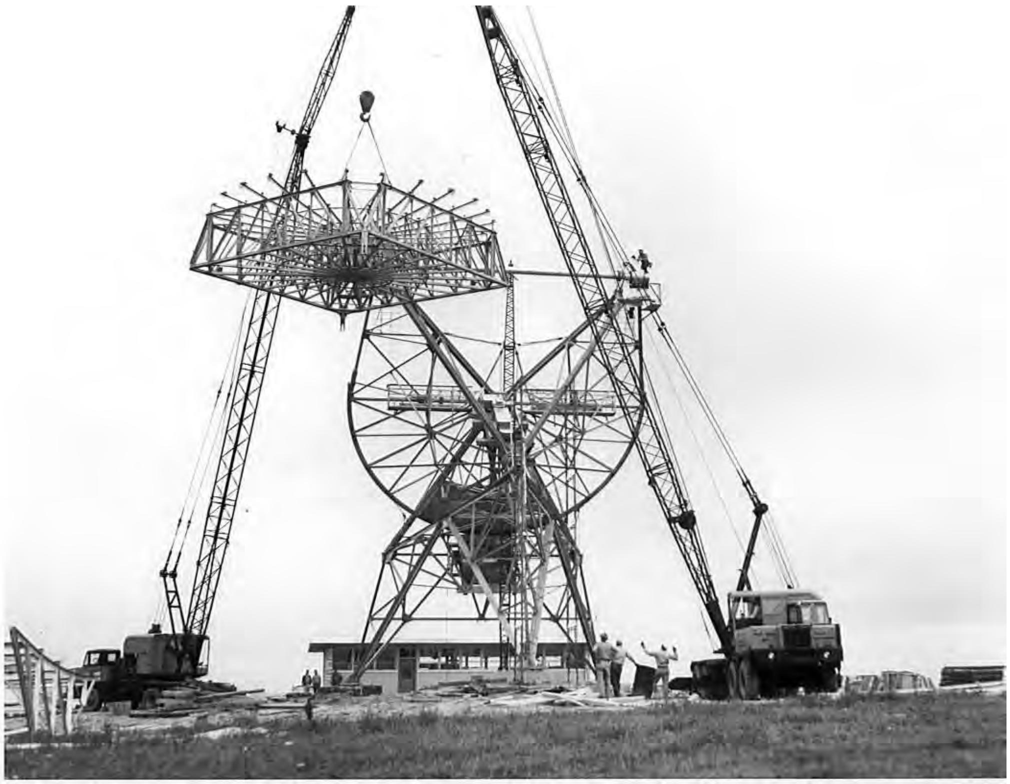
But it was Fun



The First Forty years of Radio Astronomy at Green Bank

Second Printing, with corrections.

Edited by

Felix J. Lockman, Frank D. Ghigo, and Dana S. Balser

Second printing published by the Green Bank Observatory, 2016.

Front cover: The 140 Foot Telescope and admirers at its dedication, October 1965.

Title Page: The Tatel telescope under construction, 1958.

Back cover, clockwise from lower left: Grote Reber in the Bean Patch in Green Bank, 1959; Employee group photo, 1960; site view of 300 Foot and Interferometer, 1971; the 140 Foot in September 1965; the 140 Foot at night; the Tatel Telescope under construction, 1958; the Tatel telescope, ca. 1980, view towards the west; the 300 Foot Telescope, 1964; aerial view of the 100 Meter Green Bank Telescope and the 140 Foot Telescope when the GBT was nearing completion, summer 2000.

Cover design by Bill Saxton

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Table of Contents

Preface

This is a scrapbook of material from the first 40 years of the National Radio Astronomy Observatory (NRAO) in Green Bank, West Virginia. The book has had a curious history. In 1987, George Seielstad, Director of the Green Bank Observatory, organized a symposium in honor of the 300 Foot Telescope, on the occasion of its 25$^{\text{th}}$ birthday. The attendees were an extraordinary bunch, in many cases the very scientists who established radio astronomy as a scientific discipline in the United States and got the NRAO going as a national laboratory.

The talks were quite interesting and had been recorded, so in the following months, they were gradually transcribed with the idea of putting them into a book. Fourteen months later, however, the 300 Foot experienced the major structural reconfiguration that is described in some detail in Part II. Almost overnight, the 100-meter Green Bank Telescope (GBT) project sprang into being and took everyone's attention and energy. The partial transcripts from the 300 Foot symposium were put aside in a closet.

Eight years later, in 1995, there was a new Green Bank Site Director and the 140 Foot Telescope celebrated its 30$^{\text{th}}$ birthday with an accompanying party and symposium to honor that instrument and anticipate the day when it would be succeeded by the GBT, which was then rising on the horizon. Again the attendees were an extraordinary bunch and included many who were instrumental in the breakthroughs in interstellar studies and very long baseline interferometry that moved radio astronomy into the mainstream of modern astrophysics. This time contributions were solicited and participants responded with everything from memoirs to research papers.

Over the next year, as the 140 Foot Birthday Workshop Proceedings were being sorted and edited, several events occurred that conspired both to delay the "140 Foot Book" and to replace it with the volume you now hold in your hands. For one, we received a call from Mark Popovich, a resident of Massillon, Ohio, who had purchased a box of miscellany at an auction and in it discovered hitherto unknown photographs of the 140 Foot Telescope under fabrication in the E. W. Bliss Company plant in Canton, Ohio. The difficulties that the 140 Foot Telescope experienced in its construction almost sank the fledgling NRAO and drove the Green Bank staff to design and construct the 300 Foot Telescope. The Popovich photographs document a key moment in the history of Green Bank and the NRAO; he generously drove to Green Bank and gave them to us. We had to include some of them in this book. Then, a bit later and out of the blue, a cardboard box with material from the 300 Foot Symposium appeared, and we realized that this material was now of more than passing interest and also deserved publication.

Finally, in the course of changes in the Green Bank physical plant, a number of old documents surfaced whose interest and importance likewise seemed deserving of a wide audience. The Monthly Reports from 1959 to 1962 paint a picture of an institution struggling on all fronts at once—discovering the need for computers, reporting the modest income gained from leasing the Observatory fields to local farmers, bemoaning the meager housing available for visiting astronomers, and trying to establish the concepts and practices of a national visitor facility. We also found material from some of the many interesting events associated with the Observatory, including notes from a joint Soviet-US Workshop held here in May 1961 at the height of the Cold War.

We are astronomers, not historians. Thus as the 140 Foot Birthday Symposium volume slowly became this book—quite slowly—we resisted the temptation to turn it into an historical account, but stuck to our goal of presenting material that might not otherwise be available publicly. To reiterate, this is not a history but a scrapbook from the first half-century of the Green Bank Observatory. It contains material from the 300 Foot and 140 Foot symposia, as well as portions of documents ranging from the report of the group that located the site for the National Radio Astronomy Observatory at Green Bank in 1955 to the report of the committee that investigated the collapse of the 300 Foot Telescope in 1988. We transcribed a colloquium given at Green Bank in 1988 by Grote Reber on his experiments with "Twining Beans," and culled the early *Monthly Reports* and the internal publication *The Observer* for descriptions of the vicissitudes of telescope operations and NRAO's first millimeter-wave telescope, built behind the Jansky lab in 1962. Some of the early memos on instrumentation for radio astronomy are particularly interesting as the technical possibilities and astronomical necessities pull each other forward. Our goal was to present the documents themselves and keep our fingerprints almost invisible.

We made one exception. The collapse of the 300 Foot Telescope was an extraordinary event in American science, but one whose full story has not yet been told. We interviewed many people who were there that night, and constructed a narrative of its fall from their words.

Because we depended on existing documentation to tell the Green Bank story, our coverage is necessarily spotty. In particular, the Green Bank Interferometer—the prototype for the Very Large Array and a significant scientific instrument in its own right—seems to have been built offstage without much fuss. This is quite a contrast to the 140 Foot, whose agonies played out in public. The development of the Green Bank Interferometer is recounted in a fine article by Dave Hogg, reprinted in Part IV, from the Barry Clark Symposium.* We also do not include much on the Robert C. Byrd Green Bank Telescope, now humming away happily not far from the site where the 300 Foot stood. That tale belongs to someone else.

Felix J. Lockman, Frank D. Ghigo, Dana S. Balser

March 2007, Green Bank, WV

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* *"Radio Interferometry: the Saga and the Science,"* Proceedings of a symposium honoring Barry Clark at 60, edited by D. G. Finley and W. Miller Goss, published by AUI, 2000.

Acknowledgements

As many people contributed to this volume as contributed to the founding and first 40 years of radio astronomical observations at Green Bank. We hope that we have represented their efforts well. For special help with this volume, though, we are grateful to Ellen Bouton, NRAO archivist and formidable librarian, for finding historical materials and for advice and editing; Mark Popovich for his discovery of the 140 Foot fabrication photos and generosity in providing them to us; Carol Whitley at the AUI offices for finding early documents and pictures; Marsha Bishop, NRAO librarian, for finding needed books and references; Bill McNeel, Editor Emeritus of the Pocahontas Times, for supplying newspaper articles about the founding of the Observatory, and giving permission to reprint them; Ron Monk for considerable darkroom work; Dave Hogg for critical reading and good advice; Ken Kellermann for comments and historical information; Tim Weadon for helpful comments; Fred Crews for comments and photo identifications; Bob Vance, Sid Smith, and Ron Gordon for photo identifications; Bill Saxton for the cover design, and Patricia Smiley for organizing and overseeing the publication of this book. We also thank Janet Ghigo and Elizabeth Lockman for editorial assistance.

We thank all those we interviewed for their recollections of the 300 Foot collapse: Fred Crews, Harold Crist, Jim Condon, Pete Chestnut, Ron Gordon, George Liptak, Ron Maddalena, Greg Monk, Don Nelson, Nathan Sharp, George Seielstad, and Bob Vance.

Most of the photographs in the book come from the NRAO photo archive, usually taken by NRAO photographers. Most illustrations for the papers given at the symposia were supplied by the authors. Credits for other photographs are given in the captions, and we are indebted to the following whose photographs have been used: Gerrit Verschuur, Bob Rood, Ron Maddalena, Tom Bania, Richard Porcas, George Liptak, Emma Beard, Bob Sheets, and Mark Popovich.

Historical Introduction

The National Radio Astronomy Observatory was established due to concern in the 1950s that radio astronomical research in the United States was lagging behind that in other countries. This was a somewhat ironic state of affairs because American engineers had created the field.

In 1933, Karl Jansky, an electrical engineer working at the Bell Telephone Laboratories in New Jersey, discovered that the Milky Way emitted radio waves. His work was followed up a few years later by another electrical engineer, Grote Reber, who built the first radio antenna dish, about 30 feet in diameter, in his back yard in Wheaton Illinois. Beginning in about 1938, he mapped the distribution of radio radiation from the Milky Way.

In 1942, J. S. Hey, a physicist working on the British radar project during World War II, discovered low-frequency radio bursts from the Sun. They had been detected at several radar stations and were thought at first to be German jamming signals. Hey also discovered radar echos from meteors high in the Earth's atmosphere.

The field of radio astronomy began to expand rapidly following World War II because of advances in electronics and radio technology brought about by the development of radar in England, the United States, Canada, Australia and other countries. Some of the physicists and engineers who had worked on radar took up the study of radio astronomy. Early radio surveys of the sky using instruments in Australia and England found sources of celestial radiation other than the Sun and the Milky Way, point-like sources, called at first, "radio stars." The nature of these radio stars was not at all obvious, and it became a priority to pinpoint their positions to an accuracy of a few arcminutes so that they might be identified with objects known from traditional astronomy. Such positional accuracy was first achieved by radio interferometers in Australia and England, enabling the identification of the Taurus A radio source with the Crab Nebula, the Cassiopeia A source with a supernova remnant, and the Cygnus A source with a distant peculiar galaxy.

In 1951, Harold ("Doc") Ewen and Edward Purcell at Harvard discovered the 21 cm radiation from neutral hydrogen, which had been predicted theoretically by the Dutch astronomer H. C. van de Hulst at Leiden Observatory in 1944. Dutch and Australian astronomers studied this new emission extensively, and by the mid-1950s had made the first map of the hydrogen distribution in the Galaxy.

Celestial radio signals are quite weak and require large instruments for their detection and study. In Australia, England, and the Netherlands, government funding enabled construction of large radio telescopes and arrays of telescopes. In the early 1950s British scientists began planning a 250-foot diameter radio telescope to be built at Jodrell Bank for the purpose of studying radar echoes from meteors and faint celestial radio sources. This telescope was completed in 1957.

The institutions in the United States that first took up the study of radio astronomy were MIT, Harvard, the Carnegie Institution of Washington's Department of Terrestrial Magnetism (DTM), the Naval Research Laboratory (NRL), the

California Institute of Technology (CalTech), Ohio State University, the University of Michigan, and Cornell, using relatively modest equipment. By the mid-1950s the largest radio telescope in the U.S. was a 50-foot dish at the NRL.

Early advocates for the development of radio astronomy in the U.S. included Bart Bok and Donald Menzel, Professors of Astronomy at Harvard. Neither were radio astronomers, but both recognized the importance of radio astronomy in making advances in their fields of study. Through their influence, Harvard was among the first universities to offer a specialization in radio astronomy for its graduate students.

Another advocate was Otto Struve of the University of Chicago. He was editor of *The Astrophysical Journal* when Grote Reber submitted his pioneering papers on radio astronomy in 1940 and 1944. Struve sent a delegation to visit Reber's telescope in Wheaton, Illinois, to assure that Reber's work was legitimate science. In 1946, Reber, with encouragement from Otto Struve, wrote a proposal for a radio astronomy research program that included the design of a 200-foot fully steerable antenna for an estimated cost of about $100,000. The proposal was sent to several influential science administrators, one of whom was Vannevar Bush, the president of the Carnegie Institution of Washington and one of the promoters of the National Science Foundation (NSF). Bush contacted some potential sponsors. One possibility that was considered was building a large radio telescope as a branch of the Mount Wilson or Mount Palomar Observatories. Funding might be sought from private research organizations such as the Carnegie Institution, or existing government agencies, such as the Naval Research Lab. Nothing came of these early efforts, but the idea of a large radio astronomy facility continued to be discussed.

In January 1954 the National Science Foundation (NSF), CalTech, and DTM sponsored a meeting in Washington, DC, of radio astronomers from both the U.S. and other countries. It became clear that discoveries and equipment development were proceeding much faster in England, Australia, and the Netherlands than in the U.S. The impetus for a large, possibly federally funded, radio astronomy center was renewed.

Following the January 1954 meeting, some of the scientists approached Associated Universities, Inc. (AUI), about the possibility of establishing a center for radio astronomy. Why AUI? Associated Universities, Inc., was a consortium of nine northeast universities formed in 1946 to build and manage a research institution for nuclear physics. They established the Brookhaven National Laboratories on Long Island, where they built several particle accelerators and a large reactor. Physicists from all over the country could come and do experiments at Brookhaven. It did not matter whether they were on the Brookhaven staff or even if they were from one of the Associated Universities. Any U.S. scientist whose project was approved could use Brookhaven. It was well managed, and the physicists liked it.

In 1951 the board of trustees of AUI hired a new president, one of whose assignments was to look for new projects to manage. This new president was Lloyd Berkner, who had considerable experience with large science projects. Berkner was quite a colorful character and a vigorous organizer. He already had some connections with radio astronomy, having attended the University of Minnesota, where his electronics engineering professor was C. M. Jansky, the older brother of

Karl Jansky. In the 1930s he worked with Merle Tuve at DTM where he designed equipment for measuring the density and height of layers in the ionosphere. After WWII, Berkner worked again at DTM for a few years. During this time he wrote a favorable evaluation of Reber's 200-foot telescope plan at the request of Vannevar Bush.

Berkner, enthusiastic about the prospect of establishing a national radio observatory, organized a steering committee in May of 1954, and planning for a new observatory began. This committee consisted of several notable U.S. scientists of the time, including Merle Tuve who had been Berkner's boss at DTM. Tuve and Berkner had considerably different ideas about government funding of science, and consequently many disagreements during the planning. Tuve was suspicious of dependency on government funding, and favored small projects directed to specific scientific questions.

The National Science Foundation (NSF) was formed in 1950 to grant funding to researchers in the physical sciences.* The idea of funding large research centers was new for the NSF, and was much debated. There were concerns that a large government-funded institution would siphon away much of the NSF funding that otherwise would go to grants for small- and medium-sized research projects at colleges and universities around the country. The balance between large and small research institutions has been a matter of contention even unto the present day. To address these concerns, the NSF requested, and obtained, funding by Congress for a new observatory that was separate from that of the project grants program.

AUI's philosophy in running Brookhaven was that basic research is best managed if the scientists themselves make the decisions about what instruments to build and what kind of research to pursue. The same idea was applied to the new observatory. Thus the observatory would have astronomers on its board of directors and would consult astronomers in planning all its ventures. The observatory would be an organization to serve the nation's astronomers.

In early 1955, the NSF provided a grant of $85,000 for a detailed planning study. In August 1956, after a year of work, the document, "Plan for a Radio Astronomy Observatory," was completed. It is quoted in several places in Part I of this book. The telescopes, the location of the observatory, the office buildings, labs, shops, and staffing were all planned in considerable detail.

Telescopes of 300, 600, and 1000 foot diameters were considered; the committee concluded that the first instrument should be of the modest size of 140 feet. They believed it could be completed in about two years; larger telescopes would be built later.

The primary consideration in deciding where to locate the observatory was freedom from man-made radio waves, which would interfere with the faint signals from space. The restriction to have the observatory located within 300 miles of Washington, DC, apparently resulted from the fact that most of the influential

* West Virginia Senator Harley M. Kilgore first proposed legislation to create such an agency in 1945. It was finally established in 1950 after much wrangling, in a somewhat different form than originally proposed.

radio astronomers at the time lived on the east coast. A new national optical observatory was being planned at the same time, and would certainly be built in the southwest. Putting the radio observatory in the east may have been thought to help provide geographic balance in funding.

In 1956 the NSF had to decide among three proposed management schemes for the new observatory: one was management by a single university—West Virginia University and the University of Virginia were considered; a second was a newly-forming consortium of university astronomy groups; the third was management by AUI. NSF decided on AUI, probably because of their experience managing Brookhaven.

The contract between NSF and AUI to establish NRAO, signed on November 17, 1956, was for a period of five years and provided the initial sum of $4 Million. U.S. Representative Harley Staggers, Sr, from West Virginia, was instrumental in ushering the appropriation through Congress.

An important priority was to preserve the radio-quiet nature of the Green Bank area. To this end, legislation was passed by the U.S. Congress establishing the National Radio Quiet Zone. The West Virginia Legislature also established an additional radio quiet protection zone within a 10-mile radius of the Green Bank site. No other place in the world at that time had legal protection from interference harmful to radio astronomy.

In mid-1957 the first scientists arrived at Green Bank and began work. Richard Emberson, Berkner's executive assistant at AUI, was the project manager for development of the site. Dave Heeschen was a recent graduate with a PhD in radio astronomy from Harvard, and headed the astronomy department of the new observatory. John Findlay, who had worked on British radar development during World War II, had been recruited by Berkner to lead the electronics department for NRAO. They were joined in 1958 by Frank Drake, another recent PhD from Harvard. Heeschen and Findlay worked at NRAO until their retirement, Heeschen becoming Director of NRAO in 1962. The writings of these four founders are quoted in many places throughout this book. They established the attitudes and practices of the Observatory and of its employees; their influence permeates the Observatory to the present day.

Lloyd Berkner was acting Director from 1956 to 1959. The first official director was Otto Struve, who served in this capacity from 1959 to 1961. By 1960 the staff at the NRAO in Green Bank had increased to about 80 people, and new buildings had been completed: mechanical and maintenance shops, a residence hall and cafeteria, and the Jansky Lab with offices and electronics labs.

Although the Observatory seemed vigorous and growing, disaster loomed on the horizon. The 85 Foot Tatel Telescope was in productive use, but it was not unique—there were other universities and research institutions that had radio telescopes with diameters of 85 feet or larger. The major planned instrument, the 140 Foot telescope, was behind schedule and the project was in trouble, such that Alan Waterman, Director of the NSF, feared that it might not be "possible of assembly." There was genuine concern that the young observatory might simply be closed down by Congress or the NSF.

The scientific staff, primarily Dave Heeschen, John Findlay, and Frank Drake, conceived of a very large telescope with limited capabilities that could be built in a short time at a relatively low price. This ultimately became the 300 Foot transit telescope, completed in 1962. The story of its origins and construction are told in Part II by Dave Heeschen and John Findlay. In a sense, the 300 Foot telescope saved the NRAO. With what was then the world's largest radio telescope, the Observatory began attracting astronomers from all over the world and started becoming a major national center for astronomy.

In Part III, Heeschen tells the story of the troubles of the 140 Foot project and how they were overcome. After the struggles and growing pains of the first ten years, the Observatory was well established and began expanding. The headquarters moved to Charlottesville, VA, in 1965, and the millimeter wave observatory was built on Kitt Peak in Arizona in 1967. Construction of the Very Large Array (VLA) began in the mid-1970s along with the opening of the NRAO offices in Socorro, New Mexico. These ventures into millimeter wave astronomy and interferometry had their origins in Green Bank, as recounted in Part IV. Over the years NRAO has fulfilled its original objective of providing state-of-the-art telescopes and instruments for radio astronomy.

For Further Reading

References for the early history of NRAO include the following, all of which are quoted in several places in this book:

"Science, Cold War, and the American State," by Allan Needell, Harwood Academic Publishers, 2000. This recounts the life of Lloyd Berkner, his career as an organizer of projects, and his interactions with Merle Tuve.

"A Minor Miracle, an informal history of the National Science Foundation," by Milton Lomask, NSF pub. 76-18, 1976.

"A Patron for Pure Science, the National Science Foundation's Formative Years, 1945–57," by J. Merton England, NSF pub. 82-24, 1982.

"National Radio Astronomy Observatory," by Richard Emberson, Science, November 13, 1959, vol.**130,** p.1307.

"Plan for a Radio Astronomy Observatory," by AUI, 1956.

"The New 140-foot Radio Telescope," by Maxwell M. Small, Sky and Telescope, November 1965, vol.**30,** p.267.

"Classics in Radio Astronomy," edited by W. Sullivan, Kluwer Boston 1982.

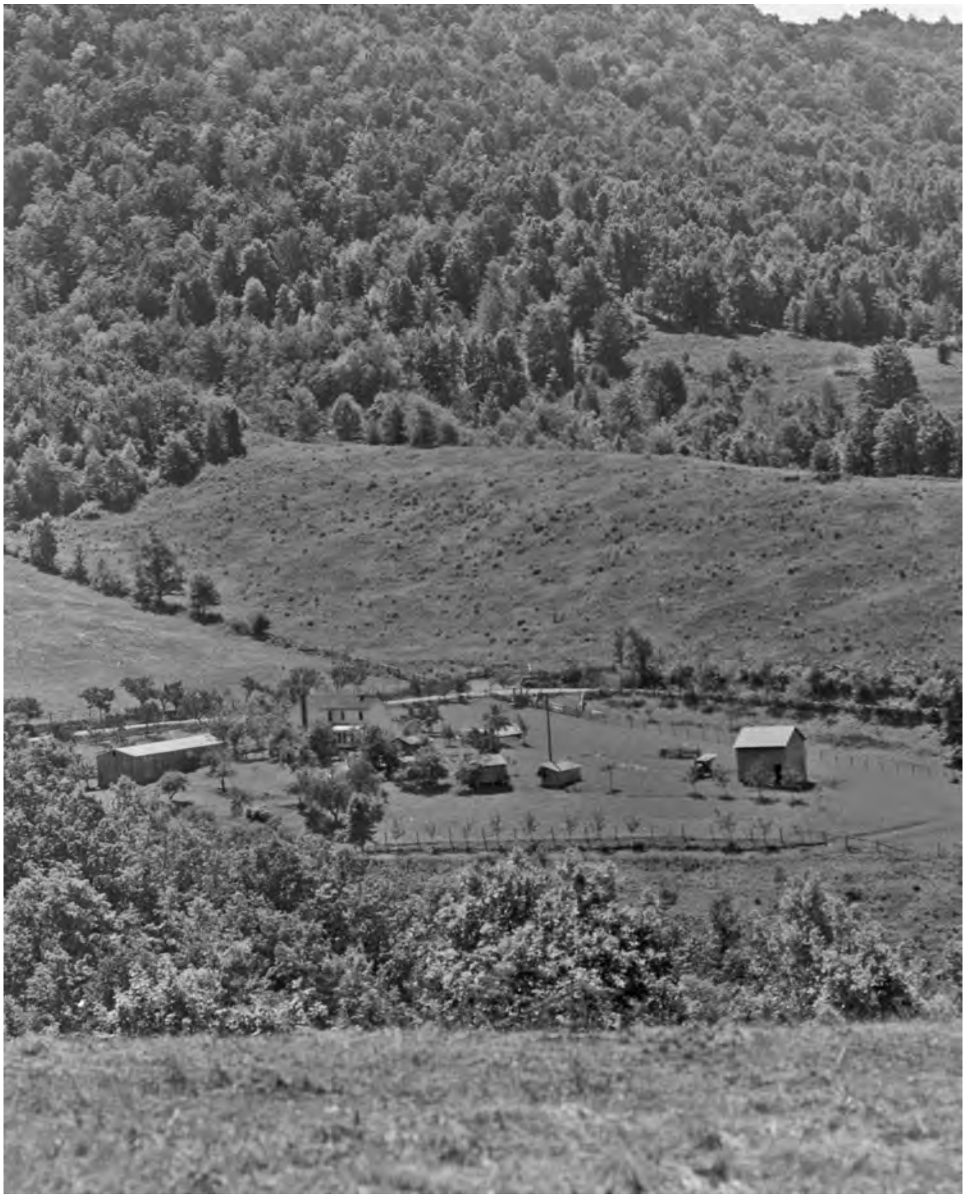
"Recollections of Tucson Operations," by M. A. Gordon, Springer, 2005.

"The Observer," the internal NRAO newsletter which ran from 1961 to 1981, is a major source of information, widely quoted throughout.

"In those days we really worked hard ... but it was fun!"

— David S. Heeschen, NRAO Director, 1961-1978

At the 30^{th} *Anniversary Symposium*
for the 140 Foot Telescope,
Green Bank, September 1995.



Before the Observatory: a view from Buffalo Mountain.

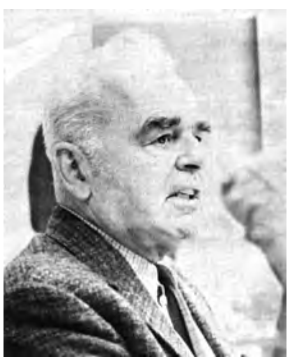
Part I

Building an Observatory, 1954–1962

Because of radio astronomy's fundamental nature and far-reaching possibilities, we urge the establishment of a Radio Astronomy Observatory to provide American science with the means and the tools to press our knowledge of the universe to the limits of our capabilities. The planning has now proceeded to the point where only vigorous action is needed to create a Radio Astronomy Observatory.

From *"Plan for a Radio Astronomy Observatory"*
prepared for the National Science Foundation by
Associated Universities, Inc., August 1956.

Radio Astronomy is obviously a very expensive science and it seems beyond the power of a single university or governmental laboratory to promote, as a part of a much larger total effort, the support and development of a radio telescope with an aperture in excess of that now being built by the California Institute of Technology [50 ft]. Since large equipment must soon become available if radio astronomy is to continue to grow in this country, the natural solution is to envisage a future inter-university radio observatory, sponsored and supported by the National Science Foundation and with equipment more powerful than that at any of the collaborating institutions.



Bart J. Bok, Professor of Astronomy at Harvard,
and member of AUI steering committee
for Radio Astronomy, writing in
"Toward a National Radio Observatory,"
August 7, 1956

1 The Need for a National Radio Observatory

excerpts from
National Radio Astronomy Observatory
by Richard Emberson
Science, vol. **130**, p. 1307, 1959.

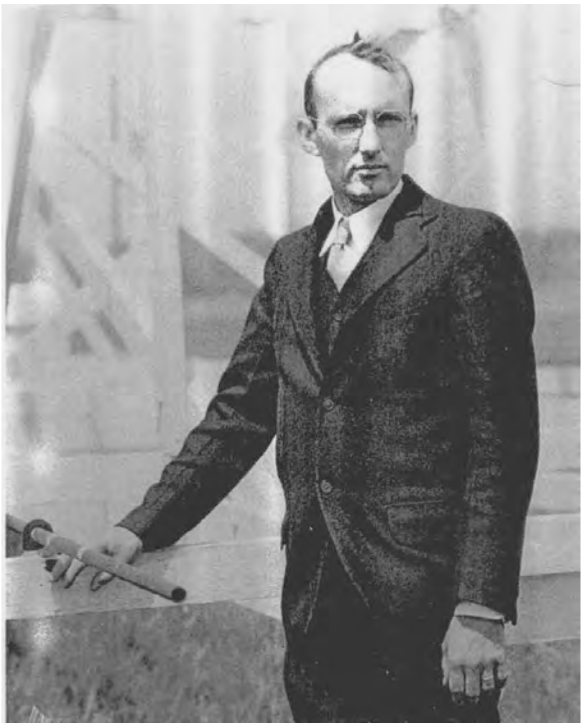
The British, spurred on by A. C. B. Lovell, had started to build a radio telescope with a steerable paraboloid 250 feet in diameter.... In the Netherlands, work was started on a 25-meter telescope, and in Australia preliminary plans were initiated for the design of a large steerable paraboloid. Meanwhile, in the United States, the 50-foot telescope of the Naval Research Laboratory was the only large instrument capable of efficient work at wavelengths as short as 21 cm....

This was essentially the state of affairs in January 1954 when a conference was called in Washington, jointly sponsored by California Institute of Technology, the Carnegie Institution of Washington, and the National Science Foundation. This conference was called to take advantage of the presence in the United States of several distinguished radio astronomers from other countries. At the conclusion of the conference it became clear to the United States scientists that radio astronomy in the United States was not keeping up with progress being made in other countries, and that unless this trend were reversed, the United States would drop further and further behind....

At this point the National Science Foundation entered the picture.... Within the Foundation the plight of the United States radio astronomer was recognized, and in order to provide a mechanism for giving more attention to the problem, an advisory panel for radio astronomy was established, in May 1954, with M. A. Tuve serving as chairman.

During that spring there were discussions among radio astronomers in the United States on the pressing problem of obtaining better observing facilities.... Julius A. Stratton [Provost of MIT] suggested the establishment of a radio observatory to be operated on behalf of all United States scientists; a somewhat analogous research institution was already in existence at the Brookhaven National Laboratory, which is operated by Associated Universities, Inc., under contract with the Atomic Energy Commission. The Brookhaven National Laboratory is essentially a postgraduate research center active in all domains of science related to nuclear energy. A permanent staff of scientists is augmented by visitors who come for terms varying from a few weeks to as much as a year or more.

Acting on Stratton's suggestion, a group of scientists directed informal inquiries to Associated Universities, seeking assistance in bringing the matter to a point at which a decision could be made on the feasibility of establishing such an observatory.... L. V. Berkner, president of Associated Universities, organized a steering committee for a feasibility study; John P. Hagen served as chairman....
During the second year of the study Bart J. Bok served as chairman.... On 18 February 1955, the National Science Foundation granted $85,000 to Associated Universities to support the feasibility study, and active work started.



Grote Reber in 1938.

Reber followed up Jansky's discovery by mapping the radio sky with a radio telescope he constructed in his back yard in Wheaton, Illinois.

Reber's radio telescope.

Karl Jansky in 1932 at Bell Labs in New Jersey, in front of the antenna used to discover radio waves from the Milky Way.

excerpts from

Plan for a Radio Astronomy Observatory

prepared for

National Science Foundation

by

Associated Universities, Inc., August 1956

I. The Development of Radio Astronomy

The detection by K. G. Jansky, in 1932, of radio waves emanating from "outer space" opened up a new and exciting field of scientific research. His initial observations were followed in the late thirties and early forties by the pioneering work of Grote Reber, who systematically investigated the background radio emission of our own galaxy. Both of these men were U.S. engineers.

. . . .

Following the end of World War II, research in radio astronomy expanded greatly, largely because of technological advances made in electronics during the war. The United States, however, did not take part in this expansion. Except for the solar studies made at Cornell University and the Naval Research Laboratory, very little radio astronomy was carried out in the United States during the first five or six years after the war.

. . . .

There are at present six U.S. institutions (Carnegie Institution of Washington, Cornell, Harvard, Naval Research Laboratory, Ohio State and Stanford) actively engaged in observational research in radio astronomy. At several other institutions (e.g. California Institute of Technology, Michigan) radio astronomy projects are still in the planning and development stage.

II. The Need for a Radio Astronomy Observatory Facility

Although scientists in the United States have made many of the basic contributions to the new science of radio astronomy, this country is not maintaining its initial leadership in this rapidly developing field, not for lack of scientific talent, but because of growing deficiencies in research facilities. As already mentioned, only six institutions in the United States are now actively engaged in radio astronomy research. Fewer than thirty scientists and graduate students take a direct part in this research.

. . . .

If the United States is to keep abreast of developments in radio astronomy, our scientists must have at their disposal larger and more powerful research equipment than is now available to them. . . . [T]here are no instruments in this country comparable with the large steerable paraboloid under construction in England, nor with the large interferometer arrays in Australia and England. The cost of such equipment places it beyond the likely means of any single institution. An observatory available to all qualified scientists is an obvious solution for the problem of inadequate research facilities.

A. Specific Objectives

A radio Astronomy Observatory would accomplish many important functions:

1. It would make available, to scientists throughout the United States, the large, powerful research equipment that is necessary to advance the science. Instruments of high angular resolution and sensitivity are essential in almost every phase of this research, and these requirements can be met only with large antennas. . . .

2. A Radio Astronomy Observatory will make it possible in the United States to integrate optical and radio studies more effectively. . . . [M]ost of our astronomers have been forced to be only bystanders in the field of radio astronomy, because their observatories cannot provide the large and expensive research equipment necessary. In a Radio Astronomy Observatory, all interested astronomers could carry out active research in radio astronomy, and a more complete integration of optical and radio studies would inevitably result.

3. A Radio Astronomy Observatory will encourage universities and other research institutions to plan radio astronomy projects of their own. If scientists know that they can begin a study at their own institutions, and then expand and complete the research with the more powerful equipment at the Radio Astronomy Observatory, they will be stimulated to initiate many projects they might otherwise consider impossible.

4. A Radio Astronomy Observatory will be invaluable in the training of students. . . . Because of the extremely difficult instrumental problems, most of the investigations made so far have been the work of scientists expert in the instrumental fields, but with no formal training in astronomy. Only a few universities in the United States now have facilities for graduate training in radio astronomy. . . . [O]nly the existence of a Radio Astronomy Observatory will make it possible for the astronomy departments at these universities to offer advanced research experience in radio astronomy to properly qualified graduate students. With an increasing number of trained radio astronomers, research in the field should develop and expand rapidly.

In summary, the establishment of a Radio Astronomy Observatory will provide the powerful tools necessary for research in radio astronomy; will stimulate interest and research in radio astronomy at other institutions throughout the country; and will assist in the training of competent scientific personnel. All these functions are vital, if the United States is to achieve a leading position in the field of radio astronomy.

Editors' note: Discussions of the early planning phases of the NRAO, including plans for telescopes of 300 foot or larger size, may be found in papers later in this volume:

- "NRAO: A View from the Outside," by Edward McClain (page 121)
- "How the 300 Foot Affected AUI," by Gerald Tape (page 127)
- "The 300 Foot Telescope and the National Center Concept," by Dave Heeschen (page 133)
- "Looking Backward: Origins of NRAO/Green Bank," by Hugh Van Horn (page 307)

2 Finding Green Bank

excerpts from

Plan for a Radio Astronomy Observatory

Associated Universities, Inc., August 1956

The Site Specifications

A. Radio Noise:

The level of radio noise or interference ... must be extraordinarily low. To avoid noise the following conditions are necessary:

1. The telescopes should be within the view of the smallest possible number of close-by inhabitants who might generate noise in the course of their daily work.

2. The telescopes should not view high tension power lines that radiate radio noise through corona discharges or otherwise.

3. The site should be in a valley surrounded by as many ranges of high mountains in as many directions as possible, to attenuate direct radio propagation from neighboring radio stations and to reduce diffraction of tropospheric propagation into the valley.

4. The site should be at least 50 miles distant from any city or other concentration of people or industries, and should be separated from more distant concentrations by surrounding mountain ranges.

5. The site should not be near commercial air route[s] with frequent over-flight of aircraft, nor in a region where commerce or industry are likely to intrude and grow in the future.

B. Location South:

The site should be as far south as possible with a southern obstruction not exceeding a few degrees to permit observation of the center of the Milky Way and other objectives having southern declinations.

C. Location North:

The site should be in northern latitudes to permit researches that involve aurorae, ionospheric scintillation, and polar blackouts.

D – F. Snow and Ice, Winds, Humidity:

Snow and ice need not be entirely absent, but they should be at a minimum.... The site should avoid a region subject to violent winds and tornadoes.... The climate should be reasonably mild, and high humidity is undesirable.

G. Size:

The site should be large enough to allow adequate separation among the installations of many types and sizes of telescopes and arrays; the latter requiring relatively flat spaces of one or more square miles.

H. General Surroundings:

Within the limits set by the basic requirements, the site should:

1. Provide as many as possible of the attributes of a university campus. . . .

2. Provide or have easy access to housing and other requirements of visiting scientists, permanent scientists, and their families. In addition to the obvious necessities of housing and meals, access to other amenities such as stores, theaters and recreational areas is desirable.

3. Within the limits of the basic requirements, the site should be easy to reach by plane, rail or automobile.

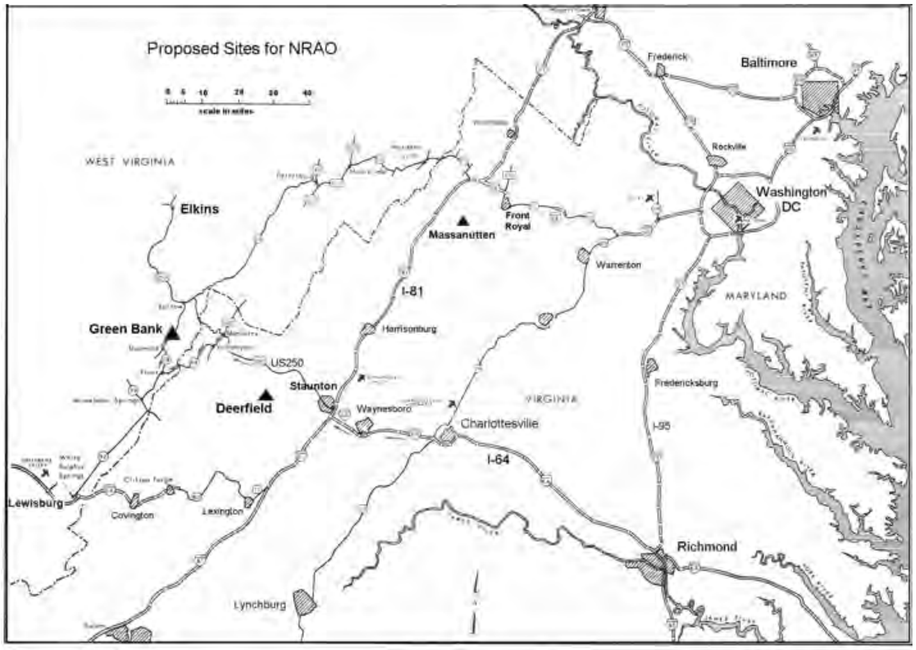
I. Geographical Location: The National Science Foundation Advisory Panel on Radio Astronomy, at its meeting of November 18-19, 1954, established one additional criterion. The request specified:

> "The Panel requests that the site survey by AUI should either be omitted or be of a scope limited to within about 300 miles of Washington, D. C., under this initial grant."

Selecting the Most Promising Sites

Site 15, Massanutten [VA], is actually a shallow gouge on the top of a mountain. Development costs would not be exceptional. Several placements could be made for the radio telescopes; with some leveling, arrangements could be made for arrays. The valley is so shallow, however, that very large telescopes would almost peek over the top of the shielding mountains at the rather extensive activities on either side. It is third in preference in regard to the radio noise measurements; third on the basis of population studies. Because the valley is so shallow, it is not well situated with respect to outside industrial and urban activity, and is third on the basis of airport activities. Strasburg and Front Royal could provide for the immediate requirements of the staff, with Warrenton and Winchester only a little farther. Washington, D. C., at a distance of about 100 miles by car, is the closest point for major requirements including transportation. This site is the closest to Washington, a fact that may be rated as an advantage or disadvantage according to the viewpoint of the rater.

Site 18, Green Bank [WV], is a triangular-shaped valley, about 4 miles across at the southern base and extending about 3 miles northward. Deer Creek Valley on the west side is some 50 feet below the average elevation of about 2700 feet. Mountains of 4000 or more feet rise in multiple folds in all directions. The site would be easy to develop for all parts of the facility, including the installation of arrays. There are about 125 houses, stores, churches, and other buildings in the valley, so the level of internal activity is relatively low. On the basis of radio noise measurements, Green Bank is clearly the first preference; it is the first on the basis of the population studies, and the population has been decreasing in recent years; it is first on the basis of the location of nearby towns and cities, and first or second on the basis of airport activities. Although Green Bank offers a good school, churches, and two stores, Marlington [sic] 30 miles to the south, or Elkins 50 miles by car to the north, would provide for most staff requirements. Elkins offers both air and



Map of Virginia and West Virginia. Triangles mark three of the sites that were considered for the location of the new observatory.

rail transportation; Davis-Elkins College is located there. The University of West Virginia at Morgantown, is approximately 100 miles distant.

Site 28, Deerfield [VA], is a valley with physical characteristics intermediate between those of Massanutten and of Green Bank. Deerfield itself is actually smaller than Green Bank. It is second in preference on the basis of the radio noise measurements and second on the basis of population studies. Difficulties might develop in the future because of its closeness to Staunton. . . . It is first or second with respect to airport activities. Staunton could provide for most staff requirements. It is about 80 miles by car from the University of Virginia.

Recommendations

The Steering Committee met in the Board Room of the National Science Foundation on Tuesday, December 13 [1955] . . . At the conclusion of the discussion, on motion by Dr. Tuve, and seconded by Dr. Bok, all members of the Committee present voting, it was unanimously voted:

(1) It is the recommendation of the Committee that the site near Green Bank, West Virginia, subject to verification down to very low field intensities of the expected low radio interference level, be selected specifically for the proposed 140-foot parabolic reflector, and possibly for two or three antennae rays [sic] or other equipments of modest cost; and

(2) This recommendation is made without prejudice to the possible location or locations which may in the future be recommended if this National Radio Astronomy Facility grows to include other specialized equipment or laboratory facilities.

Conclusions

A recommendation was made by a unanimous vote of the Steering Committee that Site 18, Green Bank, Site 28, Deerfield, and Site 15, Massanutten, be considered as sites for the National Radio Astronomy Facility, the preference being in the order listed above.

Lloyd V. Berkner (1905-1967) was a physicist and radio engineer, notable as a pioneer in measuring the height and density of the ionosphere. He accompanied Richard Byrd's first Antarctic expedition (1928-30) for which he installed and operated the shortwave communication and navigation systems. During World War II he was organizer and leader of the Radar section of the U.S. Navy's Bureau of Aeronautics. In 1951 he became president of AUI, which at the time managed the Brookhaven National Lab. He organized the planning for NRAO, and was the acting director during 1956-1959. He was one of the organizers of the International Geophysical Year that took place in 1957-58.
[Photo courtesy AUI]

Richard M. Emberson (1914-1985) was a physicist who worked on the development of radar at the MIT Radiation Laboratory during World War II. Berkner hired him as his executive assistant for AUI. Emberson was the project manager for the initial development of the Green Bank site.
Berkner wrote of him in the first NRAO Annual report (July 1959) as "the man above all others who is responsible for making the Observatory possible. Dr. Emberson has mothered the whole plan and followed every detail until the present plan has come to fruition."
[Photo courtesy IEEE history office]

About the site selection and acquisition:

excerpts from

National Radio Astronomy Observatory
by Richard Emberson

Science, vol. **130,** p. 1307, 1959.

An *ad hoc* panel ... compiled a list of more than two dozen possible sites. Independent inquiries were also addressed to the U. S. Forest Service, the U. S. Park Service, the Geological survey, the Tennessee Valley Authority, the Army Map Service, and the Real Property Disposal Office of the U. S. General Services Administration. Through the early stages of the search there was some hope that a suitable site could be found on land already owned by the federal government. This hope faded and died because the search showed that any oasis of relatively flat land had been discovered by settlers more than a century earlier, and that all these coves and valleys have been in private hands for many years.

Finally, there were 30 site possibilities that seemed worthy of closer examination. Many were eliminated by visual inspection, usually because of existing urban and industrial centers close by. The five most promising sites were then subjected to a careful and detailed study.... On the basis of these detailed studies the 18th possibility on the list, at Green Bank, West Virginia, stood out. The radio-interference measurements showed that the Green Bank site was in a class by itself. Also, it was first on the basis of the population studies and first on the basis of the location of nearby towns and cities....

The steering committee unanimously recommended the selection of the Green Bank site for the proposed observatory. The committee further urged that nearly all of Deer Creek Valley at Green Bank be acquired, to insure better local protection against interference, or, if direct purchase of all the land was not feasible, that suitable controls be arranged to insure continued suitability of the site for the National Radio Astronomy Observatory....

After completion of the contract between Associated Universities and the National Science Foundation, in November 1956, for the establishment and operation of the observatory, one of the first tasks was site procurement. It was decided to let the [purchase options that had been obtained on 6000 acres] lapse and to arrange for the U. S. Army Corps of Engineers to acquire the site on behalf of the Foundation and the federal government. This decision brought an agency with vast experience in land problems to the important task of acquiring the site. The valley was divided into regions or zones, and the Corps of Engineers was instructed to start acquisition proceedings in the central zone and work outward until a total expenditure of about $550,000 had been made. At that stage, no more land was purchased. The site, thus determined, consists of about 2700 acres....

ASTRONOMY CENTER

From the Charleston [West Virginia] Gazette,
Sunday, February 19, 1956.

A Pocahontas County site is the obvious Number 1 choice for the location of a huge Federal astronomy center. The Gazette learned yesterday that a 3,500 acre tract—four times the size of Coonskin Park—is being sought in the Green Bank/Arbovale area of Pocahontas County as the site of what President Eisenhower has called the "nation's first major astronomy center."

The President already has asked Congress for an initial appropriation of about $3,500,000 to the National Science Foundation for the project. The first appropriation will be used for construction of a 140 foot radio telescope, and it is believed that consideration is being given to building another as large as 500 or 600 feet in diameter. Plans for the bigger telescope, however, are far in the future. If they materialize, eventual cost for the center might reach 25 or 30 million dollars.

Prof. Bart Bok, of Harvard University, first confirmed that a West Virginia site might be chosen in an exclusive interview granted Gazette Reporter, Dan Seagle, a journalism scholarship student at Harvard, and Seagle said yesterday that he had it from "authoritative sources," that a decision had been made in favor of the Pocahontas County tract. Bok is an astronomy instructor at Harvard and a member of the National Science Foundation's Advisory Panel on Radio Astronomy. . . .

It was subsequently learned by The Gazette that options are being taken on the Pocahontas County property and that a condition of locating the center there is that the owners of the land "must sell the property willingly."

There are other conditions too. Close scientific and geographic tests must be made to determine the suitability of the site, and the location must be approved by the National Security Council.

The observatory will be built by the National Science Foundation because no private institution could afford the costs. The center won't be operated by the Government and there is little likelihood it will undertake classified defense work.

There apparently has been little organized effort on the part of West Virginia communities to bring the Center to any particular area, although the Hinton Daily News has suggested Kenney's Knob, a high mountain in Summers County near Alderson, as a location; and the West Virginia News of Ronceverte has published accounts of "rumors" that a Pocahontas County site is being sought.

Radio telescopes are similar in appearance to radar antennae and operate in much the same manner, although they do no sending. At present, the British are building the world's largest. It is 250 feet in diameter. The largest in this country are a 60 foot model at Harvard Observatory and a 50 foot telescope at the Naval Research Observatory in Washington.

Pocahontas County, West Virginia

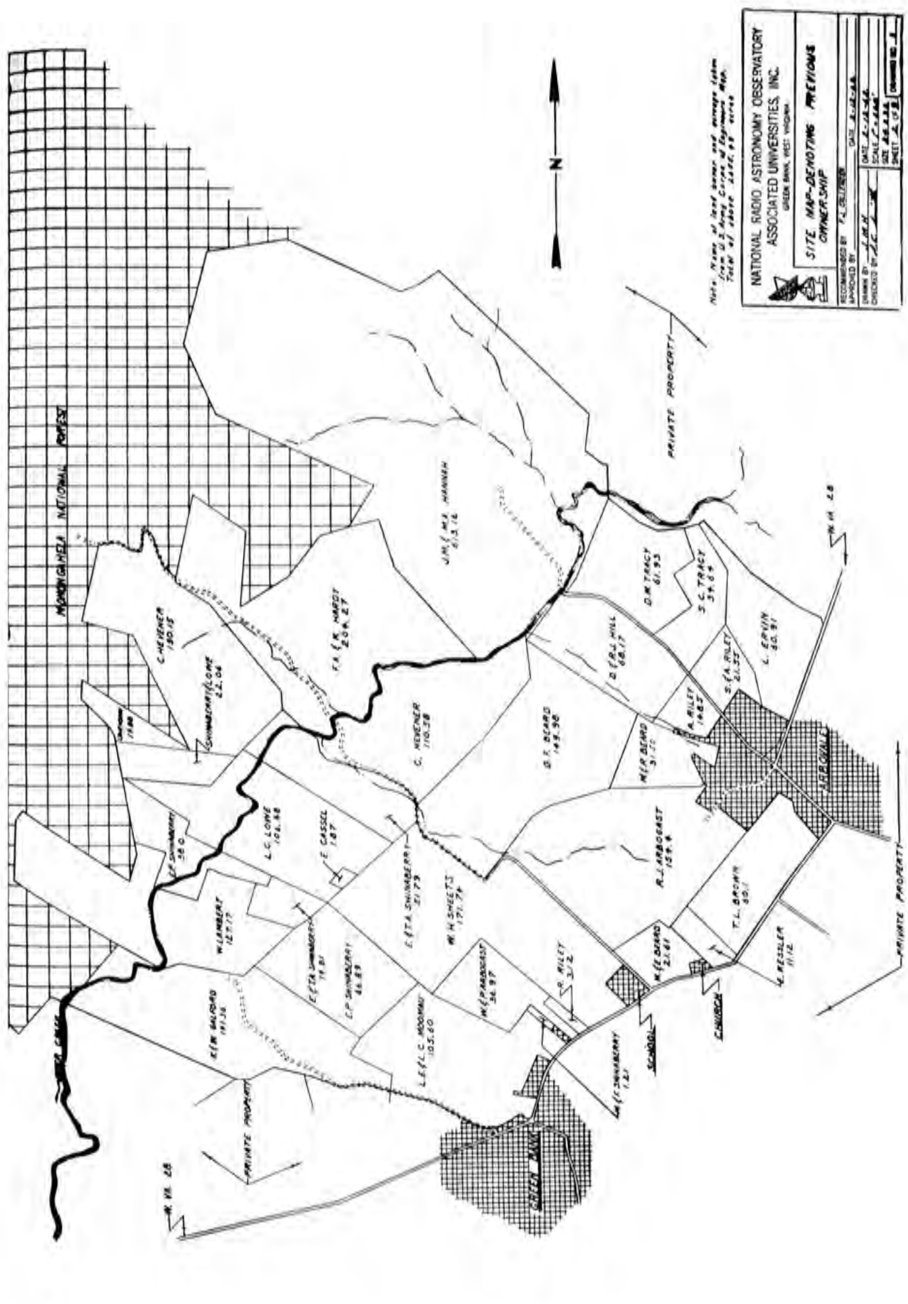
From an unsigned article
in *The Observer**, March 1977

Pocahontas County, named for the famous Indian Princess, was formed in 1821. Pocahontas was the hunting ground of the Shawnee Indians whose home was in Ohio. The original Indian Trail, known as "The Warriors' Road," established by the Iroquois along the mountains from northern New York to Georgia, went through Pocahontas and may still be seen at several points today.

Varying from 2000 to 4842 feet above sea level, Pocahontas County has an average altitude exceeding that of any other county east of the Rockies. It lies on the eastern border of West Virginia, slightly south of center, and is the third largest county in area in the state. Its 942.61 square miles contain a score of mountain peaks above 4000 feet, copious springs, beautiful mountain streams, 55 caverns, the famous Cranberry Glades, interesting geological formations and fossils, and its beauty of mountains and valleys is unsurpassed. It is known as the "Birthplace of Rivers" for from its highlands flow the Tygart, Cheat, Elk, Greenbrier, Williams, Cranberry, Cherry and Gauley Rivers westward to the Ohio, while a very small area drains to the east into the Potomac and James.

The eastern part of the County lies in the folded Appalachians and the western part in the Appalachian Plateau with the Greenbrier River as the approximate dividing line. The average annual rainfall is 49 inches. Winters are relatively mild in the valleys, more severe at the higher elevations. Summer weather is ideal. Of its 942.61 square miles (603,270 acres) over half are owned by the United States of America and the State of West Virginia. The Monongahela National Forest covers 277,037 acres.

* *The Observer* was the NRAO staff newsletter, published 1961 through 1981



NRAO site showing land owners prior to AUI–NSF purchase.

A Brief History of the NRAO Site

Excerpts from an unsigned article
in *The Observer*, May 31, 1963.

In observance of the West Virginia Centennial, we will give a short history of the NRAO site. The Shawnee Indians were the first inhabitants of this area, and the county was named for Chief Powhatan's daughter, Princess Pocahontas. The first white man to scout the area was Knapp Gregory, and thus Knapp's Creek. Stephen Sewall and Joseph Marlin crossed over the mountains from Frederick County, Virginia in 1779. They camped all winter in a spot which was referred to as "Marlin's Bottom" and was renamed Marlinton.

By act of the Virginia Legislature at Richmond, assembled Dec. 21, 1821, Pocahontas County was formed from Bath, Pendleton, and Randolph counties. The first county seat was at Huntersville. Early settlers were of German, English, Scotch, and French origin. Pocahontas County consists of 943 square miles, of which 2655 acres belong to the NRAO.

Adam Arbogast was an early pioneer who settled this area in 1796. He lived to be nearly 100 years old. He owned most of the land which is now the NRAO site, much of which he gave to his 4 sons and 5 daughters. One of Adam's great, great grandsons, Jerry Shears, is employed here. As the area became more civilized, more people settled, and Arbovale was founded— "Arbo" for Arbogast and "vale" for valley. Cass received its name from a New York lumber man, and Bartow was known as "Traveler's Repose."

The Civil War took its toll and land began to change hands much more so than before.* The map on the facing page shows the NRAO site and the land owners previous to purchase by AUI-NSF. Though our records are incomplete, the land owned by Frank Hardy was sold in 1868 by W. W. Slaven, son of John Slaven, another pioneer to the area. The land owned by Joel and Mary Hannah (site of our recreation area) was sold by Uriah Hevener in 1886. We can find no record of where he obtained this tract. The land owned by Clyde Hevener was sold by Lee Burner, son of the pioneer Abram Burner, in 1884. The land owned by R. J. Arbogast (site of the Works Area and surroundings) was sold by John Arbogast, son of Adam Arbogast, in 1894. The land owned by Moro Beard was sold by M. Arbogast in 1898.

The National Science Foundation acquired the site in 1957 (official ground-breaking ceremonies were on October 17) and construction of roads and offices were

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* *Editors' note:* Robert A. Sheets, a Green Bank resident and descendent of several early settlers of the valley, notes that during the Civil War property taxes had to be paid in "Northern Dollars" at the County Courthouse—a direct attempt to punish those residents of the newly made State of West Virginia who took the Confederate side. Allen Burner, who had owned the land later acquired by Uriah Hevener, was one of these, having enlisted in the Confederate Army at Green Bank in 1861. The sectarian tensions and subsequent consequences resulted in considerable change of land ownership for several decades during and after the Civil War.

started. The small white house on Route 28, just north of the Liberty Presbyterian Church, served as administration offices and lab until 1958 when the lab moved to the "Nut Bin" house.

Construction was begun in 1958 on the 85 Foot telescope and the Works Area building, and was completed in early 1959, at which time the lab moved to the Works Area. The Little Big Horn, Jansky Lab, and Residence Hall were also begun in 1958 and completed in late 1959. At that time the electronics, scientific services, and administration departments moved into the Jansky Lab building. Construction of the 300 Foot telescope started in 1961 and it was completed in 1962.

Moro Beard, ca.1960.
[*Courtesy E. Beard*]

Editors' note: The "Beard House" was used from 1958 to 1960 as an office building and an electronics lab by Heeschen, Findlay, Hvatum and Drake. Findlay renamed it the "Nutbin." Warren Wooddell was the lab technician and Beaty Sheets the secretary. The house was built in 1901-1902 by Irbe Beard and his neighbors. Moro Beard, Irbe's son, was born here.

In 1969 the "Nutbin" was moved to a new location just south of the Presbyterian Church on Route 92, where it remains an employee residence.

Green Bank Assured of Great Astronomy Center

And How Truly Thankful are We All, Too!

Special Dispatch to The Pocahontas Times

WASHINGTON - West Virginia will become the world centre of research in radio-astronomy; when the National Science Foundation constructs its new "window to the Universe," at the site, Green Bank, selected this morning, Monday, July 23, at a meeting of the advisory panel, composed of leading scientists and astronomers of the United States in Ann Arbor, Michigan. This decision is subject to zoning and other arrangements, which will have to be authorized by the West Virginia State Legislature. The Governor will have to call a special session of the Legislature. This Governor Marland has signified his intention of doing in order to make the project possible.

$3,500,000 has been authorized for starting construction of this laboratory. Congressman Harley O. Staggers was instrumental in seeing that cuts affecting this program were restored by Congress, and the $3,500,000 was earmarked for this special project. Congressman Staggers has been working very closely with the National Science Foundation and Association of Universities, Incorporated, on this project, which has been in the planning and developing stage for the past two years.

The overall cost of this project will amount to $30,000,000 and more.

From the Pocahontas Times, July 26, 1956

Here in beautiful Deer Creek Valley in the Allegheny Mountains of West Virginia work is under way on the new National Radio Astronomy Observatory, which is destined to become one of the great centers of scientific research in the United States.

One of the principal limiting factors in radio astronomy observations today is man-made radio "noise."

Interference can be blocked off by intervening barriers. Thus, if there is a large hill between a radio transmitter and a radio telescope, the transmitter is much less likely to be troublesome to radio astronomy observations. For this reason a valley surrounded by mountains is a good choice for a radio telescope site.

All of these factors entered into the choice of a site for the National Radio Astronomy Observatory. Green Bank is in Deer Creek Valley, at an elevation of 2600 feet, with mountains rising all around to about 4200 feet. The valley is relatively sparsely populated, there are no large population centers nearby, and the mountains effectively shield the valley from the nearer cities. As a result, the level of manmade noise in the valley is very low. The valley has a relatively mild climate, and is not subjected to very high winds, nor excessive snow and ice. It is probably the best location for a radio astronomy observatory in the Eastern United States.

From a booklet prepared by Associated Universities, Inc.
to mark the occasion of ground-breaking ceremonies
October 17, 1957

Green Bank Observatory

From the Pocahontas Times (Marlinton, WV)
November 2, 1956

Washington, October 30

After speaking with officials of the National Science Foundation today, Congressman Harley O. Staggers announced the Foundation is negotiating with Associated Universities, Inc., for the contract to construct the Radio Astronomy Facility near Green Bank in Pocahontas County, West Virginia.

"Although no actual contract has yet been signed, it is contemplated that Associated Universities, Inc., will have the management of this scientific 'window of the Universe' project," said Mr. Staggers.

The Second District Congressman was instrumental in seeing that $3,500,000 was "earmarked" for this special project by the Appropriation Committee of Congress. He has worked very closely with the National Science Foundation and Associated Universities, Inc., on the Observatory, which has been in the planning and developing stage for the past two years.

From the very beginning, Mr. Staggers "has urged that West Virginia be selected as the site for the Radio Telescope. The Green Bank site was selected over 29 other sites after extensive study was made by Associated Universities, Inc., for the National Science Foundation, over areas in the eastern part of the United States, as it fulfills the majority of requirements for an ideal site for this highly sensitive project."

"This important project will bring to West Virginia some of the leading scientists, not only of the United States, but of the world," continued Congressman Staggers. "The radio astronomy facility will be instrumental in furthering radio astronomy research and training radio astronomers in our nation. We must keep a constant drive in effort to catch up with and go ahead of the rest of the world in this important phase of science."

The Foundation's telescopes proposed to be built in Pocahontas County will range from 140 feet in diameter and larger, and the over-all cost of the project will run into several millions.

Congressman Staggers has received many inquiries from citizens living in the Green Bank area, as to whether or not they should plant crops this fall on their farms which may be acquired as part of the site for the Observatory. He has been informed by officials that farmers may plant crops, provided the crops can be harvested within one year after the land is actually acquired by the Foundation.

3 First Steps: 1956-1957

The first contract with The NSF
November 17, 1956

Editors' note: In 1956 the NSF awarded the contract for establishing and managing the new radio observatory to AUI[1] *(Associated Universities, Inc.). The NSF had to choose between three alternative plans: 1)A consortium of university astronomy departments; 2) management by a single university; and 3) AUI.*

The choice of AUI was probably due to the success of the Brookhaven National Laboratory, which AUI had been managing for several years, and also due to the influence of Lloyd Berkner, the president of AUI, whose earlier experience in radio engineering made him particularly enthusiastic about establishing a radio observatory[2]*.*

The five year term of the initial contract began November 17, 1956, and provided the initial sum of four million dollars.

Some provisions of the contract are as follows:

ARTICLE I – OBJECTIVE

The Contractor, as provided herein, shall organize, construct, operate and maintain an observatory for research in the field of radio astronomy. In carrying on such research it shall be one of the objectives of the Contractor to strengthen basic research and education in radio astronomy throughout the United States and its Territories and Possessions. The Observatory shall be made available to qualified personnel, to the maximum extent possible, for the conduct of research in radio astronomy.

ARTICLE II – DESCRIPTION OF THE OBSERVATORY

The Observatory shall be located on land in and near Green Bank, Pocahontas County, West Virginia, now, or hereafter to be, acquired by or on behalf of the Government for that purpose. The Observatory shall contain facilities and equipment appropriate for the conduct of research in radio astronomy (including one or more radio telescopes, at least one of which shall have a diameter of approximately 140 feet) and appropriate ancillary buildings and facilities all as mutually agreed upon from time to time.

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[1] The original "Associated Universities" were Columbia, Cornell, Harvard, Johns Hopkins, MIT, Univ. of Pennsylvania, Princeton, Univ. of Rochester, and Yale.

[2] See *"Science, Cold War and the American State"*, by Allan Needell, Harwood Publishers, 2000.

excerpts from

The First Twenty Years

by John W. Findlay

The Observer, March 1977

Twenty years ago in January 1957 the plan and work to build NRAO at Green Bank had been a reality for several months. The real start was the first contract between the NSF and AUI to build and operate the Observatory, and this was signed in November 1956.

Dave Heeschen and John Findlay, 1977.

By January 1957 David S. Heeschen, Richard M. Emberson and John W. Findlay were all working for AUI in its central office on the 72nd floor of the Empire State Building. Dave Heeschen had joined AUI from Harvard in the summer of 1956. He continued to live in the small town of Harvard, near the Agassiz radio telescope, for several months until he moved to Green Bank. John Findlay came to the U.S. at the end of 1956, having been recruited to the project by Lloyd V. Berkner, the president of AUI. Dick Emberson was Lloyd's right-hand man in AUI. All the early work of studying and planning for NRAO had been done by Dick under Lloyd's general guidance. I believe it is not an over-statement to say that NRAO would not exist had it not been for Lloyd's efforts.

The first weeks in New York were full of a variety of tasks. The 140 Foot design was already essentially decided, as far as its concept went, and the work on preparing the design for bidders was going ahead. The design engineer was Professor Ned L. Ashton of Iowa State University.

By May 1957 Dave Heeschen and I decided to move our operations to Green Bank. The land buying was going ahead, and we asked the Corps of Engineers to get us the use of the Kessler House (then empty) for offices. Lewis Taylor, Grover Taylor, and others cleaned out the coal and built me an electronics lab in one of the attached rooms. French Beverage was hired and Harry Wooddell followed shortly. Beaty Sheets was our first secretary.

By the groundbreaking on October 17, 1957, we were deep in the mud— but we had started.

Groundbreaking at Green Bank *
October 17, 1957

On 17 October 1957 a few hundred people gathered in the high school gymnasium at Green Bank, West Virginia, to take part in the ground-breaking ceremonies for the National Radio Astronomy Observatory. These ceremonies marked the beginning of a major effort to restore the United States to a place among the leading nations in radio astronomy as well as the culmination of a search for the most suitable site for an observatory of this kind.

The United States has lagged behind several other nations—Great Britain, the Netherlands, Australia, and the Soviet Union—in the development of radio astronomy despite the fact that radio waves of extraterrestrial origin were first detected by Karl G. Jansky, a Bell Telephone Company engineer, in this country in 1932. The rapid progress abroad has been made possible by the construction of large radio telescopes . . . [financed] either directly as in the Soviet Union or indirectly as in Great Britain. . . . In the United States, until recently, no large-scale support was available, . . . the largest radio telescope in the U.S. is the 60-foot paraboloid at Harvard.

from *Radio Astronomy at Green Bank*,
the editorial in Science, 8 November 1957, Vol.**126**
by Graham DuShane, Editor

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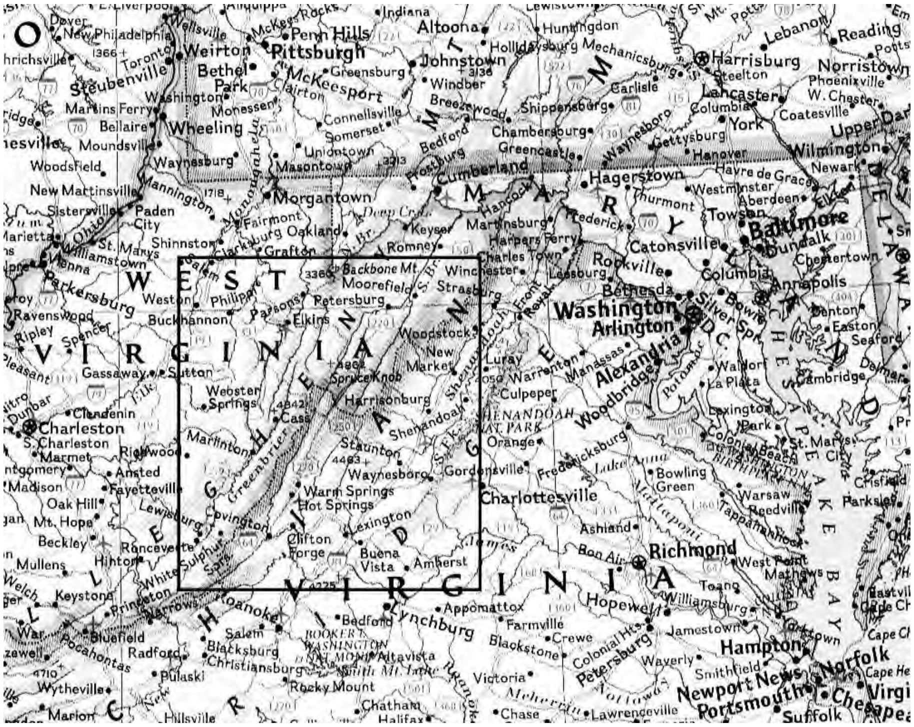
* This occurred 13 days after the launch of Sputnik, the first artificial satellite, by the Soviet Union.



The National Radio Quiet Zone was established by the Federal Communications Commission in 1958 to protect the NRAO and the U.S. Navy Sugar Grove stations. It is bounded by latitudes 37°30' and 39°15' North, and by longitudes 78°30' and 80°30' West.

National Radio Quiet Zone*

The special radio-noise problem at Green Bank and Sugar Grove was taken to Washington. After thorough hearings and several reviews had been completed, special rules were promulgated to establish a radio quiet zone for both Green Bank and Sugar Grove.

The special rules provide that civil applications for new or revised transmitters in the quiet zone shall be brought to the attention of the director of the National Radio Astronomy Observatory, who is responsible for bringing the matter to the attention of the Navy at Sugar Grove and submitting a coordinated reply or comment to the Federal Communications Commission.

West Virginia Radio Astronomy Zone*

Contact was made with the governor of West Virginia, William Marland, and he and members of his staff were briefed on the proposed observatory. The West Virginia officials were favorably disposed toward the plan.... a special session of the Assembly and Senate convened on 9 August 1956.... The legislature enacted the Radio Astronomy Zoning Act. [*This provides protection within ten miles of the Observatory.]*

* from Science vol.**130,** p.1307, 1959, by Richard Emberson

4 Equipment plans

The Radio Astronomy Facility will be equipped with 3 reflectors. Present plans show one reflector of 150 ft. diameter, one of 250 ft. diameter, and one of 600 ft. diameter. It is contemplated to start the project with a 150 ft. reflector, and to add the larger equipment at a later date.

H.J. Bade of Eggers and Higgins, Architects
Feasibility Report for the National Science Foundation
on Construction of a National Radio-Astronomy-Facility
May 1, 1955

It is against the background of developing research experience and a growing acquaintance with instrumentation that we must see the plan for a national radio observatory. Whereas, four years ago, the construction of a 50-foot paraboloid was considered a major new engineering effort, we contemplate now with at least equal confidence the construction of a 100-foot steerable paraboloid. The step from a 100-foot to a 140-foot paraboloid seems like a relatively modest one and, from the engineering point of view, a safe one.

Bart J. Bok
Toward a National Radio Observatory
August 7, 1956

Dr. Struve has told [an NSF official] that the 300 Foot Antenna is an absolute necessity and that funds should be provided to meet the cost of studies for the Very Large Antenna. He emphasized that NRAO is not like Kitt Peak, which can fulfill a useful function by providing conventional optical observing equipment, of which there is a shortage. If NRAO is to be a truly national observatory, it must have the largest possible telescopes.

from the Minutes of an NRAO Staff Meeting, August 27, 1959

One cannot simply build a radio telescope and present it to a radio astronomer or to all radio astronomers as one might give a Cadillac to someone for Christmas. It appears to the Panel that the telescope construction programs which have been the most successful have been carried out under the enthusiastic direction of some very competent person who has dedicated several years of his life to the design and construction of the telescope, usually in anticipation of using the telescope when completed. It is hard for a committee to plan or build a useful radio telescope, and doubly hard (if not impossible) if the members of the committee are not to be the actual users of the telescope.

Report of the Advisory Panel on Radio Telescopes
of the National Science Foundation (the "Pierce Committee")
Astrophysical Journal, 1961, **134**, 927

Basic Equipment

from "Plan for a Radio Astronomy Observatory"
Associated Universities, Inc., August 1956

A. Philosophy

The choice of antennas for the Radio Astronomy Observatory is based on the philosophy that the facility should provide research opportunities not available elsewhere. The availability of the large steerable paraboloids (radio telescopes) depends to a large extent on the cost, which varies probably as the cube of the diameter of the reflector. In offering the use of other types of antenna systems that cost less, and are thus more widely available, the facility is nevertheless offering unusual research opportunities: a favorable intellectual environment and an observational environment that will probably become more and more unique with the passage of time as a consequence of the growth of electromagnetic transmitting systems elsewhere throughout the United States. . . .

[M]any research projects require the use of very large radio telescopes; since their cost is beyond the means of the average university, it has been agreed that such instruments should be the principal items in the equipment program for the Facility.

B. Very Large Reflectors

At the outset we face the question: Are there any real technical limitations of construction on the maximum size of steerable radio telescopes? A fixed system such as a parabolic bowl hollowed out of the earth, might conceivably be made a mile or more in diameter. But when we require also that the reflector be steerable, we confront serious structural problems and one of the most fundamental is the strength of the materials. This question has been put to several structural experts and the consensus is that diameters of several thousand feet are feasible with the structural materials now available, provided that cost is no object. . . .

The 140 Foot Radio Telescope Program

At its March 26, 1955 meeting, the Steering Committee concluded that the Radio Astronomy Observatory should be established and put into operation as promptly as possible. The Committee therefore proposed that immediate steps be taken to provide a radio telescope with an aperture of about 150 feet. It was suggested that several military designs for paraboloids in the 50- to 80-foot diameter range might be extrapolated to a greater than 100-foot size and still be essentially an "off-the-shelf" design and construction task.

5 Getting Started—1958

Main Entrance to NRAO in 1958.

The founding of NRAO presented AUI with some problems that had not confronted it in 1946-47. In the case of Brookhaven National Laboratory, once the site was selected, AUI moved at once into a large Government-owned establishment with numerous buildings and an ancillary staff (e.g., guards, maintenance personnel, groundskeepers, and janitors) and many facilities, such as power, water, and telephone, were already available. Thus, the Director and his staff were able to devote time to substantive long-range problems without the pressure of acquiring the bare necessities of institutional life. At NRAO, on the other hand, the site of 2700 acres was acquired by the Government from more than a dozen different owners, and in taking it over in November 1957, AUI began the development of the Observatory from scratch.

from "Twenty-five Years of Research Management"
Associated Universities, Inc., Washington DC, 1972

Green Bank: 1958

Selections from the NRAO Monthly Summary of Activities

September 9, 1958

[by R. M. Emberson]

In this first issue of the NRAO monthly reports, a few remarks are in order concerning the Observatory organization and operations. Pending the designation of the permanent Director, L. V. Berkner is the Acting Director, and R. M. Emberson the Acting Deputy Director. Staff activities are divided into four departments: astronomy, electronics, construction and management, of which D. S. Heeschen, J. W. Findlay, R. M. Emberson and F. J. Callender are the respective heads.

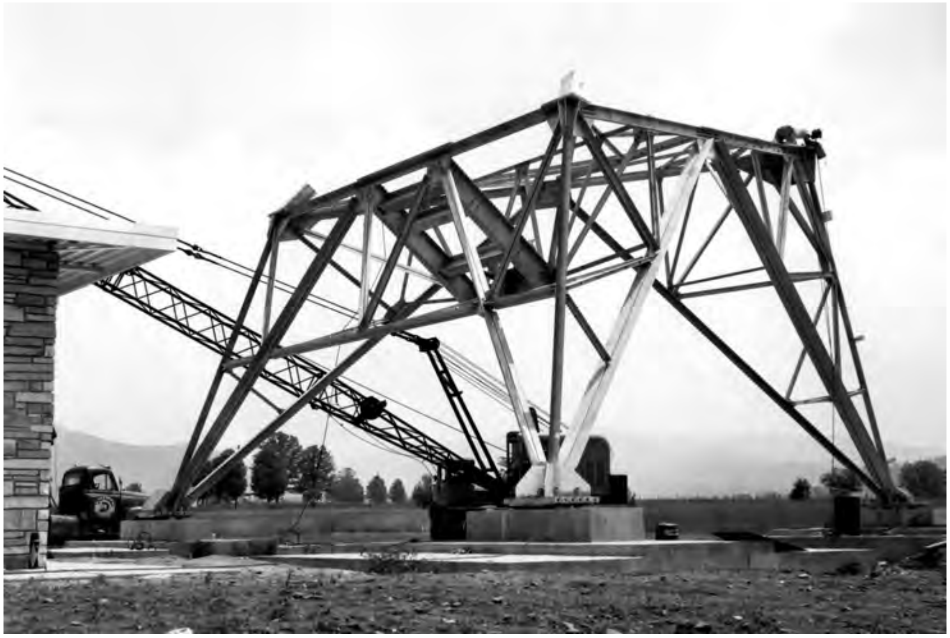
Mr. Sidney Smith joined the staff on August 1, 1958 as an assistant engineer. He is a civil engineer and will be working primarily on field engineering and inspection. Mr. Spencer Greenwood has been employed as a member of the engineering and inspection staff and will report at Green Bank on September 2. Mr. Dewey Ross was added to the staff of the Electronics Department as an electronics technician on July 1, 1958.


Sidney Smith, 1966.

Arrangements have been made to establish an Exchange Visitors Program for the Observatory in connection with the appointment of Professor Hein Hvatum as a research associate for one year, starting October 1, 1958. Professor Hvatum is a physicist, presently working at the Research Laboratory of Electronics, Chalmers Institute of Technology, Gothenburg, Sweden. He will join the Electronics Department and participate in such programs as receiver development, absolute flux measurements, and antenna studies. Mr. Grote Reber, appointed NRAO's first visiting astronomer, postponed his arrival from July until early Fall.

Beginning on the first of July the Observatory entered into a temporary program of land rental, primarily to former owners, for grazing and haying privileges. Rates were established which were approximately mid-way between the rates charged by the National Forest (which are low) and the private rates prevailing in the area. It is estimated that during this summer and fall such arrangements will cover 70 to 80% of the pasture and meadow land on the site.

In July bids were received for the Works Area Building and a contract awarded to the B. F. Parrot Co. of Roanoke, Virginia. The contract calls for construction of approximately 11,000 square feet of space at a cost of $10.68 per square foot. . . . in August bids were invited on the Laboratory building and the Residence Hall and Cafeteria. . . . it is hoped that work will begin on or about October 1, 1958.



The base of the 85 Foot Telescope under construction, August 1958.

Erection of the 85 Foot Telescope was begun July 26 by the Radio Construction Corp., under sub-contract with the Blaw-Knox Co. A preliminary adjustment of the polar axis was made August 15-18. . . . Erection is progressing satisfactorily. The declination axis will be erected and adjusted during the first two weeks of September.

12-foot Reflector
An aluminum parabolic spinning that is a very close scale model of the 85 Foot and 140 Foot reflectors has been procured for feed development and field pattern measurements. A nine-foot square building has been constructed, to serve as a pier for mounting the reflector and to house the electronic and other associated equipment.

——— October 7, 1958 ———

[by R. M. Emberson]

The position of telescope operator is one which was provided for in the Position, Wage and Classification System approved by the Trustees last Spring. It is new in concept and the evolution of the duties of the position as we actually get into operation as an Observatory will be watched with great interest.

Erection of the 85 Foot Telescope has proceeded at a satisfactory pace during the past month. . . . Observatory personnel are now working with the erection contractor to line up the reflector surface supporting structure and check it for deflections in various orientations.

The telescope is nearing completion, with only the surface panels, feed support, and gears remaining to be erected and aligned, and the drive system wired and aligned.

Three scientists have definite, firm plans for observing with the 85 Foot Telescope during the next 6 to 9 months. In addition, 6 others have tentative plans for work at NRAO.

——— November 5, 1958 ———

[by R. M. Emberson]

During the month of October, the staff of the Observatory was increased by four – all in the Astronomy and Electronics Departments. As noted in last month's report, a successful attempt was made to recruit two telescope operators for the 85 Foot Telescope. The first of these to join the staff was Mr. William Meredith, now a senior at West Virginia State. Mr. Meredith will receive his B. S. in physics and mathematics in January, and while working at NRAO he is completing the last remaining seven credit hours necessary for the degree. Mr. James F. Crews, a man of similar background and training, joined the staff in the last week of October.

Bill Meredith, 1962.

> "Back when Fred and Omar and I were hired, the first question they asked us was could we read or write. Nowadays they ask 'Do you know UNIX?' "
>
> *Bill Meredith, reminiscing in 1992*

Since July 1, 1958, approximately $1,000 has been realized in site income from rentals for grazing and haying. Where possible, some winter grazing will continue.

Furniture has been purchased for the bachelors quarters on the site, which should be occupied by November 15, 1958. Likewise the residence for Mr. and Mrs. Hvatum has been furnished. It is understood that Mrs. Hvatum is having a wonderful time introducing herself to American pop-up toasters, steam irons, automatic washing machines, and the like.

A number of difficulties and delays [in the 85 Foot construction] were encountered this past month. The principal problems arise in the feed support legs, and in the making of certain measurements, particularly in connection with the surface and with the drive gears. It was found that the focal point underwent a lateral displacement of almost 2 inches as the reflector was moved from the zenith to an elevation of 45°. . . . It was also discovered that, under certain wind conditions, the feed support legs could go into oscillations of uncomfortably large amplitude, presumably due to a Kármán Sheet being set up behind the cylindrical leg. . . .

Excavations for the [140 Foot] foundations, which started September 22, progressed down through strata as predicted from our core borings and at approximately the estimated level the footings for the foundation were placed on October 28.

Grote Reber* has arrived in Green Bank, and will be here for some time as a research associate. He plans to make radio astronomy observations at very long wavelengths, probably within the frequency range 30 kc – 70 kc, using a long wire antenna stretched across a valley. Little is known of the spectra of sources and galactic background emission at very long wavelengths – Reber's work in Tasmania being about all that has been done. It is a problem of very great interest, particularly in connection with the mechanism of rf energy production, and the interstellar electron density.

Reber will also supervise rehabilitation of his original 30-foot paraboloid, which is being done to preserve it as an exhibit of considerable historical interest, and perhaps provide an antenna for preliminary testing of new receivers as well.

During the third week of October the Observatory, on three consecutive days, was host to the Radio Astronomy Advisory Committee, conducted a dedication of the 85 Foot Telescope in memory of Howard E. Tatel, and handled arrangements for the annual meeting of the AUI Board of Trustees. We were pleased and honored to have Dr. Waterman participate in the dedication ceremonies, as well as to have the opportunity to show other guests the progress made during the past year. In terms of logistics, considering the remote location, it is perhaps interesting to note that during the three days 315 meals were served, 6,000 passenger miles were driven (within a radius of 75 miles), and over-night accommodations were arranged for approximately 75 individuals.

The "Field Offices," a combination of the Kessler house and other buildings, were the site of the Observatory's first administrative offices. They were located north of Liberty Presbyterian Church on Rt. 28.

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* Accounts of Reber in Green Bank are given in part IV.

Installation of the polar shaft on the 85 Foot Tatel Telescope.

6 The First Telescope

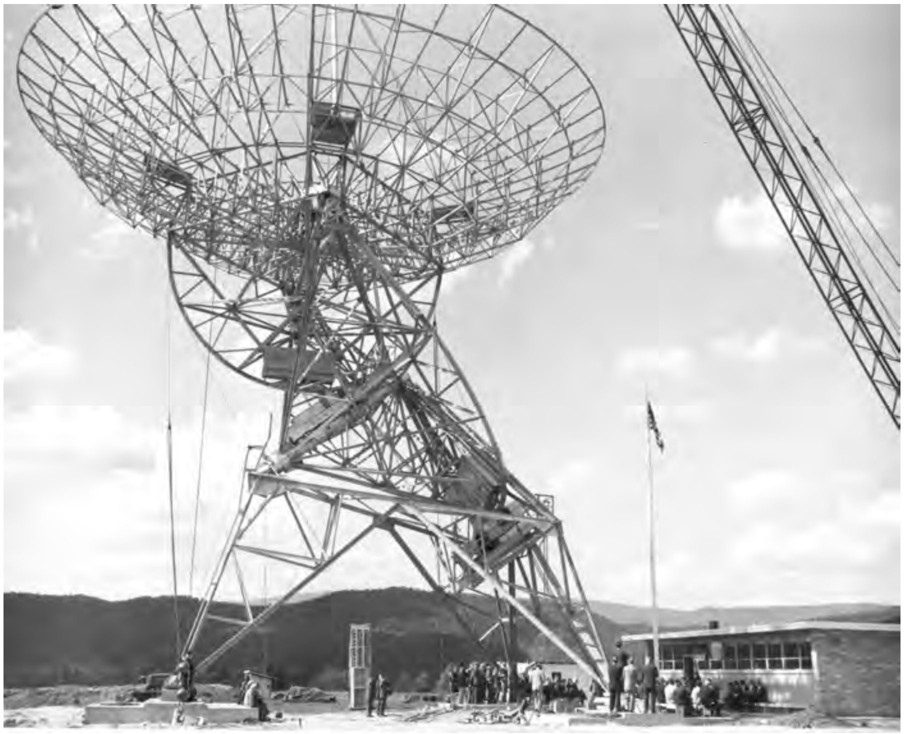
Perhaps the first thing we did to modify the original NRAO plans was to buy the first 85-foot telescope. Dave Heeschen and I met for the first time at an American Astronomical Society meeting in New York in January 1956, and almost at once we said we should try to buy a telescope in advance of the completion of the 140 Foot. Things went so fast in those days that we had the word from Lloyd Berkner (who was acting as director of the non-existent observatory) within weeks. We were thus able to join JPL and Fred Haddock [University of Michigan] in buying one of the first three 85-foot antennas built by Blaw-Knox Corp.

John W. Findlay
in the *Observer*, March 1977

The site was first occupied in May 1957, at which time a temporary AUI office in Marlinton was moved to Green Bank. Ground-breaking ceremonies were held on October 17, 1957. The dedication ceremonies for the Howard E. Tatel telescope were held on October 16, 1958. Dr. Berkner presided. Dr. Waterman, Representative H. O. Staggers, and Dr. Otto Struve spoke. Mrs. Tatel was introduced. Professor E. J. McShane represented the National Science Board. The NRAO Advisory Committee, the AUI Trustees and officers, Mr. A. H. Jackson and Mr. R.D. Hall representing the Blaw-Knox Company, and others particularly interested in the Observatory and telescope were present, including Dr. M. A. Tuve, who collaborated with the late Mr. Tatel in the design of the Telescope, and Mr. C. M. Jansky, Jr., brother of the late Karl Guthe Jansky.

NRAO Annual Report, July 1, 1959.

Howard E. Tatel (1913-1957)



The 85 Foot Tatel Telescope at the time of its dedication on October 16, 1958. The surface panels had not yet been installed.

R.D. (Bob) Hall (1922-2010) completed the detailed design of the 85 Foot telescope, based on Tatel's concepts. (photo from 1995).

Dedication of the 85 Foot Radio Telescope
October 16, 1958

Transcribed from a recording of the event.

DEDICATION
OF THE
HOWARD E. TATEL TELESCOPE
AT THE
NATIONAL RADIO ASTRONOMY OBSERVATORY
GREEN BANK, WEST VIRGINIA
OCTOBER 16, 1958

MASTER OF CEREMONIES

DR. L. V. BERKNER, PRESIDENT
ASSOCIATED UNIVERSITIES, INC.

PRESENTATION OF FLAG

THE HONORABLE HARLEY A. STAGGERS
REPRESENTATIVE 2ND CONGRESSIONAL DISTRICT

GUEST SPEAKER - HONORING SCIENCE

DR. ALLEN T. WATERMAN
DIRECTOR, NATIONAL SCIENCE FOUNDATION

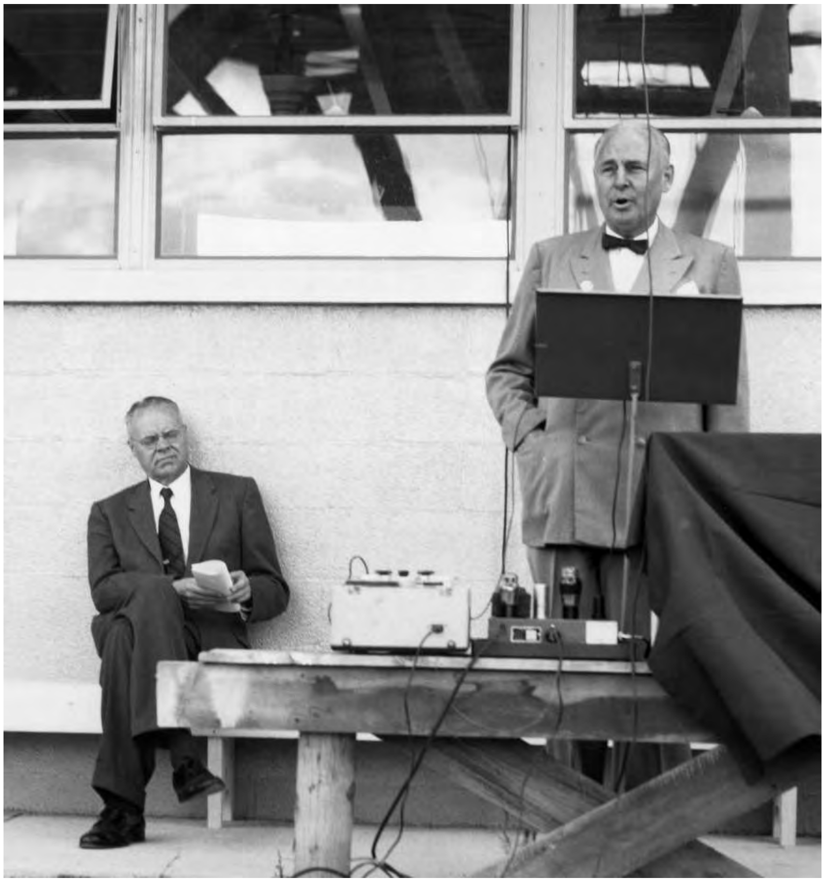
DEDICATION OF TELESCOPE

DR. OTTO STRUVE, TRUSTEE
ASSOCIATED UNIVERSITIES, INC.

Dr. Berkner[1]: On this occasion it is especially pleasing to be able to present to you Representative Staggers who has worked with us since the very inception of the project and is here for this meeting today and tomorrow. We are also honored to have a great many very special guests here today. We did not want to make a very large ceremony of this, but we are delighted to welcome some of our neighbors here too.

Representative Staggers has worked with us in the Congress to make satisfactory arrangements for us to occupy this site. I am most happy to present to you Congressman Staggers. We are delighted to have you here today.

Representative Staggers[2]: Thank you Dr. Berkner. Dr. Berkner, distinguished guests, ladies and gentlemen. It is my high honor and privilege to be here at this dedication, I assure you, for two reasons: first to present the American flag to Dr. Berkner, President of Associated Universities, Incorporated, for use at this Observatory. And second, to pay tribute to three great Americans. First of these is Dr. Alan T. Waterman[3], Director of the National Science Foundation in Washington.



Otto Struve (seated) and Lloyd Berkner.

Dr. Waterman is not only a scholar, a scientist, a great American, but he is a man of integrity and a man that I am proud to call friend. Second is Dr. Lloyd Berkner, President of Associated Universities, Inc. Dr. Berkner, as you have seen here today, is a man of great energy, a man of vision, a man of science, a man that will get things done. The third is Dr. Richard Emberson[4]. I don't even know what official title he has, but I will tell you that Dick, to me, is one of the finest type of American citizen that we can find anywhere in the land: scientist, organizer, great fellow and just a grand guy, and another man that I am glad to call friend. We can be proud of these men.

I don't know how history will record them, but their activities in behalf of this institution, and other activities similar to it across America, will have a great deal to do with the destiny, not only of you and your children and of this land, but of civilization itself.

We are here to dedicate this telescope, and I am here to present the flag. It was 133 years ago that one of our great presidents, John Quincy Adams, said in his first State of the Nation speech to the Congress of the United States, something to the effect that in accordance with building a university, it would be worthwhile to put there an observatory with an astronomer to watch the happenings of the

Struve, Berkner, and (standing) Harley O. Staggers, Sr. (1907-1991), U.S. Representative 1949-1980.

heavens and to record those happenings and to publish them so that the people could know[5]. Now here, 133 years later, we are doing just as he envisioned at that time: putting up a "lighthouse of the skies." This telescope and the large one being built over there [the 140 Foot] will help to make this a great land, will aid in our progress and in our security, and in our way of life.

This American flag that I am to present to Dr. Berkner is symbolic of the understanding and the working relations between the elected representatives in Washington and the scientists who will work here, at Green Bank, and with the other scientists across America, who are working together to make this a better America, a stronger America, and a better place in which to live. Dr. Berkner, it is my sincere pleasure to present to you this flag, with my compliments, and to all of you assembled I say, may your future path through life be through green pastures and beside the still waters.

Dr. Berkner: Thank you Representative Staggers. Could we have the flag raised at this time. May I say while the flag is raised that I think I could pay you no better compliment than to say that we are all very happy to be your neighbors here in West Virginia. I think it is very symbolic that this flag, which has flown over the US Capitol, should now fly aside of our new telescope which is just being completed here. I have in front of me a statement from George Stewart, architect of the Capitol, and it says, "Dear Congressman Staggers: This is to certify that the enclosed flag has flown over the Capitol of the United States. It is my understanding that you will present this flag to the National Radio Astronomy Observatory at Green Bank, West Virginia." Representative Staggers, we are very happy to accept this flag and we are very grateful to you for thinking of us in bringing it here today. I may add that this flag will be saved, and when we dedicate the big laboratory at a later date, it will fly on the permanent flagpole at the laboratory and be the number one flag at the Observatory.

Dr. Bronk could not be here today for reasons that you may have seen in the paper[6], but he sends the following telegram: "As chairman of the National Science Board, and President of the National Academy of Sciences, I send to you and your colleagues congratulations on the inauguration of this new facility for the exploration of our universe, and the extension of man's minds. Best wishes for the future of your great adventure." signed, Detlev W. Bronk, President of the National Science Board and of the National Academy.

We have three very special guests here today and I would like to introduce them at this time. Would Mrs. [Mollie] Tatel, Miss Judy Tatel and David [Tatel] stand up so that everyone can see you as our very special guests here this afternoon[7].

It's now my real pleasure to ask our old friend, Dr. Waterman, who has worked almost as hard in getting this facility as any of the others of us, and in some cases, I think, a good deal harder, to make a few remarks at this time: Dr. Waterman, the Director of the National Science Foundation.

Dr. Waterman: Dr. Berkner, Congressman Staggers, Friends and Guests: Today is one of those all too rare occasions on which we may give ourselves over to honoring science and the men who make it possible. Most of our day-to-day efforts consist of the toil, sweat and tears which seem to be necessary to ensure the continuance of scientific effort. The Green Bank Project has had an ample share of this kind of effort, as Dr. Berkner has just suggested, and as those of us can testify who have lived with its budgetary vicissitudes. Today, however, we can put these things behind us, and join in the general satisfaction that the accomplishment of a major step forward brings with it.

Just one year ago we were meeting here to join in a symbolic ground breaking ceremony marking the beginning of a new Observatory. On the anniversary of that occasion, we are here to dedicate the first of the two major instruments that will constitute the National Radio Astronomy Observatory. The National Science Foundation congratulates Associated Universities, Inc., and the Blaw-Knox Corporation on having passed the first milestone.

Such a ceremony as this is of course in no way essential to our final objective. We could push the Observatory through to completion and put it into operation without so much as an hour's loss for ceremonial purposes. But this occasion affords opportunity for the scientific community to observe a significant step in an impressive undertaking, while at the same time it attracts the attention of others to the plans and the thinking of those engaged in the pursuit of pure science. Such an

L to R: Alan T. Waterman, first Director of the National Science Foundation. Mrs. Howard Tatel, Judy and David Tatel.

individual chooses science as a career not because he thinks that it is going to make him rich, or even famous, but because it offers spiritual and intellectual satisfaction beyond those of other fields and because it satisfies a creative urge associated with all pioneering. He may not think of it in those terms, but he has, in effect, chosen to dedicate himself to the service of mankind.

It is appropriate therefore that we should, from time to time, pause to honor the intellectual labors that have done so much to enrich and benefit mankind, and in so doing, to honor the men who have devoted themselves to their pursuit.

Fifty years ago *The London Times*, commenting on the Jubilee of the Oxford Museum, observed that "There was no practical need for Tycho Brahe to don his richest robes when he entered his observatory, but our hearts warm to him for this visible sign of reverence for his work."

Similarly, it could be said that there is no practical need for us to gather here today to dedicate the great steel structure that will shortly give us yet another window on the universe. But we do so because we wish to honor those activities that are undertaken solely because they widen our horizons and increase our storehood of knowledge.

The United States has lagged behind other nations in the proper acknowledgment of, and tribute to, intellectual achievement. Where is there a street named for a poet or a scientist, outside of some scientific communities like Brookhaven and Los Alamos? In Russia, by contrast, the great thoroughfares are named for musicians and poets, the big new university in Moscow honors a scientist, Lomonosov, and there is scarcely a square that has not its statue to a writer or a scholar. Nor must we lose sight of the fact that these visible tributes to intellectual activity are powerful incentives, along with the material advantages the state holds out as a reward for scholarly achievement.

For years we have been in this country glorifying the businessman, the banker, the entrepreneur – in general, men of affairs, men of action. While we do not begrudge this to them, is it any wonder that they have become the symbols of achievement, and the scholar [is seen as] the longhair who failed to make the grade? Surely there is room in the great diversity and richness of our culture for the proper respect for both.

Circumstances have conspired to make this a particularly happy occasion. We may be pardoned if we still bask a little in the glow of Pioneer's spectacular flight[8]. We take special satisfaction in the role played by radio astronomy in the successful tracking of this moon rocket. It is typical of scientists, incidentally, that we should receive such proud and generous congratulations from fellow scientists around the world.

We are especially fortunate in having with us today, to dedicate the new telescope, a man whose family has for generations been in the habit of founding observatories. Otto Struve's great grandfather, Friedrich Wilhelm Struve, built and became the first director of the great Observatory at Pulkovo, a post which passed to his son upon his death. His uncle moved to Neubabelsberg and completely remodeled the old Observatory at Berlin, and his father was professor of astronomy at the University of Kharkov.

Dr. Struve's own career in astronomy is too well known to need recapitulation here, but I might remind you that it includes, in a long list of distinguished achievements and honors, the founding of the McDonald Observatory in Texas. We are glad to have him take part in this ceremony today, for his presence brings with it the aura of a great scientific tradition. We hope for the Observatory that is being established here, some of the luster that has attended the observatories associated with the name of Struve.

For all of us, the occasion is one of high anticipation. The realization of fine new research facilities is always a cause for rejoicing, but certainly never more so than in this, the dawn of the so-called "space age."

The Foundation joins with Associated Universities, with the staff of the Observatory, and with friends here at Green Bank, in wishing for the Observatory a brilliant future as it prepares to join the ranks of those whose task it is to probe the unknown.

Dr. Berkner: Thank you very much, Dr. Waterman, for honoring us here today. You will notice that we have a tape record of your remarks on the occasion of this dedication. It now gives me great honor indeed to ask one of our distinguished trustees, one of the world's distinguished astronomers, to dedicate this telescope: Dr. Struve.

Dr. Struve[9]: Mr. Staggers, Dr. Berkner, radio astronomers and just ordinary astronomers: I was touched by the remarks which Dr. Waterman made about my family and assure you that he knows the history correctly.

Otto Struve, who became the first director of NRAO in 1959.

Ten days ago I was attending a meeting of the International Council of Scientific Unions in Washington, and I was talking with Dr. Oort, the famous astronomer of Leiden, Holland, who wanted me to participate in a small conference pertaining to the International Astronomical Union. And I had to tell Dr. Oort that I would not be able to attend this conference because the President of the National Research Council's Space Science Board, Dr. Berkner, had just appointed me to Professor [Leo] Goldberg's Committee on Astronomy in Space Research, and had given me strict orders to report to Dr. Goldberg the following day. And I added that the President of the International Council of Scientific Unions, [also] Dr. Berkner, had told me that I could safely cut short my attendance at ICSU and go to Ann Arbor. And Oort turned to me and he said "Struve, I can see that you are a good soldier of General Berkner's army."

Well, as a soldier in the armed forces of an allied nation long ago, I learned that it is the duty of a soldier to obey the orders of his superior officers, and now I am standing here because I could see nothing unlawful in the orders which I had received from Lloyd Berkner and I knew that the only excuse that a soldier has for disobeying orders is if he recognizes that the order violates the law.

It is true that I can see no very good reason why an old fashioned stellar spectroscopist should talk to radio astronomers about radio astronomy at the dedication of a radio telescope. But perhaps while we are assembled here for this happy occasion, it is appropriate that we should also celebrate the 70th anniversary of the great work of Heinrich Hertz, who in 1888 discovered the existence of radio waves.

From left: Mollie Tatel, Merle Tuve, Winifred G. Whitman (Tuve's wife), Judy Tatel, David Tatel.

As most of you know, he became interested as a very young man in Maxwell's theory of electromagnetic waves—a theory that was published just a little more than 100 years ago. Hertz was experimenting with oscillating discharges by means of a spark, and he was able to detect sparks between the ends of a coil of wire placed at a distance from the transmitter. He discovered that metal plates would reflect the radio waves and that when the beam of the radio waves fell at right angles upon the reflecting surface standing waves were created so that the spark in the coil would be observed only at regular intervals and not throughout the space between the transmitter and the receiver.

It would be inappropriate for me to try to tell you what this 85 Foot Telescope will accomplish. But I think it might be appropriate for me to say a few words primarily to the young astronomers who are now connected with this Observatory and who will be coming here from other institutions. As you know, most of my work in the past has been concerned with research, but after I went to the University of California, I found that one of the most rewarding activities was teaching, and especially the teaching of very young students. And I found to my surprise that these young students, the brightest among them, would frequently ask me questions of very profound and deep interest, and they caused me to realize that these fundamental questions of physical science such as the meaning of inertia, or the meaning of the concept of the age of the universe, are questions that I used to have when

Commemorative Plaque, on display near the Tatel Telescope Control Building.

I was a very young man. I want to ask the young astronomers to try to realize what I think I have realized too late in my life, that the aging process of the human mind involves a gradual attempt to stay away from the most profound problems of science, those problems that are most difficult to answer. Now I do not recommend that the young astronomers should all become visionaries who are only thinking about problems that they will perhaps never be able to answer. But in looking at this telescope, I hope that the younger astronomers who have an extraordinary opportunity to carry on important research in the field of astronomy, will carry on the routine tasks of science that must be done, in an atmosphere of great ideas.

And now on behalf of the Trustees of AUI, and on behalf of the astronomers who are assembled here, the astronomers of the United States and of the whole world, I want to call especial attention to this tablet, which honors the memory of Mr. Tatel, Howard Tatel, who has done so much to develop radio astronomy. Many of his ideas are incorporated in the telescope we are seeing here. I thank you.

Dr. Berkner: Thank you very much indeed, Professor Struve. I have here a telegram from the Dutch radio astronomers, which I think we all particularly appreciate because of their position of leadership in the science of radio astronomy. It is to the National Radio Astronomy Observatory and it says: "Best wishes for your venture into the unknown."

Before we close this ceremony, there are two things that I think we should do; one is to introduce you all to the staff of the Radio Astronomy Observatory. Would the staff stand up so that everyone can see you. These are the people that are primarily responsible for the large piles of dirt that you see around and the structures that you now see growing on top of them.

Secondly, would Mr. Jackson from the Blaw-Knox corporation and Mr. Hall[10], the designer of this telescope, come up here so that we can give them a hand. We are very pleased to have you here. Mr. Jackson is responsible in the Blaw-Knox corporation for this work, and Mr. Hall is the engineering designer of the telescope itself. Hall, as you know, used the model originally developed by Howard Tatel as a basic concept for this telescope.

May I now say to you on behalf of the staff of the Radio Astronomy Observatory and on behalf of the Trustees of Associated Universities, Incorporated, how appreciative we are for your presence here today. We are all delighted to have this opportunity to dedicate this new instrument, which, as Dr. Waterman has said, will now soon go into service to press through our window into the universe around us. I hope with this tool and other tools like it that we will have here at the Observatory, we can do our share to extend the boundaries of human knowledge, and to direct our attention to some of the really fundamental problems in human thought that remain unanswered. Thank you very much for being here today.

Editors' Notes

[1] Dr. Lloyd V. Berkner (1905-1967) was President of Associated Universities, Inc. 1951-1960, and Interim Acting Director of NRAO 1956-1959. His part in the founding of NRAO is described by Allan Needell in *Lloyd Berkner, Merle Tuve, and the Federal Role in Radio Astronomy*, 1987, OSIRIS, 2nd series, vol 3, p. 261 and in Needell's book *Science, Cold War, and the American State: Lloyd V. Berkner and the Balance of Professional Ideals*, Harwood Academic Press, 2000.

[2] Harley Orrin Staggers, Sr. (1907-1991) represented the 2nd WV district in the 81th-96th Congresses, 1948-1980.

[3] Dr. Alan T. Waterman (1892-1967) was the first Director of the National Science Foundation 1951-1963. Further information on him and his role at the NSF can be found in *A Patron for Pure Science*, by J. Merton England, 1982, National Science Foundation, Washington, DC. His speech is transcribed here as it was delivered; it differs in a few small ways from the version in a press release circulated by the NSF.

[4] Dr. Richard M. Emberson (1914-1985) was executive assistant to Lloyd Berkner and Acting Deputy Director of NRAO in 1958. He had principal oversight of the 140 Foot Project and was based at AUI offices in New York city. In 1962 he joined the Institute of Radio Engineers, which later became the Institute of Electrical and Electronics Engineers (IEEE). There he became Staff Director of Technical Services, Executive Director, Director Emeritus and member of the Board of Directors. A prize is awarded by the IEEE each year in his name.

[5] The reference is to John Quincy Adams's first State of the Nation Address in 1825 in which he said:

> *Connected with the establishment of an university, or separate from it, might be undertaken the erection of an astronomical observatory, with provision for the support of an astronomer, to be in constant attendance of observation upon the phenomena of the heavens, and for the periodical publication of his observances. It is with no feeling of pride as an American that the remark may be made that on the comparatively small territorial*

> *surface of Europe there are existing upward of 130 of these light-houses of the skies, while throughout the whole American hemisphere there is not one. If we reflect a moment upon the discoveries which in the last four centuries have been made in the physical constitution of the universe by the means of these buildings and of observers stationed in them, shall we doubt their usefulness to every nation? And while scarcely a year passes over our heads without bringing some new astronomical discovery to light, which we must fain receive at second hand from Europe, are we not cutting ourselves off from the means of returning light for light while we have neither observatory nor observer upon our half of the globe and the earth revolves in perpetual darkness to our unsearching eyes?*

Milton Lomask notes that "many voters decided they had put a fool in the White House when careless reporters quoted the new Chief Executive as speaking of 'light-houses *in* the sky'." (*A Minor Miracle: An Informal History of the National Science Foundation,* NSF pub. 76-18, p. 11, 1976.)

[6] This probably refers to the Pioneer rocket flight. See note [8].

[7] This is the family of Dr. Howard E. Tatel (1913-1957). Tatel was a physicist on the staff of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington and was instrumental in developing their 60 foot reflector at Derwood MD. Many of his ideas were incorporated into the design of the Blaw-Knox 85 foot telescopes. In the First Annual Report of the NRAO (July 1, 1959) Lloyd Berkner notes: "The basic design concept for the Blaw-Knox 85-foot telescope came from the radio astronomy group working with Dr. M.A. Tuve at the Department of Terrestrial Magnetism of the Carnegie Institution of Washington. The late Howard E. Tatel was one of the principal contributors to this design and the NRAO instrument has been officially named as the 'Howard E. Tatel Telescope' of the NRAO."

[8] On October 11, 1958, the U.S. launched the Pioneer spacecraft in a nearly successful attempt to reach the Moon. There is an extensive discussion in *Sky and Telescope*, November 1958, vol. 18, no. 1.

[9] Dr. Otto Struve (1897-1963) was Director of Yerkes Observatory, Editor of The Astrophysical Journal (1932-1947), President of the International Astronomical Union (1952-1955), an AUI Trustee (1957-1959), and the first Director of NRAO 1959-1961.

[10] Robert D. Hall (1922-2010) was an engineer who has been involved in the design of many radio telescopes including the NRAO 300 Foot (see the article by Findlay in this volume, page 145), the NRAO 36 Foot, and the antennas of NASA's Deep Space Network. He was the Project Manager for the Green Bank 100 meter Telescope (GBT) Project (1991-2000).

The 85 Foot Tatel Telescope

by J. Fred Crews

from *The Observer,* March 1964.

The Observatory's oldest telescope—often referred to as "The Work Horse"—is more correctly known as the Howard Tatel 85 Foot Telescope. Howard Tatel, in the early 1950s, working for the Department of Terrestrial Magnetism, Carnegie Institution of Washington, began looking at the problem of developing a Hydrogen Line receiver. Later he became involved in the procurement of a telescope for DTM.

Fred Crews in 1968

The telescopes available at that time used small, high-precision, expensive gearing and Tatel, looking for a new, cheaper way to build a telescope mount, was advised to see Blaw-Knox Co. of Pittsburgh. Tatel developed the concept of the 85 Foot mount with large gearing, with Blaw-Knox doing the dish design itself. Collaborating, Tatel and Blaw-Knox produced the Blaw-Knox telescope in 60-foot and 85-foot diameters. Unfortunately, Tatel died in 1957, about one year before his telescopes started rising not only on this continent, but on others as well.

Our 85 Foot was started in the summer of 1958, but for several reasons was not completed until early February 1959. As a matter of fact, I remember very well the date that the telescope was first used—February 13, 1959 (a Friday, no less). At that time, the only scientists at the Observatory were D. Heeschen, J. W. Findlay, F. Drake, and a freshly arrived H. Hvatum.

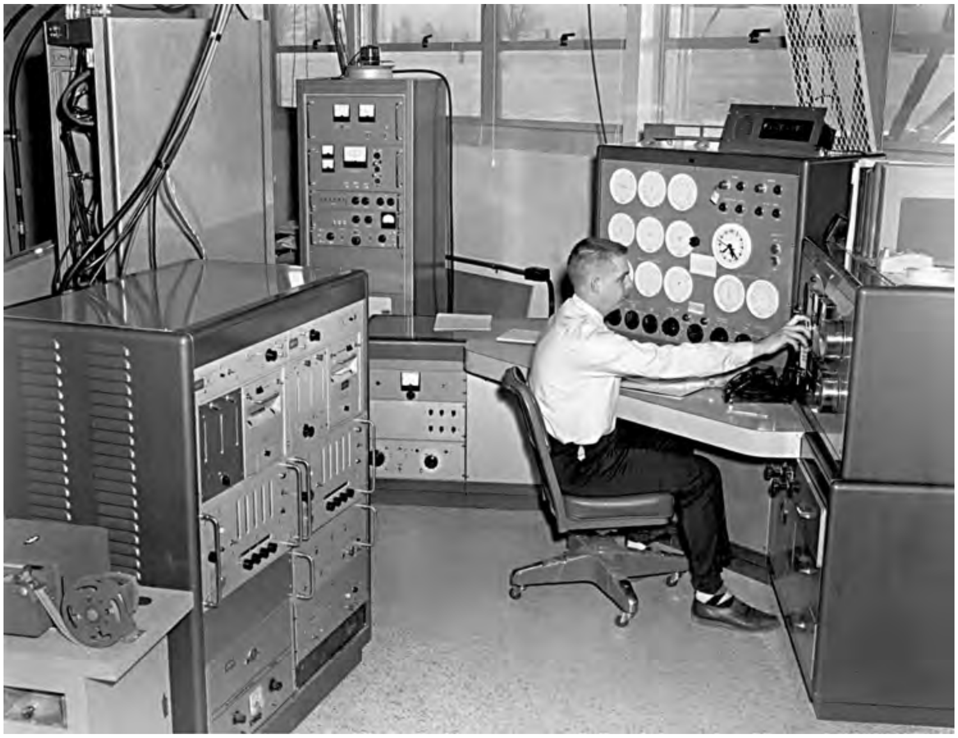
Initially, there were only two operators, *[Bill Meredith and Fred Crews]* who worked 12 hour shifts keeping the telescope going 24 hours a day except for weekends when the scientists did their own observing. Later another operator was added, and then another, bringing the crew to a sufficient number to operate the telescope 24 hours a day, seven days a week, in 8 hour shifts.

Through the years the telescope has observed from the lower frequency of 327 MHz up to 14.5 GHz. In addition to our own staff of scientists, scientists from other institution have used the 85 Foot: G. Westerhout, T. K. Menon, R. Stockhausen, Field, G. Reber, R. Hobbs, R. Lynds, D. Hogg, C. Wade, J. Wanner, S. Weinreb, D. Harris, T. Orhaug, Y. Terzian, M. DeJong, B. Gary, I. Pauliny-Toth, and P. Mezger.

What about its future? Due to the quality of the instrument, no major changes have been made in it, nor are any anticipated. Immediate plans call for it being used as one element of the interferometer along with the new 85-foot telescope. Len Howell, who is chief operator, along with his people, are currently revamping the control room so that both telescopes can be controlled from the existing room. The 85 Foot Tatel Telescope has now been in operation for 5 years. Its life expectancy is 20 years. We are sure that its remaining 15 years will be as fruitful as the first 5.

Editors' note: In the mid-1960s two additional 85 foot telescopes were built at Green Bank on the same design as the Tatel Telescope. These became the Green Bank Interferometer. A description of the construction and use of the Green Bank Interferometer is given in Part IV, and in articles by D. E. Hogg, R. M. Hjellming, and E. B. Fomalont in the book "Radio Interferometry: the Saga and the Science" (NRAO, 2000). The 85 Foot Tatel Telescope continued in operation as part of this instrument until October 2000.

	85-FOOT RADIO TELESCOPE
REFLECTOR:	Paraboloid Diameter 85 feet Surface: 1/8 inch aluminum plates Surface area: 5700 square feet Surface Tolerance: 1/8 inch
FOCUS:	36 feet above surface Carries 500 lbs. of receiving equipment Position relative to paraboloid stable to 1/4 inch Maximum height above ground: 115 feet.
GEAR SECTORS:	Diameter of polar sector: 48 feet Diameter of declination sector: 40 feet Tooth spacing: 2 inches
AXES:	Polar: 23 feet long, 28 inches diameter Declination: 40 feet long, 16 inches diameter Ride on roller bearings
MATERIAL:	Galvanized steel, except reflector which is aluminum
DRIVE RATES:	Scan: 0.05° per minute to 4° per minute Slew at 20° per minute (fixed) Track at solar or sidereal rate
BRAKES:	Spring set, hydraulically released
WIND LOADING:	Precise operation 20 mph Impaired signal at 45 mph Can be driven to stow position at 60 mph Structure stable in any position at 87 mph Structure stable in stow position at 120 mph
TOTAL WEIGHT:	205 tons
SKY COVERAGE:	6 hours E to 6 hours W in hour angle 90° North to 50° South in declination



Bob Vance at the 85 Foot Tatel Telescope controls, 1962. Note the paper tape punch at lower left.

David Heeschen, 1965.

Frank Drake, 1962.

7 A Working and Growing Observatory

Following the award of the management contract, a Director will be selected, the nucleus of a staff will be appointed and the building program should then be able to get under way without delay. Once the new radio observatory is completed and in operation, American radio astronomers will have access to a research facility unequaled anywhere in the world.

Bart J. Bok
Toward a National Radio Observatory
August 7, 1956

The immediate tasks faced by Dr. Lloyd V. Berkner, who in addition to holding the AUI presidency served as Acting Director from November 16, 1956, until July 1, 1959, and a small initial staff, were diverse and formidable. Remodeling the existing farm buildings, recruiting, planning laboratories and other necessary structures, obtaining adequate utility services, and a host of other mundane but important tasks had an inevitable tendency to divert attention from more substantive matters, such as choosing a permanent Director, selection of research tools, and developing a research program. The remoteness of the site was (and still is) a further complication. There is no town with a population over 4000 within 50 miles. However, from small beginnings, NRAO developed rapidly under the leadership of Dr. Berkner, whose interest not only in NRAO as an AUI-management institution but in radio astronomy as a science was profound.

from *"Twenty-five Years of Research Management"*
Associated Universities, Inc., Washington DC, 1972

The First Observations: 1958 – 1959

Selections from the
NRAO Monthly Summary of Activities *

December 8, 1958

[by R. M. Emberson]
85 Foot Telescope

One unplanned event occurred on November 3.... Over the preceding weekend the reflector had been left pointing to the zenith, held in place by tie cables. On the morning of November 3, the principal cable on the north side parted while the work forces were on the ground, getting ready to mount the structure. The reflector rotated about the declination axis and came to rest pointing to the southern horizon. Had the men already resumed the survey work inside the reflector, they would have been thrown to the ground. Some of the minor structural members of the reflector frame were bent when they hit the much larger and stronger parts of the polar shaft assembly; Blaw-Knox elected to replace these with new members rather than to attempt to straighten them. A re-survey of the reflector surface indicated that no measurable deflections had been produced except in two localized areas....

New feed support legs have been installed. Wiring of the drive and control system is in progress, and should be completed in a week.

140 Foot Telescope

Work on the [140 Foot] Telescope is progressing at both the Bliss plant at Canton and at the Green Bank site.... The basement floor has been placed and forms, reinforcing bars, etc., are being readied for the next level of foundation.... At Canton, sub-assemblies of the yoke are being welded together and work on the sections of the polar shaft is proceeding.

Astronomy Department

Dr. Drake has built and put into operation a new receiver for the corner reflector interferometer. The first few weeks of operation of the corner reflectors indicated that there is present a considerable amount of interference, mostly ignition noise, during the daytime. At night and on weekends the site is very quiet at 38 mc/s. To reduce ignition noise, all Observatory vehicles and cars of Observatory personnel will be checked, and suppressors installed where necessary.

Report on a Visit to Europe by J. W. Findlay

Taking an overall view of the places visited, the Dutch must get pride of place as a group. Their program is rather limited but is beautifully led, organized and carried out by the team under Professors Oort and van de Hulst. The reason lies, I think, in the excellent integration of first-class astronomy with first-class electronics.

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* Figures have been interspersed with the text to illustrate and clarify it. They were not part of the original monthly reports.

February 3, 1959

[by R. M. Emberson]

The Hill House, the largest of the farm houses renovated during the program that ended last fall, is now operating as a guest house with meals served on a fixed noon and evening schedule, and breakfast available on a somewhat more flexible basis. The capacity is limited, even for the activities during the winter months, and it is important that any visitors convert from tentative to firm dates, to avoid or minimize inconveniences upon arrival at Green Bank. The lease on Pine Haven Lodge at Minnehaha Springs has been terminated.

The Blaw-Knox Co. completed the basic erection and assembly of the 85 Foot Telescope on January 20. NRAO staff has been busy installing RF, electronic, and related components. A series of tests will be made prior to discussions with Blaw-Knox on final adjustments or corrections.

In anticipation of early availability of the 85 Foot Telescope for research purposes, provisional plans have been made for the late winter and spring months, as follows:

I. Visitor Programs to start at once:

a) H. Hvatum, Chalmers Institute, Gothenburg, Sweden
   1. Isophotes of sun, at 3 cm.
   2. Isophotes of moon as function of phase, at 3 cm.

b) T. K. Menon, University of Pennsylvania
   1. 21 cm study of Cygnus Loop.
   2. 3 cm study of regions around Orion Nebula and NGC 2244.

Hein Hvatum, 1963.

T. K. Menon, 1961.

II. Visitor Programs to start this spring:

a) G. B. Field, Princeton University
   1. Search for intergalactic absorption

b) T. K. Menon
   1. 21 cm study of random velocities in spiral arms.

c) G. Westerhout, Sterrewacht te Leiden
   1. 3 cm galactic plane survey.

Two other visitors, R. Fleischer (RPI) and M. S. Roberts (University of California at Berkeley) have not yet finalized their programs. Both will be at the NRAO during the spring.

III. Staff Programs

a) F. D. Drake
   1. 3 cm surveys of Cygnus X and the Galactic Center.
   2. Search for HI line at 9850 mc/s.
   3. Accurate positions of sources at 3 cm.

a) D. S. Heeschen
   1. Relative intensities of discrete sources at 1200 mc and 1400 mc.
   2. 3 cm and 22 cm study of continuous emission from galaxies.
   3. HI emission from galaxies and clusters of galaxies.

Grote Reber is developing the receiving and test equipment he will use in his long wavelength work. He is also directing rehabilitation of his original 30 foot antenna, which will be set up as a working exhibit of considerable historical interest.

The first Observatory colloquium was given by Grote Reber on "Cosmic Static at Kilometer Wavelengths."

——— March 6, 1959 ———

[by R. M. Emberson]

The most noteworthy item is that the 85 Foot Telescope "went on the air" with both the 21 cm and 3 cm receivers on February 13-15. . . . the potential power of this radio telescope is very great, and is likely to be exceeded soon in this country only by the 140 Foot Telescope.

140 Foot Telescope

Work at Green Bank on the 140 Foot Telescope has proceeded with few weather delays. The foundation walls are now completed above the second floor level. . . . All segments of the 22-foot spherical bearing have been inspected and tested at the Lukens plant both by our own, Bliss and Lukens engineers, and, independently, by Sperry engineers, and all segments were found to be satisfactory; shipment has been made to Bliss and partial assembly has been started.

Receiver Procurement

Reactance, or parametric, amplifiers represent the easiest way to get a possibly major improvement in receiver performance. A reactance amplifier "front-end" is scheduled to be delivered to the Observatory early in March by Microwave Associates. The amplifier is supposed to have a bandwidth of 1 Mc and to be tunable over a range of frequencies around 400 mc/s. The noise figure is to be about 1 db, which, if it is achieved, represents a receiver temperature of about 80 K. Present conventional receivers at this frequency have temperatures about 700 K. . . .

Budget and Fiscal

Discussions between the NSF and NRAO led to the conclusion that specific provision, in the amount of $750,000, should be made for radio astronomy research from space vehicles. The consensus is that the program should start immediately at a modest level, and grow during FY 1960 with an average rate about one-half to one-third of the FY 1961 level. Preliminary discussions with Dr. Keller (NSF) and Dr. Schilling (National Aeronautical and Space Agency) explored the relationship of the NRAO research programs to the NSF and NASA interests, and the possibility of a division between the two agencies of the needed support. A request for supplementary funds for the remainder of FY 1959 is being submitted to the NSF.

May 4, 1959

[by R. M. Emberson]

Observations

Four visitors have started observational programs with the 85 Foot Telescope. T. K. Menon, University of Pennsylvania, spent three weeks at NRAO observing 21 cm line emission from the vicinity of the Cygnus Loop, and 3 cm emission from the Orion Nebula. . . . G. B. Field, Princeton, spent a week at NRAO making observations to determine the atmospheric extinction at 22 cm and 3 cm. This is a preliminary step in a program Field will undertake this summer. M. S. Roberts, University of California at Berkeley, is currently working on a program of observations of globular clusters. H. Hvatum, Gothenburg, is determining the brightness distribution of the moon as a function of lunar phase, at 3 cm wavelength. . . .

Morton Roberts, 1964.

The Observatory staff have also started several observing programs with the 85 Foot Telescope. D. S. Heeschen is doing a two wavelength photometry of discrete sources, and has started observations of both the continuous emission and the hydrogen line emission from galaxies. F. D. Drake is observing the source Sagittarius A at 3 cm and 22 cm wavelengths. . . . With the considerably greater resolution available from the 85 Foot at 3 cm wavelength, Dr. Drake finds that Sgr A is not in fact a single source at all. It resolves into at least four small sources, which, if they are in the region of the galactic center, have diameters of the order of $10 - 20$ parsecs.

The first Fiscal/Accounting Department, 1960. (L to R: Harry Wooddell, Naomi Daniels, Pearl Clarkson)

Data Processing

The first few weeks of observations with the 85 Foot Telescope have brought to light very clearly the inadequacy of present methods of data recording and handling generally in use in radio astronomy. In many cases two hours of observing requires up to 50 hours of purely routine data reduction. To alleviate this situation, the Astronomy and Electronics Departments are developing plans to digitize receiver outputs in a form that can be readily handled by the small digital computer the Observatory now has. Such a procedure should save many hours of data reduction time. One simple digitizer has already been assembled, and has proven to be extremely useful and reliable.

140 Foot Telescope

At Green Bank, the major concrete to cap the foundation was placed April 17-18 (over 500 cubic yards) and work is proceeding rapidly for the next concrete that will be the northward portion of the anvil or deck.

After a lull in shop activities at Canton during March, there has been a burst of activity in April, with work proceeding on the polar shaft, the bearing sphere, the yoke, the polar and declination gear segments, the aluminum reflector structure, and the aluminum surface panels. The limitation on the rate of work still appears to be in the engineering Department at Bliss.

Personnel

Mrs. Phyllis Jackson was employed as secretary to the Chairman of this Department; . . . Mrs. Naomi Daniels employed as Accounting Clerk; Mrs. Estella Lambert employed as Telephone Operator; Mr. Jamie Sheets as Driver-Messenger; and Mr. French Beverage reassigned from the position of Driver-Messenger to the position of Property and Procurement Clerk.

June 5, 1959

[by R. M. Emberson]

Research

Dr. G. Westerhout, of Leiden Observatory, spent 3 weeks at NRAO in May. He had planned to do a survey of the galactic plane at 3.75 cm wavelength. He found, however, that at this wavelength the amplitude and time variations of atmospheric radiation are such as to make survey observations impractical. Variations of the order of 1° K were observed over periods of 4 minutes. Heavy clouds or rain give still larger variations. Because of the implications of this effect on 3 cm work with large telescopes, Dr. Findlay plans to study it in some detail, using the 85 Foot Telescope.

Dr. Roberts, University of California at Berkeley, spent two weeks at NRAO in an attempt to detect 21 cm line emission from globular clusters. His observations have not been completely analyzed yet, but preliminary inspection indicates that there is no signal greater than 0.1° K.

Dr. Drake has observed Jupiter at 3.75 cm, 22 cm, and 75 cm wavelengths. He finds that the spectrum of the cm wave emission from Jupiter is not that of a thermal emitter, as had previously been thought. The apparent brightness temperature of Jupiter increases from approximately 200° K at 3.75 cm to greater than 30,000° at 75 cm. Dr. Drake believes the emission may arise from "Van Allen belts" in Jupiter's atmosphere.

Visitors

Visitors to the Observatory this month included: Professor J.H. Oort, Leiden Observatory; K.S. Strand and O. Franz, U.S. Naval Observatory; Dr. and Mrs. E. Raimond, The Netherlands; David Hogg, David Dunlap Observatory. . . .

140 Foot Telescope

At Canton steady progress has been made on the fabrication of components for the aluminum structure of the reflector . . . and the interior bracing plates for the spherical bearing have been cut and are partially assembled. Additional concrete has been placed for the foundation at Green Bank and exterior utilities (electric power conduits, drain tiles, etc.) have been completed or are well along. The progress reflects a desire at the working level to get the job done; even more progress would have been possible were it not for Bliss' decision to hold up certain items (and to withhold information from the site construction subcontractor) pending resolution of an issue over the interpretation of portions of the basic contract between Bliss and AUI.

The foundation of the 140 Foot Telescope.

Site Management

The Hannah House is being readied for occupancy by six undergraduate students and two graduate students during the summer months. Bicycles are being procured to provide means of transportation for students on the site.

At the present time three of the original owners of property which formed a part of the total site are still in residence. Two of these families will vacate during the summer or early fall, and the last family will vacate upon final disposition of their case now before the courts.

Other

Dr. Struve attended the usual monthly Departmental Chairmen meeting on May 20, 1959. Mrs. Struve accompanied him east and visited the Observatory for the first time. Needless to say, the staff was delighted to have an opportunity to greet both Dr. and Mrs. Struve for the first time since the announcement of his appointment as the Observatory's first Director.

The Hannah House in 1962.

July 2, 1959

[by R. M. Emberson]

Astronomy Department

The 85 Foot Telescope was in almost full time operation during June, with visitors from four institutions engaged in observing programs, in addition to the staff. Programs in progress include observations of centimeter-wave planetary emission; studies of ionized and neutral hydrogen in other galaxies; and studies of the spectra of discrete sources. A 440 mc/s receiver was assembled by the RED [Research Equipment Development] Department to permit an extension of observations of Jupiter to these longer wavelengths. The temperature thus obtained is consistent with work previously reported.

Dr. D. E. Osterbrock, University of Wisconsin, spent a week at Green Bank setting up a program of observations of planetary nebulae at 3.75 and 22 cm wavelengths. These radio observations, together with optical data, may give electron densities and rough values for electron temperatures in several planetary nebulae.

Personnel

The following tabulation lists [some] appointments made during the month of June.

T. K. Menon, University of Pennsylvania, Visiting Associate Astronomer
G. Field, Princeton University, Visiting Associate Astronomer
D. Osterbrock, University of Wisconsin, Research Collaborator
R. Fleischer, Rensselaer Polytechnic Institute, Guest Astronomer
R. Stockhausen, University of Wisconsin, Research assistant
George Grove, Telescope Operator.

Administrative Department

Because of the rapid growth in the Observatory staff in the past three months, a get-acquainted party was arranged on June 25. Over 80 people attended the party, and a good time was had by all as a result of the care and hard work which went into the arrangements. Some of us were introduced for the first time to the rigors of West Virginia square dancing. Although there have been rumors of protesting muscles, no casualties have been reported as of this date.

Public interest and visitation to the Observatory has been increasing steadily. During the past month or six weeks, we have been visited by groups of college, high school, and junior high school science students, plus a steady stream of individual visitors. This activity is taking a disproportionate amount of the time of our small and already over-worked professional staff.

140 Foot Telescope

At Green Bank work continued on the foundations and the north side is complete to the top deck level. . . . At Canton assemblies have been made of all portions of the polar shaft except the extreme north end. The interior bracing for the 22-foot spherical section is being welded but with some difficulty, because of the thickness of the plates (up to six inches) and the complex pattern of their arrangement.

August 5, 1959

[by R. M. Emberson]

This month has been remarkable for the visitor activity. Visiting scientists and the undergraduate training program have filled the present housing capacity to over-flowing. Public visitors have increased to the point of seriously interfering with the work of the Observatory.

Astronomy Department

The 85 Foot Telescope was in operation almost continuously during the month on a variety of visitor and staff programs. A 440 mc. feed, designed by Dr. Hvatum, has been permanently installed at the focus, and it is now possible to simultaneously observe at 3 frequencies – 440 mc., 1400 mc., and 8000 mc. Receiver outputs have been digitized, and are punched on paper tapes which can be fed directly into the IBM 610 computer. The resultant saving in data reduction time is very great.

Receiver Development

Dr. Menon from the University of Pennsylvania has a program to use the 85 Foot Telescope in an attempt to measure the Zeeman splitting of the hydrogen line as seen in absorption due to a possible magnetic field in interstellar space. This experiment requires a receiver of rather specialized design and the use of a feed capable of accepting either right-handed or left-handed circularly polarized waves. Such a feed has been designed and built by Dr. Jasik.

Major Design and Construction Department

The foundation work for the 140 Foot Telescope has been completed up to the deck level.... At Canton, progress continues on the fabrication of the metal parts of the Telescope. Revised procedures have been devised which, it is hoped, will speed up the task of welding together the spherical bearing to be placed at the north end of the polar shaft. Work on the polar shaft is essentially completed, and Bliss now plans to ship the shaft with the spherical section on approximately August 10.

September 22, 1959

[by R. M. Emberson]

Site Protection

There are two main subjects which are being discussed with the Federal Aviation Agency. One is the possibility of getting a restricted flying zone around the Observatory. The second is the possible interference from a projected air route which is planned to fly across the radio quiet zone, about 20 miles to the south of the Observatory.

140 Foot Telescope

Work on the 140 Foot Telescope has been drastically curtailed. Darin & Armstrong is within 200 cubic yards of finishing the top of the foundation pier, which must await Bliss engineering of the telescope supports so that a proper pattern of bolts may be placed in the top of the pier. The Bliss plant at Canton was closed by a strike on September 1. Two sections of the polar shaft were shipped on August 29.

October 5, 1959

[by R. M. Emberson]

Astronomy Department

Dr. Field has completed his observations of Cygnus A. He obtained observations at 5 megacycle intervals from 1250 megacycles to 1450 megacycles. He will use this data in an attempt to detect, or put a meaningful upper limit to, the absorption by neutral intergalactic hydrogen between our galaxy and the Cygnus A source.

Dr. Drake has completed observations of the region of the Cygnus X source at 3.75 cm wavelength and of the planet Venus at 3.75 cm wavelength. The most intense source of emission in the Cygnus X region appears to be associated with the super-giant star, Gamma Cygni.

The two parts of the first 140 Foot polar shaft, loaded at the E. W. Bliss Company for shipment to Green Bank. *[Bliss photo courtesy M. Popovich]*

Dr. Heeschen has essentially completed his observations at 4 wavelengths of the stronger discrete sources.*

Buildings

Special efforts have been made to prepare the Residence Hall and Laboratory for occupancy.... Mr. Callender has given daily attention to the completion of the Residence Hall and Laboratory, and it now appears that both buildings will be ready before the National Science Board and the AUI Trustees come to the Observatory on October 13.

140 Foot Telescope

Two sections of the [140 Foot] polar shaft, shipped by Bliss on August 29, reached the rail terminal 12 miles north of Green Bank, and have been moved to the site by tractors and trailer.

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* Editors' note: multi-frequency observations with the Tatel Telescope were published by Heeschen in ApJ **133,** 322 (1961).

——— November 12, 1959 ———

[by R. M. Emberson]

General

The move of the Laboratory and offices to the new Laboratory building and the visit of the National Science Board and AUI Trustees took place in the early part of October.

140 Foot Telescope

At the Canton shops, progress continued on the aluminum super-structure for the 140 Foot reflector. The yoke is being fitted together and lugs will be added to facilitate re-assembly in the field. Welding continues on the stiffener diaphragms for the spherical segments of the polar shaft. On October 26-28, conferences with Bliss engineers accomplished a great deal with respect to welding procedures for use at Green Bank, and many design details including ladders and walkways, the declination bearings, and the focal feed support system. No acceptable design is yet available for the main bearing support.

——— December 9, 1959 ———

[by R. M. Emberson]

Astronomy Department

Dr. C. R. Lynds joined the Observatory staff at the beginning of the month. Dr. Lynds received his degree from the University of California at Berkeley, and then spent a year at the Dominion Astrophysical Observatory as a National Research Council of Canada postdoctoral fellow. Mrs. Lynds, who is also an astronomer, has taken on the job of organizing and expanding the library.

Dr. Drake's projected search for coherent signals from life on other planets [*"Project Ozma"*] has received much recent publicity. Dr. Drake has been planning this program for several years, and work on the equipment—which will also be used for another, more conventional, program—was begun last spring. The recent publicity has gotten somewhat out of hand. The odds against success in this initial attempt are obviously extremely high. However, it is well worth trying with presently available equipment. Dr. Drake has written an article about the project for the December issue of *Sky and Telescope**.

——— January 8, 1960 ———

140 Foot Telescope [by R. M. Emberson]

Work at Green Bank has continued on the polar shaft.... Preparations are being completed for welding to start on the girth joint between the two hugh [sic] sections as shipped from the Bliss plant.

At Canton, progress has continued on aluminum components for the telescope. Except for this, little other work has gone forward in the shops. Problem areas in the diaphragm sections for the sphere were cut out and repair procedures were devised by installing new replacement metal. The yoke fit-up ran into difficulties....

* *Sky and Telescope,* **19,** *No.3, p.140, 1960*

Comments by the Director [Otto Struve]

I wish to express my gratitude to the NSF for having provided the additional sum of $50,000 to our 1960 budget in order to start at once a good scientific library at the NRAO. Dr. Beverly Lynds has been employed since October as half-time librarian in the Astronomy Department, and has already placed orders for several hundred books....

February 8, 1960

Impetus for a New, Large Telescope

Astronomy Department [by D. S. Heeschen]

Listed below are some of the programs contemplated for the proposed 300 Foot antenna. The programs are based on the assumption of the following approximate antenna characteristics: beamwidth of $9'$ at 20 cm, $18'$ at 40 cm, $27'$ at 60 cm; gain about 12.5 times 85 Foot gain and 4.5 times 140 Foot gain at a given wavelength; wavelength range of 20 cm and longer (the Telescope will probably be operable to 10 cm with reduced gain — down perhaps a factor of 3 at 10 cm. The resolution at 10 cm should not be seriously deteriorated, however, and should be about $6'$); sky coverage $\pm 15^\circ$ from some fixed point, probably the zenith.

1. Brightness Distribution in Galaxies.

Several tens of galaxies have been detected at radio wavelengths. Some of them are known to have angular sizes at radio wavelengths that are much larger than the optical size, and in a few cases the radio brightness distributions have been measured in some detail. With the 300 Foot Telescope, radio diameters and brightness distributions should be obtainable for many more objects.... This program would be done at several wavelengths, to provide spectral information and to separate out thermal and non-thermal components of the radio emission.

2. Radio Intensities and Spectra of Galaxies

Observations at several wavelengths with the 85 Foot suggest that relations may exist between the spectra and absolute luminosities of galaxies, somewhat analogously to the stellar color-magnitude and color-color diagrams of optical astronomy.... this type of study requires high quality data from a large number of sources, and acquiring this data would be a major job for the 300 Foot.

3. Polarization Studies

Very little work has been done on the polarization of continuous emission from radio sources.... with the 300 Foot, attempts will be made to measure polarization throughout the extended galactic and extragalactic radio sources.

4. Radio Emission from Stars

No star, other than the Sun, has yet been detected as a source of radio emission. It appears possible, however, that certain types of stars, such as flare stars, U Gem type variables and perhaps others, may ultimately be detectable at radio wavelengths.... with the 300 Foot detection is probable. Detection of this type of object will require a long, time-consuming patrol of the likely stars, and is not the type of program that can be easily fit into the schedule of a versatile, expensive instrument like the 140 Foot. The 300 Foot, on the other hand, will be well suited for this type of program, and will be used to monitor certain stars.

5. Variable Radio Sources

Whether any of the known radio sources are variable has not been determined. A search for variability ... is again well-suited to the 300 Foot. This, and the previous program, will keep the 300 Foot well occupied for several years, after shorter programs have been completed, and other telescopes become available.

6. 21-cm Line Studies

The 300 Foot will also be used for some 21-cm line studies of galaxies, and of particular problems in the Galaxy requiring higher resolution than that obtainable with the 140 Foot.

February 8, 1960

140 Foot Telescope [by R. M. Emberson]

At Green Bank, hand welding progressed steadily during January on the inside of the girth weld for the polar shaft and about $1\frac{5}{8}$ inches of weld has been successfully placed, free of any cracks or other defects.

At Canton, work on the aluminum superstructure for the reflector has continued. Sub-assemblies as large as can be shipped (limited by dimensions, not by weight) are being put together. The two halves of the stiffener diaphragms for the spherical section of the polar shaft have been repaired as needed to eliminate cracks that developed at some joints from time to time as the fabrication proceeded. The halves are now being fitted (trimmed or built-up) by template in preparation for the next step of attaching the 4-inch thick orange peel segments.

The Zeeman/Ozma Receiver [by J. W. Findlay]

Mr. R. W. Meadows, of the Radio Research Station, Slough, England, left the Observatory on January 20th after a three months stay. During this time, in addition to joining in a variety of Observatory activities, he continued the development of the receiver which is planned for use in the Zeeman experiment by Dr. Menon and in the Ozma experiment. This receiver, except for the front end amplifier, is now completed and ready for testing.

The receiver is intended to fulfill a twofold function. It will be used to attempt to detect the Zeeman splitting of the absorption line of neutral hydrogen. With some changes in the feed system, and in the filter system, the receiver will be used for the Ozma experiment.... The experiment to detect the Zeeman split consists in scanning a receiver whose pass-band is very narrow across the absorption line and during this process switching the receiver continually between the two opposite senses of circular polarization.... When the receiver is used for the Ozma experiment, the front end is switched between two horns — in one of which will be the signal from the source and in the other of which will be signals from the adjacent sky background.

Comments by the Director [Otto Struve]

With the arrival of Dr. Campbell Wade at the Observatory, the astronomy department now consists of five astronomers (counting the Director, but not the telescope operators), three of whom received their training at the Harvard College Observatory. It is appropriate to acknowledge the great service to American Science which Harvard University and, especially, Professor Bart J. Bok have rendered in their early recognition of the importance of radio astronomy, and in the training of several able research workers in this field. We expect that Dr. S. von Hoerner of Heidelberg University will join the staff in May as a theoretical astrophysicist. Efforts are under way to bring to the NRAO several additional radio astronomers.

Cam Wade, 1971.

Among the scientific developments, the following deserves special mention: The Observatory was visited by Dr. G. Pettingill and Dr. P. E. Green of the Lincoln Laboratories, for the purpose of discussing with us the possibility of mounting on the 140 Foot Telescope a powerful transmitter that would enable them to repeat and improve the recording of radar echoes from Venus. Unfortunately, this experiment can be carried out only when Venus ... is near inferior conjunction; in all probability the 140 Foot Telescope will not be completed in time for the next conjunction on April 11, 1961 ... although no radar astronomy has been planned at the NRAO, the ingenuity of the Lincoln Laboratory physicists is of great interest to us.

——— March 2, 1960 ———

Astronomy Department [by D. S. Heeschen]

David Hogg, a visitor from the David Dunlap Observatory, is observing HII regions IC 1795, IC 1805, and IC 1848, at 22 cm and 3.75 cm. He will combine his radio results with optical observations of the nebular lines to obtain models of the electron temperature and density distribution in the nebulae. This work will form part of Hogg's Ph.D. thesis at the University of Toronto.

David Hogg, 1969.

Dr. Lynds is also observing HII regions, and has obtained data on the 22 cm emission from several regions of interest. He is particularly interested in studying the distribution of neutral hydrogen in the vicinity of ionized clouds. Dr. Wade is making a detailed study of the brightness distribution of the peculiar galaxy NGC 5128, at 22 cm and 3.75 cm.... Heeschen is making a search for radio emission from the types of normal galaxies which have not yet been detected as radio sources, namely E, S0, SB, and Sa galaxies.

140 Foot Telescope [by R. M. Emberson]

The girth weld of the polar shaft was completed during the last week of the month. After templates have been fitted to the shaft ... there will be no work remaining at Green Bank and the contractor plans to shut down operations until about April 1.

At Canton there is some engineering activity, but work in the shops was barely perceptible during the month. Steps are being taken to determine Bliss' intention and plans for satisfactory completion of the work.

Personnel [by F. J. Callender]

During the month of February, Dr. Wade joined the staff of the Observatory as a member of the Astronomy Department. Dr. Wade received his Doctoral Degree from Harvard University and comes to the Observatory after approximately two years of research in Australia. Mrs. Wade is serving as a part-time research assistant to the Director. Mr. David Hogg of Canada is spending several months at the Observatory as a guest research assistant in the Astronomy Department.

Plant Maintenance [by F. J. Callender]

Topographical surveys are underway on the sites for the Director's Residence, the proposed site of the 300 Foot radio telescope, and that part of the site on which new residential housing will probably be located in Fiscal Year 1961. . . .

On Thursday and Friday, the 18th and 19th of February, the heaviest snow-fall of the winter was experienced. On the 19th there was a power outage of approximately 20 hours. This completely eliminated all heat and light. Furthermore, during this time, high winds made every road on the Observatory site impassable. During this emergency the maintenance staff under the direction of Mr. Riffe acquitted themselves extremely well. An emergency generator was set up in one of the renovated homes on the site and several families, particularly with small children, were evacuated to that location. By this device they were able to ride out the storm in comparative comfort.

——— April 8, 1960 ———

Astronomy Department [by D. S. Heeschen]

The Observatory's computing needs have outgrown the capabilities of the IBM 610 computer considerably more rapidly than was originally anticipated. The 610 computer was leased about a year ago, and by mid-winter it was apparent that a somewhat faster and more versatile machine would soon be needed. A Bendix G15-D computer was obtained in March, and is now in operation. This machine is considerably faster and more versatile than is the 610, and programming is only slightly more difficult.

140 Foot Telescope [by R. M. Emberson]

Work at Green Bank is shut down, pending delivery of fabricated telescope components from Canton. In the Bliss shops the tempo of work has increased. The welding of the spherical surface plates to the internal diaphragms is proceeding on a three-shift basis, but, at best, is a slow task. . . . A conference between Dr. Berkner and Mr. Potter, President of Bliss, resulted in a promise to schedule all work out of the Bliss plant before the end of 1960.

May 6, 1960

Astronomy Department [by D. S. Heeschen]

During April the 85 Foot Telescope was used for the Ozma and Zeeman experiments. The Ozma experiment has had negative results thus far. It may continue for a few weeks, but will then be discontinued for an indefinite period. The observations to detect Zeeman splitting in the absorption lines from Cas A are completed, but the results are not yet known. There is an instrumental effect which may, if it cannot be fully sorted out, limit the sensitivity of the experiment.

A regular exchange of visits between the staff of the KPNO and NRAO is planned to discuss programs and problems of mutual interest and to develop cooperative research programs.

Research Equipment Development [by J. W. Findlay]

A regular series of measurements with the calibration horn was started at the end of March. Bad weather had prevented earlier use of the horn. Snow or rain prevents observations since the gain of the horn may be reduced and since there is a very pronounced increase of horn antenna temperature when rain is falling into it.

140 Foot Telescope [by R. M. Emberson]

Construction work at Green Bank was shut down throughout the month of April. [At Canton, Ohio] welding on the polar shaft sphere proceeded without incident. . . .

excerpts from

Telescope Operator's Manual (1963)

by Fred Crews and Arnold Davidson

The telescope operator must be aware of his importance in making observations and how his work will affect both the quality and quantity of the final data. Sloppy observing, although not always easily spotted, can completely destroy a program. . . .

[The telescope operator] should be thoroughly familiar with all observing techniques, corrections, and areas of telescope safety. One thing is mandatory, and that is that we must be internally consistent. Therefore, the telescope operator must be dictatorial in exercising control over all aspects of the operation. . . .

Methods of observing change from time to time and require that the telescope operator acquaint himself with new techniques as they are developed. . . .

Safety — Winds of 25 MPH begin to impair the accuracy of the program data by distortion of the telescope figure, while winds of 35 MPH and above threaten the actual safety of the structure. . . . The point to remember is that while data are important, it should never be allowed to jeopardize the safety of the telescope.

An accumulation of 4-6 inches of snow would constitute a safety hazard, and in order to prevent this it has become accepted policy in times of heavy snowfall to empty the dish at frequent intervals . . . this entails tilting it over and allowing the accumulation to drop out. If this fails to dislodge the snow, then a bit of judicious hammering with a rubber mallet about the lower edge of the telescope often turns the trick.

Quality and Quantity of Data — The astronomer depends entirely on the telescope operator for accurate and reliable data. Mistakes such as positional errors, erroneous corrections, receiver malfunctions and others that may crop up should be noted and fully explained on the observing log. Lengthy program interruptions are to be avoided and any major changes should be cleared with the astronomer, preferably in advance. . . . quality should never be sacrificed for quantity; a small amount of good data is of much more value than a large amount of marginal data.

The Horn under construction in 1959.

Warren Wooddell in the completed Horn.

The Calibration Horn

From an unsigned article in the *Observer*, March 29, 1963.

The Calibration Horn, better known as the Little Big Horn, was designed by Dr. John Findlay and constructed by the Plant Maintenance Division in 1959.

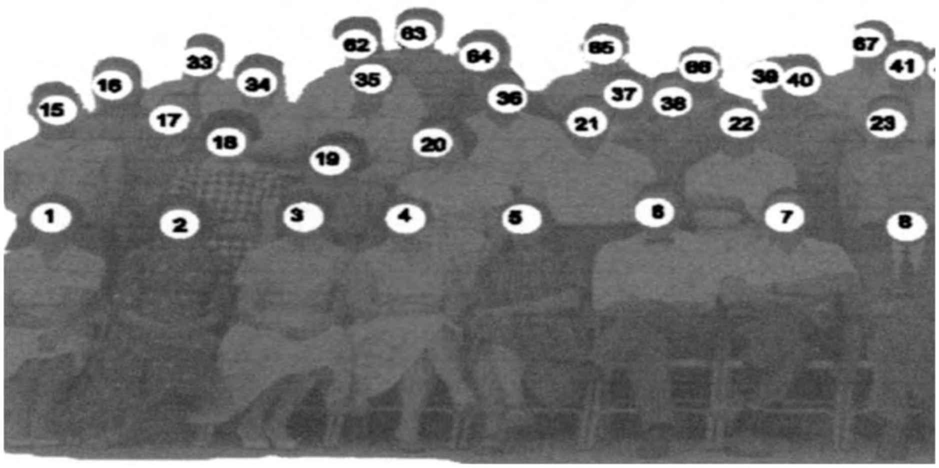
The horn is 120 feet long and the aperture at the upper end is a rectangle 13 feet by 17 1/2 feet, while at the lower end it is approximately 3 inches by 6 inches (standard L-band waveguide). The design of the horn is such that the collected power can be calculated precisely from its dimensions, therefore providing other telescopes with a standard of measurement. It is inclined at an angle of 30° and fixed in a position so that it can observe and measure accurately, once each day, the intensity of the incoming radio waves from the radio source in Cassiopeia, Cas A. This source was chosen because it is the strongest source in the sky, with the exception of the Sun, is visible throughout the entire Northern Hemisphere, and is believed to be extremely constant from year to year. It completes its transit through the horn in approximately 50 minutes.

While this is the primary purpose of the horn, other information, such as the absolute knowledge of the spectrum of Cas A over a frequency band from 1400 Mc down to 900 Mc, a check that the flux from Cas A is invariant with time, and a measurement of the absolute temperature of the sky background, will also be of great value.

The first observations were made in October 1959 at 21 cm (1400 Mc) and have been continued, with the exception of a couple of months each year. The horn is not covered by a radome, and therefore our weather hinders observations. Results have shown that Cas A has an average flux density of approximately 250×10^{-25} watts per centimeter per cycle per second, or a temperature of $\approx 9^\circ$ at 1400 Mc. [See Findlay, Hvatum, & Waltman 1965, ApJ, **141,** 873.] The Little Big Horn is maintained and operated by the Electronics Division under the supervision of Dr. Hein Hvatum. The present operator is Mr. James Oliver.

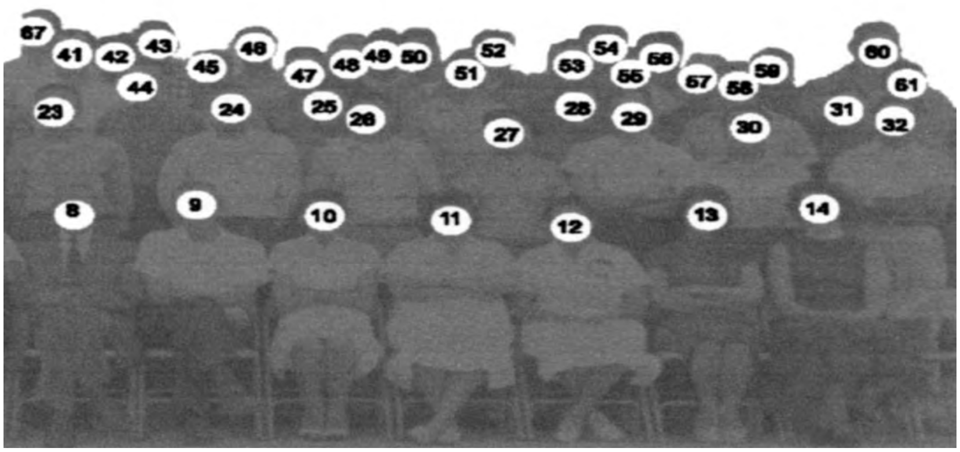
The Completed Horn, 1967.

NRAO Employees in May 1960.



1. Phyllis Jackson
2. Beverly Lynds
3. Naomi Daniels
4. Beaty Sheets
5. Harry Wooddell
6. Frank Callender
7. John Findlay
8. Otto Struve
9. Dave Heeschen
10. Virginia Irvine
11. Nellie Arbogast
12. Verna Tracy
13. Estella Lambert
14. Mary Jane Wade
15. French Beverage
16. Bill Kuhlken
17. James F. Wanner
18. Margaret Hurley
19. Ellen Chan
20. Ellen Gundermann
21. Sebastian von Hoerner
22. Mike Waslo
23. Roger Lynds
24. Frank Drake
25. Warren Wooddell
26. Arnold Davidson
27. Lyndell Brooks
28. Jim Elliott
29. Ed Monahan
30. Kwan-Yu Chen
31. Dick Hiner
32. Eugene Capriotti
33. Ted Riffe

NRAO Employees in May 1960 (cont)



34. Charles Phillips
35. Mike Belton
36. Bob Elliott
37. Carl Adler, Jr.
38. Bill Waltman
39. Lillian Ness
40. Hein Hvatum
41. Peter Vandervoort
42. T. K. Menon
43. Jamie Sheets
44. Don Bodner
45. Joe Carter
46. Cam Wade
47. Dewey Ross
48. Merritt Gum
49. Marvin Taylor
50. Basil Gum
51. Jim McLaughlin
52. Bob Aldridge
53. Dewey Pritt
54. Maxie Gum
55. Clifford McLaughlin
56. Fred Cole
57. George Grove
58. John Dickel
59. Forrest Ervin
60. Troy Lusk
61. Bedford Taylor
62. Bill Gandrud
63. Bill Brundage
64. David Brown
65. Don Hobbs
66. Fred Crews
67. Lewis Hobbs

L to R: John Findlay, Harry Wooddell, Dave Heeschen, Beaty Sheets, Fred Cole. Employees since 1958 or earlier at their 10-year pin party in 1968.

A group photograph at the 10-year pin party, 1968.
L to R: Wally Oref, Fred Crews, Omar Bowyer, Paul Devlin (behind Omar), Hein Hvatum, Roger Lynds (behind Hein), Elsie Hungerbuhler, Tom Williams, John Hungerbuhler (in front of Tom), Mrs. Fred Cole, Don Hovatter, Jamie Sheets, Marguerite Crews, Rose Bowyer, Mrs Powell, Bill Powell, Mrs. Williams.

8 1960-1962: Towards a Very Large Antenna

Editors' note: The Monthly reports between May 1960 and May 1961 show the development of NRAO as an institution and the increased use of the 85 Foot Telescope, but also the continued frustration at the lack of a large telescope. The period ends with the announcement of the signing of a contract for construction of the 300 Foot Telescope. The 140 Foot construction still lagged.

Excerpts from the NRAO Monthly Summary of Activities

—— June 2, 1960 ——

The NRAO and a Very Large Antenna [by Findlay and Heeschen]

The staff of the NRAO desires strongly that a very large antenna should be designed and built at Green Bank by a group of workers on the staff of the Observatory.

The way this should be done is also clear to NRAO. A group of capable and enthusiastic scientists and engineers will grow at Green Bank over the next year to be ready to plan and carry out the VLA* project. This group already exists as a nucleus of men who are ready to spend the time necessary to complete a project of the size of the VLA. The group at present has started work on the design of an antenna system of 300 feet effective aperture good to 21 cm wavelength and of limited sky cover. This antenna is scheduled for completion within one year at a cost of $300,000. This is a challenging project. It will provide a very valuable research instrument very quickly for radio astronomy. The project will also demonstrate the ability of the NRAO group to carry out a difficult task in a short time at a low cost.

By the time the 300 Foot is completed (July 1961) the NRAO will have a tested group of men capable of planning and building the VLA. This group will be led by a scientist of ability and enthusiasm who will be directly responsible to the Director of NRAO. The group will, if financial support for the VLA is supplied in FY 1962 (i.e., by July 1961), be expanded during the first year of the VLA project to 10 to 15 scientists and engineers, with a suitable supporting staff of draftsmen and technicians.

The first task of the VLA group will be to decide what form the antenna will take. . . . the decision as to the type of VLA to build will be based on a real and careful evaluation of the cost, complexity, site needs and value to radio astronomy of the competing designs. The choice of the best design concept will be of vital importance and when made will be final. . . .

The radio astronomers at the NRAO—Drake, Heeschen, Lynds, Wade— believe that a very large antenna is urgently needed at the Observatory. This belief is based both on general considerations of the instrumental needs in radio astronomy and also, more specifically, on the needs which have arisen as a result of current

* *Editors' note: "VLA" here means "Very Large Antenna"*

research activities at the Observatory. These activities, in several broad areas of astronomical research, have already pushed the 85 Foot Telescope to the limit of its usefulness. When the 140 Foot and 300 Foot Telescopes become available the problems can, in some cases, be taken one step further, but it is clear that here too the limits of instrumental capabilities will be reached short of satisfactory solutions of the problems.

