RESOL. (Hz)	OBJECTS	FLUX LIMITS (W/m^2)	TOTAL HOURS	COMMENTS	REF.
1979 & ON	JPL UCB SERENDIP II	DSS 14	64	S & X BAND B = 10 MHz	19500	APPARENT POSITIONS OF NASA SPACECRAFT	8.E-24 **	400 TO DATE	Automated survey parasitic to spacecraft tracking operations using 512 channel auto-correlator and 100 channel correlator with micro-processor control.	
1979 TO 1981	TARTER, CLARK, DUQUET, LESYNA	NAIC	305	1420.4 +/- 2 MHz & 1666 +/- 2 MHz *	5 & 600	200 STARS	1.E-25 **	100	Rapid 1-bit sampler and high speed tape recorder run in parallel with 1008 channel correlator. Software direct Fourier transformation as in Black, et. al., 1977.	22.
1980	WITTEBORN	NASA- U of A MT. LEMON	1.5	8.5μ - 13.5μ	1μ	20 STARS	N MAGNITUDE EXCESS < 1.7	50	Search for IR excess due to Dyson spheres around solar type stars. Target stars were chosen because too faint for spectral type.	
1981	LORD, O'DEA	U.MASS	14	115000	20,000 125,000 $4x10^8$	NORTH GALACTIC ROTATION AXIS ℓ = 5° → 90°	1.E-21 **	50	Search for signals near J=1-0 CO line frequency from a transmitter somewhere along the Galactic rotation axis.	
1981	ISRAEL, TARTER	WRST	3000 MAX BASELINE	1420	$4x10^6$ $10x10^6$	85 STAR FIELDS	8.E-22 TO 6.E-24	600	Parasitic search similiar to Israel and De Ruiter using 'uncleaned' maps stored at Groningen and Leiden, and AGK3 catalog.	19.

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DATE	OBSERVER	SITE	INSTR. SIZE(m)	SEARCH FREQ. (MHz)	FREQ. RESOL. (Hz)	OBJECTS	FLUX LIMITS (W/m^2)	TOTAL HOURS	COMMENTS	REF.
1981 & ON	BIRAUD, TARTER	NANCAY	40x240	1665- 1667	97.5	300 STARS	1.E-23 **	80 TO DATE	8-level 1024 channel auto-correlator with stepped 1^{st} LO to extend frequency coverage at modest resolution.	
1981	SHOSTAK, TARTER "SIGNAL"	WRST	3000 MAX BASE- LINE	1420.4 REL.TO GAL.CEN. B = 156 KHz	1200	GALACTIC CENTER	1.E-24 **	4	Use of interferometer to search for pulsed signals from Galactic Center in range of periods from 40 seconds to 2 hours.	
1981 TO 1982	VALDES, FREITAS "SETA"	KPNO	0.61	5500 Å	----	EARTH-MOON L1 thru L5 SUN-EARTH L1, L2	$10 < m_v < 19$	70	Attempt to see discrete artifacts (>few m in size) in stable orbits near Lagrange points. Studied 137 IIIaF photographic plates.	23.
1982	HOROWITZ TEAGUE LINSCOTT CHEN BACKUS	NAIC "SUITCASE SETI"	305	2840.8 B=4 KHz AND 1420.4 B=2 KHz	0.03 1-LINEAR 0.03 2-CIRCULAR	250 STARS 150 STARS	4.E-26 ** 6.E-28 **	75	Dual 64K channel real time microprocessor based spectrum analyser with video archiving and swept LO frequency to test "magic frequencies".	
1982	VALLEE, SIMARD- NORMANDIN	ARO	46	10,522	185 MHz	GALACTIC CENTER MERIDIAN	1.E-19 **	72	Search for strongly polarized signals by mapping field 1/4°x25° along ℓ=0°.	
1983	HOROWITZ "SENTINEL"	OAK RIDGE (HARVARD)	26	1420.4 AND 1665.4 AND 1667.3 AND 2840.8	0.03 DUAL CIRCULAR B= 2 KHz	SKY SURVEY	8.E-26 **	CONT	"Suitcase SETI" as the backend of automated sky survey at 4 or 5 magic frequencies over a 5 year observing period.	

SUMMARY OF SETI OBSERVING PROGRAMS (JUNE 1984)

DATE	OBSERVER	SITE	INSTR. SIZE(m)	SEARCH FREQ. (MHz)	FREQ. RESOL. (Hz)	OBJECTS	FLUX LIMITS (W/m^2)	TOTAL HOURS	COMMENTS	REF.
1983	DAMASHEK	NRAO	92	390 +/-8	$2x10^6$	SKY SURVEY (PULSARS)	2.E-22	700	16 MHz sampled at 60 HZ; 8 contiguous frequency channels. Search for single dispersed pulses and telemetry (bit-stream) signals.	
1983	VALDES, FREITAS	HCRO	26	1516 +/-2.5	4.9 KHz 76	80 STARS 12 NEARBY STARS	3.E-24 ** 2.E-25	100	Search for radioactive tritium line from nuclear fusion.	
1983 & ON	GULKIS	DSS 43	64	8 GHz AND 2380 +/- 5 MHz	40 KHz	PARTIAL SOUTHERN SKY	2.E-22 **	800 & ON	Sky survey of constant declination strips (3 from -28.9 to -34.3 by April '83) whenever antenna stowed.	25.
1983 & ON	GRAY	SMALL SETI OBS.	4	1419.5 - 1421.5	40	SKY SURVEY & -27° δ	1.E-22 **	CONT	Dedicated search system constructed by amateurs, operated during evenings.	
1983 & ON	CULLERS	AMSETI	2	~1420 & < 1000	-	-	-	CONT (>1984)	Low noise GaAS FETS & micros with sattellite TV dishes, by Silicon Valley Hams, with NASA-Ames consultation.	
1983 & ON	STEPHENS	TERRESTRIAL RESEARCH INSTITUTE @ HAY RIVER NWT		1400 - 1700	30 KHz	SKY SURVEY	-	CONT	Surplus tropo-scatter dishes used as dedicated amateur SETI observatory.	
1984 ?	TROITSKIJ	USSR	1 X 100	1420	-	STARS & SKY SURVEY	1. E-19 **	CONT	Planned array of up to 100 dishes of 1m size. ????????????????????????	

*These experiments corrected frequencies for the motion of the observed stars with respect to the Local Standard of Rest.
**Quoted sensitivities refer to signal/noise ratio = 1.

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SITE ABBREVIATIONS

NRAO	National Radio Astronomy Observatory Green Bank, West Virginia Tucson, Arizona Socorro, New Mexico
CSIRO	Commonwealth Scientific and Industrial Research Organization Epping, New South Wales, Australia
NANCAY	Observatoire de Nancay Nancay, France
OSURO	Ohio State University Radio Observatory Columbus, Ohio
ARO	Algonquin Radio Observatory Ontario, Canada
NAIC	National Astronomy and Ionospheric Center - Arecibo Observatory Arecibo, Puerto Rico
WSRT	Westerbork Synthesis Radio Telescope Westerbork, The Netherlands
HCRO	Hat Creek Radio Observatory Castel, California
MPIFR	Max Planck Institut fur Radioastronomie Effelsberg, West Germany
HRO	Haystack Radio Observatory Westford, Massachusetts
	NASA Deep Space Network
DSS 14	Goldstone, California
DSS 43	Tidbinbilla, Australia
U. MASS	Five College Radio Astronomy Observatory Amherst, Massachusetts
KPNO	Kitt Peak National Observatory Tucson, Arizona

Ultra-narrowband SETI at Harvard/Smithsonian

Paul Horowitz
Harvard University

I'd like to tell you about a series of experiments I've been involved in, which all have the same theme of looking for very narrow band carriers, i.e., high resolution spectral analysis. I'll tell you about narrow band SETI (call that Part I), experiments at Arecibo (Parts II and III), what we're doing now at Harvard (Part IV), ana I think perhaps the most interesting, what we're about to do over the next year (Part V).

By "ultra narrowband SETI" I mean really going to the limit of spectral purity that a galactic carrier can have. The obvious reason you would like to match your spectrometer's resolution to the narrow bandwidth that these carriers might have is the signal-to-noise ratio improvement that comes from such a "matched filter" to the signal, imbedded in a continuum noise background. In addition, there is a not-so-obvious advantage, in the form of a surprising rejection of interference, which I'll talk about a little later. It turns out if you look at the numbers, the limits to the spectral purity of a received carrier are not technological. Even in our primitive state of development, our ability to make stable oscillators, and the accuracy with which we know our orbital ephemeris (needed in order to take out doppler terms), are both adequate to the task, and the ultimate limit to spectral purity is natural in origin, namely, multipath scattering from turbulent ionized material in the galaxy.

Now let's see, turning on this thing [the overhead projector] has been an IQ test for everybody -- you passed, look at that -- the minimum IQ to give this talk! The dominant limit to spectral purity of galactic beacons is this effect shown in figure 1, the so-called Drake and Helou "phase modulation broadening through multiple scattering in a turbulent

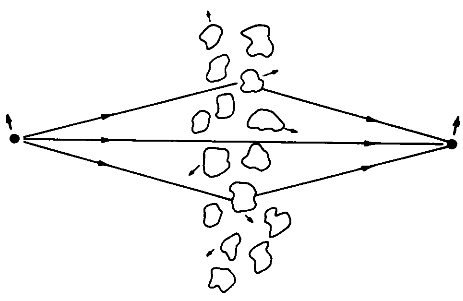

Figure 1

inhomogeneous medium," a fancy name for an intuitively straightforward effect: If you have a green man transmitting over there, and us listening over here, we see not only the direct propagation but also radiation that is scattered, with doppler shifts, from ionized gas in motion. So the net effect is to broaden the infinitely narrow carrier, spreading it out over some finite bandwidth. You can make a model, and put numbers into it based on pulsar scintillation data (caused by the same effect), and here's what you get, courtesy of Drake and Helou (figure 2). You're typically talking distances of the order of kiloparsecs, or thereabouts, and for frequencies of the order of L band (neutral hydrogen, and so on), we're talking spreading widths of the order of perhaps 0.01 - 0.1 Hz. This is the kind of thing you would receive if somebody at the other end cooperated by transmitting with their best cesium stabilized oscillators at L band, and you can't get better than this, because this is what the galaxy is doing to you.

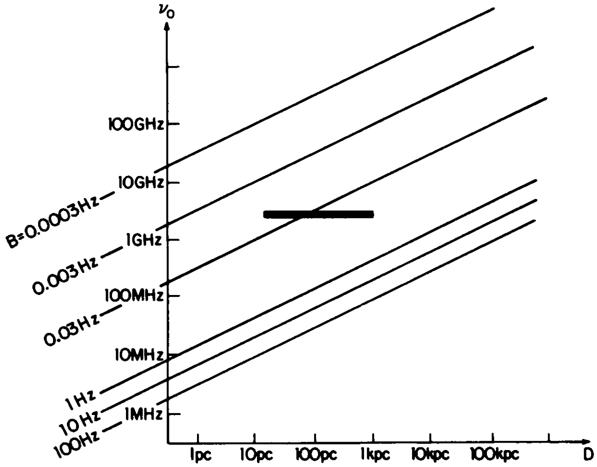

Figure 2

Now in order to have resolutions of the order of hundredths of a Hertz you are going to have to integrate a time series for the inverse of that, or one hundred seconds. And a curious thing happens when you do this -- you have to take account of the earth's motion in a rather detailed way, because you get a doppler chirp, a change of the doppler shift along the line of sight because of the earth's spin. (Although the orbital motion dominates the doppler shift itself, the spin dominates the rate of change of doppler). I apologize to those familiar faces whom I've harangued with this spiel before, but there seem to be some unfamiliar ones, so I'll go through it rather briefly. If you just take the first order doppler shift, df/f = v(radial)/c, and if that radial velocity is changing because, for instance, you're in circular motion, then you get a rate of change in received frequency given by df/dt = (f/c) dv(radial)/dt. This is maximum when you're looking overhead at the equator, in which case we could put in for dv/dt just the acceleration and we get $df/dt = \frac{f}{c}\omega^2 R_e$. Omega is the spin angular velocity here. You can put in everyone's favorite f = 1420 MHz, then df/dt turns out to be 0.16 Hz per second, so in a hundred seconds, for instance, you're going to get 16 Hz drift of a carrier. In fact, in general you have a factor of cosine of this guy (I always get latitude and colatitude mixed up) times the cosine of the viewing angle with respect to the equator. But since cosines are always of order one, the effect is always close to what we calculated, particularly if you're doing a transit instrument type of search (unless you're up there with Stevens in the chilly northwest where cosines are closer to zero).

Anyway, here's the point: We have a rate of change of received frequency of the order of 0.16 Hz per second, so if we're going to look with 0.01 Hz resolution (to match the galactic spreading), i.e., integrate for a hundred seconds, the first thing we have to do is make our receiver drift in the same way as the expected signal (or do the equivalent operation in computation) so that this signal looks narrow, not broad. But, having done that hard work, a nice thing happens, because over the hundred seconds you're observing to get 0.01 Hz bins you have a total drift of 16 Hz and that's 1600 bins. Thus if you do compensate, the celestial signal will pile up in one channel or a few channels, but now local interference will be smeared out over 1600 bins. In other words we rejected interference here by looking for the signature of a true external source.

Now let me make a couple of other brief comments about ultra-narowband spectrum analyzers, and then I'd like to show you what we've been doing at Harvard. The problem with ultra-narrowband, as we go to 0.01 Hz or thereabouts, is that you now require not megachannel or even gigachannel, but terachannel analyzers -- I had to look up to find out what that prefix was because I don't use it often -- to cover the microwave window. To cover 10 GHz at 0.01 Hz you need 10^{12} channels, that's a terachannel and that's a bit of a problem, at least in the next year and a half until Silicon Valley (or Japan, Inc.) catches up. The silver lining on this cloud is that it merely makes magic frequency strategies especially attractive; you don't want to do 10^{12} channels if you can help it (or if they can help us), so we should look at special frequencies, if we're fond of the advantages of ultra narrowband receivers. Then the total bandwidth you need is only enough to allow for doppler shifts that you can't calculate, and I'll have more to say on this later. Just to give you a feeling for that, one kilometer per second at L band turns out to be one half million channels of 0.01 Hz apiece, so you need megachannel analyzers just to handle doppler uncetainties of kilometers per second.

Part II is called "Arecibo, 1978". I first got involved in this dubious sort of activity at Drake's suggestion when I had a sabbatical in 1978. I went down there with nothing clear in mind, but wound up implementing an ultra-narrowband search off-line, because there was no hardware to do Fourier transforms on very long time series. The basic idea (Figure 3) was just to use a straightforward double conversion receiver, but one in which the local

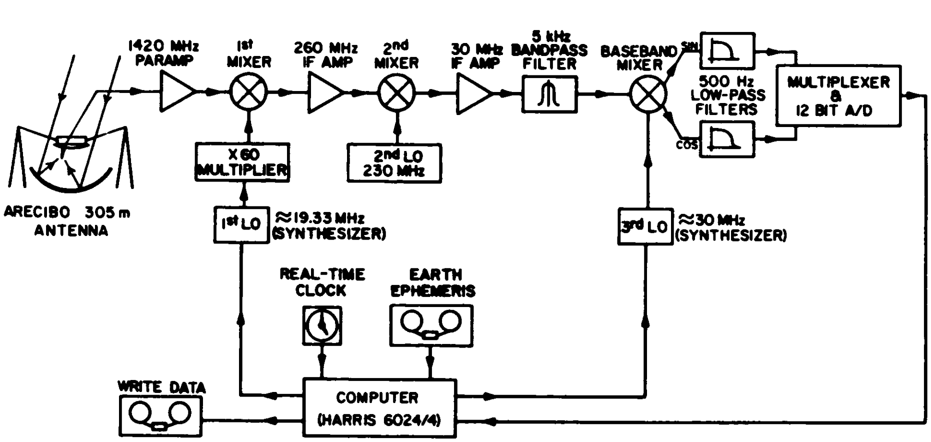

Figure 3

oscillator could be changed in real time under computer control, while looking at a planetary ephemeris and a real-time clock. This kind of procedure was done already at Arecibo for planetary radar and pulsar observations. So, rather than doing spectral analysis in real time to hundreds of thousands of channel resolutions, the baseband quadrature amplitudes were converted to 12-bit numbers and put on 9-track tape. Now usually when you do that you end up with tapes just piling up in the hallway, and you have other things to do so they rarely ever get analyzed. But since I was on sabbatical and captive down there in the jungle I didn't have anything else worthwhile to do, so I actually analyzed all the data! It turned out that at the relatively leisurely rate at which the data was being taken (to get high resolution) and the amount of time I had I could actually keep up, the computation was roughly real time; it took about a minute to process a dual 64K channel FFT, that is the two polarizations of 64K channels. I used a resolution of 0.015 Hz, which required about a minute of integration; then the 64K channels gave a total bandwidth of 64K x 0.015, roughly 1 kHz. I looked at the 200 F,G,K dwarfs of the RGO catalogue that are visible from Arecibo. Jill Tarter can run off a personalized list of these things for any observatory, and, in fact, I learned about RGO and names like "FGK" and "Jill Tarter" while on this little junket.

Well, the bottom line is that I looked at those 200 stars, plus a few extra goodies (pulsars that don't slow down, curious flat-spectrum sources, and a few other favorites), a minimum of ten minutes each, all at 1.4 GHz, and did off-line FFT's. And found essentially nothing! Oh, I should say something about those terrible doppler shifts: What about this orbital and spin doppler, what about peculiar radial motions of stars, what about their orbital motions? How do you get that into one kilohertz of total observational bandwidth when I said before you need a half a million channels just to cover a doppler uncertainty of one kilometer per second? Well the assumption during the Arecibo observations was that we compensated our site motion around our heliocenter, and assumed that the smart guys out there knew the velocity of our sun relative to them, and in fact precompensated their transmissions near the neutral hydrogen hyperfine frequency to arrive in our heliocenter precisely at "laboratory" frequency. And why shouldn't they, they're going to the bigger effort, and they're smart and all that kind of stuff, they can show how smart they are. Our orbital doppler term, by the way is ±150 kHz, and our spin term is ±2.5 kHz, both taken tangent to the motion.

So we assume they take out their dopplers along the line of sight, and we did our half of the equation which was to take out our motions relative to the sun, including the spin term. Although the Arecibo observations of 1978 may set new records for frequency spectrum not covered, it definitely sets a record for sensitivity. With a big collection area like that, and the narrow band channels, and a minute to integrate, you really do well. It would have detected 5 kilowatts transmitted from the farthest star which is 80 light years out (or 25 parsecs for those who like obsolete units) using an identical dish, that is an Arecibo talking to an Arecibo, if we should be so lucky.

Let me show you the one interesting result that did come out of that search. All these experiments have null results or we wouldn't be here talking about them, but one thing I did get a chance to check out was whether this rejection of interference really works or whether it's just a pipe dream. Here's a crazy looking spectrum that you probably can't understand at all [next four figures too detailed to reproduce in this volume]. If you want to plot 64,000 channels on a piece of paper you're in trouble, and one of the ways I did it here was to make a raster. This isn't a Cornell raster showing naked people pointing at their children or holding hands, but a raster showing the frequencies going from plus a half a kilohertz to minus a half a kilohertz centered on the neutral hydrogen frequency near zero. You plot intensities with specks and dots and using little a's, b's and c's and so on giving the sort of thing you see here. If you don't have telescopic vision you won't be able to see what's going on here, so just trust me. But what's going on is that we injected a signal at the IF at a particular frequency, at a level 30dB below the 1 kHz total noise, and here it is sticking up in the spectrum at 14.5 sigma, that's kind of reasonable for 30dB down, in 64K channels, since with 1,000 channels you would be even steven (i.e., you would recover unity SNR). Anyway, a fixed frequency signal shows up in this Fourier transform, that's comforting to know. Here's the next test, a chirping signal 30dB down injected against a chirping oscillator shows up, too. You hope that would be true because that's what the experiment is basically looking for! Again, here's the spectrum, here's the biggest peak. In this case its smeared out to three bins because the tracking synthesizers weren't tracking perfectly, but the peak is still 11 sigma high at the place it's supposed to be, so chirping receivers see chirping signals, and that's nice. And here's the final one, in which I cranked up the signal power to be equal to the noise power in the kilohertz of bandwidth; that's 30 dB stronger than the chirping experiment. But in this test I let the signal stay fixed, and used the chirping oscillator. There are no big peaks in the power spectrum. In fact the strongest peak in this whole graph, this little salt and pepper graph here, turns out to be 5 sigma, and it's at a completely unrelated frequency.

So the chirp really works. As long as I'm showing these funny graphs, people always like to see what real data looks like. You can crank up the sensitivity until the noise starts showing and you get pretty little things like this, which is one minute's worth of RGO number 9672 on some date or other in 1978, printed in fine letters there. Isn't that a handsome pattern of spots and dots? And here's what a signal looks like when plotted the usual way (figure 4). Let's see, this is one foot's worth of a 100-foot graph of 64K

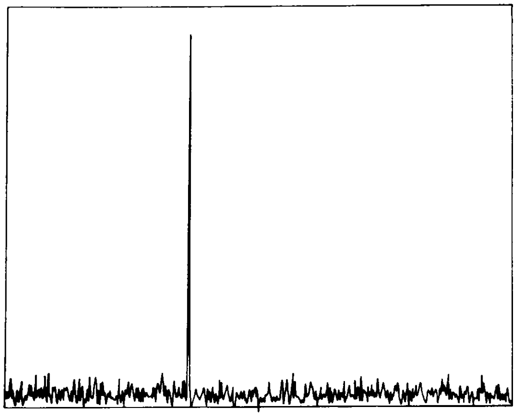

Figure 4

channels, this is what the power spectrum of a signal 10 dB down below noise looks like, so having lots of channels really does help you out. And here finally is a summary of parameters that were used in that search (Table I). Radioastronomy deals in tiny amounts of power generally. For this search the sensitivity is such that a micro-micro watt total earth flux would have given us a 20-sigma signal; perhaps on the occasion of Ozma, you'll like this one here, a "Mega-Ozma per minute." Or maybe its the wrong thing to say at the celebration of Frank's pioneering project !

Table I (Science 201, 733 (1978))

Objects Searched:
All 185 F,G,K main-sequence stars within 25pc visible from Arecibo (declination 0^{o} to + 38^{o})

Frequencies:
Terrestrial frequency corresponding to "1420 MHz" received at the heliocenter
Each run - total BW = 1 kHz
resolution = 0.015 Hz
64K bins each circular polarization

Summary:
14 one-minute runs/object (average)
Each run - sensitivity of 4×10^{-27} w/m^2 (5σ)
(1 pW total earth flux for 10σ)
5σ detection for 5kW + Arecibo at farthest target
1 MegaOzma/min
Total bins = 3.4×10^{8}

OK, on to the next ultra narrowband crusade here, this is called "Part III: Suitcase SETI". In 1981 I had an NRC Fellowship at NASA Ames Research Center, which was working with Stanford University on hardware for NASA's multichannel analyzer. As a little one year project along with Ivan Linscott, Cal Teague, Peter Backus, and Kok Chen, I hatched a scheme to build a hardware version of this ultra narrowband spectrum analyzer, so that you don't have the tapes accumulating, and you wouldn't be locked into one site, with its special hardware and computers. The idea was to make a little portable suitcase-size analyzer that would look for very narrow signals and is similar in operation to that previous search at Arecibo except that it's portable, a little box that hooks in at the IF signal, keeps track of the ephemeris and sidereal time and chirps its local oscillator starting at IF. It has its own baseband electronics, digitizers, etc., and includes a dual hardware FFT to do a pair of 64K complex transforms, complete with line signal recognition and archiving (on home videotape, believe it or not. We generated 20 tapes from the search we did down there and they have never been played. I have a pile of once-used tapes if anybody wants them).

Here's a block diagram of Suitcase SETI (Figure 5), since it seems to be a day for block diagrams and the celebration of nifty hardware. It's basically the same scheme as

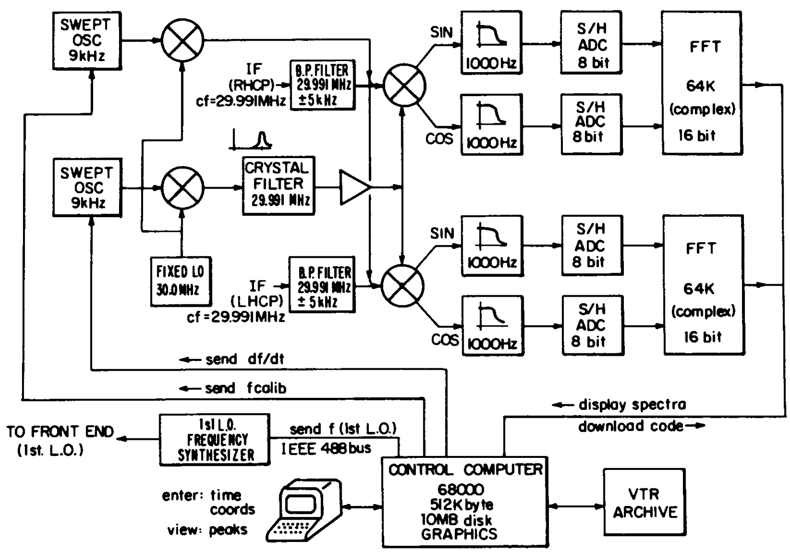

Figure 5

used at Arecibo in 1978. Two polarizations get mixed against an agile local oscillator which is formed by modulating a fixed precision local oscillator with a little digitally programmable rate multiplier, in order to generate a chirping local oscillator near 30 megahertz with extreme accuracy and chirpability of the order of a few hundred hertz. This essentially synthesizes a chirping receiver front end, and then it is mixed down to baseband with quadrature mixers, followed by active anti-aliasing low-pass filters, sample/hold amplifiers, and 8-bit analog/digital converters. The digital time series enters these custom Fourier processors, which were built at Stanford, in Allen Peterson's lab. After doing the transform, the full 64K power spectrum is shipped back to another little computer here that looks for peaks, puts things on video tape (where no one will ever read it -- it's called a "write only memory") and beeps when it thinks it has a big signal. It also keeps busy plotting spectra on its screen.

Let me show what the thing looks like; there it is, "Suitcase SETI" (Figure 6). It's got handsome labeling, because it was pretty on the inside, and we decided it should look

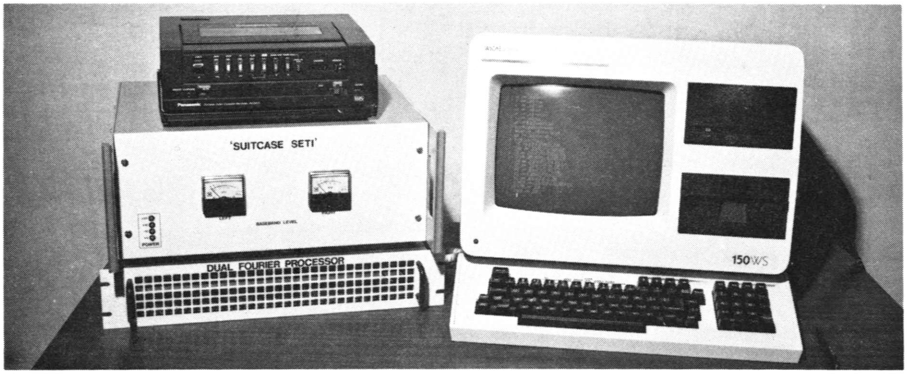

Figure 6

pretty on the outside. Physically it's comparable in size to about three or four suitcases ("steamer trunk SETI"?) , but the basic idea was to make it self-contained and portable, so you could take it to somebody's observatory. If they could oblige you with a 30 megahertz IF, off you go. Here's a photo of the dual Fourier transform processor (Figure 7). You're

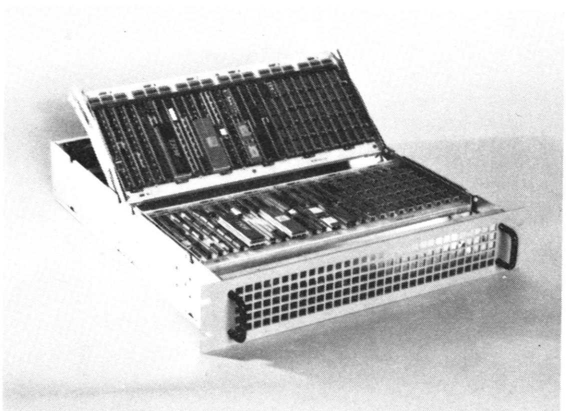

Figure 7

not seeing double, it's actually 2 channels of FFT, each done with a 68000 CPU, a hardware 16x16 multipler, a bit-reverser for the FFT shuffle, and plenty of memory. The processors do a pair of 64K complex transforms in 20 seconds, which allows you to increase the bandwidth to 2 kHz (each polarization) with 0.03 Hertz resolution, narrow enough to retain good rejection of interference.

This project was done with funding shared equally between the Planetary Society and NASA, and cost about $25,000 to build the suitcase SETI hardware, not counting my NRC Fellowship and a little help from HP. We took Suitcase SETI down to Arecibo and used it at 1.4 and 2.8 gigahertz, looking at Jill's RGO's again, and as usual we didn't see anything; but we didn't see any false alarms, interference, or anything like that, either. We felt we had to look at something with some narrow spectral features just to make sure it was working, so we looked at W49OH (which sounds like some new FCC call letter scheme, doesn't it?), one of these maser sources with lots of sharp wiggles. Just going through the 1665 line turned out to require about 35 adjacent spectra, and generated what I think must be the world's highest resolution spectrum of anything. If we plotted the points 200 dots per inch (high resolution versatech), the graph of just that single line would have stretched across the Arecibo dish.

Now for "Part IV, Sentinel." What do you do with a 64K-channel Fourier analyzer and associated portable doodads that took almost a year to build, a lot of hard work, and then in a 60 or 70 hour run at Arecibo was all played out? Well, a targetted search of 200 stars is nice, but you'd really like to do a million stars because the Drake equation says that you probably should. What I wanted to do was to take all the SAO catalogue numbers and stick them in a computer and have the Harvard-Smithsonian 84-foot retired dish look at all these stars. Mike Davis straightened me out quickly. You see, I never took any astronomy, so I really don't know things like this, but Mike Davis did. With an 84 foot dish you can't look at a million places in the sky. Why don't you just point at the meridian and let the sky go by; that's much faster. Indeed he's right. With a half degree beam that's the way to tessellate the sky.

So, with help from John Forster and David Brainard at Harvard, we re-commissioned the dish and set up an all-sky transit search, using the suitcase SETI hardware attached to new RF hardware. This again was funded by the Planetary Society, it cost us another 30 grand, which isn't much, it's cheap. Contrary to public belief (Figure 8). Sentinel (the

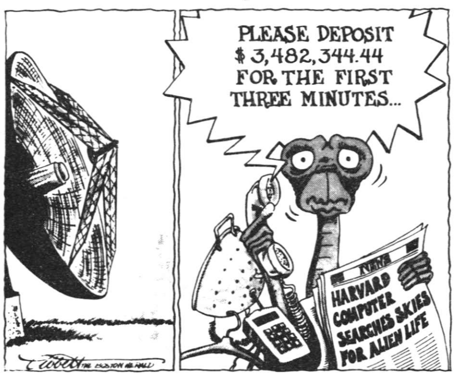

Figure 8 -- Reprinted with permission, The Boston Herald

codename selected by a contest among Planetary Society members) operates with the same backend parameters, namely a pair of 64K complex transforms with 0.03 Hz resolution and 2 kHz total bandwidth. We cover from the south horizon (which is -45 degrees declination at which we get an extra 3 or 4 dB of noise) up to +60 degrees; we don't go any farther north because the sky just turns too slowly and you get impatient up there. That turns out to be 80% of the sky. We have a half degree beam width at 1420 MHz, and we move the antenna 1/2 degree along the meridian each day, so it takes about 200 days to do the whole sky, if nothing breaks. A source transits through this beamwidth for a minimum of 2 minutes (at 0^{o} declination) and of course longer as you move away from the equator.

The finite beam size, which is of course a position uncertainty for any radio signals you might receive, forces you to have a minimum total bandwidth: That is because the source could be anywhere in your beam, with a spread of doppler shifts set by the dot product of earth's orbital velocity and source direction. It turns out that occupies about a kilohertz and a half of doppler ambiguity, for our dish at L-band. So you actually need bandwidth if you have beamwidth in this business.

Now to the obligatory block diagram (figure 9). Beginning at the 84-foot cassegrain equatorial dish, the signal enters the feed horn, where a noise diode adds signal for system

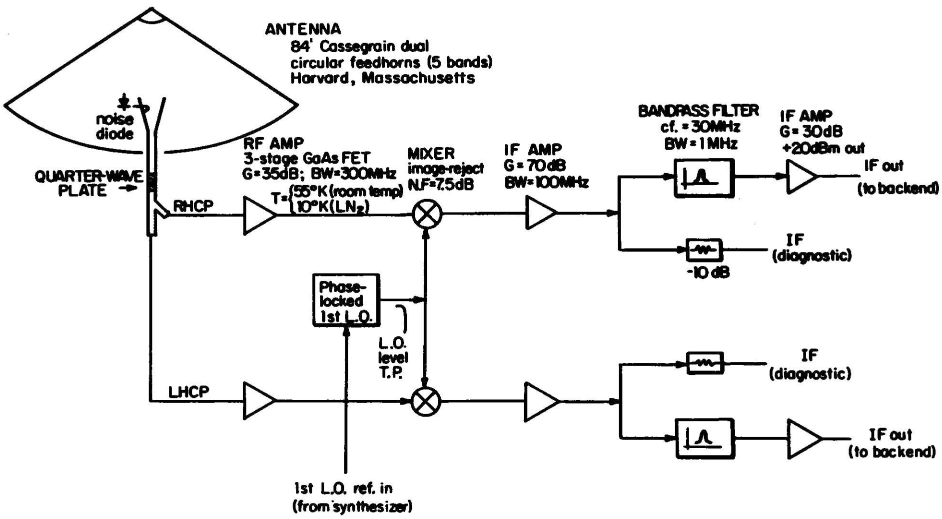

Figure 9

check-out. There's a waveguide quarter-wave plate to separate the circular polarizations, which go through coaxial transitions into a pair of "Lumplifier" GaAs FET amplifiers, a single-conversion down-converter to 30 megahertz and then off into our building. Let me show some pictures of the thing. This (figure 10) is the Harvard Smithsonian 84-foot

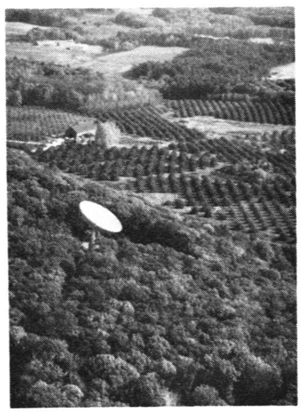

Figure 10 -- © Frank Siteman 1984

fully-steerable cassegrain, with fiberglass radome covering the feed horn. It lives in the apple orchard country of western Eastern massachusetts, about an hour from Boston. It's on Oak Ridge, which I'm told is the highest ridge between Mount Wachusett and the sea. And there it is (figure 11) looking big. I picked the smallest object around, my six year

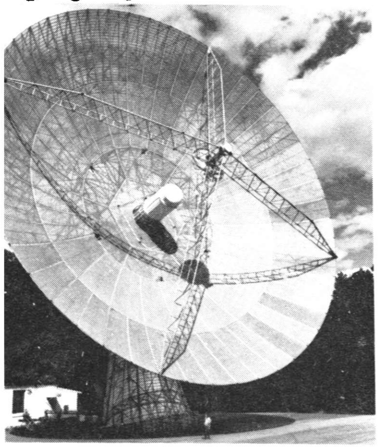

Figure 11

old, a little kid, it makes it look big. It is big, anyway, 84 foot. These (figure 12) are the Lumplifiers, which were built at Berkeley. I hesitate to say that I built them. What I did

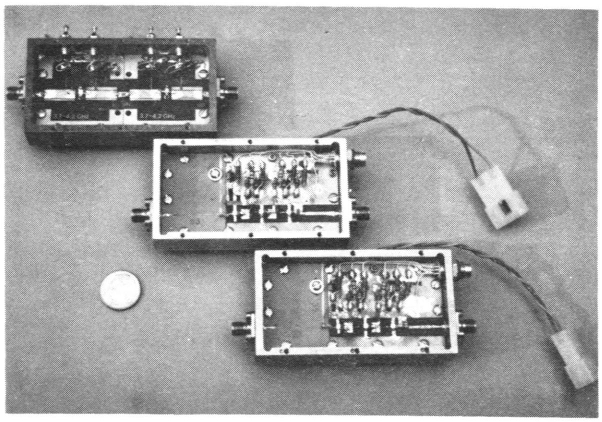

Figure 12

was I painstakingly Chinese-copied an object that Tap Lum put in front of me, then he made them work.

This photo (figure 13) shows the universal way we all build our RF hardware these days, using rigid coax going between magic litle boxes with lots of SMA connectors. In this case image-reject mixers at lower right mix the output of the GaAs FET preamps to 30 MHz, using a phase-locked LO. Then lots of amplification, filters, splitters, etc., and finally the IF signal goes "downstairs" to the control building. The next photo (figure 14) shows the control room, where "Suitcase SETI" is teamed up with some timekeeping,

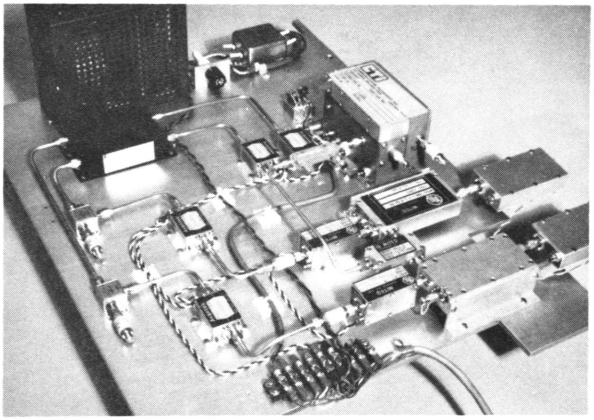

Figure 13

Figure 14 -- © Frank Siteman 1984

oscillators, etc. The last slide in this series (figure 15) shows what happens all the time, namely a graph showing the whole 64,000 channels (compressed, of course) as the top graph of each polarization pair, and the 256 channels centered on the highest single peak as the bottom graph of each pair. We also have a listing here of the 8 highest peaks -- their height in sigma and their frequency offset from the center of the band in hertz. The reason it's going crazy and beeping, it goes beep and says "LARGE SIGNAL" (of course, you could ask the deep question whether it beeps when no one is there to hear it!), is because we are injecting a weak (-25dB relative to noise) second chirping oscillator at IF, offset by a few hundred hertz, as a calibration signal. It checks out the operation of everything beyond the IF's and it generates 60 or 70 sigma peaks in the power spectrum.

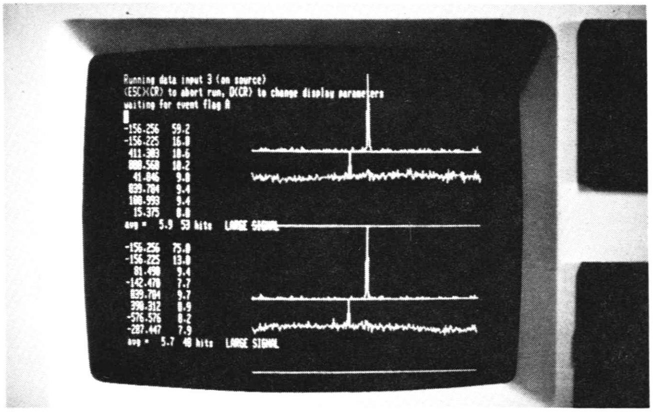

Figure 15

Well, let me zip right along here. What did we find out running this thing? Well this sketch (figure 16) summarizes a little over a year's running. We started at 1420 MHz at -30

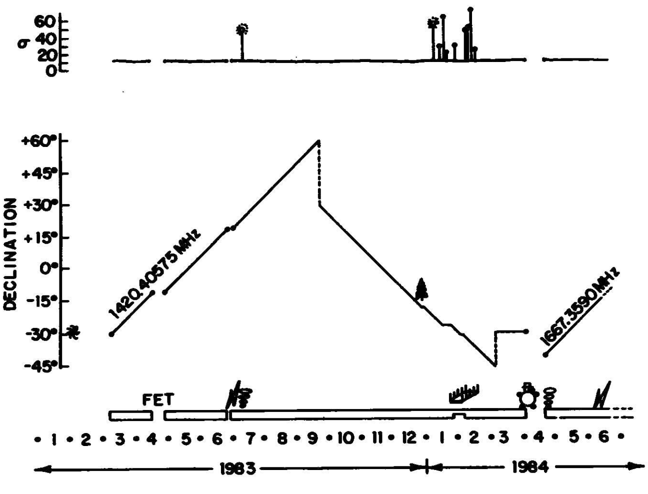

Figure 16

degrees declination and simply advance the declination a half a degree north per day, that's a half power beam width. Then we had a failure of the front end amplifiers here, due to leakage in a bad batch of chip capacitors. This cute little crab thing here is the galactic center. I didn't know where the galactic center was when we started and so I thought "well, 30 degrees South, that sounds low enough", but it turns out the galactic center is at -29.5 degrees. So we now go down all the way to -45° declination, close to the South horizon. Then we went along for a while and then there was June, bad time in the Northeast when we had a big lightning strike and BLAM! it blew the thing to smithereens, it took us about a day to recover. That's the lightning bolt on the graph. But then what happened was the power company put a new transformer on the pole, but they wired it wrong so it put 180 volts in the observatory and generated these little smoke-puff things you see here.

Soon after that we discovered our first big signal, you can see that from the upper sketch which shows each day's largest peak. This is a do-everything graph. It was a 49 sigma peak at 0 frequency and it happened when we were up at around +22 degrees declination and it was in June, does that give you any hints? It was the sun, and it just flattened the IF amplifiers, it desensitized them and left the dc channel. We finished the northern sky here, and then decided we'd better head down a little farther south this time. Oh, here's where a chip blew out, there it is. A big snow storm here knocked out the wires, and then it was a week before they got everything going and you know what, the power company came along with 180 volts again -- here is the smoke! Then we started running an OH line (1667), (we're now doing the sky a second full time at OH), and here we had a tremendous lightning strike, but no interruptions this time because now our observatory is just festooned with those pretty little red things you buy from GE that stop lightning in its tracks.

In the winter we discovered the sun again. These scientific things should be repeatable. Then we discovered this stuff here, a little cluster of a half dozen winter events, which disappeared but showed up again a year later. I'm not sure exactly what it is; it's narrow band, and I'll show you some of the signatures. It has the property that it occurs in different places in the sky, the ET's are jumping around but they always arrange the signal so it arrives at the site at precisely the same <u>received</u> frequency: Not exactly the characteristics you expect of an ET. Let me just show you the signatures here (figure 17).

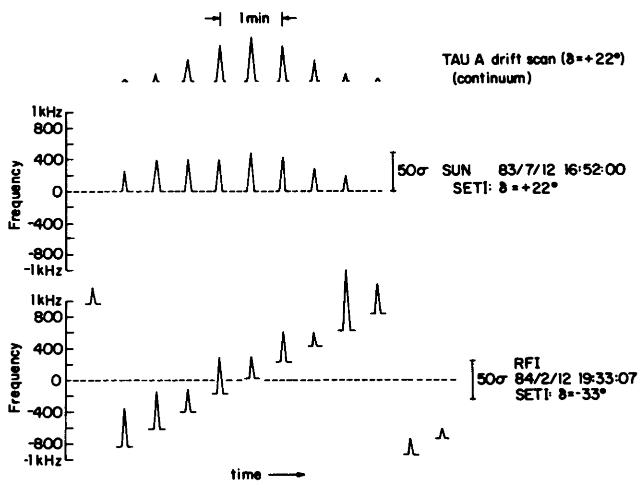

Figure 17

The top graph is a drift scan through Taurus A, just to get the beam size calibrated. Of course it is a continuous bump, but what I've done is to plot it every 20 seconds, which is how often we do spectra while searching, just to see what the envelope of a real source should look like. The next graph is the sun, which always showed as dc as I said before, but it shows a reasonable amplitude envelope. Then here is one of those winter events, which show a marching through of frequency with successive runs. Why is that? Because we set our center frequency at each run for the source coordinate that would be in the <u>center</u> of the beam, so if the source is off the beam center, it will have a frequency offset. So a real cw source should march through the band, as this one does. What's a little wrong here, though, is that it doesn't seem to have a very good amplitude envelope, and what's doubly wrong is that the site (received) frequency, for a succession of about half a dozen such events was always the same. So, they can't be ET's, unless they're spread over the sky, all transmitting cw at earth, rigged up to arrive in eastern Massachusetts at identical frequencies, after all dopplers are accounted for. So this is the closest thing to a real signal that we've had. We were jumping around alot, and in fact I remember even calling Jill Tarter and saying "Jill, never mind why I'm asking this, but can you tell me what's at such and such a position?"

Part V, the last section, is called META (when you're funded by the Planetary Society, everything has to have a name; META stands for Megachannel Extra-Terrestrial Assay). Look, what's wrong with the Sentinel scheme is that we're expecting these guys to preshift their transmitted frequency so it arrives in our heliocenter at some known atomic transition frequency. They have to do that because a radial velocity of, say, 10 km/sec generates a frequency offset of 50 kHz, and so with our meager 2 kHz total bandwidth you get completely out of band for typical radial velocity terms,even peculiar velocities of nearby stars, or orbital velocities like the 30 kilometers per second of the earth going around the sun. It might be reasonable for extra-terrestrials to send such targetted precompensated beacons if they know we're here, perhaps from detecting Hertz's early experiments, or maybe they have seen the color of the copper maples blooming in the spring, or something like that. So if they have made really sensitive observations and know we're here, or if the galaxy is so full of civilizations that each one targets only its local neighborhood, then pre-compensated and directed magic-frequency transmissions make sense. But for a beacon transmission protocol our present system has totally inadequate bandwidth, and the doppler shifts are going to kill us.

So what are these doppler shifts, and how large are they? Here is a quick rundown (Table II). The Drake-Helou effect is really a bin width, not a shift, and it tells you what your resolution should be. The second entry is a doppler shift because these guys might be

Table II

Effect	Shift	Comments
Multipath FM ("Drake-Helou")	0.01-0.1Hz	A good *bin*width! Must chirp, gives RFI rejec.
Barycenter/heliocenter ambiguity	±60Hz	Jupiter
Beamwidth (84' → 0.5°)	1.5kHz	Untargetted only
Spin (uncorrected)	±2.5kHz	Spectral smear: must correct
Orbit (uncorrected	±150kHz	
Stellar Radial Velocities		
Galactic Rotation	±75kHz/kpc	Survey. No attempt to correct
Peculiar Motion	~±100kHz	
Big Bang Rest Frame		
Offset	1.5MHz	
Uncertainty	±150kHz	COBE should improve
Galactic "Center"	±125kHz	

so smart that they know where our solar system barycenter is rather than our heliocenter. Maybe they'll compensate into that one if they really want to show off, but it turns out that's only 60 Hertz away, and Jupiter's the thing that does it. Next there's the beamwidth ambiguity, but that's within our current system bandwidth, and that only occurs if you're doing untargeted searching. The earth's spin contributes +/-2.5 kilohertz, which has to be corrected because that has the highest rate of change of all these dopplers, as I've explained. Then there is the orbit, which uncorrected would be 150 kilohertz, and which we correct for in the current scheme. Stellar radial velocities consist of two terms, namely the peculiar velocities of nearby stars, typically of the order of 15 kilometers per second, and then in addition, because of galactic differential rotation, there is an additional 15 kilometers per second per kiloparsec in the worst direction. And you would like to be able to encompass this if these guys are not beaming at you, in fact you've got to if they are doing an isotropic beacon or something like that.

In addition there are other frames that it may make more sense to transmit signals with respect to. Why transmit in your local star's reference frame, or in the local standard of rest? Why not transmit in the rest frame of the remnant radiation of the big bang? The big bang appears, as far as we know now, to be purely isotropic with a small dipole anistropy, explainable as our motion at 300 kilometers per second with respect to it. That motion has a 30 kilometer per second error bar, which translates to 150 kilohertz required bandwidth at 1420 MHz. Sam Gulkis assures me that the COBE (Cosmic Background Explorer) satellite's observations will put us in great shape, but in the meantime a multichannel analyzer of the order of a couple hundred kilohertz bandwidth would enable us to observe in that rest frame an extra-terrestrial civilization that is transmitting in this attractive scenario suggested by Phil Morrison. By the way, here's a pretty picture I can't resist showing, that's what the sky temperature looks like when you subtract out the constant term at 3 degree kelvin. The contours show anistropies of the order of millidegrees, and this was made by the Russian satellite called Prognoz 9. It took about a year and it was done at a radio wavelength of 8 mm. Another rational choice of reference frame in which to transmit is the galactic barycenter, whose velocity uncertainty is again of order 30 km/sec.

So, given all these dopplers, what bandwidth do you need? You need something like 400 kilohertz total bandwidth to do any of those three experiments (LSR, Cosmic, Galactic rest frames). You want, let's say, 0.05 hertz per channel, that's 8 million channels. So we're building an 8.4 million channel analyzer.

Let me just give the credits at this point. This new analyzer is funded by the Planetary Society and Harvard University (through salary, facilities, etc.). The original work on the Suitcase SETI was done at NASA and Stanford, and in fact that group is contributing the front-end processor for this thing. The major players in the META project are Brian Matthews and John Forster at Harvard, and Ivan Linscott at Stanford; here are others who have joined in one way or another:

Telescope Renovation
- Mal Jones
- Mike Williams
- Gene Mallove

Suitcase SETI
- Cal Teague
- Ivan Linscott
- Kok Chen
- Peter Backus
- Allen Peterson

GaAs Fet Amplifiers
- Tap Lum

Oak Ridge Operations
- Skip Schwarz
- Dick McCrosky
- Arny Aho

Soldering
- Carl Quillen
- Elaine Kuo
- Teddy Kim
- Mike Coughlin
- Suzanne Amador

This might give you the feeling that if you go into the building you'll find 15 or 20 people working hard, but these people sort of drift in and work in your lab for a few hours and then go away for two weeks, so it's a peculiar sort of motel that we run there.

Here's a block diagram of META (figure 18). It's similar to sentinel, but has an array of 128 computers, each of which does a 64K transform, giving us 8.4 million points. Here's

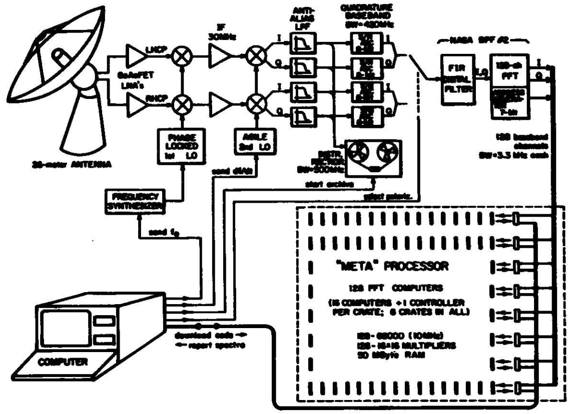

Figure 18

the NASA bandpass filter that prefilters the signal to make it into 128 channels of about 3 kilohertz each. A fast-start tape recorder of 1 MHz bandwidth can record the baseband signals, in case we find a big peak in the spectrum. The computer controls the first LO, alternating among favored rest frames in each 20-second integration.

