1/8-10/79 at AAS Meeting in Mexico City

—interviewed (without taping) Harlan J. Smith about 1957-63 Jupiter work at Yale ~~[illegible]~~ (w/ J.N. Douglas)

rec'd —he will send me a written summary of what he said, along w/ any pictures, etc. he can find

—most interesting aspect was that ~1961-62 Hewish visited Yale + Smith claims he there first saw the possibilities of interplanetary scintillations (which Smith + Douglas had somewhat thought about)

—first pub about this is by Smith at a ~1961 USNC/URSI meeting + final pub (very late) in ~1967 ApJ by Douglas & Smith

3/1 - Sent Thanx note

"H.J. Smith"

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February 19, 1979

Dr. W. T. Sullivan
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Seattle, WA 98915

Dear Dr. Sullivan:

Following up our brief discussion at the Mexico City meeting, I would like to summarize my recollections and those of Jim Douglas concerning the interesting period of early Jupiter radio astronomy at Yale.

Around the beginning of 1956, while I was still a Yale Instructor, Ken Philip and Jim Douglas began to haunt my office at the Observatory to suggest that we begin a program of radio astronomy. In those days only four or five of us in the astronomy faculty tried to run a complete graduate and undergraduate program plus research, and in particular we of course had no radio astronomy. However, Douglas suggested that I knew some astronomy and he knew radio, thus we should be able to get something started. Being willing to try almost anything once, I soon agreed. The plan was to utilize the war surplus equipment and energies and skills of the nearly-all-undergraduate Yale Radio Club, of which Jim was the head. The only scientifically interesting object on which their primitive equipment could profitably be used was the newly discovered decametric radiation from Jupiter (the sun was already somewhat beyond us in terms of sophistication of equipment and research programs but Jupiter was new and strong). A ham radio receiver and two primitive Yagis serving as an interferometer gave us a few signals which we thought might be Jupiter, as seen on an old Esterline-Angus chart recorder. In particular they emboldened us to approach the Research Corporation, which around 1958 gave a small grant (as I recall, $1,000) with which we were able to purchase several better amateur communications receivers, a couple of amateur Yagi antennae, a bit of test equipment, and some miscellaneous supplies. We skinned off branches from the south sides of some relatively tall trees, and hung the antennaes from them with guide ropes in such a way that they could be swung from east to west. A pair of these antennaes out in the country about a dozen miles from the center of New Haven made a perfectly respectable Jupiter interferometer which picked up quite a number of noise storms and allowed us to begin to look at their structure. The results from that year lead to a larger Research Corporation grant the following year, and to our first serious approaches to the National Science Foundation. As more substantial grants began to come in, we were able to buy better antennaes and even drives for them so as to be able to track Jupiter for a longer time, to mount them on towers both in New Haven and at the out-of-town site, and to install others at still more remote sites. Already we had adopted as one of our primary goals to try to unravel the fine structure of the Jupiter noise and isolate its various causes. The initial assumption of a number of people had been that much or most of the fine structure was simply caused by ionospheric scintillation at these relatively long (decametric) wavelengths. Because ionospheric scintillation should not be coherent over distances of many kilometers, we determined to establish a network of 20-MHz stations over baselines reaching up to 100 kilometers in order to find what the coherence characteristics really were. We also needed a very high speed recorder, capable of working down to a few milliseconds time constant. By 1960 our grants, and the energies of many of the Yale Radio Club members but most especially Douglas had made all of this possible. The most specific program on which we were embarking with the high speed recorder was to follow up on an idea of Kenelm Philip (now Professor of Astronomy at the University of Alaska) that it should be possible to measure the then-quite-unknown (within orders of magnitude) electron density in the interplanetary medium, from the dispersion of time of arrival of brief but identifiable elements in the Jupiter noise observed at frequencies separated by a few megahertzs. Again the widely separated stations would serve to show which elements in the noise were really extraterrestrial, and might therefore be used at the single station to look for the dispersion timelags at multiple frequencies.

