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THE NEW ASTRONOMY: OPENING THE ELECTROMAGNETIC WINDOW AND EXPANDING OUR VIEW OF PLANET EARTH

WAYNE ORCHISTON
Editor

Springer

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VOLUME 334

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THE NEW ASTRONOMY: OPENING THE ELECTROMAGNETIC WINDOW AND EXPANDING OUR VIEW OF PLANET EARTH

A Meeting to Honor Woody Sullivan on his 60th Birthday

Edited by

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Cosmic evolution is depicted in this image from the exobiology program at NASA Ames. Upper left: the formation of the stars, the production of heavy elements, and the formation of planetary systems, including our own. At left: prebiotic molecules, RNA and DNA are formed within the first billion years on the primitive Earth. At center: the origin and evolution of life leads to increasing complexity, culminating in intelligence, technology, and astronomers. Upper right: contemplating the Universe. The image was created by David DesMarias, Thomas Scattergood and Linda Jahnke at NASA Ames in 1986, and reissued in 1997.

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FOREWORD

This is an unusual book, combining as it does papers on astrobiology, history of astronomy and sundials, but—after all—Woody Sullivan is an unusual man. In late 2003 I spent two fruitful and enjoyable months in the Astronomy Department at the University of Washington (UW) working on archival material accumulated over the decades by Woody, for a book we will co-author with Jessica Chapman on the early development of Australian astronomy. The only serious intellectual distraction I faced during this period was planning for an IAU colloquium on transits of Venus scheduled for June 2004 in England, where I was down to present the 'Cook' paper. I knew Woody was also interested in transits (and, indeed, anything remotely connected with shadows—see his paper on page 3), and in discussing the Preston meeting with him it transpired that his 60th birthday was timed to occur just one week later. This was where the seed of 'Woodfest' began to germinate. Why not invite friends and colleagues to join Woody in Seattle and celebrate this proud event? I put the idea to Woody and others at UW, they liked it, and 'Woodfest' was born.

The hard work involved in organising the logistics of the meeting fell into the capable hands of the Local Organising Committee, Bruce Balick, Jim Evans, Bruce Hevly (co-Chair) and Karl Hufbauer (co-Chair), while the program was addressed by a Scientific Organising Committee comprising John Baross, Ron Bracewell, David DeVorkin, Steven Dick, Ken Kellermann, Wayne Orchiston (Chair), Robert Smith, Richard Strom and Virginia Trimble. For his part, Woody became a *de facto* member of both Committees, and an excellent sounding board for our various ideas and the innumerable problems associated with organising a meeting.

Woody was also invaluable when it came to attracting speakers, particularly those in the astrobiology fold who lay well outside my own sphere of contacts. The final product was a meeting that had an almost equal mix of history of astronomy and astrobiology papers interwoven through the program, and three papers which we referred to as 'Woody indulgences', on sundials, art and the history of science. The full program is shown on pages xi-xiii. In addition, Frank Drake kindly agreed to present a public lecture, which was organised by the LOC and Linda Khandro, in conjunction with the University's Center for Astrobiology and Early Evolution.

The SOC also arranged a reception and the Conference Banquet. This unforgettable latter event was on the night of Woody's birthday, down on the waterfront, and in addition to singing "Happy Birthday" to Woody we all joined him in a rather forgettable rendition of a number of rather forgettable Tom Lehrer songs, including "The Elements"! All in all it was a memorable night.

At the end of the Conference, some participants took part in a one-day outing to Mount Rainier. This enjoyable scenic field trip (see page xvi) was organised by the SOC.

From the start, we hoped to publish the papers from Woodfest, so it was very pleasing when Harry Blom from Kluwer (now Springer) heard about the Conference and approached us with a proposal that we could not refuse. My only regret is that most of those who presented astrobiology papers at Woodfest had already published much of their material elsewhere and so decided not to contribute to this volume. Those papers that are included are marked by asterisks on pages xi and xii.

Finally, it remains for me to thank the UW Kammeyer Fund and members of the LOC and SOC for making Woodfest happen; all those who presented papers at the conference; Harry Blom, Sonja Japenga and Kirsten Theunissen from Springer for seeing this volume safely through the press; Wolfgang Dick, Hilmar Duerbeck and Sandra Ricketts for helping me chase down those all-too-elusive references; Bruce Balick, David DeVorkin and Joe Tenn for kindly supplying photographs; and last but not least, our dear friend and colleague, the inimitable Woodruff T. Sullivan, III, for making the very concept of 'Woodfest' a reality. Happy Birthday Woody; these proceedings are our final—if somewhat belated—present to you. Enjoy …

Wayne Orchiston
Chair, SOC

PARTICIPANTS

Peter Abrahams (USA)
Bruce Balick (USA)
John Baross (USA)
William Baum (USA)
Keith Benson (USA)
Ron Bracewell (USA)
Ron Brashear (USA)
Christopher Chyba (USA)
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Marshall Cohen (USA)
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Frank Drake (USA)
Jim Evans (USA)
Richard Gammon (USA)
Miller Goss (USA)
Mott Greene (USA)
Alastair Gunn (UK)
Tom Hankins (USA)
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Richard Strom (The Netherlands)
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Paula Szkody (USA)
Joe Tenn (USA)
Daniel Tomandl (USA)
Virginia Trimble (USA)
Ed Turner (USA)
George Wallerstein (USA)
Peter Ward (USA)
Dan Werthimer (USA)

‘WOODFEST’ PROGRAM

Papers published in these Proceedings are followed by an asterisk.

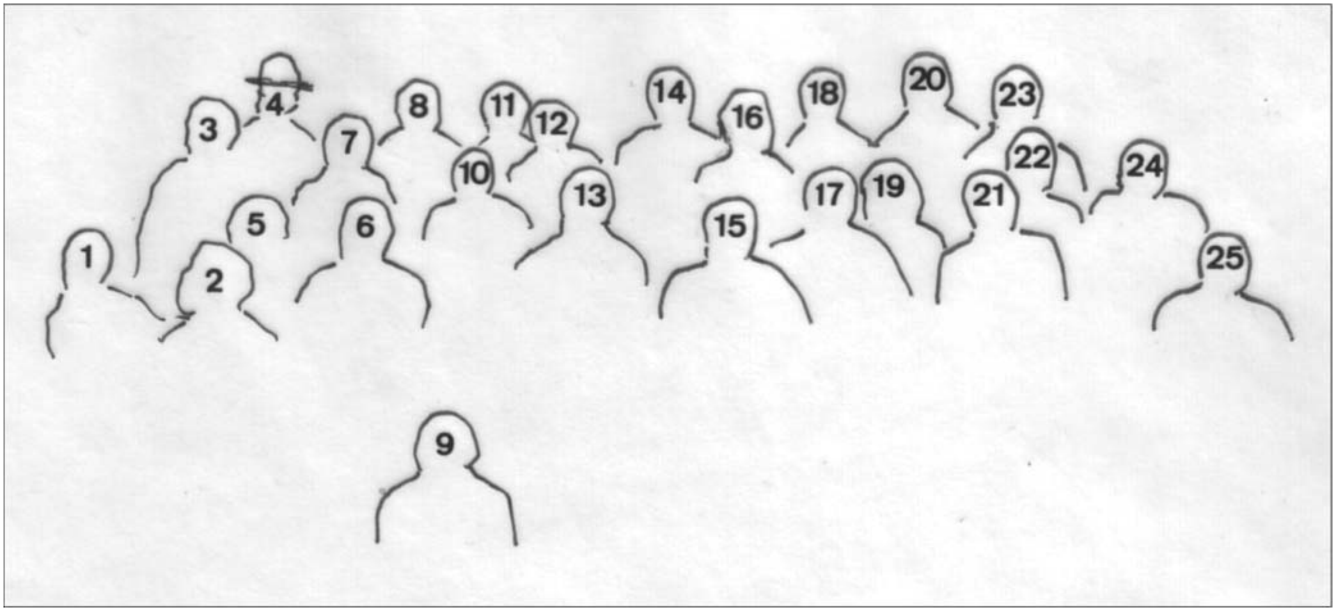
Day	Time	Program
Wednesday 16 June		*Chair: Bruce Hevly & Karl Hufbauer*
	9.00	Bruce Hevly & Karl Hufbauer: General house-keeping
	9.10	Woody Sullivan: Welcome
	9.15	Bruce Balick: Introduction
	9.30	Steven Dick: “The Biological Universe revisited”*
	10.00	Chris Chyba: “Contingency and the cosmic perspective”*
	10.30	*Morning Tea*
		Chair: Robert Smith
	11.00	Ken Kellermann: “Grote Reber: founding father of radio astronomy and professional amateur”*
	11.30	Alastair Gunn: “Rays, radiants and radishes: early radio astronomy at Jodrell Bank”*
	12.00	Richard Strom: “Radio astronomy in Holland before 1960: just a bit more than HI”*
	12.30	*Lunch*
		Chair: Steven Dick
	2.00	John Baross: “Parallel habitats on Earth, planets and moons”
	2.30	Peter Ward: “A taxonomy of habitable planets: assessing the odds of life in variable planetary systems”
	3.00	Carol Cleland: “The advent of historical science”
	3.30	*Afternoon Tea*
		Chair: Bruce Balick
	4.00	Robert Smith: “The history of space astronomy: an attempt to see the big picture”*
	4.30	Peter Abrahams: “Telescopes lofted to space”*
	5.00	David DeVorkin: “SAO during the Whipple years: Project Celescope”*
	5.30	Reception
	8.00	Public Lecture *Chair: Woody Sullivan* Frank Drake: “New approaches in the search for SETI”

Thursday 17 June	 9.00 9.30 10.00	*Chair: Ken Kellermann* Wayne Orchiston & Bruce Slee: "The Radiophysics field stations and the early development of radio astronomy"* Marshall Cohen: "Owens Valley Radio Observatory and dark matter"* Wayne Orchiston: "Dr Elizabeth Alexander: first female radio astronomer"*
	10.30	*Morning Tea*
	 11.00 11.30 12.00	*Chair: Virginia Trimble* Geoff Marcy: "Extra-solar planetary systems" Ed Turner: "Detection and characterization of extra-solar planets and plants" Rebecca Cummins: "Light work: contemporary artists consider the Sun"*
	12.30	*Lunch*
	 2.00 2.30 3.00	*Chair: David DeVorkin* Ron Brashear: "The transits of Venus and new astronomies: a time to reflect"* Bruce Hevly: "X-rays, extreme ultraviolet and the life of instruments" Virginia Trimble: "The origin of gamma ray astronomy: when one photon was a discovery, two was a spectrum, and three was the Rossi Prize"*
	3.30	*Afternoon Tea*
	 4.00 4.30 5.00	*Chair: Richard Strom* Jim Evans: "Gnōmonikē Technē: the dialer's art and its meaning in the Ancient World"* Karl Hufbauer: "Radio studies of solar phenomena, 1946-1958: their significance?" Richard Jarrell: "'Radio astronomy … whatever *that* is': the marginalization of early radio astronomy in astronomy"*
	5.30	*End of Session*
	7.00	Conference Banquet
Friday 18 June	 9.00 9.30 10.00	*Chair: Jim Evans* Mott Greene: "Astrobiology and anthropomorphism: are we still looking for ourselves?" Frank Drake: "Lessons from the history of SETI" Dan Werthimer: "Do we know how to search for ET?"

	10.30	*Morning Tea*
		Chair: Wayne Orchiston
	11.00	Bruce Balick: "Discovery of Sgr A*: an exposé"*
	11.30	Miller Goss & Ron Ekers: "The discovery of Sgr A and the almost discovery of Sgr A* in Big Pine – The impact of Galactic Center research since 1951"
	12.00	Woody Sullivan: "Points of view: shadows, planets and life"*
	12.30	*End of Conference*

CONFERENCE PHOTOGRAPH

Some of the Woodfest Conference participants (for identifications see the following page).



Key: 1 = Alastair Gunn, 2 = Carol Cleland, 3 = Steve Dick, 4 = Karl Hufbauer, 5 = Virginia Trimble, 6 = Joe Tenn, 7 = Bruce Balick, 8 = William Baum, 9 = Peter Abrahams, 10 = Wayne Orchiston, 11 = Marshall Cohen, 12 = Richard Strom, 13 = Ken Kellermann, 14 = Ron Brashear, 15 = Woody Sullivan, 16 = Ed Turner 17 = Bruce Hevly, 18 = Dan Tomandl, 19 = Rebecca Cummins, 20 = Richard Jarrell, 21 = Geoff Marcy, 22 = Dan Werthimer, 23 = Miller Goss, 24 = Frank Drake, 25 = David DeVorkin.

Top: Woody discusses the basics of sundial design. *Bottom:* The Mount Rainier excursion.

INTRODUCTION

POINTS OF VIEW: SHADOWS, PHOTONS, PLANETS, AND LIFE

WOODRUFF T. SULLIVAN, III
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Abstract: I combine here a potpourri of topics that have been of keen interest over my career. The first is gnomonics, the art and science of sundials: at its heart are the *shadows* that elegantly chart the motions of the Sun on a defined surface. More broadly, shadows have been amazingly productive in astronomy through the centuries. Examples range from the ancient Greeks (the distances of Moon and Sun) to the Scientific Revolution (Roemer's determination of the speed of light). They also include twentieth century science (solar eclipse observations relative to general relativity) and current twenty-first century research (transits of extrasolar planets).

The ability to observe a shadow depends on one's *point of view*, and I have been fascinated with how our astronomical points of view have been reconstructed over time. For instance, our previous views of the Universe have been continually challenged as we have accessed new types of *photons*, i.e., the opening of the electromagnetic spectrum that began with radio astronomy seventy years ago.

Notions of what a *planet* is and how it works have been revolutionized by our capability to relocate to other planets and to look at planet Earth from the outside, as well as, over the past decade, to chart new worlds in orbit about other stars. This change is part of a long historical sequence in which we have gone from the seven planets of the ancient Greeks, to the six planets of Copernicus, to the nine planets of the mid-twentieth century to the ~150 of today.

Finally, we are in the midst of attaining an entirely new vantage point from which to surveil the phenomenon of *life*. Although still confined to one example, this astrobiologist believes that the signs augur well that life too, will eventually lose its uniqueness, just as has already happened to our Sun, our Earth, and our Galaxy.

Key words: History of astronomy, radio astronomy, shadows, sundials, extrasolar planets, astrobiology

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For age is opportunity no less
Than youth itself, though in another dress,
And as the evening twilight fades away
The sky is filled with stars, invisible by day.

Henry Wadsworth Longfellow (1874)

1. WHY THESE TOPICS?

The meeting from which this volume emerged was a marvelous gift to a fledgling sexagenarian. I was able to invite colleagues and friends who share my interests in history of astronomy, astrobiology, and sundials. But then how to tie together these disparate topics—were there any common intellectual threads? I (mostly) gave up on integrating sundials,[1] but the other two are central to understanding the remarkable new views of the Cosmos that have come to the fore since the middle of the twentieth century (just before I began graduate school in 1966). Are these as a whole enough to define a *New Astronomy*? Kepler wrote about a 'New Astronomy' four hundred years ago, and the rise of astrophysics in the late nineteenth century was termed a 'New Astronomy'. Have we also experienced one?

The opening of the electromagnetic spectrum has been a hallmark of astronomy's past fifty years. Trained as a radio astronomer under Frank Kerr and Gart Westerhout, I began research in the early history of radio astronomy as a sideline as I was obtaining my Ph.D. in 1971. Radio astronomy was of course only the first of many non-optical photon regimes that revealed novel phenomena and objects. Most of these windows only became possible because of the launch of the Space Age (men first walked on the Moon during my graduate school years) and the concomitant capability of sending telescopes and detectors above the Earth's atmosphere.

Simultaneously, the quest for extraterrestrial life entered a new phase with the invention of SETI in 1959-1960 and NASA's support for an exobiology initiative that culminated with the Viking mission to Mars in 1976. I became involved in SETI in 1977, which in turn opened up for me questions of the origin and evolution of life and intelligence. Eventually this all led to another NASA-led field in the mid-1990s, *astrobiology*, which has grown spectacularly, fueled by discoveries of extrasolar planets, a water ocean under the ice of Europa, putative evidence for life in a Martian meteorite, solid evidence for liquid water in the past on Mars, extremophile microorganisms that survive in an amazing range of conditions here on Earth, and new insights into the origin and early history of life on Earth. It is fun to think that the excitement that I experienced in the emerging years of astrobiology must be akin to that felt by the early radio astronomers of half a century ago.

Carl Sagan, a man whom I very much admired, used to say we needed a "cosmic connection", that we should take on a "cosmic view".[2] I try to instill this in students. I believe that astronomers and other physical scientists indeed today have very different cosmic views than they did a half century ago. Steve Dick is right (see his paper in this volume)—a new Biological Universe has been spawned, one in which the phenomenon of life is integrated into the evolution of the Universe from the Big Bang onwards. And this is not only affecting astronomers, but also causing a profound change in many biological scientists; they now look at life as a phenomenon whose characteristics (as studied on Earth) are potentially only a starting point for how life might develop elsewhere. Our *point of view* toward our planet and ourselves has been radically altered. Note that this shift is independent of (and before) any discovery of actual extraterrestrial life. Sagan would have been delighted with how the past decade has played out.

2. SHADOWS IN ASTRONOMY

I have been fascinated with shadows ever since I fell into the world of sundials and gnomonics in 1992.[3] Shadows generally have a negative connotation (dangerous shadows, plotting in shadows, chasing shadows, being in someone's shadow), but I love them. Philosophers debate their ontological status ("Is the absence of light something in itself?"), but they are real. There is no doubt that one's perception and experience of a shadow depends on point of view.

A well-designed sundial can have a profound aesthetic and contemplative effect on viewers, even in this high-tech new millennium. Although stationary (no moving parts!), it records fleeting time in a timeless manner. Its surface is a marvelous projection of the celestial sphere, written by a 'shadow-pen'. Although passive in character, it speaks strongly: of the Sun, of the Cosmos and our place in it, of time and its passing, of history and mortality.

Although sundials have been obsolete as timekeepers for two centuries and of no importance in astronomy for twice as long, shadows more generally have continued to be vital in assembling our present picture of the Universe (see Casati, 2003). An incomplete list of major astronomical episodes in which shadows have been central includes:

- Aristotle: Earth's shadow on the Moon during a lunar eclipse → Earth is a sphere
- Ancient China, Egypt, Mesopotamia, Greece: shadows of vertical rods (gnomons) establish accurate measurement of seasons, latitude, and time (most famous is Eratosthenes' third century BC measurement of the Earth's circumference)
- Aristarchus (third century BC): lunar phases and lunar/solar eclipses → shape and distance of the Moon, distance to the Sun
- Galileo (AD 1610): Venus' phases; height of lunar mountains
- Rømer (1676): eclipses of Jovian satellites → speed of light
- 1761 through 1882: solar transits of Venus[4] → length of the astronomical unit
- total solar eclipses over centuries → secular slowdown of Earth's rotation, geophysics of tides, and chronology/provenance of historical documents
- early twentieth century: eclipsing binary stars → masses and sizes of stars
- 1919 onwards: total solar eclipses → evidence for general relativity
- 1963: lunar occultation of radio source 3C273 → optical identification with highly red-shifted object (the first quasar)
- 1999 onwards: transits of extrasolar planets in front of host stars → sizes and densities of the planets

3. NEW ASTRONOMIES

3.1 New Photons

New kinds of photons have been detected only because of new tools—collectors and detectors, and rockets to lift them above our opaque atmosphere. The interdependence between technology and scientific discovery is an old story. For instance, Galileo (1610) says in the opening words of his *Sidereus Nuncius*: "I propose great things for inspection and contemplation ... great, I say, because of the excellence of the things themselves, because of their newness … and also because of the instrument [his 20× telescope]."

Although the Sun, Moon, and bright stars had been observed in the infrared before World War II, the results were limited and did not lead to wholly new classes of astronomical objects. There was no field of infrared astronomy *per se*. The study of extraterrestrial *radio* waves over the decade 1945-1955, however, led to a revolution in how we viewed the Cosmos. The practitioners were not astronomers, yet they remade astronomy, reporting new photons distributed over the sky in a manner corresponding not at all like optical photons. By the late 1940s they called themselves radio astronomers and said they were doing *radio astronomy* (which by the way soon necessitated the new term *optical astronomy*). This was indeed a New Astronomy, but succeeding decades showed that it was just the first of many to follow as rockets and satellites opened up unexplored ranges of photon energy: X-ray,[5] infrared, ultraviolet, and γ-rays.

These New Astronomies have profoundly changed our cosmic outlook. Because we have explored the limits of photons, we may think that no further New Astronomies remain. But this is short-sighted, for why should new 'windows' be confined to the electromagnetic spectrum? I imagine that over the *next* fifty years we may well see a comparable revolution as we sensitively map the sky in neutrinos, gravitational waves, dark matter, dark energy, 'quintessence' (how Aristotelian!), etc.

3.2 New Planets

A seminal event in the emerging field of astrobiology was the first detection of a 'normal' planet orbiting a 'normal' star (Mayor and Queloz, 1995). Most astronomers certainly already believed at that time that planetary systems occurred around other Main Sequence stars, but there is nothing like the *reality* of an actual detection to solidify one's thinking, in this case, that we may not be unique.

Similarly, in the remarkable, small volume *The Nature of the Universe* Fred Hoyle (1950) argued that the reality of the first photograph of the Earth from outside would transform how we think of ourselves—people would gain a visceral appreciation, not just intellectual knowledge, that the Earth is a planet. And he was proved right when in 1968 humans (Apollo 8 astronauts) for the first time saw the entire Earth as a small ball, and indeed could cover it with a thumb at arm's length. Spaceship Earth and the iconic image of a blue/white/brown planet became a reality. And this was not just abstruse knowledge—it also had *practical* consequences: the Full Earth image importantly contributed to the young environmental movement (e.g., the first Earth Day was in 1970).

Stepping back farther, we gain a better point of view on the magnitude of today's dawning era. The ancients had *seven* wandering planets (Moon, Sun, Mercury, Venus, Mars, Jupiter, and Saturn). Copernicus changed the number to *six*, tossing out the Sun and Moon, but adding in the Earth. Over the next four hundred years the number of planets slowly grew to *nine*, although only with the Apollo missions did we for the first time truly appreciate the Earth as the ninth. Now we have about *one hundred and fifty* extrasolar planets, exhibiting a remarkable range of properties. Future prospects seem limitless.

3.3 New Life

Astrobiology is sometimes derided as a science without a subject to study. But this misses the point. Completely independently of actually finding extraterrestrial life, astrobiologists are looking in entirely new ways at terrestrial life, life as we know it. Motivated by wanting to know where and how to look for *extraterrestrial* life, we gain novel insights into the origin and evolution of *Earth's* life, the extremes under which that life can thrive, and the biochemistry of that life.

Whether focused on microorganisms or intelligent life, astrobiologists are not willing to accept the assertion that life has happened only once and in only one place. Since we do not know how rare 'Good Planets' are, we take the empirical approach: it's a fundamental question, we have a powerful set of tools to attack the problem, today's best evidence points to exciting possibilities, and so let's look! Astronomers who used to have little interest in biology and biologists who had been even less concerned with the Cosmos, are now talking to each other in ways that were rare a decade ago. And the same goes for all the other specialties that contribute to astrobiology: geology and geophysics, paleontology, evolutionary biology, biochemistry, atmospheric sciences, genomics, oceanography, etc. Ever since its founding in 1998, the University of Washington graduate program in Astrobiology has endeavoured to foster these conversations by breaching disciplinary walls to overcome academia's typical balkanization of knowledge. This in itself I find tremendously exciting and stimulating—for me it's like being a graduate student all over again!

Of course I do not know whether evidence of extant life or fossil life will be discovered soon … or ever. But I am convinced that it is unlikely and that the pursuit of such a discovery is fundamentally valuable.

4. IS THIS REALLY A SPECIAL TIME?

There is a tendency for every generation (at least over the past century or two of rapid change) to look upon its own time as unprecedented in its accomplishments and challenges. And yet a long historical view does reveal certain epochs when the term 'New Astronomy' is apt, and sometimes is even used by the participants themselves. Ron Brashear discusses several of these in his paper in this volume, beginning with Kepler's fundamentally new *physical* Universe, as enunciated for example in his 1609 book *Astronomia Nova*. Much later we find a counterexample when in 1761 C.F. Cassini (III) exclaimed about his era: "Happy our century! to which is reserved the glory of witnessing the one event which will render it noteworthy forever in the annals of the sciences!" (cited in Sheehan and Westfall, 2004: 141). Cassini was excited about prospects that the upcoming transits of Venus would accurately determine the scale of the Solar System. But today we consider his assessment exaggerated, just as future historians may disagree with my feeling that the past fifty years have fundamentally altered astronomy.

Continuing onwards, John Herschel in 1852 wrote to Faraday regarding the newly discovered correlation between solar activity and changes in the Earth's magnetic field: "If all this be not premature we stand on the verge of a vast cosmical discovery such as nothing hitherto imagined can compare with" (Meadows, 1984: 8). This coincided with the start of an era when another New Astronomy, fueled by photography and spectroscopy and eventually called *astrophysics*, overtook visual and positional astronomy.

Trying to be objective, I nonetheless conclude that my generation has experienced a special epoch that ranks well with previous ones. A nexus of politics and technology made the post-World War II decades the trigger—Otto Struve (1960) talked about "astronomers in turmoil". And the era was defined not just by a single area, but the confluence of several: the ability to do science and exploration in space and on other planets, the opening of the electromagnetic spectrum, new cosmological understanding of the Universe (not discussed in this paper), and finally, and perhaps most profoundly, the incorporation of life into the central arguments of the previously 'dead' world of astronomy and space science.

5. A FINAL REFLECTION

Despite my involvement in SETI for over twenty-five years, I think it highly *un*likely that we'll detect an ETI signal in my lifetime. But we have the tools and should do the searches—one has to start somewhere. The real value of the SETI enterprise to me, regardless of any direct success, is that contemplation of *Them* ineluctably leads to thinking about *Us* in wholly different ways. This is equally true for astrobiology, although the discovery of microbial extraterrestrial life would likely have less impact on society as a whole. Astrobiology makes us see ourselves as a particular carbon/water/ DNA-based form of life that has given rise to one technological species and a vast array of other species on a small, rocky, ocean-covered, thinly-veiled planet near a middle-mass star. Politically speaking, I think that this enhanced appreciation of commonality can only be helpful as we struggle towards a world society at peace (see Chris Chyba's paper in this volume).

This reflexive mode has been nicely expressed by Adrienne Rich (1975):

Is any light so proudly thrust
From darkness on our lifted faces
A sign of something we can trust,
Or is it that in starry places
We see the things we long to see
In fiery iconography?

In a similar vein, at the meeting upon which this volume is based, Mott Greene made arguments at once compelling and provocative that modern SETI and astrobiology are engaged in a scientific search for a twenty-first century version of God, with characteristics largely a reflection of our own. He aptly ended with Edward Young's famous line: "An undevout astronomer is mad" (from *Night Thoughts*, 1741). I agree that astrobiology is both a scientific and a spiritual quest.

Notwithstanding today's astonishing scientific progress, we must also not forget how little we know. I close by returning to shadows and recalling Plato's allegory of the cave in *Republic.* The denizens of the cave could study the shadows on the cave wall and come to many logical, empirical conclusions, but the 'true' phenomena forever remained beyond their ken. I sometimes think that we too are at such a primitive stage in our understanding of the Universe (not to mention how humans behave). Hence the motto I chose for the University of Washington sundial:

What you seek is but a shadow.

6. SPECIAL THANKS

I too thank all of the people that Wayne Orchiston mentions in the Foreword, especially the SOC and LOC and of course, Wayne himself, who put in a tremendous amount of skillful work for the meeting itself, as well as in editing this volume. I will never forget Woodfest, for which my colleagues and friends travelled from afar and gave of their valuable time to honor me with a first-rate intellectual feast. There was also a great *physical* experience for about ten of us who hiked up to the Carbon Glacier at Mount Rainier the day after the meeting.

Thanks also to Linda Khandro, Vickie Graybeal and Pat Taylor for logistical help, and to Ester Baum and Linda for music at the banquet and reception. Finally, my wife Barbara helped out with many arrangements for the social side of the meeting, only the latest instance of forty years of love and support in my life

7. NOTES

1. There is a saying, however, that deals with local solar time and is relevant to the unusual mixture of topics. The French say: *Il voit midi à sa porte*, which literally means "he sees noon from his door", implying that each observer can define his own local noon when the Sun transits his own particular longitude. The meaning of the aphorism is that "he has his own vision, his own unconventional ways", which may apply to the author.
2. Benjamin (2003: 174-203) has a nice discussion of SETI, Sagan (and others), and related religious ideas.
3. My gnomonics phase began with the design of a large wall sundial on the University of Washington Physics/Astronomy Building. Further details can be found in Rebecca Cummins' paper in this volume, and on the following web site: www.astro.washington.edu/woody/sundial_tour.html
4. The next transit of Venus occurred 122 years later, on 8 June 2004, just one week before the meeting upon which this volume is based; I was thrilled to see it from Marseille, France. In another coincidence, the first day of the meeting was the centennial of Bloomsday, the day in Joyce's *Ulysses* of Leopold Bloom's peregrinations around Dublin.
5. *Solar* X-ray astronomy began just after World War II, but the field did not expand until Galactic sources were first detected in the early 1960s. Ultraviolet astronomy had a similar story.

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ASTROBIOLOGY

THE BIOLOGICAL UNIVERSE REVISITED

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Abstract: Cosmic Evolution has been seen as leading to two possible world views: a physical universe in which life is rare or unique to Earth, and a biological universe, in which the processes of cosmic evolution commonly end in life. These two worldviews now hang in the balance, in the same way that the heliocentric and geocentric worldviews were in the balance four hundred years ago when Galileo wrote his *Dialogue on the Two Chief World Systems* (1632). Astrobiology is the science that will decide which of the two modern astronomical worldviews is true. A third world view, the postbiological universe, is also possible and deserves more discussion. The confirmation of one of these worldviews will have profound implications for human destiny.

Key words: artificial intelligence, biological universe, cosmic evolution, cultural evolution, Drake Equation, postbiological universe, Shapley

1. INTRODUCTION

Almost ten years ago I documented the twentieth century history of what I described as a major cosmological worldview, the biological universe—the idea that the universe is full of life (Dick, 1996). In this paper I want to revisit that claim, and suggest there is another possibility beyond the biological universe. To put it another way, I want to claim that cosmic evolution harbors at least three vastly different possibilities for the universe. The ultimate product of cosmic evolution may be only planets, stars and galaxies—a *physical universe* in which we are unique or extremely rare. By contrast cosmic evolution, through biological evolution, may commonly result in life, mind and intelligence, an outcome that I term the *biological*

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universe. Finally, there is another possibility not often discussed, but that I wish to argue needs to be taken seriously. Taking a long-term view, cultural evolution on other planets may have already produced artificial intelligence, constituting a *postbiological universe*.

Seen within this framework, these possible outcomes are not just speculation—they are the result of taking cosmic evolution seriously in all three components of the Drake Equation: astronomical, biological and cultural. Just as the outcome of astronomical evolution was once speculative, and just as the outcome of biological evolution in the universe still is speculative, so the outcome of cultural evolution is also speculative—even on Earth. But given the existence of extraterrestrial intelligence, the fact of cultural evolution beyond Earth is not speculative, and must be taken into account.

2. COSMIC EVOLUTION: THREE POSSIBLE OUTCOMES

Because cosmic evolution is the basis of my claims, I want to elaborate briefly how we came to the idea of cosmic evolution. A century ago, it is safe to say, cosmic evolution was not the accepted worldview. The worldview of that time was graphically captured in A.R. Wallace's volume on the plurality of worlds (Wallace, 1903). Wallace, co-founder with Darwin of the theory of natural selection, argued that the universe was only 3,600 light years in extent, a common view at the time. He noted how all the conditions for life on Earth had to be just right—the same kind of arguments recently used to claim that Earth-like planets are rare (Ward and Brownlee, 2000). Wallace concluded that humans were alone in the universe, that the Solar System was nearly at its center, and that humans were its ultimate purpose. Despite all the contributions he made to the study of biological evolution on Earth, Wallace believed in the physical universe, not a biological universe. And it was devoid of cosmic evolution in any significant sense.

A century later our view of the universe has immensely enlarged. We now know that the universe is about 13.7 billion light years in extent, and full of galaxies, as graphically captured by the Hubble Space Telescope. And size has not been the most important change in our ideas about the universe over the last century. We now detect not only the expanding universe, but also the accelerating universe; we also know about space-time, inflationary cosmology, and dark energy. But arguably no concept has been so radical as cosmic evolution, which encompasses all these new concepts, and in fact embraces all we know about the universe. Despite his belief in biological evolution, Wallace's universe was static. Ours today is evolving, and cosmic evolution is the guiding principle for all of astronomy. Thus, in contrast to a century ago, we can speculate on the destiny of life—on Earth and in the universe—based on what we now know about cosmic evolution.

The intellectual basis for this guiding principle of cosmic evolution had its roots in the nineteenth century when a combination of Laplace's nebular hypothesis and Darwinian evolution gave rise to the first tentative expressions of parts of this worldview (Dick and Strick, 2004). But cosmic biological evolution first had the potential to become a research program in the 1950s and 1960s, when its cognitive elements had developed enough to become experimental and observational sciences, and when the researchers in these disciplines first realized they held the key to a larger problem that could not be resolved by any one part, but only by all of them working together. Harvard College Observatory Director, Harlow Shapley, was an early modern proponent of this concept, which he spoke of in the 1950s in now familiar terms. The Earth and its life, he asserted, are "… on the outer fringe of one galaxy in a universe of millions of galaxies. Man becomes peripheral among the billions of stars in his own Milky Way; and according to the revelations of paleontology and geochemistry he is also exposed as a recent, and perhaps an ephemeral manifestation in the unrolling of cosmic time" (Shapley, 1958). Shapley (ibid.) went on to elaborate his belief in billions of planetary systems, where "… life will emerge, persist and evolve." Shapley's belief in life was unproven then, and remains to be proven today. The transition from belief to proof is tantamount to discovering whether cosmic evolution commonly ends with planets, stars and galaxies, or with life, mind and intelligence. Put another way, does cosmic evolution produce not only a physical universe, but also a 'biological universe'? In recent years, Joann Palmieri has even found that Shapley in his correspondence used the term 'biological universe', unbeknownst to me when I titled my book *The Biological Universe* (see Palmieri, 2001).

Already as the Space Age began, then, the concept of cosmic evolution—the connected evolution of planets, stars, galaxies *and* life—provided the grand context within which the enterprise of exobiology was undertaken. The idea of cosmic evolution spread rapidly over the next forty years, both as a guiding principle within the scientific community and as an image familiar to the general public (Chaisson, 1981; Reeves, 1981; Sagan, 1980). NASA enthusiastically embraced, elaborated and spread the concept of cosmic evolution from the Big Bang to intelligence as part of its SETI and exobiology programs in the 1970s and 1980s (Figure 1). And when in 1997 NASA published its Origins program Roadmap, it described the goal of the program as "… following the 15 billion year long chain of events from the birth of the universe at the Big Bang, through the formation of chemical elements, galaxies, stars, and planets, through the mixing of chemicals and energy that cradles life on Earth, to the earliest self-replicating organisms—and the profusion of life." (NASA, 1997). With this proclamation of a new Origins program, cosmic evolution became the organizing principle for most of NASA's space science effort, and the concept continues to be elaborated today in ever more subtle form (Chaisson, 2001; Delsemme, 1998).

Figure 1. Cosmic evolution is depicted in this image from the exobiology program at NASA Ames. Upper left: the formation of the stars, the production of heavy elements, and the formation of planetary systems, including our own. At left: prebiotic molecules, RNA and DNA are formed within the first billion years on the primitive Earth. At center: the origin and evolution of life leads to increasing complexity, culminating in intelligence, technology, and astronomers. Upper right: contemplating the Universe. The image was created by David DesMarias, Thomas Scattergood and Linda Jahnke at NASA Ames in 1986, and reissued in 1997.

Today, the Big Question remains: "How far does cosmic evolution commonly go?" Does it end with the evolution of matter, the evolution of life, or the evolution of intelligence? In this sense two astronomical world views hang in the balance in modern astronomy, just as they did four centuries ago when Galileo wrote his *Dialogue on the Two Chief World Systems* (Dick, 2000). The two chief world systems in 1600, of course, were the geocentric and the heliocentric. The two chief world systems today are the physical universe and the biological universe. But even NASA's early SETI discussions hinted at a third world view opened up by cosmic evolution—the postbiological universe based on cultural evolution. That is a worldview that deserves a great deal more attention than it has heretofore received.

3. THE PHYSICAL UNIVERSE

I will say only a few words about the first possible outcome of cosmic evolution—the physical universe—because almost all of the history of astronomy, from Stonehenge through much of the twentieth century, deals with the people, the concepts, and the techniques that gave rise to our knowledge of the physical universe. Babylonian and Greek models of planetary motion, medieval commentaries on Aristotle and Plato, the astonishing advances of Galileo, Kepler, Newton and their comrades in the Scientific Revolution, thermodynamics, the physics of stellar energy and stellar evolution, the elegant results of modern astronomy—all these and more address the physical universe. The physical universe has been the subject of astronomy for millennia, and it now boasts a whole bestiary of objects unknown a century ago—blazers and quasars, pulsars and black holes, and more familiar objects like planetary nebulae, which have now been beautifully rendered in detail undreamed of before, thanks to space telescopes such as Hubble and Chandra. The quest for a biological universe should in no way obscure the fact that the physical universe—the domain of the entire field of astronomy and astrophysics—is in itself truly amazing. Thousands of astronomers worldwide are working on understanding its dynamics, structure and composition. The choice of worldviews I have given certainly does not deny there is a physical universe—the distinction comes when one considers the endpoints of cosmic evolution.

4. THE BIOLOGICAL UNIVERSE

The second possible outcome of cosmic evolution is the biological universe—the universe in which cosmic evolution commonly ends in life. Ideas about a possible biological universe date back to ancient Greece, in a history that is now well known (Crowe, 1986; Dick, 1982, 1996, 1998; Guthke, 1990). The Copernican revolution, which made the Earth a planet and the planets potential Earths, provided the theoretical under-pinnings for the concept of extraterrestrial life.

Unlike the physical universe, we have addressed in a substantive empirical way this new worldview of the biological universe only over the last four decades. Despite false starts like Lowell's canals of Mars, only in the 1950s and 1960s did four intellectual elements—planetary science, the search for planetary systems, origin of life studies, and the Search for Extraterrestrial Intelligence (SETI)—converge to give birth to the field of exobiology. At first quite separate in terms of researchers, techniques and common goals, these fields over four decades gradually became integrated. Early signs of the potential marriage of astronomy and biology occurred in the 1950s, for example, with what was billed 'the first American symposium on astrobiology' in 1957 (Wilson, 1958).

More significant was what was thought to be the first empirical evidence of extraterrestrial life, in particular William Sinton's claim of spectroscopic evidence of life on Mars (Dick 1996: 122). Although it was soon disproved, this finding played an important role as the Space Age began. The beginning of the Space Age offered the means to actually go to Mars, and thus NASA became an important patron of the new science of 'exobiology'. Also in the early 1960s, Peter van de Kamp's claim of a planet around Barnard's star raised great excitement—we now know it was about three decades premature. Meanwhile, with the ideas of A.I. Oparin and J.B.S. Haldane in the background, origin of life studies took a giant leap forward with the Urey-Miller experiment in 1953. Already in 1959 Urey and Miller saw the relevance of space to their work, arguing that the discovery of life beyond Earth was a test-bed for theories of the origin of life. The following year Frank Drake undertook project Ozma, and began SETI, another thread in the new discipline of exobiology. So, by the mid-1960, practitioners began to declare the beginnings of a new discipline (Dick, 1996).

The Viking landers were the highlight of NASA's early foray into exobiology. The negative result for life on Mars caused a period of decline in the field in terms of the in situ search for life beyond Earth, even as NASA's exobiology program supported path-breaking work in life in extreme environments, the Earth's primitive atmosphere, Lovelock's Gaia hypothesis, Carl Woese's three domains of life, among other areas (Dick and Strick, 2004). By the 1990s many events conspired to revitalize exobiology's search for life in the solar system: the Mars rock ALH84001, the Mars Global Surveyor observations of the gullies of Mars and the Mars Odyssey detection of water near the surface, and the Galileo observations of Europa indicating a possible ocean. The discoveries of circumstellar matter, extrasolar planets, life in extreme environments such as deep sea hydrothermal vents, and increasingly complex interstellar organics fueled the possibilities of life beyond the solar system. All these elements fed into NASA's new Astrobiology Program, which emerged from a deep organizational restructuring at NASA in 1995 (ibid.). 'Astrobiology' involved much more than renaming a discipline; it was much more broadly defined than exobiology, and was to include research in cosmo-chemistry, chemical evolution, the origin and evolution of life, planetary biology and chemistry, formation of stars and planets, and expansion of terrestrial life into space. Astrobiology today is a much more robust science than exobiology was 40 years ago. Despite all the activity, the circumstantial evidence that the universe may be 'biofriendly', and the recent Mars Exploration Rovers' discovery of likely past standing water on Mars, the biological universe remains to be proven (Darling, 2001; Goldsmith and Owen, 2001; Jakosky, 1998, Koerner and LeVay, 2000).

5. THE POSTBIOLOGICAL UNIVERSE

Although the biological universe remains unproven, the two chief world views today are the physical universe and the biological universe, with many believing it is only a matter of time until proof comes for the latter. I have only skimmed the surface of a subject that has been documented in detail. We now come to the third option, distinct from the physical and the biological universe, an option that thus far has not been taken seriously. But if we take seriously physical and biological cosmic evolution, we also need to take seriously cultural evolution as an integral part of cosmic evolution and the Drake Equation. Those familiar with the vast sweep of time in Olaf Stapledon's *Last and First Men* (1930) and *Star Maker* (1937) will know what I mean when I say that we need to think in Stapledonian terms. While astronomers are accustomed to thinking on cosmic time scales for physical processes, even they do not commonly think on cosmic time scales for biology and culture. But cultural evolution now completely dominates biological evolution on Earth. Given the age of universe, and if intelligence is common, it may have evolved far beyond us. I have recently argued in the *International Journal of Astrobiology* and elsewhere (Dick, 2003a, 2003b) that cultural evolution over thousands or millions of years will likely result in a postbiological universe populated by artificial intelligence, with sweeping implications for SETI strategies and for our world view

Let me just give you the outlines of this idea. MacGowan and Ordway (1966), Davies (1995) and Shostak (1998), among others, have broached the subject, but it has not been given the attention it is due, nor has it been carried to its logical conclusion. The two methodological principles are those I have already mentioned: that long-term Stapledonian thinking is a necessity if we are to understand the nature of intelligence in the universe today, and that cultural evolution must be seen as an integral part of cosmic evolution and the Drake Equation. The three scientific premises are: 1) that the maximum age of ETI is several billion years; 2) the lifetime of a technological civilization is greater than 100 years and probably much larger; and 3) in the long term, cultural evolution will supersede biological evolution and produced something far beyond biological intelligence. Let us look at each of these premises in turn.

It is widely agreed that the maximum age of extraterrestrial intelligence, if it exists, is billions of years. Recent results from the Wilkinson Microwave Anisotropy Probe (WMAP) place the age of the universe at 13.7 billion years, with a 1% uncertainty, and confirm that the first stars formed at about 200 million years after the Big Bang. The oldest Sun-like stars probably formed within about a billion years, or 12.5 billion years ago. By that time enough heavy element generation and interstellar seeding had taken place for the first rocky planets to form. Then, if Earth's history is any guide, it may have taken another 5 billion years for intelligence to evolve. In a universe 13.7 billion years old, this means that the first intelligence could have evolved 7.5 billion years ago. Norris (2000), Livio (1999), and Kardashev (1997) have all argued that extraterrestrial civilizations could be billions of years old, and this assumption is commonly accepted among SETI practitioners.

But what about the second premise, that the lifetime of a technological civilization (denoted as 'L' in the Drake Equation, and defined as starting when a civilization becomes radio communicative), could be billions of years? It is true that the only data point we have is ourselves. Sagan, Drake and others generally assigned L values in the neighborhood of a million years, and even some pessimists admit 10,000 years is not unlikely. Of course there are a variety of natural and societal catastrophes that could prevent civilizations from reaching ages of millions or billions of years. But the key point is the age of extraterrestrial intelligence does not have to be large for cultural evolution to do its work. Even at our low current value of L on Earth, biological evolution by natural selection is already being overtaken by cultural evolution, which is proceeding at a vastly faster pace than biological evolution (Dennett, 1996). Technological civilizations do not remain static; even the most conservative technological civilizations on Earth have not done so, and could not given the dynamics of technology and society. Unlike biological evolution, L need only be thousands of years for cultural evolution to have drastic effects on civilization.

But how can we possibly predict the course of cultural evolution? We certainly cannot *predict* anything, least of all cultural evolution on Earth, much less in the universe. Darwinian models of cultural evolution have been the subject of much recent study (Lalande and Brown, 2000), but they are fraught with problems and controversy—we need only think of the controversies generated by sociobiology, behavioral ecology, evolutionary psychology, gene-culture co-evolution and memetics.

While theoretical and empirical studies of cultural evolution hold hope for a science of cultural evolution, lacking a robust theory of cultural evolution to at least guide our way, we are reduced at present to the extrapolation of current trends supplemented by only the most general evolutionary concepts. Several fields are most relevant, including genetic engineering, biotechnology, nanotechnology, and space travel. But one field—artificial intelligence (henceforth 'AI')—may dominate all other developments in the sense that other fields can be seen as subservient to intelligence. Biotechnology is a step on the road to AI, nanotechnology will help construct efficient AI and fulfill its goals, and space travel will spread AI. Genetic engineering may eventually provide another pathway toward increased intelligence, but it is limited by the structure of the human brain. In sorting priorities, I adopt what I term the central principle of cultural evolution, which I refer to as the 'Intelligence Principle' and define as: "The maintenance, improvement and perpetuation of knowledge and intelligence is the central driving force of cultural evolution, and that to the extent intelligence can be improved, it will be improved." The Intelligence Principle implies that, given the opportunity to increase intelligence (and thereby knowledge), whether through biotechnology, genetic engineering or AI, any society would do so, or fail to do so at its own peril. I have elsewhere attempted to justify this principle (Dick, 2003a), but what it comes down to is this: culture may have many driving forces, but none can be so fundamental, or so strong, as intelligence itself.

The field of AI is a striking example of the Intelligence Principle of cultural evolution. Although there is much controversy over whether artificial intelligence can be constructed that is equivalent or superior to human intelligence —the so-called Strong AI argument —several AI experts have come to the conclusion that AI will eventually supersede human intelligence on Earth. Moravec (1988) spoke of "… a world in which the human race has been swept away by the tide of cultural change, usurped by its own artificial progeny." Kurzweil (1999) also sees the takeover of biological intelligence by AI, not by hostility, but by willing humans who have their brains scanned, uploaded to a computer, and live their lives as software running on machines. Tipler (1994), well known for his work on the anthropic principle and the Fermi paradox, concluded that machines may not take over, but will at least enhance our well-being. But the self-reproducing von Neumann machines that Tipler foresaw in his explanation of the Fermi paradox may well exist if his view of the Fermi paradox is wrong.

It may be that Moravec, Kurzweil and their proponents underestimate the moral and ethical brakes on technological inertia. But such objections fail to take into account cultural evolution, and may lose their impact over the longer term, as the Intelligence Principle asserts itself. When one considers the accelerating pace of cultural evolution as we enter the third millennium of our era, radical change of the sort foreseen by Moravec and Kurzweil does not seem so far-fetched.

Thus, it is possible that L need not be billions or millions of years for a postbiological universe scenario. It is possible that such a universe would exist if L exceeds a few hundred or a few thousand years, where L is defined as the lifetime of a technological civilization that has entered the electronic computer age (which on Earth approximately coincides with the usual definition of L as a radio communicative civilization).

The postbiological universe cannot mean a universe totally devoid of biological intelligence, since we are an obvious counterexample. Nor does it mean a universe devoid of lower life forms, as advocated by Ward and Brownlee (2000). Rather, the postbiological universe is one in which the majority of intelligent life has evolved beyond flesh and blood. The argument makes no more, and no fewer, assumptions about the probability of the evolution of intelligence or its abundance than standard SETI scenarios; it argues only that if such intelligence does arise, cultural evolution must be taken into account, and that this may result in a postbiological universe.

Although some may consider this a bold argument, its biggest flaw is probably that it is not bold enough. It is a product of our current ideas of AI, which in themselves may be parochial. It is possible after a few million years, cultural evolution may result in something even beyond AI.

6. SUMMARY

The new universe, driven by the astronomical, biological and cultural components of cosmic evolution, may result in any of the three outcomes described here: the physical universe, the biological universe, or the postbiological universe. Which of the three the universe has produced in reality we do not yet know. But we can say that these three possible outcomes of cosmic evolution have very different consequences for human destiny. If life is to be played out in this physical universe, the destiny of life is for humans, or their robotic ancestors, to populate the universe. In such a universe, where we are unique or very rare, stewardship of our rare pale blue dot takes on special significance. The destiny of human life in a biological universe is quite different from that in a physical universe. Rather than populating a universe empty of life, the destiny of humanity is perhaps to interact with extraterrestrials, to join what has been called a 'galactic club' whose goal is to enhance knowledge. The destiny of human life in a postbiological universe is as a fleeting stage in cosmic evolution prior to being lifted to a higher nonbiological intelligence. If other civilizations in the universe are already the postbiological outcome of cultural evolution, it is possible that we will find in them the future destiny of life on Earth. Whether or not postbiological humans remain human I leave as an exercise for the reader.

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CONTINGENCY AND THE COSMIC PERSPECTIVE

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Abstract: There are limits beyond which the Copernican Principle (the idea that there is nothing special about our place in the Universe) cannot be extended. Determining where these limits lie depends in part on the tension between contingency and convergence. Those who favor contingency dwell on how many things must not have gone "wrong" in order for us to be here. Those favoring convergence argue that there are often many parallel paths to similar functional outcomes, even if any given path is highly contingent. Regardless of how both points of view ultimately come together into a complete account, our particular civilization is unique, whether there are many others in the Galaxy or it is entirely alone. It would be good for universities to restructure themselves in ways that foster the interdisciplinary work needed for that civilization to meet the challenges it faces this century.

Key words: astrobiology, extra-solar planets, evolution of life, SETI, the Drake Equation, rare Earth

1. INTRODUCTION

When the Scientific Organizing Committee was planning this Conference in honor of Woody Sullivan, they asked me to speak on "Astrobiology and the cosmic perspective." I will begin by saying a little about what that perspective might be, then emphasize a tension within this perspective, namely that between *contingency* and *convergence*—a tension that arises with respect to both biological and planetary evolution. I will

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close by stepping even farther back and addressing another issue that I believe is also close to Woody's and my own concerns in this area.

2. THE COSMIC PERSPECTIVE

Let me begin with a tip of my hat to Woody's career as an historian of science as well as a scientist, and begin this conversation about the cosmic perspective with a discussion of Aristotelianism. As Steve Dick (2005) has already hinted in his paper, there was more than one worldview at the time of the ancient Greeks. We can not delve into that now in detail, but let me recall what became a dominant worldview, Aristotle's physics and cosmology, and in particular, the radical dichotomy that Aristotle (in his *De Caelo*) proposed between the terrestrial realm and the heavenly realm.

Aristotle observed that here on Earth objects move in straight lines—they move away or toward the center of the Universe—whereas in the heavens objects are in eternal circular motion. Therefore, the laws of motion that apply in the terrestrial realm (and to the four elements of which it is made) are clearly different from the laws that apply in the heavenly realm. And that means, of necessity, that the stuff of the heavens—the fifth element, the quintessential element—is fundamentally different from the stuff down here. So the heavens are both made of different stuff and they obey different laws.

Had *that* cosmic perspective turned out to be correct, astrobiology as a discipline simply would not exist. The question of life elsewhere in Aristotle's cosmology would not arise, the suggestion would be incoherent. And in that sense the overthrow of Aristotelianism made the Universe safe for astrobiology. What is important here is not so much whether the Earth is going around the Sun or the Sun going around the Earth, but rather the idea that the same laws apply here on Earth as in the Cosmos. It was not until Newton that this idea was secure. The same Newtonian law of gravity in the *Principia* explains the acceleration of my pen if I drop it, or the parabolic motion of a cannon ball; and the same law also explains the elliptical motion of the planets (Newton, 1686). So there are not separate laws that apply in the two realms, and at least the path is open for the material of the heavens and the Earth to be the same.

The development of spectroscopy and its placement on a firm basis by quantum mechanics allowed Payne-Gaposchkin (Payne, 1925) in her Ph.D. thesis to demonstrate the chemical homogeneity of the stars and their similarity to Earth (apart from large differences in hydrogen and helium abundance). Her work demonstrated that the heavens not only obey the same laws (in this case, quantum-mechanical laws) as on Earth, but they are also made of the same material. In doing so, she largely completed the overthrow of Aristotelianism begun by Copernicus in 1543.

I realize that there is a bit of irony in this observation now, since it is appearing that the vast majority of the Universe is made of something—dark matter and dark energy—that is in fact radically different from what we encounter down here on Earth, and we do not know what it is! So there is a kind of whiff of Aristotelianism that is creeping back into what we know about the Universe. But nevertheless the point remains: at least with respect to what we see, it appears that the Universe obeys the same laws and is made of the same stuff as here on Earth, so that astrobiology is plausible.

Let me make one other comment, a more narrow one, about how the cosmic perspective or the cosmology to which we adhere shapes astrobiology, the way we think about it, and what falls into its domain. And that is the victory this century of big bang cosmology over the steady state cosmology. It might have turned out that steady state cosmology was correct. In the middle of this century, given what we knew, that could have been the case (Bondi and Gold, 1948; Hoyle, 1948)—just as two and a half millennia ago it could have been the case, based on what we knew then, that Aristotelian cosmology was correct. Had steady-state cosmology turned out to be correct, then we would be under no obligation to explain the origin of life. If the Universe were infinitely old, then life could be infinitely old. We would have to explain how it arrived at new solar systems, so we might make an appeal to something like microbial transfer between solar systems ('panspermia') if we wanted to avoid origins, but there would be no intellectual obligation to explain an origin of life. However, we *do not* live in that Universe. It turns out that we live in a Universe of finite age, so in fact we are obliged intellectually to understand the origin of life. And that is a pressing sub-discipline of astrobiology, made necessary by the triumph of a particular Einsteinian cosmology over another cosmological theory.

This repeated triumph of what is called Copernicanism has led to what is sometimes called the Copernican Principle—the idea that there is nothing special about us: nothing special about our Solar System; about our planet; perhaps about our biology; and perhaps even about our civilization. That is, I believe, what the idea of Steven Dick's *Biological Universe* (1996) is meant to capture—a certain cosmic perspective. Historically, for the last several centuries, those who have placed their bets on our having a privileged position, those who would bet against the Copernican Principle, have lost the bet.

Yet there is obviously a point beyond which the Copernican Principle cannot be safely extended. That point has to arise. In one sense it arises because in the end we can only make these observations and investigate these principles because in fact we exist. That is to say, suppose planets like the Earth were in fact extremely, extremely rare, so rare that there was only one per galaxy. There is nothing that we yet know that excludes this possibility. Then there might be only one planet in each galaxy on which a civilization could arise. So that would be the only planet on which the question of whether civilizations are common or rare could ever be asked, and until one does the observations to find out, there would be no way to know the answer to that question. That is, there is really no requirement that Earths are typical. These sort of musings are sometimes referred to by the problematic label of the "Anthropic Principle" (Carter, 1974; Bostrom, 2002) but let us not let a name get in the way of drawing what is an important conclusion, that our current observations inevitably suffer from a deep selection effect.

Are Earths common? In just a few years' time we should know the answer to that question, unless there is a major failure of the Kepler Mission, or it is delayed for budget reasons. I think the Kepler Mission is one of the most exciting missions in the history of space science. It is poised to answer a question that human beings have been asking literally for millennia. Aristotle asked whether other Earths exist. He knew the answer—it was almost *a priori* obvious in his cosmology that the answer was 'No'. Newton returns to the question in the *Principia*. But *we* are actually going to *know* the answer to that question. We are going to know it statistically, and we are also going to know the distances of any other Earths from their stars, and thus whether those worlds could be in habitable zones. Kepler is a remarkable mission. But even if it turns out that Earths are common—and it may not, we do not know, the point is we are going to find out soon—we will not know whether the origin of life is common, or whether the origin of multicellular life is common, or for that matter whether the origin of technical intelligence is common.

I call the "Copernican chain," this intellectual progression from the recognition that our Galaxy is not unusual, and that our Sun is not unusual, to the claim that our planet is not unusual, that the origin of life is not unusual, that the evolution of advanced life is not unusual, that the evolution of technical civilization is not unusual, and so on. At some point along the way this progression must no longer hold: the Copernican chain must come to an end. You reach a certain link in the chain and you clearly can no longer safely generalize from your own supposed typicality. The biologist George Gaylord Simpson (1964) suggested one possible break point in the chain four decades ago in his piece in *Science*, "The non-prevalence of humanoids." At a minimum, humanoid creatures seem unlikely to arise elsewhere. So at a minimum, even if technical intelligence were common in the Galaxy, we would not expect a species that originally underlay one of those civilizations to look like us. And in that sense our biosphere, and our civilization, remains precious and unique, regardless of whether other civilizations are common or not.

3. CONTINGENCY VS CONVERGENCE

But Simpson's point that other intelligent beings would not look like us does not answer the question of whether they would exist at all. The break point could come earlier in the Copernican chain; perhaps technical intelligence or intelligence at all is rare or even nonexistent elsewhere. That is fundamentally now an empirical question, but to the extent we can reason about it based on observations here on Earth, the question turns on the tension between *contingency* and *convergence*. Those who favor contingency emphasize how many things must have gone right or conversely, how many things must *not* have gone wrong, for us to wind up as what we are today, with the civilization we have now. For example, they might point out that if the asteroid had not hit us 65 million years ago, we would not be sitting in this auditorium today. And that was fundamentally a statistical event.

This viewpoint is in tension with those who emphasize convergence, the idea that there may be many parallel pathways towards a common functional outcome. This confrontation of contingency and convergence is now, given our current ignorance, the central debate in this area. The problem is that it is extremely difficult to look back at the history of the Solar System, or that of the Earth, and reach confident judgments about which of those arguments is the dominant one, or which of them in a particular context should be given the advantage. Again, somewhere the Copernican chain has to break down, but where it breaks down is not clear: our Sun is not exceptional, our planet may or may not be exceptional, our biosphere may or may not be exceptional, but Woody Sullivan is clearly exceptional. We can not extend the Copernican Principle out to the conclusion that we can posit an infinite number of Woodies on an infinite number of worlds, however pleasant and agreeable that particular Universe might be ... Woody's existence is clearly a contingent outcome; it is not a necessity. Where in the Copernican chain do we make a transition from necessity to contingency?

Carl Sagan (1974) discussed some of these issues a few decades ago in his paper, "The origins of life in a cosmic context." He did not use the vocabulary I have been using here, but I think that these were some of the issues with which he was concerned. In his 1974 paper, Carl talked about a number of questions that were essential for understanding the origins of terrestrial life, questions that could be illuminated with an extraterrestrial perspective. One of the questions that he emphasized was what he called distinguishing the contingent from the necessary. And he argued that only the study of extraterrestrial life would allow us to avoid dangers inherent in trying to reach general conclusions about the nature of life based on the single example we have from terrestrial biology. Carl wrote that: "In our present profound ignorance of exobiology, life is a solipsism. There is no aspect of contemporary biology in which we can distinguish the evolutionary accident from the biological *sine qua non*. We cannot distinguish the contingent from the necessary." Broadly, I think that this point remains correct, and it remains true that an extraterrestrial perspective, once made possible by empirical knowledge of other life, would go a long way towards helping us answer those questions. But unless SETI hits the jackpot, it is unlikely that many of those questions, at least insofar as intelligence and civilizations are concerned, are going to be resolved anytime soon.

In the absence of that knowledge, we are free to examine the terrestrial record or the record of our Solar System to try to draw some more general conclusions. I want to caution that at every step, whether one is an optimist or a pessimist, there is danger lurking. Let me give a particular example: it is becoming a commonplace now to cite George Wetherill's (1994) elegant paper showing that if Jupiter did not exist, the flux of comets upon the Earth could be 1,000 times greater, and that that would have important and grim implications if not for the origin of life, then probably for the evolution of life, and in particular for the evolution of a civilization.

What troubles me with respect to taking the next step, and concluding that this provides an example of just how unusually favorable for civilization our particular planet turns out to be—because look, if you just take Jupiter away, suddenly things are very grim for us—is that what one is doing in that case is reaching into a very complex system with good *post hoc* knowledge to remove exactly the item one needs to remove to get a conclusion that causes the collapse of civilization, as it were. But a generalizable analysis requires a broader context, and that broader context has to be: how would the system have changed overall if you had built in the required difference *from the very beginning*? How different would the Solar System need to have been to have produced the no-Jupiter outcome in the first place, and how likely is such a Solar System, compared to one with a Jupiter or Jupiters?

In fact, George Wetherill spends some time in his paper talking about this, and he runs a suite of models. In the case of the model that is commonly cited now—the one that looks more or less like our system but with comet fluxes that are 1,000 times higher—he asks, "How would you wind up with a solar system that looked like this?" And he considers a Solar System in which the 10 to 15 Earth-mass core of a potential Jupiter either did not form at all, or formed too slowly to accrete hydrogen and helium before the gas was swept away. In the latter kind of Solar System there would evidently be a much higher comet flux on Earth, although I do not think that we know enough yet to quantify that Solar System's theoretical likelihood. But in the former case, Jupiter goes missing because the mass density of the original disk from which the planets accrete falls off too quickly with heliocentric distance for Jupiter to form. So there are simply too few comets in proto-Jupiter's feeding zone to wind up with a Jupiter. The question then becomes how many of the candidate early Solar Systems that lead to this outcome still produce enough comets for a much higher collision flux on Earth, or if some or many of those candidates fail to form a Jupiter because the disk out of which the comets and planets accrete gets truncated or falls off in mass so quickly beyond a certain heliocentric distance that the inner planets do not experience much impact flux after some heavy early bombardment. And how frequent or rare are these various possibilities? Until we can answer these questions, theoretically or empirically, we cannot assess whether an Earth with a low enough impact flux for advanced life to arise is rare or not.

It is also worth mentioning that in our own Solar System, comets currently account for only a fraction, and probably a minor fraction, of the impact flux experienced by the Earth. Some Earth-crossing objects are thought to be burned-out short-period comets, but most are asteroids originating in the asteroid belt. Most Earth impacts are due to asteroids. But the asteroid belt likely exists because Jupiter's gravitational stirring prevented the accretion of these objects into a single world. Had Jupiter not been present, the asteroid impact flux experienced by the Earth would be lower than today.

So I think these arguments for contingency need to be applied with great care. As a final example, note that in his paper Wetherill looks at a number of other scenarios including one in which Jupiter and Saturn form more or less as they are, but the mass distribution in the nebula is such that Uranus and Neptune grow to be the size of Saturn. That is, the system looks more or less like ours, but there are another two Saturns out there. And in that kind of solar system, because of the now Saturnian-size Neptune and Uranus, there is essentially no cometary flux that the Earth experiences at the present time—so here is another type of solar system where the cometary flux is reduced to near zero. If we adopt the panglossian view that our Earth is the best of all possible worlds and our cometary flux is just right, then one can say that both these extremes—too many cometary impacts or too few—are bad, but of course we do not know, within some broad range, what comet flux is the best for life, or for multicellular life, or for civilizations to arise.

I would suggest, but this is no more than a speculation, that what we are going to find is that there are a wide variety of planetary systems that exist in the Galaxy, that ours is not going to turn out to be at all typical, but that ours is also not going to turn out to be extremely rare—it is just going to be one of a vast variety of planetary systems that can exist. But that is a speculation, and the important and exciting thing is that we will actually begin to answer that question later this decade, with the Kepler mission. One can also hope for that kind of insight in other realms, for example with respect to detecting technical civilizations via SETI. But I fear that, unless we are very lucky, that kind of empirical information is much further away.

4. LIFETIME OF CIVILIZATIONS

Let me conclude by saying a bit more about what we might do in the absence of that kind of information, and in that context I would like to recall the Drake Equation (Sagan, 1973; Drake and Sobel, 1992). The Drake Equation is one way to order what we do or do not know about the prospects for intelligent life elsewhere in the Universe. It is clearly not an equation like many physics equations, like, say, the Ideal Gas Law. It is not an equation where you have embedded a kind of hypothesis about the way the physical world works, that you can then go and test. Rather it is what is sometimes called a 'Fermi equation' (Webb, 2002). I am not referring to Fermi's Paradox, rather to Fermi's famous Ph.D. exam question of "How many piano tuners are there in the city of Chicago?" If you can not answer that question off the top of your head, what you *can* do instead is explode that number into a product of terms: how many people live in Chicago, what is the average size of a family, what fraction of families own pianos, how often do pianos have to be tuned, how many pianos can a piano tuner tune in a day, and so on. You explode the calculation into the product of a set of terms that you hope you can estimate.

You can do that successfully in the case of the piano tuners equation, but you cannot do that with all the terms in the Drake Equation. In the piano tuner case, this approach makes an initially seemingly intractable problem tractable. It potentially does something else as well, which is to point out some relationships that might not otherwise necessarily spring to mind. One of those relationships that the Drake Equation illuminates is that the number of civilizations in the Galaxy, should any other civilizations exist, is proportional to the average lifetime of the civilizations.

I wish to end with a final comment about the cosmic perspective that I know is important to Woody as well as to many others in this lecture theatre. The idea of thinking about the prospects for extraterrestrial civilizations through this last term in the Drake equation necessarily makes us think about the lifetime of civilizations, and therefore what is required to preserve our own civilization on this particular planet. My remarks here will be personal. That is risky, because it is too easy for such remarks to sound either trite or pompous. I will try to avoid that but I may not succeed, and if I do not, I apologize.

For the last decade I have spent much of my time dealing with issues of proliferation of so-called 'weapons of mass destruction'—nuclear weapons proliferation and biological terrorism in particular (e.g. Braun and Chyba, 2004; Chyba, 2002). My appointment is now split between two terrific organizations, the SETI Institute and the Center for International Security and Cooperation at Stanford University. I would like to use a recent personal example to illustrate what I think the scientific community is doing wrong, or at least not doing well enough, and how we in that community have a responsibility to do better.

Just under a year ago, I had the remarkable experience of giving a couple of talks on biological terrorism to two venues. The first was in Washington DC, at a foreign policy think tank. The talk was about how rapidly advanceing biotechnology—one possible aspect of getting to the post-Biological Universe that Steve Dick talked about—is soon going to enable new types of bioterrorism that will be available to small groups of the technically competent (e.g. Chyba and Greninger, 2004). It is clearly coming. If you make Moore's Law type diagrams for the evolution of fundamental aspects of biotechnology, such as the speed of sequencing or the speed of synthesizing DNA, you see that biotechnological power is increasing exponentially (Carlson, 2003), and as fast or faster than Moore's Law in computing power (see Figure 1). And these capabilities are spreading around the world for very good reasons. The details would be a separate talk; I will not dwell on it, but will make just one point.

