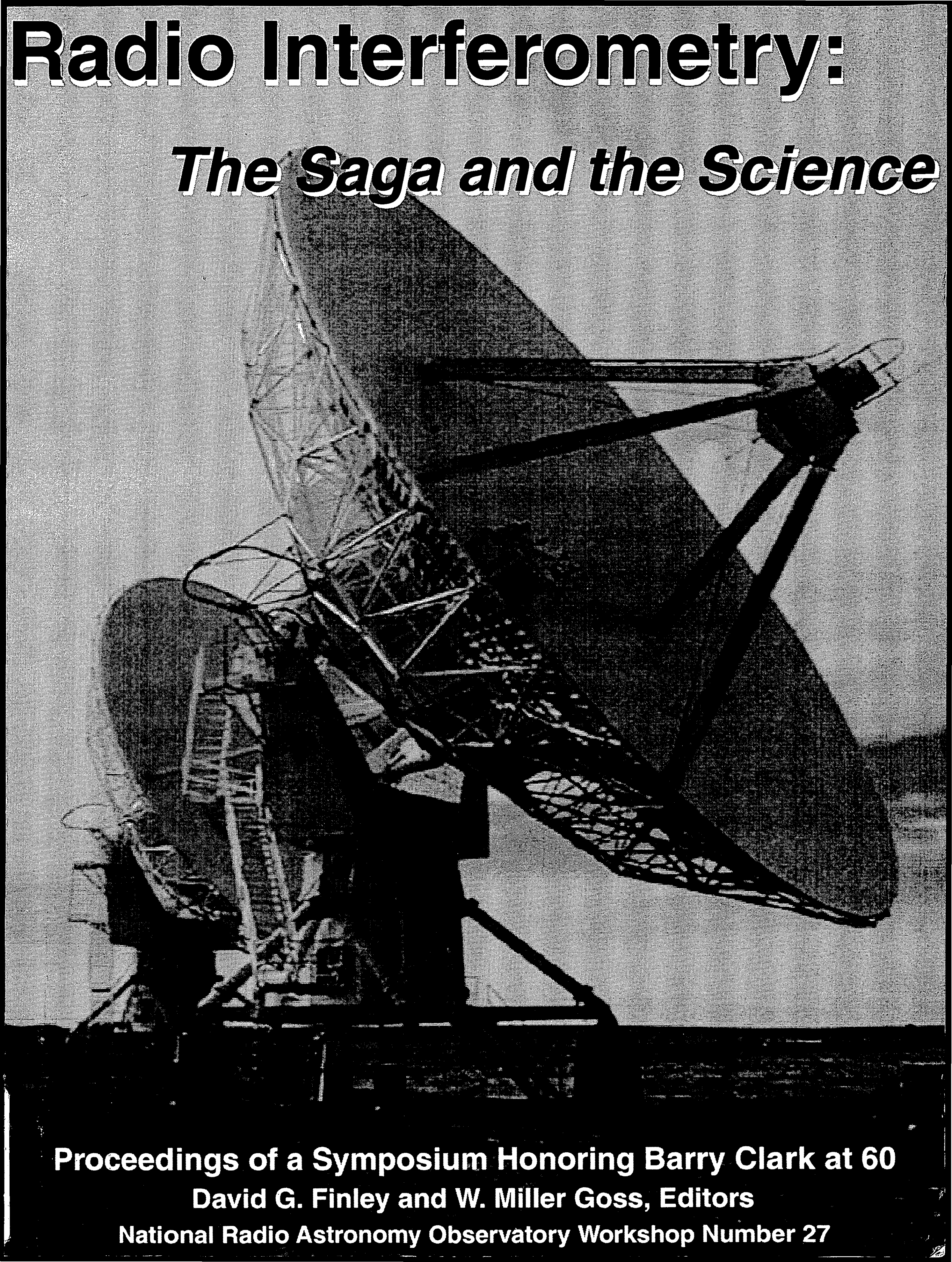
Radio Interferometry:

The Saga and the Science



Proceedings of a Symposium Honoring Barry Clark at 60

David G. Finley and W. Miller Goss, Editors

National Radio Astronomy Observatory Workshop Number 27

Radio Interferometry: The Saga and the Science

Proceedings of a Symposium Honoring Barry Clark at 60
Held at Socorro, New Mexico
June 25 - 26, 1998

David G. Finley
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National Radio Astronomy Observatory

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Dedication

On behalf of the National Radio Astronomy Observatory and the world-wide radio astronomy community, we dedicate this volume to Barry and Betty Clark.

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Foreword

June of 1998 was a busy time for the National Radio Astronomy Observatory staff in Socorro. In a single month, we hosted an NRAO Users Committee meeting, a Synthesis Imaging Summer School, and a scientific and engineering meeting on the VLA Upgrade, now known as the VLA Expansion. Sandwiched between the Summer School and the Upgrade Meeting, there was another, very special event: the symposium honoring Barry Clark.

Officially, the Clark Symposium was in honor of Barry's 60th birthday, although his birthday actually had passed on March 5. The symposium was attended by nearly 150 astronomers from four continents, all of whom came to honor the man considered to be "the brain of the VLA," but whose contributions to radio astronomy began long before the VLA was constructed. From single-handedly developing the on-line program for the Green Bank Interferometer in the early 1960s, to playing a key role in the first Very Long Baseline Interferometry experiments in the summer of 1967, to guiding the design and implementation of the control system for the Very Long Baseline Array in the 1980s, Barry Clark has devoted himself unselfishly for nearly four decades to building ever-better tools for the world's radio astronomers.

Among all the Socorro events that June, the Clark Symposium had a distinct, definitely celebratory, flavor. In many ways, this symposium was like a Homecoming Week for radio interferometry. Not only did we have in attendance many of the leading pioneers of this technique but also many astronomers whose research had benefited from the work of those pioneers. It was a time that saw much reminiscing about the early days of interferometry and also an unabated enthusiasm for the future of radio astronomy. We are happy to note that much of the flavor of pride of achievement, humorous commentary, and eagerness for the future that permeated this meeting has found its way into the pages of this volume.

This symposium was a pleasure to host and a joy to savor as speaker after speaker, sporting the suspenders that Barry issued to each, told us about their topic and the contributions Barry had made to the field they discussed. Through it all, Barry endured with great good humor all the stories told about him. We, as well as all the participants, are grateful to Barry, not only for all his contributions to astronomy outlined in this volume, but also for his graciousness and humor during the symposium.

We wish to thank the authors of the papers in this book, for spending the time to preserve in print both the proceedings of an excellent symposium but also, we feel, some important aspects of astronomical history. We are impressed by the quality of these papers, which we think make a valuable contribution to the astronomical literature. Our additional thanks go to those authors who submitted and re-submitted files, as we fought our way through a number of file-format difficulties.

We also wish to thank all those who helped organize and conduct the symposium of which this volume is the record. Special thanks go to Ron Ekers, Ken Kellermann, Paul Vanden Bout and V. Radhakrishnan in putting together the scientific program and Terry Romero in organizing the conference.

David G. Finley
W. Miller Goss
Editors

Introductory Remarks

Paul Vanden Bout

Director, National Radio Astronomy Observatory

Welcome to this symposium. It was 1933 when Jansky published his discovery, and it made the front page of the *New York Times*. It also was noticed by the *New Yorker* magazine. The Talk of the Town columnist said it was "...believed to be the longest distance to which anyone had gone in search of trouble." Astronomy was never the same, of course. Five years later there was an event in the state of Texas that the *Times* and *New Yorker* missed, but it had a very profound effect, in due course, on Jansky's field. Of all the little quaintly named towns in Texas where you could choose to be born, Barry has one of the best. It is not Noodle, Oatmeal, Raisin, Plumb, or Muleshoe. Not Dimebox, Tuxedo, Blessing, Cut and Shoot, or Wink. It is Happy!

But this is a serious scientific symposium, and we want to set the tone right at the beginning. So I am going to tell a Barry Clark story. It was my first conversation, I believe, with Barry. Certainly the first one on the telephone. Quite a while ago, I had asked for some VLA time and in due course a notice came in the mail. It was terse to the point of being cryptic, but I could figure out that due to some happy lapse in judgement, the referees had decided that I should have a few hours. But these were to occur on a very bad, very inconvenient day. Ah well, no big deal, I would just ring up this B. Clark and get a better day. So I dialed the number and waited. After a while I heard, "Clark." "Hey, Barry, how are you doing? Good to talk to you there...I got this notice and I'm really happy about my time, and uh, looking forward to it. But you know it's on a bad day. You know how it is around the holiday season...things get busy. It would be tough to make it. (And of course you had to go to the VLA.) -- pause -- Hey Barry, you still there? You know it really is a bad day, and I think on reflection, it might be our wedding anniversary." -- long pause -- "Well, it really would be very difficult to change." "Oh, you mean you think it would be best if I just came on the day you picked?" "It would be best." And so a valuable lesson was learned very early - Barry really does know best.

I have just one thing to point out to the speakers that follow. This conference has the highest Johnstown Index of any conference that I have ever seen. That index, of course, is named after the fellow who got swept away in the great Johnstown flood and went on to make a reputation for himself in heaven telling his story. He started out small over coffee, then worked his way up to Kiwanis Club and Rotary events, and then a huge mass audience as far as the eye could see. He was introduced and as he was getting to his feet to go to the podium, St. Peter leaned over and said, "Watch yourself...Noah is in the audience."

Section I: HI and the Interstellar Medium

Opening Remarks by Session Chair

V. Radhakrishnan
Raman Research Institute and NRAO

There have been many exciting periods in the development of radio astronomy over the last half century, often associated with the appearance of a new instrument revealing unknown phenomena or the application of new techniques to the study of existing phenomena. The driving motivation for the Dutch, who pioneered the search for HI, was to use it to probe the rotation of the Galaxy beyond the tiny volume in which one could see stars before obscuration closed our view. The appearance of the Caltech Interferometer working at high (for those days) frequencies, and its pioneering applications to spectroscopy and polarimetry, contributed to shifting the focus from Galactic Rotation, which was Oort's passion, to looking more closely at the temperature, density and magnetic structure of the ISM. In this first session, Bob Wilson is going to review the progress made in those early days, and Carl Heiles will bring us up to date on the present status of our understanding in this area of Galactic astronomy.

My recollections of the early days at Owens Valley are of a period of intense cooperation with a bunch of talented individuals who had been collected by John Bolton, and the great enjoyment I got from talking and working with them. If you ran into Barry in those days, he would have been manipulating a shovel, not a keyboard, and appearing consequently more perilous to approach. And conversation, then like now, tended to be one-sided, apart from the occasional grunt, generally of disagreement.

A man of few words, you might say, but not of little action. Some time ago, I was chatting with a distinguished and perceptive scientist who remarked of a younger man who was rapidly climbing to fame, "he has done well for himself, hasn't he." Knowing both parties, I couldn't help noting how well chosen were his words. And it also struck me that that was a description which could never have been applied to Barry Clark, whose career has been to do well for others, for you and me, for the development of radio astronomy, and for the progress of science. It is to acknowledge precisely this for which we are all gathered here on this occasion.

Caltech Contributions in the 1960s

R.W. Wilson
Harvard-Smithsonian Center for Astrophysics

I. Introduction

Barry Clark was a smart, strong, and practical Caltech undergraduate and John Bolton hired him to help out in the early days of establishing radio astronomy at Caltech. I joined the group later in 1958 and my first summer in the Owens Valley was in 1959. My first memory of Barry was that after my wife Betsy and I had gone to the observatory that summer we heard that Barry was coming. Barry had a Vespa motor scooter instead of a car and was driving his Vespa from Pasadena to the observatory that day. As expected, a very sunburned Barry arrived on his Vespa late in the day. A few days later, Barry invited Betsy to go for a Vesp with him which she did with pleasure.

The first paper in the "Yellow Jacket" series of preprints from the Owens Valley Radio Observatory is outside my assigned topic, but in looking back at it, I feel compelled to comment on it. The paper was "A Solar Occultation of the Crab Nebula at a Wavelength of 12 Meters," by Bolton, Stanley, and Clark. In contrast to today's typical paper, it consisted of two typed pages and one page of figures. The results paragraph was aimed more at other radio astronomers than the general astronomical community.

II. Pre-Interferometry

The Owens Valley Radio Observatory was built to investigate the small scale structure of extragalactic sources, but as the two 90-foot antennas were completed, they were fitted with radiometers. A program of single-dish astronomical observations was started and grew well beyond what was needed to check out the individual antennas. The first antenna received a 960 MHz continuum receiver and the first project with it was Dan Harris and Jim Roberts' CTA small diameter source survey. The second was a map of the Galaxy which John Bolton and I started and eventually became my thesis. We produced a source list (CTB) and a map of the Galactic plane visible to us. I later analyzed the disk component into models of the distribution of thermal and non-thermal components as a function of galactic radius and height above the plane. I don't think that these results had much effect on the subsequent understanding of the ISM, but it did give me a good introduction to characterizing antennas and extracting results from maps. Curiously enough, I now discover that in fitting the models to the maps, I needed to add an isotropic 2.8K to the model's thermal component to obtain a good fit. This must mean that the extrapolation I used to obtain the zero level of my map at 960 MHz happened to produce a remarkably fortuitous result.

The second 90 foot antenna received a 21 cm receiver which could be used in a frequency switched mode to observe the hydrogen line. The most important ISM work of the pre-interferometer period was done with it just two days before John Bolton converted the observatory to interferometry. V. Radhakrishnan began making Hydrogen line absorption measurements and measured a cloud "at a temperature certainly no greater than 60 K," corroborating earlier suggestions by Heeschen and Davies. This started one of the themes of the observatory for the next few years which culminated in Barry's thesis.

III. Interferometry With the 90-Foot Antennas

If one is lucky, graduate school days can be a heady experience of intellectual challenge and learning that one can contribute to a field. We had a new instrument and the correlation receiver of an interferometer is amazingly powerful at rejecting unwanted signals compared to the desired source in the main beam of the antennas. In my memory, Rad provided the intellectual challenge for exploring what the interferometer could do. We had many long discussions and much private thinking about how to combine feed polarizations, the two sidebands, delay setting, and fringe phase to measure what we wanted.

A. Polarization

Rad, Dave Morris, and I concluded that our interferometer would be particularly good for measuring polarization of radio sources. Radiation in the polarized side and back lobes either doesn't correlate or has a different fringe rate. By using cross polarized feeds (either circular or linear) on the the two antennas, the unpolarized radiation of the source itself could be canceled out, leaving fringes from the polarized component alone. Since the interferometer was not designed to measure polarization, one of the first things we did was to build a feed rotating mechanism for the antennas. The first model used an automobile starter ring gear and pinion from an Owens Valley junk yard with a stepping motor as a drive system. Rad and I were particularly proud of a vernier drive for the stepping motor which we made with two microswitches and a hand turned cam. Barry will describe the mechanical computer which Fritz Bartlet made for tracking fringes and delay and integrating fringes in the correlator output.

Rad, Dave, and I decided to look for the Zeeman shift from the magnetic field in cold neutral Hydrogen clouds with narrow HI absorption lines as had been suggested by Bolton and Wild earlier. In the course of the measurements, John lost faith in our ability to succeed and Morris, Clark, and Wilson wrote a paper summarizing the results so far. We showed that Davies, et al.'s measurement of a relatively high field in the cloud in front of the Crab nebula was wrong and Sandy Weinreb was right. This paper consisted of four pages of single spaced typewritten text.

There followed a number of papers on the polarization of radio sources and a paper on the interpretation of polarization measurements with an interferometer by Morris, Radhakrishnan, and Seielstad, but these are outside the scope of this talk. A final ISM result was published by Morris and Berge who analyzed the rotation measure of the sources and concluded that the local magnetic field is directed along the local spiral arm approximately in the direction of l=70 deg.

B. Hydrogen Line Absorption

The interpretation of measurements of the absorption of continuum radiation of background sources by foreground cold HI clouds with single dishes was made difficult by the need to interpolate the radiation of the foreground cloud at the position of the radio source using measurements far enough from the center of the background source to exclude it from the main beam of the antenna. An interferometer can help by interpolating on the scale of the fringe spacing rather than the beam size. Looked at another way, if the cold cloud is large enough in diameter to be resolved out by the interferometer, a measurement of the background source through it will give the optical depth of the foreground cloud directly. The first paper on this technique by Radhakrishnan, Morris, and Wilson was followed by another by Clark, Radhakrishnan, and Wilson which reported measurements of six sources and laid out the principles of these measurements.

In my opinion, the most important ISM result from the 90-foot interferometer was in Barry's thesis. He did a much more thorough job of measuring the 21 cm Hydrogen line absorption in front of five sources and analyzed the results to obtain statistical results about the distribution of cold clouds in the ISM and their properties. The important result, however, is relegated to the last section and not mentioned in the abstract. In that section, Barry suggests that Hydrogen in the ISM exists in two phases, a hot ($T > 1000K$) phase with negligible absorption and the cold clouds ($T < 100K$) which are seen in absorption. He suggests that the two phases are in pressure equilibrium since the cold clouds are not bound by gravity and would otherwise disperse. This ushered in the modern understanding of the ISM and its multiple phases in pressure equilibrium.

[The basic idea that the Hydrogen seen in absorption is physically different material from that seen in emission came from John Bolton, who remarked that this seemed to him to be the only explanation for the very different appearance of the spectra. - B.G. Clark]

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HI and the Interstellar Medium:
A Selection of Current Problems

Carl E. Heiles
University of California, Berkeley

1. HI Temperatures

Barry Clark's thesis (Clark 1965) was entitled "An Interferometer Investigation of the 21-cm Hydrogen-Line Absorption," and he never returned to the subject again. On the one hand, this is a pity; people of his quality are needed in every field. But from the standpoint of history it is perfectly understandable. Apart from showing that interferometers were far and away the best way to measure HI absorption profiles against continuum sources, his interpretation laid the basis for all future discussion of the interstellar medium (ISM). He realized his data demanded the *two phase* HI, with cold clouds producing absorption and warm ones none; and, further, postulated *pressure equilibrium*, which ever since has been the basic theoretical tenet in ISM studies. Compared to these contributions, everything done since has been filling in the details. Once you've laid the fundamental cornerstone, it's time to move on!

The theoretical two-phase ISM was invented by Field, Goldsmith, and Habing (1969). Basically the idea is that, in pressure equilibrium, the ISM heating and cooling mechanisms dictate that there are not one, but two stable temperatures for the neutral gas. These are the warm neutral medium (WNM) and the cold neutral medium (CNM), labeled phases F and H in the FGH phase diagram; field G lies between and is unstable. More recent diagrams exist, but they are not terribly different. In the real world, we think we know what produces the CNM: shocks, in which the initially warm gas cools rapidly under conditions approaching pressure equilibrium. We can see these shocks with enough angular resolution, either in the 21-cm line or in the 100 μm IRAS emission. Good examples are 3C111 and 3C123, which exhibit strong 21-cm absorption (Dickey, Salpeter, and Terzian 1978); the IRAS maps show that they are sitting behind "filaments"—just as we expect.

The major feature of the FGH diagram is that the *temperatures* of the two phases are *very well defined.* All we observers have to do is measure the temperatures and we have determined the major facts about ISM thermal processes. Depending on the details of the heating process and the depletion of Carbon, one expects the temperatures to be about 80 and 8000 K.

The trouble is that the observations *don't* yield well-defined temperatures. For the CNM, Mebold et al (1982) provide a histogram of the derived spin temperatures. They find the distribution to be roughly flat between 20 and 140 K, with a tail extending up to 200 K. For the WNM, the temperatures are very difficult to measure because the 21-cm line opacities are so small, and in fact there are very few reliable values. However, there are disturbing indications based on line width that temperatures in the neighborhood of 1500 to 3000 K are not uncommon (Heiles 1989; Verschuur and Magnani, 1994); such gas cannot possibly be in thermal equilibrium, because this is lower than the WNM temperature.

Going further in considering theoretical predictions, let's look at the inner Milky Way (Milky Way), where the supernova rate is higher than nearby. This should raise the average interstellar pressure according to supernova-dominated theories of the ISM (e.g. McKee and Ostriker 1977) and, in turn, should produce more CNM because the higher pressure pushes gas above the F-G transition in the FGH diagram, where the only stable phase is the CNM. But on the contrary, Garwood and Dickey (1989) find that there is fractionally *less* CNM in the inner Milky Way than nearby.

So we are in a quandary: when examined in any more than the grossest detail, observations don't agree with theory. Perhaps the reason is that all measured HI temperatures are *wrong*. Because of the juxtaposition of gas of different temperatures along the line of sight, the absorption of the cold gas can be filled in by emission from intervening warm gas, and this leads to derived temperatures being too high. This is shown elegantly by the high-resolution study of Kalberla, Schwarz, and Goss (1985) towards 3C147: knowing the angular structure in detail allows them to correct for these effects, and they obtain HI temperatures in the range 35-65 K. Compare this to the range of 70-300 K they get using conventional techniques! Mebold et al (1997) have invented a clever technique that they applied to the Magellanic Clouds to derive temperatures in the range 30-40 K; their technique doesn't require high resolution observations of the emission and could easily be applied to Milky Way data. Perhaps if these better techniques were used, then the derived temperatures would cluster around a single value and be more in accord with theory. Who knows?

We can do a lot in this area. Milky Way work needs to be done more carefully to get accurate HI temperatures; being wrong by factors of 2 to 4 makes comparison with theoretical ideas impossible. And we need to extend the observations to different environments. Do supernovae increase the ISM pressure in accordance with theories like McKee and Ostriker? The way to find out is to observe gas in environments having widely different supernova rates. We can do a lot by sticking within the Milky Way, but external galaxies offer new laboratories. Mebold et al (1997) have made a first probe of the Magellanic Clouds, and Walterbos and Braun (1994) and Dickey and Brinks (1993) have made the first steps outside the Magellanic Clouds. They valiantly attempt to compare the HI temperatures in these galaxies to those in the Milky Way, but given the uncertainties in the Milky Way temperatures this is a fruitless undertaking!

2. Tiny-Scale Atomic Structure (TSAS)

There are many indications that the neutral HI is smooth on scales smaller than a few arcmin. Green (1993) and Crovisier and Dickey (1983) used interferometers to directly measure the angular power spectrum and found $power \propto size^{-n}$, with $n \sim 2.4 \rightarrow 3$[1]. Gautier et al (1992) found a similar result for diffuse IRAS 100μm data, computing the spectrum from maps. Crovisier, Dickey, and Kazès (1985) measured what was in essence the two-point structure function of 21-cm line opacity versus angular separation by using double-lobed radio sources[2]. Figure 8 of Dickey and Lockman (1990) shows good agreement between HI column densities from (a) the tiny pencil of a star, observed in UV absorption lines and (b) the broad radio beam used for 21-cm line emission. All of these studies point to an absence of significant small-scale structure.

However, there is one important caveat: in all measurements of column density, one averages along the line of sight, and if the medium consists of a large enough number of small enough blobs, then "root-n" statistics takes over and makes the medium look smooth. The extreme example of this phenomenon is the terrestrial atmosphere: it consists of discrete little blobs—air molecules—that are so numerous that the atmosphere seems totally smooth on macroscopic scales. Could this be happening in the ISM? Read on!

From the theoretical standpoint there is good reason for thinking that the ISM cannot consist of dense blobs. The thermal pressure of the gas P_{th} has an absolute upper limit given by hydrostatic equilibrium in the Milky Way atmosphere; this means $\frac{P_{th}}{k} = nT \lesssim 28 \times 10^3$ cm^{-3} K (Boulares and Cox 1990). More realistically, we'd expect P_{th} to be the typical cloud pressure, which

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[1] Curiously enough, this exponent differs (Lazarian 1995) from the value 3.7—the Kolmogoroff value—found from pulsar scintillation for ionized gas (Spangler 1988).

[2] It isn't *really* the structure function, or even its square root, because the normalization of the amplitudes isn't correct.

was measured using UV absorption lines to be $\frac{P_{th}}{k} \sim 3000\ \mathrm{cm}^{-3}$ K (Jenkins, Jura, and Loewenstein 1983). With this pressure and CNM temperatures $\sim$ 100 K the typical volume density $n(HI) \sim 30$ cm^{-3}. For the typical column densities $N(HI) \gtrsim 10^{20}\ \mathrm{cm}^{-2}$ that are seen in radioastronomical measurements, this means that typical scale lengths are $\sim$ 1 pc, and at typical distances of several hundred pc this means an angular size $\sim$ 10 arcmin. So it is no surprise that structure on smaller scales is not prominent.

That's one reason why, when the first indication of tiny-scale atomic structure (TSAS) was first observed with VLBI techniques in 1976 by Dieter, Welch, and Romney (1976), nobody really believed it—to the extent that it took 13 years before a confirmation was finally published by Diamond et al (1989). Now the TSAS is observed not only with VLBI, but also with time-variable 21-cm line profiles against pulsars (Frail et al., 1994) and optical spectra against closely-spaced binary stars (Watson and Meyer 1996).

It's interesting to put all these observations together on the "structure function" plot of Crovisier et al (1985). Figure 1 shows their plot for double-lobed sources; their points are the neat, printed, ones to the right of 0.01 pc on the horizontal axis. By hand, I've extended their horizontal axis down to $< 10^{-4}$ pc and inserted points from the VLBI, pulsar, and optical data mentioned above. We need to discuss some details.

First consider the optical points, represented by stars. These points seem anomalously high compared to other points at small length scales. We conclude that this is a signature of the different measurement technique, and specifically the signature is the following: the optically-measured opacity differences are systematically larger than the 21-cm line opacity differences. This tells us something very important!

The 21-cm line opacities are very straightforwardly given by

$$\tau_{21-cm} \propto N(HI)T^{-1} .$$

In contrast, the NaI optical D line opacities have no explicit temperature dependence: it's just $\tau_D \propto N(NaI)$. However, NaI is the minority ionization species—most of the Na is NaII because the first ionization potential is smaller than that of HI. This means that the NaI fraction is given by the ionization equilibrium, $\frac{N(NaI)}{N(Na)} \approx \frac{n_e \alpha}{\Gamma}$, where n_e is the electron volume density, α the recombination coefficient, and Γ the ionization rate. In the CNM clouds of interest, we assume that starlight can penetrate; this means that atomic Carbon is fully ionized and $n_e \sim n(C) \propto n(HI)$. Because of the tendency for thermal pressure equilibrium, we expect $n_e \propto T^{-1}$. Furthermore, all recombination coefficients tend to vary as $\alpha \propto T^{-0.7}$ or so. This means that

$$\tau_D \propto N(HI)T^{-1.7} .$$

Thus, the systematically higher fractional opacity differences for the optical measurements most likely are a temperature effect.

So for now let's disregard the optical points in Figure 1 and, also, the extreme case of 3C138 observed by Davis, Diamond, and Goss (1996). Excluding these points, the distribution looks roughly flat for length scales smaller than 0.1 pc. It rises for longer scales, which is in accordance with our above argument. It seems from both the data and the distribution of points that the TSAS is a *separate class of structure* from the ordinary structure seen at resolutions of an arcmin and above.

Heiles (1997) presented the beginnings of a comprehensive interpretive discussion of the

TSAS. For one, he used an approximate thermal equilibrium calculation to calculate a temperature $\sim 15K$. This confirms what we conclude from the discussion of the optical points in Figure 1: the TSAS is *cold*, and maybe even colder than 15 K: the colder the TSAS, the easier is its pressure equilibrium because its $P_{th} \propto T^2$. Heiles also includes the possibility that the TSAS morphologicy is not spherical blobs, but instead randomly-oriented anisotropic structures. With the combination of 15 K temperatures and disks or sheets, he can reproduce the observations without the thermal pressure reaching impossible heights.

Knowing this, we can go back and look at older data and indulge in some speculation. Dickey, Salpeter, and Terzian (1978) found a few absorption lines that are unusual, and I think that they are epitomized by 3C237. First, when one examines the IRAS 100 μm map, one sees that 3C237 sits in the "middle of nowhere." It lies in an empty region with $N(HI) \sim 2 \times 10^{20} cm^{-2}$ that is surrounded, on the scale of several degrees in angle, by denser gas. But on the IRAS map there is no discernable structural feature anywhere nearby 3C237. On this basis, one would expect the gas all to be WNM. But no! The absorption spectrum reveals a 21-cm line peak opacity $\tau \sim 0.23$. Combining this in the usual naive way with the off-source emission spectrum gives $T = 22$ K and $N(HI) = 1.2 \times 10^{19}$ cm^{-2}; a crude attempt at by-eye Gaussian fitting reduces both of these by a factor of about 2. These cold, low-$N(HI)$ clouds are exactly the parameters Heiles (1997) finds for TSAS. Add this to the fact that 3C237 is a small double radio source (separation $\sim 1.3''$, lobe size $\sim$ 200 mas; Fanti et al 1985) and you have a good candidate for TSAS. If this is correct, then the important thing about this *particular* source would be that TSAS would *dominate* the 21-cm line absorption, in contrast to the usual case where it is more like 10% of the total. Physically, this would mean that here the TSAS is not embedded within the CNM, as it is elsewhere, but rather sits by itself, probably surrounded by the WNM.

Where do we go from here? More measurements! HI measurements are important, of course. A large quantity of VLBI and time-variable pulsar measurements will provide the statistical information we need so badly—and somebody should look at 3C237. But that's not enough! UV absorption line measurements of selected heavy-element transitions provide the physical conditions directly. We desperately need to know whether the temperatures are as cold as we suspect, and what the volume densities really are. There may be other things going on, too: Cox (1997) thinks that magnetic fields might be able to confine small blobs, and maybe the TSAS is the observational realization of these.

3. It's Not Just HI!

We radio astronomers have a unique handle on the most massive component of the diffuse interstellar gas. But this should not make us complacent. Other components are important too. The Hot Ionized Medium ($T \sim 10^6$ K) is produced by supernovae and pushes the HI around, creating the beautiful shell structures that are so prevalent. The Warm Ionized Medium (the "Reynolds' gas"—e.g. Haffner, Reynolds, and Tufte 1998) constitutes roughly 25% of the mass of the diffuse ISM and, because it probably makes a transition from neutral to ionized gas at some point in its career, can create temporary regions of huge overpressure. Magnetic fields and cosmic rays exert forces on the gas that are at least comparable to thermal pressure; magnetic fields, at least, can be measured with the Zeeman effect and polarization caused by aligned dust, and although these measurements are difficult, they are of crucial iomportance for gaining a global comprehensive understanding of the ISM morphology and structure. And in any cloud containing a significant column density of HI, there should also be H_2; this component is often neglected, but both theory and detailed observational studies of individual clouds of modest column denstiy show that it is plentiful, even in clouds too thin to form CO molecules (Reach, Koo, and Heiles 1994).

In short, we need to move beyond considering HI and its properties in isolation. Rather, all components of the ISM interact. The Eridanus superbubble is a specific case in point. It is in a middle evolutionary stage, having originated long ago but still being energized by massive stellar winds and supernovae; this means it exhibits the full range of astrophysical processes that occur whenever all of the interstellar gas phases lie in close proximity. It is nearby, so only modest angular resolution is required and, more importantly, it is the only object along the line of sight so its maps can be interpreted unambiguously. But it is a sample of only *one*.

We cannot hope to understand such regions without a decent statistical sample. To get that, we can do nothing other than look further within the Milky Way in as many ISM phases as possible. HI is of primary importance: physically it dominates the mass, and observationally not only its amount but also its velocity can be observed. For the Milky Way, mapping HI emission with angular resolution of the order of 1 arcmin is the minimum required, and we need to map large quantities of sky, particularly in the Galactic plane. The Canadians are in fact doing this with their Galactic Plane Survey project—but only in the outer Milky Way because they can't perform a proper synthesis at low declinations. And their sensitivity is barely adequate. What we really need is something like the square km array, which would provide the combination of angular resolution and surface brightness sensitivity to do the job properly. And we need *lots of time*. This particular juncture, on the eve of the planning for the new decade astronomy reports, is the time to push for this instrument!

It is a pleasure to acknowledge helpful comments by Don Cox, Dave Green, Alex Lazarian, and Steve Spangler. This work was supported in part by an NSF grant to the author.

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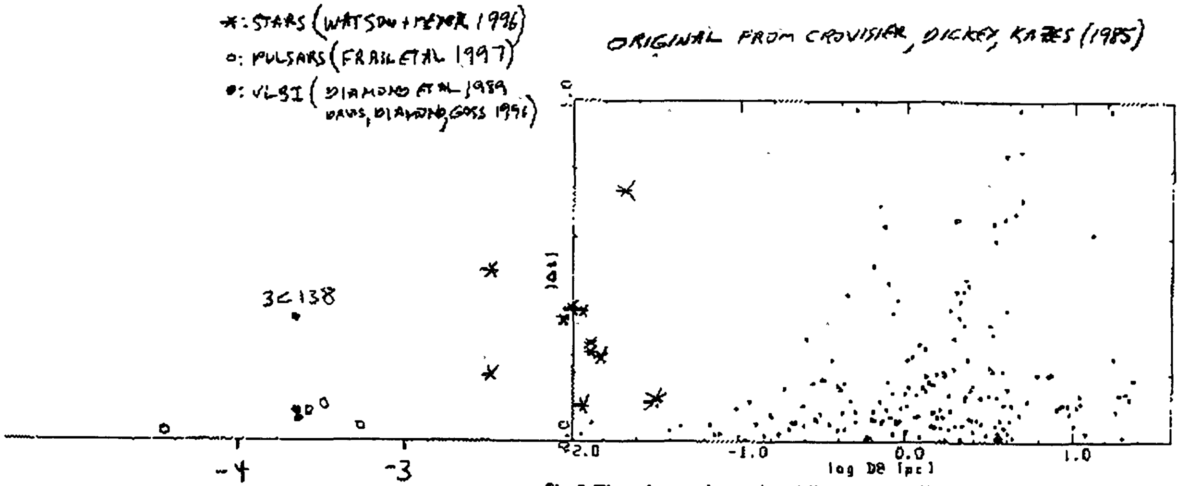
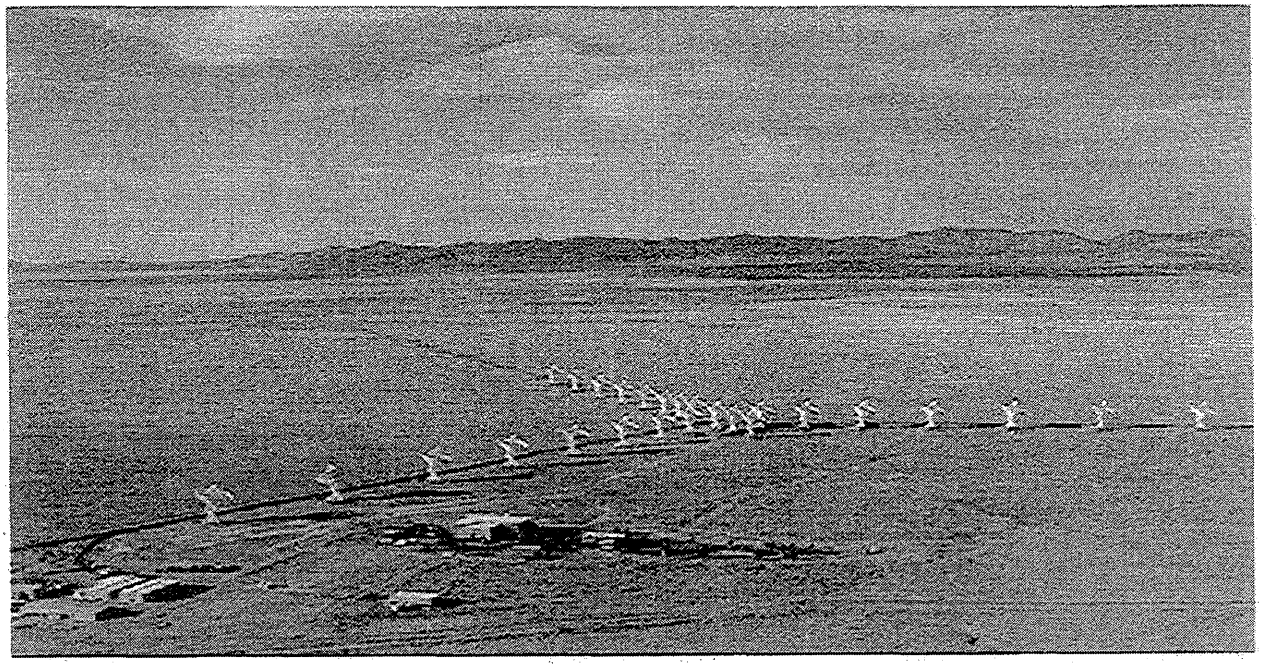


Fig. 9. The relative absorption difference, $|\Delta t(v)| = |a_A(v) - a_B(v)|/(a_A(v) + a_B(v))$, for all velocity channels and all sources, plotted as a function of linear separation Dθ (only channels for which a_A or a_B is greater than 0.025 are considered)

FIGURE CAPTION

Reproduction of the "structure function" plot for double-lobed sources from Crovisier et al (1985), with hand-drawn additions. Their original figure exhibits points only above 0.01 pc; their points are the neat, printed, ones. By hand, I've extended their horizontal axis down to < 10^{-4} pc and inserted points from the VLBI, pulsar, and optical data mentioned in the text.

Section II: Development of the VLA



The VLA in D Configuration, looking north-northeast, seen from the air in 1999.
(Photo by D.G. Finley, courtesy NRAO/AUI.)

The Caltech Interferometer and the State of Interferometry

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ABSTRACT

The Caltech Interferometer was the earliest sensitive microwave interferometer, and as such foreshadowed much of the science and many of the technical features of modern interferometers. However, the great technical developments of the last three and a half decades have evolved from things perceived as needs, as soon as the Caltech Interferometer started operating, rather than from seeds in that instrument. At its birth, the Caltech Interferometer had just enough hardware, working just well enough, to get to the astronomical results in the still nascent field of radio astronomy.

Subject headings: interferometry, imaging, electronics

1. Introduction — The history of radio astronomy instrumentation

If we look at the development of radio astronomy instruments, from the first detection of celestial radio waves to the latest instruments now being planned, we can discern three great themes. They are, first, more of everything — more antenna elements, more receivers, more bandwidth. Second, digital everything. And third, a growing reliance on ever more sophisticated data reduction algorithms. This paper is a look back at the earliest days of these themes, with particular reference to the early days at Caltech's Owens Valley Radio Observatory. As usual, when one examines the history of technology, one finds an interesting mixture of amazing sophistication and incredible naivité.

2. Origins of Owens Valley Radio Observatory

Although the origins of radio astronomy date back the the 1930s, the great expansion of the science came after World War II. This is due not only to the natural extension of the science, but to the great technical strides in radioscience made during the war in furtherance of radar technology, and to the realization by the defense agencies that a strong technical infrastructure, even if devoted to matters of pure science, is a valuable asset to national security. This last led to a favorable funding climate for basic research.

By 1951, some of the mystery of the celestial radio waves began to be dissipated by the identification of localized sources of radio emission with well-known astronomical objects — the

Crab Nebula [Bolton & Stanley, 1949], the bright, nearby galaxies M87 and NGC 5128 [Stanley & Slee, 1950], and the surprisingly faint Cygnus A [Baade & Minkowski, 1954]. At this point, it became clear that radio astronomy was indeed related to astronomy as a whole, and not just the plaything of radar technologists. Exactly what it could tell the rest of astronomy started to become clear a few years later, with the detection of the 21cm emission of neutral hydrogen by Ewen and Purcell, and the developing suggestion by Alfvèn et al., 1950, and many others that the continuum radiation was primarily electron synchrotron radiation.

An exceptionally farsighted group of men at Caltech, Walter Baade, Rudolph Minkowski, and Jesse Greenstein, felt that a radio observatory would be a valuable, even necessary, complement to Caltech's optical observatory at Palomar. The account of how they managed to establish the Owens Valley observatory, and of the early days of the observatory, has been given by Marshal Cohen in the Caltech alumni magazine [Cohen, 1994].

Since there were, at the time, no American radio astronomers of stature, they looked abroad for someone to head the venture, and settled on John Bolton, an Englishman living in Australia, who had been involved with the first radio source identifications. Since the emphasis was on radio astronomy working together with Palomar, it was felt that the most important problem the nascent observatory could tackle was to provide more radio source identifications. The clue to identifications was good radio positions, and this dictated that the principal instrument of the new observatory would be an interferometer. Photographic plates of the era showed about one object per square arcminute, so positions of a few arcseconds accuracy were necessary to identify faint objects. And a peculiarity of the photographic process was that faint emission line objects could be seen in spectra for objects that did not appear on photographs. The faintest objects thus would need a positional accuracy better than the slit width projected on the sky, perhaps two arcseconds. This accuracy was considered extremely challenging.

The transition of the Owens Valley Radio Observatory from a basic idea to a working astronomical instrument proceeded with what seems in retrospect, amazing speed. John Bolton arrived at Caltech in January 1955, and, in the tradition of the day, proceeded to work on various small instrumental projects using student labor and not much money. The largest of these was a thirty-two foot, mesh surfaced, equatorially mounted antenna for hydrogen line survey work. This was erected near Palomar Observatory in 1956.

By the summer of 1957, the site infrastructure — The control building and shop building — was in place at Owens Valley. The two 90-foot dishes of the interferometer were erected in the summer of 1958, and the first radio observations were made that winter. They were first connected as an interferometer in late 1959.

3. Early technology at Owens Valley Radio Observatory

The early receivers at Owens Valley were based on the radar technology originated during the war, with some improvements engendered by the specialized application, and some resulting from tinkering and experimentation by Gordon Stanley and others. This technology was coaxial germanium diode mixers, followed by low noise vacuum tube amplifiers. The receivers were not only not cooled, they were not temperature controlled. The only environment control was a minimum protection from being rained on. Even at that time, it was known that this type of receiver was a technological dead end, and would be replaced by other technologies, but the receivers could be designed and built with a minimum of time and labor, an important consideration given the rapid advances then being made in radio astronomy.

In fact, much about Owens Valley was designed for flexibility and to cover as much parameter space as possible, an entirely sensible approach when you know almost nothing about the objects you are going to be studying. The initial spacings of the interferometer were exponential, 200 feet, 400 feet, 800 feet, and 1600 feet, which does not make a very nice looking u, v plane as we later conceived the matter. But with five stations, it gives you a general idea of the overall size of a radio source over a large range of possibilities.

In line with the idea of maximum flexibility, there was no multiplicity at Owens Valley. There was one baseline, one receiver per antenna (others sitting on a shelf, to change frequency), one polarization, and, for spectral line work, one frequency channel. This minimizing of equipment was due not only to a desire to maintain flexibility, but because of the vacuum tube technology of the time. The fewer tubes you had, the quicker the ritual of plugging all the tubes through the tube tester at every equipment change, or whenever something became flaky. The idea of hundreds or thousands of channels was inconceivable, though the thought of having more than one had its attractions. I am amazed that we attempted a neutral hydrogen Zeeman effect measurement with a single channel, and even attained interesting sensitivity [Morris et al., 1963]

The mixer frontends passed both sidebands. This was regarded as an advantage, for continuum work. Any phase change in the IF system changes the phase of the two sidebands in opposite directions, so there is no net change in the phase of the interferometer fringes, which was nice for position measurements. One could worry only about the phase of the local oscillator system, without having to carefully consider what the delay line is doing to the phase. For spectroscopic measurements, the first idea was simply to put a cavity filter in front of the mixer. This added to system noise, and also had imperfect rejection. Later, I realized that the local oscillators at the two antennas could be set to different frequencies (2 MHz different was convenient in the LO system as it was at the time), and the unwanted sideband could be removed with an easy filter at the IF frequency. Then one IF could be mixed with the LO difference frequency, or the two IFs could be multiplied, and the resulting 2 MHz fringes phase detected against the LO difference frequency [Clark, 1965]. (A similar scheme is contemplated for ALMA.)

Having only minimal numbers of devices was not an idea restricted to Owens Valley. It was a fairly general condition in the community. Even spectroscopists thought in terms of a single channel, or possibly a few channels. (When you are used to one channel, six seems like a lot.) There is only one example for the time which gave us a mental image of what a radio astronomy array should look like — Christianson's solar array of 32 paraboloids [Christianson & Warburton, 1953]. But soon after, designs for multielement arrays began to surface — the Five Kilometer Telescope, the WSRT, and, eventually the VLA. Even Owens Valley had multielement, centimeter wavelength, array aspirations; the current 130 foot antenna was originally intended to be the first of six, and some small investigation of the restrictions of putting a 50 km array into the constrained geography of the Owens Valley was done.

Certainly part of having large numbers of devices is the trend to "Digital everything." Only with the revolution in digital electronics can devices be made sufficiently inexpensive and sufficiently reliable to conceive of making correlators with millions of correlating elements. I believe that the 100 channel autocorrelator that Sandy Weinreb built for his dissertation research [Weinreb, 1961] was the first use of a digital IF in radio astronomy. Then having him at NRAO kept us to that track in the early days, when it was much less obviously the right approach than it is now. One thing that even now I look back on and bless our fortune is that, had the NSF been able to fund the VLA at the time the design was first done, we would have had to proceed with the design that we had in hand. This consisted of delay lines built of mechanically variable bulk acoustic delays feeding analog correlators. There is no way the VLA would have been as successful as it has been had it been built with that hardware.

4. Early data processing at Owen Valley Radio Observatory

Most of the early results from Owens Valley depended on measuring things with rulers on chart recorder paper. This is a reasonably efficient way to measure amplitudes, and an incredibly tedious way to measure phases.

From fairly early days there was an analog to digital converter coupled to a paper tape punch, which punched the correlator output on paper tape every ten seconds. To the best of my knowledge, there was never a successful program to read this paper tape. It took a couple of years to realize that software didn't just happen, that it took an organized effort to construct. In the mid 60s, the paper tape punch was replaced with magnetic recording, and real software.

There was a data processing computer at Owens Valley from early times, though. It was a mechanical analog computer, based on a ball-and-disk continuously variable transmission. This device acts like a multiplier, multiplying a rotation supplied to the disk by the linear position of the ball, and the output represented as a rotation of the output cylinder. A synchronous motor, with appropriate gearing for configuration and observing band, was the input to the device. This was multiplied by cosine declination, which was entered by a micrometer. A synchro slave was connected to one antenna's hour angle readout, and was converted to cosine hour angle, by a mechanical converter (essentially a crank). Multiplying by this gives the fringe phase. A belt drive with an appropriate pulley was used to convert from fringes to nanoseconds, and used to drive a lumped constant variable delay line.

That setup, to track delay through an observation, was used for most observations. There was in addition a data processing part of the computer. The fringe phase, calculated as above, was fed into a resolver, which was driven by the correlator output, chopped at a 400 Hz rate to make a signal suitable for the resolver. This effectively multiplies the correlator output by the sine and cosine of the fringe phase. This, after phase detection of the chopping signal, was converted back into mechanical motion, and, again with ball-and-disk transmissions, was converted into a mechanical rate. This was integrated up in mechanical counters, odometer type — digital at last. I believe that one of the first uses of this device was the Zeeman effect measurement mentioned above [Morris et al., 1963].

Imaging via Fourier inversion was done by Al. Moffett and others, using a simple FORTRAN program. This was originally run on an IBM 7090 computer at JPL that Caltech rented time on. Data were read from the charts and entered on punched cards. However, because of the sparse spacings, these Fourier inversions never produced very satisfactory images. For something publishable, model fits were usually done. I'm not sure how sophisticated the model fitting was. The little of it that I did was typically two small gaussians with the same size, and fitting was done by cut-and-try with a slide rule. The real imagers probably had much better methods.

Although there were various quasi-implementations at earlier times, the FFT algorithm was published in 1965 [Cooley & Tukey, 1965], and it rather revolutionized thinking about the VLA design. For the first time, it became possible to contemplate making images on a computer we might house at the VLA site, instead of bringing the data to a large computer center elsewhere.

Jennison had, by this time published his algorithm [Jennison, 1958] which is really the root of phase closure and self-calibration methods. I certainly did not appreciate its power at the time. For purposes of explaining how the method was to work, he presented it as a sequence of measurements with a three element interferometer, and I did not have the wit to see the more general application. With successive measurements, phase error would rise as square root of baseline length. With round-trip phase measurement systems (then being pioneered by Dick Thompson, among others), the electronic and LO system error could be made nearly independent of baseline. I knew little about atmospheric phase errors, and incorrectly discounted their importance, and I was mostly interested in sufficiently high frequencies that the ionosphere did not seem important. So a method with errors increasing as the square root of the baseline did not seem as impressive as it might have. Later, when we actually had a three-element interferometer in Green Bank, I realized that point sources should show phase closure, and looked at it carefully. Because the correlator for that interferometer was analog instead of digital, there was an apparently stable offset of a degree or two. My thought for what to use it for was to look for low level emission in the neighborhood of strong point sources, and tracked a few, looking for variation of the closure phase with time. Having measured this, I discovered that I then had no idea of how to convert these variations into an image. The clue about how to do so was provided by others, in VLBI, who began to use phase closure in a much simpler situation — to decide which of two mirror image model fits was the correct one.

So, returning to the introductory remarks, we see that the three great themes of radio astronomy instrumentation — increasing multiplicity of devices, increasing use of digital electronics and computation, and increasing sophistication of algorithms, started from very minimal levels indeed in the Caltech interferometer, but grew to current levels from seeds planted at about that time, and driven by the science that people wanted to extract from that interferometer and its successors.

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The Green Bank Interferometer

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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 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 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

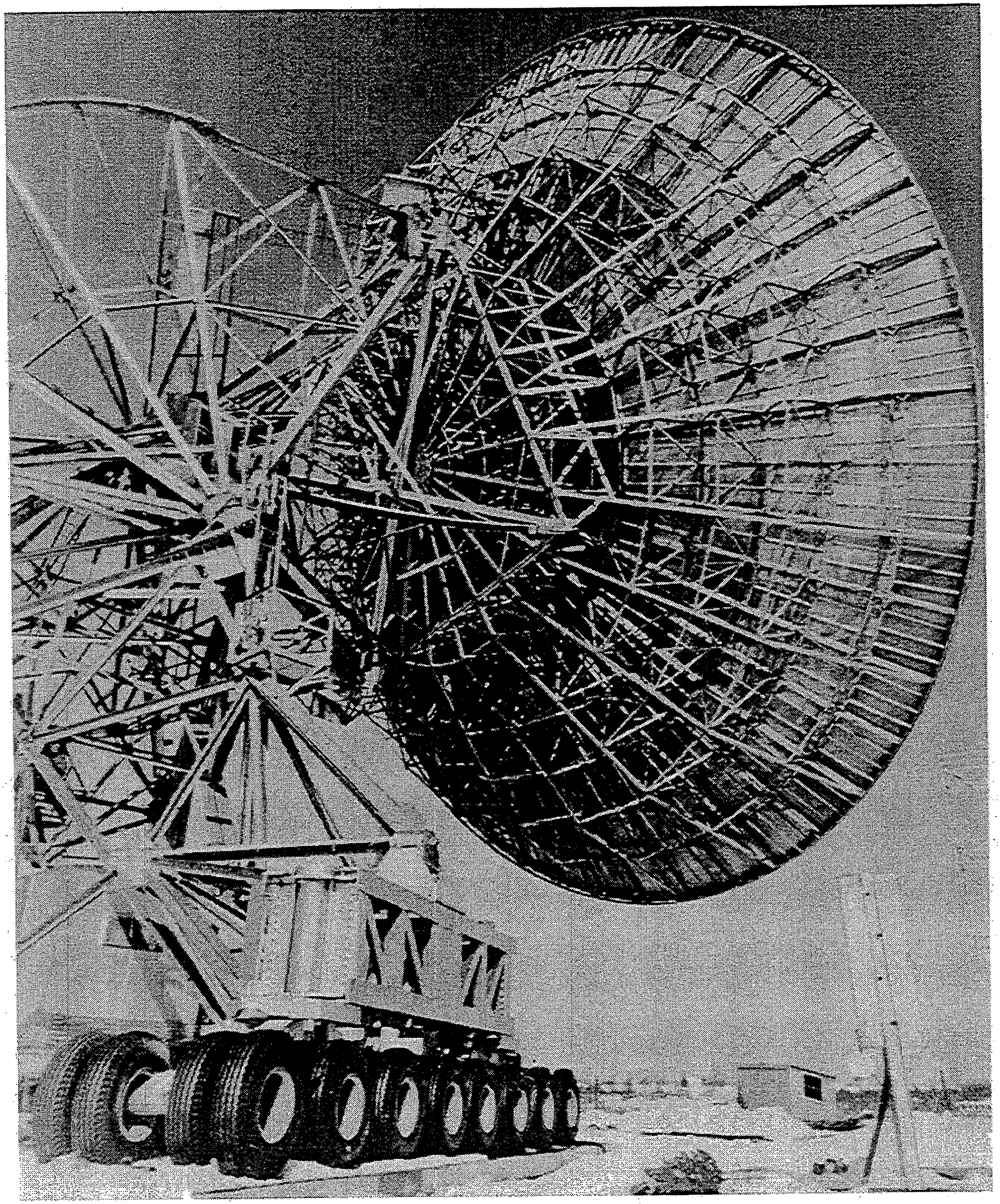

Figure 1. The second element, 85-2. This telescope was mounted on rubber tires, and was towed along a roadbed in order to move from one antenna pad to another.

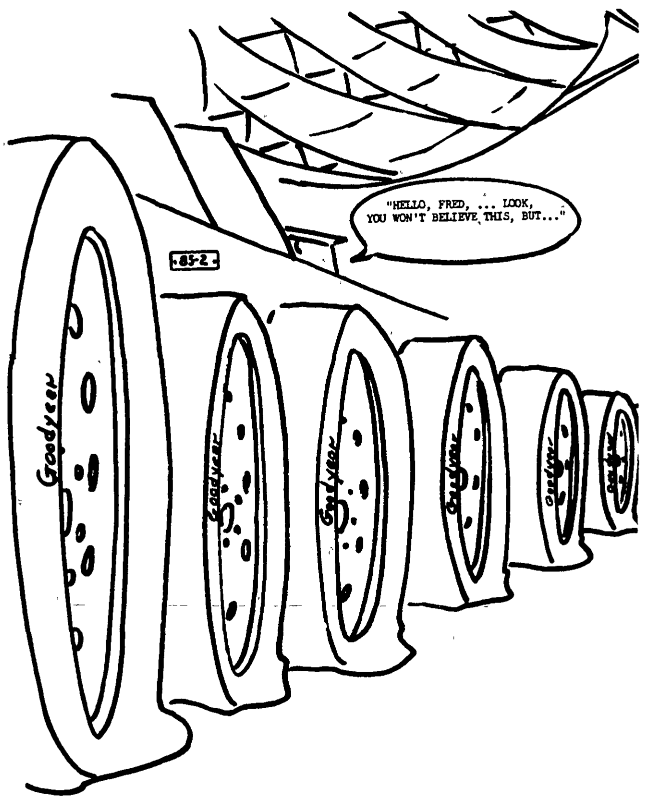

Figure 2. A cartoon from the *Observer* (Vol. 10, No. 5, September 1970) memorializing the summer when most of the telescope tires went flat.

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 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 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

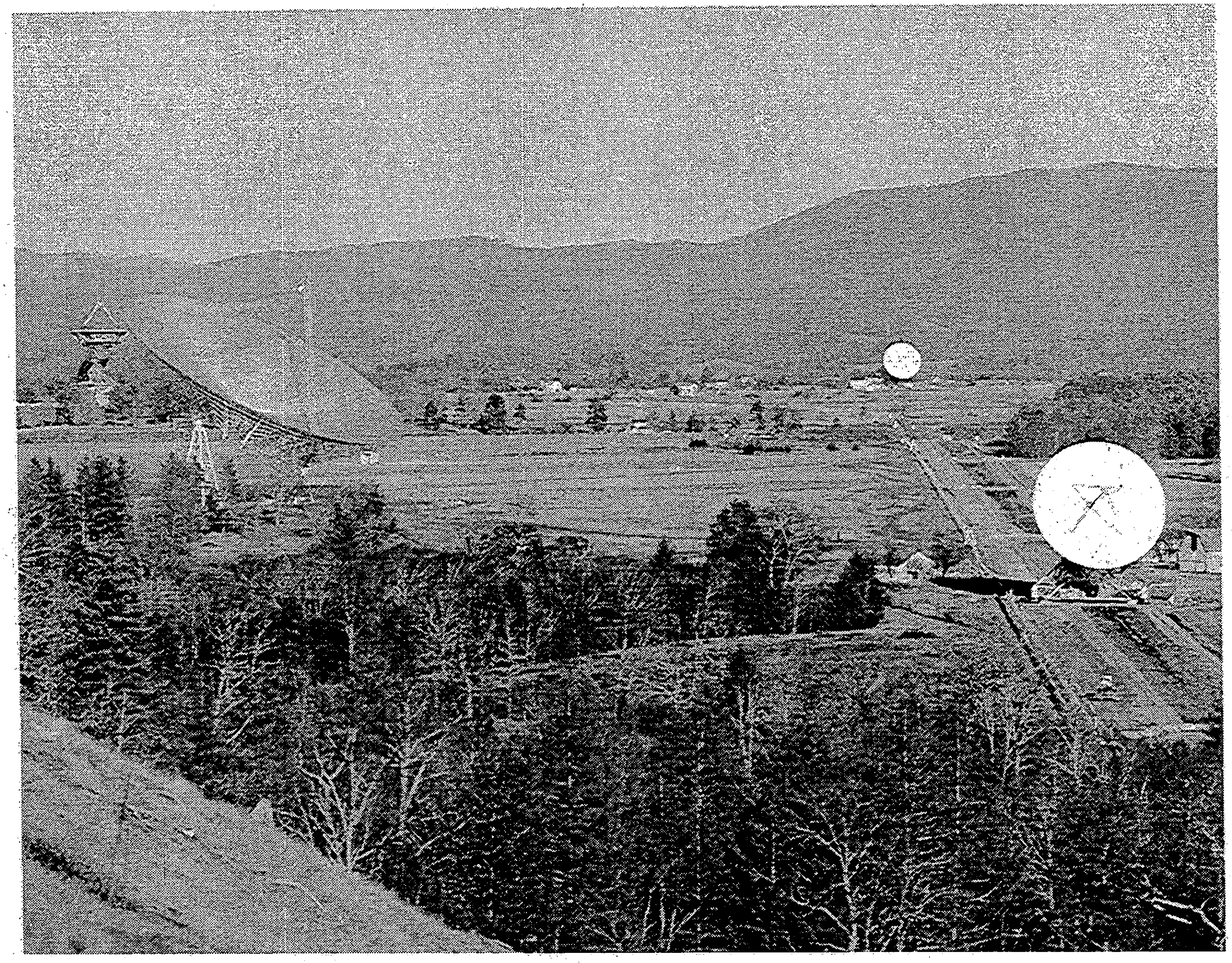

Figure 3. The two-element interferometer in January 1965. The antenna sits on station 5 (2400 meters), having been moved there for the first time shortly before the photograph was taken. The interferometer backend was in a trailer midway between station 1 (1200 meters) and 85-1; it is, unfortunately, not visible in this photo.

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.

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).

Of course, Barry was heavily involved in the development of the interferometer system, at the same time as he was plugging along on the 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

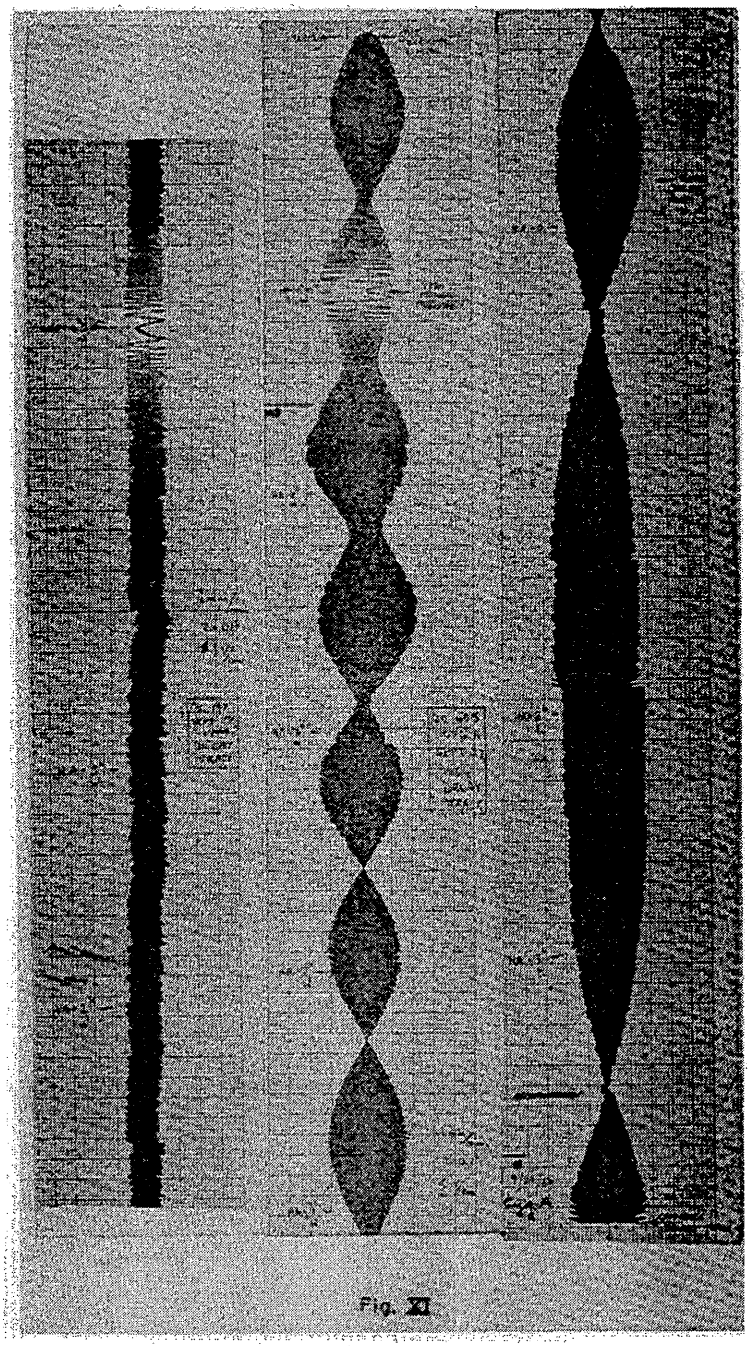

Figure 4. Delay Tracks on Cygnus A and 3C147. Since 3C147 is a point source at this resolution, the amplitude of the fringes is expected to be constant. The fringe amplitude for Cygnus A varies, showing the dominant double structure, but also suggesting structure within the two components of the source. In each case the fringe rate goes to zero near 5 hours West hour angle.

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 6).

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 7). 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. Bob Hjellming will talk about some of the results in a later paper. 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.

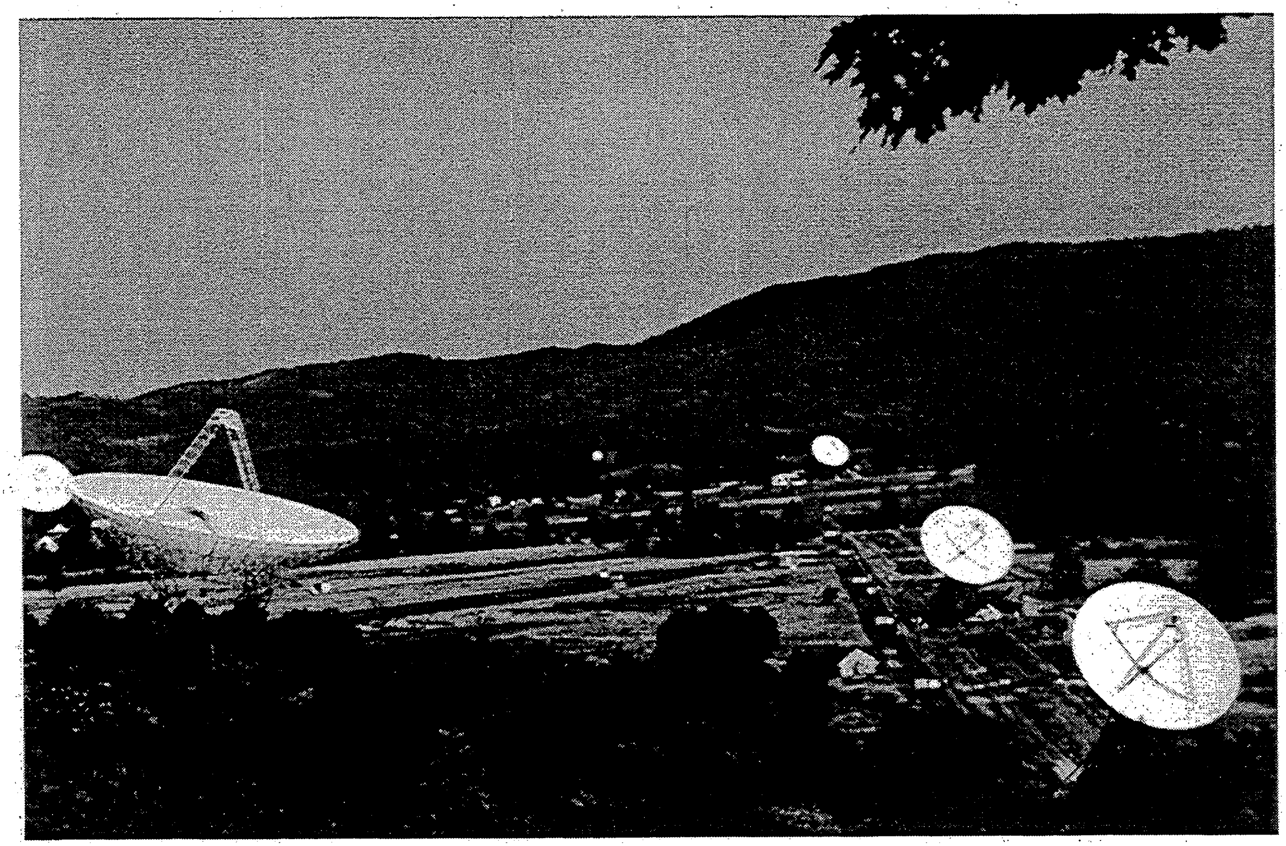

Figure 5. The Three-Element Interferometer. This scene from 1971 shows the three elements, the Control Building, and the relay tower used to transmit and receive signals to and from the remote antenna.

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 8), 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.

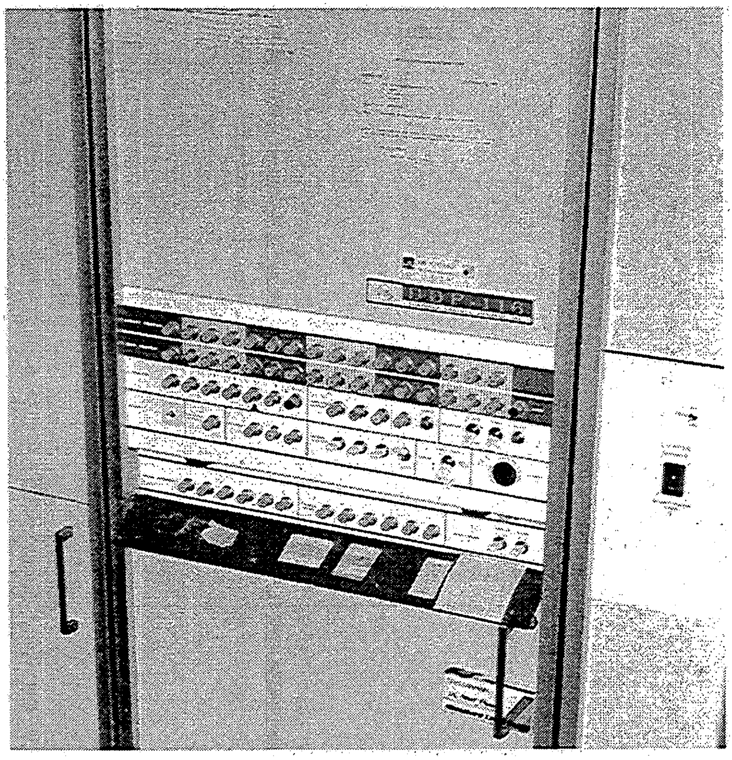

Figure 6. The DDP-116 Interferometer Control Computer. This photograph was taken in June, 1998, approximately thirty years after the computer began operating the interferometer. The bootstrap loader is keyed in by hand following the recipe written on the inside of the front panel, here shown folded down to reveal the recipe. A supply of panel lights is kept nearby, in the handle of one of the panels.

Figure 7. 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.

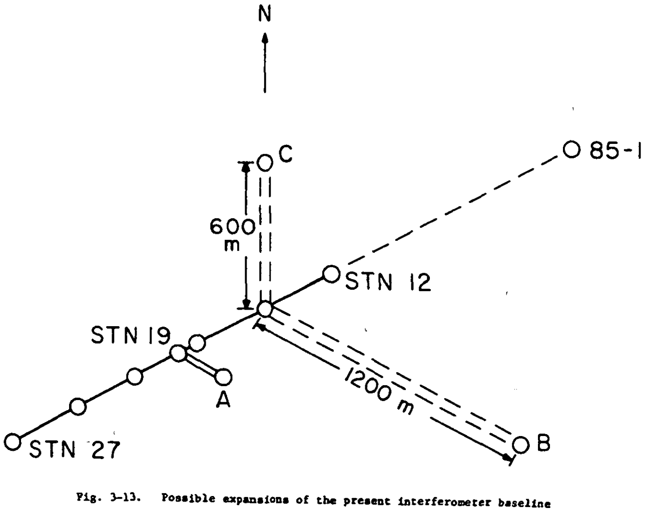

Fig. 3-13. Possible expansions of the present interferometer baseline

Figure 8. 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.

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.

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Science with the Green Bank Interferometer

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The Conception of the Green Bank Interferometer

The Green Bank interferometer (GBI) began as an instrument for NRAO staff and users to make radio interferometer measurements as part of a long-range plan for NRAO to develop a major aperture synthesis instrument. It was a "test-bed" instrument for what later became the Very Large Array (VLA). It was conceived and built by NRAO staff with major contributions by Dave Heeschen, Hein Hvatum, Campbell Wade, Warren Tyler, Dave Hogg, and Barry Clark. It is this group of astronomers that worked with NRAO engineers and technicians to build the GBI, and it is this group I refer to as "the builders." Barry's principal role was a combination of programming the on-line computer, a DDP 116, and making the entire system work. Initially the GBI consisted of two 25-m telescopes that were occasionally moved to different stations to make measurements at different baseline lengths. Later a third 25-m antenna was added to the basic interferometer. The two and then three element interferometer began making scientifically useful, and publishable, measurements in Oct. 1964.

A fourth element to the GBI, a 40-foot antenna operated with a radio link at a location 35 km from the three-element interferometer, was added to verify that it was possible to maintain phase coherence over the long baselines that were planned for the VLA.

Other chapters in this book by Dave Hogg and Ed Fomalont cover different aspects of the Green Bank Interferometer.

Early Science With the GBI

Table 1 lists some of the first papers published in 1965-1967 based upon data from the early GBI. These papers involved the builders, other members of the NRAO scientific staff, and NRAO users. The titles of the papers indicate the science being done, mainly in the areas of astrometry and the structure of radio sources.

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[1] A facility of the National Science Foundation, operated by Associated Universities, Inc. under cooperative agreement with the National Science Foundation.

Table 1 - Earliest Papers, 1965-1967

Author(s)	**Publication**	**Paper Title**
Wade, C.M., Clark, B.G., & Hogg, D.E.	1965 *Ap. J. Letters,* 142, 407	Accurate Radio-Source Position Measurements with the NRAO Interferometer
Wade, C.M.	1966 *Phys. Rev. Letters,* 17, 1061	Fine Structure of the Radio Source Cyg A
Clark, B.G. & Hogg, D.E	1966 *Ap.J.,* 145, 21	Radio Source Fringe Visibilities with an Interferometer Of 21500-Wavelength Baseline
Kaftan-Kassim, M A. & Kellermann, K I.	1966 *Nature,* 212, 1336	Angular Sizes of Quasi-stellar Radio Sources
Clark, B.G.	1967 *Astron. J.,* 72, 601	Position Determinations for Radio Astronomy

There were three main areas of science that were pioneered by the GBI with papers published between 1968 and 1971: studies of the structure of radio sources, principally extra-galactic sources; radio astrometry; and studies of the radio emission from stars. Tables 2, 3, and 4 list most of the major papers published in these three areas.

Table 2 - The Structure of Radio Sources - 1968 and later

Author(s)	**Publication**	**Paper Title**
Basart, J.P., Clark, B.G., & Kramer, J.S.	1968, PASP, 80, 273	A Phase Stable Interferometer of 100,000 Wavelengths Baseline
Bash, F.N.	1968 *Ap. J.,* 152, 375	Observations of the Angular Structure of Radio Sources
Bash, F.N.	1968 *Ap.J. Supp.,* 149, 373	The Brightness Distribution of Radio Sources at 2695 MHz
Clark, B.G. & Miley, G.K.	1969 *Ap. Letters,* 4, 207	The Structure of 3C9
Hogg, D.E., MacDonald, G.H., Conway, R.G., and Wade, C.M.	1969, Astron. J., 74, 1206	Synthesis of Brightness Distribution in Radio Sources
Fomalont, E.B.	1970 *Ap. J.,* 160, L73	Polarized-Brightness Distribution in 3C20
Webster, W.J. & Altenhoff, W.J	1970 *Ap. Letters,* 5, 233	Aperture Synthesis Observations of Orion At 2.695 GHz
Miley, G.K., Hogg, D.E., and Basart, J.	1970 *Ap.J.,* 159, L19	The Fine Structure of Virgo A

Table 3 - Astrometry - 1970 and later

Author(s)	**Publication**	**Paper Title**
Wade, C.M.	1970 *Ap. J.*, 162, 381	Precise Positions of Radio Sources: I -Radio Measurements
Kristian, J. & Sandage, A. et al.	1970 *Ap. J.*, 162, 391	Precise Positions of Radio Sources: II - Optical Measurements
Sandage, A. Christian, J., and Wade, C.M.	1970 *Ap. J.*, 162, 399	Precise Positions of Radio Sources: III - Comparision of Optical and Radio Measurements
Brosche, P., Wade, C.M., and Hjellming, R.M.	1973 *Ap. J.*, 183, 805	Improved Solutions and Error Analysis for 59 Sources
Wade, C.M. & Johnston, K.J.	1977 *Astron. J.*, 82, 791	Positions of 36 sources measured on a baseline of 35 km
Sramek, R.A.	1971 *Ap. J.*, 167, L55	A Measurement of the Gravitational Deflection of Microwave Radiation Near the Sun, 1970 October

The papers dealing with astrometry listed in Table 3 began with fundamental work by Campbell Wade and collaborators that carried determination of positions of radio sources to a level, and accuracy, better than traditional optical methods. The extra accuracy of position and time determinations made using 35 km baselines was the first step in the direction of establishing radio astrometry at even longer VLBI baselines as the most precise method of measuring time (earth rotation, wobble, etc.) and positions of sources.

Radio Stars

It was not expected that the study of radio emission from stars and stellar systems would become a major area of scientific study for the GBI and later radio interferometers. Because of this, and because it was a major turning point in my own career that changed me from a theoretician mainly working on the physics of the interstellar medium to a radio observer mainly working on stars, I am taking this opportunity to tell how Campbell Wade and I began work on radio stars. In early December 1969, Dave Heeschen, Campbell Wade, and others were sitting in Heeschen's office at the NRAO headquarters in Charlottesville discussing the preparation for arguing the case for building the VLA before the Greenstein committee. Someone commented that since all the astronomers on this committee worked on stars, perhaps they might be more enthusiastic about the VLA if the VLA could observe stars. At that time there had been many papers on radio flare stars published by Lovell and colleagues, and a couple of other papers on other stellar radio observations; however, it was not considered to be a major research area for radio astronomy. The result of the conversation in Heeschen's office was that Campbell Wade went down to my office and posed a question. If an instrument with the specifications of the VLA were built, would it be able to see stars, and if so, how many, what kind, and would it be scientifically interesting? As perhaps the first person hired at the NRAO as a theoretician, and having had a "classical," optically-oriented, astronomical education at Yerkes Observatory of the University of Chicago, they hoped I might have some basis for answering these questions. It took me a couple of hours to come to two conclusions. If one took a conservative position on what was known about thermal and non-thermal processes on stellar surfaces, the VLA should be able to detect the emission from winds of red supergiants, but not much more than that. However, if one took high brightness temperatures as known on the surface of the Sun, and allowed them to occupy volumes the sizes of stars, an even more optimistic scenario could be envisioned whereby 10^5 to 10^6 stars might be detectable with the VLA. When I informed Campbell Wade about my conclusions, he immediately took me to Dave Heeschen's office to repeat the story. Dave said that we should do something to see whether the optimistic scenario was possible, and picked up the phone to arrange for Campbell and me to go over to Green Bank the next day and try to detect some stars with the Green Bank Interferometer. Thinking that anything interesting had to be at the Jansky flux level, a large number of short observations of many types of stars were made -- all with negative results. The idea of VLA observations of stars had little role with the Greenstein committee, which did make a recommendation that the VLA should be built. However, the idea led to Campbell Wade and me deciding to ask for GBI time to make more, longer observations. We received two weeks of GBI observing time on the then 3-element GBI in March 1970, and successfully detected the radio emission from Antares, a red supergiant, and Sco X-1, the strongest "steady" X-ray source in the sky. The detection of Antares was new; the detection of Sco X-1 was a confirmation that Ables (1969) observations of highly variable radio emission, doubted by some radio astronomers, was correct. In any case, these positive results led to an even longer observing run in June 1970 where the March results were confirmed and, in addition, the radio emission from the classical novae Delphini 1967 and Serpentis 1970 was detected.

Table 4 - Radio Stars

Author(s)	**Publication**	**Paper Title**
Hjellming, R.M. & Wade, C.M.	1970 *Ap. J.*, 162, L1	Radio Novae
Wade, C.M. & Hjellming, R.M.	1970 *Ap. J.*, 163, L65	Further Radio Observations of Novae
Wade, C.M. & Hjellming, R.M.	1971 *Ap. J.*, 163, L105	Detection of Radio Emission from Antares
Hjellming, R.M. & Wade, C.M.	1971 *Ap. J.*, 164, L1	The Radio Sources Associated with Scorpius X-1
Hjellming, R.M. & Wade, C.M	1971, *Ap. J.*, 168, L21	Radio Emission from X-ray Sources: Cyg X-1 & GX17+2
Hjellming, R.M. & Wade, C.M.	1971, *Ap. J.*, 168, L115	Radio Emission from Antares B
Hjellming, R.M. & Wade, C.M.	1971, *Science*, 173, 1087	Radio Stars

Table 4 lists some of the early publications on radio observations of stars by the GBI by Campbell Wade and me. In addition to the things already mentioned, it included the identification of the radio counterparts of the X-ray source Cyg X-1, which led to its identification with the star HDE 226868, and the X-ray source GX17+2. The first sign that high resolution imaging of radio stars might become important was the detection of radio emission from both the A and B components of the Antares binary star system. Later VLA observations by Newell and Hjellming (1979) established that the Antares A emission was from the partially ionized wind of the red supergiant, while the radio emission at the position of Antares B was a resolved nebulosity caused by the companion B-star ionizing portions of the supergiant wind.

The expectation that the VLA would make a significant contribution to observations of the radio emission from stars was confirmed with time. The first scheduled scientific program with a few VLA antennas in November 1976 involved simultaneous, six-frequency observations of time variable events in stars; results that were presented in the January 1977 meeting of the AAS in Hawaii (Hjellming et al. 1976). By the early 1980s nearly 1/3 of VLA observing time was for observations of stars of one kind or another.

The GBI Until 1980

The GBI continued to be a productive radio interferometer until it was shut down upon the completion of the VLA in 1980. Because the VLA was then a much more powerful 27-element interferometer, the GBI was no longer considered a "frontier" research instrument and NRAO ceased operating it as an NSF-funded instrument for users. However, because it was still capable of carrying out systematic astrometric observations of stellar and extra-galactic radio sources, NRAO continued to operate the GBI for the USNO-NRL astrometric program between 1980 and March 1996.

USNO-NRL Program for Determination of Positions & Time

There were three principal goals for the 1980-1996 astrometric observations by the GBI. The first was the determination of time. The precise measurement of radio positions indirectly measures changes in the rotation of the Earth which contribute significantly to the measurement of sidereal time. For about 17 years, the GBI measurements played a major role in the determination of time as carried out by the USNO using a weighted mixture of radio and optical determinations of positions of sources. The second major goal was establishment of an extra-galactic reference frame based upon very distant radio sources. The third goal was establishing a common reference frame for sources with coincident radio and optical emission. The latter involved both distant quasars and stars that were radio sources and were also in fundamental catalogs of stellar positions. The need for these stellar observations was the reason why the GBI began long term monitoring of radio stars like SS433, Cyg X-3, and LSI +61 303.

One of the major serendipitous results from the astrometric observations of distant quasars was observations of flux variations due to scintillation of small background radio sources due to the interstellar medium. This included the discovery of the so-called Extreme Scattering Events (caustics) by Fiedler et al. (1994).

The USNO-NRL era ended in March 1996 when USNO-NRL stopped funding the operation of the GBI. This was because radio VLBI had taken over radio astrometry and timekeeping because of its superior precision for the determination of positions of sources.

However, the realization that the GBI determination of radio variations of stellar, particularly X-ray, sources was scientifically valuable led to the re-birth of the GBI in Nov. 1996 as a radio monitoring instrument.

Monitoring X-ray Sources - November 1996 to June 1998

Discussions among astronomers working with X-ray satellite data , provided by NASA instruments, led to the so-called NASA-GBI program that began in November 1996, and continued until now. NASA programs, mainly related to the Rossi X-ray Timing Explorer (*RXTE*) and the Compton Gamma Ray Observatory (*CGRO*), funded the operation of the GBI monitoring of old (and new) radio-emitting X-ray sources (and a few interlopers). The basic program covers ~ 25 sources per day with daily to hourly monitoring, depending on what is useful for the sources in question. The data are calibrated and placed in the public domain by providing tables of the data, updated on a daily basis, principally by Elizabeth Waltman (NRL) and Frank Ghigo (NRAO). An executive committee consisting of Marco Tavani (Columbia), myself, Waltman, and Ghigo decide on priorities for the scheduling of old and new sources. General policies such as the open invitation for proposals for new sources to be monitored, and placing all data in the public domain, were formulated in 1996 by an advisory committee of about a dozen X-ray and radio astronomers.

A Few Examples of Recent Results with the GBI

Let us examine some of the results that have been obtained with the GBI since the beginning of the NASA-GBI program.

McCullough et al. (1999) have made detailed comparisons of *RXTE* ASM and *CGRO* BATSE X-ray light curves with 2.25 and 8.3 GHz light curves of the strong X-ray source Cyg X-3. Comparison of these data show very specific relationships between the hard X-rays and radio emission. Most of the time Cyg X-3 is in a quiescent state with radio emission slowly varying at levels of a few hundred mJy; in this state the hard X-ray variation are anti-correlated, appearing as if log(radio flux) is inversely proportional to hard X-ray flux. During the hard X-ray quenching preceding a large flare, the radio and hard X-ray variation are correlated, and for the several days of a strong radio flare, the hard X-rays make a transition between the correlated and the anti-correlated states.

Plots showing typical multi-wavelength data for Cyg X-3 from Dec. 1996 to mar. 1997 are shown in Figure 1. In the middle of this time range there was a 10 Jy radio flare with the associated X-ray state changes. The radio flare was preceded by a 40 day high-soft X-ray state where the hard X-rays are quenched, and there was a transition to a low-hard state at the time of the big radio flare.

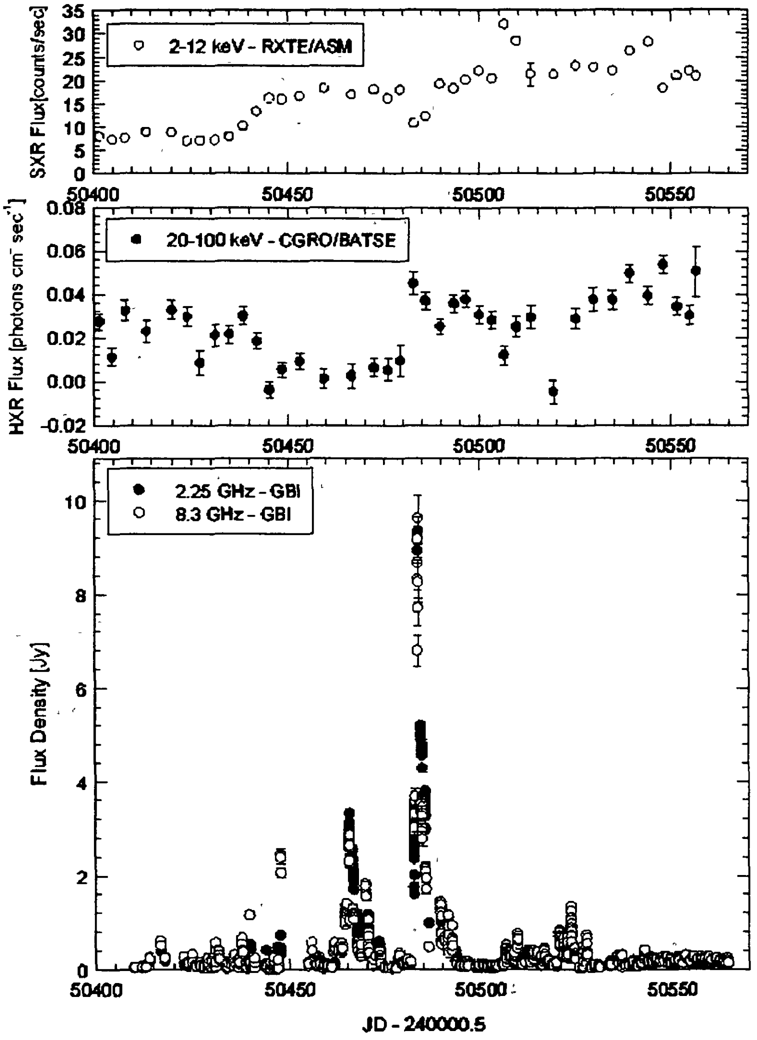

Figure 1. Soft (2-12 keV), hard (20-100 keV), and radio light curves for the X-ray binary Cyg X-3 before and after the 10 Jy radio event in Feb. 1997

Although it is hard to see on the radio light curve scale of Figure 1, Cyg X-3 shows the well-known quenching of radio emission from normal quiescent levels of a few hundred mJy to a few to ten mJy for a few days just before the largest radio flares. In Figure 1, one sees an even longer period of hard X-ray quenching that occurs for about 40 days before the big radio flare. Because of the early warning from the GBI for this 10 Jy flare, Mioduszewski et al. (1999) were able to make a VLBA image sequence showing that the jet emission was highly relativistic (v>0.8c) and one-sided. This was an early example of using the GBI to trigger time-critical observations by other radio and X-ray instruments.

Sco X-1 was the earliest X-ray source observed by the GBI in the 1970s. During that time it exhibited flares lasting typically a few hours that sometime rose to levels of 0.25 Jy. Since then simultaneous radio and X-ray campaigns with the VLA and the Ginga X-ray satellite established a clear correlation between the radio and X-ray states of Sco X-1. Since the beginning of the NASA-GBI program, more extensive radio and X-ray coverage has been possible using the GBI, BATSE, and the *RXTE* ASM. After a nearly 15 year period of weaker radio emission, the GBI data starting in 1998 once again showed radio flaring with events on the time scale of hours which are closely related to hard X-ray events, as shown in Figure 2.

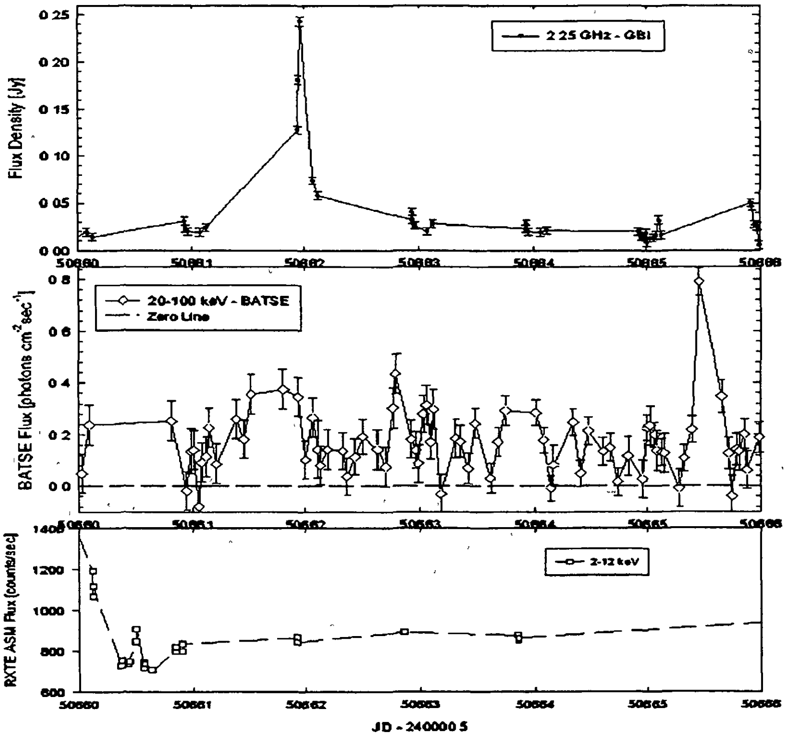

Figure 2 - GBI 2.25 GHz radio light curves for Sco X-1 in 1997 are plotted vs. function of time (Julian Days) along with BATSE 20-100 keV and *RXTE* ASM 2-12 kev data.

A 0.25 Jy radio flare in Sco X-1 is shown in Figure 2 as occurring just at the end of a roughly day-long X-ray enhancement in the BATSE data. Later in Figure 2, an even stronger hard X-ray event is seen to occcur during a gap in radio coverage, while Sco X-1 was below the GBI horizon, and it appears as if the following radio points were dropping from normal levels in what may have been a radio flare. More examples of this type of correlation indicate that the radio flaring appears to occur during the sudden decay of hard X-ray emission from the inner portion of the accretion disk of Sco X-1. This is the same correlation seen in a number of other X-ray transients, including the first two Galactic superluminal sources GRS 1915+105 and GRO J1655-40.

The Galactic Superluminal Source GRS 1915+105

The X-ray transient GRS 1915+105 was the first Galactic X-ray source shown by Mirabel and Rodriguez (1994) to eject jets with an apparent speed of 1.4c and a true speed of 0.92c. Since the NASA-GBI program began, several changes of state in radio and X-ray emission have been seen with a great deal of detailed study by *CGRO* BATSE, *RXTE* ASM, infrared telescopes, and other radio telescopes such as Merlin, the Ryle Telescope, the VLA, and the VLBA. Figure 3 shows an example of the sequence of state changes seen in GRS 1915+105.

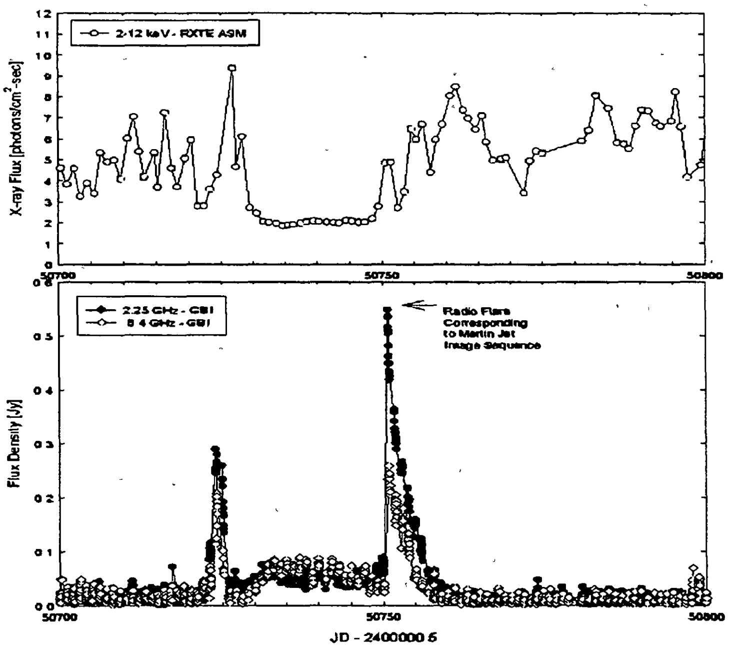

Figure 3 - *RXTE* ASM and GBI radio light curves for GRS 1915+105 during the fall of 1997.

Figure 3 shows that quiet radio levels of GRS 1915+105 are related to soft and highly variable X-ray states as shown here in the RXTE ASM light curve; and that the X-ray state changes to a more quiescent, weaker, and harder X-ray state at the time the radio emission is in a stronger, stable, optically thick state. During this "plateau" state, Dhawan et al (1999) have used the VLBA to show that there is only slowly moving "jet-like" radio emission. Finally, the end of the radio plateau state results in a strong radio flare with relativistic jet ejection, which in this case was imaged by Merlin (Fender et al. 1999) and the VLBA (Dhawan et al. 1999).

CI Cam's X-ray-Radio Flare and Radio Afterglow

In a 1-2 day period beginning May 31, 1998, the X-ray transient XTE J0421+560 was detected by *RXTE* ASM and *CGRO* BATSE. Within 24 hours of the beginning of the event the radio source was put on the NASA-GBI monitoring program and was observed with the VLA, the VLBA, ASCA, BeppoSAX, and many optical telescopes. Figure 4 shows the ASM and BATSE light curves together with the radio light curves from the GBI.

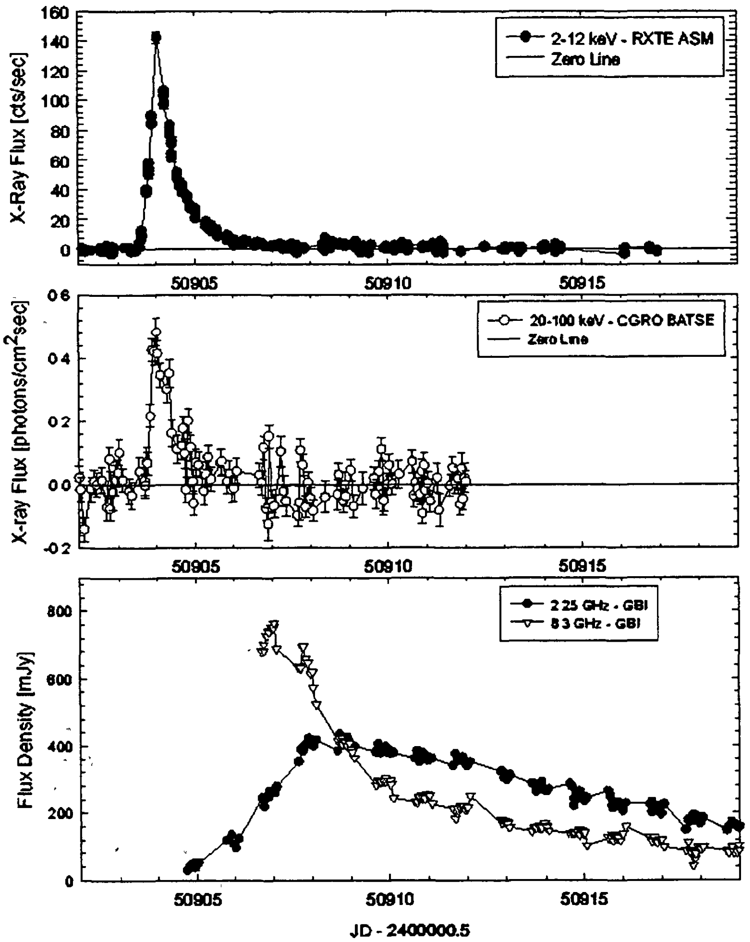

Figure 4 - RXTE ASM (2-12 keV) CGRO BATSE (20-100 keV) X-ray light curves CI Cam are plotted as a function of time (Julian Day) with the GBI radio light curves at the bottom.

The VLA observations by Hjellming and Mioduszewski (1998) placed the location of XTE J0421+560 at the position of the emission line star CI Cam. Once called a symbiotic star, it is generally believed that the strong X-ray, extremely short-lived X-ray source shown in the light curves in Figure 4 could not have been produced without a compact object like a neutron star or a black hole being one of the stars in the binary star system called CI Cam.

The later portions of the GBI light curves for CI Cam (cf. Figure 5) show a slow and steady decline with a power law that starts at $t^{-1.1}$ and evolves to $t^{-0.9}$.

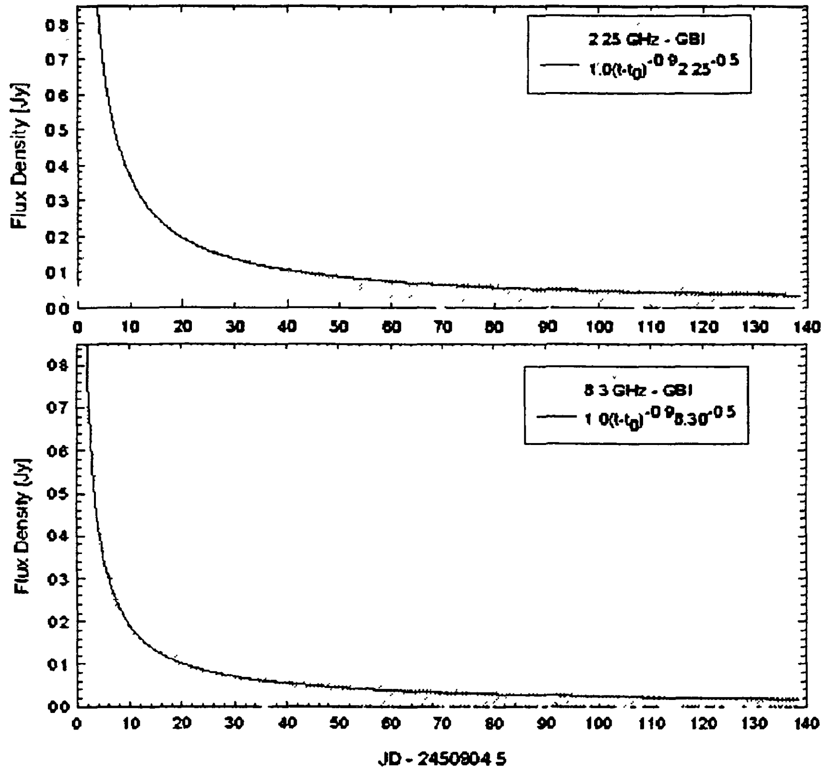

Figure 5 - GBI radio light curves showing the slow decay of the "radio afterglow" of CI Cam at 2.25 and 8.3 GHz.

Slow decays of the radio emission from black hole X-ray transients has been seen before in GS2023+32 (V404 Cyg, Han and Hjellming 1992) and GRO J0422+42 (Shrader et al. 1994). It was speculated that this could only come from a very slowly expanding synchrotron radio source. When CI Cam showed a similar slow power law decay, it was realized that this was the first chance to do high resolution imaging of the radio afterglow of an X-ray transient with the VLBA. Mioduszewski et al. (2000) had previously obtained VLBA images of CI Cam within one and five days of the X-ray outburst. This imaging of a slowly decaying radio afterglow was successful when June 14, 1998 images, followed by July 16 and October 15 VLBA images were obtained. Figure 6 shows the April 4 and June 14 images of the CI Cam radio remnant or afterglow. It shows an asymmetric radio source expanding to the NE and SW with velocities of ~1000 km/sec and to the NW and SE with velocities of ~600 km/sec.

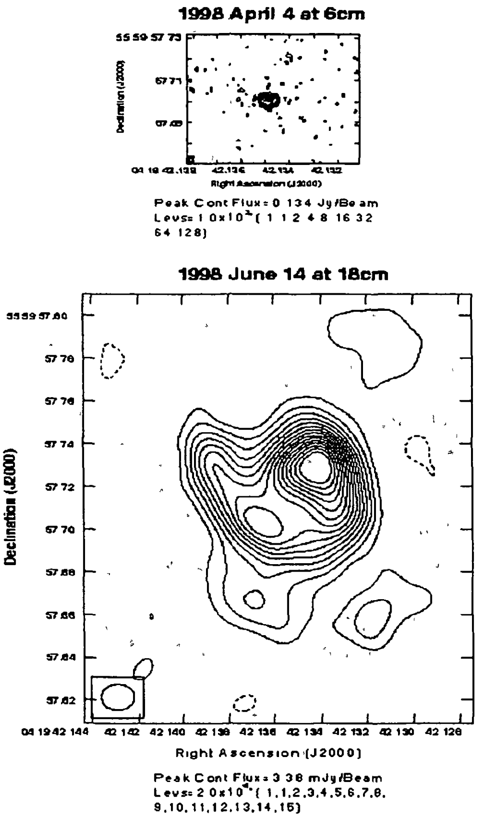

Figure 6 - VLBA images of the radio remnant for CI Cam made by Mioduszewski et al. (2000) on April 4 and June 14, showing the slow expansion of the synchrotron radio source. The synthesized beam for each image is shown in the lower left corner.

The Future of the GBI

The examples just discussed illustrate the special role that the GBI has played, and can continue to play, as a radio monitoring instrument supporting X-ray and other instruments. During June 1998, a new NASA-NRL-USNO-NRAO collaboration was formed to continue monitoring of radio sources in support of multi-wavelength campaigns/observations. Money or personnel working on the GBI programs is to be provided by NASA, USNO, NRL, and the NRAO. The new program will extend the current practice of putting all data in the public domain within 24 hours to nearly real-time calibration and data availability.

Plans are under way for NRAO to promote educational outreach for NRAO data sources, starting with the GBI & 85-3 pulsar monitoring telescope, to make data and related information about radio sources available to the public at both visitor centers and on the Internet. If plans develop as hoped, Java-driven web pages with access/analysis software will make it possible for student and professional researchers to plot and analyze these public domain data; it will become a National Internet Radio Observatory (NIRO).

Replacement of the on-line computer system with a more modern PC-driven system, together with upgrading of certain GBI hardware components, will be necessary in the near future. If the new collaboration between NASA, USNO, NRL, NRAO, and possibly other institutions, continues the operation of the GBI it should continue to be a scientifically productive instrument. There is no doubt that the second interferometer in which Barry Clark was one of the principal "builders" has been very productive to the present time, and may continue to be productive in the future.

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The NRAO Radio-Link Interferometer

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Interferometry in the 50s, 60s and 70s:

The period from 1955 to about 1975 was a very exciting time for the young science of radio astronomy. Radio sources were identified with extremely distant objects; their energy output was enormous and they were located at the 'edge' of the universe. The emission from stars, much more energetic than that from the sun, was detected. Emission was also detected from molecules located in stars and gas from which temperatures and densities could be determined.

Although many of the initial discoveries were made with large single antennas, arrays of antennas were needed to resolve details in the sources and to avoid the blending of individual sources. The major interferometers and arrays built in this period had three somewhat separate goals. Some arrays were used mainly to survey the sky to catalog hundreds of new radio sources. Examples are the arrays at Cambridge to generate the 3C and 4C catalogs, the Mills Cross to generate the Mills, Slee, Hill (MSH) catalog. Telescopes run by The University of Texas, Ohio State University and the University of Illinois also catalogued many new radio sources. Single antennas such as Parkes and the 140-ft in Green Bank also were important in compiling source catalogues, especially at relatively high frequency.