Let me show some pictures. Here's a pile of these home-made processor boards (figure 19), we fabricated 150 of these by hand; that's my knee. Again, I used the 68000 at 10 MHz,

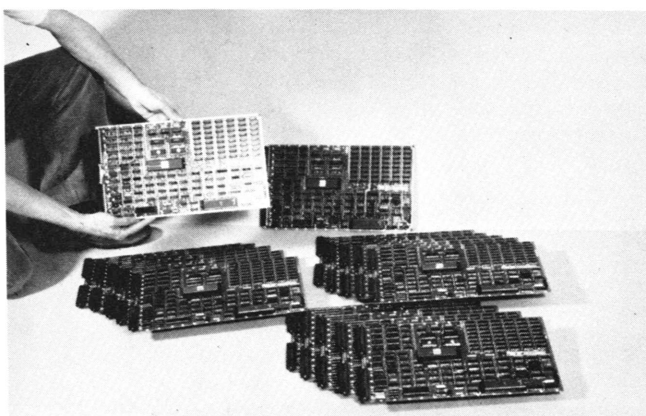

Figure 19

hardware multipler, bit reverser, and lots of ports. The next slide (figure 20) is a close up. And this (figure 21) shows how they live, in little houses we call "crates", that hold 16

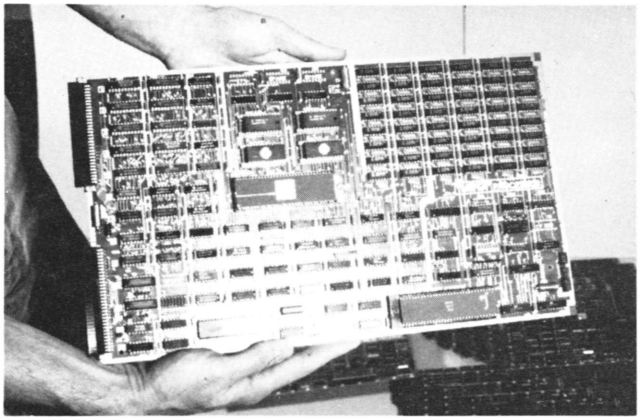

Figure 20

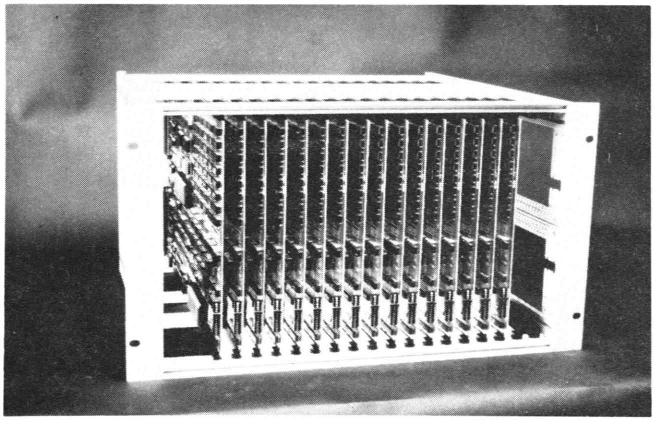

Figure 21

processors or a million channels. So you're looking at a million channel analyzer, with many pretty little lights on the end that light up, and it's really quite beautiful in a darkened room. The next slide (figure 22) shows our progress as of last week -- a bank of 4 crates, or 4 million channels. They all work together, do Fourier transforms, and talk to the central computer. The next slide (figure 23) shows a rack from an angle; roll the thing around on its wheels and you can see the cables in here that bring all the signals from these individual processors together to the central node of this processor array. Here's looking in the back (figure 24), its all done with ribbon cables. There are actually four bidirectional parallel ports on each one of the processors which are bussed together on a backplane circuit board and buffered by a bank controller card, which has differential line drivers, to bring all signals out to these twisted pairs that snake up and down the rack. Two racks of these will make an 8 million channel analyzer.

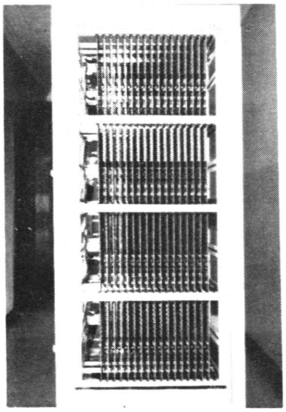

Figure 22

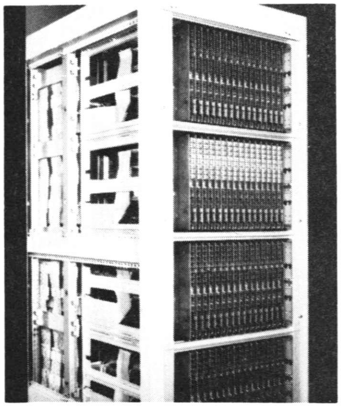

Figure 23

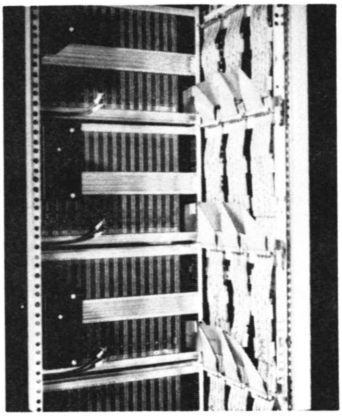

Figure 24

Let me just say some last things here. This whole "supercomputer" is being built with basically coolie labor, that is underpaid students. It altogether has the integer processing power of 150 VAX's or two to three Cray's. Altogether it has 50 megabytes of RAM, it has 16,384 back plane connections, and it has half a million solder joints (all done by hand!). In actual operation we're going to alternate the three favorite rest frames: We're going to look in the big bang rest frame, the galactic rest frame, and the local standard of rest, that will take one minute, and in the next minute we'll do the other polarization again at the same three frames. So we'll get to do all of our experiments in one scan of the sky. Someone asked me about "roving processors", he wanted to know how we patched holes in the spectrum if a processor fails. The system basically knows when something is broken, by having extra "rovers" check regular processors. It attempts to re-boot a bad processor once; if that doesn't work, it shuts off that processor and permanently assigns a rover.

So in summary, with these 8 million channels and about 400 kilohertz of bandwidth, we lift the most serious constraint of the present experiment, namely that the magic frequency carriers we're sensitive to must be beamed specifically at our star, pre-shifted in frequency to arrive in the heliocenter at "laboratory" frequency. In the new system we still require magic frequencies, but they can be transmitted in any of 3 natural rest frames. We cover 80% of the sky in about 200 days (-45^{o} to $+60^{o}$ declination), sensitive to carriers at the 5×10^{-25} w/m^2 level. Using a natural figure of merit linear in number of channels, sensitivity, etc., we can restate it as a "megaOzma per minute", or 100,000 years of Ozma per minute. Thank you.

Sullivan: I've harangued you about this in private, but now I'll get you in a public forum. Are you doing anything about recording the broadband signal so you could be monitoring the milky way, comparing that versus standard radio sources to see if there is any

Yeah. This is Woody's shtick, this is the right kind of telescope to keep track of the galaxy in case things pop up, continuum type things, like a supernova or something else that we don't know about. There's room on the disk to keep a map of a single number which is your total power per integration, and the software will include comparison of the nearest four pixels in space, from the year before.

Sullivan: So you'll be checking against a standard catalogue.

What I thought we would do is create a catalogue our first year and then just compare it.

Sullivan: Right, right. Great, great.

Tarter: But you can also add six numbers together because we are looking for continuum things, so your rest frame differences don't make any difference...

We're going to add all channels together, it will be the sum of all of them.

Oliver: Why do you do that, why don't you measure the total analog power?

OK, but then to do that we actually have to stick on an A/D and put an extra port, whereas we have these numbers already.

Tarter: My point is, Paul, you're making six independent observations per half power beamwidth. Your three rest frames and your two polarizations, you can add those numbers together for looking for continuum unpolarized.

Oh, oh. Oh, I see. Yeah, o.k.

Sullivan: It will only help root six.

Sullivan: What about RFI rejection, you were worried about that and you haven't mentioned that at all here, and it's a bigger problem with your new system?

Yeh, bigger and more channels and I'm worried about it, and we're also going to slightly greater channel bandwidth, from .03Hz to .05Hz. The ace up my sleeve on that one is that if it turns out to be a problem, we will replace all the 64K RAM by 256K RAM, and that will give us a 32 million channel analyzer with .0125Hz resolution. That will probably finesse that problem.

Bania: Paul, I'm sure the Planetary Society would be very pleased to discover that you have given us a brand new figure of merit for SETI systems -- MegaOzma per minute, call it "MOM".

Seielstad: Let's not finish on that one!

Weinreb: What is the cost of your 64K FFT board?

We're paying about $500 bucks a piece, fully stuffed. That turns out to be more than it should because memory crashed about 6 months ago. The current price I think would be about $300 to $350 per processor. It's got a 10 MHz 68000 without wait states, it's got 384K RAM, it's got a 16x16 CMOS multiplier, 4 bidirectional 8-bit parallel ports, 2 serial ports, a flag port, pretty lights and switches, a bit reverser to do the Fourier shuffle, a couple of things like that.

Seielstad: I bet you struck a resonance from the right guy.

Drake: How did you arrive at .05Hz bin width and how flexible is it, can you increase or decrease?

It's hard to increase, that's running flat out. The reason we went to .03 to .05Hz is that this new processor, being designed two years after the other one, is basically faster, by the faster clock and some other speed enhancements, so we can get that much binwidth from running flat out. We can always clock down, that's cheap, we just wait longer. You do a computation and just spin your wheels until the guy wants it. We couldn't go any faster than .05Hz with this particular technology. But I don't think we would want to go much faster because the crossover to the point which you don't get Drake-Helou cancellation of interference -- can I call it that? -- turns out to be around 0.3Hz. That is a 3 second integration, in that amount of time your smear equals a channel. This rejection of interference goes quadratically in the resolution, which is nice.

Tarter: You have taken the wraps off there, you've now allowed a beacon, and the beacon's going to drift through the channels, and what are you doing about signal processing to find such a drift?

Why does the beacon drift?

Tarter: Well, because it's now no longer targetted.

They're not stupid enough to put their beacon in their northern hemisphere and just transmit it with all the spins and orbits. They're going to stick it at their pole, or put in some really slow orbit, or tesselate the sky with a lot of little mini beams, each of which is compensated for the motion if they want to go to that much trouble.

Tarter: Ok, but you're still not going to do any signal processing for finding something that's narrow band with changing frequency slowly?

No. Only to accept it, we take out our motion. We're not trying to do a NASA kind of thing, we don't have the computational power or anything else. We can't find pulses, we can't find intrinsic chirps coming from the other side. Just straight carrier, give me a carrier and I'm all set.

Gray: How big does it have to be?

In strength? You mean watts per square meter? It turns out with this system you're talking of the order 10^{-25} watts per square meter. That turns out to be somewhere of the order of fractions of a nanowatt, total earth flux, if that helps, or something like a megawatt transmitted from an Arecibo at a few hundred parsecs.

Boyce: Well I see you're driven by the combination of your carrier and your shtick (groans).

THE NASA SETI Program: An Overview

Bernard M. Oliver
NASA-Ames Research Center

Today I'd like to give you a little overview of the NASA-SETI program. First, a few words about its history. It all started in 1970 when the people at Ames decided to hold a set of lectures on the subject of interstellar communication. Frank Drake and I were speakers at that program. As a result of the interest stirred up by that series, the decision was made to hold a summer study program on the engineering design of a system for achieving interstellar contact. This was one of the NASA summer faculty fellowship programs. George Swenson, Charlie Seeger and Bob Dixon were in on that; you participated in what came to be known as Project Cyclops. That study found that if it were needed we could design a receiving array with square miles of collecting area--enough area to eavesdrop on signals that other civilizations might be radiating at distances of perhaps 100 light years or more. Many people got the idea that to achieve interstellar communication, you had to have a system that big. So we went through a long period of trying to convince people that we wouldn't build anything that big, until we had proved we needed it.

After Cyclops, John Billingham and others at Ames Research Center kept SETI alive on a shoestring for many lean years. There was an interstellar communication study committee and then a Science Advisory Group headed by Phil Morrison that assisted in laying the plans for the program we have today. Finally, we did get a small amount of funding from headquarters in 1981 and then, approximately the same day, the Golden Fleece Award from Senator Proxmire. So we had to do a little lobbying to take care of that detail. In 1983 with the help of the Field committee report, we were able to get funded, not at the 2 million we asked for but at a 1.5 million dollar level. We are now in the third year of a five year R&D phase of the program. That means we are spending our time and our money defining and designing the data signal processing equipment that will eventually be used in the search. The search has not started yet, we are in the field with some of this equipment, but we are not conducting a SETI search with it, we're merely testing it under the RFI conditions that will be present there.

The advisory group recommended a program with two distinct search modes--a two-phase search strategy. One mode is an all-sky survey, the other a targeted search. The all-sky survey, because it is forced to point in many more directions in a given search time, cannot remain in any given direction for as long as the targeted search, so the sensitivity is modest: about 3 orders of magnitude below the targeted search. However, the sky survey might detect powerful signals coming at us from unexpected directions.

The targeted search assumes that we know what direction signals are coming from. It assumes that we know all the good suns that might be radiating these signals. But there might be some radiators far out in space away from any solar type star we know. The sky survey covers that possibility. It is, however, responsive to CW signals only because with the antenna scanning the sky, the signal, to be detected, has to be there at the moment the antenna points at it and therefore must be present at all times. The folks at JPL are giving their major attention to the sky survey. Ames is concentrating on the targeted search and the design of the special equipment needed for it.

The targeted search will examine about 800 solar type stars within 100 light years of earth. It has enough sensitivity to detect signals having powers that we presently radiate if they were beamed at us. Because the targeted search remains on each target for a substantial amount of time, perhaps on the order of 1000 seconds, it can not only detect CW signals, the traditional SETI signal, but also pulse trains. This needs further hardware, as you'll see in a moment. Like all SETI searches to date, the search is confined to the terrestrial microwave window, which extends from about 1 to 10 GHz.

The sky survey will use a modest resolution in its multi-channel spectrum analyzer, about 32 Hz according to present plans. The targeted search will have the capability of going to bandwidths of about 1 Hz for CW signals and also will probably have several bandwidths available in the final design so that we can supply approximately matched filters for various lengths of pulses. Emphasis will be placed on the frequency range from about 1 to 3 Gigahertz with a special emphasis on the water hole between the hydrogen and hydroxyl lines.

We have no hope to detecting signals having complex modulation or spread spectrum or anything of that nature. We can only detect signals that are concentrated spectrally, that is, signals that have no more bandwidth than their duration requires. The simplest signals are CW signals, like our TV carriers, or pulses. The detectability of a pulse depends only on its total energy. So, in order to be just as detectable as a one-second long pulse, a pulse having a duration of a microsecond would have to have a million times as much peak power. Since the powers are already high for long pulses, we feel that short pulses are unlikely. With long pulses, both the pulses and the CW signals will be narrow spectral features. We are faced as always with the task of searching Gigahertz of spectrum to resolutions on the order of a few Hertz. To accomplish that we have developed a prototype of a spectrum analyzer. It is a design capable of expansion to 8 million channels. It is not a straightforward digital Fourier transform type of analyzer, rather it uses two banks of digital filters in cascade, finite impulse response (FIR) filters having essentially square passbands with 60 to 70 db rejection on either side of the bands. The point is that strong RFI signals in one of the final filter bands will not appear in any of the others. Thus the filters will isolate these strong RFI signals and permit us to receive over the remaining 99½ percent of the spectrum without interference. Two levels of digital filters bring the 8 Megahertz down to essentially kilohertz channels. Then multiplexed microprocessors take it from there on down to various bandwidths like 32 hertz, or 16, or 8 or 4 or 2 or 1 Hz. The resolution is programmable, and can be downloaded from a disk file to get whatever bandwidth we want. It's a very flexible machine.

Figure 1 shows the whole system as it appears at Goldstone. At the left is the VAX 750 computer, in the middle there's the tape drive and disk file, and on the right is the MCSA itself. These are affectionately known as the washer, dryer, and refrigerator.

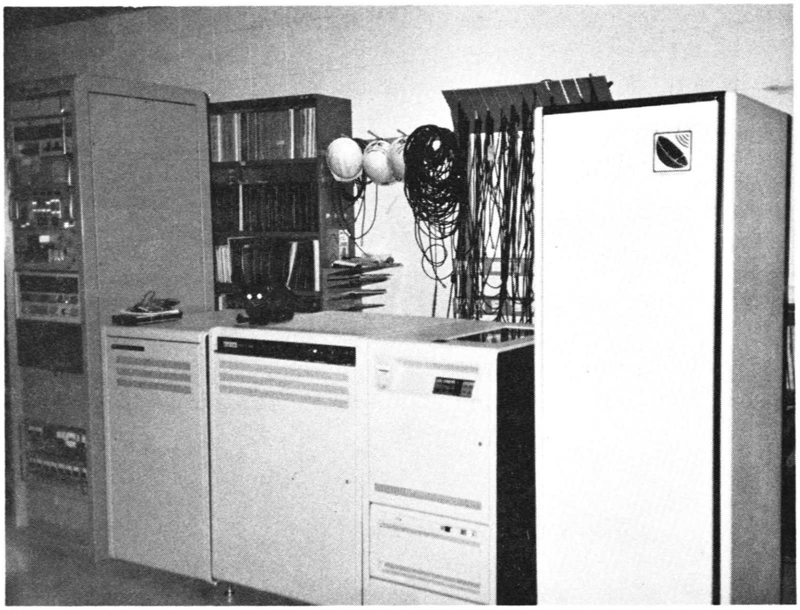

Fig. 1

Figure 2 shows what a small fraction of the output of the MCSA might look like. Each raster line displays, as brightness, the power in 120 adjacent bins for one spectrum sample. On this scale the full MCSA output

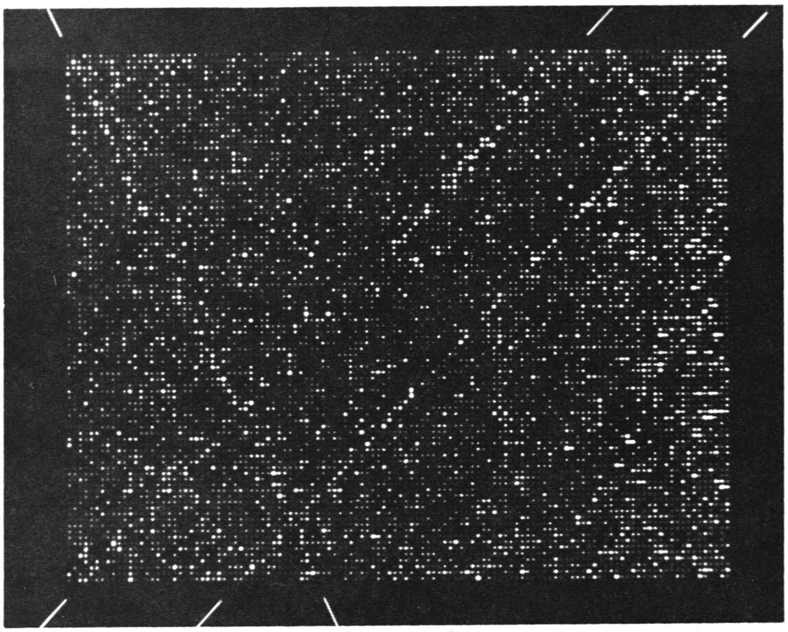

Fig. 2

of 8 MHz would be over 8 miles wide. About 90 successive samples are shown. There is a general field of noise in which three straight lines are visible. The two with a slope of 45° represent "CW" signals that are drifting one bin per sample time. The right hand signal is always centered in the bin at the middle of the sample, while the left hand signal always straddles two bins at that time. The loss in sensitivity from de-centering is evident. The third signal drifts upward one half bin per sampling time and is alternately centered and straddling two bins. Such drifts may be due to frequency instability of the sources or to acceleration along the line of sight.

None of these "CW" signals is evident in a single sample. Only when the entire raster is viewed does the problem stand out. One of our tasks is to automate the detection of such patterns. In the Cyclops study, we proposed to detect a drifting CW signal by storing all the spectra on magnetic disks, then reproducing them in analog form with various relative delays to skew them this way and that until we found the right skew that would make the slanting line vertical. Then all the signal energy would accumulate into a single bin to give a high resulting signal to noise ratio. That's a fine way to do things, but it has the great disadvantage that if you're going to observe, let's say for a thousand seconds, then you need to store a thousand complete spectra, and that is rather expensive. We are currently exploring an algorithm that saves memory and computation by carrying out the accumulation in three phases. In phase I about n=20 spectra having N bins each are stored in n full spectrum registers, and are tested for excess energy along all N(2n-1) lines. All these, say 1/10, that pass a lenient threshold are retained.

In phase II, which is longer, each of the qualifying lines of phase I is allotted some memory for a total of about 1/10 of a full spectrum register. Samples are now de-drifted before storing, using the crude drift rates found in phase I, in order to confine the signals to the assigned memory blocks. To store 200 spectrum samples again requires only 20 full spectrum registers. The possible mismatch in drift rates is now only 1/10 as great. Only those peaks now surviving a less lenient criterion are retained.

In phase III, only 1% of a full spectrum register is needed to store the surviving candidates, so in principle we have enough memory for 2000 samples. In the calculation made to date, 800 were used. The drift rates are further subdivided and the number allowed to pass the final threshold is small enough to keep the overall false alarm probability low enough.

Preliminary calculations indicate that this overall performance of this as yet unoptimized process is within about 1 dB of the Cyclops method, but the memory and computations needed are about 1/50 as great.

Figure 3 is a graph that shows the detection statistics associated with the integration or averaging of a number of samples. We're accumulating samples in such a way that the signal and noise always add into the same bin. The heavy curves show the thresholds needed to give the indicated false alarm probabilities. The lighter curves show the signal-to-noise (SNR) needed to have a 50-50 chance of detecting the signal. With a synchronous detector each doubling of the number of signals averaged decreases the required SNR by 3 dB. But since we don't know the signal phase, we must use a square law detector for which the improvement is less rapid, especially at low SNR's. With a single sample and a false alarm probability of 10^{-12}, we see we'd need an SNR of 14.33 db, while with a 100 sample average the SNR drops to -0.57 DB for an improvement of 15 dB. We were very happy about all that in the Cyclops report. We said, "Gee, that's a very good achievement, we're working below unity signal-to-noise ratio." It wasn't until quite a while later that it dawned on us that if all the energy that we had received in those 100 samples had been put out in one burst, so that we had received it in one sample, then the signal-to-noise ratio would have been 20 db higher than -0.57dB or up at 19.43 db, five db higher than actually required with a single sample.

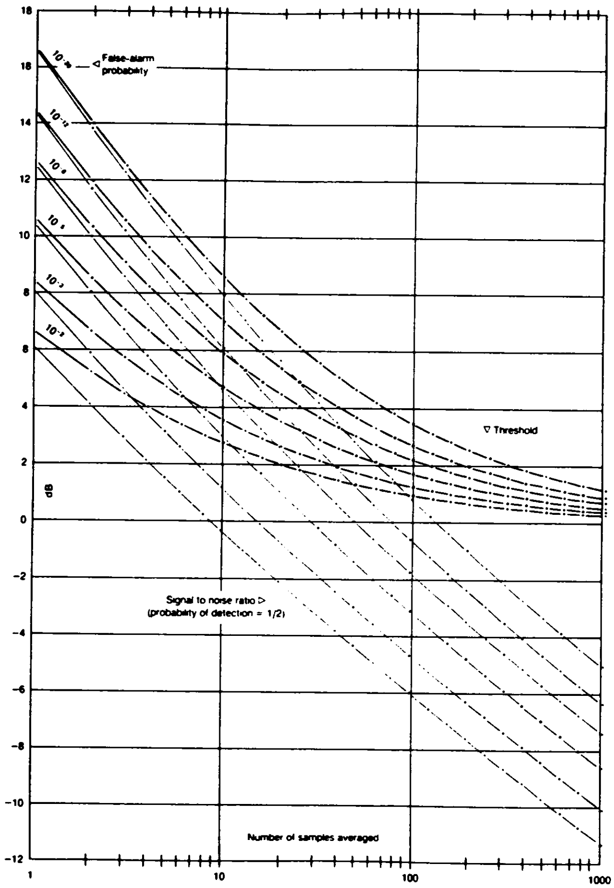

Fig. 3

What this says is that for a given <u>average power</u>, pulses are more detectable. The reason is you don't operate down at poor signal-to-noise ratios where the square law detector becomes inefficient. Instead you hold your fire, then you put everything out in a big blast which raises the signal-to-noise ratio. Then you don't waste any power between the pulses. The improvement gets greater the lower the pulse duty cycle. For a 1-second pulse every 1000 seconds, the saving is about 10 dB. So, as a practical limit, we require something like 1/10th the power in pulse transmission as against CW transmission. The thought now arises: If you were designing a beacon and you had the energy cost in mind, how would you do it? You would probably put out pulses. Maybe they would come to the same conclusion.

So we think we ought to be prepared to receive pulses as well as CW signals. Nobody has done that in the past, so that's another way in which our approach is a little more eclectic. For pulse detection we plan to set a rather low threshold so that about 1 bin in a 100 in the multi-channel spectrum analyzer will exceed threshold and to produce a "hit." Figure 4 is a field of hits produced by noise or maybe by noise plus a signal, but in any case by the total content of a bin exceeding a certain threshold. Displayed in Figure 4 are about 120 bins horizontally and about 90 spectrum samples vertically. As these hits appear sample after sample the procedure is to look for regular patterns: straight lines of hits, equally spaced in time and frequency. Kent Cullers has written algorithms that do this automatically. I don't know whether or not any of you have found the pulse train that's present in Figure 4, but Figure 5 shows what the computer found after 1.2 milliseconds. There is indeed a regular pulse train present.

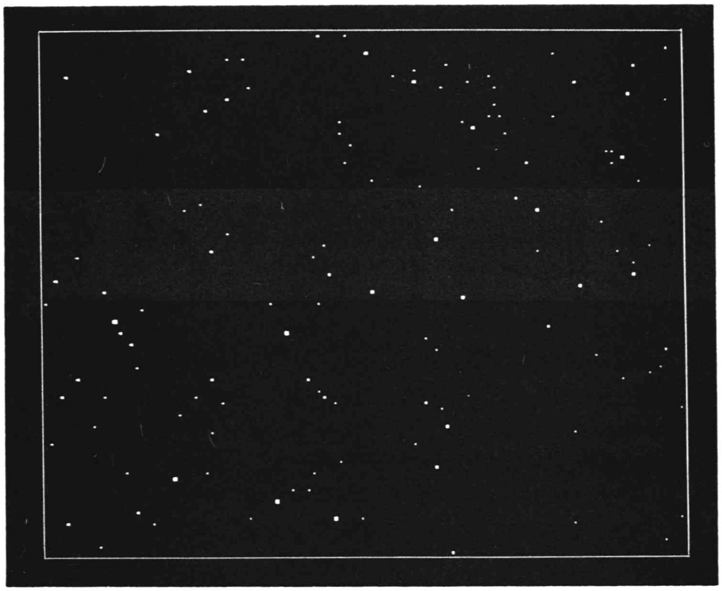

Fig. 4

We're looking for pulses regularly spaced in time that may, like CW, also be drifting in frequency within a certain narrow range. We're not looking for dots and dashes, we're looking for pulses having the same shape on each occurrence. We're also receiving both left and right circular polarizations on the hypothesis that a beacon will be constructed in the way that makes reception easiest. One wouldn't want to use linear polarization, because that gets rotated by the interstellar medium. With circular polarization the receiver has to be prepared for only two polarizations rather than six and there will be no polarization loss. That simplifies the receiver's job. If I were making a beacon, I would want to make it easier for you to hear me.

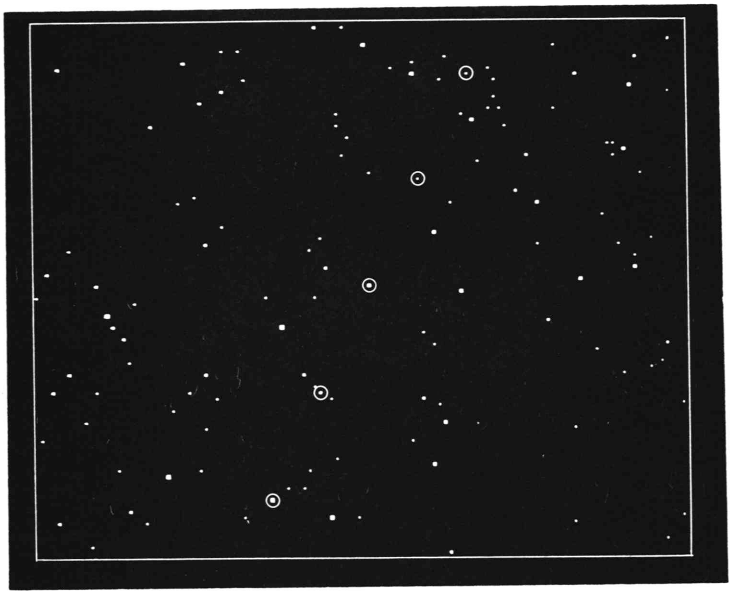

Fig. 5

Having two polarizations, we can assign one polarization to 0's and the other polarization to 1's and have a perfect binary channel which can be used for signaling without degrading the detectability of the signal in any way. That seems to us like kind of a natural way.

T. Bania: Could the algorithm work equally well for the pulse drifting up in frequency and down?

Yes.

G. Verschuur: What if the pulse is missing because of noise driving it down?

K. Cullers: I wasn't going to talk about that, but we have studied the problem.

We've studied the statistics and concluded, counter intuitively that you do in fact gain a small amount by allowing one or two pulses in a "train" to be missing. It is our plan to let the algorithm operate in this way.

M. Papagiannis: You can detect with this system only the pulses that appear at specific intervals, and if they don't appear at those specific intervals, you won't see them.

Yes. Well, you may question that choice of signal. On the other hand, there has to be some regularity about a signal to distinguish it from a natural ergodic process and certainly regularity of occurrence is one such thing.

M. Papagiannis: But if they do use the process that you just said, that is, use left and right polarization to send 0's and 1's, you would be missing a lot of these points.

Oh, no, we won't miss them at all. We receive both polarizations and look for regularity in the sum.

The prototype multi-channel analyzer has at least one board of every type needed in a complete 8 MHz machine. This results in a bandwidth on the prototype of 74,000 channels; we haven't made the whole thing because of money and time limitations, but we have a machine that does all the tasks that the final machine will do, and it has 74,000 channels. We propose to test Kent's algorithms using the VAX to do the searching for patterns. It can handle 74,000 channels in real time but it would be absolutely swamped by 8 million channels or more. So in the final system we're going to have to build a special purpose pattern detector. This will be a highly parallel type of machine, because the same operation is performed all along the spectrum. Although we haven't built it yet, the general form of the architecture is beginning to take shape in the minds of the design team.

Figure 6 shows in block diagram form what we visualize as the SETI system. First, there is the antenna, which we're not building, since we're

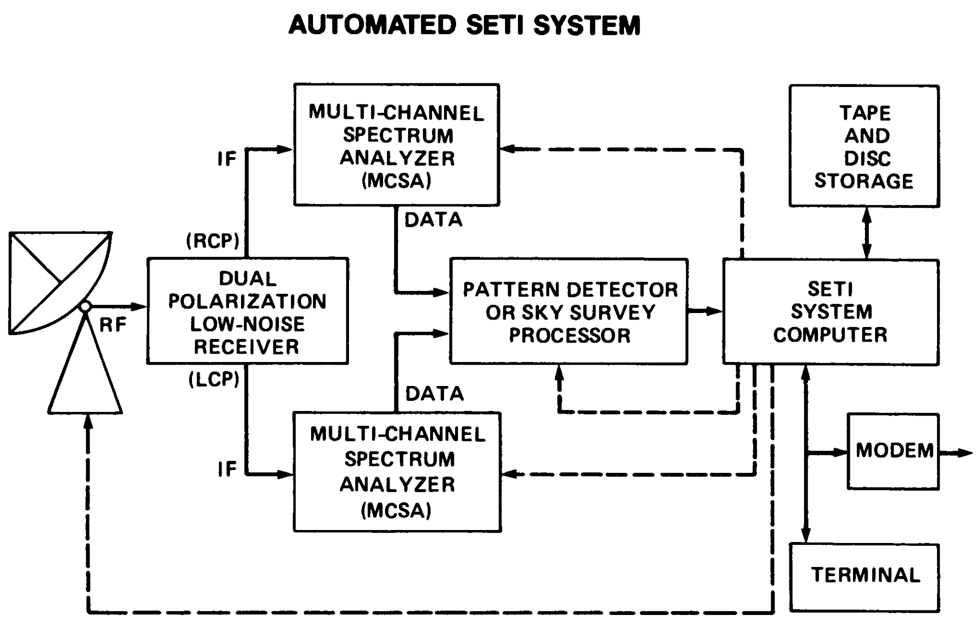

Fig. 6

planning to use existing radio astronomy antennas. Then there is a set of dual polarization low noise receivers to cover the bandwidths of the two searches. The IF signal from each polarization is fed into a multi-channel spectrum analyzer. The spectra from both analyzers are delivered to the pattern detector which examines the combined output for regular patterns or pulse trains. The pattern detector for the targeted search and the post processor for the sky survey are the next steps in our R&D program. The whole system will be ultimately converted to VLSI to make it smaller and cheaper. That's another task that lies before us.

We know how to build the MCSA's. We're going to learn in the next year and a half how to make the pattern detector and sky survey post processor. Then the R&D phase will be largely complete. Our plan is to make the system operate as automatically as possible under computer control. Only a computer can stand years of negative results and remain diligent. In the sky survey mode the computer can make the antenna scan the sky in a predetermined way and note the coordinates of any signals that exceed threshold. Later, in a verification mode it can return the antenna to these same coordinates and examine for an extended time any signal found.

In the targeted search mode, the computer can select from a tape file a succession of target stars as they near the meridian. If a signal were detected, the computer would interrupt the search and execute a series of tests to verify that the signal was of extraterrestrial, possibly ETI, origin and not mundane RFI. Such tests might include:

-- Move the antenna off target and see if signal disappears.

-- Restore antenna pointing and see if signal reappears.

-- Start recorder.

-- Conically scan the antenna around the line of sight with half angle calculated to drop signal strength 1 or 2 db. (If signal strength remains constant, source is on main lobe.)

-- Restore pointing and track sidereally long enough to be sure celestial coordinates of source are fixed.

-- Check ephemeris to make sure no known spacecraft lie in that direction.

If the signal meets all tests, the computer, using the modem, wakes the director with, "Hey boss, I've got something interesting. Better have a look." If the director agrees that it is interesting, the final test is to call, by prearrangement, some other observatory and say, "We have an interesting signal, right ascension so and so, declination so and so, frequency such and such. Do you copy?" Without an affirmative answer from at least one other observatory, we would hesitate making an announcement because there might have been a Caltech student in our software.

T. Kuiper: Barney, I guess I am laboring under some kind of misconception, maybe you can clarify it for me. It seems to me that if you *have a given amount of power available, then what matters is how you distribute it over cells in* $\Delta\nu - \Delta t$*, and the more you can pack into a small number of cells, the better off you are. But given some practical limitation after that it doesn't matter how you distribute them around as long as the people on the other side know what your pattern is. Is that correct or is that wrong?*

Well, that's the point; you radiate a pulse that has no broader a spectrum than its duration requires. What its duration is, we can't say; we have no natural length we can assign to it. We don't know of any natural phenomenon that suggests a "natural" duration on the order of a fraction of a second or seconds, so we have to be prepared to receive a wide variety of pulse durations using approximately matched filters.

There are some problems with pulse detection that I haven't mentioned. There is a problem arising from the fact that all of the data that comes out of the MCSA and goes into the pattern processor is discrete. We have discrete bins and a pulse can fall between bins. It can also fall between two spectrum samples. If it falls between two samples, it is only half as long in each so it spreads out in frequency in both spectra, and the resulting loss is 6 db or more. We're presently looking at ways to eliminate this loss. I hate to lose 3/4 of our antenna area when it's too small already. So we're looking at ways of developing "inter-bins" by adding the real and imaginary parts of the adjacent bins. Another thing we think we can do is to make inter-spectra, in other words, spectra composed of 1 hertz wide bins that come 2 times per second instead of once per second. With twice as many spectra per second, there is overlap. Now, a pulse can't fall in the cracks. There are also more general processes that are being studied, such as applying convolutional filters to the data to find where the peaks really are. We're looking at the process that that requires. So there are some other aspects, when you get down to the nitty gritty of this, that are important to consider.

K. Cullers: Yes, I'll show a slide that shows some of these problems.

W. Sullivan, III: I'm fully in favor of looking for pulses as well as CW, but don't you overstate the case for pulses in the sense that there's no free lunch? It seems to me that the fundamental disadvantage of putting all your energy into a pulse is that now it's quite possible to spend your 1000 seconds or any number of seconds on a star, go away and have missed a single pulse, because the spacing was longer than your observation.

That's possible.

W. Sullivan, III: That is the great disadvantage; otherwise, why not save up energy for 100 years, and spread it all out in a single pulse.

If you are conducting a search and you're going to cover 1,000 stars, or 10,000 stars or more, there is a limit to the time you can spend on each one. A beacon designer should make the pulse repetition period short compared to that time. Then the receiver will be sure and get at least one pulse.

W. Sullivan, III: You mean they should be thinking this way.

Right, it's a joint game.

W. Sullivan, III: Well, if they have some idea of how many stars we might be searching.

Well, they would too.

W. Sullivan, III: No, they have to know how many years we would get funded.

No, seriously.

C. Seeger: No, seriously, you can't look for all signals.

W. Sullivan, III: No, but otherwise this is a disadvantage of pulses which you don't mention.

It is a disadvantage, I agree. The advantage of CW is that it is always there. A disadvantage is that, per se, it is not information bearing---

W. Sullivan, III: Well, pulses aren't either.

Yes, they are; these pulses are information bearing.

W. Sullivan, III: No.

You didn't hear.

W. Sullivan, III: You can also reverse the polarization of a CW signal.

Not without effectively converting it into pulses. The receiver has to wind down and then come back up again in the opposite phase, so we've lost some detectability.

G. Verschuur: Can you contrast this system with Paul's? What are you doing that's very different?

In the first place, we are looking for pulses. In the second place, we are looking for signals that have drift. They need not be Doppler compensated to the extent he requires. He's got a large number of very, very narrow channels, so he requires a high degree of monochromaticity in the signal. He is testing the hypothesis that they are beaming something at us and are Doppler correcting it for their acceleration. If we do the same Doppler correcting at our end, we end up with a signal that's highly monochromatic. Under those assumptions, you can get the high sensitivity which he has shown you. But you have restricted the signal space quite a lot and you may be missing other types of signals. We've broadened quite a lot the class of signals that we will receive and have to do a lot more data processing on a faster scale to do it.

F. Drake: Then what is in the NASA system is about 100 times greater than Paul's system, so the overall bandwidth covered is about 100 times *greater because they have the same number of channels. This means that they can cover the whole microwave window in a reasonable time, but Paul is restricted to a much narrower region.*

G. Verschuur: So, these are very complementary experiments.

S. von Hoerner: What about your time schedule? What comes in the next years and when will it operate?

Looking to the future, we are trying to get additional manpower on board to design and build the pattern processor. We would like to get it designed, built, and tested in about a two-year period so that we would have the hardware defined, and know what we want to build in time for a 1988 new start. If we don't get some additional funding, we won't make that date. In the new start, we'll ask for a sharp increase in funding for a few years, 2 or 3 years, and go out for a request for bid on the actual equipment to be designed by a commercial supplier. We would like to get several replications of the equipment so that we would have a northern hemisphere observatory and a southern hemisphere observatory, and more than one observatory working at a time. One of the serious limitations on this program is that we do not have a dedicated facility, and therefore we cannot be on the air 100% of the time, except at Ohio State. I think I'm going to have to move ahead and if there are more questions to be asked of me, we can do that during the break.

Note added later:

Figure 7 shows the display on the video monitor of the MCSA. The top window shows the power in 144 adjacent channels each normally 1152 MHz wide.

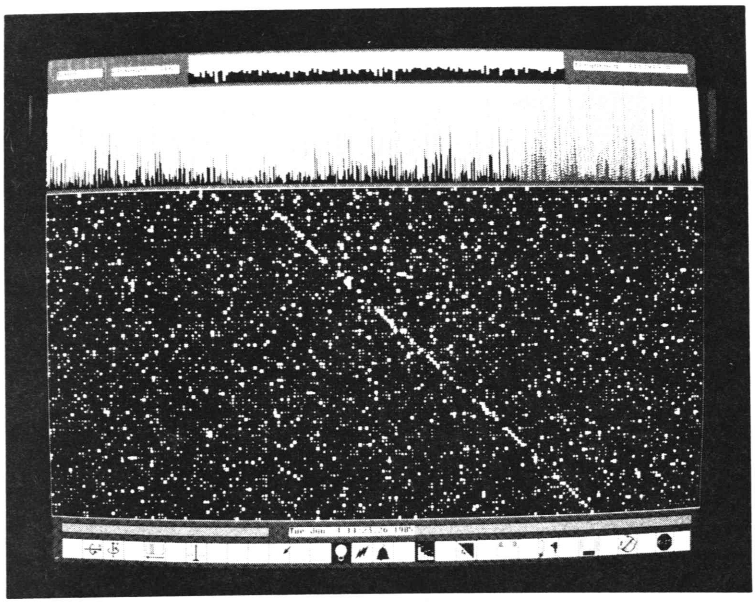

Fig. 7

The middle window shows the power in the central 1024 bins (each normally 1 Hz wide) from one of the 1152 Hz channels. The bottom window displays, as brightness, the power in 200 adjacent bins, for 100 successive samples. As each new sample is added, the display scrolls up by one line. The icons along the bottom are selected by the cursor and are used to modify the display. For the display shown, the MCSA was running at half speed, so the channels are 576 Hz wide and the bins are ½ Hz wide.

The drifting CW signal is the 1 watt carrier of Pioneer 10, now 3.3 billion miles away and outside the orbit of Neptune. The 32.6 dB antenna gain of the spacecraft makes the effective isotropic radiated power 1800 watts. The receiving antenna was the 26 meter dish at DSS13. The signal is too weak for the receiver at this location to achieve lock, so a stable synthesizer was used as the local oscillator. Most of the drift is due to the earth's rotation.

Narrowband Signal Detection in the SETI Field Test

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Well, as you know, NASA is soon going to have an MCSA. It's going to put out a large amount of data. If we can do synchronized pulse detection we have gained probably a factor of 5 or so in sensitivity in a 1000 spectrum long observation.

Now, the very first thing I'm going to explain is a synchronized pulse and an unsynchronized pulse. CW is probably what you know about already. In CW you accumulate the power for 1000 seconds along the line in which the signal exists and you test that accumulation. What you do to compare any system, or group of systems, is first that you set the false alarm rates to be the same, somehow. You choose, in other words, a set of system parameters that gives you the same number of false alarms in all cases. Then you put in a signal. The system that detects the signal in presence of noise more often is the more sensitive system.

Consider a synchronized pulse, an unsynchronized pulse, and a CW signal. Each of these three signals deposits the same energy in 1000 seconds. There are four pulses in the pulsed signal. So each one of those pulses has a lot more energy than any single sample of the CW signal. That's why the pulse detector is more sensitive than the CW detector. You have to look at fewer samples for pulse detection. Furthermore, each one of those samples has a much better signal to noise ratio than one of the 1000 samples summed to concentrate the power in the CW signal.

Let me clarify the difference between a synchronized and unsynchronized pulse. In a digital instrument you have a frame of data which you Fourier transform. If the pulse is synchronized with the data frame it is periodic and it's on the center of a channel, so it doesn't spread power around. It lasts as long as the data frame, which means that it deposits all of its energy in a single cell (a single channel in a single spectrum). For an unsynchronized pulse, one can imagine a particular bad example. Split the pulse exactly between two cells. Make it one data frame, say a second long, but a half second pulse in each of two consecutive frames. So, for example, the last half second of one frame, and the first half-second of the next frame would be filled with energy. This cuts the power in half in those two spectra where the pulse is. Furthermore, since it's not matched to the frame anymore, the power spreads in frequency, which accounts for the loss. That is enough loss that you're almost as bad off with a very simple minded pulse detector as with the CW detector. If the detector doesn't try to save you in some way by lumping this

*National Research Council - NASA Ames Research Associate power back up and recognizing that it's a single pulse spread around you have only the sensitivity of the incoherent CW detector.

Consider the following equations:[1, 2]

DETECTOR SIMILARITIES

Synchronous Detections

$$G_{1/2}(P) = (1/\sqrt{\pi P})e^{-P}$$

where $G_{1/2}$ is the probability of false alarm and P is the total energy.

Square Law Detection

$$G_1(P) = e^{-P}$$

$$G_n(P) = \sum_{i=0}^{n-1} (P^i/i!)e^{-P}$$

Regular Pulse Detection

$$G_r(P) = q(r)e^{-P}$$

where g(r) is the number of distinct regular patterns containing r pulses.

The General Form

$$G(P) = h(P)e^{-P}$$

$$\frac{\ln G}{P} = \frac{\ln h}{P} - 1$$

$$P = -\ln G$$

These equations give the false alarm rate for a bunch of different detectors as a function of threshold in power. Synchronous detection is matched filtering. Square law detection can be performed on a single sample or on a lot of samples added together. I should probably explain regular pulse detection. When you look for a lighthouse beacon you perform regular pulse detection. You perform a thresholding operation to eliminate background noise which makes the operation efficient. It eliminates a lot of data that you would have to work on otherwise. Then you look for regularity in the above threshold events which brings your false alarm rate down further. Basically the pulse detector looks for three or more pulses that are regularly spaced and then extrapolates and says "Are there some more out here, where they should be?" If they are there, then it qualifies the train when the probability of occurrence is low enough. If they're not there at some point it quits looking at that group of possible regular events.

The general form for these equations implies that when the false alarm rate is low and the threshold is high, you get the same threshold for all detectors. Thus, they're all equally sensitive. The point is that you might be able to save a lot in processing by using square law detection. You don't necessarily have to do matched filtering at low false alarm rates which of course is computationally very intensive.

Figure 1 is less theoretical.

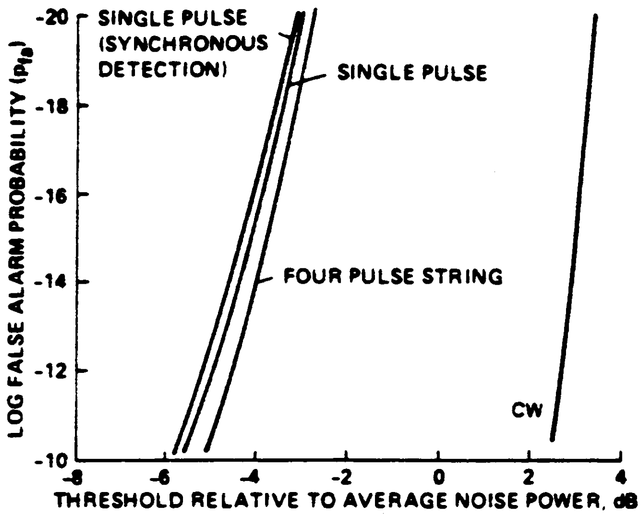

Figure 1.

That last set of equations implied that all the thresholds are the same, but of course, they're not. This figure is showing the false alarm rate as a function of threshold, 100 samples are tested here. As you can see, matched filtering does the best. The single pulse detector does quite well. The four pulse detector still does pretty well, and CW looks terrible. I can explain why these curves look like this. First of all, matched filtering is going to do best. Square law with a single sample only adds a little bit of noise to that. So you only have to raise your threshold a little bit to get your false alarm rate back down. When you have more pulses to look for you're adding more noise in, and of course, with CW, you're adding a great deal of noise into the channel. So part of what you're seeing in the CW case is deceptive, because really when you want to know what signal you can detect you want to know signal to noise. In the CW detector there are a hundred samples of noise added together and so you need much less signal to actually have signal plus noise be above threshold. So the CW detector is not as bad as it looks.

These results are altered slightly if false alarm rate is plotted vs. signal to noise ratio. In this case, CW is still not as good as the others but it gets better. Often as a rough cut, you can say that systems that have lower thresholds are going to be more sensitive, but you've got to worry about certain difficulties. One of them is all the noise in the CW detector. The other one is associated with the multiple pulse detector. You can wipe out that detector by missing a single pulse; the noise can knock it out, and you actually have to raise your individual pulses, not just set them at the signal plus noise equal to the threshold, but you have to set them higher so that the noise won't destroy your pattern. That is a problem we are continuing to work on and to which we have some reasonably good solutions.

I want to talk about some of the problems that we're now coping with. First of all you might not think that too much sensitivity can be a problem but it can. In the pulse detector when the false alarm rate is low and the threshold is high and you have this unsynchronized pulse problem where the pulse doesn't fit neatly into a bin but sprays power around, there is an easy way to pick it up again called pseudo-binning. In pseudo-binning you sum up all the powers in nearby cells and test the total. If you do that, just on power, that's a pretty effective method when the signal-to-noise is high. This is because the signal-to-noise is high anyway, you're adding a little bit of noise, you have to raise your threshold a little bit and that doesn't hurt your sensitivity very much. Now as we have worked on the pulse detection algorithms and realized that there's going to be some thresholding in hardware on the MCSA, we've realized that we can set our threshold lower and lower. When the threshold comes down to 4 or 5 times the noise instead of 9 or 10, then suddenly the method of adding power starts to hurt you a lot, because you're adding quite a bit of noise in with the signal you hope you're picking up in adjacent bins. So what we really need are true interspectra and true inter-bins. We need true samples so that if a pulse falls between two spectra it will be coherently picked up. That is, there will actually be a bin to match it. And also if it falls in between in frequencies, you can pick it up. Thus, we've got to work on complex data. There are a couple of methods for doing inner spectra which Barney alluded to. Inter-binning looks like it's pretty straightforward. There's a very nice effect where if you simply subtract the complex numbers from two adjacent bins something that's halfway in between two frequencies pops right up, and you only lose about a dB in sensitivity. Of course, that's not a function of the signal-to-noise ratio anymore since you're still in the linear regime.