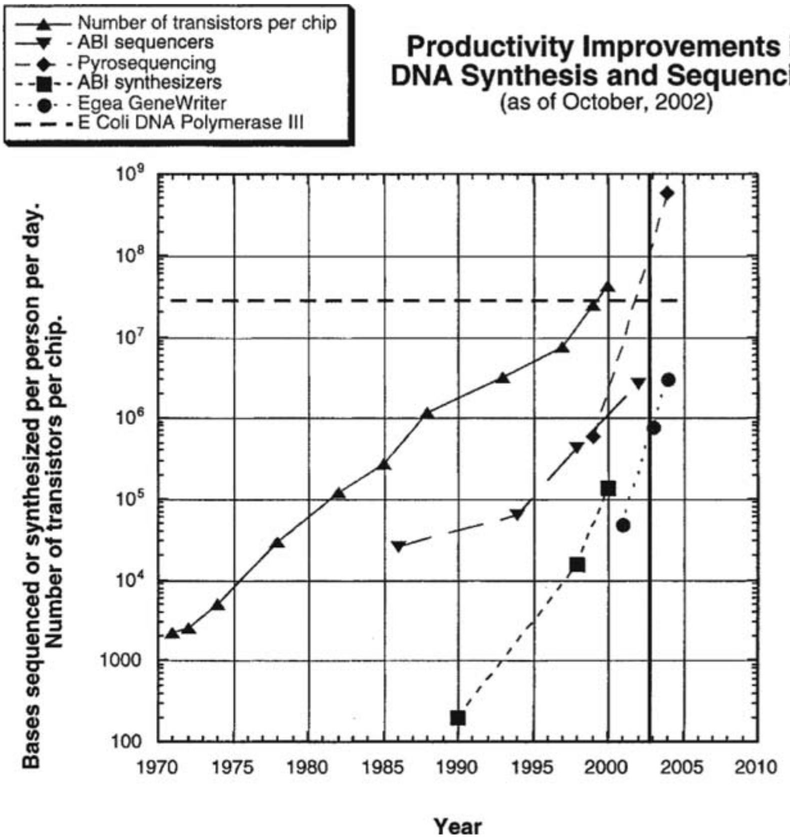

Figure 1. The increase in DNA synthesis and sequencing productivity through time for a variety of technologies is compared to Moore's Law for increasing computer power. The rate at which a single molecule of DNA Polymerase III replicates DNA in the bacterium *E. coli* is also shown. All biological data are referenced to an eight-hour day. Biotechnological power, as represented by DNA synthesis and sequencing productivity, is now increasing as fast or faster than Moore's Law (after Carlson, 2003: 204).

For my talk at the think tank in Washington, I was confident that everyone in that room knew what the Biological Weapons Convention was, knew roughly when it had been negotiated, knew roughly what it said, and knew the history of the efforts in the 1990s to negotiate a protocol to the convention that would put some monitoring and inspection teeth into the global prohibition against biological weapons. They would know that those negotiations had failed, and they might well know the reasons the Bush administration gave for why they had withdrawn from these negotiations. They might even know the name of the Hungarian ambassador who had led that six-year-long negotiation effort. But I also suspected that, with the exception of a certain few participants, they would have little technical knowledge of biotechnology. For example, they probably would not know what the polymerase chain reaction was, a method for amplifying DNA. They probably would not know about modern techniques for directed molecular evolution. So I spent some time in that seminar describing those technologies, and little time describing the Biological Weapons Convention or its draft protocol.

Two days later I was in San Diego giving what was ostensibly the same talk. I was giving it at a leading biotechnology company, one that I particularly admire, and it was satisfying that many in the company came out for the talk. In that audience it would have been laughable for me to have described the polymerase chain reaction. These people do biotechnology in their sleep; they use it every day of their professional lives. But conversely, again with the exception of several individuals, I was confident that I could not expect most to know details of the Biological Weapons Convention, the attempt to negotiate a protocol to it, why that protocol negotiation had failed, whether that was a fundamental problem with the protocol or whether it was a political issue, or how those two factors interacted.

We are in a situation where the people who develop and use the technology day by day are too disconnected from the different set of people who follow the treaty negotiations. This is a recipe for disaster, and matters have only gotten worse over the last decade. It got worse in 1995 when Congress eliminated its Office of Technology Assessment, deciding to shed its own scientific and technical in-house staff. At the other end of Pennsylvania Avenue, the President's science advisor has grown less and less powerful over the last three decades. But I do not want simply to complain about the Government; I think that the academic community and the scientific community also bear responsibility. Too often, the academic and scientific communities do not reward scientists who have policy interests. These remarks are *not* personal now. I have no complaints in that regard; I have been very fortunate. But it is still too common that scientists who are interested in policy can at best hope not to be punished for those kinds of interests.

5. CONCLUSION: THE CHANGING UNIVERSITY

I will end with a related point, that universities are currently not well designed to address many of the most important problems we need to address this century. Many of the problems we have to address are intrinsically interdisciplinary; that is clear to many of us in astrobiology, but we also have to step back and see the interdisciplinary scope of so many of the grave questions our civilization faces—a scope that extends well beyond scientific disciplines. Somehow universities have to find a way to structure themselves so that interdisciplinary collaborations and interdisciplinary individuals are rewarded. Really doing that requires meaning it in hiring and promotions. Disciplinary knowledge will always be important; it is an essential part of what universities do—but somehow they have to find a way to restructure themselves so that interdisciplinary work is just as natural and just as respected.

It would be nice to say that that could be facilitated with new money from the Government, just as astrobiology was facilitated by an institutional grants program begun by NASA—and we saw how positively the scientific community responded to that. We are all astrobiologists now. But the academy community should not wait for money from the Government before it begins to do what is needed to help provide the knowledge base and the kind of people we need to address some of civilization's biggest challenges. That is perhaps not a 'cosmic perspective', but it is a call for a broader perspective about our own future, and our responsibilities towards it.

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HISTORY OF ASTRONOMY

GROTE REBER (1911-2002):
A Radio Astronomy Pioneer

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Abstract: A forceful personality and self-confidence led Grote Reber to a series of remarkable discoveries in radio astronomy, and later to a wide variety of research in many other fields of science and technology. Although he worked primarily as an amateur, independently of the scientific establishment, Reber was ultimately recognized with many of the major prizes in astronomy.

Key words: Grote Reber, radio astronomy, electronics, amateur radio, history, beans, geology, archeology

1. INTRODUCTION

In 1959 Grote Reber (Figure 1) arrived at the National Radio Astronomy Observatory (NRAO) in Green Bank, West Virginia, to supervise the reconstruction of the 32-ft (9.75-m) radio telescope which he originally built in 1937 in Wheaton, Illinois.

Figure 1. Grote Reber at Green Bank in 1960.

W. Orchiston, (ed.), The New Astronomy: Opening the Electromagnetic Window and Expanding our View of Planet Earth, 43-70.
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Over the following decades I got to know Reber well during his visits to Green Bank. Then, in 1988, I visited him in Tasmania, where he lived in a modest home (Figure 2) which he characteristically designed and built himself.

Figure 2. Reber's energy-efficient home, Bothwell, Tasmania.

His house was very sparsely furnished with only a few pieces of furniture, and mostly was filled with old radio receivers, cartons and cartons of scientific records, notes, logbooks, correspondence, souvenirs from his travels, old radio tubes, ham radio equipment, and other electronic components from the 1920s and 1930s, etc. (e.g. see Figure 3).

Figure 3. Reber demonstrates a piece of equipment to Karl Jansky's son, David.

As a ham radio enthusiast myself, I drooled when a saw all this and suggested to Grote that it belonged in a museum. He did not respond, but six years later, he wrote me (Reber, 1994b):

> I still have most of the electronic equipment used on the dish. Also chart records, logbooks, literature, etc. They should be preserved for posterity. No one lives forever. It is a good thing. Currently I am in good shape. I can place the palms of my hands on the floor without bending my knees. Can you? I'll be around for quite some time, however the above should be arranged before too long. Find out how much it will cost; then add a round trip air fare for me plus some expense money. Place this in your next NRAO budget.

I asked Grote to get several quotations for the shipping costs, which he did. Hobart-based Allied Pickfords appeared to offer the best service, and they asked for AU$4,400 to deliver everything from Bothwell to Green Bank. NRAO Director, Paul Vanden Bout, agreed to pay the bill. But after I had mailed a check to Allied Pickford, I received the following telegram:

> Dear Mr. Kellermann
> There was considerably more than was quoted for. We quoted Mr. Reber for the removal of 50 small cartons from Bothwell to Green Bank. In fact, there are 80 boxes which most are wooden crates. There needs to be an adjustment from $4400 to $7500.
> What do you intend to do? (Allied Pickford, 1994).

I went back to Paul Vanden Bout, and thankfully he agreed to cover the additional costs. Grote's consignment finally arrived in Green Bank in the spring of 1994 (see Figure 4), and we discovered that many of the wooden crates had clearly been made in Green Bank for Grote during his 1959/1960 visit, and apparently had travelled to Tasmania and back again without ever being opened!

Figure 4. Part of the Reber collection in the Green Bank warehouse.

In the summer, Grote turned up at Green Bank, and he proceeded to help the local staff sort out all his papers and artifacts. He also spent some time working to bring his Wheaton antenna to operational status (see Figure 5).

Figure 5. Grote Reber poses in front of his reconstructed antenna at Green Bank in 1994.

2. THE GOLDEN YEARS IN WHEATON

Grote Reber was born in Chicago on 22 December 1911 and grew up in the Chicago suburb of Wheaton, Illinois. His father, Schuyler Colefax Reber, was a lawyer, and part-owner of a canning factory. His mother, Harriet Grote (Figure 6), had been an elementary school teacher in Wheaton. Among her seventh and eight grade students was young Edwin Hubble. Red Grange, later to become a legendary football hero, delivered ice to the Reber home. Grote claimed that his parents forgot to name him, so his birth certificate merely gives his name as “Baby Reber”. Although he was called Grote by his parents, it was only when he was 20 years old that he officially had his name verified on a revised birth certificate by Cook County Clerk, Richard E. Daley, who later became the Mayor of Chicago.

Figure 6. Harriet Grote Reber.

When he was only sixteen, Grote Reber obtained his amateur radio license W9GFZ (see Figures 7 and 8), signed by then Secretary of the Interior, Herbert Hoover. His early radio receivers had separately tuned stages. In the late 1920s he noticed that if he connected an antenna to his receiver, the noise level increased when all the stages were tuned to the same frequency, but not if the antenna was disconnected. Probably he had detected Galactic radio noise at 10-m wavelength, but he did not realize this until many years later.

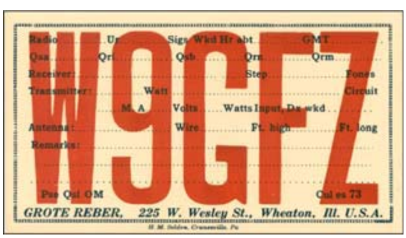

Figure 7. The QSL card that was used to confirm Reber’s amateur radio contacts.

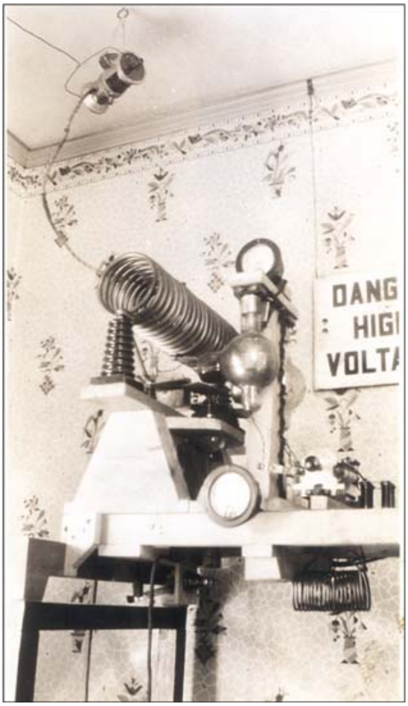

Figure 8. Grote Reber's amateur radio station in the early 1930s.

After graduating from high school, Reber attended the Armour Institute of Technology (now the Illinois Institute of Technology). Just a few months before the death of his father, in 1933, he graduated with a degree in Electrical Engineering (and specializing in electronics and communications). He did well in his electronics courses, but less so in mathematics. After receiving his degree, Grote (Figure 9) held a series of jobs with various Chicago companies, including General Household Utilities (1933-1934), the Stewart-Warner Corporation (1935-1937), the Research Foundation of the Armour Institute of Technology (1939) and, finally, the Belmont Radio Corporation.

Reber had read Karl Janksy's papers in the *Proceedings of the Institute of Radio Engineers* and had listened to Jansky's 'star noise' which was rebroadcast by NBC. He was intrigued by the concept of cosmic radio emission, and after contacting more than fifty countries via his amateur radio station, he was looking for new challenges. He wrote to Jansky and asked for a job working with him, but was surprised and disappointed to learn that Bell Labs did not plan any further work in this area. He then contacted a number of observatories and university departments, but found that very few astronomers were interested in Jansky's 'star noise'; they were all busy with their own projects. He tried to interest Otto Struve and other astronomers at the Yerkes Observatory, but they showed little enthusiasm. Later, Reber recalled that Gerard Kuiper felt that "… the whole affair was at best a mistake and at worst, a hoax." Reber claims that

> The astronomers were afraid of it because they didn't know anything about radio. The radio people weren't interested because it was so faint it didn't even constitute an interference. Nobody was going to do anything. So, alright, if nobody was going to do anything, maybe I should do something. So I consulted with myself and decided to build a dish!

Figure 9. Young Grote Reber in 1934.

In 1938 he took optics and astronomy courses at the University of Chicago, including a course in astrophysics from Philip Keenan, but during the summer of 1937 he used his own funds to build a 32-ft parabolic transit dish in a vacant lot next to his mother's house (Figure 10). Neighbors speculated about the purpose of the unfamiliar-looking structure rising in the small town of Wheaton, but Reber's mother found it a convenient place to hang her washing!

Figure 10. Reber's wooden dish in Wheaton. In the front is the service tower used to gain access to the feed and receiver.

Reber then built and tested a series of sensitive radio receivers which he placed at the focal point of his dish (Figure 11) the connecting wires running through a coal chute to his observing room in the basement of his mother's house. Although Jansky's work was carried out at a wavelength of 15 meters in the shortwave band, Reber initially decided to observe at the much shorter wavelength of 9 cm, where he thought he would get better angular resolution, and where he expected the radio noise to be very much stronger if it was due to thermal radiation. But he was unable to detect any radio noise from the Galaxy, from several bright stars, or from the Sun, the Moon or nearby planets. He tried moving first to 33 cm where more sensitive and stable instrumentation was available, but without success, and finally to 1.9 meters where, in the spring of 1939, he finally succeeded in detecting Jansky's Galactic radio noise, which he called "cosmic static".

Figure 11. Grote Reber atop the service tower, accessing the focal point of the dish.

Automobile ignition noise interfered with Reber's measurements, so he observed at night, laboriously writing down the readings from his detector output every minute. At times, when Grote was not available, his brother's wife, Jean, would mark down the readings, but if she missed a reading, he would rant and rave about it when he got back home (Andrea Reber, pers. comm., 2003). During the day Reber worked in Chicago, and he commuted by train; the journey took one hour each way. When this author went from Chicago to Wheaton in 1999 using the same train, it still took an hour.

Upon returning home, Reber would catch a few hours sleep each evening before returning to his night's work. On weekends, he analyzed his data and produced the first high-resolution map of the radio sky. His discoveries were first received with skepticism by the astronomical community, and he had great difficulty in getting his papers accepted for publication in the astronomical literature. Reber wanted to reach astronomers instead of just radio engineers, so he submitted a paper to the *Astrophysical Journal.* According to Jesse Greenstein, then a young Yerkes astronomer, since, Reber "… had no academic connection and unclear credentials …" his paper "… produced a flurry of excitement …" at the *ApJ* editorial offices at the Yerkes Observatory. Several delegations of astronomers including Bok, Chandrasekar, Greenstein, Hertzberg, Keenan, and Struve went to Wheaton to inspect Reber's equipment. Keenan reported that Reber's apparatus "… looked modern …" and that his work, "… looked genuine." Reber's paper was finally accepted by Otto Struve, the Editor, as a short note, but only with some editorial censoring of his speculative interpretation (Reber, 1940a). Nevertheless, he had succeeded in forcing the first links between radio scientists and astronomers (cf. Jarrell, 2005).

Reber later described how

> Otto Struve didn't reject my 160 MHz paper. He merely sat on it until it got moldy. I got tired of waiting, so I sent some other material to the Proceedings of the IRE. It was published promptly in the February, 1940 issue (Reber 1940b). From a much slower start, this beat the ApJ by four months. During the early days of radio astronomy, the astronomy community had a poor track record. The engineering fraternity did much better!

Among the few optical astronomers who paid serious attention to Reber were Bengt Stromgren (who was visiting America) and Jesse Greenstein. Greenstein was fascinated by Jansky's discovery of cosmic radio noise and in 1937 had written a paper with Fred Whipple unsuccessfully trying to explain the nature of the radio signals as thermal emission from interstellar dust (Greenstein and Whipple, 1937). Greenstein had met Reber when he was sent by Struve to evaluate the Wheaton facility, and later he and Reber became good friends. Shortly after WWII they teamed up to publish the first review of what later would be called 'radio astronomy' (see Reber and Greenstein, 1947).

As a result of his job as a radio engineer, Reber had access to state-of-the-art test equipment and the latest high-frequency vacuum tubes. He then built equipment to work at a shorter wavelength of 62 cm to improve his angular resolution, and went on to map the Galactic radio emission. To his disappointment, he find—contrary to theory—that at 62 cm the cosmic static was weaker than at 1.9 meters (160 MHz), but he realized that "… if you can't change the data, so you have to change the theory." In September 1943 he detected radio emission from the quiet Sun, and a few months later, while demonstrating his equipment to visitors from Washing-ton, powerful solar radio bursts drove his recorder off scale.

During a period of less than a decade Grote Reber had demonstrated the non-thermal nature of Galactic radio noise, mapped the radio emission from the Milky Way, noted the evidence for spiral arms in the Galaxy, published evidence for the first discrete radio sources, and detected radio emission from the quiet Sun as well as detecting spectacular radio bursts from the active Sun. He also began to build a receiver to detect the 21 cm hydrogen line, but never finished this project.

3. ESCAPE TO HAWAII

In 1946, Reber developed plans to build a large fully steerable dish 200-ft in diameter and operating to wavelengths as short as 10 cm. He estimated this might cost $100,000, but was unable to find financial support for his ambitious plan. Meanwhile scientists in Australia and the U.K were making striking discoveries in the new field of radio astronomy (see Edge and Mulkay, 1976; Orchiston and Slee, 2005; Sullivan, 1984). In 1947 Reber accepted a position at the National Bureau of Standards (NBS) Central Radio Propagation Laboratory (CRPL) with the prospect of building a dish in the 75 to 100-ft diameter range, and he sold his Wheaton dish and all of the associated instrumentation, including a 1,400 MHz amplifier and feed, to the NBS for $18,570. His dish was then moved to Sterling, Virginia, near to the current location of the Dulles Airport.

However, Reber quickly became discouraged by the lack of support for his planned large radio telescope, and he had become increasingly frustrated working under the Government bureaucracy as well as the deteriorating atmosphere in Washington reflected by the growing impact of McCarthyism. He was impressed by the discoveries reported by Australian radio astronomers using the sea interferometer technique, which measured accurate positions from the precise timing of radio sources as they rose over the sea, and he realized that in order to obtain the accurate two-dimensional coordinates needed for identification with optical counterparts, he would need to make measurements at both rising and setting, and that the best place to do this was from a mountaintop in Hawaii. Apparently, one day he left NBS for vacation in Hawaii, and he never returned.

Once settled in Hawaii, he built a rotating antenna (Figure 12) at the top of the 3,000 meter Mt. Haleakala on the island of Maui. This sea interferometer was designed to observe at between 20 and 100 MHz, and the effective baseline was 6 km, giving a resolution of ~1′. However, ionospheric refraction and terrestrial interference limited the effectiveness of this novel radio telescope, and Reber (1955, 1959) was only able to obtain useful results for a few of the strongest radio sources. He finally concluded that "… mountain tops are not suitable for radio telescopes."

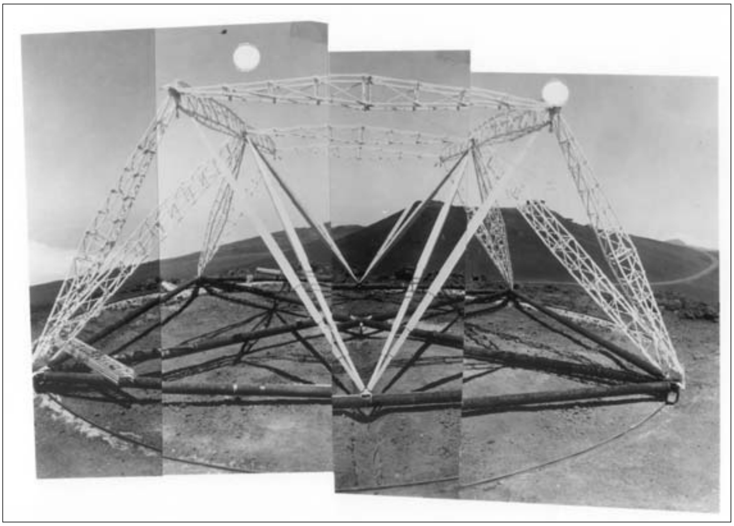

Figure 12. The rotating antenna system built by Reber on the top of Mt. Haleakala, Maui, Hawaii.

4. RADIO ASTRONOMY DOWN UNDER

While the rest of the world moved on to shorter and shorter wavelengths in the quest for better resolution and in search of the multitude of molecular transitions that exist in the millimeter and submillimeter bands, Reber took pride in not following popular fashions and chose to concentrate on the extremely long hektometer wavelengths where he felt he could make a bigger impact. In November 1954 he moved to Tasmania, where he expected that ionospheric effects would be at a minimum. The southern winter of 1955 was near a sunspot minimum, and initial results at 141, 209, 333 and 576 meter wavelengths were very encouraging (Reber and Ellis, 1956). Over the next twenty years, Reber designed and built a series of arrays to study Galactic radio emission at wavelengths as long as 2.1 km. His 2 MHz array (Figures 13-15), which consisted of 192 dipoles 3500-ft on a side, was, and still is, the largest 'filled-aperture' radio telescope ever built (Reber, 1968) and stands as a challenge to the current ambitions to build a square kilometer array. At these long wavelengths, Reber noted that the sky is very bright everywhere, especially at the poles, and that the Milky Way appears as a dark absorption band which he correctly understood was due to free-free absorption by interstellar electrons (Reber and Ellis, 1956). Reber hoped to exploit fluctuations in the ionosphere, which he claimed would create narrow holes that would form a high resolution window to the Universe. Unfortunately, the excellent observing conditions of 1955 never returned, and increasing levels of ionospheric absorption, along with the increased density of the interplanetary plasma following the unusually low sunspot minimum of 1955 and increasing levels of broadcast interference, served to limit the results from Reber's hektometer observations.

Figure 13. Grote Reber's 2 MHz (144 meter) array near Bothwell, Tasmania.

Figure 14. Adjusting the 2 MHz array.

Figure 15. Grote Reber in 1988 at the site of his 2 MHz array.

5. GROTE REBER AND COSMOLOGY

Inspired by reading Hubble's *Observational Approach to Cosmology* (1937), which was given to him by his mother, Reber did not accept the concept of an expanding Universe—which he considered "religious dogma". He argued vigorously in favor of a tired-light theory rather than the conventional interpretation of the redshift, and gave many lectures on the theme, "The Big Bang is Bunk", or "Red Shifts and Hubble". For many years, he was unsuccessful in getting his ideas published and concluded that "… astronomers and physicists have a vested interest in the status-quo of the big bang cosmology …", remarking that there is "… nothing malicious, sinister, or corrupt in this, just narrow minded incompetence." *Science*, *Nature* (which he referred to as a "… weekly science fiction magazine"), the *New Scientist*, *Physics Today*, the *Astronomical Journal*, the *Journal for the History of Astronomy*, *Publications of the Astronomical Society of the Pacific*, and *Mercury* all ignored or rejected his paper, which he circulated privately under the title, "Endless, boundless, stable Universe" (Reber, 1977). Later this was finally published in the *Proceedings of the*

Astronomical Society of Australia, with the somewhat revised title, "A timeless, boundless, equilibrium Universe" (Reber, 1982). He was particularly opposed to what he considered the arbitrary nature of "prepublication review," which he felt "… endangers academic tradition." He compared himself to Galileo, but noted that "… the establishment has not yet started to burn heretics."

In order to prove his theories, Reber conceived the idea of releasing liquid hydrogen into the ionosphere, so it could recombine with free electrons and thus make the ionosphere temporarily transparent to wavelengths as long as 300 meters. For the last twenty years of his life, he relentlessly tried to obtain demobilized ICBMs to carry a canister of hydrogen into space, but he was frustrated by the seemingly endless bureaucracy and the large cost associated with any rocket-based activity. He wrote letters to colleagues, friends, observatory directors, NASA laboratory heads, and congressmen to solicit their help, but he stubbornly refused to take 'No' for an answer and continued to seek surplus rockets until shortly before his death. In 1985, however, Reber, Ellis and others arranged for the ill-fated *Challenger* Space Shuttle to fire its engines during a pass over Bothwell, in a partially-successful attempt to create a temporary hole in the ionosphere.

6. BEANS AND MORE

Grote Reber was curious about everything he came in contact with, and did not accept conventional wisdom without detailed scrutiny. In addition to his pioneering work in radio astronomy, he also pursued and published research in a variety of fields ranging from radio-circuitry and ionospheric physics, to cosmic rays, meteorology, botany, biophysics, and archeology.

Aside from his radio astronomy work, Reber is probably best known for his innovative experiments with growing beans in Hawaii, Tasmania, and Green Bank (Figure 16). He claimed that pods which grew on vines that he forced to wind in the opposite direction from their natural right-screw pattern had a larger ratio of beans to shucks than a control sample which grew on vines that were allowed to grow naturally (Reber, 1960; 1964). But he never made clear if this unexpected result was due to a decrease in the weight of the shucks on the reversed vines, or to an increased yield in the beans. Curiously, this effect was only apparent in pods that grew more than a few feet above the ground. To demonstrate that the direction of twining was genetic and had nothing to do with the motion of the Sun or the coriolis forces, on one long boat trip to Tasmania he grew beans to see if they would change direction when he crossed the Equator. He also used his beans for more conventional experiments in genetics, which dealt with the propagation of bean colors along multiple generations of plants (see Reber, 1967).

Figure 16. Preserved bean vines found among Reber's papers sent to Green Bank.

While working in Hawaii, Reber was plagued by the effects of the ionosphere and atmosphere on his sea interferometer observations, which were necessarily made at low elevations. Rather than be discouraged, he turned his attention towards understanding the offending medium and during this period he published several important papers on the ionosphere (Reber, 1954) and the atmosphere (Reber, 1955). He also estimated of the age of lava flows on Haleakala by accounting for the family genealogy back to an original observer of the eruption, as told by a local Hawaiian. Guessing that the age of an average Hawaiian generation was 25 years, he estimated that the flow occurred in AD 1762 ± 15 (Reber, 1959). While working in Tasmania, he used radiocarbon dating techniques to determine the age of Aboriginal campfire sites (Reber, 1987). He also studied the bacterial content of streams in Tasmania, finding one mysterious stream that flowed through several pastures yet contained no bacteria. In addition, he analyzed seven years of accumulated data from an underground muon telescope, and claimed evidence for both a solar and a sidereal component of cosmic ray intensity, which peaked up near 04h 30m and 09h Right Ascension (Reber, 1966a). By analogy with growth of radio astronomy since the time of Jansky, Reber predicted a strong future for cosmic ray astronomy, but pointed out that suitable equipment, with adequate sensitivity and resolution, would be needed.

Throughout his life, Reber maintained an interest in antennas, electronics and communications theory, and he published a number of papers in these areas in the popular and amateur literature (Reber, 1938; Reber and Conklin, 1938; 1939) as well as in professional journals such as the *Proceedings of the Institute of Radio Engineers* (Reber, 1959; 1960; 1961). He also held several patents, including one for a radio sextant that could be used to determine positions, even on cloudy days.

7. THE NAS, THE NSF AND THE NRAO

In 1964, the National Academy of Sciences completed a report planning for the future of ground-based astronomy in the U.S.A. Reber was greatly offended by the emphasis on "huge instruments," writing that future panels "… must be composed of people with more ability, imagination, wisdom, integrity, and an open mind." In a letter to *Science*, (Reber, 1966b) he argued against the construction of a large radio array, and commented that the plans for a 400-600 foot radome-enclosed dish "… displays an acute lack of imagination."

He was especially critical of the NRAO activities in Green Bank, commenting,

> Green Bank might as well be closed down. The best work likely to ever be done there has already been completed and published; namely my beans. Some of the things that go on there in the name of administration shouldn't happen to a dog. The net effect is that of a mortgage on astronomy. Only the duller members of the new generation will try to find a place at these institutions.

In 1967, he unsuccessfully applied for an NSF grant of $1.25 million to construct a large array in the northern hemisphere to operate at a wavelength of 144 meters. His suggestion that the array could be funded by "… slight curtailment of routine operations at Green Bank …" was not well received in Washington or at NRAO, and Grote did not easily accept the rejection of his proposal. He researched previous NSF astronomy grants and challenged the legality of the national astronomy centers, which he considered to be "… prestige institutions designed to impress the ignorant."

He wrote letters to the NSF Director, Lee Hayworth; to the President of the National Academy of Sciences, Fredrick Seitz; to various congressmen, including Senator William Proxmire; and he testified before a Congressional Congress, claiming that the "… staff at the NSF are mostly clerks quite uninterested and incapable of imaginative scientific leadership … and that the National Science Board were stuffy old men and would be better composed of butchers, bakers, and candlestick makers!" He then sought help from Ralph Nader and his Center for the Study of Responsive Law. From the National Academy of Sciences, he asked for "… positive leadership instead of basking in renown." To establish his credentials, so that that the recipients of his letters did not think they were coming from a crank, he would often refer the reader to the *Encyclopedia Britannica* article describing his accomplishments.

8. OTHER ACTIVITIES

Reber did not confine his activities to scientific areas, and was deeply concerned about a variety of social and political issues. He was in favor of increasing taxes in order to have a balanced budget. He was concerned about the U.S. presence in Korea and other parts of Asia, arguing "… to get out of Europe and stay out of European entanglements and to eliminate loans and gifts to foreign countries." He was furious when, in 1951, General Douglas MacArthur was relieved of his command in Korea. He wrote a scathing letter to Senator Paul Douglas of Illinois, demanding that President Truman and Secretary of State, Dean Acheson, be removed from office, and threatened Senator Douglas with his removal if he did not act promptly.

In his later years, Reber became concerned about the world population growth and the growing energy crises facing mankind. He argued against the increasing use of fossil fuel and built an energy-efficient home in Bothwell, where he lived for many years. According to one report, it worked so well that it melted the plastic pipes carrying the heated water. He was a long-time member of the Australian Electric Vehicle Association and designed and built a battery-powered car, which he named 'Pixie' (Figure 17). He stubbornly resisted the demands by the local automobile registration authorities to place the required license plate on the front of Pixie, arguing that it would destroy the car's aerodynamic properties.

One of his last scientific acts was to write the following warning:

> The human race is headed for disaster. Our activities are very dependent on our transportation system. In turn, it is dependent on liquid fuels mined from the ground. When this runs out, our civilization will collapse. Man will not disappear. However, his activities will markedly change. Go to a busy intersection at morning or evening rush hour. Cars are closely packed front to back. A 2000 pound vehicle is needed to move a 160 pound man, perhaps one in ten has a second person. Less than ten percent of the fuel consumed has any useful effect. This is the place greatest fuel saving may be secured.

Figure 17. Grote Reber stands outside his home and beside his electric car, 'Pixie'.

Except for the brief period between 1948 and 1951 (when he was at the Bureau of Standards in Washington) Reber spent his working life as an amateur, relying on his deep curiosity, his imagination and his skills as a professional electronics engineer, in combination with his persistent, forceful personality and stubborn disregard for conventional opinion. At various times during his career he held guest appointments at the National Radio Astronomy Observatory, Ohio State University, the University of Tasmania, the Australian Ionosphere Prediction Service and at the Australian Commonwealth Scientific and Industrial Research Organization. Although he never had a salary during this period, he did have access to the shops and to the test-equipment needed for his research. Similarly, while doing his pioneering work in Wheaton, he enjoyed the support of his employer and access to the latest vacuum tubes and other instrumentation.

Reber scorned the establishment. Throughout his career, he was unable to secure funding from any of the conventional sources, such as the National Science Foundation or the Department of Defense, and except for the modest support which he received from the Research Corporation, he relied on his own personal funds to give him the independence in his work that he insisted on throughout his career.

Grote Reber had an intense curiosity about everything he saw and heard. He kept meticulous records not only of cosmic phenomena but on everyday activities, such as how far he walked each day, and how long light bulbs from different rooms in his house lasted. While observing with his radio telescope in Wheaton, he counted the cars going by in each direction, and was puzzled by the apparent lack of conservation, as he recorded more cars going one way than the other. While in Tasmania, he noticed that parrots stand on their left foot, and hold berries with their right claw, and concluded that Australian parrots are 'right-handers'. Whenever he received a letter or catalogue, he would note the date of receipt along with the date of mailing, and he would report these dates to the sender. He seldom threw anything away, so he left behind a wealth of material for historians, including copies of all correspondence, the original drawings of his Wheaton antenna and much of the associated instrumentation, bills and receipts for personal as well as scientific purchases, and travel records. He collected souvenirs such as restaurant menus, hotel stationary, and napkins from airplanes, as well as old broadcast radios, vacuum tubes, and other early electronic components, and he had a large collection of catalogues and brochures.

9. RECOGNITION

Although he operated outside the mainstream of the astronomical community, and often ridiculed establishment science, Grote Reber was ultimately recognized by the astronomy profession with most of its major prizes, including the 1962 Bruce Medal of the Astronomical Society of the Pacific, the prestigious Elliot Cresson Medal of the Franklin Institute, the 1962 Russell Prize of the American Astronomical Society and the 1975 AUI Jansky Lectureship. He received an honorary D.Sc. degree from Ohio State University, and in 1999 he was named by the Illinois Institute of Technology as a 'Man of the Millennium'. In 1987, he was inducted into the DuPage County (Illinois) Heritage Gallery Hall of Fame.

10. AMATEUR OR PROFESSIONAL?

Grote Reber was the world's first radio astronomer, and for nearly a decade the only person in the world devoting significant effort to this field. His 32-ft home built radio telescope was the largest parabolic dish ever built at that time. Reber went to great effort to demonstrate the importance of his work to the astronomical community. His maps of the radio emission from the Milky Way and his report of intense non-thermal radio emission from the Sun provided much of the incentive for the dramatic growth in radio astronomy following the end of WWII, when former radar scientists and astronomers, primarily in the UK, Australia, and the Netherlands, built a series of ever more powerful radio telescopes leading to a series of remarkable discoveries which have changed our fundamental understanding of the Universe (e.g. see papers in Sullivan, 1984).

But, also the reverse was true. The low noise receivers that Reber developed for his radio astronomy studies were state-of-the-art devices. According to Sir Bernard Lovell, Reber's work was known to the radar scientists, who adopted his receiver designs.

Reber also had another, perhaps less well recognized, impact on radio astronomy. Of the many astronomers who were first exposed to radio astronomy through early visits to Wheaton, three went on to play major roles in the future development of the field. Otto Struve later became the first Director of the National Radio Astronomy Observatory, now the pre-eminent radio observatory in the world; Jesse Greenstein went on to found the radio astronomy group at Caltech; and Bart Bok began the radio astronomy program at Harvard and later, as Director of the Mt. Stromlo Observatory, promoted co-operation between Australia's radio and optical astronomers.

Although Karl Jansky was the first to detect cosmic radio emission, it was Grote Reber who—through his innovative experiments, forceful personality and stubborn persistence—finally convinced astronomers that it might be important, thus opening a new window on the Universe. He worked alone in a previously unexplored part of the electromagnetic spectrum, designing and building his own equipment.

Although Reber was an amateur in the sense that he was not compensated for his Wheaton research, starting in 1951, he received support for his research, and, for several years, a modest stipend, from the Research Corporation. They funded some of his activities in Hawaii and, until 1981, in Tasmania, as well as looking after many of his personal affairs. Over this thirty-year period, he received more than $200,000 in grants from the Research Corporation. Reber's educational background was on a par with many of the later pioneers of radio astronomy. He held a degree in electronics, took courses in astronomy at the University of Chicago, and through his employer had access to state-of-the art equipment and machine shops to support his Wheaton research. While working in Wheaton, and later in Washington, he corresponded with many of the then 'key players' in astronomy: Donald Menzel, Harlow Shapley, and Fred Whipple at Harvard; Jan Oort and Henk van de Hulst in Leiden; Bengt Stromgren in Denmark; Joe Pawsey in Australia; J.S. Hey in England; and Bill Gordon at Cornell; as well as many of the staff at Yerkes, but especially Jesse Greenstein. Over a career that lasted more than 70 years, Reber published over 80 scientific papers in more than 40 different professional scientific and engineering journals, and in semi-popular journals and magazines like *Sky and Telescope* (Reber, 1949a), *Scientific American* (Reber, 1949b), and the *Astronomical Society of the Pacific Leaflets* (Reber, 1950). He also presented papers at meetings of the American Astronomical Society, the Institute of Radio Engineers, and other professional societies. He enjoyed telling his own story, and published a number of delightful papers about his work and career (e.g. see Reber, 1958, 1984a, 1984b, 1988).

In a 1945 letter to Jan Oort, Reber stated that the total cost of parts for his Wheaton antenna was $676:96. Instrumentation cost about $500 to which he added another $550 after his initial success. In 1959, Reber's dish was rebuilt in Green Bank, West Virginia, where it stands today at the entrance to the National Radio Astronomy Observatory (see Figure 5), and the original equipment which he used in Wheaton is on display at the Green Bank Science Center. As early as 1947, he recognized the sensitivity limitations imposed by radar and the FM broadcast services; and he urged the creation of protected bands to study astrophysical lines, which he predicted might be observed in emission or in absorption.

From about the age of 40 Reber's hearing started to fail, and he wore a hearing aid, which he found convenient to turn off if he did not want to bother hearing what others were talking about. He once remarked, "I put my aid in my good ear and turn it off. This shuts out the noise." From his youthful experiments in Wheaton, until his death, as his nephew Jeff Reber described, "He obviously learned to ignore the opinions of those he didn't agree with." He had no patience for negotiation or compromise, and he was always forcefully direct in choosing his words. He knew what he wanted, and usually got it!

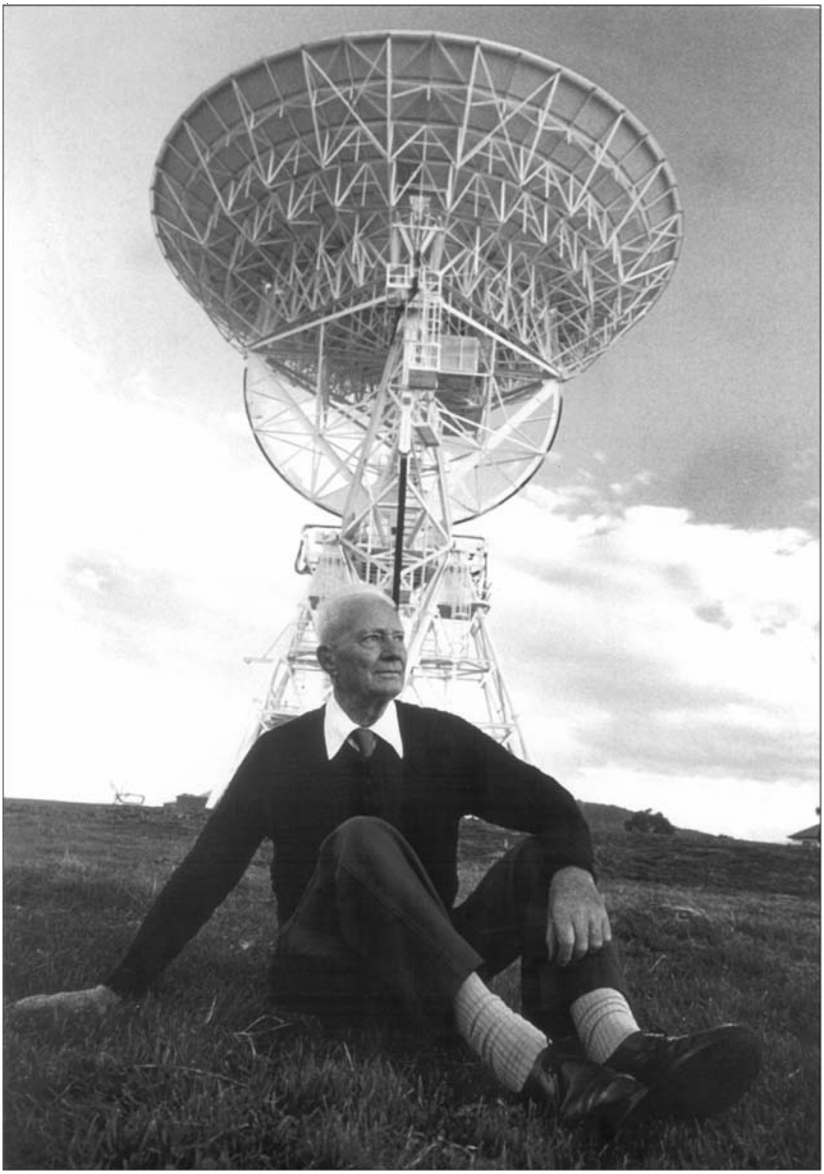

Figure 18. An obviously contented Grote Reber at the site of the University of Tasmania's 23 metre Mt. Pleasant radio telescope (reproduced with the permission of *The Mercury).*

As a result of investments made in the 1930s and 1940s and, in his later years, Social Security payments from both the United States and Australia, Reber (Figure 18) was financially comfortable throughout his life, even though he did not receive a regular salary after he left the Bureau of

Standards in 1954. But he was frugal in conducting his financial affairs. He used the back of old reports to make carbon copies of his letters in order not to "waste paper"; he drew circuit diagrams and drafted notes and reports on the back of old bank statements. Rather than hire someone to cut the grass in his fields, he borrowed a neighbor's sheep. Much to the chagrin of the local postmaster in Bothwell, he reused envelopes by writing "Return to sender", and he would remove from received letters any un-cancelled stamps for later use. But, he was not selfish or greedy. When writing for a reprint or catalogue he often would enclose a check to pay for the postage. In 1937, he spent a considerable fraction of his annual salary to build his Wheaton radio telescope. He also donated generously to charities, and he helped to support radio astronomy activities in Canada with substantial financial gifts.

Until a few months before his death on 20 December 2002, Grote remained active in a variety of scientific, political, and social issues. But he did his most productive work as a young scientist in the decade from 1937 to 1947, when he built the world's first radio telescope, made the first maps of the radio emission from the Galaxy, detected radio emission from the quiet Sun and discovered the powerful radio emission associated with solar flares.

At the time of his death his good friend, John Krauss remarked: "He was a one man self supporting laboratory. He was bold, individualistic, curious, outspoken, meticulous, brash, perseverant, and a colorful maverick."

Reber took great pride in his independence, pointing out that, "… there were no self appointed pontiffs looking over my shoulder to give bad advice. The things I want to do are the kind establishment men will have no part of."

11. ACKNOWLEDGEMENTS

In preparing this paper, I have made use of the scientific papers and correspondence of Grote Reber that are in the NRAO Archives[1] and at the Queen Victoria Museum in Launceston, Tasmania, the Archives of the Research Corporation, and in the Jesse Greenstein collection at the Caltech Archives. Personal discussions with Reber, tapes of various interviews and lectures, an oral history given to the Dupage Historical Society, as well as discussions with Woody Sullivan, Jesse Greenstein, Joe Tenn, Tony Tyson, Dave Jauncey, Paul Feldman, Jim Moran, Brian Andreen, Martin George, Grote's friend, Dale Blanchard, his niece, Andrea Reber, his nephew, Jeff Reber, and other accounts of Reber's life and work by John Kraus and Woody Sullivan, have all been invaluable. Unless otherwise specified, photographs are from the NRAO archives. It is a pleasure to thank Ellen Bouton, who compiled the list of Reber's publications, for many stimulating discussions about Grote Reber's life and work.

12. NOTES

1. The archives of the National Radio Astronomy Observatory contain original correspondence, technical and research materials, manuscripts and published papers, speeches, ham radio materials, newspaper and magazine clippings, photographs, and other miscellaneous materials relating to the life and career of Grote Reber. The finding aid to these papers may be found at http://www.nrao.edu/archives/Reber/reber.shtml

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DR ELIZABETH ALEXANDER: FIRST FEMALE RADIO ASTRONOMER

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Abstract: During March-April 1945, solar radio emission was detected at 200 MHz by operators of a Royal New Zealand Air Force radar unit located on Norfolk Island. Initially dubbed the 'Norfolk Island Effect', this anomalous radiation was investigated throughout 1945 by British-born Elizabeth Alexander, head of the Operational Research Section of the Radio Development Laboratory in New Zealand. Alexander prepared a number of reports on this work, and in early 1946 she published a short paper in the newly-launched journal, *Radio & Electronics*. A geologist by training, Elizabeth Alexander happened to be in the right place at the right time, and unwittingly became the first woman in the world to work in the field that would later become known as radio astronomy. Her research also led to further solar radio astronomy projects in New Zealand in the immediate post-war year, and in part was responsible for the launch of the radio astronomy program at the Division of Radiophysics, CSIRO, in Sydney.

Key words: Radio astronomy, New Zealand, 'Norfolk Island Effect', solar radio emission

I dedicate this paper to Woody Sullivan, who introduced me to Elizabeth Alexander's work.

1. INTRODUCTION

Although radio astronomy had its origins in the 1930s through the pioneering efforts of Karl Jansky and Grote Reber (Kellermann and Sheets, 1983), it only blossomed as an emerging scientific discipline after WWII. Part of the reason for this was the technological developments that occurred during the war, particularly those relating to radar (e.g. see Lovell, 1977).

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One of the wartime discoveries that provided an impetus for the post-war focus on radio astronomy was the independent detection of solar radio emission in Denmark (Schott, 1947), the United States (Reber, 1944; Southworth, 1945), England (Hey, 1946), Australia (Orchiston and Slee, 2002) and New Zealand. This paper is about Elizabeth Alexander's investigation of solar radio emission in New Zealand during 1945.[1]

2. ELIZABETH ALEXANDER: A BIOGRAPHICAL SKETCH

Frances Elizabeth Somerville Alexander neeé Caldwell (Figure 1) was born on 13 December 1908 at Merton, Surrey, but spent her early life in India, where her father was the first Professor of Chemistry at Patna Science College and later was its Principal. At the end of WW I she returned to England and after completing her secondary schooling entered Newnham College, Cambridge, graduating in geology in 1931 with First Class Honours and the Harkness Prize. In 1934 she was awarded a Ph.D. from the same University, with a thesis on the main outcrop of the Aymestry Limestone (A.J.B., 1959).

Figure 1. Dr Elizabeth Alexander, 1908-1958 (courtesy: Mary Harris).

In July of the following year Elizabeth married a young New Zealand physicist, Dr Norman S. Alexander, and in 1936 they moved to Singapore when he accepted the Chair in Physics at Raffles College; Elizabeth then began a study of tropical weathering. Their three children, William, Mary and Bernice, were born in Singapore, in 1937, 1939 and 1941 respectively. In 1940 and 1941 Elizabeth worked with the British Royal Navy (at the

Singapore Naval Base) on radio direction-finding, with the rank of Captain in the Naval Intelligence Service (In Memoriam …, 1959; Mary Harris, pers. comm., 2005; Obituary, 1959).

In early 1942, as the Japanese invasion of Malaya approached, Elizabeth took her children to safety in New Zealand, intending to return to Singapore with radar equipment from Sydney. But Singapore surrendered before her children were settled, and she was told her husband was dead, so she decided to remain in New Zealand. In April 1942 she was appointed head of the Operations Research Section of the Radio Development Laboratory[2] in the nation's capital, Wellington (Figure 2). At 33 years of age, she was considerably older than most of those who worked under her, one of whom later recalled: "She was much respected by us much younger folk, because of her experience of the wider world." (E.R. Collins, pers. comm., 1999). Elizabeth remained head of the Operations Research Section until the end of the War, while Norman Alexander (who in fact had survived) was interned in Changi.

Figure 2. The nondescript office building that housed the Radio Development Laboratory (courtesy: Mary Harris).

The Operational Research Section of the Radio Development Laboratory was involved in New Zealand's wartime radar development program (see Unwin, 1992; *World War II Narrative* .., 1948), and Elizabeth Alexander was responsible for the Section's own research radar unit on Mount Wellington, Wellington, where radar prototypes were operated and tested. She was particularly interested in studying propagational effects and developing fundamental theory so that radar performance could be predicted from meteorological data, and vice versa (Figure 3). Consequently, she became a key player in an ambitious US-British-Australian-New Zealand radio-meteorological project to investigate 'anomalous propagation', radiation that appeared to originate from over the horizon. Towards the end of the War, this would evolve into the 'Canterbury Project'. Another—but very different—instance of 'anomalous propagation' that she researched was the 'Norfolk Island Effect', which unwitting took her briefly into a field that would later become known as solar radio astronomy.

Figure 3: Elizabeth Alexander busy with her calculations (courtesy: Mary Harris).

At the end of the War, Norman Alexander joined Elizabeth and the children in New Zealand, but in March 1946 flew back to Singapore to help reopen Raffles College. Elizabeth and the children remained in Wellington, and in July 1946 sailed for England, where Norman joined them for a while. He was busy acquiring equipment for Raffles College, while Elizabeth wrote up some of her pre-war geological research. Then, in 1947, Norman and Elizabeth returned to Singapore, leaving the children with a relative in England—where they eventually went to boarding schools (Alexander, early 1990s; Mary Harris, pers. comm., 2005).

The next few years saw the evolution of Raffles College into the University of Malaya, with Elizabeth acting as Registrar during the transition period. She then became a geological consultant to various bodies, and in 1949 was appointed Geologist to the Government of Singapore, "… with a main task of surveying the island's resources of granite and other useful stone, and in 1950 published a report which included the first reasonably complete geological map of Singapore Island." (A.J.B., 1959: 140).

In 1952, the Alexanders moved to Ibadan, Nigeria, when Norman accepted the Chair of Physics at the University College. Elizabeth was appointed a Lecturer in Soil Science in the Department of Agriculture, and again began researching tropical weathering. When the University founded a Department of Geology, in 1958, she was promoted to Senior Lecturer and became Head of the Department (Alexander, early 1990s). After just three weeks in this new post she had a stroke, and died a little over one week later, on 15 October 1958; Frances Elizabeth Somerville Alexander was just two months short of her 50th birthday. She was fondly remembered for the "… warm welcome for those who dropped in to the Alexander household, and there are many who are grateful for the hours they have spent there, either quietly listening to Beethoven, or else to conversation in which above all an air of sanity prevailed." (Obituary, 1959: 5). Meanwhile, those who worked with in New Zealand would have recalled her "... sound training, unremitting energy, a remarkably co-operative personality, and high intellect …" (World War II Narrative …, 1948: 489). Currently, one of Elizabeth Alexander's daughters is preparing a biography of her illustrious mother (Mary Harris, pers. comm., 2002).

3. INVESTIGATING THE 'NORFOLK ISLAND EFFECT'

Between 27 March and 1 April 1945, a "very striking" increase in 'radio noise' was noted by the officer in charge of the Royal New Zealand Air Force 200 MHz COL radar unit located on Norfolk Island (see Figure 4 for radar stations mentioned in the text).[3] This enhancement was shown to originate from outside the radar antenna, turning gear and receiver, and only occurred within half an hour of the rising or setting of the Sun. Furthermore,

> The maximum increase of noise was on the bearing of the sun and rotation of the aerial showed noise fluctuations corresponding fairly closely to the radiation diagram of the aerial. At its maximum the noise reached saturation on the azimuth of the sun and peaks of noise were also observed on azimuths corresponding to the first and second pair of side lobes. Switching off the Transmitter had no effect on the noise … (Alexander, 1945d: 1).

Elizabeth Alexander was assigned to investigate this phenomenon, which was dubbed the ‘Norfolk Island Effect’.

Figure 4. The location of the five RNZAF radar stations involved in solar monitoring in 1945 (1 = Norfolk Island; 2 = North Cape; 3 = Whangaroa; 4 = Maunganui Bluff; and 5 = Piha).

Dr Alexander arranged for monitoring of the Sun to take place within an hour of sunrise and sunset at five different RNZAF radar stations: at Norfolk Island, and in the northern sector of the North Island of New Zealand (Figure 4). A contemporary photograph of the Whangaroa radar station is reproduced below in Figure 5. All five stations were instructed “… to record the increase in noise and the azimuth of maximum increase every few minutes and the time observations were taken … [together with] A general description of the weather at the time of taking the observations …” (Alexander, 1945d: 1).

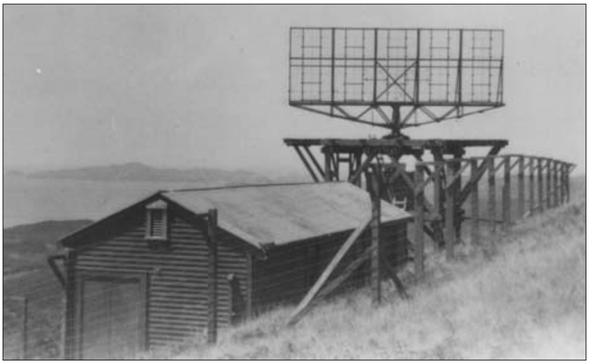

Figure 5. The Whangaroa Radar station, showing the 200 MHz broad-side array and associated technical building (courtesy: Gordon Burns).

The monitoring took place between 10 and 23 April, although few stations were able to continue with this project past April 18, and solar detections were recorded at all five stations. But, as Table 1 illustrates, even when all five stations were tracking the Sun, solar radio emission was never detected at more than three of these. It should be noted that the data in Table 1 for Piha are slightly misleading in that on four sunrise observing periods and one sunset observing period the radar antenna had to be used to track aircraft, so solar monitoring was not possible.

Table 1. Days when solar monitoring took place (dashed lines) and when solar radio emission was detected (crosses) at the different RNZAF radar stations

Radar Station	Date 1945 April														No of Days
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	
Norfolk Island			x –	– –	– x –	– –	– –	– –	– –	– –	– x –	– x –	– –	– x	5
North Cape		x –	– –	– x –	– –	– –	– –	—							2
Whangaroa			—	– –	– x –	– x –	– x –	– x –	– x						5
Maunganui Bluff		—	– x –	– –	– x –	– x –	– x –	– x							5
Piha	—	– –	– –	– x –	– –	– –	– –	– –	– x –	– x					3
No. of Stations	0	1	2	2	3	2	2	2	2	1	1	1	0	1	

Part of the reason for the inconsistent results indicated in Table 1 lay with the design of the COL radar antennas, which could only rotate in azimuth and detect solar radio emission as the Sun rose or set through the antenna beam. The output, meanwhile, was displayed as ‘grass’ on a cathode ray tube, and it was a matter of making a subjective assessment as to whether the amplitude of this ‘grass’ exhibited a meaningful increase. Because the increase was often marginal, this was a problem. In order to increase sensitivity—and provide some means of quantification—the Commanding Officer at the Whangaroa radar station decided to install a microammeter between the receiver output and the diode limiter, and “Immediately a change was apparent and results [were] obtained.” (Marsden, 1945: 1). Meter readings of ‘normal noise’ were taken either side of the Sun and as the radar antenna was slowly swept across the azimuth of the Sun, and up to five sweeps were made when solar noise was detected. Interestingly, “Where more than one complete sweep was taken the azimuth of the noise peaks could be seen to have drifted in the same direction as the changing azimuth of the sun.” (Alexander, 1945d: 3). Following this altered *modus operandi*, solar radio emission was detected at both sunrise and sunset on five successive days (see Table 1).

Elizabeth Alexander analysed the April observations made at the five radar stations, and concluded that “… at sunrise and sunset a detectable amount of noise over and above normal noise is received from a direction roughly that of the sun." (Alexander, 1945d: 4). She stressed that while the observations were crude "… they do seem to indicate that more energy is sometimes radiated from the sun on 200 Mc/s than would be expected on black body theory." (ibid.); in other words, the emission was non-thermal. And while the levels of solar noise seen at the New Zealand radar stations were so small that they might have been missed in the past, she noted that "At Norfolk Island, however, the increase was quite striking." (ibid.).

In a later, much shorter report, Elizabeth Alexander (1945c) noted that on 26 March 1945 'jamming' at the azimuth of the Sun was also detected for 15 to 35 minutes before sunset at two non-New Zealand radar stations. One of these was in the Lingayen Gulf in the Philippines and operated at 106.8 MHz, and the other was an Australian 200 MHz station sited on Montalivet Island in the Darwin area. She concluded that "In both cases the jamming was almost certainly due to Solar Radiation. Probably other stations [in the region] were affected but, as is frequently the case, the phenomenon was not recorded." (ibid.).

On the basis of the initial New Zealand results, Elizabeth Alexander planned an elaborate solar monitoring program for the second half of 1945, which would involve the original five Air Force radar stations, "… and any Army and Navy stations that can take the observations ..." (Alexander 1945d: 5), and all were to be supplied specially-designed vacuum tube voltmeters (to be inserted immediately after the second detectors in the radar receivers) and calibrated signal generators. The following observing procedure was adopted:

> … switch off the transmitter, turn the aerial about 90° from the sun where the noise is normal, connect the vacuum tube voltmeter and record the meter reading. The signal generator is then connected in and the attenuator adjusted until the vacuum tube voltmeter reading is a little above the normal noise reading. These meter readings and the attenuator reading are recorded. The signal generator is disconnected and the set is ready for noise observations.
>
> When an increase in noise voltage is observed, either of two procedures is adopted. Either the aerial is swept backwards and forwards across the sun's azimuth from normal noise through maximum to normal noise again and meter reading, azimuth and time is recorded every two degrees. Or the aerial is swung just sufficiently to determine the azimuth of maximum noise and meter reading,

azimuth and time is recorded every one or two minutes. These observations are to be carried out over the sun rise and sunset period daily operational requirements permitting. Weather observations, with particular reference to amount and position of cloud cover, are to be made for each set of measurements. (Alexander, 1945d: 6).

Unfortunately, two factors combined to prevent widespread adoption of this program: (1) an early, successful, outcome of the War was anticipated, so there was on-going reduction of staff at the various radar stations, but despite this (2) there was "… the necessity of keeping up operational watches [which] placed considerable obstacles in the way of a regular observation programme." (Millar, 1946a: 1). Nonetheless, the five original RNZAF radar stations were able to carry out some solar monitoring. The Norfolk Island station began observations on 24 July, but the other radar units were not in a position to join the program until September, and all continued through into December 1945 (Alexander, 1945c).

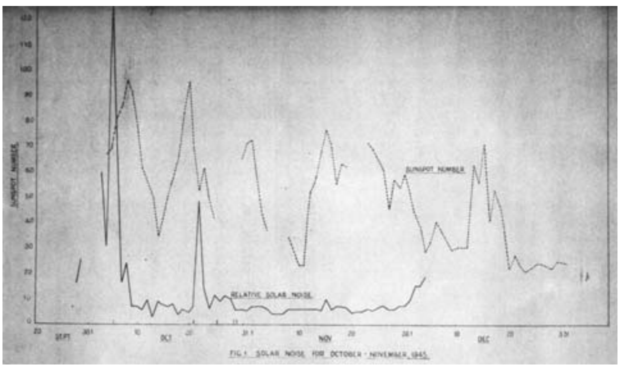

Figure 6. Plot of sunspot numbers, and 200 MHz solar noise recorded at the Norfolk Island and Piha radar stations, September–December 1945 (after Millar, 1946b: 5).

Sadly, this monitoring revealed just the one short-lived period when the Sun was particularly active at 200 MHz, centred on October 5 (Millar, 1946a) when "… violent surges of noise were observed at irregular intervals. These surges were of momentary duration and sent the noise meter needle hard over." (Alexander, 1945c). When this occurred, the officer in charge of the Piha radar station installed a simple Yagi aerial that could track the Sun, and although the gain was much less than that of the adjacent radar antenna, "… the noise could still be observed, its intensity remaining the same throughout the day whenever the aerial was directed towards the sun …" (Alexander 1946: 16). Elizabeth Alexander (1945c) also reported that "… The signals fluctuated rapidly but did not completely disappear until sunset." However, a plot of the solar noise observations made at this time (Figure 6) reveals that solar radio emission was also detected on October 21, and that both periods of solar activity correlated with enhancements in sunspot numbers. It is noteworthy that two later periods of higher than average sunspot numbers were not associated with 'solar noise'. Nonetheless, Elizabeth Alexander (ibid.) felt in a position to conclude: "Such evidence as we have so far in New Zealand points to a direct correlation between sunspot number and solar noise ... Though we have no absolute measure of the power received, there is strong evidence that during the periods of intense activity … Long wave Solar radiation is far removed from black body radiation." The October activity reinforced her view that she was again dealing with non-thermal emission.

At the end of 1945, most of the RNZAF radar stations were closed down, and at the end of January 1946 the Radio Development Laboratory was disbanded and most of the staff returned to their previous positions or to interrupted university studies (Galbreath, 1998; *World War II Narrative …*, 1948). But before Elizabeth Alexander and the children sailed for England in July 1946 (Mary Harris, pers. comm., 2002), she had one final radar-related task: to write up an account of her 1945 solar research project. This appeared as a 3-page paper titled "The Sun's radio energy" (Alexander, 1946), and reported the March-April and October activity. The association between sunspots and solar noise—first put forward in her brief December 1945 report—was repeated, and it was noted that both March-April and October 1945 were periods of notable sunspot activity. She also mentioned the non-thermal nature of the 200 MHz emission, but was premature in stating that "To deduce solar temperatures of millions of degrees from this radiation, as has been suggested in some press reports, is absurd." (Alexander, 1946: 20). Later that same year, Martyn (1946) and Pawsey (1946) published papers in *Nature* in which evidence of a coronal temperature of 1 million degrees was presented. In her paper, Elizabeth Alexander (1946) also discussed wartime observations of solar radio emission made by other researchers, in England, Australia and the USA; some of these projects were only undertaken after receipt of her reports on the "Norfolk Island Effect".

Elizabeth Alexander ends her 1946 paper by pointing to the need for further research, and suggesting a role for amateur astronomers and radio enthusiasts:

> What is required at the moment is more experimental evidence on the following aspects:–

> 1. The way the received power varies with wavelength, over the whole band of the shortest microwaves up to the largest wavelengths (15-metres) which will penetrate the ionosphere (it almost looks at present as if the power might be greater at longer wavelengths).
> 2. The variation, at all wavelengths, with angle of elevation of the sun. This will permit evaluation of atmospheric absorption.
> 3. Exact times of onset, cessation, or change in character. This will permit correlation either with visible changes in the sun, or with other associated phenomena, magnetic storms, etc., and might lead to methods of predicting radio fadeouts.
> 4. Seasonal fluctuations, and variation with geographical position, particularly latitude.
>
> In observations of this type, amateurs can play an important part. Anyone who cares to build an ultra-shortwave receiver can be fairly sure of collecting useful information. The time is appropriate, since the sun is just entering a new phase of activity, and sunspots may be expected with increasing frequency over the next few years. (Alexander, 1946: 20).

The tragedy is that Elizabeth Alexander's interesting little paper appeared in the inaugural issue of a New Zealand-based journal titled *Radio & Electronics*, which at the time had virtually no international visibility, and certain never came to the attention of radio astronomers—or indeed those researching the history of radio astronomy—until very recently. Nor did Elizabeth send out reprints (if indeed any were issued), or maintain a correspondence with colleagues she had met who were involved in the formative days of British and Australian radio astronomy. So her pioneering research was soon be forgotten, only to be rescued from obscurity when Woody Sullivan (1988) came upon archival reference to the 'Norfolk Island Effect' in the course of his research on the world-wide development of radio astronomy.

4. DISCUSSION

4.1 The Radiophysics Solar Program

On 1 August 1945, Elizabeth Alexander penned a letter to J.L. Pawsey at the Radiophysics Laboratory in Sydney, and included as an attachment a copy of her R.D. 1/518 report on the 'Norfolk Island Effect', which "… describes our present and proposed investigations …" (Alexander, 1945a).

Pawsey passed the letter and report on to the Chief of the Division, Dr Briton, and they were also seen and initialed by Deputy-Chief, Taffy Bowen, Frank Kerr, Lindsay McCready and Ruby Payne-Scott. The contents of Alexander's letter and report created considerable interest in Sydney, and were partly responsible for the launch of the Division's solar radio astronomy research program. After perusing this material, Taffy Bowen (as Acting Chief of the Division of Radiophysics) wrote to Dr Ernest Marsden in New Zealand: "We were very interested to hear about the radar observations … and will attempt to repeat them here in Sydney." However, this was not the only intelligence on solar radio emission to arrive at this time, for Ruby Payne-Scott (1945) reveals

> … the almost instantaneous arrival of three reports in the [Radiophysics] laboratory, one recording severe noise interference in the direction of the sun on G.L. stations (55 – 85 Mc/s.) on 27th and 28th February 1942, coinciding with the passage of a large sunspot, another reporting … [the] Norfolk Island [Effect] … and a recent report by Reber in which he mentioned that, using his equipment for plotting "cosmic static" on 160 Mc/s., he obtained considerable radiation when the aerial was pointed at the sun.

Payne-Scott's report (ibid.) also indicates that it was these three reports that collectively inspired her and two colleagues to conduct the first Australian search for solar radio emission.

These observations were made from 3 to 23 October 1945 using the 200 MHz COL radar antenna at the Royal Australian Air Force's radar station at Collaroy, a northern Sydney beachside suburb, and revealed the presence of solar radio emission. Moreover, variations in mean intensity were found to correlate with changes in the total area of visible sunspots. When they came to write up these observations for *Nature*, Pawsey, Payne-Scott and McCready (1946) specifically mentioned Elizabeth Alexander's work, and one of the four references listed at the end of the paper was her 1945 report.

The paper was completed on 23 October 1945 and immediately submitted to *Nature*, but by the time it appeared in the 9 February 1946 issue it had been foreshadowed by two other contributions. The first of these was penned by Sir Edward Appleton, dated 24 September 1945, and was published on 3 November. It discussed the evidence for non-thermal radio emission from the Sun (Appleton, 1945). The other paper, dating from 17 October 1945, was by H.S. Hey, and it appeared in the 12 January 1946 issue of *Nature* (Hey, 1946). This important contribution reported on 1942 detection of solar radio emission at radar stations in England, publication of which was only possible once that the war was over and the research no longer carried a classified 'tag'.

When quizzed by the Australians about the publication order of the three papers, and the lengthy delay in appearance of the Sydney paper, Appleton replied:

> I am sure your people were only anxious to acknowledge prior work … I have been much concerned with this solar noise myself and so know the history of it fully. Fortunately for us, my letter and Hey's letter to Nature preceded the Australian one, so no harm was done, as it turned out, so far as we are concerned; *though I feel rather sorry about the New Zealand people who were the next to conclude that it was really radio noise in their Norfolk Island experiments*. (Appleton, 1946; his underlining; my italics).

