THE ORIGIN AND EVOLUTION OF THE N.R.A.O.-CORNELL VLBI SYSTEM

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ABSTRACT

The Mark-I N.R.A.O.-Cornell VLBI system began operations in May, 1967 with digital recordings (720 kbps) on standard 7-track computer tape. The first experiments, within the U.S., were quickly extended to Sweden, Australia, and the Soviet Union. The early interactions with the Canadian group and the discovery of superluminal motion are reviewed, as are the origins of the U.S. VLBI Network, and the VLBA.

I. The Beginning. By the mid-1960s several radio-astronomy groups had begun to think about independent-oscillator tape-recording interferometry. This was originally called V.L.B. for Very Long Baseline in the U.S. (intended as a pun on the VLA), and L.B.I. for Long Baseline Interferometry in Canada, and now called VLBI for Very-Long-Baseline Interferometry by nearly everyone. The idea was certainly an obvious one and had been considered earlier by the Jodrell Bank group as well as by radio astronomers in the U.S.S.R. (Lovell, 1973). But the big break-through in the 1960s came with the widespread availability of relatively inexpensive commercial atomic frequency standards and broad-band tape recorders.

The first published discussion of VLBI was by Matveyenko, Kardashev, and Sholomitski (1965) who incorrectly concluded that the required frequency stability depended inversely on baseline length. A more practical analysis was given by Slysh (1965).

The radio astronomy group in Florida was the first to use an independent oscillator-tape recording interferometer for astronomical observations. Carr *et al.* (1965) operated separate crystal oscillators and audio tape recorders to do post-detection (intensity) interferometry to measure the angular size of the 18 MHz radio bursts from Jupiter. On January 18, 1967 they used a coherent interferometer, although the tapes were not correlated and fringes found until after the first Canadian paper (Broten *et al.* 1967a) was published (Carr, private communication). The paper by Broten *et al.* reported the first complete system test, including correlation, which was made by the Canadian group in March, 1967.

Our own work grew out of differing interests. K.I.K. was involved in the

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observation of time variability and of the flat-spectrum compact radio sources, both of which implied very small angular dimensions that would require interferometer baselines of thousands of miles to resolve. M.H.C., then at Cornell, had already been using lunar occultations and interplanetary scintillations to explore radio-source structure on scales of 0.1 arcsecond. Barry Clark had been doing connected element interferometry and was heavily involved in software systems for the Green Bank interferometer and in the design of the VLA. Dave Jauncey had recently come to Cornell with a background in cosmic rays and computing. We were, also, all aware of the radio-linked interferometer system at Jodrell Bank, which was giving dramatic results on a scale of a few hundredths of an arcsecond.

The first well-defined discussion which led to the start of the N.R.A.O.-Cornell collaboration was at the August 1965 meeting of the American Astronomical Society, where K.I.K. and M.H.C. discussed in general terms (over a pitcher or two of beer), the concept of using stable independent local oscillators and tape-recorders to do radio interferometry. Shortly after that, discussions with Barry Clark in Green Bank and Dave Jauncey in Ithaca convinced us all that the concept was both feasible and important. George Swenson, who was working in Green Bank at the time on the VLA project, actively participated in many of these discussions and in the early planning of the project.

Dave Heeschen, then Director of N.R.A.O., had good judgment and foresight and gave us remarkably prompt funding. A quick estimate yielded $100K as the project cost, and we received $50K immediately to get started. Cornell later reimbursed N.R.A.O. for half of the cost of the initial pair of recording terminals. To keep things legitimate, however, a short proposal was prepared. This "proposal" (dated November 22, 1965) provides the earliest written discussion of our project.

II. Development of the MK I VLBI System. The primary goal, when we began the design of the MK I VLBI system, was to demonstrate the technique of tape-recording interferometry and to verify the theoretically expected small size of variable opaque radio sources. Thus, we put great emphasis on keeping the system simple and cheap. We wanted to use digital recordings which would be self-clocking and would reduce the need for precise stability of the record/playback system. Consideration of both cost and simplicity drove us to narrow bandwidths (data rate of 720 kbps). We chose to use a standard computer-tape drive to minimize the development effort and to be able to do the correlation in a general purpose computer. This certainly facilitated the initial development effort, but later led to immense computing loads.

In a typical experiment, about 100 tapes were recorded each day at each recording site. The processing time, for a pair of tapes containing three minutes of data, was about ten minutes on the Caltech IBM 360/75 computer, and more than an hour on the N.R.A.O. IBM 360/50! The hard-wired MK I correlator, later used at Haystack in conjunction with a CDC 3200 computer, proved to be much more effective in getting through the large quantities of tape generated during each observing session, particularly for multi-station observations.

Although the narrow-band digital recording system was expected to be easier to implement than the broad-band analogue system used by the Canadians, the sensitivity was limited. We anticipated that this would be balanced by the large collecting area of the Arecibo 1000-foot reflector. Because of the limited surface accuracy of the Arecibo reflector and the limited coherence time of the rubidium frequency standards, we were restricted to relatively long wavelengths. We chose to use 611 MHz (Mc/s as it was then called) which was the highest frequency then operational at Arecibo. We thought we were being daring, compared with the 448 MHz planned for the Canadian LBI system! As it turned out, our design criteria were conservative. The first successful observations were made with a much smaller dish; and within a year we were observing at frequencies as high as 5 GHz. In fact, tests were made during our first year of operation showed that even crystal oscillators would remain coherent for several minutes, even when multiplied up to several GHz.