Another important problem is that you need to save memory size in CW. If you're going to do incoherent CW detection, which really looks like the only practical thing to do right now for 1000 seconds, you must test millions of channels at 1000 drift rates if you're going to test up to a Hertz per second or so. This might be necessary if the signal came from the surface of a rotating planet somewhere. If you're going to do this on all your data, that means that you're saving gigabites of data. The question is how can you use less memory? You can do that by creating a multiple stage process. If you take, say, the first 10 or 20 spectra or so you can't differentiate 1000 drift rates at that point. The number of drift rates you can differentiate is proportional to the length of the observation stage that you're processing. So you start, and as with pulses, you save by thresholding. You start at the beginning and have a relatively short observation. You set a very high false alarm rate on that stage. A lot of things will pass. They'll supply you with a frequency and a drift rate which is a guess that there might be something there. Then you have a longer stage and you subdivide your drift rate, so that you're testing more drift rates over a longer period of time at each possible good frequencies. Then perhaps you form a third stage.

The process described above keeps the memory usage constant from stage to stage. For example, you may pass along one in 10 candidates from the previous stage. Then you divide into 10 more drift rates in the next stage keep the whole process moving along using about the same amount of memory or maybe even less than the first stage used. In Figure 2 we have a specific example of this.

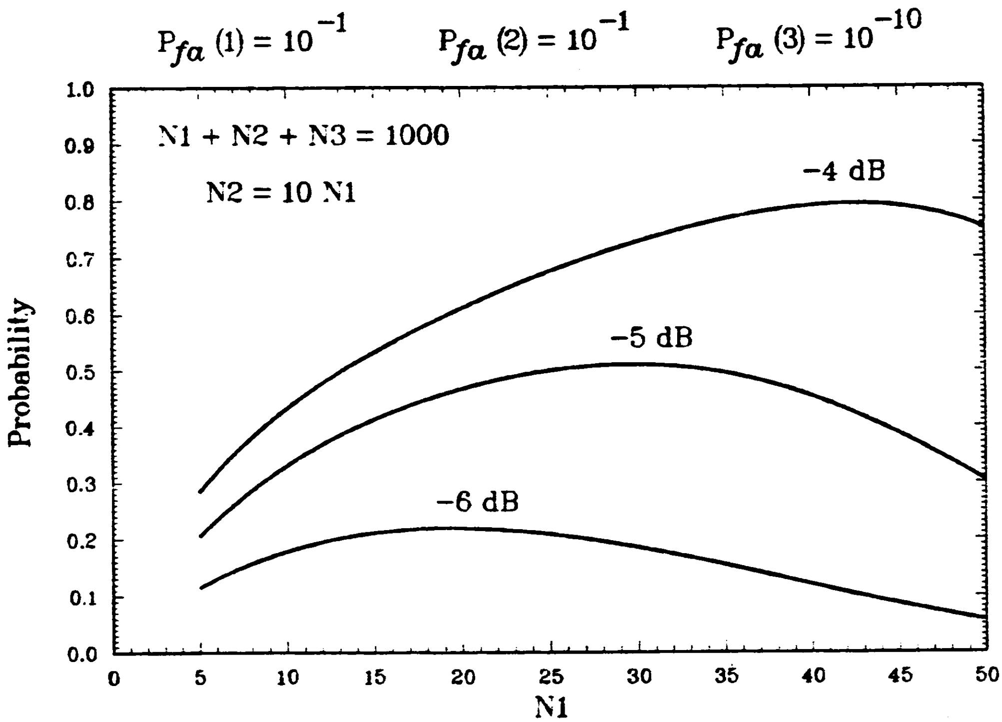

Figure 2.

The thing that's being plotted on the vertical axis, is the probability of detecting the signal. Obviously you can badly mismatch these stages if you do this process incorrectly. For example, if you set the false alarm rates in the first couple of stages too low, then, since you haven't observed for very long, you have to have a terrifically strong signal in order to see it. On the other hand, if you set the false alarm rates very high in the first stage so that everything passes the first and second stages, then you are not doing any work there and the third stage does it all. You're back to a one stage observation. You've got to jiggle these numbers to get your best results and what you see here is something that we did. This was a particular guess that we tried on one day. It represents a reasonable set of parameters that do not appear to lose much sensitivity. Certainly you do not expect that when you divide your process into many stages and threshold, that you're going to gain sensitivity. What you would like is to keep as much sensitivity as possible and save a lot of memory. What these curves show, especially the one at -5 dB is that you can detect the signal about half the time, if your first stage observation is about 30 spectra long.

We fixed some parameters in here. The second observation should be 10 times as long as the first, and the total should be 1000 seconds. The significance of the -5 dB is that it's only about 1.2 dB worse than you can do if you did the whole observation for 1000 seconds, and yet you've used a fortieth as much memory as you otherwise would use to do the task. Also, you use much less computing power because you don't have to process so much data all the time. So you've really generated a significant saving.

In addition to the good things that have happened with pulse detection, if in fact we can do it effectively, we are now developing a strategy to save memory in the CW detection phase. It is important to remember that this is not yet an optimized process. Remember that what you see in Figure 2 are numbers that we threw in one day to make a curve that was easy to generate. Fortunately our guesses were not too terrible because we didn't lose too much sensitivity. But it's clear that you can optimize that process, and that's really what we're working on now.

Let me discuss briefly current software features. We do, in fact do non-drifting CW detection. This is all done with a VAX on as many channels as we can handle. That number is one of the things we're going to find out in the field, working with a real-time operating system that Ivan Linscott may talk about a little bit after lunch.

In asynchronous pulse detection we're going to try many things. We're going to try pseudo-binning. We're going to try interbinning and interspectra in software, which, of course, will be very slow. (We've got to eventually have hardware to do this stuff.) There was also another technique which we tried for awhile to actually estimate where the next pulse was going to be, and how much data you have to sum together to pick it up. We found that technique a failure, you just don't have enough data after 1 or 2 pulses to make that estimate, and if you can't make a decision where the next pulse is after one or two pulses you miss a lot.

Now, I'll explain the interpolative and extrapolative pulse detector. Interpolative just means that we used to wait until the end of the observation, and then find the pulses in between the first and last one. That has some computational advantages, but the extrapolative predictor in fact turns out, overall, to be faster although it requires more data storage. It also gives the advantage that if you have a big strong signal, it gets recorded early in the observation, and you may not want to look at that one anymore.

One other thing I should mention is thresholding on single pulses and total power. One way to do pulse detection is not to worry so much about missing pulses, but to set as low a threshold as you can for your pulse detector determined at the rate by which you can process. Then, just test the total power in any string. You may see some group of pulses and it implies that there should have been one in the middle that you missed, and some at the end that you haven't seen yet. You just do a test on the total power in that whole group. In fact we've got the statistics worked out for that, and now an almost working program to do those kinds of tests. My guess is that might be the most efficient pulse detector we have.

The issue of finite and infinite pulses is the last thing I want to mention. You can define many different regular pulse trains in an observation. That's why, in fact, the pulse detector isn't quite so effective in some sense. There are more ways you can get a false alarm than if you require, for example as in a CW detector, that the signal is there all the time. You can create a lot of different looking pulse trains. The finite train just means that you've got regular spacing, but the train doesn't continue through the observation. This doesn't seem very likely as a SETI signal. The infinite pulse train which we typically look for means that the first and last pulse that you eventually identify imply that the earlier ones and the later ones fall beyond the ends of the observation. That's why you didn't see them.

That is a summary of the statistics and how they work and what we're looking for now as field tests begin. Now, I would be happy to entertain questions. Thank you. Any questions?

Bob Dixon: How does this duty cycle on the pulse train enter into any of this?

Kent Cullers: If the duty cycle is low, the power per pulse is very high, which means the signal to noise ratio is high when the pulse is on. You've got a lot of signal and not much noise per sample and that's effective. As you run the number of pulses up, the cost of thresholding becomes significant. You distribute the energy around so much that the signal to noise ratio per pulse becomes low and noise destroys the pulse train. That's somewhere around 12 or 13 pulses in an observation of 100. So the duty cycle just affects the energy per pulse.

References:

1. Signal Recognition, D. K. Cullers, B. M. Oliver, and E. T. Olsen. Report of the SETI Science Working Group, Appendix C. NASA Technical Paper 2244.

2. Sensitive Detection of Narrowband Signals, D. K. Cullers. Presented at the 35th Congress of the IAF, to be published in the Acta Astronautica, 1985.

Are 100 Million Channels Enough ??

Ivan Linscott

RadioScience Lab
Stanford University

This talk is concerned with the question: "How many channels are needed to detect an artificial signal of extraterrestrial origin ?" I feel the number of channels you need is closely tied to what you expect to find, and there are many views as to what that might be. For this talk I will confine my attention to the search for two types of signals, drifting CW, and pulses. These are the types often found in discussions on the nature of an ETI signal.

To find out how many channels are enough to insure a reasonable chance for detecting ETI signals, I would first like an estimate of how many "individual" signals are within these two categories. Then I would propose to build a matched filter for each individual in the set, and finally check to see if all my filters can possibly fit into a box for shipment to a radio telescope. For I suspect that only when the box is full to overflowing would I be willing to consider that I have enough.

Let me proceed with my first estimate of the number of individuals, or "channels" as I am prone to call them. I will consider a "channel" as a label that can characterize a signal capable of being detected through the use of a matched filter. In so doing, I will make the additional assumption that my interesting signals can in fact be characterized by a signal processing procedure known as a matched filter, and I will define that concept in a minute. This assumption is true for simple signals, like the two previous types.

Under the conditions I have just described, the number of channels I need is the product of the number of different types of signals I intend to look for, and the number of matched filters contained in each type. For this talk I am considering only two types, drifting CW, and pulses. A signal within each type can possess several independent attributes, like frequency, drift rate, and repetition rate. Each attribute in turn could be chosen from a range of interesting and desirable values. By multiplying the number of values times the number of attributes times the number of signal types you could easily conclude that you need a very large number of matched filters, and therefore channels, to detect an ETI signal.

Matched filter signal processing is, as we have heard earlier, an expensive proposition. You don't want the number of channels to be too large. But how large is too large ?? We will need estimates for both the number of channels and the cost per channel to answer that question.

Following my prescription for estimating the number of channels, I will use the following for estimates of the range of interesting signal characteristics. My estimates will be biased toward those choices that I might actually build into hardware. Please keep in mind that I would like to converge on choices that result in a cost effective instrument with enough channels to give us a reasonable chance of detecting signals we think are interesting.

frequencies	10 Million	(10 MHz band with 1 Hz resolution)
drift rates	2000	(up to 1 Hz/sec over 1000 sec)
repitition rates	500	(limited by 1000 sec window)

You will notice that the number of frequencies is an order of magnitude lower than the number of channels in the title of my talk. I picked this as a starting place because when you perform the multiplications I just suggested you find that the number of matched filters we need is then 10 Trillion, (10-13 "channels").

In SETI we have often discussed the prospects for increasing the number of frequencies covered by an MCSA from 10 Million to 100 Million. As you can see that would boost the number of matched filter channels to 100 Trillion. I won't say that 100 Trillion "channels" is enough, but it would be interesting to estimate the cost of implementing that many channels.

The cost of a matched filter is closely related to its complexity. A matched filter is ideally a signal processing technique that is capable of projecting all a signal's energy into a single measurement of a physical quantity, such as power. As an example, the combination of a narrow bandpass filter with a power law detector, would be a matched filter for a monochromatic non-drifting sinusoid. I will estimate for you the complexity of matched filters for the drifting CW and pulsed signals, and then show you how the complexity estimates can be related to cost.

From here on I will assume that matched filter signal processing is implemented as a set of numerical operations performed on digitally sampled sequences and these sequences faithfully represent our signals. The assumption of a digital process is not essential, but it does at times simplify the ability to estimate the complexity. I will define the complexity, C, of a process as the number of operations performed per sample. By "operation" I mean a simple numerical operation such as addition, or multiplication. In these general terms,

$$C = \frac{\text{Total number of numerical operations required to implement a matched filter.}}{\text{Total number of samples of the signal used in the matched filter process.}}$$

Introducing the concept of complexity in this manner provides us with a simple means of proceeding to a cost estimate.

Whenever you match to a range of signal characteristics, such as frequency, or drift rate, you find the number of operations you must perform to achieve the match is typically of order N*N, where N is the number of samples of the signal's waveform. In these cases we say the process is represented by a

2-dimensional transformation of the signal into N matched filters. The Fourier transform is an example of this, and can be thought of as having N matched filters for sinusoid sequences of length N.

But, as in the case of the Fourier transform, you can hope to find an efficient algorithm for performing the transform. If you are successful, the number of operations and therefore the complexity is less. For the Discrete Fourier Transform (or DFT), there are a variety of efficient algorithms, e.g. the fast Fourier transforms or FFT's, where the number of operations is portional to N ln N, where ln N denotes the logarithm to the base 2 of N. For the FFT's complexity is,

$$C = a \ln N$$

and the factor "a" is the proportionality constant.

According to a theorem by Y. Pan, an associate I met at Stanford in the Computer Science Department when I first came to Ames, ln N, is the minimal complexity for a 2-dimension transform. I am going to assume for the purposes of our estimates that our matched filters are characterized by 2-dimensional processes, and that we will be fortunate enough to find efficient algorithms for them all. Thus, all our matched filters can be assumed to be minimally complex and of the order of $C = a \ln N$, where the constant a, is a scale factor peculiar to a particular matched filter. For the Fourier transform, $a = 4$, and I would say that a scale of 4 is not unreasonable in general. We will have reason later on to challenge this assumption for the case of drifting pulses.

Minimal complexity is an important condition. It means that I only have to spend (Na ln N) operations to compute N matched filters. Or, for a machine that is running in real time, I need to compute just (a ln N) operations for each sample of data. Consequently, if I am sampling complex data at a rate of B samples per second, then I must perform B*C operations per second to keep pace with my data. This implies that for a real time matched filter process, I need a performance of,

$$\begin{aligned} \text{Performance} &= B^{*}C \\ &= B^{*}(a \ln N) \end{aligned}$$

operations per second to support signal detection in a bandwidth B.

Now that we have the performance requirement, we can estimate costs by choosing a hardware processor architecture. This in turn provides us with an estimate of a processor's performance and cost. Taking the ratio of total performance to performance per processor gives us the number of processors required. Knowing this, and the cost per processor, we arrive at the cost estimate for implementing the matched filters.

We are now able to estimate the performance needed for our 10 Trillion matched filters. I view the matched filter process as proceeding in two stages. First a simple match filter is applied to the data, with the expectation that an approximate match is obtained to the signal type. This first stage is the current MCSA of the NASA-SETI approach, where the signal is filtered by cascaded bandpass filters into 1 Hz resolution channels. For both slowly drifting CW signals, and pulsed CW signal, the 1 Hz channels of the MCSA are a good approximate match to the signal's characteristics. In both cases, however, the signal cannot be integrated without further processing beyond the 1Hz outputs. (The complete MCSA will have resolutions of 1,2,4,8,16, and 32 Hz. The matched filter approach outlined here for 1 Hz will be applied to each of these coarser resolutions.)

In the second stage, an additonal matched filter is applied to further integrate the signal. This process could, for example, integrate channels in successive time frame, where the channels are chosen to lie along a line in the frequency - time plane. Alternatively, another process could look for pulses as a regular sequence of bright channels in successive time frames.

Invoking the assumption of minimal complexity for the matched filters in each stage, the total complexity becomes,

First Stage		Second Stage
$C = a \ln N1$	+	$b \ln N2$

I will choose N1 = 10 Million, since I need a sequence at least that long to obtain a resolution of 1 Hz in a bandwidth of 10 Mhz, and I will further choose N2 = 1000, since that is the total number of time frames I expect to integrate using the 1 Hz resolution. I will further choose a = 8, since stage one is that part of the MCSA that is producing bandpass filters, and bandpass filters roughly double the complexity over just an FFT. Finally, I will choose b = 200. This last choice is very sensitive to the efficiency of the CW and pulse detector algorithms. The choice made here is based on some preliminary, aggressive tree pruning algorithms, and is the least certain. I will return to discuss other choices later.

Combining these estimates gives a total complexity to compute our matched filters of,

$$C = 8*23 + 200*3$$

$$= 1000 \text{ operations/sample, (approximate).}$$

Since the sample rate is 20 Million samples/sec, the total performance required in order for this machine to function in real time is,

$$\text{Performance} = 20{,}000{,}000 \text{ samples/sec} * 1000 \text{ op's/sample}$$

$$= 20 \text{ billion operation/sec}$$

$$= 20{,}000 \text{ MIPS.}$$

In the last line I have incorporated the assumption that one operation can be performed in one machine instruction, consequently 1 Million op's/sec is 1 MIPS. It is interesting to conclude this exercise by saying that the prototype MCSA we have developed at Stanford uses processors that perform at the rate of 20 MIPS. Although we do not intend to use the existing processors for the second stage matched filter processing, (often referred to as the pattern detector), if they were suitable, and if we choose to use them, then you see that a total of 1000 processors would be needed to support the required performance.

The requirement of 1000 processors raises several interesting issues. One is whether an efficient means of matched filter processing exists such as to justify the assumption of minimal processing complexity. Another is whether a processor architecture can be designed to insure that both stages of the MCSA possess comparable performance. Lastly, can an implementation of the processor be managed so as to keep costs down to where a 1000 processor machine is affordable.

Currently, all three issues are undergoing rapid development in our group at Stanford and at Ames. I will present a brief review of the work in progress, and hope to leave you with our feeling of optimism that a 10 Trillion matched filter machine is affordable.

With numbers like a trillion flying around I would like to give you some confidence that we at least have the MCSA bandpass filter stage in hand. I will briefly review the organization and implementation of the bandpass filters in the MCSA. Figure 1 is a block diagram of the functional components of the MCSA. I draw your attention to the two bandpass filters followed by the final DFT which brings the resolution down to 1 Hz.

Each bandpass filter produces a bank of filter channels. The bandpass filter signal processing is Allen Peterson's technique which first takes a "coarse" convolution of the sampled data sequence with the filter's impulse response, and then follows the coarse convolution with a Fourier transform. The convolution is performed in a weighting network, as is shown in Figure 2. One weighting network apiece for the two filter banks are used. Bandpass Filter 1's was designed by Kok Chen, a student of Professor Peterson, and Bandpass Filter 2's was my design.

Next the convolved sequences are Fourier transformed, which has the result that the channels of this DFT represent successive filters in a bank. The DFT's are computed using an efficient decomposition algorithm based on prime number factorization developed in the thesis of Professor Peterson's student, Shankar Narayan. The DFT algorithms run on a high performance, microcoded signal processor, designed at Stanford by another of Professor Peterson's students, Patrick Barkhordarian.

We felt that the key to a successful design for the multiple resolution MCSA was to incorporate a microcoded signal processor. In this manner the same hardware could be used at each bandpass filter level, as well as the final DFT, each running it's own version of DFT code appropriate to that level. The architecture of the processor Pat designed, capable of doing this is shown in Figure 3. It consists of a double buffered memory for real time data management, and three arithmetic components: adders ALU1, ALU2, and the MAC. The connectivity of the arithmetic units was chosen to optimize the performance for the short length DFT's associated with Shankar's prime factor DFT algorithms.

This is a highly concurrent processor, where it is necessary to specify a combination of 12 arithmetic and register transfer operation simultaneously. We experienced a great deal of difficulty hand coding even the simplest of the short DFT's on this processor and I decided to build a microcode compiler to manage the concurrency and give us a reasonable chance at optimizing the code. The compiler has since been used to generate code for all three levels of the MCSA.

The prototype MCSA consists of a single instance of the hardware components needed to compute the bandpass filters and the final DFT, as well as perform simple baseline modeling and thresholding of the high resolution bins. The MCSA currently consists of 24 wirewrap boards in a single 6-foot high equipment rack, and is controlled through a DMA channel by a VAX 11/750.

As an example of the response of the MCSA's bandpass filters to narrowband signals, here in Figure 4, are several filter's outputs as a frequency synthesizer is swept across their bands. The bandpass filter response is flat to within 0.1 db in band and drops to more than -60 db out of band.

The MCSA is now at JPL's Goldstone Tracking Facility in the Mojave. It is attached to the Venus Site's 26-m antenna and is undergoing field test. I was able to acquire the signal from Pioneer-10 using the MCSA at the Venus Site. Pioneer-10, as shown in Figure 5, is approximately 40 AU from earth, and it's signal is now too weak for the Venus antenna's acquisition electronics to lock on. The Pioneer-10 signal in the MCSA spectra seen in Figure 6, appears slightly brighter than the background noise, which is at -184 dbm.

The MCSA spectra in Figure 6, was photographed from the display on our SUN graphics 68000 workstation. The workstation is linked via an ehternet to the VAX, and is dedicated to displaying MCSA spectra. The display in Figure 6, is managed by an icon driven window based graphics program, developed at Stanford by Ernst Kimler, and consists of three windows viewing spectra from different MCSA resolutions. The top window is used for the 144 bands from bandpass filter 2. The middle window is for 1152 channels from one of the 144 bands, selected by the user. The bottom window displays a subset of the 1152 frequency channels, again selected by the user. This display uses a a pseudo grey scale to represent signal power, and maintains a scrolling time history for 100 frames in the vertical.

I feel we have the first stage of the MCSA comfortably in hand. However, we are just beginning the design phase of the second stage, or pattern processor. The outstanding issues are in algorithm and processor design. We have preliminary CW and pulse detectors in software. We have verified that these detectors will work, but they are non optimal in the sense that they are not minimally complex. We know that e.g. (a ln N) algorithms with minimal complexity are possible with agressive pruning of the decision trees, but are not sure that they have the desired sensitivity.

We know that we can choose an architecture matched to these preliminary algorithms. An example of a processor to run the CW and pulse detectors is here in Figure 7. We need a comparable degree of concurrency as we have in our DFT processor if we are to achieve processing balance in the MCSA. By balance, I mean the processor's performance should be proportional to the complexity of the processes it is running.

As Figure 7 shows, we would have of order 8 of the new commercially available digital signal processors (DSP's) running their own copy of the CW or pulse detector. In this way we could achieve a performance of 20 MIPS on a single board, which is identical to our present DFT processor.

The interesting difficulty that the CW and pulse detector algorithms introduce is that they are non-deterministic. You see, the DFT's done in the MCSA are fully deterministic, and can be performed using a single microcode controller, and as many copies of the DFT processor as it takes to process the bandwidth. Further, the DFT processor is like most highly concurrent processors and is pipelined. It has 12 pipeline stages.

Because the DFT algorithm is fully deterministic, the pipeline is never broken, i.e. the operations in the next machine cycle can always be correctly linked to the previous machine cycles. In a non-deterministic process, there are branches, and the pipe is broken whenever the processor fails to correctly predict the outcome of a branch. You then have to flush the pipe, and effectively loose as many machine cycles as there are stages in the pipe. This can result in a severe loss in performance in the presence of frequent branches. This is exactly the case for the CW and pulse detectors. That is why we need 8 DSP's to reach the same level of performance as our single DFT processor.

I think you can see that, while we may not have found solutions for the algorithms and architectures that are close to optimal, we can reasonably expect that we will achieve balance in the performance of the MCSA and it's pattern detector. Thus I can use the 20 MIPS figure to estimate costs, and in particular, the need for 1000 of these processors is real.

The thought of replicating the processors 1000 times is most unattractive when each processor is one or more boards. Even if we convert the wirewrap design into printed circuit boards, the mechanical complexity of a system that size is formidable. It would be most unreasonable with one processor per board to attempt to increase the bandwidth beyond the expected 10 MHz to say, 100 MHz through replication.

Driven then by these concerns, we have begun the process of designing the DFT processor architecture in VLSI-CMOS. Stanford has recently dedicated its new Center for Integrated Systems, wherein it intends to provide the capability for the development of integrated circuit chips fully within the University. In addition, our Stanford SETI group has acquired the VLSI-CMOS design tools and ported them to one of our SUN-100 graphics workstations. We have organized several of our graduate students into a design team, and they have begun to lay out the components of the DFT Processor in CMOS.

I brought with me a couple examples of their CMOS layout. Figures 8 and 9, are components of the adder and multiplier for the SETI chip. From these layouts we estimate that we will be able to place all the arithmetic and logic components on the DFT Processor board into a single chip approximately 4mm x 5mm in size. This is comfortably within the 7mm x 7mm die boundaries. The yield won't be wonderful, but it won't be zero either. For the quantities SETI is interested in, the yield should be adequate.

Only the data memories will be left off the chip. Memory uses proportionately much more space than arithmetic logic, and is typically implemented with even different IC technology. The memory should probably be separate then. Using this chip and 4 static memories for the data, we will have reduced the DFT Processor from one board of 110 chips to just 5 chips. The five chips will be inside larger than average packages, due to big pin counts.

We may likely fit 8 of these chips sets on a single board. That should be easy within the present board geometry, and I could double that to 16 sets by deepening the board slightly. This suggests that the board count of the MCSA can be reduced by a factor of at least eight if our CMOS conversion is successful. Remember though, that the scaling of the pattern detector processors may be different that the DFP Processor. We are in the middle of the pattern processor design cycle, and it is too early to tell if we will be able to maintain the balance between the two processors all the way into silicon.

If we are successful, I think you begin to see that an MCSA could be built with a 10 Trillion matched filter capability (10 Million frequency channels), and the physical scale of the machine would be very close to what it is today at Goldstone in prototype form.

It would then be very interesting to speculate on the prospects for the MCSA as the device densities in CMOS continue to increase. Our designs today are predicated on 2-micron CMOS technology. Remember that the VHISIC program of DOD believes today that they have 1-micron CMOS available. If that becomes available to us, it would quadruple our densities. Possibly the board count could be reduced by a similar scale. Then you could even have your own MCSA, or alternatively retain the current physical scale in a 100 MHz machine.

I may not have answered the question as to whether 100 million channels are enough, but I hope I've started you thinking that here is a way to build an instrument capable of conducting a first class tour of search space.

MCSA - Prototype

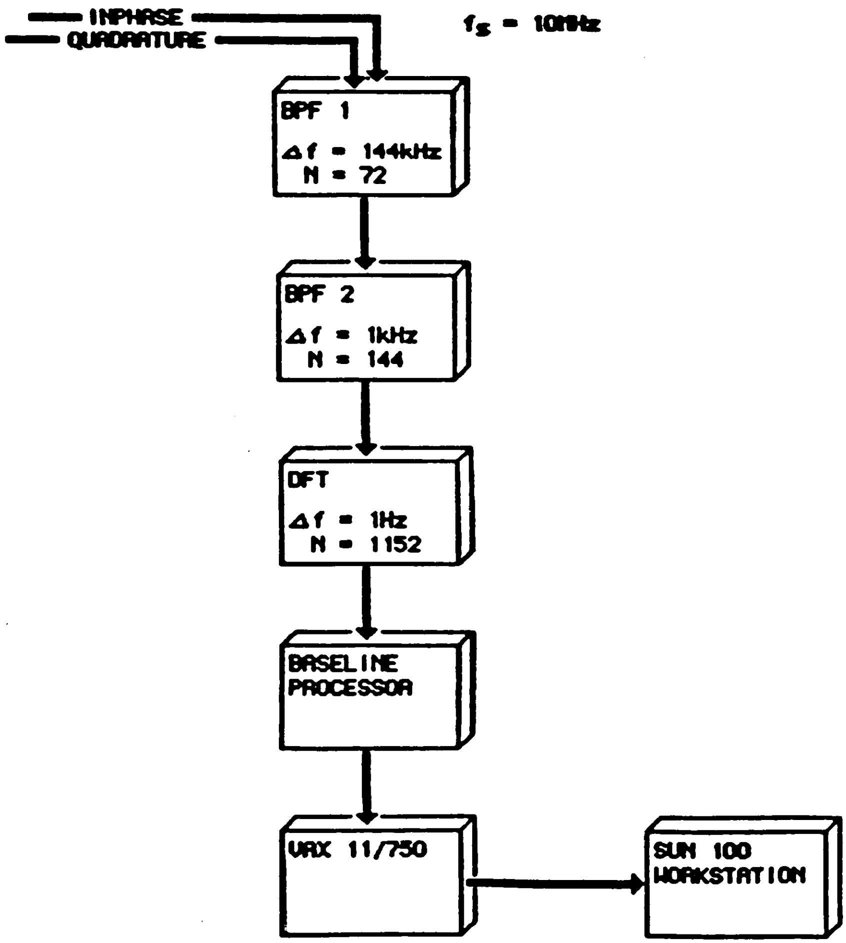

Figure 1. MCSA, Functional Components. This is the prototype now in fiel test at Goldstone Tracking Station's Venus Site. Principal components are the two bandpass filters and 1152-pt DFT. Spectra are sent to a VAX 11/750 for display on the SUN-100 graphics workstation.

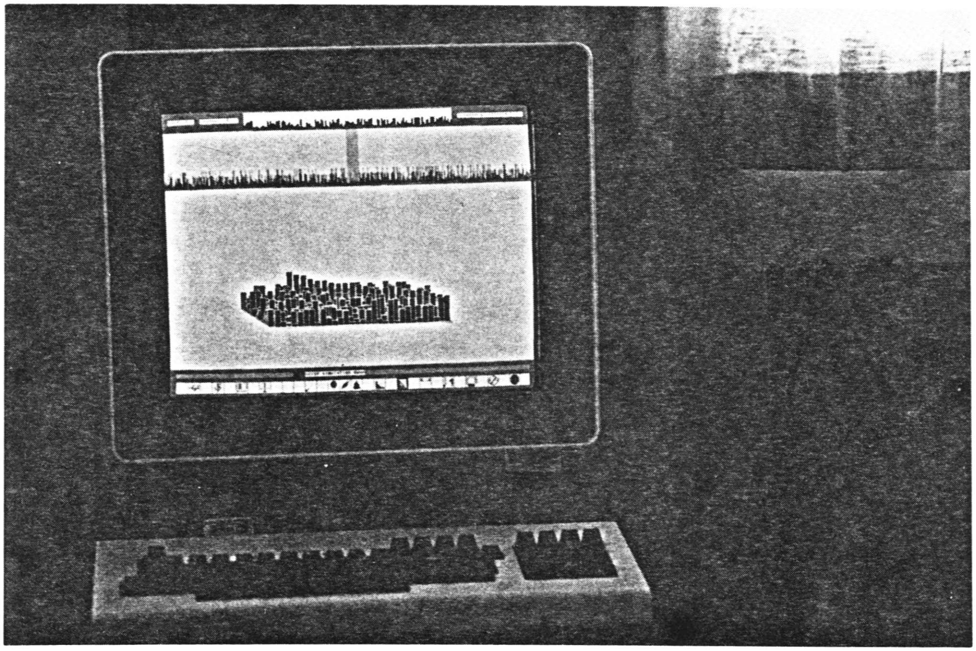

Figure 1b. SUN-100 Graphics Workstation. Spectra from the MCSA are displayed in multiple window views by an icon driven program running in a 68000 resident inside.

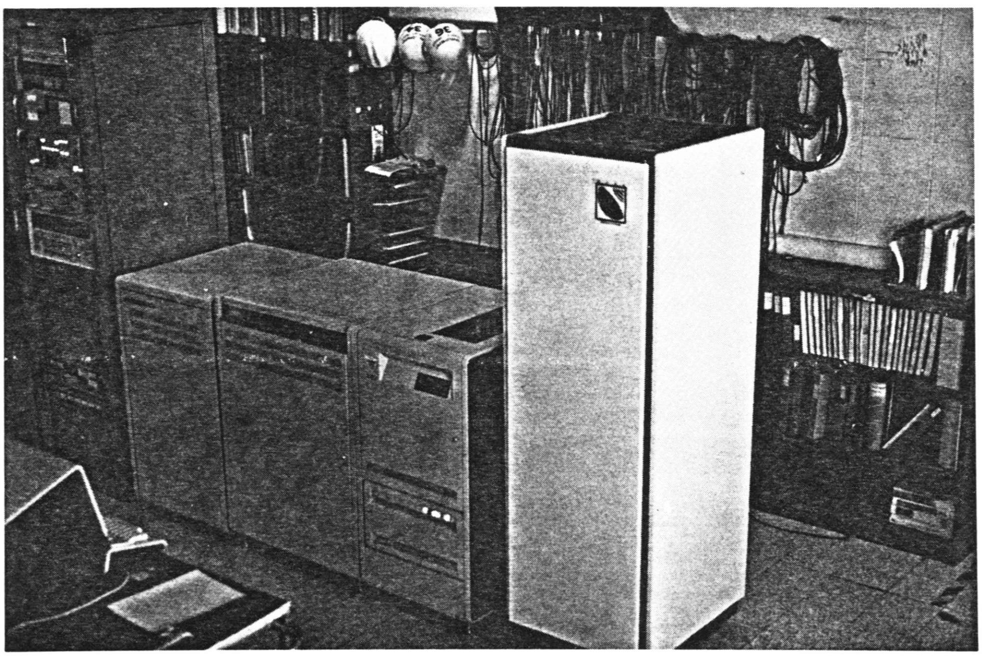

Figure 1c. MCSA at Goldstone's Venus Site. MCSA cabinet is on right, VAX 11/750, tape drive, and power conditioner to the left.

Finite Impulse Response Filters

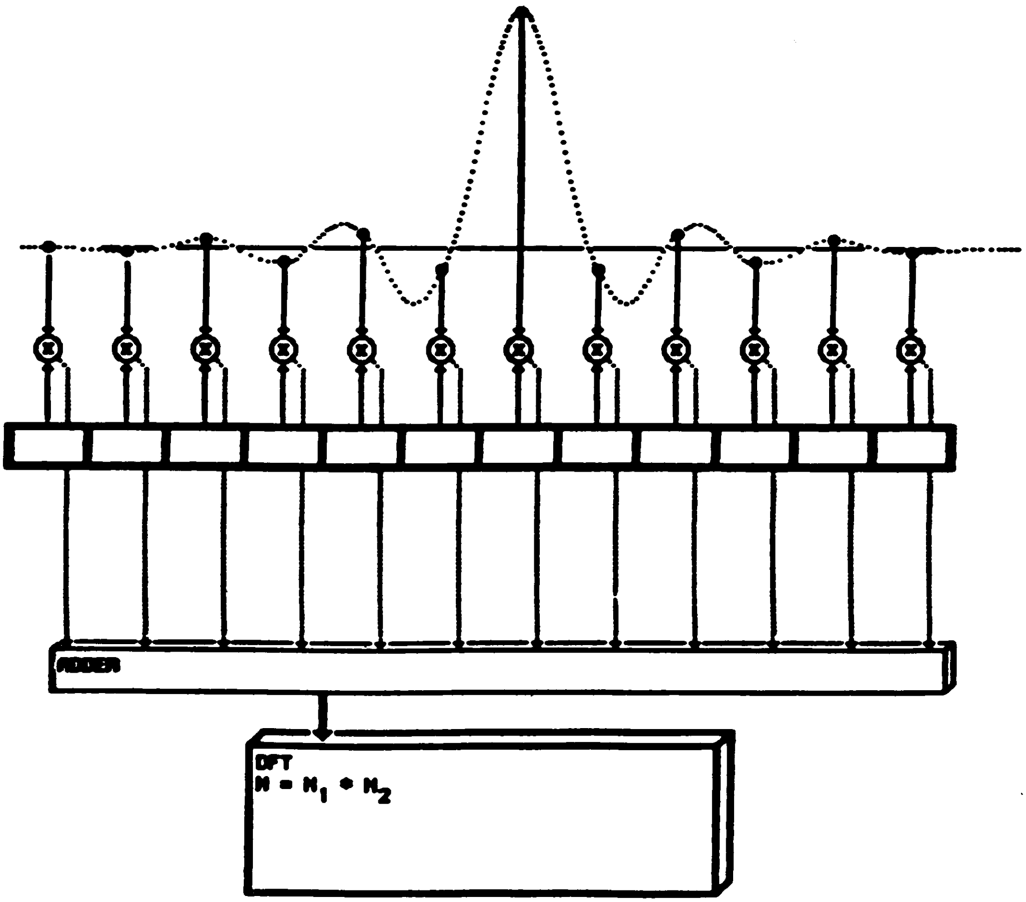

Figure 2. Finite Impulse Response signal processing for bandpass filter banks in the MCSA. Data in center shift register is multiplied by filter impulse response, at top, and accumulated in adder, above DFT box. Accumulated samples are sent in N-sample frames to a Fourier transform processor, shown at the bottom.

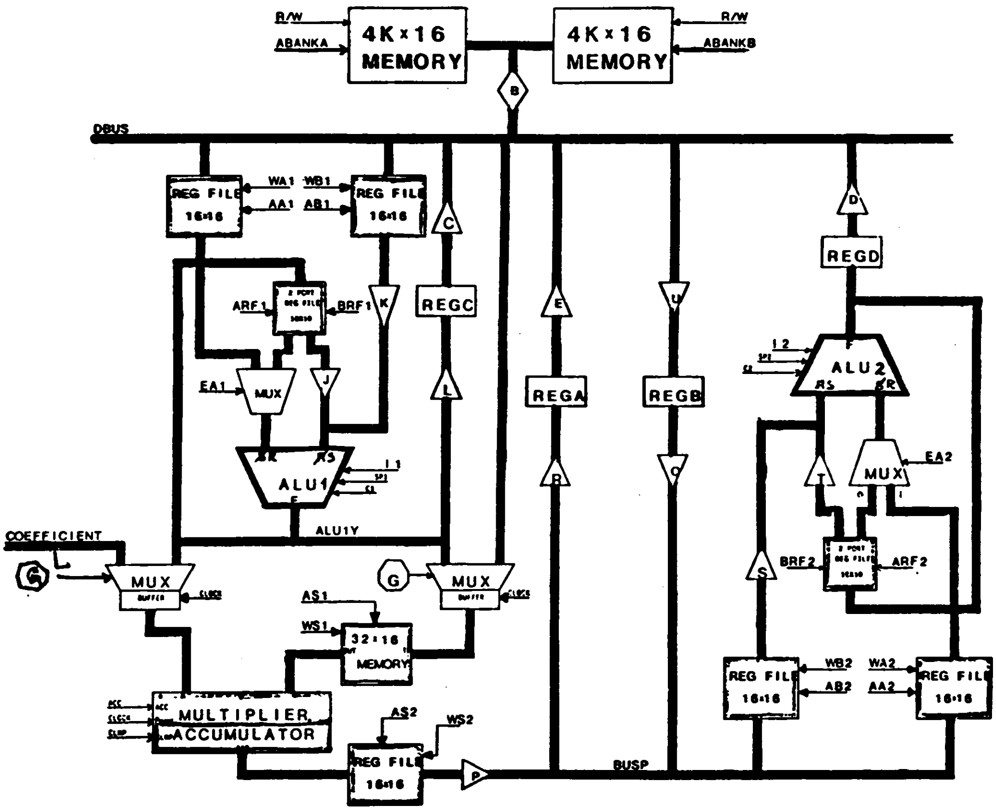

Figure 3. DFT Signal Processor Architecture. Through microcode control, data is moved from the double buffered RAM's, at the top into register files ahead of ALU1 for pre-adds. These partial sums are moved into the MAC (Multiplier-Accumulator), and sent on to ALU2 for post-addition. The data has then completed its path through the processor and is returned to the RAM. This processor computes a short DFT in a single pass. By sequencing microcode from a separate control store, transforms of up to 16-points may be computed where the data need pass through the processor elements only once.

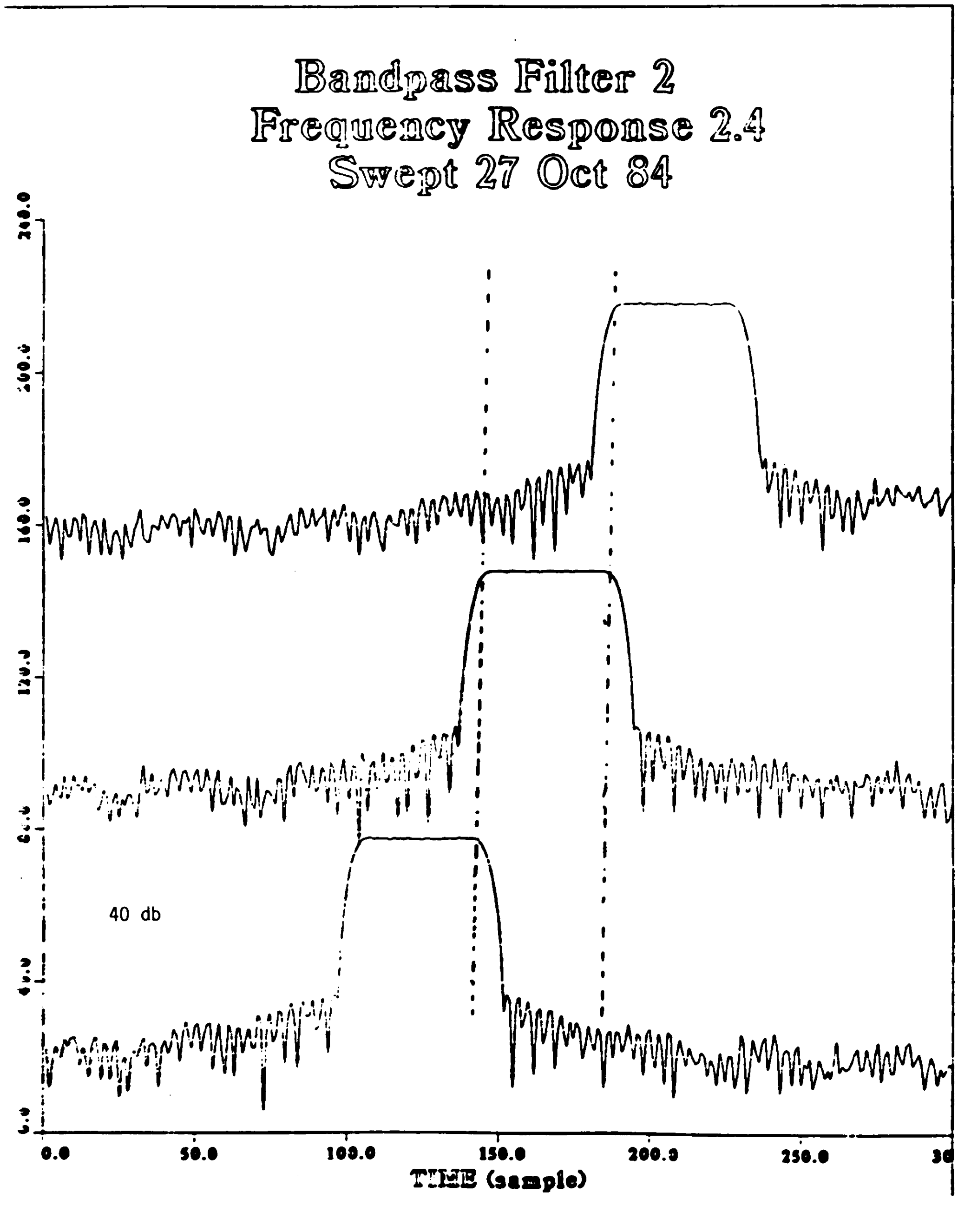

Figure 4. Response of Bandpass Filter 2, to a test signal swept across the input band of the MCSA. The test signal was swept slowly so as to appear monochromatic to the bandpass filter.

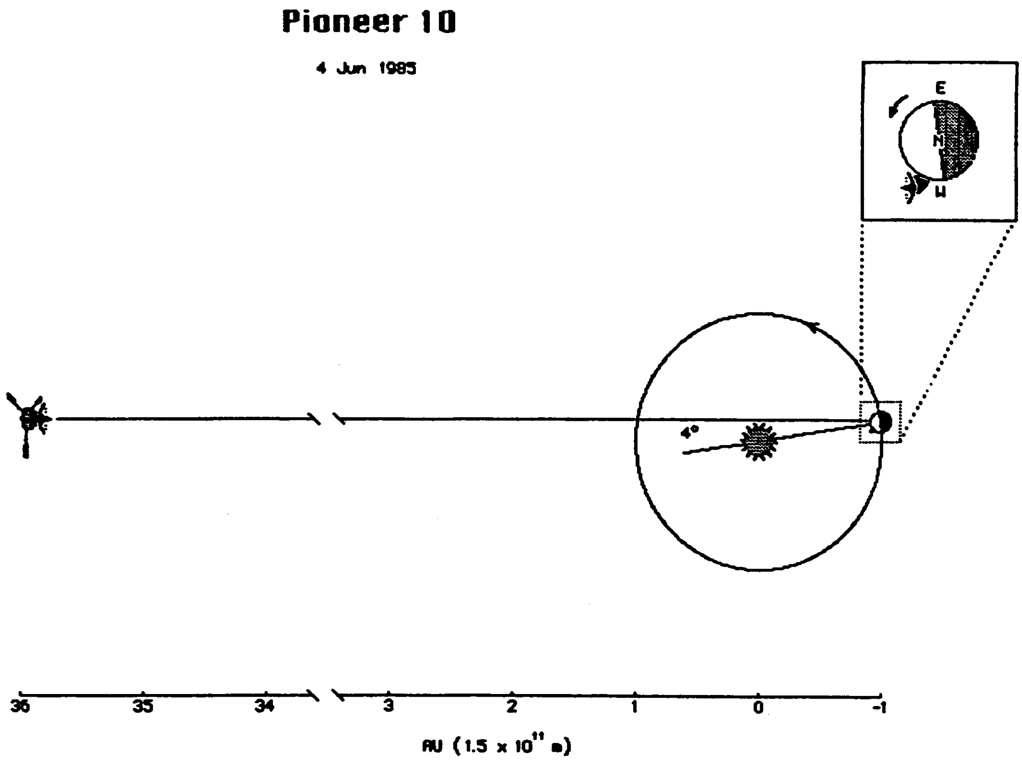

Figure 5. Relationship of Earth, Sun and Pioneer-10 during 4 June 1985 Observations. Signals from the spacecraft were recorded by the MCSA at the Goldstone tracking station's Venus Site.

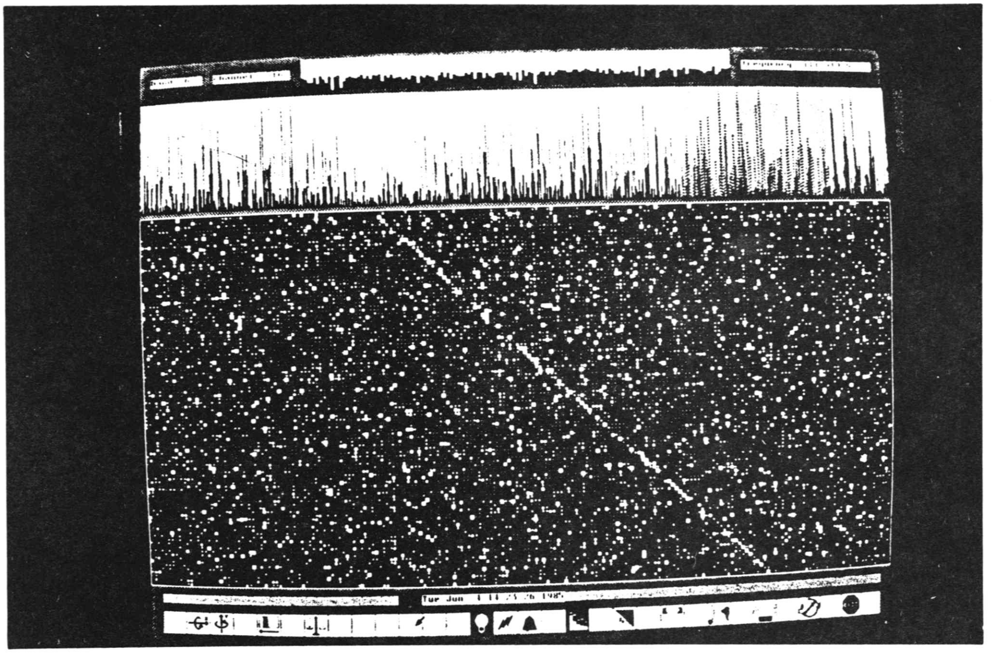

Figure 6a. Pioneer-10 signal on the SUN graphics workstation at 14:23 PDT. The large, rectangular window at the bottom is displaying 200 frequency bins across, and 100 time frames vertically, where the most recent is inserted at the bottom.

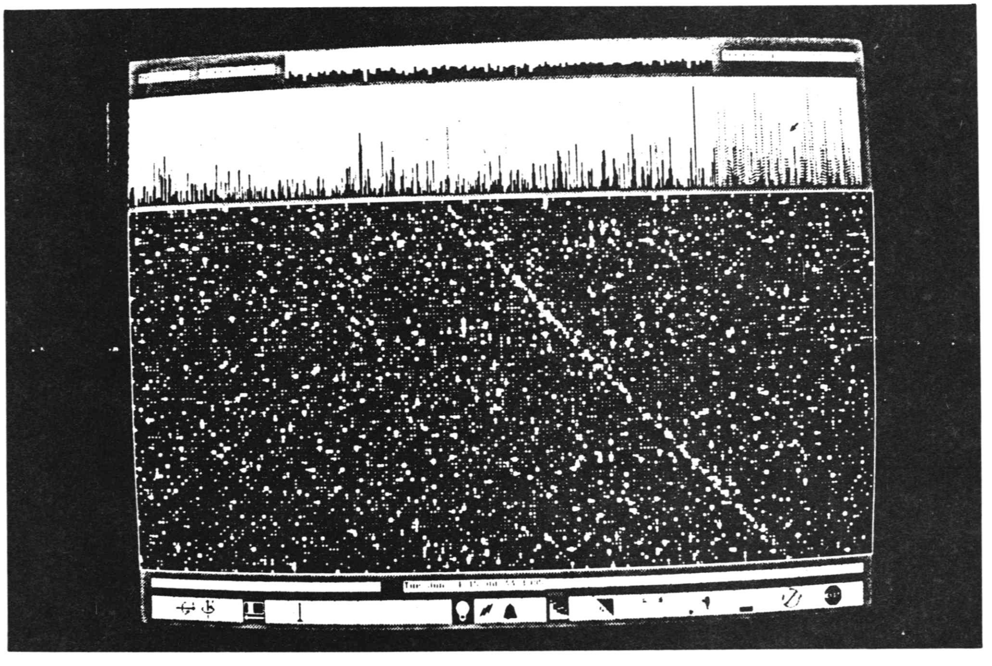

Figure 6b. Pioneer-10 signal on SUN graphics workstation at 15:00 PDT. Note slight change in slope due to earth's rotation.