Drake's study of the Galactic Center at 3.75 cm wavelength and the Leiden hydrogen line observations have shown the existence of very energetic phenomena which may provide much of the driving force for the dynamics of the Galaxy and be an important feature of the evolution of the Galaxy. To investigate these phenomena further, Drake wants to make Hydrogen line and continuum observations of the nuclear regions of other, nearby, galaxies. To do this with resolution comparable to that used in the studies of our Galaxy will require an antenna much larger than the 140 Foot or 300 Foot.

The investigations of the spectra of extragalactic radio sources that have been made with the 85 Foot by Heeschen show that precision observations may yield much information about the nature of galaxies.... He would like to measure the relative intensity of several hundred sources, at several wavelengths, to an accuracy of better than 1%. To do this will require a very large antenna of high angular resolution....

[M]easurements of polarization in different regions of nearby galaxies might be possible with a very large antenna, and if so would be of great value. Wade is concerned with the radio brightness distribution in galaxies and he too desires the resolution of an instrument considerably larger than the 140 Foot or 300 Foot.

The Pierce Committee* has selected characteristics for a VLA on the basis of a single problem of major importance— the study of the distribution of hydrogen in M31. We agree that this is an extremely important problem and we heartily endorse the recommendations of the committee. The antenna needed to study the distribution of hydrogen in M31 is also the antenna needed for the problems described above, and the scientists at NRAO wish to build such an antenna at the Observatory as soon as possible.

The programs outlined above are those which the NRAO staff recognizes now to be of direct personal interest to staff members. Many radio astronomers in the U.S.A. have equally pressing and important pieces of research to do on the very large antenna ... we recognize very naturally the responsibilities the NRAO will have in making sure that all visiting radio astronomers have the use of the antenna. The NRAO was set up and is operated to provide for all radio astronomers the very large instruments they need. We hope very strongly that the NRAO will be the location for a very large antenna, that the NRAO staff will build and operate it, and that all radio astronomers will use it and benefit by it.

J. W. Findlay, Chairman,
Research Equipment Development Dept.

D.S. Heeschen, Chairman,
Astronomy Department.

* *Advisory Panel on Radio Telescopes of the National Science Foundation; see* ApJ Vol. **134,** p. 927, 1961.

Personnel [by F. J. Callender]

Dr. Sebastian Von Hoerner of Heidelberg, Germany, joined the staff during the month of May as a Post Doctoral Research Associate. Dr. Von Hoerner is a theoretical astronomer and will spend one year in residence at Green Bank. Mr. Troy Henderson joined the staff on June 1, 1960 as a telescope operator.

140 Foot Telescope [by R. M. Emberson]

On May 24-25, Mr. Burchill (AUI Controller) and Dr. Emberson were in Canton, at which time the following general information was obtained: (a) Bliss plans to ship the main bearing support girder and the spherical segments for the polar shaft on or about June 15; (b) the yoke will be shipped in the week following the sphere shipment; (c) Darin and Armstrong will reopen the Green Bank office when the spherical segments are shipped from Canton and will start requalifying welders in order to be ready when the first shipment arrives. . . .

——— June 28, 1960 ———

Astronomy Department [by Heeschen and Drake]

The resolution of the 85 Foot Telescope is inadequate for 440 Mc observations of any but the more intense sources. This aspect of the observational program has therefore been discontinued. In its stead, observations will be made later this year at 10 cm wavelength.

Observations connected with the Ozma Project have been made daily for several weeks, and it is anticipated that the first phase of the Ozma Project will be completed about July 1. As a by-product of this program, it has been found possible to voltage-tune the parametric amplifier being used, greatly facilitating the delicate adjustment of the device. . . . It has already become apparent in the Ozma Project that the type of radiation being searched for could be much more easily detected if more refined information handling techniques were employed. Theoretical studies of possible approaches to this problem are being made.

For general information, a list of the colloquia given at the Observatory since the first of the year is included here:

Jan. 6 — J. W. Findlay, “Reactance Amplifiers”
Feb. 5 — F. D. Drake, “The Galactic Center”
Mar. 3 — M. Rudkjöbing, “Metallic Line Stars”
Mar. 4 — D. S. Heeschen, “Radio Observations of Discrete Sources at Four Wavelengths”
Apr. 6 — V. V. Vitkevich, “Current Radio Astronomy Activities in the USSR”
May 24 — D. S. Heeschen, “Radio Frequency Observations of Galaxies”
June 8 — Grote Reber, “A Trip to MacQuarie Island”
June 15 – S. von Hoerner, “A Model of the Orion Nebula”
June 28 — J. Sahade, “Recent Work on Spectroscopic Binamics [sic]”

140 Foot Telescope [by R. M. Emberson]

At Canton, welding of the spherical segments for the main polar shaft bearing was completed, and the pieces have been treated in the stress-relieving furnace. The two pieces were then fitted together and in the rough were found to have held very well to the desired spherical shape. . . .

During the initial trimming of the interior diaphragms, some small cracks were discovered at the inside edges. These discoveries led to a thorough review of the situation and a series of tests have been started on all welds. . . .

Food and Housing Service [by F. J. Callender]

The influx of summer visiting scientists and students, coupled with the continuing steady growth in the permanent staff, is taxing the Observatory's housing facilities to the utmost. All of the usable dwellings on the site are occupied. This includes not only those residences which were renovated but also those which must be pressed into service for the summer. These latter are not truly adequate, but the Observatory has no choice. For instance, Dr. Fleischer and his family will live in a house which has no heat whatsoever. This is of some discomfort in a location where night temperature will occasionally be in the low fifties or high forties. Further, this house has no telephone and can only be reached by driving across more than a quarter mile of open meadow and pasture. No significant amount of renovation has been possible on the quarters used for male undergraduate students.

In the residence hall, eighteen men are now or will be occupying thirteen rooms. Three other rooms are held for overnight transients or short term visitors. In order that the Observatory may be able to accommodate Mr. Lovell from Jodrell Bank, England, during his visit to the Observatory in July, together with the rest of his party which will probably include Mr. Glennan and Mr. Dryden of NASA, it will be necessary to transport and house several of the present occupants in a motel thirty miles away during this important visit.

The Hon. Cecil Underwood, Governor of West Virginia, visited the Observatory on Thursday, June 9, 1960.

One hundred members of the West Virginia Press Association visited the Observatory on Saturday, June 11, 1960.

Comments by the Director [O. Struve]

I am pleased to announce that Messrs. F. Haddock and A. Sandage have accepted appointments as senior consultants at the NRAO in 1960-61, with the stipulation that they will spend at least one week at the Observatory during the year. The third nominee, Edward F. McClain of the NRL, has verbally indicated that he will also accept if the Naval authorities allow him to do so. . . .

It is appropriate here to record, on behalf of the entire staff of the NRAO, our feelings of shock and sorrow over the sudden death of Dr. Carl K. Seyfert, Trustee-at-large of AUI. As an astronomer, Mr. Seyfert helped immeasurably in all stages of the development of the Observatory. At Trustees' meetings, at meetings of scientific societies and in personal conferences, his generally optimistic outlook often aided us in overcoming difficulties which arise from time to time in every large project. . . . [H]is most important work on the turbulent emission nebulosities in the central regions of many galaxies was carried out at the Mount Wilson Observatory in 1940-42. . . . His widow, Muriel Elizabeth (Mussels) Seyfert, was also trained as

Visit of West Virginia Governor Cecil Underwood to Green Bank, June 9, 1960. (L to R:) D. Heeschen, (unidentified), Governor Underwood, (unidentified), Otto Struve, Frank Callender, John Findlay. [Underwood was Governor 1956-1960 and again in 1996-2000, making him both the youngest and oldest governor in WV history.]

an astronomer at Harvard. Their son, Carl Jr., a mineralogist, took part in the testing of various sites for the NRAO.

October 10, 1960

Plans for a 300 Foot Radio Telescope [by J. W. Findlay]

1. Introduction

The following paper is intended to summarize briefly the need that exists for the 300 Foot Telescope and the design work which has already been completed towards deciding how such an instrument should be built.

2. Astronomical Requirements and Specifications.

The astronomical needs for a 300-foot telescope were outlined by D. S. Heeschen in the monthly report of the Observatory for January 1960. In this report Heeschen adopted as approximate specifications for the antenna that it would

A 300 foot telescope concept with a rotating wedge mount. This design was considered as part of a feasibility study for a 300 foot parabolic antenna with limited steering ability, conducted by the Stanford Research Institute (SRI) for NRAO, July 1960.

have an aperture of 300 feet, that it would be useable at wavelengths as short as 21-centimeters, and that it would cover not all the sky, but at least 15 or 20 degrees from the zenith. With such an instrument he considered that measurements could be made of the brightness distribution in galaxies, of the radio intensities and spectra of galaxies, and that it would be useable in making polarization measurement on a variety of radio sources. It seemed possible also with such an aperture that radio emission from stars, particularly flare stars, could be detected. It would have obvious applications in extending 21-centimeter line studies of galaxies and also problems within our own Galaxy which require higher resolution.

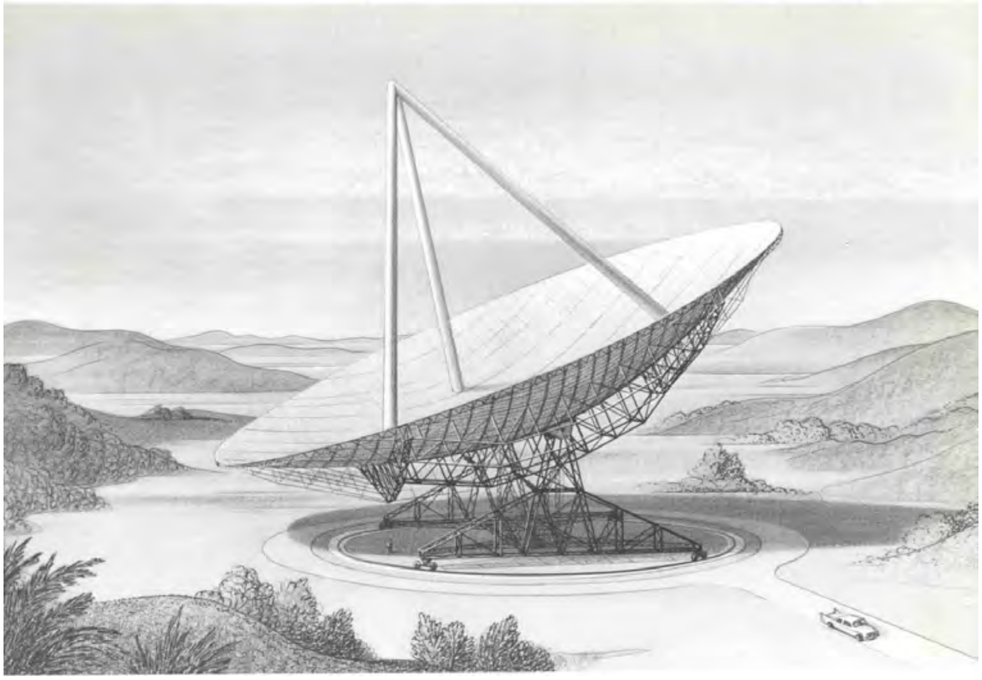
3. Early Design Studies.

Preliminary studies of such an instrument showed that a radio telescope with a fixed spherical surface and a feed which could be moved to give $\pm 15°$ of sky cover on all sides of the zenith could probably be built for a sum of about \$300,000. As a result of this preliminary investigation a sum of \$300,000 was included in the budget and in the appropriation for the Observatory for the Financial Year 1961. Design studies have been carried out for some months on various alternative designs for the telescope and the rest of this note describes these studies in more detail.

4. Design Studies

The following three types of instrument have been considered and studies have been made of their design and approximate cost.

a) A fixed spherical reflector with a movable feed.



A 300 foot telescope design with a limited-tilt Alt-Az mount, from the SRI study July 1960.

b) A parabolic reflector steerable about two axes.

c) A parabolic reflector mounted as a transit instrument.

6. Parabolic Reflectors Steerable about Two Axes

a) The bi-conical design.

A 300-foot paraboloid of simple and light construction is supported on two rotating cylindrical structures. The lower cylinder has its axis vertical and it rotates on a crane rail track supported by a foundation in the ground. The upper surface of this cylinder is inclined at an angle of 10° to the horizontal. On this upper surface the second cylinder rotates and this cylinder carries the dish. The dish is mounted on the upper cylinder at an angle of 10° from the axis of the cylinder. Rotation of both cylinders then allows the dish to be directed to point anywhere within a cone of semi-angle 20° with its axis directed to the zenith. . . .

b) The Alt-Azimuth design.

In this design a 300-foot parabolic dish similar to the one described above is carried on an elevation axis supported by two short vertical towers. The height of the towers is chosen so that 20° of elevation motion of the dish from the zenith is possible. The two supporting towers rotate through more than 360° on a horizontal crane rail track. This is a very conventional design and has the advantages of being well understood. . . .

c) The four actuator design

In the third design, developed independently by North American Aviation, a 300 foot paraboloid of conventional radial spar construction with circumferential trussed rings is supported on four screw-jack actuators spaced 90 degrees apart on the main support ring. The two horizontal axes normal to each other thus formed are located in the North-South and East-West directions. The actuators are universally mounted on each end to permit two degrees of freedom for the antenna without inducing bending loads in the screw part of the actuator.

Sky coverage is limited only by actuator and linkage lengths and rigidity of the basic structure.

7. The Transit Instrument.

The possibility of achieving more sky coverage for the same cost by mounting a 300 foot paraboloid as a transit instrument has also been considered. Such an instrument of course does not have quite the same degree of flexibility of use as fully steerable instruments, but it may prove more valuable for many problems. Studies have been made of such a design using a 300 foot dish of the same design as proposed by SRI [Stanford Research Institute] and considering the mounting of this dish so that it would cover either 30°, 45°, or 60° from the zenith. Such an instrument would be an extremely simple structure to design and build and would of course require a not very complicated drive system.

8. Conclusion.

The preliminary design study phase of the work is almost completed and detailed work will soon be started on the chosen design for the 300 Foot Telescope.

– J. W. Findlay

——— November 11, 1960 ———

140 Foot Telescope [by R. M. Emberson]

Dr. A. B. Kinzel, at the request of Dr. A. T. Waterman, agreed to serve as chairman of an ad hoc committee to review the fabrication problems of the polar shaft.* All parts of the yoke not directly involved in the polar shaft problems are being loaded for shipment to Green Bank.

——— January 5, 1961 ———

[by O. Struve]

It is appropriate to mention at the beginning of this report that the NRAO staff is greatly indebted to Dr. L. V. Berkner, who recently retired from the presidency of the AUI. The whole concept of the Observatory grew out of two conferences, one held in Washington in January, 1954, and the other in May of the same year. At this early stage of the planning, Dr. Berkner's leadership was of paramount importance to the project, and the form in which the Observatory was created was largely due to his vision and active interest. In November, 1956, the National Science Foundation assigned to AUI the contract for building and operating the

* *A discussion of the 140 Foot construction problems is given in "The 140 Foot: The First Ten Years," by D. Heeschen, later in this volume, p.265.*

Observatory. Dr. Berkner became acting director of the Observatory and supervised all activities of the new institution until the middle of 1959. Even after my appointment as director, Dr. Berkner continued in charge of the 140 Foot Telescope project. He visited the Observatory regularly, once each month, and gave us the benefit of his experience in planning our activities.

The general supervision of the 140 Foot Telescope project has now been taken over by Dr. Leland J. Haworth, acting president of AUI.... The Bliss Company has indicated that, provided additional funds are made available by the NSF, the Telescope could be completed late in 1962; otherwise, the completion date could be some time in the latter part of 1963....

Since the original date for completion was September, 1960, the radio astronomers of the NRAO have been exploring various relatively inexpensive types of instruments that could be used for the study of special radio astronomical phenomena. The most important of these is the 300 Foot Telescope, described separately in this report. Other possible instruments are a simple 40-foot parabolic antenna having a large sky coverage and intended to track continuously over periods of several hours a few strong radio sources such as Cassiopeia A, the suspected variable radio source Hydra A, and perhaps one or two other objects.

300 Foot Telescope [by J. W. Findlay]

The Advisory Committee at its October meeting considered the relative merits of several different kinds of design for the 300 Foot Telescope. Very shortly after this meeting it was decided to proceed with the design of a transit instrument 300 feet in diameter, mounted on two fixed towers and capable of movement from the zenith through 60° to the north and south. The surface of this instrument should be good enough to be used at 21 cm.

A design group under the leadership of Mr. Robert D. Hall, who, while he worked with the Blaw-Knox Company, was responsible for the 85 Foot Telescope, was set up. This group consisted of Mr. Hall and six other engineers, all experienced in radio telescope design. The group started work on November 7 and by the end of November had essentially determined all the important features of the design of the instrument. It is hoped that in the near future this design work and the necessary detailing will be completed to the stage where bids for the fabrication and the erection of the instrument can be obtained.

February 6, 1961

Astronomy Department [by D. S. Heeschen]

Observations with the 85 Foot continued full time during the month at 10 cm wavelength. Among the programs in progress are position measurements by Drake, flux density measurements of extragalactic sources by Heeschen, brightness distribution of sources by Wade, HII region studies by Menon, and measurements of Galactic non-thermal sources by Lynds.

Recent research programs [by C. R. Lynds]

During the summer of 1960, [C. R. Lynds] ... undertook an observational program designed to detect radio frequency radiation from interacting galaxies.... In the main the results of the program were negative and this, coupled with other evidence, makes the colliding galaxy hypothesis seem more and more unlikely as an explanation for the majority of radio galaxies.

The Flare Star telescope [by C. R. Lynds]

The apparent success of Lovell at Jodrell Bank in detecting radio frequency emission from flare stars indicates the need for independent confirmation and a cooperative program of simultaneous optical and radio frequency observations. Because of the mutual interest of staff members of the NRAO and the Kitt Peak National Observatory in this problem, preliminary design studies are being made at the NRAO for a special-purpose radio telescope intended primarily for the detection of radiation from flare stars.

The flare star antenna design currently held in favor consists of a cylindrical paraboloid having a rectangular aperture measuring 10m by 100m oriented so that the generating elements of the paraboloid are parallel to the rotational axis of the earth.... Plans for rotating the paraboloid about the polar axis will provide a sky coverage of three or four hours on either side at the meridian. The operating frequency of the system has been tentatively set at 300 Mc.

140 Foot Telescope [by R. M. Emberson]

The principal steel components of the telescope are the polar shaft and bearings, the yoke, and the polar gear and girder. Of these, the shaft and all of the yoke except the hub section are now at Green Bank....

The special advisory committee, under the chairmanship of Dr. A. B. Kinzel, met on January 13 to consider an engineering study, prepared by the E. W. Bliss Company.... Dr. Kinzel has circulated to the committee members a draft of his report. If no serious objections are raised, no further meetings of the committee will be held.

——— March 10, 1961 ———

300 Foot Transit Telescope [by J. W. Findlay]

The structural design of the telescope was essentially completed by mid-February. The design drawings, 27 in number, specifications and bid material were mailed to a bidders' list of 15 chosen firms....

——— April 10, 1961 ———

[by O. Struve]

With the consent of the President and Board of Trustees of AUI, Mr. J. W. Findlay was appointed deputy director, and Mr. David S. Heeschen as assistant to the director of the NRAO. These appointments will be effective for one year, or as long as the undersigned [Struve] remains as director. Mr. Emberson will continue as chairman of the Major Design and Construction Department and will continue devoting most of his time to the problems connected with the 140 Foot Telescope. Mr. H. Hvatum returned to the Observatory on April 1 and was appointed chairman of the Electronics Department (formerly Research and Development Department).

Astronomy Department [by D. S. Heeschen]

Observations at 10 cm and 40 cm wavelengths with the 85 Foot Telescope continue through March. Drake has begun a series of daily measurements of the brightness temperature of Venus at 10 cm wavelength.

The first issue of "Publications of the NRAO," a paper by Menon on the Orion Nebula, will be ready for distribution in early April.

F. D. Kahn, of the University of Manchester, arrived on March 22 for a 6 months' stay as a visiting astronomer.

The following report was written by S. von Hoerner on his research work.

I. Analysis of radial velocities in the Orion Nebula. Wildon, Münch, Flather and Coffeen published in 1959 a catalogue of 45,000 radial velocities in the Orion Nebula, taken (at Mt. Palomar) at different positions in the Nebula and of different spectral lines. A thorough analysis of these observations should lead to a dynamic model of the Nebula, as well as to a better understanding of supersonic turbulence in general; a comparison with the radio observations of K. Menon and the model derived by him would be interesting. . . .

II. Very large antennas for the cosmological problem. Two papers on this subject have been finished. . . .

III. Estimate of the mean distance between technical civilizations. A paper on this subject is in preparation and will be completed in about one month.

Electronics Department [by J. W. Findlay]

As will be noted elsewhere in this report, I have resigned as chairman of the Electronics Department. . . . This group was formed about four years ago at the Observatory and has since grown to its present size of 16 scientists, engineers, technicians and clerical staff. The equipment for test and repair and for laboratory research work has increased from a few items . . . to a present value of above $170,000. . . .

From the earliest days it has been our intention to develop at NRAO electronic techniques and skills equal to the needs of radio astronomy. Probably all that I can claim for the electronics group is that we have studied and have learned humility. Building good radio astronomy receivers, as many know, is not easy. We have made some progress in both arts, but there is much left to learn. I once asked Charles Seeger, in the early days of my experience with radio astronomy, what he thought were the important things in receiver design and construction. I am now very inclined to agree with his reply, which was "good soldering is high on the list."

Many people have commented in the past on the apparent separation in the Observatory of electronics and radio astronomy. This separation into two departments is convenient, administratively, and has certain practical advantages for NRAO, particularly in making easier the work of visiting astronomers. But this apparent separation has never in the past been permitted to extend to the scientific and technical area; nor will such a separation occur in the future. Within NRAO, astronomers and electronic scientists work together across apparent lines of departmental divisions. The electronics group is truthfully a service group to astronomy, but it is also a partner in astronomy. This attitude of mind will, I am sure, continue and grow within the Observatory.

Mr. Hvatum has taken over the department and, with it, a fine collection of unsolved problems. I leave the group with many thanks to all the workers in electronics and with my very best wishes for the future.

– John W. Findlay

300 Foot Transit Telescope [by J. W. Findlay]

Bids from ten companies were opened on March 20, for the detailing, fabrication and erection of the structural steel of the telescope. About 500 tons of steel was involved in the bid.... Fabrication bids, which included detailing, fabrication, shop assembly of the main trusses, painting and shipping to Bartow, West Virginia, were as low as 26 cents per pound of steel....

140 Foot Telescope [by R. M. Emberson]

During the past two months, Messrs. Haworth and Reynolds, representing AUI, and Mr. J. A. Lindberg, representing Bliss, agreed on the general terms for terminating the existing lump-sum prime contract and negotiating a cost-plus-fixed-fee arrangement for continuation of the work....

May 8, 1961

[by O. Struve]

The contract for the fabrication and erection of the 300 Foot radio telescope was signed on April 28, 1961, with the Bristol Steel and Iron Works.

Messrs. Heeschen, Drake, Lynds and Menon attended a two-day meeting at Kitt Peak National Observatory. It is the purpose of these annual meetings—at Kitt Peak and the NRAO in successive years— to discuss problems of mutual interest. Among these are the observations of flare stars and the identification of radio sources by optical means....

The second issue of the Publications of the NRAO, consisting of a paper by S. von Hoerner, entitled "Very Large Antennas for the Cosmological Problem I. Basic Considerations," is ready for distribution.

Negotiations are under way for the construction of a 40-foot transit type telescope intended for the observation of bright radio sources that are suspected of variability. The decrease in flux of Cas A has already been demonstrated by Heeschen and by several other radio astronomers. I. S. Shklovsky has recently predicted that there should be a somewhat similar secular decrease in the flux of Taurus A. Other radio sources will undoubtedly also be found to vary in radio intensity.

10 cm Observations of Venus [by F. D. Drake]

A program to observe Venus as often as possible [using the 85 Foot Telescope] has been underway since March 17. Phase angles of 132° to 168° before inferior conjunction, and 168° to 150° after conjunction have been observed, and the program will be continued, it is hoped, until phase angle 100° is reached.... Data is taken in the form of punched tape, and a day's observations can be reduced in about 15 minutes with the Bendix G-15 computer....

The data gathered to date show the following: An apparent black body temperature of about 595° K, with no day-to-day variation of more than a few per cent. This again confirms the high temperatures measured previously, but this time with a fundamental calibration independent of the previous standards used....

Electronics Department [by T. Orhaug]

With the construction of new telescopes at NRAO there will be a great need for radiometers of the continuum type. Our intentions are to make these radiometers exactly alike, so parts from one receiver will be interchangeable with those in others....

NRAO Observations of Venus
from *The Observer,* March 29, 1963

The results of the Mariner II studies of Venus, made at a cost for space vehicles alone of $50,000,000 have shown that: The microwave radiation from Venus comes from its surface; The surface temperature is about 600±120 °F; The bright and dark sides are at nearly the same temperature.

About a year ago, the NRAO published studies made with the 85-foot telescope in 1961 which showed: The microwave radiation from Venus comes from its surface; The surface temperature is 613±65 °F; The bright and dark sides are at nearly the same temperature.

Therefore, although it is hard to find this in all the publicity about the Mariner exploits, our relatively inexpensive observations appear to be both correct and better than any that have been made. This reflects well on the Observatory, and will certainly be recognized in the years to come when the history of such studies is reviewed.

Editors' note: The Venus results were published by F. Drake in Physics Today, vol. **14,** p.30, April 1961.

Selections from the NRAO Monthly Summary of Activities

—————— June12, 1961 ——————

[by O. Struve]

At the request of President Bronk of the National Academy of Sciences, the NRAO arranged a symposium (between May 15 and 19) on galaxies and related problems. This symposium was the first in a series of USA-USSR symposia which will take place alternately in the U.S. and in the Soviet Union. The Soviet delegation was headed by Dr. V. V. Vitkevich. [*See "The First Green Bank Workshop," in this volume (§ IV.1), page 473*]

Dr. Allan R. Sandage of the Mount Wilson and Palomar Observatories attended the symposium and spent about two weeks at the Observatory as Senior Consultant.

300 Foot Radio Telescope [by J. W. Findlay]

The contract for fabrication and erection of all the structural steel of the telescope with the Bristol Steel and Iron Works was signed on April 28. . . . In general, all the work on the telescope is proceeding in such a way as to permit the original schedule to be met. The first deliveries of steel to the site are expected towards the end of July.

Astronomy Department [by D. S. Heeschen]

Shakeshaft and Hogbom have recently reported, in *Nature,** a secular variation of the intensity of Cas A. . . . The variation is of the order of one per cent per year, but neither the Cambridge nor the NRAO observations suffice to determine the rate accurately. Nor is it known whether the rate of variation is a function of frequency.

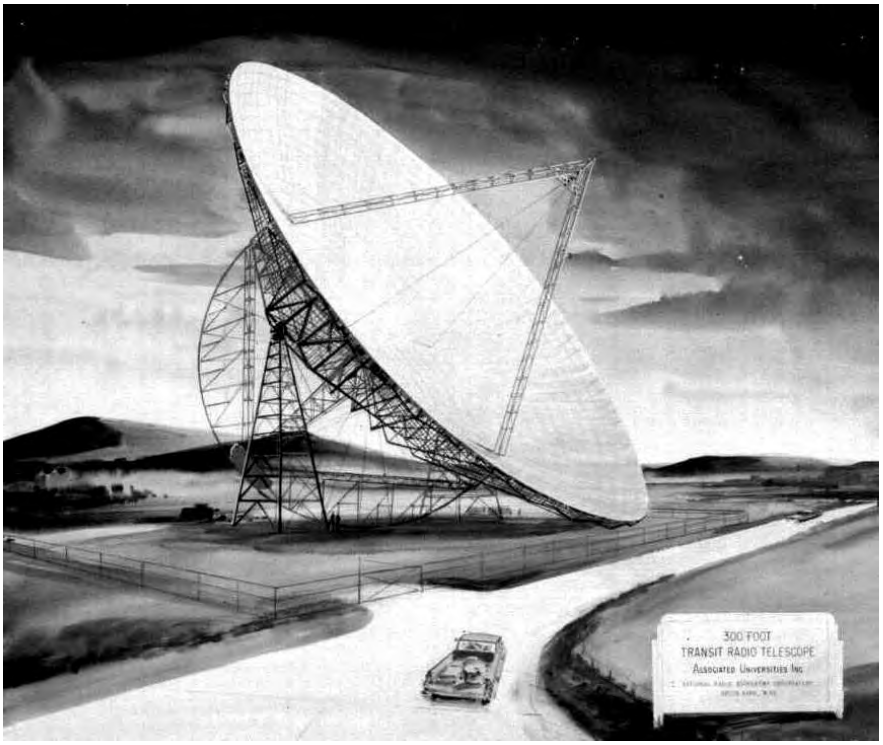
In order to investigate this problem further, and to search for possible similar effects in other supernovae remnants, a 40-foot diameter transit telescope is being built at the Observatory. It is hoped that with this instrument a high degree of internal consistency may be maintained over a long period of time. The telescope will be used for routine daily observations, at two wavelengths, of Cas A, Tau A, and possibly several other Galactic sources, and several (hopefully constant) comparison sources.

—————— July 7, 1961 ——————

[by O. Struve]

After the resignation of Lloyd Berkner as president of AUI, Leland Haworth as the new president, and Edward Reynold, of the Board of Trustees, undertook to study the 140 Foot Telescope project and to recommend to the NSF and to the AUI Board of Trustees necessary changes in the construction and in the future management of the project. When Mr. Haworth became Atomic Energy Commissioner, the full responsibility fell upon Mr. Reynolds who has spent a great amount of time

* Nature, vol.**189,** p.561, 1961



From the April 10, 1961 Monthly Summary of Activities: This "reproduction of an artist's impression of the [300 Foot] telescope ... does not show the drive system nor the observing and control building. It is also not likely that there will be such a beautiful road, and it is certain that no road will run directly on the south side of the instrument."

and effort in an attempt to straighten out the very complicated contractual and management problems that had accumulated during the past few years.

Although further delays in the construction of the telescope occurred during the past year, these delays were probably inherent and would have occurred no matter what decisions were made concerning the construction. After having been elected vice president of AUI, Mr. Reynolds arranged with the engineering firm of Stone and Webster to study the mechanical problems of the telescope and to undertake the entire task of supervising the construction. In order to supply Mr. Reynolds with the necessary scientific information concerning the expected performance of the instrument and concerning other radio telescopes of similar size, I appointed Mr. Heeschen as the representative of the Observatory on the 140 Foot Telescope Project.

The astronomers at Green Bank have suggested that a careful study be made of the other large radio telescopes, in particular of the so-called "Haystack" antenna of the Lincoln Laboratory. Messrs. Heeschen and Findlay have visited the Lincoln Laboratory.... Their report indicates that the Haystack antenna of 120-foot aperture and weighing only a fraction of the weight of the 140 Foot Telescope, would probably be a very fine instrument. . . . There are some indications that it would be possible to construct one or even two Haystack-type telescopes for the amount of money that has not yet been spent for the 140 Foot Telescope but has been appropriated by the NSF toward the latter project.

However, at the time of this writing, I am not prepared to recommend that so drastic a change be made in the character of the large telescope for Green Bank. The Haystack instrument has not yet been built. . . . It seems to me necessary to continue with the efforts . . . in bringing to a satisfactory conclusion the construction of the 140 Foot Telescope. . . . If these efforts should turn out to be unsuccessful, or if the NSF should find it impossible to provide the funds that will be needed for the completing of the 140 Foot, it may well be that in eight or ten months we shall recommend adopting as our concept the Haystack antenna or some other similar mounting.

It is now certain that the usefulness of the 140 Foot Telescope will largely be restricted to the wavelength range between 3 and 10 cm. Hence, the precision of the surface should meet this requirement. We believe that a study should be made of the thermal deflection that may result from unequal heating by solar radiation and by other causes.

The scientific work of the Observatory is continuing in a very satisfactory manner. The Observatory has now published four issues of its own publications, and has distributed 22 reprints.

As has been pointed out previously, the principal interest of the astronomers at Green Bank lies in the high frequency region of the radio spectrum. Theoretical and laboratory experiments had shown some years ago that the emission of water vapor in the earth's atmosphere gives irregularities in the observed fluxes of the cosmic sources. These were found to be serious at a wavelength of 3.5 cm by G. Westerhout, who was a visiting scientist at NRAO in the summer of 1959. It has been suggested that for short wavelength studies, of the order of 3 to 5 cm, climatic conditions may turn out to be of major importance. Hence, the Observatory is now conducting an experiment at Green Bank with a 12-foot dish pointed toward the celestial pole and equipped with a 3 cm receiver. A similar instrument and receiver will be placed in operation as soon as is possible at a desert site in one of the western states. A comparison of the results should make it possible to decide whether there would be a real advantage in conducting 3 to 5 cm astronomical observations in a very dry climate, rather than in Green Bank where variable humidity has compelled us to use a 10 cm receiver at the 85 Foot Telescope for a large part of our work.

– Otto Struve

Engineering Group [by J. W. Findlay]

The present tasks of the engineering group include work on: a) the 300 Foot transit telescope; b) the flare star antenna; c) the 40 Foot transit telescope. Work on the foundations and the steel fabrication for the 300 Foot proceeds satisfactorily, and this report is being devoted to a brief summary of the needs of the engineering group at the Observatory.

At present, J. W. Findlay, H. B. Lindstrom, and S. Smith are working on engineering for new antennas. The work of such a group is easy to define. New antennas required by the Observatory should be thought out by the astronomers and engineers together. Various design ideas should be worked out so that the best idea can then be turned into a fully engineered design. All these phases of the work should be centered at the Observatory, so that astronomical, electronic and engineering problems can all be worked out together. The result should be a set of design drawings suitable for fabrication. . . .

The Observatory already has the nucleus in personnel for its engineering group, and adequate drafting room space and equipment is available. For some time a search has been going on for an engineer to lead the group as department chairman. This search continues to be unsuccessful. Several very desirable engineers have refused the position. More than fifty applicants have been considered and many have been interviewed. The job is difficult but interesting, but most of the refusals have been based on the isolation of Green Bank. All those who have been interviewed have said that the work was most attractive.

There must be, somewhere in the United States, an engineer who would be willing, as many scientists have already been willing, to move to NRAO and to build novel instruments for radio astronomy. If this note reaches anyone who can interest possible candidates for the job, please ask them to get in touch with O. Struve or J. W. Findlay.

– J. W. Findlay

Astronomy Department [by D. S. Heeschen]

A 40-foot diameter transit telescope has been ordered from Antenna Systems, Inc. It is expected to be in operation in September or October. The project for which the instrument will be used was described in last month's report.

August 7 , 1961

Major Design and Construction Department [by R. M. Emberson]

A new contract has been signed between AUI and E.W. Bliss Company, and is now before the National Science Foundation for review and approval. . . . During the contract negotiation period, progress in the shops and at Green Bank has been slow. The first reflector panel, which was fabricated earlier in the year, has been made to approximate the desired paraboloid by a considerable amount of hand work and refitting.

The following report was written by Mrs. Beverly T. Lynds on her work:

Studies of Dark Nebulae:
A catalogue of dark nebulae is now in preparation, based on a study of the Palomar 48-inch Schmidt Sky survey. The prints of the sky survey have been examined, and all of the dark nebulae visible on these photographs have been recorded. . . . Estimates of the opacity of these objects have been made on an arbitrary scale . . . the area, in square degrees, of each cloud has been measured and positions in the $\alpha - \delta$ coordinate system have been recorded. It is planned to include these data in the catalogue, together with the galactic coordinates of each cloud and a brief description of the isolation of the cloud or its association with other bright or dark nebulae. When the catalogue has been completed, several statistical studies are planned. . . .

September 6, 1961

The 300 Foot Transit Radio Telescope [by J. W. Findlay]

The Bristol Steel & Iron Works, Inc., arrived at Green Bank on August 11, a date very close to the original estimates of the best schedule for the work. . . . A 50-ton and 45-ton crane have been used on the site, though the 45-ton crane has mainly been unloading steel at the Bartow rail-head.

Now, at the end of August, the north and south erection towers have been built and the two main telescope towers (except for the cap on the east tower) have been erected. A large amount of the steel shipped from Bristol has been brought to the site and laid out, and sub-assembly work started. The Observatory has made a rough road to the Lowe house, which has been lent to Bristol Steel & Iron Works Inc., as a field office.

At Green Bank the following have worked on the project: Mr. Sidney C. Smith, Field Engineer for foundations and erection; Mr. H. Bernard Lindstrom, Mechanical Engineer for drive systems . . . and Mr. John N. Ralston, Junior Engineer. Mr. S. R. Greenwood, although not officially connected with the project, has kept a watchful eye on work at NRAO whenever his 140 Foot duties have brought him to Green Bank.

October 2, 1961

[by O. Struve]

The scientific group consists of the following persons:

Drake, F. D.	Associate Scientist	Lynds, C.R.	Asst. Scientist
Emberson, R.	Scientist	Menon, T.K.	Assoc. Scientist
Findlay, J. W.	Scientist	Orhaug, T.	Vis.Assoc.Sci.
Heeschen, D. S.	Scientist	Struve, O.	Scientist
Hogg, D. E.	Asst. Scientist	Vinokur, M.	Asst. Scientist
Hvatum, H.	Assoc. Scientist	Wade, C.M.	Asst. Scientist
Kahn, F. D.	Vis. Scientist	Waltman, W.	Jr. Res. Assoc.
Lynds, B. T.	Research Assc. (1/2 time)		

In the first half of September the Observatory was visited by a number of astronomers who had attended the Berkeley meeting of the International Astronomical Union. Among the outstanding lectures that were given at the NRAO were three colloquia by Professor Bertil Lindblad of Stockholm, describing his gravitational theory of the formation of spiral arms. . . .

An outstanding lecture was given at the NRAO by Dr. J. E. Blum, a member of the French Radio Astronomical Laboratory at Nançay. The title of his talk was "Developments in Antenna Techniques and Correlation." Dr. G. Westerhout spoke on polarization measurements made in the radio wave region at the Dwingeloo Radio Observatory in Holland. Dr. B. A. Lindblad of Lund, Sweden, presented results of his radar observations of Perseid meteors, extending over approximately ten years. Prior to the Berkeley meeting, Dr. Martin Ryle of the University of Cambridge, England, discussed his radio observations intended for the solution of the cosmological problem.

Mr. Findlay has devoted most of his time to the construction of the 300 Foot Telescope and to activities of several commissions concerned with the West Ford project.* Mr. Heeschen will continue serving as assistant to the Director, with special assignment to the 140 Foot Telescope project. His main responsibility is to keep in touch with the decisions made by Mr. Edward Reynolds and his collaborators and to pass on the relevant information to the NRAO staff in Green Bank.

The relatively inexpensive 40 Foot radio telescope, mentioned in previous reports, will be installed at the NRAO during October. The dish is finished at the shops in Hingham, Massachusetts, and the installation is expected to take only a few days. A small control building, partly underground, is now under construction. The receivers and recording devices will be almost entirely automatic, and the instrument is primarily intended to provide for the continuous observation of bright radio sources, some of which may be variable, as in the case of the supernova remnant Cas A.

Mr. S. Weinreb, graduate student at MIT, has spent about two months at the Observatory, trying to detect the line of deuterium in the radio spectrum. He brought with him a very sensitive receiver that he had built at MIT and used it in connection with the 85 Foot Telescope. The results were negative, but indicate an upper limit for the abundance of deuterium in the Milky Way that is somewhat lower than is its abundance on earth.

Mr. Struve was primarily engaged in the preparation of two manuscripts, one being a short book on the history of astronomy in the twentieth century, while the other is an attempt to bring together all up-to-date information on the extraordinary eclipsing variable β Lyrae. . . .

November 3, 1961

[by T. Orhaug]

S. Weinreb has tested his digital receiver in the 85 Foot Telescope, and from the high performance of his system it became clear that we should also consider the digital system. From H. Adams (Control Equipment Corp.) we have received a preliminary price estimate for a 100-channel receiver having a frequency coverage of 5 mc. . . .

* Editors' note: Project West Ford proposed to place one or more belts of thin microwave dipoles in orbit about the Earth at an altitude of a few thousand kilometers for the purposes of radio communications. Its effects on astronomy are discussed in a series of articles in the Astronomical Journal, 1961, vol. **66,** pp. 105-118. An issue of Proceedings of the IEEE (1964, vol. **52,** p. 452ff) gives a retrospective on the project, and includes an article by Findlay.

The 40 Foot Radio Telescope Project

by Omar Bowyer and Bill Meredith
excerpts from *The Observer* in 1961 and 1968.

It has long been known that the optical brightness of some stars changes from day to day and it has been suspected that radio sources vary in intensity. The change in intensity is very small and is easily hidden in the uncertainty of the results. Many factors affect the uncertainties and to minimize these effects, as many things as possible must be kept constant during the entire experiment. It is not practical to tie up a very expensive instrument for such a long period of time as is needed for this experiment, so the relatively inexpensive 40 Foot Telescope was built.

This telescope will be devoted exclusively to the present experiment for at least 5 years as will the receivers now in operation at the telescope. The 40 Foot Telescope, like the 300 Foot Telescope, moves about one axis only, north or south along the meridian. These telescopes can point to any declination in the sky (within limits) but must make use of the earth's rotation to change their position in right ascension.

Eight radio sources are observed each day: 3C 48, 3C 144 (Taurus A, the Crab Nebula), 3C 218 (Hydra A), 3C 274 (Virgo A), 3C 295, 3C 358, 3C 405 (Cygnus A), and 3C 461 (Cas A). These series of observations are the interest of Dr. Heeschen.

Because of innovations in manufacturing and assembly techniques, the 40 Foot was erected in about two days. The telescope's surface is aluminum mesh similar to the 300 Foot, but with smaller openings. The telescope is capable of being moved in wind up to 40 miles per hour, but for safety reasons, it is automatically prevented from moving at considerably less wind velocities. One of the chief criteria for the system is reliability, since the telescope will be unattended.

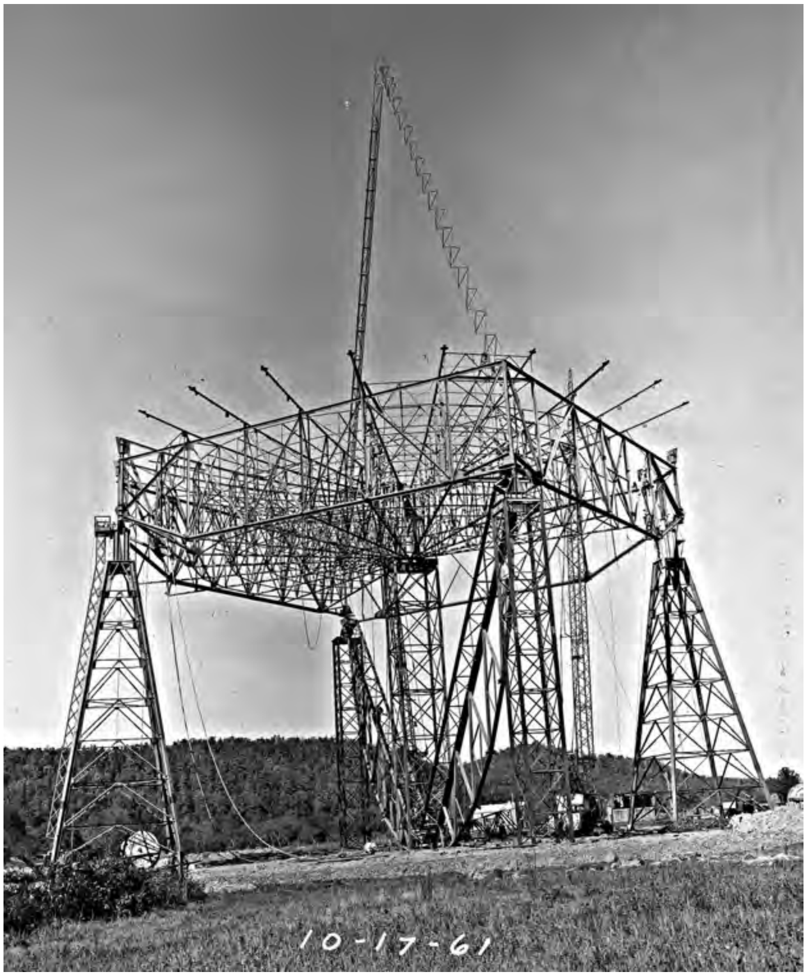
The control system positions the telescope in the sky at appropriate times, initiates the beginning of an observation, times the complete observation to the nearest second and signals that the observation has ended, selects and identified data to be punched and printed on tape, and initiates receiver calibration at appropriate times.

The system is designed to observe at 750 Mc and 1400 Mc simultaneously. Two standard NRAO receivers are used with a Jasik feed. Most of the control equipment has been designed and built at NRAO because of the need for specific items which are not stock items in the electronics industry. The outputs of the 750 megacycle and 1400 megacycle receivers are recorded in three ways. First, an analog record is provided on a Sanborn Recorder, which is used for pinning down interference and is indicative of overall performance. The analog record is not satisfactory for use in data reduction with the computer, so two more outputs that are digital in nature are provided. One of these is a printed record much like adding machine tape, and the other is punched [paper] tape.

The 40 Foot Telescope and its underground Control Room in 1962.

The data tapes, which are collected every three to five days, are read directly into the Observatory's IBM 1620 digital computer. The value of the calibration and the relative value of the source intensity are computed. A ratio of source intensity and calibration factor is then derived and the result punched out on cards, with some other useful information.

The 40 Foot telescope was tested on December 14, 1961. Everything worked exactly as planned. By February 1, 1962, the NRAO will have the first fully automatic radio telescope on this planet. Congratulations should be bestowed on Dr. Heeschen, F. Crews, and B. Waltman for a job well done.



The 300 Foot Telescope under construction October 17, 1961, four years to the day after the Observatory's groundbreaking ceremony in 1957.

Selections from the NRAO Monthly Summary of Activities

—————— January 8, 1962 ——————

Personnel [by D. S. Heeschen]

Dr. Struve retired December 1 as Director of the Observatory. He had originally planned to retire in October, 1962, but moved the date forward for personal reasons and also in order to devote full time to his research and writing. It has been a great privilege to have had Dr. Struve as director and the entire Observatory staff very much regrets his departure. Dr. J. L. Pawsey* will be the new director and will take up this position in the fall of 1962. The staff welcomes his appointment

* *Dr. Pawsey died in Nov 1962 before he could take the position as director. Dave Heeschen moved from acting to permanent Director in October 1962.*

and looks forward to his arrival. David S. Heeschen is the acting director of the Observatory until Pawsey takes over.

C. R. Lynds resigned, effective December 15, to accept a position at the Kitt Peak National Observatory. T. K. Menon resigned, effective December 31, to accept a position at Ohio State University. Dr. S. von Hoerner has accepted a position as scientist, and will come to the Observatory in May. He is presently at the Astronomisches Rechen-Institut in Heidelberg, Germany.

Equipment Development [by D. S. Heeschen]

The steel erection of the 300 Foot Telescope was completed in early December and the erection contractor has left. The preparations for measuring the superstructure and installing the surface are now under way. This will be done by the Observatory personnel, under the direction of J. W. Findlay. The drive components are expected in January, and will be installed shortly thereafter. An automatic control system for the telescope has been designed and will be built at the Observatory.

All the components of the 40 Foot Telescope are now ready for testing. Operation of the Telescope and receivers is fully automatic. The control system, designed and built at the Observatory by J. F. Crews and O. Bowyer, has been tested and it is operating satisfactorily.

Research [by D. S. Heeschen]

S. Weinreb has completed observations with the 85 Foot Telescope and his autocorrelation receiver. In addition to a search for deuterium, he has attempted to detect Zeeman splitting of the 21 cm absorption lines from Cas A and Tau A. The material has not yet been fully reduced and, therefore, no results can be reported.

Part II

The 300 Foot Telescope, 1961—1988

The Committee discussed the need for a Very Large Antenna for astronomical purposes. Dr. Deutsch wondered if information on the Navy design should be obtained for possible application towards the design of a telescope at Green Bank. Dr. Burke pointed out, however, that an aperture of at least 1000 feet would be necessary for some of the problems that have been mentioned. Prof. McVittie noted that there had been discussion of a 2000 foot aperture last year, and wondered if we shouldn't hold to this and not be distracted by immediate requirements for smaller instruments. In this connection, mention was made of the proposal by NRAO for funds to construct a 300-foot telescope with limited steerability. Dr. Heeschen has prepared preliminary cost estimates that indicate the general nature of the proposed instrument.... Dr. Heeschen pointed out that if there were a question of having either a 300-foot or a Very Large Antenna, he would favor dropping the smaller instrument. On the other hand, he pointed out that the 300-foot instrument could probably be completed in 1960, whereas the Very Large Antenna would not be available until years later.

from the Minutes of the NRAO Advisory Committee Meeting
Green Bank, West Virginia, September 15-16, 1959

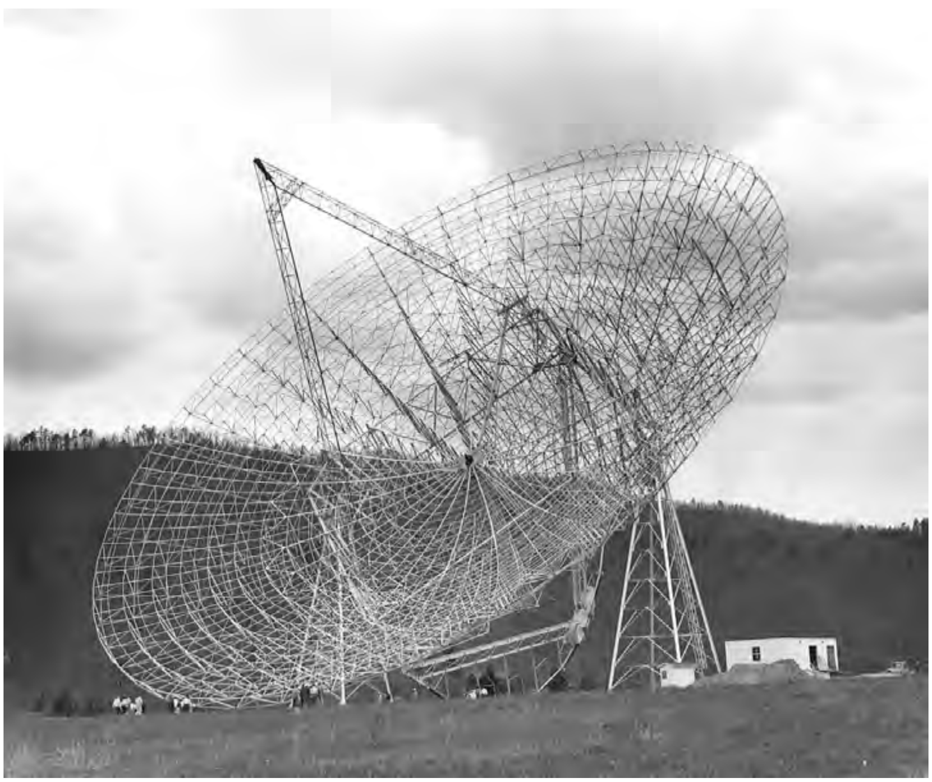
The site of the 300 Foot before the start of construction, March 30, 1961.

"The choice of 300 feet for the diameter of the dish was based on the obvious need for a considerable advance in both resolving power and collecting area beyond those of instruments existing in 1960. But this desire had to be weighed against the high cost, both in money and time, of a large fully steerable telescope. If the new instrument could be built fairly quickly and cheaply, its full scientific value would be realized before progress of the science overtook the instrument's purposes."

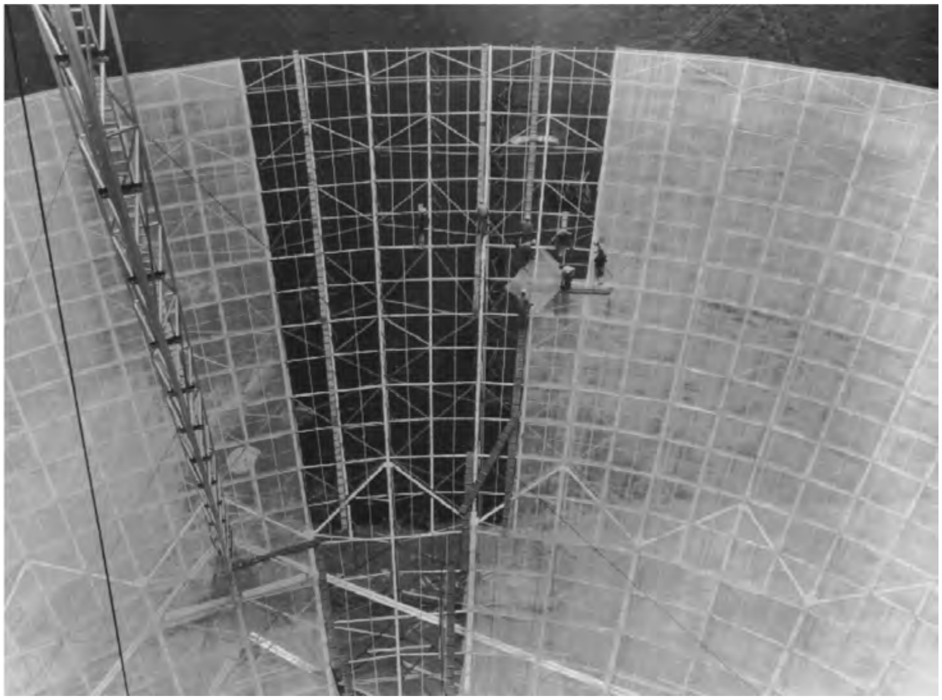
John W. Findlay
Sky and Telescope, vol.**25,** No.2, p.68, February 1963.

1 Chronology of the 300 Foot

John Findlay at the groundbreaking for the 300 Foot Telescope, April 27, 1961.



Top: Construction of the box girder, October 5, 1961. Workers are inspecting one of the gusset plates.
Bottom: First tilting of the telescope before installation of the surface, May 4, 1962.



Installation of the first surface, August 1962.

The First Receiver Installation
recollections by Dewey Ross, NRAO

The day of the scheduled installation of the first receiver on the 300 Foot approached faster than the completion of the receiver. Hein Hvatum (then Head of the Electronics Division) decided to delay the installation until we could test the system in the lab. Shortly (10 min.) after announcing the delay, he left for New York. As he turned his car onto Route 28 outside the Observatory, Frank Drake came into the lab.

It seems that news of the first observations had leaked to the press and reporters were on their way. The "Old Green Bank Spirit" came alive. The front end was loaded on a truck, and technicians, with tools at the ready, continued wiring the receiver on the way to the telescope.

After a hasty installation, some frantic trouble shooting and the arrival of reporters, we observed the first source.

Top: The original 300 Foot Telescope control panel assembled in the Jansky Lab before it was moved to the control room.
Bottom: The completed 300 Foot Telescope, September 1962. F. D. Drake (right), probably Noresh Mathur (second from right). Otto Struve is seen in the distance, with a crutch.

Digging the ditch to extend the southern declination limit, October 1962.

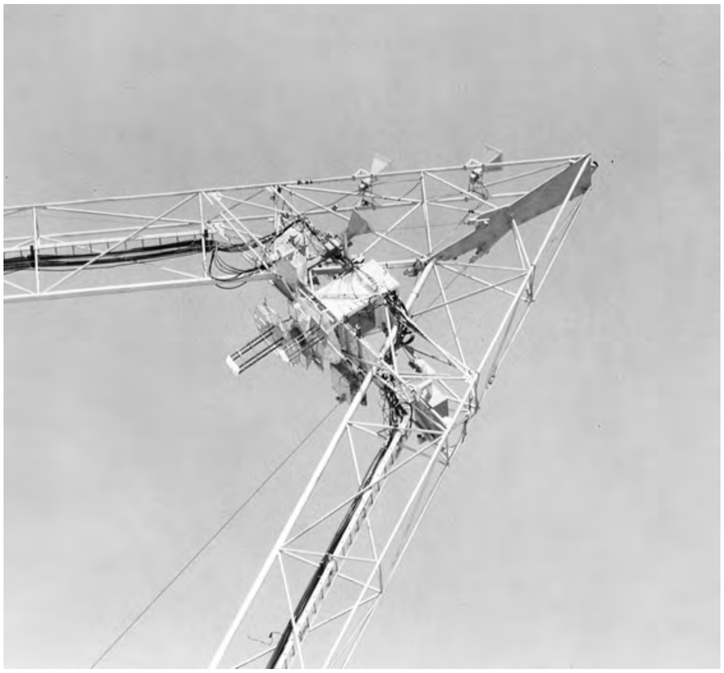
First Observations

from an unsigned article in the Observer, September 30, 1962.

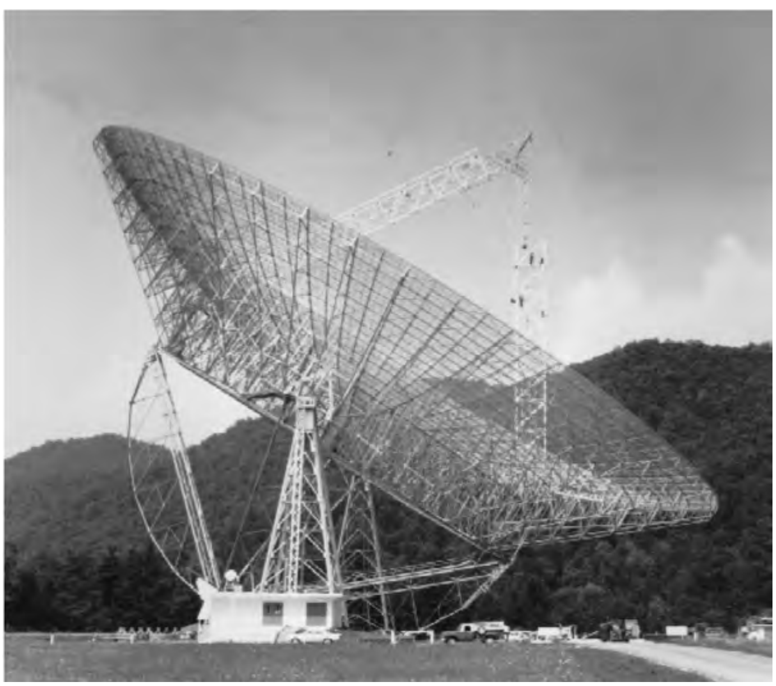
On Thursday, September 20, 1962, the telescope operators and the electronics division installed the 750 Mc (40 cm) and 1400 Mc (22 cm) front-ends at the focal point of the 300 Foot Telescope. The feed was placed at the point where, it had been calculated, the reflected energy from the dish would be focused.

The first identified source that was seen was the remnant of Tycho's supernova shortly after midnight. About a dozen sources were observed between midnight and 4:00 AM.

The beamwidth of the telescope at both frequencies appeared to be about right, and the gain of the instrument also appeared to be correct. This was regarded by everyone as a sufficiently remarkable achievement to justify some champagne provided by the Observatory.



Focal point of the 300 Foot in April 1964. Six receivers were in place covering bands from 234 to 1400 MHz, and several sky horns were used for the receivers' Dicke switching systems.



View of the original control building during construction of the new feed support legs, 1966.

Snow Removal
from the Observer, December 1972
by B. Viers and J. Spargo

Before the advent of the now-famous jet engine to remove snow from the surface of the telescope, the choice of snow removal fell to the backache brigade. This hearty corps, consisting of personnel from Telescope Operations and Plant Maintenance, were called upon on two memorable occasions to remove, using brooms, an accumulation of one foot of snow from the telescope's surface. A quick calculation shows that 1.8 acres of snow one foot deep equals roughly 78,000 cubic feet of snow or about 38 railroad cars full.

Structural improvements and damage to the surface
from "300 Foot Telescope Technical Evolution, 1962-1987"

During the summer of 1966, the backup structure was strengthened and stronger feed support legs were installed. A large vertex cabin and motor driven focus mount allowed installation of heavier receivers in the standard front-end box. The structural work badly damaged the antenna mesh surface. Soon observers were reporting in the literature that "over half of the total power received by the antenna at 21-cm came in the large sidelobes or error beam."

Snow Removal by Jet Exhaust
from the Observer for December 1972
by John Findlay

It was at about this time that the notorious jet engine was used to blow snow out of the surface. I recall this activity as an example of how somewhat light-hearted remarks are sometimes made and then turned into not very sensible actions. The problem of snow loading of the 300 Foot has always been a quite serious one. In the design stage I was asked several times what snowload the telescope would stand (it is in fact about 10 pounds per square foot) and what would be done if more snow than this occurred and the telescope was in danger of collapse. I looked at several ways of removing snow; one of which was to send an electric current up one tower, through the structure and down the other tower in order to warm all the steel and melt off the snow. This required about a gillion amperes and was obviously unacceptable. I therefore turned off many of the questions by saying that the best source of a large amount of heat in the right place would be to blow the exhaust of a jet engine at the telescope. This would blow the snow away and melt it at the same time. This idea was taken up by people who can now remain nameless and a suitable jet engine was procured, mounted on a trailer, tested many times to the acute discomfort of everyone around Green Bank and finally used on two or three occasions to blow snow out of the telescope. It did in fact work but the trouble and inconvenience far outweighed the advantage.

De-Icing the dish

From interviews with Fred Crews,
head of telescope operations, 1962-1992.

In the first years of operation of the 300 Foot telescope there was concern that heavy snow and ice conditions, which are fairly common in Green Bank winters, could damage the parabolic surface. The first surface was a mesh without much structural strength. The most straightforward way to remove the snow and ice was to send up people with brooms. But they had to be very careful to walk on the struts, not in the middle of the panels, to avoid bending them.

The matter of how best to de-ice the dish was the subject of much discussion. One engineer came up with the idea of building many small fires under the dish. He calculated that this would provide enough heat to remove the snow and ice if the fires were distributed throughout the area directly under the dish. This method was actually tried, but there was a fatal flaw in the plan. The fires at first worked and the snow began melting, but then the water dripped down and put out the fires!

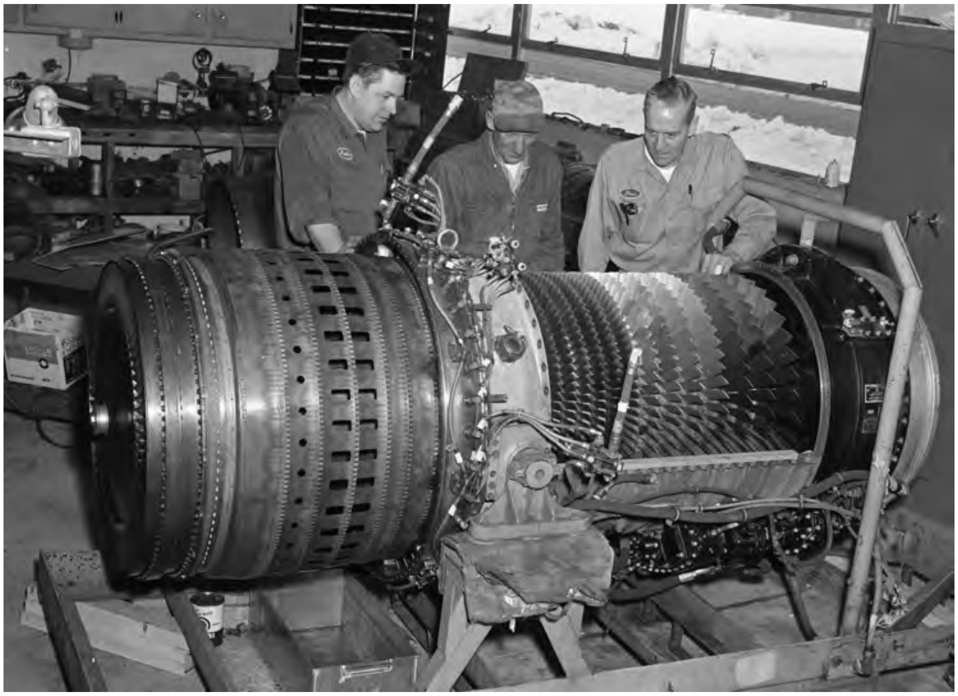
Acting on a not entirely serious suggestion of Findlay, a government surplus jet engine was obtained. It was fitted to a trailer so that its exhaust could be directed upwards towards the underside of the telescope dish, and by angling the trailer back and forth the exhaust would sweep around on the dish. There was, at first, some concern that the material in the exhaust could damage or distort the dish surface, but after several tests, it was found that wasn't a problem.

It happened that two of the observatory employees (Jim Simmons and Sam Taylor) had experience as jet mechanics for the Air Force in World War II. They worked on the engine and maintained it, after attending a jet engine school in Cincinnati.

The jet engine was successful at de-icing the dish and was used during two winters (1964-65 and 1965-66). It was ultimately abandoned partly because it required constant maintenance and spare parts were very difficult to find. The amount of effort required to keep the engine in repair was not worth the benefit.

Nevertheless, the mesh surface had become very lumpy by 1966 due to the work on the feed support legs and vertex cabin. An attempt was made to flatten it by taking off the surface panels and rolling them with a large paving roller. This was not successful because the rolling made the aluminum less flexible and generally did not remove the lumpiness.

Finally in 1970 a new surface was installed with surface panels that had considerably better structural strength. It was possible to walk on the panels without distorting the shape, and they held up much better under snow and ice loads.



The Jet Engine in the shop, January 1963. Left to Right: unidentified, Clifford Barkley, Paul Devlin.

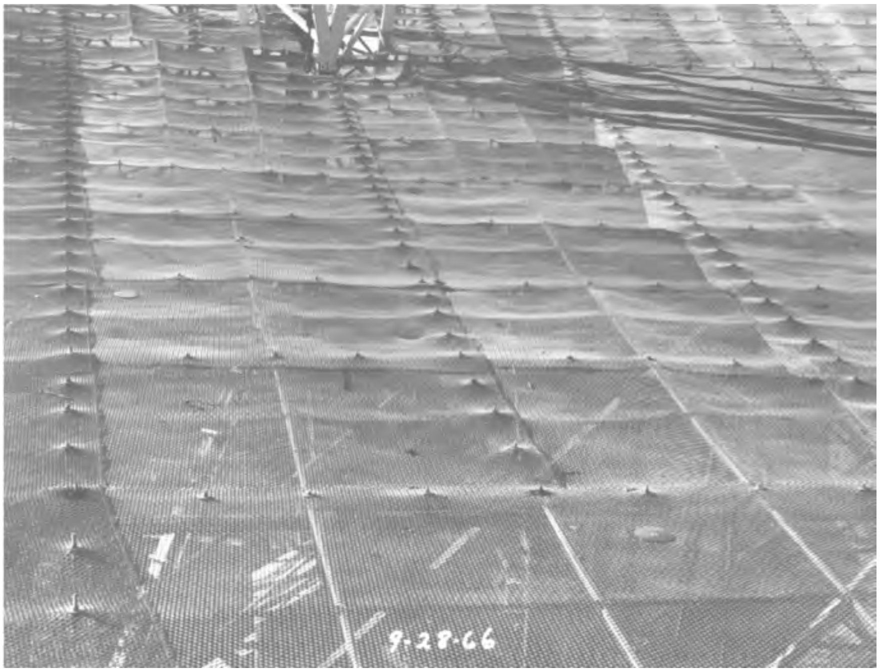
From the Observer of February 28, 1964

NRAO Flying School???
No, it's just that the guys down in Engineering are working hard on a project of adapting a jet engine as a de-icing unit for the welfare of the scopes.

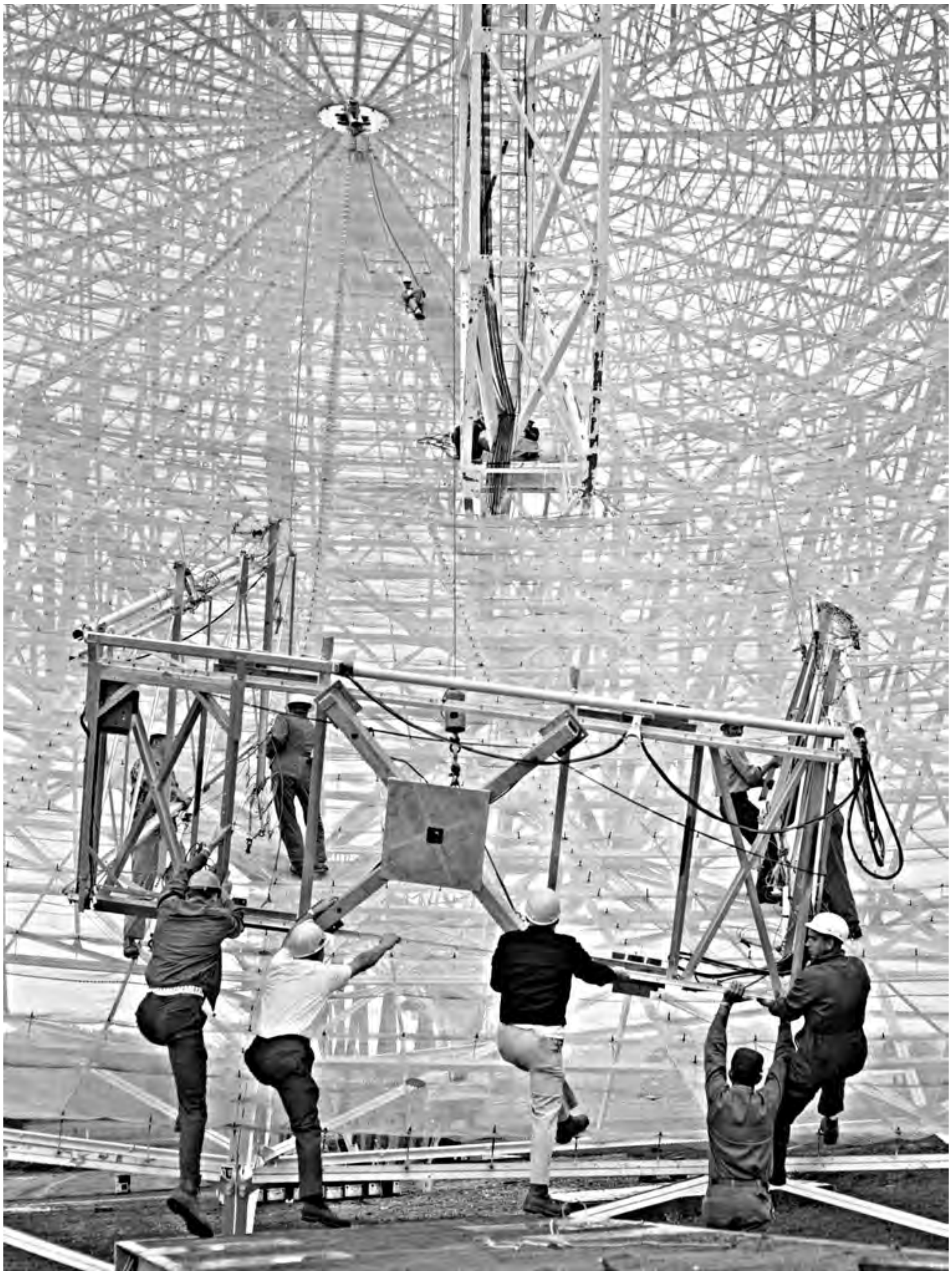
From the Observer of March 31, 1965

AIRPLANE LOST???
On the morning of Friday, March 19, time 0230, the ring of the telephone shattered the quiet at the guard shack. Jim Ryder, then on duty, answered, "Hello, guard house."
"Hello" cried a voice half anguished, half asleep.
"Can I help you, ma'am?" asked Jim.
"No, not me, but can you help that lost airplane?" wails the woman. "It must be a lost jet. He's been circling overhead for the past three hours."
Jim asks, "Does the jet noise ever stop?"
"Yes," she says, "but it'll be back. It goes and comes, but it'll be back."

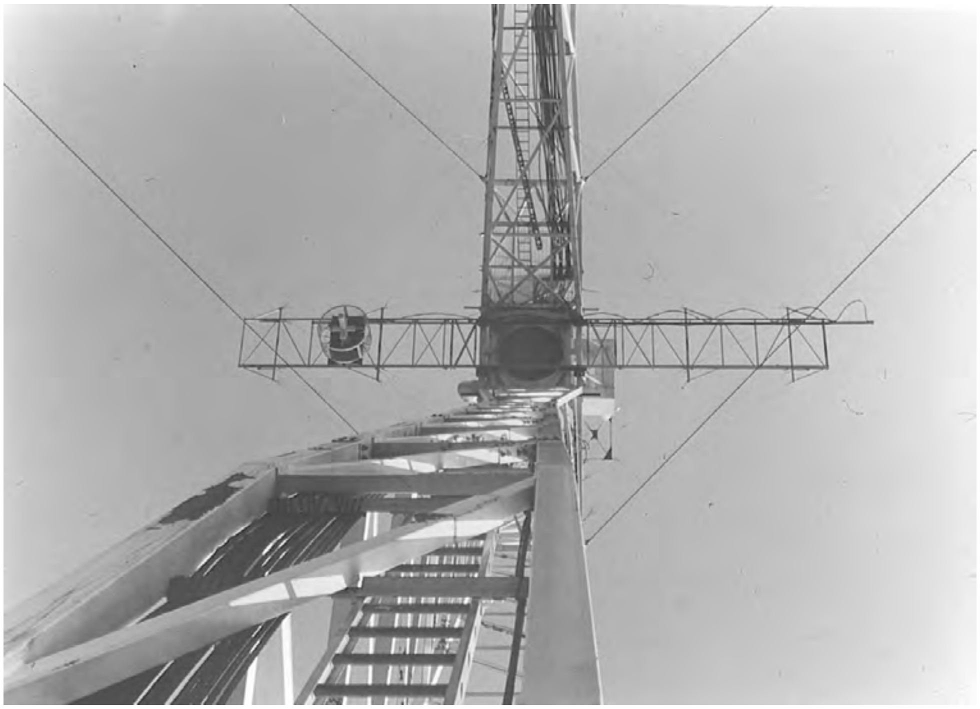
The noise the lady heard wasn't a lost jet airplane trying to land, but actually a lost jet engine de-icing the 300 Foot telescope.



Top: The original surface showing its poor condition in September 1966.
Bottom: Rolling over the original surface in an attempt to flatten it, October 1966.
Fred Cole is operating the roller.



Installing the first Travelling Feed System, September 1969. Wendell Monk (way up near the center of the dish, Ron and Don Gordon (mostly hidden behind the structure about half way up), Bedford Taylor and unidentified (on the travelling feed structure), Bill Brundage, Bob Viers, Charles Cassell, and Basil Gum (in front).



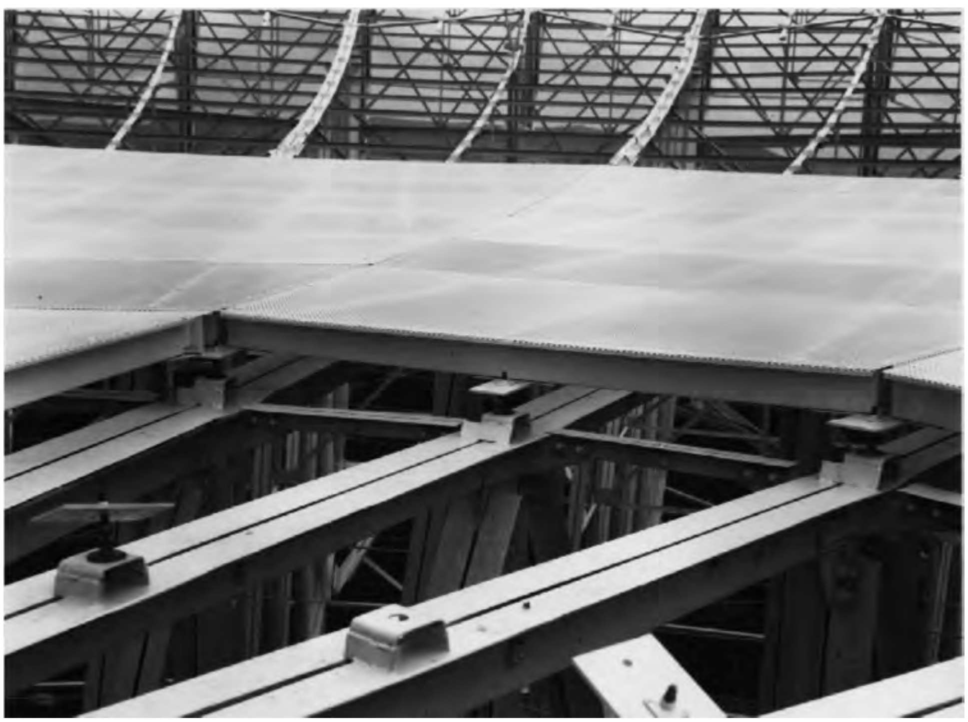
The Traveling Feed
Troy Henderson and Ken Cottrell
in the *Observer*, November 1969.

The recent addition of a traveling feed system has moved the 300 Foot a little further up the versatility scale. It could even be said that the 300 Foot is now encroaching on the celestial territory of its more fully steerable neighbors, the 140 Foot and the interferometer.

During the telescope's early days, the Naval Research Laboratory first applied the concept of a traveling feed to the 300 Foot. Their feed assembly was constructed so that radio sources could be tracked for some time in right ascension, thus increasing the possible observing time on any particular source.

Following the discovery of pulsars, in mid-1968 a visiting research group from Harvard made the proposal for a second traveling feed system, specifically for pulsar research. A joint arrangement for design and construction was worked out, and fabrication began at the NRAO shops on November 1, 1968.

The feed assembly consists of a set of rails and a feed carriage which is driven along the rails at various rates. The rails are 51 feet long allowing for 46 feet of carriage travel between limits. The completed traveling feed system was installed on the telescope for initial testing on June 27, 1969.



Top: New surface panels being tested for loading by ice, at Kennedy Antenna in Cohassett MA.
Bottom: New surface panels September 1970.

Installation of new surface in 1970.

Editors' note: With the new, more accurate surface, higher frequency work became practical. The efficiency at 5 GHz was about 35%. With the old surface, the antenna had been virtually useless above 1.4 GHz.

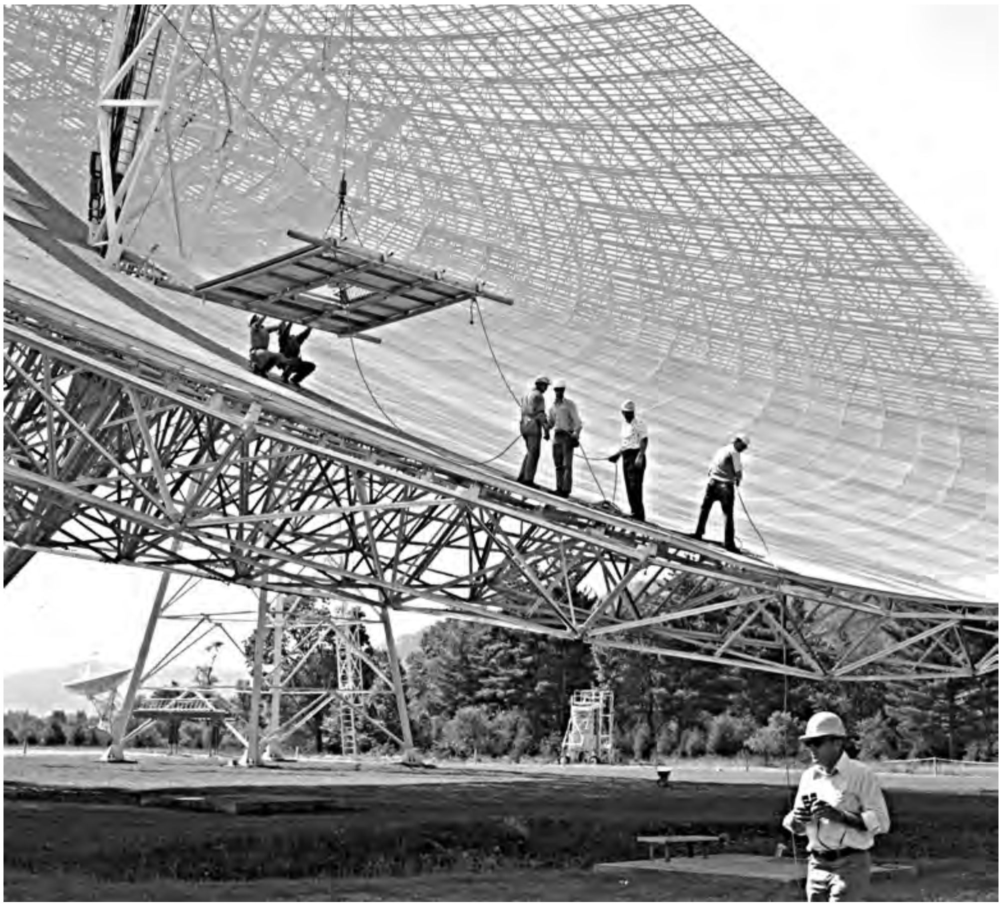
from the Observer for December 1972
by John Findlay

[During the resurfacing], two other changes were made to the telescope; one was that an entirely new variable speed drive system was added, and the second was that the final version of the traveling feed which increased observing time, permitted polarization measurements and also gave focus adjustment, was installed. This traveling feed was working by April 1971. Soon after, that is by November 1971, the new control building for the telescope had been completed and the move into it was made in mid-November. This new control building is an extra room of an area of about 1000 sq. ft. which is an addition to the old control room which is now mainly used for office space. The new control building is shielded by steel mesh to give protection from interference from many of the instruments within the control building. The new temperature control system was included and this for the first time has given the telescope a good environment for both the telescope operators and the electronics.

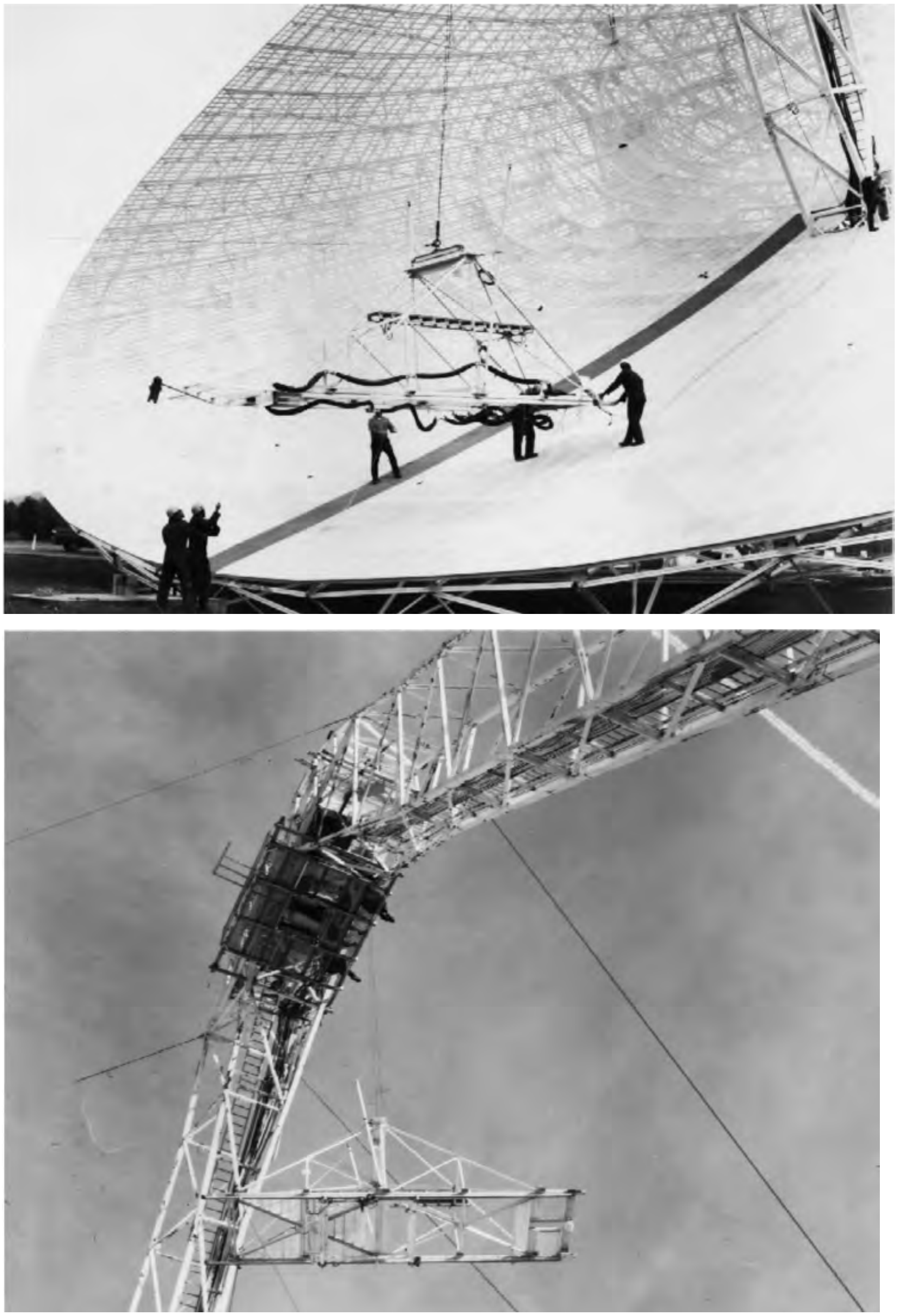
The new 300 Foot Telescope control building addition in November 1971. Left to right: Bob Viers, Troy Henderson, George Liptak, Al Hogan.

New telescope control console in 1972. Left to right: Bill Brundage (NRAO), George Resch (U.Md. student, later at JPL), Nancy Vandenberg (U.Md. student, later NASA/Goddard).

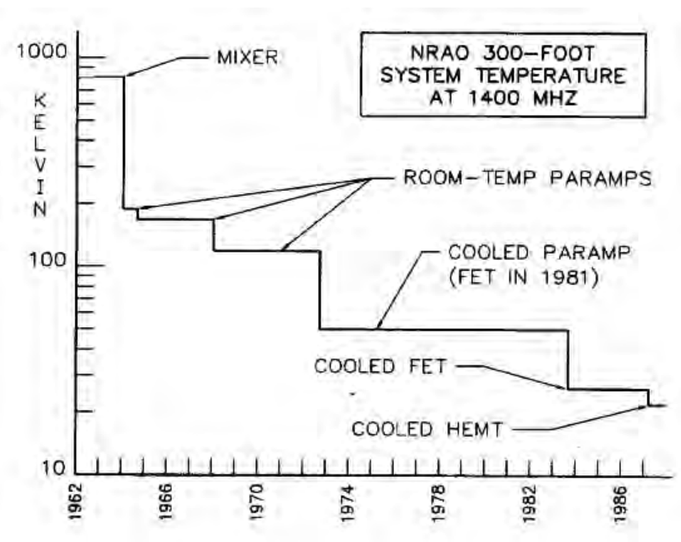
The telescope operators for the 300 Foot, Nov. 1971. Left to right: Bob Viers, Troy Henderson, George Liptak, Al Hogan.



Lifting the working platform, October 1980. This platform was put in place to provide a platform for the telescope mechanics to stand on while working in the prime focus area. Fred Crews in foreground. Ron and Don Gordon (holding the platform), Basil Gum, Wendell Monk, Al Hogan, Bob Viers (on the dish)



Installation of a stronger, more accurate traveling feed, October 1980.



Top: Telescope mechanic Don Gordon working 225 feet above ground on the installation of the improved traveling feed track.
Bottom: The evolution of receiver system temperature with time showing the effect of developments in electronics.

Technical Development

Events in the History of the 300 Foot Telescope*

September 21, 1962 — First observations occurred at 00:42. Dual frequency receiver (1410/750 MHz) at focal point.

By 1963 — A 20-channel, 95 kHz per channel filter bank spectrometer available.

March 1964 — Model I Autocorrelator spectrometer installed. It provided 100 lags over a 2.5 MHz band.

By early 1964 — Dual frequency receiver improved with parametric amplifier at 1410 MHz and tunnel diode amplifier at 750 MHz.

1965 — Initiated design of solid-state Standard Receiver for continuum observations.

Summer 1966 — New feed support legs installed and backup structure strengthened. Variable speed drive put in place.

Winter 1966 — Surface mesh removed and rolled in an attempt to improve surface accuracy.

October 1967 — The 4-channel, 21-cm receiver first used.

January 1968 — DDP-116 telescope control and data acquisition computer installed.

December 1968 — Model II Autocorrelator first used. It provided 384 lags with a 10 MHz bandwidth for two IF channels.

July 1969 — Traveling feed installed for receivers up to 500 MHz.

1970 — New surface. Cryogenic lines run to focal point.

1971 — Control building addition completed.

April 1971 — Model III Autocorrelation backend installed. This was similar to the Model II, but had four IF channels, and a synchronization system for pulsar observing. pulsars

April 1971 — Installation of tracking focus and rotation (Sterling) receiver mount.

May 1971 — First cryogenic receiver, cooled 18-cm, used.

December 1971 — The 3-feed, 11-cm receiver first used.

October 1972 — The cooled 21-cm receiver first used.

November 1974 — H-316 computer installed to position telescope, and control Sterling mount and traveling feed systems.

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* *From a handout at the 25th Anniversary Symposium*

October 1976 — The 6/25-cm receiver first used.

Summer 1977 — Modcomp II/25 analysis computer installed.

January 1979 — First inductosyn installed to replace encoders for position readout.

Fall 1980 — New, heavier traveling feed mechanism installed to accommodate cryogenic receivers that operate up to 1000 MHz.

May 1981 — The 300-1000 MHz receiver first used. First cryogenic receiver on traveling feed.

September 1983 — The 1.3-1.8 GHz receiver first used.

August 1984 — Installed a "spoiler" in the center of the antenna surface to reduce solar interference scattered by the feed support legs.

August 1984 — Added north-south motion capability to Sterling mount to compensate for deformation of the surface when the antenna is tipped.

August 1986 — The 7-feed, 4.8 GHz receiver first used.

May 1986 — Masscomp 5500 computer installed to replace DDP-116 telescope control and data acquisition computer.

April 1987 — The 300-1000 MHz receiver upgraded with FET amplifiers, and the 1.3-1.8 GHz receiver upgraded with HEMT amplifiers.

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300 Foot Telescope Operators 1962-1972

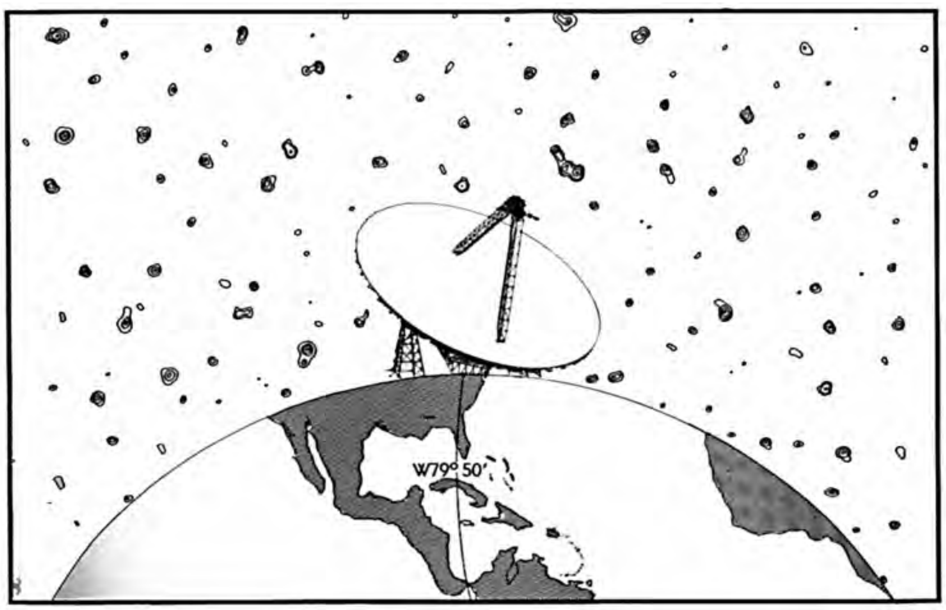
From the Observer for December 1972

George Grove	Roy Walker	Roy Paitsel
Robert Viers	Richard Spurlock	George Liptak
Omar Bowyer	Harold Crist	Al Hogan
Fred Crews	Dave VanHorn	Ken Cottrell
Troy Henderson	Don Cardarella	Bob Nichols
Bill Hunter	Leroy Webb	T. J. Gladwell
Ralph Hawkins	Shep Sutton	Roy Sharp
Darrell Southern	Ralph High	Dave Williams
Bob Vance	John Weaver	Jon Spargo
Richard Bird	Spencer Everly	Paul Giguere
Bill Terrell	Ralph Graham	Jaap Baars

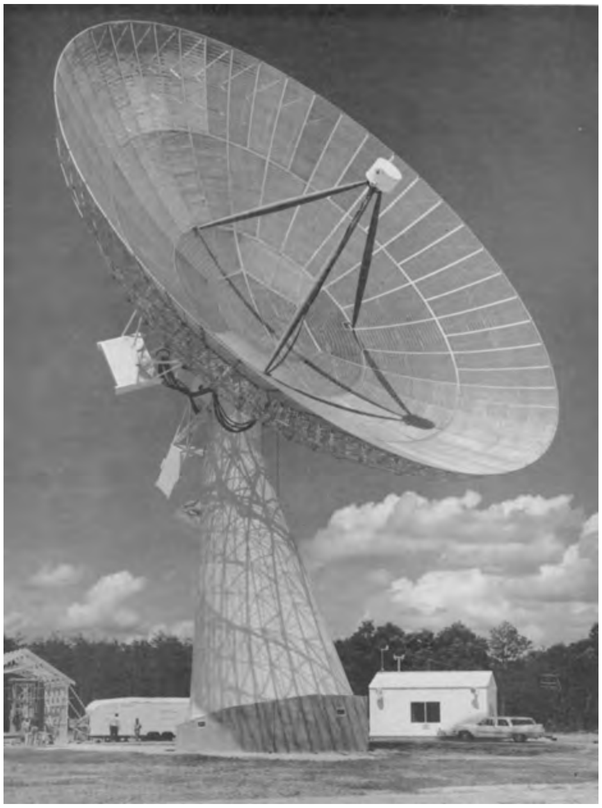
Left to Right: Alan Mollohan (Representative to the US Congress from West Virginia), Paul Vanden Bout (Director of NRAO), Kurt Riegel (NSF), George Seielstad (Green Bank site director), John Findlay (NRAO), Bob Hughes (President of AUI, at the podium). At the 300 Foot 25th Birthday Celebration, September 27, 1987.
[Courtesy G. Verschuur]

2 The Twenty-Fifth Birthday Symposium, September 1987

"A Quarter Century of Science on the Meridian"



Editors' note: A symposium was held September 26-27, 1987, on the occasion of the 25th anniversary of the completion of the 300 Foot telescope. Papers were presented on the origins of the telescope and on many of the early results and projects. On September 27, there was a celebration and an open house attended by the community as well as Symposium attendees and other friends of the Observatory. There were addresses by West Virginia Representative Alan Mollohan, and NSF and Observatory officials, followed by barbecue, blue grass music, and tours of the Observatory including the 300 Foot control room.



The Maryland Point 84-foot Telescope built by McClain and colleagues at NRL. It was the largest fully steerable radio telescope in the United States prior to construction of the Green Bank Tatel Telescope.

Ed McClain at the 300 Foot Silver Anniversary Symposium, September 1987.
(Courtesy G. Verschuur)

NRAO: A View from the Outside *

Edward F. McClain, Jr.
Naval Research Laboratory

My perspective on NRAO is that of an outsider. As everyone here knows, there is another installation across the mountain at Sugar Grove run by the Government, and in the 1950s it began to build a 600 foot telescope that would be good to 10 cm. That project, though perhaps not in its finally envisioned form, came about prior, I believe, to any thought of a National Radio Astronomy Observatory. Let me say at the outset that I was not involved in the intelligence aspects of that project, but as everyone knows, that project involved the Moon, and when the Moon wasn't up, the instrument could be made available for use by radio astronomers [see endnotes]. My function in the Navy was to try and see if it was possible to make that telescope available to the astronomical community.

In 1956 I became Branch Head of the Radio Astronomy Group at the Naval Research Lab (NRL). Prior to that John Hagen had done radio astronomy at NRL for some time. He knew Bart Bok and Donald H. Menzel and others at Harvard, and had become acquainted with people like Lloyd Berkner of Associated Universities, Inc., (AUI) and Merle Tuve of the Carnegie Institution of Washington. At that time, people at many institutions on the east coast (and possibly also on the west) had a general feeling that we needed to build a large antenna for radio astronomy. Now John Hagen knew what was going on in the intelligence community—they wanted to build a 500–600 foot dish for their purposes—and I am sure that is one of the reasons Lloyd Berkner wanted Hagen involved with AUI from the outset.

The first meeting that I went to in connection with AUI was on May 26, 1955, in Lloyd's offices in the Empire State Building in Manhattan. At that time I believe that AUI had a contract with the NSF to see what the possibilities were. I remember (though perhaps not from the very first meeting in New York) a table with an architects' mockup of a little 85 foot telescope, a second telescope around 150 feet, and an architects' display board showing a huge 600 foot telescope. A fellow named Jacob Feld, who was responsible for the display, had gotten a lot of publicity and I believe a design award for a 600 foot dish somewhere about that time, 1954 or 1955. At that time, it was decided that AUI should proceed and look for a site for a radio observatory.

On December 2, 1955, a number of people made a site selection trip to many places in the Virginia and West Virginia mountains. Now as part of my work for NRL, I had been on a trip to Burkes Garden, which is a bowl somewhere down in Virginia. It is a perfectly flat plain 2-3 miles across with a complete ring of hills around it. To get into it you came down a road and there were some tremendously excruciating hairpin curves. This little narrow road wound down into the perfectly level plain. Turns out it is full of sink holes and you can't build anything there. But my visit was part of that classified Government project.

* From an oral presentation made at the 300 Foot Birthday Symposium, September 1987. Ed McClain was Head of the Radio Astronomy Branch of the Naval Research Laboratory in the 1950s.

Artist's conception of the proposed 600 Foot antenna at Sugar Grove, WV.

I am possibly the only person here today who came through this Deer Creek Valley during that first trip. We had looked at a place near Front Royal over in the Massanutten Range, another near Deerfield, VA, as well as the Green Bank/Arbovale valley. I think that someone made a side trip to Burkes Garden. There were two groups of us as I remember. There was Dick Emberson and me and John Hagen and possibly Fred Haddock in one car. Merle Tuve, Howard Tatel and Bernie Burke were another group who came out of Roanoke Virginia. We met and discussed all these sites and went back to Roanoke.

It was decided as a result of that site survey that this particular valley was the place to put instruments. There was relatively level ground, with mountains on the west, mountains on the east, and it was within 300 miles of Washington, which was also a criterion.

So this site was picked, and there was then much activity out of New York: core drillings, surveys, options on the land, all the things that are involved in setting up a new facility. The West Virginia State Legislature passed a law giving the Observatory some protection from locally generated interference. In that connection I got back in the loop again. I got a call from someone, I forget who, asking if I would come down and meet Dave Heeschen and bring some kind of equipment so that we could claim that we made astronomical observations in the valley. I got in touch with Dave, got an old Hallicrafter receiver and a long length of antenna twin lead, came out, and we made two, I believe, folded dipoles cut to around 30 MHz, and ran wires back up to the Hannah House. We set the Hallicrafter up there along with an old Easterline Angus recorder, and tried to watch the sun come up. This action put the law into effect.

In the very early days of the first meetings that I went to it became obvious that Merle Tuve and Lloyd Berkner, who had known each other from the year one, had different points of view. They had both worked at Carnegie, they had been involved in various activities in World War II, but they were very different people. Tuve was, you might say, a renaissance man, the essence of a scientist, a sealing wax and string-type person. His attitude was: send the young fellows out in a field to build an antenna, turn them loose and let them work.

Lloyd Berkner, on the other hand, was probably the Barnum of scientific organization. Of all the people I have ever known in my professional life he was probably the best "operator"—and I use that word not in a disparaging sense—the best operator I have ever seen. For example, he knew that Tuve was going to oppose him on setting up NRAO. So what does he do? He gets Tuve on every committee he sets up. Brings him right in so that if he objects he objects as a member of the committee and not as an outsider. And he did this, I think, with a lot of people.

There was a lot of acrimony when the NSF contract was being discussed, because Tuve, I am sure, didn't want it to be awarded to AUI. Tuve and Berkner had personal differences, probably, as well as professional differences, and at one meeting I know one or both walked out of the room at NSF because it got so hot. But eventually things settled down and I guess Tuve accepted the inevitable and Lloyd was obviously an excellent choice. I don't think this place would ever have been here if Lloyd Berkner hadn't been the man running the show to begin with.

I was not involved closely with NRAO while the 300 Foot was being built. At that time, I was not too interested in NRAO because we had an 84 foot dish at NRL, while all that was here in Green Bank was the 85 Foot, and the 140 Foot which was having these terrible labor pains. Personally, I wanted to see some big antennas built by somebody and I didn't much care who, although a national center such as NRAO, where everybody could have access to the instruments, was desirable. It wasn't until the 300 Foot came along that we at NRL became interested in NRAO. It surpassed anything we had available to us. So we came out and did quite a bit of observing in 1963, 1964 and 1965.

Finally, the desire for a really large instrument began to flicker into existence again in the late 1960s. I did a stint for a couple of years as a member of the NRAO Visiting Committee. This was after the headquarters were moved to Charlottesville. One of the bones of contention between Tuve and Berkner was location of headquarters. Berkner said: build it at Green Bank, build the buildings, get the buildings built, for God's sakes, right off the bat. Don't worry about the telescopes, get all your public works done with the initial money. And that was the way it was done, obviously. Later, Tuve's correct idea of having a university nearby was met by the move of headquarters to Charlottesville.

The Interferometer was built at Green Bank and this was an introduction, I guess, to what was to come later, which some wiser heads than mine probably foresaw: the Very Large Array (VLA). When I was on the Visiting Committee in the late 1960s we talked primarily about the VLA. I really think that the 600 foot idea, this huge monstrosity, finally simmered down and became the array in New Mexico.

I will relate one interesting thing about the Sugar Grove 600 foot. The initial plans for this dish involved two different concepts. My side of the lab came up with a design by Ned Ashton, who was going to servo the structural members. This is, they would have sensing rods in the structural members which would actuate servo systems and keep the members at constant lengths. The other design used servoed panels where you have a multiplicity of panels and you servo them optically to a source in the focus. When the project got down to the Office of Naval Research it was declared to be a "structure" and not an "instrument," which meant that it fell under the authority of the Bureau of Yards and Docks. (When we built our second dish of 85 feet, I arranged that it would always remain an "instrument" and not get to the Bureau of Yards and Docks.) When the 600 foot got to the Bureau of Yards and Docks it was foredoomed. The first meeting with the Chief of Civil Engineering from the Bureau of Yards and Docks, who I am sure builds excellent yards and excellent docks, revealed that he apparently had no concept of a servo mechanism. He made some statements and I asked him what he meant and what he was saying, and he said "Are you an Engineer, are you a Civil Engineer?" And I said, "No, I'm sorry but I'm electrical." Well, he was incensed, but the man had no concept of what he was dealing with.

They started out to build the thing but didn't start at the focus and come down, as we radio astronomers know you have to do in order to find out how heavy your railroad tracks have to be. They just started building foundations and railroad tracks and substructures and finally when someone indicated what the weight was going to be up at the focal point, all this had to be scrapped and started over.

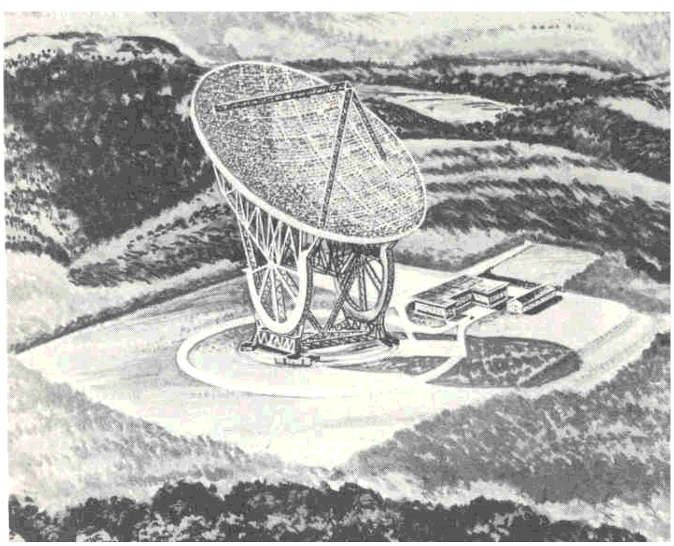
An occasion I will never forget was when I was down at the office of the prime contractor for the job in Washington, DC, and I heard one fellow say to another, "On projects this large you can always double the cost." Not only that, they just about quadrupled the cost before the project was abandoned, as I remember.

Editors' Notes

The 600 foot antenna project is described in detail by McClain in *Scientific American*, January 1960, **202**, p. 45. After the project was cancelled, The General Accounting Office issued a report which is discussed in *Science* with particular emphasis on the split that developed between the scientific community and the military construction group (1964, **144**, p. 1111). An engineering retrospective is given by Michael F. Wolff in *IEEE Spectrum*, October 1976, p. 89. The 600 Foot antenna is also discussed in relationship to activities of the National Security Agency in *The Puzzle Palace* by James Bamford, Penguin, New York, 1983. The function of the antenna would have been to listen to Soviet radio communications and radar signals reflected from the Moon.

A brief announcement in the October 1958 issue of *Sky and Telescope* noted that although the Navy's new research facility would be near Green Bank:

> *"The two installations are separate and have quite different purposes.... Considerable confusion was caused by a press release in June which suggested that the Sugar Grove facility was primarily for astronomical research. This misconception was corrected in the July hearings before a House of Representatives subcommittee on appropriations.*
>
> *"At these hearings, A Navy spokesman stated that information valuable to radio astronomy would result as a by-product of the new station's program.... The spokesman mentioned the possibility that the large instrument might be used in the reflection of radio signals from the moon for communications purposes."*



Dr. Gerald Tape, AUI President, and family visit Green Bank for the AUI trustees meeting in October 1969.

How the 300 Foot Affected AUI*

Gerald F. Tape
Associated Universities, Inc.
Washington, DC

I was really an observer (but not the astronomical type) when the 300 Foot Telescope was being designed and constructed. It was at a time when I was Deputy Director of Brookhaven National Laboratory and had little to do with the Observatory. I did attend all of the AUI Board meetings, and I heard the reports, principally by Lloyd Berkner, and listened to the discussions. Since I was only an observer, I decided to go back and review some of the Board's minutes, to make sure that my memory was not too far off.

I am reminded of the time that I was on the Atomic Energy Commission and the second volume of the "History of the Atomic Energy Commission" was being produced. All of the Commissioners had to review the draft before it went to press. I read it over and found sections that differed from my memory. I went to the authors and said, "That's not the way it happened." One author looked me in the eye and said, "Let me see your notes!" What notes? He said, "If you can't give me a piece of paper, substantiating your position, written at the time it happened, I won't even listen to you." So, I decided at least for this talk I had better make use of official notes written at the time.

Let me digress a little bit and give some background. Most of you know, but I think it's worth repeating, that Associated Universities, Inc., is a not-for-profit organization chartered under the State Education Laws of the State of New York. It has a board of directors, like any corporation. It has officers, employees and so on. Now, if you think about a corporation, it is the board of directors that has the ultimate responsibility for the activities of that organization. So, in that sense, our Board of Trustees is the ultimate manager, not for day-to-day operations, but in a policy sense. But we very carefully try to give a lot of responsibility to the Directors of the operating institutions in managing their respective endeavors. Although AUI has a number of objectives, its primary objective is to provide major user facilities on a national basis, and that goal has remained throughout the years of the organization. Brookhaven had its 40th Anniversary just two weeks ago.

For NRAO, the AUI contractual arrangement is with the National Science Foundation (NSF). The original contract was signed in 1956. To a considerable extent, the success that occurred at BNL in its first nine years was very persuasive to the NSF. The center concept was a good idea, and Berkner, with his drive, was

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* Edited transcription of comments made at the 300 Foot Birthday Symposium in September 1987. Gerald Tape (1915-2005) was a physicist who worked on the development of radar at the MIT Radiation Laboratory during World War II. He was acting director of Brookhaven National Laboratory in 1961 and president of AUI 1962-1963 and again in 1969-1980. He was a member of the President's Science Advisory Committee, chair of the Central Intelligence Agency's Nuclear Intelligence Panel, a Commissioner on the Atomic Energy Commission, and the US representative to the International Atomic Energy Agency with rank of Ambassador 1973-1977.

able to convince the Foundation that a radio astronomy center should be established under contract with AUI.

I would like to start in 1958. I guess John Findlay had been around here for about a year or two by that time. In looking at the situation in early 1958, AUI had a contract with NSF for NRAO. The preplanning exercises and the work that went on prior to the signing of the actual contract for the establishment of the Observatory had the general direction pretty much in hand. The concept of the 140 Foot Telescope was certainly in hand at that time. There were AUI advisory committees considering the kinds of instruments that would be suitable and appropriate for an institution such as NRAO.

The Advisory Committee played a very prominent role during those days, and I might say the tendency to utilize advisory committees is one that has stayed with AUI through the years. For new endeavors, the attitude is to bring the community in, get the best community advice you can, and plan in the interest of the totality of community, not just a small segment of it. Advisory committees through the years have been formed to address specific areas or topics, for example, the Visiting Committee, the User Committee, and ad-hoc review committees with which you are all familiar.

At that time, Lloyd Berkner was Acting Director of the Observatory as well as President of AUI, and although a search for a Director was high on the priority list of the Corporation, it went on for some period of time and Lloyd continued as NRAO Acting Director. AUI at that time had a New York Office that was basically the President's office. The Staff Officer there was Dick Emberson; the old hands here today will remember him. I was personally acquainted with both Berkner and Emberson because we had worked together during WWII. Emberson and I were at the Radiation Lab at MIT, and Berkner was in the Navy in charge of the Aeronautical Electronics Branch. He used to come to the Radiation Lab and push us to move forward on some of the developments for Navy Air.

At the same time, with Green Bank getting under way, all the astronomers (I think John Findlay will let me call him an astronomer at this point) were here. Those who took the responsibility for the development of the site and building the new instruments were in New York City, whereas those responsible for getting the science started were in Green Bank. I must add, they were getting science started without any major instruments. Later on the 85 Foot Telescope came along, but that was hardly a user instrument. It was a good idea, at least in the eyes of the Board, to bring the 85 Foot on-line, simply because one needed to get research going for the astronomers at the site. That was not a new idea for the Board. Going back to the early Brookhaven experience, Brookhaven didn't have any "big" facility to start with. The user concept had been formulated, and it was being implemented. They were designing and building an accelerator and a reactor. But what did the people, the scientists on site and the users, do in the meantime? There were 60-inch cyclotrons and 3MeV Van de Graaff proton accelerators available in some universities, but in order to get nuclear research underway, they were also built at Brookhaven. Cosmic rays were generally available, and so for a while a number of the high energy physicists did cosmic ray research until the new instruments came along. So the concept of getting research going early was started at Brookhaven and then followed at Green Bank.

Let's look at a bit of the chronology from 1958 on. I would like to read a couple of excerpts from Board meeting minutes. In April of 1958, you will recall, the 140 Foot Telescope was the primary instrument for which everybody was trying to get the design done and construction started, the funding secured, and so on. The cost estimates were all over the map from low to high. The Science Foundation really was not, at that time, that well acquainted with the acquisition and funding of big facilities. There were money problems. I noted that "The Trustees discussed the problems created by the delays in receiving funds for the 140 Foot Telescope and the rest of the then current construction program. Mr. Berkner said that, if there were substantial delays, perhaps several months, it might be necessary to reappraise the situation and perhaps change the entire plan for the Observatory. Can we really rely on the 140 Foot to carry the goals which we want or are we going to have to look at other situations?"

All through those minutes you will find references to the Advisory Committee and the work of the Advisory Committee. And in the reports that went to the Board, I was struck by the fact that I kept reading about the VLA — that was in 1958! But then I realized that they were referring to the VERY LARGE ANTENNA! The very large antenna that was being talked about always came in as at least 1000 feet in diameter, to operate at about 21 cm, and usually was referred to as "fixed." Nothing was said about being steerable. The question: do we really want or need something even larger than 140 feet, kept coming up, but there was that background noise you always hear that says, "Why don't you finish what you are working on and get some experience before you go to the next stage?"

Then the notion of a fixed antenna, a zenith antenna of 200 to 300 feet was presented. It overcame the dilemma that one can't [propose an antenna of] 1000 feet unless one has experience in building a smaller instrument. So we asked the NSF for funds to build a fixed zenith antenna with a diameter of 200 to 300 feet. This instrument would provide valuable experience for the design and construction of a very large antenna. We were trying to keep the door open for the large instrument.

Then there was another reference that struck me. It came up in a different way later on. There was a large Navy project at Sugar Grove, a 600 foot antenna. The project was highly classified and we had no idea what it was for. But it raised the question: couldn't one sneak a little research time in for the astronomers? Sugar Grove is not that far from Green Bank, so the question had to be explored. By this time Otto Struve was Director of NRAO and he recognized, and the scientists here recognized, that it was going to be very difficult to work with a classified project. You don't know when you will have observing time and you don't know whether you will be able to look at the sources you want to look at. There were a whole lot of complications. I expect that, from the Navy's point of view, they didn't want outsiders there anyway. Questions like this came up and had to be answered.

The final decision, which Struve made and with which the Board concurred, was not to pursue use of the Navy instrument. If there is merit in building a 1000 foot antenna, it was thought that we ought to do it for the science program itself and not get involved with the other projects. In the following year, I noted that there was general debate as to what a large antenna might look like, but by July of 1960, we actually came up with a firm request for a 300 foot antenna. And here my notes from reading minutes say that John Findlay reminded the Trustees of the Stanford Research Institute design work. To me, that was the first reference to abandoning the zenith instrument and developing something that was more like the meridian instrument. I am sure the discussions were hot here at Green Bank, but, as far as the Board was concerned, that was the first indication of it. From there on in, we went through a time of budgetary planning, designs going out, cost estimates coming in, and so on.

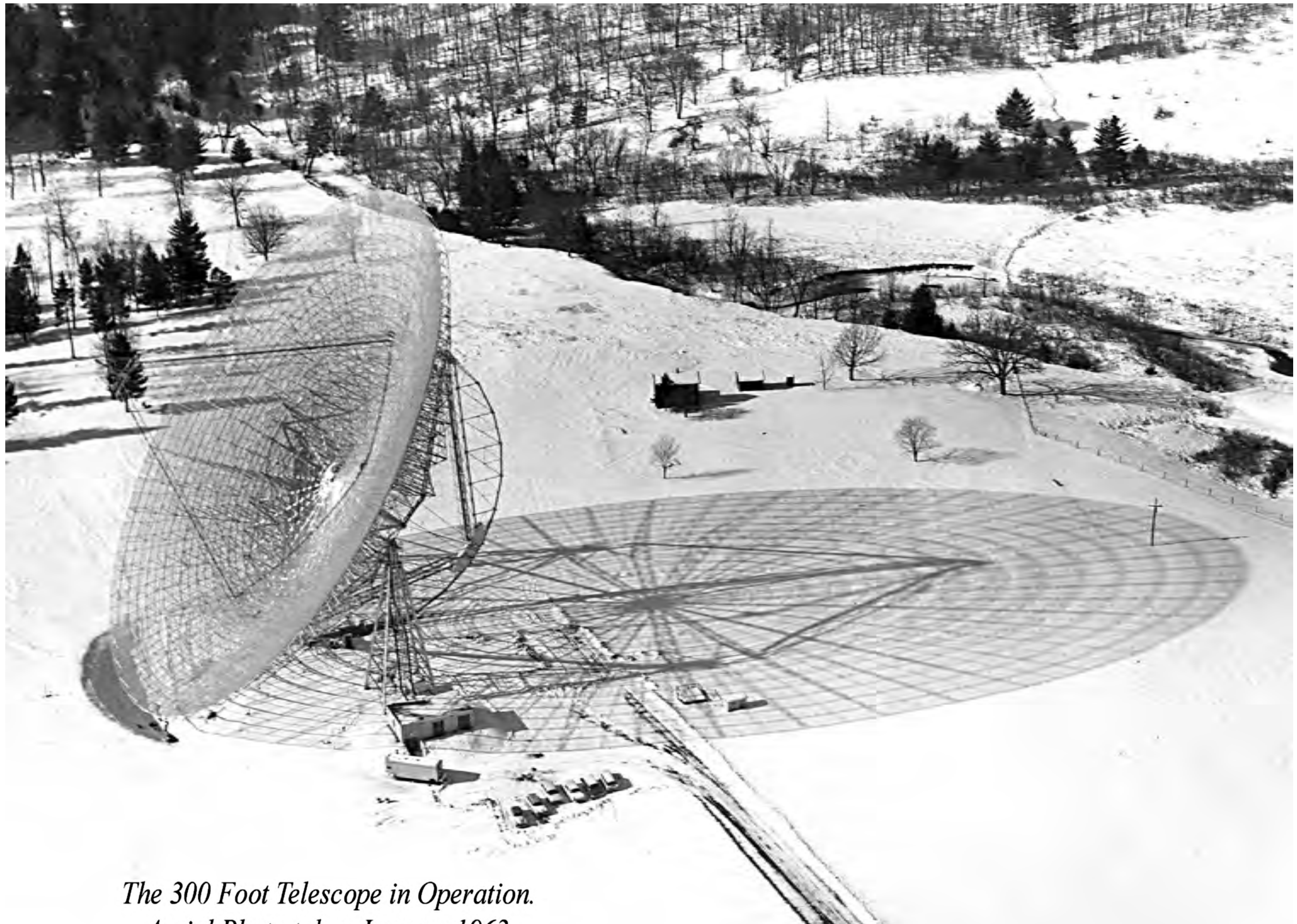
Let me then go back a little bit and take an overview of what I have been talking about. Before I do that, I might say that there were other notes in the Board Minutes that said, "Our congratulations on the progress." I thought that was a nice little addition that the Secretary included.

I remind you that Berkner was Acting Director until July 1959 when Otto Struve became Director. Otto was here until December of 1961, at which time Dave Heeschen took over as Acting Director, and then became Director in October of 1962. During that same period, Berkner left as President of AUI (in November 1960) and there was a series of Presidents — Haworth, Rabi, Tape and Reynolds. And it was during that time that the 140 Foot issue was really coming to a crisis. Ted Reynolds, one of the Trustees from Harvard University, took the Corporate responsibility for the 140 Foot project. There were changes in overall leadership at the AUI level and at the Observatory. But I personally have to say that, when things settled down and Dave became more involved and eventually became NRAO Director, things progressed effectively. The 140 Foot got under control and the 300 Foot came in as a user instrument. The 85 Foot Tatel Telescope, as I said, was really not a user instrument. Why should astronomers from universities with such capability available at their own institutions come here? The 140 Foot, the primary goal of those early days, was long delayed and it was an embarrassment. The real user instrument then was the 300 Foot Telescope, which came on-line in 1962.

I can't close without saying to John Findlay that there certainly was skepticism expressed in the Board meetings. I didn't see that in the minutes, John, but I do remember it. For example, there was doubt that the 300 Foot would even stand up for the five years you were talking about. And it was intentionally, I hate to say, referred to as "quick and dirty," but it was at least quick. And when it was completed and Dave announced the first observation at an Executive Committee meeting, everybody was relieved. It was really recognized as a great success.

We used to have our Board Meetings in October at Green Bank. It was a nice place to come to in October and everybody enjoyed meeting here. It was at that October Board meeting of 1962, as I recall, that we had a chance to see the finished instrument. We were all invited to come to the dedication to be held one week later. I came, and I was happy that I came. And I am happy that I am here at the Twenty-Fifth Anniversary today.

(facing page) The 300 Foot Telescope in operation.
Aerial photo taken January 1963.



The 300 Foot Telescope in Operation.
Aerial Photo taken January 1963.

David Heeschen in 1968.

Groundbreaking for the 300 Foot Telescope, April 27, 1961. Bernie Burke (left), and Otto Struve (with shovel).

The 300 Foot Telescope and the National Center Concept*

D. S. Heeschen
NRAO
Charlottesville, VA

When I agreed to give this talk I thought I would simply reminisce about events as I remember them that led up to getting the 300 Foot, and about the atmosphere and attitudes vis-a-vis national centers that prevailed then. I thought I could do this without any effort on my part simply by depending on my memory. That turned out to be a mistake. It doesn't work. I do remember that I got up at 7:00 this morning, but I can't remember much about what happened earlier than that. So I had to go back to the old files, and put together a verbally annotated chronology, beginning in 1954. Figure 1 gives the chronology.

I will start with a word about the 300 Foot and the concept of a national center, because I think the real motivation for the 300 Foot was AUI's concept of a national center that Jerry Tape has described to you already—the idea of building and operating major facilities for all users. That concept, coupled with the perception at NRAO in the late 1950's of how well or how poorly NRAO was living up to the concept was, I think, the driving force behind the 300 Foot. The concept goes all the way back to the group of people who originally approached AUI with the idea of setting up the NRAO.

A meeting on radio astronomy, held at the NSF in January 1954, helped focus attention on future instrumentation. Around about that same time various people in the East had been talking about the need for a large radio telescope. What they had in mind was larger than what they thought would be appropriate for any one of their individual institutions. They just didn't want to be saddled with it themselves.

These people included Bart Bok and Donald Menzel from Harvard, John Hagen from NRL, and Jerome Wiesner from MIT, and there may have been others as well. But those four approached AUI, which was already operating the kind of national center that they envisioned, to see whether AUI was interested in developing a national radio facility, and of course AUI was. In May 1954 AUI was asked to do a feasibility study. As I'll try to describe, this was the beginning of the 300 Foot. But the original goal of that group was not a 300 Foot telescope. They wanted something much larger. I think 600 feet was specifically discussed. A very large fully steerable dish was the primary goal of those astronomers at the time. They talked about starting with a smaller dish just to get going and have something to do while the larger instrument was being built. This was basically the idea that they approached AUI with. AUI submitted a proposal for a feasibility study to the NSF. It took the NSF a long time to decide to fund that proposal, and it wasn't until February 1955 that AUI received the grant. While waiting for the grant to be approved, AUI used its own funds to set up an advisory committee and get started

* From a presentation at the 300 Foot Birthday Symposium, September 1987. Controversies surrounding the initial instrumentation for NRAO are discussed further in Heeschen's article on page 265.

1954 – Jan.	**NSF Conference on Radio Astronomy in D.C. Discussions of Need for a "National Facility".**
1954 – May	**AUI Asked to do Feasibility Study for a RAO.**
	Goals: National Facility open to all.
	600-Ft or Larger Dish
	Start with Smaller (150-Ft) Dish that can be Quickly and Easily Obtained.
1954 – July	**Proposal for Feasibility Study Submitted to NSF.**
1955 – Feb.	**AUI Receives Study Funds.**
1955 – Mar.	**Steering Committee Recommends Major Instrument be a Large (≥ 600 ft.) Reflector, but also Recommends "First Telescope be a Reflector of about 150-ft. Diameter, Because it Could be Constructed Relatively Quickly and Establish the Facility as an Operating Institution."**
1955 – July	**Steering Committee Recommends... 2. The initial major instrument ... will be a precision 140-ft parabolic reflector designed to provide a radio telescope of maximum flexibility of utilization. ... 4. At some future time (5 to 10 years), a much larger radio telescope of perhaps 600-ft aperture may be erected at the facility, if considered essential for continued advance in the science of radio astronomy.**
1956 – Nov.	**AUI signs 5-yr contract with NSF for construction and operation of NRAO. Contract called for site acquisition, buildings, 140-ft construction, and operations. (But no other telescopes, and NSF tells Congress NRAO will not grow).**

Fig. 1— Early chronology of NRAO.

on some preliminary design work. I was not yet involved with AUI and was unable to find much about this period in the files available to me, but I believe most of the effort went towards design of a steerable telescope 600 feet or larger in diameter.

In March of 1955 the steering committee made a formal recommendation to AUI that the major instrument be a large, greater than 600 foot, reflector. The committee also recommended that the first telescope be a reflector of about 150 foot diameter, because it could be constructed relatively quickly, and would establish the facility as an operating institution. The time scale envisioned for the 150 Foot was about a year to a year and a half. It was to be an "off the shelf" telescope. That was in March. Four months later, in July, the steering committee issued a second report in which it recommended that "the initial major instrument will a precision 140 foot parabola. . . . " The committee also recommended that at some future time, perhaps five to ten years (not very far in the future in terms of what we think of today as the time scale for development of a new instrument), a "much larger radio telescope of perhaps 600 foot aperture may be erected if it is considered essential."

Now if you look at these two statements, it is clear that there was a big shift in emphasis in just four months time. That shift paved the way for the 300 Foot. The view of the Foundation apparently was that AUI's efforts should be concentrated on a more modest plan, with particular emphasis on the design for

Principal NRAO Priorities 1957 – 1962

1. Build 140 Foot.
2. Develop Green Bank Site and Facilities.
3. Design Very Large Antenna.
4. Establish Concepts and Practices of a National Center, Gain Acceptance of NRAO.

Actual Situation Late '57 – Early '58

1. 140 Foot Behind Schedule and in Trouble.
2. NSF Not Interested in Developing Very Large Antenna.
3. NRAO Not Functioning as a National Center.
4. European and Australian RA Flourishing.
5. U.S. had CIT Interferometer, Several 60–85 Ft. Dishes, Some Plans (OSU, Arecibo, Stanford).
6. NRAO Scientific Staff (Drake, Findlay, Heeschen) and Some Others Wanted to Fill the Gap Until the 140 Foot and 600 Foot were Available – Something that would be Competitive and make NRAO a National Facility.

Fig. 2— The priorities versus the reality.

an intermediate size telescope with a diameter of 140 feet. Apparently NSF and its advisors felt that the March proposal for a 600 foot telescope plus an "off the shelf" 150 foot (there were other elements of the plan too, including several 60 foot dishes) was too grandiose. It just wasn't going to sail—the NSF advisors weren't buying it—so sometime between March and July the NSF told AUI to back off and try something more modest. AUI of course complied, but this 600 foot or larger steerable telescope remained as the major goal of AUI and the advisory committee for a long, long time, as Jerry Tape noted in his presentation. AUI, and NRAO after it came into existence, annually requested funds from NSF for this large telescope but the NSF remained cold towards it. Eventually it became our LFST (largest feasible steerable telescope) project, and finally died. Meanwhile, by late 1955, the contract was signed that brought NRAO into being. That contract called for site acquisition and buildings, and construction and operation of the 140 Foot Telescope, but it made no mention of other telescopes. I remember our dismay when we saw a record of an NSF hearing during which the NSF director told Congress that NRAO would not need anything beyond the 140 Foot Telescope.

During the next few years our principal concerns at NRAO were, as I remember: 1. Build the 140 Foot telescope. (This was in the hands of the AUI group and not in the hands of NRAO); 2. Develop the Green Bank site and facilities; 3. Design a very large antenna (that was still a primary goal of NRAO and of the Visiting Committee and the advisory committee throughout this period); 4. Establish the concepts and practices of a national center. I think that was explicitly mentioned in reports that we wrote and presented to groups such as the Visiting Committee and the AUI board; how should a national radio astronomy center behave? So we thought these were the priorities of NRAO in this period. They're listed in

Figure 2. But the actual situation, in 1957 and 1958, also listed in Figure 2, was dismal. First, the 140 Foot was behind schedule and in trouble. It was in trouble financially, and its design was in trouble. Nobody knew exactly what was going to happen. Second, the NSF had expressed no interest in developing the large antenna that was to be the centerpiece of the NRAO. We just hadn't gotten anywhere with that. Third, NRAO was not functioning as a national center. We had an 85 Foot telescope under construction (completed in 1959), but it wasn't a major national facility of the type that we and AUI thought we were supposed to provide to the community. In comparison, radio astronomy was flourishing in Australia and England. There was the Mills Cross at Fleurs, the Chris Cross at Fox Hill, the 3C and 4C surveys in Cambridge, and so on. Things were really perking along there. In the United States the CalTech interferometer was just getting underway. That was the only really major new thing going on. There were several 60 to 85 Foot dishes around, and there were some plans at Ohio State, Arecibo, and Stanford, but that was about all. So NRAO's priorities weren't doing well, and U.S. radio astronomy wasn't doing well.

I think the scientific staff at NRAO (which consisted of Drake, Findlay, and Heeschen) and some of NRAO's advisors felt a need to change things. We were having trouble with the 140 Foot, and we didn't know when the large antenna was going to come, so the idea of something to fill that gap developed. As nearly as I can remember, that's how the desire for the 300 Foot at NRAO came about. The idea of using it as a test bed for a larger instrument was certainly part of it, but I doubt if it was our major objective. I think we simply wanted a telescope that would be attractive to visitors and to us, and would fill a gap until we had the large antenna which we all thought we would eventually have, or if not that, at least the 140 Foot, someday.

In May of 1958 we submitted a budget request to the NSF, (for FY1960) which included $300,000 for a 300 Foot fixed zenith antenna. That's the first mention of this telescope in a budget. The NSF wasn't very interested in it in that year, and it didn't survive. In May of 1959 we resubmitted it for the FY 61 budget. In July of 1959 Otto Struve became the director of the NRAO. In retrospect I think his influence on the 300 Foot was extremely important. He adopted it enthusiastically. And I believe largely as a result of that, it was favorably treated by the Visiting Committee and by the NSF. After all, up to that time, there were only Drake, Findlay and Heeschen, and we weren't dry behind the ears. We didn't have any great reputations, and weren't able to sell something to the community, but Otto was someone else. He had a massive reputation, as you all know, and the fact he didn't know anything about radio astronomy didn't matter. He had a good sense for science, and everybody knew it, so if he wanted the telescope it must be all right. I clearly remember that the Visiting Committee had been very lukewarm to the telescope the year before 1958. In 1959 the committee was still pretty lukewarm to it, but by golly, if Otto wanted it, it must be a good thing to have, and the Visiting Committee, in fact, endorsed it that year.

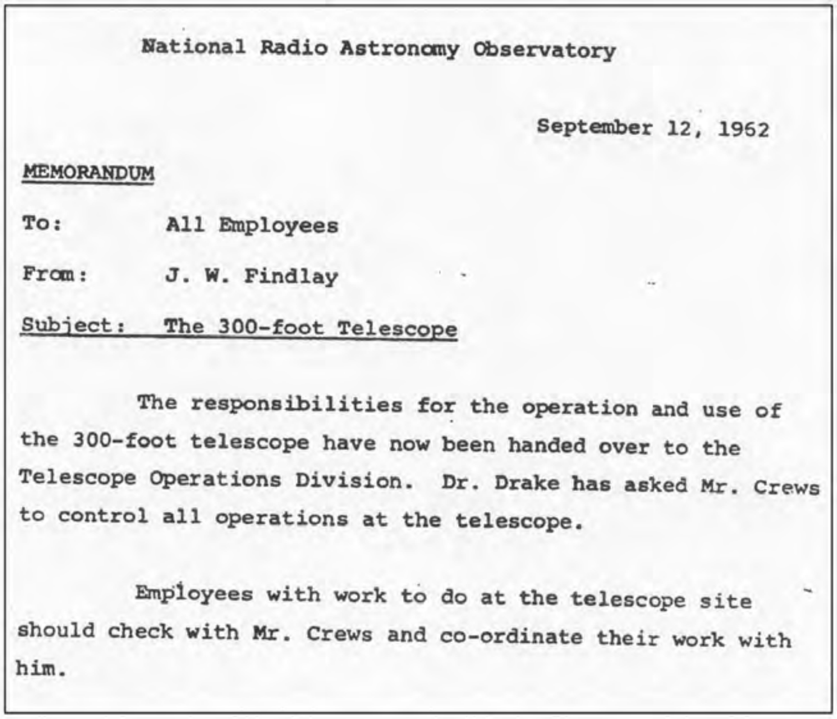
Now by this time we began to think that the 300 Foot really was going to survive the budget process and be in the NSF budget for 1961, so we chose a site. We originally chose it north and west of the Hill house, for reasons I no longer recall. Then in January 1960 the telescope was in the budget that eventually went to Congress, and it did get funded. Somewhere about this time Findlay began to

1958 – May	FY60 budget request includes ... fixed zenith antenna with 300-ft diameter; cost $300,000.
1959 – May	FY 61 budget request again includes 300-ft.
1959 – July	Otto Struve becomes Director of NRAO, strongly endorses 300-ft.
1959 – Sept.	Visiting Committee endorses 300-ft.
1959 – Dec.	Site chosen, N & W of Hill House.
1960 – Jan.	300-ft included in President's FY 61 budget.
1960 –	Findlay leads studies of various design concepts. Specs: 21 cm wavelength, "good" sky cover, "cheap", 5-yr life expectancy, 12 months construction time. Possibilities: Fixed spherical dish, SRI parabola, zone plate antenna, alt-az with limited motion.
1960 – Oct.	300-ft "transit" concept chosen.
1960 – Nov.	Bob Hall designs 300-ft transit telescope.
1961 – 1962	Design and construction of 300-ft. Findlay project manager.
1961 – Apr 7	NRAO requests NSF approval of contract for 300-ft.
1961 – Apr 11	NSF Approves. 4 days for NSF approval!
1961 – April	Construction begins.
1962 – Feb.	Hvatum memo outlines RX plans: 750/1400 MHz continuum Rxs 20 chain filter Rx Correlation Rx Dig. output on punched paper tape.
1962 – Sept 12	300-ft completed; Findlay formally hands it over to telescope operations group.
1962 – Sept 21	First Observations by Drake and Grove at 0042 EST – Champagne at 0130.
1962 – Nov.	First visitor programs – Burke and Turner, DTM.

Fig. 3— Early chronology of NRAO (continued). Grove, George

lead various design studies and explore various concepts. That is his story, which he will tell himself [Findlay's article is on page 145]. The specs as I recall them were for a 21 centimeter wavelength instrument which would have good sky coverage, be cheap, have a five-year life expectancy, and could be acquired in 12 months. And all of these except the wavelength and sky coverage requirements were built around the concept that the thing was a stop-gap measure until we got the large telescope. This was just something to have around for a little while, to truly get us going as a national facility. It would have as much collecting area as anything available and therefore be attractive and useful to many scientists. In October 1960 the transit concept was adopted. In November Bob Hall and his group designed the telescope. Again this is John's story and he will tell it.

Now I'll mention a couple of minor points that I find interesting. The whole project was fun, and done on a short time scale, but one time sequence in particular really boggles my mind. On April 7, Struve wrote a letter to NSF requesting approval for the 300 Foot contract with Bristol Steel company, and on April 11 he got an affirmative letter in reply! The U.S. mails can hardly even deliver a letter



National Radio Astronomy Observatory

September 12, 1962

MEMORANDUM

To: All Employees

From: J. W. Findlay

Subject: The 300-foot Telescope

The responsibilities for the operation and use of the 300-foot telescope have now been handed over to the Telescope Operations Division. Dr. Drake has asked Mr. Crews to control all operations at the telescope.

Employees with work to do at the telescope site should check with Mr. Crews and co-ordinate their work with him.

Fig. 4— Formal end of 300 Foot construction.

one direction in 4 days anymore, while to get an approval through the NSF system these days is a major undertaking—more like four months than four days.

I found a memo in the files that Hein Hvatum wrote in February 1962—Hein was already head of Electronics by that time—in which he outlined receiver plans for the 300 Foot. They were: 350/1400 MHz, 20-channel filter receiver, and a 21 cm correlation receiver, all with digital output on punched paper tape. Incidentally, in going through the files I found in a Trustees' meeting report (this has nothing to do with the 300 Foot) a statement that we had been approached, in 1958 or 1959, by a company that wanted time on the Tatel 85 Foot to test whether a company, for its own gain, could use a national telescope. It was finally referred to somebody at Brookhaven because they had already had experience along those lines. I don't know what the outcome was.

When the telescope was completed, Findlay formally handed it over to the telescope operations group via this memo, Figure 4. Fred Crews was head of telescope operations then, as he is today. The first observations were on September 21 at 0042 and they all had champagne at 0130 (Findlay claims it was beer). From September 21 until sometime in November, there were tests and some staff programs run, but the first visitor programs were in November, and the very first one was Burke and Turner. They did a hydrogen line study using a receiver they brought

OPERATIONS OCT. 23 THRU OCT. 31, 1962, at 85' and 300' TELESCOPES

		Oct. 23 Tues.	Oct. 24 Wed.	Oct. 25 Thurs.	Oct. 26 Fri.	Oct. 27 Sat.	Oct. 28 Sun.	Oct. 29 Mon.	Oct. 30 Tues.	Oct. 31 Wed.
85'	0000–0800	No Observing								
	0800–1600	LH	JFC	RB	RB	RB	RB	RB	RB	JFC
	1600–2400	BV RS	BV RS	BV RS	BV	BV	BV	BV	HB RS	HB RS
300'	0000–0800	HB	HB	HB	LH	LH	LH	LH	LH	LH
	0800–1600	JFC	TH	TH	TH	TH	TH	TH	JFC	GG
	1600–2400	GG	GG	GG	GG	GG	GG	OB	OB	OB

LH – Leonard Howell
JFC – J. Pred Crews
RB – Richard Bird
BV – Bob Vance
HB – Howard Brown
RS – Richard Spurlock
TH – Troy Henderson
GG – George Grove
OB – Omar Bowyer

Fig. 5— An early 300 Foot Telescope operators' work schedule.

with them from the Carnegie Institution of Washington. Figure 5 is an early telescope operators' work schedule. You can see from this who was operating the 300 Foot in October of 1962, and several are still here in Green Bank today.

I have one other thing to show. Figure 6 is the first observing schedule for the 300 Foot, for the period September 27th until October 20th. It doesn't look much like a modern schedule, and curiously there aren't any names on it. However there was also a memo of the same date, September 27th, which outlined the interests of the various staff members (Figure 7). I've extracted from that memo the names of the people who were involved at that time. It includes the entire scientific staff and a couple of visitors, namely D. J. Crampin, F. D. Drake, J. W. Findlay, D. S. Heeschen, D. E. Hogg, B. Hoglund, H. M. Johnson, J. McLeod, I. Pauliny-Toth, V. R. Venugopal, M. Vinokur, and C. M. Wade.

I would like to say a little bit about how I think the 300 Foot really did affect NRAO in teaching us how to behave in the manner in which AUI expected us to behave as a national center, and also the impact I think it had on the community. Nowadays most astronomers, and certainly most of the people in this room, have grown up with the concept of national centers in astronomy, where the equipment is there for you, and the help is there for you, and you just go and do your own thing. A couple of generations have now grown up with that concept and take it

300-Ft. Observing Schedule

September 27 - October 20, 1962

All times Local Sidereal Time

0000 - 0148	Galaxies
0148 - 0300	Galactic nebulae
0300 - 0345	Nova Persei
0500 - 0730	Supernova remnants and HII regions
0730 - 0930	Maintenance
0930 - 1012	Elliptical galaxies
1012 - 1030	Uranus
1100 - 1130	NGC 3587
1200 - 1333	Galaxies and Coma Cluster
1333 - 1400	M3
1548 - 1612	T CrB
1620 - 1700	M13
1800 - 1900	Novae
1945 - 2005	NGC 6853
2005 - 2110	NGC 6888, HB 21, Odd numbered dates Cygnus X, Cygnus Loop, Even numbered dates.
2120 - 2145	Extragalactic sources
2200 - 2230	Jupiter
2230 - 2300	Magnetic Stars
2300 - 2330	Planetary nebulae

Notes:

1) On September 25 Local Sidereal Time = Local Civil Time. On October 20, sidereal time will lead civil time by about $1^h\ 40^m$.

2) Calibration programs (pointing, sidelobes, beamwidth and shape, beam and aperture efficiency, thermal calibration) will be in progress until about October 7. These programs have priority over the above program, and the people in charge of them may take observing time when required.

3) There may be changes in the above program as experience and new ideas dictate.

Fig. 6— The first observing schedule for the 300 Foot Telescope.

NATIONAL RADIO ASTRONOMY OBSERVATORY

September 27, 1962

MEMO TO: Scientific Staff

FROM: D. S. Heeschen

I have divided the initial programs for the 300-ft. into several groups, and suggest joint work on them as follows:

1) Supernovae remnants (Cyg Loop, NGC 2359, NGC 6888, W 44, IC 443) and H II regions (Orion Nebula, Rosette Nebula, IC 1795)--- HMJ, DEH, MV, VRV

2) Galactic continuum emission (anti-center, spur, possibly a high or intermediate latitude region) —— JWF, CMW, IPT, BH

3) Galaxies --- DSH, CMW, J.McLeod

4) 3C Sources --- DSH, IPT, MV, DJC, CMW

5) Planets --- FDD, BH

This listing does not imply a hard and fast delineation of areas of vested interest, but it does put specific responsibilities on specific people for getting things accomplished. I'm sure there will be considerable overlapping of interests and, I hope, lots of interaction. In each of the above areas the first person named will be considered nominally in charge of that particular group.

In addition to the above, several smaller programs will be done:

1) Cyg X — FDD

2) Magnetic Stars — FDD

3) HB 21 — MV

4) NGC 2261 —HMJ

5) Globular Clusters -- HMJ

The 3C Source program will probably not be done for one or two weeks, at which time it will become the first-priority program for a period of about a week.

Calibrations are the responsibility of FDD (positions, thermal cal. of of noise source) and CMW (antenna efficiency and pattern). These have first-priority until they are completed.

Fig. 7— NRAO staff use of the 300 Foot Telescope, 1962.

for granted, but it sure wasn't taken for granted in 1962. There was constant fuss about what the role of NRAO, if any, ought to be in astronomy. A very powerful segment of the community was pretty much opposed to having NRAO exist at all, and so we felt very strongly that we somehow had to prove ourselves, and prove that this whole thing was meaningful. If we couldn't prove it, we might be out of jobs. I think the 300 Foot was the first telescope with which we had some chance to prove that. It was the first really large telescope, radio or optical, that was readily available to all comers, and it was powerful enough to be at the forefront of radio astronomy. It made a big impact on the attitude of radio astronomers towards national centers. Here at NRAO it gave us the opportunity to learn how to deal with a visitor oriented instrument: how to schedule an oversubscribed telescope; what was required in terms of calibration and documentation for an instrument that was used by people other than those who were thoroughly familiar with it (that's all different of course if you have your own captive telescope); what kind of reliability was expected by visiting observers (a visitor who rearranges his teaching schedule so that he can come to NRAO and then finds that the telescope doesn't work gets very unhappy with us, and we learned that quickly). We began to learn how to accommodate a variety of programs and equipment. And we learned that, in spite of what all of you probably think, not all radio astronomers are completely reasonable all the time. But the point is that what really makes a national center is the attitude of its people, and I think that with the 300 Foot here, we all really began to learn, not just the scientists, but also the telescope operators, the secretaries, and even the janitors, that in some sense, even the unreasonable astronomer was right even when he was wrong. He was right because if he wasn't, there wasn't any excuse somehow for our being here. I think that the 300 Foot really had a large part to play in developing our attitudes.

I mentioned that the 300 Foot was the first large national instrument that was powerful enough to be at the forefront of its science. That was 25 years ago, and it is still at the forefront. There was a statement in one of the early documents that was used to justify the 300 Foot to the effect that the existence of a large cheap collecting area for long-range programs would be extremely valuable. And I would like to think that has been true, and is still true. It seems to me that the 300 Foot still today attracts some of the most innovative, most talented, people with new and exciting instrumentation and programs, and I for one think that the next 25 years are going to be a lot more exciting even than the past 25, and I hope to be invited back to the 50th anniversary.

Discussion

T. K. Menon: I have a comment in regard to the attitude the community had towards NRAO in the early days. There was a distinction between those universities which had some radio astronomy activity at that time and those which did not have. I was looking at some old files and I found a copy of the job offer I had with the University of Pennsylvania, in 1958. One paragraph in the offer was taken up by the discussion of how the national facilities in Green Bank would be a major reason for starting radio astronomy activities at the University of Pennsylvania and encouraging anyone who comes there to be a major user of the facility. There would be radio astronomy at Penn but the national facility was to be the centerpiece of any activity. Whereas I know those who had radio astronomy facilities of their own were less interested in Green Bank. Not all were negative, but some were.

Unidentified: It is even worse than that. Not only was there this correlation in their attitude toward NRAO, but there was also a strong correlation in their influence. The "haves" had a lot more influence in advisory groups than the "have nots."

Gerrit L. Verschuur: Was there any interest from Sputnik?

Heeschen: I think that the interest from Sputnik helped funding [of science] in general during that period, but I don't recall that Sputnik inspired us to do anything other than what we were already doing.

THE FIRST CALL FOR PROPOSALS

The Memo below was distributed in 1962 to NRAO astronomers
by
David S. Heeschen

Memo to addressee:

A conservative guess
By Jay Double-U Eph *
Of the happy day for which we all wish
When observing begins with the 300 ft dish,
Is early next May.
In view of this news
Your weapons please choose
And give me your views ere another moon passes
Regarding your plans for astounding the masses,
With results sans pareil.

In other words, who wants to do what with the 300 ft.?

* *(Jay Double-U Eph refers to John W. Findlay's initials.)*

John Findlay at the 300 Foot Symposium, September 1987.

"From the time we started design to the time we started observing was slightly less than 23 months, and the telescope in its original form cost about $850,000."
John Findlay writing in the *Observer*, December 1972

A Telescope in 700 Days: Building the 300 Foot *

John W. Findlay
NRAO
Charlottesville, VA

I. Introduction

This paper is derived from a talk given at the symposium on the 300 Foot Telescope, held in Green Bank on September 25 and 26, 1987, to celebrate the first 25 years (or 9131 meridian crossings) of observations made with the Telescope. The audience was made up of users of the Telescope and many of those who have operated, maintained and improved it over the years. All speakers tried to tell the truth as they saw it about the Telescope and its work. I have transcribed the words I spoke without making any more alterations than seemed necessary to me to keep the text readable without losing the feeling of pleasure that I got in giving the talk to an excellent and friendly audience.

II. The Talk

Just about everything I say in this talk will be recollections of what happened, looked at through my own eyes. This is the only way I can do it. I have checked facts fairly well, but the feelings and opinions that I had at the time may well appear every now and again. To start, I should like to go back in history to before AUI was invented and recall that I was associated with some of the inventors. In the years 1936 and 1937 Norman Ramsey and I were students together in Part II of the Natural Sciences Tripos in Cambridge; we worked together for two years attending the same lectures and going to the same supervisor. For our final year that supervisor was Maurice Goldhaber, later to become the Director of Brookhaven. He taught both of us nuclear physics, and by the end of the year and the onset of the examinations, we knew it. The other man whom I must mention, although others of you have already done so, is Lloyd Berkner. All I need say, because you already all know it, is that the Observatory would not be here now if were not for him and his work.

Now, how did we come to the 300 Foot Telescope? We were trying to get the Observatory working as quickly as possible, and by 1960 I believe many of us often felt angry and frustrated because the 140 Foot, the "off the shelf" telescope, was flat on the ground. It really was not there at all and the only thing being built was the foundation. And there was Gart Westerhout with his cigar in his mouth, working away at the 85 Foot Telescope. He has been for many years one of our most consistent users. But in those days we didn't really have a telescope worthy of him, or many other users, and certainly not in the least worthy of the plans, money and effort going to make up a National Observatory. Fred Haddock had a telescope as big as ours, so also did Ed McClain (and he was shortly to add a second), and Bernard Lovell had brought the Jodrell Bank 250 foot into use in late 1957. We had built a lot of buildings at Green Bank, the staff had grown to more than 60,

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* Presented at the 300 Foot 25th Birthday Symposium, September 1987.

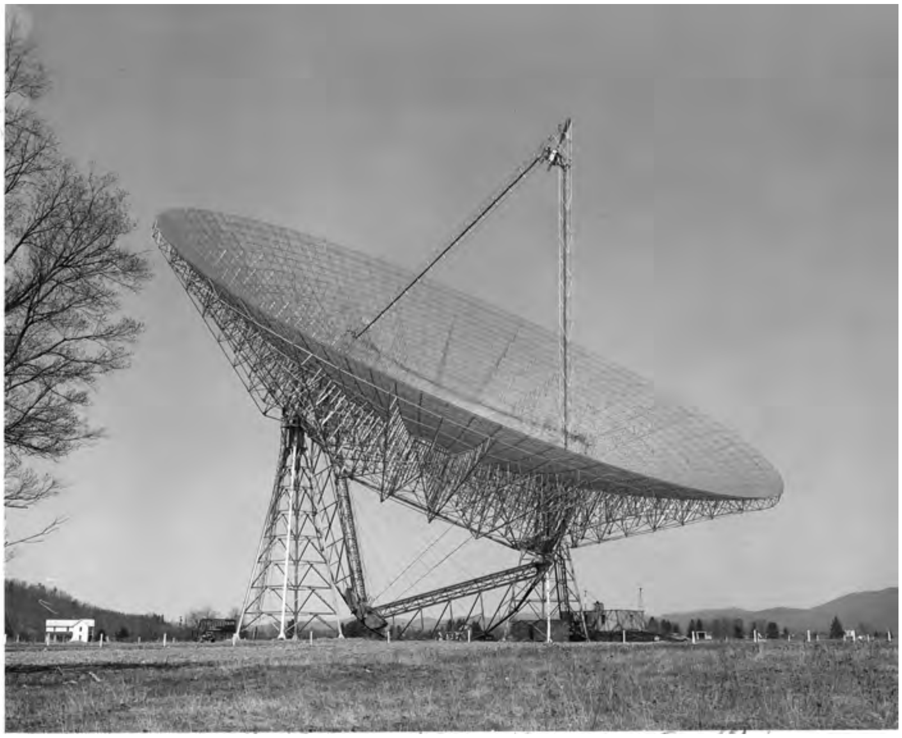


Fig. 1— The 300 Foot Telescope in its original form.

and the annual operating budget was more than half-a-million dollars. And we had persuaded Otto Struve to become our first director. We were frustrated.

So we searched for ways to get some observing done with a large but easy to build telescope. At the time, I called it the search for a "quick and dirty" telescope, and I still stick to this description, though it irritated some people. (Isidore Rabi was one; he did not like it at all and he told me that the USA does not build cheap and dirty instruments.) We tried several ways to go, and tested two before making a choice. I looked back over the suggestions, and one we did not follow up came from Frank Drake, who suggested using a zone plate. A zone plate does, of course, bring a plane wave to a focus (actually to several foci) but the focal length is wavelength dependent. We did in fact ask Neil Stafford of the Stanford Research Institute (SRI) to try an alt-azimuth design with a very low elevation axis, so as to lower the cost at the expense of giving only sky cover near the zenith. SRI had built several quite simple 150 foot telescopes—one on the Stanford campus and at least one for air defense on the coast of Scotland. Stafford came up with what you might call a "stubby" alt-az design, which might be built for a few hundred thousand dollars. The other design to consider was obviously a small edition of Arecibo, which was then being designed. We actually considered using the top end of Hospital Run as a site; it was not a hole-in-the-ground, but it was a trench and I thought cables could be put across it, and we could build a 430 foot spherical antenna like Arecibo for

Fig. 2— Otto Struve at the 300 Foot groundbreaking April 27th, 1961.

about this \$300, 000 we were talking about. So, before I reached the first day of my 700 days, I went up to Ithaca and talked with Bill Gordon and then at some length with Marshall Cohen. As many of you know, Marshall is clever, and he described the Arecibo feed design. After about an hour I said, "You may be able to do it Marshall, but I am sure I can't; it is too difficult for me." And I was right; wasn't I? For a long time it was too difficult for them. It was the feed difficulties which turned me away from the spherical reflector. In addition, I could not see how frequencies could be switched for different programs as quickly as we felt would be needed. In this I was probably too cautious.

So we were left essentially with the idea of a simple transit telescope. For this we already had Bob Hall, who was the designer for Blaw-Knox of the 85 foot telescopes. Blaw-Knox had supplied one to Fred Haddock, two to JPL, and of course, one to us. The design was fairly simple, and I remember one day sketching a transit telescope on the blackboard with Dave Heeschen and saying, "This is easy; we just have to tilt it." And so I went up to see Bob Hall to talk to him about the design; that day was the first of my 700 days and it was November 7th, 1960. He met me at the airport; he was of course at that time working for Blaw-Knox, and his first words were, "Well, I'm fired." This was something of an overly dramatic statement. Bob had already decided to join the Rohr Corporation in Chula Vista, and so he told Blaw-Knox that he planned to leave them in two or three months. The reply he got was that he might just as well leave at once, and so he did. It turned out that he did not have to arrive in Chula Vista for about six weeks, and Bob told me that, in that time, he could set down the overall design of a transit telescope which would meet our plans. And in fact, that is what he did. He set up drafting tables in the basement of his house, and I hired (following his advice) five engineers (who were in fact on the Blaw-Knox payroll) and in six weeks these, with Bob, working at nights and weekends, produced enough drawings to set the design of the telescope. They didn't get it quite right, but never mind. At the end of the six weeks I had enough drawings to make it possible to go out for bids on the steel structure in the February of 1961.

In the interval it was necessary to increase the number of drawings and to have an engineering office devoted to the telescope, and I asked the advice of Max Small at Brookhaven. He proposed Ed Faelten, who had a small engineering firm in Buffalo, and who was well known to Max from their work in the shipyards of WWII building Liberty ships for the German submarines to try to sink in the battle of the Atlantic. Incidentally, earlier in the 300 Foot story I had asked both Max and Bob for a rough estimate for the cost of the structural steel erected and both had said that, if it were kept simple, it might be bought for about fifty cents a pound, or \$1000 a ton, which I felt was really quite cheap, and we already had NSF approval to spend $\$300,000$.

So, as I said, by February we had about 30 Faelten drawings of the steel and adequate foundation designs, and so we were able to ask for bids on the telescope and its foundations. In those happy days we got ten bidders for the steel. We bid it separately, to fabricate the steel and to erect the steel; the erection, however, had to include also the placement of the main bearings, the drive package and the chain. These items, as well as the foundation work, were designed and bid separately.

During the bidding process I got what I think was the only pressure which might have been intended to affect the contract award. This came about because Blaw-Knox complained about the use of Hall and the engineers in the early design. I felt that Bob Hall was a free agent, but looking back I am not so sure that I was quite correct in using the five engineers. But nothing came of this difficulty; I, of course, visited Blaw-Knox and explained what had been done, and the bidding resulted in a contract for fabrication and erection being let to Bristol Steel and Iron of Bristol, Virginia. Many will know Bristol as the city where the Virginia-Tennessee state line runs along the center of Main Street. The foundation work went to B.F. Parrott (who had already built the Works Area building), the drive package and drive chain were done by Link Belt, and the bearing mounts were from Lake Erie Engineering.