It was also during these exciting days that I received a remarkably prescient letter from Geoffrey Keller, then Program Director for Astronomy at the National Science Foundation, dated December 28, 1960. I enclose a copy of his letter and of my reply. This letter must have been what triggered my subconscious to note and to realize at once the implication, in the late fall of 1961-62, during a lonely night observing run with the equipment, when the signals from the two stations 100 kilometers apart showed the same structural detail but with a timelag of about half a second. For some reason it had never previously occurred explicitly to any of us that we should be able to watch the "shadow pattern" of the solar wind zip at several hundred kilometers a second over the surface of the earth reflecting the radial motion of the solar wind. [In a note published in the November 25, 1961 Nature (page 741) but submitted a couple of months earlier we had called attention only to the occasional almost perfect correlation seen over 100 kilometers, likewise with a brief discussion in an abstract in the March 1962 Astronomical Journal; in both cases we noted irregularities in interplanetary plasma as a probable source of some of the effects.] However, if I correctly recall it was at the 1962 spring meeting of URSI Commission 5 in Washington that I gave a first brief report on this first direct evidence for the solar wind from the drifting bursts. We were still rather cautious since we had seen the event really clearly only once, more marginally on several other occasions. Unfortunately, I don't find in my files either the exact date of that meeting or a copy of the brief paper which I presented with a couple of slides showing the effect. But in the summer of 1962 Alan Barrett had a conference on radio astronomy at MIT. I do have a copy of the paper which I presented there at the end of July, one paragraph of which said,

> "Finally, some of the structure shows clear-cut delay ranging from several tenths of a second to several seconds between the stations 100 kilometers apart; this presumably arises from diffraction patterns produced by yet another class of ionospheric or magnetospheric wave moving at only about 100 kilometers a second, or perhaps from interplanetary plasma clouds with such velocities."

That spring semester of 1962, Tony Hewish had joined us on a sabbatical from Cambridge. I find a letter from him dated 9 August 1962 in my files with the sentences,

"...back home again we need to pinch ourselves occasionally to realize that it all happened. But we have a tremendous store of happy memories that will last a lifetime. Certainly our visit will rank as one of those fantastically nonstatistical peaks which sometimes occur in the noise function of human experience.

I have particular fond memories of Yale and its friendly astronomers...."

On several occasions including in our little radio astronomy seminars we had discussed the drifting patterns and their possible interpretation as irregularities in solar wind. Tony was quite interested in these developments at the time.

During the 1962-63 observing season we were able to get significantly more, better and clearer observations of this effect, and also to test hypotheses such as whether the drifting patterns would seem to change direction on the two sides of opposition, as should be the case if the effect was truly arising from the solar wind. But this was the period in which my ten years at Yale drew to a close and I began to make arrangements to move. That interrupted many things including publications; it was not until Douglas followed me to Texas several years later and got his radio program established again, that we settled down to write the definitive paper on the solar wind effects on Jupiter noise, which appeared in Astrophysical Journal, Vol. 148, p. 885, June 1967.

Meanwhile, Tony Hewish back in England had the insight to extend the idea of a tiny scintillating Jupiter source, casting its diffraction pattern on the earth through irregularities in the solar wind, to extra-solar-system sources, in order to use this technique to pick out those of very small angular size--the scintillators. To do so required building first a relatively large-aperture low-frequency array which would be able to record these relatively faint sources fast enough to permit the use of short enough time constants to look for scintillation effects in the solar wind. As I understand it, grad student Jocelyn Bell was assigned the job of building this antenna and putting it to use. Thus came into existence the first antenna which had a good chance of detecting the pulsars, and thanks to Jocelyn's skill and persistence (and obstinance ?) as an observer, they were indeed discovered. Their properties were then tracked down through the combined efforts of the Cambridge astronomers including especially Tony Hewish. Here ends our version of the story of the background to the discovery of pulsars, and how in a real sense it began with an attempt at Yale to use Jupiter to measure the density of the interplanetary electron plasma.