Other interferometers were used to obtain high quality images of radio sources. The need for many baselines required many elements and/or movable antennas. Such arrays were the Owens Valley twin 90-ft antennas, the Cambridge 1-mile antenna, the Green Bank three-element interferometer and the Westerbork array, in addition to several arrays in Australia. These instruments covered the resolution range of $5''$ to $60''$. A few interferometers were already searching for emission at resolutions less than one arcsec. Three examples are the Jodrell radio link experiments, early Mills Cross long baseline experiments and the first VLBI experiments, which are documented elsewhere in this book.

Around 1963, it became clear that a multi-element, synthesis instrument with optical resolution ($< 1''$) was needed to understand the complex angular emission coming from quasars and galaxies. Arrays in Cambridge and Westerbork were extremely successful in imaging radio sources and the need for more resolution was obvious. In the early 1960s two arrays capable of arcsecond resolution were considered in the USA: The Caltech Owens Valley Array, an 8-element array with 130-ft antennas; and the NRAO Very Large Array (VLA), a $\sim$30-element array of 25-m antennas. Both array designs and institutional capabilities had their strengths and weaknesses. Discussions of the American array continued through most of the 1960s.

The NRAO Radio Link-Interferometer at Spencer's Ridge

The group at Caltech admittedly had more experience and expertise in interferometry than that at NRAO in the early 1960s. In order to ameliorate this deficiency, the NRAO built a second 85-ft antenna and placed it about two kilometers from the original 85-ft Tatel antenna to produce an interferometer. Operation began in 1963 and this was the first instrument at NRAO where hands-on interferometric experience could be obtained. When Radhakrishnan, on a visit to NRAO in 1963, was asked for advice to improve interferometry at NRAO, he suggested two additions: add a third antenna to obtain short spacings and get Barry Clark, just graduating from Caltech. Both suggestions were implemented relatively quickly—the second acquisition having much longer-term ramifications for NRAO, the VLA and for VLBI.

Observations with the three-element Green Bank interferometer (GBI) produced good images of many extragalactic sources with about 10″ resolution, and it was among the first instruments to detect radio stars. However, the experience needed for the VLA with its intended 1″ resolution, required a much longer baseline array. Some of the known technical problems faced by the VLA were: multi-frequency feeds with dual polarization, large delay elements with accurate tracking; computer control of antennas and data aquisition; and connecting the antennas to a correlator. The effect of tropospheric refraction irregularities was also found to be a serious problem at arcsecond resolutions—in fact, imaging at arcsecond resolution had been called into question by recent studies of tropospheric irregularities (Hinder & Ryle 1971).

NRAO thus decided to add a small antenna of diameter 42 feet and place it about 11 km away on Spencer's Ridge to the northeast, overlooking the three-element interferometer. At this time the operating frequency of the system was 2.7 GHz. The design and construction were led by George Swenson from the University of Illinois, and in 1966 observations on this 11-km baseline began. The control and data signals were carried to and from the interferometer control building and the 42-ft antenna by means of a high bandwidth radio link.

For a two-year period Barry Clark and John Basart (now at Iowa State University) observed for about one week per month. Barry concentrated on the myriad of computer control, delay tracking and other technical aspects of this interferometer; John looked at the resulting correlations and tried to understand the atmospheric and instrumental changes, some of which Barry could fix and some of which were external properties of this relatively long baseline interferometer. George Miley (now at Leiden University) observed a large selection of high redshift quasars with this instrument and was the first to comment on the possible cosmological interpretation of the angular size-redshift relationship. Images of radio galaxies were made by Frank Bash (now at University of Texas), Dave Hogg and Campbell Wade.

The NRAO Dual-Frequency Radio Link-Interferometer

The three-element Green Bank Interferometer was upgraded in 1968 to dual-frequency (2.7 and 8.1 GHz) and dual polarization and it seemed 'natural' to also upgrade the radio-link station. So, NRAO decided to build a 45-foot antenna with the same capabilities as the GBI and, after testing, installed it on a mountain top at Huntersville, about 35 km southwest of the GBI. The software for the GBI was improved so that all four elements (three 85-ft and the 45-ft) could be operated together. Many people worked hard and long hours to make the 4-element system operational: George Conant and Barry Clark for the on-line software; Jim Coe, Ron Weimer, Bill Shank, Dwayne Schiebel and Larry Rudnick on the electronics and new delay system (Coe 1973). George Grove, Claude Williams and Ed Fomalont helped with the general operations, including the pointing and delay setting of the antennas, as well as with visitor use. The reduction packages were written by Eric Greisen and Melvyn Wright.

A new radio link was built to connect the interferometer control building with the Huntersville site and much of the development was done by N.V.G. Sarma who was on sabattical from Ooty for a year. After a year of debugging and operation, it was found that the signal attenuation on the radio link was small in winter, but very high during the summer. It turned out that the line of sight went through the top of a mountain and the extra attenuation in summer was caused by leaves on the trees. In order to obtain a direct line of sight between the interferometer control building and the radio link at Hunterville, a reflector was put on the hill behind Green Bank which had a direct line of sight to the tower 35 km southwest at Hunterville.

The Radio Bending Experiment

Irwin Shapiro pointed out (1967) that the measurement of the bending of radio waves near the sun would provide a better test of general relativity than by the optical observations, generally made during solar eclipses under difficult conditions. Between 1969 and 1973 there were, in fact, ten such experiments which are listed in Table 1. All of these observations monitored the sources 3C273 and 3C279 in order to determine the change of their relative position over a period of a few weeks. Around October 8, 3C279 is actually occulted by the sun. All of these experiments produced results in agreement with general relativity, with an error between 3% and 17%.

TABLE 1.
RADIO GRAVITATIONAL BENDING EXPERIMENTS

Experiment Period	Array	Bsline (km)	Bending Rel to GR	Reference
1969 Oct 30-Nov 15	Owens Valley	1.1	1.01 ± 0.11	Seielstad (1970)
1969 Nov 02-Nov 10	Goldstone	21.6	1.06 ± 0.12	Muhleman (1970)
1970 Oct 30-Nov 15	Mullard	1.4	1.07 ± 0.17	Hill (1971)
1970 Nov 02-Nov 15	Golds./Haystack	3900	1.03 ± 0.11	Shapiro (1972)
1970 Nov 02-Nov 26	Green Bank	2.7	0.90 ± 0.05	Sramek (1971)
1971 Nov 02-Nov 25	Green Bank	2.7	0.97 ± 0.08	Sramek (1972)
1972 Nov 02-Nov 13	Mullard	5.0	$1.04 \pm 0;08$	Riley (1973)
1972 Nov 05-Nov 13	Westerbork	1.4	0.96 ± 0.05	Weiler (1974)
1972 Oct 23-Nov 20	140-ft/Haystack	845	0.99 ± 0.03	Counselman (1974)
1973 Nov 03-Nov 12	Westerbork	1.4	1.038 ± 0.033	Weiler (1975)
1974 Mar 29-Apr 25	Green Bank 35km	35.3	1.015 ± 0.011	Fomalont (1975)
1975 Mar 26-Apr 27	Green Bank 35km	35.3	0.999 ± 0.011	Fomalont (1976)
1980-1990	VLBI-IRIS	5000	1.0002 ± 0.002	Robertson (1991)

Dick Sramek, who ran the GBI relativity experiment in 1970, was well aware that the Green Bank Radio Link interferometer was an excellent instrument for this fundamental experiment. The baseline was sufficiently long so that the bending accuracy of 0.03″ needed for one percent accuracy would produce tens of degrees of phase change; the system was sensitive so that a good signal-to-noise ratio would be obtained even when the sources were close to the sun; and the dual frequency capabilities would permit the removal of the solar coronal refraction which becomes even larger than the relativistic bending at 2.7 GHz near occultation.

With keen anticipation, the observations were made by Ed Fomalont and Dick Sramek on seven days during March and April of 1974. In order to minimize systematic errors, three sources were observed in sequence for three minutes each, observing simultaneously at 2.7 GHz and 8.1 GHz. The source 0116+08 was occulted by the sun on April 8 and produced most of the gravitational bending. Two nearby calibrator sources, 0111+02 and 0116+08, on nearly opposite sides of 0116+08, provided for the calibration of the instrument. The appropriate difference between the 2.7 GHz and the 8.1 GHz phases removed the coronal (and ionospheric) refraction. The appropriate sum of the phases for 0111+02 and 0116+08 provided a calibration which removed any systematic spatial phase in the sky. The relatively fast switching amongst the sources removed systematic temporal phase changes in the instrument and troposphere.

The bending found from the 1974 experiment is listed in Table 1. Only half of the general relativity bending is expected from classical Newtonian theory. For the Brans-Dicke theory, a bending of 93% to 96% of GR is predicted and is well outside the margin of error of the results from the radio-link interferometer. A duplicate experiment performed in 1975 produced a results with similar accuracy and somewhat closer to the value predicted by General Relativity.

These two experiments made with the radio-link interferometer at Green Bank were among the most accurate tests of general relativity made to date. Oddly enough, the bending experiment was never attempted with the VLA. The resolution was not any better and simultaneous dual frequency capability was not available. It took about 15 years for VLBI observations, with a potential increase of resolution a hundredfold, to became more routine and stable and surpass the accuracy. The result reported by Robertson et al in 1991, based on 340,000 VLBI observations over more than ten years, is shown at the bottom of Table 1.

Further Observations and Other Use

The four-element GBI was used until October 1978, when 13 elements of the VLA with a 12-km baseline were operational. In its last five years of operation, about 15% of the GBI experiments used the 35-km radio-linked baseline for high resolution imaging and polarization work. Direct imaging with this long baseline was difficult because of the gap of baseline between 2.7 km and 32 km.

After 1978, the USNO/NOAA/NRL groups near Washington, D.C., used the 35-km baseline for measurement of earth rotation and orientation. In order to provide an orthogonal baseline needed to distinguish between polar motion and earth rotation, a similar antenna was built and located about 30 km west of Green Bank near Webster Springs. In 1981, dedicated VLBI arrays took over this type of astrometric observation and the radio link baselines were closed down.

The 45-ft antenna at Huntersville was returned to Green Bank in 1993. In 1997 the Institute for Astronomy and Space Sciences in Japan placed an 8-m antenna, called HALCA, into earth-orbit for high resolution radio imaging of quasars and galaxies. Five tracking stations around the globe were needed for real time telemetry of the radio signals and Green Bank was designated as one of the sites. The 45-ft antenna was then refurbished to become a tracking station for the VSOP project.

Epilogue

The Green Bank Interferometer and, particularly, the 35-km radio link interferometer gave Barry Clark and NRAO the experience needed for many aspects of the VLA design, construction, control and data reduction. The thousands of detailed images and the multitude of scientific results from the VLA are impressive and in no small part due to the adequate preparation from the Green Bank instruments. The significant scientific returns of the Green Bank three-element and radio-link array were clear indications of what the VLA could do.

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The VLA: Planning and Construction

D. S. Heeschen

Planning for the VLA began in 1961, and construction was completed in 1981. The goal from the beginning was to obtain an instrument that would produce maps with the highest feasible resolution, sensitivity, dynamic range, sky coverage, frequency coverage, and speed. It was to be a "National Observatory" telescope, accessible to any competent scientist with an appropriate program. The following chronology, adapted from the first of the references mentioned below, gives a rough overview of some of the significant events during the design and construction of the VLA.

1961-63	**Preliminary design work**
1961	Report of NSF Advisory Panel on Large Radio Telescopes
1962	Phrase "Very Large Array" came into use
1964	Barry Clark arrived in Green Bank
	"Whitford Committee" report issued
	Green Bank interferometer project begun
	VLA design group formally organized
1964-67	**Intensive design effort**
1965	NRAO Users Committee formed
	First visit to Plains of San Augustin by NRAO personnel
1966	First VLA design report distributed
1967	VLA Proposals, Vols. I and II, issued
	Plains of San Augustin proposed as site
	First "Dicke Committee" meeting
1967-69	**Further design work**
	Design group disbanded in 1969
1969	VLA Proposal, Vol III, issued
	Second "Dicke Committee" meeting
1971	"Greenstein Committee" recommendations established
	VLA design work recommenced
	Stanford Research Institute did a VLA feasibility study for NSF
	VLA Proposal, Vol IV, issued
1972	Construction project management organized
	VLA approved. First funding received.
1973	Antenna contract signed

1974	Site work begun
1975	VLA project personnel move to New Mexico
	Two antenna interferometer operating at the site
1976	Ad Hoc Advisory Panel reviews VLA for NSF, Congress
1977	Scientific observing begun, with six antennas
1979	Twenty-eighth antenna completed
1980	VLA dedication
1981	VLA construction completed

I have written and talked about the VLA planning and construction many times and do not propose to do so again here. Anyone seriously interested might wish to read the following two articles:

THE VERY LARGE ARRAY, in Telescopes For The Eighties, an Annual Reviews Monogram; Burbidge & Hewitt editors, 1981.

REMINISCENCES OF EARLY DAYS OF THE VLA, in Radio Interferometry, IAU Colloquium 131, ASP Conference Series Vol 19, p150; Cornwell and Perley editors.

I do however want to say something about Barry Clark's role in the VLA. Barry has a longer continuous association with the VLA than does anyone else. He joined NRAO in 1964 and immediately became an important participant in the design of the instrument. During construction he was responsible for all computer hardware and software, for both on-line monitor and control and for data processing. He had to contend with rapidly developing computer technology, and with rapidly expanding demands and techniques for data processing. And he had a very tight budget. But he handled it all beautifully.

He also handled a sometimes meddlesome director pretty well. I used to come out to the site at least once a month during construction and always stopped by Barry's office. My visits with him were usually pretty short, partly because he wasn't nearly as talkative then as he is today, and partly because I could never find a place to sit down. I eventually began to suspect he was borrowing stacks of computer printouts from neighboring offices to put on every flat surface I might otherwise sit on, whenever he anticipated a visit from me.

Barry's contributions to the VLA did not end with the construction, as we all know. He went on to play a leading role in just about all subsequent software and hardware development, in scheduling, and in dispensing sound, concise advice. Anyone who has been involved with the VLA in any way owes some debt to Barry. His influence, spanning 34 years and still counting, has been enormous. Besides his specific contributions, I personally am most grateful to him for his unswerving faith in the concepts of NRAO and the VLA, from the very beginning, and for the fact that he was always (and still is) a voice of calm and reason just when those qualities were most needed.

The Early Days of VLA computing

Gareth Hunt

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1 Introduction

While I was looking back over past quarterly reports of NRAO to the NSF, I came upon the one for the fourth quarter of 1964. In that report:

- In Green Bank, on November 10, the superstructure of the 140 Foot Telescope was raised, and on December 21 the completed antenna was rotated to the zenith for the first time.
- In Charlottesville, the excavation for the building on the grounds of the University of Virginia was started on December 2.
- On December 14, the AUI Trustees agreed upon the location of the 36 Foot antenna on Kitt Peak.
- "The problem of the Very Large Antenna Array is being considered in general. An ad hoc panel of astronomers from various institutions was convened on December 11-12 to discuss the mission and the astronomical parameters of the proposed antenna." This was the first such meeting towards the design and construction of what is now known as the Very Large Array.

and also

- "Dr. Barry G. Clark joined the research group as an assistant scientist on November 30."

With hindsight, we can now see that the three week period (November 30 to December 21, 1964) was an extremely important one in the history of NRAO, including significant milestones affecting all of today's major sites.

2 VLA Computer System Original Design

The original mission of the VLA computer systems was basically threefold. The first was to control and monitor in real-time the antennas and the electronics in the antennas, the electronics in the back-end racks, the correlator, and other ancillary equipment such as the weather station. The second was to apply calibration to the data. Finally, the computers were to make images from a complete synthesis observation. It is interesting to note that this was in the days before the full acceptance of the need for CLEAN, and before the self-calibration algorithm had been developed.

The design was to transmit the observed data continuously to the computers which would perform the calibration and imaging, so both the control and monitor computer systems and the calibration and imaging computers were, in a sense, on-line computers. So new terms had to be coined for the two. The real-time control computers processed interrupts with the heartbeat of the VLA electronics, so they were clearly Synchronous; the data processing computers were not so closely coupled, hence Asynchronous.

3 The Synchronous Computers

As we have said, the fundamental purpose of the Synchronous computers was to control and monitor the hardware, sending control information and receiving monitor data at the fundamental waveguide cycle of 19.2 Hz. Correlations were consolidated with the observational parameters and written in an archive format both to an archive tape and to a circular buffer on a dual-ported disk for the Asynchronous computers.

The Synchronous computer system was a network of mini-computers, each with its own function. The hardware was made up of four Modular Computer (ModComp) 64k (16 bit) word machines and a Floating Point Systems (FPS) Array Processor (AP) model AP120B. In addition, there was a fifth computer used as a backup and for software development. The policy maker (BOSS) also ran the astronomical correction programs and wrote the collated data to archive tape and to the dual-ported disk. The monitor computer (MONTY) primarily dealt with the control and monitor communication and provided the monitor data displays for the VLA operators. Two correlator computers (CORA and CORBIN) were needed to transfer the data from the AP. Since these computers had slightly different hardware configuration as befitting their function, the backup computer (BACCHUS) had the most peripherals – at least one of each peripheral of the other computers as a working spare. Later we realized that MONTY was overloaded servicing the individual interrupts for the terminal screens, and a sixth computer to deal with unit record devices (EUNICE) was added. In fact, CORBIN was never used in production. By the time the second correlator computer was needed, there were other bottlenecks which required the entire Synchronous computer system to be upgraded.

The AP was the first purchased by the NRAO. Several others were later used very successfully in conjunction with VAX computers to provide image processing functions. This first one has outlasted them all – it is still in use today.

The ModComp computers were connected together using a home-built network using point-to-point parallel links. This was the first network at the NRAO, designed and initially implemented by Barry Clark in 1974-5.

The whole system design was very carefully thought out. It was documented in a series of memoranda by Barry – VLA computer memoranda 101-106 and 108-116 – between April 1973 and October 1974. The design has stood the test of time. Although the VLA on-line computers were upgraded in January 1988 to ModComp Classics, the underlying design is unchanged – even the names of the computers (BOSS, MONTY, BACCHUS) have been retained, reflecting the original functionality. It is an outstanding model of the way computer systems should be designed.

Where possible, the system was coded in FORTRAN, but the vast majority of the code was in ModComp assembler. With the advent of a better FORTRAN compiler, more memory, and a larger address space, much of the assembly code was replaced by FORTRAN for the upgrade in January 1988.

Data read from the correlator was initially passed on untouched except for a binary rescaling. However, the antenna gains and phase corrections were gradually understood sufficiently that automatic corrections were made to the data in the on-line system. The data was written to the dual-ported disk and to the archive tape in a very robust format, designed by Barry for the days when magnetic media were much less reliable than today. All tape records are fully self-contained, so that the loss of a record results in the loss of just a single integration. A format such as FITS, which is used by most modern telescopes, does not have this advantage; it can benefit from the increased media reliability. A derivative of the original VLA archive format is still the standard output today.

Under the leadership of Barry Clark, the people involved in the early implementation of the Synchronous system were Phil Dooley, Gareth Hunt, Ernst Raimond (on leave from NFRA), Bill Randolph, and Ken Sowinski.

Four (Barry, Phil, Gareth, and Ken) of the original group of five are still working for the NRAO more than 25 years later.

4 The Asynchronous Computers

The purpose of the Asynchronous computers was to read the correlations either from the dual-ported disk or from an archive tape and to provide the software for data calibration and imaging. This group was initially led by Bob Hjellming.

The initial hardware selected was a DEC-10 (or PDP-10) with a KI-10 processor, initially with 192k (36 bit) words. In that era, the DEC-10 was the best mid-range computer for interactive use. Unusually for its day, it supported interactive source code editing, compiling and linking, and job execution. This was still at the time that most large computers supported primarily sequential job execution from punched cards.

The selection of the compiler was difficult. The FORTRAN compiler was not very advanced, the ALGOL compiler was deemed to be worse, and there was no PL/1 compiler, which most of the programmers would have preferred. Fortunately, there was an elegant ALGOL derivative from the Stanford Artificial Intelligence Laboratory (SAIL). The SAIL compiler was actively used in AI research at the time, and had a rich macro capability.

Work began on a computer language to be used for the VLA calibration and imaging. Recognizing the problems they were facing, the language was christened the Command and Algorithmic Notation for the Data Inundation Device (CANDID). The design was way ahead of its time. Many of the concepts in CANDID – interactive and batch capabilities, loop control, automatic variable declaration, multi-dimensional arrays, heterogeneous structures, procedures, nestable scripts, etc. – were visionary for their time. Indeed, the capabilities are very reminiscent of Glish, the command language used by AIPS++. The philosophy of use was also to be similar: code in the scripting language, then code directly in SAIL (C++ for AIPS++) if and when this became necessary.

Unfortunately, the implementation was far too slow on those old machines. So a new Standard Command Package was developed. The underlying disk structures of CANDID were retained, but the programs used by that generation of VLA observers were a suite of stand-alone programs, with a uniform user interface. This also stood the test of time. With very little software maintenance (<1 FTE) after 1984, the programs were still in full use when the DEC-10 was switched off for the last time in 1990. These programs provided not only the standard VLA calibration and editing capabilities, but also imaging and CLEAN deconvolution.

Barry's major contribution to the VLA calibration was the Antsol algorithm. This brilliant innovation is based on the simple fact that the Fourier Transform of a delta function is a constant. Hence, an interferometer observing a point source should detect the same signal, independent of baseline length and orientation. Any observed deviation from this must be due to atmospheric and instrumental effects in the individual antennas. By observing a radio source that is unresolved at the longest baseline, it is thus possible to determine the instrumental corrections for each antenna. Synthesis arrays, such as the VLA, form interferometers using all possible antenna pairs, so $\frac{1}{2}n(n-1)$ measurements are made with n antennas. For the VLA, with $n = 27$, this represents a problem that is over-determined by a factor of $\frac{1}{2}(n-1) = 13$, leading to a very stable solution for the instrumental defects.

Although the basic idea was Barry's, the original implementation was done by Nancy Vandenberg, and Larry D'Addario wrote the numerical convergence algorithm. This revolutionized the calibration procedures, and it is still used today as the basis for self-calibration, after the observed visibilities are divided by the source model. Antsol is also used in the Synchronous computers to provide on-line calibration.

Barry also rationalized the flagging system used, and implemented process log files. This too was revolutionary at the time; the log file was formatted so that it could be edited, if necessary, and then submitted as a batch file to recreate the calibration and editing steps.

Under the initial leadership of Bob Hjellming, the people involved in the early implementation of the Asynchronous system were Al Braun, Dave Ehnebuske, Jerry Hudson, Bob Pariseau, Dave Rosenbush, Jim Torson, and Nancy Vandenberg. Later Barry Clark took over the group and many others contributed significantly to the project: Carl Bignell, Ina Cole, Bob Duquet, Kerry Hilldrup, Gareth Hunt, Bob Kummerer, Bob Payne, Arnold Rots, Chris Salter, and Ramesh Sinha.

5 The Data Inundation Device

The data rate of the VLA, a fairly modest 35 kBytes/sec by today's standards, provided one of the major problems. Hence the Asynchronous group's moniker of "Data Inundation Device." There were two major attempts to address the image formation of the data produced at this rate. Both relied on being able to edit and calibrate the visibilities fully on the DEC-10 before attempting imaging.

5.1 The Optical Processor

The NRAO commissioned a nine-month study into the possibility of using optical techniques to form the images, and hired an optical engineer (Lew Somers) to pursue this internally. The fundamental idea is, of course, that optical systems perform Fourier Transforms trivially easily. Although a working prototype was developed as part of the study, there were several problems that made it difficult to use.

Basically, fully calibrated data had to be written onto one film and sampled from another, which was imaged by the Fourier Transforming lenses. Since the input film decayed with time, the data had to be written as fast as possible. Solutions including synchronously rotating computer disks were explored. If the data was stored in baseline order, the visibility as a function of time could be recorded acceptably fast on the film. However, there was still the unresolved problem of how to handle crossing baselines. Also, this device could not handle iteration, of course, nor could it deal with the non-coplanar problem, i.e., the Fourier Transform in the third dimension.

Barry was, of course, directly involved in the evaluation of this device. When it was obvious that it would never have the needed flexibility even if it could handle the data rate, he came up with an alternative proposal.

5.2 The Pipeline

For the time, this was a creative proposal to attempt to produce VLA images. The original proposal by Barry was in November 1975. The data was to be fully calibrated and edited using the Asynchronous software. The idea was to fill the calibrator data only into the DEC-10 and subsequently to transfer the calibration information to a Pipeline processor, while the bulk of the data was to be filled directly into the Pipeline. As conceived, these were three PDP-11/70 computers aided by two FPS 120B Array Processors and a custom designed transpose memory. The data would be binned in the Synchronous system, passed to the first PDP-11 (SORTER) to complete the sort into (u,v) order. The data would then be read from dual-ported disks into the second PDP-11 (GRIDDER) to be convolved for Fast Fourier Transforming. The third PDP-11 (MAPPER) would then pass the gridded data to the first AP, which would do the first one-dimensional Fast Fourier Transform (FFT). The partially transformed data would be written into the dual-ported transpose memory in (x,y) order and be read out in (y,x) order into the second AP for the second FFT.

In fact, although much of the equipment was procured, it was never completely implemented as designed. A data filler into the SORTER computer was written, and a complete imaging package written by Wim Brouw using the transpose memory was written inside a single AP controlled by MAPPER. This latter was used successfully for many spectral line observations for many years. The Pipeline, too, was conceived before it was realized how important Self Calibration would become. The required iteration from an image back to the raw data was not easily included. So routine imaging was done in AIPS on a VAX computer with an AP starting in 1979.

In the process, the first network at NRAO between heterogeneous computers was created to communicate between the DEC-10 and PDP-11s; Harvey was written by Al Braun. When DECNET became a reality, Harvey was re-engineered to use it, and the first (thickwire) Ethernet at the NRAO was installed to carry the increased load. Subsequently, the VAX computers were also added.

Primarily involved in the programming of the Pipeline were Al Braun, Wim Brouw (on leave from NFRA), Barry Clark, Bob Duquet, and Bob Payne. Technical evaluation and the early use involved Jacqueline van Gorkom, Miller Goss, and Pat Palmer.

6 The Change in the Computer Environment

Reflecting on the initial computer systems of the VLA, it is perhaps not obvious to younger members of the community today just how much has changed in the last 15 years. The computers then at the state of the art were ≈3 orders of magnitude slower than today's Personal Computers, and they had smaller memories – 32k words of memory was large for a process control computer. All machines had very individual instructions – they were Complex Instruction Set Computers (CISC); Reduced Instruction Set Computers (RISC) were still in the future. Compilers that optimized well were very rare, so it was beneficial to spend a programmer's time recasting a program line by line for efficient execution. There was no standard operating system – Unix, too, was in the future – so it was expected that each new computer had a new operating system to learn. Also, the economics have changed dramatically; each computer in the VLA Synchronous system cost several times a programmer's annual salary. In that sense, spending several man years of effort programming a new computer was cost effective. How times have changed!

7 Personal Reflections

I originally applied for a position with the VLA project in 1973. I received a polite but firm rejection from Bob Burns, who guided NRAO's computing effort throughout the development of the VLA. He obviously unbent and later made me an offer that I could not refuse. However, during his tenure at the VLA site, I was able to tack a copy of that rejection letter to the board in our shared office.

When I was hired to work on the VLA, I did not realize how fortunate I was to be working on the Synchronous system under Barry's tutelage. The Synchronous computer system really was the center of everything. All hardware had to be controlled directly and indirectly by the Synchronous computers, and, to be controlled, they had to be understood by the programmers. As the reliability of the hardware gradually increased, so gradually automatic calibration was applied – more understanding needed. The correlator data had to be packaged in a way that the Asynchronous system (and the Pipeline) could handle, so handshaking between heterogeneous computer architectures was necessary. This really was a wonderful learning environment.

All this could have been predicted. However, what was not predictable, at least by me, was that Barry Clark was the center of the VLA universe. Countless times I heard engineers and scientists discuss an idea and finish by saying: "Let's run it by Barry." So all problems were ultimately discussed with Barry. I was not party to most of these discussions, of course, but enough of them were done casually, perhaps interrupting Barry as he typed his wisdom into the computers, that I was thereby a party to many of the interesting technical issues. A great environment for a young scientist/programmer!

One aspect of Barry's management style was that he took care of all the administration and let the programmers get on with the job. This was to be a very useful lesson to me in the future.

Another facet of Barry was, and still is, his catholic interests and knowledge. His professional knowledge is all-embracing – from cosmology to electronics design. On one famous occasion, he and Phil Dooley chased down a problem, and located a missing wire-wrap connection on the backplane of a computer. I can also remember discussions with Barry on literature from Socrates to Borges, on music, on history ranging down the ages, etc. – all of this with a refreshing insight and a very refined sense of humor. He also plays Bridge and was a founding member of the bus Bridge game, with Ina Cole and Mike Duggan and myself. Originally, almost everyone had to make the daily bus trip to and from Socorro. Reading, talking, and napping helps to pass the time, but we came up with another idea. Initially, on the hour long trip home, we played Bridge on a briefcase perched precariously between two seats across the aisle. Others soon became interested, and it became popular enough that we requested special modifications to be made to the buses. Two seats were reversed to allow a table to be added for greater convenience and enjoyment. Even more became interested, and two other seats were reversed to allow two games. Competition for these seats became quite fierce!

8 Acknowledgments

I would like to thank Ken Sowinski, my longtime colleague in the VLA on-line computer systems group, for helpful and insightful comments while preparing this article.

This is a short, perforce personal, summary of the early days of computing at the VLA. It has been necessary to summarize the contributions of many individuals into a few lines. Some contributions have probably been overlooked. While this is probably unavoidable, it is nevertheless regrettable, but hopefully understood and forgiven.

I would like to thank all of those who made those days so interesting, challenging, and rewarding – foremost among them Barry Clark.

The VLA — *AIPS*

Eric W. Greisen

Abstract

At this writing, the *AIPS* package of software has been in active development and use for more than 20 years. The present manuscript is an attempt to summarize the discussions and earlier software packages that led to the creation of *AIPS* and to describe what *AIPS* was like during its formative years.

1 Introduction

The NRAO Astronomical Image Processing System (*AIPS*) is a software package for interactive (and, optionally, batch) calibration and editing of radio interferometric data and for the calibration, construction, display and analysis of astronomical images made from those data using Fourier synthesis methods. Design and development of the package began in Charlottesville, Virginia in 1978. It presently consists of over 1,000,000 lines of code, 100,000 lines of on-line documentation, and 300,000 lines of other documentation.[1] It contains more than 386 distinct application "tasks," representing *very* approximately 70 man-years of effort since 1978.

In contrast with modern practices, *AIPS* was not designed on paper and then translated into code. It was not accompanied by code and documentation management systems, and even reports to management and oversight committees were essentially informal and irregular. The first *AIPSLetter* did not appear until November 1981, the *AIPS* Memo Series was not begun until April 1983, a code checkout system was not in place until June 1984, and a proper code management system with full accountability and recoverability was not instituted until December 1990. Files recording transactions in the earlier years were allowed to disappear when they were judged "obsolete." In fact, records of the export of *AIPS* were originally kept only in the form of the n^{th} copy of the tape shipping order forms thrown into a table drawer. As a consequence of this casual attitude, the historic record for this project is spotty. The files have yielded remarkable original design documents in some areas and no hint that major committees even existed in others.

Nonetheless, it has been fun and instructive to delve into the historic record and I hope people whom I may slight will forgive me for the following summary. Further current information on *AIPS* can be obtained by writing by electronic mail to `aipsmail@nrao.edu` or by paper mail to the *AIPS* Group, National Radio Astronomy Observatory, Edgemont Road, Charlottesville, VA 22903-2475, U.S.A. *AIPS* information is also available on the the World-Wide Web at URL `http://www.cv.nrao.edu/aips`. *AIPS* Memos 61 and 87 are particularly helpful, along with an early article by Don Wells.[2]

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[1] Counted on 29-May-1997 and omitting the GNU copyrights, PostScript files, and obsolete areas.

[2] Greisen, E. W., *AIPS* Memo No. 61, "The Astronomical Image processing System," September 1988 and Bridle, A. H., Greisen, E. W., *AIPS* Memo No. 87, "The NRAO *AIPS* Project - a Summary," : April, 1994 and Wells, D. C., "NRAO's Astronomical Image Processing System (*AIPS*)," *Data Analysis in Astronomy*, Eds. Di Gesù, V., Scarsi, L., Crane, P., Friedman, J. H., Levialdi, S., Plenum Press, New York and London, 1984.

2 Early Committees

The debates about software for the VLA probably began with the first design documents for the telescope if not sooner. The January 1967 VLA Proposal Volume II contains an interesting and insightful view of the computing problem in a chapter attributed primarily to Barry Clark. Its opening paragraph was:

> "Early in the design stage of the VLA, it was realized that an array of several tens of antennas connected to several hundreds of receivers would present problems in control and display far beyond those encountered in any radio astronomy system presently in operation. The most immediate solution to these problems is to have a digital computer perform the detailed functions, receiving from the operator only a generalized description of the task it is to perform. Once one conceives of using a digital computer for control and monitoring of the antennas and receivers, it is very natural to conceive of extending its duties to the manipulation, control, and display of the data of the array as well. Indeed, the computation problems in data manipulation are very quickly seen to be of much greater magnitude than those in monitor and control."

The Proposal goes on to say

> "After the completion of the observation, the computer will sort the observed data points onto the u-v plane, calculate and apply various calibration corrections, combine observations, apply a weighting specified by the observer, perform the Fourier inversion, and output a map of the region of sky under study. This will be done asynchronously with the computations necessary for observations, but at a rate such that the computations will not fall behind. No backlog should be allowed to form."

After a detailed analysis, it was concluded that a computer of 2 million floating point operations per second (MFLOPs) would suffice. The total cost would be $3.4 million for computer hardware including communication to the telescopes and $150,000 for the software. More insightful perhaps was the remark

> "In either case, the output map would be recorded on magnetic tape for further computer processing, though it is not immediately anticipated that this special processing should be programmed on the VLA computer system. This is probably more suitably done on a large, general purpose computer."

This last sentence has occupied a lot of us ever since. It was soon decided that we should move the asynchronous portion of the computing to a computer system fairly isolated from the real-time machines. A DEC-10 was purchased for the purpose and over the years several software systems developed to run on it. These are the subject of other papers and will not be described here other than to remark that the study and design documents for these systems helped to refine the lists of operations which we hoped to do on the *uv* data and images produced by the VLA. It was both amusing and frightening to watch as the "required" computer power grew with every review of the data processing needs.

In May 1976, three months before VLA antenna number 6 was supposed to be delivered, a VLA Advisory Committee meeting was held. In preparation for the meeting, memos were written by Bob Hjellming and Bob Burns. Hjellming's memo was mostly concerned with immediate problems but asked whether one-fourth of the asynchronous group should devote its time to solving imaging questions related to bandwidth and time smearing and to curvature of the celestial sphere (problems not really solved to date) and whether "the basic assumption that all calibration should be done on-site" was correct. He did propose an output *uv*-data format not unlike the Export format to be described below. Burns was more concerned with questions related to off-site data processing, in particular a plan to use the Charlottesville IBM 360/65 to begin an "interim post-processing development." Showing considerable foresight, Burns asked questions about computer independence, mini-computers, and the use of large computational centers. In July 1976, Burns wrote a lengthy memo, later called VLA Computer Memorandum 139, entitled "VLA Post-Processing: An Initial Discussion and Proposal." He defined post-processing to include:

1. "Additional editing and calibrating, if required,
2. *uv*-plane data display,
3. further map synthesis,
4. map correction,
5. map analysis and interpretation, and
6. map display for user and for publication."

After an analysis of available facilities, current practice and estimates of the software needs for VLA processing, he concluded "The scientific output of the VLA will be diminished if the NRAO does not provide adequate facilities for all stages of the data manipulation and analysis." He discussed machine independence again and described a plan to develop software both on the existing IBM and on a mini-computer which would be equipped in time with interactive display devices and an array processor.

So we talked about it for another couple of years. In March 1977, Dave Heeschen, NRAO's Director, formed an in-house scientific committee chaired by Mort Roberts to investigate a number of questions related to off-site processing both at some NRAO facility and at the users' home institutions. The report of this committee appeared in October 1977 and supported the concept of a central large computer with several mini-computer systems, of which some would be at non-NRAO facilities. They deduced that the software would have to be provided by NRAO and urged immediate commencement of a project to produce a post-processing system. In November 1978, Mort Roberts, by then the NRAO Director, asked Dave Shaffer to chair a committee "to propose a unified approach to VLA post-processing." Judging by private notes of mine and remarks on drafts by Shaffer, the discussion had become considerably more acrimonious, dividing primarily on Socorro versus Charlottesville lines. A report was finally issued in February 1979, that basically defined the initial *AIPS* project, concluding

> "Our principal recommendations are thus: A Development Group in Charlottesville; a complete off-line system for New Mexico, to be ready and delivered in 1980; sufficient on-line capability for the VLA site; and at least one, preferably two, systems for Charlottesville. Additional systems depend on user demand."

3 Charlottesville Software Packages and Critical Steps

The last half of the 1970s did not consist entirely of committee meetings and reports, even if it sometimes seemed that way. A number of much more practical developments that eventually led to *AIPS* also took place. The first important step was the development of two formats for the export of data from the VLA. In September 1975, Bill Randolph wrote VLA Computer Memorandum 126 detailing the "Data Format from Synchronous System." This synchronous format was a clever, scalable format with a directory at the beginning giving individual length and address pointers to the areas containing data on sub-arrays (and sources), antennas, bad correlators, and the actual visibilities. This structure allowed more information to be appended to each area without doing major violence to existing software, allowing the format to go through numerous revision numbers during its lifetime. (Revisions 2 and 3 were described by Randolph in an addendum to Memo 126 in July 1976; by April 1982 a re-written memo detailed revision numbers up to 7.) This format was as nightmarish in its binary form as it was clever in its logical form. The data values were in ModComp integer, floating, and extended precision binary forms, but each 32 bits worth of data were expanded to 40 bits on tape with 36 bits intended for the DEC-10 asynchronous computer and 4 bits always ignored. Despite these choices, which were intended to make convenient the reading of these data by the DEC-10, the program that filled the DEC-10 data base was still known by the name "blood sucker" for what it did to everything else attempting to run on that eventually overloaded machine.

The Export format was intended to deliver calibrated and edited data from the DEC-10 on magnetic tape. The meeting to design this format, held at the VLA in September 1976, became somewhat acrimonious since many of the VLA personnel assigned to the problem believed deeply that, and I quote, "no one outside the

VLA will ever be able to read or make sense of these data." Fortunately, Barry Clark, if my memory serves correctly, stepped into the discussion and persuaded us to develop a reasonable format anyway. I wrote a memo to Clark dated November 1976 summarizing the format we had designed. A later undated memo by Dave Ehnebuske and Jerry Hudson described an improved Export format that *AIPS* can still read. The tapes were written in IBM VSB (variable, spanned, blocked) format with the unusual specification that each logical record ended with 4 16-bit words which would describe the next logical record. The tape began with format definition records, but the practical problems of negotiating changes meant that we never used this invitation to upgrading the format over time. Fortunately, the logical records and the very structured form in which they occurred were well designed and did not require serious modification. Bytes were in standard industry order and tape form and all data were in integer with decimal points at defined locations within the words. These attributes made the data easy to read on a wide variety of computers but must have been hard work for the DEC-10 to write. The Export format was supposed to be in production, according to some of the 1976 memos, by Spring 1977. However, the memo I wrote to Barry Clark indicating that I had received a usable tape was dated December 1977 and I wrote a number of other memos to Clark through most of 1979 indicating residual problems with the Export tapes written by the DEC-10.

Spurred, I suspect, by the "no one outside the VLA" remark, Fred Schwab and I decided to write a package of VLA data reduction programs to run on the IBM 360/65 in Charlottesville. We had both worked on the Green Bank Interferometer software, so we simply converted all of that package to work with a VLA-appropriate format similar to the one used for Green Bank data. It took us four weeks of furious effort to get the package going, although rewrites to allow for more antennas and to add new capabilities occupied us for some time thereafter. By March 1977[3] this package was able to read synchronous system data tapes and perform a variety of operations on the data. It had an elaborate algorithm to find and flag bad data, which were all too common in the early days before sophisticated on-line flagging. It also had the Green Bank package's clever routine to find antenna locations and a full suite of routines to correct data for changes to source and antenna positions and for various atmospheric and elevation effects. It did the standard gain and polarization solutions and applied them to the data. It had averaging, sorting, and model fitting and subtracting programs as well as several printer displays for the *uv* data. It also was able to map the data, do a standard Högbom Clean, and display the images on printers, CalComp plotters, and the Dicomed film recorder. It even had a Users Guide. The programs were in PL/1 and were run in sequences managed by IBM Job Control Language, `PARMS` and `INCLUDE/EXCLUDE` cards with data normally read from and written to tape.

By September 1977[4], a second package of IBM PL/1 programs had been created based on the Export format. This "`DEC`" package had *uv*-plane capabilities similar to the "`VLA`" package and used the same map Cleaning and display programs and program control logic. Development of this package continued through all of 1978 and 1979. It was slowed by problems with the Export data and by my devoting much of my time to a direct fore-runner of *AIPS*. Although powerful, both of the IBM packages suffered from the inherent defects of batch systems. As stated by Burns and Greisen,[3]

> "However, we feel that the long waits required in batch mode for the results of each sub-operation lengthen the data reduction process enormously and cause the astronomer to lose his concentration and to take shortcuts in the processing. The latter effects can degrade the final results. The combination of real-time displays with a responsive computer system, similar to that used for single-dish processing at NRAO, would allow the astronomer to process his data more rapidly, to maintain his concentration on those data, to check fully the results of his data manipulation, and to discover more easily unexpected problems or results which may be present in his data."

While waiting for management to give us lots of expensive hardware, I began an ill-advised software system eventually called `NIPS`.[5] The code that was generated was all in ModComp assembly language, although

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[3] Burns, W. R. and Greisen, E. W., VLA Computer Memorandum 140, "VLA Post-Processing: Phase I," March, 1977.

[4] Greisen, E. W., VLA Computer Memorandum 141, "VLA Post-Processing: Phase I Continued," September, 1977.

[5] Greisen, E. W., VLA Computer Memorandum 144, "Post-Processing — Phase I: Technical Memorandum: The Beginnings of `NIPS`", March 1978.

sections in Fortran were planned, and the program structure was geared to use every arcane bit of the ModComp architecture of the day. I learned a lot by doing — and abandoning — this system. It was a heroic attempt to make a small computer do more than it really could, but it completely ignored the wisdom of writing machine-independent code and systems in which more than one programmer could participate.

The last critical pre-$\mathcal{AIPS}$ event was the development of the FITS format. Don Wells has written a lovely history of the event which may be found at the FITS World-Wide Web site.[6] In December 1976, Ron Harten (then at the Netherlands Foundation for Radio Astronomy) and Don Wells (then at the Kitt Peak National Observatory) began a discussion of data interchange formats. They exchanged test data in several forms over the next two years. The National Science Foundation organized a meeting in Tucson in January 1979 whose primary purpose was to make image processing capability more widely available in the U.S. astronomy community. This led to a task force on image interchange formats and a meeting at the VLA organized by Bob Burns 26–29 March 1979. Prior to the meeting, Wells sent around documents describing his efforts with Harten which produced the following remarks from Barry Clark:

> "A single physical block size of either 1440 or 2880 Bytes sounds to me like a reasonable record length. Shorter is inefficient use of tape, longer will encounter buffer problems in very small systems. I suggest the header information should correspond to some reasonable standard, with keywords being the main definition effort. I suggest as a standard for the header that keywords be limited to six characters, be followed by `'='`, `' = '`, `'= '` or `' ='` and then by a single value. A string of blanks would be equivalent to a single blank. Values would be in the form for Fortran 77 list directed I/O."

Clark included a page titled "Suggested List of Keywords," five of which made it into the final Basic FITS Agreement: **BSCALE**, **BUNIT**, **OBJECT**, **HISTOR** and **COMENT**, although the last two were re-spelled when it was decided to use 8-character keywords. This meeting was remarkable in that it generated a consensus on a very general format, including a flexible way to describe multi-dimensional images initially suggested by Harten. This format, *without change* other than the addition of further forms to follow the Basic FITS images, is an international standard to this day.[7] The importance to the insides and outsides of $\mathcal{AIPS}$ of the general FITS way of looking at data cannot be overstated. The Charlottesville "**DEC**" package produced the first FITS tape in April 1979, a tape was returned by KPNO in September, and the VLA asynchronous system (DEC-10) produced its first usable FITS image tape in November 1979.

4 Things Start to Get Serious

Most of the period April 1978 through June 1979 was spent in innumerable design discussions, trying to figure out what we should do and how and where we should do it. In the Fall of 1978, I visited the VLA to take a close look at the IMPS package developed by Jim Torson with help from Al Braun. A year later, I visited Groningen with Ed Fomalont to get a good look at their GIPSY package. Both of these were ultimately rejected, perhaps because we wanted to develop and/or have full control of our own software. It is true that both systems were not coded for portability and were otherwise tied to their local hardware; IMPS in particular was heavily committed to a particular, very nice graphics device.

The issue was beginning to get serious not only because the VLA was actually producing a lot of data, but also because money began to become available for additional computing hardware. Dave Heeschen finally (in our view) responded to our memos (and undoubtedly the input from a great many other people) to offer Bob Burns about $300,000 to buy peripherals for the ModComp in Charlottesville to begin a post-processing project. Burns admitted to me that he was so tired of asking for the money that he almost turned it down; fortunately, he did not. Let me remind the reader what things cost in 1977:[4] an 84-Mbyte, 3330-type disc

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[6] in particular `http://www.cv.nrao.edu/fits/documents/overviews/history.news`.

[7] Wells, D. C., Greisen, E. W., and Harten, R. H., 1981, "FITS: a flexible image transport system," Astronomy and Astrophysics Supplement Series, 44, 363-370. Further references and description may be found at WWW `http://www.cv.nrao.edu/fits`.

with one port and a controller was $32,000, 64 Kilobytes of ModComp memory was $17,500, a Floating Point Systems array processor was around $130,000 with 64 kilowords of fast memory, and an IIS Model 70 TV display cost roughly that amount as well. I remember that IIS memory was $1100 per 512 × 512 bit plane, or roughly $40,000 just for the basic memory in the displays we eventually acquired. And these are 1977 dollars. The purchasing process for an array processor (AP) and image display was begun in the first quarter of 1978; the AP was installed on the ModComp early in 1979 and the image display in the Spring of 1979.

Digital Equipment Corporation announced a new, eventually revolutionary mini-computer called the VAX 11/780 in October 1977. About a year later, Tom Cram and I walked into Bob Burns' office and jokingly suggested that, if he really wanted us to write machine independent code, then he should buy us one of the VAXes to go with the ModComp. We took it to be a joke because the total cost with peripherals would be around half a million dollars, but the joke was on us. Burns pulled some of his magic and a VAX known as `VAX1` was purchased for the VLA but delivered to Charlottesville in the fourth quarter of 1979, with the array processor and image display arriving a few months later. This new computer had real software development tools, a virtual memory operating system (VMS), and dynamic disk file creation and other modern concepts. The ModComp was an excellent real-time computer, but its text editors were simple minded and its debugger non-existent. Furthermore, its 128-Kilobyte memory limit meant that programs had to be heavily overlayed and hence a simple link edit could consume 0.5 to several hours. The ModComp, however, kept us honest in our coding and was largely responsible for the high degree of machine independence eventually achieved by *AIPS*.

Another matter that made things feel serious was the attitude of the National Science Foundation. To quote from a letter signed by William E. Howard, III, Director of the Division of Astronomical Sciences and dated June 5, 1980:

> "... I should reiterate that the VLA data processing problem is one that NRAO must solve within its own budget, that the NRAO must spend its own funds on a computer system that promises to meet visitor needs adequately and that we expect that the data reduction needs of all VLA users, visitors as well as staff, should be met to the same degree and to the same extent that such needs have been met at all the other NRAO telescopes in the past. ... If it appears that visitors must spend unacceptably and unreasonably long periods of time at NRAO reducing and thinking about their maps, the NRAO must place a higher priority on their computer expenditures for VLA reduction at the expense of other programs. ... We expect NRAO to solve the VLA data processing problem, not the NSF."

5 Starting on *VPOPS*, *RANCID*, and *AIPS*

The ModComp computer became a very busy place in the middle of 1979. Fred Schwab developed a stand-alone program named `SCAL` to do self-calibration of VLA data. It took input in the form of time-baseline ordered Export format *uv* data on magnetic tape and source model data on punched cards. The model data could be produced by a special IBM program run on the components map generated by one of the IBM map Cleaning programs. Schwab's July 1979 user instructions are very nostalgic in that they even describe all the devices that need to be powered up and which panel switches to press to start the computer. `SCAL` had a very nice option: it used the front panel sense switches to allow the user to turn on and off displays of the solutions. Parameters were provided to `SCAL` with a question-and-answer session at its beginning.

At the same time, I began coding routines to drive the image display and Tom Cram began in earnest to translate the `POPS` code used by single-dish programs into a program called `VPOPS`. This program was intended to perform quick operations to access the users data catalog and interactive devices and to start separate programs called tasks to perform longer operations. The `POPS` code, developed in the 1970s by Jerry Hudson and put to practical use by Tom Cram, remains the heart of the `AIPS` program today, although we have added functionality to the `POPS` language and code over the years.

The Post-Processing Group in 1979 consisted of Ed Fomalont (scientific direction), Bob Burns (technical management), Tom Cram (systems, `POPS`), David Brown (system support), Fred Schwab (algorithms), and myself (whatever). In September 1979, Tom Cram left NRAO for KPNO and in January 1980 Bill Cotton and Walter Jaffe joined the Post-Processing contingent. There also was a Post-Processing Committee chaired by Carl Bignell which began meeting in April 1979 and continued to meet until it was dissolved at its own recommendation in April 1981. This committee was onerous for two main reasons. The first was the obvious problem of significant preparation plus a week's travel between Socorro and Charlottesville for a significant number of people every couple of months. Less tangible, but more serious, was the somewhat natural state of tension between the two groups, both the tensions between VLA personnel and "outsiders" and between anxious users and overwhelmed programmers. The report of the January 1980 meeting of the Committee, which I cannot find, must have been particularly "interesting." It provoked three significant reactions. The first was a contract let to Jerry Hudson, who no longer worked for NRAO, to study the use of `POPS` for post processing. His report of March 1980 concluded that `POPS` was in need of improvement but was adequate for a "first-generation post-processing system." He also approved of the "task-shedding" scheme. The second reaction was a well-prepared and lengthy report by Ed Fomalont to the Committee for its April meeting outlining major changes including the virtual operating system interface ("`Z`" routines), working file management and I/O, progress on the Clark Clean for the array processor, and a new internal header format. The third reaction was our decision to rename the package to something less likely to be used in every sentence as a rallying point for opposition. That new name was *RANCID*, which stood for Radio Astronomy Numerical Computation Imaging Device, and it achieved its desired effect. For the next year people did not take us so seriously and allowed us to get on with the early design and coding phases with only a reasonable level of political interference.

Finally, of course, the NRAO Director had to go to the Visiting Committee to explain why he had spent around one million dollars on *RANCID* software. This was distasteful to him and he "requested" a new name for the project. On March 31, 1981, the name *AIPS* (Astronomical Image Processing System) was chosen. It beat out `AIDA` (Astronomical Image Display and Analysis), `MIRAS` (Map Imaging Reduction and Analysis System), `DIANA` (Data Imaging and Numerical Analysis), `MADRE` (Map Analysis and Data REduction), `IRIS` (Image Reduction and Improvement System), and a variety of others that were either already taken or unprintable even in those politically incorrect days. I guess that, if the first of these had won, then we would all be knowledgeable today about opera rather than primatology. The effect of the other names is hard to estimate.

In early 1980, a committee of users engaged in a remarkable exercise of attempting to design an internal header for *VPOPS*. I have no papers left from that committee, but remember the results as being rather worse than a cynic would normally expect. Fortunately, Don Wells visited me on April 11, 1980 in Charlottesville and together we explored the idea of using a FITS-like header as an internal one. We realized quickly that a binary data structure based on FITS principles could be used to describe both images and *uv* data and up to 20 "extension" files in fewer than 256 integer words (512 bytes in those days). By the end of that month, the structure we defined that day was adopted as the internal header along with a scheme of computed, mnemonically-named pointers to each of the components which is still in use today.

6 The Joys of Coding and Using Early *AIPS*

The creation of truly portable code was difficult in 1980. Fortran 66 was not a standard language in modern terms. There was no official ANSI standard and word lengths and the relationships between word lengths were not defined. There were no character variables and there were essentially no standard input/output methods. Computers in those days were also very variable. DEC's PDP 11s were 16-bit byte addressing computers which limited a program to 64 kilobytes in length. ModComps allowed programs twice as long, while DEC VAXes allowed any length due to its new Virtual Addressing eXtension. There were 24-bit, 36-bit, and 60-bit computers in widespread use as well as several character encoding schemes. We were told by various committees that we had to write code to run on the PDP 11s, which were owned widely in the astronomical community at that time, but, even with the ModComp to help, we never managed to squeeze our programs down enough to try that porting job. We did set ourselves the goal to run on all of the larger machines we could find and, over the years, had some success in that.

In April 1980, Frank Ghigo took a copy of *RANCID* to install on the University of Minnesota's main Cyber 74 computer. Our ModComp and VAX had 16-bit integers, 32-bit floating-point, and ASCII characters. His computer had 60-bit integers and floating-point and a 6-bit character. When he wrote us in September 1980[8], he had gotten about 70 subroutines to run, which was about enough to get the `POPS` processor to run without most of the current verbs. He was very complimentary about the general portability of our efforts — which were good for the time — although he did write that "initial attempts to decipher `MSGWRT` and its attendant Z-routines led me to suspect the work of a madman." His suggestions for system-wide parameters, better handling of characters and equivalences, and the like caused me to conduct an initially surreptitious re-write of all of *RANCID* as the rest of the group continued to generate new code.

The result of all the attempts to achieve portability and efficiency was an *AIPS* coding style that was ponderous and demanding. Although we hoped to allow astronomers to code in *AIPS*, the complexity of things was too high a barrier for all but the most determined. Among its attributes were:

- All integers were explicitly 16-bit integers, capable of counting only between −32768 and 32767. Integers to count over a wider range consisted of 2 standard integers and all operations on them were performed by subroutines, especially the infamous `ZMATH4`.
- No numeric constants were allowed in call sequences since compilers did not agree on the type that numbers were assigned. Thus, to send a constant to a subroutine, the programmer had to declare and initialize a variable. Our habit was to use obvious names and `DATA` statements such as `DATA N2 /2/`.
- Character strings were stored as 2 characters per integer, 4 characters per float, or as packed strings (as many as would fit). The latter were required for `ENCODE` and `DECODE` of numeric variables, while the former were used for output and string handling. Numerous subroutines were created to access characters and to switch between the forms.
- No assumptions were allowed regarding variable lengths other than certain minimums and that a floating variable held an integer number of reals, etc. System-wide parameters were available in a Fortran `COMMON` to compute pointers into data structures, which were `EQUIVALENCE`d arrays of integers, floats, and doubles.
- Useful statements like `WHILE` and `IF` with `THEN` and `ELSE` were not allowed because some compilers were strict in their Fortran 66 standards. We even had to require all variable declaration statements to precede all `COMMON` statements which preceded all `DATA` statements.
- Because tasks were required to fit in the ModComp address space (or preferably half that), all tasks had to be constructed so they could be overlayed. This meant that `MAIN` routines were themselves short and used to declare all `COMMON`s and to call a sequence of functions (*i.e.*, initialize, do the operation, write history, end).
- System services such as file access, printing, I/O to a terminal, and the like were available only through a virtual operating system interface as presented by the call sequences of a significant number of "`Z`" routines. Over time, virtual device interfaces to television displays ("`Y`" routines) and array processors ("`Q`" routines) were also developed. This allowed us to have multiple versions for multiple operating systems and devices, but restricted programmer access to basic services.
- All images cataloged on disk were in 16-bit scaled integer form. Normally, this required that an image be computed in floating-point form, written to a scratch file, and then re-read to scale and write to the cataloged file.

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[8] Ghigo, F., *AIPS* Memo No. 3, "Adapting *RANCID* to the U. Minn CDC CYBER 74," September 1980.

- For efficiency, computation was expected to overlap disk operations. The code to do this was non-intuitive, requiring, for example, "writes" of a row *before* the data were filled into the row. Buffer pointers, returned by elaborate subroutines, were required at all times.
- Coding was done in one room located between the ModComp and VAX rooms on a variety of simple terminals. Code management was handled by asking around to see if anyone else was working on a particular routine. Debuggers only worked on the VAX, but the ModComp was able to turn up a large number of the bugs. Link edits on the ModComp took hours in many cases since each leaf in an overlay tree had to be link edited separately. There were frequently 3–6 programmers and several others all trying to use one VAX, a VAX that only became a 3-Megabyte computer in the second quarter of 1981.

Things were not much easier for users. The first VAX was shipped to the VLA site in the last quarter of 1980, as planned, along with the FPS array processor, IIS image display, and *RANCID* already on disk. (A replacement VAX was delivered to Charlottesville early in 1981.) A third VAX was turned over to *AIPS* use at the VLA in the middle of 1981, but it was not equipped with the advanced peripherals until early in 1982. To give priority to users assigned priority, a "roller" was added to all array-processor programs. It would periodically roll all of the users' data out of the AP to a disk file and then (after August 1983) check for higher-priority (lower *AIPS* number) AP tasks waiting for the device. If one was found, the task would suspend itself for a time and then check again. Machines became so crowded, however, that the roller action occasionally would fail because a lower priority task could not get the time to roll itself out of the way!

In August 1982, Tim Cornwell wrote an illuminating summary of the rules for post-processing use at the VLA. Users and projects could only use one of the two VAXes and could sign up for time with Ina Cole up to two weeks in advance for a maximum of 8 hours per week. Ina had rules by which she would reduce the time she had assigned to users if others with greater priority requested time, and Ina controlled only half of the available user 1 and user 2 time on each computer. Sign-up sheets for the other half would appear on Monday afternoon for Tuesday and Wednesday, Wednesday afternoon for Thursday and Friday, and Friday afternoon for Saturday through Monday. The maximum time that any group of users could have on each of **AIPS1** and **AIPS2** was 2 hours between 8 am and 5 pm weekdays, 4 contiguous hours between 8 am and midnight any day, and 6 hours between midnight and 8am. Total times were limited to 6 hours in any day and 20 hours in any week. Data were deleted from disk if untouched for 14 days or if the users left the Site.

Why would users put up with such conditions and be willing to work 24 hours a day and to fight each other for resources? Some of the visitors to the VLA did have VAXes at their home institutions and the fraction that did have such facilities grew rapidly with time. However, very few of them had access to the expensive display and, more importantly, array-processor peripherals that NRAO owned. Many of the early users did not particularly like *AIPS*, but the algorithms found in **UVMAP** (optimal *uv*-data gridding and FFT), **APCLN** (image-plane Clean using the Clark algorithm), and **ASCAL** (self-calibration of *uv* data using a Clean-component model), all done with the speed made possible by an array processor, made *AIPS* irresistible. Barry Clark in particular deserves our thanks for not only inventing an efficient algorithm to implement Högbom's Clean technique,[9] but for coding the inner portions of the algorithm in FPS microcode and for allowing Bill Cotton to install that code directly into **APCLN**. Fred Schwab[10] ported his stand-alone self-calibration code with numerous enhancements to *AIPS*. For a while, it retained the nice interactivity of the ModComp control panel, although VAX implementations could not support the option.

7 Progress into a more Modern Era

The first *AIPSLetter* appeared November 1, 1981, and this newsletter has been published more or less regularly ever since. We just sent out Volume 18, Number 1! The first three issues employed special text plotting software, but, with the May 15, 1982 edition, *AIPS* became one of the first astronomical users of

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[9] Clark, B. G., VLA Computer Memorandum No. 152, "An Implementation of Clean," December 1979.

[10] Schwab, F., VLA Scientific Memo NO. 136. "Robust Solution for Antenna Gains," September 1981.

Donald Knuth's typesetting software known as TEX. For many years, the *AIPSLetter* contained a typeset copy of the full `CHANGE.DOC` file so that users and programmers could review all the changes before deciding whether they required an updated release of *AIPS*. This file is now readily available off the World-Wide Web[11] which saves us the enormous labor of typesetting that text and the user the lesser labor of ignoring it. The *AIPS* Memo Series began in May 1983 with a lot of earlier memos plus one that is still used on coordinate representations.[12] The *AIPS CookBook* was initially written by Alan Bridle from notes he made while trying to figure out how to use *RANCID*. It was first offered to the public in November 1981 and appeared in TEX form, edited by members of the *AIPS* group, in September 1983. In March 1984, the old programmer documentation files were re-written and very greatly improved by Bill Cotton. They were published under the title *Going AIPS* Over the years, the *CookBook* has undergone numerous revisions and is still quite current. *Going AIPS* also was revised a couple of times, but has languished since 1990. Current *CookBook* chapters and most of the *AIPS* Memo series are available via the World-Wide Web and are distributed with every release of *AIPS*.

A nostalgic article about *AIPS* would not be complete without reference to the fun we — and others — have had with the name. The first *AIPSLetter* to have an image of an ape appeared in January 1983. That image required special arrangements to be made for enough disk space (say 10 Megabytes) and took literally hours to compute and print using home-brewed dithering software and a dot matrix printer. It was so expensive that we did not do another until April 1985. The third image to be used to fill the mailing sheet then appeared with the April 1986 *AIPSLetter*. These three appear in a montage labeled Figure 1. The title page of the September 1983 *CookBook* was also decorated with a grey-scale ape. This ape got further publicity in a popular article on the VLA, reproduced in part as Figure 2. The outside covers of the *CookBook* and *Going AIPS* gave NRAO's graphic artist Pat Smiley an opportunity to display her talents, reproduced in black and white as Figure 3. And, of course, the *CookBook* would not be complete without proper recipes such as the earliest ones reproduced as Figure 4.

The first *AIPSLetter* listed the *AIPS* group (with a *) and supporting cast as:

Al Braun	VLA		DEC/NET and systems work
David Brown	CV	*	VAX/ModComp systems, *AIPS* on the IBM
Bob Burns	CV		Overall NRAO computer capability
Tim Cornwell	VLA		VLA VAX manager/friend
Bill Cotton	CV	*	U-V software, liaison with VLBI
Ron Ekers	VLA		Overall *AIPS* priorities
Gary Fickling	CV	*	VAX system, installation, general software
Ed Fomalont	CV		*AIPS* Project Manager, *AIPS* priorities
Eric Greisen	CV	*	Software manager
Kerry Hilldrup	CV	*	IBM and general user support
Arnold Rots	VLA		VLA/*AIPS* spectral-line coordinator
Fred Schwab	CV	*	Applied mathematics
Don Wells	CV	*	Measuring engine, liaison with optical

By this time, Walter Jaffe, who had contributed substantially to the early design, had already left for a year in Holland. In preparation for that trip he suggested and helped to code the pseudo-array processor, a software emulation of the hardware for those who could not afford the real thing. That emulation is now the only "AP" anyone has. A number of the people listed above still work for NRAO, but I am the sole survivor still — or again — working in the *AIPS* group. A longer list of *AIPS* participants prepared in 1988 is given in Figure 5.

The first *AIPSLetter* listed 23 institutions that had received *AIPS* tapes. By May 1983, a similar list included 50 sites outside of NRAO. The July 1982 *AIPSLetter* had a variety of interesting quotes from outside users on the costs of running and keeping up with *AIPS*. Among them were:

> "At present our *AIPS* is at a standstill because we have, for the moment, run out of money. This is a result of both the high charges made by the U of M computer center and the considerable

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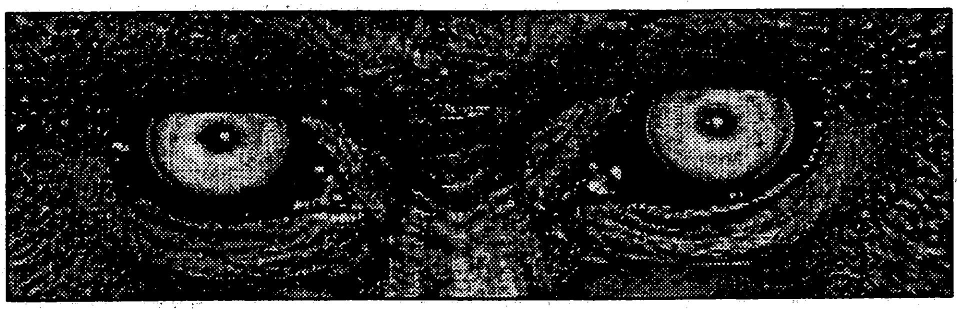
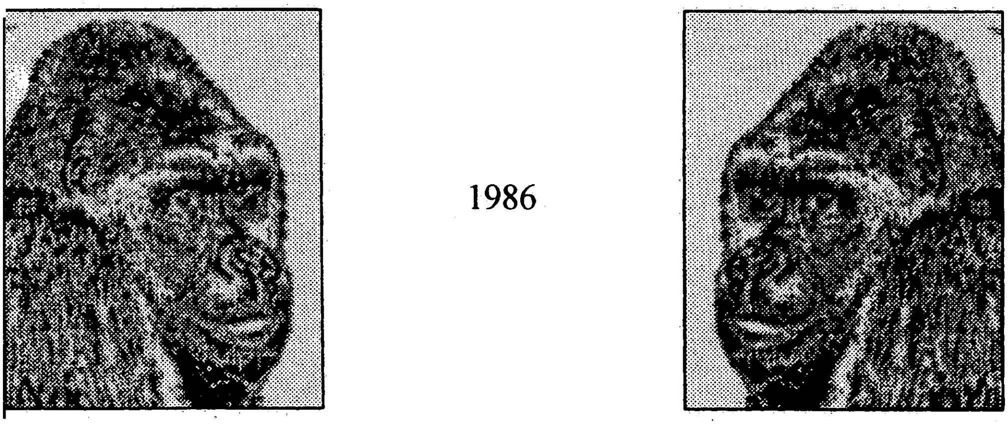
[11] `http://www.cv.nrao.edu/aips.`

[12] Greisen, E. W., *AIPS* Memo No. 27, "Non-Linear Coordinate Systems in *AIPS*," November, 1983.

1983



1985



1986

Figure 1: Mailing-page artwork for early *AIPSLetters*.

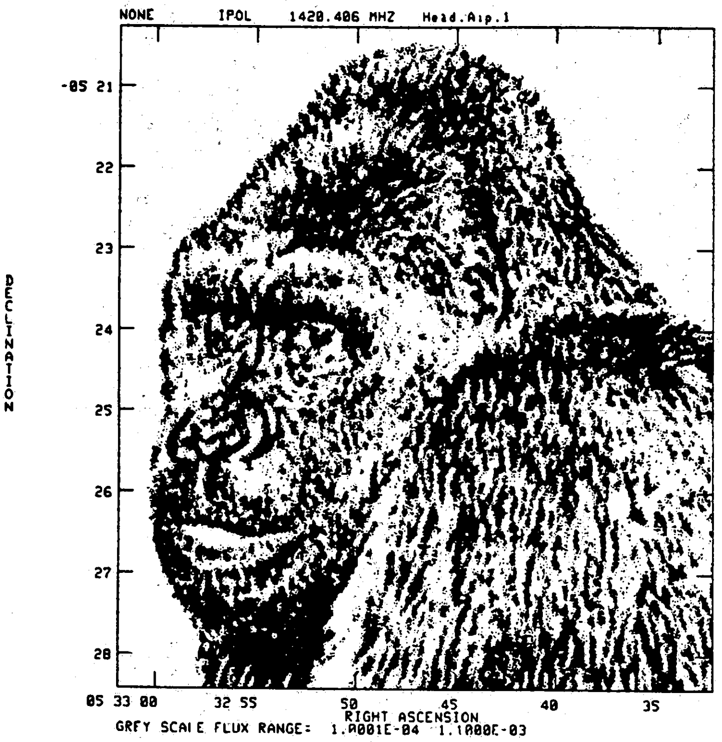


Fig. 11. — « Gorille radioélectrique »(!)

Cette figure illustre à la fois le résultat d'une des formes de traitement d'images VLA, et. le programme utilisé pour y parvenir : « AIPS » (*apes* = singes en anglais)

(Document NRAO)

Pour traiter l'image, c'est-à-dire la nettoyer, mais aussi la modifier à volonté en fonction de certains objectifs, en extraire des chiffres comme des flux ou des positions, etc., un programme spécial, AIPS (Astronomical Image Processing System) a été mis au point. (Ce qui donne lieu à d'amusants jeux de mots, AIPS se prononçant « *apes* », c'est-à-dire « singes » en anglais : la salle de traitement d'images du NRAO à Charlottesville, en Virginie, est communément appelée « AIPS Cage », la « cage aux singes » !) (fig. 11). Avec l'ordinateur approprié, AIPS permet de traiter une image n'importe où; de fait, plusieurs observatoires (dont l'Institut de Radioastronomie Millimétrique à Grenoble et l'Institut d'Astrophysique de Paris) en disposent. Déjà, les observations elles-mêmes peuvent se faire « in absentia »; le temps n'est plus loin où une observation au VLA pourra se faire pratiquement sans quitter son bureau habituel!

Figure 2: Page from Montmerle, T., 1985, "Le Plus Grande Radiotelescope du Monde: Le 'Very Large Array'," l'Astronomie, 99, 487.

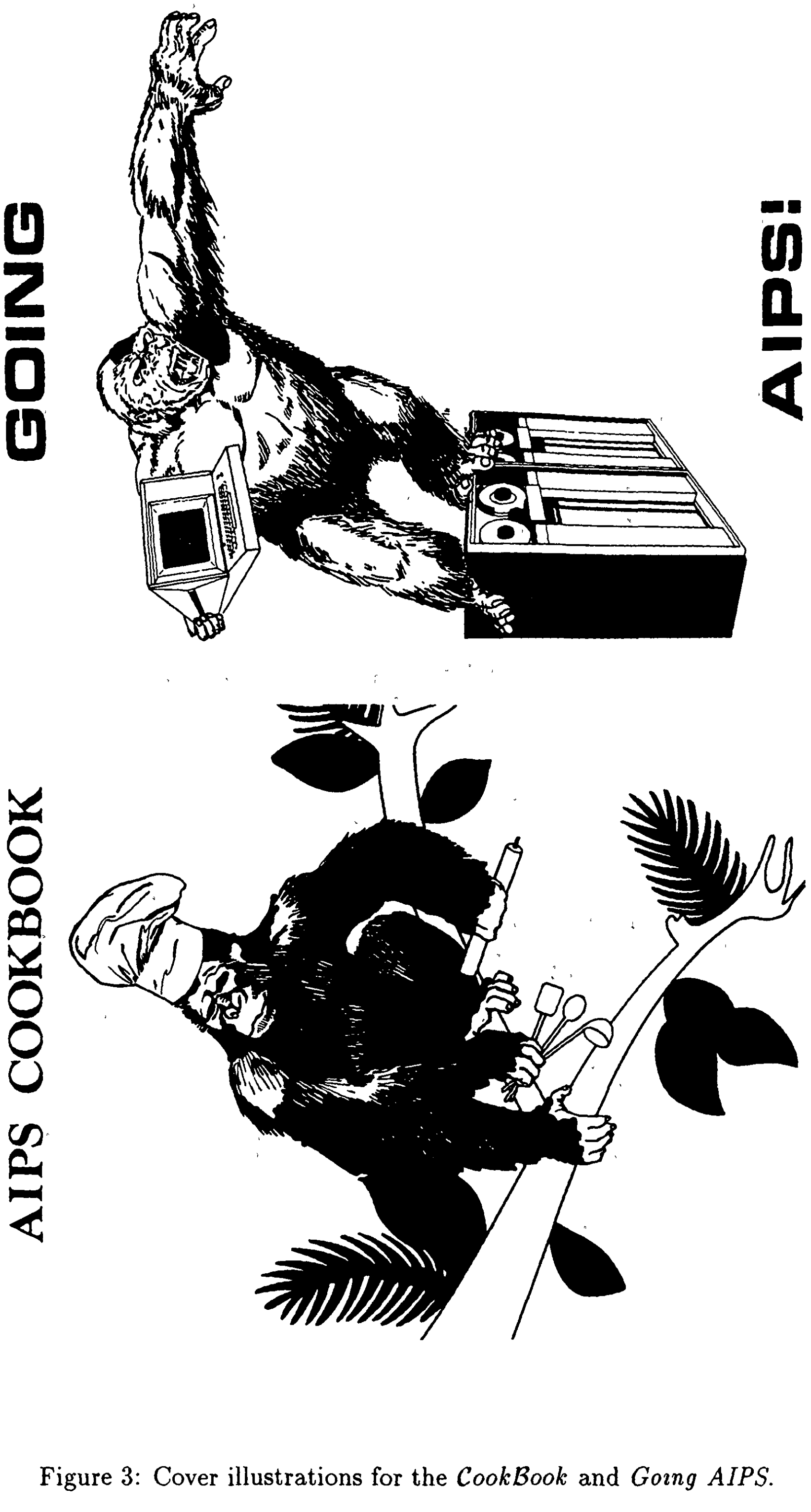


Figure 3: Cover illustrations for the *CookBook* and *Going AIPS*.

2.6. Banana daiquiri

1. Combine in an electric blender: 2 oz. **light rum**, 0.5 oz. **banana liqueur**, 0.5 oz. **lime juice**, 1/2 small **banana** peeled and coarsely chopped, and 1/2 cup crushed **ice**.
2. Blend at high speed until smooth.
3. Pour into large saucer champagne (or similar) glass. Serves one.

4.7. Hot banana soufflé

1. Preheat oven to 375deg.
2. Select a 6-cup souffle dish or other mold and grease it liberally with 1 tablespoon **butter**.
3. Place 6 **eggs**, 1/2 cup **cream**, juice of 1/2 **lemon**, 1 tablespoon **kirsch**, and 1/4 cup **sugar** in blender. Blend until the batter is smooth.
4. Peel 2 large **bananas**, removing any fibers and break into chunks. With blender running, add the chunks one at a time.
5. Break 11 ounces **cream cheese** into chunks and add them to the blender.
6. When all the ingredients are thoroughly mixed, run the blender at high speed for a few seconds.
7. Pour batter into prepared dish and place it in the hot oven. Bake 45–50 minutes until the top is lightly browned and puffy. You may quit when the center is still a bit soft or continue baking until the center is firm.
8. Serve at once. A whipped cream flavored with Grand Marnier makes a nice topping.

5.4. Bananes rôties

1. Preheat oven to 375 deg.
2. Place 6 (peeled) **bananas** in a baking dish.
3. Sprinkle bananas with juice of 1/2 **lemon**.
4. Pour 2 tablespoons melted **butter** and 2 tablespoons **dark rum** over the bananas. Sprinkle with 2 tablespoons **brown sugar**.
5. Place in oven for 10 minutes.
6. Pour on 2 more tablespoons **melted butter** and 2 more tablespoons **dark rum** and bake for 5 minutes more.
7. Serve at once, spooning some sauce over each banana.

8.4. Golden mousse

1. Combine 1 cup mashed ripe **bananas**, 2 tablespoons **orange juice**, 1/4 cup shredded **coconut**, 3 tablespoons **brown sugar**, a few grains **salt**, and 1/8 teaspoon grated **orange rind**.
2. Whip until stiff 1 cup **heavy cream**.
3. Fold whipped cream into fruit mixture and turn into freezing tray. Freeze rapidly without stirring until firm.

Figure 4: Chapter ending recipes from the September 1983 *CookBook*.

current AIPS group

Ernie Allen	tape and documentation distribution
Bill Cotton	calibration and imaging software, VLB
Phil Diamond	spectral-line software, VLB
Eric Greisen	project design and management, general applications
Kerry Hilldrup	UNIX and Cray systems, Z routines
Nancy Wiener	Gripes, documentation, general assistance

former AIPS group

David Brown	VMS and ModComp systems
Tom Cram	initial design discussions
Gary Fickling	VMS systems, applications software
Ed Fomalont	scientific advisor, applications software
Walter Jaffe	applications and basic software
Thad Polk	geometric corrections software
Gustaf van Moorsel	spectral-line analysis software
Don Wells	management and software design advisor

advisors

Alan Bridle	scientific friend and advisor
Bob Burns	management advisor, Head NRAO Computer Division
Ron Ekers	scientific and management advisor

software assistance

John Benson	VLB software
Stuart Button	early general applications
Tim Cornwell	mosaicing and maximum entropy tasks
Bob Duquet	super-computer port
David Garrett	preliminary UNIX implementation
Brian Glendenning	SUN image display routines
Jerry Hudson	POPS language
Neil Killeen	image analysis tasks
Pat Moore	VLA AIPS manager
Arnold Rots	TV display applications
Fred Schwab	self-calibration and other mathematical tasks

Figure 5: AIPS participants list circa 1988 from AIPS Memo No. 61.

> demands placed by *AIPS* on any system. To give a few examples, the cost for storing the executable *AIPS* modules, HELPs, and INPUTS files and a catalog of 15 maps on the disk is in excess of $100 per week. The test runs of APCLN cost about $25 each... The difficulty of running *AIPS* under these conditions only serves to underscore the need for a dedicated Astronomy Department computer, a point we have of course been making to NSF for years."[13]
>
> "I had hoped that updates would be possible through phone links, but the rate at which code is being modified makes this impractical. In the time from 31 October 1981 to 1 January 1982 more than 5000 blocks of code were modified. Even with 1200 baud line this represents about 6 hours to transmit. At regular long distance rates this is about $200. The link is run by a routine similar to VAXNET and is not totally free from parity errors and dropped characters. The error rate transmitting that much code could be a problem. Clearly tape transport is the most economical way to do a full update. I have used the link to get specific tasks for which we wanted an update as quickly as possible."[14]

AIPS sites were surveyed nearly every year from 1985 through 1990 to determine how *AIPS* was used. We have not continued the survey since then because desktop workstations are not amenable to measurements of the "fraction of time devoted to *AIPS* processing" that formed the basis of the earlier surveys' results. In the 1985 to 1990 period, the number of active *AIPS* computers rose from 54 to 345.[15] More importantly, the total computing power running *AIPS* full time went from 9.1 to 164.7 in units of VAX 11/780s with array processor. The fraction of that power outside the NRAO went from 51% to 86%. These numbers are a clear measure of the success of the software portability strategy. (Modern users may wish to note that a VAX 11/780 with array processor was very approximately 0.3 AIPSMarks and that modern PCs have been measured to have performances around 10 AIPSMarks, close to a Cray X-MP/4 which used to cost in excess of $10 million.)[16]

The *AIPS* group, primarily because it needed all the computing power it could get, kept abreast of developments in computer hardware and attempted to make *AIPS* available to run on it. This began with the VAX at the beginning of 1980. A port of *AIPS* to IBM mainframes under OS was begun late in 1981, helped us find numerous problems in the code, and was even partly successful by July 1982. That was abandoned in September in favor of a port to a Unix operating system provided by Amdahl for IBM mainframes. *AIPS*' dependency on correct Fortran compilers was soon apparent, as were layers upon layers of bugs in Amdahl's compiler. These were not solved, and the port declared successful, until June 1984. David Garrett of the University of Texas, beginning in May 1982, had also ported *AIPS* to run under Unix but on a VAX 11/780. These were but two of many flavors of Unix which forced us to have many versions of some of the Z routines. As a result, it took another year for the Unix versions to be merged, to be regarded as reasonably "standard," and to be shipped to a variety of sites. The surveys mentioned above measured the change from dependency on VMS in 1985 to a preponderance of Unix systems, at least as measured by computing power in 1990. We also went after "big iron" as the supercomputers were known. These vector computers were interesting because the pseudo-AP (array processor emulation) routines, which had been in *AIPS* since 1981, were readily adapted to, and highly vectorized by, big-iron compilers.[17] A port to the Cray-1 of the Minnesota Supercomputer Institute was in progress by September 1984, worked on in part by Bob Garwood who is now at NRAO. Time on the Cray X-MP at Digital Productions in Los Angeles was made available to the NRAO under an NSF supercomputer initiative. Bob Duquet and Kerry Hilldrup began work on this project in early 1985 and, by the October 1985 *AIPSLetter*, it was considered functional. The cpu times achieved were 15–50 times better than a VAX 11/780 with array processor, but the *real* times taken to run the programs were distressingly long (about the same as the VAX plus AP). Fortunately, in

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[13] Frank Ghigo, University of Minnesota.

[14] Stuart Button, University of Toronto.

[15] Bridle, A. and Wiener, N., *AIPS* Memo No. 59, "The 1988 *AIPS* Site Survey," March, 1989 and Bridle, A. and Nance, J.., *AIPS* Memo No. 70, "The 1990 *AIPS* Site Survey," April, 1991.

[16] Greisen, E. W. *AIPS* Memo No. 85, "DDT Revised and AIPSMark Measurements," February 1994.

[17] Wells, D. C., Cotton, W. D., *AIPS* Memo No. 33, "Gridding Synthesis Data on Vector Machines," January 1985. See also Wells, D. C., *AIPS* Memo No. 47, "Installing NRAO's *AIPS* on vector Computers," June, 1985.

1985, both Convex and Alliant Computer Corporations announced vector or vector/parallel computers that were a lot cheaper to buy and to operate than Crays. We tested these computers in 1985[18] and bought one for Charlottesville's Christmas 1985 to replace the IBM. A second Convex C-1 was obtained for the VLA in January 1987. These were very powerful computers, but they were still a central, shared machine with all the attendant troubles related to inadequate disk space, sign-up sheets, and the like. The breakthrough to the modern era of computers on everyone's desk began when *AIPS* was ported to a Sun-3 in Princeton in October 1986. We found that our user community was reluctant to trust this port, so Don Wells persuaded Sun to loan me a Sun 3/110 for my desk. This was put to work late in 1987, developing the final parts of the Sun Screen Server implementation of an *AIPS* TV display written by Brian Glendenning, then of the University of Toronto. By the time of the code overhaul (see below), the Sun workstation was regarded as the best platform on which to do the initial debugging of a full code rewrite.

All of the computer testing and evaluation done from 1985 to the present at NRAO has depended on a certification and benchmarking suite developed initially by Don Wells. This suite is implemented in *AIPS* procedures written in `RUN` files to execute a sequence of *AIPS* tasks on standard data sets, comparing the results with previously computed answers. This suite, called `DDT`,[19] was first described in 1985[20] and has been the subject (or tool) of numerous Memos thereafter.[21] `DDT` enables us to insure that the principal tasks run correctly on new computers and new versions of *AIPS* and to measure the performance of a computer as a typical *AIPS* user would see it.

The *AIPS* group also tried to stay abreast of developments in computer networking. We actually had a computer network established between our ModComp Classic and VAX 11/780 in Charlottesville. It was used to copy text files back and forth and was faster than magnetic tape, but only when the ModComp would not get tired of waiting for the VAX's slow operating system. There were a number of experiments with DECNET and, eventually, a link was established between Charlottesville and the VLA using a leased phone line. At that point we were finally able to keep the code in New Mexico current with the code in Charlottesville. Electronic mail has become important in the project. The first e-mail address announced for the group in October 1985 was `nancy%cvax%deimoscaltech.bitnet` or `cit-hamlet.arpa`. This used a dedicated phone line from CalTech to NRAO in Tucson which was rented to support the work at Digital Productions. We finally wrote an article in the *AIPSLetter* in July 1986 describing our e-mail connectivity through four different networks. In June 1987, we announced "exploding bananas," an e-mail forwarding system that would allow subscribers to receive all e-mail discussions of topics affecting *AIPS*. By now, most of our users receive their *AIPSLetter* and even their copies of the *AIPS* source code and binaries directly through the Internet and World-Wide Web.

Snapshots of the *AIPS* code were given to users whenever they asked until September 1982. At that time, we introduced the concept of frozen releases named, *e.g.*, `15OCT82`, which would be shipped to non-NRAO sites. Releases were done every 2 months until July 1984 when the schedule became every 3 months. Beginning in April 1985, we introduced the concept of `OLD` (shipped), `NEW` (bug fixes only), and `TST` (active development) versions, with a "midnight job" that kept the VLA copies of *AIPS* current with those in Charlottesville. The *AIPS* code remained in the complicated Fortran 66 dialect discussed above until a "code overhaul." This overhaul was announced in April 1987, begun in July 1988 (after a serious reorganization of the `Z` routines), and released as the `15OCT89` version. The overhaul was begun with a powerful text transformation program written by Bill Cotton and driven by a long symbolic list of the transformations desired. Unfortunately, the output of this program still required manual intervention to convert the code to our new standards of function and legibility. The result, however, was code in ANSI-standard Fortran 77 supported by an *AIPS* pre-processor program to allow `INCLUDE` files and `HOLLERITH` variables which are not supported by all compilers.

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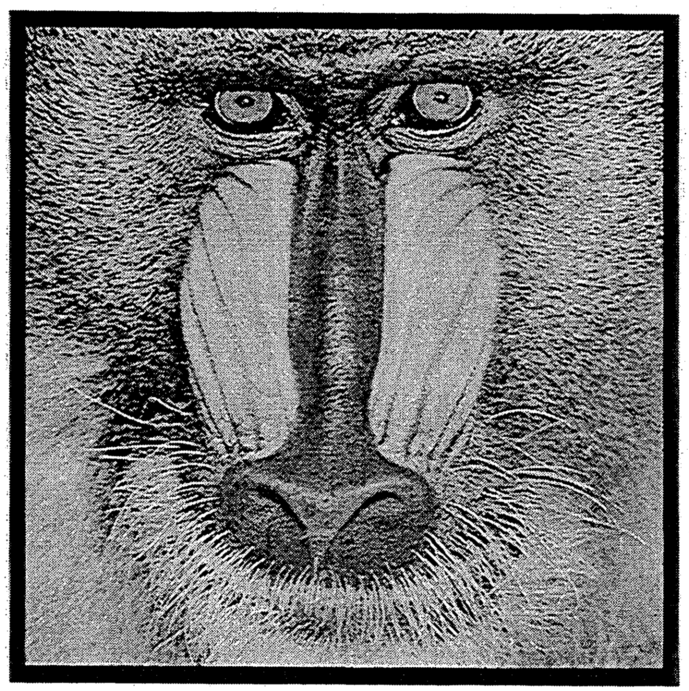
[18] Hilldrup, K. C., Wells, D. C., Cotton, W. D., *AIPS* Memo No. 38, "Certification and Benchmarking of *AIPS* on the Convex C-1 and Alliant FX/8," November 1985.

[19] A typical *AIPS* play on words referring to the "dirty dozen" *AIPS* tasks used, the bug killing aspects of the insecticide, and the macho endurance of the characters in the movie by that name.

[20] Wells, D. C., Fickling, G. A., Cotton, W. D., *AIPS* Memo No. 36, "Certification and Benchmarking of *AIPS* on the VAX-8600," June 1985.

[21] See in particular Langston, G., Murphy, P., Schlemmer, D., *AIPS* Memo No. 73, "*AIPS* DDT History," May, 1991.

AIPS was originally conceived as a "map-processing" package to read in *uv* data only for the purpose of doing the gridding and Fourier transformation. The original code handled continuum images and read *uv* data from Export-format tapes directly into the imaging software, primarily **APMAP**. However, by the middle of 1981, the desire to do self-calibration on VLA data and to begin to do some processing of VLBI data caused the project to develop a format and input/output routines for *uv* data on disk and a variety of tasks to handle these data. Tasks such as **UVLOD**, **UVFLG**, **UVMAP**, and **UVSUB** date from these early days. At the same time, the map input/output routines were revised to handle multi-dimensional images and tasks like **MCUBE** and **TRANS** were written. VLBI applications began appearing in March 1982 with a full global fringe-fitting task (**VBFIT**) in May 1982.[22] User competition for scarce resources led to accounting and **TIMDEST** (automatic deletion of old files) in January 1982, lock files for tape and display devices in May 1982, a roller for array processor tasks in September 1982, a full queueing algorithm for these tasks in September 1983, and task **NOBAT** in March 1984. This last task did not actually use the AP, but it allowed a higher-priority user to block lower-priority users from the device. *AIPS*' high standards for handling celestial and other coordinates began in May 1983. The first openly interactive task, **XGAUS**, appeared in September 1983. The multi-field, multi-channel, *uv*-data-based imaging and Cleaning task **MX** first appeared in November 1983. This "battery-powered Clean" algorithm was developed by Bill Cotton and Fred Schwab as an enhancement of the Clark Clean algorithm. The Fourier transform of the Clean-component model is subtracted from the *uv* data and the residual data are re-gridded and transformed. In this way, problems related to gridding and aliasing are sharply reduced and a larger field of view may be used. A calibration package for *AIPS* was begun in July 1984, but did not appear until the **15JAN87** release. The package was limited initially to continuum calibrations; polarization calibration appeared in **15OCT87** and spectral-line bandpass calibration arrived with **15JAN88**. The task **TVFLG** appeared in October 1987 to allow users to edit data interactively with the TV display. The calibration package did not get much use outside of Charlottesville until after the code overhaul and the decommissioning of the VLA's DEC-10. At that point, *AIPS* development and politics definitely became "interesting," but that is the subject of another manuscript, perhaps one entitled "*AIPS* in its Later Years or Freon is in Short Supply."



Title page illustration for the 15APR98 *CookBook*.

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[22] Schwab, F. R., VLBA Memo No. 82, "Global Fringe Search Techniques for VLBI," April 1982.

The Very Large Array: Imaging and Self-Calibration

Tim Cornwell (NRAO)
Ron Ekers (ATNF)

Introduction

Both of us worked alongside Barry for a substantial time at the Very Large Array, Ron as Director and Tim in various roles, starting as a post-doc. During this time, Barry was responsible for the on-line computing, at times for the off-line computing, and always for the scheduling and testing of the VLA. It's worth emphasizing just how much Barry was at the heart of the VLA. He had been a key person responsible for the Green Bank Interferometer, but at the VLA, he was quite dominant. Above all others, he understood how the entire system worked, and could make contributions in literally any area. Here we give our view of the development of imaging and self-calibration for the VLA, a topic in which Barry (of course!) played a vital role on a number of occasions.

Performance Design Goals:

The VLA outgrew its original performance specifications by large factors. It is interesting to compare the 1967 specs with the performance as the telescope was officially commissioned in 1980. Table I shows this comparison for a number of vital attributes. That the performance improved over the design and construction phase is testament to a philosophy of continual improvement of designs that seems well embedded in NRAO. Perhaps even more surprising and gratifying is the tremendous continuing improvement *after* commissioning. Table II shows the multiplicative improvement in some of these attributes from 1982 to 1986 (summarized in a document by Ron Ekers on the first seven years of operation of the VLA). Taking the two most impressive numbers from Table II (for dynamic range and map size), we see an aggregate improvement of about 2 million. How did this come about? To find out, we need to look at the development of imaging techniques during this time.

	Goal (1967)	Achieved (1980)
Resolution	1"	0.1"
Sensitivity	0.1 - 1 mJy/beam	0.050 mJy/beam
Sidelobes	-20 dB	-30 dB
Field of View	1' to 10'	1' to 30'
Declination range	-20 to +90	-40 to +90
Speed	3 images/day	100 images/day
Spectral Line	Not to design out	256 channels
Map Size	~100x100 pixels	512x256 (routine) 4096x4096 (max)

Table I: Performance design goals for the Very Large Array

	1982	1986
Resolution	10	25
Sensitivity	2	10
Sidelobes	10	1200
Field of View	3	9
Wavelengths	2	3
Speed	30	60
Map Size	25	1600

Table II: Performance Improvement (1982-1986) relative to 1967 goals

The Pre-VLA Era

In the fifties and sixties, there were two main approaches to determining radio source structure from synthesis measurements: model-fitting or complete Fourier synthesis. The latter was preferred, of course, and arrays were designed to measure all Fourier components up to some maximum resolution. A Fourier inverse, which often strained the computing resources available, then gave an image of the object. Weighting techniques were used to improve the sidelobes but the basic ethic was to measure all the Fourier components. One might call this the Cambridge-Sydney-WSRT axis, though other notable telescopes did the same thing. The realm of model-fitting was inhabited by those unfortunate souls for whom complete Fourier coverage was impossible. Mainly this meant those doing Long Baseline and Very Long Baseline Interferometry (for example, Jodrell Bank, OVRO, the Parkes variable baseline interferometer, and the Green Bank Interferometer). The push to high resolution to determine, *e.g.*, the nature of radio stars (later found to be radio quasars and active galactic nuclei) led to long, single baselines from which only simple models could be derived. Actually, given a few "single" baselines, surprisingly good models could be derived.

In the late sixties and early seventies, various people at WSRT began to investigate methods of improving images when complete Fourier coverage was sacrificed for shorter observing time. In lots of cases, the sky was mainly empty but the sidelobes from those sources present could spread over the whole image. After a number of false starts, Jan Högbom (in collaboration with Ekers, Schwarz, and Rogstad) invented the CLEAN algorithm (published in 1974), and the whole game changed. One could then anticipate true imaging by sparse arrays.

Working from the other end: the LBI and VLBI crowd, mainly in the U.S., at Caltech and MIT, began to think about the calibration problem. Given unstable VLBI arrays, how could one make an image? Well, the answer was to use Roger Jennison's wonderful invention of the closure phase (Jennison, 1958), a story that has been told elsewhere. Although quite a few people were working on the same basic idea, Tony Readhead and Peter Wilkinson published the first papers (Readhead and Wilkinson, 1978) demonstrating just how good an image made from a sparse, unstable synthesis array could be.

Enter the VLA

So the VLA came along at a very propitious time from the point of view of imaging. A number of developments were there to be exploited using a superb instrument. We now encapsulate the story of these years in a number of key developments:

VLA approved: (1972)

The construction of the VLA was approved by the U.S. Congress and design and construction started in earnest.

Högbom Clean algorithm published: (1974)

Jan Högbom's work over the previous years culminated in a very influential paper. The algorithm was simple, intuitive, and could be programmed easily. It is interesting to speculate that had this development occurred earlier, the VLA design goals could have been meet with many fewer antennas and we would have had a much less powerful instrument today!

MEM applied to radio images: (1976)

Steve Wernecke and Larry D'Addario (1976) published the first radio image processed using a Maximum Entropy method. Later work by Steve Gull and Geoff Daniell (1978) described a more widely used algorithm (and philosophy). Now Clean had a competitor.

Contract for an Optical Processor: (1976)

It was clear that the processing of spectral-line VLA data would strain existing computers. A contract for an optical processor was issued at that time. This would accomplish imaging from visibility data by using optical transforms from specially constructed masks.

Pipeline proposed, Optical processor cancelled: (1978)

Barry Clark proposed that in place of the optical processor, a specially designed and constructed digital processor, could process VLA data (especially spectral line data) in a pipeline. Data would be sorted in real-time by one PDP 11 (with attached array processor) and transformed using another PDP 11 (with array processor and special memory unit for the transpose operation). The pipeline was approved and started, and the optical processor was cancelled. Initially Barry Clark, with assistance from Bob Duquet, tackled the whole pipeline project but it proved more difficult than expected. Finally, the two big men in computational radio interferometry, Barry Clark and Wim Brouw, succeeded after splitting the problem between them. Getting the pipeline to work reliably generated much angst for numerous people. Although the calculational aspect was doable, the networking required was at the so-called bleeding edge, and various people shed blood! Eventually, by the valiant efforts of a few very determined people, most particularly Bob Payne, Jacqueline van Gorkom, Miller Goss and Ed Fomalont, the system became both usable and used, and bore the brunt of spectral line imaging until Moore's Law made general purpose computers feasible for spectral line imaging. This happened in the mid-late eighties and so the pipeline brought a few years of spectral line astronomy that would otherwise not have been possible. It also provided an important performance benchmark for AIPS to match.

Self-calibration algorithm used: (1979)

Barry Clark noted that calibrating a 27-element array should clearly be done per antenna, rather than per interferometer (351) as had been previous practice: it saves computer words (precious then), and over-determination helps beat down the noise. So the VLA was designed to calibrate per antenna. A serendipitous side-effect is that if the calibrator is not actually a point source, then the self-calibration procedure averages down the discrepancies anyway. This principle then leads to self-calibration, whereby a model of the source is divided out for calibration purposes and iteratively improved in an imaging step. Later, the equivalence of this procedure to the use of Jennision's closure phase was shown.

Clark Clean algorithm implemented: (1980)

In the early days of the VLA, Clean was reportedly a somewhat deprecated procedure.

Nevertheless, a version was available and soon began to chew up computer time on the Dec 10 "supercomputer" then being used. About this time, the FPS array processor AP120B was coming into use. It did linear algebra and Fast Fourier Transforms spectacularly fast. Barry came up with a variant of the Clean algorithm (Clark, 1980) to exploit the AP120B: he split the Clean into two parts, one that cleaned only the brightest points, and one that subtracted sidelobes for these points using a convolution computed using an FFT. The AP120B code that Barry wrote for the first step was handed on from person to person with few people understanding it in its entirety. The speed improvement was from factors of a few to more than an order of magnitude. Fred Schwab and Bill Cotton would later improve this further by adding a step of going back to the original visibilities for the subtraction phase.

Cray supercomputer deconvolves Cas A image: (1984)

Perhaps the culmination of this short dizzying spell was the deconvolution of a multi-configuration VLA synthesis of Cas A, using a Cray Y-MP computer at a film studio in Los Angeles. This resulted in a spectacular 2048 by 2048 pixel image, which is still breathtaking to look at. Comparing the VLA image of Cas A to the original expectations for the image of such a source, one can see the vast improvement in the science allowed by the improved imaging capability. As one example, one could now follow over time the trajectories and brightnesses of the faint knots from ejecta in front of the main shell.

Subsequent developments in imaging have expanded the scientific capabilities (for example, mosaicing, wide-field imaging, multi-frequency synthesis, fringe fitting, etc).

Looking back over the developments described above, we can see some missteps. Probably the pipeline was an over-reach, but development of the optical processor would have been a much bigger mistake as it would never have reached the dynamic range finally achieved with the VLA. It certainly was a dead-end developmentally since the complexity of the environment was only mastered by a few people. The advent of the AIPS package gave the then-nascent imaging techniques a place to grow and a mechanism to spread to other telescopes. For example, the self-calibration and cleaning tasks, ASCAL and MX, were vital to many users of the VLA (and other telescopes), and could be used at a home institution if a VAX with an AP-120B (to run Barry's Clean algorithm) was available. In retrospect, the early mismatch between computer capacity and data output from the VLA was correct. Computer power increased rapidly following

Moore's Law and was well matched to the VLA a decade later. Only now, two decades after the VLA was first operational, do we have data-starved computers and an obvious case for a VLA backend upgrade.

VLA Science

So how did this tremendous improvement in capabilities affect the science that was performed with the VLA? The original proposal focused on observing radio galaxies and quasars at optical resolution and using them for cosmology. It was noted that some other areas might also benefit: planets, galactic studies, and the 21-cm hydrogen line. For comparison, the observing statistics for the first eleven years (1980-1991) are:

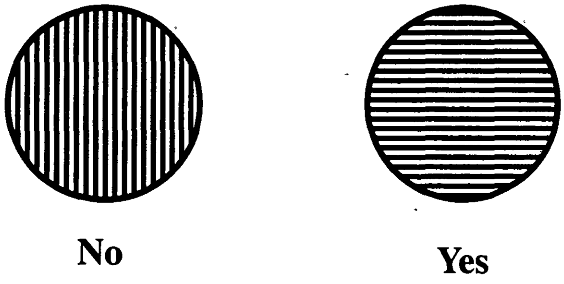
stars (16%)
galaxies (14%)
radio galaxies (13%)
quasars (9%)
star formation (9%)
solar system (6%)
AGN (5%)
supernovae (4%)
interstellar medium (4%)
cosmology (4%)
molecules (3%)
galactic center (3%)
VLBI (3%)
pulsars (2%)
X-ray, etc. (1%)
astrometry (1%)

The original objectives comprise a quarter of the observing and many new unforseen areas opened up. In expanding the range of science, the imaging-driven improvements in sensitivity, dynamic range, and image size have been particularly important. The addition of spectral-line and the expansion of wavelength coverage have been the key hardware changes. The huge improvement in imaging speed also has been vital in increasing the number of projects on the telescope, and in enabling the new era of VLA surveys such as FIRST and NVSS.

The Clark Display

Images need display. That much is clear. Surprisingly for someone who has devoted his life to synthesis radio interferometry, Barry is not particularly interested in image display. In the early VLA days, he thought a line printer was already excessive as an output device and images were only for those who couldn't clearly pose a problem. We cannot resist ending with a diagram of the ideal Clarkean display unit. This gives the ultimate answer to any observational project. In a characteristically terse manner:

The Clark Display Unit



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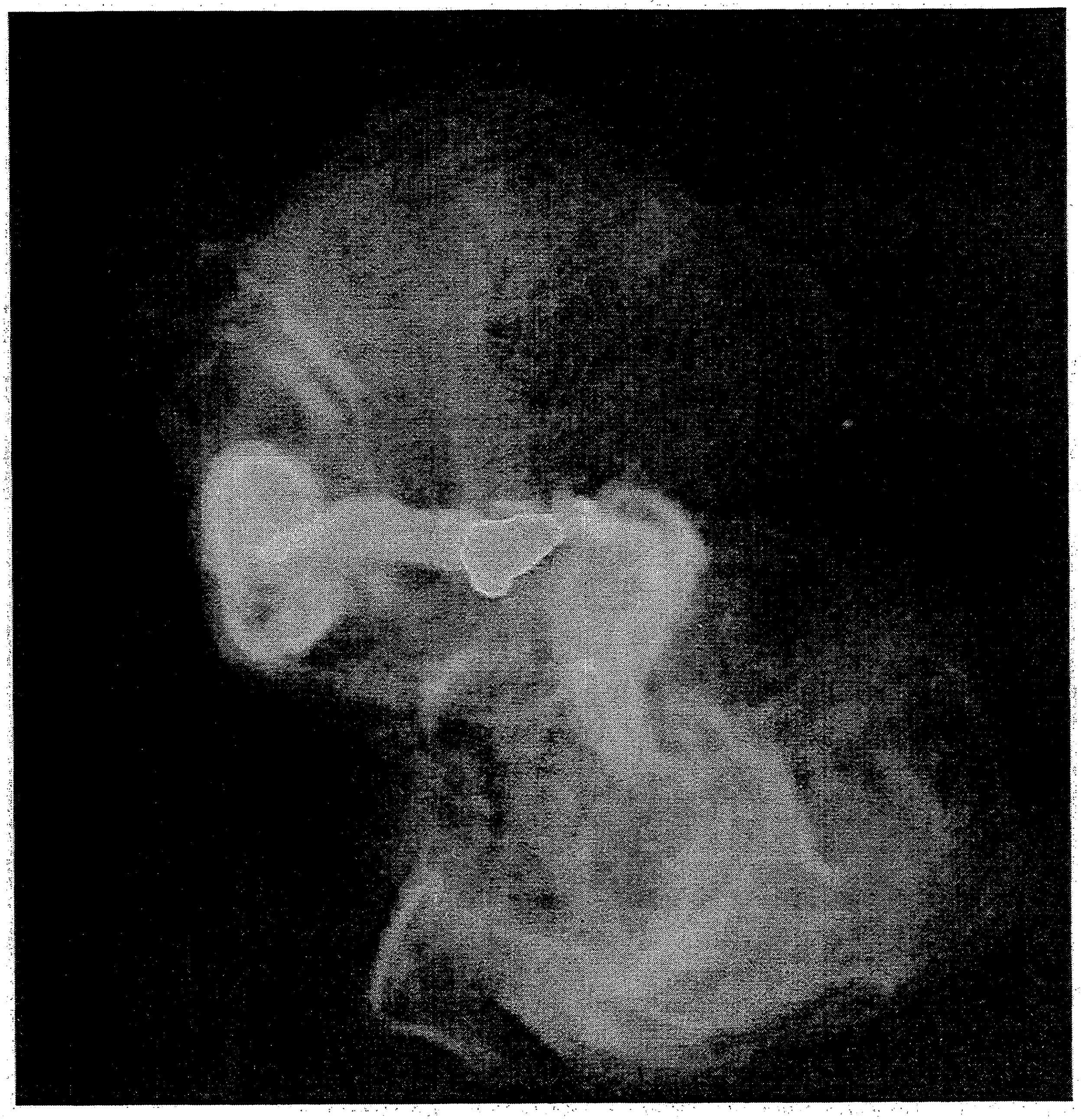
Section III: Impact of the VLA



M87, observed at 90 cm wavelength with the VLA; Large-scale structures revealed by this image are approximately 200,000 light-years across. (F.N. Owen, J.A. Eilek and N.E. Kassim, NRAO/AUI)

Planetary Radio Astronomy

Glenn L. Berge

I feel privileged to take part in this tribute and birthday celebration for Barry Clark. I've always had a great deal of respect and personal regard for Barry. I first met him when I showed up at Caltech as a graduate student. He was only a year ahead of me, but it took a long time before I realized that. He seemed like such a veteran; this was partly because he had been a Caltech undergraduate and knew all the ropes, and also he was smarter than most people.