Let me show here a photograph (Figure 1) of the telescope as it was after completion. It was changed quite a lot in later years, as some of the following

Fig. 3— 300 Foot Groundbreaking, April 27th, 1961. From left: M. Howell, BS&I Erection Manager; John Hawkins, BS&I VP for sales; Jim Tilley, BS&I Executive VP; E.R. Faelten, Engineer; J.W. Findlay; Otto Struve, Director NRAO; Charlie Bush, BS&I Engineer.

speakers may wish to describe. We held a ground-breaking ceremony on April 27th, 1961; Figure 2 shows Otto Struve with the spade and Figure 3 is a group of the men from Bristol, Ed Faelten, the author, and Otto. Fabrication of steel started in April 1961; by that time work on digging the square excavations, which went down to a layer of rather fragmented rock, had also been started. Foundations were also prepared for the drive package, which was to be below ground level, and for the two temporary erection towers. A fairly novel method for transporting the mixed concrete from the batch plant to the excavations was used—a pump sent the material through a pipe from the plant to the hole to be filled. This was quicker than wheel-barrows, at least until there was a pump failure, when the workers were presented with a long six-inch pipe filled with rapidly-setting concrete. Nevertheless, we did get the foundations successfully in place without too much trouble.

Some of the audience saw the less-than-perfect video tape last night which was recovered from a film made by a young English student (George Gilbart-Smith, the son of a friend of mine) who spent the summer of 1961 at Green Bank. I had intended to try to do a time-lapse sequence of the telescope which, when speeded

Fig. 4— The telescope on October 6, 1961.

up, would show it growing like a flower. So George went every morning to the site, mounted a movie camera onto a fixed post, and shot some film with the camera always pointed in the same direction. Well, this was a stupid idea, but only recently Fred Crews recovered the film, and with the skilled help of Brookhaven, managed to put together the video that you saw.

So, during that summer, the telescope structure started to grow. It was a very simple structure; there were the two towers to carry the elevation bearings, two erection towers at the North and South corners, and another similar tower at the center to position the hub. The dish structure was built on these—the steel was bolted together using high-tensile bolts, and the inner and outer rib structure was added after pre-assembly on the ground. Figures 4, 5 and 6 show various stages in the steel erection and the way two mobile cranes were used. During the bidding process Blaw-Knox commented to me that they did not believe that the design was adequately strong. It had been done by the old-fashioned methods, and I had already made plans to review the design work by making a computer check. So, as soon as Bob Hall got to Rohr, I gave him his design back and asked for an evaluation using a suitable computer program. He was able to do a stress and deflection analysis over the period when the bidding was in process and during the start of the steel fabrication. He made a number of design changes, and, if anyone looks at the structure now, it is possible to see a number of main members which

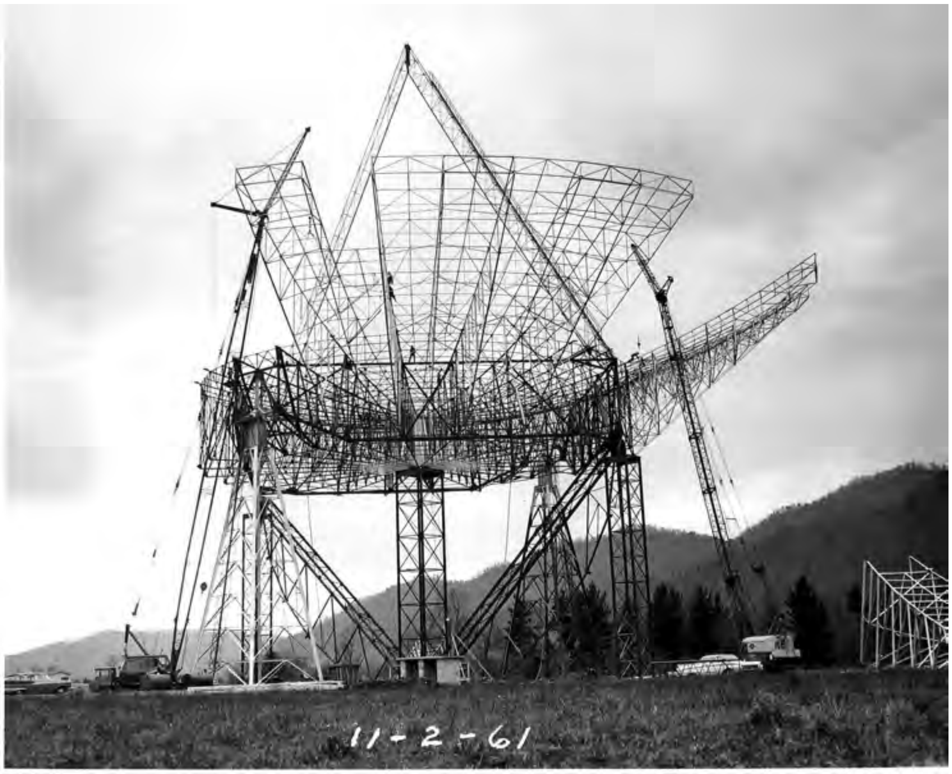


Fig. 5— The telescope on November 2, 1961.

are made up of small cross- section members backed up by bigger ones. It was these bigger ones which were added as a result of the Rohr analysis.

My dealings with the Virginia Gentlemen (as I called them) of Bristol Steel and Iron during the summer of 1961 were thus punctuated by the arrival of changed drawings from Rohr while the first work of detailing and fabricating the steel was being done at Bristol. There was a considerable body of work needed to turn the design drawings into the detailed drawings needed for the fabrication of members, joints and sub-assemblies. All this work was done at Bristol and while it was in progress I, with Faelten, used to go to Bristol about once every ten days or so with about a page or page-and-a-half of member changes to be made. I use the name "Virginia Gentlemen" because a typical day in such a visit would start at eight in the morning at the plant. We would be greeted with "Tee-off time is 12:30, so let's get started." And we had to keep to this. I would hand over my list of changes, sometimes quite large amounts of steel, and after a period of study the conversation would go somewhat as follows:

Bristol: "This looks OK. We can do it."
JWF: "Good, what will it cost me?"
Bristol: "We will work it out and bill you."

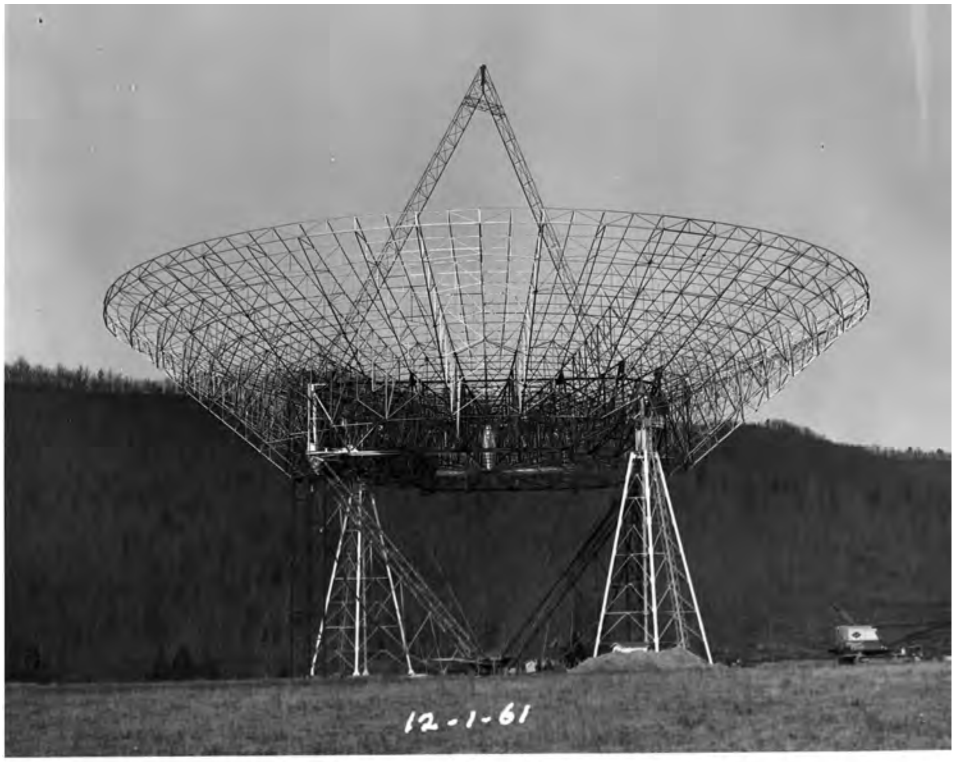


Fig. 6— The telescope on December 1, 1961.

It was quite an experience, but they really were gentlemen. The first time I went through this I asked how they would bill us and was told that, since the contract was in terms of the cost per unit weight of the steel for fabrication and erection, they would bill us at this rate for the steel which I wished to add. When I suggested that they might already have cut and punched steel which the changes would waste I was told, "Why don't you leave our business to us?" And this is the way it was worked. As we checked when the bills came in, we paid only for the added steel. And we always teed-off at 12:30. Throughout the course of the steel-work at Green Bank there was only one accident, which turned out not to be too serious. The dish structure outside the main square frame was erected one pre-assembled sector at a time using two cranes. When one such sector was bolted in place, a crane was unhooked too soon and the steel fell. One iron worker, who was on the falling steel, held on until the steel struck the ground in a crumpled mess; he then stepped off safely.

The work went ahead steadily at Green Bank, as the Figures 4, 5 and 6 show, until by the beginning of December, the main structure was in place, and about 400 of my 700 days had been used up. We were very fortunate in not being stopped by weather, for it was important that the steel should be completed before winds became too strong. Through the winter, all we did was to measure the shape of the

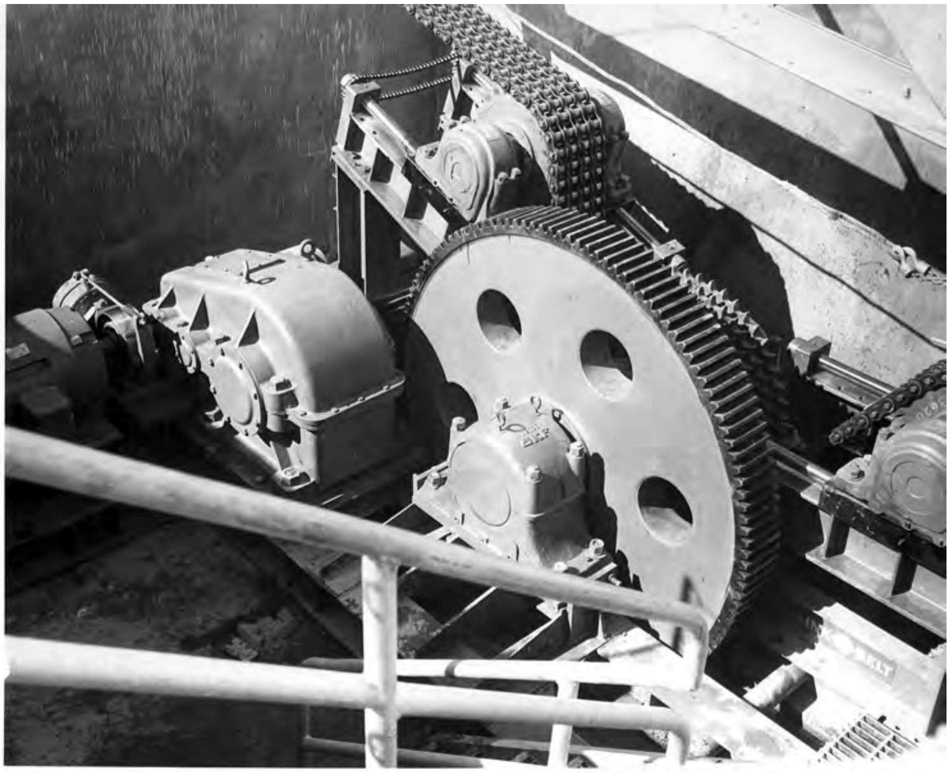


Fig. 7— The Drive.

steel on which the surface was to be placed. I had not demanded any accuracy from the steel fabrication and erection, other than that the steel as fabricated should fit together in erection. I had not decided exactly how to attach the surface mesh, since I did not know how closely the steel would lie to its desired position. Sidney Smith and John Ralston did the tape-and-transit survey of the steel and we found that the properly shaped surface could be fitted to lie within a few inches above the steel. ("A few" was in some cases as much as eight inches.) The surface, which was to be made from sheets of 3/8 inch Squarex mesh identical to that proposed for the 600 foot Navy telescope at Sugar Grove, could then be supported by Nelson studs the correct height above the steel. Nelson studs are lengths of threaded steel rod, which can be shot into the steel in a spot-welding operation. The plan was to set a nut onto each stud at the required elevation and to bolt the sheets of Squarex onto these nuts (see Figures 8 and 9).

Fred Hoyle had sent one of his graduate students, Miss Joan Crampin, to work at Green Bank, and she accepted the task of planning and computing where all the studs should go and where the nuts should be set on them. She did this correctly, as can be seen from the fact that the telescope later worked adequately at 1400 MHz. The only other thing I remember about Joan Crampin was the skill and enthusiasm with which she danced the "twist" (a dance of the early sixties made famous by

Fig. 8— Shooting Nelson Studs.

someone called Chubby Checker). We got the surface in place that winter, with only one problem caused by Union rules. I proposed to ask our own telescope mechanics to place the surface sheets, but was told by Darin and Armstrong, who were now on site working on the 140 Foot foundation, that this was not correct and Union labor must be used. The Virginia Gentlemen came to my help and took the extra task of placing the surface, using of course their own erection crew.

The drive package and chain (Figure 7) were delivered and installed on the Telescope in February 1962. Cabling was begun at about the same time and by May 1962 we were faced with the fact that the instrument was ready to be tilted. I had been helped a lot by Spencer Greenwood, a top-level mechanic who had been a shipyard superintendent (in the same yard as Max Small) in WWII and who was [now in Green Bank] working on the 140 Foot. In fact, there is a story that he prevented a major disaster when that telescope was being erected. One test which was made when the polar shaft and hydrostatic bearing were in place, was to pump oil into the bearing for the first time. As it floated, the bearing was supposed to move very slightly down-hill. But Spencer, for some reason unknown to others,

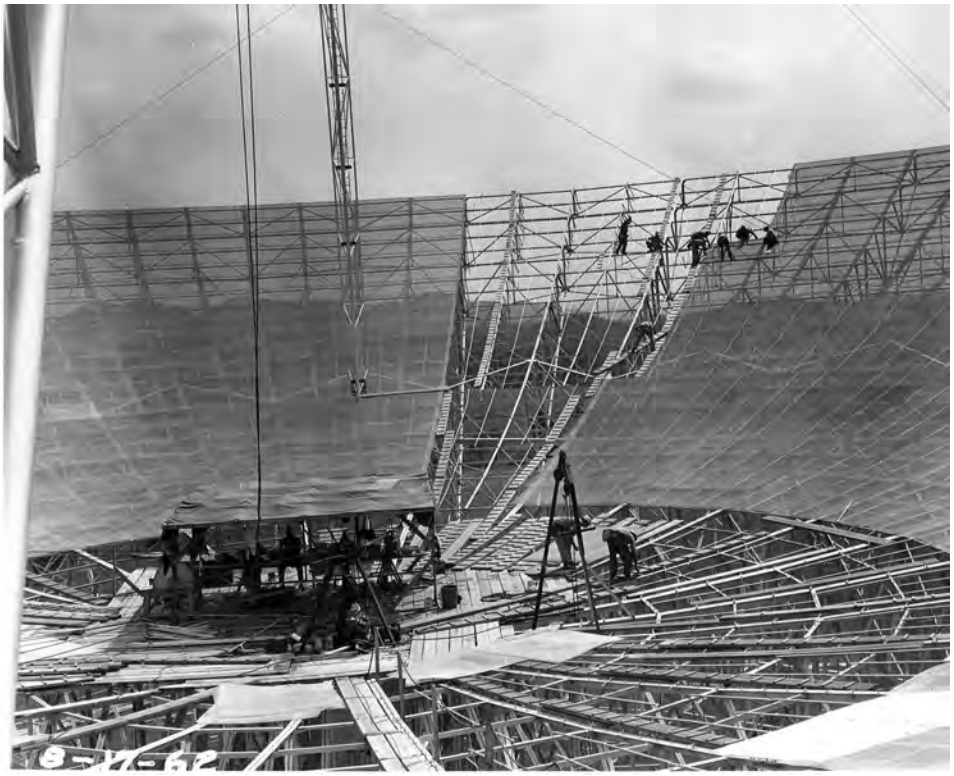


Fig. 9— Placing the surface, August 17th, 1962.

had welded a strong steel stop at the lower end of the shaft. When the oil was pumped in, the coefficient of friction at the sphere fell to zero—as it should. But the center of gravity of the sphere and shaft was so far below the sphere, that the sphere could ride up-hill out of the bearing pads as the geometry allowed the center of gravity to fall. And so it did, and the only reason that the shaft did not fall all the way and hit the ground was Greenwood's stop. John Ralston was standing with a millimeter scale to measure the shaft movement; in dead silence someone handed him a yard-stick. (Unfortunately, I was not present, and I cannot confirm this story.)

We tilted the telescope for the first time on May 4th, 1962, and, as Spencer Greenwood had helped a lot with the drive, I left it to him and Fred Crews to drive the instrument to its South and North limits. I was (as is not unusual at such times) quite uncertain whether everything would work, so I went away and sat in the field and watched the operation from a distance. All went well. I described the surface setting earlier, but we deferred the installation until the summer of 1962, since there was plenty to do with completing the control building and cabling the instrument, for all of which the summer work period was essential. I remember showing the incomplete surface to Joe Pawsey when he visited Green Bank in July 1962, shortly after he had accepted the directorship.*

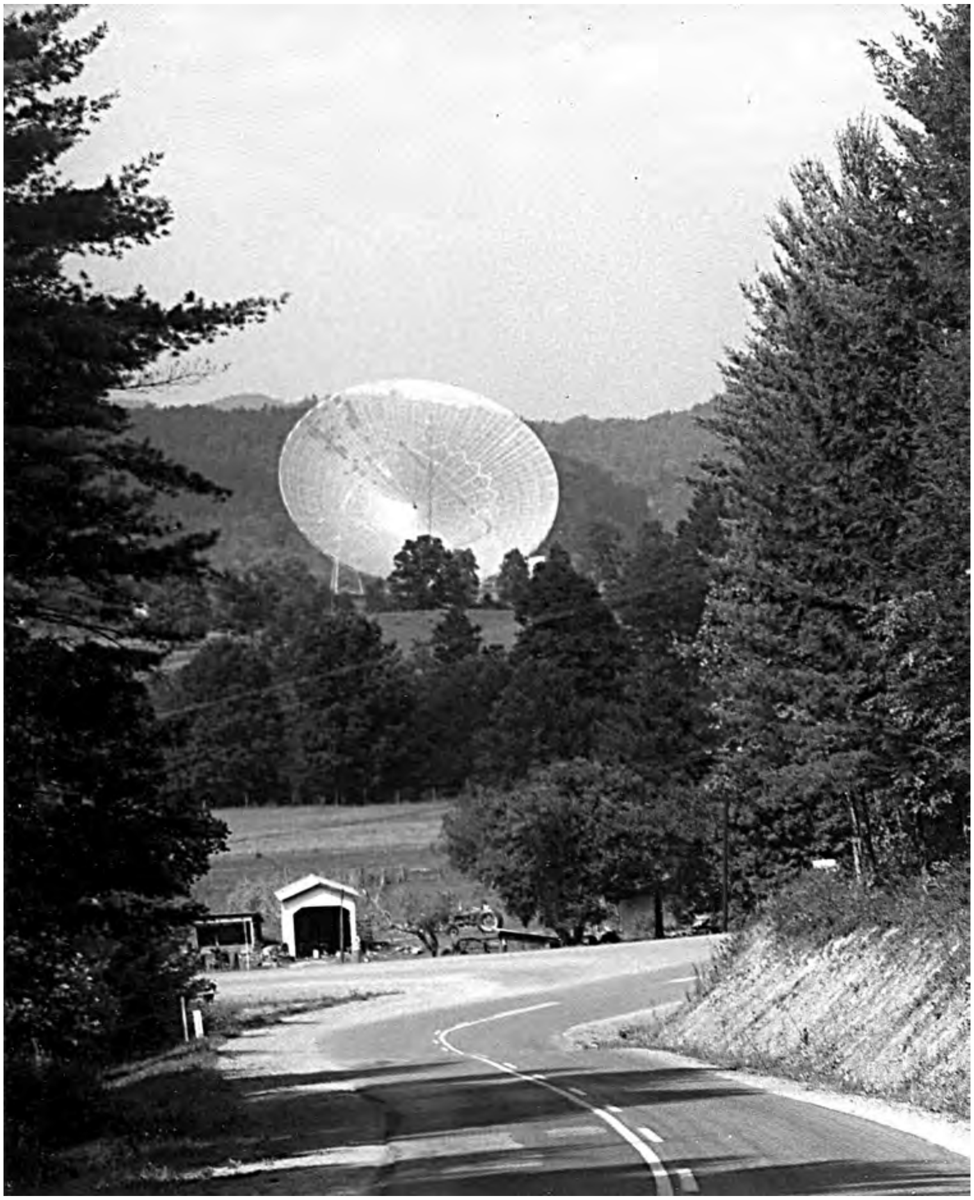
The final work to bring the telescope into use was shared among many in Telescope Operations, Electronics and Site Maintenance. Frank Drake saw to the installation of 750 MHz and 1400 MHz continuum receivers, and by October 1, 1962, observations had begun. I suppose the end of the project from my point of view was when the instrument was handed over to Fred Crews as head of Telescope Operations. I have no exact date for this, but I recall that I and a few others were sitting near the telescope one evening when Fred came past, entered the control room and tilted the telescope. As he came out I called to him, "OK Fred, it's yours." I have always remembered this as the hand-over.

From one of the discussion sessions: —

Findlay: Since we are telling stories, I told you that we had to play golf every day we went to Bristol when we were building [the 300 Foot]. And Jim Tilley was not a very good golfer and he had never made a hole-in-one.... I took a nine-iron to the 300 Foot to see if I could hit a ball high enough to get it into the dish. If I had been able to do so I would have offered Jim the opportunity of making a hole-in-one. If this had worked then at the opening ceremony I would have allowed the golf ball to run down the dish and fall through the hole in the middle and with any luck break some small receptacle containing an alcoholic liquid and that would have completed the circuit. But I couldn't hit a ball into the dish!

John Findlay in 1963.

* *Editors' note: Shortly after Pawsey accepted the offer to succeed Otto Struve as Director of NRAO, he fell ill and was not able to assume the office. He died in November 1962. David S. Heeschen, who had been acting Director since December 1961, was appointed permanent Director in October 1962.*



The 300 Foot Telescope as seen over the farm of J. A. Sheets, approaching Green Bank on Rt. 28 from the South, August 30, 1962. This was a familiar sight to everyone in the area.

Fig 1. — The 36 Foot transit telescope at Potts Hill, near Sydney, used for early 21 cm work.

Frank Kerr speaking at the 300 Foot 25^{th} Anniversary Symposium, September 1987.
(Courtesy G. Verschuur)

Extragalactic Hydrogen on Two Transit Telescopes*

F. J. Kerr
University of Maryland

I was a quite early visitor to Green Bank, here first in January 1958, and I want to say a bit about those early times. In listening to the talks about the plans for the 300 Foot, I was reminded about the comment attributed to the famous astronomer Walter Baade in about 1952. He was reported to have opposed the building of any new or bigger radio telescope because, as he said, "It is obvious that in the last six or seven years, radioastronomy has gone about as far as it can. Why do any more?"

My title is "Extragalactic Hydrogen on Two Transit Telescopes" because the two ends of my radioastronomical life have been connected with transit telescopes: one in Australia and this one here. I first want to draw attention to the fact that extragalactic 21 cm work actually started with a transit telescope; it had the amazing diameter of 36 feet (see Figure 1). It was, I believe, the largest telescope in the world at that time, and held the record for a little while until NRL came out with their 50 foot dish on top of their roof, where it can still be seen as you go down the Potomac.

Until that time I'd been working on radar astronomy echoes from the Moon and Sun. I then went to Harvard for a year, in 1950-51, and was there when Doc Ewen first detected the 21 cm line. Ewen was a graduate student doing a thesis, and somewhat before his actual detection he came to Ed Purcell, who was in charge of this project, and said regrettably he couldn't find the line and would Purcell accept a negative thesis. Purcell very wisely said: "yes, but a negative thesis has to be ten times as good as a positive thesis." Ewen went back and worked harder and of course, as we know, he found the line. That should be a lesson to all concerned, and that's probably why not many people publish negative results, by the way. On that same day, van de Hulst who was there sent a cable to Leiden and I sent one to Sydney, and that inspired further work at each place. In Sydney, actual work had not started on the 21 cm line but Christiansen and Hindman got going under Pawsey's instructions and put together a setup in just a few weeks, and detected the line, and made what was really the first survey of galactic HI radiation.

When I got back to Sydney later in 1951, I too began working on the 21 cm line. The Christiansen and Hindman work had been done on the 17 x 17 Foot antenna, or aerial as we called it then, and we decided to build a bigger one. We decided, eventually, that we would set up a transit telescope. This system worked very well for several years. I don't think we knew the exact shape of the surface very well, but it gave a nice beam pattern at 21 cm, which was all we wanted. It

* From a talk given at the 300 Foot Telescope 25th Birthday Symposium, September 1987. Frank Kerr (1918-2000) worked during World War II with the Australian CSIRO radiophysics laboratory on radar and atmospheric studies. He was among the first to obtain radar echos from the Moon (1948). He established a program in Australia for mapping neutral hydrogen in the Galaxy and the Magellanic Clouds. He was also involved in studies for the Parkes 210-foot radio telescope.

worked nicely, except one morning we found that it was not working at all, and when we went up to the focal point, via the big ladder, we found that a possum had come and chewed through some polyethelene cable, which it seemed to like, and that put us off the air.

We began by looking at the Magellanic Clouds and soon detected the line. I think, from memory, our first detection was in the Small Cloud, which therefore has got the record as being the first external galaxy seen in the 21 cm line. The shed beside the telescope was not quite as palatial as some of our observing buildings these days, but it kept the rain out; there was no snow of course. We looked at both the Magellanic Clouds, the Large and the Small. When we got a nice picture of hydrogen in the two Clouds (Figure 2), we showed that the HI extended much further than the optical images. Subsequent observations having higher sensitivity showed there's a connection right across. We got a velocity distribution and found that the large Cloud is rotating.

It is interesting to recall how we actually did observe on a transit telescope. Of course, the transit telescope has got its advantages and its disadvantages, and skill is required to use it in the best way. At that time, we just had a single channel receiver, in fact all 21 cm work up to that time was done with a single channel. The early work at Leiden and Harvard and in Sydney had been done by following a point and scanning the single channel across the frequency spectrum. We decided to start thinking about multi-channel equipment as that, of course, would fit much more appropriately with a transit telescope. Our Cloud work was done with just one channel, while at the same time, preparations were being made, first for a four-channel receiver that was actually used in our first Galactic work, and then for a 48 channel receiver. The interesting trick is that the Magellanic Clouds are at a quite high southern declination, so they passed rather slowly across our telescope beam, especially because the beam width was of the order of 1 degree, giving us a lot of time. What we did on one single transit of the Large or Small Cloud was to set on a particular velocity, or frequency, and we scanned up and down in declination as they passed. We would actually get a complete map of the Clouds at that single frequency in the single passage. So then of course, next day or next night, we would use another frequency, and so on, and that's the way in which we solved the transit telescope problem. Eventually we gave up using that telescope and went on later to the work on the Parkes 210 Foot dish.

I said I first came to Green Bank in January 1958. The context of that visit was that I'd been visiting Gart Westerhout and Jan Oort at Leiden for a few months, putting together the northern and southern parts of the HI surveys of the Galaxy, and on the way back came to this country and stopped off at Green Bank, in January, which was a bit of a shock. I guess I was interested in coming because I was already under the influence of Bart Bok who had been in Australia for three years by then. He was talking very much of Green Bank and its future, and he and Joe Pawsey both urged me to go to Green Bank. I saw Dave Heeschen, John Findlay, and Dick Emberson directing the future of NRAO from a small farmhouse; that's about all the buildings that there were at that time. I recall that we stayed at a hunting lodge down at Minnehaha Springs, and I recall also from that time, referring to January, that I was driven back to Washington by John Findlay. Being an Australian, innocent of snow and ice, I admired the way in which he was able to

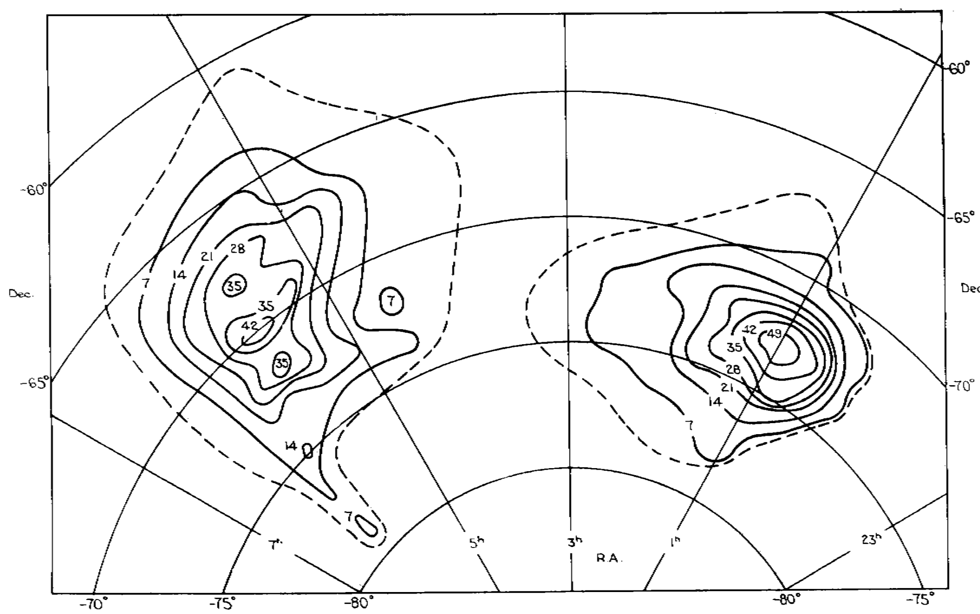


Fig. 2— First map of neutral hydrogen column density in the Magellanic Clouds.

see a patch of ice and either slow down or drive around it, so we got successfully to Washington, but not in 3 and 1/2 hours.

I was back again in January 1961, this time after a visit to Argentina to discuss the Magellanic Clouds (there was a meeting of an IAU committee on the Clouds). Again, I came in January, and just to tell some of the young people how people traveled in those days, I came by train to White Sulphur Springs. It was an overnight train and I arrived early in the morning. I remember that Otto Struve was getting onto a train going in the other direction to Washington, which I presume he did fairly often. As I got off the train, a person came up and asked where I was going. He apparently didn't understand my accent, because he was the driver from the Greenbriar, not Green Bank, and he drove me over to the Greenbriar and I was amazed at its palatial character! I expressed my amazement and he took me back to the station as quickly as he could and fortunately we caught the driver from Green Bank, who was still waiting there for me. I stayed on that occasion with Campbell Wade who at that time was living in a house somewhere down near where the 300 Foot is now. It was rather a difficult getting out in January and I know that he was giving close attention to weather forecasts the whole time I was there. Then interestingly enough, at the end of that stay of a few days, I left the place by going by train to Charleston, and from there catching a flight west. Traveling then was more difficult than it is now.

My principal interest at that time was in the 210 Foot telescope at Parkes. I believe it was Dave Heeschen who spoke about the importance of beating Lovell and his 250 foot antenna size. That loomed large in Taffy Bowen's mind also at that time, so we were planning for a 255 foot dish. Unfortunately, the cost worked out a bit too high for that, and the size was cut down by some magic formula that said the cost was proportional to some power of the diameter, and brought down to 210 feet. As it turned out, the 210 Foot cost as much as the original estimate for a 255 foot; it always seems to go that way. Like the the 300 Foot, the Parkes dish also has been resurfaced, and the surface improved from time to time. I think it's interesting that all radio telescopes seem to be performing better than was originally intended; the structures are stronger than the first calculations suggested.

Throughout the years I've been associated with the 300 Foot on quite a number of different projects, but I'll skip over those and just talk about the most recent one. I've come back again to do a new project for which the 300 Foot telescope is extremely well suited, and that is in looking for hidden galaxies, or galaxies behind the Milky Way. We're doing a blind search at a large number of points to see how many we can detect. It's a long, long job. So far we've only covered a couple of percent of the whole hidden region which is accessible from Green Bank. A full survey would be very interesting but it would be a big job. Searching for galaxies behind the Milky Way is actually something I've been interested in for a long, long time and it's one of the obvious things you can do with the 21 cm line. I looked from time to time on the 36 Foot dish long ago, and then from Parkes.

It's interesting to consider the difference in sensitivities, i.e., the sensitivity of the 36 Foot dish as compared with the 300 Foot dish. First of all you've got the ratio of the square of the diameters and then you've got the difference between having a system temperature of 700 K or 800 K as it was then, and 25 K now, so the difference in sensitivity for a small-diameter source is about 1500 times. We must have been pretty optimistic to look for anything in those days. There is still a difference of about 8 times between the sensitivity of the present 300 Foot Telescope and the early days of Parkes, when we were working with a smaller antenna and a system temperature of about 100 K. It's really only in recent times that the receiver sensitivities, and also the greater ease of data analysis, have made a full survey something you might think of, and so we've been doing a pilot study. I'll get to talk more about this at the Workshop on surveys next week * so I won't go into it at present. After many years, the wheel has turned full circle and I'm back again to observing galaxies on a transit telescope after starting off that way.

Discussion

J. R. Fisher: Can you tell us when the 210 Foot was completed and when it started?

Kerr: It was started to be thought about, seriously, in about 1955. Incidentally, John Findlay referred to a zone plate this morning. That was one of the things we considered at great length at one time. It's a good solution because you've got an almost flat structure, but it only works at one wavelength, or a series of harmonics.

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* *Large Scale Surveys of the Sky*, 1990, ed. J.J. Condon and F.J. Lockman, NRAO

The Parkes telescope was opened up, or commissioned the official word is, by the end of October or early November 1961. On the actual day of the commissioning by the Governor General of the country there was a great crowd of bigwigs, plus locals, plus all sorts of people who came to the telescope. It was a big occasion. We were all set up for a beautiful ceremony, and at the end of it Norm Broten, the visiting Canadian, was going to unfurl the Australian flag at the top of the tripod as the telescope came down impressively to sort of address all the people in a supposedly spectacular scene.

Unfortunately, on that day there was a very, very strong wind at this place, at which the wind had been certified to be quite commonly, quite small. It was a terrible day and there was dust blowing around and it was impossible to move the telescope. It wasn't too easy to hear what people were saying because the microphone picked up all these wind noises as well. At the end of it people were all supposed to go to a reception and I think I've never seen so much beer drunk in my life so quickly, because everybody had extremely dry throats and it was possible to drink large amounts of beer without any effect whatever. The Governor General and all the others really downed the beer. But the telescope was opened anyway and I think we started observing very rapidly after that.

Bernard F. Burke: Frank, I can attest to the fact that the Governor General remembered that day very well. At the Magellanic Clouds Symposium in 1963 Bart Bok shepherded us up in groups of four to meet with the Governor General. You can only talk about so many things with the Governor General then you run out of topics. Bart had it exactly, perfectly, timed. He knew how long it would take to run out of topics and would slide another group of people in. The trouble was, when our turn came, I remembered your story of the dedication and so I annunciated Kerr's theorem that when you're selling a site for radio astronomy the wind never blows, but when you dedicate it the wind does blow. And immediately, the Governor General sprang to life, and he was in the middle of a long story when Bart came along with the next group. Remember that the next time you have to make conversation with a Governor General.

Editors' note: The 210 Foot Telescope is discussed extensively in "Parkes: Thirty Years of Radio Astronomy," ed. D. E. Goddard and D. K. Milne, 1994, CSIRO Publications.

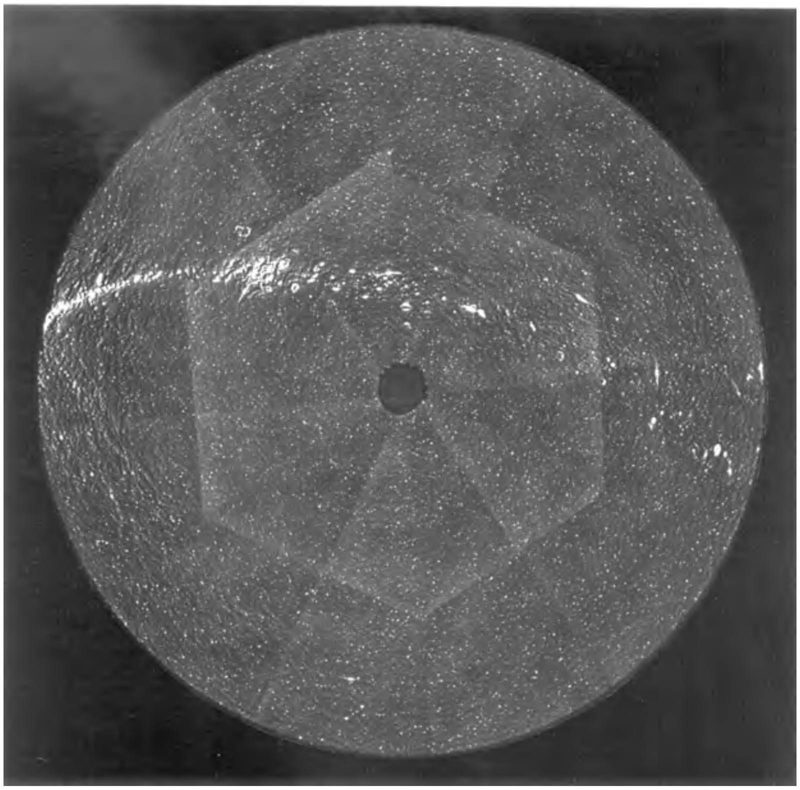
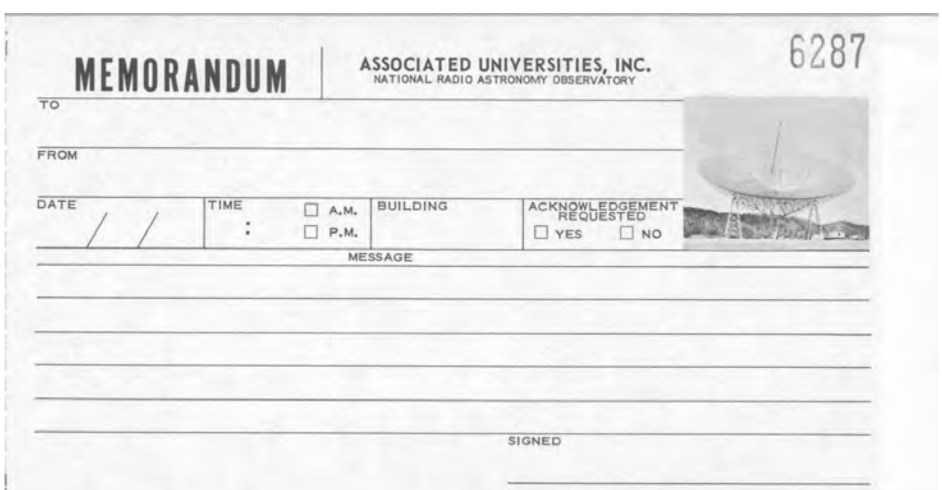


Fig. 1— 1400 MHz map of the northern sky made with the 300 Foot Telescope.



MEMORANDUM

ASSOCIATED UNIVERSITIES, INC.
NATIONAL RADIO ASTRONOMY OBSERVATORY

6287

TO

FROM

DATE / /

TIME : ☐ A.M. ☐ P.M.

BUILDING

ACKNOWLEDGEMENT REQUESTED ☐ YES ☐ NO

MESSAGE

SIGNED

A form for memos and phone messages, used in the 1970s.

Extragalactic Continuum Surveys with the 300 Foot Telescope *

J. J. Condon

National Radio Astronomy Observatory
Charlottesville, Virginia

I. Introduction

In his 1963 Sky and Telescope article describing the new 300 Foot Telescope, John Findlay (1963) wrote of the scientific pressure for quick construction: "If the new instrument could be built fairly quickly and cheaply, its full scientific value would be realized before progress in the science overtook the instrument's purposes." The 300 Foot Telescope is now 25 years old and radio astronomy has advanced more rapidly than could have been imagined then, yet fears that the telescope would become scientifically obsolete have not materialized. The history of extragalactic continuum surveys made with the 300 Foot illustrates why:

1. The telescope itself has been upgraded (resurfacing, lateral focus tracking).

2. Receivers used on the telescope have improved in system temperature, bandwidth, gain stability, and reliability.

3. The data-taking and analysis computers and programs are much faster and more versatile.

4. The scientific goals themselves have evolved considerably.

II. Extragalactic Continuum Surveys

Figure 1 shows the map made from a 1400 MHz continuum survey covering the northern hemisphere. The bright curved band traces the galactic plane, and the myriad small "stars" scattered randomly over the entire sky are intrinsically luminous sources in distant radio galaxies and quasars. When the 300 Foot was built only a few hundred of these extragalactic sources had been found, primarily in low frequency (e.g., 178 MHz) surveys. Many of the early surveys were made with low resolution and were notoriously unreliable because "confusion" caused by several faint sources blended in the telescope beam could mimic a single strong source. Indeed, one of the first 300 Foot observing programs was a "secondary" survey to map small areas around 3C survey sources at the then "high" frequency of 1400 MHz to verify their existence (Pauliny-Toth, Wade, and Heeschen 1964).

Several factors limit the numbers of sources that can be detected in sky surveys—confusion, receiver noise, observing time, and analysis effort. The rms confusion σ_c from unresolved blends of faint sources varies quite rapidly with frequency (Fig. 2) because the beam area of a given telescope is proportional to ν^{-2} and most sources have "steep" spectra S $\propto \nu^{-0.7}$. The faintest sources that can

* Presented at the 300 Foot 25th Birthday Symposium, September 1987.

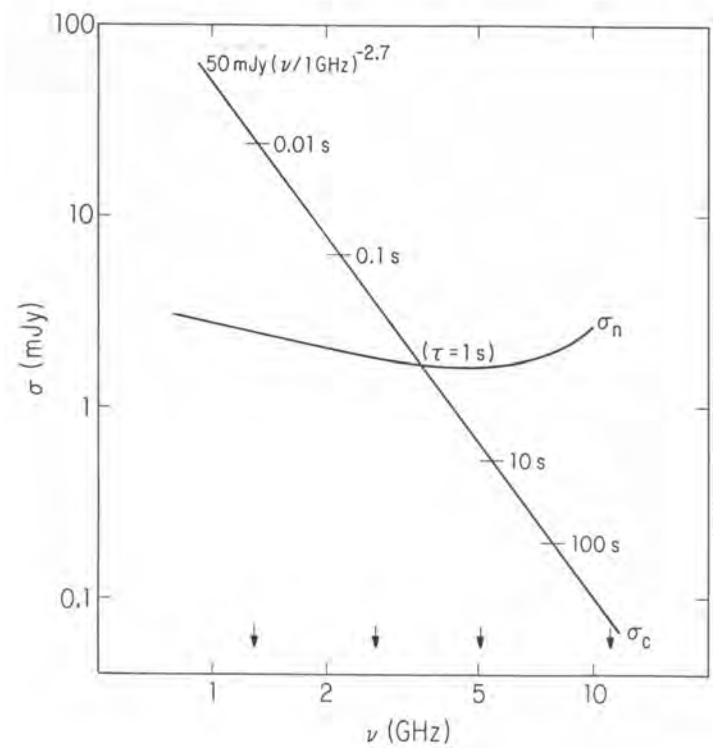


Fig. 2— This plot shows the rms noise σ_n for an integration time $\tau = 1$ s and the rms confusion σ_c as functions of frequency for the 300 Foot Telescope and current receivers.

be detected reliably have flux densities S $\approx 5\sigma_c$, so surveys made at higher frequencies can ultimately detect more sources. Receiver noise varies only slowly with frequency, however, and the integration time τ per point mapped required to reach the confusion limit increases very rapidly with frequency. This time is nearly independent of telescope size. But the number of points that must be sampled (Nyquist sampling requires at least two points per FWHM beamwidth) is inversely proportional to beam area, so very long observing times are necessary to map a given solid angle down to the confusion limit at high frequencies with a large telescope. The 1400 MHz map in Figure 1 is strongly confusion limited. Confusion-limited surveys have been made only over very small areas at 5 GHz, and no confusion-limited surveys have been made at higher frequencies. Thus the 5 GHz frequency limit of the 300 Foot Telescope is not a significant drawback for making large-scale continuum surveys —upgrading the 300 Foot to operate at 10 or 15 GHz would be of little use in the near future.

These considerations favor interferometers and unfilled-aperture instruments for surveys below $\nu \approx 1$ GHz. At higher frequencies aperture synthesis arrays like the VLA can map small areas with very high sensitivity, but only filled-aperture (single dish) telescopes can cover large areas of sky. In general, bigger is better for survey telescopes. Since the 300 Foot is one of the largest steerable telescopes capable of operating at frequencies up to 5 GHz, it is well suited to making extragalactic continuum surveys.

III. Past Surveys

The first "primary" extragalactic continuum survey made with the 300 Foot telescope covered the "strip of sky" $+18° < \delta < +20°$ and detected 458 sources (Hoglund 1967). The observations were made for eight weeks during 1964 January-April and consisted of sidereal-rate drift scans at 750 and 1410 MHz. Data from one hour's observation are shown in Figure 3.

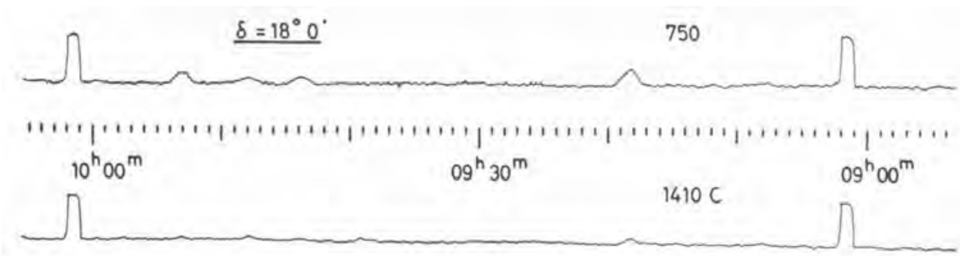


Fig. 3.— One hour's data from the "strip of sky" survey. The calibration signals at the beginning and end of the scans correspond to S≈ 10 Jy.

The two receiver outputs were recorded on punched paper tape every 10 s. The 750 MHz survey reached the confusion limit $\sigma_c \approx 0.2$ Jy but the 1410 MHz survey did not, primarily because of "noise" caused by gain fluctuations in the load-switched parametric amplifier used. The scientific goals of this survey were exploratory since so little was known about extragalactic sources at that time. Hoglund (1967) used his data to derive the spectral index distribution of sources selected at 750 MHz and his source counts to detect evidence of cosmological evolution.

Single-beam sidereal-rate surveys with the 300 Foot Telescope cover the sky very slowly. The four-feed 1400 MHz receiver was built in 1967 to increase the sky coverage rate by a factor of four. Four was chosen because it was thought to be the largest number of receivers (paramps) that could be operated with reasonable reliability (Davis, private communication). The four-feed receiver in its original form was used to make four extragalactic continuum surveys between 1967 and 1975 (Davis, unpublished; Maslowski 1972; Machalski 1978; Rys and Machalski 1987). Its $T_s \approx 150$ K, $\Delta\nu \approx 60$ MHz, load-switched paramps were sufficiently sensitive and stable to reach the confusion limit $\sigma_c \approx 20$ mJy in $\tau = 4$ s, at least when they were working well. A map from one of these surveys is shown in Figure 4. Unfortunately, receiver unreliability proved to be a serious problem. If just one of the four paramps became unstable, it was necessary to repeat the whole affected observation. As a result, the observing time needed to map a given area of sky was about 2000 hours per steradian. Each survey covered a few tenths of a steradian with

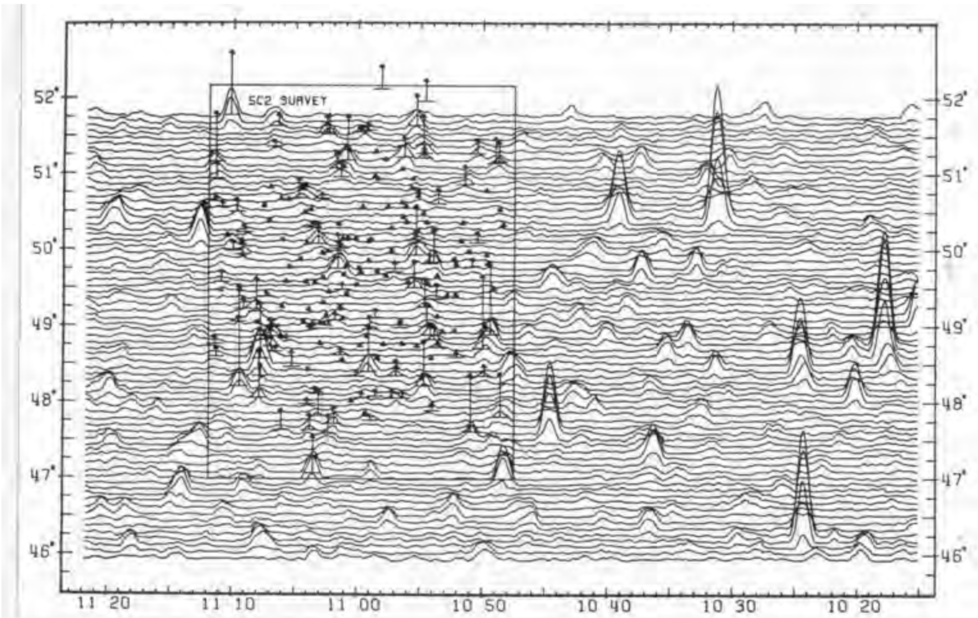


Fig. 4— Scans from Maslowski's 1400 MHz survey (1 Jy = 1° of declination).

about a month of observing and detected over 1000 sources. Even so, the receiver was not the limiting factor— the amount of data generated by the four-feed receiver strained the analysis programs and computers, as well as the scientists reducing the data, who were forced to fix many problems with the data "by hand." Note how the primary limiting factor changes with time as old problems are fixed—sometimes it is receiver sensitivity or reliability, then data-taking or analysis, or telescope performance (e.g., aperture efficiency and excess noise at short wavelengths).

The principal scientific result from these 1400 MHz surveys was to determine the source counts with low statistical errors (since the number N of sources detected is $\gg \sqrt{N}$) down to $S \approx 150$ mJy. This flux-density range is particularly important because the excess count produced by cosmological evolution is the greatest. The 300 Foot results are so accurate that improving them would have little or no effect on their interpretation; these surveys have effectively solved the problem that they addressed.

The next big advance for continuum surveys with the 300 Foot was the new reflector surface permitting observations at higher frequencies. The first program on the telescope after it was resurfaced in 1970 December was Mike Davis' (1971) 5 GHz drift-scan survey. Between 1970 December 18 and 1971 January 15, a $T_s = 150$ K, $\Delta\nu = 60$ MHz beamswitched single-channel paramp was used to detect 254 sources with $S \geq 67$ mJy in a narrow strip near $\delta = +33°$ (Fig. 5a). The declination range was chosen because the aperture efficiency of the 300 Foot telescope fell off rapidly away from the zenith. This area was resurveyed with the "6/25" receiver in 1981 to detect variability in a complete sample of sources selected at 6 cm (Altschuler 1986) (Fig. 5b).

A much larger area of sky was covered with slew rate ($\pm 10°$ per minute) declination scans in 1972 March and April. Pauliny-Toth *et al.* (1978) found 259 sources with $S \geq 0.5$ Jy in the 1.71 str region $+35° < \delta < +70°$. They noted that the high scan rate (40× sidereal) helped to reduced the time lost to bad weather. This was another "exploratory" survey designed to detect large numbers of the strongest flat-spectrum sources.

The new "6/25" dual-channel cooled paramp was the first with sufficient sensitivity ($T_s \approx 70$ K, 580 MHz bandwidth) to approach the confusion limit at 6 cm (Ledden *et al.* 1980) in a very narrow 0.00956 str strip of sky near $\delta = +35°$. This survey demonstrated the convergence of the 6 cm source counts at low flux densities. A related survey covering 0.0691 str detected 480 sources stronger than 35 mJy (Owen, Condon, and Ledden 1983). These were the last surveys made with the 300 Foot whose main scientific goal was to obtain source counts (in this case, of flat-spectrum sources), and they showed that flat-spectrum sources could evolve at the same rate as steep-spectrum sources. Most source-count work of this type is now based on more sensitive surveys of very small sky areas made with the VLA or other aperture-synthesis telescopes.

The last 6 cm survey to use the "6/25" receiver was the MIT-Green Bank sky survey covering the 1.87 str declination band $-00°30' < \delta < +19°30'$ and detecting 5974 sources above the $5\sigma_n = 53$ to 106 mJy flux-density limit (Bennett et al. 1986). Although this survey has produced the traditional "source counts," its real purpose was to detect gravitational lens candidates for further observation with the VLA. A very large number of compact flat-spectrum sources must be found since gravitational lenses are rare (only about one in a thousand sources is gravitationally lensed). The 300 Foot telescope is necessary for such a project because the VLA cannot survey they sky rapidly at 6 cm, and the VLA is necessary because the 300 Foot does not have the ≈ 1 arcsec resolution needed to resolve the image of a point source seen through a galaxian gravitational lens. This use of the 300 Foot as a finding instrument for follow-up work with the VLA is analogous to the use of the Palomar 48-inch Schmidt telescope to cover a large area of sky optically and find samples of objects for detailed study with the 200-inch telescope.

IV. FETs, AIPS, and Current Surveys

The current generation of surveys on the 300 Foot telescope has been made possible by improved receivers (FETs) and analysis programs (AIPS).

The poor reliability of the four-feed receiver and other continuum receivers with paramp front ends discouraged their use, and their poor gain stability prevented them from approaching their theoretical sensitivity. During Users' Committee meetings of the 1970's there were many complaints about this situation, usually followed by comments (from nonusers of these receivers) that the problem wasn't urgent because few people were using NRAO telescopes for single-dish continuum work any more. Ken Kellermann invariably had to point out that poor receiver performance was the reason that so little was being done. In 1982 I requested that the load-switched room-temperature paramps in the 1400 MHz four-feed receiver

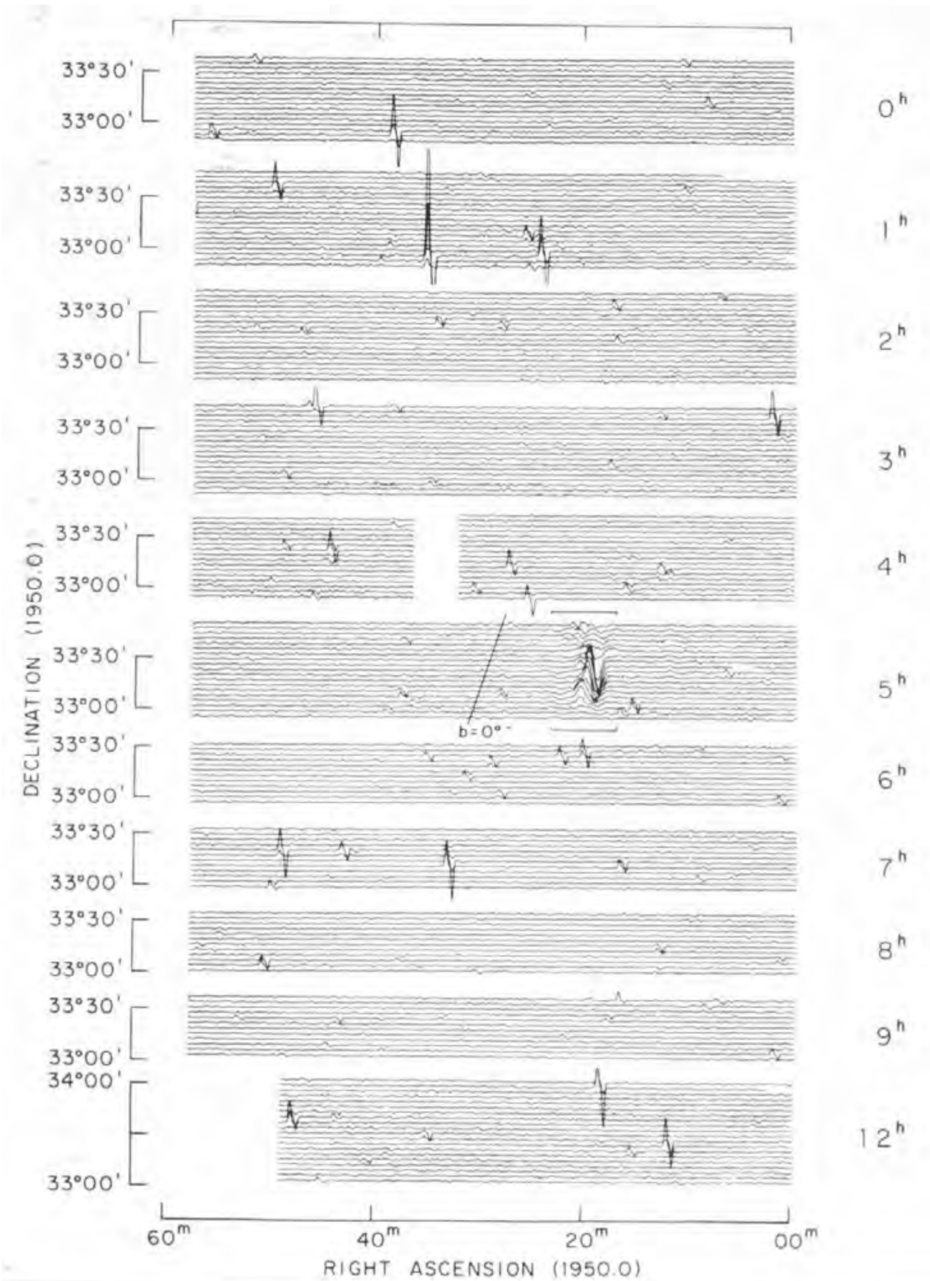


Fig. 5a— Scans from the first 5 GHz survey made with the 300 Foot Telescope. At 5 GHz the beamwidth is only about 3 arcmin, reducing the confusion error by more than an order of magnitude. This survey is noise limited.

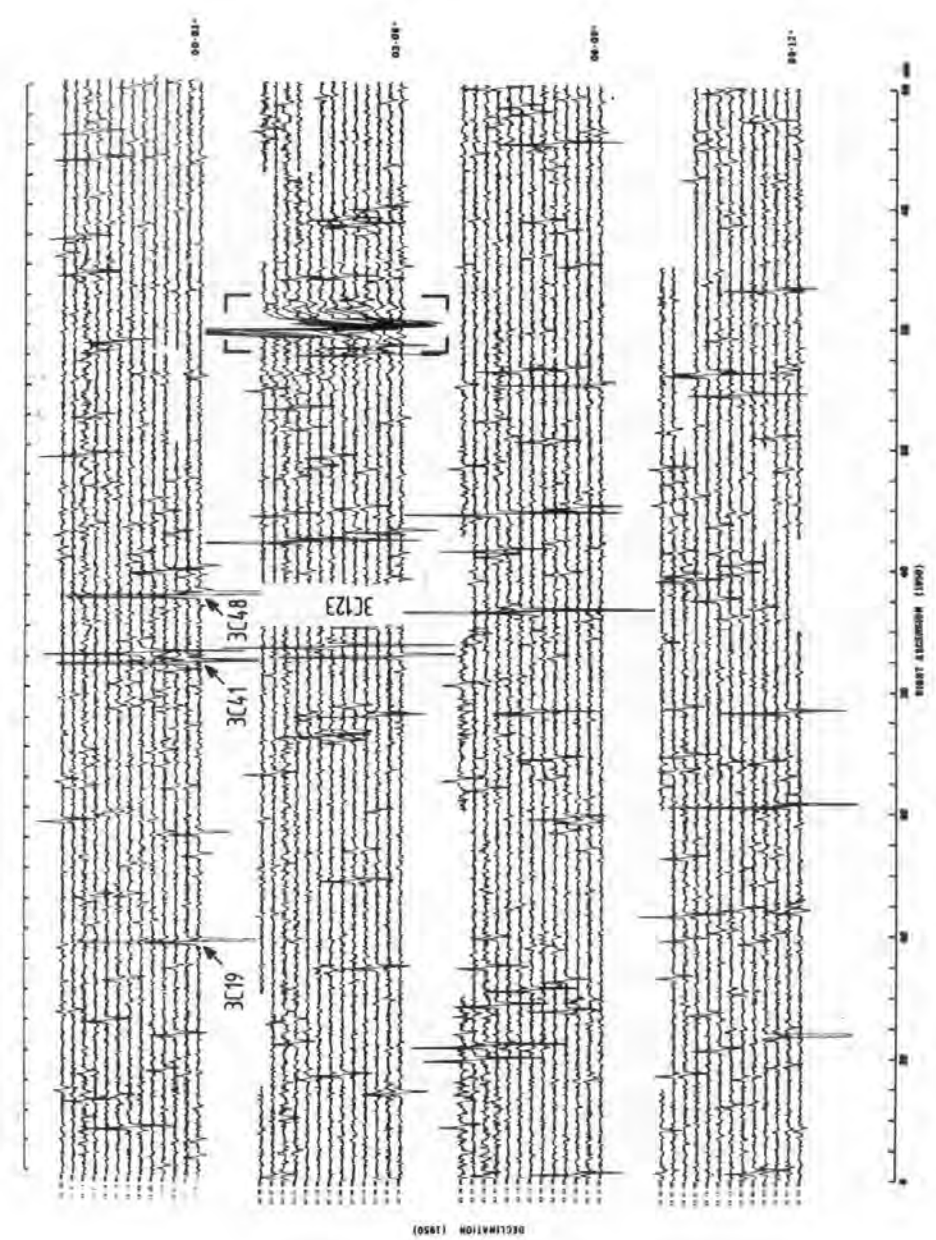


Fig. 5b— Scans from the Altschuler (1986) resurvey of this area with the more sensitive dual-channel "6/25" receiver in 1981. They approach the confusion limit.

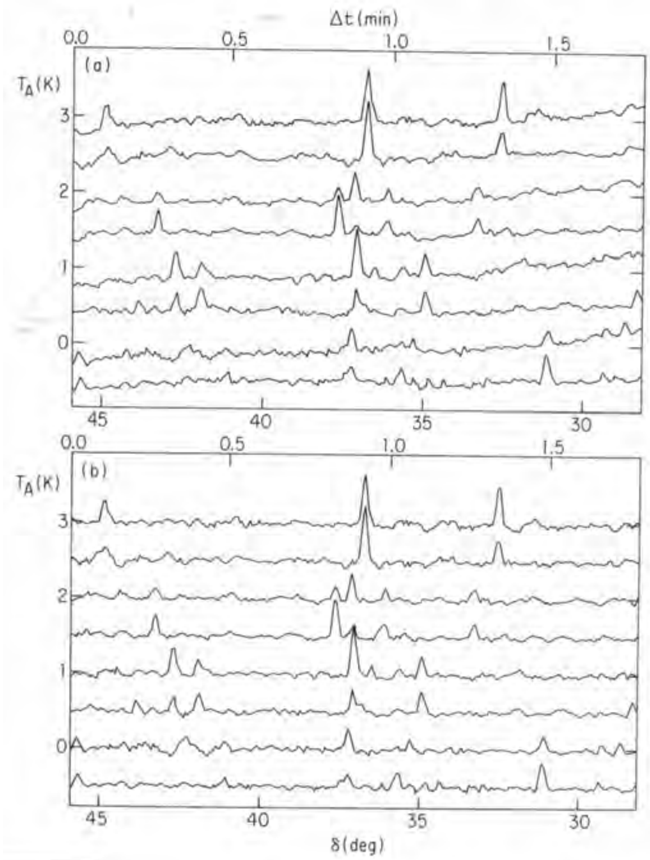


Fig. 6— Three minutes of data taken with the upgraded four-feed receiver scanned at ±10° sidereal rate.

be replaced by total-power field-effect transistor (FET) amplifiers. This change did not lower the system temperature significantly, but it dramatically increased the receiver gain stability and reliability. A sample of the total-power output from this receiver is shown in Figure 6. It was used during two three-week periods separated by six months in 1983 to scan the northern sky between declinations $\delta = -5°$ and $\delta = +82°$. All four receiver channels operated perfectly throughout the observations (not even one baseline glitch), and the receiver gains changed by less than 1% between observing runs. Such rock-solid performance makes the data analysis much easier, especially in large surveys, where the special effort required to fix up marginal data and delete bad data can be greater than the effort needed to process all of the good data automatically—the old saying that "bad data is worse than no data at all" applies to surveys with a vengeance!

The NRAO single-dish continuum analysis program (CONDAR) was written in the early 1970's and has been essentially dormant since then. It is capable of processing a limited number of (one-dimensional) scans, but it cannot make or analyze large two-dimensional maps. Since the late 1970's the major NRAO analysis package has been the Astronomical Image Processing System (AIPS), whose main function is to make maps from VLA data and to analyze those maps. By bringing single-dish data into the AIPS system, it became possible for the first time to make genuine two-dimensional maps from arbitrary (i.e., not just parallel) scans. This allows the telescope to be scanned in *elevation* at rates up to 40× sidereal and cover much larger areas of sky in a given time (most 300 Foot surveys have been assigned about one month of telescope time). After single-dish continuum scans have been calibrated and baselines have been subtracted, making a map from arbitrary scans is similar to the making of a synthesis map from arbitrarily sampled interferometer (u,v) data. The AIPS task UVMAP takes arbitrary (u,v) data samples, projects them onto a uniform grid in the (u, v) plane, smooths them with a two-dimensional convolving function, and finally Fourier transforms the gridded, convolved data to make a map in the sky (i.e. α, δ) plane. To make a map from single-dish data samples arbitrarily located in the (α, δ) plane, it is also necessary to place them on a uniform grid and convolve these discrete samples with a two-dimensional smoothing (sample restoring) function. This can be done simply by tricking UVMAP into thinking that the single dish (α, δ) data are actually (u,v) data and bailing out of the program before it does the Fourier transform. Bill Cotton wrote such a task (GRIDR) in AIPS in 1984, and it was first used to map 1400 MHz data from the FET version of the four-feed receiver. The greatest reason for using AIPS on single-dish continuum surveys is not just to make maps, but to access the enormous amount of AIPS map display and analysis software that has been written over the years. For example, the northern sky map (Fig. 1) was made in AIPS by geometrically transforming and combining 144 smaller, partially overlapping maps.

The 1400 MHz survey covering $-5^\circ < \delta < +82^\circ$ (Condon and Broderick 1985, 1986a) demonstrated that large sky maps could be made with modern multibeam receivers and data-analysis systems. But it is strongly confusion limited even though the telescope was being scanned at its maximum (slew) speed of $\pm 10^\circ$ per minute. Better surveys require smaller beams and hence higher frequencies.

What is the "ultimate" continuum survey that the 300 Foot Telescope could perform? For reasons given in §II, the best combination of resolution and sensitivity is obtained at $\nu \approx 5$ GHz, which also happens to be the current high-frequency limit of the 300 Foot. At this frequency the FWHM beam is about 3 arcmin and the rms confusion is only about 1 mJy, so we need multiple beams to cover the sky in a reasonable amount of time and very sensitive receivers to approach the confusion limit. The number of receivers is no longer limited by receiver reliability. The prime focus of an $f/D \approx 0.4$ paraboloid like the 300 Foot can accommodate seven point-source feeds in a filled hexagonal pattern (Fig. 7); adding more feeds farther from the focal axis would yield unacceptably bad patterns and sensitivity. This feed pattern is especially good for survey scans because it produces seven tracks uniformly separated by about one beamwidth if $r \approx 3$ beamwidths and $\theta = \tan^{-1}(\sqrt{3}/5) \approx 19^\circ$. It is also compact-enough that all seven feeds can fit into a single dewar at $\lambda \leq 6$ cm. The maximum number of receivers that can be used with this number of feeds is 2 polarizations x 7 feeds = 14. To get all 14 FET receivers into a single dewar, minimize cost, and maximize sensitivity and reliability, a very simple (primitive) receiver has been built—a total-power TRF ("Tuned Radio Frequency") receiver. That is, there are no input Dicke switches,

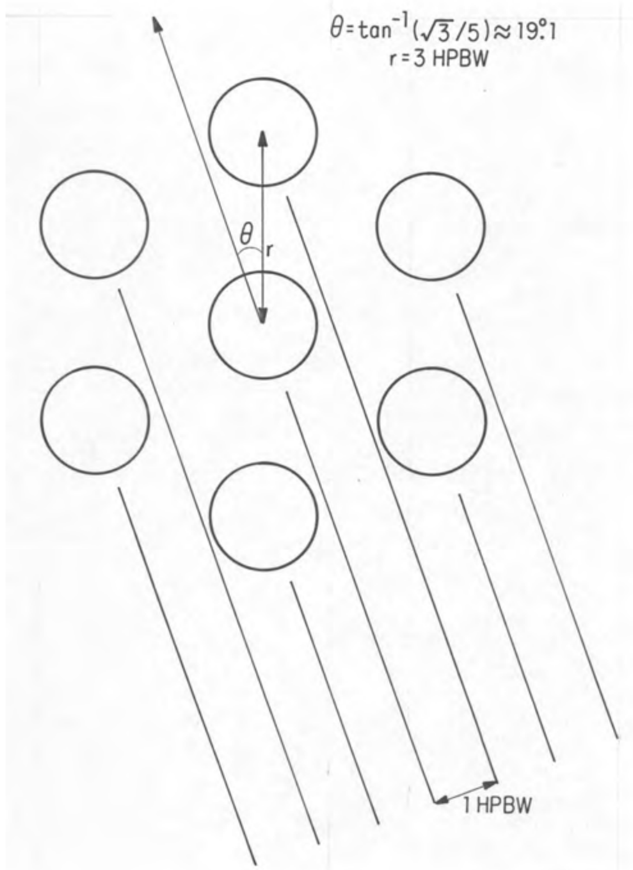


Fig. 7— The largest array of point-source feeds with good efficiency and low coma lobes that can be accommodated at the prime focus of an $f/D \approx 0.4$ reflector.

and the amplified RF signal is detected directly, thereby eliminating local oscillators, mixers, and IF amplifiers. This receiver has a 600 MHz bandwidth and is operated with a $\tau = 0.1$ s postdetection integration time, so gain fluctuations larger than $\Delta G/G = 1./\sqrt{6 \times 10^7} \approx 10^{-4}$ on times scales of seconds cannot be tolerated. The receiver engineer George Behrens took great care to minimize thermal, mechanical, and electrical sources of gain fluctuations, and the receiver operates within 20% of its theoretical noise on the telescope. The system noise is $T_s \approx 60$ K on the 300 Foot, of which about 30 K is receiver noise and 10 K is caused by ground radiation seen through the coarse mesh reflector.

Two other recent improvements to the 300 Foot are also needed for a large-scale 6 cm sky survey. The loss of gain away from the zenith is caused primarily by a lateral shift of the reflector axis, and a computer-controlled lateral focus corrector was installed to keep the feeds on the reflector focal axis. With this correction the 300 Foot is usable over the declination range $-1° < \delta < +73°$ at $\lambda = 6$ cm. The data rate for scanning the 14-channel receiver at $\pm 10°$ per minute and sampling each channel every arcmin of declination (0.1 s of time) is 140 samples s^{-1}. The new data taking and telescope control computer installed at the end of 1986 is capable (barely) of handling this data rate. (Between 1967 and 1986 the data rate of continuum surveys on the 300 Foot Telescope has had a doubling time of 2.4 years.)

The total amount of data produced by such a survey is very large, since it takes 26×24 hours of observing at 140 samples per second to cover the whole sky between $-1°$ and $+73°$ declination. Data from the first survey, made in November 1986, filled about 125 telescope tapes and were later compressed to 20 6250-BPI FITS tapes. Attempts to display and analyze the data with the existing single-dish programs (e.g., CONDAR) running on a VAX 11/780 failed miserably. Compare these data requirements with those implied by Cam Wade's memo of 1960 (Fig. 8).

[Note added December 1987: Bill Cotton has just written several new AIPS tasks to process single-dish survey data on the Convex C-1 computer. They read the FITS tapes, convert the data to pseudo-(u, v) format, subtract baselines, remove interference, and make gain and pointing corrections. The October 1987 data have already been compressed to FITS tapes and sorted by right ascension. A test map 1024×40 arcsec on a side and centered on $\alpha = 1^h 40^m, \delta = +30°$ has been made from the October 1987 survey data (Fig. 9). Even on the Convex, processing this much data is slow and cumbersome, but I am now confident that all of the 6 cm survey data can be mapped. The rms map noise is a $\sigma \approx 5$ mJy per survey, and at least 10^5 sources will be detectable (5σ) in 6 str with the data already taken.]

The main scientific goal of the 1400 and 4850 MHz northern-hemisphere surveys is to provide a radio analog of sky maps at other wavelengths (e.g., the IRAS infrared survey, the Palomar Sky Survey, or the planned ROSAT X-ray survey). If the 4850 MHz maps are plotted on the same scale as the Palomar Sky Survey plates, the beamwidth is about 0.1 inches and there are over 200 sources per $6° \times 6°$ "plate." As the survey is repeated and approaches the confusion limit, all sources stronger than $S \approx 5$ mJy will be detected. These radio maps can be used directly to study large populations of objects (e.g., to make radio "identifications" of IRAS galaxies—Condon and Broderick 1986b) or as source finders for further radio observations. For example, the NRAO VLBA currently under construction has such a narrow field of view that it is not capable of finding completely new objects—it can only map those with known positions. The 4850 MHz sky maps will contain every source strong enough to be observed with the VLBA.

V. Future Surveys

The 6 cm survey observations will probably be repeated annually for a number of years for improved sensitivity and detection of time-dependent sources. Some straightforward improvements can be made to reduce the system noise by about 20 K—the first stage FETs can be replaced by HEMTs, and a finer mesh covering could be placed on (or just below) the existing reflector surface. But really significant improvements in survey sensitivity and speed will require technological advances of the type discussed by Rick Fisher and Sebastian von Hoerner in this workshop —phased array feeds to produce very large numbers of simultaneous beams and low-noise telescope designs, for example.

NATIONAL RADIO ASTRONOMY OBSERVATORY
Green Bank, West Virginia

October 18, 1960

MEMO TO: G15D Computer Users

FROM: C. M. Wade

SUBJECT: Use of the MTA-2 Magnetic Tape Unit

At present, we have only a single roll of magnetic tape for the MTA-2 accessory. Since up to 300000 words may be recorded on this tape, it should be adequate for all of our present requirements. However, if several people use the same roll of tape concurrently, serious confusion is inevitable unless a very definite and inflexible procedure is followed in recording information.

Blocks of information on the tape are identified by file numbers, which may take on values from 0000 to 3000. File numbers impressed on the tape must be in serial order; thus a block numbered 1761 may be preceded by one numbered 1432, but not by one numbered 1848. To avoid difficulties, successive blocks on our tape shall be numbered serially, starting with file number 0101.

The following procedure, suggested by Dr. von Hoerner, should be followed in recording data on magnetic tape:

1) Find the last file number recorded in the log book; for example, 0134.

2) Search the tape for this number:
   08 1870
   15 0134

3) Write your data on the tape:
   08 1870
   12 ADDR

4) Write the new file number:
   08 1870
   13 0135

5) Enter the new file number in the log book.

If, by mistake, you write over information previously recorded on the tape, please inform the persons affected at once!

The log book referred to above is not the regular G15 log book, but separate book labelled "MTA-2 Log".

Fig. 8— Data storage in the good old days.

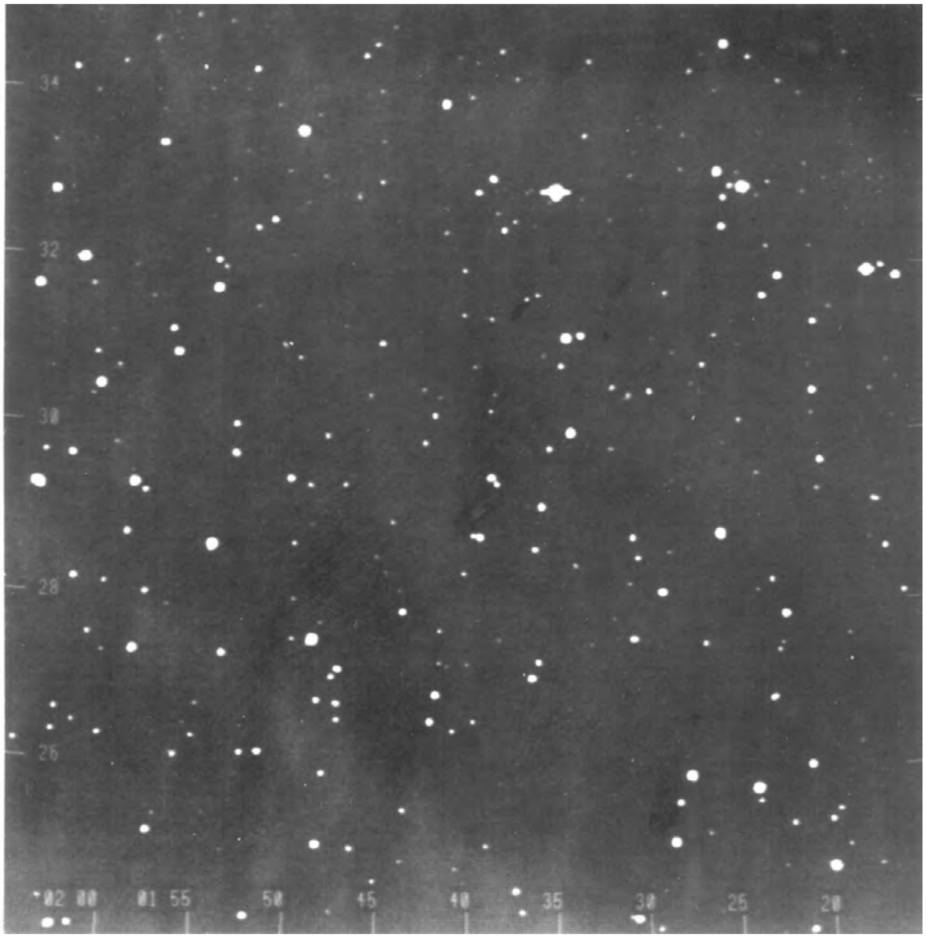


Fig. 9— First map made from the 4850 MHz sky survey. It covers about $10^\circ \times 10^\circ$ centered on $\alpha = 1^{\rm h}40^{\rm m}, \delta = +30^\circ$ with 3 arcmin resolution and $\sigma_n \approx 5$ mJy.

VI. Conclusion

The 300 Foot Telescope has been making extragalactic continuum surveys throughout its 25 year history. Figures 3 and 9 demonstrate that although the 300 Foot telescope is still only 300 feet in size, its capabilities have grown enormously. The success of the 300 Foot depends on continual improvements in a variety of areas. So long as the required manpower and money remain available, it is in no danger of becoming scientifically obsolete in the foreseeable future.

Fig. 10— Jim Condon in 1982.

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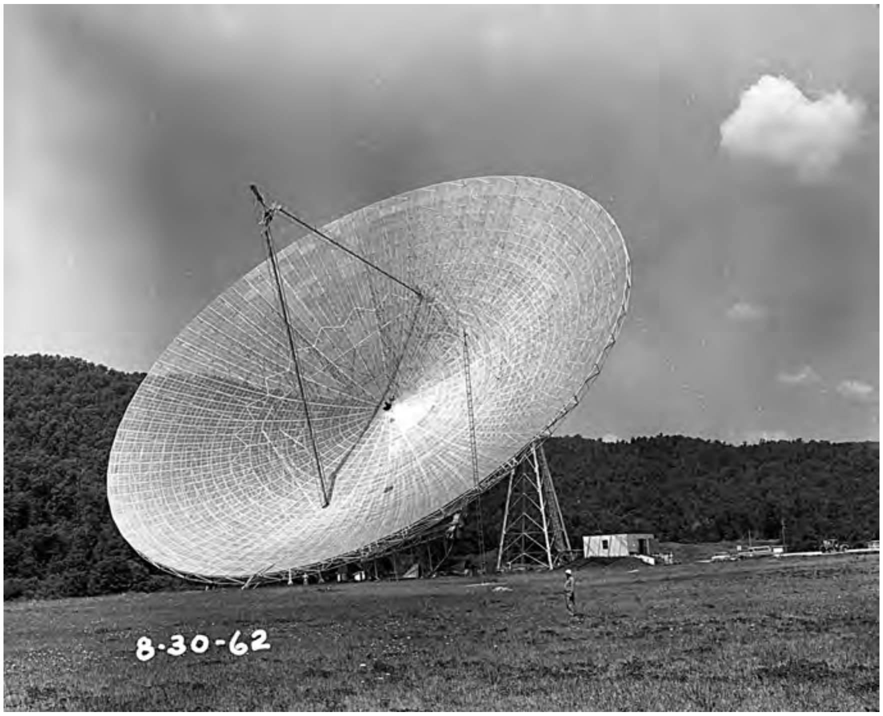
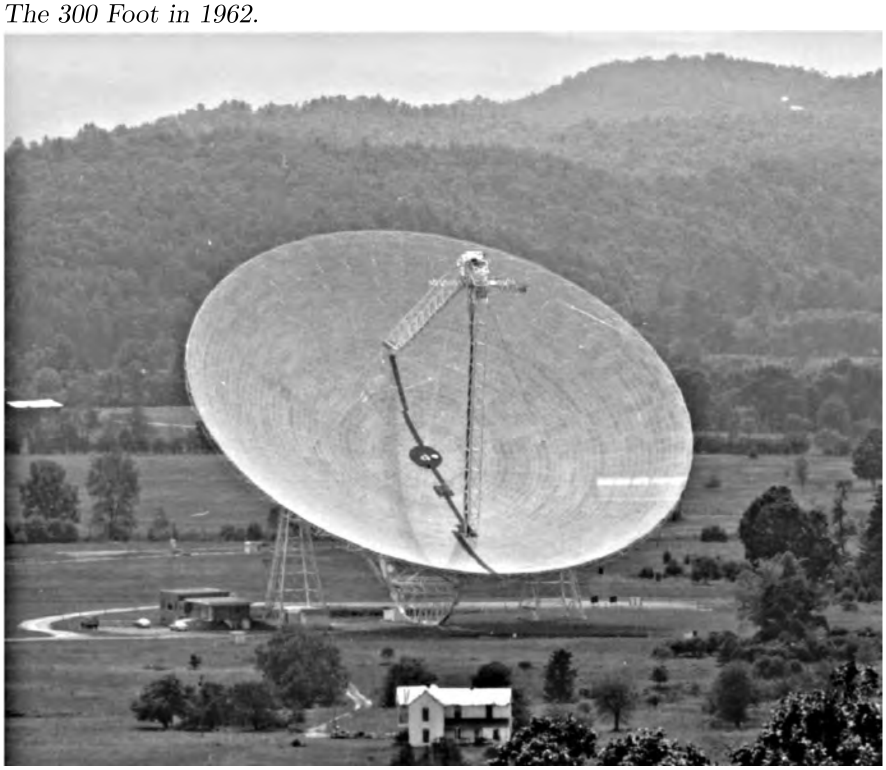
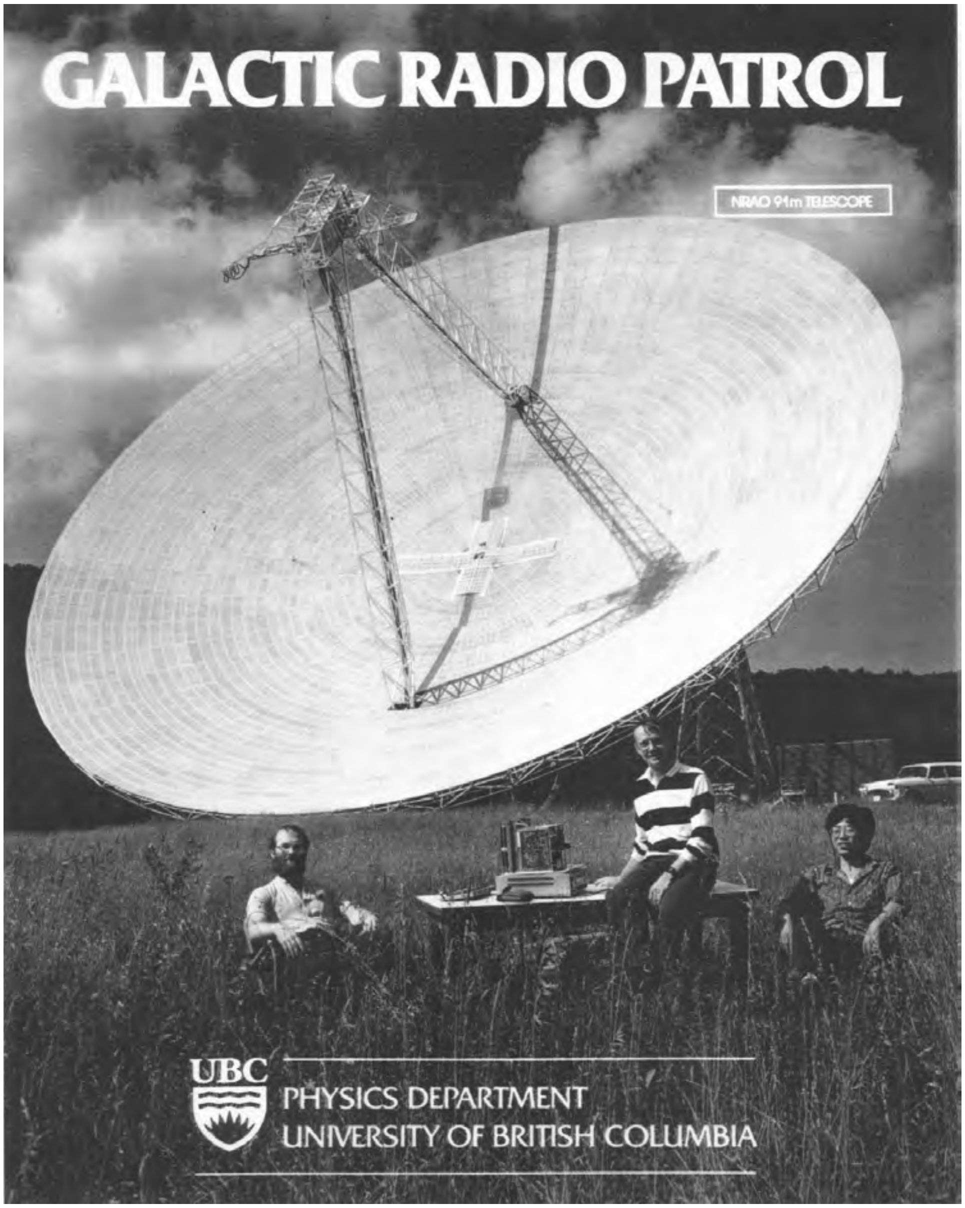


The 300 Foot in 1962.



The 300 Foot in 1979.



A Poster commemorating the galactic radio patrol.
(L to R: Chris Backhouse, Phil Gregory, and Huang-Jian Xu.)
[Courtesy P. Gregory]

300 Foot Galactic Plane Radio Patrol Project*

P. C. Gregory
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University of British Columbia
Vancouver, B.C. V6T 2A6, Canada

Abstract

This talk is a summary of the history and progress of the 300 Foot Galactic Radio Patrol Project, which has been operating since 1977. The purpose of the patrol is the detection of transient and highly variable continuum radio sources at a wavelength of 6 cm.

1. Introduction

I'd like to share with you my experience in running a large project, one which has been going on now for 10 years. The project attacks the limits of the 300 Foot telescope and I still wrestle with the challenge of how to stay on top. I will attempt to pass on some tidbits of wisdom that I've been able to glean from this experience.

The motivation for the project goes back to the fun that I had in 1972 with the discovery of the giant radio outbursts from Cygnus X-3. Some of you may remember the commotion that produced. The adrenalin that I experienced was really a double whammy because as soon as I returned from three weeks of giving birth to Cygnus X-3 outbursts, my wife gave birth to our first child. Cygnus X-3 continues to this day to be a fantastic laboratory for high energy physics (Table 1). At gamma-ray energies it appears as a 12.6 millisecond pulsar, and the cosmic ray luminosity that this object must be putting out to explain the gamma-rays that are detected is sufficient to account for essentially all of the cosmic rays in our Galaxy in the energy range 10^{16} to 10^{17} eV. Clearly sources like this are extremely interesting and it behooves radio astronomers to try and find more of them.

How do you go about looking for an object like Cygnus X-3? Since the first outburst we have witnessed a number of large outbursts from Cygnus X-3. It is one of the brightest objects in the centimeter sky for one day out of every hundred, statistically speaking. The rest of the time it's not bright enough to be detected even in modern-day surveys. Part of the reason for this is that it lies in a highly confused region of the Galactic plane. The other reason, of course, is that it normally fluctuates down at a very low level. So, if one is going to find other objects with such a low duty cycle, namely one day out of every 100, then you really have to repeatedly survey or patrol a large region of the sky. This is exciting from another point of view, since we are opening up a new dimension of time and any new search dimension is likely to turn up interesting new objects. I think we have come up with a number of interesting objects and I want to share some of them with you.

The Galactic patrol work hasn't been done single handedly. There has been one principal investigator and a large number of graduate students and summer

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* From a talk given at the 300 Foot Symposium, September 1987.

students, including Robert Braun, Russ Taylor, and Neb Duric. The project has been ongoing now for 10 years and one of the major problems has been that the lifetime of the database, from the point of view of its scientific interest, is much longer than the duration of graduate students and typical lifetime of computers. So the most challenging problems that I've faced and still face, is how to contend with issues of continuity. For example, when you introduce a new student to the project, how attractive is it to a young creative person to go back into an old database because there are some good questions to ask? That would mean that they would have to become familiar with lots of old software. Or, should you be encouraging them to do new things involving the acquisition of new observations? My answer is you really can't take them too far back. You really have to start new phases. So if you talk to me about the history of this project I'll say things like, "That's what we did in Phase One," and "Yes, that's my plan for Phase Three." You have a better chance of attracting new students if you give them an exciting new challenge. On the other hand, if you select a problem carefully enough, then you can get a bright summer student to look through some data eagerly and not worry about the fact that he hasn't taken this data and doesn't know anything about how it was calibrated.

Table 1

Cygnus X-3, A High Energy Laboratory—Brief History

Date		Discovery		Reference
1966		Discovery of X-ray source		Giacconi et al.
1972		Discovery of Giant Radio Outbursts		Gregory et al.
1972		Discovery of 4.8^{h} X-ray orbital period		Parsignault et al.
1983		Discovery of γ-rays with energy $E > 10^{16}$ eV		Samorski & Stamm
1985		Discovery of 12.6 msec γ-ray pulsar		Chadwick et al.
Neutron star	$\Longrightarrow$	$> 10^{17}$ eV cosmic rays	$\Longrightarrow$	cosmic rays interact with companion star environment $\Downarrow$
Pev γ-rays	$\Longleftarrow$	decay	$\Longleftarrow$	neutral pions
1985		Muons from Cyg X-3?		Battistoni et al. Marshak et al.

2. History of the Patrol

Table 2 shows the various phases of the project to date. It really started in 1975 with Phase 0 at the Dominion Radio Astrophysical Observatory in British Columbia, with a 25 meter dish which wasn't doing very much at the time—exactly what I was looking for. However, when observing in the Galactic plane at 20 cm

Table 2

Radio Patrol Phases

Phase	Location	Telescope	Sensitivity (mJy)	Area (steradians)	λ (cm)	Resolution (arcmin)
Phase 0 (1975-76)	DRAO Penticton, B.C.	25m	1000	0.026	20	30
Phase I (1977-84)	NRAO Green Bank, WV	91m	25 (5σ)	0.15[1]	6	3
Phase II (1986-95)	NRAO Green Bank, WV	91m[2]	25 (5σ)	6.0[3]	6	3

[1] $\ell = 40^\circ$ to 220°; $b = \pm 2^\circ$.

[2] New 7-feed, 14-channel receiver.

[3] Entire Northern Celestial Sphere between declination 0° to 73°.

with a half power beamwidth of 30 arc minutes, there is a large confusion signal due to supernova remnants and HII regions. If you want to find weak transient sources which tend to be very compact, and you want to look to the limits of your sensitivity, you have to be able to subtract off all this Galactic seaweed, which, of course, is some one else's bread and butter. The subtraction can only be done accurately if the telescope has good pointing stability. That is, it has to reproduce the same position time and time again. It turns out that the 25 m telescope was not sufficiently stable.

In 1977 I started what I call Phase I with the 300 Foot which ran through 1984, and then last year started Phase II with the new 7-feed receiver. I first became interested in using the 300 Foot because of a paper I wrote with Ernie Seaquist in 1973 on interpreting the radio continuum spectrum of V1016 Cygni, an emission line star. We were the first to point out that the $v^{2/3}$ spectrum could be explained by a r^{-2} radial electron density distribution, indicating mass loss via a stellar wind (Seaquist & Gregory 1973). In 1976 we decided to look for other examples of stellar winds using the 300 Foot to examine the 5000 early type emission line stars in the Wackerling Catalogue (Wackerling 1970). Well, after 900 negative results we lost interest in that project but in the meantime my exposure to the 300 Foot made me realize that this was the ideal instrument for my project. Just before my first radio patrol run in 1977, Ernie called me to say he had found another catalog of emission line stars which looked interesting. It was the catalogue by Stephenson and Sanduleak (1975). I decided to insert 5 of the brightest emission line stars into the patrol program as drift scans. And sure enough, I detected one of them, SS433. So, we put out a telegram and that was the very first reported radio detection of SS433 (Seaquist, Gregory and Crane, IAU Circular No. 3256 1978).

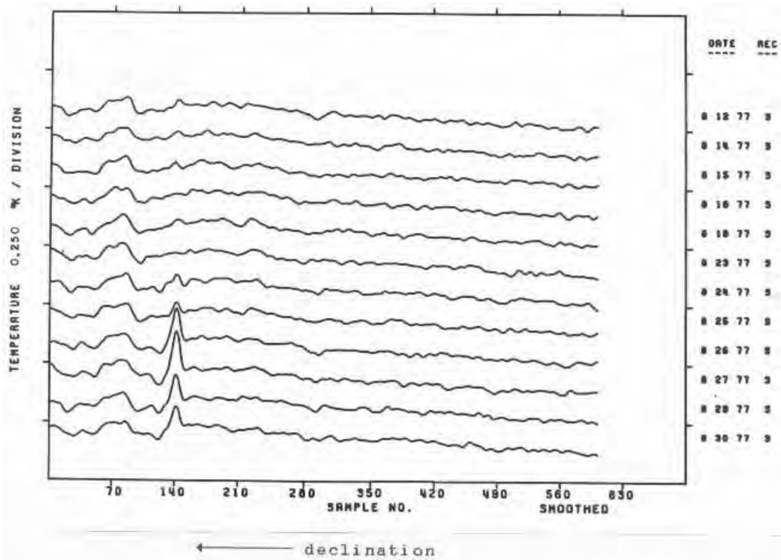

Fig. 1— Twelve repeats of one scan showing the variable source GT0236+610 located at sample 140.

3. Periodic Variable, GT0236+610 = LSI+61°303

If I had known more about the excitement that was brewing over SS433 I probably would have stopped what I was doing and focused my attention on SS433, and there would be no Galactic radio patrol because an object like that can seduce you for many years. Fortunately, I had my own excitement going because in our very first observing session we discovered a very interesting variable GT0236+610. Here it is in Figure 1. What is shown is a single scan across the Galactic plane repeated for 12 days in 1977. As you can clearly see, the scan repeats very identically except in one location (sample 140) where a source suddenly appears only to decline again in few days. This was our first variable, and the object that convinced me that this was a worthwhile project.

We have had a lot of mileage out of this source (Gregory and Taylor 1978 & 1979). It turned out to be periodic in its radio emission with a period of 26.5 days (Taylor and Gregory, 1982; 1984). Figure 2 shows the 26.50 day light curve at 2.8 and 6 cm for the star which we have optically identified with an emission line star LSI 61°303. The 2.8 cm observations were taken at Algonquin Park. Because we've now got an extensive database extending over nearly 10 years, we can examine the long-term variability of these flares. At times the flares are very low, reaching a maximum of perhaps 50 mJy, and at other times as high as 300 mJy. We have been studying the long term modulation of the flare maxima and appear to have found preliminary evidence (Figure 3) for a possible 4 year modulation. The dotted line is a relativistic precessing beam model with this period. If confirmed, this would imply a super-orbital period, perhaps arising from the precession of an accretion

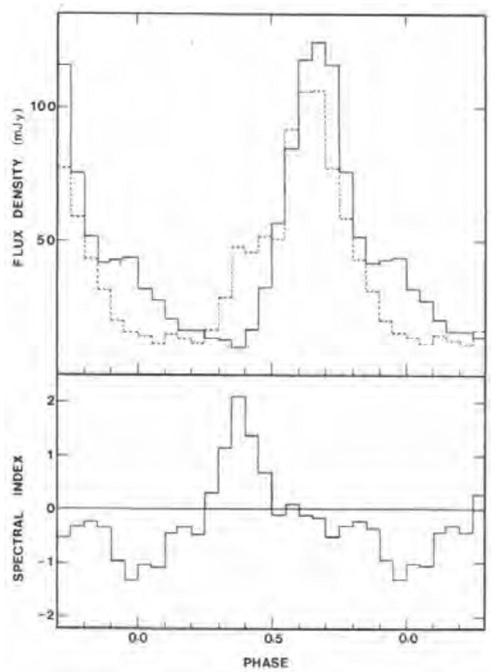

Fig. 2— Mean radio light curves at 5 GHz (solid) and 10.5 GHz (dashed). At bottom is the two- frequency spectral index derived from mean light curves.

disk or relativistic beam. On the basis of these results we would expect the next large flare to August 1989.

While writing up our results on the detection of GT0236+610, I noticed in the latest issue of Nature the first COS B catalog of gamma-ray sources (Hermsen et al. 1977) and sure enough, GT0236+610 coincided in position with the gamma-ray source CG135+01. Of course, the gamma-ray position was fairly crude (3 degree diameter error circle), but since that time there have been a good half dozen gamma-ray experiments and the only candidate remaining in all of these error boxes is GT0236+610. If it is a gamma-ray source, then it should be an X-ray source as well. We did in fact find an an X-ray source with the Einstein Observatory, but not before the γ-ray astronomers did (Bignami et al. 1981).

4. Serendipity, New SNR and Binary Pulsar

In any major survey serendipity often plays an important role. This was certainly true in our work. We had been awarded guest observer time on Einstein to look at 7 of our variable sources. However, the right ascension range excluded some of our better candidates, so to make up our quota I inserted two possible variables. In the field of one of these we found this very remarkable new supernova remnant, G109.1-1.0. It had not been found before because it is less than 2.6 degrees away from Cas A, the brightest radio source in the sky and a strong X-ray source as well (Gregory and Fahlman 1980; 1981; 1983; Gregory et al. 1983; 1985). The SNR is

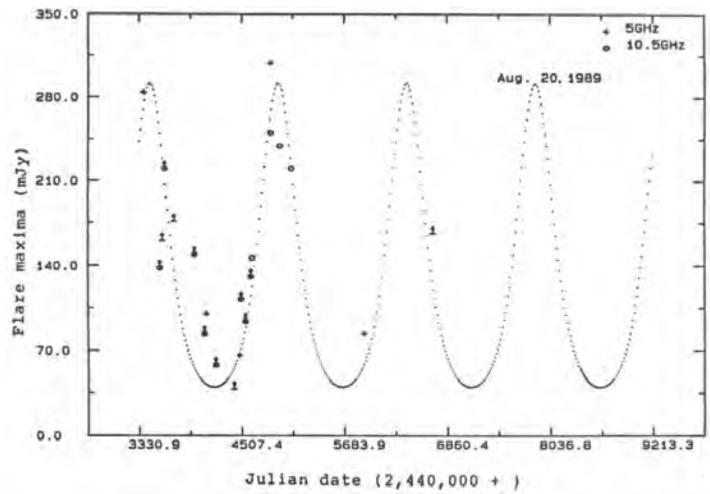

Fig. 3— Binary radio flare maxima from August 1977 to September 1986. For many flares the peak was missed and only a lower limit is available which is indicated by the arrows. The dotted line is a relativistic precessing beam model with a period of 1458 days.

semicircular in shape with an angular diameter of 33 arcmin, and a curious jet-like structure radiating out from the strong compact source.

From our X-ray timing data, we discovered that the compact source is a pulsar with a 6.978632 second period (Fahlman & Gregory 1981; 1983). The X-ray timing data also shows evidence for an orbital period of 2300 sec., indicating it is a binary pulsar. Additional evidence for the binary nature of the pulsar has been obtained at IR and near IR wavelengths (Middleditch et al. 1983; Fahlman et al. 1982).

While we were carrying out the variable source analysis I realized that with the radio patrol data base we should be able to generate total intensity maps of the Galactic plane (including our new SNR G109.1-1.0) with much better sensitivity and resolution than any previous survey. I invited Steve Gull from Cambridge to visit UBC and he installed a maximum entropy package to enable us to deconvolve our beam-switched maps. I put one of the students I had at the time, Robert Braun, onto the problem of reconstructing a total intensity map of G109.1-1.0 and here is our 6 cm radio map (Figure 4). Superposed on the radio contours is the outer boundary of X-ray emission (dashed T's), X-ray jet (fine dashed line) and CO emission contours (thick dashed lines).

The CO contours indicate the locations of molecular clouds within which a number of Sharpless HII regions are seen (S147, 148, 149, 152 & 153). Plugging in the radio surface brightness versus diameter relationship gives distances for the SNR in the range of 3.6 to 5 kpc. Recent CO observations of Tatematsu et al. (1987) strongly support the view that the SNR is in contact with the molecular cloud. Good distances are available for S152 and S153, which are thought to be associated with the cloud, of 3.6 and 4 kpc, respectively (Crampton et al. 1978).

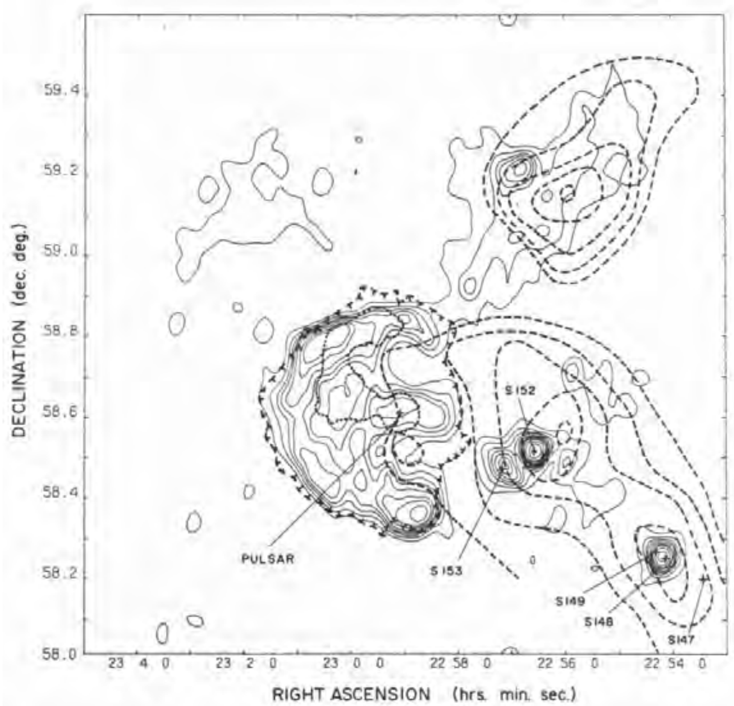

Fig. 4— 6 cm radio map of G109.1-1.0 made from the radio patrol data. Superposed are the outer boundary of X-ray emission (chicken tracks) and CO emission contours (large dash). The fine dashed curve demarcates the X-ray jet feature.

The jet-like feature in the X-ray emission doesn't have any counterpart in the radio, nor is there a radio counterpart for the X-ray pulsar. That's not surprising because X-ray pulsars are usually binary systems and there's a lot of surrounding gas which would be optically thick at radio wavelengths. In fact, most X-ray pulsars are not radio pulsars and there are a large number of X-ray pulsars known.

5. Highlights

I don't have the time to go into all of the results that we've gained from this project, but I'd like to share with you some of the highlights. We've now published two catalogs of sources from the survey. These catalogs are basically lists of sources that we have turned up and for each one we have variability information as a result of our repeated observations. Our latest catalog (Gregory & Taylor 1986) lists variability information for 1274 sources. Very few sources have turned out to be variable at the levels of variability that we are able to detect, and that needs a few words of explanation. In the catalog we claim discovery of 58 new variable sources. That doesn't mean that many of the other 1274 aren't variable, it just means that they aren't variable at the levels we can detect, which is typically 50% or greater. This is because the majority of our sources are very weak—75 percent of our sources are weaker than 75 mJy.

1. First catalog (1983). Variability information on 543 sources: 31 new variables.
2. Second catalog (1986). Variability information on 1274 sources: 58 new variables + Cyg X-3.
3. Discovery of periodic variable (P = 26.5 d) GT0236+610 = LSI+61°303 X-ray binary, probable counterpart of Cos B γ-ray source CG135+1.
4. Detection of transient source GT0351+543a > 1 Jy flare on one occasion.
5. Serendipitous discovery of supernova remnant G109.1-1.0 with central binary X-ray pulsar lE2259+586: Pulse period = 6.978632 s, Orbital period = 2300 s.
6. Proposed identification of transient gamma-ray source Cas $\gamma - 1$ with short term variable GT0116+622.
7. Proposed identification of X-ray source 4U2316+618 with short term variable GT2318+620.

What type of sources are the 58 variables? We have obtained VLA follow-up observations of most for accurate positions and structural information. The only successful optical identification has been of the periodic source, GT0236+610. The rest are all blank fields on the Palomar plates. Three different groups are now attempting deep CCD optical searches.

We have found two other positional coincidences with X-ray and γ-ray error boxes:

a) GT2318+620 falls in the error box of 4U2316+61, which must be a transient because it was not detected by Einstein.

b) GT0116+617 with Cas $\gamma - 1$. This was perhaps the sweetest detection, because of the way it came about. Cas $\gamma - 1$ is a transient TeV gamma-ray source that was detected on two occasions (Stapanian et al. 1975). I first became aware of it at a meeting of the AAS from Richard Lamb. Lamb and Weekes (1987) had proposed 4U0115+634, an X-ray pulsar, for the identification of Cas $\gamma - 1$. The X-ray source lies just outside the nominal gamma-ray error box. The radio patrol archive provided upper limits to any radio emission from 4U0115+634 on 18 days. We then noticed that the position of one of our variable sources, GT0116+622, coincided closely with the center of the gamma-ray error box. For GT0116+622 we were able to extract 53 daily flux density measurements between 1977 and 1984. The source varied by a factor of 7 and showed significant variability on time scales as short as one day, with some evidence for a characteristic timescale of 5 days.

I think this is potentially a very exciting object. We have an accurate VLA position, a spectrum, and it is a point source at 6 cm in A configuration (Gregory and Taylor 1981; Taylor, Seaquist & Gregory 1984; Duric, Gregory & Taylor 1987). As usual there is no optical identification. When it comes to optical identifications I find it very discouraging working in the Galactic plane; in general all we are

Table 3

Highly Variable Radio Sources ($S_{\rm max}/S_{\rm min} \geq 3$)

Name	$S_{\rm max}/S_{\rm min}$	$S_{\rm typ}$ (mJy)	VLA Radio Structure	Distance	Identification
GT0026+627	5.2	40	Extended	Extragal.	
GT0116+622	10	35	Point $\leq 0.1''$		CAS $\gamma - 1$
GT0232+589	5.5	20	Jet $3''$		
GT0236+610[1]	28	50	Point $\leq 0.1''$	2.3 kpc	Periodic 26.5d
GT0244+586	3.1	20			
GT0351+543a	> 40	< 1			Transient
GT0459+415	3.4	220	Point $\leq 0.1''$	Extragal.	
GT0508+380	4.7	100	Point $\leq 0.1''$		
GT0545+265	66	2	Nucleus of triple		
GT0556+238	8.2	400	Point $\leq 0.1''$		
GT1953+306	3.7	30			
GT1945+241	10	2	Nucleus of triple		
GT2030+407[2]	6.7	100	Jets $0.1''$	≥ 11 kpc	X-ray binary
GT2100+468	3.1	180	Point $\leq 0.1''$	Extragal.	
GT2129+492	34	0.5	Nucleus of triple		
GT2134+536	4.8	10	Point $\leq 0.1''$		
GT2318+620	4.0	20	Point $\leq 0.1''$		4U2316+61

[1] LSI 61°303; X-ray binary; γ-ray CG135+1.

[2] Cyg X-3; $S_{\rm max}/S_{\rm min} = 2000$; γ-ray binary.

successful in doing is ruling out nearby objects such as RS CVn and Algol-like variables.

Table 3 gives some of our more highly variable sources. These are ones which have varied by a factor ≥ 3. Notice that Cygnus X-3 appears on this list. We rediscovered this object and if we hadn't known about it's previous spectacular history, we might not have concluded it was a particularly interesting object compared to other sources in the list. Based on our observations, it has an $S_{max}/S_{min} = 6.7$ but there are several others which appear more variable. Any attempt to optically identify Cygnus X-3 would also have been negative. This means that, potentially, some of these other objects may turn out to be as exciting as Cygnus X-3. But the actual slogging that you have to do to get a better understanding of any one of these sources takes many years. I think the value of any major new survey is often not appreciated in the first few years and I'm hoping the big reward will come 5 to 10 years from now, perhaps when the next generation of X-ray and γ-ray telescopes are launched.

Objects shown as extragalactic are those for which we have neutral hydrogen absorption measurements. In fact, there are quite a few more of these now but I haven't got the data yet. Russ Taylor has been doing a program of neutral hydrogen absorption measurements at Westerbork and the VLA. There's a good indication—well my gut feeling is—that many of these sources are extragalactic.

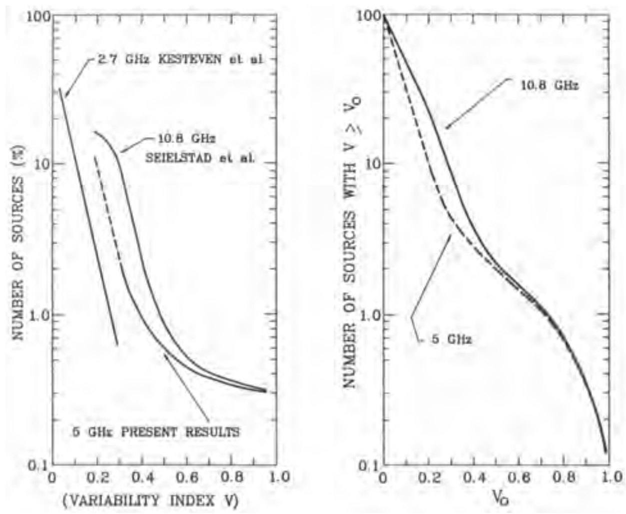

Fig. 5— Left: Source fraction as a percentage of complete sample versus variability index V. Right: Integral source fraction as a percentage of sample having $V \geq V_0$.

6. Variable Source Statistics

In closing I would like to share with you some interesting statistical games that we have played with these sources. We decided it would be worth trying to compare our results with other variability studies done at centimeter wavelengths of complete samples, that is, sources selected in some complete sense. The ones that came to mind were a subset of the Kesteven, Bridle and Brandie variability study at 2.7 GHz of sources selected at 1.4 GHz, and the work of Seielstad, Pearson and Readhead at 10.8 GHz of a complete sample selected at 5 GHz. If you plot the number of sources expressed as a percentage of the sample versus an index of variability which I call V:

$$V = \frac{S_{max} - S_{min}}{S_{max} + S_{min}}$$

where infinite variability = 1 and no variability = 0, you get the rather interesting set of curves shown in the left panel Figure 5 (from Gregory & Taylor 1986).

One finds that there's an exponential describing the low V end which appears at both frequencies. The frequency dependence apparent at low V has already been described in the literature although not shown in this particular fashion. Our own results are not reliable for low values of V < 0.3 but they are for V > 0.3. Our results are not too different from those of Seielstad for high V and there's a good suggestion here, and only a suggestion at this point because of the limited numbers of sources involved, of two distinct populations, with a division around V = 0.5. Highly variable sources are on one side and the not so highly variable sources on the other. Another characteristic that we can identify with the high V sources is that 93 percent of our sources in that range are daily variables whereas only about 40 percent in the low V range are daily variables—they tend to vary on a longer time scale.

Taking these data one step further, you can then construct the analogue of the log N – log S curve for variability. The right panel of Figure 5 shows the number of sources expressed as a percentage of the complete sample with $V > V_0$ as a function of V_0. For the first time we can read off what fraction of any complete sample we expect to vary at what percentage level and again you see a bump occurring around $V = 0.5$ which seems to show up at the 2 percent level. So the implication from comparing very different samples is that in any complete sample selected at 5 GHz approximately 2% will be highly variable sources. One can estimate then that there are, for example, down to the hundred mJy level, about 400 of these around somewhere in the sky and we've only found 58. We have quite a ways to go.

Birthday Present

That is the end of my scientific presentation. However, I do have one other presentation to make to help celebrate the 25th anniversary of the 300 Foot. I brought along a birthday present. I'm not quite sure who to give it to, but perhaps John Findlay would be kind enough to accept this present on behalf of the 300 Foot.

Findlay: "It's not explosive is it — shall I open it? — Let the record show that this is a very nice picture." (Framed poster of the Galactic Radio Patrol)

Discussion

John Broderick: Do you think any of the variable sources might be occultation events like those that the Naval Research Lab group has seen?

Gregory: It is a very good possibility. One of the problems is that we don't have data sequentially everyday for a long period of time the way they do.

Broderick: You observe sequentially every day for short periods of time.

Gregory: Typically the repetition interval is 3 to 4 days during month-long observing sessions, then there is a gap until the next year.

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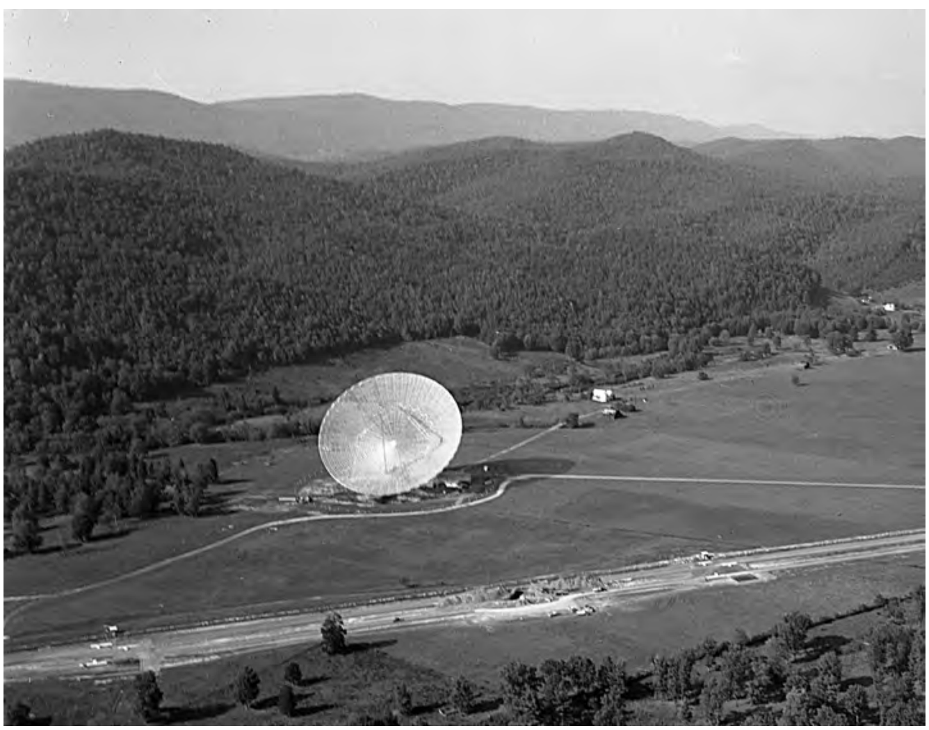
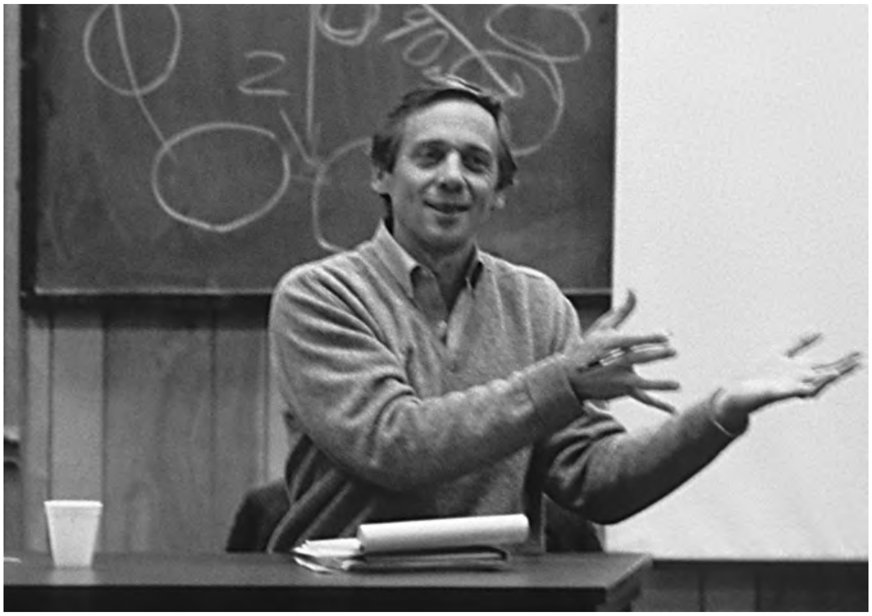
Bill Meredith at the IBM 1620 computer in the Jansky Lab, April 1963.



Aerial view of the 300 Foot Telescope in 1965.



Don Backer at a GBT planning meeting, ca. 1989. [photo courtesy R.Rood]

No. 2110

PULSATING RADIO SOURCES NEAR CRAB NEBULA

W. E. Howard, III, National Radio Astronomy Observatory, Green Bank, West Virginia, transmits the following information from D. H. Staelin and E. C. Reifenstein, III: "Two pulsating radio sources have been found with the 300 ft. transit antenna of the National Radio Astronomy Observatory. These sources, tentatively designated NP 0527 and NP 0532 pending the determination of more accurate positions, are both located in the vicinity of the Crab Nebula and could be coincident with it. Each source was observed on three days in October, 1968 with a 50 channel receiver covering the band 110-115 MHz with 0.1 MHz resolution. Both sources are so sporadic that no periodicities are evident. The pulse width appears less than 120 ms. The maximum observed pulse energies for NP 0527 and NP 0532 were 207×10^{-26} and 16×10^{-26} ($jm^{-2}Hz^{-1}$), respectively. Other source parameters are:

NAME	α_{1950}	δ_{1950}	$\int N_e d\ell$ cm^{-2}	Period
NP 0527	$5^h27^m \pm 6^m$	$+22°30' \pm 2°$	$1.58 \pm 0.03 \times 10^{20}$	$<0^s.25$
NP 0532	5 32 ±3	+22 30 ±2	$1.74 \pm 0.02 \times 10^{20}$	<0.13

The discovery of the pulsar in the Crab Nebula by Staelin and Reifenstein, using the 300 Foot Telescope. From IAU Circular No. 2110, 6 November 1968. This was the first pulsar discovered to be associated with a supernova remnant and clinched the identification of pulsars with neutron stars. (The Central Bureau for Astronomical Telegrams, International Astronomical Union)

A Pulsar in CTB 80*

or "I found this preprint in the Jansky Lab. . . . "

D. C. Backer

Astronomy Department and Radio Astronomy Laboratory
University of California at Berkeley

Introduction

The subtitle of this talk is from a phone conversation in June of this year: I called up my colleague Shri Kulkarni and said, "I found this preprint by Richard Strom (1987) on the shelf in the Jansky Lab." The story that I will tell you is what unfolded from that conversation. But I first want to go back a little bit and mention a few other incidents. A question that was circulating in 1986 was: "What is this object in the globular cluster M 28?" Kulkarni, Trevor Clifton and I set about answering that question. Even further back in 1979 a question circulating was: "What is this object 4C 21.53?" A group of us were involved in answering that one.

I am going to give a personal chronology and incomplete history of a number of radio astronomical studies—objects & topics: investigations of the Crab Nebula, 4C 21.53, M 28 and CTB 80; radio wave scattering by plasma irregularities in various intervening plasma media; and a few thoughts on surveys, the topic of next week's workshop. In the late 40's the radio source Taurus A (the Crab Nebula) was found by Bolton & Stanley (1949). In the next decade the Third Cambridge (3C) survey of radio sources was conducted. In the 60's interplanetary scintillation (IPS) was stumbled upon (Clarke 1964; Hewish, Scott & Wills 1964; see the historical account by Cohen 1969) and numerous investigations, both *of* IPS and *using* IPS were started. The IPS discovery contributed to early studies of a compact source in the Crab Nebula. This was a *very* strange object, and it is the starting point of this talk. The 4C catalog was available in the 60's, and a survey of the IPS of the 4C objects was conducted by Readhead & Hewish (1974, 1976). With this as an introductory sweep through events, let me turn to my personal experience.

IPS, Crab object, ISS and 4C 21.53/B1937+21

I showed up at Jodrell Bank/University of Manchester in 1966 to do an MSc degree. In the summer of 1967, with the guidance of Henry Palmer and Peter Wraith, and with the additional tutelage of a few gentlemen in this current audience, I did a 38-MHz interferometer experiment between the Mk I and Mk III

* From a talk given at the 300 Foot Anniversary Symposium, September 1987, revised November 1999. By coincidence, Backer was scheduled to use the 300 Foot during the Symposium, hence his occasional reference to observations made "last night."

telescopes looking at Cygnus A and Cassiopeia A. Our calibrator for that experiment was the "well-known" 200-Jy point source in the Crab Nebula. "Everybody" knew that it was a good calibration source for low-frequency interferometer measurements. Andrew, Branson & Wills (1964) discovered the compact source with a lunar occultation. Hewish & Okoye studied the Crab object further: first with interferometry (1964) and later with IPS (1965). Well, the moral of this tale is that your calibrator may be more interesting than your program source. Pulsars were discovered that year as a serendipitous byproduct of IPS investigations (Hewish et al. 1968). Here is where the 300 Foot enters my tale: Staelin and Reifenstein (1968) used the 300 Foot to discover impulsive radiation from the point source in the Crab Nebula.

The pulsar phenomenon led to the discovery of interstellar scattering of radio waves (ISS) which, although few people realize this, would have come out of IPS investigations even if pulsars did not exist (Readhead & Hewish 1972): there was a zone of avoidance of objects displaying IPS along the Galactic plane, and the IPS investigators would have been clever enough to have figured out that angular broadening by interstellar scattering caused the zone of avoidance. In turn, the angular broadening which results from scattering by the interplanetary medium was observed (Hewish 1955; Vitkevich 1955) prior to the discovery of interplanetary scintillation, which required observation of very compact extragalactic sources.

In the early 70's Bill Erickson, Tom Clark, John Broderick and others were doing VLBI on the compact source/pulsar in the Crab Nebula and other objects. They used the 300 Foot in this work at very low frequencies (Erickson et al. 1972; Vandenberg et al. 1973). A particular reason for mentioning this effort is that they came up with a rather clever technique for detecting pulsar signals in the VLBI correlator. They offset the fringe rate and delay by harmonics of pulse period in time and in frequency to extract harmonics of the pulsed amplitude.

The idea of a zone of avoidance of IPS objects along the Galactic plane led to the "scintar" notion to designate the few objects which violated this zone of avoidance. J. J. Rickard, Will Cronyn, and many others were involved in these mid-70's developments (Rickard & Cronyn 1979; Rickard et al. 1983). The 4C 21.53 object was the prime scintar example. As I recall, I learned about this object from Tony Readhead during a pulsar workshop at Caltech in late 1978. In 1979 Dave Cudaback, Stu Vogel, John Middleditch, Mike Davis, Val Boriakoff, and I all participated in various attempts to find pulses from this object. Bill Erickson (1983), Alan Purvis (1983), and others were conducting parallel investigations. These efforts, along with important imaging at Westerbork by Miller Goss and an inspirational talk with Franco Pacini on the bus en route to Delphi during the 1982 IAU meeting in Patras, led to our detection of B1937+21, the 1.6-ms pulsar, at Arecibo in late 1982 (Backer et al. 1982; see the account in Backer 1984). This object suggested that there was a class of rapidly spinning pulsars distinct from the slower ones. The explanation for these objects had its origin in the late 70's with the realization that in a binary system you could spin up a neutron star to fast periods even though it started old and spundown (Blandford & Teukolsky 1976). I will come back to the recycling of neutron stars as pulsars in a moment.

Recycled Pulsars, FPSM, and M 28/B1821-24

So there was a new category of objects in the early 80's—the spunup, or recycled, millisecond pulsars (Backer & Kulkarni 1990). This led to a number of surveys on the 300 Foot Telescope. Mary Stevens and collaborators (1984) at Berkeley investigated the Galactic plane for objects that showed ISS independent of whether they were pulsating—the analog of the Cambridge IPS survey mentioned earlier. There are still some pulsar candidates from the ISS survey —in fact we looked at some of them last night on the 300 Foot. What we are doing now on the 300 Foot is a survey for millisecond pulsars. Other people are doing similar surveys at other telescopes around the globe. We have developed a signal processor that we call the Fast Pulsar Search Machine (FPSM, Backer et al. 1990); you are all invited to come down and see it in operation at the 300 Foot on the weekend. One key design element of the FPSM is the use of an autocorrelator as a multichannel sampler instead of a filter bank. This design came to me one day while driving down Interstate 5 in California during August 1983 after a VLBI run at our Hat Creek Radio Observatory. I thought, "Gee, if Bill Erickson (1972) used a CROSS correlator to detect pulsars, then I ought to be able to use an AUTO correlator to do the same thing." That is what we have now produced in the FPSM.

I *will* get to CTB 80 in a moment, but before I do that we have the M 28 time line to cover. The idea that came after the first millisecond pulsar, that pulsars could be recycled in binary systems to very short periods, led Hamilton, Helfand and Becker (1985) to do a survey at the VLA in 1983 of globular clusters. They looked for steep spectrum objects that might be pulsar candidates within the core radius of the cluster. That survey came up with one object, in the cluster M 28, out of a survey of a dozen clusters. Erickson et al. (1987) looked at this object at low frequencies at Clark Lake and at the VLA. They found that it had an extremely steep spectrum, and with the VLA at 327 MHz, determined that it was strongly polarized. Well, that gets me back to my introduction. In late 1986 Shri Kulkarni, Trevor Clifton and I were sitting in my office talking about various things. We said: "There is a steep-spectrum, polarized point source in M 28; it *is* a pulsar, and it *is* our job to go out to determine what its period, *merely its period*, is." But it is not a simple task to detect a 1-mJy radio source at 1400 MHz that you believe is a pulsar, when you do not know its period or its dispersion measure. We wanted to throw the book at it, and developed Plan A and Plan B approaches.

Plan A was to take our FPSM to the VLA, record data, and drive up to Los Alamos and meet up with John Middleditch who, I believe, has the world's record for doing Fourier transforms of excessive length. He is working now with up to 32 million points, and wants to try 512 million in the near future. Well, as we thought about the VLA, and the effort that it would take to carry our machine there and get set up on a new telescope, we thought of Andrew Lyne in Manchester who had access to a large collecting area at Jodrell Bank which was within a factor of two of the VLA. We decided to call him up and see if he would be interested in collaborating by getting data there first (Plan B). So in December 1986 and January of 1987 Andrew did observations. The data from these observations came first to Berkeley and then to Los Alamos. John Middleditch had to add dedispersion code to his analysis which is something he did not need for his optical data. In May of this year he came up with a 3-ms pulsar which Andrew promptly confirmed (Lyne et al. 1987). This is an observation that with our machine in its present state we could have done on the 140 Foot Telescope! However, that's hindsight—we were not ready at the time. The 3.0-ms pulsar B1821-24 is located in the center of the globular cluster M 28. We have updated the period and determined the period derivative of this pulsar with 140 Foot observations (Foster et al. 1988). I won't elaborate on the astrophysical consequences of this measurement despite the interest.

CTB 80/B1951+32

This brings me, at last, to my final object, CTB 80. CTB is designation of the Caltech B survey (Wilson & Bolton 1960). CTB 80 is a Galactic object which was understood in the 70's to be a supernova remnant—a flat-spectrum, polarized Galactic source. Later the notion of a composite supernova came into use to describe those with a flat spectrum interior and a steeper spectrum surrounding shell. CTB 80 is a classic example of this except that its exterior with its "wings" is not quite a shell. With the Einstein Observatory an X-ray nebula and point source were found in this object (Wang & Seward 1984). In 1984-85 we had done searches for pulses from CTB 80 at Arecibo; I will come back to these data in a moment.

The "preprint on the shelf from Richard Strom (1987)" that I saw in the Jansky lab said that there was a polarized, point radio source in CTB 80 near the X-ray position. While Kulkarni and Clifton were not initially planning on coming to Green Bank for the observations in June 1987, after I saw the preprint we "brought in the SWAT team" and got ourselves going in a mode where we could record longer data streams from our machine than we could process in real time for offline processing. That led in early July to the detection of a pulsar with a period of 39 ms. At the same time, Andy Fruchter and Joe Taylor, triggered by the same preprint, were analyzing data taken at Arecibo. We also had a Plan B to go to Arecibo if we failed to find the object with our 300 Foot data. The discovery results were published by Kulkarni et al. (1988) and Fruchter et al. (1988).

Now for another moral from the 1984-85 pulsar search at Arecibo: Mary Stevens, who led the Arecibo search, processed all her tapes at the observatory with a short Fourier transform analysis and did not find anything. She then brought the tapes back to Berkeley, and further processed four out of the five data sets with a 1 million point Fourier transform code and still found nothing. The fifth data set was hiding behind a double file mark which designates EOT, or end of tape. A month ago she went back and got the data set from behind the EOT mark and processed it; the CTB 80 pulsar signal was detected in this hidden data set, and if she had analyzed it, she certainly would have been the first to find the pulsar. Science is cruel—you have to turn over every rock!

There is an optical nebula in the core of CTB 80 that surrounds the pulsar. It is about an arcminute in size and is lit up by the rotational energy loss of the pulsar. The X-ray nebula is centered in the core. All this emission is better termed today as a Pulsar Wind Nebula (PWN) rather than a supernova remnant (SNR), although this is a somewhat semantic detail. The SNR pieces are on a much larger scale and are just about to fade into the ISM. We now have a period and period derivative from Arecibo but have also detected the pulsar last night on the 300 Foot with the FPSM in its timing mode. Other properties of the pulsar such as its proper motion may be determined from timing observations underway, and will shed new light on the radio nebula CTB 80 (Foster, Backer & Wolszczan 1990).

Discussion

Tom A. Clark: Did you find any more pulsars last night?

Backer: No we didn't, although we have been processing the data all day.

John J. Broderick: I have a minor comment. You have Becker and Helfand listed in the M 28 time line. They were also almost involved in the 4C 21.53 discovery. Becker was at VPI at the time and he and Helfand had observed this source at the VLA, found out about Erickson's observations, and said: "This must be a pulsar. How do you use Arecibo to look for pulsars?" and while trying to figure this out, you in the meantime discovered it. They were running right behind you.

Backer: I have certainly not mentioned everyone's effort. I have a repeated sense of *deja vu* in these experiences. These days, when I walk into a preprint room in June and read a report dated March about which nothing has been done, I am surprised. This is part of the fast turnaround that we are forced into by electronic communications. But I can go back to the point source in the Crab. That object was sitting there and someone should not have let it lie without chasing it to its final conclusion, though to be fair, I do not know what people could have done at that time.

Bernard F. Burke: Do we have enough fast stable pulsars around so that it is conceivable that they provide an ultimate coordinate system ?

Backer: At the present time the millisecond timing at Arecibo, or elsewhere (although because of sensitivity Arecibo is best), provides a clock coordinate frame. They give you "t" but not α/δ.

Burke: It does not have to be α/δ. Someone else can do that.

Backer: Not to the accuracy that one needs for timing. The pulse timing with reference to an atomic clock only takes one object. If you have a pair, then one can be measured against the other. There are astrometric errors of the Earth's location as a function of time. These have a dipole signature and we need two pulsars widely spaced to limit them. We don't quite have that, although the purpose of our 140 Foot observations timing the M 28 pulsar against 1937+21, which are separated by 45 degrees, is a start in this effort. If, as some cosmologists would have us believe, there is a gravitational-wave background from the early universe then you need at least three sources to assess its presence. We don't have that. We barely have two that are in independent directions. (1620-26, which was discovered after this talk, is also independently directed by the same degree).

George A. Seielstad: Rachel Dewey has been involved in this same business. Do you want to make any comments?

Rachel J. Dewey: We have not found any pulsars which are nearly as exciting. I did the last slow pulsar search on the 300 Foot. Now I am in the same game as this at Arecibo trying to find fast ones. So far we haven't been as lucky. Actually we haven't been as smart. I was here in May and didn't read the preprints.

T. K. Menon: That brings me to a related question. It used to be said that there are sources in the literature that are not being followed up and lie fallow for years. Now you are talking about days on the preprint shelf. Is this fundamentally going to make a significant difference in the pursuit of science?

Unidentified: Yes, before long if you are not on DECNET you are going to be out of it.

Menon: Is that good or bad?

Backer: I am upset about the speed with which one has to operate. But I think all areas of science have phases. There is a phase when someone makes a startling jump and others can make further steps quickly—things change very rapidly in the matter of months or a few years. But after a while you know where pulsars are or where the Hydrogen is, and everyone can pursue further investigations from the literature. We are all more vigilant today for the pulsar leads than we were five years ago.

Menon: But are we being vigilant for the same things? We are merely vigilant in some ways. But are we missing some things for which we are not being vigilant? There is the argument as to whether pulsars could have been detected earlier than they were, or whether the detection only happened when it did because that instrument was run in that particular mode—that is, nobody else in the world could have seen it.

Backer: The moral of the pulsar discovery is the value of a complete survey. That's the key: they were doing a complete survey at Cambridge. It happened to be with a new time constant, too. Other people were doing IPS, but could have thrown the scruff out if not done in the same way. And there is also a bit of luck.

C. Glyn T. Haslam: We missed the brightest pulsar years before Cambridge [found it]. [During the Jodrell 408 MHz survey] the pen went mad, we gave it a kick, and it went quiet again.

Seielstad: You have to have a complete survey, but someone has to be willing to look at things that don't fit in the framework that you are looking for. And that's where scientific judgment comes in, because a lot of the junk on the periphery is junk, and you waste your time trying to thrash through it. But some people stay with it. I have a story like this on IPS. A radio astronomer, whose name will remain anonymous, told me that he was observing at 430 MHz, looked at 3C 454.3 as a calibrator, and the pen was going like this [gesticulation]. He said, "Oh God, urgh" so he whipped to another calibrator and marched on. But it was at the right place for the sun at that time of year. He had seen IPS, but, of course, galloped over it because it was not part of what he was after. Your best comment is "science is cruel."

Clark: Another really important purpose that the 300 Foot has served over the years (Bernie Burke touched on it earlier) is that the suitcase observers could make use of this as an instrument for testing some rather weird and wonderful things. I am thinking of the experiments that Joe Taylor did. His group would bring in their own special widgets. We also did the same thing by bringing VLBI equipment to the place. I was thinking in retrospect about how many operators of the 300 Foot were ready to kick us out because we would descend on the place with our boxes of equipment and cables and so forth. In fact, some of them even pointed this out to me as recently as this last night! They remember this from 15 years ago. The ability to have an instrument available where you can go test something new and exciting is an extremely valuable resource that we can't lose sight of. The fact is that people in other laboratories have interesting new ideas that they want to try. They really must have a place where they can be welcome and can hang those things on a telescope.

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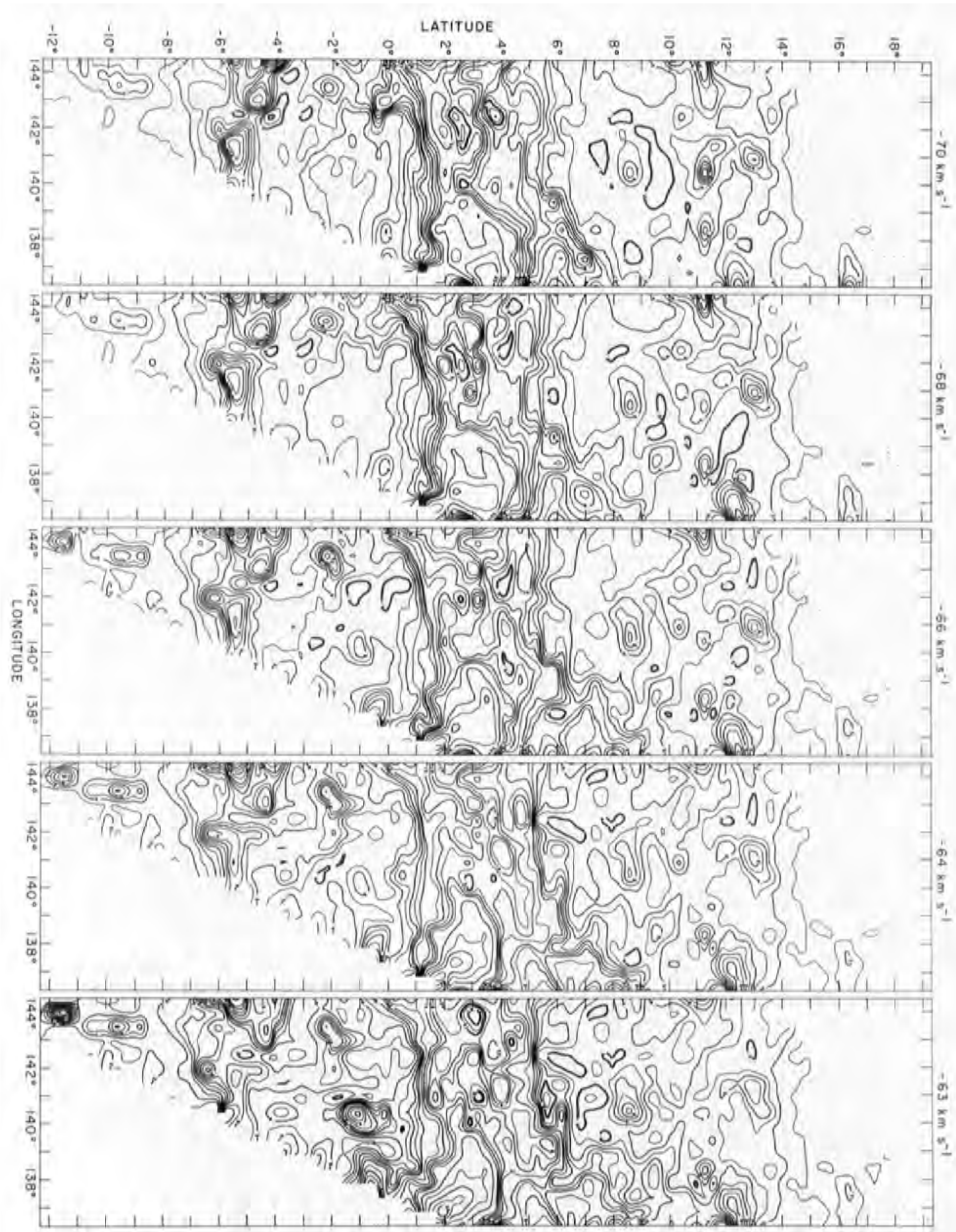


Verschuur: Fig. 1 — Unpublished HI channel maps of a region of the galactic plane — latitude +18 to -12°, longitude 137–134°.

How the 300 Foot Changed My Life *

Gerrit L. Verschuur
Lakeland, TN

I was at Jodrell Bank for 7 years before arriving at the NRAO in 1967 and want to give a brief flavor of our distant impression of what was going on at the NRAO. I can remember stories that the 140 Foot was in trouble and the 600 Foot a disaster and then suddenly, out of nowhere, there was this operational 300 Foot telescope. We (the graduate students) didn't know how on earth that came to be, because as far as we knew there was nothing being planned. Our impression from England was that suddenly, "There it was!"

Another impression: Going to the 300 Foot telescope for my first observing run. My memory of walking into that control room feels a little bit like what it must be to walk into someone else's subconscious. There were all these dark pieces of equipment and cable troughs and things dangling and noise of fans humming; all very subterranean. In the corner was this center of consciousness, Gart Westerhout, sitting at a table. I was filled with trepidation about how to use the telescope and Gart immediately said, "Ah, so you're going to do the pointing corrections."

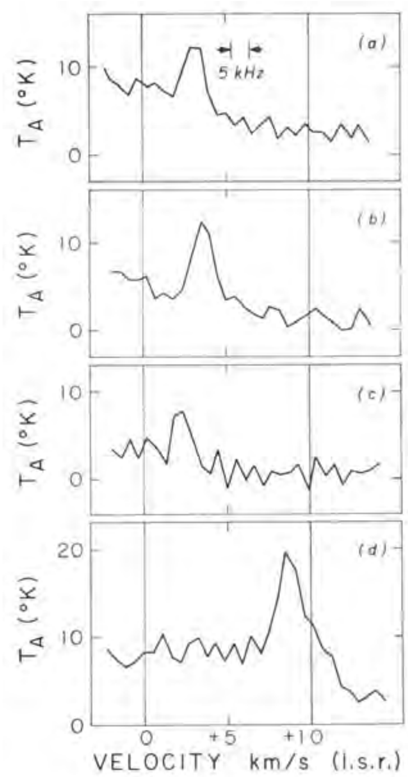
Pointing corrections! What pointing corrections? I soon learned that whenever you observed you had to make sure the telescope was still pointing, and now, after hearing yesterday's discussion, I know why. That was 1967, about the time the thing was expected to fall down. I guess the pointing corrections were a quick way to see whether it was actually in the process of doing so. I felt a bit overwhelmed by Gart because he assumed that anyone who was a radio astronomer understood the correlator from beginning to end. To me it was worse than a black box. It was total mystery, but at least I learned to use it.

Well, I had a lot of fun observing and won't give technical details today about the more exciting results. I did develop a technique for getting more observing time which worked very well. I discovered that when Westerhout was on the telescope together with someone else there would always be these little gaps of time left over. Then I'd go to Bill Howard to request this time by using one of two methods: either make a reasonable scientific case, or get on my knees and beg.

So I ended up with all these bits of pieces of sky that I surveyed in HI. My feeling was that I was involved in this giant treasure hunt. Westerhout was doing the Galactic plane and Carl Heiles had done another region for his thesis, but the rest of the sky was there for anybody to look at. As result I found many new phenomena and ended up publishing 25 papers on 300 Foot data.

I'd like to show you one of the more interesting discoveries I made and how it came to be. In 1967-68 I did a lot of observing on the 300 Foot but also got involved in doing the Zeeman experiment on the 140 Foot. One day I asked whether there were any very narrow emission lines in the sky where I might search for the Zeeman

* From a talk at the 300 Foot 25th Anniversary Symposium, September 1987.



Spectra of four cold HI clouds (Ap.Let. vol.4, p85, 1969)

effect. The quickest way to find out was to call up Carl Heiles, who had done a giant survey on the Berkeley dish. He said that there was a region of the sky where he had noticed a line about 3 km/s wide, which in retrospect really wasn't very narrow. So I ended up with this little list of clouds and noticed one of these was in an area I had partially mapped. I found the cloud he referred to in my data, but it produced a relatively wide line. In addition, however, there were extremely narrow lines nearby (Verschuur 1969; Verschuur and Schmelz 1989). This cloud has a temperature of about 15 K, very, very cold, and it is a very strange object because no one has found anything else, such as dust or molecules, in these clouds. This is very surprising really. Why are they so cold?

In the process of observing on the 300 Foot I did rediscover IC 342 twice, a regular occurrence for people observing galactic HI. It is a galaxy whose emission overlaps that from our Galaxy. It was very exciting the first time, but the second time . . . .

So how did the 300 Foot really change my life? The story begins with that famous night of the fire in 1971 [in the office in Charlottesville that I shared with Barry Turner]. I looked up this article yesterday, in an "Observer" of that era. It has a picture of Barry Turner's desk after the fire. For those of you who know his office, you may wonder whether it has changed since then.

This fire destroyed everything I had except a little bit of the manuscript for the original Invisible Universe book plus a couple of papers where I had some hydrogen line data at home. The fire destroyed 9 man-months work, including some great stuff on the mean optical polarization in clusters and how that depends on distance (something that should be redone someday by somebody). As a result of that fire several things happened. First, the office was burned out, and then I burned out myself, in a rather unusual manner. NRAO was very kind and I was given six weeks of 300 Foot time, 24 hour a day, just for me. I believe Riccardo Giovanelli was involved in some of the analysis. It was an extremely stressful time in my life.

As a result of the fire in 1971 I published 12 papers in 1973, 11 of which came out of that observing run. But there is more to the story. I must have worked very hard and that is why I wanted to take a break (an interpretation aided by hindsight) and accepted a job offer from Colorado which I couldn't refuse, despite admonishments from NRAO colleagues that I should have. At any rate, I took a lot of unpublished data with me to Boulder where I figured I could analyze it in peace while the Fiske planetarium was being built. This peace of mind I expected would come from being cured of my addiction of going to Bill Howard every time I saw an open time slot on the 300 Foot.

Bruce Balick, who happened to be heading to Boulder en route to a job in Seattle, transported my furniture, personal belongings, and data in a truck. Later I drove across in my TR6, moved into my house and then went to empty out Bruce Balick's mother's garage, where my stuff was stored. I had six boxes of 300 Foot data and drove to my office on a Sunday night, put them in the elevator, placed them by my office door, and found I didn't have my office key with me. Well, it didn't matter, I thought, I'll just come back tomorrow morning and finish the job.

I cycled in to the University and arrived at 7.30 AM to find the boxes gone! To say that I "freaked out" is an understatement. It was a terrible shock. I discovered that there was a local tradition in the department. If you wanted boxes of stuff to be tossed out you just placed them outside the office door. Nobody had told me because I had just arrived. I doubt anyone would have thought to tell a newcomer in any case.

The boxes had apparently been shipped to the city dump. A secretary in the department lent me her Mustang. I drove frantically to the dump, the first landmark in Boulder with which I was to become intimate. Some kind person working out there loaned me a shovel with which I spent three hours shoveling through the University trash, which was easily identifiable. So there, in this terrible stink, I got terribly dirty but I could not find my data. Some control cards for running data had been in my luggage and I managed to run those scans again, but essentially a vast amount of work disappeared, log books and all.

Maybe there was a message there—in '71 a fire, in '73 a janitor. But perhaps it saved my life, because if I had tried to write papers at the rate that the stuff had been coming out I probably wouldn't have survived.

It is all very well getting a lot of observing time but if you are on your own it is a strain. Here is a map of one of those unpublished regions, around $l=140\,^{\circ}$showing antenna temperature as a function of galactic latitude and longitude at a number of velocities (Figure 1).

There is another story associated with the 300 Foot, and that is how I came to do the first sensitive SETI experiment. While I was planning to do the six-week run I thought—at the time it was taboo to do SETI— well, here are these areas I want to map but who's to say where the boundaries between them will be. So why not place the boundaries at LSTs equivalent to the right ascensions of some of the more interesting local stars such as Tau Ceti, 61 Cygni and so on. Then, because I just happened to have a break in my observations at these times, I would have four minutes to track each star every day, a simple self-justification in case anybody asked me what I was doing.

I really did think it was worth just taking a look. About this time (1971) Ken Kellermann came back from Armenia where he had attended a SETI meeting where they had discussed "international cooperation between East and West" and I took that with more than a pinch of salt and figured, well, someone should do some observing and publish something. Thus I felt even more justified in slipping the stars into my observing program. I didn't see ETs of course, but that wasn't the point. I wanted to get some data because Frank Drake's Ozma data were never published.

My six-week run went fine and every day I would look at these stars at the boundaries of the areas I was mapping and then came the interesting question: what was I going to do with the data? Could I tell the rest of the world that I actually did this? It seemed unlikely, since this wasn't part of my proposal.

I came up with what I thought was a glorious British-type compromise. I wrote an observing proposal to study these stars and began with a statement to the effect that "I'd like to continue my observations." I don't know if Bill [Howard] remembers this, but I phrased it so that if the referee wasn't really alert he'd miss the word "continue" and if he was alert he'd call me on it. That is what happened. The kind referee recommended I be given spare time slots on the 140 Foot, if they became available, but not make this a big priority. [After this talk the referee from that era, who was present, identified himself.]

Well, in the end I did get official time to make the first SETI observations in the western hemisphere that were actually published (Verschuur 1973). The data on Barnard's star (observed on the 140 Foot) were such that if there had been an alien civilization near it with a 300 Foot telescope pointed at earth at the right time I would easily have picked up the signal if they had used only a 660 kw transmitter. That was a reasonable expectation. With Carl Sagan's help, these data were published in *Icarus*. So that was how that SETI program was done in 1971-72, and published in 1973.

Let me finish with an anecdote. After I left the NRAO and entered the planetarium community I went to a planetarium meeting and sat at a dinner table with perhaps 10 or 12 others who were either planetarians or amateur astronomers. They had all heard about me because I had published The Invisible Universe. One of them asked me an extraordinary question. 'Do you still ride your motorcycle?"

I stared at him, surprised to say the least, since I have never ridden a motorcycle in my life. "Why do you ask?" I responded.

"We heard you used to ride your motorcycle around inside the 300 Foot telescope." They weren't kidding, since quite a few claimed they had heard that story.

The moral of the story is when you hear rumors about people they probably have as much truth as this story of my riding a motor cycle.

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Gerrit Verschuur in 1969 when he was an NRAO staff member.

Bill Howard at his desk in the Jansky Lab, 1964. He was the Site Director at Green Bank 1974-1976.

Staff and visitors use diesel vehicles to drive on site near the telescopes. Here is one of the diesel Checker cabs in 1977.

Scientific Highlights of Twenty-Five Years of Observations with the 300 Foot*

The past quarter century, although but an instant on a cosmic calendar, has been a Golden Age of Astronomy. Human senses were extended by the great power of new telescopes, but the most significant extension was to the radiation the eye cannot detect. The first of these "invisible" radiations to be exploited was the radio, and the 300 Foot telescope at the National Radio Astronomy Observatory in Green Bank, West Virginia has been a giant at investigating it. Its contributions have been many, a small sample of which are summarized below.

The Universe Beyond the Milky Way

• Census Taker of the Universe

Nearly every northern radio object studied by any other telescope is in a catalog compiled using the 300 Foot telescope.

Condon, J. J. and Broderick, J. J. 1985, AJ, **90,** 2450. "A 1400 MHz Sky Survey. I. Confusion-Limited Maps Covering 7h30m $< \alpha <$ 19h30m, $-5^\circ < \delta < +82^\circ$."

Bennett, C. L., Lawrence, C. R., Burke, B. F., Hewitt, J. N., and Mahoney, J. 1986, ApJ.Suppl., **61,** 1. "The MIT-Green Bank (MG) 5 GHz Survey."

• Source Sizes from Variability

The signals from distant quasars and radio galaxies have been found to vary rapidly. The radiating objects are Solar-System size, but contain the energy of a thousand Milky Ways.

Spangler, S. R., Cordes, J. M., and Meyers, K. A. 1979, AJ, **84,** 1129. "Scintillating Confusion: Evaluation of a Technique for Measuring Compact Structure in Weak Radio Sources."

Heeschen, D. S. 1984, AJ, **89,** 1111. "Flickering of Extragalactic Radio Sources."

• Distances to Spiral Galaxies

The most abundant element in galaxies like our own is the simplest, hydrogen. By measuring how fast the hydrogen in galaxies is moving, astronomers using the 300 Foot telescope learned how to estimate the distances to those galaxies.

Tully, R. B. and Fisher, J. R. 1977, Astron.&Astrophys., **54,** 661. "A New Method of Determining Distances to Galaxies."

Fisher, J. R. and Tully, R. B. 1981, ApJ.Suppl., **47,** 139. "Neutral Hydrogen Observations of a Large Sample of Galaxies."

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* *Editors' note: Based on a handout prepared for the 25th Birthday celebration in 1987, probably written by then site director George Seielstad.*

• Hydrogen in Galaxies and Dark Matter

Most of the matter in the Universe escapes direct detection by any telescope. The existence of dark (or missing) matter became apparent when the motion of hydrogen was traced well beyond the visible extents of many galaxies. The amount and distribution of hydrogen in a galaxy is also intimately related to its overall properties.

Roberts, M. S. 1969, AJ, **74,** 859. "Integral Properties of Spiral and Irregular Galaxies."

Roberts, M. S. and Rots, A. H. 1973, Astron.&Astrophys. **26,** 483. "Comparison of Rotation Curves of Different Galaxy Types."

Roberts, M. S. and Whitehurst, R. N. 1975, ApJ, **201,** 327. "The Rotation Curve and Geometry of M31 at Large Galactocentric Distances."

Rubin, V. C., Ford, W. K. Jr., and Roberts, M. S. 1979, ApJ, **230,** 35. "Extended Rotation Curves of High-Luminosity Spiral Galaxies. V. NGC 1961, the Most Massive Spiral Known."

• Megamaser Galaxies

Not all the matter in a galaxy is hydrogen. Some exists in the form of molecules. Occasionally collections of molecules lockstep their energy to produce intense radiations—sometimes so intense they can be detected in galaxies hundreds of light-years from our own. The 300 Foot telescope has discovered several of these galaxies via their emissions of hydroxyl.

Baan, W. A. 1985, Nature, **315,** 26. "Powerful Extragalactic Masers."

Baan, W. A., Haschick, A. D., and Schmelz, J. T. 1985, ApJ, **298,** L51. "The Fourth OH Megamaser: Markarian 273."

Cosmology

• Filaments and Voids

The galaxies in the Universe are not distributed evenly. They bunch into clusters, which string together into filamentary superclusters separated by relatively empty regions called voids. Filaments stretch all the way across the sky. Their lengths are measured in hundreds of millions of light-years.

Richter, O. G. and Huchtmeier, W. K. 1987, Astron.&Astrophys., **68,** 427. "HI Observations of Galaxies in Between the Local and the Hydra/Centaurus Superclusters."

Haynes, M. P., Giovanelli, R., Starosta, B. M., and Magri, C. 1988, AJ, **95,** 607. "A 21 cm Survey of the Pisces-Perseus Supercluster. III. The Region North of +38 Degrees."

• Counts of Radio Sources

Weak sources are typically very distant. Because radio signals from them have been traveling a long time, the sources reveal the Universe as it was billions of years ago. Astronomers count sources at various distances to reveal a history of violence.

Machalski, J. 1978, Astron.&Astrophys., **65,** 157. "The Differential Radio Source Count at 1400 MHz from the GB2 Sky Survey."

Pauliny-Toth, I. I. K., Witzel, A., Preuss, E., Kuhr, H., Kellerman, K. I., Fomalont, E. B., and Davis, M. M. 1978, AJ, **83,** 451. "The 5 GHz Strong Source Surveys. IV. Survey of the

Area Between Declination 35 and 70 Degrees and Summary of Source Counts, Spectra and Optical Identification."

Bennett, C. L., Lawrence, C. R., and Burke, B. F. 1985, ApJ, **299,** 373. "Source Counts at 5 Gighertz from the MG Survey."

• Absorption of Radiation from Distant Quasars

Some of the energy from the most distant objects in the Universe is missing by the time it reaches the 300 Foot telescope. It has been absorbed by the matter in intervening, though still remote, objects. This provides information about the absorbing material and has also been used to probe the constancy of fundamental physical constants.