In order to optimize the sensitivity, we implemented a double-side band interferometer using degenerate paramps. The 5-MHz reference output of the rubidium standard was used to drive a commercial multiplier, which gave the required local oscillator and coherent pump signals. This turned out to be an unfortunate decision, because the phase-lock unit was late in delivery and unreliable in operation.

Our whole development effort from the time funding was made available to the time of the first fringes lasted about a year and a half. Serious work began in late 1965, with the design of the recording hardware by Claude Bare and Barry Clark, the correlator software by Clark, and the choice of frequency standard by K.I.K. and Dave Jauncey. The first successful bench tests were made in October, 1966, and in early November we optimistically sent one of the recording terminals to Puerto Rico. Jack Cochran, then an N.R.A.O. technician, also flew to Puerto Rico to reassemble and operate the equipment at Arecibo. Unfortunately, Pan American lost the shipment, which was finally traced to a Baltimore warehouse after several weeks of frantic telephoning. But, by that time, Cochran had returned home to spend the Thanksgiving holiday with his family in Green Bank.

The first observations between Green Bank and Arecibo were made in January 1967, but produced no fringes. A second experiment in February was equally unproductive. We never understood, for sure, just what went wrong with those first observations, but most likely they failed because of the faulty phase-lock unit. Ironically, by the time we actually started with the first experiments, N.R.A.O.

had obtained commercial frequency synthesizers. However, we did not use them because we were unsure if they would meet our requirements.

III. First Fringes. Following the second unsuccessful experiment, all the equipment was returned to Green Bank from Puerto Rico. On the night of March 5/6, 1967, we ran the VLB system using one of the Green Bank interferometer 85-ft. antennas, together with the 140-ft., and found fringes on March 6.

Our first successful test on well-separated antennas came on May 8, between one of the 85-foot antennas in Green Bank and the Maryland Point Station of the Naval Research Laboratory; the distance was 220 km or 460,000 wavelengths at 611 MHz. These first results were submitted for publication on June 5, 1967 (Bare *et al.* 1967). Meanwhile, the M.I.T./Haystack group had been planning to make VLBI observations of OH maser sources (Burke, this symposium), and a few days later we began a series of joint observations between Green Bank and Haystack at 1.67 GHz. We observed quasars and radio galaxies, while the Haystack/M.I.T. group made the first high-resolution observations of OH masers using their own recording equipment at Haystack, which had been developed for this purpose. In July we made further observations at 1.67 GHz, this time from Green Bank to the University of California 26-metre telescope at Hat Creek ($d/\lambda = 1.9 \times 10^7$). Ultimately, we did observe successfully between Green Bank and Arecibo at 611 MHz in August, 1967.

Those first four months were very hectic. We shipped (or carried) tape-recorders, receiving equipment, and clocks to six observatories around the country, including Puerto Rico. We ran four successful sets of observations at two wavelengths, setting an example of the frantic pace which has since been characteristic of the world of VLBI.

IV. Interaction with the Canadian LBI Group. We first became aware of the Canadian LBI project in December 1965 at a meeting in Miami, Florida, attended by K.I.K. and Norm Broten. However, some earlier discussions about independent-oscillator tape-recording interferometry took place between K.I.K. and N.B. during long nights spent together in the control room of the Parkes radio telescope in Australia sometime during 1964 or 1965. During the design and development period in 1966 and 1967, a friendly competition existed between the two groups. Information on progress was frequently and freely exchanged. The two groups were using quite different technologies, and there was little worry that information from one group could be exploited by the other.

From the beginning, we were concerned about our poor sensitivity, and soon after our first successful experiments, we began development of the broader-bandwidth MK II system. We wanted to keep the simplicity of digital recording, but we used TV recorders which were then becoming available at reasonable cost.

This meant we no longer had computer-compatible tape, so we also had to design a hardware correlator. This had the virtue of freeing us from interminable sessions at the computing centre, but it led to interminable sessions at the correlator trying to play back tapes. We were made aware by Allen Yen at Toronto, of the difficulties that he was experiencing with the Ampex VR660 recorder, but regrettably we did not change our plans and finally ended up several years later burying several thousand pounds of useless 2-inch video tape.

During the mid 1970s, a group of Canadian and U.S. radio-astronomers joined forces in a series of experiments with the Canadian Technology Satellite (*Hermes*) to transfer broad-band IF data, as well as the local oscillator link, to form a real-time VLBI system between Green Bank and A.R.O. (Knowles *et al.* 1977, Yen *et al.* 1977). This effort, which was led by Allen Yen and George Swenson, involved groups from the University of Toronto, the National Research Council of Canada, the University of Illinois, the N.R.A.O., and the Naval Research Laboratory. Although we succeeded in demonstrating the technique, real-time satellite linked VLBI has not come into common use, because of the high cost of the space transponder, and the lack of a pressing need for real-time fringes. A real-time telephone-line fringe checker was also developed at the Center for Astrophysics, but it has not been extensively used.

Until the closure of the A.R.O. 150-foot telescope in 1987, we continued to use each others' observatories for our VLBI observations. Although the Canadian group continued to use their own VLBI system, which ultimately developed into a hybrid digital-analogue system, they also participated in MK II observations in collaboration with U.S. and other telescopes. Allen Yen, in particular, has provided valuable technical support to our programme, especially during extended visits to Caltech, N.R.A.O., and the Max-Planck Institut für Radioastronomie. The current MK II system, now in widespread use around the world, is based on the original N.R.A.O. MK II system, but uses home Video-Cassette Recorders modified to record digital data following design concepts originally formulated by Allen Yen.