Despite these protestations of concern, it is telling that Appleton neglected to mention Elizabeth Alexander's research in his paper, even though he was fully aware of the New Zealand work and the non-thermal nature of the emission by mid-1945. More than this, at some time prior to 1 August 1945 Appleton claims to have even gone to the trouble of replicating the New Zealand observations (see Alexander, 1945a), yet he also fails to mention this additional evidence for non-thermal emission in his *Nature* paper. By this stage, Sir Edward was already beginning to gain a reputation for claiming credit for other people's work, and helping delay the publication of papers submitted to *Nature* by those he viewed as 'competitors' (e.g. see Bowen, 1985; Kerr, 1987), and at the time Taffy Bowen (1946) wrote Dr Frederick White at CSIRO Head Office: "I am sorry that Appleton is making a song and dance about our letter to "Nature", but I suppose he is just expressing his well-known "ownership" of all radio and ionospheric work." (Bowen, 1946).

After their initial observations at Collaroy, Pawsey and his Radiophysics colleagues decided to mount an even more ambitious solar monitoring program, extending from October 1945 through to March 1946. At first this involved the 200 MHz radar units at Collaroy and Dover Heights, but the limitations of sea interferometers soon prompted them to install 200 MHz steerable Yagi arrays at Collaroy, Dover Heights, the North Head radar station, and at Mount Stromlo Observatory. A few observations were also made at 75 and 3000 MHz. The results of this major investigation were published in the prestigious *Proceedings of the Royal Society* (McCready, Pawsey and Payne-Scott, 1947)—again after inordinate and inexplicable delays—and Elizabeth Alexander's work was once more referred to and referenced: the authors specifically state that "Observations on 200 Mcyc./sec. *similar to those of the New Zealand stations* were begun by us towards the end of 1945 …" (McCready et al., 1947: 358, my italics). McCready et al. were not only able to confirm the New Zealand findings, but to make important new contributions to solar radio astronomy, thus placing Australia's foray into this emerging discipline on a firm observational and theoretical footing (for further details, see Orchiston, Slee and Burman, 2005).

4.2 The New Zealand Sequel

One of Elizabeth Alexander's principal preoccupations in 1944-1945 was radio-meteorology, which led to the 'Canterbury Project'. This joint British-New Zealand research project was approved in 1945, but the end of the War delayed its launch as funding had to be re-negotiated under civilian peace-time conditions (see Alexander, 1945b). Given Elizabeth Alexander's relocation to England, the Canterbury Project came under the direction of one of her associates, Dr Bob Unwin, and in early 1947 an ex-WWII radar antenna and field trucks were set up at Wakanui Beach on the Canterbury coast, 85-km southwest of Christchurch (Figure 7). In October 1947 the field station was transferred to Ashburton Airport (Unwin, 1947). Although this equipment was intended for the study of radio propagation across the Canterbury Plains under varying meteorological conditions, Dr Unwin arranged for his staff to observe the Sun at 97.5 MHz for an hour and a half after sunrise and before sunset from March to December 1947, and "… a large number of solar bursts of short duration were detected. On many occasions these occurred when sunspots and other visual signs of solar activity were in evidence." (Orchiston, 1994: 68). The observations were forwarded to Ivan Thomsen at the Carter Observatory in Wellington, and it is to be regretted that he never published the results of this interesting study.

On 23 November 1945 Elizabeth Alexander (1945b) warned Joe Pawsey that New Zealand's solar radio astronomical future looked bleak after the close-down on the COL radar stations at the end of the year: "I doubt that New Zealand will be able to put sufficient effort into building aerials adequate to investigate the phenomenon. It is a large scale job, if it is to be done properly, and the Canterbury Project is taking all available men and cash." This forecast proved remarkably accurate, and the part-time excursion into solar radio astronomy during the Canterbury Project was all that staff from the DSIR were able to attempt. Unlike in Australia, where many of the staff at the Radiophysics Laboratory were retained after the war (and became the nucleus of the Sydney radio astronomy group), most of those in New Zealand's Radio Development Laboratory left the DSIR; and although Marsden succeeded in forming a Radar Section within the Dominion Physical Laboratory on 21 February 1946 (*World War II Narrative* …, 1948), this began with a staff of just 13 (Atkinson, 1976: 66) and would always be a small-scale operation with limited funding and research capability. Within post-War Government-funded science, there simply was no place for radio astronomy in New Zealand, and it was left to other institutions and individuals to progress this discipline. Those that were involved in solar radio astronomy prior to 1950 are discussed below.

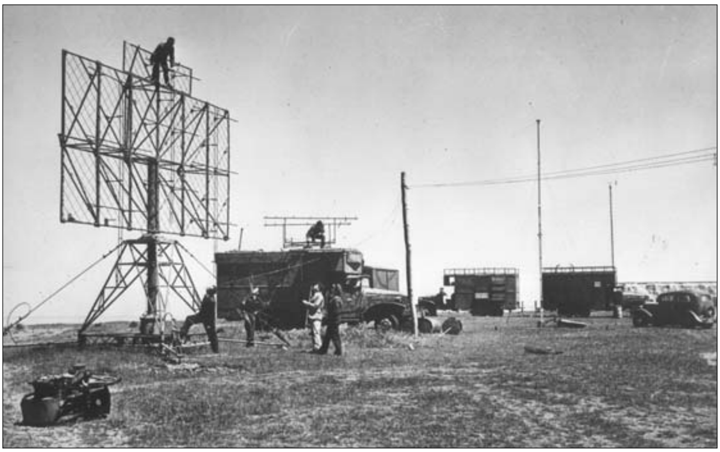

Figure 7. Installation of the 'Canterbury Project' 97.5 MHz radar antenna at Wakanui Beach near Ashburton. In the background are associated equipment trucks (courtesy: Bon Unwin).

While the Canterbury Project was in progress, a student named Alan Maxwell was engaged in a solar radio astronomy project for his M.Sc. degree at what is now the University of Auckland (see Maxwell, 1948). His supervisor was Dr K.S. Kreielsheimer, and he also found ready support from Professor P.W. Burbidge, both of whom were interested in astronomy, radiophysics and upper atmospheric physics. In mid-1947 Maxwell erected twin Yagis on the roof of the Biology Department (Figure 8), and tracked the Sun at 100 MHz for the remainder of the year and into the second half of 1948. Maxwell (1948: 82) found that "In general, when solar noise was received there were sunspots on or near the sun's meridian." Of special interest was "… a period of solar activity between 1948 August 5-9, when there were numerous small-scale bursts of radio noise." (Orchiston, 1994: 69), and "On at least two days an indication of a general solar noise background was noticed by pointing the array into and away from the sun … [Furthermore] A rough correlation of bursts with those observed in Canterbury [at Ashburton] has been established on several occasions." (Burbidge and Kreielsheimer, 1947). Despite this being one of the first post-graduate theses on solar radio astronomy ever written anywhere in the world, Maxwell also failed to publish his work—it simply was not the custom at this time—and soon after completing his studies he moved to the dynamic astronomical environment of Jodrell Bank (at the University of Manchester) where he was quickly immersed in new research for a Ph.D. (Alan Maxwell, pers. comm., 1993). Later he would go on to build an international reputation in solar radio astronomy while at Harvard College Observatory and the Radio Astronomy Station at Fort Davis, Texas.

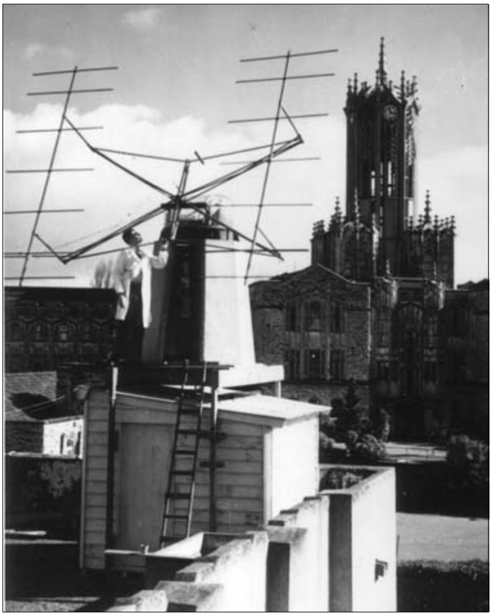

Figure 8. Alan Maxwell adjusting the 100 MHz twin Yagi antenna set up on the roof of the Biology Building at Auckland University College in 1947. The small hut below that antenna and mounting housed the receiver (courtesy: Alan Maxwell).

Carter Observatory in Wellington specialized in optical solar monitoring, and from the start its Director, Ivan Thomsen, was vitally interested in the projects carried out by Elizabeth Alexander and Alan Maxwell and supplied both with relevant optical data. While his involvement in the solar radio monitoring associated with the Canterbury Project was disappointing (in that no results were published), he did in fact publish one paper pertaining to solar radio astronomy. Thomsen (Co-ordination …, 1947) was particularly interested in the relationship between solar emission, sunspots and solar-terrestrial effects—such a short-wave radio fadeouts—and the 24 January 1948 issue of *Nature* features a paper where he compares Ryle and Vonberg's (1947) radio data for December 1946-April 1947 with sunspot records and finds "… a surprisingly general agreement." (Thomsen, 1948: 134). Looking more closely, when the general level of radio emission for February-March 1947 was plotted against the position of photospheric features recorded at the Carter Observatory (Figure 9), "… it was nearly always possible to ascribe some significant sunspot group to each of the maxima of the [radio] curve …" (Thomsen, 1948: 134-135). Furthermore, the radio emission tended to coincide with the central meridian passage of the associated spot group, and "… in general, groups in the early stages of vigorous development, or showing activity by large umbral movements and changes and accompanied by flares, give the greatest emission." (Thomsen, 1948: 135).

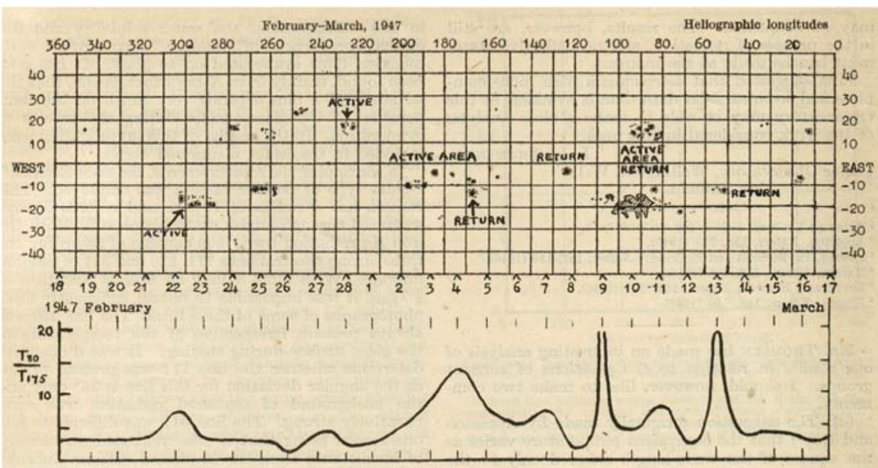

Figure 9. Plot of solar radio emission in February-March 1947 and associated photospheric features (after Thomsen, 1948: 135).

Surprisingly, New Zealand can boast a fourth solar radio astronomy project dating to the immediate post-War years. Perhaps inspired by Elizabeth Alexander's 1946 'call to arms', Kaiapoi's Robert Francis Joyce constructed a corner reflector that could track the Sun, obtained an ex-WWII radar receiver, and from mid-1949 carried out solar monitoring at 515 MHz, sending his records to Ivan Thomsen at the Carter Observatory. By avocation, Joyce was a well-known amateur astronomer, whose Neptune Observatory boasted a much-used 11.4-cm (4.5-in) Wray refractor, and he was an ardent astronomical photographer (see Howell, 1967; Murray Geddes …, 1956). By vocation he ran a radio manufacturing and repair business, so he was in an ideal position to tap into the newly-emerging field of solar radio astronomy.

5. CONCLUDING REMARKS

New Zealand was merely one of several countries involved in the secret investigation of solar radio emission during WWII, and the person responsible for this Antipodean research was a British-born scientist, Dr Elizabeth Alexander. Rare amongst New Zealand-based scientists at the time, she was a woman, was married with a young family, held a senior post in the Department of Scientific and Industrial Research, and therefore moved in the upper echelons of New Zealand science (see Figure 10). Being somewhat older than most of her colleagues, she had a wealth of research experience to fall back upon when investigating the 'Norfolk Island Effect', and it is fortunate that her academic training as a geologist at Cambridge included units in physics and mathematics.

Figure 10. In September 1944 Dr Elizabeth Alexander poses with (left to right) Dr Ernest Marsden (Director of Scientific Developments, DSIR), Sir Cyril Newall (the Governor General of New Zealand), Dr Ian Stevenson (Director of the Radio Development Laboratory) and the Governor General's Aide de Camp (courtesy: Professor E.R. Collins).

As it turned out, the 'Norfolk Island' project was but a momentary diversion in a long and eventful career dominated by geology and academia, but it was a significant diversion nonetheless, for by being in the right place at the right time she ended up being the first women to conduct research in the fledging new discipline of 'radio astronomy' (although it would take another five years of intensive research—mainly in England and Australia—before this term would begin to find common usage).

Her solar work in 1945 was the catalyst that led to several other New Zealand research projects in solar radio astronomy in the immediate post-War years, yet unlike in neighbouring Australia, this discipline did not succeed in gaining a sustained foothold in New Zealand at this time. Had Elizabeth Alexander remained in New Zealand after the war and been able to pursue her interests in radio astronomy and radio-meteorology—instead of returning to geology and academia in Singapore—then the situation may very well have been quite different.

Nonetheless, Elizabeth Alexander was a remarkable woman, and her place in the annals of radio astronomy deserves to be fully recognized and applauded.

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7. NOTES

1. Short, semi-popular accounts of the Elizabeth Alexander's research are presented in Orchiston, 1994; Orchiston and Slee, 2002: 25-26; and Sullivan 1988: 316.
2. The Radio Development Laboratory was a Division of the Government's Department of Scientific and Industrial Research, and was set up in 1941 under Dr Owen Pulley with an initial staff of ~100. Based in Wellington (see Figure 2), it had branches in Auckland and Christchurch. When Pulley returned to Australia in 1942, C.N.M. Watson-Munro became Director of the Laboratory (for details, see Galbreath, 1998). According to Elizabeth Alexander's husband, Norman (later Sir Norman), her appointment was partly facilitated through 'the old-boy network', as they both knew Pulley, and people like Taffy Bowen, Joe Pawsey and Fred White who played key roles in the development of radar in Australia (Alexander, early 1990s).
3. At an approximate longitude of 168° E and latitude of 29° S, Norfolk Island is located about 1,400 km east of Brisbane and 750 km northwest of the northern-most tip of New Zealand's North Island. Although considerably closer to New Zealand, it is Australian territory. When the USA set up separate command areas in the Pacific in 1941 Norfolk Island fell within the New Zealand (South Pacific) command rather than the Australian (Southwest Pacific) command, and this explains why a Royal New Zealand Air Force radar station was established there. The radar station was located "… at a height of 1,000 feet near the north western corner of Norfolk Island … [with] an unobstructed view all round …" Alexander (1945d: 2).

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RADIO ASTRONOMY IN HOLLAND BEFORE 1960:
Just a Bit More Than HI

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Abstract: The history of early radio astronomy in the Netherlands is explored, especially that which did not involve neutral hydrogen studies. Although little of this early—mainly solar—work was published in professional journals, there is some information in a popular Dutch astronomy magazine. From this it is clear that the early radio observations of the Sun were driven as much by the needs of radio communication as by solar physics. The important role which A.H. de Voogt, Head of the PTT's radio service, played in both Dutch and international radio astronomy is described. A brief sketch is given of the history of the two PTT stations where the early observations were made.

Key words: history of radio astronomy, solar radio astronomy, Holland, Kootwijk

1. INTRODUCTION

The birth of radio astronomy in the Netherlands can be rather precisely dated to 15 April 1944. The exact location is also known: the old Observatory on the Sterrewachtlaan in Leiden. As might be expected for such an event, there were a fair number of doctors in attendance, although curiously quite a few other spectators also managed to slip in. What they witnessed would have a significance that probably no more than a handful could fathom that day, but their thoughts on the matter have gone largely unrecorded. Once the business was done, most would have quietly gone on their way—it was wartime and the country was occupied, and liberation was over twelve months away. Even a birth announcement would not be issued until the following year.

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That spring day in Leiden, the Nederlandse Astronomenclub (Dutch Astronomy Club) held one of its regular meetings, and the two speakers, C.J. Bakker and H.C. van de Hulst, had the task of explaining how radio signals from space can be detected, and whether the radiation might have a spectral signature. Unusually for such a technical subject, there was as much emphasis on theory as practical implementation. Bakker (from the Philips Physics Laboratory) explained how radio receivers and antennas work, while Van de Hulst presented the results of his investigation into atomic quantum transitions at microwave frequencies (Bakker and Van de Hulst, 1945). In particular, the ground state of atomic hydrogen has two levels whose energy difference corresponds to radiation with a wavelength near 21 cm. This discovery would set the agenda for generations of radio astronomers in Holland and elsewhere. Yet in 1944, few would have predicted the active participation of the war-drained land, for as Oort (1952: 115) would later note: "The circumstances were unfavorable in that we had neither great quantities of equipment from the war as in Australia and England, nor a large contingent of radar-trained researchers." (English translation of Dutch original). How, nonetheless, the newborn would survive and flourish makes a fascinating story.

2. RADIO EMISSION FROM SUN AND MILKY WAY

The above title is a near-literal translation of the original Dutch name (Stichting Radiostraling van Zon en Melkweg, or SRZM) of what would be internationally known as the Netherlands Foundation for Radio Astronomy (NFRA). Its purpose, as the title suggests, was to research both solar and galactic radio emissions. This it did, though for reasons upon which I will speculate at the end, the galactic—HI—efforts would come to dominate.

2.1 A Brief Sketch of the Early HI Research

Before any birth there must be conception and a gestation period, and so it was also with Dutch radio astronomy. After the occupation began in May 1940, Bart Bok at Harvard set up a network to ensure that astronomy journals from the US would reach Holland. In one of the early contraband issues, Jan Oort read Reber's (1940) account of radio emission from the Galaxy and realized that this long-wavelength radiation could penetrate the light-absorbing dust in the galactic plane. He further reasoned that it probably had a featureless continuum spectrum, and here the concept came to him. Might there be some form of line radiation at these long wavelengths?

It was rather natural that Oort should turn to the student Henk van de Hulst with his question, for the two were already working together on the topic of interstellar 'smoke' (dust), and the young man was well-equipped to tackle this new problem. His investigation would lead to the prediction that the hyperfine ground-state transition at 1420.4 MHz of neutral hydrogen from interstellar clouds might be detectable, culminating in that April Leiden Astronomenclub meeting. Once the war was over, Oort set about organizing the new field of research. Working with Minnaert and others, SRZM was set up in 1948. The first task was to detect the 21 cm HI line, and a German radar dish was acquired for the purpose. After an inauspicious start (a fire which destroyed much of the equipment, and led to the departure of the first receiver developer), a recently-graduated radio engineer, Lex Muller, took on the task of building a suitable amplifier and detector system.

Neutral hydrogen emission from space was first detected at Harvard (Ewen and Purcell, 1951), with Dutch success some six weeks later (Muller and Oort, 1951). But the detection, which would be rounded off by a second colloquium co-sponsored by the Astronomenclub and held in Utrecht late that year (Oort et al., 1952), marked only the end of phase one of Oort's grand plan to use radio to understand the Galaxy. There followed complete mapping of the Milky Way visible from Holland, with steadily improving sensitivity as Muller refined his receiver design (and better electronic components came along). This resulted, among other things, in Maarten Schmidt's (1956) kinematic model of the Galaxy. The data were combined with Australian measurements to produce the Leiden-Sydney map of the Milky Way, and with the purpose-built Dwingeloo 25-m telescope ready in 1955, higher angular resolution and greater sensitivity became available. The achievements in less than a decade were impressive.

2.2 Radio Research to 1955 Which Did Not Involve HI

Outside of the HI studies there did not, on the face of it, seem to be much to report. There were talks at the 1951 colloquium, on radio emission from the Sun: quiet (Houtgast, 1952) and disturbed (Minnaert, 1952); on the relationship between radio phenomena in the Sun and the ionosphere (De Voogt, 1952b); and on continuum radiation from the Milky Way (Van de Hulst, 1952). Beyond that, Westerhout and Oort (1951) compared radio continuum emission from the Galaxy with models of its structure. And

Seeger (1955) reported on 400 MHz emission from the Sun during a partial solar eclipse. Two other papers—one on galactic structure, the other solar photometry—make passing references to the radio continuum, but that is all there is in the professional literature.

But there is another source of published information, an amateur magazine called *Hemel en Dampkring* (*H&D*, or "Sky and Atmosphere"; it included meteorology), which provides additional insight into what was being planned and happening in Dutch astronomy. Even as early as 1941, there was an article by D. Koelbloed describing Jansky's initial discoveries, and the work that Reber reported in his 1940 research paper (including a photograph of his radio telescope, taken from that paper). Who, we may wonder, provided the inspiration for this? Oort? Koelbloed himself? The editor? It shows, in any event, that early Dutch interest in radio astronomy was not limited to a small circle, even though Koelbloed, who would later become a Professor of Astronomy in Amsterdam, never pursued research in radio astronomy.

Table 1. Hemel en Dampkring radio astronomy titles (English translations), 1940–1952

Title (and description)	Author	Year
Cosmic radio waves (Jansky's discovery, Reber's 1940 article)	D. Koelbloed	1941
Radio waves from interstellar space (1944 Leiden meeting)	J.J. Raimond	1946
Sun and ionosphere (see §2.2)	J. Houtgast	1946
A fierce solar 'outburst' (flare radio emission, 3 Mar 1946)	J.J. Raimond	1948
Radio emission from the Sun	J. Houtgast	1949
The temperature of the Moon (Piddington & Minnet detection)	J.J. Raimond	1950
Sydney radio observatory, I. Point sources or radio stars, II. The Milky Way, III. The Sun, IV. The Moon	H.C. van de Hulst	1951
Radio emission from HI in interstellar space (reports 21 cm line discovery)	J.J. Raimond	1951
Radio emission from distant hydrogen clouds	J.J. Raimond	1952
The nature of Dutch radio astronomy research and some results (see §2.2)	A.H. de Voogt	1952

From 1946 to 1952 another nine radio articles would appear in *H&D*, half of them authored by its editor J.J. Raimond (whose son, Ernst, would later become a noted radio astronomer). An overview is given in Table 1. Interesting from the viewpoint of Dutch research which did not involve HI are the first of two articles by Houtgast and the last one by De Voogt. The former describes in a general way how the Sun's emission affects the ionosphere, which in its turn influences radio propagation on the Earth. But it is mainly a plea for greater co-operation among geophysicists (ionosphere, geomagnetism), solar physicists (activity on, and emission from, the Sun) and radio engineers (transmitters, receivers, for ionospheric monitoring, communication). Noting that the Netherlands has a special interest in such research, not only from a scientific perspective but also because of its overseas territories and extensive sea and air transport, Houtgast proposed the creation of a new institute for solar and ionospheric research, with observing stations at home and in the colonies. A note added in proof announced commencement of co-operation between Utrecht Observatory, the Meteorological Institute, and the Radio Laboratory of the PTT.

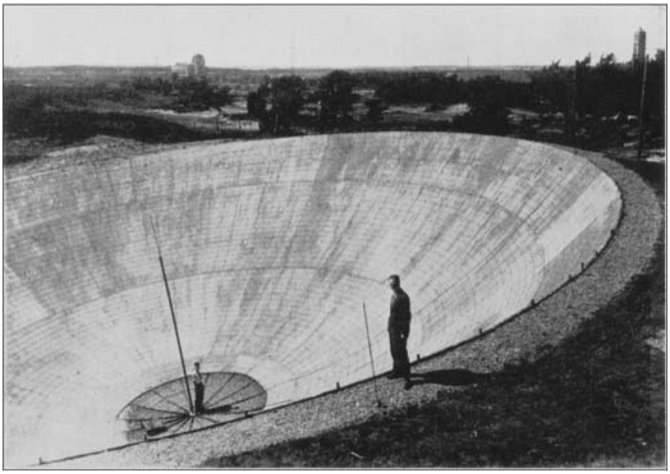

Figure 1. The 30-m 'kuil' (pit) antenna at Kootwijk. Hugo van Woerden (pers. comm., 2004) has pointed out that he is probably the figure standing on the rim. Note the profile of Gebouw A on the horizon, left of center (after De Voogt, 1952a: 212).

It was with the PTT (Post, Telephone, Telegraph = the Post Office) that the author of the second piece was associated, and the work he described bears a striking similarity to the sort of research Houtgast espoused six years earlier. Whether there was a causal relationship is unclear. Despite its title, De Voogt's article covers little more than radio monitoring of the Sun and the ionosphere, but it does provide insight into his motivation and plans at the time. He begins by noting that solar activity influences the ionosphere, and thereby radio communication. He then describes a network—a "worldwide-survey"—to monitor the Sun twenty-four hours a day, which began observations in January 1951 with eight stations, including one in the Netherlands. Each instrument observes the Sun daily on agreed frequencies, but he notes that not all instruments are able to observe in a set of uniformly-chosen bands. The Dutch observations were made using a 7.5-m Würzburg antenna operating at 140 and 200 MHz, and an example is shown of a radio outburst, together with evidence of its influence on the ionosphere. At night the instrument was used to monitor sky emission in the hope of determining the ionospheric electron content. However, the attenuation at 140 and 200 MHz being rather small, De Voogt describes a newly-built 30-m parabolic antenna (Figure 1), dug out of a natural depression and operating at 50 MHz (which, at the time, was the largest single antenna built for radio astronomy in the Netherlands and one of the largest in the world). Examples of observations are shown, and a 21 cm receiver is said to be under construction. The hydrogen-line work is briefly mentioned in passing (with reference in a general way to other *H&D* articles on the topic).

3. WHO WAS A.H. DE VOOGT AND WHAT WAS HIS ROLE?

Ir A.H. de Voogt[1] was a radio engineer and, at the time he wrote his article, he held the position of Head of the radio service of the PTT. For its radio communication in the 1950s, the Dutch PTT operated two stations. The receivers were at Nederhorst den Berg, just outside Amsterdam, a facility which had moved there from the coastal village of Noordwijk in 1950. The transmitting station was established in 1920 at Kootwijk, to the west of Apeldoorn. These installations were built and used for communication with the colonies in the Caribbean and the Indonesian archipelago, but the link was sometimes adversely affected by ionospheric conditions. De Voogt clearly hoped that radio monitoring of the Sun would provide early warning of a disturbed ionosphere, and was apparently able to convince his PTT superiors of its possible utility. Shortly after the war, he took the initiative to save several abandoned Würzburg radar dishes from the scrap heap, and had them moved to both Nederhorst den Berg and Kootwijk so that they could be used to carry out research. It is possible that this was a result of the co-operation with Utrecht mentioned by Houtgast in 1946, although many initiatives appear to have come from De Voogt himself (indeed, in a talk in 1968, C.A. Muller refers to the "pioneering work" of De Voogt's group after the war).

De Voogt was probably the PTT interlocutor in the discussions that led to the foundation of SRZM. Oort (1952) notes that among several bodies helping to lead the organization were "… the Radio Service and Radio Laboratory of the P.T.T." De Voogt was, in this capacity, a member of the board of SRZM, and in 1948 he decided to lend it one of the Würzburgs in Kootwijk to search for, and study, the 21 cm hydrogen line. The PTT would host this astronomical venture until the Dwingeloo Observatory was established in 1955. And active solar research would continue at NERA (Nederhorst den Berg Radio Astronomy) through the International Geophysical Year (IGY) at the time of solar maximum in 1957-1958, and into the early 1960s.

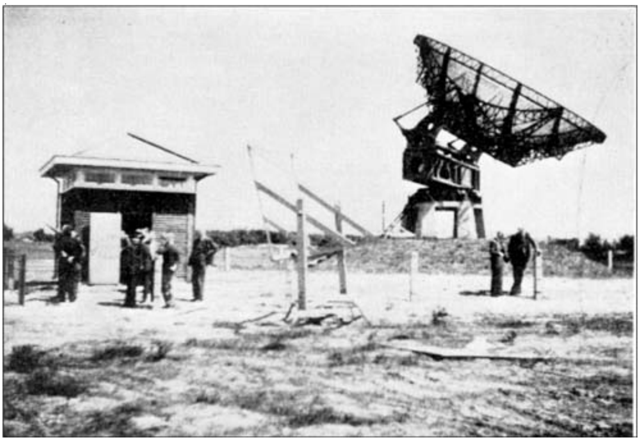

Figure 2. Würzburg 7.5-m antenna and corner reflector at Kootwijk in 1949. The photograph was taken during a visit of members of SRZM and of the workgroup 'Sun and Ionosphere', and judging from the shadows the two antennas may have been observing the Sun (after Houtgast, 1949: 186).

According to A.D. Fokker (pers. comm., 2004), De Voogt had a personal amateur interest in astronomy, and this may have been a factor in his favourable disposition toward the discipline. Most of the initiatives for the early work emanated from De Voogt himself. As early as 1948, solar observations were being made in Kootwijk (Figure 2), and he set up observing stations in Paramaribo and Hollandia (New Guinea) which became part of the "worldwide-survey" of the Sun. NERA can also be traced to De Voogt's effort, and he organized IRA (Ionospheric research and Radio Astronomy), which would for many years publish bulletins of Dutch solar observations. He may have also been deeply involved in the initial solar observations in Utrecht (which began at least as early as 1948, see Figure 3), although this is uncertain.

De Voogt also played a significant role internationally. His involvement in the worldwide solar survey has already been noted, wherein Holland collected and disseminated information on solar activity for the members of the network, in addition to making observations. De Voogt was also active in URSI (the International Union of Radio Science), chairing one of the sub-commissions (Va) and playing a leading role in the area of protecting radio astronomy from harmful interference. This may have stemmed from his appreciation of the need for the solar network to observe in agreed frequency bands, coupled with the HI effort in Kootwijk and the problems it faced in being in close proximity to powerful transmitters. In 1952, the then President of URSI, Sir Edward Appleton, noted that "… de Voogt … has drawn the attention of CCIR to the need for reserving a frequency band centred on [21 cm] free from radiocommunication traffic." By 1957 he was a member of Sub-Commission Ve on Frequency Allocation.

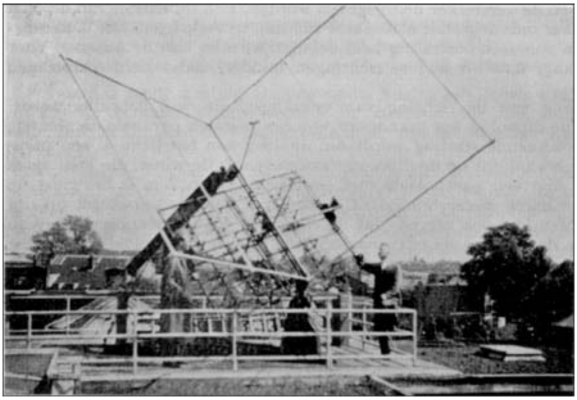

Figure 3. Corner reflector for solar observations at 4 m wavelength, Utrecht Observatory, 1948 (after Houtgast, 1949: 186).

3.1 Radio Kootwijk

The transmitting station at Kootwijk dates back to the 1920s, when it was used for maintaining contact with the Dutch colonies. By 1933 voiced transmission was possible, and the radio telephone became a popular means of communication, as advertisements from the period show (Figure 4). After the War there were at least three (and possibly four) Würzburg antennas in Kootwijk, as well as smaller instruments like the corner reflector (Figure 2) which were probably built by De Voogt's group. Certainly the 'kuil' (pit) antenna (Figure 1) was homemade, built in 1951. An interesting question is, where did the idea originate? It was very similar to the Dover Heights 'hole-in-the-ground' antenna which was built near Sydney in 1951 (see Orchiston and Slee, 2002), but this was constructed in secret (initially, at least) and did not make use of a natural depression, so it is unlikely to have been the inspiration. More likely, the idea of a fixed reflector looking directly upwards could have come from the Jodrell Bank 218-ft above-ground parabola, which was built in 1948. However, it is not unlikely that the ideas originated more or less independently of each other. The Kootwijk reflector, which was pointed slightly to the south so Cyg A would pass through the beam each day, was intended for use at 6 m wavelength, in the hope of monitoring the ionosphere through its absorption of background sky emission. In his *Hemel en Dampkring* article listed in Table 1, De Voogt (1952a) shows several scans through the Milky Way, and also mentions a plan to install a 21 cm receiver system.

By the mid-1950s, the HI work had moved to Dwingeloo, and about that time several of the Würzburgs were transported to NERA, where they were used for solar research for some years. Two can still be seen in the Netherlands, one in a museum, and the other at an observatory for amateur astronomy (where it still observes the Sun). Sadly, the original 'HI Würzburg' cannot be traced, and was probably scrapped sometime between 1955 and 1963. The 'kuil' antenna was eventually filled in and no longer exists, and the radio transmitters at Kootwijk have since gone silent. There are still several landmarks, however. The monumental main edifice, 'Gebouw A' (a prosaic name: Building A; see Figure 5), and recently was saved from the wreckers' ball. It is an historical monument, designed by J.M. Luthmann and completed in 1923, in a style inspired by the Finnish architect Eliel Saarinen.

Figure 4. 'State radio telephone service' advertisements from the 1930s: *Talk with Java*, and *Cheap family calls on Saturday afternoon* (reproduced from the website 'Radio Kootwijk' with the permission of the webmaster).

Of all the radio astronomical research done at Kootwijk, the HI work certainly had the most lasting influence (especially if one goes by the published record). Why was this? The hostile interference climate that has already been alluded to must have played a major role. Even at 21 cm it was a problem ("you learned to shield everything" was one comment), and the 'HI people' were more than happy to move north. One can only wonder how trying it must have been to observe with the 'kuil' antenna at 6 m wavelength. This was a major reason for the limited success of the Kootwijk solar instruments, and the reason for the move to NERA where the Sun was already being studied intensively.

Figure 5. Gebouw A, the monumental main building at Radio Kootwijk (reproduced from the website 'Radio Kootwijk' with the permission of the webmaster).

3.2 NERA

In the relatively benign radio climate of a receiving station, solar work flourished throughout the 1950s. Much of it was devoted to studying the radio emission from active regions, and participation in the IGY would have suited the research effort. From merely monitoring solar radio outbursts on a variety of wavelengths, attention turned to determining whether they were physically associated with a particular active region (and which one). This required higher angular resolution than the parabolic antennas could provide, so interferometers were also built and employed (Figure 6). In addition to this simple two-element interferometer (baseline ≈ 100λ) operating at 254 MHz, there were two 7.5-m Würzburgs and a 10-m parabolic reflector built by the PTT and placed on a Würzburg mounting. These three dishes were situated on an east-west baseline, and could also be used as an interferometer.

To get a better understanding of the astrophysics of solar outbursts, mere monitoring of the temporal variations over a wide frequency range proved insufficient. The group soon turned to measuring the polarization state of the emission as well. In addition, a series of multi-channel receivers would be designed, built and operated by the team, in order to study the fine-scale structure of the radio outbursts with good frequency resolution. Some of this work appeared in the mainstream astronomical literature (e.g. Cohen and Fokker, 1959), but much of it was published in IRA Reports. Results also appeared in a series of Ph.D. theses, including those by A.D. Fokker, T. de Groot and F. van ‘t Veer in the 1960s.

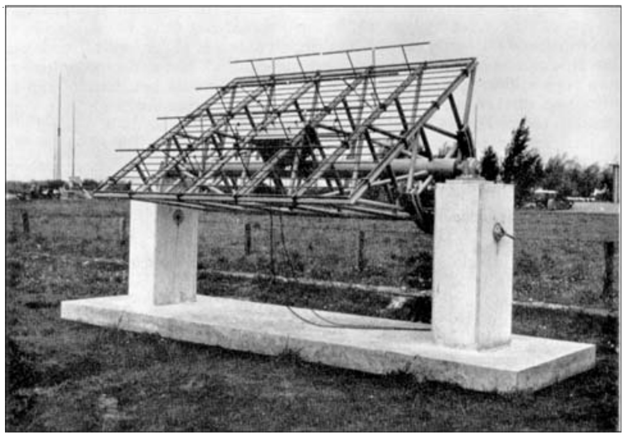

Figure 6. One of the NERA 254 MHz interferometer elements for solar observations (after Fokker, 1959: 142).

4. CONCLUSIONS

The low profile of non-HI research throughout the 1950s would slowly but steadily change in the following decades. In addition to the solar work at NERA, the arrival of the Dwingeloo 25-m telescope would benefit not only the hydrogen effort. Westerhout (1958) would carry out his survey of compact sources in the galactic plane. Studies began of the background continuum, and in particular of its linear polarization. There was also a survey of discrete continuum sources in the northern sky (Davis, 1967). The end of the 1960s saw the arrival of the Westerbork synthesis array, the original purpose of which was to look for faint (distant) discrete sources, though it would later do both continuum and line research (as it still does).

But turning again to the 1950s, why does the HI research appear to have been so much more successful than the rest, in particular that of the Sun? To begin with the latter, the interference problems at Kootwijk mentioned above seem to have been overcome by the transfer to NERA, so that cannot have been the reason. In fact, there were probably a number of reasons, so here are a couple which I suspect played a crucial role. The original motivation included both pure science (study solar physical processes) and a technological application (predict when solar activity will affect radio communication).

The mere phenomenology of solar radio outbursts may not have been sufficient to predict their likely effect on radio transmissions, let alone indicate what action to take. At the same time, communication technology began to change rapidly. A fundamental limitation of low radio frequencies is their inherently small bandwidths. This would change radically with high-frequency microwave links over land, increased numbers of trans-oceanic cables, and after 1957 the possibility of satellite communication. (Politically, Indonesia's independence might have slightly diminished the need for voiced transmission between Holland and that part of the world.) So I suspect the importance of the communication aspect rapidly declined.

But even if the technological motivation had still been there, the scientific problem was a complex one involving the magneto-hydrodynamics of solar active regions, propagation of waves and particles into the corona, their transmission through the solar wind, passage through the geomagnetic bow shock, to final deposition in the ionosphere. In effect, one had to understand the physics of several coupled plasmas, and it must have been quickly apparent that this was more likely to take decades rather than a few years. I think that the Sun was essentially too tough a nut to crack on the time-scales over which company boards expect results.

In the case of the HI, the work was entirely driven by the astrophysics. While there were many complex factors from the viewpoint of pure physics, there were three simple facts: neutral hydrogen is ubiquitous in the Galactic Plane; the only significant shift in the 21 cm line's wavelength is caused by the Doppler effect; and the Galaxy is essentially transparent to 21 cm radiation. From the standpoint of studying the kinematics of the Milky Way, the problem was straightforward—challenging to be sure—but the goal was clear and achievable. It would of course require many months of observation, and the effort of quite a few people, but it is perhaps not too much of an exaggeration to say that in this respect, our Galaxy is simpler than the Sun.

And of course the HI work had behind it the driving force of Jan Oort. It was his vision which led the effort from the start, and he had clearly laid-out plans for both instrumentation and the astronomy. As sensitivity and resolution improved, he split the Galaxy into sections, assigning each region to a Ph.D. project. Here he must have borrowed a leaf from the book of the man who inspired him to become an astronomer, his teacher Jacobus Kapteyn.

Finally, Oort was quite prepared to rely upon the skills and talent of engineers like De Voogt when technical challenges arose. As he noted at the 1951 symposium (Oort, 1952: 115) which followed the detection of the 21 cm line, "That we have … succeeded in getting radio research well underway is largely thanks to the cooperation of all interested groups with their diverse viewpoints." (English translation of the Dutch original), which is surely a fitting summary of those early years.

5. ACKNOWLEDGEMENTS

I am grateful to Aad Fokker, Arie Hin, Wim Brouw, Hugo van Woerden and Ernst Raimond for suggestions and discussions on the early days of Dutch radio astronomy. I thank the Leids Kerkhoven-Bosscha Fonds for a travel grant. I also thank the webmaster of the site "Radio Kootwijk" (www.radiokootwijk.com) for permission to reproduce the images in Figures 4 and 5. ASTRON receives financial support from the Netherlands Organisation for Scientific Research (NWO).

6. NOTES

1. Ir (= *Ingenieur*) was the engineer's title awarded by Dutch 'technical high schools' or institutes of technology (which were renamed 'technical universities' some years ago).

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JODRELL BANK AND THE METEOR VELOCITY CONTROVERSY

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Abstract: The initial impetus for the creation of the Jodrell Bank Experimental Station (now named Jodrell Bank Observatory) was the search for radar echoes from high-energy cosmic rays. But almost immediately, attention was directed toward echoes from meteor trails in the upper atmosphere. This paper details the meteor researches carried out at Jodrell Bank during its first five years of existence and pays particular attention to the meteor velocity controversy, which was eventually solved by radar observations.

Key words: History and philosophy of astronomy, radar astronomy, meteors, meteoroids, Jodrell Bank

1. THE ORIGINS OF JODRELL BANK

The early history of Jodrell Bank has been documented elsewhere by Lovell (1968, 1990). Its inception was the result of Bernard Lovell's work in radar research during World War II (see Lovell, 1990; Saward, 1984). In 1936, Lovell (Figure 1) had joined the Physics Department of the University of Manchester and eventually became involved in the study of cosmic rays using the cloud chamber technique developed by P.M.S. Blackett (see Lovell, 1976).

Blackett was particularly interested in the origin of cosmic rays. He noticed that their energy spectrum followed an inverse-square law over a range of energies of 10^{10}. He was convinced this fact must have some

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cosmic significance, in the same way that the Hubble Law has for the expansion of the universe. At the time of Lovell's involvement, the upper range of the energy spectrum was only known to about 10^{16} eV, and Blackett was keen to investigate to what energies this inverse-square law applied. In the summer of 1939, Lovell was planning a trip to the French Pyrenees to study cosmic rays at these higher energies. The project would involve cloud chamber measurements at the Pic du Midi Observatory.

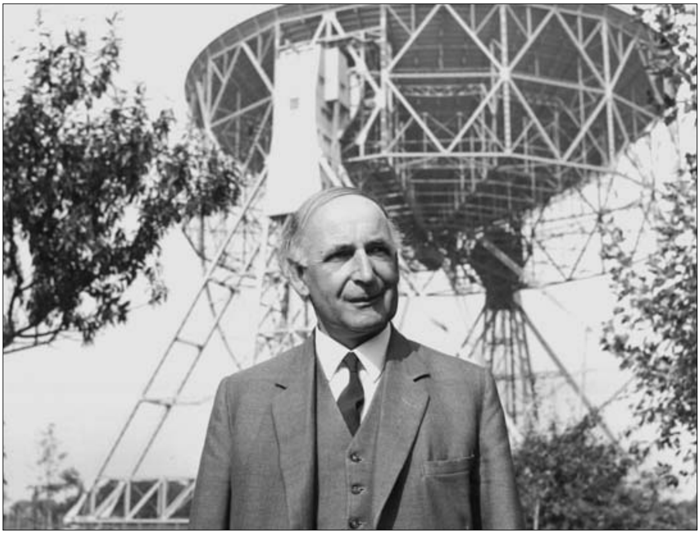

Figure 1. Sir Bernard Lovell (b. 1913).

However, before he could set out on this trip, Lovell was seconded to the Air Ministry to work in the development of radar techniques. As part of his induction, he found himself at the Chain Home early-warning radar station at Staxton Wold in northern England (for a description of the Chain Home radar system see Neale, 1985). On Sunday 3 September 1939, the young scientist was standing behind the radar operator when Prime Minister Neville Chamberlain announced that Britain was now at war with Germany. Lovell fully expected to see enemy aircraft appear on the cathode ray tube (CRT). He noticed several apparent radar echoes, and was confused that the operator was not reporting them to Fighter Command. She explained that the echoes were not from aircraft because they were sporadic and at the wrong range. Her explanation was that these common reflections were from the ionosphere.

Lovell suddenly wondered whether these sporadic echoes could be caused by high-energy cosmic rays ionising the upper atmosphere, and the possibility that radar techniques might provide an entirely new and important method for their study was intriguing. As the harsh winter of 1939-1940 progressed, Lovell became more embroiled in radar development, while Blackett was immersed in the bureaucracy of the Ministry of Defence. But when the two men next met, Lovell mentioned the idea of radar detection of cosmic rays. Missing the stimulation of pure research and still eager to investigate the high-energy spectrum, Blackett was very keen on the idea, and he hoped that such a technique might reveal that cosmologically-significant fact he believed cosmic rays held. In January 1941, Blackett and Lovell published a paper entitled "Radio echoes and cosmic ray showers" in the *Proceedings of the Royal Society*, which was to have far-reaching effects after the War.

After returning to the University of Manchester in July 1945, Lovell began to follow this line of enquiry. After some wrangling, he borrowed a GL II radar system from J.S. Hey at the Army Operational Research Group. The GL II radar was a mobile system, working at 4.2 metres, and was used to assist anti-aircraft fire. It consisted of three trailers; one for the trans-mitter, one for the directional Yagi aerial system, and the third for the generator. In early September, Lovell had the radar equipment working where it stood, in the quadrangle outside the Physics Department. However, the radar's cathode-ray tube was awash with interference, and Lovell soon attributed this to the electric trams and other vehicles running past the University.

At this time the University of Manchester ran a small botanical research station in the Cheshire countryside, about twenty miles south of the city. It was located in a remote spot called Jodrell Bank. The Vice-chancellor gave permission for Lovell to move his equipment, for a two-week period, to the botanical research grounds. So, on 10 December 1945 the three trailers arrived at the remote spot (with the Army's help), and were set up next to the botanists' huts. After several days the equipment was operational, and Lovell began observations. As hoped, the transient echoes were visible on the CRT, but to Lovell's surprise, he was recording up to several per hour, rather than the one or two a day that he expected.

2. METEORS

It was not long before Lovell realized that the large number of echoes he was observing with the GL II radar were not from cosmic rays. In late 1945, he wrote to Hey asking whether he had any experience of these transient echoes with his equipment. Hey replied by sending Lovell a copy of a classified memorandum he had produced for the Army Operational Research

Group.[1] In late 1944, London came under attack from V2 rockets, and the GL II radars were used to provide early warning. But many echoes of the appropriate range were detected when no V2s had been launched. Hey was charged with finding the cause of the false alarms, and he soon concluded that they were due to sporadic meteors in the upper atmosphere.

Even after reading this report, Lovell was still firmly fixed on the idea that the meteors were masking the cosmic ray echoes, and the next five years or so saw him trying to disentangle the supposed cosmic ray echoes from the meteor returns (see Gunn, 2005). In doing so, he inadvertently managed to discover many interesting facts about the meteor phenomenon.

Lovell was not an astronomer and soon sought help in understanding the meteor returns. He was put in touch with J.P.M. Prentice, the Director of the Meteor Section of the British Astronomical Association. Prentice was a Suffolk solicitor and amateur astronomer, and he instantly showed great interest in collaborating on the research. Their first task was to confirm the association of the radar echoes with visual meteors, and Prentice suggested the best time to do this was during one of the known meteor showers.

The first observations were performed during the Perseid shower of August 1946 with Prentice lying in a deck chair outside the radar trailers and shouting when a meteor streaked across the sky. The results were a little disappointing—although many returns seemed to be associated with visual meteors, many were not (see Prentice, Lovell, and Banwell, 1947). Several months later, in October 1946, Lovell and colleagues observed the Giacobinid shower and finally showed that the echoes of all durations and types were from meteor ionization (Lovell, Banwell, and Clegg, 1947; cf. Hey, Parsons, and Stewart, 1947). Furthermore, the realization that the echoes were specular in nature, and hence gave a maximum response when the meteor trails were perpendicular to the antenna beam, enabled the researchers to pinpoint the positions of the shower radiant.

In May 1947, while observing the η-Aquarids in the dawn sky, Clegg, Hughes, and Lovell (1947) noticed that the meteor echoes increased in rate well beyond the documented radiant point. It became clear that there existed a series of meteor showers occurring during daylight, invisible to visual observers. By 1951, four substantial showers had been established (the Arietids, o-Cetids, ξ-Perseids and β-Taurids) and several smaller areas of activity investigated (see Aspinall, Clegg, and Lovell, 1949; Ellyett, 1949; Aspinall and Hawkins, 1951; Davies and Greenhow, 1951; and Almond, 1951). It was later realized that some of these streams were the opposite orbital sectors of well-established cometary debris trails already associated with night-time showers.

Although other work was done at Jodrell Bank in these early years (including auroral observations, and radar studies of the Moon), most observations concerned various aspects of meteor research—and for an excellent review, see Davies and Lovell (1955). Indeed, it was really the success of this work that led to the University officially sanctioning Lovell's now long-expired two-week excursion into rural Cheshire.

3. THE METEOR CONTROVERSY

When the velocities of meteors first became measurable by photographic techniques, a dispute erupted as to whether or not hyperbolic meteoroids existed. The shower meteors that Lovell had been observing since the inception of Jodrell Bank were associated with cometary debris trails and were thus known to be in elliptical orbits. But there still remained a question over the origin of the sporadic meteors. If these could be proved to be in hyperbolic orbits, they could provide significant information about the size and distribution of interstellar dust. In the late 1940s, the debate over sporadic meteor velocities was at its height.

Figure 2. Ernst Julius Öpik (1893–1985).

The controversy first emerged with the publication, in 1925, of a meteor catalogue by von Niessel and Hoffmeister, which declared that 79% of meteors were in hyperbolic orbits. The hyperbolic argument was later championed by E.J. Öpik (first at Harvard, then at Armagh Observatory; see Figure 2), who was convinced a reservoir of comets far from the Sun was the source of high velocity meteoroids. This was prior to Oort's inference of the comet reservoir in 1950. In 1940, Öpik published a comprehensive analysis of visual meteor observations in which he found that 60% were hyperbolic. F.L. Whipple at Harvard and J.G. Porter at the Royal Greenwich Observatory (an expert in the computation of cometary and meteoric orbits) were amongst those convinced that hyperbolic velocities were simply measurement errors (e.g. see Porter, 1943, 1944).

In late 1946 and early 1947, C.D. Ellyett and J.G. Davies at Jodrell Bank had been working on radar techniques for measuring meteor velocities, unaware of the importance of the debate concerning meteoric origins. Their technique was based on observations of the Fresnel diffraction pattern associated with a meteor ionization trail formed in the upper atmosphere (see Figure 3). The first results on the velocities of Quadrantid and Geminid meteors were published in April 1948 (see Ellyett and Davies, 1948). By this time, Lovell's shower meteor work was gaining considerable attention amongst astronomers studying meteors by more classical means. In view of this, Lovell convened an international conference on meteor astronomy held in Manchester in September 1948. Fortuitously (or not), the leading protagonists in the velocity debate were in attendance, and it was not long before horns were locked.

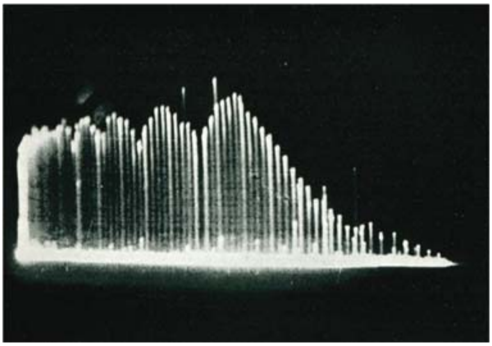

Figure 3. The diffraction pattern (amplitude versus time) of radio waves scattered from a meteor trail as photographed on a cathode ray tube. The velocity of the meteor is calculated from the distance between successive maxima and minima.

As Lovell showed a group around the site at Jodrell Bank, Porter and Öpik began arguing loudly about the interpretation of Öpik's results. Whipple was neutral, declaring only that he had found no evidence of hyperbolic orbits to date. Whipple did, however, suggest that the radar technique could be used to settle the issue by searching for sporadic meteors above the parabolic limit of 72 km/s (observed at the apex of the Earth's motion). Lovell, now aware of the controversy and eager to put the Ellyett/Davies technique to good use, confidently asserted that the problem would be solved by Christmas 1948. Indeed, by mid-December the team had successfully measured 67 sporadic meteor velocities. Initially, the results seemed to confirm Öpik's view that there was a strong hyperbolic component to the meteoric velocity distribution (Lovell, 1948). But a re-analysis proved conclusively that all the velocities were too low.

However, a cautious Lovell was concerned that the radar method may have been biased against higher velocities, and so he decided to repeat the experiment at a lower frequency. It took the better part of 1949 to build the new equipment and carry out the observations again (see Figure 4). This time, 187 velocities were derived and they again showed no evidence of a hyperbolic component.

Figure 4. The array of Yagi aerials working at 8.2 metres wavelength used in many of the experiments on the velocity distribution of sporadic meteors.

In December 1949, confident of the results, Lovell wrote to the protagonists of the controversy with the announcement that the debate was settled. Naturally, Porter was delighted, and he wrote: "Your results leave no doubt that the meteors are contained within the solar system." (quoted in Lovell, 1968: 14). Öpik and Hoffmeister, however, both violently argued that the experiments were still biased towards low velocities. Öpik even remarked: "Your results confirm my measurements that the meteors are from interstellar space." (ibid.). Whipple accepted the radar results but wondered whether fainter meteors, which were more likely to be interstellar in origin, might be investigated. Rather than settle the meteor debate, Lovell had only managed to fuel it.

Although convinced that the experiments were not selective, Lovell decided to repeat the observations once again, this time observing meteors coming from the antapex of the Earth's motion where the parabolic limit was much lower (12 km/s). By April 1950, another 87 velocities had been measured, again showing no hyperbolic component.

But, before Lovell could inform people of the results, Hoffmeister (1950a), who had been following Lovell's progress, published a paper in *The Observatory* in which he said of Lovell's work: "There are very strong selection effects in favour of slow meteors. " Lovell was somewhat annoyed by Hoffmeister's public discussion of unpublished work, and he wrote to Hoffmeister telling him of the recent results of the antapex observations (Lovell, 1950a). At the same time, he sent a response to Hoffmeister's *Observatory* paper, refuting the claim that the observations were biased and emphasized that recent observations had been carried out precisely in order to address the concerns of Hoffmeister and Öpik (Almond, Davies and Lovell, 1950).

Shortly afterwards, in August 1950, a letter appeared in *The Observatory* by Millman and McKinley (1950) of the Dominion Observatory, Ottawa. Unbeknownst to Lovell, Hoffmeister and Öpik, the Canadian group had already investigated the meteor velocity problem using a different radar technique (see McKinley, 1949). They drew attention to their own work by stating that "No evidence of hyperbolic velocities has appeared in over eleven thousand measurements …", and they went on to criticize Hoffmeister's position. Hoffmeister appears to have had a pre-publication copy of this letter because he wrote a response to *The Observatory* editors even before it was published (Ellison, 1950). In this he accepted the results of Lovell and the Canadians, although he still argued it did not conclusively prove that all meteors were members of the Solar System.

During these first few months of the argument, Öpik had been uncharacteristically silent on the matter. But at the end of August Hoffmeister (1950b) wrote again to Lovell, this time referring to a letter he had received from Öpik. The Armagh astronomer had written: "I have his [Lovell's] unpublished data at my disposal which clearly shows the interstellar component … Dr Lovell is well at home in the experimental side, but he has a weak idea of statistics … You notice he does not publish his actual data. This is a bias difficult to explain." (Hoffmeister, 1950b).

This incensed Lovell, and he replied to Hoffmeister: "I do not see why we should be accused of a bias against publishing our actual data when we have intentionally deferred publication in order to carry out another experiment for the purpose of meeting some of Öpik's criticisms." (Lovell, 1950b). On the same day, Lovell (1950c) wrote to the Editors of *The Observatory* expressing dismay that the debate had been forced into the open before publication, and informing them of the insulting remarks Öpik had made to Hoffmeister. Lovell asked the Editors to delay publication of Hoffmeister's response to Millman and McKinley until he was given the chance to review Lovell's new results.

The affair caused Lovell (1950d) to circulate a rather stern memorandum to Jodrell Bank's researchers, saying that publication of their data was now the highest priority, and that he hoped the controversy would subside, allowing them time to publish the results.

But this was not to be. Öpik's attack on his opponents moved up a gear in October 1950. In a lengthy paper, written with some vitriol, he talks of Porter's "… lack of responsibility …" and his "… Babylonic confusion of systematic errors …", and he refers to attempts to discount Öpik's work as "… pathetic and arrogant." (Öpik, 1950). He also gave a precise discussion as to why he was rejecting Lovell's work, even though, contrary to his claims, he had no access to Lovell's data.

Öpik's attack was published in only the third issue of the new *Irish Astronomical Journal*, of which Öpik was Editor (and often the sole author). Porter (1950) was of the opinion that Öpik was using his position as Editor unscrupulously, and he was considering a response to Öpik's criticisms. But Lovell (1950e) urged him not to enter the 'tirade', and admitted to Porter that he had "… been forced to the conclusion that he [Öpik] must be somewhat unbalanced." The parabolic camp went silent, which had the desired effect of silencing Öpik.

After some months, Hoffmeister (1951) published a revised paper in *The Observatory* in which he conceded to Lovell's most recent results. However, Lovell was still troubled by Whipple's concerns that the high-velocity meteors may occur at much fainter magnitudes. With a much larger aerial and increased transmitter power, he proceeded to measure a total of 1,095 velocities by November 1951. The results of these investigations were published in a series of four papers between 1951 and 1953 (Almond, Davies and Lovell, 1951, 1952, 1953; Clegg, 1952), and the final result, as before, was conclusive. All sporadic meteors were members of the Solar System (see Figure 5).

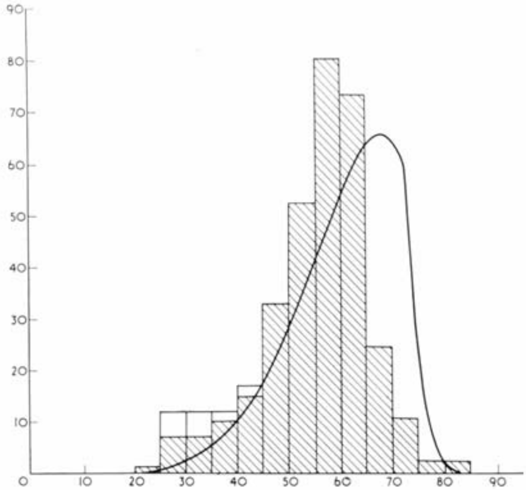

Figure 5. The velocity distribution (number of meteors versus velocity in km/s) of sporadic meteors observed in the Jodrell Bank experiments. The smooth curve shows the velocity distribution calculated on the assumption that the meteors are travelling in parabolic orbits in random directions.

There the debate seems to have been decided. Eventually, in 1969, Öpik admitted failure in the velocity dispute. He offered no apology and stood by his observations and statistical analysis, but claimed some of the original assumptions were incorrect. It is interesting to note that in a recent review of the meteor velocity debate, I.P. Williams (2004) points out that the controversy seems to have re-emerged in recent years.

4. CONCLUDING REMARKS

The study of meteors using radar techniques continued at Jodrell Bank well into the 1950s, and the final paper on the topic was published by Davies and Gill in 1960. But eventually, with the construction of the 76-m Lovell telescope, the interest of scientists turned to objects much further away.

5. NOTES

1. AORG Report No. 462. The work immediately after the War, which confirmed the association of transient echoes with meteors, was described

in a further Report by Hey and Stewart (No. 348), and in papers published in *Nature* (Hey and Stewart, 1946) and in the *Proceedings of the Physical Society* (Hey and Stewart, 1947).

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THE RADIOPHYSICS FIELD STATIONS AND THE EARLY DEVELOPMENT OF RADIO ASTRONOMY

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Abstract: During the period 1946-1961 Australia was one of the world's leading nations in radio astronomy and played a key role in its development. Much of the research was carried out at a number of different field stations and associated remote sites situated in or near Sydney which were maintained by the Commonwealth Scientific and Industrial Research Organisation's Division of Radiophysics. The best-known of these were Dover Heights, Dapto, Fleurs, Hornsby Valley and Potts Hill. At these and other field stations a succession of innovative radio telescopes was erected, and these were used by a band of young scientists—mainly men with engineering qualifications—to address a wide range of research issues, often with outstanding success.

Key words: Radio astronomy, Australia, field stations, Badgerys Creek, Dapto, Dover Heights, Fleurs, Georges Heights, Hornsby Valley, Murraybank, Penrith, Potts Hill, solar radio emission, 'radio stars', H-line emission

1. INTRODUCTION

Although radio astronomy was born in the 1930's with the pioneering efforts of Karl Jansky and Grote Reber (Kellermann and Sheets, 1983; Kellermann, 2005; Sullivan, 1984), this new branch of astronomy only blossomed after WWII. Between 1946 and 1961, two nations which established remarkable reputations in radio astronomy were Britain and

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Australia (Edge and Mulkay, 1976; Sullivan, 1984, 1988), the latter largely through the achievements of those employed by the Commonwealth Scientific and Industrial Research Organisation's Division of Radiophysics (henceforth RP). Prior to the opening of the Parkes Radio Telescope in November 1961, most of the observations conducted by RP staff were made at a network of field stations and associated remote sites in and near Sydney (Robertson, 1994). This paper examines the contribution that these field stations made to international radio astronomy.

2. THE RADIOPHYSICS FIELD STATIONS

In an unpublished report, Ruby Payne-Scott (1945) reveals that the RP radio astronomy program owes it origin to the more-or-less simultaneous arrival at the Radiophysics Laboratory of three crucial communications: secret reports of war-time detections of solar radio emission by radar units in England and New Zealand, and a copy of Grote Reber's (1944) research paper on 'cosmic static'. The New Zealand research was carried out by Elizabeth Alexander (see Orchiston, 2005a; Orchiston and Slee, 2002a), and this had greatest impact given that Taffy Bowen, Joe Pawsey and others at RP knew her personally, and that they had access to radar antennas similar to the ones used in New Zealand. Thus, RP's initial observations, dating between October 1945 and March 1946, were specifically designed to replicate the New Zealand work. Reports published in *Nature* (Pawsey, Payne-Scott, and McCready 1946) and in the *Proceedings of the Royal Society of London* (McCready, Pawsey and Payne-Scott, 1947) show that they were able to achieve this and to make additional contributions, thereby facilitating Australia's entry into a fascinating yet challenging new field of scientific endeavour.

Between 1945 and 1961, RP staff carried out radio astronomical observations at the Radiophysics building (which was located within the grounds of the University of Sydney), and at nine different field stations in the Sydney area and near Wollongong (see Figures 1a and 1b for New South Wales and Victorian localities mentioned in the text). In addition, the Collaroy and North Head WWII radar stations were home briefly to solar radio astronomy projects in 1945-1946, and during the 1950s and 1960s a number of short-lived 'remote' sites were used in conjunction with the regular field stations. In all, research by RP staff was carried out at twenty-one different sites in the general Newcastle-Sydney-Wollongong region during the early days of Australia radio astronomy, as well as at two solar eclipse sites in Victoria, two further solar eclipse sites in Tasmania, and two temporary field stations in the North Island of New Zealand (for further details see Orchiston, Sullivan and Chapman, 2006`). In addition, during the 1940s RP staff members were closely associated with the radio astronomy that was conducted at Mount Stromlo Observatory near Canberra (see Frame and Faulkner, 2003).

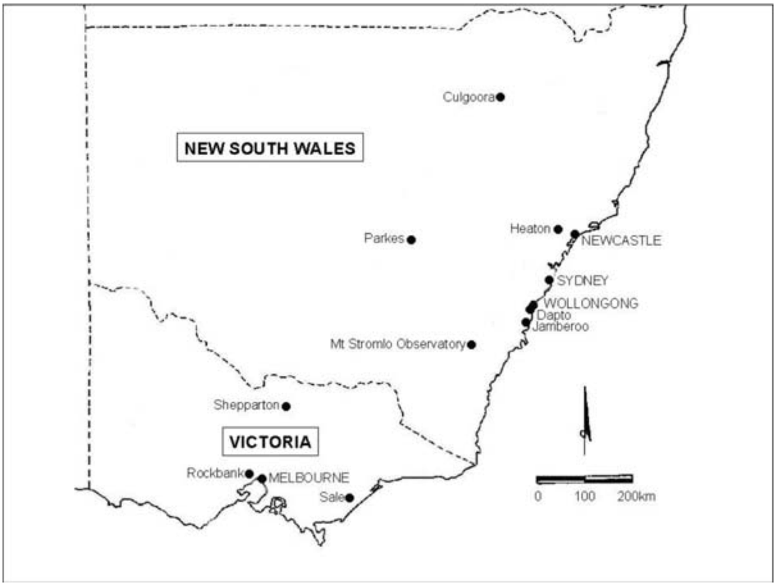

Figure 1a. New South Wales and Victorian localities mentioned in the text. For details of the Sydney region see Figure 1b.

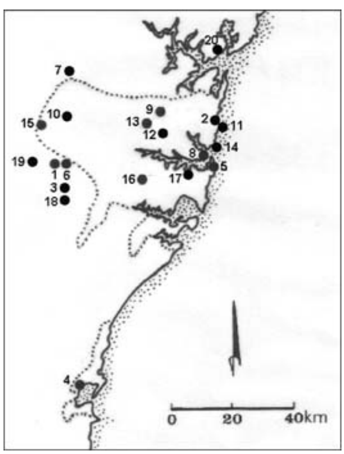

Figure 1b. Radio astronomy localities in the Sydney-Wollongong region. The dotted outlines show the current approximate boundaries of the Greater Sydney and Greater Wollongong regions. [Key to localities: 1 = Badgerys Creek, 2 = Collaroy, 3 = Cumberland Park, 4 = Dapto, 5 = Dover Heights, 6 = Fleurs, 7 = Freeman's Reach, 8 = Georges Heights, 9 = Hornsby Valley, 10 = Llandilo, 11 = Long Reef, 12 = Marsfield (ATNF Headquarters), 13 = Murraybank, 14 = North Head, 15 = Penrith, 16 = Potts Hill, 17 = Radiophysics Laboratory (Sydney University grounds), 18 = Rossmore, 19 = Wallacia, 20 = West Head.]

The bulk of Australia's early radio astronomical observations were made at the nine RP field stations by small close-knit teams of researchers, typically with radio rather than astronomy backgrounds, who at any one time worked on a limited number of specialized research projects. Initially they commandeered surplus WWII receivers and other equipment or built their own primitive instrumentation, but with the passage of the years some amazingly innovative new types of radio telescopes were developed in response to specific research needs.

Field station life was primitive, "… completely informal and egalitarian, with dirty jobs shared by all. Thermionic valves were in frequent need of replacement and old and well-used co-axial connectors were a constant source of trouble … there was no place for observers who were incapable of repairing or maintaining equipment." (Christiansen, 1984: 113, 115). On the other hand, those of us lucky enough to have lived through this era remember the field stations with genuine affection. There was a freedom not experienced by those back at the 'Lab' (as the Radiophysics Laboratory was known): the pervading sunshine, the clean fresh air, those incident-packed return trips from home to field station by Commonwealth car, and the sense that we were somehow making history. There were also snakes to contend with, wet days when antennas still had to be aligned and observations made, floods that had to be negotiated, and those times—fortunately they were few and far between—when vehicles became bogged and had to be rescued by a co-operative local farmer (Figure 2). Slide rules were the norm and computers but a future dream. Signal generators, not sources, provided calibrations, and results were displayed in real time on Esterline Angus and other all-too-familiar chart recorders. These were pioneering days!