As another loose end I should add that around 1959 or 1960 Jim Douglas began to develop the bright idea of the white-light (or band-width-directivity) interferometer, for which over the next three or four years at Yale he developed a prototype primarily with NSF funds. However the problems were truly heroic to try to build and operate such a system under those conditions of granite rocks and hills, tall dense oak forests, massive radio interference, seemingly continuous rain or ice and snow...and although promising the system could scarcely have been called a real success at Yale. In 1965 Douglas brought it to Texas and in late 1966 and early 67 began to re-erect it in a somewhat improved form at our new radio site near Marfa about 40 miles south of McDonald Observatory in West Texas.

This is one of the world's ideal radio astronomy sites. It has grown to comprise about 4 square miles loosely covered with antennas and transmission lines, constituting an extraordinarily efficient radio-source-mapping telescope, which is now determining accurate coordinates to about a second of arc in both right ascension and declination of the brightest 50-100,000 radio sources at the two frequencies 330 and 380 megahertz, with some additional information concerning structure and variability. (There has been no radio astronomy at Yale since Douglas left.)

In addition to this account of the background to the discovery of pulsars, I have asked Jim Douglas for his memories of those early days (copy enclosed), and trust these reminiscinces may be of some interest to you and to the history which you are assembling.

Sincerely,

Harlan J. Smith

HJS:jcp

W. T. Sullivan III

FEB 21 1979

Radio Astronomy at Yale

(Recollections Without Looking up Documents)

JAMES N. DOUGLAS FEB. 1979

1. Inception

During my senior year at Yale (1955-1956) I applied for graduate study at various places - and accepted Yale because at that time (April 1956) the beginning of radio astronomy at Yale already seemed probable, and I couldn't resist being "in at the beginning".

I'm not sure who was primarily responsible - but I suspect Ken Philip was the one who principally had Harlan Smith more than half-convinced of the viability and interest of the idea. I knew that radio astronomy was in Yale's future in 1956, because I took a copy of Pawsey and Bracewell to my summer job at Lincoln Labs.

It wasn't yet decided (I entered grad school in Physics) but it was expected, and I felt that physics was the most flexible of all fall-back positions.

And the most wonderful thing of all was that none of us - Harlan, Ken or I - felt that there was the slightest question that given application success would be our reward.

We were three very different people - Harlan the astronomer, Ken, the iconoclast, skeptic, and incredibly creative jack-of-all-trades, and me the hardware man. We all grew to partake of each other's characteristics, and the sum of the three was greater than three.

We all learned from one another, and the first radio astronomy graduate class at Yale was given in 1956-57, with Harlan Smith as the responsible professor, and Ken and I as students and Charlotte Douglas as an auditor.

Never was professor-student relationship more blurred - we were all enthusiastic blind men leading each other all over the place. Lectures in this course alternated between the professor and the students - all of whom were simply trying to learn the subject from the ground up.

Lecture notes were mimeographed (!) and distributed to four people. Many wrong topics were treated (space charge in vacuum tubes) and many right ones (Appleton's equations).

Ken and Harlan had already concluded before I came on the scene that study of the recently discovered decametric radiation from Jupiter was a good entry topic into the field. As I recall, this was based on two things: First, Harlan had talked to Bernie Burke about it (one of the co-discoverers of the radiation); Bernie said it was a great entry level problem - strong source, simple equipment, etc. Second, Ken was attracted by the possibility of using the millisecond pulses (reported by Gallet and Kraus in 1956) as a means for determining the electron density in the interplanetary medium by dispersion measurements - exactly the same experiment now routinely carried out with pulsars. Ken gave a paper on this experiment at the AAS Harvard meeting (1956?, 1957?), probably the first suggested use of radio dispersion in an astronomical context. (Shapley thirty years earlier had noted that two-color observations of variables in external galaxies set upper limits on the interstellar and intergalactic medium.)

So we settled on Jupiter, and particularly on fast time structure, whose reality was then in doubt - the first Jupiter spaced-receiver experiment had been reported by Gardner and Shain, with no correlation of the 1-second bursts and no evidence of the millisecond pulses at all.