There has been much discussion in the literature (and more privately) about who did what first. As far as we can tell the first successful Michelson-type independent-oscillator tape-recording interferometer was operated in the United States by the Florida group on the night of January 18, 1967. This was several months before the successful observations by the Canadians and ourselves, but the Florida tapes were not immediately correlated, in part, because they did not have an A/D converter. The publication of the first paper by Broten *et al.* in the April 4 issue of *Science* gave them a powerful spur. They quickly obtained an A/D converter which enabled them to find fringes from Jupiter (Carr, private communication). Their results (Brown *et al.* 1968) were not submitted for publication until November, 1967, after our first papers (Bare *et al.* 1967, Broten *et al.* 1967a) had been published. The Florida interferometer was used at 18 MHz to observe strong radio bursts from Jupiter. With the low observing frequency and narrow bandwidth (a few kilohertz) these observations made only modest requirements on local oscillator stability, recorder technology, and timing accuracy. Apparently for these reasons, and because the results were not published promptly, the Florida work did not make the impact it deserved.

The first successful demonstration of a broadband high-frequency VLBI system appears to have occurred with the February 2, 1967 observations by the Canadian group over a 200-metre baseline, which was reported in a short letter to *Science* (Broten *et al.* 1967a, Broten, p. 233). No date is given when fringes were found but the paper was submitted for publication on March 28, about a month before the successful test of our system with a 650-metre baseline between two antennas in Green Bank. Both of these experiments, however, involved antennas that could be (and indeed were) used as a conventional connected-element interferometer. On May 8, we jumped to a baseline of 220 km and within a few days obtained the first fringes, using independently operated radio telescopes. Our record for the longest interferometer baseline, however, was short lived. As described by Broten (p. 233) the Canadian group used a much longer baseline (3034 km) as early as April 13. They did not find interference fringes until May 21, after our own results on the Green Bank-Md. Point baseline were known. The Canadian observations were the first broad-band VLBI results of real astrophysical significance (Broten *et al.* 1967b). The results of both groups were presented for the first time at the meeting of U.R.S.I. held in Ottawa in late May.

It should also be recalled that the M.I.T.-Haystack group operated an independent-oscillator interferometer as early as November 1966, when they unlocked the oscillators in a radio linked 18-cm OH interferometer operating over a 13.4-km baseline between the Millstone and Agassiz antennas (Moran, 1988), but curiously no mention was made of this instrumental development in their paper (Moran *et al.* 1967). Apparently no attempt was made to extend the baselines further, and the first successful tape-recording OH interferometry used the N.R.A.O.-Cornell MK I tape-recorder together with a MK I compatible system built at Haystack.

V. On to Europe. By the summer of 1967, it was clear that there were still unresolved sources, and that we needed to go to shorter wavelengths and longer baselines. Six centimetres seemed to be the shortest wavelength that we might try without too much difficulty, and we had planned an experiment for January, 1968, with the Green Bank and Hat Creek telescopes. However, at that time receivers were changed only rarely on the Hat Creek telescope. In the autumn, the Hat Creek management decided that it would be too difficult to mount a 6-cm receiver in mid-winter, and the experiment was cancelled.

About this time, we received a visit from Olaf Rydbeck and Bert Hansson of the Onsala Space Observatory in Sweden, who were interested in doing VLBI at Onsala. We had not seriously considered transatlantic baselines before, and the jump from the successful 18-cm transcontinental observations to 6-cm transatlantic baselines seemed, perhaps, a bit ambitious. Thus, we observed at both wavelengths, using Green Bank, Haystack, and Onsala at 6 cm, and adding Hat Creek at 18 cm to make the first four-station observations. In the 18-cm phase of the programme, we were joined by the M.I.T./Haystack group who made transcontinental and intercontinental OH observations, while our group concentrated on the continuum sources. These observations, which were made in January, 1968, were the first in our long collaboration with the Swedish group, and were successful. In less than a year, interferometer baselines had been extended from about one million to over 100 million wavelengths, with a corresponding increase of two orders of magnitude in angular resolution.

VI. The Russian Connection. Since the first transatlantic experiments in 1968, there has been an improvement of another order of magnitude in effective resolution, which has been achieved by going to yet shorter wavelengths and by extending the baselines into space. But in 1968, the only moderately large short-wavelength antenna known to us outside of North America was the 22-metre dish at Serpukov, near Moscow, in the U.S.S.R. Would it be possible to do VLBI between the U.S. and the U.S.S.R? The idea seemed intriguing, and we had nothing to lose by trying. In early 1968, K.I.K. and M.H.C. wrote to Victor Vitkevitch in Moscow to enquire about interest in the U.S.S.R. Five years earlier, Soviet radio astronomers had discussed the possibility of doing VLBI between Jodrell Bank and the U.S.S.R. with Sir Bernard Lovell. These discussions ultimately led to the development of the Jodrell Bank VLBI system based on a 14-track analogue recorder, which was used for VLBI observations between Jodrell Bank and Onsala, and between Jodrell Bank and Arecibo (e.g., Fort, 1971). No VLBI collaboration with the U.S.S.R. ever grew out of those discussions (Lovell, 1973), but they apparently set the stage for a favourable response to our letter.

We received no reply from the U.S.S.R. for nearly six months, after which a telex arrived informing us that they had accepted our proposal–although they offered an antenna in the Crimea, rather than Serpukov. Much later, we learned that this six-month period was spent in convincing the Soviet military authorities that the proposed VLBI experiment would not compromise the security of the Soviet Union. Our group, however, had not discussed the proposed observations with anyone in the U.S. government, and we spent the next six months convincing the U.S. military and intelligence authorities that the proposed VLBI experiment would not compromise the security of the United States.