Brown, R. L. and Roberts, M. S 1973, ApJ, **184,** L7. "21-Centimeter Absorption at z = 0.692 in the Quasar 3C286."

Roberts, M. S., Brown, R. L., Brundage, W. D., Rots, A. H., Haynes, M. P., and Wolfe, A. M. 1976, AJ, **81,** 293. "Detection at z = 0.5 of a 21-Cm Absorption Line in AO 0235+164: The First Coincidence of Large Radio and Optical Redshifts."

Briggs, F. H. and Wolfe, A. M. 1983, ApJ, **268,** 76. "The Incidence of 21 Centimeter Absorption in QSO Redshift Systems Selected for Mg II Absorption: Evidence for a Two-Phase Nature of the Absorbing Gas."

• Gravitational Lenses

Matter between the Milky Way and quasars reveals itself another way, a consequence of Einstein's General Theory of Relativity. Radio signals from afar are bent by the gravity exerted by any massive concentration the signals pass by. The signals are "lensed."

Langston, G. I., Conner, S. R., Heflin, M. B., Lehar, J., and Burke, B. F. 1990, ApJ, **353,** 34. "Faint Radio Sources and Gravitational Lensing."

• Environmental Astronomy

How galaxies form remains a mystery, but what type a galaxy will be is strongly influenced by the medium it forms from. Environments around galaxies have been studied with the 300 Foot telescope.

Haynes, M. P., Brown, R. L., and Roberts, M. S. 1978, ApJ, **221,** 414. "A Search for Atomic Hydrogen in Clusters of Galaxies."

Fisher, J. R. and Tully, R. B. 1981, ApJ, **243,** L23. "Upper Limits on the Space Density of Intergalactic Neutral Hydrogen Clouds."

Roberts, M. S. and Steigerwald, D. G. 1977, ApJ, **217,** 883. "A Search for Neutral Hydrogen Clouds in Radio Galaxies and in Intergalactic Space."

The Milky Way Galaxy

• Structure of the Milky Way

Observations with the 300 Foot telescope have located the spiral arms of the Galaxy, as well as the distributions of cosmic rays and magnetic fields. A warp or bending of the disk's extremities has been discovered.

Burton, W. B. and Verschuur, G. L. 1973, Astron.&Astrophys.Suppl., **12,** 145. “Observations of neutral hydrogen near the galactic plane.”

Westerhout, G. and Wendlandt, H. U. 1982, Astron.&Astrophys.Suppl., **49,** 143. “The Maryland-Green Bank Galactic 21-cm Line Survey.”

Bania, T. M. and Lockman, F. J. 1984, ApJ Suppl., **54,** 513. “A Survey of the Latitude Structure of Galactic H I on Small Angular Scales.”

Lockman, F. J., Hobbs, L. M., and Shull, J. M. 1986, ApJ, **301,** 380. “The Extent of the Local H I Halo.”

• High-Velocity Clouds

Clouds full of hydrogen rain onto the disk of the Milky Way from above and below. Their speeds are fast compared with those of the hydrogen in the spiral arms.

Cram, T. R. and Giovanelli, R. 1976, Astron.&Astrophys., **48,** 39. “Two-Component Structure in the Profiles of High Velocity Clouds.”

Giovanelli, R. 1980, AJ, **85,** 1155. “Studies of High-Velocity Clouds. I. A High-Sensitivity Survey.”

• Exotic Galactic Variables

Some of the most peculiar radio objects known have been discovered by scanning the Galaxy’s plane repeatedly and noting the objects that change strength rapidly.

Gregory, P. C. and Taylor, A. R. 1978, Nature, **272,** 704. “New Highly Variable Radio Source, Possible Counterpart of Gamma-Ray Source CGL35+1.”

Heeschen, D. S. and Hammond, S. E. 1980, ApJ, **235,** L129. “A Radio Flare in SS 433.”

Gregory, P. C. and Taylor, A. R. 1981, ApJ, **248,** 596. “Radio Patrol of the Northern Milky Way: a Survey for Variable Sources.”

• The Electrically Charged Interstellar Medium

Flooding the space between the stars of the Milky Way is a medium of electrons and protons threading their way along tangled lines of magnetic fields. This medium bends and distorts in other ways the radio signals moving through it. Distant objects appear to scintillate, twinkle, or flicker. The amount of distortion reveals the density and the charged particles.

Backer, D. C. 1975, Astron.&Astrophys., **43,** 395. “Interstellar Scattering of Pulsar Radiation. I. Scintillation.”

Dennison, B. 1979, AJ, **84,** 725. “On the Intracluster Faraday Rotation. I. Observations.”

Armstrong, J. W. and Rickett, B. J. 1981, MNRAS, **194,** 623. “Power Spectrum of Small-Scale Density Irregularities in the Interstellar Medium.”

• Chemistry in an Extreme Environment

The largest atom ever known was not found in a chemistry laboratory. It was discovered with the 300 Foot telescope. It is an atom of carbon whose outermost electron is 0.003 cm (a thousandth of an inch) from its nucleus.

Anantharamaiah, K. R., Erickson, W. C., and Radhakrishnan, V. 1985, Nature, **315,** 647. "Observations of Highly Excited Radio Recombination Lines Towards Cassiopeia A."

Anantharamaiah, K. R, Payne, H. E., and Erickson, W. C., 1988, MNRAS, **235,** 151. "Detection of Carbon Recombination Lines below 100 MHz towards the Galactic Centre and M16."

Pulsars

• Surveys for Pulsars

The number of pulsars, their distribution, and major properties have been studied with the 300 Foot telescope.

Huguenin, G. R., Taylor, J. H., Goad, L. E., Hartai, A., Orsten, G. S. F., and Rodman, A. K. 1968, Nature, **219,** 576. "New Pulsating Radio Source."

Taylor, J.H., and Huguenin, G.R. 1969, Nature, **221,** 816. "Two New Pulsating Radio Sources."

Reifenstein, E. C. III, Brundage, W. D., and Staelin, D. H. 1969, ApJ, **156,** L125. "Searches for Pulsars."

Damashek, M., Taylor, J. H., and Hulse, R. A. 1978, ApJ, **225,** L31. "Parameters of 17 Newly Discovered Pulsars in the Northern Sky."

Dewey, R. J., Taylor, J. H., Weisberg, J. M., and Stokes, G. H. 1985, ApJ, **294,** L25. "A Search for Low-Luminosity Pulsars."

• Pulsar in the Crab Nebula Supernova

The association between pulsars and supernovae was clinched when the 300 Foot telescope discovered a pulsar in the Crab Nebula, a star which exploded in 1054 A.D. At the time of discovery, the Crab pulsar was the fastest known.

Staelin, D. H. and Reifenstein, E. C. 1968, Science, **162,** 1481. "Pulsating radio sources near the Crab Nebula."

• Timing of Pulses

The regular pulsing of the radio signals may constitute the best clocks in the Universe.

Helfand, D. J., Taylor, J. H., Backus, P. R., and Cordes, J. M. 1980, ApJ, **237,** 206. "Pulsar Timing. I. Observations from 1970-1978."

Early Users of the 300 Foot Telescope

The users and projects of the first 10 years of the 300 foot telescope are listed here. Only the first project for each observer is listed.

NAME	PROJECT	INSTITUTION
1962		
Drake, Frank	Positioning and Focus	NRAO
Heeschen, David	Selected Point Sources	NRAO
Findlay, John		NRAO
Wade, Campbell	Normal Galaxies	NRAO
Hogg, David	Scorpius X-Ray Source	NRAO
Pauliny-Toth, Ivan	Sky Survey	NRAO
Hoglund, Bertil	Extragalactic HI	NRAO
Burke, Bernard F.	H-Line Studies	CIW, DTM
Johnson, Hugh	HI in Vicinity of ScoX-1	Lockheed Research Lab.
Venugopal, V. R.	M33	NRAO
1963		
DeJong, Marvin	3C Sources	Rensselaer Polytechnic Inst.
Menon, T. K.	Supernova Remnants, M31,M33	NRAO
McClain, Ed	Polarization	Naval research Labs.
Bologna, Joe	Polarization	Naval research Labs.
Sloanaker, Russel	Polarization	Naval research Labs.
Roberts, Morton	Extragalactic Hydrogen	Harvard University
1964		
Terzian, Yervant	3C Sources	Indiana State University
Williams, David R.	Hydrogen Absorption	University of California
Westerhout, Gart	Galactic Plane	University of Maryland
Turner, Ken	H-Line Studies	CIW, DTM
Wendker, H. J.	Flux Measurements of 3C123	Ast. Inst. U. Münster, Germany
Mezger, Peter	Gal. Continuum, Extended Sources	NRAO
Baars, Jaap	Telescope Efficiency	NRAO
Heiles, Carl	Interstellar Clouds	Princeton University
Rougoor, G. W.	High Velocity Clouds	Leiden Observatory
1965		
Riegel, Kurt	HII Regions	University of Maryland
Kaftan-Kassim, May	Planetary Nebulae	NRAO
Palmer, Patrick	Polarized Radiation From Mars	Harvard University
Swenson, George	M31	University of Illinois
Ellis, Fred	Mapped Galactic Spur	Louisiana State University
Howard, William	Galactic Clusters	NRAO
1966		
Herbig, George	HI Centered On Zeta Oph	Lick Observatory
Erickson, William	Solar Occultation of Taurus A	University of Maryland
Henderson, A. Peter	HI, Evaluate Surface	University of Maryland
Davies, Robert	HI	University of Wisconsin
Miller, Joseph	HI Absorption Near HII regions	University of Wisconsin
Varsavsky, C.	HI in Taurus	Argentine N.R.A.I.
Davis, Mike	Dwingeloo List of Radio Sources	Leiden Observatory

1967

Kerr, Frank	Hydrogen in The Galactic Center	University of Maryland
Maxwell, Alan	W40 & W51 Regions	Harvard University
Downes, Dennis	W40 & W51 Regions	Harvard University
Altenhoff, Wilhelm	3C Fluxes	NRAO
Kraus, John	Galactic Survey Verification	Ohio State University
Rickard, Lee J	High Velocity Regions	University of Maryland
Dickel, John	M31, Sharpless Map	University of Illinois
Jauncey, Dave J.	4C Fluxes	Cornell University
Carr, Thomas	Galactic Clusters	University of Miami
Thaddeus, Patrick	HI Stars	NASA/Goddard Institute Space Studies, NY
Verschuur, Gerrit	Hydrogen Clouds 0800-1700 RA	NRAO
Struch, C.	HI in RR Lyrae Globular Clusters	University of Rochester
Mast, Joseph	High Latitude HI	University of Virginia
Ko, H.	Mapped Continuum intensity	Ohio State University

1968

Neill, Arthur	4C Fluxes	Cornell University
Gordon, Kurt	Extragalactic Hydrogen	University of Michigan
Huguenin, Dick	Pulsars	Harvard University
Taylor, Joe	Pulsars	Harvard University
Goldstein, Sam	Pulsars	University of Virginia
Emerson, L.	HI Toward S147,IC443, Cyg. Loop	Cornell University
Staelin, David	Pulsars	MIT
Reifenstein, Ted	Pulsars	MIT
Felli, Marcello	Mapped Sharpless HII Region	Observatorio di Arcetri, Italy
Churchwell, Ed	Mapped Sharpless HII Region	Indiana University

1969

Ahern, Frank	Pulsar VLB	University of Maryland
Miley, George	VLB Quasars	Cal. Tech.
Cohen, Marshall	VLB Quasars	Cal. Tech.
Kesteven, Michael	Absorption Low Latitude Sources	Queens University, Canada
Bridle, Alan	Absorption Low Latitude Sources	Queens University, Canada
Sutton, John	Pulsars	NRAO
Sullivan, Woodruff	High Velocity Clouds	University of Maryland
Turner, Barry	OH Strip of Sky	NRAO
Dixon, Robert	OSU Confirmation	Ohio State University
Ehman, Jerry R.	OSU Confirmation	Ohio State University
Fitch, L.	OSU Confirmation	Ohio State University
Ewing, Marty	Pulsars	MIT
Price, Mark	Pulsars	MIT
James, J.	Pulsars	University of Virginia
Miesel, David	Pulsars	University of Virginia
Burns, Robert	Pulsars	NRAO
Snyder, Lew	New Molecule Search	University of Virginia
Buhl, David	New Molecule Search	NRAO
Zuckerman, Ben	New Molecule Search	University of Maryland
Gottlieb, Carl	New Molecule Search	Smithsonian Astrophys. Obs.
Dickerson, Dale	New Molecule Search	Smithsonian Astrophys. Obs.
Pasachoff, Jay	New Molecule Search	Harvard University
Ball, John	New Molecule Search	Harvard University
Seaquist, Ernest	Algonquin Source List	Univ. of Toronto, Canada
Ross, Hugh	Algonquin Source List	Univ. of Toronto, Canada
Harten, Ron	Mapping HI Galactic Plane	University of Maryland
Lindblad, P.	HI in Outer Spiral Arm	Stockholm Observatory, Sweden
Scharleman, E.	Molecule Search	Ohio State University
Brundage, William	Molecule Search	Ohio State University
Tolbert, Charles	High Lat. High Velocity Clouds	University of Virginia

Manchester, Richard	Pulsars	NRAO
Kardashev, Nikolai	Pulsars	Cosmic Space Institute, USSR
Broderick, John	Pulsars	NRAO
Pfleiderer, Jorg	Elliptical Galaxies	NRAO/Astron.Inst.Bonn
Gottesman, Stephen	Extragalactic H Line	NRAO

1970

Maslowski, Jozef	Cosmological Evolutionary Invest.	Jagellonian Univ., Poland
Batchelor, Robert	Pulsars	MIT
Knowles, Steven	VLB Pulsars	Naval Research Labs.
Hvatum, Hein	Evaluation of new Surface	NRAO

1971

Webber, John	Mapping Pol. of SN Remnants	University of Illinois
Kundu, Mukul	Intensity + Pol. of SN Remnants	University of Maryland
Velusamy, T.	Intensity + Pol. of SN Remnants	University of Maryland
Saslaw, William	Radio Emissions From Haro Sources	University of Maryland
Purton, Christopher	Extended Galactic Sources	York University, Toronto
Kennedy, John	Extended Galactic Sources	York University, Toronto
Feith, D.	Pulsar Search	MIT
Krolic, J.	Pulsar Search	MIT
Murdoch, Hugh	20 cm Spectra of Weak Sources	Univ. of Sydney, Australia
Grasdalen, Gary	Survey At 28 cm of IR Stars for OH	Univ. of Cal.,Berkely
Wright, Melvin	HI of Poss. Extragalactic Obj.	NRAO
Mader, Gerald	Galactic HI	University of Maryland
Simonson, Christian	Galactic HI	University of Maryland
Arp, Halton	Spiral Galaxies	Hale Observatory
Wardle, John	2695 MHz Emissions	NRAO
Burton, Butler	21 cm HI	NRAO
Knapp, Jill	Dust Clouds, 21 cm Beam Efficiency	University of Maryland
Goss, Miller	21 cm HI Profiles Extra. Gal.	MPIR, Germany
Vandenberg, Nancy	VLB	University of Maryland
Clark, Tom	VLB	NASA (Goddard)
Resch, George	VLB	University of Maryland

1972

Lockman, Jay	Recomb. Lines Toward Pulsars	University of Mass.
Giovanelli, Riccardo	Mapping High Velocity Clouds	University of Indiana
Baker, Paul	HI Distribution Non-Thermal	NRAO
Gordon, Courtney	Strong HII Regions	Hampshire College
Sramek, Richard	Normal Galaxies At 6 cm	NRAO
Owen, Frazer	Abell Clusters At 20 cm	University of Texas
Tovmassian, H.	20 cm Observations of Capricorni	Byurakan Obs., Armenia
Backer, Donald	Pulsar Sub-Pulse	NRAO
Kapitzky, John	Flux, Pol. of Variable Sources	University of Mass.
Dent, William	Flux, Pol. of Variable Sources	University of Mass.
Giguere, Paul	6 cm Cont Em From Dark Clouds	University of Virginia
Fisher, Rick	Region Along Path of Moon At 20 cm	NRAO
Sarma, N.	11 cm Flux Densities	Tata Institute, India
Bechis, Dennis	Maryland-GreenBank Survey	Harvard University
Hulse, Russell	Galactic OH Regions	University of Mass.
Shostak, Seth	HI in Sel. Galaxies	NRAO
DeNoyer, Linda	Supernova Remnants	University of Illinois
Cram, Thomas	High Velocity HI	NRAO
Balick, Bruce	M33, M31 At 20 cm	NRAO
Becker, Robert	Pol. in HB21 At 20 cm	University of Maryland
Seacord, Andrew	Cyg. X Complex HI	University of Florida
Arny, Thomas	Search For HI in Young Clusters	University of Mass.
Greenberg, J. Mayo	21 cm HI in Dark Clouds	SUNY, Albany
Minn, Young Key	21 cm HI in Dark Clouds	MPIR, Germany
Hutton, Kate	Pulsar VLB	University of Maryland
Perley, Rick	Pulsar VLB	University of Maryland

1973

Brandie, George	Variable Sources	Queens University, Canada
Condon, Jim	11 cm Extragalactic	NRAO
Crane, Pat	Red Shifts	MIT
Spencer, John	Red Shifts	MIT
Giuffrida, T.	Red Shifts	MIT
Peterson, Steven	HI Peculiar Galaxies	Cornell University
Tully, R. Brent	Dwarf Galaxies	Observatoire de Marseille
Parrish, Alan	H166 Recombination	NRAO
DeYoung, David	HI in Peg, Per, and Cancer Clusters	NRAO
Fomalont, Ed	Ohio State University Source List	NRAO
Brown, Robert	Redshifted HI in Quasars	NRAO
Rubin, Vera	HI in ScI Galaxies	CIW, DTM
Anand, S.	2695 MHz Surv. Zwicky Galaxies	NRAO
Sulentic, Jack	Markarian Galaxies	SUNY, Albany
Yerbury, M.	1400 MHz Brightness Temp. Saturn	Cornell University

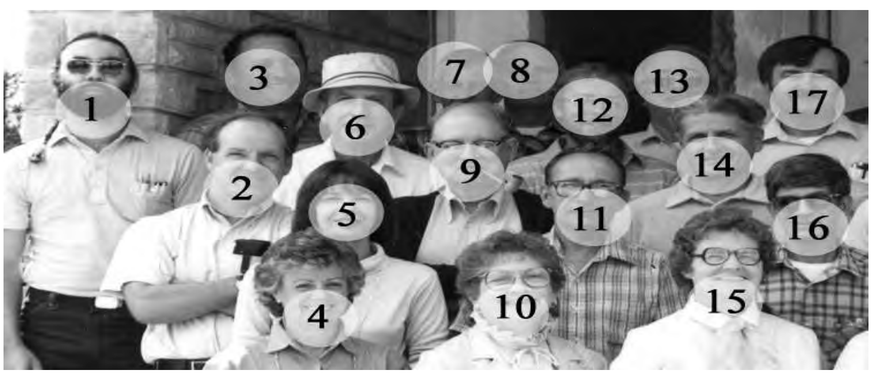
1974

Thonnard, Norbert	ScI Galaxies At 1421 Line of HI	CIW, DTM
Haschick, Aubry	Recombination Lines of HII Regions	MIT
Gibson, David	Search At 5006 MHz For New Sources	University of Virginia
Hjellming, Robert	Search At 5006 MHz For New Sources	NRAO
Bowers, Phil	1612 MHz Search For OH Sources	University of Maryland
Rood, Herbert	Red Shifts of Galaxies	University of Michigan
Tifft, William	Double Galaxies And Planetary Neb.	University of Arizona
Stocke, John	Double Galaxies And Planetary Neb.	University of Arizona
Dressel, Linda	6 cm Survey of Brighter Galaxies	University of Virginia
Blankenship, Linda	Cyg X-3 At 610,775, & 965 For Vari.	NRAO

L. to R. : Gart Westerhout (USNO), Bob Hughes (president of AUI), Hugh Aller (U. Michigan, in background), Bill Howard, Kurt Riegel (NSF), at the 300 Foot 25th Birthday celebration, September 1987.

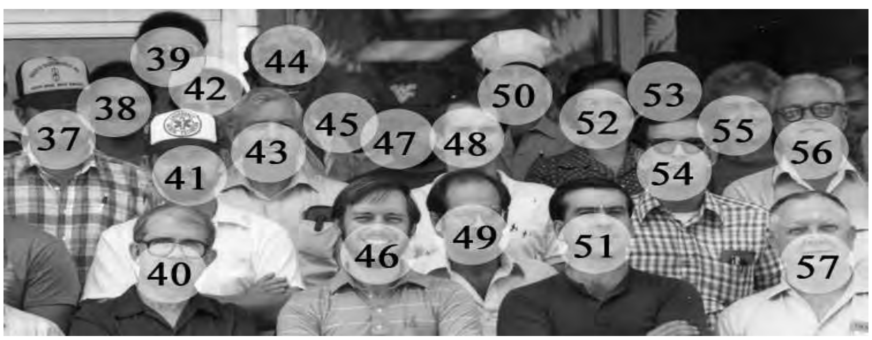
[courtesy G. Verschuur]

4 Green Bank Staff — September 14, 1987

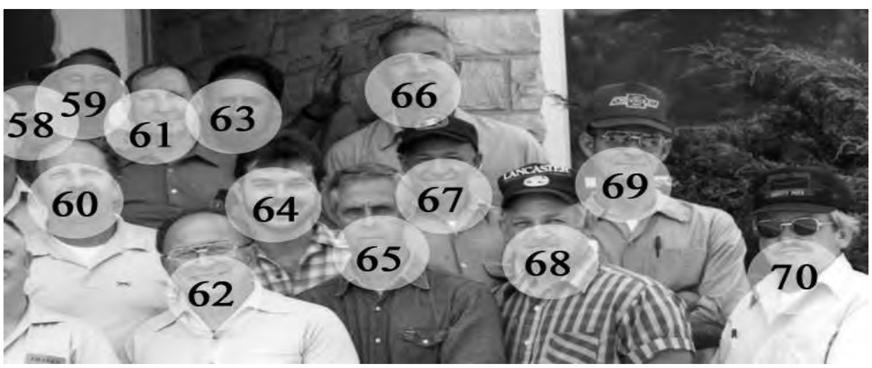


1	Wesley Sizemore	7	Jim Oliver	13	Ron Weimer
2	Rick Fisher	8	Lewis Beale	14	Chuck Brockway
3	Ron Gordon	9	George Grove	15	Beaty Sheets
4	Becky Warner	10	Carolyn Dunkle	16	George Liptak
5	Carol Ziegler	11	Don Stone	17	Ed Childers
6	Don Hovatter	12	Rich Lacasse		

18 Jim Gibb
19 Fred Bierer, Jr.
20 George Seielstad
21 Berdeen O'Brien
22 Troy Henderson
23 George Behrens
24 Jerry Turner
25 S. Srikanth
26 Wally Oref
27 Mark Clark
28 Harry Payne
29 Mark Schenewerk
30 Bruce McKean
31 Allen Ferris
32 Ron Maddalena
33 Fred Crews
34 Bill Vrable
35 Kenny Lehman
36 Dwayne Schiebel



37 Don Gordon
38 Bob Simon
39 Nathan Sharp
40 Boyd Wright
41 Dwayne Barker
42 Tom Dunbrack
43 Bob Vance
44 Harley Carpenter
45 Carl Chestnut
46 Roger Norrod
47 Rusty Taylor
48 Steve White
49 James Porter
50 John Sparks
51 Dewey Hoover
52 Henrietta Reigel
53 Merle Kerr
54 Len Howell
55 Dottie McLaughlin
56 Omar Bowyer
57 Dick Hiner



58 Bill Radcliff
59 Richard Fleming
60 Martin Barkley
61 Harold Crist
62 Bob Viers
63 Dahrl McLaughlin
64 Mike Hedrick
65 Wendell Monk
66 Ray Hanshew
67 Bedford Taylor
68 Russ Poling
69 Charles Cassell
70 Bill Campbell

Gart Westerhout and the 300 Foot telescope in 1966.

Editors' note: Prior to the 300 Foot symposium, George Seielstad, then Green Bank Site Director, asked for humorous anecdotes and reminiscences both from Astronomers and Telescope Operators. These stories were distributed to the meeting participants, and titled "Recollections and Humor from the First Twenty-Five Years with the 300 Foot Telescope." Some of these stories are reprinted here and earlier in the 300 Foot section of this book. One may note that there are many more stories by the operators about the astronomers than vice versa!

5 Recollections and Stories

The 300-Foot: Some Early Memories.

Gart Westerhout
U. S. Naval Observatory

Those were the good days. It is remarkable that the harder one has to work and the more primitive the equipment, the more satisfied one is when the results are not only good looking but actually useful. Nowadays, one punches in some numbers and fools around with pretty colored pictures on display screens without knowing what really went into those pictures. Not in 1963 we didn't.

Bernie Burke, then of DTM in Washington, DC, built his entire 21 cm, 90-channel receiver into a trailer and pulled the affair to Green Bank in the spring of 1963 (or was it the previous winter?). There was still one piece in that trailer belonging to the old Ewen-Purcell 21 cm line discovery receiver (1951) which Merle Tuve inherited (or swiped?).

That summer, we observed. Bernie the Andromeda Nebula, I the Perseus arm. Halfway through, Fred Crews anxiously announced that the telescope encoder might conk out any moment. Not to worry, said the always inventive Burke. Overnight, we attached a considerable number of 8-foot boards to the big circle holding the giant bicycle chain, around the elevation of the Andromeda Nebula. A TV monitor (a novelty and the only one around) was pointed at the boards, and with a pot of paint we painted lines on it every 5 minutes of arc, using the—still working—encoder as the calibrator. Little did I know that I would be steeped in "divided circles" (optical transit circles) later in my career. Everyone had to come and see it. As it turned out, the encoder held out throughout the observing period so we never used the divided circle. But it sure worked!

That winter (or was it the previous one?) was a cold one. All the same, my old station wagon started at -20 F at 7:00 a.m. in front of the 300 Foot, just like that. It didn't a few days later in Cass, where we stayed in an NRAO house next to Kane's Groceries. So Kane gave me a pull. Not realizing I had a stick shift, he figured that to start an automatic by pulling you have to go really fast. So when 100 feet down the road when my engine had started, I blew the horn, he accelerated. Two miles toward Green Bank, after more hooting and hollering, and a hair-raising 60 mph on a snow covered road (with the family in the back), he finally released us.

By-the-way, those 1963 data were: 1) recorded on a pen recorder, 2) recorded on punched tape, 3) transferred to punched cards via a tape-to-card-converter, 4) given their first reduction on the IBM 1620 Green Bank house computer. Jim Rickard, in his Masters thesis, wrote: "Each channel has an independent zero level and gain. This increases the noise in a random fashion (it depended upon how well the technician adjusted the receiver, dirt on the step switch which sampled the 90 capacitors, etc.).” But it worked, and it worked well, and we got data, and it was fun, and it was exciting!

In the summer of 1964, Art Shalloway was finishing his 100- channel autocorrelation receiver. Its data were recorded on a 200 BPI stepping magnetic tape recorder. Initially, that was all we had. Later, it was connected to the IBM 1620 and the data fed into the 1620—which could not do a proper Fourier Transform but could at least print out an AC function. Before that, however, we went through a brief period where Art Shalloway produced a magic box of iron filings, which were sprinkled on the tape. With a magnifying glass we read the bytes and jotted them down. For a while, the tapes were then shipped, via Heinz Wendlandt or one of the students, to Maryland to be Fourier transformed on the IBM 7094/1401 computer system there. Later in 1964, Green Bank got its own IBM 704 in the warehouse so tape shipping was no longer needed. That is, until computer and all moved to Charlottesville and we had to ship all over again.

When I started using the 300 Foot, word on my previous experiments with the 85-foot telescope in 1959 had preceded me. So I was handed string, tape and paper clips by the operators to build a digitizer/marker. It was not needed at the 300 Foot, as DTM had a beautifully automated system—you only needed a pencil to annotate the charts. But in 1959 at the 85-foot it sure helped the operator save the soles of his shoes: instead of walking to the brand-new HP printer to make notes, he pulled a string dangling in front of him which ran up to the ceiling, 20 feet over, and down again to the “manual paper advance” on the HP!

What was needed, in later years at the 300 Foot, was paper tape. As the equipment became more complicated, more people became involved and, especially on maintenance days but often at odd times, experts would expertly change the setting of a switch which I had carefully set at one particular setting. Attenuators were among those. In those days I made it a point to carefully measure detector currents, linearity, gain, and receiver noise before starting a 3-week series of observations. I could then follow gain fluctuations, etc., later. Of course changing attenuator settings would screw up these gain calibrations as they were not recorded. Solution: after calibration, I put a signed and dated tape over all important switches with a “do not touch” sign, guarding against the experts. The operators would have tape ready the first day I came in and, would you believe it, when I showed up at the Bonn 100-meter telescope in Germany in 1973 to observe, the operators handed me a roll of tape for the switches.

Once I followed a student (who, in spite of the incident about to be related, turned out all right). The student, just before he left after his 2-week observing period, told me in confidence: “Something is wrong, the meters don’t seem to read the right numbers, but my line profiles (which were very low intensity) are OK so I didn’t tell anybody because when you tell the electronics people they come in and you lose a day or a week of observing time.” I asked him whether this “malfunction” had been there during the entire 2 weeks; he said, “No, it started about halfway.” After he left, and 5 minutes into my checkout procedure, I found an attenuator with 10 db too much. During maintenance day, electronics had accidentally left 10 db in. The detector was therefore at a very low (a linear) level, but the student was right: it did not influence his low-intensity data.

We had a real-live jet engine on a trailer which was meant to blow the snow off the disk. It roared like the dickens, but that's about all it did. I have been told that the jet-engine fuel in the tank truck grew green plants (algae?) after a number of years. I don't know where they dumped it.

And then there was the spring of the elephants. The telescope structure was strengthened in 1966 and the footsteps of the workmen on the surface mesh were enormous, as though an elephant had walked the surface. Fred Crews took all the panels down and rolled them. Unfortunately, that removed some of the tensile strength and the panels sagged. Yes, we learned a lot about the "error beam" : 40% of the energy went into a 10° diameter error beam, 43% into the gaussian main beam, and 17% into the "far sidelobes."

We had a fire in the corner of the roof. The operator first calmly put out the fire, then called in to report it. We were busily observing again having almost forgotten the incident when we saw the lights. The roar. The automobiles. Three of them, half in pajamas, bringing in one (yes, one) fire extinguisher. Then the fire truck arrived 10 minutes later. We went on observing.

And then there is the North Pole Hole, dug patiently by Fred Crews when it suddenly occurred to me that the North Pole is always in the same place and therefore an ideal calibrator (as it turned out, it wasn't, at least not for the polarization measurements I attempted, as everything moved when slewing). Before that hole, we could only get to declination 85°.

I could go on. This was a marvelous telescope. It is better, much better now. It has such a ridiculously low noise front end that they even think they occasionally smell the Navy doing interferometry next door. It is entirely automated. It does wonderful science. But it's not the same....

> Troy Henderson shared an office at the 300 Foot with Bob Viers. One day Gart Westerhout came in, looked at Bob's desk, and asked, "Whose desk is that?" Troy said, "Bob Viers." Gart said, "What does he do?" Troy replied, "Not much." Gart then said, "What do you do?" Troy replied, "I help Bob!"
>
> *from Jon Spargo*

Mort Roberts and the 300 Foot in 1969.

Observing with Dr. Roberts

(From *The Observer* July 1970)
by Ken Cottrell

In March [1970] we were privileged to host the NRAO's Assistant Director, Dr. Morton Roberts, for a week of intense observations on BL LAC. Dr. Roberts shared telescope time with NRAO associate scientist Stephen Gottesman. He was searching for neutral hydrogen absorption. Dr. Gottesman was researching galactic spiral arms and mapping Cygnus X.

To indulge in hyperbolic conjecture it would seem that Dr. Roberts may have had an ulterior objective in mind for his program—something like determining the telescope's peak operating efficiency breakdown point. In any event, two operators were kept "busier than bobcats in a chicken coop," one manning the console, and the other manning the Houston Omnigraphic Plotter. The program called for a plot-out every ten seconds of time, which is probably not too far from equipment design capabilities.

Dr. Roberts synchronized operations from his position at the Tektronix memory scope. On signal, he would cue the operators with a spirited, "Wait!" or "Now!" indicating when a plot-out should be made.

Despite the hectic pace all critical schedules were met, and all necessary data was obtained.

Jon Spargo, telescope operator, 1970.

The 300 Foot: Anecdotes

Jon Spargo
NRAO - VLA

Late in 1970 when we had finished the new surface of the telescope with 4 or 5 workers climbing around doing all kinds of jobs, we marveled that no one had fallen or had even gotten so much as a hangnail. So everyone was pleased. Then a Field Engineer climbed to the edge of the dish to take a picture and gave everyone anxious moments when he suddenly slipped and slid on his posterior all the way to the center of the dish. The effect to that part of his anatomy was as if he had slid across a cheese grater instead!

The operators, technicians and engineers used to try to set records racing up the stairs and ladders to the receiver house at the apex. The record is held by Magne Hagstrom—2 minutes.

Herb Hanes was very upset one day when someone dropped a hammer from up at the apex and it fell through the newly installed mesh surface. Someone said, "Don't worry Herb, it went right through that hole!"

The story goes that Bob Viers always worried that someone would fall from the apex, crash through the mesh surface and come to an abrupt halt on the pavement below. He was always cautioning people about that possibility. One day the mechanics were up in the apex. They decided to take a break and have a cigarette. The day was warm and Odell Johnston had taken off his coveralls to cool off. After a few minutes he looked down and noticed Bob Viers on the ground below the telescope. Odell yelled, "Aaaaaaaaaah," as he tossed his coveralls out over the dish and laughed merrily as he watched Bob Viers look up, cringe, cover his head with his arms and almost expire from panic.

One of the duties of the 300 Foot operators after a hard rain was to drain the water from the drive control pit, because the oil would float around on top of the water. So they had to open a drain valve and let the water out. One night Roy Sharp was on duty, and after a hard rain, went to the pit to open the valve. While he was doing that, he noticed that a kitten had fallen into the pit and was covered with oil. Roy thought, "Poor Kitty," and reached down and picked it up. However, it turned out that the kitten resided in a nearby barn, had never been touched and was about as wild as a "wildcat." Needless to say, Roy dropped that kitten with haste! Evidence of the encounter adorned Roy's face, hands and arms for several weeks!

One of the operators, Ralph Graham, drove a beat-up old pick-up truck that generated lots of radio interference to the 300-ft and produced a distinctive pattern on the spectrum analyzer located in the control building, so that everyone knew when Ralph was coming in or leaving. One afternoon after Ralph had been working day shift, Bob Viers came in to relieve him. Ralph left, and Bob noted the pattern on the spectrum analyzer, and pointed it out to an observer who was there. Later that evening Bob saw the pattern again and remarked to the observer, "Here comes Ralph Graham." The observer looked out and said, "Bob, you'd better come see this." Indeed, Ralph Graham was headed down the road toward the 300-ft, but riding a bicycle!

After steelworkers had installed additional backup structure in preparation for the new panel surface, the old mesh surface was a mess! It was horribly deformed, the result of heavy foot travel by the steelworkers. Since it was some time before the new surface arrived, something had to be done in the interim. The brilliant solution was to peel the old surface off in strips, lay it on the road, and iron out the wrinkles with a pavement roller! It worked! The 300 Foot was able to resume observing until the new panels arrived.

Mike Davis speaking at the 300 Foot 25th Birthday Symposium, September 1987.
[Courtesy G. Verschuur]

The 300 Foot: The Case of the Perfect Clutch

Mike Davis
Arecibo Observatory

Shortly after the 300 Foot was resurfaced, I carried out a highly successful drift scan survey of a small region at 6 cm, during which the telescope showed what it could do at this wavelength. Inspired by this success, I decided with my co-conspirators, Ken Kellermann and Ivan Pauliny-Toth, to try to get more rapid sky coverage, to do the whole region north of 35°declination, which had not yet been covered by the 140 Foot 6 cm survey. With a 3 arcminute beam, the only way to get sufficiently rapid coverage was to use the 10°/minute slew rate. Fred Crews got a little worried when I told him we were only going to scan for about a minute before stopping to reverse direction. And he got really concerned when I said this was going to continue uninterrupted for several weeks! There were dire predictions of disaster, which seemed to be borne out when smoke began to boil up from the drive mechanism within the first few hours of the observing, in March of 1972.

We stopped the run and went out to see what the problem was. It turned out that the clutch which links the variable speed drive into the system had been rubbing slightly, and the longer we ran, the hotter it got, until it finally gave up the ghost. It looked like all was lost, until Fred (or maybe it was Bob Viers) turned to me and asked, "You going to need the variable speed drive for this run?" When I responded in the negative, the clutch was promptly removed, and the resulting air gap provided no further fireworks! We then successfully completed several weeks of the most flawless observing it has been my pleasure to analyze.

Later analysis of the pointing showed repeatability of 10 arcseconds— on a telescope moving 600 arcseconds per second! The data formed a finding survey for strong sources at 6 cm which would have been very difficult to get in any other way. And all due to the clever invention of the perfect clutch replacement—a six-inch air gap.

Don Nelson, telescope operator.

The 300 Foot — Tales

Don Nelson
NRAO - Green Bank

On a typical summer night in Green Bank in the 1970s when the fog was hovering above the ground about 100 ft. and all one could see was the dish on the 100-ft. tall tower at the interferometer. As the astronomer started to leave, he came running back in all excited saying he saw a UFO. Of course it took me by surprise so I walked to the door and looked outside at what he saw only to find that the UFO was the dish standing on the tower above the fog. Nevertheless, it took me about five minutes to convince him that that dish had a 100-ft. tower attached to it. By the way, I have looked in the American Astronomical Society Directory and don't see the astronomer listed.

Then there were always a couple of Guards who would try to sneak in the building around 4 a.m. and catch the operator napping. This was characteristic of one guard in particular, so I decided to have some fun also. I brought my "Game in distress" call in and waited for the Guard. As he idled into the parking lot, I slipped out the side door and waited until he got out of the vehicle and started up the front steps. That's when that "animal" almost ate up the Guard. From that night on the Guard made noises when entering the building. Also I think that is why that Guard decided to retire a little early.

Those "game calls" also work on astronomers who ride bikes late at night. They have been known to almost change color and to set record times back to the dorm.

Probably one of the most exciting times was during an electrical storm, and things were beginning to get a little scary. It definitely was one of the worst storms that I have experienced here, and I thought the building would split any time. The gods were working overtime on this storm and I didn't know where to go or what to do next. I started out of the control room, and upon opening the door another crack of lightning and thunder occurred just as a charge of smoke came up the stairwell. This was the point at which I thought I had been had, but it turned out that it was only the Insecticide Dispenser giving off its spray.

Don's Groundhog

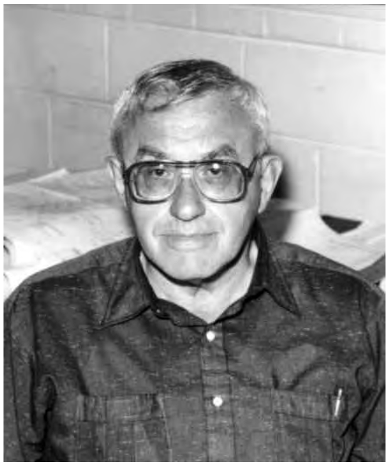
Frank Corwin (formerly NRAO)
and Rick Davis (NRAO - Green Bank)

Groundhogs are an ever-present problem at NRAO, they munch on the cables, undermine foundations, and of course dig lots of holes. One summer day a few years back, Don Nelson decided to wage war on the groundhogs. He brought a steel trap to work and thought maybe he could remove a few of the pesky critters.

There is one groundhog hole on the south side of the south hole wherein resides a particular old and wizened groundhog, Don's first victim. Well, the first day, Don carefully laid his trap in the mouth of the hog's hole, and we all waited. Early the next morning several of the 300-ft staff scurried out to see the results of the first round of the battle, the trap had been sprung, but there was no hog to be seen! The same routine occurred for three or four more days with always the same results—sprung trap and no hog.

Having tired of losing, Don decided to take on a less intelligent foe, a younger groundhog. As it happened, there was a handy young hog living in a hole in the bushes near the FM antenna tower, Don's next opponent! Again Don carefully laid his trap, and again we all waited. The next morning, Don went out to reap his reward (did he ever!). After a few minutes, he returned looking for help in freeing a prisoner of war.

As it turned out, the young hog had subleased his hole—to an old and wizened skunk, which was Don's first victory! During the ensuing attempts to free the skunk from the trap, using a variety of stocks, poles, cardboard shields and the like, the old skunk managed to position his (or her) backside pointing directly at Don. Needless to say at this point, Don's worst fears were realized! Someone lowered a corner of a shield and sure enough, that skunk planted a large dose of scent right in the middle of Don's forehead. Poor Don wasn't fit to be near for the rest of the day, and as I remember, he had to go home early that day.



Fred Crews, head of Green Bank Telescope Operations, about 1990.

The 300 Foot: Various Stories

Fred Crews
NRAO - Green Bank

Construction trouble — Sid Smith tells the story that during the erection of the telescope, he noticed one day that among the pile of preassembled trusses lying on the ground, one was about a foot or so longer than the rest. This was bad news. He rushed up to John Findlay's office to tell him about it. John immediately swung into action. Get the Business Manager going! Then call up Bristol Steel. There has to be an engineering meeting in Green Bank immediately. Charter a plane, get here, etc. And so the next day all arrived and went rushing off to the 300 Foot site to look at this problem. Lo and behold, the long truss was not to be found! Finally, the field erection foreman volunteered that he had noticed the problem, and had cut the truss off the evening before!

Pulsar Story — Al Hogan tells the following story regarding some observers during early pulsar days. This observing team had strong differences of opinion as to the details of observing and equipment configuration. Just before the transit of a newly found pulsar, the group fell to arguing and continued to do so all through transit time. By the time the air cleared, the pulsar had long since transited and it was all over—no data at all.

Groundhog Story — For some reason groundhogs have figured a lot in the 300 Foot stories. They have always been a nuisance, and have chewed insulation off cables and generally made pests of themselves.

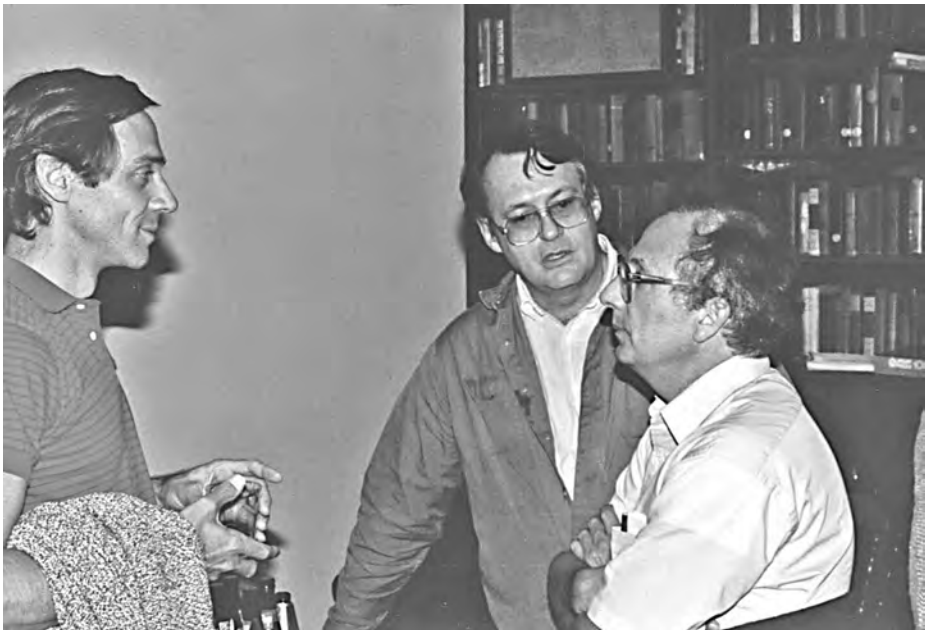
I recall a time that I had to fill in on a few evening shifts for an operator who was on vacation, and I kept watching a groundhog hole some 200 yards away knowing those critters had ruined some cables. Shooting in that area of the site was known to be against the rules, but I just happened to have my .243 with a 6 power scope in my car, and finally I couldn't stand it any longer. I crawled through the window, went to the car and crawled back into the control room with my rifle. I eased open the door, got a good crosshair on "Old Sam" and let go. "Dang" — I missed!

The Interferometer operator, another few hundred yards away, heard the blast and, following my instructions very dutifully, called the Guard. This Guard happened to be Ben Gragg, who had about the same interests as I did. Anyway, he came down and looked around and wondered if I'd heard any shooting. Though my ears were ringing, I honestly didn't believe I had. Then I thought that if I could just incriminate Ben in some way, this incident might be completely forgotten. While we were passing the time of day, I noticed over Ben's shoulder that "Old Sam" had popped his head up again.

So I began to tell Ben of all the difficulties those groundhogs were causing and that we were losing telescope time at the rate of about $800 per hour, and I was real busy, and since he was a real good conscientious Guard he ought to help out the Observatory. He wondered how he might do this. I mentioned (after some thought) that maybe I had left my groundhog gun in the car before I came to work. Sure enough, on looking, I had! Believe it or not, Ben proceeded to wipe out groundhogs from four different holes. Ahh, that was what I wanted!

I thought all was forgotten until a few days later, I was called on the carpet for firing a gun in the vicinity of the 300' and the reason the Guard knew, he had found a spent .243 round in the chamber of my gun when he opened it. I just kept my mouth shut. Ben Gragg had the last laugh.

"also desirable that the Observatory be situated in a location where nature is not unduly severe. . . . " [From the Observer *February 28, 1964]*



(Left to Right) Don Backer, Tom Clark, Ken Kellermann at the 300 Foot 25th Birthday celebration. [Courtesy G. Verschuur]

Early VLBI with the 300 Foot

Tom Clark, NASA-Goddard

Bill Erickson and I had a lot of fun back in the early 70's. The VLBI experiments in question were ones where we had three stations on the air: the 300 Foot, the 150 Foot antenna at Sugar Grove (the second largest antenna in West Virginia), and Arecibo, the largest antenna on American soil. John Broderick single-handedly took on the task of the work at Arecibo.

I remember one experiment where we brought on a new frequency for the first time. We were observing the pulsars in circular polarization. We started the experiment between here and Sugar Grove before Arecibo came on the air, and decided to take some test tapes and hot-foot them back to Goddard for correlation.

[It quickly became clear that the two telescopes were observing opposite polarizations.] It was agreed that Sugar Grove had the problem. However, because Arecibo hadn't joined us yet, we went ahead and made the change at Green Bank in order to have the polarizations match for the rest of the night. I was going to contact Bill Erickson in the morning. Meanwhile I went off to bed. One of my students called up Sugar Grove and said the polarizations were wrong and so Sugar Grove also changed.

Bill and I started exchanging notes when we got up the next morning, and realized this double parity error had occurred. We immediately called up John to say "We blew it, the last two hours we have been observing in the wrong polarization." John said, "That's all right. I just found out that for the last two hours I have been observing on load." A perfect experiment !

The Coldest Chipmunk I've Ever Seen

G. W. Swenson, Jr.
University of Illinois

The year was 1965 and we were living in one of a row of government-owned houses on the grounds of the National Radio Astronomy Observatory, in the pastoral beauty of the Deer Creek Valley of West Virginia. One day, at my office in the lab building, the 'phone rang and an excited voice told me that Julie had been bitten by a wild animal. The story tumbled out: my 13-year-old daughter had been watching a literal cat-and-mouse game in the front yard. A chipmunk would poke its head out of a hole in the lawn, Julie's cat, Freckles, would dash toward it, and the rodent would withdraw, only to reappear at a different hole. After a few cycles of this, though, Freckles caught on to the scheme, feinted toward the occupied hole, and captured the chipmunk at the other hole when it incautiously appeared. Julie rushed to rescue the unfortunate creature, which bit her on the thumb as it was freed from its captor's grasp. This time the chipmunk did not reappear.

I was concerned. Sick and disoriented skunks and foxes had been reported in the neighborhood and there was discussion of a possible rabies outbreak. Over the 'phone the physician at the neighborhood clinic urged me to recapture the animal and have it autopsied. I was unsure of the appropriate strategy, but Julie merely turned the problem over to Freckles, who repeated her earlier performance quickly and efficiently and delivered her prey without argument. I put the chipmunk into a shoebox and weighted the cover with a brick.

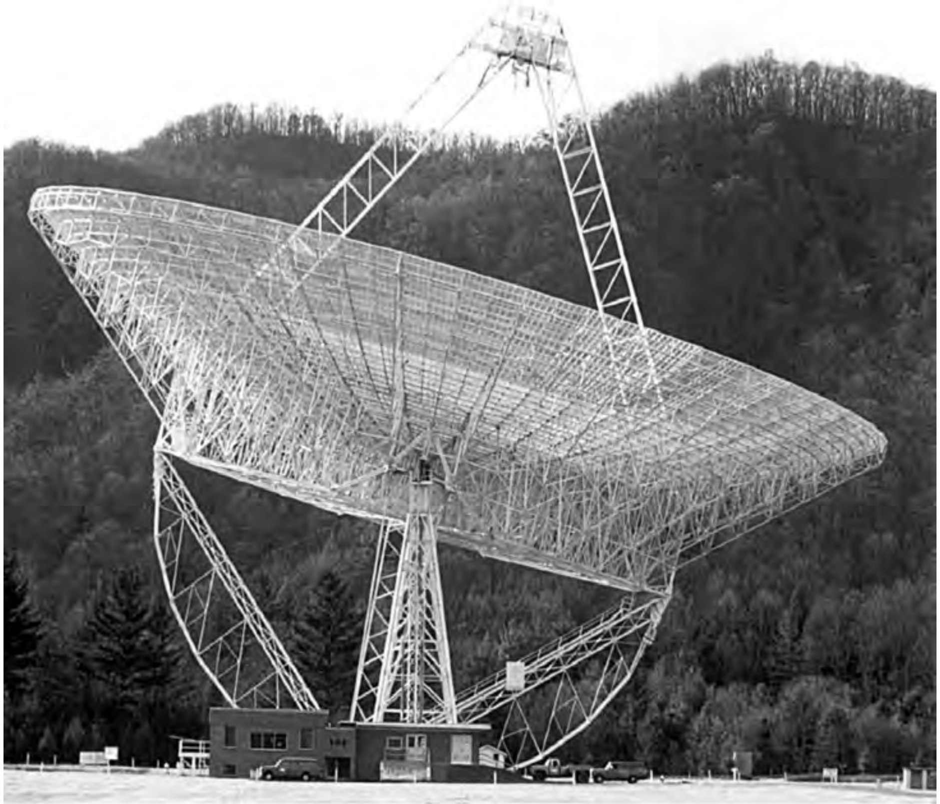
Now what to do? I telephoned the State Health Department in far-off Charleston. The pathologist suggested I decapitate the animal and send the head to him. Who, me? The idea did not appeal. The animal was alive and scratching in his box. I called the County Public Health Nurse and asked if she'd do the job. Indignantly I was informed "That's man's work!" Another call to the pathologist revealed that he'd accept the whole animal, that gassing would be a quick, painless, and medically-acceptable mode of execution, and that the remains could be frozen for shipment. I didn't want the specimen to deteriorate enroute. Cutting a hole in the shoebox, I slipped it over the tailpipe of my car. In about twenty seconds the scratching ceased and a peek revealed the victim peacefully at rest.

In the electronics lab we had facilities for cooling the low-noise amplifiers of radio telescope receivers. I got a large tin can from the cafeteria, lined it with polyurethane foam, and laid the chipmunk to rest therein, surrounded by plastic bags of water to add thermal inertia. Then I poured liquid nitrogen (boiling at –196 degrees C) over the ensemble and wrapped the can in more foam and layers of kraft paper. Driving at top speed the fifty miles up the valleys to Elkins, I met the afternoon DC-3 airliner to Charleston and dispatched my precious package.

Two anxious days later the pathologist 'phoned with the good news that the chipmunk had not been rabid. His parting words were, "That was the coldest chipmunk I've ever seen!"

6 The Collapse

"Fred, we've got a problem . . . "



Top: November 15, 1988.
Bottom: November 16, 1988. Photographs by Richard Porcas.

The Night the 300 Foot Fell*

Monday, November 14, 1988

Don Nelson, 300 Foot Telescope operator:

On Monday Bob [Bob Viers, 300 Foot Telescope Supervisor] and I were standing out at the relay building underneath the telescope. We heard something that sounded like an angle iron catching and snapping as the antenna was slewing back and forth. Bob said, "I'm about half afraid to stand under this damn thing; it might fall on us." They had previously heard noises, so I guess he was getting a little jumpy from that.

Tuesday, November 15, 1988

Fred Crews, head of telescope operations:

During that day the operator from the 300 Foot called and said that they were hearing a noise up on the telescope and they couldn't imagine where it was coming from. It was like something scraping—it would scrape and hang a little bit then scrape and hang a little bit.

I climbed up on the telescope and couldn't pinpoint the location of the noise, but I could hear it. I got Ron and Don Gordon up there and they were a bit more brave out on the structure than I was. [The noise] had to do with an unused cable—the connector was loose and it had been scraping on a piece of steel when the telescope moved. We fixed it right there, and we moved the telescope and there was no more noise, so everybody dreamed that everything was fixed.

Ron Gordon, telescope mechanic:

We were on the telescope a couple of hours before it fell. I am pretty sure that there was a receiver change scheduled for the next day, so we would have been up on it then. Donald and I, with assistance from other employees, did all the receiver changes—sometimes once a week, sometimes twice a week. We were always up on it. It was close to evening—we were ready to go home, and Bob Viers called and said that he heard a noise up on the telescope. So we went down, like we normally did, and went up the east and west towers, Don on one side and me on the other. Bob ran it all the way over, all the way north and all the way south. It sounded like it always did, as calm as it could be. Of course you heard that cracking and popping of the surface like you always did, the sounds the surface always made. When we got down Bob asked what we heard and I said that we didn't hear anything unusual.

* From interviews in 1988, 1998, 2001, 2002, and 2006.

Tuesday Evening, November 15, 1988

Greg Monk, 300 Foot Operator:

Greg Monk

Bob Vance had been down earlier that evening to check on something; George Seielstad, the scientist supervising the observing for this program [and the site Director], was there for some time. After a call to Jim Condon in Charlottesville to discuss the program, Seielstad left.

The telescope was doing a south to north scan and it was my intention to go into the kitchen to get some food after the telescope finished this scan and started south. I got up to go down the hall, and I am sure the telescope turned around and was moving south but only for a short time.

Harold Crist

There was a crack. I now remember a low rumble like an overhead jet aircraft and then a crash—I think all this took about 2 or 3 seconds—something came through the ceiling of the old building, and I could see dust falling down the hall. The clock fell and a ceiling tile or two fell in the control room where I was. The ceiling buckled up and dust and dead flies and other debris started to come down. I believe a fluorescent light fixture cover or two fell also. All the lights went out except for the emergency lighting.

I was at the phone, and now realize that I dialed the wrong number for the 140 Foot control room. I thought the phone wasn't working. At the same time, I pushed the telescope control panel stop button which was right in front of me. I remember that the 3-digit coarse readout was reading +74.7 degrees.

I ran out of the building to get into my car and go for help. I discovered that I had forgotten my keys and ran back inside to get them. On returning, I started the car, and as I swung around at the north end of the parking lot, I saw some structure lying on the ground west of the pavement, and thought that only some part or member of the telescope had fallen. I had not heard any noise at all as I left the building and was outside. As I drove to the 140 Foot, I heard some glass falling and discovered that the back window of my car was out.

Harold Crist was operator at the 140 Foot— he had been my science teacher in high school before he became a telescope operator. When I arrived at the 140 Foot, I ran inside where Harold Crist and George Liptak were working. I told them that part of the 300 Foot had fallen. They thought at first that I was joking.

Harold Crist, 140 Foot Telescope Operator:

I was standing in the lobby of the 140 Foot talking to George Liptak who was the supervisor of the 140 Foot. He had come into the scope and we were standing in there discussing what might have happened to the clock [which was malfunctioning], when suddenly the door flew open and Greg Monk came into the lobby. His nose was bleeding slightly because in his anxiety to get into the building he had crashed into the stationary fixed door rather than the one which would swing open. He was excited, his eyes were glassy, he couldn't speak very fluently, and he blubbered something about something falling off of the 300 Foot.

Well, George and I looked at one another, and I said "Greg, I think that you have had a little too much to drink" because I thought probably he had been down to the local bar and had maybe had one too many and someone had punched him in the nose. But he kept on saying, no, something fell off the 300 Foot. And that was the reason why I thought maybe he was making the story up. I didn't think he was scheduled to operate that night, but later I found out that he had replaced one of the regular operators on the evening shift.

Finally, he convinced us that something may have fallen from the 300 Foot, and George said that he would drive down and see what happened. When we went outside and Greg opened his car door, there was glass scattered all over the car, his rear glass was broken out, and a large bolt painted like the 300 Foot lay in the back seat, which was startling to see and gave us the feeling that something serious had happened at the 300 Foot. I went back to operating [the 140 Foot] and George went with Greg down to the 300 Foot.

George Liptak, 140 Foot Telescope Supervisor:

I was at the 140 Foot that night. Harold had trouble with some piece of equipment, so I came in from home. We got the problem resolved, and then a car pulled up to the front door and Greg came walking in. It was the funniest thing—Harold was standing by the console and I was standing nearby and Greg walked in with this funny look on his face. He didn't look at either of us but was staring at the back window above the console. For a second he didn't say anything then he said, "I think the telescope fell down."

We didn't say anything but did not think he was being serious.

He said again, "I think the telescope fell down." He was nervous and it looked like there were pieces of glass on his shoulder. I decided to go down to the 300 Foot with Greg, and when we got outside I could see that the back windshield of his car was knocked out.

We went in my vehicle. We never spoke a word on the way down. There wasn't a light on anywhere. When my headlights hit the straight stretch of road I saw the telescope. It looked like a rotted mushroom that had collapsed. In the parking lot there was debris all over—nuts and bolts. My first impression was that maybe it drove into the ground to the north —went through the limits and drove into the ground.

Greg Monk:

Liptak and I drove to the 300 Foot in his truck. As we were approaching the 300 Foot, with the help of the truck lights, we realized that the entire structure was down. As soon as we could get inside, I called Rick Fisher to ask him to come down and shut off the computers, telling him that the telescope had fallen. To my knowledge, I did not stop the computer program, as I thought that any information might be valuable.

George Liptak:

We went in the front door—it was the only one that would open. There was debris over the door to the back kitchen area, and there was no exit out the back of the building on the south side because the structure was lying over it.

We got inside and we walked into the console area. Stuff from the roof had fallen over the floor, and you could see right through the roof. The oddest thing was that the big electric clock was knocked off the wall but its cord was attached and it was hanging down still running.

First thing I did was to look into the other room in case there was a heater going that might catch fire. I was worried about the possibility of a fire and I asked Greg to gather up fire extinguishers. But there was no fire, nothing was sparking, though lots of the equipment was still running. I took a trip through the kitchen area and opened up the bathroom door and there was a girder there—it must have been $4'' \times 4''$—that had pierced the top of the building and come through the roof. If someone had been sitting on that commode it would have impaled them.

I called Harold and told him what happened because I didn't want him to be sitting up there wondering what was going on. I told Harold to call Fred Crews.

Harold Crist:

About ten minutes later the phone rang and George said, "Harold, you call someone out, the 300 Foot is flat on the ground." Well, you can imagine my shock. It was hard for me to believe. He said, "Call someone in, 'cause we got troubles down here."

Well, I called Fred Crews first of all and I said "Fred, we've got a problem. The 300 Foot has fallen." And he said, "Harold, I think that you do have a problem. What is the matter? Is something wrong with you?" I said no, Greg Monk and George Liptak were down at the 300 Foot and said that it had collapsed. He said, "Harold, don't be pulling my leg. What's going on here?" And I said, "You'll have to come in and see for yourself. I haven't been down there but George called back and said that the 300 Foot had fallen and get someone in here." Fred said "All right," kind of nonchalantly, "OK, I'll check it out."

In the meantime I needed some information from George Grove and I called him and in our conversation I told him that there is a possibility that the 300 Foot had collapsed. You know how George Grove is, he kind of laughed and said, "I can't believe that." And I said that was just what George Liptak told me, I haven't seen it. Well it wasn't maybe 15 or 20 minutes later George Grove came lumbering in and he said, "Well you were right, that thing is flat on the ground."

Fred Crews:

I was at home when I got a call from Harold Crist and he said, "Fred, the 300 Foot just fell down." and I said "Harold, you've had one too many." But I went on down anyway.

[After arriving at the 300 Foot control building,] we were really worried—there were at least two or three computers sitting there running, there was a data computer and it was recording telescope positions, there's also the driving computer and they were very separate. We wanted to be sure we preserved all the information we had in the room. Rick Fisher, Bob Vance, and George Seielstad showed up, and there were a number of people, and boy they were busy working. The number one idea was to protect the data, but also if the weather turned bad that night we could have water all over everything, so we covered everything up.

George Liptak:

I called Bob Viers and told him, "Bob you're out of a telescope, it collapsed." He was in shock. Fred came in almost immediately. Fred called Ron Weimer [digital engineer]. We started surveying equipment, and the decision was made to shut off power to everything we could. We went down into the basement where stuff was still running, and Ron [Weimer] got the fire truck there. Richard Fleming [business manager] showed up and we rounded up all the plastic we could find and covered up all the equipment we could find and turned everything off.

George Seielstad, Green Bank Site Director:

I was at home when Fred Crews called at about 11 PM, and he said, "You have a telescope down." I'll never forget that. I did not know what he meant—you never assume that the telescope has collapsed—perhaps a receiver is down or something had burned up which would take several weeks to fix.

I went straight down to see it. Rick Fisher was there and everyone was standing around in a state of disbelief. There was not one thing we could do at that time. Its not as if the next day we could get wrenches and fix it. We stood around for an hour or so with our mouths open. I called Paul Vanden Bout [NRAO Director] around midnight.

Harold Crist:

Well the 140 Foot telescope schedule wasn't too pressing, so I said to George Grove, "Why don't you sit here at the telescope and if anything happens hit that emergency stop— I want to go down there and see what happened." So he said OK and I drove down to the telescope and you couldn't believe it, it looked like a big steamship collapsed or capsized and the spires sticking up in the air and nothing but a big pile of rubble and steel lay strewn on the ground.

So I came back to the telescope and it wasn't long before Nathan Sharp came to relieve me [and work the midnight shift at the 140 Foot] and once again I prevailed upon George to watch the telescope and I told Nathan, "Come on, I want to show you something." And Nathan said, "Where are we going?" and I just said "Come on." We got in the car and we drove down toward the 300 Foot and I kept my lights dim till we got almost halfway down and I turned my high beams on the 300 Foot and Nathan looked up and said "My God, I never saw such a mess." And that was what all of us felt when we saw the 300 Foot lying on the ground.

Nathan Sharp, 140 Foot Telescope Operator:

I was on the midnight shift at the 140 Foot. I got to work that evening about 20 or 25 minutes early and there was Harold Crist and George Grove in the control room. Harold said, "The 300 Foot fell down, let's go see it." Now Harold always was a prankster, and telescope operators are a tight group—sometimes we would pull little tricks on each other—but there he was all big-eyed, and I knew something was up. He went on and on about the 300 Foot, and I wasn't buying it, but I knew that something was going on because he was so excited. So we hopped in Harold's vehicle and drove on down. On the drive there you couldn't see a thing, but the moment the headlights hit it I was dumbfounded. My jaw just dropped in total disbelief.

I went all over the 140 Foot that night, checking the cracks in the wall and listening for sounds.

Bob Vance, computer programmer:

I was working in the Lab that evening with Ron Maddalena—my wife, Bunny, was sick and in the hospital and I worked a lot of evenings then —when George Seielstad came down the hall. He said that we had just experienced the worst catastrophe in NRAO history; the 300 Foot had fallen. Well, I knew he was serious because his face was white. Ron and I went down and helped shut things down—they were very concerned about keeping all the computer files intact. Greg Monk was there and was pretty shaken up.

Nathan Sharp

Ron Maddalena

Ron Maddalena, staff astronomer:

Bob and I got one of the vehicles and drove down to the telescope. [Probably about 40 minutes after the collapse had occurred] When the headlights hit, it was definitely a sight to behold. My first comment was that I didn't know steel could bend like that: it looked more like caramel than steel.

We walked around the telescope right up to the edge of the dish as it was lying on the ground. We didn't realize it was still settling and we really weren't in a safe spot, nonetheless we didn't hear anything or see anything move.

We went into the control room where Fred Crews and George Liptak and a couple of guys from the works area were. Everyone was calm and collected; they were taking plastic and putting it over the computers to try to protect them from the rain, because the roof had holes in it. It was quite amazing to see such a dramatic event and everyone remaining extremely calm. Everyone was doing their job and that was one of the more impressive things.

George Liptak:

It was such an eerie feeling, because everything had been shut down and the place was super-quiet—no noise or flashing lights on the receiver racks. That was a night I saw a lot of guys with tears in their eyes. Bob Viers was absolutely crushed—he just couldn't believe it.

Fred Crews:

Greg Monk didn't leave until about one thirty or two, but Wally Oref, who was the relief operator, came in at midnight, and after fiddling around for a little while he said, "Aw, shucks. This isn't going to amount to anything," and he went back home.

Bob Viers

Don Gordon

Bob Vance:

Later on we took Greg's car, which had its rear windshield broken by something falling from the telescope, and put it in the warehouse for the night. After we were done I offered to drive him home. It was about two in the morning. When we got to his house he asked me if I would come in and tell his dad, Wendell, exactly what had happened, because he was afraid he would never believe it. Well, Greg and I woke up Wendell and told him what happened, and at first he didn't believe us but finally was persuaded.

Harold Crist:

It was quite an evening. I came home and told my wife and took her down to see the fallen structure. It was kind of like the sinking of the Titanic—a night I will always remember. November 15 at 9:43 in the evening when the 300 Foot fell to the ground.

Wednesday Morning, November 16, 1988

George Liptak:

I was there till way after daylight. Then I had to go back home and change clothes—I may have even had my pajama tops on the whole time. I went home and changed because I had to work the day shift on the 140 Foot. I had only seen the wreckage in the dark. The next day there were buses taking our people down to view the telescope but I couldn't go because I had to work 8 hours. I answered a lot of phone calls when the word got out. It was a sad day.

Ron Gordon:

We don't know what noise Bob heard when he called us down there [Tuesday afternoon]. If you aren't there you can't pinpoint it. The noise he heard could have been something cracking, you don't know. We were concerned over the years. In the summer we would have inspections and find some small members broken, then there were the studs that held the panels down which started to break. There was a lot of that. We were working on it the day George Seielstad first came to work here. Basil Gum was up on the scope and he looked down and said "Who are you"? And George said, "I'm your boss."

Winston Cottrell called me that evening and told me it had collapsed. He used to work outside with us. He called me that evening on the telephone, and said "Gordon, the telescope, the 300 Foot fell down." I said, "Boog, no" and he said "Yes it did, it fell down." I couldn't believe it. I called my brother and he said he was going on in. I got up the next morning about daylight and I came in and it was really sad. It made you sit back and think. We had to go down there to check things out, to see what could be done. We had to mark the place off to keep people from getting hurt.

Ron Gordon

Pete Chestnut

Pete Chestnut, 300 Foot Telescope Operator:

I was scheduled to work the day shift at the 300 Foot coming off a four day break. Nobody had called me or said a word about what happened, so I just drove into work that morning. I came north on Rt. 28, down the hill at Allen Sheets' house, and up the straight stretch when a little light went off in my head: I hadn't seen the 300 Foot. Well, I thought that I must not have been paying attention, or maybe they had it tipped way over to work on it. But even with it tipped over I still should have seen it. I must not have been paying attention. I went back and forth with myself and by the time I reached the Grade School I had convinced myself that the telescope had been there. Then I entered the Observatory through the gate at the Grade School, got down to the intersection, and saw this big mess.

Just the day before I had been at the bank applying for a loan to buy a house. I stood around the 300 Foot wreckage, stunned, from 8 AM until 9 AM when the bank opened, then drove up to the Interferometer building, called the bank, and said, "Hold the loan, I may not have a job."

Nathan Sharp:

In the morning someone from the local radio station (WVMR) called me—they would always call for the weather information—and said that they had heard something about the 300 Foot falling down. I didn't want to be giving out official information so I said that they would have to check with the site Director, but told them, "If you drive into Green Bank, you'll find that the view has changed."

Don Nelson:

I was in a restaurant over at home on Wednesday. I walked in and a friend said did you hear about the 300 meter falling. I said, "You mean the 300 Foot." "Yeah, yeah," he said, "It's been all over the national news." That was the first I heard about it.

When I got back home there were two messages on my answering machine: one was from Carolyn Dunkle [Electronics Division Secretary], and the other from Bob Viers. They said we think you should come on over here; we've got a catastrophe. They were right.

George Seielstad:

I had a meeting for all employees in the auditorium, and I told everyone that we would go down and see it. My main thought was, "OK, this event has happened, but what are we going to do in the future?" That was the message I tried to leave: we are going to have to recover from this. Then we went down to the site in buses and stood around gawking.

The telescope had been well taken care of. The staff had been conscientious and careful. My attitude was that we should clean up the mess, get it out of here, and focus on the future, not the past.

What surprised me was the speed with which word spread through the media. I got a call about 6 AM from a radio station in West Virginia, not anywhere near Pocahontas County, and I wondered how they heard about it. I got calls from Japan, New York City, everywhere. TV crews flew in with helicopters.

The remains of the traveling feed atop the control building. *[Photo courtesy G. Liptak]*

Post-Mortem

The Final Data*

Jim Condon, NRAO staff astronomer:

The computers were recording data up to and during the collapse— all the data were preserved. They showed that the telescope pointing and its beam shape had changed in the weeks before the collapse.

The pointing hysteresis had increased from the normal 10 arcsecond difference between north-going and south-going scans to 30-50 arcseconds. The beam width of the telescope had also increased during the last week from the normal 3 arcminutes to something more like 4-5 arcminutes. It was clear in retrospect that the dish was beginning to open up. The data from the last few minutes show a sudden increase in system temperature, as the receiver fell onto the roof of the control building.

The Investigation

Fred Crews:

The next day, Lee King [NRAO's chief structural engineer] called me to ask about our maintenance procedures. I told him that every summer we climb all over the structure for about a month, check for any breakage, and clear it up. We usually found a few small structural members out toward the edge of the dish that had broken. Because of the symmetry of the telescope, if we found a member broken we would look for three others like it. Anything that was broken would be replaced. If it broke three times, we would put in the next larger size piece of metal and that ended that. Lee pointed out that every time we did that we were re-designing the telescope.

A technical assessment panel was formed, and one of the consulting engineers asked me to look for a piece of metal in the wreckage that had broken in two and had a break with a smooth curving appearance like sand that has been pushed up by the tide. The next morning I just walked out of the control building door and saw a piece of metal which was one end of a gusset plate—at that time I didn't know exactly where it had come from. It was part of a piece that had come in two and looked just as the engineer had described—perfect.

I called him back and described it, and he said that over the years he had been involved in a lot of investigations and sometimes it is virtually impossible to figure out exactly what happened, but here's a case that we might be able to solve.

Months later at a meeting of the technical assessment panel in New York, a guy gave a presentation and said that according to his finite element analysis "The

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* *Condon's project, a collaboration with G. Seielstad and J. Broderick, had been running for about a month. It was a survey of the northern sky at 6 cm using a 7-beam feed. The telescope was driven at its maximum rate of 10 degrees per minute from the south limit to the north, then back again, repeating this movement continually over the course of the 6 week observing session.*

member that Crews is talking about is not the member that brought this telescope down."

I said, "Well that guy is wrong—he's wrong!"

He left the room and came back with full drawings and pointed to the gusset plate and said, "Is this the member right here you're talking about?"

I said, "That is the member I'm talking about."

He said, "In our analysis that was a $\frac{3}{4}$ inch thick plate."

I said, "You're wrong; it's only half an inch thick."

He said, "I'll be back in a minute."

He came back in and said, "Boys, this is it—we sized it for $\frac{3}{4}$ but it's a half." And he said, "Can you prove that?"

I showed him my photos of the gusset plate where it was split. Then they went and wrote the final report.

View from the South, November 1988.

The West tower.

from the NRAO Observatory Report in Bulletin American Astronomical Society

Vol 22, No 1, 1990.

The major event of the year reported, perhaps the most dramatic event ever to occur at a radio observatory, was the collapse of the 300-foot telescope on November 15, 1988. Although the telescope was a total loss, no one was injured.

The cause of the 300-foot telescope's collapse was studied by two teams, one internal to NRAO, chaired by D. S. Heeschen, the other completely independent from NRAO, jointly constituted by NSF and Associated Universities, Inc., under the chairmanship of Dr. Robert Matyas.

The latter report attributed the most likely cause of the collapse to fracture of a single steel gusset plate in a critical location of the support structure. The fracture began and progressed slowly in an area of the plate covered by adjoining steel members, preventing detection in regular antenna inspections. The fracture progressed to completion in a sudden ductile rupture that quickly led to complete collapse.

Shortly after this study was completed, bids were received for removal of the scrap metal. Dismantling and removal of the wreckage of the 300-foot telescope began in June 1989.

Report of the Technical Assessment Panel

for the

300 Foot Radio Telescope at Green Bank, WV*

March 1989

SUMMARY

The 300 Foot Radio Telescope at Green Bank, West Virginia which collapsed under its own weight while transiting on November 15, 1988 appears to have met and exceeded the original expectations of astronomers.

A post-collapse finite element space frame stress analysis, under self weight only, indicated that the stresses in a large number of circumferential and radial members were substantially higher than those currently permitted—in some cases by as much as 100% and more. Although only one connection was reviewed, it can be extrapolated that many gusset plates were overstressed by "secondary" forces not considered in the original design.

The fractured gusset plate which was observed during an inspection of the wreckage, was reviewed and found to be a critical connection in the diamond truss.

It was found that the stresses were high and the stress range during telescope transits indicated a limited fatigue life. This was verified by the fractographic examination which indicated that fatigue crack propagation under cyclic loading resulted in eventual fracture of the plate.

The Technical Assessment Panel concludes that the fracture of this gusset plate connection is the most probable cause of the telescope collapse. From a review of Observatory records, the failure of the telescope structure was not a result of inadequate maintenance or inappropriate operation of the telescope.

The analysis methods used in this study were not available at the time the telescope was designed. The understanding of fatigue and crack propagation under cyclic load have greatly advanced since.

This collapse points dramatically to the importance of having an accurate and comprehensive stress analysis for this type of movable structure, which can identify fatigue and crack propagation susceptibility in critical elements in order to establish programs of inspection and subsequent strengthening, repair or replacement.

There are no unfavorable implications about the current ability to engineer future telescopes of this or larger size.

* All illustrations have been added by the editors.

I. BACKGROUND

The 300 Foot diameter Radio Telescope at Green Bank was designed and built over a period of two years and put into service in October, 1962. At the time of its construction, this telescope was conceived to be an interim facility which would satisfy certain immediate instrument needs not being satisfied as a result of delays and cancellations of other planned or projected instruments. A high priority was placed on minimum cost and expeditious construction. The minimal environmental requirements imposed on the designer support the conclusion that the telescope was not intended to be a permanent facility. The scientific usefulness extended many years beyond the expectations of the astronomy community as a result of the rapid advancement of detector technology which greatly improved the scientific yield. Since no other comparable United States instrument has been built, the machine never became obsolete.

Another factor which must be understood is a major improvement which has occurred in the design methodologies used by engineers working in the early 1960's as compared to engineers working today. This improvement, called **finite element space-frame stress analysis**, has been made practical by the advent of large capacity digital computers of widespread availability. This analysis method allows the structure being designed to be represented by a large number (e.g., thousands) of mesh points whose stress relationship is determined simultaneously, whereas earlier methods required a complex structure to be simplified and typically neglected "secondary" flexural stresses due to frame distortions. Also, this antenna must be considered as a machine and as a dynamic structure, parts of which undergo large stress ranges in each transit of the structure. In this review, we used these modern methodologies not generally available to designers at that time. Our current understanding of the stresses of the structure thus greatly exceeds that available to the original designer, and our knowledge of fatigue and crack propagation due to cyclic loading is much improved.

2. HISTORY

The operators of the telescope, in routine visual inspection of the radial ribs and circumferential ring structure, observed and repaired occasional failures of smaller structural members from the very beginning of telescope operation. These failures were usually at the connection plates where structural members were joined by bolting. Over the 26 year life of the telescope, a few hundred such repairs were made, some in anticipation of failures in areas with high incidence histories. These repairs were never a detriment to the scientific performance of the instrument.

On a cool, windless night, on November 15, 1988, the telescope collapsed without warning while in transit motion. On November 18, 1988, senior officials of the National Science Foundation (NSF) and Associated Universities, Incorporated (AUI) established a Technical Assessment Panel to determine the cause of the telescope failure. On November 22, 1988, Panel members, E. Cohen and R. M. Matyas joined NSF and AUI officials together with a representative of the House of Representatives, Science, Space & Technology Committee, and visited the Observatory at Green Bank. Interviews were conducted along with an examination of the wreckage.

A third Panel member, Dr. G. F. Mechlin, was added on November 28th. The first official meeting of the Technical Assessment Panel was held on December

View from the north of the surface with the feed legs and traveling feed at left. The traveling feed penetrated the ceiling of the control building.

The control building.

9, 1988 in Washington, D.C. A key decision at this meeting was to order a finite element analysis of the telescope structure.

Another visit to the Observatory was made on January 4, 1989 by Dr. G. F. Mechlin and R. M. Matyas. During this visit and inspection of the telescope wreckage, there was observed a cracked major gusset plate connecting a lower element of the diamond truss which, at its other end, is connected to the bearing support framework. Based on a visual examination of the cracked surface, which suggested a significant crack which pre-existed the failure, one half of this cracked gusset was recovered and sent to a qualified metallurgical testing laboratory for examination. This laboratory examination is discussed in section 4 of this report since the gusset plate is believed to have played a significant or causative role in the telescope collapse. There are three other corresponding gusset plates in the structure. Two appear not to have a pre-existing crack. One (not yet recovered) exposed an indication of a pre-existing crack which appears significantly smaller than the crack on the plate already examined.

3. RESULTS OF THE FINITE ELEMENT STRESS ANALYSIS

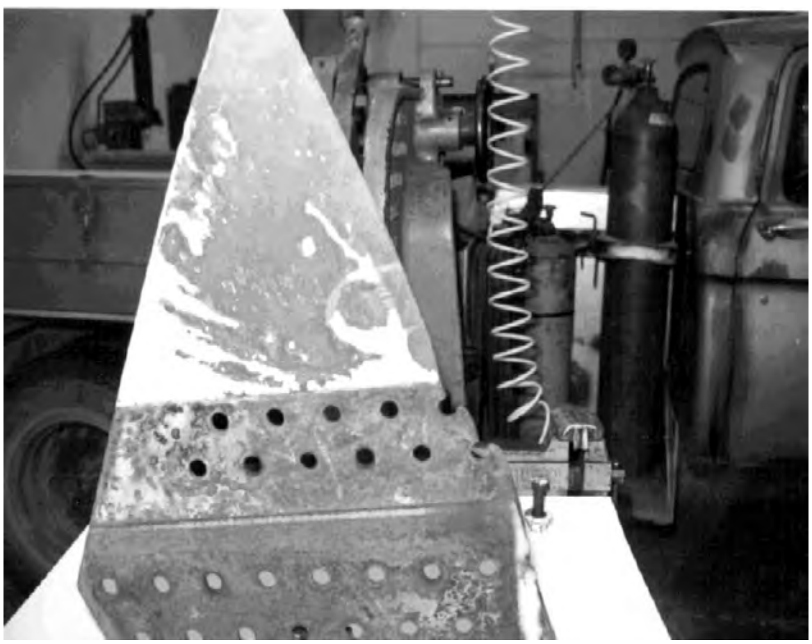
The salient findings of this analysis are that 1.) certain lower chord gusset plate connections of the diamond truss underwent numerous cycles of severe stress and 2.) the radial rib and circumferential ring structure in the vicinity of the trunnion truss bearing support structure operated at stress levels at which buckling and plastic deformation would be expected to take place. Two consequences of resulting deformations are predictable. First, it would cause a redistribution of loads into other adjacent members, which would generally cause loads of these members to increase even beyond the levels calculated by our stress analysis. The second consequence would be the occasional failure of individual structural elements as observed. A design analysis performed four years after completion indicated some such overloaded elements. Some remedial repairs were undertaken accompanied by additional modifications to stiffen the structure and to improve the image stability.

It must be understood that the current analysis was made of the telescope in its idealized dimensional state prior to the collapse. Such a state is never achieved since stresses developed in a partially completed state during erection impose initial deformations and stresses which vary from ideal, which then would be additionally altered by the modifications and repairs mentioned above. As earlier suggested, these effects tend to make actual stresses greater than calculated for the ideal structure.

One concludes from current analysis that, from the beginning of its life, the structure was marginal with respect to structural failures of a minor or perhaps a major nature. A very significant portion of this marginal status was that the diamond truss structure depended for stability on the interactive support of the radial ribs and circumferential rings which, in turn, contained members required to operate beyond their safe load carrying limit.

Because of complex load redistribution effects, one does not expect a strict correlation between individual members determined to be overloaded and observed individual failures. There is, however, a general correspondence between areas showing calculated overload and observed damage. Early in its life there were several significant modifications made to the telescope. These included the reinforcement and stiffening already alluded to. A new feed structure consisting of a dish-mounted,

Fred Crews examines the wreckage.



Portion of the failed gusset plate, one half inch thick, as recovered from the wreckage. The plate fracture was 37 inches long and intersected the two holes at the right.

guyed bipod was added and the original open mesh dish surface was replaced with a finer mesh surface suitable for higher radio frequency operation. The finite element analysis reported herein found that after these modifications, the structural integrity of the directly affected portions of the telescope were somewhat enhanced.

4. SEQUENCE OF FAILURE

The Technical Assessment Panel concludes that the probable cause of telescope collapse was the progressive cracking of the gusset plate at the end of the lower chord of the diamond truss at the northeast corner. This lower chord at its other end intersected with the support bearing structure. The failure of the lower chord of the diamond truss in this location destroys the ability of the truss to carry load as a truss and collapse ensues. The progressive cracking was caused by excessive stress in the gusset. As calculated in the idealized state of the structure, the stress was far beyond limits which would have precluded such progressive cracking. The crack origin was probably associated with two punched bolt holes where the severe working produced by the punch could have left an initiating small crack.

Results of the metallurgical investigation of the fractured gusset plate revealed that the plate failed as a result of progressive cracking in the nature of fatigue. Propagation of these fatigue cracks under cyclic loading from both bolt holes eventually resulted in a fast ductile fracture when the combination of cyclic stress range and crack size exceeded the fracture toughness of the plate material.

The results of the fractographic examination revealed [that] secondary fatigue cracks also had originated at the bolt hole surfaces. The presence of secondary fatigue cracks at the bolt holes indicates the presence of intermediate to high cyclic stresses.

It cannot be unambiguously determined whether the subject progressive crack had simply grown to a point where the remaining material in the gusset could no longer support the load or whether some otherwise minor event or failure immediately preceding the collapse added a new increment of load to the gusset. Two such minor events can be postulated. One event might have arisen from an increased friction force or jamming of a support bearing. The west bearing assembly was recovered for inspection while the east bearing is still inaccessible. During this salvage operation, the unloaded shaft was rotated with ease, but when the bearing case was opened, the grease was observed to contain a myriad of metal flakes and the spherical rollers exhibited a peened surface demonstrating progressive damage. The appearance of the bearing rollers suggest only a modest increase in frictional torque, however, the bearing was in the initial stages of failure and probably would have itself prevented use or caused structural failure of the telescope at some future time. Another likely event is one or several failures of already overstressed radial rib or circumferential ring members shifting additional load onto the box frame truss.

The panel sees no merit in terms of lessons for the future in further tedious and perhaps impossible tasks of determining whether the gusset failed first or was driven to failure by such a preceding event. It is very clear that this gusset was rapidly approaching failure prior to the event and that the failure of this plate was the key element in the total collapse of the telescope.

The Panel recommends two additional corroborating investigations be performed by the Observatory as appropriate prior to or during wrecking. One is the recovery and visual examination (only) of the second gusset believed to be cracked. The second is disassembly and visual examination of the east bearing assembly for signs of distress which might indicate high friction or jamming.

The Panel further recommends caution and extreme care in recovering any artifacts or structural elements from the wreckage. In such activities as well as the subsequent disassembly and removal of the wreckage, it is imperative that the work be supervised and monitored by a competent structural engineer working with an experienced industrial wrecking crew.

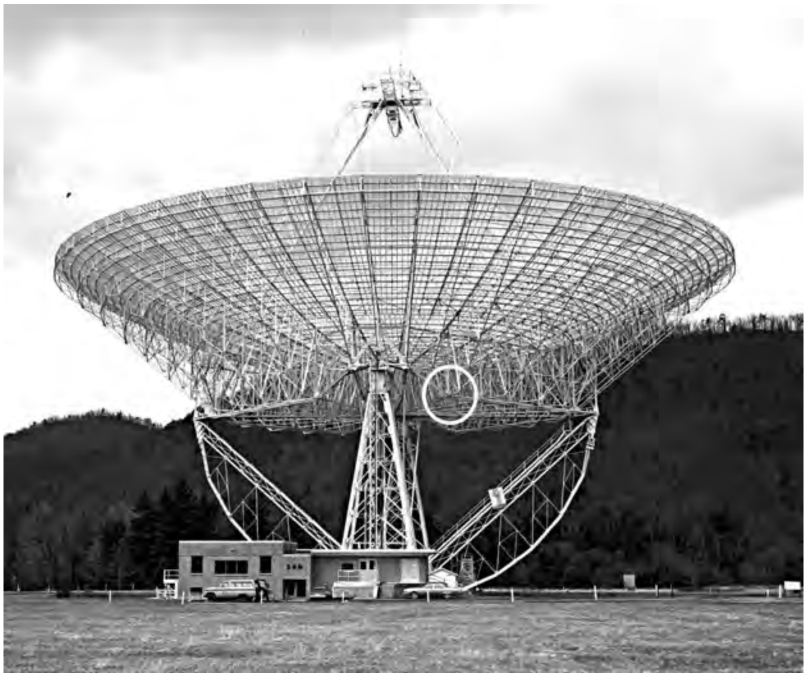
5. RESPONSIBILITY

From a review of the records, it is the opinion of the Technical Assessment Panel that the failure of the telescope structure was not a result of inadequate maintenance or inappropriate operation of the telescope.

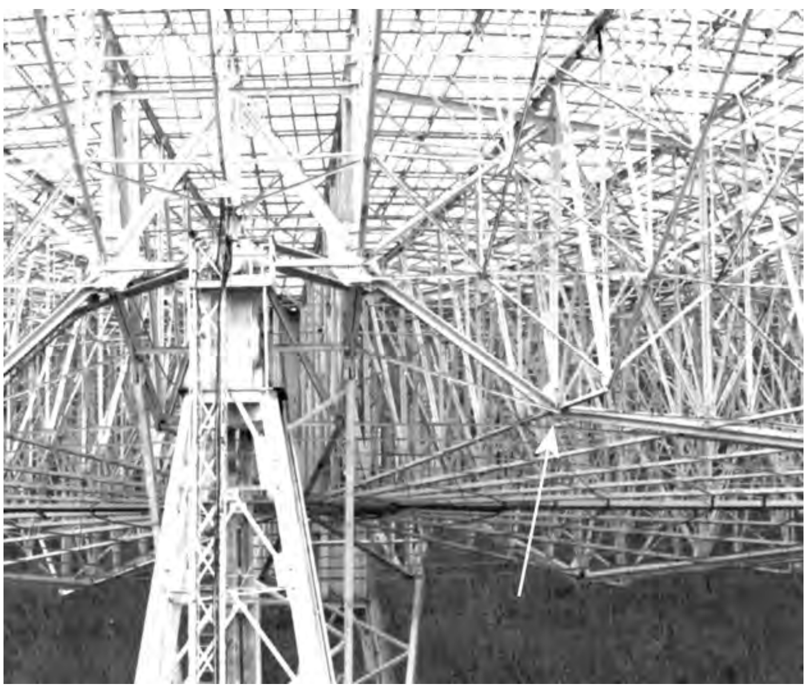
The contributory roles of the designer, the constructor, or of the subsequent reviewer cannot be sensibly commented upon after so long an interval other than to say that the telescope performed longer than the expectations which the Observatory and the designers must have shared.

There were no observed structural failures in the history of the telescope which would have suggested a need for a third engineering analysis of the sort performed in this investigation. The gusset plate in question was cracked in a fashion such that most of the crack was concealed beneath the structural elements to which it was connected. There was lacking any signal that the gusset was failing and an examination was not possible without disassembly, which could not be performed.

A bolt hole along the fracture line of the gusset plate.



The circle (above) and arrow (below) shows the location of the gusset plate that was believed to have failed and precipitated the collapse.



6. RELATION TO OTHER RADIO TELESCOPES

The Review Panel sees no direct implication from the failure of the 300 Foot Telescope to other radio telescopes. There were no phenomena observed in the operation of this telescope that could not be dealt with using modern design practice. It does point very dramatically to the importance of having an accurate stress analysis which would identify critical elements, crack propagation susceptibility, and required frequency of inspection, replacement, strengthening or repair.

The other, relatively minor, structural failures observed in the radial ribs and circumferential rings were detected through periodic inspection and repair. How these repairs contributed over the years to stress increases in the diamond truss gusset plates cannot readily or unambiguously be determined even with today's best state-of-the-art in stress analysis.

It should be understood by all telescope operators that their instruments are more akin to moving machines than to static buildings. Inspection and maintenance plans based on adequate knowledge of structural loads and service environment are a normal requirement of this or any other kind of machinery which the owners wish to keep in service.

It should be a requirement for the designer to identify from his design analysis, places and times where inspection for "fatigue type" progressive cracking should be made. Further, limited life components such as seals, hydraulic components and bearings should be identified and provision made for their inspection and/or replacement.

There are no unfavorable implications about the present ability of engineering science to design and build telescopes of this or larger size.

Edward Cohen, *Ammann & Whitney*
Robert M. Matyas, *consultant and Vice President Emeritus, Cornell University*
George F. Mechlin, *Vice President for Research and Development (ret), Westinghouse Electric Corp.*

View from the South, March 2002. The Control Building, with a new roof, is now the laser ranging lab for the GBT. The 300 Foot Telescope stood in the open field to the left of the building.

Part III

The 140 Foot Telescope

The 140 Foot radio telescope will be a precision instrument that will be used for the measurement of accurate positions of small radio sources, for making accurate maps of larger sources, for the determination of the radiative fluxes of many sources at different frequencies, for the study of variable sources of several kinds, for the measurement of the refraction and scintillation of radio waves, etc. Experience with other instruments has demonstrated the need for a fairly large and exceptionally stable telescope that will remain in use and retain its characteristics over intervals of many years. In optical astronomy the most accurate measurements of stellar coordinates have been obtained through the use of large transit instruments that have remained essentially unchanged for several decades.

It would be an exaggeration to suggest that the 140 Foot telescope will do for radio astronomy what modern meridian circles are doing for optical astronomy, but it should perform considerably better than Ulug-Beg's or Tycho Brahe's large visual quadrants. It would also be unrealistic to expect the 140 Foot telescope to compete in photometry with present-day photoelectric receivers. But we should certainly do no worse with radio radiation than photographic photometry is doing in the optical range.

....

Since the telescope is being built for the use of all competent American astronomers, the designers have attempted to make it as diversified as possible. The original plan for the 140 Foot Telescope contemplated its completion in the fall of 1960, but the contract with the E. W. Bliss Co. included the phrase "time is not of the essence," because all concerned with the project realized that some of the novel design specifications and procedures would have to be tested before they could be approved.... Nevertheless, we are acutely aware that in fact "time is of the essence.... "

O. Struve, R.M. Emberson, and J.W. Findlay
Publications of the Astronomical Society of the Pacific
December 1960, **72**, 439.

Groundbreaking for the 140 Foot Telecope August 14, 1958. Left to Right: Eugene Halik (AUI), unidentified (Bliss Co.), Jack Gilgallon (Bliss Co.), Frank Callender (NRAO), Richard Emberson (AUI).

But the Observatory does not yet fulfill its intended function of serving as a "national laboratory." This is due to several causes, the most important of which is the delay in completing the construction of the 140 Foot Telescope. This project is not in the hands of the director, which is an anomalous and, I believe, unprecedented situation in the field of astronomy.

O. Struve, 1961, Reports of Observatories,
Astronomical Journal, **66**, 465

Generally speaking, the facilities originally projected for the [radio astronomy] center went up on schedule. Construction of the most ambitious of them, however—a high-precision radio-wave detector with a solid-surface paraboloid reflector, 140 feet in diameter— encountered so many snags that 4 years after work on the instrument began [NSF Director Alan T.] Waterman was expressing the fear that it might not be "possible of assembly."

from *"A Minor Miracle"* by Milton Lomask
An Informal History of the National Science Foundation
Washington, D.C., 1976

1 The First Ten Years, 1955–1965*

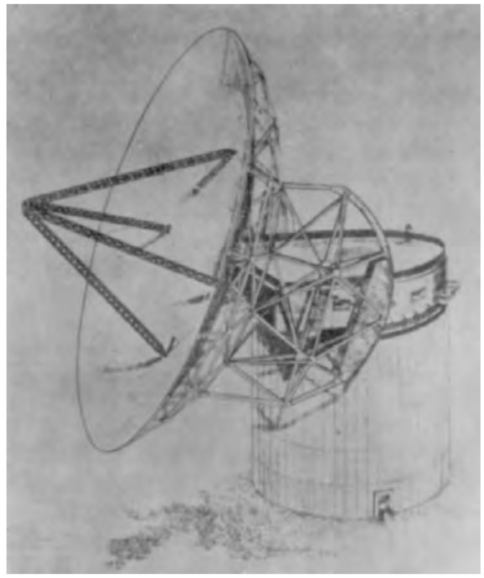
David S. Heeschen
NRAO
Charlottesville, VA

The origin of the 140 Foot Telescope dates back to the origin of NRAO itself. Several East Coast scientists, especially Bart Bok and Donald Menzel from Harvard and John Hagen from the Naval Research Lab (NRL), began talking in late 1953 and early 1954 about the possibility of some sort of cooperative effort in radio astronomy. They approached Associated Universities, Inc. (AUI), with the idea and in response AUI sponsored a meeting in New York City on May 20, 1954 to discuss it. The meeting was attended by most of the people then active in radio astronomy in the US, together with other interested astronomers, engineers and physicists. The consensus at the meeting was that AUI should conduct a feasibility study for a radio observatory that would provide radio telescopes for use by any qualified scientist.

AUI set up a steering committee, chaired by John Hagen, to help guide the study, and submitted a proposal to the National Science Foundation (NSF) for funds. The committee was comprised of B. J. Bok (Harvard), A. J. Deutsch (CalTech), L. Goldberg (Univ. of Mich.), W. E. Gordon (Cornell Univ.), J. P. Hagen (NRL), J. D. Kraus (Ohio State Univ.), A. B. Meinel (Univ. of Chicago), M. A. Tuve (Carnegie Inst. of Washington), and J. B. Weisner (MIT). Lloyd Berkner, president of AUI, had overall responsibility for the study, and R. M. Emberson, assistant secretary of AUI, was the study manager. An NSF Advisory Panel on Radio Astronomy, chaired by Merle Tuve, also had a lot of influence on the study. Although there was considerable overlap between the two groups (Tuve, Bok, Hagen and Kraus served on both) they were often at odds.

The Steering Committee held its first meeting on March 26, 1955. From the earliest discussions in 1953–54, interest centered on a large steerable paraboloid, and diameters of up to 2500 feet were discussed. At this meeting the committee agreed that "size had to be balanced against cost," and decided that 600 feet was the largest practicable diameter to consider at that time. After the meeting AUI commissioned Jacob Feld, a consulting engineer from New York City, to undertake a 600 foot design study. Feld completed his study by early July, after two months of effort. The Steering Committee already recognized that acquiring a 600 foot would be a long process, and that it would be desirable for the new observatory to get into operation as quickly as possible with a smaller, more readily available instrument. They therefore decided that a telescope of about 150 foot aperture should be obtained as the first instrument. It was suggested that existing designs

* Presented at the 140 Foot Birthday Symposium, September 1995. Illustrations have been added by the editors.



The Kennedy proposal for the 140 Foot Telescope.

in the 50 foot to 80 foot size range could be readily extrapolated up to 150 feet and still be essentially "off-the-shelf."

In 1955, the primary research objective for a large parabola was the study of the distribution of hydrogen in our own and other galaxies. The 21cm line of HI had been detected in 1951 and immediately sparked a large effort to determine the spiral structure of the Galaxy, in greater and greater detail. Shortly thereafter HI was detected in the Magellanic Clouds, opening the door to studying HI in other galaxies as well. The first work was done with parabolas of 25–30 ft diameter, but everyone wanted a bigger dish, and instrumental developments came fast. By 1955 a second generation of 60–85 ft diameter telescopes, designed specifically for 21cm line work, was getting into operation. A third generation, 150 foot diameter telescope, should be an easy next step and would greatly increase the power of 21cm line research.

It was also recognized that other spectral lines might be observable with larger telescopes. Unsuccessful searches had already been made for lines of deuterium at 327 MHz and OH at 1668 MHz. The 150 foot, especially if it were good to shorter wavelengths, would bring new power to such searches. Another rapidly developing area of interest was in the study of thermal emission from HII regions. These had recently been detected by Fred Haddock, using the 50 foot dish at NRL. It was also expected that thermal emission would be detectable from the nearer planets: Venus, Mars, Jupiter and Saturn. The study of thermal emission from HII regions and planets was considered the main reason for pushing the short wavelength limit of the 150 foot beyond 21 cm to 10 cm or shorter.

In May 1955 AUI requested proposals for the design and construction of a 40 meter diameter telescope that would operate to about 10 cm wavelength. Quotes

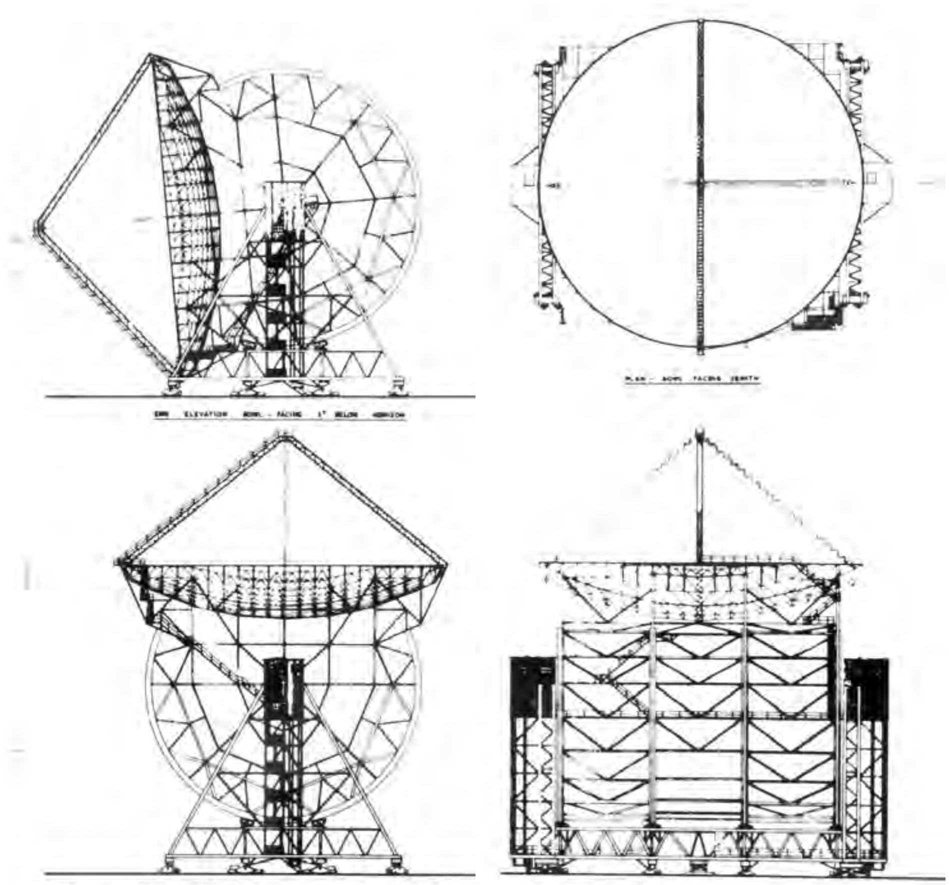


Figure IV-4. Four Drawings by Husband & Company of Sheffield, England for the proposed 140-foot Altazimuth Telescope Design. The two end elevations show the reflector pointed to the Zenith and Horizon. The front elevation again shows the reflector pointing to the Zenith. The Plan View shows the quadrapole support for the antenna feed.

were requested for both alt-azimuth and equatorial mounts. AUI hoped to obtain a "turnkey" contract, that is a fixed price contract from a single firm or group of firms to design, fabricate and erect a telescope that would meet the stated performance requirements. Prospective bidders were given about one month to submit their proposals. Twenty companies responded, most with expressions of interest or a proposal to do part of the job, but five companies offered to do the entire job. It was concluded however that none of the proposals would produce a telescope of the desired performance, and that a different approach was needed.

At its meeting in July 1955 the Steering Committee decided to follow a two-step procedure suggested by J. C. Husband. Husband was the designer of the Jodrell Bank 250 foot telescope which was under construction at that time, and had been brought in as a consultant to AUI. Step one would be to develop a detailed design. Step two would be to contract for the fabrication and erection of the telescope, to the design produced in step one. The D. S. Kennedy Co. (builders of the Harvard 60 foot radio telescope and various military antennas up to 84 ft in aperture), Husband, and Jacob Feld, were each asked to develop a design for an alt-azimuth mounted 140 Foot telescope. The diameter of the telescope had gone from "about 150 feet" in March, to 40 meters in May, to 140 feet in June, reflecting the tentative nature of the early discussions. The last change, from 40 meters to 140 feet, came about because an unidentified government official decided that the size of a U.S. telescope should be expressed in feet. In making the conversion the diameter was rounded upward to 140 feet. Partly on the advice of engineers that an equatorial mount would be more difficult and 25%–50% more costly, and partly because AUI wanted the designs and subsequently the telescope itself to serve as a possible prototype for a much larger radio telescope, it was decided to concentrate on alt-azimuth mounted designs. While there were differences of opinion about the feasibility of an equatorial mount for a 140 ft, everyone was agreed that larger telescopes would have to be alt-azimuth mounted.

The decision to drop consideration of an equatorially mounted telescope did not sit well with some members of the Steering Committee. Merle Tuve, in particular, was very much in favor of an equatorial mount. At its December 1955 meeting the committee recommended that a study be made of the feasibility of an equatorial mount, possibly with restricted sky coverage. Following this meeting AUI's engineering consultants did a quick study and concluded that the extra structural cost of an equatorial telescope would outweigh the savings on the drive and control system. But nothing more was done until further prodding was received from the NSF. On March 22, 1956, R. J. Seeger, assistant director of the NSF, wrote Berkner and requested that AUI develop an equatorial design and cost estimates from "at least one contractor like Kennedy." This request resulted from the advice of the NSF Advisory Panel on Radio Astronomy, which had met in January. This was one of several occasions where Tuve used his NSF Advisory Panel chairmanship to modify decisions of the Steering Committee. As a result of the NSF request AUI contracted with Jacob Feld to develop an equatorial design with limited sky coverage. Husband also volunteered to produce an equatorial design and D. S. Kennedy Co. offered to scale up its 60 foot equatorial design.

The Steering Committee reviewed all of the designs in October, and did not like any of them. Therefore, in late October N. L. Ashton was asked to develop an equatorial design for the 140 foot telescope, with the "advice and assistance" of an ad hoc committee. Ashton was an engineer from the University of Iowa who had designed the NRL 50 foot antenna. His original involvement with AUI was as a consultant to review the three alt-azimuth designs. The ad hoc committee was chaired by T. C. Kavanagh, a consulting engineer with the firm of Praeger-Kavanagh, and included J. G. Bolton, F. T. Haddock, E. F. McClain and H. E. Tatel among its 12 members.

In August 1956, AUI submitted to the NSF its "Plan for a Radio Astronomy Observatory," describing the results of the feasibility study. This document became the basis for the first contract between NSF and AUI for the establishment and operation of the NRAO, signed in November, 1956. The document describes a program that would include a 28 ft telescope, two 60 foot telescopes, and the 140 Foot telescope in the initial construction. The three alt-azimuth 140 ft designs were described in some detail but the report did not recommend a particular design or

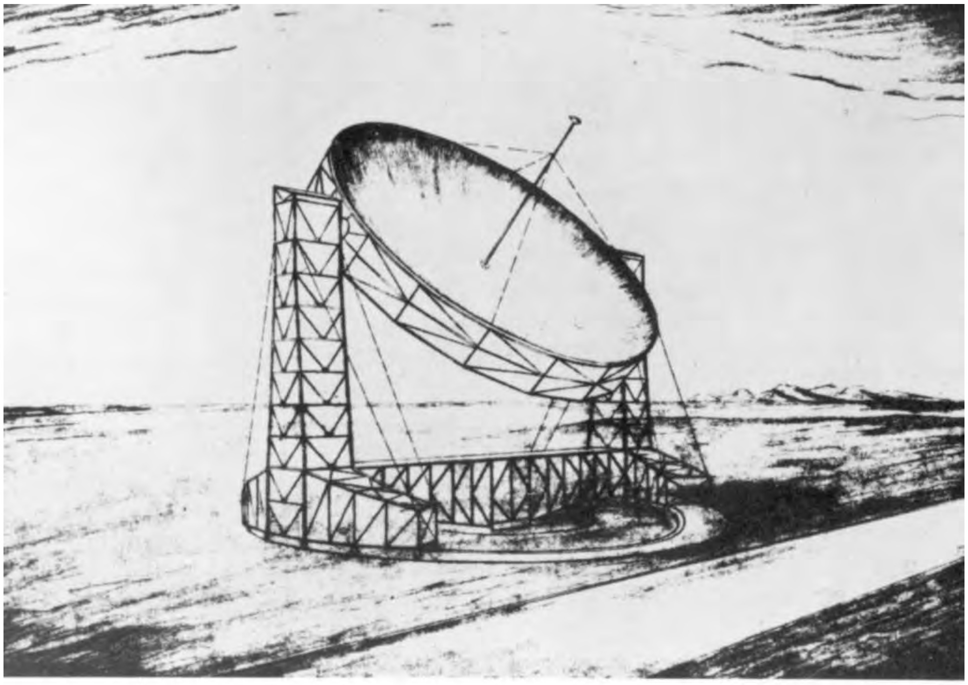
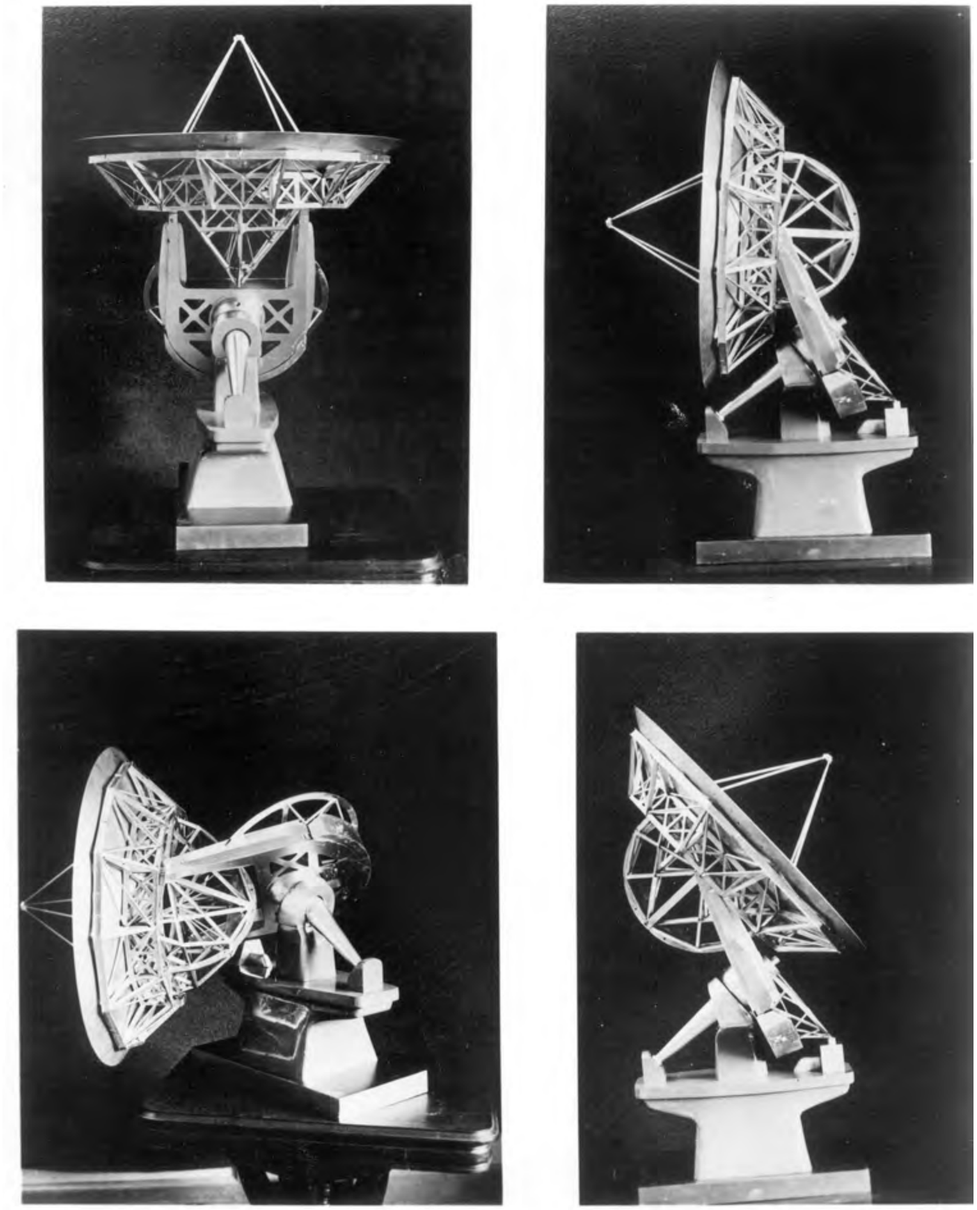


Figure IV-1. General View of the 140-foot Altazimuth Radio Telescope Design Proposed by Dr. Jacob Feld of New York City.

even type of mount. The 600 foot design by Feld was also described at some length.

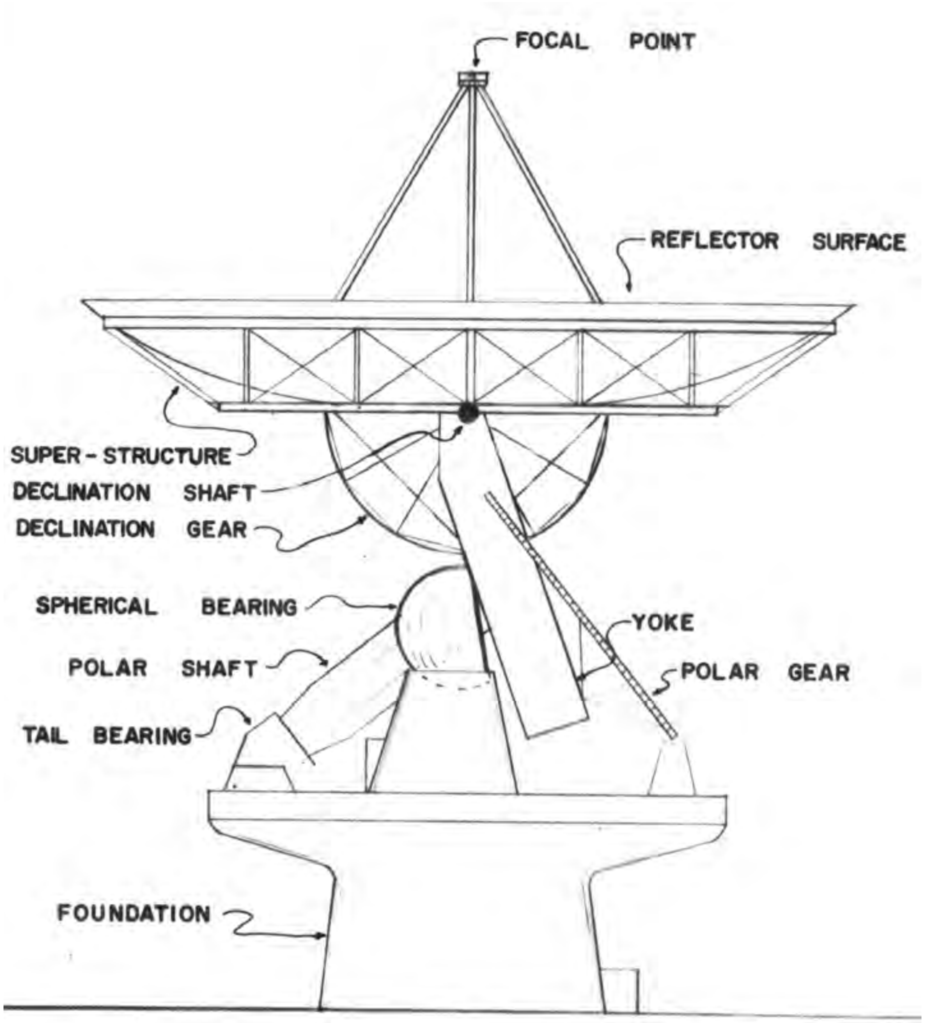
The question of alt-azimuth vs equatorial mount was finally settled at a meeting of the ad hoc committee in January 1957. After reviewing all the arguments the committee decided to table all alt-azimuth designs and concentrate on the Ashton equatorial design. The committee's decision was based on its belief that both types of mount were technically feasible, that the equatorial design could be built within the budget, and that the drive and control system would be less complex and more reliable for the equatorial mount. In particular, it was assumed that tracking or scanning in hour angle (about the only observing modes really envisioned at that time) could be accomplished by a simple open loop drive about the polar axis only with an equatorial mount, while the alt-azimuth mount would require analog or digital coordinate conversion, and variable speed motion, possibly servo'd, in both coordinates. The coordinate converter was considered a very difficult problem. Two other perceived disadvantages of an alt-azimuth mount were also cited: the 'zone of avoidance' around the zenith, and the rotation of the focal plane with hour angle.

The question of sky coverage was also finally settled at this meeting. Several members of the various committees, especially Bolton, Tatel and Tuve, had long favored limited sky coverage, of perhaps ±4–5 hours in hour angle, on the grounds that it made an equatorial mount structurally simpler, more rigid and less expensive,



The Ashton design for the 140 Foot Telescope.

and indeed earlier specs had called for ±5–6 hours of sky cover. However at this meeting it was argued by one member of the committee that it was important to be able to observe the Sun at any time it was above the horizon. This requires ±8 hours of hour angle coverage at the latitude of Green Bank. Ashton assured the group that the increased sky coverage would not require any significant change to the design he was developing. Somewhat surprisingly no one challenged Ashton's assertion and the larger coverage was accepted. In fact, the increased sky cover must have



A drawing of the Ashton design for the 140 Foot Telescope.

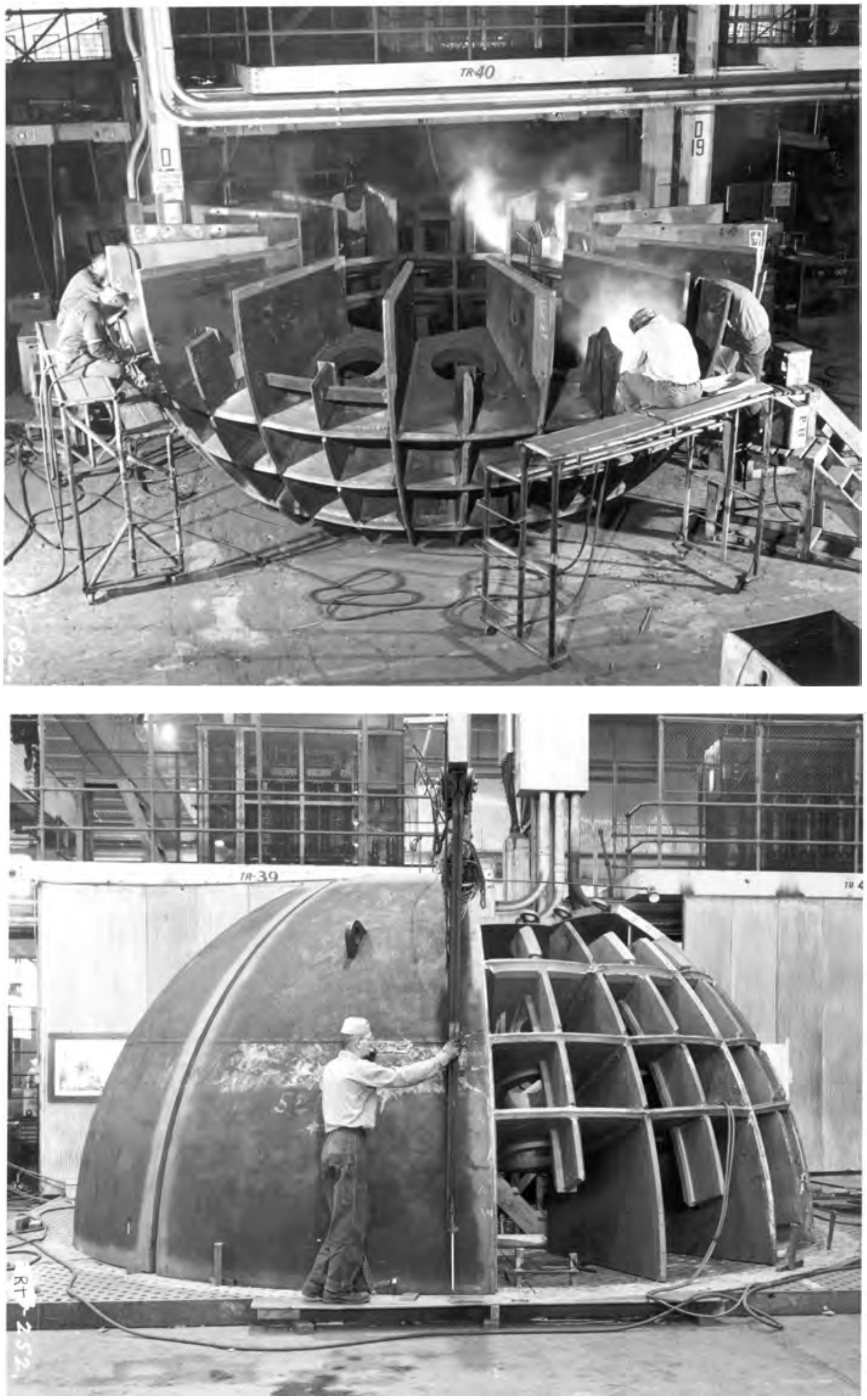
had a very significant effect on the design, although Ashton never acknowledged this. It required longer yoke arms, which in turn made the structure less rigid. Maintaining the surface deflection became more difficult. Longer yoke arms required more counterweight which in turn meant more weight on the polar bearing. So the decision had a ripple effect throughout the design.

Ashton continued his design work throughout 1957. His design consisted of a welded steel polar axis and yoke assembly, mounted on a concrete foundation and supporting an aluminum reflector. The transition from steel to aluminum occurred at the declination axis. The polar axis was comprised of a 65 ft shaft with hydrostatic bearings at each end. The lower tail bearing was cylindrical, with four oil pads. The upper bearing was a hemisphere 22 ft in diameter, supported on nine oil pads. A later redesign reduced the number of pads to four, and still later the diameter was reduced to $17\frac{1}{2}$ feet. The entire moving weight of the telescope, about 2600 tons, would be carried on the spherical bearing. A yoke structure perpendicular to the polar axis was attached to the upper side of the bearing. The bottom of the yoke carried counterweighting and provided support for a large diameter polar drive gear.

On Aug 1 AUI issued a request for fixed price proposals for the construction of the 140 Foot, to Ashton's design. About 40 proposals were received, eight of which were for a fixed price contract for the entire job. Bid prices ranged from \$3.96M to \$12.02M. Even the lowest bid was almost a factor of two higher than the budget and the most recent estimates by AUI's expert engineer-consultants. The wide range in bid prices should have been recognized as a strong warning that something was wrong somewhere, but it was not. Five proposers were eliminated on various grounds. AUI then held meetings with each of the remaining four separately, which resulted in a reduced price from each of them. After examining the new bids, AUI selected the E. W. Bliss company of Canton, Ohio. Bliss's reduced price was \$4.75M, down from its original bid of \$5.77M. There followed a lengthy series of negotiations with Bliss about details of the job and the contract, which culminated in a contract finally being signed on June 9, 1958. It called for completion within 24 months, at a price of \$4.75M, and did not provide any incentive or penalties for early or late completion. In fact, the telescope was not completed until the summer of 1965, and the final cost was about \$13.5M. What went wrong?

First, the telescope which had started as a simple extrapolation of existing designs to a somewhat larger size had, in the course of its development via many committees and consultants, gradually and almost imperceptibly become a precision instrument that challenged the technical capabilities of the time. This was probably the underlying cause of many of the subsequent difficulties. But there were also two huge technical problems with the Ashton design and implementation that were not recognized until much later. And finally, solution of the technical problems was made more difficult by the character of the Bliss Co. and the nature of the contract between AUI and Bliss.

The first of the technical problems was that the Ashton design proved to be almost impossible to actually build. At the time the contract was signed apparently neither Ashton nor Bliss had given much thought to the shop and field procedures that might be used to fabricate and erect the telescope. The full extent of the problem wasn't realized until much later, but Bliss experienced difficulties from the beginning in completing details of the Ashton design and in fabricating the sphere. Some of these problems will be described later. By the spring of 1960, when the telescope should have been nearing completion according to the original schedule, it was more than two years behind. Fabrication of the spherical bearing, polar shaft and yoke arms was then in progress at the Bliss plant. In welding the sphere many small cracks had developed in the steel. Investigating these cracks led to recognition of the other major problem. Because of the nature of the steel used in the spherical bearing, minute cracks could, under certain conditions, propagate rapidly throughout the structure and cause catastrophic failure of the bearing. This behavior is known as "brittle fracture," and the necessary conditions are low temperature and localized triaxial stresses. More than 70 small cracks were found in the sphere, caused by the welding process, which also created triaxial stresses in the steel. And winter temperatures in Green Bank are low. So all the conditions for



Early stages of fabrication of the Ashton-design 22 foot diameter welded spherical bearing at E. W. Bliss. *[Bliss photos courtesy M. Popovich]* "brittle fracture" existed, and metallurgists consulted by Bliss and independently by AUI all agreed that there was a small but finite chance that the sphere could fail catastrophically. Failure of the sphere would probably result in loss of the entire telescope. The probability of brittle fracture occurring was recognized to be very small but no one could say just how small. Although this brittle fracture problem first arose with the sphere, it also existed for the shaft and yoke. Both of these were welded structures, made of the same type of steel as the sphere. The reason that a better steel was not originally specified was that the problem of brittle fracture was not widely understood in 1957-58, and Bliss asked to use the type of steel it was used to working with and had a supply of on hand. However, brittle fracture had been known about at least since WWII, when several Liberty ships broke in two while still under construction because of brittle fracture. By 1957 at least one book and several articles had been written about brittle fracture. I doubt that much new was learned about the subject between the summer of 1958 when the "bad" steel was selected and spring of 1960 when the problem surfaced. It was unfortunate that neither Ashton nor any of the consulting engineers was aware of brittle fracture.

There appeared to be three basic solutions to the problem:

1. *Change Nothing.* Continue with the Ashton design as it stood, use the existing shaft, sphere and yoke, and accept the small risk of failure. This option would cost no extra time or money. Several ways of further reducing the risk of brittle fracture were considered under this option. One was to drill small "mouse holes" through the steel at the ends of each crack, to inhibit their propagation. Another might be to repair the cracks, by welding or perhaps with epoxy. A third would be to enclose and heat the structure during the winter, to keep it above the critical temperature.

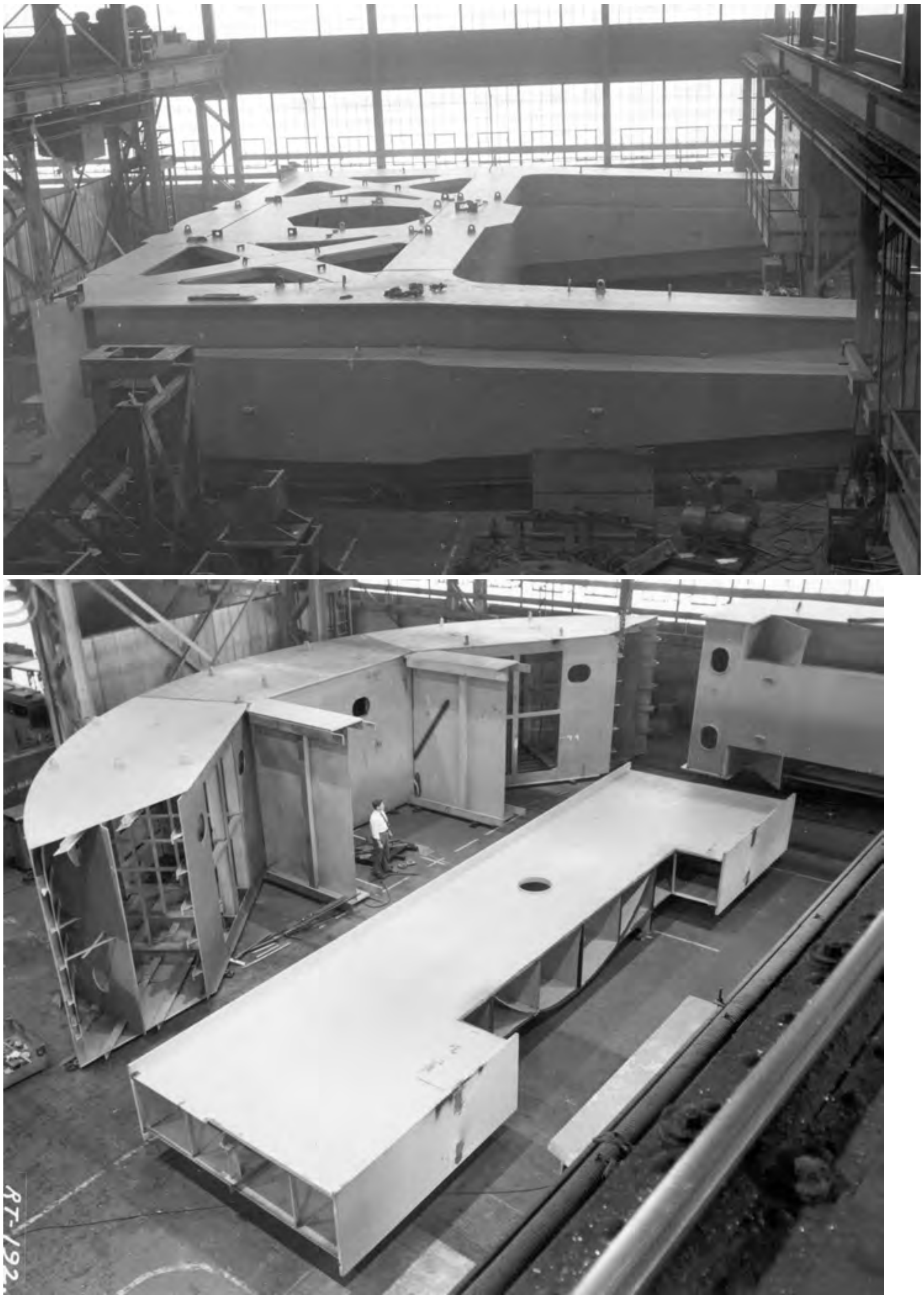
2. *"Normalize"* the sphere by heating it to > 1600 F and thereby relieving the triaxial stresses. Heating the sphere to such a temperature ran a small risk of destroying it and a much larger risk of causing distortions that would have to be repaired somehow. Furthermore, subsequent welding of the shaft and yoke to the sphere would cause new cracks, stresses and distortion. Suggested solutions to the latter problem included bolting the structure together rather than welding it, which would require major changes to the existing structures and was considered by Ashton to be unfeasible, or cut off segments of the shaft and yoke and weld them to the sphere before normalization. Since the sphere was already 22 feet in diameter this would make shipping the piece to Green Bank almost impossibly difficult.

3. *Start Over.* Throw away the existing sphere, shaft and yoke and make new ones out of a type of steel that was much less susceptible to brittle fracture.

The third option, "Start Over," was initially not seriously considered by anyone, because it was the most costly and time consuming of the three. The other two—"Change nothing," and "Normalize"—both involved risks that were extremely difficult to assess. They, and various perturbations of them, were hotly debated for the next six months or so. Ashton very strongly favored the "Change nothing" option. He wanted to proceed with his design and the existing components just as they were. He claimed that the risk of brittle fracture was negligible and that the safety factor built into the sphere was so great that there was simply no chance that it would ever fail. Bliss and Emberson were just as strongly in favor of "normalization," while D. Gurinsky, a Brookhaven metallurgist who was brought in as a consultant to AUI, didn't think normalization was necessary, but favored it anyway.

It was a complex issue, further complicated by some other factors. First, all attempts to find a solution by "changing nothing" or "normalizing" ran up against the other major problem with the Ashton design. That design required welding the polar shaft and the yoke to the sphere, but the procedure for doing this had proven very difficult to develop and neither Ashton nor Bliss had yet come up with a satisfactory one. Some engineers now wanted to bolt the pieces together, particularly if they were to be "normalized" first. That would require some redesign and modification to the already fabricated shaft and yoke. Ashton was vehemently opposed to bolting, claiming it could not be made strong enough. Second, the individual components were already very large and heavy, and shipping them to Green Bank was difficult and costly. If any of them were made larger, for example by welding a section of the shaft to the sphere, the shipping problem would be still further exacerbated. Third, the project was already way over budget and behind schedule and there was considerable pressure to find as quick and inexpensive a solution as possible. Fourth, it was by this time clear to AUI that Bliss was a reluctant and perhaps incompetent contractor, and AUI had begun internal discussions about possibly terminating the Bliss contract. And lastly, a question of "design responsibility" had arisen and was a major issue. Bliss claimed the brittle fracture problem was caused by the design (for which Bliss accepted no responsibility) and the grade of steel called for in the contract. AUI claimed the problem was caused at least in part by Bliss's decision to not follow the fabrication procedures for the sphere and shaft that had been envisioned by Ashton, but instead adopted their own (which according to contract provisions would have had to be approved by AUI, and presumably were, prior to their being adopted). Bliss flatly refused to do anything more to the sphere, or even ship it out of their plant at Canton, until they were formally relieved of any "design responsibility." It was particularly unfortunate that this contractual question was so intimately intertwined with the technical ones. Both issues might have been more expeditiously settled if they could have been separated, but neither side was willing to do this. The lawyers for the two sides were unable to even agree on exactly what was meant by "design responsibility."

The disagreement over "design responsibility" had a murky history. The original request for proposals, back in August 1957, included a concept of joint responsibility. The successful contractor would develop shop drawings, fabrication and erection procedures, etc., based on the Ashton design. When these were accepted by AUI the contractor would be responsible only for materials and workmanship, while AUI would be responsible for the overall design and telescope performance. Given a finished, acceptable, design this appears to be a very reasonable way to proceed. But Ashton's design was not finished. During the lengthy negotiation with Bliss over contract terms the concept of joint responsibility was discarded and Bliss accepted, or seemed to accept, responsibility for design as well as for fabrication and erection. Apparently AUI and Bliss both assumed that the Ashton design was sufficiently complete that only detailing and shop drawings were needed in most areas, and where further design was needed it would present no problems. The actual wording of the contract is wonderfully ambiguous as it relates to responsibility. One reasonable interpretation of the wording is that Bliss had full responsibility for



The first yoke made by Bliss Co. The sections shown on the bottom would attach to the left-most side of the piece shown at the top. This yoke was discarded after Stone and Webster took over the project. [Bliss photos courtesy M. Popovich] meeting the performance requirements, using the Ashton design with minor modifications or additions. AUI, however, would retain the authority to approve, or disapprove, every detail. In effect, Bliss had full responsibility but no authority. AUI later took the position that the Ashton design was, in effect, just an example that Bliss was free to use or not use, but the contract doesn't make this clear and in essence really assumes that the Ashton design would be used. In retrospect, it's almost impossible to understand why Bliss, or even AUI, would agree to such a contract since it was an open invitation to all sorts of misunderstanding and trouble. It is equally difficult to understand how all the parties involved could so misjudge the degree of completion of the Ashton design, and the extent of the unresolved problems.

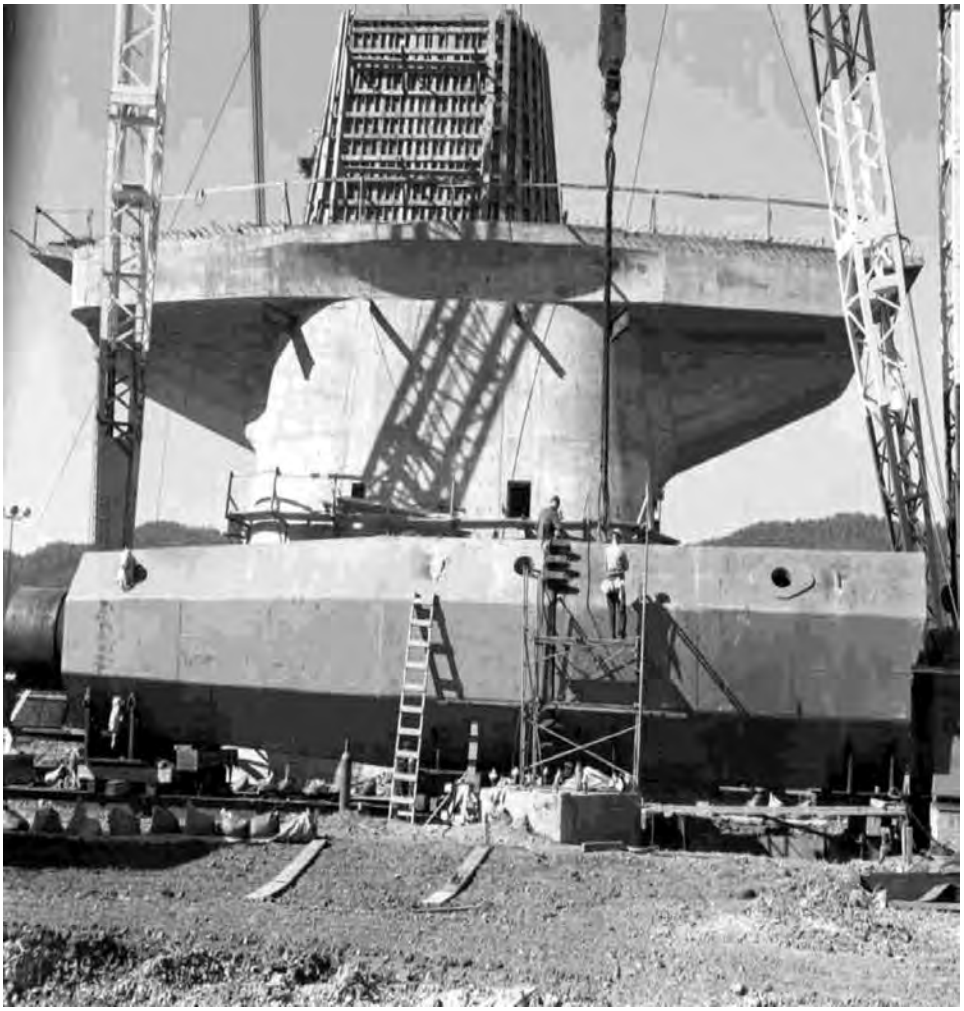
Arguments, sometimes heated, within AUI/NRAO and between AUI and Bliss, went on over brittle fracture and "design responsibility" for several more months. Finally, on August 30, 1960, AUI, NRAO and NSF personnel met at the NSF and after a lengthy review of the situation it was agreed that the "Change Nothing" option would be adopted. Ashton's design and the existing components would be used, without "normalization." This option was pushed very hard by Otto Struve, director of NRAO, and was adopted over the strong objections of Emberson. Berkner was in London at the time of the meeting. He was apprised of, and confirmed, the decision via cablegrams on September 2.

Before this decision could be implemented AUI and NSF representatives met again, in late September, probably because of Emberson's continued strong concern about the possibility of brittle fracture. Following this meeting Alan Waterman, director of NSF, appointed an ad hoc committee, chaired by Auguste Kinzel, to study and make recommendations on the problem of the sphere and shaft.

The Kinzel Committee issued its report in January 1961 and recommended that the existing sphere and shaft be normalized and then bolted together. AUI accepted this recommendation, but Emberson and Ashton were not happy with it. They were particularly concerned about the proposed bolted connection between the shaft and sphere. They therefore produced an alternative design which would require a new sphere and a new shaft, or portion of shaft, that would connect to the sphere. The sphere and shaft would be bolted together, as would the sphere and yoke. The sphere, shaft and yoke would be specifically designed for bolted connections, thus avoiding the problems they foresaw in adapting the existing pieces to bolted connections. The new components would also, of course, be fabricated from steel that was not susceptible to brittle fracture. Although their plan would require discarding most of the steel components already fabricated, it would solve the brittle fracture problem. After considerable discussion among all the concerned parties, AUI, NSF, and Kinzel, it was agreed that both designs would be developed.

In early April, 1961, the Stone & Webster Engineering Co. was brought in by AUI to provide engineering and management services for the 140 Foot. Among other things, Stone & Webster would act as AUI's agent in all dealings with Bliss and other contractors.

As a condition of their taking the job Stone & Webster insisted on a thorough review of the design and of the status of all work in progress. Following that review, which was essentially completed in October, 1961, Stone & Webster rejected the



The E W. Bliss polar shaft being welded on site in Green Bank, 1959.

Kinzel Committee recommendations and proposed instead to adopt the alternative design concept developed by AUI, namely a shaft, yoke and sphere fabricated of steel not susceptible to brittle fracture, and bolted together. In redesigning these components Stone & Webster discarded the Ashton designs, citing various technical deficiencies, and developed their own. They also discarded most of the other Ashton/Bliss designs and existing components, mostly on technical grounds, but in some cases because of the threat of brittle fracture in the steel that had been used.

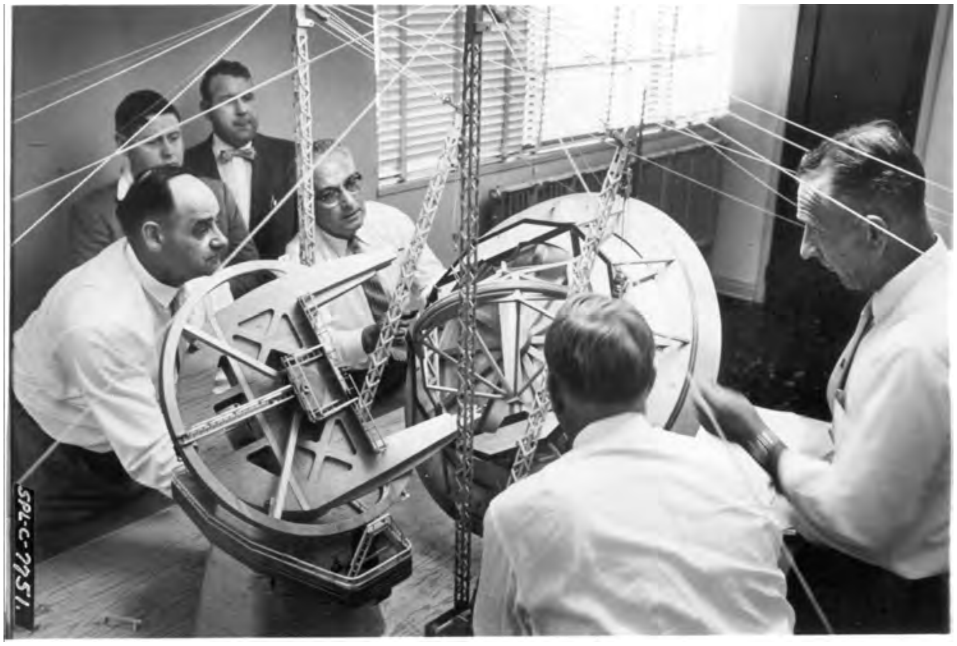
When the smoke finally cleared on the Stone & Webster review, about all that remained of the original Ashton design was the basic concept: an equatorial telescope with a steel support structure and an aluminum surface and backup structure, all resting on a hydrostatic spherical bearing and driven through large gears and pinions. The concrete foundation and aluminum backup structure (which was essentially complete) were retained, but almost all other fabricated components were discarded.

AUI and NSF accepted all of the Stone & Webster recommendations. The recommendations were received at Green Bank with a sense of relief that finally some positive action was being taken to resolve the controversies that by that time had been raging for more than 18 months. Struve and the staff at NRAO had often disagreed with Emberson and the AUI staff on the course that should be taken with the 140 Foot. But the principal complaints of the Green Bank staff were that it was not allowed to have any real say in the management of the project and, even more important, AUI was not making decisions promptly and had not acted with force and leadership in trying to solve the problems. The staff at NRAO felt that AUI was excessively slow in making necessary technical and management decisions, that AUI relied too much on consultants and committees and their recommendations were adopted without sufficient internal screening (this criticism dates back to the very early days of the project, pre-NRAO), and that AUI's engineers were not particularly competent. We all believed that the project should be run from NRAO, by Struve and a staff reporting to Struve. In the NRAO annual report for 1960–61 (AJ 66, 465, 1961) Struve wrote: "But the Observatory does not yet fulfill its intended function of serving as a 'national laboratory.' This is due to several causes, the most important of which is the delay in completing the 140 Foot telescope. This project is not in the hands of the director, which is an anomalous and, I believe, unprecedented situation in the field of astronomy." There was a real schism between AUI and NRAO, and a feeling of despair at Green Bank. Hiring Stone & Webster was the beginning of the end of this schism. But AUI still did not relinquish control of the project to Struve, and because of this and his deteriorating health he resigned in November 1961.

Ordinarily AUI does not directly manage development or construction projects. But the 140 Foot project began before NRAO came into existence and was necessarily managed initially by AUI as part of the feasibility study. When Struve became director, in 1958, he and Berkner agreed that AUI would continue its management of the 140 Foot and turn the telescope over to NRAO as a finished product. Since everything appeared to be going well at that time under the existing arrangement it seemed sensible not to change it. However, when things began to go bad he believed he should have been more involved.

Otto Struve had great impact on NRAO, and on me personally, during his two year tenure as director. His last year or so at Green Bank was quite unhappy, in part because of his health and in part because of the 140 Foot situation. I have always felt badly about that. He was a great scientist and a fine person and he deserved better in the twilight of his career.

Following Struve's resignation AUI made a number of changes in the project organization, which his criticism probably helped bring about, and of which I'm sure he would have approved. Max Small was named construction manager, the contract with Bliss was terminated, thus finally ending the long controversy over "design responsibility," and NRAO's role was gradually increased. So at the end of 1961 the 140 Foot project was finally back on track, though it would be another $4\frac{1}{2}$ years before it was completed. It was almost as though the project were started afresh, with only the foundation, the aluminum superstructure, and a few odds and



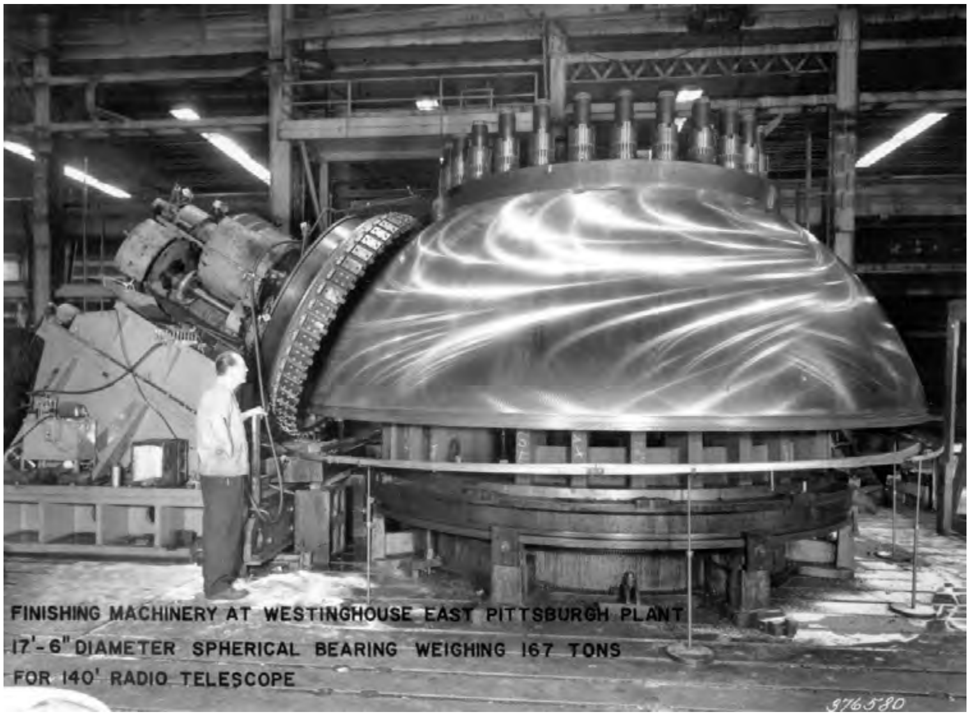
E. W. Bliss company engineers developing procedures for the superstructure lift.
[Bliss photo courtesy M. Popovich]

ends left over from the previous effort. The design was new, the contractors would be new, and even most of the AUI personnel were new. AUI would now act as its own prime contractor, with Stone & Webster providing design and engineering, and construction supervision. From this point forward the project moved smoothly, if slowly, to a successful conclusion, with only the normal problems common to a large complex construction job. Stone & Webster invariably adopted the more conservative solutions to the myriad normal problems that arose. At NRAO we often felt this approach was overly costly and/or time consuming. Given the past history of the project and the nature of Stone & Webster's involvement, their conservative approach was understandable however, and probably the better way to proceed.

Ashton's concept of the polar axis had consisted of a spherical bearing 22 feet in diameter, to which the polar shaft and a hub section of the yoke were welded. All three components were to be welded structures, fabricated in the shop, shipped to Green Bank, lifted onto the foundation and then field welded to each other. Each of the components was so large and heavy that transportation to Green Bank and lifting them into place on the foundation were major problems. The 22 ft. diameter sphere would be made of an internal web structure with "orange peel" surface pieces welded to the web and to each other. After fabrication the sphere would be machined to the required tolerances: sphericity and concentricity $< .003$ inch, and local mirror finish $< .005$ inch. Both shop welding of the sphere and field welding of the polar shaft and yoke hub to the sphere would have to be done in such a way as to allow these tolerances to be achieved.

Testing a mock-up of the $17\frac{1}{2}$ foot spherical bearing for clearance in the rail tunnel through Droop mountain in 1961. The size of the bearing allowed for three inches of clearance.

Bliss encountered many problems in fabricating the sphere. The internal webbing caused “hard spots” on the surface and these together with the welding procedure and sequences used made it very difficult to control distortions. And it was in the internal webbing that more than 70 small cracks appeared during the course of the welding. Bliss also had trouble designing the joints between shaft and sphere and yoke and sphere, and in developing procedures for field welding them. It was these problems that led to the long controversy and delay which culminated



The Stone & Webster design $17\frac{1}{2}$ foot diameter sphere being finished at the Westinghouse plant in Pittsburgh, Pa.

in discarding most of the Ashton design and most of the already fabricated parts, terminating Bliss and bringing in Stone & Webster.

In the Stone & Webster redesign, the spherical bearing became a casting $17\frac{1}{2}$ feet in diameter. The diameter chosen was the largest that could be shipped by rail to Bartow, the nearest railhead to Green Bank. The polar shaft consisted of a cast tail bearing welded to a cylindrical shaft of 6 inch steel plate which in turn was welded to a transition casting at its upper end. The transition casting was bolted to the sphere with twenty-eight 5 inch diameter bolts, pretensioned to 950,000 lbs. The hub section of the yoke is also a casting, bolted to the sphere with pre-tensioned bolts. By using steel not subject to brittle fracture and using castings and bolted joints, the problems of the earlier design were overcome. Field welding of yoke sections to its central hub was still required but did not involve maintenance of close tolerances. The spherical $17\frac{1}{2}$ foot casting was the largest ever poured, up to that time. It was poured by General Steel Industries and machined by Westinghouse. The 167 ton initial weight of the casting was reduced to 150 tons during machining.

Fabrication of the Stone & Webster redesigned telescope components began early in 1962, and was completed in mid 1963. The work was subcontracted to various firms, under the supervision of Stone & Webster. In addition to machining the sphere, Westinghouse fabricated the polar shaft and declination shaft. Sun Shipbuilding fabricated the yoke, and Bethlehem Steel cast and machined the tail bearing for the polar shaft. Heavy, spring actuated, brakes for the polar axis were fabricated by Goodyear Tire and Rubber Company. They are held open by hydraulic pressure, and are a major safety feature of the telescope. Gear segments for driving the telescope were fabricated by Philadelphia Gear Company. The polar drive gear consists of 28 separate segments on an arc of 42 ft radius. All of the components were designed by Stone & Webster. The reflector superstructure was the only major component of the Ashton-designed, Bliss-fabricated telescope that was retained. Surface panels were fabricated by the D. S. Kennedy Company. Electric Boat Company continued as subcontractor for the drive and control system. Pacific Crane & Rigging Co. replaced Darin & Armstrong for the field work in Green Bank, which got under way again in 1963. Stone & Webster managed all aspects of the job until 1963, when NRAO finally took over supervision, with Max Small as project manager.

A good description of this final phase of the 140 Foot project has been given by B. K. Malphrus* and by M. M. Small.**

The 140 Foot was finally completed in the summer of 1965, and a formal dedication was held in October. In spite of its checkered design and construction history, the telescope was an instant success from its first observations, and it has been a mainstay of U.S. radio astronomy for the past 30 years. The first observations with the 140 Foot, made in May 1965, before the telescope was actually completed, were of lunar occultations of 3C273 and other sources, by S. von Hoerner at 234 and 405 MHz. On July 9, 1965 Mezger and Höglund detected 6cm hydrogen recombination line emission (Sky & Tel. 30, 127, 1965). Recombination line emission, arising from transitions between high energy levels, 110 to 109 in the case of the Mezger-Höglund line, had been detected somewhat earlier by Russian radio astronomers, but their results had been marginal. The Mezger-Höglund result provided important confirmation of the earlier result and established recombination line observations as a new and important tool for studying the interstellar medium. These early observations, before the telescope had been thoroughly tested and calibrated, already demonstrated its power and versatility. A regular program of testing was begun in July. Some early results, at 11, 6, and 2cm wavelengths, were published by Baars and Mezger (Sky & Tel. vol. **31,** 7, 1966).

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* *History of Radio Astronomy and NRAO,* by Benjamin K. Malphrus, D.Ed. Thesis, West Virginia University, 1990; also published as book by Krieger, 1996.

** Sky and Telescope vol. **30,** 267, 1965

2 Chronology of the 140 Foot

A second reason for accelerating the construction program is that the scientific prestige of the nation is involved. Through NRAO and AUI, the United States is committed before the scientists of the world to complete this powerful instrument and bring it into use. Untoward delays will be acutely embarrassing to America, and indeed to all but her implacable rivals. The opportunity for discovery and leadership does not long endure in these times. When it is not grasped, there remains only to follow, as we have recently been all too often reminded.

A third reason is that, in a sense, the future development of NRAO is at stake in this project....

Advisory Committee on Radio Astronomy, Armin J. Deutsch, Chair
as quoted by O. Struve, R.M. Emberson, and J.W. Findlay
in *Publications of the Astronomical Society of the Pacific*
December 1960, **72**, 439.

The foundation of the 140 Foot telescope under construction in November 1958.

The pedestal building under construction in March and August of 1959 (left, right)
[Bliss photos courtesy M. Popovich]

An aerial view in January 1963.

One half of the Bliss polar shaft being delivered to Green Bank, in 1959. It was eventually abandoned and later buried at the Green Bank site. [Bliss photo courtesy M. Popovich.]

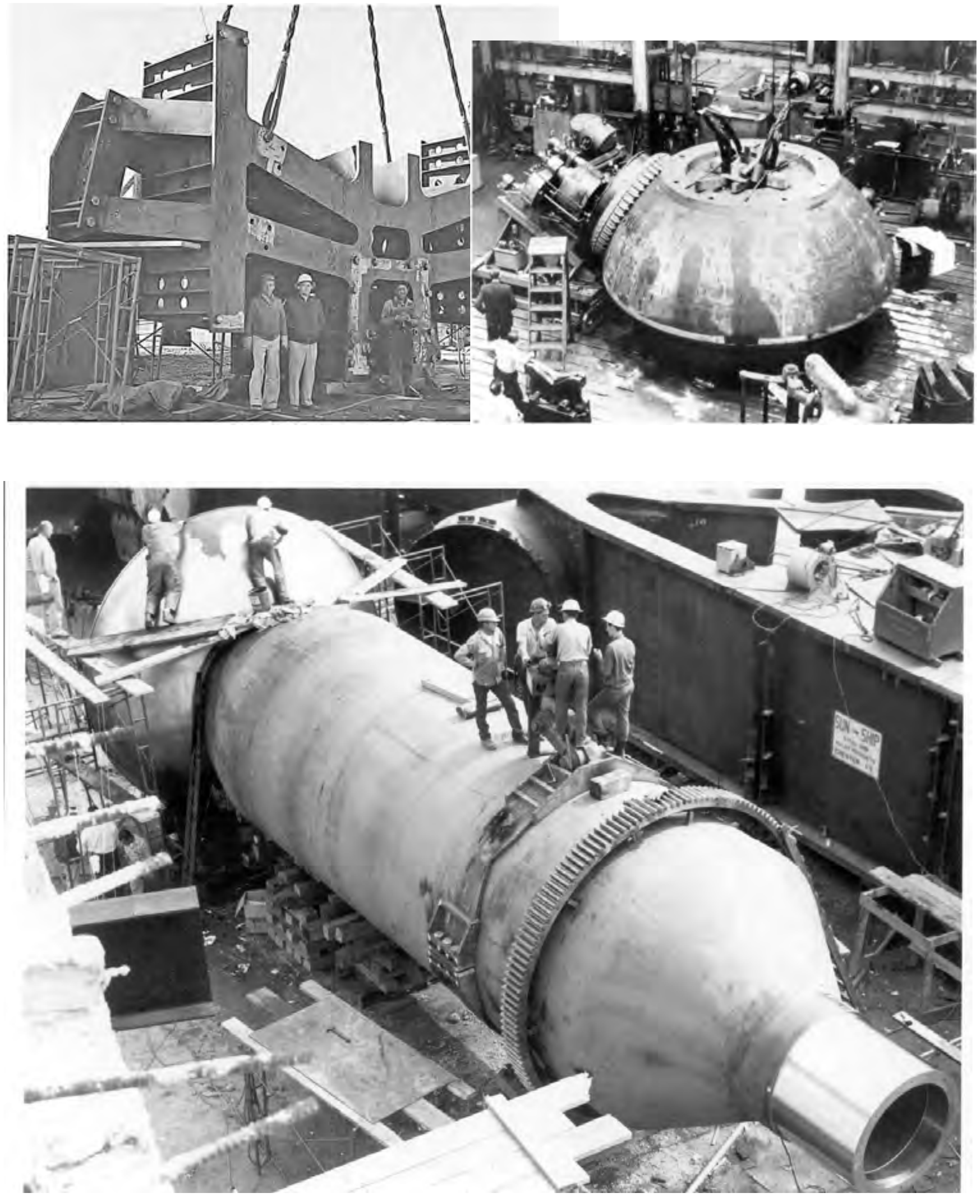
From an interview with Max Small, construction manager for the 140 Foot 1961-1965

Several components had to be re-planned and remade, notably the huge polar axis and the yoke. What became of the steel that Bliss had put into these elements?