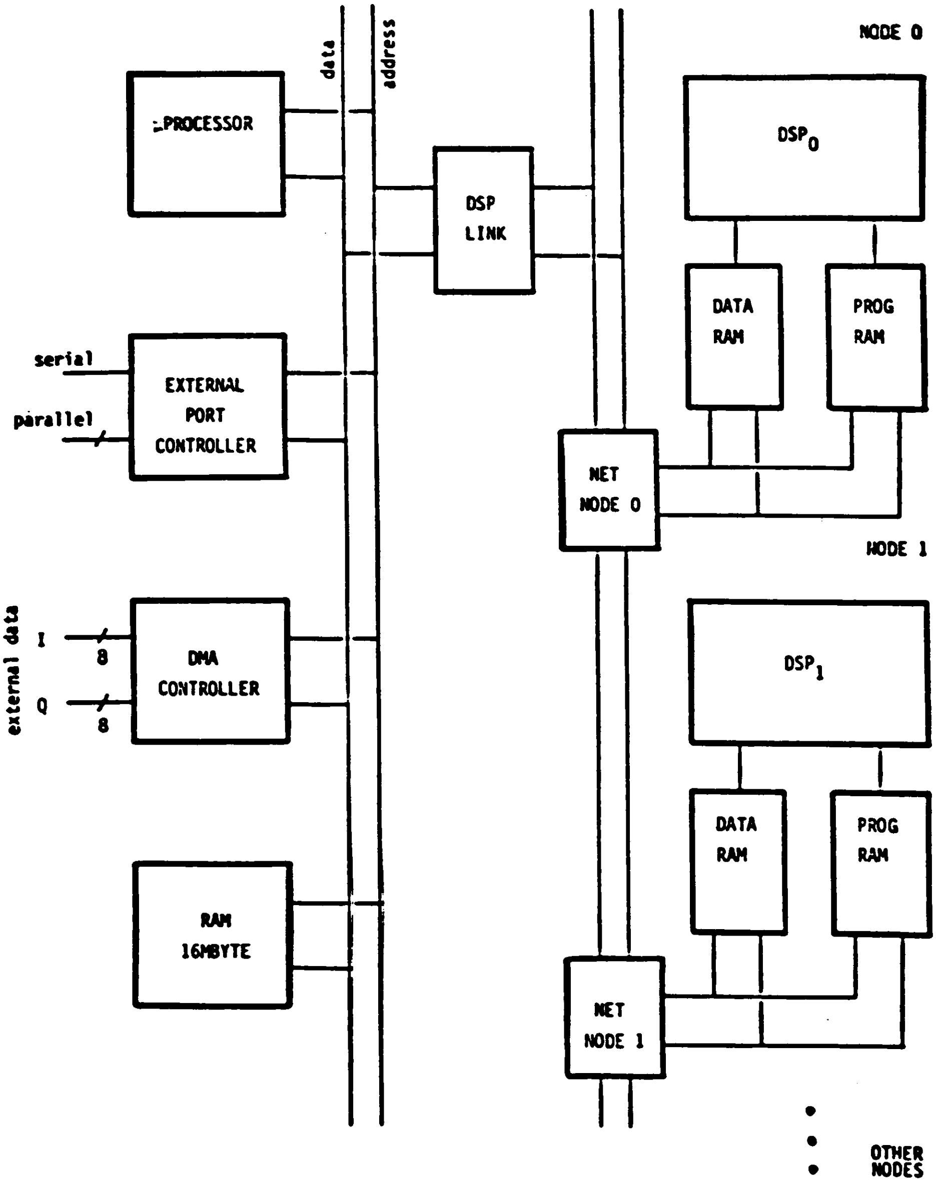

Figure 7. Pattern Detector Architecture. Digital Signal Processors (DSP's), are linked together and operate concurrently. Each DSP node operates primarily on its own data, e.g. an MCSA band, and is controlled by its own microsequencer.

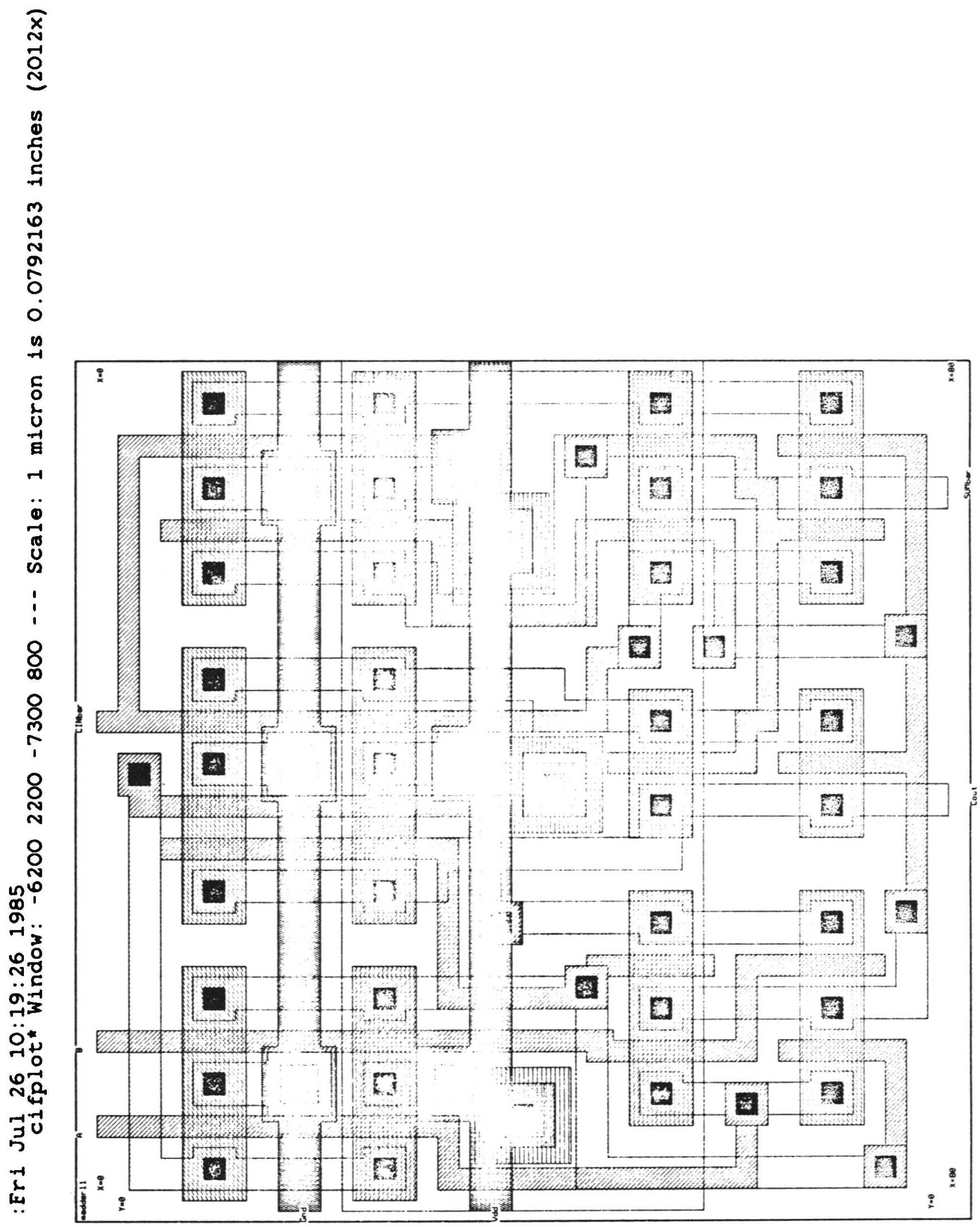

Figure 8. Integrated Circuit Layout for CMOS-chip version of the MCSA DFT-processor. Layout is for one bit of a Full Adder which will perform the addition function now contained in the DFT-processor's ALU.

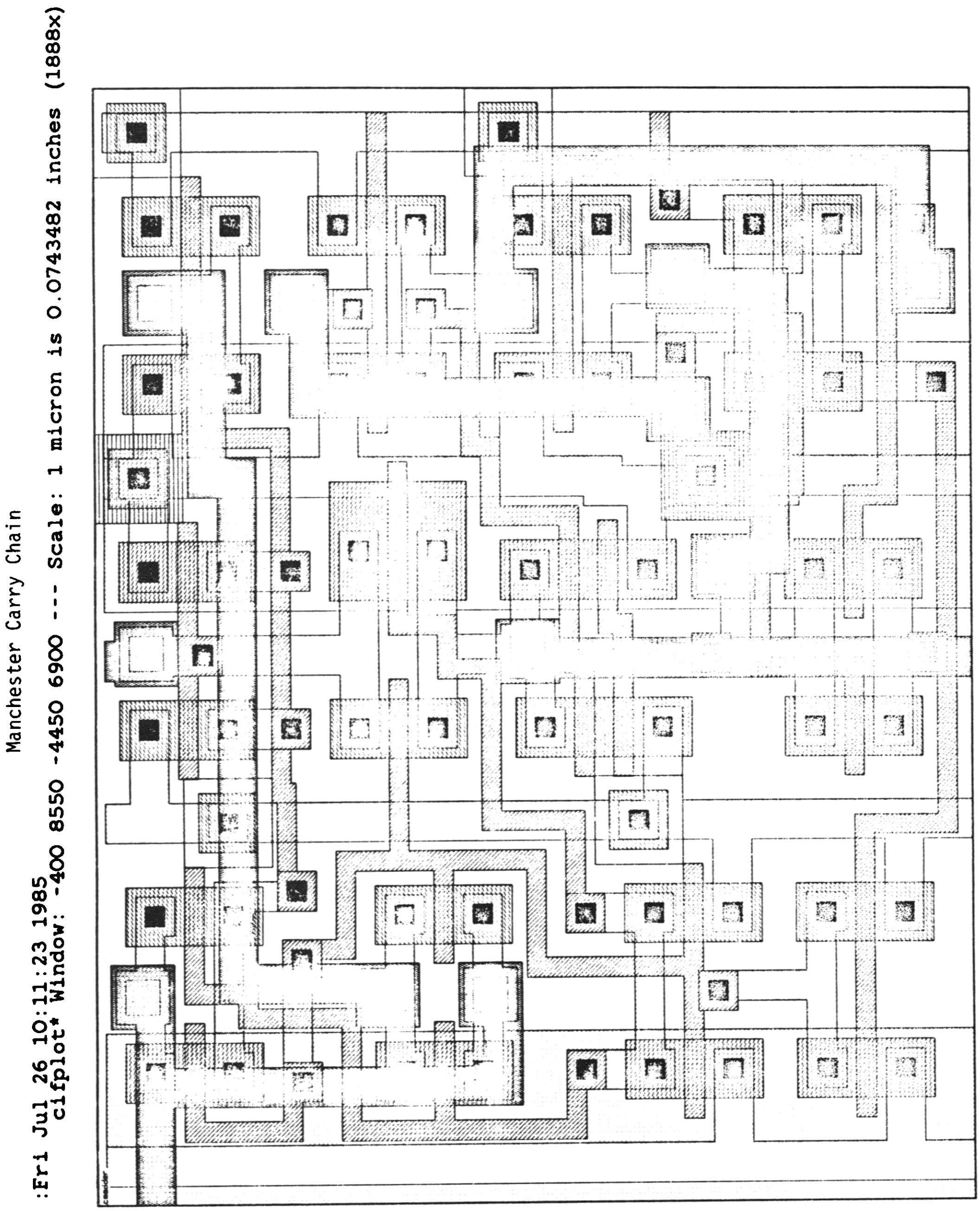

Figure 9. Integrated Circuit Layout for CMOS-chip version of the MCSA's DFT-processor. Layout is for the Carry function needed in multiple bit addition.

THE NASA SETI PROGRAM AT JPL

Sam Gulkis and Edward T. Olsen
Jet Propulsion Laboratory/Caltech

The major difficulty in designing the correct search program for SETI is that we need the results of the search before the search is started. By this I mean that we would like to design the search based upon the characteristics of the ETI signals that are present. We need to know the frequencies, polarizations, and modulations of the signals as well as the power and spatial distributions of the transmitters in order to calculate the probability of success of any search program. These are all parameters of which we have no knowledge at the present time. Phil Morrison has mentioned the analogy between a search strategy and a fisherman's net. A net is designed to catch certain kinds of fish but the inexperienced fisherman does not know in advance what kinds of fish lie beneath the surface of the ocean. The best we can do at present is to design the SETI net so that it has the capability of trapping the kinds of signals we believe may be transmitted. We cannot estimate the probability of success without having additional information in hand. Because no bounded program can be sensitive to all possible signals, many types of ETI signals which might be present will escape the SETI nets cast by any one program.

My presentation today will focus on the NASA All Sky Survey strategy for detecting signals of extraterrestrial origin. However, I would like to start the presentation with a brief discussion of the overall NASA SETI search strategy. Oliver has mentioned that the search strategy is comprised of two complementary approaches, a Target Search and an All Sky Survey. The parameter space covered in these searches is multidimensional. Three principal parameters are:

1) the solid angle or number of sources observed,
2) the sensitivity achieved in each direction, and
3) the frequency range covered.

Figure 1 shows the volumes in this three dimensional parameter space covered by the Target Search and All Sky Survey strategies. In addition, each search strategy will be designed to detect certain kinds of modulation and polarization. The capability of detecting pulses or polarized signals is described by these latter parameters. If we compare the Target Search and the All Sky Survey on the basis of the three principal parameters and disregard polarization and modulation, we see that the major emphasis of the Target Search is to achieve a very high sensitivity - something in the order of 10^{-26} Watts/meter^2, or perhaps 10^{-27} Watts/meter^2 if Arecibo is used for a very long integration time. Thus the Target Search net has a very fine grid in order to detect weak signals.

The All Sky Survey expands coverage in two dimensions: spatial direction and frequency. The entire celestial sphere is observed, compared to the nearly 800 beam areas of the Target Search. It also covers a frequency range of 1 to 10 GHz, compared to the Target Search range of 1 to 3 GHz. The choice

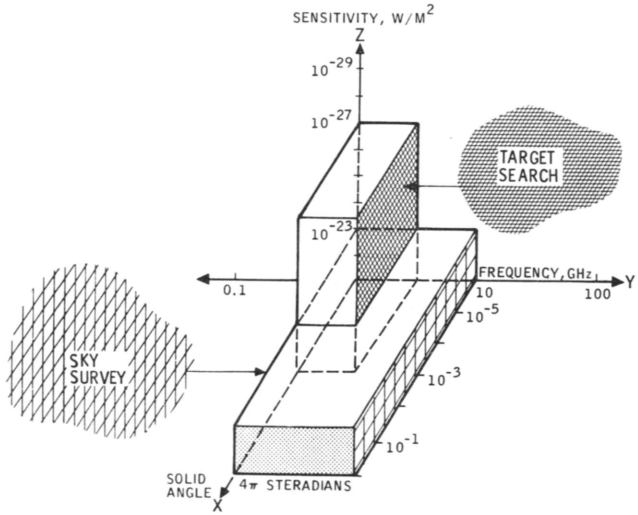

Figure 1. SETI Search Space

of these limits is affected not only by the variation in background noise temperature but also by a requirement that the search can be completed within a given time span with presently available instrumentation. The broad coverage of the All Sky Survey comes at the expense of sensitivity. Thus the All Sky Survey net is very large, but it has a coarse mesh. We can only catch very big fish with it.

Oliver has already discussed the background brightness temperature as seen from free space. Figure 2 shows the background brightness temperature as seen from the ground. This figure includes the additional contribution of the terrestrial atmosphere, which reduces the frequency range of the microwave window. The All Sky Survey will cover the total frequency range over which the background brightness temperature is a minimum, i.e., 1 GHz to 10 GHz. In this frequency range, the background brightness temperature is approximately 5 K. We expect to be able to achieve system temperatures in the range 15 K to 25 K using low-noise amplifiers.

I mentioned earlier that the selection of the frequency range for the All Sky Survey was dependent on the time allocated to the survey as well as on the background temperature. The survey time depends on the area of sky surveyed, the telescope diameter(beam size), scan rate, the angular separation of the scans, the bandwidth of the receiver, and the frequency coverage. In addition, the efficiency of utilization of the telescope time also is a factor. The time to carry out a survey of a fraction, G, of the sky at a constant angular rate, ω, over the frequency range delimited by ν_1 and ν_2 is:

$$T = \frac{70G\pi^2\eta D\nu}{2c\varepsilon\omega Nb} \; \frac{(1+\zeta)}{\kappa} \; (\nu_2^2 - \nu_1^2)$$

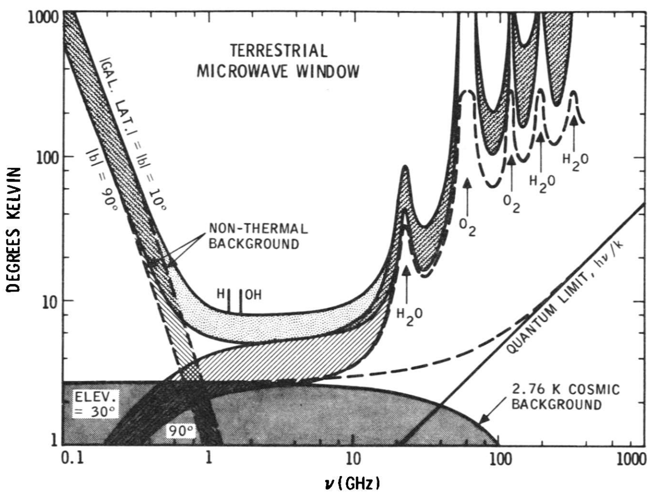

Figure 2. The Terrestrial Microwave Windows

where η is the aperture efficiency, ε is the beam efficiency, D is the diameter of the circular equivalent aperture, N is the number of channels in the spectrum analyzer and b is their width (thus Nb is the instantaneous bandpass of the system). The spacing between the neighboring scans in HPBW is κ, and the ratio of the turnaround time to the average time to complete a scan is ζ. The time required to carry out the All Sky Survey is therefore strongly dependent upon the size of the antenna and the rate at which the beam is scanned, as well as upon the range of frequencies covered and the instantaneous bandpass of the system. It is also dependent upon the scan strategy and mechanical considerations for the antenna, which are implicit in the scan spacing and turnaround time. For the moment, let us take $G = 1$, $\omega = 0.2$ deg/sec, $\kappa = 1$ HPBW, and ignore the turnaround time. Utilizing a 34m antenna full time at one frequency setting, the time (in days) required to carry out a survey of the entire sky is about 3 times the frequency (in GHz). In the case of a 64m this factor becomes 5.6. The time required to complete the survey between 1 GHz and 10 GHz with 300 MHz bandwidth under these assumptions with a 34m is about 500 days. The sensitivity achieved at 32 Hz resolution, assuming accumulation time is equal to the time required for the beam to sweep through 1 HPBW is about $4 \times 10^{-24}\, a\sqrt{\nu}$ Watts/meter^2. Here α = SNR corresponding to the false alarm rate which is tolerable. Thus the sensitivity at which the All Sky Survey will be complete, is something on the order of 10^{-23} Watts/meter^2.

The distance at which a transmitter can be detected for a given sensitivity is shown in Figure 3. The All Sky Survey line corresponds to a limiting flux of $4.3 \times 10^{-24}\, a\sqrt{\nu}$ Watts/meter^2. This line is typical of the flux achievable in an All Sky Survey using a 34-meter telescope. As an example, consider the distance at which the Arecibo transmitter could be seen. The Arecibo telescope has a gain of approximately 10^7 and it has a megawatt transmitter on it; thus the effective radiated power of Arecibo directed into the main beam is about 10^{13} Watts. With this effective power, Arecibo can

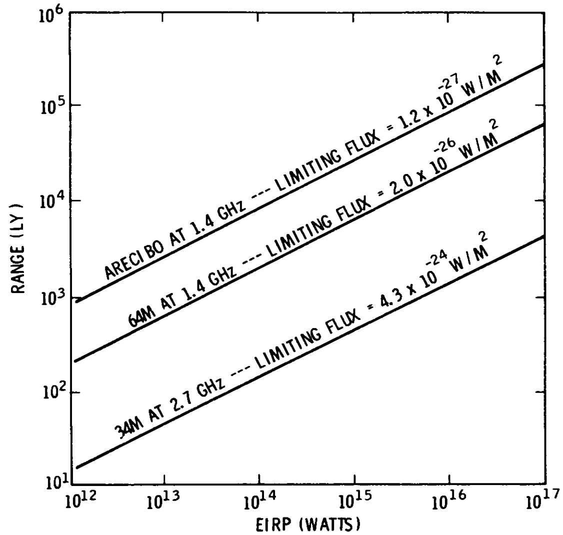

Figure 3. Range of Detectability as a Function of EIRP

be detected at distances as great as 50 light years, depending on the transmitted power and signal to noise ratio. Thus the All Sky Survey can detect an Arecibo-like transmitter, directed at the solar system, from nearby stars. If you want to think about the EIRP required of a transmitter to be detectable at a thousand light years, it is in the range 10^{15} to 10^{16} Watts. These are certainly large powers; however, without a priori information about the power distribution of ETI transmitters, they cannot be ruled out. Large solar collectors in orbit around the earth have been proposed to collect this amount of power and transmit it back to earth. It is easy to imagine that the same power could be transmitted into space as a beacon.

Although it lacks the high sensitivity of the Target Search, the All Sky Survey has other features that make it interesting. First, it is a survey of the entire celestial sphere; the survey completely covers one dimension of the parameter space. All potential life site directions are examined using this strategy. It answers the question of whether strong CW radio signals exist once and for all. Of course we may look in a given direction at the moment that the transmitter there is turned off and thus wouldn't see it. But if there is a transmitter continuously on and in the frequency range that we are searching, we will pick it up.

Second, the All Sky Survey uses relatively small radio telescopes, which brings to mind several interesting points which I should mention here. One point is that more telescope time is available for SETI on these systems since these telescopes are less heavily subscribed than the larger ones. It takes a long time to carry out an All Sky Survey over 9 GHz because at present we are only able to consider doing so with a 300 MHz instantaneous bandwidth. We must map the sky at one frequency setting, step 300 MHz, and map the sky again - repeating the process approximately 30 times. Another point is that the optimum way to look for a strong CW signal is to maximize the search volume. Basically, the argument here is that if there is a transmitter somewhere, and you are operating a survey which has the sensitivity to see it, the way that you'll find it is to look everywhere rather than focusing on one particular direction at higher sensitivity than required to detect it. A final point is that the All Sky Survey is more sensitive than any of the radio astronomy surveys that have been carried out to date at a narrow bandwidth and far more complete in frequency coverage. There is new information coming in here which has not already been observed.

Third, the All Sky Survey requires its own unique signal processing. Oliver may have indicated that the only signal processing in the NASA SETI program was required in the Target Survey. However, the All Sky Survey has complex signal processing problems associated with it as well, and we are working on these at JPL now. I will get into that shortly. Briefly, we have to worry about what the scan pattern strategy is going to be and how to minimize the spatial inhomogeneity in the sensitivity of the survey - something that we call scalloping. If we do not take care in our signal processing as we scan the beam across the sky, there will be variations in sensitivity along the scan tracks as well as loss of sensitivity between the tracks. We must worry about the accumulation strategy, the detection algorithms, and the baselining of the spectrometer output. This translates into requirements for high-speed and low-speed memory and for real-time processing. This will all become more evident as I proceed. Seeger has mentioned RFI in his discussion and I'll also cover that soon. We also want to maximize the radio astronomy benefits that might result from the All Sky Survey by making sure that they are not designed out. Mike Klein is going to talk about that later. Finally we must consider what part of the data should be archived, and how to do the archiving.

We have already started some field tests at the Goldstone (Venus Station) Tracking Station in Southern California. The block diagram of the system that we are putting together is shown in Figure 4. The radio telescope indicated on the figure is the 26m antenna at the DSN R&D Venus Station. The station is equipped with S-band and X-band receivers and calibration sources. The IF signals can be easily directed to a spectrum analyzer. At the present time, we are working with a JPL-built, pipeline Fast Fourier Transformer which performs two independent 32k-channel FFT transforms. I'll show you what that looks like later. We have no hardware for signal processing, but we are currently putting together a software package in the old Modcomp computer, which controls the pipeline FFT. There is a microprocessor-based monitor and control system for the station, and there is an independent RFI spectrum analyzer to monitor the RFI. All this output will go into the SETI computer.

In reality, we don't have that system connected up exactly as I show in the figure. Our computer is back at JPL, and we have to go back and forth between the field test system and the laboratory computer with floppy discs and magnetic tape. Thus we have to think through what we want to do at home and build precanned station control files on floppy disks. We carry these out to the field and bring monitor data back on other floppy disks along with spectral data on magnetic tape. Back at the lab we examine and merge the two data streams. The process is inconvenient and slow, and our goal is to get to the real-time system shown in the figure as resources permit. The pipeline

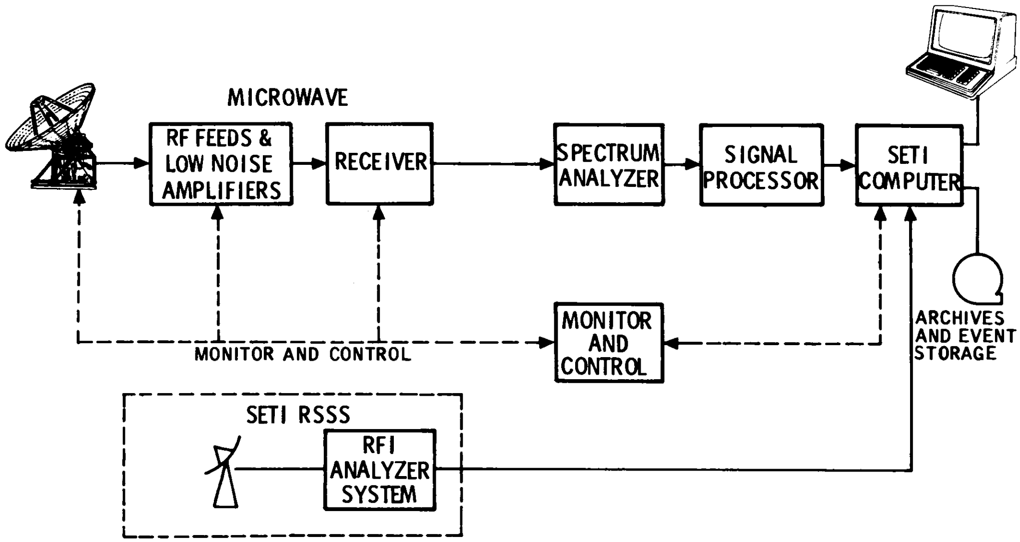

Figure 4. SETI Sky Survey Observational System

FFT spectrum analyzer, its Modcomp computer, and the RFI analyzer system are inside a van beside the DSS 13 station control house. The monitor and control microprocessor is within the station control house. We have modified the spectrum analyzer so that it can be clocked down from its designed speed of 20 MHz to achieve resolutions finer than 305 Hz. I will show you a few spectra that have been obtained with different resolutions. Figure 5 is a small part of the spectrum showing the Pioneer 11 spacecraft signal at a resolution of 305 Hz. The horizontal scale is the sequence number of the frequency resolution elements, and the vertical scale is the system temperature in 10^{-2} deg K. The spectrum analyzer itself does two 32k-channel fast fourier transforms in 3 milliseconds and has a total bandwidth of 20 MHz. Here it is running at fastest speed, a resolution of 305 Hz. Figure 6 shows the output when you clock down to 32 Hz and look at Pioneer 12. To show all the sidebands, we have grouped the data and plotted the strongest frequency resolution element in every 8. The strong monochromatic signal below the third harmonic to the left of the carrier is a test signal which was injected into the IF. Figure 7 shows one of the sidebands of Pioneer 10 at 1 Hz resolution.

Figure 8 shows an unidentified transient RFI source which appeared at X-band while the pipeline FFT system was being used in an astrophysical program.

R. Dixon: Is that a genuine unknown signal or is it artificial?

No, that is a genuine unknown signal. There are many unknown signals which pop up even in the DSN-protected bands. Some of the signals you can

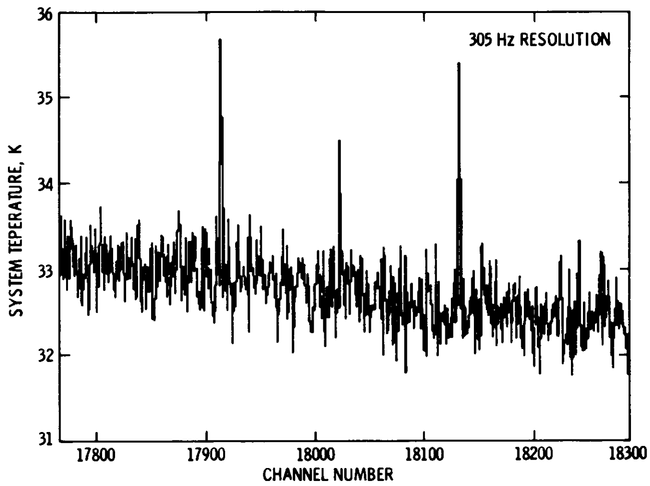

Figure 5. Pioneer 11 Spacecraft Signal at 305 Hz Resolution

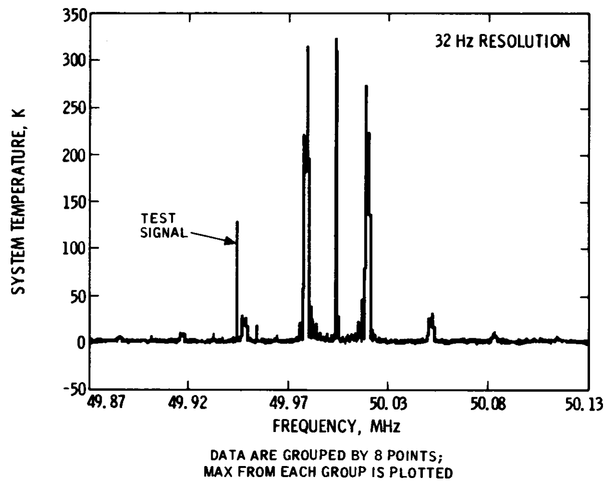

Figure 6. Pioneer 12 Spacecraft Signal at 32 Hz Resolution

legitimately guess what they might be; for example, the downlink of a spacecraft. However, there are many truly unknowns and we hope to get a handle on those outside of the protected bands as well, with our RFI analyzer system.

Figure 9 shows the W49 line features around the OH line at 1667 MHz at 305 Hz resolution in the top diagram (and at 2 KHz resolution from a published paper in the lower diagram). The horizontal scale covers 10 MHz. The entire pipeline FFT spectrum was taken in a few seconds, while the lower spectrum was

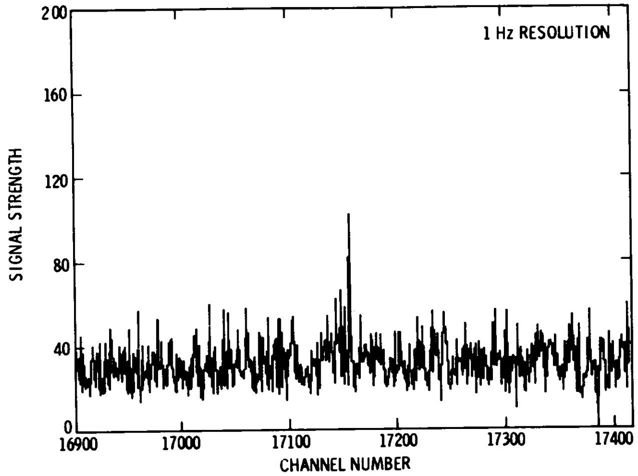

Figure 7. Pioneer 10 Spacecraft Signal at 1 Hz Resolution

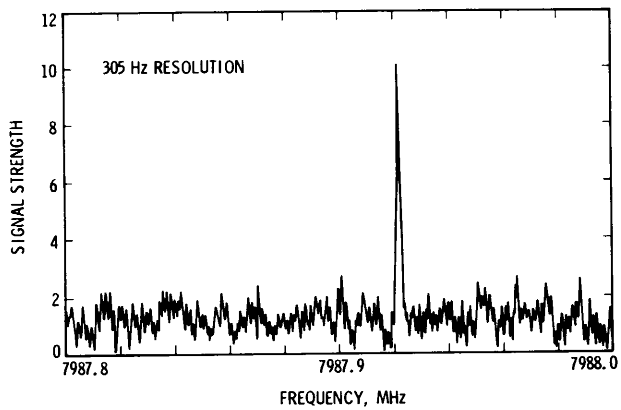

Figure 8. Unidentified Transient RFI Source

painfully acquired over many hours of time by constantly resetting the local oscillator. I just tossed this out to show that there will probably be some radio astronomy fallout from SETI. You can see that, by improving the resolution, there still is structure here. We took this down to about 100 Hz, and you can see the structure start disappearing at that fine a resolution.

I want to turn now to the sky survey signal processing. What we wanted to study was the mutual interaction between our scan strategy and various accumulation strategies and its effect upon the sensitivity of the survey. Our approach was to look at four different signal processing scenarios for carrying out the All Sky Survey and try to evaluate these on the basis of data

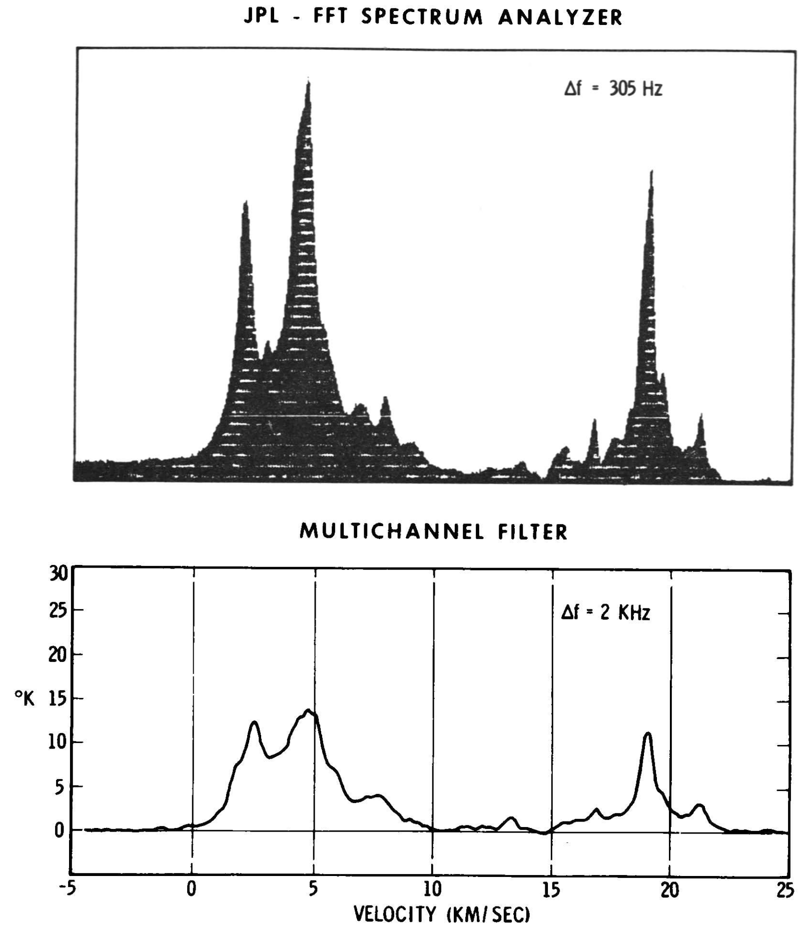

Figure 9. The W49 OH Line at 1667 MHz. (Upper: 305 Hz resolution, 10 MHz instantaneous bandpass. Lower: 2 kHz resolution, 0.2 MHz instantaneous bandpass.)

rate, cost, and the probability of detecting a signal as a function of its SNR. In terms of how we carry out the sky survey, one of the things that we looked at was "pixelizing" the sky in a manner such as that shown schematically in Figure 10. The pixelization scheme itself is complicated by considerations of antenna scheduling, mechanical limits of the antenna, and minimization of system temperature. Now consider the detailed process of scanning one of these pixels in the sky; Figure 11 is a blowup of one of those pixels. So, if you consider the details of carrying out a survey like this, again you have to worry about how your sensitivity varies along the scan line, how it varies between the scan lines, how you turn around at the end points, what kind of mechanical modes you might introduce into the antenna, what the spacing between the scan lines are, and all those details.

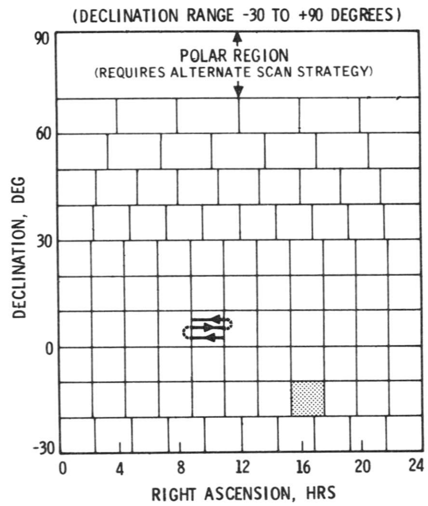

Figure 10. All Sky Survey Sky Pixel Scheme and Scan Strategy

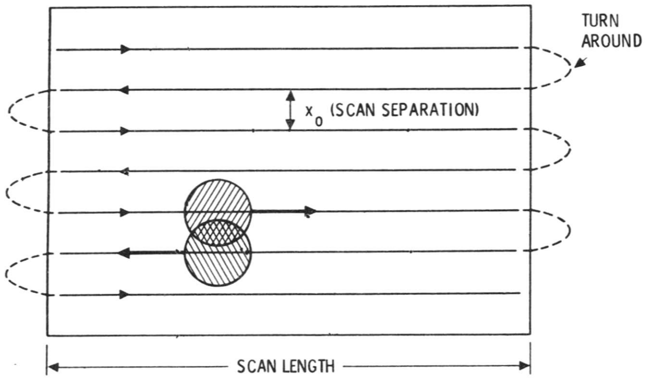

Figure 11. All Sky Survey Pixel Scan Details

<u>*R. Dixon:*</u> *Excuse me, Sam. Why does the sensitivity vary along the scan line?*

Because the beam response is non-uniform, the sensitivity will depend on how the data are accumulated and analyzed. For example, you could integrate during the entire time the beam swept through one HPBW, repeating the process each HPBW. This process will give you an uneven sensitivity along the scan line. The sensitivity will be best at the points of the sky which transited the beam at the midpoints of the integrations. The sensitivity will be worst at the points of the sky which transited the beam just when one integration ended and the next began. In the former case, most of the signal from that point appears in one accumulation, whereas in the latter the signal will be split equally between two accumulations. If you are only thresholding on individual accumulations, your sensitivity varies by a factor of two for sky points along a scan.

Okay, I mentioned that we looked at four cases. This work was primarily done by Jerry Solomon, Edward Olsen, Maureen Quirk, and Wayne Lawton at JPL and is the subject of an internal JPL report (TDA Progress Report). Figure 12 shows a brief summary of the four cases considered. In the first case, the data are accumulated over some fraction of a HPBW and a threshold is applied. The second case applies a "weight" factor to each accumulation which is taken over a small fraction of a HPBW (about one fourth HPBW). The "weight" factor is taken to be the beam response. In other words, we try to match the response with a convolutional filter in time. This requires as many accumulators as there are weighting coefficients and effectively removes the sensitivity scalloping along the scan line. The third case is an extension of the second to mitigate sensitivity scalloping between scan lines and includes a data decimator. You scan in one direction, applying the convolutional time filter but only save those data which exceed a certain threshold until the return scan. The same convolutional filtering and thresholding is done on the return scan and the data saved from the first scan are compared to those passing threshold in the return scan. Coincidences in frequency in neighboring beam areas are saved, the rest are discarded from the first scan memory. This process continues scan by scan. The fourth case retains all the data which result from the convolutional filtering for coincidence testing, without the data decimating threshold.

We have investigated the performances of these four cases. Let me first mention something that is known as an ROC curve or Receiver Operation Characteristic curve, which is familiar to communication engineers. The basic problem of SETI is to hold down the false alarm probability while raising the probability of not missing a signal that is there. The ROC curve shows the probability of detection of a signal as a function of its SNR for a receiving system operating at a given false alarm probability.

It's always a trade-off. Because the SETI systems have so many channels in them, the false alarm probability has to be set very low or all of your time will be spent going back and checking false alarms. The ROC curve is a convenient way to see the operating characteristics of a given system. Consider Figure 13 for example. If you have a false alarm probability of 10^{-10} for this particular procedure here, which is a correlation detector, you have a 90 percent probability of finding the signal you are looking at if the SNR is about 7. Of course, you have a probability of 10^{-10} that any signal that you detect with an SNR of about 7 is a false alarm.

B. Oliver: How can you detect a signal when the signal to noise ratio is zero?

The x-axis on the ROC curve (Figure 13) is a logarithmic axis although it is not shown on the slide (added in proof).

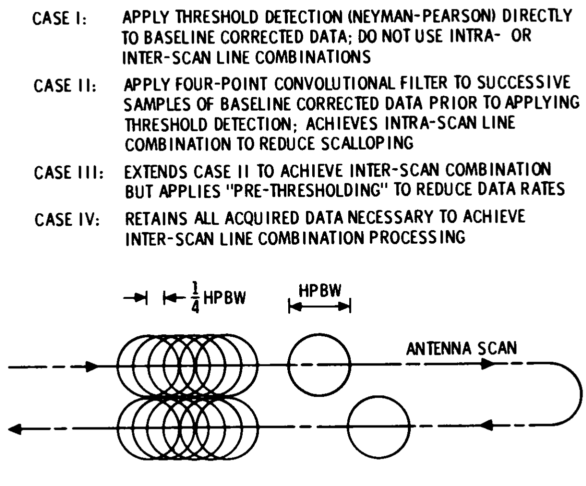

Figure 12. Four Sky Survey Signal Processing Cases

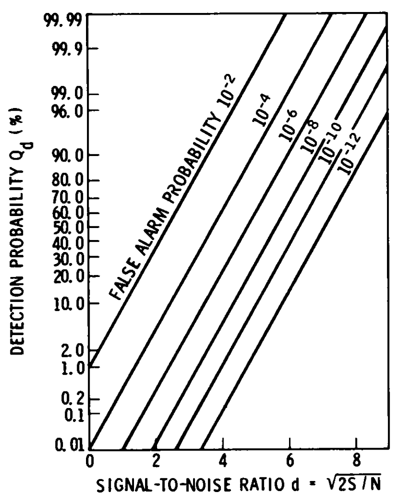

Figure 13. Typical Receiver Operating Characteristics versus Signal Detection Probability for a Receiving System Operating at a Given False Alarm Probability

Now that I have shown you a textbook example of ROC curves, I want to turn to Figure 14, which shows the results of the four cases we have examined. The figure shows the probability of detection versus the SNR for the four cases I described assuming we are operating at a false alarm probability of 10^{-12}. As you can see, case I has a very low probability of detection. We pick up about an order of magnitude across the board by going to case II,

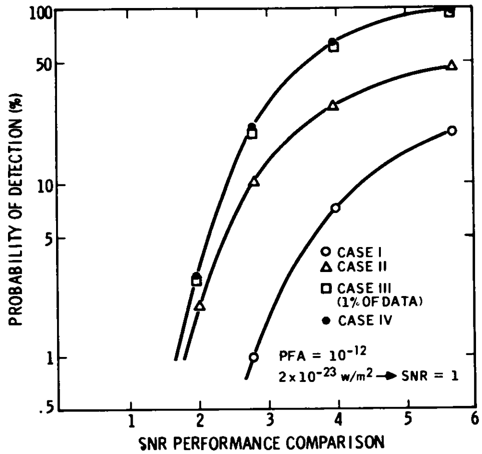

Figure 14. Comparisons of Four Signal Processing Cases and the Probability of Signal Detection at a False Alarm Probability of 10^{-12}

where the convolutional time filter is applied. Another factor of approximately two is gained by moving on to case III, where interscan combination is included although with a data decimator which only allows 1% of the data through. There is almost no improvement upon moving to case IV, where the data decimator is removed. At an SNR of about 5, you have nearly 100% probability of detecting a signal (if it is there) using the processing scheme for case III, and there is almost no gain to be had by keeping all the data (case IV). In other words, as you scan along in one direction, you can threshold, reduce your data rate to one percent of the data that you keep, and simply save that until you come back in the opposite direction and then combine those and you get this very large gain. So this cuts down the complexity of having to process all of the data. Figure 15 shows a companion study we have carried out for case III, but this time we are showing the probability of detection as a function of interscan separation for signals of different SNR.

Now this analysis assumes that we observe a pixel in the sky within a fixed amount of time. The pixel chosen was 60 degrees wide by 10 degrees high, and the time allotted to observe it was 3 hours 20 minutes. This means that the scan rate when the interscan separation is 1 HPBW must be half the scan rate when the interscan separation is 0.5 HPBW. In each case, calculations were made to determine what the detection statistics would look like. We varied the interscan separation and at the top of the plot you can see the scan rate in HPBW/sec, which is required. Here you see, if you want to maximize the probability of detection of a signal with SNR = 4 in this system, then you should choose your interscan separation to be about 0.7 and scan your antenna at about 1.1 HPBW/sec. For signals with higher SNR, obviously the probability of detection is larger. This example was carried out for a false alarm probability of 10^{-10}. We are continuing studies like this to define the parameters as to how to carry out the All Sky Survey efficiently.

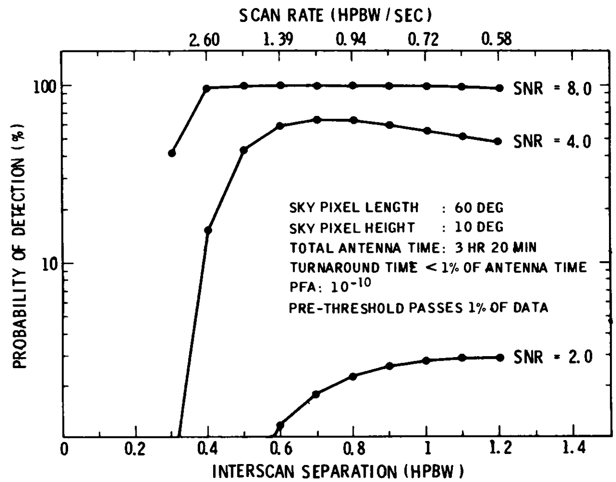

Figure 15. Detection Probability as a Function of Interscan Separation for Signals of Different SNR Using Signal Processing Case III

Now I am going to get off the processing strategy and give you a slightly different look at RFI. Seeger has already shown several figures which I had in my packet, so let me just turn to what the spectrum occupancy at Goldstone looks like in the 1-10 GHz band. Figure 16 shows the number of known transmitters that we have within a 250 mile radius and in a sector azimuth of 45 degrees. The protected DSN frequency range is this small gap near 2300 MHz and in this other one near 8500 MHz. Both are only 10 MHz wide. Hence we are very concerned with RFI and its effect upon the All Sky Survey. We at JPL have built, using commercial parts, a single-channel, swept frequency spectrum analyzer, which steps from 1 to 10 GHz. The system consists of a 1 m antenna with a rotary joint, 7 FET amplifiers, noise diode and switches, and a Tektronix 494P spectrum analyzer. These are all under control of a Tektronix microcomputer so we can observe RFI in the range from 1 to 10 GHz automatically, calibrating the system and outputting hits to floppy disks for transfer to a data base back at JPL under the relational data base management package called INGRES. We can operate over a wide range of resolutions and integration times. The idea is to get a first look at what the radio frequency interference is at Goldstone and the system has been built so that it's transportable and we can move it to other SETI observatories.

Let me mention one other thing that we're just starting to look at. This is actually very premature to talk about, but I think it's worthwhile mentioning at a meeting like this, since it is a workshop. I want to talk a little bit about the acousto-optical spectrometer. Consider Figure 17 and suppose you take your IF frequency output and drive a transducer to form a series of acoustical waves down a Bragg cell while illuminating the Bragg cell with a laser. This forms a diffraction grating in the cell and you get a spectrum on the output. You are all familiar with this. Now, the resolution of a system

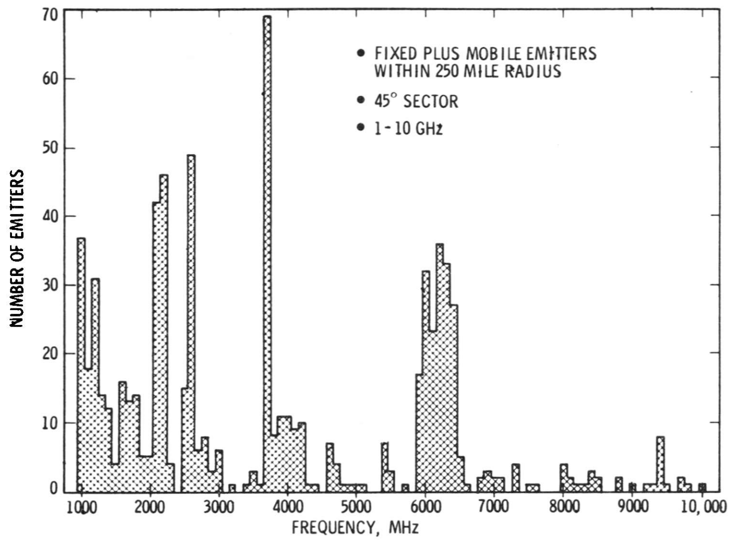

Figure 16. Spectrum Occupancy at Goldstone. (From ECAC Study - draft report 1982.)

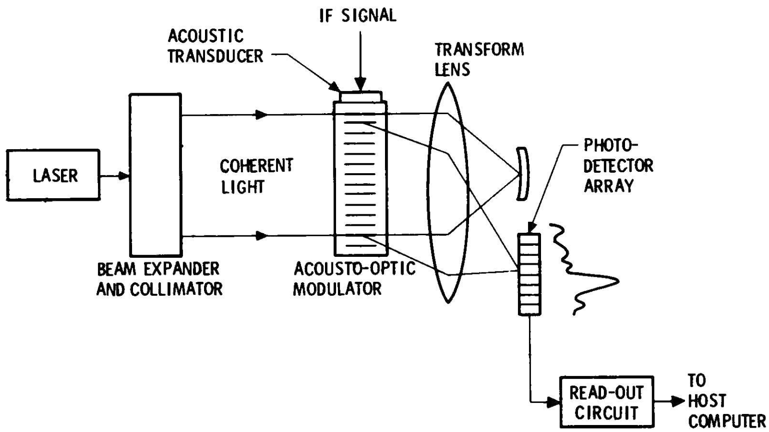

Figure 17. Typical Accousto-Optical Spectrometer with Low Resolution and Wide Bandwidth

like this depends on the acoustic velocity time across the cell. Typically, what has been achievable are resolutions which are on the order of 10 kHz to 100 kHz. The advantage of systems like this is that they support very broad bandwidths. You can put 500 to 1,000 MHz in this cell and it works very nicely for radio astronomy applications where you desire maybe 2 MHz resolution down to perhaps 40 kHz resolution. But if you want to get down to the Hz level they are not very good. Figure 18 shows some of the existing AOS spectrometers that I am familiar with; for example, CSIRO, the Owens Valley Radio Observatory, several that have been built at ITech and the one at Nobeyama. They operate at resolutions of between 37 kHz and 5000 kHz. You can see these resolutions are all limited by the Bragg cells that are available.

Now, for the application to the SETI All Sky Survey, we want the demonstrated instantaneous bandwidths of 300 MHz or 500 MHz, but we also require much higher resolution. Figure 19 is another drawing of the spectrometer, but with a second cell added to that system at the point where the coarse spectral output of the first cell appears. This second cell is crossed with respect to the first and its transducer is driven by a reference signal which is locked to the laser. You can now think of having a two-dimensional detector array which allows you to integrate on that signal. In other words, you are no longer limited by the transit time of the acoustic wave in the first cell, provided you can phase lock reference it to the laser. This work is being carried out by Professor Dimitri Psaltis at Caltech; he's investigating the feasibility for using this for SETI. There are a number of different configurations. Figure 20 is my last slide; it shows an interferometric configuration which is probably a more practical approach. Dimitri Psaltis is now carrying out a study in collaboration with the optics people at JPL. The idea is first to carry out a study to look at how such a device would serve SETI purposes and finally the next step of that would be to build one of these. Thank you.