In his book, *Out of the Zenith*, Lovell suggests that the ideas for the U.S. VLBI programme originated from his early discussions with the Soviets which were passed on by Shklovsky during his visit to the United States in 1968. Our programme was several years old at the time of Shklovsky's visit, but we were aware of the Lovell-U.S.S.R. discussions even at the time of our initial planning in 1965. Moreover, Lovell's early contact with the Russians apparently played a significant role in getting support from Shklovsky and other Soviet astronomers for the U.S.-U.S.S.R. experiment.

The first experiments between the Crimea and Green Bank encountered enormous technical and logistical difficulties. Communications at that time between the U.S. and the U.S.S.R. were primitive, but as a result of the painstaking efforts by the technical and support staffs at both observatories, as well as the extraordinary help received from the Onsala group when our atomic clock failed, we managed to obtain fringes in our first experiments at 6 cm and 3 cm in October 1969. Our VLBI collaboration with the U.S.S.R. has continued through the 1970s and 1980s, including periods of strained political relations between our two countries, and the 22-metre Crimea telescope is now regularly used in global VLBI experiments with little more difficulty than telescopes in other countries. Indeed, Soviet radio astronomers are now planning an exciting space VLBI mission, and together with radio astronomers from other countries, we hope that this will provide the opportunity to extend our previous fruitful VLBI collaboration.

VII. The Discovery of Superluminal Motion. Superluminal motion, which refers to the faster-than-light apparent transverse speed measured between the separated components of many compact extragalactic objects is, perhaps, one of the most important astronomical discoveries of VLBI. In retrospect, it is surprising that it took nearly four years for this discovery to be made.

By the late 1960s, the model of a variable source as an expanding sphere full of synchrotron-emitting material had become popular and it was natural to apply it to the early VLBI observations. We interpreted visibility measurements on 3C 84 and 3C 120 in terms of a sphere, found an earlier intensity outburst which gave a time origin, and calculated radial expansion velocities that were fast, but less than c (for 3C 120, however, the velocity became greater than c for H_0 = 50 km s^{-1}Mpc^{-1}.) The next year we observed 3C 84 again, and we were able to find a component motion, which gave $v/c = 0.35$.

The Australia-Pasadena group (Gubbay, *et al.* 1969), with observations at one (u, v) point, but over three epochs, saw the correlated flux of 3C 273 remain constant while the total flux density decreased. Thus, the variable component had to be bigger than the resolution limit, and the age could be estimated from the light curve. A lower limit to velocity was calculated to be $3c$. They interpreted this using the Rees (1967) model of a relativistically expanding sphere, which gave a lower limit to the Lorentz factor, $\gamma \geq 3$. This was reported as "...the first direct evidence that the expansion in variable sources may take place at relativistic velocities." In a later paper they retracted this result, because of a presumed misidentification of the starting time of the outburst, and they substituted 3C 279, which on the basis of more data they cautiously interpreted as expanding with $\gamma \geq 2$ (Moffet *et al.* 1972). Much of this work was motivated by a desire "...to test possible models of variable radio sources by direct observation of diameter changes..." (Moffet *et al.* 1972). It was natural for all of us to think of relativistically expanding spheres; but we know now that was wrong–the visibility changes were due to component motions, not expansions.

The Canadian VLBI work at 408 MHz and 448 MHz (Broten *et al.* 1969) showed that some sources were complex, i.e., required multiple separated components. The first short-wavelength tracking observations appear to be the 13-cm Australia experiments run in late 1969 and May 1970, which definitively showed a wide-spaced double in PKS 1934–63. But, this work did not become well-known until it was published several years later (Gubbay *et al.* 1971). Nevertheless, the Canadian work was suggestive. We had been thinking of successive flux outbursts producing independent components, but we thought of them as concentric. Why were we not moved by the Canadian work, and the article on linear relativistic motion by Ozernoi and Sazanov (1969), to calculate the angular separations one might expect from a three-year-old outburst? In fact, no one designed a full tracking experiment to measure definitively the shape of one or two sources, and to try to correlate outbursts with separated components.

We did have data which could not be interpreted in terms of circular symmetry. But, we were suspicious of low visibility points, because all known experimental problems produced low visibilities. Thus, we did not trust our own or others' low points, and so we ignored our own hints of separated structure. Our primary goal was to study expanding sources and the inverse-Compton limit, and we specifically deferred full tracking observations while we looked for ever smaller variable components.

The story of the actual discovery of superluminal motion is well-known. The first-epoch observations were made for an entirely different purpose. Irwin Shapiro and the M.I.T./G.S.F.C./Haystack group were studying gravitational ray bending and observed 3C 279 as it went behind the sun in October 1970. They alternated observations with 3C 273, and made full-tracking observations using a Goldstone-Haystack (Goldstack) baseline. The resulting visibility curves astounded everyone. We saw them before they were published. The curve for 3C 279 could be fitted with a cosine with extraordinary precision, and that gave the separation and position angle of an equal double. The 3C 273 curve was not as clean, but still could be fitted by a double source with a well-defined component separation (Knight *et al.* 1971).

We already had been granted observing time on the Goldstack interferometer in February 1971, and we tailored our observations to track 3C 273 and 3C 279 and look for changes. The M.I.T. group also applied for and got observing time, and joined us in the February run. Our tapes were processed on the IBM 360/75 computer at Caltech by Alan Whitney's program. Both groups saw immediately that the visibility functions for both 3C 273 and 3C 279, had changed in the sense that the doubles had increased their separation with an apparent velocity considerably greater than the speed of light (Whitney *et al.* 1971, Cohen *et al.* 1971).

