HAVE GEN, WILL TRAVEL

IMPERFECT IMAGES FROM THE LIFE OF A RADIO ASTRONOMER

J.A. (JIM) ROBERTS

<u>To Readers of this July 2002 Draft</u>

The origins of this strange concoction of travelogue and science are explained in the Preface.

Now I am wondering if it is worth publishing. Would there be a sufficient audience interested in such a hotch-potch of attempts to explain some of the physics that I encountered in my career as a radio astronomer mixed in with simple stories of my travels?

Having come this far I now plan to spend my time collecting possible illustrations. In the meantime I have had these few copies printed with the hope that while I collect pictures you might look at some parts of the screed and make general comments. I would be most grateful for suggestions or criticisms of any nature.

Obviously one aspect that concerns me is the contrast between the parts that are just travelogue and the parts where I attempt to describe the research that we undertook. Would a reader be interested in both parts, or are they just so different that they do not belong together? But you might see much bigger problems elsewhere.

At this stage suggestions & criticisms of a general nature, rather than detailed editorial comments, would be the most useful. However any criticisms of any nature would be most welcome. And don't hesitate to be critical. I have a thick skin and it is only <u>critical</u> comment that will improve the product.

Thanks,

Jim Roberts
73 Ryan St
Lilyfield NSW 2040
(02) 9810 8919

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J.A. (JIM) ROBERTS

DRAFT FOR COMMENT

JULY, 2002

With thanks to those who made it possible:

My parents, teachers at all levels, employers, colleagues, friends

and

a society prepared to invest in understanding the world around us.

Preface

I often tell yarns about happenings in my life as a radio astronomer, and thought I should record these stories. But for the stories to make sense a reader needs to know quite a lot about radio astronomy, and about the course of my life. So, at the rate of a few hours per week, I began writing this account of my life in radio astronomy. The result is a far cry from my original intention: the yarns are lost in the bush of detail and it has become a superficial autobiography. About thirty percent is just travelogue; another thirty or forty percent is an account of the radio astronomy research in which I was involved, along with an explanation of the physics underlying this research; the rest of the book just tries to tie all this together.

Writing presumes a reader. Who might read these jottings? Who would find anything of interest? Only readers with some fondness for the physical sciences will be interested in the sections where I have attempted to explain physical concepts. These parts are not designed for someone with no knowledge of physics, but rather for someone with an interest in physics, but without specialised knowledge of the way that electromagnetic waves are produced and travel. Others who might find something of interest could include those wishing to learn something of the ways of scientific research, or those interested in the history of radio astronomy in Australia.

Why the title? The title of one of Sir Bernard Lovell's books, *Astronomer by Accident*, would have been a very appropriate title for these musings. I don't share that intense fascination for the heavens that typifies the amateur astronomer. I called myself a physicist, not a radio astronomer. My interest was in the way in which cosmic objects generate radio waves, and the effects of the intervening space on these waves as they travel to the Earth. I always wished that the objects we studied were here in the laboratory rather than out in space. Then we could have *experimented* on them; a far more powerful method of study than just *observing* them from afar! For that reason much of my research was concerned with the study of *variable* cosmic radio sources. If you can't experiment on them, then second best is that *they* change, and you observe what happens as they change.

A Fortunate Life, another title already in use, would also have been very appropriate. I am sure you will agree that it describes my life more accurately than Albert Facey's! I have enjoyed great opportunities, and have been spared the tragedies and traumas which are the lot of many.

The chosen title is meant to convey the notion that, in an age when travel was not as universal as it is now, my working knowledge of radio astronomy made it easy for me to travel and work abroad. The sub-title is a reminder of the imperfections of memory, while at the same time alluding to a central problem of radio astronomy: making *images* of radio emitting objects. Radio cameras don't exist. Radio images are formed indirectly, and from incomplete data. Much of the success of radio astronomy derives from the development of methods for coping with this problem, developments which have been a highlight of radio astronomy since its inception. Many of these techniques are now adopted in optical astronomy.

This is a personal account. I have attempted to be accurate, but in truth these are just incomplete memories and I make no claim to absolute accuracy. Ensuring such accuracy would require more time and effort than I am prepared to invest. Memory is notoriously unreliable, but nor can one simply rely on accounts written at the time of the events discussed. Those reports are necessarily written from a certain point of view, by authors who are normally not in possession of all the facts. Indeed in some cases, regrettably, those accounts are purposely slanted so as to bolster the point of view, or ego, of the author. So take my account as an attempt to recall what happened, not as a deeply researched historical document. I just hope that any unintentional departures from the historical truth will be unimportant. Should anyone feel offended by any aspect of my account I apologise.

J.A.R.
17 July 2002

SECOND HALF SHIFT

'I wonder if they have eaten all the sandwiches?'

Max broke our silence, returning our thoughts to earthly matters. As we walked from the observers' quarters to the radio telescope we had been admiring the carpet of stars above us. The avenue of trees sheltered us from the few lights on the telescope, and our eyes had become dark adapted. Now the sky was a magnificent sight. So many stars that it needed a second look to isolate the Southern Cross amongst the myriad stars of the Milky Way. Such a contrast to the city sky, where only the Cross and a few other stars are visible. Our senses were lulled by the sound of the wind sighing through the casuarinas and the low hum reaching us from the motors of the telescope. Such peace, such a relief from the stress of these 'dog watch' observing sessions. And to top it off, over in the east Jupiter shone like a brilliant jewel. What a marvellous camouflage for its true nature - a body 1,000 times as massive as the earth, surrounded by belts of lethally speeding electrons, spiralling in the planet's strong magnetic field, and emitting the radio waves that we had come to study.

As we emerged from the trees we were confronted by the full face of the dish. Frank and John had the dish tipped over to catch a radio source at the moment that it rose above the limit of the telescope's range. The lower edge of the dish almost touched the ground. Above us and to either side the dish curved away; 64 metres - 210 feet - in diameter. One leg of the tripod structure that supports the focus-cabin rose from the lower edge of the dish to tower above us, where it was met by the other legs sloping down from the far edges of the dish. And there the red aircraft-warning light glowed atop the cabin. It was this view of the telescope, but of course in daytime, that was offered to VIP's visiting Parkes. Then they were invited to ride on the edge of the dish, and be lifted far above the earth as the dish was driven upwards.

As we entered the tower structure that supports the telescope I was wondering: 'Will Frank and John finish on time, or will they want "just another few minutes to finish this calibration"'. It would take us precious minutes to drive the telescope all the way back up to the zenith so that I could climb to the focus to make the changes needed for our observations. As we reached the first floor we were greeted by an aroma of coffee and toasted sandwiches, but there was no one there. On the second floor, in the control room, we found John bent over the chart-recorder, writing notes on the chart as the pen moved to the left, and then back to the right, recording the emission from a radio source being carried through the beam of the telescope by the rotation of the earth. Racks of equipment filled the space to John's right. To his left, the control desk, with its impressive array of dials and changing numbers, followed round the curve of the room for a quarter of a circle. Cliff sat in front of the desk in the grandiose high-backed driver's chair that we joked about. Behind the desk Frank stood looking out through the windows at the network of steel tubing - the underside of the tipped dish.

'Had a good night? Any problems?'

'Not bad. No wind problems. The receiver played up at one point and Frank had to twiddle the L.O. power, but since then it's been OK.'

We were saved from asking when they would finish when Cliff asked Frank: 'Will this be your last source? Max and Jim will want the dish stowed to make the changes at the focus.' So there would be no waiting to take over the telescope, and I could start up the spiral staircase in the centre of the control room, to be ready in the 'hub room' when the dish had been raised far enough for me to venture out onto the walkway beneath the surface of the dish: a walkway that leads to the foot of that tripod leg that we had seen at ground level. But for safety, first switch on the outside lights; lights that turn the telescope into a fairyland and attract romantic couples to come and park - a disaster in the cooler months when they leave the car's motor running for warmth and cause radio interference. Important that I switch the lights off again as I return!

A trapdoor at the end of the under-dish walkway gave access to the dish surface, and to the little lift in which I crouched as it carried me up the tripod leg to the focus cabin. There, on the gridded floor beneath the cabin, 25 metres above the dish surface and almost 60 metres above the ground, it took a few minutes to change the 'feed' antenna to suit our requirements. Then it was back down in the lift, along the walkway and into the hub, to tell the control room that I was clear, and that it was OK to tip the dish again.

Meanwhile, in the control room Sid, the driver for the second-half of the night, had taken over from Cliff. He now started tipping the dish over to intercept Jupiter as it rose above the zenith-angle limit of the telescope. Frank and John were on their way back to the quarters, and Max was organizing things for our first observation, a check of the accuracy of the control-desk 'readouts' that show where the dish is pointing.

Since Jupiter is a planet - a word that means *wanderer* - Max had consulted the astronomers' bible, the *Nautical Almanac and Astronomical Ephemeris,* to find Jupiter's position for that day. Supplied with the *right ascension* and *declination* of Jupiter - its longitude and latitude on the sky - Sid had then taken only a minute or two to set the *master equatorial* guider to this position. Now he was concentrating on driving the 1,000 ton radio telescope, controlling the speed of its tipping in *zenith angle*, and rotation in *azimuth*, so as to bring the dish to point in the same direction as the guider. When the model on the control desk showed the two pointing in the same direction within a degree or so, it was a matter of pushing the *auto-lock* button to have the servo-control take over, and align the radio telescope to the guider. From then on, while ever the guider was moved only slowly - as in tracking Jupiter against the rotation of the earth - the radio telescope would follow faithfully. Only rapid movement of the guider would break the lock.

With the radio telescope slaved to follow, Sid now drove the guider slowly in declination past the expected position of Jupiter, and then back again. Meanwhile we annotated the chart as the pen moved in response to the radio waves received from Jupiter; radio waves that had travelled 600 million kilometres before encountering the surface of the dish and being reflected back to a focus in the *feed* antenna. As each 5 arc minutes of declination was passed the control-desk readouts triggered a marker on the chart. We labelled every second or third of these markers, and after a forward and reverse scan quickly analysed the record to determine the actual declination dial-setting needed to receive the maximum signal from the planet. Sid then set the telescope to this declination and made a similar set of scans in right ascension. With these analysed we knew the settings for both right ascension and declination that ensured the telescope pointed directly at Jupiter. To measure the *polarization* of the radiation from Jupiter we would be making measurements with the feed antenna at the focus rotated on its axis to different orientations. To confirm that rotating the feed didn't alter the pointing, we next repeated the pointing tests with the feed rotated by 90 degrees.

Now we entered the routine we would follow for the next six hours while Jupiter stayed above the 'horizon' of the telescope. First the telescope was pointed several beamwidths away from Jupiter to record the background level of radiation, then directly at Jupiter, then away from Jupiter on the other side. Next the feed antenna at the focus was rotated through 30 degrees, and the process repeated, and so on. A set of six such 'on-offs' at 30 degree intervals of feed-rotation provided one measurement of the linear polarization and the total intensity. Each such set was followed by a 'noise calibration', when the power response of the receiver was checked by recording the signal from a standard 'noise tube' connected to the receiver input. There were just occasional breaks from this sequence when we rechecked the pointing, or checked the noise calibration signal against one of the well-studied radio sources used as standards of intensity.

Ten years later this whole routine would be performed automatically under computer control. The observer would simply entered the nominal position of the source to be studied and then the telescope would make the necessary observations and calculations to determine the true pointing. Using the measured position it would then do the set of six 'on-offs' and feed rotations, and record the noise calibration. Throughout, the receiver output would be digitally recorded on magnetic tape for final analysis off-line back at the headquarters in Epping. However tonight our only assistance was from a sequencing box that Doug had devised. A telephone uniselector lit lights to tell the driver when to drive the telescope, and in which direction, or indicated to the observer that the feed should be rotated or that a noise calibration was required. It was a demanding night for the driver who had to drive the telescope on- or off-source every minute, and sometimes had to remind us to rotate the feed or switch on the noise source when we forgot. Since driving was an overtime job for technicians who worked on the telescope by day, you can imagine that our observing sessions weren't as popular as other programmes in which one radio source was tracked during long integrations!

While either Max or I took charge of the observations, and annotated the chart record, the other busied himself analysing the charts from previous nights. Radio signals from astronomical sources are 'noise signals', and fluctuate randomly. To reduce the size of these fluctuations a 'time constant' averaged the output of the radio receiver for a few seconds, but there were still residual fluctuations. Consequently, even with the telescope tracking a fixed point on the sky the line drawn on the chart was a wobbly line, not a straight line. The analyst had first to draw an average line through these fluctuations. He then used a millimetre scale to measure the height of the pen deflection produced by Jupiter, and converted this millimetre-measurement to units of flux density using the noise calibrations, and the observations of the standard sources. Each set of measurements at the six position-angle settings of the feed was then plotted, and a sine wave fitted through the points. The sum of the maximum and the minimum of this sine wave gave the total flux density, the difference between these levels gave the linearly polarized flux density, while the angle at which the sine wave reached its maximum gave the position angle of the linear polarziation.

We were studying the changes in Jupiter's radio emission as the planet rotated. Jupiter takes almost ten hours to complete a rotation, but was above the 'horizon' of the telescope for only about six hours each night. So to record the radio emission from every face of Jupiter we had to make observations on a number of nights. The incredible weakness of the radio signals received from space provided another reason for observing on several nights. The radio power from Jupiter that was collected by the 64 metre diameter Parkes dish and fed to the receiver was a thousand, million, million times less than the power in the light of a torch bulb! By combining observations made on different nights, but at times when Jupiter had the *same* face towards the earth, we improved the accuracy of our measurements.

Periodically during the night someone asked: 'Who wants tea and who wants coffee' and then disappeared to the floor below while the other two carried on the observations. On some of these breaks the smell of toasted sandwiches wafted up the stairs and prompted a second person to visit the floor below. Then around 6:30 in the morning Sid gave us the call that we were about to lose Jupiter beyond the zenith-angle range of the telescope. There was just time for a last set of Jupiter observations before the telescope would hit the limit switch. Then after Jupiter had set we had an hour before the day staff would arrive, which left time to record two of the standard radio sources used for calibrating the scale of intensity, and then to stow the dish and check the zero of the polarization angles. Checking this zero required another trip up to the dish, but this time it was up through the vertex room above the hub room, and out onto the surface of the dish through a trapdoor in its centre. Here I set up a linearly-polarized horn antenna, aligning its edges with the radial rib in the dish that stays in the vertical plane as the dish is tipped. The signal radiated by this horn would be polarized in the vertical plane. Measuring this signal using our standard observing method allowed us to check for any zero error in the polarization angles that we had measured.

By the time the day staff arrived at 8 o'clock, anxious to hear an account of any overnight problems, and to take over the dish for routine maintenance and improvements, we were packing up, ready to return to the residence for breakfast. After that we hoped for some sleep, but knew we would probably be up soon after lunch to analyse more records. However with any luck, during the afternoon we'd fit in a bike ride on one of the local dirt roads, or a game of ping pong with Frank and John. After dinner at 5 o'clock we'd catch another nap while the others were at the telescope, and then front up again at midnight.

BEGINNINGS

The ringing of Mother's alarm clock at 6 am was the signal for 'action stations'. There were eight children, and although several had flown the nest by my youth, we usually had at least one country cousin or nephew in replacement. All needed access to the bathroom, breakfasts prepared, and lunches cut. It was an organised household with jobs for all: cleaning the shoes, setting the table, helping cut the lunches, washing up, working in the garden, and doing the 'messages', i.e. going to the butchers and the greengrocers. I don't recall a lot of time for play. In the afternoons, and in the evenings, we were expected to do our school 'homework'.

Having grown up on the goldfields in a family scratching for a living, having lived through the depression of the 1890's, and now seeing the effects of the 1930's depression, my Father was determined that we children should get a good education. He saw this as the key to our securing a good job. And it was Mother who saw to it that his desires bore fruit. Six of the eight children were supported through teachers' college or university, all on Dad's salary as a teacher. Certainly a testimony to my parents' dedication and careful management of their resources.

Obviously money was tight. However we weren't poor. As headmaster of the local state primary school my Father had a tenured job - a godsend in the Depression - and Mother was a good manager. She made many of our clothes, including shirts for the boys. We grew a lot of our own vegetables. My parents never owned a car, and we certainly didn't go to the 'flicks' on a Saturdee 'arvo, as many kids did. But we never thought of ourselves as lacking. If others spent money in ways which we did not (because we didn't have it), we children were given to understand that those other people were wasteful. 'Wicked waste makes woeful want' was one of the many aphorisms instilled into us by Mother.

As my parents had spent their childhood years on the Hill End goldfields, and much of their later lives in country towns, they had developed the careful attitude of those who have known the severe shortages of country life. If you have lived on tank water in western NSW you don't waste water! That feeling of guilt that comes over me when I let a hot-water tap run until the hot water appears dates from my childhood. We were conservationists long before the word became popular. In that Sydney suburban home of my childhood all the household food scraps were buried in the garden to help improve the soil, and the lawn cuttings were used to mulch the topsoil: practices commonly promoted nowadays by conservation bodies, or even by local councils, but pretty uncommon in those days.

Our Burwood home is still there, in Shaftesbury Road, down near the bus depot. Then, and still today, that's, 'on the wrong side of the tracks'. But times must have changed. That Federation house, with its huge rooms, high ceilings, cedar doors, fireplaces with marble mantle-shelves in many of the bedrooms, bay windows at the front, and a steeply sloping slate roof, was said to have been built for the Beard Watsons. It looks quite naked these days. The thick hedge along the front fence, the big magnolia tree and the tall palm tree are all gone. And painted white, instead of cream, it looks more stark than when we lived there.

The rooms at the back of the house with their galvanised iron roofs - the kitchen, laundry, 'breakfast room', pantry, maid's room (we assumed), bathroom, and outside toilet - were arranged in two wings extending out from each side of the house. The area between the wings was roofed over, had a bitumen floor, and was enclosed with lattice. Originally it must have been a conservatory. For us it was the bedroom for half the family, with Father and Mother in the big bed, then Brian and I in fold-up beds, and Helen in a third fold-up round the corner. The rest of the family slept in the bedrooms in the main part of the house. This 'conservatory' was not really waterproof. Dad had installed canvas roller-blinds to stop most of the rain from coming through the lattice. However in a thunderstorm water rushed down the gutter above the hall in the main part of the house, and cascaded down into the conservatory, prompting a mad scramble to fold up the beds, and to rescue all the *Gould League* material that was stored on the 'landing'. For you see for many years my Father was the secretary of the Gould League of Bird Lovers, an organization that encouraged school children to appreciate and protect wild birds. Many packages of Gould League membership cards and Gould League magazines were stacked on that landing.

I remember occasional family outings and picnics, when we played rounders and French cricket, but most of our leisure time activities revolved around the church. My Father was raised as a Roman Catholic and my Mother in the Church of England. I am told that as a result of the marriage my Mother was disowned by her Father, and my Father was disowned by his church. I cannot remember my Father speaking of religion, except in the broadest of terms, until near the end of his life. But my Mother sent us to the local Church of England, which she occasionally attended herself. For the three youngest children, and for our cousin Benedicta who lived with us, St Pauls, with its good youth clubs and young active ministers, became the social centre of our lives. And it was there that I met a life-long friend, Lachlan (Lachy) Harper.