ID	BRAGG CELL MATERIAL	TRANSIT 10^{-6} S	B MHz	ΔB KHz	T x B
• CSIRO	FUSED SILICA	10	100	100	10^3
• OVRO	?	10	102	160	10^3
• ITECH	TeO_2	0.2	700	5000	150
• NOBEYAMA	?	4	250	260	10^3
• NOBEYAMA	?	27	40	37	10^3

Figure 18. Operating Characteristics for Existing Acousto-Optical Spectrometers

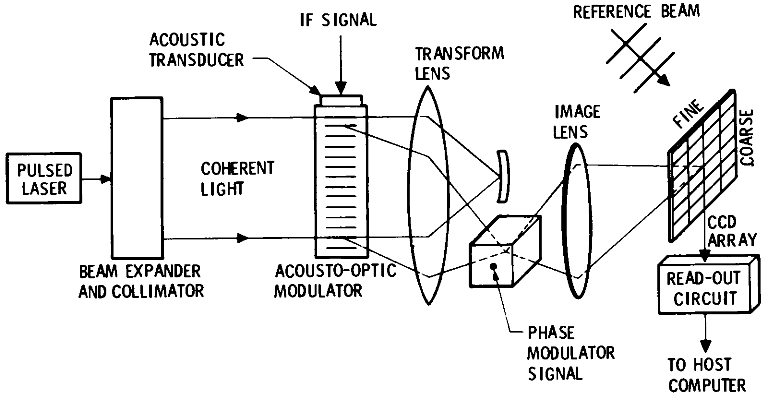

Figure 19. Acousto-Optical Spectrometer Incorporating Crossed Bragg Cell Concept

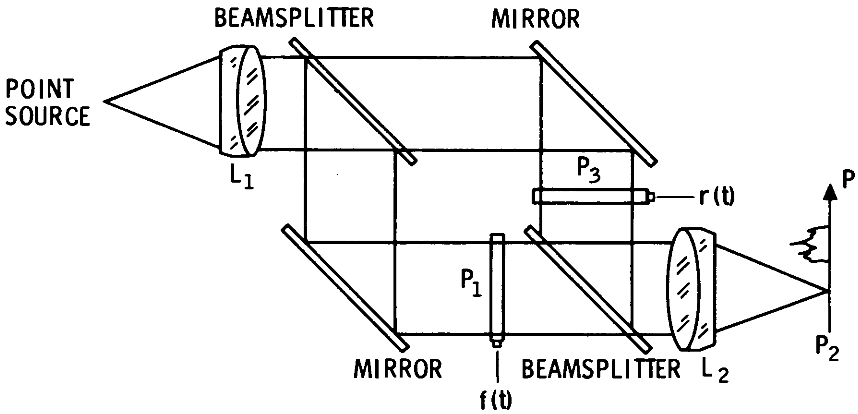

Figure 20. Interferometric Spectrum Analyzer Optical Schematic

ACKNOWLEDGEMENT

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RADIO ASTRONOMY ASPECTS OF THE NASA SETI SKY SURVEY

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It has long been recognized that radio astronomical investigations could benefit from the technology developed for SETI and from the data bases produced by observational searches. This potential synergism for several classes of SETI instrumentation and search strategies has been summarized by Cuzzi and Gulkis (ref. 1)and in the Report by the SETI Science Working Group (ref. 2). Now that the NASA SETI program is formulating search strategies and building prototype instrumentation, we believe it is time to consider how we will collect and archive subsets of the SETI data for use by the radio astronomy community.

The NASA SETI program consists of two search strategies: a Sky Survey (SS) and a Targeted Search (TS). Both searches will very likely yield valuable radio astronomy data. Today, however, I shall focus on the Sky Survey, and in particular, the utilization of the survey to produce one or more radio source surveys at frequencies above 1 GHz.

You may recall from Sam Gulkis' description of the Sky Survey that we are now planning to sweep the antenna beam back and forth across small areas on the sky. As each area is surveyed, we plan to save some of the data, temporarily, so that events that pass threshold on one scan can be correlated with events recorded on the adjacent scans. Recall, however, that one of our objectives is to process the data "on the fly". We are not going to save vast quantities of data that would have to be analyzed at some later time. We must complete most of the processing on line because the data rate is so great.

Baseline and threshold operations will reduce the data rate by a factor of at least one-hundred. The spectra of events that pass threshold will be passed on to the SETI Sky Survey Processor and to a Natural Source Processor where radio astronomy information will be extracted and stored on disk or tape. Algorithms for the Natural Source Processor have yet to be developed. We are beginning to consider the various kinds of data that should be processed and saved.

Some catagories of radio astronomy that could benefit from the Sky Survey have been identified; others will be serendipitous. Who can predict what we might discover as we survey the entire sky with spectral resolution much greater than anything done so far? The Sky Survey will be working at a resolution of 30 Hz, or greater. We will have the capability to survey certain regions of the sky with even greater frequency resolution.

Today I would like to report on some work relating to a data type that we know will be available. We know we will be counting and cataloging thousands of continuum radio sources in the 1-10 GHz region and at other selected frequencies above 10 GHz. One of the higher frequency bands will probably be at the 22.235 GHz transition of molecular water. How many sources are we likely to detect as we carry out SETI? That is the question I wish to answer today.

I've found a few papers that seem to be particularly relevant to this problem. One is a recent paper by Peacock and Gull (ref 3) which describes models of the radio luminosity function (RLF) at different frequencies. From their work I was able to interpolate and extrapolate to see what the RLF might be at SETI frequencies. For example, I will show you results at 2 GHz, 8 GHz and 22 GHz.

A second paper that I found especially useful is the Deep Sky Survey work done by Fomalont, Kellermann, Wall and Weistrop. They published just last summer in Science (ref 4). Using their results, I was able to extrapolate to flux levels that I think we will reach in the SETI Sky Survey.

A third piece of work, which I have not seen published yet, is the University of Texas Low Frequency Survey at 365 MHz. I have a report (ref 5) of their survey of an 18-degree declination strip, which is the first of twelve similar declination strips that will constitute the full Texas Survey of the entire sky north of -35° declination. I spoke with Jim Douglas and he estimates that their full survey will list something like 70,000 sources, so they are going to have a very large low-frequency database at 365 MHz. If we are able to complement that work with a large database at high frequencies, then one could perhaps extract new information on the spectral indices of radio sources. I'll talk more about that in a few minutes.

First let me describe my analysis which leads to a prediction of how many sources we might expect to catalog. I begin with the models of Peacock and Gull and calculate the different RLF's for the steep spectrum sources and the flat spectrum sources at several frequencies. Their models fit the published differential source counts at 408 MHz, 2.7 GHz and 5 GHz quite well. I then integrated the differential source counts and plotted the integrated source numbers vs flux density in the usual manner, i.e., the Log N vs Log S plot. Figure 1 is the result for frequencies near 2 GHz. The two solid curves show the results for the steep (ST) and the flat (FL) spectrum populations.

Now to determine how many sources we will detect, we must know the sensitivity of the Sky Survey at the frequency of interest. For the NASA Sky Survey we are planning to use a 34 meter antenna with a system noise temperature of about 25 K. The system sensitivity near 2 GHz will exceed the confusion limit, which is about 250 milli-Janskys. Therefore, we cannot reliably catalog sources weaker than a few hundred milli-Janskys. I don't believe we would be able to contribute much new information at that flux density limit at frequencies near 2 GHz.

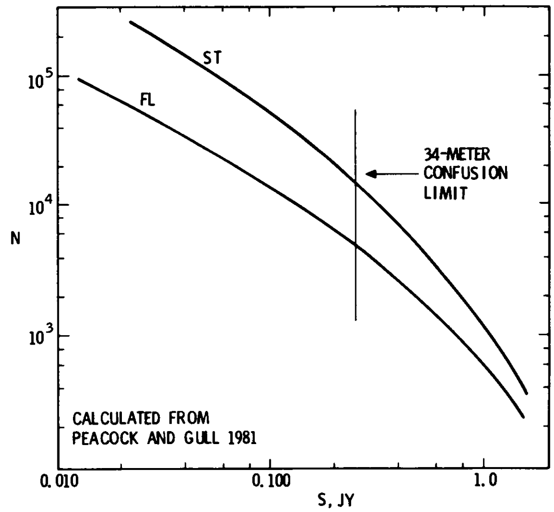

Figure 1. Source Number vs Flux Density at 2 GHz Density

So let's consider the higher frequencies of the Sky Survey and, at the same time, consider improving our sensitivity by combining data over several contiguous frequency bands. We will be carrying out the Sky Survey in frequency increments of 250 MHz. The survey processor will be programmed to recognize continuum radio sources and to archive vital statistics such as flux density and celestial coordinates. We could improve our sensitivity by a factor of four if we average the data from the 16 frequency bands required to complete the survey from 6 GHz to 10 GHz. The higher frequency surveys would be the most interesting because that is where the surveys will become sensitivity limited and not confusion limited.

The source count estimates for 8 GHz, the center frequency of the sixteen 250 MHz bands, are shown in Figure 2. Once again the two curves show the integrated source counts for the source populations with flat and steep spectral indices. The confusion limit for the 34-m antenna at this frequency is less than 0.01 Jy, significantly below the sensitivity limit of approximately 0.03 Jy. Operating near the sensitivity limit, we should be able to detect around 14,000 flat spectrum sources and about 41,000 steep spectrum sources if the luminosity functions are correct.

The large-area survey data at high frequencies tend to cut off near the 0.100 Jy flux density level, and for this reason we might question the extrapolation of the RLFs to flux levels below 0.100 Jy. To gain insight into this problem, we turn to the deep survey at 5 GHz by Fomalont et al (ref 5) used the VLA to map a small field (0.02 square degrees) at flux levels below 100 micro-Janskys. The results of that survey, shown by the dotted curve in Figure 2, suggest that the integrated source count for flux densities greater than 0.03 Jy at 5 GHz is only about 50,000 sources. Since the number of steep spectral sources brighter than a given flux density declines as frequency increases, this result suggests that the number of steep spectral sources predicted by the Peacock and Gull model may be too large.

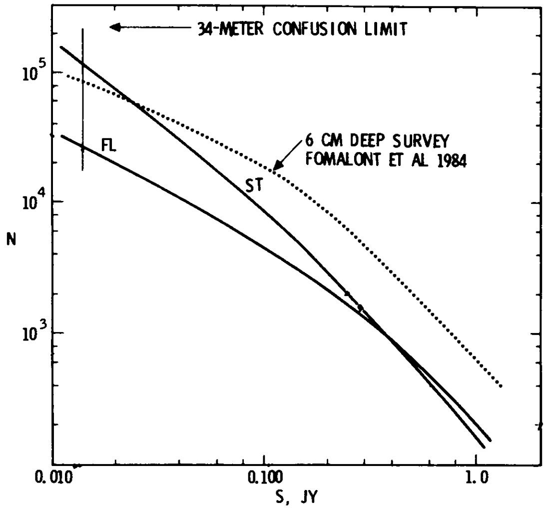

Figure 2. Source Number vs Flux Density AT 8 GHz

A reasonable compromise might be to predict that the Sky Survey should detect and catalog something like 50,000 sources with flux densities greater than 0.03 Jy.

Now to summarize, we can see how many sources we can expect to see at the two frequencies. I have summarized the results in this table:

	2 GHz	6-10 GHz
• CONFUSION LIMIT [34-METER ANTENNA]		
• ρ_C [SOURCE/30 Ω_B], PER STERADIAN	1,460	230,000
• S_{MIN} [CONFUSION]	0.250 JY	0.008 JY
• SOURCE COUNT PREDICTIONS		
• S_{LIMIT} (CONFUSION OR SENSITIVITY)	0.250 JY	0.030 JY
• N_S (STEEP SPECTR. SOURCES)	14,000	36,000
• N_F (FLAT SPECTR. SOURCES)	5,000	14,000
• TOTAL FOR ALL SKY SURVEY	~19,000	~50,000

Table 1. Summary of Source Count Predictions

At the top I show the confusion limit as a surface density (sources per steradian) and the corresponding flux density limit. For these calculations I assumed that the confusion limit is reached when the average surface density is one source in 30 beam areas (Ω_B = one beam area).

W. Sullivan: What is the crossover frequency, Mike, for the confusion limit and the sensitivity limit?

Oh, I didn't calculate that frequency, but it's obviously between 2 and 8 GHz ... it must be about 5 GHz or so.

In the lower half of the table I summarize the source count predictions. At 2 GHz I get about 14,000 steep spectrum sources and about 5,000 flat spectrum sources. The total at 2 GHz is about 19,000 sources. At 8 GHz we can expect to catalog about 50,000 sources with about 2/3 of them coming from the population of steep spectrum sources. Note that I have reduced the predicted number of steep spectrum sources to compensate for the overestimate I spoke about.

Now I think the value of a continuum source catalog in the 6-10 GHz region will be to compare source positions and flux densities with other surveys. In particular, a high frequency catalog might be very useful for comparison with the 70,000 sources that have been seen and cataloged in the Texas survey, which to my knowledge is the largest survey to date. That is, it has the largest number of radio sources cataloged to date. A comparison of a high frequency 34-m survey, which is not confusion limited, with the Texas survey, which does have some confusion problems, should be done. While the Texas survey was done interferometrically, I'm not sure how clean the beam was. It would be worthwhile to try to confirm the positions of individual sources in the two catalogs and, of course, compute the spectral index of each mutually identified source.

By the way, I have also calculated the number of sources we should see at 22 GHz. I do not have a slide to show you, but the numbers seem to fall in the range 15,000 to 20,000 sources if we use the 34-m meter antenna and archive the continuum sources detected in a single 250 MHz survey centered at 22.2 GHz. Of course that particular band of frequencies includes the water line at 22.235 GHz, so we might detect new water line masers, for example. I have a fondness for doing SETI at 22.2 GHz because it is surely among the prime candidates of 'magic frequencies', and it has great potential for radio astronomical results as well. As far as I know, there has not been an all sky survey at 22 GHz. I think that's true.

K. Kellermann: There hasn't even been a significant piece of the sky surveyed at that frequency.

We might follow Woody Sullivan's suggestion to concentrate on the galactic plane ... spend more time there and increase the sensitivity of the 22 GHz survey near the plane.

Well, that's the end of my paper. It looks like we will have a large number of sources to look at, to calibrate and to catalog. The next step in this analysis will be to develop a plan to handle the job. We'll also continue to explore other types of radio astronomy data that might come from the SETI observations to see just how much we can squeeze out.

S. Gulkis: *I'm not sure you made it explicitly clear that you don't get down to 25 milli-Janskys on a single scan, but you need to superimpose all of the data from 6 to 10 GHz in order to get the bandwidth you need.*

Oh, that's absolutely correct. I thought I explained that. We will have 250 MHz bands and we'll superimpose the data from 16 bands to fill out the 4 GHz total bandwidth and achieve the factor of four gain in sensitivity. I may not have made that clear.

B. Burke: *Just a small 'plug'. I don't think it's generally known that the MG Survey exists. It is a survey done to a flux limit of about a tenth of a Jansky, at 6 GHz, and therefore it gives you a good datum point on these projections. In other words, Peacock and Gull, I think, is not the most reliable way to go. It may be better to use the MG point at 0.1 Jansky and then use the Kellermann et al extrapolations to lower fluxes. You'll get about the same numbers, but I think that they are more reliable.*

Excuse me, MG stands for...?

B. Burke: *...the MIT Greenbank survey. The work was done here at Greenbank.*

S. Gulkis: *Is Chuck Bennett involved in that work?*

B. Burke: *That's right. It was Chuck's thesis. It's going to be published in the Ap. J. Supplements.*

J. Broderick: *There's already a 6 GHz survey from Greenbank published. It's a smaller range of the sky, but it did see enough sources to do number counts.*

B. Burke: *We're producing a paper on the number counts specifically.*

J. Broderick: *We've done a paper on the number counts.*

B. Burke: *I know —— ours will be an improvement.*

ACKNOWLEDGMENT

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MACRO EVENTS IN A SETI ARCHIVE

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Ohio State University

The OSU radio telescope has been engaged in a full-time SETI program since 1973. Figure 1 is an "aerial" view that shows the overall structure of the telescope. The large field in the middle is three acres of heavy duty aluminum foil which serves only to prevent ground radiation from getting directly into the feed horns that are sitting in the middle of the field. The receivers and computers are located in an underground room directly beneath the feed horns.

Figure 2 is a not-to-scale cross sectional diagram of the telescope. The only thing that moves is the flat reflector, which can stand almost straight up and can come down quite a bit further than shown there. The underground laboratory, since it contains all the electronic equipment, makes it very easy to work on things without climbing up to the focus of a dish, which is sometimes a problem. Changes are gradually being made in this arrangement and I'll talk more about that as we go on.

The telescope is a meridian transmit instrument having a collecting area of about 2200 square meters, which is about the same as a 175 foot circular

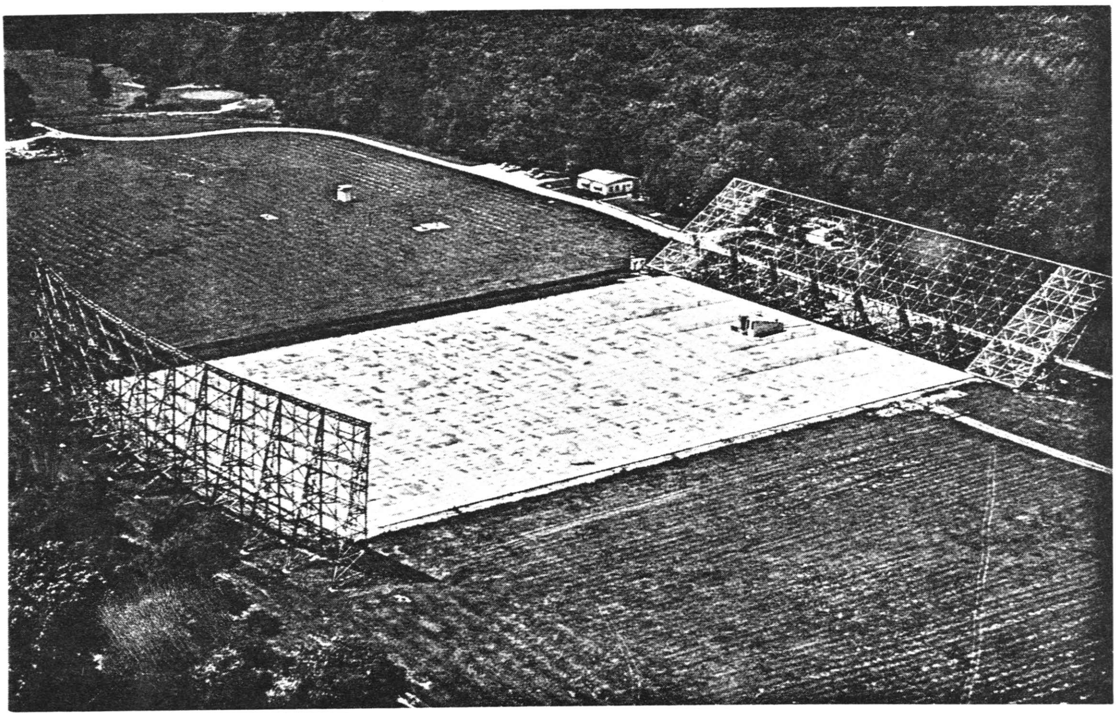

Figure 1. An "aerial" view of the OSU radio telescope.

dish. The beam width at 21 cm is 8 arcmin. in right ascension and 40 arcmin. in declination. It can cover a total declination range of about -36 to +63 degrees. We have been searching for narrow band signals near the hydrogen line, as Doppler corrected to the Galactic Standard of Rest. The strategy that we have adopted is to assume that the other civilization knows all of their motions and that therefore they will correct for them in their transmitted signal. We know all of ours and we will correct for ours by offsetting our receiver appropriately. The only place that is a common origin for everyone in the galaxy is at the center of the galaxy, so we assume that we will both correct to that. The largest correction of all is our galactic rotation, and that also puts the largest uncertainty into it, so we are covering a total instantaneous bandwidth of 500 kHz which is enough to allow for all of those uncertainties.

The receiver system is shown in figure 3. We have actually two feed horns, so we are operating two beams separated by a few minutes of time in the sky, using a beam switching technique so we are really just looking at the difference between those two signals. If there is a genuine signal there, we see it first going positively, and then going negatively as the beam sweeps through a particular place in the sky. The second local oscillator is computer controlled to continuously update the receiver frequency to compensate for the various Doppler shifts of the earth-moon system, the earth-sun system, solar motion, and galactic rotation. The back end is not too complicated. We have a 50 channel filter-bank receiver operating which we borrowed from NRAO. Each channel has a bandwidth of 10 kHz. The entire system is controlled by a rather ancient (but very reliable) IBM 1130 computer right now, although we have a much better one sitting in the wings. It even has a card reader and a punch attached to it. As a matter of fact, we now have three of these machines; we plan on cornering the market

THE OHIO STATE UNIVERSITY RADIO TELESCOPE

Operational Diagram
(Elevation cross-section view, not to scale)

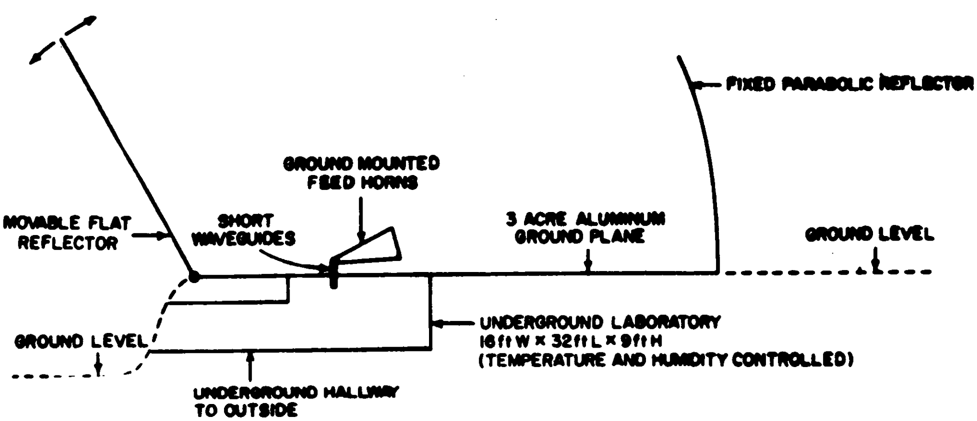

Figure 2

in them, so eventually we will be in a very strong position.

J. *Tarter:* Why do you have three of these machines?

One of them is the observatory control computer. Another is located on the campus 25 miles away and is used for off-line statistical analysis of the data. The third one is used for spare parts.

Figure 4 shows the portion of sky surveyed thus far. Theoretically, if we ran everything at the maximum speed, we could cover the whole sky visible to the telescope in about a year. But intentionally it goes much slower than that because we typically will take several scans at each declination and there are times when the telescope is down temporarily for new equipment installation, repair of equipment, and all sorts of things. Due to all those things put together, we have yet to cover the entire sky once.

One of the three regions that has been covered in greater detail is the one way up at the northern end. We did that simply because it was at the northern declination end of the telescope and we wanted to see how things behaved up there. Another zone near +40 degrees was chosen because M31 is in this area and we wanted to look at that in some detail. And then another region around -30 degrees was chosen because the galactic center is in that area. I'll talk a little bit more later about what we've seen in each of those places.

All of the equipment and the entire operation is computer controlled. There does not have to be anyone there when the telescope is operating, and usually there is not. There is a full-time mechanical technician that is there during

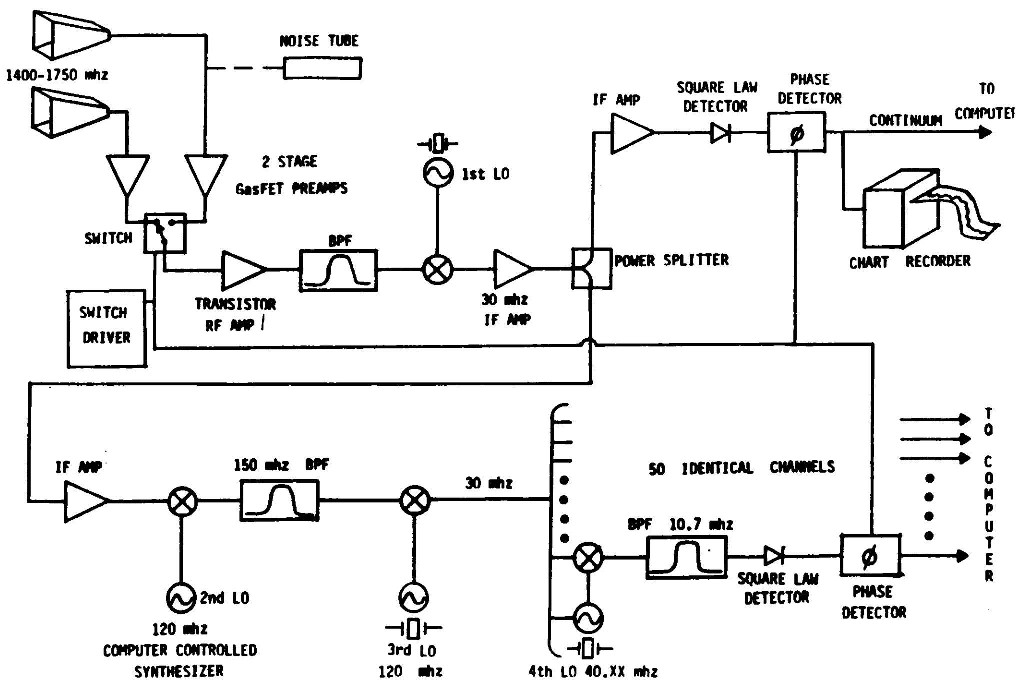

Figure 3. Block diagram of the receiving system.

normal working hours Monday through Friday. The only people there at other times will be some of us or students who are there on a volunteer basis just checking on things or making improvements in the equipment.

All of the data reduction is done in real time and on line. The data is accumulated from each of the 50 analog channels. Since they are all independent from each other they all have slightly different gains and slightly different baselines and so the computer continuously calculates all of those things and normalizes the data into units of sigma. By the time a human being sees it they see numbers (1, 2, 3, etc.) representing how many units of sigma the particular power was on that channel at that instant of time.

There are a number of strategies, in addition to this general processing, that the computer applies to the data to see if there are any signals there.

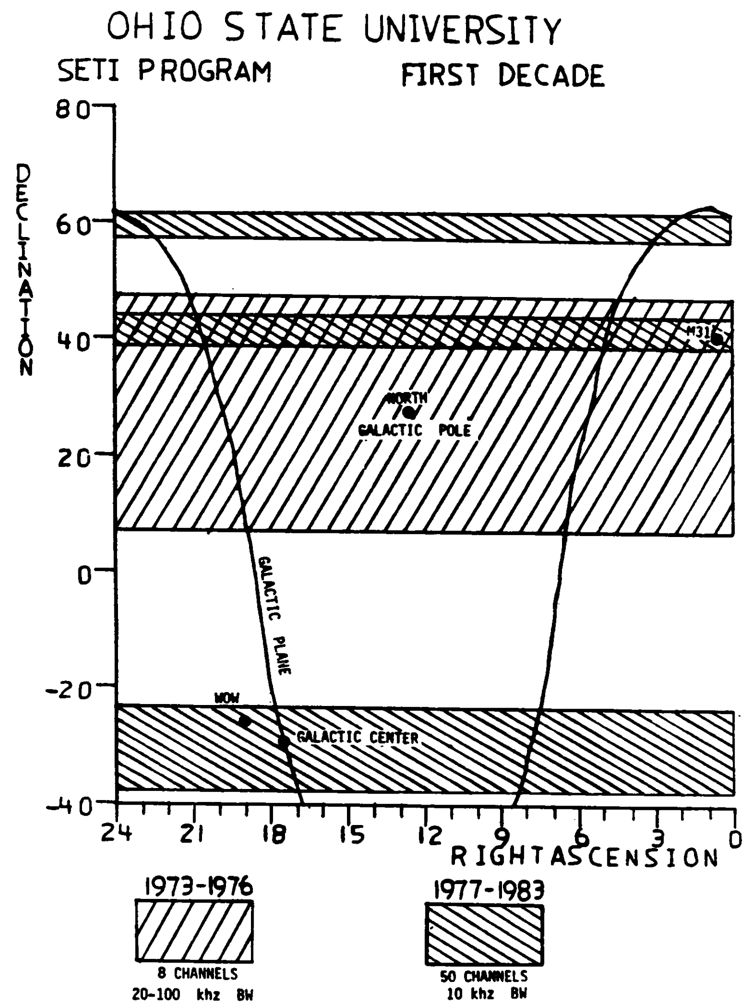

Figure 4. Sky regions surveyed thus far in the Ohio SETI program.

Some of them are very simple such as looking for a single high channel or looking for a single channel which has two successive integration points, or three successive integration points in which a signal exceeds a certain threshold. The most powerful one is when, for a single channel, the computer (having recorded data for about a minute, the time an object takes to go through a beam) calculates the normalized cross-correlation function between the antenna pattern and the data. That is essentially a matched filter because any point source has to map out the antenna pattern as it goes through the sky. That technique enables us to go a little bit deeper into the noise. All of these detection strategies are running at the same time and if any of them exceed a threshold the computer will flag the appropriate data and print out a special message. I'll show you some examples of what those look like later.

Figure 5 is an example of what typical data coming out of the computer looks like. The area on the left that looks like hen-scratching is the actual data. There are 50 channels, so each column running down the left is the output as a function of time of a single channel. The channels are labelled across the top. This information on the right tells us what we are looking at - right ascension, declination, local oscillator frequency, galactic coordinates, and so on. Zero is suppressed in the data for clarity so there is just a blank that means the

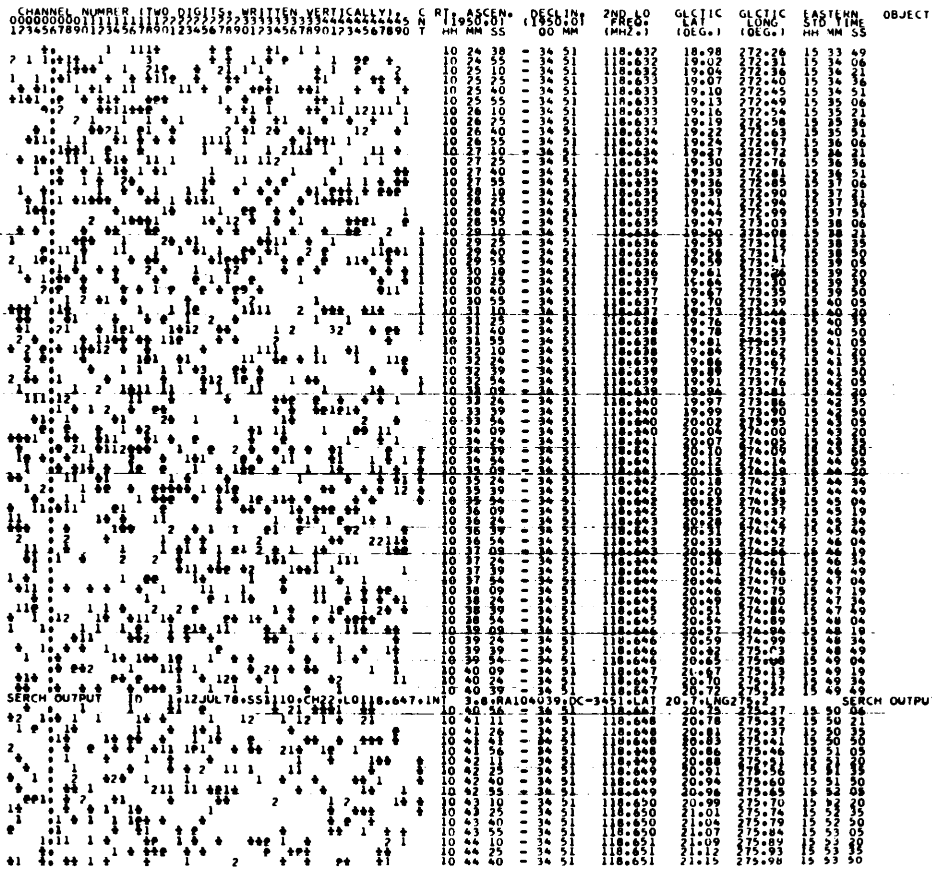

Figure 5. Typical data.

signal level was somewhere between plus and minus one sigma at that particular time. If the signal goes down, which it can - either due to noise fluctuation or because it is coming in the beam which is going down - it overprints a minus sign on top of the number to save space on this printout.

B. Oliver: You mean you're recording amplitude rather than power?

If the signal goes down from zero....

B. Oliver: You mean you're recording amplitude rather than power?

J. Tarter: Its two beams.

There are two beams in the sky. We are measuring the difference in power between the two.

The detections which occur from time to time are all recorded, and in the ten years we've been doing this we have about 30,000 of these and they are all filed away in a big archive.

Figure 6 is an example of what happens when we are observing galactic

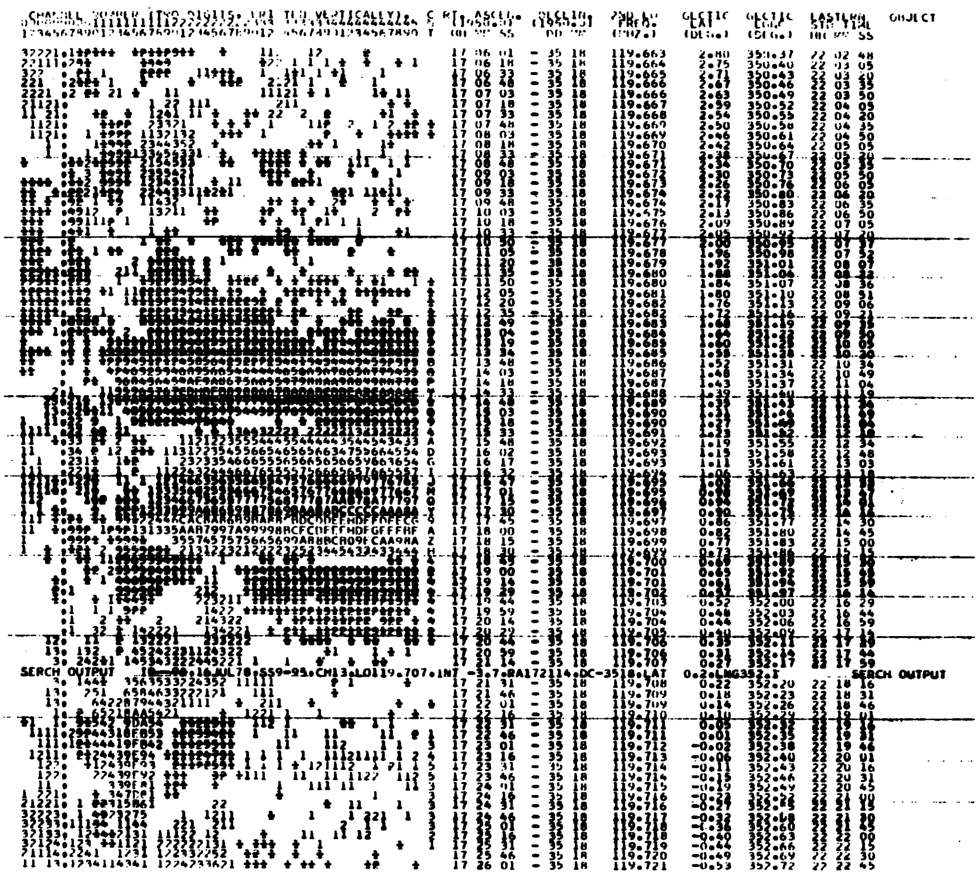

Figure 6. Galactic hydrogen.

hydrogen. We have nice big cloud structures that are either going positive or negative, depending on which antenna beam they are in at any moment. You can see generally that there are no detections or false alarms caused by hydrogen, except in rare cases. Notice there are some 6's in there, and some 5's which would be above the threshold, but the computer is programmed to ignore such signals. If a signal occurs on any two adjacent channels, it says, 'I don't want this, no matter how strong it is.' Or if it persists for more than some certain length of time, meaning it is an extended object in the sky, again the computer says, 'I don't want this because it is not a point source.' We recognize that we may be intentionally throwing away something important by these exclusions, but on the other hand we can't deal with all the false alarms that would otherwise occur.

M. Papagiannis: What channel there, Bob, is continuously positive?

That is actually a comma that is printed continuously in channel 6. One thing I forgot to say is that the computer is also testing all of the 50 channels all of the time to determine if they have reasonable values of DC level or reasonable values of RMS noise. If they are outside of an acceptable range, it says, 'That channel isn't working.' Then it prints out various symbols in that particular column, to tell why it wasn't working. In this case that particular channel wasn't working so it printed a comma, meaning it failed a particular test.

We took all of these 30,000 detections that we have amalgamated in about the last ten or so years and tried to see if there were any statistical or overall macro significance to this whole thing. That is - were they random in the sky; were they random as far as channel number goes or frequency or all sorts of things like that. And we made many many different maps by varying practically all of those different quantities that you can imagine and the net result is that almost everything shows absolutely nothing. Everything seems to be random. I have some examples of some of those maps. Figure 7 is an example of a computer printed map that we made. This is search strategy nine, which is the one that matches observed data to the antenna pattern, and this is in the area of the galactic center. There isn't anything there that is actually too exciting.

W. Sullivan: What are you actually showing there?

These numbers represent - on the average - the number of events per 10 days which occurred in blocks of sky of half an hour in right ascension by 20 arc min in declination. The purpose here is not necessarily to look for specific objects in the sky, but to see if there is anything at all unusual related to galactic structure or anything else.

W. Sullivan: What does the signal look like on a block like this?

If there were a strong signal that persisted through every observation, then you would see some large numbers sitting in that block.

Gulkis (?): We want to know what number you think is significant. What would turn you on?

In other words, what would make my day?

Gulkis (?): Yes.

Some of this data shows much more clearly on the contour maps I'll show in a moment. There is a certain amount of quantization that occurs sometimes in amalgamating these things together. So some of these numbers, even though they may seem particularly interesting, are not. For example, note all those higher numbers that happen to fall along a single row at -27 degrees and 40 minutes, which also happens to be the declination of the WOW object. This effect occurs because we spent over 100 days of observation at that particular place looking at a wide range of frequencies. Therefore, the data at that

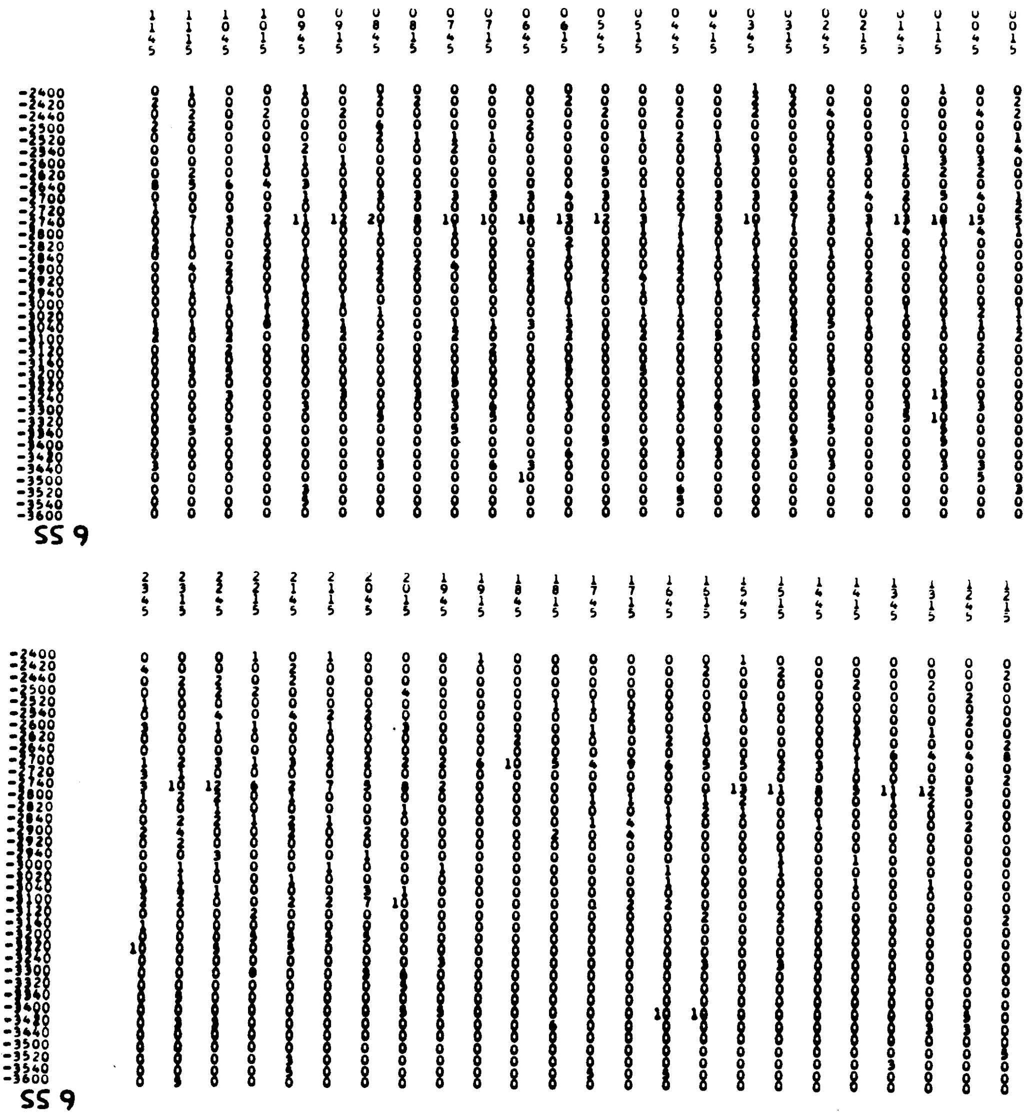

Figure 7. Search strategy 9 maps.

particular declination is not uniform compared with the data at the other declinations. Its not at the same frequency. The statistics are not the same. I don't mean to imply necessarily that in all these ten years everything is exactly the same. Therefore this is not exactly a uniform survey. For some of the things like that that occur on these maps, we know the reasons why that happened, so I don't get too excited. But I would say if there was a number - even a 10 in there - such that when we go back and look at the original data and determine that nothing explainable contributed to that number, then I would get excited about that.

It gets even less exciting if you look at the galactic center area in search strategy three, which means three successive integration times. (Figure 8) And about the same with two successive integration times. (Figure 9) But then again there is a 10 in there. Its a little bit unusual.

B. Oliver: How can you get a 10 out of two successive integration times?

Because these are averages over a block of sky that could contain a number of separate events.

The interesting thing is when we look at the isolated pulse-like detections which occurred for only one integration period - then the result is no longer random. (Figure 10) The galactic center is near the center of the figure. The galactic plane runs down almost vertically through the center of the figure. You see that more or less in the polar areas there is a big blob of things, whereas near the galactic plane itself there is a string of zeros. There is an anti-correlation with the galactic latitude. We don't know what causes that.

B. Oliver: What does each column correspond to there?

This is essentially a right ascension and declination map of the frequency of events in various blocks of sky.

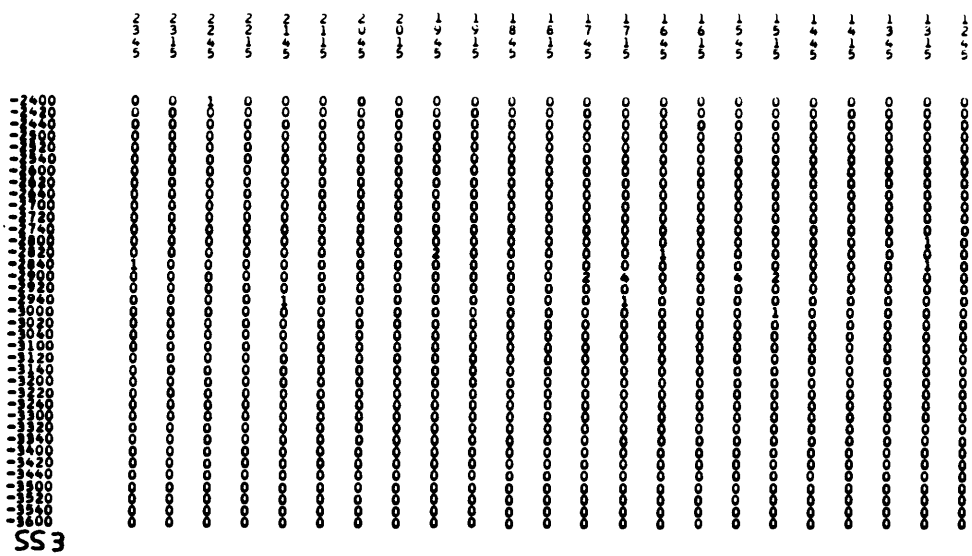

Figure 8. Search strategy 3 map.

Figure 11 is a contour map of the same information shown in that last map. This particular one is in units of events per day, and it shows the galactic center and the galactic plane. Ignore the anomoly at -27 40 that I mentioned before. There is a distinct peak which occurs just before the galactic center, and it is not just due to any isolated single point; there are many independent sky blocks contributing to it. The best explanation I can make for that (and I don't really know that this is true) is that as we approach the galactic plane

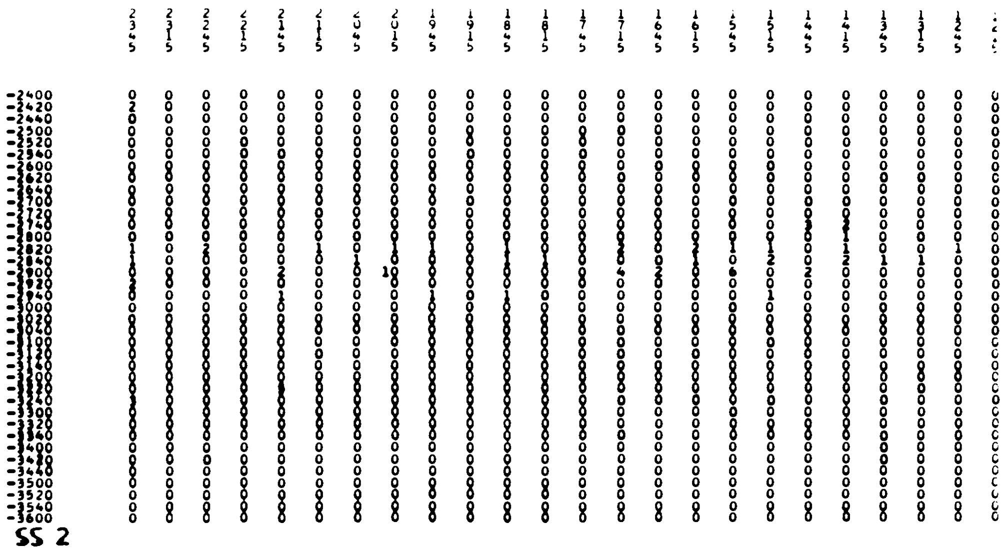

Figure 9. Search strategy 2 map.

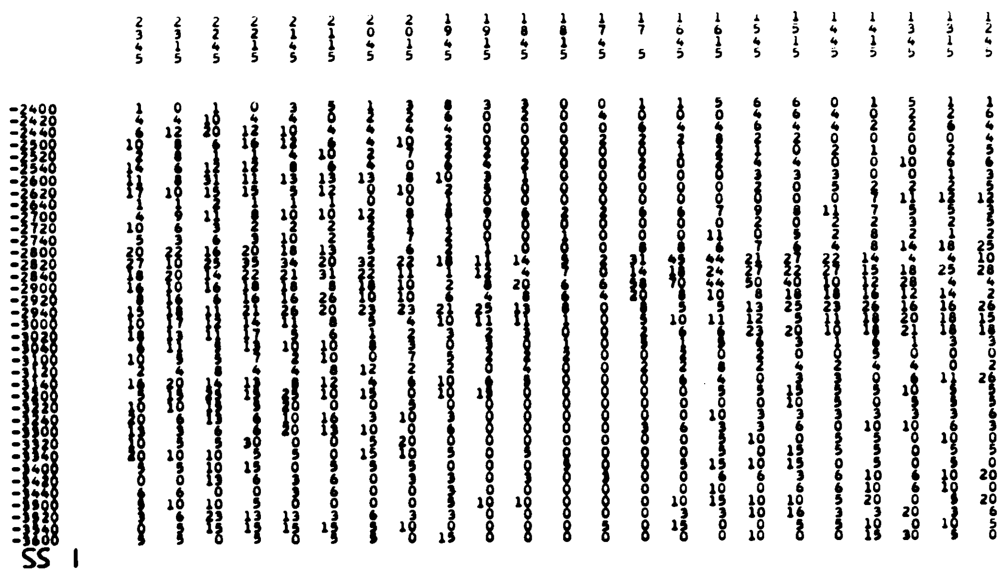

Figure 10. Search strategy 1 map.

(in fact the galactic center), as we are drifting along we encounter a very steep gradient in the continuum background, and it may be that the baseline level subtraction routine gets a little bit behind where it should be and therefore causes large events like this to happen. But on the other hand, if you go back and look at the raw data, and I'll show you some samples of that, these are not long, broad things at all. They're just things that go 'poof' in the night. In one integration period we have an isolated high intensity point.

Figure 12 is the same information except it is weighted in a different way. In this case, it is a map of average intensity, instead of events per day as the previous one was. In the map of events per day (the previous one), a high value implies that there are a large number of not necessarily strong events which occur in any particular place, because the intensity is ignored in the calculations. If you include the intensity, you can have a smaller number of strong

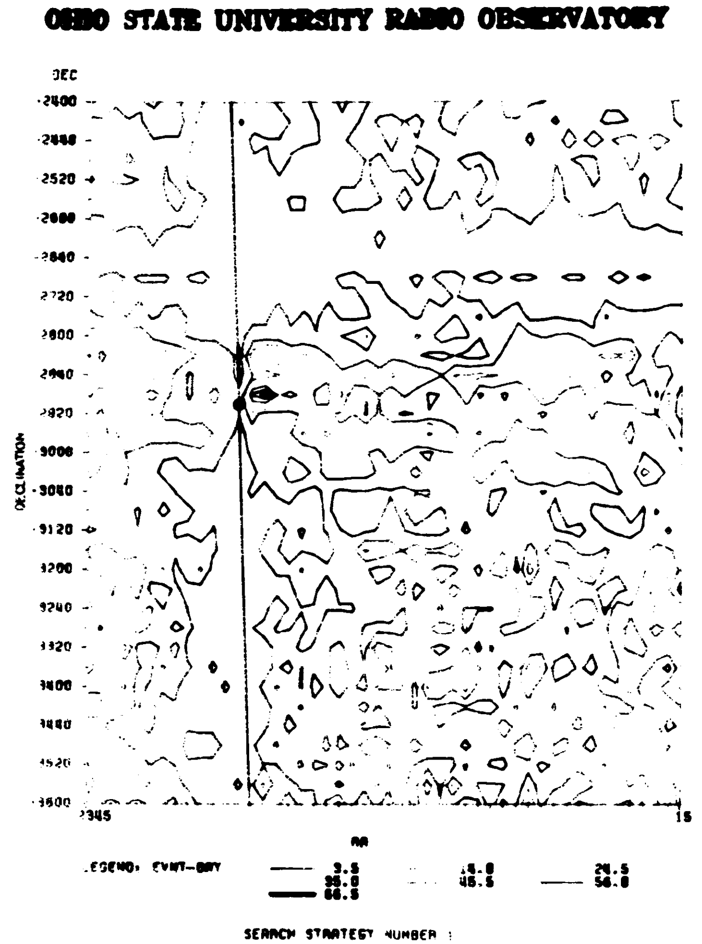

Figure 11. Map of the average number of isolated single-point detections per day, disregarding their intensities.

events, creating the same high value. This object - or rather effect - that was present in figure 11 is no longer present; but on the other hand, there is another 'thing' that has appeared lower in declination. That particular thing I have no explanation for.