To me, Barry has always seemed to be an interesting combination of finesse and brute force, with the insight to know which is appropriate for the situation at hand. One exception might be his 250-mile commute from Caltech to the Owens Valley Observatory on his Vespa motor scooter, as Bob Wilson has described. I recall a different instance when John Bolton received a phone call at the observatory and headed out driving south to rescue Barry and the disabled Vespa.

Many people may think that Barry is a man of few words, and they are correct unless they conclude that he has a dislike of words. On the contrary, I think that he has a great love of words and of correct grammar and that his parsimonious use of words is due to a desire for precision and a dislike of squandering good words needlessly.

He was the heaviest user of the dictionary in the OVRO library in those student days. Still, his knowledge of vocabulary left a few gaps that he filled on his own. My student colleagues and I developed a glossary of Barry's terms. It was simple because there were only three: umph, grumph and murmph, in order of increasing annoyance.

I've found that Barry is always helpful in answering technical and scientific questions. To utilize this resource, however, you must construct the question accurately. This, in itself, solves the problem half the time. Then, you must be prepared to listen carefully to every word and analyze the answer with care. What may sound like a casual brush-off is probably quite profound.

Now I should begin to approach my subject of planetary radio astronomy. It probably is not known widely anymore that Barry was a pioneer in high-resolution radio measurements of Venus. This was in addition to his thesis work and other activities while a graduate student. Barry and Chuck Spencer observed Venus with the Owens Valley interferometer near the inferior conjunction of late 1962 (Clark and Spencer, 1964). With short baselines they determined the disk temperature at 10.7, 18, and 21 cm wavelength. Then they made some highly resolved measurements at 10.7 cm in the equatorial direction using spacings from 55 percent to almost 100 percent of the spacing predicted for the first visibility zero of a uniform disk. Sensitivity was a problem, and beyond 75 percent of the spacing to the first zero, only upper limits were reported. This could be regarded as a feasibility study.

Near the subsequent inferior conjunction in mid-1964, Barry worked with a visitor from the Soviet Union, A. D. Kuz'min, to observe Venus at 10.6 cm with high resolution much more accurately and comprehensively than was done in the 1962 work. (Clark and Kuz'min, 1965). This time the baselines extended beyond the visibility zero for a uniformly bright disk the size of Venus on out to the second maximum. To see whether the emission arose from a dielectric sphere (solid surface rather than atmosphere), the observations were made with linear polarization in the extreme opposite cases; that is, with the electric vector parallel and perpendicular to the projected baseline.

The results were impressive for that time. There was a polarization difference yielding a model fit of the surface dielectric constant of 2.2 +/- 0.2 and a radius 0.7 percent +/- 0.9 percent less than the ephemeris value and consistent with radar determinations. The value of dielectric constant was less than radar determinations, even after a correction for possible roughness. One possibility considered, which we now realize is correct, is that at this wavelength there is some absorption (and emission) in the Venus atmosphere that dilutes the observed properties of the surface emission. At the time there wasn't enough information to sort this out.

Barry finished his Ph.D. in 1964 and soon left for NRAO, where he worked on various activities including the Green Bank interferometer and Very Long Baseline interferometry and after several more years, the development of the Very Large Array. This brings me finally to the subject I'm supposed to be discussing: planetary radio astronomy in the context of the VLA.

Many competing requirements went into the VLA design and they encouraged a lot of flexibility. Even so, it is remarkable that there is such a good match between the range of capabilities of the VLA and the range of needs for planetary observations. I'm thinking here about field of view, angular resolution, brightness sensitivity, and polarization capability. The frequency coverage is also a good match except that there is much to do at short millimeter and submillimeter wavelengths as well. That is where the millimeter array will fit in some day.

For such a large, high resolution instrument observing nearby objects, one has to examine whether the instrument is truly operating in the far field. Is the plane wavefront approximation adequate when the array geometry is being calculated? The moon is in the near field and is a problem except for the smaller configurations and larger wavelengths. (Of course, the moon presents field-of-view problems as well.) Mars and Venus at closest approach can be uncomfortably close to the near field at the largest configuration and smallest wavelengths. It is impressive that the linear size of the resolution element on an extraterrestrial object can be comparable to the linear size of the VLA.

What sorts of things can we learn about planets and satellites using the VLA? There are a number of interesting problems to study, mostly dealing with atmospheres and surfaces. Let me begin, however, with a couple that are uniquely different.

For Jupiter, the dominant emission source for wavelengths longer than about 6 cm is synchrotron emission from relativistic electrons moving under the influence of Jupiter's magnetic field. It is interesting to contrast this radio source with extragalactic synchrotron emission sources because here we have a very symmetric, almost dipolar magnetic field that is very strong by comparison, and electron energies that are much less, though still highly relativistic. The VLA has produced some very impressive and useful images of this emission and its polarization. (An early VLA study was that of Roberts, Berge, and Bignell, 1984).

The rotation of the source, locked to the bulk of the planet, plus the tilted magnetic field as seen in the E-vector variation (about 10 degrees with respect to the rotation axis) produce beaming and circular polarization effects that give us physical insight. In fact, even before direct spacecraft measurements, and before the VLA, for that matter, we knew the polarity of the field, its approximate intensity, and how the electrons were distributed in energy, pitch angle, and radial distance.

The other unique case is the Saturn ring system, which can be seen faintly throughout the VLA frequency range. Until one gets to wavelengths of about 1 cm and shorter, the signal from the rings is primarily scattered emission. This is atmospheric emission from the disk of Saturn that is scattered to Earth by ring material. The VLA has produced some beautiful images of Saturn plus rings (Grossman, Muhleman, and Berge, 1989). The frequency dependence, phase dependence, and radial dependence, plus the extinction where the rings cover the disk, bear on the nature of the ring material. It is rather hard to fit it all satisfactorily, but the data seem to require a rather pure and very cold ice, so as to have high transparency and active scattering properties, with a broad size distribution from a few meters down to one or two centimeters.

As stated earlier the other VLA research on planets and satellites all boils down to atmospheric and surface/subsurface studies. (For example, see de Pater, 1990; de Pater, Brown, and Dickel, 1984.) The satellites, with the exception of Titan, all present us with surface emission and are modestly resolved at best, and thus can be examined just for their gross thermal and dielectric properties. For Mars and Mercury, we also see surface emission, but the larger angular size allows these properties to be determined with a regional resolution. Mars has thin atmospheric water vapor that has been studied spectroscopically with the VLA (Clancy, Grossman, and Muhleman, 1992). The emission of the major planets is all atmospheric emission that is generally dominated by the ammonia absorption spectrum. The Venus emission is dominated by pressure broadened carbon dioxide emission up to a wavelength of several centimeters and by surface emission at longer wavelengths. The Titan emission is dominated by pressure broadened molecular nitrogen.

Instrumental artifacts are a problem with planets. Even with a well sampled, but largely unfilled array, the instrument couples strongly with the perfectly regular circular brightness plateau and sharp cutoff at the edge to produce artifacts. I remember the first time I was producing a contour plot of Venus at 6 cm with new VLA data. With a 10-percent contour interval it looked so impressive - a perfect circular image just as expected. Then it slowly sank in that this was what we already knew; what we wanted to study was mostly in the top 10-percent or even 5-percent contour interval. When that interval was expanded, there were ugly brightness bands that obviously had nothing to do with Venus, but were instrumental effects.

It became clear that we should subtract from the visibility data all the information that we knew or thought we knew, thus removing the associated artifacts, so that the residual image would represent the part that we didn't know. Several variations of the subtraction were developed and were very successful.

Before closing, I should acknowledge the planetary radar activity that has been performed with the VLA. Originally, the VLA did not have any receivers that were compatible with the Deep Space Network, particularly the 3.5-cm wavelength used for spacecraft communication and data return. For the Voyager mission, NASA was particularly eager to increase the data rate during flyby and, to that end, a deal was struck whereby NASA would outfit the VLA with 3.5-cm receivers in return for precious observing time to test the system and collect data.

This worked out well, I think, and it had the added bonus that the VLA could be used as the receiving station for a bistatic imaging planetary radar. The high-power transmission and

Doppler frequency tracking could be done with the large antenna at the Goldstone tracking station. Several programs have been run with impressive results (Muhleman, Grossman, and Butler, 1995). Included in these results are the discovery of probable water ice deposits in the polar regions of Mercury (Butler et al. 1993), the discovery of the lack of a deep global ethane/ methane ocean on Titan (Muhleman et al. 1990), the discovery of the very bright radar reflectivity of the residual south polar ice cap on Mars, and the discovery of a very large region which has radar reflectivity indistinguishable from the noise (dubbed "Stealth" by the discoverers - see Muhleman et al. 1991). These discoveries have all proven fundamental in revising the thinking about these bodies and their properties.

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The Contributions of the VLA to the Study of Radio Stars

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Abstract. This review is an attempt to cover developments in the field of stellar radio astronomy (across the whole HR diagram) that have been influenced by VLA observations. The VLA has completely revolutionized the field; it can be argued that stellar radio astronomy would barely exist without the VLA. Different areas of physics and stellar astrophysics that can be addressed with VLA observations are mentioned, including the Sun (briefly). Among the major discoveries is the fact that many main-sequence stars like the Sun possess a nonthermal corona which is completely absent on the Sun.

1. Introduction

Stellar radio astronomers like to argue that the discovery that a huge range of stars of widely disparate types emit detectable radio emission is one of the major and unexpected breakthroughs made by the VLA. After all, the VLA was never expected to detect stars when it was designed: its primary goal was to be the spatial structure of extragalactic sources. Volume I of the original proposal for the VLA (dated January 1967; a copy may be found in the NRAO library) discusses the science expected to be done with the VLA in just 9 single-sided, double-spaced typed pages (out of several thousand pages in the whole document: by modern standards, the science justification was remarkably lightweight and nowadays would never pass muster). Over a third of this is devoted to extragalactic sources, but other topics mentioned include thermal galactic sources such as planetary nebulae and HII regions and solar system objects including the Sun. The only mention of a star (other than the Sun) is a reference to "stellar envelopes," which in modern parlance meant thermal stellar winds, and we shall return to that particular reference later in this review. (Note that the authors did cover themselves by remarking that "The capability of the VLA is so far beyond that of any existing instrument that it can be expected to open up vast new vistas and areas of research which cannot now be predicted." This was of course correct, although this statement too would probably be frowned upon by the modern reviewer for being too wishy-washy.)

Stars weren't included in the science justification (I assume) for the simple reason that there was no reason to expect them to be detected, certainly not main-sequence stars: it is a straightforward calculation to take the Sun, put it at the distance of the nearest known stars (the M dwarf Proxima Cen and the G dwarf solar analog α Cen at 1.3 pc) and show that you could not detect its steady emission (fluxes of order 15 μJy at 20 cm and 75 μJy at 2 cm). On that basis, the prospects for detecting "ordinary" stars even further away seem dim. However, by the time the VLA actually started observing 10 years later quite a number of stars had been detected by the generation of interferometers which operated through the 1970s (the NRAO 3-element interferometer and Westerbork) and which provided the positional discrimination missing from earlier single dish observations. Reviews of the state of the field at around the time when the VLA started operating may be found in Barlow (1979), Hjellming & Gibson (1980), and in Hjellming's (1988) comprehensive review. A number of hot mass-losing stars such as P Cygni, WR 140 and MWC 349 were comfortably detected at centimeter wavelengths, as were about 20 members of the class of evolved binary stars named after the prototypical system, RS CVn. Huge meterwave flares from M dwarf flare stars and microwave flares from red giants such as Betelgeuse were reported in single–dish observations, but were widely regarded with suspicion. And pre–main–sequence stars made up two of the weakest detections ever reported with the NRAO interferometer, T Tauri (8 mJy at 3 cm) and LkHα 101 (Spencer & Schwartz 1974).

So when the VLA started operating, certain classes of stars were already known to be radio sources, but with relatively few members of each class detected, and there were no radio detections of "ordinary" main sequence stars. Surveys of the classes known to provide detections were quickly started, as was work on the main sequence: Dave Gibson persuaded Paul Fisher to do his M. Sc. thesis on M dwarf flare stars, and the close association of solar and stellar groups in Boulder led Jeff Linsky, Dale Gary and George Dulk to look at a small number of active stars from a range of spectral types. Once these groups reported their detections (Fisher & Gibson 1981; Gary & Linsky 1981; Fisher 1982; Gary, Linsky & Dulk 1982; Linsky & Gary 1983) the field grew quickly, and subsequently almost every conceivable stellar class has been surveyed with the VLA.

The VLA has a number of properties which make it by far the preferred instrument for stellar radioastronomy: in addition to its excellent sensitivity, it has frequency agility which makes it feasible to obtain a continuum spectrum for an object in a short time; it has high spatial resolution, which avoids confusion with the myriad of background extragalactic sources except in the most compact configuration at the lowest frequencies; and perhaps most important of all, its "Y" configuration, together with the large number of dishes, produces excellent snapshot *uv* coverage, which means that a given target field can be mapped adequately in a few minutes, something simply not feasible with a linear array. The latter property makes the VLA unmatched at carrying out surveys, and for studying time–varying phenomena which occur on stars as well as on the Sun.

As mentioned earlier, we now know that many main–sequence dwarf stars are unexpectedly strong radio sources, because they possess something completely unknown on the Sun, a nonthermal corona: this discovery constitutes a major advance in our understanding of stellar atmospheres. The VLA has completely transformed the field of stellar radioastronomy, and observations with the VLA are the foundation of virtually all advances in this field. In the remainder of this paper, I will try to cover some of these advances. This will not be a comprehensive review, and the material presented is necessarily biased by my own interests and by the availability of figures. I apologize in advance for the fact that for lack of time I will omit some references which deserve to be included. I will not cover at all the important work done on systems containing compact objects such as neutron stars or black holes, even though some of the brightest "stellar" radio sources (e.g., Cyg X–3) fall into this category. I will at the end briefly mention solar work carried out with the VLA, since it is not covered elsewhere in these proceedings.

I will assume a passing acquaintance with the basic properties of the main radio emission mechanisms. *Thermal free–free* or *bremsstrahlung* emission is responsible for the classic stellar–wind radio emission. The opacity for this mechanism varies as $n_e^2 T^{-1.5} \nu^{-2}$, where n_e is the electron density, T the electron temperature and ν the radio frequency. The fact that the opacity decreases as frequency increases, while density in an outflow decreases with radius, leads to a fundamental property of free–free radio emission from stellar winds: higher frequencies probe deeper into the stellar wind. For a constant–velocity wind ($n_e \propto r^{-2}$) the radius of the optically thick surface, which limits how deeply we can see, scales with frequency as $\nu^{-0.7}$. The combination of this scaling of optically–thick source dimension with frequency and the ν^{+2}-dependence of the blackbody emission law produces the classic $\nu^{0.6}$ spectrum of a constant–velocity free–free–emitting stellar wind. *Nonthermal synchrotron emission* is the basic microwave emission process operating in solar flares and in stars which show solar–like magnetic activity. This mechanism requires nonthermal distributions of mildly ("gyrosynchrotron") or highly ("synchrotron") relativistic electrons in magnetic fields of order gauss or stronger. With magnetic fields of hundreds of gauss, which are typical of stellar coronae, this mechanism can have a spectral peak (corresponding to the transition from optically thick at low frequencies to optically thin at high frequencies) in the microwave range, as observed for solar flares. For much lower magnetic fields, such as one expects outside stellar atmospheres, this mechanism should be optically thin at VLA frequencies and the radio spectral index $-\alpha$ is then related to the nonthermal electron energy spectral index $-\delta$ by the synchrotron relationship $\alpha = (\delta - 1)/2$.

2. The Radio HR Diagram

To emphasize the changes in our knowledge of the radio properties of stars, Figure 1 presents a sample of the *radio–detected* stars plotted on an HR diagram (an idea which goes back at least to Gibson 1985). Stars believed to be nonthermal emitters are shown with filled symbols, while thermal emitters are shown with open symbols. Recall that prior to the VLA this figure showed a number of cool subgiants in binary systems, a few hot luminous stars, two pre–main–sequence objects and possibly some M dwarf stars. The detections presented in Figure 1 cover virtually the whole of the HR diagram where stars are found, with the possible exception of ordinary main sequence A stars (see below), isolated white dwarfs and brown dwarfs. Figure 1 contains by no means all the stars detected by the VLA: I did not have time for a complete survey of the literature, and an HR diagram is necessarily restricted to stars for which reliable absolute magnitudes and colors are available. This is not yet possible for many classes of less well known objects, such as the stars identified in *ROSAT* X–ray surveys which are believed to be runaways from the Taurus star–forming region (e.g., Carkner et al. 1997).

As is readily apparent, this restriction (reliable magnitudes and colors) had to be relaxed for some classes, notably hot stars, symbiotic and Be stars, and classical T Tauri stars: all these classes of object tend to lie in heavily obscured areas, which affects both colors and magnitudes, while the catalogued distances to the more luminous stars are usually just estimates. I have attempted to correct the colors and magnitudes of the hot stars for extinction where data are available (although this is to some extent a circular procedure since the appropriate corrections are often determined by assuming that a star of a given spectral type should lie at a particular place on the HR diagram). I have not attempted to separate binary components in spectroscopic binaries. For the Algol systems the hot primary generally dominates the light and the plotted position is that of the primary; for the RS CVn systems, the two stars often have similar magnitudes but different colors, so that the position of these systems on the HR diagram does not accurately represent either component. Wherever possible, Hipparcos data have been used for M_V and $B - V$: this applies to nearly all stars with $M_V > -2$.

The following sections will highlight the radio properties of different classes of star within this diagram.

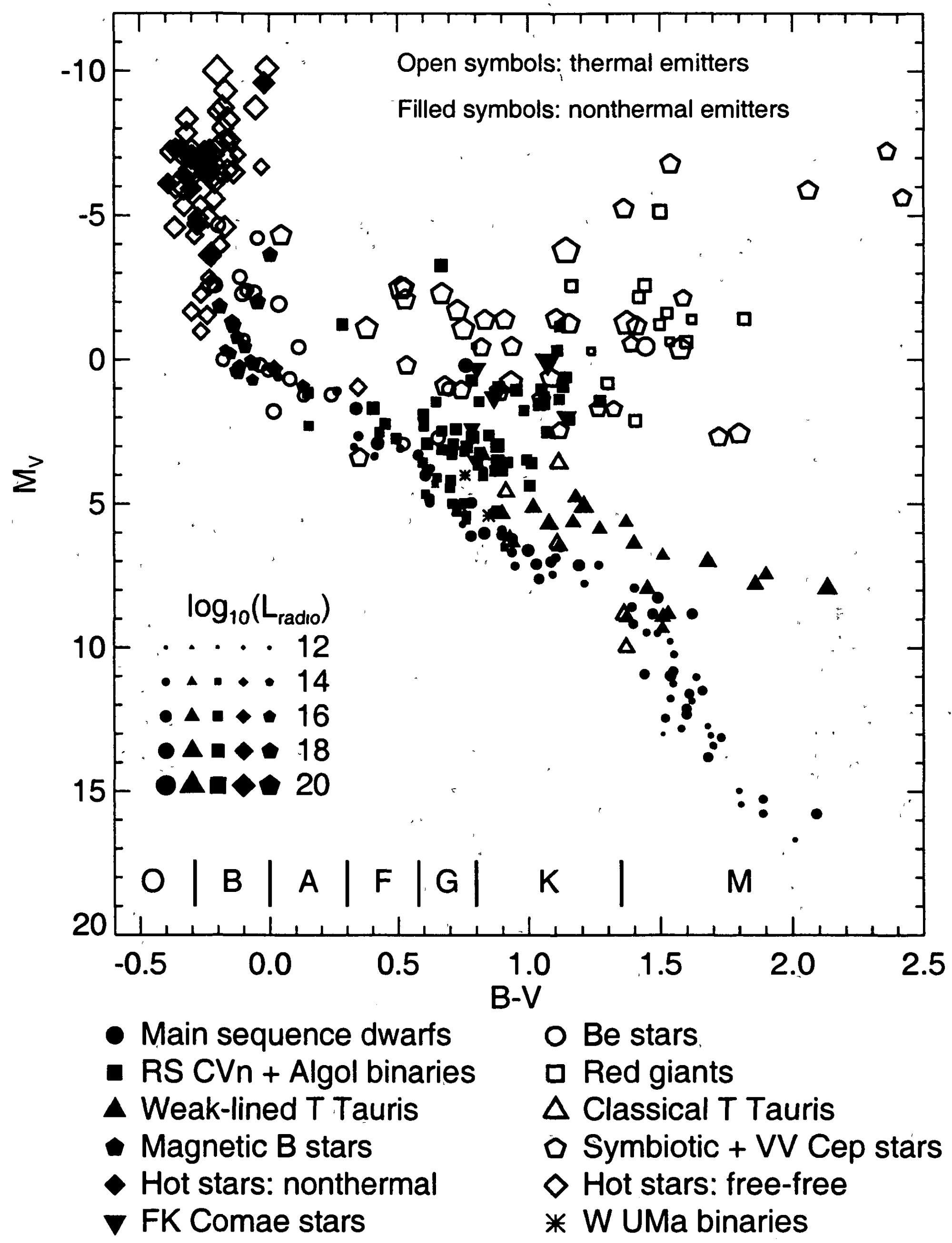

Figure 1. An HR diagram for stars detected as radio emitters, compiled from the literature using the Hipparcos catalog for the luminosities and colors wherever possible. For hot stars corrections for extinction have been applied where available. The size of the symbol reflects the radio luminosity of the star as labelled (in ergs s^{-1} Hz^{-1}). The diagram is restricted to stars for which distances are available, and therefore shows only a small (but reasonably representative) sample of all the stars detected by the VLA. Stars believed to be nonthermal emitters are represented by filled symbols, while thermal emitters are shown by open symbols. A color version of this figure may be found on the web at http://www.astro.umd.edu/~white/images/radio_hr_diag_full.gif.

3. Luminous Hot Stars

Large numbers of very hot luminous stars are now known to be radio sources, thanks to surveys of OB stars (Bieging, Abbott & Churchwell 1989; Leitherer, Chapman & Koribalski

1995; Scuderi, Panagia & Umana 1998) and Wolf–Rayet stars (Abbott et al. 1986; Leitherer, Chapman & Koribalski 1997). On theoretical grounds most very luminous hot stars should be detectable via the free–free emission from their dense powerful winds, and the spectra of the radio emission of most of these stars is consistent with this interpretation (which implies the $\nu^{0.6}$ spectrum of an optically–thick stellar wind). However, there is a significant fraction which have radio spectra incompatible with free–free emission, and these stars are believed to exhibit nonthermal synchrotron emission which gives us information on conditions within the winds of these stars. The interpretation is that the nonthermal emission is produced by electrons accelerated at shocks which form in the inhomogeneous winds: the characteristic speed of a hot star wind is generally in excess of the ambient sound speed in the wind, so that any significant velocity fluctuation, such as a fast knot overtaking a slower one or two winds colliding, has the potential to result in a shock. Once a shock forms, electron acceleration apparently takes place (by an as yet not completely understood mechanism). A high Mach-number shock produces a characteristic power–law energy distribution of spectral index -2, which results in a $\nu^{-0.5}$ radio flux spectrum (in the optically–thin limit believed appropriate to these sources). A detailed model for nonthermal emission from a single–star wind carrying random shocks was worked out by Rick White (1985). Note that the acceleration must take place at some distance from the star; the powerful radiation field of a hot star can quench shock acceleration close to the star where the inverse–Compton mechanism depletes energy from a high–energy electron faster than a shock can supply it (Chen & White 1994). This is a mild problem for models in which the magnetic field in the synchrotron source is a stellar field carried out by the wind, since it will diminish with distance from the star. An alternative is for the magnetic field also to be generated in the shocks as a byproduct of the plasma turbulence there.

We will discuss two particular stars in more detail, as examples of thermal and nonthermal sources with particular historical importance.

3.1. MWC 349

MWC 349 became famous amongst radio astronomers as probably the strongest thermal radio source in the northern sky and an important example of a stellar wind. It is one of the most extreme Galactic members of the class of luminous B stars showing forbidden emission lines, known as B[e] stars. These stars are characterized by very strong Balmer lines, forbidden FeII lines and very strong emission from hot dust at infrared wavelengths. These features are assumed to derive from the combination of a very dense equatorial disk, which provides the Balmer emission and the dust, together with a powerful ionized polar outflow which is responsible for the forbidden lines. MWC 349's radio spectral index of +0.6 motivated the fundamental theoretical work on the radio spectra of stellar winds (Olnon 1975; Panagia & Felli 1975; Wright & Barlow 1975). While the first models assumed a spherically–symmetric wind, observations with the VLA have shown that this is not appropriate (Cohen et al. 1985; White & Becker 1985; Rodríguez & Bastian 1994). On a scale of arcseconds (left panel of Fig. 2), the radio source is resolved with pronounced polar and equatorial axes of symmetry. At higher spatial resolution (right panel) the core is seen to break up into two sets of curved "horns" on either side of the star with much weaker emission in between. The peak brightness temperature in the horns is 10^4 K, while it drops to 4400 K at the inferred position of the star. The interpretation of this image is that the outflow from MWC 349 has low velocity but high density in the equatorial plane (the low–brightness region between the horns), and higher velocity but lower density in the polar directions (the directions of the horns). The

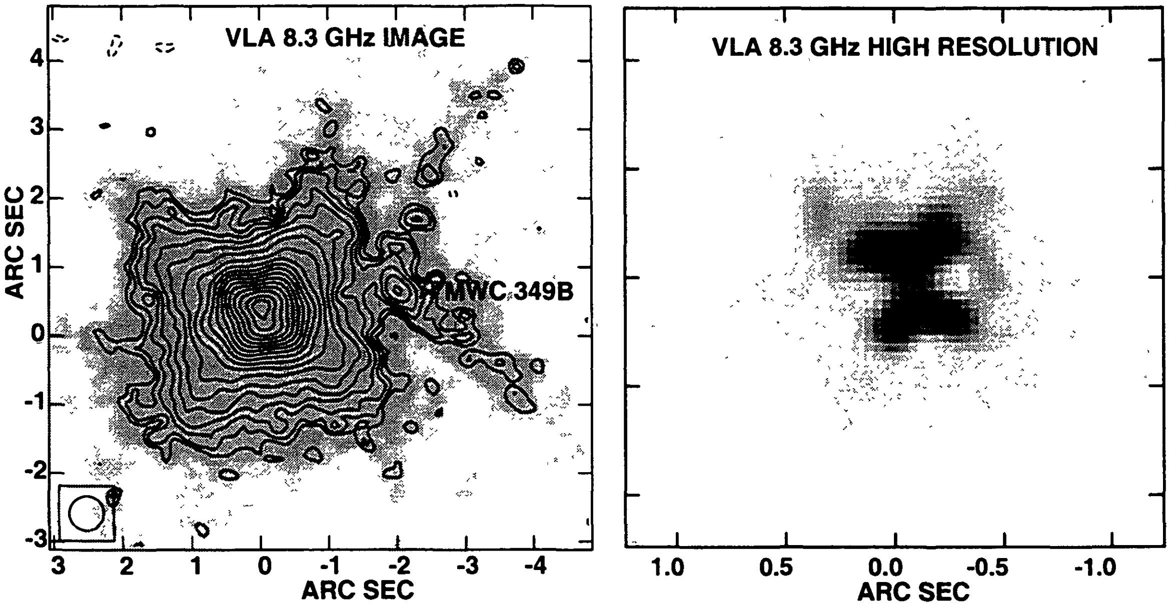

Figure 2. VLA 8 GHz images of the famous thermal stellar-wind source MWC 349, a B[e]star. The left panel shows a naturally-weighted cleaned image intended to emphasize weaker features: in addition to the thermal emission from MWC 349 itself, a weak bow shock is seen at the location of the interaction with the wind of a B star companion, MWC 349B. The polar axis of the system is slightly left of vertical, and the equatorial plane is nearly orthogonal to the plane of the sky. The right panel shows a super-resolved maximum-entropy deconvolution of the same visibilities in which high spatial resolution is achieved in the region of strongest flux (data of Rodríguez & Bastian 1994).

density is so high in the equatorial plane that the gas there has completely recombined and is neutral, while in the polar directions the gas is still ionized and thus can be optically thick at the temperature characteristic of a photo-ionized wind (10^4 K).

MWC 349 is also notable as one of the few stars whose thermal emission is strong enough for high spatial resolution recombination line observations to be carried out with the VLA (Martín-Pintado et al. 1993; Rodríguez & Bastian 1994). At millimeter and shorter wavelengths these recombination lines become extraordinarily bright due to maser action (associated with the fact that the rate of downward transitions of a recombining electron as it drops down the energy-level ladder increases as it gets to lower and lower levels) and this effect has opened up a whole new field of research (e.g., Martín-Pintado et al. 1989; Thum et al. 1994; Strelnitski, Ponomarev & Smith 1996).

3.2. WR 140

No discussion of stellar observations by the VLA would be complete without a mention of the star HD 193793, also known as WR (i.e., Wolf-Rayet) 140, which Barry Clark faithfully scheduled for Rick White and Bob Becker once per month for 8 years. The results of this effort are plotted in Figure 3, from White & Becker 1995, and they demonstrate how much physics can be addressed by a simple set of radio light curves for an unresolved system. WR 140 consists of a (secondary) WC7 Wolf-Rayet (WR) star and an O4-5 star (primary) in a highly elliptical orbit with a period of 7.9 years. It was known to have a nonthermal radio spectrum before the VLA began operating; the VLA's characteristics made it possible to

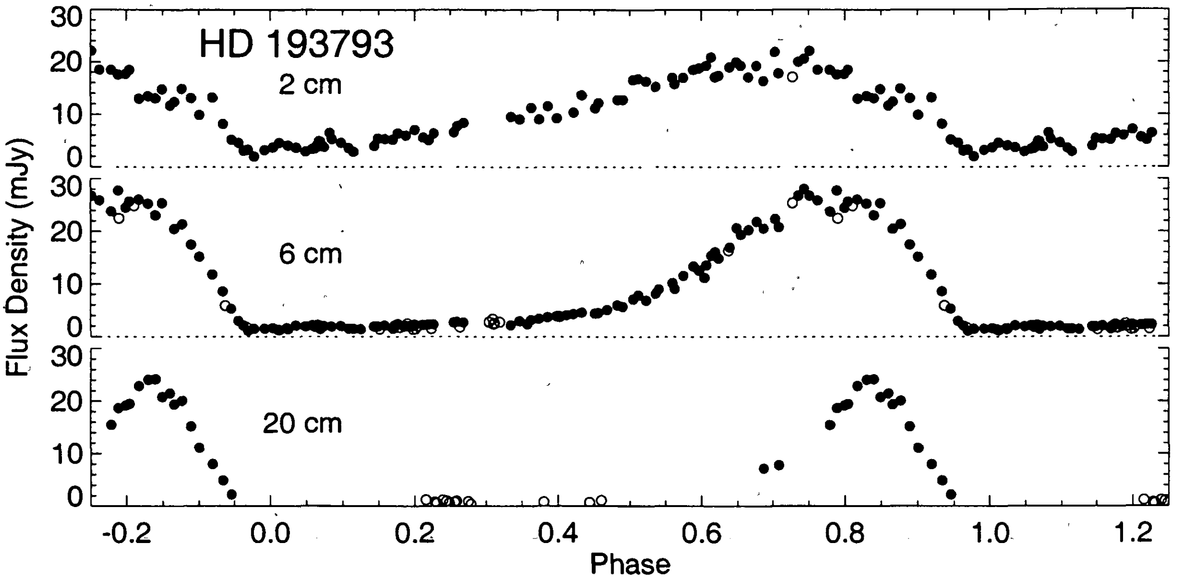

Figure 3. The radio light curve for WR 140 plotted against orbital phase over the 7.9–year orbit at each of three wavelengths. Filled circles are VLA data taken every month by White & Becker (1995); open circles represent other data compiled by Williams et al. (1990) covering more than one cycle, indicating that the light curve repeats almost perfectly from one cycle to the next. (Figure provided by Rick White.)

monitor the evolution of the radio spectrum of this source using just a modest amount of observing time per month.

The basic model is that the nonthermal emission arises due to synchrotron emission from energetic electrons accelerated in the shock where the powerful winds from the two stars collide. There is also some free–free emission from the winds, and the nonthermal radio emission is modulated by free–free absorption as the location of the shock changes with respect to the winds. The shock strengthens near periastron and weakens near apastron. The orientation of the orbit is such that the O star is closest to us during most of the orbit; the WC7 star passes between us and the O star just after periastron, at phase 0.01. The O star wind has a terminal velocity of 3100 km s^{-1} with $\dot{M} = 1.8 \times 10^{-6}$ $\mathrm{M}_\odot$ yr^{-1}, while the WR wind has a velocity of 2600 km s^{-1} with $\dot{M} = 6 \times 10^{-5}$ $\mathrm{M}_\odot$ yr^{-1}. In principle, to model this system one needs to know all the physical properties of the binary orbit and the winds, and understand the shock physics and its variation with stellar separation, electron acceleration at the shock, the source of magnetic fields and absorption in the wind; conversely, all these phenomena affect the radio light curves.

Free–free absorption should be largest at the lowest frequency and the 1.5 GHz (20 cm) light curve is consistent with this expectation: it shows no significant flux except for a narrow peak near phase 0.8. At 5 GHz (6 cm) the peak is much broader, with only weak emission just after phase zero when the strong wind of the WR star is between us and the wind–interaction region. The 15 GHz (2 cm) emission, which should suffer the least absorption and therefore best represent the intrinsic emission profile, peaks at phase 0.70, somewhat earlier than the 1.5 GHz peak: between phases 0.70 and 0.85 the 15 GHz flux drops while the 1.5 GHz emission rises. From phase 0 to 0.5, the spectral index is positive, consistent with thermal free–free emission. The fact that there is any 1.5 GHz emission detected at all is difficult to explain, since the parameters for the WR wind imply that it should be optically thick at 1.5 GHz well outside the binary orbit. Williams et al. (1990) showed that a model with spherically–symmetric winds could not explain the light curves and argued that the shadow of the O star creates a cavity in the WR wind where the opacity is greatly reduced. White & Becker (1995) argued that even the O star wind will be optically thick and absorb any high-brightness-temperature nonthermal 20 cm emission behind it; they suggest instead that the WR wind is confined to an equatorial plane inclined with respect to the orbital plane. In this geometry, the WR wind sweeps across the position of the O star twice per orbit; one of these occasions is presumably at phase 0.70, where the radio flux density is maximum. The number of possible factors playing a role in these simple radio light curves demonstrates just how much physics can be addressed with them.

4. Be stars

Be stars are stars of spectral type B which show strong emission lines indicating the presence of circumstellar material. They have strong infrared emission indicating the presence of dust, and all the evidence points to the presence of disks around these stars. This description by itself includes a very disparate array of stars, including the B[e] stars mentioned above: here we use the term to cover two types of stars. The *classical Be stars* are of uncertain evolutionary status. They are believed to be rapidly rotating, and may be a natural evolutionary state of all B stars. A number of classical Be stars have been detected in surveys by the VLA, and appear to have radio spectral indices intermediate between the thermal stellar wind value of +0.6 and the optically thick bounded source value of +2.0 (Taylor et al. 1990; Dougherty, Taylor & Waters 1991). Some Be stars are well–known for their association with neutron stars in close binaries which are strong X–ray and sometimes radio sources (notably LSI +61° 303).

A subset of the Be (and Ae) stars is found in regions of current star formation and is associated with reflection nebulosities. These stars are known as *Herbig Ae/Be stars.* The class consists of young massive stars that have only recently formed. They are the several–solar–mass counterparts of T Tauri stars (stars of age of order 10^6 yr and mass less than 1 $M_\odot$). These stars have dusty disks and energetic outflows. VLA surveys have detected more than ten of these stars and the radio emission is believed to be thermal free–free emission from the outflows (Skinner, Brown & Stewart 1993; Di Francesco et al. 1997).

5. Symbiotic stars and VV Cephei binaries

Symbiotic stars are interacting binary systems consisting of a cool star and a hot star, with the cool component usually being a late–type giant and the hot component a white dwarf. VV Cephei–type binaries are similar in that they consist of a cool supergiant and a B main–sequence star, and are generally not interacting. Most single cool giant (or larger) stars are not notable radio sources because even though they may have strong stellar winds, the supply of ionizing photons from the red star is insufficient to maintain high degree of ionization in the outflow and the neutral wind then produces no continuum radio flux. Symbiotic stars and similar systems get around this problem by having a hot companion star to supply the ionizing photons which allow the outflow of the cool giant to become a radio source (Seaquist, Taylor & Button 1984; Taylor & Seaquist 1984).

The relatively nearby VV Cep–like system Antares (M1.5 Iab + B2.5V) illustrates this effect beautifully; the early VLA image of Hjellming & Newell (1983; Figure 4) shows a

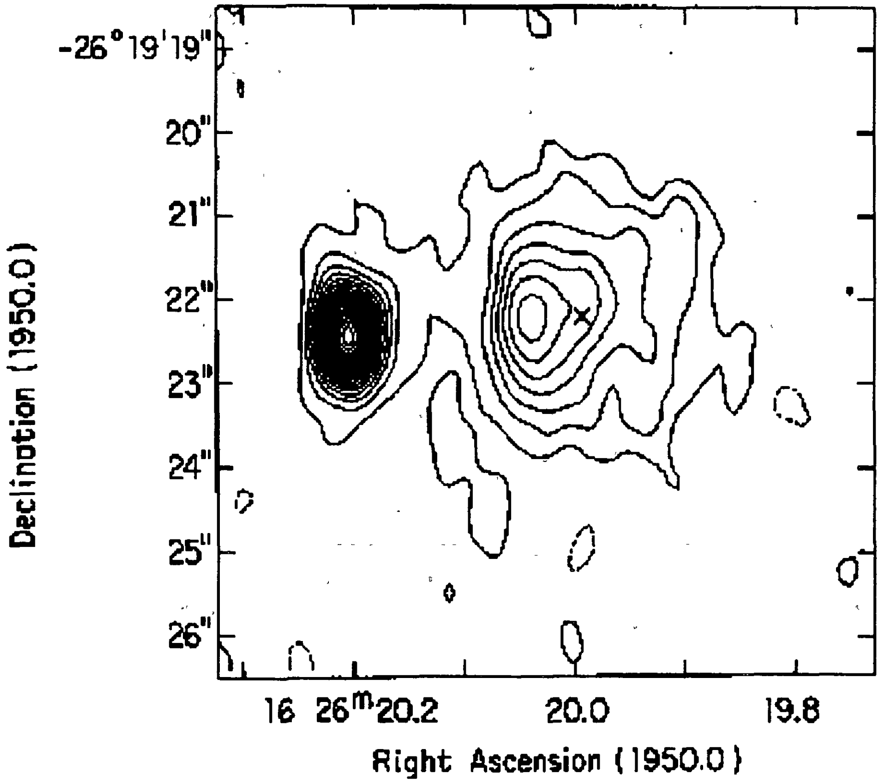

Figure 4. VLA 4.9 GHz image of Antares made from tapered A+B+C configuration data (Hjellming & Newell 1983). The cool M1.5 supergiant star is located at the unresolved source to the left of the image; the B2.5V star is located at the cross. The extended source around the B star can be well fit by a model of the subregion of the stellar wind from Antares which is ionized by Lyman continuum photons from the B star.

compact optically–thick thermal source associated with the M supergiant while 3″ to the west lies a diffuse extended optically–thin radio nebula associated with the B star. The compact source associated with the M star is just the disk of the star, similar to the emission from the single M supergiant Betelgeuse (see section 10), while the diffuse source represents the region of the M supergiant's dense wind which is ionized by ultraviolet photons from the B star. As expected for an r^{-2} density dependence in the cool star's wind, the diffuse source is asymmetric about the B star, being brighter on the side closest to the M supergiant (i.e., the higher density side). Seven VV Cep–like systems have been detected as radio sources: most have shorter binary periods than Antares and hence the density at the site of the B star is higher, giving larger optical depths, than for Antares (Hjellming 1985).

Surveys of symbiotic stars by Seaquist et al. (1984), Seaquist & Taylor (1990) and Seaquist et al. (1993) make up the bulk of the symbiotic detections shown in Fig. 1; note that since symbiotics are evolved binaries in which the two components have very different properties and often lie in obscured areas, their positions on the HR diagram have little meaning. Roughly half of the 99 symbiotics observed have been detected as radio sources. There is a formal correlation between the UV luminosity of the system and the radio luminosity, in accordance with the idea that the radio emission comes from a region of the cool–star wind ionized by the hot star (Seaquist & Taylor 1990). Most symbiotics do not show variability in their radio flux, although there are some notable exceptions (e.g., the system HM Sge which shows a resolved shell, and may be due to interacting stellar winds rather than a cool

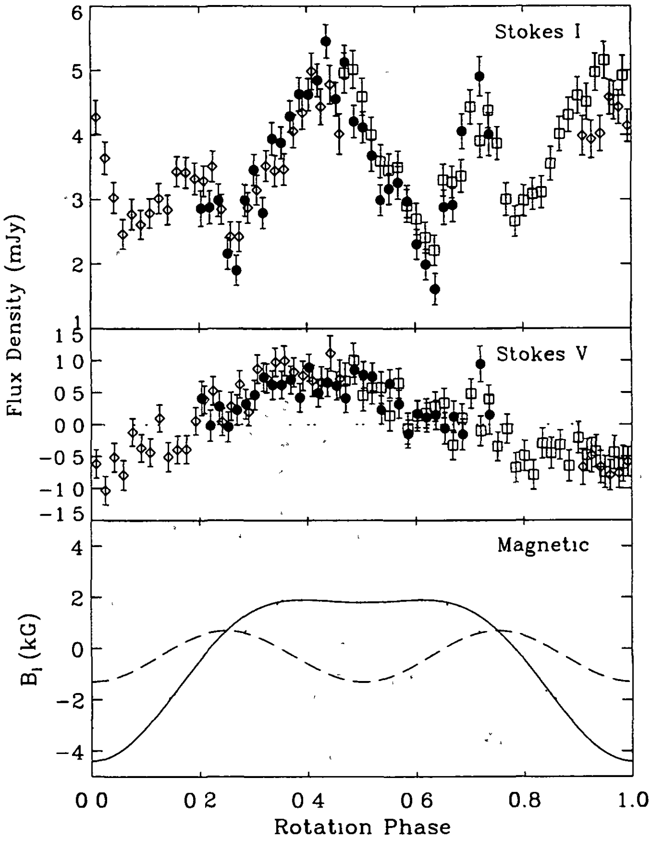

Figure 5. Radio and magnetic light curves for the magnetic Bp star HR 5624 plotted as a function of the phase of the 0.88 day rotational period. This star has a prominent quadrupolar component to its magnetic field in addition to a dipolar component: the bottom panel shows how the (optically) measured longitudinal field (solid line) for this star varies as a function of rotation phase, together with a decomposition into dipolar (dotted) and quadrupolar (dashed) components. The upper two panels show how the 5 GHz radio emission total intensity (upper panel) and circularly polarized flux (middle panel) vary as the star rotates (Lim, Drake & Linsky 1996). The three different symbols represent observations on three different days (spaced 2 days apart), showing that the modulation is repeatable.

wind ionized by a hot star: Purton, Kwok & Feldman 1983; Kwok, Bignell & Purton 1984). The radio spectra are often rising through the microwave range and flattening at higher frequencies: ; if the turnover frequency can be determined, the binary model for symbiotics permits an estimate of the binary separation (Seaquist & Taylor 1990).

6. Magnetic B and A stars

The class of early-type stars known for the chemical peculiarities in their photospheric spectra, and for the kilogauss magnetic fields which they possess, is a remarkably prolific and poorly understood supplier of radio sources. Surveys by Drake et al. (1987), Willson et al. (1988), Linsky et al. (1992) and Leone et al. (1994) have detected 25 of these stars (known variously as chemically-peculiar A and B stars, Ap/Bp stars or magnetic A and B stars). The chemical peculiarities are believed to result from gravitational stratification effects in the non-convective outer layers of these hot stars, while the magnetic fields are thought to be fossil fields left over from the collapse of the stars out of magnetized molecular clouds. The magnetic field geometry is usually dipolar, with the magnetic axis often offset from the rotation axis in what is known as the "oblique rotator" configuration.

This class of stars can be broken into four subclasses on the basis of different chemical properties, roughly corresponding to different temperature ranges: Helium–strong (He-S) stars (typically spectral types O9 – B5), Helium–weak (He-W; B5 – A0), Silicon (Si, also B5 – A0) and SrCrEu (the coolest subclass, spectral type A). Radio detections have been found amongst the three hotter subclasses, but not amongst the SrCrEu stars (Drake, Linsky & Bookbinder 1994). As a general rule, the earlier spectral types have much larger luminosities: σ Ori E (B2V) has a 5 GHz luminosity of $10^{17.9}$ ergs s^{-1} Hz^{-1}, while θ Aur (A0) is at $10^{14.7}$ ergs s^{-1} Hz^{-1}. Linsky et al. (1992) argue that the radio flux is related to the stellar winds: the mass loss rate (presumably driven by radiative flux) increases as one goes to earlier stellar types, so that the outflow provides a source of free energy available to generate a radio source. The favored mechanism is based on the Havnes & Goertz (1984) model for the magnetospheric structure of these stars. The interaction between the dipolar magnetic field and the strong stellar wind results in regions of high–density plasma trapped in the equatorial zone where the non–radial magnetic field inhibits outflow. Since the magnetic field energy density falls off rapidly with radius, at some point in the outflow the thermal energy density of the plasma will overwhelm the magnetic field and drag it outwards, resulting in the formation of a current sheet where acceleration of particles to nonthermal energies can take place. Accelerated electrons can then radiate by the gyrosynchrotron process in the strong magnetic fields nearer the star: observed high degrees of circular polarization are consistent with such a mechanism.

An interesting aspect of this class of stars is that their radio properties are quite similar to those of active cool stars; their radio spectra are typically fairly flat (Linsky, Drake & Bastian 1992; Leone, Umana & Trigilio 1996), their quiescent emission shows moderate degrees of circular polarization, and one of them has even been observed to show a radio flare with variability on timescales of minutes (Drake & Linsky 1989). Yet in the case of active cool stars the radio emission is believed to be produced by processes ultimately tied to the generation of complex magnetic fields in a dynamo associated with convection, which Ap and Bp stars, with their radiative interiors, should not produce. On the other hand, unlike radio–bright cool stars their X–ray luminosities are relatively weak (Drake et al. 1994), radio flares are clearly not common and the properties of the radio emission seem to be quite stable on long timescales. Some of these stars are just bright enough to be able to look for rotational modulation of the radio light curve with the VLA, and such modulation has been found (Leone & Umana 1993; Lim, Drake & Linsky 1996; Lim, Drake & White 1998), including modulation of the degree and sense of polarization (an example is shown in Figure 5). This is an important result because the characteristics of the modulation can be used to infer the magnetic geometry of the emitting region.

7. Pre–main–sequence stars

This has been a productive area for the VLA which overlaps with interstellar medium (e.g., star formation and molecular cloud) studies. Probably more than one hundred stars belonging to this class have now been detected (Cohen, Bieging & Schwartz 1982; Bieging, Cohen & Schwartz 1984; André, Montmerle & Feigelson 1987; Stine et al. 1988; O'Neal et al. 1990; White, Pallavicini & Kundu 1992a; Chiang, Phillips & Lonsdale 1996; Suters et al. 1996;

Carkner et al. 1997; White et al. 1998; Stine & O'Neal 1998). Some rich areas of low-mass star formation may contain many detected radio stars in a single VLA 5 GHz primary beam (Leous et al. 1991; Felli et al. 1993).

In evolutionary terms, there are at least three stages during star formation that are important for radio studies. First are the so-called "Class 0" sources, which are highly embedded, presumably very young stars, not visible in the optical but often the source of molecular outflows and optically-visible Herbig-Haro jets. These sources are often very weak microwave continuum sources, which has proven to be a useful property since VLA observations can then be used to identify the exact location (with high spatial resolution) of the source of an outflow which is otherwise obscured (e.g., Pravdo et al. 1985; Curiel et al. 1990; Leous et al. 1991; Anglada et al. 1994; Bontemps, André & Ward-Thompson 1995; Rodríguez, Anglada & Raga 1995; Anglada 1995; Rodríguez & Reipurth 1996). Even the outflows associated with stars in this stage have proven to be detectable with the VLA: thermal jets indicating the presence of ionized gas in the outflows (Hughes 1989; Curiel et al. 1993; Rodríguez et al. 1994; Martí, Rodríguez & Reipurth 1995; Torrelles et al. 1997) and, even more remarkably, synchrotron emission indicating that acceleration of electrons to very high energies must be taking place in the outflows, possibly at the site of shocks (Yusef-Zadeh et al. 1990; Reid et al. 1995), have been observed with the VLA.

The next evolutionary stage corresponds to the classical T Tauri stars (CTT): low-mass stars found in star-forming regions which are generally optically visible but with veiling of the optical spectra. They are often associated with optical nebulosity and tend to show infrared excesses attributed to the presence of dust disks. The disks are interpreted to be remnants of the accretion process that formed the star. CTT's lie above the main sequence on the HR diagram, indicating that they are still contracting. Typical ages are thought to be 0.1 - 2 million years. The CTT stars which have been detected as microwave continuum radio sources typically show spectra rising at microwave frequencies, suggesting an origin as free-free emission from an ionized outflow. At higher frequencies the dusty disks themselves become strong radio sources, and several VLA observations have spatially resolved these disks, allowing their physical parameters to be determined (e.g., Rodríguez et al. 1992; Wilner, Ho & Rodríguez 1996). Note that reddening corrections have not been applied to the CTT stars on Fig. 1, which accounts for their erratic distribution on the figure.

Members of the third class of low-mass pre-main-sequence stars are called weak-lined T Tauri (WTT) stars. They are apparently very similar to classical T Tauri stars, i.e., lie in the same region of the HR diagram, but they lack the signs of circumstellar material that are characteristic of CTTs (infrared excess, optical veiling). They are also called "naked" T Tauri stars for this reason. Not surprisingly, since they are young, generally rapidly-rotating cool stars, WTTs have radio properties similar to those of active main sequence stars: they tend to be very strong nonthermal radio sources (luminosities of order 10^{17} ergs s^{-1} Hz^{-1}, i.e. much larger than active main sequence stars of the same spectral types), but with lower degrees of circular polarization detected than on most other active stars and apparently quite long timescales for radio variability (White, Pallavicini & Kundu 1992b). It is notable that despite the similarities in the underlying stars, including similar X-ray luminosities, no confirmed classical T Tauri star has ever (to my knowledge) shown any evidence for nonthermal radio emission. It may be that the stellar activity that leads to nonthermal coronae in WTT stars is somehow suppressed in CTTs by interaction with the circumstellar material close to the star, or else the nonthermal corona exists close to the stellar surface, but the radio emission from it is unable to penetrate a cloak provided by cool but ionized gas at greater distances

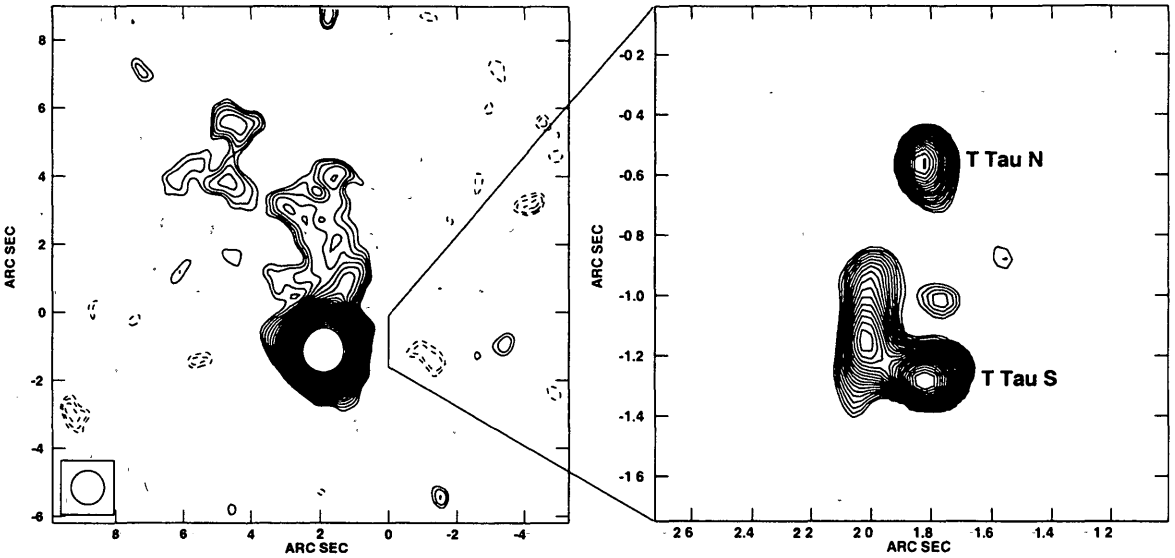

Figure 6 VLA "A"-configuration images of the T Tauri system (White et al, in preparation). Left panel: a 4.5 GHz image made using a 1″ beam to emphasize the larger-scale structure. The stars are located in the core of the brightest emission. Extended emission is seen trailing off to the north-east. Right panel: a super-resolved maximum-entropy deconvolution of the 8 GHz emission in the core of the system, showing emission from the two stars (labelled) together with quite bright extended emission to the north-east of T Tau S. The morphology of the extended emission seems to change with time.

from the star, which may be optically thick even without being bright enough to be observed itself (Montmerle & André 1988).

The prototypical system T Tauri displays a number of remarkable features all by itself. As noted earlier, it was detected as a radio source before the VLA was built, and its (relatively) strong radio flux allows it to be studied in some detail. VLA observations were instrumental in showing that in fact it is a binary system consisting of two stars 0.6″ apart (Schwartz et al. 1984); both are radio sources. The optically visible star is the northern component, and is a classical T Tauri star with a rising microwave spectrum and a dusty disk detected at millimeter wavelengths. The southern companion is embedded, but is also detected as an infrared source; its radio characteristics are those of a WTT star (nonthermal spectrum, circular polarization, variability: Skinner & Brown 1994; Phillips, Lonsdale & Feigelson 1993). In addition to the two stellar sources, there is also extended radio continuum emission in the system on small scales, between the two stars (Schwartz, Simon & Campbell 1986; Ray et al. 1997), and on a larger scale corresponding to the optical nebulosity associated with the system (see Figure 6). The nature of this extended emission is not yet well understood.

8. RS CVn and Algol binaries

Since most of the confirmed radio stars known when the VLA started observing were RS CVn systems, it is natural that they received a great deal of attention from VLA observers. RS CVn binaries are detached but close binaries in which at least one component has evolved into a cool subgiant, typically consisting of an F – G main sequence primary and a K subgiant or giant which was the primary in the system until it evolved away from the main sequence. During their main-sequence life these systems may have been quite inactive, consisting of two stars each typically with a mass in excess of a solar mass. Once the original primary depletes its core hydrogen, it moves into the Hertzsprung gap and develops a convection zone as it cools. The combination of the rapid rotation enforced by corotation in the binary and the presence of a convection zone leads to the generation of intense solar–like magnetic activity. The activity is thus attributed to the cooler evolved component of the system. Most single, cool subgiants do not show similar levels of activity due to their slow rotation (exceptions being the small number of FK Comae systems discussed in section 10).

The RS CVns are the most prominent members of a broader class of active close binaries containing a cool star (Strassmeier et al. 1993). These include the optically bright Algol systems, which contain a hot star and a cool star (typically a B or A primary and a G – K subgiant secondary) in a close binary. The mass ratio in Algol systems is very far from unity, whereas in RS CVns it is generally close to unity; and mass transfer plays a major role in the evolution of Algol systems but is incidental in the evolution of RS CVns. The "BY Draconis" binaries are a separate class comprising close detached binaries consisting of main–sequence dwarfs, i.e., they differ from RS CVns because neither component is evolved. In each class, the crucial ingredient for activity seems to be the presence of a cool star with a convection zone in a binary system where corotation maintains rapid rotation. Not all close binaries containing cool stars show the same radio properties, however. The "Serpentids" are strongly interacting binaries superficially resembling Algols in that they consist of a hot and a cool star (typically B + K), but with with much greater mass transfer taking place. They and the similar "peculiar emission–line Algols" (Elias II & Güdel 1993; Güdel & Elias II 1996) are detected but are much weaker radio sources than are Algols (Elias II 1990), and may be thermal free–free emitters rather than nonthermal radio sources. Another related class is the W UMa binaries, consisting of two late–type (F to K) main–sequence stars in contact with one another and thus having the shortest possible binary periods (0.1 to 0.3 days) for systems consisting of non–degenerate stars. They are underactive in X–rays compared to other stars of similar spectral type in short–period binaries, and their radio luminosities are also much weaker than those of RS CVns (Hughes & McLean 1984; Beasley et al. 1993; Rucinski 1995), so the fact that the stars are in contact does seem to influence their activity levels.

Major VLA surveys of RS CVn systems have been carried out by Mutel & Lestrade (1985), Morris & Mutel (1988) and Drake et al. (1989,1992), while Algols have been surveyed by Umana et al. (1991,1998). At least 71 RS CVns and 17 Algols have been detected as radio sources by the VLA (Drake, Simon & Linsky 1992). They exhibit at least three types of radio emission (there is no obvious difference in the radio properties of the two types of binary):

1. *"Quiescent" emission* which is steady on a timescale of minutes but may be slowly modulated on a timescale of hours. This emission typically has a flat spectrum and is significantly circularly polarized (Mutel et al. 1987; White et al. 1990; Umana et al. 1993). It is attributed to gyrosynchrotron emission from a steady population of nonthermal electrons trapped in the corona of the cooler star. The flat spectrum comes from having a source which is optically thick, but with the size of the optically thick source increasing as frequency decreases. Brief examples of quiescent emission may be seen in Figure 7. VLBI measurements indicate that the size of the quiescent source can be larger than the stellar diameter, suggesting that the emission is not necessarily only associated with the cooler member of the system.

2. *Radio outbursts*, in which the flux rises dramatically, the radio spectral index is positive and the degree of circular polarization is very small (e.g., Morris, Mutel & Su 1990).

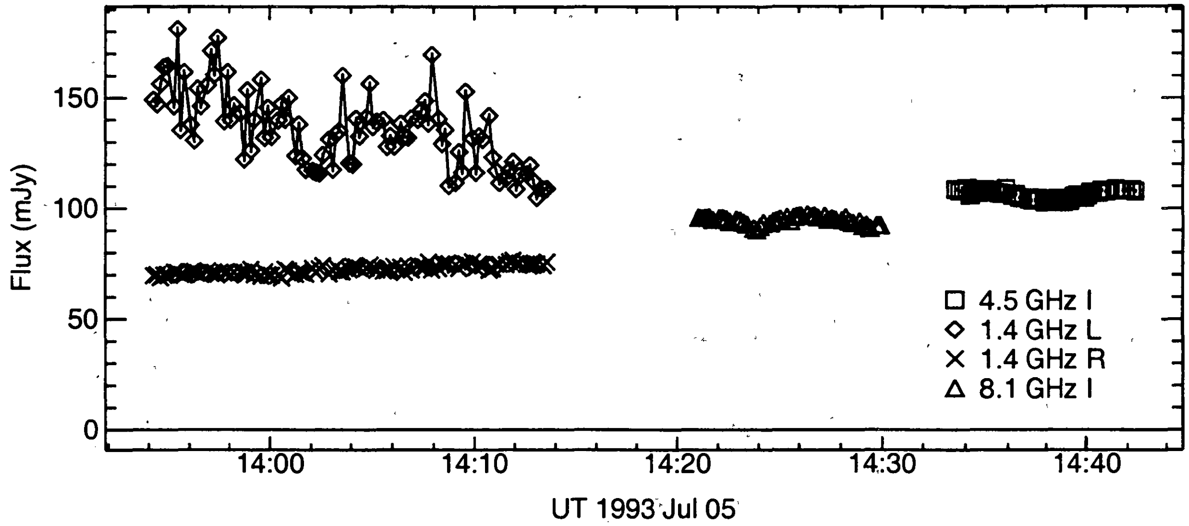

Figure 7. VLA observation of HR 1099 at 1.4, 4.5 and 8.1 GHz. At 1.4 GHz right (R) and left (L) circular polarizations are plotted separately, while at 4.5 and 8.1 GHz only the total intensity (Stokes I) as a function of time (10 s time resolution) is shown. At 1.4 GHz the left circular polarization shows a strong, highly polarized and rapidly fluctuating component which is attributed to coherent emission, present much of the time at low frequencies (White & Franciosini 1995). The 1.4 GHz R data and the 4.5 and 8.1 GHz data are representative of quiescent emission (albeit in a high state) from such systems.

These are attributed to the impulsive acceleration of a large number of very energetic electrons which then form an optically thick synchrotron source of finite extent (possibly in the form of a plasmoid) which naturally has a rising spectrum. VLBI data suggest that as the radio outburst decays, the source expands (Trigilio, Umana & Migenes 1993). These flares are many orders of magnitude more luminous than solar microwave bursts, and represent the signature of an impressive energy release.

3. *Highly polarized coherent bursts* at low frequencies (e.g., Simon, Fekel & Gibson 1985; Lestrade et al. 1988; van den Oord & de Bruyn 1994; White & Franciosini 1995; Jones et al. 1996). At 1.5 GHz some form of this emission is often present when a star's quiescent flux is at a high level, and this fact led to some confusion regarding the natural sense of polarization at low frequencies. When the coherent emission is not very strong, it is difficult to recognize it as a phenomenon different from the quiescent emission, but because it is typically 100% polarized it dominates the observed polarization signal. The polarization of the coherent component is opposite to the polarization of the high-frequency quiescent emission, so for many years theorists tried to come up with gyrosynchrotron source geometries in which the sense of polarization reversed in going from high to low frequencies. In such models, it is very difficult to achieve degrees of polarization as high as those observed at 1.4 GHz. A natural explanation for the bursty coherent emission is that it is some form of plasma emission, common on the Sun at low frequencies. Plasma emission is polarized in the sense of the O mode, whereas gyrosynchrotron is polarized in the sense of the X mode. A striking example of this phenomenon is shown in Figure 7, where one sense of circular polarization at 1.4 GHz varies dramatically on a timescale of seconds while the other remains steady. The VLA's sensitivity and ability to avoid source confusion made the identification of this separate component possible.

One might expect that rotational modulation would be a valuable tool in the study of the radio source in these binary systems, but there has been no evidence so far for rotational modulation or even eclipse of the radio emission in RS CVns or Algols. Partly this is due to the level of intrinsic variability in the radio flux, which tends to mask any repeated pattern, and partly this may be due to the difficulty of obtaining light curves over several periods of typically several days each. One binary system, V471 Tau, has shown radio eclipses when the white dwarf in the system passes behind the K dwarf, implying that the radio source is between the two stars and possibly tied to magnetic field lines connecting the two stars (Patterson, Caillault & Skillman 1993; Lim, White & Cully 1996).

I would be remiss if I failed to note that Barry carried out one of the first VLBI observations of a star (Algol), and succeeded in resolving it (Clark, Kellermann & Shaffer 1975)!

9. Cool main sequence stars

Figure 1 shows detections of main–sequence stars in all spectral classes from F down to M. The dramatic result of VLA observations of cool main–sequence stars is that they possess something unknown on the Sun: a nonthermal corona which produces strong and steady radio emission. That is, in addition to the thermal populations at 10^{6-7} K in stellar coronae which radiate X–rays, there are nonthermal populations of electrons, extending up to MeV energies, which are trapped on closed magnetic field lines and produce strong radio emission. There are several pieces of evidence for this conclusion: the radio spectra of the steady emission from active stars tend to be very flat even out to high frequencies, where thermal radio spectra would fall off rapidly with frequency; and the brightness temperatures of the radio emission, either measured directly by Very Long Baseline Interferometry (VLBI) which can resolve the sources, or inferred from observed flux levels using reasonable source sizes, are often in excess of 10^9 K. Such temperatures can only be produced by very energetic electrons unlikely to be in a thermal distribution. The fact that the radio emission is steady means that the corresponding population of gyrosynchrotron–emitting MeV–energy electrons is present continually in the corona. The Sun has no counterpart to this population; such nonthermal electrons are only present in the Sun's corona for very short periods during flares. This marks a fundamental difference between the coronae of active stars and the solar corona, a discovery made possible by the VLA's sensitivity.

The radio emission of M dwarfs was detected fairly early in the history of the VLA, as noted earlier, and there have been several surveys since (e.g., Linsky & Gary 1983; Willson, Lang & Foster 1988; White, Jackson & Kundu 1989; Güdel et al. 1993). Radio emission of main–sequence dwarfs in classes F–K was largely unknown (e.g., Drake et al. 1991) until the realization that X–ray luminosity could be used to select the stars most likely to be radio sources, and this led to systematic surveys of X–ray–selected stars in these spectral classes by M. Güdel and his collaborators (Güdel 1992; Güdel, Schmitt & Benz 1994; Güdel, Schmitt & Benz 1995; Güdel, Guinan & Skinner 1997), which have detected a number of stars at luminosities as high as 10^{15} ergs s^{-1} Hz^{-1} (although generally microwave fluxes are weak, below 1 mJy). It follows that the stars most likely to be detected are those which are observed to be extremely active at all wavelengths. Such high activity is well correlated with rotation and therefore also with youth, since it is believed that magnetic activity leads to the spin–down of main–sequence stars as they age. But not all the detected stars belong to this most active class; as the VLA ages, it has become possible to obtain long observations of faint stars which were not feasible in earlier years. Drake, Simon & Brown (1993) observed the inactive nearby F5 IV-V star Procyon on 5 occasions and, in 4 cases, observed flux levels ($\sim 33\mu$Jy; $L_R \sim 10^{11.7}$ ergs s^{-1} Hz^{-1}) consistent with optically thick emission from a 2 $\times 10^4$ K chromosphere plus a weak optically–thin coronal contribution from the known X–ray–emitting material in the corona. A similar long observation of the nearby A7 IV-V star Altair (Rucinski 1990) failed to detect it with a luminosity upper limit of 6×10^{12} ergs s^{-1} Hz^{-1}; ordinary A stars remain undetected as radio sources (Brown et al. 1990).

Active F to M dwarfs show the same sorts of radio activity exhibited by RS CVns: steady quiescent emission with a fairly flat spectrum (e.g., Güdel & Benz 1989; White, Kundu & Jackson 1989; Güdel 1994; Lim 1993), usually unpolarized or very weakly polarized (less so than for RS CVns); highly polarized coherent bursts at low frequencies (Gary, Linsky & Dulk 1982; Lang & Willson 1986; White, Kundu & Jackson 1986; Jackson, Kundu & White 1987; Kundu et al. 1988); and impulsive microwave flares at higher frequencies which look quite similar to solar flares (e.g., Rodonò et al. 1989; Güdel, Schmitt & Benz 1995; Lim & White 1995). Quiescent luminosities scale with the sizes of the stars: from 10^{12} ergs s^{-1} Hz^{-1} for M dwarfs up to 10^{15} ergs s^{-1} Hz^{-1} for F stars. One study of coherent bursts made novel use of the VLA's correlator to turn the spectral line system into a device to look at the dynamic spectrum of an electron beam passing through a stellar atmosphere, as is commonly done in solar radio astronomy (Bastian & Bookbinder 1987).

As noted above, recent detections of single stars amongst classes not previously known to be radio sources have been based on X–ray–selected samples which have become more readily available now through the *ROSAT* all–sky survey. The success of these surveys in making radio detections can readily be understood in general terms: both the X–rays, a result of coronal heating, and the radio emission, a diagnostic of nonthermal accelerated electrons, are the result of the free energy available from stellar activity. However, the relationship appears to be even more specific than this (Güdel et al. 1993; Güdel & Benz 1993). As demonstrated by Figure 8, which shows X–ray versus radio luminosity for stars covering a wide range of spectral types, the two are approximately proportional, with some notable qualifications, over about five orders of magnitude, and even solar flares appear to obey the same relationship: $\log L_R = \log L_X - 15.5$ (both in cgs units). The spread about this relationship is generally about 0.3 in the log. When simultaneously–measured radio and X–ray luminosities are compared, the relationship is even tighter (Benz & Güdel 1994). The exceptions to this relationship so far nearly all fall on the side of being more radio–luminous: the active subgiant binaries (RS CVns and Algols), the WTT stars, and some M dwarfs (notably UV Ceti and Rst 137B). The long–period binary Capella (G0III + G5III) is radio underluminous (Drake & Linsky 1986).

The good correlation evident in Figure 8 is puzzling because one can think of so many ways in which it might be destroyed. One of the most obvious is the well–established fact that the radio luminosity of a star is a much more variable quantity than is the X–ray luminosity. UV Ceti is one of the least–variable steady radio sources, but even it varies by about a factor of 3 (Güdel 1994), and it is very difficult to define a quiescent level for RS CVns (Drake, Simon & Linsky 1989), whereas a number of studies have shown that, when flares are discarded, the X–ray luminosity of active stars tends to be remarkably stable on time scales ranging from days to years, varying typically by less than 50% (e.g., Pallavicini, Tagliaferri & Stella 1990a; Stern, Schmitt & Kahabka 1995). The Sun's X–ray luminosity varies by orders of magnitude over the solar cycle. Another curious feature of Fig. 8 is that it mixes radio luminosities at different frequencies. This will affect the correlation unless all the radio spectra are approximately flat, which is fortuitously the case.

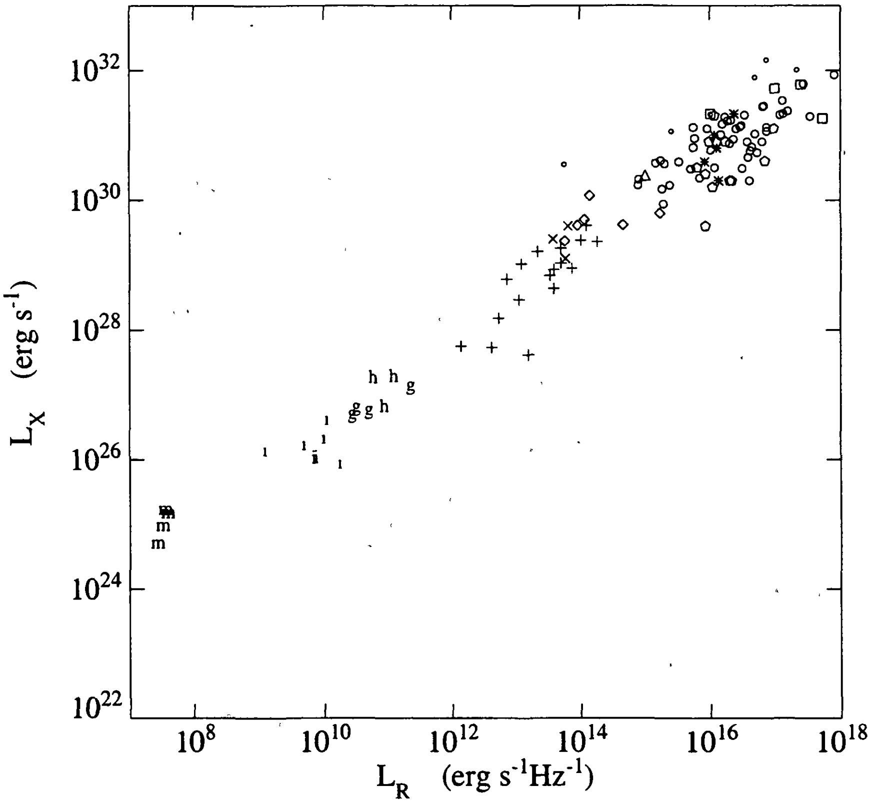

Figure 8. Comparison of soft X-ray and radio luminosities for several classes of stars (+, dMe's; ×, dK(e)'s; ◇, BY Dra binaries; ○, RS CVn binaries; o, RS CVn binaries containing two subgiants; △, AB Dor; *, Algol binaries; squares, FK Com stars; pentagons, WTT stars) and for solar flares of varying sizes (letters: m, solar microflares; i, impulsive solar flares of intermediate size; h, gradual solar flares with large impulsive phase; g, pure gradual events). From Güdel (1994).

Some of the correlation is no doubt due just to the scaling of coronal volume over the range of stellar types. If we assume that soft X-ray emission is the main energy loss mechanism for the stellar corona, and that the radio emission is optically thin, it may be shown that the condition $L_R \propto L_X$ requires that the parameter $B^n\tau$ be the same over a wide range of stellar classes and stellar sizes, where B is a magnetic field characteristic of the radio source, $n \approx 2.5$ and τ is some mean lifetime of a nonthermal electron in the corona (Güdel & Benz 1993; White 1996). If the radio emission is optically thick, which may well be the case at the commonly used frequency of 5 GHz, then there is no simple relationship between L_R and L_X.

10. Cool giant stars

As described earlier, single, cool giant stars by and large lack the characteristics that make other stars radio-bright: they are cool enough to have convection zones but usually rotate too slowly to generate via a dynamo the sort of magnetic activity which makes young cool dwarfs and rapidly rotating subgiants such strong nonthermal radio emitters; and they have massive outflows, but the coolest of these giants have no source of ultraviolet photons that can ionize the outflow and make it visible via its free–free emission. What some of these stars do have are large photospheres.

A number of surveys of different classes of cool giant have been carried out, mostly by Steve Drake and collaborators: a survey of nearby G0 – M5 giants and supergiants detected four K and M giants (Drake & Linsky 1986) but none of the G giants with coronae; none of the coolest 22 M and C giants surveyed by Drake et al. (1087) were detected; none out of nine S–type red giants were detected by Drake, Hollis & Brown (1991); while Drake et al. (1991) detected three out of 16 F – M giants and supergiants, including IRC +10216, previously detected by Sahai et al.(1989); and Luttermoser & Brown (1992) failed to detect any of 7 N–type carbon stars. There is some overlap with planetary nebulae in this area of research: Knapp et al. (1995) observed 21 evolved stars in order to look for newly formed compact planetary nebulae, which might be detected as strong free–free sources since the new hot star can ionize the dense circumstellar shell left by the cool giant; they detected several sources.

FK Comae stars are also formally single cool giants, but their properties are suspiciously similar to those of the active close binaries: their spectral types are those of G2 – K2 giants, and they have rapid rotation. FK Comae itself has a rotation period of just 2.4 days, while the other possible members of the class have periods of up to 200 days. They show light-curve modulations suggestive of starspots and very strong X–ray and chromospheric activity. It is speculated that they are actually coalesced W UMa binaries, with the cores of the two stars now inside a common envelope. Their activity extends to their radio emission: like RS CVns, they are very strong nonthermal radio sources. About 60 stars in this class have been identified (they are generally hundreds of parsecs away) and about a dozen have now been detected by the VLA (Hughes & McLean 1987; Drake, Walter & Florkowski 1990; Rucinski 1991; Skinner 1991; Drake, Simon & Florkowski 1999). The southern FK Comae star HD 32918 = YY Men may hold the record for the largest recorded radio luminosity of any of the nonthermal active stellar sources, during a flare which reached 10^{20} ergs s^{-1} Hz^{-1} (Slee et al. 1987).

In the case of some cool giant stars, such as IRC +10216 (Knapp et al. 1994), the radio continuum emission may be produced by dust which has formed in the cool dense envelope. Drake & Linsky (1986) argue that the emission from the K III giants they detected is from an ionized wind. For the M supergiants, the emission is apparently dominated by the stellar disk. In the introduction, I noted that the sole specific mention of stars in the science proposal for the VLA was a reference to "stellar envelopes," inspired by a paper by Weymann & Chapman (1965) entitled "On the possibility of detecting stellar coronas at radio frequencies." This was an estimate of the radio flux of Betelgeuse based on the assumption that it possesses a dense wind at a coronal temperature of order 10^5 K, but with an ionization boundary at 8.5 stellar radii. This paper correctly calculates the photospheric contribution to the radio flux (from a disk of size 44 milliarcseconds at a temperature of 3150 K), but strongly overestimates the contribution of the "corona" or ionized wind, which turns out not to exist for M supergiants like Betelgeuse.

Since the stellar photosphere is a source of (roughly) fixed temperature and size, its radio flux increases with the square of the frequency, and resolution improves directly with increasing frequency, so that higher–frequency observations are very attractive for these sources. In principle the VLA can resolve the size of many M supergiants in A configuration. However, at higher frequencies the atmosphere becomes a serious impediment to such measurements, introducing phase errors which smear out the typically weak flux from these stars. In a technical tour–de–force, Reid & Menten (1997) overcame this problem in a study of long–period

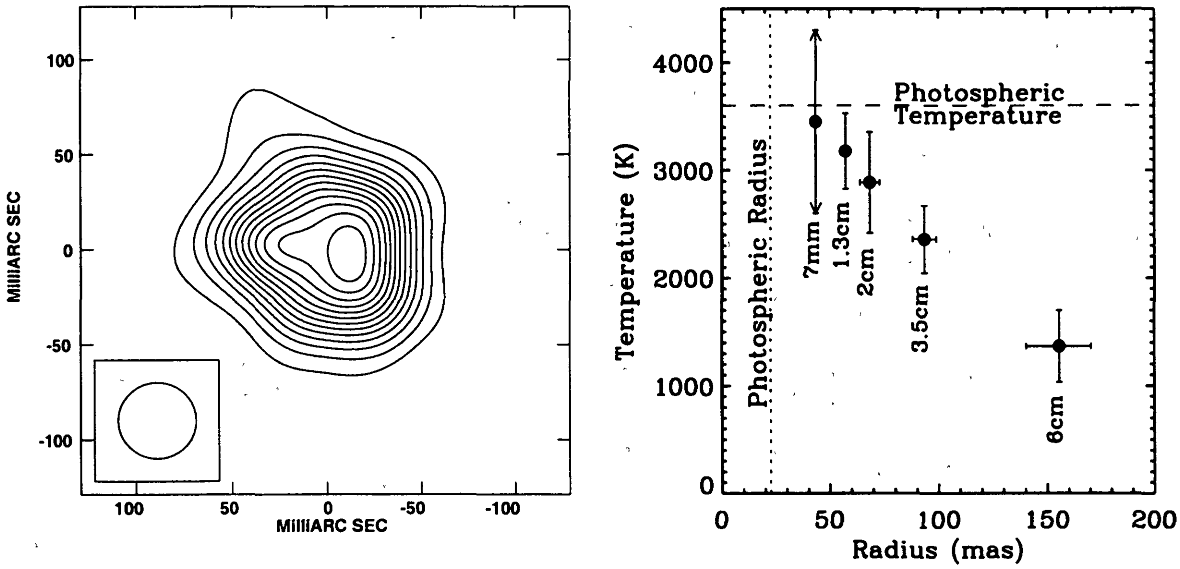

Figure 9. VLA 43 GHz image of Betelgeuse (left). The resolution is 40 milliarcseconds and the contour interval is 250 K (in brightness temperature). The peak temperature on the disk is 3450 K, which is consistent with the photospheric temperature of 3600 K. At right is shown the variation of temperature in the atmosphere with radius, determined from the VLA observations of Betelgeuse at different frequencies which resolve the source size (Lim et al. 1998).

Mira variables by using strong H_2O masers in the atmospheres of the stars: they observed the maser lines at one frequency and self-calibrated them to determine atmospheric phase corrections which they then applied to the continuum data at a nearby frequency. This yielded a source size of $0.08''$ for W Hya at 22 GHz.

With the addition of 7 mm receivers to the VLA, it has become possible to do better than just measure source size: we can now actually image the "surface" of a star other than the Sun with several resolution elements across the disk (for red supergiants the concept of a surface barely applies: their outer atmospheres are very puffy, with enormous scale heights, making them quite different from a main-sequence star such as the Sun). Figure 9 shows the VLA 43 GHz image of Betelgeuse (Lim et al. 1998), which has an optical disk size of 45 milliarcseconds. A hot spot is seen on the west side of the disk. At 43 GHz the radio disk has a size of 90 milliarcseconds, and the size increases as frequency decreases (Figure 9). Since the flux density and disk size can be measured with the VLA at a range of frequencies, the dependence of temperature on radius in the extended atmosphere can be determined, allowing stellar atmospheric models to be tested directly.

11. The Sun

We close this review with a brief discussion of VLA observations of the Sun. This topic could take up a whole additional chapter, but since it is a star and not covered elsewhere in this volume, the Sun deserves a mention here. The Sun is simultaneously an excellent and a difficult target for the VLA. Its is strong and close, making it possible to resolve small structures in the atmosphere. However, it violates all the assumptions of aperture synthesis: it has flux on all spatial scales from $< 1''$ to the solar diameter of $\sim 2000''$, and its radio emission varies on all possible timescales, from sub-second fluctuations in flares and low-frequency

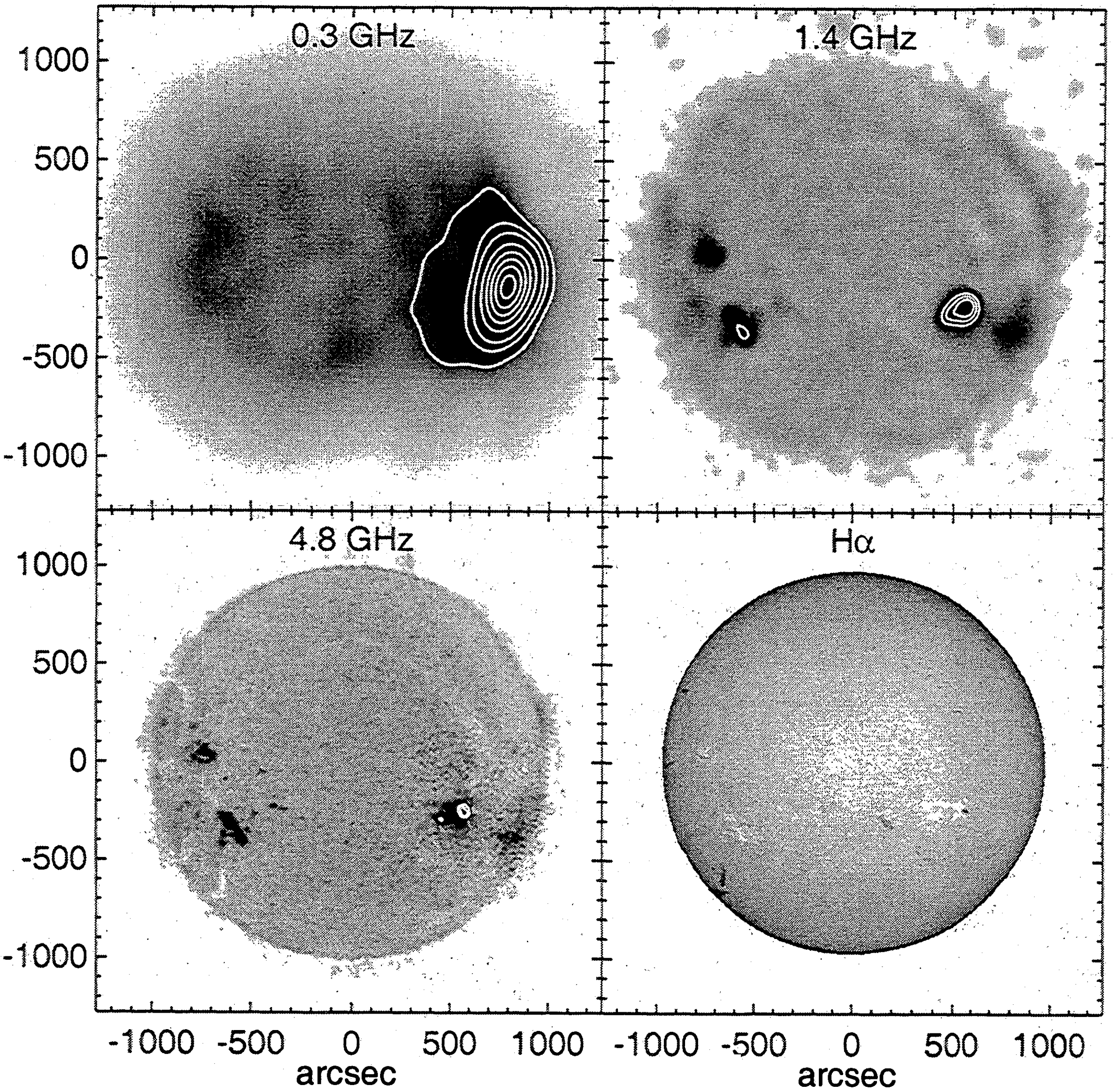

Figure 10. VLA images of the whole Sun on 1993 November 7 at three different radio frequencies, compared with an Hα image from Big Bear Solar Observatory. The 4.8 GHz image is a 26–field mosaic; both the 4.8 and 1.4 GHz images are maximum entropy deconvolutions using a disk as a starting model. The data were taken in "D" configuration: the spatial resolution is 220″ at 0.3 GHz, 50″ at 1.4 GHz and 12″ at 4.8 GHz. White contours highlighting the brightest features are plotted at brightness temperatures of 0.8, 1.2, 1.6, 2.4, 3.2, 4.0, 4.8 and 5.6 $\times$ 10^6 K. The color table saturates (is black) at 0.8 $\times$ 10^6 K at 0.3 GHz, 0.6 $\times$ 10^6 K at 1.4 GHz and 0.1 $\times$ 10^6 K at 4.8 GHz. Solar west is to the right.

radio bursts through variations on timescales of hours, such as active region evolution, out to variations on solar cycle scales. Nonetheless the VLA is still the preferred telescope for solar microwave studies for reasons of instantaneous snapshot *uv* coverage, resolution, sensitivity, frequency coverage and the fact that the primary beam includes the whole Sun at 20 cm wavelength.

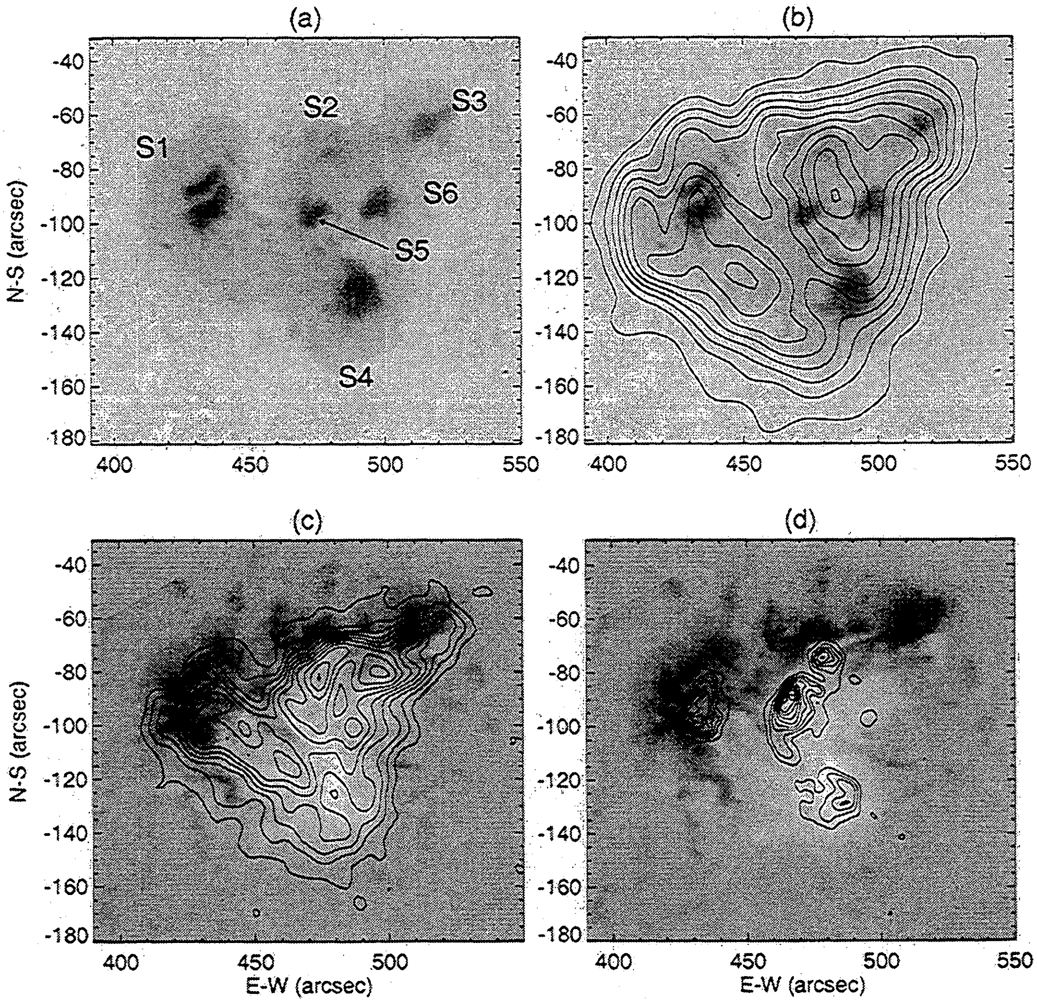

Figure 11. VLA observations of a complex solar active region reflecting the magnetic field and temperature distribution in the corona over the active region. The white–light image (a) shows a number of spots within the same penumbra. In panel (b) contours of the VLA 5 GHz emission are overlaid on the white–light image: since the radio emission is due to the gyroresonance process, the radio image corresponds to the electron temperature distribution on the surface in the corona where the magnetic field B equals 450 gauss. Panels (c) and (d) show contours of the 8.4 GHz ($B = 750$ gauss) and 15 GHz emission ($B = 1350$ gauss), respectively, overlaid on a longitudinal photospheric magnetogram which indicates the direction of the magnetic field at the surface (white = upgoing, black = downgoing field). The maximum brightness temperatures in the radio images are 4.4×10^6 K at 4.9 GHz, 4.6×10^6 K at 8.4 GHz, and 1.8×10^6 at 15 GHz, respectively. Contours begin at 10% of the maximum brightness temperature and then are 10% apart. From Lee et al. (1997).

Radio observations can be used to study virtually all phenomena in the solar atmosphere. Thanks to the properties of the main sources of radio opacity, bremsstrahlung and gyroresonance opacity, different radio frequencies can be used to probe different levels of the atmosphere. This is illustrated in Figure 10, which shows VLA images of the full Sun at three different frequencies together with an optical image. Low frequencies (< 1 GHz) are optically thick high in the corona along all lines of sight, so that the whole disk is at a brightness temperature of order 10^6 K. At these frequencies one also observes many examples of high brightness–temperature, coherent plasma emission from energetic electrons both during flares and at non–flare times as well. At frequencies between 1 and 3 GHz, the quiet corona is optically thin and one sees down below the transition region except over active regions, where high–density, hot, X–ray–emitting plasma makes the corona optically thick to thermal bremsstrahlung. At these frequencies images need large dynamic range to reveal both the features at coronal brightness temperatures (10^6 K) and those at chromospheric temperatures ($\sim 10^4$ K). At yet higher microwave frequencies, the only regions of the corona to remain optically thick are regions of high magnetic fields ($>$ 400 gauss) where gyroresonance opacity is strong; radio observations provide the only means for studying the magnetic field properties of the solar corona (e.g., Figure 11). This is an important topic since magnetic fields are believed to be involved in virtually all the major unsolved problems of the solar atmosphere: heating of the corona and chromosphere, solar flares, and active region formation. The VLA has and will continue to make important progress in our understanding of coronal magnetic fields (e.g., White & Kundu 1997).

At frequencies above about 20 GHz there are no regions of the corona which are optically thick (except possibly during the largest of all flares, which can increase the coronal emission measure by many orders of magnitude). Radio wavelengths penetrate into the chromosphere, where temperature contrasts are much smaller than in the corona. Here the value of VLA observations lies both in the spatial resolution achieved as well as in the fact that radio data provide a direct measurement of electron temperatures, whereas optical and ultraviolet observations must be compared with non–LTE radiative transfer calculations in order to make quantitative use of them.

Excellent snapshot *uv* coverage makes the VLA suitable for mapping phenomena variable on timescales from 0.1 seconds (using the high time resolution processor) upwards. The studies of flares and transient phenomena such as coronal loop brightenings, tiny flashes in the network of weak magnetic fields driven by solar convection and small coherent bursts have all benefited from the power of the VLA.

Acknowledgements

Solar and stellar radio astronomers are a demanding bunch for a telescope scheduler: we are always trying to coordinate VLA observations with some X–ray satellite or other, or with some ground–based observatory with an impossible schedule, and sending Barry e–mails at the last minute with desperate requests for a change. Barry has put up with these demands for many years and although we don't always get what we would like, as long as other commitments permit it, Barry has tried to schedule VLA time to make these multi–wavelength campaigns a success, including allocating system time when no other option was possible. We are all very grateful to Barry for his herculean scheduling work, as well as all the other features of the VLA which he had a hand in and which make it the world's premier radio telescope. I also thank Manuel Güdel for his advice on reddening corrections. Solar radio astronomy at the University of Maryland is supported by NSF grant ATM 96-12738 and NASA grants NAG 5-7901, 5-7232 and 5-6257.

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The VLA in Galactic Center Research

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The Galactic Center (GC) presents an unusual assortment of source types and radiation processes, with both stellar and interstellar sources, thermal and nonthermal continuum emission, and all the radio spectral lines that can be studied anywhere else. As such, it gives to the casual observer a sense of forbidding complexity. However, recent research, largely with the VLA, has led to a set of plausible and coherent paradigms that draw a great deal of order out of the apparent complexity. It is noteworthy that this subject is clearly near and dear to the hearts of past and present NRAO researchers, and that they have been at the forefront of research on the GC; indeed, the bibiliography below illustrates the unusual frequency with which several of their names reappear in the literature on the subject.

In what follows, I present a brief historical sketch of radio observations of the GC, with some emphasis on interferometry, and an account of early observations of the GC with the VLA (a related overview of radio observations of the GC was published by Liszt 1988). Then, rather than trying to describe the plethora of VLA observations which have been made of the GC, I touch upon an incomplete[1] set of recent VLA projects which, in my judgement, are pushing the frontiers of GC research. First, the reflection of a VLA user on the particular challenges one faces in getting on the schedule.

1 Scheduling Galactic Center Time on the VLA

Ron Ekers gave the statistic that, between 1980 and 1991, only 3% of VLA observing time was allocated for the GC. My first reaction to hearing that was that it was inconsistent with Barry Clark's frequent claim that there is a great deal of pressure on GC time at the VLA, which constrained what projects could be scheduled. At least, that is what he has always told me when justifying the amount of time I am given. However, further reflection reveals that several considerations affect this statistic. First, the southern declination of the GC (-29^o) leads most observers to propose the hybrid arrays, which are scheduled for only a few weeks per trimester. Secondly, at this declination, the GC is only at adequate observing elevations for 7–8 hours per day, a further time constraint. Thirdly, GC researchers (and I count well over a dozen of them at this gathering) should not be chauvinistic; the rest of the inner Galaxy needs to be scheduled at about the same time (and Juan Uson subsequently told me that, in compiling the time allocation statistics, the AOC includes only Sgr A and Sgr B2 under the rubric of "Galactic Center," meaning that many GC projects are categorized as "Galactic Astronomy" instead (in my opinion, the GC arena really extends to an angular radius of 2^o). Finally, I would also estimate that the fraction of time allocated to GC research probably has increased, not only between 1980 and 1991, but also in the time since then, reflecting a growing interest in this subject. In fact, Barry Clark reports an increase in the VLA time allocated to GC projects to 3.75% between 1991 and 1999. Indeed, GC time is in great demand, and justifiably so, given the large number of very interesting questions raised about that region, questions that the VLA can address.

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[1] for example, to delimit the scope of this discussion, I don't mention the vast literature based on VLA observations of Sgr B2 or other GC HII regions such as Sgr D & E, and little mention is made of the important HI and molecular line observations of GC sources.

2 A Brief Radio History

The dominant GC radio source, Sagittarius A, was first identified by Piddington and Minnett in 1951, observing at 1210 MHz with an antenna having a beam size of 2.8 degrees. The observation that brought Sgr A to the full attention of the worldwide scientific community was that of McGee and Bolton (1954), who observed at 400 MHz with an 80-ft paraboloid. This early history of radio observations of the GC has been well documented in a recent paper by Goss & McGee (1996), so I won't repeat it here. Another milestone was reached with a 1959 observation by Frank Drake, using the 85-ft telescope of the NRAO. In his 3.75 cm observation, one clearly sees the distinct Sgr A, B, and C radio sources, and in addition, one can note the northern "appendage" to Sgr A, the hook-shaped object we now know as the Radio Arc. (Single-dish observations of the Radio Arc reached their culmination with the 77" resolution observation by Pauls et al. (1976) using the 100-m Effelsberg telescope, and already in this observation, the structure of this object was evidently quite unusual.)

During the 1960s and beyond, single-dish observations provided increasingly detailed radio images of the GC. An examination of extant observations carried out by Downes and Maxwell (1966) was used to locate the center of SgrA in the (then) new l^{II}, b^{II} coordinate system to within about a minute of arc, and to determine that Sgr A itself is a nonthermal source, while the other prominent sources in the region are thermal. Single-dish observations after that came from many quarters, including the 64-m Parkes radiotelescope (4.1' beam at 5 GHz; Whiteoak & Gardner 1973), the 36-m Haystack antenna (133" beam at 15.5 GHz; Kapitzky & Dent 1974), the 100-m Effelsberg telescope (2.4' beam at 5 GHz and 1.2' at 10.7 GHz; Altenhoff et al. 1978; Seiradakis et al. 1985), and the 45-m Nobeyama Radio Observatory (2.6' beam at 10.5 GHz; Sofue & Handa 1984; Handa et al. 1987). [This list is not meant to be exhaustive, and the low-frequency observations are mentioned separately below.] While the gross features of all of the large-scale structures were well represented in these surveys, the typical resolution of these observations (1' = 2.5 pc at a distance of 8 kpc) precluded a clear determination of the detailed nature of these structures.

Interferometry began to be applied to the study of the GC during the 1970s. Using the Cambridge 1-mile telescope at 2.7 and 5 GHz (11" & 6" beams, respectively), Downes and Martin (1971) were the first to distinguish clearly between Sgr A East and West, and they noted that these distinct sources are nonthermal and thermal, respectively, on the basis of their falling and flat spectra (the Owens Valley Interferometer was also used by Ekers & Lynden-Bell [1971] to study this region at 5 GHz). Subsequently, several investigators produced 2-dimensional images showing Sgr A East and West in increasing detail (Sandqvist 1974 [using lunar occultations]; Whiteoak et al. 1974 [aperture synthesis with the Owens Valley interferometer]; Balick & Sanders 1974 [using the 3-element NRAO interferometer at 2695 and 8085 MHz]). In a study by Ekers et al. (1975), a full 5-GHz synthesis map was produced by combining data from Westerbork and the 90-ft telescopes of the Owens Valley Interferometer, with a resultant resolution of 6.3" x 34". Those authors confirmed the nonthermal and thermal natures of Sgr A East and West, respectively.

During this epoch, SgrA* was discovered with the NRAO interferometer by Balick and Brown (1974). This compact source is buried in the relatively strong emission of Sgr A West, so was not recognized until relatively long baselines were used, for this experiment, the interferometer had just been augmented with an outrigger antenna located 35 km away, giving the resolution needed to pull SgrA* out of the background at 8.1 GHz. It is interesting that much longer baseline VLBI measurements did not detect SgrA*–its apparent size due to scattering is rather large, and thus it is resolved out with intercontinental baselines. The significance of this finding was immediately appreciated; all groups had been seeking such a compact, nonthermal source as the expected manifestation of a massive black hole, following suggestions like that of Lynden-Bell & Rees (1971) that central black holes with such characteristics might be relatively common in galactic nuclei, even in somewhat inactive nuclei like our own.

The most ambitious pre-VLA interferometric study was done by Downes et al. (1978), using the Westerbork Synthesis Radio Telescope. Mapping a large region around the GC, and several of the known HII regions, this group found numerous compact radio sources, many of which have been subsequently studied by others. Because of a lack of short spacings, they were not sensitive to extended structures, so for example, the Radio Arc manifested itself as no more than a cluster of small sources coincident with some of the maxima of the extended emission, and the knots in the extended thermal structure, G0.18-0.04.

3 Early VLA Results

The GC was notably not among the first objects of study with the VLA. The opinion was often expressed during the first 5 or 6 years of operations that the unusually strong and extended emission would present a major challenge for aperture synthesis, because of aliasing and because of the likelihood of strong emission in the sidelobes of the synthesized beam pattern located outside the map area. Indeed, such problems have been recognized in practice, but they have been less severe than some of the early predictions might have indicated. During the 1980s, these problems were largely overcome as the increases in disk space and computer speed permitted large maps to be made routinely – maps that went beyond the primary beam at any desired resolution. In the first four VLA studies, progressively larger regions about the GC were taken on, and the spatial resolution on Sgr A West was pushed to unprecedentedly small scales.

The first VLA study of the GC was enacted by Brown, Johnston & Lo (1981), who observed the inner arcminute with 2" by 8" resolution at 6 cm. They noted that, at this wavelength, Sgr A West has a structure similar to that in the mid-infrared, at 10 μm. The good correspondence between mid-infrared emission and thermal radio continuum emission in Sgr A West is still upheld in present maps, even with sub-arcsecond resolution (except, of course, for cold stellar objects), indicating that dense, free-free emitting gas and warm dust are coextensive. There is, of course, much science to be extracted from the subtle differences.

The second VLA study of the GC was carried out a few years later by Ekers et al. (1983), who identified most of the significant continuum components in the Sgr A complex still being studied at the present time, including the 4 components of the Sgr A East HII regions, G-0.02-0.07 (which had been seen as a single source by Downes et al. 1978; their source 13). They also produced the first detailed spectral index map of the Sgr A complex. Their images of Sgr A East detailed it nicely as a shell source surrounding (but not centered on) Sgr A West, so raised the possibility that Sgr A East was produced by a centrally located supernova or by some energy release from a hypothetical, exotic central object. Their images also revealed, with 2" x 3" resolution, unprecedented structure in Sgr A West; the now well-known "mini-spiral" was clearly shown for the first time, and SgrA* was seen for the first time with the VLA.

Soon thereafter, Lo & Claussen (1983) presented 6 cm VLA observations of SgrA West with a resolution of 1" x 1.3", showing even finer scale structure in the three arms of the triskelion spiral. Noting that the 3 arms do not meet at a single point, and using the kinematical information obtained from the NeII observations of Lacy et al. (1982), Lo & Claussen argued that the plasma arms of SgrA West consist of matter infalling at a rate of $\sim 10^{-3}$ $M_\odot$ yr^{-1}. For the first time, SgrA* was seen as a distinct point source clearly separated from the extended thermal emission from SgrA West.

