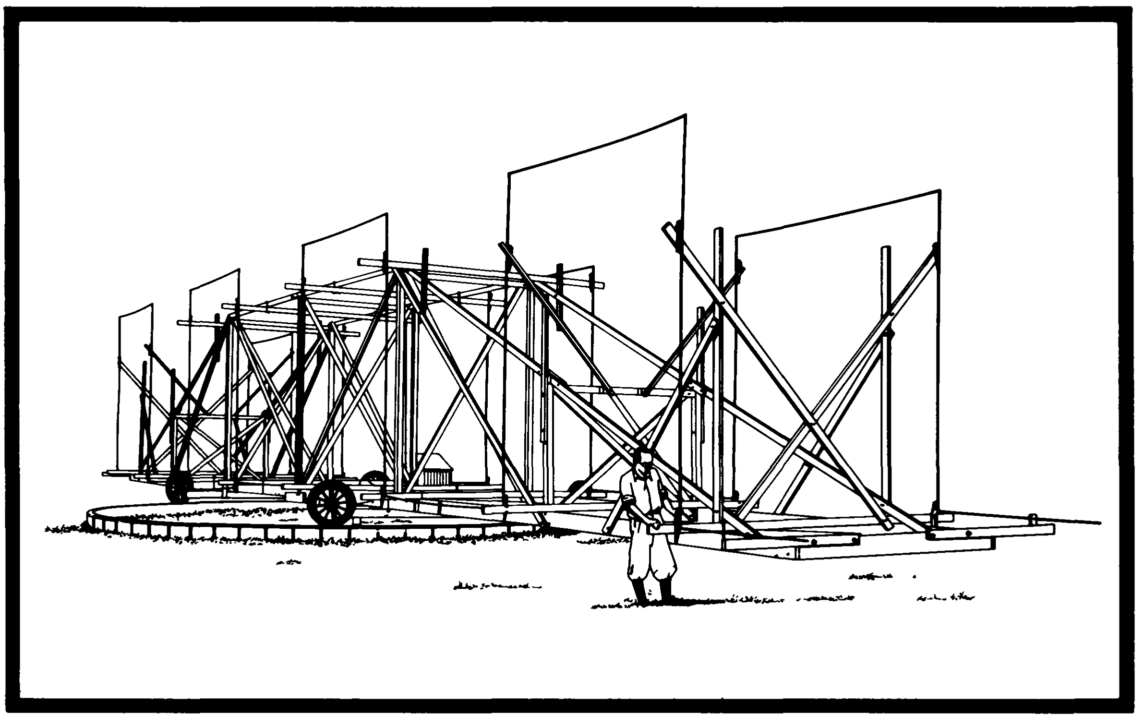
SERENDIPITOUS DISCOVERIES IN RADIO ASTRONOMY

Proceedings of a Workshop held at the National Radio Astronomy Observatory Green Bank, West Virginia on May 4, 5, 6, 1983



Honoring the 50th Anniversary Announcing the Discovery of Cosmic Radio Waves by Karl G. Jansky on May 5, 1933

Edited by K. Kellermann and B. Sheets

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Workshop No. 7

Distributed by:
National Radio Astronomy Observatory
P.O. Box 2
Green Bank, WV 24944-0002 USA

The National Radio Astronomy Observatory is operated by Associated Universities, Inc., under contract with the National Science Foundation.

An early radio astronomer was Janzen, who used the telephone to hear noise coming from the centre of the universe. Rebel heard it too. After WW II a couple of radar operators, Lowel and Ryan, picked up a lot of second-hand stuff cheap and took off . . . .

From a student paper on the
history of radio astronomy.

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PREFACE

Several years ago Bernie Burke pointed out that the 50th anniversary of Karl Jansky's discovery was approaching, and he suggested that some sort of commemoration should be organized. Ron Ekers and I discussed various possibilities with Bernie, Arno Penzias, Woody Sullivan, and Bob Wilson, and about a year ago Ron Ekers and I decided to hold this workshop. We didn't get around to serious planning until early 1983, and then we learned that Penzias and Wilson were also planning a similar event at Bell Laboratories. It seemed to us more appropriate to hold this commemoration where Jansky worked at Bell Labs, but Penzias and Wilson were quick to realize that it would be less work for them if we met at Green Bank. That is why this workshop was held in Green Bank! Bob Wilson and Arno Penzias did, however, share the burden of organizing the program. Regrettably, however, telephone company business prevented Arno from attending the workshop. We also had considerable help and advice from Woody Sullivan, who displayed, at the workshop, pictures and copies of many interesting papers relating to Karl Jansky's life and work.

We were very fortunate to be joined by 27 members of Karl Jansky's family including his widow, Alice; his son, David; his daughter, Anne Moreau Parsons; David's wife, Tedi Ann; Karl Jansky's four grandchildren, Gregory and Christopher Jansky, and Pamela and Karla Parsons; Karl's sisters, Helen Sanford and Mary Striffler; his brother-in-law, Austin Sanford; sisters-in-law, Marguerite Jansky (wife of brother C. M. Jansky, former President of the IRE) and Margaret Jansky Fable; nephews, Curtis and Donald Jansky; and Donald's wife, Mavis; nieces, Mary Beth Jansky Murphy, Marguerite Froscher and her husband Clarence; and eight grandnieces and grandnephews, Lee and Laura Jansky, Mary Ann Edwards, Matt Jansky, Robert Anderson, Clinton, Terry, and Judy Froscher.

A highlight of the workshop was a slide presentation by David Jansky who gave us an unforgettable account of Karl Jansky as a scientist and as a father. David's talk and illustrations are reproduced in the first paper of the workshop proceedings. The second paper of these proceedings, by Karl's daughter, Anne Moreau, was not presented at the workshop, but she has prepared a delightful manuscript sharing with us her childhood memories of growing up in Karl Jansky's family. We are all grateful to the entire Jansky family who took the time to come to Green Bank from all over the country to tell us firsthand about this remarkable man, his life and his research.

The story of Karl Jansky's observations and analysis, followed by more observations, and finally the realization that he had detected radio noise from the Milky Way is a classic of scientific research which George Southworth has compared to a Sherlock Holmes detective story (Scientific Monthly 22, 55). Contrary to many popular versions of the story, Karl Jansky was not a radio engineer who stumbled on cosmic radio noise. He was trained as a physicist, and when he went to work at Bell Labs, he wrote his parents that because he had not studied engineering, terms such as attenuators, double detection, etc. were almost foreign to him.

Karl Jansky had developed a keen interest in astronomy and he fully understood the significance of his discovery and how it should be followed up. His discovery of cosmic radio noise was by no means ignored, and created considerable interest throughout the scientific community as well as in the popular press. In fact, Karl Jansky probably received more immediate popular recognition than has any subsequent radio astronomer. Following the URSI presentation in April 1933, his work was reported on the front page of the New York Times and in many other newspapers. He was also interviewed on the radio which rebroadcast his "star noise". By no coincidence, it was pointed out that the signals from his New Jersey receiving site were sent to the New York broadcasting studio by AT&T Long Lines. As was the case following the 1979 Nobel Prize award to Penzias and Wilson, the Bell Laboratories was not unaware of the popular appeal of astronomical research at the Telephone Company, but in circumstances remarkably similar to Jansky, Penzias and Wilson also found it difficult to break away from their day-to-day responsibilities at the Laboratories to devote more time to their astronomical research.

Jansky's experiments were by no means ignored and received considerable attention in engineering as well as astronomical circles. Harlow Shapley, then Director of the Harvard College Observatory, wrote to Jansky in 1934, and apparently considered continuing the research at Harvard. At Caltech, Potapenko and Folland successfully reproduced Jansky's results in 1935, and Fritz Zwicky urged that Caltech build a large radio telescope. But it appears the gap between astronomers and radio communicators was too great. Everyone was interested, but no one seemed to know what to do next.

Throughout the workshop there was considerable discussion as to why Karl Jansky did not continue his "star noise" research after 1933. We heard from his wife and children, and from his friend and colleague, Al Beck, and heard quotations from letters which Karl wrote to his father about his research. History may never fully appreciate the complex issues involved, but we can piece together some of the story.

The years following 1933 were difficult ones; the country was in the throes of the Great Depression. Bell Laboratories had to reduce their technical staff, and for a while there was concern that the whole laboratory where Jansky worked might be closed. Jansky enjoyed his work at Bell Labs and considered himself lucky to be working at one of the premier research laboratories in the world. His immediate superior, Harald Friis, may not have fully appreciated the importance of Jansky's discovery and he apparently did not encourage Jansky to pursue this line of research. While clearly disappointed that he did not have the opportunity to continue his astronomical research, it appears that Jansky was not unhappy with his work. Somewhat later he, like nearly all other scientists and engineers in the U.S., became involved in the war effort, and for this he was honored. By the end of the war when things started to return to normal, his health had begun to fail. Opportunities did arise for academic positions, but Jansky knew that he was dying, and that the welfare of his wife and two children would depend on his relatively secure and well-paid position at Bell Laboratories. Also by the end of the war, communications technology was already shifting to centimeter wavelengths, and as Fred Haddock pointed out, there was very little radio astronomy one could do at centimeter wavelengths until the technology of the 1950's had been developed.

If Karl Jansky were alive today, he would take great pride in the fact that many of the objects which dominate the current astronomical literature were discovered directly or indirectly as a result of radio observations. Quasars, pulsars, active galaxies, giant molecular clouds, and the cosmic background radiation can be (and in some cases were) observed in other parts of the spectrum, but they went unnoticed until their radio emission was detected.

Perhaps even more significant, however, is that Karl Jansky was the first man to look at the universe outside the traditional optical wavelength band. The modern astronomer uses not only radio waves, but the infrared, ultra violet, X-ray, and gamma portions of the spectrum - an increase from one octave to about a factor of 10^{12} in wavelength - and Karl Jansky started it all only 50 years ago! It is appropriate that the "jansky", which is the unit of radio flux density (10^{-26} watts m^{-2} Hz^{-1}) adopted by the IAU at the 1973 General Assembly, is now being used not only by radio astronomers, but is increasingly replacing archaic units such as magnitudes and UHRURU counts throughout the entire electromagnetic spectrum.

Radio astronomy has come a long way since 1933. In only fifty years the sensitivity and resolution of radio telescopes have both improved by nearly a factor of 10^{10}, or a factor of 100 per decade. Along the way as new, more powerful (and more expensive) instruments were built, there have been many exciting new discoveries, mostly serendipitous.

At least in the past, radio astronomy has been a technique-oriented science. Starting with Jansky, the major discoveries have been made primarily by skilled scientists who, because they completely understood their equipment, were able to spot and correctly interpret their unexpected results. Although as many of the speakers at the workshop related, the path toward the correct answer was often circuitous, and there was a strong consensus that major discoveries require the "right person, in the right place, doing the right thing, at the right time." Several speakers also noted that it sometimes helped "not to know too much."

In holding this workshop, we wished not only to commemorate the founding of our science of radio astronomy, but to try to understand the ingredients necessary for new discoveries and, in particular, to evaluate the current climate of our science with its 3-level peer review filter (grant proposal, telescope proposal, and journal referee); large computers, and the increasing division of our science into hardware, software, and theoretical specialists. This form of "regulated" science is, of course, the result of the tremendous increase in the cost of scientific instruments. Jansky's antenna probably cost less than $1000. The VLA cost about $100 million. That represents an "improvement" of 10^5 in 50 years - or a factor of 10 per decade. This part of the workshop was organized as a panel discussion chaired by Ron Ekers, and included the papers by Harwit, Hanbury Brown, Burbidge, Broderick, van der Laan, and Lovell.

We held the workshop at a time coinciding with the 50th anniversary of the front page headline which appeared in the May 5, 1933 edition of the New York Times which read:

NEW RADIO WAVES TRACED TO THE CENTRE OF THE MILKY WAY

Jansky's discovery, along with the other Bell Labs discovery 32 years later, were probably the only times that radio astronomy made the front page of the New York Times, so in that sense it has been somewhat downhill ever since. A casual glance at the other headlines from that 1933 edition shows distressingly familiar problems including reports of an airplane crash, a kidnapping, threats of war in Europe and Asia, financial problems in the League of Nations, trade inequalities, farm subsidies, and presidential optimism about economic recovery. The price of the newspaper was only 2¢ compared to the present day price of 30¢. (Using this to calibrate the dollar, we may conclude that the real price of the VLA is only 10,000 times greater than Jansky's antenna.) In 1933, Prohibition was repealed; the U.S. went off the gold standard; Babe Ruth hit the game winning home run in the first All Star Game; Franklin Roosevelt and Adolph Hitler had just come to power; and here in West Virginia a young man, Jennings Randolph, went to Washington as a Freshman member of Congress. Babe Ruth, Roosevelt and Hitler are gone, but Jennings Randolph is still in the U.S. Senate! On the scientific scene, Anderson and Millikan discovered the positron, while Dirac and Schrödinger won the Nobel Price in Physics.

Most of the papers reproduced in this volume were transcribed from recordings of the oral presentations. We have tried to retain to a large extent in these written papers, the informal nature of the workshop, including audience interruptions (relevant as well as irrelevant ones) and the discussion following the presentations. To help the reader follow the speaker's remarks, all audience comments and questions are printed in italics in the text. In deference to the reader's sensibilities, the authors and editors have edited the transcripts in an attempt to make the written version conform somewhat to the rules of English while trying to retain the flavor of the original talk. In the end, the style of the papers varies quite a bit, depending on how alarmed the author was upon seeing the original transcript of his talk. Some authors chose to rewrite their papers in their entirety, preferring to emphasize historical accuracy and understanding. We have also included a paper, originally published in the Proceedings of the Astronomical Society of Australia by John Bolton, describing the early radio astronomy research in Australia.

All of the authors have given generously of their time to research their talks, and to edit their manuscripts. George Seielstad has read all of the manuscripts and has corrected numerous errors. Any remaining errors are the responsibility of the editors. Many of the papers contain photographs of historical interest; the editors thank the authors for making these available, and Ron Monk, Brown Cassell and Elaine Ollis for their painstaking work in preparing the reproductions. George Kessler designed the cover.

Financial support for the workshop and in bringing the Jansky family to Green Bank came from the National Radio Astronomy Observatory,* Associated Universities, Inc., and the Bell Telephone Laboratories.

K. Kellermann

* The National Radio Astronomy Observatory is operated by Associated Universities, Inc. under contract with the National Science Foundation.

INTRODUCTORY REMARKS

M. Roberts: This is a joyous occasion, for we gather to pay homage to Karl Jansky. I am happy to see that we have an excellent turnout of the Jansky family to aid us in this celebration.

As is appropriate for anniversaries, we will look back and reminisce. The route was exciting for it was unchartered. It is as exciting today. This is an important point to remember during the next three days of reminiscing. It is exciting today for the world is filled with large radio telescopes: synthesis arrays, and large filled apertures with accurate surfaces. Radio astronomy has come of age; we're going into space clearly, a step I'm sure neither Jansky nor Reber dreamed of 50 years ago. I'm pleased to note that this growth is continuing on a worldwide basis.

We here are particularly excited over the great enthusiasm within the government for a dedicated array of radio telescopes that will stretch from Hawaii to Puerto Rico to allow us to do very long baseline interferometry. I expect to see such an array in operation five years from now. This is the time scale that I think will follow from the enthusiasm that we encounter today in the government. So clearly it will be pleasant to look back during the next few days and it will be very exciting to look forward.

The title of this workshop is appropriate, chosen for obvious reasons, "Serendipitous Discoveries in Radio Astronomy," an intriguing title. The word "serendipity" is a relatively new word. It was coined in the mid-18th century, by Horace Walpole, the writer and historian, and he took it from a fairy tale in which three princes of Serendip (an old name for Ceylon) were forever making fortunate and unexpected discoveries. It is not clear to me, however, that the proper meaning of the word "serendip" is fully appreciated. Walpole, in recording this word, states that it should be the word appropriate for making discoveries by accident and by wisdom. It is the last aspect, of wisdom, that we should keep in mind as the various speakers tell of the serendipitous discoveries of radio astronomy. Louis Pasteur said it much more forcefully,

> *In fields of observation, chance favors only the mind that is prepared.*

I would like to introduce to you legislators from the West Virginia State House, who joined us in time for the pictures. It was very kind of them to find time in a very busy schedule, to share part of that schedule with us; they know this is a great occasion and obviously an important aspect of the northern part of Pocahontas County. They will not be able, unfortunately, to spend the afternoon with us. I understand they will have to leave and make another appointment right after being introduced, but it is indeed a great pleasure and privilege to first introduce Senator Jae Spears from the West Virginia State House, and then Delegates Joe Martin and Charlie Jordan.

Senator Jae Spears: I'd like to first comment on your astuteness in noticing that we did arrive in time for the picture. All I can say about that is "At least you don't have any dumb legislators!" I do feel a great privilege to welcome you to the Twelfth Senatorial District of West Virginia of which Pocahontas County is a part. It happens to be the largest senatorial district area-wise east of the Mississippi. And it also happens to be the most beautiful. I am delighted to say that because I understand we are welcoming the brightest and the finest of radio astronomers from throughout the world, and I'm very, very pleased to extend our hospitality to you and to tell you how welcome you are in our area. I am also very pleased to be allowed to represent what I consider to be, and I'm sure all of you do too, one of the most sophisticated scientific facilities in not only West Virginia, the United States and even the World. I think it is particularly fine that this place, during our talks of war and our unfriendliness with other countries, is available for use throughout the world without restriction. It shows we can work together. This facility and its personnel have brought so much to this area. Pocahontas County has certainly had an enrichment program just by having it here, and we welcome that enrichment. However, we don't take a step backward in what we have to offer. We have to offer a way of life which you can't find many places in the world. We have an honesty, we have an integrity, we have a forthrightness that you can't purchase in very many places, so we're proud to have you and proud to be a part of your program.

Joe Martin: First, Mr. Director, it was not the picture -- it was lunch! I'll be very brief; I will simply add my voice to the Senator's and tell you that I am very happy to have you here in this part of West Virginia in our district and to tell you that you certainly are welcome here.

Charlie Jordan: Now I have to follow two like that. She came for the picture and he came for the lunch! I don't know what to say except that I came for both and some people told me there were a lot of votes over here. The support personnel here does translate into a lot of votes, so as the Senator said, you don't have any dumb politicians representing this district. But I, too, would like to take the opportunity to welcome you here to our part of the world. We're very proud of it over here. As the Senator said, she represents the Twelfth Senatorial District, Joe and I represent the Twenty-Seventh District which Randolph and Pocahontas Counties happen to be a part of. That is probably the largest delegate district, maybe in the country too. We're very proud of this part of the country; we're very proud of the observatory. I probably don't know as much about it as I should and I intend to learn more in the future. I assure you I will return on a later date; I have a 12-year old son who is very interested in what goes on here. I don't know what you and your system has done to incur the wrath of the weatherman here, but I certainly apologize for that. Enjoy yourselves, come back soon, and thank you for having us over.

J. Kraus: I think that this was an admirable sentiment and it is really wonderful to have the politicians interested. Green Bank is in a secure position here, I can see. In Ohio, radio telescopes are an endangered species! Do you know who the first ones to come to the rescue of our telescope were? It was not necessarily the universities; yes, they did around the country, but almost immediately they came from the state government, the state senators and the director of development for the State of Ohio. So politicians are interested. They recognize the value of a high technology facility in their area. It is important. Ohio's days of only building steam shovels and steel products have passed, and it must head toward high tech; and so there are many people interested in helping our telescope survive because they feel it's important.

K. Kellermann: I want to add my welcome to everybody here, and of course we're particularly happy to have so many of the Jansky family - 27 of them! We are especially pleased that Karl Jansky's widow, Alice Jansky Knopp, is here with us today. We are also joined by Karl's two children - his son, David, and his daughter, Moreau Jansky Parsons. We are very glad that you all could join us.

Mrs. Alice Jansky Knopp: I do thank you - thank you so much!

Moreau Jansky Parsons: Thank you very much! He certainly would have loved this party - all of his family, and all his former colleagues in radio astronomy - it's the best of all worlds!

R. W. Wilson: As the Bell Labs representative, I'd like to welcome you here; I know that I'm personally looking forward to hearing about the beginning of radio astronomy and the early developments of the various serendipitous discoveries which have been made. As scientists, we have to be constantly aware; I know that I have been part of some serendipitous discoveries, and I've also been part of serendipitous non-discoveries when I failed to pay the attention that I should have to something.

When I first joined Bell Labs, I worked for Art Crawford, whom you have heard mentioned in the previous talk. He had been Jansky's roommate, and there were a number of other people around on Crawford Hill who had been at Bell Labs at that time. I think that says something for the stability of the group. They had endured the depression together and a large fraction of them stayed together through their whole career at Bell Laboratories.

One of the things I first discovered when I went to Bell Laboratories was the carpenter shop. That was a long tradition, and the man in charge of the carpenter shop when I got there was Karl Clawson; he is the man who had built the original Jansky antenna. At the time I got there, Al Beck, was supervising the work in the carpenter shop which led to the Jansky antenna replica on the front lawn here in Green Bank. It was interesting to see them working on this thing as time permitted; it didn't have the highest priority, I guess, but whenever the carpenters had time, they would go work on it. One of Al's jobs, in fact, was to look over their work and convince Karl Clawson, the carpenter, that he should not apply the many things that he learned over the years since then on how to do it better, and not apply the ready materials that he had, but he should do it just as it was done originally even with the shortages that they had at the time. That project obviously was carried off very well, and you can see the results out front.

Al Beck joined the Labs at essentially the same time that Karl Jansky did and he worked with him over the years. He is here and he has a very direct memory of what went on during those early years.

MY FATHER AND HIS WORK

David Jansky

First of all on behalf of the Jansky family, I want to express to NRAO and Bell Labs our deep appreciation for your thoughtfulness to include us on this occasion. You have accomplished a feat that might never have occurred again, and that is to bring this family together for a reunion.

I believe there are only two of us who are not here this morning; and one of those is in England. This family in years past when all the children of Mother and Father Jansky were still living, regularly got together for reunions, even during the war years of the forties. I personally can recall trips to Wisconsin, Oak Moorings, which is at Shadyside in Maryland, and of course gatherings in Little Silver, New Jersey. Many of the family here today are of the new generation. For them, this is a new experience; I can't remember really the last total reunion but I do know there was a gathering at Mary Ann's wedding, and that was nine years ago. Before that, it had to be Donald's wedding - twenty years ago. Once again, we all thank you!

The logistics of bringing this family together have been involved and at times trying to say the least. The letters and phone calls have been many. It is a tribute to Ken Kellermann that we are all here. One relative indicated last week that after all we had put Ken through we ought to do something for him. Frankly, I told her, I think the best thing for Ken will be when we all leave!

Judging from what I have seen over the past thirty three years, one would think that few photographs of Karl exist. I will show you a group of pictures taken over the years mostly by family members and some other pictures of interest. Photography has changed greatly over the last sixty years. Picture taking is much simpler today. Years ago, many of the photographs were home processed and many were of the contact print variety, that is, the negative size equals the final print size. Photography was one of Karl's hobbies. He used a plate film camera, and later an early model of the 35 mm type. It was a *Weltie*, I believe. He processed many of his own pictures, some of which I have included. The source of pictures I have used were a hodge-podge of sizes, many were curled and/or faded. If nothing else, this effort will help preserve these images for a long time.

For any photographers present, I used close-up lenses of +1 thru +7 diopters, Kodachrome 64 daylight film, a tripod, and a sunny day (that was the hard part). I believe in the last 8 weeks on the shore we've had one sunny day on the weekend. I opened the lens one-half to one stop wider than indicated by the meter and shot at 1/125th of a second. Please excuse the mistakes. You'll see them. It is the content I was after. I had interruptions like phone calls (you can guess from whom), children, and a neighbor's dog that paraded through the piles of pictures. I'm sure I don't have all the facts straight, I think really my mother should be up here for some of the explanations. Figure 1 is a popular picture. It was taken by Arthur Gregory in Red Bank, New Jersey and is the last known photograph taken. It has often been credited to Bell Labs; but I don't know whether Bell Labs actually authorized the portrait or not. It's the best portrait of Karl that exists.

I don't know when Figure 2 was taken, probably about 1946 or 1947.

Alice Jansky Knopp: About one year before you got braces.

Figure 2 has been shown to death, and really I imagine the reason is because not too many pictures of Karl exist with the actual antenna to make it a logical presentation to the uninitiated. But it was a good one. And I show it because I'm going to show you another picture later on.

Figure 3 is a blow up of that same picture, it was done by Moreau Jansky, for a presentation that he made, and it was a high quality blow up, so he did a very good job. The picture shown in Figure 4 is from the ad, December 1953, by BTL, which appeared in 22 scientific publications and look what they did to him.

AJK: Changed the knickers to long pants!

And gave him a full head of hair! We can all laugh. This appeared in 1953; I can understand why it was done. My apologies to Bob Wilson and the rest of Lab people who are here, but they're going to hear it! This was a very small picture in the ad, by the way, because the title of the ad was "How Silent was the Night?", and the top half, I should have shown it I guess, had a silhouette of a man, a dog and a boy looking at the stars, and it was a very clever ad.

R. Wilson: David, I've seen another copy of this same picture, or two copies together, one is the original picture with a circle around Karl, and it says "Remove Man!"

The next four pictures (Figures 5-8) are not seen often and they originally appeared in a popular radio magazine in May or June 1933. The first is a picture, that is another picture, of the antenna involved. But it's such a long shot that it never got used because the detail would be too small, but it did appear in that magazine.

AJK: That was known popularly at the Bell Laboratories as "Jans's merry-go-round."

In Figures 6 and 7 he is shown listening on his short-wave receiver and examining the recorder records. It was a primitive receiver, really. Figure 7 was taken at a different time than the big shot.

Figure 8 also appeared in a local newspaper for a two-part Sunday supplement on radio astronomy. The writer came to me and he decided he would like to use that picture. It was actually a story that was started out by discussing the VLA and that led him to find out by looking through his morgue files that the science was actually born right in Monmouth County, and so he looked me up to get the story.

Figure 9 shows a picture that you have all seen of his two receivers and the two strip chart recorders, the long wave one is on the left and the short wave is on the right. Figure 10 is a long shot of the Holmdel Labs looking east about 1950 and in the foreground was a grounding project that was underway at the time. The building on the upper right is the caretaker's home. Someone asked me that last night, where was it in relation to the Lab, and my recollection is that it was out on the side. The road came down past the caretaker's house and and up into the front of the Lab.

For those of you who are familiar with the Holmdel site now this building was destroyed; it was burned and there is now a lake there that is part of a huge complex of modern four or five story buildings - Bob, how many floors?

R. Wilson: Six, if you count the basement.

Scientists, you know! It houses four thousand people. This site here when they first moved down from Cliffwood Beach had about 15 or so and went up to about 30 which was a constant figure into about the fifties. Now Bell Labs has an employment of over 6 thousand people in Monmouth County alone, on sites all over the county.

Figure 11 shows the Roberts House. It was on the northern edge of the property and was used for labs in the late forties. That's where my Dad had his office. The building in the foreground was put up for a Bell Labs project in the forties. It was destroyed along about the time when they built their modern structure.

I will now turn to a topic I hope you will like. Figure 12 shows a picture of Karl one year old. He was born in Norman, Oklahoma on October 22, 1905 and this picture was taken a year later. Oklahoma at that time was not a state, it was a territory. I cannot find pictures of his early boyhood; the picture in Figure 13, if you believe the date on the back of it, was taken in 1921, and it would put him a little over 15 years old, delivering ice. Somebody asked me if that was the picture with the tongs; I don't see any.

AJK: Ice tongs!

Oh, he's got them on the ice! Sorry! The back of the picture said 1921.

In Figure 14 you see the family homestead, 2117 Jefferson Street in Madison, Wisconsin, as it looked in June 1967. It has not changed over the years as I recall; it's the same building. Figure 15 shows a picture of the university hockey team where he played varsity for three years, and Karl, if you don't know, is right here without any glasses; and my only question is, how do you ever play ice hockey if you don't have eyesight. Good lord! I can't believe it.

AJK: But look at the resemblance to the young Jansky's here!

After this shot was taken, and I had prepared this presentation, I found a picture, an action picture of him playing; they played an outdoor game, and hockey in those days was a beautiful game of crisp passing and deft stick handling - not the blood and guts game it is today - you had to be fast, and he was the fastest skater. He was also the star.

AJK: He also got a permanent niche in his chin!

There are many hockey stories, but I'm not going to go into them.

The next illustration (Figure 16) shows Karl wearing his varsity sweater outside the family homestead in 1926 or 27. This picture was a tiny picture to blow up and I thought the resolution came out fairly well.

Graduation, June 1927 is shown in Figure 17. He looks the same in that picture as I remember him.

AJK: *I think that was when he got his Masters, David, in 1935.*

Nope, I have that! Sorry! The next picture (Figure 18) was taken at Devil's Lake.

AJK: *That was when he became engaged!*

You don't want to tell us about that? All right! The next picture (Figure 19) is his twenty-third birthday picture, which would make it 10/22/1928, shortly after he joined Bell Labs. He joined Bell Labs in July 1928, and that's a Bell Labs car.

AJK: *Do you know what it said on the back of it?*

Back of what?

AJK: *That picture.*

Go ahead.

AJK: *"This is what your sweetheart looked like on his twenty-third birthday."*

I like that coat. Could be an undertaker!

Figure 20 was taken on the 6th of June in 1933 and they had been in this site roughly two years. What is interesting is a picture taken in 1960 of the exact building and the people sitting in their exact position, those that were still living, of the original picture. This is an interesting picture. Art Crawford is seen on the left, England who was a co-director, and Harold Friis next to him. Of course Karl was front center; Merlin Sharpless was to his left, over on the end is Al Beck - gee, Al - you look the same! The next picture (Figure 21) taken at Bell Labs has no date, and I would guess it was in the early thirty's. It was one of three or four shots taken and this appeared to be the best of the lot. Figure 22 shows the house on 22 Bergan Street in Red Bank where Karl lived. This photo was taken this year; the house is basically the same, except the siding is aluminum. It was run by Nora Long, whom they nicknamed "Nonie." She had a family of three children and she took in four young engineers.

AJK: *She was a recent widow.*

The four young engineers are shown in Figure 23. Art Crawford is the one wearing the conservative jacket. Art was from Ohio State, Merlin Sharpless was from Minnesota, and Karl Feldman also from Minnesota. Is that a Phi Beta Kappa key Karl is wearing?

AJK: *Yes it is.*

This picture was taken in Nora's back yard.

Figure 24 shows a picture of Karl on the steps; he stayed here from when he joined Bell Labs, July 1928, until his marriage in 1929. Although Art on the telephone the other day said he stayed here five years.

AJK: *Oh dear!*

Karl went back home...

AJK: *Oh dear!*

... in December of 1928.

AJK: *Is this necessary?*

I don't know what he went back for!

Figure 25 was taken in front of a train and I don't know where. In Figure 26 Alice and Karl are in front of 2117 Jefferson Street, and it is December 28. Were you still in school then?

AJK: *That's why he came back!*

Oh, I knew that! Here they are on the running board of a current car (Figure 27).

AJK: *Oh, I suppose it was the family car. A Studebaker.*

Eventually, August 3 of 1929, they were married at the family homestead, Figure 28. Now, family, we'll try to tell them who these people are. That is Curtis, the child in front of Karl; and you see Marguerite. I don't know who this is -

Family: Helen.

That's Helen?

AJK: *That's Don Britton.*

Is this Cleo?

AJK: *No, that's - she was Helen Hanson Mason.*

Anyway - my father looks in shock. It happens to us all!

Figure 29 was taken in Mount Vernon in September of 1929. Do you recall why you went there? Was this on a vacation trip?

AJK: Margie invited us to come down when we were married - this was the Labor Day weekend after we were married in August, and I had never been to Washington. We were invited down and they showed us the sights. We went down to Mount Vernon where this picture was taken.

There were others but this is the only one I included. That's a good one. They lived in an apartment in Red Bank on Broad Street and Leroy Place. Figure 30 was taken this year.

Anne Moreau Jansky Parsons: Oh - your first apartment!

Is that the one?

AJK: NO!

Yes it is.

AJK: It is not! That isn't even Leroy Place![1]

Figure 31 shows Karl in formal attire at brother Nelson's wedding in 1931. It was a formal affair in Malden, Mass. The next picture (Figure 32) was taken in Chevy Chase, Md. while visiting the Moreau Jansky's in May of 1932. That is my sister, Anne Moreau, he is holding, and if the date is correct, that is probably when he delivered his first paper. Here again there was a sequence of pictures of which I thought that one was the best.

All right, here's a family gathering (Figure 33) of which there were many. This was Christmas 1933 in Chevy Chase and family members may or may not recognize themselves. That is Karl on the left, his brother Nelson, Marguerite, Curtis, Nelson's wife Muriel. Young Marguerite on the right, my mother, and the youngest is my sister, Anne Moreau.

AJK: Where were you?

In 1933? You tell me!

AJK: A twinkle in your Daddy's eye!

Not even that! All right, the next picture (Figure 34) shows noon-time activity at Bell Labs. I question the catcher's glove because he never caught when he played in any of our games. What a competitor he was!

AJK: Tell them about the time that he dove to catch the ball and broke his collar bone.

Keep going!

---

[1]Mother was right!

AJK: They brought him into Red Bank to the hospital. I met him and he looked up at me, grinned and said, "Well, I caught the ball and I hung on to it!"

That's the way he was! Figure 35 was taken outside of the home in Little Silver on Silverton Avenue. It's May 1936, and that is yours truly! I think what is significant about the picture is the apron. Alice and Karl moved from the apartment to a rented home on Rumson Road run by a family with a last name of Roosevelt. When FDR got elected, they changed the spelling of their name! That's the only political comment I will make! Figure 36 is a family picture taken in front of the Silverton Avenue home in the summer of 1936. So that places me a little over one, and Anne Moreau about three and a half! Is that right?

AJK: Right!

Next is the Masters Degree (Figure 37)! Karl spent a year at the University of Wisconsin after he graduated to do his graduate work. He went back there for the thesis and collected the degree on June 16, 1936, and here again, that's in front of the homestead.

AJK: And the reason he didn't get his degree the first year that he stayed, was that his work was on static, and there wasn't enough static on which to take data to write his paper. A few years later they used his star noise paper in lieu of a masters thesis, and gave him his degree.

Here's a picture (Figure 38) taken in 1938 in Oconomowoc, Wisconsin, near the Congregational Church. The other woman is Mrs. O. W. Smith who was the minister's wife. That was also my mother's Sunday School teacher.

AJK: Right. When I was growing up!

Next (Figure 39) is an example of Karl's photography. The family cat - Ceasar! This was taken in the apartment. Notice the garage is in the back. Yes, still there today!

AJK: But not behind the building you showed!

We'll let that slide. Anyway, he entered this in a contest, and his comment as I recall was that the judges didn't give it a second look. Do you want to tell the story about the Sharpless cat and all that now?

AJK: The Sharplesses used to dangle a string down from their window from up above -

You were on top of them, right?

AJK: No, they were on top of us.

Oh, Sharpless dangled the string down and Ceasar fell out. He came down and said, "Oh, I killed the Jansky's cat!"

AJK: The cat showed up about three days later.

As an example of some of Karl's photography work, I have used in chronological order, not all, but some of the Christmas cards that he made every year. He did all the work himself. He processed the negatives; he did the printing with a homemade print box, and he did the stenciling work where it was necessary. He even scored the picture to put a frame on it. This was Christmas in 1929 (Figure 40). It was their first Christmas, and the back of the card had a note because it was sent to somebody; it took them six trips to the shore at Sea Bright to get the flash to synchronize right. A comment from Father Jansky that I can't recall was a very good one; but he said this was always his favorite card. Is that right?

AJK: Yes, he was very moved by it. The temporary message was that "it would soon be washed out by the waves but we would always remember."

Next, Christmas 1930 (Figure 41) together with the Christmas tree.

AJK: Notice how slim I was!

Figures 42 through 50 show Christmas cards from 1932 to 1938. 1933 was the first Christmas in the "new home". I remember the 1938 picture. Those books are Readers Digests!

Anne Moreau Jansky Parsons: Those pictures were taken by Karl by holding a string in his hand and when everyone was just right he pulled the string, and that was the picture.

Yes, he did. He controlled the camera by a hidden string. Number 49 was taken during the war. Note the various uniforms. I will point out the dog only for the interest. The dog was found on the Bell Labs property in the trunk of a tree; it had given birth to a litter of puppies. It had been abandoned or had run away, and the people at the Lab took the puppies and we took the mother as a family pet. Obviously, the dog had been beaten, because whenever you got the broom the dog ran and hid.

Anne Moreau Jansky Parsons: It jumped on mother's lap - a great big German Shepherd.

AJK: I would like to point out in that picture that everyone is in uniform except the one person who was doing a tremendous job for the war effort - Karl!

He was a warden one year also, air raid warden was he not? Civil defense.

AJK: He was doing tremendously important war work.

Yes, cub scouts, girl scouts, and the Red Cross! Figure 50 is the last one of the cards I have used, and I only can guess at the date - 1947 or 1948. That's a new dog, not the same one. Figure 51 was taken at the family home on Silverton Avenue. Anne Moreau, I believe you took that picture?

Anne Moreau Jansky Parsons: I think I did.

It does snow in Jersey, in spite of what they say. The next pictures are taken from the only colored picture slides of Karl that I know exist, and if anybody has others, I would sure like to find out. Figure 51 was taken in our backyard on his 35 mm camera, and it shows the quality of Kodachrome film back then. This was their first effort at it, in 1942. Figure 52 was taken in the front yard, and Figure 53 was taken in the backyard, again with Mrs. O. W. Smith. Figure 54 was taken at Camp Ocanicken in August 1942. Here he is building outdoor furniture in our backyard, approximately 1943 (Figure 55). My head's in that picture. Next (Figure 56), we are cleaning up the results of a hurricane in 1944. That was a locust tree that came down. Here he is on a Sunday (Figure 57), I know that, because that's a buttoniere you got for ushering in the church, with our first dog; the dog that he found at Bell Labs. It was about 1943. Nothing really changes, does it? Everybody has to do this.

Figure 58 was taken on the property right behind us. The next picture (Figure 59) was taken on the running board of the famous 1937 Chevrolet looking down Silverton Avenue. Figures 60 to 62 were taken in the mid to late forties. The family spent a week each summer at his brother's summer house in Shadyside, Maryland, called Oak Moorings. It was always a lot of fun, we sometimes had reunions there, swimming, sailing, motor boating.

Guess who needed braces!

AJK: *Yes! And got them!*

These were all taken at relatively the same time at Oak Moorings. In Figure 63 he is receiving an Army/Navy citation.

If I had to choose a favorite, I think I would choose this picture (Figure 64) taken at the shore in Sea Bright in 1929.

I hope these pictures have enlightened you a little bit about Karl Jansky, the man. About his work and discovery, I don't think it can be said better than was said by his brother, Moreau, at the American Astronomical Society meeting, March 26, 1956, at Ohio State. I quote:

> *We should remember that his philosophy of life was that of a true scientist, perseverance in the accumulation of data, objectivity and analysis, modesty, credit to others for their contributions, and a willingness to leave the ultimate evaluation of one's work to the future.*

The pictures, I believe, showed that he dearly loved his family. Some other traits that pictures probably cannot show would include honesty, competitiveness in play, discipline, a definite line between right and wrong, a love of nature, gentleness, and respect. All traits that are essential for successful living in any age. These are the qualities we attempt to pass on to our own children. If I can accomplish that, it is a tribute to the man I called "Daddy".

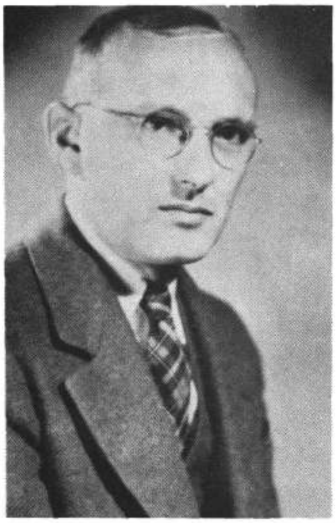


Fig. 1. Last known photo of Karl Jansky.

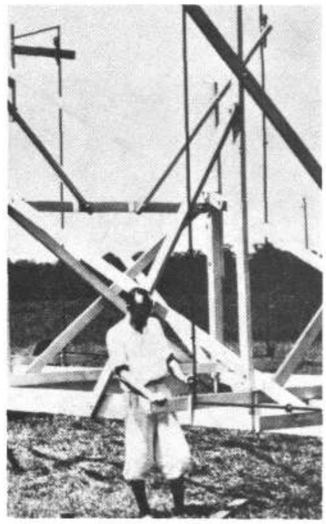


Fig. 3. Blowup of Figure 2.

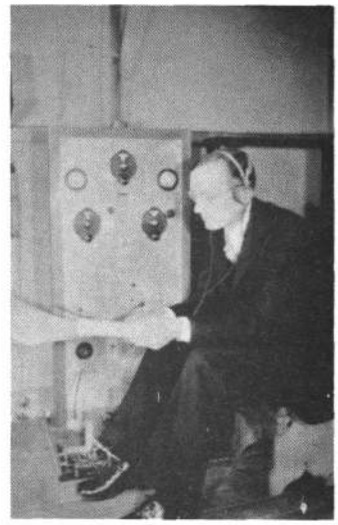


Fig. 6. From a 1933 radio magazine.

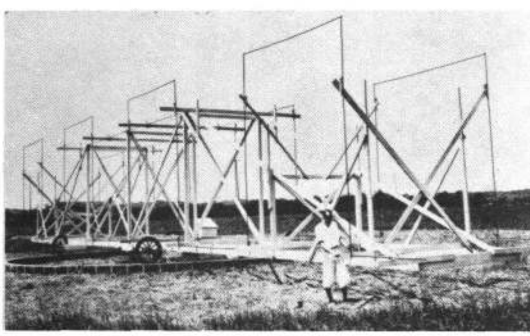


Fig. 2. Karl Jansky with his antenna.

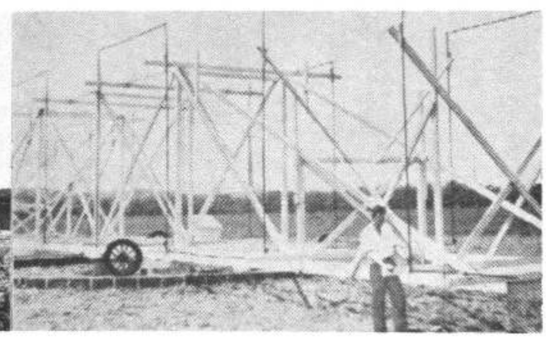
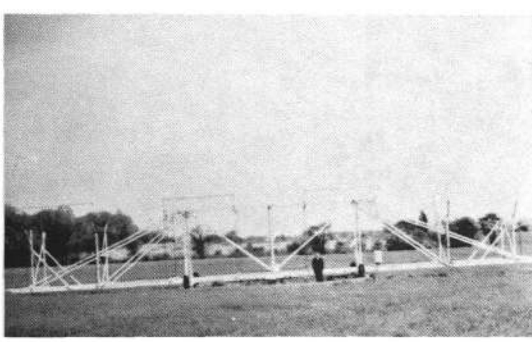
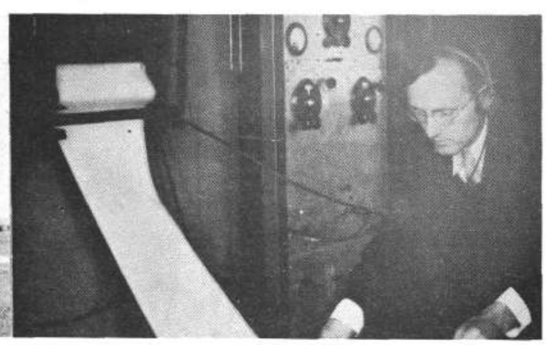


Fig. 4. Picture used in December 1953 BTL ad.





Figs. 5 and 7. Taken from a 1933 popular radio magazine.

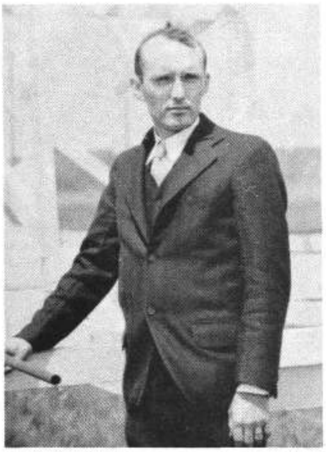


Fig. 8. From a 1933 radio magazine.

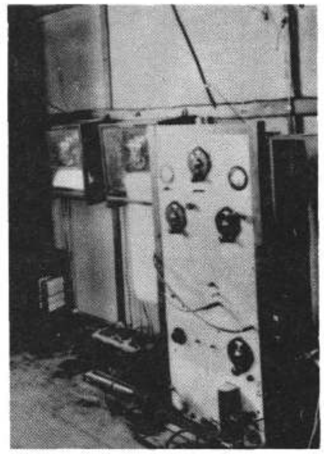


Fig. 9. Two receivers used by Jansky.

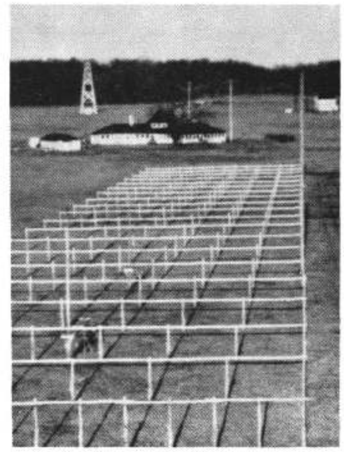


Fig. 10. Holmdel Labs about 1950.

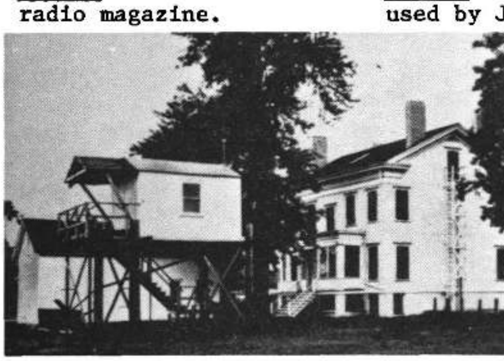


Fig. 11. Laboratory building, in late 1940's.

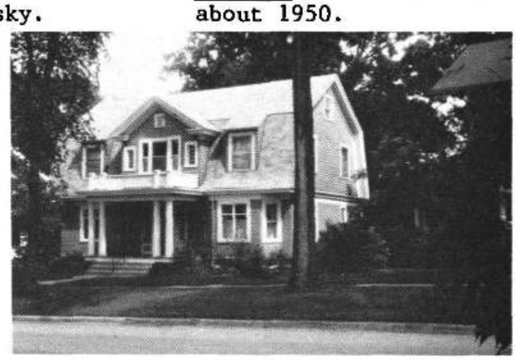


Fig. 14. Jansky family home.

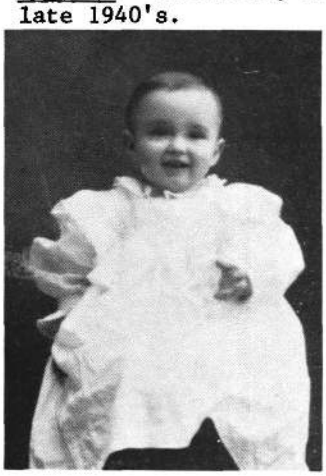


Fig. 12. Karl Jansky at age 1 year.

Fig. 13. Karl Jansky at age 15.

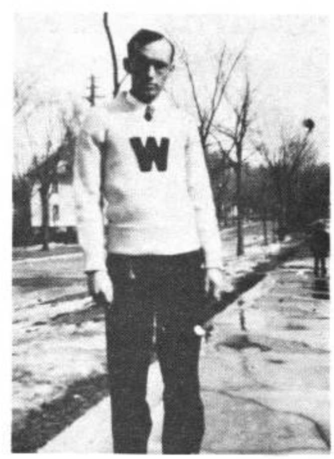


Fig. 16. Hockey star Karl Jansky.

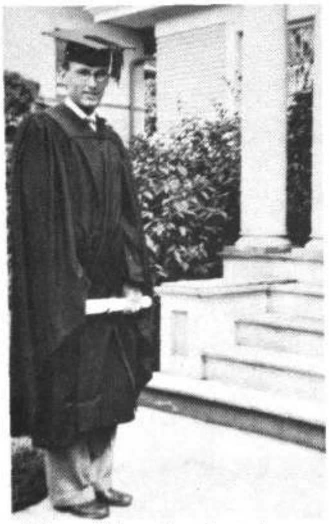


Fig. 17. Graduation U. of Wisc., 1927.

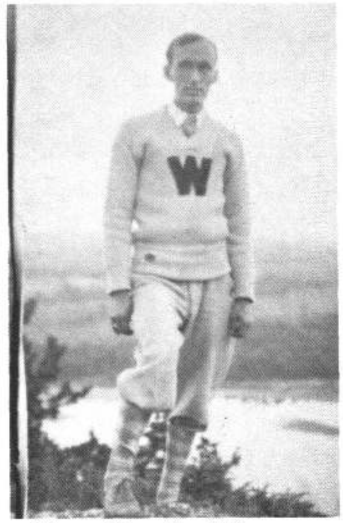


Fig. 18. Devil's Lake, June 1928.

Fig. 19. 23rd birthday, Oct. 22, 1928.

Fig. 15. Wisconsin hockey team. Jansky fifth from upper right.

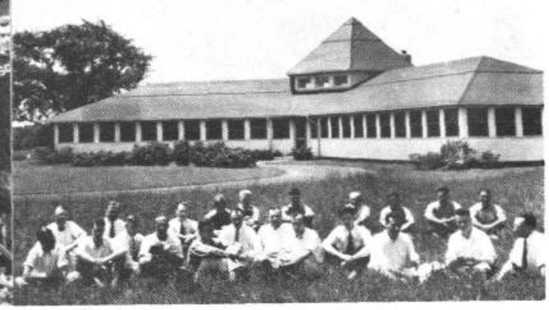


Fig. 20. Bell Labs group, June 1933.

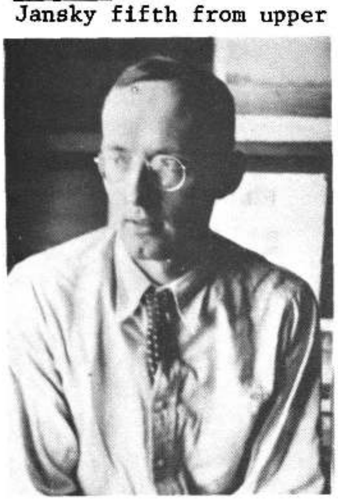


Fig. 21. Karl Jansky, early 1930's.

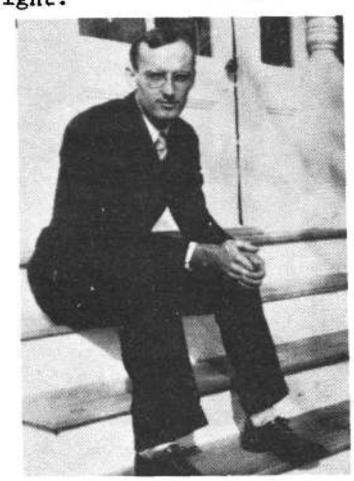


Fig. 24. On steps of his home in 1928.

Fig. 26. Karl and Alice, Dec. 1928.

Fig. 22. House in Red Bank where Karl Jansky lived in 1928 and 1929.

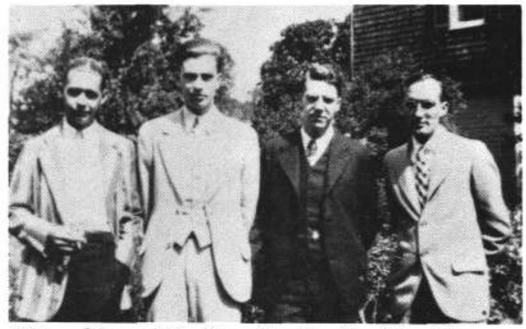


Fig. 23. Art Crawford, Merlin Sharpless, Karl Feldman, and Karl Jansky.

Fig. 25. Karl and Alice, December 1928.

Fig. 28. Family picture, 1929.

Fig. 27. Karl and Alice (family car).

Fig. 29. Karl and Alice, Mt. Vernon, Sept. 1929.

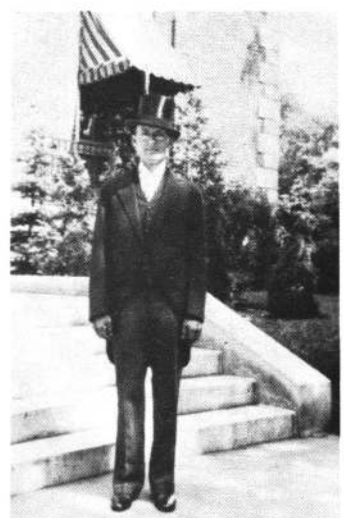


Fig. 31. Formal attire at brother Nelson's wedding.

Fig. 30. Alleged first home of Karl and Alice Jansky.

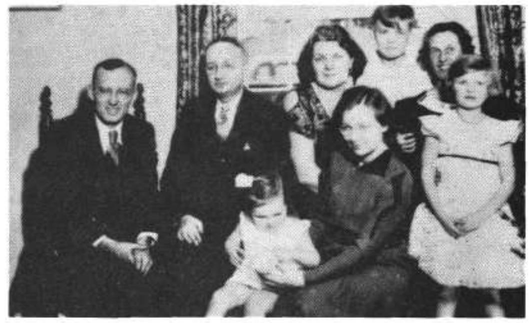


Fig. 33. Family picture taken in June 1933.

Fig. 32. Karl with daughter, Anne Moreau.

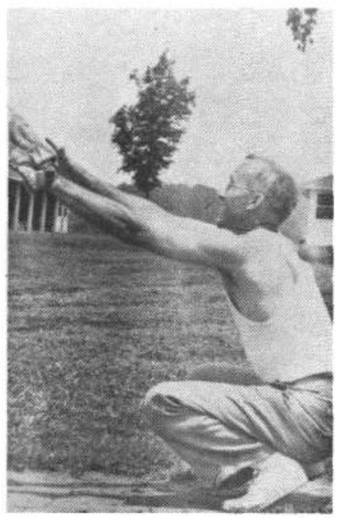


Fig. 34. Noontime activity, Bell Labs.

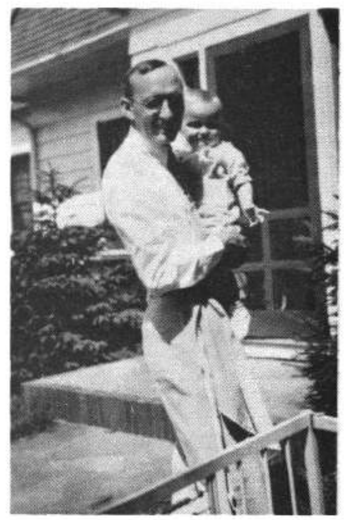


Fig. 35. Karl and son, David.

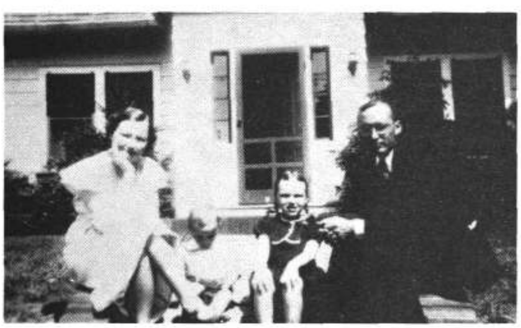


Fig. 36. Family picture taken in summer of 1936.

Fig. 38. 1938 family photo.

Fig. 39. Caesar, the family cat.

Fig. 40. Christmas, 1929.

Fig. 37. Master's Degree, June 1936.

Fig. 41. Christmas, 1930.

Fig. 42. Christmas, 1932.

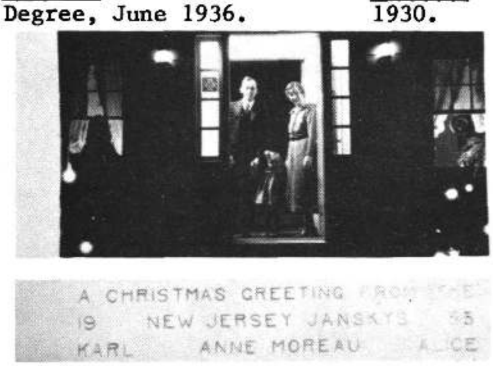


Fig. 43. Christmas, 1933.

Fig. 44. Christmas, 1934.

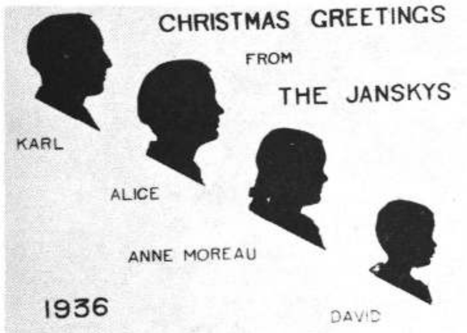


Fig. 45. Christmas, 1936.

Fig. 46. Christmas, 1938.

Fig. 47. Christmas, 1940.

Fig. 49. Christmas, date uncertain.

Fig. 48. Christmas, 1941.

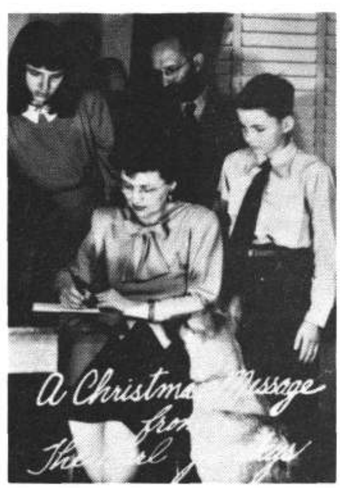


Fig. 50. Christmas, 1947 or 1948.

Fig. 52. Karl and son, David.

Fig. 51. New Jersey snow scene, 1947?

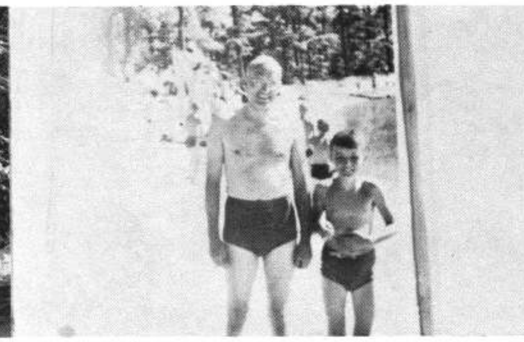


Fig. 54. Camp Ocanicken, 1942.

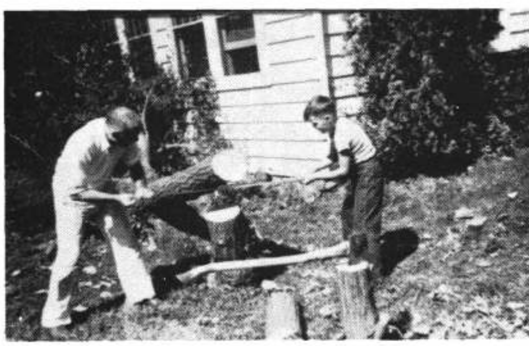


Fig. 56. Cleaning up results of 1944 hurricane.

Fig. 64. 1929 picture at Sea Bright, New Jersey.

Fig. 53. Karl Jansky, 1942.

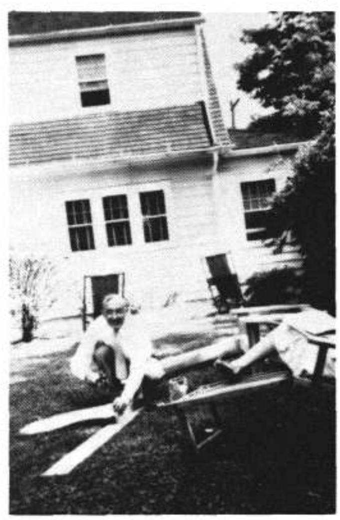


Fig. 55. 1943 or 1944.

Fig. 57. About 1943.

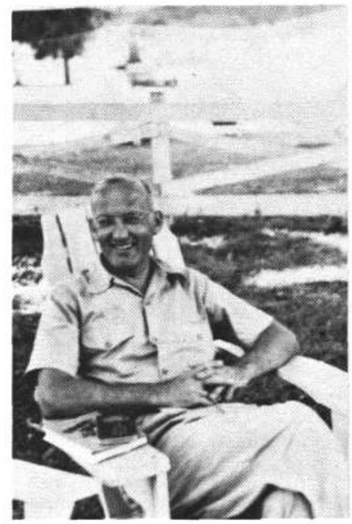
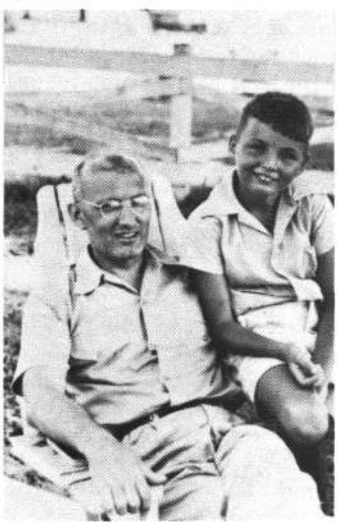


Fig. 58. 1943 or 1944 photo.

Fig. 59. 1944, with family car.

Fig. 60. Oak Moorings, Maryland, late 1940's.



Figs. 61 and 62. Oak Moorings, Maryland, late 1940's.

Fig. 63. Receiving Army/Navy citation, 1946 or 1947.

PERSONAL RECOLLECTIONS FOR THE GREEN BANK SYMPOSIUM[1]

Anne Moreau Jansky Parsons

How grateful and pleased my father would be to know we had all come together to honor this fiftieth anniversary of the birth of radio astronomy. However, for him, the fun of the twenty-seven member "family reunion" and the opportunity to meet and share ideas with his contemporaries and successors in the field, would far outshine the personal honor, for he was, though gregarious, an unpretentious man.

In trying to pull together some comments for the occasion, I must remind you that I was just eighteen (my brother, David, not quite fifteen) when my father died. This may explain why I cannot add anything to what is already known about my father's original research, which was referred to occasionally at home as "Daddy's star noise work." Sometimes, however, we did hear the wish expressed that he would like to resume that work. For many reasons, most of which you know about, this was not to be. The only other connection with his work that we had as children, were visits to the laboratories at Holmdel on weekends when he needed to check on something that he was doing. In the rotunda of the main building, which no longer exists, there were ranks of electrical equipment lined up against the wall. I remember its peculiar "hot wire" smell and its constant low hum. Inside glass cases were fascinating automatic pens that continuously wavered across a moving roll of graph paper in response to what, I don't know. My father explained, but I didn't understand. David and I thought it was great fun to race up and down the ramps that branched off from the rotunda and led out to the various offices of the engineers. As a small child I also remember playing on the circular track left in place when the original antenna with which my father made his discovery, had been removed. This was called "Daddy's merry-go-round" and it made a wonderful balance beam for me.

Holmdel was all farmland then, and we kids enjoyed exploring the fields and woods surrounding the "labs." Because it was such a pleasant place, the engineers and their families sometimes gathered there on a weekend afternoon for a picnic and informal games. The fathers of many of my friends in Little Silver and Red Bank worked in New York City, and had to commute there by train every day. I always thought my father a very lucky man to be able to work in the country in an office full of sunshine with views of fields, woods, and sky.

In March, 1956, which was already six years after he died, my father's older brother, Moreau, his sister, Mary, and I attended a banquet of the American Astronomical Society at Ohio State University. Aunt Mary lived in Marysville, Ohio, and I, at that time, was working in the education department at the Buffalo Museum of Science. We were the family members closest to

[1]This paper was not presented at the Workshop, but was later prepared by the author, Karl Jansky's daughter, for inclusion in these Proceedings. - ed.

Columbus, and so were asked to attend. Uncle Moreau gave a talk, "Beginnings of Radio Astronomy" (a written version of this talk was printed in Cosmic Search 1, No. 4, pg. 12, 1979), which described my father's life and work, and included some humorous anecdotes which served to relieve the seriousness of the topic. Aunt Mary and I sat with Uncle Moreau at the speaker's table, thrilled to share this occasion, which for me, was the first public recognition of my father's work since his death.

It is interesting that the AAS chose this topic for their meeting in 1956, for at that time radio astronomy was still not considered a proper branch of astronomy, and I heard puzzled comments at that banquet, such as, "Well, it was great that he (KGJ) did what he did, but he wasn't really an astronomer." Perhaps astronomers at that point were wondering just how to accept and categorize the practitioners in this growing new field. I remember receiving a gracious note afterwards from John Kraus, who is one of the speakers at this symposium. Ohio State had a new radio telescope at the time, a huge array, which I was amazed to see. I believe that same array is now being used in experiments designed to detect evidence of intelligent life elsewhere in the universe.

I can't pinpoint the beginnings of my father's interest in astronomy, whether or not it predated his discovery, but I know that it persisted afterward through my childhood. He had some basic references on astronomy, including a star atlas, which I loved to read, and, according to my mother, he owned all the textbooks used at the time in the astronomy courses at Princeton, and read them avidly.

One probable effect of his informal pursuits in astronomy was his habit of dragging us all out of bed in the middle of the night to gaze at some celestial phenomenon. On such a night I would gradually awaken to the realization that Daddy was pacing back and forth in my bedroom, leaning far out of one after the other of the windows, which were the only ones upstairs with an uncluttered view of the sky. Then, at the right moment for viewing the "event," he would get me up, and I would run outside with him, while my mother would try to get my brother out of bed. This was always quite traumatic for David, who always resisted any interference with his sleep, and would thrash around in his bed, objecting vociferously to my mother's pleading. She'd call, "Karl, David doesn't want to wake up!" My father would call back from out on the front lawn, "Well, you've just got to get that boy up - he may never get a chance to see this again in his entire life!" Finally, we'd all stand outside, with or without David, depending on who won the battle. Shivering in our pajamas, we'd watch an eclipse of the moon or a colorful display of the aurora borealis. Another indication of his interest was a black box device he made and set up on the front stoop, which enabled us to see sunspots.

My father had a natural curiosity that was evident in our daily life. On Sunday afternoons we often went for rides in the country to look at various properties we dreamed of owning one day, or to explore little-used dirt roads. On one of the latter we beat a hasty retreat one day when my father felt that we had stumbled upon a bootlegger's hangout. He enjoyed tinkering with the car, with radios, with anything mechanical. As children, we developed a faith that he could fix anything, and would bring him our friends' defective bicycles and table radios. This habit may not have been especially appreciated, but he seemed to have endless patience and never complained. Sometimes, though, my mother, in an effort to relieve him of our childish harassment, would remonstrate.

As a father and husband I am afraid he was idolized, though he may not have known it, nor did I realize it while he was alive. As a child growing up, I could only know that I was loved and secure with him. What were the qualities that fostered such a happy family situation?

Like all good fathers, he spent a lot of time with his children. My father excelled in sports, and the neighbors joined in with us for softball and stickball in the street after dinner on summer evenings. My parents regularly golfed and went bowling, and as a family we went skiing or skating on the river. I remember when David and I were learning how to ice skate on the Navesink River, we would hold on to Daddy for balance, while he skated backwards. Later, as we gained more skill, he would let go of us, and we would try to chase him, but he was very fast, even skating backwards, and would dodge and dart unexpectedly, so that we never could catch him. His skill on the ice had been fostered at the University of Wisconsin where he was a varsity hockey star for the Badgers.

He belonged to a table tennis league, and so he built his own ping-pong table and set it up in the basement. As a result we all learned how to play, and of course we taught our friends, who would come and play with us on rainy afternoons. When my father's ping-pong partners came to practice with him, David and I would sit on the cellar steps and watch what seemed to us a spectacular performance. At the beginning of a game, my father would take out his handkerchief and roll it into a band to tie around his forehead, for he would perspire profusely during the match. Then he would crouch at his end, a wide grin of anticipation on his face. His serves were so graceful, a sleight of hand maneuver in which the ball would streak out from under his paddle, barely skimming the top of the net, to fall by his design anywhere but where his opponent might expect. And then the wild play would begin. The players would wheel and stretch, slamming the ball back and forth at high speed. Occasionally, their bodies would rebound from the concrete block walls after spectacular leaps to save the ball. Sometimes there was a strange crackling sound when the celluoid ball bounced, and we'd know it had been slammed down so hard it had broken.

In the summer months we spent a lot of time at the beach in Sea Bright. There was no pool, so my father taught us to swim in the ocean. With the waves constantly rolling in on us, this was not the easiest place to practice, but we learned very well. Later, when I went away to summer camp for the first time, I remember how surprised I was to find out how easy it was to swim in a lake where the water stood still!

On Sunday nights we usually played board games, such as Parcheesi or Pollyana, or card games such as Flinch, Pit, and ferocious rounds of quadruple solitaire. All winter long, usually just the four of us would play these games, but in the summer, children in the neighborhood would sometimes join us, and shouts of "Corner on rye!" could be heard three houses away, until finally it would get so late that parents would come looking for their children, and we would reluctantly decide to call it an evening, while promising another session the next week.

Other activities I enjoyed with my father included the yard and garden work, probably because even though it was work, it was work that brought us outdoors to space, freedom of movement, and the natural world, our preferred habitat. In the side yard he always planted a flower garden, which included several rows of his beloved gladiolus plants. During World War II we planted a large Victory Garden in the farmer's field across the street from our house. This turned out to be a lot of work, not only for my father and me, who planted and weeded, but for my mother who canned and preserved for weeks. She made so much piccalilli one year that she never had to make it again.

The annual Christmas card photograph was always a big undertaking. Since almost always it consisted of a picture of the whole family, including our German shepherd dog, we all had to get dressed in our best clothes. When I was little, this meant that I had to have my braids redone for the evening photo session, and then redone again in the morning before I went to school. That was a trial! My father used an old Graflex set up on a tripod, and two small floodlights to increase the light available from wall or ceiling fixtures. To the shutter he tied a fine black string, the other end of which he held in his hand. When we were all posed just right, he would pull the string. If we were lucky, it took just one session, but sometimes the light wasn't right or our expressions were silly, or the dog would decide to walk away. The we would have to have another session, and David and I would moan in protest. But because of the attention to detail, those pictures are still quite good, considering the state of the art and an amateur's equipment.

Our darkroom was the family bathroom (in a single-bathroom house!). To save space the ironing board was set up there with its rear legs inside the bathtub. Then my father's old maroon and gray flannel bathrobe was pinned across the window, and his homemade print box and enamel pans, along with the tongs, were set up on the ironing board. Although he always developed the negatives himself, I was allowed to stay up late and help with the printing. He would tell me how many "chimpanzees" to count for each print and he would take the tongs and slide the exposed print back and forth in the developer until it had just the right contrast. Then another set of tongs was used to quickly pick up the print and put it into the fixative. We would switch jobs occasionally to break up the monotony, as this procedure took quite awhile. In the morning the bathtub was full of newly processed prints bathing in water kept very slightly agitated by water dripping from the slightly opened faucet. After the cards were printed, they had to be dried in a blotter book. Next, they had to be pressed flat, as they would always curl up after they were removed from the blotter book. To do this, we usually spread them in small piles on the dining room table and held them down with all the heavy books we could find in the house. My mother, who is clever with words, would write a verse in India ink on cellophane and this was added to the bottom of the negative, along with our names and sometimes the year. For a final touch my father would take the orangewood stick from my mother's manicure set, and with the help of a ruler, emboss a fine line around the picture and verse to frame it. He showed Mother how to do this, and she could help while he was at work. We would all help stuff and stamp the envelopes. It was truly a family enterprise from beginning to end, and not a year was missed from 1929, when my parents were married, to 1946. After that I was away at Northfield (Northfield Mt. Hermon School) and did not get home in time for the cards to be made.

My parents shared almost all of the work at home, from breakfast preparation to dishwashing to child care. Mabel Feldman, who was the wife of Carl, another Bell Labs engineer who lived next door to us, once remarked to my mother that their (my parents') marriage was the nearest thing to a fifty-fifty arrangement she had ever seen. This was certainly true, and considering the times, quite remarkable. They took turns reading to us at night and putting us to bed. A. A. Milne's poetry, "Winnie-the-Pooh," Kipling's "Just So" stories, and "The Hollow Tree and Deep Woods Book" and others by Albert Bigelow Paine, are a few titles that I remember. When I was old enough to read on my own, I used to sneak into David's room to listen to my father read "Bambi" or Lois Lenski's "Little Train." David had memorized the latter, but never seemed to tire of it, especially the way my father would read the line, "Bumpetty, bump, bump, bump- the cars went over the tracks." As he read this, my father would bounce the bed up and down, and both of them would dissolve in laughter. Then David would shout, "Read it again, Daddy!" And so my father would have to start from the beginning and read the book all the way through again in exactly the same fashion.

An incident involving my father's role in teaching David to read is worth relating here. When David was in second grade, it was felt he was not learning to read as well as expected. His teacher sent home a dismal old Winston reader for my mother to use with him during the summer. However, the little tutorials attempted after dinner had no appeal for David, especially when his friends were waiting outside for him to come out with his ball, bat and glove. After several particularly stormy sessions, my mother gave up her attempt, fearing she would ruin forever his chances of learning to enjoy reading. David happily resumed his baseball games. About this time, David began to ask his father what the newspaper said about the latest Brooklyn Dodger game. Soon it became a daily ritual that the two of them would sit together in the "big chair" in the living room and read all the sports news together. Gradually, my father helped David to read parts of this himself, and David discovered something very important - a purpose for reading.

It is my belief that learning to read becomes each child's personal "puzzle" to solve. A teacher can provide the materials, the "rules" such as phonics, and some of the motivation. But each child's perceptions, experiences, and learning styles are so different that children can't really "learn to read" as a group, i.e., an individual lock-step course of reading development may speak to only a few in any one group at each session. The others have blithely gone ahead (and do not need the lesson) or are scattered back at earlier levels so that the current lesson is meaningless to them. Ideally, of course, each child should be taught individually. David was one of those that became "stuck" at one of the earlier levels, and my father, who knew nothing about formal reading instruction (fortunately, in this case) instinctively provided just what David needed - patience, short pleasant sessions that inspired self-confidence, and the right motivation in a subject most dear to both their hearts - baseball! Needless to say, David was soon reading on his own, and no teacher ever again sent home a stuffy old reader with no baseball stories in it.

Although my parents played with us as children, had fun with each other, and appreciated good humor, they were not particularly adept at telling jokes or creating humorous situations. Life was apparently regarded as a rather serious business. Our questions were always taken seriously, never ridiculed, so we asked thousands of them. I remember an especially long discussion begun when I asked my father what would happen if all the people left New York City. What would rot or break first? When would grass grow up through the concrete? When would the lights go out, and when would the Empire State Building fall?

Another question I remember asking, as do most children at some point in their growing up years, was, "Daddy, were there always people on the earth - where did we all come from?" And he said, "We probably evolved out of some prehistoric slime." Though I imagined at the time that people crept fully formed out of seaweed onto the beach at Sea Bright, I had nonetheless received my first lesson in human evolution, and it made an indelible impression on me. Adam and Eve were rejected forthwith, and I was later to refine my father's comment when I studied evolution as part of my anthropology major in college.

Besides the interests already mentioned, my father and I enjoyed working on our stamp collections together, sometimes with Brooklyn Dodgers baseball games accompanying us on the radio. Another hobby we shared was puzzle-solving, and riddles, but especially jigsaw puzzles, the most difficult wooden ones we could find or borrow, for they were mostly too expensive to buy, and the cardboard ones were no challenge at all. I did have a fine wooden puzzle of a detailed map of the United States. Each state was separately cut, and varied in color so that when one state was removed there was no hint, other than the shape, of what it was. My father devised a little game for me so that I could memorize the states and their capitals. After the map was assembled, he would have me shut my eyes while he removed a state. Then I would look at the empty space and try to identify the one that was missing. If I missed, I would not get a point and would then try to stump him. I learned all the states and their capitals in this fashion, and now that I am teaching fifth grade, am pleased to see that I still remember most of them. The original puzzle with all its pieces intact, is still a treasured possession which my own children used in the same way to familiarize themselves with the states, all but Hawaii and Alaska, that is, as they were not yet states when I was a child.

My parents loved music, and even dancing, when they had the opportunity. We possessed a record player and a collection of records, rather modest to be sure, but each was played until worn completely out. One album was of the Big Ten university songs, and at a very early age, David and I had memorized them all, from "I'm a Ramblin' Wreck from Georgia Tech" to my parents' alma mater's song, "On Wisconsin." There was an album of John Phillip Sousa marches which David would accompany loudly with his drums until, even with the sun porch door closed on the din, we were finally driven to distraction. On rare occasions my parents would attend a show in New York and buy the recordings for it afterwards. I learned all the songs for "Porgy and Bess" as a result of one of those trips. There were other records of a popular nature, including "Rhapsody in Blue," one of our favorites. My father also loved classical music. On Sunday afternoons he would stretch out on the living room rug and listen to the Philadelphia Symphony Orchestra, and on Sunday nights there was the Longines Symphonette. As a child, the sound of classical music somehow depressed me, and I would beg my parents to turn it off. If that didn't work, and I don't remember that it ever did, I would go outside, or if I was in bed, I would put my pillow over my head. Although I had been taking piano lessons for several years, and enjoyed the classical "pieces" I learned, I had not yet made the emotional or intellectual connection between classical piano and the larger, more complicated symphonic sound. When I was about twelve, this connection was made when I saw a frivolous movie called "I've Always Loved You," which featured Rachmaninoff's Second Piano Concerto. My friend, Anne, and I had attended the early show, and when we did not appear at home at the expected hour, my father went down to the Carleton Theater in Red Bank to look for us. So entranced was I with the music, I convinced Anne we should stay for the second show, never thinking to ask for permission. Needless to say, since the families were alarmed, my father, visibly relieved to find us, hastily ushered us out to face a different sort of "music" at home. But from then on, classical music in all its forms has become a lifelong passion for me.

Although we lived far from my parents' home state of Wisconsin, the extended family was kept viable by family reunions and the Jansky "round robin," a bundle of family letters which was sent from one to another of my grandparents' six children. When the fat envelope arrived, our old letter was removed, and a new one inserted before the bundle was sent on. The news was often very stale, but always welcome, and it was all read avidly, and responded to with care.

Somehow, on our slim budget, we managed several trips back to Madison, usually by car. When we crossed the state line into Wisconsin, my father would slow down, smile in a special way at my mother, and triumphantly honk the horn - they were home again! At the large house on Jefferson Street, the children with their spouses and offspring would completely fill all available space, and some rather creative arrangements for sleeping would have to be made. Anyone who has ever slept head to feet with one's cousin on a narrow cot, or stretched out over unsteady couch cushions lined up on the floor, will understand implicitly. Alas, the era of sleeping bags had not yet arrived.

My grandfather enjoyed many lively discussions with his now grown and far-flung children. A son of Bohemian immigrants, he was raised in a log cabin in Indian country northwest of Madison. He grew up to become a professor of electrical engineering at the University of Wisconsin, married my dear grandma, who was of French and English heritage. They had met at Valparaiso College where they were students. They were proud of their six children, who, like themselves, went through college and trained for various careers. Two of the boys went into radio engineering, one became a music critic, and one a lawyer. One of the girls became an administrative librarian, the other a bacteriologist. They brought many insights to those living room gatherings during the family reunions. My grandpa especially enjoyed following the political scene. One time he got so worked up, he shouted, in reference to our current leaders in Washington, "They should all be sent across Lake Mendota and kept there!" I was only about six years old at the time, but the comment and its attendant emotion was not forgotten. Many years later, when I was a senior at Beloit College, I learned that the deranged student who had set a $500,000 fire in Eaton Chapel, had been sent to Mendota. Only then did I realize that Grandpa was referring to the state mental institution located there. Obviously, he didn't think much of those long-ago national politicians. Those happy family reunions were important to my immediate family, for they gave us a sense of belonging to a larger entity that cared very much about us, and added to our feelings of security and well-being.

At home, these feelings were nurtured by the comfortable closeness we shared. My parents adored each other, and their affection for each other was open and obvious, sometimes to the consternation of the family dog which would stand between them when they would embrace in the hall after dinner. Not knowing who to protect from whom, she would bark and growl nervously. This was sort of a nightly ritual, perhaps an opportunity for Daddy to commend Mother on the marvelous meal (she is an excellent cook) or for them to just be happy to be together again at the end of the day. Afterwards, they would break apart and race towards the living room couch for a short rest, where the last one to arrive would have to cling to the outside edge. Stretched out horizontally, side by side, neither one could have been very comfortable, but they were blissfully content. This sort of display can be disconcerting for children, and I would sometimes tease them, but when we were on the receiving end, we would revel in such attention.

Our family structure began to change as early as 1947 when, due to the inadequate high school situation at home, an alternative was sought for me. That my brother and I would strive for a good education was taken for granted by my parents, and whatever sacrifice was necessary to achieve it, would be made. My father was willing to accept certain inadequacies in a school situation, for he was, above all, tolerant. But he was not willing to risk having my high school diploma, due to the school's substandard state accreditation, prove useless for college entrance. Private schools seemed financially out of the question, but there was one, Northfield School for Girls, located in the Berkshire Hills of western Massachusetts, which catered to families of modest means. It also stressed academic excellence, racial equality, a world-wide outlook, music in all its forms, religious ecumenism, and the outdoor life. It was to prove the answer to all my young yearnings, and though it meant leaving my family at an early age, at the beginning of my sophomore year, it also made me begin to appreciate it sooner.

The fees at Northfield were kept low by endowed scholarships from grateful alumnae and by eight hours per week of physical labor, which all students performed to help keep the school running. I was on scholarship so that my first year there cost the family only about five hundred dollars. Since I was a notoriously voracious eater at home, my father wondered if the school could afford me at that rate. Considering that my fee covered room, board, tuition, laundry, entertainment, all but books, it seems a mere pittance in light of today's inflated costs.

Letters from the family became emotional lifelines for me. I love to write my father at work ("K. G. Jansky, T.R.R.E."), and he would write back to me on Bell Laboratories stationery... "Dear Snooks....Love, Daddy." He would write about his Boy Scout work or special things he was doing for me, like making a special long extension cord for my dorm room, or arranging for our dorm to get a new record player, which he got wholesale and for which we girls paid with small donations. My mother wrote volumes of news of the family and my friends. I sent detailed letters and postcards, and each semester would send a weekly schedule, so that the family would know exactly what I was doing from 6:15 morning bell to 10:00 p.m. lights out, every day of the week. When I came home I saw it posted on the wall near the kitchen sink, where my mother could easily refer to it. It was in this way that a strong support system was created to carry me through that time of adjustment to a wonderfully different and rigorous world of new friends and ideas. It must have been harder for

David several years later when he enrolled at the (then) coordinate school for Northfield, the Mt. Hermon School for Boys, as his beloved father was gone and his sister's letters had dwindled to distressing infrequency, leaving my mother to provide this support almost singlehandedly.

In December of 1947 I came home and asked my father to tutor me in algebra during the winter break, so that I could pass the algebra review test required before the completion of geometry. He spent patient hours with me, trying different approaches to various equations until I finally understood and could work them on my own. I must have been an early case of "math anxiety," but with this extra help I was able to pass the test without difficulty.

One family tradition I missed while I was at Northfield was the gathering of driftwood along the shore for our wintertime fireplace fires. During the late fall of my junior year, I came home for a weekend, especially so that I could once more participate in this bit of fun. The beaches were deserted in November, so we could walk anywhere along them, regardless of property lines. Believing that the salty water somehow made the wood burn with a brighter fire, we would collect all that the car could hold. That last time I remember finding an especially attractive log, blond in color, of heavy, dense wood. Surely it was a section of the mainmast of some doomed sailing ship! My father hesitated, thinking of the springs in the car, no doubt, and the effort that moving it would require, but he finally agreed, and somehow the four of us pushed, rolled, and lifted it down the shore and up the beach to the car. A simple experience, this gathering of the driftwood, but it always gave me an inner joy and a feeling of tranquility.

Despite all my father could do to prevent it, by 1949 his health was in a serious decline. He was faithful to Dr. Kempner's (Duke University) Rice Diet, the best known defense against hypertension known at the time, and he had curtailed many of his strenuous activities. David had taken over the lawn mowing at home, and my mother shielded him as much as possible from stress, without his apparent knowledge of her efforts. For the most part, his condition was not discussed, and life proceeded in a normal fashion until a massive stroke in February, 1950 proved fatal. David was a freshman in high school and I a senior at Northfield. My mother was completing courses at the Bank Street School of Education, preparatory to starting her own nursery school and private Kindergarten. Her plan was to be able to help my father with college expenses for David and me, but she knew she might have to do this alone.

It seemed so unfair to us that my father had to die so young, for he had a passionate love for life. I likened our feelings to those of John Gunther, who wrote "Death Be Not Proud," a tribute to his brilliant young son who died of a brain tumor just before he should have entered Harvard College. But time is a great healer, and we at last rose above our grief, as must all families faced with such a loss.

We had many helpful memories about my father to carry us into adulthood. My brother and I could remember how he encouraged our curiosity, applauded our successes, and minimized our disappointments. He played with us and showed us his affection. It was a fortunate legacy. As a teacher, I realize it is not always this way. Children often grow up with terrible fears regarding their parents, or suffer severe deprivations, physically, emotionally, and educationally. Although I do not feel our family experiences were particularly unique for a middle class family of the 1930s and '40s, I do feel we were lucky to have such a lovable man within it for as long as we did. Likewise, the scientific world is fortunate that his intellectual curiosity led him at a very young age to contribute substantially to their field of knowledge.

The Jansky Bedtime Procession

Like many little family rituals, the origin of this one is a mystery, but it probably evolved out of a parental need to lure two reluctant young children upstairs to bed at night. My father or, rarely, my mother would stand on one of the lower steps of the staircase and begin this little verse, slowly, and with loud enthusiasm. Then David, and finally I, who preferred to be last up to bed, would fall in behind, in rag-tag fashion. We would slowly ascend the stairs, stamping hard on each succeeding step, to every "beat" in the rhythm of the rhyme. By the end of the verse, the parent in charge had got us both up the stairs--and there was no turning back! It worked for years, because it amused us so much.

Daddy or Mommy: "To bed, to bed, you sleepyheads!

David: 'Let's tarry awhile,' said Slow.

Anne Moreau: 'Put on the pot,' said Goofy Glot--

All: 'Let's have a sup before we go!'"

December 1983

PERSONAL RECOLLECTIONS OF KARL JANSKY

A. C. Beck
Bell Telephone Laboratories, Rtd.

I want to thank David and greet you, ladies and gentlemen. It is a privilege to be here and to meet many of you that I have heard so much about. I have had no direct connection with radio astronomy, but I have been involved with some of the antennas and with Karl Jansky.

That was a hard act to follow, David. That was tremendous! One thing about the replica of Jansky's antenna out in front of the Observatory. It is there at the suggestion of Grote Reber. Reber wrote to George Southworth at Bell Labs who had just retired and wasn't too well and said that it would be fitting and proper to have the original Jansky antenna at the NRAO entrance. Of course, there wasn't any original anymore and I was one of the last ones who had used that rotating structure for antenna testing, and had modified it. So, it fell to my lot to see if we could reproduce it, and that's why he mentioned that I was supervising somewhat the carpenters who were building it. And I'm glad that it's here as a tribute to Karl Jansky and the Lab.

At the time that it was presented at NRAO, Bill Baker, who was later president of Bell Laboratories, made the presentation speech down here; I was here and my wife was here also, and he told this story which may have a little bearing on serendipity. He said that there was a lady who complained that she was worried about her husband because he sat all day dangling a fish hook in a pail of water. She said, "Well, I'd do something about it except for the fact that we need the fish!" Bill Baker continued, "Karl Jansky caught the fish!" I noticed in the definition of serendipity that the word "agreeable" appeared. I didn't know that was in the dictionary definition; I can think of cases in which it may not be agreeable. For instance, there was a case of a man who looked forward to enjoying a very luscious, beautiful apple. And he took the first big bite out of it and then looked down at that apple and made the serendipitous discovery that in the apple was half a worm! I am sure that all these serendipitous discoveries in radio astronomy have been agreeable. And in fact much more than agreeable, because it has been extremely fortunate they happened, and perhaps particularly Karl Jansky's discovery.

It has already been said that in the summer of 1928 both Karl Jansky and I reported for work at Bell Telephone Laboratories in New York City. At that time there were many openings in all departments of the Laboratories, and the employment department interviewed new employees about assignments. Those interested in radio were told that there was a disadvantage to working in the radio research department because the work location would not be in the city, but "somewhere out in the sticks of New Jersey." At that time there were two field locations for radio research, one doing transmitter research and development at Deal Beach, and one doing receiver, antenna and propagation research at Cliffwood Beach.

We were assigned to the Cliffwood Beach laboratory, and started work there after a two-week introductory survey course in New York. It was located in the northeastern part of New Jersey, a mile or so west of Raritan Bay. It consisted of a few acres of land between a busy railroad and a main highway.

A house on the property was occupied by the one shop machinist, who also served as the caretaker, and his family. A small building had been erected to serve as a laboratory, and the staff of about a dozen people had desks and space for equipment and experiments there. There was also a small machine shop which could be used by the staff as needed, and one machinist. A carpenter and cabinet making shop was also included, staffed by two men, one of whom would later build Jansky's antenna.

There were some small homes and farms in the area, but it was mainly a summer shore resort for people from New York City, some thirty or forty miles away. The young Laboratories staff found few, if any, places where room and board were available, so most of them lived in the area of Red Bank, New Jersey, which was a very nice section some ten miles away. Later, as they married and had families, this was a suitable place for homes and the amenities of a good life. Since public transportation between there and the Laboratories was unavailable, automobiles were necessary. Car pooling arrangements were made, and the Laboratories assisted by allowing the two company station wagons to be taken home at night and used for commuting.

At Red Bank there was ample opportunity for a social life. Some of the staff were good bridge players, Jansky among them. There were shops, schools, theaters, churches and all the advantages of a nice residential community. New York City was less than an hour away by train, and frequent service was available, as this was a commuting area for the city.

Since the staff was young, athletics provided some interest. Horse shoes, table tennis and softball were all available for noon hour relaxation. Since there were no nearby places to buy lunches, they had to be brought in by the staff. Noon hours also provided a chance for interesting discussions about the work and problems of all concerned, and this often took the place of more physically rigorous activities. Karl Jansky was an excellent athlete in spite of his health, and excelled in all of the sports. There were Bell Laboratories teams in Monmouth County leagues playing table tennis and softball a little later when the staff grew. Jansky was Monmouth County champion in table tennis, and had the highest batting average on the softball team for several seasons. He also played a good game of tennis, and was a good skater and hockey player.

There were also other advantages in working at a small field laboratory. Officers of Bell Laboratories and other branches of the Bell System often made visits, and so members of the staff had more opportunities to know them and discuss their work with them than many employees in the larger urban locations. Company parties, put on several times a year at local restaurants, complete with skits and satires by the staff in which Jansky often took part, became so well known that many people came to them from other locations. After a few years a colloquium at Holmdel was held monthly where company and outside technical experts presented reports that broadened the knowledge of the staff.

All in all, there was a feeling that working at these locations was more of an advantage than a disadvantage. Experience at these laboratories thus became a factor in the later decision to move most of the work of Bell Laboratories out of New York City to Murray Hill, Holmdel and other such places.

The Bell System had begun to exploit radio for its communication needs. Long wave circuits to Europe were operating at regular service. However, short wave radio was just coming into use, and little was known about long distance propagation effects or equipment and antennas for this service. Therefore the radio research group was faced with very interesting and challenging opportunities in this field. At Cliffwood Beach there was a group developing and measuring antenna systems, one working with propagation research, one involved in the development of better receiving apparatus, and one looking into the generation and use of higher frequencies, called ultra short waves then, which later led to microwave communications, although that term had not been coined at that time. Since the important thing in radiotelephone communication was the signal to noise ratio, work was also being done to study the noise situation, both in the equipment and in the medium itself.

Jansky was assigned to continue the noise work already in progress on long waves. He worked on the low frequency equipment, improving, recalibrating and maintaining a static recording system that was then set up, using a rotating loop and vertical antenna arrangement. A modified Leeds and Northrup temperature recorder adjusted the receiver gain to maintain a fixed output, and record it on a strip chart. Static was normally the interfering factor at these frequencies. At short waves, static was sometimes troublesome, particularly during local thunderstorms, but other types of noise were also bothering the radio circuits. Some of these came from man-made devices such as electrical equipment, automobile ignition systems, and other similar equipment and sources. Jansky set out to investigate and learn more about this important field, and its influence on radio communications.

One of the first steps in this direction was to build a suitable receiver. Bell Laboratories, and particularly the research groups, had always stressed the great importance of careful and thorough experimental work, which called for very accurate and precise measurements. A radio field strength measuring set had been developed at Cliffwood, using the most advanced techniques available at the time. Most new members of the radio research staff were given as their first assignment the task of building one of these, incorporating all possible improvements, and also adapting it to their own proposed needs. The one Jansky built was much like the others then in use, but special attention was given to reducing noise originating in the receiver itself, and to obtaining the best possible stability in long term gain. In order to measure static and noise, because of their high peaks of energy levels, he also had to give special attention to the output circuits to avoid overload, and then to integrate that output over some definite period so that the received energy levels could be recorded. This receiver was of the superheterodyne, or as we called it, the "double detection" type with a specific known bandwidth and a very accurately calibrated step type attenuator in the intermediate amplifier, which was controlled by the recorder drive mechanism. Incorporated in this set was a calibrating oscillator for use as a signal generator, and a precise vacuum tube voltmeter so that an accurately known signal could be recorded for calibration purposes, thus making possible accurate field strength measurements. Very complete shielding of the components of such a set was required to avoid feed-back and coupling that could affect its accuracy.

In order to have a static and noise measuring system for short waves which would determine their direction of arrival, an antenna whose directional pattern was moderately sharp, and which could be rotated in a complete circle, was required. After much study and consideration, it was decided to use a shortened version of an array then in use on the new short wave long distance circuits. Here again, cooperation and assistance from other Bell Laboratories people, especially the antenna research group, proved helpful. In Jansky's case, however, planning and arranging for tracks, wheels, connections and a driving mechanism were special problems which he had to solve.

In order to record static and noise, it was necessary to use a radio frequency channel where there were no transmitting stations in operation. Jansky studied channel assignments and did a considerable amount of listening "on the air" to determine the best frequency to design the antenna and operate his equipment, finally choosing 20.5 MHz. It was customary, in both the antenna and propagation research groups, which used recorders like Jansky's, to tune a vacant channel and record the background noise as a part of the calibration process. As time went on, and it became more difficult to find a vacant channel in the spectrum for this purpose, the suggestion was made that the receiver's coaxial input be connected to a shielded matching termination resistor to make this calibration of set noise. This also made it possible to perform the calibration without changing the tuning of the receiver. In the antenna research group, coaxial lines from various antennas were brought in to a coaxial jack panel, where coaxial plugs from the measuring sets could be plugged into whatever antenna was having its output recorded. Such shielded terminations were made a part of the jack panel for convenience in making this calibration. It was then noticed that the recorded noise level from the matching termination was always lower by a small amount than the output of any antenna when the receiver was tuned to a vacant channel. This level was not the same for all antennas, but changed depending on which of the antennas was connected, and even depending on the time of day. This was not understood, and was discussed with many members of the staff, including Jansky, who studied it at that time in detail. The possibility of extraterrestrial noise was not thought of at that time, but Jansky's later work, of course, explained these observations. There was no way to identify extraterrestrial signals from a fixed antenna. To do that required Jansky's equipment, his thorough, persevering work, and his perceptive mind.

All of this work of getting ready to record noise on short waves took more than a year. Then, in late 1929, construction of the rotating array began at Cliffwood Beach. By then, the radio research staff had increased and required more space, better facilities, and a location with less local noise and interference. It was therefore decided to move the laboratory to a much more desirable location several miles away in Holmdel, New Jersey. Three farms, consisting of more than four hundred acres, were acquired there. There was some local criticism about taking the land out of agricultural production, in this case mainly corn and potatoes, but hay was grown and harvested on some parts of it by neighboring farmers for several years. A new main laboratory was constructed away from outside roads to reduce interference problems. A special site was chosen some distance from this building and other possible interference sources for Jansky's rotating antenna, and near it a small building, called a "shack" by the staff, was placed to be used exclusively for his measuring equipment. It was now necessary to build a new circular track and move the antenna down from Cliffwood Beach. Because of this, a considerable delay took place in static recording.

In late 1930, however, regular recordings were being made, and Jansky began studying and analyzing them. In 1932 he presented a report on this work at a meeting of the International Scientific Radio Union in Washington, which was published later in the Proceedings of the Institute of Radio Engineers. After describing his equipment, he gave some results, classifying three types of static. The first was due to local thunder storms which could be visually observed, and he went into some detail about them, their energy levels, and the distances at which they could be heard on both high and low radio frequencies. The second type was steadier static with peaks and characteristics like the local storm static but much weaker. He thought this probably came from thunderstorms much further away by Heaviside layer refraction.

The third type he called a "very steady hiss type static, the origin of which is not yet known." This was always very weak, and might easily have been ignored, since normally it would not interfere with radio signals. But Jansky was a very thorough observer, and his scientific curiosity was now aroused. He always had a very inquisitive mind, which was also evident in out of hour courses taken by the staff when he asked questions about any points not made completely clear. Sometimes this slightly annoyed other class members, who would audibly comment "static." Jansky continued to record this type of hiss noise and study it, and talked with numerous associates about it. He found that these signals showed a peak signal strength at a position in the same horizontal direction as the sun, and suspected some connection there, perhaps the first time his thoughts turned to an origin outside the earth. But with the perseverance that is a characteristic of a good scientist, he continued his recording and found that in later months the direction of the steady hiss noise was no longer in the direction of the sun, but was gradually changing. After about a year of recordings, at the suggestion of another member of the Bell Laboratories staff, he began studying texts on astronomy, and as a result of some very capable analysis, was able to prove that this hiss noise was coming from a direction that is fixed in space. In April, 1933, he presented his classical and very well written paper, "Electrical Disturbances Apparently of Extraterrestrial Origin" at a meeting of the International Scientific Radio Union held in Washington. In June he gave it at the annual convention of the Institute of Radio Engineers, who published it in October 1933.

Of course that was the occasion for the publication department of Bell Labs issuing a press release which was picked up by the newspapers and gave the beginning of the publicity which he received, including the article in the New York Times. It is the anniversary of that which we are celebrating here.

It is interesting to note that Jansky's work proved to be of considerable value to the radio research department and to Bell System radio communications in a number of ways. In addition to recording noise, he used his equipment to record the direction of arrival of short wave transmissions from a number of stations in various parts of the world. From the results that he obtained in this way came further understanding of radio propagation effects, and information directly useful in the design of directional receiving and transmitting antennas. Later, antennas were developed which were steerable by electronic means over the ranges indicated by these and other measurements. At one time, several years after this, he again listened to what he called "star noise" on a steerable system of rhombic antennas and found that there was nothing about it that was changed from his original observations. He continued to work on the direction of arrival of radio waves, and published a paper on this subject in 1941.

Jansky was an excellent and lucid teacher, and he organized and taught, together with another member of the staff, out of hour courses to classes of technical assistants at Holmdel. He also expanded his research to other phases of noise and its reduction in receivers and circuits, and here, too, he made substantial contributions. When the war came, and the Bell Laboratories staff became heavily involved, he was assigned to a highly classified project where his experience and knowledge made a very important contribution to the war effort. For this work he received an Army-Navy citation. After the war, when some of the Holmdel staff had the assignment of developing and setting up the first microwave repeaters as prototypes for the systems in such wide use now, Jansky was responsible for the design and development of the intermediate frequency amplifiers. These had to have low noise and broad bandwidth characteristics, and his expertise in these areas was most helpful to this Bell System project.