Unlike most churches of the Sydney diocese St Pauls was 'high church'. The pomp and drama of the ritual, with processions on feast days accompanied by magnificent music, appealed to Lachy, as it did to me. Lach was a born actor, and even if I was a duffer in that area, I do enjoy a good spectacle. But don't get me wrong. We were devout believers. We were both 'servers' at the altar and ardently discussed points of faith with the assistant priest, Ian Shevill, an extremely intelligent and talented young man who later became a bishop. He had a flair for appealing to young people. He ran the youth clubs in great style; he produced plays that were performed with much pageantry at Christmas, indeed he wrote some of the plays himself; he performed conjuring tricks in the pulpit - a great way to get our attention so as to make a point during a sermon.

Ian Shevill was ever willing to discuss problems of belief with members of the youth clubs. However the Christian Endeavour group that I associated with at high school were of the 'fundamentalist' persuasion and required a literal belief in the Bible. My association with this group tapered off after I spent a week-end with them at Stanwell Tops, south of Sydney. On a walk in the nearby bush we found ourselves in the midst of some blackberry bushes, and unable to see our way through. The leader had us pray that we be shown a way out, and the group believed that divine guidance led us through. The budding scientist couldn't accept that.

As the years progressed both Lach and I came to have problems in accepting the teachings of the church. Strangely, it was one of the books that Ian Shevill suggested that we read to help resolve our doubts that crystallised some of my *dis*belief. This was *Miracles* by C.S.Lewis. The argument in this book depended on the idea that there are *natural* and *supernatural* events. In science I was just learning about 'operational' definitions and so I wanted a definition that would allow me to decide if an event was *natural* or *supernatural*. I couldn't find such a definition, and decided that it made much more sense to put all events in the same class, and to try understand them on a common basis. Over the years my level of belief in religion gradually diminished, and when I left to go to Cambridge I finally took the step of saying I did not believe.

'Early to bed and early to rise' was the rule at our house, but when I'd go up to Lachy's place I'd sometimes find him still in bed at 10 o'clock in the morning. He'd be reading what he called his 'blood and thunder' magazines. They had lurid covers, and titles like 'Amazing' and 'Astounding'. No such magazines ever appeared in our house. Perhaps Lachy acquired them through his Father, who was a journalist with, I think, the Sydney Daily Telegraph. I don't remember ever reading any of those magazines, so I don't know about the blood and thunder. Isaac Asimov relates in his autobiography that those magazines launched him on his writing career, so perhaps I should have read them. But then I have never been attracted to science fiction. Perhaps that is because I have little imagination, but, on the other hand, I have been disappointed by the lack of imagination in the few science-fiction movies that I have seen. The 'creatures from outer space' seem to be grotesque distortions of earthly species. Aren't we capable of imagining something that looks completely different to what we know? And why *grotesque?* Is it that we fear the unknown and assume that anything strange is malevolent? It's interesting that we seem to be able to create *sounds* which are unlike the sounds around us, and some music, at least, is anything but grotesque.

Lach was very good at sports, and could easily outplay me at tennis. He was a bronze medal swimmer. The only physical activity where I came close to him was in gymnastics. When Ian Shevill arranged for Mr Mattingly to act as gym instructor for the church youth club Lach and I vied with each other on the parallel bars and vaulting horse, or competed to see who could somersault over the greatest number of crouching boys. It was great fun. But I suspect that it was Lach's intellect that was the main attraction. Over the years we spent hours arguing over puzzles and brain teasers, and discussing the latest science discoveries. And Lach had a fund of quirky sayings. He would quote from Ogden Nash and Lewis Carrol, and chortle catchy choruses from Gilbert and Sullivan. Lots of his sayings are still with me 50 odd years later.

One of the best of the brain teasers from that era was about the three chaps who wanted accommodation for the night but found that all the pubs in the town were booked out. Finally one publican said there were three beds in his store room and they could stay there for thirty shillings. After they had moved in he felt that he had overcharged them and gave his young son five shillings to return to them. When the lad found that dividing five shillings by three was too hard he solved the problem by giving them back one shilling each and pocketing the remaining two shillings himself. So here's the enigma. Each of the three chaps has now paid nine shillings, giving a total of twenty seven shillings. The lad has another two to make twenty-nine shillings. But they originally paid thirty shillings. Where has the other shilling gone? You can set people arguing for hours with that story.

And it would have been Lach who told me the story about the drunk looking for a sovereign under a street light - a story which decades later I used to illustrate an aspect of scientific research. You will have heard the story:- Late at night a passer-by finds a drunk searching around on the footpath and in the gutter under a street light. Asked what he is looking for the drunk replies 'Shovereign. Shi dropped a shovereign.' After spending a few minutes helping him look for it the passer-by asks 'Are you sure you dropped it here?' 'Shno. Shno, mate, I dropped it over there. But shere's no point looking over there. Shere's no light over there.'

And so it is with much scientific research. Most of the studies undertaken are determined by the facilities available. Some particular piece of information might appear to be the key to advancing a subject, but if the means to find that information are not available then most will settle for using the available equipment to measure something else. Much successful research results simply from being at the right place at the right time when the right piece of equipment happens to be available. It is only occasionally that a strong, deeply committed, assertive, sometimes domineering and aggressive, personality changes the course of development by amassing the resources needed to construct a light out there where it is thought the lost sovereign is to be found.

My friendship with Lachy stemmed from our membership in the church youth club. While we both attended Burwood Primary School, we weren't in the same class, and for the last two years of primary Lach attended a special school for above average pupils at Ashfield. Dad ruled that I should not go to Ashfield and perhaps Lach missed out on something by going. At Burwood we had an excellent teacher, Eric Bailey. He made sure that we learned what we needed to know to get good passes in the 'Primary Final', the public exam which determined which secondary school we could attend. And he used the stick to make sure that we learned. I owe a great deal to Eric Bailey. I did well in school work, and was regarded by the other boys as a 'swot'. However it put me in favour with my parents, who believed very strongly that we should 'better ourselves', and that a good education was the only way to ensure a good job.

I am not sure what started my interest in science. My Mother certainly encouraged an enquiring mind and one of her sayings - 'Never believe anything you are told and only half of what you see' - no doubt underlies my sceptical attitude. According to Gina my favourite word is 'rubbish'! Then I recall quite a large book with a title like 'How it Works' or 'How it's Done' which I was given one Christmas. I enjoyed that book. Whether the book started my interest in things technical or was given to me because I had already shown an interest in such things, I don't know.

I suspect that my Father had a far wider range of interests than I imagined when I was a kid. At some time he had taken an evening course in Geology, so he had some interest in scientific matters. But Dad never succeeded in communicating an enthusiasm to me for anything - except birds, of which more anon. By the time I was 10, my Father was 63, and seemed a stern and rather remote figure. No doubt the fact that he was the headmaster of the school I attended reinforced this separation, though fortunately I was never in my Father's class. Like me, Dad had a rather short fuse, and his attempts to teach me to hold my cricket bat correctly, or to do arithmetic, usually ended in frustration. A caricature of my Father, a gift from a one-time pupil who became a cartoonist with *The Bulletin,* hung in the dining room. It summed-up the way I saw him. There stands the headmaster - Daddy Roberts the school kids called him. Thin and upright in his white coat, with that coarse white moustache typical of men in those photos taken on the goldfields; and the cane is held ready for use, exaggerated by the artist to look like a thick knobbly piece of wood.

What other influences prompted me along the road to science? My brother Norman, twelve years my senior, became a scientist, so his interests must have made some impact. After winning a scholarship to be trained as a teacher, Norman took a degree in Science, majoring in Physics. However he told me recently that he had set out to major in the arts, and took up science only when Jack, our eldest (step) brother, persuaded him that he could handle the maths. Norman in fact never became a teacher. In my earliest memories of him he was working at AWA (Amalgamated Wireless Australasia) while he did an MSc part-time in some branch of what is now called nuclear physics. But these memories are of later, during the war, when I was completing high school or in my early years at university.

Of course in those immediate pre-war, and wartime years there was a general interest in technical advances. Amongst us lads they were a great topic of conversation. And Lach would pass on snippets of science and technology gleaned from his 'blood and thunder' magazines. In later years I was certainly influenced by Lach's developing passion for science. All in all I suspect that Lach Harper was the greatest early influence leading me towards science.

Fort Street Boys' High School was the top local high school and it was assumed that all the good students from Burwood Primary would go there. But somehow, probably from Lachy Harper, I had heard of Sydney Technical High School, and surprised my parents by saying I wanted to go there. At this time high schools pursued a classical education. Technical subjects were taught in the junior technical schools, which were schools for students not wishing, or, for one reason or another not able, to complete full secondary schooling. Sydney Tech High had been the first, and was then one of only two high schools in the state, to include subjects like woodwork, metalwork & technical drawing in the curriculum in the early years, and branches of mathematical physics like mechanics in later years. When my Father investigated and found that Tech High had the highest entry requirement of any high school in the state he decided it must be OK and he allowed me go there.

One of the great things about Tech High was the absence of Latin. Before I started high school this dreadful omission was not known to my family, and I have memories of my sister Helen, acting on instructions from Dad, attempting to interest me in Latin in the school holidays before high school. I recall that she tried teaching me Popeye the Sailorman in Latin - you'd have to give her an A for trying! While I might have avoided Latin, I couldn't escape foreign languages completely. University entrance required a pass in at least one foreign language, and to meet this requirement Tech High offered a choice of French or German. I chose German (which was said to be easier) and struggled through to achieve a B grade in the Leaving Certificate. Lachy also went to Tech High, but since he chose French rather than German, we were in different classes; our main contact continued to be through the church groups. In all except German, (and History, a non-examination subject taught by a teacher whom I strongly disliked) I did well at Tech High, occasionally dislodging Doug Armitage from the top place.

The science taught at Tech High was not very modern - we learned nothing about atoms! We must have learned the basics of classical physics, but many of my memories of high school science revolve around experiments made at home. *Experio* - I learn. I believe that is what the science teacher taught us. Certainly try and fail figured largely in my experiments with electromagnets. They are made by winding insulated copper wire around an iron core and passing an electric current through the wire. As my first effort I wrapped just a few turns of *bare* copper wire around some nails and plugged it in to the power point at home. My device had essentially no resistance and tried to draw an enormous current. At least by then I knew how to replace the house fuse!

My parents must have been *very* understanding. Some other 'experiments' that I recall inflicting on my parents at home included the usual ones of making 'rotten egg gas', and blowing glass tubing to produce Cartesian divers. And I rigged the vacuum cleaner in reverse to supply air to a gas blowpipe, which I then used to melt lead and aluminium. The lead was used to make solid disks to be threaded on double strings and set rotating - lots of mass, so lots of momentum and far better than the lollies they sold for the purpose. The aluminium was cast into a doughnut shape to be levitated by electromagnetic induction, but that must have been in university rather than high school days.

In our first year class at Tech High the expert on all things electrical - spark coils, buzzers, bells, solenoids - was one Percy Naylor. Not only did I learn from Percy how to make these devices, he also introduced me to the shop near Central Railway where you bought bits and pieces for their construction. This was a dusty old shop full of the remnants of old telephones, magnetos, solenoid controllers, etc. And it was run by a very peculiar old man; in retrospect I suppose he had a goitre, a large protuberance hung in front of his throat. I'm sure I never told my parents about this shop, nor about the old man - I just sneaked in there as I walked from school (which was in an old primary school building in Paddington) down to Central Railway, to catch the train home. Nor would I have told my parents much about Percy who was quite a character and often in trouble with the authorities. I've often wondered what happened to him in later life. While he was my 'tutor' on electrical matters, Percy wasn't a 'friend'. Indeed it was he who gave me that hated nickname, Rene. In any plays that were performed in this all-male school the girls' parts were necessarily played by boys. It fell to my lot to be Rene, and to suffer stage-fright, and completely muff my performance. I hated that nickname.

My main memories of school science classes are of the treatment that the science teacher metered out to the announcements sent round by the music teacher. These announcements would be along the lines of 'All members of the choir are reminded to bring their regalia' and would be translated by 'Mary' Luke, the science teacher, as 'All angels bring their nightshirts'. Our first contact with this music teacher that Mary Luke liked to rubbish - Mr Bellhouse, alias Ding Dong - was when he marched us down the stairs to the music room with the promise: 'If I hear a sound from anyone I'll rip his guts out and smear them over the wall.' On another memorable occasion the Headmaster, accompanied by an inspector, entered the music room to find Harry Savage, (who happened to be the son of the Modern Languages master), squatting on the floor picking up scraps of paper. For talking in class Harry had been given *five hundred lines*, and when he had handed them in Ding Dong had torn them up, thrown them on the floor, and told Harry to pick them up. It was an ironic scene with Harry scrabbling around on the floor beneath the honour boards which adorned the wall of the music room, boards inscribed in gold with the names of those from earlier years who had earned the title 'Dux of the School'. An embarrassed Mr McKinnon suggested 'You had better go and get a dustpan and brush to clean that up, lad.'

And yet it was this Ding Dong who first gave me a taste for classical music. In the corner of the music room there was a record player that produced sounds quite unlike those that emerged from the prized, wind-up, 'acoustic' HMV Salonola which stood, rarely used, in our lounge room at home. Orchestral music was transformed. I recall Ding Dong teaching us:

Poppa Haydn wrote this tune,
And a chord is coming soon,
It will be a big surprise
Open sleepy eyes, Pom!

The Surprise Symphony. Another was:

Once there was a little breeze
That wandered idly through the trees
In search of someone fond of play...

Dvorak's *Humeresque.*

Another teacher with a rather obvious nickname, 'Snoz' Lowe, 'learned' us our 'guzinters' in first year. He also left impressions - though I don't recall receiving any of those 'impressions' myself. If someone was not paying attention, Snoz Lowe's arm would retract, and zing!, the chalk he was using would hit the offending party with unerring accuracy. But when he actually intended to inflict punishment, he would intone in his slow, drawling voice 'Come here, son. Lean over, son', and the offender would lean over, putting his face down towards the front desk. Then might follow 'Take it out, son', referring to a book the offender had placed inside his pants to ward off the expected blows from a ruler.

'Treadle man, treadle' is another Tech High memory. This was the second year metalwork teacher exhorting us to keep the lathe turning. I managed the treadling, but wound the cutting tool in too rapidly, and broke the tip off the tool. That upset the teacher, and hence the student, to such an extent that my metal-lathe work was not a success. I managed better with the wood-turning lathe, but I completed only one pawn of the chess set that I began making! Another exhortation of the metalwork teacher concerned filing. The thumb had to be placed on *top* of the file handle. According to a story he liked to tell (which I found hard to believe), when one of his ex-students started his first job the foreman came running over to congratulate him on the way he held the file, and to ask him who had taught him.

Tech High had very little playground space, and all of it was bitumen. As a safety measure running in the playground was forbidden. Perhaps as some compensation for the restrictions of the playground the school had a unique way of honouring Empire Day, which was celebrated on Queen Victoria's birthday, 24th May. On this day most schools held some form of patriotic celebration in the morning, either at the school or a nearby hall or cinema, and declared a half-day holiday in the afternoon. Tech High went *en masse* by train to the (now Royal) National Park on the southern edge of Sydney. From the National Park railway station we marched down to Audley and assembled beside the Hacking River for a short patriotic remembrance. Then we rushed to rent rowboats. As far as we were concerned the general idea of the day was to sink as many of the other boats as possible! I remember using my newly acquired skills in sheet-metal work to construct a pump for the purpose. The raw materials included a piece of four-inch diameter downpipe for the cylinder, and a piece of the leather that Dad kept for repairing shoes, for the a piston. Filled up, and squirted at close range, the pump was quite successful.

That same national park was the locale of more tranquil scenes when my Father took me to stay in the 'Naturalists' Cabin', which was down the hill from Waterfall. He must have had access to the cabin through his work with the Gould League of Bird Lovers. It was magic to be allowed to wander on my own in the bush near the cabin in the early mornings. On several occasions I saw a lyre bird perform, displaying his tail and stamping his feet while calling; a performance that I have seen only a few times since. I also have memories of Merv Graham, one of Dad's Gould League colleagues, setting up a large (half plate?) camera, facing a stump, on which food was placed. Butter on this stump would attract the glorious green cat bird. Mervyn, sitting on the verandah of the cabin, would pull a string attached to the shutter release, to capture the image in glorious black and white!

The Second World War formed the backdrop to my high school and early university years. The war began when I was in first year at high school, and ended when I was in second year at university In many ways I was shielded from the effects of the war. My Father retired at age 65 just before the outbreak of the war. He became an air raid warden, and we dug a trench in the back yard. A brother-in-law joined the army and joked that, because he was a carpenter by trade, they put him in the butchery! Fortunately he was never sent out of Australia, but he was in Darwin during the first Japanese air raids. Eric Bailey, the teacher who had done so much for me at primary school, newly married, volunteered and was sent to the Far East (as it was called). He was captured and imprisoned in Changi (Singapore). Eric survived, and in later years used to visit my Father and Mother. I never heard him speak of what must have been terrible times in Changi. Indeed it was really only with the 50th anniversary remembrance of the ending of the war that I came in any way to appreciate the sacrifice that he, and so many others, made at that time.

Yes, we children were certainly very much shielded from the horrors of the war. Some things were rationed, which made it difficult for Mother, but had little effect on us. My main memory is of a fascination with the technical side of war; radar and aircraft were much discussed. And as a strange side-effect of the war we scored a far better maths teacher. The deputy headmaster, Bunny Austin, replaced our regular maths teacher who joined up. Our new maths teacher might have impressed us, but the teacher of mechanics fell pretty low in our estimation. At one time he contrived a means of getting an answer to a textbook problem that would agree with the answer in the back of the book, when that answer was clearly wrong. (The question involved weights and pulleys. You were required to find the load on the hook supporting the whole arrangement. Unfortunately the answer in the book was not equal to the total of all the weights supported.)

I didn't succeed in beating Doug Armitage at the final high school exams, so those honour boards that adorned the walls of the music room should show Doug as Dux of the School for 1943. However I did get a good pass in the Leaving Certificate, sufficient to earn me a University Bursary which helped a bit with the family finances. And so, after a longish vacation, mostly spent working on an orchard at Griffith, I enrolled in Engineering at Sydney University, intending to become an aeronautical engineer. I would design something far more streamlined than the Spitfire! One aspect of my choice of engineering was a great relief to my Mother. Engineering was a 'reserved occupation', meaning that, for the sake of the war effort, I would be expected to continue in that occupation during the war, and not to volunteer for one of the services. Did 'they' really expect the war to last another four years or more, or were they already planning 'post-war reconstruction'?

SYDNEY UNIVERSITY

'Six days shalt thou labour' was the rule in first year engineering at Sydney University. From Monday to Friday we had lectures in the mornings, and prac work in the afternoons, then on Saturday mornings we went down to Sydney Tech College for 'workshop practice.' With six days of labour - well five and a half - university wasn't the social experience that it can be for arts students. And, of course, there were no girls in engineering!

Engineering students joined the science students for the three science basics: Maths, Physics and Chemistry. Then, while science students had one additional subject, we had four: Building Construction, Workshop Practice, Engineering Drawing and Design, and Descriptive Geometry. In Building Construction 'Bwickie' taught us to equip window sills with 'dwips' so that dust, accumulating on the sills and then washed off by rain, would not run down the wall. In Workshop Practice there was more filing, though this time I don't remember any command to keep the thumb on top! I do recall something new - blacksmithing - with lots of hammering of red hot steel. Engineering Drawing and Design was taught by Geordie (Professor Sutherland). He was instrumental in my realising that engineering probably wasn't for me. When we had to design a *plummer block* - a housing for a bearing - I asked Geordie how to determine the thickness needed for the base of the block. His reply, 'Make say 5/16 inch', was hardly the exact science of my dreams!