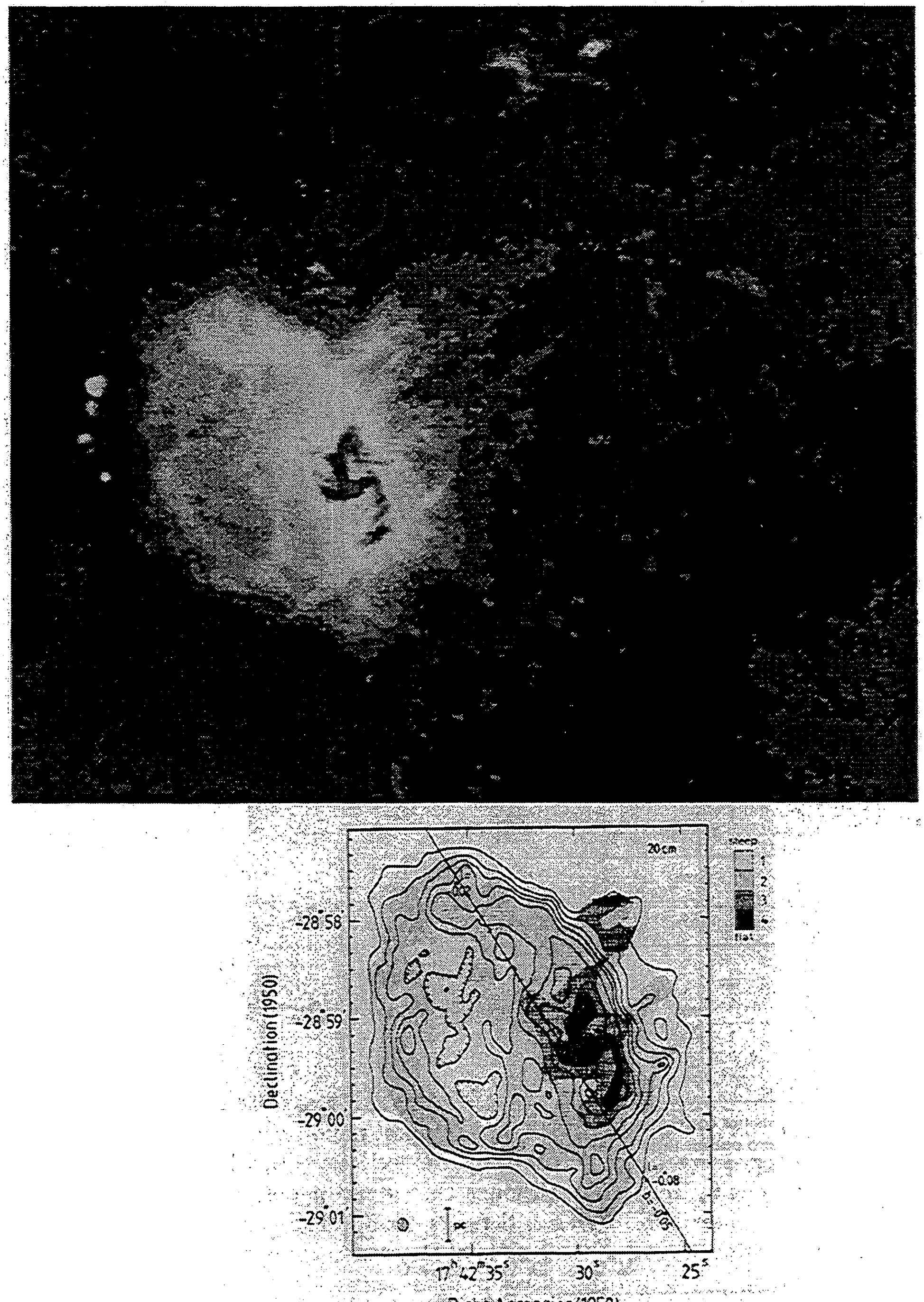

Figure 1. Top: 6 cm VLA image of the SgrA complex (Yusef-Zadeh & Morris 1987a). Bottom: The spectral index distribution for SgrA East and West, as reported by Ekers et al. (1983).

Figure 2. The original 20 cm image of the Radio Arc, with Sagittarius A dominating the flux sef-Zadeh, Morris & Chance 1984). The Galactic plane runs through SgrA and lies at PA=31°.

The fourth early VLA study of the GC was that of Yusef-Zadeh, Morris & Chance (1984), who, motivated by the numerous compact radio sources found at Westerbork by Downes et al. (1978), and presuming that many of them might be compact HII regions, set out originally to study star formation in the remarkable radio Arc structure. Was this vertical radio feature a wall of newborn stars provoked into formation by a very-large-scale shock? (Vast GC explosions and large-scale outflows were very much in vogue at that time [c.f., Oort 1977], although the current paradigm focuses more on the long-term inflow of gas toward the center [e.g., Morris & Serabyn 1996]). The first observation we did, with an expanded configuration, was reminiscent of the results of Downes et al., but when we returned to acquire data with a compact array, the extended structures became apparent. At first, the multiple, parallel filaments of the radio Arc (see Figure 3, for example) looked very much like an artifact -- a bad visibility datum that had not been properly edited. Farhad Yusef-Zadeh, a graduate student at the time, worked long and hard at the VLA site to edit the data set, but the "stripe" in the map persisted. It was only when he set aside the 20 cm data to work with 6 cm data acquired in the same program, that it became evident that the same structures are present at both frequencies, hardly something that could result from a conspiracy of bad data. The polarized, and therefore nonthermal filaments (NTFs) of the Radio Arc were interpreted as magnetic flux tubes illuminated by the synchrotron emission from the relativistic electrons trapped within them, a hypothesis well supported by subsequent data. This study led to the suggestion that the magnetic field geometry of the GC is poloidal, in contrast to the rest of the Galaxy.

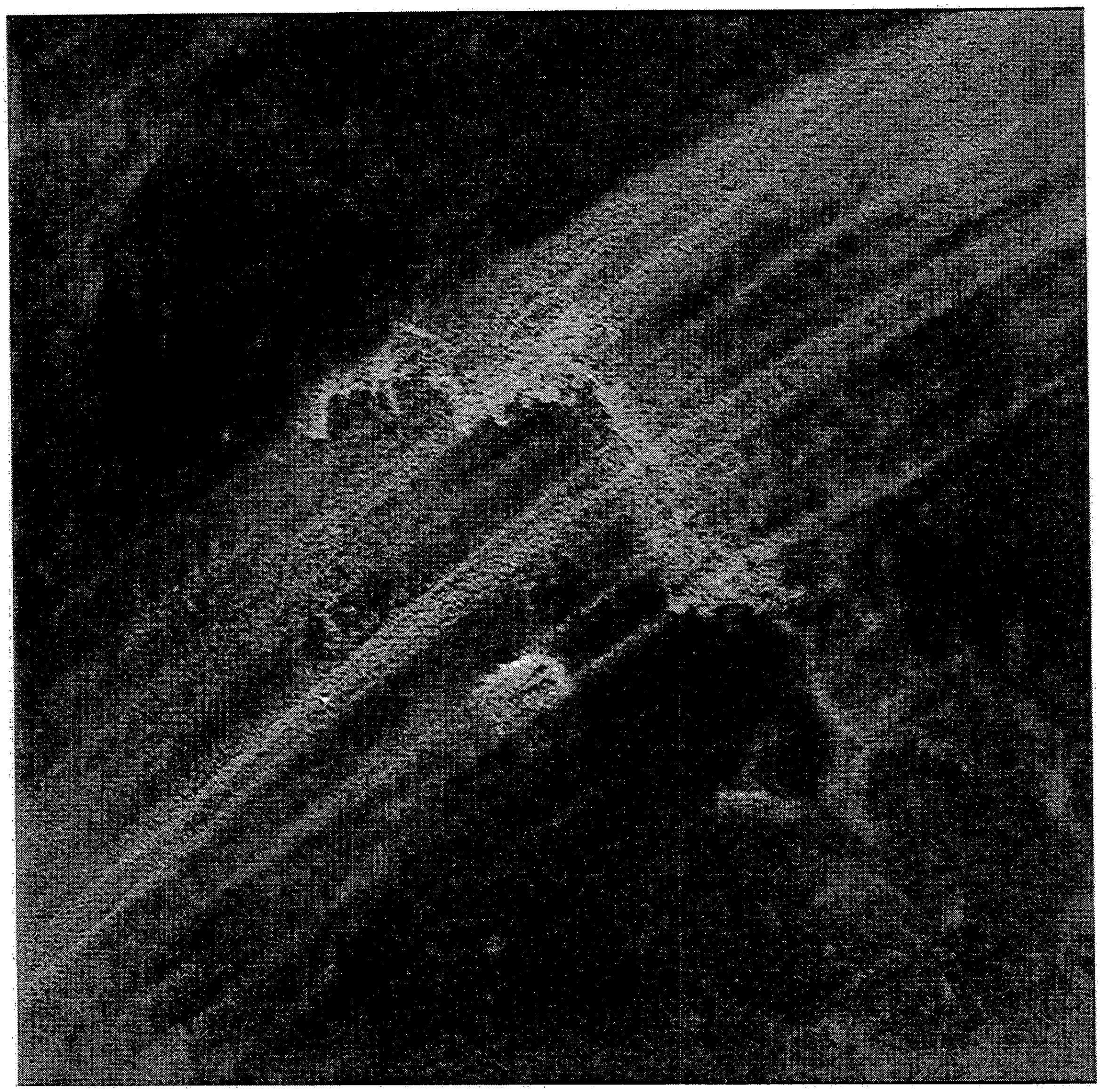

Figure 3: G0.18-0.04, the heart of the Radio Arc, where the nonthermal filaments cross through the HII region, the Sickle, imaged here at 6 cm with the VLA (Yusef-Zadeh & Morris 1987, with the addition of subsequent data).

4 Some Recent Directions in Galactic Center Research

4.1 Radio Filaments

One of the most interesting breakthroughs attributable to the VLA is the finding of numerous filamentary structures within the inner degree of the Galaxy. Following the revelation that the Radio Arc contains numerous parallel, nonthermal filaments, several other filaments were found, all much more isolated than the filaments in the Arc. Two isolated radio "threads" were reported by Morris & Yusef-Zadeh (1985; a recent VLA study of these magnetic structures has just been prepared by Lang et al. 1999b, both in the positive latitude and longitude region near the center. A single filament associated with the HII region Sgr C is well-characterized in VLA images published by Liszt (1985) and Liszt & Spiker (1995; these works also show the HII region in spectacular detail). Other NTFs include G359.54+0.18 (Staguhn et al. 1998 and references therein), and the "Snake" (G359.1-00.2; Gray et al 1991; 1995).

A review of the NTFs was given recently (Morris 1996), so here I summarize only some of their most salient features. They are polarized, magnetic structures, with the magnetic field oriented predominantly parallel to their length (Tsuboi et al. 1986; Yusef-Zadeh, Wardle & Parastaran 1997, Lang et al. 1999a,b). One of their primary hallmarks is their uniformity of brightness and curvature over large scales; that is, despite being in the tumultuous GC environment, they appear almost entirely unperturbed. Yusef-Zadeh & Morris (1987b) have interpreted this observation in terms of the rigidity of the magnetic field lines, and have therefore deduced that the magnetic field strength in the filaments must be at least 1 mG. The Snake is an exception; its kinks may well be caused by interactions with interstellar density and velocity inhomogeneities, implying that the field at its large distance from the center (at 135 pc, this is the most distant filament) is beginning to decline. [Benford (1987) offers an alternative explanation for the kinks in the Snake.]

Curiously, almost all of the NTFs are located at positive Galactic latitudes, or in the Galactic plane, with the exception of a few filaments reported by Yusef-Zadeh et al. (1990), which are located East of the Radio Arc. Until recently, all filaments found (about 8 systems) were roughly perpendicular to the Galactic plane, and this had been interpreted in terms of a pervasive dipole magnetic field throughout the inner few hundred parsecs, illuminated here and there by the synchrotron emission from locally injected relativistic electrons. Very recently, however, a nonconforming filament oriented roughly parallel to the Galactic plane was found with the VLA operating at 90 cm. Subsequent 20, 6, and 3.6 cm VLA observations confirmed that this filament, "The Pelican," shares the same characteristics as the rest of the GC NTFs, with strong linear polarization and an intrinsic magnetic field orientation that follows the orientation of the filament itself (Lang et al. 1999a). Its large projected distance from the GC (~225 pc) will help delimit the size of the region within which the field orientation is perpendicular to the plane, an important constraint for theories purporting to account for a pervasive vertical field at the GC (e.g., Chandran, Cowley & Morris 1999).

The origin of the NTFs is still an open question. Serabyn & Morris (1994) have noted that every sufficiently well observed filament is associated with an HII region at the surface of a molecular cloud somewhere along its length (this has been upheld since: see Uchida et al. 1996 and Staguhn et al. 1998), and they suggest that the MHD interaction between the surficially ionized cloud and the strong ambient field accounts for the electron acceleration required to produce the filaments. Testing of this hypothesis is under way, and will involve a detailed examination of such interfaces, making use of both the VLA and the OVRO millimeter interferometer.

Not all GC filaments are seen in polarized emission. Another class of filaments, still awaiting detailed study, consists of filaments evidenced in the Faraday rotation caused by the magnetized plasma situated between polarized sources and the observer. Since the rotation measure depends on the line-of-sight integral of the product of the line-of-sight component of the magnetic field and the electron density, a magnetized plasma filament will show up as a linear enhancement in the rotation measure. The rarity of polarized background emission screens makes this kind of filament difficult to find, but in front of the extended, polarized emission from the radio Arc, such filaments have been noted. In the data of Inoue et al. (1989), these show up as filamentary depolarization structures, because the rotation measure varies so rapidly across these narrow structures that the polarization averages to zero within a single resolution element. It is interesting that these prominent depolarization structures have very inconspicuous counterparts in the total intensity images.

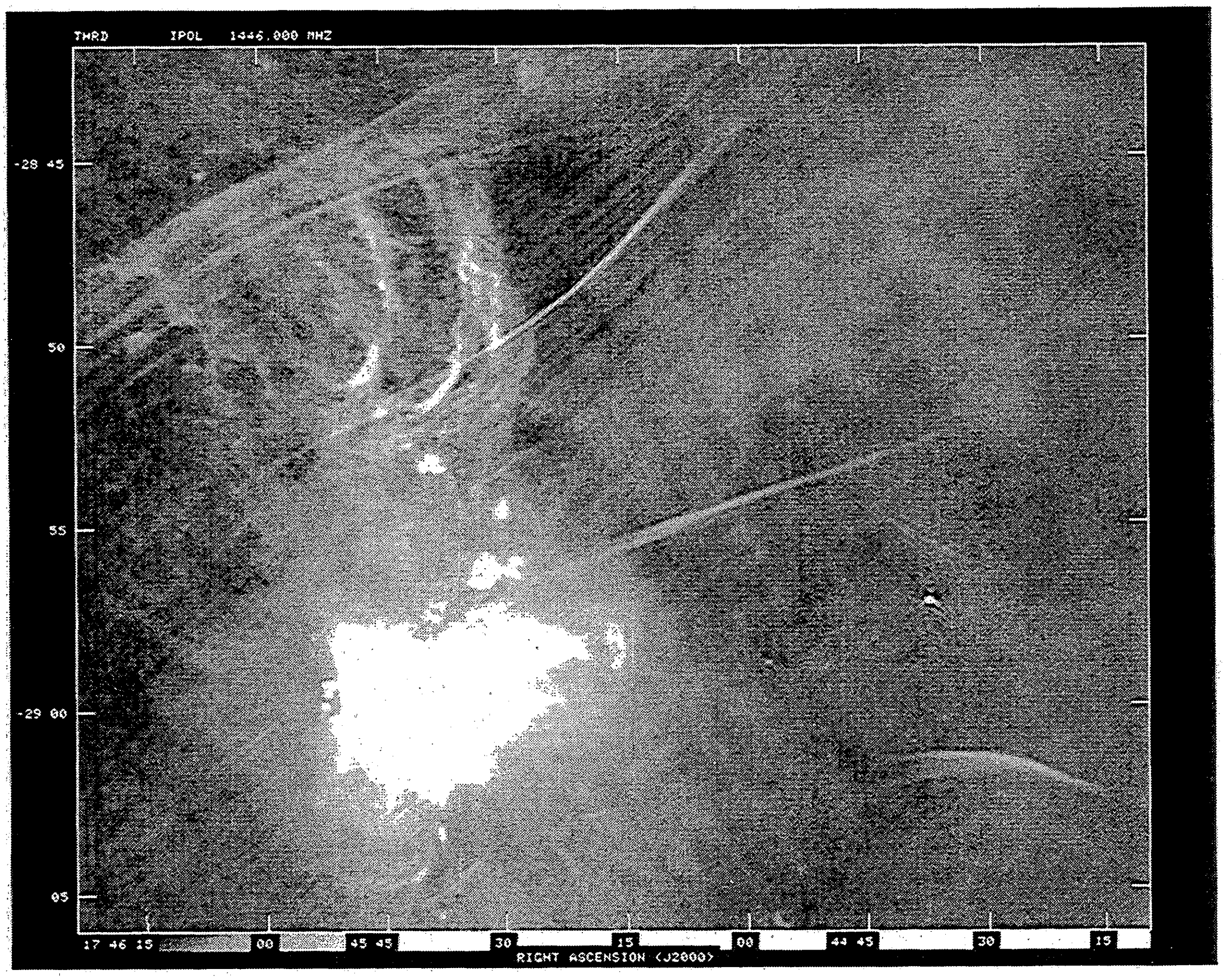

Figure 4: The Northern and Southern Threads, imaged at 20 cm with the VLA (Lang et al. 1999b). SgrA and the Radio Arc are prominent at the left. Numerous other linear features ("streaks") are also visible. The Galactic plane is oriented at position angle 31°, so it is roughly perpendicular to all the NTFs in this image.

4.2 Dynamics

4.2.1 Radio Recombination Lines

Observations of radio lines have been used in many cases to probe the dynamics of gas near the GC. Thus, radio recombination lines, 21-cm HI emission and absorption, and molecular lines (including NH_3, H_2O, OH, & H_2CO) all have been observed with the VLA toward GC sources. Here, we mention only observations of radio recombination lines (RRLs) in Sgr A and the Radio Arc, which have been, and will continue to be, essential for understanding gas flows in this complex region.

The first report of RRLs in the GC was by van Gorkom et al. (1985), who observed the H76α and H110α lines in Sgr A West. They noted that the large linewidths that had earlier been observed with single dishes were largely a result of unresolved, ordered, large-scale motions. Within their spatial resolution, they noted that the ionized gas kinematics agreed with what had been found from 12.8-μm [NeII] line observations. They also found that the electron temperatures in the "bar" – the part of Sgr A West which appears to be closest to the dynamical center of the Galaxy – are unusually high. In their follow-up paper (Schwarz, Bregman & van Gorkom 1989), this group achieved a spatial resolution of 4.5" x 7.5" and a velocity resolution of 32 km s^{-1} with the H76α line, and noted that the GC is surrounded by an ionized ring rotating at 100 km s^{-1} (the inner edge of the circumnuclear disk). This implies a mass of 2.5×10^6 $M_\odot$ within 1.3 pc, close to the current value of the central black hole mass, though a little shy if one accounts also for the stars, and it led them to suggest that the source of ionization must be centrally located.

Publishing his VLA-based thesis work, Doug Roberts, working with Miller Goss, provided the highest resolution (1") view of the kinematics of Sgr A West, by observing the H92α line. Roberts and Goss (1993) also corrected the previously incorrect high electron temperatures found in the "bar" region, by finding that most of the emission in this region of Sgr A West arises from velocities at -250 km^{-1}, which were not covered in the first observations. Their expanded velocity coverage resulted in strong detections of the H92α line, and electron temperatures over Sgr A West were found to be ~7000 K, consistent with T_e* for ionized gas at the GC. Among other things, they modelled the "Western Arc" (the inside edge of the circumnuclear disk) as half of a 1-pc radius ring in circular rotation about the center and improved on the model of Schwarz et al. (1989), getting an interior mass of 3.5×10^6 $M_\odot$. They also used the absence of Zeeman splitting to set an upper limit on the magnetic field strength in the Northern Arm at 15 mG, an interesting limit, considering that mid-infrared polarization studies had led to magnetic field estimates of this magnitude. Later, Roberts, Yusef-Zadeh & Goss (1996) studied the dynamics of gas in the "mini-cavity", a feature in the bar very near SgrA*, first pointed out by Yusef-Zadeh, Morris & Ekers (1989). The velocity gradients are enormous around this cavity in the plasma of the bar, largely in response to the gravitational field of the central black hole. Roberts et al. (1996) present a model orbit for this gas, arguing that it is part of a larger, partially neutral structure.

High-resolution VLA observations do not reveal some of the more interesting extended structures. Yusef-Zadeh, Zhao & Goss (1995) observed the H110α line in Sgr A West with 11 x 20" resolution, and detected several new kinematic components whose kinematics deviated strongly from circular motion. Even outside of SgrA West, they detected RRL emission from the "streamers": thermal structures oriented perpendicular to the circumnuclear disk which may represent gas flow emanating from the interior edge of that disk (Yusef-Zadeh & Morris 1987a).

The Radio Arc has been another popular target for RRL observations with the VLA. The first report (Yusef-Zadeh 1986; Yusef-Zadeh, Morris & van Gorkom 1987) showed striking, large-scale velocity gradients along the thermal arched filaments, and this is now being followed up by Cornelia Lang and her collaborators. The "Sickle" HII region (G0.18-0.04), lying at the surface of a molecular cloud situated where the NTFs of the Radio Arc cross through the Galactic plane, has also been widely studied with RRL's, perhaps because it has been suspected of playing a key role in shaping the Radio Arc. Yusef-

Zadeh, Morris & van Gorkom (1989) reported observations of this region, focussing on the unusually wide recombination lines from the nearby "Pistol nebula," G0.15-0.05. This nebula, discovered with the VLA, is now understood to consist of the ejecta from a massive, luminous blue variable star, the "Pistol Star," which has been heralded as one of the most luminous stars known (Figer et al. 1998). A much more thorough RRL observation of the Sickle and Pistol was reported by Lang, Goss & Wood (1997), including an important detection of the He92α line in the Pistol Nebula. Examining the complex velocity field of the ionized gas in G0.18-0.04, they suggest that there may be an interaction of the gas and the NTFs at positions of intersection. They also consider that the winds from the Quintuplet cluster, which is presumably the source of ionization for this region, may also affect the velocity field. Taking a low-resolution view of G0.18-0.04, Yusef-Zadeh, Roberts & Wardle (1997) found two new kinematic components at anomalously high velocities (-35 and $+150$ km s^{-1}, compared to the mean RRL velocity of $\sim$35 km s^{-1}). They interpret this in terms of the acceleration of gas away from the HII region interface at the surface of the underlying molecular cloud either by the stellar winds from the Quintuplet stars, or by magnetic field pressure associated with the NTFs.

Nearby the Radio Arc, lying between it and the GC, is a group of HII regions lying in the same forbidden velocity cloud that underlies the arched filaments of the radio Arc (Serabyn & Güsten 1987). Zhao et al. (1993) studied the RRLs from these discrete sources, collectively known as G0.04+0.03, and confirmed that they are probable sites of star formation within this cloud. How stars might have formed in this sheared, tumultuous cloud is an interesting puzzle, so these objects warrant continued study.

4.2.2 Proper Motions of Gas Features

Another approach to the determination of gas dynamics is to literally measure the lateral motions of gaseous structures. Because such structures are typically extended, and often lacking in extremely fine-scale structure to which to assign an accurate position, this has always been regarded as a very difficult operation. However, two groups have lately succeeded in measuring proper motions of thermal radio continuum sources by observing over a long time base at short wavelengths (Zhao & Goss 1998 at 1.3 cm, with a 5-year time base, and Yusef-Zadeh, Roberts & Biretta 1998 at 2 cm with a 9-year baseline), and by taking advantage of the very high velocities, hundreds of kilometers per second, in the central parsec. Yusef-Zadeh et al. (1998) find proper motions throughout SgrA West, and, after combining their results with radial velocity measurements, conclude that the ionized gas in the northern arm and some other features is not gravitationally bound to the Galaxy, and that the gaseous and stellar systems are therefore uncoupled. Zhao & Goss (1998) find proper motions for 57 HII components in a smaller region, and the agreement with Yusef-Zadeh et al. seems to be good for sources in common. Examining the "mini-cavity" near SgrA* at 7 mm as well as at 1.3 cm, Zhao & Goss detail its structure and show that it is expanding at $\sim$200 km s^{-1}. They also raise the point that wave motions, rather than material motions, may account for some of the apparent proper motions. This fresh, new area of inquiry is clearly one that will be attracting a great deal of future attention.

4.3 Stellar Masers

While bright stars are, in principle, among the best probes of galactic structure, the huge extinction to the GC (30 visual magnitudes) makes direct observation of stars very difficult, except in the radio. A group led by astronomers at the Leiden Observatory recognized early the value of OH/IR stars as probes of stellar density and velocity distributions, and used the VLA to infer the Galactic mass distribution (Habing et al. 1983). This group later returned, in the context of Michael Lindqvist's Ph.D. research, to carry out a larger survey involving 6 fields at 18 cm, all within about 100 pc of the center, and covering a velocity range of ±217 km s^{-1}, which required 3 frequency settings (Lindqvist et al. 1992a). All told, they found 134 *bona fide* double-peaked OH/IR stars. In a follow-up paper, they offered a particularly

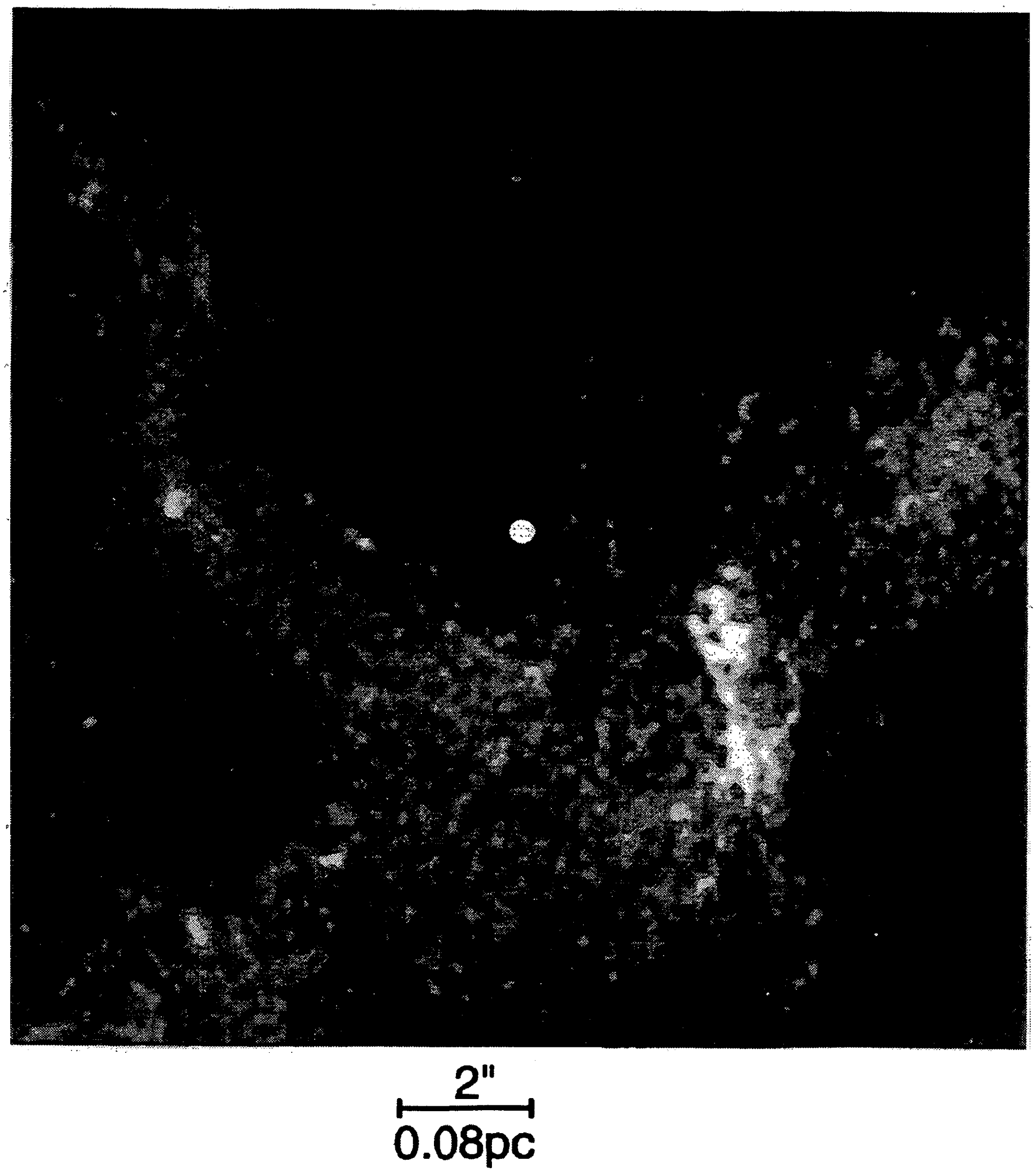

Figure 5. High-resolution (0.1") λ1.3-cm continuum image of the central portion of SgrA West, made in the proper motion study of Zhao & Goss (1998). Note the location of SgrA* (the unresolved white spot) outside of the thermal emission. The mini-cavity, where some of the proper motions have been measured, is located 2" to the southwest of SgrA*. The IRS2 and 13 regions are located on the bright, western rim of the minicavity. IRS7 (top, middle) is detailed in Figure 9.

revealing analysis by dividing the sample according to the separation of the two OH peaks (i.e., in terms of the outflow velocity, v_{exp}, of the masering circumstellar envelopes). According to the lore of mass-losing evolved stars, the expansion velocity is statistically related to stellar mass, and thus to age. Lindqvist et al. (1992b) found that both populations, divided at $v_{exp} = 18$ km s^{-1}, displayed galactic rotation (Figure 6), but that the stars with larger expansion velocities have the smaller dispersion in both radial velocity and galactic latitude, consistent with their being a younger population formed in the rotating disk, but not yet dramatically diffused in phase space by scattering processes. The radial velocities measured in this study gave the best measure of the Galactic mass distribution on scales of 10 to 100 parsecs. Another Ph.D. student at Leiden, Huib Jan van Langevelde, used the VLA to find a few very high velocity stars (−309 and −355 km s^{-1}). At first sight, these velocities (and that of one other such star, found earlier) are surprising for motions in a spherical potential, but these stars can apparently be interpreted as bulge stars on elongated orbits passing close to the GC.

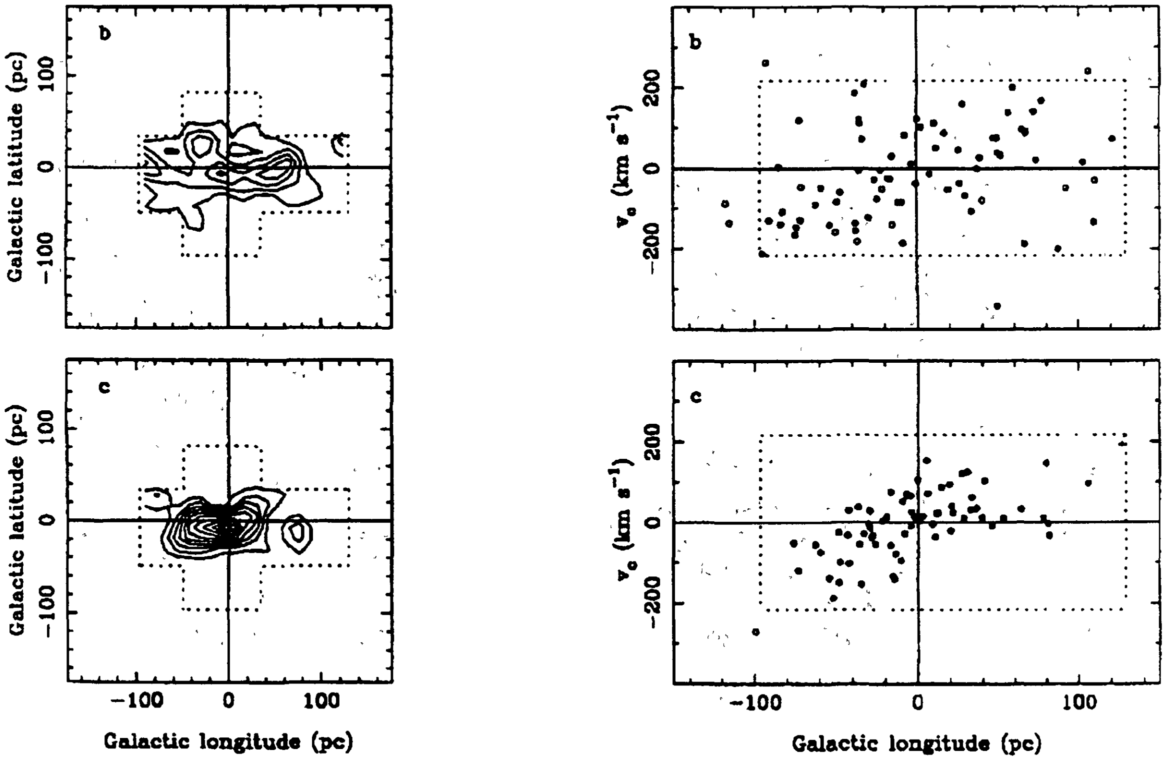

Figure 6. Left: Surface density distribution of OH/IR stars at the GC for those having $v_{exp} < 18$ km s^{-1} (top) and $v_{exp} > 18$ km s^{-1} (bottom). Right: Longitude-velocity distributions for the same two groups (Lindqvist, Habing & Winnberg 1992b).

In another portion of his thesis work, van Langevelde used the VLA to monitor GC OH/IR stars. With observations at 20 epochs spanning 3 years, van Langevelde et al. (1993), followed the periodic intensity variations of 37 sources. With this data, they were able to derive the phase lag between the front and back of the envelopes because of the different light travel times, and thus to determine the sizes of the maser regions. They also concluded that long-period OH/IR stars are relatively less abundant in the GC, compared to those with shorter periods, than those in the disk.

The push for better statistics in stellar dynamics using OH/IR stars has continued with the surveys by Sevenster et al. (1997), using the Australia Telescope (ATCA) to survey the bulge, and Sjouwerman et al. (1998), using both the VLA and ATCA. The latter survey has unearthed a total of 155 OH/IR stars within a projected distance of 40 pc of the center, so is likely to lead to an improved estimate of the mass distribution on such scales. Among other generalizations, Sjouwerman et al. remark that the distribution of expansion velocities in the GC is different than that of any other OH/IR sample known, and they attribute this to the expected higher metallicities in the GC.

Other maser lines are also being used to probe the stellar dynamics of the GC. More than 100 water masers have been found or remeasured with the VLA in a region encompassing ±2° in both longitude and latitude by Levine et al. (1999, following the initial study by Taylor et al. 1993). This sample is complex, because it consists of both masers from circumstellar envelopes around evolved stars and masers from protostars and shocks in regions of star formation. In general, the two groups can be distinguished on the basis of infrared colors. There is a small overlap with the OH/IR star sample (the OH/IR stars have not yet been systematically observed for H_2O masers), but most of the H_2O masers are apparently associated with star formation. Thus, with sufficient statistics, the H_2O masers may ultimately outline Galactic-scale shocks and thus be a probe of large-scale gas dynamics.

SiO masers present some exciting possibilities for the VLA. SiO masers from evolved stars are being observed in the bulge and at the center with single dishes, primarily the Nobeyama 45-m telescope (Izumiura et al. 1995; Izumiura, Deguchi & Fujii 1998), but they can readily be detected at the GC with the 7 mm system at the VLA. Menten et al. (1997) found several stellar SiO masers near SgrA*, and since the masers have near-infrared counterparts, they were able to use their observation to locate Sgr A* in the infrared reference frame. This observation has been extremely valuable in the context of stellar proper motions studies, because it has placed Sgr A* squarely (within the now rather small uncertainties) at the inferred location of the central black hole.

4.4 Sgr A*

Considering that the intrinsic size of SgrA* is completely unresolved with the VLA, I find it interesting to note how much VLA time has been devoted to the study of various aspects of this object. Investigatgors have explored its spectrum, its time variations, its apparent size, its proper motion, and its polarization. Of course, the interest in this object has long been based on the likelihood that it is the radio counterpart of our Galaxy's centrally located supermassive black hole, and recent near-infrared studies have all but confirmed that expectation (Eckart et al. 1997; Ghez et al. 1999).

4.4.1 Spectrum of SgrA*

The VLA has been used along with many other radiotelescopes to show that the spectrum of SgrA* rises through the centimeter spectrum, with flux density roughly proportional to $\nu^{0.25}$ (e.g., see Zhao et al. 1992; Serabyn et al. 1997; Falcke et al. 1998). On top of that, there is a near-millimeter/submillimeter excess of about a factor of two above the extrapolation of the centimeter spectrum, likely owed to a separate, synchrotron self-absorbed emission component. The variability of SgrA* complicates the determination of the spectral shape, so the most recent studies have endeavored to make the measurements across a broad spectral region as simultaneous as possible (Serabyn et al. 1997; Falcke et al. 1998). The spectrum is extremely important for constraining models for SgrA* (Falcke & Melia 1997; Narayan et al. 1998), and at the present time, the emphasis (and challenge) is at the shortest wavelengths, where information on the most compact structures can be obtained.

4.4.2 Variability of SgrA*

SgrA* is variable at all wavelengths, but the causes vary across the spectrum. Zhao et al. (1989) compiled a summary of all available measurements at four wavelengths from 3.7 to 20 cm, made between 1974 and 1987, and found that the variability timescales at the longer wavelengths fit the λ^2 law expected for refractive interstellar scintillation. In later reporting the results of a dedicated VLA monitoring program, this same group confirmed that result, and argued that, at wavelengths shorter than 3.7 cm, intrinsic variability dominates (Zhao et al. 1992). They also found 3 "outbursts" during their 1990-91 monitoring period, and interpreted them as low-level accretion activity in the region close to the central object. Monitoring observations done at millimeter and submillimeter wavelengths (Gwinn et al 1991 at 0.8 and 1.3 mm, and Wright and Backer 1993 at 3 mm) also have revealed supposedly intrinsic time variations on time scales as short as 10 days, but not on time scales of 24 hours or less, leading to the conclusion that the source size must be > 0.1 AU, and the corresponding brightness temperature $< 0.5 \times 10^{12}$ K, which is below the Compton limit. The scintillations observed with the VLA require plasma fluctuations of scale size ~ 0.1 AU to be moving across the line of sight to SgrA* at velocities exceeding about 100 km s^{-1}, so they may be located quite close to SgrA*.

4.4.3 The Size of SgrA*

The size of SgrA* is much better explored with the VLBA than with the VLA, although its scattering disk can be observed with the VLA at 20 cm (Yusef-Zadeh et al. 1994). The size follows the expected λ^2 relation for interstellar scattering (see recent review by Backer 1996). As indicated in the previous section, some portion of the scattering medium may be associated closely with SgrA*. However, VLA observations have shown that the large-scale Galactic center medium broadens all radio sources within a few hundred parsecs (van Langevelde et al. 1992), and furthermore, that because of an anisotropic electron density distribution, anisotropic motions, and/or magnetic fields, the scattering disks of small sources are noncircular (Frail et al. 1994). This same medium is responsible for the Faraday rotation, or the depolarization, of polarized GC sources (Yusef-Zadeh 1996). At a high enough frequency, the size of the scattering disk should become comparable to, or smaller than, the source size. Millimeter VLBI observations may therefore be required to reach the 0.1 - 1 AU size expected for this source, although Lo et al. (1998) claim to already have reached the intrinsic size of the source (3.6 AU) along the minor axis of the scattering disk using the VLBA at 7 mm.

4.4.4 The Proper Motion of SgrA*

The proper motion of SgrA* has been measured with the VLA over a time base of well over a decade by Backer (1996; see also Backer and Sramek 1987). The position in each epoch is precisely determined with respect to three extragalactic reference sources located within a degree of SgrA*. The very significant proper motion is attributable entirely to the solar motion of about 220 km s^{-1} about the GC, and the errors in the motion both perpendicular and parallel to the Galactic plane, combined with some uncertainty in the solar motion, allow for a transverse peculiar motion of SgrA* of only about 25 km s^{-1} at most. A VLBA study which has been under way for two years, and in which relative positions of SgrA* and two extragalactic sources can be determined with an accuracy approaching 0.1 millarcsec, already gives a comparable limit to the proper motion of SgrA* (Reid et al. 1999).

4.4.5 The Polarization of SgrA*

Polarimetric studies of extragalactic radio sources with the VLA have been instrumental in the determination of their properties. This success motivated a VLA study of the polarization of SgrA*. No linear polarization was detected to instrumental limits from 4.8 GHz to 43 GHz (Bower et al. 1999a, Bower et al. 1999b). Furthermore, VLA spectropolarimetry at 4.8 and 8.4 GHz indicate no linear polarization for rotation measures as large as 10^7 rad m^{-2}. Together, these results imply that SgrA* is not depolarized by the scattering medium, but is intrinsically unpolarized. Surprisingly, circular polarization was detected in SgrA* with the VLA (Bower et al. 1999c). This represents one of the most sensitive measurements of circular polarization made with the VLA. The origin of the linear and circular polarization is not well understood. Further study of these properties may reveal significant new information on the synchrotron source and the surrounding medium.

4.5 Low Frequency Imaging

Prior to being done with the VLA, aperture synthesis studies at low frequencies gave images that resembled the best high-frequency single-dish maps (e.g., Little 1974 [408 MHz]; Mills and Drinkwater 1984 [843 MHz]), although at the lowest frequencies, the maps look quite different because of foreground absorption and the predominance there of nonthermal sources (La Rosa & Kassim 1985 [80 and 57.5 MHz]; Kassim et al. 1986 [111 and 123 MHz]; Yusef-Zadeh et al. 1986a,b [160, 327 MHz]).

The VLA was first used at 90 cm (332.4 MHz) by the NRAO group (Pedlar et al. 1989 and Anantharamaiah et al. 1991). With the large field of view, they saw in their images all the radio sources known from higher-frequency observations, including all known systems of nonthermal filaments, thus providing the most striking visualization of these local manifestations of the (presumably) large-scale dipole magnetic field. The extension of the frequency domain to the lower frequencies also led to a much clearer separation between the thermal and nonthermal structures in this complex region.

The increase of opacity of thermal gas toward lower frequencies led to some important line-of-sight placements for various sources. Sgr A West is seen in absorption againt Sgr A East, indicating that Sgr A West is largely in front of Sgr A East (Yusef-Zadeh & Morris 1987a; Pedlar et al. 1989). On the plane of the sky, Sgr A East was already known not to be centered on the dynamical center of the Galaxy and the presumed lair of the supermassive black hole, which is buried in Sgr A West, although it did surround these places in projection, but to also displace Sgr A West and East along the line of sight raised questions about whether Sgr A East really had anything at all to do with the activity within the central parsec of the Galaxy. A recent suggestion by Khokhlov and Melia (1996), however, explains these displacements in terms of a directed ejection from the central black hole. Another case of line-of-sight ordering made possible by low-frequency measurements is G0.18-0.04, seen in absorption against the radio Arc. This indicates that at least part of this HII region is in the foreground of the Arc (although Serabyn & Morris 1994 have argued that they are intimately connected).

Most recently, Kassim & Frail (1996) combined data from a variety of telescopes, but have reprocessed the calibrated 333-MHz VLA visibility data of Anantharamaiah et al. (1991) using a wide-field imaging technique (with U,V,W visibilities). The resultant image is clearly improved, and a number of new sources, such as the Pelican NTF (Lang et al. 1999a), are being discovered in the reprocessing and cataloging reported in LaRosa et al. (1999). When combined with images at other frequencies, it shows that the extended shell source, G0.33+0.04, has a steep spectrum; Kassim & Frail interpret it as a supernova remnant near the GC, reminiscent of Sgr A East.

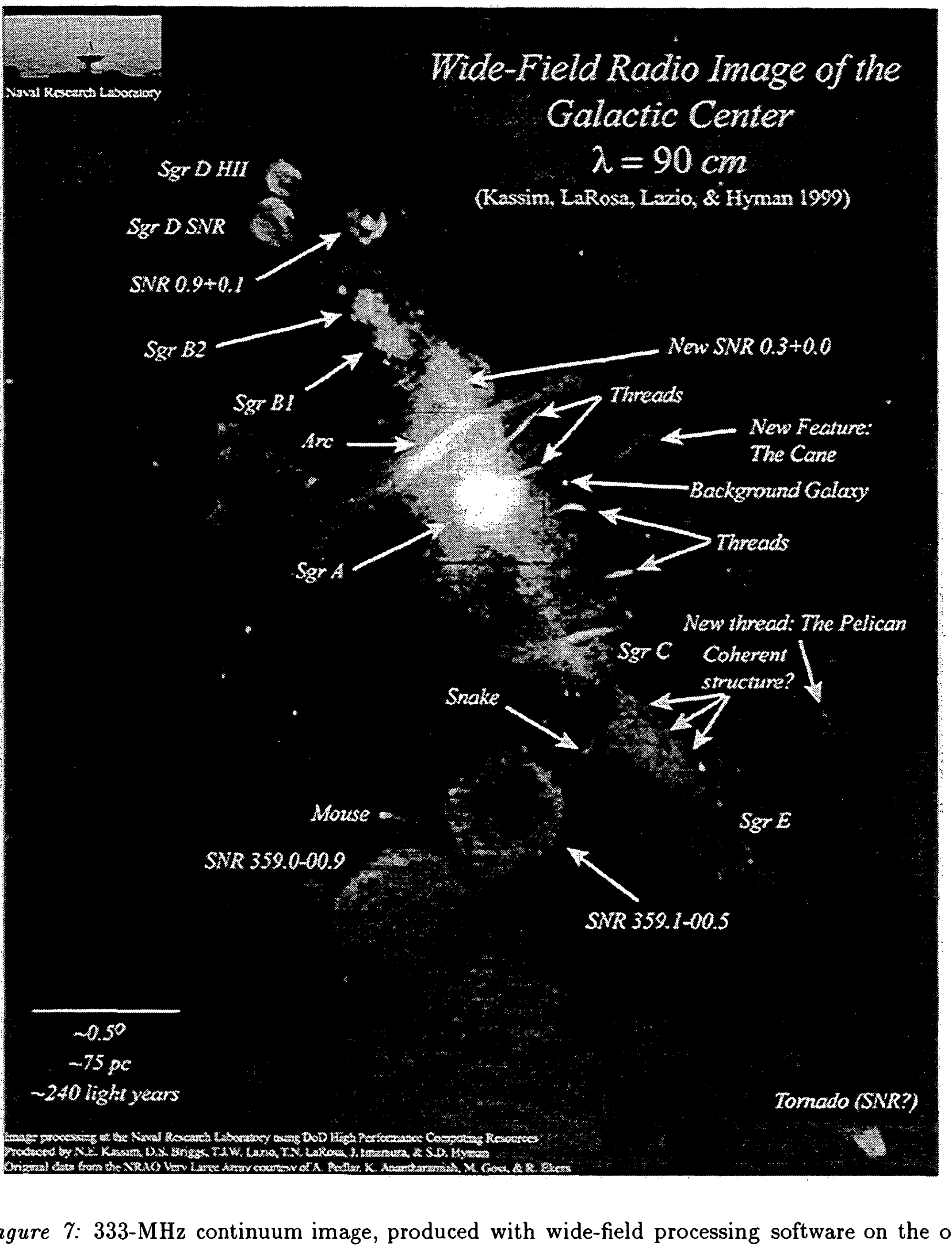

Figure 7: 333-MHz continuum image, produced with wide-field processing software on the origin data of Anantharamaiah et al. (1991) (Kassim et al. 1998; LaRosa et al. 1999).

4.6 Shock-Excited 1720 MHz OH Emission

In the dense medium of the GC, clouds have a sufficiently large volume-filling factor that the impact of supernova shocks upon molecular clouds is a fairly frequent occurence. It has recently been recognized that several Galactic supernovae interacting with nearby clouds give rise to 1720-MHz OH masers (Frail et al. 1996). In the GC region, several such masers were first found in G359.1-0.5 (Yusef-Zadeh, Uchida & Roberts 1995), a supernova remnant surrounded by a ring of molecular gas (Uchida et al. 1992; Uchida, Morris & Yusef-Zadeh 1992). Subsequently, Yusef-Zadeh et al. (1996) found 1720-MHz OH maser emission at 7 different positions within a few arcminutes of the Galactic center, most of them at the presumably shocked interface between Sgr A East and the molecular cloud M-0.02-0.07. Not only are these masers interesting new probes of the kinematics of the shock interface, but they also provide invaluable information on the magnetic field via the Zeeman effect, as described below.

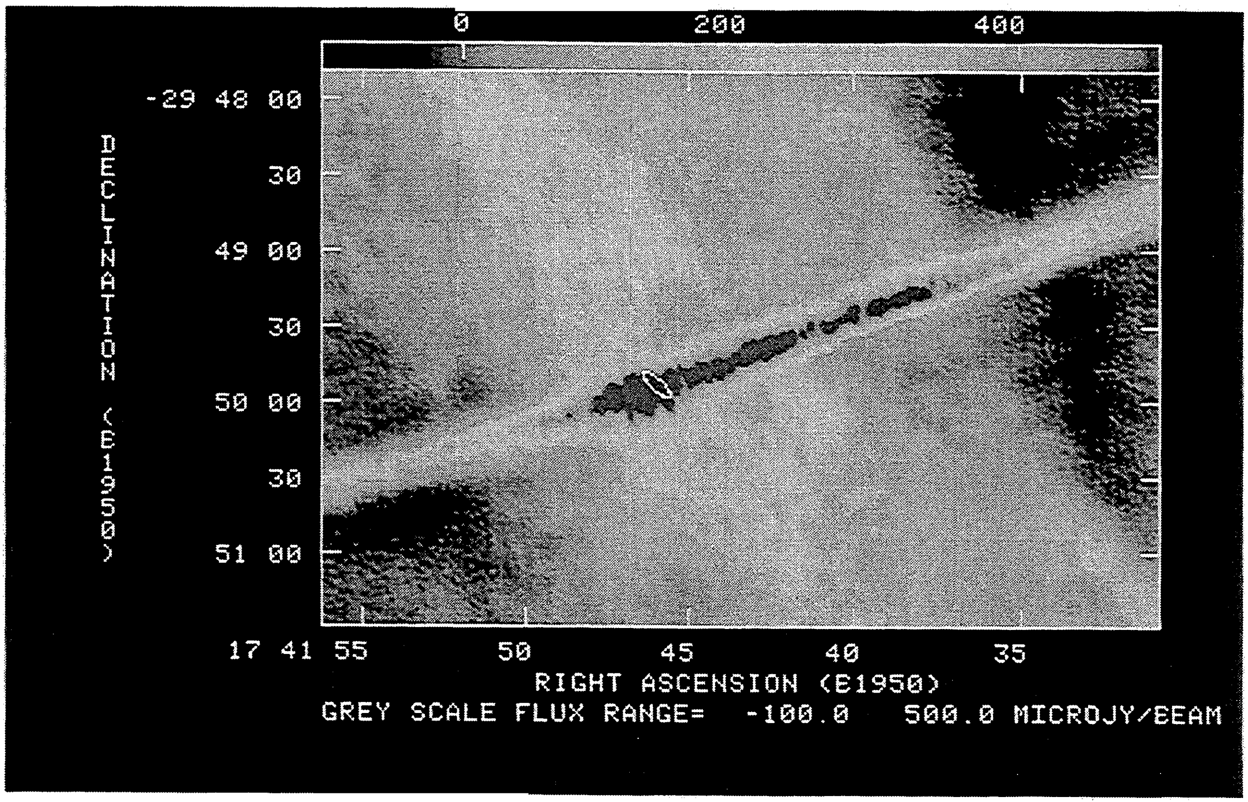

Figure 8: A remarkable confluence: the broad shell of the supernova remnant (or superbubble), G359.1-0.5 crosses from lower right to upper left, and the Snake filament (G359.1-0.2) crosses from lower left to upper right. A 1720-MHz OH maser is superimposed on the brightest point of their intersection. (Figure courtesy of F. Yusef-Zadeh).

4.7 Ionization and Ablation of Red Giant Winds

The density of red giants near the GC is very high, with perhaps as many as 10^3 - 10^4 of them in the central parsec alone. Those that are not stripped by collisions with other stars (Sellgren et al. 1989; Genzel et al. 1997) may be subject to the unusual ionizing radiation field of the GC ($\sim 10^7$ $L_\odot$, with characteristic temperature of 35,000 K, and if they are close enough to the central stellar cluster of windy emission-line stars, they are immersed in a rather intense wind ($\sim 4 \times 10^{-3}$ $M_\odot$ yr^{-1} moving at $\sim$ 700 km s^{-1}). Those giants which are losing mass in the normal process of stellar evolution therefore may have: 1) externally ionized envelopes, or inverse Stromgren spheres (Morris & Jura 1983) surrounding them, and 2) bow-shock envelopes shaped by the dynamical encounter between the red giant and GC winds. The VLA was used to find these effects in the red supergiant IRS7 (Yusef-Zadeh et al. 1989; Yusef-Zadeh & Morris 1991; Yusef-Zadeh & Melia 1992), and infrared observations independently supported the picture (Rieke & Rieke 1989; Serabyn et al. 1991). More recently, Yusef-Zadeh, Wardle & Roberts (1996) have sought a similar effect in other mass-losing stars in the inner parsec, and have tentatively identified one other case in which a wind is being externally ionized: OH359.88-0.087. This effect can also be used to constrain the line-of-sight locations of mass-losing stars, since the absence of ionization of a stellar envelope of, say, an OH/IR star implies that it is not within or near the central parsec (unless it is fortuitously shielded by one of the gas streams present there).

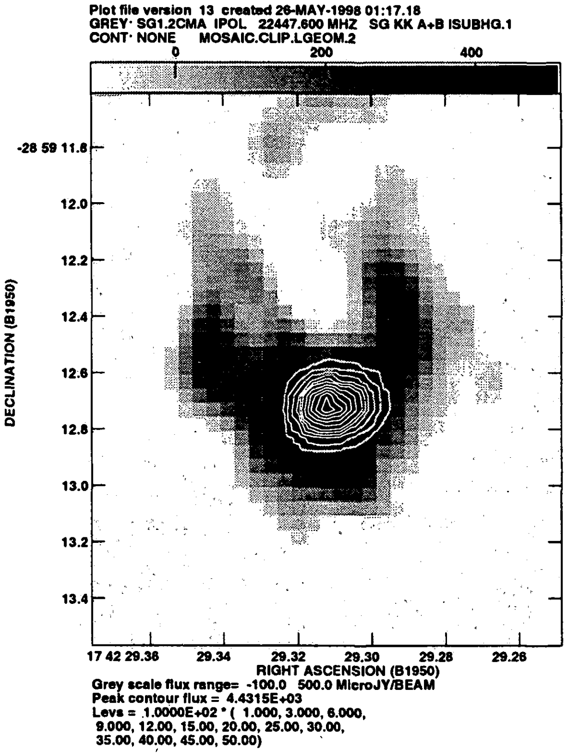

Figure 9: The red supergiant star IRS7. Contours: 2-micron image of the star. Grey-scale: 1.2 cm continuum measured with the VLA, showing the "cometary" tail of gas, apparently blown northwards by the winds from the young stellar cluster (IRS16) about a light year to the South. At longer wavelengths, the tail extends ~3" to the North of IRS7 (image courtesy of F. Yusef-Zadeh)

4.8 Zeeman Measures of Magnetic Fields in Neutral Clouds

In principle, the best measure of the supposedly strong magnetic fields in the GC is their direct detection using the Zeeman effect. Such measurements of line-of-sight field components can be complemented by far-infrared polarization studies, which reveal the orientation of the magnetic field component in the plane of the sky.Interferometric Zeeman measures toward Sgr A were initially tried with the Westerbork array (Schwarz & Lasenby 1990), in an observation of HI absorption with a 12" x 50", with a positive result (0.5 mG) of 3-σ significance toward the circumnuclear disk (a predominantly molecular structure with an inner radius of ~1 pc).

The first attempt to carry out Zeeman measures with the VLA focused on 1667-MHz OH emission. Killeen, Lo & Crutcher (1992) observed with 3" x 4" resolution, and found significant Zeeman signals indicative of 2 mG fields when they averaged over broad regions of the circumnuclear disk. HI Zeeman measures of this region were carried out with the VLA by both Plante, Lo & Crutcher (1995) and Marshall, Lasenby & Yusef-Zadeh (1995). The first group detected a Zeeman signal at several positions toward Sgr A West and the circumnuclear disk about an arcminute North of SgrA*, again with field strengths of a few mG, and they found upper limits of ~1 mG elsewhere, but the second group obtained only upper limits of ~ 0.5 mG by averaging over 45" x 45" fields. In general, Zeeman measures of GC clouds have proven quite difficult, in part because the subtle Zeeman line splitting is difficult to pull out of the very broad absorption lines, and perhaps in part because the magnetic fields in the observed clouds are likely to have substantial structure along the line of sight, possibly even with field reversals which can reduce or null out the Zeeman splitting.

The circular polarization of 1720-MHz OH masers, on the other hand, has been readily measurable with the VLA, thus providing an excellent probe of the magnetic field in a few very limited areas. Using the strong 1720-MHz maser lines, Yusef-Zadeh et al. (1996) find field strengths comparable to the HI limits, ranging from 2 to 4 mG in all seven of their maser sources.

4.9 Transients

Bright transient sources presumably associated with eruptions in stellar systems have been detected in a few instances toward the GC. Prior to the operation of the VLA, Davies et al. (1976), using the Jodrell Bank radio-linked interferometers, reported a strong, nonthermal transient within several parsecs' projected distance of the GC, and identified it with the X-ray transient, A1742-28. The VLA has caught one such transient, located ~35" south of SgrA* (Zhao et al. 1992). At its peak in 1990 December, the flux density of this GC transient (GCT) was comparable to that of SgrA*, and it declined on a time scale of about 3 months, maintaining a steep spectrum throughout. It was interpreted as synchrotron emission from a plasma involved in an instability or eruption in an X-ray binary system. The VLA will very probably find other examples of such transients in coming years, especially if monitoring projects are undertaken; it will then be interesting to compare them to measurements with AXAF and XMM.

4.10 Counterparts to High-Energy Sources

Some of the Galactic superluminal sources are naturally found toward the GC, where the stellar density is maximized. The VLA has been essential for demonstrating that these high-energy sources have radio counterparts characterized by superluminal motions. Thus, these objects, presumably jets from accretion disks around compact objects, have been termed "mini-quasars". Most of the VLA work on the most interesting of these objects has been carried out by Luis Rodriguez and Felix Mirabel, so I refer you to their review article (Mirabel & Rodriguez 1998).

4.11 Cold, Ionized Regions

A phenomenon which has been noticed in VLA data, but which will require much future research before it is understood, is the presence of dark lanes at radio wavelengths, seen against continuous background emission structures, both in Sgr A West (Morris & Yusef-Zadeh 1987; Yusef-Zadeh & Wardle 1993), and toward the radio Arc (Yusef-Zadeh & Morris 1988). Presumably, these lanes are due to free-free absorption in relatively cold, partially ionized gas, but further information is badly needed to assess that hypothesis, as opposed to the hypothesis that these discrete and well-defined lanes are simply evacuated. What ionization mechanism can account for the necessary emission measures, and still leave the medium as cold as a few hundred degrees?

5 Future Prospects

While the VLA has dug many of the obvious nuggets out of the GC radio mine, there clearly remains much to be done. Deep images with high dynamic range, for example, will continue to reveal new structures. It will be fascinating to see if the radio filaments are multitudinous at lower flux levels, to use all of the detectable maser stars in the inner few parsecs – many more than are now known – to model the mass distribution near the center, to couple proper motions with radial velocities to get a complete picture of gas motions in the inner parsec, and to catalog and understand the population of transient sources. And it will be interesting to be on hand with the VLBA if and when Sgr A* undergoes an extreme fluctuation, perhaps due to an impulsive accretion event. It is good to know that Barry Clark will be overseeing and scheduling these and other GC findings, as he remains an essential part of the history of discovery with the VLA.

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H I Images That Changed My View of the Universe

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Abstract: This talk centers on three H I images that have given me a different view on the universe. The first shows that H I disks around ellipticals may be more the rule than the exception and that polar rings around S0s are more likely the inner edges of very extended disks. The second image emphasizes how little we know about the true extent of spiral galaxies: Lyman α clouds are seen at large projected distances from the nearest spirals but covering the same velocity range as the H I emission in the nearest galaxy. Finally H I images of clusters of galaxies make it possible to identify the removal processes at work and may help elucidate the dynamical state of the clusters.

1. Introduction

At this Barry Fest I will concentrate on the VLA as an imaging instrument. As you probably know I have for a long time been wanting to make an H I image of the universe, and I secretly began the project many years ago, gratefully accepting any bits of time that Barry would give me. What I am really interested in is, if you look at the universe in H I does it tell you something you did not know? Does it change your perspective? Although an H I image of the universe still has some way to go, today I would like to show you some images that did change MY view of the universe.

2. H I in Shell Elliptical Galaxies

The first image portrays H I associated with the optical shells in NGC 5128 (Centaurus A for radio astronomers). Thanks to the work of David Malin, who with his deep optical photography has profoundly changed my view on the local universe, it has become clear that a large fraction of ellipticals and S0 galaxies possess sharp arclike features called shells or ripples (Malin and Carter 1983; Schweizer and Seitzer 1990). Figure 1 shows a schematic image of the shells in NGC 5128, the solid and dashed lines, the inner radio jets, dust patches, and in contours the neutral hydrogen from the inner well-known dust lane and the accidentally discovered H I associated with the shells. At right, the velocity field of the hydrogen is shown overlaid on the total H I emission in greyscale. Note that where there is a break in the shells, in the northeast, there is also a break in the H I distribution, while the velocity field is completely smooth. These results are at first surprising. Until quite recently the most widely accepted model for the origin of these shells is the merger model developed by Quinn (1984), in which shells are the phased wrapped remnants of a low mass disk galaxy trapped in the potential of a larger elliptical.

Shells, formed from a nearly radial merger or accretion, should have a low orbital velocity, in disagreement with the velocities observed. Moreover, the gas in the progenitor disk galaxy is not expected to survive successive passages through the central regions of the elliptical, with most of the gas settling in about a crossing time into a compact ring or disk at the center of the potential (Weil and Hernquist 1993). More extensive modelling has shown that shells can be formed in mergers involving a wide variety of morphological types and initial conditions; shells also can be formed from the spatially wrapped remnants of a companion infalling on a near circular orbit (e.g., Dupraz and Combes 1987; Hernquist and Quinn 1988,1989; Hernquist and Spergel 1992; Thomson 1992; Quillen, Graham and Frogel 1993), which may explain the H I results.

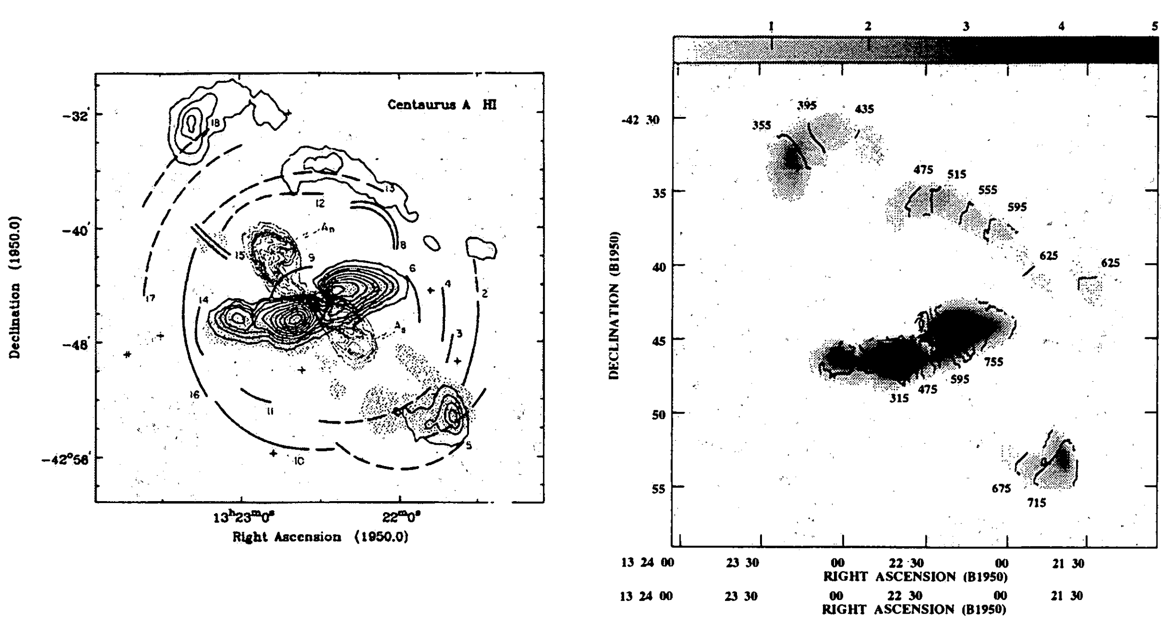

Figure 1. Total H I contours on a schematic image of the shells of NGC 5128. Solid and dashed arcs indicate the location of the shells. At right the intensity weighted velocity field (contours (km s^{-1})) superimposed on the total H I image (greyscale) (Schiminovich et al. 1994).

The discovery of rapidly rotating H I associated with the shells of NGC 5128 has led to a whole industry of H I observations of shell galaxies. Schiminovich et al (1997) have embarked on an H I survey of shell ellipticals, selected from the catalog by Malin and Carter (1983). The detection rate is high (12 out of 22) and the results are somewhat messy. The H I distributions range from chaotic (i.e., unsettled), to partial rings, to things that masquerade as almost complete disks with surprisingly regular velocity distributions. In some cases the H I sits at the outside of the outer shells; in other cases there seems to be an anti-correlation between the H I and the shells. The real eye opener is Figure 2. It shows what looks like an almost complete disk around the polar ring galaxy MCG -5-7-1, with some degree of correlation between the H I and the shells.

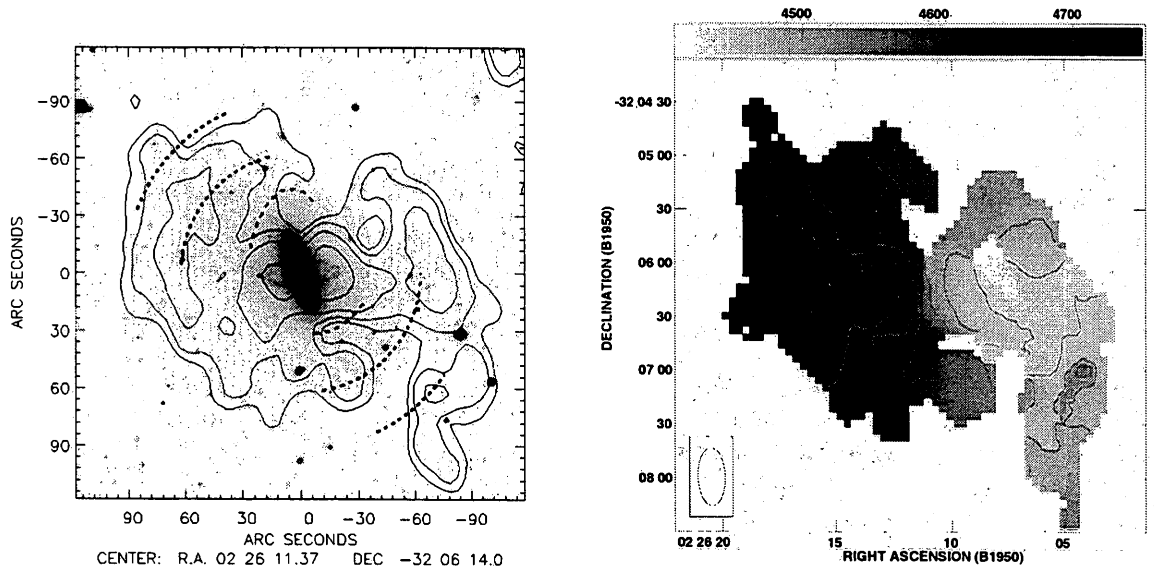

Figure 2. Overlay of total H I on the polar ring galaxy MGC -5-7-1. The locations of the shells are indicated with dotted lines. At right the remarkably regular velocity field of the H I masquerading as a disk. Greyscale values are indicated on top in km s^{-1}.

The polar ring forms the inner edge of the H I disk. This is not just one case; several shell ellipticals are known to have an inner ring, e.g., Arp 230 and, of course, NGC 5128 (Schiminovich et al. 1997). Recent Australia Telescope observations of NGC 4560A by Arnaboldi et al (1997) show that this propotypical polar ring galaxy looks more like a failed bulge in a huge disk, where the polar ring forms the inner edge of the disk.

These images suggest that gaseous disks around early type galaxies are much more common than current lore suggests. In the case of the shell galaxies, these disks have a very faint stellar component as well. In fact the extent of H I and faint stellar light is remarkably similar in most cases.

What is the origin of the gas? Several scenarios are possible: fallback after a major merger is a plausible explanation for systems in which there is a correlation between the stellar and gas kinematics in the outer parts, such as in NGC 2865 and in NGC 5128, while accretion of small amounts of gas may be a possibility in other systems. On the other hand, secondary infall may have the same observational signature and it remains a fascinating possibility that we are watching the growth of disks at the current epoch (van Gorkom and Schiminovich 1997).

3. The H I environment of nearby Lyman α absorbers.

While the first images revealed that there is a lot more stuff around ellipticals than meets the eye, the second image may tell something about the true extent of spiral galaxies. With the launch of HST it has become possible to study in the UV the local Lyman α forest at redshifts less than 1.6. Although the observed column densities of these systems are low, between 10^{12} and 10^{16} cm^{-2}, the nearness of these systems makes them ideal targets for searches for H I and optical emission from the vicinity of these clouds, in the hope of learning more about the parent population of the unsaturated Ly α absorbers. At this time a large number of optical searches have been completed with somewhat mixed results. Several groups (Morris et al 1993, Stocke et al 1995) conclude that absorbers are not randomly distributed with respect to the galaxies, although the absorber-galaxy correlation is not as strong as the galaxy-galaxy correlation. Grogin and Geller (1998) reanalyzing the same data sets conclude that the Ly α absorbers are randomly distributed with respect to the galaxies. Clearly the nature of the low-z Ly α absorbers is still an open question.

Searches for H I emission from the vicinity of the absorbers have so far been much more limited, but have nevertheless produced some intriguing results. Several deep searches have been performed at the VLA, in which a fixed volume (typically 1000 km sec^{-1} by 500 kpc x 500 kpc) around a known absorber is searched for H I. Those searches are complementary to the optical work; in 21-cm H I one can locate the gas rich but optically faint galaxies. The results so far support the findings that the Ly α absorbers trace the large scale structure outlined by optically luminous galaxies. Often gas rich galaxies are found precisely at the velocity of the absorber but at large projected distance. In principle, one can see if a physical association is plausible by comparing the resolved H I kinematics of the nearest galaxy and the location of the Ly α absorber. Unfortunately, in most cases so far, the H I is seen in the direction of the minor axis of the galaxies, giving no evidence for a physical association between absorber and nearest galaxy, but there is also no evidence against such a connection (van Gorkom et al 1996). One of the most striking results is shown in Figure 3 (Shull et al 1998). It shows the H I environment of a cluster of Ly α absorbers found toward the BL Lac PKS 2155-304. Five galaxies are detected in H I emission in the velocity range covered by the absorbers. The H I emission is shown in contours. The projected distance of each of the galaxies to the sightline toward PKS 2155-304 is indicated next to the lines connecting the galaxies to the BL Lac. The velocity range and approximate major axis of the H I emission for the individual galaxies also are indicated. A comparison with an HST/GHRS spectrum at 20 km s^{-1} resolution toward PKS 2155-304 shows that the galaxy closest to the sightline at a projected distance of 400

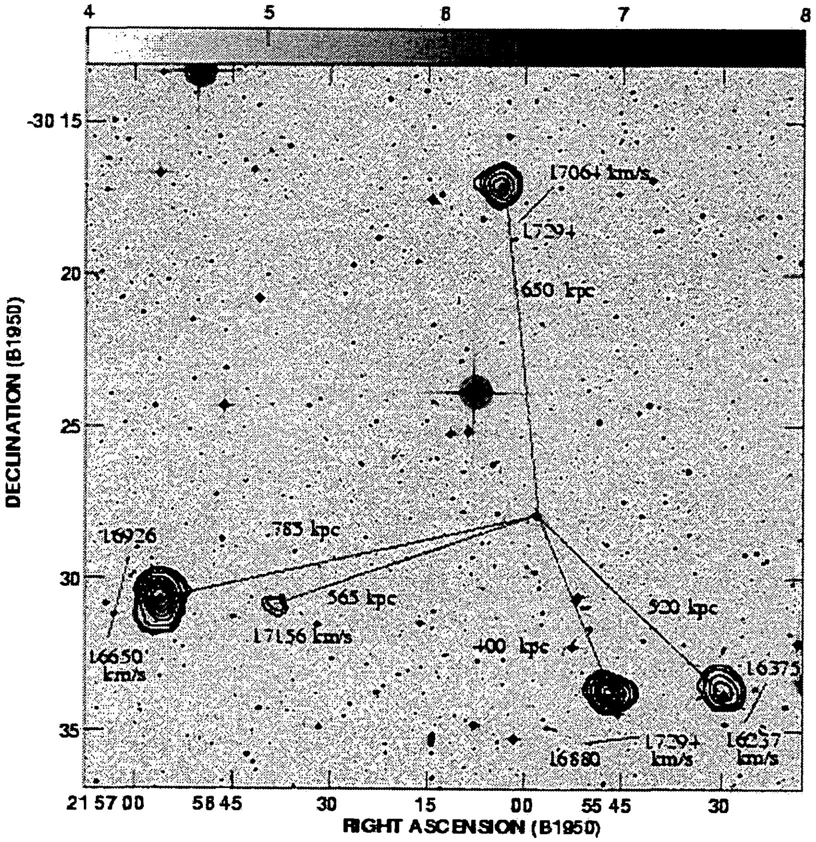

Figure 3. An overlay of the total H I emission (contours) toward PKS 2155-304 in the velocity range (16,283 - 17,571 km s^{-1} on an image of the digitized POSS. Five galaxies are detected in H I at projected distances from the sightline toward PKS 2155-304 of (400-785)$h_{75}{}^{-1}$ kpc. Contour levels are 1.65, 3.3, 6.6, 13.2, 19.8, 26.4, 33.0, 39.6 x 10^{19} cm^{-2}. The image has been corrected for the primary beam response. Projected distances from the sightline are indicated in $h_{75}{}^{-1}$ kpc. For each of the galaxies, the velocity range and approximate major axis of the H I emission are indicated. Note that the galaxy closest to the sightline covers in H I emission exactly the same velocity range as the broad Lyα absorption seen near 17,000 km s^{-1} The galaxy to the southwest covers about the same velocity range as the absorption feature near 16,200 km $^{-1}$

kpc has H I in emission over exactly the same velocity range as the deepest Lyα absorber. The next nearest galaxy at 520 kpc distance has H I in emission over approximately the same velocity range as the second deepest absorber. It is hard to understand this result. The large projected distances between absorbers and emitting galaxies make it unlikely that the absorbers are located in halos associated with individual galaxies: in this case the halos would overlap. The kinematics are even more puzzling. If the Ly α absorber were in the halo of the galaxy, its velocity width should be narrow and close to the systemic velocity of the galaxy. The precise coincidence of the velocity range of the H I emission and Ly α absorption must be fortuitous. Most likely the width of the absorption line reflects the depth of the potential well of the group rather than that of an individual galaxy. It nevertheless remains somewhat of a puzzle that while the filling factor of Ly α clouds is 0.5 to 1 out to hundreds of kpc from galaxies, so far no diffuse intergalactic H I emission has been found close to these clouds, down to column densities of a few times 10^{18} cm^{-2}. If the Lya α arises in mostly ionized gas, this gas must be distributed rather smoothly, even close to the galaxies, since any amount of clumping would lead to recombination and a larger neutral fraction.

4. H I Imaging of Clusters

The final image concerns the fate of H I in cluster galaxies. While single dish studies have shown that spirals in clusters are H I deficient (Giovanelli and Haynes 1985), the VLA produced the first striking images, which showed that the H I disks in the Virgo Cluster are stripped to well within the optical disks (van Gorkom et al 1984). A composite image of the H I emission from the 23 brightest spirals in Virgo is shown in Cayatte et al (1990). Clearly the galaxies close to the center have much smaller H I disks than those farther from the center The imaging work has made clear that it is the distribution of H I with respect to the optical disk that indicates best what removal processes (if any) are at work (Cayatte et al 1994).

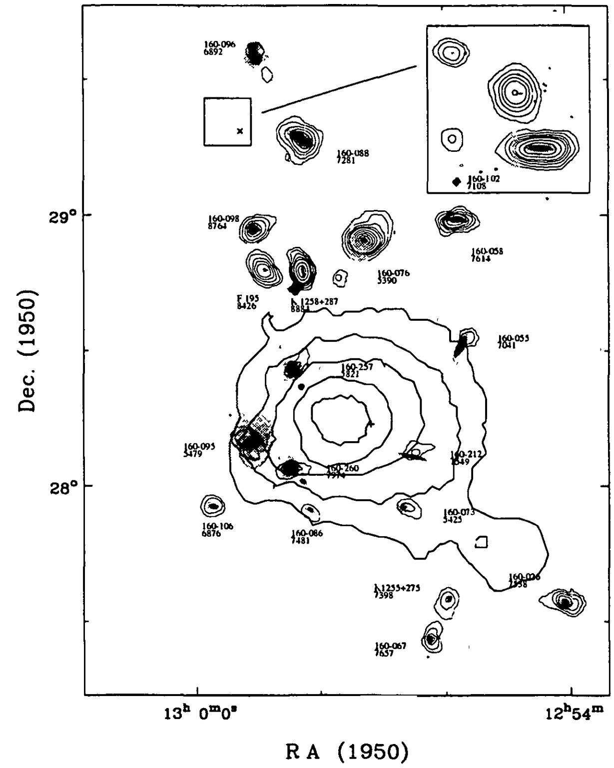

Figure 4. Integrated neutral hydrogen contour images of the brightest spirals in the Coma Cluster Center (Bravo Alfaro et al 1999). The HI contours are overlaid on DSS optical images. The X-ray emission from the center and south west is indicated with the larger contours.

Even more spectacular results have recently been obtained on the Coma cluster by Bravo-Alfaro (1997). Figure 4 shows the H I in Coma overlaid on optical images of the Digital Sky Survey (Bravo-Alfaro et al 1999). The X-ray emission from the central and south west region is indicated by the large contours. Galaxy names (CGCG) and heliocentric velocities are given next to each galaxy. Obviously only galaxies within the central X-ray emitting region have very unusual H I morphologies. The very asymmetric H I distribution in some of the galaxies is evidence that they are currently in the process of being swept by ram pressure. What is not shown is the distribution of the non-detected spirals. These are all located in the center and to the east and south west. These galaxies have crossed the center at least once in order to lose their gas. Thus the H I can help trace the orbital history of cluster galaxies.

H I imaging studies of clusters have only recently become an industry at the VLA. I believe they have great potential for studying the evolution of galaxies in clusters as well as the dynamical structure of the clusters as a whole. At larger redshifts entire clusters fall within the primary beam of the VLA and a complete census of the H I in the cluster can be made. An example is shown is Figure 5. It shows the total H I in the central regions of Abell 2670 at a redshift of z=0.08. Although this cluster was long considered to be the prototype of a dynamically relaxed cluster, the H I observations show that the gas rich galaxies occupy a narrow range in phase space, being tightly grouped spatially and in velocity. Thus, contrary to previous evidence, this cluster is far from being dynamically relaxed.

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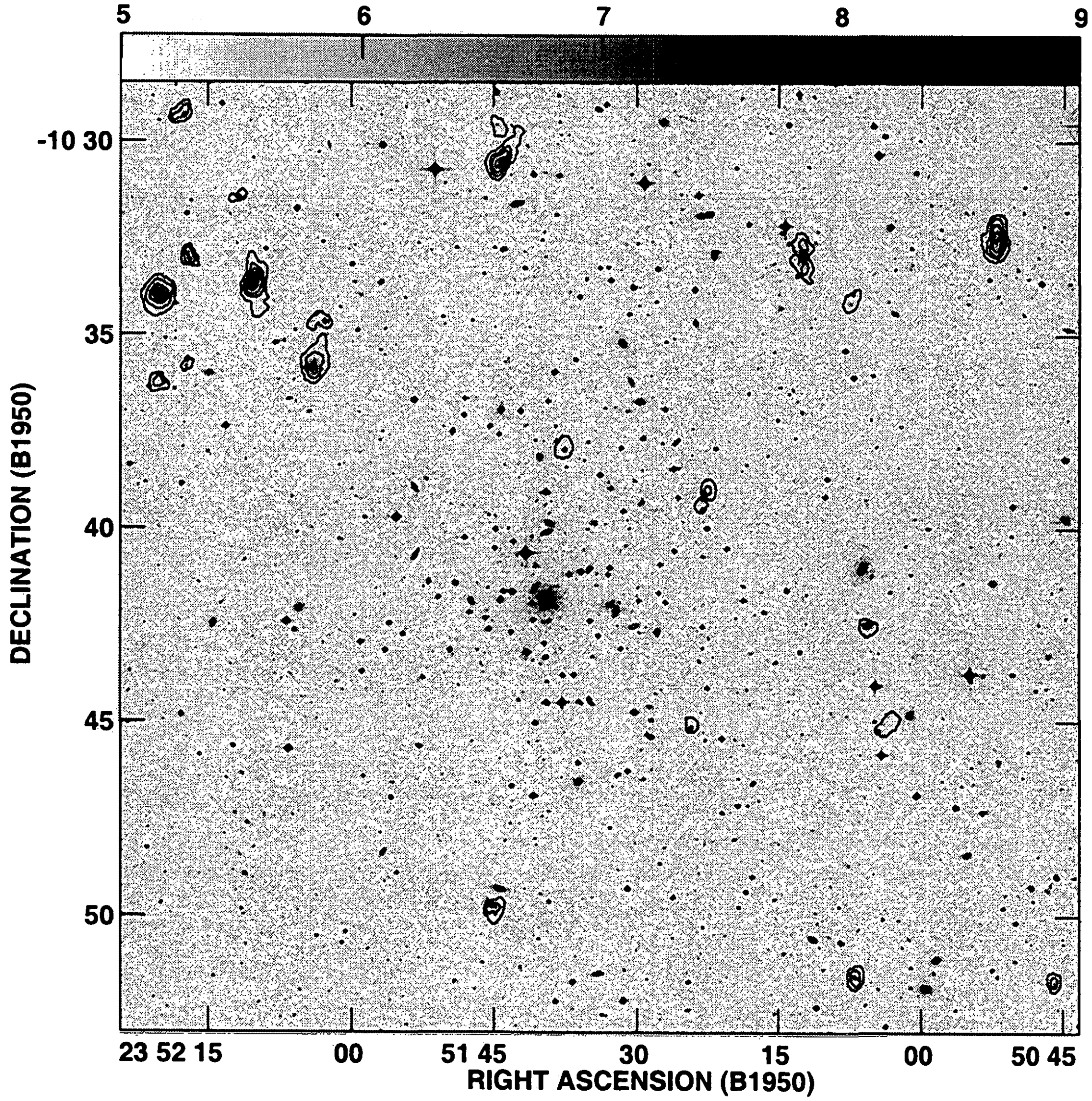

Figure 5. Integrated neutral hydrogen emission (contours) overlaid on an optical image (greyscale) of Abell 2670. Gas rich galaxies appear to be falling in from the north east and south west. Note that the sensitivity is uniform across the image and that the entire volume of the cluster has been probed in H I.

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TWENTY YEARS OF GRAVITATIONAL LENS STUDIES WITH THE VLA: HIGHLIGHTS

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The discovery of the first gravitational lens in 1979 coincided with the earliest operation of the VLA. The sensitivity and high angular resolution of the VLA turned out to be particularly well suited to gravitational lens studies, and nearly two decades of fruitful work followed. These studies embraced many aspects of gravitational lensing. This review does not attempt to be comprehensive; rather, some of the highlights of VLA gravitational lens studies are addressed. For a more complete summary the reader is referred to the proceedings of the recent international gravitational lens conference in Melbourne (*Astrophysical Applications of Gravitational Lensing*).

1 First Observations

Gravitational lensing became a topic of observational astronomy when the first gravitational lens was discovered in 1979 (Walsh, Carswell, and Weymann). Dave Roberts, Bernie Burke and Perry Greenfield of MIT were quick to recognize the necessity of bringing the nascent VLA's unprecedented combination of sensitivity and resolution to bear on unraveling the properties of the first example of multiple imaging by the action of a gravitational field. In a series of papers (Roberts, Greenfield, and Burke 1979; Greenfield, Roberts, and Burke 1980; Greenfield, Burke, and Roberts 1980) they developed the radio side of the emerging 0957+561 story. Beginning with healthy skepticism, they first cast doubt on the lensing hypothesis, then developed the essential features of our understanding of the object today: two images of the core of a high redshift quasar, complete with spectacular radio lobes (of which only a small piece is doubly imaged) an even radio emission from the lensing galaxy. Figure 1 is a reproduction of the original 1979 VLA image of 0957+561 that appeared on the cover of *Science*. It was laboriously constructed using the site Dec10 computer from data acquired during six minutes of observation at 5 GHz with fourteen VLA antennas. Figure 2 is a modern VLA image of the same object, showing the remarkable imaging capabilities of the full VLA, enhanced by the CLEAN and self-calibration algorithms.

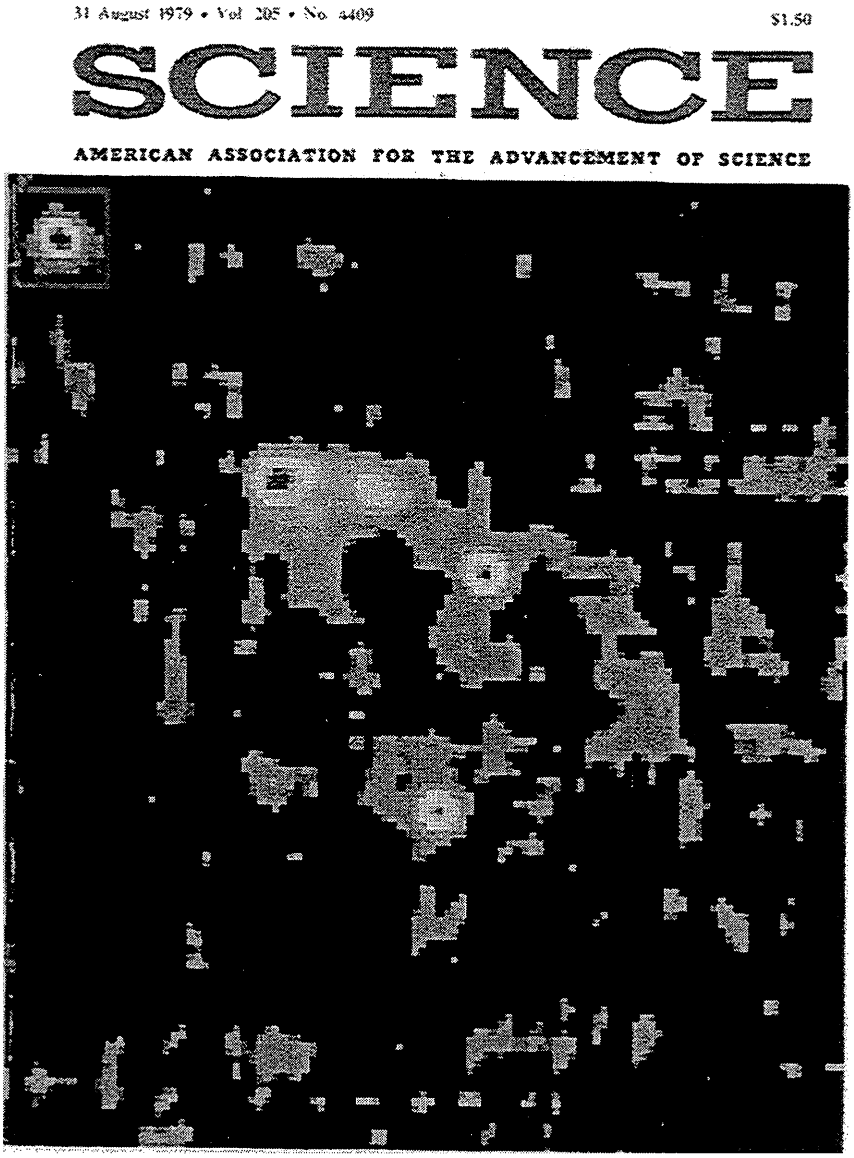

Figure 1: A reproduction of the cover of the 31 August 1979 issue of *Science*, featuring the first VLA observations of the recently discovered gravitational lens 0957+561. The VLA was still under construction when the data for this image were taken.

2 Searches for Gravitational Lenses

During the early years of gravitational lens studies, an active research project at MIT was the survey of a large number of radio sources drawn from the MIT-Green Bank (MG) 5 GHz survey (Bennett et al. 1986). The snapshot capability of the VLA allowed the production of hundreds of maps from a single day's worth of VLA data. Two more gravitational lenses had been discovered in optical quasar studies (Weymann et al. 1980; Weedman et al. 1982), and soon the serendipitous discovery of the fourth gravitational lens was made in the VLA-MG

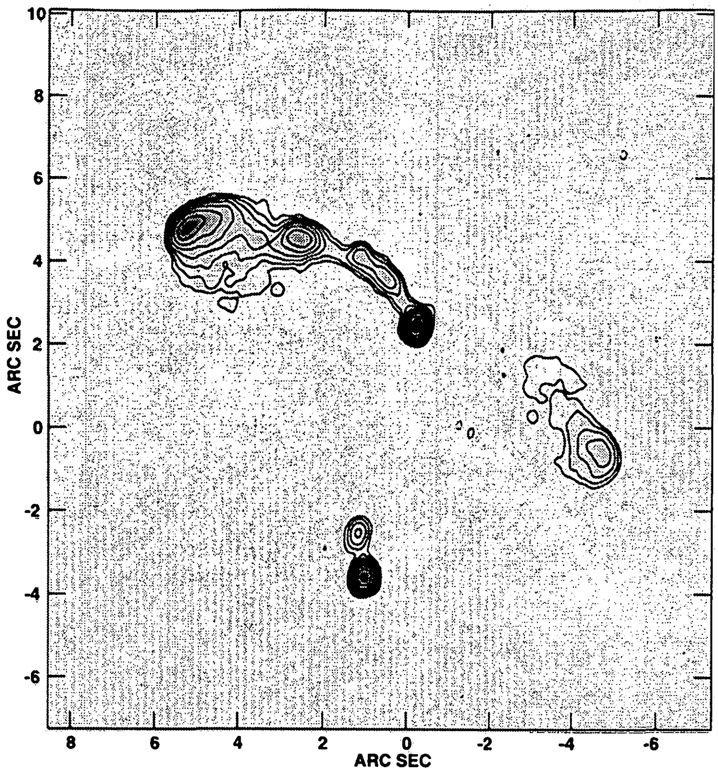

Figure 2: A modern image of 0957+561, computed from data acquired with the full VLA and enhanced by the CLEAN and self-calibration algorithms. Note that the emission from the lensing galaxy, just north of the southern quasar image, is clearly visible. The high quality of these images has allowed detailed studies, such as lens modeling and monitoring, to be carried out.

survey (Lawrence et al. 1984).

By the early 1980s, theoretical studies of the statistics of gravitational lensing had led to an understanding of the expected rate of gravitational lensing of high redshift objects. For example, Turner, Ostriker, and Gott (1984) predicted that about one quasar in 200 should be strongly lensed. This theoretical expectation, coupled with the by now well recognized capability of the VLA to image hundreds of sources, led to an MIT effort to search deliberately with the VLA for gravitational lenses. Our proposal was at first not well received by the referees. However, with Barry Clark's encouragement, we revised and resubmitted the proposal. It was then successful, and the MIT-VLA search for gravitational lenses was launched. This effort resulted in a total of six new gravitational lens systems. The most spectacular was probably the first "Einstein Ring" gravitational lens, shown in Figure 3 (Hewitt et al. 1988).

Other searches for gravitational lenses followed. The JVAS survey of flat spectrum radio sources (Patnaik et al. 1992), originally designed to develop a MERLIN calibrator list, proved to be an efficient gravitational lens search engine as well. The success of JVAS in this regard, and the desire for lensed images that vary in their flux and polarization properties (for the purpose of monitoring, see below), led to the most ambitious lens survey to date, the Cosmic Lens All-Sky Survey (CLASS). The CLASS effort has exercised the

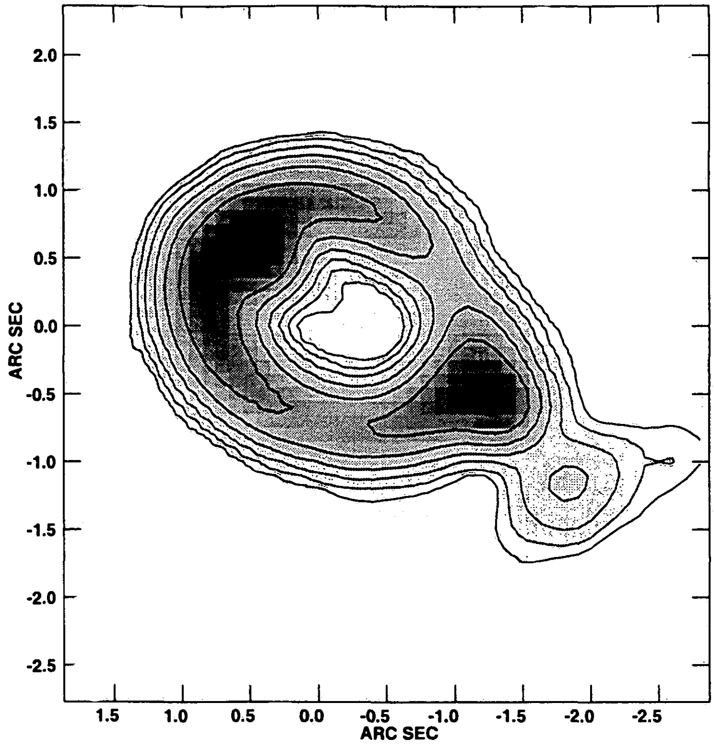

Figure 3: 15 GHz VLA image of MG1131+0456, the first "Einstein ring" gravitational lens. A particularly symmetric arrangement of the source and the lens causes the background source, in this case the lobe of a radio source, to be imaged into a ring. The two bright features are two images of the compact core of the radio source; the radio lobe at the lower right is not multiply imaged.

capabilities of the VLA the most rigorously, racing from source to source with just thirty seconds of integration time for each object. With about 10,000 sources in the northern sky surveyed, the CLASS project has been remarkably successful, with eleven confirmed lenses to date, and more lenses are likely to emerge as the candidates are further studied (Browne et al. 1998). Figure 4 shows a VLA image of a JVAS gravitational lens.

Searches for gravitational lenses continue, with the emphasis turning now to the southern sky. The VLA has excellent imaging capabilities as far south as -40 degrees in declination and undoubtedly we will soon have a southern sample of radio-selected gravitational lenses. It is probably fair to say that the most important contribution the VLA has made in gravitational lens studies is simply the identification of new lens systems. Of the approximately fifty strong lenses known, nearly half were discovered in radio surveys. Since only about ten percent of quasars are radio loud, this is obviously a disproportionately large number.

3 Galaxy Mass Distributions and Dark Matter

It has long been recognized that gravitational lenses offer a unique means to detect the presence of gravitating matter even if it is not luminous. However, the inversion problem of inferring the mass distribution from observed properties of lensed images is, unfortunately,

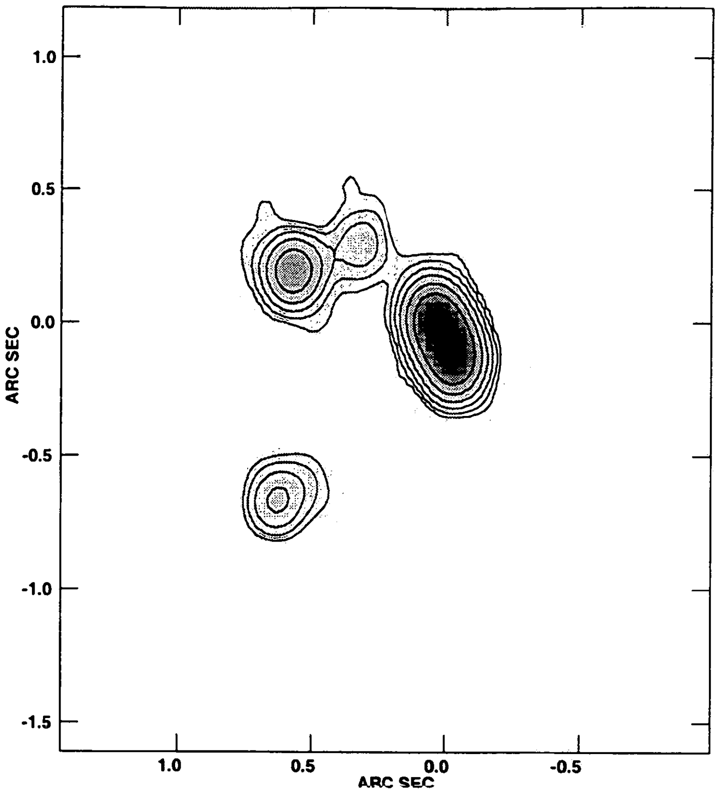

Figure 4: VLA map of 1938+666, discovered in the JVAS survey. This is an example of a "quad" gravitational lens in which four images of part or all of the background source are produced.

not straightforward. This is a pursuit for which the VLA not only has provided data, but also has provided inspiration for the analytical techniques. The most sophisticated lens inversion algorithm is that of Kochanek and Narayan (1992) which they have named "LensClean." Based on the CLEAN algorithm, familiar to radio interferometrists and made practical by Barry Clark's implementation (Clark 1980), LensClean also reconstructs a point source model of the background source. However, an extra layer of complexity is imposed by requiring that all lensed images of the point source component be subtracted from the data. The resulting residual map is treated as a measure of goodness of fit, and lens model parameters are varied in an optimization procedure. A particularly successful example of the LensClean algorithm is in the analysis of the Einstein ring gravitational lens MG1654+1346 (Kochanek 1995; Ellithorpe, Kochanek, and Hewitt 1996). As shown in Figure 5, the best fit lens and source models are an extraordinarily good fit to the data. The modeling procedure rejects constant mass-to-light ratio models in favor of isothermal models, an important result that gives strong independent evidence for a dark matter halo.

4 Gravitational Lens Cosmography

A prediction of the gravitational lens model is that, since we are viewing two background sources along two different lines of sight, variations in the source will be observed in both images, separated in time by a gravitational lens "time delay." The magnitude of the

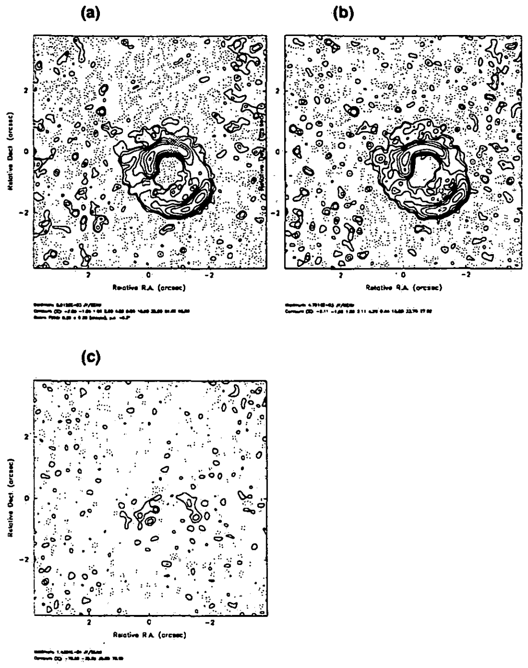

Figure 5: LensClean model of MG1654+1346. (a) VLA image. (b) Model image simulating the imaging of a background source by the best-fit mass distribution. (c) Residual image.

time delay scales with the angular diameter distance to the lens; its measurement provides an independent measure of distance on the largest cosmological scales. It is a credit to the VLA scheduling policies that extensive monitoring programs have been carried out. Figure 6 shows light curves of the 0957+561 (Haarsma et al. 1999) and 0218+357 (Biggs et al. 1999) systems, yielding time delays of 409 days and 10.5 days, respectively. Time delays have also been reported in 1608+656 (Fassnacht 1997) and 1830-211 (Lovell et al. 1998). Relating these time delays to angular diameter distances requires a good understanding of the mass distribution; we eagerly await the results of the lens modeling studies under way.

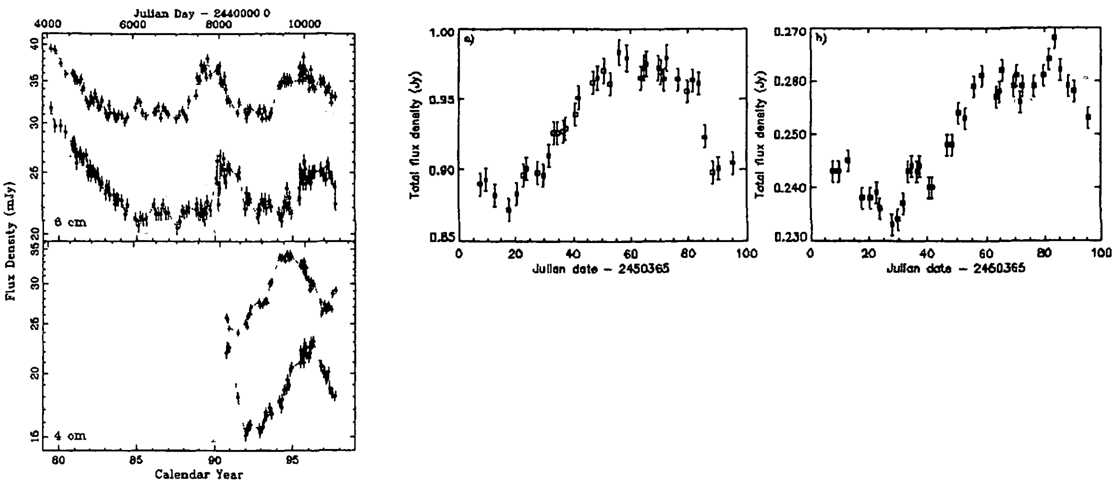

Figure 6: Light curves of the 0957+561 (Haarsma et al. 1999), left and 0218+357 (Biggs et al. 1999) systems.

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Impact of the VLA: Physics of AGN Jets

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1 Introduction

I will begin by revisiting a VLA observation of the radio jets in NGC 315 that was made twenty years ago almost to the day. I make this my starting point partly because it gave us an early hint of how the VLA could impact the study of AGN jets, and partly because it involves (yet) another "Barry Clark story." In what follows, I will not try to summarize all of the different ways in which the VLA has added to our knowledge of AGN jets, but will focus on questions about jet velocity fields and the differences between the two primary jet "flavors," using NGC 315 as a guide.