A few years after Jansky's work with it, the rotating antenna he designed and used was modified mechanically and served a number of purposes in ultra short wave, and what is now known as microwave, antenna research and testing by several members of the staff. During the war it also served in the development and measurement of radar antennas, particularly of the long range search type. It was then dismantled, and all traces of it vanished except the gear reducer in the drive mechanism. This was found and is now incorporated in the replica at the National Radio Astronomy Observatory.

Jansky was also involved in the work of the International Scientific Radio Union and the Institute of Radio Engineers. He was one of the organizers of the Monmouth Sub-section of the latter society, and served as its chairman. He was elected a Fellow of the Institute of Radio Engineers for his contributions to radio science, an honor which he appreciated very much.

It was unfortunate that his health deteriorated so early in his life. He spent some time at the Duke University Hospital, but the treatments there did not seem to be very effective. In spite of continued health problems, and an extended period on a diet of rice and grapefruit juice, he always remained the same warm, friendly, earnest and admirable person. Just as the science of radio astronomy which he had started was beginning to produce such important results, and making so many contributions to our knowledge of the universe, his condition worsened, and he passed away in 1950. Certainly, had he lived, many more honors including the Nobel Prize, which he deserved, would have been awarded to him. It is good that the Laboratory at the National Radio Astronomy Observatory is named for him. It has been a great privilege not only to have known Karl Jansky as an associate, but also as a respected and admired friend.

K. Kellermann: For a better part of the last six months, a number of us have been trying to figure out how Karl Jansky fed that antenna. It is not at all obvious or clear in any of the written material we have been able to find. Before Al Beck leaves, we want to get him to show us how to connect up that thing.

J. Findlay: I want to ask any other old fogey, "Can you remember when you read Jansky's paper and what you thought of it?" I can. I was a graduate; I joined Ratcliffe's research group in 1937, and those of you who know Jack Ratcliffe know him as having a perfect reference system. One task was to read the references that Ratcliffe had and those papers, of course, went in the card file. I remember to this day saying, "There is something here that should be looked into, but I have to do what Ratcliffe has told me!"

J. Greenstein: I have the honor of having beaten you by two and a half years. When I was 11 years old, I was a radio amateur without a license. I heard somewhere on my return to Harvard as a graduate student, probably in early 1934, that somebody had claimed to have gotten radio static. Of all the journals that astronomers did not read, one of the most distinguished was the Proceedings of Institute of Radio Engineers, or URSI. I don't know where I found it, but I think it was in fact due to a man named George Washington Pierce at Harvard, who was an electrical engineer at the time. Fred Whipple and I, a couple of years later, even got the units straight. Jansky had the misfortune to use practical radio engineers' units, as some of you know like stat volts per meter. Does anyone know how to turn that into a Jansky?

K. Kellermann: Do you still have a valid amateur radio license?

J. Greenstein: No, no. I never got one!

K. Kellermann: We should look into people like that! The reason we were trying to understand how to feed the Jansky antenna is that we plan to use it this weekend with a transmitter on the 15 meter amateur band. We finally did figure out how to feed it.

KARL JANSKY AND THE BEGINNINGS OF RADIO ASTRONOMY[1]

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Fifty years ago, plus one week, on 27 April 1933, Karl Jansky read a paper entitled "Electrical Disturbances Apparently of Extraterrestrial Origin" to a small audience in Washington, D. C. at a meeting of the US Committee of the International Union of Radio Science (URSI). We are here today to honor the discovery which that paper reported, for today we view it as the beginning of radio astronomy. But at that time it was neither the birth of a new science nor greatly acclaimed by Jansky's scientific and engineering colleagues. Jansky himself wrote to his father a week after his talk (I'll be giving you many quotes from his letters to his father, who was an engineer):

> *I presented my paper in Washington before URSI, an almost defunct organization. It was not my wish that my paper was presented there, but at Mr. Friis's* [his boss] *insistence. I wanted to present it at the IRE Convention in Chicago in June, but Friis said No. The URSI meetings in Washington are attended by a mere handful of old college professors and a few Bureau of Standards engineers. The meeting was conducted in such a manner that there was time for discussion of only a couple very short papers. Not a word was said about mine except for a few congratulations that I received afterwards. Besides this, Friis would not let me give the paper a title that would attract attention, and made me give it one that meant nothing to anybody but a few who were familiar with my work. So apparently my paper attracted very little attention in Washington.*

But of course today Jansky's paper has attracted much attention, and it is my task today to attempt to do good history, by which I mean to re-create and interpret the events and ideas of those times half a century ago. I want to give a realistic picture of Karl Jansky, the man, as I have learned of him, the environment in which he worked, and the sequence of events which led to his famous discovery. The primary sources I have used, besides the scientific and popular periodicals, are the extensive archives, including laboratory notebooks, of the Bell Telephone Labs, correspondence of Karl Jansky with his father available at the University of Wisconsin archives, and other letters which his son, David, and his widow, Alice, have shared with me, and interviews with many of those who worked with and knew Jansky.

First of all, the biographical background of Karl Jansky. He was born in 1905, the third of six children, in the territory of Oklahoma where his father, Cyril, was head of the School of Applied Science at the University of

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[1] A more detailed and fully documented version of this paper is contained in The Early Years of Radio Astronomy (ed. W. T. Sullivan, III: Cambridge University Press, in press).

Oklahoma. His father's parents were Czech immigrants who came over in the 1860's, while his mother was of French and English background. His father was a professor of electrical engineering in several Midwest schools, ending at the University of Wisconsin from 1908 to 1940. And thus Karl grew up in Madison in an academic environment. Karl was named after Karl E. Guthe, a German-American physicist with whom his father trained at the University of Michigan and whom he greatly admired. He attended the University of Wisconsin and obtained his B.A. in physics in 1927; he was Phi Beta Kappa. His undergraduate thesis was on vacuum tubes, under E. M. Terry, who by the way also worked on atmospherics in about that same era. Despite his small size of 5 ft. 7 and 140 lbs., he played varsity hockey, as we have seen, for the Badgers; and throughout his life he was a fierce competitor in sports and parlor games of all kinds. He was an excellent bridge player. He stayed on for an extra year of graduate study in physics at Wisconsin, finishing the course work for a Masters, but not the thesis until many years later (at which time he simply took one of his Proceedings of IRE papers and made it a thesis).

In July 1928 he began work at Bell Telephone Labs at $33 a week. That wasn't too bad; his room and breakfast was only $5.00 a week. His company physical made the Labs leery for it showed a kidney disease, Bright's disease, which a couple of years earlier had disqualified him from ROTC, and which would eventually lead to his relatively early death. Only through the intervention of his ten-year-older brother, C. Moreau Jansky, who was then a professor of electrical engineering at the University of Minnesota and had many connections at Bell Labs, was the medical department persuaded to accept Karl. And largely for reasons of health they didn't keep him at the main Bell Labs in New York City, but put him out in a field station, as we have heard, "in the sticks" at Cliffwood, New Jersey, under Harald Friis, and immediately Friis set him on the problem of shortwave static.

Let's now take a look at the setting of radio communications research and development in that era, so we can see the milieu in which Jansky started in 1928. Recall that although the existence of an electrified layer in the upper atmosphere had been proposed in 1902 by Oliver Heaviside and by Arthur E. Kennelly, it was not until about 1924 that transmission experiments by Appleton and Barnett in England and pulsed radar by Breit and Tuve in the US established that indeed there existed such a layer, about a hundred kilometers high. And it became clear this was why intercontinental communications were possible at frequencies higher than about 100 kHz, commonly in use in those days. In the 1915 to 1920 era, frequencies pushed into the high frequency region, at frequencies as high as 1.5 MHz, allowing more message capacity and more diversity in communications channels, depending on the conditions in the atmosphere. In the 1920-25 era, it became apparent that the short waves, defined as wavelengths less than 200 meters or frequencies greater than 1.5 MHz, also surprisingly could be used for long distance communications. As the technology improved, the high vacuum triode became a key element because it was useful both as a power tube in transmitters and a sensitive amplifying tube for reception. Short waves really began to become feasible.

But you must realize that until the early 1920's these communications were only Morse Code, i.e., radio telegraphy. For radio telephony, one needs much more bandwidth, two-way service obviously, much greater reliability, much greater quality of transmission, and 24-hour service - all in all a much more difficult task. In 1927, the first AT&T radio telephony was opened up between

New York City and London - at $75 for 3 minutes - and it was at 60 kHz - long waves. But for several reasons it soon became clear that short waves were the wave of the future for radio telephony. First of all, there was less need for huge antennas and transmitters in order to get the required directivity and power at the receiving site. Secondly, it was becoming clear there was a lower level of atmospheric noise, especially in the summer time when tropical thunderstorms killed you on long waves. And thirdly, there was much more bandwidth available for telephone channels. So in 1929, one year after Karl Jansky joined the Labs, the first short wave radio telephony across the Atlantic, with a 15 kilowatt transmitter and quartz crystals, was opened by AT&T. But there were many problems that needed work. There were all kinds of new interference - automobiles, intrinsic receiver noise, different kinds of atmospherics than were known in the long waves. There was multipath fading (different paths for the same radio signals causing problems), and a new class of problems, magnetic storms which could cut you off for days at a time, and no one quite understood what was going on there. It was these problems that Friis and their colleagues in the New Jersey field stations were tackling when Karl Jansky began his work.

Now let's take a more specific look at the setting at Bell Telephone Labs. In the first decade of this century, several major industrial research labs were established in the US, in particular at G.E., DuPont, Kodak, and AT&T. In 1925 the research departments of Western Electric and AT&T merged to form Bell Telephone Labs, which then had a total of 3,000 employees. It was headed by Frank Jewett, the first director, who set the tone of the Lab with an emphasis on step-by-step attacks on complex problems in communications engineering; precision of measurements was a vital desideratum. Bell Labs quickly established an international reputation; for instance, in the late 1920's the work of Nyquist and Johnson on intrinsic receiver noise was done, and Davisson and Germer's famous work on electron diffraction by crystals was also done then (work for which Davisson later shared the Nobel Prize). In fact, if you count Physical Review articles in the late 1920s, Bell Labs was among the top ten institutes in the U.S.

Now I wish to focus on radio work at Bell Labs. In 1914 Carl Englund, really sort of the father of all this, developed precision methods of measuring long wave signal and static levels. In 1919, Ralph Bown and Harald Friis, a Danish immigrant, joined the Labs and they continued with this research. The research always started off as applied to some concern of telephony, but it often led into more fundamental questions of techniques or of earth sciences. In the 1920's, radio measurements continually were improved and the tools of the trade became more portable, more accurate, and more convenient. In general, radio research was moving from a "seat of the pants" era to quantitative studies of the level and character of both signal and noise. In other words, it was becoming a science. Thus in the 1928 to 1935 era, we find a group of around 20 men, of whom you have seen a picture, in Friis's and Englund's group at Holmdel investigating many aspects of antenna design, receiver circuitry, measurement techniques, static, and propagation of signals.

Thus it was in 1928 that Karl Jansky joined a top-notch group of experts and that he, along with several others such as Al Beck, all fresh out of college, himself soon became an expert. Friis assigned Beck to build a field strength recorder for trans-Atlantic signals and he assigned Jansky to build one for, essentially, trans-Atlantic noise, if you will. So they worked closely together, as we have heard. The basic chronology of Jansky's work can be split up into four eras or phases. 1928 to 1930 is the orientation phase - learning the ropes, recording long wave static with Friis's set-up, and building a short-wave set and rotatable antenna. 1930 to 1932 is Phase Two, consisting of short-wave observations and several diversions, as we'll see. 1932 to 1933 is the climactic phase - the astronomical discovery and the analysis of it. And 1934 to 1936 is the last phase - sporadic measurements of "star static," as he called it, amidst his main work on more practical aspects of radio noise.

Phase One. He writes to his father in September 1928:

> *I've been building apparatus for the last few weeks for my new short-wave recorder. It will be several months yet before I get any actual results.... When I first came here, the language they spoke was almost foreign to me, but I'm beginning to get used to it now. At Madison I had never heard of such things as attenuators, T.U.'s* [Transmission Units, which then became a decibel shortly afterwards], *gain controls, double detection, etc., but that's what I get for not taking engineering.*

I'm sure his father had urged him to become an electrical engineer! On 24 August 1929, we find in his notebook, "Mr. Sykes will start work on the merry-go-round next Monday", so that's when the antenna began to be built by the carpentry shop. The antenna grew directly out of one that Friis had designed circa 1926 for long waves, shown in Figure 1. We see a picture of Friis directing someone to push the antenna around; these loop antennas, one at each end, are for long waves and they were beating the two of them against each other. What Jansky needed was the equivalent of this in short waves; what eventually emerged is the famous antenna shown in Figure 2. It was made out of 400 feet of brass pipe of 7/8" diameter; fir lumber, 2" x 4" primarily; Model-T wheels to turn it around on a concrete track; and a quarter horse power motor was all that was needed. It's about a hundred feet long, which is precisely two wavelengths operating at 14.6 meters. Each of the sections is a quarter of a wavelength long, and in this so-called Bruce array there's a driven element and a reflector element, which is about 15% larger. You can, of course, go outside here at Green Bank and study the replica for yourself.

Phase Two. In February 1930, once the work got going on this at Cliffwood, the entire Cliffwood Lab moved a few miles, as we've heard, to Holmdel, causing Jansky to lose several months on his project since his concrete foundation and track had to be entirely re-done; and of course whenever you move, it's always a mess. But the group now had what it really needed. They had much more room, 400 acres of rolling farmland and woods, a large central wooden frame lab, which was nicknamed "the turkey farm", and many small out-huts for individual experiments. We find that by the second half of 1930 that he is finally beginning to record some short-wave static, but only a little bit. In the winter of 1930-31, however, he is using the antenna to study the distribution of the angles of arrival of short-wave signals from a transmitter in South America. He said he was going to wait until next spring for the static because "there is no short-wave static in the winter," and he

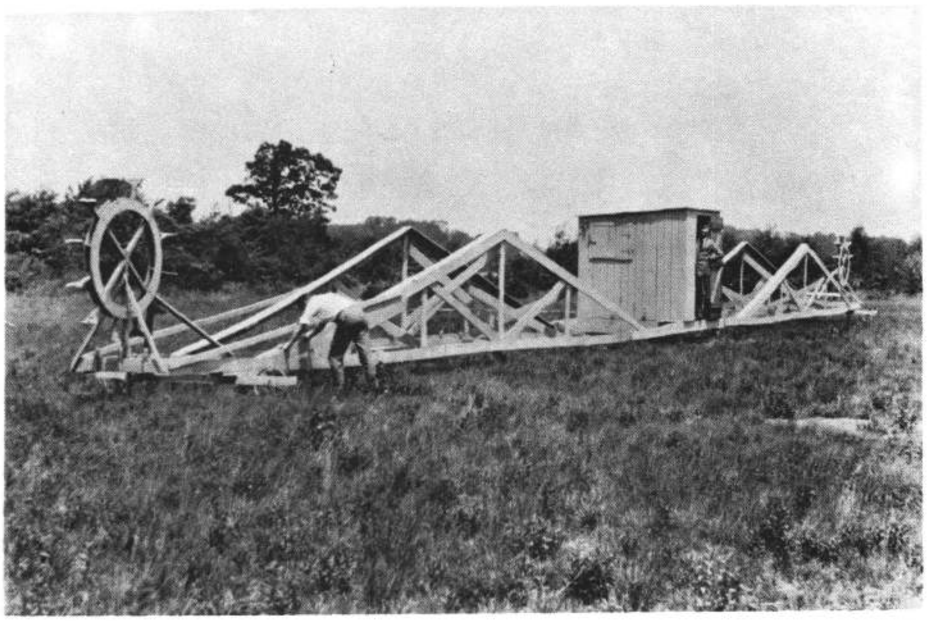


Fig. 1. Harald Friis directing the operation of his long wavelength antenna, 1926 or 1927.

was expecting that there would be much more in the warmer months because of thunderstorms, etc. He also built at that time, I suppose to keep himself occupied, an ultra short-wave receiver. ("Ultra short waves" were defined as less than 7.5 meters wavelength -- his was at 4 meters.) And he was doing a few odd static observations, but not verv many. It is in his August 1931 work report than we can perhaps first see an indication that he has picked up extraterrestrial noise, but it definitely was not recognized as such then. The report indicates a night-time, weak static which was moving across the sky, and we can say it probably was the Milky Way. As you know, if you look up in the evening in the summer, the Milky Way is right there, and through the autumn this static continued and Jansky became intrigued.

Phase Three. The first real recognition, according to Jansky himself, and it seems that this is correct from everything that I have seen, was in January 1932 when he writes in his monthly work report: "... A very steady continuous interference - the term 'static' doesn't quite fit it. It goes around the compass in 24 hours. During December this varying direction followed the sun." And a letter home, also in January 1932, he not only talks about the birth of his first child, Anne Moreau, who is here with us today,

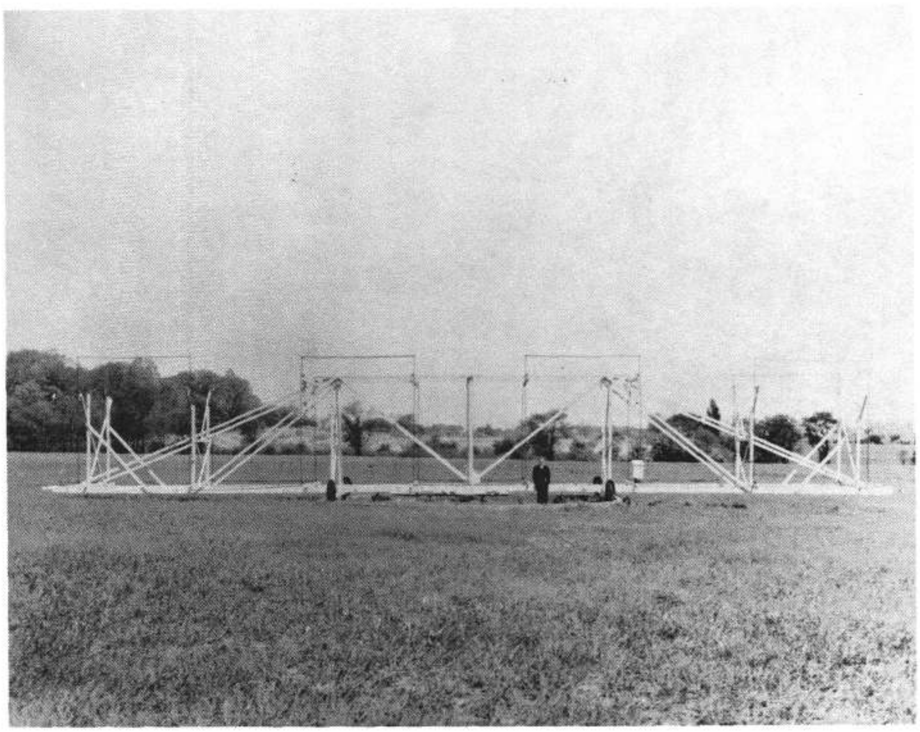


Fig. 2. Karl Jansky and his famous antenna.

but also is the first one that reports to his father about this very weak, steady static - so we have a nice juxtaposition of births here.

In February 1932, Friis told Jansky to prepare a paper on his results up to that point for the April meeting of URSI in Washington, D. C. But already Jansky was seeing that the "sun static," which he called it for a while, was better called "hiss type static", because now its daily peak was preceding the sun by as much as an hour. Over the spring of 1932, he was mainly working on the written and oral versions of this paper, but he was also noticing (taking lots of data, but not analyzing so much) a continual shift "in accordance with the approaching summer season and the lengthening day," and he is quite curious as to what is going to happen after June 21st, after the summer solstice. His idea is that the sun is moving north, and this is the main reason why there is this shift -- it has something to do with the changing position of the sun. And so in his first article which was published in the Proceedings of the IRE in 1932, we find that he talks about the three types of static which Al Beck has told us about, including a "very steady hiss-type static, the origin of which is not yet known."

In the middle of all this, in June 1932 - I'd like just to give you some flavor of the times - the work week at Bell Labs was cut from five and a half to four days, and 20% of all the people were fired. This caused Karl to ask his father about possible teaching jobs in Wisconsin, and he feared that even the entire Holmdel station might be closed. But he said, "I can't think of a better company to work for."

And then there was a diversion. He began studying the general effects of bandwidth on received signal and designing a new receiver that could automatically change bandwidths. But he was still taking data, about five to ten days a month, in order to follow this curious low-level noise which he later called "star static." There happened to be a partial solar eclipse in New Jersey in August, and he took advantage of that, but he found no effect of the eclipse on the hiss-type static (by doing the same experiment the day before the eclipse, the day of the eclipse, and the day after). In August 1932 the indications are, he says in his work report, that the curve is <u>not</u> going to shift back to the spring positions. Now he is two months past the solstice, and, by golly, it's not going back, it keeps marching forward two hours every month. It's going to continue on!

In December 1932, the astronomical aspect finally hits him. At that time George Southworth (who himself during World War II was the first to detect microwaves from the sun) asked Jansky to plot up data over the long term - put the whole year together, he said, in more coherent fashion - to see if it correlated with diurnal changes in "earth currents" that he was then studying. And that may have been an important suggestion for Jansky, because by the end of that month of December 1932, Jansky had determined that the direction of the source of the hiss-type static "always lies in a plane fixed in space, at a right ascension of 18 hours and a declination of -4° (he never published that figure, but that's what he said at that time in his work report). He also talked to Melvin Skellett, who was in a very unusual position for that era of being at work on his Ph.D. in astronomy at Princeton and working at Bell Labs as a radio engineer, and so Skellett undoubtedly clued him in on many astronomical details such as the solar apex, with which this position agreed rather closely. So Jansky wrote to his father at that stage:

> *There is plenty to speculate about, isn't there? I've got to get busy and write another paper right away before somebody else interprets the results in my other paper in the same way and steals the thunder from my own data.*

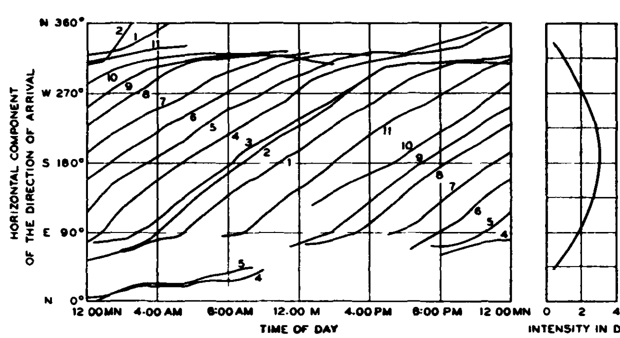
He also tried to determine the <u>vertical</u> angle of arrival by using a "horizontal antenna set-up," as he calls it - I'm not sure exactly what he was using - but he found that he could not get any variation in the signal dependent on the elevation angle. He felt throughout all this investigation that he had no useful information at all from his antenna on the vertical angle of arrival, but that's quite wrong, as I will discuss a bit later. I have never figured out why he didn't follow that up a bit more. He writes to his father in February 1933:

The evidence I now have is very conclusive, and, I think, very startling. When I first suggested the idea of publishing something about it to Friis, he was somewhat skeptical and wanted more data. Frankly, I think he was scared. The results were so very important that he was timid about publishing them. However, he mentioned them to W. Wilson, the department boss, and Wilson discussed it with Arnold, who is in charge of the whole research department of the Bell Labs (he reports directly to Jewett), and Arnold wanted the data published immediately. Evidently he thinks the results are quite important too.

The next month he writes:

Have you any idea what could be the actual source of these noise waves? I've been giving my imagination free rein, but without result as yet. I imagine that it will take an astronomer who knows something about the outer regions of space to answer that question.

In Figure 3 we see a key illustration from his second paper, published in 1933 and the first one to discuss the astronomical interpretation. Here we see the time of day versus the direction of arrival in azimuth, or, if you wish, a plot of sidereal time versus solar time over the year. The curves are labelled essentially by the different months of the year, and you can see how it was shifting about two hours every month. Half of his paper, six out of twelve pages, was spent explaining what right ascension and declination are to the engineers. He concluded that the main extraterrestrial signal was coming from a right ascension of $18^h\ 00^m \pm 30^m$ and a declination of $-10° \pm 30°$. What is interesting, and I don't think well appreciated, is that the entire analysis in this 1933 paper was done in terms of the changing azimuth as a function of time, and that he was considering that when this <u>single</u> source, whose position



1. Jan. 21, 1932
2. Feb. 24, 1932
3. March 4, 1932
4. April 9, 1932
5. May 8, 1932
6. June 11, 1932
7. July 15, 1932
8. Aug. 21, 1932
9. Sept. 17, 1932
10. Oct. 8, 1932
11. Dec. 4, 1932

<u>Fig. 3</u>. Direction of arrival vs solar time throughout the year. Taken from Jansky's second paper (IRE, now IEEE).

I just gave, was below the horizon, he was still picking it up. The radio waves were hitting the earth and following the curve of the earth (and he was worried about how much it got attenuated) so that he picked up this single source - as he thought of it - for 20 hours a day! This is quite clear from his paper if you read it carefully. He also mentioned that the radio waves that he was detecting might be secondary, that it might be (what we now call) cosmic rays hitting the upper atmosphere and triggering the waves, but the cosmic rays then came from a definite direction in space. And he also mentioned that not only did this position agree reasonably well, to his accuracy, with the solar apex, but also with the Galactic Center.

Now we are back to the beginning of my talk where he gave his paper at URSI in Washington, D. C. fifty years, plus one week, ago today. The Bell Labs' press release came fifty years ago today, and The New York Times article fifty years ago tomorrow. For several weeks Karl was in the media limelight. Figure 4 shows a typical P.R. shot of that day.

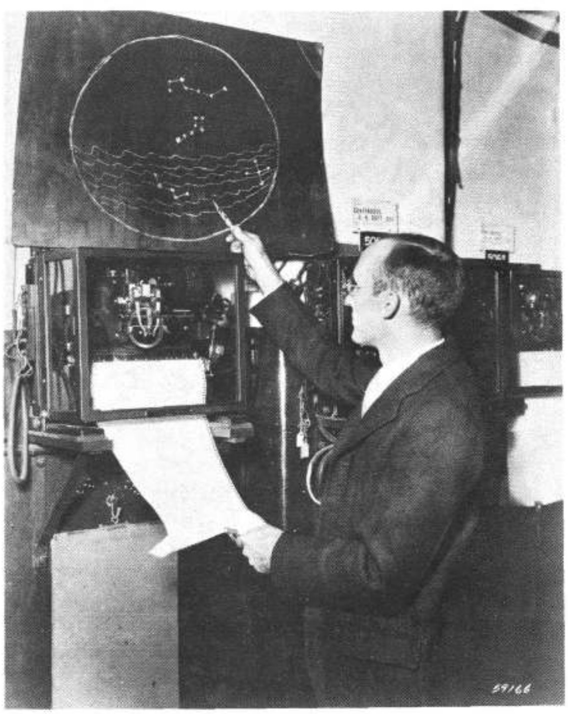


Fig. 4. Photo used in Bell Laboratories publicity release.

On 15th May the NBC Blue Network broadcast his hiss radiation via a direct hook-up from Holmdel to their New York City studios, and it went around the nation. It was described by the press the next day as "sounding like steam escaping from a radiator." That evening it followed Lowell Thomas and the News and Groucho and Chico Marx. The New Yorker magazine harrumphed: "It has been demonstrated that a receiving set of great delicacy in New Jersey will get a new kind of static from the Milky Way. This is believed to be the longest distance anybody ever went to look for trouble." To give you a further idea of what was going on at that time, Babe Ruth hit the winning home run in the first All Star game in Chicago, the film King Kong was released that spring, FDR was in the middle of his "first 100 days" (Karl Jansky was a rabid anti-New Dealer), and the Graf-Zeppelin flights were going on, in fact at one time Karl listened with his antenna to their radio transmissions as they came in to Lakehurst, New Jersey.

Following the oral presentation of his discovery, he worked on the written version, and via his brother who was a big-wig in the IRE (and in fact became president shortly thereafter), he got an invitation to give the paper at the Chicago IRE National Convention. He writes:

> *I haven't the slightest doubt that the original source of these waves, whatever it is or wherever it is, is fixed in space. My data proves that, conclusively as far as I am concerned. Yet Friis will not let me make a definite statement to that effect, but says I must use the expressions 'apparently fixed in space' or 'seem to come from a fixed direction', etc., etc., so that in case somebody should find an explanation based upon a terrestrial source, I would not have to go back on my statement. I'm not worried in that respect, but I suppose it is safer to do what he says.*

By August he says that more analysis is beginning to show very clearly that there are two peaks, exactly as expected if the waves come from the entire Milky Way, in other words, one peak when the anti-center comes into the beam of his antenna, and the other when the center comes into the beam. So very soon after the submission of his 1933 paper, in which he only talked about one position, it was becoming clear that it was really a whole band of emission there. Also Al Beck helped him with some 4-meter wavelength experiments, but it seems to me from the evidence that I have that they were inconclusive; they just were not able to detect anything and there were problems of calibration, etc. He also used a couple of other antennas at that time, for instance a rhombic antenna at 24 meters wavelength, to try to see something about the spectrum. But none of this gave any useful information.

Phase Four. All this follow-up work happened during the six months after the discovery paper in the spring of 1933, but now we come to the last phase. All of a sudden, in January 1934, there's nothing in the work reports on interstellar noise, but rather now he was assigned to optimizing circuits for measuring noise bursts. What bandwidth should we have, what kind of detectors do we want, what kind of integration times? And he writes to his father at that time:

> *Have I told you that I now have what I think is definite proof that the waves come from the Milky Way? However, I'm not working on the interstellar waves anymore. Friis has seen fit to make me work on the problems of methods of measuring noise in general. A fundamental and necessary work, but not*

near as interesting as interstellar waves, nor will it bring near as much publicity. I'm going to do a little theoretical research of my own at home on the interstellar waves, however.

He continued working on general questions of ultra short wave static through 1934. In early 1935, he writes to his parents:

> *I have finally succeeded in stirring up considerable interest among the men in the New York Laboratories over my work. It all started late last fall when Mr. Buckley, the present director of research of the Bell Labs, called me in to give him some pointers on static and noise in general for a speech he was giving in Toronto. In fact, his whole talk was pointed towards a discussion of the importance and implications of my data. He concluded his speech with a statement that he thought it was the most interesting discovery made in recent years!... A short time afterwards, Friis came around and suggested (he had heard about Buckley's talk) that I write another paper for publication setting down my ideas on the subject, as well as giving certain other deductions I had from my own data.*

It is quite clear that the environment was such that you didn't write a paper unless your boss approved it, and in this case, even suggested it, because Karl had long had these data in hand, but only when Friis was willing to let it happen did it happen.

And so in his third paper, which was published in 1935 although he had the results over a year beforehand, he talks about the interpretation of the star noise as coming from the entire Milky Way. He is thinking of the signal being proportional to the number of stars that are in his beam at any given time. He says that the sources could be stars or the interstellar medium, it is not clear which, but if they are stars, then why isn't the sun detected? You see, that's the fundamental problem, which I don't have time to go into any more, but I think you can see what I mean. He also speculates: "Since it sounds so much like 'set noise' [intrinsic receiver noise], couldn't it be due to thermal agitation of charged particles?"

The basic characteristics which were to puzzle radio astronomers until the 1950's are already clear from these early data: very high intensity, high brightness temperature, concentration to the Galactic Center, and concentration to the Galactic Plane, but nowhere near as much concentration as the stars. Why is there so much signal away from the Galactic Plane? In Figure 5 we see the only published data, as well as the only extant data of Jansky - one day's worth of strip chart recordings - and you can see clearly that the signature of the star static is changing over the day depending on what part of the Milky Way is being swept by his antenna. The width changes depending on whether the Milky Way is going through the beam in its "long sense" or whether it is cutting through across the Plane. Every 20 minutes the antenna would turn, and so you see three bumps every hour. By the way, on this day the Galactic Center crossed the meridian, 21° above the southern horizon, at 5:56 p.m.

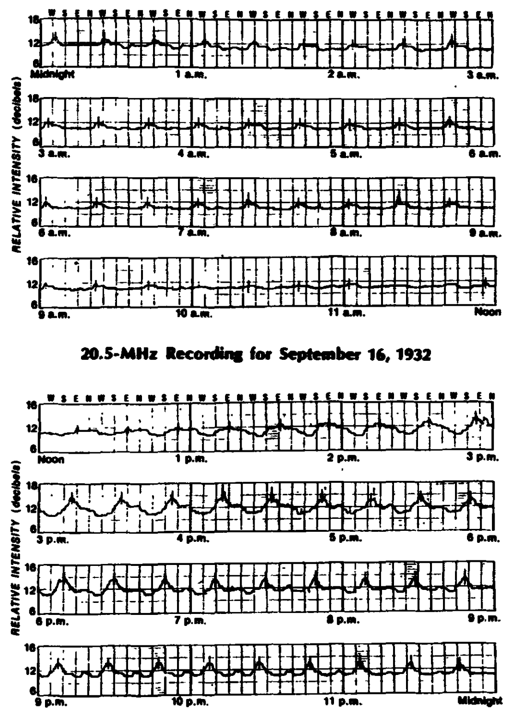


Fig. 5. Strip chart recording of Sept. 16, 1932. (1935 IRE, now IEEE.)

Figure 6 shows on the bottom the antenna pattern (E field in the horizontal direction) that he published, measured with a transmitter on the site. Although he never worked with the vertical antenna pattern, I have calculated it to be what is shown on the top - about 36° FWHM and centered at about 22° above the horizon. If you put in the estimated effect of the ionosphere, it raises the effective beam to an elevation of 26°, shown as the solid line. Figure 7 then shows the map that I have derived of Jansky's one day's worth of data; of course all days were in essence equivalent. We see the concentration to the Galactic Center and indications of the North Polar Spur and of Cassiopeia A, but not Cygnus A. The small unmapped region is his zenith - can't quite get up there. The positions of Jupiter and the Sun on that one day of data are also indicated, but I found no sign of bursts from them on that day. Further details on the derivation of this map can be found in Sky and Telescope for August, 1978.

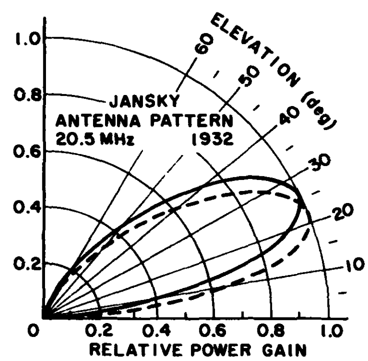
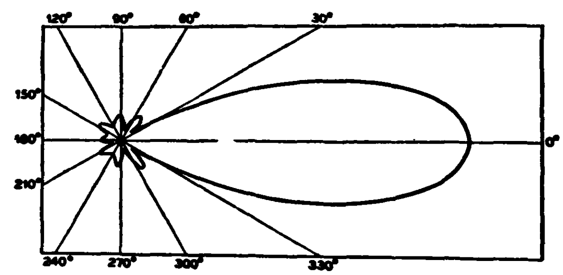


Fig. 6. Top: calculated vertical antenna pattern (power). Bottom: measured horizontal antenna pattern (E plane) (as published in 1932, IRE, now IEEE).



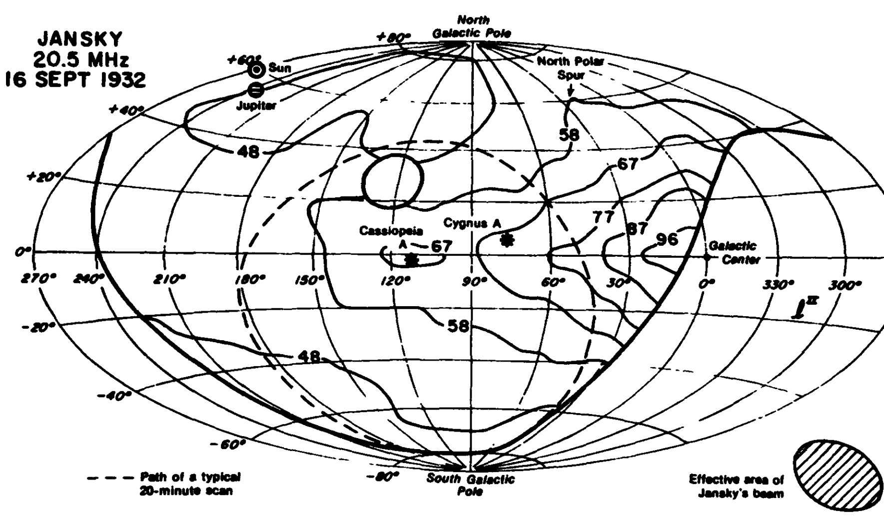


Fig. 7. Sky brightness temperature distribution derived from one day's observations. Each contour level corresponds to ∿1000 K in brightness temperature.

Jansky presented that paper at a Detroit convention of the IRE and gave the Milky Way interpretation, and he writes to his father after that meeting:

> *My trip to Detroit was entirely successful. I had a very good time and my paper was well received. I met a Mr. DeWitt from Nashville, Tennessee, who has been attempting to duplicate my experiments, or rather I should say is attempting to receive the radiations on a wavelength of one meter. So far he has had no success. That, you know, is what I'm going to attempt myself if the powers that be will ever give me enough time from my other jobs.*

DeWitt later in 1940 did detect galactic radio waves, but he is more famous for directing Project Diana, the first radar bounced off the moon, in 1946.

In 1935 and '36, Jansky worked on various kinds of static and ultra short waves, including a little bit of star static. And in 1937 he published a paper in which he makes the statement that "on shorter wavelengths and in the absence of man-made interference, the usable signal strength is generally limited by noise of interstellar origin." In other words, he was pushing the fact that star static was beginning to be of more practical importance once you had good receivers and once you began to get to higher frequencies. He tried to measure the star static at some different wavelengths, but was largely unsuccessful, because by now it was solar maximum and the effect of solar activity on the ionosphere had many adverse consequences for observations at these wavelengths. In fact, this turns out to be a key ingredient to Jansky's success. It was quite by accident he was taking his data in 1932 at the time of the solar minimum every 11 years and this made it much easier to distinguish weak signals over a long period of time.

In the late 30's he began working on a variety of tasks to do with static, for example, motor boat ignition noise, and there are several times where he says that he wants to get back to his star noise, but Friis says that something else is more important to do right now, and maybe he can do the star noise later. At one point he applied for a job at Iowa State (he did not get it) and he made the following statement in May, 1936: "Of course I would ask for the time and facilities to carry on my research [at Iowa State] which would be more than I have had for the last two years here." Until 1950, he continued working on noise from the atmosphere and from receivers. There were three more publications, ending with work on microwave links. During World War II he worked on direction finding of U-boats, finding where the submarine was by triangulation and also by identifying transmitters by their particular signatures. In the late 40's his health became worse and he had a very restricted diet to deal with extremely high blood pressure resulting from his old kidney ailments. This all led to a stroke and he died in 1950 at the age of 44.

Now just a little bit of commentary on some of the developments here. First of all, on the matter of Friis and Jansky. Harald Friis had a distinguished career; he was known as the "baron of Holmdel" and he worked on receiver design, antenna design, coaxial cable, microwave repeaters, and received several medals over his career. He was knighted by the King of Denmark, had 30 patents, etc. He was noted for his ideas on management, famous for his coffee-table conferences in the morning, and ran a very tight ship. That's pretty clear, I think, from what I have seen. The controversy which has developed originates in a book written by John Pfeiffer in 1956, The Changing Universe, in which he claimed essentially that Friis took Jansky off the job - Friis is the bad guy in the story - and how could anyone have done that? This prompted Bell Labs in 1965 to put together a case study on this. "Is this true?" they asked. And so they interviewed all Jansky's old colleagues and predictably came to the conclusion that it wasn't true. Friis in 1965 wrote his own apologetic article in Science saying in effect that this had been charged, but there is nothing to it at all.

Now I've talked to many of his old colleagues myself and there are many different opinions. I've concluded that it's impossible to disentangle with certainty what "really" happened. (When you get into history you find out there is no such thing as "reality".) But most of his colleagues say they never heard Jansky complain about this, that they never saw him upset about it, and I don't think they are lying to any degree at all. But others say that in fact he really wanted to go on and do this work, but he just did not have the opportunity. In 1938 there was an important decision made by Ralph Bown, namely to drop the shortwave work and go to microwaves. And you certainly couldn't do any radio astronomy after that decision was made. And that was the same time that one of his colleagues, Lloyd Espenschied, remembers that there was a discussion about whether to go on with the star noise and that a decision was made by Bown and others not to go on. Now Friis was a much stronger personality than Jansky in the sense of asserting his will in a professional environment, and, despite his article, I think he and his superiors must take some blame for not fully recognizing the significance of this work. But this was, after all, the Telephone Company, its mission was the very practical one of radio communications, and this was the Depression. Jansky did want to do more, but I don't think he ever pressed the point after Friis typically would say, "Well no, let's do that a little bit later." Jansky was a team player - it's clear from one letter where his father exhorts him to be a loyal worker, to honor his boss, and so forth, and in the end things would work out all right. And so there never was any overt issue made of this. Jansky was basically a mild mannered person, although very stubborn and determined in other regards, but he was not one to rock the boat at the office. He had a young family to support, and basically a quite good job for the Depression (he was making $2800 a year by the mid-thirties) which he didn't want to jeopardize. In summary, there was a stalemate, although it is not clear exactly how much further Jansky could have taken his astronomy work before the war. In any case, certainly the post-war development of radio astronomy would not in the least have been affected.

Since there was not the opportunity for radio astronomy to continue at Bell Labs, we might think that maybe the astronomers of the day could have done it. And he did make approaches to the astronomers; he wrote an article all about this in 1933 in Popular Astronomy, the Sky and Telescope of its day. He had some correspondence with Harlow Shapley at Harvard. Shapley was quite enthusiastic about it and said he wanted to talk to some radio boys and see what could be done, but nothing came of it. He visited Henry Norris Russell at Princeton; Joel Stebbins gave a talk at Wisconsin (which Jansky's father attended) at which he said this was the greatest happening since Lindbergh's flight. "The Jansky center of the Galaxy," he called it. But it was just not possible for any astronomical observatory to hire a radio engineer; it was the Depression for them, too. So his discovery was well-known, but was neglected, and also neglected was an appreciation of how it could open up entirely new avenues. You must remember that optical astronomy at that time was only just shifting from visual, looking with your eyes, to photographic techniques, from the refractor to the reflector. There was very little interest in the interstellar medium, in general. The world of decibels and superheterodyne receivers and of sounds called rumbles, clicks, crashes, fluttering, grinds, grunts, grumbling, and hiss was far too removed from that of binary star orbits and stellar evolution for a connection to be forged. I think Grote Reber has put it quite well in an interview:

> *No, I wouldn't say the astronomers were short-sighted. You have to remember, in that day even the photoelectric tube was a mysterious black box; when it came to vacuum tubes and amplifiers, tube circuits and all the rest of it, they just didn't have any comprehension of these matters. And they didn't build radio sets; they weren't even radio amateurs. If they needed a radio, they went off to a store and bought one. And consequently, from their point of view it would be foolish to embark on anything like this. The chances of them going wrong would be about a hundred to one....This branch of physics related to this kind of electromagnetic waves just wasn't part of their repertoire.*

Karl Jansky was made a Fellow of the IRE in 1948; in essence, it was the only award he was given for this fundamental work. Obviously, more should have gone his way and undoubtedly much more would have if he had lived longer. To close, I'd like to quote Karl himself in a letter to Sir Edward Appleton, who had praised him at an URSI meeting in 1948. After Karl had learned about this, he wrote to him:

> *As is quite obvious, the actual discovery, that is, the first recording made of galactic radio noise, was purely accidental and no doubt would have been made sooner or later by others. If there is any credit due me, it is probably for a stubborn curiosity that demanded an explanation for the unknown interference and led me to the long series of recordings necessary for the determination of the actual direction of arrival.*

M. Roberts: I don't understand the situation with Buckley, who apparently called it a very important thing.

Well, Karl in his letter says that Buckley said this. I don't have a transcript of his speech. Obviously, Buckley at that time thought it was great stuff, but apparently didn't follow through. Once again, I think the whole move into shorter and shorter waves - in 1938 there was a major decision made to go to microwaves - made it clear that there was no longer a need to continue the short wave static studies. But I don't really know the answer to that one, since Buckley could have done something in 1934-35.

A. Moffet: Woody, it has always seemed to me that the discussion of whether or not he was "shoved off" the project was a little academic, in that before he would have had time to build up equipment to do things at another wavelength, the war intervened and Bell Labs began doing war work almost *exclusively and probably nothing could have been done until the end of the war anyway. That which was done during the war was serendipitous - the discovery of solar radiation, for example.*

More could have been done before the war, I think, but there is no doubt that the war effort would have quickly interfered. One point I didn't mention - a point made by many of his colleagues - is that he wasn't taken off of "radio astronomy;" there was not such a thing as "radio astronomy." He continued studying static. Now there were many different aspects of static, and this was something that had more fascination to the public and so forth, but it was an aspect of static. And especially in the early thirties, it didn't have too much practical importance; by the late thirties, at least Jansky was arguing that it was becoming the limiting noise. If you want to say he was "taken off something", you should say that he was taken off one aspect of static. He certainly wasn't taken off "radio astronomy" per se.

G. Burbidge: Woody, after his publications weren't there some astronomical investigations? Surely Jesse Greenstein and Whipple wrote something. Weren't there others?

I know that Jesse is going to talk about his contribution, but indeed the only contribution of the 1930s was the paper by Whipple and Greenstein in 1937 in which they tried to explain the origin of this radiation as heated dust and they found it just didn't work.

G. Burbidge: Was that the only one?

That's the only one in the 1930's; there's an abstract of a paper by R. M. Langer at Caltech in which he tried to think of it as being charged dust, but that never got beyond an abstract.

F. Drake: Why is the beam of Jansky's antenna elevated above the horizontal?

Because of the finite conductivity of the ground. There's an image antenna below the actual one and you must consider them both.

N. Broten: What connection, if any, was there between George Southworth and Jansky?

Southworth worked in a separate section at Holmdel. He was sort of a loner - he had a small little group off in a separate building and he was working on microwaves very early in the early thirties. They talked with each other, but they did not work directly with each other. And then during the war Southworth decided to point his small dish at the sun and quite easily detected the sun at microwavelengths.

Marcia Bartusiak: Was the 20.5 MHz signal, as we know it today, coming from a single phenomenon, or a combination?

It's primarily a single phenomenon - simply due to relativistic electrons spiraling in the magnetic field of the galaxy - there's a weak magnetic field and these electrons radiate energy which is very strong at what we now consider the low frequencies that Jansky was using.

M. Kundu: Since Jansky was observing from 1937 to '38, which you mentioned was a time of solar maximum, how come he did not discover solar noise?

Well, first of all, his follow-up work was not done with antennas that were rotatable. They were huge rhombics pointing in one direction and he just had to take the Milky Way when it came past, so that made it tricky. But I'm sure that the sun came past sometimes also. I've never seen any mention (at any time during the 1930's) where he says, "I tried to detect the sun." And that's a little bit of a mystery also.

K. Kellermann: The ionosphere would have been opaque in the daytime.

In the daytime... That's true - that is the best time to see the sun! However, he was working at some higher frequencies where he could have tried it, but you're right, at the lower frequencies, it definitely knocked him out entirely.

K. Kellermann: When you think about it, that's probably one of the most serendipitous parts of the whole thing, that he was working during the time of the solar minimum - his experiments could not have done otherwise.

A. Moffet: Type III bursts are seen at 20 MHz. It is possible to see the sun at that wavelength.

K. Kellermann: Woody, do you know why a specific frequency of 20.5 MHz was picked?

Well, the antenna was designed for 14.5 meters, and then he found that there was interference there, so he moved it to 14.6. 14.5 was chosen after a survey of all possible wavelengths in the spring of 1930. Do you know more?

K. Kellermann: When we were trying to use the Jansky antenna a few weeks ago on the 15 meter amateur band, we noticed that the elements are exactly 12 feet long; the size of pipe that you buy from a plumber shop. 12 feet is a quarter of a wavelength at 20.5 MHz.

When we were organizing this meeting last January, Woody wrote to me and asked, "Can't we have the workshop a few weeks earlier, because my wife is expecting a baby at the end of May." I told him, "No, the date has already been set, April is kind of cold, whereas by May we have nice warm spring weather; and besides it will be in early May, the baby isn't due until late May, so there shouldn't be any problem." Apparently, Woody has just heard this morning that the baby may arrive in early May after all.

Fig. 8. ed.- Sarah Jansky Sullivan was born in Seattle, Washington at 11:50 a.m. EDT on May 5, 1983, just 50 years after Karl Jansky's announcement, and at the same time her father was giving this talk!

KARL GUTHE JANSKY'S SERENDIPITY, ITS IMPACT ON ASTRONOMY AND ITS LESSONS FOR THE FUTURE

John Kraus
Ohio State University

You may not be aware of it, but in the 1950's, I was a member of the ad hoc committee that established the National Radio Astronomy Observatory and set up Green Bank. Those were years of great promise and much of that promise has been fulfilled, but I think it is so exciting to see how radio astronomy continues to move out into new frontiers.

As a boy, in Ann Arbor, I heard the name, Karl Guthe, almost every day. You've heard he was a German-born physicist, how he came to the University of Michigan, and became a famous teacher there. His name was always given the German pronunciation "Goo-ta." My father, a professor of mineralogy at Michigan, spoke of him frequently and I often heard him mentioned by many others on and off the campus. So, I think it is intriguing that the person we are honoring here today was named after this Michigan scientist, and you heard the connection, how Father Jansky studied under Karl Guthe and admired him so much he named his third son Karl Guthe Jansky.

In 1933, I received my doctor's degree in physics from Michigan, and published the results of my dissertation in the Proceedings of the Institute of Radio Engineers, on "Some Characteristics of Ultra High Frequency Transmissions." We were using 5 meter wavelengths, the ones that are now used for television broadcasting, and the article appeared in the September 1933 Proceedings. In the very next issue, the lead article in the Proceedings was Karl Jansky's famous paper. Yes, John Findlay, I noticed that article too and read it as soon as I took the Proceedings out of the wrapper.

Two years later in 1935, when Karl gave his talk at the National Convention of the Institute of Radio Engineers in Detroit's Hotel Statler, I made it a point to attend his talk. His paper was one of the last that was scheduled for the convention that ended on the afternoon of July 3rd. This turned out to be a hot, sultry day, and while rushing down the corridor to the room where Karl was to give his paper, I met some radio engineers coming the other way who asked me if I could direct them to the nearest bathing beach. So attendance was small. There were scarcely two dozen there, and of these I was responsible for bringing five, and as you have heard, Jack deWitt from WSM, who later made the moon bounce experiment, was one of those there.

Whereas most of the papers at this IRE convention were read in a monotone that put you to sleep in a moment, Karl Jansky was an animated speaker, and conveyed the excitement of his discovery that the interstellar interference was received any time his antenna was directed at some part of the Milky Way system. With some excellent slides he went on to talk about the fact that since the interference was not detected from the sun he felt that it must be coming from the interstellar medium in some way. I found Jansky's talk of interest for other reasons too.

Figure 1 shows the Jansky antenna and Figure 2 is an antenna I built in 1935 - a Bruce beam antenna devised by Edmond Bruce, a fellow radio engineer

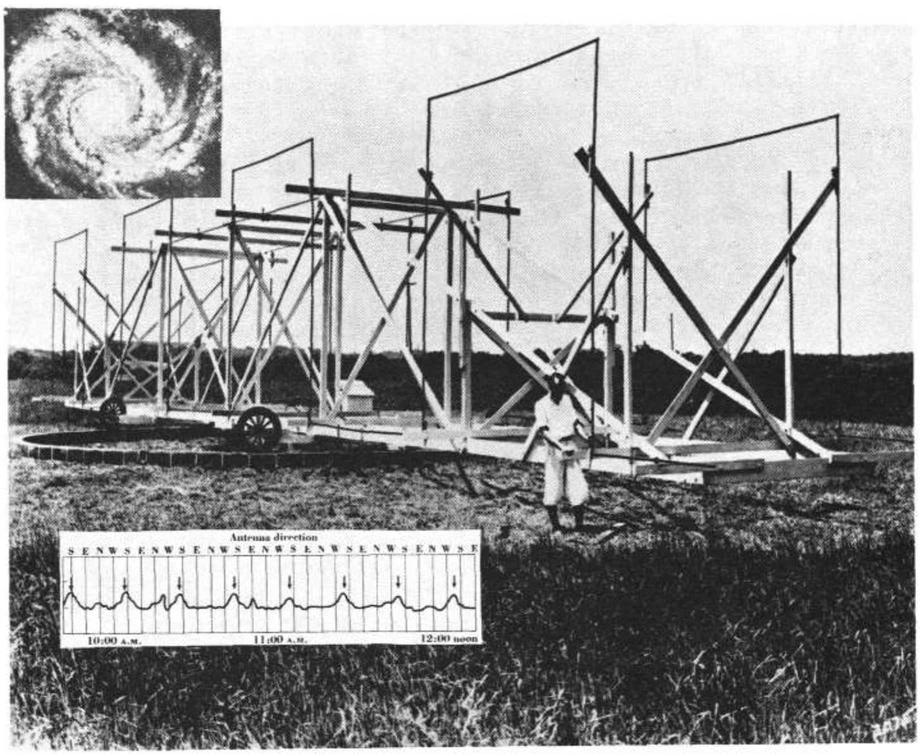


Fig. 1. Karl Jansky and his "merry-go-round" antenna - the first radio telescope with record of hiss noise he detected from our galaxy as indicated by small bumps, one for each revolution of the antenna every 20 minutes.

of Jansky's at the Bell Laboratories. My antenna was of identical design with the same number of sections as Jansky's for use on 20 meters at my amateur station W8JK. Jansky's was designed for 14.6 meters, so mine was a little bigger and was different than Jansky's in that it was fixed; it did not rotate and mine was also bi-directional. He had a reflector curtain behind his to make it uni-directional. I pointed mine toward Australia and the results with amateurs there were phenomenal.

At about this time Edmond Bruce was developing some long-wire antennas for beaming short waves and these were called "rhombics" because of their geometry--diamond shaped. They soon began to replace the Bruce antennas for radio telephone circuits. I recall that on a visit to the Bell Laboratories

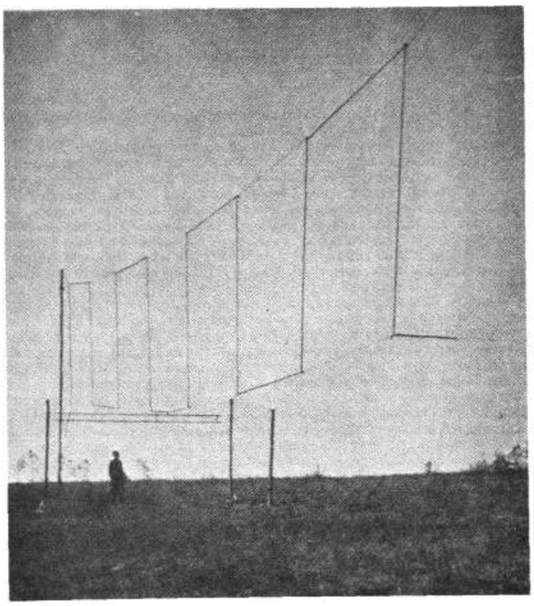


Fig. 2. In February 1935, "I constructed a folded antenna called a Bruce type which was identical to the one Karl Jansky used when he discovered radio waves of extraterrestrial origin except that Jansky's rotated while mine was fixed."

in those days I discussed this trend with Edmond Bruce who reflected wryly that although it was nice to have his name associated with an antenna, it would have been nicer if the type were not one which might soon have only historical interest. The Bruce's were going out, the rhombics were coming in. Well, I could add that even though historical, what a great history, because it was with a Bruce-beam antenna that Jansky discovered radio waves of extra-terrestrial origin. Actually, Bruce's later development, the rhombic, would have been less well-suited for Jansky's purpose because it would have to have been bigger in size, more unwieldy, and it had horrendously big sidelobes which would have made interpretation very difficult.

In the summer of 1933, at the University of Michigan, Arthur Adel and I tried to detect 15 millimeter waves from the sun. We used a one meter diameter search-light mirror as the antenna, and the ammonia absorption line detector of Cleeton and Williams as a receiver. Ours was probably the first radio telescope with a parabolic dish antenna. And although we detected no solar radiation, we were radio astronomers for a moment in 1933. If we had had the kind of receiving equipment available after the war, we might have succeeded.

Well, this experience furthered my interest in Jansky's work. In 1956, Geoffrey Keller and I were hosts of the American Astronomical Society meetings at Ohio State University. As the banquet speaker, I invited C. M. Jansky, Jr., Karl's brother who was a prominent radio engineer from Washington, D. C. to tell about his brother Karl's early work. In response to my invitation, Karl Jansky's sister Mary, and Karl's daughter and son also attended the banquet. So it was a family reunion to do homage to Karl, the founder of radio astronomy, much like the meeting we are having here today.

Then in the 1950's, I was an active sponsor of the name Jansky for the unit of flux density used in radio astronomy, but it took twenty years before this was officially adopted by the IAU. Over the years I've been privileged to have frequent contacts with members of the Jansky family. Karl Jansky's sister, Mary Striffler, lives a few miles from me in a neighboring county, and her daughter, Mary Ann Edwards, lives in Columbus, Ohio. Also some years ago, C. M. Jansky's radio engineering firm, Jansky & Bailey, was engaged by Ohio State University to help solve a problem with a projected television station that could have interfered with our radio telescope. Then one of my graduate students, Reed Crone, related how he had been caretaker and general handyman at the Jansky homestead in Madison, Wisconsin, while he had been attending the university there. At that time Father Jansky, then retired, was still living. I have written a number of articles about Karl Jansky and I have established a repository for Jansky correspondence along with other radio astronomy pioneers in the archives of the Ohio State University library. So from Ann Arbor's Karl Guthe to the present, I have been encompassed by an aura of Jansky lore.

The talk given by Karl's brother at the Astronomical Society meeting in Columbus has been published in Cosmic Search. The full text is given - it's the only place I know that it has been published. Another issue has an article that I wrote on Karl Jansky on the occasion of his fiftieth anniversary of the discovery. There is a third issue of Cosmic Search with an article I wrote about Grote Reber which will tie in with the talk this afternoon.

I don't need to say very much about Karl Jansky's time at Wisconsin and other things because this has been covered in the preceding talks, but I do have some things to say about the Bruce antenna. In Figure 3 are a number of

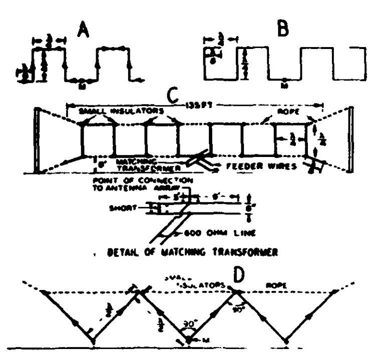


Fig. 3. Folded Types. A, B, and C are Bruce types of various lengths. D is a Chireix-Mesny type. Direction of antenna currents is indicated by arrows in A and D. The matching transformer used to feed the Bruce antenna is shown in detail in C. Antennas in A, B, and D may be fed at the points indicated by the letter M.

designs, that I gave for the Bruce antenna in an article I published in 1935, which I fed with a very simple open-wire transmission line. Figure 4 is a field pattern that I calculated and also shown are the field patterns I measured for my Bruce antenna as compared to a half-way vertical with the same power input.

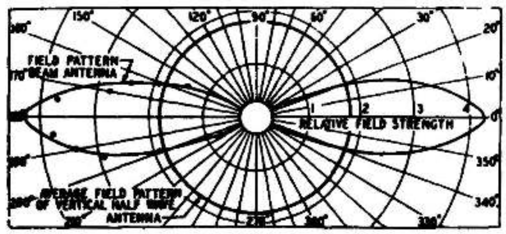


Fig. 4. Field Strength Pattern of Four Wavelength Bruce Beam-Antenna Compared with Vertical Half-Wave Antenna Having Same Power Input. Both antennas used for 14 mc. operation. Small circles indicate experimental values measured at a distance of 550 feet (about 8 wavelengths). The beam gives a stronger signal than the vertical half-wave in two directions over an angle of about 40°.

Figure 5 shows three-dimensional models of the bi-directional patterns of the Bruce antenna compared to the half-wave. So, yes, I had interest in this type of antenna.

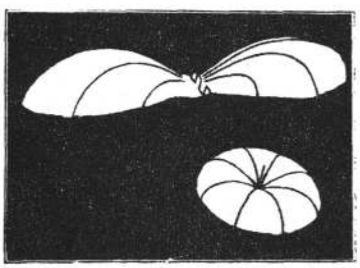


Fig. 5. Three-dimensional patterns of antennas of Fig. 4 with same power to each antenna. This figure and also Figs. 3 and 4 are from my June 1935 article in "R9" magazine.

Now, about Karl Jansky's serendipity. The system he built for studying the direction of arrival of short wave thunderstorm static to assist Bell Labs in getting better radio telephone service to Europe, had several unique features that I would like to mention. First, a directional antenna which to my knowledge was the largest rotatable antenna in existence at that time. Second, a receiver that was as quiet as the state-of-the-art permitted. Third, a receiver responsive to a relatively wide band of wavelengths, much wider than in conventional receivers. And fourth, an averaging arrangement to smooth out the pen trace on the recorder chart. All four of these characteristics are now usually considered essential to a radio telescope. I think that Jansky was the first person to combine all of these things together and in so doing he built the first radio telescope.

Also at the wavelength of 15 meters which Jansky chose, he was able to take advantage of the fact that the galactic radiation was almost at its peak at that wavelength. Yet his wavelength was just short enough not to be blocked by the ionosphere (Figure 6). Thus, if he had gone to a longer wavelength he would have had problems, and at shorter wavelengths the galactic radiation would have been too weak for the equipment available at that time. Furthermore, the solar activity was near a minimum which meant that the ionospheric cut-off was at longer wavelengths than it would have been otherwise.

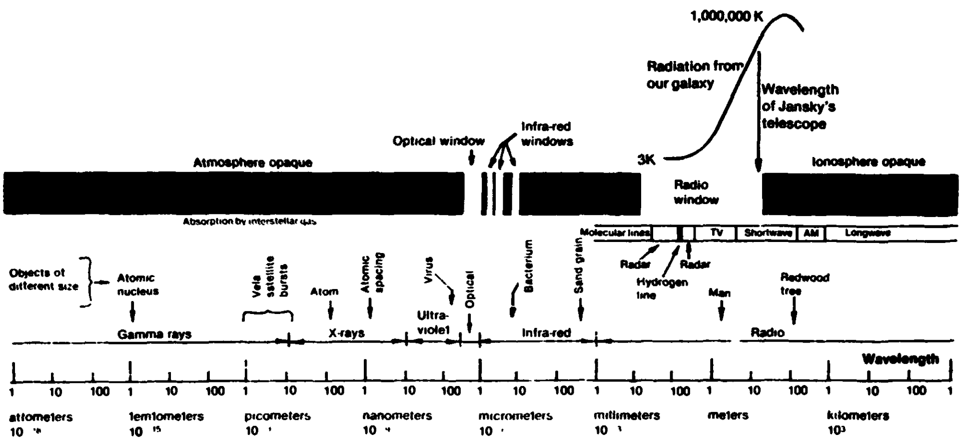


Fig. 6. The electromagnetic spectrum from the shortest gamma rays to the longest radio waves. The opacity of the earth's atmosphere and ionosphere is shown with the optical and radio windows in evidence. The wavelength Jansky chose for the rotating telescope was by chance one where the radiation from our galaxy is strong, as shown by the curve at the top. Luckily, this wavelength is also short enough for the radiation to reach the earth's surface and not be blocked by the ionosphere.

This little bump that Karl saw on his record (Fig. 1) was feeble enough that he could have ignored it or he could have been persuaded not to waste his time investigating it; but Jansky was a true prince of serendipity because the thing he found which he did not expect or anticipate captured his attention like a magnet and he went on to investigate it thoroughly. On a National Broadcasting Company Blue Network program on Monday, May 15, 1933, that's just 50 years ago, Karl Jansky was interviewed about this "hiss-like" static that he had picked up and he said:

> *It seemed to come at all wavelengths. It happens that my observations were made at 14.6 meters, but I feel sure these impulses will be found all up and down the radio spectrum.*

He later did work at 10 meters; Figure 7 shows the non-thermal and thermal spectrum of the galactic radiation. Reber started at short wavelength, assuming that it might be thermal, but not finding it he kept working to longer wavelengths until he picked it up at 1.87 meters.

So, Jansky's prediction was correct; it is found all up and down the radio spectrum. He did plan to make a study of it in the range from 3½ to 10 meters but he was unable to carry out this study being assigned instead to the more practical problem of studying ignition noise from motor boats. However,

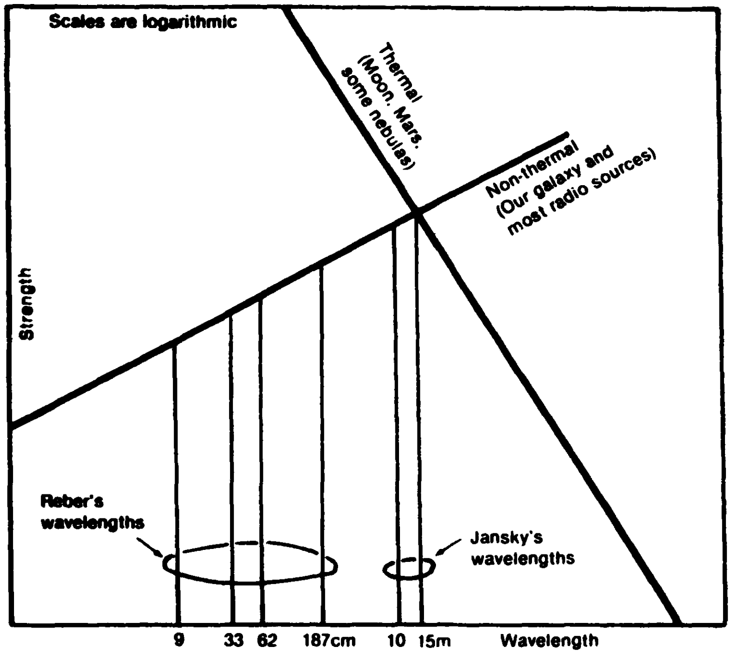


Fig. 7. Strength or flux density (power per unit area per unit bandwidth) of the non-thermal radiation from our galaxy which Jansky and Reber observed. The variation of thermal radiation versus wavelength is also shown. This radiation is characteristic of radio waves from the Moon, Mars, Venus and some gaseous nebulas.

his advice was frequently sought concerning the ultimate sensitivity of receiver systems. In an article in 1936, he pointed out that the weakest signal you can detect is not determined by the receiver itself but by the "star noise" which sets the lower limit to the signal strength that is usable from a given direction at a given time at a given wavelength.

About this time, Jansky also proposed building a new larger antenna to obtain better position and resolution of the star noise. But his proposal was apparently lost in company bureaucracy. Jansky was disappointed and considered a professorship at Iowa State University where he might have the freedom to pursue his break-through, but nothing came of it. He applied there but he didn't get the job. Interestingly, two or three years later I applied at Iowa State and I didn't get the job!

Karl Guthe Jansky died in 1950 at the age of 44 of Bright's disease or nephritis that he had contracted at an early age resulting in a chronic kidney condition from which he never fully recovered. It is regrettable that he didn't live to witness the astronomical revolution that resulted from his discovery. But there were some prior indications from Grote Reber's work prior to World War II and also of others after the war, and Jansky's name is commemorated in the radio astronomy unit, the "jansky", putting him in the illustrious company of other electrical pioneers for whom the watt, ampere, volt, hertz and coulomb are named.

In retrospect, Jansky's equipment could readily have detected decametric radiation from Jupiter but because of its sporadic nature it could easily have been overlooked. Grote Reber recognized this possibility and wished to examine Jansky's records but found, unfortunately, that they had been destroyed.

Now with this as background, I would like to look to the future and consider the phenomenon of serendipity and its possible role. History is replete with serendipitous discoveries. I have a long list here but I won't read it - it would take too long. I've had a couple personal serendipitous experiences. For example, my discovery or invention of the now widely-used helical antenna was serendipitous; also our discovery at Ohio State University of radio sources that turned out to have the highest red shifts was serendipitous.

What are the lessons from Karl Jansky's discovery? I think one of the lessons is that your label as a physicist or astronomer or electrical engineer is less important than being "in the right place with the right equipment doing the right experiment at the right time." This is a quotation from my book, Big Ear.

Let's do a little speculating. Consider four points in time: four hundred years ago, 1583; one hundred years ago, 1883; fifty years ago, 1933; and now.

First, 400 years ago. I will mention only Tycho Brahe and his observatory on the island of Hven, Denmark, shown in Figure 8. This was the most elaborate observatory of the pre-telescopic age; he had the most accurate position sighting instruments and the best clocks available. Note the huge quadrant; the observer is sighting on the star or planet through a slit in the wall; Tycho sits up there directing operations. These are the most accurate clocks available and the man holding the candle is reading the clocks while the assistant seated at the table is recording the data, and all the while the night watchman up here is sleeping soundly. But it was from these very accurate measurements that Johannes Kepler's laws of planetary motion came and from these Isaac Newton's law of universal gravitation. Here we have a case for the value of precise measurements and it is remarkable that this was done before the invention of the telescope.

Let's turn now to 1883, a hundred years ago. Electromagnetic theory had been unified by James Clerk Maxwell, but experimental verification of his ideas had not yet been accomplished. It was not until 1888 that Heinrich Rudolph Hertz constructed the first radio transmitter and receiver and demonstrated that except for their much greater wavelengths radio waves were one with light. In 1883 radio waves were unknown, X-rays had not been discovered, relativity had not been proposed, neither had quantum theory. Cosmic rays were unknown; there were no airplanes, blood letting with leeches was a standard medical cure-all; Edison's incandescent light was making slow headway against the entrenched gas illumination industry. In the United States controversy raged over whether the electric streetcar or the horsedrawn car

Fig. 8. Tycho Brahe and his 17th century observatory on the island of Hven, Denmark. The most elaborate of its time, it had the most accurate position sighting instruments and the best clocks available (The Bettman Archive).

was better, and in England the Red Flag act, not repealed until 1896, required that any self-propelled highway vehicle be preceded by a man carrying a red flag by day and a red light by night! This was the picture only a hundred years ago!

1933: This is the time we are commemorating in this Jansky serendipity workshop. Although Jansky had found radio emission from the center of our Galaxy, no one at that time could have predicted the great astronomical revolution that followed. Physics was undergoing its own revolution, with the discovery of the neutron, the positron, and deuterium. Only three years earlier Ernest Lawrence had built his first cyclotron ushering in an era of high-energy nuclear physics. Penicillin had not been discovered, transistors were unknown, television was only a toy, and jet aircraft and atomic bombs were yet in the future.

Now, 1983, the present: What can we predict for fifty, a hundred or more years ahead? I'll consider three possibilities. Of course, there are others, or a blend of them. (1) Our civilization may be wiped out or greatly retarded by a nuclear holocaust. (2) Our civilization may go into a status quo and remain at a relatively fixed level as did ancient Chinese civilizations. (3) Our civilization may continue to advance technologically and, in particular, to move out into space.

This last possibility is one of promise, and developing it further we can envision a Clarke orbit with a hundred-fold more communication satellites, permanent manned-stations of the earth engaged in manufacturing, space research and astronomical observations, artificial mini-planets between the earth and the sun, where solar power is greater, used for manufacturing purposes, permanent manned colonies on the moon or Mars, exploration of asteroid belt or more distant planets, and finally probes or manned expeditions to the stars. Many of these are feasible now requiring only a spirit of inquiry and exploration and of course capital.

Like aquatic forms venturing onto dry land eons ago, we are now entering an era of human adaptation to a space environment. It is now possible to travel to anyplace on the earth within twenty four hours. One can also communicate by voice and vision with anyone on the earth on an almost instantaneous basis - fractional second delay. In this respect we are at a unique point in history where the entire human family is intimately bound by communication techniques never before possible, and as the years pass will never again be possible. As mankind moves out into space, first to the planets, then to the stars, communication times will stretch from minutes to hours to years, and as Arthur C. Clarke has pointed out, mankind will again be separated as it was two hundred years ago into isolated groups both in space and time.

Now for a few thoughts about what might occur in the future. I have devoted quite a bit of time to studying gravitational waves and generating them. There is no evidence as yet of the detection of the gravity wave via a gravity wave receiver or Doppler tracking of distant spacecraft. Concerning gravity waves, we have an interesting connection with radio in that a hundred years ago we were between Maxwell and Hertz. Radio waves had not been demonstrated. They had been postulated but not detected. Now, we're between Einstein and gravity wave detection. Who is going to be the Hertz of the gravity wave?

I have designed a frequency-modulated continuous-wave gravity wave communication system shown in Figure 9. The transmitter consists of two large

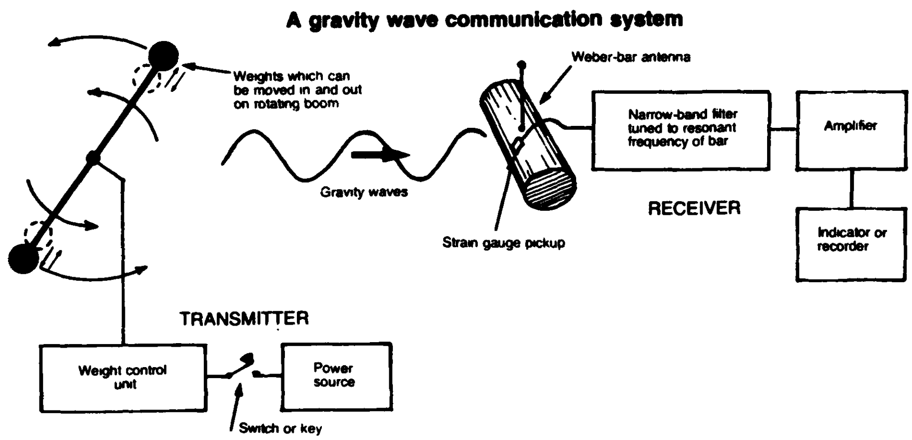


Fig. 9. Arrangement which could be used for gravity wave communication if sensitivity could be increased sufficiently. Rotating boom idles with weights out. With weights pulled in, the boom speeds up sending out gravity waves at frequency to which massive aluminum bar antenna is tuned.

rotating masses on a beam under control of a telegraph key which moves the masses in and out. When the masses are moved in, conservation of momentum results in faster rotation and a higher frequency output. In this way with frequency shift keying, Morse Code transmission is possible. The receiving station has a Weber-bar antenna, and narrow-band receiver tuned to one of the two frequency-shift wavelengths. And so, on paper, this is a gravity wave communication system. But even if we use masses of a hundred tons and a 15 meter length rotating beam spinning up as fast as the beam's elastic limit will allow, the radiated gravitational power is only of the order of 10^{-25} watts. So the arrangement does not appear too practical. Well, think back a hundred years! The commonplace now was not practical or even dreamed of then. According to Professor Douglass of the University of Rochester, gravity wave detection systems are improving much more rapidly than those of radio telescopes. The radio telescope line (Fig. 10) is mine and the gravity line is Douglass'. He predicts we will hit the quantum limit pretty soon. If we're going to get something, we'd better get it before we hit the quantum limit. You see, radio astronomy has been in a position of having detected objects from Jansky all the way down through all the tremendous increase in sensitivity. But whereas gravity wave receivers are being improved, we as yet have detected nothing. How far do we have to go?

Gravitational amplification of radio waves has interesting possibilities; for example, the sun acts as a gravitational lens that would give an amplification of ten million at a one millimeter wavelength. But to utilize this

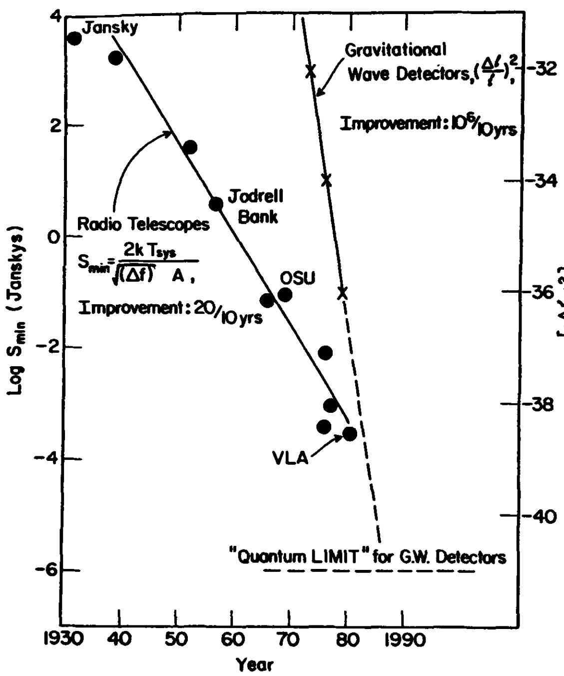


Fig. 10. Rapid rate of improvement of gravity wave detectors as compared with much slower rate of improvement of radio telescopes. Gravity wave data courtesy of Professor David Douglass, University Rochester.

effect, the receiver would have to be 600 astronomical units from the sun or 15 times the distance of Pluto. This seems far out now, but may be a feasible experiment in the future. Another radio technique of the future is a proposal by Soviet radio astronomers of using the near-field far-field transition effect of an antenna to measure distances of radio sources directly with three large radio telescope units (Fig. 11). With two near the orbit of Saturn and another one near the earth it would be possible to measure, in principle, the distance of all detectable objects in the universe directly by this means. Richard Fisher and Alan Sandage might become very much interested in this so as to skip over all of the steps now having to be used to measure these big distances. Thus, a direct measurement is possible.

The Grand Unified Theory postulates magnetic monopoles as well as quarks and other particles, but as yet these theories have not achieved a unification of all of the five forces of physics including gravitation. You might ask the question, "Will this unification come soon, later or never?" Neutrinos may also be a key to the future and might provide the basis for a practical space communication system as shown in Figure 12. The rudiments of such a system have already been tested at Fermilab.

In conclusion, here are some points to ponder:

Will anti-matter be produced and stored in amounts that would facilitate travel to the stars? That would be a beautiful fuel for a starship.

Are there forces or processes of which we are presently unaware?

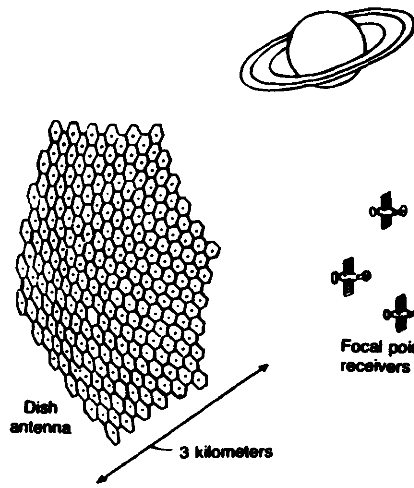


Fig. 11. Proposed Soviet radio telescope with which 3-dimensional or holographic pictures could be obtained of all observable objects in the universe. Planets the size of the earth could be detected from their thermal radiation alone at distances of 100 light-years and planets like Jupiter at 1000 light-years. Two units, each like the one shown, would be deployed near Saturn's orbit with a third unit near the earth.

A Neutrino Communication System

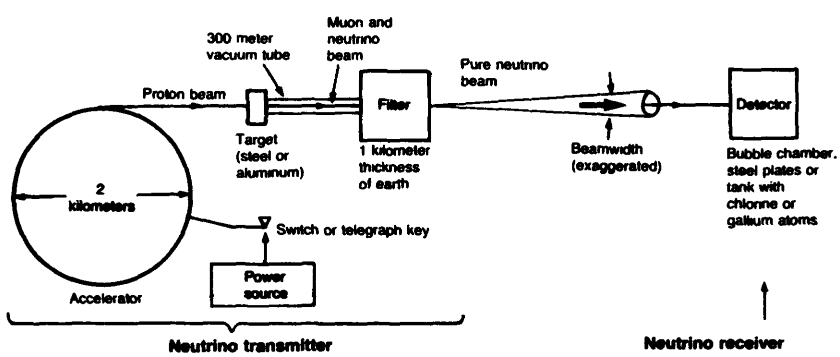


Fig. 12. A neutrino "communication system." We are just approaching a capability when, with large particle accelerators, we can generate copious numbers of neutrinos, but we are still far from developing an interstellar neutrino communication system.

Will extraterrestrial life be detected?

Will the question "What is life?" be answered?

Then, there is the question "Is the velocity of light an ultimate?" There is no physical evidence to the contrary, but let me pose a philosophical or theological question. "Is God restricted to the speed of light? And thereby isolated in real time from essentially all of the universe."

I view the earth with its multitudinous life forms and the mysterious universe beyond with the deepest respect, awe, and wonder. We have witnessed marvelous advances, but the greatest adventures and the most unexpected and serendipitous events may be yet to come. The real future is unpredictable. In four hundred years will our most sophisticated technology of today seem as primitive as Tycho Brahe's observatory now looks to us?

H. G. Wells put it this way:

> *The past is but a beginning of a beginning, and all that is and has been is but the twilight of the dawn.*

Credits:

Fig. 1 is courtesy Bell Telephone Laboratories.

Fig. 2 is from "Big Ear", and Figs. 6, 9, 11 and 12 are from "Our Cosmic Universe", both by John Kraus, Cygnus-Quasar Books, Powell, Ohio.

RADIO ASTRONOMY BETWEEN JANSKY AND REBER

Grote Reber
Bothwell, Tasmania, Australia

John Kraus: Grote Reber is in Tasmania right now. He shuttles between hemispheres in synchronism more or less with the sunspot cycle, and he was not able to come for this talk; but I am very happy to read the paper he sent. I've known Grote personally for almost forty-five years. I was his sponsor for the honorary doctor's degree he received from the Ohio State University, and at my invitation he was a guest scientist at the Ohio State University Radio Observatory for a number of years prior to his leaving for Tasmania for the last sunspot minimum. Now, I'd like to remind you again of these COSMIC SEARCH magazines; there is one issue that has an article I wrote on Grote and his work; it is a historical article.

Figure 1 shows Grote's famous radio telescope he built in his backyard in Wheaton, Illinois. We also have a picture (Fig. 2) of him with Arthur Clarke on a visit of Clarke's to Ohio. I will now present Grote Reber's paper which he sent.

Fig. 1. Grote Reber's dish in Wheaton, Illinois.

Fig. 2. Grote Reber (left) and Arthur C. Clarke on Clarke's visit to Ohio about 1970.

I am reputed to be the only person who was interested in following up the discoveries of Jansky. During later years, I learned there were others. These forgotten people should be given their due.

Fred Whipple and Jesse Greenstein wrote an article "On the Origin of Interstellar Radio Disturbances," Proc. Nat. Acad. Sci., March 1937, p. 177-181. The last sentence says in part "it is ... necessary to investigate ... the dependence of received intensity on wavelength, a problem which is now being attacked by one of the authors." After I arrived in Washington at the National Bureau of Standards in 1948, I asked Fred about this. He told me that, as a graduate student at Harvard in 1936, he considered doing a test to confirm Jansky's discovery. His plan was to put outriggers on the dome of the Harvard 60 inch telescope, then string wires around the ends of the outriggers as a rhombic antenna. When the dome was rotated, the antenna would scan around the horizon. However, Harlow Shapley, the director, decided the whole business was too adventurous. Nobody at Harvard Observatory knew anything about the necessary electronics. No money could be found. The proposal never got beyond the idea stage.[1]

About 1935, Fritz Zwicky became interested in celestial radio waves ("Discovery, Invention, Research," Fritz Zwicky, 1969, p. 90 & 91). He proposed a square turntable with a rhombic antenna supported by slant beams extending from each corner. The device was to scan around the horizon Jansky style. No dimensions were given, but the turntable appeared to be roughly 30 feet on a side with the rhombic antenna perhaps 50 by 100 ft. No wavelength of operation was mentioned. Zwicky suggested a cost of $200. Even in 1935, such a sum would be low by an order of magnitude. A few years ago, I saw a drawing of the proposed device by Russell Porter. Such may still be in the archives at Mount Wilson or Caltech.

The rhombic antenna was really diamond shaped. It was popular during the 1930's because of its relatively broad band characteristic, its simplicity, and promotion by Bell Labs. However, it has high side and back lobes. For radio astronomy, it is much poorer than the Bruce array used by Jansky. Essentially, both proposals were to repeat Jansky's observations using poorer equipment. Both failed to realize that scanning around the horizon has no astronomical value. It merely makes the data difficult to reduce.

About 1935, Potapenko and Folland (professor and student) at Caltech became interested. A short note giving notice of intention appeared in Science News Letter, 29 Feb. 1936, p. 131. Nothing more was heard of the matter. About 1950, Jesse Greenstein showed me pictures of their gear. They went into the desert east of Los Angeles using an ancient Chevy touring car. Small poles were erected and a horizontal dipole strung atop the poles. The observing frequency was near 20 MHz with a battery operated receiver and output on a meter. Apparently no one listened in to learn what was being received. No standard signal generator was available for calibration. No

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[1] Jansky corresponded with Shapley in 1934 about the cost of repeating his experiments. (ed.)

data in form of meter indications versus time was recorded. Sometimes the meter went up, sometimes it went down. This affair can hardly be called science, but deserves a mention.

The Bell Technical Journal, July 1937, carries a long article by Friis and Feldman entitled "Multi-Unit Steerable Antenna" which consisted of six rhombic antennas stretched out in a line along the greatest diagonal of the diamond. The main beam was only a degree or two wide in elevation angle. Operating frequency was in the range of 10 to 20 MHz. The elevation angle could be raised or lowered, or steered, by changing the phase between elements of the antenna. The assembly also had high side lobes, particularly above the main beam. Among data on page 413, are some about star static of Jansky. The intensity was found to change as the main beam was raised or lowered. Fortunately, dates, times, elevation and azimuth are given. I was able to reduce this and found the direction being examined was in Cygnus.

During 1935, R. M. Langer gave a paper before the Berkeley meeting of the American Physical Society. He proposed free electrons combine with multiple ionized dust particles. This action was supposed to produce meter wavelength radio waves. ("Radio Noises from the Galaxy," R. M. Langer, Physical Review Abstracts, Vol. 49, 1935, p. 209).

During the late 1920's, the ten meter band was being explored by amateur radio operators. Several mentions appear in QST and CQ magazines about abnormally high and variable hiss noise coming in on the antenna. These were only during the day and were associated with fade outs. The cause was completely unknown, but certainly not in the receiver. In retrospect, it seems that ten meter solar bursts were being encountered. This appears to be an example of a missed discovery. A search through the above magazines should turn up details. Also, see "The Evolution of Radio Astronomy", by J. S. Hey, 1973, p. 17 and 18. Names are H. W. Newton, D. E. Heightmann, Nakagami and Miya. I also encountered this phenomenon on ten meters during 1928 at Wheaton, Illinois as W9GFZ, but was unaware of its cause. This was a period of high solar activity.

In Tasmania, I have been using the concept of the ionospheric hole. The ionosphere becomes transparent to radio waves at a frequency called the critical frequency. It is proportional to square root of the electron density. At frequencies above the critical frequency, man-made radio waves disappear into the cosmos and celestial radio waves come down to the surface of the earth. If the observing frequency is fixed and the electron density gradually decreases, a small hole will appear first near the zenith. As the critical frequency continues to drop, the hole will become larger. If the electron density is low enough, the hole will expand and envelop the entire celestial hemisphere. During the early 1930's, this was the situation at 14 MHz every night. Some nights it occurred even at 7 MHz a few hours during early morning. Then only stations within 30 miles or so could be heard weakly on the ground wave. The early 30's was a period of low solar activity. At 20 MHz, the hole enveloped the entire hemisphere day and night. This allowed Jansky to use the horizon sweeping technique. A few years earlier, or later, solar activity would have been so high the celestial phenomenon would have been obliterated during the day and confused at night by ionospheric effects. At one place Jansky mentions the maximum intensity as 0.39 microvolt per meter. This is several times maximum intensity on his charts. In retrospect, it seems probable the high value was Jupiter. Another possibility is solar bursts. However, 1932 and 1933 was a period of very low solar activity. I made daily observations of solar radio waves at 160 MHz for many months during the solar activity minimum of 1943 and 1944. Not a single instance of solar bursts was encountered. Again the discovery was missed because of inappropriate timing. Today, with high solar activity and high critical frequencies, Jansky's observations would be impossible. Jansky was an example of getting the right man at the right place doing the right thing at the right time.

I've seen it argued that Marconi cheated; he did not get the letter "S" across the Atlantic on December 12, 1901 at 12:20 P.M. The reason being that it couldn't be done today. The last is quite true. However, 1901 and 1902 was a period of very low solar activity and low D region absorption. The sunspot number was zero for December 1901. Not a single spot was seen all month. Marconi was another example of getting the right man at the right place doing the right thing at the right time. Attempting to guess the past using present conditions is dangerous. I should have been doing hecto (100) meter radio astronomy with Marconi; or even better, during the latter half of the 17th century.

In "Radio Astronomy" by John Kraus (1966), there is mention (on page 9) that Jansky proposed building a large dish. Toward the end of the war, or immediately afterward, I met Jansky for the first time at a conference in Washington. It was a dull affair relating to communications. Neither of us had any business there. I suggested we have lunch together and discuss subjects of mutual interest. He stated that he had arranged to have a noon meal cooked without salt at a nearby restaurant. I joined him and ordered from the standard menu. During the meal, he mentioned that for astronomical studies, a dish with a meridian transit mounting would be the best instrument. The wavelength could be easily changed by merely changing the antenna at the focus. Rotation of the earth would sweep out zones in the sky. The device would operate over at least a decade in wavelength, etc. His reasoning was exactly the same as mine in 1936 before constructing my 31 ft. 5 in. dish during 1937. His dish was to be 100 ft. in diameter with initial observing wavelength of 5 meters. He further stated that during 1936, he drew up a memorandum and proposal for the dish and "sent it up the line, where it became lost." This is not surprising. The expense could not be justified to auditors as appropriate for communications purposes. Contrary to Whipple and Zwicky, Jansky had a very clear idea of the astronomical apparatus needed to carry forward his fundamental discovery.

Thirty years ago, I stumbled across an article by Fredrick A. Kolster entitled "Ultra Short Waves in Radio Communications," (Proc. IRE, Dec. 1934). Operating wavelength was four meters. Figure 13 on page 1349 shows a parabolic reflector 2 wavelengths aperture, or 26 feet diameter. It was made of sheet copper during 1928. The dish was very deep. A magnetic dipole, or loop antenna is at focus which is 1/4 wavelength from the bottom. Thus the F ratio is 1/8. Correspondence during 1979 elicited the information that there were two of these dishes, one for transmitting and one for receiving. Experiments were performed using airplanes with a view to designing an air navigation system. This was <u>not</u> an early radar. The designer of these dishes failed to realize that at least a third of the reflecting area in front of the focus produces inverse phase wavefronts. The same design error may be seen in a lesser manner in the German Wurzburg radar dishes made in 1939.

Finally I come to my work, which, at the time, I probably didn't realize was under an auspicious set of circumstances. In retrospect, this was very true. I had graduated from the electrical engineering school at Armour Institute of Technology in 1933 with specialty in electronics and communications. I had been an amateur radio operator W9GFZ for several years. My operator's license is signed by Herbert Hoover as Secretary of Commerce. I was employed by a major radio receiver manufacturer and was rapidly gaining experience in the design of receivers. The best state-of-the-art test equipment plus machine shop facilities were available. I lived in a small suburb west of Chicago with several vacant lots nearby. These were available for constructing such devices as I wished. The small amount of astronomy needed could be secured easily from elementary text books. I had adequate financial resources of my own. I was not part of, or in any way dependent on, an institution, foundation or school. There were no self-appointed pontiffs looking over my shoulder giving bad advice. During later years, I've attempted, rather successfully, to maintain this freedom and independence, which I value so highly. Finally, I was still young and ambitious. It was another chance circumstance getting the right man in the right place doing the right thing at the right time. I relate the story in considerable detail in the January 1958 issue of the Proceedings Inst. of Radio Engineers.

The peculiar contraption I built was an item of local curiosity at first. After a few months, it became merely a local land mark like churches, schools, the city hall, or courthouse. However, cars frequently would stop, people would get out, walk around my device and take pictures. A few even had the fortitude to ring the door bell and inquire about the purpose of the contraption. These people were obviously strangers. At one time, I considered placing a jukebox out front with a sign "Drop Quarter in Slot and find out what this is all about." During the late 1930's, air navigation rules were not strict. Small putt-putt private planes would frequently come in low, circle around several times, pass over, back and forth. Apparently my dish was a spectacular object of curiosity from the air. Plane ignition systems could easily be heard and measured on the recorder. By 1938, it was obvious that a war was brewing in Europe. Speculation was rife. About this time, one of the putt-putt planes had its engine fail. It just barely made a forced landing in a field a quarter mile away. This event produced stories that I had invented a death ray which would cause plane ignition systems to fail! Fortunately, the rumor soon died out.

It has been pointed out to me that at Wheaton my dish was on concrete piers. At Green Bank, it is on a turntable. How come? By 1940, it was clear that high sensitivity receivers were necessary. Automobile ignition interference was a dominant limitation to observations during the day. Also, severe winters at Wheaton produced a great weight of unbalanced snow. Such could not be dumped off because it froze on tight with ice. This locked up the telescope so that it could not be turned for days until a thaw came. About 1935, Yerkes Observatory became the operating agency for McDonald Observatory. Otto Struve and I discussed the matter. It seemed best to find a location at lower latitude with more southern sky available and milder winters. There was, and still is, a lot of vacant land in west Texas far from man-made electrical disturbances. Something out in the area where Jim Douglas is now was contemplated. If such a move was to be made, the value of the telescope would be greatly enhanced if it had motion in both azimuth and elevation. Accordingly, during 1941, I designed and had constructed a turntable. Then the war came on. The turntable was put into storage in my garage. After the war, circumstances were completely different, so instead of west Texas, the telescope was moved first to Sterling, Virginia, in 1948 and finally to Green Bank, West Virginia in 1959. The sunspot cycle maximum period of 1937, 1948, 1959 has been broken. I hope I never have to reassemble the dish a fourth time.

Several times I have been asked about the early lack of interest in radio astronomy by the astronomical community. In retrospect, there appear to have been two difficulties. First, the astronomers had a nearly complete lack of knowledge of electronic apparatus, viewing it as black magic. Second, and more important, the astrophysicists could not dream up any rational way by which the radio waves could be generated, and since they didn't know of a process, the whole affair was at best a mistake and at worst a hoax. I've encountered this attitude at other times and places. If the why and how are not known, observations are discounted by the intelligentsia. Contrariwise, the engineering fraternity had a clear understanding of the electronic equipment. More important, they were not inhibited by mental hang-ups about the origin of the radio waves. On the latter subject, the attitude was - who cares? - quite materialistic. I was in the middle of two groups not speaking the same language.

Returning to Marconi; the pundits of his day said his ideas would not work because Hertzian waves are similar to light and will not bend around the curvature of the earth. Even after December 12, 1901, many doubted his results because there was no known way Hertzian waves would perform as he reported. However, the cable company believed him. They served him with a writ to cease and desist because they had an exclusive monopoly on transatlantic communication. Once again the intelligentsia fell flat on its face. Today we have big-bang creationism. This religious dogma is vigorously promoted by the intelligentsia. This alone probably condemns it to being wrong. For an antidote to this mental disease, see article by me entitled "A Timeless, Boundless, Equilibrium Universe" (Proc. Astronomical Society of Australia, Vol. 4, No. 4, 1982, p. 482 and 483). Also, see Occasional Paper No. 9 referenced therein; and Physics Today, Nov. 1982, p. 108 & 109. Fundamentally, the kind of things I want to do are the kind establishment men will not have any part of.

John Kraus has a story in his book, "Big Ear," about my mother being Edwin Hubble's teacher. In January 1952, on my way to Hawaii, I stopped in Pasadena and called on Hubble. My mission was to ask him if he could remember the name of his 7th and 8th grade teacher.

"Yes," he replied, "it was Miss Grote."

This was my mother's maiden name. My mother had picked Edwin out as a bright boy and followed his career with admiration. She even bought me a copy of his Rhodes Memorial Lectures given at Oxford in 1936 entitled "Observational Approach to Cosmology." Apparently she hoped they would interest me and be of some assistance. They were, 30 years later. I still have the volume. My mother died in August 1945. Hubble was a tall fellow about 6'2" or better. However, like Lincoln, he didn't look tall sitting down. "The Realm of the Nebulae" by Edwin Hubble, 1936, has been reprinted with a new foreword. (Yale University Press, 1982.) Cloth $30, paper $8.95. It should be required reading for all young (and old) astronomers. Contrary to popular opinion,

Hubble was <u>not</u> a promoter of the expanding universe. He merely used it as a tool similar to a saw by a carpenter.

Life has been a wonderful adventure. A recent medical check-up by a presumably able practitioner gave me a biological age of 50. I suspect there is considerable blarney in this! With luck, I'll keep adventuring another 20 or 30 years. I thank my parents for giving me this opportunity. I thank you for listening.

<u>K. Kellermann:</u> Questions to Grote Reber will have to be handled by correspondence or by telephone. We are truly sorry that he wasn't able to be here with us.

<u>J. Broderick:</u> Maybe on the weekend you could use amateur radio.

<u>K. Kellermann:</u> I am not sure if he still has his amateur radio license.

<u>M. Price:</u> Ken, just one note about the broad range of interest and creativity of Grote Reber. Perhaps some of you from Australia can help me on some of this. I don't recall all the facts; I do know that at one stage when he attended a meeting of what is essentially the Australian Physical Society he gave four different papers; one of these papers was on the construction of the long wavelength array that he was currently undertaking there in Tasmania; one paper was on the carbon dating of the earliest of original campfires found in Tasmania; a third paper was on the effect of the direction of bean spiraling upon the productivity of the bean plant. He made this famous experiment, I believe, within a hundred meters of where we are now sitting. I'll say no more about his experiment. The fourth paper - I don't remember what it was exactly, so I'll need help on this one - it was on a topic related to radio propagation or the ionosphere, but I don't remember exactly.

<u>J. Kraus:</u> Could it have been on cosmic rays? He did a lot of work on that while he was at Ohio State, too.

It was the sidereal component of cosmic rays. (G.R. 16/12/83)

<u>M. Price:</u> It was something that had to do with astronomy, but I don't recall the details of it. The last time I personally visited with him was probably ten years ago; at that time he had three projects going. One was this long wavelength array, of course, that he's been working on in Tasmania. The second was building an electric car which basically had as its body an inverted canoe with a hole cut in it. He pointed out that this got a lot of comment from the locals. The third project was the investigation of the quality of water in a number of mountain streams in the area. He had found one mysterious stream that seemed to have zero bacterial count in it in spite of the fact that it went through pastures etc. And he was trying to find out what the bacteriacide was that was causing all this.