We of course had no grant money at all; Dick Brouwer, the observatory director made a few hundred dollars available for buying resistors, capacitors, and "ham" Yagi antennas. At least I presume it was Brouwer - Harlan made all the arrangements. At this point I enlisted the interest (and junk box!) of the Yale Radio Club (W1YU) and began building a very simple 2-element interferometer at 21 MHz, to be mounted on the roof of Hendrie Hall in downtown New Haven! This construction went on in the fall and winter of 1956-57, and we were on the air in March of 1957, with a 2.3λ baseline 21 MHz interferometer, whose key component was the W1YU NC-183 communications receiver.

2. The Learning Years: 1957-1960

The first Yale interferometer was a very crude device indeed, and poorly sited. And the three of us were just learning the observational part of the subject as well. But it worked. A dozen Jupiter storms were picked up during a couple of months of 1957. And in addition, other noise activity was recorded when Jupiter was out of the beam, which we in our enthusiasm thought was Saturn. Harlan even derived a rotation period for Saturn from the data, and we took our results on Jupiter and Saturn to the AAS meeting at Illinois. As I recall, we did not claim that Saturn was a source (perhaps John Kraus's troubles with Venus warned us) but that it might be, or probably was.

Tony Hewish was at that meeting, giving an invited paper on the then-raging Cambridge-Mills controversy; he was the first live radio astronomer I had met. We had brought Esterline-Angus charts of our "Saturn" events and we talked to him. I can remember him saying before looking "I'm going to try to shoot you down" (which was fine with us). Joe Pawsey was also there, and made a number of suggestions about improving our directivity so as to make identification more certain.

I myself felt that Hewish and Pawsey "shot us down" (in private, however); Hewish destructively and Pawsey constructively.

I was a first-year grad student, and learned a very important lesson; in fact, maybe I over-learned it since many a good result has been initially as inadequately supported as ours was.

From my present vantage point, I now believe that our "Saturn" events were terrestrial static in a fortuitous pattern; we have looked since then with instruments a hundred fold more powerful and with minds a hundred fold more sophisticated, and no Saturn emission was found in the 20 MHz region.

Meanwhile, Harlan had wangled a Research Corporation grant for our work, and had submitted a proposal to NSF (which also came through). Further, the Yale Observatory had acquired an out-of-town observing station, in Bethany, and construction of a building was underway.

Taking Joe Pawsey's words to heart, and also benefiting from some conversations with Burke, I completely re-designed the interferometer system, with two vital changes: (i) location of antennas in Bethany, at a much greater spacing and (ii) phase-sensitive detectors in the receiver (the earlier version only recorded the magnitude of the cosine component of fringe visibility). The antennas were longer Yagis with higher gain and were neatly hung from trees.

The output during 1958 was transmitted via telephone lines to the W1YU club-room at Hendrie Hall, so that again the volunteer club member observers could annotate the charts. Also coming into the Hendrie Hall basement were telephone lines from remote receivers we had set up - in Pound Ridge, N.Y. (actually in the backyard of Alfred Kellaher, a director of Research Corporation!), in Middletown, Conn. (with the assistance of Frank Zabriskie at Wesleyan) and at Pomfret, Connecticut (with the assistance of Jim McCullough at Pomfret School).

These spaced receivers were intended to determine whether the Jupiter fast time structure was imposed by the ionosphere or present on it intrinsically; one of the volunteer observer's duties was to make a high speed recording whenever Jupiter was active according to the Bethany interferometer. (One of the volunteer undergraduate observers who sat up all night with the equipment was John Dickel, another Barry Lasker, another Mike Davis - all now active professional astronomers.)

The results of the 1958 observations were presented by Harlan at the 1958 Paris Symposium on Radio Astronomy - basically that Jupiter wasn't very active that year, but also that there was significant correlation between the spaced receivers, suggesting that the fast time structure was indeed on the Jupiter radiation before hitting the ionosphere.

The next two years saw the construction of a ten-receiver spectrograph for investigating the frequency correlation of the fine structure (Barry Lasker), and of a full Stokes-parameter polarimeter, together with a second frequency channel to investigate Faraday rotation (Mike Davis). Bernie Burke's description of Jupiter as a simple entry problem was correct insofar as detecting the radiation goes, but far out in other ways! We were coming to the realization of the horrible complexity of the problem, and its interconnection with severe ionospheric observing difficulties.