Figure 2. Fleurs' indispensable mobile field laboratory, 'Flo', being extricated from the mud by the radio astrono-mers and a helpful local farmer (ATNF: B3923-4).

Figure 3. John Bolton, Gordon Stanley and Joe Pawsey (left to right) at the Radiophysics Laboratory (ATNF: 11833-6).

Overseeing the work at the various field stations was Dr Joe Pawsey (see Figure 3), a revered father figure and head of the Radio Astronomy Group within the Division of Radiophysics (Chief of the Division, Dr E.G. (Taffy) Bowen, was responsible for the Division's other major research areas, cloud physics, rain-making and air-navigation aids). Joe liked to make unheralded visits to the field stations, and sometimes turned up for morning or afternoon tea armed with a supply of his greatest gastronomic weakness, 'lamingtons'—those tasty cubes of sponge cake coated with chocolate icing. Notwithstanding these rather popular 'bribes', Joe was "… very good at getting the best out of people. He had this method whereby when you came to talk to him about some problem, he'd often propose some other way of doing it … he took an interest in everything …" (Gardner, 1973). But the downside of Joe's unbridled enthusiasm, Ruby Payne-Scott (1978) recalls, was that "… you were lucky to get home for dinner when he showed up at the end of the day." Yet, through these visits Joe was able to chart progress at the field stations, discuss problems, and keep everyone abreast of relevant developments back at the Lab. Field station staff also got to hear about work at other field stations when they attended seminars and occasional meetings at the Radiophysics Laboratory.

While most of the early observational work was carried out at the field stations, in 1948 and 1949 some important research was achieved using a 1.1 metre (44-inch) dish mounted on the 'Eagle's Nest' (Figure 4), a small room and associated flat-roofed area located at the very top of the Radiophysics Laboratory (Figure 1b). This recycled WWII searchlight mirror was used by Norman Labrum, Harry Minnett and Jack Piddington (in various combinations) to observe the Sun at 9,400 and 24,000 MHz and the Moon at 24,000 MHz before being transferred to the Potts Hill field station.

Figure 4: The 'Eagle's Nest' at the top of the Radiophysics Laboratory, showing the small antenna used by Labrum, Minnett and Piddington (ATNF: B1641).

Potts Hill and the other field stations shown in Figures 1a and 1b are now considered individually, and for each we indicate the period of existence, the types of instruments and leading scientists found there, and their major research achievements.

2.1 Dover Heights

The Dover Heights field station (see Bolton, 1982; Slee 1994) was at the site of an Australian Army WWII radar station in suburban Sydney, 5 km south of the entrance to Sydney Harbour (Figure 1b). Located atop a 79 metre high coastal cliff, it soon became known internationally for the solar research carried out by Ruby Payne-Scott in 1945-1946 and for the pioneering work on discrete sources undertaken mainly by John Bolton (Kellermann, 1996), Gordon Stanley (Kellermann et al., 2005) and Bruce Slee (Orchiston 2004a). Bolton and Stanley are both shown in Figure 3. Most of the early observations were conducted with Yagi antennas operating at 60, 85, 100 and 200 MHz, which were mounted on the roof of a cliff-side WWII block-house (see Figure 5). These simple radio telescopes were often used in sea interferometer mode—and for a description of the sea interferometer concept see Bolton and Slee (1953).

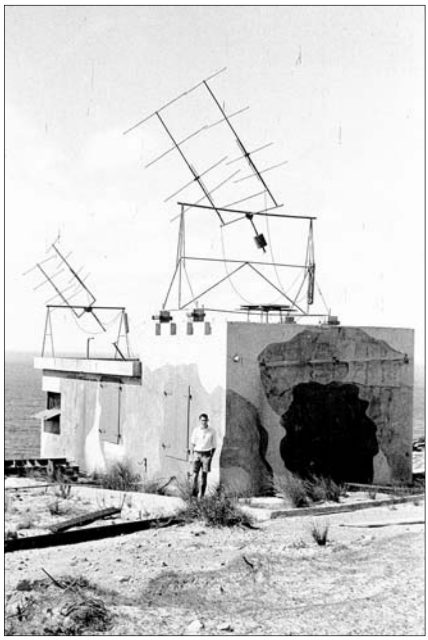

Figure 5: John Bolton and the cliff-side WWII block-house at Dover Heights, with 100 MHz (left) and 60 MHz (right) twin Yagis on the roof (ATNF: B1031-6).

In 1946 Hey et al. announced the presence of a small, variable radio source in Cygnus, and in mid-1947 Bolton and Stanley confirmed this discovery at Dover Heights and over the next few months observed this enigmatic source at 60, 85, 100 and 200 MHz. Slee joined the team in September 1947, and soon after this the three young researchers conducted what was probably the world's first spaced-antenna experiment, using aerials at Dover Heights and at Long Reef and West Head (about 15 km and 35 km away, respectively, to the north of Sydney Harbour—see Figure 1b) in order to measure the degree of correlation between the intensity variations from Cygnus A at the three sites. They found there was a high correlation, showing that the variations were either an intrinsic feature of the source itself or else were caused by an ionospheric, interplanetary or interstellar diffraction pattern with a scale size less than 15km sweeping across the Earth. Subsequent observations by British colleagues confirmed their ionospheric origin.

Meanwhile, at Dover Heights the pressure was also on to search for more radio stars, and this led to a survey with the 100 MHz sea interferometer, starting in November 1947. The first success came on November 6 when Taurus A was found (see Figure 6), and it was followed over the next few months by two others, Centaurus A and Virgo A. This was quite an achievement given the primitive nature of the equipment, which "… was very cranky … you got interference patterns one day and wouldn't get them the next. Equipment would fail … The sea interferometer had a lot of nasty habits, like you could get interference wiped out by refraction problems and get sources rising ten minutes of time late and all this sort of crazy stuff …" (Stanley, 1974).

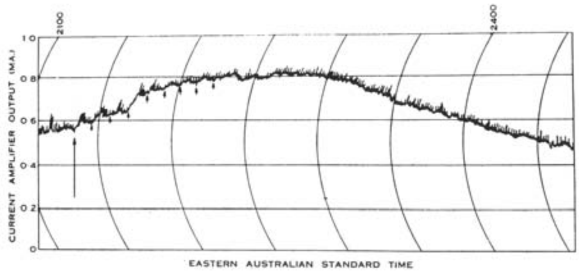

Figure 6. The 6 November 1947 chart record, showing the discovery of Taurus A. The interference fringes are indicated by the arrows (after Slee, 1994).

The next priority was to determine accurate positions for these sources so that optical correlates could be sought. This was achieved in mid-1948 when observations made from high coastal cliffs near Auckland, New Zealand, allowed the identification of Taurus A with the Crab Nebula, and Centaurus A and Virgo A with extragalactic nebulae (Bolton, Stanley and Slee, 1949), showing convincingly that the term 'radio star' was a misnomer and that these objects generated almost unbelievable levels of radio energy. But more than this, these identifications revealed the potency of radio astronomy to many optical astronomers for the first time, and marked the start of 'bridge-building' between these two disparate groups of scientists (c.f. Jarrell, 2005). The New Zealand field trip that led to this remarkable breakthrough, and to the establishment of RP's two most easterly—albeit temporary—field stations in Stanley's ancestral homeland, is discussed further in Orchiston (1993, and 1994).

With the realization that 'radio stars' were actually discrete extended sources, the search was on, in earnest, for further sources. In 1948, more sources were discovered with the 2-Yagi antenna, including Fornax A, Hercules A and Hydra A. In 1949 a steerable 9-Yagi array was installed on the roof of the blockhouse and used to survey Milky Way emission at 100 MHz. When used in sea-interferometer mode, it revealed 14 new sources. At this time, Stanley and Slee also used a number of 2-Yagi antennas to measure the intensities of Centaurus A, Cygnus A, Taurus A and Virgo A at a number of frequencies between 40 and 160 MHz, and published the first acceptable source spectra (see Stanley and Slee, 1950). These strongly suggested that the radio emission was non-thermal in origin.

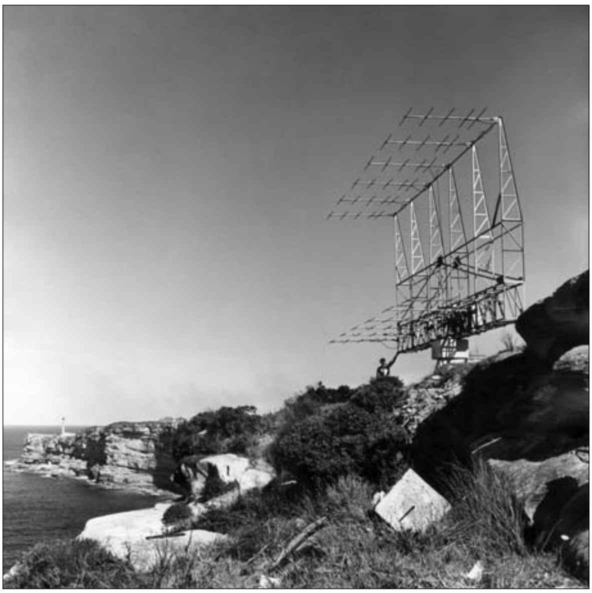

Figure 7. The 12-Yagi sea interferometer (ATNF: 2763-8).

Late in 1951, an 8-Yagi sea interferometer was installed near the blockhouse, and this was soon converted into a 12-Yagi array (Figure 7) which was used at 100 MHz to identify 122 different sources scattered across the entire sky. At that time, this was the most comprehensive survey ever undertaken, and showed that discrete sources were far from rare; in fact, increased sensitivity seemed to bring increasing numbers of them! The celestial distribution of the 122 sources is shown in Figure 8, where the predominance of stronger sources along the plane of the Galaxy and of weaker sources in the southern polar cap is apparent. Meanwhile, new optical identifications suggested by Bolton, Stanley and Slee (1954) only served to reinforce the view that most discrete sources were extragalactic in origin and associated with radio galaxies.

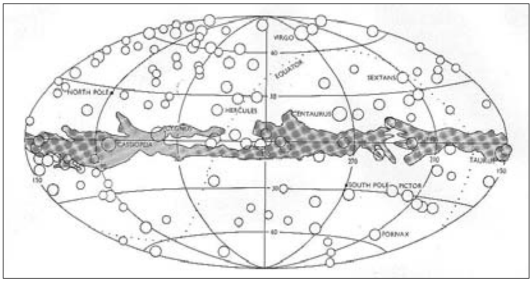

Figure 8. Celestial distribution of the 122 sources detected with the 12-Yagi array.

The last radio telescope installed at Dover Heights has an interesting history, and one associated with both ingenuity and initiative. When Bolton, Stanley and Slee were unable to obtain funding for a large new radio telescope in 1951 they resorted to building one themselves as a three month lunchtime project. During this period, they excavated a 21.9 metre (72-ft) diameter parabolic hole in the sand just north of the blockhouse, lined the sand with discarded strips of metal from packing cases in order to provide a reflective surface, installed a mast with a dipole, and connected this crude radio telescope up to a 160 MHz receiver. As the Earth rotated, this transit instrument received emission from the sky overhead, in the process revealing a new radio source near the centre of our Galaxy. This initial result inspired Pawsey to assign funds for an expanded 400 MHz hole-in-the-ground antenna (Figure 9), and by altering the position of the aerial mast with strategically-placed guy ropes the new concrete-coated 24.4m (80-ft) dish was able to map a strip of sky and record fourteen different discrete sources.

Figure 9: The 24.4 metre (80-ft) hole-in-the-ground radio telescope. Gordon Stanley is using a theodolite to record the position of the aerial mast (ATNF: B3150-7).

The strongest of these was the Galactic Centre source, which was assigned the name Sagittarius A, or Sgr A (Figure 10). At the time, this remarkable radio telescope was only exceeded in aperture by one in England and a slightly larger hole-in-the-ground antenna in the Netherlands (see Strom, 2005), and a detailed account of its construction and research achievements is provided by Orchiston and Slee (2002c).

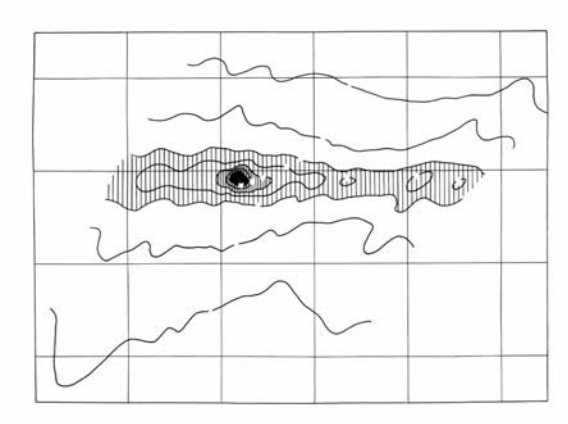

Figure 10. 400 MHz radio emission along the Galactic plane, showing the Galactic Centre (Sgr A) source (ATNF: B3274).

In 1954 Stanley was involved in the final research project undertaken at Dover Heights when he and a visiting American radio astronomer used the hole-in-the-ground antenna to search unsuccessfully for emission from the postulated 327 MHz deuterium line. Later that year the Dover Heights field station was closed down, as the Division's focus shifted to Potts Hill and a new field station at Fleurs.

2.2 Georges Heights

The Georges Heights radar station occupied an attractive strategic position on Middle Head, overlooking the entrance to Sydney Harbour (Figure 1b), and during the war was home to a number of different radar antennas. In 1947 and 1948, this site was used by RP as a short-lived field station for solar radio astronomy (Orchiston, 2004b).

One of the wartime radar antennas at Georges Heights was an experimental unit featuring a 4.3 × 4.8 metre (16 × 18 feet) section of a parabola and a cumbersome altazimuth mounting (Figure 11), and from August 1947 this was used to monitor solar radio emission at 200, 600 and 1,200 MHz. The only way the antenna could be employed effectively was to place it ahead of the Sun, let the Sun drift through the beam, hand-crank it ahead of the Sun again, and repeat the process throughout the day. This procedure produced a distinctive 'picket fence' chart record.

Figure 11: The ex-WWII radar antenna used for solar radio astronomy at Georges Heights (ATNF: B1164).

Assigned to this antenna were two young RP radio engineers, Fred Lehany and Don Yabsley. Lehany (1978) relates that his involvement with this project "… came about in a typical 'Pawseyian way', before I knew what was happening … there was an observing program and … Yabsley and I were a suitable pair to share not only the week days but also the weekend duty …" For a few months in 1947, Lehany and Yabsley were assisted by Bruce Slee, before he was re-assigned to Dover Heights.

In the second half of 1947, Ruby Payne-Scott and John Bolton were monitoring solar activity at Dover Heights using 60, 75, 100 and 200 MHz Yagis, and the Georges Heights antenna allowed the frequency-coverage to be extended to up to 1,200 MHz. As at Dover Heights, Lehany and Yabsley recorded many bursts at 200 MHz, but they were rare at 600 and 1,200 MHz, where the general flux variations with time were correlated with sunspot area. This research by Lehany and Yabsley helped our understanding of the association of 200 MHz bursts and solar flares, and of the correlation between sunspots and radio emission at 600 and 1200 MHz.

The final role that the Georges Heights field station played in radio astronomy was to serve as a test-base in mid-1948 for two portable 3.05 metre (10-ft) altazimuth-mounted dishes that were constructed in order to observe the 1 November 1948 partial solar eclipse from Rockbank in Victoria (Figure 1a) and Strahan in Tasmania. Ironically, this eclipse was the death-knell for Georges Heights as a radio astronomy facility, for a decision was made to transfer the ex-radar antenna to the Potts Hill field station, where it would be used to monitor the eclipse. After less than two years operation as an RP field station, Georges Heights closed down, but its legacy lived on in the form of the ex-radar antenna. With the passage of time, this historic radio telescope would come to be Georges Heights' greatest contribution to Australian radio astronomy.

2.3 Hornsby Valley

This field station (Orchiston and Slee, 2005) was established in 1946 at what was then one of Sydney's most northerly suburbs (see Figure 1b). Hornsby Valley was accessed by train or car, and this radio-quiet site was located on farmland in a picturesque valley that was surrounded by low tree-covered hills (see Figure 12).

Figure 12. Panoramic view of the Hornsby Valley field station, showing antennas, instrument huts, and (far left) a farm house (ATNF: B2802-10).

The first research conducted at this field station was radar astronomy: in 1947-1948 Frank Kerr and Alex Shain spent a year bouncing signals off the Moon in order to investigate the structure of the upper ionosphere. A rhombic aerial linked to a modified communications receiver recorded the bounced signals, which were broadcast at 17.84 and 21.54 MHz by Radio Australia from Shepparton in Victoria (see Figure 1a). Thirty different experiments were carried out, and echoes were received on twenty-four occasions; as expected, these provided further information about the Earth's atmosphere, but from an astronomical viewpoint the interesting conclusion that Kerr and Shain drew was that the nature of the echoes showed the Moon's surface to be "rough" rather than smooth. This project was to be Kerr's sole foray into low frequency research, and he soon transferred to Potts Hill field station where he went on to make a name for himself through his H-line work.

Ruby Payne-Scott was keen to expand the solar work she had begun at Dover Heights, and towards the end of 1947 she moved to the Hornsby Valley and set up Yagi antennas for observations at 60, 65 and 85 MHz, together with an 18.3 MHz broadside array. She also made use of Kerr's Moon-bounce rhombic antenna. Her study of solar bursts ran from January through to September 1948, when she too transferred to Potts Hill field station.

After Payne-Scott and Kerr left Shain stayed on, and he developed Hornsby Valley into RP's forefront low frequency field station. During 1949 and the early 1950s he and Charlie Higgins built 9.15 and 18.3 MHz horizontal arrays that were distinguished by their simplicity: ordinary posts were used to support the dipoles, with the ground serving as a reflector (Figure 13). The most ambitious of these radio telescopes was an array of 30 horizontal half-wave dipoles, and by moving the beam electronically a strip of sky extending from declination –12° to –50° could be surveyed. These Hornsby Valley antennas were used to produce the first maps of Galactic emission at low frequencies.

Figure 13: Australian and overseas radio astronomers attending the 1952 URSI Congress in Sydney are shown discussing the Hornsby Valley work, surrounded by antennas of the 9.15 MHz array (ATNF: 2842-131).

A common problem at low frequencies was terrestrial interference—which Shain tended to dismiss as rather a nuisance—but when Burke and Franklin reported the discovery of decametric burst emission from Jupiter in 1955 he was forced into a rethink. When he revisited some of those periods of 'intense static' recorded at 18.3 MHz in 1950 and 1951 (see Figure 14), Shain found that these were indeed Jovian bursts, and this serendipitous 'pre-discovery' proved to be one of RP's most notable 'lost opportunities'. Shain (1956) noticed that the bursts were not uniformly distributed in Jovian longitude but tended to cluster between 0° and 135°. In another words, much of the radiation appeared to derive from a localized region on the planet, and its rotation period was timed at 9h 55m 13±5s, only marginally longer than Jupiter's System I rotation period.

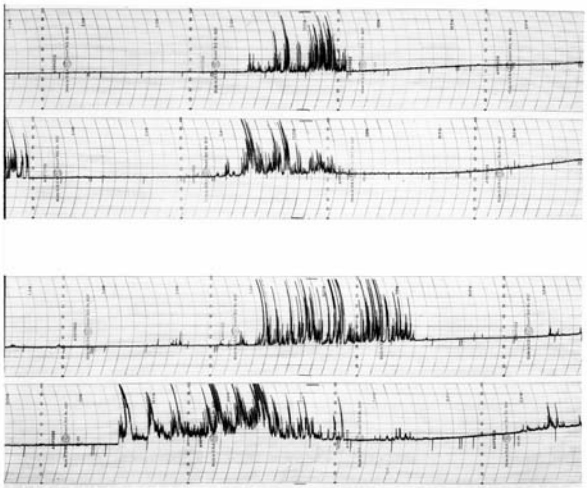

Figure 14. Examples of 18.3 MHz Jovian bursts noted on the 1950-1951 chart records (ATNF: B3719-13).

The Hornsby Valley field station closed in 1955, after contributing pioneering studies in lunar, solar and Galactic astronomy. Shain and Higgins then transferred their low frequency research to Fleurs.

2.4 Potts Hill

This field station (Figure 15) was situated beside a metropolitan water reservoir in what at that time was an outer Sydney suburb (see Figure 1b), and began operations in 1948. The site appealed because it offered an area of easily accessible flat land in a radio-quiet setting, and as such was ideal for solar radio astronomy. A number of radio astronomers built their international reputations at Potts Hill, the most notable of whom were W.N. (Chris) Christiansen, Jim Hindman, Frank Kerr, Harry Minnett, and Jack

Piddington. In addition, Rod Davies, Norman Labrum, Alec Little, Don Mathewson, Bernie Mills, John Murray, Ruby Payne-Scott, Gil Trent, Joe Warburton, Don Yabsley and a rather youthful Brian Robinson carried out research at this field station.

Figure 15. Aerial photograph, looking south, of the eastern reservoir at Potts Hill. Eventually, radio telescopes and associated huts were located along the southern and western margins of the reservoir, and across the extended area of flat land in the foreground, to the north of the reservoir (ATNF: B3253-1).

The first radio telescopes at Potts Hill were situated at the northern end of the reservoir and comprised a single Yagi antenna (used by Little to observe the Sun at 62 MHz), and a 3.05 metre (10-ft) diameter dish (which was employed by Piddington and Minnett to survey radiation from the region of the Galactic Centre at 1,210 MHz).

In the second half of 1948 the experimental radar antenna that had previously been used for solar monitoring at Georges Heights was transferred to Potts Hill and mounted equatorially (see Figure 22), so that it could be used to observe the 1 November solar eclipse. The observations were made at 600 MHz by Christiansen, Yabsley and Mills, in conjunction with 3,000 MHz observations made by Piddington and Hindman with a 1.7 metre (68-in) dish. Apart from these two Potts Hill radio telescopes, the eclipse was observed by RP staff at 600 MHz from Rockbank near Melbourne (Figure 1a) and Strahan, in Tasmania, using portable 3.05 metre (10-ft) dishes (see Orchiston, 2005b). Following the New Zealand 'radio stars' field trip of June-August 1948, this eclipse therefore continued a tradition of establishing radio telescopes at temporary remote sites for special projects. When the various observations of the eclipse were combined they allowed the sources of solar radio emission to be pinpointed with considerable precision (see Figure 16). But more than this, these eclipse observations confirmed the existence of two discrete components of non-burst solar emission: a basic component of thermal origin, which originates from the entire solar disk, and a slowly-varying component that is generated in small localized regions that are often associated with sunspots.

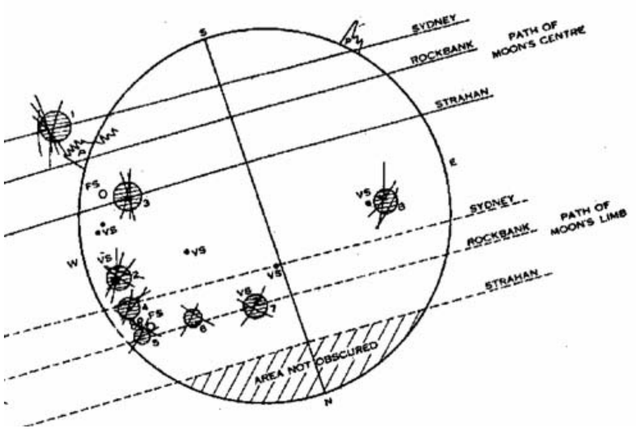

Figure 16. Solar map showing the distribution of sunspots (marked by 'VS') and regions associated with radio emission (hatching). The radio observations were made from Potts Hill, Rockbank (Victoria) and Strahan (Tasmania) during the 1 November 1948 partial solar eclipse (ATNF: B1983-3).

In 1949, the small parabola that had been used at the RP Laboratory for lunar and solar work in 1948–1949 was transferred to Potts Hill, and on 22 October it was used in conjunction with other radio telescopes at Potts Hill and the two portable 3.05 metre dishes to observe another partial solar eclipse. These latter radio telescopes were located at Strahan—once again—and near Sale, in eastern Victoria (Figure 1a).

Not content with merely detecting solar bursts, Payne-Scott and Little wanted to record their positions, angular sizes and polarization, so in early 1949 the RP Workshop constructed a new interferometer comprising three 97 MHz Yagi aerials, which were aligned E-W, near the northern edge of the reservoir (and one of these is shown in Figure 17). These were on equatorial mounts and could track the Sun for four hours daily, centered on midday. Crossed dipoles allowed them to receive left-hand and right-hand circular polarization. The Yagis could be used as either swept-lobe or fixed lobe interferometers, depending on the type of investigation desired (see Little and Payne-Scott, 1951). Frank Kerr (1971) has described how this interferometer "… was the first one in the world which could locate a source-position on the sun sufficiently rapidly to be able to operate on the shortlived bursts." Analyses of thirty noise storms, six outbursts and twenty-five randomly-polarized bursts detected between May 1949 and August 1950 showed a link between noise storms and the magnetic fields associated with sunspots (even though the noise storms were located in the corona), and between outbursts and solar flares. Outbursts were observed to move away from the Sun at velocities of between 500 and 3,000 km/sec., and Payne-Scott and Little suggested that they were initiated by corpuscular streams responsible for terrestrial magnetic storms.

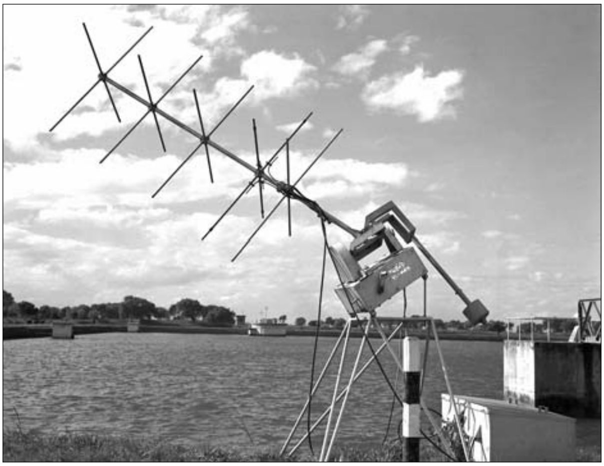

Figure 17. View looking south across the reservoir, showing one of the 97 MHz position interferometer crossed Yagis (ATNF: 2217).

During 1949 and through into the 1950s the position interferometer, the ex-Georges Heights antenna, a 62 MHz Yagi and two small parabolas were used to record solar activity at 62, 97, 600, 1,200, 3,000 and 9,400 MHz, with support monitoring from Mount Stromlo Observatory (Figure 1a) at 200 MHz. These observations confirmed an earlier discovery, namely that over time intervals of many months variations in the level of solar emission at frequencies above 200 MHz mirrored changes in sunspot area. Many bursts also were detected, and sometimes these were associated with solar flares and terrestrial effects. In his analysis, Davies (1954: 90) suggested that "… there may be two separate components of bursts, one which shows rapid fluctuations and predominates at the lower frequencies, and one which is smooth and is characteristic of the high frequencies (although it may occur at low frequencies also)." He further suggested that these components may be due to plasma oscillations and thermal emission, respectively. Davies'

1954 research paper was published after he had left RP and joined the Jodrell Bank radio astronomers, and even earlier, in 1951, Ruby Payne-Scott had resigned so that she could start a family. In those days, married women were prevented from accepting permanent positions in the Commonwealth Scientific and Industrial Research Organisation, so Ruby had kept her 1944 marriage secret up till that point.

Further escalation of the solar radio astronomy program took place in 1951 when Christiansen oversaw the installation of the world's first solar grating array along the southern margin of the reservoir. Designed to track the Sun at 1,420 MHz, this novel radio telescope comprised 32 solid metal parabolic dishes each 2 metres (66-in) in diameter and spaced at 7 metre intervals (see Figure 18). This novel radio telescope (Christiansen and Warburton, 1953) provided a series of 3′ fan beams each separated by 1.7°, which meant that the Sun could only be in one beam at any one time. The array was operational from February 1952, and was used daily for ~2 hours, centred on midday, to produce E-W scans of the Sun. These showed up the positions of localized active regions situated low in the solar corona.

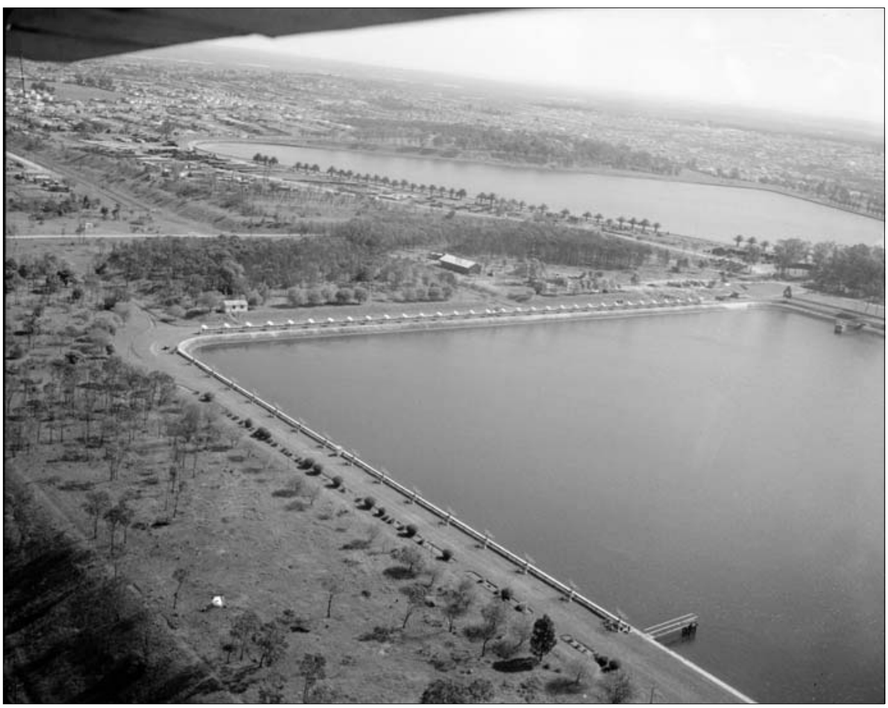

Figure 18. Aerial view of the Potts Hill reservoirs, looking south-west. Antennas of the two 1,420 MHz solar grating arrays are clearly visible along the edges of the nearer reservoir (ATNF: 3475-1).

A second solar grating array was erected along the eastern margin of the same reservoir in 1953 (this, too, is shown in Figure 18). This also operated at 1,420 MHz, but contained just 16 equatorially-mounted mesh dishes each 3.4 metres (11-ft) in diameter. From September 1953 to April 1954 this was used to generate a series of N-S scans of the Sun. By taking many months of observations and manually deleting all evidence of active regions, Christiansen and Warburton (1955) were able to build up an image of the quiet Sun, and show that this exhibited limb-brightening (Figure 19). Moreover, the radio Sun was seen to be non-circular, with the limb-brightening confined to the near-equatorial regions. Christiansen (1976) would later regard this project as particularly important, given that this was the first time that the concept of Earth-rotational synthesis had been used in radio astronomy.

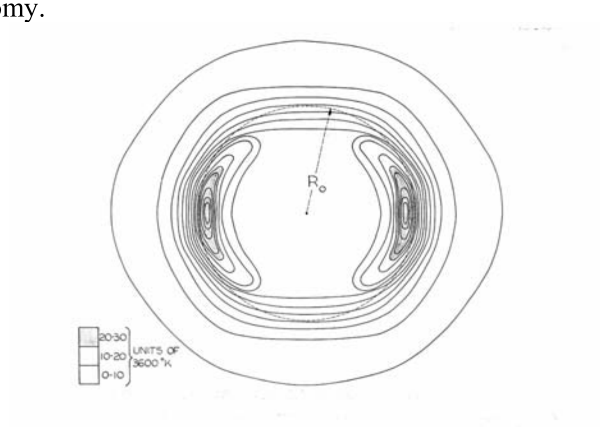

Figure 19. The quiet Sun at 1,420 MHz, showing equatorial limb-brightening (ATNF: 3480).

The final solar radio astronomy project at Potts Hill occurred in 1954-1955 when two Indian visitors to RP, Swarup and Parthasarathy, modified the E-W solar array so it could observe at 500 MHz, and used this instrument to investigate radio emission from the quiet Sun at this new frequency. Soon after this research was completed, the 'Chris Cross' was constructed at Fleurs, and the Potts Hill solar program transferred to that field station. Arrangements were then made to transfer ownership of the redundant Potts Hill grating array and the 500 MHz receiver to the National Physical Laboratory in India, and at this time the second solar grating array was also closed down.

To return to non-solar radio astronomy at Potts Hill: between 1948 and 1950 Piddington and Minnett used the ex-Georges Heights antenna and one of the smaller radio telescopes for source surveys at a number of different frequencies. Undoubtedly, their most significant discovery—made in 1950 —was "… a new, and remarkably powerful, discrete source." close to position of the centre of our Galaxy (Piddington, and Minnett, 1951: 469). This was the first published record of the Sgr A source, which—as we have seen—was subsequently rediscovered by McGee, Stanley and Slee at Dover Heights (c.f. Figure 10).

Apart from pioneering solar radio astronomy, Potts Hill is famous for its involvement in early H-line work. Following Ewen and Purcell's pre-publication announcement of their 25 March 1951 detection of the 1,421 MHz hydrogen line in the USA (see Kerr, 1984), Pawsey asked Christiansen and Hindman to construct H-line receivers. At first they worked away independently and unbeknown to each other in adjacent instrument huts at Potts Hill, and each made significant progress before discovering what the other was up to and deciding to combine their efforts. The result was that in just six short but hectic weeks they were able to cobble together a primitive H-line receiver, which was "… the most terrible piece of equipment I've ever seen in all my life ... It was a monster …" (Christiansen, 1976). Nonetheless, Christiansen and Hindman proceeded to attach the "monster" to the ex-Georges Heights radar antenna, and soon succeeded in detecting the line.[1] Ewen and Purcell's 'discovery' paper appeared in the 1 September 1951 issue of *Nature*, and was immediately followed by a confirmatory paper by the Dutch and a hurriedly-composed note by Pawsey, dated 12 July, announcing the initial RP results (see Pawsey, 1951). Thus began Australia's assault on the H-line.

From June through September of 1951 Christiansen and Hindman carried out exploratory H-line observations at Potts Hill, and published their results in 1952. Their paper included an isophote map of H-line emission extending over 270 degrees of Galactic longitude (including the Galactic Centre) along the Galactic plane and from l = +40° to –50°, and they concluded that "… the source of line radiation occupies roughly the same part of the sky as does the visible Milky Way. Hence it may be assumed that the hydrogen is concentrated near the equatorial plane of the Galaxy." (Christiansen and Hindman, 1952: 454-455). The existence of double line profiles over a considerable range of Galactic longitudes was interpreted as evidence of spiral arms in our Galaxy.

With increasing international interest in hydrogen-line work, a new, more suitable, radio telescope was required *in lieu* of the aging ex-Georges Heights radar antenna, and this came in the form of an 11 metre (36-ft) transit parabola that was constructed in 1952-1953 (see Figure 20), along with "… the world's first "multi-channel" receiver, which had all of four 40 kc/s channels!" (Kerr, 1984: 139). With Christiansen back on solar work, it was left to Hindman, Kerr and Robinson to take the H-line work further. They began by making the first H-line observations of extragalactic objects, in this case the Large and Small Magellanic Clouds, and found that the neutral hydrogen extended well beyond the optical boundaries of each Cloud; that the total mass of neutral hydrogen in the Large and Small Clouds was $\sim 6 \times 10^8$ $M_\odot$ and $\sim 4 \times 10^8$ $M_\odot$ respectively; that the ratio of dust to gas in the two Clouds was very different; and that both Clouds were rotating (Kerr, Hindman, and Robinson, 1954). Kerr and de Vaucouleurs followed up by studying the three-dimensional distribution of gas density and rotational motion. Radio data supported the view that the Large Magellanic Cloud is a flattened system tilted by ≥65° relative to our line of sight. The tilt angle of the Small Magellanic Cloud is only ~30°, and at 1,420 MHz it has "… a large prominence, or wing, extending towards the Large Cloud." (Kerr and de Vaucouleurs, 1955: 515).

Figure 20. The 11 metre parabola built for H-line work in 1952-1953, and associated instrument hut (ATNF: 2975-21).

One of the most fascinating H-line studies carried out at Potts Hill was associated with the collaboration between the Sydney and Dutch groups to map the locations of the spiral arms in our Galaxy. Kerr, Hindman and Carpenter's seminal 1957 paper in *Nature* provided overwhelming pictorial evidence of the spiral nature of our Galaxy (see Figure 21), and the Potts Hill observations yielded evidence of at least four major spiral arms. Their study also produced interesting information on the distribution of hydrogen gas: in the outer regions of the Galaxy it was distorted downwards in the direction of the two Magellanic Clouds, suggesting that this may be evidence of some sort of gravitational tide produced by the Clouds (Kerr, Hindman and Carpenter, 1957: 679). Kerr also summarised these and other Sydney H-line findings in a number of other papers. With the opening of Murraybank in 1956, the Division's H-line research was transferred to this new field station.

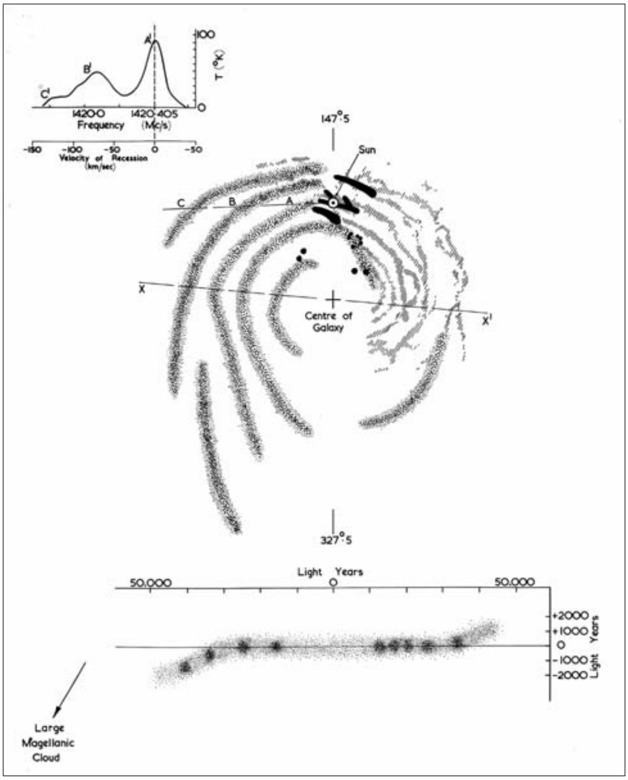

Figure 21. H-line evidence for the spiral nature of our Galaxy, and the distortion of the hydrogen gas away from the plane in the outer reaches of the Galaxy (ATNF: B5152).

Piddington and Trent then took advantage of the vacant 11 metre antenna, and used it for an ambitious survey of the whole southern sky and part of the northern sky at 600 MHz. This was one of the most comprehensive all-sky surveys conducted since Reber's pioneering efforts in the 1940s, and its angular resolution was an order of magnitude higher than the 100 MHz map produced at Dover Heights.

One of the most interesting radio telescopes erected at Potts Hill was the prototype Mills Cross (Figure 22), built by Mills and Little in 1952 and early 1953 to test out the cross-type telescope concept. At the time, Mills (1976) felt that he "… had to convince people it would work, and there were also a number of basic problems I wasn't quite clear about myself which I wanted to experiment with …" The new radio telescope consisted of N-S and E-W arms, each 36.6 metres (120 feet) in length and containing 24 half-wavelength E-W aligned dipoles backed by a wire mesh reflecting screen (Mills and Little, 1953). This novel instrument operated at 97 MHz, and had an 8° pencil beam which could be swung in declination by changing the phases of the dipoles in the N-S arm. The success of the 'Potts Hill mini-cross' was to justify the founding of a new RP field station, at Fleurs.

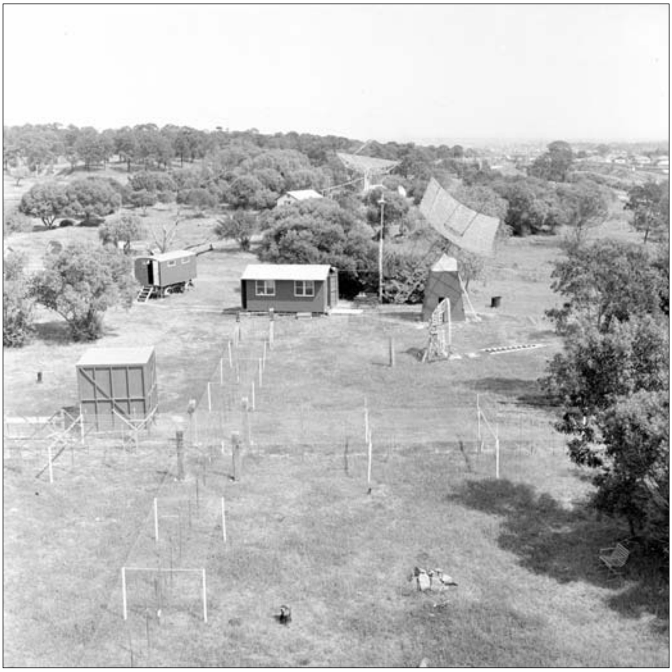

Figure 22. The prototype Mills Cross (foreground), and in the background the ex-Georges Heights radar antenna, the 11 metre H-line dish, and various instrument huts (ATNF: 3171-4).

The final radio telescope erected at Potts Hill was a simple 19.7 MHz antenna that was completed in 1956. The dipoles were suspended between telegraph poles, and the ground served as a reflector. This antenna was used by Shain and Gardner in conjunction with the Shain Cross at Fleurs to carry out simultaneous observations of Jupiter (although no attempt was made to link the two as a long-baseline interferometer).

After making important contributions to solar, Galactic and extragalactic radio astronomy, the Potts Hill field station closed in 1963; most of the remaining Galactic and extragalactic programs there were transferred to Parkes.

2.5 Badgerys Creek

This field station was located 50 km west-south-west of central Sydney on a Commonwealth Scientific and Industrial Research Organisation cattle research station (Figure 1b), and was founded by Bernie Mills at the end of 1949 so that he could study discrete sources free from the electrical interference that had plagued him previously at Potts Hill at 101 MHz.

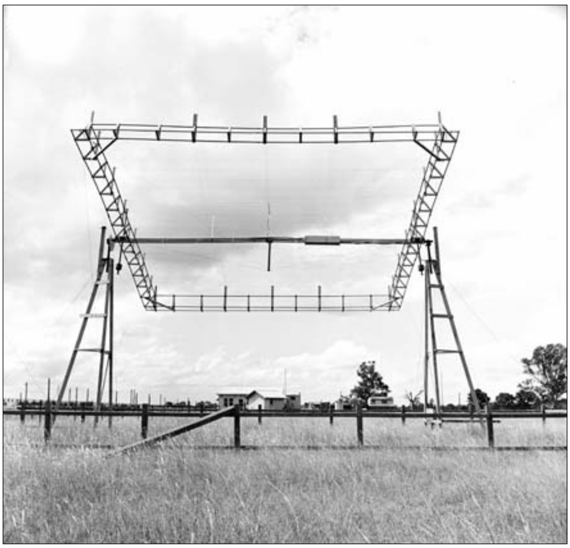

Figure 23. One of the three 101 MHz broadside antennas at Badgerys Creek (ATNF: B3923-7).

Initially there were three identical radio telescopes at this site: broadside arrays positioned along an E-W line, and mounted so they could be titled about their E-W horizontal axes (Figure 23). Each antenna contained 24 half-wave dipoles, backed by a reflecting screen. Antenna 2 was located about 60 metres to the east of Antenna 1, with Antenna 3 a further 210 metres to the east. Two different receivers (Figure 24) were used with the antennas, so that the outputs of any two aerial spacings could be recorded simultaneously. Between February and December 1950 Mills used this interferometer to conduct a survey of the Galactic distributions of discrete sources. The 76 sources catalogued fell into two major classes,

> … the stronger sources being closely confined to the Galactic plane (Class I) and the weaker sources apparently randomly distributed over the sky (Class II) … The latter class included known extra-galactic members, some of which were of appreciable angular size … It could not be determined, however, whether this class was compos-

> ed entirely of galaxies like those identified, whether it included other classes of extragalactic sources, or, indeed, whether it included a proportion of the conventional "radio stars". (Mills, 1984: 150).

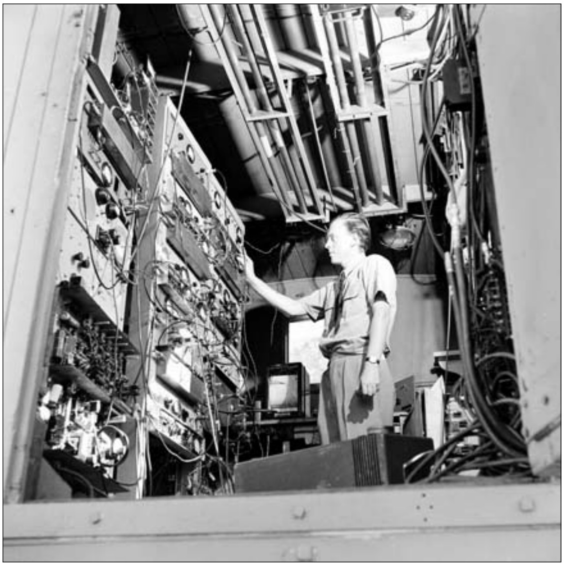

Figure 24. Bernie Mills and the receiver used with the three-element interferometer (ATNF: B2774-1).

Mills then wanted to study the positions and angular sizes of some of the strongest Galactic sources, and to do this a two-element variable baseline interferometer was set up. This used one of the three broadside arrays plus a mobile two-element Yagi array (operating at 101 MHz) and a radio link. This was the first time such a link had been used in radio astronomy, and to Mills (1976) was simply a matter of logic: "If we wanted to try different spacings and different places, then obviously you couldn't go coiling and uncoiling miles of cables. The radio link was the obvious way of doing it …" During 1952, observations were made of four strong discrete sources at nine different E-W spacings (ranging from 0.3 to 10 km), and three spacings in other directions. At the end of this project Mills transferred to Potts Hill, where he would build the world's first cross-type telescope. Much later, he explained that "This [Badgery's Creek] survey was actually the basis for the Cross because I realized that it was necessary in any survey to have an instrument which would respond to close spacings and large angular size structure. Otherwise, one would simply miss it and miss a lot of the information available in the sky. And it was as a result of this survey that I thought of the Cross as being the sort of thing one must use. One must use pencil beams for surveys. That was the basic idea I had in mind." (Mills, 1976).

Meanwhile, the Badgery's Creek field station was retained by RP and was subsequently used by some of the radio astronomers based at Fleurs. It was finally closed down in 1956.

2.6 Penrith and Dapto

In 1948-1949, Paul Wild needed a radio-quiet site in the general vicinity of Sydney where he could study the spectra of solar bursts, and this is how the short-lived Penrith field station came into existence. It was located on farm land near Penrith railway station. At this time, Penrith was a small town 50 km west of central Sydney, at the foot of the Blue Mountains (Figure 1b); now it marks the western boundary of greater Sydney, with its bustling population of more than 4 million.

Figure 25. The 70–130 MHz rhombic aerial at the short-lived Penrith field station (ATNF: B2086-1).

Apart from a motley collection of huts, this field station featured a single rhombic antenna (Figure 25) that was anchored at one end. In order to follow the Sun, the aerial was moved every twenty minutes or so by making adjustments to a number of different guy ropes. With this one antenna, solar radio emission was received by sweeping over the frequency range 70–130

MHz, and was displayed on a cathode ray tube where it was photographed. Successive photographs could be taken at intervals of one-third of a second, which allowed the radio astronomers to investigate the ways in which burst intensity changed with frequency and with time (Wild and McCready, 1950). Apparently it was Pawsey who suggested using a rhombic aerial for this world's first radio spectrograph, while Bowen came up with the idea of the swept-frequency receiver (as he was familiar with their use in a WWII radar context).

The first serious scientific observations were made in February 1949, and by the end of June spectra of three different types of solar bursts had been constructed from the photographs (and producing these spectra manually was a very trying and time-consuming process—today it would all be done automatically by computer)! These were designated Types I, II and III and described and discussed in a series of four papers written by Wild and published in the *Australian Journal of Scientific Research* in 1950-1951. Type I bursts occurred in large numbers (hundreds, or more typically thousands) during so-called 'noise storms', which usually lasted for many hours, or even days. Bursts normally came in small discrete groups, were strictly localized in frequency (most had bandwidths between 3 and 5 MHz) and in time (typically 1-8 seconds), and showed strong circular polarization. Type II bursts (more properly, 'outbursts') were major events and were rare. They lasted several minutes, and had clearly-defined upper and lower frequency boundaries at any one instant. The emission drifted from higher to lower frequencies with the passage of time at a mean rate of ~0.22 MHz per second. Type II bursts were often associated with solar flares. A third distinct group of bursts belonged to Type III, characterised by narrow-band events that only lasted a few seconds and drifted rapidly from high to low frequencies (at mean rates of ~20 MHz per second). Type III bursts were particularly common, and sometimes occurred in groups near the start of solar flares.

With the potential of the radio spectrograph proven, the search was on for a more 'radio-quiet' site where further antennas could be set up. A reconnaissance trip down the New South Wales south coast revealed Dapto, a sleepy valley with a dairy farm 80 km from Sydney, and shielded from Sydney and Wollongong to the north by surrounding hills (Figures 1a and 1b). With the passage of the years, Dapto would play a key role in the international development of solar astronomy, and apart from Wild, other notable RP radio astronomers associated with this field station included John Murray, Jim Roberts, Kevin Sheridan, Steve Smerd and, in later years, Shigemasa Suzuki.

Initially, the radio telescopes at Dapto comprised three different crossed-rhombic aerials in a N-S line, which covered the frequency ranges of 40–75, 75–140 and 140–240 MHz, respectively. Each aerial was supported by an equatorial mounting, and could track the Sun. Meanwhile, the crossed-configuration allowed different polarization measurements to be taken. Inside the receiver hut the signals from the three aerials went to three swept-frequency receivers, and then to cathode ray tubes where they were photographed with cine cameras. This ingenious system allowed a complete spectrum to be obtained every half-second. Development of the aerials and the supporting receivers took time, so the three solar radio spectrographs only became operational in August 1952 (see Figure 26). Between 1958 and 1963 four new rhombic antennas were added, allowing the lowest frequency received to successively be reduced from 40 MHz to 25 MHz (in 1958), 15 MHz (in 1960) and finally 5 MHz (in 1961). Then in 1963 a 10 metre (33-ft) parabolic dish with a log-periodic feed was installed, and the upper frequency limit was extended from 210 MHz to 2,000 MHz.

Figure 26. The Dapto field station, showing the three crossed-rhombic antennas and associated buildings (ATNF: B5279-3).

Soon after the field station became operational, it was noticed that some Type II and Type III bursts exhibited harmonic structure, with a near mirror image of the initial burst following in close succession at a frequency separation of 2:1. In addition, by 1958, three further spectral classes of events had been identified: Type IV noise storms, Type V bursts and "Reverse drift pairs". Type IV noise storms, well-documented at Dapto but first described by a French radio astronomer, were rare continuum events, characterized by a high-intensity broadband featureless spectrum and linear polarization. They lasted from around half an hour to six hours, and generally occurred after Type II bursts. Type V bursts looked like Type IIIs but with broadband continuum 'tails' that lasted anywhere from half a minute to three minutes and were associated with between 25% and 33% of all Type III bursts. "Reverse drift pairs" (RDPs) were rare very short-duration bursts seen only below 50 MHz, and occurred in pairs separated in time by only 1.5 to 2 seconds. The pairs typically drift rapidly from lower to higher frequencies at rates of 2-8 MHz per second. RDPs tended to occur in storms lasting from hours to days, and about ten percent were associated with weak Type III bursts.

Typical examples of the various Dapto spectral types are illustrated in Figure 27, and this scheme was soon adopted by solar radio astronomers worldwide.

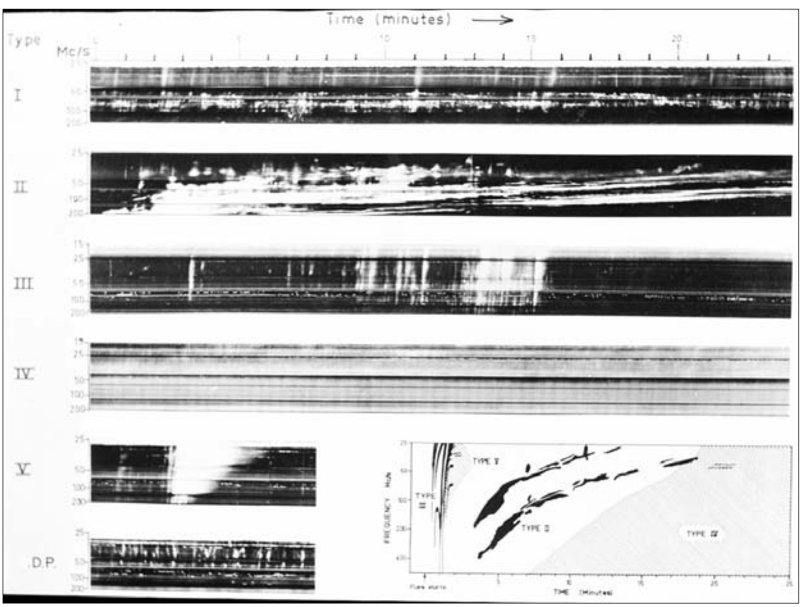

Figure 27. Typical examples of the six different spectral types of solar bursts identified at Dapto, with a schematic (bottom right) that summarizes their relative features (ATNF: 6317).

In 1957, three further rhombic antennas were installed at Dapto. Two of these were used as an interferometer to record real-time changes in the positions and sizes of different burst sources over the frequency range 40-70 MHz, while the third antenna (a crossed-rhombic) could track the Sun and record the polarization of bursts (Wild and Sheridan, 1958). The system was set up so that the operator could manually switch between the position interferometer and the polarimeter, as required. After passing through the receivers the signals were initially displayed on a 12-inch cathode ray tube and photographed with a 35-mm cine camera, but in 1959 this arrangement was altered and the results were combined and preserved as a facsimile record (see Figure 28).

Figure 28. Kevin Sheridan and the position interferometer receiver (Sullivan: Dapto 02).

Members of RP's illustrious Solar Group then used data from the radio spectrographs, the position interferometer and the polarimeter to produce a succession of seminal research papers on the properties of the different types of solar bursts, and this is surely Dapto's greatest legacy. For more than a decade this field station was at the forefront of international solar radio astronomy, and through its innovative instrumentation (Sheridan, 1963) was able to provide important new information on coronal properties, solar burst generation, and the association between solar radio emission and photospheric and chromospheric events and features (see Smerd, 1964; Wild, Smerd and Weiss, 1963). In this, the impact of Wild's leadership cannot be underestimated. When quizzed on this, Steve Smerd (1978) was moved to comment: "Wild built up a group which was quite unusual compared to all the other research teams that I have seen. I think our solar group under Paul was perhaps the happiest, frictionless collection of people you can imagine. We were keen and dedicated and would have done anything that Paul even half-mentioned or suggested, let alone explicitly asked."

The Dapto facility was closed down in 1965, and this sad event also marked the end of a proud tradition at RP: those unforgettable Dapto parties (see Figure 29). Some of the Dapto antennas were relocated to Culgoora, site of the Division's 'next generation' forefront solar radio telescope, the Culgoora Radioheliograph (Wild, 1967), but most remained at Dapto and were inherited by the University of Wollongong when it took over the field station.

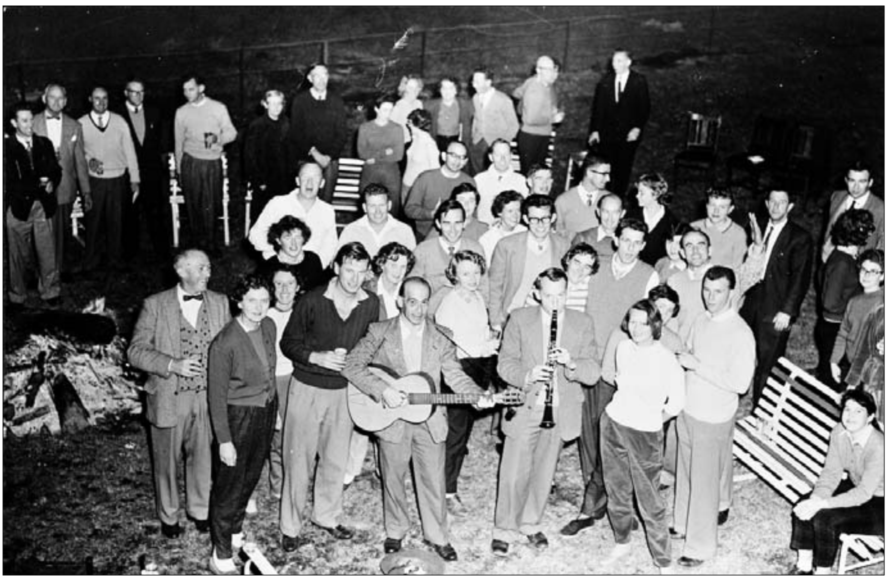

Figure 29. A typical Dapto party showing Steve Smerd with his guitar, and immediately to his right Paul Wild, Head of the RP Solar Group (ATNF: B5865).

2.7 Fleurs[2]

One of the last Radiophysics field stations set up in the pre-Parkes era was at Fleurs (Orchiston and Slee, 2002b), the site of a WWII airstrip 40 km west-south-west of central Sydney (Figure 1b). Situated on an expanse of flattish land between two streams, Fleurs was home to three different cross-type radio telescopes. Leading radio astronomers associated specifically with this site were Alan Carter, Chris Christiansen, Eric Hill, Norman Labrum, Alec Little, Bernie Mills, Dick Mullaly, Alex Shain, Kevin Sheridan and Bruce Slee, assisted by people like Frank Gardner, Bruce Goddard, Charlie Higgins, Wayne Orchiston (briefly) and Arthur Watkinson.

The first radio telescope at Fleurs was the Mills Cross (see Mills, et al., 1958), which was constructed during 1953-1954 following the success of the Potts Hill small-scale prototype. The Fleurs Mills Cross had 460 metre long N-S and E-W arms, each containing 250 half-wave dipoles (Figure 30). Operating at 85.5 MHz, and with a 49′ beam (in those days regarded as remarkable!), the Mills Cross was effectively a transit instrument that relied on Earth-rotation, but by altering the phasing of the dipoles in the N-S arm it was possible to observe different regions of the sky. Signals from the two arms were channeled to the central hut where the receivers and other equipment were located (see Figure 31).

Figure 30. Close-up of the centre of the Mills Cross, showing individual dipoles, and the receiver hut and microwave link used for long baseline interferometry. In the background are some of the dishes of the Chris Cross (B5689-8).

This unique new radio telescope was used for three different projects. Soon after its inauguration, Mills, Little and Sheridan searched for radio emission from specific types of celestial objects, including known novae, supernovae and planetary nebulae, but were spectacularly unsuccessful. The only object they could detect was the radio source associated with Kepler's supernova of 1604.

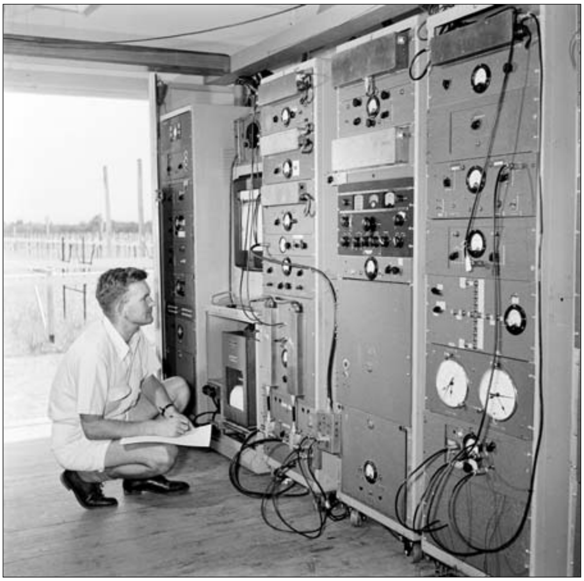

Figure 31. View inside the Mills Cross receiver hut showing Bruce Slee and some of the equipment (ATNF: B3868-10).

Sheridan also used the Mills Cross to study some of the strongest known radio sources, and was able to produce isophote maps for Centaurus A, Fornax A, and Puppis A. By today's standards these are crude, but in 1957-1958 they were regarded as impressive.

These projects may have been interesting, but they were simply forerunners to the main work of the Mills Cross which was to produce a detailed survey of the sky at 85.5 MHz. The observations for this were carried out by Mills, Hill and Slee between 1954 and 1957, and in the process they recorded about 2,000 discrete sources, publishing their results in a series of research papers in the *Australian Journal of Physics*. Although a number of the sources in their famous 'MSH Catalogue' were associated with Galactic objects, the majority related to extragalactic nebulae, and this had profound cosmological implications in terms of the competing 'Big Bang' and 'Steady State' theories which were prevalent at the time and led to the notorious 'Fleurs-2C Controversy' (see Mills, 1984; Sullivan, 1990). When they compared the distribution and intensity of the MSH sources with those listed in the Cambridge 2C catalogue, Mills and Slee found there was very little correspondence (see Figure 32). They immediately came up with resolution effects as the explanation, but when Mills tried to raise this with Ryle his letters went unanswered. Later he was to remark: "We were a bit fed up with the Cambridge attitude at this time, I might say … They just ignored us. So we went ahead and did what we felt had to be done." (Mills, 1976). This was to publish a paper reporting the Fleurs-2C discrepancies (Mills and Slee, 1957), and explain them away largely in terms of shortcomings associated with the Cambridge interferometer. As might be anticipated, this led to bad blood between British and Australian radio astronomers, and it was some years before the Cambridge scientists finally recognized that their survey had serious shortcomings.

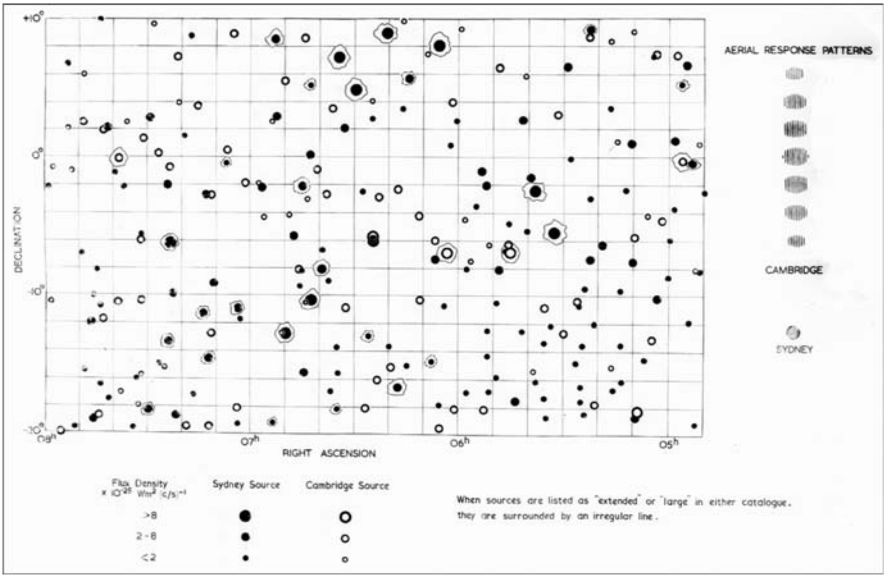

Figure 32. Comparison of sources recorded in the MSH (filled circles) and 2C (open circles) catalogues (ATNF: 5073-12).

Evolving out of the Mills Cross survey were a number of other projects that related directly to the MSH Catalogue. In 1954 and 1955, Carter used a 101 MHz broadside array at Fleurs and a similar antenna at Badgerys Creek to investigate the variable radio source, Hydra A, and the sizes of weaker sources in the MSH Catalogue. In 1958 and 1959, Goddard, Watkinson and Mills also used long baseline interferometry to research the sizes of these smaller sources at 85.5 MHz. Their observations were made with the E-W arm of the Mills Cross, a 91.4 metre section of the S arm of the Cross, and an identical 50-dipole N-S array at Wallacia 10 km to the west (Figure 1b). Then during 1961 and 1962, Slee and visiting Cambridge radio astronomer, Peter Scheuer, used the E-W arm of the Mills Cross and barley-sugar arrays erected temporarily at Cumberland Park, Rossmore, Llandilo and Freeman's Reach (respectively 6 and 10 km south, and 17 and 32 km north of Fleurs—see Figure 1b) and connected to Fleurs by a radio link to research the sizes of selected sources in the MSH catalogue.

The Mills Cross was also used for a number of other studies involving the use of discrete sources to probe the solar wind. In 1956, 1957, 1958 and 1960 Slee used elements of the Mills Cross to carry out four different studies relating to electron density irregularities in the solar corona. His fourth, and most ambitious, project took place between June and October 1960 when he used the E-W arm of the Mills Cross and the Wallacia array (mentioned above), to observe different discrete sources as they passed close to the Sun. The results were spectacular: “Sporadic large increases in the scattering first became noticeable when the angular separation was as much as 100 $R_{\odot}$ and at separations of less than 60 $R_{\odot}$ the effects of scattering could be detected on every record.” (Slee, 1961: 225). Figure 33 provides a graphical illustration of this result; open circles indicate days when the source interference fringes appeared to be unaffected by scattering, while filled circles refer to days when fringe amplitudes were greatly reduced. The dashed line indicates the region with the majority of filled circles, where the average scattered distribution exceeded 30″ to half-power points in the E-W direction.

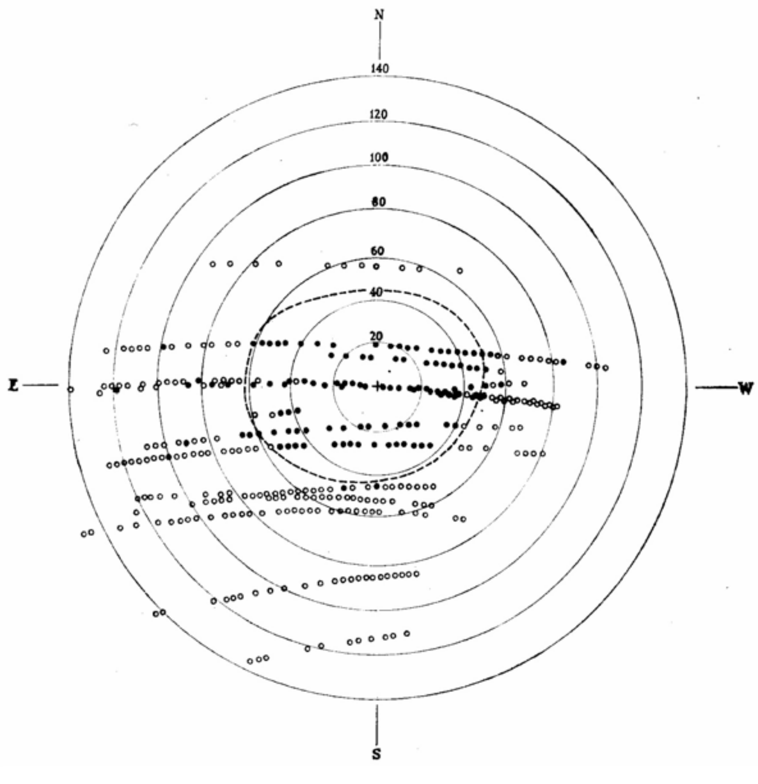

Figure 33. Diagram showing coronal scattering (filled circles) suffered by thirteen different discrete sources observed in 1960. The Sun is marked by the cross, and coronal scattering is primarily restricted to the region enclosed by the dashed line (ATNF: B6328-1).