2 NGC 315 twenty years ago

On 23 June 1978, Ed Fomalont, Mike Davis, and I were given time to observe the radio galaxy NGC 315 with the "sub-VLA" — a dozen antennas on 10.5 km of the West arm and 1.5 km of the East arm. NGC 315 is one of the "giant," *i.e.*, Mpc-scale, radio galaxies, covering almost a degree on the sky (Bridle *et al.* 1976). We had previously used the Green Bank Interferometer to show that the radio source contained a narrow, kiloparsec-scale feature next to an unresolved component in the nucleus of the elliptical galaxy. The narrow feature was aligned with a bridge of emission linking the galaxy to one lobe of the giant structure, so we thought that it might be the innermost part of an exceptionally long "radio jet." Although the largest-scale structures would be resolved out, we hoped that a long synthesis would allow us to image the narrow feature in some detail because most of its flux density could be captured whenever the short-baseline fringes aligned with it.

We were not disappointed. We got good data from most of the antennas at 4885 MHz and 1465 MHz. The bases of the jet and of a counterjet were not only detected, but also *transverse*-resolved. We could therefore measure the jet's opening angle, or spreading rate, as a function of distance from the nucleus. We also saw that to make full use of the data we needed to deconvolve the sub-VLA's sidelobe pattern from a large area of sky by 1978 standards — fully 256 × 256 pixels! We needed to make a 512 × 512 dirty image and beam to do this using the Högbom (1974) CLEAN algorithm. This was a non-trivial computation in 1978, because the CLNMAP program had to be run in the same heavily-loaded DEC-10 that handled most of the off-line computing at the VLA site. Only 128 × 128 CLEANs could be submitted to the DEC-10 routinely, so we needed Barry Clark's permission to run ours.

At this early stage of VLA work, deconvolution did not have the central role in image processing that it has today. CLEAN was still seen as a temporary stop-gap, a processing step that might not be needed when the VLA was completed. (The VLA had been designed to give sidelobe levels low enough that its "dirty" images could be used directly for science.) Despite the nuisance value of our request, Barry let us run an "over-sized" CLEAN on NGC 315 on a weekend when the DEC-10 was lightly loaded.

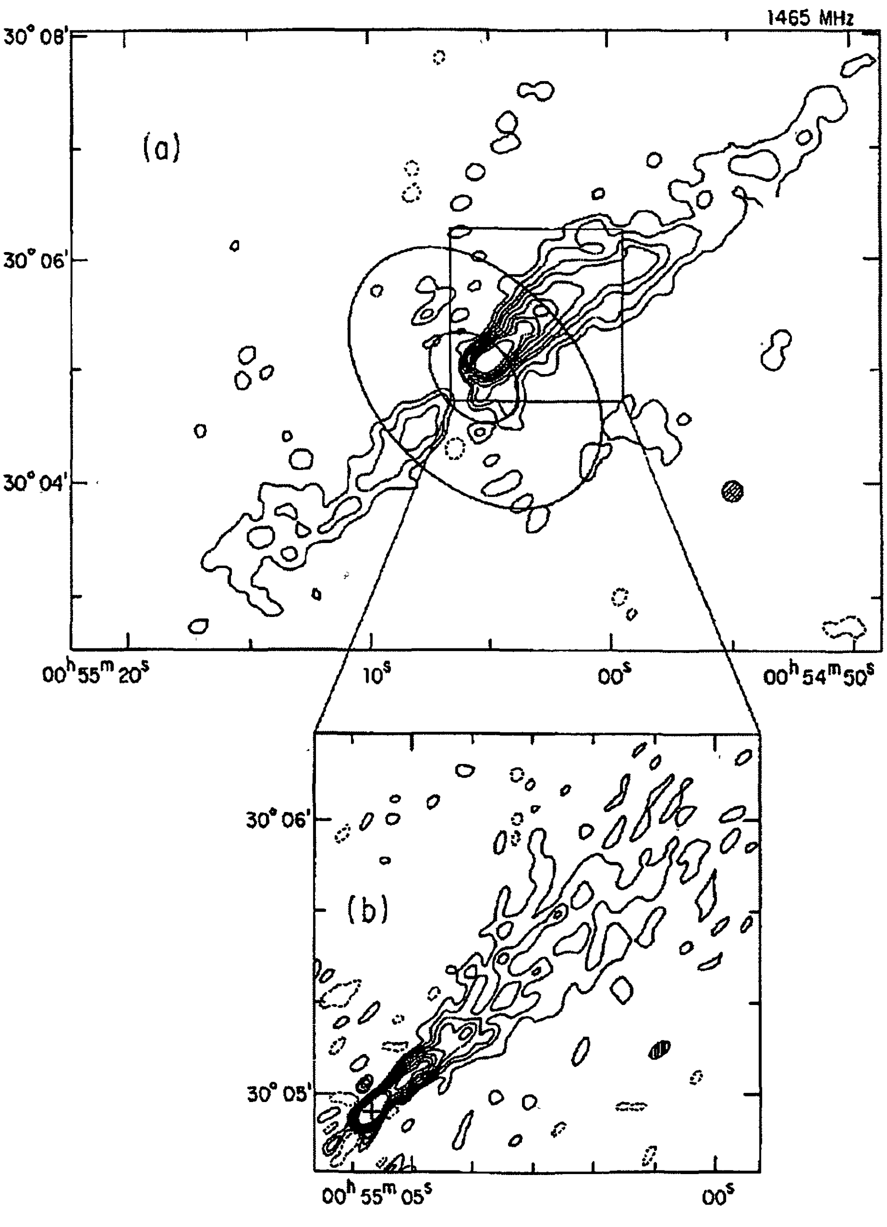

Figure 1: The upper panel shows the VLA 1465 MHz contour plot of NGC 315 at 11″ resolution; an unresolved nuclear source was subtracted. The ellipses show the size and orientation of the overexposed core and outer envelope of the galaxy on the red-sensitive *National Geographic Society – Palomar Observatory Sky Survey* print. The inset shows the VLA data at 2.5″ resolution. (From Bridle *et al.* 1979.)

Our 1465-MHz data traced the jet and counterjet for a few tens of kiloparsecs away from the nucleus (see Figure 1). The key result was that the structure of these jets changes with distance from the nucleus in several ways. The jet ridge lines "wiggle," the brighter jet clearly spreads at a variable rate, and its degree of linear polarization increases outwards. This was evidence for *ongoing* changes in the jets' collimation and internal structures, including the magnetic field configuration, on scales that were easily resolvable by the VLA. VLA observations of such jets might therefore be able to provide important clues to the physics of jet propagation on kiloparsec (and greater) scales in galactic environments.

The clientele for CLEANing "large" fields of view ballooned as more extended sources were studied with the sub-VLA. But the DEC-10 could not handle the growing demand for the Högbom CLEAN. Fortunately, Barry turned this computing problem into a wonderful opportunity, and gave us the "Clark CLEAN" (Clark 1980).

To move some of the deconvolution load out of the DEC-10, Barry coded a CLEAN for a PDP 11/70 and FPS 120B Array Processor that were later to become part of the spectral-line "pipeline" system. The FPS 120B had three control panel lights: one for power, one showing data transfer activity, and one showing array processor activity — actual computation. Barry says that watching these lights showed him that his first CLEAN code spent too little time doing the computations. Most of the time instead went into shuffling data in and out of the AP memory. The more efficient algorithm that he developed to keep the "AP activity" light lit up saved our bacon in the early 1980s. Without it, the practice of feeding CLEAN models back into self-calibration, which was the route to high image fidelity, would have been slow to develop. Barry's efficient AP microcode later went around the world as part of AIPS, and the "computation" light stayed lit up on the APs of thankful VLA users for years thereafter.

Our early look at NGC 315 with the sub-VLA previewed some features of FR I jets which may still be central to understanding differences between jet propagation in FR I and FR II sources; but it may have advanced the field more by being one of the early projects that got Barry Clark thinking about an efficient CLEAN algorithm.

Figure 2 shows the radio jets and the galaxy in modern dress, superposing a 4885 MHz VLA multi-configuration image of the radio jets on the red-sensitive image of NGC 315 from the *Digitized National Geographic Society - Palomar Observatory Sky Survey.*

3 AGN jets before 1978

Use of the term "jet" in extragalactic astronomy dates back to Baade & Minkowski (1954), who described an optical feature in M 87 as "a unique peculiarity known for a long time ... a straight jet extending from the nucleus in p.a. 290°, bluer than the nebula itself ... several strong condensations." This feature was first recorded by Curtis (1918), as "a curious straight ray apparently connected with the nucleus by a thin line of matter." Its linear polarization at optical wavelengths (Baade 1956) provided early evidence for synchrotron emission from extragalactic radio sources.

The existence of radio emission from AGN jets was also known for many years before the VLA went into operation.

The first sign of radio emission from a "jet" had come via Schmidt's (1963) identification of "a star of about thirteenth magnitude and a faint wisp or jet" near the accurate positions of the radio components of 3C 273 (Hazard *et al.* 1963). Schmidt's identification of radio component 'B' with the "star" marks the start of the quasar industry. His identification of component 'A' with the tip of the "faint wisp or jet" likewise marks the start of a radio jet industry that developed much more slowly. Six years passed before Hogg *et al.* (1969) showed that a compact extranuclear radio component in 3C 274 coincided with the brightest knot in M 87's optical jet (thus providing evidence for radio emission *from*, rather than just *around*, that optical feature).

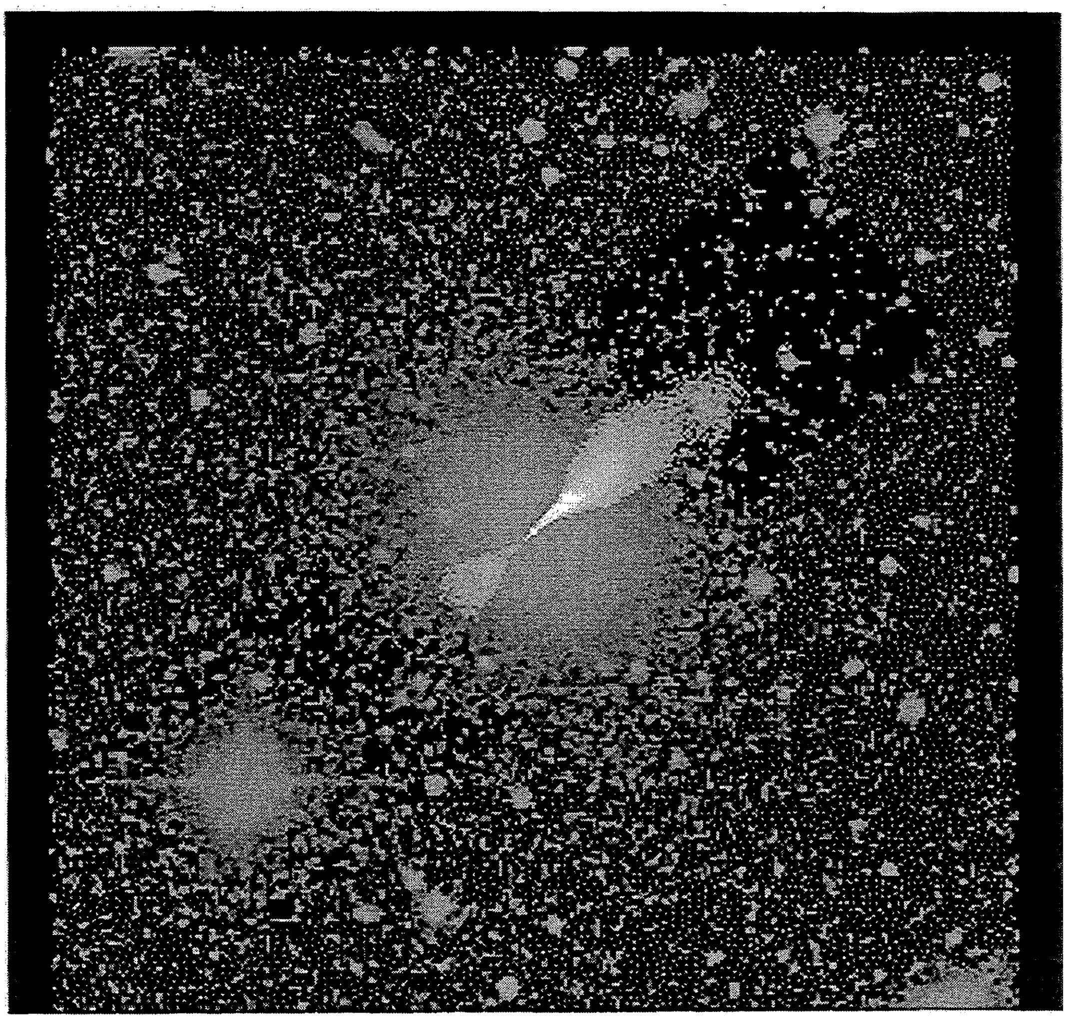

Figure 2: A modern VLA image of NGC 315 at 2″ resolution superposed on an image of the galaxy from the E plate of the *Digitized National Geographic Society – Palomar Observatory Sky Survey.*

The next evidence for radio jets came from observations made at Cambridge. Northover (1973) found a "narrow jet of emission linking the galactic nucleus to one of the extended regions" in the low-power plume-like radio source 3C 66B, and suggested that this implied a continuous resupply of energy from the nucleus to the extended source (in compact sub-components that he interpreted as buoyant "bubbles"). Hargrave & Ryle (1974) used high-resolution imaging of Cygnus A's hot spots and spectral-aging analysis to argue for "continuous replenishment of energetic electrons within the two main compact components," favoring models with "continuing ejection of beams of energetic particles or low frequency waves from the nucleus of the galaxy." The first *direct* evidence for a radio jet in a powerful "classical double" source came when Turland (1975) detected the abbreviated jet in the radio galaxy 3C 219. A spectacularly long and narrow one-sided jet was found in the giant radio galaxy NGC 6251 by Waggett *et al.* (1977).

Radio jets also were recognized in WSRT images of several radio galaxies at about this time: van Breugel & Miley (1977) reported jets in B0844+319 and 3C 129, and gave retrospective evidence for jets in several other galaxies, including 3C 449 (Högbom & Carlsson 1974) and 3C 83.1 (Miley *et al.* 1975).

4 The VLA and large scale features of jets

The VLA greatly accelerated the study of radio jets, for several reasons. It had the sensitivity to detect weak jets with short observations, the dynamic range to do so in the presence of bright unresolved emission in the galactic nuclei, and the angular resolution to separate the jets convincingly from surrounding extended structures. It also allowed polarization imaging with good sensitivity and resolution, and this revealed key details of the jets' magnetic configurations.

VLA observations quickly provided examples of jets in all types of radio-loud AGN. The detection of radio emission from, or at least closely associated with, the presumed pathways of energy transport in continuous-outflow models cemented the case for these models[1]. Furthermore, numerous correlations between the properties of the jets and other attributes of the radio sources became apparent. These included:

- **Correlations between jet properties and the Fanaroff-Riley structure classes:** The plume-like, low-luminosity, Fanaroff-Riley Class I sources (Fanaroff & Riley 1974) have two-sided, rapidly spreading and prominent jets (*e.g.*, Figure 3). The "classical double," higher-luminosity, FR II sources have one-sided, narrowly-collimated jets that are more prominent in quasars than in radio galaxies (Bridle & Perley 1984).

- **Correlations between kiloparsec and parsec scales:** The brighter kiloparsec-scale (VLA) jet is always a plausible extension of the brighter parsec-scale (VLB) jet. The kiloparsec-scale jets are also well aligned with the parsec-scale jets in the FR I sources (Giovannini *et al.* 1995; Venturi *et al.* 1994, 1995), as exemplified by NGC 315 in Figure 4, and in lobe-dominated FR II sources. The *angular* relationships are more complex in core-dominated sources whose jets appear more bent, but even in these sources the brighter large-scale jet is usually a plausible continuation of the brighter small-scale jet.

- **Correlations between jet sidedness and depolarization asymmetry:** In sources whose jets differ greatly in brightness, the brighter jet is on the side of the source that depo-

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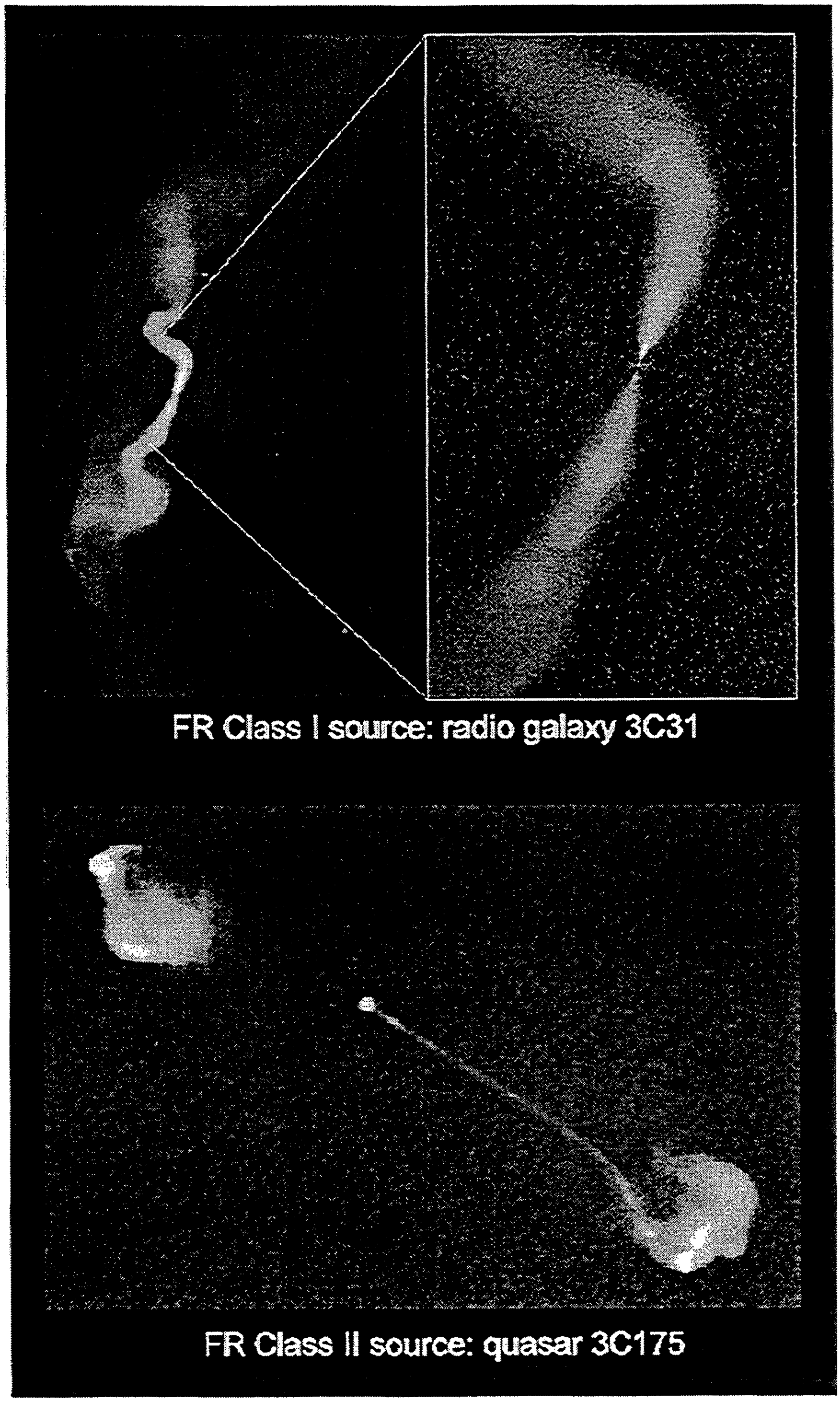
[1] The models had their roots in early papers by Morrison (1969), who outlined a pulsar-like model for an AGN emitting a continuous relativistic beam, and by Rees (1971), who suggested that the sources were powered by low-frequency electromagnetic beams. Longair *et al.* (1973) argued for an energy transport time scale "comparable with the age of the source" and Scheuer (1974) explored the dynamics of radio sources powered by relativistic beams. Blandford & Rees (1974) suggested a "twin-exhaust" collimation mechanism for relativistic plasma flows (on 100-pc scales).



FR Class I source: radio galaxy 3C31

FR Class II source: quasar 3C175

Figure 3: VLA images of jets in a Fanaroff-Riley Class I source (3C 31: 1.4 GHz, 5.5″ FWHM and 8.4 GHz, 0.3″ FWHM) and a Class II source (3C 175: 4.9 GHz, 0.35″ FWHM).

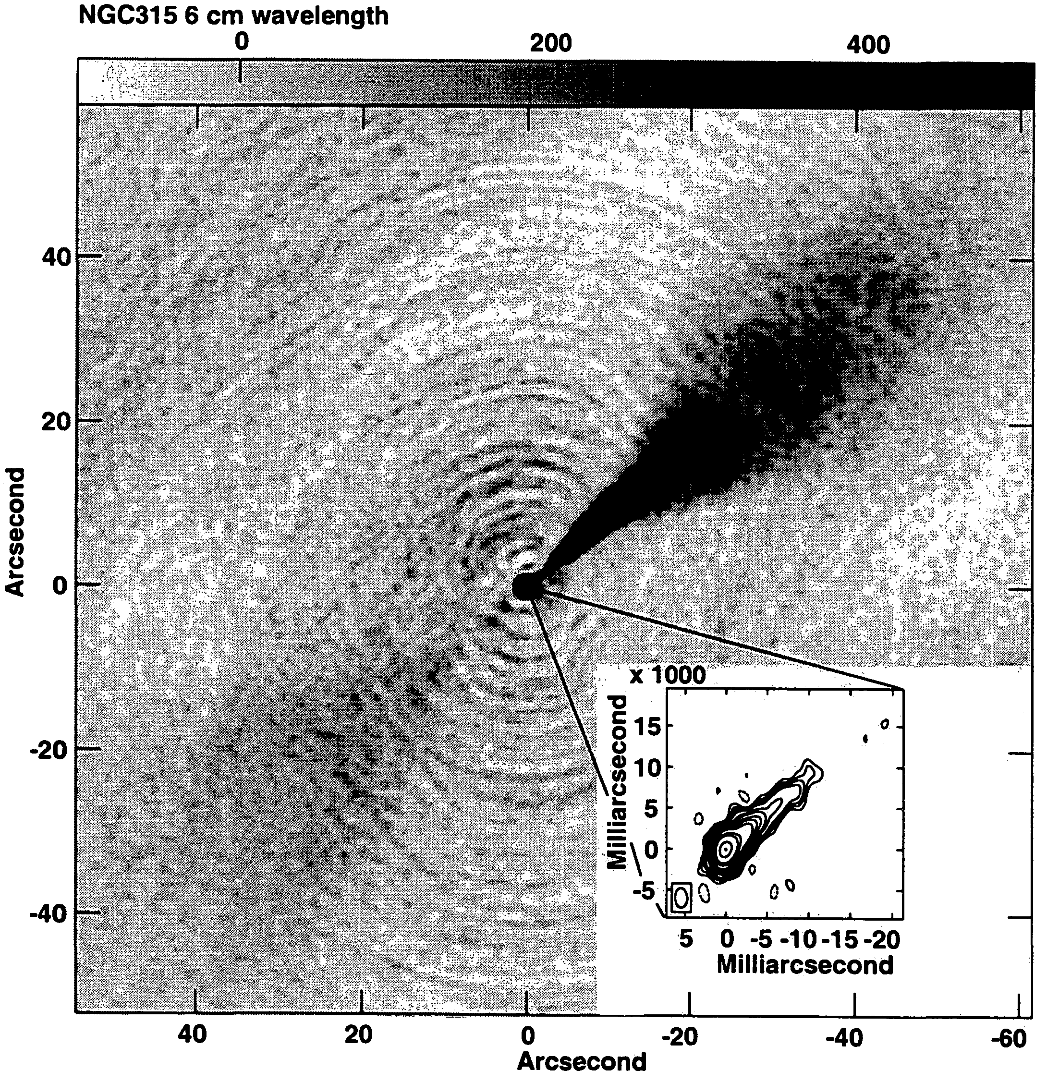


Figure 4: The correlated sidedness and good alignment of the kiloparsec-scale and parsec-scale jets in NGC 315 (superposition kindly provided by W.D. Cotton). The grayscale image is from VLA B configuration data at 4885 MHz; the inset contour plot is from VLBA data at the same frequency.

larizes less at long wavelengths (Laing 1988; Garrington *et al.* 1988, 1991; Parma *et al.* 1996). This is consistent with the brighter jet always being on the nearer side of the source.

- **Correlations between magnetic field orientation and jet sidedness:** well-resolved jets are often highly linearly polarized, so their magnetic fields are partially ordered — one-sided jet features in FR II jets and at the bases of FR I jets are dominated by field components along the jet axes, but straight FR I jets are dominated by perpendicular field components farther from the AGN (Bridle 1984).

Jet modelers now had some clear observational correlations to explain. Much early debate centered on side-to-side asymmetries and their implications for jet velocities. The bending of "head-tail" sources, many of which resolved into U-shaped twin jets, was modeled as an effect of the relative motion of the host galaxy and an intracluster medium: their symmetries in the presence of the bending argued for subrelativistic velocities and moderate Mach numbers (O'Dea 1985). The excellent collimation of jets in FR II sources suggested low jet densities and high Mach numbers (Payne & Cohn 1985; Williams 1991). The brightness asymmetries of FR II jets on many-kiloparsec scales encouraged the idea that their bulk velocities stay relativistic, like those of their one-sided and often superluminally-moving parsec-scale counterparts, even on these large scales.

5 The VLA and internal structures of jets

VLA images of bright, well-resolved FR I and FR II jets have shown that they exhibit a rich variety of internal knots, rings, loops and filaments, including some apparently helix-like structures. These internal details also provide input to models of jet physics, including:

- In FR I sources, as NGC 315 shows quite dramatically (see Figure 5), jet collimation is not determined once and for all on parsec scales. The side-to-side brightness asymmetries of FR I jets also decrease with distance from the nucleus, and towards the outer edges of the jets from their axes (Laing 1996). The innermost regions of FR I jets also resemble FR II jets (well-collimated and dominated by magnetic field components parallel to the jet axis). The collimation and internal structure of these jets evidently change dramatically on just the scale where their sidedness characteristics also change.

- The detailed radio structure of M 87's jet closely resembles that of the optical jet (Biretta 1996), and superluminal proper motions have been inferred for several features (Biretta *et al.* 1995).

- Semi-periodic strings of knots are seen in some FR II jets (*e.g.*, Bridle *et al.* 1994). A variety of phenomena could produce this effect: nonlinear growth of Kelvin-Helmholtz instabilities, criss-crossing oblique shock patterns in confined jets, or regular perturbations of the flow velocity or direction. Higher resolution imaging and polarimetry (*e.g.*, with the upgraded VLA) are needed to clarify the nature of this phenomenon, which could lead to (model-dependent) estimates of Mach number in the jets.

- In the jets of some FR II sources, limb-brightening and flat-topped emission profiles (Swain *et al.* 1996, 1998; Carilli *et al.* 1996, their Figure 5) imply diminished emissivity on the jet axis relative to the jet edges, in regions away from bright knots. It is not yet clear whether this effect is due to differential Doppler boosting in a relativistic jet with a slow boundary layer (as suggested below) or to enhanced *in situ* particle acceleration or magnetic field amplification in a boundary layer. In either case, our visualization of phenomena in FRII jets may be biased toward their outer (boundary) layers. Again, higher angular resolution and sensitivity (*e.g.*, an upgraded VLA) are needed to explore this fully.

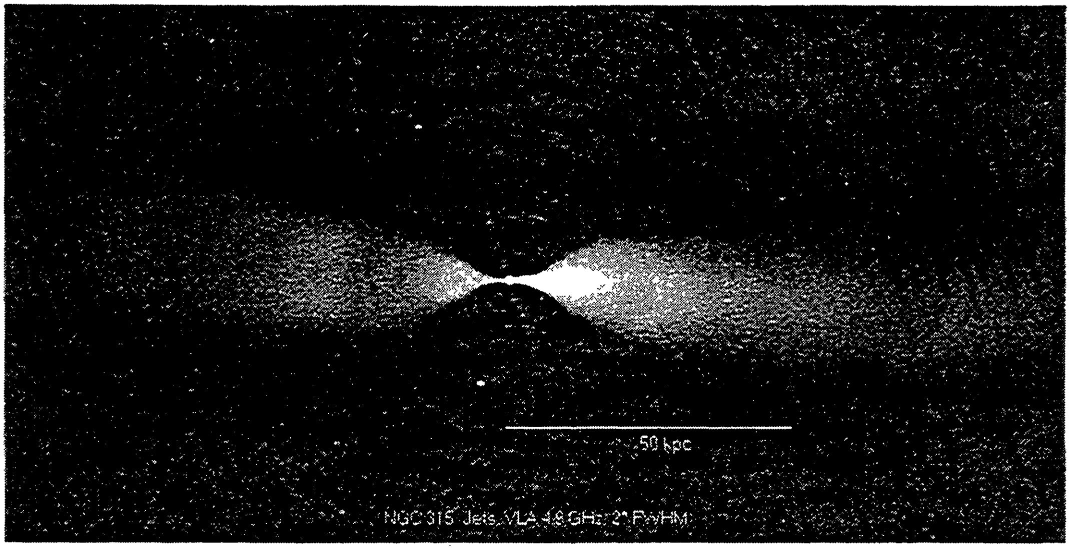


Figure 5: The recollimation of the NGC 315 jets on 20 to 30-kpc scales is clear in this VLA 4885 MHz image at 2″ resolution. The data have been rotated so that the axis of the large-scale jet is horizontal. The data combine syntheses in the B, C and D configurations (Cotton, W.D., Bridle, A.H., Laing, R.A., & Giovannini, G., in preparation.)

6 Implications for jet physics

Many aspects of AGN radio jets can now be understood in the context of bulk-relativistic outflows launched from relativistically-deep potential wells in galactic nuclei. The most likely launching mechanism is "B++" — a *b*lack hole *plus* a rotating accretion disk *plus* a magnetic field that is wound up by the rotation of the disk. In all models of jet launching from AGN by this mechanism, the jets emerge perpendicular to the axis of the disk. Direct evidence for the black holes, and for larger-scale disks around them, in active galactic nuclei is now coming from VLBI (*e.g.*, Miyoshi *et al.* 1995; Herrnstein 1998), the HST (*e.g.*, Harms *et al.* 1994; Ferrarese *et al.* 1996; Bower *et al.* 1998), and X-ray spectroscopy (*e.g.*, Tanaka *et al.* 1995).

The VLA lets us examine how these outflows evolve as they propagate in the environments of their host galaxies and surrounding groups or clusters. It now seems likely that most jets, whether in FR I or FR II sources, retain a fast relativistic component out to the kiloparsec scale where the VLA begins to resolve them transversely. A key difference between the two Fanaroff-Riley classes (Fanaroff & Riley 1974) may be that the fast central "spine" decelerates to subrelativistic speeds within the galaxy in FR I sources, but persists as far as the distant hot spots in FR II sources.

Doppler favoritism of the approaching relativistic jet can explain why the observed brightness asymmetries (or sidedness) of the kiloparsec-scale (VLA) and parsec-scale (VLB) jets are so well correlated with each other, and with the depolarization asymmetry, in both Fanaroff-Riley structure types — these attributes all diagnose which of the two jets is approaching us.

Doppler boosting and dimming in relativistic jets at different angles to our line of sight can also explain why the jets in FR II radio galaxies are harder to detect than those in lobe-dominated quasars. If the broad-line region of radio galaxies is obscured by material in a torus in roughly the same plane as the accretion disk, and if the jets indeed emerge perpendicular to this plane, then FR II radio galaxy jets will generally be closer to the plane of the sky than those in lobe-dominated quasars. (The latter are tilted towards us, allowing a view of the inner, broad-line, region.) The approaching jet in an FR II quasar can therefore be Doppler *boosted*, making it more prominent relative to the lobes (as well as strongly "one-sided"), while both jets in an FR II radio galaxy are Doppler *dimmed*, making them hard to detect until they terminate at the hot spots.

In the context of relativistic-jet models, the symmetrization of the jets in FR I sources with increasing distance from the galactic nucleus suggests that they decelerate as they propagate through the galactic environment on kiloparsec scales. The idea that some relativistic flow extends to ~ 1-kpc scales even in FR I sources is bolstered by the large proper motions found in M 87 on these scales (Biretta *et al.* 1995). Bicknell (1995) showed how the bulk deceleration of FR I flows by mass entrainment from galactic atmospheres might explain why the FR I to FR II transition depends on both the luminosity of the radio source and on the optical luminosity of the galaxy (acting as a proxy for the central gas pressure).

If these ideas are correct, then high-resolution imaging of the bases of jets in FR I sources like NGC 315 and 3C 31 offers a way to probe how the galactic environment decelerates outflows whose initial momentum fluxes are lower than in FR II sources. Robert Laing and I are exploring how well models of decelerating relativistic jets can match the detailed intensity and polarization distributions of several FR I sources that are well resolved by the VLA. We assume that the fast-moving jet spine develops a slower-moving boundary layer, across which there is transport of gas from the galactic atmosphere, causing the spine to decelerate. The flow is initially fast, resembling that in an FR II source. But for a source at an intermediate angle to the line of sight, first the slower moving flow in the boundary layer, and then the flow in the jet spine, pass through the velocity at which their emission is optimally Doppler boosted. As pointed out by Komissaroff (1988, 1990) this can explain why both jets appear to brighten after an initially dim region (as they emerge from Doppler "hiding"). It can also explain:

- why the bases of FR I jets look so much like the FR II jets (both are fast, well-collimated, and faint on both sides because of Doppler "hiding" at high speeds and large angles to the line of sight),
- why the FR I jets spread so rapidly where they apparently brighten (the deceleration that brings the spine to optimum Doppler boost also dumps bulk kinetic energy into internal energy and inflates the jet against the surrounding gas pressure), and
- why the outer isophotes of FR I jets are more side-to-side symmetric at all distances from the nucleus than their inner isophotes (they are where the flow is slower, at all distances).

Figure 6 compares a model of the emission from a decelerating relativistic jet with VLA data for 3C 31 at the same resolution. In this model, the fast-moving spine, which occupies about 40% of the jet by radius, has a single velocity across the jet but decelerates with increasing distance from the nucleus. In the outer layer the flow velocity decreases linearly from the spine velocity closest to the axis to a low value at the edge of the jet. The jet and counterjet are intrinsically identical. The velocity field, the variation of the emissivity with distance from the nucleus, and the jet geometry have all been adjusted to fit the data. The (fitted) angle of the jet to the line of sight is 52°; the spine velocity decreases from $0.88c$ closest to the nucleus to $0.17c$ farthest from the nucleus, while the velocity at the edge of the jet decreases from $0.7c$ to $0.11c$.

We have also specified the jet's internal magnetic field configuration. We assume that the spine contains random loops of magnetic field with no component along the direction of motion, while the boundary layer contains random loops with no component across the velocity shear. By accounting for the relativistic aberration of these partially-ordered magnetic fields we can also account for both jets' polarization properties, as shown in the lower half of Figure 6.

Although some details of the polarization "weather" (particularly a loop-like structure in the brighter jet) are unexplained, the model describes the total intensity and polarization "climate" in

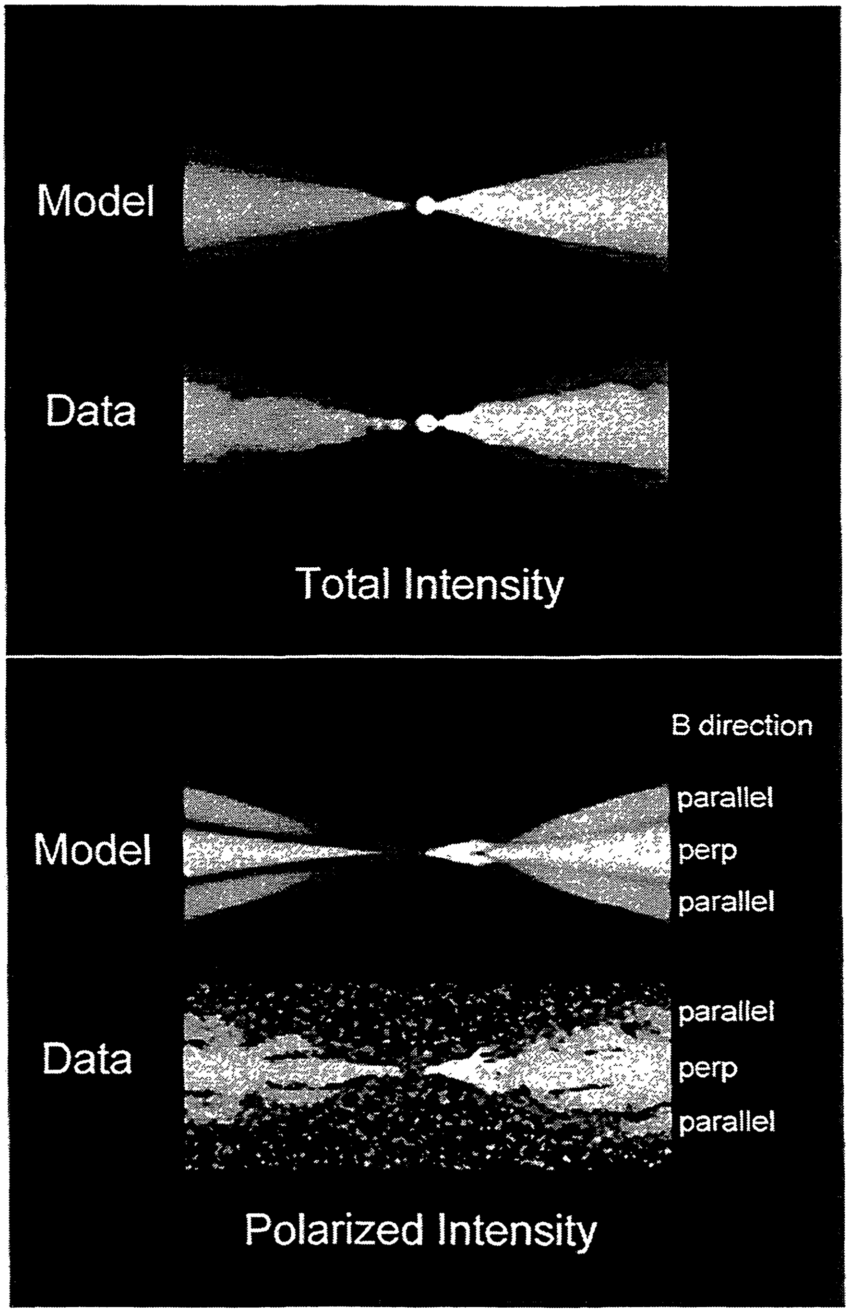


Figure 6: Direct comparisons of a decelerating relativistic-jet model (see text for details) and the VLA total (upper panel) and polarized (lower panel) intensity data for the inner parts of the jets in 3C 31 at 8460 MHz (Laing, R.A., & Bridle, A.H., in preparation).

3C 31 well. Its ability to fit the major features of the observed total and polarized intensities for *both* jets with the same velocity distribution and magnetic field parameters suggests that the basic precepts of the decelerating-jet picture are correct.

Well-resolved VLA data on FR I sources like NGC 315 and 3C 31 may therefore allow us to infer information about the jet velocity fields, and thus about the kinematics of jet-gas interactions, in elliptical galaxies. This will be an important step towards understanding the galactic-scale processes that determine the differences between FR I and FR II sources.

7 Looking ahead

The decelerating-jet picture of FR I sources associates their apparently parallel-field regions with the jet boundary layer, where we expect to find a strong velocity shear. The inter-knot emission from jets in FR II radio galaxies appears to be dominated by parallel-field configurations, so this emission may also be visualizing slower-moving material at the jet edges. If so, outflow velocities derived from the side-to-side brightness asymmetries of such jets may systematically under-estimate the velocities in the jet spines, which may stay hidden from us until they reach the hot spots. It will be important to learn whether the innermost regions of FR I jets, and of jets in FR II radio galaxies, generally appear edge-brightened, or at least flat-topped, when observed with good sensitivity and high transverse resolution. As mentioned earlier, there is evidence for a lack of emissivity near the jet axis in the large-scale jets of the FR II sources 3C353 (Swain *et al.* 1996, 1998) and Cygnus A (Carilli *et al.* 1996) but improved angular resolution and sensitivity (such as offered by the VLA Expansion) will be needed to pursue these questions in the general population of FR II sources.

It also will be important to determine whether jets and counterjets in the same FR II source have similar transverse intensity and polarization profiles, and whether these profiles are systematically different for narrow-line radio galaxies, broad line radio galaxies, and quasars (which are expected to be at systematically different angles to our line of sight in orientation-based "unification schemes").

Although rapid deceleration of the FR I jets on kiloparsec scales may *de*collimate these jets, the jets also rapidly *re*collimate, as Figure 5 clearly shows. The hot, *extended*, component of the galactic (or circumgalactic) atmosphere required to produce the recollimation by thermal pressure alone has so far eluded detection at X-ray wavelengths in NGC 315 (*e.g.*, Birkinshaw & Worrall 1996). Chandra may tell us how serious this problem is for decelerating-jet models. Alternative explanations for the recollimation phenomenon might be found in the two-fluid (beam-wind) model of Sol *et al.* (1989), which seeks to explain it using $\mathbf{J} \times \mathbf{B}$ forces.

If decelerating-jet models of FR I sources pass further scrutiny, then jet kinematics inferred from VLA imaging may be used to constrain models of mass entrainment into relativistic jets traversing galactic atmospheres. The eventual goal should be to explain the major features of the collimation changes, brightness distributions, and polarimetry of these jets using a full dynamical model of the flows, rather than the simple kinematic modeling illustrated in Figure 6.

8 Acknowledgments

I thank Bill Cotton and Robert Laing for many discussions and ongoing collaborations; and Barry Clark for everything he has done, and still does, to make the work described here possible.

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VLA SNAPSHOT SURVEYS

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1. The VLA as a Survey-Dependent Instrument

A major design goal of the VLA was speed, but with moderation. The NRAO VLA Report No. 1 (NRAO 1965) rejected an array capable of making a fairly good dirty image with a single short scan, or "snapshot," as both scientific overkill:

"A 73 element array can yield a complete picture, to a resolution of 10″, of a 5′ diameter area in about one minute of observing time. Even with generous allowances of time for pointing the antennas and for maintenance, such an array could map tens of thousands of sources per year, far more than could be utilized by all the world's astronomers."

and engineering folly:

"Furthermore, the difficulty of maintaining in working order 73 antennas and receivers and 2620 correlators is frightening to contemplate."

Consequently, the final VLA proposal (NRAO 1967) concluded soberly:

"In view of these considerations, the speed requirement of the VLA has been set at 12 hours—that is, it should be possible to map one field of view in one continuous observing period of no longer than 12 hours, without any change of array configuration."

This speed would allow astronomers to survey limited areas of sky and detect enough new sources to study the cosmological evolution of radio galaxies and quasars: "Thus, some 1,000 sources could be observed in one month, for statistical studies." Still, the proposed VLA was clearly not intended for surveying the whole sky. Following the NRAO specifications, John Kraus (1977) noted in his review of radio surveys that:

"... the new Very Large Array (VLA) of the National Radio Astronomy Observatory being constructed at a cost of $80 million on the Plains of San Augustin in New Mexico will have such high resolution that to survey the entire sky at this resolution would take centuries. The VLA is not an all-sky survey instrument but a *survey-dependent* instrument."

He was not exaggerating—covering the 217,446 fields of the all-sky NRAO VLA Sky Survey (NVSS) at 12 hours per field would take 3.0 centuries of observing time. I know of only one astronomer who is bold enough to ask for 3.0 centuries on the VLA, and it is not me.

Being "survey-dependent" was a serious scientific limitation of the proposed VLA. Its survey capability was barely adequate for counting small numbers ($\sim 10^3$) of faint sources in small areas of sky. Moreover, making such source counts was no longer a high scientific priority even before the VLA was finished, as Barry Clark noted with one of his trademark zingers lurking in an otherwise straight memo on VLA computing requirements (Clark 1974):

"Observer B wishes to measure the log N–log S relationship at all four frequencies. Because he is rather discouraged by what the relation has done for science so far, he decides it is worth only a half day at each frequency."

Beyond this, the proposed VLA was largely confined to studying known sources. This is efficient for learning more about the old but is unlikely to find something new and different. Radio galaxies, quasars, BL Lac objects, pulsars, and gravitational lenses are important objects or phenomena that were discovered serendipitously in earlier radio sky surveys. Unfortunately, detecting 10^3 new sources is no longer likely to yield new discoveries. The reason is that most of the 10^3 sources in a flux-limited sample are just the usual radio galaxies and quasars that (1) are so distant that they are difficult to identify optically and follow up in other wavebands and (2) have already been studied to death. If the VLA is to observe other objects and become more relevant to the rest of astronomy, it needs radio surveys extensive enough to detect those sources which are intrinisically rare in flux-limited samples. These include gravitational lenses, X-ray galaxies, optically bright normal galaxies and low-luminosity AGN, Galactic stars, pulsars, planetary nebulae, and "new" objects. Tens of thousands of sources may indeed be too many for all the world's astronomers to utilize, but hundreds of thousands of sources are needed to yield usable numbers of interesting ones.

All-sky single-dish surveys, such as the 5 GHz GB6 (Condon et al. 1994) and PMN (Griffith & Wright 1993) surveys, have detected $\sim 10^5$ sources and provided the VLA with some statistically useful samples of these "rare" objects to study. For example, about 350 of the brightest 10^4 galaxies in the northern hemisphere (Nilson 1973, UGC) were detected at 5 GHz and later imaged by the VLA (Condon et al. 1991). Still, single-dish surveys are not very sensitive by VLA standards, and their positional accuracy is generally not good enough for making reliable optical identifications.

2. VLA Snapshot Surveys

Shortly after the VLA was completed, it became clear that fairly good CLEAN images could be made from snapshot data. However, making these images was a very slow process. In 1980 Ken Mitchell and I surveyed 25 deg^2 of sky at 1.41 GHz in SA 57 near the north Galactic pole. Each primary-beam area was observed for only 3 × 2 min, but Ken considered himself lucky to produce one CLEAN image per night using the IBM 360/65 in Charlottesville. Such survey observations could have covered the sky in 3 years instead of 3 centuries, but making the images would have taken 6 centuries! It is no wonder that the VLA and its associated off-line computers were compared with "a jet engine on a bicycle." Fortunately for the NRAO, we never got a Cray to replace the bicycle. Instead, Moore's law brought us a fleet of smaller, faster, cheaper bicycles, so that by the early 1990s it became practical to process an all-sky VLA snapshot survey with a few desktop workstations. At about the same time, the VLA L-band upgrade halved the system noise, so that the observations could be speeded up without loss of sensitivity. For the first time, a VLA sky survey was technically feasible.

But is an all-sky VLA sky survey scientifically worthwhile? The VLA clearly brings an order-of-magnitude improvement in sensitivity, resolution, and position accuracy. Will 10^6 sources tell us more than 10^5? If the universe of radio sources were scale-free, the answer might have been "no." However, the radio universe is far from scale-free. VLA surveys can reach certain characteristic flux densities and angular scales which allow *qualitatively* different science to be done for the first time. For example, luminous radio galaxies and quasars dominate the radio source population at flux densities $S \gg 1$ mJy, but starbursts and normal galaxies become important near $S \sim 1$ mJy. Cosmological evolution is so strong that the median redshift of extragalactic radio sources is $\langle z \rangle \sim 1$, independent of flux density. Thus most extragalactic radio sources are produced by rather faint galaxies, and position uncertainties $\sigma \leq 2''$–$3''$ are needed to make reliable optical identifications. Within our own Galaxy, planetary nebulae are strongly concentrated around the center, at a distance $D \sim 8$ kpc. The most luminous contain central stars on the horizontal branch have ionizing luminosities $L \sim 10^3 L_\odot$ and are thermal radio sources with characteristic flux densities $S \sim 10$ mJy. Consequently, the quantitative gains from VLA sky surveys translate into *qualitative* changes in scientific opportunities. The human factor is important, too. With single-dish surveys, it was usually necessary to confirm radio source identifications by laborious and time-consuming follow-up VLA observations. This discouraged use of single-dish surveys by all but the most determined astronomers. In contrast, images and catalogs from the two VLA snapshot surveys [FIRST (Becker et al. 1995) and the NVSS (Condon et al. 1998)] contain most of the necessary information in easy-to-use form on the Web, bringing my dream of "armchair radio astronomy" much closer to reality. Making radio surveys more accessible extends their user community beyond the small group of hard-core radio astronomers and greatly increases the likelihood that the data will yield new science.

Some examples of NVSS sources are shown in Fig. 1. The NVSS has detected more than 50 of the brightest radio stars, most of which are new. It detected 79 of the known pulsars by their continuum emission, and may find some new ones by their steep ($\alpha > 1.5$) radio spectra. About 80% of the known planetary nebulae with $\delta > -40°$ were detected by the NVSS, and more than 100 very good new candidates were found by selecting warm IRAS sources with radio emission. About half of the brightest (> 0.1 counts s^{-1}) extragalactic X-ray sources in the ROSAT catalog (Voges et al. 1996) are NVSS sources, and the more accurate NVSS positions can generally resolve ambiguous optical identifications. Over half of the NGC and UGC galaxies were detected by the NVSS. The NVSS is about as sensitive as the IRAS Faint Source Catalog (Moshir et al. 1992) for galaxies obeying the famous far-infrared/radio correlation, and the smaller NVSS position errors again help resolve uncertain optical identifications.

3. Interferometry and Imaging

Making the low-resolution, wideband, widefield NVSS snapshot images required violating, or at least bending, the rules underlying the standard algorithms designed to image small fields. While trying to solve these problems, I frequently encountered the work of Barry Clark, who had considered them decades before.

For example, the NVSS snapshot observations are made with a nominal IF bandwidth $\Delta\nu \approx 50$ MHz, so sources far from the phase center are significantly blurred by bandwidth smearing. Barry dreamed up the following deconvolution scheme (Clark 1975): make a normal image, regrid it in polar coordinates, cut the image like a pie into narrow slices, and replace the radial coordinate with its logarithm. Bandwidth smearing is radial and proportional to distance from the phase center, so it can be represented by a convolution in logarithmic radial coordinates and removed by deconvolution. Simply Fourier transform each wedge and divide by the Fourier transform of the bandwidth-smearing "beam" to eliminate the bandwidth smearing. Then retransform this quotient and regrid to get the unsmeared image. Logarithmic radial coordinates diverge near the phase center, but bandwidth smearing isn't important there, so the original map center may be left intact and merged with the corrected image. This algorithm has not yet been implemented in AIPS.

After reading how to do it right, I decided to fake it. Fortunately, the amount of bandwidth smearing in the NVSS is small, so it can be tamed by a correction applied during CLEANing. Before each CLEAN component is added back to each residual visibility, it is multiplied by a factor > 1 which compensates for the visibility reduction on that baseline caused by bandwidth smearing at that offset from the phase center. A normal image made from the corrected visibilities is nearly free of bandwidth smearing.

A computationally expensive three-dimensional Fourier transform of the three-dimensional (u, v, w) visibility data is formally required to image any finite portion of the spherical sky away from the instrumental zenith. However, the VLA baselines are nearly horizontal, so the VLA is almost coplanar during a snapshot. The usual two-dimensional transform of the (u, v) data can be used to make a widefield image from a single snapshot observation, but the direction cosines in the resulting image differ slightly from the correct values. This distortion causes position errors as large as $18''$ at the edges of the southernmost NVSS snapshot images. Barry derived the equations for the distortion in a memo (Clark 1973), which we applied to correct our snapshot images geometrically. This memo is famous for the classic line:

"This effect arises because the world is round (Magellan 1522)."

In these and numerous other aspects of making the NVSS, I depended on unpublished advances in the theory of interferometry and imaging made by many people, especially Barry Clark. At this conference it is appropriate to acknowledge their essential contributions and thank them for making possible the VLA all-sky survey that could not have been made without them, not even in 3.0 centuries.

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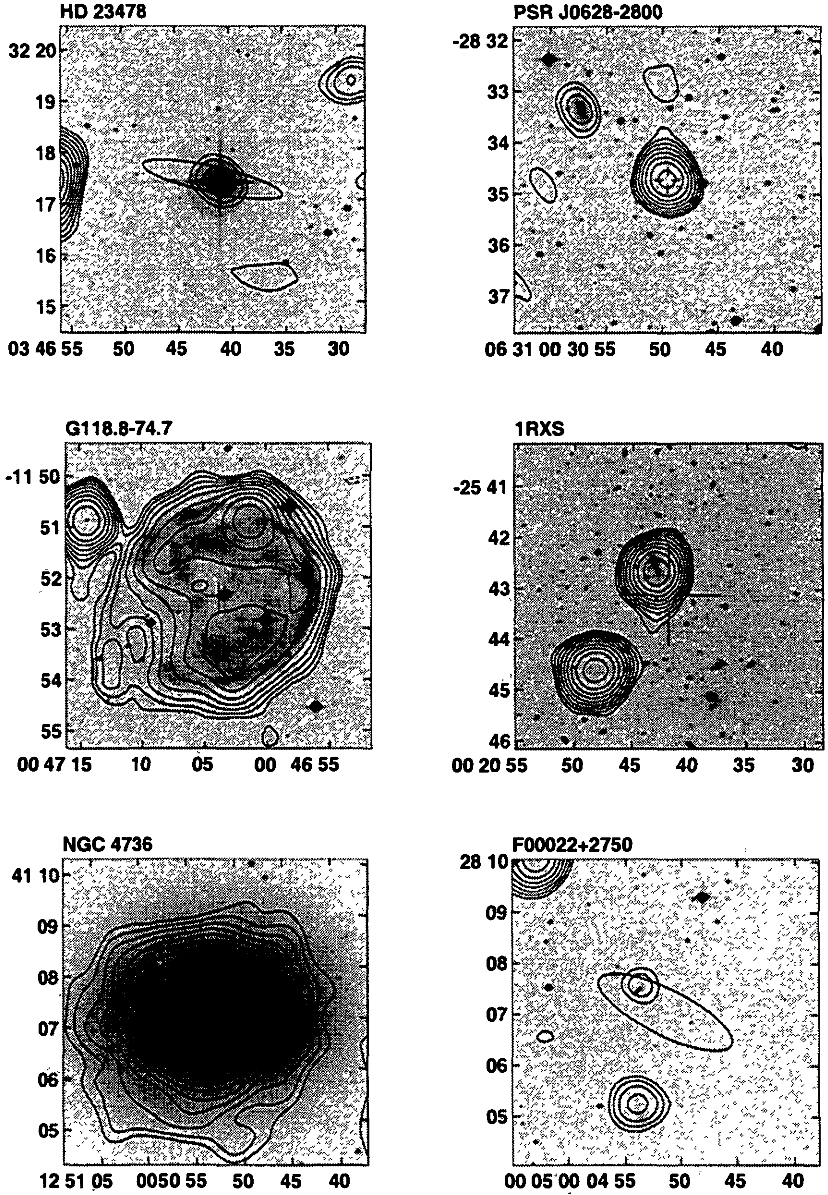

Fig. 1.— Six postage-stamp images showing the NVSS (contours) over Digitized Sky Survey optical images (gray scale). The contours are plotted at 1 mJy beam^{-1} × $\pm 2^0$, $\pm 2^{1/2}$, $\pm 2^1$, $\pm 2^{3/2}$.... Abscissae: J2000 right ascension. Ordinates: J2000 declination.

Section IV: Development of the VLBA

Opening Remarks by Session Chair

George W. Swenson, Jr.
University of Illinois

My interaction with Barry was primarily in connection with the early planning of the VLA, in Green Bank and Charlottesville during the period 1964-68. Shortly after I arrived in Green Bank, Barry came, fresh out of graduate school but already a mature scientist and clearly the person most experienced in interferometry. He was immediately added to the VLA Design Committee.

I was pleased to observe that he owned a well-worn pair of hiking boots and that he was not averse to squirming through a muddy crawl passage in a limestone cave. It was comforting to know that if he could squeeze his impressive physique through there, I undoubtedly could as well. Those weekend adventures in the Allegheny caves and on the trails are pleasant memories.

Barry quickly emerged as a sought-after consultant on all kinds of technical problems. Sometimes he could be ambushed in the hall; at other times he accepted more formal assignments. It was not unusual, as has been observed by others in more recent years, for a problem to be presented in some detail by an anxious colleague, only to be disposed of in a word or two, or possibly by a skeptical grunt.

At other times the inquiry might be met by a noncommital "Hmmmm" and this might be followed in a day or a week by a carefully crafted, insightful memorandum giving the solution. Too often on such occasions I had to ask myself, "Why didn't I think of that?" I looked in the "Barry" file among my VLA memorabilia and discovered the following titles, among others, all dated in the mid-1960s:

A CONTINUOUS APERTURE APPROACH TO THE VLA

THE CALCULATION OF THE BRIGHTNESS DISTRIBUTION USING THE ALGORITHM OF COOLEY AND TUKEY

HOLES IN THE U-V DIAGRAM

TWO BIT CORRELATOR

THE COMPUTER SYSTEM OF THE VLA

ON THE PHASE CENTER OF A PARABOLOID

I recall particularly the last memo as a particularly nifty example of the last scenario, triggered by a hallway conversation on a vexing little problem that Barry solved with an elegant analysis.

Thus, in the earliest days of the VLA program, Barry Clark was making fundamental contributions. These feats are all the more remarkable when one recalls that in the same time period he played a central role in the pioneering development of VLBI.

Early Days of VLBI

Marshall H. Cohen

California Institute of Technology

Introduction

The early days of VLBI have been well-documented (e.g., Cohen et al 1968; Burke 1969; Broten, 1988; Galt, 1988; Kellermann & Cohen 1988; Kellermann 1992; Moran 1998; see also chap 1 in Thompson, Moran & Swenson 1986). There is little reason to repeat that material, and I content myself here with a brief recapitulation of the scientific and technical developments of those days which made VLBI necessary and possible, with some remarks on the early VLBI equipment. Along the way I will provide some anecdotes including a few concerning Barry. Then I will discuss the VLBI Network, which was useful and sociologically interesting, and has not had much publicity.

The Situation in 1965

In 1965, Henry Palmer and his colleagues at Jodrell Bank were operating a radio-linked interferometer with a baseline of 127 km at a wavelength of 21 cm, giving $d/\lambda = 6 \times 10^5$ (Anderson et al 1965, Palmer et al 1967). The resolution was well under $1''$, and quasars were still unresolved. Similar constraints came from interplanetary scintillation studies (Little & Hewish 1966; Cohen et al 1966), and lunar occultations (e.g., Hazard, Gulkis & Bray, 1967). In addition, some sources (the same ones, of course) were known to be variable, and the short time scale showed that the sources were tiny (Dent, 1965). Another strong argument came from the curved spectra; synchrotron theory (plus an educated guess for the magnetic field) showed that the sources should have diameters of 1 to 10 milliarcsec (Slish 1963; Williams 1963).

Thus, in 1965, there was a pressing need for an interferometer with two orders of magnitude more resolution than was available. Why did VLBI not arise earlier? The answer lies mainly in the lack of readily available frequency standards. It was clear that radio-linked interferometers could not be greatly extended, because of cost and technical problems. Intensity interferometers, which make no attempt at coherent detection and so could be as long as you like, had in fact been in use for a dozen years, but were severely limited by low SNR (Hanbury Brown & Twiss 1954). The solution would have to come from using *independent local oscillators* at the two ends, and recording the IF signals on tape for later correlation. But until the mid-60s commercial oscillators did not have the required stability, which can easily be computed to be around one part in 10^{11}. In 1965, the Varian Company started selling a rubidium frequency standard which put out a clean 5-MHz signal. Another commercial development at that time was relatively cheap tape recorders for television; the Canadians recorded on these TV tape drives, but we used digital tape drives. In 1965 there were several written discussions on the possibilities of VLBI: two papers from the USSR (Matveyenko et al 1965; Slish 1965), and three reports by H. Gush that circulated privately in Canada (Gush 1988). (The Matveyenko et al. paper incorrectly stated that the required oscillator stability depends on the length of the baseline.)

The NRAO/Cornell Collaboration: Mk I

In 1965, Ken Kellermann and I were talking about compact sources and interferometry, and then Ken found out that Varian was selling a good rubidium standard. So we began promoting a VLB system (a pun on VLA) to operate between the 140-foot antenna at Green Bank and the 1000-foot at Arecibo, at 610 MHz. The initial group was Ken, Barry Clark and Claude Bare at NRAO, and Dave Jauncey and me at Cornell. We opted for a simple one-bit digital system that would be self-clocking, and the data at each end would be recorded on standard computer tape. This minimized both the software and hardware requirements. We started with double-sideband parametric amplifiers at 50 cm. Claude and Barry designed the hardware, which involved assorted amplifiers and filters with a clipper and tape control circuitry. Barry wrote the software. At each antenna we recorded ones and zeros on a modified computer tape drive, using standard 7-track tapes. A tape was written at maximum speed - 720 kbps, and the effective bandwidth was 330 kHz. A full tape lasted about 3 minutes, and took an hour to correlate on the IBM 360/50 machine at NRAO. A few years later, at Caltech, we were correlating on an IBM 360/75 and that took 10 minutes per 3-minute observation.

All that may sound easy but there were difficulties. I'll tell a story that illustrates our *modus operandi*. My role was mainly to be a *nudge* and keep asking people how things were going. I would occasionally visit Charlottesville and we would hang around the computer room in the basement until very late at night. One night there was trouble – some test was not coming out right but there was nothing obviously wrong anywhere. I said, "Are you sure the code is correct?" Barry grunted or something. I said, "Well, let's go over it." So Barry and I sat there and he went down this Fortran code line by line, explaining it to me. Suddenly he stopped talking and all went quiet. After a minute he got up, went to the key punch machine (remember that?), punched up a few cards, shuffled them into the deck and ran the program again and it worked. It had to do with carrying the phase across the data gap. The remarkable thing is that Barry did all this in his head; there were no formulas on paper to look at.

Interestingly, I did a similar thing at Caltech ten years later with Marty Ewing, another person who understood computers. We were trying to make the Mk II correlator work with 3 stations, and there was the interesting matter of the fractional bit shift. Something wasn't right, and Marty was explaining it to me line by line (actually, bit by bit) when the solution became apparent. That time, however, we had to convince ourselves with diagrams on paper. Anyway, now I can claim to be a correlator consultant, and if Jon Romney had called on me the VLBA would have been working a year earlier.

I have another story from the early days, when we crawled around on the floor in the computer room in Charlottesville, looking for fringes. It was the night of January 25, 1967. Barry and I were scratching our heads over why we had no fringes. Barry was on the phone occasionally. It got late. Then Barry said he guessed he would leave, as his wife had had a baby that day.

Such heights of cool are rarely reached. The baby, now Dr. Marin Clarkberg, is an Assistant Professor in the Sociology Department at Cornell. And, it was a while before we actually got useful fringes.

Frustrations

From the beginning, it was clear that multi-station experiments were important, and already in January 1968, only 8 months after the first fringes, we did a 3-station observation at 6 cm, using the 140-foot at Green Bank together with telescopes at Haystack and Onsala. The system worked without trouble at 6 cm, although the original design was for 50 cm. We had worried about stability more than was necessary, but that was good because it let us jump to 6 cm without changing the equipment, other than to add a synthesizer.

After the discovery of superluminal motion in early 1971, multi-baseline observations got even more popular. In the spring of 1971 we proposed to set up regular observations on the FOG baselines (Fort Davis, Owens Valley, and Greenbank). The Fort Davis telescope was used primarily to study the sun, but it was the only suitable telescope in the mid-US to fit in with the cross-country baselines and we were able to use it. Originally we planned to make observations every three months, but we couldn't keep to that schedule. Often we added in other telescopes, especially the 46-meter at Algonquin Radio Observatory (FOGA) and the 100-meter in Bonn (FOGB). These observations were a real pain in two ways. First, the run had to be organized, with innumerable phone calls. Each telescope was jealous of its prerogatives and each director had to be convinced that each run was worth his telescope's valuable time. Then we had to send collaborators to each station, to run the telescope and the VLBI equipment, and change tapes. That wasn't easy either, as some telescopes had helpers and others didn't and in fact often two people had to go to a telescope, to run around the clock. All that was the first pain. The second pain was that the correlator (by now the Mk II version with a hardware correlator and 1.8 MHz bandwidth) ran only one baseline at a time, so frequent experiments with more than two telescopes meant that the NRAO correlator was running ever farther behind. (The queue got longer than 6 months.) Worse, the correlator was unreliable, so reruns were common. And those of you used to the VLA and the VLBA should not think that tapes went to Charlottesville and fringe amplitudes came back. The reality was that you had to personally go to Charlottesville (or send a student) and tend to the care and feeding of the correlator. Now that I try to recall the Ampex recorder, with its 2-inch tape full of dropouts and its multi-dimensional alignment scheme designed for an octopus by George Grove, I marvel that we got any results.

Actually, there was also a third pain, which I would just as soon forget because it often derived from our own foolishness. The handwritten letter from Barry that I now quote gives the idea.

Dear Marshall,

> Imagine my amusement when, on dumping a few seconds of the three cm VLB tapes, I found the NRL tapes were all zeros, and the CalTech tapes were all ones. We have found our loose wire, (which I personally disconnected – shame, shame) and I recommend you find yours posthaste.
>
> Love,
>
> Barry Clark

Unfortunately, this letter is undated, but Dave Shaffer says it must have been in the Summer of 1968. Perhaps we could be forgiven for mistakes then, for it was soon after the start, and there were lots of connections to be made and tests to be done. But years later similar things were still happening, crossed polarizations, mis-set synthesizers, bad clock settings, and such. The VLBA was supposed to eliminate all these types of problems, and, fortunately, now it usually does.

The spectroscopy and geodesy people avoided the correlator problem by sticking with the Mk I system (a 3-station Mk I system, with a hardware correlator, was built at Haystack) until Mk III came along, but we felt that the extra bandwidth, with a noise reduction factor of 2.3, was worth the pain. I am not sure we were right. Had we stuck to Mk I until Mk II was available with 1-inch IVC recorders, and spent that same effort on carefully monitoring and studying the strongest sources, we might have learned more.

In 1972, Arthur Neill and I couldn't take it any more, and we began a correlator program at Caltech. This was a joint Caltech/JPL effort, and the prime workers were Dave Rogstad at JPL and Marty Ewing at Caltech. In early 1973 we got the first funding, $13K from the Caltech President's Fund. Those of you who have built correlators know that it is easier to start a correlator than to finish it. We finally did, and it began producing real data in 1976. It was expanded to three stations in 1977, and then to four and five in 1978 and 1979. At the beginning we used 2-inch tapes but soon switched to the more reliable IVC 1-inch system. VCR technology came later, and that was even better. In the 70s NRAO also built a new correlator, with 3 stations. So our correlator power went up a lot, but the demand always exceeded the supply. More groups, in the U.S. and Europe, came into the game, and probably more importantly, the experiments got bigger. In the late 70s, closure phase became important, and 8-station experiments were common. And, once people got the taste of 8 stations they went to 12 and even to 20 (but not before the 16-station Block II correlator was built). A 24-hour 8-station experiment takes 5 24-hour days to process on a 5-station correlator, assuming perfect efficiency. On the Charlottesville 3-station processor that would take 13 days, so most big experiments were done at Caltech.

The VLBI Network

The frustration of the early 70s that I have described was spread over the country and into Europe and something had to be done. VLBI promised great opportunities, but they seemed unreachable. In April 1974, a number of us got together in Charlottesville to talk about it. Three themes were expressed: (1) we had to get organized so that each experiment did not have to be treated as if VLBI had been invented only the week before, (2) the (u,v) coverage was intolerable and a new antenna was needed to close the "Midwest gap," and (3) the fundamental problems were incurable and a dedicated VLB array was needed. In July 1974 Dave Heeschen invited people to form a Design Group to investigate these possibilities. Ken Kellermann agreed to lead the effort to promote a dedicated array; and I agreed to lead group 1, to invent an organization that would turn the existing telescopes into a useable array on occasion.

The Network had to be bootstrapped into existence. To get it going we organized a small committee, the VLBA Design Group; soon after this we were enjoined by Bill Howard from using the term "array" with this effort, because NRAO already was working on an array (the VLA) and Congress might wonder why the radio astronomers needed two new arrays. Hence the "VLBI Network of Existing Telescopes." In early 1975 we sent a draft report to the observatories; this was essentially a communal proposal for observing time, plus a recommendation for upgraded facilities. The Network would be the group of telescopes, bound together at regular intervals to do VLBI observations. There would be a Network Users Group (NUG) which would arrange for telescope time and act as a buffer between observers and the telescopes. The NUG would collect and referee observing proposals, as if it were an independent telescope. We proposed to start Network operations with 6 runs per year, a week every two months, and the first period would be March-September 1976.

Late in 1975, we invited the VLBI community to submit proposals for the first observing semester. Forty people, from 16 organizations in the US and Canada, responded to this first call for proposals. Many of them are in this room today. That group of 40 became the first members and included users plus any other interested person. (See the Appendix.) On behalf of the NUG the final Report was written in December 1975. It was simultaneously a proposal to 7 telescopes for VLBI time at specific dates, and a recommendation to the telescopes to get new and better equipment and to get new people to help with the VLBI observations. This report was sent to the Directors of the telescopes, and an information copy was sent to the NSF. In a sense we were asking for money, but we were not a legal organization and could not submit a formal proposal to NSF. The telescopes approved it, probably with trepidation, and we got underway in April 1976. I was the first Chairman of the NUG, to be followed two years later by Jim Moran, who was followed by Don Backer. The numbers of proposals increased rapidly, and soon after the start we had to add a Scheduler, Jim Moran. Later schedulers were John Broderick, Don Backer, Bruce Leslie, Bob Phillips, Colin Lonsdale, and Craig Walker. The schedulers actually did a lot of work, receiving proposals, getting them refereed, and negotiating with the telescopes.

The first immediate success of the Network was to provide observing time – in this respect it acted like a normal telescope. It received proposals, had them refereed, and the Scheduler shoehorned them into the available time. In essence, the telescopes gave up a substantial part of their autonomy during the NUG sessions - a foreign organization fixed the details of the observing schedule! Some of the telescopes (Green Bank, Arecibo, Haystack) did not relinquish their scientific responsibility, however, but insisted that the proposals also had to pass through their own normal reviewing machinery. Other successes included a technical committee that promoted standardization (Alan Rogers, Benno Rayhrer, Marty Ewing), and a useful newsletter, mostly written by Jim Moran. The Network also lobbied the telescopes for increased VLBI support, with limited success, as that took money.

The NUG was the most raucous organization I ever have worked with. Those of you who went to the NUG meetings in Boulder and elsewhere will know what I mean. The correlator situation probably was the main cause but in-absentia observing stimulated a great deal of shouting also. As proposing and scheduling were successfully implemented, groups began asking for bigger experiments, with more antennas, and of course that strained the correlators. But there also were complaints about poor receivers, and difficulty in getting tape, and the need for more observing assistants, and the irritating inattention to detail that caused failed experiments. These problems could only be cured with money, and we were singularly impotent in that regard. Only the observatories could ask for money, and their priorities seldom coincided with ours. Our biggest failure was with the Midwest Telescope. We encouraged George Swenson and Bob Mutel to submit proposals to build a telescope at their home station, and we lobbied NSF to support one of them. This was not entirely self-serving on the part of Bob and George, for the telscope would become the first telescope of the future dedicated array. But NSF did not buy this idea, and years later Pie Town was built as the first antenna of the VLBA.

At the October 1979 NUG meeting in Boulder, Bernie Burke and Irwin Shapiro proposed that the parent organizations of the Network telescopes should form a Consortium for VLBI, which would have more clout and (hopefully) could address problems which were beyond the reach of the NUG. The first meeting to discuss this in detail occurred in San Francisco in January 1980. Joel Orlen, a vice-provost of MIT and an exceptionally good organizer, provided some draft bylaws and progress was smooth and rapid. Jerry Wiesner, president of MIT, invited 5 institutions (U. of Iowa, U. of Illinois, U. of California Berkeley, Harvard, and Caltech) to join MIT in a Consortium to provide better management for VLBI. All this worked and the Consortium came into being in 1981. The NUG continued but no longer had a managerial function. The Consortium, however, did have a treasurer, Bob Mutel. Every participating institution paid $2,000 into Bob's pot as needed (but not more than once per year), and Bob doled (often loaned) it out so that, for example, filters at all telescopes could be the same. For all I know, Bob and the University of Iowa still have a small sum which somehow belongs to the VLBI community.

The Consortium was supposed to help and it did, but only in part. We did not get a Mid-West telescope, although we get some NSF money for VLBI support at the telescopes. Life has become much better with the VLBA. The

VLBA was sold on the idea of good (u,v) coverage, high-frequency capability, and complete instrumental compatibility, but it also has provided greatly improved reliability and convenience. Irwin Shapiro had cautioned us not to talk about those last two, as they wouldn't sell at NSF or in Congress. But in fact they are now of major importance, for many purposes as important as the other considerations.

I think that the NUG and the Network were rather special. We were a self-appointed, self-organized, self-refereeing group. We were not chartered by NSF or by any observatory. Our reason for existence was to make VLBI easier and better. There are other organizations, like the Divisions of the American Astronomical Society, with a similar role but we had no legal status, no corporate home, no memorandum of understanding, no budget, and not even any bylaws. Remarkably enough, we did have some influence, and it was a lot of fun.

I thank Don Backer, Barry Clark, Rini Clarkberg, Ken Kellermann, Jim Moran and Dave Shaffer for comments and filling in gaps in my memory.

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Appendix. The First NUG

The first Network Users Group, listed here, was formed in December 1975. Those present at the current meeting to honor Barry Clark, in June 1998, are marked with an asterisk.

Backer, D.C.*	University of California, Berkeley
Brandie, G.W.	Queen's University
Bridle, A.H.*	Queen's University
Broderick, J.J.	Virginia Poltechnic Institute and State University
Burke, B.F.*	MIT
Dieter, N.H.	University of California, Berkeley
Erickson, W.C.*	University of Maryland
Ewing, M.S.	Caltech
Fomalont, E.B.*	NRAO
Hashick, A.	MIT
Howard, W.E. III*	NRAO
Johnston, K.J.*	Naval Research Laboratory
Kellermann, K.I.*	NRAO
Knowles, S.H.	Naval Research Laboratory
Lo, K.Y.	Caltech
Maxwell, A.	Harvard College Observatory
Mayer, C.H.	Naval Research Laboratory
Moffet, A.T.	Caltech
Moran, J.M.*	Smithsonian Astrophysical Observatory
Mutel, R.	University of Iowa
Niell, A.E.	Jet Propulsion Laaboratory
Rayhrer, B.	NRAO
Readhead, A.C.S.*	University of Cambridge
Reid, M.*	Smithsonian Astrophysical Observatory
Rogers, A.E.E.*	Haystack Observatory
Romney, J.D.*	Caltech
Schilizzi, R.T.*	Caltech
Schwartz, P.R.	Naval Research Laboratory
Seielstad, G.A.*	Caltech
Shaffer, D.B.*	NRAO
Shapiro, I.I.	MIT
Spencer, J.H.	Naval Research Laboratory
Swenson, G.W. Jr.*	University of Illinois
Thacker, S.L.	Naval Research Laboratory
Waak, J.A.	Naval Research Laboratory
Walker, R.C.*	MIT
Waltman, W.B.	Naval Research Laboratory
Wilkinson, P.N.	Caltech
Yen, J.L.	University of Toronto
Cohen, M.H.*	Caltech, Chairman

THE EARLY DAYS OF VLBI

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1. INTRODUCTION

I would like to give an idiosyncratic view of the early days of VLBI, as seen through the eyes of a young MIT graduate student. I had the good fortune of arriving at MIT when the radio astronomy program there was just getting off the ground. This experience first brought me into contact with Barry Clark in the spring of 1967. That was an exciting year for me and everyone involved in VLBI, and it resulted in the awarding of the Rumford Prize by the American Academy of Arts and Sciences to three groups whose members and current activities are listed in Table 1. In this paper, I describe the inception of MIT's radio astronomy program and its role in the development of VLBI, as an outgrowth of the effort to resolve OH masers. In all of this, Barry Clark had a big influence on me.

More comprehensive descriptions of the early development of VLBI can be found in the literature. Cohen et al. (1968) and Kellermann & Cohen (1988) described the early efforts at NRAO and Cornell; Broten (1988), Galt (1988), and others described the Canadian VLBI program in a special issue of the Journal of the Royal Astronomical Society of Canada; Burke (1969) wrote about the MIT program on masers; Kellermann (1992) recounted the history of the early Russian VLBI experiments; and Moran (1998) described many details of the 12 experiments undertaken in 1967.

2. RADIO ASTRONOMY AT MIT

We heard earlier in this conference of the important role that E. G. "Taffy" Bowen played in the development of radio astronomy at Caltech while Lee DuBridge was its president. He was instrumental in getting John Bolton from CSIRO to Caltech, where Bolton founded the Owens Valley Radio Observatory and built its first interferometer (Cohen 1994; Kellermann 1996; Radhakrishnan 1993). Bowen also played an indirect role in the founding of radio astronomy at MIT. He had extensive connections with the radio physics community in the U.S. through his involvement in the MIT Radiation Laboratory during World War II. He came to the U.S. in September 1940 with the Tizard Mission, whose purpose was to share Britain's recent technical advances with the U.S., in exchange for help on further technical development and production. He arrived carrying the newly invented cavity resonant magnetron in a suitcase (Bowen 1987). The visit led directly to the formation of the Radiation Laboratory at MIT later that year, under the directorship of Lee DuBridge. During World War II, the Radiation Lab developed airborne radar at wavelengths of 10 cm, 3 cm, and finally 1.3 cm. Another product of the Radiation Lab was the Dicke radiometer, invented by Robert Dicke and used by him to detect the moon at 1.3-cm wavelength and to study the water vapor in the atmosphere (Dicke 1946). Most of this work took place in MIT's Building 20 on Vassar Street in Cambridge. This temporary construction of wood and asbestos was hastily assembled in 1940 for wartime research. It was finally torn down this year, almost 60 years later, to make way for a

TABLE 1: 1971 Rumford Prize

NRAO/Cornell	Canadian	MIT
Bare (D)	Broten (R)	Moran (A)
Clark (A)	Legg (R)	Barrett (D)
Kellermann (A)	Locke (D)	Burke (R/A)
Cohen (R/A)	McLeish (D)	Rogers (A)
Jauncey (A)	Richards (R)	Carter (A)
	Chisholm (D)	Crowther (C)
	Gush (R)	Ball (A)
	Yen (D)	Hyde (R)
	Galt (R)	

NOTE—A: active in radio astronomy; C: changed fields;D: deceased; R: retired.

dramatic new laboratory designed by Frank Gehry. Building 20 was an experimenter's dream: walls could be knocked down easily and holes of arbitrary size could be bored into floors and ceilings to accomodate equipment. Before its demolition, a conference was held in its honor by its many loyal former occupants.

Another radar research facility, the Radio Research Laboratory (RRL), was established at Harvard under the direction of Fred Terman and housed in a wing of the biological laboratories (Bethell 1995). It specialized in radar countermeasures (e.g., chaff and active jamming). At RRL, J. H. Van Vleck worked out the simple relationship between the correlation function of a Gaussian signal and its clipped representation. The relation was published in a classified report (Van Vleck 1943) and much later in the open literature (Van Vleck & Middleton 1961). Van Vleck's motive was to find a way to generate broadband interference. Many years later, his calculation provided an efficient means of implementing digital spectrometers.

When the war ended, the Rad Lab was closed. The community of physicists there, who had been so effective in developing radar, dispersed and returned to promising areas of physics research. The Research Laboratory of Electronics (RLE) took its place for peacetime research at MIT.

Why radio astronomy flowered after the war in England, the Netherlands, and Australia, but not initially in the United States—at least not at MIT—is a matter of some conjecture (see discussions by Buderi 1996; Kelves 1978; Moran 1994; and Townes 1995). One contributing factor may have been that the U.S. Department of Defense (DOD), which sponsored most research at that time (the NSF was not established until 1950), was more interested in continuing the development of microwave electronics, which were not then sensitive enough for radio astronomical work. (Much later, DOD agencies such as the Office of Naval Research provided a great deal of support for radio astronomy.) In any event, Albert Hill, a Rad Lab alumnus and director of RLE, invited Taffy Bowen to MIT in 1951 to lecture and discuss the possibility of developing a radio astronomy program there. Bowen's advice apparently fell on deaf ears—with the exception of a pair belonging to Bernie Burke, then a young graduate student at MIT, who heard the lectures and became interested in the topic (Fleischer 1990). After finishing his thesis on the Zeeman effect on the rotational spectra of polyatomic molecules under M. W. P "Woody" Strandberg in 1953, he joined the fledgling radio astronomy group in the Department of Terrestrial Magnetism at the Carnegie Institution in Washington.

About 1961, Henry Zimmermann and Jerome Wiesner, with encouragement from Strandberg, started the radio astronomy program at RLE that was to grow into the present-day effort (Zimmermann 1987). Zimmermann was the director of RLE and Wiesner was then a professor of electrical engineering, before going on to become science advisor to President Kennedy and later president of MIT. Under Wiesner's direction, Sandy Weinreb built the first digital spectrometer for radio astronomy (Weinreb 1961, 1963), which he used with the first 85-foot antenna in Green Bank to search for the deuterium line at 327 MHz and the Zeeman effect in the HI line. Both observations yielded negative results (Weinreb 1962a, 1962b).

It was a stroke of good luck for all of us that got Sandy Weinreb into radio astronomy. In the spring of 1957, he was looking for a summer job between his junior and senior years at MIT and started dialing phone numbers out of the yellow pages under the heading of electronics. He got as far as the listing for the Ewen-Knight Company, called them, and was offered a job. Ewen-Knight had just built a spectrometer for Tom Gold at Harvard and it didn't work; Weinreb and a fellow MIT student, Ron Weimer, were assigned to fix it. (Ron Weimer would go on to a long career in the electronics lab at NRAO in Green Bank and Socorro, retiring in 1999.) It was Ewen who suggested to Sandy the idea of searching for interstellar deuterium. Sandy pursued this project for his bachelor's thesis under the direction of William Siebert, an electrical engineering professor specializing in signal processing, and continued the project for his Ph.D. thesis at MIT. From his experience at Ewen-Knight, he knew the inherent problems and inflexibilities of filter banks, and he decided to build a digital correlator for the task of searching for interstellar deuterium. His investigation of the number of bits required for signal quantization led him to the one-bit design. He described his idea to Irv Stiglitz, who told a crestfallen Sandy that Van Vleck had analyzed the problem in 1943. Sandy went to Wiesner, who in very short order raised $60,000 for the project from the NSF (Wiesner 1987). Weinreb's correlator had 21 lags and a clock rate of 300 KHz. It had one flip-flop circuit per board and occupied three racks. The output appeared on 21 rows of neon lights, from which the operator copied the information by hand and punched it onto cards for analysis by an IBM 1620 computer.

In 1962, with encouragement from Ed Lilley at Harvard, MIT hired Alan Barrett as its first radio astronomer (Zimmermann 1987). Barrett, who came from the University of Michigan, had done his thesis on microwave spectroscopy with Charles Townes at Columbia in 1956, and he set off to implement the ideas of Townes and Shlovsky of finding molecular lines in the radio spectrum of cosmic sources (Barrett 1958). Barrett's early searches for OH at the Naval Research Laboratory with Ed Lilley were unsuccessful, probably because the frequency was not known accurately enough.

Barrett had a very clear idea of what he wanted to do when he came to MIT, as can be seen from his first progress report at RLE in January 1962 (Barrett 1962). He had his sights on millimeter spectroscopy of the earth's atmosphere, planetary atmospheres, and beyond. He wanted to build receivers for antennas at MIT and Lincoln Laboratory and participate in planetary fly-by missions. The other members of the group at that time included Bill Graham (faculty); Tom Anderson, John Blinn, Vic Chung, John Cummings, and Dave Staelin (graduate students); and Steve Badessa, Jack Barrett, and Vincent Bates (engineering staff). One of Barrett's first projects was to install a 10-ft antenna on the roof of Building 26 and equip it with a receiver at 4-mm wavelength. I think he had in mind searching for molecules in that wavelength region, but the technology was too difficult at the time for the expected signal strengths. His first Ph.D. student was Dave Staelin, whose thesis was an attempt to detect the highly pressure-broadened line of water vapor in the atmosphere of Venus with the 28-ft spun-cast antenna at Lincoln Laboratory. He didn't find any water. If they had known where to look, they could have detected water masers even with their modest sensitivity and wide filter bandwidths.

By the time Barrett came to MIT, accurate frequencies for the ground-state OH transition were available from Townes' lab (Ehrenstein, Townes, & Stevenson 1959; also Radford 1962). Barrett teamed up with Sandy Weinreb, Lit Meeks, and Joseph Henry, and in the fall of 1963, using Weinreb's enhanced one-bit digital correlator on the Millstone Hill radar antenna, the group detected OH in absorption against Cassiopeia A. I was a first-year graduate student enrolled in Barrett's radio astronomy course at the time. Hearing the blow-by-blow description of the detection and confirmation was thrilling. Barrett was very conscientious about his teaching obligation: I don't think he skipped a single class during this busy time.

The MIT Lincoln Laboratory completed the 37-m Haystack radar facility in 1964. It was originally conceived for communications experiments and optimized for operating at 3.5-cm wavelength. To calibrate the antenna by radio astronomical observations, Sandy Weinreb was hired to build receivers and a correlator. Lincoln Lab also assembled a powerful group in planetary astronomy, whose members included John Evans, Tor Hagfors, Gordon Pettengill and Irwin Shapiro. Barrett gained access to Haystack in 1964 and with Alan Rogers and others, discovered the strong polarization of OH masers. They actually stumbled onto this result because of the dramatic changes observed in the spectrum as a function of the parallactic angle of the alt-azimuth-mounted telescope. From its inception, Haystack was superbly well equipped with the latest and most expensive technical gadgets. It had one of the earliest production hydrogen maser frequency standards, built by Len Cutler and Bob Vessot at Varian Associates (for timing with the planetary radar), many of the newfangled Hewlett-Packard frequency synthesizers, and computers with direct a/d interfaces.

So MIT was an exciting place to be for radio astronomers in the early 1960s, with a growing program and connections to Lincoln Laboratory. MIT was also very strong in antenna theory, led by Len Jan Chu, John Ruze, and Herb Weiss. A major focus of research was signal processing and information theory. Claude Shannon was there (although few ever saw him), as were Amar Bose, Robert Fano, Y. W. Lee, Alan Oppenheim, and Harry van Trees. All the ingredients were in place for cutting-edge work in spectroscopy, complex signal analysis, and EM devices.

There were other astronomy activities at MIT in the early 1960s. The high-energy research program was founded by Bruno Rossi, who came to MIT in 1946 from Los Alamos, where he had worked on the Manhattan Project (Rossi 1990). In the beginning, he worked with Herbert Bridge and others on cosmic-ray measurements. Early students who later became prominent in the high-energy astrophysics programs at MIT included George Clark (Ph.D. 1952), Stanislaw Olbert (Ph.D. 1953), and Hale Bradt (Ph.D. 1962). Rossi had strong connections with the local firm American Science and Engineering, Inc. (AS&E), which was founded in 1957 by his student Martin Annis. Rossi collaborated with Giaconni's group at AS&E on the discovery of the first extrasolar X-ray source, Scorpio-X1, made in 1962 with a rocket-borne detector. This project was funded by the Air Force with the primary objective of searching for X-ray florescence from the moon. Shortly thereafter, George Clark and Hale Bradt started balloon and rocket X-ray programs. Walter Lewin and Herb Schnopper came in 1966 and 1967, respectively. Early students in the X-ray program included Jeff McClintoch, Saul Rappaport, and Harvey Tananbaum. In addition, Herb Bridge and Alan Lazarus studied the solar wind with satellite-borne experiments. Bill Kraushaar, with help from George Clark and Gordon Garmire, discovered the first cosmic gamma-ray emission with a satellite-borne detector. There was other activity in addition to high-energy experimental astrophysics at MIT. Townes was at MIT as provost, but his research program involved laboratory spectroscopy and lasers. He left in 1965 to start an infrared astronomy program at Berkeley. Theoretical work during the 1960s included pioneering computer modeling of stellar evolution by Icko Iben, developments in the theory of density waves in spiral galaxies by C. C. Lin, the dynamics of colliding galaxies by Alar Toomre, and wide-ranging work by Phillip Morrison.

3. Early VLBI

Bernie Burke returned to MIT from the Carnegie Institution as Professor of Physics in 1965, and he immediately started an interferometry program to measure the sizes and positions of OH masers at 18-cm wavelength (L band). He drafted two of Barrett's students, Alan Rogers and me, to help build a 700-m-baseline interferometer with the Haystack and Millstone antennas in 1965, and a 13-km-baseline interferometer with the Millstone and Agassiz antennas in 1966. We measured the fringe visibilities of OH masers on the Millstone–Agassiz interferometer and found them to be unresolved, and we were eager for higher resolution. In late 1966, the development of VLBI systems was underway in Canada and the U.S., but no fringes had been successfully observed. Burke suggested that OH masers could be used as fringe finders because their narrow bandwidths obviated the need for a broad delay search. He started in motion a frantic effort to build a VLBI station at Haystack from scratch. Our group consisted of John Ball, Al Barrett, Bernie Burke, Joe Carter, Patricia Crowther, Jerry Hyde, Alan Rogers, and myself. Alan Rogers designed and built the phase-locked LO, the IF electronics, and the video converter. John Ball, with help from Norm Brenner, wrote the machine language software necessary for one-bit correlation. This job was conceptually simple, since the desired operation between the two bit streams was an "exclusive-or" instruction; the difficulty lay in shifting the data streams across word boundaries. Patty Crowther wrote the interface software to the Univac computer. She had an undergraduate degree from MIT and was working as a programmer at Haystack. (Her father was Robert Page, one of the pioneers in the American development of radar at NRL in the 1930s.) Jerry Hyde consulted on frequency stability issues. I wrote the data analysis software, did much of the wiring on the receiver, handled the logistics, and worried a lot. The NRAO/Cornell system had a sampling rate of 720 kilobits per second. In the MIT system, we had two filters of 6 KHz and 120 KHz matched to the linewidth and spectrum extent of typical OH masers and recorded data at the appropriate rate. With the NRAO terminal, we used the 720-KHz sampling rate but extracted only every 60th or every 3d bit, depending on the bandwidth.

It is interesting to note that NRAO was less of a self-sufficient observatory in the 1960s. There were spare receiver boxes for the 140-ft telescope in which outside groups could install their own receivers. Henry Taylor, one of the colorful characters on the early support staff at Green Bank, drove one of these boxes to Haystack in April 1967, where we installed the L-band receiver with its parametric-amplifier front end. On the morning of the changeover to the VLBI experiment, the box belonging to Connie Mayer's group at NRL was being removed. Virtually every cable in the control room was uncoupled, and a whole new cabling setup was installed for the VLBI experiment.

The first L-band VLBI experiment, between the 37-m Haystack telescope and the 43-m NRAO telescope, got underway on June 5, 1967. Burke brought the first tapes back to Haystack midway through the observations. Reams of paper poured out of the line printer, but no fringes. Then they suddenly appeared (Figure 1). As it turns out, I had programmed the delay compensation in the computer with the wrong sign. The fringes appeared when the delay went through zero, which happened on this baseline at a local hour-angle of about 4 hours at NRAO. One might think that we were very lucky to have hit the zero crossing of the delay with 3-minute observations taken every 15 minutes or so. However, in the 6-KHz mode, the samples were spaced every 83 microseconds, and a 16-point correlation function was being computed. Hence, the delay range was about ±600 microseconds, while the baseline length was about 3000 light microseconds, and strong fringes could be seen for about a half an hour on either side of the baseline transit. Even with 700 K system temperatures, the SNR was enormous, and the behavior (and misbehavior) of the frequency standards could be readily seen (Figure 2).

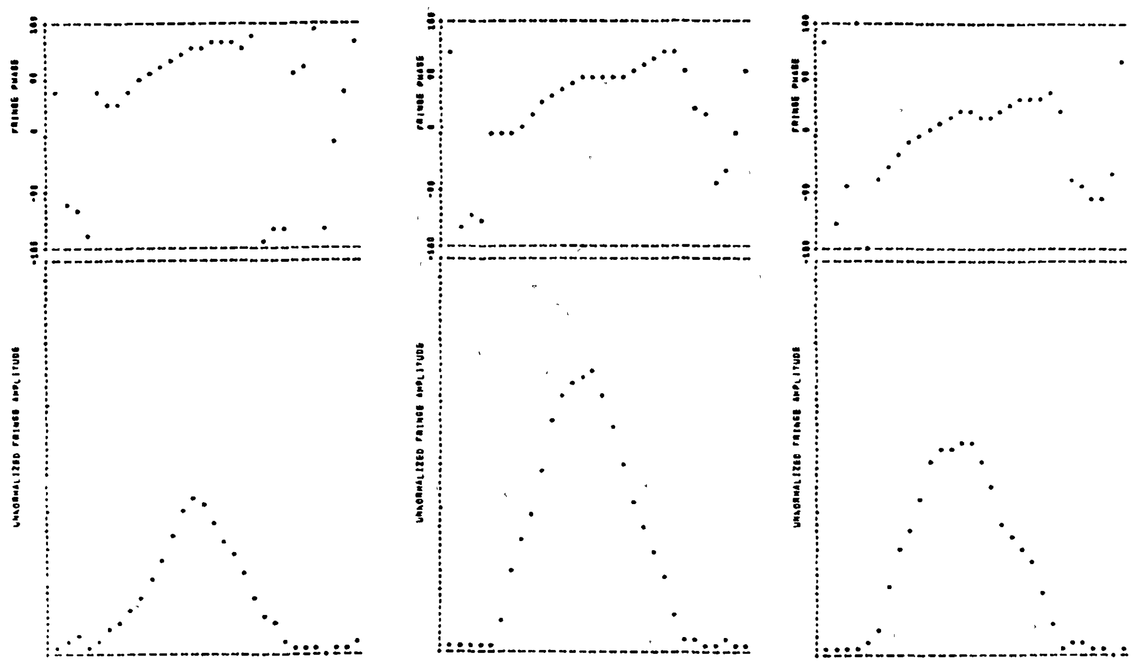

Fig. 1.— The first VLBI measurements of an OH maser, W3(OH), from data recorded on June 8, 1967 at 1665 MHz. The three panels show fringe amplitude and phase versus frequency across a 6-KHz band for different fringe frequency offsets. The resolution was 500 Hz and the integration time was 200 seconds. (Moran 1968)

The first experiment involving more than two stations simultaneously was run in January 1968 with the Haystack, NRAO 140-ft, Hat Creek 85-ft, and Onsala 84-ft telescopes. We succeeded in measuring the angular size of an OH maser for the first time (Figure 3). After that, I disappeared from the scene to write my thesis.

VLBI activity intensified in the following decade at MIT, especially with the connection to the group at Haystack headed by Alan Rogers. Irwin Shapiro came to the MIT campus from Lincoln Laboratory in 1967. He had done his Ph.D. work in nuclear physics at Harvard under Roy Glauber, graduated in 1955, and moved to Lincoln Laboratory, where he worked on orbital dynamics, radar astronomy, and relativity. He was not a member of RLE and operated independently of the group around Barrett and Burke. After 1968, Barrett turned his attention back to single-dish spectroscopy, while Burke vigorously pursued VLBI. Table 2 lists all the radio astronomy Ph.D. students at MIT from the inception of the program in 1963 until 1978, about half of whom were involved in VLBI research.

4. TRAVELS WITH BARRY

I first met Barry Clark in the Spring of 1967, when we were preparing for the first OH VLBI experiment. In the early L-band experiments between NRAO and Haystack and between NRAO and Hat Creek, the time was assigned as a block of days to the NRAO/Cornell group and the MIT group. Barry was the

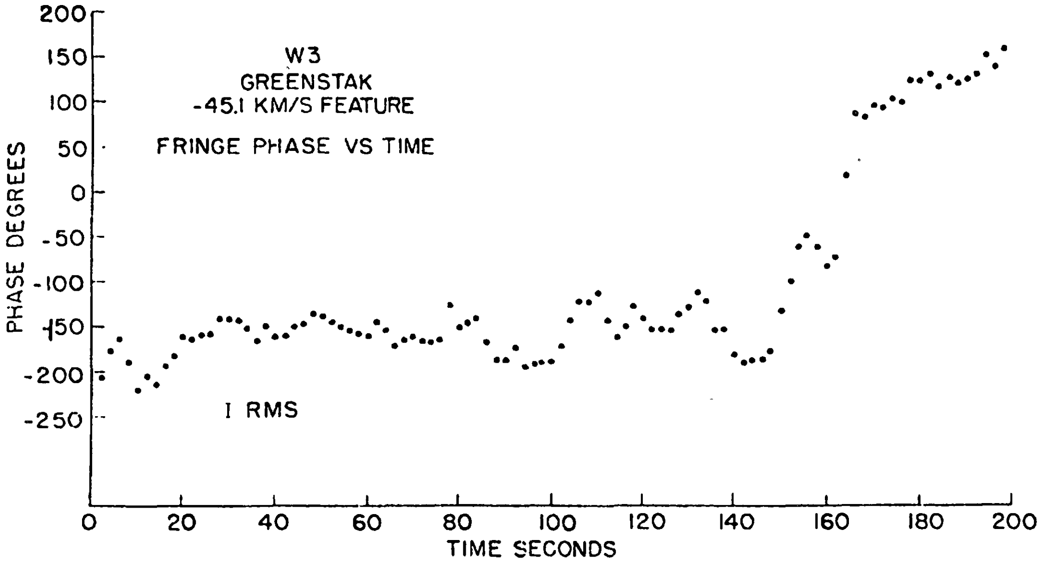

Fig. 2.— Fringe phase versus time on the $-45.1\ \mathrm{km\,s^{-1}}$ feature of the OH maser in W3(OH). Greenstack refers to the Haystack–Green Bank baseline. The phase jump at 160 seconds was probably caused by a jump in the Varian R-20 rubidium frequency standard at Green Bank. (Moran 1968)

scheduler for the former group and I for the latter. This was my first experience with Barry the scheduler. The rough division of time was that the MIT group got the "Galactic time," around 18 hours LST, and the NRAO/Cornell group got the "extragalactic time," around 6 hours LST. Unfortunately, our prime source, W3(OH), had a right ascension of 3 hours and was available for the full range of the 140-ft telescope, and this spilled well into the extragalactic time period. Negotiating with Barry was tough!

Barry and I ran the 140-ft telescope during the second L-band VLBI run in late June 1967. This had been scheduled as a backup to the first experiment in early June, in case there were a catastrophic failure. We were exhausted after the euphoria of the first successful experiment and the strain of trying to process the data quickly. There was a knob on the front of the HP5100 synthesizer that could be set to internal or external reference. I mistakenly set it to internal, which meant the synthesizer was running off its internal crystal instead of the external signal from the frequency standard. No one else noticed. We also had a lot of trouble with the receiver phase lock. I recall riding up to the prime focus in the middle of the night in a rickety freestanding elevator to try to fix the problem, leaning precariously out over the receiver. This was before the service building was installed. So this experiment wasn't much fun and was pretty much a bust. It may have served a purpose in getting Barry thinking about broken-coherence averaging, because he submitted a paper on the subject later that year (see below).

Barry and I joined Marshall Cohen and David Cuddaback at Hat Creek for the first VLBI experiment there in August 1967. I was startled one morning soon after we arrived to see Barry standing naked in the recreation room, leaning on a washing machine. His baggage hadn't made it, and he was washing everything

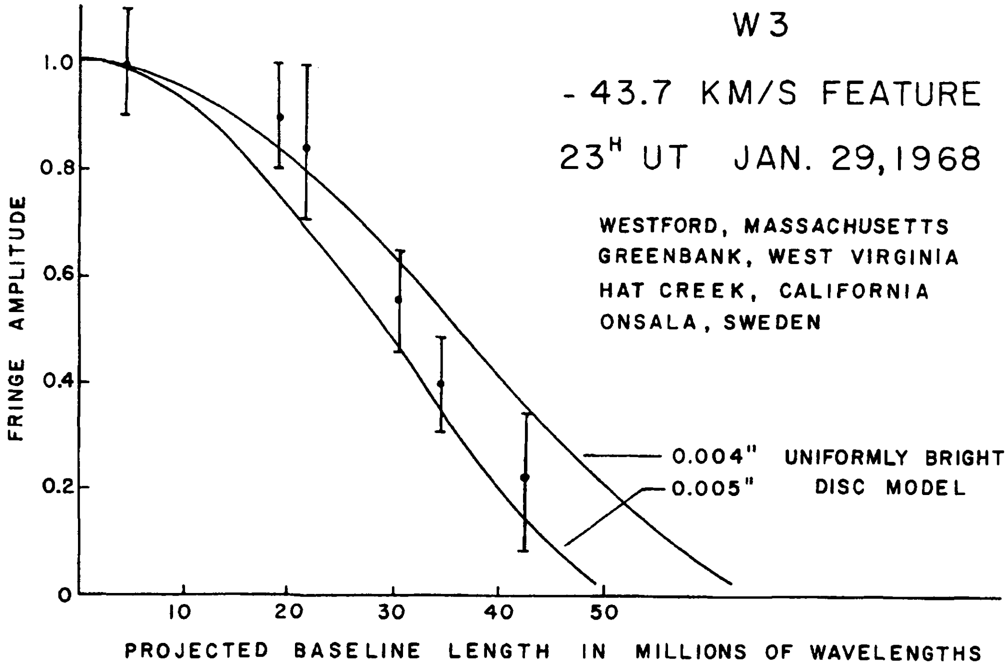

Fig. 3.— A result from the first multibaseline VLBI experiment in January 1968. The normalized fringe amplitude of the -43.7 km s^{-1} feature of the OH maser associated with the HII region W3 shows that it is clearly resolved. Before this measurement, there was doubt that maser features could be resolved. (Moran 1968)

he had with him.

My final observing run with Barry was in the Crimea in 1971. Barry was in charge of getting the brand-new Mark II system hooked up. We were there about a week in advance to get everything set up. Every afternoon, just when time seemed most precious, Barry would put on his bathing suit for a dip in the Black Sea, which was about 100 feet from the telescope. I spent a most frustrating summer at Green Bank with Barry and George Purcell trying to align the tapes mechanically and find fringes on this experiment. We did find them eventually. It wasn't until 1974 that the Mark II system began to work with pretty good reliability (see Cohen, this volume).

5. BARRY'S INVENTIONS

Barry has done many things during his long career, but I want to highlight two of his inventions that especially impressed me: the three-level fringe rotator and the analysis of broken-coherence averaging.

