PLANETARY RADIO ASTRONOMY, FIFTY YEARS AGO AND FIFTY YEARS HENCE

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Abstract

Finding that Jupiter was a powerful source of Radio Noise was completely unexpected fifty years ago. The textbooks had nothing to say about what the radio properties of the planets might be; their thermal radio emission might have been noted, but radio astronomy was not expected to be a subject for planetary studies. The period 1933–1955 had been a rich time for new discoveries by the radio astronomers, the discoveries had revealed new classes of phenomena that revolutionized both galactic and extragalactic astronomy, and hindsight might lead to the question "Why should not the planets show unexpected properties also?" but as usual it was the observers who had to lead the way. The radio bursts of Jupiter provided the first step in remaking planetary astronomy.

It is always dangerous to look into the future, but predictions can amuse later generations and sometimes turn out to be right. The next phase of planetary studies is already developing: the study of exoplanets. Already, the optical observations have shown that Jupiter-like exoplanets occur with unexpected frequency close to the parent star, with orbital periods of days to months. It is not an unreasonable projection to expect that radio studies of exoplanets will yield similarly unexpected results, a prediction already made at the time of the third workshop on planetary radio noise a decade ago. The first detections might occur soon at decametric wavelengths, with existing equipment, but much larger collecting areas will surely be needed, and some decades may elapse before the appropriate radio telescopes can be built. The observational results will at the least give insight into the strength and orientation of the magnetic fields of exoplanets.

1 Introduction

My colleague Ken Franklin has given an accurate account of the events of that day in late March of 1953, when three months of data from the 22 MHz Mills Cross of the DTM revealed that Jupiter emitted radio noise that was far more intense than anyone had any reason to expect. This followed the pattern of earlier radio astronomy discoveries,

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starting with Jansky's first discovery in 1933 of the intense radio background of the Milky Way System. There was a new world to be discovered, and the main tool to study that unexpected world would come from radio and the young technology of electronics. Only Grote Reber followed up Jansky's first work, but during the Second World War Hey had shown that the powerful radio signals that alarmed the English radio countermeasures people did not come from a new and fearsome German jamming device but from the sun. Immediately after the war, both the English and Australian radio physicists established that such powerful radio outbursts were associated with active regions on the sun, and their accompanying flares. Hey, in following up Reber's radio maps of the sky, noticed that there was a region in the constellation Cygnus that fluctuated in intensity. Learning of this, Bolton and Stanley used the Lloyd's-mirror interferometer, that had been used on Dover Heights at the mouth of Sydney Harbor to study the solar outbursts, to show that the signals came from a compact radio source. They subsequently showed that there were other discrete radio sources in the sky, one of them being the Crab Nebula, which was known to be the remnant of the Supernova of 1054, observed by the Chinese. Other identifications were made, culminating in the discovery in 1951 that the Cygnus radio source was a distant galaxy, even though it was the second brightest radio source in the sky, excepting the sun. In the same year, Ewen and Purcell discovered the 21-cm radio line of atomic hydrogen, following the prediction by van de Hulst a few years earlier. Radio astronomy produced surprises all along the way.

A search of the literature reveals that planetary astronomy was believed to be in good shape. Schiaparelli's observations of Mercury were believed to have established that it kept the same face toward the sun, tidally locked. The rotation of Venus was less securely known because its surface markings were so vague, but it was generally believed that it, too, was tidally locked the same way Mercury was. Infrared measurements had shown that Venus was cold, close to what one might expect in thermal equilibrium with the sun, showing no sign of a greenhouse effect. It was known that ejecta from the sun caused magnetic storms on earth, but the conjecture that there was a solar wind lay a few years in the future. The earth's magnetic field had been well measured, but dynamo action to generate that field was regarded as a possible, but still speculative, hypothesis, and there seems to have been no serious work on what magnetic field might be possessed by the other planets.

First, though, let me say a few words about the work at the DTM that made our work possible. The Department of Terrestrial Magnetism of the Carnegie Institution of Washington (DTM-CIW) was founded in 1904, but developed into, in effect, the Physics Department of the CIW, largely through the force of character of Merle Tuve. While he was still a graduate student, he invented the radiosounding technique for studying the ionosphere, then he entered nuclear physics and performed the experiments that demonstrated the charge-independent nuclear forces, and then, during World War II he was director of the effort that developed the proximity fuse. Under his directorship after the Second World War, the DTM concentrated on forefront research areas that had not yet become "big science". Tuve believed that there was a place for a laboratory that explored new areas of science, but without dependence on big government money, except in unusual cases. He had a marvellous nose for new fields before they opened up, and as an expert in radio techniques, he could not resist the attraction of exploring extragalactic radio sources, studying unknown physics at great distances. Ewen and Purcell's discovery of the 21-cm hydrogen line, followed by Oort's preliminary study of the spiral structure of our galaxy, opened up the possibility of studying gravity across the Milky Way. Tuve had already contacted Purcell, who gave him his 21-cm spectrometer.