Slee was also involved in the other post-MSH Catalogue observations of considerable significance. Between September 1960 and May 1962 he and Higgins used the N-S arm of the Mills Cross to carry out pioneering observations of radio emission from UV Ceti, a nearby dMe flare star (Slee, Higgins, and Patston, 1963). Parallel observations were made with the neighbouring Shain Cross at 19.7 MHz, and with the Parkes Radio Telescope at 408 MHz. Slee and Higgins, plus England's Sir Bernard Lovell, must be credited with the discovery of the first *genuine* 'radio stars' (see Orchiston, 2004a).

During 1955 three low-frequency radio telescopes were constructed at Fleurs, namely a 19.6 MHz two-element E-W interferometer (each aerial consisting of four full-wave dipoles suspended between telegraph poles) and 14 MHz and 27 MHz single in-line arrays of four and eight half-wave dipoles respectively. Shain and Gardner used these antennas during 1955-1956 to investigate burst emission from Jupiter. Jovian emission was recorded at all three frequencies, but was most common at 19.6 MHz where a rotation period for the source of 9h 55m 34s was derived—close to the earlier result derived from Hornsby Valley. However, the 19.6 MHz and 27 MHz data tended to indicate the presence of three different sources: the main one at a Jovian longitude in System II of 0° and two much less active secondary sources at longitudes of –100° and +80°. Shain and Gardner suggested the the bursts resulted from plasma oscillations in an ionized region of the Jovian atmosphere.

In 1956, just two years after the Mills Cross was operational, the Shain Cross was completed. Shain was largely responsible for this, but Little helped with the antenna design and Sheridan with the receiving equipment. This large new radio telescope (Shain, 1958) was built alongside the Mills Cross, operated at a frequency of 19.7 MHz, and had a beam width of 1.5°. It evolved out of Shain's earlier exploits at the Hornsby Valley and the 19.6 MHz interferometer at Fleurs, and drew inspiration also from the Mills Cross concept. The N-S arm was 1,151 metres in length and contained 151 dipoles, while the E-W arm was a little shorter, at 1,036 metres, with 132 dipoles. The dipoles were 4 metres above the ground and strung between telegraph poles, with the ground serving as a reflector (Figures 34).

Initially Shain used this new radio telescope to survey 19.7 MHz emission in the Galactic plane. Observations of a strip of sky extending ±10° from the Galactic equator, showed a conspicuous dip in the chart records, indicating that "… absorption of 19.7 Mc/s radiation is occurring in a band of HII regions near the galactic plane." (Shain, 1957: 198), and when an isophote map was prepared this showed Sgr A in absorption. Shain also investigated selected strong sources, producing 19.7 MHz isophote maps for Centaurus A and Fornax A, which looked rather similar to those generated at 85.5 MHz by Kevin Sheridan.

Figure 34. Looking south along the N-S arm of the Shain Cross. On the left is the N-S arm of the Mills Cross, and the broadside antenna used by Alan Carter for his long baseline study of source sizes (ATNF: B3868-19).

Shain also planned to observe Jupiter with the new Cross and arrange for simultaneous optical monitoring with a view to identifying any optical features that could conceivably be associated with the radio emission, but his untimely death in 1960 put paid to these plans. Instead, it was left to Slee and Higgins to take up the Jovian challenge. In August 1962 they erected a square array of 19.7 MHz dipoles at Fleurs and an identical array at Freemans Reach, 32 km to the north (Figure 1b), in order to investigate the size of the region responsible for the Jovian bursts, and in 1963 and 1964 they expanded this project by setting up radio-linked arrays at Dapto and Jamberoo far to the south of Sydney, and at Heaton, near Cessnock, to the north (see Figure 1a). Analysis of the observations suggested that the emitting regions were typically 10-15 seconds of arc in size, but Slee and Higgins concluded that they were probably very much smaller and that scattering in the interplanetary medium gave anomalously large angular sizes. This conclusion turned the Radiophysics Jovian decametric project in a new direction: what had started as a quest for emission source size now became an investigation of scattering by the interplanetary medium. Slee and Higgins then used their 1963-1964 data on burst arrival times, burst rates, angular position scintillations and apparent angular size to successfully investigate interplanetary diffraction patterns and electron irregularities in the solar wind. This study would mark the final contribution of the Fleurs site to planetary radio astronomy.

The Shain Cross was also used for one other important, albeit short-lived, project: from September to December 1961 Slee and Higgins used the N-S arm to search successfully for 19.7 MHz radio emission from selected flare stars (Slee, Solomon, and Patston, 1963). Parallel observations were made with the neighbouring Mills Cross at 85.5 MHz.

Figure 35. The centre of the Chris Cross, looking south along the N-S arm (ATNF: B9097-12).

Fleurs gained its third large radio telescope in 1957—just in time for the International Geophysical Year—when a major new solar array, the Chris Cross, was constructed (Orchiston, 2004c). Named, appropriately, after 'Chris' Christiansen, this innovative instrument represented an amalgamation of his Potts Hill solar grating arrays and the Mills Cross concept (Christiansen et al., 1961). Most visually-appealing of all early Australian radio telescopes (Figure 35) this array comprised 433 metre long N-S and E-W arms each containing 32 parabolic equatorially-mounted dishes

5.8 metres (19-ft) in diameter. Antennas in the E-W arm produced a series of N-S fan beams, and antennas in the N-S arm a series of E-W fan beams. Electronically combining the signals received from the two arms produced a network of pencil beams at the junction points of the fan beams. Each pencil beam was 3′ in diameter and was separated from its neighbour by 1°, so the Sun could never be in more than one pencil beam at any one time.

The Chris Cross operated at 1,420 MHz, and was built to explore the nature and evolution of radio plages (Figure 36), research those rarely-observed bursts seen at this comparatively high frequency, and investigate the distribution across the solar disk of emission from the quiet Sun. It was the first radio telescope in the world to generate daily two-dimensional high-resolution radio images of the Sun, and in focusing primarily on non-burst emission was the perfect complement to the Dapto radio spectrographs.

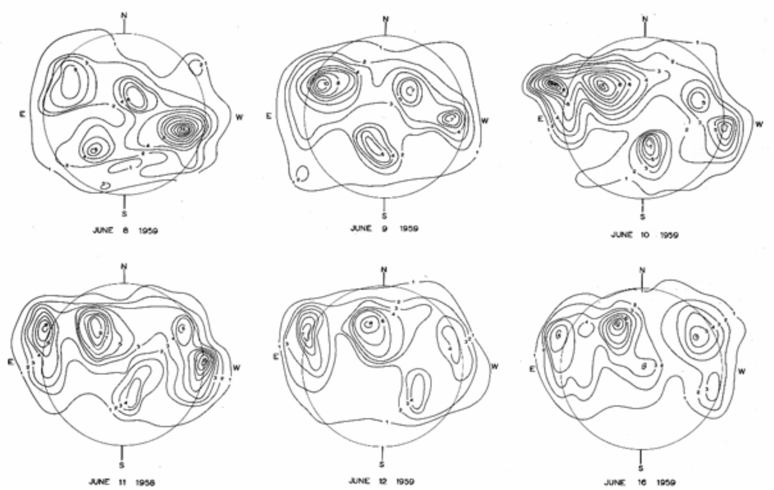

Figure 36. A series of daily 1,420 MHz solar maps showing the presence of several radio plage regions. (ATNF: B6453-3).

From their accumulated observations, Christiansen and his colleagues found that radio plages had typical diameters of 2-6′ (representing from 10^5 to 3×10^5 km in actual areal extent), were situated from 3×10^4 to 10^5 km above the photosphere (with an average height of $\sim 4 \times 10^4$ km), and had peak temperatures of $<2 \times 10^5$ up to $\sim 1.6 \times 10^6$ K (with a median value of $\sim 6 \times 10^5$ K). Christiansen and Mullaly (1963: 171) concluded that radio plages "… consist of large clouds of gas (principally hydrogen) … [that are] much denser than the surrounding atmosphere … [and] are prevented from dissipating presumably by magnetic fields …" The virtual absence of circular polarization indicated that the emission was thermal in origin.

The final phase of development at Fleurs occurred in 1959 when an 18 metre (60-ft) prefabricated American antenna, known colloquially as the 'Kennedy Dish', was installed at the eastern end of the E-W arm of the Chris Cross (Figure 37). This array, known as the Fleurs Compound Interferometer, had a 1.5′ fan beam, and it was used at times when the Sun was not being observed to investigate 1,420 MHz emission from some of the brightest radio sources.

Figure 37. View looking west along the E-W arm of the Chris Cross, showing the recently-installed 'Kennedy Dish' (ATNF: B6499-5).

A major change took place at Fleurs at the end of 1962 when the Kennedy Dish was transferred to Parkes (see Figure 1a), to be used in conjunction with the 64 metre Parkes Radio Telescope. By this time the research programs that justified the construction of the Mills and Shain Crosses had come to an end and Fleurs had served its purpose. No longer required as a field station, it was handed over to the University of Sydney, and the School of Electrical Engineering (under Professor Christiansen) spent the next decade converting the Chris Cross into the 'Fleurs Synthesis Telescope'.

In summary, during the ten-year interval, 1954-1963, Fleurs was one of the world's foremost radio astronomy sites, and it played an important role in furthering solar and non-solar radio astronomy. Bowen (1984: 97) would go so far as to claim that the three Fleurs cross-type radio telescopes "… were among the great successes of the 1950's and were responsible for a large part of the Division's research output over that period." They consolidated the international standing of Christiansen and Mills, helped build the emerging reputations of people like Shain, Sheridan and Slee, and served as stepping stones to the Division's next major advances in instrumentation: the Parkes Radio Telescope and the Culgoora Radioheliograph.

2.8 Murraybank

The Murraybank field station was located at suburban West Pennant Hills (Figure 1b), on an orchard ('Rosebank') owned by the father of RP radio astronomer, John Murray, and was set up in 1956 in order to carry out H-line observations with a new purpose-build radio telescope and receiver. Apart from John Murray, Dick McGee was the other radio astronomer who spent time at this field station.

At 6.4 metre (21-ft), the Murraybank radio telescope (Figure 38) was considerably smaller than its Potts Hill predecessor, but its altazimuth mounting meant that interesting areas of the sky could be accessed at will. At 1,420 MHz, the beam width was 2.2°.

There was also marked improve-ment in the ancillary instrumentation, as the old Potts Hill 4-channel unit was replaced by a 48-channel receiver built mainly by Murray and McGee. This contained 44 separate narrow-band channels, spread at 33 KHz intervals across the H-line frequency of 1,420.9 MHz, and four wide-band channels at either end of the range, which were used to obtain reliable zero levels (see McGee and Murray, 1963).

Initially the Murraybank facility was used by McGee and Murray to investigate the distribution of neutral hydrogen in the Taurus-Orion region, as a test of the overall system. More than 3,500 H-line profiles were obtained, and the level of emission suggested that a large single neutral hydrogen cloud or an association of connected clouds spans the Taurus-Orion region and that this is rotating as part of the general structure of the Galaxy.

McGee and Murray followed up this localized study with a survey of the distribution of neutral hydrogen over the whole sky visible from Sydney and found that in general the gas was stratified parallel to the Galactic plane, and concentrated in a number of massive spiral arms.

One of the problems encountered with the Murraybank receiver was that a complete H-line profile could be obtained in just two minutes, sixty times more quickly than at Potts Hill with the old equipment. Reduction of the large bodies of observational data obtained therefore posed a major challenge, and this prompted the development of a digital data-recording system that would ultimately see extensive use with the Parkes Radio Telescope (Hindman, et al., 1963). However, this innovative system was first trialed at Murraybank during a low resolution H-line survey of the Magellanic Clouds. The digital recording and data handling system successfully converted 250 hours of observations to printed profiles in just eight hours of computer time, but more than this, the survey reinforced the earlier finding of Kerr, Hindman and Robinson that an extensive gaseous envelope enclosed both Magellanic Clouds. The total mass of hydrogen in the two Clouds derived in the earlier study was also confirmed. An interesting new discovery was the detection of a tenuous 'bridge' of hydrogen gas between the Large and Small Magellanic Clouds.

For the Radiophysics Division, Murraybank served an important role as the test-bed for the innovative 48-channel H-line receiver, and this field station only closed down when the receiver was transferred to the newly-opened Parkes Radio Telescope at the end of 1961.

Figure 38: The Murraybank field station, showing installation of the 6.4 metre parabola, and the adjacent building which housed the multi-channel H-line receiver (ATNF: B3973-4).

3. CONCLUDING REMARKS

During the critical fifteen years from 1946 to 1961 Australia played a key role in the international development of radio astronomy (Mills, 1988; Orchiston, Sullivan and Chapman, 2005; Pawsey, 1953, 1961; Sullivan, 1988), largely through the research that was carried out at the Radiophysics Laboratory and the nine different field stations located in or near Sydney and Wollongong.

In addition to standard Yagis and parabolic dishes, innovative new types of instruments were invented, including solar radio spectrographs, solar grating arrays, cross-type radio telescopes, H-line multi-channel receivers and an assortment of long baseline interferometers (see Christiansen, 1959; Mills, 1963; Pawsey and Bracewell, 1955; Wild, 1953).

Collectively, the radio telescopes at the field stations and associated remote sites were used to address a wide range of research problems, and important contributions were made to solar, Jovian, Galactic and extra-galactic radio astronomy (e.g. see Bolton, 1955; Haynes, et al., 1996; Kerr and Westerhout, 1965; Mills, 1959; Pawsey, 1950; Pawsey and Hill, 1961; Pawsey and Smerd, 1953 Sullivan, 1984), largely through the key roles played by such luminaries are John Bolton, Chris Christiansen, Frank Kerr, Bernie Mills, Joe Pawsey, Ruby Payne-Scott, Alex Shain, Bruce Slee, Gordon Stanley and Paul Wild. It is a sobering thought that more than half of these are no longer with us.

Just as the world's pioneering radio astronomers are being taken from us, few of our pioneering radio telescopes have survived. Visits to the sites of the Badgery's Creek, Dapto, Georges Heights, Hornsby Valley, Murraybank, Penrith and Potts Hill field stations quickly reveal a total absence of instrumentation, buildings, or relevant earthworks, and only Dover Heights and Fleurs hold any promise.

At Dover Heights the landscape has changed markedly since the field station days, and the site is now a reserve and playing field. However, the badly-rusted remains of the mounting that once supported an 8-Yagi and later a 12-Yagi sea interferometer survives, and during the July 2003 IAU General Assembly staff from the Australia Telescope National Facility installed a replica of the 8-Yagi array near it, along with a commemorative plaque and historical panel display (see Orchiston, et al., 2005). Those visiting Dover Heights can now get a feel for the astronomical importance of this famous site.

For its part, Fleurs retains some of the 13.7 metre (45-ft) antennas that formed part of the Fleurs Synthesis Telescope, but the twelve centrally-located original Chris Cross dishes that were preserved and refurbished by University of Western Sydney Engineering staff and students in 1991 (see Orchiston, 2004c) deteriorated rapidly after the close-down of the site in 1998, and—unbeknown to the authors—were recently bulldozed. This was a tragic loss for Australian radio astronomy.

The only sites where historic RP radio telescopes survive *in situ* are Parkes, Culgoora and Marsfield. At Parkes are the 64 metre Parkes Radio Telescope and the 18 metre Kennedy Dish. While the former is still in regular used for forefront research, the latter antenna is in poor condition, but recently a policy decision was made by the Australia Telescope National Facility to retain and preserve it. Culgoora (Figure 1a) is the site of Australia's forefront synthesis instrument, the Australia Telescope Compact Array, but many of the 96 parabolas that once formed the Culgoora Radioheliograph and some of the antennas associated with the Culgoora radio spectrographs are still there—in varying stages of disintegration. Finally, the Headquarters of the Australia Telescope National Facility at Marsfield in suburban Sydney (Figure 1b) boasts a robustly-mounted 4 metre parabola that was acquired in 1974 and was used extensively over the following decade for molecular line work. All of these surviving radio telescopes played vital roles in the history of Australian astronomy and deserve to be documented and preserved while such action is still possible.

And this surely raises an important philosophical issue. We believe the time has come for historically-significant radio astronomical hardware to be shown the same respect and veneration that is enjoyed by optical instruments that have played a key role in the evolution of astronomy. Historically-significant radio telescopes and associated instrumentation (horns, feeds, polarization screens, receivers, chart records, signal generators, etc.) are important objects in their own right and they do deserve to be preserved. This is one of the charters of the IAU Working Group on Historic Radio Astronomy which was formed by Commissions 40 (Radio Astronomy) and 41 (History of Astronomy) at the 2003 General Assembly in Sydney.[3]

4. ACKNOWLEDGEMENTS

We are grateful to Professor Woody Sullivan for access to archival material used in this research, and to the ATNF for permission to publish many of the figures included in this paper.

5. NOTES

1. Christiansen (1976) recalls that 'Doc' Ewen came out to Australia in 1952 to attend the Sydney URSI Congress, to see the RP H-line receiver for himself, and "… to see how these God damn Australians did in three weeks what took [other] people eighteen months to do." When he saw the receiver, "… he just about passed out ..."!
2. This particular field station has special memories for both authors. W.O. operated the Chris Cross and produced the daily solar maps from 1962 up until the time when Fleurs was handed over to the University of Sydney, while Bruce Slee was closely associated with the Mills Cross and the Shain Cross throughout the life of the field station.
3. One of the authors (W.O.) is the Chairman of this Working Group.

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DARK MATTER AND THE OWENS VALLEY RADIO OBSERVATORY

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Abstract: Neutral hydrogen gas in a spiral galaxy generally extends far beyond the visible stars, and radio observations that can detect this hydrogen were used to study galactic rotation to great distances. Around 1970, radio astronomers showed that the rotation curves were flatter than Keplerian beyond the optically visible galaxy, and hence that they had to contain a large amount of invisible, or dark, matter. The interferometer at the Owens Valley Radio Observatory was able to study this in greater detail than was possible with the filled-aperture telescopes that existed then, and made the first curves that were unambiguously flat. Within a half-dozen years, however, the field became dominated by more powerful instruments. With these the rotation curves were extended and many more galaxies were measured, and by the late 1970s astronomers generally had become convinced of the need for dark matter.

Key words: Radio astronomy, Owens Valley Radio Observatory, dark matter, spiral galaxies

1. INTRODUCTION

Radio astronomical studies were a major factor in the general acceptance of the concept that dark matter is a major constituent of the Universe. In this paper I will discuss the role that the Owens Valley Radio Observatory had in helping to establish this idea.

The Owens Valley Radio Observatory (OVRO) was founded in 1955 by John Bolton and Gordon Stanley, both from Australia (see Kellermann, 1996; Kellermann, et al., 2005). In 1960 the first major instrument, the twin

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90-foot interferometer, was completed. It had two 90-foot diameter dishes that could be stationed at various points along two 1600-foot tracks, one east-west and the other north-south, and the operating wavelengths were 21 and 31 cm; the angular resolution was 2′ at 21 cm.

Studies of neutral hydrogen (HI) started immediately, using the 21-cm spin-flip transition in the hydrogen ground state. The first work was on HI clouds in the Galaxy, studied via their absorption of 21-cm radiation from background sources. In 1964 the equipment and techniques had improved enough that the more difficult task of studying HI emission could be undertaken. The first full synthesis map of the HI in a spiral galaxy (M 101) was published by Rogstad and Shostak (1971), and in 1972 they published rotation curves for five spirals, four from OVRO and one from the 300-foot telescope at NRAO (Rogstad and Shostak, 1972).

Rogstad and Shostak (1972) emphasized that all of these galaxies had rather flat rotation curves; i.e., they did not show evidence of a Keplerian falloff ($v \sim r^{-1/2}$) at large radii. The hydrogen clouds were not simply orbiting the interior galaxy of stars, the way the planets orbit the Sun. Rather, they had to be embedded in a much more extensive mass distribution, one that was essentially invisible. This is described by saying that the mass-to-light ratio (M/L) increases with distance from the center of the galaxy. (For the Sun, M/L is defined as unity; most stars are dim and have M/L > 1)

A requirement for dark matter was already seen in 1933, although in a different context. Fritz Zwicky (1933) found that the velocity distribution in the Coma cluster of galaxies, when interpreted with the virial theorem from classical dynamics, required a gravitational field that could not be supplied by the visible galaxies. His conclusion was that the Coma cluster had a high value of M/L, and thus contained a great deal of ‘dunkle Materie’. A similar result was obtained by Smith (1936) for the Virgo cluster.

Early optical work on the rotation of galaxies is reviewed by several authors (e.g. see Faber and Gallagher, 1979). In the 1950s telescopes and detectors became more sensitive and rotation curves for spiral galaxies, especially the large and bright Andromeda Nebula (M 31), were published. Rubin and Ford (1970; see Figure 1 for an early photo of Vera Rubin) showed a rotation curve for M 31 that extended to about 24 kiloparsec (kpc). It had only a few points at that distance but there did not seem to be any Keplerian falloff, and the authors fitted the data with a polynomial that went flat at 24 kpc. This was an interesting departure from the normal procedure at the time, which was to fit the data to a ‘Brandt curve’ which contained several constants to be determined empirically, and which would asymptotically go to a Keplerian curve. Users of the Brandt curve implicitly assumed that the mass of the galaxy was confined to some central region; beyond that Kepler's orbit law would hold. This has not been borne out, for in most spiral galaxies the rotation curve remains flatter than Keplerian out to the modern limits of detection of HI.

Figure 1. Vera Rubin (photograph courtesy V. Rubin).

2. EARLY RADIO STUDIES

The HI generally extends well outside the optical image of a galaxy, and so radio studies are favored for studying rotation curves. Early radio astronomical work used rather small dishes with resolutions ~0.5°, and consequently the results were of limited accuracy. However, this work did produce a rotation curve for M 31 that agreed well with later measurements. When large paraboloids were built around 1960 more detailed and more accurate measurements became available. In 1966 two extensive studies of M 31 were published, one from the 300-foot dish at Green Bank, West Virginia (Roberts, 1966), and the other from the 250-foot dish at Jodrell Bank, England (Gottesmann, et al, 1966). These gave similar results. The rotation curve was defined to about 25 kpc, but did not give any hint of a

Keplerian fall-off. They showed, in agreement with earlier work starting with Babcock (1939), that in M 31 M/L increases with radius. At the same time, the 210-foot dish at Parkes, Australia, was used to study HI in several rotating irregular galaxies, including NGC 300 (Shobbrook and Robinson, 1967). They showed that the HI extended beyond the optical image, and used various models to derive rotation curves. Freeman (1970) interpreted these results to show that there had to be "... undetected matter beyond the optical extent of NGC 300."

None of these papers referred to the work in the 1930s by Zwicky and Smith on clusters of galaxies. The later authors apparently did not connect the need for dark matter in clusters of galaxies with the similar need in individual spiral galaxies. That separation generally persisted until two theoretical papers were published in 1974, as discussed below.

At OVRO the first attempt to measure rotation curves of galaxies was made with the two 90-foot dishes separated by 100 feet east-west. Interference fringes were measured independently in 12 frequency channels spanning the HI emission line, and from the phase of the fringes the angular position of each frequency (i.e. velocity) could be found, with an accuracy generally between 1 and 2 minutes of arc. This gave seriously incomplete interferometer data, but in many cases a model-dependent rotation curve could be established. In this way Rogstad, Rougoor and Whiteoak (1967) studied HI in 30 galaxies.

It is interesting that the OVRO interferometer technique just described is closely analogous to the optical interferometers that Michelson (1920) built on the telescopes at Mt. Wilson. In both cases the interferometer could fit inside the large telescope (the optical interferometer is literally inside the aperture) and obtains information below the diffraction limit of the full aperture. However, the radio measurement gets small-scale *position* information from the phase, which was unavailable to Michelson.

The primary beam of the 90-foot dishes was 32′, and the OVRO system was best suited for galaxies whose angular size was smaller. (Mosaic techniques to map large fields were only developed many years later.) The beams of the 300 and 210 foot dishes were 10′ and 13.5′, respectively, and they could only study galaxies substantially larger. Hence the large-dish work and the interferometer work were somewhat disjoint; see the combined rotation curves made by Huchtmeier (1975) and by Roberts (1975). By 1970 it was known that the HI went far outside the optical galaxy in many cases (Roberts, 1972), and this meant that sensitive observations with a large dish should be able to probe the velocity field to great distances. The interferometer lost its advantage there because it was only sensitive to HI close to the nucleus.

3. FURTHER WORK AT OVRO

In the late 1960s the OVRO interferometer began to be used in a synthesis mode for studying HI in galaxies. This means that interference fringe measurements (i.e. measurements of the 'visibility function') were made with many baselines, and over the full range of hour angles. This synthesised a measurement made with a telescope as big as the biggest baseline. However, not all elements in the synthesized aperture could be measured because some baselines were not available; and the effective aperture was, roughly speaking, a 1600-foot ellipse with substantial blacked-out regions. This led to a great deal of work on schemes to reconstruct an image from partial visibility data, a subject with a long history in radio astronomy and other areas. The reconstruction process was helped enormously when the algorithm CLEAN was developed. CLEAN takes advantage of the fact that the blacked-out regions lead to artifacts that, because they are deterministic, can be exorcized reliably. In many cases this leads to a good image from incomplete data, and it is now used in essentially all interferometric reduction schemes. CLEAN was developed by Jan Högbom around 1968 (see Högbom, 1974; 2003), although the germ of the idea had been used earlier by Dixon and Kraus (1968), in a different context.

Figure 2. David Rogstad at the Caltech-JPL VLBI correlator ca. 1980 (photograph courtesy D.H. Rogstad).

David Rogstad (Figure 2), a Caltech Ph.D., was a post-doc at OVRO in 1967-1969, where he worked on HI in galaxies with graduate student Seth Shostak (Figure 3). In 1969 Rogstad went to a post-doctoral position at Groningen, Holland, where he learned about CLEAN from Högbom. He then wrote a computer program for CLEAN and sent it, with the then-ubiquitous cards, to Shostak at OVRO, who was struggling to generate an image of the galaxy M 101. CLEAN worked very well and resulted in a paper, Rogstad and Shostak (1971), containing the first published synthesized image made with CLEAN. A companion paper (Rogstad, 1971) contained a discussion of the results, especially the reasonable agreement of the HI with predictions from spiral density-wave theory. Testing spiral density-wave theory was one of the main motivations at OVRO for making HI observations on spiral galaxies. However, the velocity field was also important. In late 1971 Shostak submitted his thesis (Shostak, 1972) containing, among other results, a rotation curve for NGC 2403 about which he said "The overwhelming characteristic of the velocity field is the practically constant circular velocity seen over much of the object." He repeated this sentence in the published paper (Shostak, 1973) containing his thesis results. This was the first unambiguous determination of a flat rotation curve in a spiral galaxy.

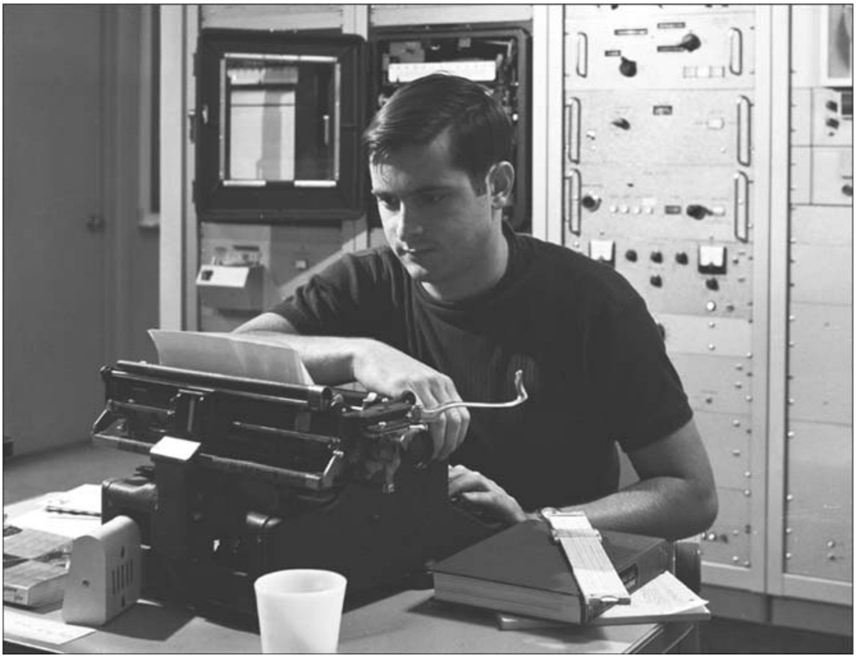

Figure 3. Seth Shostak in 1967, in the control room of the OVRO interferometer. This looks like a staged photo: note that the book under the slide rule is *Intelligent Life in the Universe* by I.S. Shklovskii and Carl Sagan (photograph courtesy of Seth Shostak).

In 1972 Rogstad and Shostak (1972) published rotation curves for five spirals, four of which were measured at OVRO and one (M 33) was from the 300-foot telescope at Green Bank. The curves are reproduced in Figure 4. The important result was that the curves were "very flat" (authors' words) beyond the maximum. They did fit a Brandt curve to the averaged rotation curve, but a visual impression suggests that any approach to a Keplerian curve, for any of the galaxies, must come well outside the measured regions. The flatness implies that the mass in the disk continually increases past the optical region, and so there is a need for 'low-luminosity material'. Of course, an increasing M/L and thus the need for low-luminosity material had been known for many years, especially for M 31; but this, apparently, was the first paper in which the rotation curves for a group of galaxies were analyzed and discussed together. The flatness was common, and identification of the dark matter became more important.

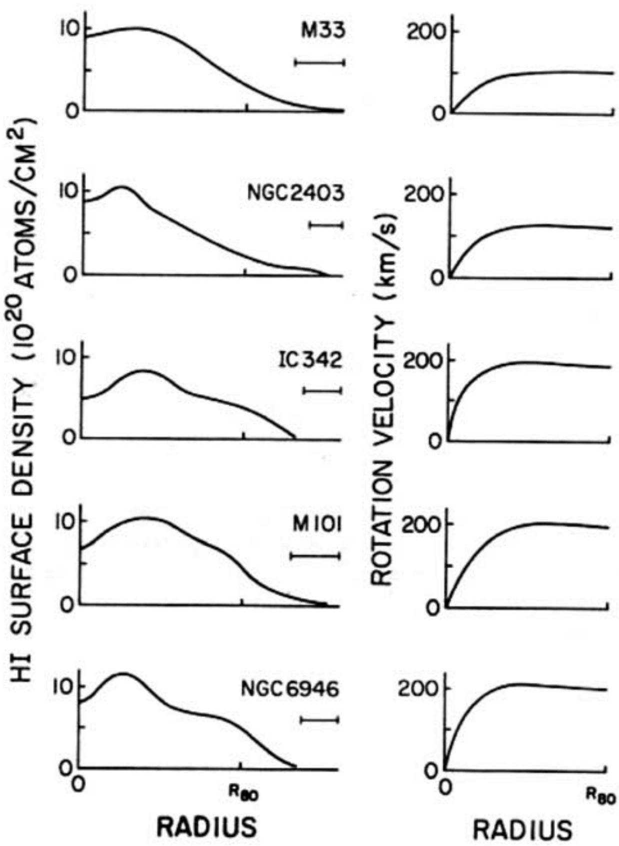

Figure 4. Azuimuthally-averaged rotation curves for five spiral galaxies (after Rogstad and Shostak, 1972, and reprinted with permission from *The Astrophysical Journal*).

Another interesting result in the 1972 Rogstad and Shostak paper is the tight correlation they found between the optical radius and the maximum circular velocity in the disk. This is an early version of the Tully-Fisher relation which, in its modern form, is important for establishing distances to galaxies. They could have enhanced their correlation by including the point for NGC 300 (Shobbrook and Robinson, 1967), which falls close to their line.

4. THE MID-1970s

In 1973 further radio work on spiral galaxies was published: details of the OVRO results that had been sketched in the 1972 paper (Shostak, 1973; Rogstad, Shostak and Rots, 1973), results from Nançay, France, on M 33 (Huchtmeier, 1973), results on M 31 and M 33 from the half-mile telescope at Cambridge, England (Emerson and Baldwin, 1973; Warner, Wright and Baldwin, 1973), and new data on M 81 as well as a comparison of rotation curves for galaxies of different types by Roberts and Rots (1973). Emerson and Baldwin interpreted their results in terms of constant M/L, but the others spoke of flat rotation curves and an increasing M/L. Note that these objects are mainly still easily-observed galaxies that had been under study for a decade.

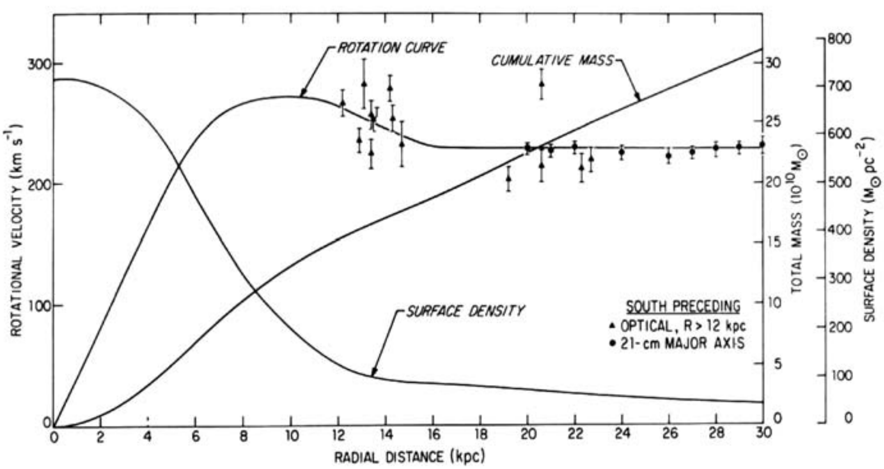

Figure 5. Rotation curve for M 31, from Roberts and Whitehurst (1975). The optical data are from Rubin and Ford (1970) and only the exterior points are shown. The radio data are from the 300-foot telescope. The data beyond about 20 kpc are consistent with a flat rotation curve. This implies that the cumulative mass increases linearly with radius (reprinted with permission from *The Astrophysical Journal*).

In 1974 the flat rotation curves in spiral galaxies were added to the velocity evidence from clusters and groups of galaxies, to make a strong case that galaxies were an order of magnitude more massive than had traditionally been believed (Einasto, et al., 1974, Ostriker, et al., 1974). Not everyone was convinced, however. Baldwin (1975) continued to interpret his data in terms of a constant M/L. Geoffrey Burbidge (1975) used the Cambridge results and theoretical ideas to cast doubt generally on the need for very high M/L. However, several papers were then published in which the rotation curves were carried out substantially farther than before. Roberts and Whitehurst (1975) observed M 31 to 30 kpc and definitively showed that the outer part of the rotation curve was flat (see Figure 5). Roberts (Figure 6) also made composite curves from the OVRO interferometer data and his 300-foot data, and the 'flat' curve was carried to 70 kpc in M 101 and to 45 kpc in IC 342 (see Roberts, 1975). Huchtmeier (1975) similarly made composite curves for eight galaxies, using data from OVRO, Nançay and the 100-meter dish at Effelsberg, Germany. These extended curves surely must have begun to make the skeptics wonder. Just how far could rotation curves go, before a Keplerian falloff could be seen?

Figure 6. Morton S. Roberts (photograph courtesy M.S. Roberts).

The large radio telescope at Arecibo, Puerto Rico, became useable at 21 cm after the upgrade in 1974; the effective diameter of the dish was about 700 feet, giving a very sensitive system with angular resolution of 3.2′. This instrument, and a powerful new array in Holland, represented a new level of sensitivity in measuring rotation curves. Krumm and Salpeter (1977) used the Arecibo dish to measure the rotation curves of six edge-on spirals. At least five of these had flat rotation curves well beyond the optical galaxy, and one, NGC 672, had a hydrogen radius five times the optical radius.

The Westerbork Synthesis Radio Telescope (WSRT) in Holland began operation in the mid-1970s. It was a multi-element interferometer with many simultaneous spacings, and could make a full synthesis of a field with a resolution of 0.5′ at 21 cm in two 12-hour periods. In his thesis, Bosma (1978) collected the rotation curves of twenty-five galaxies, many of which were obtained at the WSRT. Of these curves he says, "A striking result ... is that most rotation curves decline slowly, if at all, and none of them reaches a Keplerian drop off in the outer parts."

The Krumm and Salpeter work and the thesis by Bosma, together with further optical work (eg Rubin, et al., 1978), made it hardly possible to question the flatness of the curves. The mass of most galaxies had to be much bigger than that in the visible stars and gas, and the nature of that dark matter is still not known. For a contemporary account of the M/L problem see Faber and Gallagher (1979), and for modern reminiscences see Rubin (2004) and Bosma (2003, appendix).

5. ZWICKY AGAIN

Near the end of his life, Fritz Zwicky (Figure 7) entertained the notion that Newton's Law of Gravitation might need correction at very large distances.

> Since, however, among the ten thousand clusters of galaxies surveyed, there exist no clusters of clusters of galaxies and, in addition, the velocity dispersion among neighboring clusters is only of the order of a few thousand kilometers per second instead of the expected ten thousands of kilometers per second, the simplest explanation of these facts is that at indicative distances of about $\lambda = 20 \times 10^6$ parsecs from any given mass its gravitational field declines more rapidly than $1/r^2$. (Zwicky, 1971).

Note that the lack of cluster clustering implies a deficit of gravitation at very large distances, so Zwicky's suggestion is opposite to the sense of the gravitation surplus that was seen in spiral galaxies. In fact it is akin to the current view of dark energy. In this, as in many other areas, Zwicky was well ahead of his time. He always believed in looking at all conceivable facets of a problem (his 'morphological approach'), and no doubt would have approved of the present attempts to modify Newtonian dynamics to account for flat rotation curves (e.g. Milgrom, 2002).

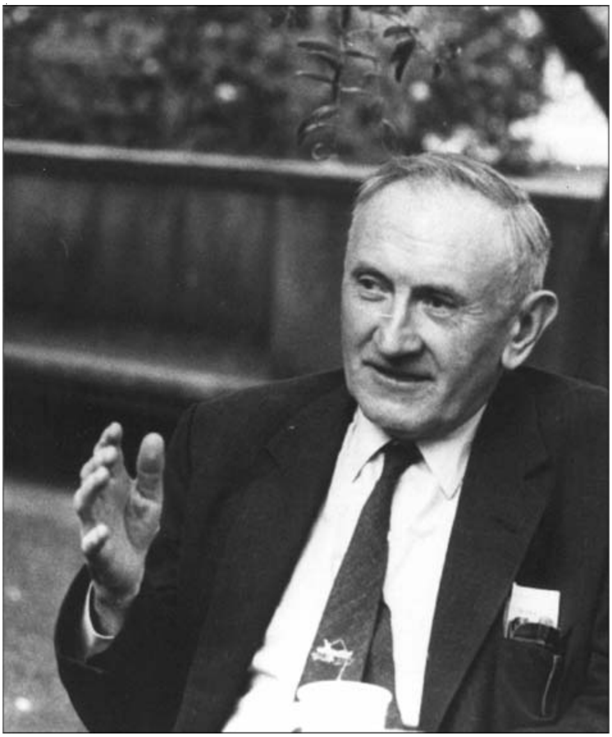

Figure 7. Fritz Zwicky (1898–1974) in 1959 (photograph by James McClanahan, courtesy of the Archives, California Institute of Technology).

6. CONCLUSIONS

Flat HI rotation curves in spiral galaxies were a major factor in getting people to accept the notion that galaxies contained much more invisible mass than had traditionally been thought, and that these more massive galaxies themselves provided some of the 'missing mass' in clusters of galaxies. The OVRO interferometer had a strong role in this, in the late 1960s and early 1970s. Although Rubin and Ford (1970) appear to be the first to use the word "flat" in connection with their final points on the rotation curve of M 31, the first observations showing substantially flat velocity well after the peak or turnover are those from the OVRO interferometer; first NGC 2403 (Shostak, 1972) and then the group of galaxies summarized by Rogstad and

Shostak (1972). The latter paper was the first to emphasize, both in the main text and in the abstract, that spiral galaxies, as a group, had flat rotation curves. This is in distinction to a later comment by Rubin et al. (1978), who attribute the emphasis on flat curves to others.

The OVRO interferometer was limited in HI studies for two reasons: it had only two (later three) dishes and so was slow, requiring many baselines done one at a time to synthesize an image; and it was restricted to a field ~30′ in size by the primary beam. The multi-element WSRT was much faster, and the large dishes with new sensitive receivers, especially Arecibo, could make measurements at great distances from the nucleus. Hence the OVRO interferometer was no longer competitive after about 1975. This was generally true in other studies the interferometer was making, as well. In 1979 general observing on the interferometer stopped, and it was devoted exclusively to solar observations. It has continued in this mode to this day.

7. ACKNOWLEDGEMENTS

I am indebted to Ken Kellermann, Mort Roberts, Dave Rogstad, Seth Shostak, and Virginia Trimble for useful comments. I thank Maarten Schmidt for the reference to Zwicky's 1971 paper.

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THE DISCOVERY OF SGR A*

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Abstract: The discovery of the point-like radio source at the center of the Galaxy, colloquially termed Sgr A*, was made in February 1974 by Balick and Brown using the then-new 35km baseline interferometer between Green Bank and a remote site near Huntersville, WV. The highly unusual properties of the radio source are now well studied; however, the story behind the discovery is equally interesting. Although the signal from the source was strong (~0.5 Jy) and the peak measured surface brightness quite high (~10^7K), the interpretation of the signal as a single point source was initially obscured by the odd geometry of the synthesized aperture. In addition, two groups unknowingly competed to make the detection observations, both based on very different scientific rationales. The other group, comprising Downes and Goss, correctly anticipated the astronomical significance of the discovery but could not make their way from Europe to Green Bank when their observations were scheduled in the fall of 1973.

Key words: Radio astronomy, Sgr A*, Galactic Center, black holes

1. BACKGROUND

The initial discovery of Sgr A*, now believed to be a black hole of approximately 2-4 million solar masses at the Galactic Center (Gezari, et al., 2002), was published by Balick and Brown (1974). The discovery was inevitable given the pace of technological development in the 1960s and 1970s because the flux density of Sgr A*, ~0.5 Jy, lies well above the detection threshold of even modest radio telescopes (>10m) with modest receivers. However, the prerequisite to detecting Sgr A* is to separate it cleanly from the complex largely thermal background nearby and to show

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that its brightness temperature exceeds that of normal thermal gas, 10^4 K. Both requirements mandate the need for angular resolution that was never available in any centimetric interferometer until late 1973.

2. OBSERVATIONS

The three-element Green Bank Interferometer ('GBI'), with its 2700m baseline and 85-foot diameter antennae, was typical of phase-coherent interferometers of the time. The smallest synthesized beam of the interferometer was of order 3″, so a radio source of flux density 1 Jy has a peak measured brightness temperature of ~2,000 K. The only means for higher angular resolution was to use VLBI techniques; however, in 1973 the narrow recorder bandwidths and limited integration times at southern declinations meant that the signal from Sgr A* would be marginal.

Figure 1. The Huntersville antenna 35km from Green Bank, ca 1972.

The 35km baselines of the Huntersville antenna (Figure 1) with each of the three elements of the GBI provided six hours of coverage towards Sgr A* with a 30 MHz bandwidth at the operating wavelengths of 3.7 and 11.1 cm. The system noise was 0.2 Jy in 30 seconds, and the maximum projected interferometer baseline is 900,000λ at 3.7 (11.1) cm, resulting in a fringe spacing that varied from 0.2″ (0.6″) to 2″ (6″) as the source moved across the sky.

Observations were conducted on 13 February 1974 and again two days later. The weather was clear, dry and stable. This fringe separation resolves all of the structure near Sgr A*, except for Sgr A* itself. The flux density of Sgr A* is 0.8 and 0.6 Jy at 3.7 and 11.1 cm, respectively. Therefore, the peak brightness temperature of Sgr A* is 10^7 K, far in excess of thermal (ionized) gas at 10^4 K. Thus, Sgr A* is a non-thermal source.

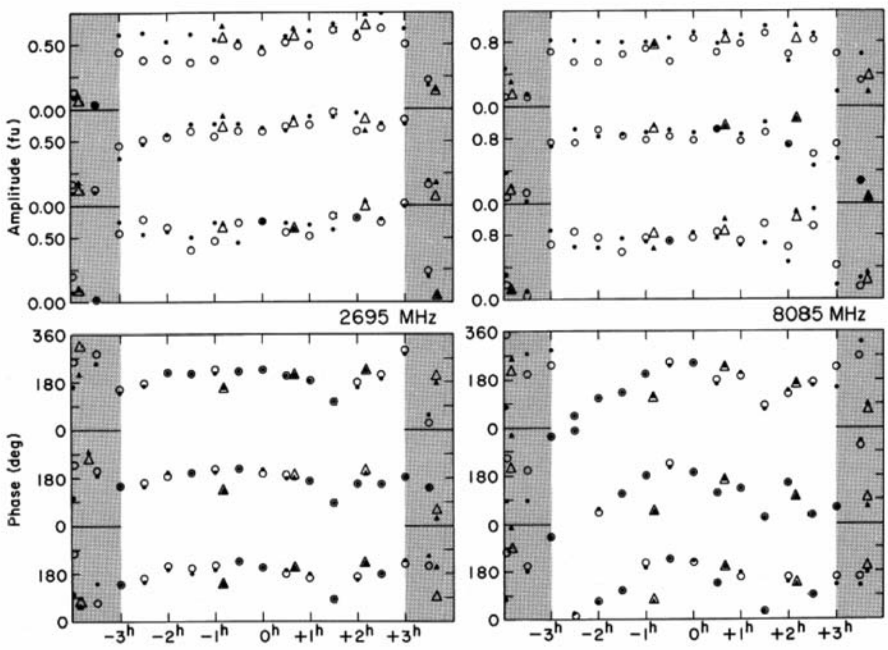

Figure 2. Observed fringe visibilities of Sgr A* at 2.7 and 8.1 GHz.

The visibility amplitudes at both operating frequencies showed no variation in time, except at low elevation angles (see Figure 2). Thus, the structure of Sgr A* is simple. A point source located at the phase center of the array would be expected to have a calibrated phase near zero degrees. Instead, the phase at 11.1 cm was about 180°, with small smooth variation of ±45°. At first glance, this is the signature of a point source at the phase center with negative flux. After several letters in which various explanations were discussed, Balick and Brown dismissed all but one model as implausible, including models of a single object of negative flux and a pair of positive point sources. They finally realized that a 1.3″ shift in position of a point source from their nominal phase center could account very nicely for the measured phase variations (see Figure 3).

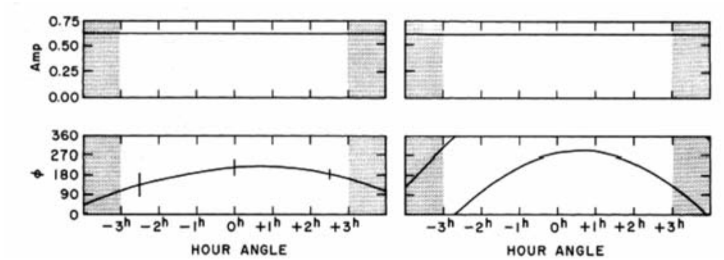

Figure 3. Model fringe visibilities of Sgr A* at 2.7 and 8.1 MHz.

3. THE CONFLICT WITH DOWNES AND GOSS

Based on earlier observations of the HII region W51 using a different interferometer (Miley, et al., 1970), Balick and Brown had expected a marginal detection of a slightly superthermal source in Sgr A with a peak brightness temperature of 10^5 K. The far stronger Sgr A* observations startled them. On the final day of their observations, Balick contacted Dr David Hogg of NRAO who was responsible for scheduling the GBI and Huntersville antenna to communicate the new and exciting results. This is when Balick, Brown and Hogg became aware of the competing proposal to observe Sgr A* with the same interferometer configuration and wavelengths by D. Downes and M. Goss. Everyone was aware that such conflicts can produce controversy and competition. NRAO and Hogg normally tried to avoid and resolve such conflicts before any observations were made.

The remainder of this paper summarises the events between the submission of the first proposal and publication of the results in December 1974. Some of the history has been taken from a paper by Goss, Brown and Lo (2003). The rest is based on the author's recollections and from interviews with Brown, Goss, Hogg and Dr William E. Howard of NRAO, all of whom were directly involved in finding a suitable solution to the conflict.

The observations by Miley et al. (1970) of 'Superbright Radio Knots' in the thermal HII region, W51, were the motivation for a proposal by Balick in December 1970 to use the new 35 km interferometer in West Virginia then being planned as a prototype for the Very Large Array project (which was completed in 1981). At this point, the new interferometer was intended to operate only at a frequency of 2.7 GHz. However, the new dish, electronics, and IF radio link were vastly superior to those used by Miley et al., whose published detections were extremely marginal. Balick's proposal involved observations of northern HII regions, since the initial design of the long baseline provided only for brief observations of southern targets.

The design effort for the test baseline went better than expected during 1971, and a second operating frequency, 8.1 GHz, was added, allowing the spectral properties of the superbright radio knots to be measured. The purchased 45-foot antenna provided full sky coverage. This meant that in addition to northern targets, southern HII regions (such as Sgr A and Sgr B2) could be observed with reasonable ranges of projected baselines, which was critical for mapping the locations of the knots. Balick thus amended his original proposal in January 1972, adding Robert L. Brown as a collaborator. However, Balick's brief proposal amendment did not mention any southern targets by name. The revised proposal was approved, though the approval date is not known.

Meanwhile, in Europe, Rees and others were starting to speculate that quasars were at the centers of galaxies (Goss, pers. comm., 2004), and that their remnants might still be observable in local galaxies, including our Galaxy. Goss recalls that he and Downes participated in these speculative discussions. Knowing that NRAO was building a 35km interferometer, they submitted a proposal in June 1972 to observe the Sgr A region, which they had mapped successfully elsewhere in previous years (Downes and Martin, 1971; Ekers, et al., 1975). Sgr A was mentioned since it was the prime target of their proposal. Thus arose the target conflict in the two proposals, but this went undetected by Dave Hogg.

The Huntersville antenna, electronics and radio link became operational in early 1973. Hogg then scheduled the Downes and Goss proposal in 1973, before the Balick and Brown proposal, presumably owing to its strong merit, but also perhaps because of co-ordination with other pending proposals or because of favorable night weather for Sgr A during the summer. However, both Downes and Goss had just moved to new positions with urgent responsibilities, and they were unable to come to the USA, so their proposal was deferred. As Goss was later to write: "With [the pressures of our new jobs] … the urgency to complete the Downes-Goss proposal … decreased." (Goss, et al., 2003).

The Balick and Brown proposal was scheduled for three days in February 1974, and Sgr A was observed nearly continuously for almost eight hours on the first day, February 13. Perfect, dry, stable weather conditions prevailed until mid-afternoon, although observations made near the horizon were accorded low weight. Balick and Brown used a phase center position that turned out to be off by ~1.3″, so the calibrated phases showed structure (see Figure 2). The observed phase structure was confirmed on 15 February, a day of only slightly less favorable weather.

As noted earlier, Balick informed both Brown and Hogg of the unexpectedly strong signals from Sgr A. (Ironically, no structure was seen in any other HII region in the observing program, rendering their original motivation for the proposal erroneous!) Hogg informed Balick and Brown of the conflict proposal, and immediately set out to inform Downes and Goss of the inadvertent mistake. He also asked that Balick and Brown not publish or widely publicize their results until some sort of agreement with Downes and Goss could be formulated.

On 25 February 1974, Balick (who was now home in California) posted a letter to Hogg in which he requested additional time to monitor Sgr A for flux variability, but Hogg declined this request in order to leave Downes and Goss time to observe the object. Balick (1974) added: "The observations of Sgr A are, of course, subject to the resolution of the proposal conflict between ourselves and Downes and Goss … I am yet to hear from [them]. I hope they write soon, for Bob and I feel that our results on Sgr A merit rapid publication."

For the next three months Balick (in Santa Cruz) and Brown (in Charlottesville) worked by mail to complete the calibration of the data (which was easy) and to find an explanation for the 180° phases and their temporal variations (and this outcome was described in Section 3, above). By the end of May they were ready to submit the paper, and Hogg lifted his request to embargo the publication. The manuscript was submitted to the *Astrophysical Journal* on 3 June 1974, was accepted by the referee without revision, and appeared in the 1 December 1974 issue of the journal (see Balick and Brown, 1974).

There is a small chapter to add to the story. Although neither Balick nor Fred Lo (then a Ph.D. student at M.I.T.) can recall the details, they managed to learn that Lo had observed Sgr A using VLBI techniques in early 1973, but found no signal. The absence of a signal can be purely a matter of low intensity, or it can (in part) be the result of using the wrong phase center position. In the latter case, the phase winds through 360° if the source position is off by a few lobe separations, and this results in a vector sum during an integration time which is artificially small.

Lo was extremely busy writing up his thesis and preparing to leave for a new position at CALTECH, but he found the time to reprocess his data using the new phase position found by Balick and Brown and detected weak fringes (~0.3 Jy) on a baseline that was about ten times longer than that used by Balick and Brown. Obviously Lo had partially resolved Sgr A, and this was soon interpreted as the result of smearing (i.e. defocusing) by strong interstellar scintillations.

Although I have not interviewed D. Downes to see how he feels about the 'Sgr A* Affair' in hindsight, Miller Goss (pers. comm., 2004) told me that they were only slightly disappointed by the Balick/Brown discovery of Sgr A*. He also noted that he and Downes created a lost opportunity for themselves by being offered observing time and declining it.

4. RETROSPECTIVE

Goss, Brown and others remained active observers of Sgr A* for the next decade and beyond. Many interesting papers resulted from their efforts, particularly once the VLA came into operation and a far more uniformly filled aperture could be synthesized for targets (like Sgr A*) situated at southern declinations. To this day, Sgr A* remains unique in the Galactic Center for its high radio surface brightness, variability, and fixed location at the dynamical center of the gas and stars located in its vicinity (Meila and Falcke, 2001).

5. ACKNOWLEDGEMENTS

I am grateful to Miller Goss for helpful comments relating to this paper.

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"RADIO ASTRONOMY, WHATEVER THAT MAY BE"

The Marginalization of Early Radio Astronomy

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Abstract: Today we see radio astronomy as a fully-integrated part of astronomy; it is now just one of several available wavelength regimes and many astrophysicists who use radio data are not radio astronomers themselves. At the beginning, it was very different. Between 1946 and 1960, radio astronomy emerged as an important speciality but it was an area little understood by mainstream astronomers. Radio astronomers rarely published in astronomical journals, gave papers at astronomical conferences or were accorded much notice. The pioneers in the field were not astronomers themselves and had little in common with astronomers. In this paper I note the various ways in which radio astronomy was alienated from the mainstream in its first decade and some of the reasons this alienation occurred. I will also speculate on when and how the integration began to occur.

Key words: Radio astronomers, optical astronomy, astrophysicists, history and philosophy of science

1. INTRODUCTION

My title comes from a story related to me by the late Vic Hughes, Bernard Lovell's first graduate student and a radio astronomer at Ontario's Queen's University (Hughes, 1992). In the 1950s and 1960s, the meetings of the Canadian National Committee of the International Astronomical Union combined a business meeting with a few papers by astronomers. At one of these meetings, according to Hughes, the session chair, the late Ken Wright, introduced a speaker with "Well, next one is a paper on radio

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astronomy, whatever that may be." This was, Hughes thought, indicative of the attitude of traditional astronomers towards his new breed. Wright was a highly-respected astrophysicist and Director of the Dominion Astrophysical Observatory. He certainly would have known what radio astronomy was, though he may have had little interest in it. But was Hughes' reaction indicative of something real? In their study of early British radio astronomy, Edge and Mulkay (1976) argued that, at least until the mid-1950s, radio astronomy was not an integral part of astronomy, nor had the integration fully occurred by the time of their study.

Here I want to consider how the emerging field of radio astronomy interacted with traditional astronomy during the period of 1946-1960. By the latter date, the pioneer days were essentially over, with large, new observing facilities being built or in the planning stages. During the late 1950s, a new generation of workers, most now trained in astronomy, was joining the post-war founders.

If we assume that radio astronomers in this period were not integrated fully into astronomy—at least in English-speaking countries—there should be some measures we can identify:

1. Was the educational or occupational background of early radio astronomers different from that of other astronomers?
2. Did they publish in different journals than their optical and theoretical counterparts?
3. Did they participate regularly in general astronomical meetings?
4. Did mainstream astronomers cite radio astronomers' work?

2. BACKGROUNDS OF THE EARLY RADIO ASTRONOMERS

First, who were the radio astronomers? I culled names from several of the early popular books on radio astronomy by J.H. Piddington (1961), Francis Graham Smith (1960), J.S. Hey (1973), D. Edge and M. Mulkay (1976) and W.T. Sullivan's collection (1984). Whilst there were important contributions from groups in France, the Netherlands, Soviet Union and Japan before 1960, I will concentrate upon the English-speaking groups in Australia, Canada, the United Kingdom and the United States. For this period I find 193 workers including graduate students. A number were likely transients and moved into other fields. By country the numbers are: Australia (37), Canada (12), United Kingdom (73) and United States (71); no doubt, a few have been missed. Interestingly, the radio astronomers *per capita* of general population followed the same order.

It is difficult to obtain biographical details for all these workers, but for those we can track (95 or half the total), their educational backgrounds are what one would expect: 61 (64%) were trained in physics, 21 (22%) in electrical or electronic engineering, 2 in mathematics and 11 (12%) in astronomy. Anyone who has studied the pioneer period recognizes that many of the early workers were veterans of wartime radar development. Thus, it is no surprise that the great majority of early radio astronomers were physicists or engineers. Those trained in astronomy were mostly not typical, however. Three pioneers—M. Stahr Carpenter, R.E. Williams and D. McRae—had conventional astronomical training but moved, briefly, into radio astronomy. The rest were products of the Harvard program of the 1950s, which provided a specialization in radio astronomy. The students of the Cambridge and Manchester radio astronomy programs were more physicists than astronomers during this period.

3. PUBLICATION STRATEGIES

Where did radio astronomers publish? We can begin with the year-by-year bibliography in J.S. Hey's study; I will refer to these as the 'key papers' of the field. Over this period, 37% of the papers were published in physics-oriented journals (the most favored being *Australian Journal of Physics*, *Proceedings of the IRE, Proceedings of the Royal Society – A* and the *Physical Review*). Another 30% appeared in general scientific journals (primarily *Nature* and *Australian Journal of Scientific Research*). Only 31% of 'key papers' showed up in astronomical journals (mostly in the *Astrophysical Journal* and the *Monthly Notices*).

Looking at publications from the opposite side, did major journals encourage the new field by publishing papers? In the English-speaking world, arguably the three astronomical journals of record were the *Monthly Notices of the Royal Astronomical Society*, the *Astronomical Journal* and the *Astrophysical Journal*. Before 1960, specialist journals such as *Solar Physics* did not exist, so general astronomical journals had a broad audience. Table 1 shows the number of articles in the *Monthly Notices* for each year from 1946 to 1960

Apart from a spike in 1947, radio astronomers provided consistently about 10% of the articles to the *Monthly Notices* throughout the 1950s, with a sharp rise in 1960. The three groups at Manchester, Cambridge and Malvern all contributed papers to the journal. However, as Edge and Mulkay (op. cit.) noted, most British workers sent their papers went to physics journals. During the 1940s and 1950s the British mainstream astronomical community was not large and the growing radio astronomy groups appear disproportionate in their contributions to the *Monthly Notices* even when not publishing the majority of their work in its pages.

Table 1. Publications in *Monthly Notices of the Royal Astronomical Society*, 1946–1960.

Year	Number of Papers	Number of Radio Papers	% Radio
1946	62	0	0
1947	50	5	10
1948	52	1	2
1949	63	4	6
1950	71	2	3
1951	59	10	17
1952	60	5	8
1953	41	4	10
1954	62	1	2
1955	56	5	9
1956	64	6	9
1957	53	6	11
1958	49	6	12
1959	53	6	11
1960	101	17	17

The *Astronomical Journal* and the *Astrophysical Journal* were the primary American journals of in the 1940s and 1950s. The American Astronomical Society (AAS) owned *AJ* and had some editorial control over *ApJ* during this period. The editorial focus of *AJ* was the narrower of the two, but sufficiently broad for the inclusion of at least some types of radio astronomy material. In Table 2, we have the publications in *AJ* from 1946 to 1960 (these figures include abstracts and notes; most radio astronomy items were notes).

Two outstanding peaks occurred in 1949 and 1956, the first the result of a symposium on microwave astronomy at the AAS Ottawa meeting, the second due to a radio astronomy symposium at the Columbus, Ohio, meeting. Otherwise, the contributions of radio astronomers were quite modest. There may have been complaints. In a 1959 editorial, Dirk Brouwer wrote:

> The accelerated development of radio astronomy in America and the need for rapid publication in this field has raised doubts in the

> minds of some radio astronomers whether the existing astronomical journals in this country are adequate for their needs. For this reason, we should like to state that the *Astronomical Journal* heartily welcomes authors to submit papers on radio astronomy and assures them that their papers will be given every consideration. This has always been the policy of the *Astronomical Journal*: during 1957 and 1958, the *Astronomical Journal* has published several papers on radio astronomy; and several are in the course of publication.
>
> Radio astronomy is a rapidly developing branch of astronomy; and any schism between this newer branch and the older branch of optical astronomy is to be greatly regretted. For this reason, the *Astronomical Journal*, already established as one of the principal astronomical journals, would like to see that papers on radio astronomy, commensurate with its growing importance, will be represented in the *Astronomical Journal*. (Brouwer, 1959: 36)

John Kraus of Ohio State had been *AJ*'s radio astronomy stalwart in the lean years. Whether Brouwer's editorial made a difference or not, by 1959, the percentage jumped.

Table 2. Publications in the *Astronomical Journal*, 1946–1960.

Year	Number of Papers	Number of Radio Papers	% Radio
1946-47	141	1	0.7
1947-48	108	0	0
1948-49	166	9	5
1949-50	147	0	0
1951	148	3	2
1952	146	4	2.5
1953	130	2	1.5
1954	156	5	3
1955	157	6	4
1956	142	17	12
1957	214	15	7
1958	155	5	3
1959	188	18	10
1960	284	26	9

Radio astronomy was even less evident in the pages of *ApJ* during this period as we see in Table 3. The field was essentially invisible until 1954 and only in 1955 did the percentage of radio papers ever rise above 5%. Those radio astronomers who did publish in *ApJ* tended to be either members or alumni of the Naval Research Laboratory (following John Hagen's lead), radio astronomers working with mainstream astronomers (e.g. D.W.R. McKinley with P.M. Millman or A.E. Covington with Helen Dodson, etc.) or the younger generation trained in both optical and radio astronomy (e.g. D.S. Heeschen, A.E. Lilley).

Table 3. Publications in the *Astrophysical Journal*, 1946–1960.

Year	Number of Papers/ Abstracts	Number of Radio Papers	% Radio
1946	85	1	1
1947	92	1	1
1948	107	0	0
1949	119	0	0
1950	132	0	0
1951	123	1	0.8
1952	147	1	0.5
1953	114	1	0.8
1954	159	5	3
1955	153	9	6
1956	150	6	4
1957	158	2	1
1958	149	5	3
1959	202	9	4
1960	195	8	4

Also, in early 1959, S. Chandrasekhar (1959: 1), the Editor of the *ApJ*, wrote the following editorial:

> The accelerated development of radio astronomy in America and the need for publication in this field has raised doubts in the minds of some radio astronomers whether the existing astronomical journals in this country are adequate for their needs. For this reason, I would like to state that the *Astrophysical Journal* heartily welcomes authors to submit papers on radio astronomy and assures them that their papers will be given every consideration. This has always been the policy of the *Astrophysical Journal*; during 1957 and 1958 the *Astrophysical Journal* published ten papers on radio astronomy, and several are in the course of publication.
>
> Radio astronomy is a rapidly developing branch of astronomy; and any schism between this newer branch and the older branch of optical astronomy is to be greatly regretted. For this reason, the *Astrophysical Journal,* already established as one of the principal astronomical journals, would like to see that papers in radio astronomy, commensurate with its growing importance, will be represented in the *Astrophysical Journal.*

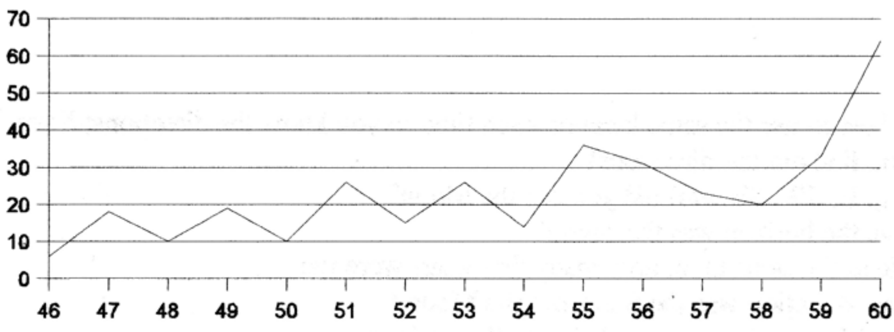
One wonders whether Chandrasekhar and Brouwer had both received complaints. In these three journals, from 1946 to 1960, some 5,273 papers, research notes and abstracts appeared, only 233 (or 4%) of which were on radio astronomy.

Table 4. 'A-List' Publications, 1946–1960.

Country	Total Papers	'A-List' Radio Astronomers
Australia	142	Bolton, Bracewell, Christiansen, Kerr, Martyn, Mills, Pawsey, Payne-Scott, Piddington, Roberts, Smerd, Wild
Canada	39	Covington, Harrower, Hartz, McKinley
UK	127	Clegg, Graham Smith, Hanbury Brown, Hewish, Hey, Jennison, Lovell, Ryle, Scheuer
USA	86	Burke, Dicke, Drake, Ewen, Haddock, Hagen, Heeschen, Kraus, Manning, McClain, Reber, Seeger
All above	394	

Were the leaders of the field more or less likely to publish in astronomical journals? I have listed in Table 4 what I term the 'A-List' radio astronomers of this period. If we track their publications by year with the Science Citation Index we find no obvious trends by nationality, but when the publication statistics of these people are combined (as they are in Figure 1), we can see a general rise in the use of astronomy journals over the 1950s, with a rapid rise in 1959–1960. As these leaders typically had co-authors (who included a sizeable fraction of the entire population of radio astronomers in these countries), the late 1950s rise may show a real trend.

Figure 1. Plot of the percentage of published papers on radio astronomy, by year.



4. PARTICIPATION IN MEETINGS

Our third question relates to radio astronomers' participation in astronomical societies. In the UK, as Edge and Mulkay (1976) noted, very few early workers were fellows of the Royal Astronomical Society; only in the late 1950s did more of them become active once research linkages with optical astronomers became more commonplace. In North American, radio astronomers' participation in AAS meetings was spotty at best, as can be seen in Table 5.