Descriptive Geometry is the name listed on my certificate as the fourth engineering subject, but I can't recall any geometry. The subject dealt with stresses in structures, and their calculation. I failed this subject, which was a severe blow to my pride. Also slightly surprising: a subject of that nature ought to have appealed to a future scientist, after all it wasn't like Geordie's 'make say'. I probably reacted negatively to the *way* it was taught. Fortunately I was allowed a second attempt at a 'post-exam' held during the vacation, and this time managed to pass. Obviously my memories of Descriptive Geometry aren't pleasant, but none-the-less a few gems of knowledge from that course have stayed with me over the years. That the stresses encountered during construction can be much greater than the stresses in a completed structure was graphically demonstrated by photographs of a bridge collapsing during construction. And the dangers of wind-induced resonant-oscillations were illustrated by a movie of the Tacoma Narrows suspension bridge. Wind sent the bridge into torsional oscillations which built up to such a magnitude that the bridge finally tore itself apart.

Another memory from the course actually proved of use later on. When a *square* hole is made in a piece of material there are greater stresses at the corners of the hole than there would be with a *round* hole having a diameter as large as the diagonal of the square. Years later I recalled this when the spacers on the transmission lines at the Dapto solar radio astronomy field station kept breaking. These spacers, which separated the two wires of the transmission lines, were made of 3/16 inch thick perspex. They were about a foot long and had slots near either end which slipped over the wires. The slots had been milled in to the required depth, leaving sharp corners at the bottoms of the slots. When new spacers were made with round-ended slots - made by first drilling a hole through the perspex and then milling the slot up to the hole - the rate of breakage dropped dramatically.

Visits to engineering projects during our first year included a trip to Brooklyn, where the New South Wales Government Railways were building a replacement for the original Hawkesbury River railway bridge. The original had been quite a bridge for its day, built in the 19th century by the Brooklyn Bridge Company, which gave its name to the township. On our visit the engineers building the new bridge contrasted their methods of constructing pylons with the methods being used a mile upstream where the Department of Main Roads were building a road bridge. This road bridge, the first across the Hawkesbury, was to replace Peat's Ferry, a long-time bottleneck on the Pacific Highway between Sydney and Newcastle. Looking back it seems strange that two such large bridge-building projects were being undertaken during the war, but perhaps it was done as a defence measure. The scuttlebutt pedalled by the railway boys was this: The pylons on the road bridge were being made by sinking huge, hollow, steel cylinders into the river bed - cylinders which were later to be filled with concrete. One afternoon, fortunately just *after* the workers had left the job, one cylinder disappeared from view! Obviously the river bottom was pretty soft. The railway boys were very superior - they *knew* what was at the bottom of the river where they were building.

Lach Harper also failed first year engineering, and did not scrape through with a post-exam as I did. He repeated first year and failed again (I think), or was it that he swapped to science and failed that? I think Lach was more talented than I; he certainly had a more enquiring mind. He was absolutely fascinated by much of science. But he hadn't been indoctrinated with the work ethic, and found other pursuits more interesting. After he left uni we still kept a close association through the church youth club, and spent many evenings fooling with radio equipment in the Harpers' garage. (Like us, the Harpers didn't own a car). There was no power supply in the garage so Lach rigged up a lead consisting of a *single* insulated wire which was plugged into a power point in the house. The return path was through the ground! After we had spent the evening playing about with the gear in the garage, Lach's Mother would call to us that the coffee was ready. By then it would be after 10 pm and past my usual bedtime. Nevertheless Lach would insist on a game of chess with the coffee. So it would be *very* late when I mounted my bicycle to ride home. Next morning the question would be: 'What time did you get home last night?'

We spent lots of time trying to make an (illegal) transmitter. I remember once scrounging some heavy copper wire from a radar station near Newcastle so as to make a tuning coil. Then in the immediate post-war years we bought army-surplus radar gear to add to our collection of tubes, coils, tuning capacitors and the like, and then the big project became making an oscilloscope using a 12 inch (huge for that time) magnetic-deflection cathode ray tube that had been a spare tube for a radar set. That project was never completed, but at least we learned something of the problems of generating a linear sweep for magnetic deflection!

First year physics covered only classical subjects such as dynamics, sound, light and heat. These were taught competently, I suppose, but without providing much insight. As I learn more quickly from being shown, than from being told, courses with demonstrations taught me most. I remember the demonstration of Leslie's cube, showing that a black surface emits much more heat than a white surface at the same temperature. This demonstration was to illustrate Kirchhoff's Law, a law of physics that was later to figure so prominently in my PhD studies, as I struggled to understand how the sun generates such intense radio emission.

I might have learned Kirchoff's Law in first year, but it wasn't until after I left Sydney University that I learned simple truths such as the speed of sound in a gas being about equal to the mean thermal speed of the molecules of the gas. And I never achieved any real understanding of thermodynamics, including the derivation of that all-embracing Second Law of Thermodynamics. According to this law, disorder (*entropy*) is, on the whole, always increasing. My lack of understanding of this derivation has meant that I have never completely accepted this sweeping statement. Nor have I accepted the corollary that the universe is running down. No doubt my scepticism is reinforced by the experience that most universal statements are later found to have important exceptions. Certainly we know that there are many parts of the universe where entropy is decreasing.

Apart from the Leslie's cube demonstration, the heat and thermodynamics lecturer made another lasting impression. To try to teach us the essence of a *law of physics* he asked round the class as to who owned a dog. Then he asked these students how many legs their dogs had. Since all our dogs had four legs, he then propounded what we came to call 'Doc Fraser's Dog Law', to whit *dogs have four legs.* The next step was for Doc Fraser to claim that his dog had lost a leg, and had only three legs, and so introduce us to the idea that 'laws' of physics are not absolute truths handed down from on high. Rather they are principles that encapsulate present knowledge. As more evidence accumulates *laws* are likely to be found to be not always true, and to need modification.

There was one outstanding physics course from those early years, *Electromagnetism*, taught by the head of the Physics Department, Professor V.A.Bailey. It was an advanced course, including Maxwell's equations, and was copiously illustrated with impressive demonstrations. These were organized by Dr Landecker, a 'reffo', i.e. a refugee from Nazi Europe. His European degrees were not recognised in Australia, so he wasn't employed as a lecturer, but as the professor's laboratory assistant! Professor Bailey was very lucky indeed to have a 'lab assistant' with the skills of Dr Landecker. (Years later Landecker was employed in a more appropriate position in another Australian university. His son became a radio astronomer.)

Bailey also gave a course on special relativity - probably in our third year. It seemed very esoteric, but I did learn the basics. Then Lach Harper, who had by this time left university, read something about a *betatron*, a new type of machine that accelerated electrons to extremely high speeds. This prompted me to study Kearst's paper on the betatron in the Physical Review. The fascinating thing was that Kearst actually had to take account of the increase of the mass of the electron as the electron's speed increased towards the speed of light. This special theory of relativity had some real application!

Nowadays, fifty odd years later, the relativistic increase of mass with speed is a commonplace in physics; clearly the theory is very close to the truth. However I remain sceptical of the theory's prediction that an *infinite* increase in mass would occur at the speed of light, and hence sceptical of the consequence that nothing can exceed the speed of light - a result that follows since no infinite force is available to push an infinite mass beyond this speed. Infinities in mathematics - the result of dividing by zero - are usually a sign that something has been omitted from the physics, something that would prevent the denominator from becoming zero. I understand that in the 19th century it was believed that nothing could exceed the *speed of sound* in air. Supersonic motion was thought impossible because at the speed of sound the air resistance would become infinite. Once the effects of turbulence were recognised, it was found that while it is hard to 'break the sound barrier', it is possible. I remain amongst that minority who believe that something similar will be found true for the speed of light. I once drafted a 'Letter to Nature' along these lines, but my colleagues at the Radiophysics Laboratory gave me little encouragement, with comments like 'It is more complicated than that' or 'You don't really understand', and as a result the letter was never submitted to the journal.

I found first year maths difficult, particularly pure mathematics which seemed strange and remote. Lachy, on the other hand, was intrigued with number theory, topology, and aspects of higher dimensional geometry. I probably enjoyed polishing specimens of different metals in the metallurgy class more than I did pure maths. A story is told of a later year, concerning a course on *tensors* given by Professor T.G. Room, an abstract geometer, and the head of the pure maths department. In a question period at the *end* of the course I am supposed to have asked 'What is a tensor?'. A commentary on both teacher and student, I think.

The teaching of mathematics was revitalised in my last years as an undergraduate when Professor Bullen arrived as head of the Department of Applied Mathematics. He *was* a teacher. After his course on Cartesian tensors I not only knew what a tensor was, but I had a very useful tool at my finger tips for handling vector relations. Bullen also gave us a solid course in electromagnetic theory, and in addition we learned much from a project he set us to produce a 200 page 'collation' of Abraham & Becker's *Electricity & Magnetism* and J.A.Stratton's *Electromagnetic Theory*.

Engineering students covered so much of the science curriculum during the first two years that they were then able to change over to the Faculty of Science, and in one year complete a BSc degree. To complete the BE degree they then needed to return to engineering for a further two years. At the end of my second year I changed over to science and never returned to engineering. I have sometimes regretted not completing the engineering training, but my interest in understanding *why* far out weighs my interest in making things. It was this wish to *understand* that led me to attempt to be a theoretical physicist. My deficiencies in mathematics meant that I was never very successful in that role. But I anticipate.

On completing the two years of engineering and one year of science we qualified for a BSc degree. But those aspiring to a career in science undertook a fourth (Honours BSc) year, studying just two subjects; in my case, Maths and Physics. It was at this stage that I became friendly with two of the science (rather than engineering) students - Ray Mitchell (who happened to live not far from me) and John Carver, naturally known as J.C. Later, when Ray and John were involved in experimental projects in the laboratory, and I was undertaking a theoretical project, I had a desk in a large room which overlooked the girls' hockey field. Eating lunch in my room became very popular!

One June vacation Mitch, J.C. and I went bushwalking. It must have been J.C who had heard that 'walking down the Cox' was the thing to do, and had found from a map that you could reach the Cox's River from the Western Highway. So we took the train to Mt Victoria and there slushy snow descended on us as we emerged from the station and hitched a lift on the back of a truck down to Hartley. Here we started walking along the Lett River, a tributary of the Cox. We had no experience of 'backpacking', but we carried 22-rifles to 'live off the land'. The rifles were cold, as well as heavy, but we did get a bunny that first night and roasted pieces on the fire. Then next morning I staggered the others by taking a bath in the freezing river. I didn't have to repeat this performance on the following days because the river entered a narrow gorge and forced us to take to the high ground. We lasted only a few days, but I remember a great last meal of our emergency chocolate rations accompanied by milk from a local dairy, and with great joy we threw the remains of the dried vegetables on the fire. What a stink! Those dried vegetables, probably left-over soldiers' rations, were ghastly.

In the honours BSc year I learned a great deal from an exercise, set by Professor Bailey, which required us to discuss at length the statement 'J.J.Thompson discovered the electron'. This introduced me to the study of the scientific literature, and, incidentally, to the 1911 Encyclopaedia Britannica, which, if I recall correctly, included a long article by J.J.Thompson. I read in depth about the research at the end of the nineteenth century which elucidated the nature of electricity. I believe I wrote a good essay which concluded that J.J.Thompson was the 'father' of the electron, rather than it's 'discoverer'. I regret that Bailey did not return that essay to me. It was these studies that provided the background to my first research work, my MSc dissertation under Victor Bailey's supervision.

Professor Bailey's interest was in 'electricity in gases', and our Honours year laboratory project involved a study of the oscillations that occur in gas discharge tubes. You have probably sometimes noticed such oscillations in fluorescent lights. While Bailey's title was Professor of Experimental Physics, he was a mathematician rather than a physicist, and was certainly not skilled in experimental work. Our laboratory project was really supervised by Dr Landecker. Dr Makinson was another staff member who came round the laboratory and answered our questions. He was the most approachable of all the staff members. While answering a question he would prop one foot up on a stool and start rolling a cigarette, an action that certainly indicated that he was prepared to spend some time discussing the question. His was such a relaxed attitude it even suggested that smoking might have some merit!

Makinson was the best physicist on the staff. While the quantum theory had arrived on the scene after most of the staff had received their education, Dick Makinson had a PhD in solid-state physics from Bristol, and actually knew something about the quantum theory. He gave a short course on quantum theory - perhaps we learnt the elementary Bohr theory of the hydrogen atom. But I have no memory of any searching discussions about quantum questions.

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At the end of my first year at university my brother Norman arranged a vacation job for me at AWA (Amalgamated Wireless Australasia), where he worked. Although the family budget must have been extremely tight, we children were never encouraged to take part-time jobs to supplement the family income. Rather, after doing our set household chores, we were expected to dedicate ourselves to our studies, while taking enough time for sport and recreation to keep us healthy. So jobs that I had during university vacations were not for the purpose of earning money for the family, but to increase my knowledge. I was paid for these vacation 'jobs', but my employers got essentially nothing in return. They must have believed education was a sufficient reason to justify employing students.

As one of their wartime activities AWA built aircraft instruments, many of which were quite unrelated to 'wireless'. I was attached to the quality control group in the aircraft instruments section and my 'work' consisted of being taught about the way the instruments functioned. For this I was paid two pounds per week. (Two pounds was also the cost of a week's full board at the 'Strathaven' guest house at Terrigal where Lach and I had a couple of holidays in this era.) At AWA I learned quite a lot about altimeters, gyroscopes, artificial horizons, and moving coil electrical meters. I recall that one of the instrument makers rewound an old 1mA meter to make me a 50μA meter. I then mounted this in a wooden 'California Chocolates' box, and using some rotary switches from an old crystal set of Norman's, made a multimeter which served me for twenty odd years.

World War II ended during my second year at university, and during the vacations at the end of both second and third year I had 'jobs' at the CSIR Radiophysics Laboratory. The

Radiophysics Laboratory, which was in the university grounds not far from the School of Physics, had been set up at the beginning of the war to develop radar in Australia. In the years immediately following the end of the war this laboratory played a leading role in radio astronomy and put Australian physics on the world stage.

At the time of my vacation 'jobs' at Radiophysics, the laboratory was exploring peacetime applications of wartime techniques. On the first occasion I was attached to Brian Cooper's group, with Jim Hindman as my immediate mentor. Both men later became well known for their work in radio astronomy. Brian Cooper was at that stage developing DME, the distance measuring equipment for commercial aircraft which was soon afterwards adopted for all Australian airports and commercial aircraft. (Tens of years were to elapse before such a system was adopted world-wide). During that vacation I learned all that I ever knew about flip-flop circuits, and a great deal about waveguides and other transmission lines.

In my second vacation at Radiophysics I worked for Tom Kaiser, who was attempting to produce power at millimetre wavelengths by multiplying the frequency of the radiation from a 10 cm magnetron. The idea was that *linearly polarized* light would first pass through a *Kerr Cell* placed in a 10 cm waveguide, then through a polaroid, and finally onto a photocell. As polarized light passes through the liquid in a Kerr Cell the plane of polarization of the light is rotated; the stronger the electric field in the cell, the more the plane of polarization is turned. The amplitude of the oscillations of the electric field of the radio wave in the waveguide were so great that the plane of polarization would make several complete rotations during each half-cycle of the wave. Hence the light passing through the polaroid, and onto the photocell, would rise and fall in intensity many times in each half cycle. The electric field in the waveguide was swinging from minus to plus 3,000,000,000 times a second, and so the output from the photocell was expected to be modulated at a frequency perhaps ten times this. My glorious contribution to this project was to test various glues for use in making the Kerr cells. The glue had to stick polystyrene, which was used for the walls of the cell, but not be affected by the liquid used in the cell.

This (unsuccessful) attempt to produce millimetre waves by the Kerr effect was indicative of the great number of things that were being tried in the Radiophysics Laboratory at that stage. Down the corridor there was a large room filled with racks and racks of valves - one of the world's first electronic computers - these days, I believe, on show at the Royal Melbourne Institute of Technology. In another project under way in the room where I worked Reg Ryan was attempting to electroform a two-dimensional array of millimetre-wave cavities. The intention was to place this array at the focus of a paraboloid and use it to make 'radio photographs', in much the way that photographic plates are used to make images at the prime focus of an optical telescope. At one end of the cavities there was to be a thermionic source of electrons and at the other end a fluorescent screen. The radio waves in the cavity were supposed to accelerate the electrons sufficiently to cause the screen to fluoresce and make a visible picture. I believe this was a project of Joe Pawsey, the leader of the group of ex-radar people working on radio astronomy.

Tom Kaiser, who was in charge of the Kerr Cell project, probably helped CSIR (the Council for Scientific and Industrial Research) become CSIRO (the Commonwealth Scientific and Industrial Research Organization.) Tom was a member of the Communist Party, and at the time I worked for him he used to sell the Daily Worker on street corners. This was the era of McCarthyism in the US and it overflowed into Australia with scares about 'reds under the bed'. When the Act governing CSIR was changed in 1949 one of the changes made was to require all staff to take an oath of allegiance to the Crown. A couple of years later, when Tom was studying in England on a CSIRO staff scholarship, he demonstrated outside Australia House in London against Menzies' attempt to ban the communist party. This lost him his scholarship and his job. He had no chance of getting another suitable job in Australia, and indeed found it quite difficult to get one in the UK.

Communism was causing paranoia amongst capitalists, but many thinking people were attracted by its principles. It was the practices that were repugnant. I understood that Chris Christiansen and Bernie Mills, who shared an office next to where I worked for Tom Kaiser, and who were starting to make names for themselves in radio astronomy, were sympathetic to communist principles. If they were members of the party they certainly didn't flaunt it, and they fared better with the 'powers that be' than Tom. Dick Makinson, of the Sydney University Physics Department, whom I mentioned above, was a party member, or very sympathetic to the cause: he did not fare so well.

Nothing to do with communism, but Ron Bracewell, later a Professor at Stanford University, also shared that office with Chris and Bernie. Ron's desk was adorned with a sculpture in the form of an ox. This was to indicate his current presidency of the Oxometrical Society. As the name suggests this was a society for the promulgation of bull! It was formed by a group in the Laboratory which, somewhat to my surprise, I later learned also included Brian Cooper.

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After the BSc honours year most of us went on to work for an MSc. At this time Sydney University did not award PhD degrees: the route to research was via an MSc, followed, for the lucky ones, by an overseas scholarship to take a higher degree. My MSc project was to work with Professor Bailey on a new theory that he had just published concerning the way that radio waves travel through an *ionized* gas that is embedded in a steady magnetic field, and is acted on by a steady electric field.

Ionized gas, or *plasma*, is very common in many parts of the universe, even if it is fairly uncommon on the earth. As there will be much about ionized gas in what follows I should say a little about it here. A gas is made up of myriads of molecules. The outer part of a molecule consists of electrons, and because the outermost electrons are not bound to the molecule very tightly, various processes can dislodge one or two from a molecule.. When this happens the gas is said to be *ionized*. The displaced electrons are free to roam through the gas and are called *free electrons*; the molecules that have lost negatively charged electrons and so become positively charged are called *positive ions*. You could say that the gas has become electrified.

Electrons can be freed from molecules by passing an electric current through a gas, as happens in a fluorescent light, and in a lightning flash. More commonly in nature electrons are kicked out of molecules when they absorb a photon of radiation, often ultra-violet light. This happens near all hot stars. It also occurs in the upper layers of the Earth's atmosphere, 100 to 500 km above the Earth's surface. There the ultra-violet light from the sun ionizes the gas to produce the region called the *ionosphere*.