"Sold for scrap," says Small, "Some of it came here"—meaning to Brookhaven, where it now serves as shielding for the accelerators used at the big nuclear science laboratory for the study of charged particles in motion.

in *"A Minor Miracle"* by Milton Lomask
An Informal History of the National Science Foundation

Top: Arrival of the final polar shaft in December 1963.
Bottom: The $17\frac{1}{2}$ foot diameter spherical bearing leaving General Steel Industries at Eddystone Pa., where it was cast in early 1963. With a weight of more than 350 tons, it was the largest nickel steel casting ever poured. Much of the material was cut away in subsequent machining. A special railroad car was cast in a single piece to transport it from the foundry to the Westinghouse plant at East Pittsburgh where it was machined.



Top left: The weldment which supports the weight of the spherical bearing and all moving parts of the telescope.
Top right: The spherical bearing being machined at Westinghouse in Pittsburgh Pa. to a near-mirror finish and a final weight of about 150 tons.
Bottom: The bearing (at upper left) and polar shaft after being bolted together at Green Bank.

Top: The aluminum backup structure in 1962. Behind it can be seen the Calibration Horn, the 40 Foot Telescope and the 85 Foot Tatel Telescope.
Bottom: The yoke being assembled on the ground, October 1963.

Top: Lift of the polar shaft and spherical bearing (covered with protective material) into place atop the pedestal.
Bottom: Lift of the first section of the yoke, a weight of almost 500 tons.

Lift of the final yoke section with a connecting ballast tank.

Top: The backup structure nearing completion on the ground in 1963.
Bottom: Preparations for the lift into place on the yoke in November 1964. Film-maker Peter B. Good documents the procedure.

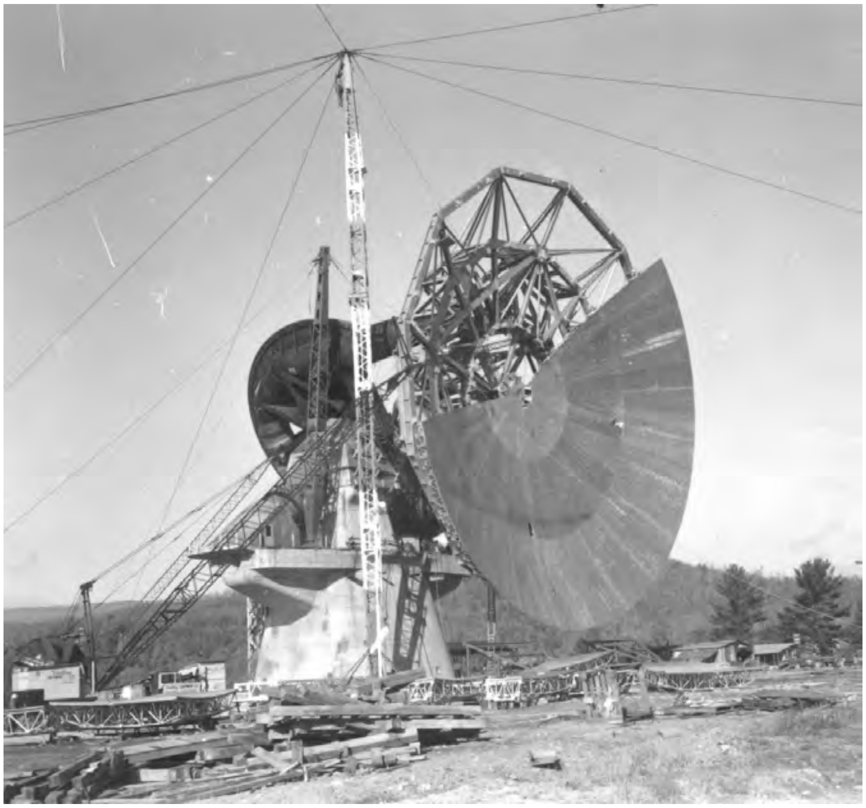
On November 4, 1964, after many weeks of preparation, the lift of the superstructure was started. This was the lift so long anticipated as it was the final large lift of the job. It was witnessed by a large group of people from the lab and by many people from nearby areas. If any of them were seeking excitement, there was some when the superstructure reached the near-vertical position. It had been out of balance to some extent from the beginning of the lift, and this condition was being controlled by a mobile crane which held the heavy side of the structure off the ground and also kept it from swinging freely. At the near-vertical position, a cable on the mobile crane parted which allowed the structure to swing away from the crane and hit the ground. Needless to say, there were many varied expressions and colors on faces of bystanders as well as persons working with the lift.

The contractor decided not to proceed with the lift at that time and secured the job for the night. On November 5 the structure was lowered to the ground and work was begun on revising the lifting apparatus and making plans for the next attempt.

The second and final attempt at lifting the superstructure was started on November 10. With the lifting apparatus revised, the structure was in good balance and no trouble was encountered. The structure was brought to near final position and again secured for the night. On November 11 it was put into final position and bolted to the yoke arms.

This now completes the major lifts at the 140 Foot, which began on May 15, 1964 with the assembled shaft and sphere. The superstructure as lifted weighs 266 tons and makes a total of 1,440 tons of steel and aluminum which have been placed atop the pedestal. The total moving weight will be in excess of 2,500 tons.

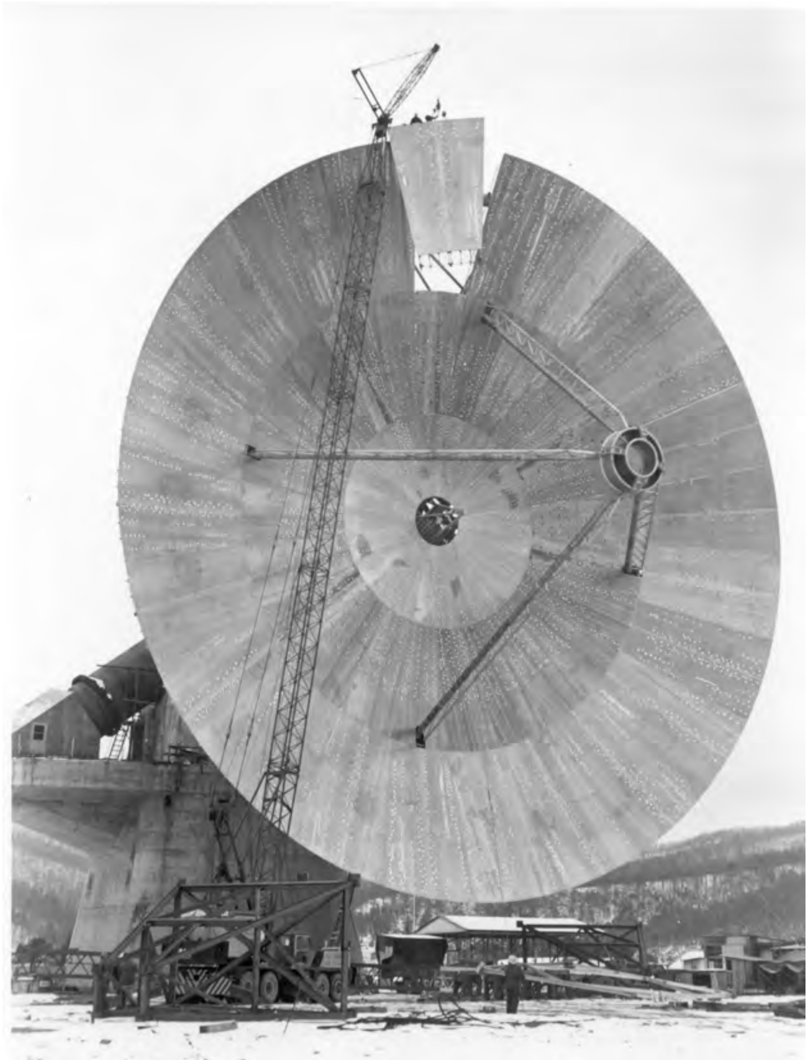
Howard Lambert
from an article in the Observer *November 25, 1964*



Top: Installing surface panels about one month after the superstructure lift. Bottom: Installation of the hour angle drive gear.

The spherical bearing sitting atop one of the hydraulic support pads.

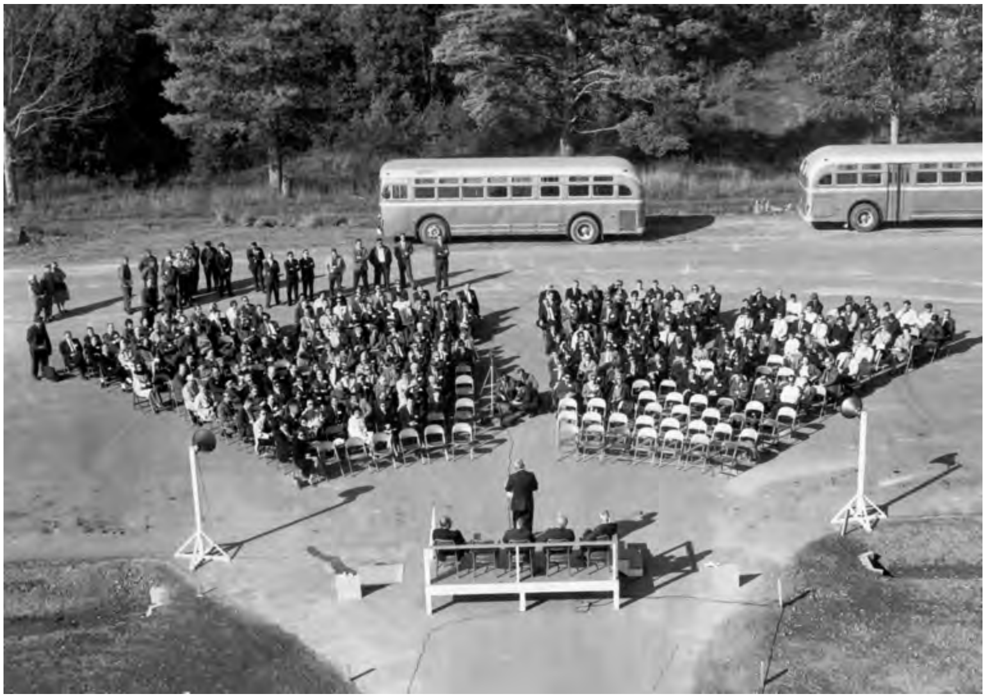
Some of the hydraulic pumps which supply oil to the bearing.



Final surface panel being set into place, December 1964.

NRAO Project Staff for the 140 Foot Construction, 1961-1964.

Maxwell M. Small .... Project Manager
Spencer S. Greenwood ... Construction Superintendent
John N. Ralston .... Civil Engineer
William G. Horne .... Structural Engineer
Richard R. Grabe .... Office Manager
Leslie H. Lambert .... Senior Clerk
Howard W. Brown .... Technical Specialist
Eugene L. Marcum ...... Technical Specialist



Top: The dedication ceremony as seen from the deck of the 140 Foot Telescope.
Bottom: The dedication of the 140 Foot, October 13, 1965.
Left to Right: T. Keith Glennan (President of AUI, standing), Theodore P. Wright (AUI trustee from Cornell), Leland J. Haworth (Director of NSF), David S. Heeschen (Director of NRAO), Max Small (NRAO, project manager).

Mark Gordon at the new 140 Foot control console, 1972.

The 140 Foot Telescope control area in 1985 with George Liptak at the controls.

Operating the 140 Foot Telescope

Hopefully today, June 18, 1965, a 6 cm receiver will be installed on the 140 Foot and full time observing will begin under the direction of Dr. Peter Mezger. Here's hoping all systems are "go" and this beautiful brute, as she's sometimes referred to, performs as we all know she can. Really, we have the utmost confidence and do not expect over a maximum of 5 or 6 malfunctions. . . .

Bill Hunter, Chief Telescope Operator
in the *Observer, June 1965*

It should be understood by all telescope operators that their instruments are more akin to moving machines than to static buildings.

Report of the Technical Assessment Panel of the
300 Foot Radio Telescope at Green Bank, WV

Some of us, being relatively new at the 140 Foot, are quite impressed and interested in the size, and the complex machinery, controls and structure included in this operation and are certain it will become a pleasant and productive place to learn and work.

Bill Hunter, Chief Telescope Operator
in the *Observer* June 1965

Just a word of commendation to those involved in our recent power outage at the 140 Foot. At 0930 we were interrupted by the whine and boom of failing motors, dimming lights and chattering relays. Dave Williams arose three feet from the console chair, quickly flipped the Bat Handle (Emergency Stop) and precharge switch. Howard Brown covered twenty stairs in three large strides and de-energized all breakers to the control circuits. The silence was eerie. The electricians were promptly on the scene and the trouble diagnosed. One of three 4160 volt feeders ruptured between the 140 Foot and the substation. Mr. Williams, Mr. Elliott, and others from several divisions began to function smoothly, and soon trucks, winches, generators, rope, people, new cables (excellent foresight that they were available), and all necessary paraphernalia were on hand to do the job. Arrangements were made to supply the clock room and Residence Hall with emergency power. The main site power was cut off. During continuing downpour (it always rains on these occasions) the old cables were removed and new ones pulled in. Main site and 300 Foot power was restored at about 2200 hours. Hot coffee was provided by Chester Cassell and John Matheny, a very welcome addition. The men were sent home at 2300 for a quick snooze and were back on the job at 0600. The 140 Foot was again humming by 1040 and everyone relaxed. At 1120 the 208 volt transformer belched forth smoke and flame and practically the whole scene was re-enacted. Two emergency transformers were acquired from somewhere and jury rigged as a replacement. By removing countless light bulbs and equipment from the line to reduce the load, we are at present limping along in partial blackout. (Bring your own flashlight).

Bill Hunter, Chief 140 Foot Telescope Operator
in the *Observer* November 1969

Changing a receiver in 1965.

Many visitors ask about the strange looking house on stilts. This is the service tower and a most important asset to the operations of our instrument. It is used to mount and service equipment located at the vertex, or focal point, of the antenna.

Receiver installations at this telescope are quite fast and furious, taxing both the Electronics Division and the installation crew to be prepared for a multitude of changes and revisions to suit the needs of different groups of observers. There may be as many as ten to fifteen similar, but different, programs in operation in a one-month period. Formerly, this work was accomplished with the aid of an extension type, portable elevator, still used to service the 85 Foot telescopes and, in an emergency, the 140 Foot. Each piece of equipment, by and large, had to be handled mostly by an artful act of balancing and various means of jury rigging (chain falls, pry bars, grunts, groans, etc.) to eventually secure to the mount. Once in place the elevator had to be relocated in order to get to different components of the receiver. These procedures were carried out many times in adverse weather conditions and were time consuming and very hazardous.

The service tower provided quite an improvement to this portion of our operation. It consists, literally, of a house built on long steel supports. The whole

Changing a receiver in 1970

structure is mounted on railway wheels. The telescope is lowered to a preset angle and secured by the stowing brakes. As soon as the duty operator acknowledges that the telescope is secured in the proper, safe position, he grants permission for the tower to be moved. Two large doors are opened; the top portion of the tower is moved back, and safety hand rails opened to permit the tower to completely engulf the ring in which the front-end equipment is mounted. An interlock is provided which prevents the operator from releasing the brakes or otherwise moving the telescope until the tower is moved beyond the clearance point.

A receiver is unloaded directly from a truck, rolled onto an elevator, and hoisted to the tower level. It is then picked up by a hydraulic jack designed and built in our NRAO shops to fit exactly our standard front-end boxes. The jacks position the box to the correct angle, and the receiver is rolled into the mount, aligned to the dowel pin holes, and bolted into place. The necessary cables are quickly attached.

The entire procedure usually takes about one hour as opposed to from three to four hours with the old method. Electronics people make various tests to insure the receiver is functioning properly. The tower is rolled back, and observing begins immediately, if not sooner.

Bill Hunter
in the *Observer*, March 1970

Technical Development

Events in the History of the 140 Foot Telescope*

May 23, 1965 — First observations: lunar occultations at frequencies of 234, 256, and 405 MHz by S. von Hoerner.

June 18, 1965 — 6-cm receiver installed; telescope performance tested.

July 1965 — Recombination lines of (excited) hydrogen detected at a frequency of 5009 MHz using a 20 channel spectrometer.

March 1967 — First VLBI experiments with independent local oscillators between 140-ft, the NRAO 85-ft telescope and the Haystack telescope.

2nd quarter, 1967 — DDP116 computer installed to enhance operations. Computer had 16 Kbytes of memory, a Franklin printer, teletype, card reader, 556 BPI tape drive, and a small CRT for data display.

2nd quarter, 1968 — Model II autocorrelator spectrometer, with 413 channels, completed.

October 1969 — VLBI experiments conducted between 140-ft and 22-meter telescope in Crimea, USSR.

March 1970 — Service tower construction finished.

July 1971 — New operator's console installed.

May 1972 — Honeywell 316 (H316) computer for telescope control installed. It was in use until 1999. Computer had 32 Kbytes of memory, card reader for program loading (replaced by a PC in 1988), CRT displays, and a position panel which controlled telescope position, errors, and slew rates. Also installed a communication link for data between the DDP116 and H316.

4th quarter, 1973 — Modcomp II computer installed for data analysis and the POPS analysis software. Computer had 64 Kbytes of memory, 25 Mbyte disk, 800 BPI tape drive. Created a data link from DDP116 to Modcomp II.

2nd quarter, 1974 — SAO hydrogen maser clock installed for accurate timing of VLBI experiments.

3rd quarter, 1974 — Cassegrain system completed: Cassegrain house, subreflector, and maser receivers installed. It was a prototype for the VLA receivers.

2nd quarter, 1975 — Temperature monitors installed throughout telescope.

3rd quarter, 1976 — The yoke arms, polar shaft, main bearing support, and deck were covered with foam insulation to minimize thermal response.

May 1977 — The DDP116 computer was replaced with a Modcomp II and POPS software for control and data acquisition. Computer was in use until 1999. It had

* *From a handout at the 140 Foot Symposium*

64 Kbytes of memory, two 1.25 Mbyte disks (replaced 1981), card reader, TEK 4012 CRT, Versatec printer (replaced 1987), tape drive (800 BPI - later changed to 1600 BPI). Retained data link hardware to analysis Modcomp. At the same time, part of the operator's console was replaced with a new data panel.

May 1978 — First generation of deformable subreflector installed.

2nd quarter, 1980 — The 1024-channel, Model IV autocorrelator spectrometer installed.

1st quarter, 1981 — A 50 Mbyte disk was added to the analysis Modcomp and a 25 Mbyte disk to the control Modcomp.

4th quarter, 1981 — Digital Continuum Receiver installed.

May 1986 — First tests of the lateral-focus mechanism.

October 1986 — First tests of the beam splitter.

October 1986 — First holographic maps of the surface of the telescope dish.

September 1987 — The surface was reset using results of holographic maps.

October 1987 — The TEK 4012 CRT and Versatec printer were replaced with a Modgraph terminal and a QMS printer.

January 1988 — The card punches and card readers were replaced with a PC running File-Ed software.

Early 1988 — The local-area network was installed.

April 1989 — The ModComp data analysis computer was replaced with a Sun workstation running SunPOPS.

June 1989 — First pulsar observations with the Spectral Processor.

April 1989 — Began replacing the Cassegrain maser receiver system with solid-state HFET amplifiers.

May 1989 — First observations with the 35 GHz receiver. This is the highest frequency at which the telescope has ever been successfully used.

January 1991 — The UniPOPS data analysis system introduced.

2nd quarter, 1992 — The MarkIII VLBI terminal was replaced by the new NASA field system.

October 1992 — The data archive system was switched from 1/2-inch, 1600 BPI tapes to Exabyte tapes.

1994 — The VLBI field system replaced with VLBA/DAR.

1995 — Receivers designed to be used on the GBT were installed and tested in the Cassegrain house – X-band, C-band, and KU-band.

1999 — The telescope was retired as a user facility.

The speakers platform at the 140 Foot Thirtieth Anniversary Celebration. (Left to Right:) Hugh Van Horn (NSF), Jay Lockman (NRAO), Bernie Burke (MIT), Paul Vanden Bout (NRAO)

Bernie Burke (MIT)

3 The Thirtieth Anniversary Symposium, September 1995

Editors' note: On September 29-30, 1995, the Observatory hosted a symposium and celebration for the thirtieth anniversary of the 140 Foot telescope. In the following pages are many of the papers and talks that were presented. These include accounts of scientific discoveries, technical improvements, and history. Astronomers that worked at Green Bank in the early years of the 140 Foot recount their discoveries and experiences. Some presentations anticipate the discoveries and innovations that would be possible with the GBT, which was under construction at the time.

Hugh Van Horn, Director of the Astronomy Division of NSF (left) talking with Paul Vanden Bout, Director of NRAO, at the 140-foot celebration, September 1995.

Looking Backward: The Origins of NRAO/Green Bank and the 140 Foot Telescope, As Viewed from NSF *

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Division of Astronomical Sciences
National Science Foundation
Arlington, Virginia, USA

Abstract

This article summarizes briefly the events that led to the creation of the National Science Foundation and the National Radio Astronomy Observatory. Some conclusions are drawn from this about the genesis of large undertakings.

I. Introduction

When I was asked to speak at this anniversary celebration for the 140 Foot Telescope, two thing came to mind. First, I have just been struggling with the annual budget, so the question of what to do with the budget allocations for FY 96-97 is fresh in my mind. Second, I have recently been reading a wonderful little volume entitled *A Patron for Pure Science* (England 1982). It is a publication of the National Science Foundation (NSF), and it concerns the early years of the NSF. In it, I also found some interesting material about the early years of the National Radio Astronomy Observatory (NRAO) and about the 140 Foot. I thought it might be appropriate to put those two things together and tell you a bit about the early history of NSF, NRAO, and the 140 Foot. I also want to draw a few lessons that I think I've learned from this.

In digging through NSF's files to find what we have available about the early history of NRAO, I found the following interesting letter from a director of a national observatory. I am going to read you an edited version of this letter, which is addressed to a person at NSF:

> *Dear Dr. X: In accordance with your telegram of December 6, 19xx, I am enclosing a revised justification for the Observatory's FY19xx budget based on the amount of xx million dollars.*
>
> *As a result of this most recent reduction in the Observatory's FY19xx budget request, Observatory activities during that fiscal year will be confined almost solely to the operation of the facility as it will then exist. This funding level is dangerously low. It will require curtailment of programs and plans, the consequence of which will carry over to later years.*

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* Presented at the 140 Foot Birthday Symposium, September 1995, revised July 1998. The opinions expressed in this article are solely those of the author and do not necessarily reflect the position of the National Science Foundation.

It is signed by D. S. Heeschen, and it is dated 1964. This letter well-illustrates that the kinds of problems we are struggling with today are not new. They are exactly the same kinds of problems with which every observatory director has struggled, almost every year.

With that as an introduction, I would now like to take you back a little further in time than anybody else has done today. The earlier presentations have gone back to 1963 or 1965, but I would like to go back to the beginning of NSF and NRAO 50 years ago.

II. The Beginning of the NSF

In 1945, at the end of World War II (WWII), the United States had many exciting opportunities for science. Scientists had saved the world. They had created the atomic bomb that ended the war. They had developed radar to help win the war. They had created the proximity fuse, sonar, and other wonderful scientific advances that had helped to establish a solid defense for the United States. Also, relevant to the development of radio astronomy, a lot of electronic equipment that had been developed during WWII suddenly became readily available for non-defense purposes.

The immense contributions of science in helping to win the war prompted some members of Congress to suggest the creation of an organization whose purpose would be to encourage scientific research. Senator Harley Kilgore from West Virginia in 1945 was the first to propose the creation of a National Science Foundation (England 1982). That proposal did not go very far. I had always thought that NSF started with Vannevar Bush's famous publication *Science, The Endless Frontier* (Bush 1945), and that is the starting point marked at the left-hand edge of the timeline shown in Fig. 1.

It did not occur to me until fairly recently to wonder why Vannevar Bush's document was written in 1945, while the founding act that established NSF was not signed into law until 1950. I had always thought that Bush had presented such a compelling argument that everyone saw the logic of it, so Congress passed a bill, and that was it. But why five years? The actual history is much more complicated than this naive view (see England 1982; see also Blanpied 1998 for significant additional information published after the presentation of these remarks at the 1995 Green Bank Workshop). One reason for the delay was that it was not at all clear that this new organization— the National Science Foundation—was something everyone wanted. Many had serious concerns. If the government puts money into something, will not government control follow soon after? Do you really want that? In the late 1940s, the United States had wonderful universities that had done a fantastic job of generating new knowledge and new science, and some people felt that we should leave them alone, fearing that the investment of Federal funds might influence the direction of research inappropriately. The debates over such issues took five years to resolve (England 1982).

In addition, there was an issue concerning the control of the National Science Foundation. The people who proposed it originally, people like Vannevar Bush, said in effect, "Look, you have lots of bright people out there: the presidents of the universities, the National Academy, *etc.* Just give them some money, put them together, and let them decide how to spend it." However, President Truman's view

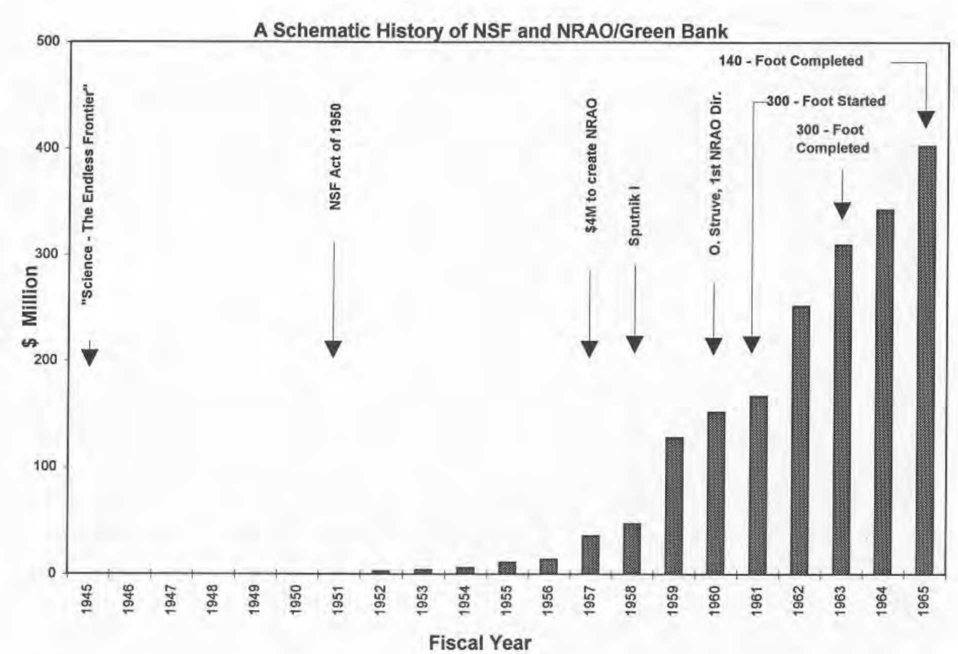

Fig. 1— A schematic history of NSF and NRAO/Green Bank, showing the annual budget of NSF for the 20-year period 1945 through 1965 (source: Mazuzan 1995). Several events of historical significance for NRAO are indicated.

was, in essence, "No way are you going to do that! We are not going to have a bunch of academics spending the federal government's money. There has to be an organization to do this—if we are going to have one at all—that the President controls." That is why we have a Presidentially appointed National Science Board and a Presidentially appointed Director for the National Science Foundation, in both cases subject to Senate confirmation.

It took five years to iron out all these things, and in 1950 the first budget for the NSF was put into the President's request (England 1982). I assure you that it is right there in Fig. 1, even though you can't see it: President Truman put in a request for $475,000 to start the NSF, and Congress granted something like $250,000. We see the same sort of interplay between the Administration and Congress today.

III. The National Observatories

For the first several years after it began, up to 1955 or 1956, the NSF operated exclusively in a mode that we think of today as the "individual-investigator" mode. People would send in proposals, they would be carefully examined, and eventually some money would flow back. There was no "big science" at all until 1953–1954, when the astronomers began talking about a national observatory.

There were actually two national observatory initiatives going on at about the same time during this era. One was an initiative to start a national *optical* astronomy observatory. (A detailed history of the national optical observatories has been published by Edmondson 1997 after this presentation at the 1995 Green Bank Workshop). At the same time, another group was pressing to create a national *radio* observatory. I won't go into the details (see Needell 1987), but here again were cases one might have thought were just good, obvious ideas, so that money would certainly be expected to flow to support them.

However, there was great controversy surrounding the proposal to build a national radio astronomy observatory, one that involved powerful personalities and profound differences of opinion. Some people said, "You don't want to build a national *radio* astronomy observatory—you're going to split astronomy! You do not want to do that!"

There was also a site issue: If a national observatory of some kind were to be built, where should it be built? The optical astronomers wanted it in the southwest. The radio astronomers didn't care. They were perfectly happy here in the hills of West Virginia. Ultimately, in 1956, Associated Universities, Inc. (AUI), submitted a proposal that in FY 1957 resulted in the first funds to build NRAO. That is the amount of "$4M to create NRAO" indicated in Fig. 1, and, as you know, NRAO started in Green Bank.

I don't know precisely when the construction of the 140 Foot started. I assume that it began around this time period. However, it ran into technical difficulties, and the actual construction was not finished until sometime in 1965, as indicated at the right-hand end of the timeline in Fig. 1. That was a long gap, and along the way some other interesting things happened, also illustrated on the timeline. Sputnik was launched in 1957, and that led to a big increment in the NSF funding. It also took a while before the first Director of NRAO was appointed, in 1959.

As the 140 Foot project proceeded and ran into various technical difficulties, various groups both at NRAO and elsewhere in the radio astronomy community decided that the Observatory needed something substantial. Accordingly, the 300 Foot radio telescope was conceived as a quick project and in fact was built in less than three years from the preliminary design study and cost estimate in 1959 through the beginning of operations in September, 1962 (see Findlay in this volume, page 145, also 1962a, b). The total cost of the project was $850,000, not including the electronics. The 300 Foot, a 600-ton transit telescope, was designed for 21 centimeter observations. It was never intended to have a long lifetime ("might fall down in five years:" Findlay 1974), but as we all know, it survived for a quarter century, and its collapse in 1987 provided the genesis of the Green Bank Telescope (GBT) currently under construction.

IV. Lessons from the Past

Out of this brief review of the early history of NSF and NRAO, I drew a number of conclusions that I would like to pass along to you. These conclusions are lessons from the past 50 years which I think can still be useful to us today.

My first conclusion is that **change provides new opportunities**. The changes at the end of WWII provided new opportunities that eventually led to the NSF and NRAO and the 140 Foot. The world around us continues to change, and this continues to provide new opportunities. One must adapt to the changing environment or stagnate and wither away.

Second, it is necessary to **imagine the future** before we can get there. Vannevar Bush imagined a future in which we had the NSF. The radio astronomy community and AUI imagined NRAO before it was built. Sometime in that same era people imagined the 140 Foot as well. Visionary leaders continue to be important, precisely because we must imagine the future before we can create it.

Third, one can see from the historical record—and can still see today— that **change is threatening and provokes resistance**. That accounted for part of the delay in the original funding of the NSF and for part of the delay in getting NRAO built. Because change threatens the *status quo*, it provokes resistance from those who benefit from the situation as it then exists. For example, the proposal to create NRAO threatened the unity of astronomy.

Fourth, however, **if one is persistent, one can accomplish great things**. The result we see here today in Green Bank and throughout NRAO is a real tribute to the people who have led this fine institution over the years and to the persistence they have shown in guiding the Observatory through many trials and tribulations. Things we now take for granted—like NSF and NRAO and the 140 Foot—required a long struggle in order to bring them into being. This was necessary to overcome the resistance generated by change.

My penultimate conclusion is to **continue to look forward**. For example, the GBT is a vitally important step for the future of NRAO. Despite the changes currently underway in the nation—as in efforts to define a new "new social contract" between the academic world, industry, and government—it seems to me that NRAO/Green Bank has exciting prospects ahead when the GBT is completed.

One conclusion that I did not include on my viewgraphs, but which I think has been obvious throughout the talks today, is that **personal connections are vitally important**. Many of the talks today highlighted instances of "who knew who" and how those personal connections helped get things accomplished.

Discussion

Harry van der Laan: I would like to provoke you into some speculation. If change provides new opportunities, why do you think that the big revolution of the end of the cold war in 1989 has not really led to a realization of many new opportunities? Is it because the science community has been showing too little imagination, or has the imagination been overwhelmed by the resistance which comes from the threat of change? What is your speculation, speaking for this country?

Van Horn: My personal speculation is that we have not yet awakened to the fact that there is a real change, a fundamental change that has taken place in the world. We have talked about that, but we have not yet come to grips with the way it affects our own institutions. That's the threatening aspect that is provoking resistance to this point. I would like you all to see the last two conclusions as a call to arms. I believe we can take advantage of change to make the situation better, but it is not always obvious how to do that. In this case it is not at all obvious. It is going to need some bright people thinking hard to accomplish that.

References

I am grateful to Jay Lockman, Bob Dickman, George Mazuzan, Ben Snavely, Kim Elliott, and Stephanie Bianchi for their help in assembling the materials for this article. I also thank Morris Aizenman, Vern Pankonin, and Wayne van Citters for comments on an earlier draft.

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The 140 Foot Telescope and audience at the Dedication, October 1965.

The 140 Foot, 1974.

The 140 Foot, A Good Old Friend*

Sebastian von Hoerner
Esslingen, Germany

When I first saw the design of the 140 Foot telescope, and especially after it was built, my impression was: "What an odd fellow!" Why should it have an expensive polar mount, on top of a huge concrete building, with a large sail high up into the wind? Of course, that is the way telescopes had been built for hundreds of years: since the Earth rotates about its polar axis, one also puts a telescope on a polar axis and rotates it backwards with a constant clock drive. These days we put telescopes down on the ground using a simple alt-azimuth mount, because we have computers for coordinate transformations — computers that are reliable! But it took years before people believed in computers, and so we got this odd fellow.

But then I started using it, and it wasn't odd any more, but a wonderful tool. And when I worked to investigate and improve it, I developed a very personal relationship with it, as many people do with their car, and the 140 Foot became a good friend and stayed that way.

Actually, I was the first one who used the 140 Foot Telescope. I used it even before it was officially delivered. I was among the ones who developed the technique of using lunar occultations, a true "gift from heaven," for obtaining high angular resolution observations (a few seconds of arc) of radio sources. This was 60 times better than the 3 minutes of arc beamwidth of the 140 Foot at its design specification for its shortest wavelength of $\lambda_{min} = 3$ cm. But an occultation of a radio source by the Moon occurs only at a very specific time. If you miss it and want to repeat it, you must wait the 19 years through a lunar cycle. In May 1965, a good occultation was just coming up. I wanted to grab it, and I did. The occultation technique works best at longer wavelengths, but that is exactly where there is lots of man-made interference. Thus, for several months before the occultation, I made a search for the three best frequencies to work at, one of which, at 405 MHz, is still used.

The scientific results of the occultation experiments were well received by the Observatory, but some of my other results were not. I discovered that not only was quite a lot of the interference man-made, but a lot of it was home-made at NRAO! From my interference data it followed (and was published in our local "Observer") that about one-third of all our employees come to work 5 minutes late (betrayed by the emission from the spark plugs of their cars); that most workers need over half an hour after arriving before they decide what to do that day (i.e., before welding and other electric activities start), and that the late arrivals are made up for by a group which leaves 5 minutes early.

These occultations were great fun for a while. Later however, the accuracy of lunar occultations was beaten (actually beaten to death) by interferometers, which could obtain an angular resolution of milli-arcseconds and not be so dependent on heavenly providence.

* Presented at the 140 Foot Telescope Birthday Symposium, September 1995.

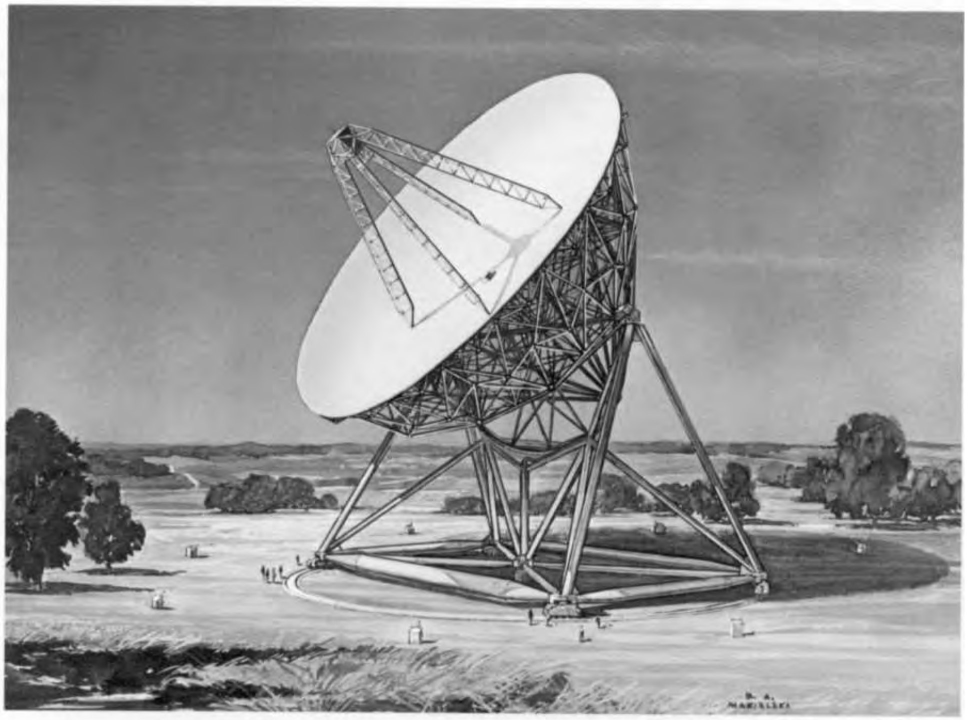
Now a good relation between friends implies also learning from, and teaching to, each other. I taught the telescope: "Don't just sit on your specifications (your genes, horoscope, or whatever); shake the reefs out of your knees, and jump to some new trick!" And I was taught: "Grab a good opportunity by the tail and use it; but don't depend on that!"

Sometimes it may also be necessary to warn a friend. Looking long hours at the Moon, the 140 Foot got bored, and it sometimes gave me regular strong signals, suggesting Moon-reflected radar. It gave me enough data to satisfy my curiosity. With trigonometry and arithmetic I derived the height and width of a rotating horizon-scanning radar antenna, and even its location on Earth. But John Findlay gave me a severe warning and I passed it on to the telescope: "Shut your 140 foot-wide big mouth about this one, or you will get confiscated by the military!"

It may also happen that you get a hint with a friendly nudge from your pal, but you don't get the message. At some occultations I had a bit more noise than I should have, which disappeared together with the source behind the Moon, and reappeared again, too. This looked rather odd, since the Moon plus receiver noise was a lot stronger than the source. Weak as it was, its flicker reminded me a bit of "twinkle, twinkle, little star," and I checked to see if it could have been caused by our atmosphere, but the answer was a clear No! Since it was too small to worry about, I declared it "one of those dirt effects" and forgot about it. If I hadn't, I would have seen a great thing: Interplanetary Scintillation.

Then I left the 140 Foot and went back to the wide open world of astrophysics. After doing cosmology for some years, I finally felt that there was really no need for any more uncertain theories, but what we badly needed was Bigger and Better Telescopes. So why didn't we build them? Investigating this question, I discovered some "Natural Limits" of telescope design, as well as the means to conquer the worst of them: the gravitational deformations. With a good small group we designed nice new telescopes (with homologous deformations). Some of the designs even got "First Priority" from review committees, but unfortunately, only when there wasn't any money. The first telescope we designed was a large Alt-azimuth 300 foot for $\lambda = 6$ mm; its design was finished in 1968. Since it got no money, we decided on a smaller one and by 1971 had developed a very good design, with several new inventions, for a 65 meter telescope that would work to $\lambda = 3$ mm (see Figure 1). The work produced a frequently-quoted book by Findlay and me, but no money. Next came a 25 meter design for $\lambda = 1.5$ mm, finished in 1975. By then I got suspicious and found I had enough data for statistics and a graph. If you plot the three diameters over their years on a simple linear paper, you see a straight line, perfect enough for extrapolation. And this told me that our telescope diameters would converge to zero in 1978.

That looked like a message, and I got it. But if there wasn't any money for new designs, what about doing some work on existing older telescopes, trying to make them better though not bigger? So I turned back to the good old friend, determined to study its shortcomings, and to suggest new improvements. From 1975 through 1980, I wrote 16 technical reports and seven published papers about the 140 Foot. There were two things to improve: surface deviations (from a paraboloid), and (non-repeating) pointing errors.



THE 65-METER TELESCOPE FOR MILLIMETER WAVELENGTHS

Fig. 1— Artists conception of the Findlay–von Hoerner design for a 65 meter telescope.

Applying the work on "natural limits" to this size telescope showed that its worst ailments must be gravitational surface deformations, and looking at beam maps for different pointings confirmed it. General reasoning convinced me that tilting telescopes should have the three different modes, in decreasing order: change of focal length, astigmatism, and lateral focal shifts, and they must be simple sine or cosine functions of the pointing angle. One after the other was found and measured, with observations of sources (by me) and with structural analysis (by Woon-Yin Wong), in good agreement.

The usual telescope dish is stiffer at the center and weaker at the rim; moving from horizon to zenith lets the rim sag (with circular symmetry), which increases the focal length; in our case by 17.6 mm (see Figure 2). A formula was provided for automatic correction with the cosine of the zenith distance. This eliminated the old manual search for the best focal length.

Next, the usual dish has one direction (X) of its backup structure stiffened by the tilt axis and its bearings (declination or elevation), while the other direction (Y) is weaker and sags deeper, by 8.7 mm for us. This makes the focal length in the Y-Z plane longer by 51 mm than in the X-Z plane. The absence of a proper focal point is called astigmatism. We cannot control the form of the big dish, but I had shown earlier that almost any deviation of a main mirror, within wide

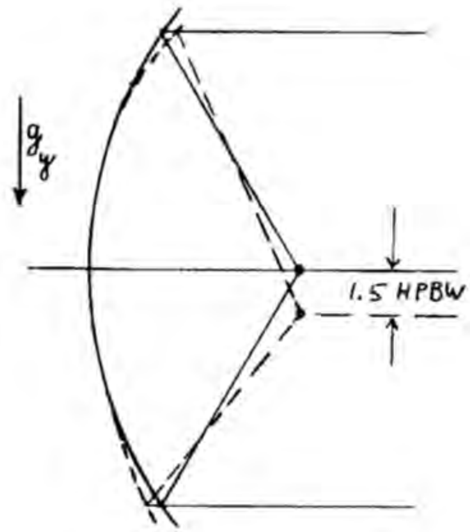


Fig. 2— Illustration from report of February 1977 showing the deflection of the 140 Foot dish that arises because the rim is less stiff than the center.

margins, can properly be corrected by a similar deviation of a secondary mirror. And fortunately, the easiest deformation of a round concave shell *is* the astigmatic deformation. Thus, Woon-Yin designed a beauty of a deformable subreflector, with two stiff 45° diagonals on its back, and, in between, two opposite motors pushing and two others pulling the surface, again with the cosine of the zenith distance. It raised the efficiency at $\lambda = 1.3$ cm by a factor of three. This nice invention has since been used on other telescopes. But measuring the astigmatism yielded a byproduct which was not so well received: the plane of the astigmatism seemed to be 5° off from the north-south direction. And indeed it was then found that the prime focus box rotation had its zero off by 5°, which means that all previous polarization directions had been 5° wrong.

If one pain has been cured, a patient is then likely to suffer from another pain which before had been overshadowed. When our friend's astigmatism had been cured, he started suffering from odd sidelobes. I investigated this, and under extreme conditions I saw up to four lobes in a row with regular spacing, which suggested coma-lobes from lateral defocussing. By computer simulations I could obtain their amount: up to 7.2 cm east-west, and 2.8 cm north-south. Since the telescope's feed legs deform no more than 0.5 cm, it must be the optical axis which was moving. The cause was again the stiff center versus the weak rim, but this time it was the antisymmetric deformation of the rim at low elevations. The upper and lower rim both bend down a bit, which for the upper rim is toward the Z-axis, while for the lower rim is away from the axis. The best-fit paraboloid then makes a "gliding rotation," gliding along the deformed surface, shifting its vertex and the optical axis upward and with it also the focus. To correct for it, we would need lateral shifts of the focal equipment, going with the sines of zenith distance and hour

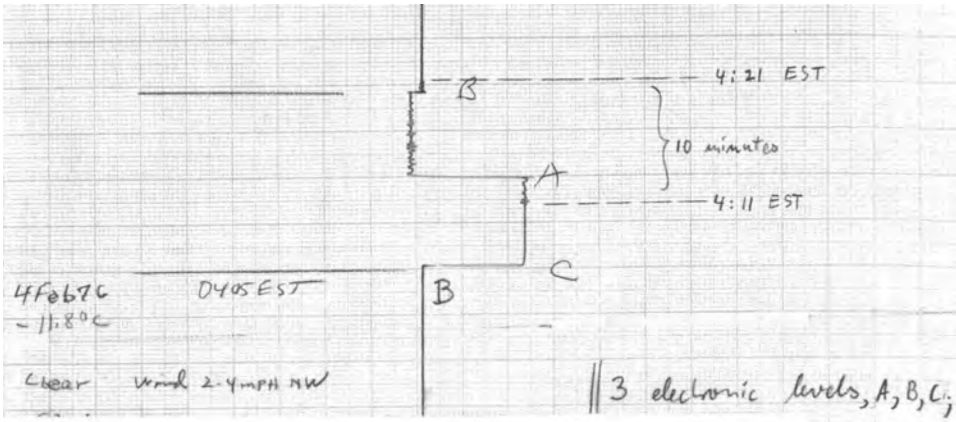


Fig. 3— The output of the three electronic levels mounted on the 140 Foot displayed sequentially: B, C, A, and B. The Guatemalan earthquake caused the oscillations between 4:11 and 4:21 EST.

angle. Fortunately, the complicated shift mechanism could be replaced by much simpler small tilts of the secondary mirror. It worked very well and removed the odd sidelobes. This then was all I could do for the gravitational surface deformations.

Now to the next pain. The pointing errors had always been too large, but improving the surface gave a better beam, and the pointing errors now hurt even more than before. The "repeating" errors (misalignments and gravity) are supposed to be corrected by the pointing program, which I checked. It had several odd mistakes which got corrected, and that improved the pointing a bit but not enough by far. The next "natural limit" to be investigated was thermal deformations of the polar mount. With thermometers and tiltmeters, I found and measured the bending of the heavy polar axis caused by sunshine above and shadow below. This explained most of the daily errors and they could be reduced with a coat of thermal insulation and white paint.

But the axis, as a whole, had a large seasonal tilt of 30 seconds of arc. This could only come from the building. How on Earth could a concrete monster with a three foot wall thickness tilt so much? It took more tiltmeters and thermometers (and lots of patience) finally to find out: the building did not tilt, but the upper platform did bend its north and south extensions, down in summer and up in winter. Since the polar axis is not supported symmetrically (the lower end is close to the platform rim, the upper tower more to the center because of its heavy load), the axis gets tilted as well. It took a while to fully understand this effect. The upper story of the building, under its platform, has a story underneath that is heated in winter, while nothing but cold weather is above it. Thus the platform expands in summer and contracts in winter, causing its bending and the tilt of the axis. With thermal insulations of tower and platform, plus some inserted heating coils, the pointing errors decreased by a factor of three.

One day (about 1976) it happened that our friend, all of a sudden, surprised us with a new trick, which really nobody ever had taught him. In the morning, before doing other work, I often stopped at the Telescope to have a look at the output graphs of temperatures and tilts that were measured during the night. One morning I found a very odd, very strong, tiltmeter wiggle (see Figure 3). I had by this time learned what a strong wind gust looked like, or a sudden break from a fast slew, but this was very different. The operator saw the chart and he joked: "This looks just like some earthquake." We both had a good laugh. Next day in the papers we saw that there had been a strong earthquake in Guatemala five minutes before our wiggle! So here we had a seismograph, and no one had known it.

Sebastian von Hoerner, 1960.

Thirty Years of Radio Telescope Development*

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1. How a Young Physicist Made it to Green Bank

While I studied physics at Delft University of Technology in Holland, several Leiden astronomers presented colloquia about the Benelux Cross Antenna Project. This made me seek a job with that group in the spring of 1963, shortly before I would obtain my M.Sc. They could not use me, because I was not interested in electronics, but Professor Oort introduced me to Dave Heeschen on his visit to Leiden in April 1963. Dave offered me a junior research position in Green Bank, and added: "you will earn only $550 a month, but I assure you that in Green Bank you will not be able to spend all that." I accepted the offer and fought my way through the visa application process. Thus, on a beautiful day in early September 1963, Brown Cassell drove my wife and me into the Deer Creek Valley and I had my first look at the 300 Foot Telescope. I was stunned and excited. I consider it a public relations error that the GBT will not be visible from that southern entrance to Green Bank. Soon after, my wife renamed the place "the valley of no return"!

I wanted to become a radio astronomer, but my knowledge of astronomy was limited to the textbooks of Baker and Struve, which I had read on the transatlantic boat trip to the New World. Imagine that we had been flying; I would have been totally ignorant. Dave Heeschen sent me to the 300 Foot to learn the trades of a telescope operator. Bob Viers was my first mentor and Mort Roberts was observing HI in galaxies. After three weeks I switched to the 85 Foot and worked for Howard Brown and Len Howell. There I learned a lot of useful things. A maser at 6 cm wavelength was under commissioning; it was noiseless, but unstable. In the only good scan which ever came out, on Jupiter, the baseline jumped just after the source passage. Hein Hvatum taught us his way of "data handling" right there. He took a pair of scissors, cut the chart at the jump and told us: "so, now I go to my office to reduce my data"!

We also looked for echos of the Westford radar from the "needle belt." ** To test the system, John Findlay first tried to see echos via the Moon. Nothing was seen the first afternoon. Since then I always double check the sign of the parallax correction if I use the Moon as a test source! I also learned not to accept chewing tobacco from Len, lest I had to swallow it, as Bert Hanson did in an effort not to show his distaste for the stuff. After I had beaten Howard on two consecutive nights in the number of pointing measurements done in a shift, all analyzed by hand of course, I asked Dave for more challenging work. He told me to talk to Peter Mezger. That set the course of much of my later professional life.

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* Presented at the 140 Foot Telescope Birthday Symposium, September 1995.

** See the note about "West Ford" on page 89.

2. Testing the 300 Foot and 85 Foot at too High Frequencies

Peter was scheduled to test the 300 Foot at 10 cm, a factor two beyond the design wavelength, and I joined him in the project, which ran over Christmas. Peter's wish that I observe Taurus A on Christmas Eve set some boundary conditions in the relationship between him and my wife. They are getting along well since, but I did not go to the telescope that night; neither did Peter.

In the spring of 1964, we also tested the 85 Foot at 2 cm with a receiver, borrowed from the Naval Research Laboratory. Heinz Wendker joined us for this project. The telescope was surprisingly good and we got several papers out of that session. I became quite immersed in trying to connect our measurements with antenna theory and it was the beginning of a life long interest in absolute calibrations, pushing telescope performance to the limits and, later, building telescopes, which move the limits further. I received a lot of advice and encouragement from John Findlay, who once characterized this type of work as "very dull but extremely important." To me it is fun!

Around that time, Sebastian von Hoerner was developing his ideas about homologous constructions and in discussions it became clear that our telescopes were behaving somewhat homologously; that is, through adjusting the focus as function of elevation, we could optimize the performance beyond what one would expect from the structural design. We saw in our measurements an indication of what Sebastian was suggesting as a new design goal. The deformed reflector should still approximate a paraboloid well, albeit one with varying focal length and changing direction of the axis. Thus a two coordinate movement of the feed or Cassegrain reflector in prime focus area would be needed.

Unfortunately, it was too late to let these ideas guide the design of the 140 Foot. In fact, the 140 Foot was under construction and we were all looking forward to bringing that machine into operation. It would be a while until that moment would arrive. I talked a lot with Max Small, the Project Manager of the 140 Foot. From the porch of the Hardee House, where the Smalls lived, he had a direct view of the construction site. Those derricks and the ugly concrete pedestal were crying for change (Fig. 1). He also had a special approach to project management, which I ascribe to his Oberlin years, where he majored in philosophy, before going to MIT to become a Naval Engineer. Once the view from his house became pretty because the telescope was completed, his job was done and he returned to Brookhaven, where he made waste water potable by purely natural means.

In the meantime, the millimeter telescope project was under discussion and Peter Mezger started a mm-receiver laboratory. I remember struggling with the first 3 mm Schottky-mixer, which required the contacting of a diode in much the same way as with my first crystal radio, some 15 years earlier. I found neither technology attractive and essentially stayed away from active receiver work ever after. The 36 Foot project was delayed beyond the time that my J1-visa expired and I had to return to Holland without getting the chance to work with this telescope. However, I almost had a chance to work on a 10 m antenna, which the North American Aviation Corporation had built at their factory site in Columbus, Ohio, just to show that they could do it! Peter and I visited there and we were quite impressed by the antenna. We were less excited about our discovery that Ohio did not allow the sale

Fig. 1— Upper panel: the construction site of the 140 Foot as seen from the Hardee House, residence of the Project Manager, Maxwell M. Small, before major assembly in Spring 1964. Lower panel: the completed telescope with the derricks already removed in Spring 1965, a few months before first operation. Here the telescope is pointing at the North celestial pole.

of “real” beer on Sundays, only “3% West Virginia,” type. Shortly afterward, NAA suddenly pulled all people from the antenna project, a few weeks before it would have been usable for us.

3. Testing Dual-beam Observing on the 140 Foot

The discussions about millimeter wavelength observing of course included the influence of the atmosphere. I took up an original suggestion of Robin Conway to suppress part of the atmospheric fluctuations by using a switched beam mode of observing. This study resulted in my doctoral dissertation. The experimental data to test the effectiveness of the dual-beam method were collected with the new 140 Foot Telescope in the fall and winter of 1965/66. I remember that it was not easy to convince Dave to give telescope time for looking at just empty sky. Because I would also take bad weather situations, I managed to get a good number of hours at several frequencies.

The principle of the dual-beam method is illustrated in Fig. 2, while some of my measurements are shown in Fig. 3. These tests were also done at 9 mm, making the 140 Foot the largest millimeter telescope for a long time to come. A simple receiver, planned for later testing of the 36 Foot, was mounted on the 140 Foot for the now usual test at the highest possible frequency. The receiver was pretty noisy and we expected a marginal behavior of the 140 Foot, so the Sun was considered a likely test source. In order to monitor the heat reflected from the dish, a thermistor was mounted next to the dual-horn feed at the primary focus. During the first observation of the Sun, an antenna temperature of 2 K was measured at the thermistor! At least we now knew how well the white Triangle paint on the reflector diffused the solar heat. Eventually, the receiver was brought into a working state and we found that the 140 Foot was pretty bad, as expected. In later years, through the work of von Hoerner and Woon-Yin Wong, the telescope was considerably improved, in particular through a deformable subreflector and thermal insulation of the structure.

4. Telescope Technology After the 140 Foot

The 140 Foot has the distinction of being one of a kind. I guess that already during the final construction years, many radio astronomers were convinced that this was not the best way to build larger and more accurate telescopes. The correct route was laid out by Sebastian von Hoerner in his detailed theoretical design work on homologous telescope structures. He showed that by proper design, a telescope could be built with a residual rms surface deviation 2-3 orders of magnitude smaller than the actual gravitational deformations. In his studies he was hampered neither by the traditional engineering approach, nor by a practical experience of building telescopes. The resulting design proposals were impressive in their structural beauty, but Sebastian’s price estimates made people like John Findlay throw up their hands in despair. The important point, however, is that von Hoerner showed, first analytically and soon with computational algorithms, that there are practical solutions to the problem. His own direct goal, a cm-wavelength telescope of some 200 m diameter for lunar occultations, was not realized, but his ideas influenced future telescope design strongly.

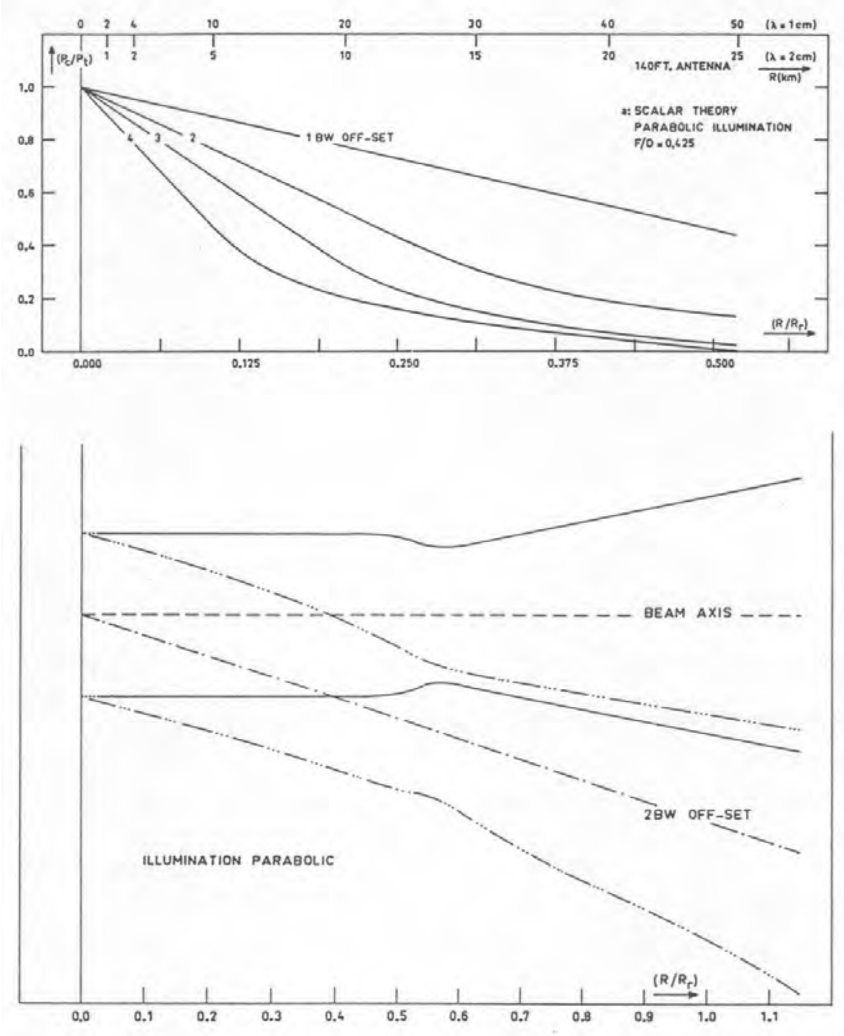

Fig. 2— Principle of the dual-beam observing method. Outline of the tubular beam in the Fresnel region at the -10 dB level with distance from the aperture. On-axis and 2 beamwidths off-axis beams overlap in the first few tenths of the Rayleigh distance $R_r = D^2/2\lambda$. *Top part shows the overlap in power* P_c/P_t *as a function of the distance from the antenna. Most of the atmospheric fluctuations occur in the lower 4 km, where the power overlap is considerable.*

Von Hoerner established a number of "natural limits" in the traditional design of structures, which the homology method could surpass. These natural limits are illustrated in Fig. 4, together with a number of modern telescopes (and telescope designs which were never realized). It is clear that gravity has been surpassed considerably in most of these, but that not all designs have taken sufficient measures to minimize thermal deformations.

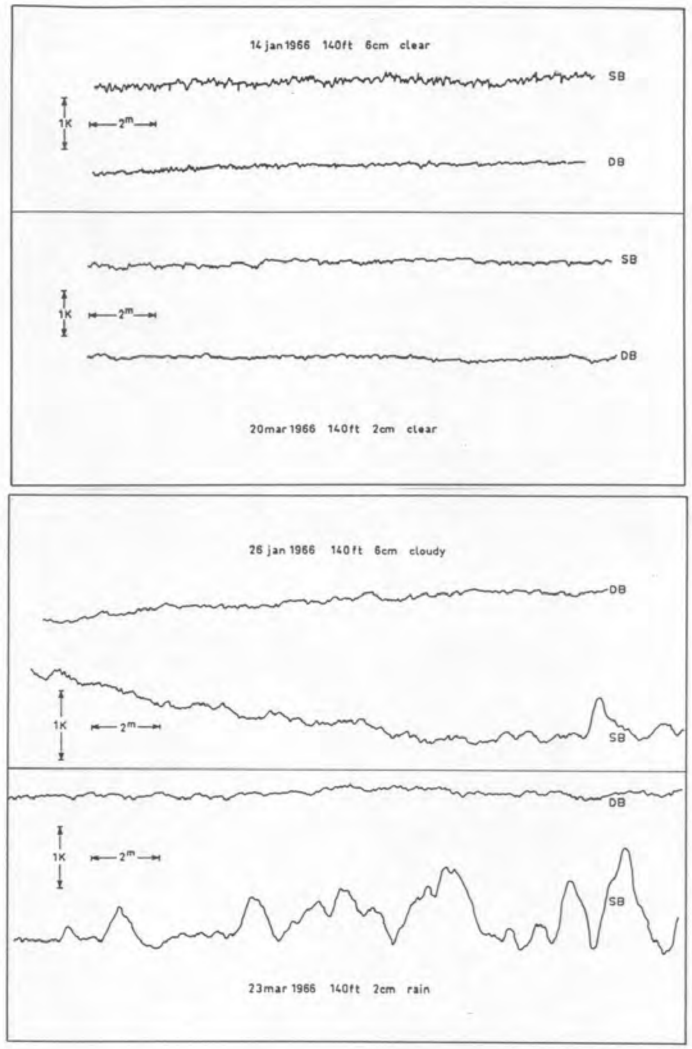

Fig. 3a— Examples of skynoise in single (SB) and dual beam (DB) mode, measured with the 140 Foot. Note that the clear weather traces demonstrate that the SB mode is atmosphere limited, because its noise level is higher than that of the DB trace. A marked improvement in the baseline is visible in the measurements with cloudy weather.

Over the last 25 years, essentially every radio telescope has been designed on the basis of homology principles. Contrary to von Hoerner's original plan, the new telescopes were not designed solely for a largest possible diameter, but at least as much importance was given to extending the observations to shorter wavelengths. The first large telescope designed along these lines, with a "trial and success" method, is the Effelsberg 100 m telescope. Planned originally to be 80 m in diameter, it could be increased to 100 m in the course of the design work. The shortest wavelength of 3 cm could be surpassed also. After installation of improved

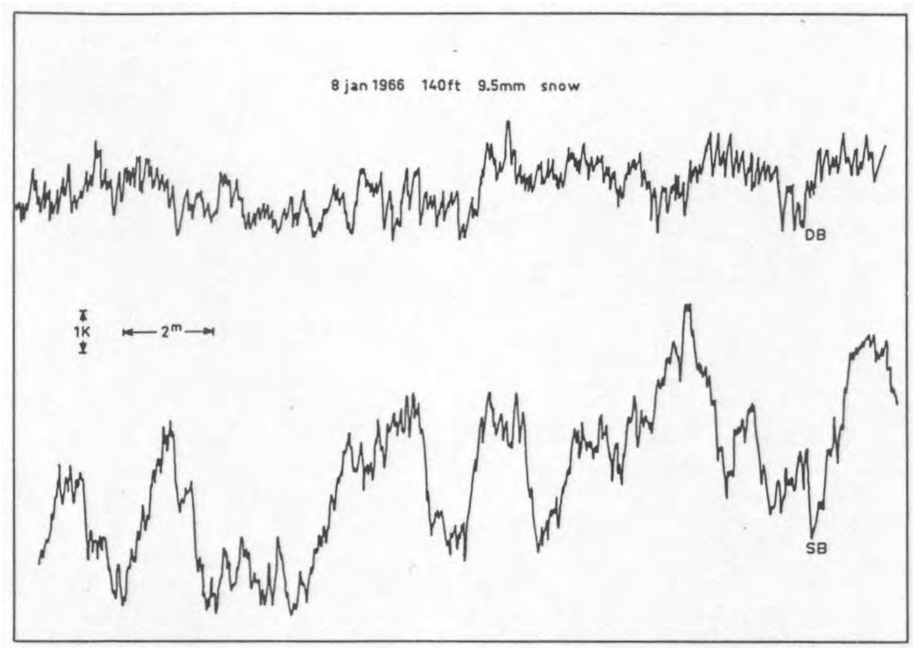

Fig. 3b— Another tracing of atmospheric fluctuations, during snow. At the wavelength of 9 mm, large fluctuations occur (SB), which are significantly reduced, but not completely canceled in the dual-beam (DB) measurement (top).

surface panels and optimum reflector setting at 40 degrees elevation, the telescope is now routinely used at 7 mm wavelength. It is interesting to note that a recent recalculation of the Effelsberg structure with up-to-date computer programs has produced deformation patterns which are very close to the original hand calculations of 1968; a tribute to the engineers of the sixties.

At NRAO, in the seventies and early eighties, several beautiful design studies were made by John Findlay, Sebastian von Hoerner, Woon-Yin Wong, Lee King and others, driving the homology principle to the limit. Unfortunately, neither the 65 m, nor the 25 m millimeter telescopes were realized. Instead, a group in Japan and we at the MPIfR built the 45 m Nobeyama and 30 m Pico Veleta telescopes, respectively. Both apply the homology principle successfully. In the 30 m telescope, an interesting addition was the minimalization of pointing errors under the influence of wind (incorporating the quadrupod in the calculations), along with the control of gravitational deformations. In Table 1 we present a comparison of the actual and the residual deformations of a number of homology designs. The highest degree of homology (ratio of line 2 and 3 in Table 1) is achieved in the NRAO design. The lower values in the IRAM and SMT structures are a result of incorporating other criteria, like wind and thermal effects, in the design.

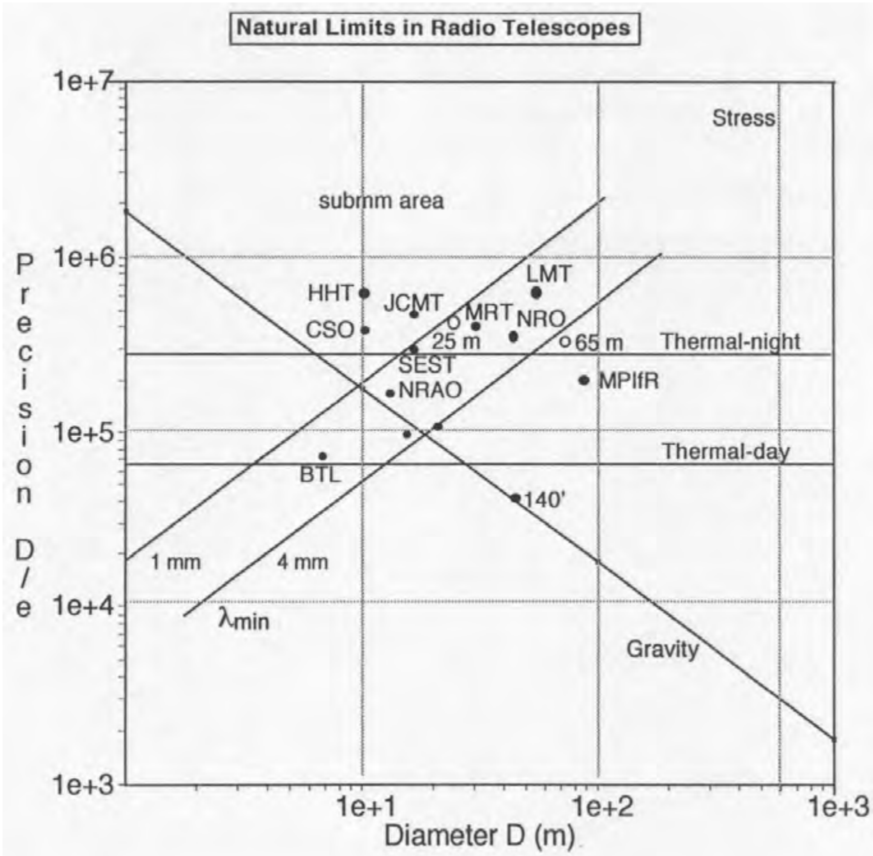

Fig. 4— The "natural limits" (stress, gravity and temperature) of a classical structural design. The 140 Foot lies on the gravity line; homologous designs are well above it. Points indicate the actual precision of telescopes and involves different ways to circumvent the thermal limit (insulation, active control, CFRP). Open circles: the NRAO designs for 25 m and 65 m telescopes .

TABLE 1

Structural Deformations of Homology Telescopes

Parameter	MPIfR	IRAM	NRAO	SMT
Diameter (m)	100	30	25	10
Maximum deformation (mm)	60	6	6	0.2
Homology residual (H_z, H_h) (μm)	300	60,80	9,2	3,5
Fabrication tolerance (μm)	400	20	12	3
Total residual (H_z, H_h) (μm)	500	63,82	17,12	4,6
Residual after setting at 45° elevation (μm)	400	<35	<10	<3

Fig. 5— The Submillimeter Telescope (also known as Heinrich Hertz Telescope, HHT) at 3200 m altitude on Mount Graham, Eastern Arizona. The 10 m diameter reflector and its backup structure employ extensively carbon-fiber reinforced plastic, which makes the telescope very insensitive to thermal gradients. Its performance during the day under illumination by the Sun is not noticeably impaired. The surface accuracy, measured and set by use of satellite holography at 37 GHz, is 20μ at present (fall 1995) and will be improved to better than 15μ in 1996. The Submillimeter Telescope Observatory is a joint effort of the Max-Planck-Institut für Radioastronomie, Bonn, and the Steward Observatory of the University of Arizona, Tucson.

Over the years, computer programs have been developed to a high level of sophistication. The recently completed Submillimeter Telescope on Mount Graham (a collaboration of MPIfR and Steward Observatory) has been designed with a multidimensional optimization, in which gravity, wind, temperature and material constants of the carbon-fiber reinforced plastic (CFRP) space frame were simultaneously considered. For these highly accurate telescopes, deformations due to temperature differences in the structure can be more serious than gravitational deformation, a fact already pointed out by von Hoerner in his early papers on the subject. The CFRP support structure of the SMT provides an almost thermally inert structure, enabling us to observe in daylight with partial illumination of the reflector by the Sun. The structural parameters of the SMT are summarized in Table 2, while Fig. 5 shows a picture of the telescope.

Now, NRAO is advancing the state of the art with the Green Bank Telescope, somewhat larger than the Effelsberg telescope (it has to be, of course) and with an unblocked aperture, unique for such a large structure. It will be fascinating to see this structure in a finished state and even more exciting to find its ultimate performance.

Because, as I pointed out earlier, any reasonably well-designed structure behaves at least somewhat homologously, the performance of telescopes has often been better than originally expected. Those structures were all rotationally symmetric. The GBT definitely is not. It will be interesting to determine how well the engineers have predicted the performance of this telescope. I hope the observers will give NRAO the chance to find out.

TABLE 2

Computed Tolerance Values of the SMT Structure and Panels

Load Condition	RMS Deformation (μm)		Pointing Error
	Spaceframe	Panels	(arcseconds)
Gravity – Zenith	5.2 (2.8)*	0	12.8**
Gravity – Horizon	3.1 (2.8)*	2	0
Wind (12 m/s) – Elev.=0 deg	2.0	2	0
– Elev.=60 deg	5.4	–	–0.3
– Elev.=90 deg	4.0	0	–0.6
Temperature – ambient ΔT = 20 K	3.2	2	0
– gradient across ΔT = 6 K	0.6	–	2.7
– gradient through ΔT = 4 K	0.6	4.5	0
– gradient in time ΔT = 5 K/h	2.9	0	0
Humidity – seasonal variation	3.0***	0	–
Total RRS at Elev. 60 deg.	6.9 μm	5.5 μm	

* effective values with panel setting at 45 deg elevation.

** systematic elevation dependence – to be calibrated to 1 arcsecond.

*** upper estimate after preconditioning of CFRP part.

5. Conclusion

How do we look back at 30 years of 140 Foot operation? From the viewpoint of the present day telescope engineer, it should never have been built this way. Nevertheless, at its conception in the late fifties, it was a bold concept and a daring project. Indeed the telescope is so original in its design that it has remained unique! It has served the astronomical community extremely well, better than any other comparable, or even bigger, telescope, not the least because of its advanced and flexible instrumentation. I feel fortunate to have been able to use the telescope for rather non-astronomical work and obtain a Ph.D. for it nevertheless.

What holds the future for the 140 Foot? The GBT is bound to get the main attention, and NRAO policy and budget constraints may well force the 140 Foot to be put to rest. I am sure, many of us would feel sad with that prospect. I guess we have one consolation: the 140 Foot will not so easily self-destruct as the 300 Foot. And turning it into scrap could be dearer than the original construction cost. So with some luck, it will still be there, if and when, a thousand years from now, some visitors from outer space will turn it into a god and worship it in the same vein as we indulged in the beauty of this beast 30 years ago.

Jaap Baars in 1965.

From a discussion session —

"At one point after having been back in Holland and working on the Westerbork telescope, I got a – for me totally incomprehensible – offer to become Professor of Acoustics at the department [at Delft University] where I had studied, and I declined because I thought radio astronomy was too much fun." . . . J. Baars

In the control room of the 140 Foot Telescope about the time of the discovery of the H109α *recombination line. Left to right: P.G. Mezger, Troy Henderson, B. Höglund, N. Albaugh.*

"We looked for the excited hydrogen line last October at a frequency of 5009 MHz using the 85 [Foot Tatel] telescope with a parametric amplifier front-end and the 20-channel back end. The results were then inconclusive; sometimes a line seemed to be present, and sometimes we could not find a trace of it. Anyway, we tried again two weeks ago, this time with the 140 Foot Telescope and with essentially the same receiver as before (the front end was somewhat improved). Now we were in fact tuned to the wrong frequency and did not find the line at first. The local oscillator proved to be the culprit. After repair of the faulty unit the lines was found at once in M17 (the Omega Nebula), and in the Orion Nebula.

"Rumors floating around that we looked for this line because Dr. Heeschen promised 2 (two) bottles of champagne for its discovery, we consider as an attack on our scientific integrity. Offenders will be sued for at least 10 bottles of champagne. (Incidentally, Dave, we have not seen any of the champagne yet)."

B. Hoglund and P.G. Mezger
in *The Observer*, July 30, 1965

The Discovery of Radio Recombination Lines *

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1. A Salute to the 140 Foot Telescope

On a foggy West Virginia morning my family and I arrived at the White Sulphur Springs Railroad Station. I had responded to an invitation by Dave Heeschen for a one year stay. Brown Cassell, the famous NRAO driver, picked us up and drove us to Green Bank. The first thing we saw of the NRAO while driving from Minnehaha Springs was the 300 Foot reflector glimmering in the morning sun. Brown made a breakfast stop at the Observatory Cafeteria, then he drove us to a newly built house located in what was later known as the "German Street" because the Mezgers, Stumpffs and von Hoerners lived therein. Actually, one of the von Hoerner boys put a board up at the street entrance imitating the one at Checkpoint Charlie: "Caution, you are leaving the American Sector."

Across the street there was a horn antenna called alternately the "Little Big Horn" or "Findlay's Folly." And farther away, separated by a little valley with a creek at its bottom, there loomed the concrete pier of the 140 Foot like a giant surfacing submarine.

A couple of weeks later I was already deeply involved in calibrating the 85 Foot and 300 Foot telescopes. Each time I went to one of these telescopes I came close to, or actually had to pass, the 140 Foot telescope. The reflector—made of aluminum—was assembled on the ground with its face down on the ground. Its sturdy hour axle was lying aside. But the whole of all these parts together conveyed the impression of power and reliability. I could not help but fall in love with this telescope. Most of my career in radio astronomy was devoted to extending observations to ever shorter wavelengths. Five years before, Wilhelm Altenhoff, Heinz Wendker, Gart Westerhout and I had completed a survey of the Galaxy at 11 cm wavelength using the just-commissioned Bonn 25-m telescope, which is located on a 400-m high mountain named "Stockert" about 40-km south-west of Bonn. Now with the 140 Foot Telescope—once it was completed—I saw the chance to extend this type of observations to shorter wavelengths. I also knew Frank Low was working on bolometers for mm observations, saw him testing these devices in the backyard of the office building, and I learned from John Findlay of plans to construct a 36 Foot telescope for radio astronomy at mm-wavelengths. I felt that I had arrived at the right place. When Heeschen offered to let me stay I told him I would. My wife Barbara was not overjoyed by this decision.

2. Thermal Radio Emission From Our Galaxy

Jansky, in the early 1930's, had discovered cosmic radio radiation at the wavelength of 14.5m, but for the next two decades its origin remained a puzzle. Free-free emission could be discounted since the observed radiation temperatures were much

* Presented at the 140 Foot Birthday Symposium, September 1995.

higher than the canonical electron temperature of an HII region of $\sim 10,000$ K. It was only twenty years after Jansky's discovery that the German astrophysicist Karl-Otto Kiepenheuer hypothesized that synchrotron emission might be the source of this cosmic radio emission at decameter wavelength. But it was also clear that free-free radio emission had to be present since one actually observed HII regions at optical wavelengths around hot and luminous OB stars.

In the late fifties Gart Westerhout, then one of Oort's graduate students at Leiden Observatory, had surveyed, as part of his thesis, the Galactic radio emission at λ21 cm using the Dwingeloo 25m telescope. He hypothesized that synchrotron emission would dominate at meter wavelengths but that it should have a rather steep spectrum: $S_\nu \propto \nu^{-0.7}$. The spectrum of optically thin free-free emission, on the other hand, was known to have a frequency dependence of $S_\nu \propto \nu^{-0.1}$. Hence, free-free emission should dominate at dcm and cm wavelengths. To check his hypothesis he combined his survey with another survey at 2.5m wavelength previously made by the Australian astronomer Bernie Mills but with comparable angular resolution. In this way Westerhout succeeded in separating synchrotron and free-free emission. Two years later, with Westerhout, we did a similar separation of thermal and non-thermal emission using our 11cm survey and confirmed Westerhout's results: at wavelengths much lower than 10cm free-free emission makes the dominant contribution to the Galactic continuum emission.

This opened a new and fascinating view of the Galaxy. HII regions form around short living, luminous O-stars which thus can be used to trace the birth-places of stars. The German astronomer Baade, making good use of the enforced black-out of southern California during World-War II, had discovered the two different populations of young and old stars which together make up the stellar content of galaxies. Specifically, he had shown that spiral arms are inhabited by Pop I stars and thus must be places of recent star formation. Dust prevents optical observations of HII regions in the Galactic plane which are much farther away than a few kpc. Radio emission, on the other hand, is not at all affected by dust extinction. Hence, we concluded if one only had a distance indicator for the HII regions discovered by their radio free-free emission, one should be able to trace out the spiral structure of our Milky Way Galaxy.

What distance indicators could that be? Thanks to the outstanding work which Jan Oort's group at Leiden did trying to trace out the spiral structure of our Galaxy with the recently discovered 21cm line of atomic hydrogen, we knew that spectral lines and their Doppler shift, together with the differential Galactic rotation, could be used to determine "kinematic distances." But what spectral line could be emitted by a plasma which consisted mainly of electrons and protons?

3. The Discovery of Radio Recombination Lines— My Personal Story

The discovery of the λ21cm line by Ewen and Purcell promoted radio astronomy from a ham-job to a respectable branch of astronomy. Many astronomers started to think about the possibility of detecting other spectral lines in the radio regime. The first thing every one of them considered were the recombination lines from hydrogen and hydrogen-like atoms. The basic theory of the line strength and frequency is simple and any graduate student in physics familiar with Niels Bohr's model of a hydrogen atom could actually do it. Protons and electrons of a thermal plasma recombine after a certain characteristic time, preferentially at the highly excited energy levels. Subsequently the electrons cascade from one level to the next lower level until they reach the ground state with the principal quantum number n=1, emitting at each jump from the level n+1 to the level n a photon of inverse wavelength $\lambda^{-1} = R[1/\mathrm{n}^2 - 1/(1+\mathrm{n})^2]$. Here R=109,677.6 cm^{-1} is the Rydberg constant. Substitution of principal quantum numbers n≥70 yields wavelengths $\lambda \geq$ 1.6cm. Of course, all the big shots in astrophysics had thought about radio recombination lines but unanimously came up with the answer "forget it, these lines will be completely washed out by impact broadening." And because they were big shots every intelligent observer followed their verdict and kept his hands from radio recombination lines.

But sometime in the late fifties, when we still were preparing for the 11cm survey with the Stockert telescope, I read a paper by Nicolai Kardashev, a student of the famous Russian astrophysicist Shklovsky. He had persuaded Kardashev to have another close look at the recombination spectrum of hydrogen. Kardashev did what he was told, but computed a considerably smaller, fractional bandwidth and thus—unlike his predecessors—arrived at the conclusion that these lines should be observable.

Reading Kardashev's paper I was fascinated. If these lines could actually be observed one could get both the kinematic distances of HII-regions and, from the line-to-continuum-ratio, the electron temperature of the plasma, the most important parameter characterizing the physical state of the ionized gas.

I went to Heeschen and proposed to build a 6cm receiver with a parametric preamplifier to test the 140 Foot Telescope once it was finished, to do a survey of the Galactic continuum emission, and to search for the H109α radio recombination line. Heeschen nodded approval and soon my newly hired technician Neil Albaugh and I worked on the frontend—actually only from Monday through Friday, since during the weekends Neil had a stint as Heeschen's car racing mechanic. Bertil Höglund, a Swedish post-doc, joined our team and matched to our frontend a multi-channel backend, which he had built for extragalactic 21cm observations with the 300-Foot Telescope.

The story of the completion of the 140 Foot Telescope will certainly be told by someone else. So we make a jump in time from 1964, when the 140 Foot reflector still sat on the ground facedown, to the summer of 1965, when the completed telescope could be seen in all its beauty. The 6cm receiver was ready and we moved it to the telescope, installed it in the prime focus and tested the continuum system and then—with it—the 140 Foot Telescope. This took us the better part of a week. We calibrated the aperture efficiency of the 140 Foot Telescope; it came up to our expectations. Then we tested the line system, found a bad connection in the LO system and fixed it, also found that the baseline depended somewhat on the continuum level and therefore we added a noise-injection device. Finally, on July 9th we made a final test of the line system and—since everything seemed to work—we decided to try it in the following night on two Galactic HII regions, M17 and the Orion Nebula. Within minutes we could see a signal on the pen recorder. After ten minutes of integration Bertil carried the data, stored on punch cards, over to the computer, which then was located in the warehouse. He returned half an hour later with the print-out. I could see that he was excited and after I had a

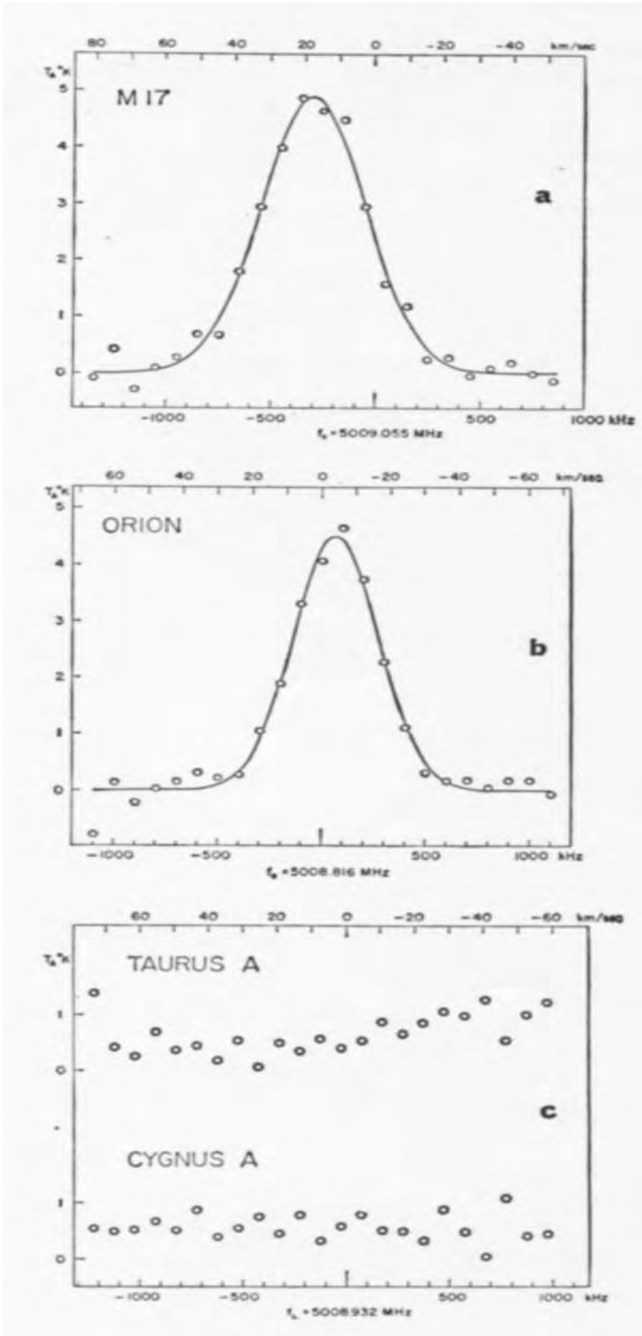


Fig. 1— First observations of H109α radio recombination lines with the 140 Foot Telescope in Green Bank, WVa., by B. Höglund and P. G. Mezger, 1965.

look at the data I got excited too: two lines (shown in Fig. 1 a and b) stuck out of the free-free continuum like sore thumbs. Incredulously we looked at each other: why had these lines not been detected earlier? Or was something wrong with our spectroscopy system? To check this possibility we pointed the 140 Foot Telescope first at Taurus A, a supernova remnant and then at Cygnus A, a radio galaxy, and were enormously relieved when we saw a flat baseline on the print-out (see Fig. 1c) as would be expected for a source with predominant synchrotron emission. "The system worked" was our conclusion "hence the lines in M17 and Orion A must be real." In the early morning I went home to German street and that evening Barbara and I celebrated the discovery of the H109α line with some glasses of Mogan David kosher wine, the only bottle we could get at the Cass Liquor store. The celebration ended with a hangover—then a wide-spread phenomenon amongst the NRAO staff.

There followed some hectic weeks. Heeschen suggested that I should report on our work at the next AAS meeting. Someone pointed out that Russian groups had already announced a year earlier at the 1964 Hamburg IAU General Assembly meeting the detection of the H104α line and of the H90α line. We had a look at their results (Fig. 2) and understood why nobody paid much attention to them. A couple of weeks after we had announced our discovery the Harvard group led by Ed Lilley tuned their maser—built for observations of the OH 18cm line—to the H156α and 158α recombination lines and detected them in due course. From there on, first the Harvard twins Palmer and Zuckerman, and then the MIT twins Reifenstein and Wilson joined our group and did their thesis work on topics as different as the spiral structure of the Galaxy, element abundances, and maser effects, using radio recombination lines as a tool.

Let me return to the incredulous Bertil Höglund and Peter Mezger looking at the H109α lines asking themselves "why have these lines not been detected earlier?" The answer has already been given at the beginning of my spiel: "Since all the big shots told the observers that the lines could not be observed due to excessive line broadening." Why they had this negative outlook will be discussed in more detail in the next chapter. Here, let me just tell you the end of my story. Sometime in the fall of 1965 Gart Westerhout, then Chairman of the Astronomy Department of the University of Maryland, invited me to College Park for a colloquium. When I told him about the line broadening problem he introduced me to Hans Griem, a specialist in this field. Hans listened politely to my problem, although he was short of time and then suggested: "Why don't you come to my office tomorrow morning. I think I may then have solved your problem." Next morning I dropped by and saw Hans' desk loaded with sheets covered with the most complex quantum mechanical equations. "Your problem seems not to be quite trivial" Hans told me modestly. Actually, Hans Griem appears to have solved the problem only two years after our conversation, when he finally published his results. The solution, however, turned out to be simple: an electron impacting on a recombined H-atom disturbs the energy level of the orbiting electron significantly and thus seems to support the earlier predictions of severe impact broadening. But—and this result was new—it disturbs two adjacent energy levels by about the same amount. Hence, when the electron jumps to the next inner orbit the energy of the released photon—which is the difference between the two energy levels—is considerably less affected than if the outer orbit were more strongly disturbed than the inner orbit.

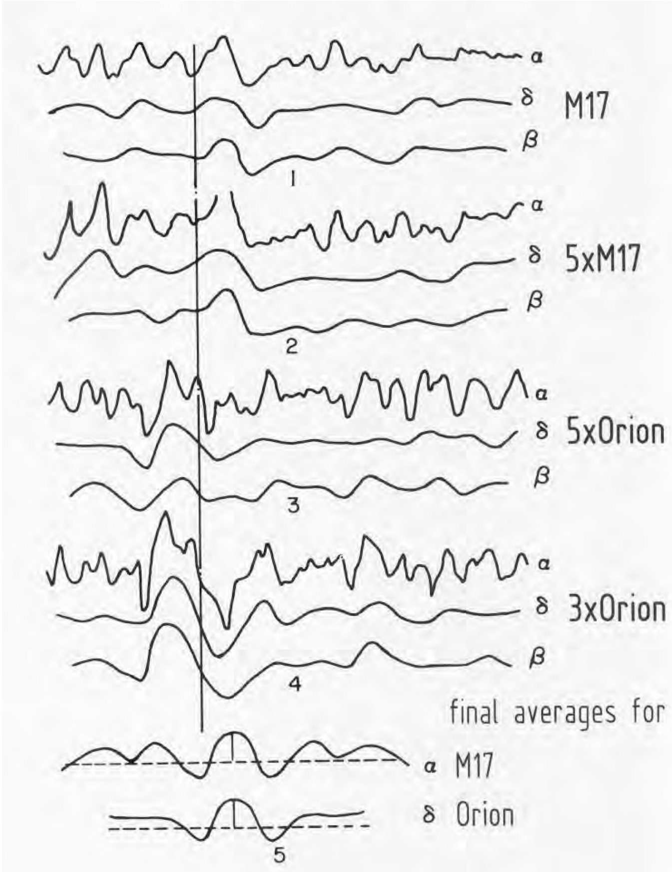


Fig. 2— The initial detection of H104α radio recombination lines by Dravskikh and Dravskikh in 1964 from M17 and Orion. Shown are various stages of smoothing and averaging.

4. The Discovery of Radio Recombination Lines — the Story Told by James Moran

In an after-dinner talk, published in the proceedings of a 1993 Ringberg Symposium on "The Nuclei of Normal Galaxies" Jim Moran gave an excellent historical review of the prediction and the discovery of radio recombination lines. As already stated above, the different estimates of the line broadening turned out to be crucial for the predictions by various investigators as to whether radio recombination lines could be observed or not. Rather than go into the details I show Moran's diagram (Fig. 3) but take the liberty to substitute "Griem" for "Brocklehurst and Seaton" since Griem published the correct result five years ahead of Brocklehurst and Seaton. Van de Hulst, in his seminal 1944 paper on the λ21cm hyper fine structure line of atomic hydrogen, also had a look at Rydberg transitions and used the Inglis-Teller relation to compute a fractional line width with the frequency dependence $\Delta\nu/\nu \propto \nu^{-5/3}$. According to Sullivan (as quoted by Jim Moran) van

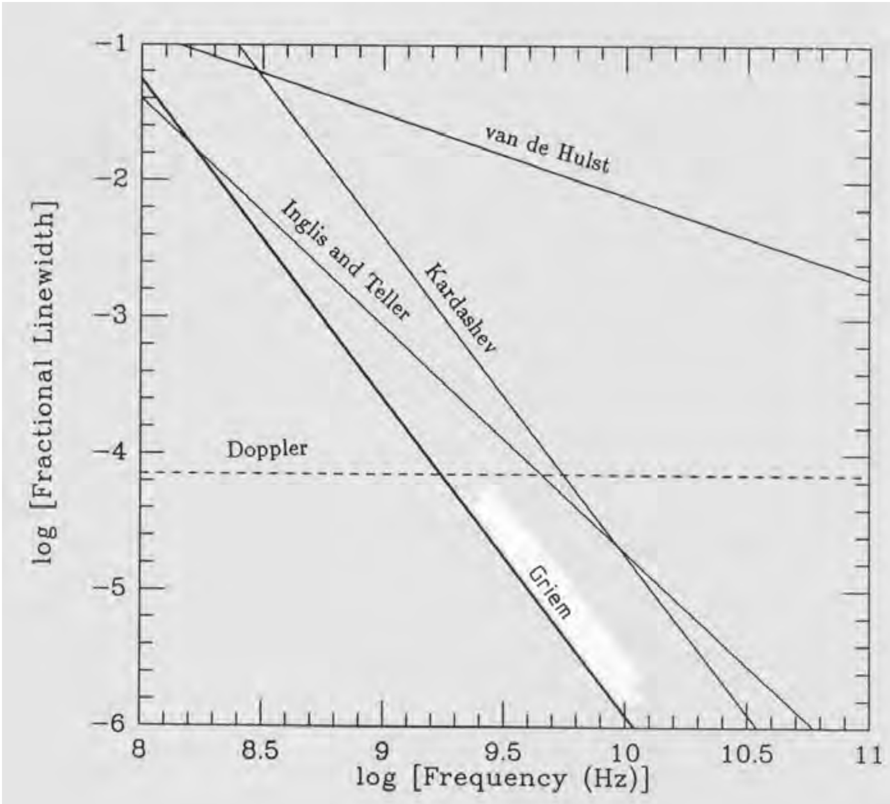


Fig. 3— Graphical representation of the fractional linewidth $\Delta\nu/\nu$ as derived by different authors for radio recombination lines (from J. Moran, 1994, in The Nuclei of Normal Galaxies, ed. R. Genzel and A.I. Harris, Kluwer).

de Hulst transcribed the exponent incorrectly and therefore published a frequency dependence of the fractional line width of $\Delta\nu/\nu \propto \nu^{-3/5}$. While the van de Hulst relation for $\nu \sim 5$ GHz, the approximate frequency of the H109α line, overestimates the line width due to impact broadening by a factor of ~ 2000, Kardashev's estimate is still off by a factor ~ 200 relative to the correct formula derived by Hans Griem.

Moran then goes on and tells the story of the discovery of the radio recombination lines from a more elevated point of view. The facts on which his narrative is based are in essence the same which Pat Palmer had assembled for a review talk in 1967 at the first (and to my knowledge also the last) Cornell-NRAO symposium on Interstellar Ionized Hydrogen: Dravskikh et al., and Sorochenko and Borodzich did their observations of radio recombination lines obviously before July 9, 1965, but their results were so marginal (see Fig. 2) that they were generally ignored.*

* I wanted to do some literature research myself but discovered that in our library the Soviet Physics-Doklady only begins at 1968.

5. Lessons to be Learned

Looking back on the summer of 1965 I feel that I have learned four important lessons:

1) Although the 140 Foot Telescope certainly was no engineering masterpiece it was the right telescope at the right time, the instrument which opened up the cm-wavelength range for radio astronomy. Ergo: Better an imperfect telescope at the right time than a perfect telescope ten years too late!

2) Don't give up doing an interesting observation because theory predicts you won't succeed. In one of his songs Tom Lehrer says: "Life is like a sewer, you get out what you put in." The same holds for astrophysics: If you enter into a theory a wrong assumption you will get out the wrong answer.

3) Give a competent project group enough time to adjust and test out their equipment at the telescope (I don't want to go as far as my friend Martin Harwit who is of the opinion that observers, using facility instruments, never have achieved a scientific break-through).

4) The fourth lesson relates to an after-dinner talk by Dave Heeschen at the same 1993 Ringberg meeting where Jim Moran gave his historical lecture. Recalling my US career in radio astronomy Dave Heeschen sees the quality of the local liquor stores as its pivotal points. He has a good point: After the celebration of our discovery of radio recombination lines with Mogan David kosher red wine in the evening of July 10, 1965, Barbara and I decided to switch the liquor store and drive as far as White Sulphur Springs rather than continue to live on Mogan David's products. This led me to discover California Napa Valley white wine, a brand to which I still adhere, although Bonn is surrounded by vineyards and wine factories.

Dr. and Mrs. Peter Mezger at the 140 Foot Birthday Symposium.

Ode to a Ski-Tow

From an unsigned article in The Observer, March 31, 1965.
(probably by Ivan Pauliny-Toth)

By Hannah's manse did Dr. M
A fiendish rope device decree
Where Deer, the sacred river ran
Through valleys measureless to man
Down to a sunless sea.

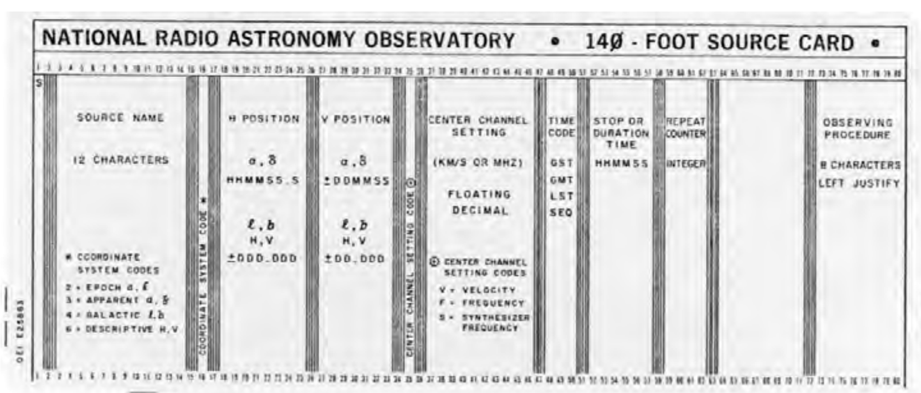
So three score yards of fertile ground
With poles and ropes were girdled around:
Upon this slope, dug with pneumatic drills
There blossomed many a car-wheel-bearing tree;
Here was a motor, ancient as the hills
Puffing black smoke upon the scenery.

After a sacrifice, and a prayer chanted
By the Winter Sports Committee, the gods of weather granted
To the green hill its ermine, snowy cover.
Straight to the spot the skiers came, enchanted
Like a woman searching for her demon lover.
Soon, with a mad, meandering motion,
Up and down hill the demon skiers sped,
Sometimes on skis, more often hands, elbows, knees and head
Making deep trails of blood and suntan lotion.

And 'mid the tumult, far-sighted men could see
Cars crowding by the doctor's surgery.
They would they cry: Beware, beware
The skier's eyes, his flashing hair!

Depart from him, all in a trice
And close your eyes in holy dread,
Better at home, in your warm bed
Than at the gates of Paradise.

Left to Right: Joe Taylor, Tom Wilson, Pat Palmer at the 140 foot birthday symposium, September 1995. [photo courtesy R.Rood]



A computer card for giving source positions, used in the 1970s.

The Green Bank 6 cm Recombination Line Survey*

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1. Introduction

Nearly all accounts of the history of radio recombination lines begin with the publication by Kardashev in 1959. In this paper, Kardashev predicted that such lines would be emitted from HII regions in the radio wavelength range. Kardashev also gave estimates of the line intensities based on the assumption that these lines are emitted in Local Thermodynamic Equilibrium. Next, there were reports of the detection of such lines by Dravskikh, Dravskikh, Sorochenko and assorted coworkers (see the account by Palmer 1968). However, the signal-to-noise ratios of these results were rather low. There was also an (unpublished) unsuccessful attempt to detect radio recombination lines with the Parkes 210 foot radio telescope following the announcement of the first observations. The first convincing detections were made with the NRAO 140 Foot Telescope by Höglund and Mezger (1965). Following this publication, a number of groups started to carry out follow-up programs. One was related to cosmology: this would provide a measurement (in addition to optical determinations) of the ^{4}He to hydrogen ratios for HII regions. A second program was the search for radio recombination lines in extragalactic sources. A third program, the one I will present at length here, was a large survey of hydrogen radio recombination lines in Galactic HII regions.

Before going further, I will summarize the state of our knowledge of radio recombination lines in 1967. The first measurements by Höglund and Mezger (1965; 1967) indicated that the electron temperatures T_e, that is, the temperature characterizing the Boltzmann distribution of electrons (and protons), was of order 5000 K. The optical data from forbidden lines gave 10^4 K. The differences were statistically very significant; the question was whether the radio data, optical data, or both, were affected by systematic errors. Because of this, it was unclear whether the radio results provided good estimates of the electron temperatures, T_e, of the HII regions. It was suggested by Peimbert (1967, see also Peimbert et al. 1993) that both radio and optical electron temperatures were correct, but that the differences were caused by fluctuations in T_e, with the radio results biased toward lower electron temperatures, and the optical values biased toward higher values of T_e. In another approach to the interpretation of the T_e problem, Goldberg (1966) made an analysis of radio recombination line emission which showed that two effects determined the populations of high principal quantum number energy levels of hydrogen. The first was an underpopulation of the hydrogen energy levels because of the allowed radiative decay to lower levels. This effect depends only on the local density. For electron densities larger than 5000 cm^{-3}, this effect is small for energy levels with principal quantum numbers, n, larger than 50. The second effect is much

* Presented at the 140 Foot Telescope Birthday Symposium, September 1995.

more complex. This involves a population inversion of these levels. This inversion is caused by the fact that levels with smaller principal quantum numbers, n, radiatively decay more rapidly than levels with larger n. Also, the effect of collisions, which brings populations closer to LTE, is smaller for levels with smaller n values. This causes the lower level to be underpopulated compared to the next higher level. For n of order 100, the statistical weight factors for the upper and lower levels are nearly equal. Thus even a small overpopulation of the upper level requires negative excitation temperatures, or, in other words, population inversion. However, population inversion alone will not necessarily lead to observable effects. For this, there must be a sufficiently large number of photons or equivalently, a sufficiently large optical depth. Since most of the photons in the centimeter wavelength range arise from continuum radiation, the question was one of optical depth in the radio continuum, or (for a given T_e) emission measure (in units of parsecs cm^{-6}). Since continuum optical depth decreases with frequency squared, line masering should be a smaller effect at higher frequencies. Also, since masering is favored in a low density, high emission measure HII region, the importance of masering depends on the structure of HII regions. We will return to this in the Epilogue.

In addition to the T_e problem, the observed linewidths did not agree with theory. The principles were clarified by Griem in 1967. The basic idea is that impact broadening affects both the upper and lower levels involved in a transition, so the widths are smaller than would be expected on the basis of a simple calculation. Thus the broadening varies as the product of electron density and principal quantum number, n, to the 4th power, but the numerical coefficient is smaller than expected. Still, even at frequencies of a few hundred MHz, there was no clear evidence for the expected collisional broadening, but a few observers suspected that noise might be masking the collisionally broadened line wings. In summary, in 1967, the interpretation of electron temperatures and line widths were thought to be understood in principle, but there were disagreements between observation and simple theory. Basically, the three most important effects—non-LTE line formation, collisional broadening and source structure—were understood in principle, but the relative importance of each was unknown. As it turned out, a self-consistent analysis of the radiative transport was also needed (see the Epilogue). In contrast, it was generally accepted that the excitation of helium recombination lines should be the same as the excitation of the corresponding hydrogen recombination lines, so helium-to-hydrogen ratios would be reliable. Also, even though the line broadening mechanisms were not fully understood, the radio recombination lines could be used to map the turbulence as well as the velocity structure in obscured HII regions. Longer discussions of all of these topics are contained in the Symposium on "Interstellar Ionized Hydrogen" (Terzian 1968).

2. The Green Bank Survey

I became involved with radio recombination line work in late 1966, as one of a number of students from M.I.T. who were occupied with radio astronomical measurements made using the Haystack 37 meter antenna of Lincoln Labs. The most senior of these students was R.J. Allen who was a PhD student of Alan Barrett's. Ron Allen was pursuing studies of continuum sources. The students most involved with spectral line work were Alan Rodgers and Jim Moran. I was certainly the most inexperienced of all. I had just started a Ph.D. thesis with Prof. B. F. Burke.

Ted Reifenstein was another student working on a Ph.D. thesis with Burke, and he later became the main person involved in the 6 cm recombination line survey made using the 140 Foot Telescope. I was first involved with an accurate determination of the continuum flux density of W49 at 7.8 GHz (Burke & Wilson 1967). This measurement was in response to a request by P.G. Mezger, who suspected that there were high emission measure, compact HII regions in the W49 complex. This work gave rise to the concept of "compact HII regions," as reported by Mezger et al. (1967).

The measurements with Haystack showed us that use of this telescope for a recombination line survey of perhaps 100 sources would not be easy. Partly this was a problem of getting sufficient support for the project, including, among other things, getting enough telescope time, overcoming the rather low antenna efficiency of the Haystack telescope, and, most importantly, obtaining a high resolution continuum finding list for the radio recombination line survey. The only continuum catalog widely used was that of Westerhout (1958), which had a $36'$ angular resolution. Altenhoff et al. (1961) published a survey with an $18'$ resolution, but even this was too coarse a resolution for our purposes, since we would have an angular resolution of about $6'$. For a detailed survey, we needed an appropriate continuum source finding list before starting. At this point, P.G. Mezger informed us that W.J. Altenhoff had carried out the first part of a continuum survey with the 140 Foot Telescope and was in the process of reducing the data in Charlottesville. This survey had an angular resolution of $11'$, so was a reasonably good finding list for the recombination line survey. Peter Mezger came to Cambridge, Massachusetts, and there was a negotiating session. The Harvard radio astronomy group had a filter backend which we needed. Although NRAO had a 100 channel autocorrelation spectrometer, Peter Mezger did not trust this device. He did trust the Harvard filter bank because the Harvard astronomers had beaten him to the discovery of the helium recombination line by detecting this with the 60 foot Agassiz station radio telescope. In return for the use of the filter bank, the Harvard astronomers wanted some of the telescope time. Since Bernie Burke, Ted Reifenstein and I were most interested in that part of the Galactic plane with a Galactic longitude less than 50^{o}, the Galactic anticenter time, that is, when Orion A, Orion B and W3 could be measured, was available. This was given to the Harvard astronomers (and P.G. Mezger) after some negotiating. These observations were to be carried out by Pat Palmer and Ben Zuckerman. Hays Penfield came to Green Bank to install the filter bank.

Ted Reifenstein and I arrived in Charlottesville to help reduce the continuum data. We were cautioned that this would be a preliminary reduction, and the final maps would be produced later. My first vision of Wilhelm Altenhoff was in the NRAO conference room with a chart record spread on a large table, and the largest roll of millimeter graph paper I had ever seen. Wilhelm Altenhoff was wearing watchmaker's magnifying glasses that slipped on over his normal glasses. This made almost as much of an impression as the big roll of graph paper. Since there was little digital data (due to magnetic tape problems) we had to reduce the data by hand. The process consisted of first establishing baselines with a large ruler, then measuring offsets with Gerber variable scales (basically stretchable linear scale rulers) and recording the contour levels on the large section of millimeter paper, again with variable scales. This was my first exposure to real astronomy! A partial map of the Galactic plane, from Galactic longitude 350° to 50°, was made in two weeks, working 12 hours per day, every day. Then we picked out the more compact sources and made up a source finding list. This was the status at the end of 1966. This survey was later partially repeated and re-reduced, appearing with two other 11′ resolution surveys at 21 cm and 6 cm as Altenhoff et al. (1970).

The next step was the recombination line survey itself. Peter Mezger, Bernie Burke and the Harvard astronomers applied for, and received, 6 weeks of telescope time. This was to be the first part of the survey. Alan Rodgers warned me that there would be trouble if we did not bring our own engineer. In a 140 Foot Telescope run at 18 cm, Pat Palmer and Ben Zuckerman lost an entire week of telescope time because of an unnoticed receiver problem. I mentioned this to Bernie Burke, and he replied "Why do we need an engineer, I'll be there at the start of the run to help get started!" He was, as was Peter Mezger, but there were still troubles, resulting in a two-week loss of time. Bernie had many other responsibilities and had to travel back and forth frequently during this period. Fortunately, Sandy Weinreb arrived in Green Bank a little before this time. I cannot judge whether Sandy's presence was the main reason for the success of the Green Bank Electronics Division, but there is a strong correlation between the arrival of Weinreb, Mike Balister and John Payne, and the presence of well-functioning receiver systems. Back to our observing run: We needed two weeks until the stability of the system was significantly improved. After this, we were able to measure a large number of sources. However, at the end of this first period, we estimated that *another* 6 weeks of telescope time was needed. Also, we needed a new spectrometer. The Harvard filter bank would not be available, since there was some friction between Peter Mezger and the Harvard astronomers.

During this first observing period I was at the telescope when Sandy Weinreb discovered instrumental baseline ripples. He showed up one Sunday morning, asked for some telescope time, which I gave him, and then started to move the axial focus of the telescope. We pointed the telescope to a strong continuum source and both noted that the slope of the baseline changed dramatically with axial focus position. Sandy Weinreb just said "Just as I expected" and proceeded to explain the effect to me. Finally, the next day, Sandy came and told me that the effect was not at all "new," but was in the M.I.T. Radiation Lab series. Thus this was a rediscovery, but a fundamental one. Instrumental baseline effects are the ultimate limit to the detection of wide spectral lines whose peak intensities are less than a few percent of the continuum intensities. This was a solution to the mystery of why no one could find collisionally broadened recombination lines. There is a summary of this effect in Bania et al. (1994).

In the summer of 1968, the NRAO filter bank was constructed. This was mostly Ted Reifenstein's project; Ted had become the key person concerned with the handling of our 140 Foot survey data, and became the main contact person for the NRAO spectrometer because of his great store of knowledge of software and hardware techniques. I had a role in the data reduction of the first 140 Foot observing run and also was impressed into soldering wires in the NRAO filter bank multiplexer. The filter bank itself consisted of fifty contiguous 100 kHz filters. This was good enough for our survey. This same filter bank was the spectrometer at the NRAO millimeter telescope on Kitt Peak during the period when a large number of molecules was discovered.

On the basis of the first set of results, the commitment of all involved, and doubtless some persuasion, we received another 6 weeks of time, in the autumn of 1967. The results from both observing sessions gave us about 100 spectra. From these data, we could obtain estimates of the radial velocities and electron temperatures of HII regions far from the Sun. Many of these were obscured, so could not be measured in the optical. The results were useful in determining the kinematics of HII regions in our Galaxy. The first comparison of the distributions of HII regions with HI gas gave a poor correlation (Kerr et al. 1968). Later, it became clear that the correlation with carbon monoxide was rather good; there is a large concentration of HII regions and CO emission between 4 and 6 kpc from the Galactic Center (Solomon & Scoville 1975; Burton et al. 1975; Bronfman et al. 1988).

When the 140 Foot data (Reifenstein et al. 1970) were combined with data taken for that part of our Galaxy observable from Australia (Wilson et al. 1970), one could just barely find a gradient in the electron temperature, T_e, with distance, D_{GC} from the Galactic center (Churchwell & Walmsley 1975). Such gradients were found for a number of external spiral and irregular galaxies in 1972, but this was the first evidence for such an effect in our Galaxy. With better data it became clear that this gradient is very significant compared to the noise. Fig. 1 contains a plot of data taken in the 1980's. This showed evidence for the T_e gradient; such a trend *was* present in the 140 Foot survey, but was not very obvious because of the signal-to-noise ratios in the data.

If you look at the signal-to-noise ratios in first detections of radio recombination lines, it is difficult to believe that a survey of 200 sources could be carried out and published less than 5 years later (Reifenstein et al. 1970; Wilson et al. 1970). The corresponding survey of the southern sky with the Parkes 210 foot radio telescope would not have occurred without the success of the 140 foot survey. In addition to these surveys of small diameter sources, there were surveys of recombination line emission from more diffuse HII gas (Lockman 1976). Still later came the more detailed survey with the 100 meter telescope (Downes et al. 1980), which gave results similar to those found in the Green Bank data. For me, there were a number of philosophical lessons. First was the realization that every result quoted in a text book was usually the outcome of a large investment in money and time. Second, the uncertainties about the relation of LTE and actual electron temperatures showed me that there can be disagreements in the interpretation of data even if many of the relevant physical principles are reasonably well known. Basically, there are a number of parameters in any astrophysical situation. These determine which of the many physical effects have the dominant role. A number of the parameters can be determined, but not all. In this case, one must collect a large amount of data, wait until the measurements are significantly improved, or wait until some new concept is employed. The first two approaches were used to attack the unanswered questions associated with radio recombination lines observationally; the last concept was used to refine models of the line transfer.

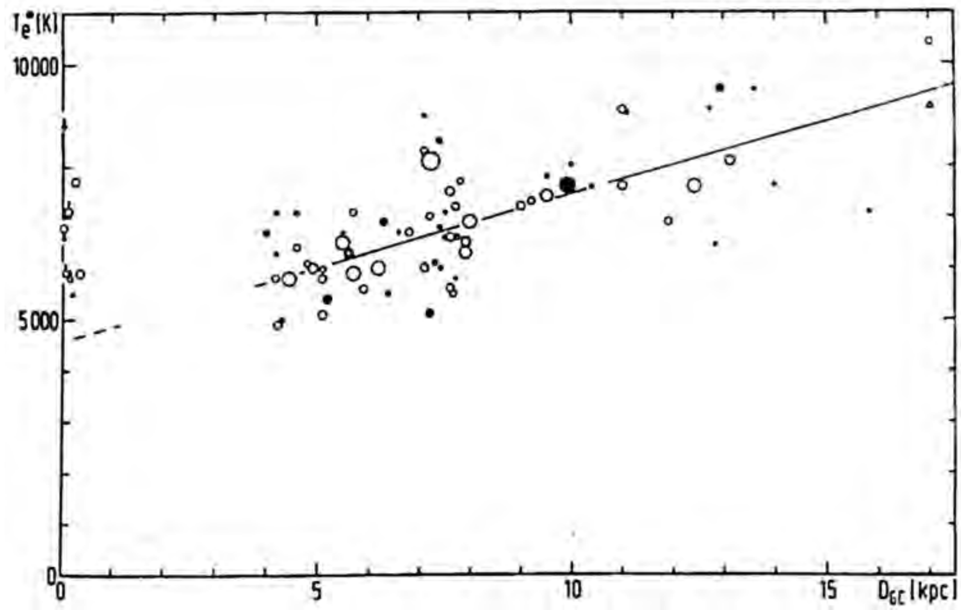

Fig. 1— A plot of H76α line data (wavelength 2 cm) for HII regions with definite distances. These results were taken with the 100 meter Effelsberg radio telescope by Wink et al. (1983). The different symbols indicate sources with different excitation parameters, which are an indication of different luminosities. The sources in the disk of our Galaxy show a clear trend for an increase of electron temperature, T_e, with increasing distance from the Galactic center, D_{GC}. This trend is not present for Galactic center sources.

3. Epilogue

The measurement uncertainties affecting electron temperatures were gradually eliminated, at least for HII regions like Orion A, in the late 1970's. After a large amount of data was collected in the period 1965-1970, these results were interpreted in a convincing way by Brocklehurst & Seaton (1972), who first recognized that one must account for the radiative transfer through the entire nebula, including both dense and diffuse gas, and that the layer nearest the observer could have a large effect on the line intensities. On the basis of such models, it became clear that measurements at short centimeter wavelengths were needed to determine accurate electron temperatures. By the mid-1970's, the data taking procedures, receiver sensitivities and stability, as well as telescope quality, allowed an order of magnitude improvement in the reliability of results. For example, the data presented by Pauls & Wilson (1977) and Wilson & Pauls (1984) showed that the radio recombination lines gave an electron temperature, T_e, of 8200 K for the $2'$ region in the core of Orion A. More than $4'$ from the core of this source, the electron temperatures decreased to ~6500 K. Presumably the cooling of the ionized gas in the core by lines of the elements carbon, oxygen and nitrogen, is quenched by the higher electron densities. A realistic model for the core of Orion A was proposed by Shaver (1980). This model uses a slab geometry, in which each slab has a different electron density but the same electron temperature. The geometry and comparison with data followed Lockman & Brown (1975), while the radiative transfer follows the approach of Brocklehurst & Seaton (1972). In this model the local electron densities are significantly larger than the densities obtained from the measured emission measures and source diameter. A generalization of this model was made by Wilson & Jäger (1987). In this model, the observed T_e variation is included in the model by means of slabs with different electron temperatures and densities. In these models, the differences between the LTE and actual electron temperatures were found to be less than 20 percent. That is, for Orion A, the effects of line masering, collisional broadening and sub-thermal energy level populations tend to balance each other. There are a very few exceptional cases, such as MWC 349, where strong masering occurs (Martin-Pintado et al. 1989), but these are truly exceptional.

Detailed studies of Orion A indicated that the results from large scale surveys of electron temperatures gave physical results. However, there is still a report that the electron temperatures determined from optical forbidden lines are significantly different from those determined from radio recombination lines (see, e.g., Peimbert et al. 1993). Such a difference, if correct, indicates significant electron temperature fluctuations, and would affect element abundances obtained for HII regions. If one assumes that radio recombination lines provide accurate electron temperatures, then the data show a gradient in T_e with distance from the Galactic center. For example, Wink et al. (1983) carried out measurements of a large number of HII regions in the H76 α line, finding a gradient of 270 K per kpc for sources in the disk. (The HII regions in the Galactic center did not fit this gradient, but there is little material in the gap between 3 kpc and the Galactic center region proper. Thus, the disk and center of our Galaxy could have evolved differently). One could interpret the gradient in T_e in terms of a gradient in elements which give rise to cooling lines in the HII region. A more direct approach was used by Shaver et al. (1983), combining optical and radio data and directly obtaining element abundances. However, the optical abundances are critically dependent on T_e values.

4. The Future

Looking a little into the future, more reliable element abundances can be made using measurements of fine structure lines of N and O, which occur in the far infrared. So far there has been only a small number of measurements, but this will change soon thanks to the Infrared Satellite Observatory, ISO. Then the N/O gradient, and perhaps the N/H and O/H gradients will be determined over the entire Galaxy and over other galaxies. With these data, more accurate models of Galactic chemical evolution will be possible. In addition, the ISO results will allow a more accurate study of the relation between T_e and element abundances in a large sample of HII regions. For the study of optically obscured sources, mapping of radio recombination lines with the Very Large Array is presently the simplest method to determine radial velocities on arc second scales. The interferometry of radio recombination lines was started by van Gorkom et al. (1980). For recent examples of VLA observations, see Mehringer et al. (1983) and De Pree et al. (1995).

The high angular resolution of the VLA also allows a more accurate determination of the ^{4}He to hydrogen ratios since the beam can be smaller than either the hydrogen or helium Stromgren spheres. In that case the difference in sizes enters linearly and not to the third power. From single dish maps of Orion A (Jaffe & Pankonin 1978; Pankonin et al. 1980) it was clear that the Stromgren sphere of helium is smaller than that of hydrogen, so that the ratios from single telescopes are an underestimate (see also Thum et al. 1980). The interpretation of the ^{4}He abundance is more complex than expected in 1967 since the stellar production will increase the amount of ^{4}He. Thus, systematic effects can either increase or decrease the ratio, so that single dish radio recombination lines are not a very useful method for obtaining cosmologically interesting ^{4}He results. The results usually considered most relevant for cosmology are obtained from optical observations (see, e.g., Pagel et al. 1992). It is clear now that species more sensitive to baryonic densities in the Big Bang, such as ^{3}He or deuterium, provide a better estimate of the initial conditions (see, e.g., Wilson & Rood 1994). However, as with ^{4}He, one must account for stellar production and/or destruction in order to obtain the primordial abundance.

A topic for the future is the search for extragalactic radio recombination lines. These were first found as a part of our survey with the Parkes 210 foot telescope in 30 Doradus. Later, theory gave possible measurements of high red shift objects a great importance (see Shaver 1975; the paper by Wadiak et al. 1983 summarizes the observations). So far, there have been only a few detections because of instrumental baseline effects. For high redshift sources, a combination of low frequency radio recombination lines, which are produced by stimulated emission, with optical Lyman lines, which are produced by spontaneous emission, would provide another method to determine the Hubble constant. The Green Bank Telescope may prove to be very useful in this research area.

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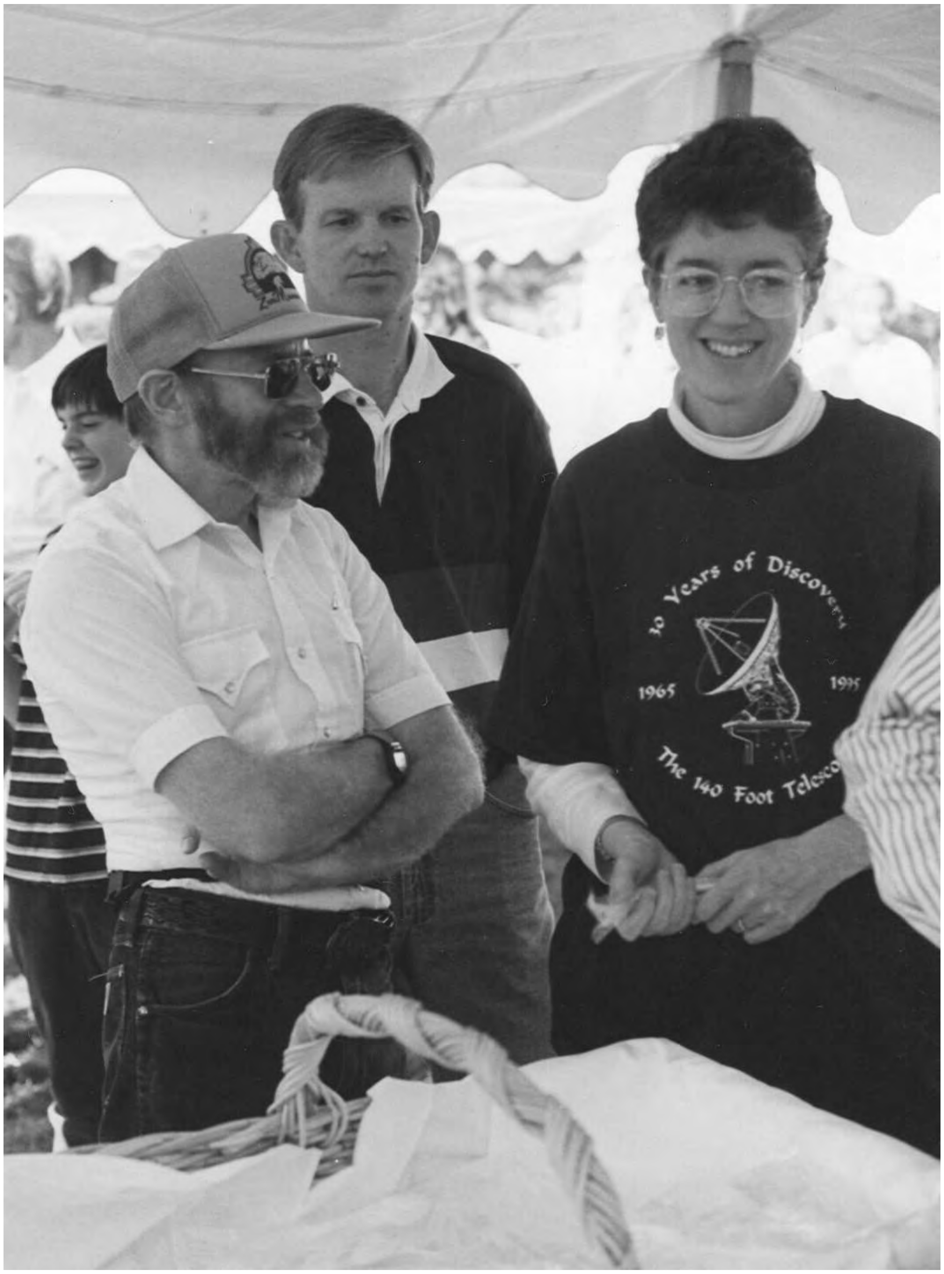
Carl Chestnut working on the receiver back ends in the 140 Foot control building, August 1990. [photo courtesy G. Liptak]



(L to R) Pat Palmer, Mark McKinnon, and Joan Palmer at the 140 foot birthday celebration in 1995. Joan is modeling the 140 Foot Birthday T-shirt.

Early Days at the 140 Foot Telescope: A Graduate Student's Perspective*

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1. Introduction

I used the 140 Foot Telescope for my thesis research. I continued to use the 140 Foot for many years, but in this account I will discuss only projects in which I was involved while a graduate student. At the conclusion, I will briefly comment on the differences between being a graduate student in radio astronomy 30 years ago and being such a student today.

2. A Graduate Student in the Mid-1960's

It is necessary to provide a bit of background that does not deal directly with the 140 Foot. I was a graduate student in Physics at Harvard University, and I collaborated in all of this work with a fellow student, Ben Zuckerman. We worked under the direction of Professor Ed Lilley and enjoyed the very able engineering support of Hayes Penfield. We helped develop two lines of study during this period: studies of recombination lines (some in collaboration with Peter Mezger, then at NRAO) and studies of maser emission of OH.

I first visited the NRAO in the Fall of 1964 when Ed and I flew down to make plans for observations of Mars around its 1965 opposition. Non-thermal emission had been reported from Mars at the previous opposition, and this report needed to be checked. It was a magical experience to fly into Green Bank in the Fall and to see the 300 Foot Telescope from the air. In roughly February – April 1965, I spent several months here taking data on Mars and made a long trip back to work on the reductions later in the Spring. During this time the 140 Foot was under construction. We didn't see any non-thermal emission from Mars, which actually didn't surprise us very much. While processing the Mars data, my wife, Joan, and I (along with our cat) were present for the first astronomical observation with the 140 Foot: Sebastian von Hoerner's lunar occultation. It is a little sobering to realize that I am now older than Sebastian was at that time – and Sebastian had seemed to me to be old then. One thing this illustrates is the youth of the entire field thirty years ago: most of the astronomers on the staff at Green Bank then were younger than 40.

In the Summer of 1965, I began working with Ben at Harvard's Agassiz station. At that time we had a 60 Foot telescope, a maser radiometer with $T_{sys} \sim 100$ K, and a 10 channel receiver. We observed the lowest frequency recombination lines to that date: 1424 MHz. These observations still showed no evidence of the pressure broadening which would determine the largest n value from which lines could be observed. In this paper we introduced the $\alpha, \beta, \ldots$ notation which caught on right away with the community.

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Line work was much more exciting than planetary work. In addition to the recombination line work, exciting developments were taking place in OH studies. "Anomalous" OH emission had been discovered in 1964 at Harvard and it was treated as a secret. As I understand it, Ed informed Alan Barrett, and both Harvard and MIT groups began programs with the newly-commissioned Haystack antenna. It turned out that OH emission had also been discovered at Hat Creek while following essentially the same strategy as used at Harvard—looking for absorption against the Westerhout sources—and that group moved faster to publication. Preprints reached everyone sometime in the Summer of 1965. As the OH emission studies progressed, one development after another emphasized how peculiar the phenomenon was. Clearly, this would also be something interesting to work on. I was soon hooked on line work.

In October and November of 1965, Ben and I had our first observations with the 140 Foot. We used a parametric amplifier with $T_{sys} \sim 190$ K and the then-new 100 channel autocorrelation receiver. This was long before the days of computer control of the telescope. We told the operator when to move from source to source and provided him with a hand-written schedule to use when we were asleep—which was not much of the time. Because of this regular communication, we got to know the operators very well. Data were recorded on tape for a day (at 600 BPI, I believe), and then the data needed to be run through two programs on the IBM 7040 computer to produce spectra of each scan. Each of these programs ran for about an hour, and only after this processing did we know if we had valid data for the preceding day.

We were originally scheduled for several weeks, but because of a sequence of problems, we got little valid data. It was extremely frustrating for us. Without Hayes' assistance, the extent of our ability to deal with instrumental problems was to identify how they impacted our observations and complain a lot. (The masses of flies that inhabited Hill House where we were staying that Fall did nothing to relieve the grimness of the situation.) I remember calling Art Shalloway at home on Thanksgiving day to tell him that the previous day's spectra were no good, and to ask him to look at the correlator so that problem could be fixed by the time we started taking data again at midnight. Art took all of this in good spirits, supported by his faith in digital engineering. I also remember complaining to him that sometimes all of the lights on the front simply lit up and the correlator stopped. After listening to our complaints in good humor (and probably politely dismissing them), he gave us a lecture on how wonderful it was to work with digital circuits because they either worked or didn't work—no ambiguities. And, I remember the look on his face as the correlator chose that moment to "do its thing" and freeze with all of the lights lit up!

Because of the succession of problems, our time was extended so that we had significant parts of six weeks. After our first week or so—in which we had accomplished very little—Alan Barrett and Alan Rogers arrived to look for circular polarization of the OH signals. (They had discovered linear polarization earlier at Haystack.) They arrived, and with Alan Roger's skill with the instrumentation, they detected circular polarization in very short order. We were chagrined that we had accomplished so little in our time while Barrett and Rogers had achieved a very significant result in a short time, but we were also excited by the discovery. Perhaps because of our embarrassment, we took special amusement one night when Professor

Barrett asked the operator why the telescope was not looking at his source yet: the telescope operator told him that it was because the 140 Foot couldn't look through the Earth! We extended our program to also measure the circular polarization of our sources as well. In May 1966 we spent another week or so at the 140 Foot completing and extending these results.

The points to which I would like to especially draw your attention are: 1) two relatively new graduate students received a really large amount of time with a new, unique instrument; 2) proposal pressure was not yet so heavy that time could not be extended (even very significantly) until we had results; and, 3) in the paper that emerged from this work, we used five separate results from Agassiz station—our university telescope also contributed significantly to the scientific quality of the final paper.

The next chapter was back at Agassiz. In February - April 1966 we made the first detection of helium recombination lines with the 10 channel receiver. It was clear that radio frequency spectroscopy could have an impact on a wide range of other fields—even cosmology. Somewhere around that time, Hayes Penfield started work on a new 21 channel filter bank receiver.

Both Ben and I were fascinated by the idea of detecting more new lines. In the Fall of 1966 we were back at the 140 Foot for two line searches. These were the first uses of the Harvard 21 channel receiver at the 140 Foot. We output the data onto punched paper tape and then converted the paper tape to punched cards. By that time the NRAO computer had moved to Charlottesville, so the procedure was to send the punched cards together with the card deck of the reduction program over to Charlottesville on the morning shuttle, and wait until the next day to see what emerged. Because the program was new (and written by me) it frequently failed in one way or another leading to another day's delay in seeing any results. (The modern generation that gets its data on Exabyte tapes does not have one advantage that we had with punched paper tape: if there was a parity error, we could find the extra hole and cover it with Scotch tape.)

We had about two weeks of observing time, and divided it between a search for lines from the $^2\Pi_{1/2}, J = 1/2$ state of OH and a search the H253α line at 403 MHz. The OH frequencies had not yet been measured in the lab, so we had to calculate them. That is fairly straightforward, and the formulae are given by Dousmanuis, Sanders, and Townes—except that the formulae contain a sequence of $\pm$ and $\mp$ signs. Which set of signs to use is explained in a long and convoluted sentence which seemed rather opaque. Because it was difficult to follow, we discussed it with Professor Townes who was spending a sabbatical at Harvard then. He seemed to indicate that we had read it correctly. (At the time we did not know him very well, so we may have missed a cue that he was politely trying to lead us to discover an error. Years later we learned what it means when he says: "I don't believe I understand what you are saying.") A parametric amplifier was available for the frequency in question, but T_{sys} was $\sim$ 600 K. We searched for a week and found nothing. The second week was devoted to the H253α line. For this search, we used a transistor amplifier with $T_{sys} \sim 600$ K. Because of the cumbersome computing situation, we had already returned to Harvard when we found that we had detected the H253α line in W80 (the North American nebula). For many years this stood as the lowest frequency recombination line detected. It still showed no evidence for pressure broadening, but for the low density of the North American nebula, this was to be expected.

As Tom Wilson mentioned in his contribution, our success in detecting the very weak helium line with a small telescope was because our receiver had very good baseline stability which enabled long integrations. The replacement of the 10 channel receiver with a 21 channel receiver led to an agreement between Harvard, MIT and NRAO. After some discussion in Ed Lilley's office, we divided things up as follows: the MIT group and Peter Mezger would do Galactic structure; the Harvard group and Peter Mezger would do a survey of helium recombination lines; and the Harvard group would measure some β lines to see if the α—β intensity ratio was the LTE one. The Harvard receiver would be used as the backend by all groups.

Finally in early 1967, a new 6 cm paramp was ready for the 140 Foot and we transported the 21 channel receiver there for the Harvard - MIT - NRAO survey. For this series of observations we used a paramp with $T_{sys} \sim 80$ K. Tom has spoken about the MIT - NRAO part. I will summarize the parts in which we were involved.

The helium work went very well. The lines were weak, but for the stronger sources, they were fairly easy to measure. The surprise was the detection of a narrow line near the position where helium was expected in NGC2024 – which showed little or no helium. We were puzzled but knew we had something very interesting. We returned from NRAO with the 21 channel receiver without telling Ed Lilley about this discovery because he was on vacation. We talked about it with Leo Goldberg—whose interstellar medium course we had audited (the closest I ever came to taking an astronomy course). Goldberg, who had worked on recombination lines – among many other things—since the late 1930's, was always very interested in radio recombination line work, and he got very excited about the new result. In a very short time he had come up with an explanation: carbon in the HII region. This was the only time Ed got angry with us: we should have tracked him down and informed him, and asked for more time to make measurements at higher resolution because the line was clearly unresolved in our 100 KHz filters. (We felt even worse because we knew he was right.) We requested observing time to make these observations and transported everything back to Green Bank. Goldberg's original model is no longer accepted, but the line's identity was correct. So, we had a new line.

Ben says that his first original thought in astronomy occurred when he and the 140 Foot operator were sitting at the telescope with Ben's feet up on the control console to the right of the operator. We just knew that the first (HII region) explanation of the carbon recombination line by Goldberg and Dupree couldn't be correct. "A light bulb popped on in my head as the Kleinmann-Low nebula came into view." We eventually published a paper suggesting that the high-frequency carbon lines originate from the outer layers of very dense HI regions. This, too, was controversial, and it took about a half dozen years to become generally accepted.

The measurement of the α—β intensity ratio was carried out in several nebulae. We were not too surprised to find that the ratio was not the LTE value, but implied that the α line temperatures published were underestimates of the true electron temperatures. This led to lengthy debates about what the temperature was, but we had established the important point that they were not as different from the optical values as suggested by earlier recombination line studies. Subsequently, speaking very generally, optical values have dropped somewhat and radio recombination line values are based on measurements made at smaller n, where the non-LTE effects are smaller. The discrepancy has pretty much disappeared.

In retrospect, as I shall return to later, one of the more interesting "sociological" things about the helium study was that our radio study contained a lot of discussion of optical helium determinations and a reanalysis all of the published optical values using uniform choices of atomic constants and reddening parameters, but it was many years before optical helium studies discussed radio ones.

By the end of 1967 we had learned from Lit Meeks that the $^2\Pi_{1/2}, J = 1/2$ line frequencies had been measured in the lab, and that our search had not covered them. The terrible sentence in DST had got us! We applied for time to do a new search, and in February 1968 received time to carry it out. Again we used the 21 channel radiometer, and now guided by maser theory that had been developed in the meantime, started with the F=1 $\rightarrow$ 1 line because the experts told us that if any of the lines from this state would be detectable, this would be the one. We spent most of our time on it and found nothing. In the last part of the program, we switched to the F=1 $\rightarrow$ 0 line (which we were told would not be detectable), and bang, on the last day of the program, there it was. We appealed to Dr. Heeschen, and he extended our program by three days so that we could make more adequate measurements. This success meant that we had again detected a new line, something that gave us great satisfaction. There were two important astrophysical implications: 1) this was the shortest lived state from which a radio line had been detected and showed unambiguously that the densities in the maser regions had to be much larger than previously considered plausible; and, 2) the high excitation of this OH line was cited as support by Snyder and Buhl in their paper on the possibility of detecting the highly excited radio lines of interstellar H_2O. For us, it was the beginning of a decade long study of excited states of OH. I should note that our paper on this detection contained the result of a search at Haystack for the analogous line in the next level up (the $^2\Pi_{1/2}, J = 3/2$, F=2$\rightarrow$ 1). Therefore, we continued to use instruments that we had access to through our university.

The above studies were done while we were graduate students. Both Ben and I received our PhD's in June 1968. It should be clear from what I have said how important the 140 Foot was to our education, and I hope it is not immodest to say that we learned to use it well enough to make some important contributions to astronomy.

3. Graduate Students Today?

How does the situation of a graduate student working in radio astronomy compare with that of a student in the era I just talked about? First, consider some of the changes in the astronomical world.

To compare with the world of literature that we were working in, one may look at the *Astrophysical Journal* for 1965. In the entire year, 379 papers were published. Of those, 40 presented radio astronomical observations. In addition 12 theoretical papers were published which directly concerned radio astronomy, and 12 optical papers were published which were motivated by radio astronomy (QSO identifications, etc.). Of the 40 observational papers, eight came from the NRAO and seven from OVRO. In all, data came from telescopes at 18 institutions. (I have counted four IR papers because they were authored by Frank Low who was then on the NRAO staff.) Of all of the radio papers published in 1965, only two were PhD theses (Barry Clark's and Marvin de Jong's).

In 1995 the *Astrophysical Journal* reached 370 total papers with the February 20 issue. However, the counting becomes much more complicated. Unlike 1965, many of the papers involved observations in a number of wavelength ranges. Several times I accidentally found new radio data buried in a paper with no mention of it in the abstract. Astronomers now are taking a much less wavelength-oriented approach. For this reason my counts are less certain. Accepting this uncertainty, 27 of the 370 papers involved new radio observations. The NRAO accounts for 13 of the papers (eight are from the VLA which was being designed in Green Bank in the late 60's) and CSO is next with four. In other words, the share of the papers from the NRAO went from $\sim 20\%$ to almost 50%. The sources of the new data are almost exactly as many: 16. Six foreign observatories are involved compared with three in 1965.

At first sight, one might argue that the NRAO has grown at the expense of the university programs. However, I do not agree with that assessment. If we look at the university programs in 1965, we find Harvard, MIT, U. of Illinois, Ohio State, U. of Michigan, U. of Maryland, Owens Valley, Cornell, Stanford, and Hat Creek. What happened to them? Harvard is now involved with Smithsonian in the Sub-mm Array, MIT is involved in Haystack; Maryland, Illinois, and Berkeley are involved at BIMA; and, OVRO, Arecibo, and U. of Michigan are still there. Stanford and Ohio State are gone, but FCRAO and CSO have appeared. In sum, the number of university programs which are involved with instrumentation is pretty much the same. What has happened is that a lot of "outsiders"—X-ray and optical astronomers and even theoreticians—now use the national facilities. Radio astronomy is no longer an area outside of the main stream.

To further complicate a simple comparison, besides the *Astrophysical Journal* there is now *Astronomy and Astrophysics*, which began in 1970 by incorporating three rather smaller European journals, and now publishes a significant number of papers. Including all refereed journals, there were more than 1400 papers which mentioned radio astronomy in their abstracts in 1994. The field certainly involves vastly more people than it did in 1965.

What of the students? Thirty years ago, the community was small. If one spent a significant amount of time at Green Bank observing, one met most of the radio astronomers in the country. This was a tremendous advantage to a student. He (and it was primarily "he" in those days) could know everyone, and, much more important, he could be known by the established people in the field. While we were somewhat outside of the main stream of astronomy, we were at a point of development where being "outside" was a source of pride: we knew what the rest of the astronomers did, but they didn't know much about what we did. Both the personal acquaintances and the sense of being part of a distinctive small group contributed to a high *esprit de corps* among the students.

Now, there are so many people and so many papers that one cannot know everyone. Also, with the opening up of astronomy to many wavelengths and the growth of the use of multi-wavelength approaches, radio astronomy became fully part of the main-stream. (One often unconsidered consequence is that a number of students obtain radio data at institutions where no one else works with such data: i.e., many students are relatively isolated.) These changes have unavoidably eroded the *esprit de corps* of the field.

Other things have changed even more dramatically:

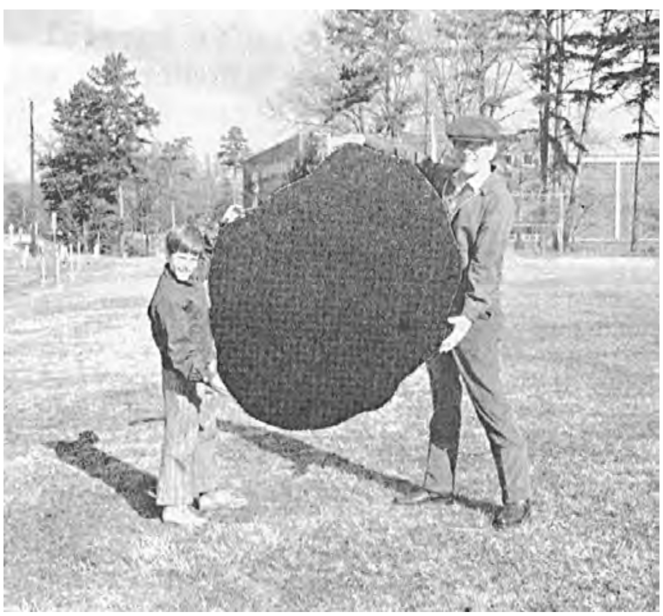
1. I don't recall ever thinking much about—let alone worrying about – getting a job teaching and doing research in astronomy. Both Ben and I went directly from graduate school to faculty positions. With the great growth in the number of PhD recipients, the picture has changed very significantly. Exponential growth—the consequence of a model in which each scientist trains N graduate students who each become professors who train N graduate students—cannot continue forever. (David Goodstein has made an interesting plot showing that the number of physical scientists in the world increased exponentially from 1700 until the early 1970's after which growth flattened significantly.) Students now have to think very hard about job prospects and about how they will eventually make a living. It is not a cheerful situation.

2. Thirty years ago, the mood of the country was that we needed all of the scientists we could get. Becoming a scientist was viewed by everyone as a good and useful thing to do—whether or not one knew what a scientist did. In other words, we enjoyed broad societal support for our career choice. This is now much less the case. A crass but, I believe, a good measure is the financial reward of science compared with other fields. I don't have precise numbers broken down by field, but in 1968 when I received my PhD, the average starting salary of a PhD in the Arts and Sciences from Harvard was the same as that of a graduate of the Harvard Law School. A few years ago, the differential was a factor of 2—and not in our favor. Consequently, in addition to the uncertainty about getting a job to live on, the student now has less confidence that he or she is actually doing something important. Eventually, this must impact the quality of students we get in the field.

I bring up these unpleasant issues on this otherwise happy occasion because we are the generation who should be trying to help students deal with this changed world. The first step is to realize that it has changed. This means that we need to make it clear to students exactly what the odds are from the start, and at every opportunity we need to discuss their full range of career options. It is critical that students are neither made to feel like failures nor that there is nothing interesting that they are qualified to do if they do not get—or decide they don't really want—a job involving academic type teaching and research. It is not easy to do because a different system worked so well for us.

Finally, I wish to thank Tom Wilson for sending me a draft of his contribution at an early stage and Ben Zuckerman for a thoughtful reading of this contribution.

Lew Snyder in 1969.



"Lew and Herm Snyder holding the only molecular cloud known to be in captivity." From the Observer February 1973.

The NRAO 140 Foot Telescope and the Beginning of Astronomical Spectroscopy of Interstellar Polyatomic Organic Molecules*

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This article is dedicated to the memory of 140 Foot Telescope operators Tom Carpenter, Ralph High, and Bill Hunter.

Abstract

We review the history of molecular radio detections on the NRAO 140 Foot and discuss the importance of these early detections.

1. Introduction

1.1 The Beginning of Molecular Radio Astronomy

By 1963, interstellar CH, CH^+, and CN had been detected via optical spectroscopy (see, e.g., Douglas & Herzberg, 1942) and OH had been detected via radio spectroscopy (see, e.g., Barrett, 1967). In the mid 1960s and early 1970s, one of the most important activities in the fledgling field of molecular radio astronomy was gathering interstellar cloud information through OH surveys (see, e.g., Palmer & Zuckerman, 1967; Turner & Heiles, 1971). The NRAO 140 Foot Telescope was used heavily for these surveys. The prevailing thinking during most of the 1960s was that only diatomic molecules could survive the harsh environment of the interstellar medium. A major change in molecular radio astronomy came about with the first detection of an interstellar polyatomic molecule, ammonia (NH_3). This detection was made at 1.26 cm wavelength in the interstellar OH source Sgr A by Cheung, Rank, Townes, Thornton, & Welch (1968); the 20 foot telescope at Hat Creek Radio Observatory was used for this important discovery. Next, water (H_2O) was detected in the OH source Sgr B2 (as well as in Ori A and W49) at 1.35 cm by the same group in 1969 (Cheung et al. 1969) using the same telescope at Hat Creek.

The NRAO 140 Foot Telescope was used in March, 1969, by Snyder, Buhl, Zuckerman, & Palmer (1969) to detect the next interstellar molecule, formaldehyde (H_2CO). Formaldehyde was the first organic polyatomic molecule found in interstellar clouds; thus its detection, at 6.2 cm wavelength (the 1_{11} - 1_{10} transition at 4830 MHz), marked the beginning of the study of organic chemistry in the interstellar medium. H_2CO was detected in absorption in the direction of 15 OH and continuum sources, including Sgr A (Figure 1) and Sgr B2. In late March, 1969, the same group (Palmer et al. 1969) detected H_2CO absorption against the 2.7 K background (Figure 2), which is a result of the now well-known H_2CO collisional pump.

* Presented at the 140 Foot Telescope Birthday Symposium, September 1995.

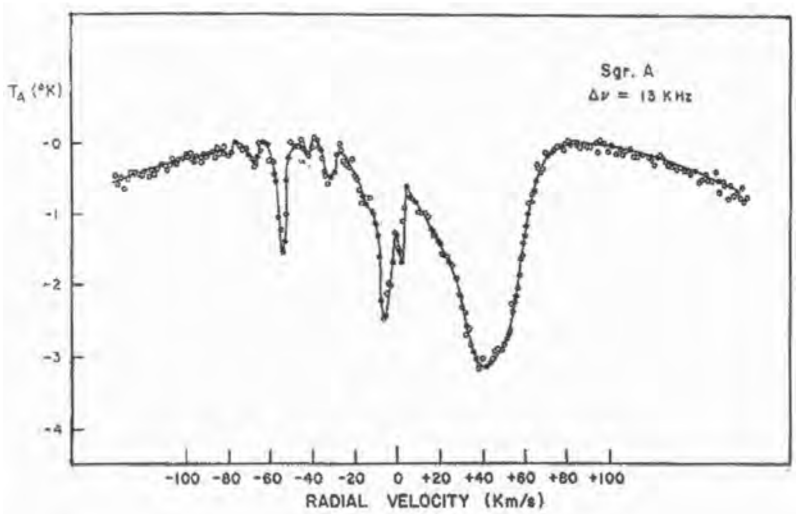

Fig. 1— Formaldehyde absorption against the Galactic Center (Sgr A), taken from Snyder et al. (1969). This spectrum closely resembles the OH absorption spectrum in the same direction. The ordinate is antenna temperature and the abscissa is radial velocity with respect to the local standard of rest. The effective resolution is $\sim$ 1 km s^{-1}.

A short while later, the formaldehyde isotopomer $H_2^{13}CO$ was detected (Figure 3) by Zuckerman et al. (1969).

1.2 Other Molecules Detected on the 140 Foot

In 1970 April, interstellar CO and the first radio lines of CN, both near 2.6 mm wavelength, were found by Wilson, Jefferts, & Penzias (Wilson et al. 1970; Jefferts et al. 1970) at the NRAO 36 foot telescope. This was the beginning of millimeter wavelength molecular radio astronomy and also perhaps the beginning of the move away from new detections in centimeter wavelength astronomy. A more detailed history of millimeter wavelength molecular detections was given by Snyder et al. (1994); in this paper, we want to focus on the centimeter wavelength detections which were made with the 140 Foot Telescope.

In addition to H_2CO, five other molecular species were found with the 140 Foot Telescope, all of which were important harbingers of species to come in molecular radio astronomy (Figure 4). In 1970 July, interstellar cyanoacetylene (HC_3N) was detected at 3.3 cm wavelength by Turner (1971) with the 140 Foot. In 1970 September, methanol (CH_3OH) was discovered at 35.5 cm wavelength by Ball et al. (1970). Immediately afterward (in 1970 October), Zuckerman, Ball, & Gottlieb (1971) found formic acid (HCOOH) at 18.3 cm wavelength in Sgr B2 only (see also Winnewisser & Churchwell, 1975). Formamide ($HCONH_2$) was detected at 6.49 cm in 1971 March in Sgr B2 by Rubin et al. (1971). In 1971 August, interstellar acetaldehyde ($HCOCH_3$) was reported at 28.15 cm in both Sgr B2 and Sgr A by Ball, Gottlieb, Lilley, & Radford (Ball et al. 1971; Gottlieb 1973).

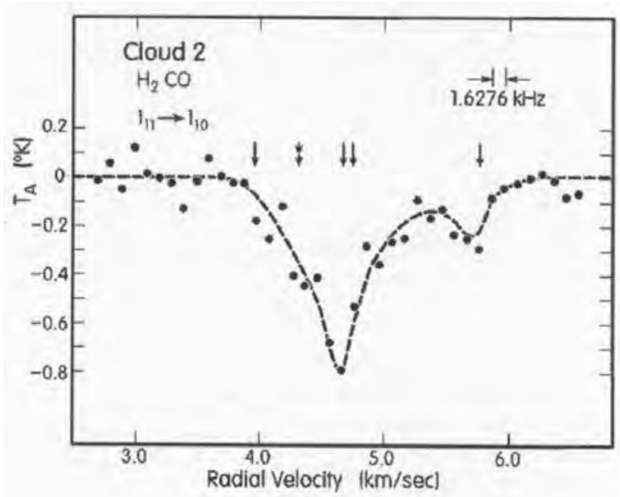

Fig. 2— H_2CO absorption against the 2.7 K background in the direction of Heiles Cloud 2, taken from Palmer et al. (1969). This shows the 1_{11} - 1_{10} transition at 4830 MHz. The arrows indicate the positions of the hyperfine components; the deepest absorption corresponds to the F=2-2 transition. The ordinate is antenna temperature and the abscissa is radial velocity with respect to the local standard of rest. Points are spaced by 1.6276 kHz, and the effective resolution is 2 kHz.

Today, at least 110 gas-phase molecular species have been found in the interstellar and circumstellar clouds of gas and dust which pervade our Galaxy. The usual restrictions of molecular stability placed on the laboratory spectroscopist by wall collisions are negligible in the interstellar clouds. The total gas content of the Galaxy is quite large, perhaps 3% of the total mass, but cloud dimensions are of the order of light years; the resulting densities are very low and in some instances may only reach 10^6 hydrogen molecules cm^{-3} in very dense clouds. Low densities and average kinetic temperatures between 10 and 100 K are indications that mean free paths between 10^9 and 10^{15} cm are not unusual. The resulting times between collisions (10^4-10^{10} s) are of the order of relaxation times for rotationally-excited molecules; thus electronic and vibrational relaxation times (typically of the order of 10^{-8} and 10^{-2} s, respectively) usually are too short to influence collisional formation processes. The chemical processes which generate interstellar molecules are efficient over large regions of the clouds and are intimately related to the presence of both the grains and stars enveloped in the interstellar clouds. In very dense clouds, it appears that the grains provide catalytic surfaces which enhance the production of the larger polyatomic molecules, whereas there is ample evidence that gas-phase chemistry is the predominant mechanism for formation of smaller molecules in more tenuous regions. Table 1 lists the known interstellar and circumstellar species which had been reported as of July, 1996. The molecules are ordered in rows by number of atoms and are arranged in alphabetical order across the page by empirical formulae according to the modified Hill system which is employed in the *Chemical Abstracts Formula Index.* Individual isotopes and a few unconfirmed species

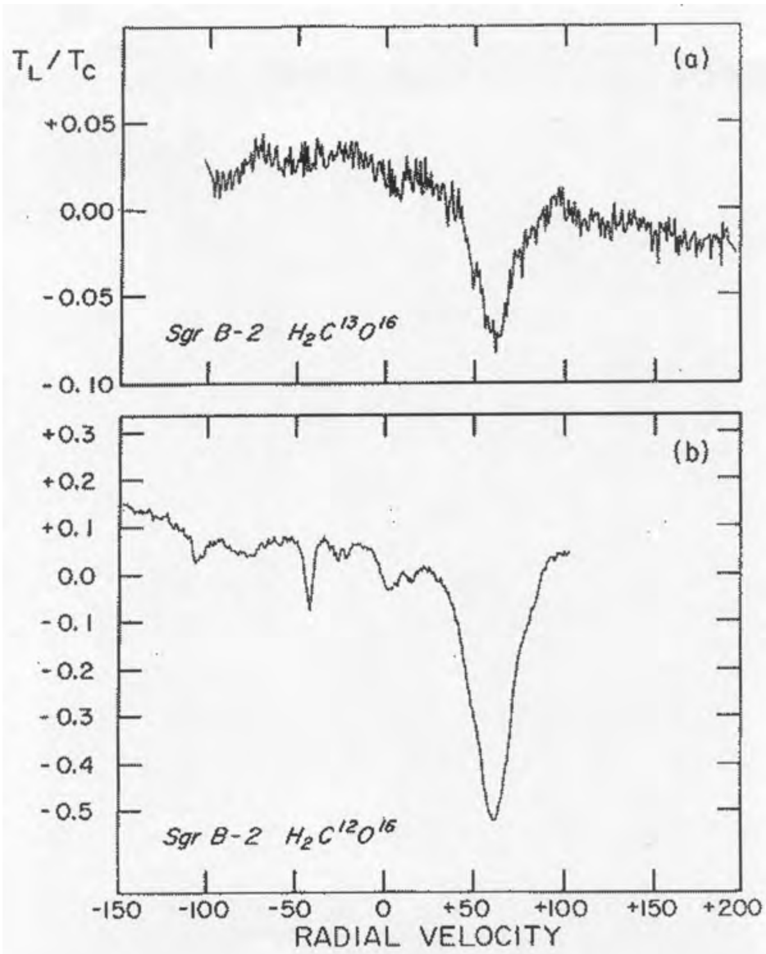

Fig. 3— $H_2^{13}CO$ and $H_2^{12}CO$ absorption spectra against Sgr B2, taken from Zuckerman et al. (1969). The ordinate is the ratio of the antenna temperature of the spectral line, T_L, to the continuum temperature, T_C; the abscissa is radial velocity in km s^{-1} with respect to the local standard of rest. The effective resolution is $\sim$ 1 km s^{-1}.

have not been listed. A question mark means that further confirmation of the detection is needed. Ring molecules are preceded by c- but where the structure is also linear an l- is used. Lists of the transition frequencies have been prepared by Lovas, Snyder, & Johnson (1979); Mann & Williams (1980); and Lovas (1992 & http://physics.nist.gov/restfreq). Many of the interstellar molecular species have also been found in circumstellar shells. Table 1 does not include the polycyclic aromatic hydrocarbons (PAHs), which are inferred to exist in interstellar grains (see, for example, Allamandola, Sandford, & Wopenka, 1987).