<u>A. Moffet:</u> I can add another Grote Reber story. I met him at the AAS meeting in Yale in 1962 when, I think, he gave the Russell Prize lecture. He had come from Tasmania, and apparently his interest in the spiraling of beans was an extensive interest because he had taken a slow boat to Tasmania and he carefully planted some bean seeds in little boxes in his cabin to see whether

or not they spiraled one way on one side of the equator and the opposite way on the other. I don't remember the results of this bean experiment!

K. Kellermann: I've heard that story but not that version!

J. Broderick: Bart Bok told me a Grote Reber story. Apparently he submitted a paper to the Astrophysical Journal. Instead of a referee, Otto Struve, the editor, sent a kind of a delegation to find out what was going on. They were supposed to have a demonstration of the thing except that it was on a Monday and he couldn't move the antenna because his mother was using it as one end of her clothesline!

K. Kellermann: We'll hear the rest of that story from Jesse Greenstein. I believe that Jesse was one of the first real astronomers to pay any attention to radio astronomy. Nevertheless, during my years at Caltech, he continually referred to the radio astronomers as "Wirewelders."

A few weeks ago, I discovered a carton containing seeds from my bean experiments of early 1960's. I planted some. Peculiarly enough, a few of these twenty year old seeds have sprouted and are coming up! (G.R. 16/12/83)

OPTICAL AND RADIO ASTRONOMERS IN THE EARLY YEARS

Jesse L. Greenstein
California Institute of Technology

I could talk indefinitely about Grote Reber or John Kraus but, fortunately, I have only limited time. I have a beautiful word-processor manuscript on the relation between optical and radio astronomy in the early years which will appear in Woody Sullivan's book, so I will spare you that. At least I got mine in on time, but if the rest of the pioneers don't get theirs in, I shall never see it printed. But already, and less humorously, a lot of people have brought up a lot of important questions. The conference isn't about us pioneers, or you pioneers, or even about Karl Jansky, but about serendipity and how discoveries in new fields are made and how they can be planned. Now we heard how to do it from John Kraus. Get the right man in the right place at the right time with the right instrument when some important phenomenon occurs that has never been seen before. What about unlucky people who might have had only one or two or three of these characteristics simultaneously? When something happens, though, the right guy is a great man and the rest of us are a bunch of old fuds. Enough about that kind of thing; I'm going to change the topic.

Now, first of all, let's look at the second radio telescope shown in Figure 1. This is the originally planned Caltech radio antenna dated 1936.

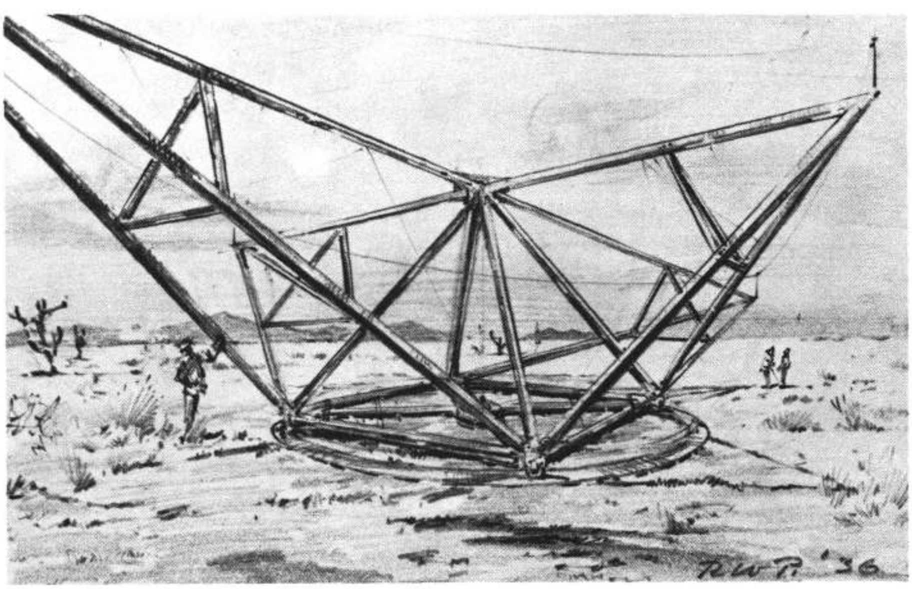

Fig. 1. Russell Porter sketch of 1936 Caltech planned radio telescope.

For those of you who grew up with textbooks on astronomy of the older generation, you know Russell Porter's style (R.W.P. in the corner). This sketch gives the scale, to my eye roughly a hundred and twenty feet. I have no idea what kind of wires are strung around that and I doubt that it is a rhombic. It looks like a dipole, presumably fed at one end, so its got a little asymmetry fore and aft. It's on a rotor; the estimate was that the woodworking shop, lumber and the iron track would cost two thousand dollars.

Potapenko and Folland had in fact checked the existence of the Galactic Center in the Mojave Desert a couple of years after Jansky, as John Kraus has already mentioned, and had also seen the Galactic Center visually! If you're out in the desert in the spring the Galactic Center is quite an impressive thing to see. They strung their wires (Fig. 2) and they found it. And I believe they actually got about the right strength signal, so they believed Jansky. On the basis of that, they developed the antenna shown in Figure 1. Folland was a bright theorist in physics and Potapenko was a gadgeteer, and there it was, a few thousand bucks. But R. A. Millikan said "Don't be silly. It's too much money". Two thousand at that time was too much and the electronics were primitive. So we didn't have a radio antenna in the Mojave Desert in 1936; frankly, it is doubtful that it would have been terribly useful if we had. It's still nice that this sketch exists in the Caltech archives; it establishes priority, of a kind.

Fig. 2. Potapenko and Folland antenna used to detect the Galactic Center in the late 1930s.

So there it is. We had the opportunity - we had even the people - we had the confirmation of Jansky and we didn't do anything. And that is fairly characteristic. Now, what was the impact? Let me say what some impact was. Whipple claimed he was going to build something but clearly he never did. I heard, and I suspect, that Karl Jansky was in touch with Shapley not only about his results in radio astronomy, but about a possible position at Harvard. But I have never seen anything written; the published history of the Harvard Observatory goes only up to the beginning of Shapley's directorship, and does not cover the Jansky period. It would have been really fascinating had there been someone with vision to try. The astronomers who influenced me as a graduate student were already at Harvard. Bart Bok was, in fact, my thesis professor; Donald Menzel taught me a great deal. His theoretical work had an enormous impact on the study of hot gaseous plasmas by optical, and other, means. So Jansky might have made a greater impact than he did. The Popular Astronomy magazine article certainly must have had some influence (Pop. Astron. 41, 548, 1933). So what did I, as a young graduate student and partly a theorist, try to do? I tried to make sense of it! And of course that is very dangerous; we couldn't make sense of it at that period. Our paper (Whipple and Greenstein, Proc. N.A.S. 23, 177, 1937) is really very good; for me it is still impressive how much mathematics I knew. We had to solve the problem of radiative transfer in three dimensions; with curved layers, not the flat, stellar atmosphere thing. It was hard to do and had only been solved by Chandrasekhar, and by a Soviet astronomer, Kosirev, who was also a theorist, before he found volcanoes on the moon which didn't exist. We followed the Chandrasekhar theory of curved layers. We imagined that in the center of the Galaxy, the energy density of starlight and the amount of dust would go up, hand in hand, to build up essentially an enormous supergiant star. With all our diddling with a density increase at the center of the Galaxy of around a hundred thousand, the hottest we could make the dust was thirty degrees. Of course, for those of you who think about it, it is very hard for dust to radiate at longer wavelengths. That was the other killer. Also, we didn't know how to change from volts per meter to ergs per square centimeter, per second. At home, I have an editorial from the Boston Evening Transcript which says "Two Young Harvard Astronomers Fail to Explain Cosmic Static." That's where we all stood for sixteen years. We all failed to explain. What astronomers did, when they thought about it, was to take a census of what is out there in space. It was also clear that Jansky's Galactic Center wasn't a point source. We knew, when we searched with an optical telescope for what was in space, there was hot ionized gas (as in the Orion Nebula), beautiful, hot, colored plasma and gas clouds. They could be seen all over. Their temperature was pretty well known, through Menzel's work, to be ten thousand degrees. That should be the hottest radiation you can get. If a thing is at ten thousand degrees, the internal temperature, the kinetic temperature, the ionization temperature, all are ten thousand degrees. Then, if it covers the whole sky, and the antenna is looking at it, you should match Jansky's rather poorly defined fluxes. The energy distribution was then not known. It was very difficult to be sure, but it seemed that Jansky's data required temperatures near a few hundred thousand. And as further observations were made later, by other people, at lower frequencies, the required temperature reached millions and tens of millions. At the lower frequencies you just can't represent the Galactic Center by any thermal source.

We had heated the dust to 30 degrees; Bob Wilson's black body is 3 degrees, a dust particle in space near, but outside the solar system, is 3 degrees; how do we get a million degrees or more? Of course, there was the solar corona, which in the thirties was a dubious thing, but it seemed to be near a million degrees. Well, maybe we could get up to a million. When more information came in, all hopes for a thermal explanation were ruined, because the new observations contradicted the energy distribution predicted by astrophysics. Many people worked on this, beginning in the early forties. The theoretical spectrum was in no way like the observed radio frequency distribution, insofar as it was known. My strongest memory of my encounters with radio astronomers, after Grote Reber and I worked together was: how do you calibrate? Please calibrate, please put it in decent units, make sure you know the actual absolute flux at different frequencies. And of course, that's not easy; it was easy for me to say, but the data certainly loused up all attempts at theory. The solar corona example, a million degrees, was fairly inspiring, but, as I've said, it didn't help us, as more energy distributions became available. People other than myself who were strongly interested must include Donald Menzel, though he never published anything specifically. His students, of course, include Leo Goldberg, the former director of Kitt Peak National Observatory, a pioneer in science politics, in which he and I were involved for many years.

I see my dear, sweet, kind-worded friend, Geoff Burbidge, sitting in front. He did in fact popularize, in the West, the synchrotron, high-energy-electron, magnetic-field explanation which is the correct one. Why in the devil didn't we all think of it? And one answer is that everybody who thought of it dismissed cyclotron radiation right away. Beginning in the (let's say) 1945 period, it was clear that while cosmic rays were in fact an important part of astronomy, nobody in cosmic-ray physics cared about where they came from. They cared what they were. What were the cosmic rays? Physicists proved they were mostly protons, and secondary products, but they had to find the energy distribution. There was an awful lot of energy out there, about as much energy per unit volume in space in the three degree radiation from the big bang, as there is in cosmic rays. So here we had an obvious explanation. Everything would have been great except for good observational physicists. This is one trouble with the right observer, in the right place, at the <u>wrong</u> time. Experiments showed there was essentially zero electron component in the cosmic radiation. Protons spiraling around magnetic fields don't do much, due to the inverse-fourth-power dependence on mass in the radiation by protons. So that didn't help. And so all reasonable explanations fizzled. I did not say that radio astronomy signals would go away someday, but I didn't know what next to do.

A well-known cosmic ray physicist from the University of Chicago lectured at Yerkes (when I was there in the forties). He said, "Well, the one uninteresting thing about cosmic rays is what causes them. Physicists really use them to study the constitution of matter." So we were turned off at all points, I don't feel terribly guilty about it.

I had a lot of fun working with Grote Reber, and I also have enough Grote Reber stories to keep you here all day. One of the worst parts of Reber's relations with astronomers was the question of his publications of his first maps in the Astrophysical Journal. It wasn't easy; one price he paid for publication was that his theoretical analysis had to be excluded, because several of my colleagues, including bright theorists, thought it wrong. He said charged particles, meaning essentially what we call bremsstrahlung or thermal free-free radiation, but he did it by sort of hand waving. That is not in his published papers; his maps are, and I'm pleased that the Journal had at least the good sense to print them even if they were not understood.

Beginning with Fermi's interest in the acceleration of cosmic rays, after the war, the presence of magnetic fields in space became universally accepted. I'm also guilty and involved with that. In 1951 with the explanation of stellar polarization, I accepted the prevalence of magnetic fields. Radio astronomers have since been very dilatory, never finding fields as large as we claimed we needed. We need 30 microgauss, on the average, unless you take a very specialized kind of dust particle to cause interstellar polarization. Think of all the kinds of troubles we faced essentially for a whole generation. There were no cosmic ray electrons; then they improved the data and there were cosmic ray electrons. But the first cosmic ray electrons had essentially equal numbers of negative and positive electrons. These clearly were pair-produced, therefore they were secondary, therefore there were no electron primary cosmic rays. Electrons should have nothing to do with radio astronomy, except if you use a bit of imagination. The reason there are few cosmic ray electrons is that there are radio signals. They do lose their energy. In the crazy, strong, radio sources, cosmic-ray electrons would drop to zero quickly because they radiate so fast. They have to be replaced continuously. In those days, I think, it would have taken a stretch of the imagination to see a whole galaxy, or its center, replenishing billion-volt or hundred billion-volt electrons on a time scale of a few days, which is necessary.

An answer to "How did we ever get to any consensus and explanation?" is a famous aphorism of a great physicist, Charlie Lauritsen, "Things have got to get worse before they get better." The next part of that is "Who says they're going to get better?" What you needed actually to push astronomers, the optical kind, into helping understand radio astronomy, were extrema, the crazy cases, identifications of sources which happened a lot later. What happened? Everybody and his brother in England, Australia, even in the United States, although we were slightly backward, began finding not point, but small radio sources; not only the Milky Way. There certainly were condensations of the Milky Way, they were also important. But right near the Milky Way there was Cygnus A, the brightest radio source, which lay near the line-of-sight along the spiral arm of our galaxy. It happens that Cygnus A is a very distant galaxy, identified by Baade and Minkowski in 1951 and 1952. It's a seventeenth magnitude galaxy, as I remember; very, very faint optically. But it's the brightest radio source in the constellation Cygnus, as well as the second or third brightest source in the radio sky at most frequencies.

It was the ridiculousness of the fact that if you look for the Andromeda Nebula at the same frequencies, you see it, but it is a very weak radio source. The crazy thing was, it was such an extremum that pushes you now to being in the right place at the right time. No rational explanation that explains the weak emission from the brightest nearby galaxy, the Andromeda Nebula, can also apply to the faint distant radio source, Cygnus A. You have to break down the prejudice that the world is pretty much as you know it, and begin to think of a world which is not like the world you understand.

Supernova remnants were the other extrema. In the constellation Cassiopia, the brightest source in the sky at those frequencies is Cas A.

Essentially invisible, until deep filter-photography by Baade and Minkowski showed these beautiful tiny little wisps of gaseous filaments. In a few years, they change shape and brightness; they move or disappear with internal velocities of many thousands of kilometers a second. One little filament may have distinct velocities of one to six thousand kilometers a second. That is extreme. Novae, exploding stars, are dramatic, but they're not as dramatic as are Zwicky's invention, the supernovae.

A nice thing then happened. I don't want to expound science politics, jealousies and bad feelings, but for some years (about 3), all over the world, people were identifying, with increasing accuracy, more sources with radio telescopes of greater sophistication and sensitivity. They wrote to Baade and Minkowski and they got very good action. In the great two papers published in 1954, but started in 1952, they write, "We are greatly indebted to the members of the radio astronomy groups in Sydney, Cambridge, and Manchester for the generous communications of information in advance of publication." That's how it was in the good years. They were pretty hectic years, it was a very exciting time to be around.

So much for the past. Reber and I wrote an article for The Observatory in 1947, an education to me. I thought astronomy had somewhere to go. Woody Sullivan found in the Caltech archives a letter written in 1946 by me to Otto Struve, but as yet there's no copy of the letter from Struve to me. In this, I tell all the reasons we can't hire Grote Reber as a professor at the University of Chicago with Navy funds. But it is interesting; I didn't remember until Woody told me a few months ago that an astronomical institution had the vision to even think about it. I had the guts to try since I was leaving Yerkes to go to Caltech, Mount Wilson and Palomar. Maybe it was for the best. I can't tell you enough how I enjoyed working with Grote Reber - how much of a pleasure it was!

Two days ago, attempting to clean out files from the third basement of the Caltech Robinson Laboratory, I found my own early correspondence from the year after the National Science Foundation was created. Beginning in 1952, I became chairman of the first advisory committee of the NSF, the one in astronomy, by some odd coincidence. And by another odd coincidence, since mid-Westerners stick together, Robert McMath from Michigan, had bullied Mr. Dodge, who was like the big boss of the OMB. Dodge was the budget chairman, an old friend of McMath's, in the automobile parts business in Detroit. McMath tried to make sure that if the NSF ever had any money to give, astronomers would be there to help funnel it! This first advisory committee had most of the then big shots in astronomy; for two years I was chairman, and for two years more, a member. But my chairmanship was supposed to be one year. From 1952 to 1956 roughly, I was in the middle of the fighting. I found a letter in my files addressed from me to R. J. Seeger of the National Science Foundation, reporting on the conference of January 4-7, 1954. We had a very successful conference to persuade my president, Lee DuBridge, that Caltech should go into radio astronomy. He was a conservative, too. The conference, at the Carnegie Institution of Washington, was sponsored by NSF, Caltech and Carnegie. We had the first really exciting meeting, published in the Journal of Geophysical Research. One of the best papers given was by Charlie Townes, who listed a half-dozen molecular lines that we ought to look for. It was never published; but there is a very brief report by, I think, John Kraus?

B. Burke: Jesse, the essence of Charlie Townes' talk was in fact published when he gave the paper with little additions at the 1955 Manchester Symposium. So it did finally appear.

We had fine papers. The beginning of the battle between the interferometer and the large antenna was fought. I referred officially to the Science Foundation some conclusions of an after-conference, of people like Taffy Bowen of Australia, John Kraus, Bart Bok, DuBridge and myself from Caltech, Minkowski and Tuve from Carnegie, McMillen and Seeger, officials of the new NSF. We told the NSF that radio astronomy had a future, that the U.S. effort is now inadequate. We proposed that the NSF support research at five universities, which will produce the graduate students needed to get us into competition with our Australian and English friends. Next, that they consider, in the long term, a 250 ft. steerable paraboloid, which we have not yet built! We proposed a budget of $250,000 a year, and said that the paraboloid will cost two million. It's interesting what primitive numbers we then had. And we said, pathetically, that astronomers are interested, that the Department of Defense will only support mission-related efforts, and that an ad hoc committee be created for the NSF, to draw up a national plan. It should consist of two chairmen, the physicist Purcell, Bok, Greenstein, Minkowski, John Hagen (who is another real pioneer), and John Kraus. So everybody who was anybody got in on it; some of us are still here.

Another thing I found interesting in this old stuff is the Harvard proposal for its second year of operation which involved Bok, Heeschen, Lilley, Tom Matthews, T. K. Menon, Campbell Wade, and Ewen. The first year of their Harvard research included the 21 centimeter line (no result yet on OH), and they observed the solar eclipse and didn't see anything. A course was given by Bok, Ewen and Münch, visiting professor. A proposal for the next year was for $39,594.50. Times have changed!

Somewhere in that file is my letter recommending that AUI be turned down in its application for $70,000 to make a site survey for a national observatory. For the year when this proposal was submitted, I believe that the Science Foundation's total budget was two hundred and some thousand dollars. So, those times have past.

I've spoken too long about history because this has been a historical meeting. Now I will talk about something a lot closer. I've been in science politics for a long time. I hope to have helped raise a lot of money, and I think that I have. I'll take credit even if not due. The problem is this. All of this, and you'll hear a lot more, breast-beating about "missed opportunities" is now somewhat irrelevant. But it does raise the problem: How do you cope with a new technology? Must we repeat all the errors of twenty years neglect in explanation, twenty years of lack of follow-up in the United States on Jansky's pioneering work? Must we do it over and over again for every new wavelength reached? There are a couple of good general statements of rules on how to operate in science, old rules whose origin I couldn't find. A version is given in Ginzburg's "Key Problems of Physics and Astrophysics" (MIR Publishers, Moscow, 1976). He quotes translated, I guess, three different ways (from English to Russian and back to English again) ideas of Freeman Dyson.

Rule A. *"Don't try to revive past glories."* Don't stick, in other words, to old ways.

Rule B is interesting. It's a contradiction. It says, *"Don't do things just because they are fashionable."*

Rule C. I think Geoff Burbidge knows the third rule by heart. I'll quote it: *"Don't be afraid of the scorn of theoreticians."* Jansky discovered something and there was no theoretical explanation for twenty years. If he had the luck to have met theorists, they would have turned him off the discovery! He would have been a more successful Bell Telephone engineer, but of course we wouldn't be here today. It's rather interesting that, had he listened to anybody, there wouldn't have been a person in the world in physics or astronomy who would have told him to do this experiment. I'm really almost certain of that. Observers? They're even worse. All right, so a new general rule, of my own,

"Don't be afraid of the scorn of theoreticians and observers!"

I have a new, different version of such rules. Because as you get older, you begin to think more about the past and all the opportunities missed.

Rule 1. *"Make sure the data is good."*

Rule 2. *"Do the observation again and again."* It isn't enough to get good data - do it two or three times, independently, of course. The next thing, however, is a more serious generalization, one which is hard to cope with.

Rule 3. *"In any new subject, each scientist must learn more about all possibly relevant physics and look much further afield than he expects."* A scientist never really knows enough physics, by any education or by any experience, to cope with information of a new kind. In a new wavelength region, or with new particles. Say we determine that there are seventeen different kinds of neutrinos, all with different spectra. We don't know what they should be. Maybe we would if some theorist could tell you. The main problem is that you don't know where to begin to look to make sense of this mess. The main thing, I believe, is a strong lesson. Re-educate yourself in physics every couple of years. Even if you use the same technique, there are many new areas to be touched.

My own next two rules are even more self-contradictory. The one I feel most strongly about is:

"Never be too conservative."

The last rule is:

"Don't be stubborn about being radical."

We need a new language every time a bunch of people, such as engineers, physicists, geologists, chemists, whatever, begin piddling around in our big universe. They tell us new things that we never heard of before, such as horrible molecules, all smelling bad.

A pride I had, when I was a young astronomer, was "chemistry has nothing to do with astronomy, thank God." I'm not alone on that. Eddington and others said that nature made the stars simple; everything in stellar interiors could be understood because matter was in the simplest possible form, stripped of all electrons. Eddington said it was simple. It isn't simple. Therefore, we really have a problem; you have to learn all new languages; you have to welcome gladly all new disciplines, and you have to re-educate yourself. If your head bursts, that's too bad for you. There's some young fellow to take your job!

There is a serious idea in Martin Harwit's book, "Cosmic Discovery." He claims we're going to run out of problems. I don't agree with him, but on the other hand, I feel we are running out of wavelength regions. Jansky's was the first great breakthrough. Astronomers used to go from three thousand to ten thousand angstroms. Now, on the long wavelength side, we go on to meters, ten million times longer wavelengths, an incredible change. On the short wavelength side, we use gamma rays, high-energy X-rays. There are real lines in the gamma-ray spectrum; we've covered the whole electromagnetic spectrum, so I don't see all that many surprises left in new technology.

Another major event is that because of the computer we're able to process data at rates higher by 10^n; n is a number as big as you like. We're used to handling it now in more or less real time. The really major breakthrough recently is data handling; not just all the new wavelengths.

We've absorbed the fact that cosmic rays come from the Sun and from the Galaxy, maybe from extragalactic space, from the first birth cry of the universe. They will eventually be part of astronomy. We have mostly failures in neutrino experiments; if there are many kinds of neutrinos, everything in cosmology changes. We got used to magnetic fields, shock waves, and magnetohydrodynamics, and blasts, etc. Sometimes when I read the Astrophysical Journal, a new guy with his Ph.D. uses words from aeronautical theory which I am sure he doesn't understand - which I know I don't understand. But it's part of the recent jargon. So we have the very interesting situation that astronomy may exhaust the means of knowing in all ways except the detection of new kinds of non-interacting particles. I have to close with the fact that supposedly ninety seven percent of the matter in the universe, that some theorists want to exist, is invisible, in no way detectable. There was a big flurry that the neutrinos might "oscillate" from one kind to another, which proved essentially wrong. But what if all the matter of the universe is in a form undetectable by electromagnetic or particle radiation detectors? What do we do next? I believe that in this audience somebody may have an idea of what to do. There must be either a simple or a complicated device. You can't let all the energy, mass-equivalent energy in the universe, 97% of it at least, go around unsought for except by its gravitational field. You've got to find a way to find it, if it is there. If it is not there, it's even worse.

Astronomers have coped with all kinds of things. I remember when 10 or 20 seconds was fast! Now fractions of milliseconds are common in stellar photometry and spectroscopy. We've got pulsars in milliseconds. I don't see a great deal of room for the next Jansky. I don't know where he is, what he will work with, and what he will be looking for. I am not satisfied with the simple recipe that you've got to be the right guy, at the right time et cetera. People say that, it's true, but how do you plan for it?

I don't know.

M. Price: The optical astronomers really should have appreciated the Reber maps because they were kind of in magnitudes. If you look carefully, the isophotes were in dbm, I think, they were just called millivolts. That's the way the isophotes were actually plotted. And it is not easy to turn it into temperature because in the paper after that I don't think he gives enough information about impedances to really do this. But they are in magnitudes.

B. Burke: I want to ask a background question, Jesse, because you and Fred Whipple did publish one of the only three pre-war papers I know of on theoretical interpretations. Did your contributions fall as a drop into a highly dead viscous medium, in other words, was it a subject which bubbled up every now and then in conversation among the theorists at Harvard, or was it forced into the subconscious?

It was bubbling up in a very strange way which just only made it worse. Menzel and I (I don't remember if Leo Goldberg was involved), and possibly Lawrence Aller were doing the quantum mechanical corrections to the free-free emission. If you remember, Menzel and Pekeris the year before had published their classic paper on the radiation from free and bound electrons near stripped ions, i.e. the hydrogenic radiation. What Menzel could do was improve on that and it didn't help at all. There were many papers in Europe. The best paper after the war, I remember, was by a man named Elwert which I thought was final word. None of it changed the shape of the optically-thin hot plasma radiation. It was followed up extensively at Yerkes and three papers by Henyey, and Keenan and myself about improvements, as more radio data came in. It just got worse. There were more points on the radio frequency curve even though we didn't believe the calibration, with good reason, and we had better theory. In Reber's and my article in The Observatory, we predict the n-alpha lines from n = 340 at 5 meters, for hydrogen and also a fine-structure line of hydrogen from an excited state, which was irrelevant. We kept worrying about this, the radio energy was part of astrophysics but it did not fit the rest of what we knew. Nobody got anywhere until these radicals came.

B. Burke: I do remember that the Henyey article has a little lament that if you could only ignore Jansky, theory could be fit quite well.

K. Kellermann: Marc Price commented that Reber published his work in terms of dbm, and Jesse mentioned that he didn't know how to convert this into useful units. Reber told me once that he knew how to do this, but he realized that if he converted his results into temperatures, the temperatures would be so ridiculously high, the astronomers wouldn't believe it and he wouldn't be able to get it published.

IMPACT OF WORLD WAR II ON RADIO ASTRONOMY

Sir Bernard Lovell
Nuffield Radio Astronomy Laboratories

I think it is worth remembering that when the European war began on 3 September 1939, all of Jansky's papers had been published, but only the first two of Reber's and the theoretical paper by Whipple and Greenstein. I'm going to talk about the developments during the war and then some of the later consequences. I think the first example is the only one that is not serendipitous, and that is the remarkable sequence of events in Holland. The circumstances are well known, I'm sure, to many of you. Oort himself has many times described to me the extraordinarily difficult conditions under which they met in secrecy under the German occupation. I would just like to quote from van de Hulst's account in the 1955 IAU Symposium at Jodrell Bank. Somehow or other the copy of the Astrophysical Journal containing Reber's paper had reached Jan Oort during the war, and in the spring of 1944, Oort said to van de Hulst (and I quote),

> *We should have a colloquium on the papers by Reber. And by the way, radio astronomy can really become very important, if there is at least one line in the radio spectrum. Then we can use the method of differential galactic rotation as we do in optical astronomy.*

Well, the future history of that is well known; I simply want to comment that it is one of the few cases, and this arising from those remarkable circumstances in the war, of a planned operation in radio astronomy which was to have such dramatic consequences. There are two other points worth noting about that episode. It is one of the few cases, and certainly, I believe, the earliest, of a classical astronomer realizing that a discovery in the radio part of the spectrum could be important to astronomy. Another point, which I've not often seen mentioned, is that one of the people at that colloquium was C. J. Bakker of the Phillips Research Laboratory, and thereby began the collaboration between industry and radio astronomy which was to become so vital, not only in Holland, but to all the rest of us in subsequent years. You'll forgive me if I talk most of all about the situation in the United Kingdom because they are the best examples of serendipity in the evolution of radio astronomy and, of course, they are also the ones I know most about.

In February 1942, the war situation was very acute as far as we were concerned, and I well remember the utmost distress and depression when we heard on the morning of the 12th February that the German warships Scharnhorst and Gneisenau had passed through the English Channel from Brest to Kiel almost unmolested. This was very serious. There were a number of reasons why they had been able to pass through this narrow stretch of water, heavily defended by the British. The most important one was that all the British radars were jammed. Now through a most peculiar circumstance, this was to have a most dramatic effect on the progress of radio astronomy. The War Office, of course, was very greatly concerned and they immediately ordered the Army Operational Research Group to give priority to the investigation of this problem of the jamming of the British radars. B. F. J. Schonland was the director and he had on his staff, J. S. Hey. Now Hey had been trained as a physicist, in Manchester as a matter of fact. On the outbreak of war he attended a six-weeks course at a Radar School given by J. A. Ratcliffe, and then joined this Army Operational Research Group. He had no previous experience of radar, but even at that time there was so little acquaintance with the subject that Hey was regarded as an expert. And so Schonland directed Hey to investigate this jamming. Well, two weeks later he was confronted with another case of apparently severe enemy jamming, this time of the anti-aircraft gun-laying radars which were working at 4.2 meters. However, the expected bombing attacks did not materialize and Hey discovered that the reports from the radar operators of jamming only occurred in daytime and that the Yagi arrays were all pointing in the direction of the sun. The Royal Greenwich Observatory informed him that on the 28th of February there had been a large sunspot group on the central meridian of the solar disk. Hey reached the correct conclusion that this suspected jamming was due to a solar outburst and he published - no, he was not allowed to - he issued his report in a secret memo which was given limited circulation amongst the radar people at that time.

In retrospect it is very odd that pre-war workers had not discovered these great outbursts from the sun because there were many reports of fading by amateur radio operators, and in particular, there were two Japanese, Nakagami and Miya, who in 1939 suspected that interference was coming from the sun, but they concluded erroneously that the noise was probably originating in the E region of the ionosphere. So, to this very strange but desperate circumstance we owe this serendipitous discovery of the solar radio outbursts.

Two years later Hey was associated with another severe problem. In June of 1944 the Germans began their V-1 missile attacks on London, the buzz-bombs. He modified the anti-aircraft radars, which had Yagi aerials, to look nearly horizontally to detect these buzz-bombs. Eventually by the 6th September, the Allies captured the launching sites on the French coast, and the V1 attacks ceased. But three days later London was subjected to the first of the ballistic rocket attacks, the V-2. Now there was certainly, at that time, no effective counter measure to the V-2. It attained an altitude of 60 miles and reached its target five minutes after lift off. So Hey was instructed to modify the anti-aircraft radars to detect these V-2 rockets in the hope that a few minutes warning could be given to the civilian population, so that the casualties could be minimized. He raised his Yagi aerials to an elevation of 45°, and he did, indeed, discover that he could detect the rockets and give a few minutes warning. But there were two other problems. He also found that he was giving many warnings of attacks when none resulted, and when our intelligence agents reported that there had been no launchings of such rockets. The other problem was that his apparatus was not sensitive enough, his signal noise was very poor, and he tried improving the noise factor of the receiver, but discovered that this had no significant effect. At the end of the war these problems were further investigated by Hey. He discovered that the echoes appearing which were not related to V-2 rockets were due to the ionized trails of meteors. Subsequently, they had an important influence on our own work at Jodrell Bank. Perhaps of more significance to the present conference is his investigation of the fact that his sensitivity was limited by factors which had nothing to do with the receiver.

In 1945 when the European war ended, Hey and a few of his colleagues found time to investigate these phenomena. Someone referred him to the papers by Jansky, particularly the one in which Jansky had suggested that cosmic noise would limit the performance of receivers on certain wavelengths and this led Hey to make his 4.2 meter survey of the sky.

Figure 1 shows the receiver cabin of the 4.2 meter anti-aircraft gun laying radar, actually in Richmond Park. This was an anti-aircraft gun site near London. The Yagi aerial system was modified in late 1945 so that Hey could do a survey of the sky by rotating the cabin horizontally. With this system he produced the first post-war maps published of the meter wave distribution of the cosmic noise.

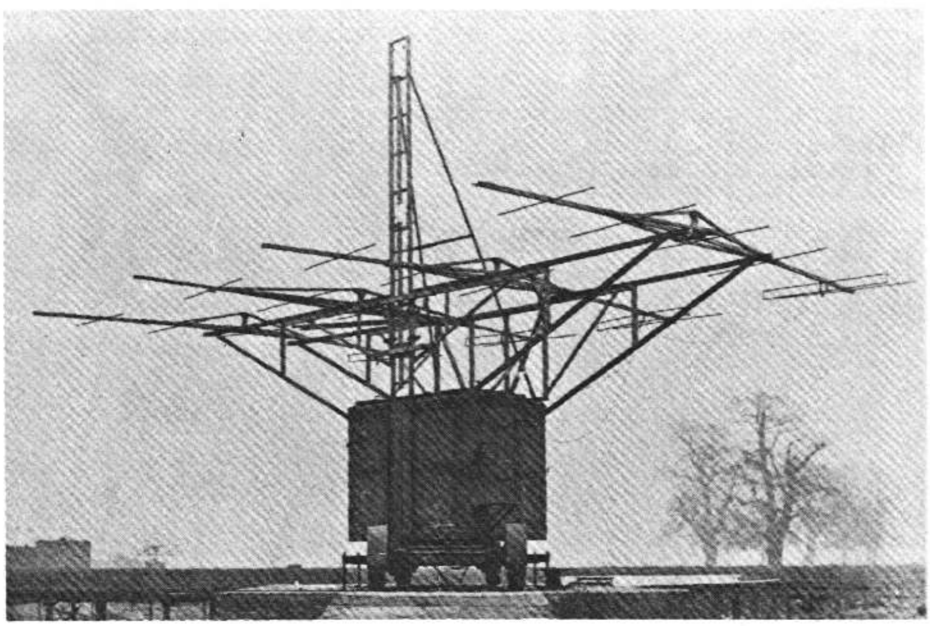

Fig. 1. The receiver cabin of the modified 4.2m anti-aircraft radar system used by Hey and his colleagues in Richmond Park, near London, in the 1946 sky survey.

Shortly afterwards, he made this discovery of the fluctuations in strength of the signals from the direction of Cygnus. By comparison with the sunspot radiation, which is shown in Figure 2, he concluded that the radio noise from Cygnus must be coming from a point source. Well, later on we found that his reasoning was erroneous, the fluctuations are caused by scintillation effects in the Earth's ionosphere, but his conclusion was correct.

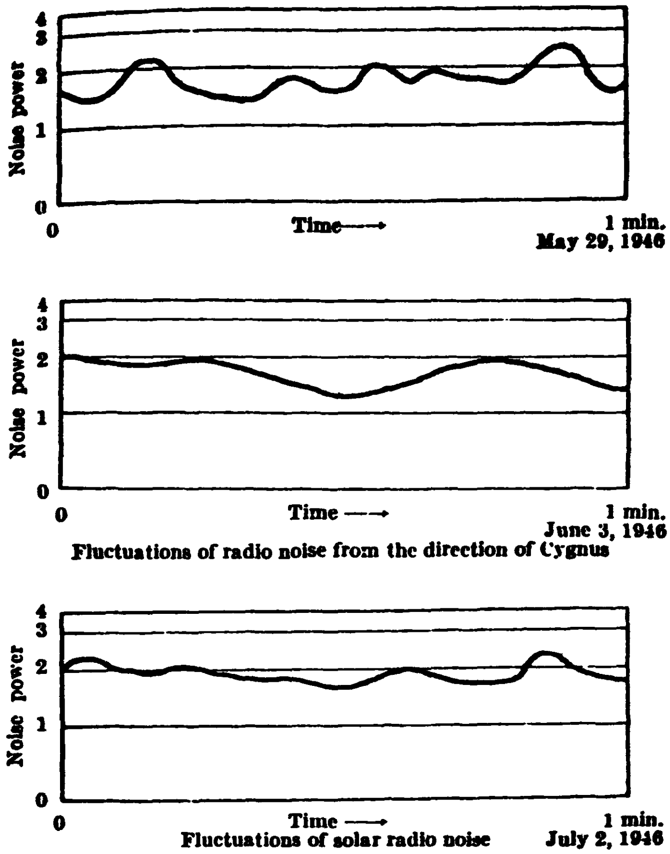

Fig. 2. The fluctuations which led to the discovery of the discrete source in Cygnus.

Hey then made some other studies of the sun and those who are interested in personal relationships might read his book The Evolution of Radio Astronomy in which he discusses his awkward relationship which arose with E. V. Appleton, whose name was mentioned this morning. But that doesn't really concern us here.

In 1949 the AORG group was disbanded and Hey moved to the Radar Research Establishment in Malvern where he built on the runways of the nearby Defford aerodrome two 83-foot diameter radio telescopes, one of which remains an important feature of the present Jodrell MERLIN network.

I would now like to refer to my own situation, if I may, because I think this is a most remarkable series of accidents in serendipity that led to the creation of Jodrell Bank, and to any radio astronomy being done there. In the years before the war I was working on cosmic rays with Blackett in Manchester and in 1939, by using two cloud chambers, had begun to study the large cosmic ray air showers in order to determine the upper limits of the energy spectrum.

Figure 3 shows a photograph that was taken in Manchester with the two cloud chambers a few months before the war began in 1939, and in which we estimated that the energy of this shower was about 10^{16} eV. We had to abandon this work because, even before the war began, the University personnel were

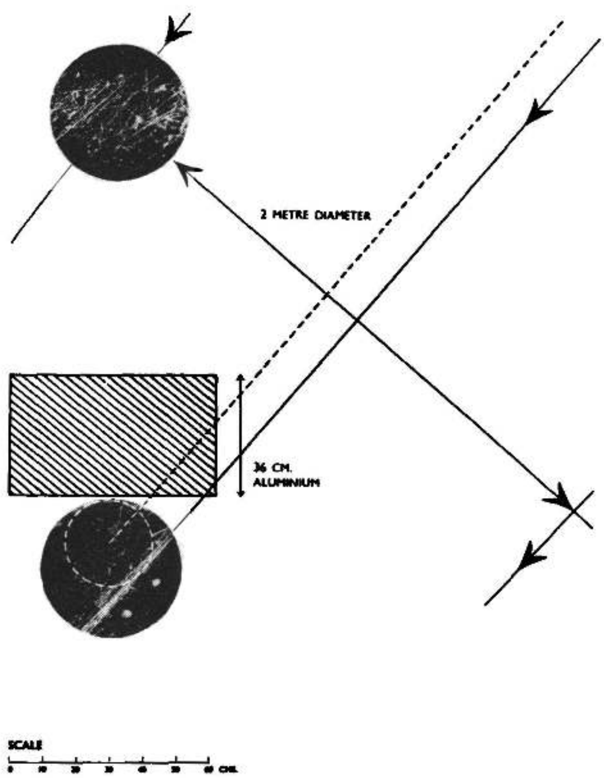

Fig. 3. The double chamber photograph of a high energy cosmic ray shower obtained in 1939 by Lovell and Wilson in Manchester.

introduced into the new radar system and when Prime Minister Chamberlain made his announcement on September 3, 1939, that we were at war with Germany, I was with J. G. Wilson in the receiving station of what was then known as a chain station, CH station. Figure 4 shows another station, not the one we were at. This is a later one of TRE but we were at a similar one, Saxton Wold on the East Coast. The radar defense chain worked in the wavelength range of about 20 to 55 Megahertz, and we were in one of these control rooms on that Sunday morning looking at the CRT display over the shoulders of the Woman's Auxiliary Air Force operator.

Now there were many echoes on the tube and Wilson and I naturally assumed that these echoes indicated the approach of German aircraft. I remember asking the WAAF operator why she didn't report to the Fighter Command Headquarters at Stanmore, and she said, "Oh, those are not enemy aircraft, they're ionosphere". Well, Wilson and I knew a little bit about the ionosphere and we could understand why the ionosphere should give a steady echo at these frequencies, but we couldn't understand why the ionosphere should give a series of separate echoes like this. So we simultaneously turned to one another and said "Radar echoes from cosmic ray showers", and we thought that the use of powerful radars might be a good means of investigating the high energy cosmic ray showers if ever peace returned, and we had an opportunity to pursue our cosmic ray research again. I mentioned this to Blackett, whom I saw frequently, in the early war years, and he was extremely enthusiastic about it. Although I hadn't known at that time, he had recently given a lecture to the Physical Society in London in which he had suggested that the inverse square law over such a wide range of the energy spectrum of cosmic rays might have some deep cosmical significance. He was extremely enthusiastic that we should try to extend the upper limit of this energy spectrum law, and suggested that I should write a paper to outline the possibilities of using radar to detect these large amounts of cosmic ray ionization. War-time aerodromes didn't turn out to be a very good place to write papers. No libraries, and constant enemy attacks; however, I eventually sent a draft to Blackett and he worked on it, and it was eventually published in the Proceedings of the Royal Society under the title "Radio Echoes and Cosmic Ray Showers."

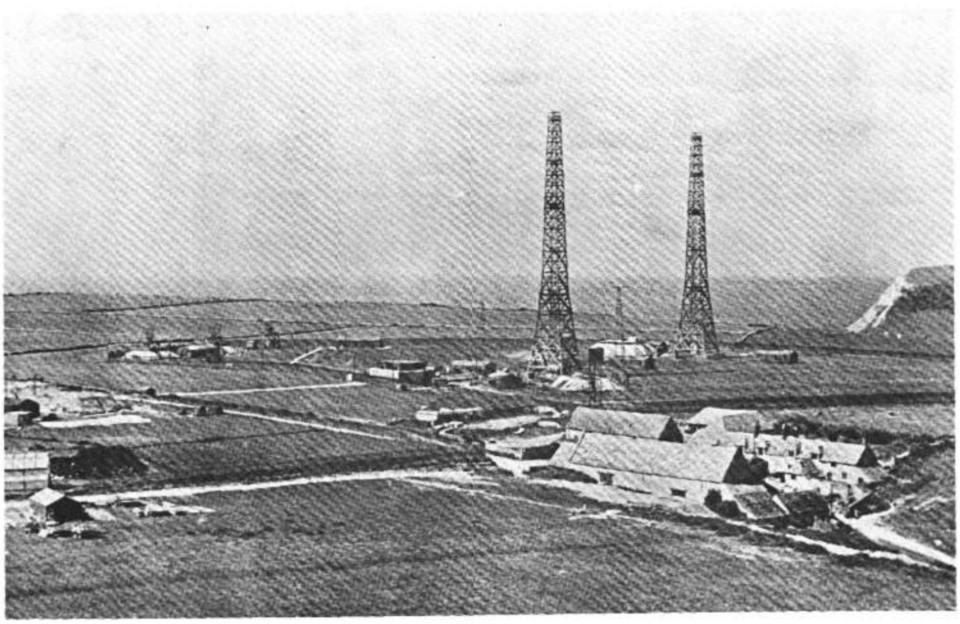

Fig. 4. The Telecommunications Research Establishment at Worth Matravers near Swanage on the Dorset coast in 1940 showing the towers of the operational CH radar station and the huts of the research establishment. (The buildings in the foreground bottom right are farm buildings not occupied by TRE.)

The important point for our purpose here is that I had written the cross section for scattering as the square of (e^2/mc^2). Eventually when peace came, Blackett assumed that I would collect some radar apparatus to start up this investigation. I had been working on centimeter radar for the Royal Air Force, and the calculations indicated that one wanted the long wavelength radar which Hey had been using. Hey very kindly arranged for me to receive an army trailer - three army trailers - a diesel generator, a transmitter and receiver cabin. I installed these in the quadrangle of the University in Manchester, but unfortunately, or fortunately as it turned out, the electric trams in Manchester, which were still running on DC, were adjacent and the cathode ray tube was completely obliterated. The Bursar of the University, whom I consulted, directed me to a botanist whom I asked if he had any land outside the city to which I could take the trailers. And that's how I went to Jodrell Bank, with permission to stay there for two weeks.

Fortunately, I had a camera with me on the first day and in Figure 5 you recognize the trailer of the 4.2 meter receiver. Figure 6 shows the background - the diesel generator stuck in the mud and the cabin of the 4.2 meter transmitter. That's how Jodrell Bank began.

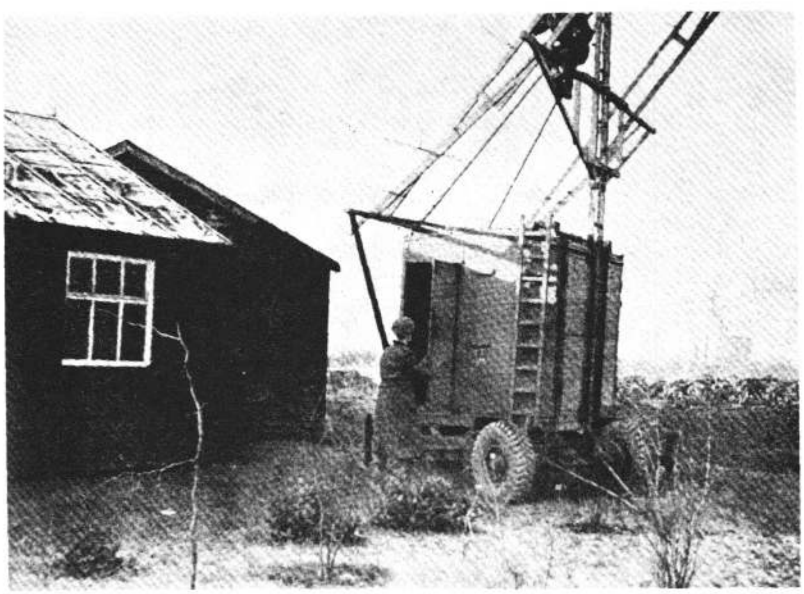

Fig. 5. The first day at Jodrell Bank December 1945. The receiver cabin and Yagi aerial of the 4.2m ex-military radar system, deposited outside huts used by the gardeners of the botanical department.

Fig. 6. The first day at Jodrell Bank December 1945. There was no electricity in the vicinity, so power had to be obtained from a mobile diesel generator which is seen here stuck in the mud.

There was nobody there but two gardeners who eventually helped me to start the diesel. When I got the gear operating, it was the middle of December, and although I did not know it, this was the peak of the Geminid meteor shower. There were many transient radio echoes which I thought must be the echoes from the cosmic ray ionization. I then heard about Hey's work on the meteors and we soon temporarily gave up the idea of investigating cosmic ray ionization and that's how our radar studies of meteors began.

Figure 7 shows Jodrell Bank two years later in 1947, and the point I want to emphasize here is that this is all ex-military equipment. We had no money whatsoever to buy any new equipment. The aerial was built on an Army searchlight mount which we could steer to any part of the sky, and with which we were able to establish the specular reflection properties of the meteor trails and determine the radiants.

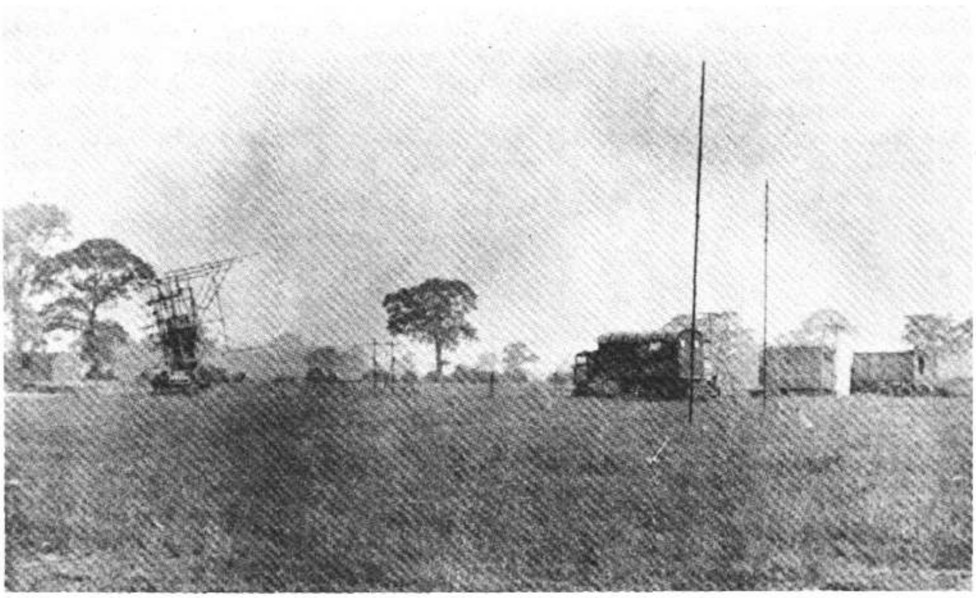

Fig. 7. Jodrell Bank in June 1947. The Yagi aerial system was constructed on an army searchlight mount.

But, Blackett soon reminded me that my real job was to investigate the ionization of cosmic ray showers, and he suggested that I should look again at the calculations of this paper. At the same time, he showed me a letter from T. L. Eckersley, who was a distinguished worker on the ionosphere. Eckersley had drawn Blackett's attention to the fact that I had missed out the factor of $8\pi/3$ in the $(e^2/mc^2)^2$. Blackett was extremely annoyed, but of course the omission of this factor was quite unimportant. But on the other hand, another remark made by Eckersley was extremely important. He said, "Would you ask Lovell if he has studied the effect of the damping factor on the calculations of the reflection coefficient of these large showers." And of course Lovell knew nothing about the damping effect, and when he looked into this he found that a factor of a million was involved. If I had done the calculations correctly, Jodrell Bank would never have started!

At that time I heard of the papers by Jansky and Reber. It sounds remarkable in this audience today, but it was not my line of work. How were we to get the factor of a million to improve the sensitivity of this equipment to study the cosmic ray air showers? Obviously, the simplest thing to do was to improve the sensitivity of the receivers. Immediately we came up against what we've been talking about, that is, the cosmic noise and the work of Jansky and Reber.

We had no money to make more powerful transmitters, and anyhow the transmitter power came into the equations as a square root, and the cosmic noise would limit the possibility of improving the receiver. J. A. Clegg had joined me, and we decided the only thing to do was to build a very big aerial. We started on a broadside array; we had to do the construction ourselves and very soon became discouraged. Then we had the idea of making a wire paraboloid. We had no money, so I applied to the Department of Scientific Industrial Research for a thousand pounds to help build this device. Figure 8 shows the situation at Jodrell Bank in 1948, it probably is not very visible, but you can see the beginning of what was the 218-foot transit telescope. Even at that time the only money we had to make the aerial was that sum of a thousand

Fig. 8. An aerial view of Jodrell Bank in 1948. The mast and some of the early construction of the 218-ft transit telescope can be seen. The circle marks the position in which the Mark I 250-ft telescope was later built.

pounds - I suppose 2 and a half thousand dollars in those years. Figure 9, taken in 1951, I think, shows the complete instrument, and Hanbury-Brown, who had joined me and also Cyril Hazard. If you want another example of serendipity, you must ask Hanbury-Brown how it was that he came to join me at Jodrell Bank to study for the Ph.D. degree. With his arrival the serious work began on the study of the cosmic radio emissions. I'm not going on about that because I think Hanbury Brown himself and Thompson will tell you tomorrow about some of the subsequent developments using that 218 foot transit telescope.

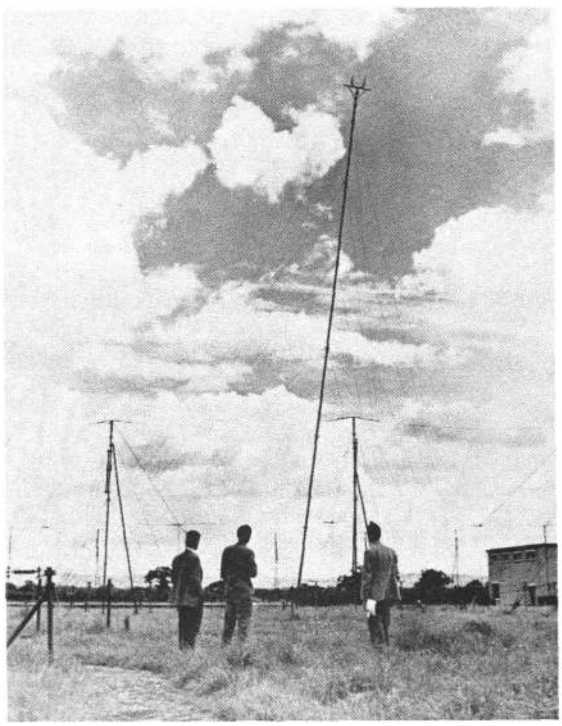

Fig. 9. The 218-ft transit telescope at Jodrell Bank (probable date of photograph about 1951). R. Hanbury Brown and C. Hazard on the left used the telescope to detect the radio emission from M31.

Now I want to say a few words about Ryle. Ryle graduated from Oxford in 1939; he had intended to proceed to Cambridge to do ionospheric research with Ratcliffe, but in the event he came straight into TRE. Now as the war drew to a close, Ratcliffe discussed with Ryle whether he would return to Cambridge. Ryle was willing but had no desire to carry on with the original idea of doing research on the ionosphere. Ratcliffe had already shown me a list of subjects which he had drawn up as possible topics for research, and these naturally included the work on the cosmic radio emission and on the solar radio noise. And so Ryle agreed to return to Cambridge with Ratcliffe and was given the job of attempting to find out whether the sun emitted radiation in the meter waveband when there were no sunspots. Southworth's centimeter measurements were known, but nobody had made any studies of the quiet sun in the meter waveband. And once more there was very little money. Like myself at Jodrell, Ryle was also able to borrow or beg ex-military radar equipment. He also acquired one or more of the German Wurzburg radar paraboloids. So that is how the remarkable developments at Cambridge began - the attempt to find out if the undisturbed sun emitted radio waves in the meter waveband.

Australia, I want to deal with briefly. The CSIRO was established in 1939 to carry out radar research for the Australian armed services, and J. L. Pawsey, who again had worked with Ratcliffe in Cambridge, was one of the leaders in this establishment. He was in charge of the defense radars on the northern coast of Australia and perhaps I could just read Pawsey's own comment on the situation.

> *The Australian development can be traced to the concentration on radar development during World War II. This brought together in a well-equipped Laboratory, a group of able young physicists with experience of radar techniques. At the conclusion of the war these men found themselves without definite commitments and anxious to establish themselves in scientific research. In some countries this situation led to an exodus of the most able people from government radar laboratories. In Australia the high scientific reputation of CSIRO which was being built up under men like its original leader, Sir David Rivett, prevented this trend. The men were actively encouraged by the executive to develop their science within the organization. It was in this environment the first tentative observations were immediately successful and the laboratory was encouraged to venture further into the field.*

Pawsey was working on the north coast, and Dover Heights and Collaroy were two defense radar sites. It was there using some of this equipment that Bolton and Stanley developed the ingenious idea of the radio equivalent of the Lloyds mirror interferometer.

You may not know that in 1944 Pawsey attempted without success to detect radio waves from the Milky Way on centimeter wavelengths. I don't think this paper has ever been published; when I had the task of doing the biographical memoir for The Royal Society after Pawsey died, I had all his documents from Radiophysics Laboratory and this most interesting paper was amongst them. I hope it is still there.

I could say quite a lot about America, but I understand that Fred Haddock is going to follow me. The only thing I want to say is to remind you that presumably Reber was able to continue his work for a little time after 1939, at least until the Americans entered the war in December 1941. Then at the Radiation Laboratory, Dicke developed his radiometer and, as you all know, with Beringer he measured the radiation from the sun and the moon at a wavelength of 1.25 centimeters. But many people here are better qualified than I am to discuss the anomaly which led Dicke to abandon this work, although at that time he had put a limit of 1°K, I believe, on the isotropy of any background radiation. It does indeed seem curious in the light of later developments, which I am sure Dr. Wilson will be able to tell us about, that he did not pursue that work. Although, according to Dicke, he did take his microwave radiometer with him to Princeton in 1946.

The only other comment I would like to make about the developments in America after the war is the rather surprising feature, that there was no immediate build-up on the work of Reber and Jansky. The first consistent series of observations in radio astronomy was at the Naval Research Laboratory, and that again was associated with military matters. The Collins Radio Company built the 50-foot dish, and Ned Ashton, of course whom you are acquainted with here, was the designer, and I think I'm correct in saying that the idea was to develop a radio sextant on radio wavelengths.

Dr. Covington is here who might say a little bit about the Canadian work, but I do not think that in Canada, apart from the use of military equipment, there was any involvement with military apparatus of the type I described in the United Kingdom.

I want to end by showing a few pictures to illustrate that what actually happened during the war technically may not have been the most important influence of the war on the development of radio astronomy. The war had tremendous influence on the number of people doing astronomy. This curve is taken from the book by Struve and Zebergs which shows the number of astronomers in hundreds from the beginning of the century. You see this peculiar gap between the wars and the sudden rise afterwards. I've added the present membership of the International Astronomical Union there - 5,400 I think it is.

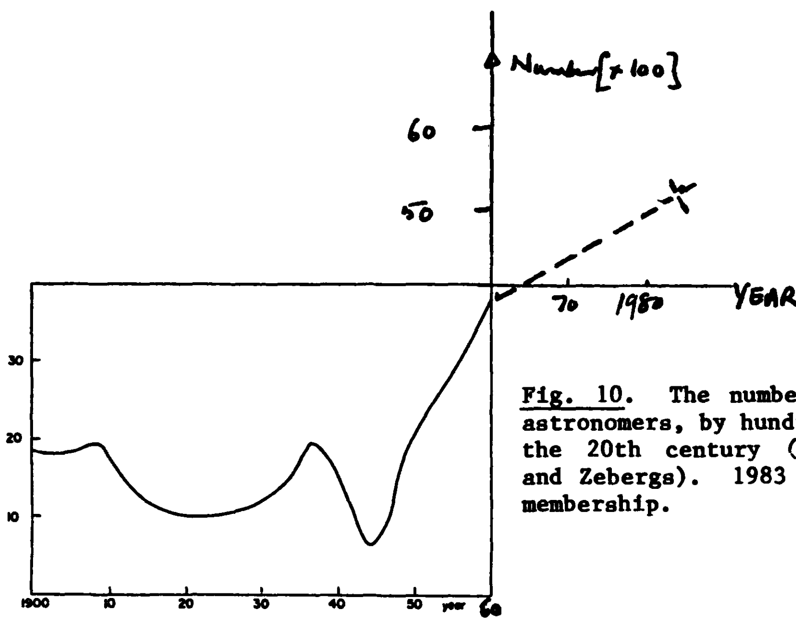

Fig. 10. The number of active astronomers, by hundreds, during the 20th century (from Struve and Zebergs). 1983 point = IAU membership.

Figure 11, I believe, is from Jesse Greenstein's report, correct me if I'm wrong, but this shows the growth of the United States membership of the

IAU. Here you see the dramatic effect which came much later at the time of Sputnik, not immediately after the war.

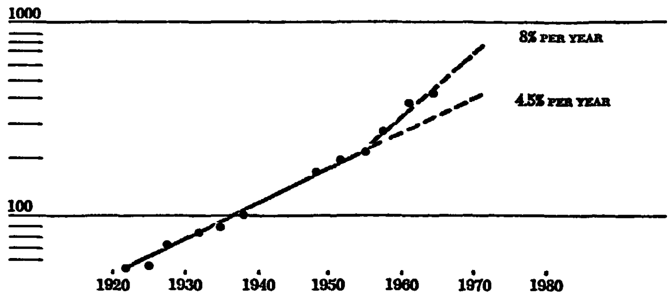

Fig. 11. Growth of U.S. membership in the International Astronomical Union 1921-1963, projected to 1972.

Figure 12 shows the astonishing situation in the United Kingdom. This is the expenditure by the Department of Scientific Industrial Research which was responsible for financing fundamental research in the universities. You see this dramatic growth immediately after the end of the war in 1945.

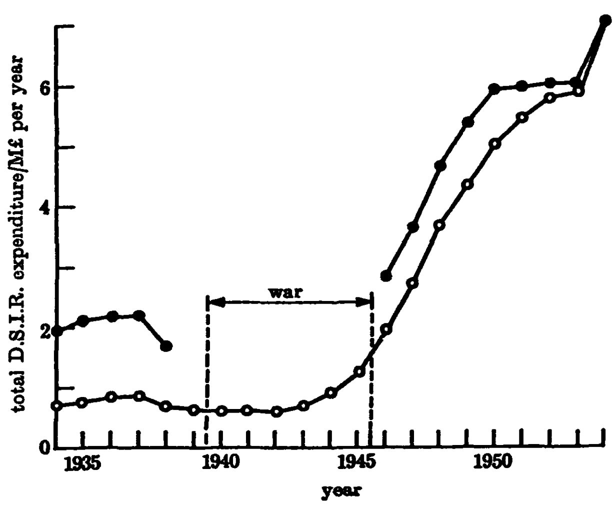

Fig. 12. Total expenditure of D.S.I.R., without allowance for inflation o, in 1954 pounds obtained by using the retail price index •. The point for the financial year April 1934-March 1935 is plotted at 1934 and similarly for later years.

Figure 13 shows the total number of research students supported by the Department of Scientific Industrial Research, and Figure 14 shows the total expenditure. The point is that scientists in this immediate period after the war were held in very great esteem, certainly in the United Kingdom, and it was possible to get money and start up these massive projects.

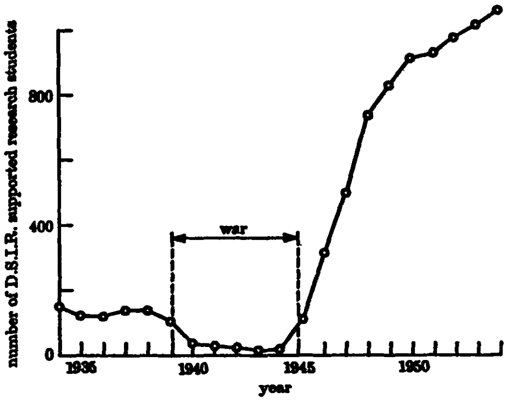

Fig. 13. Total number of research students supported by D.S.I.R. during each year. The point for the academic year October 1934-September 1935 is plotted in 1934 and similarly for later years.

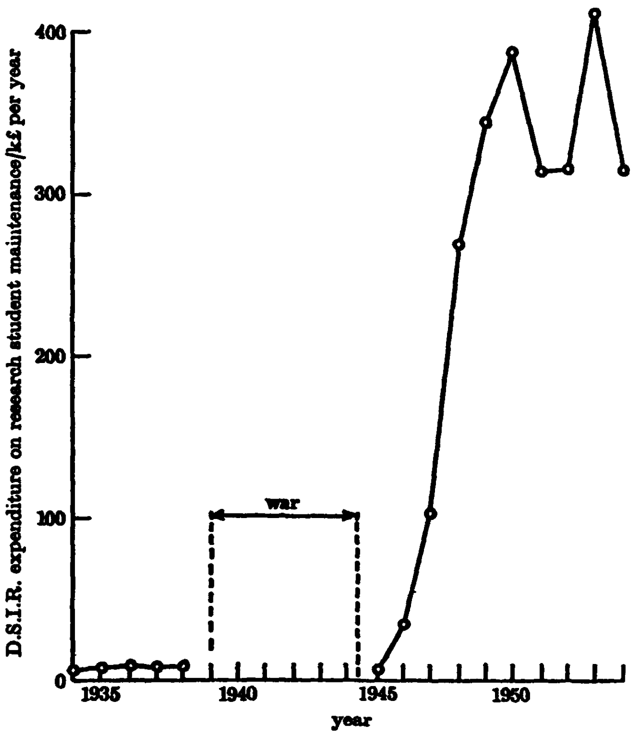

Fig. 14. D.S.I.R. expenditure on grants for scientific investigations in universities; no allowance has been made for inflation. The point for the academic year October 1934-September 1935 is plotted at 1934 and similarly for later years. Information for 1939-49 is not easily available.

Figure 15 shows the difference between the United Kingdom and America. This is taken from the book by Edge and Mulkay and is their analysis of the proportions of space devoted to radio material in two astronomical journals - in the Monthly Notices and in the Astrophysical Journal. You see we have this delay of about 4 years between the curves, with the MN leading Ap. J. A nice illustration of the delay in the impact of radio methods in the United States after the war compared with the UK.

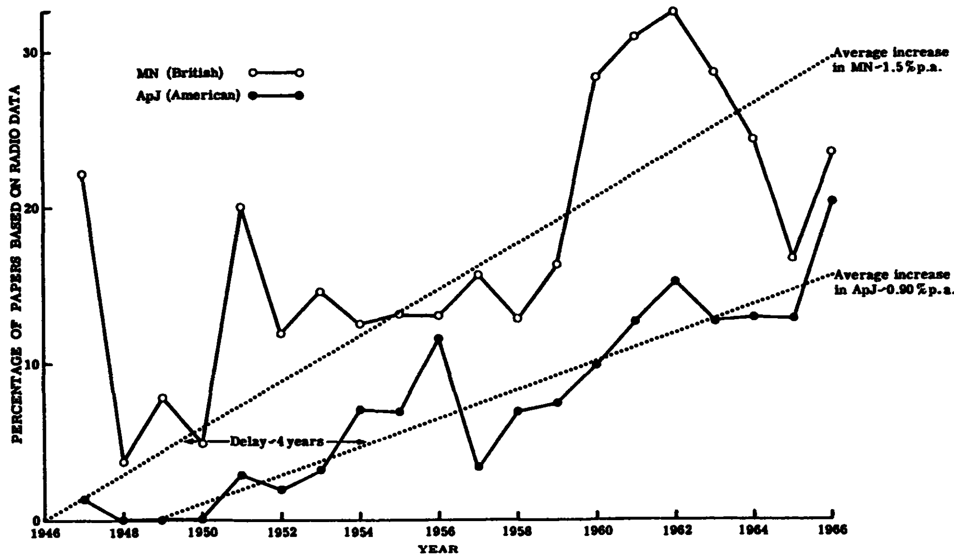

Fig. 15. Proportion of space devoted to radio material in two astronomical journals.

My final comment is that perhaps apart from the actual serendipity of the whole business, the most important feature of the war was that it introduced those of us who were young to completely new techniques, to the use of massive pieces of apparatus, and gave us access to apparatus which would have been impossible to obtain on any university resources. The factors were enormous, and this I think has continued in the way we are funded today. In much of the equipment we use, there has been a massive investment for military purposes which would probably not have been possible for pure research work. So I think it is not only the actual facts of what happened, that is the scientific element, but the change of tempo and the attitude of those of us who were young to what was to become known as "big science", that was probably one of the most important impacts of World War II on the development of radio astronomy.

EARLY RADAR RESEARCH AND A BEGINNING IN RADIO ASTRONOMY

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Herzberg Institute of Astrophysics
National Research Council of Canada, Ottawa

The post war growth of radar and radio astronomy into many branches occurred rapidly, and undoubtedly will be described in many ways and from different viewpoints for future generations. Even though the broad outlines in the post war renaissance of radio astronomy are known, some of the details in its evolution are not known or have been incorrectly presented (Edge & Mulkay 1976). I have been able to make unexpected contributions in studies of microwave solar radio astronomy, and have often been asked the question, "Why did you select the wavelength of 10.7 cm for solar patrol?" An answer will be attempted by outlining some details of critical events in which I have had varying degrees of involvement, from the arrival in North America of the high powered magnetron developed by the British in 1940, to its use in the Canadian Microwave early warning radar, and to last minute preparations for making observations at 10.7 cm during the partial solar eclipse of November 23, 1946.

But first of all, on this occasion when the outcome of Jansky's pioneering studies is so visible at this radio astronomy observatory, I am reminded of my early activities in radio which commenced in 1930, more or less at the same time as Jansky was undertaking his pioneer work at the Bell Telephone Laboratories. They are in sharp contrast to those of Jansky, for I was learning the Morse code and the rudiments of radio sufficient to build an amateur radio station. One technical aspect not to be forgotten was the use of "slop jars" to provide d.c. for the transmitter tube. The station, with call sign VE5CC, was operated successfully for two years until I undertook studies to obtain a commercial radio operator's license; during the post depression period of the 1930's, I was fortunate to be employed as a seasonal wireless operator on ships plying the coast of British Columbia, and it was there, sometime in late 1939 as a graduate student in Physics, that I first became aware of Jansky's work. I was browsing through the library journals and came across a now unidentified article written about Grote Reber. As an ex-wireless operator who has spent many hours listening for possible distress calls amidst static during prescribed silent periods, the idea of using a parabolic reflector to receive static from stars opened new vistas. However, there was little I could do except to discuss it with friends and place the idea in the back of my mind. It resurfaced six years later in 1945.

I graduated from UBC in 1940 with the Master of Arts degree and enrolled in the Graduate School of the Physics Department of the University of California at Berkeley. The course on "Electricity" was given by E. O. Lawrence using the well-known textbook on electricity and magnetism written by Jeans. After a few lectures, Lawrence informed us that he would be absent for an extended period on a visit to the East and assigned a number of problems. Herbert Childs, in his biography of Lawrence published in 1968, relates that Lawrence attended a meeting of the Microwave committee at the home of its chairman, Alfred Lee Loomis, when members of the British Technical Mission disclosed the existence of their high powered magnetron operating at a wavelength of 10 cm. Sir John Cockcroft's photograph of the American and British members of this meeting is reproduced, and in an early perusal of the book I immediately recognized Lawrence and Bowen. But when I read the caption to identify the others, I was surprised to read E. G. (Taffy) Brown, correct initials but wrong family name. Perhaps this name was one used in the interests of security at the time and has survived. I have read that in those times even Niels Bohr was introduced as N. Baker on more than one occasion.

Fig. 1. American and British scientists at home of Alfred L. Loomis, Tuxedo Park, New York, discussing wartime developments in radar arising from the development of a high powered microwave magnetron. E. G. (Taffy) Bowen is third from right. Photograph from "An American Genius, The Life of Ernest Orlando Lawrence, Father of The Cyclotron" by Herbert Childs, E. P. Dutton & Company, 2968, N.Y., courtesy of Sir John Cockcroft.

The need for a special laboratory to develop radar using the intense short wavelengths generated by the magnetron was recognized by those gathered at the Loomis home, and in a few weeks, the Radiation Laboratory located at Massachusetts Institute of Technology was established. The name is the same as Lawrence's Laboratory in Berkeley, given according to Loomis, to honor Lawrence for his work on the cyclotron and as disguise to confuse at a time of pending war. It should be remembered that the American physics community had voluntarily restricted publication of work related to atomic energy. An external view of the 10 cm magnetron which was discussed that Sunday afternoon is shown in Figure 2. The spiral curves of the artistic background suggest

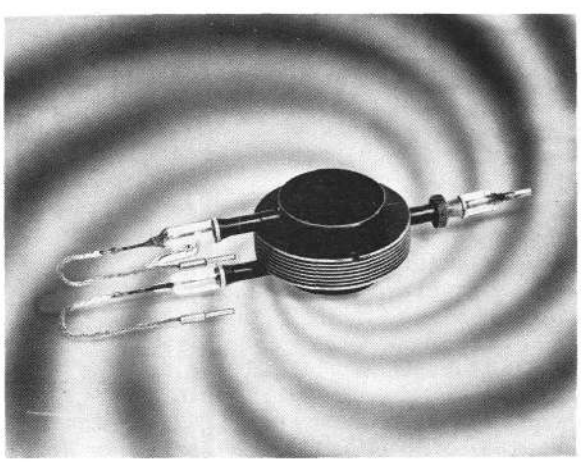

Fig. 2. External view of the 10 cm magnetron described in "Scientists Against Time" by James Phinney Baxter, historian for the Office of Scientific Research and Development as "...the most valuable cargo ever brought to our shores ..." Now in the National Museum of Science and Technology in Ottawa, Canada.

the paths taken by the electrons as they stream from a centrally placed inner cathode to the outer anode under the influence of a magnetic field. In the process of non-destructive examination for the purpose of manufacturing, x-ray views of the anode block were taken to show the nature of the eight Hertzian cavities which serve as electrical oscillators. The upper view in Figure 3 was taken at the Laboratories of the National Research Council (Covington, 1977) while the lower view of the same tube was made later at the Bell Telephone Laboratories when its power capability was being verified (Fiske et al. 1946). Five years ago, the original magnetron tube was "rediscovered" in Ottawa (Phillipson 1977) and is now located in the National Museum of Science and Technology.

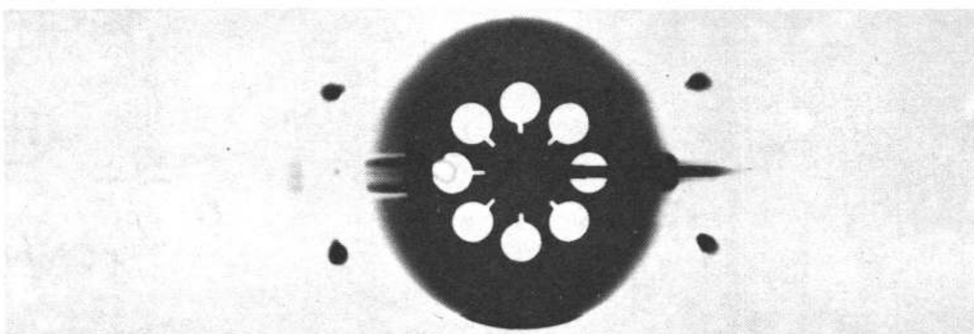

Fig. 3a. X-ray photograph of the 10 cm magnetron from the J. T. Henderson collection of NRC, Ottawa. A similar photograph is shown on page 270, Bell System Technical Journal, Vol. 25, April 1946.

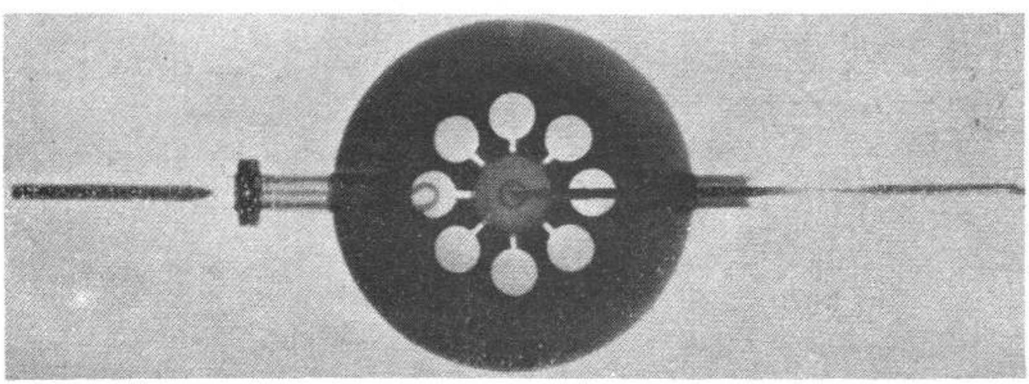

Fig. 3b. An X-ray photograph of the 10 cm magnetron oscillator brought to America by a British delegation in October 1940 and copied at the Bell Laboratories. It is the prototype of most magnetron oscillators in the centimeter wave region developed in Great Britain, the United States and Canada during the war.

When Canada entered the war in 1939, the NRC was already engaged in a program of radar development centered at the Radio Field Station (RFS) located outside Ottawa (Middleton 1981). Growth of the staff was rapid and when space in the first permanent building was unavailable, one technician made antenna patterns from a tent in midwinter so that developments in antennas could continue. An aerial view of the station made a few years later shows a 200 foot high tower used to support the antenna for two radar sets, a rotating array at the top and a variable elevation array on the side, Figure 4. When I

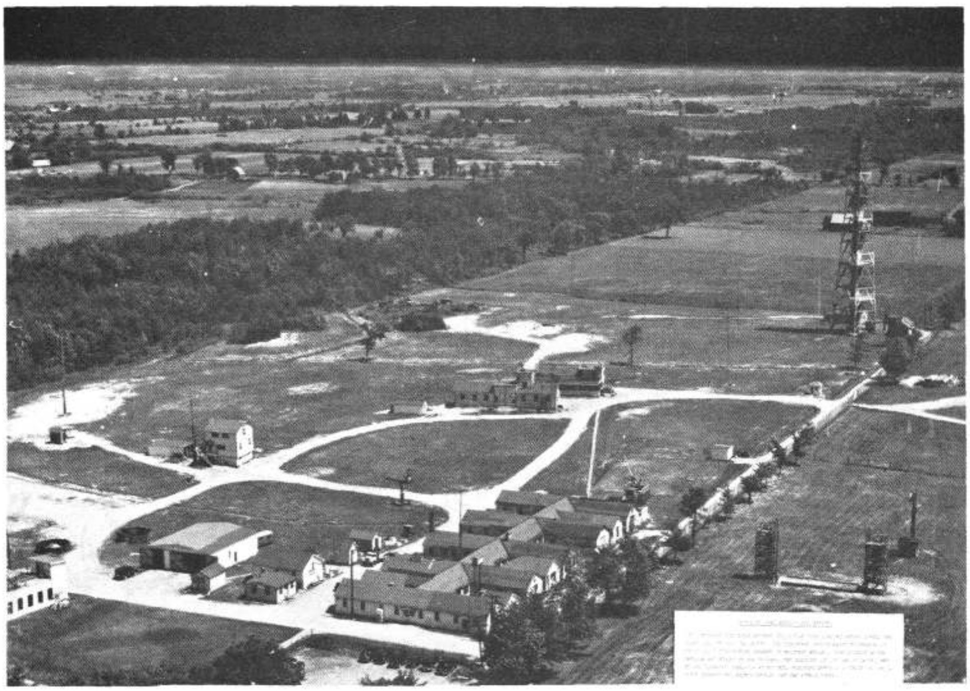

Fig. 4. Aerial view of the Radio Field Station of the National Research Council of Canada outside of Ottawa, circa 1949. The 10.7 cm partial solar eclipse observations were made from the roof of the building in the lower left. The 200 ft tower used for the 150 cm radars is on far right.

arrived at this station in the summer of 1942, my first assignment was to assist in the manufacturing of the 30 MHz IF amplifiers needed for the 200 MHz radar sets on the tower. Shortly afterward, I was assigned a place in the microwave section to design a magnetron connection so that its power would be available in 3 x 1-1/2 inch waveguide transmission line. The work was soon extended to include a microwave receiver for use with the slotted waveguide array placed at the line focus of a parabolic cylinder, 8 ft aperture and about 30 ft long. The superior performance of the microwave early warning (MEW) radar was immediately apparent and stopped further work on the wavelength of 1.5 meters. The needs and demands of wartime situations led to the setting up of a small production line for the manufacture of seven MEW radars at the RFS. An end view of the line showing the antenna on a 30 ft tower is shown in Figure 5 with the 200 ft tower in the background. In June 1945, one of these units was installed at the Winnipeg airport and is probably the first civilian application of this type of radar for the ground control of aircraft.

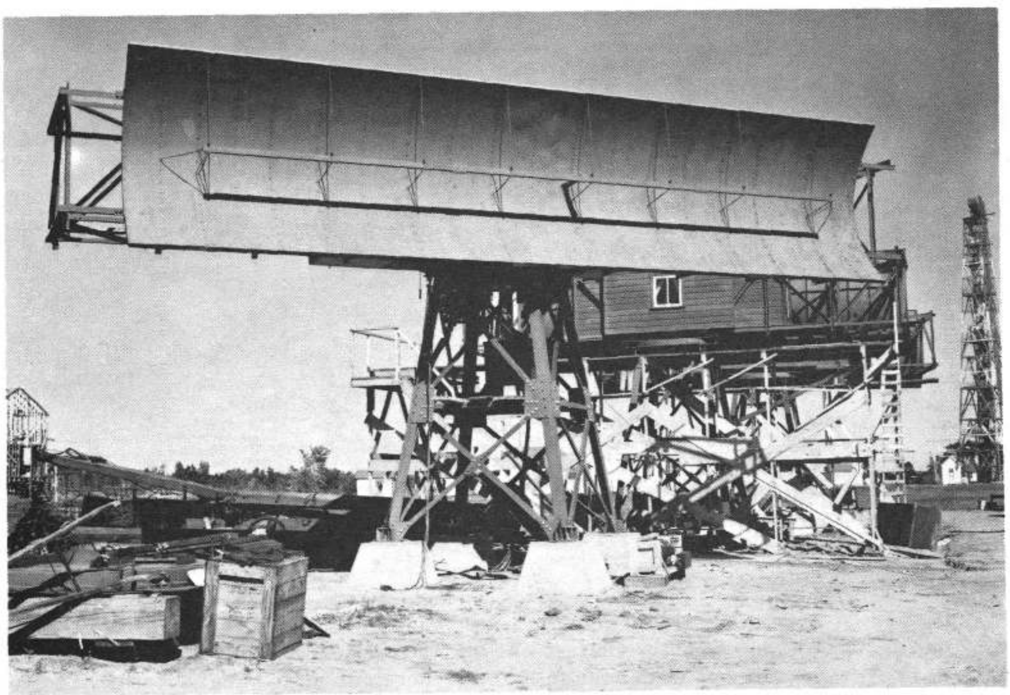

Fig. 5. Assembly line of MEW radar sets at the Radio Field Station, circa 1943. The use of the shorter wavelength displaced the longer wavelength radar using the 200 ft tower seen in the right hand background.

The Radiation Laboratory was dissolved after the war, its technical achievements recorded in twenty eight volumes. These became well known and extensively used by those who were involved in new radio projects which were emerging in various places. Appropriately, the cover and title pages of the series have imprints of the corporate seal of the Radiation Laboratory, based upon an outline of the multi-cavity magnetron centered upon a PPI view of the Boston Area. In Canada at this time, the radar programs of the National Research Council of Canada were re-oriented for civilian needs, and the Defense Research Board was created for continuing the scientific studies needed for defense. It was at this time that I recalled the account of Reber's continuation of Jansky's studies and proposed that research in cosmic static at a wavelength of 10.7 cm be undertaken. A Dicke radiometer had already been made for the maintenance of radar receivers at the Radio Field station and could be used as the receiver for a radio telescope.

This telescope was designed from various radar components by the Mechanical Engineering Section under H. E. Parsons and is shown in Figure 6. When

Fig. 6. Radio telescope constructed for the NRC radio astronomy program from radar components in 1946 using a 4 ft. paraboloid reflector.

a large naked eye spot appeared on the sun, it was first used without a tracking drive as a transit instrument on July 26, 1946. For the next few days, daily drift curves were taken until the spot disappeared, and then the temperature of the relatively spotless disc was calculated from the temperature of the antenna radiation resistance. It was found to be about ten times the expected value of the photosphere temperature of 6000° Kelvin found by G. C. Southworth of the Bell Telephone Laboratories, derived by observations made not only at 10 cm but also at 3 and 1 cm. The discrepancy was extremely puzzling. The next four months were spent reviewing black body theory, checking the various parameters used in the calculations and watching the installation of a tracking motor for the telescope. During this period, W. J. Henderson, Head of the Microwave Section drew my attention to the errata published by Southworth, indicating that the solar temperature was higher than had first been published. This immediately removed the need for further analysis.

The 10.7 cm radio emission from the solar disc was first monitored continuously throughout November 22, one day before making observations of the partial eclipse of the sun. I had learned of this event about two weeks earlier through my wife's comment while reading the newspaper, but unfortunately the radio telescope and radiometer were then undergoing modifications in the machine shops. However, the opportunities for making observations were recognized and enabled the necessary shop priorities to be obtained and overtime work to be undertaken.

The day of the eclipse was clear so that we could use a small telescope to determine when the eclipse started and ended, and when a large sunspot was covered or uncovered. As the eclipse progressed, it was obvious without any detailed analysis, that the variations of noise on the recording meter chart showed that the sunspot was an intense emitter, just how much could not be said in view of the calibration difficulties. A few days later, the record was shown very briefly to Sir Edward Appleton while he was inspecting the radio field station on a previously arranged visit. Only the general terms of the unexpected eclipse observations could be discussed. Ultimately, the calibrations were clarified and with the aid of photographs taken at the Dominion Observatory in Ottawa, the radio observations were reduced to show that the sunspot had an equivalent temperature of 1.5 million degrees and that the solar background was about 50,000° Kelvin. A letter was sent to Nature (Covington 1947).

The excitement of the eclipse observations was soon followed by the sobering thoughts that solar radio emission from sunspots would be variable, and eclipses at a fixed observatory occur infrequently. The initial plans for studying cosmic noise included a precision 30 ft parabolic reflector, which at this time was on the drawing boards of the Mechanical Engineering Section. Its sharper beam would have been an advantage to separate the spot and background radio emissions for different solar conditions, but obviously there was no hope of obtaining it immediately. After a while, it was realized that daily patrol observations of the total flux would provide numerical values for the daily level which could be compared with optical data such as sunspot numbers. Thus in February 1947, patrol observations from mid-morning to sunset were commenced. Almost immediately the anticipated, short lived solar radio bursts were observed and associated with optical flares through the occurrence of sudden ionospheric disturbances in short wave radio circuits. Observations of solar flares as known today were simply not available. The daily level of total radio flux varied slowly with the appearance of sunspots as was expected (Covington 1948). After a few weeks of observations, when sufficient material had been obtained to justify making a qualitative report, I was urged to present a paper for the resumption of the historic annual spring meetings of IRE-URSI held in Washington, D. C. which had been suspended by wartime conditions. My paper, entitled "Microwave solar noise observations at Ottawa, Canada," was presented (Covington 1948), and after the session Karl Jansky and Grote Reber introduced themselves to me.

Since then, even though other solar observations have been made, the daily observations of flux have been continued. Thirty years of total daily solar flux measures from 1947 to 1977 are shown in the two graphs of Figure 7 (Covington 1979). The upper one is comprised of mean flux values, the lower one derived from bars which show the monthly high and low values of the solar emission. The width of the band shows the presence of the 27 day slowly varying component and is an indicator for the sunspot cycle of 11 years. When no sunspots are present for extended periods, such as occurred in 1954, 1965 and 1975, the radio flux is a measure of the undisturbed solar atmosphere defining the basic quiet radio sun. At other times, when sunspots are present, the quiet sun flux may be separated from the slowly varying component by statistical studies of the daily radio flux versus sunspot number. This has

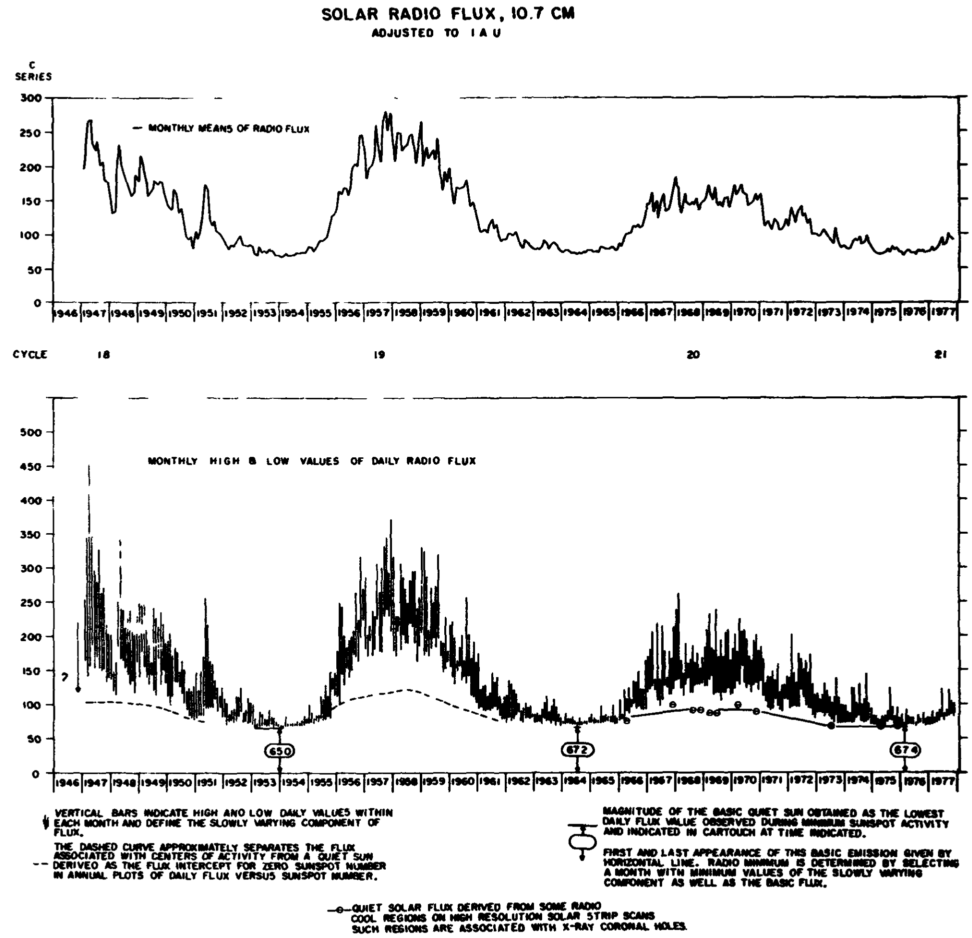

Fig. 7. Thirty years of daily solar flux 10.7 cm values, 1947-1977. Upper curve shows monthly means of the daily flux, the lower curve monthly highs and lows for the slowly varying component associated with sunspots. (A. E. Covington, Journal RASC, 1979 vol. 73, p.1.)

been done for the first two sunspot cycles. During the last sunspot cycle, the cool radio regions seen on high resolution, 10.7 cm strip scans have been associated with coronal x-ray holes and have provided another means of assessing the basic quiet radio sun. Its variation during the three sunspot cycles is indicated by the dashed line. The beginning of the solar patrol program in the Radio and Electrical Engineering Division of the National Research Council of Canada has been entirely serendipitous, arising from plans to study cosmic radio static. The availability of surplus microwave radio equipment on the wavelength of 10.7 cm with which I was familiar determined the wavelength of the initial experiment; the wavelength in turn was that generated by the high powered magnetron needed for the transmitted pulses of an effective radar.

The program of receiving radio noise from the sun matured, providing the means for its own continuation and ultimately enabling broader studies in the new astronomy founded by Jansky to be undertaken.

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Note page 35, line 14, "One team engaged in solar research,..." is hardly correct when only one professional and two technicians were initially involved in the radio astronomy program in 1946. The reference given in note 20, page 68 is in error. In 1950 a group had been formed with four professionals: A. E. Covington, W. J. Medd, G. A. Harvey and N. W. Broten. They were involved in radiometer design, solar patrol, and embarking on programs for the 150 slotted waveguide array high resolution antenna and absolute calibrations. W. J. Henderson was the first section head and was followed by G. A. Miller.

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Hanbury Brown: I wonder if I could ask a silly question. Is the magnetron related to the Colt revolver? There were both made in Birmingham, and they both look very much the same. It looks like they were both bored on the same machine and is that the origin of the 10 cm wavelength?

I heard another story!

Hanbury Brown: I just wondered if the wavelength of 10 cm was determined by the size of the cartridge.

The size of bicycles can be related to the hub of the bicycle wheel.

A. Moffet: I heard a different story which may be apocryphal, and that was that in Oliphant's Lab they were looking for copper plates to use as the end plates and they wound up using British pennies. That more or less determined the size of the cavity in the first ones that were built. They soldered a British penny on the end. I did want to ask you though, how did you settle on 10.7 cm when I think 10 cm radar in British and American military use was 9.2 cm (3200 MHz)?

The early magnetrons were 9.2 cm, and as they strapped them and different people manufactured them, the frequency crept to 10.7 GHz. In maintaining the radar sets of the field station, I looked through these magnetrons and measured their wavelength and put them in different categories.

M. Bartusiak: Were you even thinking of looking at the sun as the first test of the telescope, or was it your wife's perusal of the newspaper that got you that idea?

Oh, my wife's over there! The only theory I've done in my life is when I was trying to figure out black body radiation laws and I was occupied for 3 or 4 months and I didn't bother reading newspapers. We had been alerted for the significance of eclipses. The year previous there was a partial one, and it received a lot of press coverage. I can remember looking at it with my telescope and binoculars, and so when my wife mentioned that another one was coming, I realized the full significance of how you could use it for getting high resolution.

G. McCullough: Taffy Bowen gave a talk at Radiophysics a couple years ago relating to his wartime experiences. He told the story of the magnetrons made by General Electric, I think. When the decision was made to take a couple of the magnetrons to the U.S., on the last afternoon they raced to the factory and got the two that gave the most power. They were packaged up and left on the bridge of the ship with instructions to the captain that if the ship were hit in the Atlantic that was the first thing to go overboard. It wasn't until they were a couple hundred miles off the American Coast that somebody realized that the density of the package was less than one. When they got the magnetrons to America and were met with open arms, everyone was very friendly until somebody came back with some X-rays of the magnetrons they had brought over and discovered that all the descriptions that Taffy had been talking about concerned 6 holes but the X-rays showed 8 holes. There was a lot of confusion as to what had really gone on. That is the reason the British used 6 cavities and the U.S. used 8 cavities for the rest of the War.

U.S. RADIO ASTRONOMY FOLLOWING WORLD WAR II

Fred T. Haddock
University of Michigan

It is indeed a pleasure to be here today. This is the most exciting meeting I have been to in some time; as you might know, I've been to many!

I think I was in the audience at MIT when Taffy Bowen was invited to talk about the magnetron. The impression I had as a youngster was of him breezing in from England with a heavy magnetron that would make centimeter wave radar possible. It was very expensive to manufacture them because of the precision of these cavities, and it is my impression that a fellow over at Ratheon developed the technique of stamping out thin plates with the precise shape and then just stacking them. The precision of the stacking didn't really matter, it was the exact shape of the holes that made the magnetron mass-production possible and this had a big impact on centimeter wave radar.

I graduated from MIT in physics in June 1941 and went to work for the Naval Research Laboratory. I was put in charge of a brand new branch that had been set up under John Hagen called Centimeter Wave Radar. My job was to do antenna work, to compare dipole arrays with horns and parabolas to see if there might be differences. Within the next year our small group was joined by Cornell Mayer and Ed McClain. Cornell Mayer and I worked together for the rest of my time at NRL. During the war I worked mostly on antennas, while he was involved in that as well as in receivers.

This got us into the new field of centimeter wave technology. I remember building my first cavity mixer and starting with a piece of galena and putting cat's whiskers in it and making waveguide out of brass. The development of finding ticker tape pipe for waveguide was a major advance, as far as I could see. We made many trips up to MIT because the Rad Lab was set up to do centimeter wave radar, and it looked to me that the decision was made for the United States and Canada to develop centimeter wave radar, and that the British who had a more urgent need for detecting the German planes and bombers, to use meter waves. Thus the Australians and the English became experts on meter wave radar.

Meter wave radar was, I think, first invented by our supervisor at the Naval Research Laboratory in the 1920's, by A. White Taylor and Leo C. Young, and then independently it was invented by Sir Watson Watt and perhaps Hanbury Brown and associates. But the U.S. Navy put it on a lower priority. As far as I could see, there was a lot of meter wave radar all over the Laboratory when I went there in 1941. Our group was just one group out of a very large group on radar. The main burden of centimeter wave work was carried by the Radiation Lab. During the war I got involved in antennas, and developed the slot antenna. I think the idea of the slot antenna came from Booker's paper on complementarity between dipoles and slots, and that was followed up by W. H. Watson, a good theoretical physicist who was at McGill University. I visited him and was greatly taken by this new technique which didn't require any matching except staggering the slots and tuning the links with a file. I stayed there long enough to learn the technique. I came back to the Lab and developed a 2" x 6" 3cm antenna to be used in the new periscope for the U.S. submarines, and we followed that through to production and then got back fleet reports on it. The submarine periscope radar was a big effort in our group. Bell Labs built the other part of it, and I'm happy to say that I beat Bell Labs out on the design of the antenna.

Toward the end of the war, we wanted to do large radar work; we wanted to get an echo from the moon. So in 1944-45 we got interested in building a large parabola. I made calculations on how large a parabola we would need with our radar receivers to get an echo from the moon, and I came up with a figure of 30 feet. There was a lot of excitement in certain quarters of the Navy and it looked like we would get funded for a 30-ft antenna to do moon radar. I think they were interested in the idea that you could pick up transmissions from Russia bouncing off the moon for countermeasure purposes. I think that was it, just as for the 600-foot Sugar Grove antenna. When the war stopped, the Navy lost interest in the big antenna; but we in this small group didn't. We became interested in radio astronomy!

I had heard about Jansky and Reber during the war from one of my colleagues there, Harold Herman. I heard about the radio signals from outer space, and Harold, being an electrical engineer, said, "Yeah, there have been two people that have done work on it, a 'Jansky' and a 'Reber'. And Jansky's work looks real good." That was about all he knew!

After the war we were pushed a bit by E. O. Hurlburt, who was originally the director of the optical division and then became the scientific director. He was interested in the eclipse expeditions. He was an optical man, he liked to travel, and he liked to paint eclipses with pastels. He really was the driving force, but we liked the idea and the first radio astronomy experiment, the eclipse off the coast of Brazil in 1947, was from a mast on top of a destroyer. I don't know whether it was a 4-foot or 6-foot antenna at 10 centimeters since I didn't go because we didn't have Dramamine at that time. They got data, but the pointing was poor. There was a record of the eclipse curve which I think I alone attempted to analyze. I ended up coming up with some of the information that Covington found from his partial eclipse with the moon going over sunspots. It was a very poor record, but it got us in the eclipse business.

After that we went to the eclipse in 1950 to Attu, the last island on the Aleutian Chain. We wanted Reber to go along. Reber had sold his antenna to the Bureau of Standards and took a job at the Bureau of Standards. He painted it red, white and blue, and set it up out in the field. At that time we had gotten into building 6-foot radar antennas to do solar burst work following Covington, We got the first bursts at 3 cm and the quiet sun. People would come up to see these bursts on the 6-foot dish, and Reber came down one day in 1947 or 1948 with a visitor. He brought down this very short Frenchman in hand-made shoes and rolling his own cigarettes, and said, "This is a Frenchman! He won't wire power supplies, he just sits around and calculates, and he wants to see your antenna." So he came out and looked at it, and I kind of explained that I didn't know anything about him, and after a few minutes of talking about these bursts, I just stopped and looked at him and said, "What do you think causes those bursts?" He said, "I think gyro-radiation". I said, "Well, how could they escape from the self-absorbing region above them?"

He said, "Well, perhaps the magnetic field gradient is steep enough." I said, "What are you doing for dinner?"

That began a long friendship with John Francois Denisse who was writing his thesis at that time. Instead of wiring power supplies he was writing up a full general theory of the quiet sun, with an additional chapter on bursts. He went back to France and built up the entire French radio astronomy group. He became the Astronomer Royal and an officer of some rank in the Government. He was in charge of the space programs in astronomy and geophysics, and I guess he probably still hasn't wired that power supply for Reber.

We went on the eclipse expedition in 1950 and we invited Reber along. We had an extra dish with a hand-crank to move it along, and all Grote had to do was bring his receiver. He finally agreed, and came along. There was much noise playing poker at night. He had to go to bed at 10, but we didn't get to bed that soon, and he had a bunk above me. He got kind of sore-headed and all he did was collect big green balls from Japanese nets that washed up on the shores of Attu.