In the first VLBI experiments, the local oscillators were commonly offset for each 3-minute run to compensate for the difference in Doppler velocity at the stations, i.e., the fringe rate. The effect of fringe acceleration was slow enough at L band and did not cause any problems over the short 3-minute tape recordings. (Clark programmed a square-wave fringe rotator into the program on the IBM 360 computer but

TABLE 2: MIT Ph.D. Students in Radio Astronomy (1963–1978)

Student	Year	Advisor[a]	Department[b]	Topic
Sander Weinreb	1963	JBW	EE/RLE	Digital correlator
David Staelin	1965	AHB	EE/RLE	H_2O on Venus
Ron Allen	1967	AHB	P/RLE	AGN/quasars
Alan Rogers	1967	AHB	EE/RLE	OH masers
James Moran	1968	AHB	EE/RLE	OH masers*
Ted Reifenstein	1968	BFB	P/RLE	Recombination lines
Thomas Wilson	1968	BFB	P/RLE	Recombination lines
George Papadopoulos	1970	BFB	EE/RLE	H_2O masers*
William Wilson	1970	AHB	EE/RLE	OH masers
Martin Ewing	1971	BFB	P/RLE	ISM/pulsars
Philip Schwartz	1971	AHB	P/RLE	OH masers
Hans Hinteregger	1972	BFB	P/RLE	Wide-band VLBI*
Philip Myers	1972	AHB	P/RLE	Molecular clouds
Robert Preston	1972	IIS	EPS	Satellite tracking VLBI*
John Spencer	1973	BFB	P/RLE	M33
Kwok-Yung Lo	1974	BFB	P/RLE	Young stellar objects*
Alan Whitney	1974	IIS	EE	Astrometric VLBI*
Robert King	1975	IIS	AA	Differential VLBI*
Curt Knight	1975†	IIS	EPS	AGN/quasars*
Douglas Robertson	1975	IIS	EPS	Astrometric VLBI*
Jill Wittels	1975	IIS	P	AGN/quasars*
Robert Martin	1976	AHB	P/RLE	Dark clouds
Colbert Reisz	1976	IIS	EPS	H_2O masers*
Patrick Crane	1977	BFB	P/RLE	Normal galaxies
Paul Ho	1977	AHB	P/RLE	Molecular clouds
R. Craig Walker	1977	BFB	P/RLE	H_2O masers*
Thomas Giuffrida	1978	BFB	P/RLE	H_2O masers*
Aubrey Haschick	1978	BFB	P/RLE	Extragalactic HI
Michael Shao	1978	DHS	P/RLE	Optical interferometry

[a] JBW: Jerome B. Wiesner; AHB: Alan H. Barrett; BFB: Bernard F. Burke; IIS: Irwin I. Shapiro; DHS: David H. Staelin

[b] EE: Electrical Engineering; P: Physics; EPS: Earth and Planetary Sciences; AA: Aeronautics and Astronautics

*thesis predominantly about VLBI

†thesis formally accepted in 1979

avoided using it routinely because it more than doubled the processing time required.) However, with the introduction of the Mark II system and longer observations times, along with the move to higher frequencies and longer baselines, a hardware implementation of a fringe rotator, essentially a single sideband mixer, became important. Barry figured out a clever way to implement this. The simplest procedure would have been to multiply the one-bit signal by a square wave of appropriate frequency. It was clear that the signal had to be divided into two parts and mixed with quadrature waveforms. Barry took this a step further and

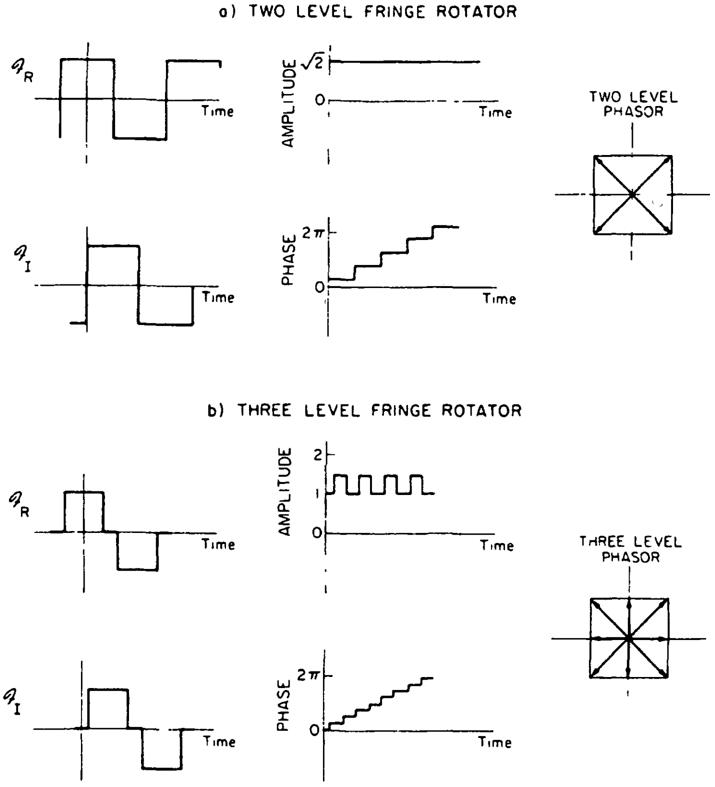

Fig. 4.— Comparison of the performance of the two-level and three-level fringe rotators. F_R and F_I are the fringe rotation functions used to approximate sinusoidal functions. (Thompson, Moran, & Swenson 1986)

devised an eight-lag square wave that had three levels (-1, 0, 1) to approximate a sine wave more accurately. The 0 level (which occurred for one sample every half cycle) was implemented by turning off the correlator during that portion of the waveform. It might seem curious that the SNR could be improved by turning off the correlator. With the complex correlator, however, all the bits were correlated. Barry's waveform had some very nice properties, which can be seen by thinking of it as a complex phasor. Figure 4, from Thompson, Moran, & Swenson (1986), shows that this approximation is a phasor moving around a box in eight steps of 45 degrees each. The SNR was improved by about six percent. Clark recently told me that he thought up the three-level rotator while a graduate student at Caltech. He added that many of his best ideas had come to him during graduate school.

Barry first described the three-level fringe rotator in the infamous user's manual for the Mark II system, entitled *The Mark II VLBI System, Principles and Operating Procedures*, by Clark, Weimer, & Weinreb (1972). Here is the complete description of the three-level fringe rotator:

> In long baseline interferometry, one frequently encounters very high natural fringe rates, ranging up to about 50 KHz. To reduce these natural rates to a more manageable value, one has two choices: the local oscillator phase at one station may be rotated to follow the differential doppler which causes the fringes, or a phase rotator in the IF at playback time can be used to accomplish the same end. In keeping with a philosophy of minimizing the complexity of the recording

> equipment, the second approach was chosen. The concept of a phase rotator for a one-bit digitized data stream may seem strange at first, but it is a precise analogue of the usual case. One builds a phase rotator (or single sideband mixer) by constructing two mixers, fed by signals in phase quadrature, and combining the mixer outputs in phase quadrature. In the digital case, a predicted fringe function is produced in square wave form. There is a phase and quadrature output from the fringe function generator. These signals are used to multiply (in the one bit sense, 0 x 0 = 1 x 1 = 1, 0 x 1 = 1 x 0 = 0) one data stream. The outputs, both the sine and cosine components, are multiplied by the other data stream, accumulated, and regarded as the real and imaginary components of the complex fringe coefficient, that is, they are combined in phase quadrature. For optimum signal to noise ratio, the signal should not be multiplied by a square wave, ±1, which is all the digital equipment can provide. It should be multiplied by a sine wave. It is however, fairly convenient to implement a three-level digital logic. The data are added in phase for 3/8 cycle, and in anti-phase for 3/8 cycle. In between, for 1/8 cycle, the data are ignored entirely and correlation is inhibited. The signal to noise loss from this quantization of the sine wave is only 6 percent instead of the 11 percent one gets if the IF is rectified against a simple square wave.

In case that was too terse, Barry again described the three-level rotator in an article on the Mark II system in a special issue of the PIEEE on radio astronomy (Clark 1973):

> In long-baseline interferometry very high natural fringe rates are often encountered. . . . In the design of the Mark II system, it was felt that complicated functions . . . should, if possible, be done at playback time. . . . The natural fringe rate is removed by phase rotation of the recovered IF at playback time. It may at first seem unnatural to talk about phase rotation of an IF consisting of a stream of 1-bit samples of the signal, but it is accomplished in a rather conventional fashion. The expected fringe function . . . is calculated in special-purpose digital hardware and is mixed with the bit stream in a quadrature mixer phased to produce a single-sideband output. . . . The expected fringe function is approximated by a square wave. . . . Both in-phase and quadrature components are produced. . . . Multiplication is inhibited for a short time near the transitions of the expected fringe function, making the multiplication in the quadrature mixer essentially into a three-level scheme, -1, 0, 1 rather than the two-level scheme natural with binary logic. The resultant is a small saving in SNR.

We all owe Barry a debt of gratitude for increasing the SNR of our fringes by about six percent.

The second invention of Barry's from the early days that impressed me was his clever analysis of interferometers with poor phase stability. He showed that for interferometric measurements of fringe amplitude, one can think of an interferometer of intermediate type between the perfectly coherent type and the incoherent or Hanbury-Brown-Twiss type. For the coherent case, the SNR improves as the square root of time, whereas in the incoherent case, it improves as the fourth root of time. Barry showed that the optimum detection was achieved by coherent integration up to the coherence time, followed by incoherent averaging, and he derived the SNR with the two integration times. In the case where the coherence time equaled the reciprocal of the RF bandwidth, the interferometer performed as a purely incoherent type. He also showed in another paper (Clark et al. 1968) that if you integrated beyond the coherence time of the interferometer, the fringe amplitude could still be estimated by using Parseval's theorem—that is, by integrating the square of the fringe amplitude over the fringe frequency and removing the Rice noise bias.

This paper has led to a rather extensive literature that is particularly relevant to the problem of detecting fringes at millimeter wavelengths (e.g., Kulkarni 1995; Rogers, Doeleman, & Moran 1997).

6. THE FUTURE

It has certainly been enjoyable to get together at this celebration in honor of Barry Clark's 60th birthday and reminisce about the good old days. One of the things we treasure from those days is the hands-on involvement we had in the instrumentation. This, of course, included a good deal of drudgery. We heard about Caltech graduate students working with shovels on various civil projects at OVRO. In the early days of VLBI this "back-breaking" work was perhaps more fun, e.g., hauling live frequency standards to remote locations of the globe. That was before D. B. Cooper, the airplane hijacker whose deed precipitated the scrutiny we all endure for air travel today.

Today there is much emphasis, especially at Harvard, on attacking scientific problems with all the tools available, regardless of wavelength. This can be a commendable and effective approach. However, the subtext of the message to students is: "Use only instruments that work; don't touch anything under development." That attitude has brought us to our current predicament of having few people who can build new instruments or who understand them deeply. I was thinking it would be neat if in 40 years, some of the young people here today would gather to remember the time, back at the turn of the century, when they hauled a hot-electron bolometer up some 6000-m peak in northern Chile to get a glimpse of the sky at 1 THz. Ah, *those* were the good old days.

This paper has benefited greatly from numerous fascinating conversations with Bernie Burke, Barry Clark, George Clark, Marshall Cohen, Ken Kellermann, Dave Shaffer, Irwin Shapiro, Dave Staelin, Harvey Tananbaum, and Sandy Weinreb.

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The VLBA - Planning and Construction

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Abstract

A chronology of the major events in the construction of the VLBA is presented and some of the major challenges in the management of the project are discussed.

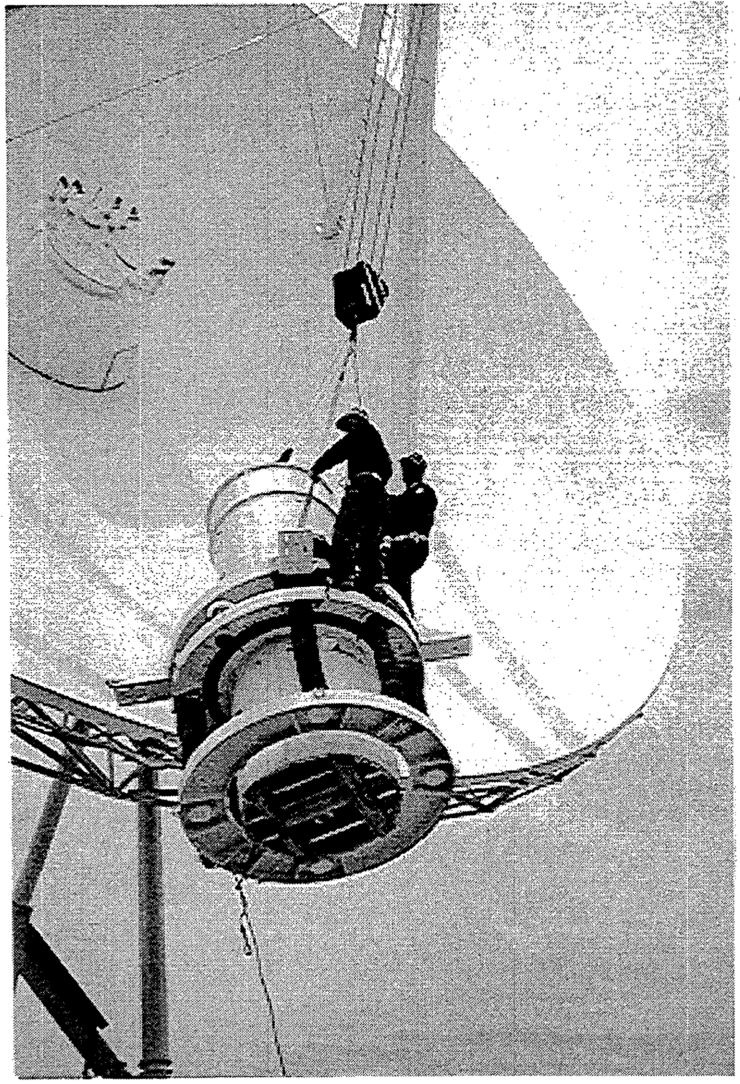
Introduction

The Very Long Baseline Array (VLBA) is an array of ten 25-meter diameter antennas dedicated for observations using the technique of Very Long Baseline Interferometry (VLBI). Technical details of the design of the VLBA have been published elsewhere (Napier et al., 1994; Napier, 1995; Thompson, 1995; Rogers, 1995; Benson, 1995). In this paper we will discuss some of the planning and construction challenges associated with the VLBA Project, as seen from the perspective of the Project Manager, with a goal of passing on some of the experience gained to the large radioastronomical projects of the future.

One of the lessons to be learned from the VLBA experience is the long period of time that it takes to advance a large NSF-funded project from first conception, through the steps of community consensus building, proposal and approval to a completed instrument. The first formal study of a dedicated VLBI array in the US was produced by NRAO in 1977 (Kellermann, 1977). Further studies by Caltech (Cohen, 1980) and NRAO (NRAO, 1981) led to the VLBA being ranked by the Astronomy Survey "Field Committee" (Field, 1982) as the highest priority new ground-based astronomical instrument for the 1980s. A formal proposal for the instrument was submitted to the NSF in 1982 (NRAO, 1982) and funds for design of the instrument were approved in March 1984. Construction funding began in March 1985 and the VLBA was dedicated in August 1993. In the next section, we present a chronology of some of the major events during the construction of the VLBA. Some pictures of construction activities at the VLBA sites also are provided.

VLBA Construction Chronology

1984	Feb	Antenna Request for Proposal (RFP) issued.
	Feb	Socorro chosen as location for Array Operations Center (AOC).
	Mar	$2.8M released as first funding for Design Phase.
	Mar	Caltech selected for correlator work, Haystack Observatory selected for tape-recorder work.
	May	Project reviewed by Taylor Committee for the NSF.
	July	Three antenna proposals received in response to RFP.
	Aug	Longitudinal recorder technology selected for data acquisition.
	Dec	Antenna contract signed with Radiation Systems Inc.
1985	Mar	First construction money approved, $9M (reduced from $15M).
	May	Construction of first antenna approved.



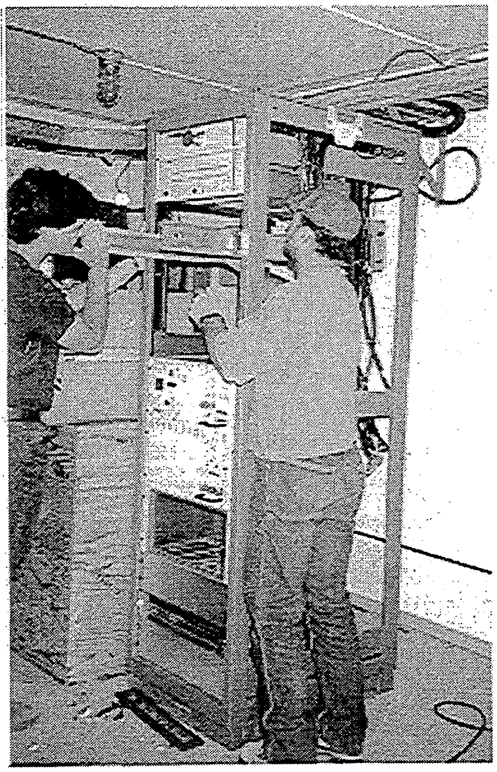


Figure 1, Left: John Wall and Malcolm Peralta ride the focus/ rotation mount to the antenna apex at Los Alamos, June 1989.

Figure 2, Above: Pete Ulbricht and Tom Baldwin install equipment in the antenna pedestal room, Hancock, August 1991.

	July	Decision to delay correlator due to inadequate budget, Caltech withdraws from correlator work.
	Nov	Sigma Tau chosen for design and construction of hydrogen masers.
1986	Feb	Antenna contract renegotiated. First site, Pie Town (PT) started; NM Legislature votes $3M for AOC.
1987	Feb	Decision to use FX architecture for the correlator.
	Aug	Pie Town first light at 300 MHz. AOC construction started.
	Nov	Pie Town used in first VLBI network observing session.
1988	Feb	RFP for correlator custom integrated circuit (IC) issued.
	June	Pie Town operational at all observing bands, first MKIII fringes.
	Aug	First remote unattended operation at Pie Town.
	Dec	Antenna contract renegotiated to 2 antennas/year.
	Dec	AOC occupied.

1989	Feb	Kitt Peak first light.
		Decision to change Array Control computer system to match correlator computer system.
	May	First prototype correlator IC delivered, failed tests.
	Aug	Los Alamos first light.
		Hurricane Hugo hits St. Croix; fortunately only concrete foundation has been built.
	Dec	Fort Davis first light.
1990	Mar	Fourth prototype correlator IC delivered. Finally passes tests.
	May	North Liberty first light.
	May	Decision to use Internet for site communications.
	Oct	Production run of correlator chips delivered.
1991	Feb	Owens Valley first light.
	June	Brewster first light.
	June	Array control system "at last acting like a real, unattended process control system."
	Sept	"Thin tape problem" fix for the tape-recorders designed.
	Nov	Hancock first light.
1992	Mar	Last construction funds released.
	May	Correlator first fringes.
	June	St. Croix first light.
	July	Correlator moved to AOC.
	Dec	11-station experiment processed on correlator.
1993	Jan	Severe ice storm on Mauna Kea damages reflector, requiring panel replacement.
	Feb	Mauna Kea first light.
		All tape recorders completed.
	May	10 antennas observing; no remaining personnel on construction budget.
	Aug	VLBA Dedication.

Construction Project Challenges

As with the construction of any large, complex scientific instrument, a number of areas in the VLBA Project proved particularly difficult. Some of the most technically challenging areas, such as the tape recorders and correlator are discussed in the references given above and in the other VLBA papers in these proceedings. Here we will mention some of the areas in which the challenges were more project management related than technical.

Budget Problems

A number of entries in the chronology above refer to budget problems and indeed, project budget planning was a significant effort throughout the project. The project was initially proposed (NRAO, 1982) at a cost of \$51 M (\$1982) over a period of 4.7 years, and the large contract for the antennas was initiated on this basis. Almost from the beginning of the project, because of the overall Federal budget situation at the time, the NSF was unable to provide funds at the planned rate and a number of painful measures had to be taken to adapt the project to a

Figure 3. Bob Stidstone, left, and Ramon Gutierrez measure the azimuth rail at St. Croix, March 1992.

slower funding rate. These measures included two renegotiations of the major antenna contract, splitting site civil works into two phases, a delay in the correlator which put this important piece of equipment out of phase with the rest of the project and organization of the equipment production runs at less than optimum rates. The project ended up taking ten years and a total expenditure of $84 M ($1989 approx), with almost all of the cost increase resulting from the inflation and construction inefficiencies of the stretched-out project. Future large projects should get a commitment from the NSF on funding rate before settling on an overall project plan and before signing any major multi-year contracts.

Another budget problem concerned the early operations funding for the VLBA. The project was planned on the assumption that an operating budget would be available as soon as the first array elements were completed and available for use. Again, because of the very tight Federal budget at the time, the NSF was unable to supply the amount of operating funds required and several of the VLBA sites had to be "mothballed" as soon as their construction and outfitting was completed until operating funds were available one or two years later. This was most undesirable from the point of view of the overall morale of the project and increased the time that it took to completely debug the instrument. It is important that in future projects the operations budget starts to build up early, when first commissioning starts. This is particularly appropriate for array projects where useful science can be accomplished as soon as the first interferometer is available.

Site Problems

One of the surprises of the VLBA construction project was the number of problems encountered in acquiring and constructing the ten different sites. Early in the project it was expected, perhaps naively, that since the ten sites were identical, after the first site was done the others would be easy. This was not the case and most of the sites seemed to have their own peculiar set of political, legal or contractual problems to be overcome. For example, a bankrupt civil-works contractor at Fort Davis and a contract award protest at Owens Valley delayed work at those sites. The DOE had a very long approval process to provide a site for the Los Alamos antenna on Los Alamos National Laboratory property while the location of the Hancock antenna in a New Hampshire Wetlands area required an extended process of environmental approvals. Mauna Kea required a slow, intricate series of state and environmental approvals but the most frustrating site acquisition process undoubtedly was St. Croix. Here problems of environmental permitting and legal opposition, compounded by delays caused by Hurricane Hugo during site construction, resulted in the final site-use approvals being received in January, 1998, more than five years after completion of construction!

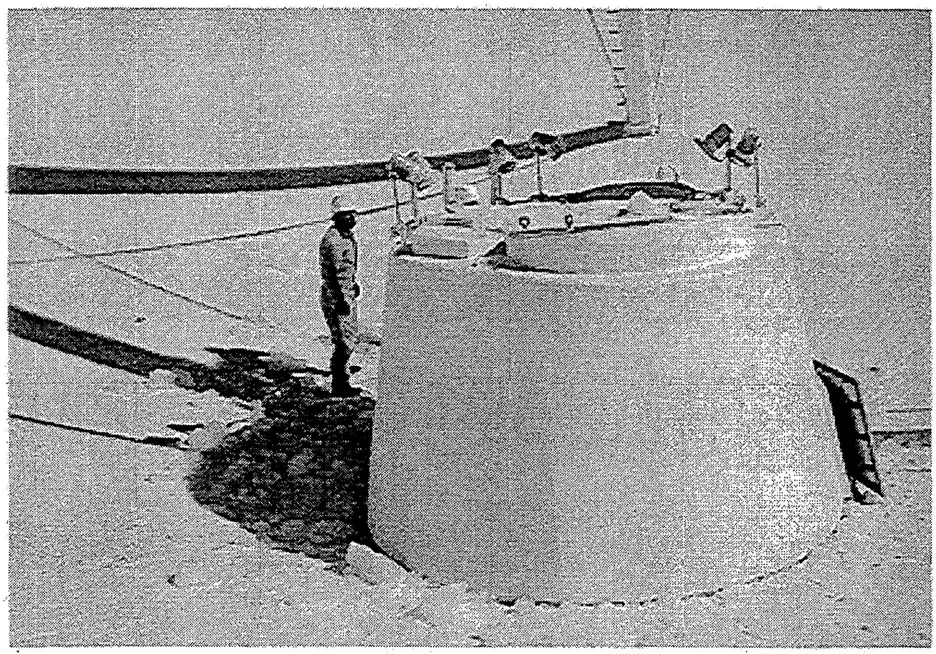


Figure 4. John Wall inspects damage to the reflector surface at Mauna Kea after the severe ice storm of January 1993.

In order to save on the number of project employees, these types of local site problems were handled by sending staff from the Project Office to the appropriate site on travel status on an as-needed basis. Similarly, the oversight of the civil works and antenna assembly contractors were handled by staff on travel status rather than by having a permanent overseer at each site. In hindsight, this probably was false economy and the cost of a permanent employee at each site would have been offset by reduced delays and improved contractor work quality.

One last point, to do with the location of the ten VLBA sites, is worth mentioning because, again with the benefit of hindsight, an aspect of the operation of the sites has turned out differently from what was expected. When the detailed locations of the VLBA sites were being chosen it was thought important to locate the sites close to another observatory or technical institute that could provide technical support to the minimally-manned VLBA sites in the future. This has not turned out to be necessary or effective, primarily because it is not feasible to provide adequate training in VLBA equipment to the staff of the nearby observatory. Any technical problems that cannot be handled by the two-person VLBA site crew are solved by sending specialty teams from the AOC to the site. It is worth noting that locating two of the VLBA telescopes close to the millimeter wavelength telescopes at Kitt Peak and Owens Valley has proven to be worthwhile because, at these sites, the VLBA data acquisition and timing equipment is used for millimeter wavelength VLBI experiments.

System Integration

The large geographical separations between the AOC and the ten VLBA sites made it essential for the Project to have a full-scale system integration test bed where system problems could be discovered and solved before equipment was sent to the remote, minimally-manned sites. A complete set of VLBA equipment was dedicated for this purpose in the AOC, but it was still essential to use Pie Town, the closest VLBA site to the AOC, as the final test bed for all new software and equipment designs. Although, as much as possible, the Project designers followed Barry Clark's admonition that "it is better to make the hardware do the right thing rather than to solve hardware limitations with complicated software," nevertheless the VLBA computer control system was very complex and many weeks of system debugging time were required at Pie Town during the early stages of commissioning. The 90-minute one-way drive to Pie Town was at the limit of feasibility of access time for the test site and with hindsight, locating an antenna closer to the AOC would have been much more efficient.

A significant problem of system integration arose because design and construction of the correlator had to be delayed in response to early budget shortages in the project. By the time construction of the correlator could be commenced a new generation of real-time computers and operating system were available, compared to the older system used for the array control system, and the difficult decision was made to completely rewrite the array control system software so as to keep it compatible with the correlator control system. An additional problem that resulted, at least in part from the correlator being out of phase with the rest of the project, was that a thorough end-to-end data flow analysis for the complete VLBA was difficult to perform and this has increased the complexity of VLBA operations.

Conclusions

The approval and construction of large astronomical instruments such as the VLBA take very long periods of time. The VLBA took 16 years from its first formal study until its dedication at the completion of construction. One of the management challenges for the VLBA Construction Project was the need to reorganize the project in response to a slower than planned construction funding rate and a slower than planned build-up of early operations funding. The extent to which the project was spread out geographically increased the difficulty of overseeing the site acquisition and contractor activities and complicated system integration work.

Acknowledgments

Credit for the success of the VLBA Construction Project belongs to the team of dedicated people who designed and built the instrument. In 1988, the year of peak manpower loading, 61 full-time equivalent employees were on the construction payroll. The basic, very successful structure and organization of the Project was established by Hein Hvatum who was the Project Manager from the inception of the Project until January 1987.

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EARLY VLBA OPERATIONS

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ABSTRACT

The early operation of the VLBA is reviewed. The period covered is approximately from first light at Pie Town in September 1987 to mid-1995 when scientific observations reached about 50% of full time. This was the period when the antennas were being brought on-line and the operational style was being settled. It also was the period of the transition for scientific VLBI observations from the Network to the VLBA.

1. Introduction

I would like to begin with a story concerning Barry Clark. Barry was in charge of developing the on-line control system used on the VLBA. One of the management charts distributed during the project had a classic blunder (I hear rumors it may have been intentional on the part of those who drew the chart) which showed what often seems to be Barry's true position in the observatory. This chart had the usual management tree starting with the director and working down to individual scientists, programmers, technicians and so forth, with lines showing the chains of command. Barry had a box and below him, shown normally, were the members of his group. But there was no line above him.

VLBA operations began with the completion of the Pie Town antenna in 1987. Within 2 months of first light, Pie Town participated in scientific observations with the VLBI Network (the loose organization of separate radio astronomy observatories under which most astronomical VLBI observations were done prior to the VLBA's completion). The first Pie Town observations established a pattern in which VLBA antennas were quickly added to the ongoing world VLBI effort very soon after the completion of construction. As the number of antennas grew, and as operations ceased at the older stations, the VLBA became an ever more important part of the Network sessions. Eventually only NRAO antennas remained involved in astronomical VLBI in the United States at cm wavelengths. The VLBA also participated in many geodetic observations — more than at present because the geodetic groups could provide their own correlator resources while other types of observing were correlator limited. Once 3 VLBA antennas were available, scientific observing using only VLBA antennas began. Such observations slowly grew from a small fraction of the total to the dominant use of the VLBA.

During the early operational period, much effort was consumed in verifying the performance of the antennas, fixing problems, obtaining good pointing values and good gain measurements and performing other tests. At first, there were many problems, especially with the recording systems, and the VLBA antennas were somewhat shaky participants in Network and geodesy observations. But as time went by, the bugs were shaken out and the VLBA sites became among the most reliable and best performing available.

The VLBA could not really come into its own until the correlator was in reasonably routine operation. Thanks to the delays described in the previous chapter, this did not happen until around 1994, after the last antennas were finished. This was one consequence of the decision to deal with reduced funding rates by slowing items such as the correlator in order to preserve the antenna contract as much as possible. The result was that there was a period in which the VLBA was limited by correlator capacity. Observations could be processed on the Mark III correlators, but capacity was very limited. Many observations were done using the old Mark II recording system which was not originally intended for use on the VLBA at all.

It was interesting to watch the evolution of attitudes about the VLBA, especially in Europe. Early, before operations began, there were frequent snide comments about the lack of sensitivity of the 25 m antennas. Those quickly subsided when the sensitivities, with good receivers and high gain optics, matched many of the larger antennas elsewhere. Then later, as operations became smoother and the quality of the VLBA data became apparent, the tone changed completely. Now the rest of the world is trying to catch up — a task that will be very difficult for networks of existing and dissimilar facilities operated by many different organizations.

Some key dates related to early VLBA operations are given in Table 1. An approximate date for first operations at each antenna is given in the first part of the table. Then other key dates related to the rest of the project are shown. Many of these dates also can be found in the contributions by Napier and Romney (this volume), but are included here to help set the context for the rest of the discussion.

2. Pie Town

The first VLBA antenna was the one at Pie Town, New Mexico. Pie Town is about 90 miles from Socorro, which is close enough that on-site testing could be done without special travel arrangements. Bringing up a completely new system is much different than bringing up a new instrument such as a new receiver or recorder. When bringing up a new instrument on an established system, many tools are available to facilitate testing. But when Pie Town first became available, it seemed that whenever one wanted to test one component of the system, it was necessary to first get other components working or to find work-arounds. The on-line system had limited capabilities. There were only limited test instruments at the site. Much of the early data was collected on strip charts. There was very limited ability to do interferometric tests — the Mark II system had to be used and someone in Charlottesville, usually John Benson, had to be convinced to process the data. To complicate matters, the Mark II system was highly unreliable at first. Software for analyzing test observations such as pointing tests was not available and had

Table 1: Early VLBA Operations Dates

Sites (first light or "operational")

PT:	Sept.	1987	OV:	Feb.	1991
KP:	Feb.	1989	BR:	Aug.	1991
LA:	Aug.	1989	HN:	Feb.	1992
FD:	Dec.	1990	SC:	Late	1992
NL:	May	1990	MK:	Feb.	1993

Other dates

Move into AOC:	Dec.	1988
First call for 'VLBA only' proposals:	April	1989
First fringes on VLBA correlator:	6 May	1992
VLBA Summer School:	23-30 June	1993
VLBA Opening:	20 Aug.	1993
First science (H_2O masers and 3C 84):	Oct.	1993
Roughly 50% operation:	Mid	1995

to be written by the testers. And, for the first several months, observations were confined to the 327/610 MHz system, with a temporary dipole feed, because the subreflector was not yet available.

First light was obtained during software test time by Barry Clark and Jim Oty in September 1987. First Mark II fringes to a VLA antenna were obtained on 23 Oct 1987. This was followed in November by the first participation by a VLBA antenna in a VLBI Network run. This established a pattern followed with many of the antennas of participating in astronomical observing very soon after first light. During this period, much on-site testing was done by Barry, Durga Bagri, and myself with engineering support from Wayne Koski and Herb Winchell and operational support from the first VLBA site tech, Eric Carlowe, and the first VLBA operator, Susan Koski.

The VLBA antennas were designed to operate unattended much of the time. Various safety features are included so that the antenna stows automatically when unsafe conditions, such as loss of power (there is an emergency generator to provide power to stow and keep receivers cold) or high winds, are encountered. Schedules can be loaded well in advance of observing and the monitor data can be collected later, although now it is normally downloaded to the AOC almost immediately. Unattended operation was begun during the latter half of 1988. Susan, or others including myself, would operate from the NRAO buildings in Socorro or even with just occasional monitoring from home PCs. It is likely that there was more unmonitored observing during the first year or two than at any time since.

The subreflector was installed in April 1988. The cooled receivers were activated quickly afterwards and good pointing equations were obtained. The first Mark III fringes followed on 1 June 1988 and a full Network session was supported that month. The dual frequency 4/13 cm dichroic system was installed and the first geodesy run was made in Sept. 1988. Tests before and after the dichroic plate was installed showed that there is considerable excess spillover at intermediate elevations with the plate. To this day, this effect is not understood, although no concerted effort has been made to determine the cause. The highest frequencies of the VLBA came significantly later. The first 43 GHz observations were done on 4 April 1991 and the first cooled 86 GHz receiver was installed in Dec. 1997. There was an 86 GHz, room temperature receiver available in the early days at Pie Town. It was used to determine the efficiency and showed that there was hope that performance at that frequency would be sufficiently high to be useful.

Very early, the first effort to establish a system to do holography on the Pie Town antenna was begun. One embarrassment of the project is that this effort still has not produced good, high resolution results. The holography project has been handed from one person to another and the basic method being tried has changed several times. Something will have to be done about this soon in order to make effective use of the new 86 GHz systems. The latest thinking is that the surface measurements should be made, not with holography, but with a photogrammetry system. Such a system can be purchased ready to use, but is sufficiently expensive that it may have to wait for special funding.

Pointing results from the first few years showed an interesting effect at Pie Town — the antenna is falling over! Well, I exaggerate a bit, but it is tipping at the rate of a couple of arc minutes per decade. The rate is not steady, with a maximum rate of about an 0.5 arcminute per year seen in 1991. The antenna must be settling on less than totally solid soil. It is tipping toward the hill on which it is built. This somewhat counterintuitive direction is, however, normal, according to soils experts who have been consulted.

3. Other Stations

The remaining VLBA stations came on-line at a rate of about 2 per year. Waking up a station was usually fairly straightforward. Once the manufacturer finished with the antenna, all the NRAO hardware was shipped and a crew was sent to install it. Jim Oty, who was on the installation crew, was generally amazed at how they could install equipment built at a variety of sites, connect everything together, turn on the power, and it would usually work. There generally was no protracted period of working out bugs. Similarily, once the antenna was turned over for astronomical testing, it was usually a quick process to find the pointing equation, get offsets for all receivers, and be ready for scientific observing.

The sequence of tests used to start up a site began with a manual effort to find a strong source at a low frequency such as 20 cm. That gave a rough estimate of the azimuth encoder offset — the only term in the pointing equation that was consistently greater than a small number of arc minutes. Then a short series of pointing observations at the low frequency would give a rough pointing equation. Using that, the collimation offsets and focus for one of the high frequencies would be found manually. Then a much longer pointing run at that frequency would provide a good pointing equation. Next, rough collimation offsets for each receiver, and a good focus value, were found. Finally an all-bands pointing run could be used to fine tune the collimation offsets. At this point, the antenna was ready for observations. There was no need for a protracted series of tests of each receiver.

Soon after the pointing was established, an effort would be made to record data and find first fringes, verifying in the process that the tape systems were working and that the polarizations were correct. My memory is that about 10% of the receivers were cross polarized. In fact, when the 7mm system was installed, all of the receivers were cross polarized so we had a few experiments with fringes between VLBA stations and fringes between other stations, but none between VLBA and non-VLBA stations. After the fringe checks the station was ready for scientific observing which might be the next Network run for the early stations or the next regular VLBA run later on. Some of the fringe checks were done with the fringe check system which sends about a Mbit of captured data back to the AOC from each station for software correlation.

As more stations came on-line, control from the AOC became more complicated. The tools provided were oriented toward having a separate window for each station, in which various status displays could be run. This worked fine with a small number of stations, but got awkward as the number increased. When it became clear that the monitor and control group was not going to deal with this, Bob Greschke, who succeeded Susan Koski as chief operator, wrote the program (*vldis*) that is still used to display a summary of all stations based on the monitor data returned to the AOC. He also wrote a number of other programs for operations, including one that keeps track of tapes.

Most of the hardware at the stations worked well from the start. There were minor problems that required tweaking over the first few years. For example, the BBC bandpass shape needed optimization and some lock loss problems had to be cleared up. The main source of problems, as always with VLBI, was the tape systems. It was a while before tape performance was reasonably reliable. This is still a weak link. Tape problems are much less common now than in the early days, and also much less common with VLBA stations than with many others. This demonstrates the advantages of having one group take care of a large number of tape drives — nearly 50 in the VLBA case (VLBA sites, correlator, VLA, Green Bank, test systems ...). But despite the improvements, tape problems are probably the major source of lost or degraded data. As for lessons learned, I would guess that the single thing that could be done to most improve the performance of any of the VLBI Networks would be to centralize the tape system maintenance under one group that does nothing else and that works with a large number of recorders.

One other area of early frustration was trying to keep track of time at the sites. The station one second ticks were lined up with UTC quite well, but the mechanisms for keeping track of the seconds was rather shaky.

4. Mark II

When the VLBA antennas were first operational, there was no VLBA correlator available. All interferometric testing and science had to be done with the older Mark II system and with Mark III format tapes recorded on the VLBA drives. The Mark III system has a significant fraction of the performance of the VLBA system so some observations could be done with more or less the final sensitivity. But Mark III correlation resources were limited. Much greater correlation capability, in terms of the ability to process observations with many stations quickly, was available for Mark II observations using the Block II correlator at Caltech. Also, NRAO had a Mark II processing capabilityin Charlottesville, for very limited experiments and for spectral-line observations. For these reasons, seven of the VLBA stations were outfitted with Mark II formatters and tape recorders. New formatters were built that were interfaced to the on-line system for remote control. Each station had 5 tape recorders in order to be able to get through the night without a site tech visit. Only seven stations were so outfitted, partly to save cost, but also to avoid having Mark II a long term part of the VLBA.

One clear lesson of the early operations, and also from early VLA operations, is that a correlator of some sort is needed early for testing purposes. This is not for testing of the correlator, but testing of other aspects of the array. It doesn't need to be a fully capable correlator, by any means. For the VLBA, testing, at first, involved recording tapes and sending them either to Charlottesville or Haystack for processing. But capacity was limited and, for quick turnaround, someone at the correlator, who generally would rather be doing something else, had to be convinced to drop everything and run the test. This proved sufficiently unsatisfactory that the Charlottesville correlator was moved to the AOC in late 1989. There it was used for testing, and for some spectral-line astronomy projects. It was operated carefully until Sept. 1993, when it suffered a serious hardware malfunction. It was already scheduled to be shut down three months later when the Convex, on which all its support programs ran, was scheduled to be removed. It was decided not to attempt to fix the hardware problem after it resisted an initial effort. That correlator, which was the original Mark II correlator and which was responsible for much of the early history of VLBI (including my thesis), now sits in storage at the VLA.

The final end of the Mark II era, at least for global VLBI, came on 15 Sept. 1995 when the Caltech Block II correlator ended Mark II operations. It continues to be used now in its original design capacity as a Mark III correlator.

5. Geodesy

During the first few years of VLBA operations, a lot of time was spent participating in geodesy observations. Partly this was because the geodesy groups have their own correlator resources and the VLBA observing was very much correlator limited. Extra observing for geodesy did not impact other types of observing. Later, when the VLBA correlator was available and observing time for astronomy increased, the amount of geodesy observing was reduced.

Geodesy observations were especially useful during the early days because they were usually processed quickly and any problems encountered were reported quickly. This is in stark contrast to normal astronomy observing in which we rarely had any feedback after the data left the VLBA, either as wideband tapes or as correlator output. We did need a bit of a thick skin during this period. Many of the normal geodesy stations were running routinely for up to half of real time, which is still not much less than the activity at a VLBA station. Their operations were well shaken out and quite reliable. Meanwhile there were these new VLBA stations that kept having problems. I even saw Pie Town referred to as "Cow Pie Town" by some of the geodetic site techs. We just had to have faith that the VLBA would eventually prove itself, which it did. Meanwhile, the geodetic groups maintained interest in the VLBA because the data quality, despite some operational problems and tape playback problems, was very good — some of the best that they had seen.

There was something of a culture conflict between NRAO and the geodesy community. NRAO is used to having experiments proposed, observed, analyzed, and written up by individual PIs. The geodesy community has operational groups who take the observations and analyze them. Then either reduced delays ready for detailed fits, or, perhaps more often, final products such as station positions and velocities, source positions, Earth orientation and so forth are given to the geophysical community and other end users. The link between those taking the data and those doing the science is not so tight. NRAO insisted on proposals for time while the operational geodetic groups are used to planning some overall strategy for use of their antennas and then running the experiments. They only really had to worry about the scientific output when dealing with funding. It took a while for them to get the knack of writing proposals that VLBA referees and managers liked. There was the additional problem that most VLBA referees and managers are astronomers who are not very familiar with the kinds of science that were being done in the geodesy programs. As a result, ratings weren't necessarily high, especially considering the amount of time being requested. I often found myself caught in the middle.

One area where the VLBA has benefited greatly from the interaction with the geodetic groups is in the correlator model. The program used to calculate the model on the correlator is CALC, the standard geometry calculation program from the Goddard group. The source positions and station locations used on the correlator are from solutions done at the USNO. And the Earth orientation parameters used for correlation are from the geodesy community.

The impact of the good correlator model has been dramatic and is not yet complete. Many users, on seeing their raw VLBA data, with very slow phase variations, have realized that phase referencing should be possible without the heroic efforts at resolving ambiguities that were required in the past. At a bare minimum, delay and rate referencing should make fringe fitting unnecessary on weak sources in most cases. With full phase referencing, the sensitivity of the VLBA is set not by the limits imposed by the requirment to detect the source in a fringe fit on the baselines to each antenna within a coherence time, but rather by the full sensitivity of the whole array integrating for the whole time of the observation. This improves the sensitivity by one to two orders of magnitude. Phase referencing already is used at most VLBA freqencies and projects are under way to make it work even better by addressing the main known model errors.

The use of phase referencing is still increasing as users realize what is possible. It already seems large with phase referenced images being reported regularily. To try to quantify that impression, I checked the VLBA schedules, excluding global and VSOP schedules, for June 1998. Nearly 40% were scheduled appropriately for phase referencing. A nice example of a phase referenced result was the detection of one of the gamma ray burst sources with a flux density of about 450 μJy using the VLBA, phased VLA, and Effelsberg by Taylor et al. (1997).

6. Correlator

The late availability of the VLBA correlator, due to funding constraints and chip difficulties, made testing and early observing difficult after more than just a few antennas were available. We had to rely on the local Mark II correlator, which was very small, or on the Caltech Mark II or Haystack Mark III correlators. Those institutions were helpful, but had other priorities. We were so anxious to get going that, when the correlator did just start to work, observations that would require its capabilities were begun before the modes used were checked out. It was very hard to resist starting to observe seriously since nearly all of the antennas were available. This led to the accumulation of a large backlog that was not flushed until about 3 years after the first science observations were correlated. The backlog consumed a lot of tapes and effort, especially for observations done in modes that, in hindsight, should not have been used.

The correlator also suffered from some problems similar to the array control. Operator interface tools were minimal. Also tools to examine the correlator output carefully were not available. Loading every dataset into AIPS to check it out was an onerous burden that was only relieved when a couple of astronomers wrote the "sniffer."

But that said, the data produced by the correlator was of high quality and pushed VLBI science forward quickly. This began with the first science continuum image of 3C 84. That source was chosen simply because it was strong and complicated by VLBI standards. It was hoped that an impressive image could be made. What wasn't expected was that new science would be found. With the high dynamic range available, a counterjet was found at that had not been seen before (Walker, Romney, and Benson, 1994). By coincidence, the Berkeley group found the counterjet at about the same time at a different frequency (Vermeulen, Readhead, and Backer, 1994). When the images were compared quantitatively, it became apparent that there was free-free absorption of the counterjet, but not the main jet. This absorption is most likely occuring in the accretion disk, or at least in an atmosphere around the accretion disk. Followup multifrequency observations, that would have been nearly impossible on the Networks, are being used to characterize the absorption and provide strong constraints on the ionized material in the accretion region (Walker et al. 1999).

It is a bit amusing that the image of 3C 84 used in the first science announcement did not show the counterjet. There was a hurry to get out that announcement and I had not yet pushed the image to the dynamic range required to be sure of the reality of the counterjet. Not wanting to get people excited about something that I did not yet trust, I simply cropped the image to exclude the area of the counterjet.

The design of the VLBA was done at about the time that efforts were being made by myself and others to push dynamic ranges (ratio of peak to off-source rms) of VLA images beyond 10^5. We came to appreciate value of keeping closure errors below about 0.1%. The VLA was designed to maintain closure at about the 1% level and this was causing problems and requiring some difficult calibrations. Throughout the design of the VLBA, we set the goal (the engineers would not accept this as a spec) of not introducing closure errors of more than about 0.1%. Once the correlator became available for testing, we were able to confirm that this effort had worked. Dynamic ranges in excess of 10^5, without empirical closure corrections, were reached in test observations. With the uv coverage of the VLBA, and the fact that the bright sources are partially resolved, reaching such high dynamic ranges required overcoming some problems with all of the deconvolution algorithms in use at the time. Dan Briggs' NNLS algorithm did this nicely and allowed the very high, noise limited, dynamic ranges that were achieved. It was with great sadness that I learned, on the day that this text was written, of Dan's death in a skydiving accident. He will be missed.

7. Transition from the VLBI Network

The history of the U.S. VLBI Network has been covered in earlier contributions to this symposium. The Network was replaced by the VLBA and several of the telescopes of the Network are now gone. The transition from the Network to the VLBA was made over several years. As new VLBA antennas were built, they quickly began to participate in Network observations using both Mark II and Mark III recordings. During this period, funding for VLBI operations at the Network stations ceased, sometimes sooner than would have been optimal given the slow VLBA funding rate. The antennas at Fort Davis and North Liberty were actually torn down and, a while later, the one at Hat Creek was blown down in a storm. The only Network station in the U.S. that still participates in cm VLBI (as opposed to mm VLBI) is the VLA. The change was definitely for the better and the data taking capabilities got steadily better throughout the transition.

Perhaps the rockiest part of the transition related to correlation of wide band observations.

The unpredicted delays in getting working chips for the correlator upset the planning for this transition. The main correlator involved is at Haystack. To plan their operational and staffing levels in a rational manner, and especially to arrange funding with NSF, they needed to know at least a year or two ahead of time when the processing load would shift to the VLBA correlator. Such predictions were made, but then not met. This created a situation in which Haystack had to extend their support for astronomical processing. Indeed, NRAO funded Mark III processing at Haystack for part of 1993.

Lessons learned from the transition probably will not really apply to other instruments. VLBI, especially Network VLBI, is in a unique position where the "instrument" consists of many independent parts. There was a working instrument before the VLBA was built and that instrument slowly evolved into the VLBA over the transition period. There was an attempt, not completely successful, to not actually reduce the amount of VLBI observing going on during the transition. The VLBA supported the Network sessions at their original, and still existing, level, while increasing the overall VLBI observing by introducing as much VLBA-only observing as possible. Most instruments will be self contained and any capabilities that they provide will be new.

The transition to the VLBA did have some unfortunate consequences for U.S. VLBI science. There was a period during which attempting to do VLBI in the U.S., especially for students, looked very unattractive because of limited correlation resources and the promise that everything would be so much better in a few years. As a result, very few students got into the field during that time. This effect was not seen in Europe where the EVN continued to operate as before. I suspect that the high proportion of foreign users that we now have on the VLBA is partly a result of this slack period.

Another consequence of the transition was the loss of instrument support money at the Network facilities. Most of the groups involved were small and made the transition to funding mainly for astronomy. But the largest and most active VLBI group in the country, the one at Caltech, lost major funding when both the Owens Valley 130' antenna and the Block II correlator ceased being supported for astronomical VLBI. Several of the principal members of the group chose to move on to other things and the VLBI group was decimated. This was expected to be one of the main VLBA user groups, but now it is almost gone. I imagine that this experience weighs heavily on the groups currently supporting mm interferometers as the age of the MMA approaches.

8. Software Lessons.

Software development is an ever more important aspect of building an astronomical instrument. With the MMA just starting and a VLA upgrade being proposed, it is useful to consider any lessons learned from the VLBA project. Management of software development seems to be a very difficult task. Even in industry, I gather that it is not a well understood art. Predictions of how long tasks will take seem to be very difficult and the resulting products can end up with either much less or much more capability than expected. Here I will give some of my impressions of lessons learned. I suspect that not all involved would agree with all of them.

One lesson would be to beware of the old attitude that "it's only software" when some part of the hardware design is going to introduce complexities in control. Such situations can cause delays in completion of the software and the extra complexities can make maintenance difficult. Also, the full implications of the limitations may not be fully understood, leading to operational problems. In the end, the cost of the software and possible lost experiments may well exceed the hardware savings. I suspect that one example is the switch between the recorder tracks and the playback interfaces in the correlator. In order to save some cost and complexity in hardware, this is not a fully general crossbar switch. While it does not prevent any given type of observation from being made, it does make it possible to record experiments in ways that cannot be processed — and we have done just that. Some of the restrictions imposed by this switch were only appreciated recently. Because of the complexity of this switch and the restrictions that it imposes, both Barry (for the on-line system) and I (for scheduling) have its block diagram posted on our bulletin boards. Generally, I would encourage the engineers designing a system to consider logical simplicity of the control software to be one of the design goals.

Another lesson is to beware of complexity. This is related to, but not quite the same as the previous point. An area of complexity is all the modes that correlators are capable of using. Both the VLA and VLBA correlators are capable, in hardware, of many more modes than have yet been implemented in software. And the effort to keep all the modes straight has considerably slowed correlator control software development. A design goal could be to identify some small set of modes which cover all, or at least the vast majority, of the potential correlator uses, and that are comprehensible and documentable. One simplification of this sort taken in the VLBA correlator relative to the VLA correlator was to consider all data to be spectral line data. You just average to fewer channels for continuum data rather than have a whole different way that the correlator works when doing continuum observations. But there are enough other modes to cause problems.

The VLBA correlator was built in Charlottesville and then, right after first fringes, was moved to Socorro. In hindsight, this was probably done about a year too early, as far as correlator development was concerned. The timing of the move had to be set well in advance, and predictions of when it would be ready turned out to be optimistic. Also, the Socorro group involved in testing, with nearly all antennas up and running, desperately needed a big correlator. The effect of the move was to break up the team that was building the correlator before the job was done. In hindsight, this was probably a rather bad idea. The MMA is starting down the same path so management needs to be careful.

There were two flavors of software tools that were not supported adequately in the VLBA development as the monitor and control group kept focused on the final core system. The first was related to support of test observations, where one would like to have had easier real time control and better tools for examining monitor results. The second was operational tools, such as the *vldis* and tape tracker programs, mentioned earlier, that wound up being developed by an operator, not a programmer.

There often seems to be a significant lag between when the hardware is finished and when the software is ready to support it well. I suspect that we should try to get software projects going earlier in the development cycle and use that time to develop those parts of the code that don't have to interface directly to the hardware. It would be very useful to have user interfaces, operations tools, data examination tools, etc available as the use of the hardware begins. Also, perhaps, problems encountered in early software design could feed into the hardware design for a better overall system. This was done to some extent on the correlator and other parts of the project, but it could have been better.

Finally, in my opinion, the AIPS++ project was started too soon, perhaps by about 2 years. We are still paying the price of having a postprocessing system that was really not ready for VLBI when a significant fraction of the programmers got moved off the project. We are still missing basic capabilities such as the ability to support VLBI style subarrays and the ability to support, in something other than an extremely awkward style, geodesy style fringe fitting. The VLBA has greatly increased the quality of data taken for VLBI observations. But, partly because of the poor support for AIPS during the critical period, processing VLBI data is not yet easy enough to overcome the reputation that doing VLBI observations is difficult. If the VLBA is to have a broad user base, this situation must change and most of the required changes are in postprocessing.

9. Summary

In assessing the impact of the VLBA, one should not forget that it did not suddenly open a lot of new "phase space." Many of the observations that it does could have been done before in limited quantity and with difficulty. The "cream skimming" discoveries, such as superluminal motion, had already been made. The VLBA was built to allow the extended and detailed studies required to really understand compact objects — studies that would have been difficult to impossible with the old Network. It also should be remembered that sources that are strong and compact enough to be seen with VLBI must have brightness temperatures greater than about 10^5 K. Thus most thermal objects cannot be observed, except in absorption. For this reason, the VLBA will fundamentally have a smaller user community than the VLA. That is the price one pays for resolution far beyond that of all other astronomical instruments (although optical interferometers, with very limited imaging capability, are catching up).

As planned, the VLBA has brought a new level of maturity to VLBI science. Observations that were difficult before are now reasonably straightforward. In some cases, such as phase referencing, polarization observations, high frequency observations, and the ability to respond to transient events, the improvements have been dramatic. We now have a much higher level of reliability and data quality than was available under the Network. And the available observing time has increased greatly now that we have a dedicated instrument. These were the reasons that the VLBA was built. For many examples or what has been done by the VLBA so far, see the contributions by Reid, Moran, Pearson, and Roberts in this volume, and the proceedings of IAU Colloquium 164 (Zensus, Taylor, and Wrobel 1998).

One experiment, now in progress, really demonstrates the great increase in capability of the VLBA over the Network. This is the Diamond, Kemball, and Boboltz project (BD046) to study the changes in dynamics and magnetic fields of the SiO maser shell just outside the surface of the Mira variable star, TX Cam. The star has a pulsation period of 557 days. For an entire cycle, the masers are being imaged, in all 4 Stokes parameters and with many spectral channels, every 2 weeks. The observing frequency is 43 GHz. The Network could not reach this frequency, had great difficulty doing polarization or spectral line observations, let alone the combination, and certainly could not possibly observe something every 2 weeks for 2 years. We look forward to seeing the resulting movie!

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The Development of the VLBA: Challenges Facing the Recording System

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MIT Haystack Observatory

History

Design concept studies for the VLBA started in 1980 and detailed design studies for the data recording system started in 1983. At that time the MK3 VLBI recording system was the only wideband VLBI recorder. However it was only capable of recording 28 635-micron wide tracks of data on a 9,200 foot long tape. Work was in progress for a narrow track version of the MK3 capable of recording 392 40-micron wide tracks in 14 passes of the tape. While a design based on an evolution of the MK3 to narrow tracks and higher bit densities was favored, there were studies of a recording system based upon the use of multiple VCRs to increase the data rate of the highly successful MK2 recording system. Barry Clark, whom we honor at this symposium, and Ron Weimer (NRAO) were largely responsible for the MK2 Recorder. Almost all the work on the MK3 was being done at Haystack by Hans Hinteregger and John Webber, while Ray Escoffier at NRAO pursued the VCR-based system with technical input from Alan Yen's Canadian group. Recorder studies also were being conducted at Caltech by Benno Rayhrer (JPL) and Marty Ewing. In early 1984, various high-data-rate recorders developed for the TV industry and the military were investigated for possible use by the VLBA but were found to be very expensive and of unproven reliability. With the recorder approach somewhat in doubt, Haystack Observatory started work on the I.F. processing and data formatter electronics. In mid- 1984 the first high-density MK3 recordings were made and correlated to yield fringes. This success and the commitment of the geodetic VLBI programs to the MK3 was probably the turning point in favor of the MK3 longitudinal recorder over the VCR cassette based system. It is interesting to note that VLBI recorders based on all 3 systems now exist. The VLBA and MK3/4

Table 1: Recording System Parameters

Recorder Type	Longitudinal Instrumentation
Head Width	38 μm
Head Gap Length	0.3 μm
Number of Heads per Stack	36
Head Pitch	698.5 μm
Tape Width	2.54 cm (1 in) nominal
Tape Thickness	16 μm
Reel Size	35.6 cm (14 in) diameter
Number of Passes per Tape	14
Track Density	20/mm
Record Time per Tape	10.5h[1]
Shipping Weight per Tape	7.6 kg
Linear Data Density	2b/μm[2]
Maximum Record Rate	256 Mb/s per recorder

Notes:
[1] At 128 Mb/x
[2] Excluding 12.5% overhead for Parity

recorders are based on the longitudinal recorder, the Canadian S2 system is based on the VCR and the Japanese K4 is based on the professional ID-1 cassette recorder.

VLBA recorder characteristics

Table 1 gives basic parameters of the recording system. The recorder employs a Metrum model 96 longitudinal transport and 1-in-wide magnetic tape. A high data density on the tape is achieved by using a stack of 36 heads spaced at 698.5 μm which write tracks narrow compared to their spacing. The headstack may then be moved a distance slightly greater than the width of the heads, and another pass may be written on the same tape without overwriting the previously written passes.

The original specification for record time was for 24 hours of unattended operation at an average 100 Mb/s rate. To meet this specification the VLBA has 2 recorders at each site.

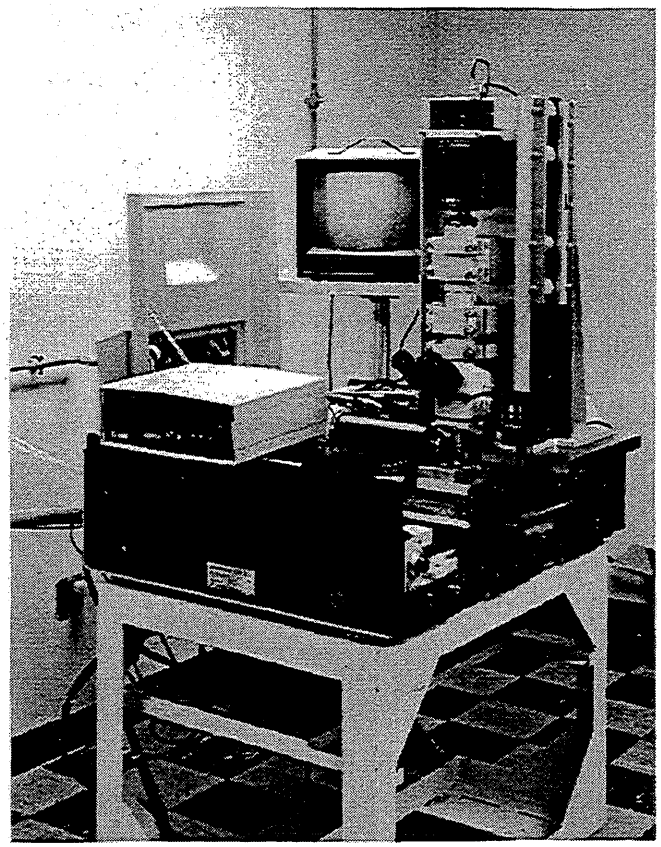


Figure 1. Computer controlled dicing saw (DISO 51) at the Haystack Observatories Westford facility. This machine was critical to the development of the techniques for fabrication of the precision "tip" plates and the subsequent accurate definition of the head locations.

Challenge 1 - Narrow track headstack

The key element of the VLBA recording system is the array of record/reproduce heads known as a "headstack." The individual heads have a 0.3-micron magnetic gap that is 38 microns wide. The heads are made of ferrite with hard nonmagnetic spacers between the heads. The challenge was to fabricate the headstack with good performance from every head along with an accurate spacing or head pitch of 698.5 microns. Attempts to make a headstack by gluing together individual VCR heads were known to produce unacceptable pitch variations. The key to this challenge came from a combination of the methods used by Honeywell (now Metrum Inc.) to manufacture wide-track headstacks and the use of a precision computer-controlled dicing saw. The saw shown in Figure 1 was used to cut away the ferrite of a tip plate, shown in cross- section in Figure 2, to precisely define the placement of the individual head gap. The technique was first mastered by John Webber (now at NRAO) and Hans Hinteregger at Haystack and later transferred to industry, thereby making the headstack a commercial product. Today a headstack meeting the VLBA specifications can be purchased from Metrum, in LIttleton, CO, or Spin Physics, in San Diego.

Challenge 2 - Understand and improve the recorder tracking

When a particular longitudinal tape transport was selected for VLBI, Hans Hinteregger realized the Honeywell model 96 transport was unique among instrumentation transports in its method of "tape edge" guidance. Other transports would allow the tape to move randomly back and forth between guides separated by more than the tape width while the model 96 biases the tape to guide on one edge so that the tape passes across the head in an almost completely reproducible manner. Figure 3 shows the tape path of the model 96. The vacuum buffers have tilted walls which force the tape into a conical shape at the turn-around points. The conical shape, combined with the force of the vacuum, makes the tape contact the precision back plate in the regions indicated in the figure. The edge guides are on the back plate. Unfortunately, these edge-guiding points are a significant distance from the heads, allowing elastic deformation and variations in the capstan surface to influence the track placement. Figure 4 shows the reproducibility of a typical tracking signature as a function of the tape footage from a recording made in the forward direction. Since a forward recording is only played back in the forward direction, the larger reverse signature only requires a larger guard band between adjacent forward and reverse tracks. I found the idler roller to improve tracking by adding constraint to the changes in elastic isotropy of the tape and advocated its inclusion in the VLBA recorder. It had been previously removed in favor of simplicity and reliability (the bearing being prone to failure). Accurate guidance is essential to keeping the heads on the recorded tracks during playback. I

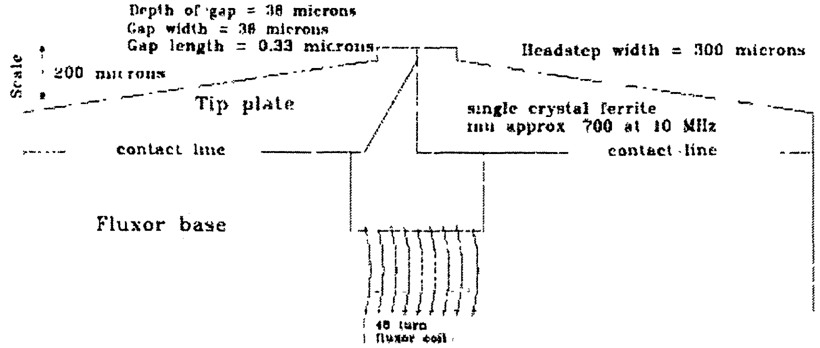


Figure 2. A cross-section of a single read/write head in a VLBA headstack. The tip plate is made from two pieces of ferrite bonded together with a 0.3 micron gap between them. The "gapped" bar is machined to define the head locations and a non-magnetic spacer comb is used to fill the regions between the heads. The tip plate is epoxy bonded to the fluxor bases and the whole assembly is lapped. The headstack is completed by machining an initial contour and running in with a lapping tape.

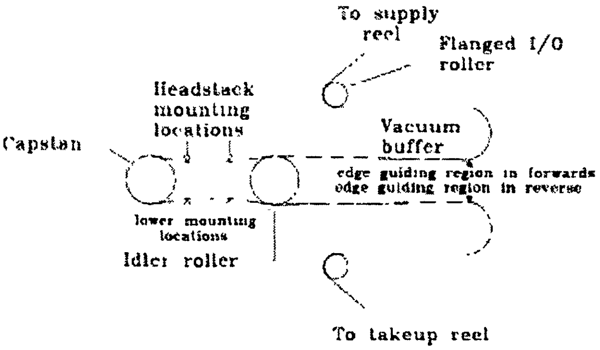


Figure 3. Tape path of the model 96. The capstan moves the tape. The reel motor and photo electric sensors servo a constant loop of tape in the vacuum buffers. The tape edge guiding occurs in the vacuum buffers.

spent a lot of time studying the model 96 guidance and evaluating the guidance limits imposed by the elastic relaxation of the tape. A "prepass" of the tape is needed to allow the strains stored during shipping to relax prior to recording or playback. With a prepass, the model 96 transport is adequate for the narrow, 38-micron-wide tracks, but may require further improvements to support even narrower tracks.

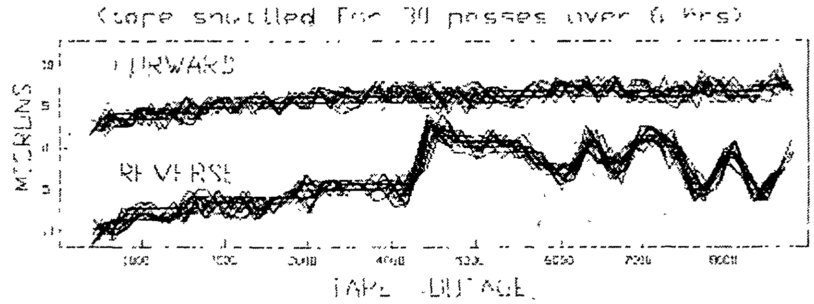


Figure 4. Reproducibility of tracking narrow track recording made in the forward direction.

Challenge 3 - Reducing head wear to ensure adequate head-life

The VLBA headstacks are expensive ($7000 to $9000) and their periodic replacement represents a major fraction of the cost of the operating the VLBA. Studies done during the development of the VLBA recorder showed that humidity is a major contributor to head wear. Figure 5 shows that above some critical relative humidity the wear rate increases very rapidly. The critical humidity is thought to be that at which a single layer of water molecules forms on the tape as it passes over the heads. The water greatly enhances the wear produced by abrasive particles on the tape. The key to low head wear thus is to reduce the humidity. This can be easily accomplished by heating the air in the region of the heads.

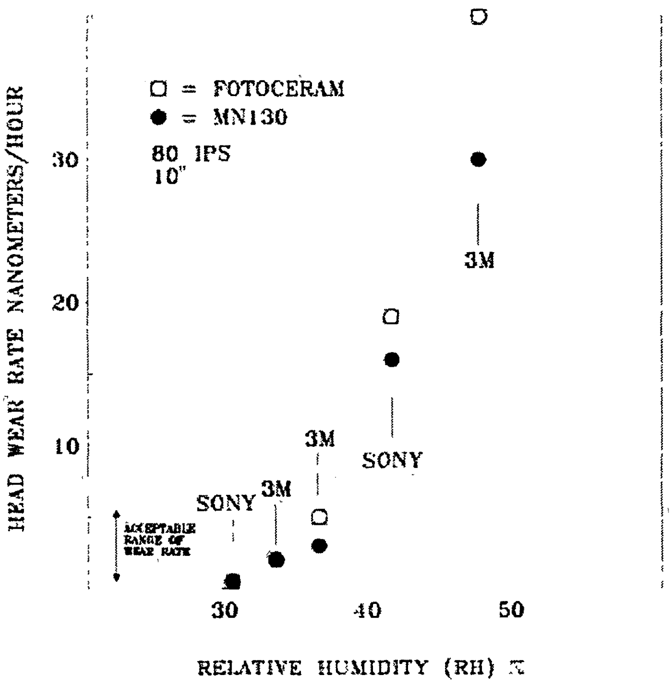
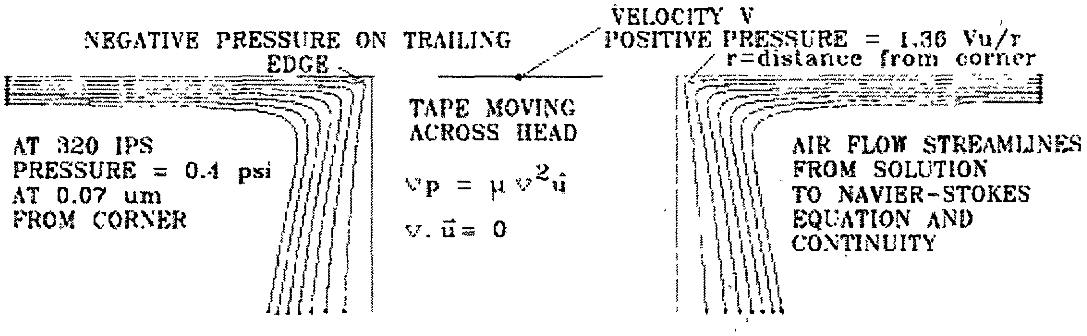


Figure 5. The dependence of head wear on the relative humidity in the region of the headstack. In order for a headstack to last more then 5000 hours the relative humidity needs to be maintained at less than about 35 percent.

Challenge 4 - Tape thickness interchange

During initial operation of the VLBA, two tape thicknesses were in use. The older was the 9200-foot-long, 25-micron-thick tapes. While the VLBA sites switched to thin tape several years ago, the VLBA correlator has been required, until recently, to process thick tapes from the EVN. The problem is that heads used for thick tape will not work with thin tape because the profile worn by the thick tape allows the thin tape to bulge up over the gap as illustrated in



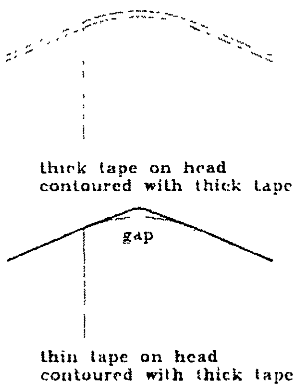


Figure 6. (Left) Trying to record or playback a thin 16 micron tape on a headstack worn to an equilibrium contour with 25 micron thick tape can result in a severe spacing loss.

Figure 7. (Above) The air flow derived from an analytic solution to the Navier-Stokes equation and continuity for regions on the leading and trailing edges of a "stepped" head. If the tape speed is sufficient, the positive pressure at the leading edge can open a gap between the tape and the head allowing an air film to form between the tape and the head.

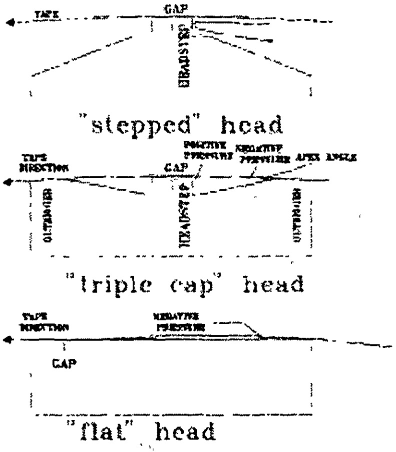
Figure 6. One solution is to raise the tension when using thick tape. For perfect compensation, the tension should be proportional to the thickness cubed, so that going from thin 16-micron tape to thick 25-micron tape ideally requires an almost fourfold increase in tension. Another solution currently under evaluation is the use of "triple cap" profiles to reduce the sensitivity of the head to tape thickness.

Challenge 5 - Tape flying

In order to record and reproduce at high bit density, the magnetic particles in the tape must be in extremely close contact with the head gap. Any separation produces a spacing loss of 55 dB per wavelength. At high tape speeds, air can be drawn into the region between the tape and the head. The theory of head-tape interface is largely one of classical fluid mechanics for which the airflow and pressures are governed by the Navier-Stokes equations and the Reynolds equation in the special case of flow in a channel of variable thickness with moving walls. Thus the more general Navier-Stokes equations are needed for the extended regions at the entrance and exits while Reynolds' equation is relevant to the channel between the tape and the head. Prior to the study of the head-tape interface, Reynolds' equation was mainly applied to the fluid mechanics of bearing lubrication for which contact is to be avoided rather than promoted. With the single cap profile of the current headstack, an entrance gap can easily develop from the positive pressure introduced by the tape motion. There is a corresponding negative pressure at the exit as illustrated in figure 7. Figure 8 shows the "stepped" head currently used by the VLBA. Recent concepts in head profile design such as the triple cap and the flat head profile of Hans Hinteregger and Sinan Muftu take advantage of the negative pressure to enhance the contact at high tape speeds.

Challenge 6 - Tape edge damage due to frictional heating

The most severe challenge during the development of the VLBA recorder was the discovery that thin tape is easily damaged. The damage is not apparent to the naked eye but manifests itself most clearly as a "bumpy" pack formed on the reel. As the damage increases, the tape pack becomes catastrophic and it becomes impossible to run the tape on the transport. The damage is the result of a microscopic increase in the tape thickness at the edge of the tape as illustrated in figure 9. The edge thickening results in an unstable tape pack as the tape winds and tries to force the tape flanges apart to accommodate uneven shape. The edge damage was found to be the result of flash heating of the edge of the tape above the temperature at which the PET (polyethylene terephthalate) or tape binder starts to flow. The solution was to redesign the edge guiding surfaces in the model 96 with what is now called the "thin tape upgrade." The edge heating is reduced by using smooth alumina (Al_2O_3) guiding surfaces, which have low friction and a resistance to the buildup of deposits. The original edge-guiding surface was slightly porous and allowed deposits to form which then increased the friction and insulated the tape edge from the cooling of the guide.



(Hinteregger and Muftu 1998)

Figure 8. The current VLBA uses the "stepped" head contour which tends to scrape the air away from the head-tape interface but is still prone to "flying." The triple cap contour is more immune to flying and is relatively insensitive to tape thickness. The flat head contour uses the negative pressure to improve head-tape contact at high speed.

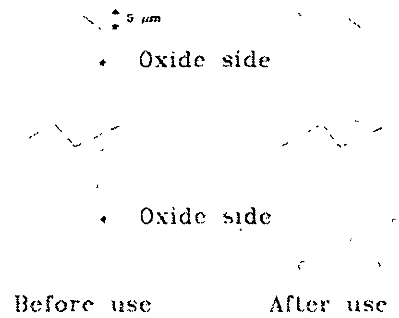


Figure 9. The edge damage caused by frictional flash heating at high speed in the original model 96 edge guide. The figure is greatly exaggerated. Only a very small thickening of the tape edge can lead to catastrophically "bumpy" tape pack on the reel.

Other Challenges:

Overwrite

Another challenge was the careful control of the write current to avoid overwriting previously recorded tracks by the cross-talk in the headstack. For simplicity, the headstack has no shields between heads and there is some mutual inductance coupling between heads. As the head gap depth wears down, the head becomes more efficient so that the write current needs to be reduced to avoid overwrite.

Tape reels and shipping containers

Hans Hinteregger developed a reel to prevent scatter-wind, the non-parallel packing of tape on the reel. The glass reel has convex flanges which have a "self-packing" effect on the tape. A well-wound tape pack is quite strong and behaves like a solid disk of plastic, which helps resist damage during shipping. Hans Hinteregger and George Peck, of NRAO, developed a rugged shipping canister by adding packing material to a commercial shipping canister.

Thin film head development - future enhancement of the VLBA recorder

The desire to increase the density of data on tape and reduce the cost of the heads has led to development of an array of heads using thin-film photolithographic technology now employed for disk heads. This work has been done in collaboration with industry. It is expected that an array of 64 heads based upon Seagate's "Peregrine" head will be ready for VLBA use in a few years. While the write function still uses a coil and magnetic pole pieces, the read function is separately provided by magnetoresistive elements.

Acknowledgements

Hans Hinteregger was the major technical contributor to the narrow track headstack and the successful transfer of this technology to industry. Others who have made major contributions to the Mark 3A and VLBA recorders are John C. Webber (now at NRAO), William T. Petrachenko (now at the Canadian Geological Survey), Roger Cappallo, Daniel Smythe, Mark Derome, Peter Bolis, Ken Wilson (Haystack Observatory), George Peck and Ron Weimer (NRAO), and Russ Ruhl (Honeywell Test Instruments Division, now Metrum Corporation).

The VLBA development (and supervision of the NRAO contract with Haystack) was initially managed by Hein Hvatum of NRAO. Hein was a very businesslike taskmaster and taught me the value of project schedules and weekly coordination telecons. Later the project management was taken over by Peter Napier. I thank both Hein and Peter for adding the air of excitement and challenge to the otherwise arduous task of project management. Hans Hinteregger and Sinan Muftu are currently developing the thin film head arrays at Haystack Observatory with support from NASA and in collaboration with industry partner Seagate, with additional support from the EVN. Current recorder research at the Haystack Observatory is also supported by the National Science Foundation.

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The Challenges of the VLBA Correlator

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As a preliminary to my story of the VLBA correlator, I'd like to start by talking about a different instrument, bearing Barry Clark's imprimatur as much as does the VLBA: the NRAO Mark 2 VLBI System. It had an enormous impact on the development of VLBI, and on my own career as well. It was, I believe, a rather un-NRAO-like concept, and very much a contrast to the VLA, NRAO's then-new, biggest, best instrument. The Mark 2 system included half-rack "terminals" that could be set up at a variety of radio observatories, requiring only an IF input and 5-MHz and 1-pps signals from a frequency standard. The correlator handled (initially) a single baseline, playing back pairs of tapes; the only other input to the correlation process was in the form of hand-written paper log forms, plus Xerox copies out of astronomers' notebooks, describing the observational setup, which often included large local-oscillator offsets. While this approach to VLBI was surely the only feasible one at the time, it was just the opposite of the start-from-scratch, do-everything-perfectly approach to which those working on the VLA became accustomed. This dichotomy had a long-range effect, setting up some of the internal conflicts with which the VLBA project had to contend a decade and more later.

I first encountered the Mark 2 correlator, and Barry Clark too, in 1972, when, as a Caltech grad student, I went to Charlottesville to correlate my first VLBI "experiment," as we called them then. Richard Schilizzi, a postdoc recently arrived at Caltech, was my mentor. I soon acquired a copy of the just-issued NRAO VLBI bible, Electronics Division Internal Report 118, by Clark, Weimer, and Weinreb -- and still have this same well-thumbed copy on my shelves. I remember reading there about the spectral profile introduced by the finite set of lags sampled by the correlator. I was sure that something about it, I don't remember just what, was completely wrong, and I hunted out Barry's office in a remote precinct of the Edgemont Road building that I hadn't known was there before. I introduced myself, and went on directly to say I thought this certain equation must be wrong. Barry did skip a half a beat, then said simply "No, it isn't." After an embarrassing (only to me, no doubt) silence, I started to explain what I meant, but Barry just said again that the equation was right as it stood, and turned back to his work. I remember reporting this incident to some of the Mark 2 correlator people. One said, "Yes, that's Barry; just think about exactly what he said for long enough, and you'll realize it's exactly what you need to know." This wasn't much help, since he'd only contradicted my impertinent criticism of the equation in question, but I did realize somewhere along the way that, of course, it *was* correct all along.

Although it produced an impressive list of scientific discoveries, everyone who used the Mark 2 system surely remembers the frustration of working with it and its output. Barry had written a good deal of post-correlation software, mainly oriented towards continuum observations. I wrote much of my own for the absorption spectroscopy I was doing for my thesis, re-discovering in the process Barry's approach of having almost all programs read and write a universal format. When I went to Bonn for a postdoc, I worked with a copy of the NRAO correlator (expanded to 3 stations by that time) that had been built there. I added a whole front end to the operational system to facilitate getting the right observing information into each of the many "scans," a process that -- with a many-year effort on Barry's part -- we've finally *almost* gotten right in the VLBA. More fundamentally, I developed algorithms for removing a number of instrumental effects from the phase of the visibility output -- an essential prerequisite for applying the new phase-closure techniques, just developed at Caltech by Readhead and Wilkinson, that I wanted to implement at the MPIfR. The Mark 2 correlator had not really been designed to deliver usable phases, so it took a lot of digging into the hardware and software, but I was able to get it to work right finally, overcoming the only really fundamental flaw in the entire Mark 2 VLBI system.

I've spent as long as I have on Mark 2 because it seems to me it can be credited with really establishing VLBI as an astronomical technique, and without it there might not have been a VLBA. By the time it was finally superseded almost everywhere (I believe it's still used locally in Italy) by the Mark 3 and VLBA systems, Mark 2 terminals had been used at virtually every major radio observatory in the world, and at a great many minor ones. This could well be considered Barry Clark's most important contribution to VLBI.

I came to NRAO from the MPIfR in 1983 expressly to work on the VLBA. Mort Roberts, then Director of NRAO, was on the MPIfR Fachbeirat, more or less the Visiting Committee, so I had an easy connection to the project as it developed, and was able to choose an opportune time to make this move. I had sent Project Manager Hein Hvatum a long list of what I thought I might do on the VLBA project, and when I arrived I found he'd picked out (possibly having read what I wrote) the correlator for me to work on. He also told me the correlator was going to be built, under subcontract, by Caltech. This was the first challenge of the VLBA Correlator that I encountered: everybody wanted to build it ("at least initially," as Alan Rogers might say) -- including both Caltech and Haystack. But it was fine with me to have Caltech involved in this way. I had a number of old friends there.

I did want to work in Charlottesville for a while, to get to know NRAO first. Hein suggested I try to find a short-term lease in Charlottesville. This was no mean feat, as anyone familiar with that town knows, but I did it. And by the time I eventually bought a house there, I had renewed my short term lease about five times. The funding situation, described further later on, never did settle down enough for NRAO to commit to the move.

With John Benson, who was in the same position, I made quite a few trips to Pasadena for preliminary discussions. (We also collected enough data to persuade Hein to agree to pay us a subsidy for the excessive cost of housing in southern California.) These trips eventually developed into a serious design effort, under the leadership of Marty Ewing, who had been responsible for the Block 0 and Block 2 correlators at Caltech. Others making major contributions to this design were Tim Pearson and Dave Fort.

Many of the VLBA correlator's basic specifications and concepts were developed during this period, including the 20-station capacity, 24 playback drives, simultaneous correlation of multiple observations, 500-kB/s aggregate output data rate, and the separate archive and distribution tapes. The 20-station capacity was suggested by Ken Kellermann as accommodating both a substantial number of non-VLBA stations recording one tape at a time, or the ten VLBA stations recording two tapes simultaneously. We derived the 500-kB/s limit in two ways: first, from a scientific requirement to observe fields of view as large as 4 arc minutes at 22 GHz without washing out the fringes; and from the capacity of the fastest nine-track tape drives Tim

Pearson and I could find in the catalog, with a number of units limited at the boundary of sweatshop conditions for the operators.

The resulting preliminary design was written up as VLBA Correlator Memo No. 41, the only memo in any of the many VLBA series that was distributed in a Caltech preprint cover. The design was a lag architecture, with a 14-station intermediate station capacity, optimizing the baselines-lags tradeoff which is almost universal in lag correlators. It included development by JPL of a VLSI chip, with specifications similar to those of the chip subsequently developed by Albert Bos at Dwingeloo. The correlator would have occupied about 5 racks (not including tape playback drives or computers) and cost about $2M in hardware.

All this while, Hein had been increasingly unhappy about the two VLBA subcontracts to university groups (Haystack's subcontract for the VLBA recording system was the other). These were a new arrangement for NRAO at the time, although we now see similar partnerships in the Millimeter Array. Hein always felt that nobody at Caltech took a sufficiently businesslike approach, until Ted Seling joined the group. Ted produced a project budget that was much to Hein's liking -- except that the cost was a good deal more than the VLBA project could afford.

At this point, what the project could afford was quite a volatile concept -- and this was the next challenge of the VLBA Correlator. NSF had just cut the VLBA project budget, resulting in a million-dollar "negative contingency" in the overall cost-to-complete. Hein was unwilling to renegotiate the "very favorable contract" (as people always referred to it) for the antennas, so a big hit had to be taken elsewhere in the project. The correlator was a convenient target, since so few NRAO people were working on it, since its cost was considered to be over budget, and since it had just reached a transition point with the completion of the preliminary design. It also was hoped that anticipated developments in the microelectronics industry would allow a better correlator to be built after a one- to three-year hiatus. Ken Kellermann argued to the contrary, that it would be crazy to delay such an essential component of the entire system, especially in view of the history of delayed completion of other VLBI correlators, at Caltech and at Haystack.

After much discussion, culminating at the Third VLBA Design Review in Green Bank in September, 1985, about the time Peter Napier took over as Project Manager, it was decided to suspend construction of the correlator, and to use the slack time to seek ways to reduce the overall cost (including personnel) by $2.5M. Neither party (NRAO and Caltech) felt they could commit to a later resumption of the existing correlator subcontract, which accordingly was terminated by mutual agreement. Full-scale funding for construction of the correlator did not resume until the beginning of 1988. Both views of this suspension quoted above were surely correct: we certainly ended up with a better, more flexible and more capable correlator; but we rued enormously, as the VLBA project neared completion, what ended up as a two and a half year delay in starting the actual construction of the correlator.

In addition to the specifications, a couple of concepts developed or considered during that early study at Caltech stayed with the VLBA correlator through its completion, and should be credited to Marty Ewing. Early in the Caltech design study, he suggested we might look into the "FX" spectral correlator concept (Chikada's paper on the Nobeyama implementation had just been published). We were tempted by this, but decided we didn't have time to study it well enough to be sure it would work out for the VLBA -- since we were then in a hurry to start building the correlator. And, of course, we subsequently did have, and exploited, the opportunity to do exactly that. Marty also suggested the specialized complex floating-point number representation, with separate real and imaginary mantissas and a common exponent, that we eventually used to good effect in the VLBA correlator.

With the suspension of correlator construction, and the withdrawal of Caltech from further involvement, I became head of the VLBA Correlator group. It was quite a small group at first; we were trying to conserve funds, after all. John Benson continued to be involved, and we added Ray Escoffier, who had been evaluating a VCR-based recording system for possible use in the VLBA.

I picked up the FX concept where we had dropped it earlier, during the Caltech study, and asked Ray to consider it. He soon found it appeared to offer some significant economies. In this correlator architecture, a fast Fourier transform is first applied to the data from each input station; this is the "F" stage. Then, in the "X" of the name, each of the resulting spectra is "cross-multiplied" by each of the others -- point by point, without a whole series of shifts as in a lag correlator. By exploiting the efficiency of the FFT algorithm, and organizing as much processing as possible on a station basis, the total number of arithmetic operations required can be reduced significantly.

To continue studying the FX architecture, we developed several simulations. John Benson developed a software simulator, which required all the power of our then-hottest computer, the Convex C-1. Ray told me of an opportunity to hire Joe Greenberg, who previously had been an NRAO co-op student, and we soon had Joe working on a hardware simulator.

A principal question in all this was the required precision of the internal number representation in each FFT stage. We tried various ranges in both the signal values and the "twiddle factors," the roots of unity used in computing the FFT. Barry had been sitting in on our frequent teleconferences in which all this was discussed. He wrote a one-page VLBA Correlator Memo (No. 70) in which he deduced from first principles that six bits was an adequate precision for the twiddle factors. We eventually settled on a (7,7,4) representation for the data -- seven-bit real and imaginary mantissas, with a common four-bit exponent -- for both signal and twiddle values. This result was to have unforeseen negative repercussions that I'll get to shortly. Ray realized that we could even increase the effective precision of the twiddle factors by looking up a large number of slightly different values in an oversized table. We hit an obstacle in transmitting the station spectra from the output of the F stage, which performs a data *expansion* operation, to the input of the X, cross-multiplier part of the FX system. After more simulations, we decided it would be acceptable to truncate the data to a (4,4,4) representation at this point. We were able to get Chuck Broadwell to move from Socorro temporarily to join the group about this point; while trying to conserve VLBA project funds during the suspension of correlator construction, I also wanted to be ready to start full-scale work once the funding could resume. Chuck worked on simulations of the cross-multiplier itself, and came up with a (14,14,8) representation, which once again led us into a trap I'll come to shortly.

We presented these results in several meetings along the way. The first was an "FX Correlator Workshop" in February 1987. This elicited a proposal submitted by Haystack which showed that the "everybody wants to build it" challenge of the VLBA Correlator was still current. The Haystack design concept was based on the VLSI chip developed by Albert Bos. It would have occupied 16 racks or more, and was budgeted at about $5M in hardware.

The first major design review of the VLBA correlator as built was held in October 1987. The work was generally very enthusiastically received, and we were urged to follow through to a more detailed design of the FX architecture. Marty Ewing participated in this design review, and made another suggestion that led to a major feature of the VLBA correlator. He pointed out that an FX architecture would allow a de-dispersing pulsar gate to be included in the system, and that a single, system-wide gate would be sufficient for the station-based delay and phase compensation we were considering.

Another neat feature of the FX architecture became a frequent subject in these internal reviews and in the meetings of the VLBA Advisory Committee: the dreaded "hybrid mode." Interest in this arose from the partial incompatibility between the many narrowband (2-MHz) channels which the Mark 3 VLBI world continued to use, and the fewer (16), wider-band channels adopted to reduce the cost of the VLBA recording system. We expected the rest of the world to migrate toward the VLBA standard (and they have done so, although not exactly as we thought), but an interim scheme was needed to preclude limiting the compatible bandwidth of global VLBI observations to only 32 MHz. The hybrid mode could provide this, by allowing us to correlate (for example) four 2-MHz baseband channels recorded at a non-VLBA station, against a single 8-MHz VLBA channel. Barry commented to me during a break in one of our internal reviews that it was amazing what tricks one could do in an FX architecture. Ray worked out a clever way to feed the input data streams to the FFT processors, but pointed out that we would need 32 fringe rotators per station, instead of the 8 required otherwise. But my real reason for calling this the "dreaded" hybrid mode is the software required to specify and implement it. In the end, it never rose high enough in our priorities before the rest of the world did catch up to the VLBA. My first thought on hearing of the Mark 4 system was that the hybrid mode was now no longer necessary. From a VLBA perspective, Mark 4 amounts to filtering back into the Mark 3A system many of the enhancements developed by Haystack for the VLBA recording system. (To be fair, it must also be mentioned that Mark 4 does go a factor of two beyond the VLBA recording system in aggregate data rate.)

Funding for correlator construction finally became available at the beginning of 1988, just in time for the first stage of major spending: development of the "FX Chip," as we called it -- the next challenge of the VLBA Correlator, and surely first in terms of importance. It was clear that a custom or semi-custom VLSI chip of some kind would have to be developed. A single, dual-purpose chip seemed a good match, with an opportunity to save on the development cost, since a complex multiply-and-add was needed both for the FFT butterfly stages and for cross-multiplication. Indeed, much of the work done previously had been aimed at setting the specifications for this chip. The optimal number representations mentioned above for the F and X sections both appeared to be perfect matches to a particular semi-custom ASIC offered by LSI Logic, one with an on-chip RAM configurable in 18- or 36-bit widths (presumably intended for a very different application, as two or four bytes with parity). The RAM seemed ideally suited for inter-stage data storage in the FFT, and for short-term accumulation in the cross-multiplier.

Because this would be a major fraction of the correlator's total cost, we had to go through an elaborate procurement process. We fully expected the LSI product to win because it was such a good match to our requirements, but we were unpleasantly surprised: the bidders' responses to our first RFP were all well beyond our budget estimates. In particular, LSI Logic offered a full-custom product, at about 50 percent over our estimate, instead of the semi-custom ASIC we had expected. It seemed that we had come too close to the edge in the number of gates our design required, and they were not confident of fitting it into the product that seemed such a good match. With hindsight, I have no doubt we should have bitten the bullet at this point and gone with the more expensive product, but the mandate to save $2.5M had been very forcefully put, and predominated. Chuck soon found a clever way to get *almost* the same performance with less internal memory, by splitting the longer FFT sizes over several parallel FFT engines. The main loss was in the spectral resolution achievable in cross-polar correlation. With this and some other changes leading to more efficient use of gates, we issued a second RFP. LSI Logic did not bid this time, but their product line was represented by Hallmark Electronics, who operated a design center for LSI in Columbia, Maryland. We eventually awarded the chip contract to them, and Chuck and Joe went off to Columbia in October 1988 to design the chip.

This was by no means the end of the FX Chip challenge. The prototype chips were not satisfactory, and it took several rounds of prototyping before the cause was understood and could be corrected. The fault turned out to be in the on-chip RAM, which had been such an attractive feature; in other words, we had expended a substantial effort to buy a lemon. The problem was exacerbated by the fact that the original designer of the RAM was "no longer with the company." And, just to keep things from getting boring, Hallmark Electronics decided to close their LSI design center. This last crisis actually led to a major improvement in the situation. LSI Logic took over our chip contract (after some veiled threats that we might have to bring in the Feds), which allowed us direct access to their expert engineers for the first time. To their credit, LSI did eventually make things right for us. But the whole process delayed our schedule by a total of fourteen months.

While waiting for the FX chips, the design of the correlator boards went ahead, gambling a bit that the designed pin arrangements would not have to be changed. These included the FFT and cross-multiplier boards, plus two boards for the newly-added "Playback Interface" function. This was a late addition to the correlator project, originally planned to be part of the recording and playback system being designed and built by Haystack. Problems in other areas of that system, particularly in completing the VLBA formatter and the Playback Drives, had left the "Data Playback Crate" (DPC), as it was known then, in limbo. We had reached a stage in development of the correlator where some fairly specific interface specifications were required. A DPC design review was convened at Haystack in October 1987, where a preliminary design was presented. We were fairly shocked at the cost estimate of nearly $100K *per playback drive*, but even more dismayed by the lengthy time forecast before these units could be available.

Ray Escoffier had another of his inspirations on the way back to Charlottesville, and told me the next day he thought we could incorporate this part of the system into the correlator's delay unit a good deal less expensively, and much faster, than the estimates we had just heard. By more-or-less mutual agreement, we redefined this backend of the playback system as the frontend of the correlator, or the Playback Interface, as we began calling it. Ray's intuition was certainly correct as to the cost: the *entire* PBI subsystem, for all 24 playback drives, cost about $140K, not including design costs. But we all underestimated the time and effort it would take to make this part of the system work, with its intimate involvement in the relatively unfamiliar tape technology. We did add one engineering position to the group, which was filled immediately by transferring Gene Runion, who had joined the group shortly before this, and one technician, but should have added more than these.

During much of this time the software effort had been carried by John Benson alone. He concentrated initially on the simulations mentioned previously. As the first software actually destined to become part of the correlator, John then took on the "wavefront model," or just "the model," in our jargon. This referred to all the computations, mainly geometric but also including other effects, required to predict the interferometer delay and phase accurately enough. Initially we had considered "accurately enough" to require only that the fringes would appear within about a microsecond of residual delay, and at residual fringe rates less than about 100 mHz. But two of our colleagues moved the goal several orders of magnitude farther out.

Barry Clark was the first of these. One day we received an e-mail suggesting, out of the blue, that we adopt the CALC software, developed by the VLBI geodesy group at Goddard Space Flight Center, to compute the model. This software was, and still is, used to recompute what the best possible model would have yielded in geodetic or astrometric observations; the difference between this ideal value and what a correlator actually used is then applied as a correction in post-correlation processing.

I called Barry and asked why on Earth we would need such accuracy. He said he expected there to be an important class of observations requiring much better model accuracy than one would need for imaging, but still short of the full geodetic/astrometric treatment. Phase referencing is the most obvious such case, and indeed this technique has been one of the areas in which the VLBA has broken completely fresh ground in VLBI. I'm not sure whether Barry was in a garrulous mood that day, or I had learned to interpret his normally Delphic explanations, but I understood instantly the value of his suggestion that we use CALC.

There were some problems in interfacing CALC for real-time use, on a station basis, but John Benson was able to get around these. Among other expedients, he replaced the GSFC interface to their external database. Other correlators have since followed NRAO's lead in using CALC, and the imminently anticipated version 9 will include features designed to support this sort of application.

Craig Walker also urged, over many years, a variety of improvements in the wavefront model. His pursuit of perfection in this area has led us to a situation where the VLBA's phase stability is better than that of many connected-element interferometers, and, in the very best cases, it's possible to do imaging without any need for fringe fitting.

Going back to the early days of VLBA correlator software, the original plan for software staffing -- to exploit the funding delay -- was that programmers working in Barry's VLBA station control group would become available to work on the correlator about the time the hardware design had progressed sufficiently that these two groups could work well together. After work finally resumed on the correlator, it soon became clear that software support would be needed sooner than it could be available following this plan. John and I also felt the need for an overall software designer, someone to provide leadership for the software effort in the way that Ray Escoffier had been leading the hardware development. I discussed this requirement with Barry, Peter Napier, and Bob Burns, among others. One day Bob told me he'd had a "crazy brainstorm": we should draft Don Wells out of the AIPS group for this purpose. I hadn't known much about Don's background, and had only discussed FITS issues with him as far as I can recall. But after some initial conversations I thought Bob's suggestion might work out quite well. Don wanted to go to Socorro to discuss the idea with Peter and Barry. I didn't go along, but have often wished I'd been there to witness this first interchange between Barry and Don, two men definitely in the Gaussian tails on opposite ends of the prolixity axis. We carried through on Bob Burns' suggestion, and Don became the correlator software designer in March 1988. As part of bringing Don on board, we had also agreed to start hiring a number of other programmers to be part of the software development group. Jim Horstkotte began working on the correlator in June.

Don's first major activity was to study the "software development environment," as he called it, meaning both the computers and operating system to be used by the programmers, and the real-time operating system in which the software would actually execute. VLBA standards already existed for both of these: Barry's station-control group had been working with DEC computers running VMS, and the Versados real-time system, for some years. These had been selected by Craig Walker's VLBA-wide computer coordination group, sufficiently earlier that the correlator input to the process had been provided by Marty Ewing. Don and Jim were both reluctant to adopt these standards, and their efforts to change them unleashed another challenge of the VLBA Correlator, the VLBA Computer Revolution.

Using VMS, a proprietary operating system, would tie us to DEC, and, several years after the original decision, there was some concern whether DEC would continue to provide suitable platforms at reasonable prices. Don was convinced a sea change was occurring, with Sun supplanting DEC as the major vendor for our sort of application, with Unix as the new common operating system. It's clear from today's perspective that he was absolutely right -- indeed, a new sea change may now be displacing Sun.. But at the time, such a suggestion evoked ardent partisanship, often likened to sectarian warfare, in many quarters.

The discussion of Versados, and VxWorks, the competing product selected by Don and Jim after some investigation, was more dispassionate and technical, probably because Barry's group had been oppressed by the shortcomings Don objected to in Versados. But still, for the correlator to adopt VxWorks meant either that the VLBA would be run with two different real-time operating systems, or that much of the station-control software would have to be rewritten.

In a series of VLBA computer coordination meetings in the second half of 1988, both sides of both these issues were presented. I believe Barry's position was basically a conservative one, not to change anything; but he also insisted on a single VLBA standard, even if it meant his group's adopting a new standard. The resolution eventually fell to Peter Napier, who decided for the change of the VLBA standard. The correlator group was soon equipped with the second through fifth Sun workstations in the Edgemont Road building (the first had been part of the AIPS group's effort to provide support on as many platforms as possible), running Unix, and started programming VxWorks real-time code. Barry's group spent quite a few months making the conversion to the new systems. I doubt anyone can really imagine the VLBA we would have today if these changes had not occurred.

One task Don took over from me was the selection of a vendor for the database management system. I had been advocating for some time that the VLBA needed a commercial DBMS in order to handle many of the automated operations that had been planned, and had been contacting a number of vendors. Many vendors started calling me, too; I thought they had gotten my number from their friends, but learned later from Barry that he had been referring them all to me! Don took over the selection process, and eventually narrowed the choice to a short list of three. Some of these came to Charlottesville to give us presentations on their systems. The first vendor presentation was by Oracle. I thought there might be more general interest around NRAO in database systems, and went around the Edgemont and Ivy buildings inviting anyone I thought might be interested. I remember Fred Schwab telling me he probably wouldn't be interested, and then asking, after a pause, "Why does NRAO need an Oracle anyway? We have Barry Clark!"

Ray Gonzalez was the last addition to the correlator group, transferring from Socorro in 1990. Ray had worked on the VLA control software, and we had him start by developing an operator interface for the correlator, using the Screens package developed by Barry Clark's group. Ray's interface was soon more advanced than the system it was to exercise, so we moved him over to write the "tape task" which controls all aspects of the playback drives, in particular the tricky mechanisms for establishing and maintaining synchronism of the data streams from multiple tapes.

All these developments finally came together in the Spring of 1992. We had four playback drives, and all four racks of the correlator hardware, crammed into one of the "laboratory" rooms in the Edgemont Road building, and began the ultimate challenge of the VLBA Correlator, one facing any new correlator: searching for fringes. In preparing this reminiscence, I must say I found this period something of a black hole in my memory. I do recall that we obtained "self-spectra," our jargon for the power spectra from the individual antennas (we can't call it the "autocorrelation spectrum" since we don't ever form the autocorrelation function), quite readily. We were working with emission-line spectra, incidentally, which I had realized were most suitable to bringing up a spectral correlator like ours.

The absence of cross-power spectra immediately left the wavefront model as the principal suspect. I spent some effort computing the exact model parameters for a particular time and observation by hand, including all the confusing scaling, and shifting of the binary point. Of course, we couldn't be sure various signs were correct, so a total of four versions had to be carried. We used these results both to check what was being set into the model registers by the system we had built, and to set static values into the registers, in hopes of seeing the fringes drift through the window. Our colleagues working on the JIVE correlator actually found first fringes this year by this technique, which they termed a "correlator drift scan," but it didn't work for us.

Eventually, John Benson found and fixed what turned out to be the "last bug." It took some time to transfer the results of our search runs to AIPS in those days, and to analyze them, so that I happened to be in a position to see the first fringes when they came up later that evening. Since these were line fringes, I displayed the cross-power spectrum, rather than the usual amplitude-against-time plot. I called John, and we spent a feverish night preparing an announcement. To make the obligatory historical plot, I had to intercept the temporary file AIPS sent to the printer, and edit the PostScript text.

That announcement was issued on 1992 May 6. Three months later, we disbanded the VLBA Correlator Group; half moved to Socorro in August with the correlator. Peter Napier and I had decided this move date at the beginning of 1992, before any fringe had been seen, but when I could already see it was close. We wanted to give everybody time to prepare for the move, especially to sell houses in Charlottesville and find new ones in Socorro. To avoid disrupting such plans, we agreed to stick to the move date no matter how things developed with the correlator.

This led to the final challenge of the VLBA Correlator I want to recount: everybody in Socorro seemed to expect it to be operational as soon as it was reassembled. Peter and I knew it wasn't, but many of the Socorro VLBA people assumed its appearance meant it was ready to use. We still had ahead of us a fairly long period of partial operation. In particular, almost the entire set of playback interface modes had to be debugged. We also discovered a variety of subtle bugs, to which we frequently attached the names of various innocent VLBA users whose data evidenced the effect, as the "Diamond effect" and the "Wrobel effect." We spent several months searching for a non-existent bug in the correlator on the basis of symptoms caused instead by improper implementation of the "barrel roll" in the recording system, a feature that in hindsight was put into use far too early. The tape-control task, which had functioned perfectly well enough for fringe searching, was not up to the range of vagaries that occurred in routine observations, and we had to draft Steve Blachman from Barry's group to take over that software. Similarly, the job-generation software also buckled under the diversity of observations which confronted it; Barry himself took over this program.

Miller Goss had appointed Tim Cornwell to bring the VLBA into operation by this time. Tim was anxious to start doing some routine correlation, and wanted to freeze the system as it was and just do whatever was possible. I argued against this, because the space of usable modes and capabilities resembled a multi-dimensional Swiss cheese at the time, and it would have been nearly impossible to comprehend, let alone describe, "whatever was possible." Eventually, with some further software development, we were able to reach a state with a coherent and relatively simply explained set of capabilities, and we did freeze the system in that state for some time, to allow for software developments aimed at making the system more "robust" (in the buzzword of that period) and maintainable.

At this point, I felt we were ready for the final milestone I want to mention, which we called "first science": being able to correlate an entire observation reliably and reproducibly, and to carry the results through to a correct image. The "reproducibly" part of this provided an obstacle for some time, since testing designed to demonstrate reproducibility soon showed the opposite: the observation we were using was afflicted by a number of dead tracks on various stations' recorders, which interacted confusingly with a similar set of dead tracks on the playback drives. Apparently none of these had been detected in any previous tests. We got the playback drives fixed, worked out how to live with the bad recordings, and produced our "first science" distribution tape.