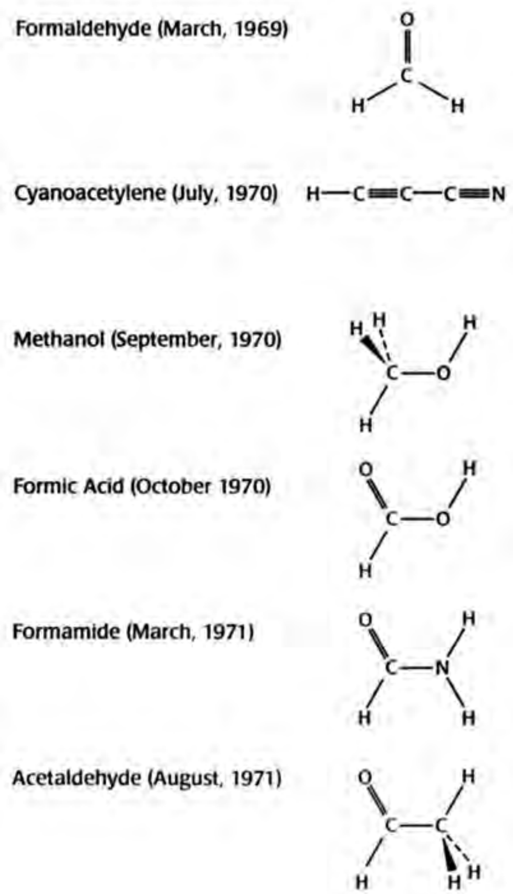

Fig. 4— Interstellar molecules discovered with the 140 Foot Telescope.

2. Why the 140 Foot Telescope could Detect Interstellar Molecules

The molecular species listed in Table 1 are too numerous to discuss individually in this brief article. But there is a simple way for the nonspecialist to visualize the molecular formation mechanisms that apply to most interstellar species. In turn, knowledge of the molecular formation mechanisms helps clarify why the 140 Foot Telescope was able to detect certain types of interstellar molecules and helps explain some of the astrochemical implications of the detections at centimeter wavelengths.

Small interstellar molecules and large interstellar molecules with a low degree of saturation (low hydrogen count) such as HC_3N, HC_5N, etc., can be formed in quiescent gas or in shock fronts by gas-phase chemical reactions, such as ion-molecule reactions and neutral-neutral reactions (see, e.g., Millar, 1994; Herbst, 1995). These

TABLE 1

The 110 reported interstellar and circumstellar molecules as of 1996 July

2	AlF AlCl C_2 CH CH^+ CN CO CO^+ CP CS CSi HCl H_2 KCl NH NO NS NaCl OH PN SO SO^+ SiN SiO SiS
3	C_3 C_2H C_2O C_2S CH_2 HCN HCO HCO^+ HCS^+ HOC^+ H_2O H_2S HNC HNO MgCN MgNC NH_2 N_2H^+ N_2O NaCN OCS SO_2 c-SiC_2
4	c-C_3H l-C_3H C_3N C_3O C_3S C_2H_2 CH_2D^+ ? HCCN $HCNH^+$ HNCO HNCS $HOCO^+$ H_2CO H_2CN H_2CS H_3O^+ NH_3
5	C_5 C_4H C_4Si l-C_3H_2 c-C_3H_2 CH_2CN CH_4 HC_3N HC_2NC HCOOH H_2CHN H_2COH^+ H_2C_2O H_2NCN HNC_3 SiH_4
6	C_5H C_5O C_2H_4 CH_3CN CH_3NC CH_3OH CH_3SH HC_3NH^+ HC_3HO $HCONH_2$ l-H_2C_4
7	C_6H CH_2CHCN CH_3C_2H HC_5N $HCOCH_3$ NH_2CH_3
8	CH_3C_3N CH_3COOH $HCOOCH_3$
9	CH_3C_4H CH_3CH_2CN $(CH_3)_2O$ CH_3CH_2OH HC_7N
10	CH_3C_5N? $(CH_3)_2CO$
11	HC_9N
13	$HC_{11}N$?

molecules are found in spatially extended clouds. In this context, an easy rule to remember for the 140 Foot Telescope is that the wavelength of observation (in centimeters) is approximately equal to the half-power beamwidth (HPBW) in arcminutes. Consequently, the HPBW of the 140 Foot was $\sim 3'$ for the HC_3N discovery at 3.3 cm wavelength, $\sim 6'$ for the H_2CO discovery at 6.1 cm wavelength, and $\sim 35'$ for the CH_3OH discovery at 35.5 cm wavelength. Therefore, at the 7.1 kpc distance to Sgr B2 (Reid 1993), the 140 Foot beam subtended 6.7 pc or 1.4×10^6 AU when the HC_3N cloud was detected, 12.6 pc or 2.6×10^6 AU for the H_2CO cloud, and 72.1 pc or 14.8x10^6 AU for the CH_3OH cloud. The Oort cloud surrounding our solar system is $\sim$ 150,000 AU or $\sim 2\times10^{18}$ cm, which is about the outer limit of a typical supergiant shell. The 140 Foot beamwidths for the HC_3N, H_2CO, and CH_3OH detections were about 9, 17, and 99 times greater than the diameter of the Oort cloud or a large circumstellar shell. Clearly the 140 Foot molecular detections listed in Figure 4 were allowed by a spatially extended gas-phase chemistry, so that beam dilution did not significantly weaken the detected signal.[1] Even with this spatially extended chemistry, the 140 Foot detections were further aided by the low excitation energies of the molecular rotational levels involved in the detected transitions. In the case of H_2CO, for example, the 1_{11}-1_{10} transition was easily detected because it involves the lowest energy levels of ortho formaldehyde (Figure 5). That is, radio photons cannot radiatively drain from the 1_{11} level to the 2_{02}, 1_{01}, or 0_{00} levels because those transitions are forbidden. Thus the 1_{11} level is effectively the ground state of ortho H_2CO, even though it is $\sim$ 11 cm^{-1} above the zero point energy. At long wavelengths, the new Green Bank Telescope will extend this tradition of detecting gas-phase molecules in extended sources.

However, a different set of circumstances applies to large molecules with a high degree of saturation (high hydrogen count), such as acetone ($(CH_3)_2CO$), ethyl cyanide (CH_3CH_2CN), and ethanol (CH_3CH_2OH). These molecules cannot be formed easily by gas-phase reactions alone; consequently, solid state chemical reactions on grain surface ice mantles are often invoked to form the large molecules, and evaporation is proposed as the mechanism that drives them into the gas phase (see, e.g., Caselli, Hasegawa, & Herbst, 1993; Charnley et al. 1995). In support of the grain chemistry hypothesis, small molecules such as CO, H_2O, CH_4, and cyano-group molecules, have been directly detected in the solid phase in the infrared (Knacke & Larson, 1991; Lacey et al. 1984; Lacey et al. 1991; Tielens et al. 1991; Whittet & Duley, 1991). Ethyl cyanide and other large highly saturated molecules are found in small, hot, molecular cores, less than 0.1 pc in diameter, that are embedded in the extended molecular clouds (see, e.g., Miao et al. 1995; Mehringer & Snyder, 1996). The small beamwidths of interferometers and single-element millimeter and submillimeter wavelength telescopes are required to observe these species.

[1] Of course, these arguments assume that the detected molecules were not in maser emission. The 18 cm wavelength OH maser had such a high brightness temperature that it could be observed routinely with the 140 Foot despite the large beam dilution (see, e.g., Palmer & Zuckerman, 1967). And there is little doubt that the H_2O maser could have been detected first on the 140 Foot Telescope by Snyder & Buhl (1969) had the NRAO proposal referee(s) decided to let them on the telescope. The paper by Buhl, Snyder, Schwartz, & Barrett (1969) shows examples of H_2O maser spectra observed with the 140 Foot Telescope.

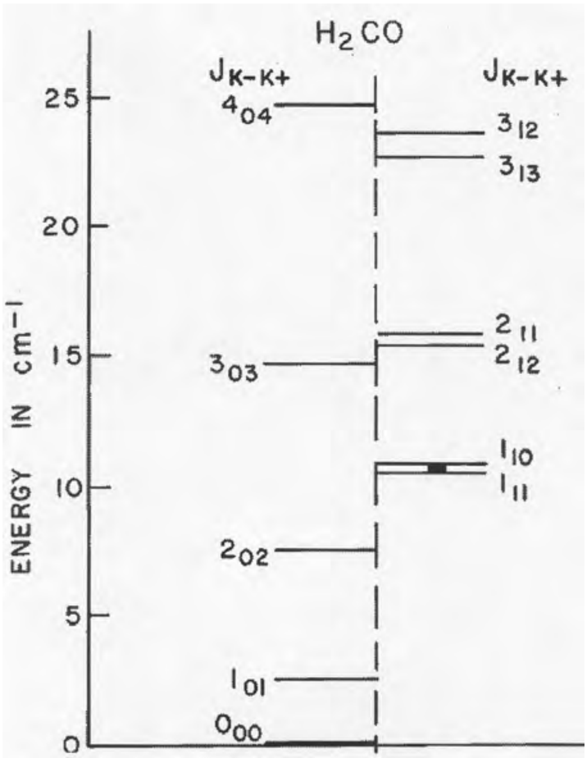

Fig. 5— An energy level diagram for formaldehyde. The para levels are on the left and the ortho levels are on the right. The 1_{11}-1_{10} transition is indicated by the small vertical bar on the right.

3. The Seminal Role of Early Molecular Research on the 140 Foot Telescope

The six molecules listed in Figure 4 (H_2CO, HC_3N, CH_3OH, HCOOH, $HCONH_2$, and $HCOCH_3$) are only a small subset of Table 1, but because they were detected very early in the history of interstellar molecular astronomy, they played a very seminal role in steering the future research directions of the field. There are a few astronomical research areas which, in my opinion, were either accelerated or were born as a direct result of the 140 Foot molecular discoveries listed in Figure 4. I will list several of these areas in the following discussion.

3.1 Formaldehyde

The H_2CO discovery influenced several areas. The H_2CO surveys which immediately followed (for example: Zuckerman et al. 1970; Whiteoak & Gardner 1970; Scoville & Solomon, 1972; Fomalont & Welcachew, 1972; Wilson, 1972; Dieter, 1973) showed that polyatomic organic chemistry is a common feature of the Galaxy.

In turn, this fact forced realistic astrochemistry models to begin appearing for the first time, starting with early ion-molecule chemistry models (for example: Herbst & Klemperer, 1973; Watson, 1973). The $H_2^{13}CO$ discovery initiated the serious examination of the value of the $^{12}C/^{13}C$ Galactic abundance ratio; previously the terrestrial ratio of 89/1 was assumed (see Bertojo, Chui, & Townes, 1974, and Penzias, 1980, for examples of early discussions on this topic). The detection of H_2CO absorption against the 2.7 K background gave a new view of the microwave background radiation. Finally, it should be noted that the H_2CO detection served as a powerful motivation to build new equipment and launch searches for other interstellar molecules with related molecular structures, such as the CO molecule. The 1970 detection of CO at 115 GHz with the NRAO 36 Foot telescope (Wilson et al. 1970) gave astronomy a new data tool for studying everything from star formation to Galactic structure to gravitational lenses.

3.2 Cyanoacetylene

HC_3N was the first of the interstellar long-chain carbon compounds (HC_nN, C_n, C_nH, C_nN, C_nO, C_nS) to be discovered. Now, 26 years later, the most sophisticated ion-molecule models have problems explaining the abundances of these long-chain carbon compounds.

3.3 Methanol

At first, the CH_3OH discovery didn't seem to have much impact on observational radio astronomy. Then, Barrett et al. (1971) discovered strong CH_3OH maser emissions from Orion in a series of lines around 25 GHz.

masers For years, no other CH_3OH masers were found until Wilson et al. (1984, 1985) detected new masering transitions at 23 and 19 GHz. So far, about a dozen different masering CH_3OH transitions have been discovered toward numerous star-forming regions at wavelengths between 2 cm and 2 mm; these new masers have been reviewed by Menten (1991). Two classes of methanol masers have been identified: class II masers are located near ultracompact HII regions, while class I masers are not. The high CH_3OH abundances required for class I masers may come from grain mantle evaporation caused by shock heating.

At the present time, both masering and nonmasering transitions of methanol are widely observed in the interstellar medium. Because the highest gas-phase CH_3OH abundances are found in regions where icy grain mantles are thought to have recently evaporated, and because gas-phase models can't explain these high abundances, the presence of abundant CH_3OH in these regions is now considered to be a prime demonstration of interstellar grain surface chemistry at work (Charnley et al. 1995). Thus, the study of interstellar CH_3OH, which began some 25 years ago with the detection of two somewhat extended gas-phase sources at 35.5 cm wavelength toward Sgr A and Sgr B2, has evolved into studies of maser emission and compact dust sources with high frequency single-element telescopes and interferometric arrays.

3.4 Formic Acid, Formamide, and Acetaldehyde

To date, HCOOH, $HCONH_2$, and $HCOCH_3$ have played only a small role in the astrochemical studies of the interstellar medium, although weak spectral lines of these species have turned up in various source bandscans. These molecules could become more interesting to study, however, because (along with H_2CO, HC_3N, and CH_3OH) they are part of the group of molecules considered to have been important for primitive biological systems on the early Earth.[2] Thus, these molecules of biological interest, or biomolecules, can possibly give insight into presolar nebular chemistry and the biological potential of the associated accretion chemistry for seeding newly formed planets during their early evolution. It is possible that some part of the terrestrial prebiotic organic chemistry occurred in interstellar space and then was transported to the early Earth by comets, asteroids, and meteorites. It is probable that interstellar biomolecules don't survive in the presolar nebula or in cometary comae, but even so they may be relevant to the chemistry of the early Earth or newly formed solar systems because the chemistry of dense interstellar regions, particularly the grain-surface chemistry, may be similar to that which occurs on the surface and in the rarefied atmosphere of a young planet.

Formic acid (HCOOH), the active agent in ant bites, bee stings, and nettle plant attacks, is potentially important for astrochemical studies. It is the simplest known molecular precursor to the common interstellar molecular methyl formate ($HCOOCH_3$) which has the C-O-C backbone, and to the elusive biomolecule acetic acid (CH_3COOH) which has the less common C-C-O backbone (Millar et al. 1988). Mehringer et al. (1996) found that $HCOOCH_3$ has a column density 20-200 times higher than CH_3COOH in Sgr B2(N). CH_3COOH is an important chemical precursor for the formation of glycine (NH_2CH_2COOH), the simplest biologically-important amino acid. Therefore, future studies of HCOOH will help us understand the successive formation of larger biomolecules in the ISM. Today, however, almost nothing is really known about the distribution and formation chemistry of HCOOH, especially the formation processes via dust chemistry in hot dust cores.

4. Conclusions

Interstellar molecular spectroscopy was originated by optical astronomy in the late 1930s and early 1940s with the detection and observation of interstellar diatomic molecules. The spectroscopy of interstellar polyatomic molecules was born at Hat Creek Observatory via the radio detection of ammonia and water in 1968 and 1969. Finally, the barrier to the study of heavier organic polyatomic molecules was overcome with the detection and observations of interstellar formaldehyde at the 140 Foot Telescope in 1969. In its time, the 140 Foot was the premier large telescope in the world for the centimeter wavelength observation of interstellar molecules. The 140 Foot Telescope wasn't the largest telescope available for this work, but it often produced the best molecular science because:

a) it usually had the best available centimeter wavelength receivers;

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[2] In fact, formamide is one of three interstellar molecules that contain the amide group. The other two are H_2NCN and NH_2CH_3.

b) its receivers could access a large fraction of the tuning range between 300 MHz and 30 GHz;

c) it had an excellent autocorrelation spectrometer;

d) it was fully steerable and therefore could track sources for long integration times;

e) the operators, engineers, and technicians were dedicated to maintaining the telescope in top operating condition and often labored into the night to restore operations during the infrequent break-downs; and

f) the NRAO telescope scheduling system allowed the best proposals to be scheduled based only on scientific merit, and not political or national considerations.

The new Green Bank Telescope will be mechanically superior to the 140 Foot Telescope, but it will have a hard act to follow in making a similar impact on radio science.

Acknowledgments

It is a pleasure to acknowledge the help and support that we received more than twenty five years ago from the operators and engineers at the 140 Foot Telescope. In particular, I want to especially mention Howard Brown, Dave Van Horn, George Liptak, and George Behrens. It's sad that three more friends, Tom Carpenter, Ralph High, and Bill Hunter are deceased, but I would like to think that they are here with us today in spirit. LES received partial support from NASA grant NAGW-2299, NSF grant AST 93-20239, and the Laboratory for Astronomical Imaging at the University of Illinois.

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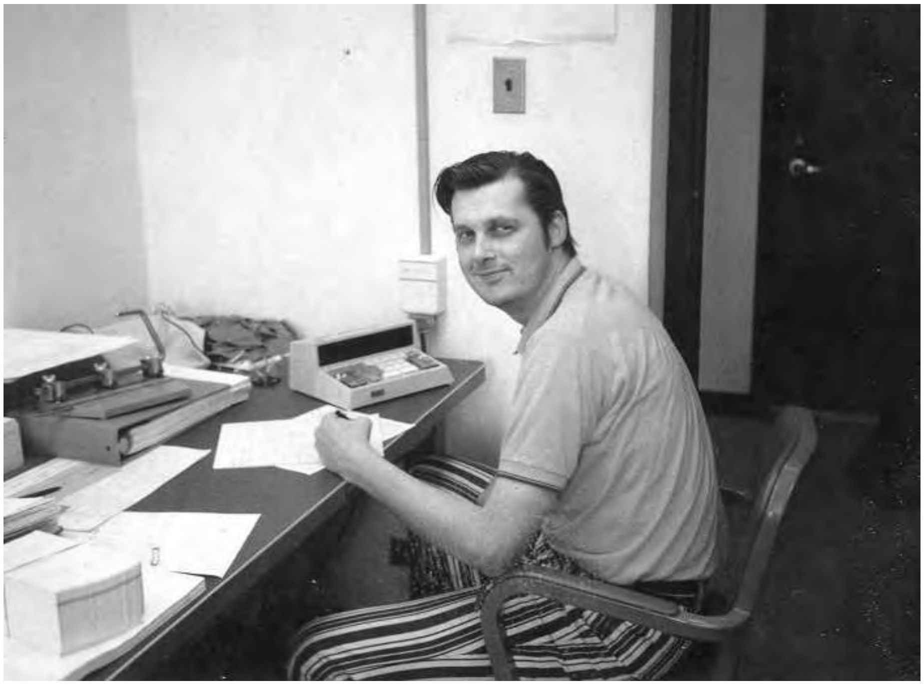
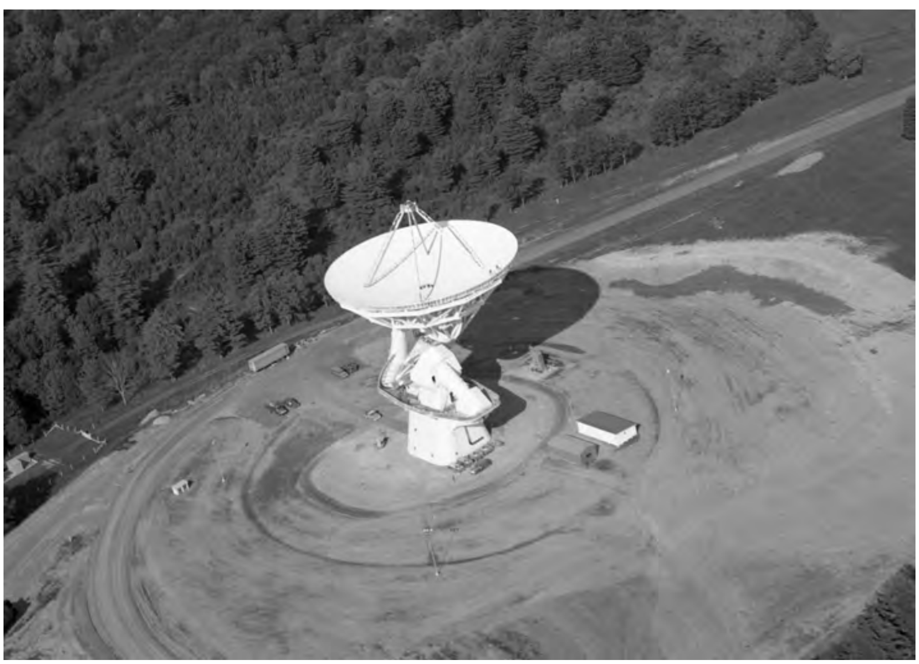
The "Ice Cream Truck, " the van used to track down interfering signals, October 1981.



Barry Turner in 1971, at his desk in the Charlottesville NRAO offices.



Aerial view of the 140 Foot in 1965.

Early Discoveries in Molecular Spectroscopy at the 140 Foot Telescope*

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National Radio Astronomy Observatory

Largely because of its unparalleled electronics, even in its early days, the 140 Foot was the instrument that ushered in the modern era of molecular astrophysics. But for an accident of history, the 140 Foot would have been the discoverer of H_2O and perhaps NH_3, which were found first at Hat Creek in 1968. The first major discovery of the 140 Foot was formaldehyde, described in this volume by L.E. Snyder. This molecule was the one that really launched the modern perception of an interstellar medium rich in complex organic chemistry, and which led directly to the work of many others, both on the 140 Foot and the NRAO 36 Foot, which had joined the fray by 1970. The two NRAO instruments remained largely unchallenged in this field of research for another decade. I will discuss, chronologically, 140 Foot work on molecular spectroscopy that I was involved in over the years 1969 to 1989.

1. Cyanoacetylene (HCCCN)

In 1968 the molecule rush was on. Table 1 shows the early chronology of molecular detections. Seven of the first 28 molecules (marked *) were detected at the 140 Foot. In those days, there was little relevant molecular spectroscopy from the laboratory, and we all relied on the "Green Book"—the early NBS tables of molecular frequencies. The reasoning behind most searches was primitive. H_2O has 3 atoms, NH_3 and H_2CO 4 each, so why not try for 5 atoms? HCCCN was convenient, since its lowest transition lay at 3 cm wavelength (9098 MHz), it is linear so the rotational energy is concentrated in fewer energy levels, it has a large dipole moment, and it has hyperfine splitting so its signature was unambiguous. HCCCN was detected in only Sgr B2 during the discovery session (Turner 1971). However the lesson learned in the early days was that if one had a receiver at the right frequency, there was a good chance of detecting a new molecule. The implications of 3 carbon atoms in a row, with intriguing alternating triple and single bonding, were not clear at the time. Later we realized that HCCCN was the lowest member of the cyanopolyyne family, the only carbon-chain family known for many years. Carbon chains became a hallmark of organic chemistry in the ISM. The cyanopolyyne family is now known up to $HC_{11}N$, and other families (C_nO, H_mC_nO, C_nS, C_nH) are also now identified. HC_3N was also the first closed-shell interstellar molecule that was unstable on earth (though it can be stored under suitable conditions). We still don't completely understand the interstellar chemistry of the cyanopolyynes, not even HC_3N itself. The other carbon-chain families are better understood. Some investigators think carbon chain species, particularly the H_nC_nO family, may be related to the 70-year old puzzle of the diffuse interstellar bands. To facilitate searches for higher members of the cyanopolyyne family, laboratory spectroscopists had to launch programs to synthesize and measure these exotic compounds on earth. This was the beginning of a long and rewarding cooperation between laboratory and radio astronomy, which persists to this day.

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* Presented at the 140 Foot Telescope Birthday Symposium, September 1995.

2. Rotationally Excited States of OH

OH was the first molecule detected by radio means, in 1963, and it was still the object of overwhelming attention in 1970, not only due to its utility as a general probe of Galactic structure and interstellar conditions, but also because of its peculiar excitation, leading to strong maser activity in the ground (J=3/2) state multiplet. Being a light molecule (only 2 atoms, one hydrogen), the rotational levels lie at high energy by interstellar standards, so it was already surprising when OH was detected by Yen et al (1969) at the 140 Foot via its Λ-doubling in the first excited rotational level (J=5/2). In those days, there was spirited competition between various pumping theories for the OH excitation, but the particular pattern of lines seen at 6 cm wavelength in the J=5/2 state did not discriminate among them. As a student at Berkeley I had been involved in pumping theories, and we (Gwinn et al 1973) had come up with the first collisional pumping theory which made use of the peculiar Λ-doublet nature of OH in which the outer molecular electron shell is one electron short of its complement of 4 (spherical symmetry) and leaves a dumbbell-shaped electron "hole" which orients itself parallel to the rotational axis for one of the Λ-doublet levels, and perpendicular for the other Λ-doublet level, thus offering a different cross-section to incoming colliders which are polar. Thus, the Λ-states can be "selected." We proposed two variants of the theory: one in which H_2O was collisionally dissociated and the OH "selected" as it breaks away from the incipient H-OH bond, the other featuring rotational collisions of OH with energetic H atoms or H_2 molecules which could populate the excited rotational levels. The OH energy levels consist of two "ladders" known as the $\Pi_{3/2}$ and $\Pi_{1/2}$, respectively. In our theory, the Λ-doublet parities in the lowest 4 rotational levels of $\Pi_{1/2}$ are opposite to all the others, and we predicted an inversion of the populations in the Λ-doublets of all but these 4 levels. Indeed, it was already known in 1969 that the first 2 levels of $\Pi_{1/2}$ showed no masers while the Yen et al result showed that the first 2 levels of $\Pi_{3/2}$ did have masers. So the theory looked good. Thus we decided to try detecting the Λ-doublet levels of the next higher state in $\Pi_{3/2}$, the J=7/2 state, at 13.2 GHz. Again, NRAO had a receiver available. We (Turner et al 1970) were rewarded with a detection of one of the two main lines of this state, just the one that fit our theory. It was also the first new molecular line detected in the decade of the 1970s. Many years later the next state up (J=9/2) was detected (not at the 140 Foot), as we predicted, and others went on to apply our theory to other "similar" molecules such as CH, for which we predicted an anti-maser in a footnote in our OH pumping paper but got a sign wrong. But a good introduction for the next topic.

3. CH

CH was probably the most exciting discovery I have been involved in, not only because of its perceived importance in 1971, but also because of the international controversy that followed its discovery on the 140 Foot in 1971. It may be less obvious now, but CH had been considered something of a holy grail of molecular searches since the discovery of OH in 1963. There were several reasons: CH had been designated a strong candidate in the famous lists of Shklovskii and Townes; it had challenged laboratory specialists for many years, who recognized its importance in understanding basic theory of molecular bonding; it was expected to be widespread in the Galaxy, like OH; it is a doublet Π species, so it has Λ-doubling like OH and was expected to be strongly non-LTE excited like OH and hence to provide important information about collisional and radiative excitation in space; and it was recognized as fundamental to understanding astrochemistry.

Several searches (at Hat Creek, Agassiz Station, in Australia and Sweden) preceded ours during the 1960s. The problem was frequencies. No one had been able to measure the hyperfine-split Λ-doubling in the laboratory, because CH is highly reactive. In 1971 the Herzberg group in Ottawa got the first measure of the Λ-doublet frequency using a new Fabry-Perot technique. The frequency was still quite uncertain, 3373 ± 30 MHz. Worse, the hyperfine splitting of each Λ-doublet level could not be measured, but theoretical calculations gave some indication. Once again, NRAO had an available receiver, again an uncooled paramp. Ben Zuckerman and I went ahead with this information, having to search many receiver bandpasses and thus reducing our sensitivity. Because of the previous unsuccessful searches and the fact that CH was expected to have very small abundance, we followed the time-honored approach of emphasizing searches for absorption lines (against Cas A), which are more sensitive than emission-line searches under many conditions. We also looked toward strong OH sources. We detected CH toward Cas A in December, 1971 but nowhere else. This was the last of the good news for this project. First, we did not consider the results compelling despite evidence (see Figure 1) for the two well-known velocity features toward Cas A. We were suspicious because, although the signal was not especially weak, it was in EMISSION, which made no sense because the brightness temperature toward Cas A was 900 K, so T_{ex} had to exceed that, but not lead to significant maser activity.

I was influenced not only by our prediction that CH should be “cooled,” not “heated” by collisions, but also by several years of theoretical work on OH masers that suggested that a high T_{ex}, but not a negative one, was very unlikely. So we decided we had to confirm it, although we did not submit a follow-up observing proposal until mid-1972. There were more (unexpected) delays until we finally got on the telescope in October, 1973. We confirmed CH. This is undoubtedly the only case where a two-year time base was obtained of a new detection before publication! Second, despite the long-standing interest in CH, we did not think we faced any immediate competition. The Onsala group had a good receiver at 9 cm, a maser which they had developed for an earlier search, but we understood it had been dismantled and they were no longer interested. We decided to announce our discovery in talks before publication. The word spread. We sent our paper to ApJL in early November, submitting ourselves to its ponderous schedule. Within a few days we received a call from Brian Marsden of the IAU Circulars saying he had heard about our discovery but had just received a submission from the Onsala group under Olof Rydbeck announcing THEIR discovery of CH. Marsden wanted to know what was going on. At that time, the Circulars stuck to their policy of accepting only time-variable phenomena of which CH was not an example, so Marsden refused the Onsala note. We thought we had a reprieve. We soon found that the Onsala Observatory had dropped everything they were doing and pressed every member of the staff into the CH effort. The radio astronomy community was soon inundated with accounts of the Onsala discovery, including a complete Galactic survey of CH. Meanwhile, they had submitted their initial detection to Nature which moves very

TABLE 1

The Prominence of the 140 Foot Telescope in Early Molecular Astronomy

Name of Molecule	Chemical Symbol	Date	Spectral Region First Observed	Telescope of Discovery
methyladyne	CH	1937	visible 4300 Å	Mt. Wilson
cyanogen radical	CN	1940	visible 3875 Å	Mt. Wilson
methyladyne ion	CH+	1941	visible 4232 Å	Mt. Wilson
Hydroxyl radical	OH	1963	radio 18 cm	Lincoln Lab.
ammonia	NH_3	1968	radio 1.3 cm	Hat Creek
water	H_2O	1968	radio 1.4 cm	Hat Creek
*formaldehyde	H_2CO	1969	radio 6.2 cm	NRAO 140
carbon monoxide	CO	1970	radio 2.6 mm	NRAO 11m
hydrogen cyanide	HCN	1970	radio 3.4 mm	NRAO 11m
*cyanoacetylene	HC_3N	1970	radio 3.3 cm	NRAO 140
hydrogen	H_2	1970	UV 1013-1108 Å	NRL rocket
*methanol	CH_3OH	1970	radio 36 cm	NRAO 140
*formic acid	HCOOH	1970	radio 18 cm	NRAO 140
formyl radical ion	HCO+	1970	radio 3.4 mm	NRAO 11m
*formamide	NH_2CHO	1971	radio 6.5 cm	NRAO 140
carbon monosulfide	CS	1971	radio 2.0 mm	NRAO 11m
silicon monoxide	SiO	1971	radio 2.3 mm	NRAO 11m
carbonyl sulfide	OCS	1971	radio 2.7 mm	NRAO 11m
methyl cyanide	CH_3CN	1971	radio 2.7 mm	NRAO 11m
isocyanic acid	HNCO	1971	radio 3.4 mm	NRAO 11m
methyl acetylene	CH_3CCH	1971	radio 3.5 mm	NRAO 11m
*acetaldehyde	CH_3CHO	1971	radio 28 cm	NRAO 140
thioformaldehyde	H_2CS	1971	radio 9.5 cm	Parkes 64m
hydrogen isocyanide	HNC	1971	radio 3.3 mm	NRAO 11m
hydrogen sulfide	H_2S	1972	radio 1.8 mm	NRAO 11m
methanimine	CH_2NH	1972	radio 5.7 cm	Parkes 64m
sulfur monoxide	SO	1973	radio 3.0 mm	NRAO 11m
*methyladyne	CH	1973	radio 9.0 cm†	NRAO 140

* Detected at the 140 Foot.

† First radio detection.

fast compared with ApJL. Their paper beat ours into print. The story has one reprieve for us. We gave a paper at the AAS in December 1973, and the BAAS abstracts appeared in print before the Onsala Nature paper. We later followed up with our own Galactic survey, at the 140 Foot and the 100-m. There was still

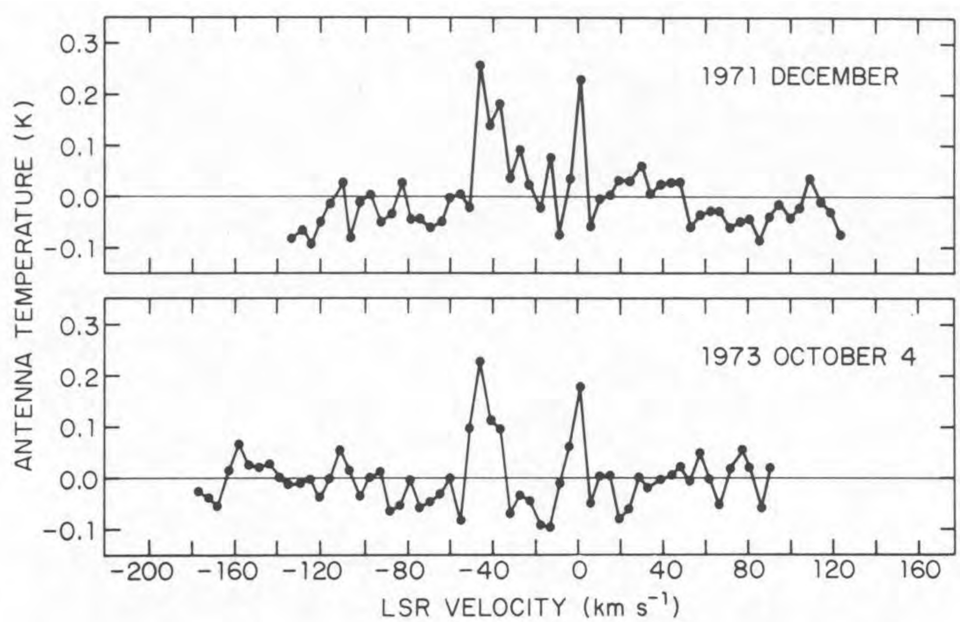

Fig. 1— The First Radio Detection of CH (140 Foot at 3.35 GHz; from Turner and Zuckerman 1974).

interesting science. It is fair to say that CH was an interesting experience fairly early in our careers.

4. OH Survey

While all the excitement of new molecules was going on, I was carrying out, during 1969 to 1975, the most comprehensive Galactic survey of OH yet done, following the early ones at Berkeley and in Australia. The survey was unique in several instances, thanks to unique new NRAO hardware. NRAO developed the first cooled paramps in 1968 and these provided $T_{sys} = 50 - 65$ K, far better than the ~150 K or worse used in earlier surveys. The 140 Foot also had one of the best spectrometers, the Mk II autocorrelator. The 140 Foot OH survey included all four lines at 18 cm with much better sensitivity than earlier surveys. It systematically sampled the Galactic plane (± 2 deg in latitude), without regard for the presence of HII regions, SNRs, etc. The survey included 16 observing sessions from August 1969 to May 1976, comprising fifty 24-hour days. Some 7000 spectra were taken at a velocity resolution of 1.4 km s^{-1}, not yet surpassed. The survey, published as A&AS 37, 1 (1979):

- Multiplied the known number of type I masers (1665 MHz) by a large factor.
- Same for the OH/IR stars (1612 MHz).

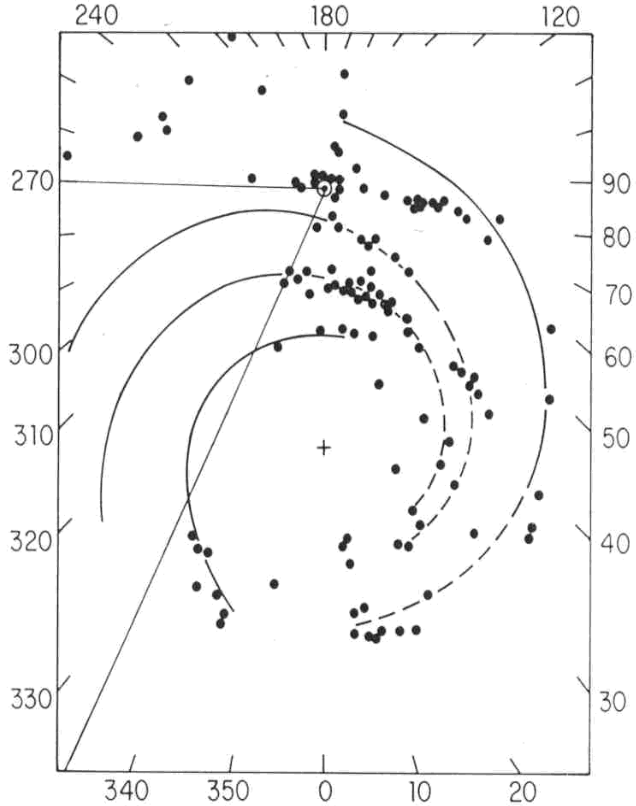

Fig. 2— The spatial distribution of 1720 MHz OH clouds in the Galaxy (points) compared with a spiral arm pattern.

- Discovered some bizarre regions of very strong, wide OH absorption not well correlated with continuum, which imply huge concentrations of mass. Hints of these were later found in CO.
- Found for the first time widespread 1720 MHz emission not correlated at all with continuum (HII regions or SNRs). The angular size of the 1720 MHz clouds is that of GMCs, corresponding to 30-40 pc.

The latter is perhaps the most important finding. The 1720 MHz line is pumped by far-IR at 83 and 120 μm, which is likely widespread in the Galaxy and expected to trace spiral arms. The disposition of these arms has proved difficult to trace by other means, in particular whether small molecular clouds and LMCs have different distributions. The 1720 appears to be a better tool. Figure 2 (from Turner 1982) shows the distribution derived for the 1720 MHz clouds (dots) on a plane view of the Galaxy, on which is sketched the 4-arm pattern proposed by Georgelin & Georgelin in 1976. Clearly seen are the Orion arm, which extends across to the Cygnus arm, and the inner Sagittarius and Scutum arms.

5. OH in Comet Kohoutek (1973)

There is much interest these days in bright comets, and radio astronomers have detected many molecules in them. The first of all comets to yield a molecular detection at radio frequencies was Kohoutek (1973). OH was detected at the 140 Foot in December 1973 (Turner 1973) and almost simultaneously at Nançay, France. The 140 Foot detection was unique in that OH was seen in absorption against the cosmic background; all other OH detections in comets have been in emission. This detection of absorption was the definitive test for the UV pumping theory of OH excitation, invented for interstellar maser sources, but found to be inapplicable. UV pumping is believed to be the excitation for all cometary molecules seen today. UV excitation elevates the OH from its ground electronic state to the excited $^2\sigma^+$ state from which it decays by a myriad of pathways back to the ground state. Basically, the theory of OH/UV pumping is that when the optical depth in the many electronic hyperfine lines at ~ 3080 Å is < 1, the ground state Λ-doublet excitation temperature is small (< 2.7 K) but positive, leading to anomalous absorption such as the 140 Foot saw, and similar to the 6 cm H_2CO transition. This small opacity corresponds to small OH column depth, as befits the early phase of Kohoutek well before perihelion when the 140 Foot data were obtained. Later, as more OH is formed by the increasing photodissociation of H_2O, the 3080 Å opacity becomes large, and the theory predicts that T_{ex} for the ground state Λ becomes negative, giving rise to maser action. The cometary masers are never powerful like the interstellar ones, because the OH column density is relatively low.

6. C_4D

Astrochemists have realized since the mid-1970s that the deuteration of interstellar molecules is a major test of ion-molecule chemistry, and that it proceeds best at very low temperatures. By 1988 astronomers had detected deuterated species up to the level of 3-carbon-atom-species (C_3HD), at which point in the huge network of hydrocarbon-ion reactions that are the backbone of interstellar chemistry, there is a big branch-point and we didn't know which branch explained higher-order hydrocarbon chemistry. C_4D offered the best test, but would surely be very weak as we understood the chemistry. The superlative K-band maser system at the 140 Foot was got working at the awkward frequency of 17.6 GHz and pushed to its limits in a 50-hour integration. It made the detection of C_4D in the very cold cloud TMC-1 (Turner 1989) and now we know how the chemistry proceeds up to C_5. Further progress using D-molecules will be challenging.

7. The C_4H Zeeman Experiment

The chemistry suggested that C_4H can form (via ion-molecule reactions) only in very dense, very cold cores. C_4H also has a large magnetic g-factor, the factor that relates Zeeman splitting to the size of the magnetic field. These two attributes provide a method to find what happens to the field in a very dense cloud core. This is of great importance in understanding how stars form from dense molecular cores, since if the field gets amplified to a large value as the core contracts, it significantly inhibits gravitational contraction. The usual relation $B \sim n^{\kappa}$ must break down at high density when the ionization fraction drops below a critical value and the field is no longer frozen in, but rather expelled. In 1988 Rich Barvainis and I used the 140

Foot in a search for the C_4H Zeeman effect at 9.5 GHz in TMC-1. We got a weak Zeeman-like signal which, if real, corresponds to B = 60 μGauss. Given $\kappa \sim 2/3$ from the low-density Zeeman experiments on OH and H, this result indicated that B had indeed been largely expelled. However, we were unsure about the polarization residuals of the 140 Foot. The Haystack antenna had too low an efficiency at 9.5 GHz to resolve the issue. We hope to pursue this project on the GBT where the signal will be much stronger and the antenna polarization residuals much smaller.

8. Translucent-Cloud Chemistry: The Cirrus Cores

In 1989 there was a lot of speculation about the nature of the cirrus clouds discovered by IRAS, in particular about the presence of small hot grains and about their dynamics (expansion, contraction, or neither). Lee J Rickard and I decided to use molecules other than CO as diagnostics to get harder physical information about the cores of these clouds. The 2 cm H_2CO and other diagnostic lines were best suited on grounds of critical densities for their excitation, and adequate spatial resolution on the 140 Foot. The 2 cm H_2CO line requires quite a high density to excite, and finding it would refute the ideas about these cores expanding in time and probably the notion of hot grains also, because UV couldn't penetrate high densities. We really didn't expect to detect it, and took care to submit our proposal on April 1 to be consistent with the style in which we wrote it. We weren't disappointed: two referees thought the idea was nonsensical and one assigned REJECT. But the other two deemed it a good idea, so we had to go ahead. We detected 2 cm H_2CO. It was clear from this initial result that a large number of molecular species could be detected in the cirrus cores. The project took off from there. Using extensive observations of all the CO isotopes and accessible transitions together with its well-understood chemistry, one can construct very detailed physical models of these objects. These in turn have been used to test in detail, or invent new, chemical schemes for all the other dozen or so species subsequently detected in an ensemble of some 38 of these cores. These cirrus cores span an extinction range of 1 to 5 mag, making them so-called translucent objects, which have never been studied before in astrochemistry. This range is very important because it is small enough that radiative (UV) processes are important, yet has sufficient column density to allow complex molecules to form in detectable amounts. The cirrus cores are, in fact, the long-sought transition region between diffuse clouds and dense molecular clouds, and they have allowed the transition chemistry to be traced in a dozen species to date. By knowing the physical conditions in detail, one can for the first time tailor chemical models to exact conditions and also derive reliable observational abundances to test these models. This has not been possible before in interstellar sources. The results have constituted a ringing endorsement of ion-molecule chemistry for essentially all species studied so far, including several (the sulfur species) that have until now not been explained. Ironically, H_2CO, which started it all, is the one species that cannot be formed by ion-molecule processes in sufficient quantity, but it can be formed on grains within the conditions found in the cores.

The 140 Foot continued to play an important role in this project after the CO data were gathered at the 12 Meter and analyzed. Nearly half of the analyzed species have been observed at the 140 Foot. The research from this single project is still going strong, nearly seven years after the initial 2 cm H_2CO detection at the 140 Foot.

The early days at the 140 Foot were certainly exciting ones, and the 140 Foot was very important in establishing the new field of interstellar molecules, because its infrastructure was able to provide rapid response to the rapid changes in the scientific requirements. The GBT will be similarly flexible, and can be expected to have success in molecular spectroscopy at a level comparable to that of the 140 Foot.

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George Seielstad and Martha Haynes at the 140 Foot Celebration.
[Photo courtesy Bob Rood.]

Tom Bania driving one of the Green Bank fleet of diesel Checker cabs.
[photo courtesy T. Bania]

Bob Brown (L), Tom Bania (R) at the 140 Foot Birthday Celebration, September 1995. [photo courtesy Bob Rood]

The Saga of 3-Helium *

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Abstract

The 140 Foot Telescope has been used for the past 15 years to study the cosmological, Galactic, and stellar astrophysics of the ^{3}He isotope by making precise determinations of its distribution, excitation, and abundance in the ISM of the Milky Way using measurements of the hyperfine transition of $^3He^+$ at a wavelength of 3.5 cm. Accurate ^{3}He abundances have important consequences for cosmology, the chemical evolution of galaxies, and the theory of low-mass stars. Over the years we have developed the techniques needed to measure the extremely weak, $\sim$ 1 milliKelvin, and wide, $\sim$ 1 MHz, spectral emission lines of $^3He^+$. The sensitivity of the GBT X-band receivers will allow the 140 Foot to measure $^3He^+$ in at least a dozen new sources—H II regions and planetary nebulae—that will probe both astrophysically strategic regions of the Galaxy and the ^{3}He production sites.

1. Prologue

The saga of ^{3}He is the story of the quest to detect interstellar radiation from the low-mass helium quantum mechanical analog to the 21 cm transition of atomic hydrogen. It is about the history of the 140 Foot Telescope and the spirit of the NRAO. It is also a personal story: I am of that first generation of students who were trained at the NRAO just after the 140 Foot became operational.

In 1957 at IAU Symposium No. 4 Townes gave the paper "Microwave and Radio-Frequency Resonance Lines of Interest to Radio Astronomy." His list contained the $^2S_{1/2}$, F=1–0 hyperfine transition of $^3He^+$ at 8665.66 MHz and began the quest for $^3He^+$. The detection of $^3He^+$ emission from the interstellar medium became spectral line radioastronomy's search for the Holy Grail. (Well, perhaps it was only a mini-Grail—a "Graillini.")

In 1967 the work of Goldwire and Goss stimulated the first major search for $^3He^+$ using the 140 Foot. Predmore began the observations that were to become his Ph.D. dissertation in August 1968 using a two stage cooled parametric amplifier designed and constructed at Rice University specially for the $^3He^+$ search. It was a prime focus instrument with a 90 K system temperature. Predmore observed a sample of Galactic H II regions but made no detections. His observing strategy evolved with time. Initially he frequency switched using a 50 channel filter bank covering a total bandwidth of 5 MHz, but when the Model II Autocorrelator became available he changed to total power observing with a 10 MHz bandwidth. It is fair to say that the spectra shown in Predmore's 1971 Ph.D. thesis were not impressive. He was forced into elaborate statistical analyses that resulted in the rather optimistic upper limits published in *Ap. J. Letters*.

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* Presented at the 140 Foot Telescope Birthday Symposium, September 1995.

In 1971 I graduated from Brown, narrowly escaped a sojourn in Vietnam, and began my graduate education in the Department of Astronomy at the University of Virginia. I quickly made my way over to the NRAO and cast about trying to join a research team. I made my first observations (21 cm H I) with the 140 Foot in the winter of 1972 with Butler Burton. The day before our observations began I got my first introduction to 140 Foot observing from Jay Lockman and Tom Carpenter. It was snowing hard and they were gracious enough to show the rookie the snow protection protocol: as the telescope moved to the Eastern Service Position, Tom and Jay took me outside to watch. They cleverly distracted me and positioned me directly under the lip of the dish. The snow was sticking to the surface and as I asked what they did to shake it loose Tom whipped a softball at the surface (Tom Carpenter in his prime could *really* throw a ball) and the bulk of the snow came down abruptly onto my face and down my back, burying me up to my knees—I went on to do my Ph.D. on CO in the Galactic Center using the 36 Foot on Kitt Peak where it was warmer and safer. However, I still kept observing in Green Bank and an H I map of the Galactic Center region was made for my thesis with the 140 Foot.

My graduate training at the NRAO was an enjoyable, productive, and illuminating experience. I learned a lot by example, of just how one needed to approach observational, experimental science. The NRAO at that time was a special place where the division between the senior staff and the students was at times informal and was always scientifically fruitful. I can still remember the day in 1972 when I realized that Dave Heeschen actually knew my name; the warm glow dissipated somewhat a few hours later when I began to fret about *why* he knew my name. I finished my degree in 1976 and took a postdoc at Arecibo where my NRAO experience made it possible for me to be an effective friend of the telescope when the post suddenly became open.

In 1976 Rood, Steigman, and Tinsley reexamined the issue of ^{3}He production by low-mass stars and concluded that these common objects should be enriching the ISM with ^{3}He at a prodigious rate. The ISM limits quoted by the Predmore group made no sense unless the ^{3}He$^+$ line intensities were far weaker than predicted by stellar evolution theory. Rood teamed with Tom Wilson to use the larger aperture of the MPIfR 100 m telescope in Effelsberg. After much Sturm und Drang they announced in 1979 a probable detection of ^{3}He$^+$ in W51. The lines were indeed weak and very hard to measure. Moreover, they were being clobbered by systematics at the 100 m and so they decided to return to the 140 Foot convinced that bigger was not necessarily better in regard to the ^{3}He experiment.

In 1980 I spent a year at UVa before moving permanently to Boston University. Bob Rood asked me to join the team and our first ^{3}He$^+$ 140 Foot observing run took place in February 1982. After about a year we managed to confirm the detection in W51 and found a much stronger line in W3 that had been masked in the 100 m data by systematic effects. We have been routinely observing ^{3}He$^+$ in a growing sample of Galactic ionized nebulae ever since.

We have made significant progress in determining the abundance of ^{3}He in the ISM. Accurate abundances require high signal-to-noise spectra as well as knowledge of the density and ionization structure of each source. We have shown that it is feasible to both detect and measure accurately the extremely weak ^{3}He$^+$ emission lines. We have also developed techniques to model sources and to derive accurate ^{3}He abundances. At present high quality abundances are available for only about a quarter of our potential sources.

2. The Importance of 3-Helium

The study of the origin and evolution of the elements is one of the cornerstones of modern astrophysics. For any given isotope of an element a crucial step is the observational determination of the abundance of that isotope and how that abundance varies temporally and spatially. Potentially observable sources of ionized gas include H II regions and planetary nebulae located throughout the Galaxy. ^{3}He can serve both as a probe of cosmology and stellar/galactic evolution.

• The Big Bang theory for the origin of the Universe predicts that during the first ~ 100 seconds significant amounts of the light elements (^{2}H, ^{3}He, ^{4}He, and ^{7}Li) were produced. The relative abundances of these light elements depend on various factors such as the average density of baryons in the Universe, the rate of expansion of the Universe, and details of particle physics. Studies of the abundances of these elements can provide information about the physical state of the Universe at these early times. When combined with other light element abundances our present limits on the ^{3}He abundance either begin to challenge Standard Big Bang Nucleosynthesis models, or call into question some of the standard assumptions concerning the chemical evolution of ^{3}He.

• Nuclear fusion reactions inside stars will change the relative amounts of the light elements from the primordial abundances produced by the Big Bang. Theory predicts not only that common solar-type stars are net producers of ^{3}He but also that the mass lost from winds generated at advanced stages of their evolution and the final planetary nebulae should be substantially enriched in ^{3}He. Planetary nebula ^{3}He abundances are therefore important tests of stellar evolution theory since these low-mass, evolved objects are expected to be significant sources of ^{3}He. We have confirmed the stellar production of ^{3}He in the planetary nebula NGC 3242. Its high ^{3}He/H abundance is consistent with the predictions of standard stellar models but it has yet to be demonstrated that the majority of planetary nebulae show enhanced ^{3}He abundances.

• The shedding and redistributing of processed material by evolved stars occurs throughout the interstellar medium. Stars then form from this gas, and subsequent evolution by this new stellar generation results in further processing of the elemental abundances. Measurement of the present ^{3}He abundance should therefore be an important diagnostic of chemical evolution in the Galaxy. Regions of recent star formation, H II regions, sample the result of the chemical evolution of the interstellar medium since the formation of the Galaxy. The ^{3}He/H abundance ratio is expected to grow with time and to be higher in those parts of the Galaxy where there has been substantial stellar processing. We have found source-to-source variations in the ^{3}He/H abundance of at least a factor of five for Galactic H II regions. The observed abundance pattern across the Milky Way, however, cannot be easily explained by existing chemical evolution models.

3. The 3-Helium Experiment

The observational sample now includes $^3He^+$ spectra taken toward 43 Galactic sources which are H II regions and planetary nebulae. The H II regions are mostly within, but located throughout, the Milky Way's Population I disk, from the Galactic Center to the far reaches of the outer Galaxy. In recent years we have concentrated on measuring extremely accurate emission line parameters for a few H II regions. Instrumental spectral baseline frequency structure limits the accuracy of the determination of these parameters. This structure, arising primarily from standing waves, is real; it *must* be removed to get accurate line measurements. We have developed observational protocols and baseline modeling procedures that can produce extremely accurate spectra.

When we first began to observe $^3He^+$ at the 140 Foot we used the Maser Upconverter X-band receiver in conjunction with the Model IV Autocorrelator. There have since been many changes to the hardware and software but the Model IV endures. At first we were completely swamped by systematic effects beyond those of the standing wave frequency structure. One example is that initially we found the baselines depended in an enormously sensitive way on the IF frequency we chose. Working with Chuck Brockway we tracked this down to a problem in the IF distribution system. We forged an excellent working relationship with the Green Bank technical staff and chipped steadily away at all the systematic effects we could find. Over the years we have explored the spectral baselines produced by a variety of observing techniques in order to discover the best approach. We have tried total power, noise adding, beam switching, and frequency switching. We have modulated the instrumental focus $\pm\lambda/8$ on a variety of timescales in order to partially cancel out the primary standing wave between the Cassegrain house and the subreflector.

Figure 1 shows spectra taken with our current observing technique toward the H II region W3. We make total power position switched observations using 6 minute integrations. We track precisely the same path in azimuth and elevation during the ON and OFF measurements and modulate the focus (MODFOC) between $\pm\lambda/8$ on one minute timescales. It is clear that instrumental frequency structure remains in the baselines and that modeling the baselines is necessary.

Compared with the Maser-Upconverter system, the new GBT X-band receiver ($T_{\rm sys} \leq$ 40 K) is a factor of 5 more sensitive at 8.7 GHz. An indication of what is now possible to do with the 140 Foot at X-band is given in Figure 2, which shows the $^3He^+$ spectrum for the H II region G29.94-0.0 taken during the March 1996 observations of program B609. *This is a new $^3He^+$ detection and among the few single-observing-epoch detections ever made.* Furthermore, the $\sim$4 mK intensity, $\sim$30 km s^{-1} wide $^3He^+$ line, which is among the strongest we have measured, is clearly apparent in an integration that took a mere 1.5 sidereal passes of the source to accumulate. This was 27 hours of integration resulting from combining two receivers detecting two orthogonal circular polarizations. Instrumental baseline effects make single-epoch line parameters uncertain, however, and so the accurate determination of the area under the line awaits further multiple-epoch measurements (see, e.g., BBBRW94).

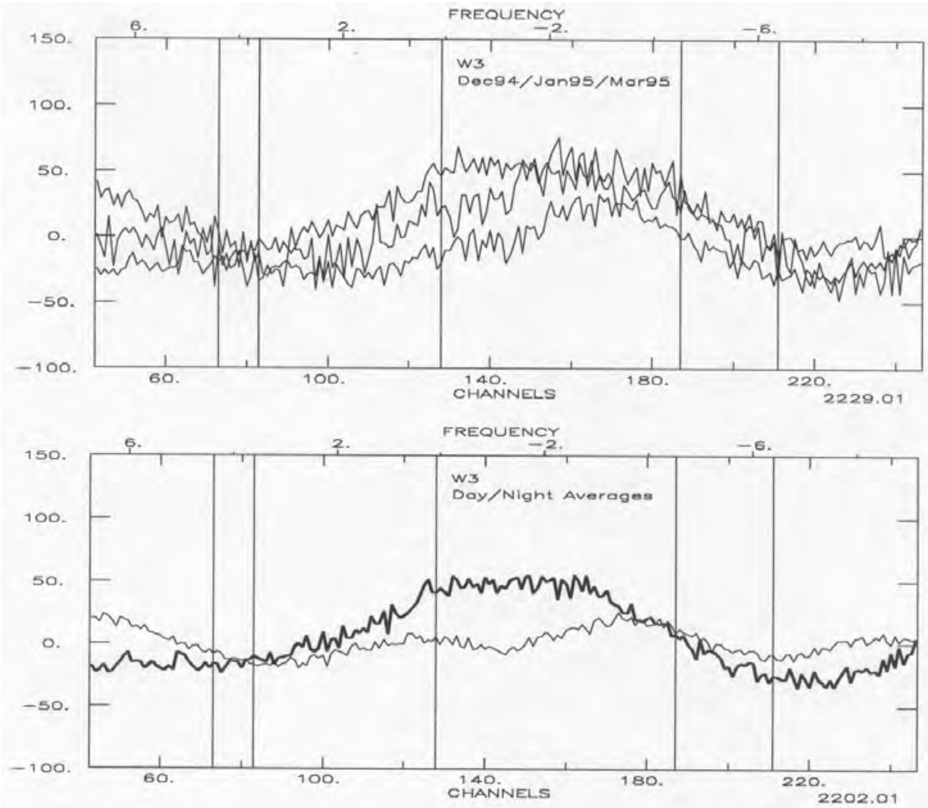

Fig. 1— Examples of 140 Foot Telescope spectral baselines at X-band. Shown are unprocessed (i.e., for the H II *region Tsys*(ON–OFF)/OFF without any baseline removed) total power position switch spectra taken of the W3 H* II *region. The intensity scales are in milliKelvins of antenna temperature and most of the 20 MHz bandwidths are shown. Vertical lines flag the positions of various spectral transitions. From left to right they are:* C171η, He171η, H171η *(Δn=7),* ^{3}He$^+$*, and* H213ξ *(Δn=14). Each spectrum results from only a few hours of integration time. Note that despite modulating the focus $\pm\lambda/8$ there is a residual quasi-sinusoidal ripple in the instrumental baseline that results from standing waves produced not only by the ~11 K source continuum but also by the Sun in distant sidelobes. This instrumental frequency structure in spectral baselines, moreover, shifts its phase during the sidereal year (TOP) and also when the Sun is somewhere in the sidelobes (BOTTOM). We have found that this phase change averages into a flatter baseline when observations made over many epochs spaced in time throughout the sidereal year are combined. Nonetheless, the instrumental baseline needs to be modeled and subtracted. The multi-epoch average spectrum for W3 gives intensities and linewidths for the* H171η *transition of 7.2 mK and 37.7 km s^{-1}; the* ^{3}He$^+$ *values are 3.1 mK and 24.5 km s^{-1}. This is the observational conundrum of the* ^{3}He$^+$ *experiment: the line intensities and widths are of the same order as the instrumental frequency structure which is exacerbated by standing waves from the continuum emission of the ionized nebulae.*

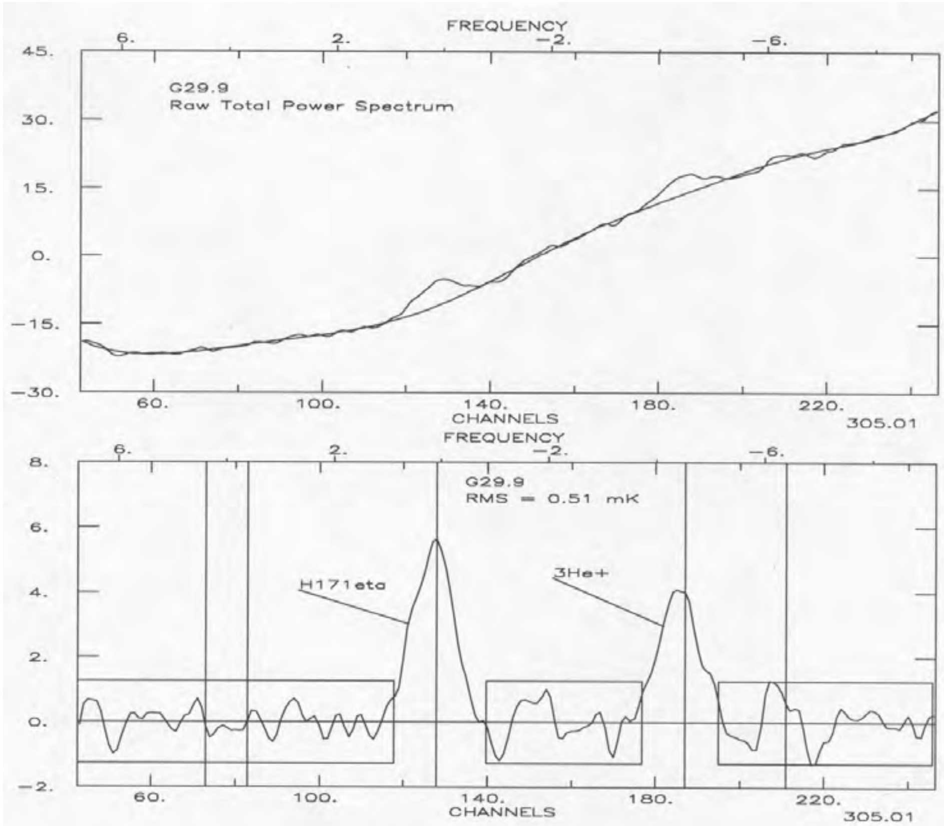

Fig. 2— Example of GBT X-Band receiver performance on the 140 Foot for the Galactic H II region G29.94–0.0. TOP: The raw total power position switched spectrum. Superimposed on the spectrum is the polynomial model for instrumental baseline. BOTTOM: The ^{3}He$^+$ *spectrum after the instrumental baseline has been removed. In both cases the intensity scale is given in milliKelvins of antenna temperature and the velocity resolution is 8.1* km s^{-1}.

At this point our techniques: (1) allow us to measure weak ($\sim$ 10 mK) recombination lines to $\sim$ 10% accuracy within any single observational epoch; (2) give an inter-epoch calibration to within 10% for the spectral transitions of interest; and (3) produce results that make astrophysical sense. Here "astrophysical sense" means that the measured ^{4}He/H ratio does not vary with the order of the recombination line transition; the recombination line strengths and ^{4}He/H ratios do not vary with time; and the H recombination line strengths decrease smoothly with the order of the transition. Further, our source models (see below) can account in a *physically consistent way* for the intensities we observe in both the continuum and recombination lines.

None of these things can be accomplished without modeling the spectral baselines to account for instrumental frequency structure. In particular, without baseline modeling the spectra neither make astrophysical sense (in the manner described above) nor do they obey the radiometer equation (see Figure 4).

4. Status of the H II Region Observations

The H II region observations are described in BBBRW94, Rood et al. (1995), and BBRW96. The results can be summarized:

- *Much longer integration times:* We have obtained extremely accurate line parameters for a few H II regions. Several sources now have more than a 100 hours of integration.

- *Multi-epoch observations:* The phase of the instrumental spectral baseline structure varies with sky frequency and thus with the time of the year. By obtaining smaller amounts of integration time scattered around the year the magnitude of the baseline structure in the averaged data can be reduced.

- *Accuracy of baseline removal:* The spectral baseline structure is real, arising primarily from standing waves in the telescope. To monitor the accuracy of our baseline removal techniques we introduced the concept of "baseline noise." Plots of the parameter $\mathcal{Q} = \sqrt{t_{\rm intg}} RMS/T_{\rm sys}$ against log $t_{\rm intg}$ demonstrated the necessity of removing a very high order baseline. Criteria have been developed which indicate when baseline removal might introduce a substantial error.

- *Accounting for weak recombination lines:* At the levels we are now reaching the spectrum is beginning to fill up with weak recombination lines which were not considered in our earlier work. In particular the H213ξ ($\Delta\, n = 14!$) line lies close to the ^{3}He$^+$ transition. Not accounting for it can distort the baseline shape in the region of the ^{3}He$^+$ line.

Figure 3 shows our experimental technique in a dramatic way. We have averaged together *all* the sources (H II regions and planetary nebulae) we have ever observed with the 140 Foot to produce a composite source ^{3}He$^+$ spectrum. This is the deepest integration ever made at centimeter wavelengths. Yet, as can be seen in Figure 4, the 140 Foot radiometer system continues to integrate down as theoretically expected for at least $\sim$2,200 hours.

5. The Abundance of 3-Helium in the Milky Way ISM

Determining the primordial abundance of any isotope involves three steps:

1) *Making a measurement.* In many cases the measurement itself is so difficult that the following steps are often overlooked.

2) *Converting the measured quantities to abundances.* The necessary quantity is x/H. As emphasized here below, making the transition from x to x/H is nontrivial.

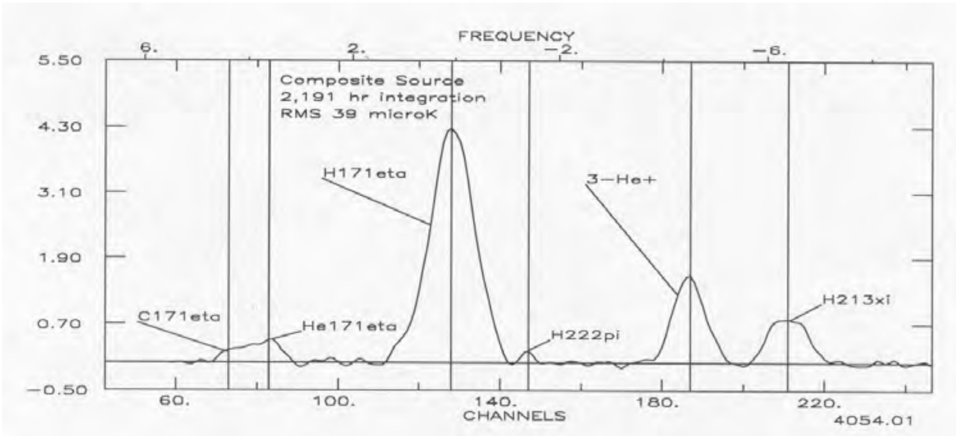

Fig. 3— The ^{3}He$^+$ *spectrum obtained from averaging all data accumulated with the 140 Foot Telescope between February 1982 and May 1996. That is, 43 Galactic sources, which included both H* II *regions and planetary nebulae, were aligned in LSR velocity and averaged. These ionized nebulae emit a forest of recombination lines from H,* ^{4}He, *and C. Vertical lines flag the positions of various transitions. From left to right they are:* C171η, He171η, H171η *(Δn=7),* H222π *(Δn=16),* ^{3}He$^+$, *and* H213ξ *(Δn=14). Although some sources were not detected in* ^{3}He$^+$*, this multi-source average clearly shows a substantial* ^{3}He$^+$ *emission line. The intensity scale is given in milliKelvins of antenna temperature and the velocity resolution is 8.1* km s^{-1}*. This ~2,200 hour integration has an r.m.s. noise level of 39 microKelvin.*

3) *Inferring a primordial value from the observed abundance ratios.* There are sources and/or sinks for all of the isotopes of interest. One must account for these in arriving at the primordial abundance. This requires: first, a theoretical understanding of the production, destruction, and evolution of each isotope; second, a sufficient number of observations both to calibrate the free parameters of the model and also to provide a consistency check.

In converting the observed ^{3}He$^+$ hyperfine line parameters to an abundance, one is faced with the problem that the strength of the collisionally excited hyperfine line from an H II region is proportional to the source density whereas the hydrogen abundance which is derived from the thermal free-free bremsstrahlung radio continuum emission depends on the square of the density. Thus the H II region must be modeled. In fact, determining the hydrogen abundance to high accuracy is as difficult as measuring the D, ^{3}He, or ^{4}He abundance. Recent ^{3}He/H abundance ratio determinations are summarized in Balser (1994), Rood, et al. (1995), and BBRW96 where ^{3}He/H abundances are derived using models that account for source density and ionization structure as well as non-LTE effects and pressure broadening by electron impacts. The models are constrained by the observed ^{3}He$^+$ and recombination line parameters and the 3.5 cm continuum emission measured with the VLA D-array and the MPIfR 100 m telescope. Models derived in this manner not only account for the continuum flux from the sources but also reproduce the observed recombination line intensities.

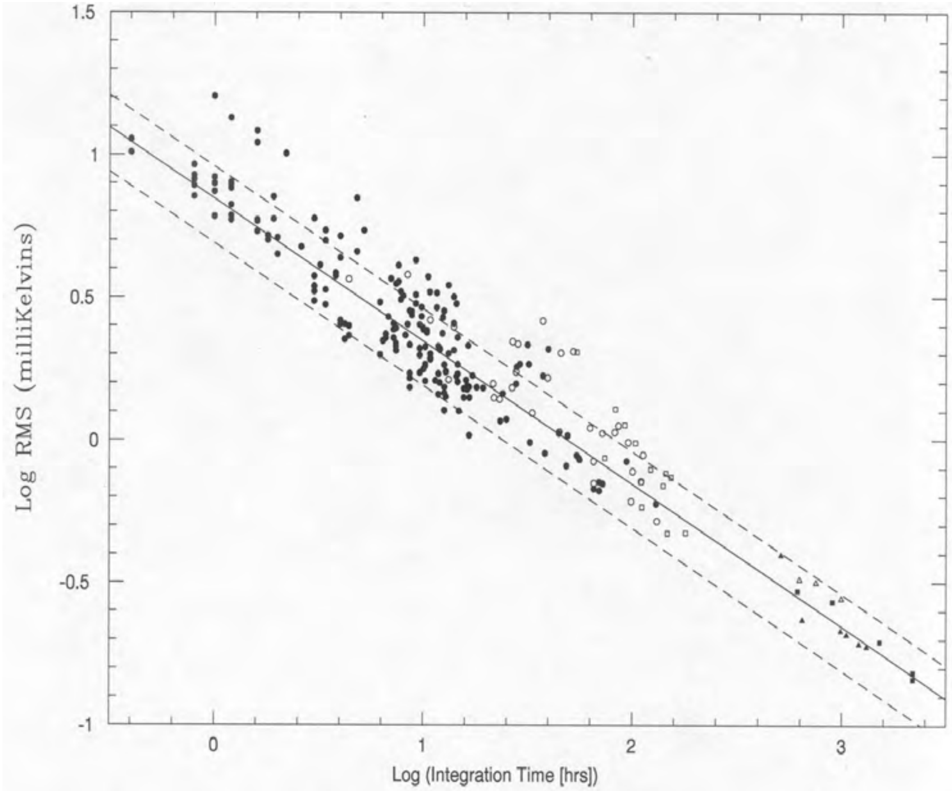

Fig. 4— The empirical radiometer equation for the $^3He^+$ *experiment sample of H* II *regions and planetary nebulae. The lines indicate the noise performance expected for a perfect radiometer. The solid line assumes a system temperature of 50*K *whereas the dashed lines refer to system temperatures of 35*K *and 65*K. *Symbols denote whether data were acquired prior to June 1990 (open) or afterward (filled). Individual sources are plotted as circles; composite sources are drawn as triangles. Composites over the entire time period (the pre- and post-UNIPOPS eras) are drawn as filled squares. The two points at the longest integration time refer to independent baseline fits to the Figure 3 spectrum. The difference is whether or not the region of the* H222π *transition is included in the baseline model.* **Note that the RMS noise measured by the 140 Foot Telescope spectrometer integrates down as expected for at least 2,200 hours.**

The ^{3}He abundances we derive have the following astrophysical implications:

• Regions of recent star formation, H II regions, sample the result of the chemical evolution of the interstellar medium since the formation of the Galaxy. The ^{3}He/H abundance ratio is expected to grow with time and to be higher in those parts of the Galaxy where there has been substantial stellar processing. The H II region abundances we derive range from $^3\mathrm{He/H} \sim 1$—5×10^{-5}. The abundance differences are real, i.e., the consequence neither of observational error nor of the source modeling (e.g., Rood et al. 1995). The observed abundance pattern across the Milky Way, however, is difficult to reconcile with current models for the chemical evolution of the Galaxy which predict a negative gradient in the ^{3}He/H abundance with galactocentric distance. Indeed, some authors have suggested local production of ^{3}He ("self-enrichment": Rood, et al. 1995) and/or local destruction (Olive et al. 1995).

• The best cosmological limits from ^{3}He currently are those based on the abundance derived for the Galactic H II region S209. Balser et al. (1996a) use improved observations and source models for S209 to constrain the baryon-to-photon ratio, η. Using the other observed light elements and standard Big Bang nucleosynthesis (Wilson & Rood 1994), they find $\eta = 4.15 - 4.50 \times 10^{-10}$. These limits on the ^{3}He abundance begin either to challenge Standard Big Bang Nucleosynthesis models or to call into question some of the standard assumptions concerning the chemical evolution of ^{3}He.

Epilogue *

It had been a very long experiment.

They considered this in silence.

Finally, Bania spoke, very slowly and carefully. For a change.

"I look at it all like this," he said. "Before I did this damn experiment, I was like everyone else. You know what I mean? I was confused and uncertain about all the little details of life." "But now," he brightened up, "while I'm still confused and uncertain it's on a much higher plane, d'you see, and at least I know I'm bewildered about the really fundamental and important facts of the universe."

Rood nodded. "I hadn't looked at it like that," he said, "but you're absolutely right. The 3-He experiment has really pushed back the boundaries of ignorance. There's so much about the universe we don't know."

They both savored the strange warm glow of being much more ignorant than ordinary people, who were ignorant of only ordinary things.

* *With apologies to Terry Pratchett.*

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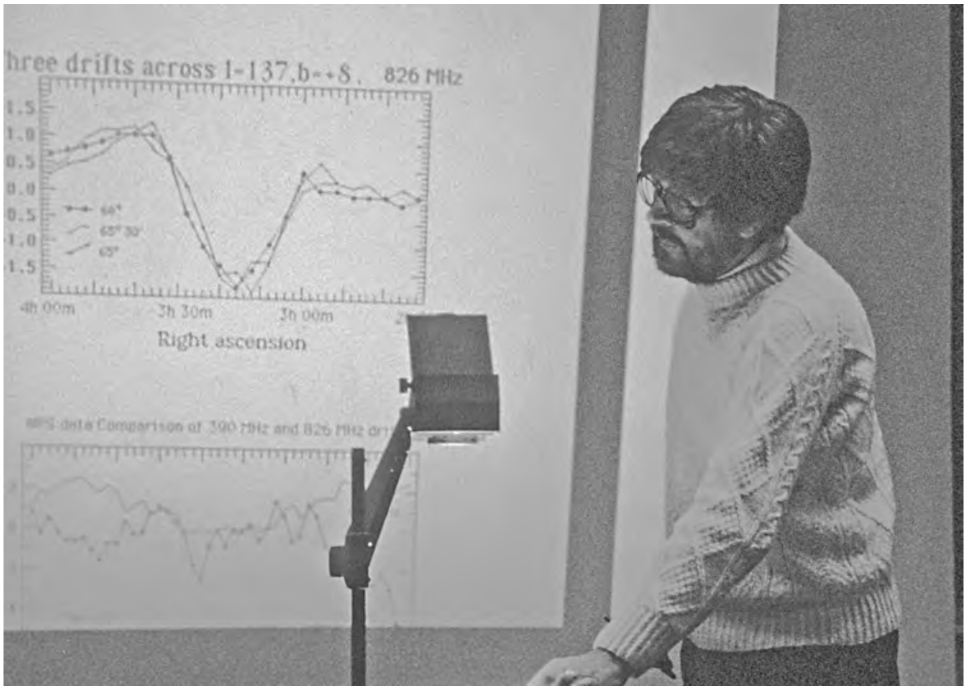
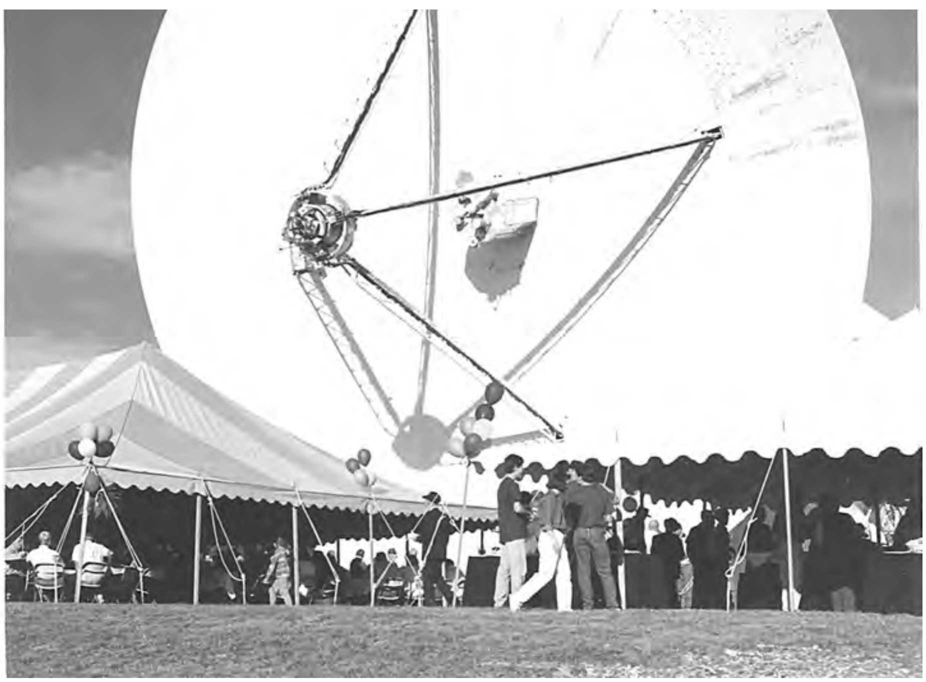


Gerrit Verschuur at a GBT planning meeting, ca. 1989.
[photo courtesy R.Rood]



A scene at the 140 Foot 30th Birthday Celebration

The 21cm Zeeman Experiment on the 140 Foot *

Gerrit L. Verschuur
Lakeland, TN 38002

In early 1960 I received an offer I could have refused. Instead, a year later I traveled from South Africa to Jodrell Bank to work on the 21 cm Zeeman experiment with Rod Davies and his team. In 1965, I handed in my Ph.D. thesis, which was largely devoted to showing why the Jodrell Bank Mk I was an inadequate instrument for 21 cm polarization studies, including the HI Zeeman experiment. In particular, I showed that the alleged magnetic field signature apparently observed toward Taurus A, and published with much fanfare in 1962, was severely flawed (Davies, Verschuur & Wild 1962). The approach to my thesis defense was a traumatic time but fortunately no sensitive issues were mentioned at that examination and I recently learned that my external examiner, Peter Scheuer, has no memory of that momentous occasion.

The irony of recounting this story today, and doing so here, is that in the next issue of ApJ I have two papers showing that observations of the Zeeman effect in HI emission features obtained on the 140 Foot as well as with the Hat Creek telescope in the last decade have all been severely compromised by systematic effects (Verschuur 1995a,b).

Anyway, after I received my Ph.D. I was not allowed to publish the negative results outlined in my thesis for reasons that had nothing to do with science. Instead, I gave a talk at IAU Symp. 31 in Noordwijk in 1966 in which I withdrew the Tau A field claim (Verschuur 1967). At that meeting Dave Heeschen offered me a job at the NRAO.

Soon after arrival in Charlottesville in August 1967, locals began to ask "Will you be doing the Zeeman experiment on the 140 Foot?"

"No way," I said, wisely.

As pressure mounted I began to nose around and learned that T. K. Menon had had an HI Zeeman feed built and tried it out some years before. It had been used for project Ozma as well, and I sometimes wonder whether we should have stuck to using it for SETI!. By now it might have picked up a few more signals … well, perhaps not!

I also learned that Sam Goldstein had tried his hand at the Zeeman experiment and when I saw his data I shuddered. In fact, lots of folks had tried their hand at the Zeeman experiment at 21 cm and gotten nowhere, except to show that the Jodrell Bank Tau A result was nonsense; e.g. Morris, Clark & Wilson (1963) at Caltech, and Weinreb (1962) at MIT. So I dug out the feed and fiddled with it on the roof in Charlottesville and was convinced that it was halfway decent. I submitted an observing proposal for two days of time to test the feed on the 140 Foot. Soon thereafter Bill Howard called me into his office and said that the referee

* Presented at the 140 Foot Telescope Birthday Symposium, September 1995.

instructed that I be given 10 days to do the Zeeman experiment. "And while you're at it, we'd like you to get the 384 channel correlator going," he said. That was the last time I, and probably anyone else for that matter, was given five times as much observing time as had been requested.

In May 1968 I observed with absolutely no way to see anything on line. The programs for processing the correlator output had not been finished. Fortunately, there was a paper printer that spewed out 384 numbers whenever I pushed a button. This I did after every 30 min. integration. As a result, I was able to ascertain that HI signals were permeating the system.

During that observing run and for two months thereafter I did a lot of signal averaging the old-fashioned way, by hand. The two sources which had become "obvious" candidates for finding a field in the HI Zeeman experiment were the narrow and deep absorption lines in Tau A, and the Cas A local, Orion arm feature. It turned out that the Perseus arm feature in the Cas A spectrum was included in the 384 channels bandpass, which at the time was regarded as a potential bonus and it certainly wasn't the focus of attention.

When I got off the telescope I still had reams of numbers to add and average and I continued doing this even on a flight to Tucson for a workshop with the Kitt Peak people. By the time I got to Tucson I was certain that the Cas A Orion arm showed no field signature but there was this block of Tau A data that showed an effect in 60 hours of integration. Another 12 hours didn't show it. Based on experience, I trusted the data that didn't show it! This I duly reported in Tucson and I doubt anyone recalls that non-event.

Back in Charlottesville we finally got an off-line program running to process 384-channel data and I was able to perform the necessary integrations and required gain corrections to the data in a formal manner rather than multiplying and subtracting endless streams of numbers. On July 4, I came in early to plot all the Cas A results, by hand because the first off-line program had no plotting capability. I still recall the huge sheets of centimeter graph paper I had taped together to accommodate all 384 channels. When I plotted the results, Barry Turner, my perpetual officemate back in those days, was there as well. After all, a little thing like a July 4th holiday did not stand in the way of his appointed rounds.

I first plotted the absorption line profile, which showed the Perseus arm to be well within the edge of the band. Then I began plotting the difference channel signal (Stokes parameter V) and found that the Orion arm data looked like the baseline region: pure noise. I continued plotting and a hundred channels later I reached the Perseus arm. There the points started to move away from noise! There it was, a huge signal! Barry was immediately convinced, which meant I could celebrate.

Elated, I drove home and met Bill Howard coming in, probably planning some fiendish scheme to make life difficult for astronomers. I stopped him in the middle of the street and told him that I had found the signal. Later, closer examination showed that the signals were due to fields of order 10 and 20 μG in two of the features in the Cas A Perseus arm profile. Within days, the off-line programs allowed me to discover that the 12-hour block on Tau A which showed no signal had been gathered with a broken correlator. The other 60 hour integration was good. Tau A showed a field of order 3.5 μG.

I chose to submit my paper to Phys. Rev. Letters (Verschuur 1968). Within a couple of weeks after it appeared, Davies, Booth & Wilson (1968) published in Nature. They claimed that I had confirmed a detection made by my friend Pat Wild at Jodrell Bank and referenced his Ph.D. thesis. Frantic, I phoned a Jodrell buddy and asked him to send the relevant pages from Pat's thesis. When they arrived I saw that Wild's data showed the one positive-going peak on the Perseus feature and not the other two negative-going signals. Wild never dreamed he'd detected a Zeeman effect signal when he wrote his thesis, and its results were never published. I was very, very upset, and Dave Heeschen gave me some good council: "Time will tell who was right," he told me, and suggested I not worry. He was right.

In December 1968 I was back on the telescope, this time with an on-line oscilloscope monitor to see the signal, and an off-line program in Charlottesville capable of doing the integrating. I looked at Cas A and in 30 minutes could see the signal on-line!!!! That was after 8 years of being involved in the experiment. I decided to look at Orion A. In 15 minutes there was a signal. I went through the roof with joy. Now the whole Galaxy lay waiting. All I had to do was point the telescope this way and that, and Zeeman effect signatures would come flooding in and we'd soon have this business of the strength of the interstellar magnetic field cleared up. How incredibly wrong that hope turned out to be.

But was the Orion A Zeeman effect signal real? I'd need to check by observing at a second feed angle. The theory was that if the signal was due to an interaction between polarized sidelobes and HI emission or absorption structure, feed rotation should produce a different signal as the polarized beam rotated on the sky. So we rotated the feed and I mapped the polar diagram, which back then was done by reading off the chart recorder and sketching it by hand. This I did, and there were slight differences, enough to possibly affect the signal if it were spurious. It was nearly dinner time so I asked the operator to integrate again on Cas A, print out the profile, and then observe Orion A and print it out.

When I got back from dinner the two plots showed nothing, just noise.

This was one of those horrible moments in life when one's nightmares become reality. I slumped into the seat by the control desk and recall saying to the operator something like "This is it. This is enough." First there had been the trauma of the thesis study in the context of politics at Jodrell Bank, followed by the withdrawal at Noordwijk, then a Phys. Rev. Letters paper announcing a detection, a confirmation from Jodrell Bank, and a signal before dinner. Now there was nothing. Was it polarized sidelobes after all? How could they have fooled us so badly?

I sat there resigned to the eternal damnation of the Zeeman experiment (which has continued ever since!) and stared at the instrument rack by the control desk. Then I noticed that a switch had not been reset to a little red dot. The correlator was marching to a different drummer from the front-end switch. Hallelujah!

The rest, as they say, is history. During that observing run, with Mike Balister's help, I got the sign of the field figured after he radiated the dish with a helical feed. His help was needed because the last four days of the observing run I was in bed with flu and had a 104° fever. During that run I also found a field toward M17. That made the score: Cas two fields, Orion one or perhaps two, Tau A one, and M17 one. Total five or six detections. The major papers on this experiment are Verschuur (1969a,b).

If I had stopped then I would have been ahead of the game, because my interpretation of the data that have since been published based on observations made elsewhere, and on my own observations with the 140 Foot over the last nine years, is that subsequently no further HI Zeeman effects using a single dish have been detected either in absorption or emission. The problem is that when observing complex HI structure with a beam that has polarized sidelobes of opposite polarization offset on the sky, you can produce horrible signals in the switched (Stokes parameter V) channel of the receiver. This has befouled the 140 Foot data on emission profiles and made it all but impossible to find fields in absorption close to the plane, which was obvious even back in 1970. Since then the polarized sidelobe amplitudes have been reduced by a factor of three and the spurious signals have gone down by the same amount.

So what do the current HI Zeeman effect data tell us? Based on dozens of upper limits and dozens of data corrected for polarized sidelobes, the general Galactic magnetic field is $<2\ \mu$G. In contrast, the fields in five dense absorption features, which are all associated with star-forming regions, are in the range $10 - 50\ \mu$G and are probably amplified by shocks.

This is it! A summary of 35 years of work, on and off, on two telescopes. How very different my career would have been if nature had been kinder, or if polarization were easier to measure at the relevant levels.

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Jay Lockman at the 140 Foot 30th Birthday Celebration. Mike Smith (behind Jay) plays the bass.

The Bing Brothers band playing at the 140 Foot Celebration, September 1995. Left to right: Danny Arthur, guitar; Mike Smith (hidden), bass; Mike Bing, mandolin; John Blisard, banjo; Dave Bing, fiddle.

Dave Shaffer (L) and Dave Hogg(R) in 1977.

(L to R)Len Howell, Howard Brown, Dave Shaffer at the 30th Birthday Celebration.

The 140 Foot and the Early Days of VLBI *

David B. Shaffer
RadioMetrics Inc.

Marshall Cohen told us the story of the initial development of the VLBI technique (then, and for many years afterward, referred to as just "VLB" by the NRAO-Caltech group of practitioners), leading up to the first successful experiments involving the 140 Foot. I would like to take the story a few more years along, a time during which the VLBI technique certainly bore out the expectations of its developers that it had much to contribute to our understanding of radio sources. I will deal only with continuum observations, leaving the story of spectral line VLBI to Jim Moran.

Although I was a summer student at Green Bank in 1966 and 1967 (and two more years, too!), I was not aware of the nascent VLBI efforts underway at the time. I was first introduced to the technique when I joined Marshall as a grad student at Caltech in the fall of 1968. George Purcell, subsequently an NRAO post-doc, and I were Marshall's first students at Caltech (Marshall also came to Pasadena in 1968), and we soon were playing our roles among the number of hands-on schedulers, tape hangers, and correlation assistants that were necessary to carry out a VLBI experiment in those early days. (I suspect many new users of the VLBA do not appreciate how easy they have it now, compared to the "good old days.")

Once fringes had been found on the Green Bank to Maryland Point baseline, the rush was on to extend both the physical and numerical (i.e. number of wavelengths) baselines. Antennas across the US and overseas were soon hanging tapes, too. The 140 Foot participated in almost all of these observations.

Within a month or so of the initial 610 MHz observations on a baseline of a few hundred kilometers, the baseline was extended to 800 km, from the 140 Foot to MIT's Haystack/Millstone complex northwest of Boston, with observations at 18 cm. Within another month came transcontinental 18 cm observations between the 140 Foot and the (now departed) 85 foot antenna at UC Berkeley's Hat Creek Observatory in California. The next step was across the water, with observations at 610 MHz between Green Bank and Arecibo, Puerto Rico. At this point, it was fully clear that there were no scientific obstacles to extension of VLBI observations between antennas anywhere on the earth. Only logistics or politics would interfere.

Early in 1968, less than a year after the first successful operation of the Mk I system (not called that until the Mk II came along!), truly intercontinental observations took place between the 140 Foot and the 85 foot antenna at Onsala, Sweden, at both 18 and 6 cm. The physical baseline was more than 6000 km, a significant increase over the initial few hundred km. In another year or so, by 1969, baselines stretched around the world: the OVRO 130 foot in California and Parkes, Australia, played together on a baseline exceeding 10000 km. Also in 1969, the

* Presented at the 140 Foot Telescope Birthday Symposium, September 1995.

Iron Curtain was nudged aside by VLBI, as observations between the 140 Foot and Crimea were made at 5 GHz.

While baselines grew exponentially, so did the number of sources observed in the early VLBI experiments. The first experiment "produced fringes" (the aficionados term for "detecting sources") on only 4 objects. The second experiment (to Haystack) detected 28 sources. The number of sources observed in the next several experiments (the first with really long baselines, to Hat Creek and Arecibo) were fewer, as our knowledge of which sources would make fringes was developing. Before the end of 1969, however, close to 100 radio sources had been observed successfully on the longest baselines—those between the 140 Foot and Sweden and between California and Australia.

Perhaps some of the most interesting particulars about these early observations are the speed with which they were correlated, analyzed, and published. Clark et al. (1967) observed between July 22 and July 26. The "received" date on their paper to Ap. J. Letters is August 10 (two weeks later!) and the paper was published in the September issue. Cohen et al. (1968) and Kellermann et al. (1968) were submitted within months of the observations and were published in the Ap. J. Letters issues of the month immediately after the "received" date.

The results from the early VLBI observations made with the 140 Foot are found in the following papers:

Bare et al. (1967): This is the first 140 Foot VLBI paper, reporting the successful observations between Green Bank and Maryland Point.

Clark et al. (1967): Reports the detection of 3C273 on the 140 Foot – Hat Creek baseline at 18 cm.

Cohen et al. (1968): A more comprehensive report on the 140 Foot – Hat Creek observations.

Kellermann et al. (1968): Reports on the 18 and 6 cm observations between Green Bank and Onsala.

Clark et al. (1968): Reports on the 18 cm observations between Green Bank and Haystack.

Kellermann et al. (1971): Reports 18 and 6 cm results for the baselines from the 140 Foot to Haystack, Hat Creek, OVRO, Onsala, and Crimea, as well as from OVRO to Onsala and Crimea. This is the first comprehensive attempt to make astrophysical sense of a big set of observations.

The 140 Foot played an important role in the first PhD theses based on continuum VLBI observations. Late in 1969, Alan Whitney and Bob Preston of MIT made their Mk I thesis observations between the 140 Foot, Haystack, and OVRO. George Purcell's Caltech thesis was the first based on MK II VLBI observations using mid-1971 observations between the 140 Foot and OVRO. Paul Hemenway (U.

Va.) and I (Caltech) used the Mk II in mid-1972 for our observations. Paul actually used two antennas of the Green Bank interferometer and the two 90 foot antennas at OVRO to make simultaneous dual baseline observations. I used the 140 Foot, the 85 foot at the Harvard Radio Astronomy Station in Fort Davis, Texas, and the OVRO 130 foot: the first VLBI "array."

I think we should look not only at the contributions of the 140 Foot to development of VLBI, but in fact at the larger role which NRAO has played in the development of this technique. In the 25 years or so since the initial success between the 140 Foot and Maryland Point, NRAO has had an important part in the development of higher sensitivity recording systems, and finally, the construction and operation of the VLBA. Table 1 lists a number of NRAO contributions. Last, but not least, we also need to recognize two long-time NRAO staffers without whom this development would probably not have occurred: Barry Clark and Ken Kellermann. These two pushed and pulled within NRAO to make sure that the items in Table 1 became a reality.

TABLE 1

NRAO Contributions to VLBI

140 Foot Antenna
Mk I VLBI System
Portable VLBI receivers for 5 GHz and 10.7 GHz used at
Onsala, Crimea, Parkes, HRAS
Mk II VLBI System
IF converter & formatter
Recorders
Ampex VR660 - maybe not a positive contribution!!
IVC
VCR - under the influence of Alan Yen
Correlator
VLBI Network studies
Satellite link - joint with NRL/NRC(Canada)
Mk III System - joint with NASA(Goddard)/Haystack
VLBA
Orbiting VLBI - VSOP and Radioastron
VLBA correlator
Ground station
Ground radio telescopes

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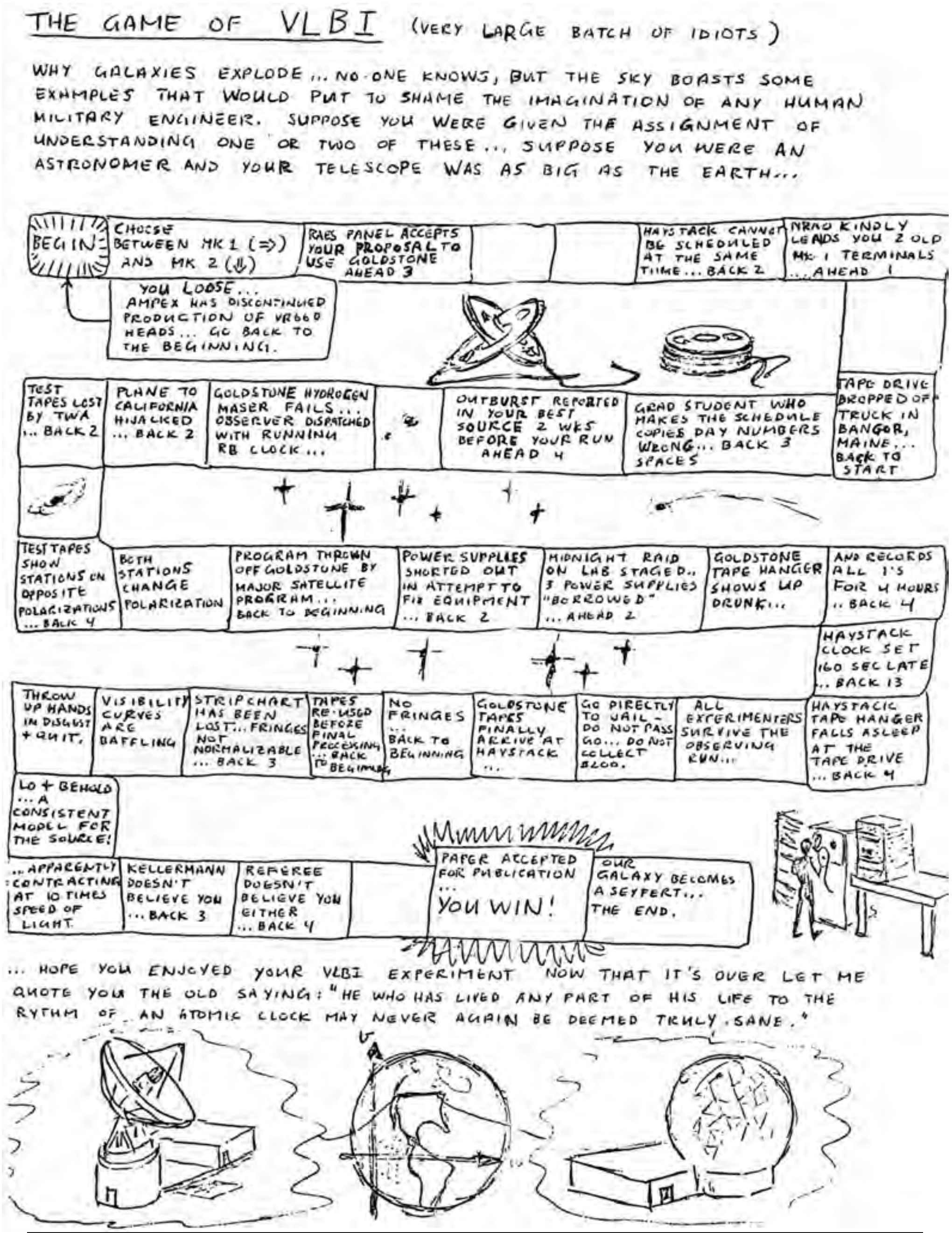

"The Game of VLBI:" Shaffer dramatizes the many pitfalls of early VLBI. From the Observer, August 1974.

Early Spectral Line VLBI with the 140 Foot Telescope and Other Reminiscences*

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I. Preamble

One statement that Dave Shaffer made in his talk is not completely correct. He showed an overhead which indicated that by 1969 VLBI observations were easy enough that they could be done by students. Apparently he was referring to Caltech students; at MIT, student involvement in VLBI started in 1967!

II. Anthropomorphizing the 140 Foot Telescope

I learned how to do radio astronomy on two telescopes, the 120 Foot telescope of the Haystack Observatory and the 140 Foot telescope in Green Bank. Both were in the middle to late stages of construction in 1963, the year I began graduate school at MIT.

As I was driving here yesterday, I began wondering what kind of person each telescope would be if it were a relative somewhere on my family tree. At the risk of exposing myself to unwanted psychoanalysis, here is what I came up with. I view the Haystack telescope as a father figure. I spent a great deal of my graduate career at Haystack and worked there almost daily starting in 1965, but the relationship was a little bit remote. The facility had a complex life that I could not begin to understand: it was doing things like radar ranging to the planets, a general relativity experiment, and range/Doppler imaging of the Moon. Radio astronomy was something of a sideline. Haystack was also very much a high-tech facility: there were racks and racks of Hewlett-Packard synthesizers and a hydrogen maser frequency standard. It was all very complex and kind of mysterious—cables disappeared beneath the floor and reappeared in other racks far away. Setups were complicated, and it was difficult to feel that you were in control.

By contrast, the 140 Foot telescope was more like a kindly grandfather. Green Bank was a place you came to like a child visiting his grandparents on summer holiday. You came here specifically to use the telescope. It was quite friendly, and as was pointed out this morning, if you needed more time on it, you just spoke to Dave Heeschen and he gave you an extra week or so. A very indulgent grandfather. The 140 Foot was a rather simple and straightforward machine. When the staff put your receiver box up at the focus, you had the undivided attention of this telescope. The IF came down into the control room on a cable, and you plugged it into the back-end equipment. So that's how I view these two different telescopes that played such an important role in my education.

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* Presented at the 140 Foot Telescope Birthday Symposium, September 1995.

III. The Summer of 1962

I first saw the 140 Foot telescope when I was a summer student in Green Bank in 1962. The summer program was fabulous, and I really got a good start in radio astronomy because of it. The 140 Foot at that time was just a cement pier. We have heard recollections today of its looking like a submarine; to me the image was that of a cement anvil. It was a big mystery as to what it was doing there. Nothing happened all summer—the pier just sat there, with the gigantic polar axis (the submarine?) rusting on the ground. The image of what the telescope was supposed to look like could be seen in the Observatory decal on all the Observatory trucks.

That summer we had lots of interesting role models and mentors. I was 19 and had just finished my junior year in college. The graduate students, who all seemed to have such a keen sense of purpose, included Carl Heiles—who lived in the dormitory with his young family—and Ellen Gundermann (who later married Harry Hardebeck), Yervant Terzian, Bob Haas, Beth Waltman, and Peter Gaposchkin. At least two of the staff members, including Dave Heeschen and Frank Drake, had sports cars, which they seemed to enjoy cruising around in. I remember Cam Wade saying to the summer students, "What we would really like to do here at NRAO is build an interferometer; if only we had someone on the staff who knew something about them." I remember the people who took an interest in me and made lasting impressions on me: Torlev Orhaug, who was trying to understand the opacity of the atmosphere at what was then the very high frequency of 8 GHz; John Findlay, who took me under his wing after Orhaug left; and Marc Vinokur, who taught me about correlation functions.

IV. Searching for CH

After college I went off to graduate school at MIT and immediately began working with Alan Barrett. In the fall of 1963, my first year at MIT, Alan Barrett, Sandy Weinreb, Lit Meeks, and John Henry discovered absorption due to interstellar OH at 18 cm wavelength. It was the first molecular transition detected in radio astronomy. As with the 21 cm line, other groups were hot on the trail of OH, and its detection was quickly confirmed by Dieter and Ewen at AFCRL, Bolton and his colleagues at CSIRO, and by Weaver and Williams at Berkeley.

A year or so later, Barrett decided he wanted to search for a similar transition in CH. He formed a collaboration with Professor Perry Miles at MIT, who was measuring infrared transitions of CH in the lab. My job was to determine the difference in frequency between two IR transitions that linked the levels of the ground state microwave transition we were interested in. That was my first experience with least-mean-square analysis. I fit the Gaussian line profiles and got a transition frequency of 3380 ± 40 MHz. Barry Turner pointed out this morning that the correct frequency of the principal transition turned out to be 3335 MHz, so we were off by 45 MHz. Moreover, our receiver was a traveling-wave tube amplifier with a system temperature of about 5000 K. Need I say more about our prospects for success? NRAO had put together this receiver just for our experiment. One of my jobs was to contract for a YIG filter from Watkin-Johnson to give us sideband rejection. That was my first taste of instrumental things. Alan Rogers and I

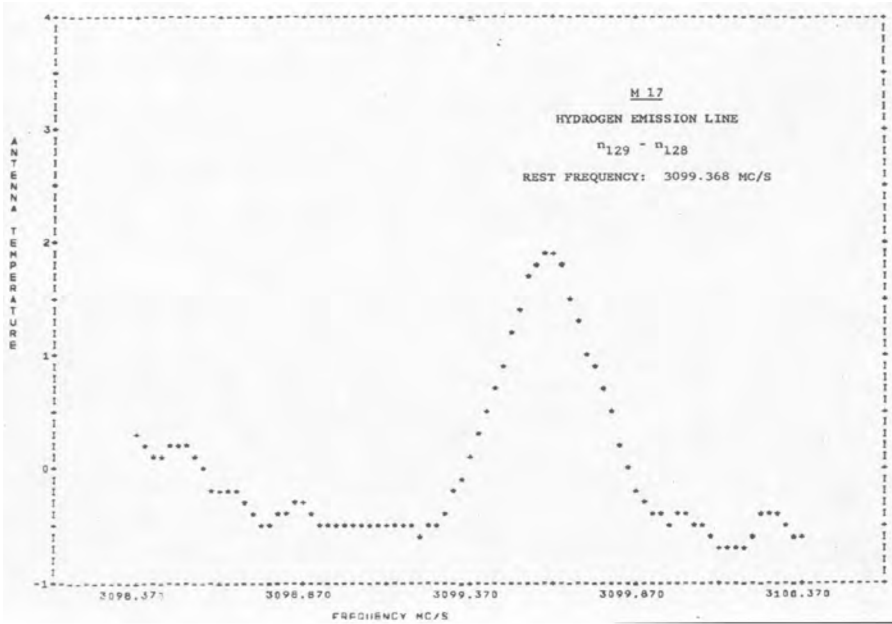


Fig. 1— A two-hour integration on the H128α line from M17. Recombination line observations gave us confidence that the receiver was working properly during our fruitless search for CH in April 1966 with the 140 Foot (Moran, Ph.D. thesis, MIT, 1968).

scanned tirelessly across the spectrum from 3000 to 3500 MHz looking for this line in Cas A, Sgr A, and a lot of dust clouds. (We went down to 3000 MHz because Miller Goss and Harold Weaver had gotten a lab measurement of 3060 MHz.) OH had just been detected in emission, but virtually nothing was known of the giant molecular clouds associated with the HII regions cataloged by Westerhout in his Galactic survey of the early 1960s.

At that time the 140 Foot was equipped with a 100-channel correlator that didn't work very well. The problem seemed to be excessive hysteresis in the sampler. So with Sandy Weinreb's blessing, Alan Rogers pulled that correlator out of the rack, put it on the floor, and began fiddling with it. He was soon soldering on the sampler circuitry. He tried to rebuild it right there, and it did work a lot better after he was finished. The only positive result we got out of that session was a measurement of a recombination line, which we looked at to prove that we could detect something to confirm the integrity of the local oscillator system. This was in April 1966 and the study of recombination lines was only about a year old. Figure 1 shows our detection of the H128α line at 3099 MHz, a transition of no special significance. This may be the only measurement ever made of this particular line.

I recall that before the CH observations Barrett was pretty optimistic of success. We had something like a week of time on the 140 Foot to look for CH, and as we planned the trip, Barrett said, "My God, what if we find CH immediately? What will we do then?" So that we'd have a backup, he said, "Moran, go out and find a frequency for silicon hydride," which was expected to have its ground state in the same band. Silicon hydride has never been detected, and to my knowledge has never been searched for.

CH was eventually detected in 1973 by Turner and Zuckerman on the 140 Foot and by Rydbeck, Ellder, and Irvine on the Onsala dish. In spite of all the initial enthusiasm and competition, CH has not turned out to be a very useful probe of the ISM.

V. The First Spectral Line VLBI Observations

Back at MIT I started building a better receiver to continue the search for CH. I soon decided that I had better get working on something more secure. The work on OH masers was becoming interesting. In the late fall of 1965, Barrett and Rogers came down here and used the 140 Foot to make the amazing discovery of circular polarization in the OH emission lines from W3(OH). Figure 2 shows the four ground state transitions in both senses of circular polarization. Notice that the right-circularly polarized (RCP) spectrum looks very different from the left-circularly polarized (LCP) spectrum. At the time there were arguments about whether this was a manifestation of the Zeeman effect. The answer was by no means clear, since the spectra did not display any easily-identifiable Zeeman patterns. Somewhat later, Davies showed that among all the OH transitions in W3(OH), the LCP components were systematically shifted to lower velocity by about 5 kHz. He argued that a magnetic field of about 5 mG was at work here. As I will describe later, conclusive evidence for the Zeeman effect was obtained later with VLBI observations, which showed that putative Zeeman components arose from the same position.

Starting at the end of 1965, Bernie Burke began promoting interferometric measurements of OH masers as a means to determine their angular sizes. Alan Rogers led the successful effort to connect the Millstone radar and the Haystack telescope into a one-baseline interferometer. The signals were alternately added and subtracted, and then fed into the one-bit autocorrelator used at Haystack for spectroscopy. I wrote the software to analyze the correlator output (i.e., phase switching, Fourier transforming, fringe stopping, and integration). I think this was the first application of digital correlation techniques for spectral line interferometry. At Jodrell Bank, CalTech, and later Green Bank, filter banks were used, with each filter followed by a simple multiplier, to obtain the desired frequency resolution. The OH masers were unresolved on the 0.7 km baseline, and they were not associated with any objects on the Palomar sky plates. Our competitors in this work were Cudaback, Read, and Rougoor at OVRO. We proceeded to link the 60 Foot telescope of the Agassiz station of the Harvard College Observatory with the Millstone radar to form an interferometer with a baseline of 13 km. The data were returned from Agassiz over an open-loop microwave link and combined with the signal from the Millstone antenna at Haystack with the correlator and software already in place. This time the different velocity features were separable and a very crude map was produced—the first interferometric maser spot map. The individual spots remained unresolved.

Burke felt we should push ahead with more resolution and decided we had better get involved with the efforts underway at NRAO and Cornell to use tape

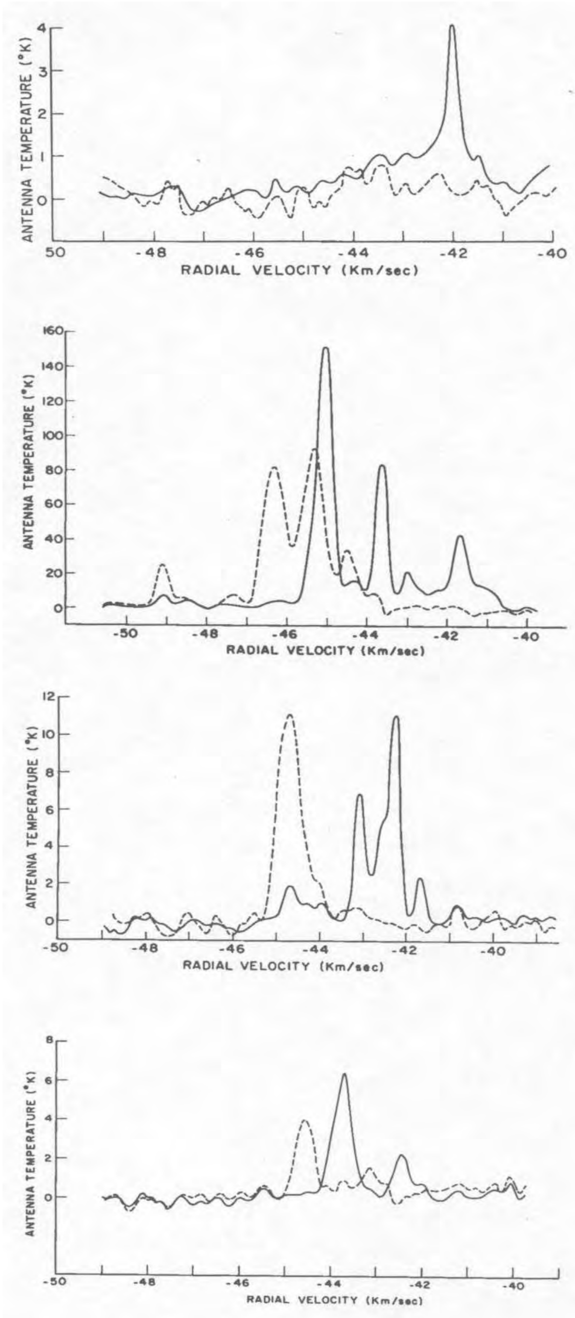


Fig. 2— First observations of the circular polarization of OH maser emission from W3(OH), made on the 140 Foot by Barrett and Rogers in 1965. From the top, the panels show the spectra at 1612, 1665, 1667, and 1720 MHz, respectively. The solid and dashed lines show right- and left-circular polarization, respectively (Rogers, Ph.D. thesis, MIT, 1967).

recorders and independent oscillators to make a virtual interferometer. Burke's idea was that since individual OH maser features had very narrow linewidths, only about 1 kHz, we should be able to get fringes with a very narrow bandwidth filter without having to know the delay very accurately. We settled on bandwidths of 5 kHz for detection and 120 kHz for mapping maser sources. The Nyquist sampling rate for a bandpass of 5 kHz is one sample per 100 microseconds, so we could suffer a big delay error and still get fringes. In the spring of 1967 the Arecibo–Green Bank experiments at 610 MHz had not yet worked, so an experiment on OH masers looked very attractive.

To do this VLBI observation, we first had to actually build a phase-locked L-band receiver for the 140 Foot. Henry Taylor, the legendary NRAO driver and handyman, drove a truck from Green Bank to Haystack containing an empty receiver box, feed horn, and parametric amplifier for the project. It was an exciting trip for Henry; he had never been to a big city before, and he regaled us with stories of getting lost in metropolitan areas. We put together the receiver, and he came back a month later to pick it up and drive it back to Green Bank. That was one of the great things in those days: You could build your own receiver and put it on the 140 Foot. In addition, Alan Rogers designed a set of video converters, while John Ball and Patty Crowther built a direct computer interface to the Haystack Univac 490 computer for recording the data. Burke and I flew down to Green Bank for the first observations between Haystack and the 140 Foot. I recall that Burke, Dave Jauncey, and I were in charge of Green Bank. On the morning of our observations, Cornell Mayer from NRL took his receiver box off and loaded it into a truck for its trip back to Washington. We put our box up. I was in a state of high agitation, to put it mildly, and I recall that Dave Jauncey had a welcome calming influence on me. As soon as everything was set up, I went back to Haystack to check on things there. A day later Burke brought the first tapes to Haystack, and I began correlating the data on the CDC 3200 computer.

Figure 3 shows the very first result we got on the OH maser with our 5 kHz filter. Each plot shows the data with a different fringe rate. We could not be sure of the frequency difference between the frequency standards or of the precise geometry of the interferometer, so we had to search over a wide range of fringe rates to find the fringes. Page after page of correlation plots spewed out of the line printer, but there were no fringes. I was getting discouraged. Finally, fringes suddenly appeared for one pair of tapes, but they quickly disappeared on the next pair. It turned out that when I had written the processing program to do the analysis, I had reversed the sign of the geometric delay, so just as the source transited the plane normal to the baseline—when the delay was zero—the fringes emerged. Since we had such coarse delay tracking with our narrow band, the fringes persisted for a significant time, and we didn't need to be that close to have caught the fringe pattern. In an atmosphere of euphoria and exhaustion, Bernie turned to me and said, "You have to go back down there with more tapes and record more data." So I flew back to Green Bank with more tapes checked as excess luggage, and we continued observing until the end of our scheduled time. That set a pattern of flying back and forth among stations that lasted for a while during the swashbuckling era of VLBI.

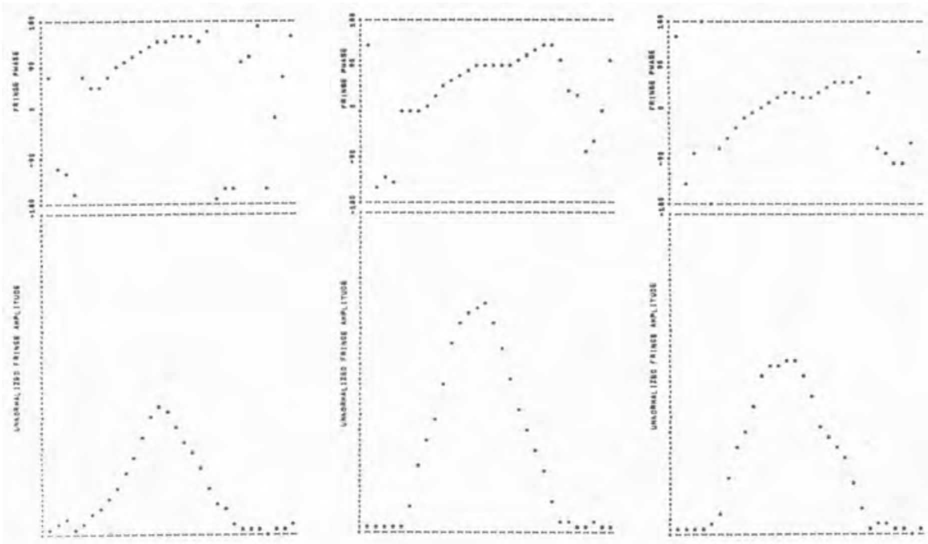


Fig. 3— The first line printer plots of fringes from the 140 Foot – Haystack interferometer, produced a few days after the observations on June 6, 1967. The angular resolution was 0.04″. The plots show the fringe amplitude and phase as a function of frequency for the $-43.7\ \mathrm{km\,s^{-1}}$ *feature in the OH spectrum of W3(OH) across the narrowband 5 kHz filter. The spectral resolution is about 300 Hz! The three sets of plots show the results of removing slightly different fringe frequencies during the 200-second integration (Moran et al., Science, 157, 676, 1967).*

VI. Data Processing and Moore's Law

VLBI processing took an enormous amount of computer time, as Dave Shaffer alluded to in his talk. Regardless, I sometimes look back to those days rather nostalgically. All the data were processed on a general-purpose computer. You could manipulate the bits and massage the data any way you wanted. For example, if you wanted to double your spectral resolution, you changed a program parameter to calculate twice as many lags in the correlation function and waited twice as long for the result. If you wanted a short integration time, you just output the data on the desired short timescale. These days, if you come to the VLBA and want a dump time that is too short, they say, "You can't do that." If you want too high a resolution, i.e., too many spectral channels, the answer is the same—"You can't do that." So we have sacrificed flexibility for what is, of course, the enormous computational power of the VLBA.

Let me illustrate how computational capability has progressed in VLBI since 1967. We were using a CDC 3200 at Haystack to process the data. The NRAO/Cornell group used an IBM 360/50, a computer of comparable power. On our machine it took 1 microsecond to do effectively a one-bit multiplication. That seems slow, but the computation involved more than just the one-bit multiplications. The data streams had to be aligned across word boundaries, a somewhat

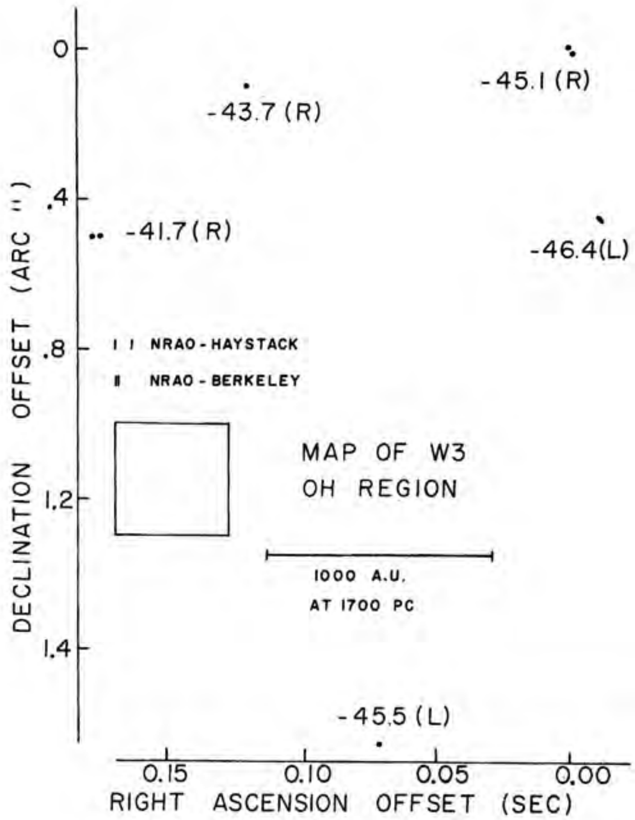


Fig. 4— The first detailed map of the OH maser in W3(OH), made from data taken with the 140 Foot to Haystack interferometer in June 1967 and the 140 Foot to Hat Creek interferometer in August 1967. The spots are labeled by their velocity and sense of circular polarization. There is an attempt to show the multiple structure of the features (Moran, Ph.D. thesis, MIT, 1968).

complicated task involving masking and shifting bits. Our "wideband" mode was 120 kHz. Since we used the standard NRAO/Cornell sampling rate of 720 kilobits per second when recording at stations other than Haystack, we extracted every third bit to compress the data set. With 200 seconds of data on a tape, 240,000 samples per second, and 200 lags of the correlation function, the processing required about 10^{10} multiplications. At 1 microsecond per multiplication that is 10,000 seconds, or about 3 hours. Haystack did not have any computer operators, so I had to babysit this operation. The first map I made of W3(OH) in the summer of 1967 (see Figure 4) took several hundred hours of data crunching to make.

Now, of course, with the VLBA one can typically do four 16 MHz bands at 128 megabits per second (Mb/s). Since the data can be processed in the same amount of time spent acquiring it, the processor must handle 128 Mb/s times 45 baselines times 256 spectral channels, or about 10^{12} multiplications per second. So

VLBI processing capacity has improved by a factor of 1 million in 30 years, or a factor of 100 per decade.

This factor of 100 per decade is typical of progress in many aspects of our field. Kellermann and others have plotted the sensitivity of radio telescopes versus time. Jansky's sensitivity in 1933 was sufficient to detect a 10^7 Jy source, the Galactic Center. Today the VLA can reach 10 μ Jy; that is 12 orders of magnitude in 60 years. Another example is to compare the spectrometer that Dave Heeschen used for his thesis work at Harvard in 1954 with modern counterparts. He had a one-channel spectrometer with a bandwidth of about 1 kHz, which he scanned across the band to measure HI. On the GBT there will be a quarter million channels with a bandwidth of 1 GHz. In terms of processing capacity, we need to look at the sampling rate times the number of channels. The progress here (2×10^3 per second to 1.2×10^{15} per second) has been about a factor of 100 per decade. By almost any measure— resolution, sensitivity, or processing capability—radio astronomy has been improving by a factor of about 100 per decade, which is a doubling time of about 2 years. So we are competing pretty well with computer technology, which is characterized by a doubling time of 18 months, as described by Moore's Law.

VII. Progress on W3(OH)

From the data we acquired in the summer of 1967, we mapped the entire cluster of masers in W3(OH). The results are shown in Figure 4. It was a combination of the very first experiments between NRAO and Haystack in June 1967 with an NRAO-Berkeley experiment that we did later that summer.

In the fall of 1967 I was trying to write up my thesis, but Burke was busy organizing more VLBI observations. Our final experiment with the NRAO/Cornell group was in January 1968. This one was a precursor of a real network operation. It had four stations: the 140 Foot, Onsala (Sweden), Millstone, and Hat Creek (Figure 5). In that experiment we succeeded in resolving a maser spot for the first time rather unambiguously (Figure 6). It was thought at that time that masers might have a high degree of intrinsic spatial coherence and would therefore appear as perfect point sources on the sky that would never be resolved. Not true!

The experiments on W3(OH) got better and better. Most of the work has been done at MIT, Jodrell Bank, and later at the Harvard-Smithsonian Center for Astrophysics (CfA). There have been at least ten major efforts to map the prototypical maser in W3(OH). Perhaps the best result to date was obtained by Eric Bloemhof and his collaborators at the CfA. In 1986 they mapped the 1665 MHz transition in both senses of circular polarization and compared the results with those obtained in 1978 by Tony García-Barreto in his Ph.D. thesis work at MIT (Figure 7). In this figure the dotted lines show the spectra in left-circular polarization, and the solid lines in right-circular polarization. In several areas you can see that both putative Zeeman components are moving in the same direction from the same point, which is very convincing evidence that the Zeeman effect is at work. Magnetic fields are typically 5 mG, as Davies speculated years earlier. This map shows that the maser gas in W3(OH) is expanding ahead of the ionization front around a newly formed O star at about 4–5 $\mathrm{km\,s^{-1}}$. These observations provided the first definitive detection of proper motions in an OH maser.

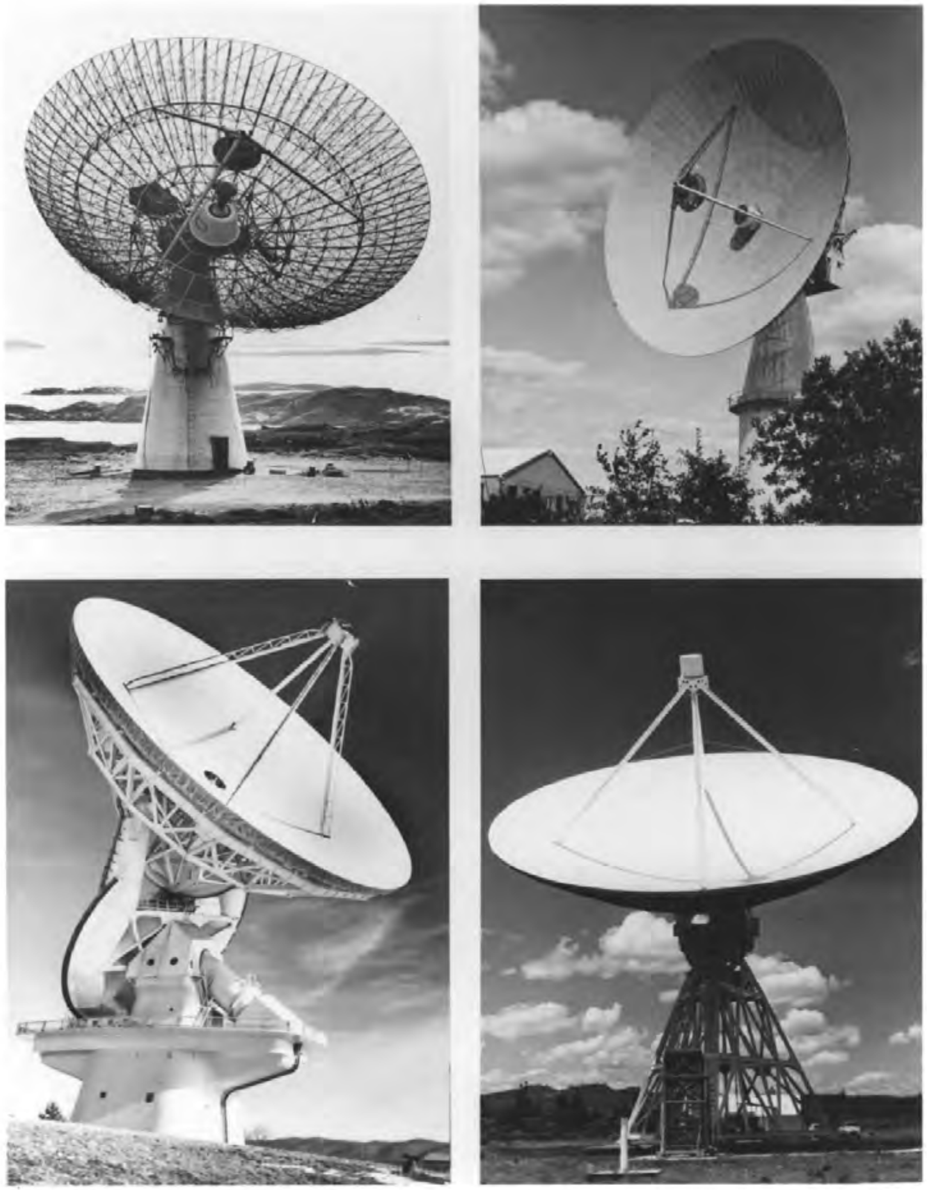


Fig. 5— The antennas of the first successful multi-baseline VLBI experiment, in January 1968. Top left, Onsala 85 Foot antenna; top right, MIT Millstone Hill 84 Foot antenna; lower left, NRAO 140 Foot antenna; and lower right, Hat Creek 85 Foot antenna (Moran, Ph.D. thesis, MIT, 1968).

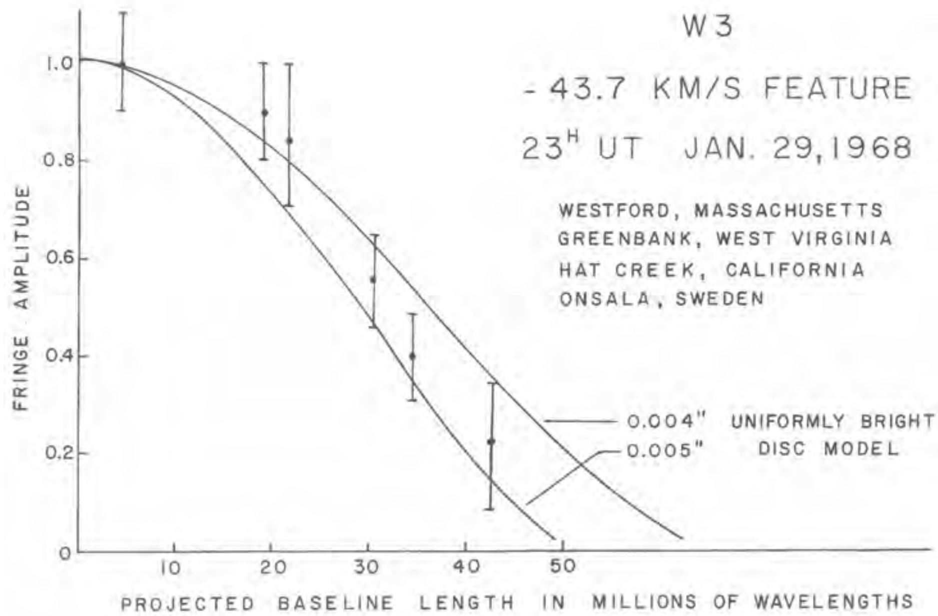


Fig. 6— The first measurement of the angular size of an individual maser component, made with the 4-baseline VLBI array shown in Figure 5. The maser feature shown is the one at $-43.7\ \mathrm{km\,s^{-1}}$. *Its angular size is about 0.0045″. This feature has lasted for decades and has long served as a phase reference feature in this source (Moran, Ph.D. thesis, MIT, 1968).*

It is interesting to note that the morphology of maser spots persists from one epoch to the next (Figure 8). You can see that the spots are elongated or have more complex appearance, but they keep the same shapes as they move. The position angles of the masers in 1978 are highly correlated with the position angles in 1986. So masers are little elongated blobs that keep their shapes as they move. OH masers arise in moving clouds, i.e., ballistic bullets whose velocities can be tracked accurately. This is the best evidence that masers are physical entities and not some traveling excitation phenomenon. Theoreticians have suggested mechanisms that could generate apparent motion in masers—for example, moving excitation or shock waves—but there has never been much evidence to support these hypotheses.

VIII. Having Fun

This morning, when Joe Taylor asked, “Were you having fun?” I was thinking about my own experiences. Looking back, it seems that the period 1966–1968 was a whirlwind of activity. In April 1966 we went off looking for CH, and in June we made the first Millstone-Haystack interferometer to measure OH positions. By December we had extended that to the Millstone-Agassiz interferometer to get higher resolution. The following spring we built the VLBI terminal at Haystack. In June 1967 we did the first VLBI experiments on masers, and in January 1968 we

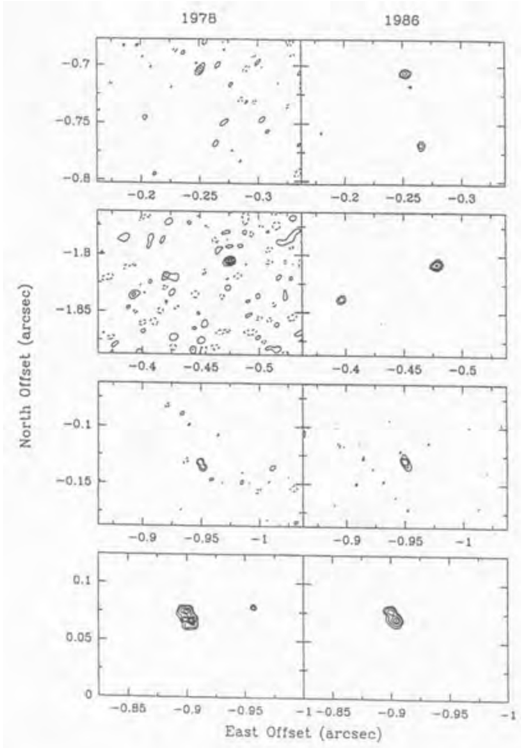


Fig. 7— A recent map of the OH masers at 1665 MHz toward W3(OH) superimposed on the contours of continuum emission from the associated compact HII region. The proper motions were measured from two VLBI experiments separated by eight years. The vectors show the proper motions of the individual maser features. Although there is an unknown constant vector (since the measurements are relative to the phase reference feature), the maser complex is thought to trace the spherical expansion of the molecular layer outside the ionization front of the HII region. The distribution of the magnetic field can be deduced from the pairing of RCP and LCP components from a given region. The magnetic field strengths are about 5 mG (Bloemhof, Reid, and Moran, Ap.J., 397, 505, 1992).

participated in the first four-station VLBI experiment. It was a very busy time. So to answer Joe's question: it was fun, but I didn't have much of a life beyond that.

IX. Early VLBI with Barry Clark

Working with Barry Clark was memorable. During the spring of 1967 I relied on Barry to teach me a lot of things. I worried that VLBI was not going to work, because of atmospheric turbulence. I said to Barry on an early visit to Green Bank, "Gee, the atmosphere might mess this thing up. These long-baseline experiments might not work. Do you think we might get messed up?" And Barry said, "yup."

Some months later I was doing a lot of scalar averaging of fringes, following a disastrous run where the frequency synthesizer at the 140 Foot was left locked on its internal crystal oscillator. When noisy fringe amplitudes are averaged, the

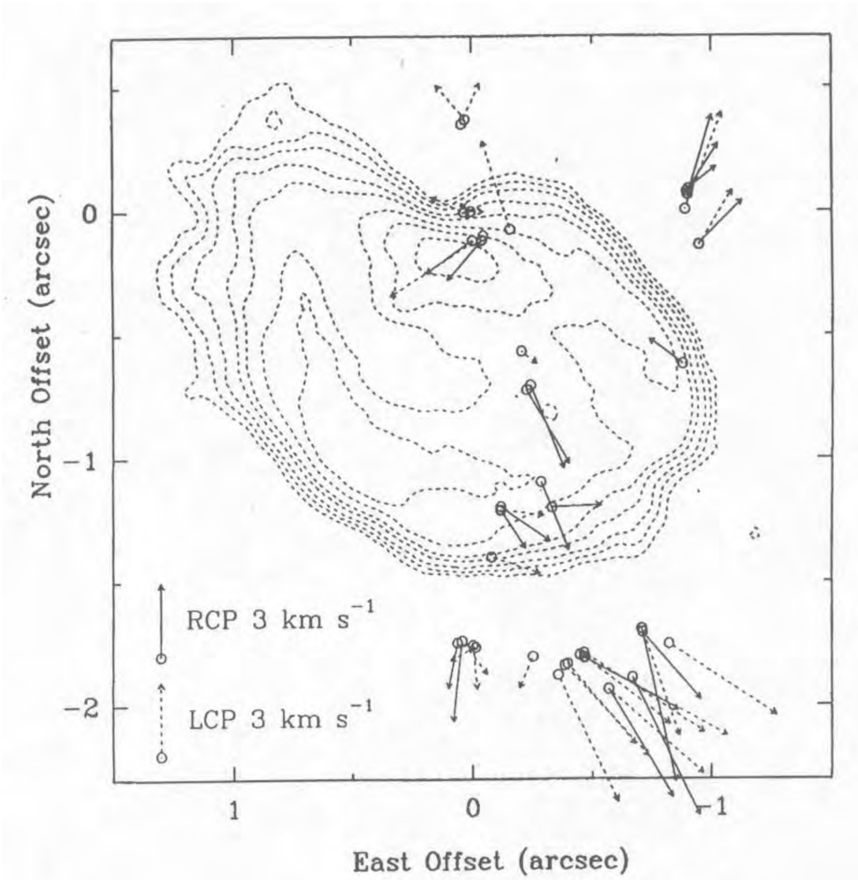


Fig. 8— Examples of images of selected maser features in W3(OH) from 1978 and 1986. Note how the shapes persist (Bloemhof, Reid, and Moran, Ap.J., 397, 505, 1992).

estimate of fringe amplitude is biased because the fringe amplitudes are always positive—you get a positive answer whether there is a signal there or not. I said to Barry, "You know, fringe amplitudes may be biased and make you think that you have a detection when there's nothing there." And Barry said, "yup." I said, "OK, but what's the probability distribution of the fringe amplitude?" "I think the answer has a Bessel function in it," he replied. That was enough to get me interested, and I worked the problem out myself. Even in the mid-1960s he was a very inspiring but intimidating person.

I finally got my thesis done in July 1968. The Vietnam War was in progress, and I moved to Lincoln Laboratory to work on military radar, where I stayed for two years.

X. Water Masers

Townes and the Berkeley radio group discovered water vapor masers at Hat Creek in 1969. The first VLBI experiments on them involved the 140 Foot telescope

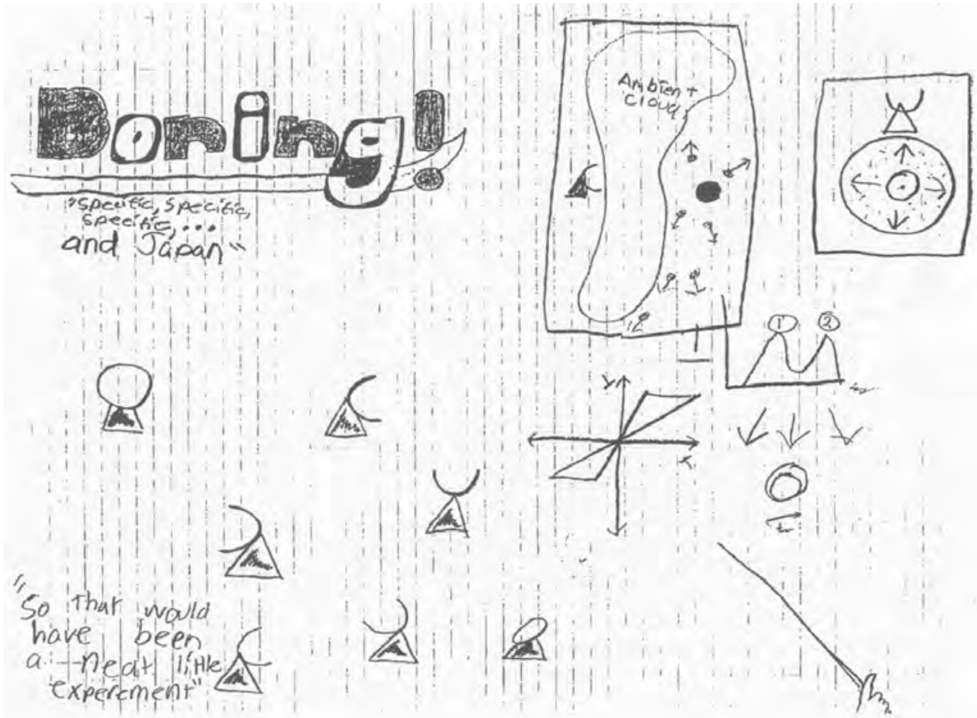


Fig. 9— Summary of a colloquium on the proper motions of water masers associated with newly formed massive stars, as compiled by Michael Moran, age 11.

connected to NRL and then to Haystack. I spent much of my time in the 1970s and 1980s mapping water masers and measuring their proper motions, and the 140 Foot was always a key telescope in this VLBI work.

In the summer of 1992 I was traveling with my family in Europe and gave a colloquium on our proper motion work at Dwingeloo. My son Michael, who was 11 at the time, was in the audience and sketched his impressions of my lecture (Figure 9). Much of my talk is faithfully captured in his cartoons, including his overall assessment. You can see a sketch of an ambient molecular cloud and newly formed stars with masers moving outward. There is a velocity-position diagram characteristic of an expanding envelope and so forth. So I'll leave that as a summary of a decade of hard work measuring proper motions of water masers in which we found, in every case, that the masers are in expanding flows away from their central star.

XI. NGC 4258

The role of the 140 Foot is winding down in the VLBA era, especially with the GBT coming along. To finish up, I would like to tell you about our current work on NGC 4258 with the VLBA. Water masers have been known for some time to exist in the nuclei of active galaxies. Here is the spectrum of the maser that

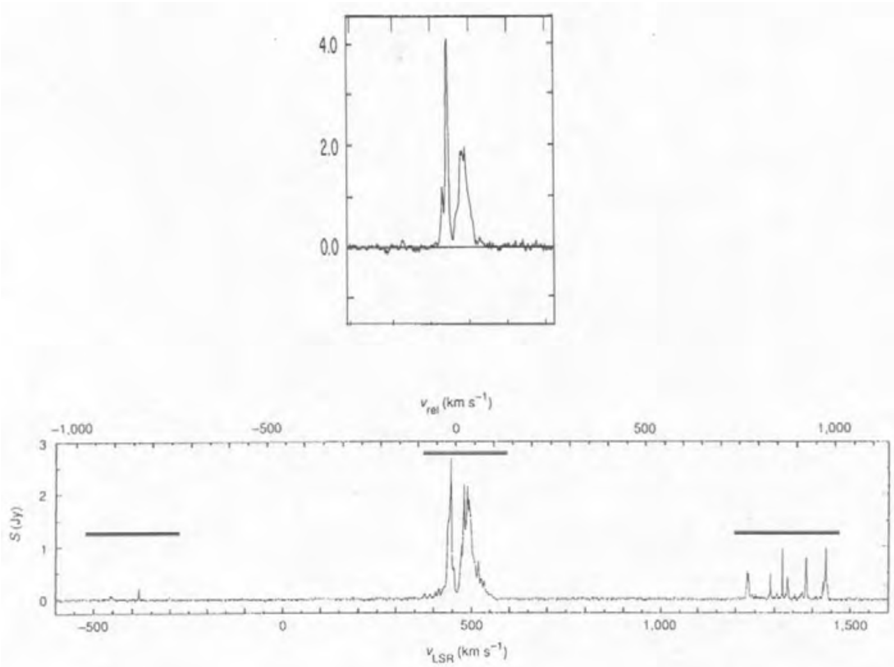


Fig. 10— The spectrum of the water maser toward NGC 4258. Top, discovery spectrum from Claussen, Heiligman, and Lo (Nature, 310, 298, 1984) and, bottom, the discovery spectrum of the high-velocity masers from Nakai, Inoue, and Miyoshi (Nature, 361, 45, 1993).

Claussen, Heiligman, and Lo found in NGC 4258 in 1984 (Figure 10). They had a 1,000-channel spectrometer on the OVRO 130 Foot telescope, and they saw masers near the systemic velocity of the galaxy. They were the first to associate masers like these with active galactic nuclei, rather than with extragalactic star-formation regions.

Our group at CfA joined them for a VLBI experiment on this maser. The first quick look at the data was reported in the proceedings of the IAU Symposium on VLBI in 1987; it didn't look especially promising. A few years later, a Chinese astronomer visiting the CfA, D. R. Jiang, needed a project, and we gave him this data to analyze more thoroughly. He found that the masers were distributed in an elongated structure with a clear linear velocity gradient across it. This was tantalizing evidence that the masers were tracing some sort of disk structure in apparent solid-body rotation.

In 1992 a Japanese group at Nobeyama pointed the NRO 45 m telescope at NGC 4258, using a ganged set of AOSs with a total of 16,000 spectral channels. In addition to the masers near the systemic velocity, they found satellite groups of features symmetrically offset by 1000 $\mathrm{km\,s^{-1}}$ on either side of the systemic group (Figure 10). They speculated that these high-velocity features might arise from a rapidly rotating disk or a high-velocity outflow. They also suggested a nondynamical possibility to explain the spectrum—stimulated Raman scattering. I was actually

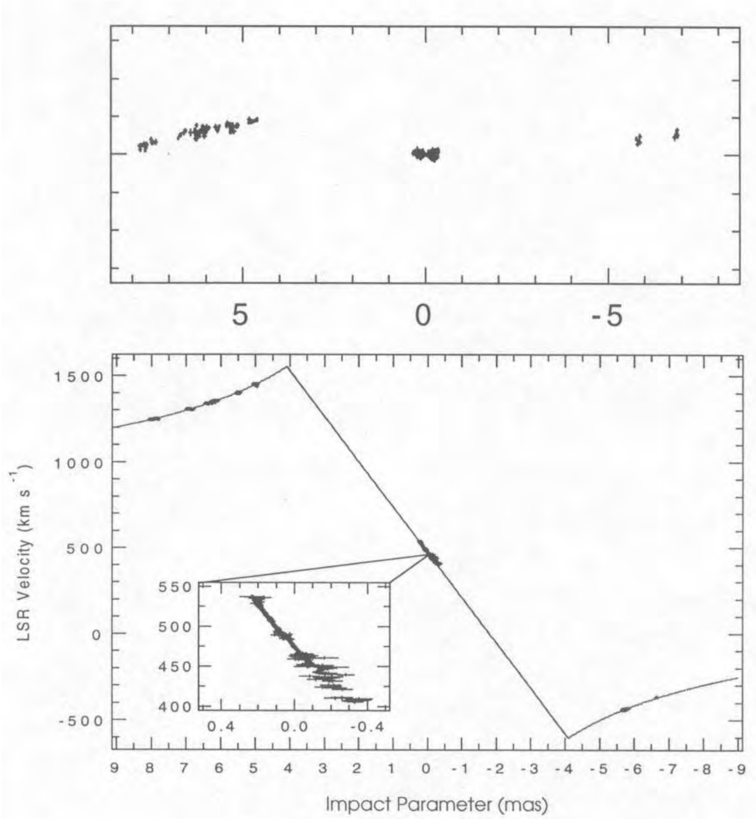


Fig. 11— The first image of the full range of masers in NGC 4258 (adapted from Miyoshi, Moran, Herrnstein, Greenhill, Nakai, Diamond, and Inoue, Nature, 373, 127, 1995).

biased in my opinion at the time, thinking that the masers showed a high-velocity outflow, because all the masers that we had found in the Galaxy traced outflows.

The first VLBA image made last October (1994) (Figure 11) showed clearly that the masers trace a rotating disk structure. One can draw a continuous, slightly sinuous line through the high-velocity redshifted features on the left, the systemic features in the middle, and the blueshifted features on the right. This suggests a very thin, slightly warped disk, seen nearly edge on. If you plot the velocities of the masers along the major axis, you get a linear part in the middle for the systemic masers, which is just a projection effect of the rotational velocity vector for masers in Keplerian motion at fixed radius. They are flanked by masers in beautiful Keplerian motion, which arise from features along a fixed azimuth angle in the disk. We see the inverse square root dependence of position on velocity without any complication from variation in projection angle. These masers show no acceleration and lie along a diametrical line in the disk that is perpendicular to the line of sight, where the line-of-sight velocity gradient is zero, so the gain path is maximum. It is easy to understand why this might be a preferential location for masers to appear. The masers fit the Keplerian curve to better than 3 $\mathrm{km\,s^{-1}}$, a fractional accuracy of less than 1%. We can do very precise model fits to show that this model is the right one. The disk is inclined by 7 degrees to the line of sight. It was very fortuitous and unexpected that the high-velocity masers would line up almost exactly in the disk along a line perpendicular to the line of sight.

The distance to the galaxy is about 7 Mpc, so we can calculate the enclosed mass. It is very high, about 35 million solar masses. More importantly, this mass must be contained within the inner radius of the maser disk, 0.13 pc. The density of a uniformly dense sphere of matter would be 4 billion solar masses per cubic parsec. This is too high to be in the form of a stable star cluster, so we think that it is probably a black hole, unless someone can figure out how to make a long-lived star cluster with this density.

The disk is remarkably thin; it is unresolved and less than 10 μas in dimension in the direction normal to the disk. The height-to-thickness ratio is less than 0.0025. If the disk is in hydrostatic equilibrium, the temperature must be less than 1,000 K. Applying the standard theory of viscous accretion disks by Shakura and Sunyaev, one can estimate the accretion rate. For an α parameter of 0.1, the accretion rate would be 10^{-4} solar masses per year and the inward drift velocity of material would be something like 1 m $\mathrm{s^{-1}}$. This is actually enough to fuel the weak X-ray source that is known to exist in this galaxy.

When we add the GBT to the VLBA, along with Bonn and the phased VLA, we will get much better sensitivity. We should be able to detect many more extragalactic megamasers and should be able to do a good job of mapping them.

Discussion

David Shaffer: Did you know right away, from the beginning, that you were going to process a bunch of lags to get your spectral resolution?

Moran: Yes, because we built the Millstone-Haystack and Millstone-Agassiz interferometers using the normal Haystack autocorrelator to determine the spectrum. We made the autocorrelator into a cross correlator by alternately adding and subtracting the signals and then synchronously accumulating the difference correlation functions. So we knew all about lag correlators. Of course, signal processing was a major focus of research at that time in the Electrical Engineering Department at MIT.

Shaffer: Sounds like you are responsible for my disaster number 14 here with your narrow-band filters. (No. 14: Leaving filters in the IF line which are narrower than the observing passband.)

Moran: You mean leaving the video converter on the wrong filter setting? On all of our experiments we shared everything with a continuum group using the same equipment. Barry Clark and I did the scheduling. He did the continuum scheduling and I did the spectral line scheduling. So it was just like it is today—negotiating with Clark over telescope time. Anyway, with interleaved observations, leaving the bandwidth in the wrong setting was a constant hazard.

Mort Roberts: In your NGC 4258 discussion you say that you are observing a disk because it is so beautifully Keplerian, but wouldn't an expansion model fit as well?

Moran: That was my predisposition—I thought we would find that the high-velocity features were part of an outflow because all the Galactic masers in star-formation regions that we had studied were outflows. But a jet interpretation would require an apparent acceleration that exactly mimicked a Keplerian dependence.

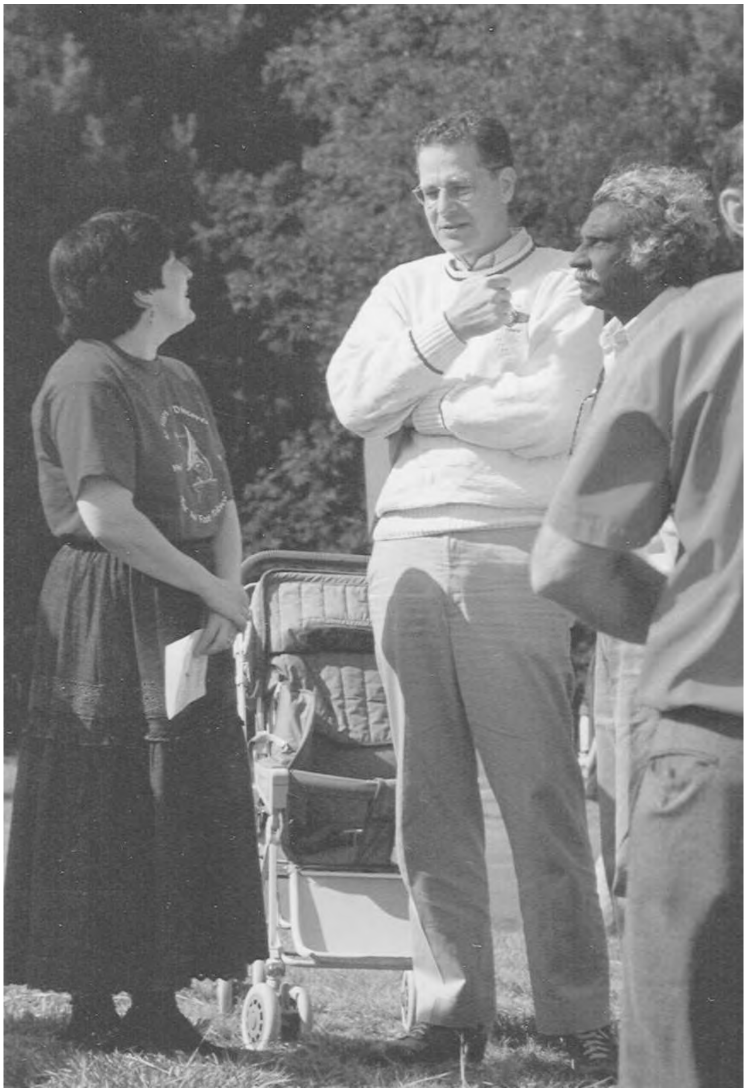
Roberts: NGC 4258 is perhaps the strangest galaxy in the sky. It has the Hα anomalous arms, it has ejecta, it has two quasars sitting in the centers of X-ray lobes. So I am just wondering how successfully you can eliminate nonrotational phenomena.

Moran: I appeal to Occam's razor. The velocity drifts of the lines have also been measured. The high-velocity lines have almost no acceleration and the systemic-velocity lines (at rest with respect to the galaxy) have accelerations of 9 $\mathrm{km\,s^{-1}}$. A rotating disk is a clean explanation of the fact that the high-velocity components have low accelerations and the low-velocity lines have high accelerations. It's a natural projection effect. An outflow explanation would probably require separate dynamical origins for the high-velocity features and the system features.

Postscript: At the coffee break Moran and Roberts continued this discussion. They decided that rather than try to resolve the issue with more scholarly discussion or a duel, they would make a bet of five dollars. Moran would win if the evidence eventually showed that the masers were in rotation, and Roberts would win if the dominant motion was shown to be an outflow. In 1997 the proper motions of the masers were measured. They showed the systemic masers to be moving at about 31 μas/yr and the red and blue high-velocity features to be stationary, exactly as predicted for a disk at a distance of about 7.2 Mpc. Roberts conceded the bet on May 1, 1999.

James Moran in 1996.

The 140 Foot Telescope at night, 1975.



(Left to Right:) Nancy Maddalena, Joe Taylor, and V. Radhakrishnan at the 30th Birthday Celebration, September 1995.
[photo courtesy R. Maddalena]

" never pay attention to a distinguished person who says a technique won't work."
... Joe Taylor

Lunar Occultation Measurements at the 140 Foot Telescope*

Joseph H. Taylor
Princeton University

Sebastian von Hoerner's observation of a lunar occultation was the very first research done on the 140 Foot telescope, but lunar occultations are an observing technique that you do not hear much about these days, for good reason. It is a technique that made sense for a relatively short period of time and was soon supplanted by synthesis telescopes.

I learned about lunar occultations as a first year graduate student at Harvard while listening to a colloquium given by Cyril Hazard. He, with Mackey and Shimmins, observed occultations of 3C273 with the brand-new Parkes telescope in Australia in 1963 or late 1962. By means of that observation they had produced the highly accurate position that Martin Schmidt then used for the optical identification and discovery of the first quasar. It was exciting stuff. To put it into context a bit, this was a time when several major meter wavelength surveys had been completed and there was some squabbling back and forth between the antipodes about which sources were real and which weren't.

After the appearance of the 3C catalog, the Mill, Slee and Hill catalog, and so forth, there were hundreds of reasonably strong radio sources known, all of which were point sources to the principal radio telescopes that could see them. Resolution was needed and was hard to get. Some clever people built interferometers and could begin to see some indication of structure, but they were far from making maps and it wasn't easy. A few people had the bright idea of observing occultations of these sources by the limb of the moon. The technique was basically to measure the signal as the source disappears, and from that, reconstruct at least a one-dimensional brightness distribution of the source parallel to the edge of the moon at the point of the occultation.

As a graduate student I certainly wasn't aware of what was motivating Sebastian in those days, but surely he must also have heard of these observations in 1962-63. He obviously got to work right away, because by 1964 he had a paper in the ApJ [ApJ **140**, 65] in which he developed and elaborated upon the very elegant technique first written down by Peter Scheuer in the Monthly Notices **, whereby one can restore the diffraction pattern observed as the source disappears behind the edge of the moon to recover a brightness distribution. It was all there in elegant mathematics in Scheuer's paper, but in Sebastian's paper it was there in a way that you could actually make some use of it—could turn it into a computer program, for example. By the way, this was a time when graduate students, even reasonably well prepared ones, did not know how to program a computer.

* An edited transcription of an audio recording of the talk presented at the 140 Foot Telescope Birthday Symposium, September 1995. *Figures taken from J.Taylor's Thesis, "Lunar Occultations of Radio Sources," Harvard 1968.*

** Actually Aust.J.Phys: Scheuer 1962.

The scientific motivation for this work was cosmology. The mid-sixties was the era of source counts. We were going to solve all the problems in the world with source counts. We were going to find out what the log(N)–log(S) slope was and find out whether the universe was open or not. Even beyond source counts we were measuring angular diameters as a function of distance and hoping to see beyond the point at which the curvature would be important. But curiosity soon came in as well. After all, the radio sources are there and they can not literally be points. Once you resolve them there must be something interesting there, and it would be nice to have pictures of these objects to see what they are. That must have been a motivation as well.