On the day of the eclipse, we'd been working very hard putting up our antennas. There had been a big typhoon in Tokyo and it came our way. So at the middle of the total eclipse through a horizontal rain of maybe one hundred miles an hour, Reber sprang into action because his 400 megacycles would not be affected. He got on there, cranked away through 3 hours of the eclipse getting data. He got a full eclipse curve well modulated by reflection from the ground in front of the antenna which Hagen had us quickly pack up to leave. So there wasn't any calibration the next day, and he couldn't take out the ground reflection. But it was interesting to see Reber work. I have visited Reber in Tasmania and seen his antennas down there.

We're talking about the sun and the eclipses. Here we were, because of the war, in centimeter wave work. During the war, a lot of brilliant people went to the Radiation Laboratory; others were ciphoned off to Los Alamos and elsewhere to do the big bomb. When the war was over there were a number left doing radio work such as Purcell, Townes, and Dicke. Those people did not go into radio astronomy. They went into other fields as you know. Townes, Dicke and Purcell did quite all right not going into radio astronomy! A big point is that they learned centimeter wave techniques and used it for what they thought was a lot more exciting physics and it was a payoff. But what kind of radio astronomy could you do at centimeter waves? You could get a 6-foot radar dish or 10-foot radar dish and get bursts from the sun or you could get an eclipse to get resolution. That was it! That's why we did it! That's why Covington did it, and that's why we went on eclipse expeditions, and that's why we were delayed, as Sir Bernard said - a big delay.

The meter wave work in the United States had already run its course on people who used meter waves for technology, atomic work and study of materials. I don't think there was very much meter wave radar done - the Naval Research Lab was about it.

So here you have the elite in England and Australia in radar, in part at meter wavelengths. They didn't have much money, but they could do something with what they had and they scrounged equipment like we scrounged all our equipment at centimeter waves from radar. So, we had to wait, I think, until the meter wave radio astronomers had made sufficient advances and exciting discoveries before we could get funding for an antenna big enough to do extragalactic or galactic astronomy. I think that's the key reason why the United States was such a laggard in radio astronomy. The delay was from 1945 to 1954 which was the take off point. During that time of course the radio sources were discovered by Bolton, Stanley and Slee in 1949. During that period we had several eclipse expeditions. We pushed for a large antenna; I had calculated that if we had a 30-foot radar antenna working at 3 cm we could pick up Cassiopeia, Cygnus and Taurus using the spectra from Graham Smith. They were all flat spectra due to their noise diode problems of impedance match.

So we thought we could easily pick those up! Then I calculated the thermal emission from the planets and said that we could pick up Venus and perhaps Mars and Jupiter at 3 centimeters and 10 centimeters, and that was the scientific justification. So when I ran into Winn Southberg, who had been around Harvard a lot and was a friend of Donald Menzel, I told him, "Boy, we're really going to get a big antenna. We're going to get a 30-foot antenna to work at 3 centimeters some day." He said, "I'm the right man. I've got a design by a mechanical engineer over here at the local university named Ned Ashton. He has done a design for us on a 50-foot parabola with cast aluminum plates, machined on the edges, put together under a circus tent, and with a two inch head mill and a bicycle chain cut them down to make a .005 inch surface." I said, "Great, we've got the justification. You make it." He said, "I'll make two, I'll bootleg one for myself".

So I went with the justification and this opportunity for a 50-foot parabola to Hagen who was good dealing with the military minds. That led to us getting the 50-foot antenna mounted on a gun mount, which was the worst feature, because you couldn't point it very easily.

When the antenna was finished, I remember showing Joe Pawsey around. He called Graham Smith and Graham Smith told him it was the world's most expensive radio telescope and all it can see is the Sun and Moon!

We went on eclipse expeditions in 1947 and 1950, and then later on in 1952 to Khartoum where we got pretty good eclipse curves. Cornell Mayer got some absolutely beautiful B shapes; symmetrical curves that would fold over to one half of one percent on the 1954 eclipse in Sweden. In 1953 I finally had access to the 50-foot dish. Cornell Mayer, Russell Sloanaker, and myself put a radiometer on the 50-foot dish and learned how to point it while looking at the sun and moon, something that previous observers who didn't know where to point it hadn't figured out. In a week we got the bright radio sources like Cygnus, Cassiopeia and Taurus, and I think the first measurement of the Galactic Center with any sort of precision. I think it was about 17 hours and 43 minutes minus 29° in 1954 coordinates. This was with a 27 minute of arc beam at 10 centimeters. That was an exciting thing to get because we didn't know what else to do. I took the Messier Table out and went down, 1, 2, 3, 4. Number 1 was the Crab Nebula. So we went on down and we started picking others up. We picked up 8 and 16 and 20, whatever these things were. There were no astronomy books around. We set the antenna in front of the Orion Nebula because we read our literature. Jesse had this paper with Grote Reber on things to look at, and he had a list of objects. One of them was the Orion

Nebula, so we picked that up for the first time, and that really then was the first observation of an HII region.

For the eclipse expedition in Sweden that Mayer went on in the summer, McCullough and I put together a 3 centimeter radiometer and we were able to pick up the same three bright non-thermal sources, the Orion Nebula and a few others. Then from the upper limits on the Orion Nebula of Bernie Mills, I was able to argue at the Manchester Symposium that we had a thermal source with a thermal spectrum. That was in late 1953. The meeting of January 1954 has been listed in a lot of source material mentioned today. The Naval Research Lab wasn't mentioned earlier, but I think the key promoters and developers of that meeting were Hagen and Tuve. Maybe they weren't the power behind it, but they organized it and put it on. I did a lot of clerical work on the papers and getting the abstracts published and I was able to give our paper on centimeter wave sources.

That meeting is what did it. By that time we had the 50-foot antenna, the meterwave people had done their homework and gotten radio astronomy on the map, and now everybody in the United States and Canada wanted to get into building big antennas. The money was forthcoming to do this. Lee Dubridge and Bacher were there. I remember Bolton and Taffy Bowen, Pawsey, a Harvard group of Bok, Menzel, Goldberg, Ewen and Purcell; Bernie Burke, Tuve, Howard Tatel, Fred Hoyle, Ryle, Graham Smith, and maybe Denisse. It was a pretty good sized meeting. It was kind of the first international meeting in radio astronomy and it was an electric meeting. A lot of papers were given, a lot of enthusiasm and the names you have heard awhile ago formed a committee and decided that this was it for universities. Taffy suggested Bolton to his buddy Dubridge, and that's where he went. Goldberg recruited me at Michigan and I went up there. Harvard got into it. We were off and running and the money was forthcoming in my case from the Office of Naval Research and also for Caltech. Bolton and I were going after the same pocket of money.

So that's my picture of how centimeter wave radio astronomy got going. It had to have the big antennas and it had to have the development at meter wavelengths, I think, to get the scientific justification. After all, a great big antenna was an expensive thing. It cost maybe a hundred thousand dollars, and it's good to 8 millimeters wavelength. It was working and put together in 1950-51, but it took a while to measure it and calibrate it, and we got it into radio astronomy in December 1953.

<u>M. Roberts</u>: Those of you who fly into Washington can see that telescope and its gun mount. Sit on the right side of the plane. Eight out of nine flights going in from this part of the world cross the Chesapeake Bay rather than going down the Potomac; you'll see it right before landing.

<u>A. Moffet</u>: There's a name that should be mentioned in connection with this development of radio astronomy in the United States and that's Arnold Shostak; I'm sorry he isn't here, but he was responsible for dishing out that flow of ONR money to Caltech, Michigan, Berkeley and to Illinois, and I don't know how many other universities. He certainly had a lot to do with the rapid development of radio astronomy, once that starting gun had been fired about 1954.

I should have said one thing about radio astronomy in the United States. Cornell University may have been the first university to get into radio astronomy with Ralph Williamson and Charlie Seeger. Seeger is the older brother of the singer, Pete Seeger. They were doing solar work at meter wavelengths. That was the first touch of radio astronomy at Cornell and maybe in any university here.

B. Burke: I can add a vignette to your argument about how things started because Lloyd Berkner, who was at the Department of Terrestrial Magnetism at the Carnegie Institution working under Tuve, wanted to build a large array of dipoles at the Derwood field station in the late 1940s. Merle Tuve was a very honest man; he said he didn't see how you could do any astronomy without angular resolution. So he wouldn't do it, and Berkner went on to become president of AUI, and at least in that particular quarter, the Carnegie Institution of Washington quarter, it was the identification of radio sources through the use of interferometers, plus the 21 cm hydrogen line which could make use of the large dishes, which at least converted several people in the early 1950s.

Sir Bernard Lovell: I want to comment about the influence of centimeter radar. I think Fred Haddock tended to suggest that in Great Britain we had all meter radar, and no centimeter radar, and that America had all centimeter radar. This is entirely incorrect! Maybe we did have all the meter radar but we also concentrated on centimeter radar. When the Tsizard mission brought the magnetron to the Radiation Lab, Hodgkin and I and Burch had already had the ground system going on 10 centimeters. In 1940 we were trying it; I think Hanbury Brown must know the reason why we were driven down to 10 centimeters. He was deeply involved in it, and the point about Bowen's air interception on one and a half meters is that the ground return is limited in range to the height of the aircraft. There was also a problem about minimum range, and this was the incentive to push the radar wavelength down. Hodgkin and I in the autumn of 1949 had a big horn working on 50 centimeters; then we had a split magnetron going on 10 centimeters and the reason that Booth and Randall did the 10 centimeter magnetron, not 20 centimeters, was that this was the wavelength we had to go to in order to get something small enough to fly in the fighters. A tremendous amount of the effort was devoted to this.

Why didn't you go on to centimeter wave radio astronomy?

Sir Bernard Lovell: The reason I didn't was because I was interested in long waves as I told you, but I knew nothing about meter wave radar. I spent the whole war on centimeter radar.

I know that!

J. Greenstein: I just wanted to say I was very glad that Fred mentioned some of the other pioneers, especially John Hagen, who was politically very important in one of the first observational and theoretical theses on the limb brightening of the quiet sun, and the NRL detection of HII regions. For the first time one had a reasonable explanation of the radio emission. But this particular area is the only happy part of interpretation at that time; you weren't groping around in the dark about these exciting new sources. It was making sense!

IMPACT OF NEWS MEDIA ON SCIENTIFIC RESEARCH

Walter Sullivan
The New York Times

Knowing that so many of my heroes are in the audience today, I'd like to talk about radio astronomy seen through the eyes of a journalist. It has been sort of a happy marriage over the last two decades, and partly, I think, because what radio astronomy deals with is so exciting and on such a tremendous scale that it really stretches our minds.

I would like to begin with a few footnotes, because Dr. Haddock talked about eclipse expeditions; I went on one with my family up to where the eclipse path crossed just south of Hudson Bay. We went up on the railroad; it was an astronomical excursion train, and we had a wonderful trip! But realizing that the weather is unreliable, I also arranged to go by little gasoline car a few more miles up the track to where the University of Illinois had converted a caboose of the Illinois Central Railroad into a radio astronomy observatory. They had all their antennas spread out along the railroad tracks waiting for occlusion of the sun, by the moon, to record the activity sites on the sun. The mosquitoes and the black flies were just terrible! I happened to have a mosquito helmet that I donated to the cause. The astronomers explained the whole setup and then the sky got dark. We did not see the eclipse visually, but of course they were assured of getting their recordings, they thought. However, just at the moment of totality, down the tracks comes a little gasoline car, brrrrrrrr. Everything went off scale. They ran out on the tracks and waved their arms, but the Indian driving this little car went right through and paid no attention. And so they were a very unhappy group.

About a year later at some meeting I ran into them, and they said, "Well, actually, we got our data." So it ended a little more happily than I thought.

One of the things that has made radio astronomy and related subjects exciting to the public and with the press has been its association with exciting events. As Sir Bernard was talking today about the evolution of the 250 foot dish at Jodrell Bank, I observed that he modestly refrained from describing his own very key role in squeezing considerable sums of money out of the foundation that helped support it and then helping the Russians observe their first encounter with Venus, and many other space missions. Of course this meant that Jodrell Bank was in the news every other day. As you may remember, as Venera I approached Venus, the Russians realized they were not in a position to be sure of receiving its transmissions and they used the Jodrell Bank antenna for that purpose. Sir Bernard became an eloquent spokesman for radio astronomy in those early days, not that he isn't still.

Those who have the gift of exciting their listeners, be they scientists or laymen like myself, play such a big role. I think of a colloquium to which I was asked at the Goddard Institute of Space Studies in New York. The speaker was Philip Morrison and he was talking about the possibility of detecting emissions from extraterrestrial intelligence and the uniquely rational - seemingly at that time rational - radio-frequency rendezvous at 21 centimeters. It was such an exciting colloquium that I was moved to write a book about it. And at about that time - it was actually unrelated - I made my first visit to Green Bank, heard about the huge steel equipment lying in a field over at Sugar Grove, which was part of that scheme of looking at the moon to hear what the Russians were saying to each other.

But the whole SETI project, the search for extraterrestrial life, was something that caught everyone's imagination, notably Project Ozma, Frank Drake's exciting encounters with some sort of rapid signal that I'm sure he'll tell about later on, and the discovery at the end that what they were hearing was indeed coming from intelligent life of a sort, namely, the U. S. Air Force.

I think, for many of us, the most exciting period began with the series of observations that led up to the discovery of quasars. First, the identification of the second most powerful radio source, Cygnus A, as a galaxy about a billion light years away. To those of us standing on the outside looking in, it was such an incredibly awesome observation that some energy source could reach that far across the universe and be our second most powerful source of radio emission from the sky. That was a first step. But there followed the sequence of events involving so many people in this room that led to that first Texas Symposium a few weeks after the assassination of Kennedy in Dallas. I remember some of the participants were reluctant to go to Dallas at that point. But what a memorable meeting that was! Oppenheimer, I think, was chairman of the opening session. He made no mention, as I recall, of black holes, but John Wheeler certainly did and Martin Schwarzschild was there, the son of Karl who defined the Schwarzschild radius. The meeting brought forth a whole array of new explanations for the quasars, notably those of Burbidge, Burbidge, Hoyle and Fowler. We were embarked on a whole new era of astronomy and radio astronomy.

These periods really got to those of us looking in from the outside because everything involved was on such a grand scale. It was very easy to get the public excited about it, and finally with the observations of Cygnus X-1 and now the Large Magellanic Cloud X-ray source, the idea became persuasive that black holes may be a reality. This has gotten through to the public as you all know. There was a movie done called "The Black Hole"; it was an outrageous movie!

I was stopped by a state trooper a few weeks ago. I had made the foolish decision to complete my work in New York and then drive to Woods Hole, not realizing how far it really is. A little bit after midnight I had passed through Providence and a bus came by doing about 75 mph, so I said to myself,

"There's a perfect radar screen; I'll get behind that bus!"

And immediately a little blue light flashed and I pulled off and the policeman came up, and I said,

"You know that bus was going like a bat out of !."

The policeman said,

"Never mind, never mind, Mister, are you an amateur astronomer?"

I said,

"Well, I have a six-inch reflector."

And he said,

"I have a ten-inch reflector", and as he looked at the license he said, "You come from Riverside; isn't there an amateur astronomy club in Riverside?"

Of course, he was thinking of California. I was from Connecticut. But he said,

"What are you looking at these days?"

And I said,

"Well, you know, I think Mars is pretty close to being in opposition."

He said,

"Oh is it really, I didn't realize that!" And so on, and finally he said, "You know you were going a little bit fast, you know you were doing about 65." Of course I was doing far more than that and that was the end of it. So I drove on to Woods Hole and pondered this during the night and then suddenly woke up, and it dawned on me that on the back of the car was a bumper sticker that said "Black Holes are Out of Sight!" That's the way things are in our country today. Such dramatic aspects of astrophysics as black holes are part of the popular culture.

So I say it's been a happy marriage.

In covering medical things we have great difficulty with certain journal editors. The editor of the New England Journal of Medicine is the villain of this story, from the press's point of view. If anything appears in the press about any new medical development that he is considering carrying a report on, he will reject it. Therefore, doctors refrain from presenting something at a medical meeting lest there be some reporter in the crowd who will then run out and report it before it gets in the New England Journal of Medicine, which is the most prestigious journal in the field of medicine. This battle has been going on now for several years back and forth.

Astronomy is not entirely immune from that. I remember when the quasar business was running hot and heavy, certain of our friends in this audience would tell me about an exciting new development and then say, "But don't write about it, Chandra will be furious!" Interestingly enough, Chandra's most important paper was turned down by the Astrophysical Journal and was published in some obscure London magazine. That was his famous paper on the limiting mass of white dwarfs. Our relations with the scientific world have not been entirely serene. I remember my colleague, Harold Schmeck, who had been a Nieman Fellow at Harvard, was visiting Harvard afterwards, and somebody said, "You ought to go over and talk to Pound and Rebka in the Physics Department. They're doing a very interesting test of general relativity using the tower of the Jefferson Physics Laboratory and using the newly discovered Mössbauer effect to see the difference in oscillation frequencies of certain atoms at the top and at the bottom of the tower because of the slight difference in the gravitational field at the top and bottom. And so Harold wrote a front page story on it and the editor of Physical Review Letters, which was about to publish an account of the experiment, was absolutely furious; he wrote a lead editorial in Physical Review Letters saying that anything that appears in the press will not be considered for publication by Physical Review Letters. Then Phil Abelson, who was at that time already editor of Science, wrote an editorial of his own saying this is silly, what the press carries is not the scientific report; it doesn't give all the qualifications, the numbers, details of how the experiment is done, etc.

We have somehow managed to live with this situation. A couple of years ago I got a call from Vicky Weisskopf who said,

"Walter, in the next few days you're going to hear something very exciting, but please don't report it! Not until it appears in Physical Review Letters."

I said,

"Well, Vicky, unless I know what it is I can't report on it!"

He said,

"Oh, you'll know what it is," and so I hung up and fifteen minutes later the phone rang and some graduate student at Columbia said,

"Have you heard what they've seen out at Brookhaven, and apparently the same thing has been seen at Stanford!" Well, this was the discovery of the Psi or the J particle; of course it was very exciting. So I called the people at Stanford and they said,

"Well, you know, we can't let you report on this until it has appeared in Physical Review Letters,"

and I said,

"Look, if all these people are phoning and it won't appear in Physical Review Letters for two or three weeks, we just can't sit on it, our competitors will report on it before I do."

And so finally they said, "Well, we'll try to see if Trigg (editor of Physical Review Letters) will agree to your publication of this story." I happened to have tickets on the fifty-yard line for the Yale-Princeton game because of an association I had with Yale at that point; it was the only time I was entitled to tickets on the fifty-yard line in my entire career. The whole family had seats on the fifty-yard line, and I spent the first half of the game up in the press box talking to the Physics Department at Stanford and finally at the half, they said, "Phys Rev Letters has agreed." So I never saw the second half of the game!

I think essentially - and I'll repeat what I've said before - that it's the enormous scale and excitement of what comes out of the radio astronomy observations that makes such good copy. Serendipity makes wonderful copy. I think of Jocelyn Bell-Burnell out there nursing her field of antennas outside of Cambridge and then seeing that little "scruff", the first detected pulsar.

It was at that Space Institute in New York that Hewish for the first time in this country described observation of the pulsars. I remember it because his slides still had them all numbered: LGM 1, 2, 3, and 4. And the excitement is holding up; sometimes it boggles the mind almost more than we can bear, like the problem of the superluminal objects that a number of you here are involved in deciphering and explaining, but serendipity makes very good copy. And I hope you keep it up.

F. Haddock: I don't know how many people in the room realize that Walter Sullivan is probably more responsible than anyone else in this room for the original allocation of frequency bands for radio astronomy supported by the United States. In 1959, the US position for the ITU conference in Geneva had been set -- all the nations in the world were for allocating bands for radio astronomy except the US had a very narrow position of 21 centimeters only. Then a story broke in the New York Times saying,

"U.S. AGAINST SCIENCE"

E. McClain: This was on a Friday, I believe. On Saturday the entire US delegation was back in Washington, Fred, John and myself, and sat down at the Academy all day Saturday; and by Saturday night we had a whole bunch of frequencies.

DEVELOPMENT OF APERTURE SYNTHESIS AT CAMBRIDGE

J. W. Findlay
National Radio Astronomy Observatory

First of all, it is absolutely clear that aperture synthesis basically belongs to Martin Ryle; there is no question. But I am going to try and sketch for you the first six or seven years at Cambridge when we all came back from World War II. I'll try to tell you what the thinking was and where it came from, who thought, who wrote, and who did things. I have just reread the literature; it is uninformative on the growth of the idea of aperture synthesis. If you want to see what Ryle (1950) was thinking of at the end of five years, read Reports on Progress in Physics. That paper contains one of the most excellent examples of what a great man Martin is. In that paper he describes 42 radio sources which had then been discovered, and says they are apparently about equally uniform in their distribution in the sky. And he doesn't quite say it, but he almost says it that this then shows that they're all well within our galaxy. Of course, that's a typical mistake of a very great man; he didn't realize they might equally be a very long way away!

The concept of aperture synthesis was at Cambridge all the time from the moment that Martin started doing research there. It is of course old, it goes back in effect to Michelson's interferometer. It's hidden away in the literature; nobody in Cambridge took the trouble to write about it. The first real paper on aperture synthesis was not published until 1957 (Blythe 1957). In that paper he draws the familiar square with the smaller squares in it and reminds us all that if you used pairs of antennas you were sampling amplitude and phase over a square and, therefore, you have the equivalent resolution of a large aperture. Although this paper was by Blythe alone, he says, finally, that Martin Ryle made the original suggestion.

Martin was, during these early years, a member of Ratcliffe's group, and the way Ratcliffe managed his research group and the way he communicated was by persuading everybody, himself included, to lecture in a weekly series to the group. And Ratcliffe, being Ratcliffe, had the content of his lecture completely thought out. And his lectures contained in effect everything that one needs to know, if you like, about Fourier transforms, about the relationship in diffraction theory between what a diffracting aperture does and what the angular spectrum is associated with it. The total thinking in Ratcliffe's lectures in essence assumed aperture synthesis. And so first of all, one must say that the climate was right, which again you can see if you read papers such as the big paper by Booker, Ratcliffe and Shinn (1950) in Philosophical Transactions, although there is nothing there to do with radio astronomy. All that Ratcliffe was concerned with were the properties of the waves reflected from the ionosphere or transmitted through the ionosphere. But he was concerned with the properties of irregular diffracting screens -- screens which modified the amplitude and also the phase, and he dealt with all that. With the help of Henry Booker, Shinn and Clemmow, he used a very straightforward application of diffraction theory together with the concept of the Fourier transform always present.

I had hoped that Ron Bracewell would have been here because Ron also sat with me through those lectures from 1945 to 1950 and listened to Ratcliffe, and Ron would not object if I said that much of Bracewell's published work goes back to and begins from the course of lectures given by Ratcliffe.

I come back to my point at the start, the concept was in Ryle's mind; it was clear. It was so obvious as not to need to be written; that's my opinion now. However, the principle was clearly stated in an early paper from Australia (McCready, Pawsey and Payne-Scott 1947). The authors say quite clearly that if you observe a source with any interferometer and you know the amplitude and phase, then you have one measure of the intensity distribution across the source.

The statement put this way is not in the Cambridge groups papers, but essentially, Stanier (1950) used aperture synthesis to determine the brightness distribution across the solar disk. He did it by taking two antennas and putting them in different spacings and he plotted a visibility curve; he used visibility, he didn't take any notice of the phase except if you look at his paper in Nature you'll find that in fact he did. He drew a visibility curve which went down to zero and then went negative. There's no way visibility, the way he should have defined it in the Michelson way, would behave like that; but he made it behave like that, which shows that he understood that basically the Fourier transform that he was using to get back to the solar disk would in fact be correctly derived from an actual visibility curve which had negative visibility. This was the first time the system was used. Ryle (1952) wrote about it in the paper on the phase-switched interferometer which was not published until 1952. However, the phase-switched interferometer had been used in the 80 MHz survey in 1950 and by Graham Smith to fix the position of Cas A in 1951.

Thus, although aperture synthesis was in the Cambridge thinking, it did not come into use until after the completion of the 2C Survey (Shakeshaft 1955). I believe Martin was concentrating more on source counts and cosmology than on source structure in those earlier years.

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A. Moffet: You're right that Stanier was the first to use the Fourier transform relation to reconstruct the source distribution, but it was clear that he didn't understand all Fourier theory very well. Bracewell pointed out later that he had sampled the visibility function every ten wavelengths, and knowing the size of the sun it would have been quite sufficient to sample it every few hundred wavelengths.

That is correct. By the way, let me say I'm speaking entirely about Cambridge. Obviously the ideas were there in other groups.

B. Burke: Al, I don't think that should go unchallenged. If you don't know what the radio sun is going to look like, you'd be very foolish to sample it every hundred wavelengths and, in fact, I think that I would say that Ron Bracewell in choosing an optimum spacing for his Cross Array at Stanford made a grievous error in choosing exactly that minimum sampling that is so eloquently given by Fourier theory. That doesn't prove that somebody doesn't know Fourier theory.

A. Moffet: It implies the far fetched notion that the sun was 5 degrees in diameter.

F. Haddock: It improves your signal to noise ratio and you get independent measurements that way.

A. Moffet: Stanier sampled very much more than any feasible theory would have suggested, and it was clear that it wasn't understood at that time!

R. Ekers: Representing Australia, I guess I should make a sort of in principle objection although I basically agree with what you said. You're implying, I think you would agree, that first published works using Earth Rotation Synthesis was from the Australian groups, and I gather your drift is that it was well known in Cambridge but that it was not published. Now by implication, are you suggesting the Australians did not have a similar period in which they understood it?

You've gathered from my inability to say anything specific on the subject that I simply do not know. All I can say is that I'm absolutely sure that the principle wasn't actually spoken of. It was so completely understood in the Cambridge group, and I'm sure it was by the Australians. Joe Pawsey, I'm sure completely understood it.

R. Ekers: I'm not suggesting in any way it went from Australia to Cambridge.

No, no. You will of course read Ron Bracewell, in Woody Sullivan's book That is very much a Bracewell point of view. And I wouldn't agree with some things that Ron is worried about there, about the interplay between those two groups. Ron is worried about things being hidden, when the Australians sent it in to Nature and things like that.

THE COMPOUND INTERFEROMETER
- A PRECURSOR OF SMART ARRAYS -

N. W. Broten
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About twenty-five years ago on a Friday afternoon, I was the last speaker of a session at the rather obscure organization which is referred to as URSI, and I met you radio astronomers coming out as I was going in to give a talk on the compound interferometer. So here you are -- lock the doors!

What Arthur Covington didn't say, but what he inferred in his talk was that the occultation experiment in 1946 piqued his interest in obtaining high resolution scans to find out more about those radio hotspots on the sun. Thus it is really no surprise that when I came to NRC in 1950 that plans were in progress for building a slotted array of some 150 feet in length. Fred Haddock mentioned the development of the array in the United States especially at the U.S. Naval Research Laboratory, and Covington talked about the microwave early warning system which was worked on in Canada at McGill University and at NRC. There were a few differences in what was proposed; I think one of the major differences was that it was just a lot longer. It was to be a non-resonant array which would result in a beam squint that would be unacceptable for radar, but which in fact would have some very real advantages for radio astronomy. A change in frequency would result in a change of squint angle of the beam; thus one could get multiple passes of the sun by the simple expedient of changing the frequency.

Figure 1 shows the essential part of the array. The waveguide had slots cut at spacings of slightly more than a half-wavelength and staggered on each side of the center line of the guide. These slots were covered with tape to keep water out of the waveguide, and that was a perennial problem as the tape kept breaking or letting go. The guide was placed at the apex of a long slender horn (Fig. 2) mostly on account of economy as one plan had been to have the waveguide at the focus of a 30-foot diameter cylinder. Differing elevations of the sun at transit could be accommodated by rotating the array

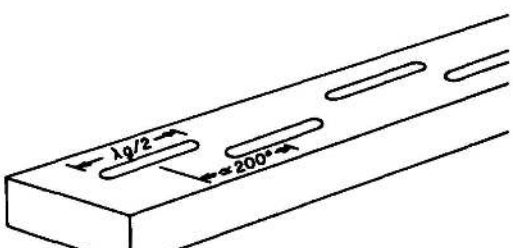


Fig. 1. Slotted waveguide used to feed antenna in Fig. 2.

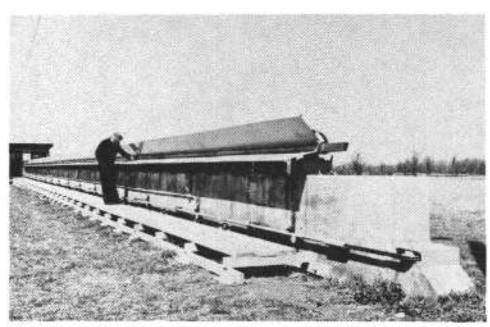


Fig. 2. Early solar radio telescope.

about its axis. The array operated in the total power mode, and stability of the receiver was a problem in those days of vacuum tubes, even though a transit of the sun took a very short time. However, the major problem was that the array still had a resolution that was inadequate. It soon became evident that a resolution of 8 minutes of arc was too coarse to learn much about the microwave hotspots on the sun. 1952 saw us exploring ways of increasing resolution, staying within a budget that was essentially zero. It was really after the 1954 URSI meeting in January that we got around to doing something about it.

Let me show the basic elements of a phase-switched interferometer by means of Figure 3. The phase switcher is indicated by α. The power out, P, is given by the product of the voltage patterns of the two antenna systems A and B, a term due to the spacing between them, and a term due to the rotation of the phase shifter. There is a second part to the equation that has not been of much use to radio astronomers because maximum power occurs at an angle where the primary response of A and B have fallen to a very low value.

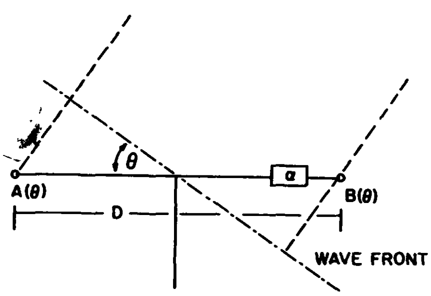


Fig. 3. Phase-Switched Interferometer.

$$P(\theta,\phi) = A(\theta,\phi)\,B(\theta,\phi)\cos 2k\theta \cos \alpha t + A(\theta,\phi)\,B(\theta,\phi)\sin 2k\theta \sin \alpha t$$

$$A_1 = A(\phi)\ B(\phi)\ A(\theta)\ B(\theta)\cos 2k\theta$$

$$k = \pi D$$

1) $k\theta \neq 0$, $A = B$

2) $k\theta = 0$, $A \neq B$

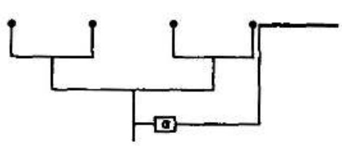
3) $k\theta \neq 0$, $A \neq B$

I have indicated three conditions. Firstly, A equals B but the spacing between them is not zero. This is the case of a simple phase-switched interferometer which astronomers such as Martin Ryle, John Bolton and Graham Smith used to such great advantage. Secondly, the phase centres of the two antenna systems are coincident, but the two systems are not the same. This is clearly the case of the Mills-Cross or the Kris-Cross. The third case where the phase centres are not coincident and the two antenna systems are different is the case of interest to us.

We see now how it may be possible to combine antenna systems to form a new interferometer, where the second antenna system may, in fact, be totally different than the first, and may indeed be quite simple. Our first effort at this involved combining the slotted array with a simple cylindrical antenna using the same type of slotted waveguide feed. The cylindrical antenna was made by cutting a framework from plywood and covering it with sheet aluminum painted flat white. This worked so well that we wanted to try a simple two element interferometer with the slotted array. A second parabolic cylinder was hastily constructed and erected at Goth Hill, as our Observatory was called (being a very small knoll in a field belonging to a farmer whose name was Goth). We were in such a hurry, with winter coming on, that we did not stop to paint the aluminum sheet. The result was that the tape covering the slots caught fire on the first transit of the sun!

With the success of this experiment our ideas ran wild and we envisaged all types of antenna systems to combine with the array. The advent of spring brought reason to prevail, or perhaps it was a lack of budget to do anything very exotic, and we settled on a four-element interferometer of simple cylindrical paraboloids each spaced 150 feet to combine with the slotted waveguide array. The configuration is shown in Figure 4. The equation shows that the resolution becomes eight times that of the array alone. At a wavelength of 10 cm, the resolution was one minute of arc, allowing resolution of many of the hotspots on the solar disk.

Figure 5 shows a picture of the completed array. The four parabolic cylindrical antennas are shown in the foreground and the slotted array at the far right. The waveguide joining the elements of the simple array was supported on the wooden trestle.



$$A_1 = A(\phi)\ B(\phi)\ A(\theta)\ B(\theta)\frac{\sin k\theta}{k\theta}\ \cos k\theta\ \cos 2k\theta\ \cos 4k\theta = AB\ \frac{\sin 8k\theta}{k\theta}$$

Fig. 4. Schematic diagram of compound interferometer.

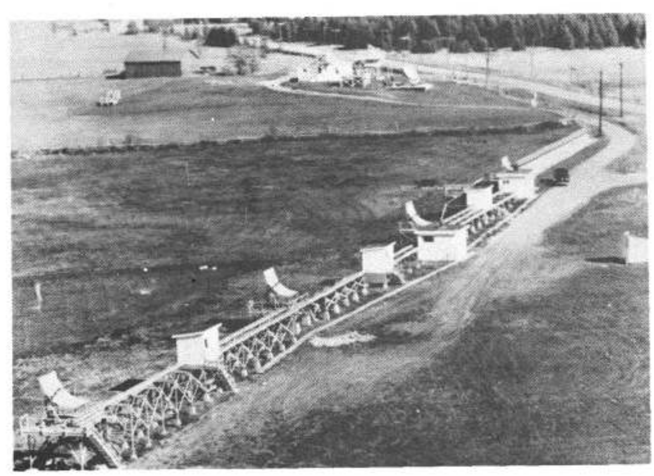


Fig. 5. Photo of compound interferometer.

The results are best shown by Figure 6. Here we see scans of the sun with different resolutions; the slotted waveguide array alone, the array with one element of four-element interferometer, the array with two elements and the array with four elements. The change of resolution is clearly evident and the need for a resolution of the order of one arc-minute can be seen by the breaking up of the central hotspot into two components. This combination of two antenna systems whose phase centres are separated so as to give increased resolution is what we have called the compound interferometer. The ability to "steer" the beam by changing frequency, i.e. changing phase, is what is meant by saying that the compound interferometer is the precursor of smart arrays.

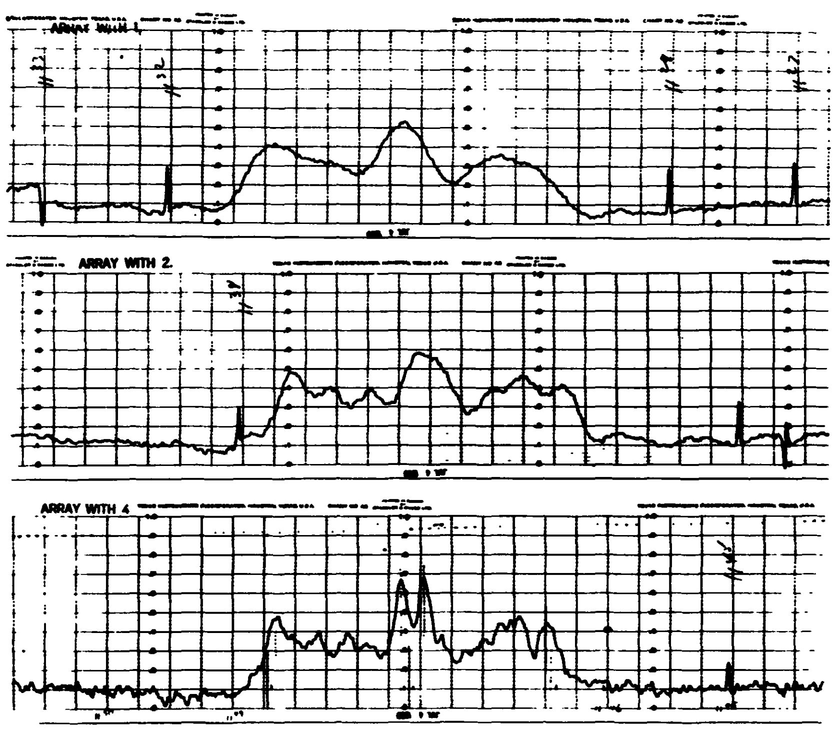


Fig. 6. Scans of the sun with different resolutions.

THE DEVELOPMENT OF MICHELSON AND INTENSITY
LONG BASELINE INTERFEROMETRY

R. Hanbury Brown
University of Sydney

In 1949 at Jodrell Bank, we were working on radio sources with the 218-foot paraboloid which Bernard Lovell described and which was a very fortunate thing to have. It was built, as you realize, for quite different purposes. It was just there and Lovell suggested to me that we might use it for cosmic static, so that was what was done, simply because the instrument was there. Victor Hughes started before me and I came to help him.

Fig. 1. Aerial view of Jodrell Bank in 1949. The 218-ft. paraboloid can be seen on the right.

At that time not much was known about the radio sources. I will deal first of all with the problem of finding out what the radio sources were. This was a great puzzle. What are these things you see in the sky? In 1949 three of them had been identified; I think largely due to work in Australia. They were the Crab Nebula, M87, and NGC 5128 in Centaurus. Nothing had been identified from Cygnus and nothing had been identified from Cassiopeia, the two strongest radio sources in the sky.

Bernard Mills had suggested to Minkowski that Cygnus might perhaps be identified with a faint galaxy which they found on the pictures of the sky. Minkowski said no, it was a perfectly ordinary galaxy; later on they found that he was wrong, it wasn't perfectly ordinary.

I might point out that discoveries have to be made not only of something that is interesting but also they have to be made at the right time. So, we were guided in what we did instrumentally by the current theory. The current theory was that the radio sources were radio stars; in the Cambridge theology, at least, these things were stars. If you read the literature, they were believed to have a space density greater than one per cubic parsec. We thought we were dealing with stars and so we had to design an instrument which could measure stars, although the only sources that had been identified were nebulae, such as Virgo and the Crab Nebula.

The problem was therefore to measure something which had the angular size of a star. And that was a problem that I faced. I am, by the way, a professional radar engineer.

The angular diameter of a star is extremely small and is measured in fractions of a second of arc. Michelson measured the angular size of Betelgeuse to be 0.047 seconds of arc. So that was our problem. To measure an angle of a few thousandths of a second of arc with a radio interferometer. In a radio interferometer you compare the amplitude and the phase of the radiation at two space points. If we work out how far apart these points have to be in order to measure a star at a wavelength of one meter in order to measure an angle of 0.047 seconds of arc, you need a baseline of 4000 kilometers. Now you realize that was difficult! So the technical problem was to preserve the relative phase and amplitude of the signal received from two widely separated points, and I could not see how to do it. There were no masers in those days. All we had were quartz crystals; there were plenty of quartz crystals, but they were not stable enough. It's very much easier now. I could go into all the numbers but I won't because you probably know them anyhow.

But the point was that we couldn't see how to do it. One night while I was thinking about this thing, I thought to myself, "Well, if the radiation from the sky is picked up at two points on the Earth, which could be, say, 4,000 kilometers apart, the similarity which Michelson would have looked at was the relation between the phase and amplitude of the wave at those two points. Is there anything else, any other parameter that we could look at?" And into my mind came quite clearly the idea of a man sitting with a radio receiver looking at the noise on a cathode-ray tube. I had an absolutely clear vision of the noise on two cathode-ray tubes, one at each end of the baseline, and I thought to myself, "Ah, those two noises are the same or ought to be!"

Next morning we worked this out; I worked it out as a matter of fact! The answer is of course that if the predominant noise is from the source, and the source is unresolved, then they are the same. In other words, we are dealing with a plane wave. You can see how that is so, if a radio station is transmitting a modulated wave or a radio program, then the modulation would look the same at two spaced points. The modulation at these two points will be in phase although the radio frequency phases bear no relation to each other. So I realized that this was the way in which we could compare the signals received at two points which could be far apart; we could record them on tape recorders, driven by quartz crystals, and in this way we could have adequate stability to make an interferometer which would span the Atlantic. I wrote a proposal for an instrument which in principle would span the Atlantic. I thought of going west from England rather than east, which reflects my ideological bias. I never thought of an instrument which would go to Moscow! I just didn't! Don't read anything into that!

Anyhow, the answer was to build the thing; how do you do that? Well, what you do is you demodulate the signals at the two spaced antennas and then compare them. That's all there is to it! You have an antenna and you have a receiver and a detector; from this detector you take out the low frequency envelope of the wave in a low frequency band which you can record with tape recorders or transmit with telephone lines; say you're limited to about a thousand cycles per second. (This was all done before Hertz had been heard of.) You have a tape recorder here and another tape there and a little man on a horse with a forked stick who brings the two tapes together. That's all; absolutely straightforward technically and easy for a radio engineer who had worked on the sophisticated radar which we had by the end of the war. Most of the technical problems of radio astronomy were actually nothing to people who had gone through development of modern radar as I had. Technically, they weren't difficult; it was simply that you had to do the actual work!

Now, the snag with this type of interferometer is that you have a low signal-to-noise ratio because in the process, the signal-to-noise ratio gets squared. I was worried about the actual calculation of the signal noise ratio, but I couldn't work it out myself, so I got a friend of mine to introduce me to someone who could; someone who had been working on low noise development at MIT during the war at Radiation Lab. His name was Richard Quentin Twiss; I'm sorry he is not here. He worked out the signal-to-noise ratio for me in quantitative matter. In fact he worked out the whole theory on about 17 pages of paper during the night in purple ink, and he came the next day and said, "This idea is no good, it doesn't work!" And in doing so he was agreeing with 98% of the physicists I've met who also came to the same conclusion - that it doesn't work. But in fact he had put the integral of $\cos^2\theta$ equal to zero in the process of the mathematics, and it doesn't equal zero! I pointed that out to him in the course of the next day and he decided that it did work!

And so we calculated that it would give us a reasonable signal/noise ratio on Cassiopeia and Cygnus; we built the thing and tried it on the sun (Fig. 2). The correlator and receivers and all that stuff were built by my students, Roger Jennison and Das Gupta (Fig. 3). To measure the sun, we split the aerial into two halves and we measured the angular size of the sun; it didn't work at all, and this was discouraging. The reason why it didn't work was that the two halves of the array had been connected the wrong way in the middle. We found that out in a few days and then we were very relieved because it did work.

I can tell you about as many stories about Roger Jennison as you can tell about Reber, and I finally lost him as a colleague because he developed an interest in relativity and he wanted to do experiments on rotating frames of reference. He went away to do that and since then has been at Canterbury and has written a lot of papers on relativity. The other gentleman went back to India.

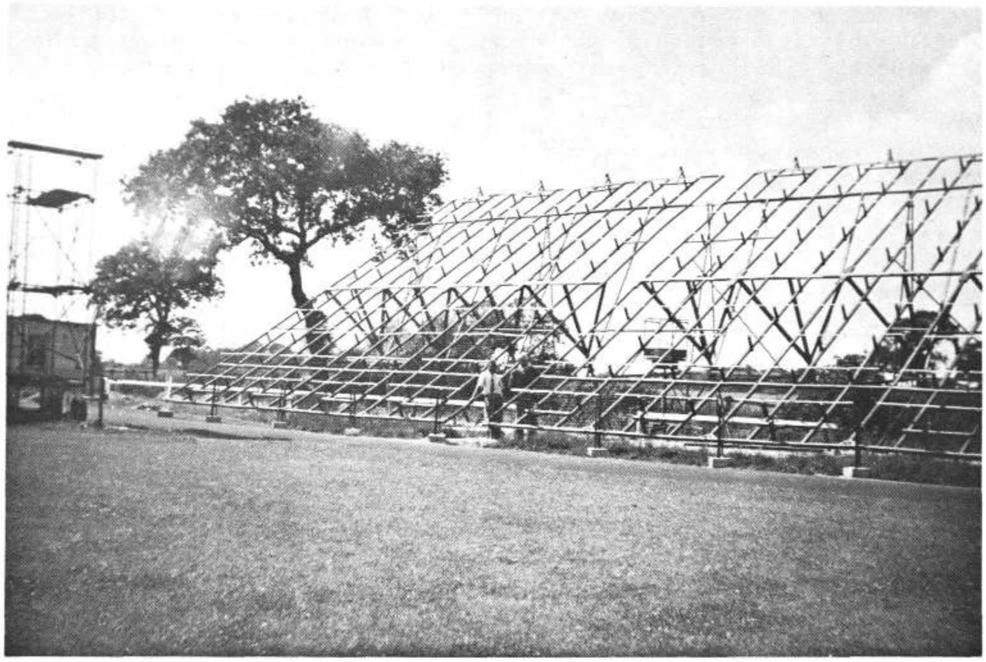


Fig. 2 The antenna of the first Radio Intensity Interferometer at Jodrell Bank (1950).

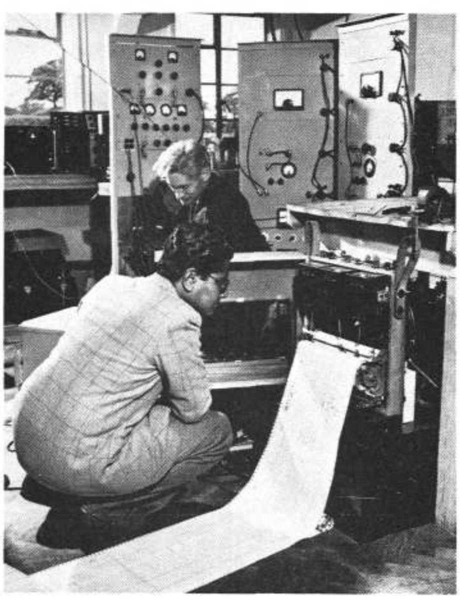


Fig. 3. R. C. Jennison and M. K. Das Gupta with the Radio Intensity Interferometer at Jodrell Bank (1950).

So what you do is receive the source on one antenna and then you receive the source on the other antenna and correlate the fluctuations in the intensities of the two signals in a bandwidth of one thousand Hertz (I'm starting to use Hertz) or a thousand cycles! Figure 4 shows the transit of Cas A through the aerial beam - the square signals are calibration signals. We built this thing and it worked on the sun, and so we built a mobile unit; we put one of those antennas on a track, we negotiated with a number of rural personages who ran farms in the neighborhood and persuaded them to allow our truck to go into their farmyards, and this way we went, farm by farm, across Cheshire.

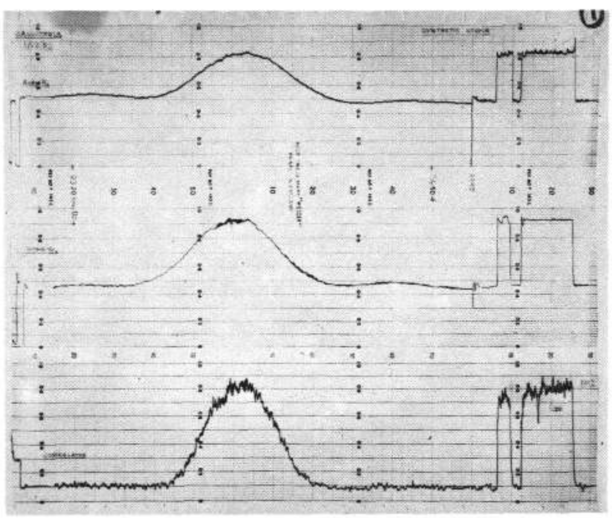


Fig. 4. A record of the transit of Cassiopeia A observed at Jodrell Bank (July 1952) with the first Radio Intensity Interferometer using a baseline of 900 ft. The upper and middle records give the total power received at each end of the baseline, the lower record shows the correlation.

I can't remember when all this happened, but I think it was 1950. We put the antenna on the truck finally in 1952; things happened very slowly, and we measured correlation as a function of the aerial spacing at a wavelength of about 1.8 m, or something like that. Figure 5 shows the points measured by Mills in Sydney and by Graham Smith in Cambridge, as well as our own. The interesting point is that our measurements at Jodrell Bank showed a second bump in the graph of correlation versus baseline, which showed that Cygnus was a double source.

Figure 6 is important for two reasons. First of all, it shows the double model of Cygnus which is derived from the measurements. Secondly, it was while trying to sort out the phase of the secondary maximum that Jennison thought of the idea of phase closure. This idea is now used in many instruments, such as MERLIN. That is where it started and it is a good idea, although there was little interest at the time; it was published and left in the literature. The interferometer showed that Cygnus had an angular size of something like 2 by ½ minute of arc, and Cassiopeia was circular with an angular size about 3½ minutes of arc. In other words, they weren't stars. And therefore the whole development of this instrument was unnecessary.

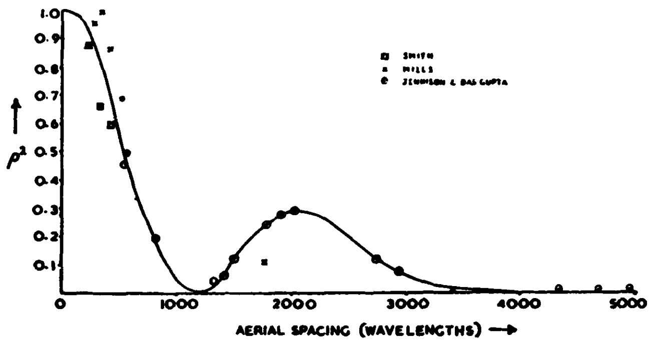


Fig. 5. The correlation from Cygnus A measured with the first Radio Intensity Interferometer in 1952. The measurements were made at 125 MHz. The ordinate shows the correlation (ρ^2) and the abscissa shows the baseline length in wavelengths.

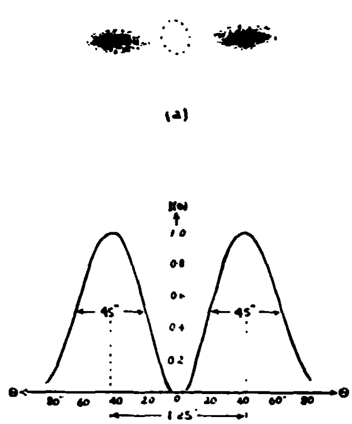


Fig. 6. The distribution of intensity across Cygnus A derived from the measurements shown in Fig. 5.

Quite true! If we had never developed the intensity interferometer, we could have done the whole job in half the time and we would have done it first. We all three, Mills, Smith and us, published together in the same issue of Nature, but we could have done the thing a couple of years before this if we hadn't messed around with the intensity interferometer. To develop an ordinary "Michelson" radio interferometer is something I could have done with my eyes shut; to a professional radio engineer, there were no problems at all!

So, that was the story. But all the effort was not wasted. First of all, it made good measurements. Secondly, Jennison invented this irrelevant thing about phase closure. And the other thing was that while Richard Twiss and I were watching the thing working - Richard Twiss is a great watcher of people's work - we saw that on certain occasions the radio "stars" scintillated like mad. They scintillate at meter wavelengths - people don't see that sort of thing nowadays. And we noticed that when we integrated the correlation, we got exactly the same result as when they were not scintillating. In other words, the instrument worked perfectly even when the sources were scintillating, and Richard Twiss and I said to each other, "OK, that's the answer to the problem of measuring optical stars through the atmosphere."

So this new intensity interferometer was a dead duck from the point of view of radio astronomy. Unnecessary development, but of course what we did with that thing was apply it to optics, right? And when you go into optics you have photons, and you have visitors with long hair talking about quantum theory and God knows what! It's a different world. In radio you have Maxwell and everything goes up and down like proper waves! I had to relearn it all; anyhow we worked it all out in the end. Figure 7 shows a picture of an optical interferometer which measured the angular diameter of Sirius at Jodrell Bank in 1956; this is the first time in the history of astronomy that the angular diameter of a main sequence star was ever measured.

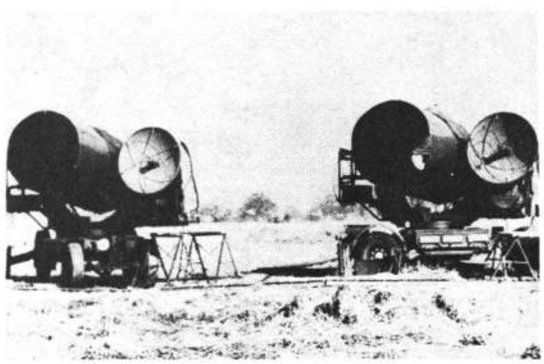


Fig. 7. Pilot model of an Optical Intensity Interferometer at Jodrell Bank in 1956. The interferometer was used to make the first measurement of the angular diameter of a main sequence star (Sirius). The phototubes were mounted at the focus of searchlight mirrors.

These big searchlights were borrowed from the army! I purloined two of the biggest searchlights you can get. I often think the next generation is going to have a problem if they are going to have to purloin an MX missile!

In Figures 8 and 9 we are back at Jodrell Bank in 1949. We have this serendipitous structure, this cat's cradle, this rat's nest, this 218-foot paraboloid with a central mast which was built to detect cosmic rays. I took the antenna over from Victor Hughes, converted it to 1.89 meters with a coaxial cable feed and, for stability, copied Ryle's receiver. I made it work nicely as a radio astronomy receiver.

Figure 10 shows a trace of a source going through the beam of the 218-foot dish, with Cygnus A on the right and Cygnus X on the left. It shows you, I think, that it used to work pretty well.

Figure 11 shows Cyril Hazard. I'm sorry about me, but that is Cyril Hazard, who worked with me and I expect a lot of you know him. I thought we ought to have a picture of him.

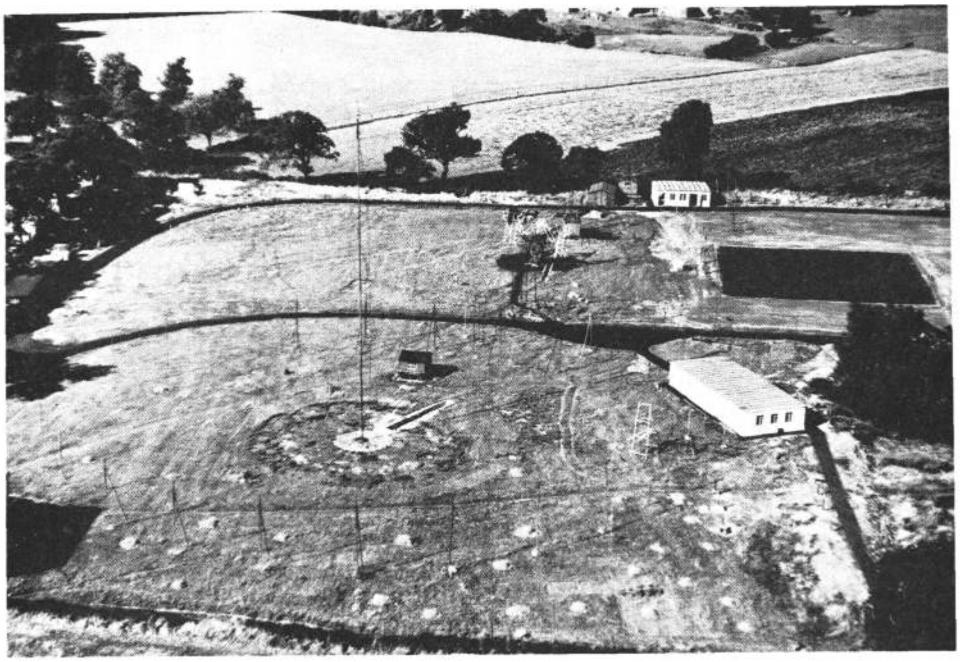


Fig. 8. The 218 ft. paraboloid at Jodrell Bank seen from the air in 1949.

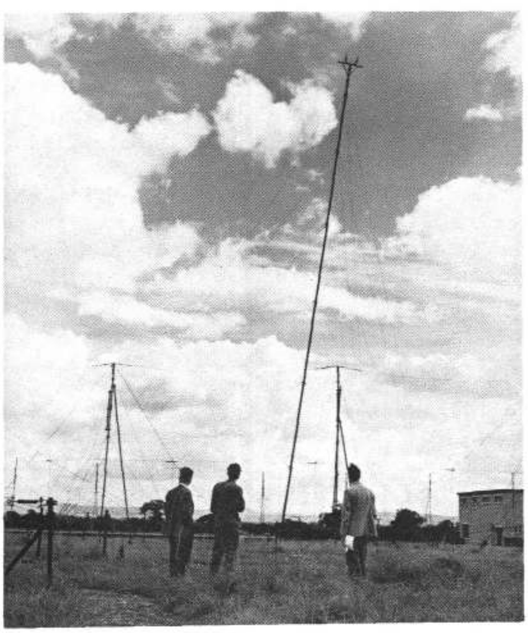


Fig. 9. The 218 ft. paraboloid at Jodrell Bank seen from the ground in 1949. From left to right, C. Hazard, R. Hanbury Brown and J. G. Davies.

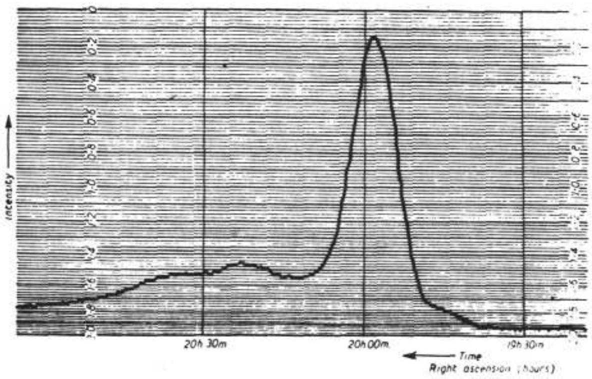


Fig. 10. The transit of the intense source Cygnus A through the beam of the 218 ft. paraboloid at Jodrell Bank. The record was made in 1949 at a wavelength of 1.89 m.

Fig. 11. R. Hanbury Brown (left) and C. Hazard looking at a recording made with the 218 ft. paraboloid at Jodrell Bank in 1949.

When you mapped the sky in those days you had two classes of sources. You had Class I sources, I'm using Mills' classification. Class I sources were concentrated into the Galactic Plane and Class II sources were isotropic. The fact that the Class I sources are concentrated into the Galactic Plane was shown by this 218-foot dish and had also been observed by the Australians. We confirmed this concentration of this family of sources with the 218-foot dish. This is an important point, because at that time the major surveys of the sky, made from interferometers, showed an isotropic distribution of sources and no concentration into the Galactic Plane. The reason was that the interferometers had too high a resolution and were resolving sources in the Galactic Plane which they didn't know had large angular sizes.

The distribution of the 13 most intense sources detected by the 218-foot dish in the Galactic plane is shown in Figure 12. We had some fairly close

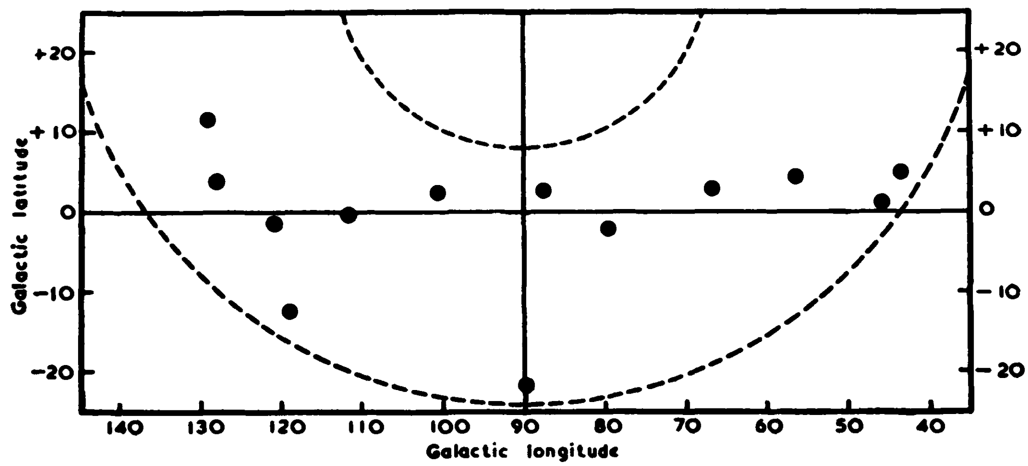


Fig. 12. The distribution in galactic coordinates of the 13 most intense sources observed with the 218 ft. paraboloid. The broken lines show the limits of the field of view.

arguments with Cambridge about this, but it is now agreed. So the first problem we had was to measure of the angular size of these sources and, to do that, I built a small interferometer with Henry Palmer, Richard Thompson and David Morris, whom probably most of you know; I had these three chaps working with me. We first of all built a small antenna, 35 meters square, which was connected to the receiver by a cable and formed an interferometer with the 218-foot dish. There were certain problems with it because the primary feed of the paraboloid looked down into the other antenna which gave us coupling of the the front end noise. There are all sorts of obscure troubles like that which made it very difficult, and of course we couldn't reduce the baseline to less than the radius of the 218-foot dish. So in fact what we did was to build an interferometer which had got too much resolution to measure some of the sources, and too little resolution to measure the rest. But messing around with this small antenna on the end of a wire we managed to show, as Richard Thompson may tell you, that 6 out of 19 of those sources had angular diameters greater than from 1½ to 3 degrees. In other words, they were really large, and Minkowski photographed one of them for me and found it to be an extended, faint nebulosity. We came to the conclusion that these sources were mostly nebulosities of low optical surface brightness in the Galactic Plane and that they were probably remnants of supernovae because of the Crab Nebula example. Perhaps all the radio stars in the Galaxy were the remnants of supernovae; an idea which was put forward in a paper from Jodrell Bank in 1954.

So in 1955 having messed around with the big sources, we went for the little ones, and for that we had to go out further and further and further.

We soon got to the end of the piece of wire and had to use radio links, which was a completely new technique. We had to develop methods of slowing down the fringe rate and a lot of other things, such as compensating for delay with supersonic delay cells, etc. We had to put all those things into the longer baseline interferometer and that took quite a long time.

In 1955 we had a baseline of 13 kilometers which is equivalent to about 6720 wavelengths, and of the 23 Class II sources that we had in the field of view of the 218-foot dish, three sources were still unresolved and must therefore have angular sizes less than 25 seconds of arc. So now we were getting into really quite small angular sizes.

In 1956 we went out to 20 kilometers; that was 10,600 wavelengths. The aerial was at the Cat and Fiddle on the Derbyshire hills, which is the highest pub in England. Extremely cold place, and while the truck was up there I told them to bring the set back one day, and they said,

"We can't, someone has stolen the carburetor."

That's all I remember about that place. It was horribly cold! Anyhow, the point was that when we went up there we still had three sources completely unresolved, which meant that their angular size was less than 12 seconds of arc. So Morris, Palmer and Thompson, in their paper, suggested that the sources were like Cygnus A because they had very high surface temperatures. In other words, they suggested that the Class II sources might be objects of the Cygnus type. So these were the main results of the work; the Class I sources looked like nebulosities in the Galactic Plane and the Class II sources looked like Cygnus objects. At that point the Mark I 250-ft. dish came into use and we used that as one element and extended our baseline over the Pennine Chain using microwave links. We took the 384 sources in the Cambridge survey and decided to measure the whole lot. And this took years. By 1961 we had finished that work using a maximum baseline of about 115 kilometers or 60,000 wavelengths, and we were left with seven sources which had angular sizes of less than one or two seconds of arc. We had resolved 3C295 but we were left with things like 3C48.

Now, we saw this little group of seven sources, with angular sizes of less than one or two seconds of arc sitting there in the distribution. We said,

"These are something odd."

We made a list of them, circulated them to all the main optical observatories in America and Russia, and I think in France as well, and said to the guys,

"Will you please look at these things?"

But of course it wasn't much use because, in fact, the positional information was not good enough to identify these with faint objects in the field. In other words, we circulated this information before the accurate positional information was sufficiently advanced.

Now you might ask, "Why was Jodrell the only Laboratory in the world which was doing this program of continuously increasing the resolution of interferometers in order to measure the angular diameter of the radio stars? Why did nobody else do it?"

There was some work with a fixed baseline in Australia, and the angular sizes of the sources in some of the early Mills catalogues are based on the information obtained, but they gave it up. Nobody else did this systematic work at that time; nobody else was really very interested in it. The reason seems to be that the interest in those days was primarily in the identification of these radio sources with optical objects - a desperate effort to link radio astronomy and optical astronomy - and people were primarily interested in precise positions. Also, they were hypnotized by the Cambridge philosophy in which it was hoped to unravel the structure of the cosmos by counting the number of radio sources as a function of their intensity. And so those were the fashionable things. This work we did was very unfashionable, and was carried on against the stream for some years. I just mention that point because in fact a lot of credit is due to people like Henry Palmer and so on who continued to plod about in the mud for years - through barnyards, fields of cabbages, and over the Pennine Chain, and God knows what else. But in the end it paid off. And now one of the main programs at Jodrell Bank is the development of angular size measurements using the MERLIN system and, of course, the other observatories in the world are now doing it as well. But there was a long time when we were the only people doing this and nobody was interested.

J. Greenstein: Why couldn't this have given decent absolute positions, at least in one coordinate? Why did you not get the absolute positions accurately enough so that we could have said, "It's this." What prevented you?

It's wire welding! I wasn't doing that. I was fully employed measuring angular size -- measurement of position is a specialized business which involves a hell of a lot of stable equipment -- something you have to go for as a topic in itself. You had to concentrate on measurement of position or measurement of brightness distribution.

J. Broderick: When you narrowed down this group of seven sources smaller than one arc second, how come you didn't fall back at that point and just conclude that these were the radio stars that had gotten the whole study started?

We thought they might be. And then when the first ones were identified - what was it, 3C48? - we thought they were. We were desperately keen for someone to identify them optically. I wrote to Minkowski, to Hubble, to Ambartsumian, and all those people, but they simply wrote back and said we can't look at these fields unless you give us a position which is accurate.

R. Ekers: Isn't it the case, Hanbury, that by the time there were accurate positions people had forgotten this work?

Oh yes.

B. Lovell: The positions of the 7 sources to which Hanbury refers were not accurate enough in the Cambridge catalogue and the people who made the position measurements which led to the identification of 3C48 in 1960 were the *people in the Owens Valley. I'm sure that Maarten Schmidt is going to talk about that.*

M. Cohen: *It was those seven sources and the 120 kilometers, or whatever it was, that was one of the main factors in going to Very Long Baseline Interferometry, which began very shortly after Henry Palmer stopped extending the baseline. It was only a few years later that Ken Kellermann and I began to talk about independent-oscillator tape recording systems. But in the middle sixties, it was the radio engineers vs the long-haired physicists. The radio engineers knew everything was perfectly OK, and that you could do coherent interferometry by recording on tape; there was no question that you could do it. But the physicists went on for years that it was impossible - until we did it!*

EARLY INTERFEROMETRY AT JODRELL BANK

A. R. Thompson
National Radio Astronomy Observatory

I want to add a few illustrations and personal reminiscences to the talk that Dr. Hanbury Brown has just given. These concern the Michelson-type radio interferometer that was built to investigate the angular widths of sources found in a survey by Hanbury Brown and Hazard (1953) using the 218-ft paraboloid at Jodrell Bank at 1.89 m wavelength (158 MHz). The sources were divided into Class I which were concentrated towards the galactic plane and Class II which were more isotropically distributed. The interferometer used the 218-ft paraboloid and a smaller, transportable antenna.

I went to Jodrell Bank in 1952 as a graduate student; I was there from 1952 to 1956 and the experiments with the interferometer began in 1952. H. P. Palmer went there the same year, and the two of us started to build the interferometer with Hanbury. Figure 1 shows the 218-ft paraboloid that was

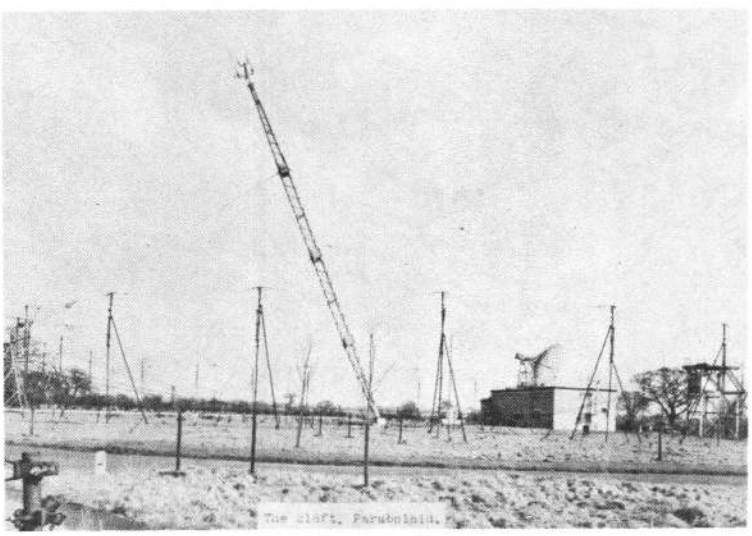


Fig. 1. The 218-ft paraboloid, circa 1955. Two feeds are mounted at the top of the mast, separated in the east-west direction, for frequencies of 158-MHz and 92-MHz. The antenna on the roof of the generator building was used in the polarization measurements made with the interferometer.

used as the main antenna of the interferometer. This was a really fine instrument for that time, but it was very difficult to photograph because there was really nothing there except free space and some wires! When the antenna was originally constructed, there was a mast at the center carrying the feed and supported by nine guy wires. The beam could be moved in declination by tilting the mast in a north-south plane. At the end of my first year as an undergraduate at Manchester, in 1950, I went to work at Jodrell Bank as a summer student. One of the things I did was to help tilt the mast, by adjusting chains at the ends of the guy wires. It was a strenuous and dirty job. However, I enjoyed the summer so much that I decided to go back to Jodrell and take a Ph.D. there later on. I was lucky, because about the time that I returned there the old mast and guy wires were replaced by a new aluminum structure, which was designed like a crane jib and could be moved by a system involving a winch and set of concrete counterweights. At the 1.89 wavelength we could get a good beam up to 16° from the zenith, and the declination covered was 48° to 70°. The polarization was limited to the east-west direction by the reflector surface wires.

The next item that we required for the interferometer was a smaller, portable antenna. I looked at Jennison's antennas used for the intensity interferometer, and decided to make something similar. Each of Jennison's antennas was an array of full-wave dipoles. I noticed that when one of Jennison's antennas was moved between different sites, it was tightly roped down to a truck, and very often transmission lines were damaged in the process. Jennison had to solder them on again in the muddy fields and farmyards. I decided to try to make our antenna a little bit stronger, but nevertheless we always had to repair some transmission lines after a move. We made four arrays of full-wave dipoles on wooden frames. The total collecting area was about 35 square meters, and for comparison Jennison's antennas were each 500 square meters. The Michelson system is, of course, much more sensitive than the intensity interferometer and the smaller antenna was adequate. Our movable antenna is shown in Figure 2, in a photograph taken at Lower Withington, about 2 km from Jodrell Bank. Incidentally, I did most of the construction of the antenna, using wood, chicken-wire mesh, and aluminum tubing. I creosoted it against the English weather, and I was really rather proud of it. The two little trailers in the photograph are the ones that Sir Bernard Lovell showed in one of his slides; they originally contained military-surplus diesel generators that he had used several years earlier. One of them was used for the receiver, and one for the radio-link transmitter, and we had a dipole antenna transmitting back to Jodrell Bank. The link receiving antenna was mounted near the top of the tilting mast of the paraboloid antenna; the mast was a very convenient device.

Fig. 2. The remote station of the 158 MHz interferometer. Receiving equipment is in the two small trailers. The antenna on the pole at the left is the radio link back to the main station at Jodrell Bank. Circa 1954.

The main receiving equipment for the interferometer is shown in Figure 3. I will not describe it in detail, but must mention that to me this is a very nostalgic picture. I had a great deal of satisfaction putting it all together, and I remember that having several racks of electronics was something of a status symbol amongst Jodrell graduate students at the time.

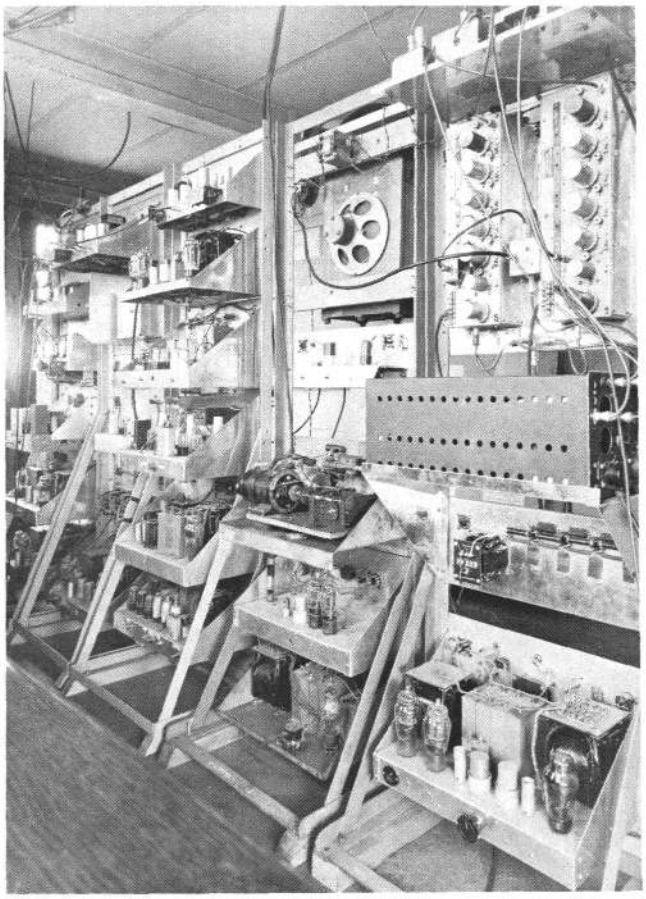


Fig. 3. 158-MHz long baseline interferometer at Jodrell Bank, circa 1954. The picture shows the back of the electronics racks in the main receiving station at Jodrell Bank. Built by A. R. Thompson and H. P. Palmer.

One interesting feature of the interferometer came from a very good idea of Hanbury's, which we described as the rotating lobe system (Hanbury Brown, Palmer, and Thompson 1955a). Figure 4 shows a simplified schematic diagram. In the actual system we used double conversion receivers, and the second local oscillators for the two channels were about 2 kHz apart in frequency and derived from independent crystals. When correlated signals entered the two antennas, they arrived at the detector with the 2-kHz frequency offset. Thus they produced a 2-kHz component at the detector output, and we used a synchronous detector to select the correlated component from the radio source. The reference frequency for the synchronous detector was obtained by beating the two oscillator frequencies. The motion of a source through the interferometer fringes produced a continuous change in the phase of the 2-kHz signal from the

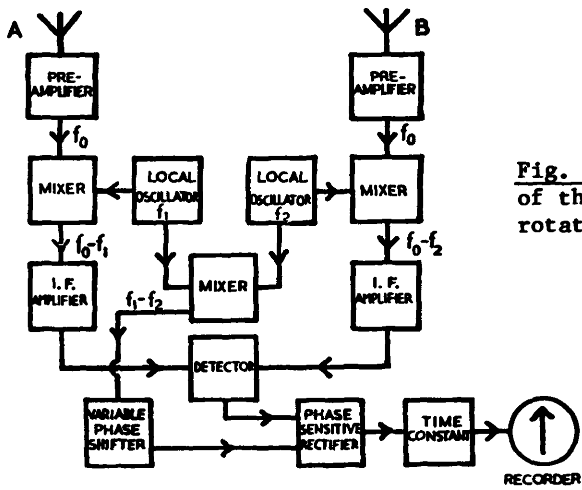


Fig. 4. Simplified block diagram of the interferometer including the rotating lobe system.

detector, so it was possible to slow the fringes down simply by changing the phase of the 2-kHz reference signal. This was easily performed using a synchro driven by a variable speed motor. The interferometer was thus the first in which it was possible to slow down the fringe oscillations which were, of course, recorded directly on a chart recorder. This was an important advantage with the long baselines, because to get sensitivity it was necessary to use a long time constant. Figure 5 shows what fringes from a fairly weak source looked like. If the natural fringe frequency is high, as in the upper

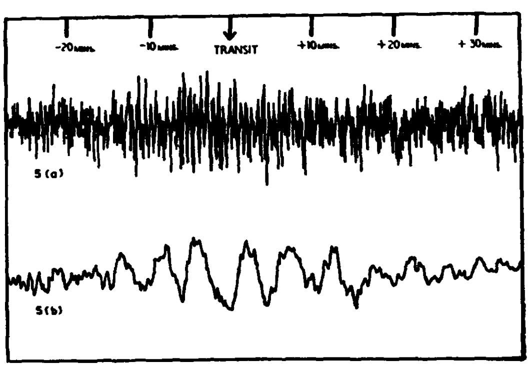


Fig. 5. (a) Record of a weak source observed with a baseline of 630 λ in a direction east and west, taken with the phase-shifter stationary. The output time constant is 4 seconds. (b) Record of the same source taken under identical conditions but with the phase-shifter rotating and decreasing the frequency of the pattern, and the output time constant increased to 30 seconds.

trace, one has to use a short time constant and this results in a relatively high noise level. However, if the fringes are slowed down, as in the lower trace, a longer time constant allows a much better measurement.

The interferometer was first put into operation using a coaxial cable rather than a radio link to bring the IF and local oscillator signals back from the remote antenna. Tests on Cygnus A with a baseline of a few hundred wavelengths showed the system to be working well, so it was something of a surprise when we first tried observing some of the Class I sources in Hanbury

Brown and Hazard's (1953) catalog and obtained no responses. Eventually we realized that the antennas must be too far apart, and we put them as close together as we could, with the array right at the edge of the paraboloid. Figure 6 shows a record of one of the sources that was then obtained. This one was near right ascension 5^h, and we measured an angular width of 1.4° for

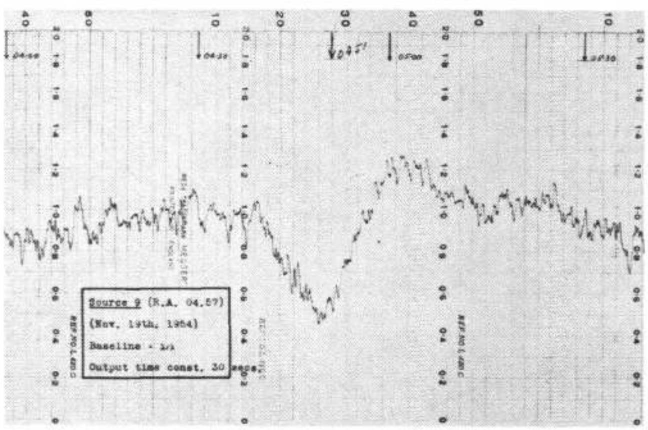


Fig. 6. Record of source no. 9 in Hanbury Brown and Hazard's (1953) catalog, taken with a baseline of 21 wavelengths on Nov. 19, 1954.

it. As a result, Minkowski was able to identify it with a supernova remnant (Hanbury Brown, Palmer and Thompson 1954). In principle, we should have been able to use our lobe rotation system to increase the output fringe frequency at this very short spacing, but in practice we found that cross coupling of the receiver noise between the two antennas caused large oscillations on the chart if we rotated the phase shifter. We examined a total of five Class I sources and were able to obtain records of two of them. The others were too wide for the interferometer, and when we re-examined the total-power survey records the large angular widths were confirmed. However, they could hardly have been deduced from the total-power records alone because the galactic background gradients caused some uncertainty in the baseline level.

The next step was to examine the Class II sources, and we soon found that we could not resolve them within the one-km baseline limit of our cable. We therefore built the radio link using three transmitter frequencies near 200 MHz. Incidentally, we never asked for an official frequency assignment as the the VHF bands were very little used at that time. The longest baseline that we used for the interferometer observations was 20 km (10,600 wavelengths) with the small antenna at the Cat and Fiddle Inn in Derbyshire. The only problem that we ever had with our pirate radio link was with the television set in the lounge bar at the Cat and Fiddle. The morning after the first night that we set up to observe we got an urgent telephone call from the landlord saying that he didn't think he'd stay in business if we kept wiping out his television reception. We found that we had slightly misadjusted the transmitter, and took care to see that it did not happen again. The Cat and Fiddle site provided the longest interferometer baseline ever used up to that time, and when the observations were completed, three sources remained unresolved. We were able to assign an upper limit of 12 arcseconds to their angular widths. These were 3C147, 3C196 and 3C295 (Morris, Palmer and Thompson 1957). Figure 7 shows a record of 3C147. The sources did not, of course, have 3C numbers at that time, and we referred to them by the numbers in Hanbury Brown and Hazard's (1953) catalog.

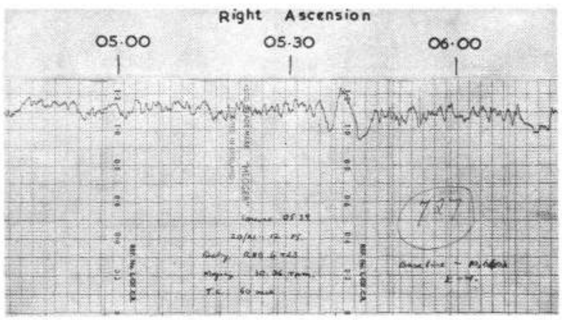


Fig. 7. Record of 3C147 taken with the interferometer. The remote station was at the Cat and Fiddle Inn in Derbyshire, which provided a line-of-sight path to Jodrell Bank for the radio link. The antenna spacing was 10,600 wavelengths at 158-MHz, the longest achieved by any radio interferometer up to that time. Taken Dec. 20-21, 1955.

I should mention one other experiment that we did with the interferometer. In 1954 we heard from Professor Kopal at Manchester that Dombrovski in the Soviet Union had measured linear polarization in the optical emission from the Crab Nebula. These observations showed that the light was 9% to 15% polarized. We therefore decided to try and make a polarization measurement on the radio emission. We could not use the large paraboloid for two reasons; first, all of the wires on the reflector surface ran east and west only, and second, the beam would not go down to the declination of the Crab Nebula. There were two smaller dishes available; Figure 8 shows one of them. This

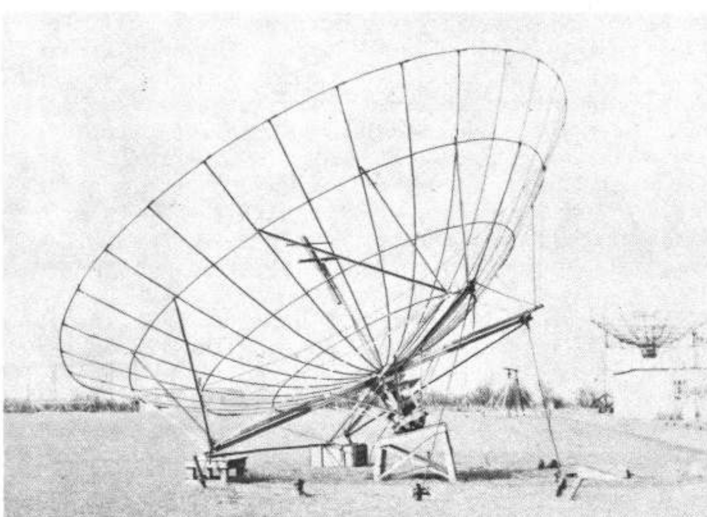


Fig. 8. One of the two small paraboloids used for the polarization measurements. The dish diameter was 25 ft. Crossed dipole feeds were used for observations of linear polarization. The other antenna was 30 ft in diameter and can be seen in the background in Fig. 1. With both of these antennas it was possible to rotate the whole dish, not just the feed, about the paraboloid axis. Circa 1954.

antenna could be pointed in many directions in the sky by just using the right sized boxes! We put crossed dipoles in each antenna, and used coaxial relays to switch between horizontal and vertical planes of polarization every few minutes. Figure 9 shows a record of Cygnus A observed with this system. With

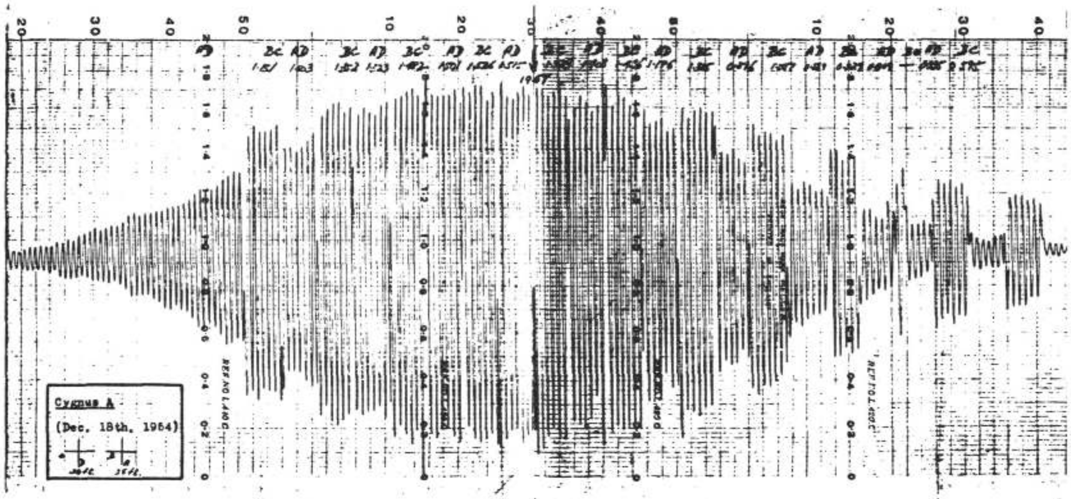


Fig. 9. Record of Cygnus A taken with the linear-polarization system on Dec. 18, 1954. The antenna beams remained fixed in the meridian and the source was observed around transit.

the source in the edge of the beam, the fringe amplitude changed on switching because the beamwidth varied with polarization, but near the beam center the responses became equal. Here we were using our lobe rotator to speed up the fringe pattern because otherwise there would have been only one or two fringes across the record. We were able to put a limit of 2% on the linear polarization from the Crab Nebula and obtained a corresponding limit of 1% for Cassiopeia A and Cygnus A (Hanbury Brown, Palmer and Thompson 1955b). I have always felt that there was some lack of serendipity in this result. We had the right source (the Crab Nebula) and the right sensitivity (about 2%), and I like to think that we would have been the right people at the right time, but of course we were at the wrong wavelength. At 1.89 meters there isn't very much in the way of polarization from the Crab Nebula. It was not until about 4 years later that Mayer, McCullock and Sloanaker, working with the 50-ft dish at NRL that Fred Haddock has described in his paper, found polarization at about the 2% level in the Crab Nebula at 10 cm wavelength.

Let me finish by saying that when I left Jodrell Bank in 1956, David Morris had joined the group just a few months earlier, and the interferometer experiments went on from strength to strength with ever-increasing baselines. By 1962, baselines of 110 km had been reached using radio links, and later the observing wavelength was decreased to 75 cm. With the introduction of VLBI techniques in 1967, the radio-link technique was overshadowed for some years, but emerged again in the 1970s in the MERLIN array (Davies, Anderson and Morison 1980). Looking back one can see that the series of high-resolution instruments at Jodrell Bank originated with the 218-ft paraboloid, and the single-dish and interferometer observations that were performed with it. Sir Bernard Lovell has described the serendipitous manner in which this antenna came to be constructed and made available for radio astronomy. The influence of these early experiments can be found in successive instruments, up to the present time.

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THE ALMOST SERENDIPITOUS DISCOVERY OF SELF-CALIBRATION

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National Radio Astronomy Observatory

Figure 1a shows a map of a quasar made with the VLA. Much of the low level positive and negative features visible around the point source are errors caused by the atmosphere. However this image, which is good to the 2% level, is as good as was ever expected from the VLA at high frequencies and with high resolution. After using a special technique of antenna based self-calibration the same data was used to obtain the map shown in Figure 1b. The same contour levels are used.

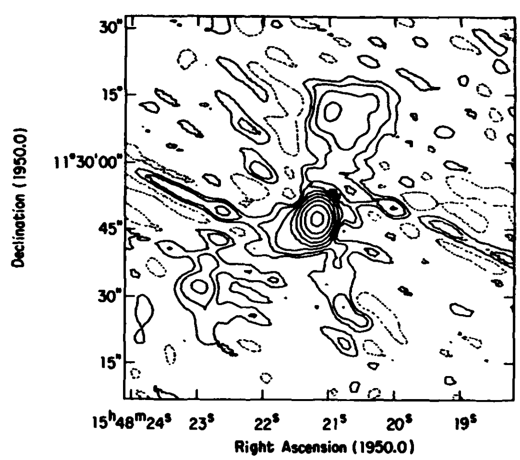


Fig. 1a

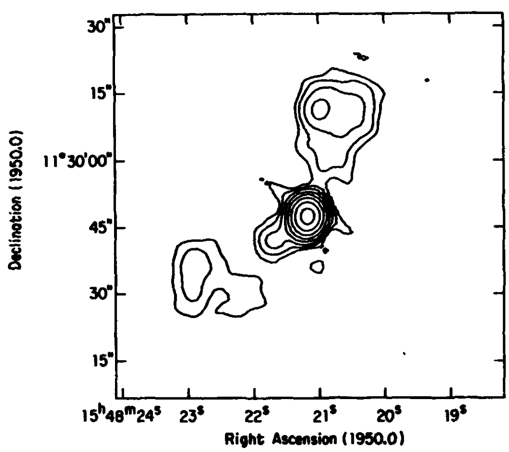


Fig. 1b

Fig. 1. Map of the radio quasar 1548+115. (a) Without self-calibration. (b) With self-calibration. The lowest contour level is 0.6% in each case.

The defects in the map have been reduced by about factor of ten by removing errors caused by the atmosphere. You can now see fainter structure and a lot more astronomy can be done with this image. The technique used to correct this map is the subject of my talk. Unlike most of the present speakers, this talk does not relate to any discovery that I have made: however, in tracking down the history of the development of this "self-calibration" technique, I have found some interesting twists that I thought would be interesting to discuss.

The construction of images such as this VLA image (similar images are being made with Westerbork, MERLIN, VLBI) is a very recent development compared with the other talks this morning relating to the discovery of the technique of aperture synthesis. There is an incorrect notion that this technical area has been fully exploited, we now know how to do it all, but what I am describing is a very new development, and because of the enormous improvement in map quality, this technique is as dramatic a step as the development of aperture synthesis itself.

First of all, the technique has many other names: it is also known as "antenna based self-calibration", "redundant spacing interferometry", "hybrid mapping", "phase closure", and "adaptive optics". In order that you clearly see the links between the development of these procedures, I will try to give you a physical picture of the technique that I am talking about.

Let us first look at adaptive optics, because that is the easiest way to visualize what is happening. Consider an optical reflecting telescope forming an image from an incident wave-front. If the wave-front is undisturbed, we will have a nice diffraction limited image, but in practice the atmosphere will generate an irregular distortion in the wave-front at any instant and these irregularities will result in a distorted image. Now if it were possible to deform the reflector in such a way that it just cancelled the irregularities in the wave-front you would again get a good diffraction limited image. In adaptive optics this is what is done. Some part of the optical system is made flexible and by using pistons, and such things, it is deformed in real time until a sharp image is achieved. If the signal to noise ratio is high enough, this technique will work and the atmospheric effects are cancelled (e.g. Muller and Buffington 1974). Since the atmosphere is changing continuously, it is necessary that the optical system be deformed at a fast enough rate to keep up. Although such a system has been built, there are many technically difficult problems.

Now what has this to do with the self-calibration of a radio telescope array? Consider a very idealized synthesis telescope which has many antennas intercepting a wave-front, which again is distorted by the atmospheric irregularities. We take outputs from these antennas and put these outputs into a black box which, by measuring correlations and doing Fourier transforms, generates an image at its output. This is analogous to what happens in the the image plane of the optical telescope. If we want to correct for the irregularity in the wave-front due to the atmosphere, we can proceed in a way which is analogous to the optical method; i.e. we could use pistons to move the telescopes up and down and continue to vary the positions of the telescopes until we obtained a sharp image. Of course this would not be a particularly practical procedure but if we go into our black box at a point where we have all the correlation coefficients between antenna pairs in digital form then, by varying the phase of the complex correlations, we have a very elegant approximation (i.e. use phase rather than delay) to the effect of moving the telescopes up and down. Furthermore this can now be all done in the computer, and since the digital data is stored it need not be done in real-time. This avoids all the technically difficult hardware problems which beset the adaptive optics systems. This is one way of looking at what the self-calibration procedure is doing. It is using the information in the image we are looking at to apply corrections which are equivalent to moving the telescope up and down in order to flatten-out the wave-front.

Now what does phase closure have to do with this? In the preceding discussion you may be puzzled about why the distorted image contains the information needed to correct the wave-front and where this information came from. An interesting way to see where the information has come from results from analysis of the phase closure relations first discovered by Jennison

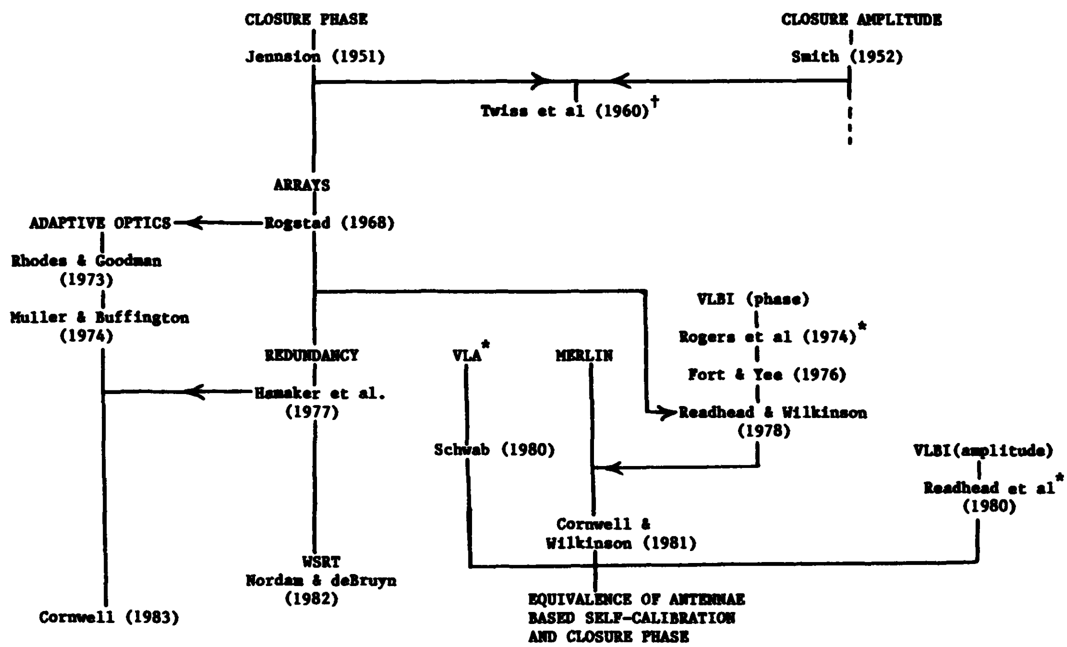
(1951, 1958). In his discovery paper he considers three aerials; A, B, and C which form three interferometers AB, BC, and AC. If the source visibility has phases ξ_{AB}, ξ_{BC} and ξ_{AC}, and the atmosphere introduces independent phase errors δ_A, δ_B and δ_C in each telescope then these telescopes errors will all cancel in a phase closure relation:

$$
\begin{aligned}
&\phi_{AB} + \phi_{BC} - \phi_{AC} \\
&= (\delta_A - \delta_B) + \xi_{AB} + (\delta_B - \delta_C) + \xi_{BC} - [(\delta_A - \delta_C) + \xi_{AC}] \\
&= \xi_{AB} + \xi_{BC} - \xi_{AC}.
\end{aligned}
$$

This gives some information about the source which is independent of the effects of the atmosphere. With more telescopes there are more relations of this kind which must be satisfied and it is from these constraints that the atmospheric contribution to the phase can be estimated and removed. In the optical case the method works because an element of the aperture does not just effect one point in the image, it effects all the Fourier components in the image, associated with that element in the aperture. Again this results in extremely strong constraints on the problem which make it possible to determine the atmospheric errors. If the same spacing occurs in the aperture more than once the constraints become independent of the model and this is known as redundant spacing interferometry (Hamaker, O'Sullivan and Noordam, 1977).

Hopefully these remarks have clarified the relation between these apparently different techniques which are in fact just different ways of looking at the same problem. When was it realized that these were all the same? It is the history of this question which turned out to be most fascinating (Figure 2). It all started with what Hanbury Brown referred to as Jennison's "irrelevant by-product" of the research he described earlier in this meeting. Although Jennison developed and used the concept of phase closure the "Hanbury Brown's" were too good, they didn't need this technique! They were so good at their "wire welding" that they could build phase stable interferometers and there was no need to rely on these tricks. Subsequently almost everybody forgot about the technique, except for one person who seems to never forget anything that he reads, Al Moffet. His name does not directly appear in my little development tree in Figure 2 but I believed he passed the information on to many of the people further down in the diagram. One of these was Dave Rogstad (1968) who generalized the phase closure relations to arrays of telescopes and noted the possible application to optics where atmospheric seeing is a major problem. This input to the optical community had an important impact on the development of the concept of adaptive optics (e.g. Rhodes and Goodman 1973) and many optical papers on this subject contain references to the original work of Jennison, more references than you will find in the radio literature at this time! About now very long baseline interferometry (VLBI) was developing and badly needed phase closures because all of a sudden the "wire welders" were no longer able to build phase stable systems. The VLBI community again resorted to the use of this technique, first Alan Rogers et al., (1974) and Fort and Yee (1976) then very successfully with algorithms of Readhead and Wilkinson (1978) and Cotton (1979). This use of the phase closure relations in VLBI revolutionized the subject. For the first time real imaging was possible. From the VLBI community the method was taken by Cornwell and Wilkinson (1981) for use in MERLIN, Jodrell Bank's long baseline microwave linked array. It is interesting that the technique returned to

Jodrell Bank through this rather tortuous route, rather than coming directly from the earlier Jodrell Bank work by Jennison who had long since left.



*Independent rediscovery

†Added to figure subsequent to presentation - see A. Moffet discussion.

Fig. 2. The development of the adaptive self-calibration concept.

Now let us look the completely independent development which occurred at the VLA. For pragmatic reasons, and I believe Barry Clark was initially responsible, the fact that the number of antennas, (N=27), was very much less than the number of possible correlations, N(N-1)/2=351, made it very inefficient to write software the way it had been done before. Previous interferometer software was designed to work on the correlations between antenna pairs. Barry Clark said "we could save a lot of computer space (some of you will recognize this remark!) if we worked with the antennas instead of the correlators." The software was designed that way without realizing the impact that this would later have. Various people, notably Fred Schwab (1980), although many other people were involved in the story from here, took this one step further by realizing that if your map contained a dominant point source you could use it to determine the antenna based gains and phases and thus self-calibrate the array using the image itself. Since there were many more correlations than gains to be measured this also worked for more complex images.

The last step necessary to complete the various connections now occurred to Cornwell and Wilkinson (1981) who each independently realized that from the antenna based calibration concept you could trivially derive both the phase and the amplitude closure relations. The full connection with adaptive optics was soon to follow (Cornwell 1983).