The Goldstack superluminal discovery is remarkable for being doubly serendipitous. First, the original objective was to measure relativistic bending and had little or nothing to do with studying quasars. Indeed, a modern experiment involving precision astrometry would avoid such complex and variable sources, and would choose others which are more compact. Second, we were accidentally observing at the right epoch. Many superluminal sources are intermittent, but both 3C 279 and 3C 273 were active in 1970–71 and had obvious simple interpretations. The expanding double in 3C 279 was ephemeral, but generated a great deal of wasted speculation. In late 1971 the character of the visibility functions of both 3C 273 and 3C 279 changed and the cosine pattern in 3C 279 did not reappear for a dozen years. Had the experiment been first done at a different epoch, the visibility function would not have been sharp, and the interpretation of the second epoch observations would have been more difficult. Ultimately changes would have been seen, but the interpretations would have been substantially more ambiguous. In fact, the modelling ambiguities were used as objections to the superluminal interpretation for years, and they were not stilled until the late 1970s when hybrid-mapping techniques gave good VLBI maps (e.g., Pearson *et al.* 1981).

VIII. Organization for VLBI. VLBI requires simultaneous observations at several or many widely-spaced telescopes, and the organizational problems this produces have been substantial. The early observations were all set up by the scientific collaborators making the interminable phone calls that were required to arrange for common observing time, for magnetic tapes, and for people to be at the telescopes to change the tapes and run the experiment. These arrangements had to be made, in addition to writing proposals and getting scientific approval for the programme. Each telescope, of course, had to give its own approval.

The large antennas of the Deep Space Network were a special case. They were particularly sensitive and, therefore, desirable, but did not have much time to give to radio-astronomy. In 1969 N.A.S.A. recognized the special value of the D.S.N. telescopes for radio-astronomy, and set a policy whereby up to five percent of the time on each telescope could be assigned to non-N.A.S.A. radio-astronomy experiments. They also set up the Radio Astronomy Experiment Selection panel to screen proposals for that observing time. The R.A.E.S. panel still exists, and roughly the same rules apply, but the community has yet to see a single year in which five percent of the time on a 64-metre or 70-metre telescope has been given to radio astronomy!

After the discovery of superluminal motion in 1971, all the observing groups moved swiftly to monitor the variable sources on a regular basis. This, of course, led to competition for observing time on the Goldstack interferometer, which had to be regulated both by the R.A.E.S. panel for Goldstone and by the Haystack scheduling committee. That combined monitoring program was dubbed the Quasar Patrol and it lasted for a number of years.

The Goldstack interferometer was sensitive and reliable, but by itself was incomplete and gave ambiguous data. Therefore, we supplemented Goldstack by adding telescopes at Green Bank and other observatories, when we could. But we wanted a wider range of observing frequencies and more observing time. In the spring of 1971, we proposed to set up a VLBI system consisting of telescopes at Owens Valley Radio Observatory, Green Bank, and at the Harvard Radio Astronomy Station at Fort Davis, Texas, now called the G.R. Agassiz Station. We convinced Alan Maxwell, the Director at H.R.A.S., that Fort Davis was crucial, because it was the only mid-continental telescope, and it was our only chance at getting reasonable (u, v) coverage. We promptly got permission to organize the 26-m telescope at H.R.A.S. for VLBI, even though no one on the Fort Davis staff had a direct scientific interest in the project.

Our original idea was to use the Fort Davis-Owens Valley-Green Bank (FOG) array every three months to monitor the superluminal sources and make good models of other sources with long tracking observations. H.R.A.S. did not have the necessary equipment, but it was borrowed from N.R.A.O. and Caltech; and we made the first observations in March, 1972, at a wavelength of 6 cm. We often tried to increase the (u, v) coverage and the angular resolution by adding other telescopes to the FOG array; and in fact our first observations in March 1972 also included the telescope at Onsala. In 1974 our regular FOG observations started to include the 100-m telescope at Bonn, Federal Republic of Germany. The FOGB array, with occasional other connections, ran regularly for a number of years, although not on a regular three-month schedule as first intended.

By the mid 1970s both the successes and frustrations of VLBI led to the growing desire for more sophisticated instrumentation and more central organization of the observing and processing facilities. In April, 1974, Dave Heeschen called a meeting in Charlottesville to assess the state of VLBI and to plan for the future. A great deal of frustration was expressed over the difficulty of scheduling and carrying out the observations, the poor reliability at the telescopes and the correlator, and the long waiting time at the Charlottesville processor. VLBI was a labourintensive science, and there was a strong consensus that the radio-telescopes being used for VLBI should be better organized and run as a coherent VLBI Network. It was also realized that, even with better organization, much more than the network of existing radio telescopes would be needed in the long run to exploit fully the capabilities of VLBI. Most importantly, the telescopes were bunched in the Northeast and in California. This geometry gave poor (u, v) coverage and, furthermore, half the telescopes had restricted hour-angle motion which further limited the possible range of projected baselines. Also, few of those telescopes worked well at the short wavelengths needed for the best possible resolution; and they were operated with a wide variety of often inadequate instrumentation. A new dedicated array of properly located modern antennas, complete with standard instrumentation and a large central processing facility, was clearly needed.