Meanwhile, I had been writing to other observers to get a full collection of all data thus far obtained for a full-dress statistical analysis of this sporadic phenomenon, which was to be my dissertation. All but one group (Florida) were cooperative in this, and their withholding their data pending their own analysis was understandable, inasmuch as we were direct competitors in this area.

So by 1960, we had interferometers at several frequencies, which really worked effectively, a 10 channel spectrum analyzer, a polarimeter and much more understanding of this difficult observational problem. We had many data in hand, and finally real results began to appear, 4 years after start-up. It seemed a long time to me then, a short time to me now in the context of decameter Jupiter.

3. First Results

The most important thing we learned by 1960 was that observational characterization of decameter Jupiter was (and is!) a very complicated problem.

Concrete results included:

(i) Determination of the rotation period of Jupiter using data I had accumulated. This period was adopted by the IAU for describing Jupiter radio phenomena (although, as we shall see below, it was in error).

(ii) Proof that the fast time structure on the Jupiter radiation was of extra-terrestrial origin. This applied both to the millisecond component (whose reality we demonstrated convincingly) and to the 1-second component.

(iii) Barry Lasker's 10-channel spectrum analyzer showed that the Jupiter radiation drifted in frequency, and that the sense of drift is correlated with the central meridian longitude. Although we did not publish this finding for a while (except in my September 1960 dissertation), we certainly discovered this important characteristic independently from, and prior to, Warwick's much more extensive and impressive work.

4. Growth and Discovery

In September 1960 I joined the faculty at Yale as an instructor, which was perhaps symbolic of the fact that Yale radio astronomy had come of age - the training period over. It happened to coincide with the end of my personal traditional training period, but more importantly was more or less the time when Harlan had added this technique and point of view to that broader area which he already commanded. It was a sign of the times that the sky was the limit, anything was possible - and we begain talking about other projects to propel us into the "big time" of radio astronomy. As we shall see below, one such project started at Yale indeed had taken root and is bearing fruit in the plains of West Texas - many years later.

One thing that Harlan pushed very hard was to get a well-established radio astronomer to come to Yale and to share with us his experience. It was very logical that we should think of Cambridge, then (as now) a world leader in the subject, and specifically of Tony Hewish, whose previous work in scintillation in the ionosphere was right in line with what we were already thinking of as a key to understanding Jupiter's fluctuating radiation, thanks to a suggestion from Geoffrey Keller of the NSF. (This absolutely correct suggestion is in a letter from Keller to Harlan dated 28 Dec. 1960.)

Tony came for the spring term of the academic year 1961-62, and broadened our horizons immensely. He taught us how to deal with scintillation problems (among many other things), and learned from us the fact that Jupiter fluctuations might be "interplanetary scintillation", and saw our data. But at this point it was a somewhat lazily considered hypothesis.

All this changed when Harlan noted that the fine-structure records we were obtaining with our spaced-receiver network showed identical L-pulse structure, but with time delays between telescopes.

So in 1962 (March) Harlan suggested interplanetary scintillation as a mechanism for the 1-sec component of Jupiter's fluctuations; this at an AGU meeting while Hewish was still in residence at Yale.

Harlan had a hard time convincing me - once burned with Saturn - but he was perfectly correct; finally events showed up on our spaced receivers which anyone would believe, and which I published in my review article on decameter Jupiter (IEEE Trans. Mil. Elec. MIL-8, July/Oct. 1964). The Yale work on fast time structure was finally completely published in 1967 (Ap.J. 148, 885-903).

I have always been disappointed that Hewish has never referred to our prior and parallel work - of which he was certainly well aware. I am also aware of a great debt to him for things he taught us at Yale - not the least of which was ionospheric scintillation theory.