Apart from the 1949 and 1956 meetings already noted, most activity can be seen at meetings from 1954 to 1956, with a slump until a sharp rise in 1959, sustained through 1960. Between 1955 and 1959, participation ran about 9%, yet radio astronomers surely accounted for more than 9% of the North American astronomical community at that time.

Table 5. Radio astronomy at AAS meetings, 1946–1960.

Meeting Number	Date	Place	Total Papers	No. Radio Papers	% Radio
74	Feb 1946	New York	30	0	0
75	Sept 1946	Madison	33	1	3
76	Dec 1946	Cambridge	18	1	5
77	Sept 1947	Evanston	31	0	0
78	Dec 1947	Columbus	51	2	4
79	June 1948	Pasadena	67	1	1.5
80	Dec 1948	New Haven	56	4	7
81	June 1949	Ottawa	61	7	11
82	Dec 1949	Tucson	50	1	2
83	June 1950	Bloomington	55	1	2
84	Dec 1950	Haverford	50	4	8
85	June 1951	Washington	61	2	3
86	Dec 1951	Cleveland	77	6	8
87	June 1952	Victoria	26	2	8
88	Dec 1952	Amherst	60	2	3
89	Aug 1953	Boulder	96	4	4
90	Dec 1953	Nashville	38	0	0
91	June 1954	Ann Arbor	71	8	11
92	Apr 1954	Princeton	100	8	8
93	Nov 1955	Troy	48	6	13
94	Mar 1956	Columbus	67	10	15
95	Aug 1956	Berkeley	95	15	16
96	Dec 1956	New York	53	5	9
97	May 1957	Cambridge	56	5	9
98	Aug 1957	Urbana	38	2	5
99	Dec 1957	Indianapolis	36	3	8
100	June 1958	Madison	58	0	0
101	Dec 1958	Gainesville	36	0	0
102	Mar 1959	Rochester	51	3	6
103	Aug 1959	Toronto	103	18	17
104	Dec 1959	Cleveland	55	5	9
105	Apr 1960	Pittsburgh	52	6	12
106	Aug 1960	Mexico City	79	8	10

5. CITATION OF RADIO ASTRONOMERS' WORK

Our last question deals with citations. Here I have done a small sampling of Hey's 'key papers'—two each for every year from 1946 to 1960. These were followed through the Science Citation Index up to 1965 (to capture citations of the later papers). I have noted self-citations, citations by radio astronomers, by mainstream astronomers and by authors from other sciences. The results are in Table 6.

To be sure, several important optical and theoretical astronomers, such as W. Baade, R. Minkowski, G. Burbidge, J. Oort, B. Bok and F. Whipple, showed interest in the early radio work. But, for most of their colleagues, radio data were simply irrelevant to their research. In fact, radio results were as likely to be of as much value to atmospheric scientists as to astronomers.

Table 6. Citations of Key Papers, 1946–1960.

	Citations		Cited by			
Paper No.*	Total No.	First/Last	Self	Radio astr's	Astronomers	Others
1	28	1947/1964		25	1: Unsold	2
2	16	1947/1964	3	9	1: Millman	3
3	42	1947/1963		39	1: Unsold	2
4	19	1947/1963	1	12	3: Millman, Prentice	3
5	19	1949/1964	3	14	2: Millman, Whipple	
6	27	1948/1964	3	22	1: Waldmeier, Muller	1
7	27	1954/1965	2	19	5: Baade, Burbidge, Mayall, Minkowski, Osterbrock	1
8	36	1949/1964	4	14	3: Gilvarry, Kopal, Sinton	5
9	31	1950/1964	2	23	3: Allen, Elwert, Tandberg-Hanssen	3
10	33	1951/1961	2	26	2: Baade/Minkowski	3
11	24	1952/1964	1	14	6: Elwert, Field, McCrea, Parker, Purcell, Van de Hulst,	3
12	23	1951/1965	3	19	1: Baade/Minkowski	
13	8	1953/1961	2	5	1: Krassovsky	
14	65	1952/1965	5	43	1: Elwert	16
15	14	1953/1965	5	6	3: Burbidge, Burbidge, Minkowski, Sandage, Wilson	
16	4	1954/1958		2	2: Elwert, Zhelezniakov	
17	50	1954/1965		20	26: Baum, Blackwell, Bohmvitense, Brandt, Cameron, Code, de Vaucouleurs, Elwert, Field, Finzi, Godfredsen, Gold, Gould, Greenstein, Hiltner, Houck, McCuskey, Morgan, Munch, Oort, Rougoor,	4

					Rubin, Salpeter, Spitzer, Stibbs, Stothers, Vansmith, Weaver, Yoss.	
18	70	1954/1965	1	37	25: Babcock, Burbidge, Elwert, Field, Hiltner, Hoyle, Minkowski, Munch, R. Parker, Savedoff, Schmidt, Sciama	6
19	22	1958/1964		19	1: Richardson	2
20	23	1955/1965		17	4: Elwert, Field, Hiltner, Munch	2
21	5	1958/1965		5		
22	46	1956/1965		23		23
23	5	1958/1964		5		
24	1	1960			1: Hoyle	
25	21	1959/1965	1	10	9: Arp, Axford, Bok, Burbidge, Elwert, Hardie, Osterbrock, Wentzel	1
26	19	1958/1964	5	9	3: Jastrow, Opik, Sagan	2
27	35	1960/1965	2	27	3: Field, Oort, Osterbrock	3
28	11	1960/1965	5	4	2: Edelson, Newkirk	
29	6	1960/1965	1	5		
30	18	1960/1965	3	15		

*Key. The following papers are represented:

1 = Dickie and Beringer, 1946. *The Astrophysical Journal*, 103, 375.
2 = Hey and Stewart, 1946. *Nature*, 158, 633.
3 = McCready, et al., 1947. *Proceedings of the Royal Society*, A190, 357.
4 = Hey, et al., 1947. *Monthly Notices of the Royal Astronomical Society*, 107, 176.
5 = Clegg, 1948. *Philosophical Magazine*, 39, 577.
6 = Covington, 1948. *Proceedings of the Institution of Radio Engineers*, 36, 454.
7 = Bolton, et. al., 1949. *Nature*, 164, 101.
8 = Piddington and Minnett, 1949. *Australian Journal of Scientific Research*, A2, 63.
9 = Smerd, 1950. *Australian Journal of Scientific Research*, A3, 34.
10 = Ryle et al., 1950. *Monthly Notices of the Royal Astronomical Society*, 110, 508.
11 = Ewen and Purcell, 1951. *Nature*, 168, 356.
12 = Smith, 1951. *Nature*, 168, 555.
13 = Brown and Hazard, 1952. *Nature*, 170, 364.
14 = Ryle, 1952. *Proceedings of the Royal Society*, A211, 351.
15 = Jennison and Das Gupta, 1953. *Nature*, 172, 996.
16 = Christiansen and Warburton, 1953. *Australian Journal of Physics*, 6, 262.
17 = van de Hulst, et al., 1954. *Bulletin of the Astronomical Institutes of the Netherlands*, 12, 117.
18 = Baade and Minkowski, 1954. *The Astrophysical Journal*, 119, 206.
19 = Burke and Franklin, 1955. *Journal of Geophysical Research*, 60, 213.
20 = Hagen et al., 1955. *The Astrophysical Journal*, 122, 361.
21 = Droge and Priester, 1956. *Zeitschrift für Astrophysik*, 40, 236.
22 = Browne, et al., 1956. *Proceedings of the Physical Society*, B69, 901.
23 = Bolton and Wild, 1957. *The Astrophysical Journal*, 125, 296.
24 = Schmidt, 1957. *Bulletin of the Astronomical Institutes of the Netherlands*, 13, 247.
25 = Oort, et al., 1958. *Monthly Notices of the Royal Astronomical Society*, 118, 379.
26 = Mayer, et al., 1958. *The Astrophysical Journal*, 127, (a)1, (b)11.

27 = Drake and Hvatum, 1959. *The Astronomical Journal*, 64, 329.
28 = Kundu, 1959. *Annales d'Astrophysique*, 22, 1.
29 = Eshleman, et al., 1960. *Science*, 131, 329.
30 = Ryle and Hewish, 1960. *Monthly Notices of the Royal Astronomical Society*, 120, 220.

6. CONCLUDING REMARKS

In summary, it appears that radio astronomers in the pioneer period published only occasionally in astronomical journals, whilst radio papers in the major astronomical journals rarely accounted for even 10% of the content before 1960. Some radio astronomers participated in astronomical societies, but participation was not common until late in the 1950s. Citation patterns seem to change little over the fifteen years. All of this argues that radio astronomy was not integrated into mainstream astronomy by 1960.

However, there are several indications that a sea change was occurring in the 1956-1960 period. Publication in astronomy journals increased, as did participation in the RAS and AAS. It is possible that the International Geophysical Year (1957-1958) brought at least some of radio astronomy more respect. That the National Radio Astronomy Observatory appointed a renowned optical astronomer, Otto Struve, as its first Director in 1959, suggests a coming of age.

Why had integration not taken place earlier? The first factor is the nature of the radio astronomy community. Given the education and wartime experiences of the pioneers, they were not likely to feel comfortable in the wider astronomical community. Their technical issues were of no interest to astronomers. Whilst it might be argued that optical astronomers were resistant to new techniques, their rapid adoption of photoelectric photometry in the late 1940s and early 1950s shows otherwise; however, photometry is still optical, and radio data must have seemed a curiosity. Another factor may have been the nature of early radio astronomy. Much of the work resembled natural history. Astrophysicists were used to solving physical problems with familiar data. The Cambridge surveys, for example, provided hundreds of radio sources in the sky, but with almost none of them identified, what astrophysical problem could be solved?

There are hints that *rapprochement* began to occur in earnest during the 1960s. Perhaps a symbolic move was the choice of Green Bank, West Virginia, as the site of the AAS 1962 meeting. Radio astronomy was becoming another 'Big Science' with expensive facilities being built at many sites around the world. More important were four key developments. The identification of the first optical quasar in 1960 showed that natural history had uncovered a wholly new class of celestial object. In 1965, the discovery of the microwave background led to a renaissance in cosmology, a speciality almost moribund for lack of fresh observational evidence. Two years later came the first pulsar observation, uncovering yet another new class of object, quickly linked to earlier theoretical work. During the same year, when Canadian and American teams demonstrated very long baseline interferometry, radio astronomers no longer had to be ashamed of the resolution of their instruments. But this is speculation. Further study of the publication, participation and citation patterns of the 1960s and early 1970s should show when the integration of radio astronomy into mainstream astronomy happened.

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TELESCOPES LOFTED TO SPACE:
An Historical Chronology

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Abstract: To become airborne was an early dream of humanity. It was a profound dream because of the meaningfulness of the perspective from aloft: the subject was able to observe the Earth and to become closer to heaven. In this context, a telescope is the most basic augmentation of the airborne experience: it expands the new perspective, allows measurement and analysis, and provides new forms of beauty. The first telescopes in space were anticipated by imaginative authors and by exacting engineers, whose dreams and proposals have a part in this story. The earliest telescopes to achieve space, the rocket-launched sub-orbital missions, both successes and failures, will be described, along with the effect they had on science and culture. Telescopes in orbit and in space probes are the current generation of instruments, a prelude to a future of lunar and planetary telescopes. Every success can be seen to have had a direct effect on the widening of horizons provided by the telescope. This paper will serve as an introduction to a very extensive subject.

Key words: Rocket-launched telescopes, space telescopes

1. INTRODUCTION

To be transported from Earth into outer space is an immemorial dream of humanity. Once this dream has been achieved, a profound augmentation of the journey into space is to travel with a telescope, to look down on the Earth from the new perspective and to look out at the heavens with new abilities. The hope of placing telescopes in space was a significant motivation behind the early space program.

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In addition, the development of the telescope was given new momentum by the space program. An important chapter in the history of the telescope is the expansion of bandwidth and increase in resolution that was enabled by space-based observatories.

The story of the space telescope can be founded in the earliest airborne telescopes—the balloon launched instruments of the late eighteenth century. The age of flight brought about significant changes in the insight and outlook of humanity, and likewise, the introduction of the telescope greatly expanded our field of view. When we moved off the Earth's surface, our experience aloft was—from the beginning—augmented by the telescope, as documented in the accounts of early balloon launches. When we bring a telescope to an elevated position, the nature of our observations and the nature of the telescope are changed, lending new perspectives to the instrument, the observer, and the observations.

2. THE ROCKET-LAUNCHED CAMERA[1]

However fascinating and romantic are the earliest tales of airborne telescopes, the subject at hand begins with rocket launched cameras, in France, in 1888. Amedee Denisse, a pyrotechnist, designed a camera that fit inside a rocket, exposed twelve photographs using twelve lenses, and was equipped with a parachute for recovery; but is not known to have been constructed or used.

A German patent was issued in 1891 to Ludewig Rohrmann, for a camera launched in a rocket, and suspended from a parachute in use, with a clockwork drive for shutter & film advance, but again this probably never was built.

In Sweden during 1897 the famous Alfred Nobel patented a rocket for photographic use, and constructed a working example. An aerial photo of a Swedish village was exposed in 1897, although sources vary on whether this was shot from one of Nobel's rockets, or from the balloons he also used for aerial photography.

Alfred Maul of Dresden designed, built, and launched in 1904 a much more advanced photographic rocket. His early models flew to 300 meters, but both the shutters and the parachutes proved problematic. In 1912, Maul launched a rocket to 800 meters, carrying an 8 × 10 camera, with a lens of 138mm focal length, which was quite reliable and captured very fine detail in a landscape photograph. The camera was mounted on a gyroscopically-stabilized platform, and used an electro-pneumatic shutter, which was fired at maximum altitude by an inertial switch, which also released the parachute. The rocket used black powder, and had a primitive booster system: a long cord from the rocket went around a pulley to a heavy iron weight, which was released at ignition, when it dropped to provide extra boost to rocket. Maul was an engineer involved in military reconnaissance, and he continued work until 1914.

3. SPACE TELESCOPES, SPACE PROBES AND SPACE STATIONS

In 1908, Robert Goddard (1970, 1: 14) wrote in his notebooks about sending a camera to orbit other planets, after which it would return to the Earth with film. In 1914, he wrote a draft of his Smithsonian paper "A Method of Reaching Extreme Altitudes" (subsequently published in 1919), where he calculated the resolution and field of view for a camera orbiting Mars and the Moon, noting the potential for observing the far side of the Moon, the solar corona, comets, and the zodiacal light (Goddard, 1970, 1: 129). In March 1920, he wrote a report to the Smithsonian elaborating on his ideas: "Taking of Photographs near the Surface of the Moon and the Planets ... The telescope, for lightness and compactness, must be reflecting, and be made compact by several successive reflections ... all reflecting surfaces being as thin as possible consistent with rigidity." (Goddard, 1970, 1: 414).

Hermann Julius Oberth published *Die Rakete zu den Planetenraumen* (*The Rocket into Planetary Space*) in 1923. In 92 pages, Oberth outlines the theory and design of a space probe, including observation stations, where ultraviolet stars could be seen, where stars would not flicker, and where observations of the Earth and its geography, ethnology, and weather, could be conducted. In 1929, he published *Wege Zur Raumschiffahrt* (*The Way to Space Travel*). These books were highly influential in the German rocket community, which was the nucleus for the American and Russian space ventures. Oberth published *Menschen im Weltraum* in 1953, translated and published in 1957 as *Man Into Space: New Projects for Rocket & Space Travel*, with the most detailed account of his ideas. He realized that telescopes in space can be much larger and of any focal length, due to freedom from gravity; they are free from atmospheric scattering of light; and need not counteract the motion of the Earth while guiding. Asteroids are considered as possible platforms for enormous telescopes. However, Oberth's prognostications are not always well-founded; he notes "Today's optical technique is so advanced that banks of clouds would be no hindrance to observation." (Oberth, 1957: 70), and "Photographing the sky with giant mirrors of this kind would enable no important advance to be made as compared with present-day sky photography from the Earth. The Bernhard-Schmidt lens could not be used and the negatives would not be sharply defined at the edges. The superiority of the space telescope would be rather in the visual field." (Oberth, 1957: 217). However, Oberth was probably the first to coherently outline the space telescopes of the future, and directly inspired those who built the first orbiting instruments several decades later.

Herman Noordung, of Slovenia, published in Berlin, 1928-1929, *Das Problem der Befahrung des Weltraums – der Raketen-motor* (*The Problem of Space Travel - The Rocket Motor*). This was far more detailed than earlier works, and includes engineering details and calculations on space stations and their uses. Noordung's design was the now-classic rotating wheel, with a hub that counter-rotated to remain stationary and included an observation station, pressurized and shaped like a boiler tank, to be used for astronomy and for terrestrial mapping, weather, and surveillance. In the section, "Telescopes of Enormous Size", Noordung writes that weightlessness allows the construction of telescopes one kilometer in length, using parabolic mirrors without mechanical support but adjusted by electricity. Noordung's work was highly influential, and Wernher von Braun used it in the design of the V1 and V2 rockets and later space vehicles.

By 1931, the awareness of what we were missing by existing under an opaque atmosphere had become commonplace to astronomers. William Jackson Humphreys, a spectroscopist and meteorologist at the University of Virginia, USNO, and the U.S. Weather Bureau, wrote about "Mining the Sky for Scientific Knowledge", regarding meteorological data, but including astronomy's need for detection of radiation outside the visible wavelengths. Referring to radio frequencies, Humphreys (1931: 23) writes, "Possibly we some day may be able to send a rocket, with a radio-recording attachment, beyond the conducting limits of our own envelope, and thus know definitely whether static of appreciable intensity is or is not knocking at our outer door." About ultraviolet wavelengths, he writes, "How we long for like confidences about the sun and the stars, and how fretfully impatient we become over the tantalizing fact that we can have them in abundance for the mere asking if only we will go a little way, merely to the confines of our own atmosphere, for them." (ibid.).

By 1933, Henry Norris Russell was lamenting the current state of spectroscopy, wherein most of the observed spectral lines had been identified and new knowledge had to wait until we were free of the atmosphere that limited our observations. He anticipated an observatory on the Moon, but not via rocket ship, instead via heavenly ascension, in his paper "Where Astronomers Go When They Die"; hoping that "The good spectroscopist ... might perhaps be permitted to go, when he died, instruments and all, and set up an observatory on the moon." (Russell, 1933: 113).

4. THE CONTRIBUTION OF SCIENCE FICTION

The earliest extra-terrestrial telescopes to be found in science fiction occurred at about this time. *Old Faithful*, published in 1934 by Raymond Z. Gallun, concerns a Martian who uses a telescope with a mirror of spinning liquid mercury, an idea presumably drawn from Robert Wood's contemporary work with such mirrors. The mirror was spun slower to lengthen focus and increase magnification, zooming in on an area of the Earth. Furthermore, the telescope was used with a light source to signal Earth.

Another science fictional telescope appeared in Robert Shirley Richardson's (1940b) "Luna Observatory No. 1", published in the February 1940 *Astounding Science Fiction*. Richardson (1947a) later detailed his ideas in an *ASP Leaflet* titled "Astronomical Observations from the Moon". A lunar telescope is driven so that it rotates about the polar axis once every 27 days. The polar axis is aligned to a spot near the star 36 Draconis, and lunar precession of the equinoxes circumscribes the star every 18.5 years.

An orbiting space telescope is not found in fiction until 1952, when "Star Tracks" by Sam Merwin, Jr. was published in *Astounding Science Fiction*. A research observatory on a space station, uses an 'electro-telescope', with an eyepiece. The increased resolution of the space telescope permits scientists to discover that "… only Earth is real as we know it, only Earth and perhaps the Sun ... Our atmosphere has hid the truth from us. The other planets are mere disks or marbles ... the stars, whatever they are, move on tracks on the inner side of a dark barrier that encloses us ... astronomers are the greatest fools of all; and our universe is property – whose we cannot guess." (Merwin, 1952: 151). The corollary of this realization is that there is no escape, and "Earth must struggle along on the planet it has ravaged" (ibid.). The shock of this discovery gives way to the realization that "Out there is a barrier, a barrier studded with what we call stars. It may not be so far away as we thought ... Are we going to be contained forever within it, never seeking to learn its nature or what lies beyond?" (Merwin, 1952: 152). To actually read this story is to be underwhelmed by its profundity, it is a 'space opera', but in spite of that it carries an underlying profound theme, namely, that any improvement in instrumentation can bring unexpected input, and a huge step such as placing a telescope into space can really shake our foundations.

The place of science fiction in the chronology of the space telescope might need some justification. Simply put, the earliest space telescope was an imaginary space telescope. The first known publication of the idea of an orbiting telescope was in the mid-twentieth century, but there is no doubt that imagination sent a telescope to space long before that era. More importantly, these imaginary instruments have their place in this history because of the inspiration they provided to the pioneers in this field. Hermann Oberth, at 11 years of age, read Jules Verne's *Journey Around the Moon* and repeatedly read Verne's *From the Earth to the Moon* (Gartmann, 1956: 49), while Lyman Spitzer (1989: 11) recalled "… my early concentrated reading in science fiction ..." and " … my long and ardent background in science fiction ..."

5. ROCKET-BORNE ASTRONOMY

The recognized beginning of rocket-borne astronomy was on 10 October 1946 when a group from the U.S. Naval Research Laboratory, led by Richard Tousey, launched a rocket-borne camera to 55 km altitude and obtained the first photograph of solar UV spectrum to 2100 Å (Baum, et al., 1946). Previous spectra had only extended to ~2900 Å.

Also in 1946 was the report by Lyman Spitzer that laid the foundation for future American efforts. "The Astronomical Advantages of an Extra-terrestrial Observatory" was written for the Douglas Aircraft Company (see Anonymous, 1946), but it was not distributed or published until decades later. In it, Spitzer discusses astronomical observations from space that could be conducted without a telescope, with a 10-inch telescope, and with a 200 to 600-inch telescope. The justification for the very large instrument was to "… uncover new phenomena not yet imagined, and perhaps to modify profoundly our basic concepts of space and time." (Spitzer, 1997: 379). This rationale was to prove very influential, since it separated the need for a space telescope from any specific issues in astronomy, and thus future budget battles need not be tied to a scientific question that could be answered and closed. However influential these ideas were in later years, the report had no effect for some time as it was classified, and in fact Spitzer had considerable difficulty in raising interest in space projects, which were so expensive that they were often felt to be detrimental to astronomy as a whole. At the time, there was no corporate or civilian funding of science in the amounts needed for space science, and it was felt that such work was most likely to be engaged by the military. Of course, the cost of space projects was seen in the light of the considerable risks involved in launching instruments, as reflected in Spitzer's recollection: "At the beginning there was widespread doubt as to the practicability of such ideas. One astronomer with whom I was discussing a projected orbital telescope remarked to me, 'Lyman, you're young. You'll live to see it fail.'" (Spitzer, 1997: 369).

Douglas Aircraft issued another report on this subject in 1946, titled "Preliminary Design of an Experimental World-Circling Spaceship". It cites Harlow Shapley's view "… that measurements of the ultra-violet spectrum of the sun and stars would contribute greatly to an understanding of the source of the sun's surface energy, and perhaps would help explain sunspots. He also looks forward to the satellite observatory to provide an explanation for the 'light of the night sky'... Because there would be no scattering of light by an atmosphere, continuous observation of the solar corona and the solar prominences should also be possible. Astronomical images, could, of course, be sent back to the earth from an unmanned satellite by television means." (Logsdon, 1995: 243).

Between March 1952 and April 1954, Collier's magazine published eight issues with a theme of space exploration, and these were highly influential on public policy and on the next generation of space engineers. Chesley Bonestell painted "Space Station over Central America" to illustrate Wernher von Braun's "Crossing the Last Frontier", which details the functions of a telescope on a space station. Surveillance of the Earth will make it difficult for preparations for war to proceed. The spinning space station will render impossible long-exposure astrophotography, and manned vessels would cause vibrations during exposure, so the astronomical observatory would be separated from the space station. Of course, astronauts would have to be at hand to change the film in these cameras. "This mapping of the heavens will produce results which no observatory on earth could possibly duplicate." (Von Braun, 1952: 27; cf. Logsdon, 1995: 186).

In 1952, Arthur C. Clarke published "The rocket and the future of astronomy" as an *Occasional Note of the Royal Astronomical Society*. His comments include, "The atmosphere has long been one of the most serious – and apparently insuperable – obstacles to astronomical research ... The only complete answer is to use instruments above the atmosphere ... perhaps even high-definition observations of planetary surfaces, by the employment of very long-focus television cameras ... automatic surveying of the nearer planets, by 'reconnaissance rockets' carrying television equipment ... the construction of really large astronomical instruments might be possible, since all gravitational forces would be virtually absent and the very lightest methods of construction could be employed." (Clarke, 1952: 8-9).

In 1953, Homer Newell issued a summary of the first half-decade of rocket missions, which after a half-century is notable for its length, listing 165 research-oriented rocket launches between 1946 and 1952 (including those used for high altitude photography). Newell (1953: 283) wrote: "... the earth and clouds have been photographed from the great altitudes reached by rockets. The last is the most spectacular use of photography in connection with rocket research. It arouses universal interest. One cannot yet ride the rockets into the upper atmosphere to see for himself what is to be seen, but one can send the automatic camera aloft as a substitute in this adventure. By means of its record one can imagine more easily what it would be like to be looking out upon the earth for hundreds of miles in all directions, with giant mountains, bays, lakes, and rivers reduced to miniature size, and with the curvature of the earth plainly visible."

Leo Goldberg summed up the situation in 1961 in evocative phrases: "Since 1946, dozens of astronomical rockets have been flown to higher and higher altitudes with better and better instruments. For a few fleeting moments during each flight they have given us glimpses of the universe which are radically different from those seen by ground-based telescopes. They are like scenes shown in a movie theater to advertise a coming attraction, the full drama of which will unfold when astronomical telescopes are placed in a more or less permanent satellite of the earth or in an observing station on the moon." (Goldberg, 1961: 112).

Orbiting radio telescopes were proposed at an early date. On 13 October 1952 Fred Whipple addressed the Second Symposium on Space Travel, at the Hayden Planetarium in New York: "Large regions in the radio spectrum will be opened up by the existence of the station above the ionosphere, and we may expect an extensive branch of radio astronomy to develop from the satellite vehicle. Furthermore, there exists the possibility of constructing huge antennas in space, so that the resolving power of radio astronomy may be greatly increased." (Whipple, 1953: 151). In 1958, a proposal for an orbiting radio 'directional antenna' was described by Lawrence Aller, Leo Goldberg, Fred Haddock and William Liller, aimed at several objects of observation, especially the Galactic background radiation (Aller, et al., 1958).

6. THE 'SPACE SPYGLASS'

Telescopes for visual as opposed to astronomical use are not usually considered in a space context, but have a place in this history. The 'space spyglass' saga begins with the Mercury spacecraft, which included a periscope for observation of the Earth, built by Perkin-Elmer, with a field of view of 175°, requiring rare-earth glasses and strong fourth-order aspheric lens profiles. The periscope was housed beneath a cover, and extended for use by the astronaut.

Use of the periscope became problematic with the launch of astronaut Ham. It was thought that Ham could probably utilize the instrument and even enjoy the view, but he would be unable to report on the images—being a chimpanzee. Thus, so as to avoid wasting this mission's periscopic opportunity, a special movie camera was built to fit inside the periscope and capture the entire 175° view on 16mm film (Cox, 1962).

The periscope was eliminated from the Gemini capsule, but in 1965, the Gemini Seven astronauts were supplied with a true space spyglass, a handheld telescope by Aerospace Controls Corp., magnification variable from 2× to 10×, used for viewing as well as measuring. The field of view was quite confined, 18° at 2× and 3.6° at 10×. Six of these telescopes were built and delivered to Houston (Anonymous, 1966).

7. PROBLEMS OF POINTING AND TRACKING

The most interesting topic in the development of orbital telescopes concerns the mechanical systems used to control pointing and stability. Here we have a problem that seemed hopeless in 1950, and was met so successfully that within a half century the technology had developed to the point where orbiting telescopes could be more precisely oriented than terrestrial telescopes.

In the beginning, one of the first purposes of cameras in rockets was for orientation; thus, the telescope could be an auxiliary to the primary instrument. The earliest rocket observing platforms were designed to function within a spinning rocket, which was often yawing as well, so that the cameras provided scans rather than images. As imaging developed, researchers had to deduce the direction of view for every moment of flight, using elapsed time and the trajectory of the rocket. Tousey's 1946 achievement was based on his new spectrograph, with slit replaced by two spheres of lithium fluoride, polished to 2mm in diameter, which provided a spectrum over a very wide acceptance angle.

The first instrument to track the Sun was built by Merle Tuve's Applied Physics Laboratory at Johns Hopkins University and launched on 29 July 1947. The design used a corrugated cylinder to reflect sunlight, and this mirror was rotated until the slit was illuminated and then stabilized using servos controlled by phototubes (de Jager, 2002).

In 1952, a University of Colorado group developed two-axis stabilization, which could point in a desired direction, but rotated about the line of sight, and was limited to solar use. Viking rockets used the 'Sun-followers', with a photoelectric sensor directing a servo system, as described by Tousey (1953), who compares the great difficulties in stabilizing with "Data recovery [which] has been effected with comparative ease. If the rocket is blown apart on the descent, the several parts fall rather slowly, owing to loss of streamlining. In this case photographic records are usually recovered without damage, and spectrographs have sometimes been retrieved in excellent operating condition." (Tousey, 1953: 659).

Also by 1953, gyroscopes were used, with limited accuracy because rockets moved violently and erratically, causing precession of the gyros. Photocells recorded the position angle of the Sun, and magnetometers the orientation of the Earth's magnetic field.

Achieving precision in pointing was made far more difficult as payloads grew to weigh tons, and objects of view were extended from the Sun to very faint celestial objects. The successful strategy was to move the satellite in a desired direction by rotating an internal mass in the opposite direction. At a conference in 1960, Lyman Spitzer introduced an innovative stabilization design, using a spherical aluminum shell, suspended in a magnetic field that was varied to induce rotation of the sphere, which in turn caused counter-rotation of the spacecraft. He describes the perturbing torques that act on spacecraft and must be accounted for in guidance: bombardment by particles and air molecules, solar radiation pressure, gravitational torque, and the effect of the Earth's magnetic field effect on metals, electric current, and rotating parts. This design was for a space observatory with a distinctive instrument, the 'anti-star telescope', to be used when a guide star is occulted by Earth, when the anti-star telescope, pointing to the opposite area of sky, maintains alignment (Spitzer, 1960: 248).

The precision with which HST can be directed and stabilized is a prelude to the multi-satellite technology to be used in future missions such as the 'Space Interferometry Mission' and the 'New Worlds Observer', when orientation will be maintained to within microns.

8. CONCLUDING REMARKS

The issues under discussion have been the importance of the telescope in space science and the importance of space science in the development of telescope. However, when the centenary of the first space missions is celebrated, the instruments will take second place. The milestone of the current era will be the expansion of astronomy into observations across the entire electromagnetic spectrum. From the perspective of the future, it will seem as if astronomers had been wearing blinders, until a very recent short epoch—which we have been lucky to live through—when nearly the entire spectrum illuminated to our view.

9. NOTES

1. The following review is based mainly on Gartmann (1956) and a variety of web sites.

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THE HISTORY OF SPACE ASTRONOMY:
Reflections on the Last Three Decades

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Abstract: Various themes in the history of space astronomy for the last three decades are examined, with emphasis on the interdependence of programs, contests for control and the effect of the availability of launch vehicles.

Key words: Space astronomy, great observatories, Hubble Space Telescope, AXAF, SIRTF

1. INTRODUCTION

From the increases in the amount of money spent to support it, to growth in the number of users of astronomical data collected from space observations as well as the number of practitioners of the discipline, to the central role these results have played in shaping the current views of the physical universe, space astronomy has been a remarkably successful endeavour in many ways. So big has the effort become, however, that despite a range of memoirs, historical papers and monographs, and policy reviews, we have available only very limited examinations of the practises and contexts of space astronomy. The roles of industry and national security needs in fashioning space astronomy have drawn relatively little attention despite their obvious and crucial importance, for example. In this paper, rather than dwell on what is missing, I shall present some thoughts on the 'big picture' of the history of space astronomy, in part drawing on what the current literature does tell us. I focus on three main themes:

- Interdependence of programs
- Contests for control

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- Influence of available launch vehicles (or lack of)

This examination will centre on the U.S. and NASA supported space astronomy programs as these have drawn the most historical work to date. In addition, although significant astronomical research has been done by astronauts—for instance, X-ray observations aboard the Skylab space station (Compton and Benson, 1983)—the emphasis will be on big programs pursued by robotic spacecraft in the last three decades.

2. INTERCONNECTIONS

To set the discussion of these themes into the shifting patterns of the overall history of space astronomy, let me offer a tentative periodization. Of course the breaks are not sharp and there is a significant amount of overlap.

- To 1945: *Speculations and Dreams* (for example, Richardson, 1940 and Russell, 1933)
- 1945: *Pioneering Days*. Sounding rockets, aircraft, balloons. Military support. Into the ultraviolet and X-ray. Almost exclusively solar studies (but, see Figure 1) (Baum, 2002; and DeVorkin, 1992).
- 1958: *Start of satellite era and very early learning stages*. Dominated in the early years by solar studies. More emphasis on planetary science than space astronomy. Relative lack of competition for flights. Into infrared, X-ray, and gamma ray wavelengths (Ezell and Ezell, 1984; Hirsh, 1983; Hufbauer, 1991; Newell, 1980; and Tatarewicz, 1990).
- Early 1970s: *Start of national facilities era*. General Observers. Shuttle era. More emphasis on space astronomy (Smith, 1993).
- Late 1970s, early 1980s: *Start of 'Battlestar Galactica' or 'Imperial Era'* with multi billion dollar missions carrying numerous instruments. Extremely competitive situation for new flight opportunities (Smith, 1993; Tucker and Tucker, 2001).
- 1986: *Post-Challenger era* and swing against very big missions.
- Early 1990s: *'Origins'* and the increased emphasis on smaller scale missions and 'faster, better, cheaper' (McCurdy, 2001; and Rieke, n.d.).
- 2000: *The New Conservatism*.

In the early years of space astronomy, its practitioners devoted almost all their efforts to solar observations. But those astronomers who turned quickly after World War II to flying astronomical instruments had soon decided not to continue with these efforts (DeVorkin, 1992). The institutional and technical landscapes, however, were transformed in the wake of the launch of *Sputnik I* in October 1957. Most importantly, a new federal agency devoted to space and advanced aviation, NASA, came into being in 1958. During its history to date, most of NASA's resources and people have centred their efforts on human spaceflight. The pursuit of science, including space astronomy, was nevertheless a very important component of its programs from the space agency's inception. With the start of NASA there began the satellite era in space astronomy. What had seemed impossibly expensive before 1957 was now brought into the realm of the not just technically feasible but also the politically feasible. But with space astronomy missions based in one agency, there were always a range of links between the different set of projects that sat in the set of approved NASA programs and proposed programs. These links can be technical, or scientific, or via the budget, or some combination of these factors. Cost overruns on one project, for example, have to be funded from somewhere, and not necessarily from additional appropriations by Congress.

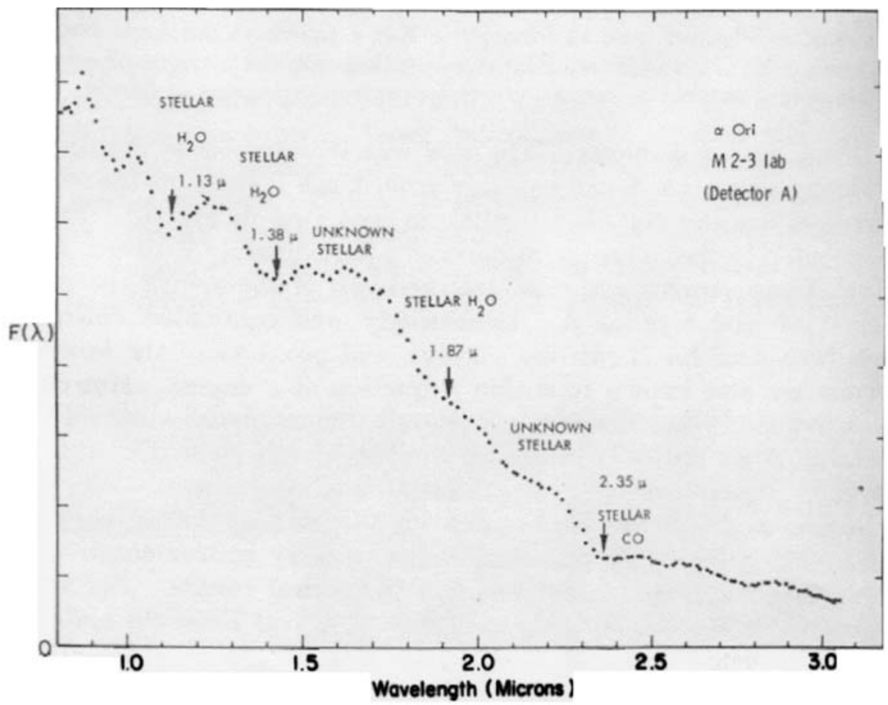

Figure 1. Infrared spectrum of Alpha Orionis, which was obtained by the Stratoscope II balloon observatory in 1963. Water valor absorption bands are visible at 1.4 and 1.9 microns.

In its early years, NASA, while supporting research in other wavelength regions, nevertheless favoured UV and optical astronomy. And researches in the different wavelength regions ran a similar course of relatively simple, survey kinds of missions, leading to highly complex, costly and large spacecraft. The pace at which a particular wavelength region reached this point differed. As UV and optical investigations had gained an early lead, the large-scale observatory in this region came first in the shape of the Hubble Space Telescope, launched in 1990.

The very large-scale missions under planning and construction in the 1970s, 1980s, and 1990s, however, offered not only the promise of extremely exciting science but also the potential for frustrations and big budget overruns. Writing on 2 February 1987—that is, when the shuttle fleets was still grounded a year after the explosion of the Space Shuttle *Challenger*—the director of what was then called the Astrophysics Division in the Office of Space Science in NASA Headquarters wrote a 'Dear Colleague' letter. It was a joke, but it was a joke with serious points and assorted sharp jabs. Jokes can serve many purposes besides attempting to make people laugh: they can act as cultural markers or be a means of diffusing tension, for example. The head of the Astrophysics Division wrote:

> The purpose of this letter is to focus your attention on the continuing opportunity to waste enormous amounts of effort by preparing and submitting proposals for NASA programs which will subsequently be either underfunded or cancelled.
>
> We, at [NASA] headquarters, fully appreciate the status of the space astronomy community in the post-Challenger era. Consequently, we realize that most space scientists are more than willing to grasp at straws. As a result, we are soliciting proposals for our latest program, the Fiscally Irresponsible Astrophysical Super-Collosal [*sic*] Observatory (FIASCO) (not to be confused with currently funded projects).
>
> Once again, we have decided to demonstrate our inability to learn from previous mistakes, and focus all of our attention and funding upon a single ill-conceived financially ruinous project of dubious potential. Nevertheless, we are certain that the astrophysics community, in its never ending effort to obtain funding, will be able to come up with stupefying arguments and, in some cases, actually convince themselves, that such a project is worthwhile.
>
> Three Phase A contracts will be awarded to those proposals which represent the most expensive and technically preposterous concepts. (Pellerin, 1987).

What's going on here? First, there is a sense of irony because the author of the letter, Charlie Pellerin, was one of the architects of the so-called 'Great Observatories program', with its planned complement of large and costly observatories in low Earth orbit covering the wavelength range from gamma rays through the infrared: the Hubble Space Telescope working in the optical, ultraviolet and near infrared, the Compton Gamma Ray Observatory, AXAF (what would become the Chandra X-Ray Observatory), and SIRTF (to be named the Spitzer Space Telescope in 2003) working in the infrared (Rieke, n.d.; Smith, 1993; Tucker and Tucker, 2001). When the memorandum was written none of these observatories had been launched. Also, the notion of the so-called Great Observatories was not the product of a long-term strategy. Rather, it was a selling tactic, one means of justifying a set of large-scale observatories already under construction or in the planning stages. Although it presented a coherent strategy, it came after the fact. By the mid and late 1980s, in fact, what by earlier standards were remarkable amounts of money were being spent on, or scheduled to be spent on, space astronomy enterprises with the Great Observatories at the heart of these efforts, observatories which were reckoned to cost in total several billion dollars to build and launch. Astronomers, too, were far more geared up by this time to advocate these projects than would have been the case a decade earlier. The funding battles to win new starts for HST (known at the time as the Large Space Telescope, then the Space Telescope) between 1974 and 1977 and for the Galileo mission to Jupiter in the same years were especially important learning experiences about fashioning and maintaining coalitions of supporters to push big projects through NASA, the White House and Congress (Smith, 1993: Chapters 4 and 5).

Pellerin's memo from 1987, then, was written at the end of the 'Imperial Era,' with the Hubble Space Telescope being the largest scale effort of all of these. It was very strongly supported in the astronomical community, but was still three years from launch and cordially disliked, in private at least, by some NASA managers and a number of space scientists as, in their eyes, it consumed big sums of money they would have preferred directed towards other projects. For NASA managers in 1987, Hubble's vast astronomical promise had to be weighed against its impact on the NASA space science budget and their ability to fund new programs. By this date, already-approved space science missions were clamouring for additional monies. NASA budget charts have been likened to geological maps: they show different 'strata,' with each layer representing an individual project's funding. The collection of such strata thereby represents all the funding for one area, say, space astronomy. Then, as a project is completed and launched, its stratum is supposed to diminish as the move is made from design and construction to operations, making room for budget increases as new projects reach the design and construction phase (Rieke, n.d.: Chapter 3). In 1987, the Hubble Space Telescope's budget was not shrinking, indeed, it seemed that it would account for a much thicker stratum than anticipated by NASA managers for many more years.

The HST at last went into orbit in 1990. Explicitly designed to be maintained and refurbished in orbit, its first Space Shuttle servicing mission, launched in 1993, cost around $1 billion, which is one reason why initial plans to service AXAF (later to become Chandra) with the Shuttle were dropped. Also, between 1979 and 1985, during its early development, the HST was at times a deeply-troubled project. It underwent assorted crises, and its budget ballooned. In early 1980, the design and development budget was $530 million. By 1984 it was at $1.2 billion, so the cost had jumped some $700 million, and there were more cost increases to come. One result was that other missions waiting to get started suffered delays. Even after the Challenger explosion in 1986, and before its launch in 1990, substantial work was needed on the HST and it had to be maintained in clean room conditions (Smith, 1993). The result was that the HST continued, and continues at the time of writing this paper, to have a very sizable budget (Senate Panel, 2004).

One big mission to suffer it its wake was another Great Observatory, AXAF, later renamed Chandra. Reckoned to be the top priority for a new space astronomy mission in 1982 by the decadal survey, known more informally as the Field Committee Report (of which more later), it finally got new start approval in 1989 and was launched in 1999. It underwent its own assorted budget and technical crises. For example, even after approval to build the spacecraft had been given by the White House and Congress, it was very substantially restructured. In 1992, NASA Headquarters, fearing that in the prevailing political climate AXAF as originally approved might not survive, prevailed against the outraged opposition of many scientists in breaking AXAF into two missions. The first was to focus primarily on imaging, and the second, to be launched later, would exploit high-resolution spectrometers. In late 1993, the X-ray spectroscopy mission was cancelled, only for the spectrometer to receive another chance when it was selected for Japan's Astro-E X-ray Observatory. Tragically for the astronomers involve-ed, when launched in 2000, Astro-E never made orbit because of problems with the first stage of its rocket (Tucker and Tucker, 2001: Chapters 16 and 17).

3. WHO'S IN CHARGE

The rocky road to the new start of AXAF raises another issue: who should be in control of space astronomy programs? One way to interpret some of the history of space astronomy is as a contest between NASA and astronomers outside the agency on the decisions about which missions should fly, as well as important decisions concerning those missions. At times these contests can be very sharp. For example, in 1975, one of the leading astronomers outside of NASA involved in the planning for the Space Telescope (later Hubble Space Telescope) complained to the Project Scientist:

> NASA seems to be controlling the [Space Telescope] program to an extent that would be unthinkable in their successful high-energy programs (or even their [Orbiting Astronomical Observatory] programs). For example, there are no full-time non-NASA employees on the program (contrast this with the stable of fine young scientists that work for [Riccardo] Giacconi [on a high-energy satellite] and [Lyman] Spitzer [on Copernicus]). NASA officials … participate in our working sessions (inconceivable in the high-energy programs). (Bahcall, 1975).

In his reply, the Project Scientist described the particular NASA officials as the "enemy", and reckoned that "These bureaucrats can immolate [the Space Telescope] if we let them, and letting them will occur if we don't make them face us in person. Otherwise, they'll stay away and do things their way." (O'Dell, 1975).

Such fights over control go back to the very origins of NASA. After the launch of Sputnik in 1957, and with the creation of a space agency soon very much in the air, the National Academy of Sciences sought to make its presence felt in the fashioning of space policy. Its chosen instrument was the 'Space Science Board,' a group of elite scientists who would debate and make recommendations on space science issues. As Norriss Hetherington (1975) explained some years ago, the Academy was pressing to ensure a strong measure of oversight of the new agency, which would become NASA.

After it had been established in 1958, the Space Science Board issued a call for proposals for projects that would follow on those programs conducted as part of the International Geophysical Year. This drew around 200 replies and the Board also offered its recommendations to the fledgling NASA (Smith, 1993: 188). But as one senior NASA manager recalled in

1980, "NASA's position was that operational responsibilities placed by law upon the agency could not be turned over to some other agency. Moreover, decisions concerning the space science program could not be made on purely scientific grounds. There were other factors to consider, such as funding, manpower, facilities, spacecraft launch vehicles, and even the salability of projects in the existing climate at the White House and on Capitol Hill—factors that only NASA could properly assess." (Newell, 1980: 204). In other words, "we're in charge." However, the Space Science Board did come to assume great importance in NASA planning and was even to become considered by some observers as a sort of shadow government, for without strong Space Science Board support a mission would lack a critical aspect of legitimacy.

At various times in the 1960s, 70s and 80s, a number of astronomers sought to create or remake other institutions to increase their control over space science projects. A key concern was to involve university consortia, with the goal for some the creation of a university consortium to run various aspects of space astronomy. In the mid-1960s, for example, in part as a means to draw more astronomers into space astronomy, plans were advanced for a consortium to be established called STAR Inc., for Space Telescopes for Astronomical Research. In the 1970s, Riccardo Giacconi, probably the leader in X-ray astronomy, worked away with a number of colleagues trying to establish an X-ray astronomy institute that would sit outside of NASA (Smith, 1993: Chapter 6).

The surveys performed roughly every decade of the state of astronomy were also designed to chart the most exciting possible future directions for the discipline. These surveys would also come to be extremely important in determining NASA's space astronomy priorities. In 1982, for example, we have already noted that the Field Committee chose AXAF as the top new space astronomy priority for the decade. In so doing, it helped drive the planned infrared observatory SIRTF into a series of crises (Rieke, n.d.). Serious planning for SIRTF had actually started before AXAF. The Field Committee, however, reported it had considered programs originally proposed for funding in fiscal year 1983 or later. It had then classed SIRTF as one of the projects that, when work by the Committee had begun in 1979, was already a candidate for a new start in fiscal year 1982 or earlier. Such candidates were not included in the Committee's recommendations (Field, 1982). Through this decision, the advocates of SIRTF were left to fend for themselves as AXAF jumped over it in the queue for a new start. In fact, SIRTF would have to wait until the next decadal survey, chaired by John Bahcall, for strong support. The Bahcall Committee concluded in 1991 that the first choice for a major new program in space astronomy was SIRTF, then envisaged as a 0.9-m cooled telescope. It would be launched by a Titan IV-Centaur rocket into high Earth orbit (Bahcall, 1991).

But to further underline the way in which the fortunes of NASA missions are interconnected, despite the Bahcall Committee's endorsement, SIRTF still failed initially to be included in the NASA budget as a new start. In part this was due to pressure on NASA's space science budget from overruns of about $500 million for both CRAF/Cassini, a very large-scale planetary mission (Harland, 2002) and AXAF, plus the spherical aberration problems associated with the HST mirror (Smith, 1993: 401-414) and the failed high gain antenna on the Galileo spacecraft to Jupiter—to say nothing of the escalating funding for what would become the International Space Station. Indeed, for a time SIRTF was in effect cancelled, only to be reborn as the top priority new mission in the 'Strategic Plan for Space Science' that NASA fashioned during a retreat of representatives of NASA's science disciplines in 1991. This still did not mean a rapid new start. That was to come only some years later, in 1996, with launch in 2003 (Rieke, n.d.).

4. LAUNCH VEHICLES

It is important to note that the finally flown SIRTF (which was renamed the Spitzer Space Telescope) differed substantially from assorted concepts that had been developed in previous years (see Rieke, n.d.). Indeed, it had been defined in the early 1980s as the Shuttle Infrared Telescope Facility as it was originally intended to operate in the Space Shuttle's payload bay.

The SIRTF/Spitzer story underlines the great influence on the history of space astronomy of available launch vehicles—or lack thereof. This is emphasized too, for example, by the story of COBE, the Cosmic Background Explorer. First proposed by researchers in 1974, COBE was being prepared for a 1988 Shuttle launch when the Challenger exploded in January 1986. Soon COBE managers and investigators were hunting for another launch vehicle, but this search was complicated by the USA's dwindling number of expendable rockets that might be exploited for such a launch. The stockpile consisted of a small number of Titan 34Ds, Atlas-Centaurs, Atlases, and Deltas. Another drawback for COBE was that it was slated to enter a polar orbit, which meant that it therefore needed to be launched from Vandenberg Air Force Base in California. This ruled out the Atlas-Centaur as these rockets could not be accommodated at Vandenberg. A few months after the Challenger explosion, a Titan 34D blew up shortly after launch from Vandenberg. About a fortnight later, on 3 May, NASA launched a GOES-G weather satellite aboard a Delta rocket (the space agency's first attempted post-Challenger launch), but the ground controllers had to destroy the rocket following the loss of power in its main engine around seventy seconds into the flight.

By October 1986 it was nevertheless a Delta rocket that was slated to launch COBE in 1989. This, however, forced a major redesign of the spacecraft. A Delta could carry to orbit a payload only about half as large as original version of COBE (which was designed to be launched by the Shuttle). Despite the exceptionally demanding redesign this entailed, COBE was indeed ready for launch in late 1989, and it went on to great scientific success (see Smoot and Davidson, 1993; and Mather and Boslough, 1996). Even as a relatively small satellite, there had been fifteen years between initial proposal and launch for COBE, and this where the scientific groups involved were very experienced in space research and had steadily evolved their plans from instruments flown aboard balloons, rockets, and aircraft.

Even fifteen years pales in comparison to the decades required to launch Gravity Probe B, which is a physics—not an astronomy—experiment, but which surely holds the record for a space science project for the period between the start of serious planning and launch. As its long time project leader, Frances Everitt (1992: 215), has put it:

> Gravity Probe B began with an idea of Leonard Schiff's, first published in *Physical Review Letters*, February 1, 1960. NASA support for it commenced with a modest research grant, awarded jointly to the Department of Physics and the Department of Aeronautics and Astronautics at Stanford University in March 1964 … The eight years to 1971 were spent forming the general scheme of the experiment and developing preliminary component hardware. The first bow to flight was in 1971 when NASA commissioned Ball Brothers Research Corporation … to perform a mission definition study ... The transition to a space program took five years, beginning around April 1979 and culminating on March 30, 1984 in a decision by the NASA administrator to authorize a limited engineering test of flight hardware known as STORE (Shuttle Test of the Relativity Experiment).

Gravity Probe B was finally launched in 2004 at a cost of around $700 Million, which is hardly in line with the slogan 'faster, better, cheaper' (or 'FBC').

Such of course was the slogan on the banner under which Dan Goldin advanced to slay what he saw as the dragon of NASA's overly cautious and spendthrift ways after he was installed as its administrator in 1992. Goldin stressed smaller-scale and less costly space science missions, but ones involving bold technological leaps. In a way, FCB has persisted because it is often all that NASA can afford, but, following some earlier successes, after 1999 the drive to FCB looked very tarnished because in that year there were spectacular ends to three spacecraft built under FCB: WIRE, a wide-field infrared explorer and touted as the first spacecraft in the 'Origins Program', the centrepiece of NASA's revamped space science program of the mid-1990s, vented its entire supply of frozen hydrogen to space before the scientific mission had even started, and two Mars spacecraft, Mars Polar Lander and Mars Climate Orbiter, were lost before they began their scientific missions. The following year, Dan Goldin conceded: "Investigators found resources were spread too thin for success. Too many risks were taken by skipping critical tests or overlooking possible faults. And nobody noticed or mentioned the problems until it was too late." He took the blame for the failures, but at the same time warned there would be no return to the days of big, costly spacecraft (NASA Chief Takes Blame, 2000).

By 2000, the reborn AXAF, now named Chandra and in orbit, supposedly the last of the large and costly spacecraft, looked much more normal all of a sudden. The failures of the Mars spacecraft and WIRE comprised massive political embarrassments. Almost overnight, the bigger missions on NASA's books, including SIRTF and the Next Generation Space Telescope (Figure 2) or the NGST (later to be renamed the James Webb Space Telescope, and in 2000 several years away from its scheduled launch date), adopted much more conservative engineering approaches (although realistic might be a more accurate term). Preparations for the NGST had already been influenced by the HST experience and the desire to avoid some of the pitfalls that had befallen Hubble during its design and construction. Far more resources and money, for example, went into early planning for the NGST to solve, or at least advance the solutions to, technological problems than had been the case with the HST in its early phases. The NGST had nevertheless been capped at $500 Million for political, not technical, reasons and its planned primary mirror size had been upped after Goldin had chided astronomers for not being bold enough. It, too, would become subject to the usual big project dynamics: huge scientific potential, along with the more conservative engineering approach, cost growth and schedule slips.

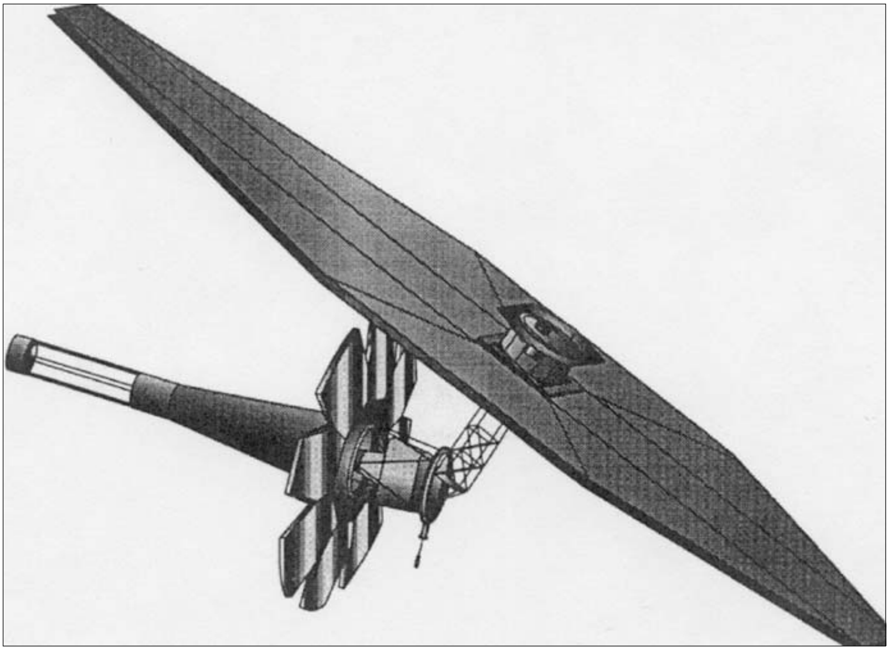

Figure 2. Early concept developed at the Goddard Space Flight Center for what would become the James Webb Space Telescope. This is currently slated for launch in 2011, but serious planning on it began in the early 1990s.

5. CONCLUSIONS

In discussing these sorts of questions, it is important not to lose sight of the key fact that despite all the political contortions, the frequent restructuring of spacecraft and missions, the hunt for suitable launch vehicles and the schedule delays, great things *have* been done in space astronomy. The extension of human observations of the cosmos to wavelengths unobservable from the ground, as well as the more acute observations at wavelengths that do reach the ground, have had an enormous impact on the way astronomers conceive of the physical universe and its workings. The enterprise has also drawn strong public support.

Yet with money always limited, it seems there should be more efficient ways to pursue space astronomy, ways that do not place such stresses on budgets, scientists, engineers and managers as well as on the training of graduate students. The current dilemma for astronomers, NASA, and national and international science policy-makers is to find ways to achieve these efficiencies within the constraints of the existing political frameworks, when often the very act of winning approval for a project can establish major liabilities that will mean cost growth and schedule slips further on.

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SAO DURING THE WHIPPLE YEARS:
The Origins of Project Celescope

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Abstract: In 1955, the moribund Astrophysical Observatory of the Smithsonian Institution closed its doors on the south lawn of the Smithsonian Castle. Vestiges of its 60-year old legacy of monitoring solar radiation were transferred to Cambridge under a new name, the Smithsonian Astrophysical Observatory, and became housed within the Harvard College Observatory complex under the direction of Fred Whipple. Whipple, restarting the SAO almost from scratch, worked within the Smithsonian's ancient tradition of maintaining a world-wide network of solar observation stations by morphing it into a similar network of satellite tracking facilities for the IGY, quickly and quietly phasing out the solar work. Under the SAO name, however, Whipple did much more, vastly expanding his interests in meteor research and hyperballistic studies, deftly orchestrated to parallel his tracking facility empire which in time included aeroballistic studies, atomic time standards, and other associated technological and scientific campaigns. He also made sure SAO played a prominent role in NASA's emerging 'observatory class' series of scientific satellites and used it to create a theoretical astrophysics unit. It is this last activity that we will introduce here, showing how Project Celescope fitted into Whipple's plan for SAO, and how it contributed to make the combined Harvard-Smithsonian Center for Astrophysics the largest astronomical organization on the planet by the 1970s.

Key words: Smithsonian Institution, Fred Whipple, NASA, Project Celescope, Smithsonian Astrophysical Observatory, Harvard College Observatory, space science

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1. INTRODUCTION

In 1955 the Astrophysical Observatory of the Smithsonian Institution (APO) closed its doors forever on the Washington Mall. It had devoted over a half-century's labors to recording the heat of the Sun and searching for correlations between incoming solar radiation and terrestrial climate. Its programs had not changed for many decades, kept alive by the dream of its long-time Director, Charles Greeley Abbot. (DeVorkin, 1990) As an institution, its life was not at an end, however. Hoping that a new Director would bring in fresh ideas and modern approaches, the Smithsonian Secretary, Leonard Carmichael, had found that no one of quality would relocate to Washington to continue work of this type. Accordingly, he began to think about moving the APO to a locale more suitable to forefront solar research. Ron Doel (1990) has shown how Carmichael decided among various options; choosing finally to accept a proposal from Harvard's Donald Menzel in 1954 that would link the Astrophysical Observatory to Harvard, continue the Smithsonian's quest for a link between solar activity and weather, and foster better training of astrophysicists in the United States. What emerged in 1955 was a new form of institutional base for astronomy that provided a new order of magnitude of research potential for solar physics, enhanced training opportunities, new pathways to governmental support and a new institutional platform for launching very large projects. Indeed, by the end of its first year, Whipple's SAO contained "7 people and $50,000 from SI in 1955." But when he turned over the Directorship in 1973, "… there were 307 people on the staff, and a basic Congressional budget of three million a year." (Whipple, 1977: 132).

Carmichael hoped that from Harvard, SAO would continue to encourage and support its traditional program of solar physics, which included a Division of Radiation and Organisms remaining at the Smithsonian in Washington, and three small observing stations in various parts of the world. The transfer to Harvard was in name only, however, allowing Fred Whipple, a Harvard Professor designated the new Director, independent and, for astronomy, unprecedented access to federal and military resources. As a result, the transfer became a wholesale transformation of the APO into the Smithsonian Astrophysical Observatory (SAO), first functionally as a department of the Harvard College Observatory, but soon as a full partner. SAO, in just about six years, grew from near-death stasis into one of the largest, best-funded, most non-traditional, progressive astronomical institutions in the world.

Here I explore one of the ways in which SAO grew so rapidly. This question is central to a larger NSF-funded study of SAO during the Whipple years being conducted by Patrick McCray and myself, where we hope to identify the many agents of institutional growth that built what is today known as the Harvard-Smithsonian Center for Astrophysics (McCray and DeVorkin, 2003). During his tenure as founding Director, Whipple's staff built and managed a huge network of optical and visual tracking stations for artificial satellites and for meteor and upper atmospheric studies, and maintained and vastly expanded Whipple's long-time interests in comets, asteroids and meteors, in the origins of the solar system, and in the evolution, dynamics and solar influences evident in the Earth's upper atmosphere. These studies ran parallel to allied military interests in the upper atmosphere and in hyperballistics that had funded many of Whipple's pre-SAO ventures. But with SAO as an institutional framework and new funding pipeline, Whipple was able to enlarge his earlier interests, explore new areas, and become active in areas which he had previously only encountered in an advisory or passive mode. Included in—but far from inclusive of—this latter category were programs in stellar and galactic astronomy, namely Project Celescope and the Multi-Mirror Telescope (MMT).

Asking how Celescope became an SAO program not only allows us to gauge how SAO grew as an institution, but also provides insight into how traditional astronomers became actively engaged in space research in the post-Sputnik years. Elsewhere I have shown that when means to transport astronomical instruments into space first became available, in the V-2 era of the late 1940s and 1950s, astronomers who initially embraced the concept failed to persist with it, returning without exception to traditional lines of research (DeVorkin, 1992). After Sputnik, some astronomers returned, typically proposing instruments to extend their well-established research interests.

Whipple's case, however, was unique. He was a charter member of the V-2 Panel in the 1940s, an informal but highly effective co-ordinating body for U.S. Army Ordnance that deliberated over, and largely determined, what instruments would be carried aloft by captured German V-2 missiles, to study the atmosphere, near space environment and solar radiation. Whipple stayed with the Panel over its lifetime, and parlayed his membership to join a broad range of advisory committees and panels dealing with the co-ordination of military-funded research. But Whipple never actively proposed, built or flew anything on the missiles themselves, even though he often advised on specific projects. In fact, he was the only member of the Panel not to do so. In part he had been limited from direct involvement by the reservations of his Harvard boss, Harlow Shapley. Carefully navigating his way through the Shapley-centric universe at Harvard, Whipple still found many avenues open to express his views of what space astronomy might look like someday, but through the early 1950s never actually did anything. Sputnik (actually the IGY) and the Smithsonian, as well as Shapley's retirement and his replacement by Donald Menzel, changed all that.

Celescope ultimately became a mapping instrument on the second Orbiting Astronomical Observatory (OAO) launched in 1968 to create a Durchmusterung of ultraviolet sources in the sky (Figure 1). To fully appreciate it as an SAO venture, however, we need to examine its conception and early development in parallel with the other projects Whipple created in his first years as Director. Ultimately we have to identify the nature of the often-complex relationship these projects had to those already underway or initiated during the same years on the Harvard side of the house, especially those fostered by Leo Goldberg after he arrived in the early 1960s. But the present paper looks only at the SAO side of the story in a preliminary fashion, at the milieu out of which Celescope eventually grew, and at its place in that growth pattern. Here we will try to argue that before NASA became the acknowledged agency responsible for managing the infra-structure of space research, people like Whipple tried to assume as much of that infrastructure as possible. Although we cannot prove it in the present paper, our contention is that in Whipple's case, it was a means to accelerate the institutional growth of SAO.

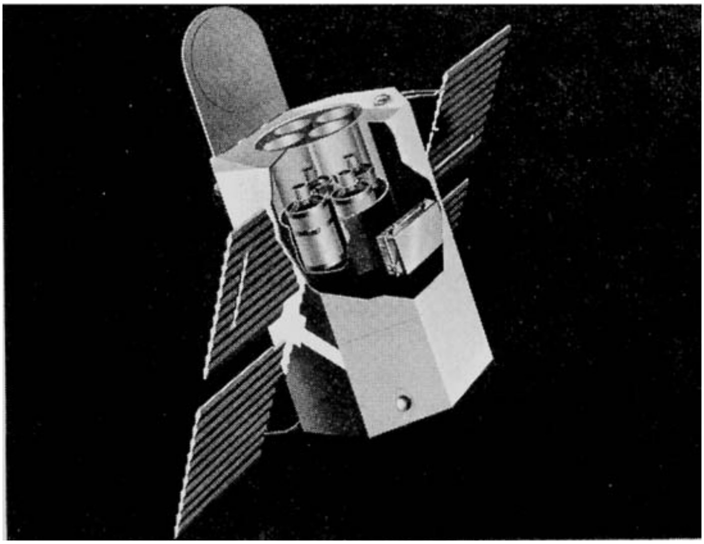

Figure 1. Orbiting Astronomical Observatory. NASA artist concept, circa early 1960s, cutaway revealing the Smithsonian Astrophysical Observatory's 'Celescope' battery of 4 photometric telescopes (Curatorial Files, 'Celescope' DSH/NASM).

2. ESTABLISHING SAO AT HARVARD

An outline of the structure of the SAO in 1956 shows plainly that Whipple's personal interests were already well articulated in meteor studies and in satellite tracking, which he claims in an oral history was the chief reason he pursued the Smithsonian connection (Whipple, 1974). It gave him the means to develop truly large-scale projects that could inform his long-term interests in the nature of the upper atmosphere and the physical and celestial dynamics of meteorites as probes of the history of the solar system.

Indeed, Whipple wasted no time establishing the presence of the new SAO. In early 1956, appealing to his many Washington connections, he secured NSF and SI support to launch a new series of publications very much in the Smithsonian tradition: 'Smithsonian Contributions to Astrophysics'. The first was a set of essays by leading astronomers based upon a conference he organized titled "New Horizons in Astronomy" (Whipple, 1956). Whipple inaugurated this series to "… expand … avenues of publication …" for research in modern astrophysics, but also to provide a forum for hybrid studies not commonly followed by more traditional journals. He was keen to emphasize the "… rapid progress in basic science and its application to technology …" which he argued made "… research in the fundamental astrophysical processes… evermore important. "Meteoric bodies …" for instance, "… ionize the high atmosphere and contribute to its chemical composition, scatter sunlight in the zodiacal light, and provide us with a cosmic ballistics laboratory of ultravelocity particles." (Whipple, 1956: iii). "New Horizons in Astronomy" had been underwritten by both the NSF and the Smithsonian, and was stimulated in part by discussions that had taken place between members of the 1954-1955 Panel on Astronomy Advisory to the National Science Foundation. The Panel had designated Whipple chair of an *ad hoc* committee on the 'Needs of Astronomy' and his new publication series provided a public outlet. Areas of the some forty essays included the traditional problems known to astronomers, but to these he added many that were new to astronomy, such as "Techniques and instrumentation", involving non-optical technologies, and the "related sciences" of rocketry, computing machines, turbulence, astroballistics, electrodynamics of fluids, hydromagnetic and plasma problems. Whipple had been critical of the myopia earlier advisory panels had shown in assessing the needs of astronomy for ONR and then NSF. Now he finally established his own platform (DeVorkin, 2000).