In empty space radio waves and light waves travel at the same speed: both are *electromagnetic waves*, they differ only in their wavelength. But when an electromagnetic wave travels through an ionized gas the electric and magnetic fields of the wave set the free electrons in the gas vibrating. The vibrating electrons radiate electromagnetic fields which combine with the fields of the original wave to transform it into a new wave that travels through the ionized gas at a different speed. The *refraction* of light when it ravels through glass is caused by a similar process. There it is the electrons that are *bound* in the molecules of the glass that are set vibrating. The fields radiated by these vibrating electrons combine with the fields of the original wave to form a light wave that (usually) travels through the glass more slowly than a light wave travels through a vacuum.

Because the electrons in an ionized gas are *free* rather than *bound* there are some differences between radio waves travelling through plasma and light waves travelling through glass. Firstly, radio waves travelling through ionized gas have a *phase* speed that is greater than in empty space. Secondly, if the density of free electrons is sufficiently great then the wave simply can't travel through the ionized gas. Instead it is reflected back. Metals, which also contain 'free' electrons, act in the same way when they reflect light waves. An ionized gas reflects all radio waves having wavelengths longer than a critical wavelength that is determined by the density of free electrons in the plasma. The earth's ionosphere reflects all radio waves with wavelengths longer than about 10 metres, i.e longer than the wavelength of waves in the so-called *short-wave band*. It is this mirror-like behaviour of the ionosphere that allows us to send short-wave radio signals round the curve of the Earth, e.g. between Australia and England.

If the ionized gas is embedded in a (steady) magnetic field - as happens in the ionosphere because of the earth's magnetic field - then this magnetic field affects the motions of the electrons that are induced by the incoming wave. This modifies the wave that is set up in the gas, and complicates the behaviour still further. Because of the importance of the ionosphere for radio broadcasting a theory was developed to describe the behaviour of radio waves travelling through an ionized gas that is embedded in a magnetic field. It is called the *magneto-ionic theory*.

Studying the implications of this magneto-ionic theory had been Professor Bailey's particular field of interest. Now, however, he was interested in developing a theory to explain the oscillations that appear spontaneously in ionized gases. As I've mentioned, these oscillations are very common in gas discharges; they are often seen in a florescent tube when it is near the end of its life. The theory that Bailey developed arose from considering what source of energy could be driving the oscillations. When a force pushes on something (in physics it is always called a 'body'!) the amount of energy given to the body by the force is determined by the strength of the force and by the distance that the force pushes the body *in the direction of the force*. The force exerted on a charged particle by a static, i.e. an unchanging, *magnetic* field is always at right angles to the direction in which the particle is moving. Hence such a field can never give or take energy from a charged particle. By contrast, the force exerted by an *electric* field is always in the direction of the field, and a static electric field can supply energy to a charged particle. Bailey presumed that the energy of the spontaneous oscillations in gas discharge tubes came from the electric field produced by the voltage applied to the tube. So with the hope of better understanding these oscillations he developed a more general theory than the magneto-ionic theory, one that would take account of the effects of a steady electric, as well as a steady magnetic, field pervading the ionized gas. He called this the *electro-magneto-ionic theory.*

At this time radio astronomers in England, and at the Australian CSIR Radiophysics Laboratory, were studying strong 'bursts' of radio waves that are sometimes emitted by the sun. This intense emission was known to arise in the ionized gas of the solar atmosphere, in the neighbourhood of sunspots, where it was known that there are strong magnetic fields. The way that the radiation was produced was a mystery. Bailey believed that the energy for the process must come from strong electric fields, and suggested that his new theory might provide an explanation for the emissions.

Bailey's theory applied to *plane* waves travelling through a uniform ionized gas pervaded by a static electric field and a static magnetic field. The theory showed that under some conditions the amplitude of the waves increased as the waves travelled. *Growing-waves,* Bailey called them. The implication was that these waves were gaining energy from the static electric field. Bailey argued that when conditions in a discharge tube, or in the solar corona, favoured *growing-waves* then any little disturbance occurring by chance would be amplified, and this could explain the oscillations in discharge tubes, and the radio emission from the sun.

My MSc project was to work with Bailey on this theory, my initial task being to check his mathematics and confirm the conclusion that waves could grow. The mathematics was fairly elementary, basically involving lots of algebra. However it was quite involved; in the most general case the equations were of the eighth degree. I became suspicious when I found that if the effects of the collisions between the electrons and the molecules of the gas were neglected then some of the growing-wave solutions disappeared. Intuitively that seemed wrong to me. I thought of collisions as destructive, as likely to take energy *from* the wave and attenuate it, not amplify it. So I worked carefully through all the algebra and found an error, a place where there was a plus sign when there should have been a minus sign. Correcting this error eliminated this type of growing-wave.

This still left other classes of growing-waves. To deal with the complicated equations Bailey developed a graphical method of determining the conditions for growing-wave solutions to occur, and a paper describing this graphical method of solution carried my name as second author - my first publication! Bailey was scheduled to present a lecture about his theory and this graphical method of solution to a meeting of the Sydney Branch of the Institute of Physics. However he became ill, and asked me to present the talk for him, which no doubt helped my reputation.

I was then given the job of investigating the relation of Bailey's general theory to the theory already developed for a type of radio amplifier called a *travelling-wave tube amplifier*. When a steady electric field is present the electrons have a steady flow motion, in addition to the oscillatory motion produced by the electric field of the wave. The theories which had been developed to explain the operation of travelling-wave tube amplifiers had shown that growing-waves could occur when the electron flow speed was just greater than the speed of the wave. It is as if the electrons push the wave along, and so give energy to the wave. These electron tubes were cylindrically symmetric, so I used what I had learned from Stratton's *Electromagnetic Theory* (in the 4th year project for Professor Bullen) to develop a cylindrical-wave version of Bailey's theory (for a restricted class of situations). I discovered a growing-wave solution of Bailey's equations in a situation where the electron flow speed was well below the wave speed. This result was written up as my first solo paper, and in the prestigious Physical Review! What a come-down when Dr Walker, from the Bell Telephone Laboratories, wrote pointing out that the result was wrong: it was the result of an inconsistent neglect of relativistic terms in the theory. This forced Bailey to develop a relativistically consistent theory.

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Our group of five students completed our MSc projects in less than 18 months; I see that my thesis is dated May 1949. By then we had applied for overseas scholarships. This was a bumper year for travelling scholarships. The traditional scholarships - the *Rhodes* and the *1851 Exhibition* scholarships - were supplemented by offerings from the just-established Australian National University (ANU), the Rocket Range at Woomera in South Australia, the Council for Scientific and Industrial Research (CSIR) and the Walter and Eliza Hall Institute of Medical Research (for research in medical physics). All these institutions needed trained staff, and the scholarships, which required a commitment from the recipient to return and work for the relevant organization, were a means of obtaining such staff.

Everyone who completed an MSc in physics that year was awarded an overseas scholarship. Surely a record? We would not have known of the Walter and Eliza Hall scholarships except for Ray Mitchell. He was interested in research in medical physics. He received a scholarship, spent a period in London, and returned to undertake research at the Institute.

He later did a medical degree and the last I heard of Ray he had become a GP in a country town. John Carver received an ANU scholarship for studies in nuclear physics. He and I shared a cabin *en route* to Cambridge, where we ended up in the same college. In later years John was Head of the Research School of Physical Sciences at the Australian National University; I believe there is a building there named after him. I was awarded a CSIR scholarship to prepare me to return and work at the Division of Radiophysics. I suppose I was disappointed not to be awarded one of the more prestigious ANU scholarships. However my work with Bailey's theory, and my interest in electronic devices, certainly fitted in with the interests of the Radiophysics Laboratory. And the Radiophysics Laboratory turned out to be a marvellous place to work.

To Cambridge

'Do you realise what you have done? A place was reserved for you in the Cavendish Laboratory and you have turned it down.' Thus Mr Ratcliffe's secretary greeted me on my arrival in Cambridge!

My scholarship provided for two years overseas study as a preparation for working in the CSIRO Division of Radiophysics. At the time the Division of Radiophysics and the University of Cambridge were the world leaders in research in radio astronomy. Hence, if I was destined to work in that field, the Cambridge radio astronomy group was the obvious place for my overseas study. However there was a problem. Australians were not welcome in the Cambridge group. Ill-feeling had arisen between the two groups; I think the Radiophysics group believed that the Cambridge group had influenced the English journal *Nature* to delay the publication of one of the Australian discoveries. Whatever the cause, there was ill feeling, and now the Cambridge group was suspicious that any Australian working in their group would be a spy!

The Cambridge radio astronomers formed part of a more general radio section which was under the direction of Mr J.A.Ratcliffe, and was based in the famous Cavendish Physics Laboratory. (Don't be deluded by the *Mr* rather than *Dr.* I don't know why Ratcliffe didn't sport a doctorate, but amongst Cambridge mathematicians it was definitely 'in' to be an MA and not a PhD - rather like surgeons being *Mister,* not *Doctor*.) Mr Ratcliffe's specialty was the study of the earth's ionosphere. Earlier, when Ron Bracewell had received a CSIR staff scholarship and been unable to join the radio astronomy group at Cambridge, he had undertaken ionospheric research in Ratcliffe's group. It had been suggested that I should do the same, and to that end an application had been made to Mr Ratcliffe who had accepted me as a research student in the radio group.

But meanwhile my brother Norman had approached Dr G.H.Briggs, the Chief of the CSIRO Division of Physics, and Norman's supervisor for his MSc studies, and asked Briggs' advice as to where I should study. On hearing that I hoped to do theoretical work Briggs suggested that I should go to Cambridge and do the Maths Tripos, i.e. obtain a bachelor's degree in mathematics. In retrospect, if I was to undertake significant theoretical work, I should have followed that advice. I ultimately abandoned my attempt to be a theorist because of the weakness of my mathematics. However, at the time, the idea of going back to undergraduate course-work, and struggling with pure mathematics, did not appeal to me in the least

Then Dr Briggs mentioned to Norman that a mathematical physicist at Cambridge named Fred Hoyle had been creating quite a stir with a proposal of 'continuous creation' as a way of explaining the expanding universe. He had also been theorising about how the corona - the outer atmosphere of the sun - had acquired it's temperature of a million degrees. The idea of working with such a man sounded to me much more exciting than working on the ionosphere, or doing the Maths Tripos. So Briggs wrote to Hoyle asking if he would be willing to take me as a graduate student for the PhD degree, and Hoyle was brave enough to say he would 'certainly look after Roberts for the next two years'. So that is how I came to turn down Mr Ratcliffe's offer of a place in the Cavendish.

[Apropos of the idea that, if I intended to be a theoretical physicist, then I should have undertaken the maths tripos: After writing those last paragraphs I came across Fred Hoyle's account of his student days at Cambridge in his autobiography *Home is Where the Wind Blows* [Oxford University Press, 1997], and was intrigued to read on page 91: 'Neither of us thought it a particularly important decision. For me, however, it was a watershed. It was to lead...to the full rigours of the later years of the mathematical tripos....the training I would receive in the next three years would have one inestimable value. When I eventually returned to science it would be with a thoroughly sound mathematical technique. It was a real beginning.']

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But we should go back to the middle of 1949 when I was completing my MSc thesis in Sydney, and awaiting the results of my applications for overseas scholarships. As I have related in the last chapter, it was just at this time that the charter of the Council for Scientific and Industrial Research (CSIR) was changed, and the organization became the Commonwealth Scientific and Industrial Research Organization, or CSIRO. These changes caused delays in the awarding of scholarships. The uncertainties about where I should go, and who would be my supervisor, caused further delays, and my departure was uncertain until the last moment. John Carver and I had booked passage on the Orient liner RMS (Royal Mail Steamer) *Orontes* to sail from Sydney on Saturday 20 August 1949, my Father's 75th birthday. However it was not until the Monday prior to sailing that a cable received from the Board of Research Studies at Cambridge finally decided that I should sail!

To undertake a PhD at Cambridge a student had to be accepted by a college, accepted by a supervisor, and accepted by the Board of Research Studies. The University at Cambridge began life as a set of colleges; any centralised administration came much later, and to become a member of the university you first needed to become a member of a college. When Fred Hoyle was approached to be my supervisor he was also asked if he could secure a place for me in his college (St Johns). However by then it was so late in the year that there were no vacancies in St Johns. Indeed it wasn't until our ship reached Fremantle that I learned that Hoyle had accepted me as a student. But that mail at Fremantle also told us that John and I would probably be accepted by Selwyn College, an arrangement made for us by the secretary of the Cambridge Board of Research Studies, who happened to be a fellow of Selwyn.

On the *Orontes* John and I shared a two-berth cabin on G deck in tourist class. The fare of £79 bought us five weeks holiday with full board, including a day or two ashore in each of Melbourne, Adelaide, Fremantle, Colombo, Aden, and Pt Said. It was a great holiday. We teemed up with Herbert and Vera David and their friends Alan and Norma Hall, plus Bea and Val Taylor from Kandos. Alan was a geographer, headed for London on an ANU scholarship, if I recall correctly. His wife Norma was the life of the party. Herbert David had been in our year at Sydney University, but in maths rather than physics. His family must have escaped from Europe before the war, and come to Australia. He was a more sophisticated person than any of my other associates. It was from Herbert that I first learned the word *naive,* which he (correctly) applied to me. (As evidence of my lack of contact with the outside world at this time - aged 22 - I had been to the movies about half a dozen times, once taking a girl.)

A diary that I kept records that the trip round the Australian coast to Perth was rather boring. However there are favourable comments on both the buildings, and the setting, of the university in Perth, while a physics seminar at the University of Melbourne, conducted by an ex-Aussie, Professor H.S.W.Massey from University College London, impressed me. As we returned to the ship in Melbourne there was an amusing happening. This related to the camera that I had bought just before leaving Sydney. I still have the camera - my first - a folding Zeiss Ikonta, that takes 16 pictures on a roll of 120 film. The price is pencilled inside; £16. Compared to the cost of our passage to England (£79) that sounds very expensive, and I wonder how I had acquired so much money. Anyway, according to the diary, as we went back to the ship in Melbourne at sailing time, I collected two rolls of film that I had left for processing. The price was six and tenpence, but between us we could raise only five and elevenpence halfpenny. The rest of our money was safely on board in the purser's keeping! We offered two tuppeny halfpenny stamps and some fruit in barter for the missing tenpence halfpenny. In the end they settled for the stamps.

Ceylon, my first foreign country! I was delighted by the picturesque sight of Colombo from the ship, the modern, light coloured buildings, the domes of mosques and temples, the bright sun, the blue sea, and the white clouds forming a backdrop. Ashore I was fascinated by the dark skins, the colourful saris, the masses of people, the snake-charmer ('No teeth, no fangs. Come close, take photograph'), and the delightful little Buddhist temple with its festive oil lamps lit in the evening for the moon festival. And I was dismayed by the poverty, the begging, the crowding. When Norma was approached by a small boy of eight or nine, his left arm clutching a baby a few months old, his right hand held out for money, Norma went rushing across the street in sheer horror.

Next stop Aden, built in the crater of a volcano. It would be hard to imagine a more inhospitable landscape. The only trees were a few planted in little hollows in the rocky ground, which were being watered from a camel-drawn cart. The foliage of the trees seemed an intense green, presumably by contrast to the stark surroundings. Again the press of humanity. Again the poverty. The standard of living here was far lower than anything we saw in Colombo. Again the beggars - occasionally with a touch of humour, as with the chap who stood way down in the bottom of the water tanks hewn out of the rock (reputedly built by the Queen of Sheba), and who called up 'Any English money? Penny, threepence, sixpence, shilling. Good English money. Me like.' Then when a few coins were thrown down to him 'Ah, Good English Captain!'

As we made our way up the Red Sea, and approached the Suez Canal, I was surprised by the number of ships we saw. I noted 40 or 50 in the Red Sea, mostly oil tankers, and more than this between Suez and Port Said. I was also surprised by the greenness and modernity of the settlements that we passed in the Canal Zone. At night I marvelled at the number of lights in what I had assumed was a harsh desert. I wrote that at night it resembled Sydney Harbour, with the lights of the Harbour Bridge roadway, and the lights of the city. Sydney was a very different city then, to now!

This was a period of ill-feeling between Cairo and London. Pt Said was under martial law, and we had only a few hours ashore. '... no cameras, diaries or correspondence allowed ashore (liable to confiscation and the owner to imprisonment). The Suez Canal police (who incidentally carry 303 rifles) warned ladies not to go ashore unaccompanied and preferably in large parties. The services of a dragoman (guide) should not be accepted unless he wore the official badge....There were more people trying to sell us things than Colombo ever thought of', to quote the diary. While it was bedlam in the streets, I found Simon Arzt's shop beautiful. Tremendously tall, very cool, modern and pleasant; as good or better than any of Sydney's stores. And Coca Cola was on sale everywhere!

On Saturday 24th September 1949, five weeks to the day after sailing from Sydney, we berthed at Southampton and took the boat train to Waterloo. 'From the train we saw the glory of the English countryside. Pretty hills and streams and more trees than one can imagine. There was no scrub at all'! Then the thrill of seeing Big Ben and other places that featured on postcards that I had been shown by my aunts. The illuminated fountains in Trafalgar Square, operating for the first time since the war, made a big impression. And St James Park:-'such peace and contentment in the heart of the city'. The sameness of the English houses, however, evoked a negative reaction. And in the East End the devastation from the bombing was horrifying. (Though perhaps even more horrifying was to find it in much the same state when I visited nine years later, by which time cities on the Continent had been completely rebuilt.)

So finally to Cambridge with its narrow streets and even narrower footpaths, with red double-decker buses making their way through the throngs of bicycles cramming the streets. Delight at the marvellous bookshops; amazement at the stalls in the market square where meat was sold in the open with bees and flies crawling about; amazement at elderly ladies riding bicycles while trailing a dog on a lead; disappointment at the way the buildings of the town crowded round the university buildings spoiling their appearance; and delight at the view of the buildings from 'The Backs', the parkland on the other side of the River Cam. John and I hired a punt and, admiring the willows draped over the river Granta, and the reflections in the still surface, we made our way up to Grantchester. At least John knew Rupert Brooke's lines

> Stands the Church clock at ten to three?
> And is there honey still for tea?

Selwyn College turned out to be an 'approved foundation'. Originally all Cambridge colleges had been religious institutions, but in the late 19th century they were secularised; the religion of a student could no longer be a condition for membership of a college. Some time after this the Church of England decided that it wanted a college restricted to those of the Anglican faith and so founded Selwyn College (named after the first Bishop of Melanesia). The university recognised it as an 'approved foundation'. By the time that John and I became members of Selwyn the religious test was quite nominal.

During our first year we were required to live in college, and eat breakfast and dinner in college. John had a room in the actual college buildings, and delighted in showing visitors that the word LADIES had been painted out on the door! (Ladies, of course, were but visitors. Selwyn was an all male enclave, as were all the colleges except Girton and Newnham.) I was across the road in a 'hostel', in a second floor attic room that had been a box room for the house. The floor measured 9 feet by 12 feet, but the ceiling only 9 feet by 9 feet, because of the slope of the roof.

We were issued with rations each week: 6 ounces of butter or margarine, 1½ ounces of cheese, and 4 ounces of sugar. We collected a milk ration of 3/8 pint each day. For breakfast, taken in college, we had to provide our own butter and marmalade. Cereals, toast and something cooked was provided. I had never before eaten kippers, and I hope I will never have to eat them again!