During the period 1961-1965 the Jupiter interferometers were up-graded again: longer interferometer baselines, tracking crossed-Yagi antennas on a clever equatorial mount designed by James P. Rodman. Realizing that the rate of adding data on this sporadic radiation for statistical purposes could only be increased by 24-hour observations, steps were taken to set up stations at other longitudes. Graduate student F. A. Bozyan while on a Fulbright to India set up a 22.2 MHz interferometer at Kodaikanal Observatory, India; this operated successfully for a number of years. A cooperative Yale-Goddard Space Flight Center (J. K. Alexander) program saw five 22.2/16.7 MHz interferometers built at Yale (under Bozyan's supervision) and installed and operated by Goddard at five spaced longitudes around the world. Parts of this system are still in operation, and have significantly contributed to the body of Jupiter data.

One of the results of the vastly increased data rate was the realization that the System III period appeared to be a bit fast, suggesting that some geocentric observing effect caused the apparent position of the source to move, or less likely that Jupiter's rotation itself was slowing down. This Smith and I published in Nature (in 1969, I believe), and was subsequently clearly explained by Gulkis and Carr as due to the changing Jovocentric declination of the earth, plus narrow beaming of the radiation.

Digitizing equipment plus large but simple antenna systems were installed at Yale in order to measure the absolute flux density of the Jupiter radiations. This work, which constituted Bozyan's 1966 dissertation, remains the most accurate to date.

Ken Philip, who was completing his dissertation on solar radio noise observed with the Jupiter equipment, once again stimulated a large project with a clever suggestion: why not get directivity by operating an interferometer as a white-light interferometer, in which only the central fringe is formed? This suggestion, made in 1960, was after but independent of Vitkevitch's earlier suggestion and Sam Goldstein's earlier use (for solar work) of this phenomenon.

I investigated the possibility extensively, noted that it was a natural for discrete-source survey work, particularly because of the ease of multi-beaming and began planning (in 1960) to install one at Yale, primarily to measure radio source declinations, which were then very poorly known. G. C. Brooks, G. F. Moseley and G. W. Torrence had then joined the group as graduate students, and construction of the system began, once again with the aid of a Research Foundation grant.

After many difficulties, the system was ready - and then Kennedy Airport activated a new radio transmitter right in the bandpass of the instrument. This plus other teething difficulties set the program back for many months; we began to use the instrument in 1964.

5. The Move to Texas

Harlan resigned from Yale in 1963 to become Director of the University of Texas McDonald Observatory and to preside over a substantial expansion of the then tiny astronomy department.

He was very interested in adding a radio astronomy capability at Texas, and West Texas was certainly a natural habitat for large radio telescopes: remote, flat, dry, spacious.

He offered me a position at Texas, in the spring of 1965, which I accepted, having clearly seen that the east coast and particularly the wooded hills of Connecticut were not optimum for observational radio astronomy.

Yale decided (for the same reasons) not to continue in radio astronomy, and generously transferred ownership of the radio telescopes to Texas. The white light interferometer was packed for moving by G. F. Moseley during 1965-66, while a last (and best!) Jupiter apparition was observed by Bozyan and L. P. Pataki, and I was a regular commuter from Austin to New Haven.

The Yale equipment, after significant upgrading, was re-installed in spring, 1967 at the University of Texas Radio Astronomy Observatory (near Marfa); less than one year of Jupiter data was missed.

Not only did the equipment come to Texas, but also essentially the entire Yale Radio Astronomy group: Smith and I, Bozyan as a Research Associate, and Brooks, Moseley and Pataki to complete their Yale dissertations in absentia; Torrence transferred to UT graduate school.

So in a very real sense, although radio astronomy at Yale is no more, the radio astronomy group at Yale changed its name and continues to grow.

6. Yale Radio Astronomy Ph.D.s

James N. Douglas; H. J. Smith, supervisor

Kenelm W. Philip; H. J. Smith, supervisor

F. Arakel Bozyan; J. N. Douglas, supervisor

Clinton C. Brooks; J. N. Douglas, supervisor

Louis P. Pataki; J. N. Douglas, supervisor

Gerard F. Moseley; J. N. Douglas, supervisor