Of course since these are single things that just go 'poof', it would be easy to say that something went wrong - one bit was wrong in the analog to digital converter or something like that, which would cause a single integration point to be wrong - and that is certainly possible, but on the other hand, I cannot understand why that would relate in any sense to sky coordinates.

Here are some specific examples of these isolated events.

W. Sullivan: Do you get the same anticorrelation with the galactic plane when you are away from the galactic center?

No. Only in the specific area shown here.

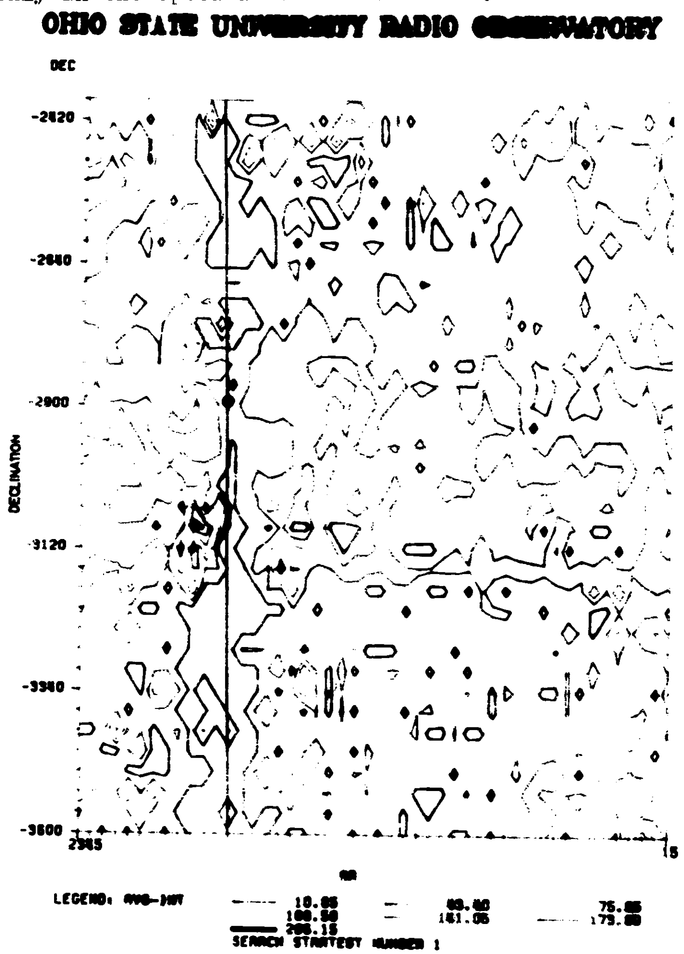

Figure 12. Map of the average intensity of isolated single-point detections.

I might also add that we have surveyed the same area of the sky twice separated be seven months and saw the same effect both times. Figure 13 is a typical event that I pulled out of the archive at random. It may not be immediately obvious, but if you look right in channel 7 just above the "serch output" line, you can see the letter 'F'. Going down this column, we see -1, 0, 1, 0, 0 and suddenly there is an 'F' - meaning that a really strong signal came in that channel at that time, or else some fluke occurred with the equipment that we cannot understand at the present time. Everything else looked perfectly normal. Note that if the intensity rises above 9, the computer prints it as A, B, C, etc. up to Z. The computer picked that out as it is programmed to do since there was a specific high event there. That data was saved in the archives. There is no correlation in these events with respect to channel number.

S. Gulkis: Bob, can't you calculate the frequency? Lets say its at pure noise. You should get 10 sigma events once every 100,000 or whatever events - I don't know what the statistics are. Have you checked against that?

No. I have not specifically done that. I imagine that's something that should be done; but even if that were true, I still can't understand why these events would relate in any sense to sky coordinates. You'd expect to find them uniformly everywhere.

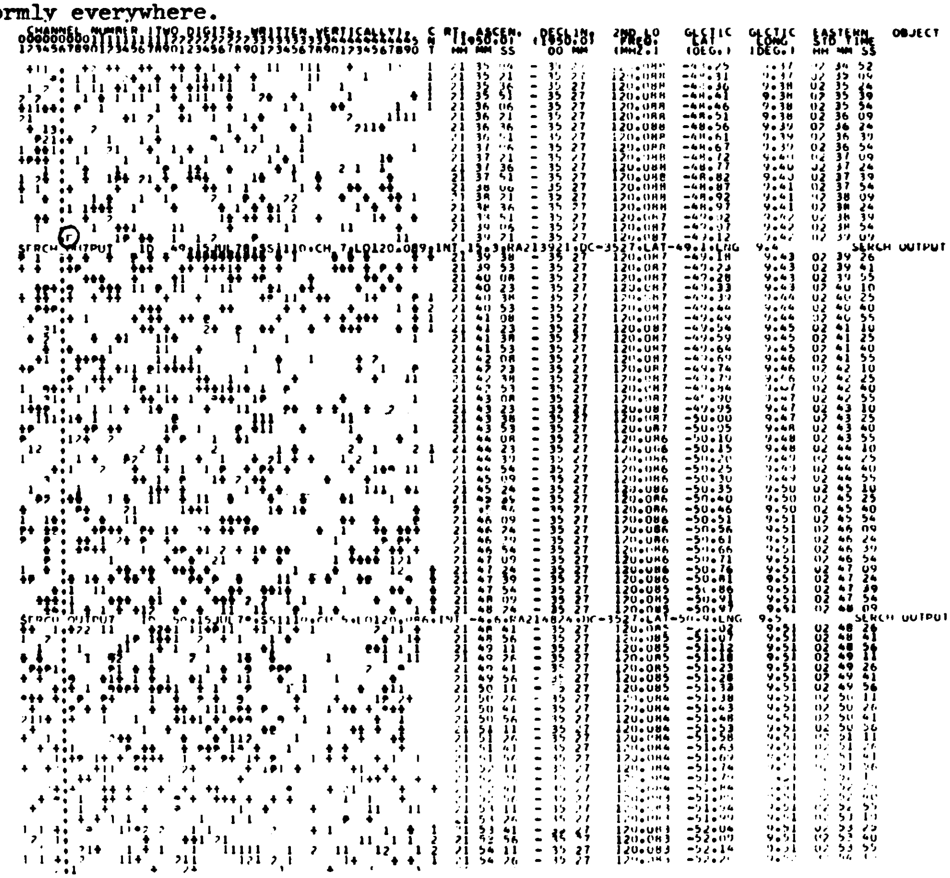

Figure 13. 'F' event.

J. Tarter: You know, Bob, there is a relationship because this is basically a transit instrument and you're doing declination scans on successive days and if its interference coming in your side lobes then basically you're looking at the same terrestrial coordinates each of those days, as well as the same galactic coordinates. So what you may be seeing is the fact that you're looking at the same place on the earth.

If this effect were seen only in observations taken in a series of successive days, I would agree. But since a resurvey of the same area seven months later showed the same effect, I still cannot understand it.

Let me tell you a little bit about the statistics of these events. In the case of the many weak events thing shown in figure 11, there were 92 detections exceeding 5 sigma, with the strongest one being 19 sigma. For the thing just shown (the southern most region, figure 12), where there are fewer but stronger events, there were 41 detections exceeding 5 sigma, 11 exceeding 20 sigma, and the highest was 30 sigma.

Olsen (?): Bob, how long do you leave the instrument at the same declination?

Several days. Two or three.

Olsen (?): Because the sky doesn't move that far in two or three days.

Thats true.

Olsen (?): It might be a good idea to interweave more. That would separate your observations over particular declinations on the sky by a week or two weeks.

Well, we did reobserve twice seven months apart - and we saw the same effect both times. It is true, as you say, that the antenna was at the same position both times.

I really would like suggestions and ideas because I am in no way claiming that we have discovered something new in the universe here. I'm just saying that as a result of observing for a long time in a sort of uniform way, we have discovered something that we didn't expect to find.

We also looked in the area of M31 and there was nothing at all unusual to talk about in that area.

I would now like to discuss a few quasi-philosophical subjects. One - if we have learned anything in doing all of this, it is that a strategy which is concerned with surveying the entire sky in some uniform way may not succeed, because there may very well be phenomena that you discover that won't be there again when you go back to look at them at a later time. A better strategy which we hope to adopt very soon when we put our tracking system in operation is that as soon as we see any kind of event at all, we put the telescope into the tracking mode and try to track that particular 'thing' and find out what it is - whether it is instrumental or receiver problems or something in the sky. I think the trade-off is instead of spending more time looking for new objects that might be there at a very low probability, your time is better spent tracking an object for some period of time that you know is there by your own detection criteria. In this way you can find out more about it and what caused it, and either eliminate it from your detection strategy or do something else about it.

We have now mounted the feed horns of our telescope on a movable cart, which can move back and forth parallel to the flat reflector. It will be driven by winches which enable us to track for about an hour. It turns out that you need to move about plus or minus 50 feet to track for plus or minus a half hour. This corroborates the well known time-space relationship that one hectofoot is equivalent to one hour.

An entire new receiver and computer system is being put into place. Essentially all of the receiver components are going to be updated with much more modern equipment. We have a number of DEC PDP11 computers which will be used to replace the very ancient IBM computer that is now in use. We plan to expand the frequency coverage to cover the entire water hole, although with less sensitivity - the philosophy being that rather than trying to guess what the frequency is very precisely we would like to look over a much wider frequency range in hopes of finding whatever strange things might be there.

In the Fall, we are going to do some observations of Halley's comet, in the OH line, which will probably disrupt our study work a little bit, but we hope that we can sandwich the two in together.

Lastly, I would like to make a suggestion for future approaches to SETI. We need to remind ourselves that everything that we are doing today is technologically limited really to observing a tiny portion of the sky in a tiny portion of the spectrum and for a tiny portion of the time. When the average person asks, 'Do you see airplanes or spacecraft?', I have to answer, 'No, because they're probably not in the beam, or probably were not at our frequency, or we weren't looking there at that time.' It makes you really realize that - yes, we are searching with extremely sensitive equipment, but only in a very tiny place in the hay stack. We are technologically forced into that. I think it is important that we constantly remind ourselves of that.

I think that we should begin thinking about other ways of doing this - such as ways of continuously monitoring the entire sky at all times and ideally at all frequencies, which is probably not going to be possible for a long time, but at least over as wide a frequency range as possible.

In many ways, Earth is blind to what could be very strong events occurring outside of the earth all of the time - not just for SETI purposes, but natural events that could occur out there. We simply won't know they're there because nobody is looking with the right kind of equipment at the right time. One way in which one might do that in the radio portion of the spectrum would be to construct a phased array, lying more or less flat on the ground, of many elements - as many as you can afford - with each one having a very broad frequency response, and form all possible beams from all those elements simultaneously in the sky. Essentially create a radio picture of the entire sky all of the time. I'm not sure that we're quite ready to deal with this yet in terms of data processing capabilities. Digitally, we could probably do that, but we might be forced to do something like either scan in frequency or scan a single or a few beams across the sky. But that's a technological problem really, so that as our technology improves we will be able to approach closer and closer to that goal. Perhaps we need to begin thinking in these particular ways. I think this would have great advantages beyond just SETI, because we might see natural astronomical phenomena, we would have an easy way of monitoring the passage of aircraft, spacecraft, or meteorological phenomena, and many things that we have no really good way of recording right now.

Food for thought. Thank you very much.

J. Tarter: Let me go back to what I said before, because it's completely wrong. It's not coming in the side lobes. These events are coming in your main beams because you were looking at results from the strategy that matched the beam pattern.

No, these particular events are ones which are coming in single integration periods - the so called search strategy one, which is one big point. These are not ones in the so called search strategy nine, which actually matches the antenna beam. These tests are all going on simultaneously.

S. von Hoerner: Does that mean that you would also pick up a single short pulse?

Yes, it does. Thats exactly what it means.

S. von Hoerner: If it is not in both parts - the plus and the minus - its only in one part?

Yes. But that is a point. One of the main reasons for doing the beam switching is so that the interference coming in over the horizon which gets into the side lobes of the horns gets cancelled out. That never exactly happens in practice, but it does reduce the interference by a whole lot. Still though, if there were a strong pulse that came in over the horizon, it could cause this kind of event because the cancellation is not perfect.

T. Kuiper: What if you had a strong pulse event detector and every time you saw a little tiny thing you'd look to see if it was just in one horn and not the other. And maybe any difference corresponded to that big thick spot that you got at the same time.

Ideally, I guess the way is not to do the switching automatically like we do now but to do it under computer control. Then the computer can really tell the difference and independently process the signals from the two horns.

W. Sullivan: In the spirit of your last remarks, I urge you to monitor the continuum - the broad band output - because you're the only place doing it continually and there could be all kinds of interesting phenomena. I'm disappointed that it hasn't happened before. I know its amazing what you do with your limited resources and so forth. But in your upgrading thats going to be happening here... I urge you to do that.

S. Gulkis: This is really the same suggestion made already. I was thinking of it at the same time. Its very difficult to look at these numbers that you show us - the 1's and 2's - and decipher them from this position. I would like to see you bin all the number 1's, all the number 2's, and all the number 3's and compare that with theoretical distribution so that we can see how well it follows. See whether maybe the 1's, 2's, and 3's follow what you predict, or whether or not they are scattering. And then you can further subdivide the data into two sets - in the galactic plane and out of the galactic plane - and see how well that does. And I think that would make it much easier for us to see the significance of the results.

I agree that that's an important thing to do. One difficulty is that most of the data that I showed here is not recorded in any computer readable form. The only thing thats really saved in computer readable fcrm are those particular detections that I showed. Those massive data displays exist only as a computer printout.

W. Sullivan: But we know that anything beyond 5 or 6 sigma is not going to be expected, even with ten years of data, because remember its only got 50 channels, and dumping every 30 seconds or whatever.

S. Gulkis: We did this with the data that we collected at Tidbinbilla, and we followed it out to about 6 sigma and it seemed to be right on the noise prediction.

W. Sullivan: I'm not disagreeing that its not a good thing to do, but what I'm saying is that once you get beyond e to the minus 6 or 7 squared, I just know that its not going to be expected at all in the random distribution.

T. Kuiper: Just a comment in case one of your remarks was misinterpreted. Its not beyond our ability to form all the beams simultaneously, because that doesn't require computation. There are people in this room who remember what a Butler matrix is. You can form all the beams; what you do with the data after it comes out is your computational problem.

I think the processing is something that would have to be distributed where you have either some sort of analog processing, as you suggest, or some sort of digital processor at each element to begin with and then perhaps further digital processing down the line. You're right, its a three dimensional, very difficult problem to do that.

E. Olsen: But, you can look at it as a question of scale. You can just do less beams and less frequencies and it becomes a problem of the same scale as the 10 million channel spectrometer.

Yes, that's right. What I would advocate is that one build such a system that starts relatively small and then as technology or money becomes available make it bigger with wider frequency coverage and more beams in the sky.

Small SETI Systems

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Introduction

This paper describes several small SETI systems - a small private radio telescope, and novel signal detection techniques that can be implemented on small computers for analysis of archival SETI data. The main merit of small systems is that they are cheap enough to be dedicated to one task or person; in some cases they offer a solution to the problems of limited observing time and budgets that constrain SETI research projects. Small radio telescopes can be dedicated to observing the locales of possible intermittent signals, and small computers can be dedicated to very intensive analysis of archival data.

I have constructed a small SETI radio telescope, one objective of which is to monitor promising locales like the position of the OSU "WOW" emission (Kraus, 1979). I am also developing pattern recognition techniques for signal detection and these techniques have successfully revealed previously unknown structure in archival SETI data. These "small systems" are intended to complement existing and planned searches.

Small systems may come to play a significant role in SETI. If surveys turn up a number of candidate but intermittent ETI sources, small radio telescopes may be needed to monitor the positions for additional evidence (freeing large instruments for other work). If searches find no evidence of ETI beacons we may eventually need to monitor the entire sky continuously to detect beacons that are intermittent. Multiple small radio telescopes provide one way to do this. Finally, we may need to develop a very broad range of signal detection techniques in order to recognize ETI signals that may be in the growing archives. Small super-microcomputers may provide a cost effective tool for doing the intensive data analysis that can not be done in real time.

A Small SETI Radio Telescope

Several years ago I designed and built a 21 cm small SETI radio telescope; it has been operational for about two years (Gray, 1985). The radio telescope consists of a 12 foot reflector on a steerable az-el mount, a 65 degree Kelvin LNA (for a system noise temperature of about 125K), a digitally tunable 1.42 GHz receiver, and a 256 channel digital spectrum analyzer (an HP3582). The system could be duplicated for approximately $25,000. Data acquisition and recording functions are automated, using a high performance CompuPro microcomputer. The system can not presently track sources, but its 3 degree HPBW yields 20-40 min. of data for typical sources in meridian transit mode. A system block diagram is at Exhibit 1.

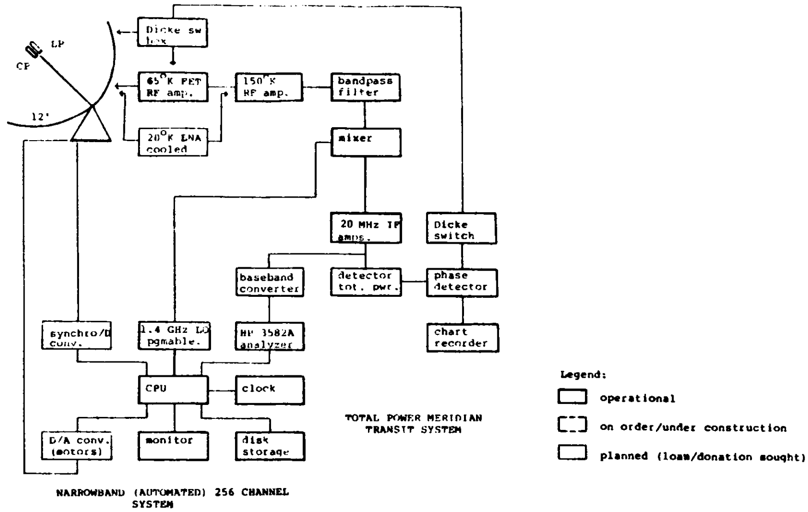

Exhibit 1. Small SETI Radio Telescope block diagram.

System sensitivity (with a 1 Hz channel bandwidth and 16 1 sec. averages) is about 10 to the -22 Watts per square meter. This is about 1/100 of that obtained in Tarter's high sensitivity search, at a signal to noise ratio of 12 for both systems (Tarter, 1980). Disadvantages of the small system include narrow spectral coverage (0.25 to 25 kHz), and a slow data acquisition rate.

Over 1000 hours of observations have been made at declination -27, between 16 and 23 hours of right ascension, which includes both the WOW locale (about 100 hours of data) and a segment of the galactic plane. No candidate ETI signals have been detected, under the strict criteria that signals must (1) match the known antenna pattern and (2) display an expected Doppler drift in frequency. A number of strong single-observation signals have been recorded, shown in Exhibit 2. These are probably RFI.

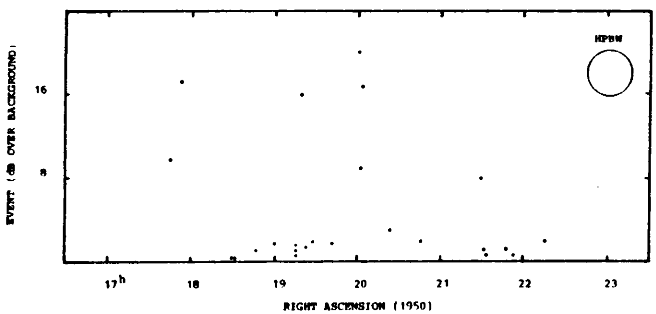

Exhibit 2. Strong single-observation events at declination -27.

Oliver: What constitutes an event in your system?

A single channel with remarkably high power. These data are 2 minute observations, each formed by averaging 256 successive spectra. The bandwidth of each channel was 40 Hz.

Small Data Analysis Systems

Small data analysis systems - consisting of a dedicated computer, specialized signal detection software, and a motivated researcher - offer a way to reduce the probability of missing a real ETI signal.

Methods for analyzing SETI data are not as advanced as the radio telescopes that collect the data; SETI is a generation younger than radio astronomy. This suggests that we should archive (at least a subset of) every observation for re-analysis with more powerful future methods. While it would be difficult to archive ten-million-channel observations, it would be easy and inexpensive to record a useful representation (i.e. the 100 or 1000 peak channels) of each observation.

I am currently investigating signal detection methods which use pattern recognition techniques as well as other approaches described in Project Oasis (Lord, 1981). I have analyzed data from the Ohio State University SETI survey and have found features which are quite interesting, and which illustrate the value of pattern recognition techniques and the intensive re-analysis of archival data.

WOW Spectral Features

OSU recorded a very strong narrowband signal (less than 10 kHz) at 1420.387 MHz on August 15, 1977. The signal has been nicknamed the "WOW" because it had characteristics of a candidate ETI transmission, but it was never seen again. My analysis of OSU's data has revealed three relatively weak spectral features apparently associated with the strong WOW signal. The strong signal was in channel 2 (at 30 sigma) and displayed the bell-shaped curve characteristic of a source drifting through one of the two antenna beams. The weak features were in channels 4, 7, and 16 (at 4, 6 and 7 sigma respectively). They seem associated with the strong signal because all three have approximately equal intensity (8 sigma) when corrected to the apparent origin of the strong signal. Exhibit 3a highlights these features, by showing only responses over 4 sigma.

These four features together suggest an orderly and unusual spectral pattern. I have found that the spacing of the first 6 Lyman UV atomic spectral lines provides a good model for the frequency spacing of the radio features (r=0.99). The Lyman model could also account for the difference in magnitude between channel 2 and the other three channels: three of the close-spaced Lyman lines fall unresolved into channel 2, which had 3 to 4 times greater power than the other features. Exhibit 3b illustrates this.

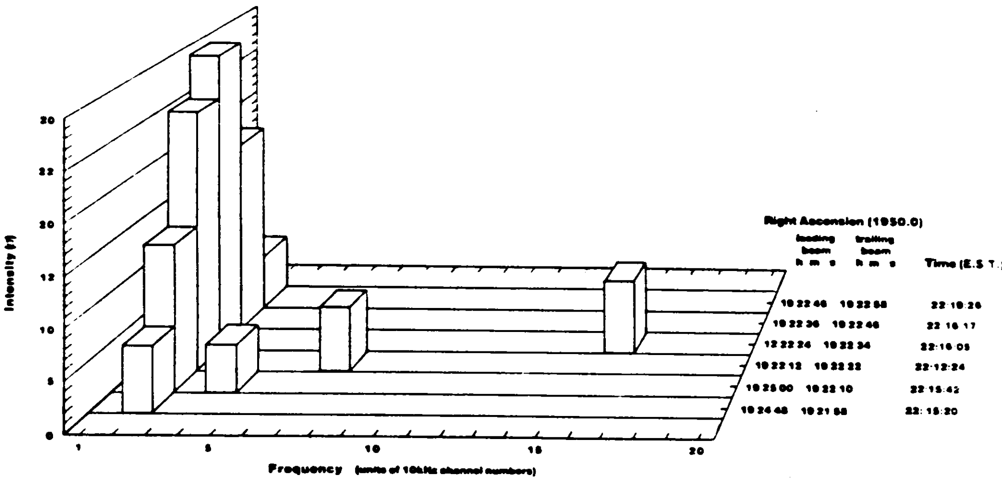

Exhibit 3a. The "WOW" signal and three other spectral features.

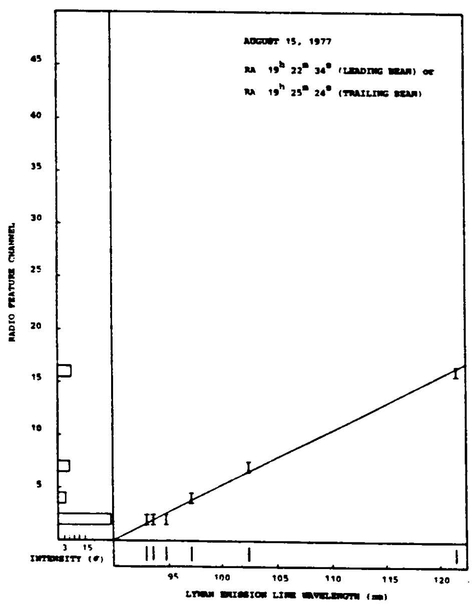

Exhibit 3b. Correlation of WOW features and Lyman spectral intervals.

The significance of this Lyman-like pattern is unclear but it suggests searching for the same pattern in other data taken at the same locale, since repeated detections at the same position could confirm a celestial source. The model spectral signature provides a matched filter for examining other data, which is a powerful tool for detecting weak signals.

I have examined OSU data taken near the WOW locale on 10 other days (about 30 min./day) and have found weak evidence of the Lyman-like spectral pattern at the same position on April 26, 1978. The pattern appeared twice, and is shown in exhibit 4. But these very weak signals appear to have come from the same feed, which suggests terrestrial RFI. The original WOW signal showed strong evidence of celestial origin (a good match to the antenna pattern), so this possible second detection of the spectral pattern - with the appearance of RFI - is not sufficient to invalidate the original WOW observations, but neither is it evidence of a constant celestial position. Only 10 days of the 172 on which observations were made in the locale have been examined in this search for the spectral signature.

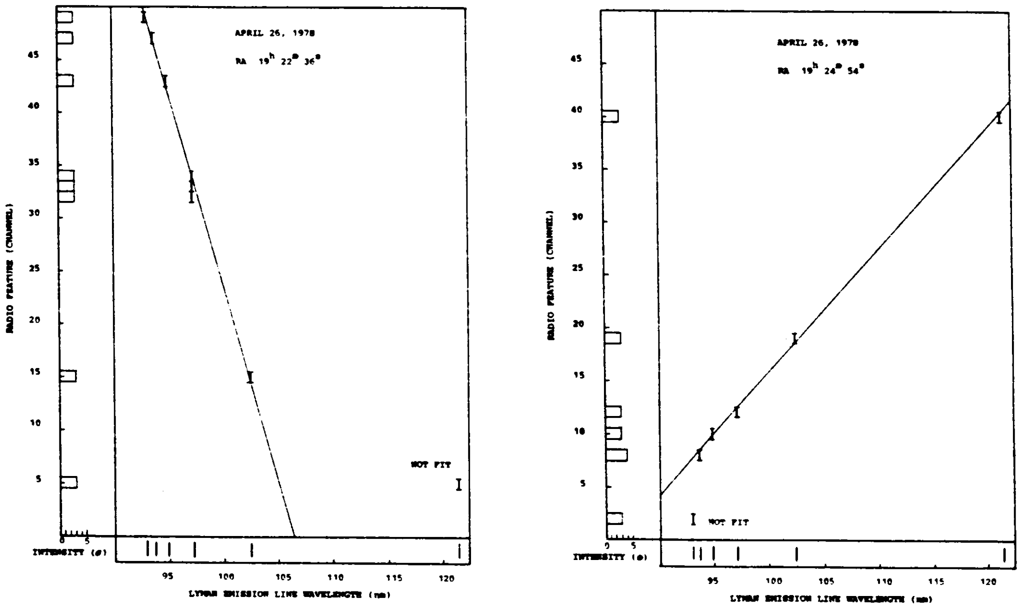

Exhibit 4. Correlation of channels over 2 sigma with Lyman intervals.

Voice: What are the two axes of the Lyman graph?

The horizontal axis is the model Lyman emission wavelength in nm., and the vertical axis is the frequency of observed radio features in channel numbers. The positions of Lyman lines and radio features are illustrated along the respective axes.

Oliver: What inference do you draw from this?

That we may have re-observed the same mechanism as the WOW signal. If we were to repeatedly observe this pattern at the same celestial coordinates -

confirming a celestial origin - it would be very interesting. Observing it at other coordinates would demonstrate it to be of local origin. Not all of the data has been reviewed yet.

Cullers: Where did you get the idea of Lyman spectral intervals?

I tried fitting a number of well-known patterns to the clearly ordered radio features in the WOW data. The Lyman series fit very well. I hope to develop fully automated procedures to test for many potentially interesting patterns in both the frequency and time domains.

Cullers: How does that help you decide anything?

Finding evidence of the same pattern at the same celestial position twice would make a good case for re-observing that locale extensively, to detect a possibly intermittent signal. In the case of the OSU data, the fact that the pattern resembles the Lyman spectrum just makes it more interesting.

What If Beacons Don't Illuminate Us Continuously?

If we were to design an interstellar beacon today it might very well use the phased array technology currently employed in some radars. The electrically steerable beams of phased arrays would be attractive for illuminating a large number of targets efficiently (although sequentially), and the modular transmitting elements seem attractive for radiating very large amounts (possibly thousands of megawatts) of RF power. A consequence of this transmission approach would be rare illumination of each target star - about once every 12 days for a 1 second signal directed at each of 1 million targets repeatedly.

Detecting this type of periodically recurring signal would probably require continuous monitoring of the entire sky. Systematic methods for doing this include numerous small radio telescopes pointed in all directions, and computing all possible beams of a receiver array. Even weak signals could be detected with a time series analysis of archival data for each direction to identify any regular increase in power. Many small, cheap systems - receivers and data processors - would be needed, plus the storage capacity for months or years of all-sky data.

To detect an intermittent signal with our current search strategies (which observe only one target at a time) we would have to be lucky, and the signal would have to be either very strong or very convincing, to attract continuous monitoring of the source locale after a one-time observation. What would it take to get one or more large radio observatories to abandon their programs and track the source of a one-time event for weeks, months, or years?

I think it would require a very high degree of structure in the signal. Spectral signatures are one kind of structure that multichannel spectrum analyzers can allow us to detect. For example, neither the receiver MCSA resolution, nor its total bandwith is known to a broadcaster, but some classes of spectral pattern would be recognizable over a large range of MCSA resolutions and receiver bandwidths (e.g. the geometric series 2, 4, 8, 16, 32...). This kind of spectral pattern could be produced by one signal stepping through a sequence of frequencies.

Conclusions

Interstellar broadcasts could be unexpectedly complex, and they might not be continuous. Pattern recognition techniques offer one kind of test for spectral patterns which might be used to indicate intelligent origin of a signal. One such pattern has been found in data from OSU, and the Lyman-like spectral pattern associated with the OSU "WOW" signal seems sufficiently unusual to merit additional attempts at observation. Small radio telescopes provide one approach to monitoring locales of candidate ETI signals for extended periods, which may be necessary to detect intermittent signals.

Verschuur: I think this is a very interesting idea. Instead of sending prime numbers or some other things, you could send patterns that refer to the Hydrogen atom spectrum; use the 21 cm. Hydrogen wavelength to send information about other Hydrogen lines, as a flag.

Drake: It's too specific to call it the Lyman spectrum, which is given by the mathematical form:

$$1 - (1/n)^2$$

What's really being recognized is that form, which is perhaps no more elegant or attention grabbing than prime numbers. It doesn't say Lyman, it only says "one minus one over n squared".

Frisch: May I protest as a Physics professor. It says Lyman. Bernie, what do you think - does it say anything but Lyman?

Burke: I'm staying out of the line of fire.

Tarter: The last two correlations you showed (for April 26 data) - were they both within the same HPBW of the system?

The data are from the OSU radio observatory, where two beams spaced about 2.75 minutes of RA apart successively sweep over a source. The April 26 responses were separated by just about the right amount to be a single celestial source appearing successively in each beam. But they seem to have occurred in the same beam, which suggests that they were terrestrial.

Tarter: So what you have done is set up a spectral model, and then done a cross correlation between that pattern and data that otherwise looks like noise, to bring out the existence of the WOW signal signature in other places.

Oliver: Without worrying about whether this signifies the Lyman spectrum or not - if you were an ETI designing a signal to be detected, why would you do this? Why complicate a signal to put that information onto it?

OSU observed a simple signal in 1977 - a strong, apparently celestial narrowband emission very near the 21 cm Hydrogen line - and the apparent origin got 172 days of re-observation, at about 2 minutes per day, for about 3 hours of study. I noticed this spectral structure in the same data, and re-observed the locale for approximately 100 hours (at a much lower sensitivity).

What would it take to get us to dedicate instruments to tracking a candidate ETI source when we had seen only one interesting event? I think it would take a signal with a lot of structure. Spectral structure is one way for a broadcaster to increase the probability that a recipient will search intensively for an intermittent signal after a one-time detection.

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Acknowledgments

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V.

A GLIMPSE OF THE FUTURE

THE NATURE OF TECHNOLOGICAL CIVILIZATIONS

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University of Virginia

I'm not quite sure what I'm doing talking to this audience. My role in the past few years has been to take opposite side of the argument in a series of debates with Frank Drake. In any case, I have argued that there is very little possibility that any extraterrestrial technological civilization other than ours exists. Still no matter how much I argued, I'm always left with the nagging question: "On the other hand, what if we are not alone in the universe?" I don't think anyone could ever rule out this possibility and what I will try to do today is present a few ideas which might provoke thought on different ways of thinking about hunting for extraterrestrials. Although these ideas are part of the argument which is frequently used to argue that extraterrestrials don't exist, such lines of reason can never be iron clad. Still let's see where we can learn from them. Fundamentally, the point that I would like to make is that all of our thinking about searching for extraterrestrials is based on the experience of a couple of centuries or so of technological existence in our society. Almost everyone agrees that searching for extraterrestrial intelligence is a fruitful enterprise only if technological civilizations typically last for at least a million years. We're basing our search techniques on experience which is on the order of 2×10^{-4} of the lifetime of the civilization that we're hunting for. What I'd like to do is to try to think about much longer term futures than ordinarily considered.

First, I would like to make the point which has been made mostly more strongly by Gerard O'Neill. I don't hope to convince you in two minutes of O'Neill's point of view that space is the natural place for technological civilization. (I heard O'Neill talk at the AAS meeting in Austin, Texas about ten years ago. I came away from that talk thinking he was completely crazy. Still, after returning to Charlottesville, several of us decided that maybe we should teach a course on space colonies. Even if it was a crazy idea, we could get more FTE's in our department if we taught about space technology. But after teaching the course, I ended up becoming a convert.) Let me point out a few order of magnitude arguments which show that space may be a more desirable place to live than a planet. Planets are good for evolving life. But planets, at least once we begin to think about what a technological society is like, are a dreadful place for a technological society. They are hospitable to life but bad for technology. What is technology? Really, technology amounts to using energy to manipulate matter into structures of various kinds. A planetary surface is a hostile environment, both in building and maintenance of structures. If you life on a planet, you're very limited in the amount of energy that you can use as a society. The bulk of the matter of a structure serves no utilitarian purpose other than holding the structure up. The atmosphere is corrosive. I see my car is rotting away year after year. It's a hostile environment. There are earthquakes, there are winds, there are thunderstorms which cause all kinds of problems. In space these problems are much less the case and O'Neill makes that point. I'm relatively convinced that at least some fairly large fraction of technological societies will become space dwelling societies.

O'Neill argues that colonization is possible, that it's not terribly expensive, and so on. I won't repeat that. I only wish to make an intuitive argument which requires that we consider things in appropriate units. (I started thinking about this about a month ago when I heard Eric Jones give a talk and he was talking about terawatts. What is a terawatt? While at a certain level I know what a terawatt is, I have no intuitive feel. I don't think anyone has an intuitive feel for anything that is bigger than megasomething or smaller than microsomething. If we want to have any kind of intuitive feel for what we are talking about, we have to find an appropriate set of units.) This morning, somebody wanted to know how much energy falls on the earth from the sun. I suggest this is an appropriate unit for the power usage of a technological civilization and (following a suggestion from Jim Trefil) define a new unit the Total Wattage Intercepted - Terrestrial which we will abbreviate as a twit. (A twit is 1.7×10^{17} watt.) It is quite instructive to use twits when discussing the energy use of a society. For instance, the total power consumption on the earth amounts to about two millitwits. You can argue that we are very near the limit of our use of energy on the earth because if we use energy, no matter what its source, at a rate of a few tens of millitwits we are going to probably screw up the climate of the earth. On a much smaller scale, a Boeing 747, depending on exactly how you figure out its power consumption, is of the order of a nanotwit at maximum thrust and a few tenths of a nanotwit at cruise.

In considering SETI we must extrapolate at least thousands of years into the future. To put some limits on the kinds of things we should be thinking about, let's go back a thousand years. The energy use in England in 1066 is in fact documented in the Doom's Day Book, in which all the waterwheels in England are cataloged. They were the dominant source of manipulable energy. Since there were about 6000 2 horsepower waterwheels, the total power was about 50 picotwits. An eleventh century Britain hearing a description of a Boeing 747 using five or six times the total energy resources of the entire country would consider it ridiculous. We must realize that energy use is likely to increase dramatically if we can get off the earth. If we move as O'Neill and others argue into space, we can ultimately manipulate energy on the order of several tens of millions twits. (Using the same fraction of $L_{\odot}$ that we now use of energy on the earth.) This is one of the reasons that I think we are likely to go into space. If we cut our growth short at our current level and we sit on the earth, we are limiting ourselves at a factor of at least a million short of our ultimate ability of growth. I don't think our civilization will have the discipline to cut its growth at that level.

There's lots of energy in space, that's why we go there. The thing that space seems to be lacking is matter. But we have a very warped view of how much matter we need to live and I want to point out again an appropriate way of thinking about mass. If we follow O'Neill's argument, it requires about 10 tons per person. So I will define a new unit called the PPOM (per person O'Neill mass) which is the amount of mass required to live in a space colony. We waste mass living on the surface of the earth. You are sitting on enough matter to make space colonies for roughly about a million people. And that matter on the earth serves no useful purpose other than keeping you from falling into the center of the earth. Planetary civilizations are a terrible waste of matter. There is matter in space; the most desirable stuff is probably the asteroids because they are most easily disassembled. One iron asteroid 10 km in diameter has enough matter to build space colonies to house 3×10^{11} people. It is worth \$5 x 10^{13} (if the price of iron doesn't go to pot). The biggest unit of dollars I can think in is the increase in the debt under Reagan's administration, call that RRD. An asteroid is about 50 RRD. As another example, one 10 km stony-iron asteroid contains enough aluminum to build a reflector ½ mm thick as big as the earth. With one asteroid a civilization could build enough reflectors to manipulate one twit. So it takes a very small amount of matter for a society that lives in space to manipulate energy at far greater rates than planetary dwellers.

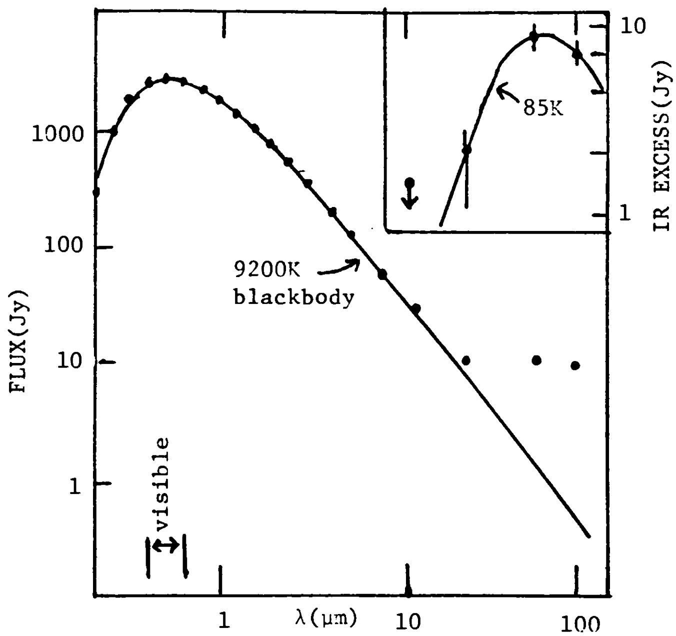
We'll go into space and I think other civilizations will go into space. Civilizations living in space will take a very different view to interstellar travel. This leads to a problem which many people have referred to as the "where are they?" problem. If we or another civilization start non-relativistic interstellar travel it takes something like 10^7 years to colonize the galaxy. To understand at gut level what this means, let's play the game that everybody teaching introductory astronomy plays, where we construct a cosmic calendar. The Big Bang happened on January 1st and now is midnight on New Year's Eve. On December 31, sometime in the afternoon dolphins go back into water, Neanderthal appears 3 minutes before midnight, and so on. Why stop at midnight? Before you pour your first glass of champagne, we'll have space colonies and we will manipulate space several twits worth of energy. And if we embark on interstellar colonization, we have colonized every star in the galaxy before the Rose Bowl games starts on New Years Day. Let's go back to when the earth was formed: about September 1. If the optimistic view about the rate of formation of extraterrestrial civilizations is true, on September 1 the earth and 40 million other planets formed. Likewise, August 31, August 30, August 29, and so on. Any of of those that formed on August 29 could have led to a civilization which colonized the solar system sometime when the Alps were forming. The easiest explanation is that we are the first civilization in the galaxy.

<u>*Voice:*</u> *First colony matured.*

Or we're the first that matured or whatever.

However, there are many reasons as to why they are not here other than the fact that they don't exist. My favorite reason is Frank Drake's suggestion which several years ago he was calling the Proxmire scenario. Basically, Frank argued that in any civilization there will always be some politicians who will argue that very large amounts of money should be spent on something more practical than going to another star. To accelerate 1 kg to 0.1 c requires 10^8 kilowatt hours. That's a lot of energy; that's 10^7 dollars. Who is going to send 10^7 dollars accelerating 1 kilogram so it can whiz through some other stellar system? But if you look at it from another viewpoint, interstellar travel starting from a <u>mature</u> planetary civilization which is manipulating possibly thousands or millions of twits of energy is not so ridiculous. To accelerate a space colony for 10^4 people to 0.1c requires only about a hundredth a twit year. So it's really a very small amount of energy compared to the amount of energy that a planetary wide civilization could use.

While I'm inclined to think that the interstellar colonization will take place, the fact remains that we haven't been colonized. If there are other civilizations they probably could, if the were willing to spend the money, embark on interstellar colonization. For whatever reason they have not colonized our system. But they still may be out there and we should think about what really large civilizations might look like. Years ago Dyson suggested that an advanced civilization might use all of its star's energy output which would ultimately be dumped as waste IR. I suspect that a civilization will never reach this point of energy saturation. Just as we are embarking on space colonization when our energy use is less than a millitwit, an advanced civilization would probably use only a small part of their star's output. Still one sign of an advanced civilization would be a star with an infrared excess as in this figure where there is an IR excess amounting to 10^{-5} L_*.



Many such stars have been detected by IRAS and the spectrum shown is that of Vega. If the IR excess is due to space colonies, the first question that comes up is why it is so cold, since the IR radiation temperature is 85 K. The answer to that is contained in the first five minutes of The Graduate where someone says that the future can be described with one word, "PLASTIC." We're evolving already to a state where our main building materials are plastic. Where in the solar system do we find the most material out of which to make plastics? Where it is 85 K -- Jupiter, Saturn. So these guys are living in plastic space colonies out where they find most of that kind of material. How many there are? If the energy use of each Vegan is expressed in units of our personal energy use (1 PPEC $\sim$ 10 kw) then the population is

$$\text{Population} = 3 \times 10^{19}/(\text{E use per Vegan/PPEC})$$

The total mount of mass in the colonies

$$M_{colonies} \sim \frac{0.05M_{\oplus}\ (\text{mass per Vegan/PPOM})}{(\text{E use per Vegan/PPEC})}$$

So the population depends on what you wish to assume about their average energy consumption but is probably huge by planetary standards. Still the mass required is modest. While it is natural to interpret infrared excess as dust, we must remember that space colonies also have a very large surface area to mass ratio.

They may be doing other things. For instance, they almost certainly are beaming energy from one place to another, probably in the form of microwaves, the frequency of which is chosen for engineering reasons. Because they don't want to waste the energy it's going to be as high a frequency as possible to achieve a narrow beam. Just as an example, suppose they have a one twit beam that they are beaming some place through their stellar system and that it has a bandwidth of 100 megahertz at an opening angle 10^{-8} radians (which is very narrow but can be done). If one happened to be pointed toward the earth, it could produce the signal 10^{16} Jy in millimeter range probably. Civilizations, once they are space dwellers, probably couldn't care less about what's happening on the planetary surfaces. They probably deal with very much higher frequencies than we are normally thinking about.

Let me bring up a final point. Could they possibly be broadcasting SETI signals to us? I have much less faith in the good nature of extraterrestrials than most people do. I can't help but wonder what's in it for them. We have trouble mustering funds to listen. Can you imagine the response of the politicians today to an effort to get funds, the much larger funds, required to broadcast? To broadcast for centuries? The politicians always ask, "What's in it for us?" What's in it for the Vegans? I don't know. What kind of free radio do we get on Earth? Most is commercial, which won't apply to interstellar communication because there is no interstellar commerce. But you can have commercials for ideas. It seems to me if there are actually conscious signals directed at planetary surface dwellers, they probably fall into two classes: interstellar Jerry Falwells who are trying to sell us their religion or possibly interstellar propaganda where they are trying to sell us their politics. This brings up a paranoid side of my view of extraterrestrials. The Vegans, if they exist, are interstellar colonizers since they must have migrated from a longer lived star. Still they and all other civilizations, every one, for some reason or another have decided not to colonize the earth and probably other planets. They'd realize that any new civilization may not share that ideal. The newcomers may wish to colonize every star: to grow like cancer throughout the galaxy. Maybe they are trying to convince us that we shouldn't grow like a cancer completely throughout the galaxy.

I have tried to be provocative. I present these ideas not to denigrate the current SETI effort, but to stimulate a broader approach to the problem. In particular, I think that we should think about civilizations not living on planets, and not necessarily living near solar type stars. Such civilizations may have engineering projects which make them detectable without having to rely on their good will.

W. Mook: How much energy is required to move an asteroid from one orbit to the other? For example, what would take an asteroid and move it into orbit around the earth?

The energies are huge and that's why I think that civilization can go to where the asteroids are. Initially, as we grow away from the earth we will be attracted to the idea of capturing asteroids, but eventually we will move to where the material is.

W. Mook: So item number 2, what's the total mass required to get a self sufficient civilization. I mean is there an estimate on the total mass required? Like if it's everybody on earth that's such a large amount.

Most of the people living in space are like most of the people living here, they're born there. You take the genetic material, not the inhabitants.

W. Mcok: Right, but when Columbus came to the Americas there was, you know, the same ecosphere. I was trying to figure out what the total mass is and then relate that to the total energy requirement that would have to be input by the originating civilization. If this requirement is greater than that available to the planet dwellers for starters, then it won't get started.

C. Seeger: I have a suggestion. This evolves down to numbers somewhat and also to what I call technological religion and should be carried on outside. I would like to make one comment myself. There sometimes is the thought that civilization is defined by technology. Technology is not all of civilization but that seems to be all there is in the O'Neill scenario.

MANIFESTATION OF INTELLIGENCE
IMPLICATIONS FOR SETI

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Introduction

To remind you of the reasoning currently used in support of SETI, Figure 1 presents a Ven's diagram representing successively smaller sets of phenomena. Probabilities are assigned to each of the subsets until finally we

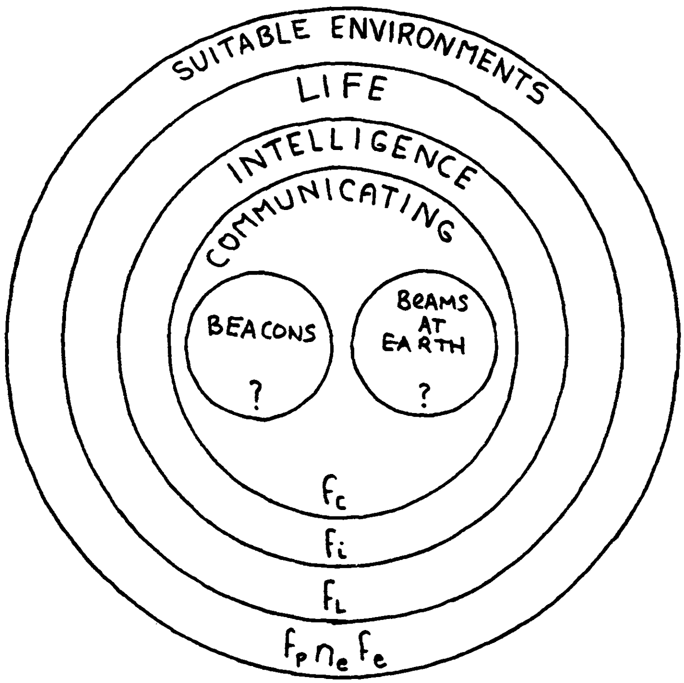

Fig. 1. Ven's diagram representing various factors inthe Drake (1961) Equation.

get down to something that we hope we can observe. In the middle of the diagram are the actual phenomena that are being looked for. I would challenge anybody to put credible probability factors on those, even with error bars. As we move from the outside to the center of the diagram, we move through the realms of physics, chemistry, and biology, until we enter areas completely outside our realm of competence: sociology and economics. Not only is it out of our competence to assign probabilities; it is also outside the competence of the sociologists and economists, because they have no credible theories with which to make predictions. This makes me uncomfortable as a physicist (astronomy is a branch of physics), and it makes me ask myself whether there is another approach we should be looking for.

We should recognize a distinction between search and exploration. When you specify such goals as interstellar beacons, you are saying that you know the characteristics of the phenomena that you are looking for, and so you go right for them. This is a search. Exploration is another approach. In general you have some idea of what would be significant discoveries, and you then look where you haven't looked before to see if you can find any of those things.