The rationing system was peculiar. You did not have to surrender coupons for meals in restaurants, so if you had money your food wasn't rationed. We ate lunch out: the best value was at the B.R. (British Restaurant), a wartime invention run by the Borough of Cambridge. There you had a two course meal (roast and two veg, plus stewed apples or steamed pudding) for one and ninepence. Roast beef was accompanied by Yorkshire pudding. As I ate two main meals each day, including lots of fat and potatoes, it is no wonder that by December the diary records that I weighed 12 stone, whereas I had been 10 stone 7 pounds when I left Australia. The three years at Cambridge marks the only time in my life when I put on weight. I would appreciate some of that fat now to keep me warm.

After the first year both John and I moved into 'digs', and our only contact with the college became the stipulated twice-a-week 'dining-in'. In my digs my landlady provided breakfast, and on Sunday she cooked my week's meat ration for my midday dinner. Other meals were taken in restaurants, with the two Indian restaurants proving popular for evening meals. The two dinners a week in college turned out to be pleasant occasions as the college provided a room where students with BA status could have coffee together after the meal. As there were students from many parts of the Empire there were some interesting discussions. At one time I asked Australia House for some tourist literature about Australia to show to the others at coffee. One of the brochures identified the girls shown holidaying on an island in the Great Barrier Reef as stenographers, and thereafter the reef became Stenographers' Reef!

There were lots of rules for students at Cambridge. Some of these arose from the fact that the university and the town are completely mixed up, and so to allow students to be distinguished from townsfolk anyone below MA status was required to wear their gown whenever they were in the town. This applied even when riding a bicycle, the normal means of getting about. It was easier for the undergraduates who had short gowns. We had BA status and wore longer gowns which could easily get tangled in the bicycle chain. The BA status might have sounded good, but the main 'privilege' that it conferred was to double any fines imposed by the Proctor and his 'bulldogs'. These fines were in units of six and eightpence for undergraduates but thirteen and fourpence for BA's! On the other hand anyone with *MA* status *was* privileged: they could walk on the lawns in the college courts!

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My diary doesn't contain much about science, so for that I have to rely on my unreliable memory. The Cambridge radio astronomy group was led by Martin Ryle. When I arrived in Cambridge I learned that Ryle and Hoyle were at loggerheads over theories of the origin of the intense radio emission from the sun. I had stepped into a hornets' nest. So it was very pleasant to be put at ease by Mr Ratcliffe, who went so far as to arrange a place in the Cavendish adjacent to the radio group where I could work (in the fluid dynamics library). This simplified interaction with the radio astronomy group, and led to many interesting discussions. Some of the discussions with Ryle became quite heated: I have a memory of Graham Smith attempting to smooth troubled waters with words like 'Well, Jim might have a point, Martin.' (At that time Graham, the friendliest of the radio astronomers, wore shorts and sandals round the Cavendish, and had such a relaxed attitude to life that he could have been an Australian! He later became Sir Francis Graham-Smith and at various times was Director of the Royal Greenwich Observatory, Director of the famous Jodrell Bank Radio Observatory, and Astronomer Royal.)

I recall that my first meeting with Hoyle was over morning coffee, taken while seated in large leather lounge chairs. This gives you the picture of the man; totally informal. We discussed the work on 'growing waves' that I had done with Bailey, and over the next few months there was much discussion as to whether or not there were circumstances in which Bailey's growing waves could be important in transferring energy from a stream of fast electrons into radio emission.

I spoke about Bailey's theory at a radio-group colloquium and Richard Twiss was in the audience. I always think of Richard as a proper English gentleman with striped pants and furled umbrella. He worked at SERL, the Services Electronics Research Laboratory, at Baldock, not far from Cambridge. Richard was a very competent theorist and had worked on the theory of electron-tube amplifiers. He thought that Bailey's growing waves might be able to exist in an infinite, completely uniform, ionized gas, for which the theory had been developed. However he believed that once the boundaries to the gas were considered, it would be found that the growing waves were unimportant. Various people worked on this topic over the next few years; I am not aware that the question was ever properly resolved.

When it became clear that continuing the work on growing waves would not provide a suitable thesis project for me, Hoyle suggested another project. At this time the sun, the moon, and the broad sweep of the Milky Way were recognised as sources of cosmic radio waves. In addition there were numerous 'discrete sources of radio noise' - isolated spots on the sky which were sources of intense radio emission. These *discrete sources* didn't coincide with bright stars; indeed there was nothing of note to be *seen* in the direction of most of the sources. However John Bolton and his colleagues from the CSIR Radiophysics Laboratory, working at Dover Heights in Sydney, had measured the position of three of these sources with sufficient accuracy to suggest their identification with visible objects. One of these was the *Crab Nebula*, a fuzzy bright patch on the sky, about 5 minutes of arc (1/12 of a degree) across. Photographs taken over a number of years had shown that this nebula was a shell of ionized gas expanding outwards at a speed of 1,000 km per second, yes, per *second*! It is the outer part of a star that was blown off in a gigantic explosion - a *supernova* - which occurred in 1054 AD. The explosion was recorded by the Chinese as the appearance of a bright new star - a star which faded over the following few months. Hoyle had me investigate the radio emission from this object.

At this time the process by which most of the cosmic radio emission was produced remained a mystery. Radiation is emitted by a charged particle, such as an electron, when it is *accelerated*. Different forms of acceleration produce radiation with different characteristics. The first serious theory of the cosmic radio emission attributed the radiation to the accelerations experienced by the free electrons in an ionized gas when they pass close to the positive ions in the gas. The positive ions attract the negatively-charged electrons and swing them off course, and this acceleration produces radiation called *free-free emission*. The name refers to the fact that the electrons are free - i.e. not bound to positive ions to form atoms or molecules - both before and after the near collisions.

Attributing the radio emission to free-free emission hit a snag when it became clear that much of the radio emission was extremely 'bright'. *Brightness* is the term for the amount of radiation emitted *per unit area* of the surface of a body (and per unit solid angle). We use the same term for light, heat, X-rays or radio waves. Thermodynamics teaches us that in that mystical state called *thermodynamic equilibrium* the brightness of a *black body* is determined solely by its temperature. (A *black body* is an object that reflects none of the radiation that falls on it, but absorbs it all). This brightness of a black body at a specified temperature is given by Plank's Law. From Kirchhoff's Laws it follows that a body that is not black will be less bright than a black body at the same temperature.

These ideas have been generalised to the belief that the following statement is true, whether or not there is thermodynamic equilibrium:

> The brightness of the radiation produced by electrons that are moving *incoherently* (randomly) can never be greater than the brightness of a black body in thermodynamic equilibrium at the *equivalent temperature.*

What we call temperature is just a measure of the energy of the random motions of the particles in a body. The *equivalent temperature* is the temperature of a body in thermodynamic equilibrium in which the energy of the random motions of the electrons would be the same as energies of the radiating electrons being considered.

Because of this belief we often specify brightness by the *brightness temperature*, the temperature of a black body in thermodynamic equilibrium that would have the same brightness as the body under study. Specifying brightness in this way tells us immediately that if the radiation is produced by an incoherent process then the emitting electrons must have energies corresponding to this temperature. (I might add that I have yet to see a proof of this generalisation, and spent much time during my thesis years worrying about the truth of this all-powerful generalisation.)

By 1949 it was known that the strong *steady* radio emission from the sun (the 'quiet sun' radio emission) had a brightness temperature of a million degrees. There was independent evidence, optical evidence from the width of spectral lines, that indicated that the tenuous outermost part of the sun's atmosphere, the *corona,* must be at this extremely high temperature. For this reason it was accepted that the 'quiet sun' radio emission was free-free emission coming from an ionized gas having a temperature of a million degrees.

In the case of the Crab Nebula, the radio brightness temperature was known to be about 100,000 degrees, but the optical (spectral-line) studies suggested that the ionized gas was at a temperature of only about 10,000 degrees. Hoyle suggested that I investigate whether both sets of observations could be satisfied if the temperature of the nebula was really 100,000 degrees. The first thing that I found was that raising the temperature *reduced* the radio emission. This is because hotter (faster) electrons suffer less deflection in near-collisions with positive ions, and consequently produce less emission. To overcome this problem we had to suppose that not only was the gas at a much higher temperature, but also that there was much more gas. But then we found the free-free *light* emission would be far brighter than the light actually observed.

This problem of the Crab Nebula is an example of a general law that was set out clearly some years later by Jack Piddington of the CSIRO Radiophysics Laboratory. As he showed, free-free emission always produces more light than radio waves. Hence, for objects like the Crab Nebula, that are outstanding radio sources, but are faint optically, the radio emission cannot be free-free emission.

When I found that Hoyle's idea for the Crab Nebula didn't work, he suggested the problem that became my PhD thesis. This was to examine a theory of the origin of a type of intense, *sporadic*, radio emission from the sun known as 'noise storm' radiation. This radiation is much stronger than the quiet sun radiation, and comes from small areas on the sun. Its brightness temperature is measured in *thousands of millions* of degrees. Some details of my struggles with this problem are set out in the next chapter, along with some general discussion of radiation emitted by electrons when they spiral in magnetic fields. But for now let me talk of other activities at Cambridge.

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The PhD degree at Cambridge was a research degree: unlike the American system lectures were not part of the programme. However I took the opportunity to attend lectures given by some of the famous physicists at Cambridge. Not that I necessarily learned much! In addition to lectures, there were many colloquia, in both mathematics and physics. I recall Bondi launching a new series of colloquia in the maths department. He made the point that they wanted new and challenging ideas. That was the hallmark of Bondi, Gold and Hoyle. They specialised in being provocative and in challenging the accepted viewpoint.

Dirac's lectures on Quantum Mechanics left me quite puzzled. Indeed, the preface to his book, which seemed to imply that if theory predicted something then it must be so, set the tone for my puzzlement. Hoyle gave an interesting set of lectures covering some aspects of astronomy. From these I recall his contention that whenever Eddington and Jeans differed on some matter it was always Eddington who was correct! Ratcliffe's lectures on the magneto-ionic theory were full of insight. But Bondi's lectures on cosmology were probably the most interesting. He was then writing his book on the subject and his lectures were very clear and illuminating.

An enduring thought that remains from this brush with cosmology is of the 'action at a distance' of the universe in determining our local inertial frame. When a gyroscope is spinning freely, and not acted on by any torque, its spin-axis remains *fixed in space*. That is why gyroscopes are used in inertial guidance systems. But what do we mean when we say *fixed in space*? From observation, fixed relative to the distant stars; and by implication, *fixed relative to the distant parts of the universe*. If you have ever seen a Foucault pendulum swinging, and then returned some hours later to see the that while the earth has rotated the pendulum has kept swinging along its original arc, then you have witnessed this grip of the distant parts of the universe on local happenings.

I might add that I find most cosmological theories unrealistic. They are based on the assumption that any sufficiently large region of the universe is much the same as any other, definitely avoiding the egocentric thinking of earlier generations. As a basis for theories of the universe, universe meaning *everything that there is*, this 'cosmological principle' might be very fine. But the theories are used to describe the piece of the universe already known to us, as if that were a large fraction of all that there is to know. To imagine that we already have knowledge of a substantial fraction of all that there is to know strikes me as a ridiculous notion. I believe the structure of the universe must be *hierarchical.* There is not just one atom, but myriads of them; there is not just one star, but myriads of them; there is not just one galaxy, but myriads of them. When we find that we live in an expanding collection of clusters of galaxies why assume there is only one such expanding group? Why assume that a principle that might be appropriate for the *universe* should apply inside this particular expanding bubble? Why should every part of this bubble be much the same as every other part of the bubble?

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By 1951 an intense argument had developed between Ryle on the one hand, and Gold and Hoyle on the other, about the nature of the *discrete radio sources.* Apart from the sun, no bright stars had been found in the direction of any of these sources of radio emission. In the direction of the majority of the radio sources optical telescopes showed only faint, ordinary-looking objects. For just a few of the strongest radio sources, for which accurate positions had been measured, there seemed to be some unusual optical object in the direction of the radio object. But to confuse matters one of these (the Crab Nebula) was the remnant of a star in our Milky Way galaxy, while a couple of the others appeared to be unusual galaxies outside our Milky Way galaxy. The big question was whether the majority of the radio sources were in our galaxy or outside the galaxy. If they were beyond our galaxy they were obviously very powerful radio emitters.

The stars and the clouds of gas in our galaxy are arranged in a huge 'Catherine wheel', with the sun and its planets about two thirds of the way out from the centre. So when we look at the sky we see our galaxy edge-on, which is why it appears as a band of light across the sky, the Milky Way. Unlike the visible stars, the discrete sources of radio emission did not seem to cluster along the Milky Way; rather they were scattered all over the sky. Gold and Hoyle took this as implying that most of the radio sources were not in our galaxy, but were associated with distant galaxies. Indeed, to appear so uniformly distributed over the sky, they needed to be associated with quite distant galaxies, and so needed to be extremely powerful radio emitters. Ryle countered by pointing out that the *brightest* of the visible stars don't cluster along the Milky Way. They are also scattered fairly uniformly over the sky. This is because they are relatively close-by, no more distant than the thickness of the Catherine wheel. Ryle suggested that the radio sources were similarly close-by in our galaxy. [It is interesting that a similar conflict had arisen between *optical* astronomers in the 1920's. Then the matter in contention was the distance to the fuzzy patches of light called *spiral nebulae.* Were they objects in the Milky Way, or, as was ultimately proved by Hubble, *island universes* outside our galaxy.]

In 1951, Graham Smith, from Ryle's group, measured the direction to the second strongest of the discrete radio sources, Cygnus A, with considerably improved accuracy. Shortly afterwards Graham Dewhirst from the Cambridge Observatories took a long photographic exposure in this direction. Within the error rectangle of the direction to the radio source there was a faint, and therefore very distant, galaxy. On the basis of a less accurate radio position that he had measured, Bernie Mills from Sydney, had already suggested that this galaxy was the radio source. When Dewhirst promoted the idea that this distant galaxy was indeed the radio source it rather put the cat among the pigeons.

Graham Smith's measurements of the radio position of Cynus A were sufficiently accurate that Walter Baade and Rudolf Minkowski were prepared to use the Mt Palomar 200 inch telescope to study the area. They confirmed the identification of the radio source with the optical galaxy by showing that the galaxy in question was *very* unusual - it appeared to be two galaxies in collision! They were able to measure the optical *red-shift* of the galaxy and show that it is so distant that if it were a factor of two further away it would have been beyond the range of the Palomar telescope. That the second strongest of the discrete radio sources was near the edge of the optically observable universe was staggering news. The papers by Baade and Minkowski reporting these results (published 18 months later in *The Astrophysical Journal* Volume 119, pages 206-234, 1954) are central papers in the history of radio astronomy.

When news of Baade and Minkowksi's findings reached Cambridge in 1952 Gold and Hoyle were cocka-hoop. I remember Gold giving a very passionate talk on the implications. He assumed that this discovery meant that most of the discrete radio sources were galaxies beyond the Milky Way; galaxies, which, because of some peculiarity, were extremely strong emitters of radio waves. How long was it before Ryle came to accept this conclusion? When I read in an early draft of the book *Astronomy Transformed* that it was not until 1955 I was surprised. David Edge, a contemporary of mine at Cambridge but in radio astronomy, sent me this draft which cited the Halley Lecture of May 1955 (*The Observatory* **75**, 137). There Ryle examines the various possibilities before concluding that the majority of the sources must be extra-galactic. In another 1955 publication (*Proceedings of the Royal Society* **A230**, 448) Ryle and Scheuer give a similar discussion, but they then proceed to use their source counts to discredit the Bondi, Gold and Hoyle Steady State theory of the universe. As far as I can see the claim that sparked my interest does not appear in the published version of *Astronomy Transformed*, authors Edge and Mulkay, published by Wiley in 1976, many years after I was sent the draft.

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Of course in 1952 these events they were of only peripheral interest to me: I was struggling with my thesis project, trying to calculate whether the gyro radiation emitted by energetic electrons shot out from the sun would be able to escape to the earth. According to the regulations it was possible for a student at Cambridge to complete a PhD in two years, but it was only the brightest of students who achieved this. Cambridge had shown me that I wasn't a fool - even on world standards - but I clearly wasn't a genius. And after the false starts in selecting a thesis topic it became obvious that I would not qualify for the degree in two years. My CSIRO scholarship was for two years, and CSIRO would not extend the scholarship. They insisted that the scholarship was for training in research, not to enable me to gain a higher degree. However if I could finance myself in some other way, CSIRO would allow me another year before I was required to return and work for them. So I began to search through the UNESCO book of scholarships, and through any other information on scholarships that was available. I found nothing that seemed applicable to my position and in desperation approached a commercial firm in Cambridge that provided information on sources of finance for students. As I recall, the list they produced contained nothing new, and at first I was furious at having to pay them for non-information. However, contrary to my reading of the UNESCO list, they suggested that I would be eligible for an International Astronomical Union Grant-in-Aid for Young Astronomers, and recommended that I should apply. I applied, and to my surprise and joy received a grant which allowed me to spend a third year at Cambridge.

As I explain in the next chapter, despite much work, which taught me a lot about the theory of radiation, and despite coming to understand much about the effects of a surrounding ionized gas on the emission from energetic electrons spiralling in a magnetic field, I did not reach a clear-cut answer to the problem Hoyle had posed. I was due to return home in the autumn of 1952. Mid-year Hoyle said that I should write up what I had done and submit it. My diary notes, written on the ship coming back to Australia, recall the mad rush to write the *dissertation*. 'Starting off thinking of a thesis entitled "Theories of Radio Noise" & ending up with "The Incoherent Radiation of Electrons in a Magnetic Field". Continually paring down the original idea because of lack of time (& space)!'

Every three years the International Astronomical Union - the IAU - holds a General Assembly where astronomers forgather from around the world. A General Assembly was to have been held in Leningrad in 1951. However East-West relations were at a low ebb at that time and the 1951 Assembly was cancelled to be replaced by one in Rome in August 1952. CSIRO agreed to my attending the Rome General Assembly, just prior to my returning to Australia. The thesis was typed in the week before I was to leave for Rome, and I planned to spend the week-end filling in the mathematical symbols, and gluing in the figures. Disaster struck on the Sunday night when I found that the acetone cement I had bought to glue in the figures was useless as it dried too quickly. I fell back on the kindness of Tom Senior, a PhD student in the maths faculty. He undertook to buy a more suitable glue (cow gum), to glue the figures into the three copies of the thesis, to have these bound, and then airmail them to me in Rome!

The train trip to Rome - via the Simplon tunnel - took a day and a half. Leaving Victoria station at 10:05 am on August 2nd I reached Rome at 10:30 pm on August 3rd. The diary comments on my exhilaration at seeing the 'massive silent mountains' in Switzerland and the 'glorious Italian lakes'. It also comments that in Switzerland and Italy large electric locos replaced the steam engines of Britain and France. No doubt this was an advantage in the Simplon tunnel. 'We entered the tunnel at 10:00 and emerged at 10:12 & our train doesn't dawdle.'

Rome was impressive. The General assembly opened with a reception at the Campidoglio on Capitol Hill. After wandering through many rooms of priceless paintings and sculptures we emerged onto a terrace overlooking Rome. Here a full orchestra dispensed music, and food and champagne was laid out on tables. Then one drifted down the hillside through a delightful garden with floodlit trees. My diary also records that: 'On the Sunday we visited Castel Gondolfo, the summer residence of the Pope, and had an audience with His Holiness. He addressed us in French, and then shook hands all round - 400 of us, so I guess his hand needed massaging.'