He knew that the work had to start with expert advice, and he invited Graham Smith (now Sir Francis Graham-Smith) to visit both the DTM and the Carnegie Observatories in Pasadena in 1953–54. He also brought in two new physics PhD's, John Firor from Simpson's cosmic-ray group in Chicago, and me, a microwave spectroscopist from Strandberg's laboratory at MIT. Graham Smith, an expert in radio interferometry from Ryle's group in the Cavendish Laboratory, had made the positional measurements that made the identification of the Cygnus A radio source possible (along with Bernard Mills from CSIRO Radiophysics in Sydney). In January 1954, he and Jesse Greenstein organized a special symposium on radio astronomy, reviewing the entire field. The event was held at the CIW headquarters in Washington DC. It was an exciting event for the two new radio astronomers at the DTM to experience; the attendees included many well-known international names, and representatives of the National Science Foundation were also in attendance, along with research directors from the armed forces research organizations. The result was a surge in US support for radio astronomy, and shortly thereafter the process of founding the National Radio Astronomy Observatory began.

Tuve expected Graham to conceive a new project, and he proposed that a low-frequency Mill Cross would be ideal (Bernard Mills, the inventor of the concept, had visited the DTM for a month in the winter of 1953–54). This fitted Tuve's philosophy well; its cost would be within the scope of Carnegie funds and it was hands-on science that would explore a new area of astronomy.

Graham settled on 22 MHz as an unexplored region of the spectrum, although Shain, at CSIRO radiophysics in Sydney, had made some observations in a nearby frequency band, but with small antennas. To test for freedom of interference, we built a simple interferometer, and established that the band was largely clear (it was a marine band for long-distance communication. We were at sunspot minimum, and the marine users mostly had to use lower frequencies). In early June, 1954, a powerful, fluctuating source passed through the interference pattern (approximately on my birthday) and Graham's analysis showed that it was close to the sun, but considerably farther out in the corona than the usual solar radio bursts. Graham commented that Shain had seen solar bursts of similar type, and after noting this, we set to work on building a big Mills Cross.

The DTM had leased two large adjacent fields near the Potomac River on River Road, near Seneca, Maryland. The largest, 96 acres in extent, would accommodate the new antenna, a pair of crossed arrays of 128 dipoles each, oriented approximately northwest-southeast and northeast-southwest, with each arm slightly longer than 2000 feet. The signals were cross-correlated by phase switching (the Mills concept) and yielded a pencil beam slightly over a degree in size. The dipoles were capacitively-loaded, made by snipping 78-ohm twinlead appropriately, giving an impedance of 300 ohms, and connected in groups of four, where a balun matched them into the branching transmission-line system that brought the two signals to the trailer that housed the receiver. We cut all the wires ourselves in the evenings, in the living rooms of our houses. The dipoles were supported by 4×4 wooden posts, 12 feet long, that had to be planted in the ground. Some management skills were needed, for the farmer who had the drilling attachment on his tractor had drunk too much whiskey, and had to be guided explicitly but diplomatically to each location to drill the postholes correctly! We filled in the postholes and mounted all the wires ourselves. First observations were taken at the zenith, showing Cygnus-A, just before Graham's departure in September 1954. I rephased the array to observe at the declination of the Crab nebula, followed by the events that Ken Franklin, newly arrived from Berkeley, has described.

Ken and I announced our discovery at the 1955 spring meeting of the American Astronomical Society, and published the results in the March 15, 1955 issue of the Journal of Geophysical Research. I attended the Jodrell Bank Radio Astronomy Symposium about four months later, and in the meantime Shain had reanalyzed his data. He showed that it was synchronous with the rotation of Jupiter, but closer in period to System II (the non-equatorial regions) rather than System I (the equatorial belt). After the meeting, Ken and I constructed a simple interferometer with each element being an array of eight dipoles, but cross-polarized so that the polarization properties of the radiation could be determined. The observations showed immediately that the Jupiter bursts were circularly polarized, demonstrating that almost certainly Jupiter had a magnetic field. We found that the radiation was right-hand circularly polarized, and published the results in 1956. Our synoptic observations continued, while two new observatories were founded, at the University of Florida (Gainesville) by Alex Smith, and Tom Carr, and by Jim Warwick at Boulder Colorado.