As I said, my own involvement started as I listened to Cyril Hazard give a colloquium at Harvard. Soon afterward I was working with Alan Maxwell and went off to the Harvard station in Fort Davis, Texas, where there was an 85 foot antenna, and we observed a few occultations. Dick Thompson had already been doing some observations there with Alan Maxwell and had a few occultation records around, just strip chart recordings which had never actually been reduced, and I got busy with those. That was my beginning.

The information that you can extract from an occultation observation is very strongly dependent upon the signal-to-noise ratio. You have to observe with short time constants because all the interesting signal variation happens in a few seconds and therefore you need strong signals so that you can get a good signal-to-noise ratio in a few seconds. It is clearly a big advantage in this work to have a large telescope, and the 140 Foot was just then about to become available.

I do not specifically remember, but I suspect that Alan Maxwell finally picked up the phone and called Sebastian von Hoerner and asked if would it be alright if this student came down and talked with him. By that time Sebastian had already persuaded a young postdoc, Marvin De Jong, to become involved in occultation studies, and they were both very gracious to accept an outsider to come and work with them.

I have my lab notebooks from the time and have found a couple of things worthy of repetition, just for fun. There is a notation on the page for October 31, 1965, which is apparently when I first came to Green Bank. It says "Arrived NRAO Green Bank on schedule. Very pleasant evening with Sebastian and Lisa von Hoerner and their daughter Ela and Leonard Chow." I must report that after Ela's name I have an exclamation point. Apparently she had an effect on a 24 year old man. My notebook goes on to say, "Zuckerman and Palmer gave me a short tour around the base but von Hoerner says he will give me a better one in the morning."

We decided to collaborate on lunar occultations. I kept on observing occultations at Fort Davis as much as I could, and I also got some time at Haystack, but it turned out that the 140 Foot was by far the best antenna we had to bring to bear on this problem, and the best results came from here. We simply agreed to try to observe as many of the predicted occultations over the next 18 months or so as we could.

By that time Sebastian had already determined a set of reasonably optimized choices of receiver frequencies and bandwidths and the like, and had the equipment built and used first on the 85 Foot telescope, and then on the 140 Foot as soon as it was available. There were receivers for a frequency in the 250 MHz range and one around 400 MHz, and an interference survey was done to try to find the best slots.

It was hard to find one around 250 MHz and so instead of picking a single frequency we chose two: 234 and 256 MHz, each with an 8 or 10 MHz bandwidth, typically. That way, if one band had interference the other might not, and you had twice the chance of getting a good observation. Mind you, the nature of the events we were trying to observe is such that they are inherently unrepeatable. You have one shot at it. If you don't get the data, for what ever reason, whether you messed up yourself or whether there was interference, the data were gone.

For a typical observing session at the 140 Foot I would come to Green Bank from Harvard for a couple of weeks and try to get in five or six interesting observations during that time. I would work with Sebastian and, frequently, Marvin de Jong. Another page from my notebook shows what I was up to. I was going to come here around the end of September and observe 5 occultations over the following couple of weeks. I was going to leave Boston on Sunday the 25th, flying on American Airlines and then connecting at Washington on Lake Central. The American Airlines flight I'm quite sure was a DC-6, and I'm very certain that the Lake Central flight was a DC-3. That was from Washington to Elkins. On this trip I had a couple of hours layover at Washington but on the way back it was not even that. It was a very good connection. You can't get from Green Bank to Boston that fast today, believe me. Notice also the round trip airfare. I have found elsewhere in the book the fact that my living expenses in the Green Bank residence hall and cafeteria were $7 a day.

Then there are notes to myself about what I have to do. I've got to call people in Green Bank and tell them to distribute so called "quiet time" notices, that would restrict gasoline vehicles around the Telescope. This was before gasoline vehicles were stopped at the gate, and before the Checkers Diesels were here, believe it or not. It might have even been before those Checkers were built. I also had to contact the crew who put up the occultation receiver feed, and notify somebody at Charlottesville to send 5 tapes over for the 5 different observations we were going to make. Some of those tapes are probably still signed out in my name. I had to tell Wally Oref that I was coming, and I had to tell Jack (I really don't remember who Jack was) to turn on the receivers. They had at least some vacuum tubes in the back end that had to be warmed up early.

Finally, the notebook says "write to L. V. Morrison about new positions." All of us who did occultation work knew this man L. V. Morrison at Her Majesty's Nautical Almanac Office in Britain, who I never met, but who did the predictions of the moon's path on the celestial sphere that we all used. And who, when we had actually observed the occultation, would even give you the position of the limb of the moon at the instant you say the source disappeared. By drawing a couple of those curves you could get the position of the source quite accurately.

Here is a figure taken from my thesis (Fig 1) which shows a schematic of an occultation. The moon comes by and we're observing down here on the Earth and we're going to get this incoming wavefront which is blocked out of one side and coming in on the other side. You do the simple integral indicated by the diagram

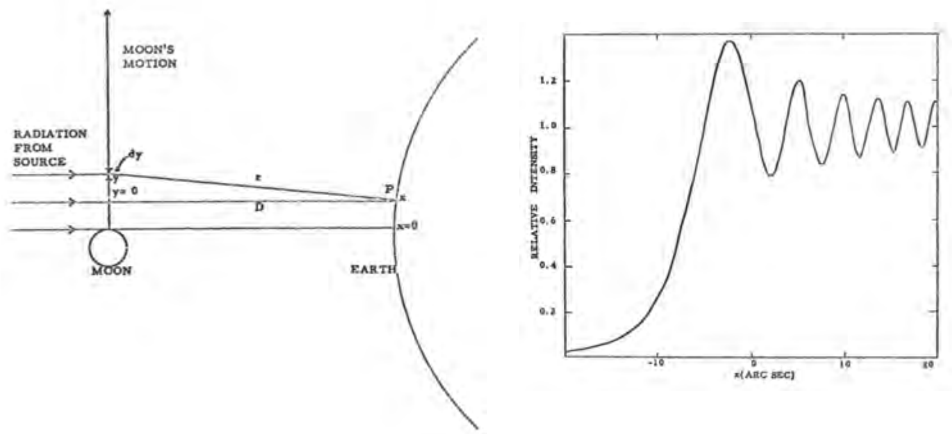


Fig. 1— (L) Schematic diagram of a lunar occultation. (R)Theoretical Fresnel diffraction pattern for a point source.

there and simply add up the contributions to the electric field on Earth and expand the distance between the telescope site and the little dy that you're sliding along there in a Taylor series. Just use the first term for the amplitude piece and the first two terms for the phase piece and integrate it up and you get a Fresnel integral. Of course you know what you get from that is a pattern that looks like that. So you'd expect that during the reappearance of a source from behind the moon the intensity would increase like that (Fig 1 (R)), oscillating a bit. Peter Scheuer's insight was to show that you could in fact deconvolve that with an accuracy which is limited only by the signal-to-noise ratio by convolving it with a restoring function which he showed how to calculate. It looked something like that (points to figure) depending on how many wiggles you keep essentially determines the signal-to-noise ratio, the angular resolution, but that in turn depends on having adequate signal-to-noise ratio in the observation.

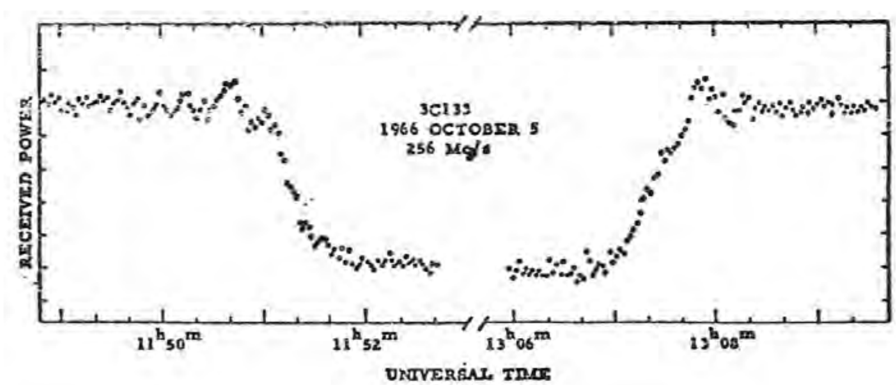


Fig. 2— Data for the occultation of 3C133 on October 5, 1966.

Here is an observation from that observing run in October 1966 also just copied out of my thesis (Figure 2). Observed power is a function of time as 3C133 disappears behind the edge of the moon at 405 MHz. You can see the diffraction fringes on the disappearance but you can also see some structure. The first maximum is not the highest one so it's clear that the source is complicated. It's got a couple of components or more that are beating against one another. It looks different on the reappearance because that's at a different position angle around the edge of the moon and in the most favorable circumstances, when a number of occultations could be observed for a particular source, one could actually put them together to make a two dimensional map of the source structure. The data were put together using a technique that involved going into the transform plane and essentially making up a UV map and then re-transforming it. This gave a map of the source 3C192 as shown in Figure 3. It looked like four components along a major axis that kind of spread out.

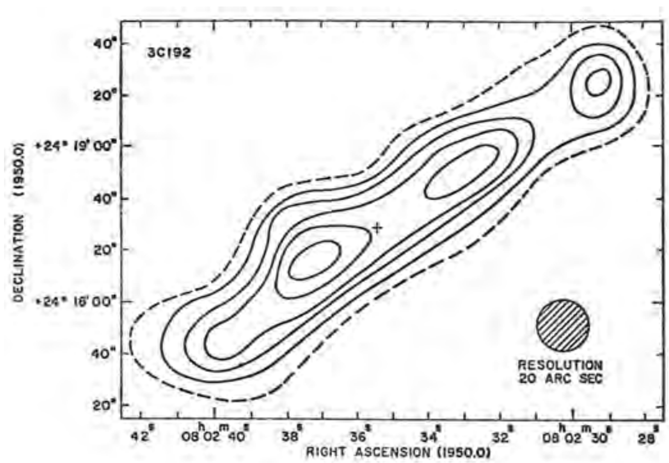


Fig. 3— Derived brightness distribution of 3C192 at 256 Mc/s.

Just for interest I scratched around in the literature and found recent maps of some of the occultation sources. The new data, taken with the 5km array and the VLA, are at higher frequencies and have higher angular resolution, but they show the same components with the same axis. The occultation map is not bad for its time and still may well be the highest resolution map of 3C192 at that low a frequency. Needless to say, the whole occultation technique became essentially uninteresting within a few years after my thesis when the 1 mile telescope at Cambridge began to make good maps using synthesis techniques, which were not at the whims of the motions of the Moon through the Zodiac and so forth.

To sum up, it *was* a lot of fun doing those observations. Several others in the room—Tom Wilson, Jim Moran, Dave Shaffer—all have described what it was like being a graduate student working here at NRAO at the time and with this fine telescope. I have one additional thought. As graduate students in those days we were all allowed an astonishing amount of freedom. Perhaps even more than most, my thesis adviser Alan Maxwell practiced management from a distance. Alan had his own research. He was basically a solar physicist and was certainly interested in the extragalactic work I was doing and kept fairly close tabs on it. But I thought of things to do by myself and did them alone. And even here under Sebastian's guidance, everyone was ready to help at any time, but no one was telling me to do this and that and then the next thing in sequence. That's a great way for a student to learn. You make a lot of mistakes early on but somehow you learn what needs to be learned more thoroughly that way too. I try to remember that with my own students.

Discussion

Frank Kerr: Sandy Sandqvist and I did a series of observations of the Galactic Center through occultations, I think in 1969. Those experiments had unprecedented resolution on the Galactic Center and gave us interesting information on the different components. The moon occults the Galactic Center a series of times every 19 years, but by 1988 it was not worth repeating the experiment.

Gart Westerhout: People apparently like to do occultation observations with telescopes that are not finished yet. In 1955, Seeger and I observed two occultations of the Crab nebula with the Dwingeloo telescope a half a year before it was finished. We looked for a point source but the distribution, actually, was exactly the same as the optical distribution of the synchrotron emission. But that was in 1955, also with a telescope that wasn't finished.

Harry van der Laan: Here is a footnote to some remarks that you made about the antipodal controversies over source counts. I was a research student of Martin Ryle's from August 1960 till the end of 1963, so I know from close observation that his first passion was to disprove the Steady State theory. And in 1960-61, he hadn't really thought beyond that. But once the source counts were such that he was convinced that the Steady State theory was ruled out he really started to think about what you could do to further cosmology in what we now call the Standard Model—to see whether the Universe is open or closed. But he didn't get an answer right away. I was thinking about structure problems and supernova remnants and radio galaxies but not cosmology. I think he had a student called Rupert Clark who was suppose to be thinking about these things. I remember that one day in 1962 a preprint came in, a big preprint from Jan Oort who had written about radio sources and cosmology. Ryle slammed it on the table. He said "Damn it Ruppert, we could have done this!" But they hadn't.

Gerrit Verschuur: When you made your map of 3C192 or other sources did you speculate as to why there was this long thing with 4 blobs in it?

Taylor: Just hand-waving speculations. I didn't make many attempts at doing the real physics of radio sources. I quoted that paper of Harry van der Laan's in my thesis because he had some model, purely theoretical, which produced two lobes. I didn't put much effort into trying to understand the structure of radio sources and certainly didn't achieve that.

Sebastian von Hoerner: I would just like to comment on how all this got started. I can't remember the year, but it was at some early astronomy meeting that I realized how important resolution is and how bad it was in all existing single telescopes. I then had this great idea that if something goes behind the moon there must be a fast cut off and the reemergence should give more information. I needed someone to talk to and Hanbury Brown came my way. I told him about it and he thought that the idea sounded interesting. Well, the next day he said that he had thought more about it and found a problem which would make the technique impossible. And he was correct, there was a problem. Well, I had a day to think it over and couldn't find a way to get around this (I forget what the details were). I went back and forth for a few days and finally had the feeling that it was all so complicated, and I had other things on my mind, so I completely forgot about it. And then one or two years later out came the first paper from Cyril Hazard and in it he thanked Hanbury Brown for so many good suggestions. In my ApJ paper I offered to send the computer program I had developed to anyone who wanted it, but the program at that time was two big boxes of cards.

Taylor: I'll relate one other piece of history. When I was researching introductory material for my thesis I found that in 1908 a Brit named MacMahon had suggested that you could observe occultations of stars by the edge of the moon and therefore measure angular diameters of the stars. But within a year or so Eddington pointed out that it's not really going to work because there is diffraction and you would never get resolution better than the angular width of the first Fresnel zone. Eddington, of course, was very distinguished and therefore the suggestion died. It wasn't until 25 years later that somebody pointed out that merely because there is diffraction doesn't mean you cannot extract the information. Scheuer in the end pointed out mathematically how to do it. So you should never pay attention to a distinguished person who says a technique won't work.

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Editors' note: After finishing his PhD on lunar occulatations, Joe Taylor left Harvard for a position at the University of Massachusetts where he began to study pulsars. Using the 300 Foot in callaboration with G. R. Huguenin, he discovered the first pulsars beyond those originally found at Cambridge. He subsequently made several searches for new pulsars at Green Bank. In a search at Arecibo with his graduate student, Russell Hulse, he discovered the first known pulsar in a binary system and found that the decay of its binary orbit resulted from gravitational radiation. This discovery led to Taylor and Hulse receiving the Nobel Prize in Physics in 1993. Taylor has been a Professor of Physics at Princeton University since 1980.

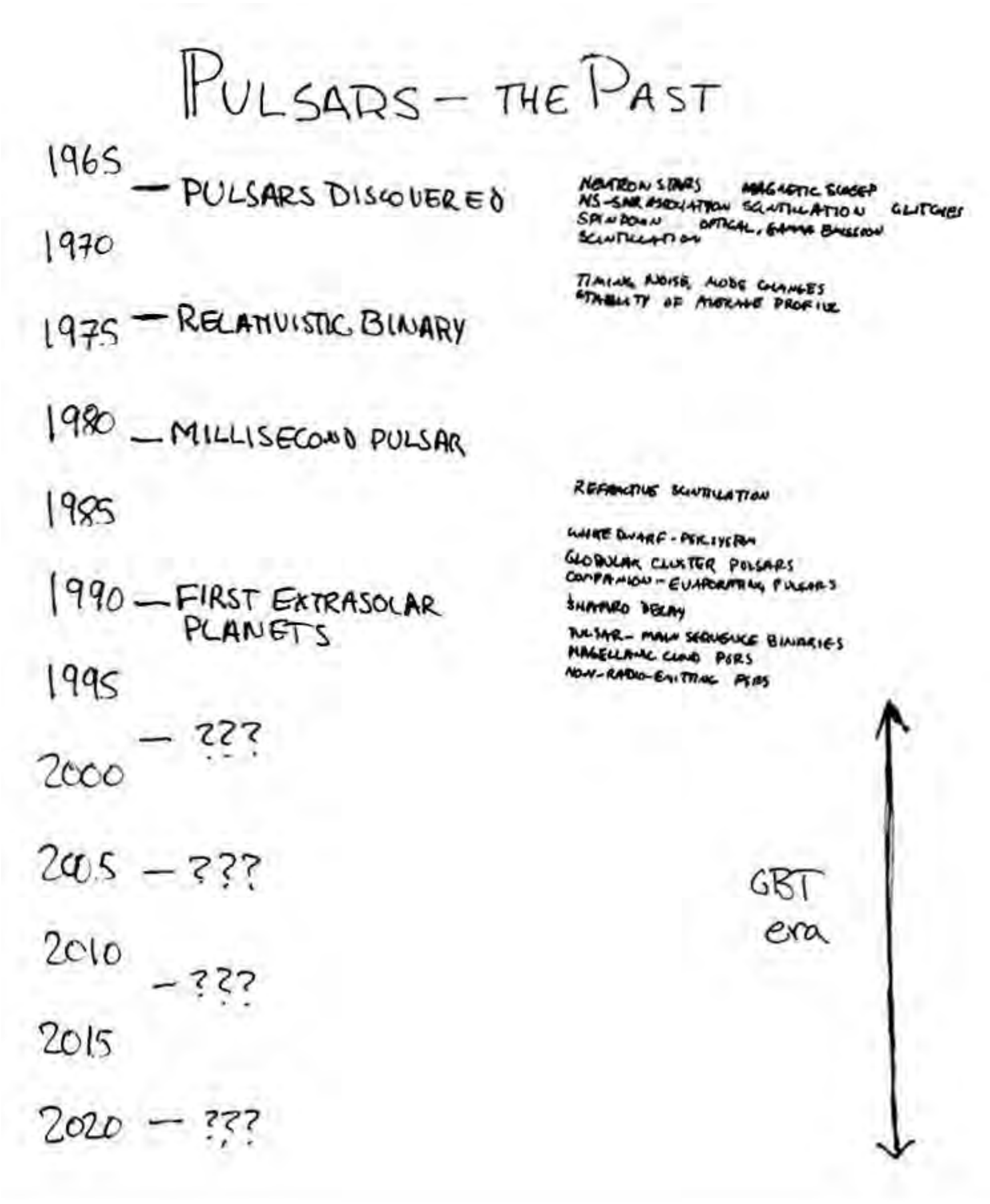

Fig. 1— A time line of major pulsar discoveries.

Pulsar Observations with the GBT*

A retrospective, and speculations on future discoveries

David Nice
Princeton University

When Jay Lockman charged me with the responsibility of talking about pulsars and the GBT I wasn't really sure how to predict what might be done in the future, and I thought one way of starting out would be to go through what had happened in the past 25 or 30 years of pulsar observations. A lot of things have happened, and many of them have happened at Green Bank – for example, the association between pulsars and supernovae was firmly established by the discovery of the pulsar in the Crab Nebula with the 300 Foot telescope.

There are four big events that opened up different areas of pulsar research (see Figure 1, facing page). The first was, of course, the discovery of pulsars by Bell and Hewish in 1967. In 1974 the discovery by Hulse and Taylor of the first binary pulsar opened up new avenues of experiments in relativity and gravity theories. In 1982, Don Backer and collaborators found the first millisecond pulsar whose astoundingly short pulse period offered unprecedented timing precision. And, finally, in 1991 Alex Wolszczan discovered the first evidence for planets beyond our solar system, something which had been sought for decades, if not centuries.

I think the most interesting thing to note about these discoveries is that all four were all serendipitous. None of them was expected. And I think the safest thing we can say about the future is that the most interesting things we come across with the GBT will be totally unexpected. We should make sure that we have observing programs that might lead to unexpected discoveries.

Three out of these four discoveries evolved from survey programs, not targeting any known source in the sky. In the Hulse-Taylor survey, for example, they certainly expected to find dozens of pulsars, but nobody expected anything as interesting as the Hulse-Taylor binary. Surveys may come up with the most interesting things that are discovered with the GBT.

I also couldn't help but notice that these four events are fairly evenly spaced—about 8 years between each of the major discoveries. Being an astronomer, if I see a periodicity like that I can't help but extrapolate. Over the next 25 years or so of the GBT era we should expect about four more discoveries comparable to the Hulse-Taylor binary. [*general laughter*]

Where do we stand today with pulsar observations? What a lot of us have been doing over the last couple years is surveys to discover more pulsars, especially more of the fastest-spinning millisecond pulsars. These surveys have been pretty successful, but they are an awful lot of work.

Let me describe one such survey, made at the 140 Foot by myself in collaboration with Joe Taylor and graduate student Ron Sayer. What we did was a

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* An edited transcription of an audio recording of the talk presented at the 140 Foot Birthday Symposium, September 1995.

fast survey of the entire northern celestial sphere using the 140 Foot to search for pulsars with periods as short as half a millisecond or so. Our scheme was to slew the 140 Foot along in the sky so that any point would go in and out of the beam in about two minutes. As we were slewing we used the Spectral Processor to dump spectra 4000 times a second. We recorded all that information on a couple hundred 8 mm tapes. Ideally this program would have taken three weeks of telescope time, but thanks to RFI it stretched out to four and a half weeks. We shipped the tapes off to the Pittsburgh supercomputing center, ran them through some pulsar detection code, and about 1000 CPU hours later out pops a bunch of pulsars.

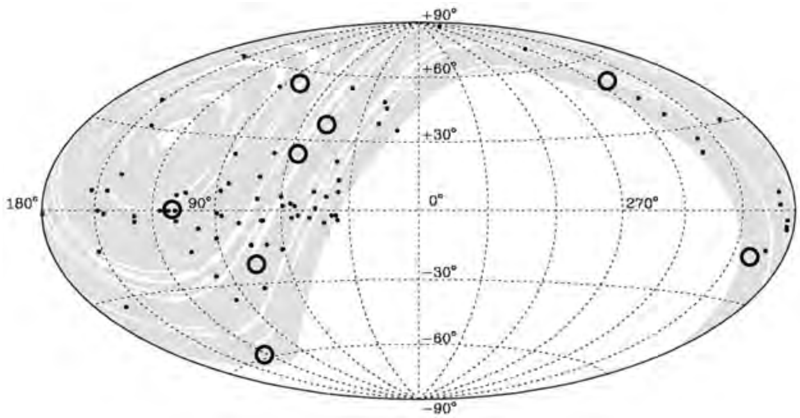

Fig. 2— Coverage of the 140 Foot survey for pulsars in the northern sky. Light gray areas indicate survey coverage. Small dots indicate previously known pulsars that were blindly re-detected in the survey. Large open circles indicate newly discovered pulsars (Sayer, Nice, and Taylor, ApJ 474, 426, 1997).

Figure 2 shows the survey coverage and results. Of the pulsars we detected, nine were not previously known. Of those nine, eight appear to be normal and, I hesitate to say, boring pulsars. The ninth is a new binary millisecond pulsar. It appears to be a neutron star - neutron star system, somewhat wider than the original Hulse-Taylor system. We have been making follow-up observations of it on the 140 Foot and we have detected the relativistic precession of the orbit although not the effect of gravitational radiation.

These surveys are an awful lot of work and to get a payoff of one source we spend a month of telescope time. I think thats typical of the amount of effort that has been put into pulsar surveys in recent years. Between Green Bank, Jodrell Bank, Parkes, and, especially, Arecibo, there are now more than 60 pulsars known that either have millisecond periods, are in binary orbits, or are in globular clusters. Pulsars in globular clusters tend to be older and therefore are more likely to be millisecond or binary systems.

Of these 60, perhaps 20 or so are strong enough that they are suitable for long term timing experiments. If we were sitting still and the pulsar were sitting still, then observations of the stream of pulses hitting the Earth would allow us

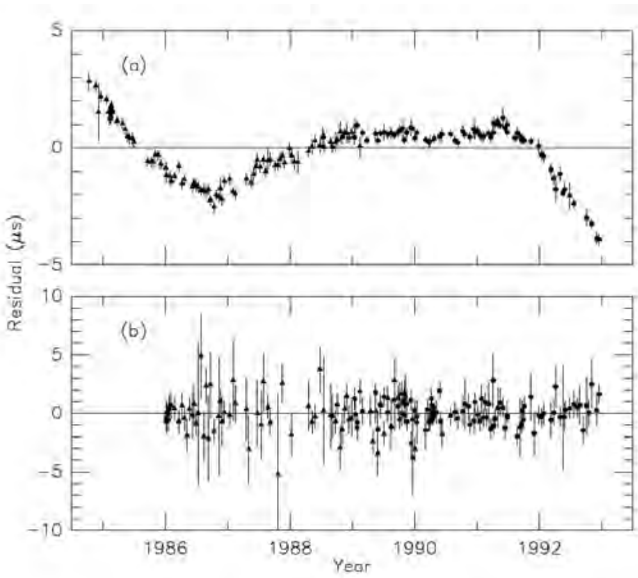

Fig. 3— Residual pulse arrival times from (a) PSR B1937+21 and (b) PSR B1855+09 (Kaspi, Taylor, and Ryba, ApJ 428, 713, 1994).

to monitor the pulsar's rotation and, if it is a very fast spinning neutron star, say spinning 600 times a second, we would expect that rotation to be very stable. In reality, we are not sitting still: we are sitting at an observatory on an Earth thats rotating and going around the Sun (actually the Solar System barycenter), so the pulse arrival times that we measure vary because of the Earth's motion and, in may cases, also due to the pulsar's binary orbit. The distance from the pulsar to us, and therefore the pulse arrival times we measure at the telescope, vary quite a bit. We can use these variations to make interesting measurements of pulsar positions to map out the binary orbits.

If we get our models right we can take out all these effects and eventually go back to looking at the neutron star rotation itself. One might hope that neutron stars would make ideal clocks equal to or better than terrestrial atomic clocks. At the moment, most of the millisecond pulsars are sufficiently new discoveries that there hasn't been time to see instabilities in neutron stars; they really can be treated as ideal clocks.

There are two sources for which there are fairly long time series. Figure 3 shows Arecibo data taken by Joe Taylor and colleagues. They measured pulse arrival times for these two pulsars over 8 years or or so. They then fit a timing model to the data, taking into account the binary orbit of the pulsars, the motion of the Earth, etc. The plot shows the residual pulse arrival times after removing such a model. What they found, unfortunately, is that for B1937+21 the pulse arrival times are not flat; there is a definite instability over a period of 8 years. This is probably intrinsic to the pulsar. You can see how amazingly good the timing is of this source —these are real error bars and they are somewhat less than one microsecond.

For B1855+09, although the error bars are much larger, you can see that the residuals are actually flatter. Its spin-down torques, caused by the pulsar's rotating magnetic field, are much smaller than those of B1937+21. We know that among younger pulsars, those with lower spin-down torques tend to have less timing noise—so B1855+09 might be the better clock of the two. We are in luck because most millisecond pulsars are like B1855+09, with relatively low spin-down torques, so we might expect them to be much more stable. The hope for the future is to observe,say, two dozen or so of these pulsars in various places throughout the celestial sphere. By treating them as independent clocks, we can separate out irregularities in their rotation behavior, systematic errors in Earth ephemerides, and possibly even effects of the Earth being buffeted around by a gravity wave background. To do this, of course, we need lots of signal – thats where the GBT comes in. We are really blessed that we have coming on line in the next few years not just the GBT but also the upgraded Arecibo and GMRT. It will be a really great time for low frequency observing.

You are all familiar with the great things about the GBT – huge collecting area; wide bandwidth, etc. The radio quiet zone is a very important for any low frequency observing. The GBT is fully steerable, making it especially useful for observations of the Galactic Center, which are not possible from Arecibo. Its frequency agility is important because multi-frequency observations of pulsars are needed to remove the dispersive effects of the interstellar medium. The ISM effects change as the pulsar and the Earth move through the Galaxy, so it is important to make observations at two, three, or even four frequencies at every observing epoch.

The final thing I want to mention is a new back end that will be available at the GBT and which we are presently using at the 140 Foot. This is called the CDRP, which stands for coherent de-dispersing radio-frequency processor, a bit of hardware that was developed by Don Backer at UC Berkeley and it will allow us to make higher precision pulse timing observations.

If you observe with a wide passband, the signal at the high end of your passband arrives earlier than the signal at the low end, and the signal ends up being smeared out. The traditional way of combating this is to make narrower and narrower filters to try to minimize the amount of smearing in each individual filter. Figure 4 shows an observation of B1937+21 made yesterday at the 140 Foot using the Spectral Processor, which you can think of as a very fancy filter bank. You can see the pulse profile collected by the Spectral Processor shows very wide peaks. This is simply because the pulse is smeared in each of the frequency channels. While you can make your channels smaller and smaller, a more effective way of dealing with the problem is to take the undetected radio frequency signal and undo the dispersion before you detect it.

Don Backer and his students have put together such a system using custom VLSI chips. Figure 5 shows data collected with his hardware at the 140 Foot. These were taken in parallel with the data shown in Figure 4. You can see that coherent de-dispersion yields much narrower pulses. That allows us to measure pulse arrival

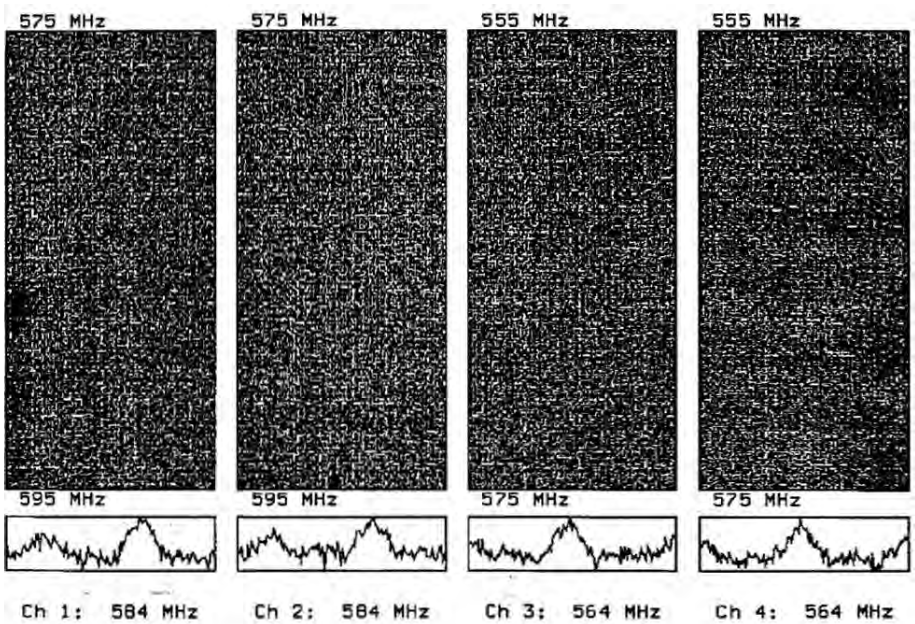

Fig. 4— A 140 Foot observation of PSR B1937+21 using the Spectral Processor. The four plots at the bottom each show a pulse profile collected over 20 MHz in one sense of polarization. The observed pulse peaks are very broad because of dispersion within individual spectral channels. The gray scale plots show the pulse profiles in 256 subbands of each 20 MHz band; B1937+21 is not strong enough to be seen here.

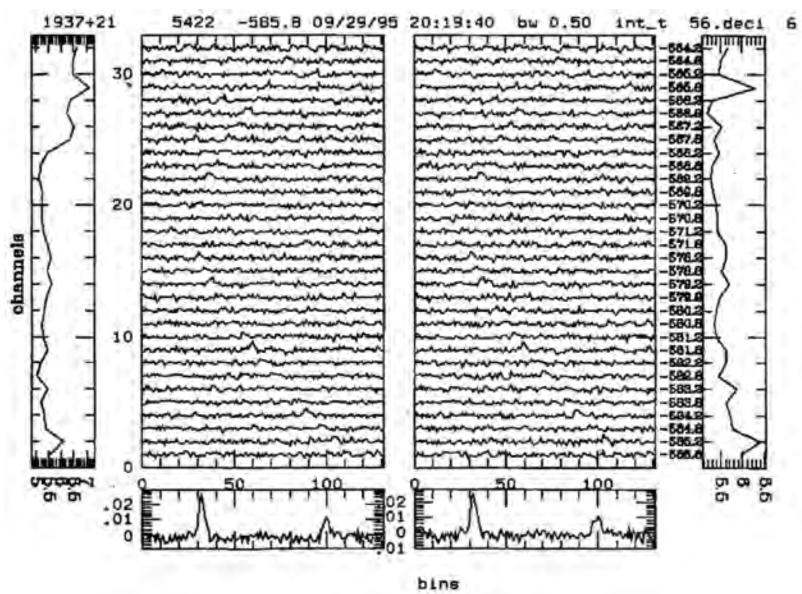

Fig. 5— A 140 Foot observation of PSR B1937+21 using the CDRP. In contrast to the Spectral Processor, the CDRP incorporates coherent de-dispersion, which restores the pulse profile to its original shape—hence the relatively narrow pulse peaks.

times much more precisely, which can dramatically improve the precision of any pulsar timing experiment.

DISCUSSION

Unidentified: What's the second pulse?

D. Nice: Probably the north pole and the south pole of the pulsar as it rotates around, although in this case I'm not completely sure of that. In a few of the younger pulsars you see a main pulse and inter-pulse, and from the polarimetry it looks as if we may be seeing the two magnetic poles. In some of these millisecond pulsars it looks like the magnetic and rotation axes are almost aligned and we are viewing them from the direction of the axis, and you end up getting a relatively messy pulse profile. In fact, most millisecond pulsars have several components rather than just one.

Richard Simon: We've gone in 20 minutes from the very first observations with the 140 Foot [Taylor's talk] to the most recent ones from yesterday.

Tom Wilson: You mentioned this idea of looking for gravitational waves, the buffeting by gravitational waves using millisecond pulsars. Can you briefly describe the uncertainties in these measurements.

D. Nice: The biggest problems right now are systematics— we saw that one pulsar has some sort of rotational instability, but we can't guarantee that it is not, in fact, the pulsar being buffeted around by a gravitational wave that is floating out there. That curve is the current best limit on the power in a gravity wave background of that sort of wavelength – a ten year time scale. If you have many sources and you see big signals from one pulsar, you can blame it on that pulsar rather than on gravity waves coming through the Earth.

B. Burke: I want one number – you've got an upper limit to the strain —what is the strain ?

D. Nice: Do you remember offhand, Joe?

Joe Taylor: The current upper limit is about 10^{-7} of the closure density in a octave band around one cycle per year.

Continuum Observations with the GBT*

J. J. Condon
National Radio Astronomy Observatory,

1. Single Dishes Versus Interferometers

"The 140 Foot is not a continuum telescope." Astronomers at this meeting were startled by this innocent observation of physicist Dave Wilkinson, as if he had blurted "The emperor has no clothes." However, his observation is pretty close to reality, and its validity is not confined to the 140 Foot—most continuum observers have switched from single dishes to aperture-synthesis interferometers. Interferometers have a number of advantages beyond their obvious potential for much higher resolution: They are more resistant to interference and small fluctuations in receiver gain, atmospheric emission, and ground pickup. (I continue to be impressed by the consistent ability of the VLA to produce data that approach their theoretical limits. Single-dish continuum observing is a risky business by comparison.) Interferometric position measurements are nearly independent of both atmospheric refraction and telescope distortions produced by gravity, solar heating, and wind. Aperture-synthesis images with extremely high dynamic range (up to $10^5 : 1$) are possible with self-calibration, weighting in the (u, v) plane, and other software techniques for correcting "surface" errors in the synthetic aperture and optimizing its "illumination." On the other hand, arrays still have severe disadvantages for continuum observations: Correlator electronics usually limit the total instantaneous bandwidth and hence sensitivity, and the finite number of correlator frequency channels limits the field of view that can be imaged without bandwidth smearing. So many receivers are required that frequency coverage and noise temperatures are normally much worse than on contemporary single-dish telescopes, and multibeaming is impractical. The heavy computing requirements of aperture-synthesis mapping hamper large-scale observing projects. The lack of short spacings affects imaging of extended, low-brightness sources.

Despite such practical distinctions and the (frequently incorrect) claims by partisans of either single dishes or interferometers, there is in principle very little difference between a single dish and a compact synthesis array. For example, the sensitivity of a single dish with collecting area A and N receivers (in a multibeam configuration) is about the same as the sensitivity of an array with the same total area and number of receivers, for both point and extended sources. Some of the advantages attributed to interferometers—the ability to suppress atmospheric emission fluctuations—are really attributes of any telescope with high resolution. A large single dish is often *much* better than a small one because it samples a larger cross-section of the atmosphere and the atmospheric volumes sampled by adjacent beams overlap more closely. The relevant measure of "size" is not simply the dish diameter D; it is D^2/λ. In this sense, the 100 m GBT will be a very large dish at short wavelengths. Since the balance of practical advantages shifts with advances in computing and electronics, the relative merits of single dishes and arrays must be re-evaluated frequently. Also, the different instruments excel in different regions of observational parameter space and often have complementary, not competitive,

* Presented at the 140 Foot Telescope Birthday Symposium, September 1995.

uses. Even those continuum astronomers who haven't used a single dish in years should be able to find new and even unique opportunities with the GBT, if they try.

2. Surveys, Galactic and Extragalactic

The relative merits of single dishes and interferometers can be illustrated by the problem of making continuum sky surveys. Will the GBT be able to make useful continuum sky surveys? How would they compare with state-of-the-art VLA surveys such as the NRAO VLA Sky Survey (NVSS, Condon *et al.* 1994)?

The sensitivity of any continuum survey is limited by noise and by confusion from faint unresolved sources. The $S_{\rm c} = 5\sigma_{\rm c}$ point-source confusion limit for the GBT is shown in Figure 1; it is $S_{\rm c} \approx 200\,[\nu/1 \text{ GHz}]^{-2.7}$ mJy. The detection limit of the NVSS is 2.5 mJy at $\nu = 1.4$ GHz, far below the $S \approx 80$ mJy confusion limit of the GBT at that frequency. The top panel of Figure 2 shows an approximately $3^\circ \times 3^\circ$ patch of sky imaged to the confusion limit by the 300 Foot (91 m) telescope (r.i.p.) at 1.4 GHz; the 100 m GBT can go just a little deeper. The GBT will be able to detect 2.5 mJy sources only at frequencies $\nu \geq 5$ GHz. Figure 2 (middle) is a noise-limited ($\sigma_{\rm n} \approx 4$ mJy) 4.85 GHz image of the patch, also made with the 91 m telescope. The FHWM resolution of the NVSS is $45''$, selected to be well above the median angular size $5'' \leq \langle\theta\rangle \leq 10''$ of faint extragalactic sources. This relatively large beam is needed to ensure photometric accuracy and completeness—survey images are actually limited by surface brightness (K or Jy beam^{-1}), not flux density (Jy). The NVSS image of the same area is shown in the bottom panel of Figure 2. The GBT resolution approaches $45''$ at $\lambda \approx 2$ cm. In principle, the GBT should be able to survey the sky at 15 GHz with sensitivity and resolution similar to those of the 1.4 GHz NVSS. What about in practice?

By coincidence, 2 cm is the longest wavelength for which a 7-beam $\times$ 2 polarization = 14 channel Gregorian-focus feed and receiver fits into a standard dewar. (A 7-beam prime-focus receiver could be built at 5 GHz, but the off-axis beams would be badly degraded by coma aberration.) The GBT and VLA have comparable collecting areas, and the $\Delta\nu \approx 1$ GHz bandwidth of the GBT makes its 14-channel sensitivity competitive with the VLA's 54 receiver channels of $\Delta\nu \approx$ 100 MHz bandwidth. The GBT could detect a $5\sigma_{\rm n} = 2.5$ mJy source after about 0.5 s of integration. Unfortunately, the primary beam area of any telescope scales as ν^{-2}, making all high-frequency surveys very slow. This is the main reason that no large-scale surveys have yet been made at frequences higher than 5 GHz. It would take too long (several years of continuous observing) for a 7-beam receiver on the GBT to survey the whole sky to this sensitivity at 15 GHz. It appears that the GBT will not be competitive with the VLA for deep all-sky continuum surveys, but it will be able to make complementary surveys at much higher frequencies with either limited sensitivity or restricted sky coverage. For example, the GBT could survey the north ecliptic pole region favored by satellites such as *IRAS* and *ROSAT*.

Are high-frequency continuum finding surveys likely to be *scientifically* worthwhile? The traditional justification for surveying at higher frequencies has been to discover "new populations" of radio sources. The first 1.4, 2.7, and 5 GHz single-dish surveys of the 1960s found large numbers of flat-spectrum blazars that were relatively rare in earlier source samples selected at lower frequencies. However,

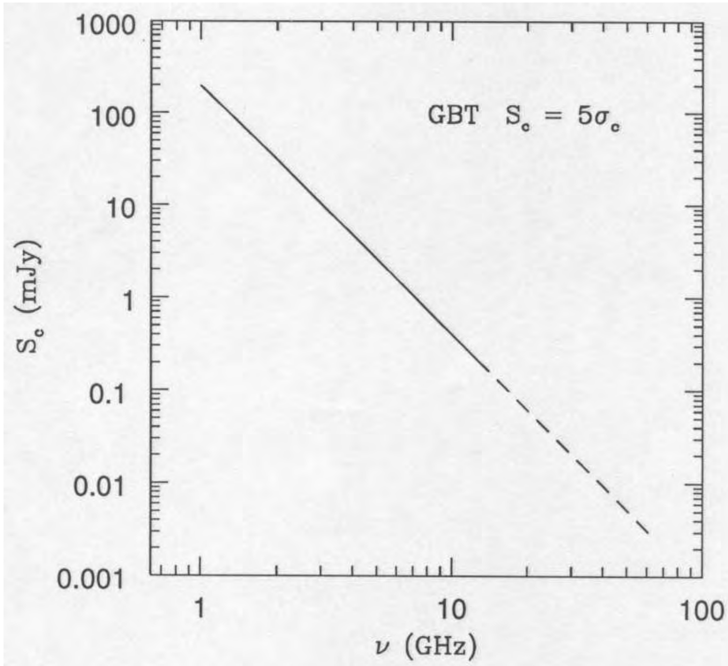

Fig. 1— The confusion detection limit $S_c = 5\sigma_c$ *of the GBT is approximately* $200[\nu(\mathrm{GHz})]^{-2.7}$ *mJy.*

very few of these have strongly inverted radio spectra above 5 GHz, so going from 5 to 15 GHz may not yield anything new. Radio astronomers can think of the *IRAS* $\lambda = 60$ μm far-infrared sky survey (Moshir *et al.* 1992) as a $\nu = 5000$ GHz radio survey with an $S \approx 200$ mJy detection limit. The only significant "new population" of extragalactic radio sources found at 5000 GHz consists of dusty galaxies emitting thermal radiation. Even highly redshifted members of this population are not likely to be strong radio sources below $\nu \approx 100$ GHz, the upper frequency limit of the GBT. Identifications of *IRAS* sources with radio sources stronger than 25 mJy at $\nu = 4.85$ GHz reveal a conspicuous absence of blazars within an order-of-magnitude of the 4.85 GHz detection limit—all *IRAS*-detected blazars are weaker at 5000 GHz than at 4.85 GHz (Condon *et al.* 1995). Therefore I suspect that very few "new" extragalactic sources will be found by GBT surveys. (Earlier in this meeting we heard the Mezger Rule: Ignore the theoretician who tells you what *not* to observe. Maybe now you are thinking that it should be generalized: Ignore the observer who tells you what not to observe, too!)

Galactic sources are another matter. We know that the Galaxy is bursting with highly variable and transient radio sources in gamma-ray sources, radio stars, x-ray binaries, novae, black holes, and other exotic objects. There is still no systematic census of the animals in this "zoo." Since many are weak, vary on time scales of hours to days, and have low duty cycles, repeated sensitive surveys of the Galactic plane will be needed to find them all. Most Galactic variables appear to have flat or inverted radio spectra, so they stand out against the steep-spectrum

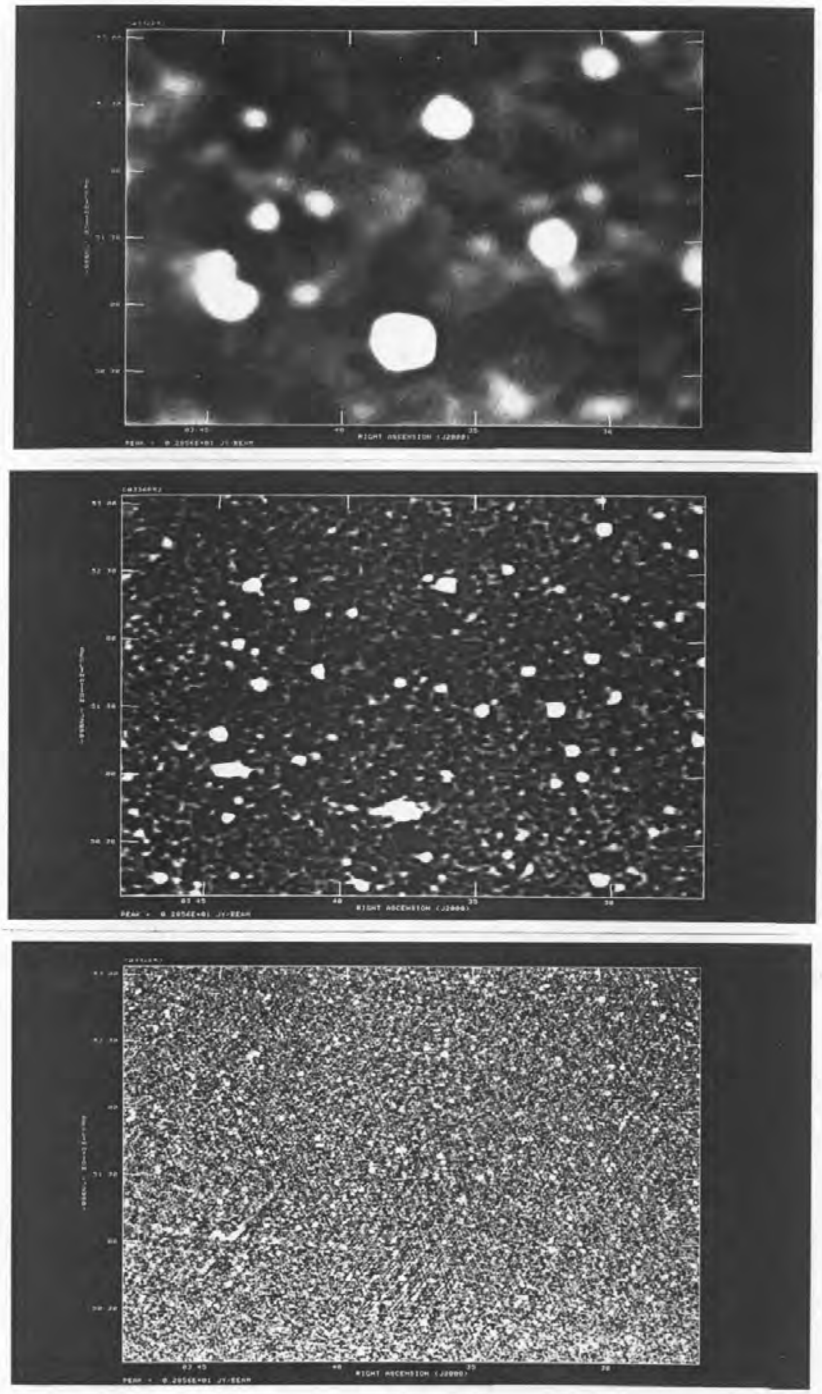

Fig. 2— Continuum images of the same area from the 91 m telescope at 1.4 GHz (top) and 4.85 GHz (middle), and from the VLA at 1.4 GHz (bottom).

Galactic background at high frequencies. Gregory & Taylor (1986, and references therein) used the 300 Foot Telescope with a dual-beam receiver at $\lambda = 6$ cm to conduct a "radio patrol" of the northern Milky Way between 1977 and 1984 that found sources stronger than $S \approx 70$ mJy. The GBT with a 7-beam receiver at $\lambda = 2$ cm would provide more than an order-of-magnitude improvement over their pioneering efforts. Scanning parallel to the Galactic plane at a rate of 10° per minute, the GBT could cover $|b| \leq 1°$, $350° < l < 260°$ *daily* with Nyquist sampling and an rms noise $\sigma_{\rm n} \approx 2$ mJy. The scans from several days could be combined to form a $\lambda = 2$ cm continuum image of the Galactic plane with excellent dynamic range (thanks to the unblocked and filled aperture of the GBT) and sensitivity (a $5\sigma_{\rm n} \approx 3$ mJy detection limit after ten days). No existing interferometer can approach this performance, especially at high frequencies—the VLA has a snapshot dynamic range of only $\approx 10^3 : 1$ and no rapid scanning capability. I believe that the GBT will yield a unique opportunity to explore and develop this important new field of continuum radio astronomy.

3. Observations of Known Sources

As a large filled aperture, the GBT has a comparative advantage for observations of extended or low-brightness sources at high frequencies. For example, there is a long-term program using the 100 m telescope of the MPIfR to make total-intensity and linear polarization images of nearby spiral galaxies at $\lambda = 2.8$ cm and longer wavelengths (Niklas *et al.* 1995, and references therein). These images reveal the large-scale magnetic field structures that distinguish between different models for the generation and maintenance of magnetic fields in spiral galaxies, and Faraday rotation-measure images reflect the distribution of H II regions and "coronal holes" in hot halo gas resulting from high star-formation rates (Ehle & Beck 1993).

Nearly all continuum observations of spiral galaxies have been made at wavelengths $\lambda \geq 2.8$ cm because existing telescopes don't have the surface-brightness sensitivity needed to detect normal spirals at shorter wavelengths. Consequently it has been very difficult to distinguish and measure the thermal emission whose flux dominates only at wavelengths $\lambda \leq 1$ cm. Thermal emission from H II regions ionized by massive stars is a direct and extinction-free tracer of the current star formation rate in spiral galaxies. In contrast, the strength of the nonthermal emission that has been observed at long wavelengths is affected by additional factors (e.g., magnetic field strength) and processes (e.g., Compton scattering), the spatial distribution of nonthermal emission is blurred by cosmic-ray propagation, and there is no simple, direct theory relating nonthermal emission to star formation. The GBT should be the first telescope capable of mapping the thermal emission at $\lambda \approx 1$ cm ($\nu \approx 30$ GHz) from a statistically significant sample of nearby normal galaxies. Such maps will yield valuable clues about massive star formation, particularly when used in conjunction with optical and near-infrared recombination-line images plus *IRAS* far-infrared continuum fluxes and ISO images.

4. Implications for the GBT

There are at least four types of continuum observers: (1) Those who look where there are no sources (e.g., microwave background observers such as Dave Wilkinson), (2) those who look where there are holes in the sky (e.g., Juan Uson mapping the Sunyaev-Zeldovich clusters), (3) those who look everywhere (me), and

(4) the majority who look where there are sources. In spite of these differences, our telescope/receiver needs are similar. Continuum observations often impose extremely strict requirements on telescope and receiver quality. For example, continuum surveys in the 1960s detected only $\approx 10^3$ of the strongest sources with the notoriously unstable and unreliable paramp receivers. After stagnating in the 1970s, the field was revived by the first FET receivers developed in the 1980s, which actually had higher noise temperatures but were absolutely reliable and exhibited fractional gain fluctuations smaller than $(B\tau)^{-1/2} \approx 10^{-4}$ on time scales of seconds. Elevation focus tracking and the seven-beam receiver on the 91 m telescope made possible the 4.85 GHz sky surveys of 1986 and 1987 that detected 7.5×10^4 sources. Only in the mid-1990s did the improved L-band VLA receivers and advances in computers permit the 1.4 GHz VLA survey observations and mapping.

To achieve the highest dynamic range at short wavelengths, it will sometimes be necessary to keep the rms surface errors well below the $\lambda/16$ value required for reasonable efficiency. The noise-limited rms position error in one coordinate is only $\sigma_{\alpha,\delta} \approx \sigma\theta/(2S)$, where S/σ is the signal-to-noise ratio and θ is the FHWM beamwidth. If $S/\sigma = 10$ and $\theta = 45''$, then $\sigma_{\alpha,\delta} \approx 2''$. The NVSS will yield many thousands of sources per steradian with $\sigma_{\alpha,\delta} \approx 0\farcs3$ strong enough to be used as local pointing calibrators, so the pointing repeatability of the GBT should be made as stable as possible on time scales of minutes. The non-repeatable pointing errors should be much smaller than $\theta/10$ so that continuum scans can be interleaved accurately.

Sensitive and stable multibeam receivers will be essential for observations of low-brightness sources (and the Sunyaev-Zeldovich effect), not only to multiply the data rate, but to improve data quality by yielding several simultaneous scans that can be analyzed to remove interference, atmospheric emission fluctuations, ground pickup, and other time-dependent sources of error. These image-plane receivers must be very reliable because the failure of even a single channel badly degrades their imaging capability, in contrast to the aperture-plane receivers of the VLA, where the loss of one or two receivers is scarcely noticeable. [The "holy grail" of multibeaming is a true array feed that synthesizes a two-dimensional grid of partially overlapping beams (cf. Emerson & Payne 1995). Simple one-dimensional array feeds have been developed for low-frequency use at Arecibo and Green Bank, but these designs are not low-noise and are not easily extended to two dimensions at high frequences. A first step might be to construct a feed array for $\lambda = 1$ cm that could image the Sun continuously, so that low noise would not be required for scientifically interesting results.]

As receivers improve, the continuum performance of the GBT is likely to be limited by very low-level telescope defects such as feed polarization, ground pickup, and imperfect beamswitching. Tracking down and solving all of these problems will probably take years of effort after the GBT is built.

5. Conclusion

With its large D^2/λ size and clear aperture, the GBT has the *potential* to be an outstanding continuum telescope. The NRAO should provide the long-term support needed to realize its potential.

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Jim Condon and Gerrit Verschuur at a GBT planning meeting, ca.1989.
[photo courtesy Bob Rood]

Aerial view of the Green Bank Observatory in 1980, looking south. The 300 Foot Telescope is at the top right; the 85-2 and 85-3 Telescopes to its left; the 140 Foot at middle right (in service position); the 40 Foot Telescope in the lower middle; and the 85 Foot Tatel Telescope at the lower left.

4 Death and Resurrection

Editors' Note: In 1999 the new 100 meter Robert C. Byrd Green Bank Telescope (GBT) was nearing completion. Because the efforts of the Green Bank staff needed to be focused on the new telescope, which would soon superceed the 140 Foot in every respect, the 140 Foot was retired.

Bob Rood describes the last astronomical observation done on the 140 Foot and expresses what many astronomers felt: that its demise was premature and that the 140 Foot was still capable of important research.

After a rest of a few years, the 140 Foot was given a new task starting in 2006: observing radar echos from Earth-orbiting satellites to study the ionosphere.

The subreflector of the 140 Foot being removed for the last time, August 1999.

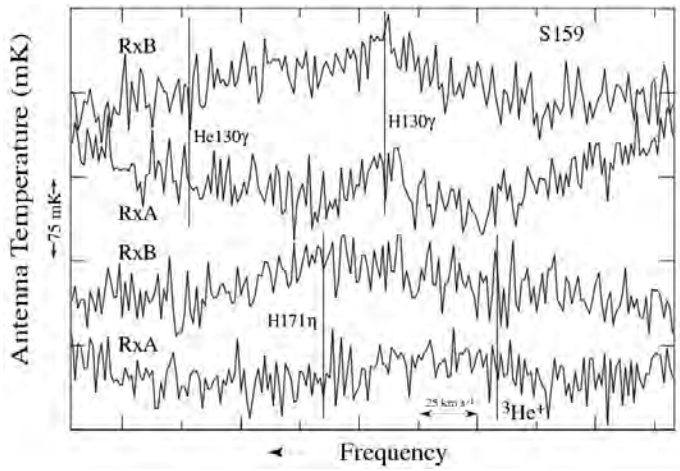


Fig. 1— The Last Scan at the 140 Foot Radio Telescope: 08:12:20 EDT, 19 July, 1999.

Fig. 2— Bob and Red.

An Old Dog's Last Hunt*

The Last Observations of the NRAO Green Bank 140 Foot Radio Telescope

Robert T. Rood (University of Virginia)
Thomas M. Bania (Boston University)
Dana S. Balser (NRAO-Green Bank)

Abstract

At 08:12:20 EDT, 19 July, 1999, the Green Bank 140 Foot Telescope of the National Radio Astronomy Observatory made its last scientific observation, having been given a 12 minute reprieve. That last 12 minute Total Power pair (Figure 1), taken toward the H II region S159, to the untrained eye looks like a noise spectrum, like most of the other roughly 25,000 pairs we have obtained since 1982. However, careful examination revealed a hint of a feature at the frequency of the H130γ recombination line. Our "target," the much weaker hyperfine line of $^3He^+$, is readily visible in the 11.8 hours of integration acquired toward S159 over the previous three nights. The fact that we could detect $^3He^+$ in a mere 11.8 hour integration, or indeed that we would be observing an obscure H II region like S159, reflects both the dramatic improvement in receivers and our change in thinking since 1982. We take this time to reflect on the last days of a noble instrument and its contributions toward our knowledge of an isotope whose cosmic abundance can set important astrophysical constraints for theories of primordial nucleosynthesis and the chemical evolution of the Galaxy.

Prolog

About 10 years after the photograph in Figure 2 was taken, Red was an old rather decrepit dog. An encounter with a milk truck had left his rear legs severely damaged.

Since Red had enjoyed hunting when young, my father and several of his friends decided to take Red on one last hunt. After some time of dragging his rear along Red pointed as if he scented birds. The hunters, thinking him to be a useless old dog, walked ahead ignoring his point. A covey of quail burst noisily into the air. This was repeated several times. Red must have been disgusted: "I may not be able to run, but I can still smell and hunt."

This memory returned during our last observing sessions at the 140 Foot. It was capable of making the best observations of its long and illustrious career. The Autocorrelator and Honeywell 316 were as cranky as ever, but great data spewed

* Adapted from a poster presentation given at the 198th meeting of the American Astronomical Society in Pasadena, CA, June 2001. *This research was supported by the National Science Foundation (AST 97-31484).*

forth. To kynomorphize* , the telescope must have thought: "I can still smell, why are they turning me off?"

At least Red wasn't euthanized after his last hunt.

The Final Observation

At the end of our observing session Nathan Sharp, the telescope operator, engaged the 140 Foot executioners in conversation. There was a 12 minute reprieve. Figure 1 shows the spectrum we obtained during that period. We simultaneously observed the $^3He^+$ hyperfine line and the hydrogen, helium, and carbon recombination lines at two orthogonal polarizations. The typically 1-2 mK $^3He^+$ line is not visible in such a short integration. However, there is a hint of the H130γ line in each polarization.

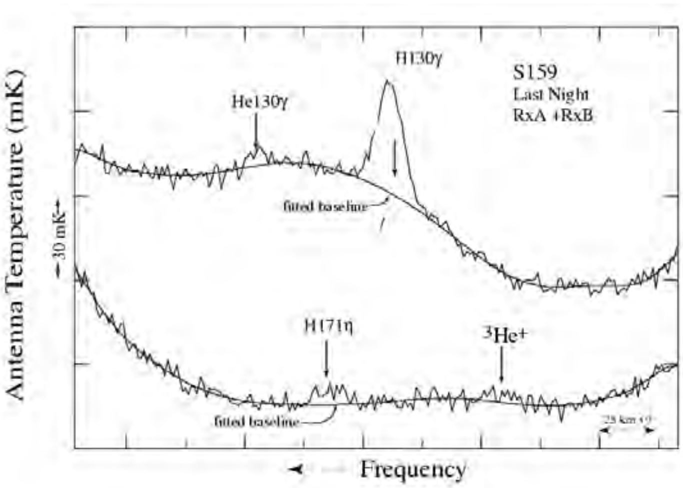


Fig. 3— Average of all data from the night of 19 July.

We were able to observe S159 for 4.2 hrs on the night of 19 July. Averaging all of this data and combining the 2 polarizations (see Figure 3) gives an integration time of 8.4 receiver hours and a noise level 6.5 times less than in an individual scan.

One obvious feature is that the spectral baselines are not flat. The baseline curvature results from standing waves created by multiple reflections from the telescope superstructure. This baseline structure is real and must be removed. This can be easily done with polynomial fits. The hard part is establishing the reliability of such fits. Over the years we have developed tests which demonstrate that our techniques are robust. Note that the fitted baselines in Figure 3 are plausible.

Several lines, including a feature at the $^3He^+$ frequency, are visible. From this data alone (and our experience) we would be relatively certain of a detection. However, obtaining a ^3He abundance requires good line parameters and considerably more integration time.

* *From Greek $\kappa\upsilon\nu\epsilon\iota o\sigma$, "dog-like"; the dog parallel to "anthropomorphize."*

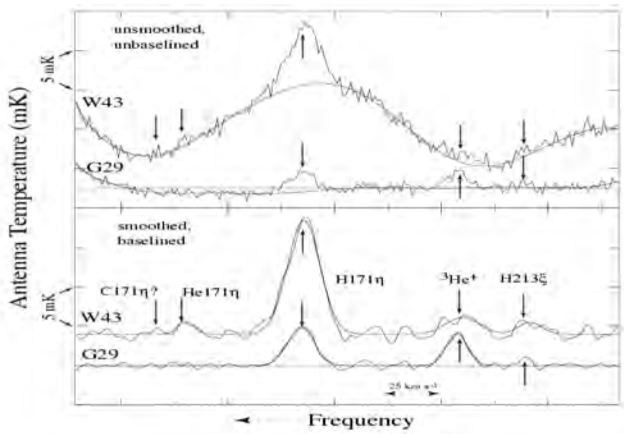


Fig. 4— The importance of H II region density.

Why S159?

If one is searching for a weak line in an H II region, wouldn't the best targets be sources like Orion A or H II regions from the Westerhout catalog? Indeed our first target in this project at 10 AM, 23 February, 1982 was W51; we moved to Orion A when it rose. This is just the first case we encountered where the conventional wisdom was wrong.

Because the strength of the $^3He^+$ hyperfine line varies linearly with density of ionized gas and the radio continuum varies with density squared, the relation between H II region brightness and $^3He^+$ line strength is rather counter-intuitive. Being big (in angular size) and distant, are just as important as being bright. We soon realized this and targeted sources like W43 & S209. However, it turns out that the best targets were not cataloged. So we began to make our own survey of potential targets.

Still we didn't have the guts to trust the equations showing the best candidates when pushed to their limits. Eventually, for other reasons we tried a few chancy targets and discovered that the plausible target list could extend to the wimpiest of H II regions. Hence, we have been surveying for $^3He^+$ over as much of the Galaxy as possible. We had gotten to S159 when the plug was pulled on the 140 Foot.

The importance of H II region density is illustrated in Figure 4. W43 is probably the best candidate of the classic H II regions. G29 is an anonymous blob in the same complex. Still G29 has a stronger $^3He^+$ line than W43. Perhaps even more important, as seen in the upper panel, is that the baseline structure is considerably less for G29. This is because it has less continuum to bounce around

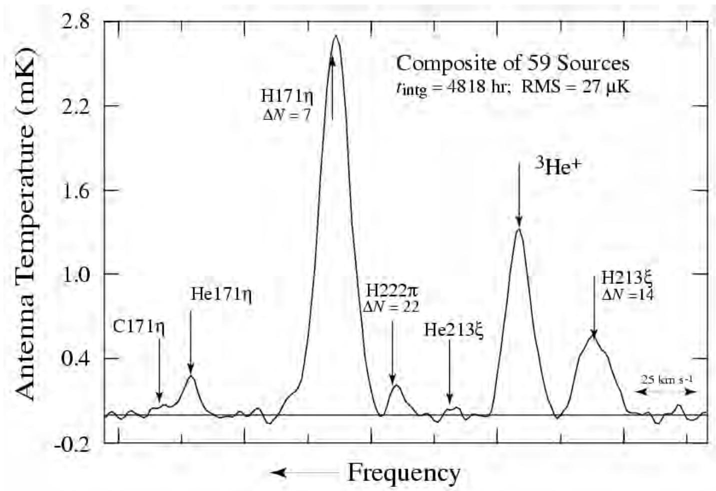
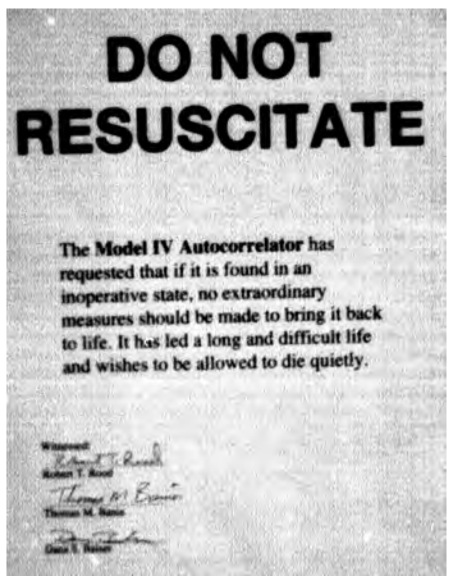


Fig. 5— 3-Helium at the Green Bank 140 Foot (1982 – 1999).

the telescope super-structure. It also turns out that one can model these low density sources more easily and determine better abundances.

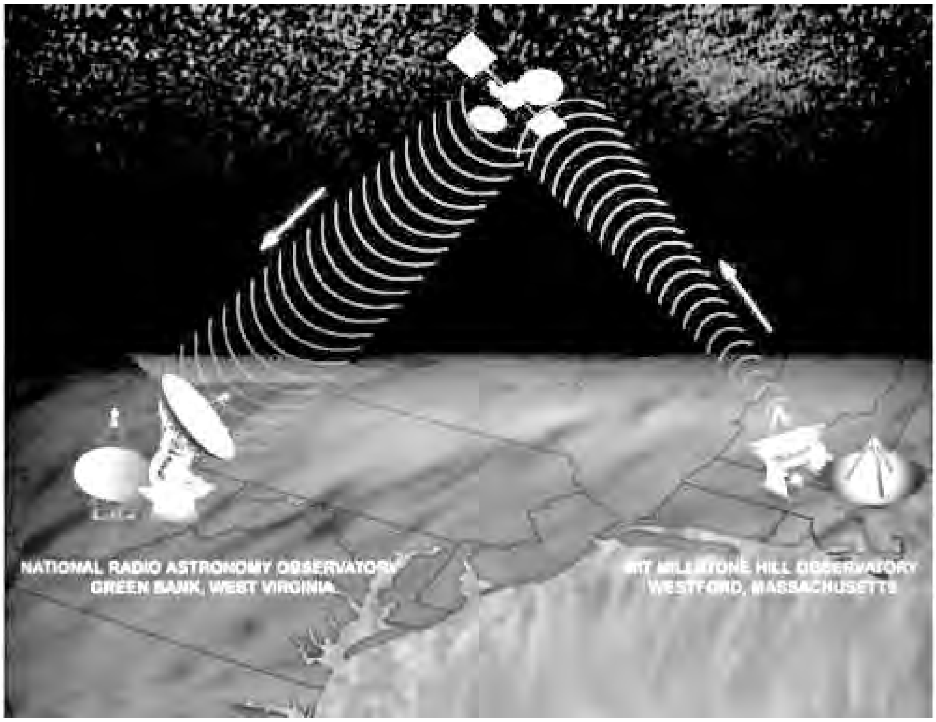
In the end we can summarize our 17 years with the 140 Foot with a grand, grand average (Figure 5) of all of our H II region observations, 4818 hours in total.

Bob Rood at the 140 Foot celebration, September 1995.
[Photo courtesy R. Maddalena]



Sign placed on the Mark IV Autocorrelator by Bania, Rood, and Balser during their last observing run, July 1999.

Resurrection: The 140 Foot is reborn as the "43 meter"



Radar signals from the MIT Lincoln Labs are received by the Green Bank 43 Meter Telescope.

Bi-Static Radar Collaboration to Measure the Earth's Ionospheric Turbulence

G. Langston, R. Prestage, P Jewell (NRAO)
From the NRAO Newsletter No.105, October 2005.

Lincoln Laboratories and the NRAO are collaborating to measure the properties of the Earth's ionosphere using bi-static radar techniques. Lincoln Laboratories is building a special wide-band (150 to 1700 MHz) feed and front end system that will be installed on the NRAO 43m (140ft) telescope. The NRAO is developing an automated system to follow Lincoln Laboratory's spacecraft coordinates. The 43m will track spacecraft beacons and also spacecraft illuminated by the Millstone radar at Haystack, Massachusetts.

Lincoln Laboratory's engineers will drive a semi-trailer full of high-speed electronics to Green Bank, where it will be installed at the base of the 43m telescope.

The trailer is shielded to contain any radio frequency interference the electronics may generate. The Lincoln Laboratory's electronics will select and sample the RF signals and write the digital data to a disk recording system. The disk packs will be mailed to the Lincoln Laboratories office in Lexington, Massachusetts for further analysis.

The 43m operations was shut down in 1999, and the NRAO feared that it might not be possible to fully restore the 43m to operations. Detailed tests of the hydraulics system were required before the collaboration could begin. The Green Bank staff have worked with dedication to restart the 43m system. The 43m hydraulic systems have now been restored to full operations, and a new control computer system has been installed. We greatly appreciate the hard work of all those who have contributed to this success.

We expect to install the Lincoln Laboratories feed and front end system in September 2005 and make the first test observations in October 2005.

Lincoln Laboratories is operated by the Massachusetts Institute of Technology for the United States Air Force.

Measuring the Earth's Ionospheric Turbulence

G. I. Langston and R. M. Prestage (NRAO)

From the NRAO Newsletter, No.106, January 2006.

Lincoln Laboratories and the NRAO have begun regular observations of the Earth's ionosphere using bi-static radar techniques. . . .

Currently the 43m telescope is tracking spacecraft Tuesday through Friday from 10:00 a.m. to 3:00 p.m. and an operator is present at the telescope to assure proper operations. In early 2006, we will complete an upgrade to the remote monitoring of the 43m hydraulics systems. At that time we will begin un-attended operations 24 hours a day.

Phil Erickson and Frank Lind of MIT have installed a second radar experiment at the 43m as well as an array of 6 "discone" antennas. Their experiment is testing the use of reflected FM radio stations as probes of the ionosphere.

Note added in 2016
Re-resurrection for space VLBI

The Lincoln Labs project ended in late 2011. In 2012 the 140-foot was refurbished and outfitted to serve as a ground station for the Russian RadioAstron (Spektr-R) orbiting radio telescope. VLBI between RadioAstron and ground stations, with interferometer baselines up to almost the distance of the Moon, have made possible a sucessful program of observing compact cores of quasars and active galaxies with unprecedented angular resolution (e.g. see ApJ Lett. **820:**L10, 2016) *(Note added in 2nd printing, Eds)*

5 Symposium Appendices

Scientific Highlights of Thirty Years of Observations with the 140 Foot*

GALACTIC ASTRONOMY

• H II Regions

In July 1965, in one of the first scheduled observations on the 140 Foot, P. Mezger and B. Hoglund made the first unambiguous detection of recombination lines at radio frequencies. Although two Russian groups had independently claimed detection of these lines, the Mezger and Hoglund measurement showed that they could be a powerful tool for studying ionized gas in the Galaxy. This discovery prompted two major recombination line survey programs which were the PhD theses of E. Reifenstein and T. Wilson.

The identification of H II regions by their radio recombination line emission has been a continuing effort on the 140 Foot. More Galactic H II regions have been detected on the 140 Foot than on all other telescopes combined.

Mezger's group also made high resolution continuum observations and discovered compact H II regions which were linked with OH masers. These objects were also discovered at about the same time by Ryle and Downs using the Cambridge one-mile telescope.

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Wilson, T. L., Mezger, P. G., Gardner, F. F., and Milne, D. K. 1970, A&A, 6, 364. "A Survey of H109α recombination line emission in Galactic H II regions of the southern sky."

• Molecular Spectroscopy

The 140 Foot telescope has been important in the study of interstellar molecules. As of 1995, the 140 Foot has discovered about one half of all known molecules that have transitions at cm wavelengths. Two discoveries were especially significant: In 1969, L. Snyder, D. Buhl, B. Zuckerman, and P. Palmer measured formaldehyde (H_2CO) in absorption against numerous Galactic and extragalactic radio sources. H_2CO was the first organic polyatomic molecule ever detected in the interstellar medium. Its widespread distribution indicated that interstellar chemistry was more complicated than previously assumed.

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* *From a handout at the 140 Foot Symposium. It reflects the situation in 1995.*

A few years later, cyanoacetylene (HC_3N) was detected by B. Turner in emission towards Sgr B2. This was the first long-chain molecule detected in the interstellar medium and demonstrated that complex molecules could exist in space. It was the discovery of new and unexpected molecules in the 1980's that allowed the 140 Foot to continue to play a significant role in molecular line work. In 1984, Matthews et al. detected C_3O, the first known interstellar carbon chain molecule to contain oxygen.

The 140 Foot played an important role in helping to identify, and in studying, the hydrocarbon ring molecule C_3H_2. In 1985, M. Bell and H. Matthews detected $HC_{11}N$ in the Taurus molecular cloud complex. This was the largest interstellar molecule detected as of 1995. The 140 Foot has been an important tool in the detection and study of interstellar masers. A. Barrett and A. Rogers observed intense circular polarization in narrow OH emission lines in the direction of the radio source W3. This emission, also detected at about the same time by Verschuur and Davies at Jodrell Bank, was evidence for maser-type amplification in the interstellar medium.

In 1968, W. Wilson and A. Barrett detected OH emission from the circumstellar envelopes of infrared stars. B. Turner used the 140 Foot to survey the Galactic plane in all four 18-cm lines of OH. In 1991, K. Menten discovered the 6.6 GHz transition of methanol, which is the second strongest masering molecule known.

Barrett, A. H. and Rogers, A. E. E. 1966, Nature, 210, 189. "Observations of circularly polarized OH emission and narrow spectral features."

Bell, M. B. and Matthews, H. E. 1985, ApJ, 291, L63. "Detection of $HC_{11}N$ in the cold dust cloud TMC-1."

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Menten, K. M. 1991, ApJ, 380, L75. "The discovery of a new, very strong, and widespread interstellar methanol maser line."

Snyder, L. E., Buhl, D., Zuckerman, B., and Palmer, P. 1969, Physical Review Letters, 22, 679. "Microwave detection of interstellar formaldehyde."

Turner, B. E. 1971, ApJ, 163, L35. "Detection of interstellar cyanoacetylene."

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- **Galactic Neutral Hydrogen**

In 1968, G. Verschuur made what is generally considered the first detection of the Zeeman splitting of the 21-cm line of hydrogen in space. These measurements provided information about the interstellar Galactic magnetic field.

In 1978-1980, W. B. Burton and H. Liszt made extensive 140 Foot H I observations of the Galactic Center which they used to develop a barlike model of the inner Galaxy. F. J. Lockman, K. Jahoda, and D. McCammon used the 140 Foot to measure the directions in the northern sky with the least H I emission. This provided a window to extragalactic objects for observations at UV and X-ray wavelengths.

Liszt, H. S. and Burton, W. B. 1980, ApJ, 236, 779. "The gas distribution in the central region of the Galaxy. III A barlike model of the inner-galaxy gas based on improved H I data."

Lockman, F. J., Jahoda, K., and McCammon, D. 1986, ApJ, 302, 432. "The structure of Galactic H I in directions of low total column density."

Verschuur, G. L. 1968, Physical Review Letters, 21, 775. "Positive determination of an interstellar magnetic field by measurement of the Zeeman splitting of the 21-cm hydrogen line."

EXTRAGALACTIC SPECTROSCOPY

In general the 140 Foot telescope was used for extragalactic H I for southern sources that could not be covered by the 300 Foot or Arecibo. In 1970, M. Roberts made the first detection of H I absorption arising from within another galaxy. R. Tully and R. Fisher used a collection of H I data from several sources and telescopes, including the 140 Foot, to develop a new method of determining distances to galaxies now widely know as the Tully-Fisher relation. This discovery has prompted several H I surveys in which the 140 Foot has played a complementary role.

In 1994, D. Frayer, R. Brown, and P. Vanden Bout used the 140 Foot telescope and the NRAO 12 m to measure CO emission toward the quasar PC 1643+4631A with a redshift corresponding to the z=3.137 damped Lyα system previously identified in the optical. This is the highest redshift CO detection reported thus far [as of 1995] and reveals the existence of large quantities of metal-enriched molecular gas in this system.

Frayer, D. T., Brown, R. L., and Vanden Bout, P. A. 1994, ApJ, 433, L5. "CO emission from the z=3.137 damped Lyα system toward PC 1643+4631A."

Roberts, M. S. 1970, ApJ, 161, L9. "The detection of 21- centimeter hydrogen absorption arising from within the radio galaxy Centaurus A."

Tully, R. B. and Fisher, J. R. 1977, A&A, 54, 661. "A new method of determining distances to galaxies."

PULSARS

During the early studies on pulsars in Green Bank the 140 Foot and 300 Foot were often used as a complementary team. When collecting area was a high priority, pulsar observers generally used the 300 Foot, at least below about 1 GHz. If longer tracking time was needed, which was almost always true above 1 GHz, or if the pulsar was located at southern declinations, then the 140 Foot was used.

The first pulsar found after the original four at Cambridge was detected by G. Huguenin, J. Taylor et al. in 1968 with the 300 Foot telescope. The 140 Foot was used to confirm this detection and to measure more of the pulsar's characteristics. In 1971, R. Manchester used observations of pulsar polarization and pulse time of arrival at 1665 MHz to derive the magnitude and direction of the Galactic magnetic field towards 21 pulsars. In 1973, K. Gordon and C. Gordon observed 21-cm H I absorption towards pulsars to improve the distance estimates to them.

In 1975, R. Manchester, J. Taylor, and G. Huguenin discovered orthogonal modes of pulsar emission by observing the linear polarization characteristics of individual pulses at 1400 MHz. More recently, there has been a large increase in the use of the 140 Foot for pulsar studies because of the collapse of the 300 Foot and construction at Arecibo.

Two groups are now conducting regular monitoring programs to obtain data on pulse periods and their variation, and to study the interstellar medium. A recently-completed all-sky survey for millisecond pulsars has detected several new pulsars, one of which is a rare relativistic binary.

Arzoumanian, Z., Nice, D. J., Taylor, J. H., and Thorsett, S. E. 1994, ApJ, 422, 671. "Timing behavior of 96 radio pulsars."

Backer, D. C., Hama, S., Van Hook, S. Foster, R. S. 1993, ApJ, 404, 636. "Temporal variation of pulsar dispersion measures."

Gordon, K. J. and Gordon, C. P. 1973, A&A, 27, 119. "Measurements of neutral-hydrogen absorption in the spectra of eight pulsars."

Huguenin, G. R., Taylor, J. H., Goad, L. E., Hartai, A., Orsten, G. S. F., and Rodman, A. K. 1968, Nature, 219, 576. "New pulsating radio source."

Manchester, R. N. 1971, ApJS, 23, 283. "Observations of pulsar polarization at 410 and 1665 MHz."

Manchester, R. N., Taylor, J. H., and Huguenin, G. R. 1975, ApJ, 196, 83. "Observations of pulsar radio emission II. Polarization of individual pulses."

VLBI

The 140 Foot has been a pioneer in the field of VLBI in the United States. The first successful independent local oscillator tape recording interferometer experiment in the US used the 140 Foot and 85-1 on 6 March 1967. The first transcontinental observations were made in July 1967 between the 140 Foot and the 85 ft at Hat Creek. In January 1968, the first transatlantic observations were made with the 140 Foot and the 85 ft telescope in Onsala, Sweden. Because of the high spatial resolution of VLBI this technique is ideal for studying masers. In 1968, J. Moran et al. produced the first VLBI image of an OH maser using the 140 Foot, the 120 ft at Haystack, and the 85 ft at Hat Creek. B. Burke et al. produced the first VLBI observation of water vapor masers using the 140 Foot and the 85 ft at Maryland Point in 1970. C. Gwinn, J. Taylor, J. Weisberg, and L. Rawley used the 140 Foot, Arecibo, and the 40 m at Owens Valley to measure the parallaxes of 6 pulsars. From these observations accurate distances were determined to the pulsars. The 140 Foot has played an important role in the evolution of VLBI and was one of the five dedicated telescopes in the VLBI network organized in 1976.

The 140 Foot was one of many telescopes involved in the observations by T. Pearson et al. that unambiguously showed the superluminal expansion of 3C273. This was one of the most important discoveries made by VLBI techniques.

Burke, B. F., Papa, D. C., Papadopoulas, G. D., Schwartz, P. R., Knowles, S. H., Sullivan, W. T., Meeks, M. L., and Moran, J. M. 1970, ApJ, 160, L63. "Studies of H_2O sources by means of a Very-Long-Baseline-Interferometer."

Gwinn, C. R., Taylor, J. H., Weisberg, J. M., Rawley, L. A. 1986, AJ, 91, 338. "Measurement of pulsar parallaxes by VLBI."

Moran J. M., Burke, B. F., Barrett, A. H., Rogers, A. E. E., Ball, J. A., Carter, J. C., and Cudaback, D. D. 1968, ApJ, 152, L97. "The structure of the OH source in W3."

Pearson, T. J., Unwin, S. C., Cohen, M. H., Linfield, R. P., Readhead, A. C. S., Seielstad, G. A., Simon, R. S., and Walker, R. C. 1981, Nature, 290, 365. "Superluminal expansion of quasar 3C273."

COSMOLOGY

Two experiments requiring the most sensitivity of the 140 Foot were performed in the area of cosmology. During the 1980's D. Wilkinson and J. Uson made a series of very sensitive continuum observations with the 140 Foot to measure small-scale variations in the cosmic microwave background. They were able to detect changes in the background which were greater than 5.6×10^{-5} K. As a result of these measurements they were able to lower the upper limit on small-scale fluctuations in the cosmic microwave background.

Beginning in the early 1980's R. Rood, T. Bania, and T. Wilson have been measuring the 8.7 GHz hyperfine line of $^3He^+$ in Galactic H II regions with the 140 Foot, which continues to be the most sensitive telescope for this purpose. They have detected the $^3He^+$ line in about a dozen sources with antenna temperatures on the order of millikelvins. These measurements constrain standard Big Bang nucleosynthesis models by providing a limit to the primordial ^3He abundance.

Uson, J. M., and Wilkinson, D. T. 1984, Nature, 312, 427. "Improved limits on small-scale anisotropy in cosmic microwave background."

Rood, R. T., Bania, T. M., and Wilson, T. L. 1984, ApJ, 280, 629. "The 8.7 GHz hyperfine line of $^3He^+$ in Galactic H II regions."

Selected PhD Dissertations Using Observations from the 140 Foot Telescope

From a handout for the 140 Foot Symposium, compiled by D. Balser.

NAME	YEAR	THESIS TITLE OR TOPIC
Reifenstein, E.	1968	The Structure of the Galaxy as Determined by Hydrogen Recombination Line Emission
Taylor, J. H.	1968	Lunar Occultations of Radio Sources
Churchwell, Ed	1970	Observations of Radio Recombination Lines of Hydrogen, Helium, and Carbon
Baars, J.	1970	Dual-beam Parabolic Antennae in Radio Astronomy
Predmore, C.	1971	Search for Hyperfine Line from Singly Ionized Helium-3 in Galactic H II Regions
Sandqvist, A.	1971	Lunar Occultations of the Galactic Center Region in HI, OH, and H_2CO lines
Knapp, G.	1972	High Frequency Resolution H I Line Observations of Interstellar Dust Clouds
Velusamy, T.	1973	Polarization of Supernova Remnants at Centimeter Wavelengths
Lo, K.	1974	Interstellar Microwave Radiation and Early Stellar Evolution
Baud, B.	1978	OH/IR Stars in the Galaxy
Haynes, M.	1978	An Investigation of the Intergalactic Medium via the 21 cm line of Neutral Hydrogen
Lockman, F. J.	1979	On the Structure of H II Regions and their Distribution in the Galaxy
Sinha, R.	1979	Kinematics of H I near the Galactic Center
Snell, R.	1979	A Study of Interstellar Dark Clouds
Jahoda, K.	1986	H I Structure and the Soft X-ray Background
Lehto, H.	1989	High Sensitivity Searches for Short Timescale Variability
Tacconi-Garman, L.	1989	Kinematic Models of Cometary Comae
Foster, R.	1990	Constructing a Pulsar Timing Array
Henning, P.	1990	A Study of a 21-cm-Selected Sample of Galaxies
Lundgren, J.	1994	A Multi-wavelength Study of Rotation Driven Pulsars
Balser, D.	1995	The Abundance of ^{3}He in the Galaxy
Arzoumanian, Z.	1995	Radio Observations of Binary Pulsars: Clues to Binary Evolution and Tests of GR.

Early Users of the 140 Foot*

The observations in the first few years done by each user are listed under the year they first observed.

NAME	PROJECT	NAME	PROJECT
1965			
von Hoerner, S.	Lunar occultation	Williams, D.	HI absorption
DeJong, M.	Flux measurements	Zuckerman, B.	OH line
Pauliny-Toth, I.	Flux measurements	Rogers, A.	OH line
Mezger, P.	Recombination lines	Palmer, P.	OH line
Kaftan-Kassim, M.	Planetary nebulae	Roberts, M.	Extragalactic
Baars, J.	Atmospheric tests	Hogg, D.	Scintillation near Sun
Hoglund, B.	Recombination lines	Hobbs, R.	Polarization measurements
Kellermann, K.	Flux measurements	Barrett, A.	OH line
Menon, T. K.	HI absorption	Hollinger, J.	Polarization measurements
1966			
Sastry, C.	Polarization	Turlo, Z.	Scintillation
Dickel, J.	Jupiter and Venus	Tyler, W.	Flux measurements
Schraml, J.	Galactic sources	Altenhoff, W.	Extinction measurements
Taylor, J.	Lunar occultations	Cohen, M.	Scintillation
Swenson, G.	M 31	Heeschen, D.	Elliptical galaxies
Drake, F.	Galactic Center	Ko, H.	Galaxy clusters
Mayer, C. H.	Polarization	Dent, W.	Extragalactic HI abs.
Goldstein, S.	Zeeman in ISM, Cyg A	Hollinger, J.	Polarization
1967			
Clark, B.	VLB with Arecibo	Booth, R.	VLB; OH
Sutton, J.	Lunar occultations	Cudaback, D.	VLB; OH
Burke, B.	Recombination lines	Bare, C.	VLB; continuum
Gebel, W.	HII regions	Cooper, J.	VLB; OH
Johnson, H.	Sco. X-ray source	Riegel, K.	HI near HII regions
Sagan, C.	Mercury and Venus	Hinrichs, L.	HI; high velocity clouds
Morrison, D.	Mercury and Venus	Gordon, K.	HI; M33 and M82
Helfer, L.	Variable sources	Henderson, P.	HI
Davis, M.	Variable sources	Buhl, D.	Lunar craters
Erkes, J.	SN remnant HB 21	Reifenstein, E.	Galactic HII regions
Wendker, H.	Cygnus X	Wilson, T. L.	Galactic HII regions
Kraus, J.	M31	Kundu, M.	Cygnus loop and IC443
Ball, J.	VLB with Haystack	Blum, E.	North Galactic polar cap
Carter, J.	VLB with Haystack	Gulkis, S.	Lunar occultations
Crowther, P.	VLB with Haystack	Coles, W.	Occul. of 3C279, 3C273 by Sun
Jauncey, D.	VLB with Haystack	Heiles, C.	OH emitting dust clouds
Davies, R. D.	VLB; OH	Cunningham, A.	OH Absorption
1968			
Kerr, F.	Lunar occultations	Moran, J.	VLB; OH
Sandqvist, A.	Lunar occultations	Walters, G.	$^3He^+$ in HII regions
Gundermann, E.	Occult. of 3C2 by Sun	Goldwire, H.	$^3He^+$ in HII regions
Harris, D.	Occult. of 3C2 by Sun	Blackwell, L.	$^3He^+$ in HII regions
Ekers, R.	Occult. of 3C2 by Sun	Predmore, C.	$^3He^+$ in HII regions
Friedman, H.	Pulsar-type radiation	Halpain, J.	$^3He^+$ in HII regions
Sadah, D.	Pulsar-type radiation	Wade, C.	Nuclei of spiral galaxies
Tlamicha, A.	High res.map of Sun	Huguenin, G. R.	Pulsars
Turner, B.	Pol. of OH line	Shapiro, I.	VLB; relativistic bending
Lilley, A. E.	Carbon recomb; OH	Hinteregger, H.	VLB; relativistic bending
Penfield, H.	Carbon recomb; OH	Whitney, A.	VLB; relativistic bending
Penzias, A.	Intergalactic HI abs.	Carr, T.	Lunar occult.of Jupiter
Meeks, M.	HI with lunar occult.	Basart, J.	Lunar occult.of Jupiter

* *From a handout compiled for the Symposium*

Rydbeck, O.	VLB; OH	Simpson, J.	H and He recomb lines
Hansson, B.	VLB; OH	Sullivan, W.	HI towards ring of stars
Ellder, J.	VLB; continuum	Gordon, C.	HI abs. towards pulsars
Verschuur, G.	Zeeman of 21-cm line	Rubin, R.	Cont. of planetary nebulae
Staelin, D.	OH emission	Felli, M.	Survey of Rosette nebula
Wilson, W.	OH in infrared stars	Churchwell, E.	Survey of Rosette nebula
Webster, W.	HII region fluxes		

1969

Knight, C.	VLB; OH	Rosenkrantz, P.	Line search in OH emitters
Harten, R.	Galactic HI	Knapp, G. R.	HI of Kapteyn areas
Helfer, H.	Galactic HI	McCarthy, D.	Pulsar search in DQ Her.
Perry, J.	Galactic HI	Lockman, J.	Recom lines in Sgr A
Balick, B.	Recom. lines, plan. neb.	Gordon, M.	Recom lines in HII regions
Klein, M.	Continuum of Mercury	Thaddeus, P.	H_2CO; Galactic Center
Howard, W.	Line search (20-25 GHz)	Solomon, P.	H_2CO; Galactic Center
Hvatum, H.	Line search (20-25 GHz)	Matveyenko, L.	Continuum of Crab Nebula
Turner, K.	H_2O in M31	Moiseyev, I.	VLB
Waters, J.	Lines in OH emitters	Knowles, S.	VLB; water-vapor
Schwartz, P.	Lines in OH emitters	Papadoupolous, G.	VLB; water-vapor

(L to R) Gart Westerhout, Jamie Sheets, Wally Oref at the 140 Foot 30th Birthday Celebration, September 1995.

Green Bank Directors through the ages

Most of the Green Bank Site Directors, in 1995 at the 140 Foot 30th Birthday Celebration: (L to R)Dave Heeschen, Mort Roberts, Dave Hogg, Bill Howard, Ken Kellermann, Bob Brown, Martha Haynes, Rick Fisher, George Seielstad, Jay Lockman.

Directors of NRAO

(Lloyd Berkner, Acting)	1956-1959
Otto Struve	1959-1961
Dave Heeschen	1961-1978
Mort Roberts	1978-1985
Paul Vanden Bout	1985-2002
Fred Lo	2002-2012
Tony Beasley	2012- —

In 1965, NRAO headquarters moved to Charlottesville, VA. After that, there was an Assistant director for Green Bank operations.

Green Bank Site Directors

John Findlay	1966-1969	Martha Haynes	1981-1983
Mort Roberts	1969-1970	George Seielstad	1984-1992
Dave Hogg	1970-1974	Dave Hogg	1992-1993
Bill Howard	1974-1976	Jay Lockman	1993-1999
Ken Kellermann	1977	Phil Jewell	1999-2005
Bob Brown	1977-1980	Richard Prestage	2006-2008
Rick Fisher	1980-1981	Karen O'Neil	2008- —

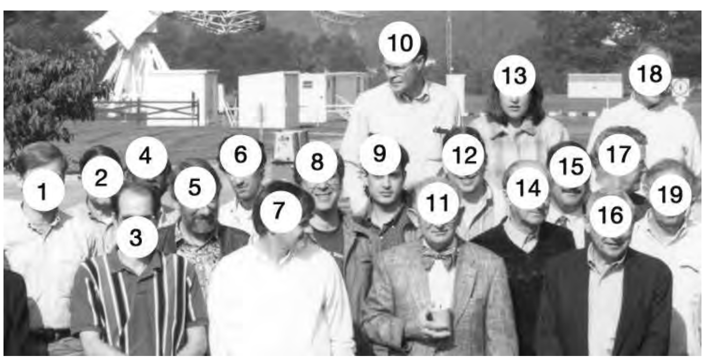
140 Foot Birthday Symposium Attendees – September 1995

1 Juan Uson
2 Dave Wilkinson
3 Tony Zensus
4 Fred Crews
5 Mark Gordon
6 Frank Kerr
7 Bill Howard
8 Frank Lovas
9 Dave Shaffer
10 Richard Hills
11 Joe Taylor
12 William Saslaw
13 Frank Ghigo

Symposium Attendees (continued)

14 Bob Rood
15 Pat Palmer
16 Jerry Nelson
17 Fred Schwab
18 Glen Langston
19 Don Wells
20 George Swenson
21 Jim Condon
22 Ed Murphy
23 Bob Dickman
24 Paul Vanden Bout
25 Robert Pound
26 Richard Simon
27 David Nice
28 Rich Bradley
29 Martha Haynes
30 Bernard Burke

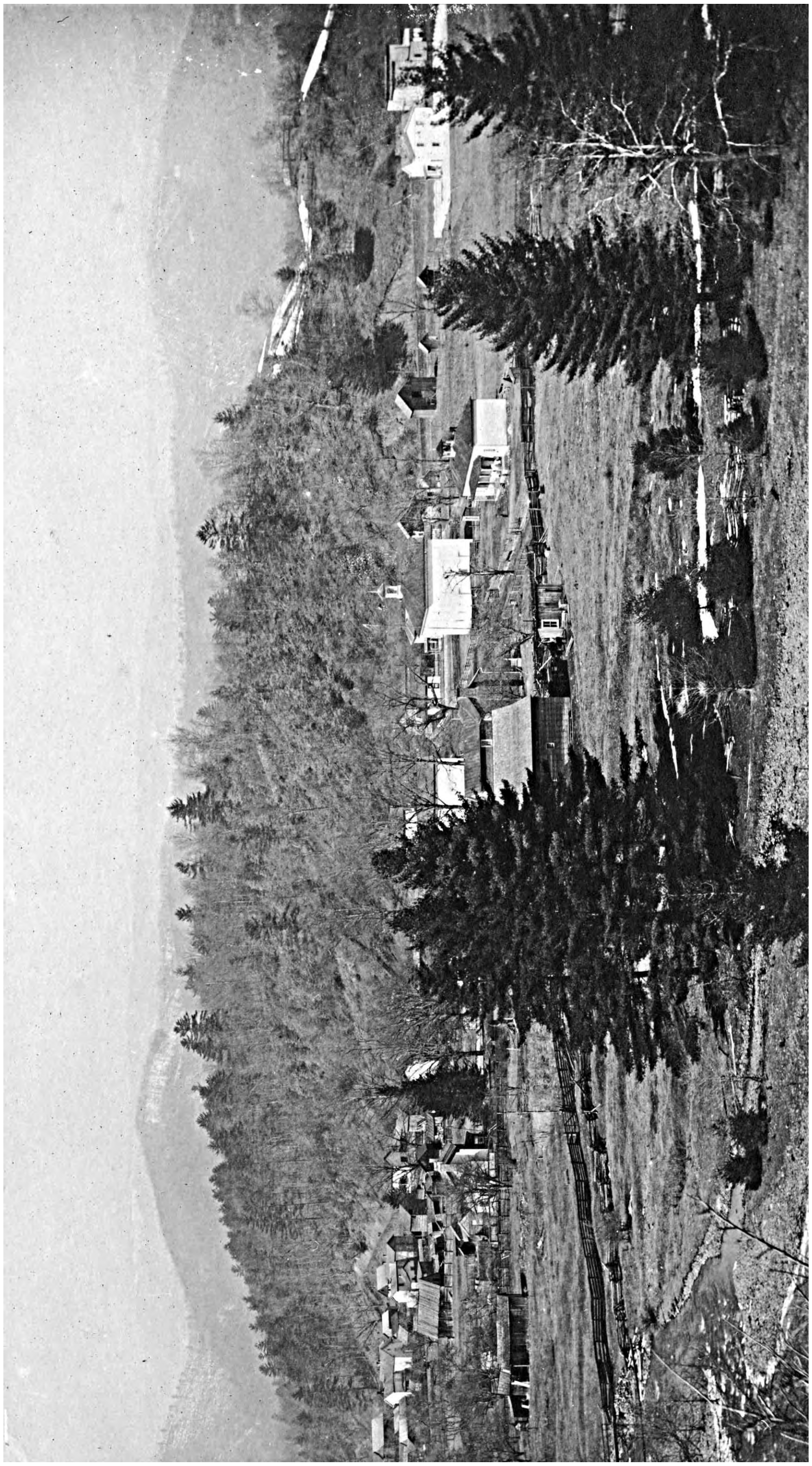
Symposium Attendees (continued)



1 Jay Lockman
2 Dave Hogg
3 Steve White
4 V. Radhakrishnan
5 Jaap Baars
6 Dana Balser
7 Seth Shostak
8 Ron Maddalena
9 Ed Wollack
10 Bob Wilson
11 Gart Westerhout
12 Toney Minter
13 Rachel Dewey
14 Sebastian von Hoerner
15 Tom Bania
16 Mort Roberts
17 Harry van der Laan
18 George Seielstad
19 Ken Kellermann

Symposium Attendees (continued)

20	Ed Churchwell	26	Phil Solomon	31	Gerrit Verschuur
21	Marshall Cohen	27	Bob Hughes	32	Mikul Kundu
22	Jim Moran	28	Barry Turner	33	Mike Balister
23	Tom Wilson	29	Peter Mezger	34	Lee King
24	Lew Snyder	30	Bob Payne	35	Bob Vance
25	Hugh Van Horn				



Downtown Green Bank, circa 1900, looking west. The Methodist Church and IOOF Hall are right of center.

[from a postcard, courtesy Robert A. Sheets of Green Bank.]

Part IV

A Valley Full of Telescopes

The NRAO has been blessed with an overabundance of interesting people.
. . . Bill Meredith (p.494)

The large amount of publicity regarding project OZMA has continued unabated and has caused us some embarrasment.
. . . Otto Struve (p.481)

For Grote, there was always an experiment waiting to be done, even in idle moments.
. . . S. A. Heatherly (p.502)

It is proposed that development of millimeter wave facilities be commenced following roughly this path . . . construction of a 10 foot antenna for use at the Jansky Lab.
. . . Frank Drake (p.507)

This is the story of how I carried the time to a radio telescope in the Crimea.
. . . Barry Clark (p.563)

At the telegraph office [in the Crimea] . . . it took a while to explain that Green Bank was not a major US city, and the telegram went off, or so we thought. Four days later the telegraph office called me at the hotel. They still wanted to know where Green Bank was.
. . . Ken Kellermann (p.548)

Joint USA-USSR Workshop participants at Green Bank, May 1961.

(Left to Right)

Front Row (seated): Getmantsev, Haddock, M. J. Wade, Edmundsen (interpreter), Minkowski, Vitkevitch, Struve, Sorochenko, Firor, Keller, Kuzmin, Bracewell, Drake.

Middle Row: C. Wade, McClain, Sanamyan, Kalachev, Stanley, Barrett, Weaver, Swenson, Mayer, Heeschen, Kraus.

Back Row: Field, Menon, Seeger, Woltjer, Sandage, Lilley, Blaauw, Kahn, Burke.

1 The First Green Bank Workshop

May 15 – 19, 1961

Editors' note: The Green Bank Observatory is an ideal venue for small scientific meetings. The isolation and freedom from distractions at the site, the great natural beauty of the surroundings, and the limited housing— which restricts the number of participants to about 50—combine to produce meetings of unusual focus and accomplishment.

The first Green Bank Workshop was a joint USA-USSR Symposium held in 1961 at a time when scientific contacts between the two nations were not common. The report below is excerpted from the NRAO Monthly Activities Summary for July, 1961.

Report on the Joint USA-USSR Radio Astronomy Symposium held at Washington and Green Bank, May 15-19, 1961

I. A joint USA-USSR Radio Astronomy Symposium arranged under Article 7 of the exchange agreement between the Academies of Sciences of the USA and USSR was held at Washington and Green Bank, May 15 through May 19, 1961. The host institution was the National Radio Astronomy Observatory. The Opening Session was held at the National Academy of Sciences, Washington, on the afternoon of May 15, when welcoming and introductory addresses were given. Technical sessions were held at the National Radio Astronomy Observatory, May 16–19.

II. Participants in the symposium were as follows:

USSR

G. G. Getmantsev, Radiophysical Institute of Gorky University
P. D. Kalachev, Lebedev Physical Institute
A. D. Kuzmin, Lebedev Physical Institute
V. A. Sanamyan, Byurakan Astrophysical Observatory
R. L. Sorochenko, Lebedev Physical Institute
V. V. Vitkevich, Lebedev Physical Institute

USA

A. H. Barrett, University of Michigan
A. Blaauw, Vanderbilt University
R. N. Bracewell, Stanford University
G. R. Burbidge, Yerkes Observatory
B. F. Burke, Dept. of Terrestrial Magnetism, Carnegie Institute of Washington
F. D. Drake, National Radio Astronomy Observatory
G. B. Field, Princeton University
J. W. Findlay, National Radio Astronomy Observatory
J. W. Firor, Dept. of Terrestrial Magnetism, Carnegie Institute of Washington
F. T. Haddock, University of Michigan

D. S. Heeschen, National Radio Astronomy Observatory
F. D. Kahn, National Radio Astronomy Observatory
G. Keller, National Science Foundation
J. D. Kraus, Ohio State University
A. E. Lilley, Harvard University
C. R. Lynds, National Radio Astronomy Observatory
C. H. Mayer, Naval Research Laboratory
E. F. McClain, Naval Research Laboratory
T. K. Menon, National Radio Astronomy Observatory
R. Minkowski, University of Wisconsin
A. R. Sandage, Mount Wilson and Palomar Observatories
C. L. Seeger, Stanford University
G. J. Stanley, California Institute of Technology
O. Struve, National Radio Astronomy Observatory
G. W. Swenson, University of Illinois
C. M. Wade, National Radio Astronomy Observatory
H. F. Weaver, University of California
L. Woltjer, Institute for Advanced Study

III. The Symposium Program

The Symposium Program was opened by Detlev W. Bronk, President of the U.S. Academy of Sciences, who welcomed the delegates, and by V. V. Vitkevich, who brought greetings from the Astronomical council of the Academy of Sciences of the Soviet Union. V. A. Sanamyan presented greetings from Academician Ambartsumian, and O. Struve welcomed the delegates to the National Radio Astronomy Observatory. Introductory papers reviewing the history, present status, and the course of radio astronomy in the United States and in the Soviet Union were then presented by F. D. Drake and V. V. Vitkevich.

Below is given a list of [some of] the technical papers presented at the symposium, with a brief description of the subject matter of each.

B. F. Burke, J. W. Firor, and M. A. Tuve: Hydrogen Line Studies. A report of cloud structure of interstellar hydrogen at high Galactic latitudes, as found with a multi-channel radiometer.

A. Blaauw: 21 cm Evidence on Interstellar Cloud Structure. The results of attempts to portray 21 cm profiles as sums of gaussian components.

C. L. Seeger: 408 mc Observations of the Polarized Component of the Nonthermal Galactic Background Radiation. Excellent evidence for the existence of polarization in the Galactic background radiation, with a preliminary map of the orientation and percentage polarization found.

A. D. Kuzmin and A. E. Salomonovich: Radio Emission of Taurus A at a Wavelength of 8 mm. Observations of Taurus A at high resolution which give evidence for a second source very close to the Taurus A source.

R. L. Sorochenko: Preliminary Results of Observations at 21 cm of the Milky Way Region with the Center R.A. $= 20^h18^m$ and Dec. $= +42°30'$. 21 cm profiles showing absorption features connected with the source Cygnus X, and indicating its location in the Milky Way.

C. H. Mayer, T. P. McCullough, R. M. Sloanaker: Radio Source Polarization Measurements. Polarization measurements of six sources, of which only Taurus A presently gives clear cut evidence of polarization.

A. H. Barrett: Observations of Radio Sources at 1.8 cm Wavelength. Extension of the spectra of several radio sources to 1.8 cm wavelength.

A. D. Kuzmin: Spectra of Radio Sources Observed with the 22-meter Radio Telescope of the Lebedev Physical Institute. A critical comparison of spectra obtained with the 22-meter telescope with older spectral data.

T. K. Menon: The Density Distribution in Emission Nebulae. The density distribution in the Orion Nebula and NGC 2244, as found from radio observations.

F. D. Kahn: Dynamics of HII Regions. A critical discussion of the observational data showing there are violent contradictions in theory which may be resolved only by making extreme assumptions regarding the evolutionary stage of the subject HII regions.

Y. N. Parijsky: On the Connection between Hydrogen Line Radiation and Radio Emission of Gaseous Nebulae. A means of determining the distance to emission nebulae from optical and radio observations, with the results of the application of the method to 32 nebulae.

A. D. Kuzmin, R. I. Noskova: Identification of Exciting Stars and Determination of Emission Nebulae Parameters on the Basis of Radio Astronomical Data. A proposal of a new method of identifying exciting stars and measuring nebula parameters using radio observations and optical spectral data only. Application to many HII regions gives very encouraging results.

A. D. Kuzmin: Concerning the Discrete Source of Radiation at R.A. $18^h53^m.7$, Dec $= +1^\circ\ 16'$. Data from the 22-meter telescope is given which indicates that the source is a non-thermal source similar to IC443.

A. E. Lilley: Results of the Harvard Hydrogen Line Maser Program. Both negative and positive results of a search for 21 cm line and continuum radiation from galaxies is given.

C. M. Wade: The Structure of Some Bright Radio Galaxies. The double emission regions in Fornax A, Centaurus A, and Cygnus A are emphasized as significant features of intrinsically bright radio sources, and the complex low intensity structure of M87 described.

G. J. Stanley: Measurements of Radio Source Diameters at the Owens Valley Observatory. A summary of the statistical data on source brightness distributions found at Owens Valley, which show that a majority of the intrinsically bright radio sources are probably double.

D. S. Heeschen: The Spectra of Some Extragalactic Radio Sources. A summary of the extensive observations of source spectra, which show that the normal galaxies and abnormal radio galaxies fall respectively on two well defined sequences in a color magnitude diagram, and that some of the bright sources have distinct curvatures in their spectra.

A. G. Little, D. D. Cudaback, and R. N. Bracewell: Structure of the Central Component of Centaurus A. The central component of Centaurus A is shown to consist of two components of unequal strength, in two possible orientations.

L. Woltjer: Some Remarks on Supernova Remnants: It is suggested that the central star in the Crab Nebula is an extraordinary object which emits much of its energy in the form of energetic particles. The source 3C48 may be an example of an older version of the same object.

G. M. Tovmastan [sic; Tovmassian?], R. K. Shachbazyan: On the Equality of Cosmic Sources. A statistical analysis of a number of source catalogs was made which indicated that radio sources tend to occur in clusters of galaxies, and they tend to be connected with double galaxies.

R. Minkowski: Radio Sources, Galaxies, and Clusters of Galaxies. A summary of the most recent results obtained from the optical identification of radio sources.

A. R. Sandage: On the Possibility to Choose Between Cosmological World Models from Observations of Distant Radio Clusters of Galaxies. An analysis of the quality of observations needed to distinguish between different cosmological world models.

V. L. Ginzburg: On the Nature of Radio Galaxies. It is shown that large numbers of relativistic electrons could be produced in the early stages of the evolution of a Galaxy when high material velocities exist.

G. B. Field: On Shklovsky's Early Picture of the Cygnus A Source. A discrepancy between the positions of the stellar and halo components of the source is shown, which seems to militate strongly against this early picture.

G. R. Burbidge: Recent Ideas Concerning the Origin of the Strong Extragalactic Radio Sources. A suggestion by Hoyle that radio galaxies are the result of an enormous flare phenomenon in the central regions of galaxies is discussed. It is shown that this demands extreme parameters of the radio galaxies. A new idea is presented that radio galaxies are the result of a chain reaction of super-novae in the Galactic nucleus.

V. A. Razin: On the Spectra of Nonthermal Radio Emission of the Galaxy and Metagalaxy. It is shown that if the exchange of relativistic particles between the Galactic plane and halo is impeded, the spectrum of radio emission from the halo should be steeper than the radiation from the flat subsystem. Extragalactic radiation should have a still steeper spectrum.

G. G. Getmantsev: On the Origin of Non-thermal Galactic Radio Emission and of Primary Cosmic Rays. Arguments are given in favor of the idea that the relativistic particles giving rise to halo emission originate in the halo during the early history of a galaxy. Supernovae are probably a source of relativistic electrons only for the flattened subsystem. The exchange of cosmic rays between the halo and flat subsystem is almost absent.

C. R. Lynds: Application of the Image Orthicon to Astronomical Problems. Observations made with an image orthicon on the 36" Kitt Peak telescope were presented, and the advantages (high quantum efficiency) and the limitations (image distortion) of the image orthicon described.

J. D. Kraus: Design Considerations and Preliminary Performance Data for the Ohio State University 360-foot Radio Telescope. A description of the 360-foot telescope, accompanied by a theoretical analysis of various aspects of telescope performance, and actual measurements which are closely consistent with the theoretical analysis.

G. W. Swenson, Jr.: The University of Illinois Radio Telescope. A description of the design of the telescope, accompanied by a progress report on the construction of the instrument.

J. W. Findlay: The NRAO 300-foot Transit Telescope. A description of the design of the telescope, and demonstration of a model of it.

V. A. Sanamyan: On the Work of the Large Interference Radio Telescope at the Byurakan Observatory. A detailed description of the seven element interferometer at the Byurakan Observatory, with examples of the high quality of observation possible with it.

G. J. Stanley: The Owens Valley Interferometer and its Applications to Problems of Aperture Synthesis. A resume of experience gained from the Owens Valley Interferometer, with some suggestions as to how to extend the instrument so as to increase its observing capabilities.

V. V. Vitkevich and P. D. Kalachev: The Large Cross Radio Telescope of the Lebedev Physical Institute. Possible Ways to Construct Large Radio Telescopes. A description of the design of the large cross telescope, and the progress made in its construction. Discussion of several novel proposals for the construction of very large reflector type telescopes.

R. N. Bracewell: Guiding Principles Leading to the Design of a Radio Telescope with a One Minute Beam. A discussion of the philosophy that should guide future designs for large telescopes, with emphasis on resolution limiting and the advantage of building an instrument which can be expanded. A design which meets the criteria, using a number of cylindrical parabolas, was presented.

C. L. Seeger: On the Design of the Giant Benelux Radio Telescope. A resume of the problems encountered in the design of very large cross type telescopes, and a discussion of various approaches which might solve these problems.

F. D. Drake: Some Economical Designs for High-Performance Radio Telescopes. A description of several versions of a telescope which combines reflecting elements with the Mills Cross principle to give high performance at low cost.

Informal Discussion. Because of the limited number of papers on the program, and the small number of participants, it was possible to devote much time fruitfully to informal discussion. To aid in this, informal study groups were organized outside the formal symposium program to discuss the following special topics:

Source Polarization Studies; 2l-cm Line Studies; Discrete Sources; Design Parameters for Large Telescopes.

IV. Social and Sightseeing Activities.

The following social and sightseeing activities were arranged for the symposium:

1) A symposium dinner, attended by all delegates.
2) Dinner parties in the homes of NRAO staff members for members of the Soviet delegation.
3) A visit to some nearby caverns by a portion of the American and Soviet delegations.
4) A return trip to Washington by automobile for the Soviet delegation, via a route which included the Shenandoah Valley and the Skyline Drive.
5) Sightseeing in Washington, including a visit to the White House and most other points of interest.

V. Other Scientific Visits made by the Soviet Delegation.

Arrangements were made for the Soviet Delegation to visit the Department of Terrestrial Magnetism of the Carnegie Institute of Washington, and the Radio Observatory of the University of Michigan. The Soviet delegation was very pleased that it was possible, on short notice, to make these visits.

VI. Suggestions for future symposia.

As a result of the experience gained from this symposium, we would like to make the following suggestions regarding future Symposia:

1) An interpreter was provided at this conference. This turned out to be very useful, and contributed very significantly to the success of the venture. There is a severe language problem with most delegates, making an interpreter extremely desirable at future symposia.

2) Hotel accommodations provided the Soviet delegates in Washington were below average in quality, and did not reflect well on the U.S. or American hospitality. Better accommodations should be provided in the future.

3) Every effort should be made to determine well in advance of the symposium the places, other than the symposium location, the Soviet delegation wishes to visit while in America. Since many places are closed to Soviet citizens, and arrangements to visit the permitted places take time to make, the Soviet desires need to be known well in advance so that side-trips can be arranged, where possible, in an efficient way.

VII. Evaluation of the Effectiveness of the Symposium.

It is felt that the technical program was well balanced and stimulating, and that its scope and length were very nearly optimum. In the discussions held in connection with the formal program and outside it, there was a free and enthusiastic flow of information between the two delegations. Because of this, major steps were made in gaining a knowledge of the progress made in radio astronomy in the two countries, and the problems now considered important in each. Since the attainment of such mutual understanding was a primary goal of the conference, it is felt that the symposium was highly successful.

At the request of the Director of the National Radio Astronomy Observatory, the undersigned organized the Symposium and prepared this report.

F. D. Drake
Associate Astronomer
July 18, 1961

Past Green Bank Workshops have also included:

Extraterrestrial Life (1961)
High Velocity Clouds (1971)
Phases of the Interstellar Medium (1981)
Interference Identification and Excision (1982)
Low Frequency Variability of Extragalactic Radio Sources (1982)
Low Frequency Radio Astronomy (1984)
Physics of Energy Transport in Extragalactic Radio Sources (1984)
Gaseous Halos of Galaxies (1985)
Continuum Radio Processes in Clusters of Galaxies (1986)
Radio Astronomy from Space (1986)
The Radio Source Cygnus A (1995)
Warm Ionized Gas in Galaxies (1999)
High Frequency Science with the GBT (2003)
Science Writers Symposium (1968)
Extended Extragalactic Radio Sources (1972)
Extragalactic Molecules (1981)
The Comparative HI Content of Normal Galaxies (1982)
Serendipitous Discoveries in Radio Astronomy (1983)
Millisecond Pulsars (1984)
The Search for Extraterrestrial Intellegence (1985)
Science with a Millimeter Array (1985)
Cometary Radio Astronomy (1986)
Large Scale Surveys of the Sky (1987)
Highly Redshifted Radio Lines (1997)
Science with the Green Bank Telescope (1998)
HI Surveys of the Milky Way (2001)
Sgr A* at 30 (2004)

Logo of the Green Bank Workshop on Millisecond Pulsars, organized in 1984 by S. P. Reynolds and D. R. Stinebring at a time when there were only two objects of this type. Their periods correspond to the frequencies of musical notes illustrated.

Frank Drake at the 85 Foot Howard E. Tatel Telescope during a visit to Green Bank for the Jansky Lectureship Award, October 1999.
[Courtesy S. A. Heatherly]

2 Project Ozma

Editors' note: Possibly the best-known experiment ever done at Green Bank, and one of the most well-known experiments in all of radio astronomy, was the first search for radio signals from intelligent beings elsewhere in the Universe. Dubbed "Project Ozma" by Frank Drake, it was carried out in 1960, while Drake was a member of the NRAO scientific staff at Green Bank.

It attracted immediate attention and controversy. Contemporary reports by Drake are in Sky and Telescope[1] *and* Physics Today[2], *written before and after the experiment, respectively. Drake gives retrospective accounts in* The Search for Extraterrestrial Intelligence[3] *and in* Cosmic Search[4].

Drake says that he intentionally kept the project at a low profile in order to avoid potential criticism. Word of the impending experiment reached the general public through an announcement by Otto Struve during a lecture at MIT in November 1959. Struve's comments were reported nationally and caused a flood of publicity for the NRAO. The NRAO Monthly Summary of Activities for November 1959 reports:

> Dr. Drake's projected search for coherent signals from life on other planets has received much recent publicity. Dr. Drake has been planning this program for several years, and work on the equipment—which will also be used for another, more conventional, program—was begun last spring. The recent publicity has gotten somewhat out of hand. The odds against success in this initial attempts are obviously extremely high. However, it is well worth trying with presently available equipment.

One month later in the NRAO Monthly Summary of Activities for December 1959, Struve wrote the first of what was to be a series of "Comments by the Director." It was devoted in large part to Project Ozma.

selections from
Comments by the Director
January 8, 1960

The large amount of publicity regarding project OZMA (already mentioned in the November report) has continued unabated and has caused us some embarrassment. The responsibility for this publicity is entirely mine. I had received an advance copy of the article on some related speculations by Cocconi and Morrison and I learned, while at MIT [to deliver the K. T. Compton lectures on "The Universe"], that their paper had been published in Nature[5]. I also heard from Dr. E. Purcell that he had been thinking along similar lines. Finally, my own research in astrophysics had led me, long ago, to the conclusion that the distribution of angular momenta of stars of different kinds renders it virtually certain that planets are the rule, rather than the exception, in the Milky Way. I therefore considered it appropriate to mention briefly the project OZMA in one of my MIT lectures. It was an integral part of the rest of the lecture on stellar rotations and it also served

Receiver and dual feed at the focal point of the 85 Foot Tatel Telescope during Project Ozma, 1960.

the purpose of establishing that OZMA was not the outcome of the ideas of other scientists, but had been independently conceived at the NRAO. . . .

I believe that it would have been wrong to maintain secrecy and impossible (in the absence of security regulations) to explain it, in view of the fact that the experiment is actually being planned. Whether the experiment itself is worthwhile can perhaps best be answered by calling attention to the sad fate of the ancient Chinese astronomers, Hi and Ho, when they were not alert to the challenge of their time.* My own attitude is expressed in a letter to the editor of the New York Times, dated December 1, 1959, of which I am enclosing a copy [see page 483].

Editors' note: In his article in The Search for Extraterrestrial Intelligence[3]*, Drake recounts that Struve's announcement "created problems for us because then the press was all over us." Struve returned to the topic publicly in an article in Physics Today*[6]*:*

> Although it is poor taste to discuss marginal problems by others, I need not have any inhibitions in talking about project Ozma, for which I, as the director of the National Radio Astronomy Observatory, carry administrative responsibility. This project has been given an unreasonable amount of publicity, often incorrect or distorted and always with the wrong emphasis. It has aroused more vitriolic criticisms and more laudatory comments than any other recent astronomical venture, and it has divided the astronomers into two camps: those who are all for it and those who regard it as the worst evil of our generation. There are those

* *Editors' note: This refers to two astronomers of Chinese legend who, through neglect, failed to predict a solar eclipse and were punished by decapitation.*

To the Editor of The New York Times:

published December 6, 1959, page E10.

Reprinted from the NRAO Monthly Summary for December 1959.

In connection with William L. Laurence's interesting article in The Times of Nov. 22 entitled "Radio Astronomers Listen for Signs of Life in Distant Solar Systems," I should like to make the following comments:

Astronomers are concerned with three distinct problems: Are there stars in the Milky Way and in other galaxies which possess families of planets? If such distant planets exist, are there any that support some form of life? If life does exist outside the solar system, what is the probability that intelligent forms of living beings are now in existence?

The answer to the first is decidedly yes: although no planet outside the solar system has ever been seen, photographed, or recorded by any other direct instrumental means, there are indirect proofs of the existence of many billions of planetlike bodies in the Milky Way alone.

"Habitable Zones"

The answer to the second is "probably yes." Everything we know about the origin of life on the earth suggests that life must occur on all planets whose "habitable zones" are similar to that of the Sun and whose central stars have "life expectancies" of at least several billion years.

The third answer is uncertain. Although it seems reasonable to believe that among the billions of distant planets there are some on which an advanced form of life is now in existence, we have as yet no basis for estimating its probability.

With regard to Project OZMA at the National Radio Astronomy Observatory, I believe that at the present time the probability of recording artificial radio signals from distances of the order of ten or twenty light years is exceedingly small. But it is not zero, and the experiment must be performed. Only after there are more powerful radio telescopes than those now available for this work will the volume of accessible space be sufficiently large to give us a choice of tens of thousands of stars that are likely to have planets in their habitable zones.

We therefore do not expect quick results; in the meantime the instrumentation that is being developed by Drs. Drake, Findlay, Heeschen and others at Green Bank will be useful in many other radio-astronomical investigations.

Observable Phenomena

I mentioned the Project OZMA in my Karl T. Compton lectures at the Massachusetts Institute of Technology in November because it illustrates what I regard as the beginning stages of a great revolution in scientific thought. Since we believe that we must say "no" to the question: "Are we alone in the universe?" we must also incorporate within our scientific theories the inevitable conclusion that not only biogenic effects upon dead matter, but also the free will of thinking, living beings can produce observable phenomena in the cosmos.

Man-made radio waves did not exist a generation ago. Those now in existence could be easily detected from distances of ten to twenty light years (after a light-time interval of ten to twenty years). We can no longer take for granted that all observable phenomena are ruled exclusively by what we call the "laws of nature."

Even if we should never detect artificial radio signals from outside the solar system (because they would require someone's will to communicate), there may well be other observable phenomena on distant worlds that cannot be explained except through the combined action of "free will" and of the classical laws of physics. No such phenomena are known at the present time, and the idea I am trying to convey is at this time of philosophical significance only.

Otto Struve
Green Bank, W. Va.
Dec. 1, 1959

Electronic equipment used during Project Ozma, 1960.

who pity us for the publicity we have received and those who accuse us of having invented the project for the sake of publicity.

Let me state at once that as far as the publicity is concerned, I alone am responsible. I mentioned the project, rather casually, in one of my Karl Taylor Compton Lectures last November at the Massachusetts Institute of Technology. I assumed then, and I believe now, that the American taxpayer has a right to know what is being done with his money, provided there are no security aspects. The question is not whether I am right or wrong, but whether the experiment is good or bad. I can only describe as shameful any thought that we ourselves are seeking publicity: in forty years of astronomical research I have almost never been pleased with publicity which I have received, and in nine cases out of ten I have felt embarrassed by it.

Struve concludes his article with a list of reasons why he supported the experiment at Green Bank even though "the probability of observing radio signals from intelligent beings on one of the two or three solar-type stars at distances of the order of ten light years is almost zero." The final justification he gives is that "There is every reason to believe that the Ozma experiment will ultimately yield positive results when the accessible sample of solar-type stars is sufficiently large."

Preparations for Project Ozma, its progress, and the final results were described in the brief notices in the NRAO Monthly Summary of Activities:

February 8, 1960
The Zeeman/Ozma Receiver.

Mr. R. W. Meadows, of the Radio Research Station, Slough, England, left the Observatory on January 20th after a three months stay. During this time, in addition to joining in a variety of Observatory activities, he continued the development of the receiver which is planned for use in the Zeeman experiment (Dr. Menon) and in the Ozma experiment. This receiver, except for the front end amplifier, is now completed and ready for testing.

The receiver is intended to fulfill a twofold function. It will be used to attempt to detect the Zeeman splitting of the absorption line of neutral hydrogen. With some changes in the feed system, and in the filter system, the receiver will be used for the Ozma experiment.... When the receiver is used for the Ozma experiment, the front end is switched between two horns—in one of which will be the signal from the source and in the other of which will be signals from the adjacent sky background.

May 6, 1960

During April the 85 Foot Telescope was used for the Ozma and Zeeman experiments. The Ozma experiment has had negative results thus far. It may continue for a few weeks, but will then be discontinued for an indefinite period.

June 28, 1960

Observations connected with the Ozma Project have been made daily for several weeks, and it is anticipated that the first phase of the Ozma Project will be completed about July 1. As a by-product of this program, it has been found possible to voltage-tune the parametric amplifier being used, greatly facilitating the delicate adjustment of the device....

May 8, 1961
by F. D. Drake

The failure to detect any intelligent signals from the stars Tau Ceti and Epsilon Eridani during the search made in 1960 has confirmed the opinion that the detection of such signals will require very expensive facilities and lengthy observation. von Hoerner has carefully redone the theoretical considerations which give some guidance as to the distances to which such a search must penetrate, and the number of stars which may need examination to insure a relatively high probability of success.

A restudy of the technology available to us now including the capabilities for communication at optical wavelengths given by lasers, indicates that the optimum frequencies for interstellar communication are near the short centimeter wavelengths. This analysis shows that we are now capable of producing an instrument giving a high probability of success in a search, but that the instrument would be extremely expensive (several millions of dollars) and the search still very long (of the order of 30 years).

Reunion at Green Bank in 1985 of people involved in Project Ozma. Front (left to right): Bob Viers, Dewey Ross, Bill Meredith, Troy Henderson, Bob Uphoff. Back: George Grove, Fred Crews, Omar Bowyer, Frank Drake, Kochu Menon.

The conclusion is that inexpensive approaches to this problem can only succeed through great good luck; to make a search with a reasonable probability of success will require a massive effort. It may be some time before the scientific profession has the personnel resources to support such an effort, and the public at large sufficient sophistication to provide the required material resources.

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[2] Drake, F. "Project Ozma," Physics Today, **14,** No. 4, p.40, April 1961.
[3] Kellermann, K.I., and Seielstad, G.A. (eds) "The search for extraterrestrial intelligence: proceedings of an NRAO Workshop, May 20-22, 1985." NRAO Workshop No. 11, 1986.
[4] Drake, F.D., "A Reminiscence of Project Ozma," Cosmic Search Magazine Vol.1,No.1,Jan.1979.
[5] Cocconi, G., and Morrison, P. "Searching for Interstellar Communication," Nature,**184,**844(1959).
[6] Struve, O., "Astronomers in Turmoil," Physics Today, **13,** No.9, p.18, September 1961.

Jack Locke visits Green Bank from Ottawa, 1957.

In March of 1957 two Canadian scientists, Jack Locke (upper photo) and Ed Argyle, made a field trip from Ottawa to Green Bank to measure radio intensities with the antenna shown in the lower photo. The photos are taken from Rt 28 at the approximate location of what is today the ball field next to the Green Bank Library. They then proceeded from Green Bank to Owens Valley, CA, then to several other locations in British Columbia before returning to Ottawa, all in the same Travellall shown here. This trip led to the selection of Penticton, BC as the site for the Canadian counterpart to Green Bank: the Dominion Radio Astronomy Observatory. Locke served as its first Officer-in-Charge.

(*Photos courtesy of Tim Robishaw and the Locke family.*)

Grote Reber in Green Bank, 1959.

3 Grote Reber in Green Bank

Editors' note: Grote Reber, the world's first intentional radio astronomer, has had a long involvement with the NRAO at Green Bank. He was in residence for a year in 1958-1959 as the first NRAO Visiting Scientist, during which time he helped in the reconstruction of his 31 foot telescope and worked on projects of his own devising, some of which are described below. He gave the first-ever colloquium at NRAO in January 1959, speaking on Cosmic Static at Kilometer Wavelengths. Reber has returned a number of times since then, notably in 1975 when he was the recipient of the Jansky Lectureship Award, in 1977 for filming of segments of "Grote Reber: the Wildcat Astronomer" by Australian TV, and in 1988, when he gave the talk on twining beans transcribed below.

In 1995, Reber donated all of the material from his original experiments in Wheaton Illinois to NRAO, including receivers, log books and his 1927 ham radio operator's license signed by Herbert Hoover, then Secretary of Commerce. He was resident in Green Bank for several months during the summers of 1995 and 1996 to unpack and organize the material, and also assisted at that time in an effort to repeat Jansky's original experiment, as described in the article by S. A. Heatherly. During this visit he inaugurated the auditorium in the new Jansky Lab addition with a colloquium called, "The Big Bang is Bunk."*

Reber was born in 1911 and built his radio telescope in 1937, inspired by Jansky's discovery of radio waves from the Milky Way. During his 1996 visit he was interested in organizing a very different project:

> "The proposed experiment has to do with radio astronomy at 520 kc. These low frequencies will not get down to ground because of high electron density of F layer of ionosphere. It is proposed to attach a container with 100 kilograms of liquid hydrogen to a rocket. Raise the container to 180 kilometers at midnight—dump the hydrogen—it will sweep up many of the free electrons. This experiment has never been done before, so it is not known what to expect."

He spent some time that summer trying to obtain a rocket from NASA or one surplus from the Department of Defense, since existing Minuteman Missiles would be adequate ("I'd like to secure ten for **free**"). *He was unsuccessful.*

Reber discusses his early work on radio astronomy in several papers listed at the end of this section. The articles which follow illustrate other aspects of his life and work.

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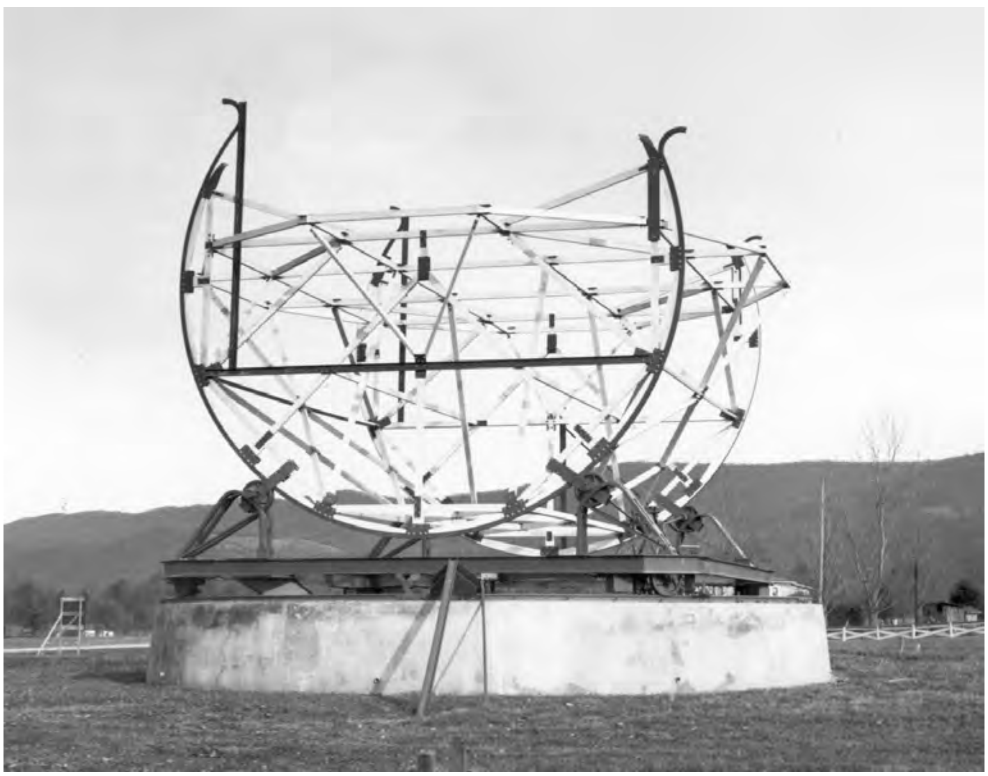
* Reber's instruments are now on display in the Green Bank Science Center and his papers are in the NRAO Archives (see http://www.nrao.edu/archives)

The Reber Dish *

M. T. Waslo
NRAO, Green Bank

The original dish built in the spring of 1937 by Grote Reber in his back yard at Wheaton, Illinois, consisted of the reflector or dish surface, four parapets (feed support legs), and their support structure on two large elevation arches positioned on four railroad wheels and friction driven. This provided motion along the North-Zenith-South meridian. The dish diameter was 31 feet 5 inches and had a focal length of 20 feet.

In 1947 the Reber Dish was acquired by the National Bureau of Standards and was moved to the Sterling, Virginia vicinity where it was erected on a turntable which provided the azimuth motion.



Reber dish being reconstructed in Green Bank, November 1959.

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* From an article in the *Observer*, January 1963.

Reber and his newly-reconstructed antenna, Summer 1960.

About 1952 the dish was dismantled at Sterling and parts sent to Boulder Colorado. Sometime in the 1950's the Reber Dish was loaned on an indefinite basis to this Observatory by the Bureau of Standards for exhibition purposes. In 1959 and 1960, under the supervision of Grote Reber, the [wooden] pieces of the 1937 Reber Dish were duplicated and with its 1947 turntable erected in its present position during the fall of 1960. The original pieces of wood ... are in storage on the Observatory grounds.

Reber, in *Serendipitous Discoveries (1983)* writes:

"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. . . . 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."

Reminiscing With (and About) Grote Reber*

Bill Meredith
NRAO

On November 19, 1975 the 10th annual Karl G. Jansky lecture was given by Grote Reber at the University of Virginia's Gilmer Hall. I enjoyed it immensely. Grote, as I hope most NRAO people know, was a pioneer in the field that keeps us all employed. Following Karl Jansky's initial discovery of extraterrestrial radio waves in the early 1930's, Grote constructed the "Reber Dish" in his backyard in Wheaton, Illinois and began making radio maps of the Universe.

It was a bit difficult to purchase "off the shelf" radiometers in those days so Grote had to design, develop, and manufacture all his own equipment. His presentation at the Jansky lecture was replete with slides of many of his original receivers. I think most people who attended were as fascinated as I with the slides and his discussion.

I met Grote in the fall of 1958 when I was first employed at the NRAO. Grote, Fred Crews, and I lived for several weeks at a lodge in Minnehaha Springs, WV, while the Hill House was being refurbished. Several contract engineers stayed there also. Fred, Grote, and I usually rode to Green Bank and back together, since everyone was on day shift then, there being no operational telescopes at NRAO at that time.

We all found Grote a delight to be around. There was scarcely a dull moment. Grote was a confirmed bachelor and somewhat set in his ways. He liked to retire early, never later than 9 PM, and would set the thermostat to 50 degrees just before going to bed. The lodge soon cooled down so the rest of us could freeze, go to bed ourselves, or turn the thermostat back up, which would arouse Grote when he became too warm. I can't remember which we did the most.

* From an article in the Observer, March 1976.

Reber with Patricia Smiley, preparing slides for his Jansky Lecture in Charlottesville, November 1975.

Another thing that disturbed Grote's slumber was light. The smallest amount shining under his door would bother him, so if we elected to freeze, we had to do it in the dark. Grote was somewhat hard of hearing, so with his hearing aid off, we could freeze in the dark, but make as much noise as we wanted.

Since Grote didn't have a car, Fred and I took turns driving ours. Grote was an inveterate back seat driver, so we never had to worry about forgetting to turn on our headlights or windshield wipers, or drive too fast. Grote always reminded us of these negligences, and we usually accepted it good-naturedly.

The Hill House became available to us after living for a few weeks in the lodge. We continued to cook for ourselves for awhile, until NRAO hired a cook for lunch and dinner. Grote bought some raw chestnuts and proceeded to roast them in the oven. As the moisture in the chestnuts became steam the pressure caused the nuts to literally explode. It sounded as if he had thrown a handful of Cal .22 cartridges into a fire. Grote frantically turned the oven off, and we all waited for the explosions to subside. After it had been quiet for several seconds, Grote opened the oven door and peeked inside. Just then the granddaddy of all chestnuts decided to explode. Chestnut meat went all over the kitchen and Grote nearly had a heart attack. Fred and I almost died laughing.

I chatted with Grote for maybe an hour during his recent visit here to deliver the Jansky lecture. He related some of his experiences in Tasmania, where he is currently living, and discussed his work there. I reminded him of a talk he told concerning himself. It seems Grote had delivered a colloquium to a group of radio engineers regarding his early experiences in radio astronomy. At the conclusion of his talk he asked if there were any questions. Apparently his lecture was over the heads of the audience. After an embarrassing silence, one individual in the back of the auditorium raised his hand and asked Grote what kind of tubes he used in his power supply. I told Grote I was going to ask him that same question after he finished the Jansky lecture, but I didn't have the nerve.

The NRAO has been blessed with an overabundant supply of interesting people. I think Grote Reber is the most interesting of them all.

Experiments with Twining Plants *

Grote Reber
Bothwell, Tasmania

This adventure started when I was in Hawaii and my neighbor, one Jerome Goodspeed, planted a row of pole beans—climbing beans. When they were about a foot high they began to send up runners.

I said, "Jerome, how long have you been growing these beans?"

He said, "Oh, 15, maybe 20 years."

"Well, in all that time have you never noticed any that turned around the pole in the other direction?"

And he looked at them and said, "You know, they do all turn the same direction, don't they."

So I deduced that Jerome was a poor observer. Then I told him: "Look, you have got a lot of beans, more than you could possibly eat. How about letting me do an experiment on these beans? I will take all the odd numbered plants, the first, third, fifth, and so on, and leave them alone, but all the even numbered ones I will arbitrarily unwind and wind backwards. I will keep on doing that till they grow to full height and we will see how the plant responds."

And he said, "Sure, no harm."

So I did it. The reverse vines seemed to grow just as tall—six, seven feet, maybe more—as the normal vines and they had luxurious foliage and prolific flowering and they had a lot of pods on them and I could not see that it made any difference whatsoever. I picked some of the pods from the normal vines and some from the reverse vines and cooked them separately to see if they were any more tough or fibrous or anything like that, and I could not see any difference whatsoever.

Well, I thought, that's whacky. The plant is being tortured. It must respond in some way and I just have not been able to figure it out. By that time I left

* Transcribed from an audio recording of a talk given at NRAO Green Bank on February 26, 1988.

Reber reversing the direction of climbing beans; Green Bank 1959.

Hawaii and came here [to Green Bank], and had Jerome give me some seeds—four cupfuls—and I brought them along. I was living at Hill House and I prevailed on one of the local farmers to dig up a plot (which is still open, I find) and I planted some beans, and also asked among the local people for other kinds of beans. I believe that I had a total of four different varieties. We planted about 30 poles worth. Again, I left the odd ones alone and tortured the even ones.

Now you have to stay at this thing. You can't let a normal vine grow up, and then unwind it all and wind it back the other way. After a few days the runner gets a "set" and becomes hard and if you try to bend it it will break. Only about the top six inches is pliable. So you have to go out every couple of days and unwind it, and wind it backwards and tie it. The plant, though, is very stubborn and new runners will continue on the original turning. After it has made a turn or two you go out and unwind it, and wind it backwards, and tie it again. You keep doing that until it gets to the very top.

The same thing happened in Green Bank as in Hawaii. You could not see any difference between the regular and the reversed vines, at least by inspection of the sizes of leaves or number of pods or the color of the flowers, or anything like that. I tried this on four different brands. One of them was white and the others had markings on them.

So instead of picking the beans, I let them mature and dry. And that seemed to be the crucial point. When you are dealing with the green beans there is an unknown amount of water in them, a large amount, and you have no control over it or any estimate of it. But if you let them mature and dry, and wither and turn light tan color, something shows up.

The pods have various numbers of beans, say, one, three, five, and seven. You take 100 pods as your sample: one hundred pods of one bean, one hundred pods of three beans, and so on. Then you weigh the pods and find that the weight of a pod of the reversed beans is less than the weight of a pod of normal beans, and the difference increases as the number of beans in the pods increases. The difference becomes rather substantial when you have long pods. OK, I thought, it probably has something to do with the beans. But I shucked the pods and got the beans out, and lo and behold, there was no difference—that is, the ratio of the grams per bean of the normal and reversed vines is one. So my activity did not change the weight of the beans: I did not get larger beans, I did not get smaller beans. On the average they were exactly the same.

If the pods are lighter, but the beans are the same, it must be the shucks that were affected. So I saved the shucks and weighed them, and when there is only one bean you can't see any effect, but with three beans it begins to show and with five beans and seven beans it is very pronounced. The pod from the reversed vine that has seven beans in it has a shuck that only weighs 65% as much as the normal shuck. So the plant reacts to the reversal by making lighter shucks. I tried to find out if the shucks were thinner with a micrometer, but they are so crinkly that you can't make anything out of it.

I did this for all four varieties of bean. Some of them were more pronounced, and the one that I brought from Hawaii was the most pronounced. So it was not any peculiarity of a particular variety—plants of all varieties responded in the same manner, more or less.

You see, I don't know anything about this stuff, I never took a course in botany in my life. I just do these things to see what is going to happen. If you keep your eyes open, interesting things happen.

At the University of Tasmania they had a great big fellow named Barker who was professor of botany, and his specialty had something to do with Eucalyptus trees. I went to see Barker and gave him statistics on some of these things. And I said "Now, the reason I have come to see you is because you have great experience in botany and I was wondering if at any time you have encountered any papers which describes things that I have done here and what the results might be?"

And he looked at me as though he was looking at a nut, and he said, "Never!"

So that was the end of Barker—I didn't get anything out of him.

The CSIRO down there has many divisions like fisheries and forestry and plant industry, and so on. There was a New Zealander there named Otto Frankel and he had worked his way pretty high up in this Division of Plant Industry and so the officer in charge there said, "Otto Frankel is in the library. Would you come in and tell him what you have been doing with beans?"

I said, "Sure."

Otto listened, and when I was all through he said “There is no reason why it should be [bangs his fist on the table], it can’t be [bang], and I don’t believe it [bang].”

Well, sir, it’s your privilege. But see, he is way up in the line of this plant industry and he has got a lot of flunkies, and instead of sitting there and deriding the whole thing as bunk, all he had to do was get a couple of his flunkies and put them to work—work that anybody past the 7th grade could do. But he didn’t.

One Saturday the summer I was here in Green Bank [1959] I got five students and we went on an expedition into the hills. We hunted around and looked for various things, and I noticed some vines. Some of these students were brought up in an apartment in New York and they were very frightened of the wildlife: every leaf had a caterpillar under it which was going to bite them and every piece of ivy was going to sting them and there were going to be snakes around and it was real frightening for some of these kids. They did not know anything of the life of the country.

Another Saturday I went over the hill to the next valley with Claude Sinclair and one other fellow who was a draftsman here, and the purpose of the expedition was to hunt for twining vines. We were pretty successful. We were over there all afternoon and, lo and behold, we found nine different kinds of twining vines—they differed in their leaf shape and color and so forth—and four of the plants turned one way and five of them turned the other way. So at any given location, if you hunt, you can find a considerable number of these twining plants, some of them going one way and some of them going the other way. This puts to sleep the dogma that plants turn one way in the northern hemisphere and another in the southern hemisphere.

I then read some more, and somehow I stumbled on a book written by some Englishman in the heyday of British India. Apparently he was a professor at the University of Bombay but he didn’t have a lot to do, and he had a lot of easy labor, and so he went out in the bush hunting for plants and trees and whatnot that had never been described before. He wrote a volume on twining plants, I think it was about 1925, published by the Royal Botanical Society of Calcutta. Half of it he devoted to left-hand turning vines, and half of it he devoted to right-hand turning vines. There was about 400 pages in the book, and in India about half the vines turn one way and half the vines turn the other way. So India is the same as the Deer Creek Valley! This began to tickle me. I was looking for someone who had done something similar to what I had done, but apparently nobody had.

A couple of years later, I was visiting a friend in Pasadena and thought I would see what the Santa Ana Botanical Gardens had to offer. So I wandered around and there was one thing that attracted me: they had a very open mesh wire netting, about 8 ft high, and beneath it they had some bulbs and these things were sending up shoots, like onions. You know that at the top of the onion plants a seed pod opens and there is a cluster of seeds. Well, these things were doing pretty much the same except they were much taller— growing up 7 or 8 ft high. The thing that attracted me was that some of them were growing up one way and others the other way. In other words, here was some kind of plant in which some individual plants were turning one way and others the other way.

So I got a hold of one of the gardeners around and she said, “Go see Mr. Ball, he is the chief gardener.” We hunted around and found Mr. Ball and I explained this to him and yes, he knew about these plants, but no one had drawn his attention to the fact that some of them were going one way and some were going the other way, and indeed they were. So I said, “Who do you think knows about this?”

We wrote down the Latin name of this plant and went to see their chief botanist, a Mr. Everett. Ball came with me. I discussed this plant with him, and I asked him where these things came from, and he did not know exactly but had one of his clerks fish in the filing cabinet. They got out a file and found that they came from somewhere up on the side of a mountain at 2500 ft, and while the plants go to seed like an onion, they don't propagate that way—they propagate like an onion—new bulbs start at the side and so on.

Well, this more-or-less intelligent discussion went on for a while, and I asked about getting some of these to take with me. He said it wasn't exactly the right season, that I should do that when they are dormant.

About this time a big, gray-haired fellow came in and injected himself into the conversation. I tried to explain that I was interested in these plants, some of which turned one way and some turned the other way, and he acted as though he wasn't even listening. He started an argument with Everett about where they got these things, and so I told him that I had been playing with beans for some years and I had raised them in West Virginia and in Hawaii, and had taken the same beans and tried them in the southern hemisphere and they all turn the same way.

He said, [bangs fist on desk] “You didn't do it right.”

And I said, “How's that?”

He said, “It's been proven in the literature that they turn one way in the northern hemisphere [bang] and the other way in the Southern hemisphere [bang].”

This went on for a little while and I got tired of it so Ball and I went out and left this joker with Everett. And I said, “Who is that big gray-haired fellow with the crotchety disposition?”

He said, “Oh, that's Dr. Munch, he's the Director.”

We wandered around and it was getting late in the afternoon, so I went back to the people I was staying with and explained what I had seen and encountered and my friend said, “Oh don't worry about it. Dr. Munch is that way. He's written a new book. We'll go over to the University Library and get this book.”

So we did. And we hunted through this book and, lo and behold, in this book it said, just as old Munch said, that twining vines turn one way in the northern hemisphere and they turn the other way in the southern hemisphere. So Munch knew what was going on in the literature but he did not have the faintest idea of what was going on in the garden.

So you see, if you think the physicists are nuts you should try the botanists.

Well this kind of thing went on and on, and I kept my eyes open, and I wrote three articles about this, and got a few requests, but as far as I know at this late date nobody has done any work to follow up my experiments. So here is an opportunity for somebody, if they are interested, to go out and manipulate their beans. Barker had not heard about anybody doing this, and Munch and Otto Frankel were dead set against it, so the matter is still open.

Discussion

R. Norrod: Do any of the beans naturally grow in the reverse direction?

Reber: Over the years I have must have handled a thousand vines and I never found one which would naturally go the other way.

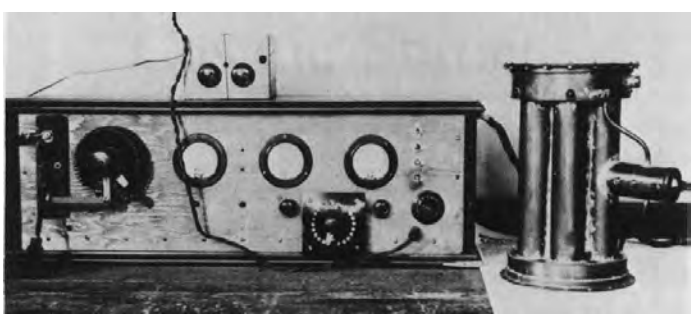
R. Lacasse: So you conclude that the same plant in this hemisphere and the other hemisphere still grows like a right-handed screw?

Reber: They will grow like that in any hemisphere. It's got nothing to do with Coriolis force, and it can't have anything to do with touch, because if it is touch it would be random. All this started over at the Hill House which is why I thought it would be appropriate to talk about it here in Green Bank.

One more thing: there is some kind of botanical society in Charleston WV and they publish a journal, I think quarterly, called Castanea. I wrote this stuff up and sent it to Castanea. And the editor wrote back and said that we're very pleased to receive your contribution, and that we are in favor of new and different kind of scientific effort, and that we can see that you probably have something worthwhile, but why did you pick us? I replied that I thought that Castanea was a suitable journal because these experiments were done at Green Bank, WV. Ultimately he published it, but I found out later that his board of Directors chastised him for this.

So you see, its the same old jazz, whether its the physicists, the astronomers or the botanists!

These experiments are described in:
"Reversed bean vines" 1960, *Castanea*, **25**, p. 112.
"Reversed bean vines" 1964, *J. Genetics*, **59**, p. 37.



Receiver (R), and Power detectors (L), of Reber's 160 MHz receiving system, used to make the first maps of the radio sky.

Travels with Grote*

S. A. Heatherly
NRAO
Green Bank, WV

John Steinbeck's "Travels with Charlie" is about a man traveling around the country in a beat-up pickup truck with his dog. The book isn't so much about the scenery as it is about the man rediscovering some nice things about people as he and Charlie stopped along the way. At the end of his journey the man was a little more optimistic about life. Well, the time I spent with Grote Reber this summer had a similar effect on me—hence the title of this article. In case you didn't know—but since Grote got a little work out of just about everyone at the Observatory you all should know—Grote Reber spent two months here this summer [1996]. I picked him up at Dulles Airport on July 6, and our trip into West Virginia presaged the lessons I would learn from Grote. Here are a few of them:

1. It is Preferable to be Accurate

On Grote's first day, on our way to Green Bank from the Washington airport, we stopped for supper. He wanted some ice cream for dessert and asked me to summon the waitress. Before she came over I attempted to find out exactly what Grote wanted. "Do you want an ice cream cone?" I asked him. "Well," he said, "I once had something—not really a cone, more like a cylinder made of edible material into which ice cream was piled and more was piled on top." How is that for being accurate!

2. Persist! and You Shall Get it Done

This is a lesson you could learn from reading about Grote's early work in radio astronomy in Wheaton, Illinois. Grote persisted in his interest in radio astronomy after Jansky indicated he would not be doing any more work; after he failed to interest professional astronomers in the subject, and even after he failed to detect anything with his first set of receiving equipment. The results of his persistence are well known. Evidence of his persistence is on display in the NRAO front yard where his original 30-foot dish sits by the main entrance.

What I learned this summer, though, was that persistence need not fade with age. One of the projects Grote got himself involved in was putting the Jansky Antenna in usable condition. This had never been done, you know. The replica was originally built to be a show piece— no one thought it would ever be used. But in 1995 a bunch of us at NRAO decided it would be fun to re-do Jansky's experiment. Since then, the "Jansky Fellers," as we call ourselves, have been using the antenna more-or-less as is. The antenna was refurbished somewhat so that it could rotate under motor control, but Fellers had to be present to bang the chain back on the gear every few minutes! Even worse, every two rotations, the antenna had to be stopped so that the Fellers could untangle coax cables which got wrapped around the center post.

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* An earlier version of this article was published in *The Tattler*, Green Bank WV, September 1996.

(L to R) Grote Reber, Sandy Heatherly, Patrick Heatherly, Bob Vance. Balloons are being prepared to hoist a small transmitter to measure the vertical beam pattern of the Jansky antenna. Summer 1996.

The Fellers had hoped to interest Grote in making some observations with the antenna this summer. But he took one look at our set-up and said it was a mess! And it was. Under Grote's supervision, a coaxial slip joint was installed which allows the antenna to be rotated while connected to a receiver without tangled coax lines. This was no easy feat since it required drilling through about 18 inches of concrete (special thanks to Harley Carpenter). He instigated reconstruction of the gear box which was absolutely necessary in order to keep the chain on the gears and not tear the motor up. The shop guys and mechanics did a beautiful piece of work there, by the way.

Grote accomplished in 3 weeks what the Jansky Fellers had not thought possible at all. It might have something to do with having the name Grote Reber, but I think his tactics had something to do with it. If Grote decided something had to be done, then we did it. Now! If he needed a part machined he might just wait in the machine shop till it was ready. Or he would sit on the Jansky Antenna and wait while I (or Carl Chestnut, or Dave Vandevender or H. A. Taylor or whoever) went to do his bidding. You can't just sneak off to coffee while Grote Reber is sitting in the hot sun waiting for your return.

I must add that Grote thought there was an awful lot of sitting around going on in the Jansky Lab. He made note of the level of activity on the second floor every time he walked down the hall, and couldn't understand all the sitting and staring that people did (at their computers!!!).

3. All of Life Can be an Experiment

Grote is not a man who rests on his laurels; he is cooking up new experiments all the time. While he was here this summer, Grote pursued, with characteristic persistence, an experiment to test the feasibility of extending the long-wave radio astronomy research he has conducted in Tasmania. As he said to Gibbs Kinderman in an interview on radio station WVMR:

"I made observations at a wavelength of 144 meters down in Tasmania which is about 1000 times as long as what most radio astronomy observations are. Now at meter waves and down, the radio sky looks much like the optical night sky. It is dark all over with a few bright spots here and there, some of which correspond with optical objects and there is a bright band coincident with the Milky Way.... [But] at hectometer waves (that's hundreds of meters), it reverses: the radio sky is like the optical day sky. It's bright all over. Brightest near the Galactic Pole (there are a whole series of dark absorption areas coincident with the Milky Way) and darkest near the center. This background radiation obviously comes from outside the Milky Way at what distance we don't know. And the absorption regions are caused by ionized hydrogen gas in the plane of the Milky Way. I would like to make observations at still longer wavelengths.

"This phenomenon is probably enhanced as you go to a longer wavelength. Now 144 meters is 2 megs (MHz) and that's above the broadcast band. The smallest change that's worth anything is 2 to 1 and that would take us into the broadcast band. So, I got the handbook out and picked out, I think it was, 27 stations at 1010, 1020 and 1040 kilocycles. I wrote to the station managers suggesting we swing a deal where they go off the air in the early hours of the morning and call that public service hours for my benefit. They could save money and wouldn't be polluting the air and I could make observations (at 1 MHz) during the time that was least desirable for them. Peculiarly enough they were dead set against it! ... In the mean time I'd made listening tests in Canada and Tasmania and there's an empty channel at 520 kilocycles, apparently worldwide. Nobody is using it. Its empty. And so, I thought it would be nice to set up and make observations at 520 kilocycles, that's about 610 meters. Well, if you are going to do that you have to do something about the ionosphere so the radio waves will come through, so I'm in the process of negotiating with NASA about sending up some rockets to clean up the ionosphere. That's the future. I was born on 22nd of December 1911, so on the 22nd of December I'll be 85. By the time I'm 90 maybe NASA will give me the go ahead!"

For Grote, there was always an experiment waiting to be done, even in idle moments. While sitting out on the Jansky Antenna waiting for me to come back with a wrench, or in his office waiting for a letter to be typed, Grote would conduct experiments with whatever was at hand. Example: for several days while out at the Jansky Antenna, Grote counted the cars that went north and south on Rt. 92. He noticed that more cars went north than south, and thought that was odd. By expanding his observations to other times during the day, Grote was able to report that it "evened out"! Another example: Grote made numerous tests on Sue Shears' [the electronics department secretary] calculator. He found out that an average time of 28 seconds elapses between the last entry you make and the time it automatically shuts off.

Reber discussing his original equipment with Darrel Emerson, 1996.

4. You Can Use a Handicap to Your Advantage

If you spent any time at all with Grote this summer you will know that his hearing aid was NOT a handicap! Witness the fact that he always turned it off before taking questions at the end of his talks and any other time he just didn't want to be bothered.

5. No Use Crying Over Spilled Milk

Did you know that Grote Reber came close to discovering the 21 cm spectral line emitted by neutral hydrogen (the most abundant element in the Universe)? He began building a 21 cm frontend 6 years before the detection was made by Ewen and Purcell at Harvard, but never completed the task. Darrel Emerson (the director of NRAO's Tucson, Arizona site) heard the story for the first time this past summer when he and Reber toured through the exhibit in the little building beside the Reber Telescope. Here is a bit of their conversation:

Darrel says (as he and Grote look at the 1420 MHz equipment): "Now that was one of your later experiments, and that was, what, in the 50s or 60s—it must of been the 60s when you were doing that."