My diary says that I found the conference scientifically a bit disappointing. The highlight must surely have been Oort's presentation of the brand new Dutch results showing the distribution of atomic hydrogen gas in the Milky Way. This information came from observations of the first *line* emission known in radio astronomy. Line emission is confined to a very narrow bandwidth of frequencies, in this case near 1420 Mhz, i.e. near a wavelength of 21 cm. The existence of this line emission had been predicted during World War II by Van de Hulst, a student at the Leiden Observatory in Holland. The emission was first detected in 1951 by Ewen and Purcell at Harvard, and the discovery was confirmed soon afterwards by Muller and Oort in Holland, and a few weeks later by Christiansen and Hindman in Australia. The Dutch were unfortunate to be pipped at the post on this discovery; their efforts had been set back by a fire in their equipment. The rapidity with which it was confirmed by the Australian group showed how easily others could have made the discovery had they taken the prediction seriously. But it was Oort's group at Leiden that realised the importance of the hydrogen-line emission as a tool to map the cold gas between the stars, gas which is invisible in the optical range.

It was in Rome that I took the oral exam for my PhD! PhD students were required to 'defend' their thesis before their examiners. Originally students were seated on a three-legged stool during the oral exam, which is the origin of the name *tripos*. In my case Hoyle had the idea that he and W.H.McCrea would be my examiners, and since both were attending the Rome IAU meeting, that the oral could be held in Rome. The exam was held, but the Board of Research Studies disallowed it. Ultimately Hoyle persuaded the Board to waive the oral, this being easier than arranging to hold one in Australia.

After Rome it was back to Cambridge for the final rush to submit the thesis and pack. Then after hurried farewell visits to friends in different parts of England, I embarked on the same ship, the *Orontes*, just a few days over three years after disembarking. The diary reads 'The Bay (of Biscay) a bit rough, but I am up for all meals. The meals good after England....The great thrill of peaches and cream & lots of green salad and tomatoes.' Another passenger on the *Orontes* was Rymill Abell, from Melbourne. After a period in England he was returning to Australia to take up a position in the optical workshop of the CSIRO Division of Physics. On the ship Rymill introduced me to the game of Canasta, and I became an addict. Back in Sydney we both became staunch Youth Hostellers.

On the return trip the ship spent a day in Naples. While the diary comments on our being pestered by 'chaps trying to sell you P.arker 51's' (fake Parker 51 fountain pens), the visit to Pompei made a more lasting impression. 'The paved streets, with stepping stones for the Pompeians to avoid the water and mud. The chariot wheel marks worn into the paving...Wine shops, bars, bakeries, public baths, Roman theatres, houses of ill-fame and Palaces of Justice. All are there...relics from the ruins. How like our present day! Egg shells, fish hooks, bread that was left in the oven and got somewhat charred, cutlery, dentists and doctors implements....Then the pathetic figures of humans and animals as they were found, caught by the ash and fumes of the eruption.'

Australians who returned from England by ship always talk about that first Australian milk shake that they enjoyed in Fremantle. I missed out on that experience as I was in the ship's hospital. One night we had all gone swimming after the dance and I had been swinging from a cleat under the poop when I slipped. I gave myself a black eye and a cut lip and knocked myself out. So I spent some days in the ship's hospital. Although I was taken ashore at Fremantle for X-rays, I saw nothing of Western Australia. My first view of 'home' was of Adelaide, where my 'most outstanding memory is galvanised iron, but it is still home! The approach from Outer Harbour is not the best. Shanty town is the only way to describe it. Gal. iron roofs, gal. iron outhouses, gal. iron fences....And the colours are sombre. Just look at the grey-green and played-down red of new gum leaves. So different from the fresh green of new leaves in the English spring!'

So finally to Sydney and, after a week or two of holiday, in early November 1952 I became a Research Officer with the CSIRO Division of Radiophysics. Thirty five years later I took early retirement at age 60.

RADIATION FROM SPIRALLING ELECTRONS

When an electron moves through a magnetic field, such as the magnetic field that surrounds the earth, it is pushed sideways by the magnetic field. As a result it travels on an ever-curving path, a spiral, or *helix*. At every instant the sideways push is at right angles to the direction in which the electron is travelling. It is also at right angles to the magnetic field. Because the force is at right angles to the direction of movement the force doesn't change the speed of the electron, it merely changes its direction of travel. Technically the field does not give any energy to the electron, it only changes its momentum. Nevertheless the change in momentum is an acceleration, and any acceleration causes a charge to emit radiation.

'Magnetic acceleration radiation' or 'magneto-bremsstrahlung' is the name given to radiation produced by this type of acceleration. Since electrons orbiting in a magnetic field are said to *gyrate*, magnetic acceleration radiation from low speed electrons (i.e. electrons with speeds well below the speed of light, or *non-relativistic* electrons) is sometimes called *gyro radiation.* When electrons are moving at *relativistic speeds,* the emission, which then has different characteristics, is called *synchrotron radiation. Gyro-synchrotron radiation* is a term covering both the relativistic and non-relativistic regimes.

Surprisingly, the emission from electrons travelling round a circle (i.e. remaining in one plane), *and including the relativistic case*, was fully discussed as long ago as 1912. This analysis is in G.A.Schott's Adams Prize Essay *Electromagnetic Radiation* - quite a substantial book published by the Cambridge University Press. For electrons travelling at a low speed (i.e. at a speed much lower than the speed of light) all the emission is at a single frequency, the frequency of rotation of the electron around the circle. However for faster electrons there is additional emission at the harmonics of this frequency. The higher the speed of the electron the greater is the emission at the higher harmonics: for extremely relativistic electrons most of the emission occurs at high harmonics of the rotation frequency.

Following Schott's *Essay* there was little further interest in this subject until the 1940's, when 'atom smashers', machines that accelerate charged particles, were being developed. During the time that it takes one of these machines to accelerate a particle to relativistic speeds the particle travels many kilometres. To avoid building machines many kilometres long, most machines use a strong magnetic field to force the particles to travel round a circular track. It soon transpired that as the particles orbited round the track they emitted light - magnetic acceleration radiation. In machines called synchrotrons, the energy lost by the particles emitting this light limited the speed that could be attained, which led to the radiation being called *synchrotron emission.* In 1949 it also led Julian Schwinger to publish a fundamental discussion of the emission of radiation by charged particles travelling along curved paths.

I studied this paper of Schwinger's in some detail. As this was before the days of the photocopier, (and much before the days of down-loading a copy from the Internet!), I made a handwritten copy of substantial parts of the paper and used this as a reference for many years. Schwinger first discussed the electromagnetic field radiated by a charged particle during the time that it moves along a minute fraction of its trajectory. The field is that of an elementary *dipole* with the dipole axis along the direction of acceleration. For a particle in non-relativistic motion the field is symmetric about this axis; it is strongest in the plane at right angles to the axis, and in that plane the electric field is parallel to the axis. However when the motion is relativistic the radiation field is boosted towards the direction of motion, and the pattern of emission is no longer symmetric.

To evaluate the emission from the whole of the trajectory of a charged particle it is necessary to *phase-coherently* add the contributions from successive minute lengths of the trajectory. In 1912 Schott had given the result for the case of a circular orbit, relevant to an electron with a component of speed at right angles to a steady *uniform* magnetic field, but having no motion along the field. Schwinger now gave it for other cases, including the case of an electron with a component of speed along, as well as transverse to, a steady magnetic field - a spiralling electron.

An important 'fingerprint' of gyro-synchrotron emission is its *polarization*. First consider the case of a low-speed electron travelling round a circle. The emission is at a single frequency, the frequency of gyration, and the electron orbits the circle once for each cycle of the emitted wave. The radiated electric field, which is in the direction of the acceleration, is at all times radial, and so rotates through 360 degrees during each cycle of the wave. Hence radiation emitted perpendicular to the circular orbit, i.e. along the magnetic field, is *circularly* polarized. The *sense* of rotation is the sense in which the electron circles the field. Interestingly, this means that *relative to the direction in which the radiation travels*, radiation emitted in the positive direction of the magnetic field is right-handed, while the radiation emitted in the opposite direction is left handed. Radiation emitted at right angles to the magnetic field is plane polarized; the electric field lying in the plane of the orbit, i.e. at right angles to the magnetic field. Radiation emitted in other directions is elliptically polarized.

If an electron spirals along a helix rather than travelling round a circle, but its speed is still low (i.e. non-relativistic), then the patterns of emission and polarization are not changed. All that happens is that the *frequency* of the radiation is changed by the *Doppler effect.*

When electrons travel at relativistic speeds, i.e. at speeds approaching the speed of travel of the radiation that they emit, then the emitted radiation is beamed towards their direction of travel. Now if you think about the motion of an electron as it spirals in a magnetic field you will see that its direction of travel traces out a cone having its axis along the magnetic field, and an opening angle equal to the *pitch angle* of the electron's motion. (The pitch angle is the angle between the motion of the electron and the direction of the magnetic field). The emission of relativistic electrons is beamed towards this hollow cone.

For extremely relativistic electrons the 'walls' of this hollow cone of emission are very thin. The radiation at the peak of this thin cone is linearly polarized, the electric field of the wave being at right angles to the steady magnetic field. On either side of the peak the radiation is elliptically polarized, right-handed on one side of the peak, and left-handed on the other. It is as if the polarization pattern of the emission from non-relativistic electrons had been tipped, with the direction at right angles to the magnetic field being moved up so as to point in the pitch-angle direction.

For less energetic electrons the 'walls' of the hollow cone are thicker and asymmetric; for non-relativistic electrons the walls become so thick and asymmetric that the pattern of emission is no longer a hollow cone but an ellipsoid. As the speed of the electron changes the pattern of polarization makes a corresponding change from the extreme-relativistic to the non-relativistic form.

To recapitulate. *Gyro* radiation, emitted by low speed electrons spiralling in a magnetic field, occurs at the frequency of gyration. The emission along the magnetic field is circularly polarized; the emission in directions perpendicular to the field is linearly polarized; at intermediate angles the emission is elliptically polarized. As the speed of the electrons increases radiation is emitted at higher and higher frequencies, the harmonics of the gyration frequency. The radiation also becomes beamed more and more strongly towards directions which form a thin, hollow cone with its axis along the magnetic field, and a cone opening-angle equal to twice the pitch angle. The pattern of polarization for the non-relativistic electron is twisted up towards this cone.

Of course, in nature, magnetic fields are not uniform, so the path followed by an electron will not be an exact mathematical helix, but will reflect the changes in the strength of the magnetic field along its trajectory. As a result the emission will not be confined to just a single frequency and its harmonics, but will spread over a range of frequencies reflecting the different curvatures along the track. And since there will be many electrons, each following a slightly different orbit (in part due to their slightly different relativistic masses), the radiation emitted by a group of electrons will be a fairly broad-band *continuum.*

The electrons will also have a range of pitch angles. As a consequence the radiation emitted in a particular direction, i.e. at a particular angle to the magnetic field, will come partly from electrons travelling exactly in that direction (and so, in the extreme relativistic case, contributing *linearly polarized* radiation), partly from electrons travelling more steeply along the magnetic field (contributing *left-hand elliptically polarized* radiation) and partly from electrons travelling less steeply along the field (contributing *right-hand elliptically polarized* radiation.) The left- and right-handed components tend to cancel, and the resultant polarization of synchrotron radiation is predominantly linear, with the electric field at right angles to the steady magnetic field.

That the strong radio emission being received from space might be synchrotron emission was first suggested in 1950. Alfvén and Herlofson proposed that the 'discrete sources' were peculiar stars with magnetic fields that had trapped cosmic ray electrons, causing them to emit synchrotron radiation. Kiepenheuer responded, criticising the idea that such stars could provide enough radio emission to explain the discrete sources, but suggesting that the background radio emission could be synchrotron emission from cosmic ray electrons moving through weak magnetic fields in the gas between the stars. For some years synchrotron emission remained but one suggested explanation of the strong cosmic radio emission, but then in 1953 Shklovsky proposed a radical solution to that problem of the Crab Nebula's radio emission that I had investigated at Hoyle's suggestion. Shklovsky suggested that both the *diffuse component of the light* from the Crab Nebula, and the radio emission, are synchrotron radiation. Within a few months astronomers measured the linear polarization of the diffuse light and found it to be very highly linearly polarized, and this was taken as proof that the light was synchrotron emission. It was another three years before linear polarization was detected in the *radio* emission from the Crab Nebula, but the discovery of the optical polarization naturally gave a strong boost to the idea that high-brightness cosmic radio emission was synchrotron radiation.

[For those interested in history:
Contrary to some later reports, in his 1953 paper (Doklady Akad. Nauk **90**, 983) Shklovsky did *not* suggest searching for linear polarization in the light of the Crab Nebula as a means of testing his theory. His paper was in Russian, but Frank Kerr had arranged for such papers to be translated for the Radiophysics Library. When I saw the translation it immediately occurred to me that a search for optical polarization should be made and I suggested this to a meeting held at Mt Stromlo Observatory at about this time. In the event the first detection was made by Dombrovsky in 1954 (Doklady Akad. Nauk **94**, 21), and this was quickly followed by other measurements.]

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Reverting now to the earlier years and my thesis studies. At that time it was accepted that the *steady* radio emission from the sun was free-free emission from the million-degree solar corona. However at times the radio emission from the sun increases hundreds or thousands of times above this steady level, and the origin of this high intensity, variable radiation was not understood. There are several varieties of this intense solar radio emission. One type, called a 'noise storm', consists of myriads of short 'bursts' of radiation, of very variable intensity, that are emitted over periods of hours or days. By 1949 it was known that noise storms occur when there are large sunspots on the sun, and that the radiation comes from small areas near the sunspots. The small size of the emitting areas means that the radiation has a high brightness temperature, of the order of 1,000 million degrees. To produce such a high brightness by an incoherent radiation process the radiating electrons must have energies of about 100,000 electron volts, and so be just slightly relativistic. (The energy equivalence of the rest mass of the electron is about 500,000 eV.) Because the noise storm radiation is quite strongly circularly polarized, and because there are strong magnetic fields near sunspots, it was natural to suggest that the emission was *gyro* radiation from electrons that had been accelerated to high energy by some process in the corona.

However Ryle had pointed to a serious problem with the idea that noise storm radiation was gyro radiation. Ryle argued that the ionized gas of the corona would prevent such radiation escaping from the sun. Just as the earth's ionosphere reflects radio waves back to the earth, so the ionized corona should reflect gyro radio emission back into the sun. Hoyle now proposed a way round this problem. For radiating electrons shot out from the sun at high speed the outward motion should *Doppler shift* the emission to a higher frequency. This same effect raises the tone of a train whistle when the train is approaching us at high speed. Hoyle argued that the Doppler shift could raise the emitted frequency above the critical frequency for reflection by the corona, and so allow the radiation to escape to the earth. Examining this proposal became the main part of my PhD thesis, and proved far more complicated than had appeared at first sight.

The first problem I had to tackle was the effect that the fast electrons themselves would have on the refractive index. Even assuming that the Doppler-shift would raise the frequency of the emitted radiation sufficiently to avoid the deleterious effects of the refractive index of the corona, i.e. raise the frequency above the critical frequency for escape from the *corona*, there remained the question of what effect the radiating electrons themselves would have on the refractive index, and hence on the emission. As you would expect, I found that if there were very few electrons in the beam that was shot out, then there was no problem. However under those conditions there was not much emission! As the density in the beam was increased there came a critical range of densities where the emission just about reached the *black body* limit corresponding to the energy of the electrons in the beam. If the density was increased above this then the refractive index was affected so much that a reflecting layer appeared above the emitting region, and prevented the radiation from escaping. I reached this conclusion after many calculations laced with comments such as 'These relations are so complicated that their meaning is completely obscured. We therefore consider…' and 'The algebra is tedious'.

The next question was whether the emission really would be Doppler shifted to a higher frequency. For the Doppler shift to be appreciable, the speed of the emitter must be a reasonable fraction of the speed of the waves. If the refractive index, which measures the speed of the waves, was bring so drastically changed by the emitting electrons, then would this affect the Doppler shift? The answer was yes.

For the rather unlikely model where the beam of electrons carries it's own magnetic field with it, the problem was simple: if the density of electrons was in the critical range, then emission approaching the black body limit could be achieved. However for the expected case, where the beam travels out through the *coronal* magnetic field, then according to my thesis 'the work of section 10.23 shows that very little emission occurs. Now that one is faced with the result the explanation becomes almost obvious. The whole concept of the electrons radiating at a frequency greater than the local gyro frequency is based on the idea that the <u>region of emission</u> moves outward. With the first model in which the field is carried outwards, such a movement of the emitting region does occur. But for the second model, the magnetic field, and therefore the emitting layer, is stationary.' At this point in the thesis I find I have left a piece of paper on which I have pencilled '14th September 1955. I think this explanation is wrong since…'!

In the last chapter of the thesis I added a much more straightforward and clear-cut calculation. I computed the power, and the degree of circular polarization, of radiation emitted at the low harmonics of the gyro frequency by mildly relativistic electrons travelling round circular orbits. At a 1951 conference Ryle had suggested emission at the low harmonics as a way of overcoming the problem of the corona preventing the escape of the fundamental. My calculation showed that mildly relativistic electrons radiate so strongly at the low harmonics that achieving the black-body limit would require very few electrons. Furthermore the circular polarization of the low harmonics turned out to be very high, which was consistent with the observations. The refractive index effects of the corona would be unimportant for the harmonics; the only unsolved problem related to any refractive index problems caused by the energetic electrons themselves.

I suggested testing the theory by searching for evidence of harmonics in the radiation. In particular I suggested looking for a relationship between radiation received at frequencies in the ratio of 3 to 2, i.e. looking for emission at the third and second harmonics. To this end I studied the radio spectra of solar emissions that had been published by Paul Wild and his collaborators at CSIR, but found no evidence of related features. However I commented that the frequency range covered by the observations was hardly adequate; if a second harmonic appeared at the lower edge of the range then the third harmonic would be right at the upper edge.

As I write this almost fifty years later I wonder why the low harmonics were not considered much earlier in my studies. We knew that the brightness temperatures were so high that the electrons needed to be mildly relativistic. It seems surprising that the low harmonics did not immediately suggest themselves as a means of overcoming the problem of escape through the corona. Was it just that most of the published material related to the extreme relativistic regime of synchrotron emission with its linear polarization?

The Other Side of Cambridge

'*6th December 1949*
The days are really short now. Less than 8 hours of daylight. At midday my shadow is some 15 ft long! It is usually quite dark by 4 pm and the street lights are on...all the leaves have fallen and the face of the earth is changed...Most people have "gone down"...as term ended yesterday. I am alone in my glory over here at 23 Grange Road.' A later diary entry records that I did not do much work in the two weeks from the end of term until John and I also 'went down', 'but had just got going on the effect of temperature broadening on the gyro refractive index.'

'*Tues. 13th Dec., 1949*
My first real snow...The lifeless trees decked with snow, the steep-roofed English houses, the robins in the snow. What could be more English! John and I were like kids. Made ourselves some snowballs and tested their elasticity, Brinell hardness and duration.'

A week after that we were cocking the thumb, hitching lifts up the A4, that old Roman Road, Watling Street. We were on our way to spend Christmas with John's aunt and uncle at 'The Grange', West Kirby, across the Mersey from Liverpool. I had some misgivings about hitch-hiking; I imaged my parents would not approve. However for students it was the 'done thing', and there certainly weren't the stories of abductions, rape and murder that we hear nowadays. The stories were of standing or walking for ages in the rain, and then when a driver did stop, finding that he was turning off at the next village. Or, on the other side of the coin, of the Bentley that went all the way to your destination with a driver who shouted you tea *en route*.