This observation was a test on "my favorite source," 3C84. After the initial work using spectral line sources, I had wanted to observe a good continuum for the next test. I asked Barry if anyone else had a claim on 3C84, just in case we discovered something interesting. He approved the test observation of this source. I didn't have time to work on the imaging, and asked Craig Walker to take it on. He did a great job on it, producing a high dynamic range image despite the various recording, instrumental, and weather problems. And his image contained a new feature, a counterjet just below the 50:1 limit I had established in my work while at the MPIfR. So, we obtained new science in the VLBA's "first science" observation. Well, almost; we didn't trust this brand-new system enough to believe the new feature, and first presented the image with it cropped out. It wasn't until we learned that the same feature had been discovered at a different frequency by a group at Caltech that we really believed it. These two results were eventually published back-to-back in the *Astrophysical Journal*.

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Section V: Impact of the VLBA

Opening Remarks by Session Chair: Recollections of Encounters with Barry Clark

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I met Barry Clark for the first time when I arrived at Caltech in the Fall of 1959 as a new graduate student. Barry had been a Caltech undergraduate and was already a legend around the astronomy department for his quiet but forceful personality. I suppose my first real encounter with Barry occurred the following winter when during a stay at the Owens Valley Observatory, Barry took me skiing at Mammoth. My previous skiing experience was confined to a hill in my parents' back yard. Having grown up on Long Island where the highest "mountain" was about 150 feet high, I found the Sierras somewhat intimidating. We bought lift tickets and rode several lifts to the top of the mountain. I was pretty scared but told myself that my friend from Texas would take care of me, and when we finally reached the top, I meekly asked, "which way?"

"Down!" grunts Barry, and he takes off, leaving me alone to work my way down.

Somehow I made it down, and a few months later, after I had acquired some modest proficiency at the sport, we returned to Mammoth for a day of skiing. On the way home, I proudly remarked to Barry that I had made it through the whole day without falling. To which Barry replied that obviously I wasn't trying hard enough things.

Barry has a reputation for being infallible and unsurpassed at any intellectual activity. This isn't really true. We spent many long periods together at OVRO working on our thesis projects. Often after dinner we would play chess and I frequently was able to beat him; not all the time, perhaps not even most of the time, but a significant number of times. And, no he didn't start with a reduced number of pieces. We played with the same number of pieces. The only "small" advantage that I had, was that I looked at the board, while Barry would be off in some other part of the room working on a physics problem or something and he would call out his moves to me. I remember once, I had him down to a King and a pawn while I still had a board full of men. I clearly had him worried, but on this occasion he decided not to resign and he came over to look at the board. After a few moves, he had maneuvered me into a corner surrounded by my own men, and checkmated me with his pawn.

A few months ago, Jim Moran asked me if I knew the circumstances surrounding Barry's invention of the three-level digital lobe rotator. Well, actually, credit for this goes to Marshall Cohen and myself. Barry just implemented the details. What happened was that we were doing a VLBI run between OVRO and the 140-ft at 18 cm. I was at OVRO working with Marshall. This was in the exhausting MK I days when it took real perseverence to do VLBI as we had to change tapes every 7 or 8 minutes and at the same time adjust the synthesizer setting in the L.O. to slow the fringes down. I think it was a 48-hour run, so we took turns working and sleeping. Eight hours on, eight hours off. At each change of shift, Marshall or I would report how things had been running, missed tapes, bad weather, etc., and also transmit a report from Barry about how things were going in Green Bank. After a few of these shift changes, we realized that it was always Barry reporting from Green Bank. He apparently was running the whole thing himself while Marshall and I were taking turns sleeping and eating. On the next to last tape, one of us (I don't remember which), noticed that we had been entering offset frequencies incorrectly into the synthesizer. Instead of entering the natural fringe rate of 1 KHz to slow the fringes down to zero, we were pushing the wrong buttons, and entering 10 KHz, and so speeding the fringes up to 9 KHz. We were devastated. Apparently the whole run had been ruined. Knowing that Barry had stayed awake the full 48 hours, neither of us had the courage to tell Barry what had happened. We rationalized that he had probably gone to sleep and it would be in poor taste to awaken him just to give him the bad news.

The drive back to Pasadena was solemn, but Marshall and I speculated that perhaps the data could be recovered, by doing the lobe rotation digitally, that is to just multiply the data stream on one tape by a square wave of appropriate frequency. We had no idea if this could really work, and if it did, how to actually implement it. The next day we called Barry and broke the news that we had messed up the recordings and mumbled something about maybe he could fix it up in the computer. He grunted and indicated that he had been thinking about doing this anyway.

Probably the biggest ego deflator I ever received from Barry came just a few years ago when we were talking about the start of our VLBI program in Green Bank. I recalled that Marshall and I had speculated over a pitcher or two of beer at the 1965 AAS meeting in Ann Arbor, Mich., about the possibility of building an independent oscillator-tape-recording-interferometer. We agreed that commercial tape recorders and atomic oscillators which had recently appeared on the commercial market might allow unlimited interferometer baselines. Upon returning to Green Bank, I went to see Dave Heeschen, then the NRAO director, to see about financing. Dave asked how much it would cost, and I replied, "about $100K." I remember Dave looking in a notebook and frowning that this would not be possible, but he asked if $50K would be enough until the end of the year with the rest coming in January! Imagine today getting the equivalent of about half a million dollars with a five minute discussion. No proposals, no committees, no endless reviews.

Of course we had no idea how to actually build an independent-oscillator-tape-recording interferometer. I recall accosting Barry in the hall in Green Bank and seeking his help. He mumbled something about, "It might be possible." Well, you know the rest of the story. Barry went on to design and build a lot of the equipment, as well as write all the software while Marshall and I, joined by Dave Jauncey, stood around and cheered. For decades afterward, I took some silent satisfaction, that my contribution to the development of VLBI was getting Barry involved. But, just a few years ago, I remarked to Barry that this was one of the few occasions he had ever been influenced by anything I had said. Barry looked at me and smiling he said, "Well, actually I had been thinking about it before that!"

Advances in Astrometry and Geophysics Made Possible by Radio Interferometry

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I. Introduction

One of the oldest problems in astronomy and life is "Where am I?" The fields of astrometry and geodesy come together to answer this question using the positions of solar system objects and stars, and in the last 300 years, clocks. Navigators using sextants accurate to an arcminute determine positions within a nautical mile on the Earth's surface. Careful measurements in the mid-20th Century pushed star position accuracies to 0.1 arcsecond, resulting in formal accuracies in terrestrial position of a few meters using astrolabes and transit circles. With the advent of space science in the 1960s, measurement of the Earth's gravitational potential, the Earth-Moon distance, etc., were made. New methods arose to meet the challenge to provide an increase in accuracy. Two methods proved to be particularly useful, laser ranging to the Moon and artificial Earth satellites and Very Long Baseline Interferometry (VLBI). This paper deals with the advances made by VLBI.

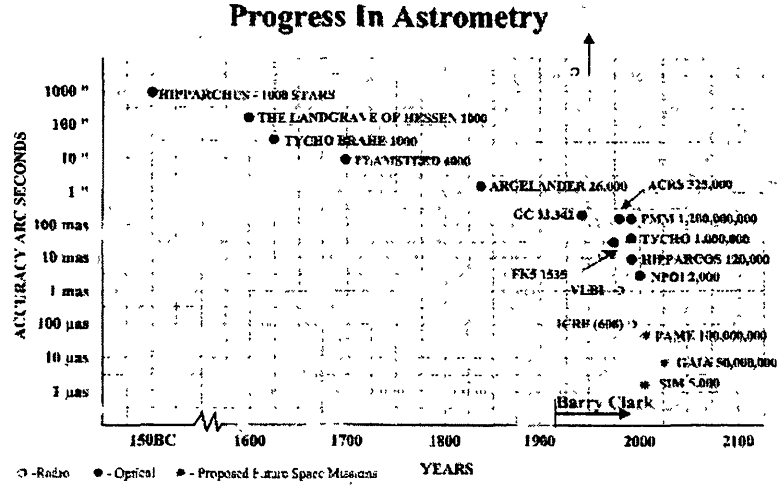

Figure 1. Progress in Astrometry.

Radio Interferometry has had a tremendous impact on the development of astrometry and geodesy. Barry Clark has played a leading role in developing the technology of radio interferometry through his work on connected-element interferometry, i.e., the Green Bank Interferometer and Very Large Array; and VLBI, i.e., the Mark I and II VLBI systems and the Very Long Baseline Array (VLBA). These efforts resulted in an increase in astrometric accuracy of four orders of magnitude. Stellar positions at radio wavelengths now can be measured with a precision of 0.1 milliarcseconds (mas). Earth orientation parameters such as UT1 and polar motion are now measured with precision's of 10 microarcseconds (µas). The precession constants and models for nutation need revision to precisions at the µas level. Tectonic plate motion was directly measured in the 1980s. These were and are exciting times for research in these fields and Barry's scientific career parallels them especially in astrometry, as shown in Figure 1.

II. Background

The astronomical reference system as taught in all elementary astronomy courses is the celestial system of right ascension and declination. This system was defined by a reference frame consisting of optically bright stars. It is only in the last seventy-five years that serious observations have been made at other than optical wavelengths. Radio observations matured quickly after World War II, and the entire electromagnetic spectrum was opened up with the beginning of the space age in the 1960s. If possible, a celestial reference frame should be based on objects that are fixed in space. A star's position in space is described by a position at a defined epoch, parallax, proper motion and radial velocity. The level of accuracy achieved with optical transit circles is about 0.1 arcsecond. A long series of observations will improve knowledge of the star's proper motion. Proper motions for bright stars were accurate at the level of about 3 mas before the Hipparcos spacecraft. All observations were made from the Earth's surface. In order to transform from an apparent position to a mean position and then to another epoch, knowledge of the Earth's precession, nutation, figure, etc., are needed. Separation of precession and nutation from the proper motions of stars proved difficult at levels below 1"/ century.

III. The Optical Reference System and Frame

The increase in astrometric accuracy achieved by radio interferometry has resulted in a new celestial reference system and frame. The previous system was defined by a frame made up of the optical positions of bright stars. Fundamental catalogs were compiled from catalogs of "absolute or fundamental" observations, i.e., the positions were on an instrumental system, the pole was determined independently and the zero point of right ascension was adjusted to a dynamical system using observations of the Sun and solar system objects. These fundamental catalogs, denoted as FK 3 (Kopff 1937), FK4 (Fricke & Kopff 1963) and FK5 (Fricke, Schwann & Lederle 1988), compiled by the Astronominischen Rechen Institut, first in Berlin and later in Heidelberg, used the positions of 925, and later 1535 stars in their initial catalogs to define the reference frame. These were well-studied stars, brighter then 8th visual magnitude. The precision of the FK5 catalog's positions and proper motions at average mean epoch 1950.0 was believed to be 20 mas and 0.8 mas/yr respectively. On later comparison of the FK5 frame with the Hipparcos frame at epoch 1991.25, regional distortions as large as 150 mas were found (Mignard & Froeschle, 1997).

The optical reference system defined by the Fundamental Catalogs has the following structures: an origin (barycenter of the solar system), a fundamental plane (celestial equator), and a zero point of the fundamental plane (vernal equinox, intersection of the mean equator and ecliptic). In defining this reference system with stars, their positions establish the reference frame defining the realization of the axes of the reference system. The reference system must have a reference epoch and specify all the necessary procedures and constants required to transform the frame from the reference epoch to any other date.

In the 1980s the FK5 system came into being with J2000 as the epoch. The FK5 system was improved over that of the FK4 by adopting the IAU 1976 value of precession (Lieske et al. 1977) and nutation (Seidelmann 1982), a new determination of the equinox and equator (Fricke 1982), a precessional correction determined from FK4 proper motion assuming a kinetic model of parallactic motion and galactic rotation (Fricke 1981) and a new definition of time (Aoki et al. 1982).

IV. The Radio Reference Frame

At the beginning of Barry's astronomical career in the 1960s, positions determined by radio observations were accurate to about an arcsecond, and then quasars were discovered. Quasars, since they are extragalactic, were soon recognized as objects whose positions could be employed in a reference frame. Since the spatial structure of their emission was not known, VLBI was developed to answer the astrophysical questions about the energy mechanisms for these objects. Barry, together with other NRAO scientists and their world-wide collaborators, performed experiments in the late 1960s and found the spatial structure to be very compact, of order one mas. The next step, and the topic of this paper, is the astrometry of these objects. This quickly improved with the "long" baseline connected-element interferometers such as the Cambridge Array and the Green Bank Interferometer. In the 1970s the positions of compact extragalactic radio source were determined to 20 mas (Wade & Johnston 1977) with the 35 km baseline of the Green Bank Interferometer. This work paved the way for more precise geodetic measurements. The Navy began operation of the Green Bank Interferometer in 1978 for the determination of Earth orientation parameters. At the same time, VLBI was coming of age with astrometric positional accuracy that equaled that of the Green Bank Interferometer (Clark et al. 1976).

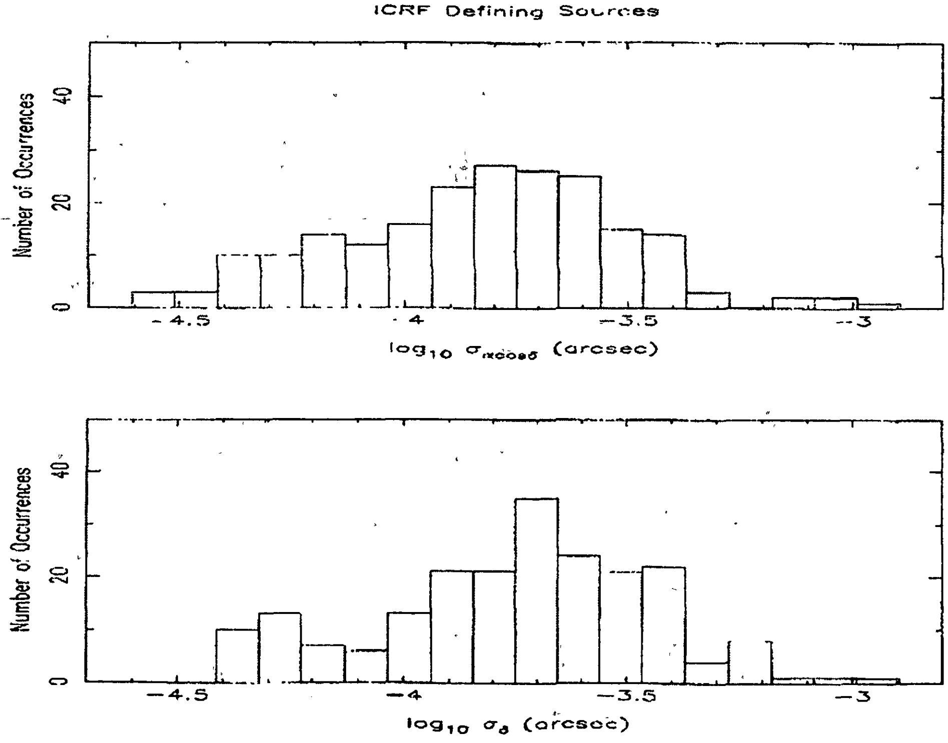

Figure 2. Formal errors in he positions of the 212 sources defining the ICRF. The mean position is below 100 μas.

In the 1980s, VLBI made major steps in its development through NASA's Crustal Dynamics Project. Various catalogs of radio source positions were published by Goddard Space Flight Center (GSFC), the Jet Propulsion Laboratory (JPL), the National Geodetic Survey (NGS), and the Naval Research Laboratory/United States Naval Observatory (NRL/USNO). These catalogs were combined by the International Earth Rotation Service (IERS) beginning in the late 1980s to form the basis for a radio reference frame. A program to establish a global radio reference frame of 400 sources was undertaken in 1986 (Johnston et al. 1988). All sources would have optical counterparts. All available VLBI dual frequency bandwidth synthesis Mark III VLBI data from geodetic, Earth orientation and astrometric programs obtained between 1979 and 1993 were used to solve for a catalog of positions from first principles in a single solution (Johnston et al. 1995). The dataset consisted of 1,015,292 pairs of group delay and phase delay rate observations. A catalog of 436 sources, all with accuracies smaller than 3 mas in each coordinate and 211 with accuracies smaller than 1 mas, was produced to define the radio/optical reference frame at a frequency of 8.4 GHz.

V. The ICRS and ICRF

The IAU at the XXI General Assembly in 1991 decided that the celestial reference system should be based on a set of distant extragalactic objects. A list of suitable candidate sources to define this reference frame was adopted at the XXII General Assembly in 1994. At the XXIII General Assembly in 1997, the IAU adopted a new reference system based on a reference frame defined by extragalactic radio sources. This system, adopted on January 1, 1998, is known as the International Celestial Reference System (ICRS) and is specified in the 1991 IAU resolutions. The origin is located at the solar system barycenter via modeling of VLBI observations in the framework of General Relativity. The motion of the pole is defined by the conventional IAU models for precession (Lieske et al. 1977) and nutation (Seidelmann 1982) and the origin of right ascension is defined by fixing the right ascension of 3C273's (Hazard et al. 1971) FK5 value transferred to J2000.0.

An IAU Working Group on Reference Frames produced the catalog (Ma et al. 1998) defining the International Celestial Reference Frame (ICRF) from 1.6 million pairs of group and phase delay rate data obtained between August 1979 and July 1995. The ICRF consists of 212 defining extragalactic radio sources. These sources have positional errors less than 1 mas. Figure 2 displays the formal errors of these positions. The reported positions in the catalog were derived by multiplying the formal

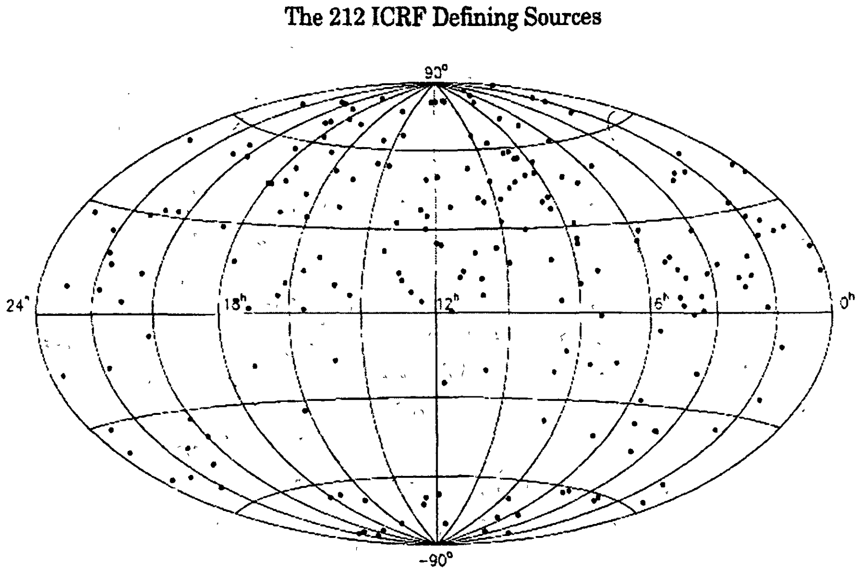

Figure 3. The location of 212 sources defining the International Celestial Reference Frame. Note that the majority are in the Northern Hemisphere.

errors by 1.5 and adding this in quadrature with 250 mas. Formal errors are typically < 100 μas which is the precision of these positions. Typically those sources with the lowest errors have been observed over several years. The geodetic experiments contributed most of the observations of these sources.

Figure 3 displays the distribution of these sources on an Aitoff equal area projection. To establish a defining source a long series of observations are necessary. Only 22 percent of the defining sources are in the southern hemisphere due to a lack of observations. An additional 294 candidate sources that may become defining sources with future observation also are given in the catalog. There are also 102 "other" sources whose positions may show variations with time or whose positions are less well known. The visual magnitudes of the defining sources are presented in Figure 4. This is seen to peak between 18th and 19th visual magnitude.

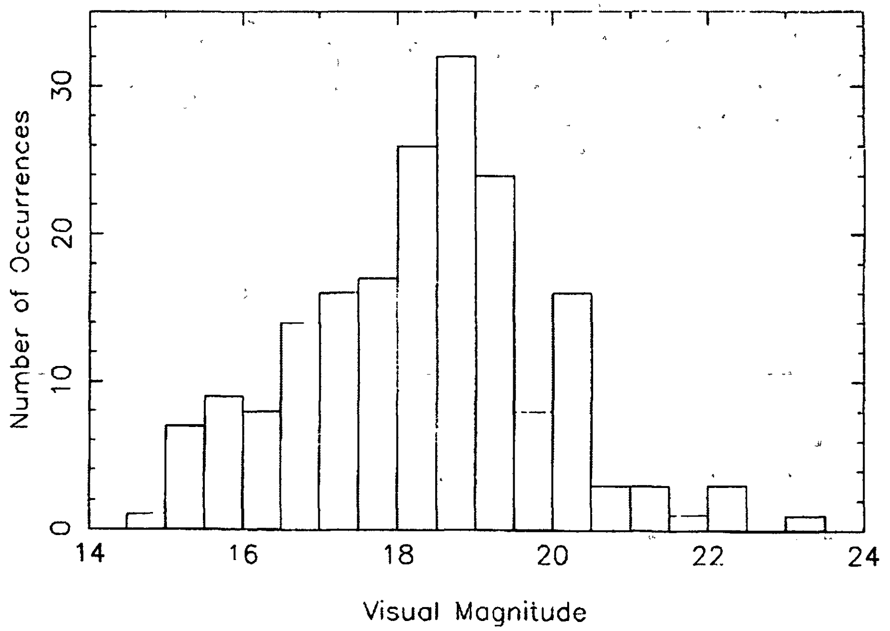

Figure 4. The visual magnitude of the optical counterparts of the ICRF sources. Note that they are much fainter than the Hipparcos stars and peak between 18th and 19th visual magnitude. Thus, bright stars with radio counterparts were used to link the radio ICRF and optical Hipparcos frames.

The geodetic data, which, by far, forms the majority of data in the data base used to generate the ICRF, was obtained over a limited number of hour angles, usually with a small number of antennas, and is limited in its ability to produce a map of the radio emission from these sources. Observations with the VLBA of these sources may be found on Radio Reference Frame Image Data Base (RRFID) maintained by Alan Fey (http://maia.usno.navy.mil/rorf/rrfid.html). This website, as well as Fey & Charlot (1997), also details the effect of source structure on the precise positions of these defining sources. A source index is given to estimate the astrometric quality of the sources. This is presented in Figure 5. An index of 1 is very good, 2 is good, 3 marginal and 4 not usable. The structure of the radio emission at 8.4 GHz is also shown in Figure 5.

Future observations with the VLBA and other VLBI arrays, especially those in the southern hemisphere, are needed to maintain and extend the ICRF. Variations in source structure will cause defining sources to be downgraded to candidate sources and candidate sources to become

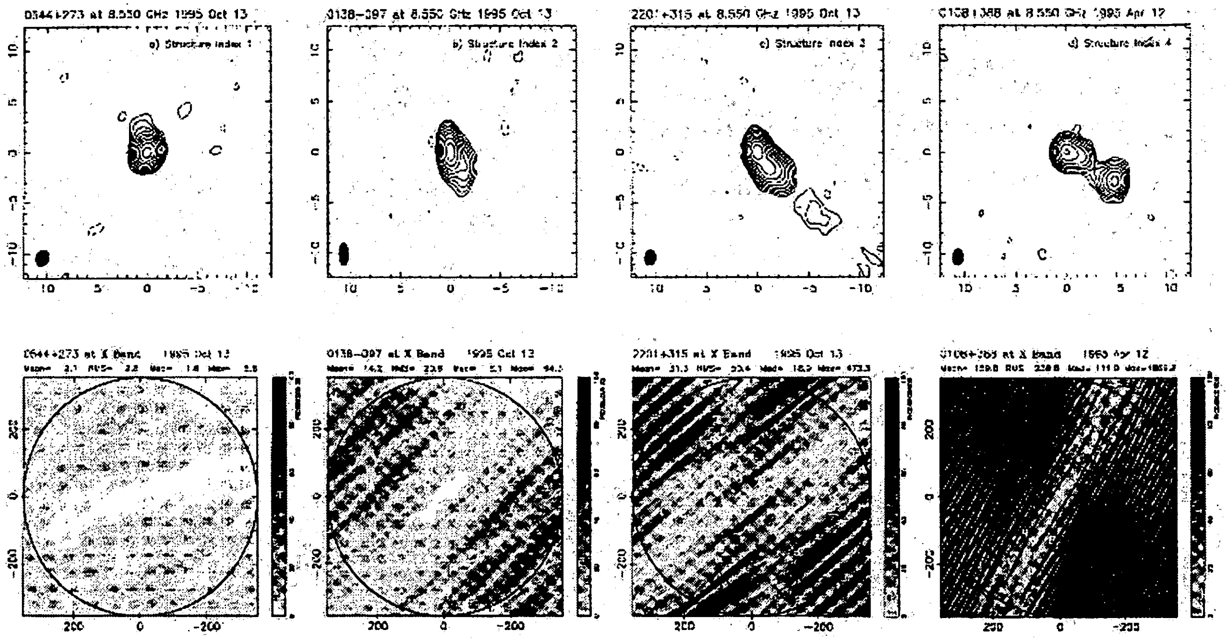

Figure 5. The source index at 8.4 GHz for the quality of extragalactic radio sources for astrometric applications, especially applied to sources for the ICRF. Note that sources with an index of 1 (left) are excellent, while those with an index of 4 (right) are unusable. This can be seen easily by a study of their spatial structure (top) and delay (bottom).

defining sources. The ICRF should be looked upon as evolving with time. The ICRF will have an epoch with the defining sources and their positions being defined at that epoch. This is necessary, as the variations in spatial structure will cause variations in position. Since the spatial emission is often in the form of "jets," these sources may display apparent proper motions. A large number of observations will be required to maintain the ICRF. At present, the accuracy of the ICRF is also determined by our lack of knowledge of systematic errors in models as well as errors contributed by the atmosphere. It should be possible to improve our understanding of nutation, the atmosphere, and other error contributing effects, resulting in a positional accuracy of less than 50 μas. Again a large number of observations are needed to achieve this. The VLBA can make a very significant contribution in achieving this.

VI. The Hipparcos Space Mission

The major limitation to optical astrometry from the Earth's surface is the errors introduced by the atmosphere. To overcome this, the Hipparcos space mission produced a global astrometric catalog of 118,217 stars. The optical magnitudes of these stars are brighter than 10th visual magnitude. The positions have typical precision in the five astrometric parameters of 1.5 mas for the majority of stars. Lack of precise knowledge of the stars' proper motions will make this catalog degrade in position from epoch 1991.25. This catalog was linked to the ICRF via objects that displayed radio and optical emission. The most precise measurements were obtained of bright Hipparcos stars, which displayed radio emission (Kovalevsky et al. 1997). This uncertainty in the link is 0.6 mas and 0.25 mas/yr in position and time dependent rotational proper motion parameters. Future VLBA measurements are needed to maintain the accuracy of this link. The Hipparcos catalog is the realization of the optical reference frame of the ICRF.

VII. Implications for Astrophysics

The position of radio and optical emission of celestial objects and the "ICRS" can now be determined to 0.25 and 5 mas respectively at epoch 2000. To accomplish this the "ICRS" must be modified using the IERS Conventions (McCarthy 1996). This can be accomplished by offsets from ICRF or Hipparcos objects. It is difficult to accomplish since the density of ICRF sources is not very great and the error in measurement is directly proportional to the separation of the object from the reference source. At optical wavelengths this is also true as most telescopes have a field of view less than a degree. The Astrographic Catalog Tycho (ACT), a catalog of 10^6 stars, helps this situation substantially but the accuracy of the positions in this catalog is about 40 mas at epoch 2000. Many astrophysical questions may be answered. A sample follows. At radio wavelengths, the parallax of the radio source at the galactic center may be measured directly; its emission at optical/IR wavelengths may be measured using the correlation in position of stars and the optical/IR emission of star forming regions with maser emission; the geometry of the shells of late type stars may also be studied via their maser emission; and stellar radio emission may be identified with particular stars in binary systems. At optical wavelengths, the scale of the universe can be determined to better than 10% via the parallax of nearby standard candle RR Lyraes and Cepheids, galactic kinematics via the study of nearby stars and the evolutionary sequences of stars may be studied in detail.

VIII. Geodesy

VLBI determines the orientation and length of the baseline separating the antennas. Using three appropriately separated antennas such as Green Bank, W Va., Kokee Park, Hawaii, and Fairbanks, Alaska, UT1 and polar motion can be determined precisely, as well as changes in the baseline providing the ability to measure continental drift, given the astrometric accuracy reported for the ICRF. NASA's Crustal Dynamics Project had, as one of its objectives, to develop contemporary descriptions of tectonic motion via space-based geodetic measurements. Satellite Laser Ranging (SLR) and VLBI were developed to provide the data necessary for this. In the case of VLBI, the data are reported as rate of change of baseline length and interpreted as velocities for the stations. Station velocities have been measured with a precision of a mm/yr between the best stations (Westford-Wettzell), with positive detection of motions of a few mm to cm/yr between the North American plate and the Eurasian plate, the Pacific plate, the Australian plate and the African plate (Ryan et al. 1993). This confirmed the NUVEL-1 geologic plate motion model and showed that plate motion is a continuous contemporary process.

The orientation of the Earth as measured by VLBI has led to substantial advances in understanding a number of geophysical phenomena. VLBI measurements of Length of Day (LOD) evolved from a few mas in 1970 to a few tenths of a mas in the early 1990s. Rotational variations have been linked to fluid dynamical processes in the atmosphere, and the LOD has been shown to correlate with changes in the atmospheric momentum. The period of free core nutation has been established and has determined that the Earth's core is not in hydrostatic equilibrium. The models for precession and nutation have been greatly improved. In the future, rotational variations will be used to determine dynamical processes in the atmosphere, oceans and core and also exchanges in water mass between the atmosphere, oceans and the "solid Earth" (crust and mantle).

Today GPS is evolving to measure precisely geodetic phenomena. The orbits of the satellites now are determined at the 5 cm level by the International GPS Service (IGS). For example, the value of polar motion is based now almost entirely on GPS data. VLBI observations are needed for highly precise measurements on global scales and to validate the GPS measurements.

IX. Summary

In the past thirty years, the technology of radio interferometry has contributed greatly to advances in astrometry and geodesy. Barry has played a leading role in its development and deserves a great deal of thanks for his efforts. The VLA and VLBA will take a leading role in the further development of these fields if a substantial amount of observing time is granted to develop further our understanding of the evolution of the structure of the radio emission in compact extragalactic radio sources as well as obtaining a more complete understanding of the contribution of the atmosphere to errors of astrometric measurements.

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HIGHLIGHTS OF GALACTIC RESEARCH WITH THE VLBA

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1. Introduction

In the last few years, the VLBA has been used to image a wide variety of Galactic sources. Considerable progress has been made in the study of SgrA*, radio emission from stars, stellar and interstellar masers, and the nature of the interstellar medium. This paper briefly highlights some of these new results.

2. SgrA*

The nature of SgrA*, the candidate massive black hole in the Galactic Center, has been a subject of considerable interest since its discovery by Balick and Brown (1974) using the Green Bank interferometer. Multi-wavelength observations with various connected element, radio linked, and VLBI interferometers clearly indicated that the cm-wavelength emission from SgrA* was heavily scattered by free electrons in the interstellar medium. The apparent angular size followed closely a λ^2 dependence, and surprisingly the scattered image was elongated in the East-West direction.

In order to peer through the scattering screen, shorter wavelengths were needed, and VLBA operation at 7-mm wavelength was eagerly anticipated. Several groups have now reported results at this wavelength. Krichbaum et al. (1993) first claimed to see intrinsic (non-scattered) structure in SgrA* in the form of a second, jet-like, component oriented toward the Northwest of the core. Bower & Backer (1998) showed that the the core-jet model did not fit their data, leaving the possibility of any jet-like component in serious doubt. Recently Lo et al. (1998) point out that the North-South extent of the SgrA* image at 7-mm wavelength exceeds that expected by extrapolation from longer wavelengths and hence that some intrinsic structure has now been detected. If true, the emission in this direction is only about 70 Schwarzschild radii in extent!

There now is compelling evidence that SgrA* is a massive black hole. The radio position of SgrA* has been transfered to 2-μm infrared images to an accuracy of 30 mas (Menten et al. 1997), and proper motions of stars within about 0.01 pc of SgrA*'s position (Genzel et al. 1997; Ghez et al. 1998) indicate an enclosed mass of 2.6×10^6 $M_\odot$. Reid et al. (1998) have initiated a VLBA program to determine a trigonometric parallax to SgrA*. Initial observations of SgrA*, phased referenced to extragalactic sources, now clearly show an apparent motion of 6 mas y^{-1} along the galactic plane. This apparent motion can be entirely accounted for by the orbit of the Sun around the Galactic Center. While it takes the Sun over 200 million years to complete one such orbit,one can measure its motion in less than one month with the VLBA! After accounting for the effects of the Solar orbit, any residual peculiar motion of SgrA* is less than about 20 km s^{-1}. This rules out a stellar-like mass for SgrA*, indicates that SgrA* is at the dynamical center of the Galaxy, and adds considerable support to the case for a massive black hole.

3. Radio Stars

VLBA observations of active stellar systems have revealed many interesting aspects of their radio emissions. Beasley and Bastian (1998) resolved emission from the RS CVn binary UX Ari and, through phase-referencing techniques, located the emission in strong flares as originating between the two stars. In contrast, Benz et al., (1998) reported that both of the dMe stars in the UV Ceti system exhibit radio emission.

Algol, one of the nearest stars with strong radio emission, has been studied for decades with VLBI techniques. However, accurate astrometry via phase-referencing on the VLBA (Mutel et al. 1998), has led Molnar & Mutel (1998) to estimate how close this star came to the Sun some 7 million years ago. Previous estimates indicated that Algol came within 3 pc of the Sun, nearly close enough to perturb Oort Cloud comets toward the inner solar system. However, the recent VLBA data reveals that Algol's closest approach was significantly more distant from the Sun and unlikely to have caused a cataclysmic cometary impact.

X-ray binaries contain a compact object, such as a neutron star or black hole, and display radio emission on a variety of time and spatial scales. Well known sources such as SS 433 and Cyg X3 have been studied for years with VLBI arrays. While the twin jet-like structure of SS433 is well established, only recently have VLBA observations shown that Cyg X3 contains a long, curved, one-sided jet (Mioduszewski et al. 1998) and unusual expansion characteristics (Newell et al. 1998). The recent discoveries of superluminal sources such as GRS1915+105 and CI Cam have provided fascinating targets for VLBA observations (e.g., Dhawan et al. 1998, Hjellming et al. 1998). Motions near the speed of light for sources at kpc distances produce very large angular motions, since, for example, $v = 0.5c$ and $D = 10$ kpc, $\delta\Theta/\delta t \approx 10$ mas per day. Thus, imaging with beam sizes near 1 mas require "snap-shots" or complex model-fitting procedures, and progress will require larger VLBI arrays or more sophisticated algorithms than currently in use. The VLBA is uniquely capable of rapidly responding to source outbursts and will undoubtedly make major contributions to the understanding of these unusual systems.

4. Stellar Masers

Stellar masers, found in the circumstellar envelopes of red giant and supergiant stars, always have been difficult targets for radio interferometers. The masers often have uncomfortably large angular sizes (for VLBI observations) or require observations at high frequencies. VLBA baselines in the sub-1000 km range and the excellent performance at 22 and especially at 43 GHz have revolutionized the study of stellar masers. The synoptic observations of SiO masers in the stars TX Cam, R Aqr, and U Her (Diamond & Kemball 1998, Boboltz et al. 1998, Marvel et al. 1998) have revealed beautiful rings of masers, evolving in a a complex manner, near the dust formation radius. Polarization observations indicate a largely coherent stellar magnetic field (Kemball & Diamond 1997). Continued, well-sampled, observations over the entire light cycle of such stars will provide the first complete "movie" of the turbulent inner circumstellar envelope of TX Cam.

5. Interstellar Masers

Interstellar masers are found in regions of active star formation and often trace the earliest stages in the birth of a star. VLBI observations of these masers allow us to probe the kinematics and physical conditions, including magnetic fields, with unprecedented resolution. The OH and CH_3OH masers surrounding cometary ultracompact HII regions have been studied with the VLBA (Baudry & Diamond 1998, Ellingsen et al. 1998). Groups of maser spots within such sources are now known to exhibit quasi-linear filaments, some with continuous velocity gradients along the structures. The origin of these structures is not certain, but theories involving shocks and dense disks have been proposed to explain them.

Water vapor masers in star forming regions are perhaps even more enigmatic than OH and CH_3OH masers associated with ultra-compact HII regions. Water masers probably trace protostellar outflows from sources so deeply embedded that they are obscured at near-IR and even mid-IR wavelengths. Toward solar-mass young stellar objects, Claussen et al. (1998), Furuya et al. (1999), and Patel et al. (1999) have used the VLBA to measure the proper motions of water masers in the outflow to within about 10 AU of the embedded protostar. VLBA studies of the polarization of the H_2O masers in W51 by Liljestrom & Leppanen (1998) show a 100-AU scale "streamer" in addition to the previously known maser spots. Finally, SiO masers, normally associated with evolved stars, are found near the Orion IRc2 source in a very active star forming region. Observations by Schwartz et al. (1998) suggest that these SiO masers trace out the walls of two oppositely directed cones which form the base of a bipolar outflow.

6. The Interstellar Medium

The interstellar medium can be "imaged" with VLBI techniques through absorption and scattering of high brightness sources. Early VLBI studies (e.g., Dieter, Welch & Romney 1976) established that HI seen in absorption against the extragalactic source 3C147 had significant structure on size scales at least as small as 60 AU. Recent VLBA observations by Faison et al. (1998) toward three other sources suggests that small scale HI ($\sim$ 10 AU) is common, can be imaged directly, and that there may be an inner scale length ($\sim$ 1 AU) where fluctuations diminish.

The interstellar medium near the Galactic Center is unusually dense and known to significantly scatter-broaden sources. The anisotropic scattering of SgrA* has been well documented (see Section 2). Recently VLA and VLBA observations of many OH masers have extended the study of the extreme scattering near the Galactic Center and partially "mapped" the distribution of scattering material (Van Langevelde et al. 1992). Looking in the opposite direction, toward the Galactic Anti-center, Lazio & Cordes (1998) have used the VLBA to constrain the radial extent of the Galaxy's ionized material. VLA and VLBA observations of other sources (NGC6334B, OH masers in W49N, extragalactic sources seen through the Cygnus superbubble) have also found significant anisotropic scattering (Trotter etal 1998, Desai, Gwinn, and Diamond 1994, Desai and Fey, ApJ submitted). Taken with the few previously reported instances of possibly anisotropic scattering (2013+070

(Spangler and Cordes 1988) and Sgr A* (see Section 2)), observations using the VLBA have established the ubiquity of anisotropic scattering in the interstellar medium.

Finally, Claussen et al. (1999) have used the VLBA to measure the sizes of 1720 MHz OH masers arising in shocked gas toward three supernova remnants. The sizes are quite large (~100 milliarcseconds), and so a scattering interpretation is likely. However, it is possible that the intrinsic sizes of this new class of masers is much larger than other interstellar masers.

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VLBA Extragalactic Research

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1. Introduction

I have been working on VLBI for just over twenty years, and although that sometimes seems like a very long time (at least to someone still some distance from his sixtieth birthday), it is slightly shorter than the history of the VLBA. The first Design Study for the VLBA – then called an Intercontinental VLBA – was produced by Ken Kellermann in 1977 (Kellermann 1977). I thought it would be interesting to look back over those twenty years to that design study, and the two later ones, and see how far we have come.

Figure 1, taken from the design study, is a contour map of the nucleus of NGC 1275 at 2.8 cm, and shows the state of the art in 1977. Note that this was before the first application of closure phases, so there was a 180° ambiguity in orientation, and I believe that the map is in fact upside-down. (And this is definitely a "map," not an "image"!). Comparison with the VLBA image of NGC 1275 at 1.3 cm (Figure 2; see also Dhawan, Kellermann, & Romney 1998) gives a dramatic demonstration of "the impact of the VLBA" (the title of this session).

2. Active Galactic Nuclei

To quote from the second NRAO design study (NRAO 1981): "One of the most fundamental problems in extragalactic astronomy is to understand the source of energy in quasars and galactic nuclei. The VLB Array will give a dramatic new insight into the central regions of galaxies and quasars where the remarkable features observed with the VLA are focused and collimated." In spite of the long gestation period of the VLBA, that is still true today, although now we have a much better defined model of a galactic nucleus that we can try to test by VLBA observations. This model – developed over the years with essential input from early VLBI observations – consists of several components, for which we have a more-or-less well developed theories: (1) A black hole, that powers the central engine; (2) An accretion disk, providing fuel; (3) An acceleration mechanism that creates and collimates two opposed jets; (4) The jets themselves; (5) The lobes and hot spots formed by interaction of the jets with the interstellar or intergalactic medium; (6) An "obscuring torus," introduced to satisfy the "unified theorists" who require that many galaxies harbor a hidden quasar-like nucleus. The VLBA has made observations (and will make many more) that have helped to illuminate all these components. I do not have time to review all these observations today, and I will mention only a few of the highlights. I will say nothing more about the black hole – the strong evidence for a black hole in NGC 4258 found by Jim Moran and his colleagues (Miyoshi et al., 1995) – but I will talk briefly about jets and hot spots, and at somewhat greater length about the accretion disk and obscuring torus where the VLBA is producing the most exciting new results.

2.1. Jets

The VLBA is now routinely making images of the continuum emission from parsec-scale jets that show much detail, as shown for example in Craig Walker's 1.7-GHz image of the famous superluminal source 3C120. At the higher resolution obtained at 22 GHz (Gómez et al. 1998) yet more detail is seen. There are many bumps and wiggles which move measurably even over 40 days between these two images, and it is possible, albeit with some ambiguity, to distinguish separate "components" and to measure their apparent superluminal velocities.

In spite of much work over the last twenty years, we do not yet have a good theory for what we see in jets. We don't know how the shock velocities are related to the underlying flow speed, we don't understand the role of magnetic fields, we don't know what the jets are made of (electron-positron plasma or electron-proton plasma, or possibly both); we don't even know whether what we see reflects the whole jet or merely a part that is favorably beamed. There are ways of approaching these problems, but we don't yet have a good way of making quantitative comparisons between observations and theory (for a review, see Zensus 1997).

Further observations with the VLBA will undoubtedly help; in particular, polarization observations (as Dave Roberts will discuss in the next talk) are beginning to address some of these questions. Another new capability that the VLBA provides is virtually simultaneous observations at several frequencies. This is illustrated by Lobanov's observations of 3C345 (Lobanov 1998). Simultaneous observations allow one to make accurate spectral-index images; but Lobanov has gone a step further, and, by fitting a synchrotron self-absorption spectrum at each pixel, he has made a map of spectral turnover frequency (Figure 3). This shows structure in the jet and in the strongly self-absorbed core. I don't know what it means, but it shows one of the powerful tools that are now available for studying the physical conditions in these objects.

A longstanding question is the cause of the different morphologies of low-luminosity and high-luminosity radio galaxies. A well-studied low-luminosity (Fanaroff-Riley class I) galaxy is NGC 5128 (Centaurus A). This shows a counter-jet (Tingay et al. 1998): the VLBA is finding many counter-jets at a level of a few percent of the main jet (some are more symmetric, e.g., Hydra A, Taylor 1996). It also shows subluminal velocities, but whether these reflect a lower flow speed than, say, 3C345 is not clear. Yesterday, Alan Bridle suggested that in FR-I objects the jets start out relativistic but are slowed in passing through the galaxy. This hypothesis can be tested with the VLBA (e.g., Giovannini et al. 1998).

2.2. Lobes and Hot Spots

One of the early results of VLBI was that bright, compact radio sources fall into essentially two classes: (a) the bright, beamed cores of quasars and radio galaxies; and (b) small double sources that are not relativistically beamed. The latter class, called *compact doubles* by Mutel & Phillips, and later *compact symmetric objects* (e.g., Wilkinson et al. 1995; O'Dea 1998), are almost certainly young radio galaxies that are still only a few hundred parsecs in size (e.g., Perlman et al. 1996). Readhead et al. (1996a, b) and O'Dea & Baum (1997) have studied the evolution of CSOs and it appears that some of them could evolve into kiloparsec doubles of moderate luminosity. The VLBA permits us to study these early stages of radio source evolution in great detail, and it has been of great service in locating the cores in these objects, which usually are not very prominent. In a recent paper, Owsianik & Conway (1998; see also Owsianik, Conway, & Polatidis 1998) have measured the separation speed of the lobes of the CSO 0710+439, and find a speed of $0.24c$; this puts the age of this object at only 1400 years.

A remarkable result from the VLBA is the discovery of two-sided jets, and two-sided motion, in the CSO 1946+708 (Taylor & Vermeulen 1997). If we assume that the object is intrinsically symmmetric, the difference in apparent speed on the two sides is due to light travel-time effects and allows the flow speed and angle to the line of sight to be determined. This in turn also puts useful constraints on the Hubble constant (since the speed must be $< c$). There may well be more objects in which such an analysis is possible (e.g., Taylor, Wrobel, & Vermeulen 1998).

The VLBA also is useful in resolving fine structure in the hot spots and lobes of larger, more mature doubles, many of which have structure on scales too small for the (present) VLA; see, for example, the observations of 3C205 by Lonsdale & Barthel (1998).

2.3. Disks and the Obscuring Torus

A number of VLBA projects have applied its unique capabilities (in particular, its polarization and spectral-line capabilities) to the study of the environment of the active nucleus, and are providing new tests of the accretion disk paradigm.

An early, dramatic result from the VLBA was the discovery of counter-jet in NGC 1275 (Vermeulen, Readhead, & Backer 1994; Walker, Romney, & Benson 1994; see also Silver, Taylor, & Vermeulen 1998). The counter-jet is increasingly prominent at high frequencies, and this suggests that it is partially hidden by an obscuring screen with a transparency that rises with frequency. Free-free absorption in a disk, inclined so that it obscures the northern jet and not the southern, is the most likely explanation. This disk is probably related to the presumed obscuring torus, and to the fuel source that feeds the central accretion disk.

Since this first discovery, there have been several similar discoveries in other objects. An example is the Seyfert 1 galaxy Mrk 231, in which Carilli, Wrobel, & Ulvestad (1998) have detected 21 cm absorption in front of the nucleus, which is probably from the inner part of a molecular disk seen on larger scales.

Another example is Cygnus A. Conway (1998; see also Conway & Blanco 1995) has made HI absorption observations of the nucleus of Cygnus A. The remarkable point is that the counter-jet is more heavily obscured than the nucleus, again suggesting an inclined disk or torus of gas (Figure 4). The study of neutral hydrogen around active nuclei promises to be a rewarding application of the VLBA (e.g., Peck & Taylor 1998).

Another approach to the environmental conditions is via Faraday rotation measures. In a beautiful piece of work with the VLBA, Taylor (1998) has mapped the rotation measures over several quasars (see also Udomprasert et al. 1997). A large fraction have very high rotation measures in their cores, but not in their jets. This suggests that there is a magnetized, ionized medium closely associated with the central part of the object, perhaps the narrow-line region.

My final example is the Seyfert galaxy NGC 1068, which has an obscured nucleus. Gallimore, Baum, & O'Dea (1996, 1997) have directly observed a parsec-scale disk-like component. The brightness temperature and spectrum suggest that the emission is direct thermal free-free emission at about 10^7 K. Direct emission from an accretion disk has also been suggested for NGC 4151 (Ulvestad et al. 1998).

3. Surveys

Systematic surveys have always been a valuable complement to the detailed study of individual objects, and this was recognized by the designers of the VLBA. The following is a quotation from the Caltech design study (Cohen 1980): "A properly designed 10-station Array would enable us to determine the structure in objects as faint as 0.1 Jy. There are approximately 7,000 compact extragalactic objects in the northern sky brighter than this limit. Statistics of large numbers of objects are essential to our understanding of both their physical and their cosmological evolution. One of the most important applications of the Array would be to make systematic observations of objects down to different flux density limits." This remains true, but it is now possible, owing to the excellent phase stability of the VLBA, to consider much deeper surveys.

I know of four major surveys being carried out on the VLBA. These are, of course, not primary surveys like NVSS but imaging surveys of known radio sources: (1) The VLBA calibrator survey at 13 and 4 cm. (2) The USNO astrometric survey at 13 and 4 cm (Fey & Charlot 1997). (3) The Caltech-Jodrell survey at 6 cm (Pearson et al. 1998). (4) The 2 cm survey of Kellermann et al. (1998).

These surveys are a valuable resource, but they will require a lot of work to extract useful information. Some statistical data are being obtained already, such as brightness temperatures, expansion speeds, and angular sizes (e.g., Guerra & Daly 1997). But a lot more information is probably concealed in the data, and we need to frame model-dependent hypotheses that can be tested by comparison with this body of data. There is a good project there for a student, or several students.

Another question we need to ask is how long we should continue the surveys: the sources keep changing, and we need to establish good statistical data on the velocities and spectral evolution. But it is probably not productive to image all 7000 (or more) sources every 6 months!

4. The Unexpected

A final quotation from the original design study: "Particularly in the case of an Intercontinental Array, with its great flexibility and high resolution image forming capability, it is difficult to anticipate the research topics which will be important in five to ten or more years." This is a truth universally acknowledged, but it is beautifully illustrated by the VLBA observations of the radio after-glows of gamma-ray bursts: something that I am sure the designers of the VLBA never anticipated (Frail et al. 1997; Taylor et al. 1997). An 8.4-GHz VLBA image obtained 8 days after the gamma-ray burst of 8 May 1997 showed an unresolved, 600 microJy source. This is a triumph of rapid scheduling and correlation, and of VLBA phase referencing. The image places a useful upper limit of 1 mas on this gamma-ray burst fireball, which is believed to be at a redshift of 0.835. A nearer burst might be resolved by the VLBA. The VLBA was also used along with the VLA to measure the light-curve of the afterglow, which shows a remarkable result: initially the fireball is very small, and shows strong scintillation which diminishes as the source expands through a critical size, believed to be about 3 microarcsec, or 10^{17} cm.

5. Conclusion

In the time available, I have only been able to present a very superficial view of a small part of the work that is being done on the VLBA; I have probably slighted many contributions, and overlooked others. For example, I have not mentioned the work on gravitational lenses (e.g., Porcas 1998), supernovae (e.g., Bartel et al. 1994, Marcaide et al. 1995, Rupen et al. 1998), or radio-quiet quasars (Blundell & Beasley 1998). But I hope that it is clear that the new and exciting results that are appearing are directly due to the VLBA's many strengths: its wide frequency coverage and flexibility, its spectral line capability, its polarization capability, and its high sensitivity (particularly with phase referencing). However, I should point out that almost all my examples come from papers published this year or not yet published. The full power of the VLBA is only just becoming apparent, so I would say it is far too early to assess the "Impact of the VLBA." But it is quite clear that the VLBA is a credit to its designers and builders, not the least of whom is Barry Clark.

My thanks to Peter Barthel, John Conway, Michael Rupen, Greg Taylor, Cathy Trotter, Jim Ulvestad, Craig Walker, Joan Wrobel and many others who allowed me to present their results; to Miller Goss and the NRAO for their invitation to this most enjoyable celebration; and – of course – to Barry Clark.

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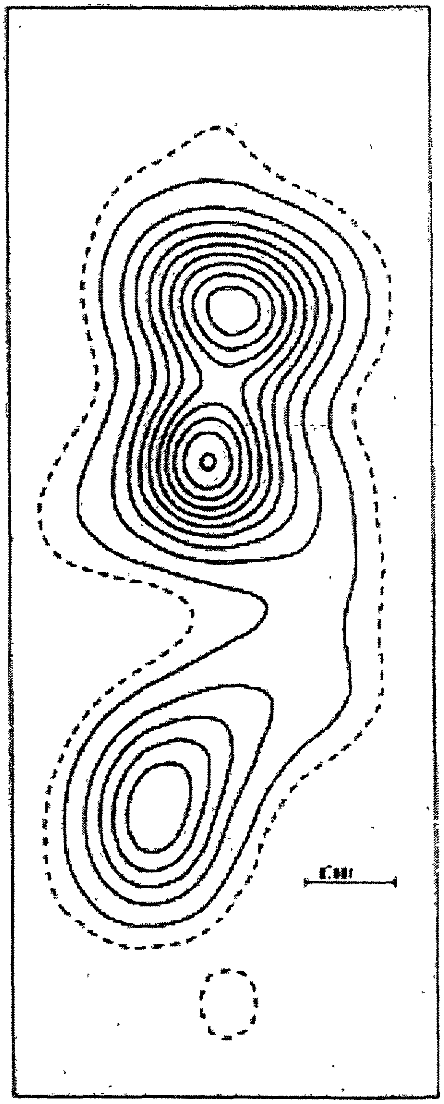

Fig. 1.— Contour map of the structure of the nucleus of NGC 1275 at 2.8 cm; contours are drawn at 10% levels and the dashed line represents the 5% level (from Kellermann 1977).

Fig. 2.— VLBA image of the nucleus of NGC 1275 at 1.3 cm (courtesy of Craig Walker).

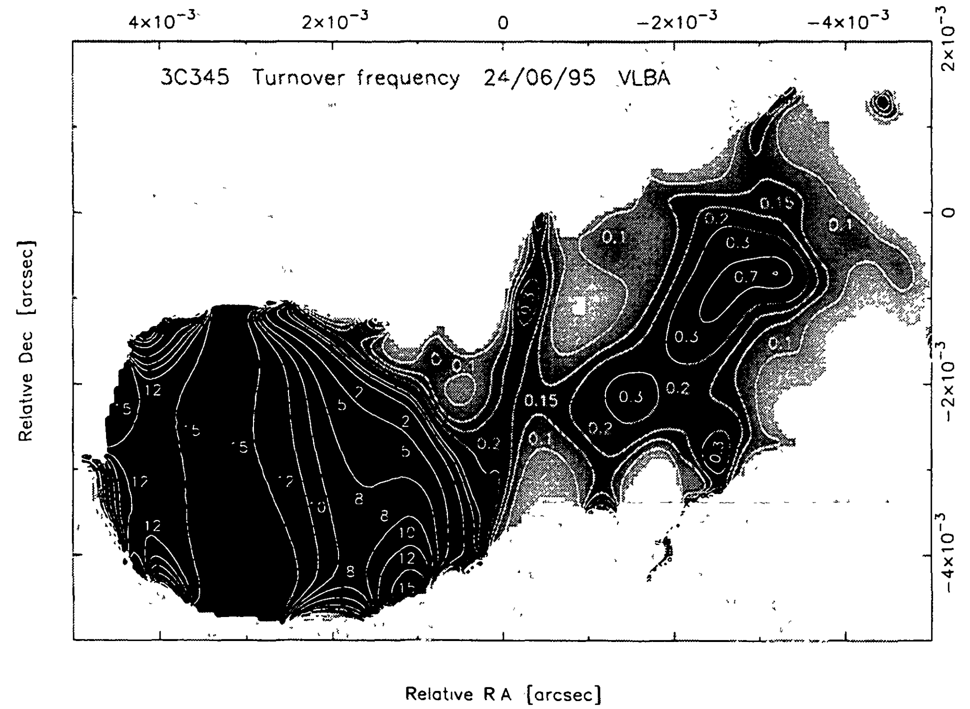

Fig. 3.— Turnover frequency distribution in the parsec-scale jet of 3C345 (from Lobanov 1998).

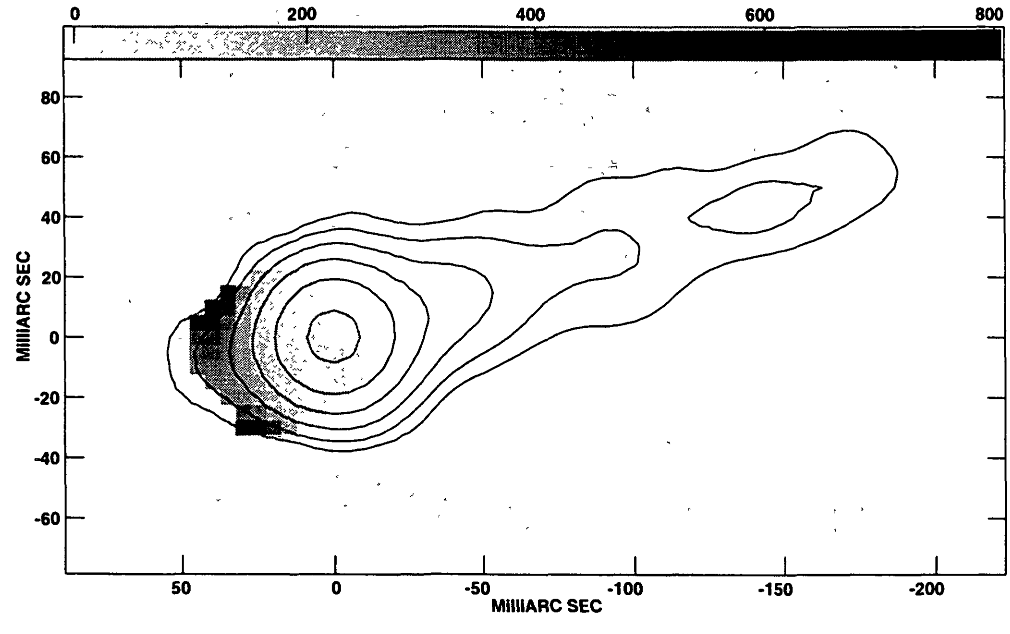

Fig. 4.— HI opacity (grey-scale) superimposed on a 1340 MHz continuum image of Cygnus A, resolution 25 mas FWHM (courtesy of John Conway).

Polarimetry with the Very Long Baseline Array

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ABSTRACT

We review the results of early parsec-scale polarimetry of extragalactic radio sources, and the shocked-jet paradigm used to understand them. Recent results from the VLBA are then presented to illustrate its great power to study these objects.

1. Introduction

The linear polarization of synchrotron radiation from radio sources is sensitive to the order and orientation of the magnetic field, the spectrum of relativistic charges and their signs, the presence of foreground or mixed-in thermal and mildly relativistic gas, and the velocity and acceleration of the radiating fluid. Thus we can use VLBI polarimetry to study a wide range of physical processes, including jet fluid flow and interaction with its environment, shocks and other structures in jets, particle acceleration, and the properties of the narrow-line gas surrounding and entrained by jets. The ability of VLBI to detect structural changes over modest time scales makes VLBA polarimetry an especially powerful diagnostic of physical conditions in compact radio sources. In what follows we present a number of recent polarimetric observations made with the VLBA, and discuss very briefly their implications for the parsec-scale physics of radio sources. In all cases we assume that any Faraday rotation is negligible.

2. Early Results from VLBI Polarimetry

Pre-VLBA polarimetry yielded a number of interesting results (Cawthorne et al. (1993a); Cawthorne et al. (1993b)): (1) blazar jet polarization can be very high, (2) galaxies show essentially zero polarization, (3) weak-lined blazars have $\mathbf{B}_{jet}$ perpendicular to jet axis ("$B_\perp$," or "BL Lacertae-like") (4) strong-lined blazars have $\mathbf{B}_{jet}$ parallel to jet axis ("$B_{||}$," or "quasar-like") (5) weak-lined blazars can have strong and variable core polarization, and (6) strong-lined blazars have low core polarization. Examples of transverse and longitudinal magnetic field configurations from our recent VLBA survey of blazars (Homan et al. 1998) are shown in figures 1 and 2.

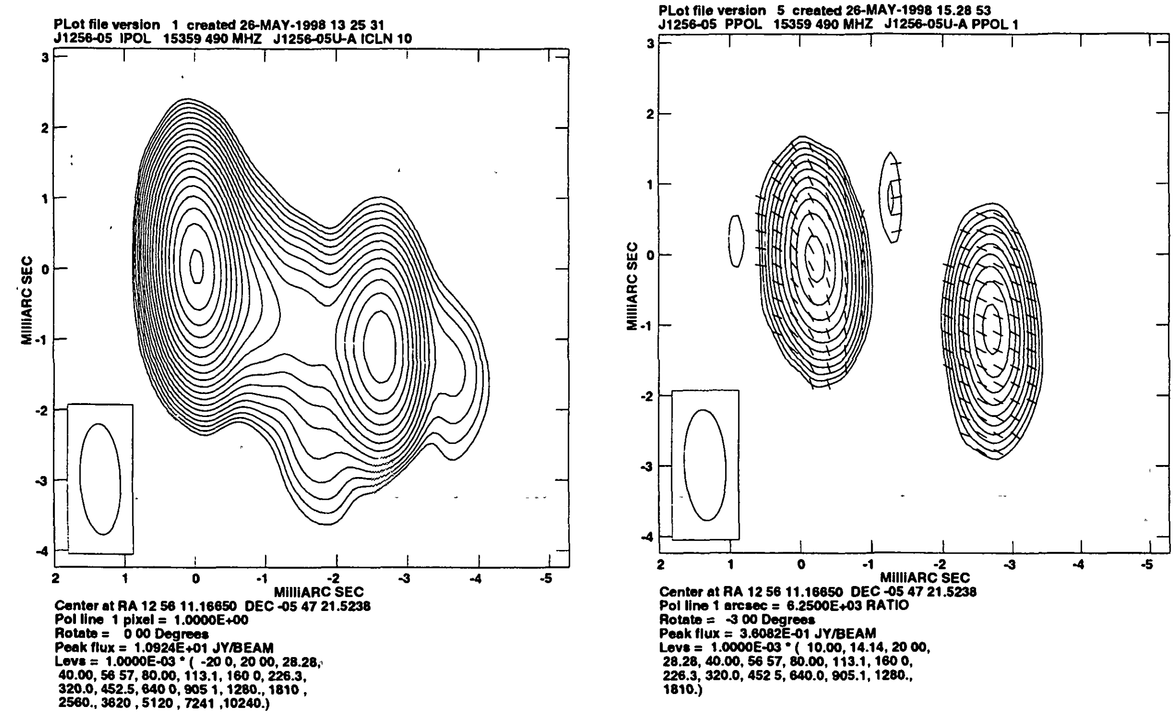

Fig. 1.— VLBA polarimetry of 3C 279 at 15 GHz, epoch 1996.05, showing the total intensity image (left), and the linearly polarized intensity image (right), with ticks indicating the direction of the electric field (Homan et al. 1998). In this source the magnetic field in the bright knot to the south-west is transverse to the jet axis.

3. The Shocked-Jet Paradigm

Our understanding of these phenomena is based in large part on the seminal work of the Michigan group (Hughes, Aller, & Aller (1985); Hughes, Aller, & Aller (1989a); Hughes, Aller, & Aller (1989b)). These authors introduced the model of a plane shock in a relativistic jet containing a tangled magnetic field (previously considered in a somewhat different context by Laing (1980)). The shock compresses a small region in the jet, partially ordering the magnetic field and creating a "Laing sheet" that is observed as a partially-polarized knot if it is observed from the side. The parameters of the model are (1) the degree of shock compression, where unit length is compressed to length k, (2) the angle between jet and line of sight θ, (3) the upstream (unshocked) fluid speed β_u, and (4) the downstream (shocked) fluid speed β_d.

If the magnetic field in the upstream fluid is completely tangled, then the unshocked-jet polarization is zero,

$$m_u = 0,$$

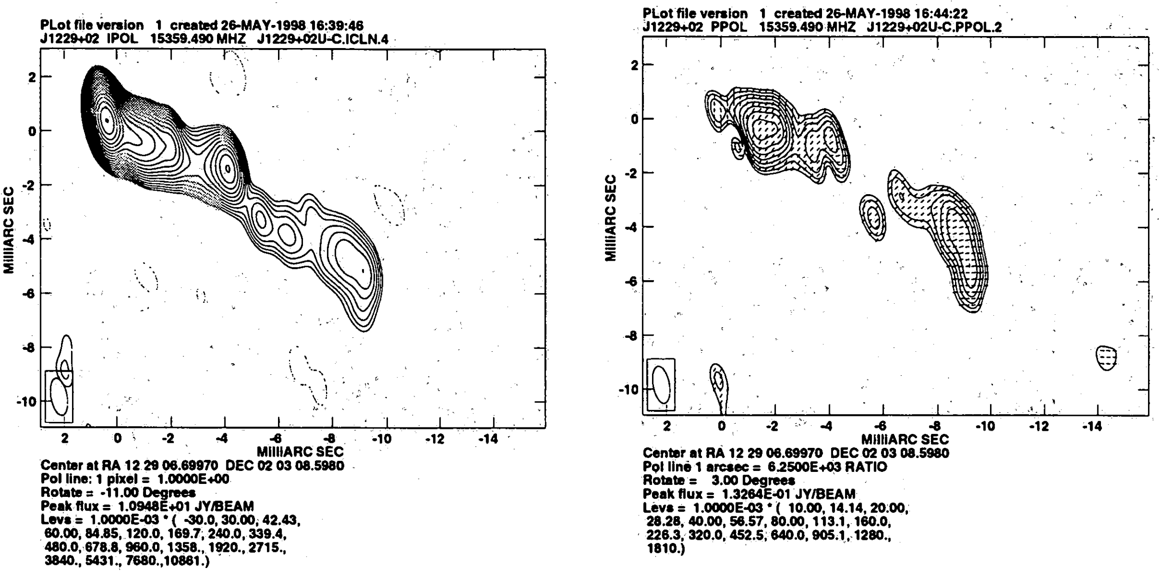

Fig. 2.— VLBA polarimetry of 3C 273 at 15 GHz, epoch 1996.41, showing the total intensity image (left), and the linearly polarized intensity image (right), with ticks indicating the direction of the electric field (Homan et al. 1998). In this source the magnetic field in the jet (extending to the south-west) is longitudinal (along the jet axis).

and the knot polarization is

$$m_d = m_{max} \left(\frac{-(1-k^2)\sin^2\theta_d}{2-(1-k^2)\sin^2\theta_d} \right) ,$$

where

$$m_{max} = \frac{3\alpha+3}{3\alpha+5} , \quad F_\nu \propto \nu^{-\alpha} .$$

The convention here is that

$$m > 0 \;\rightarrow\; B_{net} \text{ parallel to the jet, “quasar-like,”}$$

$$m < 0 \;\rightarrow\; B_{net} \text{ perpendicular to the jet, “BL Lacertae-like.”}$$

In this model the polarization is always BL Lacertae-like because the only order in the magnetic field is in the plane of the shock.

3.1. Add Some Magnetic Field Along the Jet

In order to account for the sources with magnetic fields aligned with the jet axes there must be some field order in that direction (Wardle et al. 1994). This might be created, for example, by shear in the jet fluid flow arising from its interaction with the surrounding material. Defining the ratio of uniform to random field intensity as

$$\xi = \frac{B_u}{B_r} ,$$

the polarizations of jet and knots are

$$m_u = m_{max} \left(\frac{3\xi^2 \sin^2 \theta_u}{2 + 3\xi^2 \sin^2 \theta_u} \right) ,$$

and

$$m_d = m_{max} \left(\frac{-(1-k^2)\sin^2\theta_d + 3\xi^2 k^2 \sin^2\theta_d}{2-(1-k^2)\sin^2\theta_d + 3\xi^2 k^2 \sin^2\theta_d} \right) .$$

The knot–inter-knot total intensity ratio in the jet is

$$\frac{I_d}{I_u} = \left(\frac{2-(1-k^2)\sin^2\theta_d + 3\xi^2 k^2 \sin^2\theta_d}{2 + 3\xi^2 \sin^2\theta_u} \right) f(k) \left(\frac{D_d}{D_u} \right)^{2+\alpha}$$

where

$$f(k) = Ak^{-\left(\frac{9+2\alpha}{3}\right)}$$

accounts for particle acceleration due to shock compression, and the Ds are the Doppler factors of the different regions in the jet. In this model (1) strong shocks ($k^2 < 1/(1+3\xi^2)$) still produce BL Lac-like knots, but (2) weak shocks can produce quasar-like knots, and (3) quasar-like knots are less polarized than the unshocked jet.

The combination of polarimetry with kinematics derived from proper motions provides a large number of observed quantities: (1) the apparent transverse velocity of a knot in the jet μ_{app}, (2) the knot–inter-knot intensity ratio I_d/I_u, (3) the knot polarization m_d, and (4) the inter-knot polarization m_d. These serve to determine completely the model parameters (one also needs the redshift z of the source, and estimates of the cosmological parameters H_0 and q_0). In the best-studied case, knot C3 of the quasar 3C 345, the data are well-fitted by a model with $k = 0.68 \pm 0.07$, $\gamma_s = 12 \pm 3$, $\theta = 3° \pm 2°$, and $f = B_r^2/(B_r^2 + B_u^2) = 1/(1+\xi^2) = 0.5 \pm 0.2$ (Wardle et al. (1994)).

4. Recent VLBA Polarimetry

In this section I give a number of examples of the superb images that can be made with the VLBA. For details I refer the reader to the references cited.

4.1. 3C 309.1

The CSS quasar 3C 309.1 has been studied in detail by Aaron (1996); in figure 3 we show his VLBA polarimetry at 5 GHz. The power of the VLBA to image complex structures and elucidate

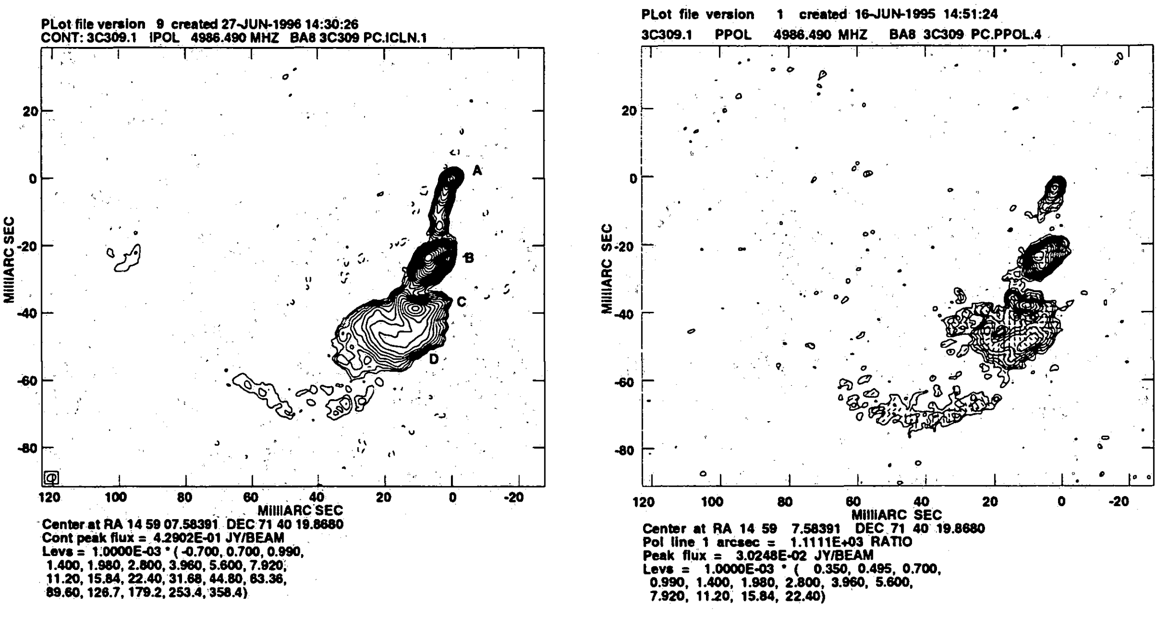

Fig. 3.— VLBA polarimetry of the CSS quasar 3C 309.1 at 5 GHz, epoch 1995.23, showing the total intensity image (left), and the linearly polarized intensity image (right), with ticks indicating the direction of the electric field (Aaron (1996)).

their origins is apparent in this figure. In particular, examination of the polarization image shows the source to be a miniature version of a kiloparsec-scale core-jet-lobe source. Moving from the inside out, we find an essentially unpolarized core to the north, and a bright, highly-polarized jet feeding a parsec-scale lobe. There are numerous signs of jet-ambient medium interactions, including a magnetic field that wraps around the boundaries of the lobe, and a number of places at which the magnetic field makes abrupt changes in direction, perhaps where strong jet-ambient medium interactions deflect and compress the jet.

4.2. Mk 501

Images of the BL Lacertae object Mk 501 at 8 GHz are presented in figure 4 (Aaron, Wardle, & Roberts (1998)). As in the case of 3C 309.1, there are clear sign of the jet-ambient medium interactions in the lobe of this source. Of particular interest is the ring-shaped "hole" near the center of the polarized intensity, about 8 mas from the core. There adjacent regions with orthogonal polarizations combine to create local minima in the polarized intensity. Close examination reveals that this part of the jet consists of a region of quasar-like magnetic field surrounding a knot with BL Lacertae-like polarization. Another example of this morphology is given in the next section.

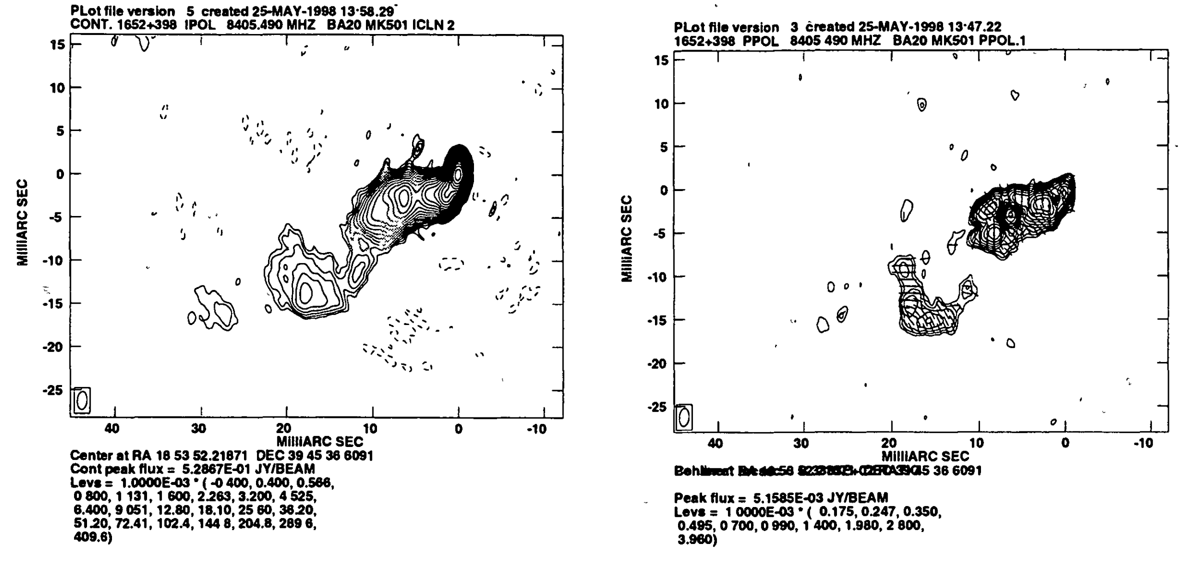

Fig. 4.— VLBA polarimetry of the BL Lacertae object Mk 501 at 8 GHz, epoch 1998.37, showing the total intensity image (left), and the linearly polarized intensity image (right), with ticks indicating the direction of the electric field (Aaron, Wardle, & Roberts (1998)).

4.3. 1055+018

In Fig. 5a we show the total intensity distribution of the blazar 1055+018 (Attridge, Roberts, & Wardle (1998)). A typical core-jet source, 1055+018 contains a bright unresolved component to the east and a jet extending at least 35 mas to the west-northwest. The jet is resolved transverse to its axis (deconvolved width $\gtrsim 3$ mas), and broadens considerably beyond ~12 mas from the core. The true nature of the jet is revealed by the linear polarization distribution shown in Fig. 5b. There we plot contours of linearly polarized intensity with ticks showing the inferred orientation of the jet magnetic field. It is apparent that this jet consists of two distinct parts, (1) a spine lying along the jet axis and containing a series of knots in which the magnetic field is predominantly perpendicular to the axis, and (2) a fragmentary boundary layer in which the magnetic field is predominantly parallel to the axis. Comparison of these two images shows that the $B_{||}$ regions lie on the outermost visible edges of the jet. A natural interpretation of this two-component structure is that (1) transverse shocks dominate within the jet spine, so the net field there is perpendicular to the jet axis, and (2) in the boundary layer the magnetic field is determined by the jet's interaction with the surrounding emission line gas, resulting in a longitudinal field. This configuration is very similar to that seen in Mk 501. Apparently these sources display at one time the magnetic field configurations characteristic of both weak- and strong-lined blazars, and as such provide us with opportunity to study both the shocked-jet paradigm and the magnetic field-optical type relationship for blazars.

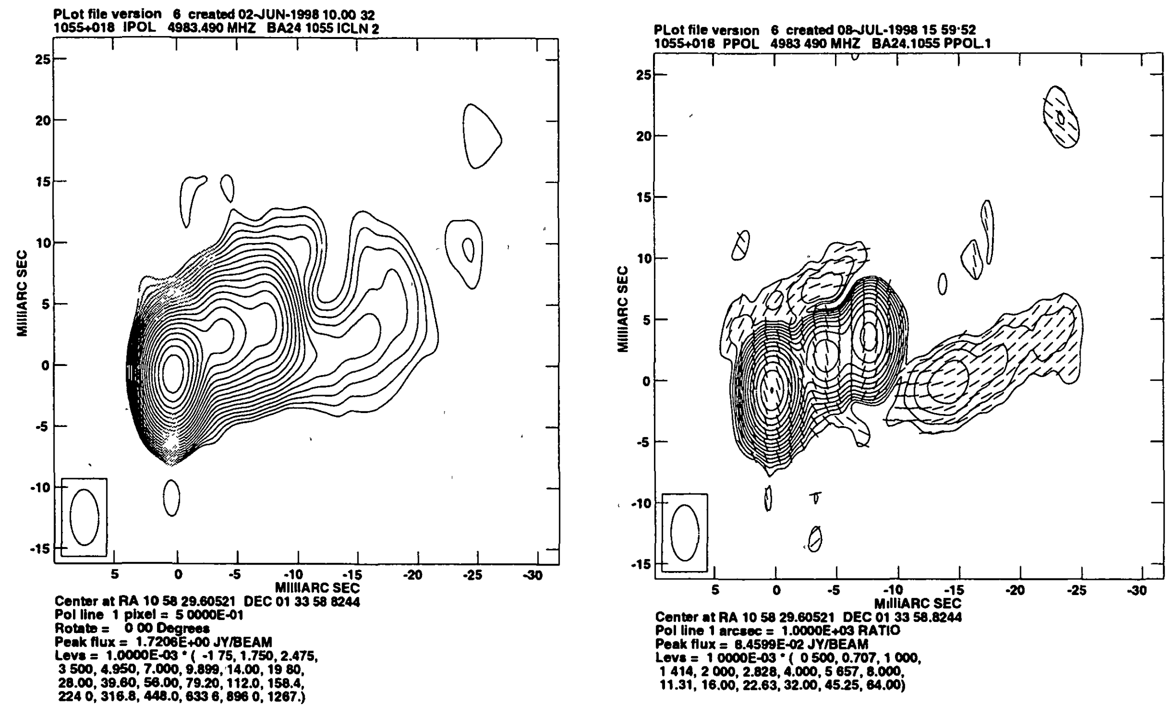

Fig. 5.— VLBA images of the quasar 1055+018 at 5 GHz as observed with the VLBA plus one VLA antenna, epoch 1997.07, showing the total intensity image (left), and the linearly polarized intensity image (right), with ticks indicating the direction of the magnetic field (Attridge, Roberts, & Wardle (1998)).

4.4. Circular Polarization

It has long been our goal to detect circular polarization on parsec scales in compact radio sources (Wardle & Roberts (1994)). Synchrotron radiation has a small intrinsic circular polarization, $|m_C| \simeq 1/\gamma$, where γ is the Lorentz factor of the radiating electrons, so measurement of m_C can be used to constrain the energy spectrum of the radiating particles. In addition, circular polarization can be created by conversion of linear polarization. This occurs because the normal modes for radiative transfer are not purely circular (which leads to Faraday rotation), but slightly elliptical. The resulting small component of linear birefringence converts Stokes U to V, and *vice versa.* Both rotation and conversion are caused by the lowest energy relativistic electrons, and therefore serve as probes of the low end of the electron energy spectrum. An important difference between rotation and conversion is that rotation is sensitive to the sign of the radiating charges, while conversion is not. Thus an equal mixture of electrons and positrons can produce conversion but not rotation, so circular polarization without significant rotation is one possible signature of an e^-–e^+ plasma.

Circular polarization is extremely difficult to measure with circularly-polarized feeds such as

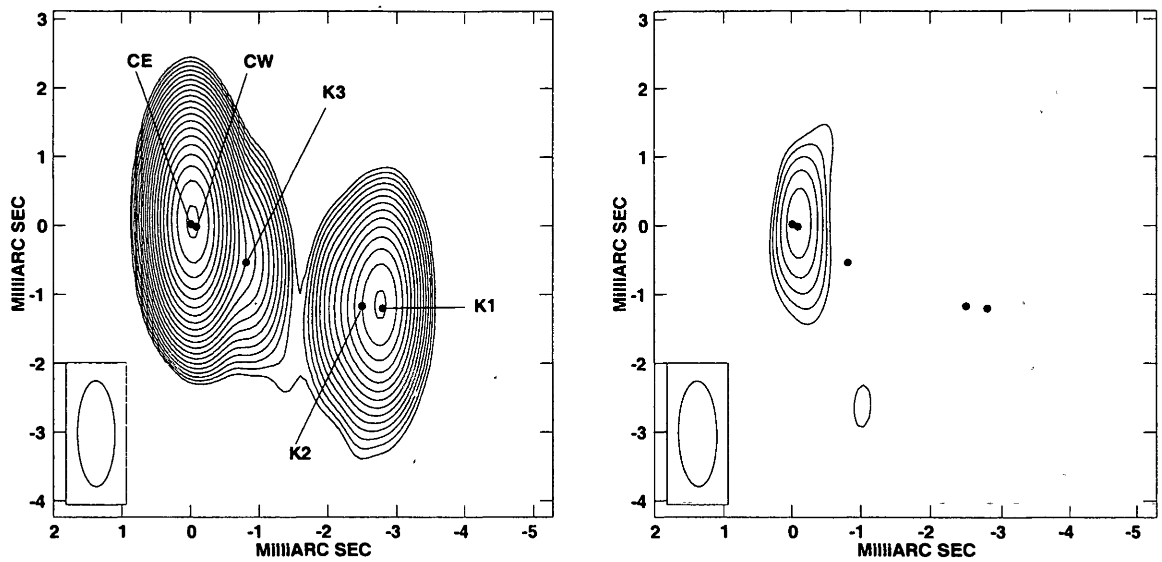

Fig. 6.— VLBA polarimetry of the quasar 3C 279 at 15 GHz, epoch 1996.57 (Wardle et al. (1998)). (Left) the total intensity image, with lowest contour 0.02 Jy/beam and peak 0.325 Jy/beam, and (right) the circular polarized intensity image, with lowest contour 0.015 Jy/beam and peak 0.078 Jy/beam. In each image the contour intervals are factors of $\sqrt{2}$.

those on the VLBA. Nonetheless, we have recently detected small but reliable circular polarization in three AGN, 3C 84, 3C 273, and 3C 279 (Wardle et al. (1998); Homan et al. (1998)). This work depends on the superb calibration and instrumental stability of the VLBA. A detailed analysis of the procedures (and pitfalls) involved in detection of circular polarization shows that the small signals we detect (locally $|m_C| \lesssim 1\%$) are not artifacts (Homan et al. (1998)).

The most interesting case is that of the quasar 3C 279. VLBA images of the total intensity and circular polarization of this source at 15 GHz are presented in figure 6. Analysis of component CW shows that its circular polarization is most likely the result of conversion of linear polarization. Combined with constraints on the energetics of the jet, this suggests strongly that the jet in this source is a mixture of electrons and positrons, rather than one of electrons and protons. This result could play a significant role in our understanding the formation of jets in AGN (Wardle et al. (1998)).

5. Acknowledgments

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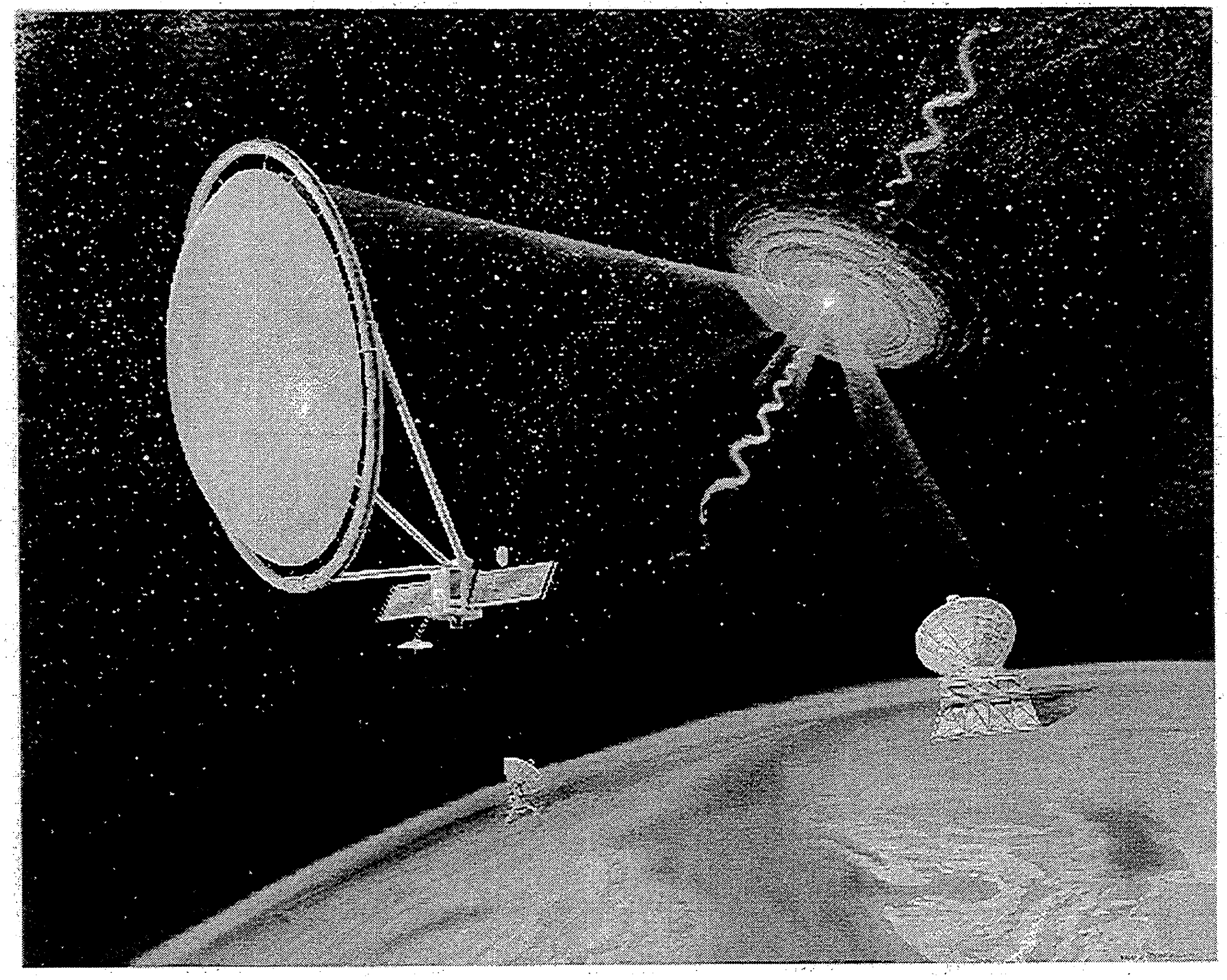
Section VI: The Future



Artist's conception of the proposed ARISE orbiting VLBI radio telescope. (NASA/JPL)

The VLA Expansion

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1 The VLA – Yesterday and Today

The Very Large Array has so transformed radio astronomy, and this audience has been so much a part of this transformation, that there is no need for me to describe the capabilities of the VLA here. Suffice it to say that the VLA has been, is now, and will remain for at least another decade, the world's preeminent centimeter-wave synthesis telescope. That there are no challengers for this distinction is indeed a testimony to the skill and genius of its designers and builders.

However, it is not enough to rest on our laurels. For science is a dynamic subject, and merely being the best instrument of a specific type is not sufficient to ensure that the instrument remains useful to science. The needs of science change, and with them, so must the instruments we use.

The VLA was designed more than twenty years ago, and made excellent use of the technology of that day. Various compromises in the design had to be implemented for practical reasons, and most of these compromises remain with us today. And in many cases, the potential science that the VLA could return is severely limited by these compromises. To illustrate this point, I list some of these lilmitations below:

- The original frequency suite of the VLA provided only four frequencies, each with a rather narrow BWR (BandWidth Ratio), as listed in the table below:

Band Code	Frequency Range (GHz)	BWR (ν_u/ν_l)
L	1.25 - 1.74	1.39
C	4.25 - 5.10	1.20
U	14.25 - 15.7	1.10
K	21.70 - 24.5	1.13

  Although L-band has been retrofitted with modern amplifiers, and the performance of the other listed bands has been improved somewhat, the limitation in tuning range remains close to these original specifications, resulting in a limited capability to detect more exotic spectral transitions, or the stronger transitions from moderately or highly redshifted objects.

- The correlator provides at most 512 channels, and that many only when the total bandwidth is 1.56 MHz or less. This limitation places very strong constraints on our capability to observe high frequency spectral transitions. For example, at our highest frequency band (40 – 49 GHz), the correlator can provide only 16 spectral channels across a very modest total velocity coverage – with a velocity resolution of only 20 km/sec. Within L-band, the correlator limits survey work to a total of about 1300 km/sec – quite inadequate for modern deep surveys, or sensitive wide-field imaging.

- The total instantaneous bandwidth is about 180 MHz (summing over both polarizations), far less than the 1 GHz or more which is available at the front end amplifiers. This restriction results in a severely reduced continuum sensitivity.
- Only two frequencies can be observed simultaneously, and these cannot be separated by more than 400 MHz. Hence, more widely separated spectral transitions cannot be simultaneously observed.
- The maximum baseline of 35 Km is insufficient to resolve a wide range of astrophysical phenomena. The NRAO has no instruments to cover the baseline gap between the VLA's outer scale and the VLBA's inner scale of about 350 Km.

None of these limitations could have been addressed at the time of the design of the array. The technology of the time simply did not allow greater capability at a reasonable cost. However, all of them can be addressed now, using proven, available technologies, and at a cost very modest in comparison to the initial investment cost of the VLA.

The scale and benefits to science of such improvements have been apparent for many years. And indeed, with an appropriate level of infrastructure funding, most of the improvements we are now seeking would already have been implemented, or be underway. But for various reasons, such infrastructure funding has not been budgeted, and we are now at a stage where it is necessary to group all of the desired, incremental improvements into a single plan, called The VLA Expansion.

2 Why Do an Expansion?

If the VLA remains the best instrument in its class by far, why bother to improve it? Why not merely continue to maintain it? The answers have to do with changing science goals, available technology, and increasing competition from other astronomical fields:

- **Science Requires It**

  The questions that science asks are continually evolving. Our understanding of our universe has changed dramatically in the last twenty years (thanks in part to the VLA!), and the questions that scientists ask have changed in concert with our understanding. In general, it is fair to say that the questions are getting harder, and demand more of our instruments. To remain relevant in the quest for answers, the instruments must evolve with our understanding.

- **Technology Permits It**

  New, but established, technologies can provide much more capability from the VLA than what was available twenty years ago. In that time, great advances have been made in the areas of (for example):

  - Feed Design

  - Polarizer Design
  - Amplifier Design
  - Wideband Signal Transmission
  - Correlator Design
  - Computational Capability

  All of these are of critical importance to array performance, and their implementation would result in greatly enhanced instrumental capability.

- **Economics Favors It**

  The benefit/cost ratio for upgrades is always good – and for the VLA it is extremely high. And it is a 'leveraged investment' – nowhere else on Earth can so little input capital into a scientific instrument result in such an extensive and far-reaching increase in scientific capability. The reason for this is the extensive infrastructure already in place – the world's largest and most flexible array combined with an established site with a professional support staff. And, because the great majority of the expansion deals with replacement of aging components with more capable elements, the increment in operational costs will be very small.

- **Our Future Needs It**

  Our science needs the continuing input of bright, young minds. If we fail to attract such people – scientists, engineers, and programmers – we will lose the race to remain relevant in a competitive world. Because a static institution will not attract the interest of a dynamic one, one key to a successful future must be to find a means to keep the VLA on the 'cutting edge' of technical capability. To ensure such a dynamic environment, the VLA must be able to incorporate emerging technologies which are of such fundamental importance in keeping the instrument at the front of science.

  To ensure a bright future for radio astronomy in general, we must keep our front-line instruments current. This is necessary to ensure the VLA is a strong bridge to the 'next generation' telescope – the Square Kilometer Array, for example.

- **The Rest of the World is Moving On**

  Other radio-astronomy instruments have been successful in securing significant upgrades – the Westerbork Synthesis Radio Telescope, the Australia Telescope Compact Array, and Arecibo, for example. We wonder why the world's premier instrument has not received similar attention! The NRAO cannot afford to remain static in its instrumental capabilities if we are to remain competitive as an organization.

3 VLA Expansion Plan Assumptions

In order to plan an expansion, certain 'ground rules' are needed to permit definition of the project. Our overall goals are to enable an expansion quickly and relatively cheaply, using current technologies. For planning purposes, we have assumed the following:

- **Use the Existing Antennas.** The most expensive part of any 'VLA replacment' would be the antennas. Because the VLA's antennas, assuming reasonable maintenance, are expected to continue at current levels for more than the next decade, we have assumed for our planning that they will not be replaced. However, modifications (such as to the quadrupod legs, focus/rotation area, or reflector surface) have been considered.
- **Use the Existing Antenna Stations.** Major changes to the array configuration plan are expensive – so we have not considered them in the plan. However, establishment of new stations for a super-compact configuration has been considered.
- **Use existing, established technologies.** This has been stressed before, and we do it again. We are not employing 'just in time' projected technologies in our planning (but would consider use of important advances, if they occur before we can implement our plan!) This permits reliable estimates of costs, timescales, and benefits.
- **Take maximum advantage of ALMA design.** The observatory is planning a major new instrument, the ALMA, making extensive use of new technologies. Many of the capabilities of the new array are very similar to those for the VLA Expansion (bandwidth, transmission system, correlator, one-line control, data processing, to name some major areas), and we will be using ALMA design wherever possible to enable the maximum benefit to the VLA Expansion at minimum development cost.

4 VLA Expansion Goals

The overall goal of the VLA Expansion can be summarized very simply –

A Factor of more than 10 improvement in all key instrumental parameters of the VLA

These parameters lie in the following categories:

- Sensitivity
- Frequency Coverage
- Spectral Capability
- Surface Brightness Sensitivity
- Angular Resolution

I expand on these categories in the next section.

5 VLA Expansion Overview

Ongoing development of the upgrade concept has demonstrated that improvements by a factor of ten or more over current capabilities are achievable in all five areas listed above. To make more definitive the anticipated performance of an expanded VLA, we define the following as primary goals of the VLA Expansion:

- **Sensitivity** The current sensitivity limit[1] for the VLA is approximately 6μJy at the most sensitive bands – 1.4, 4.9, and 8.4 GHz, and considerably worse at all other bands. Improvements in bandwidth, receiver design, and antenna efficiency can greatly improve the sensitivity at all bands, most especially at the higher frequency bands. We define as a goal:

  1 μJy Sensitivity between 2.0 and 40 GHz

  The means by which this sensitivity will be obtained between 2 and 40 GHz will also give significant improvement at other frequencies – typically by a factor of 5 at frequencies below 2 GHz, and a factor of 10 to 30 for the 40 - 50 GHz band, over current capabilities.

- **Frequency Coverage** The current VLA has very limited frequency coverage – essentially covering only the major spectral lines (*e.g.* HI, OH, H_2O, NH_3) plus selected narrow continuum bands. Many considerations, based both on continuum and spectral line observing, argue persuasively for a much expanded frequency tunability. We thus define as a goal for the VLA Expansion:

  Complete Frequency Coverage from 1.2 to 50 GHz

  This frequency range is that which can be accessed from the Cassegrain secondary focus ring. We would like to obtain similar frequency availability at lower frequencies as well, although not at the cost of diminishing the array's capabilities at higher frequencies. We thus define a secondary goal:

  Complete Frequency Coverage from 50 MHz to 1.2 GHz

  which must be accomplished from the prime focus.

- **Spectral Line Resolution, Bandwidth, and Flexibility** The current correlator severely restricts the potential science from the VLA. A modern correlator is critically needed to keep pace with the array's current and future observing capabilities. A primary goal is:

  A modern correlator handling at least 40 stations, 8 GHz bandwidth per polarization, and 8000 spectral channels per polarization product.

- **Angular Resolution** There are strong scientific benefits to increasing the VLA's resolution. Because of the certain complexity in the objects we wish to observe, it is not sufficient to add a sparse sampling of baselines at intermediate distances. To ensure the maximum scientific return, we define as a goal:

  Expansion of the VLA's maximum baseline to ~400 Km, while maintaining the imaging performance characteristic of the current VLA.

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[1] We define the sensitivity as the expected r.m.s. in an image made with 12 hours' data, natural weighting, all available antennas, and the maximum available bandwidth

Finally, although the VLA can make excellent low resolution images on the scale of the antenna primary beam, the array could perform low surface brightness surveys of extended objects much more quickly and accurately if the antennas were more densely packed than is currently available in the 'D' configuration. We set a goal:

Development of the 'E'-configuration, comprising a dense packing of antennas within a $\sim$300 meter maximum spacing.

These improvements, when implemented, will provide us an instrument which will continue to set the world standard for radio astronomy in the meter-to-millimeter bands for at least another generation.

5.1 Major Components of the VLA Expansion Plan

Broadly speaking, all elements of the VLA Expansion Plan can be placed in one of two groups:

5.1.1 An Ultra-Sensitive Array

These enhancements are made entirely within the existing infrastructure at the VLA site.

- **New Cassegrain receivers**: Lower noise temperatures and much wider bandwidth performance (up to 8 GHz in each polarization channel) in existing bands; add 2.4 GHz and 33 GHz bands; improve the performance of the 1.4 GHz band with a modern feed.
- **New Prime Focus Receivers**: Rebuild the FR mount to permit removal of the subreflector to provide prime focus accessibility to a low frequency feed/receiver system.
- A **fiber-optic data transmission system** to transmit the broadband signals and monitor data, replacing the original waveguide.
- A **new correlator**, able to support 40 antennas, with 8 GHz in each polarization and up to 8192 spectral channels per baseline per polarization product, and the ability to independently process up to 8 different frequencies from one or two frequency bands.
- **New antenna stations** for a "super-compact" E configuration to enable fast mosaicing of large fields.

5.1.2 The A+ Configuration

These enhancements involve development of new infrastructure at locations away from the VLA site:

- **New antennas** to provide now-unavailable baselines between those in the VLA and in the VLBA. In order to provide excellent imaging fidelity, it is felt that at least 8 new stations will be required, located within NM and AZ at distances of up to $\sim$ 300 Km from the VLA.

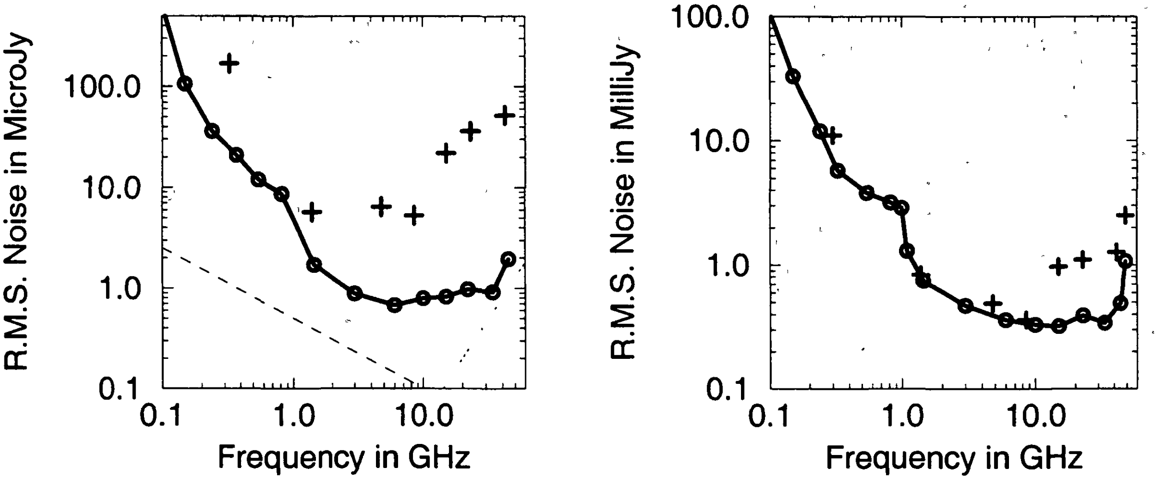

Figure 1: The current (+) and predicted (o) continuum (left) and spectral line (right) sensitivity of the VLA after the upgrade. For both panels, a 12-hour observation with 27 antennas is assumed. The line sensitivity is based on a bandwidth equivalent to 1 km/sec. For the A+ configuration and 36 antennas, the sensitivity would be improved by a further factor of 1.33. The left panel also shows a typical synchrotron spectrum (dashed) and an optically thick thermal spectrum (dotted), to demonstrate the relative sensitivies of the proposed new bands for objects of these common types.

- **Fiber-optic links** between the VLA and the inner VLBA antennas, and between the VLA and the proposed new antennas, to provide for real-time data correlation with the upgraded correlator.

These new antennas will be VLBA stations for ~75% of the time, and must thus be equipped with a complete VLBA data recording system. The addition of eight or more new antennas to the VLBA will give to that instrument an enormous improvement in imaging flexibility and performance.

The combination of these enhancements will yield an instrument with many fundamentally new capabilities. The continuum sensitivity will improve by more than an order of magnitude in some bands, as shown in Fig. 1. New and powerful spectral line observations will be possible and significantly more frequency choices will be available.

Linkages to the innermost VLBA antennas and the new antennas will increase the maximum angular resolution by a factor of ~ 8. The sensitivity increases will allow the increase in angular resolution to be exploited fully when observing a wide (and in many cases for the first time, representative) variety of thermal and nonthermal objects, both galactic and extragalactic.

6 Expansion Costs and Schedule

An advantage of this proposal is that fairly accurate costs can be estimated reasonably easily, since we are dealing with familiar technologies and schedules. Dick Sramek has reviewed the costs, and has made the following estimates:

- **Ultra-Sensitive Array**

System	Cost($M)
Cassegrain Receivers	14.3
LO System	6.4
IF System	4.9
Fiber-Optic System	5.4
Correlator/ FIR filter	11.0
Antenna Mechanical	2.4
Civil Construction	1.4
Computing	7.2
Systems Integration	4.3
E/PO	0.5
Project Management	2.7
Contingency/Mgt. Fee (17%)	10.3
Total	70.8

- **A+ Configuration**

System	Cost($M)
Eight Antennas (fully equipped)	50.0
700 Miles of Fiber	10.5
Miscellaneous	0.1
Interconnection	2.0
Contingency (15%)	9.4
Total	72.0

To this we must add an important unknown – the cost of renting fiber-optic cable for stations too distant for us to consider connecting directly with our own cable.

Although the total cost of this plan may generate concern, it must be remembered that the replacement cost of the VLA, in current dollars, including the improvements already made, must surely exceed $250 million. Assuming the VLA Expansion costs will be spread over a total of ten years, the annual investment would be less than 5% of the total – lower than the industry standard for infrastructure improvements to working facilities.