The synoptic data showed clearly that the periodicity did not match System II, a fact independently recognized by Jim Warwick, Roger Gallet, and myself. The apparent rotation period of the burst regions appeared to be slightly longer than System II, and Roger proposed that it should be called System III. We all recognized that the radio rotation period was probably more closely related to the rotation of the main body of Jupiter, unaffected by the systematic motions in the atmosphere that affected Systems I and II. System III was officially adopted by the International Astronomical Union, largely through the efforts of Tom Carr, and now appears in the tables of the Astronomical Ephemeris.

Warwick equipped his station with a radio spectrometer, demonstrating clearly that the Jupiter noise usually cut off abruptly at about 38 MHz, although on some occasions the upper limit was somewhat higher. This, combined with the observed circular polarization, meant that the ionosphere and the magnetic field of the planet were directly involved in determining the character of the radiation; the decimetric observations of the synchrotron radiation from the Jovian radiation belts (described below) provided the final proof. The surface magnetic field of Jupiter had to be of the order of 10 gauss, although the exact value depended on the radiation model and upon the plasma frequency. The state of the art in the 1960's can be found in the reviews by Warwick (1967) and by Carr and Gulkis (1969).

Along the way, we carried out an experiment that, in retrospect, might have had bad consequences. At this stage, a natural question to ask was whether or not we were seeing lightning storms on Jupiter. We set up a simple omnidirectional antenna and in the late spring of 1955, a prime season for thunderstorms in Maryland, we waited for a lightning storm to come by. A good storm came in due course, and we quickly set up the equipment.

As Ken was making the final cable connection, a cloud-to-cloud lightning stroke flashed overhead, and the induction field was so strong that he received a good buzz. We were asking for trouble, in the open field, a prime target for a lightning stroke to the ground. Well, the results were clear. The terrestrial lightning strokes generated a strong signal at 22 MHz, but the noise power was at least a million times too weak to explain the Jupiter bursts.

The year 1957 brought our Jupiter studies to an end. Ken had already left to take up his appointment at the Hayden Planetarium, and during the autumn of that year I joined the Carnegie seismic expedition to the High Andes, as an amateur seismologist and truck driver. We used the artificial earthquakes set off by mining in the great copper minds of Peru and Chile, and were searching for the roots of the Andes as part of the International Geophysical Year. The roots could not be detected in the data, and the anomalous absorption of the seismic signals gave an indication, only verified years later, that the oceanic plates being subducted at the continental margin, pushing up the Andes and generating volcanoes, jumbled up the substructure. Two other significant events, both having an influence on the DTM radio astronomy program, occurred. The first was the launch of the Soviet Sputnik (it was called El Satellite in the local newspapers because "sputnik" is a Slavic word that is not euphonious to Spanish ears). The second event was tragic; Howard Tatel, whose role has been described by Ken Franklin, fell ill, and within a few weeks of the end of the expedition was dead of a brain tumor. I threw my efforts into the radio hydrogen program, in which Howard had been a mainstay, and enjoyed a fresh direction in my scientific career. The study of Jupiter's radio noise was in other capable hands by that time, a brand new 60-foot dish had been constructed at the Derwood field station, and the hydrogen distribution of the Milky Way System was a fascinating new subject.

I kept track of the developments concerning Jupiter's radio noise, however, and the space age developments eventually cleared up the main points of the puzzle. The first key event, in 1958, was Jim van Allen's discovery of the earth's radiation belts. It did not take long to realize that the circular polarization of the decametric radiation meant that there was a magnetic field on Jupiter, and as the radio observations of Jupiter at shorter wavelengths proceeded, it became clear that the decimetric (not decametric!) radiation was neither plasma radiation nor thermal radiation — it had to be synchrotron radiation from energetic electrons in the van Allen belts of Jupiter. I think it was Frank Drake who first articulated this in 1959, when his Green Bank observations showed that the radiation from Jupiter at 33 cm wavelength was clearly nonthermal. The observations of Drake and Hvatum, followed up by the work by Roberts and Stanley and by Radhakrishnan and Roberts clinched the matter. Jupiter had a dense and lethal van Allen belt, and at the Owens Valley Observatory the finishing touches were added by Glenn Berge and his collaborators observing the linear polarization of the radiation, rocking back and forth with the period of System III. Not only was the magnetic field inclined to the rotation axis, but the dipole component was offset from the axis, as it should be in order to satisfy Cowling's theorem. One more astonishing fact was discovered by Bigg in 1964: the radio bursts, in addition to being synchronous with the System III rotation, were also controlled by the apparent longitude of the Galilean satellite Io. The puzzling question of how Io might control the Jupiter bursts was resolved by Goldreich and Lynden-Bell in 1969, who noted that Io, moving through Jupiter's large magnetic field, was a homopolar generator, with the emf generating a current tube along the magnetic field lines to the ionosphere of Jupiter, where the radio bursts would be excited. The current tube would not respond to the changing aspect of Jupiter's magnetic field because the large inductance would give too large a time constant. It became clear that the emf generates Alfvén waves that propagate along the field lines, transferring the energy to Jupiter's ionosphere. Incidentally, the combined decametric and decimetric radio observations had an unforeseen practical result: the radiation belts would have been lethal to ordinary spacecraft designs, and the missions that NASA would fly to Jupiter had to be radiation-hardened.