A three-phase development program was envisioned. First, the existing telescopes should be organized to the extent possible, given the limited funds and the independent nature of the various telescope managements. Second, a "Mid-West Telescope" should be built. This was badly needed to fill in the "Mid-West Gap" in the existing (u, v) coverage. Finally, N.R.A.O. would study the design of a new array dedicated to VLBI. M.H.C. and K.I.K., respectively, agreed to lead the near-term and long-term studies which resulted in a series of reports called "VLBI Network Studies":

(i) A VLBI Network Using Existing Telescopes, Cohen *et al.* 1975.
(ii) Interim Report on a New Antenna for the VLBI Network, Swenson, *et al.* 1977.
(iii) An Intercontinental Very Long Baseline Array, Kellermann, 1975.
(iv) On the Geometry of the VLBI Network, Swenson, 1977.

The attempt to construct a new mid-west VLBI antenna, which was intended to become ultimately the first antenna of the multi-element dedicated array, was unsuccessful. Encouraged by the National Science Foundation, George Swenson and Bob Mutel submitted proposals to build the Mid-West antenna in Illinois and Iowa respectively, but neither proposal was funded. The 60-foot antenna at North Liberty, Iowa later came to be used for VLBI, but it had limited sensitivity and only two operating wavelengths. The Mid-West Gap is still there, and it will not be filled until 1989, when the Iowa VLBA antenna comes into operation.

The meeting at N.R.A.O. in the spring of 1974 catalyzed the community, and was the start of both the Network of Existing Telescopes and the dedicated VLB Array. During 1974 and 1975 the near-term group met a few times to try to decide what an appropriate Network might consist of, and how it might be organized. The first report (Cohen 1975) stated that the primary objective of the Network was "...to provide reliable, versatile, and convenient facilities for VLBI observations;" a further objective was "...to provide an organization in which VLBI problems of national interest may be discussed." Five telescopes at Haystack, Green Bank, H.R.A.S., O.V.R.O., and Hat Creek agreed to make one week, every two months, available for VLBI Network Observations. Two other telescopes, at Maryland Point and the University of Illinois, became associate members of the Network, and planned to observe when their special facilities were needed. The Network also featured a Network Users Group (NUG), which would elect officers who would arrange for receiving and refereeing propósals, and coordinate all the observations.

The first Network observations were in March, 1976, and the new organization quickly solved the scheduling problem. The inconveniences of observing and correlating tapes were only partly alleviated, however, and in 1981 there was a refinement of the organization. A University Consortium for VLBI was formed, consisting of M.I.T., Harvard/S.A.O., the University of Iowa, the University of California (Berkeley), and Caltech, with N.R.L. and J.P.L. as Associate Members. Since then, N.R.A.O., the Max-Planck-Institut für Radioastronomie (F.R.G.), and the Instituto di Radioastronomia (Italy) have joined as associate or full members.

The Consortium took over the job that the NUG had been doing, with the objectives of making the observing both more convenient and more reliable, and of facilitating coordination with telescopes in Europe and elsewhere. Under the prodding of the Consortium the major observatories now have a Friend for VLBI and, also, have finally organized themselves to do "*in-absentia*" observing for all the Network programs. The system, unfortunately now, is still far from fully reliable, but it definitely is convenient. A small group, or even one person with a good scientific project, can readily get simultaneous observing time on a dozen or more of the world's largest telescopes. This goal, at least, has been reached.

IX. The Very-Long Baseline Array (VLBA). Following several years of work by many scientists from the U.S. VLBI community, the first VLBA design study was carried out at N.R.A.O. At that time, however, N.R.A.O. had just recently begun serious construction work on the VLA, and there were inadequate resources to pursue the VLBA concept seriously. The emphasis continued to be on obtaining better results from the network of existing antennas, and adding the Mid-West antenna to the network. During this period the VLBA was discussed within the U.S. VLBI community, and on several occasions at meetings of the N.S.F. Advisory Committee on Astronomy and its Sub-Committee on Radio Astronomy. At that time there was no consensus on the need for a dedicated VLBI system, even among practising VLBI users, let alone among other radio astronomers! N.R.A.O. was pre-occupied with the major undertaking of building the VLA, and pressure was growing for N.R.A.O. also to build a 25-metre radio-telescope for millimetre radio-astronomy. The design effort for the millimetre radio-telescope, which was a high-priority item in the National Academy of Science ten-year programme for astronomy, was further advanced than that of the VLBA. It seemed, at the time, that N.R.A.O. might not be able to begin serious work on the VLBA for a number of years.

At the triennial I.A.U. General Assembly, which was held in Montreal in August, 1979, we learned for the first time of a conceptually similar proposal in Canada, the C.L.B.A. Unfortunately, at least from our perspective, rather than stirring the spirit of competition in Washington, the news elicited the response from N.S.F., "Well, why not let the Canadians do it?"

About this time, the National Academy of Sciences' review of U.S. astronomy priorities for the 1980s was beginning, and the VLBA was one of the projects to be discussed. Partly due to the uncertainties in the community about the intentions of N.R.A.O., Caltech began a separate design effort, although many of the same individuals contributed to both the Caltech and N.R.A.O. design programmes. In January, 1980, a meeting was held in Pasadena, which was attended by about 30 scientists active in VLBA research. At this meeting, it was agreed that the number of antenna elements should be "ten" representing the best compromise between (u, v) coverage and cost.

In September, 1980, Caltech issued the results of their design study, and in October a group of some 70 scientists and engineers met in Green Bank for a three-day intensive workshop to discuss the Caltech and N.R.A.O. studies for a dedicated Very Long Baseline Array. In December, N.R.A.O. issued its second design study. By this time the work of Readhead and Wilkinson (1978) and Cotton (1979) had shown how phase closure could be used to make two-dimensional images from VLBA data. Meanwhile, developments made at the Haystack Observatory, under N.A.S.A. sponsorship, indicated that the MK III VLBI recording system could be modified to allow uninterrupted recordings for periods up to half a day at data rates of the order of 100 Mbps.