We stayed in Youth Hostels; another J.C. discovery. We hadn't heard of youth hostels in Australia, and presumed they hadn't reached the antipodes, but they had. Shortly after I returned home I saw an advertisement for YHA in the window of the Sydney County Council in the old Queen Victoria Building, and that started a long association with a grand group of people. But of the Youth Hostels of England and Wales, some in castles, and some in peasant's stone cottages, I have great memories.

Stops on the way to West Kirby included Rugby, Leamington Spa, and Stratford-on-Avon where the thatched cottages delighted us. However Manchester and Liverpool, in the gloom and murk of winter fogs, with their buildings blackened by years of coal-burning, and damaged by wartime bombing, were quite depressing. In Liverpool, even the new cathedral, of light-coloured sandstone, and still under construction, was blackened by soot.

At West Kirby John's relations made us very welcome, and we had a great Christmas. And I was introduced to mince pies, containing mincemeat, but not the mince meat that I knew! After Christmas I spent a few days in Hartford with some distant relations of mine who took me to see the black-and-white town of Chester, and then in the New Year I rejoined John to hitch round North Wales - Carnarvon, Bangor, and the flanks of Mt Snowdon. There could not have been much time for writing diary entries as these degenerated to a few scrappy pencilled notes which give the total miles walked as 40, miles hitched 430, and miles for which we paid passage 116!

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Back in Cambridge it wasn't all work. During the first six months or so we had lots of visits from the crowd we had known on the ship, and it was fun to show them the sights of Cambridge. In good weather this would include punting on the Cam, with its accompanying hazard of the pole sticking in the muddy bottom of the river. Then the choice was keep hold of the pole and end up in the water, or let go the pole and, so to speak, 'be up the creek without a paddle'. In later years there were occasional visits by others from 'down-under'. I remember I welcomed my second cousin Ursulla to Cambridge when she came to study at Girton, one of the two women's colleges. *Ecology*, her subject of study, was a new word for my vocabulary.

But after those first few months, we were for the most part left to the heavily male-dominated companionship of Cambridge. So my diary entry regarding May Week records that we didn't attend the May Balls with dancing from 10 pm to 6 am, traditionally followed by punting up to Grantchester (in dinner suit) for breakfast. 'Having neither the money nor the companion I didn't join the happy throng.' Just before Easter Mitch (Ray Mitchell) had arrived in London to undertake his biophysics training, financed by a Walter and Eliza Hall scholarship. He visited Cambridge during May Week and we watched the 'bumps' on the Cam. The Lady Maggy crew scored a bump each day to become 'head of the river' and then celebrated by burning their boat. 'Prewar they actually burnt the boat they rowed in - now it's an old clinker.' And one evening we went to hear madrigals sung from punts under Kings Bridge, with the punts floating off down the stream during the last bracket. Very romantic.

I learned to play squash. Tennis was great on those long summer evenings, with twilight till 10 o'clock. But summer occupied only a small part of the year. In winter there was no tennis. Instead we played squash in the courts beside the Royal (Real) Tennis court, across the Cam near the University Library. Hugh Michael was my squash instructor. I met Hugh in the hydrodynamics library, where, like me, he had a desk. However in his case hydrodynamics was actually his subject. Hugh always beat me at squash, but I always enjoyed the game. And Hugh and his wife were good to me in many other ways.

On Sunday afternoons I often walked. Unlike Oxford, Cambridge is a small town, and the farmlands start just a few blocks from the town centre. Across these fields there are established walking paths, and on some Sundays John Carver and I would follow local practice and take one of these paths to a neighbouring village, where we would indulge in afternoon tea in a cafe. Before I learned the rules I walked across a field in Cambridge where there was no established path. That would have been OK in Australia, but the Cambridge farmer hunted me off. He said I might give his cows TB!

And I occasionally went to the cinema! You queued to get in, sometimes in the cold and rain. By contrast, in Australia at that period you booked your seats ahead of time. And inside an English cinema the air was full of smoke, while smoking was never permitted in cinemas at home. I also went to the theatre a few times, both in Cambridge, and on visits to London. A new experience for me.

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Thanks to the Youth Hostels and a good bicycle, and, as I shall relate later, also to the British Council, I saw quite a lot of the U.K. during vacation periods. My bike, unlike the average Cambridge 'hunk of iron', was reasonably lightweight, and it was equipped with a hub dynamo and a four-speed, wide-ratio, Sturmey Archer gearbox. I was so impressed with these features that I brought both a front hub and a rear hub back to Australia and had them fitted into a bike back here. Hub dynamos cause no added friction and are far superior to the type that rub against the tyre. For touring in England I installed a pair of panniers on my bike: a couple of ex-army haversacks stiffened with pieces of plywood and hung either side of the 'carrier' fitted over the back wheel.

That first Easter (1950) saw me cycling in Cornwall. I took the bike on the train to Penzance, well St Austel, actually. 'My, what a place! Steep narrow streets. Down, round and down. Stone shops and houses hemming in the very narrow streets...The instructions say "continue on through Mevagissey." A terrifically steep hill down into the village - stone houses lining the street - the street narrows, and is this the end? Oh no! It does a right angle bend there and strike! what a hill. So we walk - or rather, shove - that bike's heavy.'

'What a really quaint old-world village!...the fishing boats all grouped together, the slaty-grey stone houses, and that funny narrow winding street, and Oh how steep! Slowly it dawns on me. I have seen pictures of Cornish fishing villages but always imagined they really existed in olden times. It is a new experience to realise that people still live in that far off way...After the climb out of Mevagissey it's another hair raising hill down to Port Mellon. Both brakes hard on and I'm scared of going over the handlebars she's so steep. Easily lose my way in the dark. Well what's this? Waves breaking over the road!...and so finally in the dark we find Boswinger YHA and I'm not late for supper!'

Next day started with a road 'UNFIT FOR MOTORS'. Then it was 'What beautiful rolling hills! Such smooth curves and lovely valleys, that glorious patchwork of red and brown squares - no wonder people rave about the English countryside...The Church of St Just in Roseland. Nestling at the bottom of the hill - the churchyard climbing up the slope and gloriously picturesque with primroses, violets, buttercups and my first rhododendrons.'

[Interesting that on a walking holiday in Cornwall in 1999 I found bulldozers removing rhododendrons which have taken over large areas of the countryside!]

At Falmouth, using the self-timer, I photographed myself sitting in the sun on the rocks and *wearing shorts!* Amazing, after the long dreary winter. In Cambridge spring had sprung, and the crocuses had been greeted with great delight. But to sit in the sun! The weather continued to hold while I visited the church at Mullion. 'Aunt Blanche had written of the exquisite carving on the pews...However it is nothing compared with the beauty of the sight which greeted me. A massed array of delicate primroses and violets set in soil on the window sills - the children's corner...most beautifully decorated with these lovely blooms. Words can't possibly convey the breathtaking charm of that scene - the tasteful arrangement and the warmth of colour. I spent a while drinking in the beauty and attempted to photograph it.' Young Jim liked the flowery language!

After a visit to some distant relations by marriage it was '...a long steady pull out of Helston, and the rain didn't help...a stiff ride against the wind - a real battle. Because of the rain I had to wear my cape and that was like trying to sail into the wind...As the road from Logan's Rock to Land's End was directly into the wind - or should I say gale - the ride was not the most pleasant...Land's End might indeed have been the end of the earth. Just "the last house in England", "the last cafe in England", "the last hotel in England", a car park, a cable station, and a pillar box - all lashed by the fury of the Atlantic.' But how fabulous to visit this place of legend.

Of the ride up the coast from Lands End to St Ives: 'Superb views and an equally superb tailwind!' In England you can be drenched one minute and in sunshine the next. Taking a photograph often meant spending ages waiting for the sun to come through. 'Newquay - several good beaches...However my memories of the place were spoilt by the rain which started to become serious.'

Next day:-'A steady climb all the morning up to 1,000 ft and then a wonderful descent to Tintagel. What a scene. The land falls sharply to a coastal plateau a few miles wide...There is the world spread at your feet! The broad sweep of farms with the coast beyond...you look back across that picturesque patchwork of fields made more beautiful by the great billowing white clouds.' Tintagel, with the romantic ruins of King Arthur's Castle: 'I was most disappointed to learn that there is no foundation for the legendary connection of the castle to King Arthur...Whatever the truth of the matter, it is...a fascinating and romantic spot. The view down the coast can only be described as sublime.' The next day when I missed the early morning train from Delabole back to London I spent an interesting morning at the slate quarry watching men sitting '...surrounded by slate and slates, an old bag over their knees, as with chisel and mallet they split off sheets from the rough sawn rock.'

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That May Ian Shevill conned me into acting as a study group leader for 5th and 6th formers attending a conference he was organising in London. The Reverend Ian had moved on from St Paul's in Burwood and was now with the Society for the Propagation of the Gospel. At this conference on 'The Colour Bar' the speaker on 'White Australia' was Roy Lee, who recalled fox-hunting with my eldest brother Jack on the hills surrounding Stuart Town, in country New South Wales! I think Roy's father had come from China to the NSW goldfields. He had been at school in Stuart Town when my father was headmaster and was grateful for the way Dad had treated the non-white. By the time that I met Roy Lee he was vicar of the University Church at Oxford, following stints at St Martin-in-the-Fields, and as Director of Religious Broadcasts at the BBC. I was intrigued to learn from his talk that in the 1930's Australians were ethnically more Anglo-Saxon than the English!

One day during the preparation for this conference I was to meet Ian Shevill at Swan and Edgar's corner in Piccadilly. I was delayed and turned up at least half an hour late. When I asked a policeman - there was always a policeman to ask in those days - if he had noticed a clergyman waiting his reply was: 'Wearing a coat like yours?' We were both equipped with thick leather coats, a recommended purchase in Australia for those venturing to the cold of the Old Country. And speaking of London: Have I recorded that at this time the street sweepers in London wore ties?

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'*Boston, Lincolnshire. 24th June, 1950*
The greatest pleasure I have had for a long while was to get back my first colour photos. They absolutely live...They are Dufay transparencies - mounted they cost 1/- a picture.'

Kodachrome had been available for 35 mm cameras for some years, but it was not made as 'roll film'. But now the chemist in Market Square had told me of Dufay Colour which was available in the 120 size. Kodachrome is a subtractive process, with three superimposed layers of different colours, but Dufay Colour was additive. Like the screen of a colour television set the surface of the film was divided into a very fine grid (a *reseau*) of the three colours. I took numerous rolls of this film over the next two and a half years, unashamedly attempting to imitate the pictures that I saw in books showing 'The Glories of England'. Since the photographs in the books were in black and white they depended on *tones* for their appeal. As a result, the composition of my colour imitations did not depend on colour alone, to their benefit, I think. If I say so myself, as shouldn't, (another of Lachy Harper's phrases) I made some good pictures. I particularly remember one, taken somewhere in the Cotswolds, of an old building that had been a wool market. The misty sunlight and soft shadows of England were captured beautifully by that film.

Since those colour transparencies were 2¼ by 1⅝ inches they didn't fit a 35 mm projector. After I returned to Australia Keith McAlister helped me to use a die in the Radiophysics workshop to cut masks from thin sheet brass so that the transparencies could be mounted in 3¼ inch square cover glasses. I then projected these with an old 'magic lantern' that Dad had acquired somewhere. Originally it operated by gaslight, but I modified it to use a 500 watt electric light bulb. Some years later when I was going overseas I disposed of those cumbersome glass-mounted slides by giving them to Steve Smerd for the amusement of his children. Then about ten years back I suddenly thought they could be of historical interest. I phoned Beth Smerd to see if, by any chance, they still existed. She said I was too late by about a year. Pity.

Reverting to the diary entry of 24th June 1950: 'I left Cambridge about 7:45 pm on Midsummer's Evening (23rd). It was a glorious evening and I enjoyed the eighteen mile ride to the Ely hostel. The sun was low and cast alluring long shadows. There had been much haymaking...and the stooks of hay arranged in rows down the fields gleamed with a new lustre as the sun kissed their cheeks.' Next morning I left 'the Ely hostel...an ex RAF establishment...and cycled steadily to reach Boston (60 miles), by 3 pm. Coming up one saw the never-ending sameness of the flat fenland, where road and <u>river</u> run above the level of the fields...I have looked forward to seeing Boston since reading "My Love Must Wait". Here today I have been able to imagine little Flinders dangling his legs over the old-world quay and watching the men that "go down to the sea in ships". I have also seen the great Boston Stump (St Botolph's Church) and recalled the story of Matthew's being taken to Squire Banks' place at Revesby (12 miles north).'

'*Scarborough Y.H. 27th June, 1950.*
Well that was Saturday. Since then I have "done" two of England's great cathedrals - Lincoln and York, and am now at this famous seaside resort...Three square towers towering above the trees gave the first indication of Lincoln - and this from some miles away...The ground drops very sharply to the broad flat valley in which the city is built. The descent is precipitous - "dangerous for cyclists and quite unfit for motors". The cathedral is awe-inspiring mainly because of its tremendous size...the place simply reeks of the past - ruins of city walls and the castle...only to be put in the shade by York on the morrow'.

'I was still looking at Lincoln at eleven on Monday morn. Eleven thirty found me and my bike train-passengers for York...Two hours by train, two days by my bike riding! From the moment I reached York I was absolutely fascinated. History is written all over the city, and in a wonderful manner they have built the past into the present...a large part of the city walls is still intact & several of the gateways ("bars") are still...in use - they form a wonderful entrance to the city...and so up to the Minster. What a sight! Those two great western towers in all their glory on the hilltop!'

'*Burley Wood Head Y.H., by Ilkley Moor   Sun. Eve 2nd July*
Scarborough has everything that one could wish for in a seaside resort - except sun and surf. There are amusement parks, penny arcades, ghost trains, cafes, theatres and open-air theatre, gardens, boating lake, promenade and a swimming pool (sea water, heated)...I think our seaside councils should learn from Scarborough some things to do and not to do. Scarborough Corporation has...built many beautiful gardens all down a gully (the "Glen")'

with a boating lake at the end. 'On the other hand they run a lot of cafes and charge exorbitant prices, and also charge for the privilege of driving (or cycling) along the sea-front road!...I didn't sample the water here but did go in at Robin Hood's Bay. It was freezing!'

'On Wednesday I...had my first sight of the moors. I was climbing steep hills, rejoicing in the glorious views and singing sonnets to the inventor of gears when the bottom gear caved in. I spent some time taking it apart but could do nothing. So I rolled down hills and walked up and finally hitched a lift back to Scarboro. Whilst the bike was in dock on Thursday...I was able...to enjoy watching some children playing with their sailing boats in the Glen. Fortunately the bike was fixed by 4 pm - gears replaced, 21/-. Then it was off to Whitby.' And as a result of watching the children play in the water I won a small prize for a photograph that I entered in a competition run by the local paper. A little girl looks up at the camera from the centre of the rings of waves that were generated by her movement.

'Climb, climb, climb, but what a superb view! All those farms straggling up the far hill. Then it was down again to Whitby and the Y.H. against the Abbey. Whitby Abbey was the most satisfying ruin I've seen. It fully lives up to its pictures. There is nothing which gives you quite the feeling that you get on seeing such a near-complete ruin quite open to the sky above. Those Gothic arches and the decorations were truly worth seeing.'

Up on the moors west of Whitby, ['bleak uplands (~500-1,000 ft) covered with low growth...mostly heather and bracken'], the village of Goathland has recently attained tourist status as the 'Aidensfield' of the TV series *Heartbeat*. When Gina and I were in Yorkshire a year or two ago we duly visited without my realising I had been there before. However according to my diary entry written on 2 July 1950: 'As it was then 12 I was glad I was going down to Goathland for lunch. Ken had told me that Harry Needham was working there at a guest house run by one A.J.Brown. I had a most pleasant lunch and cider there (at Harry's expense). He was most surprised to see me. This A.J.Brown is an author - writes on Yorkshire - he gave me a few pointers on what to see.'

At the Lastingham hostel: 'The warden was a fascinating chap, but he could talk the leg off an iron pot. He was well versed in local lore and instructed me in the 4,000 years of history written in the neighbourhood. Three howes, or burial mounds, told the story of the Bronze Age habitation. Roman forts of a peculiar type - apparently built for practice and instructional purpose - forged the next link. The most fascinating part was the coming of Christianity with St Cedd and St Chad from Iona, the building of stone crosses on the moors to mark the way, and the 900 years old stone crypt of the church, which we examined (as tradition has it) by the light of a lantern. The ruins of an iron ore mine brought us up to date.'

'When I could finally get away on Saturday my way led through Hutton-le-Hole to Kirby Moorside, Helmsley and...after a three mile climb, up on to Scrawton Moor. Again those superb views looking back across the valleys! But the real joy of scenery was yet to come.

At the far end of the moor...the land drops sheer - like the edge of Burragorang Valley. There indeed was a feast for the eyes and, since the sun obliged for a while, also for the camera. One looked out across the neat rectangles of fields until they faded into the distance and in the haze one could see the moor again 15-20 miles away.'

'The descent down Sutton Bank was certainly rapid. "Steep Hill", "Gradient 1 in 5", "Gradient 1 in 4", "Gradient 1 in 5"...It is through this valley that the Great North (Old Roman) Road runs, and also the Flying Scotsman (which incidentally I saw at York)'. After an overnight at 'a most pleasant backwater called Ellingstring that...cast a spell over me and made me think I was back in the Australian country...I think it was the clear blue sky and this morning the warm sun that nurtured these dreams...it was certainly a joy to be alive as I made my way down to Ripon and its cathedral. Then another climb to Fountains Abbey. Begun in early Norman times this abbey must have been a splendid building...Again that thrill to see the huge nave...all open to the sky...to look through Norman and Gothic arches to occasional blue patches above.'

'*Bradby Y.H. 8th July 9 pm*
Well Burley was a good hostel. We all enjoyed playing ping pong with the warden. However the weather was crook and my knee was rather sore. On Monday I cycled towards Ilkley and duly walked up on to the moor just to say I had been on Ilkley Moor...and then off down the valleys and over the hills to Shipley and then Bradford. What a place! Every street cobblestones! The main road from Bradford to Huddersfield ...practically all cobblestones...was about the most unpleasant thing I have ever ridden on.'

'Bradford was nearly as bad as you imagine it. Small houses all covered with centuries of soot - the washing hanging in the side streets - mills struggling up the hillsides - more houses, always right on the street, of course. Ultimately I staggered into Huddersfield and was pleasantly surprised. The centre of the town looks well with broad, straight streets. I visited the hospital and was reassured about my knee - nothing seriously wrong with it, just go easy. Well I survived till Holmforth but the climb to the hostel just about did for me.'

'*Selwyn 21st July, 1950*
Holmforth Y.H. was good. The wardens...did my washing and I got generally well treated...I decided to spend Tuesday doing a bit of walking to rest my knee.' Then in the afternoon I hitched a lift over to Manchester. 'I was most pleasantly surprised. The city centre is well laid out - broad streets, huge bus station and car park - modern shops. I suspect bomb damage has helped them in their planning. I was amused by the mills. They all appeared to be constructed of red brick, with a square tower - the name of the mill being worked in white on the tower and on the main building.'