The years following 1955 were dramatic times for radio studies of other planets in our solar system, studies led by Cornell Mayer and his group at the Naval Research Laboratory, who published their findings in 1958. They found an exceptionally high apparent temperature for Venus, in contrast to the early infrared measurements. As it became clear that Venus had a high surface temperature, this led to Alan Barrett's 1960 model for Venus that invoked a thick carbon dioxide atmosphere, with an admixture of water; the result was a greenhouse effect of heroic proportions, hot enough to melt lead at the planet's surface. Mercury was also detected, but did not show a phase effect, as it should if it always kept the same face to the sun. Pettengill's radar measurements showed conclusively that, although Mercury was tidally locked to the sun, it was in a 3/2 mode, making three rotations every two revolutions. The conclusions from the optical observations were wrong; they were flawed by data selection. Schiaparelli had observed at Mercury's greatest elongation, and bad weather had kept him from detecting the true rotation. Shortly afterwards, radar measurements by Goldstein at Goldstone Lake, NASA's deep space tracking station, showed that Venus was not in tidal lock to the sun, but was in slow retrograde rotation, a result not dreamed of by earlier astronomers.

Now, having discussed the unexpected character of our own solar system when radio observations began, let me turn to the future. The fifty-year horizon is speculative indeed, but even now a program can be foreseen, and some of the possible developments are already in preparation. At the third Planetary Radio Noise Workshop here in Graz, in 1991, I launched my first prediction, or rather a suggestion, and the background is a story worth telling. A few years earlier, in 1984–85, I was enjoying a sabbatical at CalTech, and as a radio astronomer much of whose work had involved interferometric methods, I pondered the question of whether or not planets of other stars might be detected, despite the glare of the parent star. Interferometry, because of its selection of Fourier components, was a natural technique to examine. An order-of-magnitude calculation showed that in the infrared and visible parts of the spectrum, not only would large planets be detectable, but one might expect to detect an earth-like planet, and even find evidence of life by spectroscopic examination of the A-band of Oxygen in the near infrared.

I wrote this up in a memorandum, which I circulated, and in a short time it developed into an article, published in 1986. This might have been the reason why I was asked, in 1988, by NASA's Office of Solar System Exploration, to head a study that would outline a program for exoplanetary detection. This became NASA's TOPS working group (Toward Other Planetary Systems, published 1992). At the time of the third Graz workshop, we were preparing our final recommendations, so the subject was very much on my mind.

In the Soviet Union (soon to become the former Soviet Union), things were in turmoil, and I knew that my friends in Kharkov, in the Ukraine, had built a large low-frequency radio telescope, for the 15-meter region of the radio spectrum. In preparing my talk at the close of the session, it was natural to estimate the chances that the Kharkov array might be able to detect radio bursts from a Jupiter-like exoplanet, orbiting a nearby star. Thanks to the array's large collecting area, a quarter of a square kilometer, detection would be easy, provided that Jupiter is not a unique planet among the stars. This is not likely, because observations starting with the 1995 discovery by Major and Queloz have shown that Jupiter-size exoplanets are common. All it takes is a magnetic field, and a close-in satellite to act as a homopolar generator. The principal unknown is the size of the magnetic field, but experience in our own solar system shows that a rotating planet with a fluid, convecting core is highly likely to generate a substantial field. The excited footprint in the planet's ionosphere would generate radio radiation and the upper frequency limit of the radiation would give the intensity of the magnetic field near the surface of the planet.

Exoplanets were only a speculation in 1991, but not an unlikely expectation, and Mayor and Queloz opened the new field with their discovery in 1995 that there was an exoplanet, not unlike Jupiter, orbiting 51 Pegasi, but much closer to the star than one might have expected. Since then, as the list of discovered exoplanets has expanded to a hundred and fifty or so, the general rule that we do not understand planetary formation very well has been fully confirmed. Giant exoplanets, remarkably close-in to the parent star, are a common phenomenon. The occurrence of exoplanets of more modest mass is not known because of the experimental limitations, but in time these will also become known. At the present time, observational selection is biasing the picture, because both radial velocity and transit methods favor the detection of massive, close–in exoplanets. The field is changing rapidly, and when astrometric observations start to deliver data (the astrometric and radial velocity methods are nicely complementary) we can expect more surprises. The work of the future depends upon the results of the present, and we can all hope that the work in progress will provide encouragement in the near future, at the next gathering of this group in Graz.