In the title I used the words "Almost Serendipitous" because the VLA antenna-based calibration was entirely independent of the original Jennison phase closure work and the other work on adaptive optics. However it would obviously have led to the discovery of the same technique. An interesting piece of anti-serendipity occurred at Westerbork. Although we were very close to exploiting this technique at Westerbork there were two reasons why we did not do so. One was that, as in Hanbury's time, the engineers were too good. When it was suggested that the redundancy of the Westerbork telescope could be used to determine the phase of an additional antenna the engineers remarked "no need, we can build a sufficiently phase stable link". The stupid mistake was that we did not realize that at the same time we could be removing the errors in the atmosphere as well as in the link! Furthermore the Westerbork software system was entirely correlator based making it difficult to easily include these antenna based effects. Such experiments were always done outside the normal calibration software and the technique did not develop properly until the VLA made better maps than the WSRT. After that international competition did the rest (Noordam and de Bruyn 1982).

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D. Heeschen: Roger Jennison's paper was known to the VLA people in the mid-sixties and was discussed during the design phase at that time. I think it wasn't followed up for two reasons. One, I don't think anybody knew how to handle the mathematics that would be involved, and nobody knew what the effect of the atmosphere was going to be on the VLA at that time. But I know that his paper was known to and discussed by people in the mid-sixties.

A. Moffet: The family tree of this business is almost as complex as the Jansky family tree would be if that had been presented to us. But it needs one more node. A very important paper, which was probably even more forgotten than the Jennison paper was one by Twiss, Carter and Little. This was the same Twiss you've heard about before in another context and he really did make a number of novel contributions by including the theory of synchrotron self-absorption. But he and Carter and Little put together an interferometer using elements of the Christiansen cross which employed both phase and amplitude closure and used the concept of complex gain in each antenna.

Twiss, Carter and Little (Aust. J. Phys. 15, 378, 1962) did use both phase closure and gain ratios, but they treated them separately and did not use the complex antenna gain concept.

M. Schmidt: Horace Babcock wrote a paper in the PASP many years ago about a rubber mirror approach.

THE DISCOVERY OF PULSARS

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This story starts properly in the mid-1960s when Margaret Clark, who was responsible for quite a bit of the 4C survey, happened to notice that some of the sources in that survey showed a kind of scintillation. It wasn't ionospheric scintillation, but some other kind of scintillation, and she noticed that it was the sources that were believed to have small angular diameter which showed this new kind of scintillation. Tony Hewish followed it up and thereby the technique of interplanetary scintillation was developed. For those of you who have long since forgotten what interplanetary scintillation is or was, you remember that the solar wind streaming out of the sun is not uniform but has density irregularities, and when you view a radio source through this irregular medium (particularly at relatively low frequencies) you are seeing through one blob, no blobs, or one and a half blobs, and the radio source twinkles as the blobs streak past.

Nature has been very kind to us in that the compact sources which show this type of scintillation tend to be the quasars, and the larger angular diameter radio sources which do not scintillate are the more normal radio galaxies. Tony Hewish realized this would be an excellent technique for creaming out the quasars in the sky, and he put in a grant application to build a large telescope specifically to monitor the sky for sources which showed this scintillation. It had to be a large radio telescope because you have to work with a relatively short time constant; we used something like a tenth of a second. To overcome the degradation of signal-to-noise which that short time constant gives you, you need a large collecting area.

I believe the grant application was only for ten or fifteen thousand pounds. That was in about 1964. So although its not a thing one likes to advertise too much when there are scientific administrators around, this was not an expensive piece of equipment. He decided to work at a frequency of 81.5 MHz, that is a wavelength of 3.7 meters or just about 12 feet. He estimated that the area this machine would have to cover would be about four and a half acres, or 57 tennis courts. Because it was being built at 81.5 MHz, it could be done with a technique that Tony Hewish loves, which is wooden posts and string, and wire and things hung on things. It was really very similar to a lot of the aerials we were shown yesterday from quite a few years before that.

Figure 1 shows an internal view of the radio telescope. It also shows Don Rolph, the technician who helped with a lot of the building. Don had been a lad in the Navy during the War and was Navy trained. And, my goodness, he nearly drove us around the bend on occasion with his meticulousness! We were anxious to get this thing built and working and were happy to have any kind of lash-up, but Don wasn't having any of this. I think it is very fair to say that it was really due to Don's standards that that telescope worked the first time it was switched on, and not many radio telescopes do that.

Fig. 1. Internal view of radio telescope used to discover pulsars.

Apart from Don and the step ladder, Figure 1 shows a lot of wooden posts. By Don's head is twin wire feeder curving down from a dipole to more twin wire feeder. Behind his shoulders is a second row of dipoles with the next lot of twin wire feeder; the thicker wires are coaxial cables. The reflecting screen was tilted because this was a solar based phenomenon. We wanted to look around the ecliptic plane, and the blaze angle matches quite nicely with the inclination of the ecliptic; the reflector was miles and miles of wires strung across these slanting beams. There were over a thousand posts; over two thousand dipoles; one and two-thirds tons of copper wire bought in the height of a Rhodesian crisis (we were always slightly afraid we'd come up one morning and find somebody had nicked it so valuable was it); eight and a half miles of cable and seventy seven more miles of reflector wire. You won't need telling it operated as a transit instrument. There were 16 of these rows and we could introduce phase delays between them and so swing the beam in declination.

We built it ourselves, four or five of us, assisted one summer by some very keen vacation students who came and spent their summer hammering these posts into the ground and were still enthusiastic at the end of it. It was amazing! It took us two years to build it, manual labor, not really anything else, working summer and winter in Cambridge. Winter in either Cambridge is pretty fierce; I would occasionally reappear in the Cavendish Laboratory, very brown from all the wind, and somebody would say, "Have you had a nice skiing holiday, dear?" We also became very strong. By the time I left Cambridge I could swing a twenty-pound hammer.

The Science Research Council is the body in Britain that funds the research studentships, and they lay down terms of reference describing the purposes of these studentships; one of the items is that the students gain experience of techniques. Research student advisors manage to hide a lot of things under that clause. So I like saying I got my thesis with sledge hammering! But actually I was let off the worst of the sledge hammering; I was given the job of making all the baluns and two to one transformers for the miles and miles of cable that we had. But it was a very healthy existence, certainly, and we did become extremely muscular. Each summer in Cambridge there was a fair - a mid-summer fair. You can imagine the sort of thing, bumper cars and helter-skelters, booths where you shoot down bent rifles, or throw crooked darts. And there was one of these test your strength machines, where you hammer. One of us broke the bell off the top!

Figure 2 shows an aerial - aerial view! It's taken illegally from up in one of the dishes of the one mile telescope. Martin Ryle was very sensitive

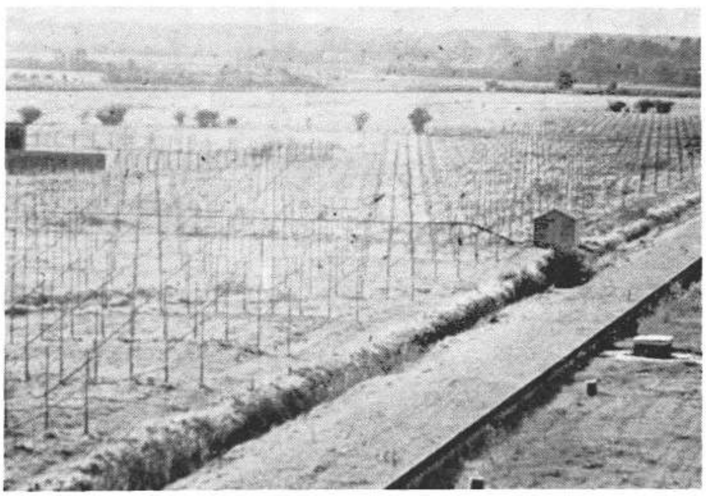
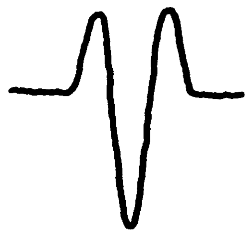


Fig. 2. Aerial view of aerial.

about people climbing over the one-mile telescope. The hut near the rail track is the kind of garden shed you'd have in your backyard and indicates the scale of the instrument. The aerial is mostly a lot of empty space, a lot of posts,and invisible wire between them. But it covered quite a large acreage. The building at the left-hand edge is significant. It was a large hangar or warehouse built of corrugated metal and owned by the Air Ministry, and it comes into the story later on.

Two years were spent building that instrument and it came into operation in July of 1967. We used four beams at a time to scan four different declinations and then switched it to a different set of beams the next day, and so on, so it took four days scanning all the sky between declinations -10 and +50. It means that each patch of sky was observed about 30 times in a 6-month interval. We operated with a 0.1 second time constant; the other bit of technicality that we might want later concerns the shape of the interference pattern. The interferometer was built all as one, but was in fact electrically in two bits. The two bits in the interferometer were touching each other, and when you have an interferometer like that, the pattern that you get out of it consists of a small bump, a big bump and a small bump, and that's it. Cambridge people will know that that is called a "Chad". It's taken from a war-time cartoon where there was a character called "Chad" who was to be seen



an ideal chad

Fig. 3. Chad.

the actual chad

peeping over a wall; two hands, a nose, a head and two eyes, accompanied by the cheerful cartoon "What, no eggs?" or sugar? or whatever it was that had just gone out of circulation. So this interference pattern was known as a "Chad".

Ours was slightly asymmetric because I never got the cables from the two halves quite the same length, which later turned out to be very useful.

We recorded the output on a 3-track strip chart: one track was the interferometer output; we also high pass filtered it to take out the "Chad" and leave us with the scintillations; a third track was used to measure the amplitude of the scintillations.

Figure 4 shows some scintillating sources: at the center top is a very good example, a nice "Chad" with scintillation; below it, on the high pass filtered output, the scintillation (actually hitting the end stops) in three bursts. On the left is a radio source which does not scintillate, just a little bit of the "Chad" getting through on the high pass filter. On the right another radio source which is not really strong enough to show terribly clearly with a separate "Chad", but clearly is scintillating.

We analyzed (actually we didn't, I analyzed) all this chart by hand. With four beams, each with a three-track strip recorder, each running at one foot per hour, we had a hundred feet of chart paper every day, seven days a week, and I operated it for six months, which meant that I was personally responsible for quite a few miles of chart recording. To analyze it by hand was a deliberate decision, partly because with new equipment you don't want to

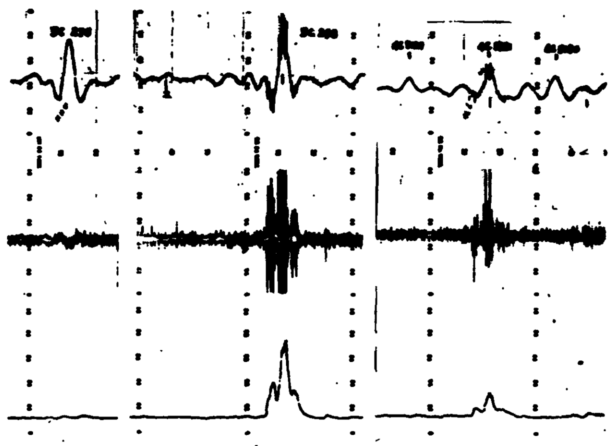


Fig. 4. Some scintillating sources.

put it straight onto the computer; you want to have a look and see what's happening and see that it's functioning OK. The other reason that it was done by hand was that we weren't at all sure that we could program a computer to distinguish between these scintillations and man-made interference. And certainly as I began to do the analysis, one could recognize the scintillating sources and one could recognize, usually as different, the man-made interference.

It was four hundred feet of chart paper before you got the back around to the same bit of sky again, and I thought (having had all these marvelous lectures as a kid about the scientific method) that this was the ideal way to do science. With that quantity of data, no way are you going to remember what happened four hundred feet ago. You're going to come to each patch of sky absolutely fresh, and record it in a totally unbiased way. But actually, one underestimates the human brain, I think. On a quarter inch of those four hundred feet, there was a little bit of what I call "scruff", which didn't look exactly like interference and didn't look exactly like scintillation.

My thunder has already been stolen by Walter Sullivan who mentioned yesterday that these bits of scruff were fantastic energy sources or little green men. The first one is shown in Figure 5 with the raw interferometer output and below it the high pass filter output. There is no obvious strong

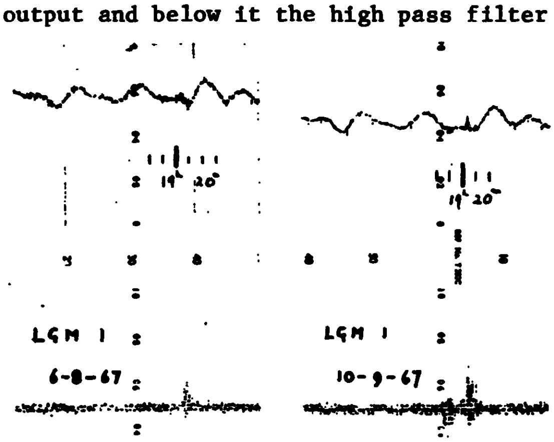


Fig. 5. Two records of LGM 1. Raw interferometer output is shown above, and below it the high pass filter output.

source, but there's something on the bottom records which doesn't look quite like scintillation, and doesn't look quite like man-made interference. I guess the first few times I saw that I would have noted it with a question mark and passed on. But after a while I began to remember that I had seen some of this "unclassifiable scruff" before, and what's more, I had seen it from the same patch of sky, at that sort of right ascension and that sort of beam setting, and it began to click. It was a little bit uncertain whether it could be scintillation or not because it was happening in the middle of the night at that time of year. The scintillation being interplanetary scintillation was a solar based phenomenon, so you expect it in the daytime. (I very carefully chose radio astronomy and interplanetary scintillation because I know I'm no good staying up late at night.) But anyway, this was happening in the middle of the night.

I was also slightly confused as to whether it was keeping constant right ascension or not. I now realize, returning to the "Chad", what was happening is that it never performed in all three bits of the Chad; it never kept going for three minutes. Sometimes you might get one minute's burst, and then the scruff was in one part of the Chad, or sometimes you'd get two minutes worth in two bits of the Chad. And this really screwed me; my right ascensions were wrong by up to a minute on occasion. The left-hand side of the diagram indicates one bit of the Chad showing, and two bits showing on the right, with the reversed polarity showing rather interestingly.

I pointed this out to Tony Hewish who said, "Yes, it's interesting. We must follow it up". But we had to put off following it up partly because we hadn't quite finished building the telescope yet. We hadn't got all the receivers, and we weren't up to full strength, and also we wanted to do a particular recording of 3C273. So it was the end of October or early November before we got time to doing these special observations. The idea of this special observation was to run the chart paper faster underneath the pen and spread out this scruff so that you could see what it was, what kind of frequency structure it had. One idea was that it was a point, or nearly point radio source, which would have been infinitely useful for calibrating the interplanetary scintillation technique. To check that it was point or near point, you would use this spread out information on frequency structure.

As a research student it was my duty to go out to the Observatory each day at the time of transit of this thing and switch on the fast recorder and get a nice recording of this scruff. Every day for the best part of November, I went out to the Observatory and switched on the fast chart recorder and got lovely recordings of receiver noise, but no sign of the scruff. Tony Hewish was getting a bit peeved. "Oh, it's a flare star; it's gone and died and you have gone and missed it". One day I skipped going out to the Observatory specifically to attend an interesting lecture in Cambridge, and when I went out to do the routine change of the charts next day, the scruff had reappeared. However, a day or two later, I did actually get it.

Figure 6 shows some low level interference and the scruff (labelled CP1919). There's not an awful lot of difference, but there's just enough difference that you don't confuse them too frequently.

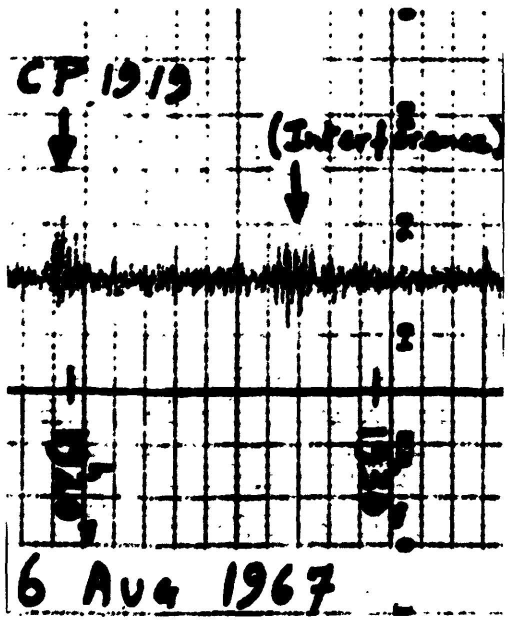


Fig. 6. Low level interference on right compared with scruff (pulsar) on left.

Figure 7 shows the fast chart recording. Along the bottom are one-second time ticks - *man-made* - along the center is the trace that we got on that first occasion. (The trace at the top was superimposed later - it's the first

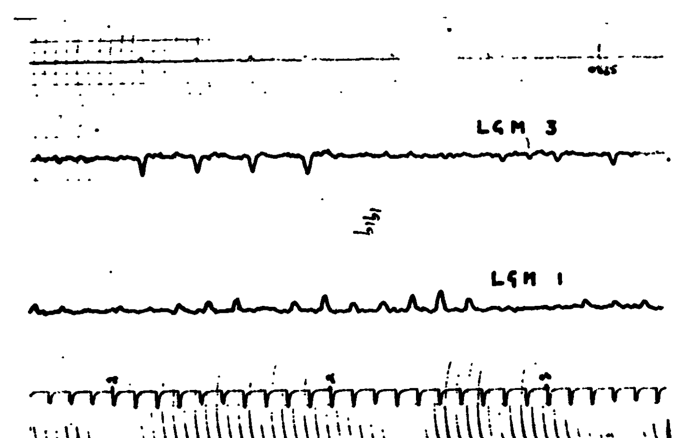


Fig. 7. Fast chart recording of pulsars.

recording of 0834.) You can see that there is a periodicity, maybe with some pulses missing, but there's definitely a periodicity, and it was about one and a third seconds. This is slightly surprising; even I knew it was slightly surprising. When the source had transited and there was nothing else I could do for twenty-four hours, I wondered what I should do next. The scientist in me said, "Ah, you must do some kind of test". The only thing I could think of to test was the time constant of the pen recorder - it's a very good pen recorder. I thought perhaps it was worth telephoning Tony Hewish. By this time this source was transiting in the middle of the afternoon so he was teaching in an undergraduate laboratory. He was probably dealing with some twit of an undergraduate who thought his diffraction grating had three lines per inch or something, and he's phoned up by his twit of a post graduate who says "That thing is a string of pulses one and third seconds apart, and it's nothing to do with the time constant of the pen recorder either." Tony Hewish said, "Well, that settles it, it must be man-made."

He was interested enough, however, to come out to the Observatory next day at transit time and, fortunately, the beastie performed to order. I realize now how very lucky I was. Those things are highly variable and just when you've got somebody coming to see them, they refuse to perform. But it did, and it clearly was pulses at one and third seconds. And that's really where our headache started. Tony Hewish took a whole load of my old chart recordings and checked the right ascension of the thing and worked out why I'd been getting in a muddle about its exact right ascension. He worked out that it had kept constant right ascension over the three or four months that we had been observing to within about ten seconds. Which means that although it looks man-made, it's not normal *earth-man-made* because normal *earth-man* works to a twenty-four hour schedule, not a twenty-three hour and fifty-six minute schedule - except of course for other astronomers. So Tony wrote to all the observatories in Britain and said, "Have you had any program going since August which might conceivably be causing interference?" Well, you know the answer, they all said No! Fat chance of anybody saying Yes to that one!

Because our Chad was a little bit skew, we could say that this thing, whatever it was, was going through the telescope beam in the same direction as the stars and at the same rate as the stars. So this means it's not *earth-man-made.* It wasn't other astronomers, it wasn't radar being bounced off the moon and into our telescope, it wasn't a satellite in a funny orbit. I got all worked up about that Air Ministry hangar made of corrugated metal, which was just to the south of the telescope, but in fact the corrugations on that have a wavelength of only a few inches, and at a radio wavelength of 12 feet of course you don't see that kind of detail, so it wasn't that either. There still remained the possibility that it was something to do with our instrumentation.

Next, Paul Scott and Robin Collins had converted the cylindrical paraboloid, which had been used in 4C, to eighty-one and a half megahertz, the same frequency as we were using, and we decided that the best test would be to see if they could pick it up with this instrument and their own receivers. So they set about to do that, and at the appointed time we all clustered round their chart recorder - and nothing happened. We had actually miscalculated when the source was due to transit in that instrument! Tony and Paul Scott had started walking down the very long lab at Lords Bridge (with me panting along behind them trying to keep up in every sense of the word) saying, "What is it that appears in our telescope but doesn't appear in yours?" Robin had stayed by the chart recorder. There was a sort of strangled cry, "Here it is!" and we all rushed back up the labs!

That meant it wasn't instrumental. And we really now were very stuck; it looked as if it ought to be a star, it went around the sky with the stars, and yet it looked man-made. This is where this silly notation LGM came from. If it's not earth-man-made, maybe it's Little Green Men out there trying to signal to us. OK, if it's Little Green Men, they're probably on a planet. Their planet goes around their sun, and we ought to be able to see the Doppler shifts, the changes in the pulse period as they go round their sun. And incidentally, we could probably do a proper motion test at the same time. So we set about making fast recordings of this thing every day to study the pulse arrival time. We proved that the earth went round the sun but we didn't find any other motion! John Pilkington and Paul Scott managed to measure the dispersion of the signal, and guessing - not a very good guess - but guessing at the electron density came up with a distance of 65 parsecs.

By this time it was about Christmas, and I went into Tony's office one day to consult with him about something and walked in on a high level discussion. We don't really believe this is Little Green Men, but we don't have any positive suggestions as to what it might be. How on earth do we publish this? Up to then we had been so scared of making fools of ourselves that we had played our cards fairly close to our chest. I went home that night distinctively peeved! I was now two and a half years through a three year studentship and here was some silly lot of Little Green Men using <u>my</u> telescope and <u>my</u> frequency to signal to planet earth. And after supper, fortified, I came back to the Laboratory, because with all these antics with pulsars, these funny things, and the routine charts still pouring out at a hundred feet a day, the analysis of them was running very far behind. Late that night, when I was in danger of getting locked in the Cavendish for the night, I was looking at a piece of chart around about eleven hours thirty right ascension. In Britain you can see Cassiopeia A at lower culmination on the northern horizon, and it has an enormous pathlength right through the ionosphere, horrible ionospheric scintillation and it's really grotty; in the middle of that grot there was another little bit of scruff. I laid out on the floor all the other records of that bit of sky, and sure enough, occasionally, in amongst the mess of lower Cas A, were bits of scruff. I went out to Lords Bridge late that night when it was due to transit. It was perishing cold. When it was cold something in the telescope and receiver system didn't work properly, and of course it wasn't working properly. But I flicked switches and I breathed on it and I swore at it and I got it to work for five minutes on the right beam setting, and it was another string of pulses. 1.19 seconds period this time.

Well, I was due to go back to Ireland for holidays later that same day, so I dumped the charts on Tony's desk and went off much, much happier. It was highly unlikely that two lots of Little Green Men could choose the same unusual frequency and unlikely technique to signal to the same inconspicuous planet Earth!

I came back after Christmas. Tony had very kindly kept the survey running, he had put ink in the ink wells and charts in the chart recorders, and piled the charts unanalyzed on my desk. When I came back I couldn't immediately find him, but it was quite clear what I had to get on with. I spread out one of the charts, and there on the same piece of chart recording, about fifteen inches apart, were two more lots of scruff. Around about right ascensions 0830 and 0950. I began to wonder if I'd had too good a holiday! Well, when Tony appeared, he said, "Look, you'd better go back through all the past charts and see how many others you've missed"! Well, I was docile in those days and I did! Also, it was really very cold for going out to Lords Bridge in the dead of night to confirm those two, so they sat around for a couple of weeks until the weather improved, and then we got them. 0833 is about 1.27 seconds, but 0950 was very different. On the bottom of Figure 8 the time scale is shown in seconds. You can see the change in the polarity as it goes through the Chad. The pulse period is a quarter second; it's about the fastest pulsar we could have detected with that tenth of a second time constant. It clearly was a rather different kettle of fish. We had really very little idea what kind of stars these were at this stage, but this was obviously going to strain any theories that were around.

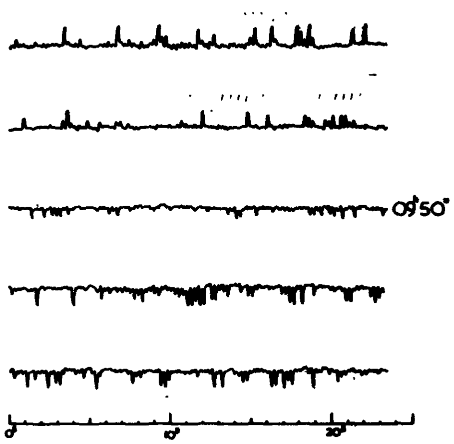


Fig. 8. Response of rapid pulsar 0950.

That was mid-January and at that point I handed the aerial over to the next research student down the line and retreated to analyze all those charts, measure the angular diameters of a lot of quasars and write a thesis. The pulsars went in an appendix. At the end of January the paper on the first pulsar was submitted. It was only based on the total of three hours observing, which I think is a bit risky, but we did it. Shortly before the paper was due to appear in Nature, Tony Hewish gave a seminar in Cambridge. He gave it a rather titillating title and it seemed as if every astronomer in Cambridge was there. Fred Hoyle was in Cambridge in those days and at the end of the talk he said, "This is the first of these stars that I've heard of." We had been trying to suggest they might be white dwarfs, but Fred's immediate reaction was, "I don't think those are white dwarfs, I think they're supernova remnants." Considering the hydromagnetic and neutrino opacity calculations that he had presumably done in his head, I think that was a very remarkable instantaneous conclusion.

In the paper we had been stupid enough to mention that the idea of other civilizations had crossed our minds, and when the press read that they descended. And when they discovered that S. J. Bell was female, they descended even quicker. I had my photograph taken standing on the bank, sitting on the bank, standing on the bank examining bogus records. One of them even had me running down the bank waving my arms, "Look happy, dear - you've just made a Discovery!" What would they have done to Archimedes?! They also asked a number of relevant questions like, "Was I taller than Princess Margaret, or not quite so tall? How many boy friends did I have at once?"

The name "pulsar," by which they are now known, was not coined by us but by a journalist. It was found written on the blackboard in one of our offices and responsibility has been claimed by the science correspondent of the Daily Telegraph.

DISCOVERY OF QUASARS

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Quasars are hard to find in the optical sky; there are as many as 3 million stars brighter than the brightest quasar, 3C 273. The situation is radically different at radio wavelengths. In the 3C catalogue 3C 273 is the sixth strongest source above galactic latitude 15 degrees. In hindsight, then, it is clear why radio astronomy was destined to lead us to the first quasars. If radio astronomy had developed much later, X-ray astronomy would have played the same role for the same reasons.

Historically, the epoch of the discovery of quasars must have been set by the gradual improvement of radio source positions coupled with the accuracy needed to select a relatively undistinguished looking star as a likely identification. I will use this occasion to chronicle the optical work that was carried out, once this stage was reached in 1960.

I am least familiar with the beginning since I was not directly involved. It all started with 3C 48 for which Thomas A. Matthews had obtained an accurate radio position with the twin 90-foot interferometer at the Owens Valley Radio Observatory. This source had a small angular diameter according to long baseline interferometry carried out at Jodrell Bank and it was expected to be a distant cluster of galaxies. However, when Allan R. Sandage took a direct plate of the field in September 1960 it showed a stellar object with faint fuzz at the radio position. Sandage obtained the first spectra of the stellar object in October 1960 which showed it to be extremely peculiar, the only prominent features being strong, broad emission lines. Photometry by Sandage of the 3C 48 stellar object showed that it had a strong ultraviolet excess, such as exhibited by white dwarfs. Guido Münch and Jesse L. Greenstein obtained further spectra in subsequent months. The results of the joint effort were presented in an unscheduled paper at the 107th meeting of the American Astronomical Society in New York in December 1960 (Matthews, Bolton, Greenstein, Munch, and Sandage 1961).

Further photometry of the 3C 48 stellar object showed it to be variable and the general impression in 1961 was that this was probably the first radio star (see Matthews and Sandage 1963).

My own work on optical objects identified with radio sources started after the retirement in 1960 of Rudolph Minkowski, following his remarkable determination of a redshift of 0.46 for the radio galaxy 3C 295. Tom Matthews supplied me with optical identifications of radio sources. Initially these were mostly radio galaxies. I am giving in Table 1 a list of first spectroscopic observations of optical objects with those radio sources that eventually turned out to be quasi-stellar radio sources.

TABLE 1

Initial Spectra of Optical Objects Identified with Quasi-Stellar Radio Sources, 1961-1962

Date	Radio Source	Optical Object
Apr 1961	3C 286	misidentification
Jun 1961	3C 280	misidentification
Jun 1961	3C 298	misidentification
May 1962	3C 196	quasar
May 1962	3C 286	quasar
May 1962	3C 273	misidentification
May 1962	3C 254	misidentification
Oct 1962	3C 147	quasar
Dec 1962	3C 273	quasar

All three objects observed in 1961 were spectroscopically uninteresting and eventually turned out to be misidentifications due to radio lobe shifts. It was not until May 1962 that two further quasars-to-be were observed spectroscopically. 3C 196 showed a continuum without emission or absorption lines. 3C 286 exhibited one broad emission line at 5170 A. I noted in a brief Letter that this line was not observed in any other astronomical object, including 3C 48 (Schmidt 1962). I also obtained in May 1962 a spectrum of a galaxy mistakenly identified with 3C 273.

The spectrum of 3C 147 observed in October 1962 showed several emission lines in the red part of the spectrum. I obtained several further spectra and discussed the material at a conference on extragalactic radio sources held at the Goddard Space Science Institute in New York in December 1962. I attempted to explain the spectrum in terms of helium emission from an expanding shell, but did not publish this interpretation as it was soon overtaken by further developments.

The next object among quasars-to-be was 3C 273. Cyril Hazard et al. had been observing lunar occultations of this strong source in April, August and October 1962, and found that it was a double. John Bolton sent us the first accurate positions obtained in August 1962. Tom Matthews found that the two sources coincided to within a few arcseconds with a thirteenth magnitude star and a nebular wisp or jet. I suspected that the jet was a peculiar nebula associated with the radio source and that the 13th mag. star was a foreground object. Since the jet was exceedingly faint and would require long exposures, I decided to take a spectrum of the bright star so that it could be eliminated from consideration.

The first spectrum of the star taken at the end of the night of December 27/28, 1962 was badly overexposed - I was not used to observing such bright objects. In the far ultraviolet the spectrum showed a broad emission line, at 3250 A. In addition, I saw emission lines at 5650 and 5820 A and suspected the presence of others. Two nights later I obtained a spectrum with the correct exposure and found several more emission lines. It was clear that 3C 273 belonged to the class of 3C 48.

Subsequently, J. B. Oke observed 3C 273 spectrophotometrically at the 100-inch telescope on Mount Wilson and detected a strong emission line in the infrared, at 7600 A. A total of seven emission lines was now known in 3C 273 and in hindsight it seems strange that with so much information no larger effort was undertaken to identify the lines. I showed the list of lines to I. S. Bowen and B. Baschek, but besides incomplete identifications with parts of the helium spectrum, no progress was made.

It was on February 5, 1963 that the puzzle was suddenly resolved. Cyril Hazard had written up the occultation results for publication in Nature and suggested that the identification results be published in an adjacent article. It was in the process of writing the article that I took another look at the spectra. I noticed that four of the six lines exhibited increasing spacing and strength toward the red. I attempted (not necessarily for any good reason) to construct an energy-level diagram based on these lines, then made an error which seemed to deny the regular pattern. I remember being slightly irritated by that, because it was clear the lines were regularly spaced - and to check on that I started taking the ratio of the wavelength of each line to that of the nearest Balmer line. The first ratio was 1.16, the second 1.16, the third .... 1.16!

Realizing that this was a redshift, I divided the wavelengths of the other two lines by 1.16 and found that they landed near those of the Mg II doublet at 2800 A and forbidden [O III] at 5007 A. Oke's line observed at 7600 A came close to the wavelength of H-alpha. Clearly, a redshift of 0.16 explained all the observed emission lines!

The extraordinary implications of a "star" of 13th magnitude having a redshift of 0.16 were immediately clear. When I told Jesse Greenstein what had happened, he produced a list of emission line wavelengths from a just completed manuscript about the spectrum of 3C 48. Within minutes, we had derived a redshift of 0.37 from the emission lines which mostly turned out to be forbidden lines. Most importantly, one of the emission lines turned out to be Mg II at 2800 A. This provided strong confirmation for the redshift of 3C 273, since the Mg II doublet had never been observed yet in an extragalactic object.

The results for 3C 273 and 3C 48 were published in four consecutive articles in Nature six weeks later (Hazard, Mackey, and Shimmins 1963; Schmidt 1963; Oke 1963; Greenstein and Matthews 1963).

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F. Kerr: There is a rather interesting story about one of those lunar occultations at Parkes that you were talking about. I forgot whether it was the August or October 1962 one, but it was in the first year of the operation of the 210-ft telescope. As you said, it was important to get both the immersion and emersion. The Parkes radio telescope will only go to 60 degrees in zenith angle. There's a pretty solid stop there. If you reach it, bells ring and the brakes are applied - hard! In case that doesn't work, there is a second stop a few minutes of arc later. The emersion could not be calculated very well but it was estimated to be 61½ degrees from the zenith. So the stops were of course removed from operation and people were posted all around the telescope to warn when extreme calamity might occur. I suspect Marc Price was actually one of those people. It shows what lengths radio astronomers will go to in order to get their data.

C. Wade: Frank, there's a little more to that. I believe John Bolton went up with a hacksaw and physically removed a member that appeared troublesome.

M. Price: It was only a ladder at the bottom and it was never bolted back!

DISCOVERY OF THE 3 K RADIATION[1]

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Princeton University

Perhaps all discoveries in sciences have some elements of a good mystery story. This one does. In 1964 the major cosmic puzzle was whether the universe is evolving (Big Bang) or Steady State; both ideas had merit, neither had proof. However, the Big Bang had left a deliberate clue - a shadowy remnant of its fiery youth. Three groups of physicists are on the case, each starting from a different premise, and unaware of the others. One group (in Russia) is putting together published theoretical and experimental evidence; they are very close, but misinterpret a clue. Another group, ignorant of the past, starts from the beginning and plods systematically toward the solution. The third group is looking, very carefully, right at the clue, wondering what it is. Thermal radiation from the Big Bang is about to be discovered. Proving again that nothing is new, the radiation had been predicted 15 years earlier, but not searched for. And at least two published observations - one 25 years old - gave strong evidence for the existence of the radiation prior to its discovery.

What follows is a worm's-eye view of the events leading to this discovery - a textbook example of a serendipitous discovery. We are not historians, and most of what we know about this comes from the scientific literature, personal experience, and randomly collected anecdotal stories. In no sense is this intended to be definitive history - just a story with some interesting parallels and contrasts to Jansky's wonderful discovery 50 years ago.

The first glimmer of the fireball came to us via the musings of R. H. Dicke - the head of our research group at Princeton. He reasoned roughly as follows. If the universe is closed and oscillating[2] (radius a periodic function of time), what happened to all the heavy elements which were cooked up in stars during the previous cycles of the universe? Most of the matter we see now is hydrogen, so somehow each cycle must destroy the heavy elements before the expansion phase of the next cycle. An attractive way to do this is to say that the matter temperature exceeds 10^{10} K in the highly contracted stage, causing the heavy nuclei to evaporate. A consequence of this assumption is that the universe tends to relax to thermal equilibrium, producing a sea of blackbody radiation.

As the universe expands out of this state, photons are red shifted from gamma rays to microwaves, but the spectrum remains thermal, so that we are left now with a sea of residual blackbody radiation. On the basis of Dicke's

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[1] Based on an unpublished manuscript written in 1968. It was dusted off by DTW for the Green Bank Workshop.

[2] One philosophically attractive option, among many.

tenuous (at best) argument, Peter G. Roll and Wilkinson began working on a radiometer to search for the Primeval Fireball -- as we dubbed it, and Peebles assumed the task of thinking about theoretical schemes of estimating the present Fireball temperature. This all took place in the late summer of 1964.

The instrument generally used to measure the intensity of microwave radiation is known (to radio astronomers) as the "Dicke radiometer." This device was invented by Dicke in 1946 (Ref. 1), and was used to measure the microwave radiation intensity from atmospheric water vapor, the moon, and the sun. Figure 1 shows Dicke and colleagues with their radiometer atop an M.I.T. building. This instrument, on a mountain top (to get above water vapor), could have detected the Fireball radiation, and, in fact, it was used (Ref. 2) in 1946 to set an upper limit of 20 K on the temperature of "radiation from cosmic matter." (By 1964, Dicke had forgotten about this result.)

Fig. 1. Early 1.5 cm microwave radiometer surrounded by (left to right) E. Beringer, R. Kyhl, A. Vane, and R. H. Dicke. Dicke is shaking the "shaggy dog" - a piece of absorbing material used as a calibration source.

While the Princeton radiometer was being built, Arno Penzias and Robert Wilson at the Bell Telephone Laboratories in Holmdel (only about 30 miles from Princeton), were trying to track down some excess noise in the front end of a 7 cm wavelength radiometer which they were using for absolute measurements of radio sources. Their instrument (whose antenna you see behind them in Figure 2) was designed as a low-noise receiving station for signals bounced from Echo satellites; consequently, its noise properties had been studied in detail

(Ref. 3), always with a measured excess which was assumed to be backlobe pickup of ground radiation.

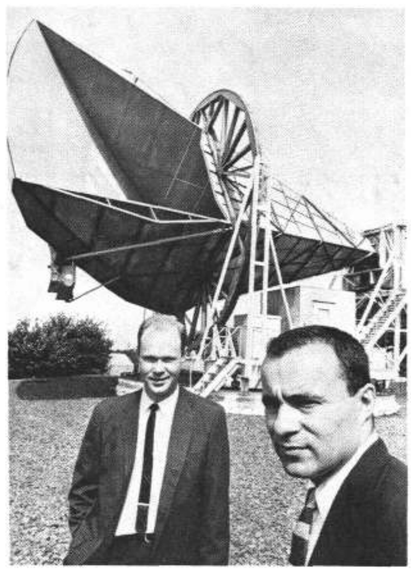

Fig. 2. Robert Wilson (left) and Arno Penzias soon after their discovery of the microwave background. Their radiometer is in the shack at the small end of the horn-reflector antenna - a giant-sized version of the ones commonly seen on microwave relay towers.

Penzias built a precision low-temperature calibration source (almost identical to one being built at Princeton) which he used to isolate this excess noise. He and Wilson decided that this noise had to be either anomalous radiation from the antenna walls, or leakage into the antenna of ground radiation (a roaring 300 K), or an isotropic cosmic background radiation. Their excess noise power was equivalent to thermal radiation with a temperature of a few degrees Kelvin. The plot thickens.

Peebles, in a lecture at the Applied Physics Laboratory, Johns Hopkins University (which enters the story again later) mentioned Dicke's idea about the fireball and also mentioned that the Princeton group was preparing to look for the predicted isotropic microwave background radiation. An old friend and peer in graduate school, Ken Turner (a physicist and radio astronomer at the Carnegie Institution, Department of Terrestrial Magnetism) was in the audience, and Peebles' remark stayed with him. Later he mentioned it to Bernie Burke (then at D.T.M., now at M.I.T.) who passed it along to Penzias via a telephone conversation. Through this devious route the Holmdel and Princeton groups came together and decided that the excess noise in the Holmdel radiometer was very likely the Fireball radiation. Hence, the "excess antenna temperature" (3.5 ± 1.0 K) was reported (Ref. 4) and interpreted (Ref. 5) as "Cosmic Blackbody Radiation." Bob Wilson tells this story from his perspective, elsewhere in this volume.

Of course, the Fireball interpretation needed to be tested by measurements of the spectrum (should be blackbody) and the isotropy (should be at least as isotropic as the matter distribution).

Fortunately, the Princeton apparatus had been designed for a different wavelength (3 cm) and when this work was completed the result (3.0 ± 0.5 K) agreed with the Holmdel result, thus supporting a blackbody spectrum. Jumping ahead for a moment we see in Figure 3 an up-to-date graph of the Fireball spectrum measurements (Ref. 6); the agreement between experiment and theory is

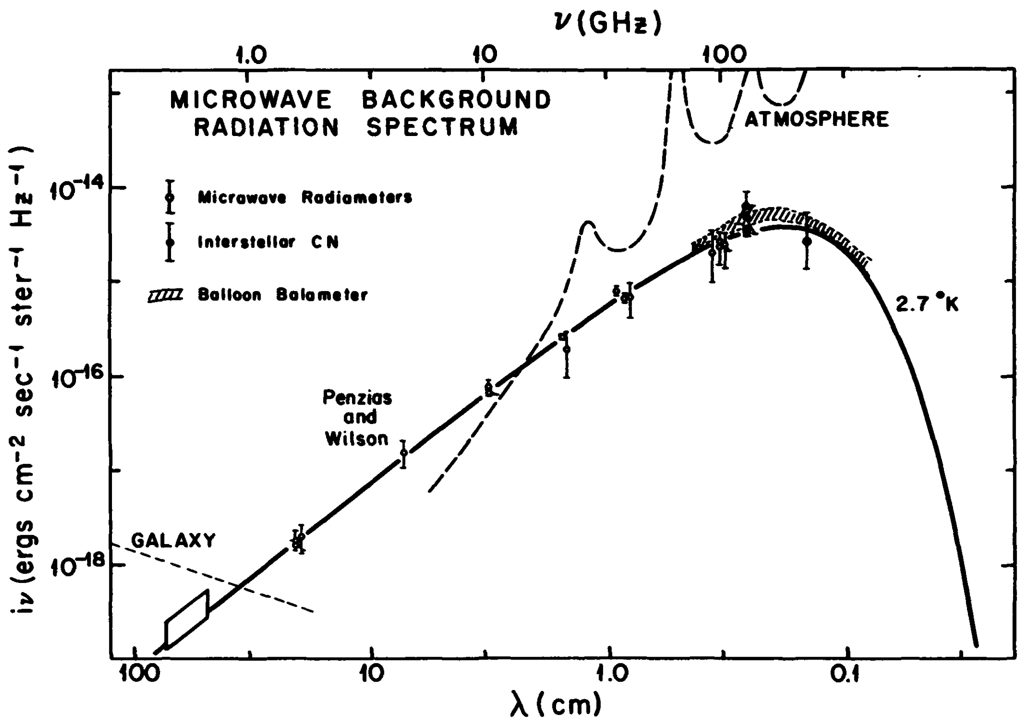

Fig. 3. Measurements of the intensity of the microwave background. Most measurements, by many groups around the world, are in reasonable agreement with the 2.7 K blackbody spectrum.

disgustingly good at long wavelengths. Only exceedingly difficult measurements (the shielded band) near the peak show interesting deviations. The isotropy of the radiation has now been checked, and the Fireball is featureless on angular scales from 1 arcminute to 90 degrees. Figure 4 shows the current results (Ref. 7). The limits around a few arcminutes are almost embarrassing. When the universe was about 10^6 years old, the matter started to form clumps, now seen as galaxies, galaxy clusters, etc. That process should have left bumps of magnitude $\Delta T/T \sim 10^{-4}$ to 10^{-5}, which haven't yet been seen. Incidentally, the dipole in Figure 4 is mostly due to the Sun's motion through the radiation, that is, with respect to the natural reference frame of the universe. This by itself is an interesting result. But we must resist these tempting digressions and get on with the story.

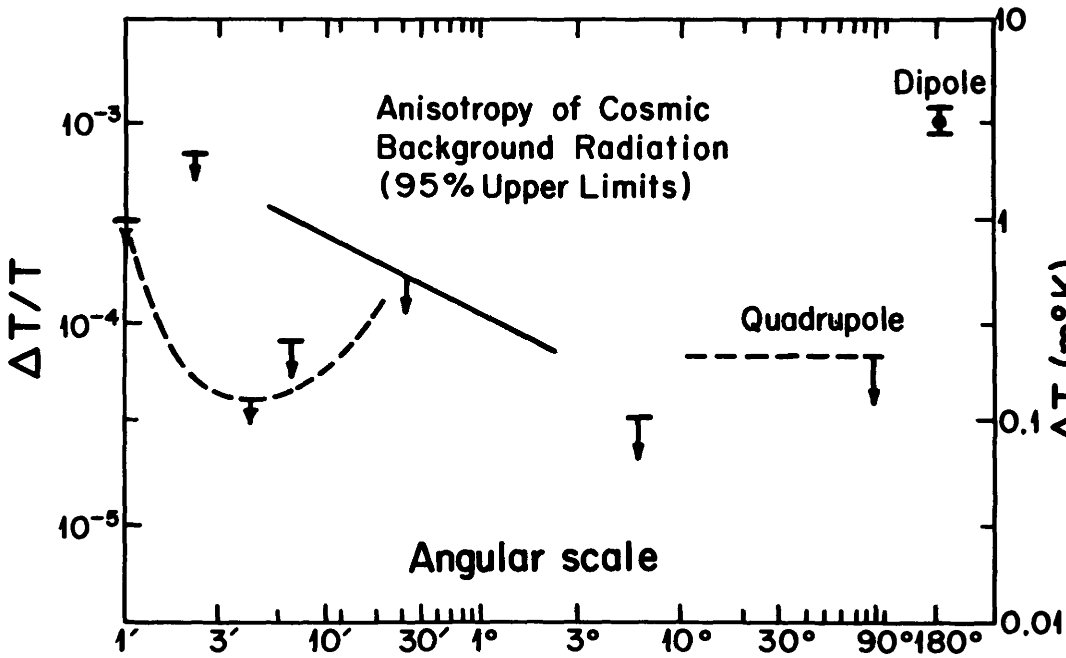

Fig. 4. Modern upper limits on spatial structure in the Fireball. The measurement at 4.5' was made at Green Bank, about 1 Km from the meeting site.

Flashback now to a part of the story, unknown to any of the three groups of physicists. In 1938, Mount Wilson astronomer, S. W. Adams, discovered in a stellar spectrum absorption lines due to interstellar cyanogen (CN) molecules. Later, Adams (Ref. 8) observed absorption from the first rotational excited state as well as the ground state of this molecule. Andrew McKellar (Ref. 9) used the relative intensities of the two absorption lines to obtain the relative populations of the ground and excited states, and hence the excitation temperature of the CN molecules. This temperature was 2.3 K. Although no process could be found to produce this excitation, it was thought that particle collisions were most likely responsible. By a fantastic stroke of luck, the first state of CN is excited by radiation of 2.6 mm wavelength, very close to the peak of a 3 K blackbody spectrum, so, if space is filled with 3 K radiation, all CN molecules must show excitations; the CN molecules are made-to-order interstellar thermometers and provide a strong existence test for the Fireball.

The connection between the Fireball proposition and McKellar's rotation temperature was made independently by N. J. Woolf and George B. Field, soon after they learned of the proposed microwave radiation. This led to new measurements of the interstellar CN excitation which gave results consistent with the radiometer measurements (see Fig. 3). The work of Patrick Thaddeus and John Clauser also indicates that the CN excitation is a universal phenomenon, as it was found in spectra of 8 different stars. This is expected for excitation by Fireball radiation, but not so easy to understand for the collisional excitation model.

The stage is now set for telling the rediscovery episode in this story. It's, of course, an old story in science; physicists, in particular, are known for reinventing the wheel. You will recall that Peebles was supposed to find some sort of theory for the present Fireball temperature. The scheme he hit upon was to relate two "observable" cosmological parameters, the present mean mass density and present radiation temperature, to the amount of helium produced in the early cooling Fireball. (This is evidently a relation of considerable interest if one can deduce the helium abundance before the stars are formed; that's an observable quantity.) We later found that this line of reasoning had been used 16 years earlier, by George Gamow and his doctoral student Ralph Alpher (who was then at the Applied Physics Laboratory, Johns Hopkins University). Gamow and Alpher were trying to cook up the heavy elements in a Big Bang stew, and they found that a necessary ingredient was the sea of blackbody radiation.

Dicke was unaware of this earlier work when he reinvented the Fireball. The story takes a curious twist - the Fireball radiation was independently predicted starting from exactly opposite premises. Gamow and Alpher were trying to make heavy elements in the Big Bang; Dicke was trying to get rid of heavy elements (from the supposed previous cycle). However, the cornerstone of both arguments is a hot Big Bang universe. And, indeed, the discovery and verification of the 3 K radiation has led most cosmologists to accept this picture. Again, working independently, and from opposite directions, Gamow, Alpher, and Herman, and later Peebles, managed to estimate a temperature for the fireball, before it was discovered. Let's go back and see how they did it (Ref. 10).

Gamow (Ref. 11) had pointed out, in 1946, the difficulties with the then popular thermal equilibrium (superstar) element cooker: among other things, general relativity cosmologies don't permit the assumed static highly compressed state of the universe. In fact, Gamow estimated that the universe would pass through the pressure cooker phase in something like one second, so that the equilibrium assumption is highly questionable, to say the least. He suggested that elements are built up during the expansion by coagulation of cold neutrons.

Gamow and Alpher then found that they could get a better fit to the element abundance data if they assumed that the elements were built up by the capture of hot neutrons in a radiation-dominated expanding universe. They were guided to this idea by newly published data on capture cross sections for hot ($\sim$ 1 Mev) neutrons, which they found showed an inverse correlation with element abundance. Their picture was that the elements were built up by successive neutron captures during the very early rapid expansion of the universe. The thermal radiation accompanying the hot neutrons dominated the expansion, and made the time scale consistent with the nuclear reaction rates.

As it happened, this later proved not to be the whole story because, as Alpher already foresaw in his thesis, the building up process gets hung up at helium by the gap at atomic mass 5. However, Gamow and Alpher's connection between neutron-capture cross section and element abundance is by now experimentally documented in considerable detail, and their neutron capture process figures prominently in the modern theory of element formation in stars. Some preliminary results were reported in the famous α- β-γ paper (Ref. 12) in 1948). But, for this story, the most important aspect of their work was the prediction that the early universe should contain thermal radiation.

In his characteristic way, Gamow (Ref. 13) reduced the problem to its essential physical parts. He concentrated on the first step in element synthesis, deuterium formation. First, Gamow knew that element formation would commence when the temperature fell to 10^9 K, for at higher temperatures the radiation photo-dissociates deuterium as fast as it forms. Second, because the mass density contributed by the radiation dominates that of the nucleons, he could use Stefan's law to get the mass density from the temperature, and then general relativity to get the expansion rate of the universe from the mass density. Knowing this rate, and the neutron-capture cross section of protons, Gamow could then observe that if the nucleon density were too low the nuclear reaction wouldn't happen with appreciable probability, and, on the other hand, if the density were too high, the reaction would go too fast and everything would end up as heavy elements, an equally sorry result. In this way he concluded that when the radiation temperature was 10^9 K, the nucleon density should have been about 10^{18} cm^{-3} and get a fireball temperature of 10 K. Figure 5 shows a picture of this remarkable physicist taken at about the time of the prediction of thermal radiation.

Fig. 5. George Gamow at about the time he and Alpher introduced thermal radiation into Big Bang cosmology. He is probably saying ".... when the universe was so big ...".

Alpher and Herman (Ref. 14) repeated Gamow's calculation with greater accuracy, using a computer. For the first time, they extrapolated the radiation temperature to the present, and got 5 K. Remember, all this happened in 1948 - 17 years before the radiation was discovered.

This story is getting complicated, so we have tried to summarize it on the flow diagram in Figure 6. The most surprising feature of this diagram is that the "Gamow" box makes no connection with the "Discovery" box. This is hard to explain. The Big Bang element production papers were widely read, and created quite a stir at the time of their publication. If there is any one reason for so many missed connections, it is probably this. In the early 50's evidence mounted that heavy elements are built up in stars, thus relieving the Big Bang of this burden. As Big Bang element production grew unfashionable, the thermal radiation idea faded with it. Even so, Gamow saw fit to write a review paper in 1956 which sets out very clearly the "Importance of Thermal Radiation in Cosmology" (Ref. 15).

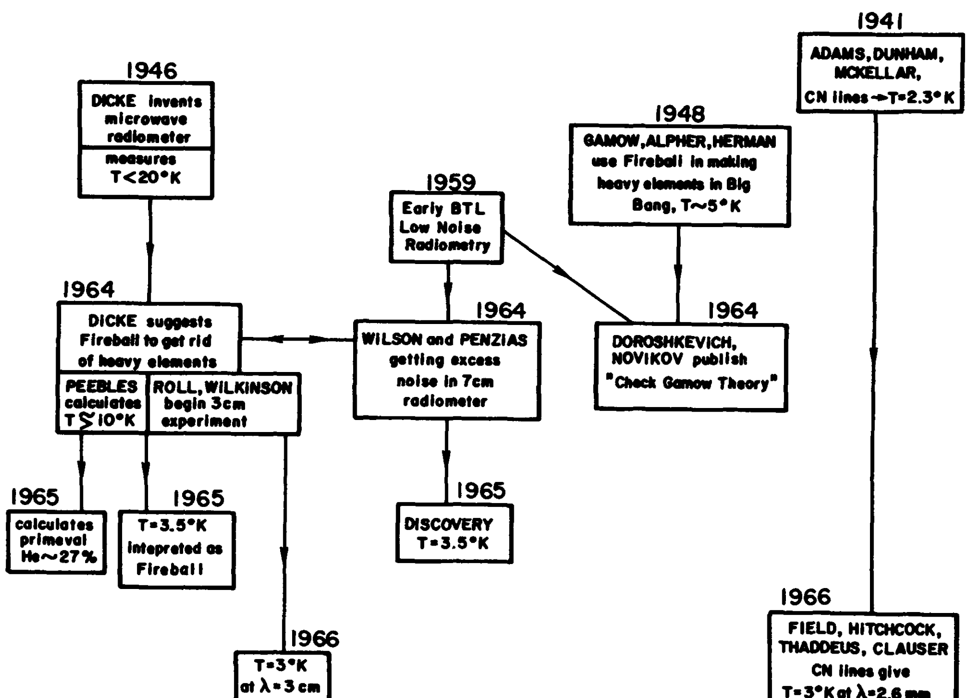

Fig. 6. Summary of the discovery of the Primeval Fireball, showing little cross fertilization. A vertical classification scheme might be (left to right) physicists, radio astronomers, theoretical physicists, and astronomers.

The one connection to the Gamow box was made (Ref. 16) by Russian astrophysicists, who apparently read the U.S. literature better than we do. They were the only ones to put together published theoretical and experimental results and suggest that microwave measurements "are extremely important for experimental checking of the Gamow theory." They refer to the early Bell Labs work (Ref. 17) where careful accounting was made of all contributions to the system noise (typically 20 K) with an accuracy of about ± 1 K. In retrospect the 3 K radiation was probably included in the 2 ± 1 K usually attributed to ground radiation into the antenna backlobes. "This estimate is based on the temperature 'not otherwise accounted for' in a previous experiment; it is somewhat larger than the calculated temperature expected from backlobes measured on a similar antenna" (Ref. 17). A radio astronomer, looking for evidence of a few degrees of Fireball radiation, would have leaped to her feet upon reading this because it is very difficult to distinguish between backlobe radiation and isotropic background radiation in the main beam. Both are approximately independent of antenna position. Doroshkevich and Novikov apparently missed this technical point, and only commented that the measured atmospheric emission agrees with theory.

We close with two remarks, which may contain a message about discovery processes. The observed excitation of interstellar CN was a quite well-known puzzle in astrophysics, yet the physicists and radio astronomers, wondering about Fireball radiation, missed it. Generally, astronomers know, and follow, the literature better than physicists do. We are impressed that at least two astronomers, learning of the Fireball idea, remembered a funny business in the absorption spectrum of the star ζ-Ophiuchi. There seems to be more coupling between physicists and radio astronomers, but the "Dicke" and "Gamow" boxes in Figure 6 were not connected prior to the discovery. Both groups published in the Physical Review, at a time when it was a relatively thin single volume. Still, the connection between a well-known instrument - the Dicke radiometer - and a prediction of the widely discussed "Gamow theory" waited for 15 years, and didn't contribute directly to the discovery.

K. Kellermann: Considering all of the laboratories and observatories in the United States, I wonder what Arp would conclude from the fact that Holmdel and Princeton are only 30 miles apart.

G. Burbidge: He would say they collaborated!

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DISCOVERY OF THE COSMIC MICROWAVE BACKGROUND

Robert W. Wilson
Bell Laboratories

In the late 1950's at Bell Labs, particularly at Holmdel, plans were made for Bell Labs to start working on communication satellites. The first satellite that was planned was the Echo balloon. It was known that on that balloon the return signal would be very weak because of course a balloon is not a very good reflector, especially while it is circular or spherical. And in contemplating the weak signal, they decided that one should have a very low noise receiver system to receive that signal. It was very convenient that there were two Bell Labs devices which would go together and ought to make a very low noise receiving system; one of them being the traveling wave maser which Derrick Scoville and his group at Murray Hill were making. This worked at liquid helium temperatures and noise temperatures of a few Kelvin, but even by the time you got to a room temperature connection to it, you probably have a ten Kelvin receiver.

The other device which seemed to fit with that is a horn reflector antenna. The horn-reflector was invented by Al Beck and Harald Friis, and I heard the story last night so let me tell you a little bit about how it happened. You know that one of the big interests at Bell Labs was microwave radio for long distance telephony. A microwave repeater consists of a tower with two antennas at the top connected by waveguide to the electronics in the base. If you want high gain antennas and you want to use horns, everyone knows that the horns have to be very long and expand at a low rate. So while it would be possible to put a horn at the top aimed horizontally, that would be quite awkward, and Harald Friis suggested putting the horn aimed up the tower and then a little reflector at the top so that the signal turns the corner. Al, after thinking about this awhile, decided that there must be some good curved surface that one could use which would help the situation a little bit, and he explained to me the algebra he went through to discover that it should be a second order surface and then it immediately dawned on him that it was a conic section and therefore parabola. Somewhat later it was realized that once you had done that, the horn might as well be short and expand rapidly. Thus the horn reflector was invented for communication purposes. It had the distinct advantage that when you put two of them back-to-back on a tower, and have a very weak signal coming in on one side, you can transmit a strong regenerated signal from the other side without interference; the front-to-back ratio of this thing was very high, and it allowed the repeater to work very well. The corollary of this is that if you take a horn-reflector and put it on its side, it will not pick up much radiation from the earth and will be a low noise antenna. Therefore, Art Crawford built a 20-foot horn reflector to be used with a traveling wave maser to receive the weak signals from Echo.

Figure 1 shows the parabolic section and the convenient cab that this has so that one could put almost any kind of receiver at the focus of this antenna and still be able to live with it. It is fairly obvious that this structure is very well shielded so that when it's looking up especially you wouldn't expect much radiation from the ground to be able to get in to the receiver which is shielded with metal.

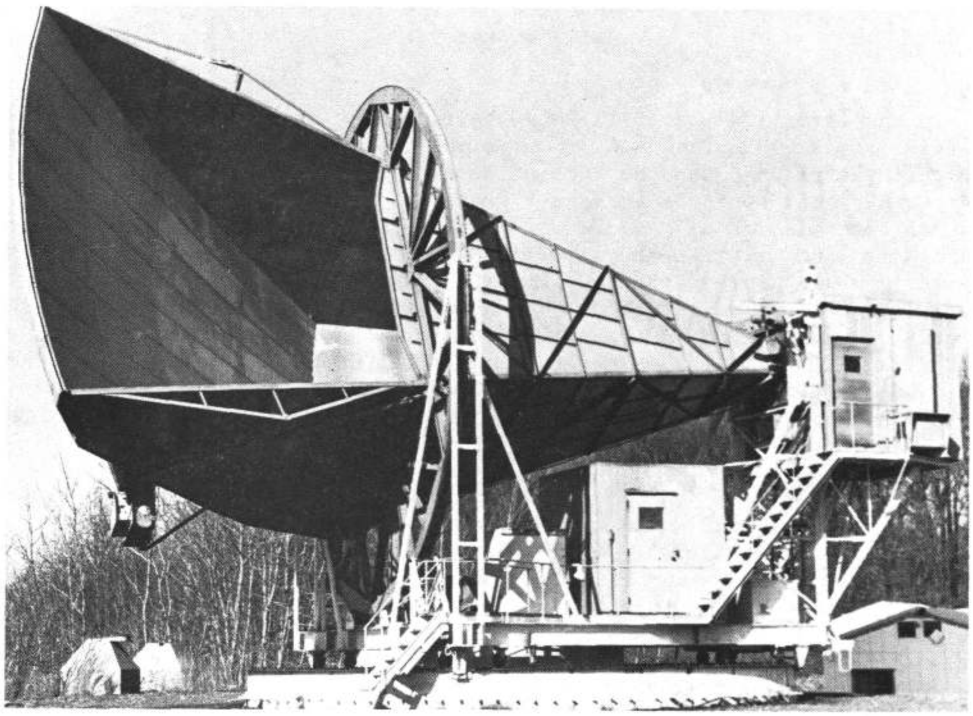

Fig. 1. 20-foot Echo horn reflector.

Just to convince the engineers and ex-engineers in the audience that one might reasonably expect this to be the case, Figure 2 shows a polar diagram of a rather smaller horn reflector antenna compared with the gain of a theoretical isotropic, uniform response antenna. If we take an isotropic antenna and put it on a field with the 300° ground down below and zero degree sky up above, we expect it to pick up 150°; half of its response comes from the ground. Now look at this horn reflector. It is certainly at least 35 db less responsive to the ground than the isotropic antenna would be. So we take 150° and divide it by a few thousand and we expect under a tenth of a Kelvin for the ground pickup from that antenna.

In 1963, knowing of the existence of this antenna, I accepted a job at Bell Labs, leaving a postdoc position at Caltech, and started working there. Arno had been there a year or so at that time, and obviously the only two radio astronomers in the place are going to get together and work together because making a radio telescope do any kind of an observation is enough of a job that you need at least two people. One might ask why two young astronomers were going to work with such a tiny little antenna, with a collecting area of maybe 25 square meters, when there were much bigger antennas around. We knew it had very special properties. One thing is that it's a small antenna and one could measure its gain very accurately. All you had to do was get maybe a kilometer away to be in the far field, and make an accurate gain measurement. And that, in fact, had already been started by Dave Hogg.

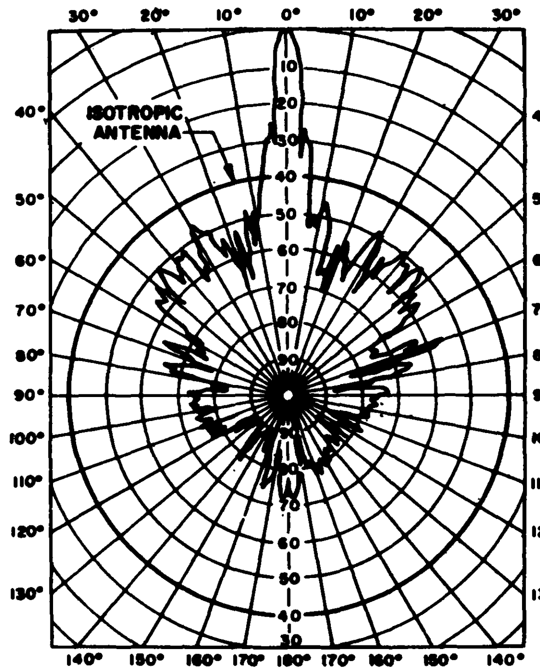

Fig. 2. Polar diagram of horn reflector antenna compared with isotropic radiator.

The availability of the traveling wave masers made this small antenna sensitive enough that you could work with small diameter sources, and if we had sources in the sky which were large diameter, it would be the most sensitive radio telescope in existence at the time. The other important thing about horn reflectors is what I've pointed out before, the good shielding of the antenna leads to the conclusion that we ought to be able to understand all the sources of noise. Radio astronomers don't often understand the background temperature when they do the usual experiment of pointing at a source and pointing away from a source. But the 20-foot horn reflector offered that as a possibility.

My interest in that possibility came directly from my thesis work at Caltech. There, with John Bolton, I had made a survey of the Milky Way. We had done it in the standard way of doing such things at the time; we pointed the antenna to the west of us of the Milky Way and let the earth's rotation steer the antenna beam through it and to the other side. Of course, we are inside the Galaxy, and there is no possibility of pointing completely away from the Galaxy to do the usual sort of on-off measurement. What we observed was a simple curve in which the power goes up as we approach the Milky Way and comes back down on the other side, and has a fairly steady level on either side. So I did the usual thing, I drew a linear baseline from one side to the other and worked above it. That was enough to see the radiation from the plane of the galaxy but it was clearly somewhat dissatisfying because I knew that there was a possible isotropic component, the halo that people were talking about, which my measurement wasn't at all sensitive to.

After I went to Bell Labs, the 20-foot horn reflector was actually released from the various satellite jobs it was doing. It had been designed for the Echo satellite which was at 2.3 GHz, but it had been used to receive a beacon from the Telstar satellite, and so when Arno and I inherited it there was a 7.3 cm maser on it. It had a communications receiver which a radio astronomer will find hard to believe. The maser was followed by a low noise nitrogen cooled parametric amplifier which was followed by a low noise traveling wave tube amplifier, and the gain stability was just unbelievably bad. So our job was to turn this thing into a radio telescope by making a radiometer, finish up the gain measurement, and then proceed to do some astronomy projects.

Well, we thought some about what we ought to do and laid out a plan, to take a few years. The first project was the absolute flux measurement. If we could know the gain of the antenna to a few percent, which we thought we could, then we could measure the standard calibration sources. We also had to understand the temperature scale of the receiver very well, we thought we could do that also. I planned to follow up on my thesis a little bit by taking a few selected cuts across the Milky Way and then confirm the spectrum of some of the sources that I had looked at.

We wanted to check the ability to measure the galactic halo radiation. At 7 cm, extrapolating from a lower frequency, one didn't expect to see any galactic halo at all, and therefore it would be good for us to prove that when we did try to make such a measurement we got a null result. After doing these projects, our plan was to build a 21 cm receiver scaled from our 7 cm receiver. We already had the maser in hand. We would then make the halo measurement and do a number of 21 cm line projects including reworking Arno's thesis of looking for hydrogen in clusters of galaxies.

At one point during that time John Bolton came for a visit and we laid out this plan of attack in front of him and asked his opinion, and he said, "Well, obviously the most important thing in that list is to do the 21 cm background measurement." He thought that it was an unexplored area and something that we really ought to do.

Actually by the time I got there, Arno had started making a cold load. He was doing it for the purpose of understanding the receiver's temperature scale, and for that purpose he really overdid the job! In Figure 3 we see it; a piece of ordinary C-band waveguide, 90% copper, which runs down inside the dewar which has a six-inch inside diameter. About halfway down, the waveguide is thinned a little bit, and finally there is a carefully designed absorber in the bottom. There's a sheet of mylar in the flange near the bottom which keeps the liquid helium out of the upper part of the waveguide. There are some holes in the bottom section so that the liquid helium can surround the absorber and there will be no question of the physical temperature of the absorber. The cryogenics has been taken care of with the baffles which make a heat exchange between the cold Helium gas and the waveguide. We realized that we had to know the radiation from the walls of the waveguide, so there are a series of diode thermometers on the waveguide for measuring its physical temperature distribution. We calculated the radiation of the walls using these temperatures and the measured loss in the waveguide.

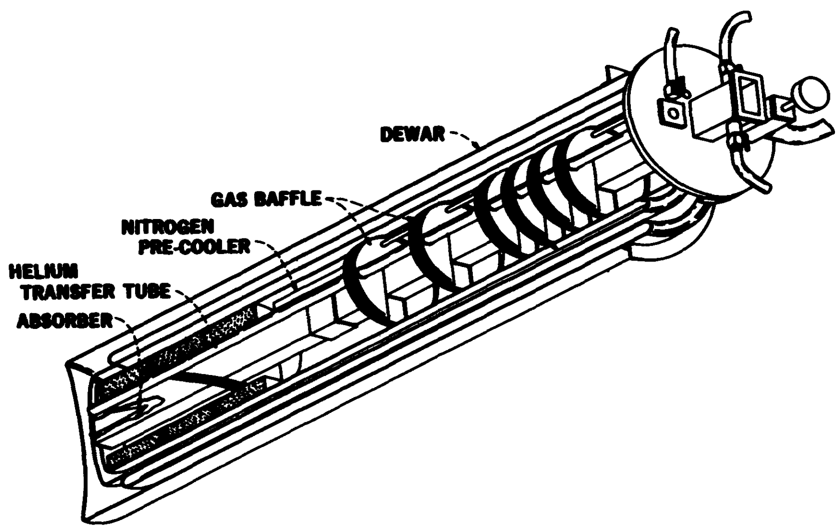

Fig. 3. Cold load.

When we first emptied a 25-liter dewar of liquid helium into this thing, it would fill up to a fairly high level and we calculated the radiation temperature at the top to be approximately 5 Kelvin - just eight tenths of a Kelvin above the temperature of the liquid helium. Fifteen hours later or so (we usually ran down before the helium did), the liquid helium level would be down near the absorber and we would calculate the flange temperature to be maybe 6°. Comparing it to the horn reflector, the change agreed within something like a tenth of a degree over that period, so we felt we had a reasonably good calibration of what was going on in our cold load.

While Arno was doing that, I set up the radiometer shown in Figure 4. As with most things at Bell Labs, this is somewhat unusual. The horn reflector had a circular waveguide at its output, and so we decided to use that property in a scheme which Doug Ring and others at Crawford Hill had used in the past. One takes advantage of the fact that two orthogonal polarizations will pass through round waveguide. The polarization coupler near the antenna couples signal from the cold load into vertical polarization traveling toward the maser and allows horizontal polarization from the antenna to go straight through. The polarization rotator is the equivalent of a half-wave plate, it's just a squeezed piece of waveguide with two rotary joints; another polarization coupler at the back picks one polarization off and sends it over to the maser. By rotating the squeezed waveguide, we could switch between the reference noise and the antenna. The noise tube was a secondary temperature standard in our measurement of Cas A and other sources. Figure 5 shows a picture of the actual installation including the rotary joint which allowed the horn reflector to turn while the receiver stayed nice and stationary in the cab. The cold load was connected to the reference port through an adjustable 0.11 db attenuator which could add a well-calibrated additional noise. You can see that the availability of a cab which moves only in azimuth was very important for this experiment.

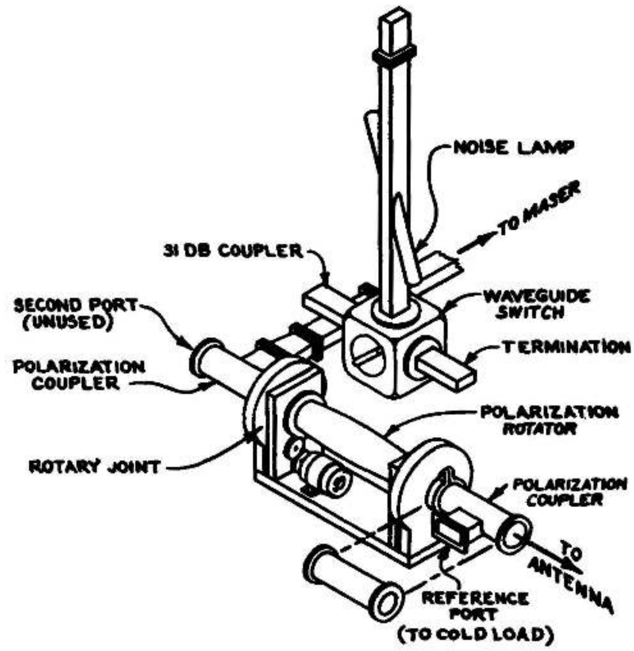

Fig. 4. Radiometer used with horn reflector.

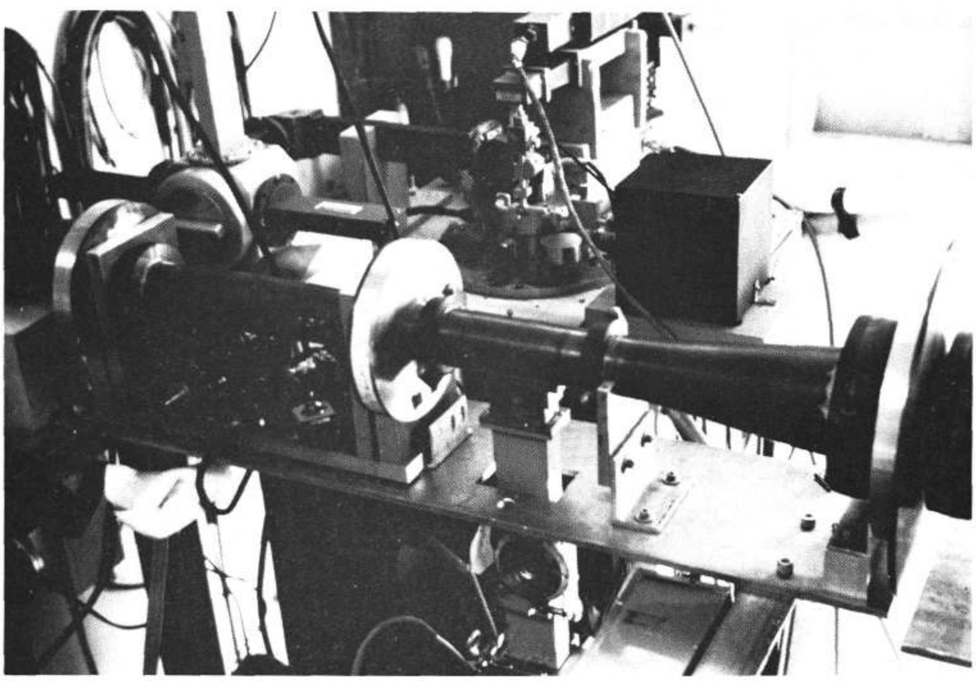

Fig. 5. Rotary Joint.

Before we started making measurements with this system, there had been previous measurements at Bell Labs. First of all before going to the trouble of building a 20-foot horn reflector, the antenna and maser groups had put together a test system. They had a 6 GHz maser and a small horn reflector antenna. They hooked the two up with a calibrating noise lamp and saw that indeed they got a system temperature of something like 18½° K which was very nice, but they had expected to do a little better.

You see in Figure 6 that contrary to the prediction I made before, they have assigned 2 Kelvin to the antenna for the back lobe and other pickup from the antenna, 2½ K for atmosphere, and 10.5 K for the temperature of the maser. The makers of the maser were not very happy with that. They thought they had made a better maser than that, but within the accuracy of what they knew about all the components, they solved the problem in making things add up by assigning additional noise to those components.

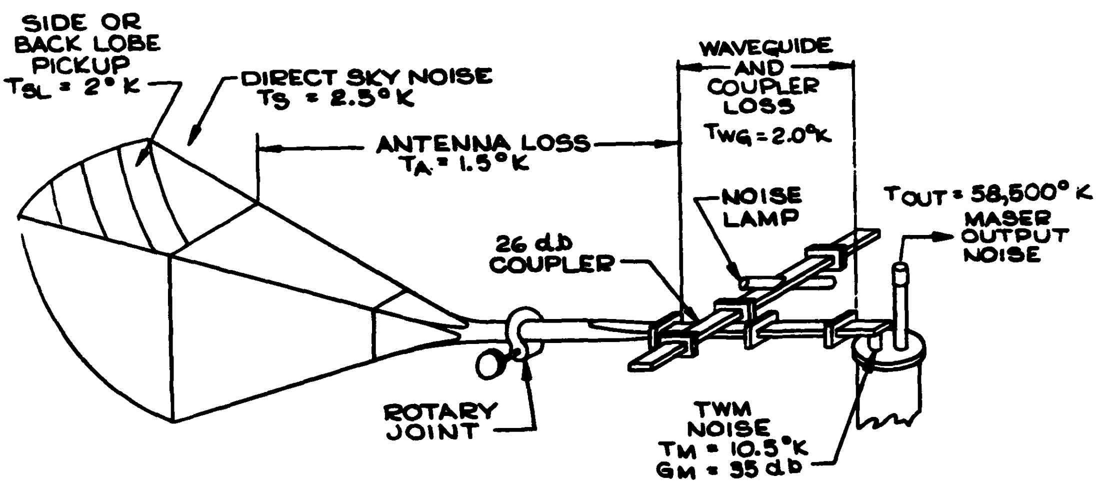

Fig. 6. Assigned noise contributions to various components of 6 GHz system.

They had measured the sky noise by the same technique that Dicke had first reported on in 1946. Figure 7 shows a chart of such a measurement made with the 20-foot horn reflector. It shows the radiometer output as the antenna is scanned from a fairly high elevation angle down to 10° elevation angle. This is a chart with power increasing in to the right, and you can see what the power out of the receiver did. The circles correspond to 2.1 Kelvin for the temperature of the sky and the crosses to 2.3. You can see that the curve is a very good fit to the expected curve down to at least 10° elevation, so a well shielded antenna makes the measurement of the atmospheric radiation very easy to do.

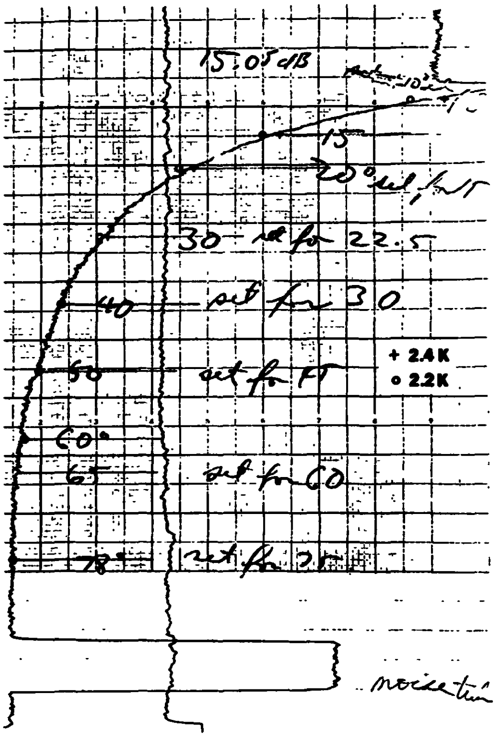

Fig. 7. Radiometer output vs elevation angle.

Source	Temperature
Sky (at zenith)	2.30 ± 0.20°K
Horn antenna	2.00 ± 1.00°K
Waveguide (counter-clockwise channel)	7.00 ± 0.65°K
Maser assembly	7.00 ± 1.00°K
Converter	0.60 ± 0.15°K
Predicted total system temperature	18.90 ± 3.00°K

the temperature was found to vary a few degrees from day to day, but the lowest temperature was consistently 22.2 ± 2.2°K. By realistically assuming that all sources were then contributing their fair share (as is also tacitly assumed in Table II) it is possible to improve the over-all accuracy. The actual system temperature must be in the overlap region of the measured results and the total results of Table II, namely between 20 and 21.9°K. The most likely minimum system temperature was therefore

$$T_{system} = 21 \pm 1°K.*$$

The inference from this result is that the "+" temperature possibilities of Table II must predominate.

Fig. 8. Early comparison by Ohm of predicted and measured system temperatures.

After the 20-foot horn was built and was being used with the Echo satellite, Ed Ohm, who was a very careful experimenter, added up all the components of the system and compared it to his measured total. In Figure 8 he predicted a total system temperature of 18.9 Kelvin, but he found that he consistently measured 22.2, or 3.3 Kelvin more than what he had expected. However, that was within the measurement errors of his summation, so he didn't take it to be significant.

Well, our first observations were somewhat of a disappointment because we had naturally hoped that these things I told you about were just errors in the experiments. Figure 9 is the first measurement with our receiver. At the bottom and top, the receiver is switched to the antenna and in between to the cold load. The level from the antenna at 40° elevation matched that from the cold load with .04 db of attenuation (∿ 7.5 K total radiation temperature). At the bottom I recorded measurements of the temperature sensing diodes on the cold load.

Well, that was a direct confrontation. We expected 2.3 Kelvin from the sky, 1 Kelvin maybe from the absorption in the walls of the antenna, and we saw something that was obviously considerably more than that. It was really a qualitative thing because the antenna was hotter than the helium and it should have been colder. But we knew that the problem was either in the antenna or beyond. Arno's initial reaction was, "Well, I made a pretty good cold load!" The most likely problem in such an experiment is that you don't understand all the sources of extra radiation in your reference noise source, but it is not possible to make it have a lower temperature than the liquid helium.

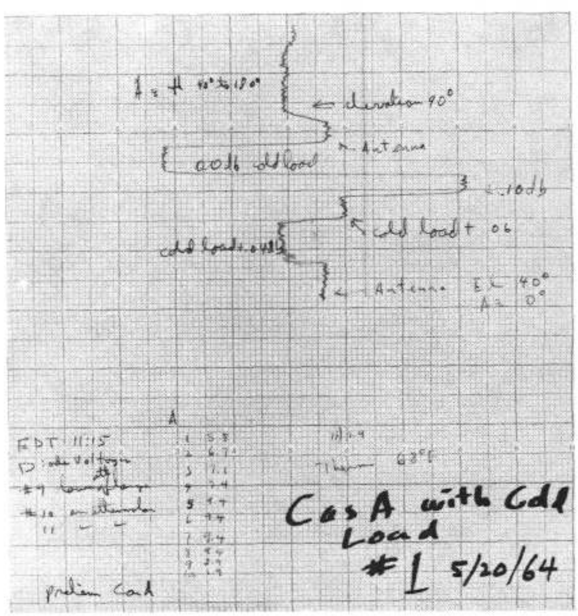

Fig. 9. Comparison of sky and cold load temperature.

Well, it did initially look like we couldn't do the halo experiment, but at that point our measurements of the gain of the antenna had started and we wanted to go on with the absolute flux measurements before really taking anything apart or trying to change anything. So we ended up waiting for some time before doing anything about our antenna temperature problem, but we were thinking about it all that time.

There were several reasons we came up with. Many radio astronomers at the time thought the centimeter wave atmospheric radiation was about twice what we were saying. That would have gone a long way toward explaining our problem. However, the curve I showed you for the zenith angle dependence would convince you that we were measuring the atmospheric absorption or emission correctly. It turned out later that the centimeter astronomers had applied refraction corrections to their measurements of radio sources in the wrong sense. John Shakeshaft finally straightened this out.

Crawford Hill overlooks New York City, it is not a Green Bank sort of site, maybe man-made interference was causing trouble; we turned our antenna down and scanned the horizons, scanned below the horizon, above the horizon, and we found a little bit of superthermal radiation but nothing that would explain the sort of thing that we were seeing.

Maybe it was the Milky Way. If one extrapolates from low frequencies though, you wouldn't expect that; it should be very, very small, and the actual plane of the Milky Way fit very well with what we would expect from low frequency extrapolations. Maybe it was discrete sources. The strongest discrete source we could see was Cas A and it was 7°. Point sources extrapolate in frequency in the same way more or less as the radiation from the galaxy, so they seemed a very unlikely explanation. That left radiation from the walls of the antenna. We calculated nine-tenths of a degree Kelvin for that, taking into account the actual construction of the throat section of the antenna which is the most important. It was a piece of electroformed copper and we could measure similar sorts of waveguides in the lab. As most of you probably know, there were a couple of pigeons living in the antenna at the time, and they had deposited the usual pigeon droppings. We cleaned those out and disposed of the pigeons, and that only made a minor improvement. We had to wait sometime to finish the flux measurement, but in the spring of 1965, almost a year later, we had completed it. The earth had made a complete cycle around the sun and nothing had changed in what we were measuring, we pointed to many different parts of the sky and unless we really had a known source or the plane of the Galaxy in our beam, we had never seen anything other than the usual antenna temperature.

In 1962 there had been a high altitude nuclear explosion over the Pacific which had filled up the van Allen belts. We were initially worried that something strange was going on there, but after a year had gone by the population of van Allen belts had gone down considerably, and we'd never seen any change. So we cleaned up the antenna, put some aluminum tape over the joints between the separate pieces of aluminum that made it up, and no change. We were really scratching our heads about what to do until one day Arno happened to be talking to Bernie Burke about other matters and after they had finished talking about what Arno had called for, he mentioned our problems; our dilemma that this experiment was not ever going to work, and that we couldn't understand what was going on. Bernie had heard about Peebles' calculations and suggested that we ought to get in touch with Dicke's group. So of course, Arno called Dicke and you've already heard the other side of the story. Dicke was thinking about oscillating big-bangs and after a little bit of discussion on the problem, they sent us a preprint and agreed to come out for a visit. They came for a visit, looked at what we had done, and I'm sure were thoroughly disappointed, but agreed that what we had done was probably right. And so, of course, we reported the results.

We made one last check before actually sending off our letter for publication. We took a signal generator, attached it to a horn and took it around the top of Crawford Hill to artificially increase the temperature of the ground and measure the back lobe level of the 20-foot horn, maybe there was something wrong with it. But the result there was null. So we sent the letter in!

Arno and I of course were very happy to have some sort of an answer to our dilemma. Any kind of an explanation would have probably made us happy. In fact, I don't think either of us took the cosmology very seriously at first. We had been used to the idea of steady state cosmology; I had come from Caltech where Fred Hoyle's influence was very strong. Philosophically, I liked it. So I thought that we should report our result as a simple measurement; the measurement might be true after the cosmology was no longer true!

Sometime between our submission of our results to the Ap. J. Letters and their actually appearing, Walter Sullivan got wind of it and put a picture on the front page of the New York Times with a nice article. The fact that he took the cosmology seriously influenced me!

We have talked about the reactions of Bell Labs management to Jansky's discovery, and there are some similarities that Arno and I observed. In 1966, Arno and I were working for Art Crawford. He was our department head, and there was a fellow named Roy Tillotson who was his boss. In 1966 Roy Tillotson came in and in effect said, "You guys have been doing radio astronomy full time; the effort is supposed to be sort of half-time, let's get on with something for the telephone company." He suggested an atmospheric propagation measurement involving setting up a 10 micron laser and a receiver a couple of miles away, and in the end we did that.

At that time my analysis of the situation was that as we looked up the management chain, Art Crawford was very much in favor of what we were doing; he had built the 20-foot horn reflector and he enjoyed its being used for scientific purposes. Roy Tillotson was very much interested in making transmission measurements for the telephone company. Rudi Komptner, who was his boss, also enjoyed the science but understood the telephone company had needs too. And John Pierce, whom most people think of as being a big friend of astronomy, was moderately neutral. He told me at lunch one day that the stars were put in the sky for man to discover gravity, and we've already done that.

Arno and I had a great advantage over Karl Jansky. The astronomical community immediately took to this result as Dave Wilkinson indicated, and there were lots of jobs for lots of people, and papers coming from all directions. Later we found other applications for Bell Labs technology in astronomy and we have managed to stay in astronomy at Bell Labs over the years.

INTRODUCTION TO THE PANEL DISCUSSION ON THE METHODOLOGY OF SCIENTIFIC RESEARCH

R. D. Ekers
National Radio Astronomy Observatory

We have been listening to talks on the most significant discoveries in radio astronomy; can we do more than just reminisce about them? Can we learn anything from the history of radio astronomy in order to use major new facilities, such as the VLA, to make such major discoveries? Despite the concluding comments of a surprisingly large number of speakers, one thing seems clear: you can't predict what the next discovery will be. It is no use trying to enumerate what kind of things you are going to find next. The fact that essentially all these discoveries were serendipitous shows that this is just not the way the subject develops. However, this does not mean that we should not think about *how* to go about making major discoveries, rather than *what* they will be. Were there underlying conditions which had to be met in order for the discoveries we have been hearing about to be made? If so, we can ask a question which seems especially relevant for a national observatory, "Are we providing the right environment for the kind of discoveries we have been hearing about in the last two days to occur in the future?"

What I have learned from listening to these discussions, and it has been especially noticeable because they have all been presented here together, is that all these discoveries were by-products of serious scientific investigations done by competent and well-motivated observers. These observers did not find what they were looking for, but nevertheless they were engaged in serious programs. One criticism of the peer review system for allocation of telescope time is that the observer with the crazy idea does not get time to test it. But so far there is no evidence that the observers with the crazy ideas make the discoveries! These discoveries have been made in programs which would have been acceptable to the kind of peer review system we now have. Hence it seems to me that it is not so much what observations you do but how well you can do them; that is one necessary condition for making discoveries.

It was with these kinds of questions in mind that we collected people who, willingly or unwillingly, offered to talk about the methodology of doing astronomy rather than the astronomy itself. We have put these people together in this panel. The reason for the panel is just to help provoke arguments and stimulate discussion. We are not going to come up with a grand conclusion on the right way to do science. The objective is to present ideas for discussion.

OBSERVATIONAL DISCOVERY VS THEORETICAL DISCOVERY

Martin O. Harwit
National Air and Space Museum, Smithsonian Institution --
on leave of absence from Cornell University

The talk I said I would like to give deals with discoveries versus understanding in astronomy. It will have two parts to it. The first one originated perhaps with a talk I gave in Groningen in the spring of 1977. I presented a discussion on limitations on observations that occur because of the structure of the universe, absorption in interstellar space because of limiting physical conditions -- as well as other, fundamental physical factors. As a concluding remark, I stated that among the techniques that were possible, those that had shown themselves most fruitful in making major discoveries generally involved new instrumentation. In addition, an unusually large fraction of discoveries was serendipitous. I pointed to many discoveries that were discussed here in the last two days, and thought that was a fitting conclusion to the talk.

Well, the audience got very upset. It was one of the two times, I can recall, that people literally started screaming in discussions afterwards. Many of them felt discoveries come about through theoretical predictions; those predictions they considered are taken seriously by the observers; the observers go and make measurements and then come back and report to the theorists, who then tell them what to do next.

In Holland this is probably understandable since one of the few such cases, namely the 21-cm prediction, did work out that way. But it showed me one thing which I think is pretty important to realize when one listens to the types of presentation that we have had this morning. There are many different sides to all of these discoveries and if you want to make any kind of a statement that is generalizing, it will probably be very modest in the first place, and in the second place, it had better be based on a complete set; that is, you shouldn't just take those discoveries that you happen to have personally seen or come to know; you should try, in some way or other, to assemble a set that is all-inclusive, or representative at least, so that whatever statistical conclusions you derive have a chance of being acceptable. I'll talk about that aspect of things first and the kind of conclusions that one finds.

Then I'll turn to the second question that people always ask when you tell them you've just done a study on observational discoveries; they say, "Well, OK, you've described all these discoveries and many of them are serendipitous, but what does all that have to do with understanding in astronomy? We don't do all this just for the hoop-la, we do it because we want to find out something about the structure of the universe, how things work, what the mechanics of all this is, and if you tell me that this or that strategy is great for coming up with discoveries, that doesn't really interest me; the end of all this ought to be a better understanding of the universe." More recently (I'm on a leave of absence from Cornell now; the Smithsonian Institution invited me down to the National Air and Space Museum where there are several historians of astronomy), I'm trying to see if I can understand how understanding of the universe tends to increase. I think that's a much more difficult problem, I certainly haven't come up with any particularly sage conclusions, but let me show you on a prefatory basis, at least, how the theoretical advances seem to contrast with the way that the type of advance and discoveries that you are talking about in the last couple of days come about.

In order to talk about the observational advances, let me first show you the set of discoveries that I picked (Fig. 1). What I plotted here as an

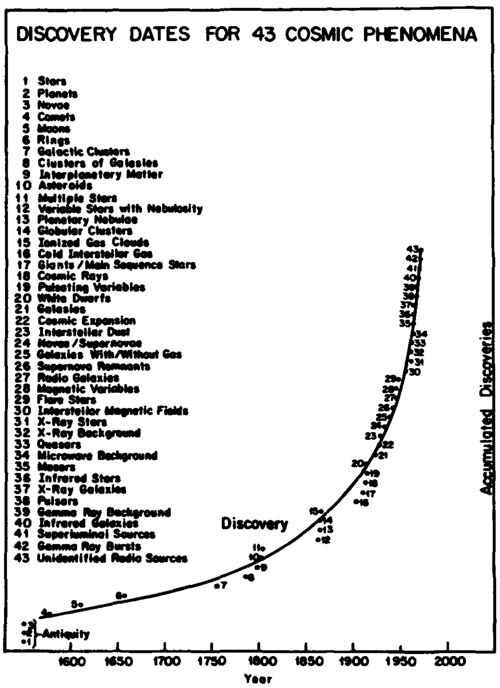

Fig. 1. A listing of observational discoveries and dates of discovery for 43 cosmic phenomena, showing their accumulation as a function of time. (After Reference 1.)

ordinate is the accumulation of discoveries with time from the period in which Tycho Brahe was working up to 1979 when I stopped working on the book Cosmic Discovery (Ref. 1). This is a listing of the discovered phenomena. I'm sure that nobody in the audience would pick exactly the same list -- it's compiled from symposium headings, chapter headings in elementary texts, table headings in Allen's Astrophysical Quantities, and so forth; but I feel fairly safe in this audience, because everything that has been discussed here at the Symposium is on that list -- with the exception of the sun. I felt that for solar observations, the technological problems are vastly different from everything else that we do because of the immensely more powerful fluxes that we receive. You see a very steep rise in discoveries, and I'm talking here about a large number of different types of phenomena dealing with gamma ray bursts, microwave background, quasars, interstellar material, white dwarfs, and so forth. What I did in Cosmic Discovery was to go through a history of each of these discoveries. One finds then, as several people here have emphasized, that many of the discoveries come about through innovative techniques.

Before I go on to describe the conclusions one reaches, let me talk about the techniques and some of the limitations that in principle seem to put constraints on what we can do. In Figure 2, I plot the angular resolution that we are capable of achieving. The wavelength is given in logarithms of centimeters here. At the extreme gamma end of the range, there's a limiting

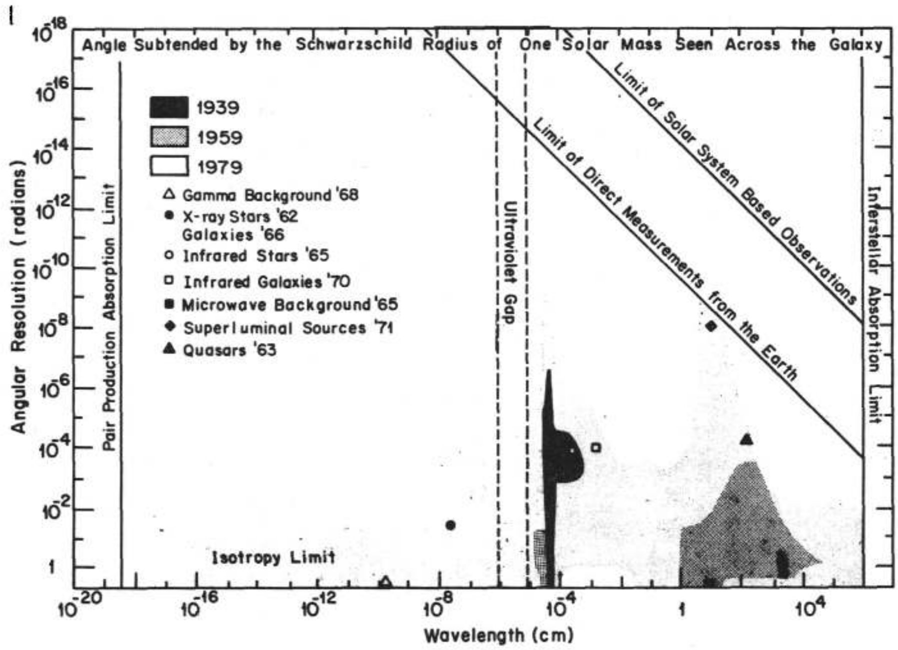

Fig. 2. Angular resolution available to observers, at different wavelengths, in different years. Dates of discovery of different phenomena made possible with the newly developed observing capabilities are also shown, and the specific capabilities required are indicated in the diagram by the locations of the respective symbols. See the text for a more detailed description. (After Reference 1.)

wavelength. Extremely high energy gamma rays collide in empty space with the microwave background radiation, forming electron positron pairs, and getting destroyed in that fashion (never reaching us here on earth). At the other extreme you have a cutoff, at the plasma frequency of interstellar space. This would be different if we were, say, living in an elliptical galaxy. The right border line then would be moved further over to the right, but in our galaxy this is approximately the border beyond which we aren't going to be able to make radio observations, in fact you don't even see to the distance of the nearer stars beyond this border.

Shown in shading are the instrumental capabilities that we had in 1939, 1959, and 1979. You see that in 1939 we had some optical capability; a little near infrared work had been done by Coblentz and a few other people, and then there's the radio work of Jansky registered over near the bottom edge on the right.

You notice I've put a number of different phenomena in here, discoveries that I marked with different symbols. There is the microwave background radiation discovery that was discussed this morning by Dave Wilkinson and Bob Wilson. Then also, with angular resolution not available in 1959, but in existence very shortly thereafter as discussed this morning by Hanbury Brown, you get the identification of the quasars whose positions then were better determined by Cyril Hazard and others, as discussed by Maarten Schmidt. Of course Jesse Greenstein was involved in this work also in the spectroscopic work in the visible. Again, there is the work that Alan Moffet, and then later on also Marshall Cohen and of course Irwin Shapiro, did on superluminal sources. These discoveries are all registered here, on the plot, together with optical, gamma ray and X-ray observations. I show a line that limits VLB work with a baseline approximately the diameter of the earth. Another limit is given for baselines equal to the diameter of the solar system.

Now what you see in Figure 2 is effectively an expanding wave of instrumental capabilities in the wake of whose front you see these discoveries crystalizing out. You can draw up a number of different charts of this kind which deal with not just the angular resolution but also time resolution, for example; you then have a similar set of borders on the left and on the right and you see, for instance, the discovery of pulsars that Jocelyn Bell Burnell mentioned this morning, becoming possible with techniques that we were able to employ sometime after 1959. Marshall Cohen had done somewhat similar modulation frequency work at Arecibo in the mid-1960's, but, as luck had it, he says they never saw a pulsar, though they had the time constants that would have made it possible. The discovery of radio galaxies by Hey and his co-workers involved observing an undulating signal as well.

Finally, I could show one more of these plots which deals with spectral resolution where we have a similar situation. Work that involved high spectral resolution led to the discovery of the masers - Sandy Weinreb was involved in that and Alan Barrett and a number of other people. Again, that discovery was made in the mid-1960's with a technique which in 1959 was just barely possible.

One could look at a larger number of these discoveries. Obviously, one is somewhat daunted talking to an audience like this because almost every one of the discoveries that I would be likely to talk about would have been made by one of you people sitting here -- Fred Haddock and the NRL people with the ionized nebulae that we talked about yesterday, for one. If you have any comments or if you think I'm presenting any of these events in a prejudiced way, please let me know.