The schedule of an expansion must take into account manpower limitations, plus the fact that the VLA is a working research instrument and cannot be shut down for extended periods. Assuming no shutdowns longer than 4 months, the necessary antenna and site modifications can be done at a rate of about 5 antennas per year – a 6 year period is thus required.

The correlator is a very large machine, and furthermore requires a significant development time and cost. Our assumption has been that these costs would be mostly borne by ALMA, so that the new correlator cost need only include parts and the extra labor for construction. Completion of the full correlator probably would not take place until 2007.

All parts of the 'Ultra-Sensitive Array' portion of the upgrade can be viewed as incremental improvements – many components can be undertaken at one time, and it is not necessary that one part be completed before another to be useful. And throughout all of these planned improvements, regular observing would go on.

The other half of the upgrade – the A+ configuration, is rather different. As a single, and rather large, component, it is probable that it will have to be funded as a separate project, and thus it may be required that it be deferred until the second half of the the next decade. However, in terms of development and manpower, there is no reason it cannot go sooner – the design of a VLA or VLBA equivalent antenna is well known to the NRAO, and the construction will be largely independent of the observatory, beyond a management role.

7 Interaction with the Square Kilometer Array Concept

There is considerable interest in the next major centimeter wave project in radio astronomy – the Square Kilometer Array, and it is reasonably asked what will be the relationship of that project with the VLA Expansion. My personal opinion is that the SKA is indeed the long term future of centimeter radio astronomy. But it also appears to me that the SKA is not yet a defined project. At this time, there is no agreement of the parties currently developing the SKA concept on the technology or specifications for this telescope. Neither is there an international body with a budget and authority to manage and direct the project. Such a structure is critically needed, but establishment of such a management organization must be years away – as is a demonstration that the technologies needed to meet the extremely ambitious goals of collecting area and frequency coverage are mature. Given the ambitions of the project, and the absence of a definition or structure, it seems clear that the SKA is at least a decade away from being funded as an active, mature project. From now and until then, the SKA must be in a design and development stage.

The role of the VLA is clear in this scenario. It must remain what it is – the premier instrument for radio astronomy. Further, to act as a useful bridge to the SKA, the VLA must be expanded so it will remain relevant in a rapidly advancing research environment.

The VLA Expansion can also be directly relevant to studies of the SKA concept. Assuming that development of suggested SKA technologies takes place over the next few years, there are a number of areas where the VLA Expansion can both help this development, and benefit by it. Three examples come to mind:

- It is generally understood the SKA will consist of 'stations', each perhaps 100 to 200 meters in diameter, which are themselves composed of many small dishes, or a large deformable reflector, or a bank of phased elements. A single station in this concept is a logical substitution for the 'E' configuration proposal of the Upgrade.

- Along the same vein, the new stations for the A+ configuration are currently imagined to be standard 25 meter antennas. But we know well that a much larger collecting area is needed to give better imaging response. A SKA 'station' would be a natural substitute.

- A major difference between the ALMA correlator and a VLA Expansion correlator is the need for the latter to be tolerant to RFI. The same characteristic is essential for the SKA. A yet-undefined group, investigating an 'RFI-smart' correlator, might well wish to consider building a VLA correlator as a prototype for a future SKA correlator.

Other scenarios can easily be imagined.

If the expansion does employ these or other technologies spawned by the SKA, there surely will be necessary compromises. For example, the current SKA concept does not imagine operation above 23 GHz – use of prototype SKA 'stations' for the VLA's 'E'-configuration, or for the new A+ configuration stations thus probably will eliminate observing at 43 and 86 GHz with those facilities.

8 Conclusion

Every ten years, near the end of the decade, the U.S. astronomy community meets to prioritize funding initiatives for the following decade – a process known informally as the astronomy decadal review. The results of this process are very influential in funding decisions made by the NSF and NASA over the following ten years. The decadal review panels for the current cycle are now being assembled, and it is critically important that the VLA Expansion receive a top recommendation. In my humble opinion, the VLA Expansion is surely the most cost-effective project in radio astronomy currently being developed for funding consideration by this panel. For a small fraction of the investment cost, we can multiply by an order of magnitude or more all critical observational characteristics of the instrument. Given that the VLA is the preeminent centimeter-wave radio astronomy tool, an expansion will surely guarantee its useful lifetime until the next generation centimeter-wave project, the Square Kilometer Array, is ready for observing, sometime after the year 2010.

THE ATACAMA LARGE MILLIMETER ARRAY

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Abstract

The Atacama Large Millimeter Array (ALMA) is a synthesis imaging telescope designed to provide sensitive, high-precision astronomical imaging at sub-arcsecond, 0."1, resolution at millimeter and submillimeter wavelengths. In order to achieve these goals it is necessary to locate the Atacama Large Millimeter Array on an extremely dry site to minimize the atmospheric background emission; the receiver noise temperatures should be near the limit imposed by quantum photon fluctuations and the antenna warm spillover contribution should be minimized. Together, these requirements lead to the technical design goals for the Atacama Large Millimeter Array. The involvement of international partners in the initiative has the prospect for further expansion and enhancement of the project. Completion is expected in approximately nine years.

Introduction

The Atacama Large Millimeter Array (ALMA) project will bring to millimeter and sub-millimeter astronomy the aperture synthesis techniques of radio astronomy which enable precision imaging to be done on sub-arcsecond angular scales. The richness of the celestial sky at millimeter wavelengths is provided by thermal emission from cool gas, dust, and solid bodies, the same material that shines brightly at far infrared wavelengths. Presently, such natural cosmic emission can be studied only from space with the coarse angular resolution and limited sensitivity that small orbiting telescopes provide. The ALMA will image at 1 mm wavelength with the same 0."1 resolution achieved by the Hubble Space Telescope (HST) at visible wavelengths and will provide scientific insight at longer wavelengths that is complementary to that of the HST, and its successor instrument the Next Generation Space Telescope (NGST), and will do so with the same image detail and clarity. In addition, the reconfigurability of the ALMA antennas gives the ALMA a *zoom-lens* capability so that it can also make high-fidelity images of large regions of the sky. Uniquely, the ALMA is a complete imaging instrument.

Objectives and Scope — Scientific

In specifying the scientific goals of the ALMA, astronomers have called for the capability for precise astronomical imaging with an unprecedented combination of sensitivity and angular resolution at the shortest radio wavelengths for which the Earth's atmosphere is transparent. This unique combination of capabilities will make available for astronomical investigation a wealth of new opportunities and new science. Scientists using the ALMA will:

- Image the redshifted dust continuum emission from evolving galaxies at epochs of formation as early as $z=10$;
- Determine through molecular and atomic spectroscopic observations the chemical composition of star-forming gas in the earliest forming galaxies;

- Map fluctuations in the microwave background that result from formation of the first mass structures in the universe;
- Reveal the kinematics of obscured galactic nuclei and QSOs on spatial scales smaller than 100 pc;
- Assess the influence that chemical and isotopic gradients in galactic disks have on star formation and spiral structure;
- Image heavily obscured regions containing protostars, and protostellar and pre-planetary disks in nearby molecular clouds with a spatial resolution less than 10 astronomical units and a kinematic resolution better than 1 km/s;
- Detect the photospheric emission from thousands of nearby stars in every part of the Hertzsprung-Russell (H-R) diagram;
- Reveal the crucial isotopic and chemical gradients within circumstellar shells that reflect the chronology of stellar nuclear processing and envelope convection;
- Establish the relative distributions of the large number of complex molecular species in regions of star formation, relating them to shock fronts, grain disruption, and energetic outflows—information which is essential to the understanding of astrochemistry;
- Obtain unobscured sub-arcsecond images of cometary nuclei, hundreds of asteroids, *Centaurs*, Kuiper-belt objects, planets and satellites in the solar system—observations that can be done for astrometric or astronomical studies uniquely during daylight or nighttime hours;
- Image solar active regions and investigate the physics of particle acceleration on the surface of the sun.

The ALMA is a long-lived user observatory. Its scientific impact at any time will be facilitated by the quality of its instruments and limited only by the creativity and industry of its astronomer users.

Objectives and Scope — Technical

The burden of designing a powerful and unique instrument is that it requires an extension of existing technology. This is as true for the ALMA—with its densely packed mosaicing configuration, broadband sensitive receivers, total-power instrumentation, and precision antennas—as it was for the Keck Telescope with its segmented, optical-quality primary mirror, and as it will be for the NGST which attempts to combine the Keck segmented mirror technology with ultra lightweight materials and metrology suitable for the space environment. Application of significant technological advances is the *sine qua non* of the design of a forefront scientific instrument.

Precision astronomical imaging and the need for sensitivity sufficient for the study of faint objects with the ALMA defines the three broad categories of technical requirements for the array: (1) development of broadband, quantum-limited receivers; (2) design of antennas of very low blockage so that the *warm spillover* is minimized; and (3) choice of a site for the array where the background emission and absorption from atmospheric water vapor is minimized. The first two points are the focus of the initial three-year Design and Development phase of the ALMA; the latter point has its resolution in the superb, dry site recommended for the ALMA in the Chilean Altiplano.

The sensitivity of a synthesis telescope such as the ALMA depends on a number of parameters that are easily understood quantitatively, e.g., the number of antennas, the size of each antenna, and the observing bandwidth (for continuum observations). The sensitivity also depends on design parameters and manufacturing precision that are less well appreciated intuitively. Among these are: the precision of the parabolic figure of the antennas, the antenna spillover and aperture blockage, the antenna and receiver optical design, the receiver temperature, and the quantization of the correlator. Finally, at millimeter wavelengths the sensitivity of a ground-based instrument depends on both emission and absorption in the Earth's atmosphere, effects that are highly dependent on frequency, time, and source elevation (air mass). The effects of the atmosphere are now the most important factor limiting the sensitivity of existing millimeter-wave and submillimeter-wave telescopes. These effects are the more difficult for us to appreciate, largely because the dependencies are embedded in frequency-dependent exponentials. An illustration is given in the example below.

Point Source Continuum Sensitivity

The flux density sensitivity, or rms noise in flux density units, for a synthesis telescope is

$$\Delta S = 4\sqrt{2}k\, T_{sys}/[\gamma \epsilon_q \epsilon_a\, \pi D^2\, (1/2\, n_p N(N-1)\Delta\nu\Delta t)^{1/2}]\,, \tag{1}$$

where D is the antenna diameter, N is the number of antennas in the array, n_p is the number of orthogonal polarization pairs that are correlated ($n_p = 1$ or 2), the continuum bandwidth is $\Delta\nu$, the integration time Δt, ϵ_q is the correlator quantization efficiency, $\epsilon_a(\nu)$ is the aperture efficiency of the individual array antennas, and γ (which we assume here is unity) is an array geometry-dependent factor related to the manner in which the u-vsdata is gridded and tapered.[1, 2]

One of the principal array design specifications is the figure of the antenna which enters the sensitivity calculation through the aperture efficiency ϵ_a. If the rms distortion of the antenna shape from a perfect parabolic shape is σ millimeters owing to all causes – manufacturing tolerances, wind, gravitation, and thermal distortions – that lead to spatially uncorrelated deformations, then ϵ_a can be expressed by the Ruze formulation.

$$\epsilon_a = \epsilon_0 \exp[-(4\pi\sigma/\lambda)^2]\,, \tag{2}$$

where λ is the wavelength of the observation being made and ϵ_0 is the long wavelength or maximum efficiency of the telescope once such effects as blockage, diffraction, and feed illumination are taken into account. For most radio astronomy antennas, $\epsilon_0 \approx 0.5$, while for the higher efficiency antennas $\epsilon_0 \sim 0.7$ (VLBA, GBT), and for the highest efficiency minimum blockage antennas (BIMA) $\epsilon_0 \sim 0.80$. The current ALMA designs incorporate many of the BIMA design approaches to reduce blockage, and we are designing to achieve this latter value. Moreover, since ϵ_0 depends so strongly (exponentially) on σ, it is very important to minimize this parameter, that is, to build a high precision antenna. For the ALMA, σ will be 25 micrometers or less.

Other design decisions that affect the array sensitivity include the correlator quantization, which enters the computation through ϵ_q. This is a complicated issue that depends on exactly how one digitally samples the analog IF waveform from each antenna. However, modern multi-level, multi-bit correlators nearly recover the entire analog sensitivity, $\epsilon_q = 0.95$. The ALMA has $N > 30$, $D = 10$ meters, an IF bandwidth of 8 GHz per polarization, $\sigma = 25$ micrometers, and two orthogonal polarizations observed simultaneously. Putting all this together and assuming we observe for one minute ($\Delta t = 60$ sec), the sensitivity, equation (1), becomes

$$\Delta S < 0.0013 T_{sys}/\epsilon_a \text{ mJy}\,, \tag{3}$$

The contribution of atmospheric emission and absorption is all contained in the expression for the system temperature, T_{sys}. Additionally, T_{sys} depends upon the antenna design through the blockage contribution to the sidelobe level that results in a spillover contribution to T_{sys}. We refer T_{sys} to a point outside the terrestrial atmosphere and compute it as

$$T_{sys} \;=\; T_{RX}e^{\tau_o A} + \epsilon_\ell T'_{atm}\left(e^{\tau_o A} - 1\right) + (1 - \epsilon_\ell)\; T'_{sbr}e^{\tau_o A} + T_{CMB} \;, \tag{4}$$

where all the primed temperatures are radiation temperatures corrected with the Planck function, $T^1 = h\upsilon/k/[\exp(h\upsilon/kT) - 1]$, where T_{RX} is the receiver temperature, $\tau_o(\upsilon)$ is the zenith value of the optical depth, A is the airmass of the observation, T_{atm} is the ambient air temperature, T'_{sbr} is the received temperature for rear spillover, antenna blockage, and ohmic losses, and ϵ_ℓ is the warm spillover efficiency of the antenna.

The expression for T_{sys} can be simplified if the physical temperature of the antenna and the ground is equal to the air temperature. In this case $T'_{atm} = T'_{sbr}$ and we can express T_{sys} as

$$T_{sys} = T_{RX}e^{\tau_o A} + T'_{atm}(e^{\tau_o A} - \epsilon_l) + T_{CMB} \;, \tag{5}$$

Here it can be seen that T_{sys} will be small when T_{RX} is small as possible (i.e. build quantum-limited receivers) and when $e^{\tau o A}$ is equal to ϵ_l. Since $e^{\tau o A} \geq 1$ while ϵ_l is ≤ 1, this latter condition obtains only when $e^{\tau o A}$ is close to unity—that is, when the atmospheric opacity is very low—and when ϵ_l is also close to unity—that is, nearly all the antenna power goes in the forward direction. This is the origin of three fundamental design goals for the ALMA:

- Quantum-limited receivers;
- Low-blockage antennas so the power is directed forward;
- Site with very low atmospheric opacity.

If these objectives are met, for the Chile site the ALMA continuum sensitivity will be $\Delta S = 0.1$ mJy and 0.2 mJy at 230 and 345 GHz, respectively, in one minute of integration.

The combination of requirements for a high-tech instrument such as the ALMA to be located at a remote site means that great care must be taken in the design of the array. The ALMA instrumentation, in addition to being technically superior to instrumentation now in use for millimeter wavelength astronomy, will also need to be exceptionally reliable so as to minimize the failure rate, and it must be modular so that it can easily be removed when necessary for repair at a laboratory located at lower elevation than the array site and perhaps very distant from that site as well.

The final general technical requirement for the ALMA, also to be addressed in the Design and Development phase, concerns ease of scientific access. Recognizing that the ALMA will be extremely fast—images of small fields can be done in minutes—and suitable for an extremely wide range of scientific investigation, astronomers will need to see images as one of the principal, nearly real-time, data products from the array. This goal should not remove the ability of the sophisticated synthesis astronomer to refine his or her image through subsequent processing, but it should allow non-expert astronomers to use the ALMA effectively.

This requirement involves development of instrumentation and software that is not part of currently operating radio synthesis instruments.

The technical objectives of the ALMA project are summarized in Table 1.

Table 1. Summary of ALMA Specifications

Array	
Number of Antennas	> 30
Total Collecting Area	> 2500 m^2
Angular Resolution	0″.07 λ (mm)
Configuration	
Compact	70 m
Intermediate (2)	250 m, 900 m
High Resolution	3000 m
Antennas	
Diameter	10 m
Precision	< 25 micrometers RSS
Pointing Precision	0″.8 RSS
Fast Switching	Cycle < 10 seconds
Total Power	Instrumented
Transportable	By vehicle with rubber tires, on roads
Receivers	
28 - 45 GHz HFET	T(Rx) < 20 K
67 - 95 GHz HFET	T(Rx) < 40 K
91 - 119 GHz HFET or SIS	T(Rx) < 50 K
125 - 163 GHz SIS	T(Rx) < 6 hν/k SSB
163 - 211 GHz SIS	T(Rx) < 6 hν/k SSB
211 - 275 GHz SIS	T(Rx) < 6 hν/k SSB
275 - 370 GHz SIS	T(Rx) < 6 hν/k SSB
385 - 500 GHz SIS	T(Rx) < 6 hν/k SSB
602 - 720 GHz SIS	T(Rx) < 8 hν/k SSB
Dual Polarization	All receivers
SIS Mixers	
Image Separating	All SIS frequency bands
Balanced	All SIS frequency bands
Integrated with IF amplifier	All SIS frequency bands
Intermediate Frequency (IF)	
Bandwidth	8 GHz, each polarization
Correlator	
Baselines	> 600
Bandwidth	16 GHz per baseline
Spectral Channels	4,096 per IF

Continuum Sensitivity of the ALMA Example: Redshifted Dust Emission from Young Galaxies

One of the key science drivers behind the definition of the ALMA has been interest in detecting the redshifted dust emission from galaxies that have experienced the formation of their first stars. As mentioned in the ALMA proposal, the prospects for detecting such galaxies are favorable because the shape of the Rayleigh-Jeans spectrum means that as we view more distant and hence highly redshifted galaxies, we observe their dust emission at increasingly shorter rest-frame wavelengths. The fact that the "K-correction" is so strongly in our favor for thermal dust emission means that, as we observe objects at greater distances, the $1/r^2$ distance effect is essentially fully compensated by the K-correction, and distant galaxies will appear nearly as bright at millimeter and submillimeter wavelengths as are the galaxies nearby. We can exploit this situation to our advantage.

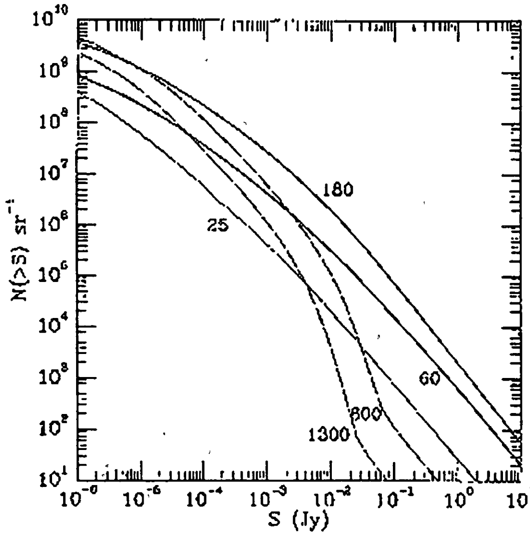

Figure 1. Integral counts of galaxies at the wavelengths shown.
The curves are labeled with the wavelength in microns.

How many galaxies are likely to be detectable? The IRAS survey allows us to make such an estimate since we can construct a far-IR luminosity function, refer this to models of stellar formation and stellar evolution and then carry the calculation out to high redshift (earlier epochs). Such a detailed calculation has been made by Franchesini, et. al., (1991). Their Figure 11, showing the number of galaxies per steradian as a function of flux density for various wavelengths between 1300 and 25 microns, is reproduced here as Figure 1. Let's just consider two of these wavelengths, 1300 and 800 microns, which correspond to the ALMA bands. Integrating over the whole sky, Table 2 gives the number of IR galaxies we expect as a function of flux density for these two ALMA wavelengths.

Table 2. Cumulative Number of IR Galaxies

Flux Density (mJy)	Frequency	
	230 GHz[1]	345 GHz[2]
1	$1.5x10^7$	$7.5x10^7$
0.1	$3.8x10^8$	$1.6x10^9$
0.01	$5.0x10^9$	$1.5x10^{10}$

[1] 1300 microns wavelength.
[2] 850 microns wavelength.

Over the whole sky there are more than 15 million such galaxies detectable at a level of 1 mJy or greater at 230 GHz, and there are more than 75 million detectable at the same level at 345 GHz.

We can compare Table 2 with the ALMA continuum sensitivity for the same two wavelengths for integration times of only one minute and we see that at an observing frequency of 230 GHz, the ALMA can detect the thermal dust continuum emission from 15 million galaxies at a signal to noise ratio of ten or greater. At 345 GHz, 75 million galaxies are detectable in one minute of observing at a signal to noise ratio of greater than five. This is a very rich scientific field indeed.

It is so rich a field of research that the redshifted dust emission from galaxies at cosmological distances will be a significant source of "confusion." In Table 3 we give the number of such confusing sources as a function of flux density expected in the primary beam of the ALMA 10 meter antennas.

Table 3. Galaxies Seen as Confusing Sources of Redshifted Dust Emission per ALMA Primary Beam

Flux Density	Frequency	
(mJy)	230 GHz	345 GHz
1	0.035 galaxies/beam	0.077 galaxies/beam
0.1	0.92	1.6
0.01	12	15

Examining Table 3, the continuum sensitivity of the ALMA, we see that in one minute of observing time the rms sensitivity is 0.1 mJy/beam, but at that same flux density level we can expect one "confusing" source of redshifted dust emission somewhere in the primary beam. With a hour of integration we expect ten or more such confusing sources per beam. Finally, with integrations of ten hours or more we can expect to reach a sensitivity that Franchesini, et al., conclude will be adequate to detect essentially every galaxy in the universe.[3] The ALMA 0".1 angular resolution will allow us to identify each such galaxy and discriminate each from its neighbors.

Organization of the ALMA Project

The ALMA Project is an initiative of the NRAO and is organized around a Project Manager with a staff assigned specifically to the Project. The Project Manager is assisted by Division Heads who have responsibility for specific areas of technical development.

It is expected that nine years will be needed to build the ALMA. An initial 3-year Design and Development (D&D) phase will be followed by six years of construction. The D&D work began in 1998. It will concentrate on construction of prototype hardware and the evaluation of technical options. Its output is a thorough description of the array to be constructed and a careful estimate of the construction cost of that array.

Role of the Atacama Large Millimeter Array Development Consortium

Millimeter wavelength synthesis astronomy was pioneered by the Radio Astronomy Laboratory (RAL) at the University of California and by the Caltech Owens Valley Radio Observatory (OVRO). The early RAL effort was subsequently broadened to include the University of Illinois and the University of Maryland under the aegis of a consortium called the Berkeley-Illinois-Maryland-Association (BIMA). Recognizing that the ALMA will benefit from the experience of astronomers and staff at the operational OVRO and BIMA millimeter wavelength facilities, an agreement was forged among the NRAO, BIMA, and OVRO to form the Atacama Large Millimeter Array Development Consortium (MDC). As described in that agreement, the MDC provides overall direction for the development phase of the ALMA. The MDC is managed by an Executive Committee of four members—two representatives from the NRAO and one each from OVRO and BIMA—appointed by the participating institutions and reporting to the Director of the NRAO. The MDC institutions participate in the D&D phase of the ALMA by working as full partners in the development tasks specified in the ALMA Program Plan. It is the expectation that the MDC will be an effective mechanism not only to recruit to the ALMA project the experience of university-based individuals but also to stimulate the long-term vitality of millimeter wavelength astronomical research and development at the universities.

Role of International ALMA Partnerships

Among the *deliverables* of the ALMA Design and Development initiative is an agreed partnership in the array by foreign countries or by U.S. agencies other than the NSF. Two partnership possibilities may have a significant effect on the ALMA planning. These are the possibility of joining the ALMA with the Japanese project, the Large Millimeter and Submillimeter Array (LMSA), or with the European Large Southern Array (LSA). Both of these initiatives have considerable support among their respective scientific communities and the leaders of both have expressed interest in discussing how their projects could be joined with the ALMA to the benefit of all. Either combination with the ALMA, or better, a combination of all three, would provide such a truly powerful imaging instrument that the U.S. community has been supportive of efforts by the ALMA staff to secure such a partnership.

The Future

With the initial funding for the ALMA Design and Development effort we are entering a very exciting time for astronomy. The ALMA holds the prospect of providing unobscured images of stars, planets, and galaxies as they form. These images will have an unprecedented clarity of detail that will permit astronomers to answer the vexing questions that can be posed today and to pose questions that will challenge the students and scholars of tomorrow.

End Notes

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The Future of Computing

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Historical Perspective

It is instructive when discussing the future of a subject to observe how it has prospered in the past. It is also, of course, safer for anyone foolish enough to prepare a talk on a subject which changes as rapidly as computing to spend as much time rehashing the past as possible to minimize the number of foolish predictions which end up in print!

Perhaps the most relevant statistic for this audience is the "speed" of the VLA. If you take the ratio of the number of image pixels per day that the VLA was designed to produce, and compare it to the peak pixels per day the VLA has actually produced[1], you conclude that the VLA is ~3000 times faster than its design. Assuming a constant (exponential) improvement, this is a doubling time of ~2 years. While I wouldn't expend much energy defending this particular method of calculating the relative speed of the VLA, or the exact value of the speedup, the fact remains that the VLA is very much faster than it was designed to be.

I think it is fair to state that most of this speedup can be attributed to computing in one guise or another. Algorithmic advances made an enormous contribution, particularly the development of Barry's "Clark Clean," as well as the (re)discovery of self-calibration. The brute-force improvement in processing speed also played a role. The available processing was also effectively multiplied by the widespread adoption of AIPS, which allowed astronomers to compute at their own institution away from NRAO, thus allowing more computers to be brought to bear on the problem.

It is well known that computer hardware has been improving at a remarkable rate over many decades. Moore's Law states that the number of transistors doubles every 1.5 years (for a fixed price!). This has remained remarkably true over nearly 30 years, although perhaps the doubling time is a bit closer to two years now. The correlation between transistor count and capacity is direct for memory, but even for CPU's the increased transistor counts have more-or-less translated directly into processing speedups, helped no doubt by the increasing system clock speeds.

Improvements in disk capacity over the last 10 years or so have been even more impressive. The disk capacity/$ doubling time is about 1.5 years. However the single disk performance (Mbytes/s) over about the same period of time has had a doubling time of "only" about 2.5 years. Compared to processing speed then, disk capacity has more than kept up, but performance has lagged somewhat.

These doubling times are especially impressive when compared to other exponential rates we are used to in normal life. For example, the growth engine of many retirement plans, the stock market, grows with an average doubling time of 7 years. And computing environments have not (yet!) had to face major declines!

Amongst all this good news there has to be some bad news, and I'm sure you have already anticipated it: *software*. While a doubling time for software is even more fanciful than that for the VLA, if you look at the length of time between new paradigms (*e.g.*, minicomputer to GUI to the internet), and new software processes (structured programming, object-orientation), to the size of large software systems (*e.g*, OS/360 *vs.* Windows NT) it is hard to argue for a number less than 10 years.

Although an exponentially rising tide may raise all computers, it can drown a pundit: small bumps can obliterate obvious trends. Proceed with caution if you are reading this near the date it was written (1998), and with forbearance if you are reading it much after.

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[1] Thought to have occurred during the NVSS survey.

Table 1 - Doubling Times

VLA	2 years
CPU	2
Memory	1.5
Disk – capacity	1.5
Disk – performance	2.5
U.S. Stock Market	7
Software	10

Future: CPU and Memory

It is generally believed that CPU's have at least 10 more years of improvements on the same technology path. The performance improvements are expected to come from a combination of the following:

1. Transistor counts.
2. Clock increases (L-band system clock rates are not that far away!).
3. Improved instruction level parallelism. This occurs when the CPU can arrange to do some operations in parallel, using techniques such as speculative execution where the CPU will execute down both sides of a branch, "collapsing" the wrong branch when the actual result is known.

During this same period, memory capacity should continue apace. However, memory *performance* is lagging. Memory was once only a few clock cycles away from the processor, now it is 100+ cycles away, and the situation is not improving. Memory access is now much like disk access – cache misses can be horrendously expensive and programmer attention is often required to avoid these cache misses.

Future: Disk

Disk capacity will continue to increase at an astonishing rate. The much-predicted ascendance of optical media over magnetic media will continue to remain in the near future for the foreseeable future. The single-disk performance will continue to lag (disks won't spin that much faster). To alleviate this performance bottleneck disk striping will become ubiquitous. Striping may ultimately be provided directly by the machine and OS, as opposed to an external RAID device.

Synthesized Supercomputers – *The Pipeline Strikes Back*

The dominance of parallel computing has been in the near future for at least as long as the final victory of optical media over magnetic. However parallel computing will finally become prominent for at least some application domains, including radio astronomy. Radio astronomy is blessed with a number of *embarrassingly parallel*[2] problems, in which independent "chunks" may be processed without much synchronization overhead. Processing spectral line channels in parallel is an example of such a problem.

For such problems, the cheapest way to obtain a "computer" capable of performing a large number of calculations is to wire many commodity PC's (either boards in a rack or entirely separate computers) together with a fast commodity network (*e g.*, Gigabit Ethernet) in a network topology makes sense for the problems to be solved. Note that these fast networks are not that much slower than the local bus, and also that if each node has local disk, parallel I/O may be performed in a fairly natural fashion.

Such systems, the best-known examples of which are Linux-based and known as *Beowulf* systems, are starting to appear at large institutes. I believe that they will become commonplace at institutes like NRAO within 5 years. I hope when this happens that we name some of the nodes *gridder*, *mapper*, ...

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[2] Believe it or not, that's a technical term!

Future: OS

Much to the irritation of some of my programming colleagues, I have been predicting for some time that a Microsoft operating system (probably the post Windows NT/9x OS) will supplant Unix for general scientific use. Religious arguments aside, the reasoning seems fairly compelling:

- Unit shipments from Unix workstation vendors are at best flat, and in many cases declining.
- Even though Linux is growing at an impressive rate, Windows NT is growing at an even more rapid rate (~40% per year *vs* ~25% per year).
- There are now high-performance systems that run Microsoft operating systems (conversely of course, Unix/Linux is available on commodity PC hardware).

One could argue that the major reason that Windows has not already made major inroads in astronomy is that there are not yet any major astronomy data processing packages available under Windows. Certainly many astronomers already use Windows applications for data analysis and other tasks, and these astronomers would probably be happy to live entirely in the Windows universe if suitable data-processing packages were available for Windows.

In general, Unix is considered to be the better OS, and Windows is considered to have the better applications. Without a strong performance driver, I believe that the attractive Windows applications will gradually drive our market to the steadily improving Windows OS.

A very interesting experiment is now taking place. Netscape has "freed" the source code for their web browser giving the open software movement a head start with a flagship application. It will be interesting to see if the open software movement can keep this browser at the forefront. If so, it might presage a movement towards the development of more "glittery" applications (*e.g.,* office suites) that are of great interest to users but have not attracted much interest amongst open software developers. I think the fairly rapid development of such applications is the only hope for those who would like to see Unix retain its dominance in our community.

Future: Peripherals

There are a number of mundane improvements in peripherals that are easy to imagine (more pixels, flat screens, virtual reality, voice input, *etc.*). However the most interesting possibility I have read about is a peripheral that apes a pre-electronic storage mechanism which has not been overtaken even by magnetic media: the book.

Probably a large number of people have thought about creating a thin-screen LCD with local memory into which a large number of books could be stored. There are three commercial products that I know of that are now coming to market that implement this idea. (However they do not have enough pixels: for easy reading at least 5k pixels on a side are probably needed, such screens must be at least several years in the future).

Real books still have advantages over such devices. Notably, flipping through pages is still a remarkably effective method for finding information. If *Electronic Ink*, currently under development at the MIT Media lab pans out, we may be able to have real books with paper (or at least flexible!) pages, the content of which is loaded from an on-book memory, which in turn could be downloaded from the network.

Electronic ink consists of two-colored bipolar particles, which can be flipped by changing the electric field. These are very low power devices since you only have to apply the field when you want to change the color of the ink particle. At present, the size of the electronic ink particle results in about 100 dots-per-inch, which is too coarse for easy reading. It is believed that 500 *dpi*, which is suitable, is achievable. Electronic ink should also allow animations at about 20Hz.

Future: Software

There is no question about it: software can be nearly magical – it can automate tedious tasks, perform calculations that would otherwise be impossible, and give one a chance to change fonts for no good reason[3].

There is also no question that software can be a terrible tar pit. If we presume that $effort \sim size^x$, then if the software creation process exhibited any economies of scale one would hope that $x<1$. If the communications complexity was constant, one might hope for a linear, $x=1$, process. In fact, the rule of thumb for software is that $x>1.2$! This is a terrible penalty to pay for ambition!

Moreover, even with this empirical rule, it is notoriously difficult to predict how long it will take to produce new software. The largest software companies routinely miss their shipping dates for their most prominent projects by months or years. A recent Scientific American article[4] states:

> *Studies have shown that for every six new large-scale software systems that are put into operations, two others are canceled. The average software development project overshoots its schedule by half, larger projects generally do worse. And some three quarters of all large systems are "operating failures" that either do not function as intended or are not used at all.*

Fundamentally, software has to be turned from a craft to an Engineering[5] discipline. Unfortunately the software development process does not yet have a well enough understood theoretical basis to turn it into one. Even if you were to believe that it does, three quarters of institutions are on the lowest rung of the *Software Engineering Institutes* maturity model, *i.e.*, most software is developed in an *ad-hoc*, non-repeatable fashion.

Until the process becomes better understood, I do think there are some Band-Aid technological solutions that will help. It is well known that the number of lines of code that a developer writes per day is roughly constant, regardless of the language being used. This implies that if we can arrange for each line to do a lot of "stuff," we can reduce the size of the systems being implemented. Given the high penalty that accrues to large-systems, this should help to keep development times under control. There are two complementary ways of achieving this, both of which I believe will become more common in the coming years.

1. Distributed Object Systems: These essentially allow existing software systems to be repackaged and reused at a very high level. One way to think of this is that they allow a common software *bus*, whereas traditional object-oriented systems provide software *IC*s. That is, a higher level of packaging is achieved.
2. Dual-level Systems: A scripting (glue) language, combined with a powerful component library (which might in turn be implemented as a distributed object system). Most Web-based applications are implemented using a system like this.

Despite this, I fear that software development will continue to be problematical since we are challenged to solve ever-more complicated problems, and it is not fair to ask Barry to write all our software.

Acknowledgements

We all owe Barry Clark many thanks. In particular, I thank Barry and Bill Cotton thanks for providing me with the pixel production rates that allowed me to come up with a speedup factor for the VLA.

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[3] After the introduction of TeX, documents tended to look like ransom notes for quite some time until people learned to use their newfound power with discretion.

[4] W. Wayt Gibbs, *Software's Chronic Crisis*, Scientific American, Vol. 271, 1994.

[5] Although "Software Engineer" is the most common title for practitioners of this craft, I think "Software Developer" is currently a more accurate term. It identifies that more is required than coding (hence it is also preferable to "Programmer"), but it does not acquire by fiat engineering respectability that it has not yet achieved..

FUTURE SPACE VLBI MISSIONS

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Abstract

This paper describes the history of the Space VLBI technique, and its natural extension to future Space VLBI programs. Two specific future missions, VSOP-2 and ARISE, are discussed. Finally, some speculations are included regarding the characteristics of highly advanced Space VLBI missions that might take place some time in the next century.

1 Introduction

Twenty years ago, in 1978, I came to Socorro as a VLA summer student, to help start the list of reference sources to be used for calibration. At that time, it quickly became clear that Barry Clark was the fount of all (or most) knowledge about the VLA. However, in my youth, I was sure that must be due to his advanced age, and that I would someday be just as knowledgeable. Now, 20 years later, on the occasion of Barry Clark's 60th birthday, I realize that I am older now than he was in 1978, yet I still don't know nearly as much as he did then. Something may have gone badly wrong in my learning process, but a more likely explanation is simply that he knew more than I realized in my naivete as a young graduate student.

Since the mid-1980s, I have been working on the concept of Space Very Long Baseline Interferometry (SVLBI), a natural follow-on to the ground VLBI technique pioneered by Clark and others (Bare et al. 1967; Broten et al. 1967). SVLBI now has taken its first steps, with both technical demonstrations and the first dedicated SVLBI spacecraft, VSOP (VLBI Space Observatory Programme), which was launched in 1997. This paper uses the history of SVLBI as a springboard to launch into a discussion of future missions, both near-term and in the more distant future, extending out to missions that will be conceived and carried out by today's students.

2 Classes of Astronomical Instruments

For many types of astronomical observation, it can be useful to divide the instrument classes into several different categories as a technique develops. First, there is the **Demonstration,** in which an instrument is built to demonstrate the feasibility of a particular technique, and perhaps to do a little bit of very general science. Second, there is the **Experiment,** where an instrument is constructed to make astronomical observations over a period of time, often for a number of users in addition to the instrument builders. Such a tool can be used to do some science, although it may not do science routinely, and there tends to be a big advantage to the "insiders" who know the details of exactly how the instrument works. Third, there is the **Observatory**, an instrument whose dominant purpose is to do science, and which is quite accessible to a wide variety of users who may not be expert in the particular technique.

Examples of the progression from **Demonstration** to **Observatory** can be found in radio interferometry, and are described in a variety of papers presented at this symposium. In space astronomy, a similar type of progression also can be seen. Table 1 gives some examples of this evolution in the maturity of several techniques. Section 3 will discuss the history of Space VLBI in similar terms.

Table 1. Evolution of Astronomical Observing Techniques

Technique: Connected Element Interferometry	
Demonstration	Sea Cliff Interferometer
Experiment	Green Bank Interferometer
Observatories	VLA, Westerbork, Australia Telescope Compact Array
Technique: VLBI	
Demonstration	First U.S. and Canadian Demonstrations
Experiment	U.S. VLBI Network ("Network Users Group")
Observatories	VLBA, Joint Institute for VLBI in Europe
Technique: Space X-ray Astronomy	
Demonstration	Uhuru
Experiment	HEAO-1
Observatories	Einstein, ROSAT, ASCA, Chandra

3 History of Space VLBI

The first SVLBI demonstrations took place using an element of the Tracking and Data Relay Satellite System (TDRSS) between 1986 and 1988. Serious planning for these demonstrations only began in late 1984 and early 1985, led by Gerry Levy of NASA's Jet Propulsion Laboratory (JPL). It was quickly realized that the dual 4.9-meter antennas used for TDRSS, together with the wideband (analog) downlink at either 2.3 or 15 GHz, made a demonstration of SVLBI possible. The whole affair was typical of the class of instruments labeled **Demonstration** in Section 2, relying on the driving force of one person, the technical expertise of a few hard workers (e.g., Roger Linfield and Chad Edwards at JPL; Alan Rogers, Alan Whitney, and Roger Cappallo at the Haystack Observatory correlator), and some amount of good fortune as well. This fundamental work enabled Linfield and me to find the first 2.3-GHz fringes late on a dark and stormy night in Massachusetts, demonstrating that SVLBI actually was a technical possibility. Later series of observations extended these results to many more sources, and then to an observing frequency of 15 GHz, demonstrating most of the technical capabilities for future, dedicated SVLBI missions. Results were described in a number of published papers (Levy *et al.* 1986, 1989; Linfield *et al.* 1989, 1990).

The successful TDRSS demonstrations supplied strong momentum to the proposed SVLBI observatory QUASAT (for "QUAsar SATellite"), then being considered by NASA and ESA (Schilizzi *et al.* 1984). Support in the radio astronomy community was fairly strong, but QUASAT ultimately was not funded by either NASA or ESA. A somewhat more advanced concept, IVS (International VLBI Satellite; see Pilbratt 1991), also was proposed to ESA, but was not successful in the selection process. However, the RadioAstron mission proposed in Russia was moving forward in the late 1980s, and the Japanese participants in the TDRSS demonstrations proposed VSOP as a combination of a technical experiment and a scientific mission.

VSOP and RadioAstron are good examples of the second phase of development of a technique, the **Experiment.** They each carry telescopes 8 to 10 meters in diameter, as well as receivers at several frequencies. However, the orbiting telescopes have very poor sensitivity relative to telescopes in ground VLBI arrays, and a great many constraints that restrict their versatility in observing. Furthermore, they require new techniques in a variety of areas, significant software and hardware developments at new tracking stations and correlators, and day-to-day operation of what may be the most complex space astronomy missions ever attempted. For other satellites, a large effort is required to gather a few telescopes for days or weeks to make coordinated observations of objects such as blazars. However, for SVLBI, *every day* requires such a coordinated observation, something never before attempted with an astronomical satellite. Many of the complexities of SVLBI, when compared with ground VLBI, are described in detail by Ulvestad (1998).

The HALCA satellite of VSOP was launched in February 1997, and has been operating for nearly 18 months at the time this paper is written. It makes 5 or 6 scientific observations per week, but insiders who understand the idiosyncrasies of the mission have a clear advantage in writing successful proposals and in making the most out of their data. Descriptions of some of the early results can be found in Hirabayashi (1998a,b) and Hirabayashi et al. (1998). Because of economic and other problems, launch of RadioAstron (Kardashev 1997) is not expected until 2001 or later.

4 Space VLBI Science

SVLBI, like ground VLBI, is a technique for investigating and imaging astronomical sources with extremely high brightness temperatures. In other words, it is sensitive to the objects that have strong emission on very compact, milliarcsecond scales, rather than to more distributed or diffuse sources of emission. Thus, the main targets of any SVLBI instrument are objects such as core-dominated active galactic nuclei (AGN), H_2O masers and megamasers, and pulsars. As the sensitivity increases, compact sources of somewhat lower brightness temperature become accessible. These include gravitationally lensed galaxies and quasars, radio stars, lobe-dominated radio galaxies, and weaker AGN such as Seyfert galaxies.

Primary scientific investigations for AGN cover a variety of topics associated with their innermost jets. In γ-ray blazars, the apparent speeds of these jets can be used in combination with the γ-ray properties to constrain the emission mechanisms for the γ rays as well as the geometry of the jets. Measurements of polarization across the jets can give important clues to the physical mechanism(s) collimating and confining the jets. Imaging at a variety of frequencies, often combined with higher-frequency ground VLBI images, can be used to study particle acceleration processes and to identify regions of free-free absorption by ambient gas. Studying the statistics of the small-scale emission of certain types of AGN, such as Gigahertz-Peaked-Spectrum (GPS) sources, can be used to better understand the global physics accounting for such objects.

H_2O megamasers also are a prime target for SVLBI. Unfortunately, the 22-GHz system aboard HALCA was damaged during launch, and the 22-GHz system aboard RadioAstron cannot tune to observe the redshifted water line in receding galaxies, so investigations of these megamasers must await a future generation of observatories. In such observatories, the additional resolution beyond that available from the ground should enable the imaging of individual maser spots that cannot be separated from the ground. The kinematics of the maser disks thus can be seen in more distant galaxies, enabling precise measurements of the masses of their supermassive black holes. If the kinematics can be determined in galaxies whose recession is dominated by the Hubble flow, the geometric distance measurements via statistical parallax of the masers may enable an accurate determination of Hubble's constant, giving the overall scale of the universe.

5 Future Observatory Concepts

In order to do the science summarized in the preceding section, certain mission parameters are desirable for a spaceborne VLBI observatory. The mission should have an antenna with a diameter of 15 meters or larger and a receiver temperature no greater than about 1.0 K per gigahertz of observing frequency (e.g., 22 K for the 22-GHz receiver). This corresponds to a System-Equivalent Flux Density (SEFD) no greater than 600 Jy at 22 GHz. The instantaneous data rate should be at least 1 Gbit s^{-1}, dual-polarization observing should be possible at frequencies of 22 GHz and higher, and a projected baseline range from 10,000 km to $\sim 50,000$ km should be sampled. Given ground telescopes in the 25-meter or larger class, the hypothetical SVLBI observatory would be sensitive to brightness temperatures ranging from 10^8 K to 10^{13} K or higher for continuum sources at 22 GHz.

5.1 VSOP-2

The Institute of Space and Astronautical Science (ISAS) in Japan has formed a working group to define a VLBI observatory that is currently referred to as VSOP-2. It is likely that a formal proposal for such a mission will be made to ISAS management in the year 2000, with a possible launch date of 2006 or 2007. The mission is not yet defined in detail. However, the current aim is for the launch of a 10-meter antenna into an orbit fairly similar to VSOP, which has an apogee height of about 21,500 km. (Both the antenna size and the apogee height are constrained by the capabilities of the M-V launch vehicle.) The antenna technology to be used for VSOP-2 is not yet determined, but the observing frequencies would be as high as 22 GHz, and possibly 43 GHz. With cooled receivers, the SEFD is projected to be ~1400 Jy at 22 GHz, and slightly lower at 5 GHz. The nominal data rate of 1 Gbit s^{-1} then would give a 5-GHz source detection threshold more than an order of magnitude better than VSOP.

Although VSOP-2 is poorly defined as yet, it has a good chance of being approved by ISAS for a launch in 2006 or later. With the benefits of the substantial experience gained with VSOP, it is likely that VSOP-2 will be the world's first dedicated SVLBI mission of the **Observatory** class.

5.2 ARISE

ARISE (Advanced Radio Interferometry between Space and Earth) is another concept for a future SVLBI observatory that is currently under consideration at NASA. It is on the long-range roadmap of NASA's Structure and Evolution of the Universe theme, with a potential launch in 2008 or later. The mission concept is to orbit a radio telescope with observational properties similar to those of a VLBA antenna—a 25-meter radio telescope with observing capabilities at frequencies up to 86 GHz. Development of such a large space-deployable telescope using conventional technologies would be both difficult and very expensive, so the baseline concept for ARISE makes use of an inflatable antenna; inflatable structures technology is currently advancing rapidly because of its uses for a variety of applications, such as Earth-sensing radiometry and solar-power concentration. Another advantage of the inflatable structures technology is the extremely light weight, permitting launch with a relatively inexpensive launch vehicle. The main difficulty will be developing the technology, either with accuracy of the primary surface or with correcting optics (deformable secondary or electronically adjustable array feeds), to a point where the inflatable antenna can operate at the high frequencies desired for ARISE.

ARISE would use cooled receivers with receiver noise temperatures of $\sim$ 0.5 K per gigahertz of observing frequency. The corresponding SEFD for a 25-meter telescope with reasonable aperture efficiency at 22 GHz would be $\sim$ 100 K, much better than the current values of $\sim$ 900 K for a VLBA antenna. A space antenna more sensitive than the typical ground radio telescopes would be a great advantage in imaging, since the correlated signal strength will usually be lower on the space-ground baselines than with ground VLBI arrays; the higher sensitivity of the ARISE antenna then would provide similar signal-to-noise ratios for the space and ground baselines. The data rate for ARISE would be in the range of 1–8 Gbit s^{-1}, and the currently favored orbit would have an apogee height near 40,000 km, with the final selection to be made based on scientific results from VSOP and RadioAstron. A nominal set of mission requirements for ARISE is given below, in Table 2. Further information on the ARISE concept can be found in several published references (Gurvits, Ulvestad, & Linfield 1996; Ulvestad, Gurvits, & Linfield 1997; Ulvestad & Linfield 1998).

Table 2. Top-Level ARISE Mission Requirements

Quantity	Requirement	Origin
Antenna Diameter	25 m	mJy sensitivity
Antenna Accuracy (corrected)	0.2–0.3 mm	$\lambda/16$ at 86 GHz
Antenna Pointing	$\leq$ 3 arcsec	$\leq$ 3% loss at 86 GHz
System Temperature	10 K–40 K	mJy sensitivity
Data Rate	$\sim$ 8 Gbit s^{-1}	mJy sensitivity
VLBI Frequencies	5–8, 22, 43, 86 GHz	Standard VLBI bands
Single-dish Frequency	60 GHz	O_2 in star-forming regions
Apogee Height	$\sim$40,000 km	15–100 μas resolution
Lifetime	$\geq$ 3 yr	Source monitoring
Total Mass	$\leq$ 1700 kg	Affordable launch vehicle

6 The Far Future

One of the difficult choices for any SVLBI mission is the selection of the optimal orbit. High orbits are important for achieving the highest possible angular resolution. However, they suffer the disadvantage of opening up large holes in the aperture-plane, or (u,v) coverage; in addition, compact sources may vary significantly during a multi-day orbit, possibly causing severe imaging limitations. Low orbits reduce these problems, enabling imaging with higher dynamic range, but they do not achieve resolution substantially higher than that accessible with ground VLBI.

SVLBI missions in the distant future will surely attempt to overcome these disadvantages by orbiting multiple spacecraft in a single program. This will enable the high resolution of very long baselines, while spacecraft at intermediate heights will help fill in the (u,v) plane. Orbits to lunar distance and even beyond may be used. However, putting a VLBI antenna on the moon is unlikely, since the long circular orbit around the Earth would be undesirable for imaging.

Simple, lightweight spacecraft will be desired, so the inflatable technology is probably the leading candidate for the space antennas. It is likely that the antenna size will vary with orbit height, with a larger antenna desired for the highest orbits, where the correlated flux density will be lowest. Since the mass of the inflatable antennas can be quite low, and the surface accuracy does not appear to depend strongly on size, it may be possible to orbit antennas 50 or even 100 meters in diameter. Ultimately, observations at 86 GHz should be straightforward, and even higher frequencies such as 300 GHz (1 mm wavelength) may be attempted. With multiple telescopes in orbit, sweeping out the aperture plane quite rapidly, it also may be possible to dispense with the ground radio telescopes and have a purely spaceborne SVLBI array.

Since there will be spacecraft in quite high orbits for a far future mission, data communication to the ground will be difficult, in view of both the great distances involved and the need for a very large communication bandwidth. Hence it is quite likely that the traditional microwave links will be replaced by optical communications using lasers. In fact, it is easy to imagine real-time data transfer among the satellites, with correlation taking place in space, and only the results of that correlation sent to the ground.

Most interferometric radio telescopes on the ground have the word "Array" in their name (e.g., VLA, VLBA, ALMA), but the use at radio wavelengths seems to be hidden as far as the names are concerned. Of course, any reasonably large system of VLBI telescopes in space should have the words "Radio" and "Array" in its name, leading to the following possible names for the far future SVLBI system:

- **BARR(I)** = **B**ig **A**rray of **R**evolving **R**adio **I**nterferometers
- **CLARK** = **C**osmic **L**arge **A**rray for **R**adio **K**nowledge

Although large VLBI arrays in space are not likely in the near future, the prospects for such high-resolution SVLBI systems sometime in the next century are quite good, as we continue to need more advanced ways to study the astrophysics of compact objects.

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Afterword: 40 Years Ago Today, Barry Clark Was...

D. E. Harris

Preamble

Wow! Miller said we could write anything we pleased: no page limit, no abstract, no mandatory Introduction, Observations, Summary, Acknowledgments, and References! No referee! Of course at that point, "writer's block" sets in so, as with all other papers for conference proceedings, it is only when the deadline approaches that something gets written. A week prior to the Clark 60th, I attended a symposium for Joachim Trumper's 65th birthday, at which, it seemed, every speaker had to relate how young JT was when the speaker first met him, and then tell a funny story that made JT look a bit silly compared to the speaker. My talk was near the end of the meeting which was lucky since I had to admit that I had never actually met Trumper at all. But for Clark, without getting into actual dates, I have decided to relate a couple of stories about Barry before he became a legend.

How Can We Know What Really Happened?

The aspect of the '98 June party for BC that caught me by surprise was the "listening to" of other people's stories. I soon came to realize that the truth, as firmly lodged in my head, is a fiction, and that there is no way that one can be sure of what actually happened if we have only our memories to guide us. I have a favorite BC story that I have told many times over the years without embellishments of any kind (I only made up the actual number -- which is unimportant). Before it was my turn to talk, another participant told my story, but changed it in several particulars, and it had become the participant's story, not mine! And I distinctly remember that Barry and I were the only two at the OVRO that weekend in 1958 or 1959.....

Barry Climbs Black Mountain
version by DEH

> One bright sunny Sunday morning, Barry said he would climb Black Mountain (the closest peak to the east of the OVRO). He may have stuffed some food in a pocket, but the memory I have of Barry's departure is a figure with a quart milk carton (full of water) in each hand striding across the desert floor, on a rhumb line towards the peak, without regard for any intervening sage or gully.
>
> He returned before sunset; I went out to meet him, anxious to hear about his climb. Barry looked at me and said simply, "Forty three thousand, five hundred and sixty three." I said, "Forty three thousand, five hundred and sixty three WHAT?" "Steps" was the only reply.
>
> Over dinner, Barry explained that he had been using his fingers to count in binary, and that after a short training period, his fingers operated without any conscious intervention by his mind which was then free to contemplate the normal affairs of a mountain climb. Obviously a very well developed hypothalamus!

Figure 1 shows Barry on that fateful evening (well, actually, he was not holding computer paper tape at the time, and he was not so well dressed, and he was not in Pasadena at the Robinson lab....but it is Barry!)

Tree Vignettes From the Sierras

Barry and I did a bit of climbing in the Sierras. In an after-dinner talk at the 60th, I explained how I owed my life to Barry because he finally stopped dropping rocks on me. We were on the East face of Whitney; we had just finished the so called "fresh air traverse" and I was tied in slightly above a thousand foot drop-off. Barry was leading that pitch and he kept dislodging sizable rocks as he worked his way over a series of ledges called "The Grand Staircase." The rocks would take a few bounces on their way towards me, and then, permanently airborne, disappear into the abyss that was just out of view. I found this rather disconcerting and initiated a verbal interchange on each occasion. Thank you, Barry.

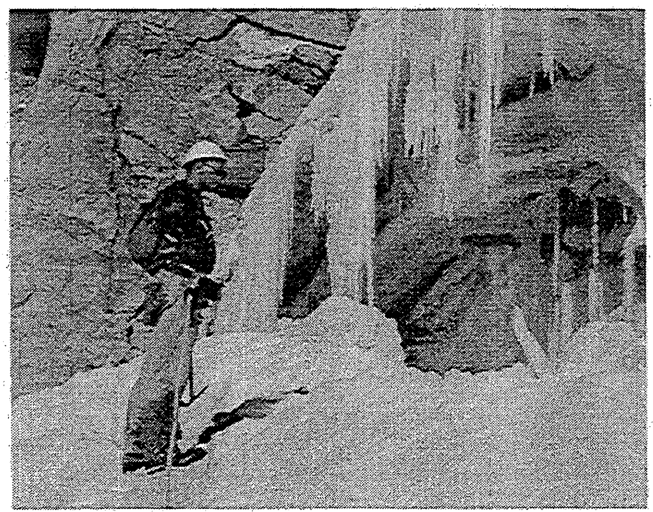
Figure 1. B. Clark, hastening the demise of punched paper tape.

On a longer trip with several others, I remember that we were overtaken by bad weather. It was cold. It was raining. It was miserable. We were strung out for some distance. The picture I have at the moment of deepest despondency ("What the hell am I doing here?") was coming around the corner and finding Barry sitting under a huge bolder with his mountain stove roaring; frying large slices of SPAM which he passed out to each of us. On that day SPAM was the best thing that could happen to a cold bedraggled hiker! Figure 2 shows Barry solving some of the serious affairs of the universe, having returned to Pasadena and having had a nice hot shower.

The last trip in this trilogy was to North Palisade, that hot afternoon in August when we attempted the summit via the couloir. [The guidebook says very plainly that this climb should be done early in the season and early in the day to avoid rocks released by melting snow.] It was on this trip that I took the picture entitled "Barry contemplates the bergschrund" (figure 3). Melbourne and I went over the bergschrund and up into the couloir. On the 40-degree incline of hard ice, with roughly 1 rock per minute whistling past us to thud onto the glacier below, we decided Barry and the others were much wiser and rappelled back to the glacier, leaving the ascent of the peak for another day. Funny thing...although I have not heard competing versions of these vignettes, Barry himself admits to remembering only the Whitney episode. In spite of the photographic evidence of Barry contemplating the bergschrund, he will not admit to participating in this witless adventure!

Figure 2. (above) B. Clark making eye contact with a fly on the ceiling. Other participants are V. Radhakrishnan and K. Wilson.

Figure 3. (below) Barry Contemplates the Bergschrund. North Palisade Glacier, August 1959.



The Moving Finger Writes, and Having Writ, Moves on....

On particularly hot days at the OVRO, Barry and I would jog up the jeep road that ran north from the observatory. After about a half mile, a smart scissor kick over a barbed wire fence was called for (at least until the day a slight misjudgment resulted in a long bloody slash on the heel). After another 1/2 mile, our destination was reached: a large gooseneck in the rapidly running Owens River. We would take our positions on the bank 6 feet above the water, and foot against foot, Indian hand wrestle until one or the other of us was thrown into the river. It was impossible to swim against the current, but stroking downstream, we felt like Johnny W. in the first Tarzan movie. Where the gooseneck narrowed, we would haul ourselves out of the river, make the short crossing to the upstream side, and have another half mile river swim at breakneck speed. In October '98 at the OVRO 40th celebration, I revisited this site. It was overgrown with 8 foot reeds; there was no longer a river, there was no current, there was no water in evidence. Playing Tarzan 40 years earlier, we had not contemplated the eventual demise of the river. Imagining the future is not easy. Remembering the past is unreliable. That leaves us the present time as our only sure reality and this book as a memento of our appreciation to Barry.

Appendix A

Barry G. Clark: Curriculum Vitae

Date and Place of Birth:	March 5, 1938, Happy, Texas	
Marital Status:	Married Elizabeth Bass; four children	
Education:	1959 B.S., Astronomy, California Institute of Technology	
	1964 Ph.D., Astronomy, California Institute of Technology	
Thesis:	The Twenty-One Centimeter Hydrogen Line in Absorption	
Employment:	1/64-06/66	Assistant Scientist, NRAO
	07/66-06/69	Associate Scientist, NRAO
	07/69-12/76	Scientist, NRAO
	01/77-09/80	Head of Data Processing/Scientist, NRAO VLA Operations
	09/80-Present	Senior Scientist, NRAO
Tenure Granted:	July 1971	
Professional Societies:	American Astronomical Society American Association for the Advancement of Science International Astronomical Union (Commission 40) International Scientific Radio Union (Commission V)	
Principal Research Activities:	VLA and VLBA Scheduling Officer; VLBA Software Development	
Award	Barry G. Clark received the American Astronomical Society's 1991 George Van Biesbroeck Award, which honors an individual "for long-term extraordinary or unselfish service to astronomy, often beyond the requirements of his or her paid position." The award committee cited Clark's "service in the development of radio astronomy instrumentation, especially in the field of radio interferometry, and for his unique contributions to the construction of the Very Large Array."	

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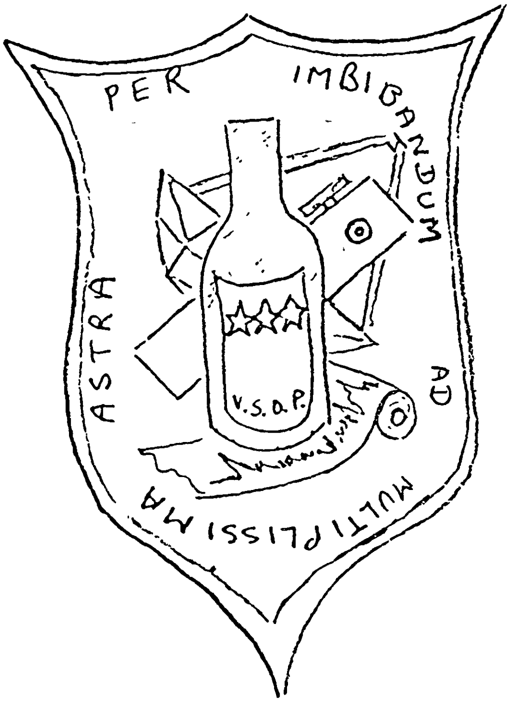
Appendix B

Additional Clark Publications

The preceding bibliography, provided by Barry Clark, for some reason does not reflect the entirety of his published works. The editors have fortunately been able to uncover some of Barry's papers that otherwise might have gone unrecognized. The following two works were published while Barry was frequenting the NRAO facilities in Green Bank (Pocahontas County), West Virginia.

JOPCAS.



Journal
of the Pocahontas County Astronomical Society

No 15 (we think)

1969

THE THINKING MAN'S 'OBSERVER'

A SECOND ORDER APPROXIMATION TOWARD THE SOLUTION OF THE PLUVIAL TRANSFER PROBLEM

Westercoot and Kaplan (1968) have attacked the famous "running in the rain" problem (the pluvial transfer problem) with a simple linear model. This paper discusses a higher order approximation which indicates the substancial correctness of the linear model by showing a qualitatively similar behavior.

The problem may be stated in the words of Westercoot and Kaplan as follows: "The problem is to transfer an individual from sheltered point A to sheltered point σ through a steady rain. What value should his velocity $\underline{V}$ have in order to minimize $\underline{W}$, the total wetness?"

Westercoot and Kaplan employed a vertical right parrellelepiped model for the individual concerned, as shown in fig 1a, and concluded that an individual so shaped should move at infinite velocity to minimize $\underline{W}$.

This theory is obviously only a first approximation, and non-linear effects must occur in practice. An infinite velocity is neither attainable nor desireable. For instance, at some velocity not exceeding 10^5 cm/sec, the rain drops will begin to penetrate the epidermis, a clearly undesireable situation.

An attempt at a more satisfactory model of a human individual is shown in figure 1b. The individual in motion is no longer upright, but makes an angle θ with the vertical, determined by the familiar areodynamic drag equation. Balancing moments about the individual's axle, we have for an individual of mass M and height h,

$$1/2\ h\ M\ g\ \sin\theta = 1/4\ h\ A_F\ C_D\ \rho\ v^2 \cos\theta \qquad (1)$$

where ρ is the density of air and C_D is the drag coefficient. We assume that the rain is light enough that the contribution of the rain droplets to the drag is negligible.

The rate of wetting is the sum of the rates on the top (area A_T) and sides (area A_F) of the individual. In this case (non-relativistic formulae are used throughout),

$$\frac{dW}{dt} = \left[A_T\ (v_{rain} \cos\theta + v \sin\theta) + A_F(v_{rain} \sin\theta + v \cos\theta)\right]\sigma$$

where σ is the number density of rain drops.

Substituting equation (1) into equation (2) and employing the dimensionless parameters

$$a_1 = \frac{A_T}{A_F} \qquad (3)$$

and

$$a_2 = \frac{A_F\ C_D}{2Mg} \rho\ v^2_{rain} \qquad (4)$$

we arrive at the specific wetness per unit length

$$W = \sigma A_F \left[\frac{a_1}{x} + a_1 a_2\ x^2 + a_2\ x + 1\right]\left[1 + a_2^2\ x^4\right]^{-1/2} \qquad (5)$$

where x is the dimensionless velocity, v/v_{rain}.

The behavior of equation (5) clearly depends on the magnitude of the parameters a_1 and a_2. If a_2 is sufficiently large, W will have a local minimum for finite velocities. $a_2 = 0$ is the case of Westercoot and Kaplan. v_{rain} is determined by an equation of the same form as eq (1),

$$m\ g = 1/2\ C_{Drop}\,\rho\ v_{rain}^2\ A_{DROP} \qquad (6)$$

With drops of 0.1 g mass, and an individual with $A_F = 6000\ cm^2$, $M = 8 \times 10^4$ g,
and $C_D = C_{Drop}$

$$a_2 = 0.029.$$

Equation (5) with $a_2 = 0.03$ and $a_1 = 0.1$ is plotted in figure 2. There is no minimum in the curve, indicating that the optimum velocity is indeed the highest available. However, the plateau at x = 2 indicates that a slightly larger value of a_2 would yield a local minimum, say for a very thin individual who runs sideways. No exhaustive search has yet been made for such an individual.

Beak Lark
Charliesville, Virgininia

References

Westercoot, G. and Kaplan, G. 1968, JOPCAS, 14, 883.

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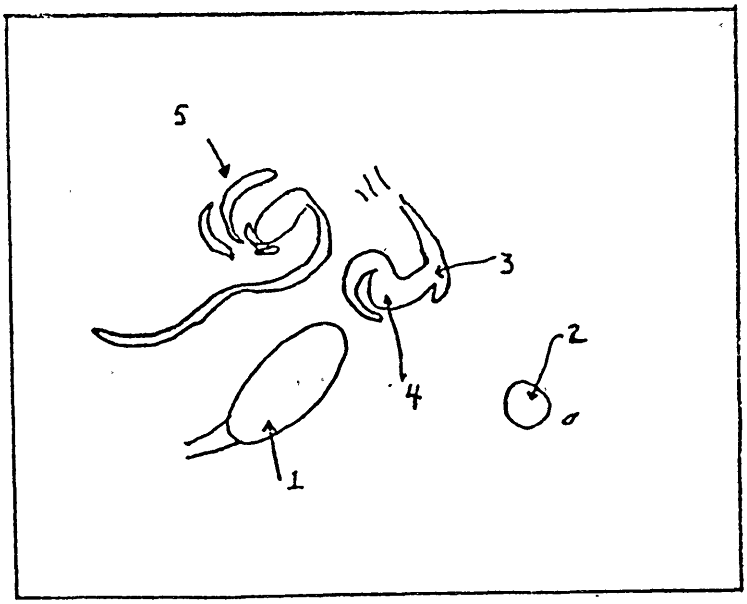
(STEPHAN'S QUINTET NUMBER)



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THERMAL INSTABILITIES

Beak Lark

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Weehawken, New Jersey

Although a seasonal variation in temperature has been noted from antiquity[1] and much attention has been devoted to the accurate prediction of such cycles (see, for instance, references 2-4), effective operative manipulations thereof were not contemplated[5]. Recently, however, local climatic modification within sealed (though not hermetically so) structures has become practical[6], and a structure known as Stone Hall[7] is so equipped. This system is controlled by the dual level feedback loop shown in the block diagram of Figure 1. The left hand loop controls only a two state switch, switching the system between thermal energy addition and thermal energy addition. This loop, although nominally provided with a high level of damping, has been known to break into oscillation due to short circuiting of a critical component[8]. This paper is a calculation of optimum loop parameters in an attempt to prevent a recurrence of this oscillation[9].

Many authors have noted the discomfort encountered under conditions of sub- or superoptimal temperatures[10]. However, the choice of a discomfort function as a function of temperature is not a widely agreed subject, neither in linearity, symmetry (is 30 degrees below optimum really as uncomfortable as 30 degrees above?), nor in zero point. The minimum discomfort point is clearly a function of activity. Clearly, the optimum temperature is lower for activities involving a great deal of physical activity (for instance discussing a colleague's most recent paper with him or writing requests for observing time) than for sedentary activities involving a great deal of manual dexterity (for instance, typing). Choosing the authority of greatest weight[11], we shall set the overall optimum temperature to be 72°.

We must now consider how the inside temperature relates to the outside. First, within the thermal enclosure there is an internal heat source of about 60 watts per square meter of wall area[12]. Assuming the thermal conductivity of the outer walls is equivalent to that of 30 cm of rock salt[13], the inside will average about 2° F hotter than the outside.

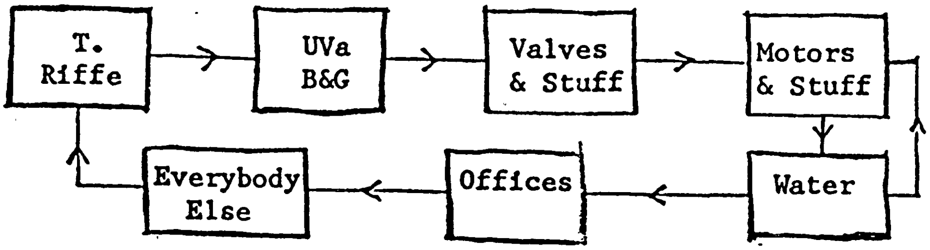


Figure 1. Stone Hall thermal environment control system.

An additional effect of the wall is to attenuate the daily temperature curve by about a factor of two and to phase shift it by about three hours. Therefore, to a reasonable approximation

$$\bar{T}_{inside} = (1/8.5)\int_{8.5}^{17} T_{inside}\,dt$$

$$= (1/2)[T_{max} + T_{min}] + (1/4)[T_{max} - T_{min}] \times \int_{8.5}^{17} \cos(t-16^h)\,dt$$

$$= 0.63\,T_{max} + 0.37\,T_{min} + 2$$

where the integral was evaluated numerically on an HP 35 high-speed digital computer[14].

We find that, during a mean spring[15],

$$T_{max} = 61.0+0.33\ (\text{Day - April Fool})$$

$$T_{min} = 41.0+0.33\ (\text{Day - April Fool})$$

from which we may readily solve

$$\bar{T}_{\text{inside}} = 72^\circ \text{ F}$$

for $$\text{Day} = \text{May } 23.7$$

However, we must remember that the "mean spring" is a statistical concept only, and does not ever, in practice, occur[16].

There appears to be very little literature about the statistical properties of daily temperatures[17], or on estimating them in advance from known properties, despite the extensive literature about the statistics of radio refractive index variations, a rather similar variable[18]. However, we can derive order-of-magnitude estimates by noting that major weather systems are about 2000 km in extent. With a mean wind of 20 km/hr[19], the autocorrelation function of temperature should decay in about four days. We thus assume a weather model[20] consisting of a hot spell with peak maximum temperature T_{max}, decaying to the seasonal mean exponentially with a four day time constant.

If we, in addition, assume a form for the discomfort function, we may calculate the integrated discomfort, and find which is the global minimum discomfort--switching immediately to thermal energy removal mode, or waiting until May 23.7. I assume the simplest suitable law--discomfort is proportional to the square of the deviance of temperature from 72° F.

Allowance must be made, of course, for the fact that any decision, once made, will be delayed by the UVa B&G intrinsic delay, assumed here to be 2.0 days.

For any day, there will be a critical temperature, T_c, above which discomfort will be minimized by immediate conversion to thermal energy removal mode. This temperature is displayed, for representative dates, in Table I, in terms of the daily maximum for that date.

Table I

Date	May 20	May 13	May 6	April 29	April 22	April 15	April 8
T_c	78	85	94	105	117	131	147

I wish to make one final note. That is, because of the thermal lag caused by the diffusion through the walls, the inside temperature is some 9° higher between noon and 8 P.M. than between midnight and 8 A.M. It is thus one of nature's most perverse tricks[22], that astronomers, who, as noted above, prefer cooler temperatures, have chosen to tailor their working habits to be in their offices at the hottest part of the day, rather than choosing to share the cool of the morning with the secretaries. It is possible that a sufficiently draconic administrative decree[23] could be drafted to reverse this situation, but the prospects appear dim. Any long term solution must take into account the possibility of the abolition of personal airconditioning as a means of stemming the energy crisis. The only viable[24] alternatives for such a situation are apparently 1) Going underground, and 2) Ceding the U.S. south of 40° N to the Seminole Indians.

REFERENCES:

[1] Imhotep, -2724. JOPCAS *2*, 497. Prior to this date, seasons being unknown, JOPCAS was not issued annually, but on the first Monday after a new moon when the dog star was less than six degrees above the horizon at sunset, unless there was water on the floor of the XEROX room due to the flooding of the Nile. The word "Quarterly" in the title did not originally refer to the frequency of publication but to the size of pieces the second reader of any given issue was likely to find.

[2] Ceasar, Julius, -26, Orders of the Day, *XVI*, LVII.

[3] Khayyam, Omar, 1121, Rubaiyat, LVII.

[4] Gregory XIII, 1582, *Tempus Incognitus*, 17.

[5] Clemens, S.L. 1880, *A Tramp Abroad*."Everybody Talks about the weather..." Sometimes attributed to Twain, M., 1880, *A Tramp Abroad*, but the Clemens reference is essentially complete and definitely earlier.

[6] Carrier,A.C. and Mary,Typhoid,1952,Advertizing Age,*318*,19736.

[7] Nonimus, Ima (pseud.) JOPCAS, 1969, *15*, n.

[8] T. Riffe (pseud.), Memo to UVa B&G. One of the few papers prevented from reaching Dr. Ellsburg, the original is believed to be among the presidential papers under subpoena by the Senate Select Committee.

[9] There is no reference for this sentence. Or excuse either, for that matter. However, if the footnote were deleted, I would have to renumber all of the following ones.

[10] See, for instance, Alighieri, Dante, 1321, <u>Comedia Divina</u>, 1.

[11] My fourth grade teacher, if you really must know.

[12] Obtained by dividing 30 watts/meter times the linear measure of the fluorescent lights in my office by its wall area.

[13] It was at the top of the list in the Chem Rubber Handbook.

[14] I wish to acknowledge the programming assistance of the Mano Derecha Digital Group.

[15] The mean spring comes from a pamphlet picked up at a real estate office eight years ago.

[16] One the other hand, the winter of 1932-33 was a mean winter.

[17] At least there isn't much under "temperature" in the library card file.

[18] See any VLA report, for instance.

[19] See note 16. 20 km/hr is not very mean at all.

[20] Lark, Beak, 1969, JOPCAS <u>15</u>, n 11. This reference is quite irrelevant, really, but it scores me an extra point in counts of citations.

[21] A constant term, which varies from individual to individual and is a measure of their grouchiness at optimum temperature is of no importance in this analysis.

[22] This, however, cannot be laid to the principle of the Innate Perverseness of Inanimate Matter.

[23] See, for example, the Draft Decree of Darius the Persian, or Oliver Cromwell's statement after the seige of Drogheda.

[24] That is, those alternatives whose names do not end in "-cide".

Appendix C

Symposium Attendees

Jaap Baars	Univ. of Massachusetts
Don Backer	UC - Berkeley
Durgas Bagri	NRAO-Socorro
Mike Balister	NRAO-CV
Tom Bania	Boston Univ.
Peter Barnes	NRAO-Socorro
Frank Bash	UT/McDonald Obs.
Tim Bastian	NRAO-Socorro
Tony Beasley	NRAO-Socorro
John Benson	NRAO-Socorro
Glenn Berge	Caltech
Steve Blachman	NRAO-Socorro
Robert Braun	NFRA
Walter Brisken	NRAO-Socorro
Bill Brundage	NRAO-Socorro
Alan Bridle	NRAO-CV
Bob Brown	NRAO-CV
Bernie Burke	MIT
Bob Burns	
Bryan Butler	NRAO-Socorro
Leonard Chow	City Univ. of Hong Kong
Barry Clark	NRAO-Socorro
Mark Claussen	NRAO-Socorro
Marshall Cohen	Caltech
Raul Colomb	CONAE
Jim Condon	NRAO-CV
Tim Cornwell	NRAO-Socorro
Bill Cotton	NRAO-CV
Larry D'Addario	NRAO-CV
Ketan Desai	NRAO-Socorro
Peter Dewdney	Herzberg
Vivek Dhawan	NRAO-Socorro
John Dreher	SETI
Jean Eilek	NM Tech
Ron Ekers	ATNF
Darrel Emerson	NRAO - TUC
Bill Erickson	U Maryland
Ray Escoffier	NRAO-CV
Michael Faison	NRAO-Socorro
Dave Finley	NRAO-Socorro
Rick Fisher	NRAO-GB
Chris Flatters	NRAO-Socorro
Ed Fomalont	NRAO - CV
Dale Frail	NRAO-Socorro
Riccardo Giovanelli	Cornell
Brian Glendenning	NRAO-Socorro
Yolanda Gomez	UNAM
Miller Goss	NRAO-Socorro
Eric Greisen	NRAO-CV
Tim Hankins	NM Tech
Dan Harris	CfA
Martha Haynes	Cornell
Dave Heeschen	NRAO-CV
Carl Heiles	U C - Berkeley
Jim Herrnstein	NRAO-Socorro
Jackie Hewitt	MIT
Hans Hinteregger	Haystack
Bob Hjellming	NRAO-Socorro
Paul Ho	CfA
Dave Hogg	NRAO-CV
Bill Howard	USRA
Gareth Hunt	NRAO-CV
Clint Janes	NRAO-Socorro
Ken Johnston	USNO
Namir Kassim	NRL
Ken Kellermann	NRAO-CV
Athol Kemball	NRAO-Socorro
Lee King	NRAO-CV

Leonia Kogan	NRAO-Socorro
Phil Kronberg	U Toronto
Cornelia Lang	NRAO-Socorro
Harvey Liszt	NRAO-CV
Jay Lockman	NRAO-GB
Ralph Marson	NRAO-Socorro
George Miley	Leiden
Jim Moran	CfA
Mark Morris	UCLA
Peter Napier	NRAO-Socorro
Frazer Owen	NRAO-Socorro
Pat Palmer	U Chicago
John Payne	NRAO-TUC
Tim Pearson	Caltech
Alison Peck	NRAO-Socorro
Rick Perley	NRAO-Socorro
V. Radhakrishnan	Raman Res. Inst.
Tony Readhead	Caltech
Mark Reid	CfA
Dave Roberts	Brandeis
Luis Rodriguez	UNAM
Alan Rogers	Haystack
Jon Romney	NRAO-Socorro
Larry Rudnick	U Minnesota
Michael Rupen	NRAO-Socorro
Renzo Sancisi	Kapteyn Institute
Richard Schilizzi	JIVE
George Seielstad	U North Dakota
Dave Shaffer	Interferometrics Inc.
Richard Simon	NRAO-CV
Malcolm Sinclair	CSIRO
Ken Sowinski	NRAO-Socorro
Dick Sramek	NRAO-Socorro
George Swenson	U Illinois
Greg Taylor	NRAO-Socorro
Dick Thompson	NRAO-CV
Barry Turner	NRAO-CV
Jim Ulvestad	NRAO-Socorro
Juan Uson	NRAO-CV
Thijs van der Hulst	Kapteyn Institute
Jacqueline van Gorkom	Columbia Univ.
Gustaaf van Moorsel	NRAO-Socorro
Liese van Zee	NRAO-Socorro
Paul Vanden Bout	NRAO-CV
Craig Walker	NRAO-Socorro
Jim Weatherall	NM Tech
John Webber	NRAO-CV
Dave Weber	NRAO-Socorro
Sandy Weinreb	FCRAO\U of Mass.
Jack Welch	U C - Berkeley
Don Wells	NRAO-CV
Dave Westpfahl	NM Tech
Stephen White	U Maryland
Alan Whitney	Haystack
Bob Wilson	CfA
Katherine Wilson	Caltech
Joan Wrobel	NRAO-Socorro
Min Yun	NRAO-Socorro
Farhad Yusef-Zadeh	Northwestern Univ.
Anton Zensus	MPIfR

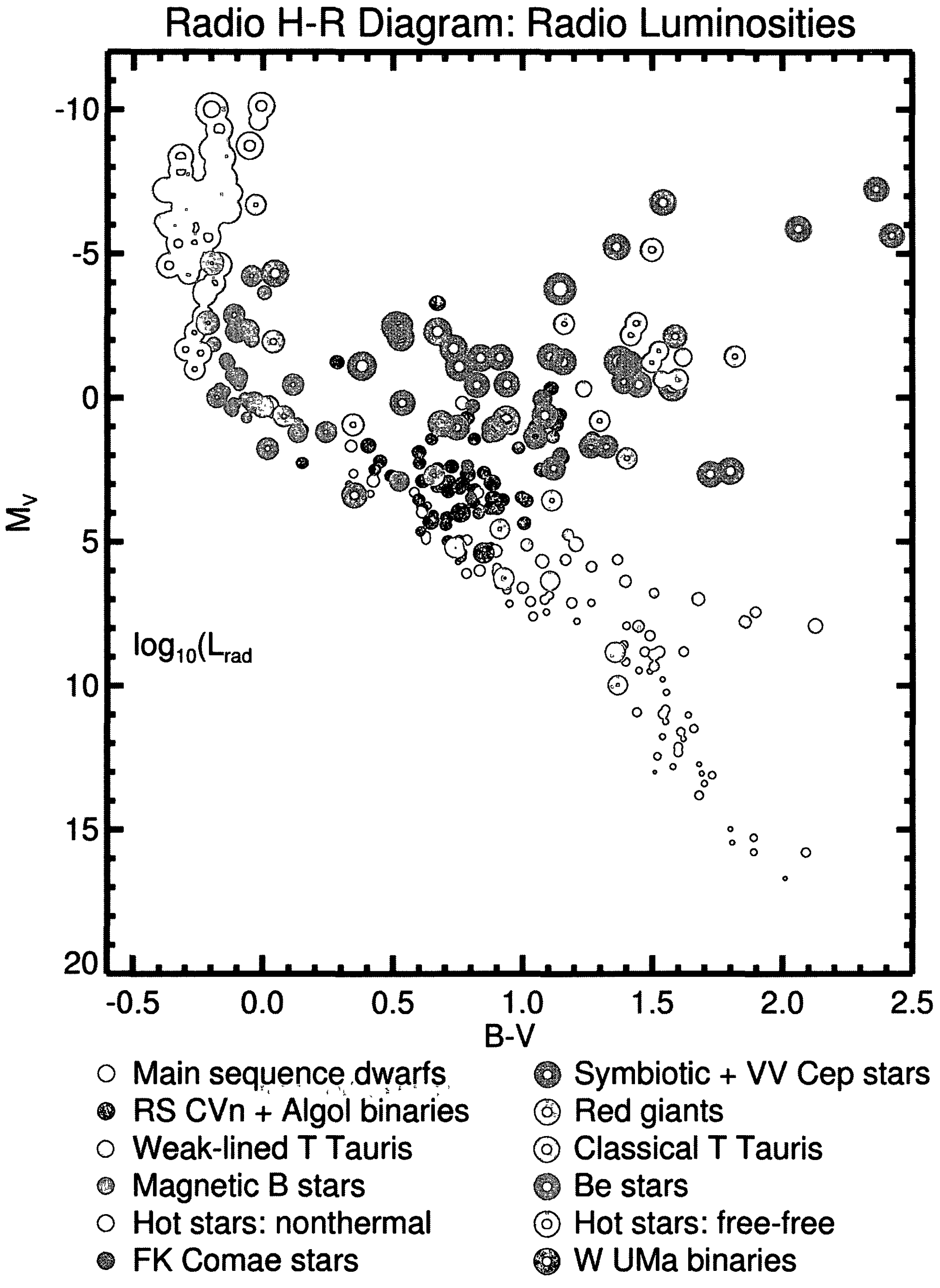


Plate 1. An H-R diagram for stars detected as radio emitters, compiled from the literature using the Hipparcos catalog for the luminosities and colors wherever possible. For hot stars corrections for extinction have been applied where available. The size of the symbol reflects the radio luminosity of the star as labelled (in ergs s^{-1} Hz^{-1}). The diagram is restricted to stars for which distances are available, and therefore shows only a small (but reasonably representative) sample of all the stars detected by the VLA. Stars believed to be nonthermal emitters are represented by filled symbols, while thermal emitters are shown by open symbols. See White, page 89.

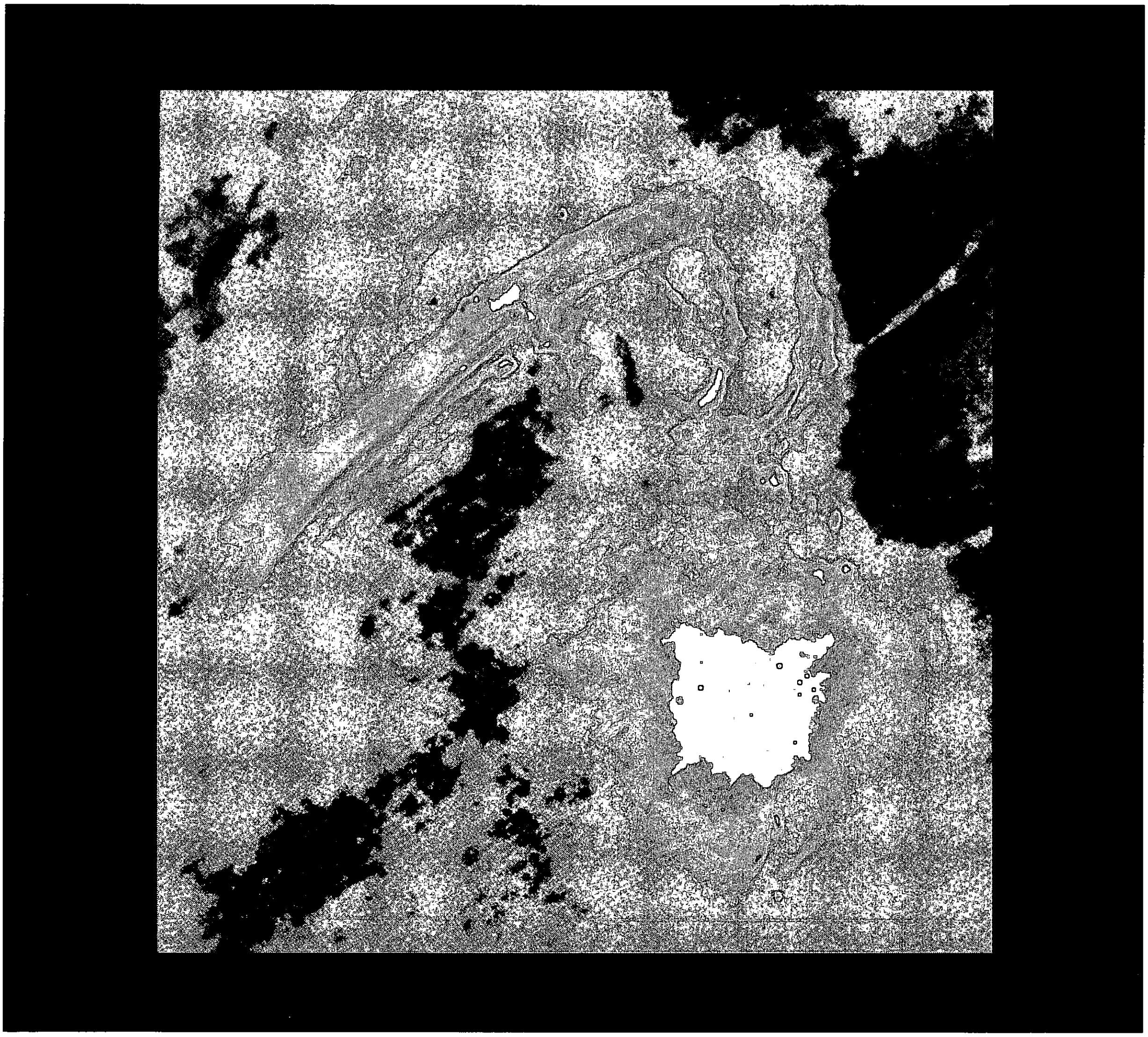


Plate 2. The original 20 cm image of the Radio Arc in the Galactic Center, with Sagittarius A dominating the flux. (Yusef-Zadeh, Morris & Chance 1984). See Morris, page 116.

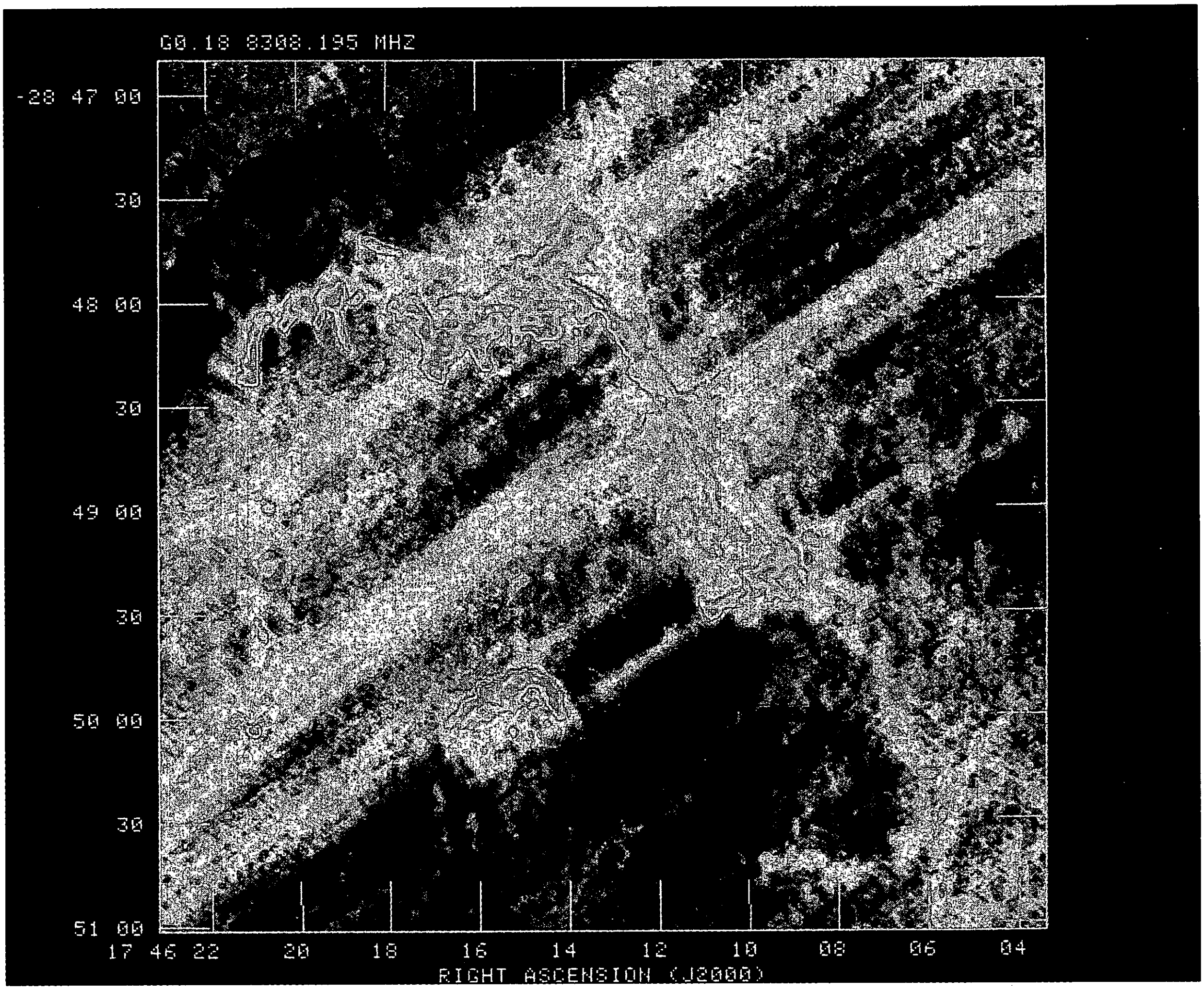


Plate 3. G0.18-0.04, the heart of the Radio Arc in the Galactic Center, where nonthermal filaments cross through the HII region, the Sickle, imaged here at 3.6 cm with the VLA (Lang et al. 1997). See Morris, page 117.

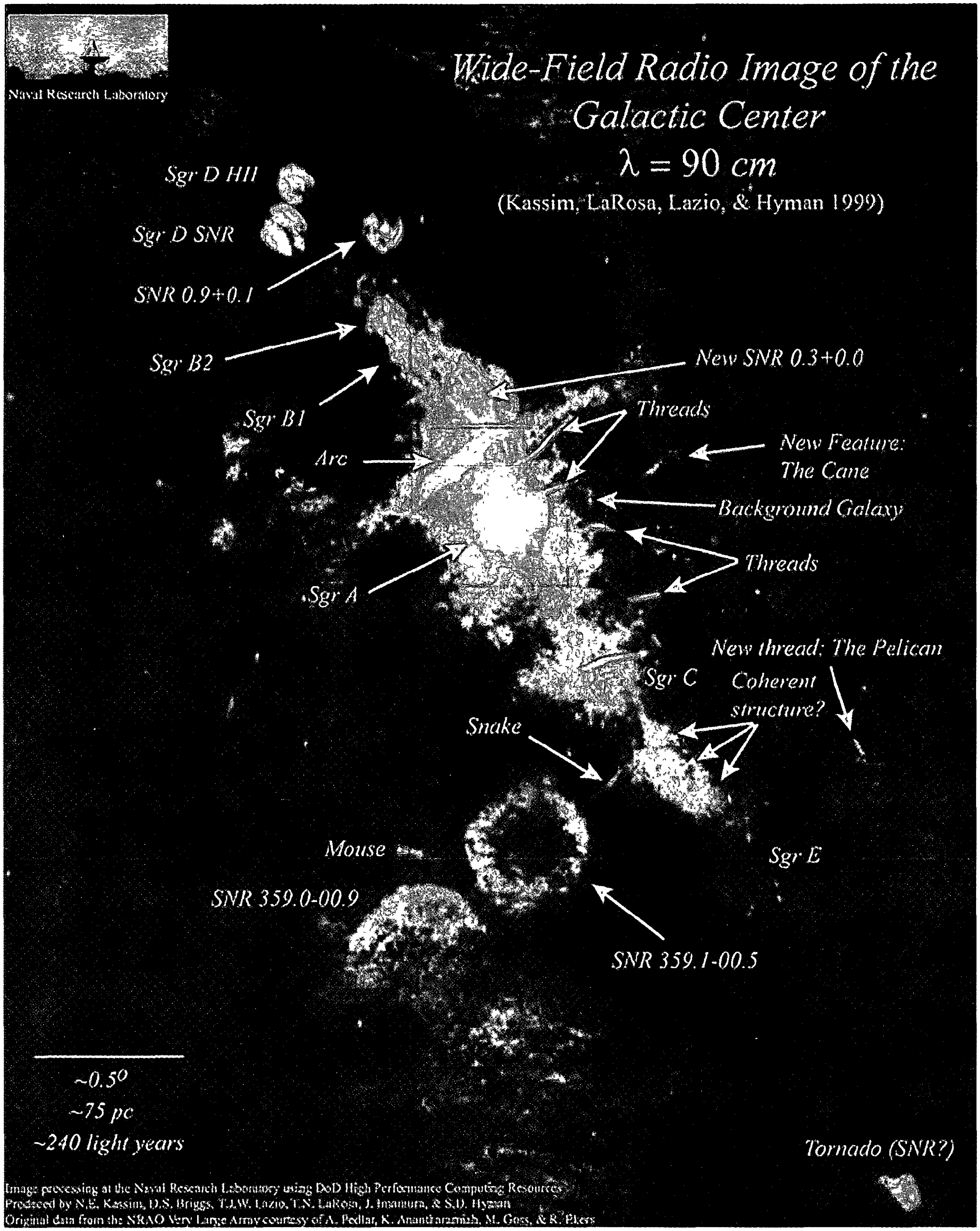


Image processing at the Naval Research Laboratory using DoD High Performance Computing Resources
Produced by N.E. Kassim, D.S. Briggs, T.J.W. Lazio, T.N. LaRosa, J. Imamura, & S.D. Hyman
Original data from the NRAO Very Large Array courtesy of A. Pedlar, K. Anantharamaiah, M. Goss, & R. Ekers

Plate 4. A 333-MHz continuum image of the Galactic Center, produced with wide-field processing software on the original data of Ananatharamaiah et al. (1991). (Kassim et al. 1998; LaRosa et al. 1999) See Morris, page 127.

Plate 5. A VLBA image of the nucleus of NGC 1275 at 1.3 cm, courtesy of Craig Walker. See Pearson, page 263.

Symposium Scenes

Plate 6. The assembled multitude, on the north lawn of the Array Operations Center.

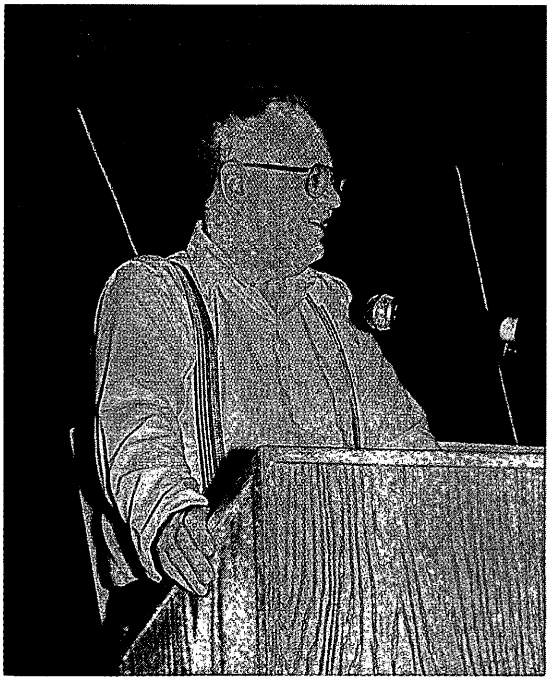
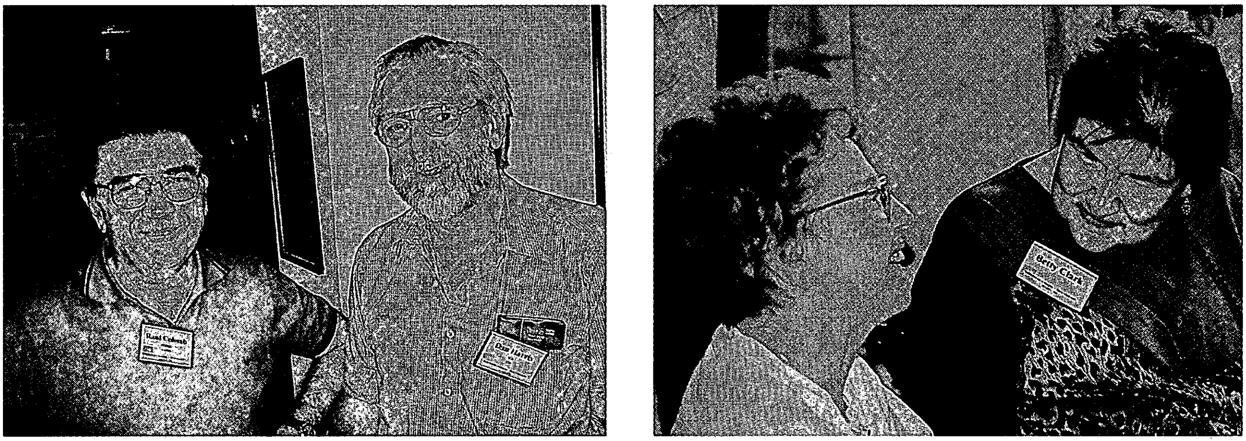


Plate 7 (Above). Bob Wilson, left, and V. Radhakrishnan chat during a break.

Plate 8 (Left). Barry addresses the dinner crowd.

Plate 9. From left: Cornelia Lang, Barry Clark, Jyotsna Anantharamaiah, and Alison Peck.



Plates 10 and 11. Raul Colomb (left) and Dan Harris; Kathy Wilson and Betty Clark (right).

Plate 12. Barry's birthday gift from Associated Universities, Inc., was a serigraph by New Mexico artist Doug West.

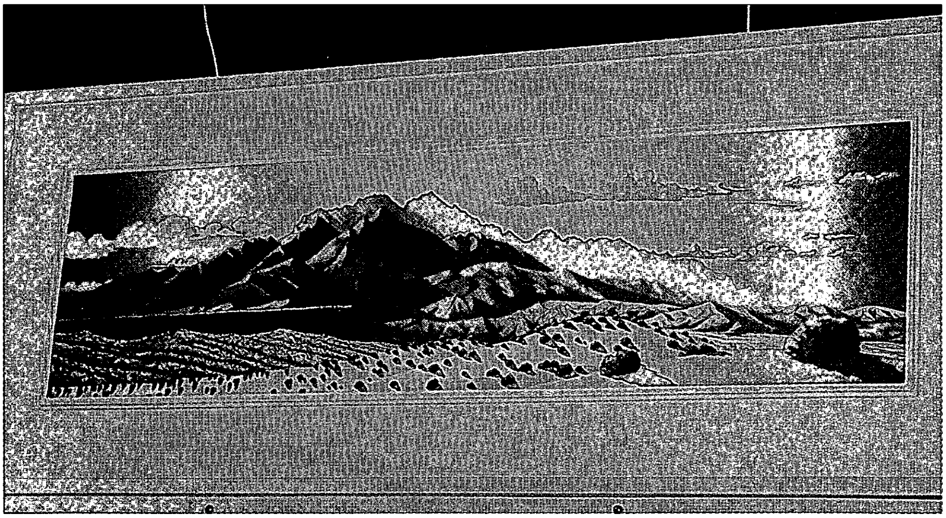


Plate 13 (Left). From top left: Jim Moran,Tony Readhead, Jack Welch, Sandy Weinreb, and Miller Goss. Bill Howard is in the background, between Moran and Readhead.

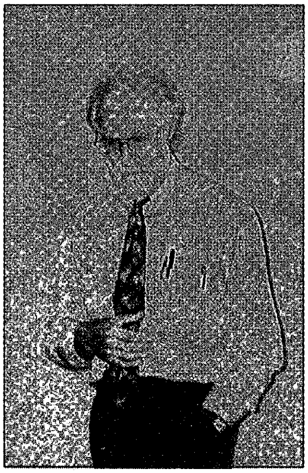


Plate 14 (Above). Ron Ekers contemplates his next remark.

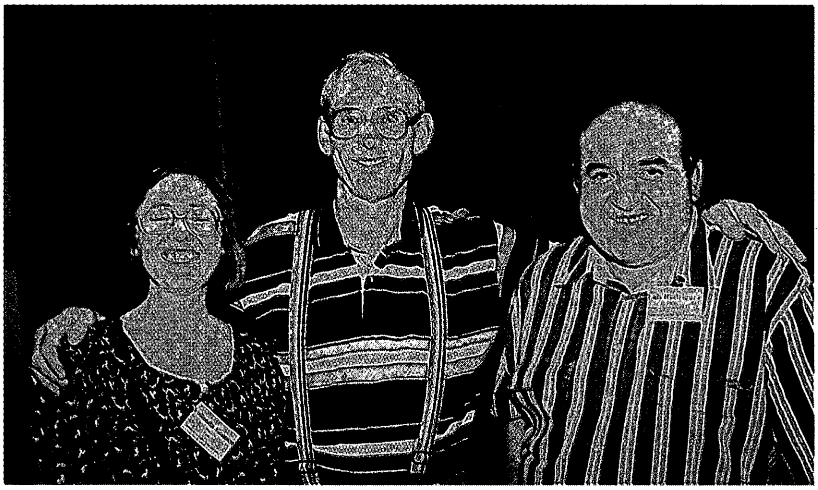
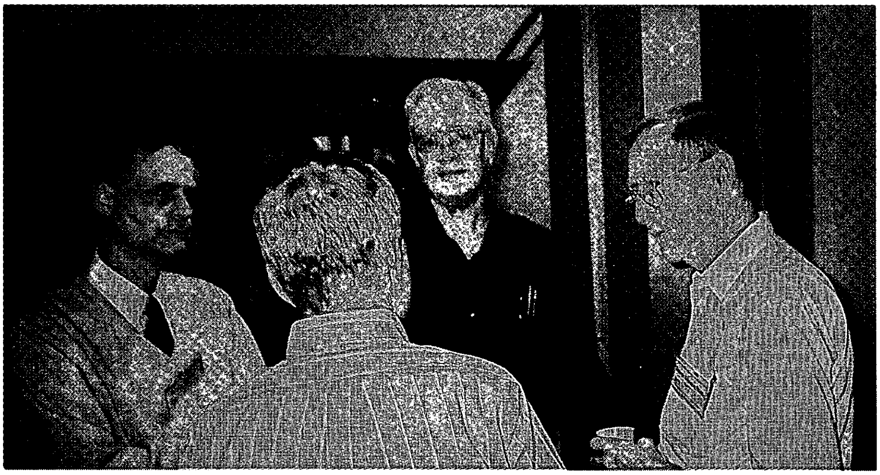


Plate 15 (Above). Yolanda Gomez, Jim Moran and Luis Rodriguez.

Plate 16 (Right). Alan Rogers, left, Barry Clark and Dave Heeschen.



Radio Interferometry: The Saga and the Scienc

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Held at Socorro, New Mexico
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