The Objective

Figure 2 is a diagram in the same sense as the previous one but it shows a different set of phenomena which I'll discuss. A problem that we encounter

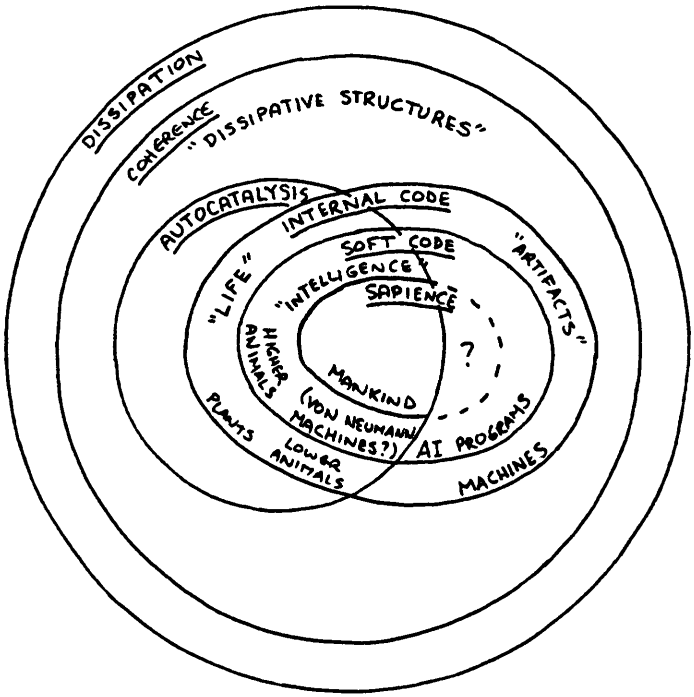

Fig. 2. Ven's diagram for 'life', 'intelligence', and 'sapience' as physical phenomena.

when we talk about 'life' and 'intelligence' and 'sapience' is that the definitions are very fuzzy. I was unable to find any consensus on acceptable definitions. In a recent article, Minsky (1985) wrote, "It's not one author's place to tell other people how to use a word that they already understand."

My first reaction was "Sure! Right on!", but he went on to say "Let's just use intelligence to mean what people usually mean: the ability to solve hard problems - like how to build spaceships and long distance communication systems."

Then I thought, "Hey! He isn't using 'intelligence' as I understand it. What about dogs, and chimpanzees, and dolphins?

So I realized that whatever it was that he was thinking about wasn't what I was thinking about.

In order to define the phenomena we are going to look for, we have to settle on some definitions as best we can. If those definitions prove inadequate at a future time, then we'll amend them.

The definition of "life" is taken from Ilya Prigogene and his colleagues (Prigogene et al. 1972; Prigogene 1978, 1980; Prigogene and Stengers 1984). Life has a number of characteristics. First, all life consists of dissipative structures, so called because they dissipate free energy in the environment. Such entities lower their internal entropy by creating greater entropy in the environment. You can think of them as "entropy refrigerators" which achieve their structure by "exporting" entropy. Life is autocatalytic; I prefer that term because, unlike "reproduction", it is not considered to be a characteristic of life alone. There are, for example, autocatalytic chemical reactions. The final and key characteristic is an internal code. For example, you can think of a convection cell as being autocatalytic, because if one cell of molecules starts moving up together it generates a disturbance which then causes other cells to form and start moving up. But the whole process is simply a working out of the laws of physics in the environment; there is nothing in a convection cell that tells the next convection cell how to shape itself. Life is different. We don't see Charlie Seegers arising spontaneously when the conditions are right. It only happens because there is a transmission of internal code from one entity to the next entity. That, to my way of thinking, is life -- without any discussion of the details of the chemistry of carbon and phosphorus, or biology "as we know it".

I'd characterize "intelligence" as being the same sort of thing but with a significant fraction of the code in software. This gives an entity the ability to modify itself. At the lower level of life, if you want a modification, you've got to destroy the earlier versions and make new ones. When you have an internal code, you have the ability to modify yourself in real time in response to changes in the environment, and you qualify as being intelligent to some degree. (These definitions are not meant to define sharp boundaries. For example, there is an issue about whether there is such a thing as "firmware", a question which has not yet been resolved by biologists.) An intelligent entity has the ability to perceive and act upon simple relationships. Conditions in the environment cause appropriate reactions on the part of the entity.

"Sapience" is a higher order of intelligence which is characterized by the ability to recognize complex relationships, ones with very many steps in the connection between one phenomenon and another. This leads to language and "nested" toolmaking. We have seen on public television many examples of animals using simple tools, such as twigs to extract ants from a tree, but what we don't see is animals making tools to make tools to make tools, and so on. This ability, a consequence of recognizing complex relationships, is the basis of our technological civilization. This leads to a very high degree of coherence in the products of our civilization: very pure materials, very regular structures, etc.

The characteristics of life may then be summarized by this definition: "autocatalytic dissipative structure based on an internal code." Each component of the definition implies observable phenomena. Autocatalysis implies growth according to the logistic curve to a saturation level which is dependent on the carrying capacity of the environment. Dissipation implies entropy increase in the environment and the release of free energy present in the environment. Structure implies coherence. An internal code implies evolution and adaptability. For SETI purposes, it should be borne in mind that, except for the internal code, all these characteristics are also manifested by non-living phenomena. Therefore, one must consider any manifestations in the context of our understanding of Nature.

Observable Characteristics

Life may be observed indirectly through its products, through the observation of artifacts. These are characterized either by excessive order or excessive disorder. Excessive order in the form of the coherence life has created, disorder in the form of high rate of dissipation, more than can be accounted for by normal physical processes.

Observing the internal code probably requires a specimen of the putative life form for observation or even dissection, and isn't very relevant in the context of SETI. Saturation may be a little more interesting from this point

$$\frac{dN}{dt} = BN(N_{CAP}-N) - DN$$

Fig. 3. Logistic equation and curve. The population grows with an initial time scale of the birthrate B minus the death rate D, and saturates at a level determined by the carrying capacity N_{cap}.

of view in that we can examine the distribution of some unexplained phenomenon and inquire whether it has appeared to fill its environment to the carrying capacity. However, this judgment is context dependent, in that you have to have some basis for deciding what the carrying capacity is, and what is an appropriate distribution in a saturated environment. (One of the things that you encounter in the "where are they" argument is the assumption that all stellar systems must be populated. This isn't clear, of course, because it makes implicit assumptions about the utilization of available resources.) So, for the purposes of detection, saturation may not be a very useful phenomenon, but it may be useful later on.

Dissipation and coherence, however, are interesting characteristics for the purposes of detection. Manifestation of dissipation that could be of interest for SETI include unusual energy release, very efficient dissipation, and energy storage. Are there instances of extreme discharge of energy which strain our ability to account for them by "natural" (i.e., non-life) processes? Can we find cases of extremely efficient dissipation? We don't know of any true blackbodies, other than black holes, but a very efficient civilization could function like a blackbody in extracting all the free energy in a given situation and emitting only waste energy with a blackbody distribution. If you could find a case of a charging system, one with more energy going in than coming out (other than a black hole), it should make one sit up and take notice.

Simple coherence is found through Nature (it has to be, otherwise the higher forms could never have arisen) so we have to consider its manifestation in detail. The domains in which we can look for coherence are those of time/frequency (the one which primarily concerns us here), of length/wavelength (morphology), and composition. The manifestations which could be interesting in the context of SETI include multiple coherences, excessive coherence (which exceeds that expected in a natural context), and improbable coherence (which occurs in the "wrong" context).

Electromagnetic Exploration

Now I want to focus on electromagnetically observable phenomena. This narrows the set to be considered. There is no question of finding an internal code. Any electromagnetic phenomenon that we observe is the result of dissipation in some form or other, so that is not a particularly useful characteristic. As I mentioned, autocatalysis is really only useful after some phenomenon has been found and we can consider its distribution. That leaves coherence and its "interesting" instances: multiple, excessive, improbable.

Spatial coherence is an excellent discriminant in the SETI context, and we see how it has been used in various searches. I think that it is the best discriminant that we have against RFI. For example, does the observed phenomenon come from a direction in the sky? That is, can we eliminate the possibility that the signal has leaked in through a sidelobe, or into our IF system? If we can establish that the direction is sidereal, we have a very powerful discriminant, but the phenomenon may not last long enough to do that. It might be due to a source in our solar system. We can eliminate fast moving sources as being too close to be interesting (e.g. airplanes and satellites) and geostationary sources as probably of human origin.

Figure 4 illustrates various forms of coherence in the time/frequency domain. Bursts and pulse trains represent clustering in time. Tones are clusters in frequency. Dual clustering is seen in dynamic spectra, which show patterns in both time and frequency. Any sort of order in the time/frequency domain is potentially interesting, although not necessarily, since natural phenomena also show these kinds of coherence (solar bursts, Jupiter decametric emission, etc.) Therefore, such phenomena have to be considered in the context of known physical processes. However, I think that we can make the general

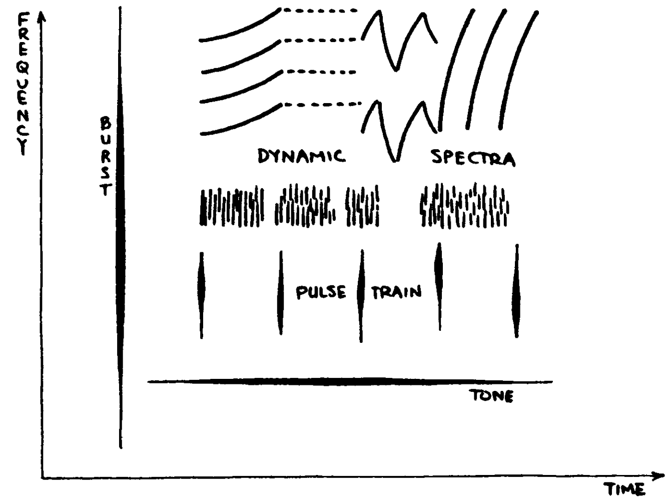

Fig. 4. Schematic representation of coherent signals in the time/frequency domain.

assertion that interesting phenomena in the electromagnetic domain would be any directionally coherent, improbable, excessively coherent, or multiple coherent signals. That is not to say that such a signal is necessarily evidence of life, technology, or civilization, but it is a phenomenon to think further about. The question to ask then is whether the phenomenon can be put into the context of Nature as we understand it. If the answer is negative, then the signal can be classified as potential evidence of extraterrestrial intelligence.

I have outlined, then, a general procedure for exploring the electromagnetic domain for phenomena of intelligence and civilization without getting bogged down in scenarios of the motivation and economics of alien civilizations.

A schematic illustration of the exploration space is shown in Fig. 5, which includes direction (represented by one dimension instead of two), frequency, and time. (Not shown here are the additional parameter dimensions of resolution in each of the dimensions shown, because they do not relate to search strategy but rather equipment capability.) The figure also shows the three general strategies by which the space can be surveyed. Covering direction and frequency is the essence of sky surveys. Covering time and direction are the all-sky monitoring programs. A third strategy would be to monitor a particular direction with wide frequency coverage for a long time. Figure 6 illustrates the kinds of searches that have been conducted to date, and how they relate to the general strategy classes. Sky surveys favor the detection of constant signals such as tones whose direction is unknown. However, sporadic phenomena are likely to be missed, and the strategy does not lend itself to building up useful statistics about sporadic events that may be intercepted.

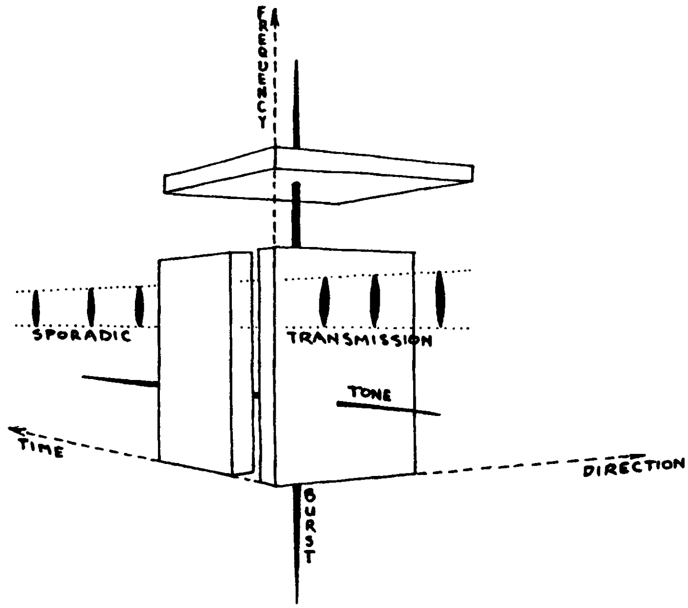

Fig. 5. Schematic representation of the electromagnetic search domain and three general search strategies: sky survey, all-sky monitoring, and source monitoring. Different types of signals are also illustrated to indicate how each strategy favors their detection.

The selected source survey strategy, which has been favored by most investigators, uses some rationale to reduce the number of directions and allocates more time to the chosen directions. The principal motivation has been to build up sensitivity for tones but, if the observing procedure is correctly chosen, it also enhances the probability of intercepting sporadic events. The probability of intercepting sporadic events depends on the duty cycle of the phenomenon and the chosen dwell time.

G. Verschuur: This is true of regular radio astronomy observations to date too.

Well, of course that's intentional because I'm approaching this as a radio astronomer saying, "What are phenomena in the universe that we know about and we don't know about."

J. Tarter: But there's also got to be an intensity threshold on the tone, because you can still miss it with the sky survey.

I couldn't show intensity.

J. Tarter: I know. It's nine-dimensional.

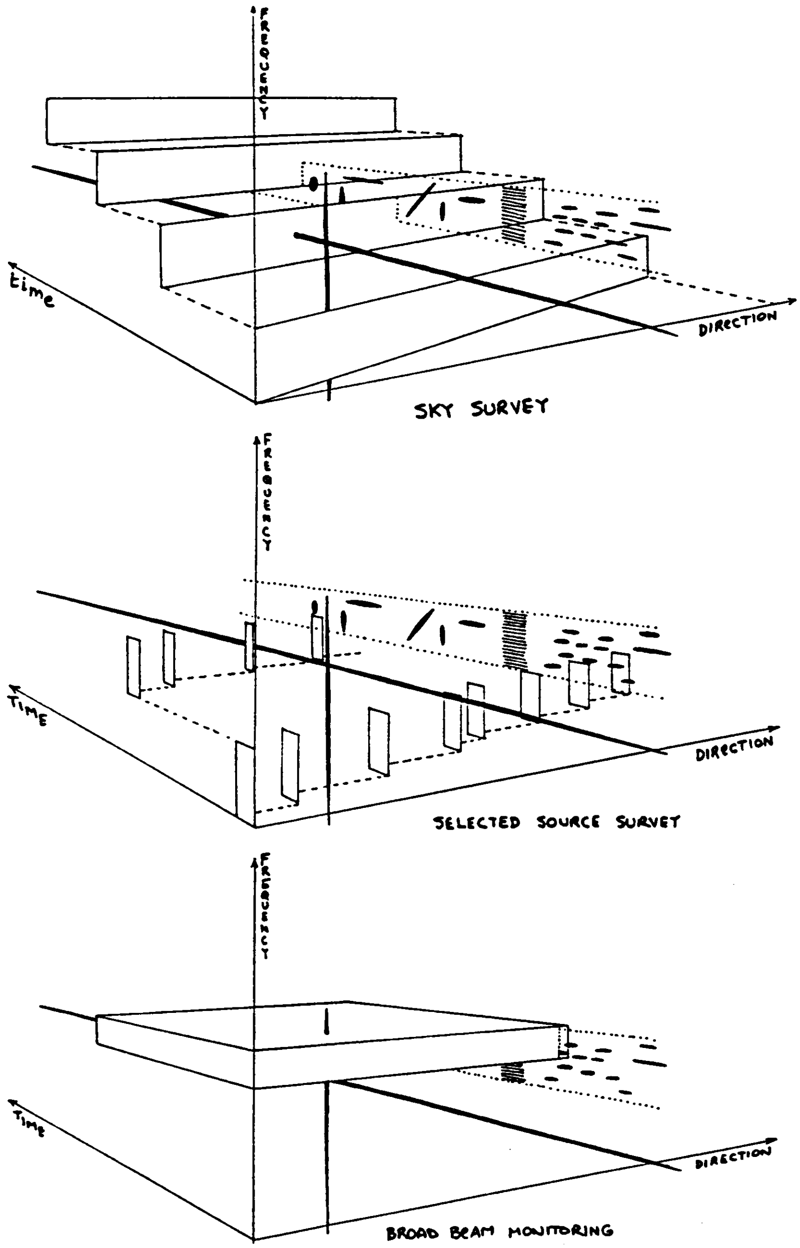

Fig. 6. Schematic representation of the kinds of searches conducted to date. The sky survey covers direction and frequency, but not time. The source survey improves the time coverage at the expense of direction coverage. Broad beam monitoring covers direction and time at a single frequency.

And the other thing I can't show is resolution, which is related to the same issue.

The all-sky monitoring strategy, which is ideal for detecting bursts, has only been carried out in Russia but, since collecting area and beam area are inversely related, this has been done at the expense of sensitivity. (There are, however, hardware-intensive ways of getting both wide angle coverage and large collecting area.)

Figure 7 addresses a gap we need to fill in our overall strategy. Primarily, we need to search for transient events in a more efficient way.

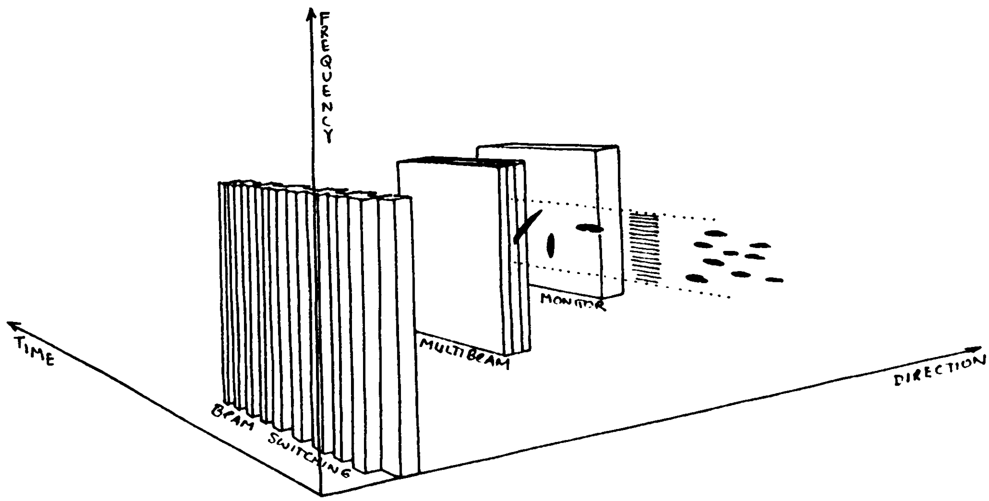

Fig. 7. Schematic of various exploration strategies to examine the time domain for sporadic phenomena. It is orthogonal to the other strategies in that it focuses on a few directions, instead of a few time samples or a few frequencies.

It is interesting that Bob Dixon (1985) came to this conclusion on the basis of looking at his own data. We need long-term, constant-direction monitoring. The ideal way of doing that is to have two telescopes that are well separated, tracking the same direction at the time, looking for coincidences. That has certain cost objections, so my next choice...

J. Tarter: But how many times do you have to see the coincidences? Would you believe it if you saw it once?

If the criterion is met that it is a unique direction in the sky, I would file it away as an event for later statistical study.

B. Oliver: But you don't need a whole SETI system at a second place. Why bother with that? Just tell them where it is.

But the phenomenon may be gone before you've picked up your telephone.

W. Sullivan: If it goes that quickly, it can't be accepted as a SETI thing!

Why not? The test that you are making is whether the signal is unique to that direction in the sky.

C. Seeger: That takes time, that test.

Well, it can take time or, if you are multibeaming, you can solve the problem right away.

B. Oliver: If it disappears very quickly, how do you know it is fixed in celestial coordinates?

Well, you don't. All you can do is file it for statistical investigation. It could have been a satellite going through your beam, or an airplane. But you've eliminated by far the largest number of interference-causing events. They are the ones that are in the sidelobes, that are coming in your IF system, and so forth.

K. Cullers: Your data may convince you as an individual of unique events, but it can't convince the outside world that way.

Look! I've got to explain something to you about your mindset, OK? I'm approaching this as an astronomer looking for phenomena that we haven't seen before. You're approaching it as somebody who says "I know what I'm looking for, and I've got to prove that that event is the real thing."

I'm happy with any set of phenomena from which I've reasonably eliminated interference, though perhaps not completely, and I'm willing to set those aside and say, "Here is a database that I'm going to work on further."

K. Cullers: I agree, but even a phenomenon isn't going to be accepted if it occurs in one data set once.

No, but what if you have a whole pile of phenomena so that, after a while, you build up statistical information about certain directions and certain frequencies?

W. Sullivan: You just call it a candidate the first time it happens. Then if you can make a statistical argument after finding many candidates, then you've got a statistical phenomenon.

Precisely!

J. Tarter: But the UFO people say the same thing.

Moot: But they don't have a database. They don't have a procedure either.

J. Tarter: They don't have repeatable individual events, but they say they have a statistical phenomenon.

C. Seeger: We're just arguing about databases. They've both got data-

bases! I don't see that it makes a whole lot of difference!

Well, at least I've achieved one goal, which is to stimulate a certain amount of discussion.

Let me get back to the points I was trying to make. If I can't have two telescopes simultaneously, then I would like to have multibeaming, which is to say, having a couple of receivers looking at adjacent positions in the sky so that I can do my discrimination that way. If I can't have that, I would settle for interferometry as the next best thing: two nearby telescopes doing an interference excising procedure. I don't consider that particularly satisfactory. Somewhere at the same level of satisfaction would be beam-switching, going back-and-forth between two horns. At the bottom of my list of preferences would be position switching, an approach to which we are largely forced at the present time for lack of better equipment.

At the moment we are covering a certain fraction of the observational space, and there is a significant fraction of the space that we haven't yet covered and need to cover. With apologies to a famous statesman I would say that, by the time that we finish the searches that we are now talking about, we'll not be at the end of SETI. We'll not even be at the beginning of the end of SETI. Indeed, we won't even be at the end of the beginning of SETI.

C. Seeger: Does anyone want to pick up that netting of the ball?

J. Tarter: I can give you a numerical summation of your last statement. If you draw a cosmic haystack the way we've done historically, which takes in many of the points that you've considered, consider the volume that's contained therein, consider what's been done to date, then the volume explored to date is 10^{-17} *of the cosmic haystack, defined as what you might have to explore if you were right about microwave signals in the first place. What we're planning to do with the ten-year program, five-year program, improves what's been done in the past by a factor of* 10^7*, and that's fantastic, except that it still leaves* 10^{10} *unexplored.*

Right! I think that there is a certain quality factor that we might try to develop, though, which is a little more than the numerical thing that you've mentioned, because it's clear that if you focus all your energy in one particular type of search, you'd leave very obvious and serious gaps. So there is some additional aspect that has to do with how you are covering parameter space with that 10^{-10} search coverage. At the moment I can't think of a good quantitative way of characterizing that, but it should be done so that we have some feel for the quality of what's been accomplished.

J. Tarter: Yes. This thing is done in terms of a nine-dimensional phase space. All cells or all axes are equally weighted, and it's real hard to do.

G. Verschuur: Has anybody explored to what extent the VLBI network could be used as a coincidence counter? It already does correlations between telescopes, but to what extent can you put limits on the detectability of other civilizations if they happen to be in the directions in which the VLBI has ever looked? I mean, what is the detectability of a pulse that comes in to five VLBI antennas?

Is there a VLBI person here who can tell us how long is the correlation time?

K. Kellermann: Not only is it long but, unfortunately, it is a one-bit system, so there is no information recorded at all about the strength of the incoming signal. It's only when it's correlated....

G. Verschuur: You could adapt VLBI...

K. Kellermann: Yes, the new system will be. It will be a multi-bit system.

J. Broderick: It would be hard for an extraterrestrial to send a signal to earth that wasn't correlated over all those telescopes. They'd have to be real clever!

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HOW DO WE REPLY?

Gerrit L. Verschuur
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I have been filled with apprehension about this talk lest it lead to consequences akin to those I experienced in the years following my last presentation to a SETI meeting. At the 1977 JPL/Caltech meeting on SETI I tried to make the somewhat satirical point that if we were to play the role of open-minded scientists and were serious about SETI, given that we knew absolutely nothing about ET at all, we should at least be open to alternative ways to search. After all, millions of people are already firmly convinced that humans are in continual contact with many extraterrestrials. You need only indulge in a superficial exploration of bookstores to find many reports of such "contact".

Following my 1977 talk I was unexpectedly projected into a series of adventures which involved me in either witnessing or experiencing many of the alternative ways of making contact with ET. My 1977 list included suggestions to further explore trance induced contact, automatic writing, personal experiences regarding contact, and the UFO phenomenon. It is not appropriate to talk about my experiences today. Details of my experiments and experiences are documented, but as yet unpublished. However, there is one interesting aspect of the alternative ways of allegedly making contact with ET I would like to discuss, especially from the point of view of making an analogy with the radio work.

In any search an alleged 'signal' may or may not arise from ET. It turns out that in personal encounters with ETs there are interfering signals present, not always noiselike, which seem to contain interesting information. These 'signals' arise from both the personal and collective unconscious. At root the source lies at the genetic level, the collective unconscious, and at the more personal level the experience is heavily modulated by beliefs and expectations.

The problem of recognizing a signal from ET amongst unwanted (or noisy) information exists, no matter what method you use in your search. In the radio domain no detection have yet been claimed, but in the esoteric realm apparent contact is continually made and some people charge only a nominal fee to open up the communications channel! I encountered and explored these alternatives and discovered some very interesting things.

The "noise" you personally experience in any effort to establish contact with ET (or any form of higher consciousness, or God) is apparently generated by the unconscious and has some very interesting properties and roots. "Unwanted signals" may arise from two levels of the unconscious. The deepest level, which Carl Jung discovered, is called the collective unconscious, and it generates "archetypal" experiences. The upper levels of the unconscious involve personal beliefs and expectations. The collective unconscious, I now believe, originates as deep as the genetic level. Information originating there may literally permeate to consciousness. When someone enters the realm of esoteric experimentation, such as indulging in trance-induced communication with extraterrestrials, that person may simple be tuning into the contents of the unconscious realm, contents which have a primarily biological root, and are manifested as the Jungian archetypes. This is interesting, but so what? Personally, this awareness led me to reconsider why we were doing SETI at all, and what sort of thinking is implied by our traditional approach.

I would like to suggest two thought experiments. Imagine that it has happened. Take this seriously. Imagine the feelings you might experience upon contact. Imagine you have discovered radio signals from an intelligent civilization amongst the stars. Imagine ET is very close. What then? Can we deal with this? This may be rather difficult because we tend to talk theoretically about searching with million channel receivers and so on, which removes us from the the issue of contact. During my adventures I was very much involved in confronting the possibility that I had, in fact, met an ET; an interesting psychological state. Try it now. Imagine that we have contacted ET. What do we do about it?

The human approach to most issues is heavily modulated by our limited lifespan. We tend to focus on issues which are shortlived with respect to our lifetimes. However, in the case of SETI we generally realize that we are unlikely to indulge in a two-way chat with ETI while we are alive and so we proceed without giving the question of a reply very much thought. However, are we acting either ethically or in a morally responsible manner if we shrug our shoulders and exclaim that the matter of a reply is something for our offspring, perhaps 'n' generations removed, to think about.

It may appear harmless to search, yet the detection of, and the subsequent communication with, ETI may be a matter of such enormous significance that it could produce a more profound discontinuity in the evolution of intelligent life on our planet than anything that has ever gone before. Because of its potential impact, should we not examine the entire question of SETI more deeply, as if we really believe in this stuff, and then closely consider how we handle a response. Or are you willing to stake your life on the hypothesis that the impact on our world will be trivial and that we need not think this through. What I am getting at is that SETI's success may overshadow even the impact of the development of the atomic bomb, and look at how we have analysed the ethical and moral implications of that work, if only in retrospect. Can we afford to ignore our responsibilities with regard to SETI?

It is my thesis that the long-range consequences of our experiments must be considered now. We might, at least, devote a meeting to an open discussion of these issues. Sebastian made the point that contact with ETI will be the most significant thing that has ever happened to the human race. I believe that. And yet in some way we are plunging ahead in this small group - there are not too many other people involved - and we are doing something which is going to influence absolutely everybody on this planet. Are we being ethically or morally responsible by not broadening the discussion? After all, the impact on the world may be profound.

So how do we think about this particular issue? I would like to propose another thought experiment. When you think about it, imagine that the whole process occurs almost instantaneously. Forget about delays of a few thousand years. Imagine that the detection, our decision to reply, the reply, and the response from ET, all occur in your lifetime or even in the next few weeks. Think about SETI and contact from that point of view. Then what do we do?

The first interesting question that has to be confronted is: "When do we know that we have picked up a signal?" And here something Barry Clark said to me years ago has stuck in my mind. He made the point that given almost any bizarre observation the theoreticians will explain it away, even, I would add, if they have to use black holes to do it. Just give them enough time.

So who decides when the detection has been made? The offical NASA SETI program involves looking for binary signals, etc, etc. But perhaps some different form of signal will be picked up serendipitously and then we have to ask at what point we would recognize it as being from ET so that we can tell the theoreticians to stop explaining it away! Can we set criteria in advance? I doubt it.

Here is a list of questions related to my general topic:

a. Who decides when a detection has been made?

b. At what point do you have the courage of your convictions to ask for funding to transmit a reply?

c. Should we respond?

d. Who decides?

e. Who determines the contents of the response?

f. Who sends it?

g. Will the work be classified?

My talk is about asking questions. Do we set up panels? Obviously! Bear in mind the thought experiment. This is all happening in your lifetime. Do we tell the Russians? Do we tell our allies? What do we do with the communication channels that have opened between ET and us?

Answer 1 - No! This is a variation on Martin Ryle's wise caution - that we not shout in the jungle. We are, by definition total newcomers on the cosmic scene and totally naîve in the business of interstellar intercourse (if you will pardon the expression). Do we ignore this fact or confront it now? Or is this just another issue we will leave to future generations?

Until we know what ET is doing out there, I personally would not want to let them know we are here. I would like to know more about them first.

Just in case Ames makes a detection in the next few weeks, how do we decide whether to answer, and who makes the decisions. Who decides? Will radio astronomers be allowed to remain involved? Does the UN take a vote? Do we have a planet-wide referendum? This event is something which is going to profoundly influence everyone. If you want to believe that potential contact with ET is a trivial issue you can avoid this question and go about it in your own way, but then what are you doing at this meeting?

So who determines what sort of response is sent? Which world leaders will speak for us all? Who pays? Which then returns me to the question I raised in 1977. If we agree that we are serious in perfomring an extended search why do we not build transmitters and transmit with a view to making contact with ETI? Isn't that how we would behave if we really believed in the search.

Will the work be classified.? I do not know how many secret agents of the CIA are in this room, but the CIA probably has contingency plan somewhere to deal with this. Perhaps they will silence all the astronomers involved.

Consider that the person that makes the first detection has a little power for a while. For a while that person has in his or her hands a piece of information which may alter the cultural and technological evolution of our planet. And then what happens? Will we then witness the Russians, the English and the Americans all frantically blasting radio signals back out into space so they can be the first to be heard by ET.

If we decide to respond, what do we say or do? No matter how bizarre and unnatural a signal appears, we may never be certain it is from ET until we have indulged in two-way contact. This will certainly involve mimicry in the flow of information between us.

An essential aspect of establishing verifiable contact with another species, or even with members of an alien culture on earth, is this phenomenon of mimicry. When the missionary entered the jungle and encountered the primitive savage initial communication was usually related to the performance of certain acts which involved mimicry.

Learning a language involves mimicry. This phenomenon occurs within familes. The little baby starts mimicking the adult and then the adult spends the next five years mimicking the baby. That is how communication begins.

The film, "The Kingdom of the Dolphins" (which I saw on PBS) revealed an astonishing example of interspecies mimicry which could provide a lesson for us all. The humans involved had built a synthesizer which generated specific whistles similar to dolphin signature whistles. On hearing this, one dolphin, who seemed to be the ambassador for the group, responded by mimicking the sound. The dolphin then added a footnote, a further comment, if you will, hoping (or perhaps expecting) that the humans would, in turn, mimic her and communication would begin. However, the humans were not prepared for this and could not respond, due to severe technological and physiological limitations. Perhaps they hadn't expected the dolphin to be "so intelligent" as to respond immediately and so profoundly. The dolphin did not give up, however. As an additional signal that she was aware and conscious of the human's efforts to communicate, the dolphin mimicked the prone position of the cameraman lying in the sand and then oriented herself into a vertical position, tail in the sand, in front of two other humans who were standing on the sea floor. It is difficult to imagine any other way for a dolphin to mimic a human except through sound and bodily position or movement. So was contact made? Sure. But who was the intelligent one, or the more conscious? Can we even begin to prepare ourselves for such extraordinary attempts to communicate with us?

We will first have to mimic any signal received from ET. This simple form of response will inform them that we are here, and are willing and able to duplicate their signal. We will surely add something to spice up our response, just as the dolphins did in that film. We will hope that ET will deal with our response better than we dealt with the dolphin's efforts.

What we add to the returned message is a major question. But are we psychologically prepared? What will be involved? We will, no doubt, someday assign a considerable amount of human energy to this question. A reply will require heavy-duty transmitters, large radio telescopes and high level committees to do the job. Which again touches upon the issue of <u>who</u> decides what to say.

We will probably be confronted with the unexpected. Mimickry may be part of contact and the dolphin connection is worth further exploring for the insights we may gain from experimenting with this form of interspecies communication. To me the dolphins are a form of ETI and provide an incredible opportunity to test our computer technology and our decoding algorithms, if we wish to do so. The cetacean species has demonstrated a willingness to communicate and highlighted an amazing inability on our part to deal with this.

Twenty four years ago, at the first SETI meeting in Green Bank, they created The Order of the Dolphin. I would like to reestablish contact with the dolphin awareness. (I have written a proposal on how to make contact with the dolphins. You need the best learning program people and the best decoding experts, build a learning machine in a small box which we submerge to literally swim with the dolphins and let the dolphins teach the machine, essentially a new-born dolphin, and therefore us, how they communicate.)

I believe first detection may be serendipitous. By definition, we cannot plan for serendipity. We can consider what our strategies might be after a serendipitous discovery, however, because no matter how a detection is made the same issues related to our responsibility to future generations have to be addressed.

For us to assume that interstellar relationships are going to be friendly, and that the communications channels will not be fought over on an international basis, may be naîve. Imagine the political leverage falling to the country that makes the first detection - for a while at least. There are many questions related to our responsibility to future generations and, of course, to our present generation which, I believe, should be confronted now, even as we begin the serious search.

There is another aspect to our view of SETI which might be considered. Technological behavior appears to be one example of a class of activities an intelligent species might indulge in. The characteristic known as intelligence, in turn, is one of many which conscious entities might manifest. Our quest for ETI is essentially a quest for ET technological societies, like ourselves. If technology is a subset of a subset of the characteristics of conscious species, should we perhaps be concerned with a broader search, a search for extraterrestrial consciousness. Unfortunately we appear unable to predict in advance what a more conscious entity is capable of doing, so thinking along these lines may, at first blush, appear fruitless. However, being a believer in exploring interesting options, I propose we ask what it means to be conscious, how consciousness emerged, if it ever did, how it evolved, and whether there may be other ways in which conscious entities could communicate, even over interstellar distances. In that regard we might usefully turn to the cetaceans for our first lessons in communication. As the defenders of the radio SETI claim, even if the chance of success is minute, the consequences could be profound.

My conclusions are mainly related to questions of responsibility. Let us consider all aspects of the consequence of contact. We should discuss this everywhere. We may find that people of our planet may simply not want to do this. Perhaps we are not ready. Ana if we are, why don't we make every effort to communicate with the dolphins, for example.

Can this small group speak on behalf of 4 billion people by ignoring these questions. I don't think we can. If we opened our discussions we might come up with unexpected ideas and as regards ET the unexpected may come to pass. Unless we ask unfashionable questions what hope do we have of ever discovering anything outside our current beliefs?

This made me think of another sciencefictionesque scenario. What if ET had thought all this through already, had explored consciousness, or whatever, and had discovered ways to make themselves visible only to those societies which have evolved out of our primitive war-mongering level. Lets think and talk about these issues.

Finally, I would ask each of you why you are involved? What is you personal motivation? I ask this question because of my own experiences.

I did this experiment, and followed wherever the answers appeared to lead. In 1977 I made the point that the SETI program has many of the hallmarks of the technological search for God. The SETI experience has many of the hallmarks of a religious quest. There is a parallel here, to the religious experience in which you make a commitment to pursuing your path no matter where it leads. In our case, pursuing the search for ETI, no matter where it leads, may lead you into interesting places of self-discovery. So, why are you involved? Contact with ETI is not going to be a personal thing. It will be collective and is going to effect the entire human race. So why are you involved?

I want to conclude with a challenge, given what happened to me after the 1977, where I mentioned, half-jokingly, half-seriously, all these other ways to search and proceeded to experience them quite unexpectedly. I really would like to make contact with ET, in some unambiguous way. So, as a challenge to the gods, or ET, or whatever, how about some response to these questions? Go ahead, make my day!

E. Olsen: There is such a broad range of issues you have brought up. I would submit we have shouted in the jungle, with our radars, our DEW lines, or TV stations. The chance is that if we do detect something which cannot be kept secret it will quickly be known worldwide. The very short term reaction will be that it won't have much of a personal effect on society. A little later on there will be a lot of religious fanatics that will come up with various ways of worshipping the demons in the stars. Then that will all die down too. There will be ripples that will suddenly die down and more rational approaches will probably absorb a detection into our society and the fact that stars are so far apart, and that even the speed of light is slow compared to the distances, means that it will take a long time for us to get all the information and we will be given the amount of time needed to absorb it.

I agree it will have a tremendous effect on society and my own personal feeling, as part of the SETI program, is that I think it will have a beneficial effect. Consider what the earth will be like three to four thousand years from now if we continue to inbreed as we do now, intellectually. We will stagnate. We will be like the major civilizations of the world. Technological civilization is very new in the history of the earth and yet it has had a profound effect, far more effect than a much older, larger civilization had. It has had an effect for both good or ill, and this also will have a profound effect upon our worldwide civilization which will be beneficial or bad depending on how we make use of the tools it may provide.

You touched on enough points to get another meeting going. You said it would have a beneficial effect. You are making that statement on behalf of 4 billion people. I think that millions, 100 of millions, might already object and claim that this would disturb their beliefs profoundly. I don't think we can speak on behalf of everyone on this planet.

B. Burke: Why not? Why not!

We have to discuss this.

B. Burke: (expletive deleted)

In that case, what do you feel about the atomic bomb type of work. You say, that's fine, the scientists can go ahead, and do it, which I agree with. But it should be discussed.

B. Burke: They did it.

W. Mook: If somebody doesn't want to learn science and isn't interested in science, why should they be asked to make a judgment on what science does? Think of someone sitting in some tribe somewhere who doesn't want to go to a university, why should we ask him?

B. Oliver: Because they can read their horoscopes.

The western mentality is that we will tell them what to do.

E. Olsen: No. The western mentality and technology is so strong that they are co-opted into it. That is why France is turning into another

United States, although they hate to admit it. The Coca Cola generation has spread worldwide. I am not saying that is a good thing.

S. von Hoerner: One of your questions was asked in 1971 in Byurakan, in private circles, between East and West. What should we do if we ever pick up a signal? First, keep absolutely quiet. Don't tell anyone, not even your wife. First, make absolutely sure that it is a signal from little green men and then, all of a sudden, tell all the world, not just your government.

I would like to see how you implement that.

B. Burke: You set up a panel.

J. Tarter: Once again there is the implication that we are acting irresponsibly. I cannot talk about everyone involved in this program, but at least in the NASA, the SETI program has to have a plan for what it does when it gets a signal. That plan is not an easy thing to write. It is being worked on.

How broad is the discussion?

J. Tarter: This is within the NASA program. It is probably going to be one of the more difficult things on which to agree. But at least it is being worked on. With respect to international considerations, in October 1986, in Innsbruck, Austria, there will be a meeting of the International Institute of Space Law and they will have as one of the topics on the agenda, SETI, and the detection of signals and who has rights to what. So I resent your position here, that once again you guys are going off and doing it (on your own with little discussion).

I don't want to give the impression that you are irresponsible, but I say that if that experiment is as earth-shattering as Sebastian was pointing out, then this thing could be the greatest thing since the invention of language, so should we not broaden our discussions? Is it irresponsible to keep this within a relatively small group.

K. Kellermann: Jill, do you want to tell us about what the NASA plan is?

J. Tarter: No. Because, as I said, this is one of the more difficult things.

K. Kellermann: You have been doing this for 5, 7, 8 years now, and you still don't have anything you want to talk about?

J. Tarter: The plan is at least that far away. There are steps relating to a certain level of verification. What steps are required to determine that there is a real signal? That is open to debate. There have been questions about which groups of experts should be constituted. Should that be done in advance, with the experts waiting to look at the evidence? Is there anybody expert enough to look at the evidence except the people already working on it?

Are there experts in an area in which we know nothing?

C. Seeger: Are there any experts in the future in this room? Was Columbus being thoughtless and careless when he set out to discover America? Was Galileo being inconsiderate of the interests of mankind when he went into mechanics? Or Newton?

From a pure research point of view, I would say of course not.

C. Seeger: The effects are certainly large.

J. Broderick: I don't think we should look at it from the point of view of Columbus or Galileo or Captain Cook, but we should look at it from the point of view of the Indians and the people on the Polynesian Islands and the people who are now cargo cultists who met with this ET civilization during World War II. Or back in the time of Columbus. All those societies no longer exist now, except in our museums. And so if we do contact ET, we are going to become them. We should think about whether we want to do that or not. Are people who are ancient peoples, who are trying to stay ancient peoples, and who are having a lot of trouble doing that (the latest you've seen in the movies are the AMISH) to be ignored? Once we contact ET we are going to become them and we have to decide whether we like that or not.

B. Oliver: I don't know of any instance where a culture has been subverted by radio alone. There have been all sorts of contact besides radio contact, and it is the other contacts which have done the job.

R. Gray: There is only speculation that there will be strong social or cultural consequence and that might be taking ourselves a bit too seriously. Consider that we might get from a globular cluster, several tens of thousands of light years away, a long sequence of prime numbers. I don't think that would change our culture dramatically. It clearly would take a long time to respond to that.

B. Oliver: In that case we would be in touch with a very stupid civilization, if they have to send prime numbers for us to recognize it as an artifact signal. They'd better send something more worthwhile. Let me say another thing about the consequence of contact. You assume it will be very momentous. I think there will be a hell of a flash of interest around the world and then it will absolutely subside. It will take centuries or millennia for the integrated effect to be felt, but it will ultimately be felt and it will be profound. This will be telecommunication between societies and not individuals, and it will go on and its effect will accumulate.

Which is why I suggested we do a thought experiment. So we can get rid of the time element and just consider the before and after aspects.

B. Oliver: But you can't get rid of it. You can't get rid of the actual delay in the actual process.

Only if you know exactly what is going to happen.

B. Oliver: Well, it has got to be at the speed of light, or do you think not?

I don't know.

B. Oliver: Well, that's your problem.

K. Menon: Broadening the responsibility is not necessarily going to produce a more rational response. In our experience two societies have consciously, deliberately, put the power to destroy our existing civilizations in the hands of two men. Is that a rational thing to do?

B. Oliver: It hasn't happened either.

K. Menon: It hasn't happened, but we have given the legal power to two men to destroy civilization.

B. Oliver: You know what. Those red buttons aren't connected to anything.

S. Gulkis: You seemed to put the esoteric searches on the same level as a technological search, where noise is a definable quantity. You made a comparison with consciousness. I know how to interpret what the signal to noise is for physical quantities. But I am very concerned that we don't understand what the noise level of consciousness is. There are going to be many false alarms in the system. Much as we observe in the UFO phenomenon. I am very concerned about the propagation of additional noise in the system. So I am against what you are arguing for.

I am saying that with regard to consciousness there is a noise-like phenomenon, but it is more like a highly correlated signal which contains information about something else.

F. Schwab: Which is the unconscious.

S. Gulkis: I don't see how you use that as a useful concept.

I was making a metaphor, of trying to pick up a signal by one method versus another and you also have disturbing signals present.

J. Findlay: I take it that you are saying that an alternative strategy might be ESP. Well, if so, it is not the noise you are up against, but lousy experiments. Until ESP can begin to clean its house, you are in trouble. I believe it should clean its house.

VI.

PANEL DISCUSSION

PANEL DISCUSSION

Frank Drake (*University of California, Santa Cruz*)
Kenneth Kellermann (*National Radio Astronomy Observatory*)
Sebastian von Hoerner (*National Radio Astronomy Observatory*)
Michael Papagiannis (*Boston University*)
George Swenson (*University of Illinois*)
Charles Seeger (*NASA Ames*)
Bernard Oliver (*NASA Ames*)

F. Drake: We're going to start our interaction with panel now. What we're going to do is first give members of the panel an opportunity to make any statement or reveal any spectacular ideas they've had. Then we're going to ask for questions from anyone in the room, and the panel will attempt to answer them or refer them to someone who can. So let's start with Sebastian von Hoerner before he retires.

S. von Hoerner: I think I would like to say something about my impressions of this meeting. This concerns the past and the future. Regarding the past, I must say I am very glad about this meeting, that so many of the old faces are still here, of the real beginners of the field starting with you, Frank. They are still active in the work, but if this were all, this would be very sad, we would be just a few crackpots who can't get away from our silly ideas! (Laughter) So it is very nice that there are so many new faces, young faces, and I would say also young people with a lot of drive who put all their energy, all their will into this field. This was very obvious in this meeting; and this combination of the old and the young is what is really needed. For example, Frank, you have been extremely happy that when you were a young fellow you didn't care about your career. You said you were too naive. I would say you were too carried away to think about it. You were under a director who was old and recognized, and still as an old man had the ability to see the new things coming up. If the director hadn't allowed you, it would be bad luck, you couldn't do it. And so I am very glad that I have seen at this meeting after twenty-five years the old fellows together with a lot of new driving forces.

Well, so much for the past. I should say that the amazing thing is that this happens in spite of the fact that during all these twenty-five years there was not a single success. Don't forget this. It is really difficult to find a field where for so long a time, there has been no success at all, and still it is full of life and going on. Regarding the future, my personal wish is that we would try to keep in very close contact with normal science (laughter). This means trying to avoid developing one-purpose equipment. For example, if we make a stellar search, that is good astronomy. If we look at our planets that is marvelous astronomy. But, I wish you would find a normal astronomical application which really calls for a ten million channel receiver. We haven't got that yet as far as I know. But keep searching; this would be a great help. So it is with other things. Try to do things which are also normal astronomy; Freeman Dyson said that in the long run, a well organized SETI search would be indistinguishable from a well organized normal astronomy search. Then, don't expect success too soon. Just keep going! Let me also say that I hope what I call the fringe benefits will become more obvious and may be even effective. That people will think more about our problems. They will put their minds out into space and then look back to earth and see how silly we behave, and then think what could be changed. Then more people (I was corrected, I should not always say politicians and military, it is all the people) should make this sort of experiment, "Gee, My God, if Little Green Men would come in my back yard and ask me questions, what would I answer? How would I explain things on earth? How would I explain what do we do next on earth? What do we really want?" I think this would be extremely healthy if more people would think "What would I explain to Little Green Men if they asked me?"

M. Papagiannis: I would like to start by expressing, on behalf of all of us, our congratulations and our appreciation to George Seielstad and Ken Kellermann for organizing such a fine meeting. (APPLAUSE.) Let me now share with you a few ideas that I believe would help our young field to continue to grow stronger and more productive.

We have undertaken so far nearly fifty search projects and have accumulated more than 120,000 hours of observations, about 100,000 of which have come from the two SETI-dedicated facilities, the Ohio SETI Program of Dixon and Kraus and the SERENDIP Program of Paul Horowitz. This is a substantial effort that might already be telling us something, but to get a global picture we need to organize the data. The same targets (sun-like stars, star clusters, nearby galaxies, etc.) have been observed at different frequencies, with different sensitivities and different spectral resolutions. They have also been included in all-sky surveys, such as those carried out by the two SETI-dedicated searches and possibly in other general surveys. I believe it would be very useful to prepare a catalogue from all the available data that would list all the targets that have been studied, giving for each one the frequencies, sensitivities, spectral resolutions, and duration of each observation. A similar list could be prepared for different regions of the celestial sphere that have been covered by sky surveys. Systematizing our diverse data will significantly help our future searches, because this way we will know what has been done so far and what gaps remain to be filled. I am sure that such a comprehensive listing of all of our previous work will also be of significant value to both the Targeted Search and the Sky Survey of the NASA SETI Program, that is expected to start around 1990.

Our new field is growing rapidly, and many historically important documents will be lost if we do not start collecting and preserving them immediately. This is probably the most important undertaking in the history of mankind, and we have an obligation to future generations to preserve an accurate account of the entire effort. We must establish a historical archive at a central place and appoint an official curator. In this archive we would keep copies of all SETI-related publications, correspondence, and pieces of equipment such as those used by Frank Drake in Project Ozma. Also included would be critical statements by fellow scientists and public figures who did not agree with SETI, cartoons on extraterrestrials, newspaper editorials, etc. I think we will do a service to history by starting such an archive as soon as possible.

Another idea that could be of help to our young field would be the establishment of an International Award for significant contributions and achievements in this new scientific endeavor. We have discussed already this possibility in IAU Commission 51, and it could become a reality if supported by other major national and international organizations and institutions. Also the idea of having a logo for SETI is an attractive one, and Jill Tarter and I have already done some work on it. I believe that all such activities are bonds that bind us together and create a unity of effort.

Finally, we must develop a scheme for the distribution of research funds that would maximize the returns and maintain the momentum we have gained in recent years. It is essential to have a strong core program, such as the NASA SETI Program, but it is also important to keep examining other alternatives and to keep many good people active in our field. I propose therefore to adopt a scheme in which about two-thirds to three-fourths of the research funds available would be reserved for the core program(s), while the remaining third or fourth would be distributed to ten or twenty smaller projects that address specific ideas or alternative theoretical possibilities. In this manner we will have a strong core effort, we will enhance our chances of success by covering as many alternatives as possible, and we will keep a significant number of scientists active in our field. All these SETI efforts may also produce some unexpected discoveries. We must not forget that the discovery of pulsars and the three-degree background radiation, both of which were rewarded with Nobel prizes, were both totally accidental.

In summary, though at times progress might have seemed to be extremely slow, and occasionally even losing ground, when the whole effort is seen in its entirety one cannot help being immensely impressed with what has been accomplished in these 25 years. We can therefore celebrate our Silver Anniversary with pride and justified confidence and optimism for the future. It has been a privilege for me to have participated in this effort, and to have served as the first President of the new Commission of the IAU on the Search for Extraterrestrial Life. In November 1985, at the General Assembly of the IAU in New Delhi, Frank Drake will become the next President of IAU Commission 51, and in a Joint Session with the Commission on Radio Astronomy we will review the progress that has been achieved in these past 25 years. We will also honor Frank Drake for his pioneering work with Project Ozma, which in 1960 opened the doors of this new field.