What you find if you look at all of these together is (perhaps not a surprising thing as far as this audience is concerned), mainly that the discovery of a new phenomenon very quickly follows the introduction of new instrumentation which was needed or was required to make that discovery. That statement doesn't sound particularly deep because after all you're not going to make a discovery unless you have the instrumentation that will allow you to make it. That is only tautological. But what isn't tautological is the surprising speed with which the discoveries are made once the instrumentation is available. I've plotted in Figure 3 the age of the technology at the time

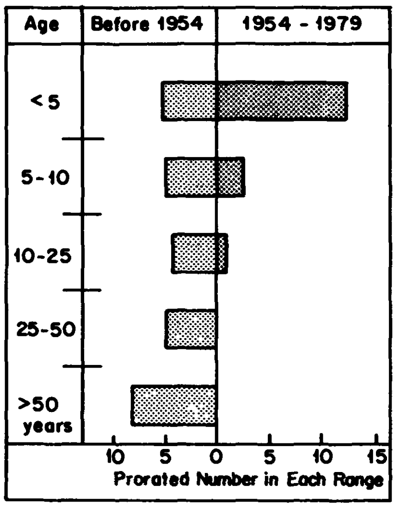

Fig. 3. Time interval between the introduction of a new technical capability into astronomy, and the discovery of a phenomenon that required this capability. Discoveries made before 1954 are shown on the left, those made between 1954 and 1979 are given on the right. We see that a large fraction of the discoveries are made with novel instrumentation less than five years old. (After Reference 2.)

a discovery was made, that is, the age after the introduction into astronomy of the required technique. You can see that by far the largest number of discoveries are made with equipment that is less than 5 years old. When I say equipment less than 5 years old, I don't mean that somebody has just added a factor of 20% or 30% to existing instrumental capabilities. What you saw in Figure 2 was a front of instrumental capabilities spreading out, eating up the decades extremely rapidly. One finds that one needs factors of 10 or 100 or a thousand in instrumental advances before one recognizes an entirely new phenomenon which previously was not known. There are relatively few discoveries that are made in successive intervals -- between five and ten years, ten and twenty-five, or larger. Notice that when split up between discoveries made before 1954 and those made between '54 and '79, the more recent discoveries did not make use of instrumentation that was older than 25 years. So one really is dependent on novel instrumentation for the modern discoveries -- gamma ray telescopes, and so forth.

Yesterday Jesse Greenstein was saying we are rapidly running out of wavelengths. What plots such as Figure 2 suggest is that new wavelengths and expanded resolving power are equally important. That is, we're not without hope of producing novel instruments, for further major discoveries. Increases in time resolution, spectral resolution, angular resolution will still surprise us many times with newly discovered phenomena.

A corollary which is somewhat sad of course is that once a novel capability has been introduced into astronomy, whatever discoveries are to be made follow rather swiftly; thereafter the instrument no longer is useful for making these discoveries. If it were, there would be many more discoveries registered in Figure 3 with instrumentation twenty five to fifty years old. But of course that doesn't mean that the instrument is no good, because I'm just talking about discoveries here. For analytical work, many of these older instruments are exactly what you want to have. So, if you have an urge to construct a plan, or if the National Academy decides there ought to be a ten-year plan, then obviously you have to take a balanced view and try to see not only what discoveries you can bring about but also what instruments and support subsequent analysis will require, and what the theorists will need. You have to take a rather broader view at that point.

G. Burbidge: Martin, are you supposing in this argument that when you've made discoveries with a certain new technique, these are all the discoveries that are to be made?

I'm saying that with that particular technique we don't seem to find new discoveries coming in at a later date. This is a statistical statement which seems to hold true.

F. Haddock: What about the 200-inch which is used on holidays?

A. Moffet: As an analytical tool.

As an analytical tool, it's doing very well. If someone shows you where a quasar is, then you can point with it and get a redshift.

R. Wilson: But your cutoff is the 25 years, and from '54 you only have a little more than 25 years.

Yes, that's true. But you saw that after 5 to 10 years discoveries had decreased greatly in Figure 3.

H. van der Laan: That's not really valid. The 200-inch in that sense, in your sense, is not an instrument by itself. You put a new spectrograph on it, or a CCD on it and it becomes a new instrument.

That certainly is true. But if you were talking just about telescope size, it turns out, and I wasn't going to mention this particularly, but it is true that if you ask about the largest telescope in existence at a time [I don't mean the first X-ray telescope which obviously is the largest one at the time, for in that case you would rather talk about the most expensive telescope in existence at the time], there have been virtually no discoveries made with the largest, most expensive telescope which is available. I think it may have to do with the way that we are using these telescopes.

K. Kellermann: I think Bob Wilson raised an important point. If you pass a 35-year filter through your data, even random data, I'm not sure you might not end up with something like what you have. It's not just the cutoff of 25 years, it's the whole thing.

Well, that's true but if you go back now and look at Figure 3, you can see that there is less than one discovery, in recent times, made at an interval between 10 and 25 years. There were not that many discoveries made in the late 1970's, and so all of these, with exception of one or two, have passed beyond the ten-year limit.

K. Kellermann: Is that because there were no new instruments in the 60's?

I think it may have to do with the circumstances, that while in the 60's there were strikingly powerful new instruments, in the 70's there were not. I believe that's the reason; in the 60's there were a lot of people in the military still willing to put out the odd million dollars because it was the last day of the budget year. And if you had a neat idea, well then you could get that money. You remember those days!

R. Ekers: The following hypothetical situation does not fit into your scheme. Following what Maarten Schmidt said this morning. If the quasars had no radio and no X-ray emission, optically they would still be in there, easy to observe. But Martin's comment, I think was, it would take an awful lot of time to find it. There were a million other objects to look at first. Now conceivably, you might still find them, for example, by increasing the speed with which you got spectra.

M. Schmidt: I've thought about it and I think that quasars in that case would have been found by looking for white dwarfs in galactic clusters. It's very important for stellar evolution to have white dwarfs in clusters, open clusters. You would have gone for the ultraviolet excess which already was done at Palomar in the 50's, for instance, M67. And I wouldn't be surprised if we look back at those objects, Jesse, and we took spectra of all of them, that we'd find quasars.

R. Ekers: My point is that it isn't easy to understand the class of object to be discovered which would not be properly handled by the kind of - -

Well, I think we're probably coming on something like this with SS433 which came up about the time that my book was going to press. There you do find that perhaps with older optical techniques that discovery would have been possible. I'm not trying to rule it out, I'm just saying whatever can happen and -

R. Ekers: For example, if you could suddenly observe the spectra a lot faster, which isn't a parameter that goes into your diagram, then you may make a whole lot of discoveries. How many other such parameters are there?

G. Burbidge: Yes, but you have other counter examples, I mean we know that some of variable QSO's are in the variable star catalogs, where they had been sitting. They were discovered as variables a long time ago. Their very different nature was never established; I mean they're there, they were found in - -

Yes, that's right. There's a real difference between detecting something and recognizing it, staring out like a sore thumb as a discovery that you ought to look at and do something with. The detection alone is not enough.

B. Burke: Maarten, isn't it true that most of those galactic clusters that would have been looked at are in the zone of avoidance and therefore one would not see quasars?

M. Schmidt: *Not M67. It is an old cluster which is at high latitude. And so is NGC188 which is near the North Pole.*

J. Greenstein: *I was going to say that the thing that worries me is what you really mean by discovery, and if I had any negative impressions about your remarkable book, it was that it was as if you knew no astronomy and were trying to sell the product, and the product was measured in a peculiarity and included as a discovery, usually because you had found an example of a deeper gravitational potential well. Let me point out that in the 1930's in astrophysics interacting B star binaries were very hot and nobody here knew about them. When X-ray binaries were found by X-ray means, essentially the whole apparatus of theory that had been developed was now transferred. Everybody was excited about cataclysmic variables or even radio sources. So the question is, are you not committing a total logical thing by saying, "The difference between analytic advances and discovery advances depends on new instrumentation because the new instrumentation gives you something so outrageous it kicks you in the teeth," because instead of being a fraction of a multi gravitational potential surface of the sun, and there's a little larger fraction of the redshift of the white dwarfs in a pulsar neutron star surface there we got 40% red shift and a typical radiation of gamma rather than optical frequency. In that case, one really has to worry about your frequency. It is pure fashion that you are describing, I tend to feel a little that way.*

I think that is right. I didn't try to put any deeper physical analysis on it. I think the criterion of many of these discoveries is that once they are made, astronomers shift their research interests into these fields. As a sociological phenomenon, it's important. When people discover pulsars, all of a sudden there are many radio astronomers who switch their interests to study these pulsars. People discover quasars and many astronomers change their interests to follow those. From that point of view it is a guideline to that which astronomers seem to think important, whether rightly or wrongly. I agree with you; I mean in that sense it's a matter of fashion.

M. Schmidt: *Would you mind defining discovery? You have a list of 43 discoveries....*

As I said at the beginning, there are a number of different criteria. One of them is that the thing gets a name attached. When we see something which is not describable in the normal language that we have, we give it a new name. We call it microwave background radiation, we call it quasar, it is so different from other things. In many cases we don't understand it at all. We have to give it a new name which is untainted. We recognize we are dealing with something completely new and so we give it a new name; this is one of the criteria. Then I mentioned that the other criteria you can give, like symposia dedicated to the phenomenon; or introduction into texts often as individual chapters. But if you then start looking back, you find in plots like Figure 2, that these phenomena actually occupy different positions in these plots. Perhaps that's not fundamental. But beyond such prescriptions, I don't know how to define it. It's just that all of us seem to know exactly when we've seen a new phenomenon, and distinguish it from one that really is not new, even though we don't know what it is that we're seeing often for several decades.

Now, if you worry about planning documents, one of the things that you ask yourself is whether the National Academy's asking for a document on a ten-year interval is necessarily best suited to the discipline at this stage. Certainly if discoveries come about with instrumentation that is only about five years old, then a ten-year document, towards the end of its lifetime, will no longer be up to date. So there is some question whether that ten-year interval was arbitrarily chosen because we've got ten fingers on our hands, or whether it really is the best one. It's something that is debatable; I think it's worth pointing out. There certainly is a time constant within astronomy defined by the rate at which we work, the rate at which we turn the field over, and I think whatever it is, we should match our planning sessions to the time constant of the field, not some sort of arbitrarily chosen interval. Such intervals may be quite different for current astronomy from what it will be ten years from now. It certainly would be different in biological work and could be much faster there even than it is in astronomy.

G. Burbidge: It's about a generation, isn't it? It's the time for the people who are pushing the field to die out.

That's more effect than cause, I think. What I mean to say is this: if you decide as a planning commission that you want to phase a field out, then the lifetime of the individuals and the lifetime of the apparatus in which you've invested do play the major roles. I have a graph on that I can show later if you want. But those are really depending on the fact that you don't want to cause a blood bath where people who are in their productive prime suddenly find their research funding cut off. So the lifetime of people does play a role. There is no question!

Now another question of some interest concerns those people who have made discoveries. Half of these discoveries that I have on my list (on a pro-rated basis, since there were teams of people contributing to the discoveries) have been made by people who came in from outside the field (Fig. 4); many have been physicists....

J. Broderick: Some of them women, too!

I didn't say they were men! Actually Jocelyn was the only woman on this list!

A further factor is that the technological innovations are usually introduced into the field by the innovator himself. They are not bought; you can't buy them off the shelf usually and so we do depend on outsiders to bring those into the field. I think this is relevant; it's worth stating this because I think, Jesse, in your report written a long time ago of course, there was a question raised concerning manpower problems in astronomy (Ref. 3). There was some calculation on how many people would be needed in the next decade and the suggestion that was made then was that this should be the number we would educate in graduate departments in astronomy. The statistics shown in Figure 4 suggest that perhaps one ought to also leave room for physicists and engineers who have unusual instrumentation available, and who would like to transfer into the field -- to leave some slots available for them. The other possibility is that we are not educating our graduate students well, if the discoveries tend to be made by physicists. I think there has been a transformation in the way that graduate students are educated, in

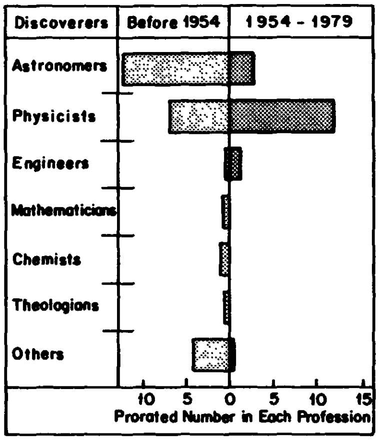

Fig. 4. Career background for scientists who made the discoveries listed in Figure 1. As in Figure 3, the numbers in each class are prorated, since discoveries occasionally take place in several steps and since several individuals may be involved at each of these steps. (After Reference 2.)

the last ten or fifteen years. There is much more emphasis, I think, on physics than there used to be.

J. Greenstein: Martin, if I may say so, at the time which was about 1970, Geoff Keller, who did the statistical study, estimated that half of the future astronomers or astrophysicists would be people with Ph.D. degrees. The count by astronomers produced by astronomy teaching departments was no measure of the output of in 1970. It's been going on for a long time and I think the only platitude you could make about a graduate education in astronomy is that you have to know so many different parts of physics - you're likely to be a less good physicist but a wider one than if you go to most typical graduate schools, MIT for example, where almost certainly you will be compartmentalized unless you're lucky enough to meet an X-ray astronomer (and not be a good astronomer). In fact, it's a strong feeling that I have; that people who have come to astronomy, it doesn't bloody matter what they do as long as they are not over-specialized.

I think that's a very good point. But it used to be, 20 years ago, that the astronomy curriculum was very specialized, and people often were not encouraged to take as much physics as they might have been.

Half the speakers in this room come from radiophysics.

J. Greenstein: But they were either in astronomy or physics. How many of my Caltech pals were in physics? Al Moffet, I know, was.

G. Burbidge: How many people ever took a course in astronomy in this room?

. . . . silence . . . .

That explains a lot!

As far as serendipity is concerned, about half of the discoveries that I was looking at took place in a serendipitous manner, and this does put into some doubt the normal criteria of the peer review we have discussed, because the normal criteria do tend to request a theoretical justification for the work that you are going to be doing. Whether you're asking for telescope time or whatever you're going to do.

The other thing I shall mention concerns military influences.

If you look at the work of the many people who have talked here, and examine almost everything that's happened for example also in the infrared since the War, you'll find a strong influence of military instrumentation. All of the infrared detectors we've had, with one exception, have been military hand-me-downs. Much of the work that we've heard about here also was done with military technology. Unless you suspect somehow that the generals had a strong interest in astronomy and deliberately planned instruments that way, this would be too much of a coincidence, obviously. So, I think the success of military instrumentation seems to be rather higher as far as applicability to discoveries goes than has been theoretical prediction.

D. Heeschen: What's your goal in proposing to change things; to speed up the rate of discovery?

I think the goal is this: if you had a wish to speed up discoveries, it would allow you to plan things better. Now to give a very specific recommendation, I would say that if somebody comes to you and with a technique which is a thousand times more powerful than anything that's available, then it would be a good bet to give him the money to do it, especially if it's not outlandishly expensive. I mean, for example, the field I'm working in - far infrared work - there are ways of increasing spectral resolving power by a factor of a hundred or a thousand without spending an enormous budget. It's more likely to be something of order a hundred thousand dollars. Now that is a good investment. It doesn't mean that if you give someone that money that he will automatically make a discovery. But it does mean that you have perhaps a one-chance-in-ten of coming up with a new phenomenon. You'd have to build ten novel instruments, each individually as powerful in its own particular way, before you made a discovery.

D. Heeschen: There have been 43 discoveries according to you. Do you think that if the peer review system were different or if the rate at which the Greenstein committees come into being, and so on, were different, or anything else, that rate would have changed?

Yes, I think if there were less requirement for theoretical justification, that is astrophysical justification for building an instrument that is very powerful, that you would build more of those, especially the ones that are not too terribly expensive and that you would then have a better chance of making a discovery than if you proceeded solely on the basis of theoretical predictions. If you look at the way that NASA is planning things now, they do look at plots of the kind shown in Figure 2, and if somebody comes in with a suggestion for an instrument that is more powerful, they pay particular attention. This was, in fact, Frank Martin's approach. He's gone to Goddard now, and is succeeded by Charlie Keller. They told me they look at these diagrams to see if the proposed instrumentation gets them into one of the blank areas. If it does, they look at it more carefully, because it does have the promise of giving us a completely new, unprejudiced view of the sky, which often is just what seems to be needed in order to make new discoveries.

G. Burbidge: But Martin, is that true for anything outside NASA? When I think of all the kinds of instruments and arguments that are made for them, it doesn't seem to me that without arguments, instruments are not built. Many people in this room know about instruments that have been built; it doesn't seem to me that they are not built because there was no theoretical justification or there was one; that seems to me not terribly important.

There's another factor, I think, if you run a committee of the type that Jesse or George Field did, I think there is a tremendous pressure from the existing groups -- the infrared community that I belong to, the theoretical community that you might belong to -- to further those studies that we are directly involved in. It seems to me that a committee like that, a fine commission like that, should look at what might be technically possible in a decade to come and then say, "Well, we know that there are these lobbies, they are fruitful communities, we should continue supporting them; but perhaps we should also see whether there are proposals for entirely new instrumentation that would widen our horizons. Let's not depend on the military hand-me-downs necessarily for that infusion of novelty into astronomy." I think it's something that one wants to be aware of at least.

Now I haven't told you about theory yet. I think I can be fairly brief there because I haven't gone as far in this analysis yet, but I'm going to show you the data as they seem to be falling into place. If you draw a plot similar to Figure 1, but showing accumulated explanations, that are at least currently accepted, though they may not be correct, you find that again from the year 1570 on you have a steeply rising curve. Again, many of these phenomena are explained in successive steps, as successive theories come. And for some of the phenomena we don't have, in my opinion at least, satisfactory explanations -- gamma ray bursts, for example.

G. Burbidge: Isn't that proportional to the number of theoreticians, more than anything else?

Voice: They are not equally productive.

I'm just telling you that obviously these explanations may be wrong. Ten years from now, some of these will be thrown out, but there seems to be a current consensus of explanations. We see a rapidly rising set of such explanations in recent times.

A. Barrett: I wonder in that graph where you've got stars; in what year?

In 1939 with Bethe's and Weizsäcker's explanation. You might also, for example, pick another date, 1924 or so, when Cecelia Payne Gaposhkin first suggested that hydrogen was a major constituent of stars, or 1929 when Henry Norris Russell, who initially was opposed to her ideas, came to the same conclusion in his fundamental paper which gave credence to the idea that hydrogen was in fact the major constituent of stars.

This listing has one weakness, mainly that there are a lot of concepts we study in astronomy for which we are not sure that there exist corresponding phenomena in the universe. For example, if you want to look at the research that's been done on black hole theory, it doesn't correspond to anything in Figure 1, because we're not sure yet that black holes exist in the universe.

They are part of astrophysics, and ought to be included among major theoretical studies. I don't know quite how to do that yet.

If you make up a time-lag plot for theory similar to Figure 3 which relates or correlates instrumentation and discoveries, you can search for a correlation between theoretical work and observational discoveries. You find that there is no close relationship at all. If you compare things that were predicted and contrast them to those that were explained after the discovery, you find a number of phenomena that took more than fifty years to explain properly. There are relatively few predictions compared to the number of explanations that come after the fact. You see a scatter diagram of theoretical predictions and explanations, far more loosely correlated than that of instrumentation and discovery, where we saw a time lag of only 5, 10, or at most 25 years.

A. Moffet: Martin, how do you class the neutron star?

I looked at the work of Oppenheimer and Volkoff in 1939 and I took the period up to the discovery of pulsars in 1968.

B. Burke: I'm going to differ with that view.

If you wanted to look at it in more detail you could also put in the work of Landau and Zwicky which preceded the Oppenheimer and Volkoff's work, and perhaps Franco Paccini's work. This is provisory because I think as I go along I would like to divide key steps up somewhat more carefully. I don't think it will make a very big difference in the overall distribution.

Now there's another time lapse which is of some interest which Freeman Dyson has discussed in an interesting article (Ref. 4). He has shown that there is a very long time lapse between the introduction of new ideas into mathematics, and their adoption in physics. Thus, for example, matrix theory was discovered almost three quarters of a century or so before being introduced into quantum mechanics, non-euclidean geometry was discovered long before Einstein incorporated it into General Relativity and so forth. But there seems to be a rather shorter time scale between the adoption of a new mathematical scheme by physicists and our adopting that scheme into astrophysics. While quantum mechanics was being developed, Henry Norris Russell and F. A. Saunders were already developing the Russell Saunder's coupling schemes. Similarly also, Chandrasekhar's introduction of relativistic degenerate matter into white dwarf models in the 30's not too long after the Fermi-Dirac statistics had been introduced.

Now there is one other thing I missed. It involves the citations that we have of older work. Unfortunately, when you look at the literature, you see that work that is much older than about five years is seldom cited explicitly. That doesn't make that much sense when you see the wide scatter between the time that predictions are made and the time that discoveries are made. I think that's in part responsible for the discussion we had here this morning where the question was, "Why wasn't the work of Alpher and Herman taken into account or even noticed at the time that the microwave background radiation was discovered?" It's a reflection on the way that we do business in astronomy that we somehow feel, "Well, this is fifteen years old, it can't be any good anymore." So, I think that's something that one ought to watch somewhat more carefully. I don't have any constructive suggestions, but the other side of the coin may be that it's just too difficult to keep track of everything.

R. Wilson: Not that you reject it, but that you're just not thinking about it anymore.

I think you really have to keep things cooking all along; ideas have to be revitalized or brought to the attention of people over and over again for them to be kept in mind one way or another. Some ideas probably don't deserve to be remembered, so it's not clear which ones to maintain.

We can also examine career background, though I'm not all the way through trying to find who was doing what yet. My first impression is that theorists divide up roughly in the same way as observational discoverers, in Figure 4.

R. Wilson: What did the theologians predict?

The theologians predicted the stellar parallax. That's what Copernicus did; his work is well known. Then there is Lacaille, who had put together a list of diffuse sources -- one of the first comprehensive lists -- and explained some of them correctly, and Piazzi, who had found the first asteroid on New Year's Eve in 1800. Lemaître could also be in there; I used mainly deSitter, Friedmann and Einstein for the cosmological models, though it is true that Lemaître had one also, it was somewhat different from some of the others.

B. Burke: North gives the prize to Lemaître.

Lemaître had a sharp beginning, and then he had a nice stage where you could evolve galaxies, and then the expansion started again. I think that part is probably not quite right.

B. Burke: North claims that Lemaître is the one who cogently formulated the expansion law in physical terms which probably was first done by Wild, but Wild's hypothesis was not generally accepted. It was in fact an object of furious controversy. And it was Lemaître who unambiguously interpreted the expansion of the Hubble Law as a physical expansion.

R. Ekers: On the grounds it won't change the statistics.

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OBSERVATIONAL INNOVATION AND RADIO ASTRONOMY

R. Hanbury Brown
University of Sydney

I've already given two thirty-minute talks on this trip, and I knew I was going to follow Martin and that he would say most of what I had to say, so I didn't prepare thirty minutes - I've got five minutes, which is fine.

If I had given another talk, I would have illustrated the question of progress in research by talking about optical astronomy. The thing that always struck me was the state of optical astronomy in the earlier days, let us say before 1950. It was an extremely slow moving business, and the thing that particularly worried me was the opposition from optical astronomers to the introduction of electronics into the dome in any form at all, on the grounds that it was unreliable. Within a few years, when one person had done it successfully, they switched their ground, and now they all tell you about their CCD's, in fact they bore you to death with their CCD's.

I tried to interest optical astronomers in the United Kingdom in testing image tubes in 1950, because I knew someone who was working on the image tube. He had never seen a telescope, and he wasn't invited to try it out on one. The optical astronomers were extremely conservative, and I attribute the conservatism of optical astronomy partly to the isolation of observatories on high places. You freeze your technique and then put it on the top of mountains!

I know what it's like. I once tried to observe at the Pic du Midi. In the middle of the night I wanted to know the position of one of the stars. I went down to the library and the most recent book was a hundred years old! I was faced with the problem of the precession of the object I was looking at over a hundred years, and I couldn't work it out; it was too cold anyhow. By the way, the isolation of technique doesn't have to be on top of a hill, it can be out in the Australian bush. The point I would have made is that a lot of the progress in astronomy has been done by engineers and physicists, and it is most important that the work should be kept in close touch with them.

I just want to conclude with a brief word about the moral of the whole of this meeting after listening to the very nice story about Jansky. I have been worrying about what the moral of the story is; if you're an earnest type, like me, you have to draw a moral, especially if you've come half way across the world to listen. It seems to me that the moral of the story that we listened to yesterday about Jansky -- the first moral which you would engrave on the side of a building in stone, like you put names like Copernicus and Kepler and Morton Roberts and so on, would be that *science cannot dispense with an appeal to experience*. That would be the first moral that I would draw from the story of Jansky.

This is a very serious thing I'm talking about, because it's one of the great philosophical illusions of history that everything can be produced from a simple series of laws and that our knowledge of the universe can be reduced to this series of laws. It's one of the great lessons of the science of the last 300 years that you cannot dispense with an appeal to experience. Most of the inventions on which our civilization depends now would not have been supported by a committee of review which gives out money, the sort of committee that I have sat on and probably many of you have as well.

The progress of modern medicine, for example, is almost entirely dependent on the development of X-rays and on the development of antibiotics, both of which were entirely accidental. Both of them were based on experience and neither of them could have been planned. They were not logical. Thus the whole of medical advance is based on accidental experience, not on scientific planning. I will just draw your attention to the fact that if you had a logical committee reviewing inventions, you would have rejected the piano as unworkable, and you would certainly have rejected the bicycle. Anybody given the drawings of a bicycle would reject it at once.

A. Moffet: Not to mention the bumblebee!

Yes, but that's in a different branch of science.

The second thing that I learned from yesterday, and I'm prepared to come 12,000 miles to hear it reinforced, is that science is based fundamentally on freedom of inquiry. This is the deepest lesson, I think, that we can draw from what we've heard of Jansky's work. We usually think, "Well, that's a problem from the past." Freedom of inquiry is all to do with the rejection of authority, Galileo, and the Church, the authority of Aristotle, and so on. We tend to think of freedom of inquiry as a battle which has been won. But of course as one of your Presidents said, I think, "The price of freedom is eternal vigilance." I can't remember which one it was, there must be some American here who can tell us! The point is, of course, that freedom of inquiry is something which actually does require vigilance. It is no longer the authority of the Church, or the authority of written documents which restricts the freedom of inquiry in science. In fact, as we saw in the Jansky story, it's not those things that you've got to be careful about and have to watch. The freedom of inquiry can very well be limited by our ideas; the current structure of scientific theory is a framework within which we must work and that restricts us. The construction of very large instruments is another thing which restricts the freedom of people; it gives them an opportunity to make new observations but it restricts their freedom of inquiry. You find yourself working for the instrument, rather than the instrument working for you.

When you look at all these things, you find that at the root of the problem of the freedom of inquiry is the basic point of self-interest. I'd turn my collar around if I could, but that's the point I'm talking about - the question of self-interest. The things which can never be said too often, and which I get from listening to the story of Jansky, is that nature has to be investigated on its own terms, not on yours. The world was not made just for you, possibly it's entirely alien. Progress in science is made by investigating nature on its own terms, not on ours. And that's what I mean by self-interest. The fact is illustrated beautifully when Jansky discovers a hiss coming from somewhere; it's not particularly beneficial to the objectives of the Bell Labs, and so the thing is not pursued. If it had been in some other part of science, it might have been investigated. So progress is obstructed by self-interest, and that is true of people preserving their ideas and theories, their positions and all the other things.

Now, I just want to answer a question that Greenstein asked in terms of what I learned from the story of Jansky. He said,

"How can you plan serendipity?"

It has been suggested that the answer is that you need the right man in the right place at the right time. But the moral I've got out of this story is different; it is that *you need the right man in the right place at the right time, but he must be a man who doesn't know too much!* I believe this is true, and it's the reason of course why physicists and engineers came into radio astronomy and made discoveries; many of them didn't know the sun from the moon, they didn't know a planet from the stars, like most of the people I worked with at Jodrell Bank! One guy spent half a paper telling people what declination and right ascension was! But that's all right. You do need some people who don't know too much. What moral, may I ask, do we draw from that if we work in an educational institute? I think I can draw a moral, which is that *you don't try to teach people a lot of facts. You just try to teach them how to get on with a job and be self-reliant.* Anyhow, those are the morals I have drawn about serendipity from the Jansky Symposium.

NON-STANDARD APPROACHES TO ASTRONOMICAL RESEARCH

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It's hard for me to talk about serendipity because of course that's not the business that I'm in. I do have some sympathy with the point that Jesse made yesterday afternoon, to the effect that since we are running out of regions of the electromagnetic spectrum which remain to be investigated, the chances of making discoveries by opening these up in a serendipitous way are probably going down. It's hard to not believe that. Also, it occurs to me after listening to Martin Harwit, that when one does make such discoveries, some further discoveries are inhibited. That is, there are shocking thoughts that one might think which we are really not seriously allowed to think after some initial discoveries are made. For example, everyone feels they have found the microwave background radiation, and it arises from the big bang or at least it has a cosmological origin. Therefore, the possibility that the flux is variable in time is not even going to be considered for a very long time. If people thought they were seeing variability, they would be so sure they knew where the microwaves were coming from that they wouldn't accept the result. If indeed they managed to convince themselves, they wouldn't convince their colleagues, and if they managed to convince their colleagues they probably wouldn't get the paper published. So there are extreme problems of that kind, and maybe others. Another good one, I think, is the history associated with the idea that radio sources are stars. People are now so convinced they're practically all quasars or galaxies that star-like objects which are occasionally identified as stars are treated as misidentifications. There probably is a stellar component, but how large it is, and how long it will take to really settle that question, I don't really know. But that's not what I was going to talk about.

What I am going to try to talk about is the way that I think we do research in this day and age in astrophysics, or in certain parts of astrophysics, and the reasons why I believe that our subject is still a soft science, and what we have to do to try and improve the situation. Of course one of the major reasons why it's soft in the sense that a good part of the experimental physics is hard, is that it is an observational and not an experimental science, and therefore, there is no way in which we can actually test theories and ideas by carrying out and repeating experiments. We are completely at the mercy of what comes in from outside; however clever you are, it's no good if the photons or the charged particles aren't there, or if they turn off for some reason. We have no way of doing anything about that.

We can talk about three categories of research problems. The first is research which is essentially problem oriented, where the problem is reasonably well defined and there is also some basis for real understanding. By that I mean astrophysics of the kind that I think we would all agree on, where most people would agree that real progress has been made. Such an area is the study of stellar structure and stellar evolution, where there is a fairly good basis in understanding, though it must be admitted we still have not carried out the ultimate test correctly, and by that I mean that we have not solved the solar neutrino problem.

The second is concerned with observational discoveries of an entirely unexpected kind. Most discoveries in modern astronomy are of this kind, and certainly all of the discoveries in radio astronomy which are being discussed here are of this kind.

The third approach which is the one that I would like to think that I have been trying to play with from time to time. It is an attempt to put a more solid base under at least some of the claims made for many of the advances which are based on discoveries of the previous kind. Most of the discoveries in radio astronomy that we have talked about here fall into the second category, and the problem that I have with the way that we're tackling them is that most people seize upon the first very superficial explanation which drops out and becomes popular. They treat the explanation in a problem-oriented way, taking for granted the superficial explanation and basing more and more on it, so that what is developed is what I have often called an inverted pyramid of ideas.

There are two tacit beliefs which underline much of what is going on - both of which should be seriously questioned. The first is that while we have very small samples, gross extrapolation is always justified one way or another. They may be complete samples, but they are very small samples, and many people tend to take the view that this kind of extrapolation is justified, and it is extensively used. Let me give you two examples. While we only know something about the initial mass function of stars in a very limited region of the universe indeed, this is normally used as a basis for calculations in a very extensive way associated with a wide range of phenomena in the stars and the galaxies. A second one is the belief, and here we don't have any good observational evidence, that the virial theorem can be applied in all systems, in the sense that it is assumed that all groups and clusters of galaxies are essentially stable configurations, and are bound. This belief is the starting point for much of the argument about the missing mass. In almost all systems there is much more kinetic energy in the visible matter than potential energy. This is the driver for the missing mass industry which as you know is practiced from the scale of the Local Group (never even proved to be a physical system) up to the universe as a whole. This is really a gross extrapolation! If you look at the Palomar Sky Atlas which after all contains the largest sample of astronomical objects ever detected, there are something of the order of 10^{10} images on those plates. However, only a few hundred thousand, if that many, have ever been looked at in any way with any instrument other than the survey instruments. On that basis, what is the chance that we've even got the beginnings of our ideas right with such small sampling techniques. It seems to me it is very small indeed.

The second belief that I think is open to very serious question is that at the stage it is generally believed and assumed that we cannot learn anything about the laws of physics from astronomy. Some of the most fundamental things in physics that we have learned, for example, the law of gravitation, have come from astronomy. Now we have apparently reached the point where everyone wants to use only the known laws of physics to explain anything that they see, but the possibility that you can turn the problem around, which is the way the subject has developed, really is apparently no longer allowed. On these two bases alone, isn't it very likely that we are hopelessly off base in our understanding of the universe?

I have to give a few examples, I suppose, since no one is arguing with me at the moment. What is the justification for doing things this way? I think there's a very crass, but real, aspect of this which applies to many young people in the way they do research nowadays, but this audience is not notable for its young people. So perhaps this argument isn't terribly important, but for what it is worth they feel that unless they engage in something fairly conventional, it's probably going to be very hard to get a job. That's trivial to us, but real to them. I think that is a very considerable indictment of the direction the field has been taken. The second argument which I always encounter any time I get into this kind of a discussion, is that it is reasonable that we should follow through a certain line of argument and only give it up when it finally breaks down. This, after all, is the normal methodology of science. You test the hypothesis to the point at which it no longer is viable and then you're supposed to turn around and admit that it doesn't work and try something else. In practice, in many of the branches of astrophysics that we are so interested in here, it won't work. Why won't it work? Well, it won't work because of the inherent ambiguities in observational science associated with the fact that we are largely basing our arguments on indirect and circumstantial evidence. So what we often get is not any real demonstration that something works, but compatibility with a set of ideas. This in no sense is proof.

I can give you an example which is very familiar to me and maybe to many of you here, associated with radio galaxies and QSOs. Consider the radiation coming out of the nucleus of one of these objects, optical radiation, radio radiation, and X-rays. We deduce from the shape of its spectrum, that it's non-thermal. Because of the difficulty of seeing how it could be coherent radiation we suppose that it is incoherent synchrotron-radiation. We also have the polarization argument, of course. In the complex sources it is entirely possible that the mechanisms are due to coherent processes, but we don't take them seriously. Then because of the difficulty of seeing how the radiation could arise from positrons or protons, we assume that it is due to electrons, though we don't have any proof. So now we have to explain the existence of a large flux of relativistic electrons, with a very large energy. Because the energy is already very large and because of the difficulty of seeing how much more energy could be released, most people ignore the possibility that protons dominate, or that the acceleration process is very inefficient. Moreover, they tend to use an equipartition argument (between particle energy and magnetic energy), which I once invented, but which probably doesn't apply in most cases. This is all to make the problem more tractable. There's no indication whatever that this is the way nature is really behaving.

We then have to connect the existence of this large flux of particles which we have estimated in this very conservative and possibly entirely unrealistic way in terms of the energetics, with the release of gravitational energy. Then follows a sequence of imaginative handwaving procedures, called theories by some of my friends, in which acceleration takes place near an accretion disk around a black hole. Now there's no direct evidence at all for the existence of a black hole or an accretion disk in such situations, nor can there be in the foreseeable future. If you want a practical demonstration of this, look at what is required. What is required indeed is to look at structures with scales of something like a few tens of astronomical units in these nuclei. That boils down to about 10^{14} centimeters for sources for which minimum distances in many cases may be a hundred megaparsecs. That corresponds to 10^{-7} arc second resolution, which is required before you can even begin to test this hypothesis at all, even in the most indirect way. But somehow this whole scenario has developed and it's growing.

Now there's an alternative way to go which of course has been around for a very long time, which is to go right back to the beginning and say, rather than supposing that this very complex mechanism is working, that Ambartsumian and Hoyle are correct and that we are seeing matter created in these regions. Of course this requires us to admit that at these levels the general theory of relativity doesn't work and must be modified. Of course it's very likely that it doesn't work once you get to these scales with large masses. There is no indication that this gravitational theory is correct in the strong field approximation, but you see what is happening. We are following a path involving a large number of hypotheses, none of which we can prove, which takes us off in one direction, and we are ignoring the other. Walter Sullivan is writing about black holes in the centers of galaxies; the textbooks are now saying that these are the answers. This leads to another problem, because the next generation reads this and thinks, "Well, that's the answer to that problem. We'd better build on this structure a little more." My suspicion is that the whole thing at least is suspect. As a gambling man I would say "Even money is the best I would do," and I probably wouldn't give you even money for most of the conventional arguments.

As I said, there is no way to test the kind of arguments that are being made, so what do people do? Well, you know very well what they do. They ignore the kind of alternative I have mentioned and if anyone writes on the subject they referee them to death.

Another criticism of the present approach is the way that we selectively handle data. Take the variability of the radio sources; when variability was first reported in CTA 102, a good many of us wouldn't have any part of it, because we knew that the object in question had a large redshift. We also knew that it was done by a Russian. Then Fred Haddock and Dent established the variability in 3C273. At that point we had to buy this argument. So then we started working like mad to try to make everything fit together. That's in fact how Fred Hoyle and I got into the redshift argument in the first place.

More recently, low frequency variability was found in radio sources. This causes even more severe problems, if you want to call them problems, in the sense that it's very hard to understand in conventional physics how to explain it. Tom Jones and I wrote a paper in which we discussed this problem in 1973. We proposed several solutions, some of them radical, but what I was greeted with was, "Oh, we don't believe in the data. There's no real evidence that low frequency variability exists." More recently, I think everyone is convinced now that low frequency variability does exist. So now everyone is very quiet about this problem. It's still there. No one is working on it; it's a real roadblock in the way of understanding in conventional views, but it's ignored as far as I can tell. It's seriously ignored.

A. Moffet: There have been symposia on the subject.

K. Kellermann: It's also not true, you do understand it.

Well, I was going to make some crack about the superluminal stuff in a moment; I don't think we do understand it, Ken. I challenge you! You have to put in most extreme conditions which you also can't demonstrate are correct.

M. Cohen: Geoff, we use your arguments!

Yes, well my problem is that I occasionally see the light and change my mind! This leads me to a set of criteria for success. I don't think you'd like to hear them. But of course there are various things that I can say about some of the other rather delicate matters, but I'll simply skip all of that.

M. Harwit: I'd like to hear the criteria for success.

Well, they are very cynical criteria.

M. Harwit: That's fine!

Here you are!

1) Don't have too many ideas.
2) Take a simple-minded view accepted by your peers.
3) Persist and persist with it.
4) Never admit of its weaknesses.

And then I wrote geometry and superluminal sources to remind myself of various problems.

5) Deny that there are problems.
6) Go to as many meetings as possible and preach it.
7) Deny the opposition a fair hearing.
8) Stop them observing if you can!
9) Always appear reasonable even if you are wrong, and you are likely to be in the long run.

Then there's a very good chance that you will become a respected leader in your field, and you will live long. The average age at death of Fellows of the Royal Society and members of the National Academy is significantly greater than that of the average populace and the average scientist. So that's my recipe for success!

H. van der Laan: Since Geoff is a Fellow of the Royal Society and holds one of the top jobs in American Astronomy, were those autobiographical notes?

I'm very exceptional!

B. Burke: Geoff, I'm sympathetic to some of your ideas, that is, that a received doctrine is always something that we're uncomfortable with.

M. Price: Burke is trying to appear very reasonable!

As I said, you've got the point! You may disarm for the day.

B. Burke: How do you propose to keep down the noise level, because obviously everybody is shooting off their mouth about non-Hubble expansion of the universe and many other things; you can get into difficulty where you will need some sort of a selection criterion to do the sorting. I think we know how one prime example of sorting is done. Namely at the close of the war, Arnold Shostak essentially made a table of those he had high regard for and gave out the money as a very personal approach. Also, you need an incorruptible money giver and under the present circumstances how would you propose to make the division?

I don't know, and the problem is a very difficult one. I refer back to what Hanbury said about the peer review system tending to favor in essence the people who are doing the things that everyone supports. I have direct evidence of this from the allocation of telescope time. People ask me, "Why don't you give time to some of the people who have these very different approaches?" and the real problem and the real danger, is that there are practically no people now who are trying new approaches. The younger people who should be making these moves aren't around. But the history of science shows that science progresses because the newest ideas come from younger people who tend to overturn what the older people believe. As far as I can tell in astrophysics at the moment practically all the radicals are over 50, and there aren't very many of them. So I don't have the answer to your question, Bernie. To be quite honest with you, I believe we have reached a level of establishmentarianism which is very high. For example, there is this incredible concentration of people working on the evolving universe and on the early history of the universe and on big bangery. And while the evidence for the big bang from the microwave background is strong, the rest is based on very little. No one knows where the galaxies came from, but everyone wants them made in a big bang. No one is considering alternate cosmological or cosmogonical possibilities in a serious way. So we manage to get everybody going down the same road all in lock step.

M. Harwit: I've been looking at this question a little bit recently because it's quite clear one can document the existence of the so-called authoritarian suppression throughout a good part of history, not just the present. I've wondered sometimes just about this question of what should the criteria be that would allow one to introduce more freedom? It seems to me there are two parts of theoretical work which you sort of abruptly separate: theoretical work depends on new ideas and it also depends on new tools, that is the actual working out a thing in sort of a serious way. Now the ideas often are much easier to come by. They get reinvented over and over again, forgotten, and then when the tools are ready there is going to be somebody who then incorporates what may have well have been an old idea into a form in which one can actually calculate things and come up with some results. I think whenever somebody says, "I'd like to calculate something seriously with a technique which I think is going to be powerful, even if it doesn't necessarily lead to an acceptable or fashionable point of view," then one should give that student or young person or whoever wants to do it, full rein to introduce that technique into astronomy because it will undoubtedly be useful or may very well be useful as a calculational device that will then allow progress. That, I think, would be one criterion that one should employ. Then if somebody is really willing to work out a new technique that is powerful and allows us to do new kinds of calculations, whether it immediately looks useful or not, one should in fact encourage people to develop that. I think as far *as the specific example of the current universe goes, I think those people who are just out hand waving, one can dispense with them. But if somebody comes up and says, "Well, I think I have a way of calculating a relationship between quantum thermodynamics and gravitons that might be of importance in the early universe," then I think one should try to encourage that.*

There's a more fundamental question associated with the fact that we can never observe the early universe. Under these conditions, at what level does each theoretical speculation cease to be science?

M. Harwit: Well, you don't know at what temperature the gravitational waves are going to detach themselves; there might be a gravitational wave background that you could observe.

If we go to the inflationary universe and things of this kind, it seems to me that this is a pure theoretical postulate which cannot have any direct effect, and the chain of arguments which takes us from it to observations is so long and so tortuous that we can never test the basic postulate.

M. Harwit: That's pretty prejudiced. Until you know what the structure of the theory is going to be, you don't know what it might predict that you can observe.

Well, in an absolute way I have to agree, but in a practical sense it seems to me that we are talking more theology than science.

THE BUFFALO SYNDROME

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Introduction

There is a slight sociological phenomenon I've noticed, which I call the Buffalo Syndrome. What it has to do with serendipity is that I discovered the Buffalo Syndrome (by almost stampeding my departmental promotion committee) serendipitously on the day I was talking on the telephone to Ken Kellermann who told me about this workshop, and I agreed to talk here about it. At first I was scared; after all, you are a mighty impressive audience and, to keep you from taking offense at my remarks, I decided that you guys were all buffaloes of the African Cape Buffalo variety - "a fiercely independent animal and one of the world's most dangerous animals." The buffaloes of my title are the American Bison. I've heard a few examples of the Buffalo Syndrome given here today - we might even consider that our present fascination with the Big Bang Universe is a manifestation of the Buffalo Syndrome.

According to the Encyclopedia Britannica:

> *"The herd may remain in one location for several days and then, for no apparent reason, move in a purposeful manner to a new locality; at other times the herd may move more or less constantly in a seemingly aimless fashion."*

And there's an irrelevant remark:

> *"The usual gait is a plodding walk...."*

The Britannica continues:

> *"Bison are unpredictable animals. Sometimes they can be approached without evincing alarm. At other times they stampede with the least provocation. It is never safe to approach them too closely."*

I was hoping to illustrate this with the Frederick Remington (at least I think it was him) picture of the Indians stampeding the buffaloes off a river bluff in order to get a meal, but the best I could do was a composite picture - Andy Warhol style - of the same buffalo five times. It's a simulated herd; close to what I wanted - a stimulated herd.

Definition

To have a concept like the Buffalo Syndrome, we need to have a definition. The excerpt from the encyclopedia sort-of works, but as for the part about it being never safe to approach buffaloes too closely - well, radio astronomers are approached quite easily. But the rest of the definition would work provided that instead of "prairie" we use Martin Harwit's "observational phase space" as the place where the radio astronomers (instead of buffaloes) wander in. Occasionally, radio astronomers have wandered off to some area doing one thing while other areas were neglected, and so on. So that may indeed be a good definition; if you don't think so, then make up your own. Now, there are other representations of this phenomenon, competing theories, if you will. One has been called the "bandwagon effect," but I couldn't possibly claim credit for discovering that, it's far too obvious. So I called my discovery, the Buffalo Syndrome. Another manifestation of it is called the Gold Effect (after Tommy Gold according to Ray Lyttleton in the Encyclopedia of Delusions). We'll see a good example of the Gold Effect after the Proceedings of this Workshop come out: Nobody agrees what serendipity is. Everybody had his own idea when he came here, and they were all different ideas. But, when we leave, we'll all go away with at most one or two ideas. In other words, the Gold Effect causes an enhancement of misconceptions, or conceptions, as the case may be. In that way the Gold Effect is kind of like the Buffalo Syndrome.

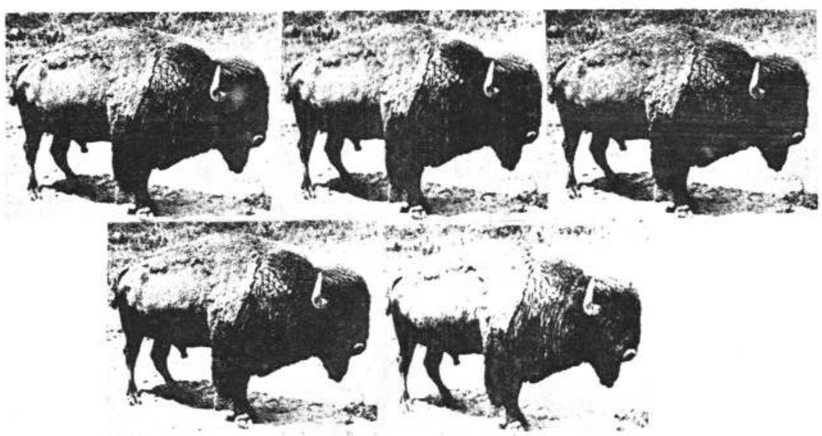

Fig. 1. Simulated Buffalo Herd.

Examples

I have to illustrate the concept with examples. I'm not sure these are good ones; I think I've heard some better ones during these talks.
The first example I wrote down was the millimeter wave telescope. It also has to do with how this concept and talk got started. That's because at the time I was stampeding the promotion committee (not against me, for I happened to be a member of it) and agreeing to make this talk, I heard that the millimeter wave astronomers had once again gathered, this time in Washington. Having not gotten the 25-meter millimeter wave telescope and having seen other nations move into that spectral region with big instrumentation, they saw that American millimeter wave astronomers were going to be left behind and, in some sort of last-ditch attempt, they tried to make yet another push. Only this time the proposed instrument became, instead of a 25 meter dish, a wye-shaped array. It seemed to me all along, before this, that everything in millimeter

wave astronomy had to be single dishes; now everyone was talking about arrays - a sudden opinion-shift had occurred. Now there may have been good reasons for it; but, anyway, there it is, my first radio astronomy example of the Buffalo Syndrome. The same day I heard that a famous millimeter astronomer was interested in putting up a millimeter wave telescope in the Southern Hemisphere, where it can best see those things which millimeter wave telescopes can best look at. That surprised me too, because during the previous five or six years there were only about two people in the world (or at least in the Northern Hemisphere) who felt that a millimeter wave telescope should be put in the Southern Hemisphere. You can find the Buffalo Syndrome occurring in a lot of different ways; this example was mostly one in terms of instrumentation, viz., the idea of new telescopes having to be arrays instead of single dishes. I suspect in a few years sentiments will change and single dishes will be in vogue and arrays, passé.

Another example I've seen (and I've only taken ones from my brief dozen years in radio astronomy - you may be able to think of some that happened before that) is the steady progression in radio astronomy to higher and higher and yet higher frequencies. At the expense of interesting stuff at lower frequencies. At least, this happened in the U.S. It's been just one big push; people are now finally going back to the lower frequency phenomena which have been neglected for so long.

The next example might not be a good one to bring up at this particular place; but I thought Grote Reber would be here, and I would get some support. Anyway, there seems to be a push to centralize observatories so that there are a few national facilities where all the observers have to go rather than a lot of individual university observatories all over the place where observers just stay and do their things there. Maybe that's a political manifestation of the Buffalo Syndrome.

In terms of analysis techniques: the thing to do nowadays is mapping. Of course, with the arrays we've got, we have to make maps. If you do fringe visibility analysis of your data, it's considered a novel technique. A couple of colleagues of mine using the VLA rediscovered the technique and thought they had invented it! The VLA software isn't (or wasn't) capable of plotting the fringe amplitude as a function of projected baseline length (and so they still claim it was novel). Or worse: I recently had trouble getting a paper published which was based on visibility analysis of some VLB data. There wasn't enough data to make a map so we modeled instead. Two referees fought the paper's publication and, after a bitter fight, finally gave in. (I don't know if they capitulated to our arguments or just said, "What the hell!" and gave in.) They felt that we couldn't really say what we were trying to say (simple double-source sizes and separations, etc.) unless we had maps (or enough data to make maps).

In my experience in VLB, I've seen organizational shifts: at one time VLB was supposed to be done by university observatories - it was the proper and fitting place to do VLB. After all, you had to have VLB telescopes located all over the world; universities are located all over the world; so the telescopes would be close to universities, and it would be a good thing. That went along all right for half a dozen years and then suddenly everyone decided: "No, one place should build a VLB Array and operate it, rather than a whole bunch of places." There were arguments for both sides; but it seemed like rather than considering all of the arguments, they considered half of them - first, the half in favor of dispersion, then the half in favor of centralization.

I've seen frequency shifts: VLB did the same thing as the rest of the U.S. radio astronomy and went quickly to the highest frequencies. Frequencies too high for the antenna surfaces, frequencies too high for the pointing, frequencies too high for the clocks and local oscillator stability, frequencies too high for the atmosphere to be cooperative. Low frequency VLB was left behind. And as I mentioned above, VLB has also shifted to map-making as the analysis of choice.

In the field of searching for extraterrestrial intelligence, there's been a couple of waves of herding to one spot or another. First, there was the idea that E.T. would communicate with us in the radio spectrum. Then when these signals weren't detected in the HI sky surveys, it was decided that E.T. would only signal with extremely narrow-band signals rather than with those detectable by the radio astronomy HI surveyors. After the possibility of interstellar travel seemed to become feasible (if not for us, then for others), the idea arose that no one was out there because they hadn't been here yet.

These are my examples. They aren't too well thought out, and you may disagree with some or all of them if you wish. In that case you should come up with your own.

Stampede Analysis

One of the problems is figuring out when is it a stampede? Well, it's certainly not a stampede if you're in it; it's the right thing to do. It's only when you're left behind, only when you're out in the cold and you're grazing in a place where the wolves can come up and catch you, that it's a stampede. It's something the other guy does (or, by the definition, other guys do).

Why do stampedes occur? Why do people move into different areas? There are lots of obvious reasons. There's probably an obvious reason for each one of my examples. First off: moving into new observational territory. When you start putting up x-ray satellites, for example, there's going to be lots of things you can discover with them, and so everyone becomes an x-ray astronomer. Or pushing to higher radio frequencies: there's lots of new things to discover at higher frequencies so you keep going higher. (Well, that was true for galactic astronomers with all their molecular lines, but for extragalactic astronomers the discoveries more or less ran out when we passed 10 GHz.) Discovery is easier than prediction. So it is better to go out with a new instrument and find new stuff than to try to figure out what to do with what you've got.

In terms of new astronomical objects, there are always lots of details to clean up. Once pulsars are discovered, once neutron stars are recognized as existing, there are questions like: "What's a neutron star magnetosphere like?" "What's the character of the interstellar medium?" Stuff like that. When a new field is opened up, there's lots of cream to skim and people move in.

Then there are inverse stampedes. It seems to me low frequency variability has been one example. Until recently, people have stayed away from low frequency variability hoping, perhaps, it was wrong and would go away. Another example of an inverse stampede is flare stars. It seems to me that flare stars should be something that astronomers would have glommed onto real hard, because they can be observed in all regions of the electromagnetic spectrum. They also make a connection between stellar and solar astronomy: the sun gives off flares; flare stars give off flares. But no one or hardly anyone seems to be interested. Finally, in a technical area, are line feeds for spherical dishes. Merle LaLonde once told me that one of his major problems was that nobody else worked with line feeds and so he had to make all the mistakes himself.

Impact on Little and Big Discoveries

In terms of little discoveries, the Buffalo Syndrome sometimes helps. It helps because, in an area that has lots of people involved, other people are making the preliminary discoveries or the mistakes; and you can build on their successes and don't have to go down their blind alleys. When there are people working conscientiously in a given area, they will eventually discover nearly all that is to be found in that area.

I don't think you can predict serendipitous discoveries, but you might call big discoveries, facilitated by a new technology, serendipitous. However, that's not quite true. You know you're going to discover something big when you put up x-ray satellites, and so you can't claim it's serendipitous when you do discover something with them. You might be lucky if you got there first, but that's where the serendipity ends. Big discoveries in a new technology area are helped through the Buffalo Syndrome by having lots of people working in the field developing the instrumentation, blocking off the blind alleys, making the big discoveries, which lead to many more little ones.

Little discoveries are hindered by the Buffalo Syndrome when they occur in a place where the herd isn't active. They tend to be ignored and have to be rediscovered once the herd moves in. Synchrotron radiation is an example of this: it was discovered theoretically in the early part of the century and had to be rediscovered once we noticed that Nature and General Electric produced it. (Much of my own work had to be rediscovered, but maybe that's for other reasons.)

I am inclined to think it can also sometimes hinder the impact of big discoveries, like radio astronomy. If World War II hadn't come along when it did and we lived in the promised peace, perhaps this workshop would have to wait another fifty years because by now we still wouldn't have noticed Jansky's discovery.

Conclusion

So is the Buffalo Syndrome good or bad? I say neither. At most it's annoying when your own work is being neglected because it's currently out of the mainstream. That concerns only the small number of people who aren't on the bandwagon or in the buffalo herd, those who are going to be chagrined by the fact that appreciation of their work in their field is going to have to wait until the herd comes by. Or worse, chagrined by the fact that the herd has to rediscover their results. At best it facilitates discoveries which would have been made sooner or later.

K. Kellermann: I want to elaborate on the telephone conversation John referred to. We were discussing some important astro-political problem and he kept referring to radio astronomers as being like a herd of buffalo. First they go thundering off one way and then they're thundering off another way, and I was impressed and asked him to give a talk on that at the workshop. He said, "Sure!" A few days later he wrote and said, "Well, maybe I shouldn't do that, they are kind of sensitive people, and somebody might be insulted; but maybe it's all right because they're all going to think I'm talking about someone else." I know I'm not included in that category, John, but you're wrong about centralized-like VLB observing. It didn't happen overnight. It took ten years to drive it into their heads!

It crystalized overnight!

P. Crane: Well, one of the extreme forms of the buffalo syndrome may be described by the catastrophe theory!

M. Harwit: There were two points that were brought up twice here. The first one was about national centers that you raised, and you also were talking about. I think one does want to pay some attention to circumstances. Certain types of work are just not possible at national centers. For example, the sort of things that Bob Wilson talked about this morning. They took apart their telescope in order to check whether or not the noise that they were getting might be explained by cracks that could be taped up or some of the bird droppings that had been left inside. It's clear that you can't take a telescope apart in a national center. Many of the efforts that have been described here, in fact I think every single one of the descriptions I've heard here, have involved a group of people who have used their own telescope or just put them under their sole control for a large spread of time. I went through the statistics of that also at one time and found that over half, or roughly half of the discoveries that I've come across have been made by people who had a telescope under their sole control. Surprising things that you see which you need to check up on, and at national centers very often you are unable to check up on those because you have your three or four nights and then you go away. By the time you come back six months later the facility has been changed in some way or another, upgraded, it works better, but it's not the same instrument anymore that you had before.

B. Burke: Have you ever had any serious problem of that sort?

M. Harwit: I've never made a big discovery! One key example is the discovery of stellar parallax by Bessel in 1838 where he writes in his papers that when his telescope was first set up there were important things that had to be taken care of and important observations that had to be conducted. It was only after three or four years that he was able to get the telescope for a whole year in order to be able to carry out these observations night after night that would allow him to see this very small effect. Now one can't patronize a telescope at a national center. I don't have any good solutions for this sort of thing, but I think it is a type of limitation on just some of the most creative types of observations that are undertaken.

B. Burke: I think Joe Taylor's pulsar received all the observing time needed at Arecibo to carry out a very long term program, and undoubtedly they were changing equipment. It came on too late to be in your book, but I think it .....

M. Harwit: I think the statistics show the problem is that ....

D. Stinebring: Joe Taylor provided his own receiving and analysis equipment. He just rolled it in and used the dish at Arecibo. And he couldn't have tolerated the sort of month by month changes

B. Burke: But he was able to roll his own equipment in.

R. Ekers: Can I suggest that something slightly different than whether the telescope is taken to pieces or not. I think the people who are going to have the most difficulty are those using the telescopes the way the national facilities usually provide them, those who are not familiar with the instrument or the field. They are expecting you to provide it at a level where they can do astronomy. Now I think it is extremely difficult for them to detect subtle effects. That's more important perhaps.

Hanbury Brown: Martin Harwit shows, I've read his book some time ago, I thought you demonstrated in your book that no discovery has been made at the national centers. That seems to be a very appropriate point to make at this place.

J. Findlay: There's an important point here. First of all, one has to understand national centers and what their function is. You haven't got the function quite right, Martin. Secondly, they are a danger. Who was it who mentioned the dangers to freedom of research? National centers can be a danger you must watch for. By building big instruments, by determining the total means of doing research, they're a vengeance. They must be understood and they must be used correctly, and they are not! Is that fair coming from a member of a national center?

G. Burbidge: Martin, are you saying that it's different in physics? Because most of the discoveries that have been made in high energy physics over the last twenty years have been made in national or international centers.

M. Harwit: I honestly don't know. You may be right that it's true in physics also.

G. Burbidge: No, it's not true. It's the other way around in physics.

M. Harwit: I really don't know.

F. Haddock: There's no other choice.

G. Burbidge: That's right, but really he's raising a very important point because he's saying that he won't make a discovery with the VLA in the next ten years.

J. Greenstein: John, I'd like to say something. I'm a well known enemy of the national centers, although I've done my best to help to build them up. It's a very contradictory position obviously. It's clear that the National Accelerator Lab does good work and there's no other way to do very high energy physics than that of CERN and so forth. It is necessary for astronomers to know how to bring together the original idea type or new instrumentation type people that "the universities" are supposed to represent but don't necessarily do.

Remember the 1970 Academy survey so extensively criticized by Martin in his book. I will point out that every recommendation I can remember was schizophrenic. It said, "Build the national centers but provide university facilities." It is clear that the National Accelerator Laboratory has a fair competition for the use of a very large fraction of their resources in which, I don't know, ten, twenty, thirty million dollars per experiment, is allotted to a university team to build the apparatus which is brought there. The teams have to move bodily and live two or three years and then get their quarter or sixth of the time on the beam. We're not used to it; we are the individualistic buffalo. We are an uncooperative bunch as a group, and really it is inevitable that the very large apparatus is going to get stuck into this national center kind of pattern. Unfortunately, nobody's got the brains at the same time to use the power and resources of the center to make sure that the flow of new ideas is continuously renewed. There is a solution but it's just not happening. Maybe just hunting around will oscillate eventually and come to the right answer.

I was brought up in the free enterprise system where only millionaires endowed university observatories, and we all got 30 nights a year. And it is interesting, let me just pursue that point. I still don't understand your discovery definition, and that I think is part of the answer. There are no discoveries going to be made in a national center which is built with a big piece of apparatus with a lot of auxiliaries. You're just going to do analytical work. I don't think that's bad. I think Kitt Peak people have done extremely good astronomy; the visitors have done good astronomy; Cerro Tololo people have done outstanding astronomy. Those are optical examples, I won't talk about this place. I have to get out of here first! It can be planned, it has to be planned, it's just that we're all a little bit bone-headed. I don't know how much is spent here. Mort, honestly, about how much is spent?

M. Roberts: At the entire observatory, or at Green Bank?

J. Greenstein: The whole thing.

M. Roberts: The whole thing is $15 million a year.

J. Greenstein: Fifteen! With the VLA. Peanuts, peanuts! That's not decisive, you see, on the scale of optical astronomy you double that.

M. Harwit: The national centers have on the average, I think, fairly regularly obtained 2/3 of the NSF funds.

J. Greenstein: That is quite acceptable, if only the division between the national centers and the entrepreneur radicals in individual small groups can be balanced.

G. Burbidge: *Come on, Jesse!*

J. Greenstein: *Communist!*

You're hitting on an important problem. It really isn't the subject of the conference, but it is terribly important. I'm not going to sell it all over again - I'm tired of it. Bernie, did you go through it in the 1980 survey?

B. Burke: *Only on one of the panels, fortunately.*

J. Greenstein: *There ain't no solution!*

RADIO ASTRONOMY: THE PROGRESS OF A TECHNIQUE-ORIENTED DISCIPLINE

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It is a historical fact that radio astronomy started and grew as a technique-oriented discipline. This has caused organizational worries in the International Astronomical Union, which structures its commissions largely by program area. Commission 40, on radio astronomy, has been an occasional target for elimination, since the governing council has noted that radio astronomers are interested in galaxies, galactic structure, the interstellar medium, planetary nebulae, HII regions; planets, cosmology, the sun, astrometry, and virtually every other commission of the IAU. So far, Commission 40 has been protected on the convincing grounds that their meetings were among the most exciting and well-attended of the IAU. United by a technique, or rather a collection of techniques, rather than by a problem or a set of problems, the radio astronomers continue to probe new areas, a process that shows no signs of becoming obsolete.

A definition of "technique-oriented research" is in order, to avoid the impression that it is any research that is pleasing and productive. This is a good debating tactic, but a more precise definition can be given. Technique-oriented research starts from the motivation given by the existence of a powerful tool or technique, generally new, that is then applied to the problems suggested by the ingenuity of the observer. Obviously, the work frequently takes unexpected turnings, and the equipment may need modifications, or totally new equipment might be needed, to follow up the work properly. The style still contrasts with "problem-oriented" research, which starts from the well-posed problem. The necessary instrumentation is then begged, borrowed, or stolen. One might say that in problem-oriented work, the problem is looking for a solution, while in the technique-oriented case the solution is looking for a problem. In this paradox lies a powerful promise. The real problems, instead of being cast in erroneous form, are still waiting to be posed. This is where real science starts, the process of discovery from which new world-pictures will emerge.

In order to sharpen the discussion, let us consider cosmology as a well-documented historical example. The grandeur of the subject has brought forth a marvelous collection of ideas, often couched in beautiful language. Consider this beautiful and stirring quotation from Shelley's "Prometheus Unbound":

> *"He gave man speech, and speech created thought*
> *Which is the measure of the universe."*

The final words were used by North as the title of his history of the development of cosmology, and introduced his final section, the philosophical overview. Despite the grandeur of Shelley's words, the quotation is misleading; it is certainly not wrong but is just as certainly incomplete.

Speech and thought are not the only attributes of man; perception and action are equally important, and our thoughts are changed by these, providing feedback for further actions. We do not think and act from abstract principles, since we learn about the universe and guide our actions by our sensory perception, and through the instruments that are the extension of our senses. The course and quality of science is determined by these interactions, and perception, thought and action are inseparable when we take the measure of the universe.

Pure thought, with only slight regard to the facts, has had a curious effect on cosmology, the most ambitious and least certain branch of astronomy. Consider the history of the cosmological principle, invented by Milne, who derived inspiration from Einstein's dictum:

> *"All places in the universe are equivalent."*

Milne amplified this Delphic statement with a much more explicit definition:

> *"Not only the laws of nature, but also the events occurring in nature, the world itself, must appear the same to all observers, wherever they may be, provided that their space-frames are similarly oriented with respect to the events which are the subject of observation."*

The Milne definition gives uniformity in space the emphasis, but Bondi and Gold generalized the concept to both space and time in lapidary form:

> *"The universe presents on the large scale an unchanging aspect."*

This last, most elegant postulate, the "perfect cosmological principle," was dismissed by Penzias and Wilson's famous measurement of the cosmic microwave background. Ironically, that experiment was guided by no such grand concept, but was conceived and carried out under the influence of more practical considerations. Penzias and Wilson found themselves with access to a superb radiometer and antenna, at a laboratory whose facilities could give strong support to their work, and the measurement had never been made before.

The history of Milne's more restricted cosmological principle is more convoluted. There was little evidence for its truth at the time of publication 50 years ago. (Radio Astronomy and the Cosmological Principle share the same anniversary!) Hubble paid only slight heed to the theorists, and his early work - the establishment of the extragalactic distance scale, and the proposal of the Hubble law progressed with no known support from theoretical constructs. Indeed, Slipher's discovery of large recession velocities for galaxies was an earlier contribution that sprung from an empirical urge to use a modern tool (the spectrograph) to study puzzling objects (the diffuse nebulae). From our present perspective it can be hard to understand how Slipher's work, known generally to the theorists, gave so little support to the cosmological theories derived from general relativity. North gives credit to Lemaitre for the modern synthesis in 1929. Robertson, who was a neighbor of Hubble, came close in 1928, in a paper in which he cites the work of Slipher and Hubble.

According to North, however, Robertson's work at that time was directed at a false issue. Einstein's theory of general relativity is not considered to be easy even today, and in the 1920's the situation was much more difficult. Even the interpretation of redshifts as Doppler shifts was far from clear, and there were no textbooks to guide the student. Einstein had originally thought that his theory specified that the universe must be homogeneous, isotropic, and unchanging - a steady-state universe. This violated the known facts flagrantly, but was also untenable on purely theoretical grounds, since such a universe would be in unstable equilibrium. When De Sitter showed that other solutions to Einstein's field equations existed (the unfortunate Friedman, who developed the full range of solutions that now are used, seems to have been paid slight heed), the elegance of Einstein's concept vanished, and the cosmologists had to come to grips with a real, observable universe. They seem, however, to have largely ignored the observers. At the very least, the cosmological theories did not provide much guidance to the observing programs.

M. Harwit: Bernie, could I differ on one point? I think there was a cosmological inspiration for the formation of chemical elements. Geoff isn't here right now but the whole Burbidge, Burbidge, Fowler and Hoyle impetus came from the requirement in a steady state theory to manufacture all the chemical elements. I think there is a need in a cosmological model to explain the existence of chemical elements.

But the only known cosmological fact was the helium to hydrogen ratio figure of one to four. The heavier elements are not cosmological; their origin is cosmogonical.

M. Harwit: No, but in the steady state theory you would have had to manufacture them someplace, and the only place you could manufacture them in the absence of a big bang was within the stellar interior, and that was, I think, a starting point for Fred Hoyle's work which then brought Geoffrey and Margaret in.

Let us now proceed to examine the progress of astronomy in a far more local context - our own solar system. The example par excellence is provided by the history of solar radio astronomy. Edlen's explanation of the coronal lines required that the corona be a hot gas with a temperature of the order of a million degrees. It is easy to show that the Bremsstrahlung from a hot corona would be seen most easily at radio wavelengths. Radio technology at that time was more art than engineering, but the measurements could have been done by a talented and motivated radio physicist. Grote Reber's experiments in 1940, in fact, came close and he did detect the quiet sun in 1944. The solar theorists, however, seem not to have urged their electrical engineering colleagues to try. No great fault should be attributed to them, however, because events of great press and moment rapidly outran them. The Second World War deflected astrophysicists from their work, and when the war ended a brilliant young crew of physicists and engineers put an enormous new technology to work. Hey had found the powerful nonthermal solar busts, Reber had detected the coronal radiation, and Southworth had measured thermal radiation from the lower parts of the solar atmosphere. The Sydney and Cambridge groups, in the decade following the war, not only measured the coronal temperature, but they described a wide variety of fascinating and puzzling phenomena related to plasma interactions and high-energy phenomena that had been completely unpredicted. The unifying feature of all the activity, however, was the use of new tools to explore the unknown. New inventions followed, of course, guided by the early work. Nevertheless, they were developments that exploited the new technology, guided by the desire to measure more sensitively, at higher resolution, with full polarization, inside smaller intervals of time. Few theoretical considerations entered. Indeed, the theoretical knowledge at that time might well have been grievously misleading.

The planets themselves have turned out, when examined by radio means, to have remarkable properties that were quite unanticipated (perhaps with the exception of Mars, which many conventional planetary astronomers would have thought the most interesting). When Ken Franklin and I detected the decametric radio bursts from Jupiter, we were using an instrument that had been designed, not to study the solar system, but galactic and extragalactic radio sources. Looking back now, that instrument should have been designed with rather different properties if it were to meet its original objectives, but optimizing its design to study radio sources might well have reduced the probability of its discovering radiation from Jupiter. Shortly thereafter, Cornell Mayer and Russell Sloanaker found that the apparent radiometric temperatures of Jupiter and Venus at decimetric wavelengths were higher than they should have been. Again, they were using instruments that had not been built specifically with planetary astronomy in mind. Nevertheless, I think that there was an inevitability in their case, since Jupiter and Saturn were *a priori* detectable projects. It was primarily their detectability, and the immediate access to suitable instrumentation that guided them. The radio results for Jupiter were brought into a larger context by James van Allen's discovery, on the first Explorer flight, of the radiation belts that bear his name. The experiment was remarkable because, like Sherlock Holmes' dog that did not bark, the radiation belts were discovered by the failure to count energetic particles. The inappropriate experiment, creatively interpreted, led to a new insight and within a short time it was clear that the Earth and Jupiter were alike in possessing rich belts of energetic particles trapped in off-axis, inclined magnetic dipole fields.

The ferociously high temperature of Venus was a surprise, but radar astronomy provided its own chapters of excitement. All the textbooks said that Mercury (and probably Venus) rotated synchronously with their orbital period, keeping the same face to the sun. This was a reasonable concept, since tidal friction would inevitably drive the planets towards this condition. It is interesting to speculate what would have happened if Gordon Pettengill had proposed that a radar be built expressly to measure Mercury's rotation period. The reviewers would undoubtedly have been severely critical of such a redundant experiment. He followed, of course, a more reasonable course. The reflectivity of Mercury at radio wavelengths was not known, the results could have interesting consequences for our knowledge of the character of the surface, and he had access to powerful radar installations (built for quite other reasons). A skilled experimenter, using new equipment in a reasonable but not dramatic program to measure what was not known, obtained results that demanded the rewriting of every astronomy textbook.

At this point, it would appear that theorists might be regarded, if not hindrances, at least neutral quantities in the history of astronomical discovery. This would be an improper conclusion, and suggests itself only because there is a widespread belief, particularly among theorists, that they are the grand strategists of astronomy. While this is demonstrably not the case, the theorists do serve as our master tacticians. The anomalous phenomena are quickly appreciated, and after an initial flurry of theoretical activity, mostly ultimately wrong, a unifying picture emerges through the partnership of theorist and observer. Nor is the erroneous character of most early speculative theory to be scoffed at: all of experimental science is characterized by false starts, wrong turnings, and naive concepts. Furthermore, one has to recognize that from time to time a new grand synthesis arises, and new general concepts emerge that have a powerful effect on observer and theorist alike. In Newton's Principia, both the axioms and postulates at the beginning of Volume I, and the Scholium of Volume II provide standards to which every theorist can aspire.

The record of radio astronomy is clear. Quasars were not found by the desire to find black holes or by the need for long-distance cosmological probes. Pulsars were not discovered as a result of a program to find neutron stars. (On this score, no theorist should make the claim that clear predictions had been made. I was present in the early 1960's at a National Academy talk on hyperon stars given by Ed Salpeter, with Hans Bethe in the chair. Bethe pressed Salpeter after the talk to come down with a definite answer to the question "Do neutron stars exist?" Salpeter would not give a direct answer.) In addition to Harwit's discovery space, I suggest that theorists have a speculation space in which they try to cover every possible outcome.

Historical examples will present problems and ambiguities, of course. Was the discovery of the 21-cm hydrogen line a product of problem-oriented or technique-oriented research? Either case can be made. As the project developed among the Leiden school, it was a classic example of problem orientation. Jan Oort framed the problem, Hank van de Hulst articulated the solution, and Dutch engineers were engaged to convert the concept to practice. Just as certainly, Edward Purcell and Harold Ewen were technique-oriented. Purcell was one of the world's foremost radio spectroscopists, and when I was a graduate student I attended the first joint Harvard-MIT Colloquium when he described the work. The main emphasis was on the experimental method (which became the method of choice throughout the world) and on the question of whether the line would appear in absorption or emission. I am less familiar with the genesis of the Australian work, but from my acquaintances with the principals, I would wager that the technique drove the science.

Does that mean that technique-oriented research proceeds as a blind groping in Martin Harwit's discovery space? The answer must certainly be negative. Scientific taste is an intangible but essential ingredient of all good research, and technique-oriented research without good taste amounts to little no matter how ingenious or elegant the apparatus. There is almost always an initial purpose: Jansky's task was to understand the sources of interference with radio communication, and Jocelyn Bell wanted to study interplanetary scintillation. There is a required set of attributes - an openness, an awareness, a readiness to understand the observed phenomena, and the preparation to relate the observations to the physical world. There also has to be a readiness to move to other methods for the follow-up. No one could imagine the study of radio sources without the optical observations that provide identification and a description of the physical surroundings.

One might argue that the time for exploration is past, and that the radio astronomers must now change their ways. Recent history (post Harwit) suggests otherwise. Joseph Taylor's binary pulsar 1913+16 is now a convincing verification of the existence of gravitational waves. True, we only see the smokestack of the power plant, and measure the output only by watching the rate of shrinkage of the fuel supply, but the evidence is now overwhelming. The small eccentric orbit is not one that would have been predicted on theoretical grounds, but is ideally suited for the test.

In another area, consider the progress of millimeter-wave studies of the interstellar medium. Giant Molecular Clouds were not predicted. Bipolar mass flows came as an unexpected feature of star formation. Indeed, the entire question of star formation and the genesis of the solar system is so convoluted, with new and difficult facts turning up regularly, that any programmatic approach is bound to fail.

Let me turn briefly to the other side of the picture: the grand failures of problem-oriented research. The most notorious is probably Kapteyn's program to understand the structure of the galaxy by systematic star-counting. The program was carefully planned, and failed utterly.

In a more controversial vein, let me also assert that the Hubble program is not yet successful. More exactly, the aim of determining the form of the cosmological scale factor $R(t)$ has so far fallen short of any meaningful understanding. The classical approach is to expand $R(t)$ as a power series, with the first-order term being the Hubble constant, and the second-order coefficient being specified by the deceleration parameter. After 50 years of work we can only state that reputable values of H_o range from 30 to 120, and the deceleration parameter is still uncertain in the sign. The Hubble law is still the one lasting result of his program, still quantitatively uncertain, but its functional form secure.

The larger claims of cosmology are still on shaky ground. One of our colleagues, in reviewing another field, wrote that he refused to take any subject seriously when everything that was known about it could be written on a single 3 x 5 card. I tried to do this for cosmology, and found that it was not difficult; the printing could be large, and there was room at top and bottom for additional facts:

<u>The Cosmological 3 x 5 Card</u>

1. All cosmological indicators appear to be isotropic.
2. Non-communicating regions in the universe are physically similar.
3. Radiation background is a black body having an apparent temperature of 3° Kelvin.
4. Galaxies in our neighborhood obey the Hubble Law.
5. The curvature of space is small.
6. Radio source counts diminish anomalously at low fluxes.
7. The age of the universe is about 15 billion years.
8. The local abundance of helium is approximately 1/4 by weight. (Only half a fact because some of that helium is going to be non-primordial).
9. Local physics appears to work on the largest scale.

A. Moffet: How about "It's dark at night!"

That was included in statement number 3.

M. Harwit: Bernie, you should write a 3-page book called, "Astrophysical Concepts."

I always did think that you were a bit long-winded there!

Note how several items came from straightforward radio astronomy programs. After a number of false starts and wrong turnings, it is now clear that the radio source counts demonstrate that there is an edge to the universe - more exactly, that the radio sources evolve in time from an as-yet undetermined set of circumstances in the distant past. The famous microwave background work of Penzias and Wilson, Dicke, Peebles and Wilkenson showed that the big bang scenario was almost certainly correct.

What are the reasons for the effectiveness of technique-oriented research? Firstly, the work is in the hands of experts, who can use the equipment effectively. Secondly, the chances of technical advance are optimized because the insight, and realization of new needs, the technical knowledge, and the means for reducing ideas to practice, are in the same hands. I have already mentioned the freedom of thought and lack of preconception that flows from this mode of working. More philosophically, one can maintain that a little untidiness in scientific planning is a good thing, since one cannot program discovery.

What of the future? Is the radio astronomy technique a mature technology now? I believe that the answer is "not yet." Ground-based work is still extending observation to shorter, sub-millimeter wavelengths, and there are active plans to explore those wavelength regions that are presently hindered by atmospheric absorption. In another dimension of Harwit's discovery space, VLBI is not even close to reaching maturity with the VLB array. Baselines longer than an Earth diameter are technically feasible, and we are currently discussing the possibility of an international project that would give an aperture approximately three Earth diameters in size.

Nor do I scorn the theorists completely. I started with references to cosmology as an example of misleading scientific guidance. Nevertheless, the cosmologists continue to work, occasionally generating good ideas. Recently, GUT Theory - Grand Unified Field Theory - was transferred in an interesting way to cosmology, with specific reference to two cosmological facts: the similarity of non-communicating parts of the universe, and the relatively small present curvature of space. The theory even has a definite prediction: Ω should be unity. (The best values point toward $\Omega \simeq .1$, but the measurements are still less than convincing.) With this admission of the fallibility of the observer, let me give equal time to the theorists by quoting a recent remark by Alan Guth, the inventor of GUT cosmology:

> *"At that time I very strongly believed that cosmology was the kind of field in which a person could say anything he wanted, and no one could ever prove him wrong. I am sure that truth does not change with time, but after three years of working in cosmology my prejudices about the subject*

have completely reversed. It now appears that it is very easy to show that a cosmological scenario is wrong, and far more difficult that I had ever imagined to develop a totally consistent picture."

The radio astronomers can take courage from the promise of these words, secure in the knowledge that their technique will be in the forefront of the complexity-generation process.

R. Price: Bernie, should we be worried that by and large our funding mechanism for individuals is based on the approval or likely approval of proposals that almost have to be problem oriented?

Oh, I think we must be worried all the time. Yes!

M. Harwit: Well, isn't it true though that the theorists do have one disadvantage. We have to supply them with facts before they can construct a theory. Astronomy in many ways is very young (people keep saying we're the oldest of the sciences, but we're really a very immature science), so the weakness of the theorists and the weakness of their position, really is, I think, that there are so many uncertainties. So in the areas where we haven't made key discoveries it is not really that surprising and we have to make these discoveries and that then we have to grope; and the best way to grope is by using sophisticated techniques.

I think I would agree with that.

Addendum:

After the session, Maarten Schmidt commented that the quasar statistics as a function of redshift show that they, too, were more abundant at earlier epochs. This can, therefore, be admitted as a tenth entry on the cosmological 3 x 5 card.

PROGRAM-ORIENTED RESEARCH[1]

H. van der Laan
Leiden Observatory

Of course Bernie is playing a home game; for me this is an out-of-town game, and I have the handicap of speaking in a foreign language!

1. Beware of the fruitless dilemma. - Observational research depends for its success upon the quality of the instrument and the interpretive skills of the analyzing astrophysicist. There are times and circumstances when the instrument gives one a decisive advantage and the history of radio astronomy abounds with examples of discoveries made because a technical advance made possible the exploration of a new domain in parameter space. In infrared astronomy the IRAS survey, mapping the whole sky in the 10 to 100 micron range with an unprecedented combination of sensitivity and angular resolution, is the most recent example of what may be designated technique-oriented research.

Of course, once basic surveys have been completed, calibrated and made accessible, a host of projects begins in subdisciplines ranging from planetary science to observational cosmology. Their direction and success depend on astrophysical insights into the best ways to proceed and on judicious use of instruments and techniques far different from the initial surveys. Such research may be thought of as program-oriented.

Surveys are only one example of technique-oriented research. If a group has exceptional skills in one technical area and an instrument plus infrastructure to extend, in one wavelength region, the sensitivity, the dynamic range or the resolution, spatial, spectral or temporal, it may become very successful by exploiting its privileged access to other areas in parameter space. This can lead to advances and discoveries in very different parts of astrophysics, as different as pulsars and gravitational lensing of quasars. It is great to have instrumentally unique capabilities; skimming cream in new fields is exciting. Nevertheless, such astronomy can lead to very fragmented research, hit-and-run exploration which leaves the deeper cultivation to carefully structured multispectral programs.

Astronomy now is unthinkable without both accents, often within single institutes and teams. As more and more of the electromagnetic spectrum is opened for astronomical exploration, the need is to single-mindedly improve instruments in each wavelength regime. Their builders will be primarily motivated by the opportunities newly extended techniques afford them. The

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[1]Note from a delinquent author: My talk in Green Bank in early May was profusely illustrated with slides and overheads. This meant rewriting it for publication purposes. The demands of an institute and its associated network upon its chairman, in these economizing times, reduced the good intentions to this inadequate summary.

astronomy community-at-large wants, needs and gets user-friendly observatories where, when the instrumentalists have had their go, a variety of programs can be pursued, programs informed and structured by astrophysical questions requiring several techniques for their solution.

2. Program-oriented research needs common user instruments. - An obvious feature of program-oriented research is the need of a single team engaged in a well-structured program to have access to a variety of facilities. In the oral version of this talk, I illustrated this by a brief sketch of five Leiden Observatory programs.

(i) The cosmological evolution of active galaxy populations.
(ii) The evolution of active galaxies in clusters.
(iii) The structure and dynamics of Messier 31.
(iv) The structure and evolution of OB associations.
(v) The structure and radiative processes in dense to diffuse molecular clouds.

All these programs require more than one telescope or technique for their proper development. Each team has to learn to use these instruments, to understand their characteristics, to apply calibration and correction methods. Clearly this is possible only if well-run user-friendly observatories provide comprehensive and transparent services to aid even technically naive astronomers. Clearly there is a danger that black-box astronomy will separate the interpreting astrophysicists from the astrophysical phenomenology. Only a team with a spectrum of skills can bridge the gap. Obviously this requires new styles of science management, analogous to earlier developments in high energy physics, both for (inter)national observatories and for the, usually university-based, user groups, as well as for their interactions.

3. Some advantages of program-oriented research. - Such research is systematic, therefore able to make long-range plans for the development of people-skills and of expensive facilities. The development of galactic structure research in Holland is as good an illustration as any of what I mean. The story is well-known and some interesting projections of it are found in the book we wrote for the program's master-mind, Jan Hendrik Oort.[2] Program-oriented research can have very strong influence upon the development of techniques and instruments, precisely because astrophysical problems set very specific observational targets which serve as technical benchmarks and exercise strong motivation for technical skills. The development of One Mile and Five Kilometer Telescopes at Cambridge and the design of the SRT at Westerbork are examples of technical responses to program-generated demands.

Technique-oriented research has the same multiplicity as the techniques at its base. It is therefore very difficult to set priorities, to choose among all possible developments. National funding agencies are thus subject to so many pressure groups. On the other hand, program-oriented research lends much better criteria of balance and choice to the policy maker. The

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[2] "Oort and the Universe", H. van Woerden, W. N. Brouw & H. C. van de Hulst (Eds.) REIDEL, Dordrecht, Holland, 1980.

famous ten-year reports of the U.S. National Academy of Sciences, of which the Field Report is the most recent one, are strongly problem-oriented in the motivation of technical choices and priorities.

Serendipity, by the way, is fairly indifferent to the context of program- vs. technique-oriented research. As long as astute people use state-of-the-art instruments to penetrate domains of parameter space hitherto unexplored, discoveries will come our way, both anticipated and complete surprises.

4. Some requirements of program-oriented research. - These are among others:

(i) Access to state-of-the-art telescopes right across the spectrum and the means to use them (travel money, user-friendly services at (inter)national observatories).

(ii) Data processing and image processing facilities of sufficient quality and quantity at or near the university base.

(iii) Astrophysically and technically all-around groups or multigroup teams (instead of: a radio group, an X-ray group, an optical group, a theoretical group, etc.).

(iv) For small groups (minimum is $\sim$ 6 experienced astronomers, including several theoreticians/interpreters), a large throughput of young people, especially Ph.D. students and post doctoral fellows.

(v) Good research - and people management at the local level combined with informed science policy at the national level (the latter providing continuity to long term programs, instead of only zigzag response to the fashions of the day).

M. Harwit: One of the things I have enjoyed most in looking at the history of the development of modern astrophysics is the real pleasure one gets in seeing how different people employ different styles that you call tastes. One of the things that gives one confidence about science continuing to progress is that those people who use one style might very well get stuck because it just isn't the right method at a given time, but there will be other people who will have a different approach and who take over the field for awhile and really clean up. So it is really encouraging that there is a difference in points of view here of how one ought to go about it, and each feels passionately that his way is the best one. I think that is the way science has always progressed, but when we haven't had that, when it has been monolithic, is when you really have to worry. If we all agreed on what we ought to be doing I think it would have been an awful disaster.

R. Ekers: I can make a comment along the same vein which relates to previous discussions and also to what John Broderick was saying about the buffalo syndrome. I think the way to get the best advantage of the buffalo effect is through international research. National barriers tend to confine the buffalos very nicely; there are styles and fashionable things which which tend to be related to a country. So as long as every country is doing it different you get the advantages both.

G. Burbidge: Rather as an international buffalo syndrome.

R. Ekers: Less noticeable.

G. Burbidge: Buffalos can swim.

R. Ekers: For example, in our field when the US was deciding that the smelly interstellar molecules were just the thing that we should be working on, the Netherlands radio astronomers were measuring the rotations of galaxies, and almost nothing was happening in this field in the US. Now because of the instrument driving the buffalo herd in a different direction, all of a sudden everybody wants to measure rotation curves of galaxies in the US now, and people are starting to lose their interest in interstellar molecules.

G. Burbidge: I challenge you to show me that in any one of these fields there are different points of view in the different countries; but maybe they set about it in different ways.

R. Ekers: Martin's point about the styles is certainly true. His style is very different than yours.

D. Wilkinson: Particularly at universities we take great pride in turning people loose. Some people say let them drift, which often happens, disastrously, but yesterday time after time we had speakers get up to explain or describe a discovery and the first thing they said was, "When I arrived at the institute, my assignment was to...", and then they went on and told about that. My first reaction was "My goodness! They were assigned the problem, told what to do!" But then they went on and told how out of that their good ideas came through; Dicke invented the radiometer, etc. There were numerous examples of exactly that happening and we tend not to do that anymore. I'm not sure that is healthy. We don't tell post docs what to do when they come.

B. Burke: Oh, yes we do.

J. Broderick: We try to, but they never do it and they wind up doing their own thing which is absolutely nothing! I'm impressed you must have well-trained buffalos involved, then you could have such directed efforts. I don't see that it is different; it is entirely technique oriented. You are using a whole bunch of techniques on a given class of objects.

R. Ekers: No, I think there is a clear difference. I would say a really technique-oriented radio astronomer is one who does a very broad spectrum of astronomical problems using only one technique. You have people who try and dabble, if you like to use that word, in all fields of astronomy doing only the radio piece. That is quite different.

P. Crane: It seems to me that both types of approaches can lead to a very stifling environment, at least in the majority of the community. Although I do not think this is the case in the radio astronomy community, at least from the outside, that is the way it appears to me in the high energy physics community where hundreds of people who are actually constructing the detectors which take longer than a graduate career. Though I don't know that that is the case, it strikes me that it must be the way it is. I think you *have probably reached the similar situation in the case of an extreme technique-oriented group.*

IMPACT OF COMPUTERS ON RADIO ASTRONOMY

Sir Bernard Lovell
Nuffield Radio Astronomy Laboratories

The only reason I am standing here to do this is that Ken Kellermann sent me the program. I suggested this important item which was not on the original one.

There are just three points I want to make. I have seen the evolution of a major observatory, Jodrell Bank, from the time when there were no computers until now it is completely monopolized like most of the major observatories by computer techniques. When I say there were no computers in the early days of Jodrell, I include the design of the major telescope. The 250-ft aperture steerable telescope was designed by using the slide rule, and a hand calculating machine. The interesting thing is that had it been designed by a computer it would probably have been destroyed in a hurricane which we had about 7 years ago. Designing it the hard way meant that a tremendous amount of redundancy was put into the structure.

However, that is not really the point I am here to talk about. If you look at the history of Jodrell Bank, for example, it occurs to me that the work done, such as that described by Hanbury Brown and Thompson this morning on the angular diameter of radio sources with increasing baselines, owed absolutely nothing at all to computers and could well be considered to be the most important output of the establishment. In later years, I have been both very impressed indeed by the results that have been obtained by use of computers but also increasingly worried. I'm sure many of you will have examples of discoveries which probably would not have been made if the modern computer techniques in programming had been applied. It occurred to me this morning for example that Hanbury Brown's example of when he and Twiss saw the scintillations and noticed that they were correlated by eye, that would certainly not have been apparent on a computer program.

I'd very much like to hear Jocelyn Bell's comments on whether or not pulsars would have been discovered in that particular equipment if the program had been digitized and coming out on tape. I was actually the chairman of the committee that received Martin Ryle's application for that, and she was quite right, it cost a very small amount of money. The total application was 38,000 pounds, but I am sure that if the committee had been faced with a proposal for a very complex program involving expensive computers it is unlikely it would have been financed. The important point, and the question that has to be answered, is, if computers had been used on that type of program, how long would it have been before pulsars were discovered? Would we still be without the discovery? The other problem is this, that the very fact that you have a major institution completely computerized means that everybody has to use these systems. It is extremely unlikely that proposals for work will be accepted which do not use the major facilities of an establishment. Therefore, this is directing the work of every observatory into narrower and narrower channels.

My second point concerns the effect on the young people; the students. I am sure we are typical, in that we have post graduates coming for three years to do a Ph.D., and instead of working with the equipment and making observations for months, they now get enough work for their Ph.D., in MERLIN for example, by only a few nights' observations. Further, they really do not understand much about the equipment, they just get the output.

The third point I want to make is a more fundamental one, and really is addressed to the theorists. It is the direction in which the use of computers may be forcing the human brain. I was very much struck by the closing remarks by Stephen Hawking in his address that he gave when he was made professor at Cambridge a few years ago. He was talking about unified field theories and his remark was that the amount of time which we had to discover a unified theory might be very limited. He suggested the end of the century simply because all the work was being concentrated on what could be done by computers. Now if it is the right line, that is fine, the answers will be found, but if, for example, the logical structure of the mathematics is wrong or inadequate, then the human brain will be directed into entirely the wrong channels. In that case it may be a very long time before the brain is divorced sufficiently from a wrong direction to achieve the right answers. Well, that is really what I wanted to say, and I hope there is time for some discussion on these points.

M. Damashek: At some risk, I might suggest that that last problem of being directed down the wrong track is due essentially to the fact that computers are program oriented; they are not technique oriented. You have to know just where you are going and just what you are doing before you can make the computer do it, because the computer won't provide you with any concept of the signals per se.

M. Price: But these days we have the problem that we have computers which are black boxes in most experiments. Black boxes almost preclude by definition serendipity because for optimal results you have to program them very narrowly, and so they constrain all of the phase space that we need to make these discoveries.

R. Ekers: It seems that somebody had better take the other side of this argument.

R. Wilson: I disagree with that. Anytime you make a perfectly matched filter to your problem, you are going to get the optimum signal to noise ratio in your problem and learn nothing about anything else. The computer merely allows one to do that very easily most of the time.

B. Burke: I am going to answer for Jocelyn Bell. I'm going to make a diagram on her digitized equipment. The signal comes in. There's an A to D converter. One surely then autocorrelates. One surely then performs the Fourier transform. The scintillating sources have a spectrum that looks one way, pulsars have a spectrum that looks differently. And so you can discover pulsars with Jocelyn's digitized system. Now I've left off a box that is always part of this digitized system, I guess we can call it the program control, and it is not the fault of the computer, it is the fault of the person who designs this program control that will determine whether you discover pulsars or not, because if it is gated to only do this calculation when there is a favorite source coming by, you surely will not discover anything, it is too well matched a filter.

H. van der Laan: I think another example is that it enables you to render very large data sets visible and something that is easily inspected using all patent recognition capacities that we have. For example, the Westerbork data set on the Andromeda Nebula contains I think 2 Gigabytes of information, so a movie has been made. You can advance through successive channels, and you can render them completely visual. You can enhance the contact in colors, and by visual inspection of that data sets you can find all sorts of thing which you could never find if you only had say contour maps or plots or another representation of the data. In this case such work would simply be impossible except by the computer rendering of the data in a humanly comfortable visible form.

D. Stinebring: I think one of the unspoken objections to the introduction of computers is how much time and effort it is taking. People of the older generation are either learning to deal with computers or seeing their students struggle with computers. I think that is just the start-up time of learning a new technology, learning a new way of approaching problems, and the students and the kids who are coming through school now are just so much more familiar with those techniques that it is not going to be the same drain away from other vital aspects of other vital research tools that I think the last generation has dealt with.

G. Burbidge: That is not entirely true. The technique is often becoming an end in itself. That's the trouble with your generation!

J. Bell: May I speak partly for myself and partly perhaps for my generation!

B. Burke: The present generation!

J. Bell: Yes, all right! I'm beginning to feel my age too. Tony and I never quite agreed about whether pulsars would have been discovered that quickly or not if we had computerized the output. Probably because it would not actually have correlated and Fourier transformed. At least not until we stopped to think about it which might be quite late by that stage. I don't accept the basis of Sir Bernard's argument. Whether or not we use computers today I would maintain is quite irrelevant to whether or not pulsars would have been discovered by computers in 1968. This is not 1968, it is one of the most painful lessons I have had to learn! After leaving radio astronomy I had a great venture in gamma ray astronomy; then I went into X-ray astronomy, and I was involved in one of the most successful satellite projects that there was over the last ten years. I was responsible for operating one of the pieces of equipment and for checking the data that came in. Being a good old-fashioned clot, I started by looking through the computer output page by page. Twelve thousand orbits later, which was about 12 miles of computer paper later, I gave up. And I am very glad I did, because the satellite ran to thirty-two thousand orbits! Not only are we no longer in 1968, but projects go in bigger quanta which incidentally as Harry mentioned makes difficulties in scheduling student projects into these. It also means (and to some extent I regret this, too) that students don't get the chance to squelch around in muddy fields at the Cat and Fiddle, or what have you. And perhaps it means that they are not as rounded as we would like them to be. They don't get hands-on experience in quite as many aspects of the problem, but maybe they are as well rounded, or maybe they are much more appropriately rounded for the circumstances that *pertain today. I really see very big changes in science over the last fifteen years or so, and as Sir Bernard was sitting on the committee that assessed Tony Hewish's grant application, I now sit on the committee that assesses grant applications. And we know that you have got to be forward looking and to quite a significant extent you have got to think big. That's the beasty we are dealing with today and regretting that it is like that is not a constructive way of operating.*

I might say that I wasn't for one moment suggesting that computers were not a good thing. I am very impressed with what is being done. The question is not that. The question is, is there a danger, particularly within the topics of this symposium on serendipity? I mean are they really going to make it even more and more difficult to have the serendipitous discoveries? You may believe that we only have to go on investigating in more detail the things we already know about in the universe. I don't believe that. Do you really believe that the discoveries of the last twenty five years that we have been privileged to witness are the only new things that have to be discovered about the universe? I don't! And the real thing that worries me is that if we go on channeling the research to what can be done by complex computerized equipment and data handling, are we going to miss the new things that are still waiting to be found?

FORTY YEARS OF SOLAR RADIO ASTRONOMY
- A HISTORY OF MAJOR ADVANCES

M. R. Kundu
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Early History (Pre-1942)

Solar radio astronomy started with the discovery by Hey in 1942 of the solar radio emission associated with sunspots. In reality, the search for radio emission from the sun can be tracked back to 1890. In the years following the discovery of radio waves by Heinrich Hertz in 1888, several attempts were made to detect radio waves from the sun. In fact the first idea of radio astronomy was thought of by a very ingenious man - Thomas Edison, really the first would-be solar radio physicist. In 1890, Edison thought that there must be disturbances from the sun which were emitted in the radio spectrum, and he tried to look for them. He had no radio telescope, no radio receiver, no radio technology; however, he seemed to sense even then that a radio telescope would have to be enormous in size, because he decided to use a huge mass of iron ore that was found in a mine in New Jersey. He thought that the solar disturbances could induce detectable currents into this mass of iron ore, so he proceeded to fit great loops of wire around the mine with the idea of connecting them up to a telephone and converting the radio waves into sound. This was a marvelous conception, but it was destined to failure because of the presence of the earth's ionosphere.

In 1894, Sir Oliver Lodge, professor of Physics at Liverpool University, continued this quest for solar radio emission. He tried to detect long wave radiation from the sun, by filtering out the ordinary light waves by a blackboard of sufficiently opaque substance. He did not succeed in this experiment because of too many terrestrial disturbances in the industrial city of Liverpool.

The next unsuccessful attempt was made in 1900 by a French research student named Nordman who, in his doctoral thesis, referred to previous unsuccessful experiments by Scheiner and Wilsing in Potsdam. Nordman used an aerial 175 m long and took his experiment to an altitude of 3100 m, because he incorrectly thought that the absorbing action of the atmosphere could influence his experiment. Nordman had correctly predicted that strong outbursts of radio emission could be associated with sunspot activities. In any case his experiment ended in failure because of the ionosphere.

Between 1900 and 1932 when Karl Jansky discovered radio waves from the Milky Way Galaxy, there was a general lack of interest in further search for solar radio emission. There are two reasons for this: people were discouraged by the early failures and, secondly, by that time there was growing appreciation that the ionized reflecting upper atmosphere was cutting off extraterrestrial radiation at $\lambda > 20$ m. In any case, Jansky's discovery took place almost as soon as the high frequency sensitivity of radio receivers became adequate to detect cosmic radio waves and he would have detected radio waves from the sun, had it not been for the fact that it was a time of minimum sunspot activity and the level of thermal radio emission from the quiet sun was very low at meter-decameter wavelengths. In the years following Jansky's discovery, there had been many reports of high level noise on short wave receivers during high solar activity, but their true significance has not been appreciated. For instance, H. W. Newton in 1936 referred in a paper to the "radio fizzlies" reported on short-wave communication links, preceding the fade-outs known to accompany large solar flares. In 1938, D. W. Heightman (an amateur) came close to a correct explanation when he received during fade-outs a peculiar radiation mostly over 20 MHz, described it as a loud hissing sound and interpreted it as being caused by the arrival of charged particles from the sun. In 1939, two Japanese researchers, Nakagami and Miya, almost discovered the true nature of the radiation when they measured the direction of arrival, including elevation, of the noise at 23 and 17 meters, and although the direction corresponded with that of the sun, they incorrectly concluded that the noise probably originated in or near the E lay of the ionosphere.