Whipple had proposed the satellite tracking network by the end of 1955, building upon his experience with distributed photographic meteor tracking systems since the 1940s, and Carmichael accepted it because it aligned with national needs and would be a central element of the IGY. He also liked the idea because a global network of tracking systems was reminiscent of the Smithsonian's traditional role in science, embodied not only in worldwide expeditions but also in the APO observing stations. A few months later, Carmichael approved funding for a major symposium and publication on Cosmic Aerodynamics, and also permitted Whipple to pursue major Air Force funding for studying meteoritic accretion by the Earth—another aspect of his continuing attack on the hyperballistics of atmospheric meteors, using them as probes of the atmosphere. By late 1957, Whipple had secured Theodore Sterne to direct the solar astrophysics programs of the SAO, which included oversight of the remaining observing stations. Sterne, however, had a low opinion of ground-based solar constant measurements because he felt it would soon be done far better from a space platform (Hufbauer, 1991: Chapter 7).

By early 1958, Whipple established a new section on upper atmosphere studies within his burgeoning SAO and, in particular, an explicit project to develop UV and X-ray instrumentation for artificial earth satellites to survey the sky in these newly-accessible wavelengths. Unlike other astronomers' satellite instrumentation proposals put forward at this time, Whipple's activity did not stem from any active research program that he had already established either at Harvard or at SAO. Very little in Whipple's own research publications prior to this time reveal an explicit interest in such mapping studies, even though earlier in his career he published broadly on variable stars, novae, nebulae, galaxies and instrumentation, as well as on comets and meteors. From his publications issued as early as 1930 one can find an awareness that the Earth's atmosphere "… prevents our directly measuring the true energy radiated …" by the stars (Whipple, 1930: 34), but this was not a remarkable admission for an astronomer. Evidently Whipple was keenly aware of these logical first steps, and felt he was well positioned once the leap into space became possible. "I always wanted to be in a sort of a ground-breaking field, away from the crowd …" Whipple recalled in an oral history interview. Although he was attracted to radio work in the post-war era, he shied away from radio chiefly because it was already rapidly becoming populated with specialists. In the case of space astronomy, however, there were as yet few who were institutionally as well-positioned as he was to dive in (Whipple, 1977: 76).

3. ACCESS TO SPACE — WHAT TO DO?

Whipple was a frequent writer about space and space travel. In line with early speculative studies by a few other astronomers, in October 1952 at the Second Symposium on Space Travel held at the Hayden Planetarium—on "Astronomy from the Space Station"—Whipple imagined robotic telescopes leading or trailing a manned space station. He envisioned photographic recording and the use of laboratory vacuum techniques for spectroscopy and photometry in the far-UV as well as in the X-ray region of the spectrum. He was among a small group of writers for a *Collier*'s series on space exploration, wrote in a similar vein for *Saturday Review* and for *Sky & Telescope*, and asked "Why Conquer Space?" in Volume 1, Number 1 of the journal *Astronautics* in 1954.

Underneath all of this, Whipple was also a member of a committee later called 'Project Orbiter', which began in June 1954 under the auspices of ONR Air Branch and became a joint Army-Navy proposal to the Department of Defense in early 1955. It was set up to launch the first U.S. artificial satellite, which was initially envisioned as a passive 'slug', and then as a balloon that could be tracked optically for geodetic purposes. As Michael Neufeld (2000: 232–236) has pointed out, Whipple played a critical role on this committee, helping to assess the feasibility of tracking such an object. Whipple's participation in this secret military project also gave him insider knowledge that helped him encourage satellite studies within the National Academy as well as within his own V-2 Panel, which by then was called the Upper Atmosphere Rocket Research Panel (UARRP).

In January 1956, in honor of the tenth anniversary of its unofficial existence, the UARRP sponsored a three-day symposium at the University of Michigan on "The Scientific Uses of Earth Satellites". Whipple did not speak, but asked one of his Harvard students to contribute. Robert J. Davis, building upon work he had done since 1951, performed a set of computations that predicted what a broad range of early type hot stars would look like in the vacuum ultraviolet. He created a rough map of the sky near Hydrogen-alpha, at 1249Å, extrapolating the known Planck functions for normal stars of varying temperatures, with special corrections for peculiar stars like Wolf-Rayets, nuclei of planetary nebulae, and peculiar A stars. He accounted for interstellar reddening and compiled a listing of all objects whose predicted brightness would be greater than +2.0 in the ultraviolet. As an under-graduate student at Harvard, Davis had studied blue-excess peculiar A stars and other hot stars. His computations in 1956 revealed that the brightest stars in the sky in the ultraviolet would be of the Wolf-Rayet class, and that the brightest two of these, Zeta Puppis and Gamma Velorum, were close together in the sky, so that "… a rocket photograph of the Puppis-Vela region of the sky would give valuable and interesting information …" (Davis, 1956: 159).

Davis' analysis was straight forward and shows that Whipple was definitely looking for ways to become directly involved in space astronomy. The specific problem Davis was given has in fact shown up in most of the discussions of what astronomers would do with access to space in the immediate post-Sputnik years. Within the next few years, leading astronomers like Lawrence Aller, Art Code, and Aden Meinel, all called for survey work in the UV to commence as soon as possible. At an Astronomical Society of the Pacific Symposium in June 1959, Meinel (1959: 372) set out the first two steps:

> 1. *Sky Surveys*. At the head of the list of observations in space will certainly be a survey of celestial objects. There will be an early need for the equivalent of the Bonner Durchmusterung and Henry Draper Catalogue surveys to delineate the more astrophysically interesting objects for subsequent researches … While attention is focused on the short-wavelength regions, the infrared regions may hold unexpected surprises for the astronomer.
>
> 2. *Stellar Energy Distribution*. The observational verification of model stellar atmospheres can be extended to the ultraviolet and infrared.

Meinel and others called for surveys of bolometric magnitudes and measurements of stellar energy distributions with filters, or simple spectrometers. By then, Whipple and Davis, along with Gerhard Schilling and Charles A. Whitney, had been pondering options for a satellite program—flashing beacons for geodetic purposes, and a 'space telescope' or 'flying eye' of some sort. From my preliminary examination of their ideas it seems that they felt they were starting from scratch, and would have to generate most of the infrastructure, and appeal to a diverse set of funding sources for support. In January 1958, with the shock of Sputnik barely three months old, no one yet knew who or what part of the Government would be responsible for space: the military services, the National Advisory Committee on Aeronautics (NACA), the newly-created Advanced Research Projects Agency (ARPA), the Atomic Energy Commission (AEC), the National Science Foundation (NSF), after its inception, the Space Science Board of the

National Academy of Sciences (SSB), a combination of one or more of these, or some new hybrid (McDougall, 1984: 166-167).

At this point in my research, I suspect that Whipple's activities over the next year can be understood as an attempt to place SAO as a central co-ordinating institution for space research. In the months during which NASA became established and then worked feverishly to stake out its territory, Whipple was one of those who both applauded the new agency, but resisted relinquishing the institutional infrastructure that was needed to make and operate a scientific satellite. He wanted SAO to build it, track it, predict and analyze its orbital characteristics, acquire its data stream, and reduce, publish and interpret the results. This is the context within which one must work in order to appreciate the role of Celescope in the growth of SAO. Of course, Whipple was already deeply involved in optical tracking, for reasons quite apart from managing a scientific satellite, but there is good reason to suspect that he wanted to position SAO as a central institution that managed the optical and possibly even the radio tracking of a goodly proportion of the American civilian space program, as well as the military program. What follows is a brief summary of the evidence that leads me to suggest this scenario, and it is within this context that Celescope must ultimately be placed.

4. NASA vs SMITHSONIAN

On 27 March 1958, J.J. Love in the Smithsonian's Office of Legislative Affairs, sent Whipple a draft bill entitled 'National Aeronautics and Space Act of 1958' which was proposed by the Bureau of the Budget and the White House and was bound for Congress. Whipple read it and had little comment, suggesting mainly clarifications. He advised Smithsonian Institution Secretary, Carmichael, that the bill "… represents extraordinary wisdom and foresight." (Whipple, 1958a). On the very same day, however, Whipple accelerated his campaign to expand his domain. Writing to A.L. Loomis, Richard S. Perkin and about a half-dozen other benefactors known both to Harvard and the Smithsonian, he pointed out that the SAO "… has become an acknowledged leader in the tracking and theory of artificial earth satellites …" and so was presently in a highly-competitive position to take advantage of a host of proposals coming from a wide variety of Government and military sources. But these proposals would be for programs, not for bricks and mortar. SAO had to secure more real estate in Cambridge and physical facilities for expanded laboratory space, computer facilities, and office space. "We have just been requested to consider assuming the responsibility for pre-launching theory, tracking, computing and analysis for planned around-the-moon satellites …" he claimed, but without adequate facilities, "… we will be forced to reject this proposal and others certainly to come." Echoing his strong assumption that a military agency (such as the Army Ballistic Missile Agency (ABMA), which housed Wernher von Braun's Huntsville, Alabama rocket team and had close ties with William Pickering's Jet Propulsion Laboratory in Pasadena) would assume overall responsibility, Whipple (1958b) made his bid:

> We have the scientific talent and the know-how. Funds from various national sources are now being placed for such research and should go to organizations of the highest scientific competence in the field.

He appealed to the importance of maintaining and strengthening "… our competitive position with respect to the USSR, and time in this case really is of the essence." Although I have not yet been able to competently assess the degree to which Whipple was a sincere Cold War Warrior, it is clear that he did not shy away from the use of such rhetoric to meet his goals. He could indeed appeal to self-interest, and was very aware that Loomis, Perkin or some other friend might one day find their name on a prominent new building in the Harvard College Observatory compound. The 'John Doe Laboratory of Space Research' would be the first of its kind, 30,000 square feet of highly-visible energetic research in support of national goals, and for a mere outlay of $600,000! Whipple (ibid.) claimed the expense would be tax deductible, but Carmichael (1958a) corrected him, cautioning that only a fraction could be claimed.

As he campaigned among private donors, Whipple employed a fiscal strategy that he later described as following the 'brinksmanship principle'. He found he could get competent people by offering permanent employment and Federal Civil Service status. So he would finance administration and support through grants and contracts, and hire scientists and obtain their support directly from the Smithsonian's Congressional appropriation. In the late 1950s, the Smithsonian Institution apparently had a surfeit of high-level posts that were unfilled. Whipple grabbed these Federally-regulated 'Full Time Employee' slots, and used them to hire scientific staff. He then leveraged administrative and technical support from Congress saying his staff needed proper support. And in parallel, he campaigned with private benefactors, pleading for a place to house all these people. And when donations lagged, he backfilled by renting space. Meanwhile, he pushed for increased appropriations when contracts lapsed (Whipple, 1974: 32-35).

In April 1958, Whipple was called to testify before the House Select Committee on Astronautics and Space Exploration. One of the major goals of the hearings was to assess the effectiveness of the NACA model, and to ascertain how a space agency built around the NACA as a core had to change to meet the Soviet challenge. On 16 April, Whipple provided extensive commentary and willingly subjected himself to congressional inquiry. His remarks dramatically reveal his view of the relationship between an eventual space agency and academe. He agreed with the Committee that the space agency had to have "… a very high status in the Government …", to operate much as the Defense Department. Further:

> I think this national space agency should be the coordinating agency for the activities in space research. That does not mean that it should control this activity, but it should be in a position of coordinating. I do not see any agency that is in that position at the present time (Whipple, 1958e: 368).

He felt strongly that space exploration should not be under the control of the military, nor should scientists engaged in space research be "… under the control of the Government." Rather, "… space exploration should be under civilian scientific control …" and the best scientists should reside in academe. Co-ordination, not control, by an informed civilian government agency, letting contracts to universities and industry, and even to the military, was the ideal solution (Whipple, 1958e: 369-370, 372). In response to a direct question from the Chairman, Massachusetts Democrat John McCormick, Whipple agreed that NACA might well act as the core of the new agency, but it would have to undergo "… a complete change of viewpoint." (Whipple, 1958: 373).

A blueprint existed for this change of viewpoint, and apparently members of the Committee agreed with Whipple, because at the end of his very long testimony he was asked to submit documentation into the record that described the contracts and agreements SAO maintained with organizations funding their IGY activities. Chief among those was the National Science Foundation grant for building and operating the world-wide satellite-tracking network (Whipple, 1958: 381).

In parallel with this testimony, and in fact quite distinct from it, Whipple's upper atmosphere group began to focus on building a survey telescope for the ultraviolet mapping of the heavens, and sought funding from the Army. Their goal was to record the brightnesses of stars, and possibly discover new classes of objects. In June, Whipple wrote to Hugh

Dryden (who had led the NACA and was to become second in command at NASA) outlining his ideas and Dryden expressed considerable interest, but his first formal proposal benefited more from contact with William Pickering, Director of the Jet Propulsion Laboratory (JPL), and from others within the military agencies that Whipple had long patronized. Whipple and Davis sent a formal proposal to the ABMA for a small orbiting telescope that would secure, in the simplest way possible, photometric data on bright stars that would be useful in extending knowledge of spectral energy distributions, and would also help to better calibrate bolometric data acquired from ground-based telescopes. This proposal was to parallel a large contract SAO already had with ABMA to manage optical tracking of ABMA missiles and payloads; evidently Whipple was one of those who believed, along with von Braun and William Pickering, that ABMA/JPL would enjoy a central role in organizing space activities, even after the Space Act, operating under something that was then referred to as operation 'Green Door' (the fledgling U.S. Army Ordnance Missile Command). Thus, when NASA asked for a portion of ABMA and contractual oversight for JPL in October, the Army put up a fight, and it was well over a year before both these elements came under NASA control (McDougall, 1984; Medaris, 1960).

Within this heady but far from stable period, when it was still not clear which horse to bet on, Whipple's small but growing satellite team developed their ideas. The satellite payload itself was a rather simple device, as first envisioned by Whipple and his staff. The idea was to develop something quickly for a first glimpse of the ultraviolet sky. The basic optical system in early 1959 was an off-axis Herschelian reflector, which used one optical element to feed some form of suitably-modified electronic image-detector based upon television technology. The satellite design they had in mind went through numerous iterations as they tried to fit the telescope into an essentially spherical buss, because their initial calculations indicated that a long thin tube would suffer all sorts of differential stresses in what was suspected to be a strong magnetic field environment in a low Earth orbit.

Davis worked on many fronts, not just the satellite and payload design. In effect, he, Schilling, Whitney, Karl Henize and others worked as if the Smithsonian would be setting up a complete infrastructure for managing scientific satellite operations, including ground-based tracking stations and data acquisition. One can see this clearly in Whipple's development of optical tracking, orbit analysis and computational expertise, as well as in his reports. Playing their cards very carefully, in September 1958 he and Davis arranged a meeting with advisors from the consultant group Arthur D. Little, Inc. and senior SAO staff to plot out management strategies for the telescope project (Davis, 1958). At the same time—and in response to a request from Congress—Whipple prepared a report as part of final deliberations over what should become NASA's role in space, and what the SAO should retain.

In the fall, as the U.S. House of Representatives Select Committee on Astronautics and Space Exploration debated the responsibilities of the new space agency, a Committee staff member contacted Carmichael and requested a statement from the Smithsonian Institution about its activities "… pertinent to the space program." (Moran, 1958). Carmichael relayed the request to Whipple asking for information, but he wanted to reply to the letter himself as he knew members of the Committee. He also told Whipple that the Committee's chief concern was the satellite-tracking program, and that Whipple should therefore provide specific arguments "… why this work should remain under the administration of the Smithsonian Institution and not be transferred to the National Aeronautics and Space Administration." (Carmichael, 1958b).

Whipple prepared at least three or four drafts for Carmichael. Although they all said essentially the same thing, earlier drafts were more explicit, with a section labeled "NASA vs. SMITHSONIAN" wherein Whipple (1958c) argued that the best research was performed by "Good men, career type …" in an academic atmosphere devoid of classification, such as that found at a university. He predicted that if the tracking responsibilities were transferred to NASA, SAO would lose half of its scientific staff. On the other hand, he saw no problem with SAO acting as a contract service: "… we would be pleased to work under contract with NASA in an academic atmosphere." Whipple always insisted that this was the most promising formula for large-scale scientific activity. He maintained such a relationship with multiple-contract projects for the ABMA, and frequently courted other agencies he thought might emerge as patrons of the space business.

The final draft sent to Carmichael did not retain this explicit willingness to act as a contractor, nor any explicit adversarial language. Rather it concentrated on the strengths of both the precision optical tracking network and its popular component, 'Moonwatch', which by then had attracted a world-wide civilian army of 6,000 volunteers organized into 236 teams (Figure 2). He reviewed its established and functioning nature, its successes, and most of all the fact that "… the administrative, operating, and scientific personnel have gained a singular know-how and a feel for this project." (Whipple, 1958d).

Figure 2. One of the Smithsonian's Baker-Nunn tracking stations (Curatorial Files, "Baker Nunn" folder DSH/NASM).

Satellite Tracking was the fifth and final section of Whipple's statement, but it was the longest and most detailed, and written in a way that implied that all the other sections somehow were related to it. The link was a complete knowledge of the atmosphere and solar-terrestrial relations, which included: 'Solar Astrophysics'—where one of the remaining APO outstations, Table Mountain, was also a tracking station; SAO's 'Scintillation Program', conducted in conjunction with the Air Force, MIT and Winzen Laboratories; SAO's 'Upper Atmosphere' Section, which studied the effect of the Earth's atmosphere on the appearance of celestial objects; and Whipple's continuing interest in 'meteoritical studies', now expanded to include microanalysis of physical structure, isotopic studies and ablation studies, as well as orbital studies. What would become Celescope was also presented in a very exploratory and general way, within the 'Upper Atmosphere' section. The way it was presented, however, made it appear more as a study of how to do such things, rather than as a pointed data collecting or mapping project:

> A new technique that will extend astronomical observations to the far ultraviolet region of the spectrum and to the X-ray region is being developed. This involves the design of a telescope in space and the

completion of related theoretical, scientific, and engineering studies (ibid.)

Implicit in this 'new technique' was a systems approach that included tracking, data acquisition and analysis. For Whipple, the satellite project was not an end unto itself. Although he recognized its importance to astronomy, he was not particularly anxious to see what the data would reveal. He was more interested in building a capability. In all of this, I argue, he was pushing to establish SAO as a central player in space research, much as the ABMA and the SSB and other entities no doubt had been doing. There was something of a 'gold rush'—or better yet a 'land grab'—going on, and no one knew who or what would emerge in control of space.

Between July and October 1958 NASA emerged on the playing field, and pushed to establish responsibility for many of the functions Whipple envisioned as centralized at SAO. It is beyond the scope of this present paper to detail how all this happened, and it has already been well-covered elsewhere (e.g. see McDougall, 1984; Neufeld, 2000, 2005); my purpose here is to identify Whipple's original vision, in order to appreciate better how SAO's working relationship with NASA emerged and how Celescope eventually grew in importance as the tracking activities became part of other agencies and organizations.

By October 1958, NASA was definitely in charge of the overall design of spacecraft, as well as the infrastructure for ground stations and data acquisition. Only then did SAO resubmit its ABMA proposal for a space telescope to NASA, on the surface suggesting that SAO still retain a few of the functions, but it would do so through its specialist interests and not as a central player or facilitator.

In this new mode, Whipple enjoyed considerable access. He deftly applied his elite status and track record in Washington circles, and soon had at least one critical placement within NASA. His former SAO Executive Officer, Gerhard Schilling, was by the end of the year directing the astronomy program within Homer Newell's Division, as NASA Assistant Director of Space Sciences. Whipple was thus well placed for negotiations when, in January 1959, Schilling and Newell asked the leading proposers of astronomical instrumentation to meet and deliberate over priorities and mutual interests.

5. THE ORBITING ASTRONOMICAL OBSERVATORY AS A NASA MISSION

People interested in proposing scientific payloads for satellites had identified themselves by responding to an appeal by Lloyd Berkner, Chairman of the SSB, circulated in the spring of 1958. Berkner canvassed hundreds of scientists asking them what they would do if they were provided with access to space. This produced numerous proposals for all sorts of activities, including solar and stellar astronomy. Whipple, of course, was one of the leading astronomical respondents, along with Lyman Spitzer, Leo Goldberg and Arthur Code. At the time of his initial telegram, Berkner expected that the SSB would co-ordinate and prioritize the responses, but by the end of the year the proposals were handed over to NASA and to Homer Newell (Naugle, n.d.).

It was frustratingly clear to Newell, and Abe Silverstein (NASA's Director of Space Flight Programs) that even by the end of 1958 many of the major proposers had not budged very much from their initial positions, which assumed the flights would be independent and for specific research purposes. Instead, NASA envisioned space research to be conducted as a set of nested 'missions', each with very broad-based and flexible goals. What it wanted from the outset were people willing to co-operate to produce multi-functional platforms (Figure 3). In a December 1958 report to NASA Administrator, T. Keith Glennan, for instance, Whipple still regarded their 'space telescope' proposal as a dedicated satellite to perform multicolor studies of the UV in an orbit between 1,000 and 2,000 km above Earth. "Nearly standard live-television techniques and an 8-inch telescope are envisaged with telemeter communication to two West Coast ground-control stations …", managed by SAO and with a co-ordination center at Cambridge (Keddy, 1958). A survey of known sources —and hopefully new sources— was the primary goal, and the flight package was to be between 100 and 200 pounds.

Accordingly, in February 1959, Newell brought the major proposers together in a working group on what they called the Orbiting Astronomical Observatory program (henceforth OAO). OAO was envisioned as a series of spacecraft capable of performing a broad range of astronomical observations, both solar and stellar. In line with NASA's philosophy of management, OAO was to be the primary responsibility of a new major NASA center based in Greenbelt, Maryland. This facility, soon to be known as the Goddard Space Flight Center, was to oversee a series of contractors for a range of missions that involved mainly unmanned space science and applications satellites. Prime contractor for the OAO eventually was the Grumman Aircraft Corporation (Smith, 1989: Chapter 1).

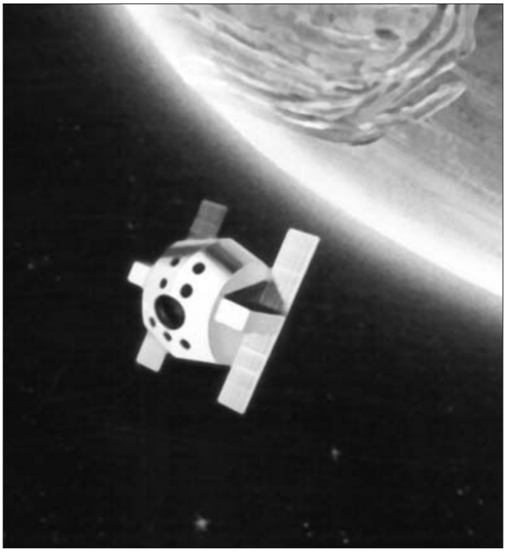

Figure 3. “Telescope Above Atmosphere”. Early NASA artist’s concept for a universal space platform (Curatorial Files, ‘Celescope’ DSH/NASM).

At this first meeting, in February 1959, NASA laid out its own vision of a standardized platform that could handle a wide variety of payloads, including solar, stellar and planetary. Davis came away from one of these early meetings, reporting to Whipple that NASA was to assume responsibility for stabilization, telemetry and the ground stations. Whipple took this as a challenge, redoubling his efforts to create a comprehensive developmental program. He accepted NASA’s existence, but resisted the ‘cookie cutter’ model of a universal platform, as did Spitzer and Goldberg. The astronomers did not share NASA’s mission-oriented philosophy, that all sorts of astronomical observations could be performed from a standardized platform. NASA also thought that SAO’s original proposal for a single small telescope in orbit was too restrictive. Whereas Whipple wanted to establish a comprehensive space operations facility using the simplest form of telescope as a foot in the door, NASA wanted SAO to concentrate on a more sophisticated telescopic array, and leave the housekeeping to them.

This was very much the state of affairs through much of 1959, with each proposer accepting preliminary funding for feasibility and requirements studies while NASA established infrastructure with industrial contractors. But from the outset there were tensions. My present impression of the situation is that NASA attempted to limit the disbursement of funds in order to control the scope of activities in each group. SAO, for instance, wanted to develop a series of Aerobee-Hi sounding rocket test flights from Wallops to refine not only the telescope payload but also ways of getting the data to the ground via its own tracking facilities. NASA hesitated to fund the facilities, and, of course, SAO objected. Working under what had been a $1.3 million dollar request for FY59-60, in September 1959 NASA approved only $300,000 of it, and then did not release the funds. This drew repeated complaints from SAO that without the funds they would not be able to contract for their ground stations' needs to manage the sounding rocket flights in a timely fashion (Keddy, 1959).

This resistance continued through 1959 and 1960. At one point, in February 1960, Newell thought the situation was sufficiently stalled to warrant briefing Glennan, especially since an external advisory committee that had been reviewing NASA priorities was likely to cause renewed problems with unruly scientists. The so-called 'Purcell Committee on Space' was created originally as a President's Science Advisory Committee (PSAC) panel in February 1958, and it had done much of the preliminary spade work that helped to define NASA. It was still more or less in business in late 1959, continuing to examine priorities for space research. At the end of the year it vigorously endorsed NASA's OAO program, but criticized it as still not completely defined and seriously under-supported, especially in the assignment of suitable launch vehicles. Moreover, the group was very concerned that an internal NASA committee was designated to choose between competing in-house and out of house proposals for scientific experiments. And it remained concerned that external "… experimenters disagree with having a universally useful and adaptable stabilized platform." (Newell, 1960a).

Newell was worried that the external scientists were trying to regain some control over the program. So in his brief for Dryden and Glennan, he revealed a definite strategy for outmaneuvering the scientists. A meeting was set up for members of the Purcell Committee from the White House (Jesse Mitchell and George Rathjens), Edward Purcell (Harvard), and also invited representatives from NASA contractors, where they would be briefed by Newell and Roman and then by James Kupperian (Project Scientist at Goddard Space Flight Center for the OAO), but none of the experimenters was invited. Newell's (major strategy was to reconfirm NASA's multi-institutional and general-use mission with the Purcell Committee. (Newell, 1960b). In his memoir covering this period, Newell (1980: 121) had a somewhat different take on the situation, which needs to be fleshed out:

"When astronomers could not agree on specifications for the orbiting astronomical observatory, and NASA found itself in the middle, the (Purcell Committee] pushed NASA to resolve the difficulties."

Newell's recollection is accurate but misleading. At the time, each of the major players wanted to run their own project. What they did not agree with was NASA's view. In Newell's briefing for Glennan in February 1960, he indicated as much to Dryden and Silverstein. Each was "… obviously interested in controlling the project and directing it to meet primarily their own needs." (Newell 1960a). The worst example for Newell was Whipple: "In the early days of getting going on this activity, Professor Whipple had in a proposal to undertake the management and conduct of the entire job. With this arrangement the proposed satellite would do very well for Professor Whipple's experimental needs." (ibid.).

Resolution with NASA was ultimately obtained in practice, however, largely according to the astronomers' views in that NASA agreed to separate solar and non-solar missions using very different platforms, and also agreed to outside peer-review evaluation of scientific proposals. But these proposals would be entertained specifically within parameters outlined by an overall mission profile set largely by NASA, and NASA would retain overall responsibility for infrastructure. Exactly how this separation came about will be a subject for further study.

It was this mission profile that continued to concern the astronomers. In June 1960, Goldberg, Spitzer, Code and Whipple pleaded for smaller initial projects to gain "… preliminary experience …" in building and operating spacecraft instrumentation (Smith, 1989: 40). Nevertheless, in the next few years the OAO program became NASA's largest and most complex scientific spacecraft series. Each platform would weigh some 3,900 lbs, with 440,000 parts and some 30 miles of wiring (Rudney, 1971). The first OAO was scheduled for launch in early 1966 on an Atlas-Agena rocket, with the Smithsonian Institution sharing space with the University of Wisconsin; the second was reserved for the Goddard Space Flight Center; and the third for Princeton, although each one also carried arrays of smaller instruments. Celescope also soon became something far more than a single 20.3-cm (8-in) telescope (Figure 4). Within a year, it was clear that many of the initial assumptions about available or adaptable technology were woefully naive. Paramount were the image detectors, which ultimately delayed Celescope from flying on the first mission. After several abortive attempts, SAO staff contracted with Westinghouse in July 1959 to develop an ultraviolet-sensitive television tube based upon its 'Vidicon' design. Called

'Uvicons', the development was slow and costly, partly because of the "… evident lack of commercial and military applications of these tubes." (Whipple and Davis, 1960: 287).

Figure 4. Pre-integration checkout of the battery of 12-in telescopes. The next step would be placing the payload within the satellite housekeeping shroud. Circa mid-to-late 1960s (photograph courtesy Bill Waggener. Curatorial Files, 'Celescope' DSH/NASM).

By 1963, Celescope had grown from being a small program within the 'Upper Atmosphere' Division into a virtual SAO Division in its own right, though it was still identified within a new Division called 'Stellar Observations' which was dominated by programs related to the mission. By then, some twelve professional staff led dozens of specialists and technicians in problem areas identified as 'UV sensitive TV cameras', 'UV pointing sources', 'UV identification catalog', and 'SAO Star Catalog'. In addition, at least one program in the 'Stellar Theory' Division covering studies in non-grey stellar atmospheres was closely related to Celescope. Explicit Divisions devoted to the IGY 'Satellite Program', or in following years to the 'Satellite Tracking Program', or even a Division explicitly devoted to 'Space Science' (which was formed in 1961 as an umbrella to coordinate SAO's many activities in studying the behavior of satellite orbits and other problem areas related to space activities), all disappeared in SAO's annual reports to the AAS for 1963—although personnel continued to accumulate, and work continued in those areas for some years on a project basis

(McCray, n.d.). Most of the programs survived, but were redefined, not in terms of a centralized capability for national purpose, but in terms of traditional problem areas more familiar to the astronomer. This may be a sign of emerging institutional maturity for SAO, for by the early 1960s it no longer needed to differentiate itself from other astronomical institutions. But it can also be considered in some ways to be an institutional retreat from early ambitions to establish itself as a national facility for space research. Vestiges of this ambition persist at SAO today, although it remains to be seen just how extensive they really were in the heady years of is formation and rapid growth.

6. ACKNOWLEDGEMENTS

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FLW = Fred L. Whipple
SIA = Smithsonian Institution Archives

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THE TRANSITS OF VENUS AND NEW TECHNOLOGIES:
A Time to Reflect

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Abstract: In the recent history of astronomy there have been occasions where 'New Astronomies' have been introduced. In the spirit of the recent excitement of the 2004 transit of Venus, I have used the periods around the historical transits to reflect on the 'New Astronomies' of those eras. Johannes Kepler's *Astronomia Nova* is a fine representation of the New Astronomy of the 1631–1639 transit pair and Pierre Simon, Marquis de Laplace's *Traité de Mécanique Céleste* reflects the New Astronomy of the 1761–1769 transit pair. A combination of Samuel P. Langley's *The New Astronomy* and James E. Keeler's 1897 paper on astrophysics have been chosen as the exemplars of the New Astronomy of the 1874–1882 transit pair. I am open to suggestions for the works that best represent the 2004–2012 transit pairs.

Key words: Transits of Venus, new astronomies, Kepler, Laplace, Langley, Keeler

1. INTRODUCTION

When Woody Sullivan contacted me and suggested that I speak on some overarching aspect relating to the theme of this meeting and previous 'New Astronomies', I must admit that I was still in the throes of finishing an exhibition on the history of observations of the transits of Venus. As the exhibition came together I was struck by the notion that, if you concentrate on the historical pairs of the transits, they open small windows onto the overall history of astronomy. And because of the wide temporal gulf that separate successive transit pairs, the practice of astronomy that we observe during one pair is very different from that of another. So in the afterglow of

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the recent transit of June 8, I thought it would be outrageously appropriate to look at the various New Astronomies that were culminating during past transits. And, hopefully, that might give some larger context in which to view the other papers at this Conference.

Now, given the nature of this group, I am going to assume some basic knowledge of the transits of Venus and what their significance was to the history of astronomy. Suffice it to say that the transits are quite rare and, in the current orbital relations of Earth and Venus, they happen in pairs with nearly eight years separating the paired transits. Each pair is separated by alternating periods of 105.5 and 121.5 years. So that is quite a major time period indeed, and it easily allows for significant changes in astronomy during modern times (i.e. since the seventeenth century). For more information about transits of Venus, see Sheehan and Westfall (2004)

So what were the New Astronomies in vogue during the first three transit pairs of the modern period? And how best to describe them? The study of the various transit pairs' history gives us some clues. And as a rare book curator, my natural instincts told me to focus on a specific and appropriate work that is the exemplar of the New Astronomy of the time.

2. THE 1631 AND 1639 TRANSITS

What do the 1631 and 1639 transits tell us? We can see that Johannes Kepler was able to predict a few years in advance that there would be a transit of Venus in 1631. It turned out that it was not visible from Europe, but a young Englishman by the name of Jeremiah Horrocks, who was one of a rare few to be tinkering with Kepler's research results, realized that a second transit would follow in 1639, and he and his colleague William Crabtree were the first people known to have observed a transit. But the ability to predict these transits came about through the work of Kepler (1609), culminating in what is his greatest work, the aptly titled (for our purposes at least), *Astronomia Nova*, or *The New Astronomy*. How was it new? Unlike the purely mathematical works of the past, Kepler's work was based on physical causes. This was a critical difference! The *Astronomia Nova* came about when Kepler sent a copy of an earlier book on planetary distances to the noted astronomer Tycho Brahe. Tycho saw some promise in the young Kepler and invited him to join him in Denmark in 1597. Kepler felt that Denmark was too far for him to move but they did eventually met up in Prague. As Kepler (1992: 184-185) described it in *Astronomia Nova*:

> Moreover, Tycho, who was indeed himself a large part of my destiny, did not cease from then on to invite me to come to him. And since I was frightened off by the distance of the place, I again ascribe it to divine arrangement that he came to Bohemia. I thereupon came to visit him at the beginning of 1600 in hopes of learning the correct eccentricities of the planets … At that time, the work which his aid Christian Severinus had in hand was the theory of Mars. The occasion had placed this in his hands, in that they were busy with the observation of the acronychal position or opposition of Mars to the Sun in 9° Leo. Had Christian been treating a different planet, I would have started on it as well.
>
> I therefore once again think it to have happened by divine arrangement, that I arrived at the same time in which he was intent upon Mars, whose motions provide the only possible access to the hidden secrets of astronomy, without which we would remain forever ignorant of those secrets.

Only by studying Tycho's Martian observations would Kepler realize his New Astronomy because, of the known planets at the time, only Mars has a sufficient orbital eccentricity to reveal that its orbit is not a circle but an ellipse.

Kepler knew that Copernicus had used the center of the Earth's orbit as the center of the Solar System. But Kepler needed a physical cause for the planetary motions, so he needed to revise Copernicus's model to place the Sun at the center of the Solar System. In placing the Sun at the center Kepler, however, needed to return to the Ptolemaic artifice of the equant to explain the variation in speed of the planets. This was fine with Kepler who felt it made sense as the planets would move faster closer to the Sun whereas Copernicus used an 'epicyclet' to explain the variation. Kepler thought that the epicyclet was hard to explain in a physically-based model and that the equant made more sense under the circumstances.

Kepler worked on establishing the Earth's orbit knowing that the same geometry and physics would have to apply to all the planetary orbits. He used the Mars triangulation method wherein Mars returns to the same point in its orbit every 687 days or so, while the Earth's position varies. This method demonstrated that the perfectly circular orbit had too large an eccentricity. Regardless of the cosmology you use, Kepler showed that a circular orbit will not work. Kepler was true to Tycho's dying wish to give his system an equal chance with the others, but as a result of Kepler's work, this is the last time you see it in the *Astronomia Nova*. Kepler also worked on a substitute mathematical hypothesis to explain the Martian longitudes. His long and tedious work on iterative solutions for his 'vicarious' hypothesis eventually paid off with an accuracy one hundred times better than others had obtained for the longitudes. Unfortunately, this model failed to predict the Martian distances (latitudes). Kepler continued his "war on Mars" to find an adequate physical model. He looked again at what caused the planets to move in their orbits. Based on the works of people like William Gilbert (*De Magnete*, 1600), Kepler felt that a magnetic-like force emanating from the Sun was the best explanation for the driving force. His study of the relation of a planet's orbital velocity to the distance from the Sun led him to develop what we call his second law of planetary motion, the so-called 'area' law. Kepler formulated the area law in chapter forty of *Astronomia Nova* with help from Archimedean techniques, but this law did not get coherently stated until his book, *Epitome Astronomiae Copernicanæ*, or *The Epitome of Copernican Astronomy* (Kepler, 1618-22).

Kepler realized that he would need to find the true shape of planetary orbits in order to finally connect his accurate vicarious hypothesis to his physical principles and thereby explain how the Solar System worked. He knew that the orbits had to be non-circular, but what was the correct shape? Epicycles, as used in most other models of the Solar System, would not do; how would a planet know to travel in an epicycle? That did not make any physical sense to Kepler. He tried various orbital shapes, ovoids, lemon-shaped, and puffy-cheeked curves, but he finally stumbled on the ellipse and that fit his requirements perfectly. The physical cause for the ellipse and the varying speed Kepler felt he found in ascribing some kind of quasi-magnetic force emanating from the Sun. Based on Gilbert's work, a magnetic axis for Mars would act as a rudder against the Sun's force. Kepler worked on this reasoning and notes in Chapter 58 of *Astronomia Nova*:

> I could not discover why the planet … would rather follow an elliptical path, as shown by the equations. O ridiculous me! To think that the reciprocation [an alternate attraction and repulsion] on the diameter could not be the way to the ellipse! So it came to me as no small revelation that through the reciprocation an ellipse was generated. This will be made clear in the following chapter … that no figure is left for the planet to follow other than a perfectly elliptical one (Kepler, 1992: 576).

Kepler completed work on *Astronomia Nova* in 1605, but the heirs of Tycho, who died in 1601, held up publication. Since the heirs were never paid for Tycho's observation notes they asked for censorship rights over any derivative works, and since Kepler obviously favored Copernicus over Tycho this displeased them greatly. Fortunately a compromise was reached, and the work was finally published in 1609.

Astronomia Nova was indeed a New Astronomy. It disposed of the stronghold of perfect circles and uniform motion. There was never a book like it. We read it and see Kepler wrestling to work through imperfect data. Kepler shows us his warts and all, unlike Ptolemy and Copernicus. His work led him to the publication in 1627 of his improved planetary tables, the *Tabulæ Rudolphinæ* or 'Rudolphine Tables' (Kepler, 1627). These tables prompted his prediction of the 1631 transit of Venus which, sadly, came one year after his death.

3. THE 1761 AND 1769 TRANSITS

The eighteenth-century pair of transits of Venus was greatly anticipated by the astronomers of the time (for more information about these transits see Woolf, 1959). In the intervening years since the 1639 transit, Edmund Halley and Joseph Nicolas de L'Isle had shown how accurate observations of the transit at widely separated points on Earth could provide the basis for an absolute measurement of the distance from the Earth to the Sun. An accurate determination of this fundamental distance, known more commonly as the 'astronomical unit', was the holy grail of astronomers at this time. Many countries mobilized to make observations of the 1761 and 1769 transits, but the scientific establishments of the colonial powers of Great Britain and France went out of their way to arrange numerous expeditions to ensure a wealth of observational data. The most famous of the voyages sent out to observe the transit of Venus was the first voyage of Captain Cook, which observed the celestial event at Point Venus on the island of Tahiti in 1769. Unfortunately for astronomers, the many observations made in the eighteenth century did not provide the desired accuracy, especially due to the unpleasant optical problem caused by the now well-known 'black drop effect'. Nevertheless, there was a tremendous improvement in the range of distances for the astronomical unit compared to previous estimates taken from the seventeenth-century measurements of the opposition of Mars.

What then, was the New Astronomy of this time? We know of Isaac Newton's *Principia* of 1687 but the application of his principles lacked the sophisticated mathematical tools that were required. Newton explained his laws of motion by using geometrical methods because he could not express them as differential equations and lacked the means to solve them. The breakthroughs came from the Continent where the mathematicians were not obligingly tied to Newton's tools and notation. The people who laid the foundation for exploiting Newtonian physics were Leonhard Euler (using trigonometric functions in calculus), Alexis-Claude Clairaut (putting Newton's laws into the form of differential equations), Jean Le Rond d'Alembert, and Joseph Louis Lagrange. But the person on whom I want to concentrate for his major contribution is Pierre Simon, Marquis de Laplace who, four years after the 1769 transit, published his first paper on an astronomical topic, on universal gravitation. Laplace is my focal point for the New Astronomy of this transit pair due to his work in what people were calling 'physical astronomy'. Laplace later called this field "celestial mechanics". This field of study provided the impetus for the development of powerful analytical tools and the best mathematicians of the time worked on related astronomical problems.

The most significant work of this New Astronomy is Laplace's five-volume *Traité de Mécanique Céleste*, or *Treatise on Celestial Mechanics* (Laplace, 1798-1827). This is Laplace's most famous work and it built his renown in France and abroad. It was the chief source of what many call the "golden age of celestial mechanics." The *Traité* combined theoretical formulation with applications in a way so as to encourage further research. Some have called it the most influential work on celestial mechanics since Newton's Principia. The *Traité* consisted of five volumes published from 1798 to 1805. In it, Laplace stated his goal thusly:

> Astronomy, considered in the most general manner, is a grand problem in mechanics, in which the elements of the celestial motions are arbitrary constants; its solution depends both on the accuracy of the observations and on the perfection of Analysis, and it is very important to banish all empiricism and to borrow nothing from observation except indispensable data. It is the aim of this work to achieve, as much as may be in my power, this interesting result (Morando, 1995: 146).

The *Traité* is filled with numerous examples conveying the entire spectrum of celestial mechanics problems. I will mention a few. From the re-measurement of the meridian of France to determine the length of the meter, Laplace derived the ellipticity of the Earth to be 1/334. Previously the limits were that it must be between 1/304 and 1/578. In Book IV Laplace provides a systematic presentation of his theory of tides. He gives a formula that yields the height of the tide at a given place as a function of the hourly coordinates of the Moon and Sun in addition to local characteristics.

He gave an example of how this compares to the measured tides at Brest from 1711 to 1716. Laplace's formula is still used to calculate the Brest-Reference Tide. In Book II of the *Traité* Laplace obtained formulas for the natural perturbations of the planets as point-masses in radius vector, true anomaly, and latitude. These formulas were the basis for solving the problem of the gradual changes in the mean motions of Jupiter and Saturn, developing the orbital theory of Uranus, and predicting the position of Neptune, considered to be the greatest achievement of celestial mechanics. In Book X Laplace derived a formula that now bears his name and it gives the astronomical refraction to the third order of the tangent of the zenith distance of the observed star. In the *Traité* Laplace also managed to show how to determine the invariable plane of the Solar System, and in his theory of comets he demonstrated that comets' orbital changes were due to planetary perturbations. I think we can say without exaggeration that Laplace's *Traité* truly defined the problems with which astronomers would be concerned up to the time of the next transit pair in the late nineteenth century.

4. THE 1874 AND 1882 TRANSITS

The nineteenth-century transit pairs occurred at the height of colonial expansion of the traditional European powers along with the participation of new nations such as Germany, Italy, Mexico, and the United States. Governments were keen to work with the astronomers in planning many far-flung expeditions to observe the transits of Venus. The recent development of photography provided everyone with optimism that an accurate value for the astronomical unit was within reach. But the success of the many expeditions in obtaining good observations, the results had little impact in uncovering a precise—for the nineteenth century—value for the astronomical unit. The efforts of the American expeditions and the ensuing results have been noted by Dick, Orchiston ands Love (1998) and Dick (2003). While a more accurate value was obtained, transits of Venus were overshadowed as alternative methods were developed to measure the solar parallax. As a result, the transit observations never did live up to their potential of being the foremost way of measuring the astronomical unit.

For the nineteenth century New Astronomy we do not have a work on the scale of Kepler's *Astronomian Nova* or Laplace's *Traité*. The logical choice to me is, what else, *The New Astronomy* by Samuel P. Langley (1887). Around the time of the 1874 and 1882 transits the new field of 'astronomical physics', or astrophysics, was coming into its own. Here, scientists were actually trying to determine more about the heavens and the bodies contained within, not just in determining what their exact position was and would be. Before long, astrophysics would become the dominant field of astronomical activity and celestial mechanics would be eclipsed as the cutting edge of research.

Samuel Langley was an eloquent spokesman for the New Astronomy. In 1883, one year after the last transit in the pair, he gave a series of lectures in Boston. These were soon published as a series of articles in *Century Magazine*. In his preface, we can see that he is aiming for a popular and supportive audience:

> I have written these pages, not for the professional reader, but with the hope of reaching a part of that educated public on whose support he is so often dependent for the means of extending the boundaries of knowledge.
>
> It is not generally understood that among us not only the support of the Government, but with scarcely an exception every new private benefaction, is devoted to "the Old" Astronomy, which is relatively munificently endowed already; while that which I have here called "the New," so fruitful in results of interest and importance, struggles almost unaided.
>
> We are all glad to know that Urania, who was in the beginning but a poor Chaldean shepherdess, has long since become well-to-do, and dwells now in state. It is far less known than it should be that she has a younger sister now among us, bearing every mark of her celestial birth, but all unendowed and portionless. It is for the reader's interest in the latter that this book is a plea (Langley 1887: v).

Langley notes that some call the New Astronomy "Celestial Physics", or "Solar Physics" or the "New Astronomy" (i.e. Astrophysics). In the book he discusses how we can learn more about sunspots, solar energy, planets, and stars, and how new tools, particularly photography, will allow us to explore these things. He stresses the practical aspects of the New Astronomy such as solar heating. And Langley mentions that the spectroscope will be a powerful tool for the exploration of the heavens. But I feel that maybe this is not the best work to choose in lieu of the previous examples.

I suggest that a better choice would be James E. Keeler's keynote paper given at the dedication of the Yerkes Observatory, "The Importance of Astrophysical Research and the Relation of Astrophysics to Other Physical Sciences" (Keeler, 1897), published in the *Astrophysical Journal*. This work was meant for other astronomers, like the other works mentioned, being a forward-looking speech extolling the importance of astrophysical research, and the topics he cited were the focus of such research up to World War II. So maybe I should cheat and include both works as those most representative of the New Astronomy of the 1874 and 1882 transits.

5. CONCLUSION

So we have just had the 2004 transit of Venus. What is the New Astronomy for this era? I will leave that for future historians but two thoughts are: (1) the topic for this conference, namely the opening of the electromagnetic spectrum to astronomy, and (2) the tremendous development in cosmology with the Big Bang and Inflationary theories creating a viable concept of the beginning and evolution of the Universe. The revolution in astronomical instrumentation through computers and sophisticated electronics is another possibility. It will be interesting to see what astronomers and historians consider to be the one work that best exemplifies the New Astronomy of 2004–2012. As for the following transit pair of 2117–2125, maybe we can speculate that contact with extra-terrestrials will develop another New Astronomy, among other things! We can also argue that astronomy is changing much faster now and that my indulgence of using the rare transit of Venus pairs as a framework really breaks down starting in the twenty-first century. In any event, I think we can agree that the future of astronomical research is bright indeed and there will be no shortage of new questions to answer and we will have many more New Astronomies.

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AND THE REMAINING 22 PHOTONS:
The Development of Gamma Ray and Gamma Ray Burst Astronomy

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Abstract: Unlike all other electromagnetic apertures, the gamma-ray window was forced open by theorists at least a decade before the first photons passed through. Sources remain few by the standards of X-ray or radio astronomy, and the class with no unambiguous optical counterpart remains large. The twenty-four years over which the gamma ray bursters were poorly understood or misunderstood seemed very long to those of us who lived through them, though short compared to the eras of incomprehensibility for coronal lines and stellar pulsations.

Key words: gamma rays, gamma ray astronomy, gamma ray bursts

1. INTRODUCTION

Optical astronomy was presumably invented by the first rhodopsin-bearing, multi-cellular creature that did not have sense enough to go to (flower) bed when it got dark, or, if he did, then he invented solar astronomy. Energy off the red end of the solar spectrum warmed William Herschel's hand, and, later, his thermometer, in 1800. The exponent of Naturalphilosophic 'polarities', J.W. Ritter then predicted that these 'oxidizing rays' should be balanced by 'deoxidizing rays' off the violet end of the solar spectrum and, using paper impregnated with silver chloride (which becomes silver when exposed to energetic photons), found this to be true in 1801 (Hufbauer, 1991). Laboratory and astronomical discovery thus coincided.

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The laboratory led for radio waves (Hertz, 1887, followed by Jansky, 1931) and for X-rays (Roentgen, 1896; Friedman, 1949). All of these people are part of our folklore, meaning that one no longer cites them explicitly. Notice that Jansky in effect discovered the Galactic plane and Friedman the Sun (with a Geiger counter borne upward on a V-2 rocket). The radio Sun came in between, with independent discoveries from several country with major World War II radar programs.

Rutherford added gamma rays to the laboratory inventory in 1903. From 1900 to 1929, the cosmic rays (defined, for instance, as dischargers of gold leaf electroscopes and clickers of Geiger counters) were generally thought to be very energetic photons. Opinion changed rapidly in the wake of Bothe and Kohlhorster's (1929) demonstration of the enormous penetrating power of the primary particles, and, while the first sentence of their short paper includes the word 'Gammastrahlung', the last word but two is 'korpuskularstrahlen'. Early rocket work (e.g. Hulsizer, 1949) showed that less than 1% of the GCR primaries were photons. Then for a while, no one looked very hard.

2. PREDICTIONS

Astrophysicists began to expect significant gamma ray fluxes in the late 1950s in three different contexts. First, the optical identification of the radio source Cygnus A with what looked like a pair of galaxies colliding (and now looks like a single giant elliptical with a dust lane down the middle; only our eyes have changed), led Burbidge and Hoyle (1956) to suggest that the colliders were actually a galaxy and an anti-galaxy. If so, then the Earth's upper atmosphere should receive 0.1 –1.0 $\gamma\ cm^{-2}\ s^{-1}$ from there. Second, the association between the time scale of fading supernovae and the half-life of Cf^{254} by B^2FH (Burbidge, et al., 1957) implied a flux at Earth of about 0.01 $\gamma\ cm^{-2}\ s^{-1}$ from the Crab Nebula. Third, Hayakawa et al. (1958) predicted that cosmic ray production of e± pairs and p+n captures should yield a photon flux at 0.511 and 2.2 MeV about 1% of the proton flux at the same energies. This turned out to be roughly right, though the other two were impressive over-estimates.

Despite all this, the first source seen was actually the Sun, in a 1958 flare reported by Peterson and Winckler (1959) that provided about $400\gamma\ cm^{-2}$ in about 10 seconds. Solar activity was declining, and the beginning of the new activity cycle, caught by Cline, Holt, and Hones (1968), was treated almost as a new discovery, just in time to be useful to the gamma ray burst crew when they started worrying about natural noise sources.

3. CRANKING DOWN THE LIMITS

The Cygnus direction was scanned with an emulsion stack (originally meant for cosmic ray work) from a balloon flight organized by Braccesi et al. (1960). The flux limit of 5×10^{-3} γ cm^{-2} s^{-1} was immediately well below the anti-galaxy prediction. Cline (1961) flew the first detector designed for gamma rays and reached 10^{-3}. The next year, Kraushaar and Clark (1962) gained another factor of three, to 3×10^{-4} γ cm^{-2} s^{-1} and set less tight limits on the Crab Nebula, the Galactic Center, M31, and so forth (below what was expected if γ supernovae were among us). After considering all the sources of noise counts they could think of, they concluded that "… the remaining 22 photons …" were part of a genuine, cosmic diffuse background. Thus it was never quite true (but almost!) that, in the words of Gerald Share, "… one photon was a discovery; two was a spectrum; and three was the Rossi Prize." He was accepting the Rossi Prize at the time for work involving a slightly larger number of gamma rays (e.g. see Share, et al., 1979).

A claimed detection of Cygnus A from the same period (Duthie, et al., 1966) at 1.5×10^{-3} γ cm^{-2} s^{-1} dropped quickly from citation lists and was presumably a source of excessive optimism rather than gamma rays unless, just possibly, Cyg X-3 was in a mega-flare state.

4. THE ERA OF DISCOVERIES

Arnold et al. (1962) flew a gamma ray detector on Ranger 3, whose primary purposes were different, and recorded rather more than twenty-two photons belonging to a diffuse background. The contributions to this background have been debated over the ensuing 40+ years, but the winner seems to be unresolved sources, probably mostly active galactic nuclei, like those that contribute to the X-ray background (Helfand & Moran, 2001; Ruiz-La Puente, et al., 2001).

And the first source outside the Solar System was, of course, the Crab Nebula with its pulsar (Haymes, et al., 1968), as had been true for the radio and X-ray windows. After the radio and optical measurements of the pulsar period, the balloon observers were able to go back into their 1967 data and identify the very slightly faster pulsations that year, providing the earliest data point we are likely ever to have!

The Galactic plane peeked up above the general background for Clark et al. (1968) whose detector rode on OSO-3, and a source in the general direction of Cygnus came next (Lamb, et al., 1977, reporting data several years earlier from SAS-2).

Now, before we let them launch COS-B, this is the time to deal with the vexed question of "What is a gamma ray?" The upper energy limit is set by nature, and is at least into the PeV-TeV range, with arrays sensitive to these energies (via Cerenkov light flashes and/or secondary particles when they hit things in the upper atmosphere) operating regularly on more than half a dozen sites, all at high altitude to catch the peak of shower development. Ground-based detection of very high energy cosmic rays is a similar challenge. At the low energy end, lines have been drawn at 0.1 or 1 MeV or at the distinction between atomic (X-ray) and nuclear (gamma ray) processes. But, as became clear during the not-always smooth process of selecting the instrument package for the Compton Gamma Ray Observatory, essentially X-rays are collected by X-ray astronomers and gamma-rays by gamma-ray astronomers.

5. MORE (BUT NOT THAT MANY MORE)

COS-B was launched in August 1975 and operated until April 1982. The catalog of discrete sources (Bignami and Hermsen, 1983) listed only twenty-five with fluxes exceeding about 10^{-6} γ cm^{-2} s^{-1} above 100 MeV. Brightest of all was the Vela pulsar, followed by Geminga (now known to be just about the closest rotationally-powered pulsar). The Crab, 3C273, and a cloud concentration around ρ Oph were there as well. And that was just about that (Bignami and Hermsen, 1983), with much the largest category being "unidentified", that is, sources with no unique counterpart at longer wavelengths. I emphasize the 'unique' aspect because of the always-limiting poor angular resolution of gamma ray instruments.

Source numbers increased very rapidly with the 1991 launch of the Compton Gamma Ray Observatory. At some point, it becomes difficult to describe this as 'history' rather than 'current events'. The largest class of identified sources are, however, active galaxies, with significant numbers of pulsars, X-ray binaries, supernova remnants, and dense clouds pervaded by cosmic rays also represented among the 271 3rd EGRET Catalogue sources

(Michelson, 2003, for instance), though 'unidentified' remains the second largest class. An optimistic inventory of TeV sources includes less than dozen (Aharonian, 2003), all either within the Milky Way or at modest red shift, because a very ordinary intergalactic photon looks like a brick wall to what we see as a TeV photon. Mkn 421 and Mkn 501 (slightly the more distant) have been particularly informative in this respect.

There have been some additional pleasant surprises along the way, most particularly the detection of nuclear gamma rays lines from Co^{56} decaying to Fe^{56} in the ejecta of SN 1987A (Matz, et al., 1987; Sandie, et al., 1988; the former a happy misuse of SMM, the latter a quickly, but carefully, planned balloon flight, one of several at the time). CGRO added Cas A to the source list and Ti^{44} and Al^{26} (diffuse) to the line list, with an occasional false alarm along the way as well. And once again, we are well outside the regime of 'history'—to any but the very young. It is, however, perhaps worth remarking that all of the anticipated processes have been seen, though not always at the expected levels. These include nuclear gamma ray lines, e± annihilation, π^0 decay, bremsstrahlung, inverse Compton scattering, and synchrotron emission.

6. GAMMA RAY BURSTERS

I have provided my 'take' on this history elsewhere (Trimble, 2005) and will summarize it briefly here. The critical stages seem to have been: (1) recognition that something interesting was out there (not 'serendipitous', in the strict meaning of the word); (2) an enormous flowering of mechanisms, models, and meditations; (3) near-universal convergence on a fundamentally wrong model, to which many held even in the face of discordant data; and (4) a very rapid sorting out, achieved when a new, critical piece of information—optical identification—appeared. Later evolutionary stages, including the association with Type 1c super(hyper)novae and Wolf-Rayet stars and the identification of a related class of X-ray flashers belong to 'current events'.

The discovery paper (Klebesadel, Strong and Olson, 1973) made clear that they had spent considerable time examining the data stream from the Vela satellites and that one of their goals was to be sure that there were no natural events (other than solar flares) that might be confused with emission of gamma rays or neutrons by atmospheric atomic or hydrogen bomb tests. They reported sixteen such natural events (and the Russians were seeing them too—see Mazets, et al., 1974) between 1969 and 1973, with a 'probable' from 1967. A very cursory summary of properties includes: (1) bimodal distribution of durations, peaked above and below 2 seconds (only the long-duration ones have counterparts at other wavelengths as we go to press); (2) time histories that vary from a single sharp spike to very complex; (3) spectra that are broken power laws with peaks in νF_ν between a few keV and 1 MeV or more; (4) fluences ranging from 10^{-3} erg cm^{-2}, down to less than 10^{-8} erg cm^{-2}, with N(S) flattening below the 3/2 power law (indicative of homogeneous distribution in space) at smaller fluences; (5) an event rate of about 1000 per year down to 10^{-8} erg cm^{-2}; and (6) isotropy in arrival directions.

This turn-over in log N – log S is now attributed to cosmological red-shifting, and, along with some correlations among fluence, duration, and spectral hardness, means that the GRBs are the first class of astronomical object ever to have statistical properties dominated by cosmological effects rather than astrophysical evolution. Cosmological effects had been sought for decades among the quasars and bright galaxies, but evolution always won.

Not much more than a year after the initial publication, a review speaker (Malvin Ruderman) at the December 1974 Texas Symposium on Relativistic Astrophysics claimed that the only astrophysicist who did not have a theory of gamma ray bursts was Ostriker. I checked with him (in the only original research done for this project), and he still doesn't! By 1992, Nemiroff (1994) had identified 118 published models. These included the prediction of shock break out from supernovae (Colgate, 1968) but not the flashes of Hawking (1974) radiation from evaporating primordial black holes. Of the wild and woolly, he caught Zwicky's (1974—nearly his last paper) exploding chunks of neutron star material but not Harris' (1990) exhaust trails from interstellar space ships. The numbers have clearly grown well beyond 100 in the following decade (to the point where Nemiroff no longer feels able to tabulate them all!). Virtually all, ancient and modern, involve neutron stars and/or black holes, forming, merging, or doing something else. Magnetic fields and rotation are also common ingredients in the recipe.

Curiously, by the 1980s, nearly the entire community had converged on a model of star quakes on the surfaces of strongly magnetic (10^{12} G for instance) neutron stars in the disk of the Milky Way. Including the old ones, these would be numerous enough to produce the appearance of homogeneity ($N \alpha\ S^{-3/2}$) and isotropy, with the expectation that fainter events should have N(S) turn over and be concentrated toward the Galactic Plane (for instance Ho, et al., 1992, the proceedings of a 1990 conference).

In retrospect there were several (rational) reasons for the convergence. First was the 1976 discovery of X-ray bursts and their (correct) attribution to neutron stars in close binaries at Galactic distances. Second was the report of several different sorts of spectral features (including cyclotron resonances in strong fields and e± annihilation lines, the latter a plus for the 'exhaust' theory as well). Third was excessive distrust of reported turnovers of N(S), even the one (White, et al., 1983) that turned out to be correct. The fourth distracter was reported optical flashes at GRB positions but at earlier times, implying that the engine had to survive a burst and do it again, something like once per century. Fifth was the no-host problem, the absence of bright galaxies in the error boxes of some of the best localized, brightest GRBs.

Old neutron stars in the disk gave way to old neutron stars in an extended Galactic halo when data from CGRO began to show a definite turn-over in N(S) but still no sign of anisotropy. Many known neutron stars are indeed high velocity objects, presumably as a result of anisotropic supernova events and/or disruption of close binary systems, and you can go a long way in 10^{10} years at a speed of a few hundred km/sec.

Even after a couple of years of BATSE data had confirmed isotropy and inhomogeneity of the bursters, there was significant support for a 'Galactic' source distribution at a spring, 1995 debate on the topic, commemorating the 75th anniversary of the 1920 Curtis-Shapley debate (see Nemiroff, 1995, and the next four papers in that journal, representing Trimble on history, Gerald Fishman on BATSE data, and the two debaters, Don Q. Lamb supporting Galactic and Bohdan Paczynski supporting cosmological interpretations). Listening to the debate changed few views from one to the other, though the 'after' vote showed many more 'undecided' than the 'before' vote.

7. RESOLUTION

It had been clear for twenty-four years that a spectrum of an optical counterpart would almost surely sort things out. This happened in 1997, when, first, the new BeppoSAX satellite caught the fading X-ray tail fast enough to provide a good position, and, second, the right optical telescopes swung there in time (see Trimble and McFadden, 1998, for detailed references). The first spectrum included absorption lines at z = 0.851, well and truly extragalactic. There are by now vast numbers of X-ray tails, a couple dozen optical counterparts with redshifts, and a dozen or so radio tails, providing a whole new field for 118 or more models. GRB 030329, which also rated a supernova designation (SN 2003 dh) was particularly informative, and there are a handful of less-clean cases suggesting that the long duration events are powered by the collapse of massive stellar cores into rapidly rotating black holes, often with a visible, associated supernova event, if you happen to be close enough.

Detailed analysis of the lower-energy tails implies considerable beaming of the gamma-rays, and so total energies of ‘only’ 10^{51} ergs rather than 10^{54}. The number of events per galaxy goes correspondingly up, to one per million years. The idea of merging neutron star pairs (or NS + black hole) survives as a possible mechanism for the short-duration events, for which there are still no counterparts, though naturally other suggestions have been made. Some recent disputes, mild by the standards of the early ones, concern the reality and interpretation of another assortment of spectral features, relationships with the X-ray flashers, and the reasons for the absence of optical counterparts in some cases. See Fenimore and Galassi (2005) or your favorite preprint e-shelf for latest words on these matters.

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SUNDIALS AND ART

GNŌMONIKĒ TECHNĒ:
The Dialer's Art and its Meanings for the Ancient World

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Abstract: *Gnōmonikē Technē*, the art of gnomonics, was a recognized branch of applied mathematics in Greek antiquity. The variety of ancient sundials that survive show us that ancient dialers were prolific and inventive. This paper places ancient gnomonics in scientific and cultural perspective and offers an overview of ancient dial types.

Key words: sundials, ancient Greek astronomy, ancient Greek mathematics, Roman science, gnomonics, sphairopoiïa, Geminus, Vitruvius

Dedicated to Woody Sullivan, in memory of Themistagoras, son of Meniskos

1. INTRODUCTION

When Woody Sullivan, Jim Bell and Bill Nye conceived a method of placing a sundial on the surface of Mars—by using parts of the lander that had been designed for a different purpose—they had more in mind than telling time (see Sullivan and Bell, 2004). The motto on the sundial, "Two worlds, one Sun", speaks of the symbolic value of the device. Thus, Woody's Martian sundial (for relevant details of this see the web site http://planetary.org/rrgtm/marsdial/index.html) has less to do with telling time than with saying something about *us*. In antiquity, dialing was no less intricately connected with the conception of the place of humanity in the cosmos.

The Greeks had a name for this science: *gnōmonikē technē*, the art of gnomonics. The Greek noun *gnōmōn* (which we still use today) refers to the stylus or index that casts the shadow. It descends from a verb, *gignōskō*,

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which means *learn*, *perceive*, *judge* and, in past tenses, simply *know*. Before it was a part of a sundial, a *gnōmōn* therefore meant *someone who knows*, *a judge*—but also *a carpenter's rule*, and hence sometimes, metaphorically, *a rule* of life. (Woody's motto on the Mars dial is also an apt rule of life. It may be paraphrased as: "Think cosmically, act globally.") According to Liddell, Scott and Jones (1996), a *technē* is an *art* or *craft*, that is, something requiring study and devotion. Hence the aphorism attributed to Hippocrates: "life is short, art (*technē*) is long." As we might infer from its use in this example from medical writing, *technē* can be used to indicate *a science* or *a system of making or doing* something.

2. GNOMONICS AS A BRANCH OF APPLIED MATHEMATICS

Above all else, dialing is a branch of applied mathematics. Geminus, a Greek astronomical writer of the first century B.C., has left us a classification of the branches of mathematics, which is the most detailed discussion of this subject surviving from antiquity. Geminus' discussion was in a book, now lost, that treated the history and philosophical foundations of mathematics. Its title was perhaps *Philokalia* (*Love of the Beautiful*). Geminus' discussion of the branches of mathematical learning has been preserved by Proclus, who quotes it at length in his commentary on Euclid (see Proclus, 1970: 31-35; for discussions, see Evans, 1999; Heath, 1921, Volume 1: 10-18; and Tannery, 1887: 38-52).

According to Geminus (see Figure 1), mathematics is divided first of all into the pure and the applied. Pure mathematics is concerned with mental objects only, such as number in the case of arithmetic and ideal lines and planes in the case of geometry. For the Greeks, 'arithmetic' means number theory of the sort associated with the Pythagoreans, and not the routine computation taught to children. Under applied mathematics, Geminus ranges six arts: practical calculation, geodesy, harmonics, optics, mechanics and astronomy. Practical calculation (which the Greeks called *logistic*) is the elementary computation that is taught in the schools. It is an offspring of arithmetic, in the same way that geodesy is an offspring of geometry. Harmonics, or the theory of concordant sounds, is an application of number theory, in the same way that optics is an application of the geometry of straight lines. Under mechanics, Geminus lists four subdivisions: military engineering (the making of catapults and so on), "wonderworking" (the making of automata operated by gas or fluid pressure, such as the gadgets described by Hero of Alexandria), the study of equilibrium and centers of gravity (pioneered by Archimedes), and "sphere-making," or *sphairopoiïa* (which has to do with making mechanical images of the heavens, such as celestial globes). According to Geminus, astronomy has three subdivisions: gnomonics (our subject—the making of sundials), meteoroscopy (involving specialized instruments such as the armillary sphere) and dioptrics (devoted to the measuring instrument called the *dioptra*).

Figure 1. Geminus' Classification of Mathematics

- Pure Mathematics
  - Arithmetic
  - Geometry
- Applied mathematics
  - Practical calculation
  - Geodesy
  - Harmonics
  - Optics
    - Optics proper (straight rays)
    - Catoptrics (mirrors)
    - Scenography (perspective)
  - Mechanics
    - Military engineering
    - Wonderworking
    - Equilibrium and centers of gravity
    - Sphere-making (*sphairopoiïa*)
  - Astronomy
    - **Gnomonics**
    - Meteoroscopy
    - Dioptrics

It is remarkable that we still possess, or know of the existence of, mathematical treatises on every one of these topics. So it is clear that Geminus is describing, not merely a conceptual division of applied mathematics, but actual genres of mathematical writing. *Gnomonics*, then, is a branch of applied mathematics, in which the geometer could show great skill, perhaps by inventing a new kind of dial.