The N.R.A.O. and Caltech reports showed that a dedicated VLBA was technically feasible, and that the costs could be reliably estimated. They played a major role in obtaining the support of the Astronomy Survey Committee, which gave the construction of the VLBA the highest priority for major new ground-based facilities for astronomy. In May, 1983, N.R.A.O. formally submitted a proposal to the National Science Foundation for the design and construction of the VLBA, and design money was made available in the 1984 N.S.F. budget. Although a three-year construction budget was initially planned, funding restrictions have considerably extended the construction period. The first VLBA antenna located at Pietown, New Mexico, near the VLA, has already been used for VLBI observations together with Network stations, but full operation of the VLBA is not expected before 1992, a full twenty years after the first internal document at N.R.A.O. proposed such a system.

X. Reflections. As we look back over the past twenty years, we have mixed reactions to the tremendous growth of VLBI. Ironically, one of our earlier selling points had been that we could do fundamentally new science by building only some simple equipment and installing it on existing radio-telescopes. No costly new major facilities would be needed. Five years later we were discussing a costly new facility, the VLBA.

Our programme started out in 1965 with a modest effort to exploit new technical developments in order to measure the angular size of extragalactic radio-sources. We recognized the potential applications to other disciplines, (e.g., Cohen *et al.* 1969), but our interests were astronomical, and the early financial support was entirely from astronomical sources. As things have developed, however, since about 1970 the bulk of the financial support for VLBI in the United States has come from the geodetic community, primarily N.A.S.A.

The early years of VLBI were tremendously exciting. We carried our clocks and tape recorders around the world, in the process making many new friends. Each experiment which gave fringes was a cause for celebration. Regrettably, some experiments did not give fringes. Nearly everything that could be done wrong was done wrong. There were occasions when there were large timing errors or the local oscillators were not set to the right frequency. Sometimes the senses of circular polarization were crossed. Once we recorded all ones at one end and all zeros at the other. It was too easy for one of the many local oscillators to be left unlocked. Sometimes there were no fringes, and we never found out why. We would like to think of these as the teething problems of the early years, but from time to time they recur, even twenty years later.

More often we had fringes, but the data were quantitatively unreliable. We tried to save money by using surplus tape, and we were reluctant to discard our ancient VR 660s in favour of a more modern recorder. That was a serious mistake. In the early 1970s it was usually difficult to replay the MK II tapes, and the error rates were often discouragingly large. Our philosophy was that errors and dropouts could only reduce the fringe amplitude, never increase it; or in serious cases they might cause the fringes to disappear entirely. Therefore we tended to disregard low fringe amplitudes, and to force the upper envelope of the data to fit simple Gaussian models. We not only did not believe our own data when they indicated a complex visibility function, but we also did not believe the Canadian data when Broten *et al.* (1969) published a paper showing complex visibilities and deduced multiple-component source structure. We stuck to our simple Gaussian models until the first Goldstone-Haystack observations, which had sufficient detail to make the evidence for multiple-component structure convincing (Knight *et al.* 1971).

Today things are much more sophisticated. We are no longer happy with just detecting fringes, or measuring a few visibilities to determine the overall source size. Six-station to twelve-station observations are routine, and as many as 18 stations around the world have been used simultaneously. Sophisticated image-restoration algorithms exploiting phase-closure relations have led us to forget the old adage "In VLBI there is no phase information," and we are publishing images with arc millisecond resolution and quality that none of us anticipated twenty years ago. Ironically, many of us have forgotten the value of model fitting, which for simple source structure shows more detail than is obtained with conventional Fourier-mapping techniques. Model fitting, however, can be frustrating since it is often unclear if one has found the simplest solution or the right solution.

Progress has not come for free. Modern VLBI is Big Science. More than 25 MK II terminals and more than 15 MK III terminals are in routine use around the world and the tapes are correlated in one of four MK II processors located in Charlottesville (Virginia), Pasadena, (California), Bonn, (Germany F.R.) and Moscow, U.S.S.R, or in one of four MK III processors located in Westford, Massachusetts, Washington, D.C., Bonn, and Pasadena. Few observers travel to the telescopes now. Local scientists run observing programmes for each other, or the observing is done by the local technical staff, often with little direct contact by any of the research scientists either at record or playback time. Gone also are the endless rounds of telephone calls among the members of the observing group, and between the observers and each of the radio telescope managements. Today, nearly everything is arranged by one of the consortia; for astrophysics by the VLBN and the EVN (or both) in the U.S. and Europe respectively; and for astrometry, earth rotation and geophysics by the NASA/NGS/NRL/USNO CDP (Crustal Dynamics Project) and IRIS (International Radio Interferometer Survey) programmes. A small number of experiments, typically at millimeter wavelengths, are still run in the "old-fashioned" *ad-hoc* manner.

The trend is to even more organization and bigger science. The VLBA will do for VLBI what the VLA has done for connected-element interferometry by allowing a greater number of scientists to obtain high-quality results with relatively little effort. But, as with the VLA, there will be less hands-on involvement by the individual scientist, and much of the opportunity for innovation will be lost. Space VLBI projects are being discussed by E.S.A., N.A.S.A, Japan, and the Soviet Space Agency. "Active" VLBI scientists can spend more time going to meetings and planning for the future than in observing. Perhaps that is the price of progress.