The first radio detection of an exoplanet may well occur before that time, as a result of the collaboration between the Austrian Space Research Institute, the Kharkov Radio Astronomy Institute, and the Meudon Observatory is in progress, and those researchers are fully aware of the potential of their work. The notion that we might measure the magnetic field of an exoplanet, and its rotation period, and detect the presence and orbital period of a close–in satellite is a wonderful spur to action.

Next, let me pass to developments in the near future. Other large low-frequency arrays are in progress. The Dutch LOFAR project is proceeding, and preliminary work on new proposed low frequency arrays is also under way. The Mileura Wide-Field Array is a joint project of CSIRO and MIT-Haystack Observatory, and the Naval Research Laboratory-Southwest Research Institute array is planned for the southwestern US. Both of these will have enough collecting area to detect radio bursts from exoplanets.

Finally, let me address the more distant future. The Square Kilometer Array (SKA) is an international project with collaborating institutions spread around the globe. Its present specifications, 100 MHz to 22 GHz, have been set for other purposes, and the low-frequency limit is a bit too high to be promising for the detection of exoplanets, but the rule of surprise is always present. Besides, its time scale (construction in the 2010-2020 time frame) is sufficiently far in the future to allow for adjustments, if new discoveries are made by the existing projects. One must recognize reality: the SKA is a billion-dollar project, and scientific projects of this magnitude take a long time to develop. The year 2020 is only 15 years in the future, but if it turns out that projects aimed at completing our understanding of exoplanets, including earth-like planets, receive enough support, the additional information from observations in the radio spectrum may well prove irresistible.

If discoveries made by the MWA, or the Kharkov array, or by the Southwestern Array turn up dramatic new results concerning exoplanets, it is not unrealistic to dream of a much larger low-frequency radio telescope, having ten times the collecting area of the Kharkov array. On the scale of projects to detect earth-like exoplanets optically, the cost would appear to be not unachievable. I would propose that an array that is usable over the frequency range 15–75 MHz, say, might well be built during the 50–year time scale that I am projecting. Expressed in wavelengths, the number of dipoles would be $N = 2.5 \cdot 10^7/\lambda^2$, for wavelength λ in meters. For a central wavelength of 6 meters, this would imply about two-thirds of a million dipoles. In reality, a single array of dipoles could not achieve the desired 5:1 bandwidth, and more elements (or more complex elements) would be needed. Scalar antennas would not do, since their collecting area scales as λ^2. Wider-band elements would be more complicated than dipoles, but perhaps with clever design they would not be excessively more complex. For the present estimate, one might take the equivalent of half a million elements. The assemblage of rods and wires should not be expensive, if produced in quantities of a million; at $100. per element, the cost would come to something like $50 million in today's dollars. The real cost would be in the interconnections and the processing, and that cost fifty years hence is hard to guess. In comparison to the SKA, there is one major simplification, because the array would not have to be spread over hundreds or thousands of kilometers, and it would certainly be placed at an already-developed site, known to have low r.f. interference levels. Only collecting area is needed, and so the array could be compact, simplifying the r.f. distribution and the signal processing. The distribution system might cost another $50 million, and the combined facility cost and signal processing might add something in the range of $50–100 million. The bottom line is that a low-frequency array of a scale suitable to the observing of planetary radio noise for a wide range of nearby stars, with an order-of-magnitude greater collecting area than anything being planned today, might have a cost of the order of $200 million or less in current dollars. Not a trivial sum, but its feasibility will hinge on the results obtained over the next few years by the new generation of planetary radio astronomers.

As the well-known American philosopher, Yogi Berra, once said, “Predictions are hard to make, especially about the future.” Nevertheless, the future depends upon today, and we can expect interesting results soon from observations by the Kharkov array. The next generation of instruments are already in the proposal and preparation phase, and the instruments half a century from now will depend upon that work of the present. The general rule has been that planetary astronomy is a science of the unexpected, and the hard work of the present will surely result in the unexpected developments of the future.

In closing, I would like to thank the hospitality of the Austrian Institut für Weltraumforschung at Graz for their hospitality in organizing this workshop and, indeed, their sponsorship of this continuing series of workshops on planetary radio emission.

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