In February 1942, Hey made his discovery of the radio sun. Hey had at his disposal highly directive radar antennas at meter wavelengths, good receivers, and most importantly he had the initiative and imagination to trace the cause of jamming of his radar antennas to the presence of unusual solar activity. The same year, Southworth detected the thermal microwave radiation from the sun using microwave radar equipment in a specially planned experiment. Reber (1944) also reported the detection of the sun at $\sim$ 2 m wavelength.

Hey's discovery signaled a radical change in the methods used to study the sun. Until then our knowledge had come solely from optical observations, which refer mainly to events occurring within a thin layer around the visible surface. Since then further discoveries on the nature of the radio sun followed in quick succession. The new results led to a series of new techniques. Each new technique produced new results which, in turn, suggested the next technical advance.

Solar Radio Astronomy at Meter-Decameter Wavelengths

In the first few years following the discovery of solar radio waves by Hey (1946), Southworth (1945) and Reber (1944), several important advances were made to our understanding of physics of the radio sun.

1. The Australian pioneer in radio astronomy, J. L. Pawsey (1946), was making daily recordings of solar noise at 1.5 m and he found that even the lowest base level corresponding to the quiet sun had a brightness temperature of 1 million degree. D. F. Martyn and V. L. Ginzburg independently gave the correct interpretation to this fundamental observation, namely, the quiet sun radiation was emanating from the solar corona where the electron density would be sufficient to render it opaque at meter wavelengths. The implication of this result was great since it meant one could use different wavelengths to explore different regions of the solar atmosphere and that it was no longer necessary to observe the sun's corona at high altitudes. However, the radio astronomers had to wait almost 20 years to do radiophotography of the sun's corona in real time, almost as easily as the optical people with an order of magnitude poorer resolution, but on an almost routine basis.

2. The early radio observations made with simple radio telescopes of limited angular resolution at centimeter wavelengths had already shown that

the undisturbed quiet radio emission was slowly variable. Besides, during disturbed periods transient emissions called bursts occurred in association with solar flares. From decimeter observations of the radio sun during a solar eclipse in 1946, Covington identified the slowly varying component with sunspots.

3. In 1946, the Australian group under Pawsey and the Cambridge group under Ryle for the first time in radio astronomy used interferometers and independently found that the meter-λ storm radiation originated in a small area in the vicinity of sunspots.

So the stage was set for the recognition of the three basic components in the solar radio emission-quiet sun, slowly varying component and bursts. In the meantime Payne-Scott, Yabsley and Bolton found that the onset of an outburst showed a definite progression with frequency, the higher frequencies preceding the lower, and the flare starting earlier than both. It was suggested that the effect might be due to the passage of a disturbing agency outwards through the solar atmosphere into regions of decreasing electron density from which progressively lower frequencies could be emitted. Thus the idea of a plasma hypothesis was already at work.

In the next few years, that is, in the late 40's and early 50's, there was a great flurry of activity for observing solar eclipses at radio wavelengths. The radio astronomers led eclipse expeditions in great numbers. These observations were aimed at obtaining quiet sun brightness distributions. At meter wavelengths, the sun was found to be elliptical in shape. This ellipticity was not a new result - the optical people already knew it - what was new is the fact that the ellipticity was more at maximum of the solar cycle than at minimum. So far there has been no theoretical attempt to explain this phenomenon. It is certainly not due to presence or absence of streamers, since two-dimensional quiet time radioheliograms shows localized enhancements with $T_b \sim$ a few million degrees K that correlate rather well with enhancements and streamers of the K-corona. At centimeter wavelengths the sun was found to have limb-brightening - a result which is by and large still true except at 3 cm and shorter wavelengths where the existence of limb-brightening has been challenged from high resolution pencil beam observations such as those obtained with the Effelsberg telescope. On the theoretical side, the interpretation of brightness distribution led to models of electron density and temperature distribution in the sun's atmosphere.

The late 40's and early 50's can be characterized by another phenomenon in radio astronomy. This was the period when many of the pioneering astronomers turned their attention to the subject of exploring the universe - the Galaxy, extragalactic sources and cosmology. Nevertheless, the sun still appeared an interesting and exciting object of study to many radio physicists, because it presented a unique astrophysical laboratory, close enough to study in detail a great variety of high temperature magneto-active plasma phenomena relevant to many physical processes that occur elsewhere in the universe. Consequently, there was emphasis on new instrumentation designed for the study of specific phenomena. The most important instrumental development at that time was the spectrum analyzer or radio spectrometer first introduced into radio astronomy by Wild and McCready in 1950. That certain radio bursts at meter wavelengths are due to plasma radiation excited by electron streams (type III) and shock waves (type II) was demonstrated conclusively from observations made with this instrument. However, it was not until some 20 years later that electrons and plasma oscillations were actually detected in situ near 1 A.U. in association with type III bursts. This, in my opinion, is one of the most important contributions made by space radio astronomy to solar physics.

It was once again with a new instrument - the 169 MHz grating interferometer built for observing solar activity during the IGY (International Geophysical Year) that the French group discovered in 1957 the type IV burst - a broadband smooth continuum radiation following a type II. Interpreted as due to gyro-synchrotron radiation, this burst observed to be moving at speeds of 2000-3000 km/s obviously represented a plasmoid behind a shock wave. This was the first time a chunk of corona was seen to be moving at high speed, out into the interplanetary space. Consequently, type IV radiation was an important discovery for a long time among the solar-terrestrial physicists. The association of type II/IV bursts with coronal mass ejections was confirmed, when the latter were observed first by a ground based K-coronameter and later by space borne coronagraphs in the early seventies.

The first two-dimensional radioheliograph was built in Culgoora in 1968. The observations with this instrument confirmed and elaborated many of the earlier results, and produced new ones. For example, one could see clearly the coronal plasmoids - sometimes in the form of long loops attached to the sun, and sometimes as isolated ejecta moving far into the interplanetary space. Closer at home, at Clark Lake, we have a multifrequency radioheliograph in operation since 1982. The first observations indicate that type III electron streams do propagate in dense coronal streamers which must then have open field lines.

Solar Radio Astronomy From Space

The first observations of solar radio bursts (type III's) at very low frequencies were obtained from the Alouette-1 satellite in the frequency range 1.5 - 10 MHz. Further observations of solar bursts were obtained at 0.2, 1 and 2 MHz, using Zond-3 and Venera-2. Perhaps the most systematic study of type III's from space have been made by RAE-1 (0.2 - 9 MHz), by OGO-III down to 25 Khz and by IMP-6 down to 10 KHz. These frequencies emanate from several solar radii to $\sim$ 1 A.U. from the sun. One must note two important results from spacecraft observations. First, simultaneous observations from IMP-6 satellite of electrons and hectometer type III's by Van Allen and Krimigis in 1965 and by Anderson and Lin in 1966 showed that the onset of type III radio emission corresponded to the arrival of 10-100 Kev electrons. Thus, the space observations of type III's at hectometer and kilometer wavelengths imply that electron streams must find open field lines all the way from the sun to near the earth. Indeed, the directional observations of type III's by using the spin-modulation of the burst envelope resulting from rotation of IMP-6 spacecraft showed the spiral nature of the interplanetary magnetic field.

Solar Radio Astronomy at Centimeter Wavelengths

After Covington's discovery of the slowly varying component (SVC) in 1946, not much happened at centimeter wavelengths until 1953, when Christiansen introduced the grating or multi-element interferometer at 20 cm-λ, which significantly advanced the technique of studying the quiet sun and SVC on a daily basis. The next major advance in the study of SVC was made in 1956-57 by the French group who used for the first time one-dimensional earth-rotation synthesis to produce the brightness distribution of active regions at 3 cm in both total intensity and circular polarization. The core-halo structure in sunspot associated sources was established, which led to a new theory of the SVC via the gyro-resonance absorption process. Since 1957 there have been sporadic measurements of SVC at centimeter wavelengths by large interferometers such as the NRAO 3-element and Owens Valley 3-element interferometers, which confirmed the earlier results. It was a significant accomplishment on the part of solar radio astronomers to be able to get observing time on the Westerbork Synthesis Radio Telescope (WSRT) in 1974. We had the opportunity to do good science; most importantly we broke the myth that large arrays should be limited to doing cosmic radio astronomy alone. We produced two-dimensional maps of sunspot associated active regions which could be interpreted as coronal magnetograms. This is important since coronal magnetic field structure or its strength are not easily or directly measurable in other spectral domains; we observed ring structure in sunspot associated 6 cm sources; and we observed that cm burst sources were located in between two regions of opposite polarity. Since 1979 we've been using the VLA, with which we confirmed the earlier WSRT results; we were able to see many active region loops, and because of the possibility of producing snapshot (10 sec) pictures, we could study the location of flare energy release in great detail. Thus, we observed that microwave burst emitting regions are located in the top parts of a loop or an arcade of loops.

In 1978, something close to serendipity occurred in solar radio astronomy. Slottje, in Dwingeloo, observed millisecond pulses from an impulsive outburst at 11 cm. Theoretical considerations indicated that these pulses must have brightness temperatures of $\sim 10^{15}$ K. It is not possible to use simple incoherent gyro-synchrotron radiation to explain these high T_b bursts. In 1979, following Twiss's original idea of 1958 to explain solar noise storm bursts, we proposed the gyro-synchrotron maser as a possible mechanism for these pulses. Other plasma physical processes such as radiation in the upper hybrid modes have also been proposed. At this point it is pretty much a theoretical problem rather than an observational one.

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THE DISCOVERY OF JUPITER BURSTS

K. L. Franklin
American Museum-Hayden Planetarium

Until yesterday I had the firm opinion that Bernie Burke could have done all this without me, but I could not have done it without Bernie. Now after hearing Hanbury Brown's ultimate criterion that you don't have to know too much, I think that I was necessary!

Just a quick review of what it was that we were working with. We had a Mills Cross in a field. The arms were 1,047½ feet long. I think there were 64 dipoles in each. Each arm produced a fan beam. When you multiply the two of them together you get the pencil beam. By adjusting the transmission lines we were able to move the beam along the meridian. It was designed for 22.2 MHz as a survey instrument to look at the sky at these long wavelengths and see what was there. This had not been done before. It had been done at shorter wavelengths but not at these long ones.

I got to DTM in September of 1954 as a research fellow. Dr. Tuve suggested I work with Bernie on this Mills Cross. I had to learn a lot, I'm not sure I did but I was supposed to. We had in mind a survey to last for a couple of years. The equipment was working but we wanted to make it stable for a couple of years, so various components were perfected and injected into the system as we went along. We kept the beam at the declination of the Crab Nebula. The Crab gave us a nice little reference point as it went by and if any of the changes we had made really altered the nature of the equipment, we would see the response to it in the next few days. After about a month or two of doing this, Bernie said, "This is enough information at this particular declination. I think we should move it and spread out a little bit so we can get a bigger map." We were standing out in the field with phasing links in our hand and he said, "Ken, which way should it go?" Well, it is either north or south, you know, it's an arbitrary decision! And I said, "South!" So we went south and we continued to go south. Every few weeks Bernie would move it about another degree. Over the months we became quite familiar with the Crab Nebula and as we went south it began to get out of the beam and decrease in intensity, but there was always something of it that appeared on the record.

Figure 1 shows the way it looked. On the right is the Crab Nebula, and once in a while we would get some interference. I remember telling Bernie one day, "Hey, you know we have got to figure out what that thing is." He said, "I suppose so," and that ended that!

The April meetings of the American Astronomical Society were coming up and sometime in March we figured we would probably have something to report, so we sent in an abstract. Then Bernie gathered up all of the papers and set aside those with interference. One Monday I came to work probably around noon time and Bernie had gathered up the papers that had the record on them without the interference and decided we needed the information from the records containing the interference also. When I got there he said, "Ken, come in here. I want to show you something." He had taken all of these records and lined them up so that the Crab Nebula was in a nice straight line and then we saw that the interference also roughly lines up. He said, "We've got something here!" In the laboratory was Howard Tatel. Howard Tatel was interested in all kinds of things. He was going over some seismic records because he was interested in the roots of mountains. He was also interested in the hydrogen distribution in the galaxy. I asked him one day, "Howard, how do you justify and bring all these things together? You're looking down and you're looking up." He said, "We live on a surface. I'm interested in anything that is off it!"

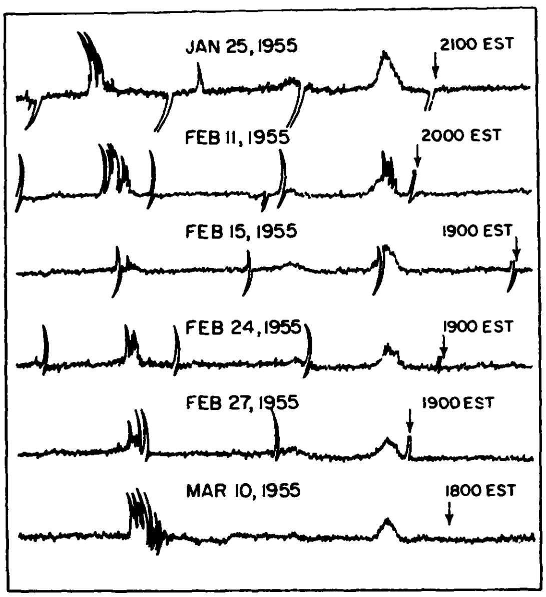


Fig. 1. Phase switching record showing Jupiter at left and the Crab Nebula on the right. Note the changing interval between the two traces corresponding to the motion of the planet.

We examined this little thing for a while wondering what it was. We went to the atlases and we didn't find anything really fascinating in that part of the sky. There was a good interesting cluster of stars, an open cluster. There was also a highly energetic planetary nebula that I remember hearing about. I have forgotten the name of it now, but that possibly was it. But then we took another closer look at this thing and noted that it wasn't vertical. It sloped; it changed its right ascension.

About this time Howard said, "Maybe it is Jupiter!" That's a ridiculous idea. So I went to the American Ephemeris & Nautical Almanac and looked at the position of Jupiter, and it was pretty close. You couldn't quite argue against it -- not from that book. I don't know how Howard came to that suggestion. It might be because a night or two before he had used the Wurzburg antenna in the back yard at DTM to look at hydrogen from Jupiter. Dick Roberts had suggested this and Howard took a look but didn't find anything. He might have had Jupiter on his mind, I don't know. Bernie, did you ever find out why he said that?

B. Burke: No.

Well, about this time we had to give up our little discussion because Bernie and I had to run out to the field, twenty miles out there to look at the equipment, and make some adjustments on it. The sun went down, beautiful clear twilight, one of those things that is just fantastic, and Bernie said, "Say, I've been meaning to ask. What is that bright thing up there?" I said, "Bernie, that is Jupiter!" We had a good laugh over that.

We did our thing and went home. Of course, I pondered this problem during the evening and it finally occurred to me how one should go about making the analysis of this. What I did the next morning was to take the information, right ascension and date. Then I plotted what I called the roots of the "interference." What I did was to plot the beginning and the end as they came along. Some of them were short and some were long. There were about a dozen, which meant about one out of every three days we would get a record. What I did was to try to build a smooth envelope through the extremes. Some wouldn't make it, but then some might go a little bit beyond. Then the two galactic objects, the Crab Nebula and IC 443, they were supposed to be straight level lines. I looked around a little more and found Uranus, but it did not line up with the "interference." Then I plotted the positions for Jupiter on the dates where we had some records and they made a line right in the middle between the envelopes. About this time Bernie came over and watched. Every time I plotted a point he said, "Wow!" This was Tuesday morning and by Tuesday afternoon the whole lab was excited. Wednesday morning, Tuve brought Vannevar Bush in. He said, "Now, tell me all about this!" And so I went through the explanation and he said, "How do you know it is not something else?"

Well, it seemed obvious to me. But you have to answer the question. Sometimes I think in two dimensions, and it is awfully hard to tell a story that way. So I had to come up with a one-dimensional explanation of it. I said, "Well, it has the position of Jupiter and the change of position of Jupiter." Fine. Well, that was Wednesday, and it was a very sudden denouement of two or three months of work. The next thing we had to do was to prepare the paper and we decided that we would not say anything about it until we got to the meetings. We would wait until the paper was presented. However, there had been a news release put out with the deadline for after the presentation: it was the afternoon of the day that Bernie was to give the paper. I think it was the third day of the meeting that we were giving this paper; on the second day we happened to have lunch with Chandrasekhar sitting across the table from us. Bernie said, "Dr. Chandrasekhar, would you have any reason at all to expect that one could get radio waves out of Jupiter?"

"No, no, absolutely not!"

"Well, we got them!" So the paper was presented the next morning and I sat there reading about it in the New York Times as Bernie was giving the paper! They had jumped the deadline on us.

Well, it set up a little stir and I must say that one of the first to get on his feet was John Kraus, and gave us great accolades for this unusual discovery which was really appreciated, John. I can say this after a quarter of a century!

Naturally we communicated to our colleagues around the world that this had happened and one of the people was C. A. Shain out in Australia. He set up operations at 19 MHz and confirmed that Jupiter was busy. He had this equipment because earlier he had set it up for another purpose. Occasionally he had some noise on this thing, but this antenna system was not very discriminating. He had heard this noise but it just sounded like, you know, swishes, and he thought something was going on over in Indonesia or someplace. So he went back and examined his records and he found out that a lot of that stuff had been Jupiter. That was a marvelous pre-discovery observation that gave about 5 years of data that he used in order to analyze things rather quickly.

As a result of the New York Times and giving the paper, the National Broadcasting Company thought this would be marvelous, because surely these were sounds. We had a speaker on the equipment, but we just turned it on to see whether or not something was interference. After all, radio astronomers don't <u>listen</u> to the universe, do they? OK, we'll take the recorder and set it up and see what happens. So we got the recorder and on Easter Sunday, there was a fantastic storm from Jupiter. The equipment was hooked up the next day. Everyday for about three weeks I raced out the 20 miles in the evening and afternoon, turned this thing on and watched the pen to see what was happening. Nothing. About three weeks later, on a Friday, I went out there and Jupiter obliged with a rather feeble effort; interesting but rather feeble. I took that tape home and all weekend I was the only one in the world who had heard Jupiter and who knew it. These guys in Australia had heard it but they didn't know it! That is an interesting experience; if you want uniqueness, there it is. Everybody was rather startled with what we got when we played it in the Laboratory on Monday morning. NBC used it on their first Monitor program. We came right after noises from oysters!

One of the results of this was the contact that I made with the chairman of the Hayden Planetarium, and he said, "Say, we can use some of that sound business in our show in January when we review what happened during the year. We would love to have it." So Bernie and I flew up to New York and I have a real quick and dirty recording of the tape. Burke described the discovery:

> *"Early in 1955, Dr. Franklin and I were observing in the vicinity of the Crab Nebula with the radio telescope at the Carnegie Institution of Washington. Our radio telescope was actually a large highly directive antenna occupying a 96-acre field. We noticed that intermittent interference was being received about one out of every three days. And after taking three months of data, we could see that the time of occurrence of the noise depended not on the solar time, but on time by the stars. This meant that the cause of interference was probably far beyond the earth. Dr. Franklin will tell you now how we identified this source of what we had thought was interference.*
>
> *The apparent position of the source actually moved among the stars suggesting the motion of a planet. The only planet having both the position and the motion of the source was found to be Jupiter. Our equipment produces a high-pitched tone when a radio source is being observed. We shall play for you a recording of this effect. The tone you will hear is actually*

due to radio radiation from Jupiter. The hissing noise is due to background radiation. ....

The cause of this radiation is not known but is likely to be due to electrical disturbances in Jupiter's atmosphere."

I always regarded that as a good safe statement! And it is probably almost true. This got a lot of people excited and doing something about Jupiter. A group down in Florida started up work down there. Alex Smith asked me at the 1958 meetings if I wouldn't recount this anecdote, and so I did. It got published in the Astronomical Journal 62, p. 145, which had proceedings of that symposium. There were other people that began to work on this subject. I don't know how many Ph.D.'s there are now as a result of studying radio noises from Jupiter. I have a theory that I have held over the years that every graduate student has it in the back of his mind that he wants to discover something that afterward every other graduate student will have to study. One of these was in Yale where Harlan Smith had a big program observing Jupiter and one of the students there was Kenelm W. Philip. What he did was a very interesting thing. He set up fast recorders on receivers at 100 kilometer separation. He would often get a pattern at one of the receivers, and at the other receiver he would get the same pattern but displaced in time. Before opposition, it was displaced one way, and after opposition it was displaced the other way. They finally explained this by having the Earth looking up at Jupiter before opposition and, then, after opposition. In one sense it was going left to right and in the other sense it was going right to left. They explained that as the solar wind. So what Ken Philip had done was a serendipitous observation of the solar wind by studying Jupiter.

About 10 years ago, Harlan Smith mentioned to me in the company of Jim Wright of NSF a story that until yesterday I thought was really the case. Now Jocelyn Bell Burnell tells her story and maybe Smith was not correct. It seems that according to Harlan Smith, Anthony Hewish was well aware of Philip's work and wanted to see whether or not there wouldn't be other scintillations that he could discern, therefore setting up his equipment to make these observations. Of course, we all know how pulsars were discovered.

It seems to me that pulsars are serendipity cubed, because our work and Philip's work and then pulsars are each a result of something serendipitous that occurred and are just simply multiplied together.

B. Burke: Old chairmen never die, and they never keep quiet. I have to add a story because the first thing that came to everybody's mind was, well, there are thunderstorms on Jupiter. The interesting thing is that there was no data as far as I could tell in the literature anywhere as to what kind of radio noise is produced by a thunderstorm at 20 MHz - quantitative data. That is, there were average noise levels from atmospheric studies, but what does a single lightning stroke do? We decided that as summer was coming on in Maryland, it was a good place to study thunderstorms in the spring, and so we set up an antenna. I remember that when we were making the first setup, because we felt that you needed the experience, Ken, I stood in a trailer out of the rain that was coming and shielded from the lightning strokes that might occur, and you were in your rubber boots out in the field making the last cable connections. Suddenly there was a lightning stroke from cloud to cloud overhead and it started to vibrate as Ken's hair stood up on end!

In synchronism with the flash!

B. Burke: We got a very good recording! We also measured a rough estimate of the low frequency induction field of the lightning stroke at the same time, but we were in the near field, so it was hard to interpret. Also, if you want to get rid of your co-investigator, so you can have all the glory, it's not a bad method!

A very curious thing about that was that I drove this vehicle back to Durwood and we had to get fuel. I had gone through this little experience here and when I gave my credit card to the fellow, he said, "Franklin! Ben Franklin?" Then there was a fellow at DTM who said, "Why bother with all the theory? It's just the Thor spots!"

J. Broderick: I have one question. I tell my introductory astronomy class the story of the discovery of Jupiter. Either I embellish it a bit or else I am telling a part of it that you missed. I understand that for a while the bursts were analyzed in terms of a couple who were parked somewhere in the boondocks, and then each night they would return, and when they were leaving you would start wondering about how come they were leaving earlier each night. Was it getting better or worse?

J. Findlay: What kind of a class do you teach?

J. Broderick: Someone had told me this story, that for a while you thought it might have been automobile interference from someone who was out in the woods sparking, so to speak.

In the A.J. article this was actually mentioned. It was a little joke that went around the Laboratory. In the A.J. article we said that we thought this might be the ignition of a farmhand returning from a date. You see there is a dangling participle there! The Observatory picked it up in their "Here and There" column, and their remark was "sparking, no doubt."

B. Burke: Basically, the story is the invention of Merle Tuve who believed that a good story should never lose anything in the retelling.

Incidentally, when we moved the beam south, we were accurately following Jupiter. If we had gone north, we wouldn't have had the 3 months of data on Jupiter.

C. Wade: I believe you said at the outset, the arms of your array were 1,047.5 feet long.

B. Burke: That is wrong by a factor of two. The units are correct though.

C. Wade: Whatever, I'm not from California but I did notice 1,047.5 is exactly the reciprocal mass of Jupiter in terms of solar mass! You can look it up in the Ephemeris!

DISCOVERY OF THE JUPITER RADIATION BELTS

Frank D. Drake
Cornell University

Hein Hvatum played a major role in what I am talking about and he agreed to be co-author on this paper only under the condition that he didn't need to say anything. So he is sitting in the back of the room saying nothing!

I'm going to talk to you about the discovery of the Jupiter radiation belts which, as you will see, was not so serendipitous after all, but perhaps more an example of the way science is supposed to be done following the textbook scientific method. Later, if time allows, I'll tell you about some real serendipity.

The story starts at the Paris Symposium which occurred in the summer of 1958. On that occasion Ed McClain from NRL gave a paper in which he described some of the first results from a new 84-foot telescope built by NRL at Maryland Point. There were a lot of exciting new results and among them he reported a few very interesting drift curves across the planet Jupiter. They were published in the Paris Symposium Proceedings. For some reason he chose to publish one of the worst drift curves I have seen from a radio telescope! Nevertheless, a number of these added together produced an extraordinary result which was a flux level which implied a disk blackbody brightness temperature for Jupiter of 580° Kelvin. Ed reported this at the Paris Symposium and not much notice was taken of it, not even at NRL for some reason, because subsequently they did not follow up on this work. If you ask, "Why?", the answer seems to be they were so busy doing other things in order to justify the cost of building the telescope that there wasn't time to follow up on this work.

The data were reduced more extensively by Russell Sloanaker, who had done most of the observations, and he subsequently reported more accurate values which appeared in the Astronomical Journal in 1959. This work was done in late 1958, and he got a blackbody temperature of 640 K. There was some indication that the radiation was time variable because he got values ranging from 300 to about 1,000 degrees, although he recognized quite wisely that most of that variation, but not quite all, could be accounted for as receiver noise.

A. Barrett: *What wavelength was that?*

It was all at 10 centimeters wavelength. There had been earlier observations at 3 centimeters by the NRL group which gave temperatures of about 145°, also the infrared temperature, and the expected blackbody temperatures. But the temperature observed at 10 centimeters was very much higher than thermal equilibrium should produce. It was quite striking.

At this point the NRAO enters. In early 1958 the NRAO became an observatory with the construction of the Tatel Telescope, named for Howard Tatel who we just heard about. It is interesting to read the Annual Report for 1959 of the observatory. You will find that the annual operating budget of the

NRAO in that year was $358,801.00. It is now fifty times that. The entire scientific staff consisted of two people. I was half, and the bigger half was Dave Heeschen. However, there was a retinue of very distinguished visitors which were an interesting lot. One was a fellow named George Field who was there trying to detect intergalactic hydrogen, which he tried over and over without success. There was also another young fellow who was visiting from Berkeley named Morton Roberts, and he subsequently came back to the observatory and assumed more important responsibilities. A third was Grote Reber, whom I will come back to briefly, and a fourth was Gart Westerhout who played an important role in the installation of the first digital system in all of astronomy. In fact, we did that at Green Bank that summer by constructing a digitizer which consisted of a capacitor circuit which integrated the voltage output from a radiometer. To it was attached one of the first digital voltmeters, and a paper printer, and if you pushed the button on the digital voltmeter, the numbers would record on a piece of paper. That was the first digital recording system in all of astronomy! Now to do this you had to get up and walk across the room and push the button, and after an hour or so that got tedious. Gart's contribution was to rig up a conglomeration of strings and ropes across the control room so that he could observe by sitting in a big easy chair instead of getting up and walking across the room. All he did was to reach up and pull a handle down. Normally, with that kind of action you would expect to hear a flushing noise! But what you heard was a paper printer. Thus the first digital records in astronomy were taken, and Gart Westerhout deserves full credit for the introduction of automation!

There are some other interesting things in the 1959 Annual Report. For example, with regard to plans for the future, it stated that the observatory was planning a fixed spherical antenna. I don't remember this at all. The reflector was to have a total aperture of 420 feet. By illuminating 300 feet of aperture and moving the feed, about 30° of sky coverage will be obtained. Now that did happen, but not at the NRAO! The numbers were slightly different, but otherwise it was an accurate description of a good project.

Another proposed program was a "preliminary search for extraterrestrial coherent signals," which I guess was a non-provocative way of announcing a search for extraterrestrial intelligent life.

In any case, the telescope went into operation in March and one of the first things we decided to do was to follow up on the report from NRL of the high blackbody brightness temperature recorded by the NRL group. The obvious thing to do was to measure the spectrum of this radiation to see if it was of thermal or non-thermal origin. Serendipity was generated by straightforward conventional radio astronomy techniques at that time. The most sensitive test would be at lower frequencies, where if the radiation were non-thermal one would see higher temperatures. Of course there were already observations on shorter wavelengths from NRL. So we used the 21 cm system on the Tatel telescope to observe Jupiter using good old drift curve technique, and we got antenna temperatures which gave an equivalent blackbody disk temperature of 3000°. This determined that there were indeed very high fluxes implying very high blackbody disk temperatures. But even 3000° possibly had a thermal origin. We had learned enough about planetary structure to know that if you looked deeper into a planet you were certainly going to see higher temperatures, and with enough opacity and greenhouse effect the 3000° might in fact be thermal radiation from deep down. To deal with this possibility, the best thing we could do was to observe on still longer wavelengths to see if the temperature got so high that it couldn't be of thermal origin. We went to the lowest frequency that was available here at Green Bank, which was 440 MHz. That frequency was available because Hein had come from Chalmers in Sweden to build diode switches and for some reason lost to history, he had chosen that frequency. The consequence was that there was a 440 MHz system available. It was a total power system, and it was very good; Hein had played a big role in building it. We started using it on Jupiter, and immediately detected Jupiter with the system. But we then quickly realized that taking drift curves was very time consuming and we weren't using the telescope well because most of the time was spent taking zero levels or waiting out the drift curve; it was very long at this wavelength because the beamwidth was several degrees.

So we invented a new technique which we called the "on-off" technique and made records of Jupiter, such as you see in Figure 1, and as you might expect, this was the best record we ever took. What we discovered was that a very good way of observing was to point off the source for awhile and then abruptly on the source, and you did best if you alternated as quickly as possible. However, if you went too fast you spent most of the time moving the telescope, so it worked out that the optimum technique was to switch every thirty seconds. Well, every thirty seconds we would go on and off the source, generating curves such as this, where you see the radiation from Jupiter at 440 MHz, 68 cm wavelength with about half a degree of antenna temperature giving a flux density of about 10 Janskys.

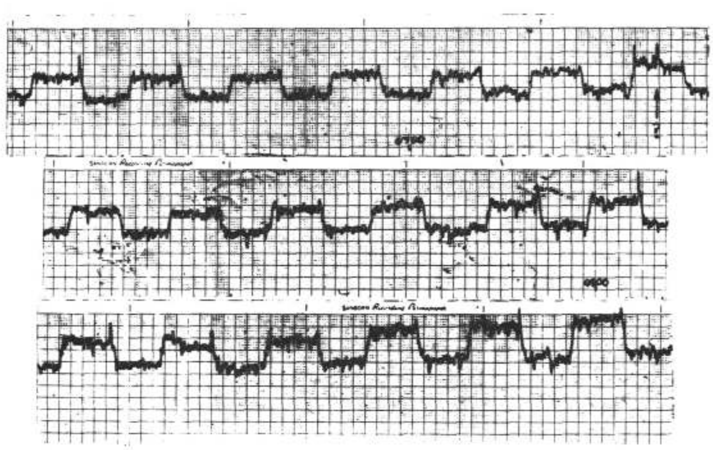


Fig. 1. Observations of Jupiter at 440 MHz made with the NRAO 85-ft telescope. The record shows changes in receiver output as the telescope is alternately pointed toward and away from the planet.

This procedure was so effective that we used it for many years afterwards in studying the planets. In fact, I guess one of the longest integrations ever done in astronomy was done using this technique later on Venus where we integrated 85 hours in order to measure the disk blackbody temperature at superior conjunction. It actually worked successfully. The only bad part about this was that it drove the telescope operators crazy, and to this day they remember those days because they would spend literally hour after hour, without computer control, moving the telescope back and forth, every 30 seconds, back and forth through the long nights and days at Green Bank.

Hein reminds me that I was very excited when I saw this record because towards the end the zero level is rising. At this time the telescope was very low, and Jupiter was about to set. It was exciting because we were actually seeing ground radiation coming in the sidelobes of the antenna as the beam approached the surface, and that was thrilling because it said radio astronomy really worked: the earth radiates! All those equations really are true about what makes the antenna temperature of a radio telescope!

The results of the measurements were as follows: Measurements at 22 cm gave a temperature of about 3000 degrees. The first measurements at 68 cm gave about 70,0000 degrees, or about 13 Janskys. They were published in the Astronomical Journal and given at the Toronto meeting of the AAS in August 1959.

About that time we learned something else about radio astronomy and that is what is now known as the confusion limit. Luckily, we did have the sense to run the observations when Jupiter wasn't in the beam, just switching on the sky, and we discovered to our horror we got a square wave then also, although with not such a great amplitude. Of course, sometimes it was negative, sometimes the other way around, and we discovered that in fact the sky was quite variable at 440 MHz when you looked at it with the 85-foot telescope. This explained why we were also detecting what we thought were variations. The flux densities we measured had varied from about 2 Janskys to 13, or over a factor of 5. That turned out to be entirely due to a failure to correct for the varying background. We then undertook a set of observations which we called "anti-Jupiter" in the fall of 1959. In these we reobserved all the positions where Jupiter had been when we observed it. Of course now we waited till Jupiter wasn't there and we observed all of this in exactly the same way. The telescope operators thought this was really crazy! There they were, cranking away just the way they had done months before, but they knew darned well Jupiter was somewhere else in the sky! Why were they doing that? The results showed that the flux from Jupiter was unchanging as far as we could see.

We could then take the data and construct the spectrum of Jupiter which is shown in Figure 2. The original NRL data points are shown as well as ours.

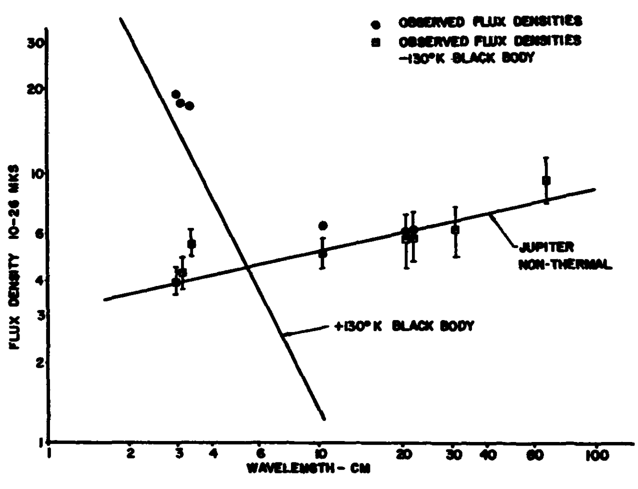


Fig. 2. Microwave spectrum of Jupiter showing the separation into thermal (disk) and non-thermal (belt) components.

We decided there must be a non-thermal component, and so we subtracted away the thermal radiation which should be there; something like a 130 K blackbody as given by infrared observations. This leaves residual radiation which can be attributed to a non-thermal component. The results gave a non-thermal spectrum with an exponent of 0.2, in fact almost identical to the spectrum of the Crab Nebula.

The spectrum turned out to be a text-book non-thermal spectrum, and it said that something was making synchrotron radiation or seemed to be making synchrotron radiation at Jupiter.

This led to the suggestion that we were observing Jupiter radiation belts. If you put the numbers in, a magnetic field at 10 gauss was implied by the decameter observations of Burke and Franklin. With that magnetic field, which turns out to be about right, and our fluxes, the total particle number required was about 10^6 times what is in the earth's belts. This was, of course, a large implied overabundance compared to the earth. But the Jupiter system is a thousand times greater in volume so the particle density needs to be only a thousand times greater than in the earth.

To us young astronomers, these results sounded rather daring and speculative. We were strongly urged to rush into print with this by Lloyd Berkner who understood better the need for establishing priority and credit and all that sort of thing. So with great timidity we included this speculation about the possible origin of this radiation in our first papers.

Very soon thereafter, there were observations at NRL by Ed McClain which also showed the high disk temperature at 21 cm, essentially the same value as ours. Then, just as we were finishing our observations, John Bolton visited Green Bank and we told him all about our results, and he went back and put Radhakrishnan and Roberts to work with the Caltech interferometer. Very quickly they exploited that interferometer to show that the source size was larger than Jupiter, elongated in the equatorial direction with a total extent of about 3 Jupiter diameters, and most importantly that the linear polarization was about 30%.

Looking back, we were all in trouble with the confusion limit, and as it was dealt with, the reports of variability slowly went away. By 1962, as shown in Figure 3, at least at 10 centimeters, we had learned how to cope with the varying background and problems it caused. Shown in Figure 3 are the results from six months in the years 1961 and 1962. It turned out that the synchrotron radiation is very steady; in fact this is still one of the mysteries of the Jupiter decimeter radiation.

As you see from all this, there was really very little serendipity involved. Science was done in the way you're supposed to do science, even if that's not usually the way it is done.

I want to tell you about something that does contain some serendipity, which is the story of how the largest radio telescope in the world actually came to be built. It reminds us of the story by Sir Bernard Lovell the other day, because this is really an epilogue to his talk. There is much irony here because it turns out that two of the largest telescopes ever built seem to have been built as a result of almost identical serendipitous events. There

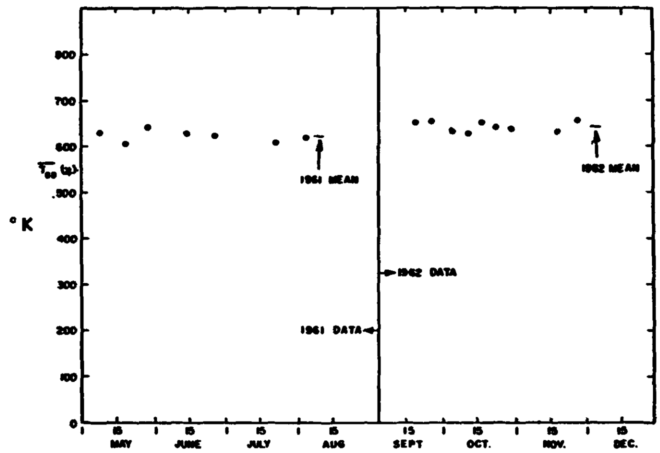


Fig. 3. Brightness temperature of Jupiter averaged over a 6-day interval for the 1961 and 1962 observing periods.

are many "largest" radio telescopes in the world; in fact there is a lot of propaganda around here about one in New Mexico, but that one is not the largest in any sense! The largest one I am talking about is the one in Arecibo, the 1,000-foot reflector, which was going to be built here but somehow ended up being built in Puerto Rico. John Findlay is evidently going to explain that later.

The history is not well known. In the middle 1950's, a very eminent ionospheric physicist at Cornell got the idea that you could learn a great deal about the ionosphere if you went beyond the traditional coherent backscatter observations of the ionosphere. If you went to incoherent scatter or Thomson scatter, the promise was that you could observe electrons at all heights and in fact deduce temperatures, ionic compositions, and motions and learn a great deal more than you could from traditional methods.

The obstacle which prevented this from being done before was that Thomson backscatter was very weak, compared to coherent deflections, and it called for enormous transmitting powers far beyond what had been available. Now, what happened was the ionospheric physicist at Cornell sat down and calculated: what would it take with our present equipment to get an ionospheric backscatter from the ionosphere? There were something like 2 megawatt transmitters available at the right frequencies and receivers had gotten quite good by then. Well, if you took those parameters with the known electron densities, it turned out you needed a reflector about a thousand feet in diameter. Pretty big! Now that was based on a straightforward and seemingly correct theory. The theory was that the electrons were up there moving around in the normal kinetic way in gases, had a temperature of about a thousand degrees, and they were moving with the appropriate motion of such particles. The large Doppler motion spread the radar echo to cover a wide band of frequency so the signal at any given frequency band was very low, and it is because of that you needed a one thousand foot reflector to get a barely detectable, that is, three sigma detections in times of ten or twenty minutes. Those were the calculations.

Now of course that calls for a telescope, so big it had to be a fixed reflector. The original plan was to have such a thing, a thousand feet diameter parabola which would just shoot straight up with its beam. About then Marshall Cohen, Tom Gold, and others got into the action at Cornell and pointed out that if you could steer the beam this would be kind of nice as a radio telescope, too. And so the plans were altered to provide a partly steerable system. It still was expensive, with costs of perhaps 9 million dollars for the entire installation including transmitter, buildings and all the rest. The Department of Defense was interested but they weren't sure this was worth 9 million dollars.

Now at this time, about 1957-58, one of our other heroes, John Kraus, unwittingly enters the scene again. He made a lot of measurements of the first Sputniks, and in the course of observing Sputniks, he reported that there appeared to be trails of ionization behind the satellites. This caught the eye of the Department of Defense because they saw this as a possible means of detecting the presence of satellites which were in a way hostile and made so that you couldn't detect them by radar reception. You could detect the presence of satellites and rockets by detecting the ionized trail. But the only way you could do that since it was above the ionosphere, that is, on the top side, is to look for the incoherent backscatter. Suddenly the 9 million dollars sounded like a good price! The project went into gear and the giant telescope in Puerto Rico went into construction.

That was all fine, things were going gung ho, and by 1962 it was about half built. The expectation was that this instrument would just barely detect echoes from ionized plasma in times of ten or twenty minutes using a big transmitter. Well, about then a young scientist, Ken Bowles, then at the Bureau of Standards, looked over the theory and realized the theory of the ionosphere wasn't so simple. In the presence of the charged particle fields, you get an effect such that the electrons instead of moving freely are dragged along with the ions. He calculated that in fact the motion of the electrons would be very strongly influenced by the presence of the ions, and the huge velocities wouldn't be there. As a consequence, the wide spread in the spectrum would not be there either. The spectrum would be very much more condensed and, therefore, with a very much higher radar echo signal strength at the optimum wavelengths.

So to check that out he went out and used a meteor radar, a relatively little thing, to bang away on the ionosphere, and sure enough there was the narrow, strong echo! The spectrum suddenly got about a hundred times narrower than first expected, with the consequence that in fact we needed a dish only about one percent the size in area of the dish under construction. In other words, to achieve the original goal, we didn't need a one thousand foot reflector. A one hundred footer would have done it, and in fact the Tatel telescope or a one hundred foot telescope in Ithaca would have done the job. The largest radio telescope in the world might never have been built.

So that is how the largest radio telescope in the world came to be. The conclusion we reach, drawing also on Sir Bernard's experience and also on Hanbury Brown's experience, is as follows: If you want a compelling argument for a big dish, get the theory wrong! A corollary is: getting the damping factor wrong is especially helpful!

Serendipity isn't always as we have been stating it here. Sometimes

serendipity is "the right person (I'm avoiding that word "man" which we've been using) in the right place at the right time doing the wrong thing!"

J. Findlay: At the beginning of the 300-ft telescope project, I realized that there were two ways of doing it. Copy Arecibo, which was then in the works, or build an easy transit telescope. I went up to Cornell, spoke to Marshall Cohen, and said, "I can't build feed for a spherical dish." So I built a transit telescope!

B. Burke: Ignorance strikes again!

E. McClain: Frank has taken the literature literally. There is something I definitely have to correct. My name is on that first paper, having to do with the 84-foot dish. I was the contracting officer and I had long since made the agreement with Russell Sloanaker that he would build the radiometer because although aerial cameras looking at the North Star are very good for aligning polar axes and such, you're really interested in the radio waves and we didn't expect the telescope to work beyond 10 cm very well. So he built the radiometer basically to test the dish initially. Then he found this Jupiter thing and we worked very close together. There was no competition or anything and we sent this thing in. That was an example of one of the first observations that turned out of course to be very important. I think it was serendipity. He didn't expect 600°; he had no reason to. So I want it cleared up. My name should not have been on the paper. I didn't read the paper, either; somebody else did. I didn't even go there.

EARLY OBSERVATIONS OF THERMAL PLANETARY RADIO EMISSION

Cornell H. Mayer
E. O. Hulburt Center for Space Research
Naval Research Laboratory

This picture (Fig. 1) shows the main things that made it possible to detect thermal radiation of the planets for the first time. Most important was the large collecting area of the 50-foot reflector at 3 centimeters wavelength, which, as you heard yesterday, was conceived by Fred Haddock and John Hagen. Second, my colleagues, Timothy McCullough and Russell Sloanaker,

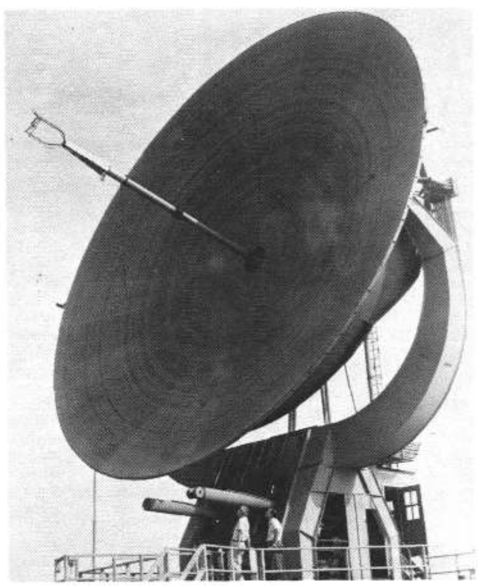


Fig. 1. NRL 50-ft. dish.

and not shown in the picture, John Boland. And third, an improved 3 centimeter radiometer. We had two things we wanted to do at 3 cm with the 50-foot initially. One was to try to penetrate the cloud covered atmosphere of Venus to get the first idea of the temperature of the surface of the planet, and the other was to look for polarization of the Crab Nebula, which had already been detected at optical wavelengths, but not at radio. At 3 cm wavelength the Faraday rotation dispersion would be much less than at the longer radio wavelengths where attempts to detect polarization by other groups had failed.

I would like to spend a few minutes on the technical improvements incorporated in the radiometer, because I think without these we would never have been able to get meaningful results for Venus. One of them I should point out first made possible a direct comparison of the radiation entering the feed antenna and the small horn antenna pointed at cold sky shown in the photograph (Fig. 1).

One important improvement was the ability to couple directly the noise calibration signal into the input line to the radiometer through a high directivity directional coupler which affected the performance of the radiometer hardly at all (Fig. 2). This could be done at any time, as frequently

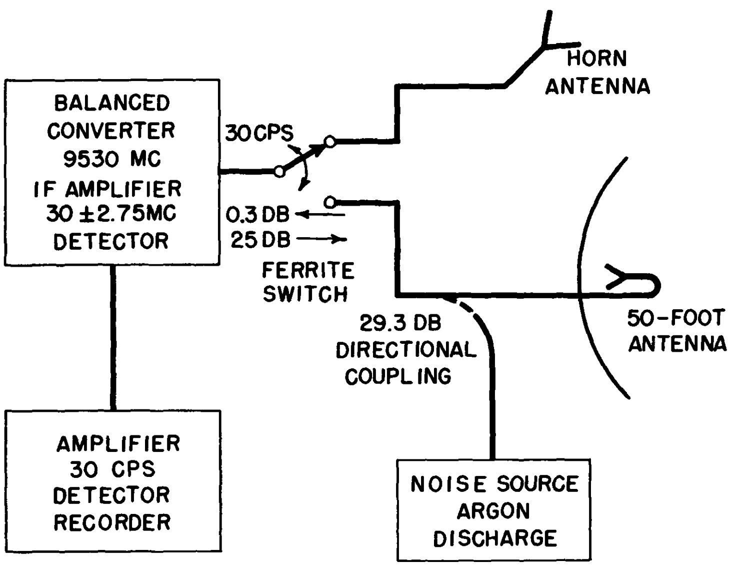


Fig. 2. Block diagram showing noise calibration input.

as necessary or desired, and the noise calibration signal at the desired low intensity level was simply superimposed on whatever radiation was in the antenna feed line. This was made possible by the insight of W. W. Mumford of the Bell Telephone Laboratories who recognized that the microwave radiation of fluorescent lamps, which we had all known about for years as a hated interference source, could be coupled into a waveguide to make an intense (10,000 K) calibration source. Further development by H. Johnson of RCA showed that if you used pure Argon gas discharge tubes without the mercury vapor that was in the fluorescent lamp, the noise source was also very stable without the sizeable temperature variations of the fluorescent lamp noise source.

The other important improvement which was developed for the radiometer was to adapt the non-reciprocal ferrite circulator invented by C. L. Hogan of the Bell Telephone Laboratories as a ferrite switch to replace the lossy disk chopper of the Dicke radiometer. I think I should mention at this point that I am really reporting NRL results, not Bell Telephone Laboratories results. This is what made it possible to compare directly any two arbitrary sources, in this case the small horn antenna pointed at cold sky and the 50-foot reflector so that a very small signal difference could be compared directly, the situation where the comparison radiometer principle gives greatest advantage. A second great advantage was that the non-reciprocal property of the switch gave high impedance isolation between the input to the radiometer and the output, and virtually made negligible the output variations of the radiometer due to different impedances hooked to the input. This made it possible to get accurate calibrations by interchanging calibration sources and antennas as you pleased even though they had different impedances, and minimized output variations with frequency drift.

This impedance effect is demonstrated by the sophisticated experiment shown in Figure 3 where a terminated dielectric plug was dragged through the

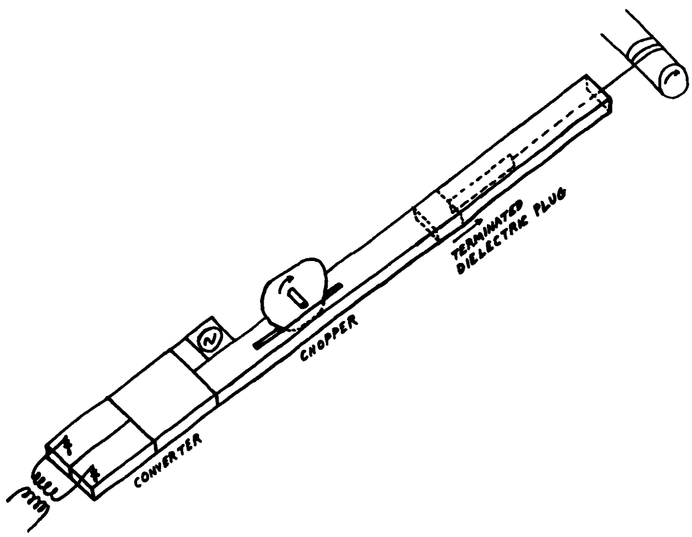


Fig. 3. Apparatus for demonstration of impedance isolation.

input waveguide by a string winding up on the shaft of a half horsepower gear reduction motor. The result (Fig. 4) for a reflection coefficient of 4/10

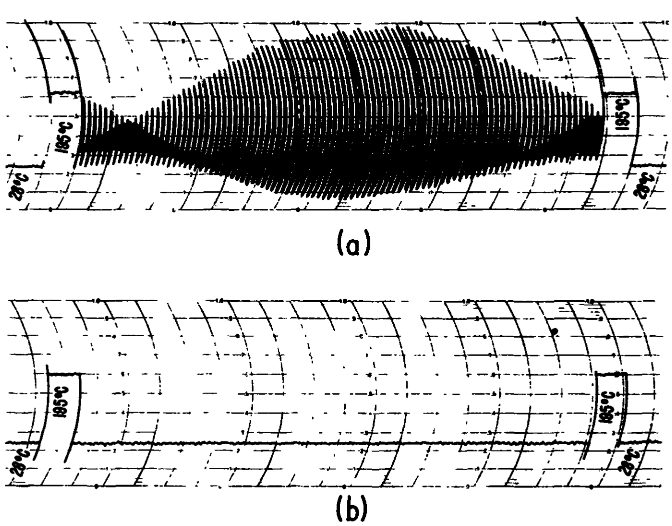


Fig. 4. Comparison of lossy disk chopper (a) and the ferrite switch (b).

(made purposely high for illustration) is shown for the lossy disk chopper (above) and the ferrite switch (below), illustrating the dramatic improvement of more than 2 orders of magnitude using the ferrite switch.

Those improvements made possible much more sensitive and accurate measurements. Figure 5 simply shows the operation of the radiometer. This is a drift curve across Venus, with a calibration signal of 13.8K.

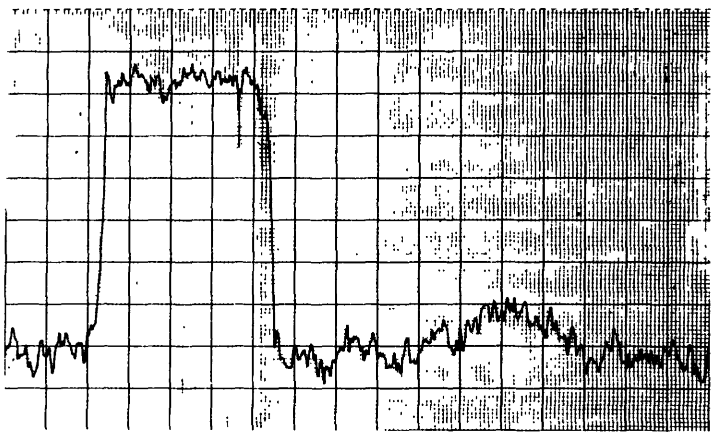


Fig. 5. Drift curve across Venus and 13.8K calibration signal.

Figure 6 summarizes the measurements of Venus in 1956 at 3 cm. The dots in the upper diagram are the antenna temperatures of individual drift scans across Venus, and the circles are the daily averages. You have heard us brag about the large effective area of the 50-foot reflector at 3 cm wavelength, but you have never heard anybody brag about the pointing accuracy of the antenna! The reflector was mounted on an old anti-aircraft gun mount which was designed to whip gun barrels suddenly and rapidly to different positions. It was not the least bit suited to the large, springy structure which was mounted on it. As a result the best possible pointing we ever saw just in terms of resetability was about a minute of arc rms; not good enough for the 8'.5 half power beamwidth of the antenna. Much of the scatter in the data was due to pointing, and it made it necessary to take all of the data in the form of drift scans. What we did was to space drift scans one minute apart in declination up and down, and up and down, and the 600 drift scans you see plotted here were selected from the 1400 taken as being hopefully within one minute of arc of the declination of Venus. The lower diagram shows the derived brightness temperatures of about 600°K, calculated from the daily average antenna temperatures and from the effective area of the reflector.

Unfortunately, Kellermann and Pauliny-Toth hadn't been invented yet, so we had to try to calibrate the collecting area of the antenna. We made measurements using a line of sight transmitter at a distance of 13 miles and a well calibrated standard-gain horn antenna for the calibration, along with measurements of Cas A and the Crab Nebula to try to calibrate the reduction in the effective area at altitude angles lower than about 25 to 30 degrees caused by a gross gravitational distortion of the reflector surface. This gave a value for the aperture efficiency of 56% to 1/2 db accuracy at elevation angles greater than 30°, with some reduction at lower elevation angles.

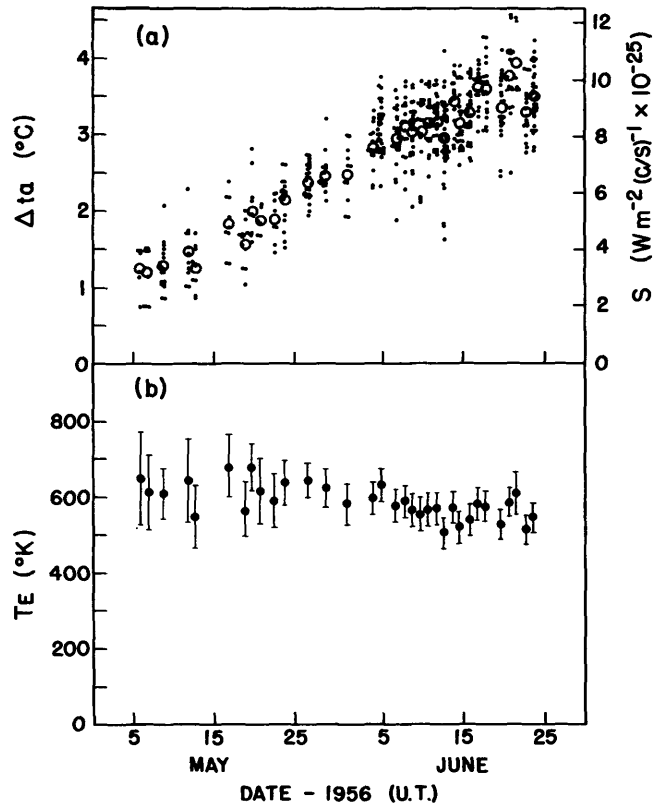


Fig. 6. Summary of 1956 Venus observations showing (a) measured individual drift scans (•) and daily average flux flux densities (o); and (b) daily observed brightness temperature.

The 600° brightness temperatures were so surprising that we made two very hurried and frantic attempts to make measurements at 9.4 cm with a radiometer which existed but which was not completely outfitted for the 50-foot reflector. So in one day's time we mounted that radiometer on the reflector and three days after inferior conjunction we got some measurements at 9.4 cm which were severely confused by interference from the sun in the sidelobes. But even so, four drift scans were usable. The average gave a brightness temperature of 430°K. A month later the interference from the sun was gone, but of course Venus was much weaker because it was farther away, and in one day we got seven good drift scans which averaged together point by point gave a brightness temperature of 740°K. So if you averaged all 11 drift scans together, you got a brightness temperature of 580°K which agreed fortuitously almost perfectly with the 3 cm results.

In spite of the very poor accuracy of the 9.4 cm measurements, the average value seemed to indicate that no appreciable part of the radiation had a spectrum greatly different from a black body spectrum. Also midway along in the period of the measurements we rotated the whole radiometer by 90 degrees so the plane of polarization rotated by 90 degrees. There was no evidence for linear polarization, so we were personally convinced that we had measured thermal radiation from at or near the surface of Venus where the physical temperature is greater than 600°K.

This was not an altogether popular result. In fact, it was very disappointing to many people, including a number of prominent astronomers, and other important people, who were reluctant to give up the idea of a sister planet and perhaps even the possibility of life.

I received a phone call one night. A belligerent voice said, "Are you the people who are claiming Venus is too hot for life?" I replied, "Yes." The voice said, "Well, you're wrong! A friend of mine named Buck Nelson just returned from a tour of the solar system including Venus, and he says it's a very beautiful place with mountains and valleys and streams and rivers, and so forth." Well, of course, I thought at the time that this was some joker in the next office! A few days later an article (Fig. 7) appeared in the Washington Daily News, which no longer exists, saying that in fact Nelson was going to address the local Flying Saucer Society on his trip to Mars! So, actually, there was a Buck Nelson, at least sort of!

GOOD GRAVY! SEND THE SAUCER EDITOR!

Diet Shop Dishes Up Surprises

By DON MACLEAN

"Send your Flying Saucer Editor right down to the Good Diet Health Shop," the tired, little woman said, "and he certainly will be surprised.

"He can't miss it. It's at 1226 H-st nw, right across from the Art Theater and the Masonic Temple."

DISPLAY

On arrival, the Flying Saucer Editor looked in the display window at cans of yogurt and wheat germ, copies of a book by Gaylord Hauser, and copies of "The Search for Bridey Murphy," and "Unidentified Flying Objects."

Inside he found a few small booths, shelves filled with jars of health foods, bags of beans, a big, vacant hornet's nest and the owner, Mrs. Iva Burkholder.

"The saucer people and other societies meet here all the time and post their coming meetings on that blackboard," she said.

WHAT'S NEXT

The blackboard said a saucer meeting was scheduled for later that night—with Buck Nelson, an Ozark farmer who claims to have been to Mars by saucer, speaking. The Theosophical Picnic was the next big event.

A pamphlet called "The Little Listening Post," displayed on the food counter, said Sept. 6 is International Saucer Sighting Day. All sightings should be reported to a man in London who is keeping track.

There were other saucer pamphlets.

"The saucer people have an office on the third floor, but everyone's over at the meeting in the building

BUCK NELSON

Nelson said. Mr. Nelson, who was wearing overalls, added that he didn't have pictures of the saucers except for some drawings in magazines.

He said Canada was building a saucer, but the U. S. Air Force took it over.

"Didn't want it to get built," a woman snorted.

Mr. Nelson agreed.

"Monopoly, that's what," the woman said.

NEWS

"Darn thing is supposed to circle the earth, round and round," the man said.

"Lawzy, I didn't know that," Mr. Nelson said.

Our saucer man skimmed off then and we haven't seen him since.

Probably in his cups again.

German Conscription Bill Nears Approval

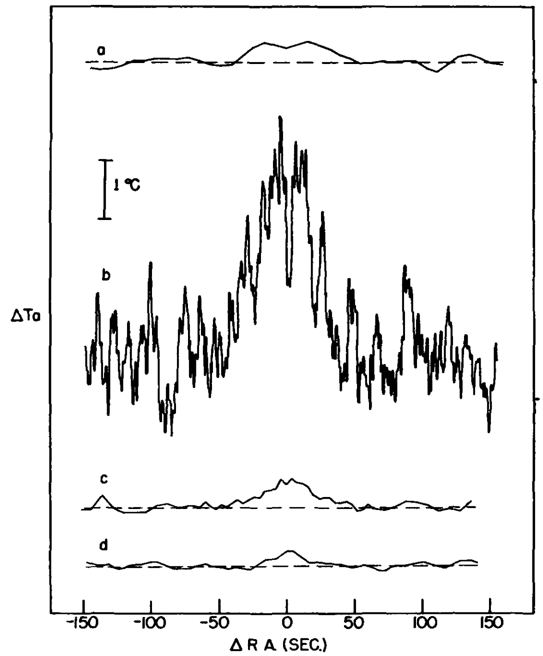
BONN, Germany, July 5 (UP)—Chancellor Konrad Adenauer's coalition government today sent its 12-months conscription law toward final passage over the protest of angry anti-rearmament Socialists who stormed out of a Bundestag meeting.

Government observers predicted the bill will be passed before Parliament begins its summer recess Saturday—a goal set by Adenauer several months ago.

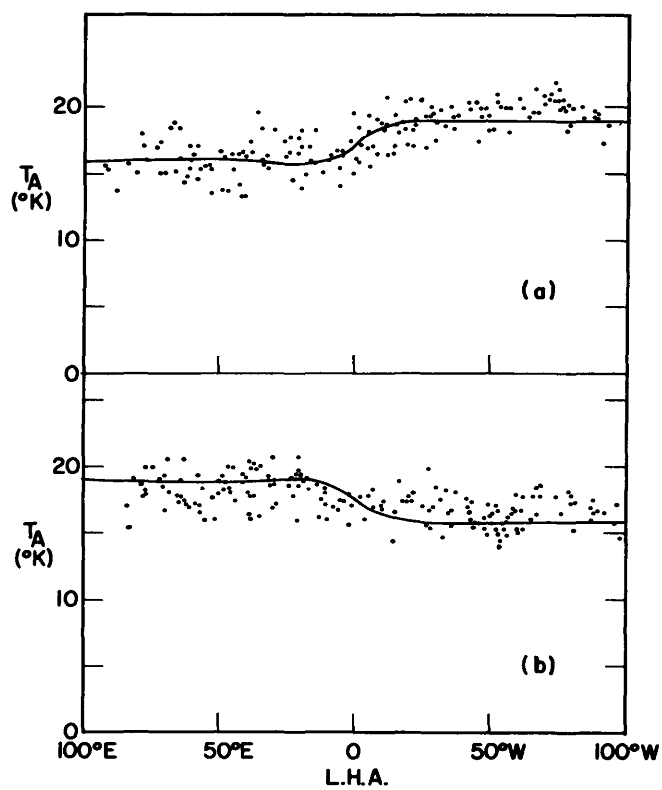
Fig. 7. Article from 1946 Washington Daily News.

Frank Drake has already given you part of the follow on results. Also in 1956 we measured Mars and Jupiter (Fig. 8). In both cases the signals were very weak and required point by point averaging in order to see anything at all. We measured a brightness temperature of approximately 145°K for Jupiter at 3 cm, which wasn't too surprising. Mars gave a brightness temperature of 218°K, again not very surprising.

Now for the serendipitous part. It just so happened that at this inferior conjunction, Venus was very close to the Crab Nebula, which we could use as a pointing object and at the same time investigate for polarization. This diagram (Fig. 9) shows the antenna temperatures measured for the Crab Nebula



<u>Fig. 8</u>. Measurements of Mars and Jupiter. a) Average of 11 drift scans at 9.4 cm across Venus; b) Single 3-cm drift scan across Venus; c) Average of 45 3-cm scans across Jupiter; d) Average of 71 3-cm scans across Mars.



<u>Fig. 9</u>. Antenna temperature of Crab Nebula vs local hour angle. Solid line represents 9% polarization in p.a. 140.

as a function of local hour angle for the two orthogonal polarizations of the two halves of the period, and the solid line represents the variation you would get for 9% polarization at 140° position angle. That, we thought, was a detection of polarization. I guess you could argue about it.

Then we arranged for the feed horn on the reflector to rotate continuously and these (Fig. 10) are examples of the variation of the output of the radiometer feed horn with rotation at 5 different parallactic angles, showing the relative phases which you expect with parallactic angle. This gave 7% polarization at 148° position angle.

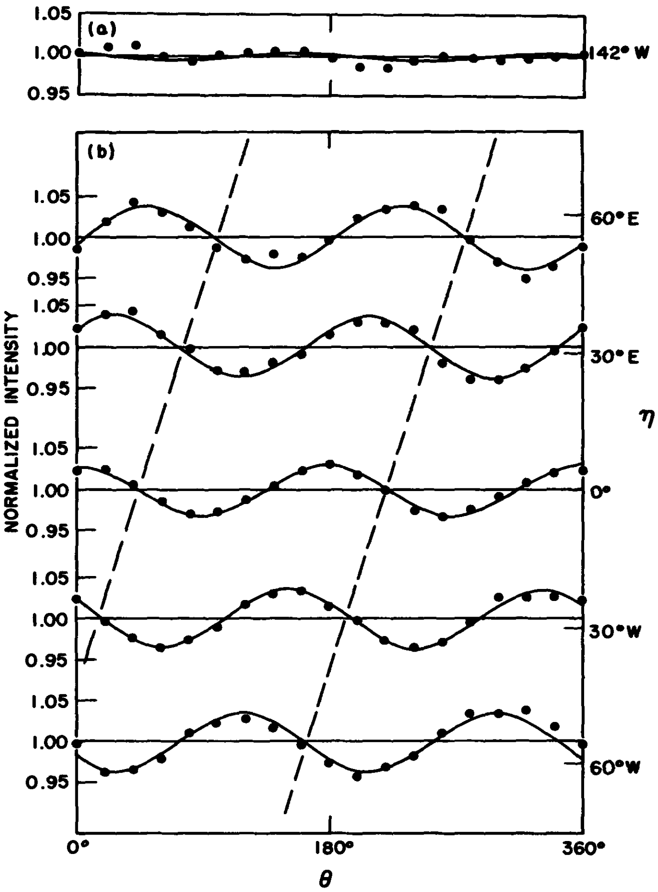


Fig. 10. Radiometer output from rotating feed at 5 different parallactic angles. Upper trace is for Cas A.

I assumed that I should talk about our stuff, since that's the only thing I know anything about, but for completeness I think this list (Table 1) represents the first observations of thermal radiation from the planets. If there are mistakes on here, please tell me, but I think the remaining first observations were in 1957, Saturn by Drake and Ewen; 1962, Mercury by Howard, Barrett, and Haddock; 1964, Uranus by Kellermann; 1966, Neptune by Kellermann and Pauliny-Toth.

TABLE 1

First Observations of Thermal Radiation

1956	Venus	Mayer, McCullough, & Sloanaker
1956	Mars	Mayer, McCullough, & Sloanaker
1956	Jupiter	Mayer, McCullough, & Sloanaker
1957	Saturn	Drake & Ewen
1962	Mercury	Howard, Barrett, & Haddock
1964	Uranus	Kellermann
1966	Neptune	Kellermann & Pauliny-Toth

DISCOVERY OF MERCURY'S ROTATION

Gordon Pettengill
Massachusetts Institute of Technology

I thought I would first try to recount some of the incidents that happened more than 18 years ago. I did want to start off with a description of the origin of Arecibo as an observatory, but Frank Drake has already given the highlights of that, so I won't pursue it further. But it definitely had a happy accident to it. Originally, Arecibo was justified by a semi-military interest. In those days we had money to put into somebody's gamble; that is in most respects not true today, at least in the areas that we are working in.

Nevertheless, the Arecibo project was seized as an opportunity to do some good radio astronomy and, with a large radar, some radar astronomy as well. Although it was a little bit inconvenient, the limited sky coverage which came with the particular design forced the site to be placed in the tropics where it could get under the ecliptic and at least have some part of the year when it could see planets without swinging over too far. So, it was decided to place the site in Puerto Rico even though it was logistically a little harder on some of the people who had to build it and operate it down there. I think it was Carl Slettin who came up with the idea of feeding a spherical reflector and allowing it to be steered, so we could use it for radio and radar astronomy. Most of the intended ionospheric use was at that time thought not to need a moving antenna, so it was a lucky accident that a lot of people were in the right place at the right time to take advantage of this opportunity. By the time it was discovered such a large antenna wasn't necessary to meet the original ionospheric objective, it was far too late to kill it. It had just rolled down the slope picking up momentum, and too many people were too heavily involved. At any rate you know the history of that, so I won't go into it further. But it's clear that without that system we never would have had the sensitivity needed to look at Mercury as well as to make many, many other types of observations.

The next point I want to make is to pick up on something that Geoff Burbidge said yesterday, that the difference between astronomy and most high energy physics was that the former was based solely on cosmic observation whereas the latter required more truly laboratory style experiments. I think what he meant was that because astronomy is subject to the whims of nature, sending this relatively incoherent emission to us, in some cases absorbing it along the way, all we can do is look out there and try to understand. The physicist, on the other hand, can go to the laboratory, build big magnets and make an accelerator, then turn the thing on and polarize his protons. Whatever he wants to do, with enough money he can do, and when he is finished he has a very tight experiment. There is a difference, a major philosophical difference as compared to passive observation, I will admit. Well, it is interesting to note that radar astronomy holds an intermediate position here. We can invite the planets into our laboratory by sending out their illumination. In the case of planets we're not restricted to a black body shining very broadband unpolarized radiation on the target, as is the case with sunlight. With radar we're sending out a coherent, completely polarized waveform. We can mark the waveform, we can frequency shift it, we can point it in different directions, and that means that we have much more information in the echo that comes back. So we're halfway in the laboratory; in a sense we're one step beyond the usual astronomy, but not quite as far as the laboratory physicists.

A direct result of the use of coherent waveforms is the technique of delay Doppler which allows you to measure the time of flight by marking the waveform, while at the same time preserving spectral information. The combination of these two observables allows you to locate the sources of echoes on a distant rigid target. This was a technique that started in the late fifties, and by the time Mercury observations came along was reasonably well developed.

Figure 1, dating from April 1965, shows spectra for a set of slices each at constant delay, taken at well-separated intervals. It shows that the earliest echo, reflected from nearly the center of the spherical target, has

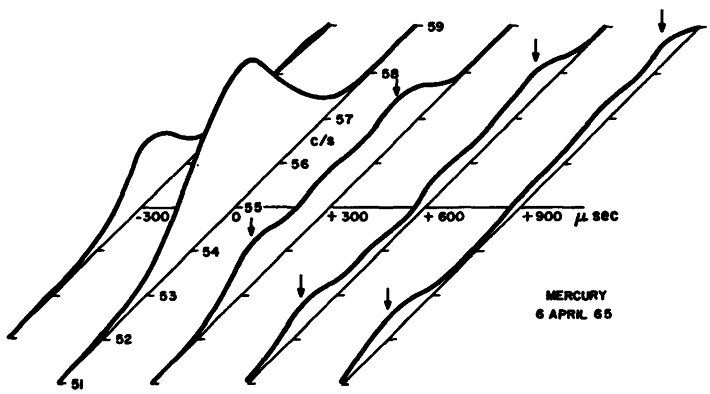


Fig. 1. Spectra for 70-cm wavelength radar echoes from Mercury, centered at an arbitrary frequency (55 Hz), taken at delay steps of 300 μsec with respect to the nearest portion of the planet. Arrows show the limits of each echo spectrum.

a fairly narrow spectrum because only a very small front cap of the rotating target is available to disperse the spectrum of the returned signal. As you move further into the target, taking broader slices further and further back from the front edge, the spectral width increases. Since you know the relative distance of each of these spectra, you can solve for both the radius of the target, assuming it's spherical, and its angular rotation as viewed from the observer. This was the approach we were using to determine the rotation of Mercury. We already knew the radius well enough, of course, so that we didn't need to estimate it in these observations.

With data of this sort, then, one can determine the apparent rotation of the target. Remember this is the sum of an intrinsic rotation as well as the change in the line of sight due to the relative motions of Earth and Mercury in their orbits around the Sun. The latter are well known contributions, of course, so they can be removed.

Figure 2 shows the results from a set of days using the technique I just described, plotted as a function of the limb-to-limb bandwidth that we extrapolated from these measurements. We went into these observations expecting to see a target rotating with an 88-day period in a direct sense. But even with the very first observation on the 6th of April, 1965, we saw that it lay well outside the 88-day prediction. So within an hour or two after the observations, I knew something was wrong, the question was what. Of course, one always suspects one's equipment, but we had been using this technique on Venus the preceding year and even the spring of this particular year, so that we really felt we had an understanding of the technique. My first reaction was not to question the technique, because I didn't see how it could be wrong for Mercury when it had worked so well for Venus. A further goad to my interest arose because we had been scooped by Dick Goldstein and the group at JPL in the discovery of the anomalous rotation of Venus. They had assembled some pretty good radar measurements three years before and concluded that Venus had a peculiar retrograde rotation, and I was trying to make sure that they didn't get ahead of us again with Mercury. So in looking back to see what sensitized us to the possibility of an unexpected result, I think I have to give some credit to JPL for raising my consciousness.

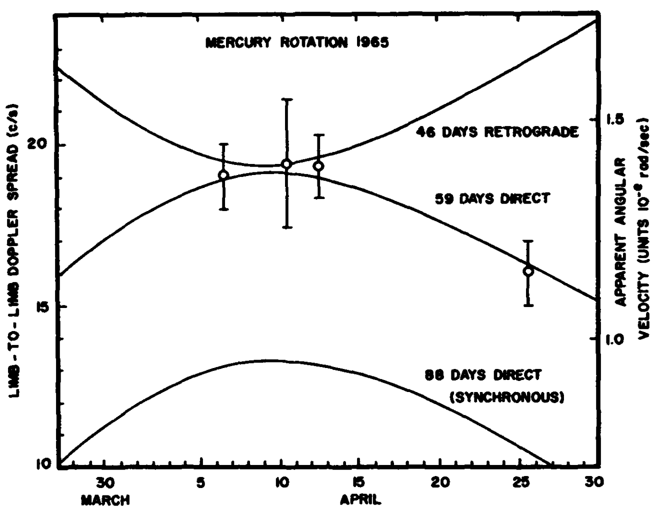


Fig. 2. Plot of the apparent rotational velocity obtained from the radar data as a function of observation date. Solid lines represent the theoretical behavior predicted for various indicated assumptions for the intrinsic spin period.

I think another contributing factor was that we had no detailed knowledge of the classic optical observations of Mercury at that time. Remember, Mercury is a difficult object to photograph because it is close to the Sun, and observatory directors are very unhappy at the thought of all that glass being exposed to sunlight with the possibility of damaging thermal effects. So you have to make the measurements when Mercury is near maximum elongation, and even then preferably near sunrise or sunset to get the Sun out of the picture as much as possible. This procedure doesn't lead to the clearest images! Thus, despite the many years of observation, one can partially rationalize the failure to observe definitive markings on Mercury which would have disclosed the 59-day rotation.

Nevertheless, I understand there were data in hand which could have shown the discrepancy, but because of a bias based on a simplistic expectation, they were disregarded. As you will see in a moment, while Mercury is not in simple one-to-one synchronous rotation, it is still in a rotational resonance. If you ignore every other perihelion passage, you will see the same markings, and that is what the optical observers were seeing. They assumed Mercury had to be locked into a synchronous period, so they just put the odd plates aside, deciding there was something wrong with them, and it wasn't until after the 59-day period was found that they went back and discovered they had two families. They had an even set here and an odd set there, and it all fell together beautifully.

At any rate, these figures show that the 59-day period is required to satisfy the Doppler spread found from the radar data. Initially we weren't certain that the rotation wasn't retrograde rather than direct which would have led to an upward curve as shown in Fig. 2. The curves for direct and retrograde rotation have different shapes because of the vector combination of the intrinsic rotation and the angular motion of the observer as the two planets slide by each other. If the former is direct, the combination leads to a maximum effect at inferior conjunction, and if it is retrograde, there is a minimum because the two terms partially cancel, as is the case for Venus.

However, it wasn't until the last day of this series, the 25th of April, that it was absolutely clear that we had a direct 59-day rotation. At that point we rushed a letter into print. Rolf Dyce and I, with Tommy Gold's help, as I remember, beat the editor of Nature down to a mere three-week publication delay. It showed up in print in May - that must be some kind of a record! We subsequently confirmed the finding at the next inferior conjunction in August (1965) where everything agreed quite well; we were quite certain of the result by then.

Now, about the radiometric measurements a few years before which you have already heard a little bit about: Alan Barrett's and Fred Haddock's measurements, which Connie Mayer presented earlier, showed that the radio temperature of Mercury was not consistent with the assumption of an 88-day rotation period, even if allowance is made for the eccentricity which introduces a fair amount of libration as the planet goes around the Sun. I think Ken Kellermann also had a series of radiometric measurements of the same sort. In any event, there were intimations that all was not well. In the case of the optical data the inconsistency was explained as the result of observations taken close to the horizon. In the case of the radiometric data, there were just too many possible explanations for the 59-day rotation to be singled out. For the radar data, there was no way out. It was structured to shout at you, and I can't claim any great credit for insight; it was just a matter of believing the data.

There was another group of people, however, who should have predicted the 59-day rotational lock. Figure 3 is a diagram of Mercury in its orbit around the Sun, where the arrows indicate the planet's orientation as it goes around. Because of the large eccentricity, the orbital motion near perihelion is nearly twice that at aphelion. Tommy Gold and Stan Peale were very quick to pick up on the fact that with a 59-day period the rotational motion matches very closely the orbital motion at perihelion. And they pointed out that this was what you would expect since the tidal torque varies as the inverse sixth power of the distance from the Sun. The inverse sixth power means that the tidal torque is 64 times greater at perihelion than at aphelion, and one would have expected the tidal dissipation to have slowed the initially much more rapidly rotating planet to about 59 days as it passed through perihelion: just

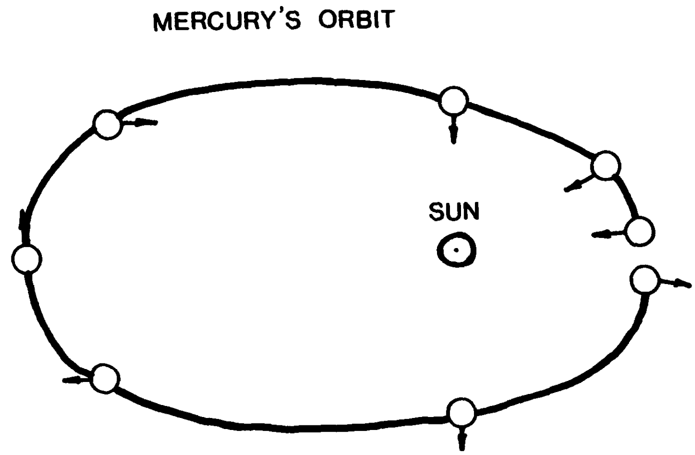


Fig. 3. The rotational history of Mercury for one orbital revolution around the sun, during which it executes one and one-half rotations in an inertial frame. The diagram shows how the orbital and rotational angular rates are nearly matched near perihelion.

what we see. Gold's and Peale's letter was published in the same issue as our observational result and made a lot of sense. What neither we nor they realized at the time, however, was that the 59-day period is very close to a 3/2 resonance referred to the 88-day synchronous value (realized shortly thereafter by G. Colombo). If Mercury was originally spinning rapidly in a prograde sense and then was gradually slowed down through tidal dissipation, it would first have to pass through the resonance at 59 days before it hit the resonance at 88 days. Even if the former were not as strong a resonance as the latter, being the first one hit it would lock the rotation. In fact, it turns out that the depth of the 59-day resonance is very much greater than at 88 days. Any theoretician who had looked at this fact, even at this stupidly simple level, would have immediately realized that Mercury had to be rotating with a period of 59 days. I think this has to be another form of oversight.

G. Westerhout: Bernie, may I keep people from coffee for one more minute? After all the discussions about Reber, I asked Omar Bowyer yesterday, "Do you still have all that Litz wire that Reber ordered when he was here in 1959 for a few years?" For the younger people, Litz wire is what you make your own coils with. Omar Bowyer said, "Sure. There is a large supply of Litz wire here somewhere at the Observatory."

B. Burke: Litz wire is a multiple stranded copper insulated wire.

THE BEGINNINGS OF MOLECULAR RADIO ASTRONOMY

Alan H. Barrett
Massachusetts Institute of Technology

I'm going to talk primarily about the early days of molecular line studies and OH in particular. I'm sure many of you know parts of the story but I don't think very many of you know it all; or you knew parts of it and have forgotten it. It began in the early 1950s when Townes and Shklovsky, approximately simultaneously, published their lists of potential lines of possible interest to radio astronomy. OH was on both lists. At that time the frequencies of the OH ground state transitions had not been measured in the laboratory. There were measurements made of higher energy states of OH by an associate of mine. We had adjacent desks in a graduate student office at Columbia University. George Dousmanis, working with Townes, as I was, made the initial microwave measurements of OH, but in higher rotational states. Townes computed the ground state frequency on the basis of the constants established from their measurements at higher frequencies.

They hit on a number which was very good. It was around 1666 MHz, but the estimated accuracy was plus or minus 10 MHz, or so. About that time I finished up my Ph.D. and was interested in radio astronomy, without knowing at the time Townes' strong interest in that area. He steered me to NRL. The first experiment I ever did in radio astronomy was an OH search at NRL, using the 50-foot antenna with Ed Lilley. We scanned over the ± 10 MHz band searching for absorption in Cassiopeia, but never saw it. The receiver wasn't what they are today and without knowing the frequency and without having digital apparatus to average all the data, we missed it completely; but then that's not too surprising. What we did see, however, we got very excited about. This was a very strong emission line from Cassiopeia. We were so excited that we began composing a telegram to send to Townes congratulating him on his prediction. But it wasn't OH at all and no telegram was ever sent. What we didn't know, but we soon learned, was that it was a meteorological weather balloon operating at 18 cm! There were a considerable number of these in the Washington area.

Eventually the line was measured in the laboratory, in 1960. The frequency was then known, but there appeared to be no interest among the radio astronomers to take a look even though anybody with a reasonable-sized reflector would have found the line had they looked.

In 1961 I went to MIT and was very interested in doing this experiment. Weinreb was finishing up his thesis, at that point, which was building the correlator. He used it for two radio astronomy experiments at Green Bank, both of which were negative. One was the search for the 327 MHz line of deuterium, and the other was the Zeeman experiment at 21 cm. His apparatus came back to MIT, he was awarded the Ph.D., and went to work for Lincoln Laboratory. The equipment was installed at the Millstone Hill telescope of Lincoln Laboratory, and we started the OH experiment. Weinreb, Meeks, Henry and myself, detected OH almost the first night after we got everything working properly. The light line of Figure 1 is the spectrum we obtained when we looked off Cassiopeia; there is essentially zero emission there. The heavy line is the strong absorption of OH at 1667 MHz on Cas A. We were very excited about this, needless to say.

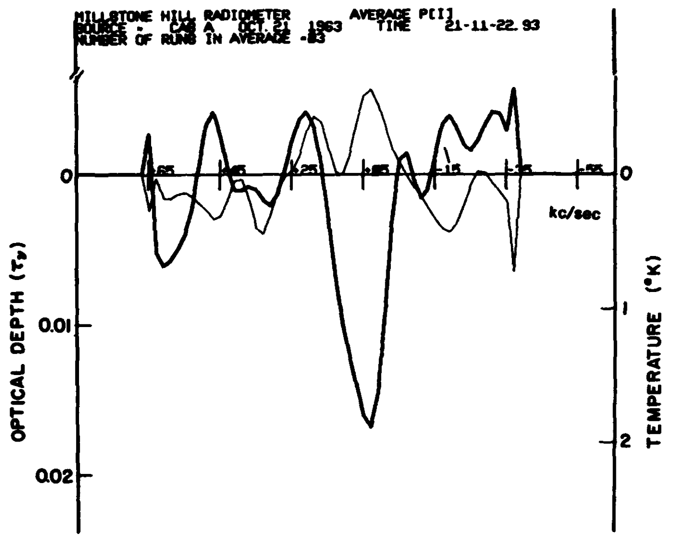


Fig. 1. OH absorption at 1667 MHz in Cas A. The light line is the spectrum obtained with the antenna positioned adjacent to Cas A and the heavy line is the spectrum when the antenna is directed at Cas A.

There was at least one other search for OH underway at the same time. Penzias was working with a horn antenna at Holmdel, New Jersey, looking for OH in emission, and we knew he was doing this. Fortunately for us, his results were negative. Also, I had attended a conference at JPL to discuss Venus, particularly Venus microwave measurements. We were proposing new frequencies for ground-based observations of Venus and the Caltech representative, my memory fails me as to who it was, volunteered that they could observe at 1666 MHz. That was not a common frequency then and, since we had not yet started our observations, I was worried they would detect the OH lines before we were on the air. It was only after we had found OH that they put their receiver on the antenna.

There is another interesting sidelight to the OH story. A meeting was underway in Geneva, which involved frequency allocations, just at the time we detected OH. A cable was sent to George Swenson, the U. S. representative, informing him that we had detected OH absorption and specified the frequencies to seven significant figures. We were naive in hoping that at an international conference you could announce a new microwave, interstellar line and expect it to have impact at the last minute. When Swenson got the cable he called up van de Hulst and asked about the possibility that what we had detected was absorption in the Earth's atmosphere. That says something about what Swenson thought of our capability, I think! Van de Hulst told him that the accuracy to which we specified the frequency precluded it being atmospheric OH.

However, the Australian representatives also were at that conference, so when Swenson posted the telegram on the bulletin board they alerted John Bolton to the OH detection by the MIT group. CSIRO astronomers immediately started their own work hunting for this line in the galactic center.

I think it is interesting that within one month of our finding the OH line three groups of radio astronomers had also detected it. They confirmed our result, which simply says that this line had been available for three years, after the measurement in the laboratory, but nobody bothered to look for it. Once it became known we had detected OH, three groups confirmed it immediately: Weaver and Williams at UC Berkeley, Dieter and Ewen (the same Ewen you know from 21 cm fame), and the Australians, Bolton and company.

The Australians found OH in absorption in the galactic center as shown in Figure 2. The result is not what one would have expected. One expected the OH absorption to occur at the same velocity as the H 21-cm absorption, primarily at 0 km/sec. When they looked at the OH frequency, 1667 MHz, what they thought was OH absorption was the small absorption at 0 km/sec and the rest of the record was thought to be due to baseline effects. They removed the receiver from the antenna, retuned it, and reinstalled the receiver on the antenna. The observations were repeated, and also extended to the 1665 MHz line, and the result was the same, i.e. strong OH absorption at velocities which did not match the 21-cm absorption. They rushed off and published this result and sent me a cable that they detected OH in absorption. I was very pleased because this was the initial confirmation to reach us. It was unexpected to find OH moving inward toward the galactic center whereas the hydrogen was moving outward. That was a surprise. Also note, the two lines are not in the expected intensity ratio of 9:5.

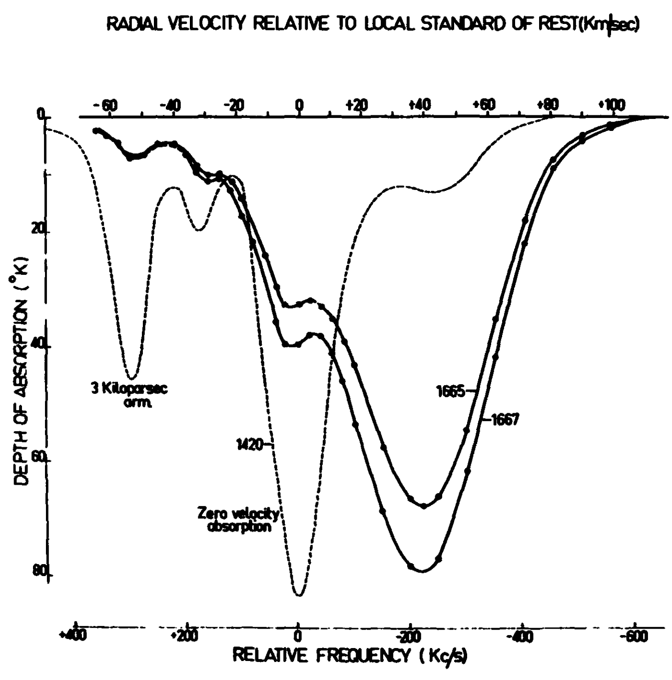


Fig. 2. Early results of OH absorption against the galactic center at both 1665 and 1667 MHz. The 21-cm H absorption is also shown by the dashed line.

[An interesting footnote: Jocelyn Bell told me, during the coffee break, that she was working as a summer student at Jodrell Bank at the time and observers there had a similar result in the galactic center. It was generally believed that the students had made a mistake in the data reduction.]

What the Australians did not realize when the results in Figure 2 were published was that they really didn't have the whole story. Shortly after, Harvard published the result shown in Figure 3. The Australians, in their initial OH search, had stopped their search at -60 km/sec. There is additional OH absorption at -100 to -200 km/sec where there is no hydrogen counterpart at all. Finally, we have the whole story of OH absorption in the galactic center.

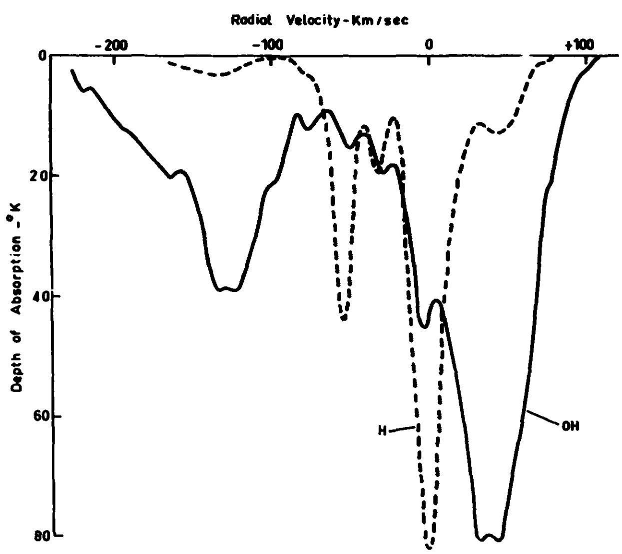


Fig. 3. The full H and OH absorption spectra in the galactic center.

It became important to detect OH emission because absorption measurements enable one to obtain the optical depth only. However, that doesn't enable one to determine the column density; one can only determine the column density divided by the excitation temperature. One has no way to unscramble these two from absorption measurements only.

The Harvard and Berkeley groups announced the detection of OH emission almost simultaneously. As you know, the emission was very strong in HII regions, obviously nonthermal because of wild departures from the thermal intensity ratios of 9:5:1. When one looked elsewhere, one didn't see it, at least with the sensitivity available in those days. The emission was so intense that the Berkeley group thought they had found a new, unidentified, line at 1665 MHz and nicknamed it "Mysterium". They were unable to find emission at all four OH frequencies, which is why they thought they had a new line. The MIT group, using Haystack, looked at all the lines and found emission at all four frequencies. It was very highly polarized at 1665 MHz, as shown in Figure 4. At the other OH frequencies the line is very much weaker. So there was no Mysterium at all; it was really OH, anomalously excited. This was the start of the idea of maser emission.

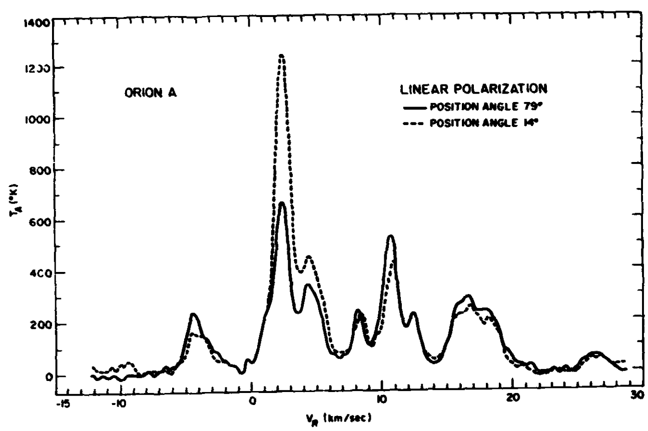


Fig. 4. An example of the intense OH polarized emission at 1665 MHz.

Observers began a long series of interferometric observations with ever-expanding baselines. There were many people involved; the principal ones at MIT were Bernie Burke, Alan Rogers, and Jim Moran. I was involved also in many of the early experiments. Work was underway at Jodrell Bank also, and both groups showed that OH emission was unresolved for all baselines in use at that time. Finally VLBI techniques were brought to bear. As the angular size of the source got smaller, the brightness temperature got higher and higher. It is 10^9 K, and higher, in some of the maser clumps of OH. That was the beginning of celestial masers.

The Australians went back and looked at their old data after discovery of OH emission was announced. Their previous results for all four OH lines are shown in Figure 5. Note the "noise bump" at 70 km/sec in the 1665 MHz spectrum which is absent in the other spectra. This had been attributed to instrumental effects and was ignored. Once the maser emission was announced, very strong, very narrow, and predominantly at 1665 MHz, they went back and observed that so-called "noise bump" with a high resolution filter. The result is shown in Figure 6. In reality, they had observed OH emission one year earlier than anybody else, but had failed to recognize it.

All that was transpiring between 1963 and 1968. Townes was at MIT at the beginning of that interval, was very interested in the OH developments, and urged us to look for ammonia. The construction of a radiometer for ammonia was started at Haystack. In the meantime, Townes moved to Berkeley and got Welch interested in studying ammonia. Ammonia has a number of lines at about 23 GHz. They built a radiometer, used a 6-meter dish, and quickly found ammonia. Townes called me one night, when he was at his farm in New Hampshire, and told me they had found ammonia in the galactic center, and several other sources. Two weeks or so later, the phone rang again. This time Townes told me of their discovery of H_2O. I was amazed. The water vapor energy levels responsible for the line are ~ 450 cm^{-1} above the ground state, and so one would expect weak emission, at best. But they found strong emission, 60 K, or something like that, with a 6-meter dish. This was not one of those discoveries where you needed new instruments and/or new techniques. The water vapor line could have been found many years earlier, perhaps even before the hydrogen line, with the apparatus available at the time.

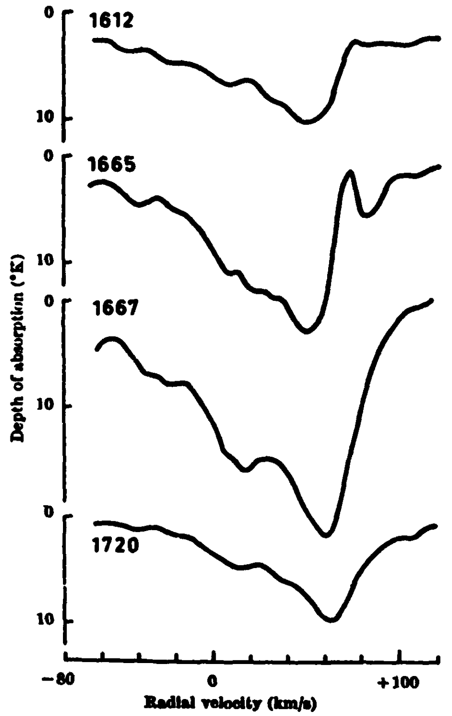


Fig. 5. OH absorption spectra against the galactic center at all four OH frequencies. The emission at + 70 km/s in the 1665 MHz spectrum is reduced because of the bandwidth.

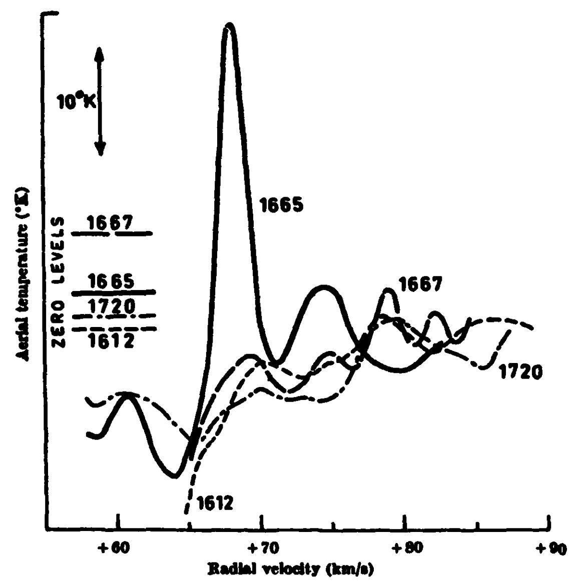


Fig. 6. High frequency resolution spectra of the + 70 km/s region for all four OH lines when viewed toward the galactic center.

At Haystack the radiometer was hurriedly completed and we began observing ammonia and water vapor. Spectra, such as shown in Figure 7, taken at NRAO with the 140-ft telescope, revealed high antenna temperatures and clearly showed the existence of another maser. Note that the 140-ft telescope was used at 1.35 cm wavelength 14 years ago.

There is an interesting aspect to the discovery of water vapor. We were talking yesterday about the fact that if you've got wild ideas you're not given observing time on an antenna. Snyder and Buhl wanted to look for water vapor, so they proposed it to NRAO in the usual way. NRAO sent it out to its outside referees. It was not treated too kindly and the assignment of observing time was seriously delayed. In the meantime, Townes, Welch, et al. had their own search underway and found H_2O first. You may draw your own conclusions from that.

The next line discovered was formaldehyde. It had its own surprises. Its excitation temperature is less than 3 K, and so is seen in absorption almost everywhere, even against "cold sky. That led Schawlow to coin the term "daser" after the maser and the laser, of course. Daser was the "darkness amplifier by stimulated emission of radiation!" But the term never stuck!

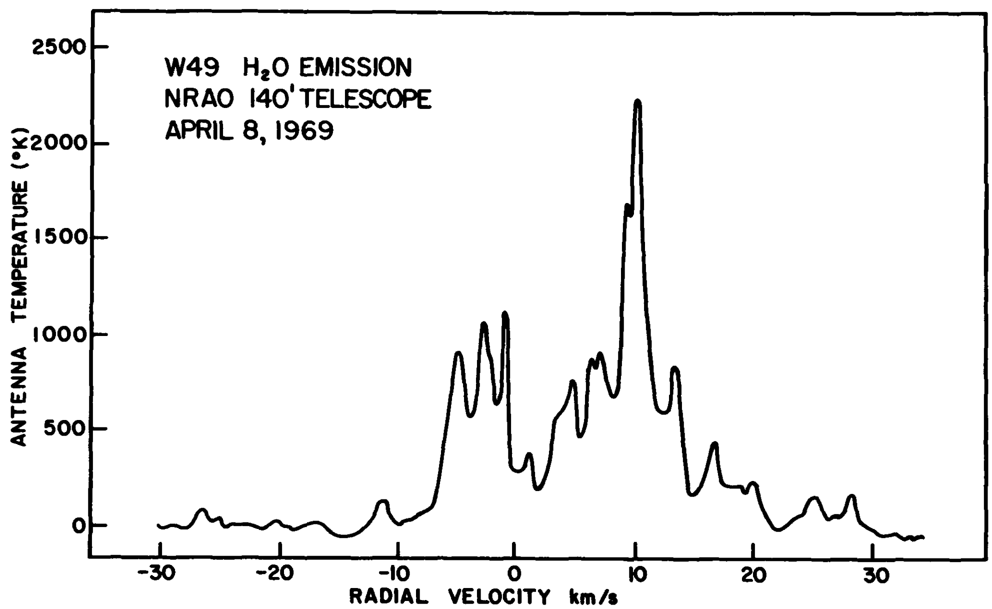


Fig. 7. An example of H_2O maser emission.