No international committees or government agencies have ever been involved in setting up standards for VLBI. Several incompatible systems were developed, including the analogue systems in Canada and Jodrell Bank, as well as a narrow-band digital system used internally by the Deep Space Network. The Jodrell Bank system was used only for a short while in the early 1970s, and the Canadian system has not been used for several years. Worldwide VLBI data are currently recorded in one of two systems, the narrow band MK II or the broad band MK III. The more common MK II system uses inexpensive VHS home video-cassette TV recorders, NTSC standard, and 60 frames per second as the global standard. The first MK II operations began in North America, and when other observatories in other countries, (including China and the U.S.S.R.), started VLBI they naturally chose to implement compatible systems. This may be contrasted with the commercial TV and power industries which have established three different TV standards (NTSC, PAL, and SECAM), two recording standards, (BETA and VHS), and two frequency references (50 Hz and 60 Hz) for a total of twelve combinations.

We, also, may take considerable satisfaction that an individual scientist can have simultaneous access to most of the world's major radio-telescopes merely by submitting a proposal to one of the VLBI consortia, which will arrange for the observing, the shipping of magnetic tapes and, often, even for the correlation of the tapes. The scientist has been freed from an exceptionally laborious series of tasks, and more time is available for the contemplation of astronomical issues.

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References

Bare, C., Clark, B.G., Kellermann, K.I., Cohen, M.H., and Jauncey, D.L., 1967, *Science,* **157**, 189.

Broten, N.W., Legg, T.H., Locke, J.L., McLeish, C.W., Richards, R.S., Chisholm, R.M., Gush, H.P., Yen, J.L., and Gault, J.A., 1967a, *Science,* **156**, 1593.

Broten, N.W., Legg, T.H., Locke, J.L., McLeish, C.W., Richards, R.S., Chisholm, R.M., Gush, H.P., Yen, J.L., and Gault, J.A., 1967b, *Nature,* **215**, 38.

Broten, N.W., Clark, R.W., Legg, T.H., Locke, J.L., Galt, J.A., Yen, J.L., Chisholm, R.M., 1969, *Mon. Not. Roy. Astr. Soc.,* **146**, 313.

Brown, G.W., Carr, T.D. and Block, W.F., 1968 *Astrophys. Lett.,* **1**, 1968.

Carr, T.D., May, J., Olsson, C.N., and Walls, G.F., 1965, *I.E.E.E. NEREM Record,* **7**, 222.

Cohen, M.H., Jauncey, D., Kellermann, K.I., and Clark, B.G., 1969, *Science,* **162**, 88.

Cohen, M.H., Cannon, W., Purcell, G.H., Shaffer, D.B., Broderick, J.J., Kellermann, K.I., and Jauncey, D.J., 1971, *Astrophys. J.,* **170**, 207.

Cotton, W.D., 1979, *Astron. J.,* **84**, 1222.

Fort, D.N., 1971, Ph.D. Thesis, University of Manchester.

Gubbay, J., Legg, A.J., Robertson, D.S., Moffet, A.T., Ekers, R.D., Seidel, B., 1969, *Nature,* **222**, 730.

Knight, C.A., Robertson, D.S., Rogers, A.E.E., Shapiro, I.I., Whitney, A.R., Clark, T.A., Goldstein, R.M., Marandino, G.E., and Vandenberg, N.R., 1971, *Science,* **172**, 52.

Knowles, S.H., Waltman, W.B., Yen, J.L., Cannon, W.H., Petrachenko, W., Broten, N.W., Costain, C., Fort, D.N., Popelar, J.S., and Swenson, G.W., 1977, *Proceedings of the Eleventh Annual PTTI Meeting,* NASA Conference Publ. 2129.

Lovell, Sir. A.C.B., 1973, *Out of the Zenith,* Harper and Row, New York.

Matveyenko, L.I., Kardashev, N.S., and Sholomitski, G.V., 1965, *Radiophysica,* **8**, 651 (*Soviet Radiophysics* **8**, 461).

Moffet, A.T., Gubbay, J., Robertson, D.S., and Legg, A.J., 1972, in *External Galaxies and Quasi-Stellar Objects* (I.A.U. Symposium No. 44), ed. D.S. Evans, D. Reidel, Dordrecht, p. 228.

Moran, J.M., Barrett, A.H., Rogers, A.E.E., Burke, B.F., Zuckerman, B., Penfield, H., and Meeks, M.L., 1967, *Astrophys. J.* **148**, L69.

Moran, J.M., 1988 (private communication).

Ozernoi, L.M. and Sazonov, V.N., 1969, *Astrophys. and Space Sci.,* **3**, 395.

Pearson, T.J., Unwin, S., Cohen, M.H., Linfield, R., Readhead, A.C.S., Simon, R.S., Walker, R.C., Seielstad, G., 1981, *Nature,* **290**, 365.

Readhead, A.C.S. and Wilkinson, P.N., 1978, *Astrophys. J.,* **223**, 25.

Rees, M., 1967, *Mon. Not. Roy. Astr. Soc.,* **135**, 345.

Slysh, V.I., 1965, *Usp. Fiz. Nauk.,* **87**, 471, (Soviet Physics Uspekhi **8**, 852).

Whitney, A.R., Shapiro, I.I., Rogers, A.E.E., Robertson, D.S., Knight, C.A., Clark, T.A., Goldstein, R.M., Marindino, G.E., and Vandenberg, N.R., 1971, *Science,* **173**, 225.

Yen, J.L., Kellermann, K.I., Rayhrer, B., Broten, N.W., Fort, D.N., Knowles, S.H., Waltman, W.B., Swenson, G.W., 1977, *Science,* **198**, 289.