Grote replies: "1946. See, I'd been operating my equipment all during the war—and it ran automatically while I was working in the city. Then as soon as the war ended the Dutch sent Van de Hulst to Yerkes Observatory at the University of Chicago, and they knew that I was doing radio astronomy, but they didn't know much of anything about it. However, Van de Hulst wanted to come and talk to me, which he did. This was maybe late 1945. And he told me about his theoretical investigations of the hydrogen line and wanted to know if I could do something. Well, I was just getting my 480 Meg system working, so there wasn't anything I could do immediately, but I put it on hold and in the meantime I took a job with the Bureau of Standards. And the idea went along with me, and the B of S fellows didn't know anything about it, and they seemed to care even less, so it failed (chuckle). It's hard to believe, but that's the bureaucracy."

D: "Now the H line was finally discovered 7 years after that, wasn't it?"

G: "Yes that was Ewen and Purcell. That was about 1951—about 6 years later."

D: "So about 6 years later than when you nearly discovered it."

G: "(Chuckle) Well I didn't really, but I could have."

6. It is Important to Challenge Dogma
(the Big Bang May Not Be Bunk)

Grote inaugurated the auditorium in the new wing of the Jansky Lab by giving the first colloquium there. He gave his controversial talk called "The Big Bang is Bunk!" He bases this notion on his observations at long wavelengths in Tasmania and an alternate explanation he has cooked up for red-shifts. He says: "I'm not a theoretical man. I'm an operating technician type of man who builds stuff, tries them out and if it fits the theory, fine; if it doesn't, you change the theory. You don't change the observations."

On his last day here, just before leaving to do some face-to-face "negotiating" with NASA at the Wallops Flight Facility, he hinted that the talk wasn't so much about his theories about the big bang (he admitted that while most of the talk was based on fact, in the middle he waved his hands a bit). The talk was really intended stir things up; to challenge dogma: "Young people don't have any trouble embracing new ideas" he said. "When I set out as a young man, the old boys in astronomy weren't a bit interested in the new idea of radio astronomy. But I was. I didn't know enough not to be interested."

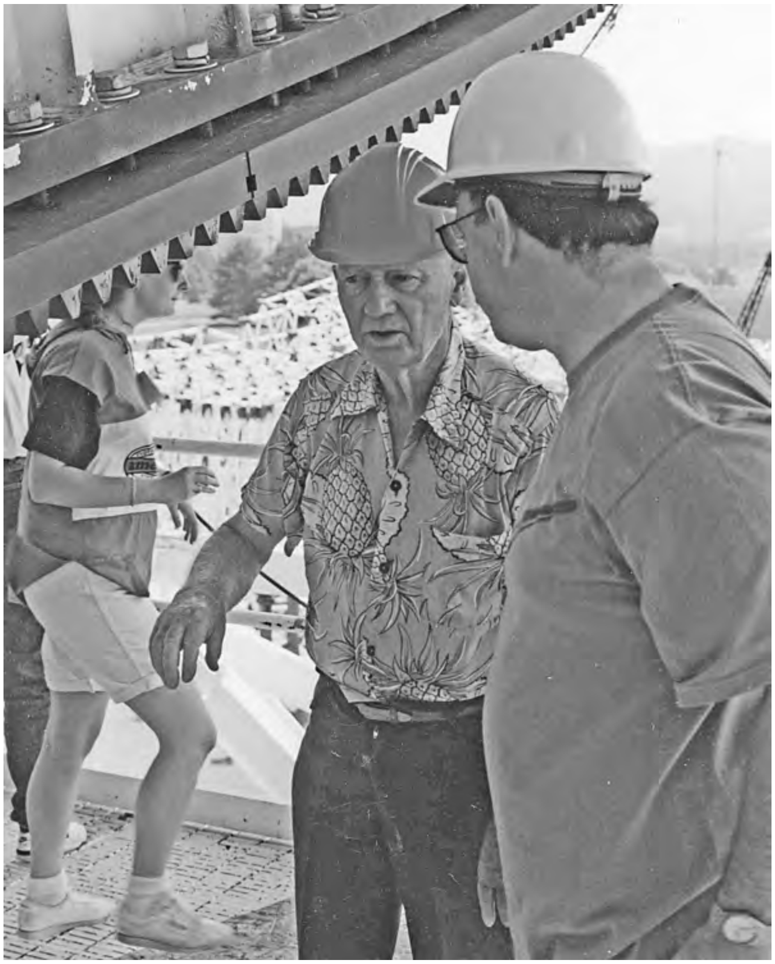
Is Grote surprised how radio astronomy has grown as a science? He says: "Well yes, and it still surprises me. Apparently radio astronomy has been a popular scientific sport, and people can get money to do it as evidenced by NRAO and why I'm here—so—I'm in favor of it!"

Selected References

Reber's accounts of his pioneering experiments —

Proceedings of the IRE, 1958, **46**, 15.

Serendipitous Discoveries in Radio Astronomy (ed. K. Kellermann and B. Sheets, 1983, NRAO).



Reber visiting the partially contructed GBT in the summer of 1996, accompanied by Tom Bania (R) and Deborah Rice (L). The trial assembly of the GBT backup structure is seen in the background. The elevation gear looms overhead.

"A Play Entitled the Beginning of Radio Astronomy," J. Roy. Astron. Soc. Can. **82**, no.3, p.93, 1988.

Reber's first astronomical results —

"Cosmic Static," Astrophys. J. **91**, 621, 1940.
"Cosmic Static," Astrophys. J. **100**, 279, 1944.

Reber died in Tasmania in December 2002; an obituary may be found in

"Obituary: Grote Reber (1911-2002)" by K. Kellermann. Nature **411**, 596, 2003.

Arnold Davidson next to the 5-foot mm-wave telescope.

4 NRAO's First Millimeter Wave Telescopes

Report of a Visit to Texas Instruments, Inc. In Connection with the Germanium Bolometer

Frank D. Drake
February 6, 1962

On February 1, I visited the Central Research Laboratories of Texas Instruments, Inc., in Dallas, to see if it would be possible to arrange the procurement of the low temperature germanium bolometer developed there. Conversations were held primarily with F.J. Low, the inventor, and J.R. MacDonald, head of the Physics section.... We might obtain by April one of the bolometers complete with a dewar containing an appropriate window for millimeter waves....

Antennas [for mm-wavelength astronomy] will have to have surface tolerances of the order of 0.1 mm.... It probably would be worthwhile to attempt to generate the proper surfaces by spin casting epoxy resins on a back-up structure, since the successful development of this technique would lead to great savings in time and money....

It is proposed that development of millimeter wave observing facilities be commenced following roughly this path:

1. Immediate procurement of the bolometer system as proposed by Texas Instruments.
2. Immediate procurement of the test equipment that would be useful in this project.
3. Immediate experiments with spin casting of epoxy resins, as well as inquiries to D.S. Kennedy [antenna manufacturers] to see what might be provided by them.
4. As soon as possible, the construction of a 3-foot antenna on an equatorial mount, for use in preliminary observations from the roof of the Jansky Laboratory. This instrument alone will give signal/noise ratios with a one-second time constant of 3000 on Venus at inferior conjunction to 90 at superior conjunction, 500 on Jupiter, down to about 1 on Neptune or a typical comet. Some radio sources will be visible. The beamwidth would be $5'$.
5. As experience allows, the construction of a 10 foot antenna for use at the Jansky Lab or perhaps on Bald Knob or Buffalo Ridge....
6. Eventually the construction of at least a 30 foot antenna at a very high, dry, site. Perhaps the proposed University of Arizona telescope will be available for this use.

Astronomical applications ... include observations of all the planets, many satellites, several asteroids, comets, and other solar system objects. Many cosmic radio sources will be visible, extending spectra some three octaves. High resolution studies will be possible. Beamwidths of $0.5'$ will be available with the 30 foot dish, and even higher resolutions with interferometer techniques that should not be too difficult. One might imagine the use of a 300 foot baseline, for instance, which would give a resolution of about 1.4″—a resolution strictly comparable with optical resolutions.

The Millimeter Wave Project: February 1963*

Arnold Davidson
NRAO

In 1960, Dr. Frank J. Low of Texas Instruments, Inc., developed a new low temperature bolometer, using a gallium doped single crystal of germanium. At Dr. Drake's invitation, Dr. Low arrived at NRAO in October of this past year to investigate the use of his bolometer in the millimeter wave region.

A 5-foot radio telescope was purchased and subsequently mounted on the old 12-foot radar mount. This telescope possesses a highly accurate gold-plated surface necessary for working at such short wavelengths. The germanium detector is mounted in a special chamber where it is cooled by the liquid helium to its operating temperature below 2 K. The millimeter waves enter this chamber through special windows and filters. Facilities for handling liquid helium as well as vacuum and other necessary equipment are housed in a new laboratory located in the basement of the main administration building.

First observations were made in the closing days of November [1962], and early results on the Sun and Moon were very encouraging. Fluctuating sky background at higher receiver sensitivities has as yet prevented detection of the fainter sources, but this, as well as other experimental problems, is in the process of solution.

One rather unique feature of this telescope is the fact that it must be guided optically at the mount rather than from a heated control room. This is due primarily to the rather narrow beamwidth of 5 minutes of arc and to the alt-azimuth type mounting which does not allow sidereal tracking. To overcome this rather inconvenient feature, a closed circuit TV system was purchased and recently installed. It is now possible to view on a TV monitor in the control building the field of view of the 5-foot telescope.

The Millimeter Wave Project: November 1963*

Perhaps everyone has noticed the proliferation of telescopes springing up on the concrete pad at the rear of the laboratory building. They are to be used in conjunction with the millimeter wave project and consist of a 3-foot, a 5-foot, and a 12-foot telescope.

3-Foot Telescope

This is a dual purpose telescope intended for use at 10μ (in the intermediate infrared) and 1.4 millimeters. It is a Kennedy spun cast dish with a diameter of 36 inches, a focal ratio of 1, a beamwidth at 10μ of less than 10 arcsec, and a

* *Excerpted from articles in "The Observer" February 1963 and November 1963*

gold-coated surface with an accuracy of better than ±0.001 inch. The mount is a modified equatorial optical telescope mount provided with local control and optical guiding. At the present time, the telescope is being used to develop better receiver systems for the 12-foot telescope.

5-Foot Telescope

The mount is of the same type as the 3-foot, only a little heavier. Both local and remote control is provided with an optical telescope for local guiding, and a closed circuit TV system for remote guiding.

The following programs are being carried on at this time:

1. Solar extinction measurements made under varying conditions of atmospheric transmission.
2. Temperature profiles of the lunar surface during the various phases of the Moon.
3. Planetary detection and measurement with particular emphasis on Jupiter and Venus.
4. Receiver development, in particular, low noise, first stage audio amplifiers.

12-Foot Telescope

This is also a Kennedy spun cast dish mounted on a modified surplus Nike-Hercules radar mount. The dish has a diameter of 145.0 inches, a focal ratio of 1, and a surface accuracy of ±0.002 in. It should have a theoretical beamwidth of $1.3'$ at 1.4 mm. The surface is a spun aluminum dish with 3 layers of cast epoxy, overlaid with a thin gold paint coating. D-C stepping motors provide a continuously variable drive rate from 0 to 20 deg/min on both axes.

All telescopes are now complete with the exception of the 12-foot; preliminary observations of the Sun have been made and the results look very encouraging.

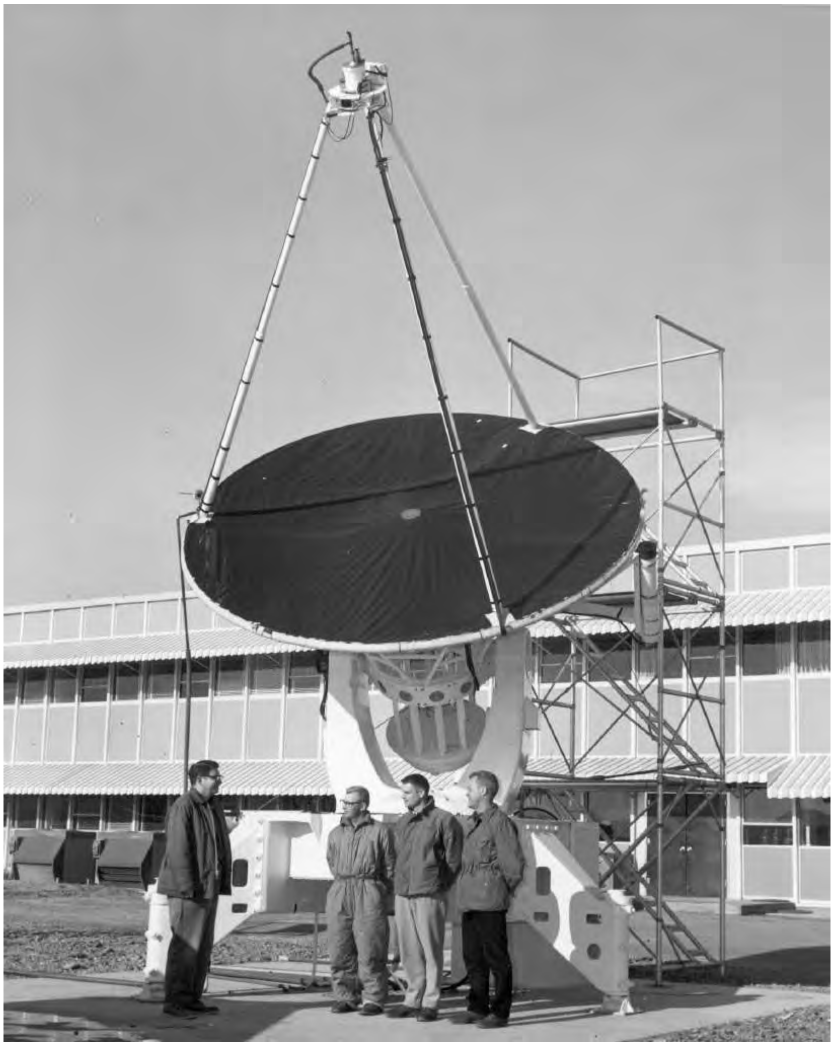
From the NRAO Quarterly Reports

October – December 1963

Observations at 8 to 14 microns were begun using the 36" telescope at Green Bank.

Although Jupiter was detected at 1.2 mm, uncertainties in the beam parameters of the 5-foot telescope prevented a brightness temperature determination. Observations of the Moon, using the Sun as a calibration source, give 221 ± 20 K for the mean brightness temperature and 286 ± 20 K and 157 ± 15 K for the maximum and minimum temperature during a lunation. These results indicate larger thermal gradients near the surface than were expected.

The new 12-foot telescope is essentially complete and is undergoing preliminary observational tests.



The Twelve Foot mm-wave telescope behind the Jansky Lab. Left to right: Frank Low, Tom Carpenter, Arnold Davidson, Omar Bowyer.

January – March 1964

The 12-foot spun-cast reflector was found to have serious departures from parabolic geometry and suffered large temperature changes. The narrowest beamwidth obtained was four times the expected value.

Radiometer development at 1.2 mm has proceeded and, despite the inefficiency of the 12-foot dish, an rms temperature fluctuation of 0.1 K was obtained with an integration time of 10 seconds. A dual-beam radiometer was tested and showed an order of magnitude reduction in sky brightness fluctuations. This indicates that milli-Kelvin sensitivities are possible in the 1.2 mm wavelength window.

April – June 1964

The original 5-foot spun-cast reflector was remounted on the Nike mount in place of the 12-foot reflector, pending a decision by the Kennedy Antenna Division as to the disposition of the 12-foot dish. This was necessitated by the large departure of the surface from a true parabola and by serious temperature effects on the shape of the antenna.

The 5-foot reflector, which was found to possess near-theoretical 3.6 arcmin beamwidth, is now being used for a brightness distribution study of the Moon at various phases and for observations of Venus and Jupiter.

F.J. Low spent the month of June at the University of Arizona using an improved 11μ radiometer in conjunction with the Arizona 28-inch optical telescope.

July – September 1964

A number of measurements of Venus have yielded an internally consistent value for the brightness temperature at 1.2 mm, which is below the infrared value of 225 K and is difficult to understand on any simple model of the planet. There are very considerable uncertainties in the calibration procedure, but they do not explain this result. The most recent measurement of Jupiter yields a temperature close to the expected temperature. Lunar temperatures are consistent with a solar temperature of 6000 K. The maria are somewhat colder than the highlands. These efforts are continuing.

October – December 1964

The following programs are being carried out at 1.2 mm:

1. Solar observations with special emphasis on the effects caused by atmospheric water content on signal attenuation in the 1.2 mm region.
2. Observations of three selected areas of the Moon during the lunar cycle in order to perform absolute temperature measurements as well as to determine cooling and heating rates during the lunation.
3. Continuing temperature measurements of the planets Venus, Jupiter, Saturn, and Mars when atmospheric conditions permit.
4. A receiver development program to reduce further the noise temperature of the detector as well as to increase the efficiency of the microwave feed system.

The total lunar eclipse of December 18-19 was observed at 1.2 mm, and preliminary data reduction indicates very good results were obtained relative to lunar cooling and heating during the eclipse.

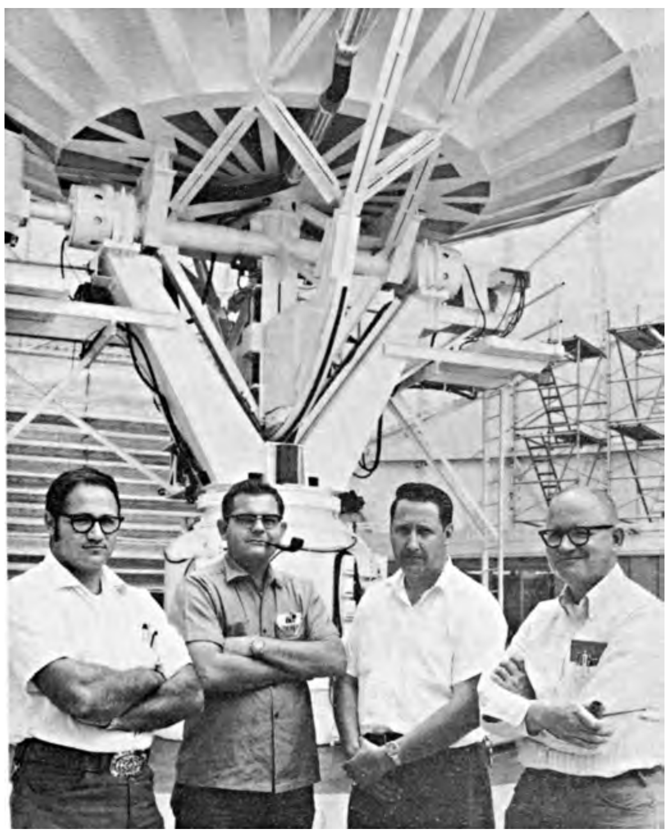
HISTORICAL NOTE – from the April-June 1964 Quarterly Report

The development of the helium-cooled germanium bolometer by Dr. F. J. Low at Texas Instruments in 1961 coincided with the start of the millimeter wave radio astronomy at NRAO. Dr. F.D. Drake used one of Low's earlier bolometers with a 5-foot Kennedy spun-cast dish on the NRAO SCR-584 mount, which had originally carried an X-band 12-foot dish. This mount was not precise enough for good observations, but with it the bolometer and radiometer techniques at 1.2 mm were tested, using the Sun and the Moon as sources.

Subsequently, in October 1962, Dr. Low joined the NRAO staff, and since then has made observations with his bolometer at 1.2 mm and at 11μ. Various instruments have been used at NRAO, mainly the 5-foot dish on a new mount and a 36-inch mirror for 11μ. Low has also observed at 11μ with the 82-inch telescope at the McDonald Observatory and with a 28-inch telescope at the University of Arizona.

By the early part of 1962, it was evident that instrumental developments were promising enough to warrant the use of a larger and more precise dish. Accordingly, a request for funding of a telescope of about 30 feet in diameter was made in 1962 and this request was subsequently approved by the National Science Foundation for inclusion in the FY 1964 budget.

Editors' note: The NRAO 36 Foot mm-wave Telescope on Kitt Peak, Arizona, was put into operation in 1967.



The 36-foot millimeter wave telescope and its operations crew on Kitt Peak, Arizona in 1969. Left to right: Don Cardarella, Bill Daniel, Bob Hogarth, and George Grove (station manager). [Photo from The Observer, September 1969.]

5 The Green Bank Interferometer

View of antennas 85-2 and 85-3 in 1968. Note the 140 Foot in the distance.

Editors' note: An excellent discussion of the origins and early science with the Green Bank Interferometer is in "Radio Interferometry: the Saga and the Science," ed. by Finley and Goss, NRAO Workshop 27, 2000. The paper by David E. Hogg from that volume is reprinted in this section.

Plans for a large interferometer system for NRAO were being discussed in the early 1960s. Eventually this led to the construction of the VLA in New Mexico, beginning in the mid 1970s. As a first step, a 2nd 85-foot dish was built in 1963 to form an interferometer with the existing Tatel Telescope. A third 85-foot antenna and an array control building was completed in 1968. Among the discoveries made with the three-element interferometer was that of radio novae (Hjellming and Wade, ApJ **162,** *L1, 1970).*

A fourth element, a 45-foot antenna, was added in 1973. The 45-foot was placed about 35 km south to gain experience with baselines similar to those planned for the VLA. Its addition to the Green Bank Interferometer was critical in the discovery of the radio source at the Galactic Center, SgrA (Balick and Brown, ApJ* **194,** *p.265, 1974), and the measurement of the gravitational deflection of radio waves to unprecedented accuracy (Fomalont and Sramek, Phys. Rev. Lett.* **36,** *p.1475, 1976).*

Excerpts from The Observer

January 31, 1964:

Work is progressing on the 85-foot Interferometer Project. The only remaining major job left on the scope itself is placing and setting the polar gears. The cable tray is up and most of the control wiring is in the tray. Also for the record, the clearances under the tray at the Hosterman road (High School) crossing is 14 feet, 9 $\frac{1}{4}$ inches.

For those few not familiar with the magnitude and complexity of the 85-foot Interferometer Project, the following is a bird's eye view. At one end is the H. Tatel 85-foot Radio telescope, and 1200 meters to the South-west on an azimuth bearing of 242 is the new 85-foot Radio Telescope. The new scope is the same as the Tatel 85-foot scope in that it is an 85 foot diameter polar mounted instrument. The one big difference is that the new scope is movable. It will ride on 80 14-ply rubber tires.

May 28, 1964:

The new telescope was for the first time lifted off its foundation and entirely supported by the wheeled dollies on May 18.

October 30, 1964:

The 85-II Telescope was successfully moved from station number 1 to station number 2 on October 9th. It took about three to four hours to actually accomplish the move but many more hours of planning were required prior to the move. All people involved were duly complimented on the job of planning and execution well done.

December 31, 1964:

The interferometer with Captain Dave Hogg and First Mate Warren Tyler at the helm sails a choppy sea. Captain Hogg is on call at the bridge day and night; unfortunately, it is usually at night. But Dave takes to the nocturnal life with nary a complaint.

Delay tracking is the program and constitutes tracking a source for long periods of time with both telescopes. We operators find it a slow program relative to Captain Hogg's rip-snorters of old. Still the results are gratifying in that source positions can be accurately measured to one tenth of a second of arc; never before done in the history of astronomy. The stability of the system is supposedly unequalled anywhere in the world. Chief electricians, John Bringe and Jim Coe might argue this point with you. . . .

Moving telescope 85-2 (1964 photo)

From The Observer of September 1969:

"So You Want to Move a Telescope"
by Jon Spargo

Imagine picking up an object weighing 15,510 tons and physically moving it 30.48 miles. This is what has already been accomplished by the experienced crews responsible for moving 85-2 and 85-3 along the interferometer baseline. Of course, this all wasn't accomplished at once but rather in 47 separate moves over the past year. The total comes from the fact that each telescope weighs approximately 330 tons and can be moved to any of 7 stations along the lower part of the 2700 meter baseline.

A typical move goes something like this. On the day of the move things start happening about 8 o'clock. Observing ends and the telescopes are stowed (pointing straight up). Then telescope operators, electronic technicians, telescope mechanics, electricians, heavy equipment operators, and civil engineers descend on the telescope and begin preparations for the move. Cables are disconnected, steps stowed out of the way, and the telescope is unbolted from each of its three pads.

A guiding string is strung to the next station the telescope will rest on. Each leg of the telescope will be supported during the move by a huge multi-wheeled dolly which also carries a hydraulic jack used for raising or lowering the telescope at each pad. The two bulldozers that will supply the locomotive force to move the scope are hitched to two of the supporting dollies.

At a signal, each hydraulic jack on each dolly is started and the scope is slowly lifted from its pads. After being pulled clear of the pads and lowered again, to reduce the center of gravity as much as possible, the final power cable is disconnected and off we go at the break neck speed of about a half a mile an hour.

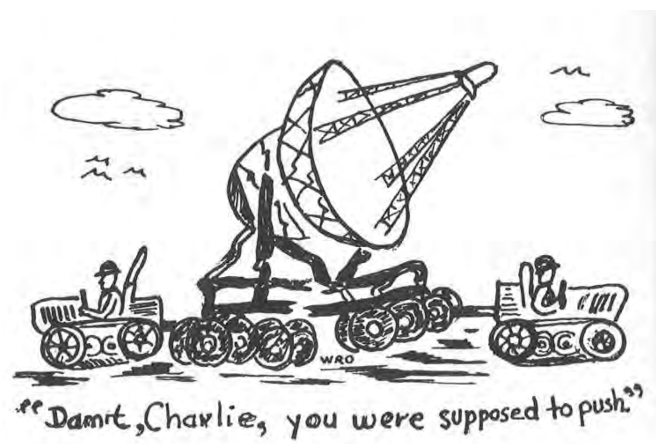
During the move the telescope is kept aligned for the next pad by two plumb bobs, one on either side of the scope. These plumb bobs are kept directly over the guiding string, mentioned earlier, by the speeding up or slowing down of either of the bulldozers. Upon reaching the next station the scope is stopped short of the pads and raised so that it can be moved over them. If the final resting point is several stations away, the scope will be lowered again on the other side of the pads and will continue on in this leaping manner until the desired station is reached.

Once over the desired station all three legs of the scope are carefully checked for alignment over their respective pads and if all is well the scope is slowly lowered onto the pads by the hydraulic jacks. If the legs are out of alignment, a small amount of jockeying by one of the bulldozers will straighten things out in short order. Due to the proficiency of the crews, however, this is seldom required.

Once resting on its new station the scope is hooked up again. Before it can be securely bolted down, however the civil engineers must perform a set of exacting measurements to assure that this scope is properly aligned. Usually some small alignment adjustments have to be made, and are accomplished by turning adjusting bolts on each leg of the scope. Once aligned and securely bolted, the telescope operators and electronic technicians proceed with a final checkout of the scope. After that the scope is ready for use.

To be sure, all this has developed into something of a routine for the crews involved. Consider, however, that an average move can be completed in 3 to 4 hours, and that on some days double moves involving both telescopes take place. Add to that the fact that these moves take place the year around in all kinds of weather. Throw in also that through all this a perfect safety record has been maintained ... an enormous job well done indeed.

So you want to move a telescope? Just ask the interferometer crews; they'll show you how.



Cartoon by Wally Oref appearing in The Observer of June 1973.

The Green Bank Interferometer*

David E. Hogg
National Radio Astronomy Observatory

1. Motivation for the Construction of an Interferometer

In 1962, the NRAO had one general-purpose telescope— the 85 Foot Telescope—and both the 300 Foot and the 140 Foot Telescopes were under construction. The 300 Foot would be completed later in 1962, with first observations being made in September, but it would be three more years before the 140 Foot was completed. Nonetheless, there was active deliberation as to what the next major telescope should be.

By this time radio interferometry was an established technique. The initial 3C catalogue had been published 3 years earlier (Edge et al. 1959), and the Caltech group had just published their important results from the OVRO interferometer— a series of papers by Moffet (1962), Maltby (1962), and Maltby and Moffet (1962). It was therefore clear to Dave Heeschen and the scientific group that the NRAO should plan to do high resolution radio studies. It was decided that an array, as contrasted with a Mills Cross or the like, was to be preferred for reasons of scientific flexibility. Interestingly, the best written evidence from this time that I could locate is a letter to the West Virginia Representative K. Hechler from G. Keller of the NSF (Keller, 1962) saying just that, i.e. the NRAO expected that its next telescope would be an array of dishes (100-300 feet each in diameter!) spread over a large distance. It was even possible that the site would not be Green Bank, because of the physical extent required by the array.

The planning of an array began primarily with the development of the basic concepts. The first memo which contains a description of a serious synthesis array is the one by Cam Wade, issued towards the end of 1963 (Wade 1963).

2. The Two-Element Interferometer

In the early days of the NRAO the scientific staff had experience primarily in single dish astronomy, and the electronics group had strength in radiometer techniques, although they were learning about correlators from S. Weinreb. Thus it was decided that an interferometer should be started as a test bed for ideas, and to gain experience. As a first step the construction of a two element interferometer was begun. The second element was a clone of the first 85-Foot, except that the base was altered so as to allow it to be towed back and forth along a roadway (Figure 1.)

There was considerable discussion about whether to use a roadway rather than tracks. It was believed that wheel and road would cost less, and it was thought that it would be easier to develop other baselines more readily. Ultimately it was decided

* *Reprinted from "Radio Interferometry: The Saga and the Science," proceedings of a symposium honoring Barry Clark at 60. Edited by D. G. Finley and W. Miller Goss. NRAO Workshop 27. Published by AUI, 2000.*

Fig. 1— The second element, 85-2. This telescope was mounted on rubber tires, and was towed along a roadbed in order to move from one antenna pad to another.

that it would be valuable to get the experience of using a roadway, since there was already extensive experience with track at other observatories. However, it turned out that moving the antenna was a big chore. One had to get the pulling forces on the two bogies well-matched, or a twist would be introduced into the telescope base, leading to difficulties in the calibration of the pointing and baselines. And

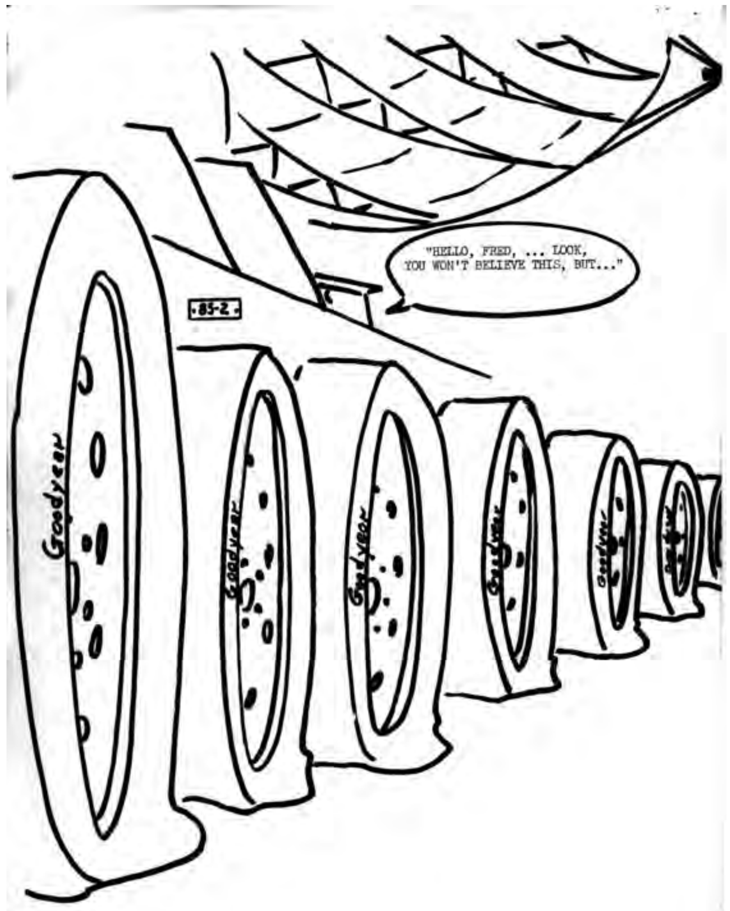


Fig. 2— A cartoon from The Observer (Vol. 10, No.5, September 1970) memorializing the summer when most of the telescope tires went flat.

there was the horrible year that the ozone got to the rubber inner tubes, and many of the tires went flat simultaneously (Figure 2). But the system could be made to work, and the telescopes were moved up and down the roadbed many times during the life of the interferometer.

The interferometer theory was developed by Cam Wade, Nigel Keen, and Marc Vinokur. Nigel was heavily involved as well in the instrumentation, and developed an ingenious computer which allowed the delays to be switched in and out automatically, assuming that the little computer was properly initialized (Keen 1964b). The interferometer backend and delays were in a trailer situated midway between the two telescopes. The first fringes were obtained with the 85-2 at station 1, 1200 meters from 85-1, but other stations were occupied over the next few months (Figure 3). The data reduction, at least in the first days. consisted of measuring the position and amplitude of the fringes on the chart recorder. Briefly, it was assumed we knew the point of zero fringe frequency, and by counting fringes back from that

Fig. 3— The two-element interferometer in January 1965. The antenna sits on station 5 (2400 meters), having been moved there for the first time shortly before the photograph was taken. The interferometer backend was in a trailer midway between station 1 (1200 meters) and 85-1; it is, unfortunately, not visible in this photo.

point we could estimate apparent source position. The procedure is described in unpublished working notes by Wade. Wade later made a rigorous discussion of phase drifts (Wade 1964a).

Figure 4 shows some of the early fringes. The records are from September 27, 1964, and are taken from a report by Nigel Keen (1964a). This reproduction of a strip chart is unfortunately of poor quality, but it does show fringes from 3C147 and from Cygnus A. At 4h 50m West hour angle, the fringe rate goes to zero, as is seen in both sources.

It did not take long before counting fringes got quite old. Wade discussed techniques for automating things (Wade 1964b), and after Barry arrived in November 1964, he also became involved (Clark 1964). The first comprehensive computer program for the analysis of interferometer data was documented in a report by Clark and Wade (1965).

The instrument was used for a number of research programs, primarily looking at source positions and visibility functions. The first Ph.D. program was that of

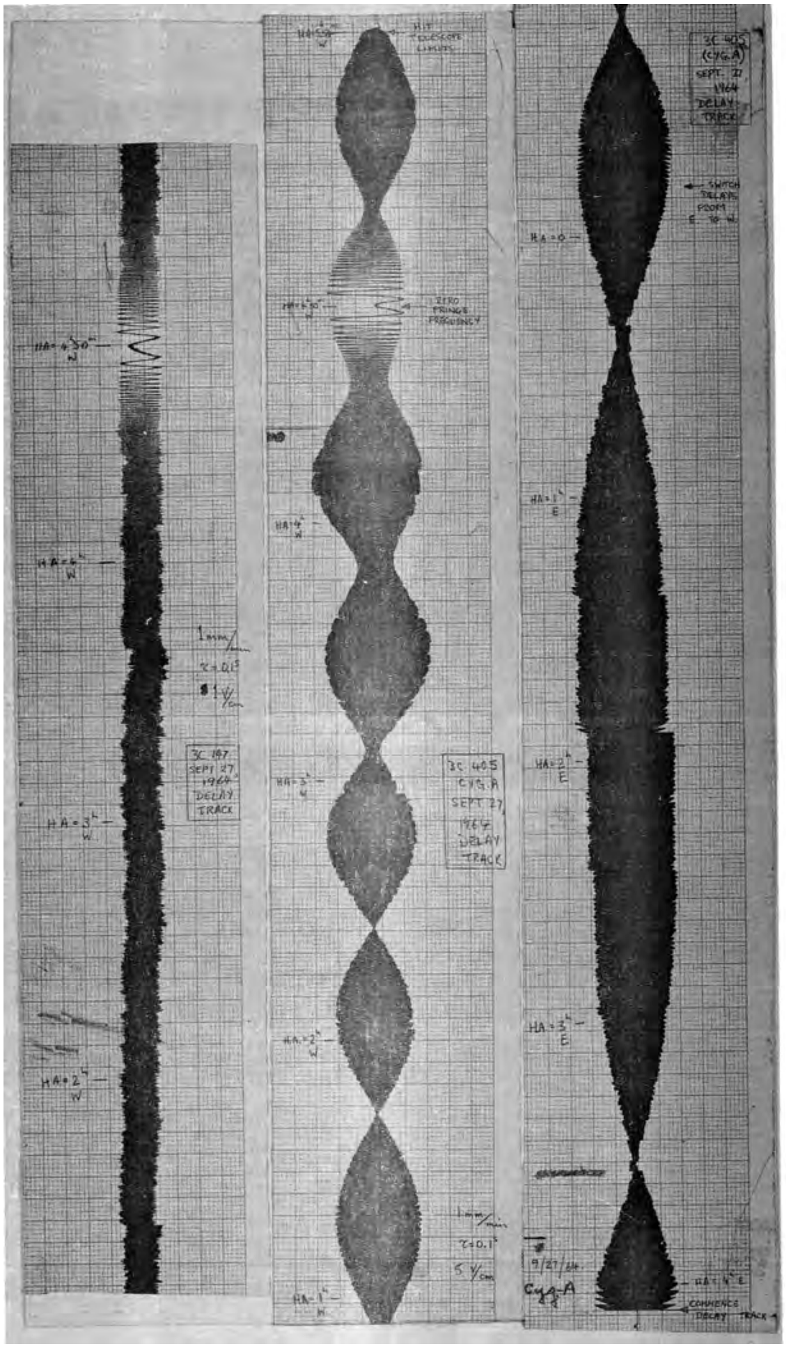


Fig. 4— Delay Tracks on Cygnus A and 3C147. Since 3C147 is a point source at this resolution, the amplitude of the fringes is expected to be constant. The fringe amplitude for Cygnus A varies, showing the dominant double structure, but also suggesting structure within the two components of the source. In each case the fringe rate goes to zero near 5 hours West hour angle.

Fig. 5— The Three-Element Interferometer. This scene from 1971 shows the three elements, the Control Building, and the relay tower used to transmit and receive signals to and from the remote antenna.

Frank Bash (1967). In this program, he used observations of the visibility functions of 234 radio sources to model their radio brightness distributions.

3. Interferometer Expansion

As work progressed with the 2-element interferometer it became obvious that good science required a faster instrument with finer spacings and better electronics. It also was becoming clear that a better system would aid in the development of the VLA design. Of particular interest was the question as to whether the atmosphere would support radio interferometry at centimeter wavelengths over baselines of tens of kilometers. The solution to this last concern was to build an outrigger antenna for use at distances of up to 35 kilometers. Ed Fomalont will describe this in more detail in a later paper [i.e., in the Barry Clark Symposium proceedings].

To implement an interferometer expansion, a proper project was defined, with Warren Tyler as the project head. In addition to new stations and a third 85-foot antenna, it would have a new control building from which to operate. The building, started in the summer of 1966, was occupied during the first quarter of 1967. The antenna was completed in the second quarter of 1967 and observations with the improved system began (Figure 5).

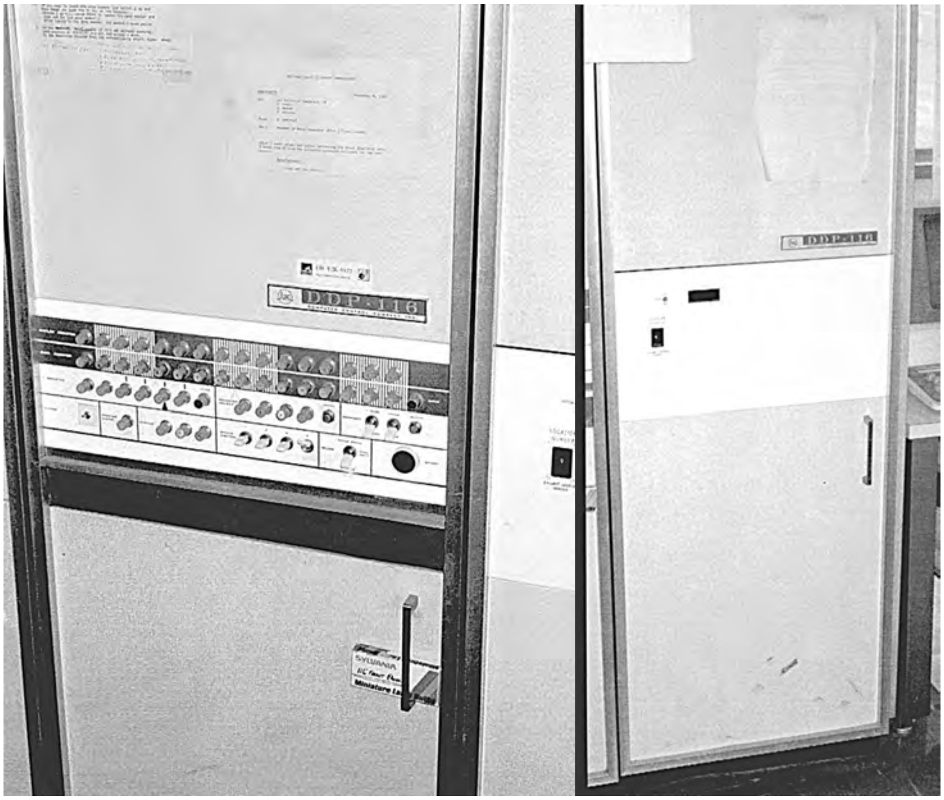


Fig. 6— 6A (left) The DDP-116 Interferometer Control Computer. This photograph was taken in June, 1998, approximately thirty years after the computer began operating the interferometer. The bootstrap loader is keyed in by hand following the recipe written on the inside of the front panel, here shown folded down to reveal the recipe. A supply of panel lights is kept nearby, in the handle of one of the panels. 6B (right). The Rack with the Computer Memory. The black marks at the lower right are said to have resulted from an incident when B. Clark attempted to get the computer's attention.

Of course, Barry was heavily involved in the development of the interferometer system, at the same time as he was plugging along on VLBI. The central computer had the responsibility for pointing the antennas, switching the delays, and performing the first fringe reduction. A memo by Clark (1968) describes the nature of the data written first to a disk and then dumped on tape for later calibration and analysis. The computer in question was a DDP 116, already a bit past the state of the art. But apparently it was adequate, because it still runs the interferometer today,* as this picture from June 1998 attests (Figure 6A).

* *Editors' note: The DDP-116 survived the transition to the year 2000, and was finally shut down in October 2000 when the GBI was retired for lack of funding.*

It will be observed that this is a “proper” computer, complete with the NRAO Y2K computer number. The computer is rebooted by hand, by keying in a bootstrap loader. The magic formula is written down inside a panel on the front, shown here folded down in the display position. It is of course a major pain to key in the loader if there are burned out lights, so a package of replacement bulbs is kept near to hand, in the handle of one of the panels.

When I was at the Interferometer Control Building to take the picture of the computer rack, the operator suggested that I make a photo of the adjacent rack containing the disk (Figure 6B). Actually it is a pretty dull rack, and I wondered what possible interest there was. Well, said the operator, these black marks at the bottom right of the panel are said to be where Barry once had to speak sharply to the machine, with his foot. I can no longer remember the incident, but this is the story.

These were interesting times scientifically. I will just add that although the telescope was very slow by modern standards it nevertheless supported 40 programs per year, about 70 observers, and there were a number of theses based on the results from it.

4. Later Developments at the Interferometer

With the completion of the three-element system and the remote smaller antenna, the telescope was ready for scientific use. As far as the VLA planning was concerned, the interferometer had pretty much done its job. However, there was one other VLA-related problem. Acceptance of the VLA by the NSF was a slow and painful process, with occasional periods when it seemed that success would be less likely. As a contingency against the possibility that the VLA would be long-delayed or perhaps never approved, a plan to expand the interferometer was developed (NRAO Staff 1969). It envisioned a new roadway to give the complementary baselines needed to improve the beam (Figure 7), the addition of a fourth 85 Foot, and the addition of three 13-meter antennas. Fortunately, the VLA was approved and the development plan did not have to be implemented.

By 1970, a number of the smaller items in the expansion program were started, since they were clearly of benefit to the users. A dual frequency system at 2695 MHz and 8085 MHz was incorporated, permitting spectral index and polarization work, and allowed the astrometrists to separate ionosphere from troposphere. Spectral-line capability at 21 cm was added, with Eric Greisen producing the line manual. And the remote antenna was upgraded to permit it to work at 8 GHz.

However, once the VLA was approved, the efforts at upgrading the interferometer were much reduced. When the VLA capability significantly exceeded that of the interferometer, the interferometer was closed as a general user instrument. This happened in October 1978. At that point, the VLA had 13 antennas, and was operating over baselines of as much as 12 km. Thereafter the interferometer was used for many years by the USNO, for timekeeping, and by the NRL for source variability studies. It is currently monitoring X-ray sources as a two-element interferometer, under an arrangement with NASA.

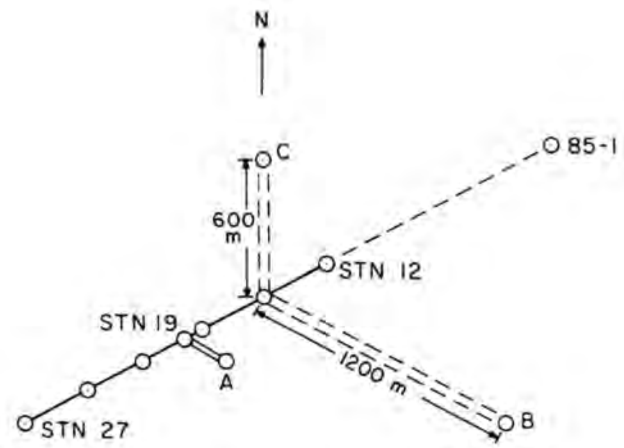


Fig. 7— A Proposed Expansion of the Green Bank Interferometer. In case the approval of the VLA was delayed too long, a plan was developed to expand the interferometer by adding additional baselines, stations, and antennas.

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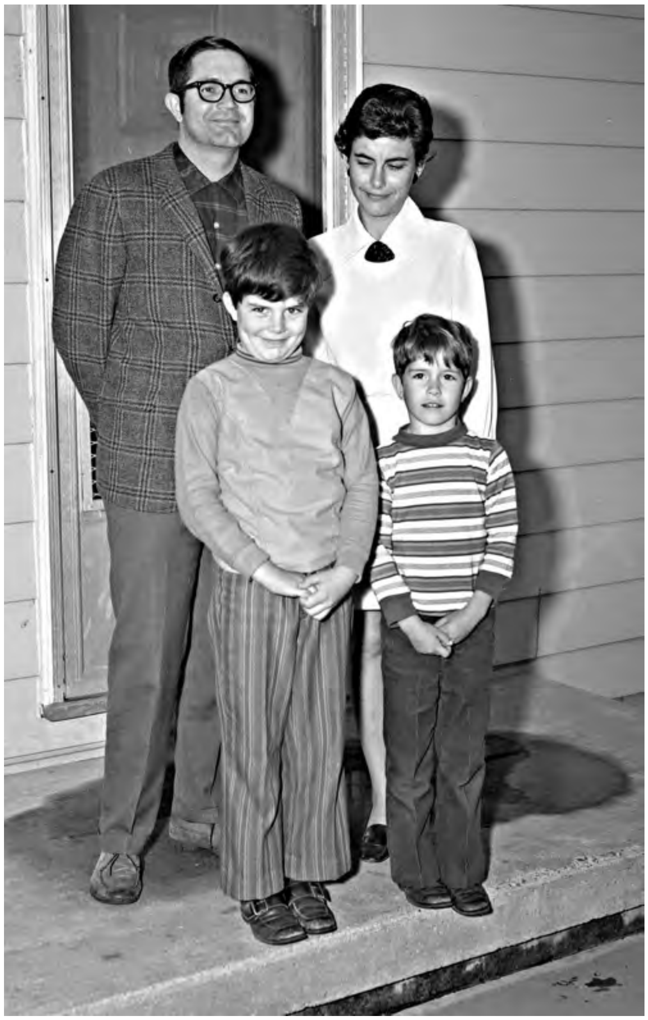


Dave and Carol Hogg, and sons Brian (left) and Doug, Green Bank, 1970.

6 The Search for the "Mad Ann" Meteorites

Editors' note: The following two articles from the Observer of September and October of 1962 describe the meteor sightings and search for fragments on "Mad Ann" ridge in Virginia. The articles are unsigned, but may have been written by Dewey Ross, who was then the editor of the Observer.

The Mad Ann Meteor
From the Observer, September 30, 1962

Over the last month, the Observatory has been conducting research aimed at finding fragments of a bright fireball which passed near Neola on September 1, about 11:12 P.M. We became aware of this through Jim Dolan, who saw the fireball while fishing in the Greenbrier River near Renick. The fireball streaked across the sky from west to east; its size and brightness being more than the full moon. A few minutes later a sonic boom reverberated through the hills near the site of the meteor, a result of the sonic boom or shock wave caused by the entry of the meteor into the atmosphere. The meteor was seen from Richmond to Charleston, and created the sonic boom from at least White Sulphur Springs to Marlinton. All these features, we know from experience, suggest that this fireball was the kind which deposits large fragments on the ground.

The study of meteorites has become very important recently. The most common meteorites, the iron meteorites, are of interest because they lock within themselves the isotopes of certain elements that have been created by the action of cosmic rays in space. By measuring the amounts of these isotopes, we can tell something of the history and age of the meteorite, which tells us of the history of the solar system. In the other common form of meteorite, the stony meteorite, organic chemicals similar to those in living things, and perhaps even remains of living things, have been found. The study of these objects in meteorites is much better done with newly landed meteorites, since they have not been contaminated by terrestrial material over the course of time. Therefore, it is very important to find a meteorite soon after it falls, if possible. Furthermore, there is a great shortage of meteorites for scientific study at present. All these things made it important to find any fragments that reached earth from the September 1 fireball.

To do this, it was first necessary to locate as many witnesses as possible, and to interview them right on the spot where they saw the meteorite. Ads were placed in all the newspapers where witnesses might be located. This brought about 100 replies. The best of these were interviewed by W. Oref and F. D. Drake. They took a special compass and made accurate measurements of the direction in which the witnesses had seen the meteor. Sometimes this was not easy. One witness was in a canoe in the middle of the Greenbrier River. Another was standing in the middle of the river fishing. Since it was necessary to take bearings and elevations from the exact positions of these witnesses the interviewers had to go out in a boat in one instance, and wade out into the middle of the river for the other.

Information derived from the interviews was evaluated and calculations made. The data indicated that the fireball was 33 miles high when first sighted and, seconds later, 4 miles high when it burned out. The fireball was moving at a velocity of 10 miles per second in a west-east direction. Its angle of descent was about 25°.

Because fireballs spray debris in an elliptical pattern and because information derived from eye-witnesses is not always precise, a 20 square mile area will be searched. The indicated impact area —Mad Ann Ridge.

The first search for the meteor will be by helicopter. The Smithsonian Institution of Washington will provide the helicopter for the search.

Meteorite, Meteorite, Where Art Thou??

From the Observer for October 31, 1962

The search for the Mad Ann Meteor ended on September 29, 1962 after an air search by helicopter. For two days meteorite investigators flew the probable impact area near Falling Springs, Va., looking for features usually associated with crashing meteors. Except for several manmade features, nothing resembling such features were seen. By evening of September 29 the investigators were convinced that the probable impact area had been thoroughly searched and they ended the search.

The Mad Ann Meteor started a very promising prospect. Many people had witnessed the descent of September 1 "fireball" into the earth's atmosphere and reported their observations to the Observatory. Eye witnesses were interviewed and data obtained from them was used to calculate the probable impact area. Allowing a margin for observational errors, an area much larger than calculated impact area was selected to be searched. Subsequently this designated area was searched.

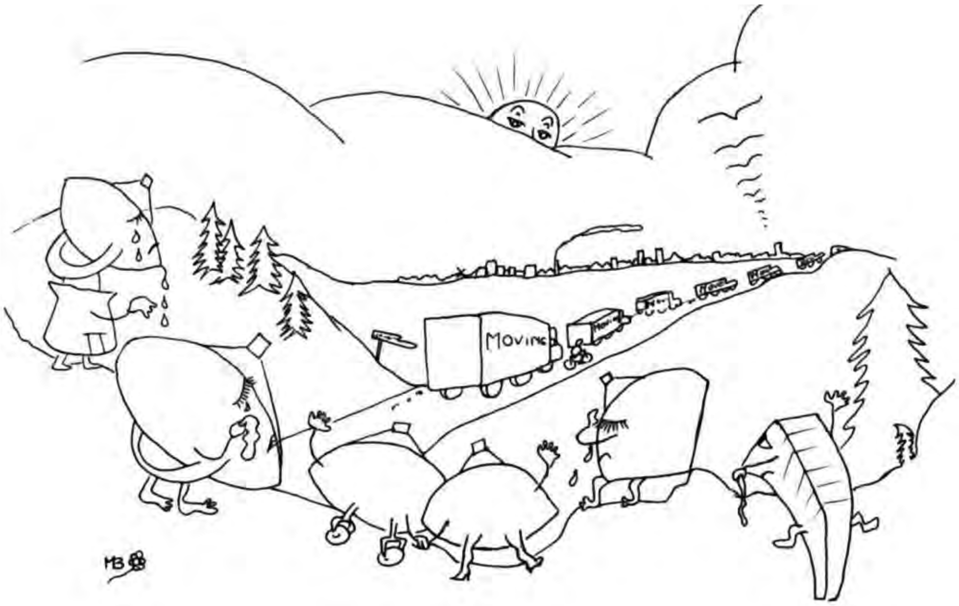
Many eye witnesses said that a sudden illumination of the sky made them look up. Others said that they had heard a strange hissing sound first and then looked up. Almost all of them agreed that it appeared as large as a full moon and as bright as one. Objects were made to cast shadows and a few said that you could have read a newspaper. The majority confirmed a "sonic boom" (the meteor's collision with the Earth's atmosphere) immediately after seeing the ball of fire, but only a few had witnessed the "fireball" disintegrate into about two dozen fragments. To date none of these fragments have been found.

There was some humor associated with the "fireball" investigation. One witness confessed that she thought it was a burning rocket plunging earthward and imagined men trapped inside. It frightened her so that she dashed inside the house screaming, "They're falling, they're falling."

Another said, "He thought it was them durned neighbors kids settin off rockets."

In several cases it was impossible to convince witnesses that it was a meteor that was being searched for. They were convinced that the search was for parts of a space ship or a man-made rocket.

Wild goose chases, erroneous reports, spectacular imaginations, and frustrations are part of the meteor search game.



Cartoon from The Observer, *December 1965, drawn by Marja Baars*

7 Exodus to Charlottesville

In late 1965, many of the scientists and engineers began to move from Green Bank to the new NRAO headquarters in Charlottesville, VA., on the grounds of the University of Virginia. This was a traumatic event for the Observatory, but foreshadowed its eventual expansion to multiple sites. An account of this event in Science *contained a number of inflammatory statements, not included here. We reprint limited excerpts of the* Science *article, also of one that appeared in* The Charleston Gazette *from United Press International, and an editorial from* The Pocahontas Times *commenting on the attitudes expressed in the* Science *article.*

Excerpts from
"Scientists Find No Culture at Green Bank, W. Va."

by Robert M. Gornall, UPI, February 1966

Green Bank is a great place to visit but the "cultural" grass is greener on the University of Virginia campus.... It was chosen in 1956 because of what scientists call "radio quietness," an atmosphere nearly void of static or other interference which would hamper the collection of signals originating light years away.... But what the National Science Foundation and Associated Universities, Inc., did not bargain for was the normal quietness that goes with this town of rural West Virginians, one service station and a general store....

The closest bowling alley is at Elkins, as are, the nearest supermarket, garage for major repairs and hospital. At least twice, wives of observatory personnel have delivered babies on the road to Elkins. And pressure from wives has caused several scientists to leave their posts.

Excerpts from "NRAO Astronomers to Leave for City"

in Science, vol. **50,** p.722, November 1965
by Elinor Langer

Integrating a small, intellectually oriented community of scientists and engineers into the nonintellectual circles of Green Bank was, inevitably, difficult.... But if there have been difficulties, there has also been a good deal of co-operation.... Storekeepers welcomed the increased trade. Among the children of the two communities if not among the adults, there are reportedly spontaneous and unaffected relationships. What is driving the scientific staff to Charlottesville is not tensions in the community, but the scientists' reluctance to pay the personal and professional price that rural isolation, however great many of its charms, entails....

"It was essentially a scientific decision," said D. S. Heeschen, NRAO director, in a recent interview.... First was the problem of recruiting. "There were two or three people we wanted who just wouldn't come," Heeschen said....

[An] effect of isolation on the scientific staff was ... subtle. "People began nesting," said one observer. "They weren't getting out quite as much as they should, traveling, talking to people. This is bad in any field." Several staff members spoke of their longing for a more diversified academic atmosphere. "We spend all our time talking to each other," one commented....

[Time] will never erase the impression of the emigrants to Charlottesville that the best environment for science is not necessarily the best environment for scientists.

Editorial from the Pocahontas Times

November 25, 1965

Pocahontas County smarted a little from a report by Elinor Langor under News and Comment in November 5 issue of Science....

There were both nice things and unkind things said about the area and its people in presenting the paradoxical situation created by the need for a remote area for observation and a desire for urban so-called advantages and luxuries for the families. However the report quotes Dr. Heeschen as saying the decision to move was made for scientific reasons—they couldn't get the scientists they wanted to come to the rural area.

A little snobbishness was revealed by some of the author's remarks and this distorted the report, we felt, and hurt our feelings.

We are what we are and we can't change or be changed to be all things to all people. Let us try to improve the quality of our educational, health, spiritual, social, and economic conditions for a better living for our own people, but retain, and enjoy, the basic simplicity and beauty of our rural way of life and try not to be upset by others' comments.

8 Tourists at NRAO: early years.

Editors' Note: The public has been visiting the Observatory since its founding, and it was clear that facilities for accommodating visitors would be needed. The tour program ran on a rather meager budget with very modest facilities for the first forty years, despite occasional attempts to obtain funding for a visitor's center. At first there were no personnel dedicated to giving public tours; the Observatory staff handled visitors as best they could.

From a memo by Richard Emberson (January 21, 1959): a proposal to the Ford Foundation for tourist facilities.

1. At the January 13 staff meeting, brief mention was made of the possibility of obtaining support for the museum and associated activities from the Ford Foundation. Subsequently Lloyd Berkner discussed the matter with some of his friends there, and he reports a considerable amount of interest. . . .

2. The proposal will consist of a basic document plus four or more appendices, as follows:

- NRAO — what it is: purposes, programs; facilities, including site; future plans; scale of operations, including scientific visitor program.

- The public visitor problem — the attraction of astronomy and the NRAO facilities; the possible numbers; interferences with the NRAO research if not planned for and controlled.

- Plan for Public Education — Facilities needed: Buildings, lecture hall, exhibit area, planetarium, exhibits, parking lot and visitor arrangements, diesel transportation.

- Operation — Personnel: director, lecturer(s), receptionist. Sales: lectures, planetarium, movies, tours, postcards, books, etc.

- Site layout (assume 10 acres, but possibility of later acquisition of land to the south).

From the Monthly Summary of July 2, 1959 — *by Richard M. Emberson*

Public interest and visitation to the Observatory has been increasing steadily. During the past month or six weeks, we have been visited by groups of college, high school, and junior high school science students, plus a steady stream of individual visitors. This activity is rapidly assuming unmanageable proportions, and taking a disproportionate amount of the time of our small and already over-worked professional staff.

From the Monthly Summary of March 2, 1960 — *by Frank Callender*

An ever increasing number of requests from various groups for conducted tours of the Observatory have been received. These include inquiries from students from both high schools and colleges, scientific and technical societies, and newspapers. At the present time conducted tours on Saturdays have been scheduled as follows: three in March, two in April, one in May, one in June. Many more are expected.

From the Monthly Summary of May 6, 1960 — *by Otto Struve*

A heavy burden . . . falls on the shoulders of Mr. Frank Callender, who is taking care of several groups each week of college and high school students who visit the Observatory. Arrangements have been made with Professor Robert Fleischer to organize the public education program during the summer months, beginning on June 15. Mr. Fleischer will also take part in the instruction of graduate and undergraduate students during the summer. The number of applications from both groups of students exceeded the number that can be accommodated. We have decided to invite eight graduate and eight undergraduate students.

From a memo, July 1960 —

Effective July 6, 1960, the Observatory may be visited by the public at 2:00 PM each afternoon *except Mondays and Tuesdays.* There will be a lecture and an exhibit in the basement of the Jansky Laboratory, followed by a conducted tour of the Observatory.

In December 1961 Wally Oref was hired as the first public information officer.

From a memo, May 18, 1962 —
TO: All employees
FROM: Wally R. Oref
SUBJECT: Public tours of the Observatory

Our schedule from 1962–1963 is as follows:

June 1 — Sept 8	Wednesday through Sunday
Sept 8 — Sept 31	Saturdays and Sundays only
Oct 1 — May 1, 1963	Observatory closed to general public. Special group tours by arrangement.

Tours will begin promptly at 2:00 PM EST from the Karl Jansky Laboratory.

From the Observer for August 31, 1962 —

This month's leading question was this: "Why don't the radio waves go through the wire mesh surface of the 300-foot radio telescope?"

From the Observer for September 30, 1962 —

Daily tours ended on September 8, but weekend tours were given the rest of the month. About 300 visitors took the tour in September. The Observatory will be closed to the general public until about June 15, 1963, but special group tours can be arranged through the public education office.

Over 10,000 people visited the Observatory up to October 1. Visitors registering at the reception desk represented 28 states and 10 foreign countries. 66% of these visitors were from Virginia, Maryland, Ohio and Pennsylvania.

The season ended with people still confusing the NRAO with the defunct Sugar Grove installation. Even after the difference was explained, they wanted to know why the NRAO wasn't obsolete too. Ah, people, the sweet mystery of life.

From the Observer for June 28, 1963 —

TWO DIESEL BUSES ADDED TO OBSERVATORY FLEET — If you see a blue-bodied bus with a silver top roaring down route 28, don't flag it to pick you up and drop you off at the next corner. It's not the Cass-Green Bank-Arbovale Express. It's only one of the NRAO tourist buses on its regular tour route. The captain at the wheel will either be Merritt Gum or Clifford Barkley, depending on which of the two buses you see at the time.

The Observatory has acquired two diesel buses for touring purposes and in doing so has struck another blow against man-made radio noise. Tourist cars with their noisy ignition systems will sit in the parking lot while their occupants take a care-free tour of the site and facilities.

After the lecture and movie in the conference room the buses are scheduled to leave the Jansky Laboratory at 3:00 pm on regular tour days. From the laboratory they will go to the calibration horn, 85-foot, 40-foot, and 140-foot by the way of the main Observatory road. They will travel to the 300-foot over the back road, past the base of the 140-foot, and return to the laboratory by the high school road and route 28. Tour time by bus is one hour—give or take a few minutes either way.

From the Observer for September 30, 1963 —

TOURS AND TOURISTS — The regular tourist season began June 15, 1963 and ended September 2, 1963. During this time 8,160 visitors were registered at the reception desk. Visitors came from 42 states. Of the 42 states represented, 84 per cent came from West Virginia and four nearby states. West Virginia contributed 66 per cent of the visitors and Virginia, Pennsylvania, Ohio, and Maryland contributed the other 18 per cent. The summer daily average was 138 visitors per tour day.

The largest single day's registration occurred on September 1, 1963 when 512 visitors were registered. The 8,160 visitors who were registered at the reception desk this summer do not tell the whole story of observatory visitors. This is because it was not possible to count the non-registering visitors or the people who drove in and out of the observatory too early or too late for the regular tour. However, periodic counts of non-registering visitors indicated that for every person who registered,

The New Tour Buses in 1963. (L to R): Wally Oref, Clifford Barkley, Dorothy Drake, and Merritt Gum

at least one other person did not register. Using this as an estimating guide, it is probable that at least 20,000 persons visited the observatory this summer.

From the Observer for September 30, 1964 —

10,215 people visited the Observatory this summer. 41 states and 4 foreign countries were represented by registered visitors. 70 per cent of these visitors were West Virginians. Over 4,000 visitors came in August. Average number per day for the season was 157, rain or shine. Most popular day of the week for visitors was on Sunday.

Largest single day's attendance was 361 on Sept. 6. Compare the increase of attendance over the years:

1960	2,490
1961	5,472
1962	5,887
1963	9,032
1964	10,125

Under present conditions, this is the maximum amount that can be handled by Mr. Oref and his group.

From the Observer of October 30, 1964 —

The following is a compilation of interesting letters requesting information from Wally Oref, NRAO's Public Information Officer:

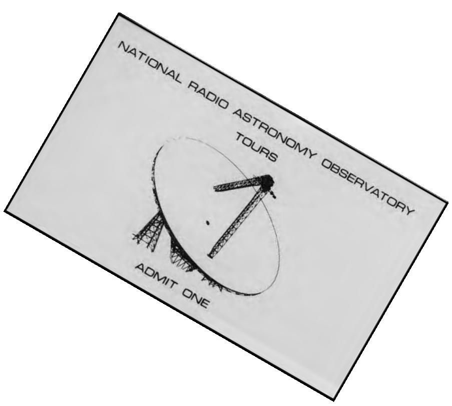
Dear Sir:
I am interested in space. Please send me some.

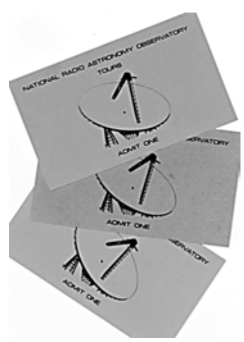
Dear Sir:
I have reason to believe that there is life on the back side of the moon. Will you please aim one of your telescopes to this area. I would appreciate the results as soon as you receive them.

Dear Sir:
Would you please ask the head astronomer (or equivalent) what the chances are of Haily's comet colliding with the [Earth in] 1986. What is the size. This may be just a roomer, but, my friend told me that he read it somewhere.

Dear Sir:
I am a high school student. In the school, I am taking a course in Astronomy. I would like to know if it is possible to send me information on Radio-Telescope, receivers at a freq. of 21 cm. or 2100cm. I would like to build a receiver at this freq. or conveter that would cover the freq. If it is possible can you please send me information and plans how to build the receiver and a small antenna system, or converter. Thanking you in advance.

Dear Sir:
When crossing the Brooklyn Bridge, I've noticed that the volume of my radio increases. Can such bridges be used for large radio telescopes?





Plans for better tourist facilities.

Editors' Note: Despite good efforts by the Public Information Officer, bus drivers, and summer students (whose duties for many years included giving tours), the public education program was sometimes regarded as inadequate. Consider for example, the opinions of Sandy Weinreb (the head of the Electronics Division) in 1967.

Memorandum to: D. S. Heeschen
From: S. Weinreb
December 11, 1967

The problem is our public education program—I think it stinks!
I believe we should have an active, effective public education program for three reasons (listed in order of importance):

1) The product of astronomy is knowledge of the universe. This knowledge should be shared with the society that supports the astronomer. I believe that most people, no matter how uneducated they are, are curious about the universe and want this knowledge at a level they can understand. This knowledge has already had and will continue to have a tremendous but very slow effect upon society.

2) The long-term financial support of radio astronomy will benefit.

3) As the National Observatory and with our summer tourist location we are in the best position to take this responsibility.

I would make the following suggestions ...

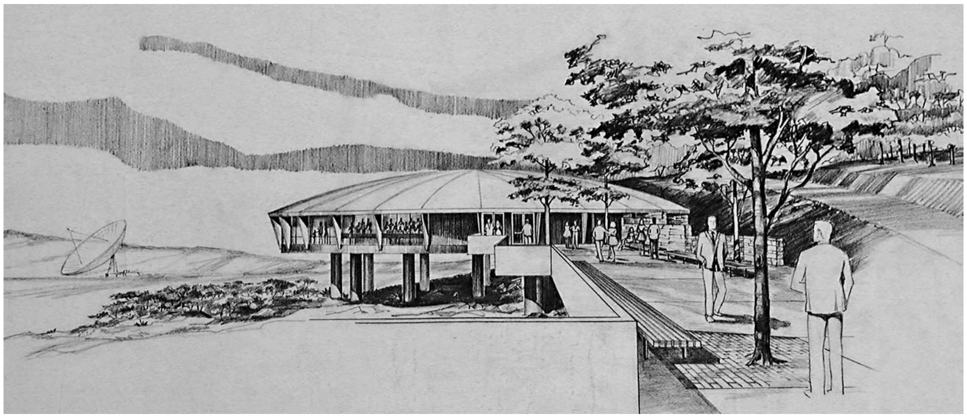
1) In addition to a standard program for casual tourists, an optional, more advanced series of movies and exhibits should be provided for the visitor with more education or interest.

2) We should make some movies of astronomy lectures by good speakers.

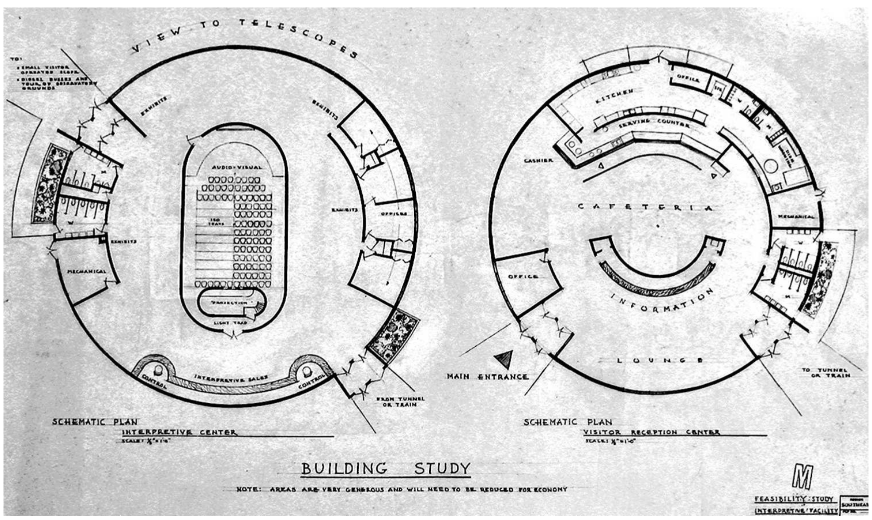
3) I think that a recorded lecture at each telescope would be better than the dull monotone the bus drivers give. . . .

4) A new building with beautiful grounds would certainly help. . . .

5) Why not build a radio-planetarium which would project the sky as seen with radio-eyes?



Conceptual design of a visitor center, shown in these architectural drawings done in 1968. There would be two saucer-shaped buildings. One a reception center, from which visitors would walk through a tunnel to the interpretive center, which would have an auditorium and exhibits. This project was never funded.



The floor plan for the interpretive center (left) and the reception center (right).

From the Observer of November 1970 —

Tourist Program, by Wally R. Oref

This summer a record number of people will have visited the NRAO. Up to October 18, 20,380 people took the regular tour.

We expanded the tour program this year. The number of tour days was increased to 91. Last year we only had 52. This summer we offered 13 tours each day. Last year we gave only 5. In 1969 the tours ended on August 24, but this year we ended daily tours on September 7 and ran weekend tours until October 31. By giving daily tours, more tours per day, and extending the tour season, we feel we accommodated most of the visitors who wanted to see the Observatory.

The content of the tour program was also increased. Three information panels were placed at the tourist center, and one each at the 2-foot, 140-foot, and 300-foot telescopes. A 2-foot radio telescope observing at 22 GHz was added to the program to show how a radio telescope works and to explain how one is used. Stereo tape systems were installed in buses this year and a tape narration of the tour used.

Questionnaires filled out by visitors showed that they particularly liked the 2-foot, the movie, and the large telescopes. However, visitors said that they were disappointed because they could not visit a control room or get closer to the larger telescopes (because of construction at the 300-foot and painting at the 140-foot).

The new Green Bank Science Center, opened in the Spring of 2003.

With the opening of the new Science Center in May 2003, the Observatory now has a first-rate tour and public education building. The building includes an exhibit hall with facilities for tours, an auditorium, and classrooms for student groups and conferences. West Virginia Senator Robert C. Byrd obtained an appropriation for the construction. The design and construction of the exhibits were funded by a grant from the NSF. All this came about primarily due to more than a decade of persistent efforts by the NRAO Education Officer, S. A. Heatherly.

Cartoon from the Observer, July 31, 1963

The Green Bank cafeteria, 1977. Left to right: Pat Crane, Beaty Sheets, Barry Geldzahler, Dave Shaffer, Ken Kellermann, Berdeen O'Brien, Louise Riley.

Bob Vance contemplating a VLBI tape malfunction.

9 The First U.S.–U.S.S.R. VLBI Observations

K. I. Kellermann
NRAO

Editors' note: The following material is excerpted from a series of articles which appeared in the NRAO newsletter "The Observer" starting in 1970, describing the first VLBI observations made between the Green Bank 140 Foot Telescope and a 22-m radio telescope located in Crimea in the U.S.S.R. These experiments, which took place during the depths of the cold war, were the beginning of three decades of collaboration between American and Russian radio astronomers which continues today with joint preparations for the RadioAstron space VLBI mission. As the original story was written for Green Bank readers in 1970, the author has added some additional explanatory material for the general reader of today.

Following the successful completion of the first transatlantic Green Bank to Sweden 6 cm VLBI observation in February 1968, the NRAO-Caltech VLBI group began to look for new, exotic places to visit. A U.S.-Australia baseline appeared attractive as being the longest reasonable physical baseline on the earth where a radio source could be simultaneously seen from both ends. However, it was clear that to obtain really high resolution it was necessary to go to short centimeter wavelengths, and at that time the only radio telescopes of sufficient collecting area capable of operation at short centimeter wavelengths, and located far from Green Bank, were in the U.S.S.R.

In February 1968 Marshall Cohen, then a member of the UCSD faculty, and I wrote a letter to Professor Victor Vitkevich, Director of the radio astronomy department of the Lebedev Physical Institute. We proposed to try VLBI observations between the 140 Foot radio telescope and the 22-meter precision reflector located at Puschino, not far from Moscow. Although we were aware that there might be some political and practical problems in doing a VLBI experiment in the U.S.S.R., I don't think any of us anticipated just how difficult it would actually be.

Initially, there was no response from the U.S.S.R. But, five months after our original letter, we were surprised to receive a telegram from Moscow followed by a letter from Dr. Leonid Matveyenko saying that Prof. Vitkevich did not answer because he was on vacation! Somewhat to our surprise he replied that the U.S.S.R. Academy of Science had given tentative approval to the proposed experiment, except that they recommended we use the new 22-meter telescope in Crimea rather than the one in Puschino, because (a) it was a better telescope, (b) it is a better baseline, and (c) the weather is better in Crimea than Moscow. Also, he proposed an exchange of personnel to discuss plans for the experiment.

Somewhat encouraged, we invited Dr. Matveyenko and Dr. Ivan Moiseyev, director of the radio astronomy station in Crimea, to visit us at NRAO. At the same time NRAO Director Dave Heeschen wrote to the NSF asking permission to bring our VLBI equipment to the U.S.S.R. But a few weeks later the U.S.S.R. invaded

Czechoslovakia and, not surprisingly, there was little enthusiasm in Washington for sending high speed tape recorders and precision atomic frequency standards to Russia. Nevertheless, Matveyenko and Moiseyev were able to obtain visas to visit the United States and arrived in Green Bank in January, 1969. We tentatively agreed on doing two experiments—one at 6 cm and one at 3 cm. Because the weather in Crimea was said to be poor in late autumn and not appreciating the enormous logistical effort that would be involved, we agreed to make our first try not later than October of 1969, just nine months later.

Fortunately, by this time Russia was behaving itself politically, so we moved ahead with making firm plans. However, we immediately ran into difficulty trying to obtain an export license from the U.S. Commerce Department. There were two issues to be settled. Our HP Rubidium atomic clock and "high speed" tape recorder (556 bpi, 720 kbits/sec) were considered sensitive equipment with military potential and were not allowed to be exported to "iron curtain" countries. Additionally, we were surprised to learn that there was concern about one of the by-products of VLBI observations—the determination of the distance between the two antennas to an accuracy of a few feet or better. We were told by the experts at the Defense Department that a 100 megaton H-bomb which landed a few feet from the 140 Foot telescope could do more damage than one which landed a few hundred feet away. We later learned that the Soviet government was similarly concerned. But as a result of frequent prodding by NRAO and the NSF, the Commerce Department finally granted us an export license in August, only a few days before I was scheduled to leave NRAO for the first observations.

On September 10 I arrived in Moscow with my wife and was met at Moscow's Sheremetevo Airport by Leonid Matveyenko. After retrieving our luggage, Matveyenko told the customs officer, "Akademy Nauk" (Academy of Science) and we passed through customs without any inspection or formality. We later found much use for the phrase "Akademy Nauk" which indicated that we were guests of the Academy of Science, and would open any and all doors from the Crown Jewels to last minute reservations on a Russian airplane.

Our equipment was scheduled to arrive from the United States on September 15 and Matveyenko was dispatched to the airport to collect it. He returned that evening and reported that he needed the "baggage ticket." I tried to explain that you don't get a baggage ticket with freight and that in any case I was already in Moscow when the shipment left the United States, and I could not possibly have any of the papers prepared when the shipment left. This appeared to cause some concern amongst our Russian colleagues.

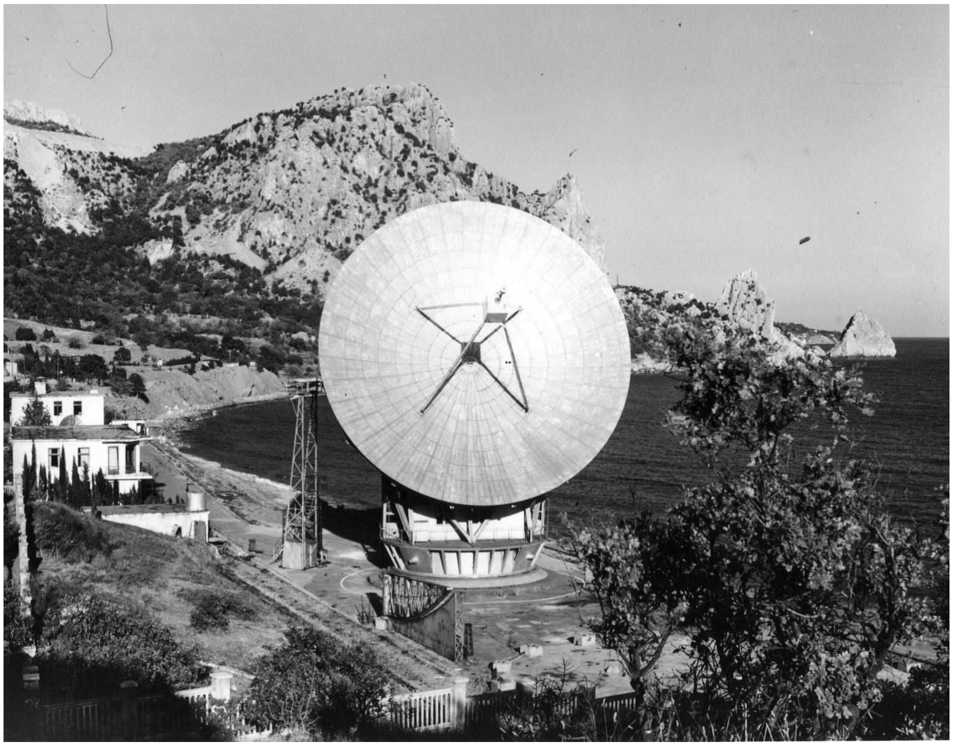
The following day we obtained a letter from the Academy of Science addressed to the airport cargo and customs people saying that it was OK for me to collect our equipment. Matveyenko and I drove to the airport in a car and were supposedly followed by a truck which was to carry everything back to Moscow. Following about an hour of being sent from one office to another and several heated discussions, we were led to a shed that contained the equipment (or as all Russians insisted on calling it, the "aperture"). Matveyenko appeared to be a little surprised at the size and weight of our "aperture" which consisted of three, large wooden crates plus 25 boxes of magnetic tapes weighing a total of about 3,000 pounds.

For some unknown reason we had to wait about two hours for the truck to arrive. But this was good practice as we were to spend a good part of the next few months waiting for someone or something, and, when the truck arrived, it turned out to be about the size of a VW bus (in fact, it was exactly identical to a VW bus except instead of the letters "VW" on the front were two Russian characters). The "VW bus" could barely hold the smallest of our three crates. It took 6 or 8 big Russian men, who looked to be the local equivalent of the Russian teamsters union, to get one crate loaded onto the truck with a lot of pushing and groaning. I drove back to Moscow in the car, being assured that they would handle the rest and get it to Moscow.

Much to my surprise, all the "aperture" somehow appeared the following morning in the basement of the Moscow University Sternberg Institute where John Payne and I had prepared to set up shop. John was a Green Bank Engineer who possessed a wide range of skills which were to serve us well. He had arrived a few days earlier in Moscow and we were looking forward to opening the crates and seeing if everything had survived the flight from the U.S. before shipping it on to Crimea. We knew that a complete set of tools including a variety of screwdrivers and test equipment was included in the crates, so that we would not have to depend on unfamiliar Russian equipment. But we had not thought about how we were supposed to open the crates, and it took a while to locate a local screwdriver so we could open the box to get at our own tools. When one was finally found, it was so worn it had more of a point than a blade. Much to our surprise, everything seemed to be intact.

As we worked, we were informed that our boxes were too big to fit in the cargo door of an Aeroflot airplane to be flown to Crimea. There then followed a big discussion (Russians seem to like big discussions) as to whether the "aperture" should be shipped to Crimea by truck or by railroad. We had considered three methods of synchronizing the time between Crimea and Green Bank. The most straight forward would have been to transport a running clock, but it appeared that this would involve complex technical and administrative arrangements. The simplest method was to synchronize a clock in Crimea with radio signals from a U.S. Loran station located on the north shore of Turkey, just across the Black Sea from Crimea. Unfortunately, however, unlike Loran stations in other parts of the world, the Turkish station was not officially synchronized with UT and it wasn't clear if we could depend on it for accurate time. As a backup, we had planned to use the Loran signals from a station located on the island of Sylt, located off the north coast of Germany. We knew the German station was accurately synchronized to UT, but it was too far away to be received from Moscow reliably. So we agreed to send our Rubidium clock and VLBI control unit to Leningrad to be synchronized with the German Loran station, which was on a clear path across the Baltic Sea. Meanwhile the tape recorder, receiver front ends, and the 25 cartons of magnetic tape, were to go directly to Crimea. After considerable discussion, it was finally decided to send the shipment to Crimea by train and the one to Leningrad by truck.

A minor difficulty developed when we were told that the maximum weight per item allowed on the Russian railroad is 100 kg, and our tape recorder together with crate weighed about 500 kg. But after obtaining a letter from the "Academy" the rule was changed, and our tape recorder departed for Crimea.



The 22 meter radio telescope in the Crimea.

The VLBI timing equipment was scheduled to leave by truck on the morning of September 18 for Leningrad. By noon the driver had not yet appeared at the Sternberg Institute and a subsequent investigation revealed that he had gone to the airport instead. He was quickly recalled and dispatched to Leningrad with strict instructions to proceed directly and not to stop for rest until he arrived (Moscow to Leningrad is about 400 miles over a poor road).

That evening the whole VLBI party—John Payne, Leonid Matveyenko, Leonid Kogan (then a young engineering graduate student who had been assigned to the project), my wife, and I flew to Leningrad aboard an Aeroflot TU 104 jet. We carried in the airplane our atomic clock which we hoped to synchronize in Leningrad with the German Loran signals. Arriving in Leningrad we were met by a delegation from the Pulkovo Observatory where we planned to set up our Loran equipment.

Following a sleepless night in an unheated hotel room with the temperature near freezing, we set out for Pulkovo. The night was sleepless because one of the banks of floodlights that was used to illuminate the side of the building was located just under our window. By this time, we were beginning to learn that the mighty Soviet Union was not the perfect machine that American propaganda had suggested. So, we were not too surprised to find that our equipment had not yet arrived. It finally did show up later in the day, and again we opened up the crate before a large audience, but this time with our own screwdriver which I had carried in my pocket to Leningrad. Everything had been modified to work off the Russian 230 volt system, which turned out to be closer to 200 volts, so we had to find a transformer. To insure continuous operation of the atomic clock we also had brought a 28 V DC battery supply with an advertised capacity sufficient to last about 75 hours.

Lacking a proper Loran antenna we strung a wire across the floor and promptly received what appeared to be the Loran transmission from Sylt, Germany. However, the Sylt Loran station was supposed to transmit with a 79.6 millisecond period and the signal we were receiving had an apparent period of 80.0 milliseconds. There seemed little doubt that this was an actual Loran signal since it clearly had the characteristic 8 sub-pulses per main pulse, with each sub-pulse separated by the usual 1 millisecond. We therefore assumed that something must be wrong with the VLBI counting circuitry and spent the next two days trying to isolate the trouble. In the course of checking the VLBI unit we used a variety of Russian test equipment which often turned out to be identical (except for the lettering on controls like the Russian "VW bus") to some piece of American (usually Hewlett Packard) equipment. After wasting two days and convincing ourselves that everything was working properly, and rejecting the unlikely possibility that Loran had changed its period without announcement, the light finally dawned: we were not receiving Loran at all, but an unadvertised (we later learned secret) Russian Loran-like imitation. The real Loran signal was found buried in interference from the most powerful transmitter in the U.S.S.R. located only a few miles away broadcasting entertainment to the Soviet ships all over the world. John considered building a filter but it was clear that it would take a week to dig up the necessary parts. We did manage to find a very ancient receiver which had fair selectivity which we tried unsuccessfully to use as an RF preamplifier. Another few days were wasted trying to dig the Loran signal out of the background with a variety of antennas in various locations and orientations. Most of the time, however, was spent repairing the Russian radio we were using as a preamplifier, which kept breaking down in one way or another. It was rumored that at various odd hours of the night on certain days of the week the interfering station would temporarily stop broadcasting, but this never materialized, and it was becoming clear that we were not making much progress.

We had previously explored the possibility of flying a running clock into the U.S.S.R. but our Russian colleagues in Moscow indicated that this would be "impossible." However, our Leningrad colleagues were more optimistic and thought that it might be arranged. On Saturday, I telephoned Bert Hansson in Sweden, one of our collaborators on our earlier VLBI experiment between Green Bank and Sweden. Bert was asked if he could synchronize a Rubidium clock in Stockholm and send it to Leningrad. Bert responded that (1) they had no batteries, (2) it was a weekend and there was no one around to prepare a proper shipping crate, nor was it possible to buy batteries on the weekend, and (3) Sweden had just experienced a major storm which blew down an antenna at their Observatory and had damaged the director's yacht. Nevertheless, Bert promised to "see-what-he-could-do."

Meanwhile, the first observations were only about a week away and we hadn't even been to the Crimea site yet. John decided to go alone to Crimea to set up the VLBI recording equipment and install the front ends. We knew that the Soviet telephone system was unreliable, so we had planned to use a specially installed

TWX machine* for communication with Green Bank. I stayed behind in Leningrad to continue the struggle with the Loran receiver and to await the clock from Sweden.

The VLBI timing equipment rack was scheduled to be sent by train to Crimea at midnight on September 23. I planned to spend all that day in a last minute attempt to receive the Loran signals. Leonid Kogan told me that the Soviet railway required that the crate be at the station not later than 10 PM in order to make the 12 PM train, and then he went off to make the final arrangements for the shipment. About 3 o'clock he burst in while I was still unsuccessfully struggling to receive Loran and announced that he had learned that we had to be at the railway station by 5 PM not 10 pm. There then followed an unprecedented flourish of activity and in the record time of 15 minutes, the 6-foot high, 300 pound rack was nailed shut into its crate, placed in the truck, and off we sped to the Leningrad train station. And speed, we did, because we were immediately stopped by the police for speeding. This was the second time out of a total of five that I was in a car in the U.S.S.R. that was stopped for speeding. But Kogan simply explained that he was accompanying an important American delegation and we were let off with a warning. The driver then got lost trying to find the station for cargo and drove all around Leningrad, and we finally arrived a few minutes before the 5 PM deadline.

It took a crew of about 8 rugged looking Russians to move the crate from the truck to the railroad car. They struggled, moving it in short spurts singing a "heave-ho-tovarishch" before each shove. To make things a bit miserable for all, it was cold and raining. Finally, after getting the crate in the railroad car, they broke out in a big happy smile and someone produced a little, square bottle (we would become very used to this little bottle in the coming weeks), and everyone took a quick drink. I asked Kogan what it was they were drinking and he replied "alcohol." I explained I understood that, but, what kind of alcohol? But, he only repeated "alcohol." After going around on this a few times, I finally realized it was exactly that— pure alcohol. Well, not quite pure, but 90% pure (180 proof).

Having no success with the Loran receiver and not hearing from Sweden, things looked a bit grim, but on the night of September 24 we went anyway to the Leningrad airport to meet the Aeroflot flight from Stockholm. To our pleasant surprise there was a heavy wooden box addressed to me, strapped in a first class seat with a safety belt. Of course, the Russian customs official (customer, as Kogan called him) wanted to see what was inside. We handed him some official looking papers of explanation, and opened the box. (I still had my screwdriver.) He took a quick look, saw a blinking red light, which told me that the main battery had run down and the clock was running on a limited capacity backup emergency unit. He gasped with astonishment at the loudly ticking clock and said "OK. Leave!" We quickly departed before he could change his mind.

At the Pulkovo Observatory, we synchronized the NRAO clock to the Swedish clock and connected a Russian car battery to the Swedish clock in case of power failure and left it running at the Observatory. We charged our NiCd batteries and set off for the airport to fly to Crimea. In order to preserve the NiCds for the flight,

* *TWX was a prefax and pretelex system for text communication which was operated by AT&T and which used a traditional teletype terminal for sending and receiving.*

we also carried two 6-volt car batteries and an inverter to supply 230 VAC. This combination gave us a battery capacity which was good for about 25 hours, more than enough (we thought) for a two and half hour plane trip.

The whole load weighed about 200 pounds and it took some explaining to get it on the airplane. The flight was uneventful and upon arriving in Simferopol, the capital of Crimea, we were met by Moiseyev who took us on a two-hour winding drive through the mountains to Yalta. As it turned out this was the first of 10 such trips I made between Simferopol and Yalta. In Yalta we were greeted by John Payne with the news that:

1) We had no communication with Green Bank as the TWX machine could not be connected because the lines were not good enough.

2) The 50 ohm, 10 dB loss local oscillator cable which was supposed to be installed between the control building and the antenna focal point was actually 72 ohms and had 20 dB loss so we could not get enough LO signal from the control building to the antenna.

3) He could not receive the Loran timing signals from Turkey, just across the Black Sea.

But the real blow came when we opened the box containing the atomic clock which we had laboriously synchronized and transported from Leningrad. Much to our chagrin, the clock had stopped on the airplane half way between Leningrad and Crimea. Apparently, the NiCd batteries had lasted only about an hour.

This was by far the low point of the expedition. We had no time, no local oscillator, and the first observations were only 5 days away. By this time I was so confused; I had lost track of the days and told everyone we had to observe in 4 days.

The time problem solution seemed straightforward enough—someone had to carry the Rubidium clock back to Leningrad and synchronize it with the Swedish clock that was still hopefully running at the Pulkovo Observatory. But in the U.S.S.R. one does not just go to the airport and buy a ticket for Leningrad, particularly if you happen to be carrying an atomic clock (size about 4 x 2 x 1 feet, weight 150 pounds). To make matters a bit worse, it was the end of the tourist season and the planes leaving the Crimea were booked solid. Matveyenko was assigned the task of somehow obtaining two return tickets from Crimea to Leningrad.

The communication problem was already being worked on. As soon as John Payne had arrived in Crimea and learned that the TWX was not working, he began to put pressure on our hosts to get it fixed. Actually, a TWX machine had been installed and appeared to be operating. But there were no lines from the Observatory to Yalta to handle TWX messages or telephone calls. Attempts at telephoning resulted in about half of the words being unintelligible. We were not surprised to learn that calls to "far off" Moscow (not to mention Green Bank, West Virginia) were nearly impossible.

The man who was responsible for arranging the TWX was an assistant director of the Observatory. Unfortunately, he had been having some heart difficulties and had been sent to one of the local sanitariums to recuperate. Since he seemed to have played a critical role in the arrangements, John Payne and Observatory Director

K.I. Kellermann in 1969.

Ivan Moiseyev went to the sanitarium to get some help. Upon arriving they learned that the poor man's electrocardiogram had gone off scale, to which Moiseyev replied that all he needed was a 10 dB pad. He was hauled out of bed to negotiate with the telephone company. Later Matveyenko and Moiseyev put on their Sunday clothes and went to confront the phone company. For the next few weeks whenever either Matveyenko or Moiseyev showed up in a suit and tie, we knew it was time for another session with the telephone company.

Nothing ever came of these deliberations, however, and we never did get the TWX working at the Observatory. It was only a few days from the first scheduled observations, so we thought it would be a good idea to telephone Green Bank and let them know we were still alive. Since the phone line to the Observatory was clearly marginal we called from our hotel in Yalta. The international operator stated that she could place the call at midnight. At 1 AM the phone rang. No answer. Back to sleep. Half an hour later the same sequence. After a few such go rounds someone finally spoke to me and in a mixed broken English-Russian conversation we learned that the call would come through the following night. That evening, again after a few false starts, the message was "Amerika Nyet." We were told that the telephones were out of order, and it would be at least two weeks before we could call the U.S.A.

The only means of communication left was telegram. At the telegraph office I waited in a few lines, filled out a telegraph form only to be told it was the wrong form, was handed a special form for international telegrams, and was led to the international desk, where the "chief" spoke French (which was no help to me). She examined the telegram, looked in a big black book, and indicated she couldn't find Green Bank in her directory of U.S.A. "cities." It took a while to explain that Green Bank was not a major U.S. city and the telegram went off, or so we thought. Four days later the telegraph office called me at the hotel. They still wanted to know where Green Bank was.

Meanwhile, John Payne was trying to get our 3 cm front-end installed and running on the telescope. Although there was an ample crew of local telescope mechanics, John was having considerable trouble getting them organized. They kept telling him that this was Russia, not America, and he should relax, have some vodka, and not be in such a hurry. John was running around tearing at his hair, mumbling "why didn't we bring Omar Bowyer?" After three or four days the local mechanics did manage to get all six bolts in, to attach the receiver to the telescope.

Finding there was insufficient local oscillator signal reaching the mixer, John and I decided not to experiment with various cables but to move the VLBI equipment over to the telescope. It took some courage to announce this decision, because the control room was on the second floor of the building, and it had taken the better part of a day to get the two heavy VLBI racks installed. But our Russian friends took it in good spirit, and in order not to damage the equipment insisted on repacking everything in the crates. Unfortunately, the crates had been damaged in opening them up, and the better part of Friday morning (Sept. 26) was spent rebuilding the crates. Getting the crates down the stairs (which were only a few inches wider than the crates) was a formidable task, but not nearly as difficult as trying to get them up to the operating room of the telescope structure which was about 15 feet above the ground, and accessible only via a narrow ladder. For this task a crane was summoned from the Crimean Astrophysical Observatory about 100 miles away. Even with the aid of the crane, access to the control room was blocked by a steel railing running around the whole telescope. This problem was easily solved with a hack saw, and the VLBI equipment was ceremoniously hoisted into place in front of 15 to 20 spectators.

Meanwhile, Matveyenko and Kogan spent the day constructing a deluxe cardboard and wood box to carry a spare storage battery to provide extra power for the clock, sufficient for any reasonable emergency. After a few hours sleep, the following morning I departed by car at 6 AM with Kogan and our rubidium clock to catch a 9 AM flight to Leningrad. The Swedish NiCd batteries had been recharged and were expected to be good for 10 hours. (Actually, when we got back to Green Bank and received a bill from Sweden for \$500 we realized that they were our batteries.) In addition, we carried a 90 ampere hour car battery in a deluxe box which provided about another 25 hours of safety. The plane was scheduled to arrive in Leningrad about noon. We planned to have the afternoon to set the clock and recharge the batteries and return on an evening flight to Crimea. But on arriving at the airport in Crimea airport, we found that the plane was full and we would have to wait until 5 o'clock before leaving.

Just before departure time, the local "chief" at the airport, a rather formidable looking Russian lady, was summoned. She wanted to know what was in our box and why it couldn't it go in the baggage compartment. She was also concerned that we had no personal luggage. We tried to explain, carefully avoiding the use of the term "atomic" clock, and had some difficulty in convincing her that we had no personal luggage because we were planning to return in a few hours! Imagine a Russian trying to get on a flight from Miami to New York for a few hours and carrying a strange looking (ticking) box with wires and batteries, and having only a voltmeter, pair of pliers, and a large screwdriver for luggage, and you get the picture.

When we arrived in Leningrad at 8 PM it was cold and raining, but it is always cold and raining in Leningrad. We were greeted for the second time by Yuri Pariskii and his colleagues from the Pulkovo Observatory. Unfortunately, he explained there was some confusion about when we would arrive (not too surprising), and there was no car to take us to the Observatory. After getting a good soaking in the rain, we finally hailed a taxi (about the same whoever-gets-to-the-car-door-first priority as in the U.S.). At Pulkovo we resynchronized our clock with the Swedish clock which was still running, charged our batteries, and were ready to return to Crimea. However, this being the end of the tourist season, not very many people were flying to Crimea this particular Saturday night and so to save some rubles, Aeroflot had canceled our return flight.

All of the dormitory rooms at the Observatory were occupied, but I was shown to a room that had an "extra bed" and sacked out for a few hours to wait for our flight which was now scheduled for 10 AM. I guess the occupant of the room was a bit surprised the following morning to find he had a "roommate." Again, for comparison, imagine waking up one morning in the Green Bank dormitory to find a Russian who can't speak English sleeping in your room. I tried a few words of greeting and quickly departed. About this time my friend Kogan reappeared as mysteriously as he had disappeared the night before. We were given some breakfast and told to wait for the driver to take us to the airport. The driver showed up soon enough but apparently couldn't find the van. A sedan-type car finally arrived at the last minute. There then followed a "small discussion" on whether the "clock" would fit into the car. Someone got the brilliant idea that if the spare tire were removed, the clock would fit into the trunk. But the driver objected because we might get a flat on the way to the airport, about 5 miles away. I argued that it was already so late that if we had a flat we would miss the plane anyway, even if we had a spare. This "decided the question" and for the second time Pariskii and his group said good-by, wished us luck, and we departed for the airport and Crimea.

Halfway to the airport I remembered I had forgotten my Simpson voltmeter. A quick calculation, including a reasonable estimate of how late the plane would be, indicated that we had time return to the Observatory and collect the meter. The send-off party had not yet disbursed and so we received yet another send off and round of "good luck wishes." This time we made it to the airport, only to find that there was a mistake on our ticket, and that actually the plane would leave at 11 o'clock not 10 o'clock. Just to be safe I suggested that since we had to wait an hour we should plug the clock in the wall socket and not drain the batteries. Upon opening the box to get the power cord, we found that the clock had stopped again! This time the batteries had lasted less than one hour. We consoled ourselves that after all it could have been worse, and that it was fortunate that the plane had not left at the expected time. It was easy enough to cancel our ticket, but a major problem to book a new flight. In Russia you can't buy airplane tickets at the airport, only at your hotel or at the Aeroflot ticket office in Leningrad. However, we were not staying at a hotel and, in fact, I wasn't supposed to be overnight in Leningrad at all, but we had all long ceased caring about details like government travel restrictions on Americans. Also, as indicated earlier, in the U.S.S.R. you usually have to make airline reservations at least three or four days in advance. To further complicate the situation it was Sunday, and in Russia no one works on Sunday, including the airline ticket office. Nevertheless, I told Kogan to go into Leningrad and get us a ticket for an afternoon flight while I went back to the Observatory to "reorganize" the clock and batteries. I was beginning to have blind faith in my friend Kogan's ability to overcome all bureaucratic obstacles.

Arriving back at the Observatory I was again greeted by Pariskii, who I was sure by this time was getting tired of seeing me keep returning to the Observatory. It was becoming increasingly clear that we were doing something wrong with the batteries. Someone suggested that maybe they were being charged backward, and perhaps the Russian definition of plus and minus was not the same as the American (or Swedish) definition. The discussion then degenerated into the difference between "electrical" plus and "physical" plus and electrons and holes, etc., which was clearly all nonsense. We later realized that our problem was that each night the line voltage, which was nominally 230 volts would drop to about 190 volts and slowly discharge the NiCd batteries which were attached to run the clock in case of power failure. Since NiCd batteries were sealed there was no way to measure the state of charge until the terminal voltage began to drop, which occurred only as the batteries were nearly discharged.

Just to be safe, we abandoned the NRAO clock and the batteries at the Observatory and packaged the Swedish clock and 50 ampere hour car battery for reserve. Although this increased the size and weight of our "portable clock," we figured we had enough reserve now to make it back to Crimea. Kogan had telephoned that he had managed to get us two plane tickets for a 4 o'clock flight and that he would meet me at the airport before flight time. Following another big send-off and round of handshakes and good wishes, we departed once again for the Leningrad airport. But when we arrived, we could not find Kogan who was supposed to handle the arrangements for getting the clock on the airplane. He finally showed up at the last minute and explained that the bus he was riding on had a flat and he had to wait for another bus.

This time we didn't take any chances on the batteries. During the 3-hour flight between Leningrad and Crimea, Kogan and I took turns every 15 minutes running to the rear of the plane to check the batteries with our voltmeter. In order not to look too conspicuous, I pretended each time to be going to the toilet, but after a few sessions realized that was even more suspicious looking. We made it to Crimea and managed to transfer everything to the car without mishap. Toward the end of the plane ride the NiCd batteries had begun to fail and we switched over to the car battery which we were carrying. Halfway during the car trip to the Observatory this too began to run down and we had to go over to the battery that was running the car. With this we made it to the Observatory and got the clock attached to the 230 (more or less) VAC Observatory power. Moiseyev handed us a pair of wires which he said went to a 100 ampere 28 V DC supply deep in the basement of the telescope. After hooking up this "emergency" supply in case of power failure, we went off for a badly needed meal. All we had eaten since breakfast was the usual Aeroflot "dinner" consisting of a crust of bread, an apple and some lukewarm tea. The next morning we found that our 28 VDC emergency supply had dropped to 22 V and so of course had discharged the "ultimate" 28 volt fifteen minute internal reserve battery. This was a very dangerous situation indeed, since even a momentary power failure would mean disaster. We managed to get a good battery hooked up and Kogan was directed under threat of exile to Siberia to see to it that the batteries were always kept charged for the rest of the experiment.

The following day (Monday) we were making some last minute system checks when it was announced that there was a telephone call from Green Bank. It was Mike Balister. We each yelled into our respective telephones, sometimes alternately, sometimes simultaneously, trying to communicate, but without much success. After about 10 minutes of this nonsense, Mike asked to speak to me. He thought he had been talking to John Payne. I found it hard to believe that I could really be mistaken for John, until a few weeks later when I was handed the phone and told "Green Bank." The noise at the other end said, "This is Howard." and thinking I was talking to William E. Howard III, I proceeded to transmit some information about our return trip-visas, etc., which obviously were not being understood at the other end. I later realized that I had been talking to Howard Brown, the supervisor of the 140 Foot.

About all I learned from Mike was that they were ready to observe the following day. Mike also gave me some numbers relating to the error in the time transmissions from the Loran C station in Turkey, but I could not understand what they meant. The same numbers were sent to us by telegram with small changes every few days for the next few weeks and did little but add to our confusion. Finally, after two weeks of trying to figure out what it was all about, a letter arrived from Barry Clark, which he had mailed a month earlier, containing a nominal ephemeris for the time signal transmissions. Realizing that the many telegrams and letters were giving just the corrections to this nominal ephemeris, I made a quick calculation and was delighted to find that the time derived from the Loran transmissions agreed with the time we had imported from Sweden to within a few microseconds. This was really a remarkable achievement because we later realized that I had forgotten what day it was and had calculated the time for the previous day. This was fortunately canceled by an error of 1 day that Barry made in computing the ephemeris.

The first observations were planned just as a test for the main run scheduled two weeks later. The plan was to run a few tapes on 3C 273 and 3C 454.3, the two strongest sources at 3 cm. The run on 3C 454.3, unfortunately, came about 3 AM local time in Crimea. When the observation was finished John and I looked about for a ride back to our hotel in Yalta and some badly needed sleep. We found our Russian colleagues upstairs breaking open the vodka, cognac, and the little square bottle of "spiritis." After completing 2 percent of the scheduled observing it was clearly time for a celebration. Following two hours of eating, drinking, and declarations of Soviet-American friendship and cooperation we were finally taken to our hotel.

Late the following morning we were met by Moiseyev, and after a leisurely lunch were told, "Oh by the way, a telegram arrived this morning." It was from Barry. The frequency had been set wrong at Green Bank and he wanted to repeat the run on 3C 273 in about two hours. Although we had left instructions at the Observatory to remove the 3 cm receiver and put up the 6 cm one in preparation for the next observing run, the telescope crew fortunately had declared a holiday and was still celebrating. We made a quick trip out to the Observatory, arriving with about an hour to spare. John and I were a bit dismayed to find the telescope locked, and no one seemed to know the whereabouts of the key. But Moiseyev finally arrived to open the door and we managed to run the tapes on time.

We had planned to return the tapes to NRAO immediately for processing before the next run two weeks later. Considering that it usually took anywhere from weeks to months to get tapes back from reasonably accessible places like Puerto Rico, Sweden, or California, this might be thought to be wishful thinking. But it had all been carefully arranged. Immediately following the second 3C 273 run, the tapes were quickly packed up and driven to Yalta, where Professor Vitkevich was waiting to leave for Moscow. There he would deliver the tapes to the foreign office of the Soviet Academy of Sciences where they would be collected by a driver from the U.S. Embassy, who would bring them to the U.S. Scientific Attache at the American Embassy who would then give them to a returning American "geologist" who was flying to Washington that afternoon. Having been alerted by the State Department, Barry Clark was to go to Washington to collect the tapes when they arrived. It seemed like a "sure fire" scheme and we could relax until the next run, scheduled for two weeks later. Actually, our real troubles hadn't yet started.

Following the "successful" test run, we had nearly two weeks before the main observing session scheduled for mid-October. The first few days were spent installing the 6 cm receiver, devising a chain of battery supplies to insure the continued operation of the atomic clock, and trying to establish some sort of communication with Green Bank. The first two projects were rapidly completed but continued attempts at telephoning only produced the now familiar "Amerika Nyet!"

Our Russian hosts decided that we were adequately prepared for the observing with more than a week to spare, and that the extra time could be used to expose us to some Soviet culture. They proposed that we take a trip to exotic Middle Asia to see Armenia, Tashkent, and Samarkand.

The trip started out uneventfully with the three hour drive to the Simferopol airport where we boarded an Aeroflot plane for Yerevan, the capital of the Armenian Soviet Socialist Republic. Since directly to the north of Yerevan are high mountains, airplanes are forced to maneuver south of the city for landing. The Turkish border is only 15 miles south of Yerevan so it is not possible for a modern jet aircraft to land without passing over the Turkish border, which would, of course, mean being immediately shot down by the waiting American anti-aircraft guarding the frontier. Thus, we were forced to fly on a relatively slow propeller driven airplane.

Although the flight was supposed to last about two hours, not too long after take off we began to descend. After landing we were told that we were in "Mineral Water," a small resort town (a Soviet White Sulphur Springs) close to the border of the Georgian Soviet Socialist Republic, and, due to bad weather, we were told that it would not be possible to land in Yerevan until the following morning. No one bothered to tell us what we were supposed to do in such a situation, so we proceeded to the airport building where we learned that there were some 10 other planes from all over the U.S.S.R. which were also forced to detour to Mineral Water on the way to Yerevan. Somewhat over 1,000 passengers from these planes were occupying a rather small airport terminal. Our fellow passengers, who had apparently decided to camp out for the night in the terminal, were comfortably spread out on chairs, tables, and the floor in various states of dress and undress. Due to the rather overcrowded conditions and the lack of adequate sanitary facilities, the whole place had the aroma of one large toilet.

Matveyenko went off to seek the local Intourist agent whose job it was to see to the comforts of foreign visitors. After half an hour of "breathtaking" waiting we were ushered into a rather plush Intourist office where we were informed that my wife and I could have the one remaining hotel room in town, and John Payne and Matveyenko could share the floor of the Intourist office. Just in case the reader may think my wife and I had an unfair advantage it should be emphasized that the difference between the hotel room and the Intourist office was negligible compared with the difference between either of these facilities and the public waiting room. All of which goes to prove that although in a Communist state all people are equal, some are more equal than others.

The following morning, after being treated to a fine breakfast in a private dining room by our friendly Intourist agent, we proceeded to Yerevan. There we were met by Dr. Grant Tovmassian from the Byurakan Observatory. He informed us that during one week every seven years the Armenian church prepares its holy water and Armenians from all over the world return for the festivities. Since, of course, this was the week, there were no hotel rooms available in Yerevan, so we were invited to stay at the Observatory. Just before arriving at the Observatory he added rather parenthetically that "of course as we must know there is no hot water available." My wife groaned a bit whispering to me, "All we need now are bugs in the bed." Her fears were justified, but fortunately there weren't too many and they were not very large.

In spite of these hardships we spent several pleasant days visiting the Byurakan Observatory and seeing a bit of Armenia. I was asked to give a lecture at the Observatory in their new lecture room equipped with numerous modern conveniences, including curtains which closed automatically by pressing a button. There were technical difficulties, however, with the automatic curtain closer, and there was no means to manually close the curtains, so I had to dispense with the slides.

By this time we were becoming increasingly curious about whether or not the Green Bank group had found fringes on the tapes. We spent two successive evenings until midnight in Tovmassian's office trying to place a phone call to Barry Clark in Charlottesville. In order to impress the Russian telephone operators of the importance of the phone call, Tovmassian told them that the call was coming from the Observatory Director, Prof Victor Ambartsumian, not only a prominent Soviet scientist but a powerful political figure as well. Sometime in the wee hours of the morning of the second night Tovmassian was awakened by a phone call and informed "Amerika. Speak." On the other end the NRAO operator in Charlottesville was told that there was a call from Prof. Ambartsumian. Thinking that a call from such an important person must be for the NRAO Director, Dave Heeschen ended up speaking to Tovmassian, rather than Barry Clark, to me. By the time things were straightened out the connection was broken. The next day we received a telegram from Barry stating that the tapes had not yet arrived and inquiring of their whereabouts.

Our immediate reaction was that somewhere in Moscow, Washington, or both, teams of experts at the CIA or KGB were unsuccessfully trying to decode a magnetic

Telescope locations and baselines used in the early VLBI experiments.

tape containing a sequence of 150 million random numbers which had apparently been smuggled out of the U.S.S.R. by the diabolical sequence of agents described earlier. A frantic phone call to the American Embassy in Moscow met with the usual delays, first being told the lines were busy, then it was lunch time in Moscow, and then that the Embassy was closed for the day.

That evening we booked, in advance, a phone call to Moscow for the following day at 9 AM from the Yerevan Post Office (also a telephone exchange) where connections were theoretically better. Arriving slightly before 9 o'clock we found the Post Office had never heard of us, but the operator arranged to place the call at noon. We then had a small discussion about sending a telegram to Moscow. Matveyenko claimed it was unnecessary since we would have the phone call in a few hours, but I was beginning to become familiar with Soviet efficiency and insisted on the telegram. At noon we found a new operator had arrived who, of course, knew nothing about our call but she said she would book the call for 3 o'clock. And at three, well, its hard to believe, but there was yet another operator on duty and we started all over again. We waited around this time until we were informed that it was 5 o'clock in Moscow and the Embassy was closed, or as the Russians say, "not working."

The next day, our schedule called for us to fly to Tashkent in the Uzbekestan Soviet Socialist Republic in Middle Asia only a few hundred miles distant from China and Afghanistan. We arrived at our hotel about 3 AM. After a few hours sleep we began the day's program which started with a lecture at the world famous Tashkent Astronomical Observatory. Following the lecture John and I were taken to the Director's office for refreshments of tea and grapes. Following an hour or so of pleasant chit-chat with the Division Head and Director, neither of whom spoke English, the Solar Physics Head insisted on showing us her little telescope in the garage. Then, since we were clearly interested in time and frequency standards we had the pleasure of visiting the Uzbekistan Bureau of Standards where one of the local astronomers spent all day comparing an old German crystal oscillator with the British time station, RGB, in Rugby, England using a receiver similar to the Loran front-end we used in Leningrad. For our second day in exotic Tashkent we went to the local bazaar to mingle with the natives. Here for the first time we found thriving capitalism in the U.S.S.R. Farmers and merchants displayed their products on a get-what-you-can-for-it basis. Competition, usually unknown in the U.S.S.R., was heavy and thriving. The highlight of the day was our introduction to plof, the Uzbek national dish. Matveyenko called it “natural food” meaning served as the natives eat it. It was quite an interesting dish consisting of rice, spices and various kinds of meats. Unfortunately, this fine dish could not be handled by our already overworked digestive systems. This, combined with the nature of the sanitary facilities, somewhat detracted from enjoying the rest of our stay in the U.S.S.R.

The following day was Monday, a working day and another opportunity to call the U.S. Embassy in Moscow. The plan was to spend the day in the Tashkent Post Office to compete with the day we spent in the Yerevan facility. At the Post Office in downtown Tashkent we were surprised to find an automatic telephone where for a few cents you could be immediately connected to any major city in the U.S.S.R. from Kiev to Vladevostok. Getting the U.S. Embassy, I found myself overhearing a conversation between the Embassy and a chap in some other remote part of the U.S.S.R. who had lost his passport. Trying again, I learned that the tapes had in fact been sent to the U.S.A. and so we presumed that they had found their way to Charlottesville.

Meanwhile, another problem was beginning. According to the original plan, if the preliminary 3 cm observations did not show fringes, Barry Clark planned to hand-carry a small crystal clock from Green Bank to Crimea for a direct time comparison. We had been regularly trying, unsuccessfully, to call NRAO since leaving Crimea. We also sent several telegrams informing Barry that we thought we had the time correctly and that everything else appeared to be functioning satisfactorily. But not hearing anything about the 3 cm results we had to plan on Barry’s arrival in the U.S.S.R. and requested that our colleagues in Moscow be prepared to meet him at the Moscow airport and to arrange for his immediate travel to Crimea. And, of course, they must also provide batteries for his clock.

Our return trip from Tashkent to Crimea was on a propeller driven aircraft. For the whole five hour flight the pilot was never able to synchronize the engines. If you have ever experienced this phenomenon you can imagine our mental state upon arriving back in Crimea. Moreover, we had the pleasure of sitting in front of a band of Gypsies that apparently had never taken a bath. On the way to the Observatory the following morning we were told by Moiseyev that “Oh, by the way, the clock stopped the other day.” After I recovered from my mild hysteria, he went on to inform us that it only stopped for a few minutes before they started it again! At the Observatory we learned that the crystal oscillator had become unlocked from the rubidium cell, but fortunately Leonid Kogan, who was faithfully watching it during our week’s absence, had immediately relocked it so that in fact no damage was done.

We also received a telegram from NRAO that the tapes had arrived but that there were no fringes. There appeared to be some misunderstanding about the position of the Russian telescope due to my confusion over the way the Russians write the number 7 (similar to the American 4). To add to the confusion, Moiseyev had misplaced the piece of paper on which he had written the “exact” coordinates of the antenna at the time of construction. The best we could do was estimate the distance and direction of the antenna from a nearby optical telescope which is listed in the Nautical Almanac. The optical telescope, which was located on a cliff overlooking the antenna, was estimated from sticks, stones, shadows, and some trigonometry to be about a kilometer away.

The new position was sent off to NRAO with the expectation that this would produce the fringes. The answer, received by telegram a few days later from our colleague John Broderick, stated that there were still no fringes, and that the new position according to his map placed the telescope several miles out in the Black Sea!

By this time we were becoming increasingly annoyed at the poor communications. Telegrams often arrived garbled up with mixed Russian and English characters. Sometimes, the same telegram would arrive several times spread over several days. On one occasion a three page telegram arrived one page at a time on three successive days. I decided to make a major effort to try to use a TWX machine located in the Intourist hotel in Yalta. Matveyenko, myself, and the local teletype operator spent one entire day trying to contact Green Bank. First we were told by Moscow that all international teletype calls had to go through Kiev. Kiev told us that the Russian teletype was not compatible with TWX. I explained that there was a giant computer in New York which understood all machines and could act as interpreter. Then somehow we got an operator in Vienna (yes, Vienna) who claimed that the Green Bank machine was broken. After many hours we had produced several feet of conversation between various operators, and our local operator excused herself for a short break.

I was alone in the teletype room, when all of a sudden the machine started to print NRAO GB WVA, the call sign of NRAO indicating that at last by some circuitous means our keyboard was directly connected to the Green Bank TWX machine, where unfortunately it was now about 3 am. A further difficulty was that although the Russian teletype machine contained English letters they were not in the standard locations found on American typewriters. It took me about half an hour to type out a single message telling Green Bank that we had a communications breakthrough, and to use TWX instead of telegrams because it was faster and more reliable. The next day we received a telegram from Green Bank wanting to know what we said in the completely garbled and unintelligible TWX message. So much for reliability.

Meanwhile we hadn’t given up on the telephone. The usual operator’s report was that the lines to the U.S.A. were out of order. In a moment of desperation I tried to call Sweden, hoping to speak to anyone in the outside world. Following some clicks and crackles a clear voice asked in English,

“Is this Helsinki?”

“No,” I replied, “This is Yalta. Who are you?”

“This is New York.” she said.

“Can you get me Charlottesville Virginia?”

“Yes, of course, just a moment please.”

Pause. Clicks and crackles. Dead line!

The next day word arrived that Barry was in Moscow. But instead of coming directly to Crimea with the clock our Russian colleagues felt he should stay a day or so to visit museums. The crystal clock Barry was carrying was supposed to be accurate to about 50 microseconds per day. Even with the delay in Moscow, we expected that with careful calibration of the rate to be able to compare the clocks with an accuracy of 15-20 microseconds. When Barry arrived in Crimea we went directly to the Observatory for the moment of truth. Much to our dismay the two clocks differed by nearly a whole second. We could have done better with a good wrist watch. Nevertheless, since our rubidium clock agreed with the loran transmissions (only because of the double error described earlier) we had some confidence that we were still in good shape and were prepared for the big observing session about to begin.

John, Barry, and I arranged to work in rotation loading tapes. The Russians divided themselves into three teams of two men each. One man was in the control room to operate the antenna while the second man, who was with us, translated and relayed our instructions via a very poor intercom system to the telescope operator. This man was the key to the whole operation as he was the only link between the English speaking tape loaders and the Russian speaking telescope operators.

Before the beginning of the three day 6 cm observing session we went to Yalta and stocked up on Russian sausage, cheese, bread, and fish, and a crate of soft drinks to last us through the session. Due to the lack of adequate sanitary facilities at the Observatory, Barry, John, and I planned to take turns going into Yalta for a bath, etc. For this purpose a car with driver was placed at our disposal. We never did find out what the Russians did about this problem, but with one noticeable exception they apparently managed.

Some hours before the run was scheduled to begin, a small boat appeared on the Black Sea and anchored just offshore about 100 yards from the telescope. When we questioned our colleagues about this vessel we were variously told that it was a “fishing trawler,” an “ionospheric research vessel,” and an “oceanographic research vessel.” At nightfall, a bright spot light on the boat scanned up and down the shore for about one hour. I was tempted to blink back with my electronic flash unit a few times to see what would happen, but John suggested that perhaps this was not too good an idea. There was also a rumor going around Green Bank about a U.S. Navy ship being sighted on Deer Creek, but this was never confirmed.

Since we still had no fringes from the first test run we concluded that it was necessary to get some more tapes back to NRAO as soon as possible for processing. This time we would avoid the cloak and dagger operation with the U.S. Embassy and would try simple air freight. Realizing that it just might take a bit of time to arrange to ship a magnetic tape from Crimea to Charlottesville, I planned to leave Crimea on Monday morning to try to make the weekly Pan Am flight to New York which leaves Moscow Tuesday afternoon. After the first hour or so when the initial excitement wore off and the numerous spectators began to depart, the operation settled down to an almost normal VLBI run. As I had not yet fully recovered from my experience with the "national " dish in Tashkent several tapes were missed for shorts breaks.

At 5 AM on Monday morning, I left for the 3 hour drive to the airport carrying two tapes. At the airport there was a message: Barry had discovered that one of many switches had been set to the wrong position, so all that we had done so far was no good. In particular, the two tapes I was about to carry to Moscow to put on a plane to New York were no good. I get back in the car and returned to the Observatory.

Barry sent a telegram to Green Bank to alert them to our blunder, and since there was no guarantee when or if the telegram would arrive, he also tried to place a call with the usual negative result. Therefore, I stopped in the Intourist hotel in Yalta, which we had established as a communications base, to get our friendly teletype operator to send a TWX. Of course, she was told by the operator that the U.S. and Soviet machines are not compatible, then that the NRAO machine was out of order, etc., but she kept trying. Meanwhile, Matveyenko and I were trying to telephone, thinking that we might have better luck from a "major" city such as Yalta than from the Observatory. The situation was fairly critical since the NRAO schedule called for the 6 cm receiver to be removed from the 140 Foot as soon as the observing program was finished. Although this was only a few hours away, our colleagues in Green Bank did not know that we had only just begun proper observing in Crimea, and that it was necessary to repeat most of the program. After several hours of fruitless telephoning, Matveyenko performed what must be considered the supreme achievement of the entire experiment. He called the U.S.S.R. Minister for Communications in Moscow and explained our problem. Ten minutes later through the miracle of electronics and some high level influence we were talking to Green Bank and for the remainder of the experiment a telephone call to the U.S. took only a few hours to complete rather than a few weeks.

Needless to say, the Green Bank end was a bit disgruntled at having observed furiously, but needlessly, for 36 hours, but agreed to repeat the beginning of the program. I was driven to the Observatory, picked up two new (presumably good) tapes, and gave a few words of encouragement to Barry and John, who were looking a bit tired and grubby, and who now had to do the whole program themselves without time for pit stops in Yalta.

Again we set off on the third trip of the day between the Observatory and the airport, my 6th, 7th, and 8th so far (out of a total of 10). By this time Matveyenko was as proficient as a U.S. travel agent at changing plane tickets. The trip to Moscow was less exciting now that I was carrying only an inconspicuous box which was not even ticking. The next morning, I began preparations to ship these tapes, already being one day behind schedule because of the previous day's fiasco. Matveyenko went to the "Akademy Nauk" in the "official" car to get an "official" piece of paper from the Akademy that would allow the tapes to leave the country. He was then supposed to meet me at the U.S. Embassy where I went to try to find out how one goes about shipping parcels in the Soviet Union. At the Embassy I was told to bring my tapes to the Pan Am agent at the airport. I had to wait almost two hours for Matveyenko because someone had absconded with his car and driver at

The celebratory cake.

the Akademy. Meanwhile, the Russian cop guarding the American Embassy was becoming suspicious of my pacing up and down in front of the Embassy still holding on to the precious package of tapes. When Matveyenko finally got a new car and arrived he still didn't have the letter from the Akademy, so we had to return. By this time it was the lunch hour and we had to wait a bit longer for the necessary bureaucrats to finish their lunch.

Arriving with the necessary paper we proceeded to the Pan Am office at the airport where I was informed that because the agent had to "check-in" a group of 120 American tourists he did not have time to help me, but that I would have to go myself to the Aeroflot freight office. There I waited in several lines, filled out many forms (in Russian) and paid my money. All this accomplished was to get a few stamps on the package of tapes which no one would take. By now it was almost plane time, so we rushed back to the departure building looking for the Pan Am agent and some English speaking help. Not finding anyone I abandoned Matveyenko, flashed my passport and eased my way pass immigration and customs officials, and gave the package to the pilot as he was about to enter the plane.

From this point everything went right. Bill Howard flew to New York and brought the tapes to Charlottesville, setting an all-time VLBI record of 48 hours to transport the tapes from Crimea to Charlottesville, beating the old record of one week from Boston to Charlottesville.

Meanwhile, back in Crimea there was no joy. A telegram had arrived from Green Bank—the hydrogen maser had broken down and a power transformer on the 140 Foot had blown up. So far one American, one Englishman, and one Texan had, if nothing else, succeeded in pretty well disrupting the entire political, economic, and social system in the U.S.S.R. with nothing to show for it. In a little over a month we had dispatched various shipments of people and equipment between Stockholm, Moscow, Leningrad, and Crimea by air, rail, and road. We had made unprecedented demands on transportation and communication facilities, and had apparently cornered the market on all the storage batteries in the Soviet Union. And at the Observatory, we had men working endless hours carrying our crates first up, then down, then up again, chopping off pieces of their antenna to accommodate our equipment, and putting our receivers on and off the telescope.

So although preparations proceeded for the 3 cm observations, morale was low all around. Reference to Siberia entered the conversation with increasing frequency. You can therefore imagine the general joy and relief when the telegram arrived announcing strong fringes on 3C 454.3. Vitkevich was at first speechless, but rapidly recovering he cried, "BRING THE VODKA!" Remembering that we still had two days of observing left, the celebration was, however, postponed.

The first day of the 3 cm run went smoothly until a telephone call arrived from Marshall Cohen, who explained that due to a few technical difficulties they had not yet started in Green Bank. I started to complain that we had needlessly worked hard all day running tapes, but remembered the reverse situation which had occurred a few days earlier and decided not to comment. Our Russian colleagues took the news in stride, and reorganized their assignments to see the run through. Now that the end was in sight, John, Barry and I thought that we should sponsor a small celebration to show our appreciation for all the help we had received. A cake was carefully designed, and John was sent to Yalta to arrange for its construction. It turned out to be such a big and important job that the details had to be settled at a high level conference between John Payne and the Yalta Director of Bakeries. John expressed some concern that his boss, Sandy Weinreb, might not appreciate his spending an entire day arranging to bake a cake.

We had planned our surprise party to follow the end of observing, but as the last hour approached the observing room began to fill with spectators. When the last tape started the Russians produced, in their usual efficient manner, a round of glasses and several bottles of Cognac, and with toasts of Soviet-American friendship and cooperation, the first Green Bank-Crimea VLBI experiment was declared a success on the basis of having analyzed less than one half of one percent of the data. The party lasted well into the night, and after a few glasses of cognac, vodka, and spirits, the language barrier disappeared. The highlight of the evening was an eloquent speech by Barry Clark delivered in excellent Russian. The following day those of us who were still mobile were treated to a dinner of shashlik (shish kabab), cognac, and champagne by Vitkevich. Following this we were taken to the Crimean Astrophysical Observatory where the director had prepared a small dinner featuring mainly wine and `vodka`. Back in Green Bank, according to reports, the exhausted observers just went to sleep!

Not too surprisingly, all the tapes and equipment were lost on the return shipment to the U.S.A. After a week of frantically calling Pan Am and Aeroflot in Washington and Moscow, everything was located in an Air France warehouse in New York! After much computer processing interference fringes were obtained on 12 radio sources at 6 cm. At 3 cm the outcome was less satisfactory due in part to an improperly operating frequency standard in Green Bank. Only through careful

Celebrating the experiment. Standing, L to R: Ivan Moiseyev (director of the Crimean radio telescope, John Payne (NRAO), Victor Efanov (telescope staff); sitting: unknown (telescope staff).

painstaking analysis by Matveyenko did we finally find weak fringes at 3 cm, thus setting another VLBI baseline record of 285 million wavelengths.

Perhaps more important than the scientific result, however, is the immense satisfaction that all the participants received from working together to successfully overcome what at times appeared as insurmountable bureaucratic, technical, and logistical difficulties. We would like to think that perhaps in some small way we have contributed to an increased understanding between Soviet and American people, and demonstrated that scientific cooperation between the U.S.A and the U.S.S.R. is possible.

Epilogue (July 2001)

Now, thirty years after these first experiments, Russian radio astronomers, still led by Leonid Matveyenko, are regularly participating in global VLBI observations. For more than 15 years, under the leadership of Nikolai Kardashev, they have been planning the launch of RadioAstron to provide earth to space baselines. Clark, Cohen, Broderick, Payne, Shaffer and this author are still active in radio astronomy. In the interim, Leonid Kogan immigrated to the United States with his family and is now an NRAO Associate Scientist in Socorro working with the VLA and VLBA.

Many years after these experiences, we learned that our exotic trip to Tashkent, Samarkand, and Armenia was arranged so that KGB engineers could have an uninterrupted week to reverse engineer our recorder, receivers and atomic clock.

10 Travels with Charlie*

Barry Clark
NRAO, Socorro, NM

John Steinbeck wrote a book called "Travels with Charlie" about a trip through the U.S.A. in a camper (in the days when this was unusual) with a large dog named Charlie. He claimed that having this non-hominid companion enhanced his appreciation of the country by forcing him to make more varied contacts than if he had been traveling all by himself. For this purpose, however, I shall now proceed to demonstrate that a dog is less effective than a Rubidium vapor frequency standard (i.e., a clock). This is the story of how I carried the time to a radio telescope in the Crimea, in the U.S.S.R., for the May 1971 VLBI experiment.

My friend, the clock, is a fairly modest suitcase, made by the Hewlett-Packard Company with wonders of quantum physics, microwave engineering, and digital logic packed tightly into its seven inch chassis, weighing something under forty pounds. Modest also is its demand for some thirty-five watts of continuous power. After all, you can light only a rather dim light bulb with this sort of power. Note, too, that Ni-cad batteries have a very high energy storage capacity per pound of weight. So, though the pounds are beginning to mount up, you must realize that thirty pounds of batteries are indeed a minimum requirement for a ten hour portable power supply. Mike Balister's battery charger is not an excessive weight— it has a transformer in it, of course, but all the circuitry is fairly light transistors. And the fact that I felt it necessary to take a 60 cycle inverter to run the whole works from an auto battery was my own decision, of course. We perhaps could have made a lighter packing case for it, of sheet metal rather than wood, but we didn't realize how heavy the wood one was until it was made, and then it would have taken too long to make a metal one. So, despite the miracles of modern miniaturization, I found myself with a seventy kilogram clock package. I still outweighed it, but not by that much. (A German acquaintance referred to it as "Ein drei-Man Uhr"—a three man watch.)

The plan was to ship Charlie to Europe cold, warm him up there, fill him up with time, and cart him off to the VLBI experiment before all the time could leak out. We planned to do it this way because the direct New York-Moscow flights take too long for Charlie's batteries to keep him warm, and we felt it would be easier to wake him up in Europe than to make the special arrangements for him to have something to eat on the airplane. So, since I would be in Europe anyway, I planned to visit two new instruments— the Westerbork Synthesis Radio Telescope in the Netherlands, and the 100 meter telescope near Bonn, Germany. Nobody in Holland seems to know what time it is (I had George Miley ask), so I planned to fill Charlie with time at Bonn, where I was told they knew the time from measuring the Norwegian Sea LORAN emissions.

So, one Friday I put Charlie on an airplane for New York, where he would wait for me to arrive the following Tuesday. And arrive I did. Asking at the National Airlines ticket counter where their airfreight depot was, I had the good fortune to

* *Excerpted from two articles which appeared in the "Green Bank Observer" in 1972.*

Charlie, the rubidium clock.

encounter the nicest man in New York. He was a National Airlines passenger service aide, and he gathered up a company station wagon, drove me over to their airfreight warehouse (where we found Charlie with his "This Side Up" arrow pointing straight down), drove me and Charlie over to Pan American airfreight, waited around while I handled Charlie's paper work, and drove me back to the passenger terminal, and refused a tip for all this.

From a sample of two, I make the following generalizations about trans-Atlantic flights leaving from Kennedy Airport: 1) They leave three hours late, 2) New York is overcast but Boston is clear, and 3) The movie projector breaks down half-way through the movie.

I was met at Schipol Airport near Amsterdam by George Miley (who had been waiting since the scheduled arrival time), and carried off to Leiden. I phoned Pan Am airfreight and found that (a) Charlie hadn't arrived yet, and (b) yes, they were open on Saturday mornings and I could pick up Charlie then. So I settled in for some sightseeing and talking at the observatories in Holland.

Then came Saturday morning, when I went out to Schipol to rent a car and pick up my clock. Disaster — While I had told the Pan Am man in New York to have Charlie held at Schipol Airport, I didn't check to make sure he had written it on the papers. He had not. Pan Am, noting the University address I had given, had given Charlie to the usual customs agents for the University, and they said that the box had been shipped by train to Leiden. While trying to call somebody at the Leiden railroad station, I encountered Harry van der Laan, who promptly gave up his afternoon for the search. We eventually found the man in charge of freight at the railroad station, and he let us in to search the freight shed, but no Charlie. Impasse. Nothing could be done until Monday.

I had told Ivan Pauliny-Toth that I would drive to Bonn on Saturday evening, so I got on the telephone to tell him I wouldn't be there. But nobody answered the phone at the Max Planck Institute. (And apparently not at the Sterrenwacht te Leiden, either. Ivan was trying to call me.) So I then spent an interesting hour with the international information operator. Everybody I knew at Bonn either had no listed telephone number (Pauliny-Toth, Churchwell, Goss) or there were several listings with the same name (Mezger, Stumpff). I gave up.

Monday morning, the customs agents found Charlie, sitting in Schipol Airport. I drove over and picked him up. Dutch customs didn't trust me not to sell Charlie on the local rubidium clock black market, so they wrapped his box with string, sealed the string with lead seals, and told me not to open him except in front of a customs official. So I plunked him in the trunk of my rented Opal, and drove off to Bonn.

Tuesday morning, Ivan and I went to the customs office in Bonn, taking Heinz Wendker with us as an interpreter, to see if they would let us open up Charlie and fill him up with time. They said they didn't really approve of such activity, but we could if we posted a bond equal in value to the clock. When I said I didn't happen to have $8000 in cash with me, they said we should see a bonding agent.

So we went to see a bonding agent. He said it was all very irregular, and that, though he was sure we were not smugglers, if we were, it was a most ingenious way to go about it. He would, however, bond us if we left a deposit of merely $2500.

Back at the Max Planck Institute, Heinz looked for somebody who was familiar with customs procedures. We found such a man, a Herr Zann, and he was able to make an arrangement by which a customs agent would go with us that evening to the telescope, watch us open the clock, and reseal it the following morning. It was now clear that I wasn't going to make it back to Amsterdam for my early morning flight to Moscow. So I called Schipol Airport, canceled my reservation for Wednesday and made a new one for Friday. I also sent a telegram to Moscow, telling Leonid Kogan that I was not coming Wednesday. It got there Thursday.

That evening, we opened Charlie up with the Customs Official there to certify that he wasn't full of marijuana or diamonds. We plugged him in, warmed him up, and filled him chock-full of high quality time. We sawed an air hole in his box, through which passed his power cord, and the following morning early, we took him to the Customs Office, had his symbolic string with lead seals wrapped around him again, and had his accompanying papers stamped and initialed.

That day I attended the dedication of the 100 meter radio telescope, whereat 600 people were treated to speeches, guided tours, and a lunch of pea soup, two beers, and one schnapps. But, that night, again disaster. In carrying the clock from the telescope back to the Institute, the symbolic string had hung on the trunk latch and snapped. Recalling in vivid detail the history of German jurisprudence from Wallenstein to the Nuremberg Trials, I went screaming for help to Heinz Wendker, who prevailed upon Herr Zann, who in turn was able to persuade his customs official friends to reinstall the string without jailing me in the process. This they did on

Barry Clark at a ten-year awards ceremony in Green Bank, 1975. L to R: Bill Shank, Mort Roberts, Barry Clark, Charles Sutton, Bill Howard, Toby Mann, James Coe. Shank and Coe designed and built the receivers and electronics for the Green Bank Interferometer.

my way back to Amsterdam Thursday morning. I got back to Schipol Airport at about 4:30 in the afternoon, just as the customs men were leaving. The man left on duty couldn't process Charlie's papers, but they did let me put him in bonded storage, plug him in, and cut the symbolic string to flip his battery charger to high.

I got back to the airport at 8:30 the next morning, figuring that was about right to unsnarl the paperwork to catch my 1 PM flight. The Customs Officials were there in force by that time. But, Disaster — The German customs agent should not have stamped and initialed Charlie's papers, but should have kept them and issued a new set. The Dutch customs agents set to work to unscramble things. I told them that if they needed me, I would be in the waiting room next door, and got out my book for a little reading.

At 11:00 I went back to the Customs Office to see how things were coming on. Disaster — The lack of a document from Germany had been overcome, but meanwhile, they had found that Charlie was on the strategic materials list. They, as a signatory to the NATO, could not let such an item go to Moscow. Meanwhile, they had forgotten I said I would wait next door in the waiting room, and had called all over Holland trying to find me. I produced a copy of the U.S. export license, which said we could take Charlie to Russia (I had left another copy with the clock, but they didn't think to look there). They considered this item awhile. Then they decided it was not sufficient, since it was a photocopy, with no rubber stamps (except a photocopied one) or initials. They would call the U.S. Embassy in the Hague to find out what they should do. They predicted that The Hague would call Kennedy Airport, where the original export license was. But anyway, the appropriate man to ask at the Embassy was out to lunch.

After waiting an hour and a half, The Hague called back, and the American at the Embassy chatted with me a while, learned that I did not profess to be an atomic spy, and told the Dutch Customs to go ahead and honor my xerox copy of the export license. The formalities completed, we walked out into the waiting room to the big window and watched my airplane take off.

Meanwhile, back at the airport, I made a new reservation, and sent a telegram to Moscow, telling my friends there that I would arrive on Saturday, not Friday (the telegram arrived Monday).

I was getting rather impatient about getting to Moscow by this time, and had gone ahead and made reservations for the next morning, despite the fact that I couldn't get a direct flight, as I had had before, but had to go via Copenhagen. So Saturday morning I showed up at the airport at eight, presented my multi-initialed and rubberstamped piece of paper, and was permitted to check Charlie, as baggage, on the Copenhagen flight, rather to the relief of the local customs boys, I think.

In Copenhagen, I spent my stopover admiring the scenery— that is, the baggage department scenery, as they unloaded Charlie's box from KLM, changed him from one cart to another, and drove him out to Aeroflot. Then off we went to Moscow, not over an hour late.

So there I was, in the Moscow airport, carrying a 150 pound clock, with no one to meet me (because my telegram would not be delivered for two more days), and with the clock battery pack using its last few minutes of capacity. After some time, I managed at last to attract the attention of an English-speaking tourist aide. "Could I plug the clock in and let it charge its batteries?"

"It would be better to do it at your hotel. In any event, it is not possible to do it without clearing customs."

"Could I call my friends by telephone to help me get my clock through customs?"

"It would be better to do it at your hotel. In any event, it is not possible to do it before clearing customs." With a final "Nothing can be done," she vanished into thin air and left me to face customs.

So there stood Charlie and I, neither of us with more than a minimal grasp of the language, and one of us capable of doing nothing more than ticking. Furthermore, Charlie's battery voltages were getting disastrously low, and he would shortly stop, and all the high quality time with which we had filled him through so much tribulation at Bonn would quickly leak away. So, with some trepidation, I got in line for a custom's inspector who had been pointed out to me as speaking a little English. As I hefted Charlie onto his counter, what to my delighted eye should appear but Matveyenko and Kogan from the Space Research Institute, and Marshall Cohen and his daughter from Caltech, who were arriving for the same experiment! With Kogan to translate, Charlie and I quickly cleared customs (the inspector seemed more interested in my large collection of paperback books and in the Dutch and German coins I had picked up for my kids than in Charlie). Safely clear of officialdom, we went outside and sat down on the sidewalk. (Kogan produced the Russian proverb equivalent to "It never rains but it pours," with the operative noun changed to Americans; he had come to meet me twice already, unsuccessfully.)

At this point Charlie's remaining life was measured in seconds. His batteries had decreased from 27 volts to below 24, the limit of the meter. His power supply had fallen out of regulation, and the stream of electrons was starting to congeal in his arteries. But, miracles abound! The Russians had brought with them a battery of their own, because Marshal Cohen was carrying a crystal oscillator which he was keeping warm (he hadn't had it filled up with time because it is so much leakier than Charlie).

I didn't dare disconnect the main batteries and plug in the Russian 24 volts directly, because I was afraid that the fifteen minute reserve battery was too low to carry the clock even for a few seconds. So we took half the Russian battery, and connected it to the inverter, which takes 12 volts DC and makes 60 Hz AC 120 volts, which the power supply converts back to 24 volts DC. Comparing in my mind the size of their battery with Charlie's own, I concluded that it was good for about an hour, which, coincidentally, is the time it takes to drive from the airport to the hotel.

The next problem was that Charlie would not fit inside the car's trunk, and it is apparently illegal to drive in the Soviet Union with the trunk ajar but tied down. (Charlie had fit nicely in the trunk of a rented Opel in Holland and Germany, but in other areas the Volga is a larger auto.) Finally, Matveyenko vanished into the hinterlands, and returned in a surprisingly short time with a taxi. Charlie and his keepers (Kogan and I) were banished to the taxi, while the others rode off in the Institute car. Charlie rode inside and I held my arms around him to keep him from scratching the dashboard (most Russian drivers take a fierce pride in their cars) or disconnecting his wires.

The next problem, at the hotel, was to get Charlie up to our room. The Academy of Sciences Hotel in Moscow has two small elevators. (I could write a separate treatise on "Soviet Elevators I Have Known" with an appendix dealing with stairs.) It was clear that, at most, Charlie and two people could fit in an elevator. Further, it was a rather busy time for the elevators, and they were carrying five or six people at a whack, and still there was a queue. Now the Soviet ethos on queue waiting is a sophisticated and developed one, with subtleties far beyond my primitive sociological insight, but it soon became clear that, as far as the queue was concerned, Charlie was an unperson, and that we who chose to associate ourselves with him could darn well wait until after the queue had dissipated. But after a while, us pushy Americans managed to commandeer an elevator and hijack it to the third floor. We hustled Charlie into the room, exchanged his European type line cord plug for a Soviet type, and plugged him in, at last, to the good old wall socket.

Sunday was spent in shameless weekend tourism. Monday, among other things, I went out to the airport to see if the other VLBI gear had safely arrived (it had) and whether it would fit in the cargo airplane the Institute had chartered to fly it to the Crimea (it would, though there was some confusion about some crates, whose weight in pounds was written on them, but was marked kilograms). When we got back to the Academy Hotel at dinner time, after about nine hours absence, we found Charlie unplugged. The hotel maids, deciding that a ticking box with glowing lights and American labels must be up to no good, had unplugged him for the safety of Hotel and guests. Fortunately, such a weighty decision apparently involved a large discussion to decide the question. This conference, what with scheduling problems and the like, did not reach its decision until 3:00 PM, and Charlie had been on batteries for only 3 hours, and was not discommoded at all.

The next morning Charlie and I went out to the airport to emplane for the Crimea. Since he was a bit heavier than the usual suitcase, we carried him up the stairs ourselves (instead of letting the Intourist lady do it, as foreigners usually do) and plunked him in the plane's luggage compartment. Three hours later we were in sunny Simferopol in the middle of the Crimea, and I was again met by Kogan and a car hired by the Observatory. This time, by removing the lining, we managed to get Charlie in the trunk with the lid closed. Charlie indicated that he had about three hours of battery life left, and it is about a three hour drive from the Simferopol airport to the Observatory. Therefore, it seemed a good idea to connect the inverter which would let Charlie run from the car battery, though not charge his own batteries. Kogan and the driver concluded that making this connection was not a suitable activity for an airport parking lot, so we drove out and parked by the roadside a few miles toward the city of Simferopol. A crowd of small boys materialized from nowhere to watch the operation. They were, however, more interested in the car than the wiring. The car was a new model Volga, which looks like a 1963 Pontiac, and is still rather a rarity. (The old model Volga looks like a 1953 Pontiac, and I never ride in one without a pang of nostalgia for a car of that vintage which was one of the cherished possessions I brought into my marriage, which my wife regarded as totally disreputable and undesirable, along with the suit I graduated from high school in.) One boy asked the driver how much the car cost, and he replied, "Fifty kopecks."

Mike Balister said the clock could be run this way from twelve volts, though the batteries could not be charged. However, the electromotive force of the lead sulphate reaction appears to exhibit a preference for political or economic ideologies, and it ended up with the car battery supplying about a fourth of the power, and Charlie's internal batteries the rest. Figuring we were at least preventing disaster, we went on out to the Observatory without incident, with only a brief stop at Yalta to ask Charlie if he was going to make it OK.

The next day the rest of the VLBI equipment arrived. There was also supposed to be a crane, which would lift the heavy tape unit up into the antenna. But late in the afternoon Ivan Moiseyev came up to me and informed me that "The crane will not." So he rounded up all of the Observatory's male employees and they picked up the 650 pound box and lifted it ten feet into the antenna base.

We then fetched Charlie from his nest in the lab building, and carried him over to join his friends. I began to hook things up to check for the dislocations of travel. As I worked, I kept getting these annoying tingles as I touched our equipment. Muttering things about "ground loops" and "induction," I asked for a ground wire to get rid of the problem. I was handed one, but when I approached our equipment with it, I was rewarded with a shower of sparks and a firm "thunk" as the breaker tripped. It seems that somewhere in his power supply, or as Mike Balister later theorized, in his power cord, Charlie's case had become connected to one side of the power line, and both sides of a Soviet 220 volt line are at 125 volts above ground. So long as Charlie was in his wooden crate, he didn't bother anybody, but when we connected him to the rest of the equipment, he was well on the way to booby-trapping half the receiver when we caught him. We very quickly converted him to 110 volt operation, where one line is indeed neutral.

This has been the story of how Charlie and I carried the time from Ghent to Aix, and it was apparently as hard on Charlie's nerves as on mine, and on those of the many people who helped me along the way. On his return from the Soviet Union, Charlie had to be remanded immediately to the intensive care unit of the local rubidium clock hospital.

Barry Clark examines digital recording equipment from the Leach Corporation, 1970. At right, Leach engineer Bob Andersen.

Epilogue

The material in this book portrays moments in the life of a scientific institution, a community, and a place. The founders of NRAO faced three challenges: to create a national facility dedicated to building and operating state-of-the-art instruments for the use of the entire community (at that time a new concept with few precedents), to foster the then-marginal discipline of radio astronomy, and to do it all at a remote location.

When the NRAO was established, the future of radio astronomy could not be predicted. Was it to be a useful though limited partner to the dominant optical studies, a hobby of radio engineers, or a full-fledged discipline supplying unique insights into the Universe? And who would end up using these enormous and expensive specialist instruments? Was the notion of a national center—an institution devoted to service—even viable in practice? And could it all be done in such an unprepossessing location: near the village of Green Bank, in the Deer Creek Valley, Pocahontas County, West Virginia?

The instruments that today populate the Valley trace the history of astronomy at centimeter wavelengths. The Tatel 85 Foot, NRAO's first telescope, discovered that the surface of Venus was hot enough to melt lead and mapped the clutch of objects at the center of the Milky Way, naming them one after the other: Sgr A, Sgr B, Sgr C, ... The Green Bank Interferometer found the radio source around the black hole at the heart of the Milky Way, and pioneered studies of radio stars. The 140 Foot Telescope detected signals from formaldehyde, the first organic molecule known in interstellar space, and mapped the star-forming regions of the Milky Way. The 300 Foot Telescope discovered the pulsar at the heart of the Crab nebula and, while charting the distribution of hydrogen around galaxies, showed that the gravity in nearby galaxies was dominated by dark matter.

By the end of its first decade, the NRAO had become a national facility known world-wide, and the discoveries it engendered dragged radio astronomy from the margins into the heart of modern science. Indeed, radio astronomy has arguably had the greatest scientific impact of any activity funded by the Division of Astronomical Sciences of the National Science Foundation.

But most of the seeds that first germinated in Green Bank were destined to sprout elsewhere. The first search for radio signals from extraterrestrial civilizations was made with the Tatel Telescope, but its successor is the Allen Telescope Array now under construction in California by the SETI Institute and the University of California, Berkeley. The first experiments at NRAO in astronomy at millimeter wavelengths began behind the Jansky Lab, but within a few years moved to a higher, drier site on Kitt Peak in Arizona with the construction of the NRAO 36 Foot Telescope (later the 12 Meter), and have now reached their culmination in the international ALMA telescope rising in the Atacama Desert of Northern Chile. Likewise, the Green Bank Interferometer taught the NRAO staff how to combine signals from widely-separated telescopes in the technique of aperture synthesis; the lessons were applied to create NRAO's Very Large Array on the Plains of San Agustin, 50 miles west of Socorro, New Mexico. And again, the early experiments in Very Long Baseline Interferometry at the 140 Foot, so colorfully described in this book, led to the creation of the NRAO's Very Long Baseline Array of 10 dishes scattered around the U.S. from Hawaii to the Virgin Islands. None are in Green Bank.

The staffing at Green Bank followed a similar curve. It is a paradox of radio astronomy that it must be done in the most remote locations possible, while requiring a diverse group of talented, creative individuals to make the enterprise succeed—the kind of people found more often on a university campus than in a mountain valley. The first and second challenges facing the NRAO were met after some struggle: radio astronomy has now contributed to our understanding of the Universe in ways even the founders of NRAO could not have imagined, and the NRAO has succeeded as a national facility dedicated to service. But the third challenge proved too much. Eight years after groundbreaking at Green Bank, and a few months after the dedication of the 140 Foot Telescope, the astronomers began to leave.

By 1967, the majority of the scientific staff had relocated to the new NRAO Headquarters building in Charlottesville, VA, and over the next decade many of the technical staff followed, not only to Charlottesville, but to the new facilities on Kitt Peak and in Socorro. By the 1980s, the future of the Observatory at Green Bank had become uncertain. The National Radio Quiet Zone was a unique and irreplaceable resource, but at its heart the instruments were no longer state of the art, though none had actually been superceded in the U.S. The NRAO has a guiding principle that it operates only unique instruments, but development of new telescopes for Green Bank ceased after the small remote dishes were added to the Interferometer in the 1970s. In 1978, the Interferometer itself was removed from service when the VLA became available with much greater capabilities. By dint of superb engineering and outstanding service to visitors, the 140 Foot and 300 Foot telescopes remained competitive, their electronics unsurpassed in sensitivity, but the central instrument for Green Bank, the Very Large Antenna envisioned at the time of the founding of NRAO, had not been built.

The Radio Astronomy Panel of the 1980 Decadal Astronomy Survey Committee noted in its report that: "All of the previous studies of the needs of U.S. Astronomy have recommended the construction of a large general purpose radio telescope to work at wavelengths of roughly one centimeter and longer. In the judgement of the panel on Radio Astronomy the arguments for such a facility remain very strong, and an instrument in the 100-meter class is an important priority for the 1980s." But it was not given the highest priority and was not funded.

Beginning in late 1987, probably triggered by the 300 Foot 25th birthday party, there were scattered discussions among the scientific staff at NRAO about the need to replace one of the existing instruments at Green Bank. For some, there was the feeling that after 25 years the 300 Foot had done about all it could and should be replaced. But Bernie Burke of MIT wrote to NRAO Director Paul Vanden Bout suggesting that the 140 Foot Telescope, not the 300 Foot, be retired in favor of a more modern instrument. His letter had the effect of crystalizing the many informal discussions around the goal of producing a report that would evaluate the situation and suggest possibilities. A few internal meetings were held, a memo series was begun, industries were queried about construction costs, and in the coffee rooms and corridors of NRAO ideas were batted about, unfettered by constraints of practicality, and oblivious to the prospects of obtaining funding for any new instrument.

Everything changed on the morning of November 16, 1988, when we awoke to news that the 300 Foot Telescope had collapsed the previous evening. The investigations of a new telescope were hastily codified in a report, and representatives of the U.S. astronomical community met in Green Bank a month later to assess the magnitude of the loss to science. The tempo of the project took a brisk turn when it appeared that the National Science Foundation would be given supplemental funding for a new telescope, through an emergency appropriation initiated by West Virginia Senator Robert C. Byrd. The subsequent years of design, construction, and now operation of the Robert C. Byrd Green Bank Telescope have been a lot of work but also, once again, a lot of fun.

Green Bank in 2007 is a different place than it was fifty years ago. For one, it is much more connected to the rest of the world. The goods we order over the high-speed internet lines offered by the local phone company arrive daily in the same express trucks that ply the streets of every major city. We routinely participate in videoconferences with other NRAO sites and attend conferences remotely. There is an active scientific and engineering staff. The Pocahontas County Libraries, with a branch in Green Bank, recently won a national award as an outstanding rural library system. The Opera House in Marlinton has been restored and holds a regular concert series, and we're half an hour from a major ski resort. But Pocahontas County is still a very rural area, with a smaller population than in 1956. We still have to travel a considerable distance for many medical services, and there's not a university in sight.

The real shock for most visitors, though, is the absence of cell phones, wireless networks, text messaging and other similar "necessities" of modern life. Welcome to the National Radio Quiet Zone! In houses at the Observatory, microwave ovens and cordless phones are restricted. Inside the gate, in the Radio Astronomy Instrument Zone, even digital cameras are banned. Because of the need to maintain low levels of radio emission—the very reason Green Bank was chosen in the first place—in many ways Green Bank in 2007 is less typical of the United States than it was in 1957.

The style of radio astronomy has also changed dramatically. When NRAO was founded, some thought that it would/should be a facility for hands-on science to which astronomers would decamp for weeks or months at a time, somewhat like archeologists going on a dig. Now the GBT is scheduled dynamically to take advantage of the weather, many programs are run each day, and it is regularly used remotely by astronomers and their students, some of whom couldn't find Green Bank on a map.

In his wise and witty talk at the 140 Foot Birthday Symposium, David Nice extrapolated from past serendipitous discoveries in radio astronomy to the conclusion that "Over the next 25 years or so of the GBT era we should expect about four more discoveries comparable to the Hulse-Taylor binary." This is a reference to the discovery of the binary pulsar whose subsequent timing confirmed Einstein's theory of gravitational radiation and won its discoverers the 1993 Nobel Prize in Physics. We're now just a few years into the GBT era, but already there are some GBT discoveries that might make it onto Nice's list. It is a grand time be an astronomer at Green Bank.

As an end to this epilogue, and an introduction to the book that will someday be written about the next 40 years of astronomy at Green Bank, we reprint portions of a letter by NRAO Scientist Ken Kellermann, sent out less than two weeks after the collapse of the 300 Foot. At that time prospects for funding a replacement instrument were extremely uncertain, but the process that led to the Green Bank Telescope, and another 40 years of science at Green Bank, had begun.

from a Letter to the Astronomical Community

by Ken Kellermann
November 28, 1988

Dear Colleague:

Earlier this year NRAO began a study of modern filled aperture radio telescopes with the thought of possibly replacing the 140-ft and 300-ft antennas. Over the past year a small group has been discussing performance specifications and design concepts, and we have talked with and have received written comments from a number of people. We have also held one internal meeting in Charlottesville to discuss these issues. As NRAO is committed to the Millimeter Array as its next major project, and as the current climate for funding has not been encouraging, this has been a low key effort.

However, in view of the recent loss of the 300-ft, it is appropriate at this time to distribute a preliminary draft of our report, in the hope that it will stimulate interest in replacing the 300-ft antenna, and that it will serve to focus the discussion leading to the final performance specifications.... Our present needs appear to call for something which is at least as big as the 300-ft and performs at least up to 22 GHz.

....

A site in Green Bank, WV, is suggested to exploit the unique support available there and to gain maximum protection from radio interference....

It must be emphasized that the kind of instrument we are discussing is not available off the shelf....

Index