Things took a quite new tack when Wilson, Jefferts, and Penzias looked for CO at Kitt Peak and found it immediately. The emission was very strong, which was surprising at the time because one thought it wouldn't live very long in its excited state and therefore it would be hard to detect in emission. CO emission might be expected in very dense regions where the density was high enough to collisionally excite the molecule. That is more or less the case. However, a density of about 10^3 cm^{-3} is sufficient and that now seems to be fairly common. From that time on molecular astronomy mushroomed and we now know about these ridiculously complex molecules floating around in the interstellar medium. CO was so especially valuable because of its three isotopes and because it is an excellent tracer of molecular hydrogen, otherwise unobservable when its temperature is less than 100 K, as is much of the interstellar medium. CO emission has traced out the large clouds in the galaxy which Scoville and Sanders first called "giant molecular clouds." These clouds are the most massive objects in the galaxy, 10^6 - 10^7 solar masses, and were completely unknown 10 years ago. Generally the gas density is of the order of 10^3 - 10^4 cm^{-3} but gets as high as 10^5 - 10^6 cm^{-3}.

B. Burke: When was it clear that giant molecular clouds existed?

The initial CO detection was in 1970. The first paper noted that CO emission in Orion extended over plus-or-minus 25 minutes of arc.

R. Wilson: The initial observation was adequate to say that there was a giant molecular cloud in Orion, but it extended over almost a degree. It was hot in the middle and dropped off, but just kept on going. I don't think we realized it immediately.

B. Burke: Well, that's what the question is. When was the term "giant molecular clouds" first produced?

I had a hard time tracking that down. I believe it was at the Third Gregynog Symposium in 1977.

Undoubtedly one of the main uses that will be made of CO, in the future, is in extragalactic studies, and I'll just conclude by showing the differences in the radial distribution of gas in galaxies using CO as a tracer of the hydrogen molecule compared to 21-cm studies. Figure 8, supplied by Solomon, shows the atomic hydrogen distribution in M51. Also shown is the molecular hydrogen distribution if the CO is actually tracing the molecular hydrogen.

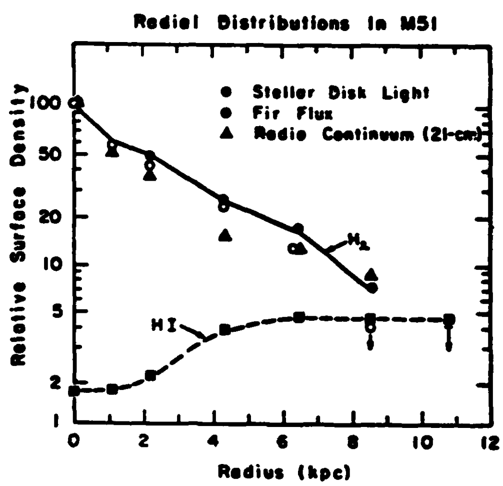


Fig. 8. The radial distribution of surface brightness of atomic H and molecular H_2 in the galaxy M51. The H_2 distribution was derived from CO observations.

Our ideas about the gas distribution on a galactic scale may be in for a drastic revision. These studies are just beginning and suffer from a resolution which is only about one minute of arc. It is also found that the distribution of gas in the Milky Way appears different from most other galaxies, as shown in Figure 9. The Milky Way distribution drops significantly inward of 6 kpc, which is not seen in most other galaxies. The only other galaxy which shows something like this is M31, a weak emitter and therefore the distribution is not clear.

Molecular studies which span the range from 800 MHz to hundreds of GHz, have added immeasurably to our knowledge of the interstellar gas, star formation, and galactic structure, and one can't possibly do justice to the subject in any one talk. One could never have guessed the molecular complexity that

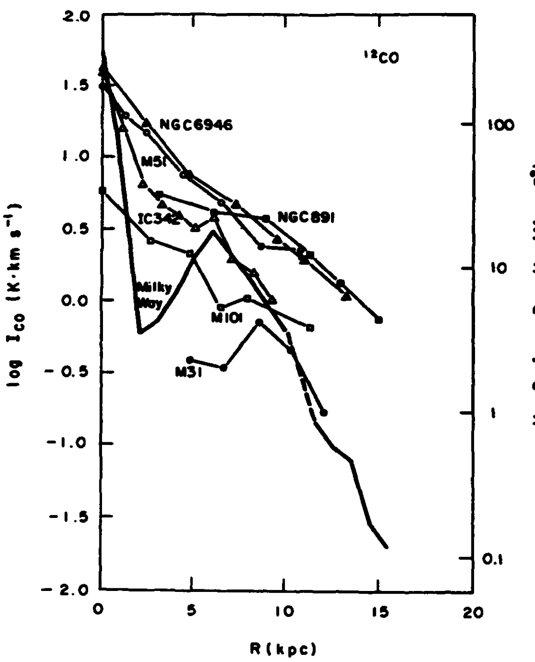


Fig. 9. The radial distribution of CO emission for various galaxies. Note that except perhaps for M31, a weak emitter, the distribution in other galaxies does not conform to what is believed to be the distribution in the Milky Way.

awaited us, let alone the discoveries of cosmic masers in OH, water vapor, SiO, methanol, formaldehyde, ammonia, and maybe other weak masers that we haven't learned to recognize.

W. E. Howard: I want to tell a very short story which amplifies a statement made earlier by Al Barrett. It has to do with a few incidents that took place before the Buhl and Snyder discovery of formaldehyde and also links to the account of the earlier discovery of water vapor and ammonia. There were three significant frustrations, I think, that Lew Snyder experienced before he discovered the formaldehyde line. The first one came because he was basically a physicist coming from Lansing, Michigan into a new radio astronomy environment, and he wanted telescope time to do these detections alone. But in his first round with the referees, they thought he ought to team up with somebody who knew his electronics a little better. That prompted the very fruitful collaboration with Dave Buhl. This first problem with the referees was frustration number one.

Frustration number two came because they had applied for telescope time, as Al suggested, for the water vapor line but they wanted to do it primarily on Venus. During the time when Venus was not up, they wanted to observe various galactic HII regions to see what might be there. That did run afoul of one of the referees, as Al mentioned, because at the time that they had chosen to do this (and, of course, they had specified the time), the velocity of Venus was parallel to the Earth's. This would have led to the optimum situation for confusing any water vapor line in Venus with the telluric line of water vapor in the Earth's atmosphere. The referee suggested the observation not be made at that time. I was faced with the possibility of either scheduling it earlier, which turned out not to be possible because of receiver *availability, or later, and so through discussions with the proposers it was decided to do it later. This was Lew Snyder's frustration number two.*

The third frustration then occurred. Dave Buhl, because of his background and contacts with Berkeley, realized that his thesis advisor, Jack Welch, was on the hunt. For some reason which I cannot understand, he wanted to have the Berkeley group have the first crack at it and he sought a delay. Berkeley discovered the line. Buhl and Snyder purposely delayed what might have been an earlier observation of water vapor; this was Lew Snyder's third frustration.

At that point, since I was in charge of the scheduling and I felt very bad about this, I said to Lew, "I don't care what you put in for next time, I don't care what the referee says, you are on the telescope first thing!" That happened to be the formaldehyde proposal!

That was another interesting story, because Buhl and Snyder had been given telescope time for formaldehyde at the end of March, the month that it was actually discovered. But earlier in March there already had been scheduled a three-week observing period for Pat Palmer and Ben Zuckerman to conduct a 6-cm recombination line program on HII regions. Zuckerman came into my office one day, and he said, "I'd like to add something to my observing program." And I asked, "What's that?" He replied, "Formaldehyde." I then told him, "Well then, you can't do that because of a commitment to another group, but give me some time to see if a collaboration is possible."

So what I did was to see whether we could arrange a joint collaboration between the two groups. The joint collaboration worked all right because it was evident to Ben that he wasn't going to get the time for formaldehyde unless there was this collaboration! It was also evident to Buhl and Snyder that Palmer and Zuckerman had three weeks of time which could be used to exploit a discovery, whereas they only had about three days later in the month for the discovery alone. That was the beginning of a collaboration which proved later to be so fruitful between Buhl, Snyder, Palmer and Zuckerman, a collaboration that led to more excitement and more discovery.

As soon as formaldehyde was discovered, there was, of course, a great effort to keep it a secret which is very hard to do at a national center, as you know. The group wanted the remainder of the observing period to exploit the line and Phys. Rev. Letters, Snyder's choice to publish the discovery, insisted that no announcement be made until the discovery was published. There were people up at the AIP who were writing a press release on the discovery, but unfortunately the word leaked out and a Boston reporter got hold of the story. One of the incidents I can remember was that Sam Goudschmidt, who was head of Phys. Rev. Letters, was going to publish the discovery paper. He recognized that this was a very significant event and I think what he did was to place it ahead of other less timely papers that were also awaiting publication. But since he had been accused of doing this in the past - of putting things at the head of the queue for people at Brookhaven, his host institution - he was very sensitive, because Brookhaven and NRAO, where the paper was originating, are both run by AUI. As it turned out, he was on a plane trip from the West Coast to the East Coast when he stopped in Chicago, bought a newspaper, and saw that the story was out. He stopped by the New York AIP office on the way back, and he was very upset. He telephoned Snyder *and me in my Charlottesville office, and read us the riot act for having leaked the discovery before publication, but of course it was not the fault of any leak by NRAO at all. Someone had apparently learned of the discovery and had mentioned it at a Cornell colloquium from which it found its way to Boston. I was rather upset about Sam Goudschmidt's phone call, so I later called the AIP person who was writing the press release and asked him what had happened for he had been present in the room when the phone call was made. He told me,*

"Oh, don't worry about Sam. I was on the other end of the line. I heard his tirade, and at the end he slammed down the telephone and he said to me with a wink, 'That will put them on ice for awhile; let's get some coffee'!"

THE DISCOVERY OF RADIO NOVAE

C. M. Wade
National Radio Astronomy Observatory

The research, if I can call it that, that I'm about to describe was done with Bob Hjellming. I have no viewgraphs; we destroyed the evidence years ago.

It would appear from the things that have been said in this conference that "serendipity" is a sort of blithe spirit that moves over the face of the earth, bestowing good fortune on worthy people who deserve it. I think I can show today that it works for other people too!

In the spring of 1970, the Green Bank interferometer was taken off the air and converted to operate simultaneously on wavelengths of 11 and 3.8 cm. Bob Hjellming and I had been thinking about the problem of stellar radio emission and, by some reasoning which at the time we thought to be rather clever, we had concluded that red supergiants offered the best chance of being detectable at these wavelengths. To our amazement and gratification, we were assigned the first three weeks of time on the new interferometer system to look for stellar radiation. This of course reflected NRAO's experience that three weeks were usually needed to make any new system work properly; Hjellming and Wade could debug the beast for the benefit of others. As it happened, however, the thing worked perfectly from the first day. We had a heavy observing list of the red supergiants that our exceptional powers of insight had told us to look at. We also had a number of stars that Bob called "wild cards" -- stars of other types that might possibly be detectable. These were to be used as fillers when there were no red supergiants around to fall before our assault.

For the first two weeks we looked at red supergiants all over the place. We even detected one, which we now realize was quite a fluke. That was Antares. Still, we were not doing very well and our hopes of a Nobel Prize were fading, perhaps because we did not have Jocelyn Bell working for us. So we tried some "wild cards". We split the work by having one of us conduct the observing in Green Bank while the other did the dirty work of data reduction in Charlottesville, alternating weekly. It happened that I was in Charlottesville when I got a call from Bob; he had put in one of our "wild cards", and this one actually looked good. It was Nova Herculis 1934, the brightest nova of the present century. It was the only normal nova in our list of "wild cards". It was so strong that it could be seen on the monitor in the control room, and the phase was holding steady. This was the best news we had had since going on the air, so we decided to try another nova.

The second brightest nova of the century was of course Nova Aquilae 1918. Since it was above the horizon at the time, we went for it immediately. We had to face the problem that the coordinates we had were for 1900, while the interferometer required 1950 coordinates. We had therefore to go through the freshman astronomy exercise of precessing the position to 1950. Several phone calls later, we finally agreed on the minutes and seconds. Bob immediately put the interferometer onto the new position. In a few minutes he called back, saying "There's a signal there, but it's weaker." Weaker, but again the phase was steady, so the source was close to the specified position. This was fun! Let's look at another one!

The third brightest of the century was Nova Persei 1901. It too was above the horizon, so we went once more through the dreary exercise of precessing to 1950. Once again it took several phone calls to agree on the result. Bob put the interferometer onto Nova Persei, and after what seemed to me a long time he called to say that this time he could see no signal. Still, a success rate of two out of three was quite a change from what we had been experiencing with the red supergiants. So we altered our observing strategy completely. We looked at novae — old novae, new novae, slow novae, fast novae, one-shot novae, repeating novae, etc. We were very busy that last week on the air. We actually picked up a couple of novae that we subsequently got quite a few papers out of, Nova Delphini 1967 and Nova Serpentis 1970. We came home happy and content.

It was clear that the best way to prove that the signals were in fact from the novae was by positional coincidence. Our radio positions were good to an arc second, but we needed better optical positions. So we approached Larry Fredrick, who ran a pretty fair astrometry shop over at the University of Virginia, and he agreed to provide good optical coordinates. To help him find the right constellations, I supplied him with a list of the positions we had fed into the interferometer.

I suppose you wonder where the serendipity in this is, since our success was clearly the result of insight and good judgment. I really don't know how to describe things from here on! Anyway, I got a somewhat agitated telephone call from Larry a short time afterwards, asking when we were planning to publish. Seems he had found discrepancies in a couple of our positions.....

Firstly, the declination we had used for Nova Herculis was wrong by exactly three degrees. It hurts to have to stand up in front of people and admit this! Why the error? Well, each observation on the interferometer was controlled by a punched card. I had prepared a card for Nova Herculis, many days in advance, and I must have struck a key in the wrong row (the digit keys were grouped by threes). It was my silly blunder. The right ascension was all right, but you sort of have to get both coordinates right....

G. Westerhout: That's why fan beams are useful!

Good point! Anyway, subsequent investigation showed that this "Nova Herculis" was in fact in the Parkes catalog. No wonder it gave good fringes. OK, that's fine, true egg on my face, but at least the blunder pointed us in a new direction.

Now about that error of precisely one hour in the right ascension of Nova Aquilae.... You don't believe me, do you? But that's the way it happened! Bob Hjellming doesn't make mistakes, but I think, Bob, it is fair to say you were the victim of circumstances. Bob was the only guy who was leaving fingerprints on the key punch at that stage, and it will be to his everlasting credit that the minutes and seconds were exactly right!

Of course, nothing was seen at the position of Nova Persei, for which we had done everything correctly. At least we were right about Nova Delphini and Nova Serpentis -- we did get real papers out of those!

Thus it was a happy combination of blunders that led us to find a new class of radio source. When you can't do it any other way, that's how you have to do it! Still, it hurts; if we had been undergraduates, our professors would have suggested that we transfer into a field more appropriate to our intellectual capacities, perhaps physical education.

Dave Heeschen, then the NRAO Director, was pleased with our discovery, although we waited for a day when he was in a good mood to confess the true circumstances. Whereupon he did something quite out of character -- he said a naughty word!

Well, the confession is made. I feel clean again, and I will return to my seat.

SERENDIPITY IN THE GALAXY: THE GALACTIC WARP AND THE GALACTIC NUCLEUS

Frank J. Kerr
University of Maryland

In the historical framework of these few days, I've been thinking of the first time I came to Green Bank; I was here in December of 1957 on a short visit. I had been spending some time in Leiden with Gart Westerhout and on the way back I did what Hanbury Brown suggested yesterday - I went westwards across the Atlantic, and then back to Australia, and I visited here briefly on the way. There were two or three interesting things about that time. The way one came to Green Bank then was by train to White Sulphur Springs, which was a very civilized way of traveling, on an overnight train. The car was dropped off at the station, and the porter would wake you up in the morning when it was time to get up. Then John Findlay or somebody who was there would pick you up in a car! At that time people were staying at a hunting lodge called Minnehaha Springs, which is halfway between here and White Sulphur, and the only buildings on the site here were a couple of houses which AUI had just recently taken over, and the only two people here that I can recall were Dave Heeschen and John Findlay.

The other interesting thing that I've never forgotten is that the telephone exchange at that time was Cass. So the number here was Cass 2; Dave apparently tried to get them to change it to Cass A, but the telephone company wouldn't cooperate.

The reason why I suggested this particular talk on serendipity in the Galaxy was because one of the discoveries which was not serendipitous was the detection of the 21-cm line; it's gone exactly as predicted. But the serendipity comes in some of the unexpected discoveries that were made, and I just want to mention two or three of the early discoveries.

The first one of those was in the Magellanic Clouds, which we observed in the 21-cm line with the 36-foot telescope at Potts Hill near Sydney, at the time the biggest in the world. It was a transit instrument (Fig. 1), which was home-made in the Radiophysics workshops, and that served us well for quite a few years. Figure 2 shows our early picture of the Magellanic Clouds, in terms of the total integrated hydrogen. Gordon Pettengill, in talking earlier, used the word mind set. I'm going to use that a few times too, because on several occasions we were working in the mind set which was produced by the current astronomy of the time, and in this case the mind set was that the Magellanic Clouds contained almost no dust, and therefore you could expect very little gas. So it was very surprising to see so much hydrogen, especially in the Small Cloud which is pretty well dust free. The serendipitous results here were the unexpectedly high intensity of the hydrogen signal, and the large size of the envelope, because the Small Cloud in particular is quite a small object optically.

So that was the first of the unexpected results, and in moving over to our Galaxy, I think one of the first unexpected results there was the so-called warp, only I don't think the word warp was being used at that time.

The first sign of this effect came from Gart Westerhout's Leiden observations, where you can see it at several different longitudes. In Gart's contour map of the hydrogen layer height, one see the contours at various heights above the galactic plane, showing the outer layer bending upwards. We came in shortly afterwards, and observed the southern part over on the other side of the Galaxy.

Fig. 1. 36-foot meridian telescope at Potts Hill, near Sydney, c. 1952.

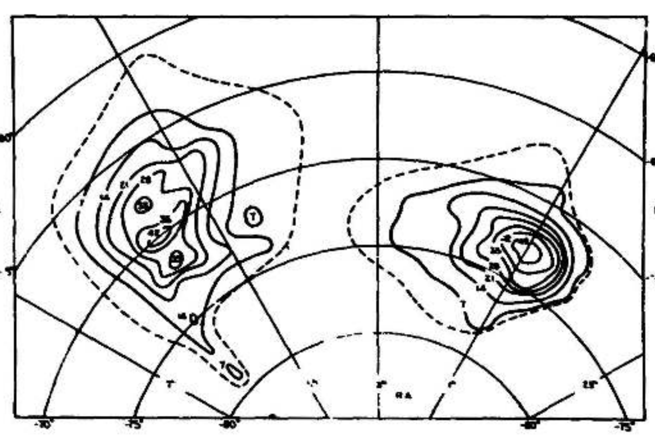


Fig. 2. Distribution of integrated neutral hydrogen over the Magellanic Clouds.

Combining the two sets of data led to the first plot of the bending in the whole Galaxy, demonstrating the warp effect, with the contours upwards on one side and down on the other (Fig. 3). We put the Magellanic Clouds in this diagram to indicate the possibility that this might be a tidal distortion produced by the Clouds, and it probably is, but other interpretations have also been suggested. So that, I think, was one of the other serendipitous results produced quite early in galactic radio astronomy.

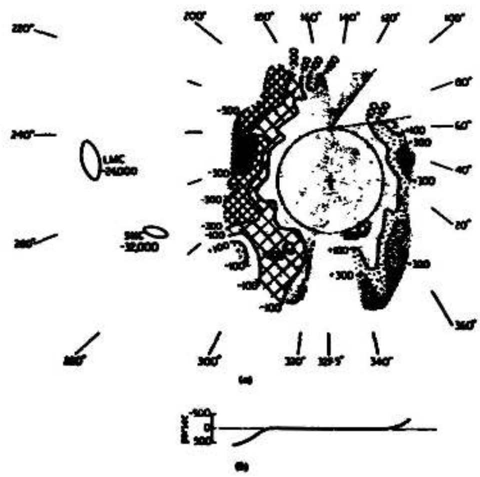


Fig. 3. Contours of the mean height of the hydrogen layer relative to the (old) galactic plane.

I said also in the title that I would talk about the galactic nucleus; I think there were two unexpected things there. Figure 4 shows possibly the first hole-in-the-ground dish of any real size; this one is parabolic, not spherical. It was made by John Bolton and Dick McGee at Dover Heights on a clifftop near Sydney; you can see the ocean clearly in the photograph. The dish was 80 feet in diameter, and it was used at 400 megacycles where the

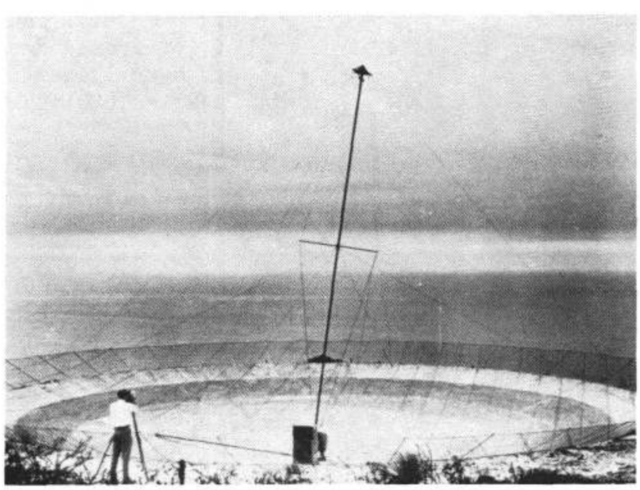


Fig. 4. Fixed-reflector telescope, 80 feet in diameter at Dover Heights, near Sydney, constructed by J. G. Bolton and R. X. McGee.

beamwidth was two degrees. In Sydney, the Galactic Center passes almost straight overhead, and so they could study the Center direction very well.

Figure 5 shows what they got, in the form of a contour map. This is in old galactic coordinates, so the intensity peaks up just slightly below the old galactic equator, at the strong point near galactic longitude 328. I claim that was a serendipitous result because at that time the mind set was very much in the style of the Galactic Center being a place where you would not expect a special source to exist. After all, the galactic nucleus was considered to be very quiet, with old stars in it, and it was not easy to see any way in which it would produce a radio source. So that was the real surprise. Their diagrams show the background across the Galactic Center region and they demonstrated that there was a small diameter source on top. I believe this result was at that time quite unexpected. About that same period, several other people were looking at the Galactic Center, but I think Bolton and McGee's paper was the first report published of a source at the center. The others, I believe, were Davies and Williams (Jodrell Bank), Priester (Germany), and Ed McClain (NRL).

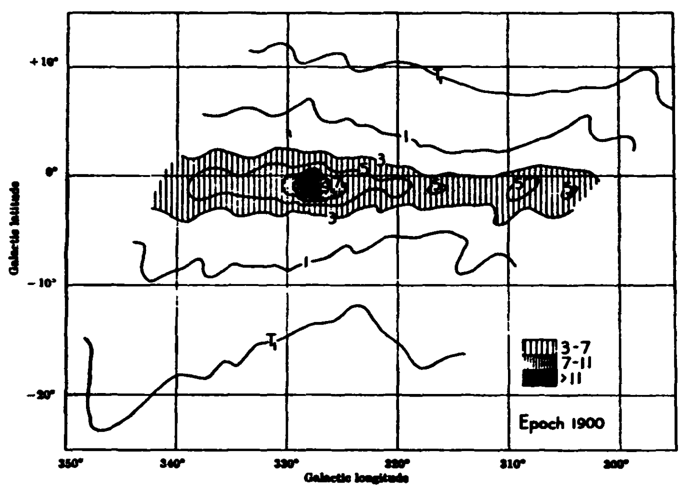


Fig. 5. Distribution of 400 Mc/s radiation near the Galactic Center. This was the first discovery of Sagittarius A.

A second unexpected result in the Galactic Center was the discovery of the expanding motion, connected with a 3 kiloparsec arm. This was in a paper from the Dutch group, the authors being van Woerden, Rougoor and Oort. It was published in the French Academy Comptes Rendus in 1954 or 1955, and the thing of interest is that you can see an absorption dip as they scanned in right ascension at a place corresponding to old latitude $-1\overset{\circ}{.}4$. That's where the famous -53 kilometers per second came from, as the velocity at which this particular absorption dip appeared.

This result, as well as being the first indication of so-called expanding motions in the galactic nuclear region, was also the observation which really established that Sagittarius A is at the Galactic Center. At the time when this work was done, several people were arguing that the Sagittarius A source was only 3 or 4 kiloparsecs away from us. That, perhaps, is another example of the mind set; people were finding it hard to believe that the bigger source was at the center, and proposed that it was closer to us. The real key to this puzzle is that the velocity of the 3 kiloparsec object (plotted in both emission and absorption) has a very steep slope as a function of galactic longitude which indicates that the source must be in the central region of the Galaxy where the velocities are high, and that was used as an argument to set the source at the Galactic Center.

I would like to close by showing two historic photographs that come from the URSI General Assembly in Sydney which took place in 1952. Figure 6 shows essentially all the 21-centimeter people in the world at that time. In case you don't recognize them, we see myself, Paul Wild and Jim Hindman (three Australians), Doc Ewen from Harvard, Muller from Holland, and Chris Christiansen from Australia.

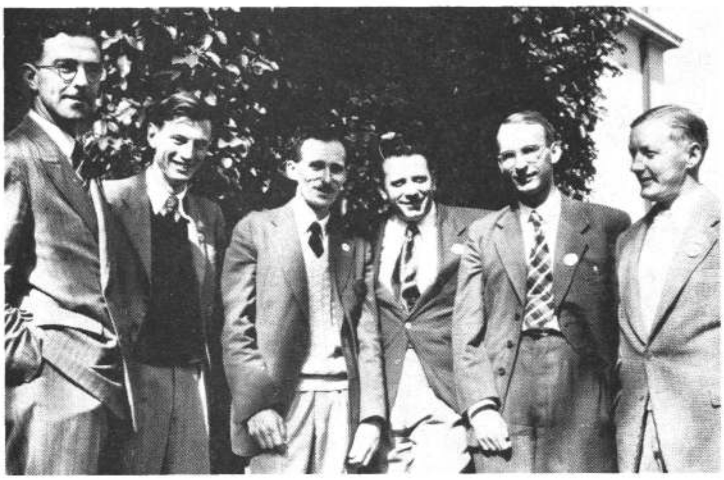


Fig. 6. Group of 21-cm astronomers at the URSI General Assembly in Sydney in 1952: F.J. Kerr, J. P. Wild, J. V. Hindman, H.I. Ewen, C. A. Muller, and W. N. Christiansen.

Finally, Figure 7 shows the entire group at the URSI General Assembly in Sydney in 1952. There were just barely over one hundred people there. The

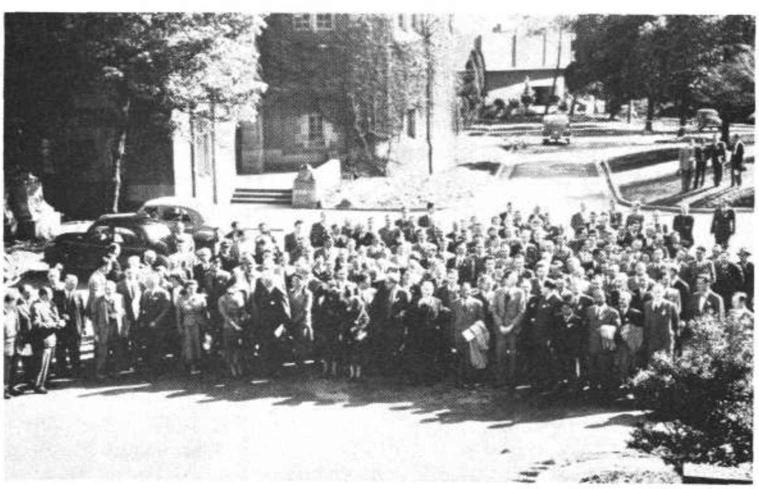


Fig. 7. The entire group of participants at the URSI General Assembly, Sydney 1952.

choice of Sydney at that time for the General Assembly was probably a compliment to the Australian work on radio astronomy, but it was a long way for most people to go, and thus the relatively small group. But it was a quite memorable meeting.

<u>G. Westerhout:</u> I want to make one comment about anti-serendipity. It was in 1956 when that result on the absorption line at 21 centimeters was obtained in one of the first observations with the new Dwingeloo telescope. In 1954 NRL published their first results on the absorption in front of Sagittarius A, which is also a beautiful profile, with a big absorption dip. They stopped observing because quite clearly the thing went off scale. You can find this actually in their publication. If they had gone on for another 200 kilohertz, they would have discovered that absorption line in the Galactic Center - the so-called 3 kiloparsec arm two years before the Dutch found it.

There is actually one other person in this room who is connected with the story of the warp. Bernie Burke, at the same time as our work, came out with the interpretation of the warp as a two-sided thing, because he was able to observe it far enough to the south to recognize that. We both had papers at the same AAS meeting.

THE FIRST YEARS AT PARKES

R. M. Price
University of New Mexico

This is indeed serendipity to come so close to the end of a program that has been filled with distinguished and very eloquent speakers, and in which nearly all the clever things I was going to say have already been said! However, let me try to make the best of it. I have decided that instead of talking about serendipity I would talk about anti-serendipity. So I was all prepared for that, and then Gart has just jumped up and said, "Here is anti-serendipity!" But again, let me pursue it.

Why is it serendipitous that my paper comes toward the end? I'm the last one that is looking to the past; Sebastian is going to look to the future. For the audience, it's serendipitous because it gives them the chance to leave politely and get most of the conference without having to listen to me. And the chairman, as you've just heard, has the opportunity to say, "Well, Price, they've said it all - we can save some time. Sit down."

J. Findlay: Well said, Marc!

But for me, you'll have to remember that I'm a Bart Bok student and I recognize the serendipity in the opportunity to deliver an epilogue! It seems interesting to me that in this whole discussion essentially nothing has been said about the serendipity that is associated with each one of us as individuals, and indeed why we are here, why we are radio astronomers, and what was the serendipitous course of events, people, jobs, or having the right piece of ex-army equipment in your hand at the right time to do the right experiment that has led to our being here today. Clearly we could talk about that for another whole day if each person got up and related this. I'm not going to start talking about that, although my talk deals a little with that, because several people have come to me and said, "Why are you talking about Australia? You're not an Australian! You're not even a senior citizen who was around in the really early days to watch all this and make it happen." But nonetheless, serendipity being what it was, I was at Parkes when they first opened what they called the Giant Radio Telescope, and I will say a few words about it. When the early edition of the program came around, there were at that time no Australians on it. Frank Kerr, of course, has come to keep me honest.

If you look in the book about the Princes of Serendip, they spoke of something that is relative to radio astronomy. They had a marvelous reflector, a mirror, which allowed you to seek truth and justice, and in that sense it was rather like a radio telescope. Whoever used this reflector, if they didn't tell the truth, their face would turn purple, or at least red! I think it would be a very useful thing for the editors of our journals and time allocation committees to use today in radio astronomy. I point that out merely because it shows a bit of the connection of the origin of the word serendipity to what we are talking about here.

As I have listened to these proceedings, two things have been clear. One is that radio astronomy is a very diverse field. It started with radio engineers. We've heard people get up here and actually admit they were radio engineers. We have some astronomers that have admitted that they indeed even went out and used radio equipment. But if you trace back some of these early strands, many of them lead to the Cavendish Laboratory where we found Joe Pawsey before he went to Australia to establish the program there. We find Ratcliffe and the influence that he had on Ryle and Findlay, and a number of people who have spoken since we are here. So again it seems to me that one of the most serendipitous aspects of this whole gathering really does have to do with the people that have been involved. Of course it's at this point, because all my other good anecdotes have been told by others, that I have to resist the temptation to tell anecdotes about individuals. I do just have one that relates to John Bolton, who of course was very much the driving force behind the research with the Parkes telescope. He had worked in the early days with the Australian Radiophysics Laboratory and then gone to Caltech to start their radio astronomy effort. He had then been lured back to Australia by the promise of this 210-ft dish which no doubt was going to be the premiere instrument of radio astronomy in the early sixties. John has a very tight cryptic form of handwriting. In the early days of Parkes, after the first Parkes Survey, he handwrote the manuscript describing how the analysis had been carried out, and it came back from the secretary and John nearly fell off of his chair because there was one place where he had written very carefully, "The results of this survey have been judged to be 95% complete." The typist had mistaken a "j" for an "f", and it read, "The results have been fudged...!"

But let's look again at serendipity. I'm not going to define it again because we have heard lots of definitions. The only thing I've found missing in these is that you have to have resources. You have to have the right telescope. All the things that are serendipitous wouldn't have happened if we had not had some resources, whether it was an old radar set left over from the army, or whether it was a Giant Radio Telescope that had been gained no doubt partly through serendipity, depending on who Taffy Bowen ran into at some cocktail party. You do need resources before serendipity can come about. This confirms of course what Dr. Harwit has told us, that you find that discoveries - serendipitous or not - occur with the best equipment that you have at the time, which confirms Bernie Burke's principle that we have technique oriented serendipity by and large.

My own definition of serendipity basically is an idea or an experiment basically whose time has come, and all the rest of this falls pretty much into line, although occasionally as with any definition you'll find things that don't quite meet the criteria.

Let's get back to the fact of why was I in Australia? Well, it was serendipitous; it had to do with the fact that Frank Kerr wrote an article about the Giant Radio Telescope in Sky & Telescope. This was seen by a rather stuffy old literature professor at the university where I was an undergraduate. At one point I was referred to him because a physics professor said to me that I ought to get a Fulbright Fellowship and go somewhere and do something. I talked to the literature professor and I mentioned astronomy. He said, "Oh yes, I've just read this article by a man in Australia where they are doing studies of radio telescopy!" So I applied to go to Australia, as a Fulbright student, to do "radio telescopy." It was perhaps anti-serendipity at this point when my formal application was sent by the Fulbright administrators to the Radio Astronomy Group, University Grounds, Sydney, New South Wales, and ended up on Harry Messel's desk. I had already written to Joe Pawsey (at the Radiophysics Lab, University Grounds, etc.); it was all settled with him. He said, "We'll be glad to have you come, it's terrific!" I told the Fulbright people that the Australians wanted me and everything was arranged. Then comes back a terse note from Messel saying, "We don't know who this guy is, we've never heard of him, and we don't think it's a good idea!" Fortunately, the Fulbright people talked to me again and it was straightened out. Indeed, I did end up in Australia.

At this point I should say that most of what went on at Radiophysics at that time was very much the result of a highly directed and well organized research effort. They had an excellent group. They had excellent facilities. Perhaps the spirit of the times could be expressed best by saying that we used to have a luncheon group at Parkes that we called The North Goobang Philosophical Society. Every lunch time we'd all get together and discuss the weighty matters of the day: which feed didn't work, which new supernova remnant needed to be looked at, and which were the latest results from all over the world that we felt we should follow up on. Of course being in a Commonwealth country, we felt that we had to have a royal charter, and so we had to seek patrons. The two people we sought were the Astronomer Royal and a senior scientist in the Laboratory. That is how the Australians got the reputation for having an organization that was indeed Wild and Woolly! That is a true story, we indeed did establish The Society and even seek the patronage of the before-mentioned gentlemen! However, they caught on to our trick and squashed it in the early days! So it went unchartered.

So in keeping with the criteria for serendipity we have heard about, there I was at the right place at the right time, with good resources, and not knowing too much. I wasn't re-learning physics, I was learning physics. So I don't quite meet all of Jesse Greenstein's criteria, but that probably didn't hurt too much because there were no theorists at Parkes to be misled by. Now, I'm going to do something nobody else has dared to do, and that is to put up a list that says "Discoveries." As you recognize these many of them are not the original discovery and none of them are serendipitous. For instance, the polarization of discrete sources - Connie Mayer didn't talk much about it - but in actual fact NRL had first found discrete sources to be polarized. But anti-serendipity had stepped in. The first three sources they looked at, the Crab, Cygnus, and finally Centaurus A, were all found to be polarized. All three with nearly the same polarization angle. Now, being very careful experimenters, they said, "Hey, we've got to be very careful. Maybe this is some sort of an instrumental effect that we don't understand." And they were very careful, and while they were being very careful, the Australians, with help from an ex-patriot, namely Ron Bracewell, were measuring and reporting the polarization of Centaurus A. Having "discovered" (confirmed) that Centaurus A was plane polarized, obviously the next thing for the Australians to do was to look for Faraday rotation. Discovery of Faraday rotation would confirm the existence of galactic magnetic fields.

Faraday rotation was the main discovery I was involved in, and here's where I did the thing that was wrong (fulfilling another criteria for serendipity). Ron Bracewell and company had done the polarization of Centaurus just before Easter. At Easter time the observatory in Parkes was closed for the weekend. As a graduate student, I was kept on site, chained to the telescope to spend my time driving second-half shifts when they couldn't find a driver and changing fuses and all the other things you would expect a graduate student to do. I was on site, trapped there over this Easter weekend, and I thought, "Well, gee, it would be interesting to see if Centaurus is polarized at different frequencies, too." So I went down to the radio telescope; of course it was closed down, but I knew every last detail about it. I even knew that you had to have a second person present during operations for safety's sake. So I invited the site manager, George Day, to go along with me, and he could do his evening reading while I drove the telescope, changed the receivers, and did all that sort of thing. Indeed, Centaurus was polarized at 21 centimeters. The previous measurements were done at 11 centimeters, and I thought, "Gee, that's nice, we've confirmed it, but there's one problem. Poor old Ron got his feed angle wrong, because indeed at 21 centimeters the position angle of the linear polarization in Centaurus is exactly 90° different that it is at 11 centimeters." And so the next Monday I called up the Lab in Sydney and said, "Hey, before you send off your paper you'd better check the feed angle, because I've done it at 21 centimeters and it's 90° out." Well, there were two reactions on that, and of course one of them was the fact that there was an unwritten rule that you don't use the radio telescope to observe when it's shut down for the Easter weekend!! That's what I did that was wrong!

But secondly, they did check their feed angle. They had it right. So here was an interesting mystery. Faraday rotation of course did come to mind. I talked with John Bolton about it; he said, "Well, that's certainly what it probably will be." Here I made my biggest mistake. I listened to the theorists. I looked at Shklovsky's book, Cosmic Radio Waves. It said, "You'll never see Faraday rotation because it will be smeared out across your bandpass." And indeed I had used a ten megahertz bandpass, two of them actually, double side band. According to the theorist the effect would have been wiped out, so we had to find another explanation. Well, the upshot was that Brian Cooper and I went back up a few weeks later and did the observation at seven different frequencies and it was very clear that it was Faraday rotation.

Now for another Parkes "discovery." Joe Pawsey had looked for background polarization for many years using the 80-foot dish in the ground that we heard about before. He very carefully calibrated the dish and observed very thoroughly, looking at about half of the sky. It turns out that when you do see background polarization, which they did indeed detect at Parkes a few years later, in the high latitude radiation, it was found that there is about half the sky that does not show it strongly or extensively. That's almost exactly the half that Joe Pawsey very carefully had looked at. Anti-serendipity strikes again!

At Parkes we also "discovered" the moon. Many radio astronomers have done this! (This was one of the first examples of the leaky memory that we see in a lot of image processing equipment these days!) We were very excited. We were doing 400 MHz survey and all of a sudden this uncharted source came booming through. We very carefully measured its position and then started trying to map it, and it turned out that indeed it appeared to be extended in right ascension. As time went on, as we went on further we found the source was a ridge of emission, it wasn't simply a single source. There was so much excitement that the astronomers had taken over even the driving of the telescope. About this time we measured the position again. No, it wasn't simply extended, it was moving! The telescope driver, who had gone outside to avoid all this noise, came back in and he said, "Gee, it's a beautiful night out there. The moon is full. It's the prettiest thing I ever saw, there's the shadow of the focus cabin right in the middle of the dish!"

You've already heard from Al Barrett how the Australians discovered the Galactic Center baseline drift effect. Let me say in defense of the Australians that when they got the word of OH they did not have any 18 cm equipment available. In true Boltonian fashion he set about to adapt the equipment we had. John started out by retuning the 21 cm dual dipole feeds - with a hacksaw. He was about to re-tune the 21 cm parametric amplifier - with the same tool - when Brian Cooper, the receiver expert, and Frank Gardner came along and coaxed him away from it, and they retuned it. However, when you retune an instrument like that too far, you have to worry about stability in baseline, etc. We all know that when you are doing line studies that you have reflections, etc. and the baseline can be a real problem. So nobody can really blame them for not being particularly worried when they saw a wildly sloping baseline underneath the couple of little absorption lines, particularly since the little lines were exactly where they were expected. Those little OH lines came exactly where the hydrogen was, and there was no strong reason to expect the huge dip off over to the side. Of course, that dip was where the action was as we later learned. Later, when they went back to study this deep absorption line, they were ecstatic for a few minutes because in a slightly different position they had "discovered" strong emission. Then they discovered they had their recorder plugged in backwards! Again, serendipity, but of a different kind.

N. Broten: Marc, they really stopped before they got to the bottom of that dip. They didn't go through the whole dip.

That's right. They didn't go through the whole dip because they didn't have the tuning range. Thank you, Norm, that's a good point. With the "modified" receiver they didn't have the tuning range to go through that entire dip, that had to wait until they did have a better receiver.

By this time the Australians believed very firmly in the anti-serendipity effect. When they did 3C273, again we've heard about this from Maarten Schmidt, they weren't going to count on any help at all from serendipity. We had multiple recorders set up. We even had staff ready with cranks to turn the great gears to make the telescope go around should there be a power failure - until somebody remembered that if there were a power failure the receiver wouldn't work either! We jimmied all the stops and safety cutoffs so that we could drive the dish clear to the ground. But as we got close to the ground in a test run, we discovered that there was a ladder that extended down from the bottom of the telescope that went below the lip of the dish. John Bolton adjusted things again with his hacksaw. So they were really ready to do this with no help from serendipity. Most of all they wanted to avoid anti-serendipity, so the different sets of records were taken back to Sydney on different airplanes by Cyril Hazard and John Bolton.

There was one other experiment that was going on at Parkes at this time that in some sense had some serendipity and some anti-serendipity. It was because of John Bolton, we've heard Bob Wilson say, that he was interested in the high latitude radiation. Well, John Bolton was still interested in the high latitude radiation and I was one of his next graduate students. He said, "Price, measure the background radiation." So I set about doing this with the great Goobang horn shown in Figure 1. Indeed we had excess noise, too, except that we were at 75 centimeters. As we saw earlier, if you look at the spectrum of the background radiation, 75 centimeters is not quite the right place to be if you want to discover microwave background emission. But, that's where we were, in that region of the spectrum where the galaxy contributes a great deal. In actual fact we could work around that and I found that indeed there was an excess of about 5 degrees. I had no idea what it was. I thought it was probably a bump of extra emission in the disk of the galaxy. We could study regions at upper and lower culmination, but we didn't have an azimuth control on our horn. (Unfortunately, we didn't have either the resources of Bell Labs or the assistance of Mr. Beck in building the antenna. Karl Jansky and Penzias and Wilson had Mr. Beck, and Australia had me!)

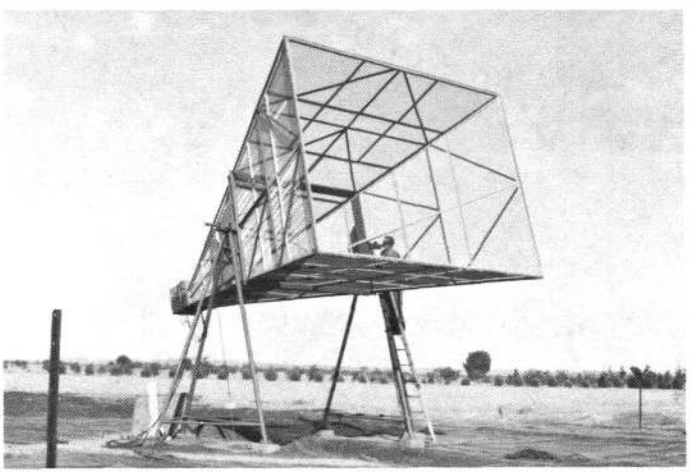


Fig. 1. The great Goobang Horn at the Parkes Radio Observatory.

We did all of the right things. We coated the ground around the horn with reflective material. We measured the far field pattern of the horn, and measured pieces of sky in upper and lower culmination, and came up with the same sort of an excess noise as Wilson and Penzias. However, we didn't know what it was, and I don't think that we would have figured it out either, the reason being that at this long wavelength we were preoccupied with the Galaxy, But, it turns out that only a few months after we had come up with this, we read in the Astrophysical Journal what 3 degrees of it was. I added a footnote in my thesis to point out that part of the 5° excess at 408 MHz came from the black body background.

The point is not that the Australians through this program would have come up with the background radiation. They probably wouldn't have on that project. But someone might have said after a few years, "Hey, where does that extra emission come from?" John Bolton might have gotten the next graduate student and said, "Try to track that down at a higher frequency!" But for doing what we were intending (studying the galaxy), we were at the right frequency.

I think I'll end my story of the early days at Parkes on this point. Again, remember that indeed most of the discoveries that we've heard about and in actual fact see, are not serendipitous. They really are, as I think Ron Ekers pointed out, the result of people carrying out careful research; very few of these things were serendipitous, they would have been discovered within a year or two, no matter what. It was just the fact that these were essentially ideas and experiments whose time had come. Now that doesn't mean we should close our minds, because every now and then, true serendipity does strike and Jupiter is there where you thought people were "parking." Serendipity is something we have to keep our eyes open for all the time. And as has been said -- doing good research is the best way to make it happen.

SETI - THE ULTIMATE SERENDIPITOUS DISCOVERY

S. von Hoerner
National Radio Astronomy Observatory

We want to listen for signals from outer space; but I guess I still have to listen for signals from inner space, and I have to stop talking when I hear too much stomach grumbling!

We like Chinese food, my wife and I, and not long ago we had a nice Chinese dinner; and when I later opened my fortune cookie I found something which really tickled me. There it said, "The smart thing is to prepare for the unexpected!" And that was a good deal of the philosophy of my own life. If we take it serious, what does it mean to prepare for the unexpected!" Now the unexpected goodies we all want to have, but how do you prepare for it? As we have heard, the unprepared ones don't get it. So it means: just do a lot of work, prepare yourself (not the observation), and keep your mind open. It does not mean how to get rich without even trying.

Then why should I speak here about SETI? Well, first of all because Ken Kellermann put me on the program, and second because Jansky opened up the whole realm of radio waves, and most of us who work in SETI have the idea that radio waves are the best means of interstellar communication. We talk about serendipity, and we really want to do something for SETI. We want to prepare things and do our best, but we are very well aware that we don't know anything about it. It might all be completely wrong what we do, and that is where we really cross our fingers and hope with our full heart that serendipity will help us where we else would fail.

About SETI itself, it's the search for extraterrestrial intelligence. To my feeling and to those of many others, it is the most fascinating question we have today. Not only fascinating, I would even call it an overwhelming question if you really think about it. I would like to mention that all the big milestones of our whole evolution on earth have been set by new ways of data handling, and by new ways of information processing. The first such milestone was self-reproducing life. It started with the genetic code, an ingenious code which is able to tell how to reproduce itself. That was the origin of life. Then higher life started with the introduction of the brain, which you might call a central switchboard, and that again is a completely new way of data processing in the living organism. Then our whole human culture, I think, was based on the development of speech, and we might right now enter a new area of artificial intelligence by cybernetics, by computers and all that. So if we keep going in our thinking this way, it is a very natural expectation that the many old civilizations of our Galaxy may have established a network of communications based on radio waves already, Ron Bracewell's "Galactic Club;" and we should try to enter the Club. Again, interstellar communication would create a culture of its own. This would mean if ever we have success, the result will be extremely dramatic; not in a few years, but in a few hundred years after success. We will lose our own culture. If we have success, we will merge into the big galactic superculture. Well, if we don't, if we really keep trying, honestly and hard and as good as we can, and have no success after a long time, or if from astronomical or biological reasons we would gain the certainty that we never should have any success, well, then we are unique in the world, and that again is a very dramatic finding! So no matter what the result is, it will be very dramatic. There just is no dull answer.

On top of this, I think one of the most important things are the fringe benefits to get out of SETI. Let me say in general, since this is part of astronomy, what do I think are the main results of our astronomy so far? Regarding the universe, I would say it is our finding that no matter how far out in space we look, no matter how far back in time, we find the same atoms obeying the same laws of physics and chemistry as we have here on Earth. This might give us confidence that the world is fairly well ordered, that what we have here might not be too different from other places, so if there is life here, it might be somewhere else, too. Regarding the beginning of our space exploration, we had some men on the Moon. Of the things they brought back from their trips to the Moon, the most important thing was not the stones and the dust they brought back. My feeling is that it is the picture of Earth as seen from the distance. And if this would reach more of the general public, and finally maybe even the minds of our politicians, it might make a lot of difference, much more than the stones and the dust. Here for the first time you see the Earth as a whole, you see this wonderful, nice little beautiful planet, and you see how fragile it is, how easily it can be destroyed, and it should be considered with thanks and it should be handled with care. And that is what we should learn out of it.

Now coming back to SETI. The first question is: is there anyone out in space? What do we know about them? The answer, of course, is that we don't know anything. We just can make guesses, more or less educated guesses, and if you don't know anything you have to start with general postulates, so let me do it this way. The first one I use always is the postulate or the assumption that "Nothing is Unique." We exist, and we shouldn't think we are unique. Then we make estimates: how frequently might all the conditions for life be fulfilled, and the first estimates may have been a little naive. They said that about half a percent of all stars should have life, and mostly higher lift, too. This would give the Galaxy 10^9 possible places for a higher level of life, and the nearest neighbors of those would be 20 light years away, which for astronomers is not far, and which technically could be bridged by our radio telescopes.

This was the first one, "Nothing is Unique;" now the second general assumption I use is "Nothing Lasts Forever." We should never think that our general state of mind is the final goal of all evolution; it will be just one link in a long chain. And what comes next we have no idea to guess. It might be a complete change of interest, it might be that all of those who get intelligent blow themselves up as soon as they can, as we seem to be doing soon. Or it might have other reasons we can't think of, but Nothing Lasts Forever.

Now I have to make a complete guess: what is the average lifetime of a technically oriented civilization? Let's take a hundred thousand years. Then this means the whole Galaxy has about ten thousand of those technical civilizations, and the nearest neighbors of those are about a thousand light years away. That sounds very distant. But still, without any new inventions, with only our present knowledge of technology, we could build equipment such that we could talk over a thousand light years distance (if we had the money).

This was the second basic assumption, but now we go to a third one. Let us generalize not only our past and our present state, but our possible future in case we have any. Many agree that space exploitation and exploration would be the next great step, that our solar system should be opened up. If minerals run out here on Earth, if we get too crowded here, well, why on Earth should we stay? This has already been worked out by engineers in many details, which I won't describe here. If we have not just little space cars, but let's call it big "Mobile Homes" with about ten thousand people orbiting the Sun, exploiting the asteroids and the other planets, and if this has gone on for many generations, then it is very probable that one or the other of these big communities will make a Declaration of Independence and just go off on their own. And since this wouldn't have changed their life drastically, space travel could very well last many generations, from one planetary system to the next, and to the next, and so on. So in this way the whole Galaxy could be colonized, from one rim to the other, in about ten million years!

Now we have to keep in mind that our Sun is not an old star, it is only half the age of the Galaxy. So most of these happenings should have happened very long ago at many places. All of the Galaxy should be teeming with life, and we should be the descendants of early explorers and not the home grown variety which we are. (If we are typical, we shouldn't exist!) Now from this many people have drawn the conclusion: if there are no extraterrestrials here, there are none anywhere. If they don't exist here, they don't exist at all. I wouldn't say I believe it, but I do feel the strong power of this argument. But I also feel the power of the other one, that nothing is unique; and so at present I do not have any explanation to the big riddle, "Where is Anybody?" I just can't tell. So it isn't certain, if we ever make a big search, whether we ever will have success; but it is fairly certain that we will never know if we never try.

So what have we tried so far. The search for signals was done here at Green Bank by Frank Drake, in his famous Project Ozma. Meanwhile, about two dozen of others have tried with bigger and better equipment, with sensitivities about a factor of 10^9 better; many hundreds of stars have been investigated, sun-like stars, or sky searches in general, also clusters of stars, even a few galaxies. So far, no success; but I would say this shouldn't disappoint us at all. If we think in astronomical distances, we also had better think in astronomical time scales. To expect success in the first few years would be naive. We should continue our searches, by whatever means we have, for a long time.

I would also like to point out that we should never say: "We're just beginners, why should we with our limited knowledge of physics try something? The other one sending signals to us might use means we have never heard of." That is true; but this can be said today, it can be said in a thousand years, it can be said forever. It is not necessary to know the best method, it is only necessary to know one possible method and we do have that. So we should go at it.

Right now at Ohio State University there is a SETI project that is going on all the time with an equivalent area of the 140-foot dish. In 1971 many people had come together at Ames Research Center and tried to find out how large an equipment should be built to really give us a good hope of success. That was Project Cyclops, and the answer was a few thousand large telescopes here on Earth, or a very large one of a few miles diameter out in space, and the cost estimate gave ten billion dollars. When this cost was mentioned at the CETI meeting in Armenia in 1971, people first laughed about it; but then someone pointed out that ten billion dollars is just three months of the war in Viet Nam. So this gives a different perspective to financial matters.

Later on, NASA had smaller projects worked out. For example, they wanted to make a census of stars, which is good astronomy anyway; or to find new ways of detecting planets, which again is very good astronomy. It is not so easy from the Earth, but we hope that with a good telescope in space it will be done. Meanwhile, they are now developing a multichannel receiver which will be a great thing and this should be put on existing telescopes. It is not the big telescope yet, it is just the receiver.

So this is as far as we have got, which isn't very far. We should be prepared that success might take a long time, it might take a lot of money, and we cannot guarantee it. But we should try as good as we can. First of all, we should stay alive as long as it might take; we should not blow each other up, but we have a good chance of doing that. On the other side, it is not only these SETI projects which may lead to success; it is also the change of awareness which has taken place to a great deal for most astronomers right now. Many of them who do normal astronomy keep an open mind for things which might be difficult to explain by normal means, and this attitude should be strengthened and encouraged. Success may come to SETI through serendipity by someone doing just normal astronomy. Something might turn up like what the pulsars first seemed to be, and unfortunately, were not. We should work hard, and keep our mind open, we should never get stuck in old ruts. We also should build SETI equipment which could do normal astronomy as well.

Now comes another nice serendipitous thing. Just two days before this meeting, I received the SETI Workshop book from Ames and JPL, and there was one chapter by Jill Tarter about "SETI and Serendipity." Now that was a nice bit of serendipity, to get it just two days before the meeting! Jill Tarter turned the problem upside down. But I shouldn't say that because I don't know which side actually is up. She said when we ever build big equipment for looking for SETI signals, we again should have our mind open for normal astronomy discoveries which might turn up, and she made heavy use of Martin Harwit's book and tried to estimate how many great normal astronomy discoveries will come up if we ever build large fancy equipment for SETI. So that's another hope we might have, if we ever try.

All this is very exciting but uncertain and expensive, and we don't know where it may lead. Thus, let me put your mind to Christopher Columbus. Please remember: Columbus started out under considerable expenditures, with wrong assumptions, for an impossible goal, but he still discovered America.

J. Findlay: May I make one remark? Frank Drake remembers that when he started Ozma, we discussed quite seriously what we would do here in this Observatory if he found a positive answer. Did you discuss the same thing, Jocelyn, or did you just go ahead and say, "No, we'll convince ourselves it's not a Little Green Man."

J. Bell Burnell: *What we were trying to sort out in Cambridge at that meeting that evening was how to present that particular result without stampeding the average housewife in Bolton to expect a flying saucer in her backyard next morning.*

J. Findlay: *Frank, I don't know whether you remember the discussion, but I remember it because it was in the control room of the 85-foot. We were sitting on packing crates. Lloyd Berkner was there. We did say, "What shall we do?" And the first answer we came up with was, "Keep quiet and think!"*

This also was discussed in Armenia in 1971, but it was not discussed in public, for good reasons. Among the astronomers the private agreement was: "Keep quiet as long as there is any doubt. Make absolutely sure that it really is Little Green Men, and after you are absolutely sure, inform the whole world, not just your own government."

J. Findlay: *We took it quite seriously, and obviously everybody else did.*

J. Broderick: *I think you ought to tell Walter Sullivan!*

'History of Australian Astronomy'

Radio Astronomy at Dover Heights

J. G. Bolton*

In this contribution, which is an expanded version of an invited lecture at the 1982 A.G.M. at Noosa Heads, the author recalls some of the early work in radio astronomy from Dover Heights.

1946

The Radiophysics field station which took its name from the Sydney suburb in which it was situated, Dover Heights, was an area of about 5 ha on the cliff-top south of the entrance to Sydney Harbour. It was an Australian Army reserve and in the later years of the war had a 200 MHz coastal defence radar; the site was also used by Radiophysics staff for tests on experimental radars.

Early in 1946 J. L. Pawsey with Ruby Payne-Scott and L. L. McCready had used the Army radar there in a passive (receiver) mode and another similar installation at Collaroy, north of the harbour, to conclusively link the enhanced solar radio emission at 200 MHz with sunspots. Shortly after I joined Radiophysics in September 1946 Pawsey had attempted to confirm J. S. Hey's newly reported discovery of fluctuations in the cosmic background from the constellation of Cygnus. He was unable to repeat Hey's result.

At the suggestion of D. F. Martyn, the ionospheric physicist, I was asked to investigate the polarization properties of the sunspot radiation. Martyn predicted that this would be predominantly circularly polarized and that in the case of bipolar spot the sense of the circular polarization would reverse near central meridian passage. I built two Yagi aerials for 60 MHz, an alt-azimuth mounting, and a switch consisting of quarter-wavelengths of 75Ω cable and a 'post office' relay to reverse the sense of circular polarization accepted by the orthogonal pair of Yagis. To these was added a modified radar receiver, an Esterline-Angus chart recorder with a microswitch operated by a cam on the chart drive; the latter provided the switching voltage to change the polarization switch at intervals related to the chart speed selected. Bruce Slee joined me as a technical assistant and we set up the equipment at Dover Heights in November 1946. The Sun at the time was almost dormant and we made attempts to detect other astronomical bodies using the two Yagis in a parallel configuration overlooking the sea. Our local library consisted of 'Astronomy' by Russell, Dugan and Stewart and 'Norton's Star Atlas'. We used the former to hazard guesses as to which types of objects might emit copious amounts of radio emission and the latter to find the position of the brightest candidate in each class. Our efforts were unfortunately not successful and after a week or two they were cut short by an unheralded visit from Pawsey, who noted that the aerials were not looking at the Sun. Suffice it to say that he was not amused and we were

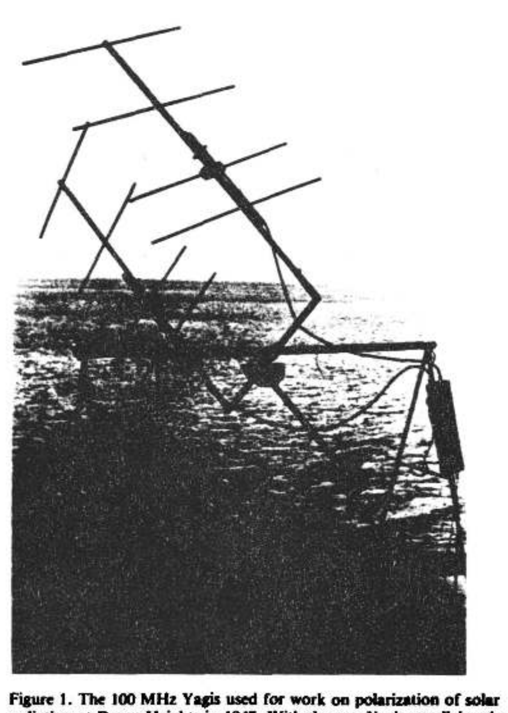


Figure 1. The 100 MHz Yagis used for work on polarization of solar radiation at Dover Heights in 1947. With the two Yagis parallel and as a sea interferometer, this aerial was used for the discovery of the first eight discrete sources.

* The author was a distinguished member of the CSIRO Division of Radiophysics from 1946 to 1955 and from 1961 to 1981. Immediately after the period described here he was appointed Professor of Physics and Astronomy at the California Institute of Technology, where he set up the Owens Valley Radio Observatory. He returned to the Division in 1961 to become Director of the Australian National Radio Observatory.

Reprinted from the Proc. Astron. Soc. Australia, Vol. 4, No. 4, pg. 349.

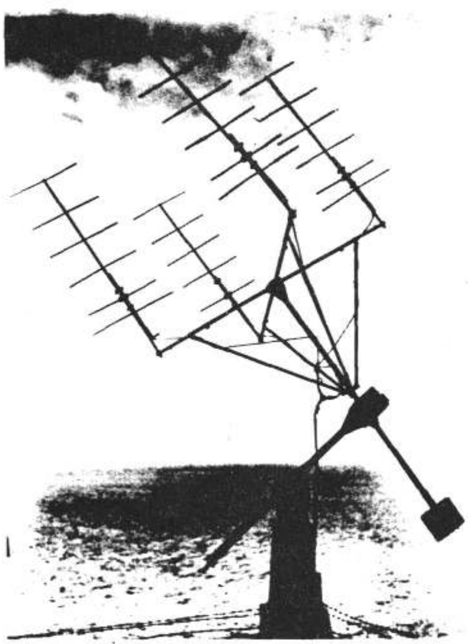


Figure 2. The 200 MHz four-Yagi array used for the study of solar radiation from Dover Heights in 1947.

both ordered back to the Lab. Bruce was reassigned to Mc-Cready to work on receiver construction and I to assist Gordon Stanley, who was building equipment to go on an eclipse expedition to Brazil early the following year. This equipment was fairly well advanced and my job was to add polarization and flux density calibration facilities. The eclipse observations were to be made at two frequencies, 100 and 200 Mhz. The aerial systems are shown in Figures 1 and 2.

1947

Towards the end of February 1947 Pawsey came into the room where Gordon and I were working and told us that the expedition to Brazil was not to take place. He then said, 'If you can think of anything to do with all this equipment — you can have it.' As he reached the door he turned round and, almost as an afterthought, and in typical Pawsey fashion, said, 'If you can think of anything to do with Gordon Stanley — you can have him too!' The opportunity was too good to miss; we spent the afternoon loading everything we had built on to a truck together with tools, spares and test equipment and early the following morning we were unloading into the former Army blockhouse on the edge of the cliff at Dover Heights. By 11 a.m. we had built a one-valve super-regenerative receiver for the broadcast band, since a Test Match between England and Australia was due to start at that time. We then started to install the solar receivers. The day we had everything in working order the largest bipolar spot seen for some years appeared on the limb of the Sun; however, it was completely inactive for almost a week. Finally on a Saturday afternoon, as I unlocked the door of the blockhouse on my return from lunch, I heard the pen of one of the recorders hit the stop at the end of its travel. It was the 200 MHz recorder. I switched all three recorders from inches-per-hour to inches-per-minute and reduced the gain settings on all receivers to a minimum. Shortly afterwards the 100 MHz recorder hit its stop as the activity at 200 MHz decreased and three minutes later the 60 MHz recorder went off scale. Activity at all three frequencies ceased after about 15 min. This was the first outburst of its kind (later designated Type II by Paul Wild) to be observed. A calculation based on the time intervals and estimates of the plasma levels in the solar atmosphere at the three frequencies gave an outward velocity of ~1000 km s^{-1} and a time of flight between Sun and Earth of 26 h. The following evening a conspicuous aurora was seen from Sydney — a very rare event. The observation of the outburst was published in Nature,[1] together with data by Ruby Payne-Scott and D. E. Yabsley on delays of the order of 1s between two frequencies on short solar bursts (Wild's Type III).

The following day the Sun rose with a violent noise storm in progress. This storm lasted through the next solar rotation. Near central meridian passage on the second appearance in April we did see the reversal of the sense of circular polarization as Martyn had predicted. In fact it changed several times at each frequency, owing, it was clear, to changes in the relative intensities of several radiating sources. I wrote up the observations for Martyn but the joint paper with his theoretical considerations never eventuated.

Solar activity continued at a relatively low level during May and dropped to zero in June, when Gordon and I decided to conduct an empirical search for radio sources using sea interferometers at 100 and 200 MHz. On the first night of observation a cable broke on the 200 MHz aerial and the same happened to our only soldering iron as we attempted to repair the cable. The 100 MHz equipment which was directed towards the north-eastern horizon gave a sea interferometer pattern of a source in Cygnus which was clearly that previously seen by Hey. A typical pattern of this period is shown in Figure 3. The ratio of the fringe maxima to the minima, the latter an upper limit due to receiver noise, showed that the source size was less than one-eighth of the fringe separation, or 8' arc. During the next few months we

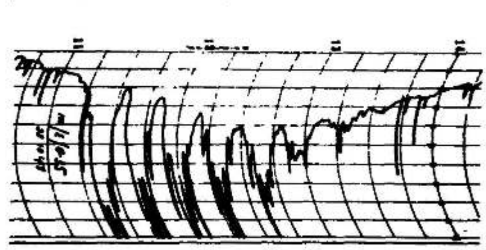


Figure 3. A typical 100 MHz sea-interferometer record of the Cygnus-A source.

determined the source spectrum between 60 and 200 MHz, made spaced-aerial observations in an attempt to determine whether the amplitude fluctuations were inherent in the source or of atmospheric origin, and measured the position of the source to the best of our ability. In the spaced-aerial observations baselines were about 20 km in a north-south direction but represented only about 2 km spatial separation because of the low observing angle of elevation. Hence there was no conclusive differences in the observations made at the two sites. For position we depended on the time of rising to give one line on the celestial sphere and the rate of change of elevation to give an intersecting line — the declination. Both quantities depended heavily on the corrections applied for refraction; had we used optical refraction corrections our positions would have been much closer to the truth than those first published. Unfortunately we used the formula devised by T. Pearcey to account for the apparent mean refraction deduced by Pawsey, Payne-Scott and McCready from their observations of the sunspot radiation. The Pearcey formula contained a substantial ionospheric term which accounted for their erroneous assumption that radio and optical sunspot positions were coincident!

In November 1947 we wrote up what we knew of the Cygnus source [2,3] and returned to the search for others. Gordon Stanley by this time had made exhaustive investigations on the causes of short time variations in receiver noise which set the limit on our ability to detect small signals. Considerable effort was spent on the design and construction of very stable H.T. and filament power supplies. Half-discharged car batteries (i.e. after gas bubbles had ceased to break away from the plates) were an early solution; this was later superseded by 100 V stable H.T. supplies driving a number of valves in series. On 6 November 1947 we obtained a sea interference pattern of our second source, Taurus-A, later identified with the Crab nebula; this is shown in Figure 4(a). The difficulties confronting us at this stage from both ionospheric effects and terrestial lightning and from instrumentation can be judged from the fact that it took almost a further three months' observation before we obtained a confirming record of Taurus-A. However, in the meantime we had detected at least two other objects, Virgo-A and Centaurus-A, and had probable evidence of another two sources. 1947 had been a vintage year!

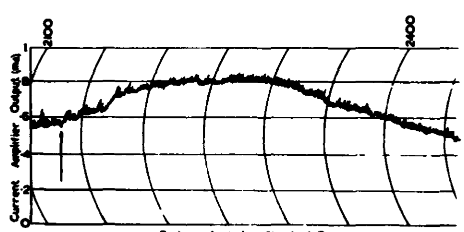


Figure 4. Discovery record of Taurus-A, the Crab nebula, obtained on 6 November 1947. Arrows mark the time of rising and interference minima.

1948

By the end of January 1948 we had evidence for at least six sources and returned to the problem of how to establish more accurate positions. Clearly the combination observations at rising and setting had great potential and Gordon and I considered the merits of potential sites in Norfolk Island, Lord Howe Island and the north island of New Zealand before we proposed an expedition to New Zealand to E. G. ('Taffy') Bowen, the Chief of the Radiophysics Division. Taffy gave us his enthusiastic support, including arranging for assistance in logistics from the New Zealand DSIR. At the end of May an ex-Army gun-laying radar trailer containing four 100 MHz Yagis, a new 100 MHz receiver, recorders, chronometers and weather recording equipment was shipped from Sydney to Auckland. We then towed it with a borrowed N.Z. Army truck to the east coast site. This was on a farm, 'Pakiri Hill', at an elevation of ~300 m about 10 km from the small fishing village of Leigh, 70 km north-west of Auckland. The coastline ran almost north-east to south-west and we had hoped to observe Cygnus-A over the whole semi-diurnal arc. Unfortunately the reflected signal was cut off near setting by some small islands. We spent nearly two months at Leigh, in periods of working 10 nights and then having four days' rest as tourists. Conditions were far from ideal; we had a long extension from an already overloaded power line and frequency variations caused variations in the recorder chart speed of at least 10%. The weather was sometimes appalling; on one occasion our barograph recorded a fall of 15 mm in 30 s, to be followed by a similar fall of 9 mm 10 min later. Nevertheless we obtained about 30 nights' usable data on Cygnus-A and in mid-July five observations of Taurus-A, one of which is shown in Figure 5. The large number of observations was made to reduce the noise in the sidereal times of the interference minima caused by irregular refraction. One of the first discoveries we made from the observations of Cygnus-A at Leigh was that the Earth was curved! This produced incomplete interference in the first few fringes and offset to some extent the increased resolution of the more elevated site. The second discovery was that the amplitude variations were of atmospheric origin and not inherent in the source. Spaced-aerial observations between Leigh and Dover Heights showed almost complete correlation between solar bursts at the two sites but no correlation between source variations. The observations at Dover Heights were made by Bruce Slee, who continued to work with us after the New Zealand expedition was over. The scale size of the atmospheric

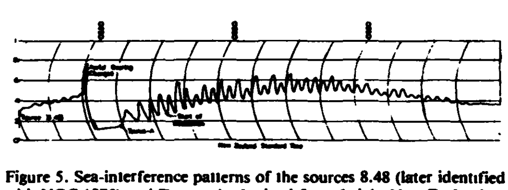


Figure 5. Sea-interference patterns of the sources 8.48 (later identified with NGC 1275) and Taurus-A obtained from Leigh, New Zealand on 13 July 1948. Evidence for a further source can be seen in the 'beat' pattern.

irregularities responsible for the scintillations was somewhere between 2 and 2000 km. *(After we had returned to Sydney I wrote to Martin Ryle at Cambridge University, with whom I had corresponded for some time, and suggested that with the Cygnus source passing almost directly overhead he was in a better position to make more definitive investigations — perhaps in cooperation with Jodrell Bank. My first knowledge of any results was to read a joint Cambridge-Jodrell Bank Letter to Nature on my arrival in London in February 1950. At the URSI conference in Zurich that year Bernard Lovell very graciously apologized for the form of this publication, for he had not been told of our prior work!)*

At the end of August we moved to a former wartime radar station on a cliff edge some 300 m above sea level with a westerly aspect. This was near Phia, a surfing resort about 30 km north of Auckland. The diesel plant for the radar station provided a supply of electricity stable in both voltage and frequency, our receivers performed faultlessly and the weather was perfect. In three weeks we had obtained good data on four sources, Cygnus-A, Taurus-A, Centaurus-A and Virgo-A. Our first attempt to observe the last object failed — our declination was so much in error that it had set before we had started to observe! In fact the source 'changed constellations' overnight and it was clear that the New Zealand data would produce a substantial revision of all our earlier positions.

Back home in Sydney I began the long and laborious process of reducing the data. The times of minima had to be corrected for the irregularities in chart speed, and reduced to a standard date. The altitudes had to be corrected for the height of the sea deduced from interpolation of the nearest tidal recording stations. Data at rising and at setting were then combined and an iterative process used to optimize the declination of the source and a refraction correction of realistic form. The mean effect of the ionosphere was nil! The first position had within its circle of uncertainty NGC 1052 — the Crab nebula. The second based on Piha and Dover Heights data pinpointed M87 and the third on similar data NGC 5128. The position we obtained for Cygnus-A was close to but not close enough to the galaxy eventually identified by Graham Smith.

Before publication I wrote to Jan Oort, Bengt Stromgren and Rudolf Minkowski, three optical astronomers who from the literature had interest in the Crab. My letters provoked enthusiastic responses and led to subsequent cooperation — and lifelong friendships. Jan Oort wrote five pages in return on the Crab nebula and then, ever cautious, added regarding M87, 'Of course there are a lot of galaxies in the Virgo cluster'.

The identification of the Crab nebula was a turning point in my own career and for non-solar radio astronomy. Both gained respectability as far as the 'conventional' astronomers were concerned.

The success which Gordon and I had had with the expedition to New Zealand was balanced late in 1948 with the dismal failure of an attempt to observe a solar eclipse from Tasmania. Delays due to wharf strikes prevented us making any observations, though D. E. Yabsley and J. D. Murray, who shared the expedition, were successful. However, it had one consolation — I got to know the late J. C. Jaeger, and he suggested it might be to our mutual benefit if Kevin Westfold joined the Dover group.

1949

Early in 1949 Radiophysics workshop staff completed the first and only fully steerable 'radio telescope' to be used at Dover Heights. It was a 9-Yagi 100 MHz array with a beamwidth of 17° mounted on an equatorial axis. It had a third axis between the polar and declination axes which, when rotated, converted the declination axis to an azimuth axis. It is shown in this configuration in Figure 6. It was used as a sea interferometer by Gordon Stanley and Bruce Slee to produce a catalogue of 22 discrete sources,[9] and also by Bruce Slee to begin a series of observations of the four strongest sources for an investigation of the seasonal and diurnal components of the scintillation phenomena. Kevin Westfold and I used the array with its mounting in the equatorial configuration to survey the background radiation from the south celestial pole to a northern limit at +30° declination. In addition to the observed contours we presented a first-order correction for the effects of the rather large aerial beamwidth.[8] In subsequent papers we attempted to use these data to discern the structure of the Galaxy in the plane[10] and the distribution of volume emissivity.[11] History was to reveal that these attempts were mere theoretical exercises; subsequent observations at higher resolution showed extreme complexity and fine detail in the galactic background radiation.

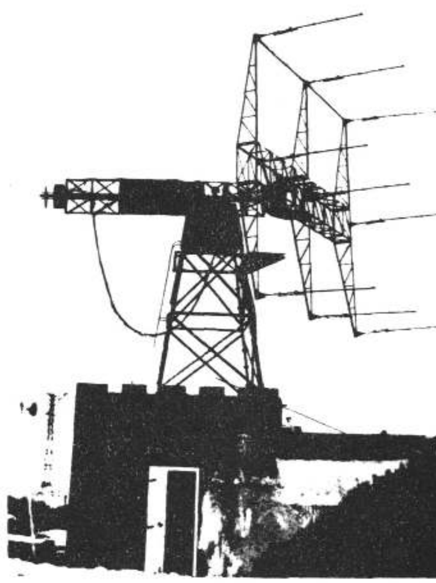


Figure 6. Nine-Yagi array at 100 MHz used for the first background-radiation survey of the Southern Hemisphere. A third axis between the polar and declination axes could be rotated to transform the mounting to an azimuth mount (as shown) for sea interferometry.

As Jaeger had suspected, the addition of a 'tame mathematician' to our group had its rewards — and frustrations. If only we could explain to Kevin what we knew intuitively was correct, sooner or later he would produce the formal mathematics to suit. Aerial or antenna temperature, previously jargon, became respectable, in addition to many other concepts. Kevin had time to read the current literature, amongst which was Shklovsky's original article in Russian on the 21 cm hydrogen line — which he translated. He gave the translation to Pawsey, with the suggestion that someone in Radiophysics should build some equipment to look for it — unfortunately to no avail. From Leiden in the following year we reported on the preparations in progress by the Dutch for 21 cm line equipment. Later that year F. J. Kerr, who was spending a year at Harvard, took me to see the equipment that Ewen and Purcell had under construction. Independently he had proposed to Pawsey that Radiophysics should take some action. Regrettably this was not to happen until after the Dutch and Americans privately communicated their detections of the H-line to Pawsey and invited a joint publication *with some southern hemisphere observations*.

Kevin and I also spent a considerable amount of time discussing what is now known as 'confusion'. I no longer recall the initial motivation for this but it was possibly a rather disappointing increase in the number of sources we catalogued with a very much better receiver and a better aerial. Signal-to-noise ratio was no longer a problem. We gave a joint colloquium at Radiophysics on 'Detectability and Discernibility'; amongst other things we had the temerity to suggest that even in the situation of infinite signal-to-noise ratio the number of sources that could be discerned (i.e. allocated a reliable flux density and position) with a simple interferometer might be somewhat less than the area of sky surveyed divided by the area of the primary aerial beam. Our conclusion was greeted with derision; it took another decade and disasters such as the 2C (Cambridge) catalogue before a figure of 50 beam areas per source was recognized as a requirement for ~95% reliability. Although we found no outside support for our conclusions it was clear to us that the way ahead involved moving to shorter wavelengths where physically possible structures could provide the needed improvement in primary beam size. The decimetre wavelength range also offered relative freedom from ionospheric scintillation and irregular refraction phenomena; however, it was a range which wartime radar had by-passed, leaving a gap in technology. Gordon Stanley was to spend most of his next 10 years, first at Dover Heights and then at Caltech, on instrumentation in the decimetre wavelength range.

1950

I spent almost all of 1950 overseas in Europe and North America. I travelled to England on the second voyage of the Himalaya — six weeks at sea for £78 — albeit in the smallest cabin on the ship! I made Oxford my headquarters in Europe, as Kevin had arrived there the previous October to spend two academic years in residence, in part-qualification for a Ph.D. During the university vacations Kevin and I made two joint excursions to Europe and during terms I visited radio astronomy centres and observatories in the U.K. I made two lengthy visits to Jodrell Bank, where visitors were most welcome. When I arrived on my first visit, Bernard Lovell handed me a dog-eared school exercise book containing the mathematical formulation of the Hanbury-Brown/Twiss intensity interferometer and asked me to see if I could find any errors. After a week of very long evenings I reported back that I could find no errors, but was at a loss for any physical understanding. The experimental proof of validity came later that year when the diameter of the Sun was measured using alternate rows of dipoles of an existing large array connected up as two interferometer elements with areas large enough to give the required signal-to-noise ratio and a separation close enough to avoid resolving the Sun.

I spent several weeks at Cambridge, where the iron curtain had already been built around the Cavendish radio astronomy activities. However, the then renegade theoretical group of Fred Hoyle, Ray Lyttleton and Herman Bondi more than made up for this. They were both hospitable and stimulating and believed in the advantages of continuing astrophysical discussions at Fenners after lunch when a county match was in progress. (Some readers may note a similarity to early days at RP in this regard!) These were the days of the beginning of nucleogenesis — the synthesis of the higher elements in stars — and the refusal of Monthly Notices to take any notice of it!

In Europe, apart from the groups at Ecole Normale Superieure in France and at Goteborg in Sweden, radio astronomy began at the optical observatories. There was in general more interest in the dynamic phenomena on the Sun than in non-solar radio astronomy. It showed up even in the colloquia that we gave, where Westfold, on the mechanism of solar bursts, generally won the major applause. The exception was of course Leiden, where Jan Oort foresaw the importance of both continuum and H-line work in elucidating galactic structure.

In October I continued my journey to North America, visiting all the radio astronomy installations in Canada and the United States and the optical observatories at Yerkes, Mount Wilson and Palomar and Lick. Rudolf Minkowski was my host at Palomar and devoted many hours to my education on the possibilities of the 48-inch Schmidt and the 200-inch Hale telescopes. It was many years before I realized that even in 1950 Rudolf had decided that there was a future for radio astronomy — by 1956 his 200-inch plate collection included the reported positions of perhaps 500 radio sources.

At Dover Heights Gordon Stanley and Bruce Slee began the investigation into instrumentation at higher frequencies. They built a 16-ft parabolic mirror which was mounted initially on the gun-laying trailer of the New Zealand expedition. The telescope is shown, at a later date and on the equatorial mount, in the background of Figure 7. Progress was very slow, particularly during the winter. A day's rain at Dover Heights was generally followed by two or three days when the run-off over the cliff edge was recycled by the updraft. In June of 1950 original rain fell on 29 days and Lake Eyre in the centre of Australia filled for the first time in this century. Nevertheless observations were obtained at several frequencies in the range between 100 and 400 MHz of the stronger sources. They

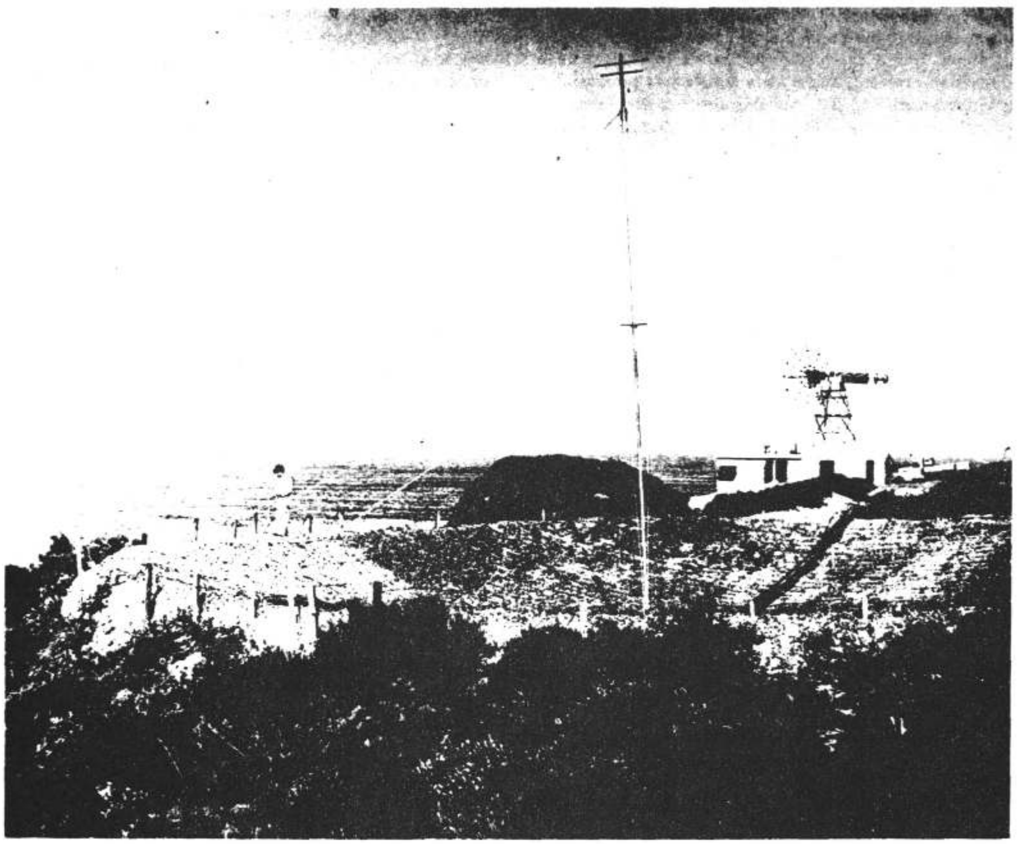


Figure 7. In the foreground is the 72-ft hole-in-the-ground built in 1951 for a survey of the region near the galactic centre at 160 MHz. In the background is the 16-ft reflector built in 1950 mainly for instrument development in the decimetre wavelength range.

were important in that they demonstrated a rapid decrease with increasing frequency in both the ionospheric scintillation and irregular refraction.

1951

1951 was a year of major changes to instrumentation at Dover Heights. The framework supporting the 9-Yagi array on the equatorial mount was dismantled. The structural components were used to build a 4 × 2 Yagi array on an azimuth-only mounting which had originally supported one of the World War II test aerials. It was later extended to the 6 × 2 array shown in Figure 8; the azimuth beamwidth was about 12° and the effective beamwidth in zenith angle somewhat less. For the sea interferometer the effect of waves combined with the output time constant of the receiver reduces the amplitude of high-order fringes and thus controls the zenith angle beamwidth. The effect is similar to that of finite receiver bandwidth and output time constant. Output circuitry was developed to permit largely unattended operation of the interferometer by partly eliminating the effects of the background level changes. The detector output was fed into an integrator with a time constant of the order of 30 min — i.e. long compared with the finge period and short compared with the time scale of background variations. The difference between the original detector output and the integrator output was recorded. An increase in contrast between fringe amplitude and background changes of about 50 was achieved, as can be seen in Figure 9a,c.

A second development was the azimuth/sea interferometer. In the 1948 observations from New Zealand Gordon Stanley and I had set an upper limit of 8' arc on the diameter of the radio counterpart of the Crab nebula, i.e. only 50% greater

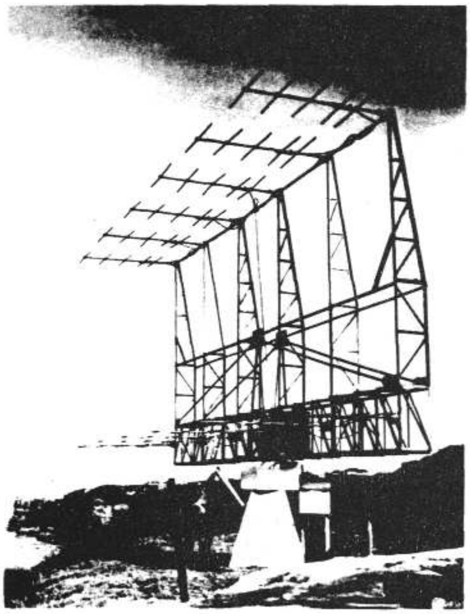


Figure 8. The final 100 MHz sea interferometer at Dover Heights, built mainly from components of the nine-Yagi array and mounted on the azimuth turntable of a World War II radar.

than its visible extent. This upper limit was due almost entirely to the uncertainty in extrapolating the background baseline to the first of the complete interference fringes, i.e. past those affected by the curvature of the Earth. The solution we devised was to cross a sea interferometer with an azimuth interferometer of much shorter baseline. The azimuth interferometer was to be phase-switched in order to eliminate the

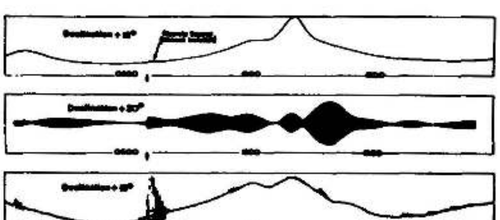


Figure 9. (a) Twenty-four-hour record of the galactic background radiation and discrete sources seen by the final 100 MHz sea interferometer.
(b) The output of the azimuth/sea interferometer at approximately the same azimuth as in (a) or (c).
(c) The same data as in (a) after processing to largely remove the effects of the background radiation. The strongest sea-interference patterns in all three figures are Taurus-A (the Crab nebula) and Virgo-A (M87), shown arrowed.

background variation and phase-swept at a rate rapid compared with the sea interference fringes, which would from a double envelope for the azimuth fringes. We selected a site for the observations on the slope of Mount Ousley near Wollongong, 450 m above sea level, and planned to space the two elements about 70 m apart. First however we made a scale-model experiment on the cliff at Dover with the two elements only 15 m apart. To our surprise we recorded a large number of sources with the azimuth fringe system which did not appear on the sea interferometer. Amongst those which had to be greater than 1/2° to be resolved by the sea interferometer were extended sources associated with Centaurus A (NGC 5128), Fornax-A (NGC 1316) and Puppis-A. The description and position of the Puppis-A source was sent to Baade and Minkowski, who were able to identify it on a 48-inch Schmidt plate as a supernova remnant. A typical azimuth/sea interferometer observation is shown in Figure 9b. The investigation of the extended sources[15] was given priority over the intended measurements of the Crab nebula; the project was finally cancelled when special interferometers for measurement of the sizes of the major sources were built at Jodrell Bank and Cambridge and by B. Y. Mills in Sydney.

The final development in 1951 was the construction of a 72-ft hole-in-the-ground parabolic reflector. Subconsciously this instrument probably followed the 220-ft reflector built by Lovell for radar detection of cosmic ray showers and used as a passive instrument to great advantage by Hanbury-Brown and Hazard. The major part of Lovell's reflector was supported on posts above the ground with only the area near the vertex excavated. The Dover Heights reflector was mainly excavated, with the spoil being used to build the outer rim. Bruce Slee and I did most of the excavation, Kevin Westfold joined in after his return from Oxford, and Gordon Stanley trucked several loads of ash from the Bunnerong powerhouse each week to stabilize the sand out of which it was formed. Finally a reflecting surface was made from obsolete steel strips formerly used for binding packing cases. They can be seen in Figure 7 secured to wooden pegs at 4-ft intervals, together with the mast and feed used at 160 MHz.

The construction site for the 72-ft reflector was not visible from the official working area of the Dover Heights station; the construction was carried out in our own time and in secrecy. Only Taffy Bowen was taken to see it when it was sufficiently advanced that its purpose was obvious. He both approved of it unofficially and agreed to say nothing about it until it was operational.

1952

Early in 1952 we completed a survey of the galactic radiation between declinations -20° and -47° at 160 MHz with the 72-ft hole-in-the-ground. The difference between this survey and the earlier 100 MHz 9-Yagi results was striking. A three-to-one reduction in aerial beamwidth had produced a three-to-one reduction in the apparent half-width of the galactic radiation in the region near the galactic centre. Joe Pawsey was also impressed with the results and had no hesitation in agreeing to our suggestion that we should attempt a further factor of three in resolution. To achieve this we would have to operate at a

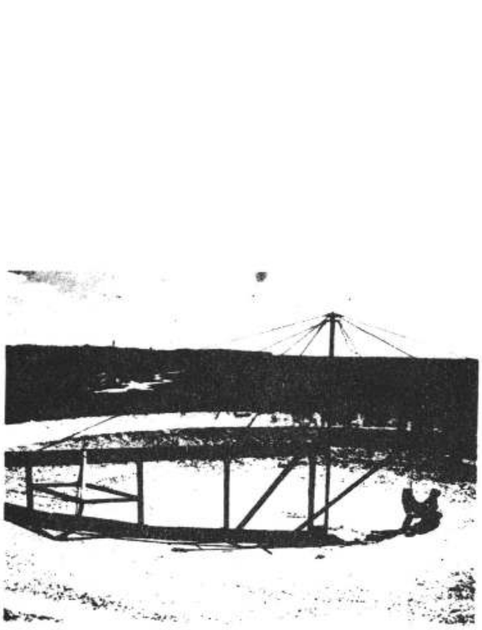


Figure 10. The concrete surface is added to the original hole-in-the-ground. The rotating template was used to position formwork for the concrete and also to finish the surface.

much higher frequency and increase the diameter of the reflector. The hole-in-the-ground was turned into an improved mirror by adding a half-inch wire mesh on top of a concrete surface over the existing ash and sand base. Figure 10 shows this operation almost complete, and — the heart of the modus operandi — a giant template. The template was used to set annular timber formwork for the successive rings of each concrete pour and as a 'screed' for finishing the surface. Short lengths of galvanized wire were left protruding from the concrete in order to secure the final wire-mesh surface. Aluminium tubes and annular tension wires provided a base for an extension of the diameter to 80 ft.

Figure 11 shows the completed reflector together with its feed and mast. Atop the mast were the r.f. switch and front end of the receiver — three 6J4's as grounded grids in cascade with a noise temperature of about 1400 K at 400 MHz. The switch was a parallel plate cavity between feed and receiver operated by a rotating sector of perspex coated with the thin aluminium foil found in cigarette packs of that era. A major problem with the switch was a gradual deformation of the

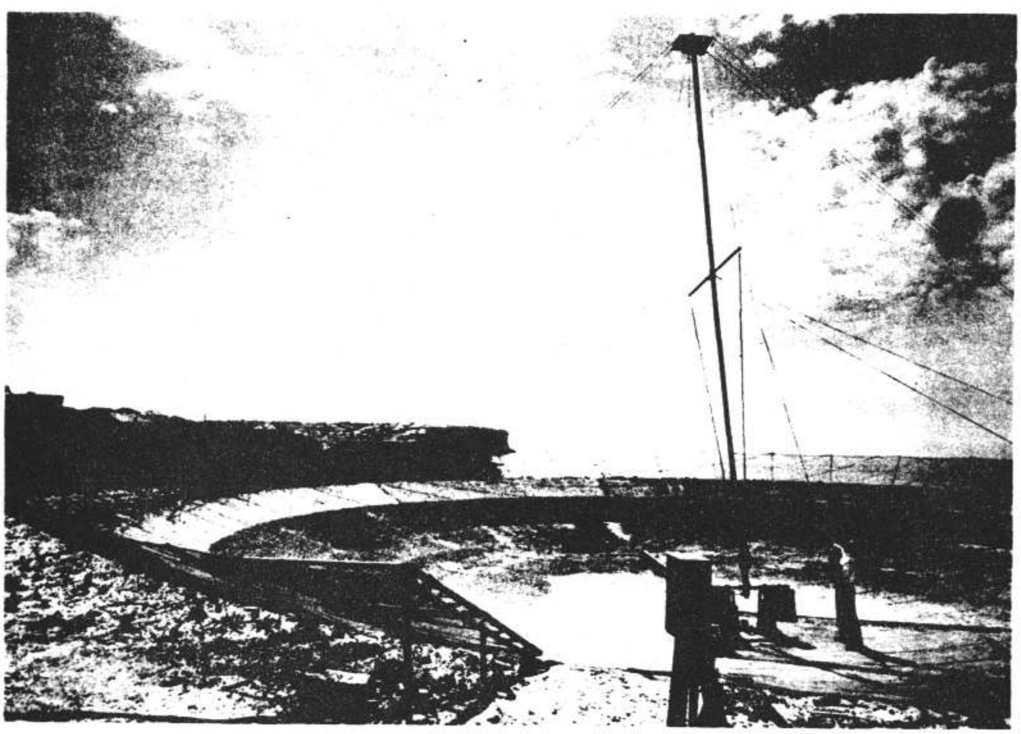


Figure 11. The complete 80-ft reflector showing the addition of the wire mesh surface, the feed mast and the housing for the second stages of the receiver at the vertex.

perspex due to relieving of inbuilt stresses and consequent contact with the sides of the slit in the cavity. Every second or third day the mast had to be lowered, the switch dismantled and the perspex immersed in boiling water in the tea urn for half-an-hour. The perspex was then cooled between flat steel plates in a vice, recoated with aluminium foil and the whole system restored. (To forestall two obvious questions, the deformation under spinning was not unique to one piece of perspex, and its replacement with a dynamically balanced metal sector plus heavier motor drive was too much of a weight penalty.)

Dick McGee, who had replaced Kevin Westfold at the beginning of the year, assumed responsibility for the 400 MHz survey, rapidly becoming high skilled in the switch repair operation. Another hazard that Dick faced in the winter of 1953 was to plunge into several feet of icy cold water to remove the debris which occasionally was to block the 'self-starting' syphon which drained the mirror. To cope with such eventualities the second states of the receiver were 'moored' in a waterproof box which went up and down with the tide in the vertical enclosure at the vertex shown in Figure 11.

In 1952 the programme of monitoring the scintillations of the four major sources which Bruce Slee had begun four years earlier was terminated. Analysis of about 2000 observations showed a correlation with sporadic E (as opposed to spread F for observations of sources near vertical incidence). Difference between data for the individual sources could be ascribed to variations in the structure of the irregularities with geomagnetic latitude and the effects of ionospheric winds.[13]

1953 and Postscript

Early in 1953 the sea-interferometer survey with the 12-Yagi array was completed and the final 100 MHz catalogue of 104 sources from Dover Heights compiled.[16] It was the first survey to show an excess of faint radio sources, which was due largely, in retrospect, to the effects of confusion.

Our major interest however centred on the early results at 400 MHz from the 80-ft reflector. In the region near the galactic centre the radiation was highly concentrated in a narrow strip only a few degrees wide (Figure 12). If the radio emission could be used to delineate the galactic plane, then it clearly lay about 1°.5 south of b = 0° in the old coordinate

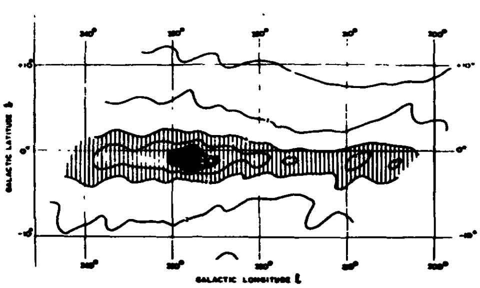


Figure 12. The region near the direction of the galactic centre as seen by the resurfaced 80-ft reflector at 400 MHz in 'old' galactic coordinates.

system. The most dominant feature was the source Sagittarius-A, which to our 2° beam appeared almost unresolved. This source, although it had been seen at higher frequencies, had not been observed at this resolution previously. Partly because of its location with respect to the more diffuse contours and its latitude and partly by analogy with Baade's then recently discovered semi-stellar nucleus in M31, Dick McGee and I suggested that it was the nucleus of our own galaxy. Three years later the IAU ratified the view, making the source position the zero of longitude in the new system of galactic coordinates.

I left Dover Heights towards the middle of 1953 at Taffy Bowen's suggestion — first to work in cloud physics and then in January 1955 to go to the California Institute of Technology to build the Owens Valley Observatory. For some months previously Gordon Stanley and I had been considering our next major move. We had three possibilities in mind. One was to build a second hole-in-the-ground to form an interferometer with the first. The second, inspired by Taffy, was to build two rolling barrels — parabolic cylinders inside circular cylinders — to form an interferometer. The third and my own choice was to build a large sea interferometer for use at 400 MHz. This would have consisted of a cylindrical paraboloid 20 ft high and 200 ft long with a focal length of about 150 ft fed by a vertical stack of dipoles. The construction of the mirror would have been similar to the fence round a tennis court and would have been rebuilt for each 40° of azimuth; the 40° interval covered by moving the dipole stack. The primary beamwidth would have been 1° in azimuth and the interference fringes 15′ arc apart. Unfortunately it was not to be financed — the Mills Cross had won the day.

Dick McGee continued to work on the 400 MHz survey and when this was finished Gordon Stanley and a Fullbright Fellow from MIT, Robert Price, used the 80-ft telescope in an attempt to detect the 327 MHz line of deuterium in absorption against the source at the galactic centre. Their negative result[20] was not published until some Russian observers claimed a positive detection well above their upper limit. Bruce Slee joined the group on the Mills Cross in mid-1954 but continued some work on apparent variations in the intensity of Hydra-A[19] until the end of 1954, when the Dover Heights field station finally closed.

I am sure that my colleagues from the Dover Heights era would wish to join me in thanking the then staff of the Radiophysics workshops, most of whom have long since retired, for their efforts on our behalf — in particular Bill Thompson, who bore the brunt of most of the outdoor construction work at Dover Heights itself.

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