3. A SAMPLE OF ANCIENT DIAL TYPES

The Greeks dialers were very inventive. The sheer number of ancient dial types proves that gnomonics was always about much more than practical time-telling. A fascinating list of dial types and their inventors has been left by Vitruvius (*On Architecture*, ix, 8.1; commentary in Soubiran, 1969), a Roman writer on architecture who lived in the age of Augustus:

> The semicircular form, hollowed out of a square block, and cut under to correspond to the polar altitude, is said to have been invented by Berosus the Chaldean; the scaphe or hemisphere by Aristarchus of Samos, as well as the disk on a plane surface; the arachne by the astronomer Eudoxus or, as some say, by Apollonius; the plinthium or lacunar, like the one placed in the Circus Flaminius, by Scopinas of Syracuse; the πρὸς τα ιστορούμενα, by Parmenio; the πρὸς παν κλιμα, by Theodosius and Andreas; the pelecinum, by Patrocles; the cone, by Dionysodorus; the quiver, by Apollonius.

Besides these dial types, Vitruvius mentions also the conarachne, the conical plinthium and the antiborean, without attributing them to particular writers.

It is clear from Vitruvius' remarks that gnomonics comprised a sizable specialized literature. The inventor of a new type of dial might announce it by writing a short, specialized treatise, as well as by actually making a dial for display. None of this literature has come down to us. The only substantial accounts of gnomonics that have been preserved are Vitruvius' short and disappointing summary and Ptolemy's more technical account of the theory of projection in *On the Analemma* (Ptolemy, 1898-1954, volume 2; for a discussion, see Neugebauer, 1975: 839-856). For this reason, the dial types mentioned by Vitruvius cannot all be certainly identified with extant dials in museum collections (e.g. see Gibbs, 1976: 59-65). Nor, of course, can we be very confident of Vitruvius' attributions to particular inventors. This reflects the agonistic character of ancient Greek society, in which

> potter is angry with potter, carpenter with carpenter,
> beggar is jealous of beggar, and bard of bard
> (Hesiod, *Works and Days*, lines 25-26).

Mathematicians were as careful to see that they received credit for a new theorem, and no doubt dialers wanted credit for a new dial. Greek and Roman writers are fond of attributing, whenever possible, any invention or discovery to a particular person, even when, from our perspective, the evidence looks rather shaky.[1]

Nearly 300 Greek and Roman sundials have survived from antiquity; 256 of these are listed in Gibbs' book (1976) and others in papers by Arnauldi and Schaldach (1997), Catamo, *et. al*. (2000), Locher (1989), Pattenden (1981) and Rohr (1980).[2] From the theoretical point of view point, the simplest variety is the spherical dial—Vitruvius' scaphe. The shadow-receiving surface is the interior of a hemisphere. The tip of the gnomon lies at the center of the spherical surface. Since the dial is merely the inverted image of the celestial sphere, the theory governing the placement of the hour lines, as well as the equator and tropics is very simple. However, since the Sun cannot be found at just any place on the celestial sphere, but must remain between the tropics, an entire hemisphere of stone is not required. The lower part of the south face of the dial can be cut away (corresponding to the part of the sky above the tropic of Cancer). This is the dial that Vitruvius refers to as a "semicircular form, hollowed out of a square block, and cut under to correspond to the polar altitude."

Figure 2 shows an idealized view of this kind of dial. Eleven hour curves serve to indicate *seasonal hours.* The period from sunset to sunrise consists always of twelve hours, all equal to one another. Similarly, the night is divided into twelve equal hours. In the summer the day hour is long and the night hour is short, while in winter the opposite is true. The *equinoctial hour* that we use today (one twenty-forth part of the whole diurnal period) is a seasonal hour evaluated on the day of equinox. Although Greek astronomers did use the equinoctial hour when they needed a uniform unit of time for precise calculation, the seasonal hour was the only one used in everyday life. All surviving Greek and Roman sundials are marked in seasonal hours.[3] The dial in Figure 2 is also furnished with three *day curves*, indicating the track of the shadow's tip on (from top to bottom) winter solstice, equinox, and summer solstice.

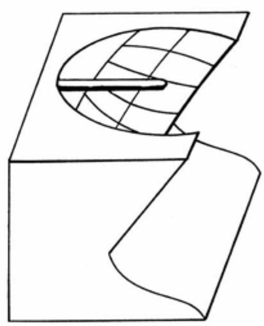

Figure 2. Principle of the spherical sundial with cut-away south face.

The gray stone dial from Pompeii shown in Figure 3 is of this same general type. The broken-out hole at the top originally carried the gnomon. It is very rare to find ancient dials still carrying gnomons. Iron gnomons would have rusted away over the centuries, and bronze ones must often have been appropriated and the metal put to some other use. A peculiar feature of this dial and a number of others from Roman sites is that the three day curves do not correspond to the equinoxes and solstices. On the Greek prototype of Figure 2, since the gnomon tip is at the center of the spherical surface, the equator curve must be equidistant from the two tropics. But this is not the case with the dial of Figure 3, on which the three day curves are just arbitrary circles of constant declination. They perhaps served a practical purpose for the dialer: each circle of constant declination is divided into twelve equal arcs by the horizon and eleven hour curves. Putting on the day curves was therefore a step toward the construction of the hour curves. In any case, the dial of Figure 3 can be used to tell the time of day, but not the season of the year. This was not necessarily a mistake—the dialer and his customers may have been interested only in the time of day—but it does represent a falling away from the quality of the earlier Greek dials, most clearly exemplified by a large number of the first century B.C. and earlier found on Delos. For a comparison of Delos and Pompeii dials see Gibbs (1976: 90-92).

Figure 3. A First Century A.D. spherical dial from Pompeii, 32 × 40 cm, Gibbs No. 1020 (courtesy: Soprin-tendenza Archelogica di Pompei, Inv. 34221).

To judge by the numbers preserved, the most common dial was of the *conical* type, which Vitruvius attributes to Dionysodorus. In a conical dial, the shadow-receiving surface is the inner surface of a cone. Typically, the conical surface was cut into a roughly rectangular slab of stone, as with the dial shown in Figure 4. The stone-working involved in making a conical dial was easier than that required for a spherical dial. But, by compensation, the theory was more complicated: it was necessary to project the celestial sphere onto a conical surface.

Large numbers of *plane* dials have also been preserved. In the dial of Figure 5 we see the typical form of a horizontal plane sundial. The central line is the meridian. The upper curve is the shadow track for summer solstice; the horizontal straight line, the shadow track for equinox; and the lower curve, for winter solstice. The hole where the metal gnomon was inserted is visible just above the summer solstitial curve. The eleven more or less vertical lines represent the hours. A photograph of the extant fragments of this dial is included in Gibbs (1976: Plate 55).

Figure 4. Conical dial found near Alexandria at the base of Cleopatra's needle, 40 high × 43 cm wide. The Greek numerals labeling the hour lines are an unusual feature and are probably Byzantine additions to the original Ptolemaic dial. Gibbs No. 3086 (British Museum No. 1936 3-9.1. Photograph courtesy of the British Museum).

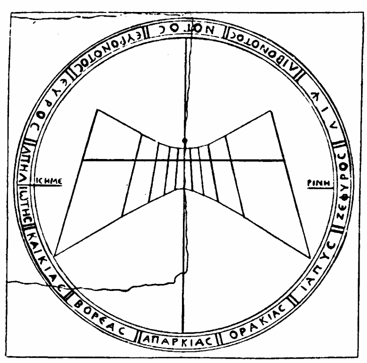

Figure 5. A horizontal plane sundial found near Rome. The extant portion of the dial is approximately the left half, inside the wavy line, and measures 35 cm × 54 cm. The remainder of the illustration is a restoration. Gibbs No. 4008 (illustration after Diels, 1924).

'Furniture' is a term used by modern dialers for auxiliary features of a dial that are not directly connected with its time-telling function. The dial shown in Figure 5 carries an interesting bit of furniture: the names of twelve winds were engraved, in Greek, in a circle around the perimeter. Notos (the south wind) is at the top and Zephyros (the west wind) is at the right. In popular Greek and Roman thinking, winds were sometimes personified as minor gods. Thus, on the Tower of the Winds in Athens, each of the eight vertical faces of the structure is graced by an individually-designed sundial and by a relief sculpture of the personified wind associated with the direction towards which the dial faces (for an illustration, see Evans, 1998: 131). In scientific treatises on winds (Pliny, *Natural History*, ii, 114-132; Theophrastus, *De Ventis*; see also Taub, 2003: 107, 149), each wind was analyzed according to its properties (hot or cold, moist or dry), as well as the time of year in which it was wont to blow. However, on sundials, wind names are often merely a poetic way of indicating a direction on the horizon.

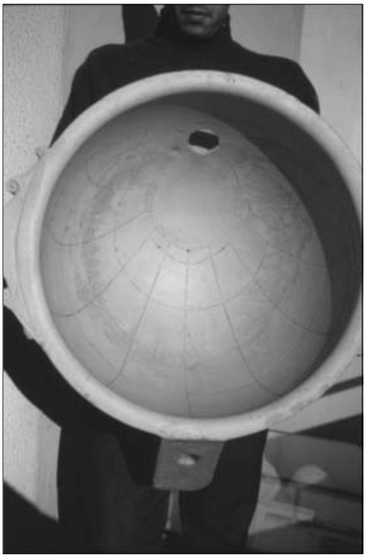

Figure 6. A spectacular First or Second Century A.D. Greek dial from Roman Carthage. Interior diameter 49 cm (photograph by Denis Savoie of a cast of the original dial).

One of the most spectacular ancient sundials preserved from antiquity is shown in Figure 6. This dial, found at Roman Carthage before the Second World War, was acquired by the Louvre in 1999, and is now on display there (Département des Antiquités Grecques, Étrusques et Romaines, MNE 1178; see Savoie and Lehoucq, 2001; Pasquier and Savoie, 2000). It is of marble, imitates the form of a fancy serving vessel or silver goblet, is decorated with oak leaves and acorns in relief, and carries two ornamental handles. In use, the vessel is mounted six or seven feet off the ground and is tipped (to correspond to the latitude) with the bowl facing downward at an angle. The bottom of the vessel is pierced by a hole through which sunlight shines. It is the spot of light falling on the interior network of curves that indicates the hour. The original hole was smaller than the opening in the marble, for there is a recess for a fitted metal plaque which must have carried a small hole. This dial is of the type called a 'scaphe with eyelet' by Savoie or 'roofed spherical dial' by Gibbs. It is essentially a hemispherical dial with the gnomon tip located on the surface of the sphere—which leads to hour curves that are rather complicated from a mathematical point of view. Finally, this dial carries an interesting bit of furniture: seven declination curves, for the dates of the Sun's entry into successive zodiac signs. These are labeled in Greek with the dates expressed in terms of the Julian calendar.

These are far from exhausting the types. The πρὸς παν κλιμα mentioned by Vitruvius in the quotation above was a dial that worked "… for every latitude." And, as Vitruvius (*On Architecture*, ix, 8.1) tells us, "Many have also left us written directions for making dials … for travelers, which can be hung up." A number of ingenious portable dials have, indeed, been found (e.g. see Price, 1969). In some cases, the sundial was combined with a gearwork mechanism that enabled the user to keep track of the place of the Sun and the Moon in the zodiac (Field and Wright, 1984).

4. GNOMONICS AND SPHAIROPOIÏA

Gnomonics had intimate links with the theory of the celestial sphere, and hence also with *sphairopoiïa*, another of Geminus' branches of applied mathematics. *Sphairopoiïa* is the art of making mechanical images of the heavens. Celestial globes and armillary spheres were two of the most common examples of this art. In the armillary sphere, the sky is represented not as a solid sphere, but by a network of rings (the Latin word *armilla* signifying an armband or bracelet). These rings embody the most important circles of the sphere: ecliptic, equator, tropics, arctic and antarctic circles, as well as the colures. None of these delicate constructions have survived from antiquity, but they are mentioned by ancient Greek writers who refer to them as *krikotai sphairai*—"ringed spheres." There are two spectacular illustrations of armillary spheres preserved in ancient art—a ceiling painting in Stabiae, near Pompeii (see Arnaud, 1984: 73; Carmodo and Ferrara, 1989: 67-68) and a mosaic in Solunto, near Palermo (Von Boeselager, 1983: 56-60 and Tafel XV). Figure 7 is a Renaissance illustration of an armillary sphere, which scarcely differs from the ancient prototypes.

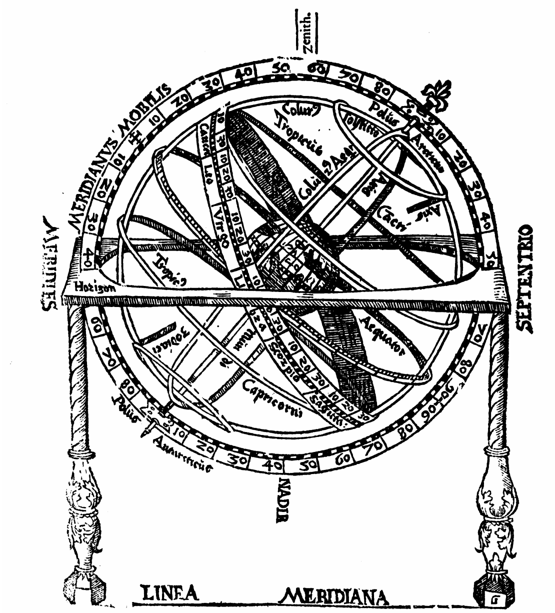

Figure 7. A Renaissance illustration of an armillary sphere (after *Cosmographia* ..., 1584) (photograph courtesy of the Special Collections, University of Washington Libraries, Negative UW18183).

Now, just preceding his list of sundials and their inventors, Vitruvius (*On Architecture*, ix, 7) gives us a fascinating look at how they can be designed. He describes a geometrical construction called the *analemma*. In Greek, *analemma* refers to a preliminary, supporting construction, which serves as an aid to the principal task. In everyday life, *analemma* could refer to 'scaffolding' or 'retaining walls', or even to a 'sling' for a wounded arm. In a graphical construction, an analemma plays a role analogous to that of a lemma in a logical proof. The analemma that Vitruvius constructs is shown in Figure 8.

The diagram lies in the plane of the celestial meridian. *AB* is the gnomon, which casts its shadow upon the ground *BT*. *EAI* represents the theoretical horizon plane. About *A* as center, describe circle *NBI* to represent the celestial sphere. Draw the axis *ZAQ* of the universe, at an angle *QAI* from the horizon. (This angle is equal to the latitude of the observer). The celestial equator is *NAF* and the two tropics (seen from the side) are *LPG* and *MOH*, located 24° above and below the equator. It is now instructive to compare Figure 8 with Figure 7. We see that *the analemma of Vitruvius is a side view of an armillary sphere.*

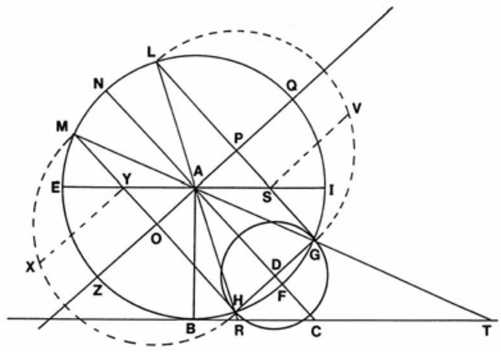

Figure 8. The analemma described by Vitruvius.

If a semicircle of the tropic of Cancer is folded down into the plane of the meridian, it becomes semicircle *LVG*. If we imagine standing this semicircle back up so that it is perpendicular to the plane of the diagram, line *SV* will lie in the plane of the horizon. *V* is therefore the sunset point—the location of the Sun at sunset on summer solstice. Arc *LV* therefore represents the six seasonal hours between noon (*L*) and sunset (*V*) on summer solstice. Sundial construction therefore begins with a division of arc *LV* into six equal parts.

In the same way, the daytime arc *MX* of the folded down tropic of Capricorn *MXH* must be divided into six equal parts. Unfortunately, Vitruvius (*On Architecture*, ix, 7.7) does not bother to tell us how to construct a sundial—lest he should "… prove tiresome by writing too much." At the end of his list of dial types and their inventors, he simply remarks, "Whoever wishes to find their projections can do so from the books of these writers, provided he understands the figure of the analemma … (*On Architecture*, ix, 8.1). Sadly, the end of Ptolemy's treatise on the analemma appears to be missing. So, in neither of the extant discussions of analemmas do we find an actual application. However, modern scholars have shown a number of ways in which the dials can actually be constructed (e.g. see Evans, 1998: 135-140).

Another interesting connection between gnomonics and *sphairopoiïa* is the occasional use of a miniature celestial globe as an ornament for the tip of the gnomon. We know that gnomons of monumental size sometimes ended in a spiked ball. This was true of the great sundial (discussed below) that

Augustus set up on the Field of Mars in Rome. The use of a sphere at the top of the gnomon aided the person reading a sundial to spot the true location of the shadow tip. Now, only three intact celestial globes have come down to us from antiquity. One of these, in Mainz, is 11 cm in diameter, is of copper alloy and is figured with constellations, the principal celestial circles and the Milky Way, as well as individual stars. The sphere has a small square hole in its top and a larger round hole in its bottom, which suggest that this globe was meant to fit over the tip of a gnomon (see Künzl, 2000).

In standard gnomonics, one places the tip of the gnomon at the center of the universe, as in Figure 8. Point *A* is simultaneously the tip of the gnomon, the entire Earth (which may be considered a mere point), and the center of the celestial sphere. In *Almagest,* i, 6, Ptolemy cites this practice as common knowledge, as part of his proof that the Earth is a point in respect to the celestial sphere: "Gnomons placed in any part of the Earth can play the role of the Earth's true center." Thus, placing a small celestial globe at the tip of a gnomon is not a mere whim, but reflects an understanding of a basic principle of gnomonics—that the tip of the gnomon is to be treated as the center of the Universe.

The quality of ancient sundials varies enormously. The workmanship varies from crude to exquisite. Some dials are engraved in marble and furnished with ornaments such as Lions' paws (e.g. Gibbs, Numbers 2017, 3027 and 3056), or are supported by a figure of Atlas (e.g. Gibbs, No. 1034). Others are quite plain, and were cut from cheaper stone, then covered with stucco. This is true, for example, of a number of the Pompeii dials (including Gibbs, No. 1026), which must have been the products of mass production.

In the same way, the time-keeping accuracy (both within the day and within the year) varies greatly from one dial to another. Some gnomonicists must have been skilled in the use of the analemma, which is suggested by the survival of construction marks on a few ancient dials. For example, the dial of Figure 6 carries several small holes situated on the declination circles, which, when connected, approximately determine a circle (see Savoie and Le Lehoucq, 2001: 32). This probably represents the *menaeus* circle mentioned by Vitruvius in the course of his construction of the analemma—the small circle with diameter *HG* in Figure 8. The *menaeus* circle is used precisely for establishing lines of a given declination. The dialer who made the this dial was a skillful gnomonicist, but even he had his limitations, as shown by the irregular, distorted shapes of the hour curves closest to the small hole. The theoretical construction of the hour curves on this ambitious type of dial is complicated. So it is possible that the dialer calibrated the hours, not by theory, but by taking time readings from an already-completed dial of simple construction.

5. SUNDIALS IN THE PUBLIC CONSCIOUSNESS

As we have seen, Vitruvius ascribes the invention of one kind of sundial to Eudoxus (ca. 360 B.C.), which does not seem impossible. Herodotus (ca. 430 B.C.) already mentions the gnomon in his *Histories* (ii, 109), saying that Greeks learned about it from the Babylonians. In the same passage, Herodotus also says that the Greeks got the twelve-hour day and the *polos* (perhaps a concave sundial) from the Babylonians. But the significance and accuracy of this statement have been much debated. Diogenes Laertius (*Lives and Opinions of Eminent Philosophers*, ii, 1) puts the first use of the gnomon by the Greeks much earlier, claiming that Anaximander (sixth century B.C.) set one up at Sparta. Of course, a gnomon can be used for purposes other than telling the time of day, such as for demonstrating solstices and equinoxes. We have no way of knowing when the first true sundials, calibrated in seasonal hours, were developed. But textual mentions of sundials, as well as the archaeological evidence, suggests that sundials did not become common until the third century B.C. After that, there must have been an explosion of interest, as new dial types were invented, and dials became increasingly common in public places.

One of the oldest approximately-datable dials is a double conical dial, with separate north- and south-facing dial faces (the north-facing one likely representing Vitruvius' antiborean type). This dial is currently in the Louvre (MA 2820), and is No. 3049 in Gibbs' catalogue (1976). It was found at Heraclea-by-Latmus, near Miletus (Turkey), and carries a dedication in Greek to King Ptolemy. This is most probably Ptolemy II Philadelphus, who ruled Egypt 281-246 B.C., and whose possessions included Heraclea for a time (and this is why the Louvre catalogue dates this dial to 277-262 B.C., "… by the historical context of the inscription."). This inscription includes the name of the donor, who presumably paid for the dial—a certain Apollonius the son of Apollodotus—and concludes: "Themistagoras the son of Meniskos, from Alexandria, made … [the sundial]." This Themistagoras, whose craftsmanship is excellent, is the earliest gnomonicist from whom we possess datable work. I suggest that the next interplanetary gnomonics mission should carry a dedication to Themistagoras.

Although sundials spread first through the Greek world they also became known to Roman society in the third century B.C. According to Pliny (*Natural History*, vii, 213), the first public sundial in Rome was set up on a column during the First Punic War, having been brought to Rome by the consul Manius Valerius Messala after his capture of Catania in Greek Sicily (263 B.C.). Pliny remarks that, although the lines on this sundial did not agree with the hours, the Romans continued to use it for ninety-nine years, until it was replaced by a better one. All good gnomonicists of course knew that sundials had to be designed for specific latitudes. But third-century Romans were as yet unschooled in gnomonics; besides, the dial stolen from Catania was probably intended to serve more as trophy than as timekeeper. And yet, the case has a number of parallels, for modern analyses show that not all ancient dials were designed for the latitudes of the sites at which they were later recovered. For example, according to Savoie and Lehoucq (2001), the beautiful dial shown in Figure 6, which was found at Carthage (latitude 37º), was actually designed for a latitude near 41º (corresponding to Rome or the Hellespont). Even today, it is notorious that sundials in garden shops, designed for a generic 45º, are often sold to buyers innocent of latitudinal concerns.

By the first century B.C., sundials had become common enough in the Greek world that writers on geography or astronomy could expect their readers to be familiar with these instruments, as well as with celestial globes. Assuming such familiarity, the writer was able to appeal to these instruments in illustration or argument. In the introductory book of his *Geography*, Strabo assures his readers that they need not be experts in astronomy, but warns they should not be so simple or lazy as never to have seen a globe and the circles inscribed upon it, or to have examined the positions of the tropics, equator and zodiac. As Strabo (*Geography*, i, 1.21) points out, this is the sort of background acquired in an introductory mathematics courses. And when referring to the daily revolution of the heavens, he remarks (*Geography*, i, 1.20) that this revolution is manifest most particularly from sundials.

A good example of the elementary astronomy curriculum that Strabo had in mind is provided by the *Introduction to the Phenomena* of Geminus, whom we met above as the author of the lost *Philokalia*. The *Introduction to the Phenomena*, which has come down to us more or less intact, was written for beginners and provides a patient and graceful introduction to many topics of Greek astronomy. On several occasions, in order to make a convincing astronomical point, Geminus appeals to objects that his reader is likely to be familiar with, including celestial globes and sundials. For example, in discussing the fact that the Sun's declination scarcely changes while it is approaching and then receding from the tropic, Geminus remarks, "This is clear, too, from the sundials, for the tip of the gnomon's shadow remains on the tropic curves for about 40 days." (Geminus, *Introduction to the Phenomena*, vi, 32; see also i, 38). Geminus expected his students and readers to be familiar enough with sundials to accept this casual appeal to observational evidence.

We can get some idea of the popularity of sundials in the first century A.D. from the large number found at Pompeii—nearly three dozen, found in public squares and private gardens. Of course, Pompeii was an expensive resort town, and far from typical. Still, these objects must have been quite common in cities of any size.

We moderns are used to seeing inscriptions on sundials, perhaps reminding us of the associations of the fleeting shadow, perhaps asking us to meditate on our cosmic connections. One of my personal favorites occurs on an eighteenth-century dial in Nyon, Switzerland: "Qui trop me regarde perd son temps." ("Who looks at me too much wastes his time"). Inscriptions of this sort do not occur on ancient Greek and Roman sundials. Indeed, most ancient dials have no inscriptions at all. And for those that do, the most common is the name of the donor, or, more rarely, the name of the maker of the dial. However, literary epigrams *about* sundials do occur. One of the jocular insult epigrams of the *Palatine Anthology* compares the nose of the object of humor to the gnomon of a sundial:

> If you put your nose to the sun and open your mouth,
> you will show the hours to all who pass by.
> (*Greek Anthology*, xi, 418. See Paton, 1969, for the Greek text).

This epigram is attributed to the Emperor Trajan, though one need not take the attribution too seriously.

A number of epigrams in this collection involve riddles or arithmetical problems. One of the latter, concerning the time of day, is made sharper by purportedly addressing itself to a maker of sundials:

> Diodorus, great glory of gnomonicists, tell me the hour since when the golden wheels of the Sun leapt up from the east to the pole. Four times three-fifths of the distance he has traversed remain until he sinks to the western sea (*Greek Anthology*, xiv, 139. Translation adapted from Paton, 1969, volume 5, p. 101).

(The answer is: 3 9/17 hours have passed since sunrise, 8 8/17 remain till sunset. For those who wish to solve the problem, remember that the time is expressed in seasonal hours.)

We are used to metaphors of mortality expressed in terms of time-keeping devices: the sands of time, the final hour. Images of sundials were occasionally used in this way in Roman sculpture. For example, there is a sarcophagus in the Louvre (MA 355), from the early third century A.D., which is decorated with a relief of the cycle of Prometheus, including the creation and the destiny of Man. Near the figure of Death, and behind the three Fates, Atropos, Clotho and Lachesis, is a sundial on a column. Other sarcophagi in the Louvre with images of sundials are MA 284 and MA 1341.

In Rome, a sundial of monumental size served to make a political statement. In 10 B.C., Augustus had constructed on the Field of Mars a horizontal, plane sundial with a gnomon 100 Roman feet (30 meters) high (for details see Buchner, 1982, and for a short account, Claridge, 1998: 190-192). The gnomon was an ancient obelisk of pink granite that had been taken from Heliopolis in Egypt (Strabo, *Geography*, xvii, 1.26). The gnomon still survives, and stands now in the Piazzi di Montecitorio, some 200 meters southeast of its original location. In the inscription on the side of the gnomon's base, Augustus dedicated the dial to the Sun, in commemoration of his own annexation of Egypt some twenty years before. So here a monumental gnomon and sundial serve imperial aims, not only by celebrating the military strength of the Emperor, but also by affirming his association with the Sun god. Although the metal sphere that now tops the obelisk is not original, we know that the gnomon originally did indeed carry just such a sphere, as it is mentioned by Pliny (*Natural History*, xxxvi, 70-73). And it is also represented on a relief sculpture of the apotheosis of Antoninus Pius (now in the Vatican Museums), which shows a personification of the Field of Mars holding the gnomon of Augustus, complete with sphere at the top (for photographs of this, see Claridge, 1998: 194; Gundel, 1992: 78).

The dial face must have been similar to that of a standard horizontal, plane dial, such as that in Figure 5. Pliny tells us that it was designed by a mathematician named Novius Facundus, who is otherwise unknown, but who must have been an excellent gnomonicist. According to Pliny, bronze markers in the pavement allowed one to measure the length of the noon shadow day by day. Remarkably, a short section of the stone and bronze meridian of Augustus' dial was discovered in 1979/1980 and answers precisely to this description. The meridian is divided into signs of the zodiac, labeled with their names in Greek, and the signs are subdivided into individual degrees (at least for the portions not too near the tropics). It is interesting that a dial commissioned for public display at Rome should be engraved in Greek. This no doubt reflects the status of Greek as the recognizeed language of science, and particularly of astronomy.

Augustus' monumental dial also incorporated some features of a parapegema.[4] At intervals along the meridian were notices of important events in the cycle of the seasons, for example, the beginning of summer, or the cessation of the etesian winds. The etesian winds are annually-recurring (hence their Greek name) north winds that blow in the Mediterranean in summer, giving some relief from the heat. They were commonly held to begin blowing at about the morning rising of the Dog Star, and to last for two months. The great dial of Augustus has them stopping shortly after the Sun enters Virgo, which agrees with other ancient parapegmata.

Pliny tells us that by his day (ca. A.D. 70), readings taken from the sundial (presumably of seasonal clues, such as the Sun's place in the zodiac, which could easily be checked against a calendar) had been out of agreement with the facts for about thirty years. He conjectures that this might have been caused by a shift in the position of the gnomon (despite its very deep foundations), by a change in the course of the Sun, or perhaps by a small shift of the Earth itself from its central position. For the latter, says Pliny, there is also some evidence from other locations.

6. CONCLUDING REMARKS

In this paper I have tried to show how intimately gnomonics was connected with other aspects of life in Greek and Roman antiquity. Gnomonics was a specialized genre of applied mathematics, with its own literature, as well as a trade practiced by skilled masons. It had vital links to the theory of the celestial sphere, as well to *sphairopoiïa* and other mechanic arts. Sundials were important enough as architectural ornaments that Vitruvius felt it necessary to treat them in his *Ten Books on Architecture*. Sundials provided a way of showing off a gnomonicist's skill, or of demonstrating a patron's wealth and sophistication, of making a bittersweet observation on human mortality, of flattering a ruler, or of asserting the grandest of imperial ambitions. All that—and they told time, too. The MarsDial, which has much more to say about human aspirations than about the time of day on Mars, stands squarely in a vital tradition.

7. ACKNOWLEDGEMENTS

I am grateful to the Soprintendenza Archelogica di Pompei for Figure 3 and permission to publish it, to the Trustees of the British Museum for Figure 4, to the Special Collections division of the University of Washington Libraries for Figure 7, and to Denis Savoie for the photograph of his own cast of the dial in Figure 6.

8. NOTES

1. See Pliny's (*Natural History*, vii, 191-209) fabulous list of inventors, including the inventors of houses, ships and cities. For an astronomical example, see Eudemus' list of astronomical discoveries, as reported by Theon of Smyrna (*Mathematical Knowledge Useful for Reading Plato*, iii, 40; Dupuis, 1892: 320-321).
2. Gnomonics showed a rich development in the Arabic Middle Ages, including its adaptation to the needs of Islam by incorporating curves on the dial to indicate prayer times. These developments are beyond the scope of this paper, but for a fascinating introduction see Savoie (2004), which includes references to a substantial literature on Arabic/Islamic sundials.
3. Several ancient dials call attention to the variation in the length of the day throughout the year, by comparing the length of the winter solstitial day to the other days of the year. These are Gibbs Nos. 1044, 1068, 3046 and 4001. For a discussion of the latter see Evans (1998: 130-131).
4. For an introduction to parapegmata, see Evans (1998: 199-204) and Taub (2003: 20-37). For a catalogue of all known examples see Lehoux's (2000) Ph. D. dissertation.

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LIGHT WORK:
Contemporary Artists Consider the Sun

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Abstract: Modern day life and timekeepers have profoundly affected the way we conceptualize time and our position in the universe. Over the past year, I have been investigating the apparent movement of the Sun both sculpturally and photographically. In this paper, I discuss my collaborations with Woody Sullivan and highlight several of the sundials, both gigantic and intimate, created by University of Washington students in the class *Where is Noon? Regarding Giant Sundials* that we co-taught in Spring 2003. I have continued to develop artistic approaches to solar events. Some of these sunworks have not been designed specifically to measure the exact time of day as a classic sundial does, but to stimulate a greater awareness of our subjective and paradoxical relationship to nature and technology. Other, almost domestic, poetic, humorous or intimate ways of interacting with science and technology are being actively explored. I will also provide a background to previous works I have done in relation to the Sun and optics, and briefly mention artists who are using astronomical events as a point of departure.

Key words: art, Sun, light, sundials

"Between the idea
And the reality
Between the motion
And the act
Falls the Shadow." (T.S. Eliot, 1925).

1. MY INTRODUCTION TO GNOMONICS

I made my first sundial in 2003, so I am fairly new to gnomonics (the art of making and using sundials).[1] Presenting this paper in the context of astronomy reminds me of the Gracie Allen phrase, "Do you realize that

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everything I know I learned by listening to myself when I was talking about things that I didn't understand?"

My gnomonic journey began when I was invited to create an outdoor work for *Spheres: The University of Washington 2003 Summer Arts Festival*. When I mentioned to Director Hannah Wiley that I was interested in making a sundial, she promptly arranged a meeting with Woody. There could have been no more highly qualified collaborator. Some who know Woody within the context of astronomy may not realize his stated mission to make Seattle the sundial capital of North America. Those who know Seattle weather may realize the irony in this endeavor, but even this accomplishment would not be as outrageous as sending a sundial to Mars, a feat Woody, Bill Nye, Larry Stark and a team from NASA realized when the *Spirit* and *Opportunity Rovers* landed in January 2004. Figure 1 shows Woody and Larry wearing a t-shirt I designed with David Halsell ("My Sundial went to Mars, but all I got was this stupid T-shirt"), featuring an image of the *MarsDial*. Plus, Woody has a highly-developed appreciation of contemporary art, formidable energy—and his curiosity is contagious.

Figure 1. Larry Stark (left) and Woody Sullivan (right) in their *MarsDial* t-shirts, 2004.

We initially had two meetings to design a large single horizontal sundial with a vertical gnomon (the gnomon indicates the time by casting a shadow on a marked surface). En route to our third meeting, I realized that almost anything could function as a gnomon—trees, light posts, buildings, parking meters, rocks, my head, etc. When I mentioned this insight to Woody, he laughed and replied, “That’s the way I’ve walked around my entire life.” So, rather than erect a modernist sculpture, we decided to co-opt existing buildings and monuments on campus.

2. THE SUNDIAL COURSE, SPRING 2003

While our collaboration began as a challenge to create a single sundial specifically for the Arts Festival, it quickly evolved into two gigantic dials and a collaborative interdisciplinary course (we can both be victims of our own enthusiasm). In *Where is Noon? Regarding Giant Sundials*, students created large-scale and intimate sundials. This project successfully bridged science and art, past and present, global and local, architecture and the cosmos. It resulted in a campus-wide exhibition and tour of large sundials and a display of handheld dials during the Arts Festival. Students from Mathematics, Physics, Philosophy, Art, Dance, Political Science, Philosophy of Science, Communications, Astronomy, and Community and Environmental Planning were introduced to the history of sundials, the fundamentals of their construction and to the range of contemporary approaches to sundial and environmental public art-making. Figure 2 shows Woody (during a class sundial tour of Seattle) marking the solar spot cast by a 25-cm diameter southwest-facing window in the offices of George Suyama Architects, Seattle. At various times the spot can fall on the floor, on any of three walls, and even down a stairway.

Figure 2. Woody Sullivan marking a solar spot. Class Sundial Tour of Seattle, April 2003.

In *Baghdad by George,* Woody and I enlisted the statue of George Washington on campus as the giant gnomon for a horizontal sundial. The shadow cast by George's head indicated the time in Baghdad as it crossed the yellow hour marker lines installed in the brick pavement. In Figure 3, his shadow is shown at 10:10 am Seattle time, or 9:10 pm Baghdad time.

Figure 3. Baghdad by George: 10:10am Seattle, 9:10pm Baghdad, Cummins/Sullivan, 2003.

Solar Arcade (Figure 4) was inspired by the astronomical experiments of seventeenth century astronomers who cut holes in European churches to study the Sun's motions. The huge southwest facing, circular window above the covered passageway between Allen and Suzzallo Libraries was utilized. We marked calculated positions for where the large spot of sunlight would fall on the bricks below. Four ellipses, two of which were partially on the library walls, were traced with aqua-colored metal discs to indicate the track of the 'sunspot' over the afternoon of the summer solstice (June 21 at 2:10, 3:45, 4:10 and 4:45pm PDT). In contrast, in the winter, the sunspot hits high above the pathway on an interior wall (a photograph of the winter position was shown on the signage). Woody had been recording the movement of the Sun at this site for years; this project made his observations and the Sun's movements publicly visible and meaningful to pedestrians traversing the passageway over the two month period it was installed.

The students also produced a lively range of eight giant timekeepers and sixteen intimate, handheld dials. In *Solar Observation Boxes,* three large wooden shipping crates were re-purposed; viewers peered into each box to perceive the movement of the Sun in relation to a tableau of forms, text and objects. In *Step Up*, a human gnomon dial, visitors were invited to stand according to their height and read their shadow to discover the time. Also, a lovely dance choreographed by Valerie Pitt for four dancers was entitled *Make Haste… Slowly*. The dance emphasized time, rhythm patterns and shadow movement—demonstrated with the dancers' bodies on the human gnomon dial.

Figure 4. Solar Arcade, University of Washington, 2003. Cummins/Sullivan.

Shirley J. Benson and Lauren Saint created *Sun Slide* (Figure 5), an interactive sundial in which the audience notated the Sun's fast-changing position by tracing and dating its elliptical projection on an acrylic cylinder. The accumulated drawings illustrated the Sun's apparent movement for each day. *Somewhere it is Noon* was a set of five wooden vertical dials[2] that mimicked the time zone clocks customarily seen in train stations and travel agencies; instead of major cities, various significant archeological sites were represented (i.e. Babylon, Pompei). The 'clocks' were appropriately mounted on the south-facing wall of the UW Burke Museum of Natural History and Culture.

Figure 5. Sun Slide, Shirley J. Benson/Lauren Saint, 2003.

In *Fleeting Light*, students took advantage of the shadows created by the three tall chimney towers in the University of Washington's Red Square that cast shadows on each other, on the ground, and on the south face of Kane Hall. The concrete columns of Kane Hall were emphasized with banners of color across which the shadows passed from 2pm to 6pm. Signage explained the correspondence between time of day and the colored columns being shaded.

In *Captain Planet* by Steven Hertzfeld and Anna Carlstrom, four horizontal dials enlisted cafe tabletops and a Captain Planet figure as the gnomon to tell time on Earth, Jupiter, Saturn and Mars. David Halsell illustrated his proposal for the *Nodus Ball Dial*; the shadow from a gigantic black ball suspended between the Art and Music Buildings would be marked to indicate the time on the grass 25 meters below.

Figure 6. Sundial Sunglasses, Shannon Palmer, 2003.

It was possible to be both stylish and on time with Shannon Palmer's *Sundial Sunglasses* (Figure 6), Alex Rojas' *Japanese Movement* (Figure 7) and Nina Zingale's *Orange Purse Dial* (Figure 8). While facing south, viewers could peer through Shannon's polarized sunglasses at the Sun, noting its position in relation to the clear numbers on the lens; on the hour, the Sun shone through the appropriate number. Alex Rojas wears his sundial on his wrist; *Japanese Movement* (Figure 7) is influenced by a Sun clock from the Edo Era (1603–1867).

Figure 7. Japanese Movement, Alex Rojas, 2003.

Figure 8. Orange Purse Dial, Nina Zingale, 2003.

Technological Wonders (Figure 9) by Laurel Rachmeler conflated technological eras by combining an iBook and a string gnomon for timekeeping. This was inspired by the sixteenth century portable diptych dial, which was often made of ivory and opened like a cosmetics compact.

Figure 9. Technological Wonders, Laurel Rachmeler, 2003.

The Shepherd's Watering Hole (Nalgene Bottle) Dial (Figure 10) by Valerie Pitt (based on the classic shepherd's column dial) is a clever device for determining the time while hiking. And of course, the *Horizontal Party Dial* by Anna Carlstrom (Figure 11) would be the perfect compliment to any barbeque; aligned to true north, the standard party keg holds the pizza box/horizontal sundial in place.

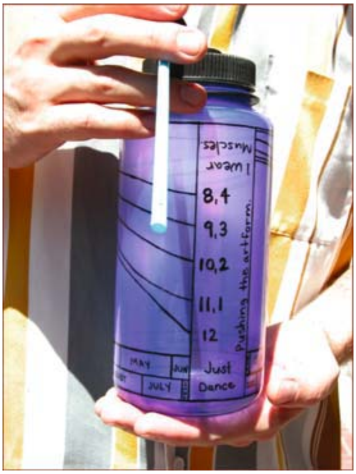

Figure 10. The Shepherd's Watering Hole (Nalgene Bottle) Dial, Valerie Pitt, 2003.

Figure 11. The Horizontal Party Dial, Anna Carlstrom, 2003.

3. SUBSEQUENT SUNWORKS

Following the Summer Arts Festival, I continued to develop artistic approaches to solar events. In *Tilt, Seattle* (Figure 12), I enlisted a kitchen shelf, a glass of water and a pencil to illustrate seasonal changes in the declination of the Sun in Seattle at solar noon.

Figure 12. Tilt, Seattle, Cummins, 2003.

3.1 An Aperture Dial in a Library

Soon after, I was commissioned to create an artwork for the new Montlake Public Library (installation 2005). My concept features a line of five circular skylights (with discs of five different colors) in the ceiling that will produce a row of moving, colorful spotlights on the Library interior floor when the Sun shines; the center skylight will become the aperture nodus for the sundial and the spotlight it projects will be marked on the floor below to indicate solar noon.

While there are seventeenth and eighteenth century European precedents for this type of sundial, the concept for the Montlake Library evolved through studying and responding to the way light tracked on a model of the building under a heliodon at the Seattle Lighting Center.[3] As far as I know, this will be the first aperture sundial in North America.

The skylight sundial invites Library users and the community to gain a sense of how the Earth's movements affect daily and seasonal light and shadow occurrences quite specific to the Library's geographical location and architecture (i.e. in another geographical location or orientation of the building, the effects would be different). Light tracks in wondrous and often surprising ways; the Library will become a small observatory, much in the way some European cathedrals were used by astronomers.

3.2 Roman Projects

In Autumn 2003, I led the University of Washington School of Art's Studio Program in Rome. There were abundant opportunities to experience remarkable sundials, both ancient and modern, but my favorite was the *meridiana* in the Church of Santa Maria degli Angeli, where I had the opportunity to watch an aperture sundial in action. This extraordinary church was designed by Michaelangelo on the site of the Baths of Diocletian (to enter the Cathedral, one walks through the ancient façade). In *The Sun in the Church: Cathedrals as Solar Observatories,* J.L. Heilbron (1999) explains that while the Roman Catholic Church did prosecute Galileo, it also allowed astronomers to dramatically bisect their cathedrals with solar noon lines (*meridiana*) and cut holes in their ceilings—so that they might finally establish the date for Easter. The *meridiana* at Santa Maria degli Angeli was added in 1701 by Francesco Bianchini.

At Santa Maria degli Angeli I experienced suspense and pleasure in watching the spot of sunlight cross the brass marker line of the *meridiana* at solar noon. On 25 October 2003, determined to document this experience, I set up a tripod and camera to record the Sun's movement every thirty seconds (starting ten minutes before solar noon). Midway through the photographic documentation, in what had been a very quiet cathedral, several men rushed over to set up a white placard on an easel directly over the *meridiana*. They began to gesture and talk excitedly in Italian; I realized they had come to watch the unprecedented solar flare activity I had heard about. There, in the 'sunspot' on the white board, were round, undulating shadows where the flares were occurring—live, of course. Graciously, they moved their easel aside so that I could record the moment when the Sun slid across the *meridiana* (Figure 13). It seemed magical that a device of great astronomical importance in the early eighteenth century, and one that the citizens of Rome had set their wrist-watches by in the nineteenth century, could still serve to inform contemporary observers.

Figure 13. Sunlight crossing the *Meridiana* at noon, Santa Maria degli Angeli, Rome, Italy, Cummins, 2003.

Heilbron (1999: 23) articulately describes his fascination with these tools that explore sunlight: “Finally, there are the instruments themselves, constructions both beautiful and useful, conduits of light through vast dark spaces, defunct sites of science, living objects of wonder … There is something romantic, even sublime, in witnessing the faithful rendezvous of Sun and rod arranged centuries ago. Among the old meanings of the word “matematizzare”, or to mathematize, was “to cast a spell”.”

While in Rome, I began several photographic projects to record the movement of shadows over regular intervals of time. In one 14-part photographic sequence, I stood as a gnomon in the ancient Roman Forum through opening hours, 9am to 3:30pm (*Gnomon at the Roman Forum, December 13, 2003*). I was photographed from above the Forum every 30 minutes in order to record my changing shadow against the backdrop of the ruins.

Gnomonics is an ongoing photographic series that records the movement of shadows—during lunchtimes. For the duration of each of these ‘events’, I celebrate the movement of shadows cast by goblets and utensils by drawing directly onto the tablecloth at regular intervals. *Café Farnese, Rome, Italy, November 23, 2003 … a recording of shadows every ten minutes over lunch (noon – 1pm)*; *Er Grottino, Campo de Fiori, Rome, Italy, December 3, 2003 … a recording of shadows every fifteen minutes over lunch (noon – 1:30* *pm)*; and *Solstice Lunch with Lee: Tate Modern, London, December 22, 2003 ... a recording of shadows during intermittent sunbursts over lunch (noon – 1:30 pm)* are three of the ten images done to date (all inkjet prints, 16 × 20 in.). Examples are shown in Figure 14.

Figure 14. Shadow drawings over lunch at café tables in Rome and London, Cummins, 2003.

3.3 Other Shadow Works

Shadow Locomotion: 128 Years After Muybridge, Stanford University, Palo Alto ... an hourly recording of shadows from 11am – 5pm, February 28, 2004 (Figure 15), and pays conscious tribute to Eadweard Muybridge, the well known nineteenth century photographer, and his early photographic horse experiments with Leland Stanford at the Palo Alto Stock Farm, Stanford University, in the 1870's. I posed astride a horse (Cherokee, a 22 year old dressage horse) at the site; our traveling shadow was recorded every hour from 11am to 5pm. Muybridge's most famous series of chronological studies of movement (often considered precursors to cinema) was entitled *Animal Locomotion*, hence my title.

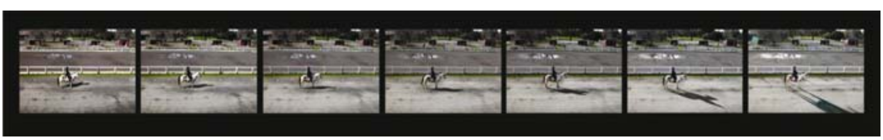

Figure 15. Shadow Locomotion: 128 Years after Muybride, Palo Alto Stock Farm, Stanford University, Cummins, 2004, 84 × 11 inches.

In *Shade Watching* I've been playfully collecting a series of singular gnomonic moments, such as *Gladiator at 11am, Roman Coliseum, December 14, 2003*; *Pigeon at 3pm, Winter Solstice, London*; *Wombat at 5pm, Narwantapu National Park, Tasmania, September 15, 2004*; *and Wallaby at 9am, Narwantapu National Park, Tasmania, September 16, 2004* (Figure 16).

I have also photographed myself as a gnomon on a full Moon for *Moondial, Banks Lake, Washington, August 29, 2004 (10pm-4am)*; the shadow created is distinct and beautiful, if subtler than the sundial.

Figure 16. Wallaby at 9am, Tasmania, Australia, Cummins, 2004.

Future 'Sun markings' will record a 24 hour series at the equinox in North America and, hopefully, a 24 hour video event in Antarctica at the summer solstice. I also am investigating the materials and techniques for creating solid, lifesize sculptural objects describing the shape of the shadow my body creates.

4. PREVIOUS WORKS RELATING TO THE SUN AND OPTICS

These gnomonic investigations are directly related to previous works I have done over the last decade in which I have explored the sculptural, experiential and sometimes humorous possibilities of light and natural phenomena (often referencing the history of optics). Frequently, obsolete technologies (such as camera obscuras, phantasmagoria and periscopes) are incorporated, often in combination with newer media technologies (such as video, computers, photography and digital imaging).

Installations have incorporated a machine for making rainbows, a camera obscura/fibre-optic journey through the center of the Earth (*700 Million Miles an Hour: Journey Through the Center of the Earth* would visually connect London and central Australia live), paranoid dinner-table devices (*Liquid Scrutiny*—Figure 17—featured a set of periscope silver goblets), an interactive computer/video rifle (*To Fall Standing* updated French physiologist E.J.Marey's photographic rifle of 1882), photographs and a periscope birdbath.

Figure 17. Liquid Scrutiny: Paranoid Dinner Table Devices, silver, glass, Cummins, 1997.

In the spirit of nineteenth century chimeras, site-specific portable camera obscuras have been merged with garbage bins (The *Throw-Away Camera,* Figure 18), flowerpots, portable toilets (*The Giovanni della Portaloo*, which paid homage to Giovanni Battista della Porta, the sixteenth century Italian who first wrote about the camera obscura effect), birdhouses, televisions and Tibetan cheese boxes.

Trucks and campervans have also been converted into traveling camera obscuras (the first was the *Upward Mobile Home*, a traveling camera obscura on wheels for the Blanden Museum in Fort Dodge, Iowa, 1994). I remain enthralled by the pristine beauty of the image the camera obscura reveals and, although I do not think painting has been buried, I significantly empathize with Christiaan Huygens' sentiments in 1622 upon seeing his first camera obscura image: "It is impossible to express its beauty in words. The art of painting is dead, for this is life itself, or something higher, if we could find a word for it." (Kemp, 1990: 192).

Figure 18. Throw Away Camera, screen view/installation (camera obscura), Cummins, 1996.

In *Simply Smashing* (Figure 19), 868 stacked wineglasses were filled with water to make pristine optical lenses that invert the view; the surrounding environment plays an unavoidable role in the final presentation.

Figure 19. Simply Smashing, Cummins, 2000, 2004.

In *The Rainbow Machine* (1998) viewers experienced full-spectrum primary and secondary rainbows when the Sun was shining onto a wall of water. *Light Rain* (Figure 20) extended this project into an interactive sound and light sculpture in collaboration with Paul DeMarinis[4] at the Museum of Contemporary Art, KIASMA in Helsinki, Finland (2004). When you enter the rainbow with an umbrella it will sing to you! Multiple water streams were specially modulated with audio signals; these sound vibrations influenced the breakup of a water stream, producing distinct visual patterns. Amazingly, these patterns preserve aspects of the sound signal itself, such that when the drops fall on a resonating surface (like an umbrella) recognizable melodies, such as *Singing in the Rain,* are heard.

Figure 20. Light Rain, Cummins/Paul DeMarinis, 2004.

5. WORKS BY OTHER ARTISTS[5]

Several other contemporary artists are actively using astronomical events as a point of departure for their artwork. The best known of these is James Turrell, who has been exploring light as a sculptural medium for decades. His most ambitious project, *Roden Crater* in Arizona, is an extinct volcano that he has transformed into a celestial observatory in the Painted Desert.

Charles Ross has also completed major works dealing with light and astronomy. In *The Year of the Solar Burns* (commissioned by the French Ministry of Culture in 1993), Ross used a fresnel lens to burn the Sun's path onto a plank of wood every day for 365 days. Shown were variations in sky conditions: on a sunny day the burn mark was flared; intermittent clouds produced interruptions in the burn; and cloudy days produced blank planks. The 365 pieces of wood were then installed in Chateau d'Oiron, a fifteenth century castle in France, to graphically show the changing trail of the Sun through the year. Ross' *Star Axis,* begun in 1971*,* is an eleven storey 'star tunnel', an inverted cone aligned with the Earth's axis carved into the side of a mesa in New Mexico. As the viewer ascends the tunnel, the circle of sky seen above represents the orbit of Polaris at a specific time in its 29,920 year cycle.

Heather Ackroyd and Dan Harvey (U.K.) manipulate the process of photosynthesis to create huge chlorophyll grass photographs with the equivalent tonal range of a black and white photograph, but in yellow and green.

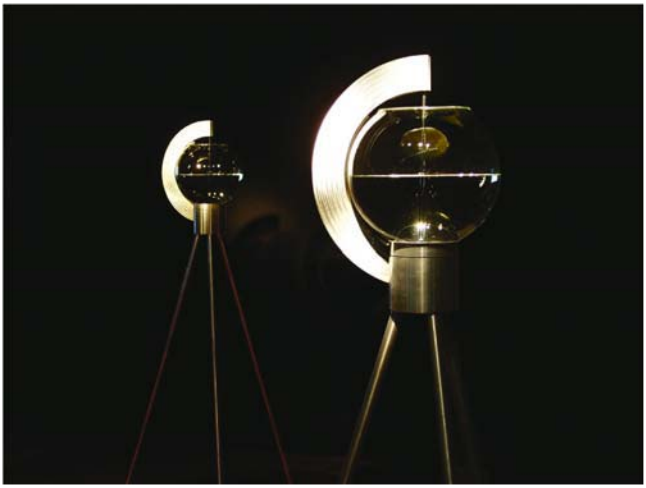

Figure 21. Tide, Luke Jerram, 2001 (courtesy: Luke Jerram).

In *Tide* (2001, Figure 21), Luke Jerram (U.K.) uses a gravity meter in the gallery to measure the gravitational pull of the Moon. These data are then used to control the water level in three large glass spheres. A friction device on the lip of each spinning sphere generates a tonal hum (like a ringing wine glass); the pitch fluctuates with the tidal ebb and surge of the water level—allowing the spheres to visibly and audibly resonate in direct response to the gravitational pull of the Moon.

6. CONCLUDING REMARKS

Like the artists mentioned, I continue to be captivated by the potential for imaginative involvement with history, science and technology as viable sources of communication, curiosity and poetic fantasy. These artworks represent the aspiration to provoke curiosity and pleasure in the boundary zone between experience and thought, aesthetics and science.

7. ACKNOWLEDGEMENTS

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Finally, a special thanks, also, to Woody Sullivan for inspiration and computations.

8. NOTES

1. For further examples of my work, see http://www.rebeccacummins.com
2. This type of dial (to be specific, a vertical direct south dial) is wall-mounted and the gnomon directly aligned with Polaris, so it protrudes out of the dial face at a downwards angle.
3. A heliodon is a mechanical apparatus that imitates the apparent movement of the Sun. The Seattle Lighting Center heliodon allows architects to record and study movements of the Sun within their buildings at various seasons and times of day.
4. For further examples of Paul DeMarinis' work see http://www.well.com/~demarini/
5. For further examples of the work by Luke Jerram and Charles Ross see respectively http://www/lukejerram.com and http://www.staraxis.org

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Marshall Cohen was born in Manchester, New Hampshire (USA) in 1926. He holds B.E.E., M.S. and Ph.D. degrees, the latter two in physics, all from Ohio State University. He is Emeritus Professor of Astronomy at Caltech in Pasadena, California. He has served on several boards and committees of the American Astronomical Society, the Astronomical Society of the Pacific, and the International Union for Scientific Radio, and was on the Radio Panel for the NRC astronomy decadal reviews in 1970, 1980, and 1990. He has been a member of numerous Visiting Committees for radio observatories. He is a member of the National Academy of Sciences, a Fellow of the American Association for the Advancement of Science, and a member of the American Academy of Arts and Sciences; he shared the Rumford Medal awarded by this organization, for the development of very-long-baseline-interferometry. His research interests have been in solar physics, radio interferometry, compact radio sources, and optical polarimetry of radio galaxies and white dwarf stars. Currently, he is working on a history of the Owens Valley Radio Observatory.

Rebecca Cummins was born in Ft. Dodge, Iowa (USA) in 1957. She holds a B.F.A. from the University of Northern Iowa, an M.A. from the University of New Mexico and a D.C.A. from the University of Technology, Sydney, Australia. She is currently an Assistant Professor in the School of Art, University of Washington, Seattle, Washington following seventeen years at the University of Sydney. She is also a Director on the National Board of the Society for Photographic Education. Her research interests include the history of media and optical devices, art and science, photography, gnomonics and installation art. She has exhibited widely; exhibitions in 2004 included the solo exhibition *Slow Light* at *I Space* in Chicago, and *Cutting Edge: Artists Experiment With Glass*, at the *Exploratorium: The Museum of Art, Science and Human Perception* and a collaboration (*Light Rain)* with Paul DeMarinis at *KIASMA, Museum of Contemporary Art*, Helsinki, Finland during the 2004 International Symposium for Electronic Art.

David DeVorkin was born in 1944, has degrees in astrophysics from UCLA, San Diego and Yale, and a Ph.D. in history of astronomy from Leicester. He is currently Curator of the History of Astronomy at the National Air and Space Museum of the Smithsonian Institution. His major interest at present is the transformation of the Smithsonian Astrophysical Observatory by the Cold War and he was delighted to have a chance to honor Woody with the first outing of this research activity. He is an Editorial Board member of the *Journal for the History of Astronomy* and *Isis*, and presently is on the Organising Committee of IAU Commission 41 (History of Astronomy). He has published extensively on the history of astrophysics and the space sciences, including the books *Race to the Stratosphere: Manned Scientific Ballooning in America* (New York, Springer Verlag, 1989), *Science with a Vengeance: How the Military Created the US Space Sciences after World War II* (New York, Springer-Verlag, 1992; reprinted, 1993, paperback study edition), and *Henry Norris Russell: Dean of American Astronomers* (Princeton, Princeton University Press, 2000), among a total of nine monographs and well over 100 papers and essays.

Steven J. Dick is the Chief Historian for NASA. He was born in Evansville, Indiana (USA), in 1949, and obtained his B.S. in astrophysics (1971), and M.A. and Ph.D. (1977) in history and philosophy of science from Indiana University. He worked as an astronomer and historian of science at the U.S.

Naval Observatory in Washington D.C. for twenty-four years before coming to NASA Headquarters in 2003. Among his books are *Plurality of Worlds: The Origins of the Extraterrestrial Life Debate from Democritus to Kant* (1982), *The Biological Universe: The Twentieth Century Extraterrestrial Life Debate and the Limits of Science* (Cambridge University Press, 1996), and *Life on Other Worlds* (1998), the latter translated into Chinese, Italian, Czech and Polish. His most recent books are *The Living Universe: NASA and the Development of Astrobiology* (2004), and a comprehensive history of the U.S. Naval Observatory, *Sky and Ocean Joined: The U. S. Naval Observatory, 1830-2000* (2003). The latter received the Pendleton Prize of the Society for History in the Federal Government. He is also editor of *Many Worlds: The New Universe, Extraterrestrial Life and the Theological Implications* (2000), and (with Keith Cowing) of the NASA Administrator's Symposium *Risk and Exploration: Earth, Sea and the Stars* (2005). He is the recipient of the Navy Meritorious Civilian Service Medal, and the NASA Group Achievement Award for his role in NASA's multidisciplinary program in astrobiology. He has served as Chairman of the Historical Astronomy Division of the American Astronomical Society, as President of the History of Astronomy Commission of the International Astronomical Union, and is the immediate past President of the Philosophical Society of Washington.

James Evans was born in North Carolina (USA) in 1948 and has a B.S in electrical engineering from Purdue University and a Ph.D. in physics from the University of Washington. After his Ph.D. he spent a year on a Fulbright Grant at the Centre Koyré in Paris, under the 'wing' of the late René Taton, where he re-tooled as an historian of science. He is a physicist and historian of science who teaches at the University of Puget Sound in Tacoma. His scientific papers in *General Relativity and Gravitation* and other journals have recently focused on optical-mechanical analogies in the context of general relativity. In history of physics he has published several studies of the life and gravitational theory of Georges-Louis Le Sage, most recently in *Pushing Gravity* (M. Edwards (ed.), Montreal, Apeiron, 2002). In history of astronomy, his work includes *The History and Practice of Ancient Astronomy* (New York, Oxford University Press, 1998). A second edition of *History and Practice*, translated into French by Alain Segonds, is shortly to appear (Paris, Les Belles Letteres, 2005). Evans' most recent work, co-authored with J. Lennart Berggen, is *Geminos's Introduction to the Phenomena: A Translation of and Commentary on an Ancient Greek Manual of Astronomy* (forthcoming from Princeton University Press). He is Associate Editor of the *Journal for the History of Astronomy*.

Alastair G. Gunn was born in Surrey (England) in 1967. He holds a B.Sc. (Hons) degree in astrophysics from Cardiff University (Wales), an M.Sc. from the University of Manchester (Jodrell Bank) and a Ph.D. from Queen's University of Belfast (Armagh Observatory). He is currently a research astronomer at Jodrell Bank Observatory (University of Manchester). He is a member of the IAU Inter-Divisional Working Group on Historic Radio Astronomy and serves on the RAS Membership Committee. His research interests include the radio environments of chromospherically-active binary stars, particular RS CVns and Algols, radio properties of pre-Main Sequence objects, and the history of radio astronomy in the UK. As well as publishing extensively in peer-reviewed astronomy journals, he has written on astronomy for regional, national and international press throughout the world.

Richard A. Jarrell was born in Connersville, Indiana (USA) in 1946. He received his A.B. at Indiana University, Bloomington, after studying astrophysics and ancient history. His M.A. and Ph.D. degrees were taken at the Institute for History and Philosophy of Science and Technology at the University of Toronto. Since 1970 he has taught at York University in Toronto, where he Professor of Natural Science in the Faculty of Science and Engineering and Professor of History in the Faculty of Graduate Studies. A founder of the Canadian Science and Technology Historical Association and its journal, *Scientia Canadensis*, he is also active in several astronomical societies. Author of *The Cold Light of Dawn: a History of Canadian Astronomy* and many articles on Renaissance and twentieth century astronomy, he is currently on the editorial team of the *Biographical Encyclopedia of Astronomy*.

Kenneth I. Kellermann is a Senior Scientist at the National Radio Astronomy Observatory where he works on the study of radio galaxies, quasars and cosmology, and on the development of new instrumentation for radio astronomy. He also holds an appointment as a Research Professor at the University of Virginia and as an Outside Scientific member of the German Max Planck Society. He received his S.B. degree in physics from M.I.T. in 1959 and his Ph.D. in physics and astronomy from Caltech in 1963. Following his Ph.D. he spent two years at the CSIRO Radiophysics Laboratory in Australia. Since 1965 he has been at NRAO except for extended leaves at Caltech as a Sherman Fairchild Distinguished Visitor, and in the Netherlands, Australia, and Germany. He has served as the Assistant Director at NRAO and Director at the Max Planck Institute fur Radio Astronomy in Bonn, Germany. He is a member of the U.S. National Academy of Sciences, a Foreign Member of the Russian Academy of

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Wayne Orchiston was born in Auckland (New Zealand) in 1943, and has B.A. Honours and Ph.D. degrees from the University of Sydney. He is a Research Associate at the Anglo-Australian Observatory, and until recently was Archivist and Historian at the CSIRO's Australia Telescope National Facility. A former Secretary of IAU Commission 41 (History of Astronomy), he is on the Committees of the IAU Astronomical Archives, Historical Instruments and Transits of Venus Working Groups, and is Chair of the Working Group on Historic Radio Astronomy. In addition to Commission 41, he is a member of Commissions 40 (Radio Astronomy) and 46 (Astronomy Education and Development). His research interests lie in astronomical history, astronomical education, meteoritics and radio properties of active chromospheric stars. He has numerous publications, including the books *Nautical Astronomy in New Zealand. The Voyages of James Cook* (1998), *Astronomical Instruments and Archives From the Asia-Pacific Region* (2003, co-edited with R. Stephenson, S. Débarbat and I-S. Nha), *The Early Development of Australian Radio Astronomy* (forthcoming, co-authored with W. Sullivan and J. Chapman) and *Proceedings of the Fifth International Conference on Oriental Astronomy* (forthcoming, co-edited with K.-Y. Chen, B. Soonthornthum and R. Strom). Since its founding, he has been Papers Editor of the *Journal of Astronomical History and Heritage*.

Bruce Slee was born in Adelaide, South Australia, in 1924. After serving in the radar section of the Royal Australian Air Force from 1942 to 1946, he joined the CSIRO Division of Radiophysics in 1946 and, except for a two-year stint at the Royal Radar Establishment in England, has been involved in the radio astronomy research of that Division and its successor, the Australia Telescope National Facility, right up to the present time. He obtained Diplomas in Radio Engineering and in Physics from the Sydney Technical College, followed by a B.Sc. (Honours) degree in physics from the University of NSW, culminating in the award of a D.Sc. from that University. He is a member of IAU Commissions 40 and 41, and a Fellow of the Astronomical Society of Australia. His participation in some 160 published papers has involved research in fields as diverse as the solar corona and clusters of galaxies. His present research interests include active stars, galaxy clusters and the history of radio astronomy.

Robert W. Smith was born in Leicester (England) in 1952. He secured his B.Sc in physics from the University of London. He then moved to the University of Cambridge, completing Part III of the mathematics tripos in 1975 and a Ph.D in the Department of the History and Philosophy of Science in 1979. He is currently a Professor of History in the Department of History and Classics at the University of Alberta. In 1989, he was awarded the Pollack Prize in the History of Astronomy, and in 1990 he was the winner of the History of Science Society's Watson Davis Prize. His research interests lie chiefly in the history of nineteenth and twentieth century astronomy and cosmology, on which he has published extensively. He wrote *The Expanding Universe: Astronomy's 'Great Debate' 1900-1931* (1982). This was translated into Spanish in 1993. He is the author of *The Space Telescope: A Study of NASA, Science, Technology, and Politics* (1989, with contributions by Paul Hanle, Robert Kargon, and Joseph Tatarewicz). A paperback version of this book, with an additional 10,000 word chapter, was published in 1993. In 2000, he co-edited *Reconsidering Sputnik: Forty Years Since the Soviet Satellite* (2000, with R. Launius and J. Logsdon). Most recently, he co-authored with David DeVorkin *The Hubble Space Telescope: Imaging the Universe* (2004).

Richard Strom was born in New York City (USA) in 1944. He holds a B.A. in physics from Tufts University, and M.Sc. and Ph.D. degrees in radio astronomy from the University of Manchester (Jodrell Bank), UK. He is currently a Senior Research Astronomer with STRON (The Netherlands Foundation for Research in Astronomy) in Dwingeloo, and an Adjunct Professor at the University of Amsterdam. He is a past Secretary and member of the Organising Committee of IAU Commission 40 (Radio Astronomy), and has been on the Scientific Organising Committees of several conferences (including IAU Colloquium 182, of which he was co-Chair). He is a member of IAU Commissions 28 (Galaxies), 34 (Interstellar Matter), 40 and 41 (History of Astronomy), and has been on a number of time allocation panels for radio, infrared and X-ray telescopes. His research interests include young supernova remnants, pulsars, large radio galaxies, radio polarimetry and interferometry, new telescopes, the history of radio astronomy, and Chinese historic astronomical records.

Woodruff Sullivan holds a Bachelor of Science in physics (Massachusetts Institute of Technology, 1966) and a Ph.D. in astronomy (University of Maryland, 1971). After a two-year postdoctoral position at the University of

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Virginia Trimble is a native Californian and a graduate of Hollywood High School, the University of California (B.A. 1964), and the California Institute of Technology (Ph.D. 1968 in astronomy). For thirty years, she oscillated between the University of Maryland (where her late husband, Joseph Weber, was tenured) and the University of California, Irvine (where she is tenured), but is for the moment back on fault-laden soil permanently. Her current research interests include the structure and evolution of stars, galaxies, and the Universe and of the communities of scientists who study them. She is Chair of the Commission on Astrophysics of the International Union of Pure and Applied Physics and President of the Division of Union-Wide Activities of the International Astronomical Union, and holds a variety of other positions in professional organizations of very little power but considerable temporal absorptive capacity.

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