G. W. Swenson, Jr.: A couple of weeks ago when Ken Kellermann phoned me to see if I really was going to come to this meeting, he said, "You're kind of skeptical about SETI, aren't you?" I had to cogitate about that. After all, I did vote once against project OZMA. I was a member of the Cyclops program, and I'm here--in the lion's den, so to speak. But nonetheless, I suppose I'm as skeptical as anybody in this room about the SETI program and the potentialities thereof. So I will accept the onerous charge of being the devil's advocate. You will probably find I'm not a very effective devil's advocate because the basic consideration in my philosophy about SETI is that the discovery of intelligent communication from an extraterrestrial civilization would be mankind's greatest achievement. Consequently, we can't merely dismiss this with a wave of the hand or consider the people who are so enthusiastic about SETI to be way off the beam. On the contrary, I have a tremendous admiration for the dedication and the intellectual power of the people who are devoting themselves to this search. So, what are my doubts?

Well, first of all, this is an extraordinarily difficult task--probably the most difficult scientific project, the most unlikely to achieve its goals, of any that you can possibly think of. We've talked a lot about the cosmic haystack. Now it strikes me that the cosmic haystack, as it's been envisioned, is short at least a couple of dimensions. It seems to be to be based upon an anthropomorphic assumption about extraterrestrial intelligence. This in itself would add a couple of dimensions if we were to broaden our understanding of what might constitute intelligence. We are looking for a needle in a haystack which is bigger than most of us are willing to contemplate. In order to have some kind of a strategy for the search, we have consciously narrowed our view about what constitutes the cosmic haystack.

So, given the importance of the project in view of its significance to the intellectual development of the human race, we can't give it up; but neither should we fool outselves into believing that we are going to achieve success in the foreseeable future. This kind of activity, as we've seen in twenty-five years of sporadic searching with no results, is not liable to earn a Nobel Prize or perhaps even academic tenure. The scientific community is faced with a dilemma here. It's an extraordinarily important activity, but it's such a shot in the dark that the typical science or academic administrator must think hard about how much of his resources to gamble on such a long shot. So the young people who enter the field have to be aware of this and should not be encouraged to think that this is a way of achieving immortality easily or quickly. Fortunately, it is such an intriguing area of thought and study that we will probably not have to worry too much about motivation.

Possibly the most achievable goal of the generalized SETI Program, not any specific agency's program, would be to get the public thinking about Sebastian's proposition or his question, "How would you explain our society to a little green man?" His memorable address the other day concerning the way we're mismanaging our society on this earth should have everybody thinking and worried. And if this is another approach to raising mankind's consciousness to the danger that we're facing, then that justifies the program all by itself.

Now, assuming that we are going to go ahead, with a few very good minds working on a very important problem which probably won't be solved in any of our lifetimes, we should be extremely professional about it. What follows is going to go against the grain somewhat. Although I believe that extraterrestrial intelligence is just as likely to be recognized by some astronomer doing his normal thing as by somebody doing a dedicated search, I believe that we should not be overly concerned with encouraging amateur activity in this field. I'm worried about the tendency of this very attractive idea to get loose in the press. The SETI program at large is too vulnerable a target for skeptics, and therefore it needs to be conducted with extreme professionalism.

I've had a lot of contacts with amateur radio astronomers in the last few years since I made the mistake of writing a long series of articles about amateur radio astronomy which achieved considerable circulation. Amateurs are very dedicated people but have short attention spans and are very anxious to achieve positive results. They are not subject to the discipline of professional training. So I've had dozens, scores, maybe hundreds of records of "pulsars", drift scans of Cas A, this, that and the other strip charts sent to me asking for comments from amateurs all over the world. I haven't seen anything on any of them that look like a real detection of a real source. It's all noise. We have to watch out for this sort of thing. Once the press gets hold of an amateur report, it can be blown all out of proportion, and the whole SETI community could be discredited.

F. Drake: You've put your finger on an important problem, and I'm probably crying "Wolf" on SETI, which the press likes to do, and the National Enquirer likes to do, and occasionally our colleagues, like happened in the Soviet Union a few times, fortunately never in the United States. It is important that that not happen. It does pull the rug out from under funding. That really wasn't a very discouraging talk, George.

G. Swenson: I did the best I could! (laughter)

C. Seeger: I want to comment on radio amateurs in SETI. One doesn't volunteer to stir up trouble; but in Silicon Valley and at various points, North, East, South and West, people have written in for help, technical and what not, and quite on their own have sometimes started short-term projects. I think that, in many cases, our SETI project ideas got into them, just a bit. Amateur SETI probably is not sufficiently rewarding, in general. We haven't published a manual, as George Swenson did, in effect, and thank goodness! We haven't been bombarded with tape recordings. We have enough problems with scanning old astronomical records taken for other reasons.

I think it absolutely useless to worry about something that is bound to happen, one of these days, when some star-struck technician, also a radio amateur, thinks he has got an ETI signal using a dipole and TV reflector. It will not be a tragedy. It will be a one-day wonder in some "newspapers". I think that in our own work, yes, we must be professional. I am concerned at the volume of pure science fiction in many SETI related discussions. My private opinion is that it serves no useful purpose in the long run.

In the normal situation where one is remarkably ignorant, having but one or two points on an unknown scale, one tends to let human imagination roll. Enormous scenarios appear, made out of whole cloth. Some are quite detailed. Some are in a fashion better done over a table with a glass of beer -- and not published!

The literature associated with SETI contains long standing, contentious arguments that I think are a natural, human response to the sudden opening of our view of the universe, and are brought about by the sudden advent of powerful technology and its application to astronomical exploration. SETI, itself, is actually a response to an ancient problem; one as old as literature itself. It touches on religion; it touches people's points of view and feelings about where they are in relation to the universe. It seems to be the sort of question almost every human asks. As they grow up, humans want an answer to this question. They adopt their parents' viewpoints or work at developing one of their own with which they feel comfortable. SETI is an unusual endeavor. It touches on the concerns of many, many people; trained and untrained, casual or serious.

I think that in SETI we must never forget that we are exploring the universe as scientists. That implies that we should be as objective as we can be. I am glad that in my life the existence of intelligence in the universe has been recognized to be a physical phenomenon which has at least the possibility of being discovered elsewhere. There is an enormous range of coherent phenomena associated with human activities, so I suggest it is appropriate to look for signals of a coherence far greater than that of any we have found so far by normal radio astronomical means.

In SETI, it is straightforward astrophysics and astronomy that we are doing and planning to do. Nevertheless, someone suggested that NASA SETI should do some astronomy along the way. So I must note that from the very inception of the Cyclops study, the idea that the instrumentation would necessarily be able, and available, to contribute to inorganic astrophysics was imbedded in the design thinking. It does not strain the facts to say that the Ames/JPL instruments now in the design stage will be good all-around astronomical instrumentation with, however, some unusual capabilities.

Now about the next twenty-five years. It is likely that we will see an enormous multiplication of our knowledge about the universe. I am not a soothsayer of repute. Anyone who is may rise and take this chair. There are some things that I would like to see happen, and believe will come about.

We should have a stellar catalog with hundreds of millions of entries, all observed in a standard multicolor system, instead of the present hodge-podge of half-million entries based on different, often uncertain, color systems, and of widely varying precision. Too often, luminosities are wrong, and many too many are not there at all.

We should have discovered planetary systems around some nearer stars. At long last, planetary exploration is moving into the modern era. Credit that development to the Planetary Workshops organized by Jesse Greenstein and Dave Black, which built upon the Philip Morrison SETI Workshops in 1976. There are some fine tools waiting to be launched into space. Extrasolar planetary detection is on the way. When I say on the way, I am counting in decades. In the meantime, we can expect some interesting results from advanced ground-based instruments.

Launching into space any instrument of appreciable size and complexity takes decades. The more complex a project, the more are the people with a hand in it, and the slower the proceedings. We have learned a hard lesson: the more complex our society, the longer it takes to do straightforward, common sense things. The carrots are wrong. Too many are watching their backsides instead of concentrating on getting things done properly. Society will learn how to do such things more efficiently and cheaply, or future important discoveries will arrive at a slower rate than even Martin Harwit suggests.

Astronomy needs truly large collectors throughout the spectrum, and in space. (Any way I look at it, I do not now see the far side of the Moon as a reasonable substitute for free space.) I think there are many obvious reasons for giant sensors in space, and we should start after them now.

Scientific exploration is no longer a luxury for our society. For better or worse, our society is now driven by knowledge and technology. We must follow this lodestar and direct it for our common good, or suffer enormously. We cannot go back.

Radio astronomy and SETI need large collecting areas in space, far away from Earth. These should not be built of girders hoisted there, but built in space of metallic elements no stronger than necessary to withstand construction forces and to last for many centuries. We should start developing the engineering technology now. While many of us increment knowledge, some should be supported to work on the truly longer term needs and the development of the required instruments. The measure of adulthood, in individuals and in social groups, is the degree of their active foresight.

How do we go about this? In olden times, astronomy was guided by a small core of gifted individuals who recognized the need for long term programs, and did not require so many inches of publications per year from those they inspired to work in that direction. We need to change the way institutional administrations decide on promotion and tenure. I believe the American Astronomical Society should organize itself and play a much stronger role in competition with and in the vigorous, critical and necessary support of the large governmental agencies. The latter, as they age, develop "natural" (but not necessary) handicaps, as we all recognize. Thus, I suggest, a bright future for Astronomy, in the widest meaning of the word, will either be dimmed or it will be the result of a more flexible driving force generated in our universities and given a long term coherence through the dynamic operation of our common instrument, the AAS.

B. Oliver: I guess the thing that impresses me the most about this meeting and the whole SETI movement is the fact that it represents a tremendous change in scientific opinion from the time when I was a boy. When I was an adolescent in the early thirties, Sir James Jeans was saying that the universe was barren because he believed planetary systems were highly exceptional and a result of catastrophic events. Shortly thereafter Spitzer showed that these catastrophic theories wouldn't work, that the stuff torn from the star would disappear into space. Next, the mechanism of magnetohydrodynamic braking as a means of shedding angular momentum was discovered and earlier theories of planetary system formation came back into favor. So the astronomical community shifted from a belief that we were exceptional to a belief that we are typical. Meanwhile the development of molecular biology and the discovery of the genetic code and all the other things that have happened in that field have made it appear that life is a forced process. So the scientific community has shifted from a belief in a barren universe to a belief in a universe that is very supportive of life. With that comes the inevitable thought that much of it might be intelligent.

The interest in SETI is not limited to a small group. Frank was a very early pioneer and met some opposition, but that opposition has melted away with the passing decades and I think that the whole idea that there must be other life has come to be the majority viewpoint rather than the minority viewpoint. It has always been appealing to the layman of course. SETI enjoys a danger of over-popularity. One of the things we have to do is to avoid becoming associated with popular crack-pot ideas.

It has been remarked that there has been no success in 25 years in the SETI program. I want to say a couple of things about that. First of all, it almost seems as if SETI is something that thrives on failure, but we hope that this won't always be so! I think that the failure of the early SETI searches, and possibly of the one that we are contemplating for NASA, is evidence that other civilizations aren't 10^3, 10^4, 10^5, or 10^6 times more powerful or more capable than we. I believe that we are evolving into a kind of maturity where we understand physical law, and we understand the certain limitations that it imposes on us. I don't think other civilizations violate natural law and therefore are constrained by these same things. So I take their absence here to be a bit of evidence that interstellar travel is exceptionally difficult for them, as it is for us, and I take the absence of detection to mean that we haven't done our share in developing our sensitivity yet. Their radiations are not such that they consume an amount of power that is comparable to the usage of the entire civilization. I think that we will join the galactic club when we build up our capability of detecting weaker signals, the kind of things we are presently radiating.

Incidentally, the things we radiate, although very high powered in certain cases like BMEWS, are very difficult signals to detect. They are beamed, they hit you very infrequently, they are chirped so they are not simple. Except for TV carriers, we aren't really radiating signals that are easy to detect. What we are engaged in now is an early phase of SETI where we are proving that we need to become more and more sensitive. It would be very wrong for any of us engaged in this program to promise or indicate that we feel success is just around the corner. I think we've got to develop an atmosphere in which people don't expect it immediately, but that they endorse these cheap efforts to detect something. But if we fail, and if the resolve is still in the community, then we can go to larger and more sensitive systems; I think we'll have to do that before we succeed.

Let me say that in this nationally sponsored SETI effort of the NASA program, the next couple of years are a very critical time. We've got to create an atmosphere of acceptance for a larger effort over the next decade than we are presently engaged in. This is not an enormous effort but it means funding going from one and a half million or so to several million for a short period of a few years in order to get the hardware we need to do this search. So we're in a politically sensitive period. We need to create an atmosphere of approval and a climate that will support a new start in NASA at a time when new starts aren't very popular. So bear that in mind as friends of the entire effort; everything you do to create support in your home territories will be very helpful in getting an endorsement by Congress of this new start.

Jill Tarter: Sebastian, with respect to your desire that we get closely tied to normal astronomy, I remind you of Mike's discussion of radio astronomy survey fallout. But (just in case you need it) another reason for building a Gregorian subreflector in Arecibo, and getting a wide-band capability, is that ten million channels of frequency resolution can be turned into a tenth of a microsecond of temporal resolution if you've got a wide enough band. The other comment is that the thing that I hope will happen is that, having built the ten million channel system, we will discover the *normal* astronomy that only a ten million channel system can do.

S. von Hoerner: Well, that's what I hope very much, yes. Especially since we are honest, and do not expect a real success of SETI in the near future. We should be really happy about any other success we have.

L. Snyder: I brought a question along from Professor Ed Olson of the University of Illinois who wanted to attend this conference, but was not able to do so. It concerns Frank and the panel. The question is: "Suppose 'N' in the Drake equation is much smaller than one?" Then the search must be directed at entire galaxies, not individual stars. So this touches on part of Woody's talk today. Then Ed asks, or he says, "ET civilizations will realize this and construct a large number of transmitters, each scanning a nearby galaxy, the beamwidth might be on the order of a few arcminutes." The question is, "Is it practical to search for such a transmission?"

F. Drake: It's a good point, and it does concern what Woody said: that is, with almost any reasonable luminosity function, your greatest chance of success is to point the telescope in any direction where you get the most number of stars in the beam. If you carry that logic to its conclusion, you point the telescope at galaxies, because even with Arecibo, you get the entire galaxy in the beam at once, with a few exceptions, such as M33. So, if that logic is correct, you're better off looking at nearby galaxies. That's how you will succeed first. As you probably know, Carl Sagan and I several years ago did such a search based entirely on that logic, but we didn't see anything. The possible flaw in the logic, the most likely flaw, is that for it to work the luminosity function not only has to be a reasonable one, but it has to extend to very high luminosities. For example, if you look at M31 or M33, the nearest galaxies aside from the Magellanic Clouds, the required EIRP for detections is about a million times greater than what is required to detect something in our own galaxy. So, we're talking about transmitters which are a million times beyond what we have in our civilization. It is an extrapolation beyond what we know has happened in the universe, whereas in the rest of SETI, conventional SETI, we always assume no more than things we know have happened. 10^6 or more EIRP is a great extrapolation; on the other hand, it is not ridiculous because it is still only one millionth of the solar luminosity. So it is a reasonable thing to do, with the caveats I just told. A way for an altruistic civilization in another galaxy to help us is to illuminate our galaxy, in which case the signal is on all the time, and you solve duty-cycle problems.

B. Oliver: I suggest the luminosity function cuts off very sharply when the waveguide melts. (laughter)

J. Broderick: Is your speculation based on the Andromeda people illuminating our Galaxy with the beam or illuminating the whole sky? Because if you were trying to signal from another galaxy, you would be a fool to send it in a direction that that galaxy isn't in.

F. Drake: I'm just saying that what our best search systems can detect from Andromeda is a signal which is 10^6 times Arecibo's transmission.

J. Broderick: And they're directing it at our Galaxy. Has anybody decided to follow this strategy of you and Sagan, and look at galaxies? I didn't get the impression that anybody else was looking at galaxies.

F. Drake: No one else has done it.

J. Broderick: You've done the definitive experiment.

Voice: No, that was with the 3,000 channel system, no automatic data analysis, and a sixty second look at each point.

J. Broderick: And you did not actually look at M31?

F. Drake: We mapped all of M31 that's available from Arecibo which is only the southern half. We did all of M33, and we did three others in a local group.

C. Seeger: I'd just like to note that the All Sky Survey will look at all galaxies.

J. Broderick: The targeted search won't, right?

C. Seeger: It may look at some.

B. Oliver: What's to keep it from looking at galaxies?

J. Broderick: Your objection that they don't have enough power.

C. Seeger: No, but we don't know.

B. Oliver: I'll be overruled! Don't worry! (laughter)

C. Seeger: We have a set number of stars we want to look at. If there is any spare time, we're going to be looking at all sorts of suggestions. I'm going to look for Sirius B.

K. Kellermann: George Swenson, I want to challenge your statement about the amateurs. I recognize many of your concerns and reservations, but remember Frank mentioned the accidental discoveries of IRAS which led to some conventional observations that could have been done years ago. That has been especially true in radio astronomy. Remember the OH lines which, after years of search, using the most sophisticated radio astronomy instrumentation, they were finally detected; then shortly after they turned up in emission, a hundred times stronger than anybody had anticipated. They could have been discovered many years earlier. That was even more true with the water vapor lines, which were a thousand times stronger than predicted

Literally the water vapor lines could have been discovered shortly after the second World War with very primitive equipment. I would think that our theoretical understanding of the interstellar medium, and predictions about what one might expect there is at least as good, if not better, than our predictions about capabilities and numbers of extraterrestrial civilizations. It may be that the signals are very, very strong, and it is just a matter of looking in the right place, at the right time, at the right frequency. The more people who are doing this, in more different ways, (and the amateurs can do it in far greater numbers), the better the chance of success. Similarly, as you did mention, doing ordinary radio astronomy may also result in hearing an ETI.

G. Swenson: I said ordinary astronomy.

K. Kellermann: What about the amateurs?

G. Swenson: Well, none of the examples you gave involved amateurs. And they all involved pretty sophisticated instrumentation.

K. Kellermann: My point is that the signals were very, very much stronger than the best experts had predicted.

G. Verschuur: And they did not require sophisticated equipment. The water line did not require it.

K. Kellermann: Even the 21 centimeter line. Several groups spent a year or more trying to build very sophisticated equipment to detect the 21 centimeter line, but when it was finally found somewhere else, the Dutch group rebuilt their equipment all over again in a few weeks.

T. K. Menon: Not only that, Ken, among the amateur astronomers over the last hundred years, there are very few instances of false alarms raised by them. They have been very valuable, extremely valuable.

G. Swenson: I understand that there are a few areas in which amateur astronomers have made significant contributions, but they are not going to help fill out the search of the cosmic haystack very effectively. I just think that if we get too involved with amateurs, for example, if we try to organize that amateur search as the variable star observers have done, and that sort of thing, that we're going to be lumped in with the UFO people in the minds of the public, and that will be the end.

T. K. Menon: There are very few UFO people among the amateur astronomers.

K. Kellermann: Maybe we don't need to get involved with them, but we shouldn't discourage them.

S. von Hoerner: I also want to mention again what I call the fringe benefits. The amateur astronomers, either radio or optical, have been very important. In fact they keep the public interest alive, a great deal. They speak in schools, and they ask questions of their neighbors, and all this I think is very important, so let's don't forget that.

G. Swenson: Well, when I decided to spend a good deal of time and effort cultivating the amateurs some time ago, it was with the idea that they could lend support in securing funding for radio astronomy and securing frequency protection for radio astronomy, and other beneficial areas. As far as I can tell, the effort fell flat on its face. Not only have I not seen any incremental improvement in our public relations, but I haven't heard of anybody who successfully built my telescope design. (laughter)

W. Mook: There's going to be enthusiasm, and amateurs are still going to observe, whether or not we have any dealings with them. Couldn't we establish some kind of protocol that says that we're not really interested in hearing anything they've got to say unless it meets particular criteria? And one criterion could be that the signal has to be detected by two independent people at the same time.

G. Swenson: Well, my feeling is, if one detects it, quite a number are going to "detect" it, whether it's real or not.

W. Mook: That's not the only criterion; there could be other criteria having to do with signal-to-noise characteristics of their receiver, and so forth. But it seems to me that there could be a protocol which could serve as a first-order screening.

G. Swenson: I was invited to speak at a national meeting of the amateur astronomers out at the School of the Ozarks a few years ago. I don't know whether you'd call these people amateurs or not, but a bunch of physics students from one of the universities had a radiometer set up observing the sun. They had a dish about two feet in diameter and about 400 feet of RG8 coax leading from it to a surplus radar receiver. There were big spikes on the record which were alleged to be coming from the sun. They had erected the thing right in the middle of a busy parking lot, and when I suggested that perhaps it was automobile ignition noise that they were observing (particularly when there seemed to be such a close correlation with the cars that were passing by!), I was shouted down because they were so eager to succeed at what they were doing. That's my concern about the amateur mentality.

W. Mook: I think that similar to ham radio licensing, if we could establish a procedure and a protocol that amateurs could get involved with and constructively channel their enthusiasm. This may encourage professionals, and even perhaps encourage professional astronomers to come out of the amateur ranks. The other thing I'd like to ask using your criterion, would you associate yourself with Jansky in 1920?

G. Swenson: He was certainly no amateur. Reber wasn't an amateur either.

J. Mook: Do you have a continuum of definition from amateur to professional? Is there room in your definition for paraprofessionals as there are in other professions?

G. Swenson: I don't know, I hadn't thought about that.

K. Cullers: I wanted to make one quick comment, having probably had the most experience with amateur SETI observers, just because they get referred to me at NASA because I'm an amateur radio operator. So far I'm really interested in George's experience here, I'm now a little more worried than I was before! I'm very skeptical about the ease of finding an extra-terrestrial, so I tell them that. I think one of the big dangers is raising the hopes of these people too high, and yes, every once in a while I get a signal that I have to look at, and I say, "No, that isn't it. It's probably this." And I'm usually right and that works out pretty well. I think that as long as this process goes on, I don't think it hurts much. What really scares me is amateurs meeting up with someone with semi-good scientific credentials who says, "Yeah, yeah, yeah," every time one of these events gets created.

Voice: Can you give us some example of somebody saying, "Yeah, yeah, yeah?"

Voice: Faces on Mars.

K. Cullers: Yes, faces on Mars, for example. It's scary, you know that's basically what's happened with UFOlogy; they've gotten a couple of scientists with sort of credentials.

Voice: Sort of, huh?

G. Swenson: You can argue with their motivation.

K. Cullers: That's right, and that I think is a real worry. But as I say, so far I have basically had pretty good luck. They've been reasonable people, and I haven't been swamped with silly reports. I've had one or two that they admitted were questionable, and when I explained them, they said, "Yes, I see what you mean." And it never went any further than that.

T. Bania: If Jill were to leave Serendip II at Arecibo permanently, much of the time it would be doing a search of external galaxies, other galactic systems; that's what Arecibo does best. I was also struck by Tom Kuiper's comments yesterday about non-planned serendipity. When he was describing characteristics of strange signals, strange phenomena that one astronomer might observe during the course of his or her experiments, it struck me that every parameter he described was stuff others might call bad data. So in the normal course of astronomical research, I think we pre-filter out a lot of stuff because we're focused in on other topics. I wonder if the committee as a whole or perhaps as individuals could help Barney out as well by using this fact as a mechanism for talking with our colleagues about this, by heightening SETI awareness in terms of well, there's something funny, maybe you ought to track it down, and certainly you ought to support the folks who are doing it systematically.

J. Tarter: Am I right that the Society for Amateur Radio Astronomers is meeting here?

K. Kellermann: Yes, next week. They have met here for the past several years.

J. Tarter: George, do you know that Association?

G. Swenson: Well, I've had people talk to me about it.

J. Tarter: Is there anybody from the Observatory participating in that meeting?

K. Kellermann: They usually do, yes.

J. Tarter: Could a suggestion about organizing some sort of SETI protocol or something be arranged like that?

K. Kellermann: Rick Fisher, you have talked to them several times.

R. Fisher: Well, I talked to them. It's probably a good idea.

C. Seeger: I'd like to say in this respect, I don't think you can tell other people unknown how to behave. About the best you can do is to treat cases as they come up as reasonably and civilly as possible, and people will learn through experience in time. We have enough trouble being professional outselves; so when we look over our colleagues, most of us are a bit critical at certain levels, and there are some fine feuds going.

J. Tarter: I'm suggesting that we allow them to set up criteria.

K. Cullers: I don't know if they have the competence to police themselves, I really don't.

J. Broderick: I don't think we have too much to worry about; radio science has suffered a false SETI alarm back in the 20's with the long delay echoes that people were discovering, and nobody is suffering badly because of that.

K. Cullers: That's one of the signals I got.

W. T. Sullivan: Mark Morris, who was on the original program but apparently couldn't make it, in fact had this idea, and I guess he's following it up. He's writing to a large variety of astronomers saying, "Will you send me a list of all the anomalies that you have observed?" Now, I'm a little skeptical about the whole idea of setting up a project like this. Who is going to spend the several days thinking about all the anomalies and trying to document them reasonably well? But this is his idea, that there should be some archive, there should be some input into this. Many of us have been saying for a long time that this is the exciting thing about an initial thorough SETI search--that all the radio astronomy that has been done, or a great bulk of it, filters out precisely the kind of thing we are looking for and is primarily dismissed as man-made interference. We all have to choose how to cut the 24 hours in a day, and usually one doesn't bother following up exactly what some anomaly was, you just want to get what you said you were going to get. With regard to scientific methodology, of course, it is almost silly. Why bother doing the experiment, why not just decide what you want to get and write it up?

J. Tarter: Mark is real discouraged, because nobody is writing back.

Voice: He got one letter. It's not a big job, keeping track of it all!

R. Dixon: I'd like to take the position that, contrary to what some of us have said, SETI has not failed. We need to be concerned about falling into the trap of saying that, even though we ourselves know what we are saying. I'm sure we are all asked by people in other fields, and when you've been doing this for twenty-five years and you haven't found anything yet, it makes us look rather negative in that aspect. I think we have failed only in the most superficial way, that we have not yet found a signal, but even in the sense that a politician might understand, certainly we have created new problems, new technological achievements that have accrued from this. We probably even created a few new jobs for people here on earth. Politicians like to hear that. That I think, on the deepest level, is the area where we may have succeeded in spite of ourselves. That relates very precisely to what Sebastian was saying, that the most important thing we might be able to do is to change the perspective of people to make them understand that the small differences we have among ourselves here on earth are really not that important. If SETI and other fields of science can succeed in doing that, it's the most important thing we can ever do, even if we never find that signal.

M. Papagiannis: I also have that same feeling.

R. Dixon: When one looks at the evolution of life on earth, which has been going on for nearly four billion years, and when you become aware of how fast things are moving now and how close we might be to a detection in cosmic terms, you have the feeling that if we blow it now we will destroy our civilization. It would be like running a whole marathon and falling flat on your face just one second before crossing the victory line. I think it is very important that, having come such a far distance in this long evolution of life, and finally reaching the stage where we can become part of life in the cosmos, we might be in danger of missing the effort at the last minute. So I think realizing that might be, at least for some of us, a stronger incentive to try to preserve what we have, and to completely run to the end and become part of the life of the universe.

K. Kellermann: I'd like to raise a question on search strategies. Most discussion one hears about search strategies centers either on a targeted search for nearby solar type stars or the all-sky searches. I wonder if there should be more emphasis on already known anomalies in the sky, specifically radio anomalies. If I were an ET[1] trying to advertise my presence by radio signals, I would try to make it clear that I wasn't a natural phenomenon. Frank Drake already mentioned yesterday the interstellar masers, where we really had to stretch our conventional ideas to interpret these observations in terms of natural phenomena. Interstellar masers really have many of the properties that one might expect of intelligent

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[1] While playing the tapes of this discussion in the course of trying to edit these remarks, my 10-year old daughter remarked, "Dad, you're silly!" *KIK*

signals. Another possibility, if I were trying to advertise myself, I would try to make it appear as though I were violating some basic physical law, like giving the illusion of something moving faster than light, to give a superluminal radio source which any physicist might recognize as being artificial. We all know that the Galactic Center is a likely target for a SETI search. We also know from interferometer observations that there is a radio source in the Galactic Center which is less than ten or fifteen AU across; the conventional wisdom is that it's a black hole, maybe an ET isn't too much more of an exotic interpretation than a black hole. Yesterday we talked about the search I did some years ago on 1934-63, which is a radio source that has a spectrum which peaks in the water hole. It drops off toward low frequencies where the galactic background is strong; it drops off at high frequencies where the cosmic background is strong. It is almost identical to the spectrum that Kardashev predicted that one might expect from a Type II civilization. I looked for the notch in the spectrum at 1421 MHz that Kardashev said might be used to indicate that it was artificial. I didn't find that notch; but that doesn't mean that it isn't an artificial signal.

J. Broderick: They were broadcasting their presence by varying the strength of the signal.

K. Kellermann: Yes, they could have been doing that; they could have been doing lots of things. There are lots of these radio anomalies in the sky. I suspect they all have a perfectly conventional legitimate interpretation in terms of natural phenomena. But I also think they're probably at least as good a SETI target as picking out some random G stars. All the information that is given of the possible detections from amateurs are probably wrong, but they should be followed up by the professionals. It seems to me that is at least as good a way to find an ETI signal as looking at random stars. That is one way of taking care of the amateur problem. Follow it up with professional answers.

W. T. Sullivan: Kardashev was very much in favor of this approach, and one thing we haven't mentioned is the Soviet effort. Although it's not what you would call a parasitic kind of thing, the RATAN 600 telescope is continually doing a massive radio source survey, and Kardashev and a couple of other people are continually looking at the weird-spectrum sources that come out of that as possible ETI from Kardashev Class III civilizations. The trouble is I don't know of any specific follow-up that they're doing on this, but they do have this idea. It's a bit similar to looking at the IRAS data base.

K. Kellermann: I hope, Jill, that in planning a targeted search, you keep this kind of thing in mind.

J. Tarter: Well, you notice that people usually quote "a thousand stars." We really only have 773. So it is clear that we are planning on looking at a few other places.

K. Kellermann: OK.

J. Tarter: Why is there only one SS433? I know we've explained it, but it's one of the few times where we've found something we know how to find more of, but we haven't found them.

T. Kuiper: What about the SiO maser in Orion?

W. T. Sullivan: That was an argument made by the Cambridge group when they were still in secrecy about pulsars and were considering that they might be ET signals (LGH's). Then they found a couple more, they said, "Aha, this can't be extraterrestrial, because there wouldn't be hundreds of them beaming at us." The argument can be used both ways. However, apparently that was one of the strong arguments that gave them the confidence that it was natural.

J. Broderick: Ken, how do you propose we look at these things that people have already looked at and discovered that they are extraterrestrial civilizations. I mean you have the inverted spectrum, and how do you try to figure out more observations? Maybe that's their only signal; they just make a very strong spectrum that's won over the galactic background.

G. Seielstad: The best thing that could happen right now is that they just turn off the transmitter, so it disappears from the sky.

T. Kuiper: The other thing to think about is that when you have an anomalous phenomenon like this, it is not intentional, but it's a side product.

K. Kellermann: That particular spectrum was proposed by Kardashev because it has the maximum amount of information transfer. There should be a beacon or something simpler which is coded to tell you how to get to the rest of the information.

T. K. Menon: Ken, I am sure that when the new Palomar Survey is completed, there will be millions of objects which are in the catalog which are not in the old plates apart from the magnitude limits. Similarly, vice versa, I am sure there will be an enormous number of objects which have disappeared. I have no doubt whatsoever. Most will be purely natural phenomena, variable stars and that sort of thing.

There is another object of the same type which Ken was looking at. Probably it is natural, but there was a definitely optical identification from the 1951 Palomar prints which just was not there on a recent 120" plate. John Bolton had been saying this for years, that all these various surveys are really not as meaningful as people make them out to be. So one has to keep in mind that the radio signals are not the only manifestations; there may be unrecognized aspects of the phenomena which manifest themselves in the optical plates.

J. Russell: I had a look comparing the original Palomar survey with the private survey we have just done. In the one plate we compared, there are a few dozen objects that just don't match between plates. They're on one and not on the other, so you're in for real field day.

Voice: How much magnitude difference?

J. Russell: We did the matching only to 16½ magnitude, so it would be unambiguous, I mean it is not like we have lost 19th magnitude objects, we're talking about 16th magnitude stars.

W. T. Sullivan: So at least a 6 magnitude change or something, you're saying.

J. Russell: No, the new survey is one limiting magnitude of 19, so it's a 3 magnitude change.

C. Seeger: They didn't just move to another part of the sky.

J. Russell: Well, they may have done that, too! Maybe one of them was Nemesis just passing by, I don't know.

C. Seeger: I'd like to say that it's no use, at least in my mind, to say "What could we look for that we know nothing about?" We can only look for signs sent to us that mean something. When we find something anomalous, we may get curious, we may learn something, but by and large we are searching. I think this is true of all the searches that have gone by of any duration. We're looking essentially for a format of a signal which is recognizably sufficiently complex in structure, with it's coherence etc., that it seems unlikely to be a natural phenomenon. Whether its a pulse or CW, we're looking at the extremes of the possibilities that are generally sought out and have been used by our own technology. But consider the infinity of possibilities of what an intelligent society like ours could concoct by way of accidental by-product of some activity unknown. There's no use, I think, to really try and catalog or discuss, or think what is likely or probable. You really do the best you can. What we are setting out to do, or are trying to do, with Cyclops, or the SETI operation which really got launched, is the kind of survey that Henry, the navigator, might have said, "Ships get out there and go down the coast a bit and come back and tell us what's out there. Go up beyond the sighted land and tell us." We're about at that stage in the work. The cosmos is absolutely huge and it's fabulous. The phenomena it's got! Speculating about what we can't do is a dangerous thing, it's a waste of time!

B. Oliver: I want to make a comment about searching other galaxies. This has never been very appealing to me for the following reasons. It is beguiling to think that if you look in one direction you have two hundred billion stars in your beam; that's very nice. But it takes more power to signal to another galaxy than it does to fill our own galaxy with a detectable signal. With the sensitivity of the receiving antennas we have now, this means the output of millions of nuclear plants. I have a great deal of difficulty finding a sufficient motivation for somebody in Andromeda to beam anything at us to go to that effort. What's the motivation? So, in four million years, you get a reply?

G. Verschuur: I want to make a comment on something Charlie just said. I'm all for SETI if you approach it from the point of view of Henry the navigator or whatever. Then why do you pretend with all this scientific justification. Nobody tried to justify exploring the ocean! In some sense we are always coming down to beliefs; we are searching for something we know nothing about. Privately a lot of people say to me, as we talked at these meetings or during them, that it is sort of a religious revival gathering. We are dealing with beliefs, not with science. I believe we might get more support if we approach this from a purely exploratory way and not try to justify it quite so much.

C. Seeger: Unfortunately, other people have other ideas, and I have no control over them. If you will read some of the things from the time of the Morrison Workshops, you'll see it right there. It's exploration. But we recognize it; people have different motivations. Why would anyone look at a galaxy? Frank did. I hope we're starting an exploration that will add to our knowledge of the universe. I think that is the reason for it. The reason we talk about theory so often is because looking back here on earth, it seems to be a logical story, a logical hypothesis with a lot of indirect data, to say that this is not an unreasonable thing to try to chase down. Difficult, but not unreasonable!

T. Kuiper: I think that you contradicted yourself, but I'm not sure. At the beginning you made some remarks about being worried about the amount of science fiction that gets involved in discussions of motivation of SETI and things like that. And then just a little while ago you said, "Well, we can't look for all kinds of phenomena that we don't understand because all kinds of things could be out there generated by advanced civilizations. So we have to restrict a subset to some kind of things that we can reasonably predict are going to be there." It strikes me that those two are a kind of contradiction because as soon as you start predicting, especially if you use words like motivation, like Barney Oliver is using, you're in science fiction up to your neck.

C. Seeger: Oh, no, I'm merely saying I cannot explore for something I cannot name. In the concrete sense of a scientific definition I can't explore for signals that I know nothing about. If I can say it's got this-and-this characteristic, I can explore for that. I've a tool for doing it.

T. Kuiper: The number of dimensions in the electromagnetic space, if you like, are not very many. You can name them very easily.

C. Seeger: There's the problem. This is the new element of the science. The minute you include invisible science, life, you have opened up a brand new type of phenomenon that only a few individuals living are willing to even consider.

Voice: Right, which means you cannot name the manifestations that might be the result.

C. Seeger: Science has progressed beautifully, and with its spinoff, technology, over the last several hundred years by clinging to verifiable experimental stuff. Philip Morrison said, "Physicists explore questions when they have a tool with which to do it, and they don't bother going for something for which they have no exploratory laboratory procedure." It is very difficult to discuss what would happen when society is stabilized after a hundred thousand years and populates a galaxy. That kind of a discussion--too far beyond any solid fact anywhere.

Voice: So is the discussion of the kind of transmitters that civilizations might choose to question.

C. Seeger: Of course, nonsense! When the Kardashev paper came up, I said, "Good Lord, here we go."

T. Kuiper: What can you do? You don't talk about transmitters!

C. Seeger: I'm not talking about Kardashev, but I'm looking with an instrument, that if a Kardashev civilization is up there, we'll pick it up.

Voice: You're saying, "We're looking for something that will be put there for us to recognize."

C. Seeger: I'm looking for a signal within the sensitivity range of my apparatus. It's the best I can do now. The fact that I think it's going to have to be a beacon or a Kardashev civilization to get in, because we have such small antennas, is another matter; that's my guess about the future. But we are working to the limit that we can with present day telescopes with our technology. That's all we are doing. That's the ship we've gotten on, it's got so much fuel, it will get back in a certain amount of time, we hope.

T. Kuiper: You have, of all reasonable possible choices for the kinds of things you might look for, you've made a choice! A choice driven by a scenario!

C. Seeger: Sheer power. Believe me. When we talk about pulses and carriers, we're talking at the limiting sensitivity to those two extremes of intelligent signals.

J. Tarter: I propose that we're not doing anything, that the field of astronomy hasn't done traditionally for the last several hundred years; that is, we're predicting on the basis of what we understand. Astronomically, we've done this historically, and almost every time we've made a prediction from what we know to what we don't know, we have been wrong! But we have made progress by making the instruments to check the predictions, and I expect that some of that will carry over to SETI. It's a fine old tradition; I see no reason to change it.

T. Kuiper: I challenge your comment about predictions, because as astronomers and physicists, and astronomers are applied physicists, we normally proceed by making predictions on the basis of our own accepted theories. Once you start mucking about, talking about motivations of civilizations, you're completely outside the domain of our accepted discipline. You're talking science fiction.

G. Verschuur: I'd like to respond to that. I don't believe that anything has been predicted in astronomy. Radio astronomy grew up because there were radio telescopes around because somebody built radar. Essentially the equipment was there, and they said, "Oh, let's point them." Galileo didn't predict there was going to be anything around Jupiter; he had a telescope and he pointed it! So that's what we're doing with SETI. We're just looking because we have the equipment lying around, except some people like Paul Horowitz here are going to a lot of extremes to build something extraordinary; but predictions aren't involved.

J. Tarter: No, I think that they are involved.

G. Verschuur: If you want to predict anything, why don't you use the data that's right around us and recognize, for example, that dolphins are conscious and communicative.. We don't want to look at them, they're too close by, and so we would rather do astronomy.

J. Tarter: Predictions have been a big part of it. You don't sell to a funding agency a new instrument on the basis of the new things it will discover, you predict that it will solve old problems.

G. Verschuur: Which is a lie!

Jill Tarter: Astronomy has made wonderful progress doing this!

K. Kellermann: That is the unfortunate thing about our whole current philosophy of funding science. All of the exciting things that have come out of new instruments have been new unexpected discoveries, but in order to build them, or to get to use them, you have to invent some theory to test.

J. Tarter: That's exactly the point! What is wrong with continuing or predicting somewhat anthropomorphically on the basis of the laws of physics as we understand them? We're making predictions about what to look for. We're building instrumentation tailored to make those discoveries. We may be wrong, but I think we'll make progress. Much more than if we don't do it.

S. von Hoerner: I would like to mention Christopher Columbus. He started out with considerable expenditures, under wrong assumptions, for an impossible goal, but he still discovered America!

W. T. Sullivan: Gerrit Verschuur pointed out that the beliefs associated with SETI become more obvious since the practitioners are trained as scientists. Don't fool yourselves that you don't also have a whole set of beliefs when you're doing normal science. It's only a different set of beliefs you're getting into with SETI, and you can then argue about which is the more valid set.

J. Russell: But I don't have religion!

W. T. Sullivan: How do you define "religion?" Who, may I ask, are in fact the high priests of today's technological society?

J. Tarter: What makes me especially uncomfortable is the remark that we get together to discuss SETI only because it's a religious revival!

T. K. Menon: Please don't forget that we have been able to convince the American taxpayer to fund the largest and the most expensive scientific instrument ever built by man, and it is being built for astronomy. We have been able to sell them on that, so let's not fool ourselves by saying that we have not been able to fool the public.

W. T. Sullivan: Well, that only says we're good sales people.

T. K. Menon: The most expensive instrument ever built by man is being built for astronomy.

W. T. Sullivan: The pyramids were built for religious reasons. I want to say one thing about Barney's earlier comment. I am personally not in favor of looking at galaxies. I included that possibility as just the extreme.

K. Kellermann: I'm not sure that that's right. I recognize the concerns about the motivation of extraterrestrials, but it's bad enough that we extrapolate our technology to the technology of the extraterrestrials. Do we really want to extrapolate our ideas about motivation and culture to extraterrestrials, about what they might want or want not to do? What is important is the laws of physics, astronomy, and mathematics. If the luminosity function of these guys is sufficiently flat, then the brightest extraterrestrial signals in the sky will be very distant powerful signals rather than nearby weak ones. We ran into that same trap in extragalactic radio astronomy. The early radio astronomers spent years looking at nearby galaxies. In fact the brightest radio sources of the sky are very distant powerful galaxies, not nearby weak ones. We've got to look at galaxies. The arguments against galaxies may be right. But we've got to look!

W. T. Sullivan: Don't conclude from what I said that I feel that the sociology and economics that we try to extrapolate out there are as soundly based as the physics and chemistry. It's just that I hear people saying we're getting into "beliefs now, rather than lack of beliefs, and I just want to temper that.

F. Schwab: In a sense this is a new area of astronomy. This is applied astronomy. I guess there's been navigation, but most other astronomy doesn't have much of an effect on society.

M. Papagiannis: I would like to share with you an idea that occurred to me during this meeting, which I believe could explain why we have not yet managed to receive any radio signals from other advanced civilizations in the Galaxy. It is generally assumed that there must be hundreds of thousands of advanced civilizations in the Galaxy currently active in interstellar communications, because if there were only a few, it would have been very difficult to justify a radio search at this stage. The large number of advanced civilizations in the Galaxy might very well be true, but the flaw of this argument appears to be that it also assumes, without clearly stating it, that all these civilizations are operating beacons trying to signal their presence to emerging civilizations throughout the Galaxy, which I doubt is true. Let us then examine an alternative scenario which seems to me to be far more plausible.

If indeed there are hundreds of thousands of active advanced technological civilizations in the Galaxy, practically all of which must be older than our own, they must have established since long ago a galactic network of interstellar communications. These contacts will probably be made with highly directive beams which would be nearly impossible for us to intercept, not being in their path.

New civilizations are likely to engage in radio searches long before they would undertake any random transmission to unknown audiences. Actually, if exhaustive radio searches failed to produce any results, it seems rather doubtful that they would ever begin to operate galactic beacons for emerging civilizations. On the other hand, if they were contacted by an older civilization they would probably also be invited to join the galactic network, which would inform them about the locations and other particulars of all the network members, issuing to them sort of a galactic telephone book.

But how are emerging civilizations going to be invited to join this galactic network? It is highly uneconomical and therefore very unlikely that all the members of the galactic network would try to contact them, not to mention the fact that the finite speed of light would favor the nearest civilization to perform this duty. It seems reasonable, then, that the network would divide the Galaxy into regions centered around each of its members, who would have the responsibility of looking for emerging new civilizations in their region.

Advanced technological civilizations would be able to know to a distance of hundreds of light-years what stars have planets with an atmosphere and liquid water and show spectroscopic evidence of biological activity. These select locations would remain under continuous observation for signs of an emerging technological civilization, and it is even possible that they might dispatch interstellar probes to the most promising of these sites for closer observation. When a new civilization has passed certain tests and criteria, such as long stability, no nuclear wars, etc., the older civilization that has jurisdiction over the region would simply contact them and invite them to join the galactic network.

In summary, this scenario, which might be called "the galactic regional jurisdiction hypothesis," explains why we have not been able so far to receive any extraterrestrial radio signals. Neither the emerging civilizations nor the members of the galactic network are likely to operate beacons for new civilizations. It also shows that the lack of any radio signals does not imply that the Galaxy is void of advanced civilizations interested in interstellar communications. Finally, I believe that we ought to continue our radio searches because we do not know when we might be contacted by our regional headquarters. In parallel, however, we must try to put our civilization in order, because this might be an essential prerequisite for receiving an invitation to join the galactic network.

B. Oliver: I fail to see any reason to expect the luminosity function of artificial transmitters to remotely resemble the luminosity function of stars or anything else of natural origin. I would suspect that the luminosity function of artificial transmitters is sharply peaked at a value of around the maximum that they can get funding for. Their Appropriations Committee won't tolerate anything bigger than a certain amount.

W. T. Sullivan: What if there is a characteristic luminosity then?

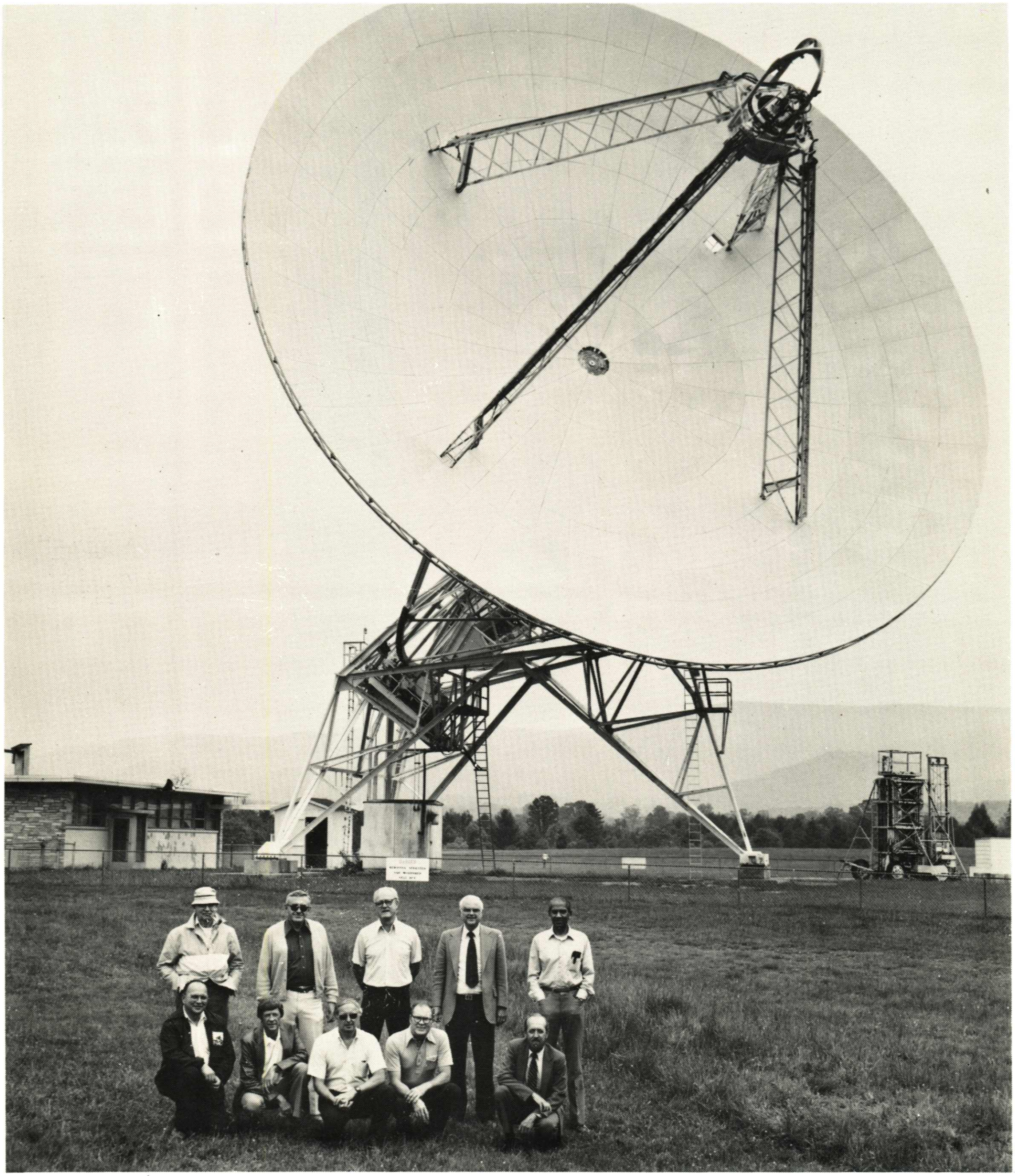
B. Oliver: That's right; a characteristic luminosity, and so I don't see any reason to have a power law like stars do. Second, I would like to add a scenario to what Mike Papagiannis just brought up. This is due to

John Wolfe. He says, "We pick up a beacon and after great anguish and soul searching, we reply. It contains instructions on how to reply; so we do that. After waiting out the round trip light time, the message comes back: 'When we have evidence that you've constructed your beacon, we'll be back in touch with you.'"

W. T. Sullivan: Mike, your scenario would imply a search strategy with a low duty cycle with a directed beam that covers a thousand stars--a hundred seconds on a star and a hundred seconds on the next star. This would then imply that we should look at nearby stars, checking them and rechecking them so that we can have a coincidence of time.

M. Papagiannis: Right. It's a certainly different philosophy which requires a different strategy. But I think that if there is indeed a society of civilizations in the Galaxy and we are now trying to break in and make contact, they must have made contact with each other a long time ago! Therefore, I think it would be unreasonable to expect that all of them would be still beaming everywhere in all directions. It would make more sense that your local civilization would be in charge of its neighborhood and will try to notify other civilizations.

B. Oliver: One thing that relates to that, which I have pointed out in an article in Icarus, is that if you assume a random distribution of civilizations in the Galaxy there must have been hundreds of pairs of civilizations in the history of the Galaxy that were fortuitously close in some point of their evolution. If they are at similar stages of evolution at that time and, say, within a light year or a fraction of a light year of each other, they could hardly help but discover each other. The proof positive that there was extraterrestrial life would probably spur both civilizations into making much more dedicated efforts than ever before toward finding still more distant ones. So I can see these nuclei forming and the network spreading, and I don't consider a Galactic Club at all a far-fetched idea.



The Howard E. Tatel Telescope, used in Project Ozma, and part of the Ozma team. (*Front row, left to right*): Bob Viers, Dewey Ross, Bill Meredith, Troy Henderson, and Bob Uphoff. (*Back row, left to right*): George Grove, Fred Crews, Omar Bowyer, Frank Drake, and Kochu Menon.

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