'I set off for Sheffield. My knee was crook...about ten miles north of Sheffield I stopped at a transport cafe for lunch...a fellow customer offered me a lift - said I could put the bike on the back of his truck. I'd had enough of hills and rushed that offer with my ears back...Forty miles of hills...Through Sheffield under a pall of smoke. Another dirty and uninspiring place. Then to Chesterfield with its church spire that really is crooked, and so to Derby...quite a passable place...As it was only 16 miles across to Nottingham I decided to head there for the night. Mark Butler had invited me to stay at his place...In the morning I was shown the city sights, the castle, and of course Trent Bridge Cricket Ground where the Test is being played at the moment.' After lunch we took a bus to Sherwood Forest '...the greater part of (the remains of) the forest is still an ammunition dump.' However we saw the Major Oak, said to be about 1,000 years old. Robin Hood and twenty of his men are supposed to have hidden in the trunk.

Then to Leicester, and on to Rugby, where 'I didn't find Mrs Carver and Ted [John's Mother and younger brother over from Australia] "in"...but was recompensed by finding a tablet in the Rugby School' that commemorates *the exploit of William Webb Ellis who, with a fine disregard for the rules of the game as played in his time, first took the ball in his arms and ran with it, thus inaugurating the distinctive feature of the Rugby game AD 1823.* 'At Badby Y.H....I did myself proud with strawberries at 8^{d} lb.'

Finally it was back to Cambridge. 'In the fortnight since I returned there have been developments. On the work side Twiss has been & given his talk on Bailey's theory - he appears to have beaten me to the bdy value problem. On the more pleasant side of life John has bought himself a car for £32/10/-, only snag being that the College will allow him to have it in Cambridge only if he surrenders the licence to the college during term!...The Halls are off to the Continent on their motorised tandem...Mitch is off to Paris and probably Berlin in September, and I still await news of my fate. Is it to be Zurich in September?'

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'*Küsterman Hotel, Ludwigstrasse 92, Garmisch-Partenkirchen, Bavaria*
Sat. 9th Sept., 1950 9 am
I just set out to wander up the hill and enjoy the view...However the weather said no! - it's raining. I left Cambridge last Friday week...'

Richard Twiss had suggested that I 'really should' attend the General Assembly of the Union Radio Scientifique Internationale, or URSI, the international union that covers the science of radio. The meeting was to be held in Zurich, in September 1950. I put this proposal to ASLO, the Australian Scientific Liaison Office in London, who administered my scholarship. Bowen and Pawsey at Radiophysics must have thought it a good idea: I was added to the Australian delegation, with CSIRO paying my expenses, including first-class rail travel. Then I persuaded ASLO to let me travel third class and use the difference to have a holiday in Bavaria en route. Just the sort of thing that gets pollies into trouble these days! I had found a book in the Cambridge Borough Library that was lyrical about the delights of Bavaria, and I had also learned that the Passion Play was to be performed in Oberammergau in 1950. Every ten years, throughout the summer months, this village devotes itself to day-long performances of the Passion of Christ: this was to be the first such occasion since before the war.

'70 Canterbury St, Cambridge 15/10/50
Well it looks as though I'm not going to get this log written up properly so I'll just list a few notes...made at various stages of the trip.'

'Monday 4th The great day
10 am train Victoria to Dover...The white cliffs of Dover!

15:50 Belgium. My first really foreign land - the first country I have been where English is not commonly understood...The 3rd class from London to Dover was superb - now it's hard wooden seats. My carriage was German - there were carriages from all over the continent on the train...

Brussels 7 pm...They served dinner 23/-. How glad I was that Cousin Sis [an elderly second-cousin in London] had loaded me up with sandwiches.

22.34 Aachen - German frontier town. The usual passport stamping, but in Germany there is also currency control...You must declare all currency...and travellers cheques. These are recorded on a form which you must produce when you leave.'

'Tuesday 5th September
00:35 Cologne. Through the darkness one could just discern the cathedral tower. It is said there is not much else standing in Cologne...

So through the night, Bonn, Coblenz, Worms, and in the early light Ludwigshafen and our last view of the Rhine. The country was lovely now. Much fine forest with occasional farms. Apple, plum and pear tress laden - they looked luscious. Tomatoes and maize growing outdoors - it clearly wasn't England.

8 am Stuttgart. Now we have an electric engine - right through Bavaria, Austria and Switzerland. They are certainly much cleaner!

11:30 Munich. I roamed Munich until 4:30 walking most of the time. It is a complete wreck. Other than the new buildings I don't think I saw an unscathed structure. Yanks everywhere [this was the American zone of occupation] - cobblestone streets with their fancy design - single-decker trams in threes. New shops are single-storied, prefab sort of things. Fruit was certainly cheap - 4^d/lb lovely apples.'

I missed the train to Oberammergau. It left at 14:55, but I was thinking 4:55! As my German wasn't the best I was grateful that the US occupation forces had a Services Transportation Office at the station where I could ask the time of the next train, though I did have some trouble understanding the American guy's accent! In Oberammergau we stayed with the villagers in their houses, houses that are magnificent works of art, as beautiful paintings cover their outside walls. 'German beds consist of mattress and sheets...and then a feather-filled thing like an eiderdown without the buttons. It is not nearly large enough to cover the bed...The food was excellent. Breakfast - coffee, rolls, butter and cherry jam. Lunch and dinner three courses, always with meat.' The Passion Play was an impressive all-day affair, held in a theatre 'like a large hanger. The chorus in simple grey cloaks open each set. They tell the story of an event from the Old testament having the same theme as the event from the Passion to be portrayed...Tableaux very impressive...Crowds that were crowds wave palms and shout Hosanna!...Christ portrayed as the pictures depict him...Break for lunch. Finish 5:30. No trouble with German - especially with English text.'

The trip, arranged by Thomas Cook, included a day visiting the fairyland castle of Neuschwanstein, ('Wander through pine forests...Great place for holiday') and the 'Church in Wiess - finest rokoko in Germany. Exotic colouring - breathtaking...almost overpowering.' Then I had three days at Garmisch-Partenkirchen. 'Superb cakes etc in shops. Custard tarts, peach tarts, chocolate. Coffee DM3.50 for 125 g (i.e. 23/- lb). Yanks buy for 25 cents in their special shops. Cameras...Wonderful selection. Mine selling DM108 = £9/10/- stg. Cost £A16 = £13 stg & sells second hand in England for ~£22 stg!' At Garmisch I inspected the ski stadium, the site of the pre-war Winter Olympics, and walked through the narrow Partnach Klamm where the waterfall produced much spray. But the main purpose of the sojourn was to take a trip to the top of the Zugspitze, the highest mountain in Germany. 'Weather clear early, but clouded. NBG for Zugspitze says hotel prop. Wait till tomorrow (my last day).'

'*Sunday Sept. 10th*
Trip up Zugspitze. People at pub mostly English - elderly ladies and three lasses - not inclined to take much notice of me till Zugspitze trip...Tickets for Bavarian State Zugspitze Bahn 20DM = 33/-! Train ordinary electric, then change to rack - holiday spirit - much English spoken - up at 1 in 5 to 1 in 4...Long tunnel to Highest Bar in Europe. Yanks taken over part of the pub. 3DM cableway to top...Cafes etc at top. How built?'

So there we were 12,000 odd feet up, on the border between Germany and Austria. A cross marked the highest point. It was a wonderfully sunny day, we were surrounded by a sea of mountain peaks, and below was the Eibsee, a magnificent blue lake with white edging. By this time I had made friends with Barbara and Jan. Barbara was the adventurous one. Shortly before this, or was it soon afterwards?, she climbed Kilimanjaro. Anglo-Indian Jan was the good-looker. We became good friends. '...train back to Eibsee. Super! Yanks taken over large pub. Yachts, aquaplaning, swimming - pine forests. This hotel US owned and operated. Deutchenvolk directed to Eibsee round a back path. Oh to go back there Youth Hostelling and clamber over those mountains.'

The trip through Innsbruck to Zurich had me extolling the superb views of mountains and valleys, with comments on the ripe corn in the fields, and the cheapness of Austria: 'good meal for 1/6'. However Zurich was expensive, and when I received a French 5 franc coin in change instead of a Swiss 5 francs I noted 'i.e. robbed of 10/-!' But 'the Vth class hotel super!...Meals - breakfast; fancy rolls, rye bread, Ovomaltine; main meals served on hot plates heated by candles - oodles of food.'

My notes list Pawsey, Martyn, Massey, Herlofson, Twiss, Macfarlane, Rydbeck and Alfvén as present at the conference, and there is a note about 'Massey's discussion group'. On the Saturday afternoon the shy young student went for a walk with Pawsey, and on the Tuesday there was a conference tour, the Three Pass Tour. First by train along a track that spiralled up through mountain tunnels to the entrance to the St Gothard tunnel, then 'Wonderful postal buses - open at the top - rugs' as we climbed up hairpins to the Furken Pass and the Rhone Gleitscher, followed by the Grimsel Pass and a visit to an underground hydro-electric power station. On those narrow mountain roads the yellow postal buses had the right of way to the *inside* on the bends, and warned of their approach by sounding their distinctive three-toned klaxons.

After Zurich I met up with Mitch in Paris where he had been attending a biophysics conference. There are photos of us up the Eiffel Tower, in the Bois de Bologne, at Versailles, and one of me holding our lunch, a long French breadstick. I liked Paris. 'Wide streets and footpaths - boulevards - fast traffic...high attractiveness quotient of women!...parks and gardens.' A wonderful sense of space, a sense of having been designed, impressive buildings, lots of parks and 'places'. But the traffic was horrendous. You took your life in your hands to get across to the Arc de Triomphe. When I asked Ray what was the point of the pedestrian crossings he joked that you were given a State funeral if you were killed on a crossing.

'Metro - slow - dilapidated. Stations renamed George V, President Roosevelt, etc - not "tube" - automatic doors and station gates' - these were the gates that shut when the train arrived and left you fuming when you could have raced in and jumped on the train! But I was impressed by the maps of the metro. You pushed a button beside the name of your destination station and the route lit up, indicating where to change trains. Mitch took me up to Pigalle 'like Port Said with people trying to sell you "dirty postcards"', and amused me with the story of his visit to a night club. Evidently a group from the conference had made a visit, and had sat at the bar and had a beer while they watched the floorshow. On his own in Paris after the conference Mitch thought he would make a repeat visit, but chose another club. Disaster. No bar. He was stuck with an expensive bottle of champagne!

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Christmas 1950 found me in Edinburgh with a group of other foreign students who were studying in Britain, enjoying a holiday arranged by the British Council. By then my diary entries have become sporadic, but my photographs show Edinburgh Castle and Princess St (with its double-decker trams) under snow. In my favourite picture, taken at 11 am on

Christmas Day looking straight at the sun, the sun *can* be seen through the fog! After Christmas we must have gone to Glasgow via Loch Lomond. The Youth Hostel at Loch Lomond, a castle with a huge baronial hall, is also shown under snow. Did we dance the old year out in that hall? I have been wont to remark that none of the girls who turned my head whilst I lived in England were in fact English: Jan (of the Zugspitze trip) was Anglo-Indian; Georgette, French; and Aida was from Bagdad. I met Georgette on this trip to Edinburgh. She called me a 'scientifiquis' and tried to expand my horizons to the world of art. My first ever visit to an art gallery was under her tutelage when she visited Cambridge and took me to the Fitzwilliam Museum.

It was on another British Council trip, again to Scotland, but this time in summer, that I met Aida. We had a magnificent week of sunshine, starting with a marvellous boat trip down the Firth of Clyde. The ferry took us as far as the Kyles of Bute and a photograph shows seagulls following the boat in just the way that albatrosses followed the *Orontes* on the trip from Australia. The coach then took us north to Inverness, via Loch Lomond, the grim Vale of Glencoe, and Loch Ness, but there was nary a sign of the Monster. North of Glasgow we passed Black Mount Deer Forest. Presumably it was once a forest, but by then there wasn't a tree to be seen. Aida made that trip for me: she was so full of fun and we had so many jokes between us. Afterwards there were many letters and a few visits between Sheffield and Cambridge. Indeed I was sufficiently smitten that (without telling her) I enquired at Australia House whether, should we marry, she would be allowed into Australia. The answer: Marrying a British Subject, Australian Citizen (as my passport classified me) would not automatically make her dusky skin acceptable to White Australia!

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Easter of 1951 found me on another cycling trip: 525 miles through 12 counties - London, Berkshire, Wiltshire, Somerset, Bristol, Gloucester, Monmouth, Hereford, Oxford, Northampton, Bedfordshire and Cambridgeshire. My scrappy notes of this trip are littered with: 'Headwind', 'Rain', 'Wet', 'Snow', but you also find 'Glorious views', 'Sun - glorious evening'. I took the bike by train via London to Reading where I started cycling down through Whitchurch and Andover to Stonehenge: 'Just got a snap before rain - massive stones morticed together - quite a lot of horizontal' stones still in place. '...mob of NZ & Aussie girls.' Then on through the Vale of Pewsey to Marlborough, the stone circle at Avebury, through Bath and over the Mendip Hills to 'Wells. Majestic cathedral - West Front in sun - poor man's bible...Back to Bath...wonderful remains - main bath still there...steaming monster behind doors.' It was an eye opener to see these Roman remains still in use.

Bristol ('more Aussie lasses - wet'), Chipping Sodbury, Wotton-under-Edge ('rain'), then, after John Carver joined me: 'Snow, lying on ground...Ferry Aust-Chepstow...Sun...Wye Valley - Tintern Abbey - splendid setting'. Yes, Tintern Abbey, another majestic building open to the sky, a never-to-be-forgotten sight. And the nearby Youth Hostel was in St

Briavels castle, one of the border castles built to keep the Welsh at bay ('sun - glorious evening...superb views...flowers and Scotch dancing.') Next day Symonds Yat Rock, where the River Wye meanders round a loop and almost rejoins itself; another magnificent view. On through Ross-on-Wye to Gloucester where I found myself in a sea of bikes as the shift ended at the Gloster Aircraft factory.

The Roman remains at Cirencester made a big impact, and in my mind's eye I can still see the tessellated floor of the Roman villa at nearby Chedworth. Then for several days I wandered round those legendary Cotswold villages with their thatched cottages:- Winchcombe, Upper & Lower Slaughter, Bourton-on-the-Water ('model village'), Stow-on-the-Wold, Chipping Campden ('snow...sheltered in hall'), Chipping Norton ('2/- lunch'), Shipton-under-Wychwood. After duly visiting Banbury to inspect the Cross, I then headed for home via Northampton and Bedford. En route I again stayed at that Youth Hostel at Badby, where I was intrigued by the speech of some young lads from Sheffield who were preparing their dinner. Their conversation consisted of phrases like: 'Poot potaatoes in put'; no waste of breathe with 'a's' or 'the's'!

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The summer of 1951 brought the Festival of Britain. At a Festival of English Music staged in the grounds of St John's College Fred Hoyle made great use of his Yorkshire brogue! In London, the South Bank exhibition made a big impression: 'Novel architecture and intriguing use of scientific devices for decoration. Concert by London Symphony in the Festival Hall at night. Super hall inside.' By this stage my diary consists of notes only, mostly written well after the events. These notes of events on Wednesday July 4th have the heading: 'Written 26/8/51'. 'Wimbledon with Mitch. Queue, but excellent standing position at centre court. McGreggor v Sturgess; Flam v Savitt; Rose & Mrs Bolton v ??'. Then for Friday 6th: 'Science Museum exhibition with Mitch. Intriguing designs again - electron density diagrams for patterns. Mechanical tortoise attracted by light and recharging itself.'

'<u>Saturday</u>: [July 7, 1951]...Rang Bowen [presumably in London!] & found he had recommended my appointment to Radiophysics staff, but to remain here a year...
<u>Sunday</u>: Return to C. to see Prof. Stratton re I.A.U grant. Changed to shorts at station & off per bus and hitch to Badby Y.H.
<u>Monday</u>: Badby to Stratford...Henry IV Pt II. Stand up seat at 2/6. Extra grouse show...
<u>Tuesday</u>: Warwick - castle, peacocks & chicks! Black and white houses.
Coventry. Lady Godiva statue, cathedral ruins, rebuilding civic centre....
<u>Wednesday</u>: Lichfield cathedral...superb carving. Hitch to Chester. Walked round walls & examined cathedral...
<u>Thursday</u>: Good lifts to Kendall...'

Someone famous (was it Mark Twain again?) is said to have dismissed the weather in the Lake District with the words: 'No day in the Lake District is worthy of the name of

Sunday'. However over the following week I didn't fare too badly, and reading through my notes recalls a week that was very different from any I had spent before.

'Saturday: Chummed up with London chap for walk up Striding Edge to Helvellyn...
Sunday:...then up to Loughrigg. Windy but gloriously sunny. Down to Ambleside. Leaving our packs ascend past Stockgill Force to Wansfell Pike. Views of Windermere superb.
Monday:...to Elterwater Y.H., leave our packs, and go on up Langdale Pikes. Weather dull at times...Wonderful view back down Langdale with Windermere and the other lakes in the distance.
Tuesday: We part company. I to Eck Haus (in cloud), down to Sty Head. Great Gable under cloud...Views down Borrowdale later in the evening...
Wednesday: Climbed up...to High Spy. Views of Derwent Water & Borrowdale superb. Back & up on to Dale Head. Glorious sunshine, but windy. Views of Buttermere & Honister Pass from Yew Crags. Descent down watercourse bit treacherous...
Thursday: Meander round Derwentwater...Friar's Crag. Keswick...Stone Circle at Castlerigg...Weather rather dull.
Friday: Penrith - Carlisle. Glorious views - superb day. Visit to castle...stories of border squabbles. *Once Brewed* Youth Hostel. Visit to Roman Wall. Not impressed.
Saturday: ...Plenty of cars but all full of holiday makers and not stopping. Finally reach Kendall at 5 pm and take to buses. After 10 pm before reach Bedford House' - the home of my relations by marriage at Hartford, near Chester. After a few pleasant days with them I was headed for home '...and have not much luck with lifts. Then the Rolls turns up and we average 70 to Badby.' And so back to Cambridge.

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Over Easter of 1952 I scored another trip to 'The Continent' when I answered an advertisement from a Cambridge undergraduate who wanted a companion to share expenses on a car camping trip to France. John P., the car owner, knew the ropes; he had friends and relations in the south of France, had a smattering of French, and had made a similar trip in the previous summer. I found it a fascinating three weeks, even if I knew essentially no French. And I've almost forgotten the unfortunate end to the trip when John rooked me of a small amount of cash.

Overnight on Saturday 22nd March we took the car on the ferry from Dover to Dunkerque. 'Drive on to top deck - train on bottom...Sun. 23rd Off at Dunkerque at 5 am French time. Wrecked and shuttered buildings...long straight roads (cobblestones - pavé) - tree-lined roads. Dawn - breakfast in a bar'. In those 'bar-tabacs' where we ate along the way, breakfast consisted of about 20 cm of breadstick holding a slice of ham (no butter), and a 'café grande'. That was coffee served in deep 'soup bowl', the equivalent of several cups.

On the trip south we passed through Amiens, Paris ('Met Georgette at Notre Dame - raining'), Versailles, Fontainbleau, Grenoble, then 'snow capped mountains. Hautes Alpes. Route Napoleon [the route he took on his triumphant return to Paris after confinement on

Elba]...dry desolate country of Basses Alpes & Alpes Maritimes. Glorious blue sky and sun. Trees in bloom - first olives...View of coast with Cannes and Nice. Palm trees, flowers in bloom...Descent to Cannes. Wonderful blue of the sea, almost purple...yachts...snow capped mountains in distance.' They do say that from Cannes you can ski in the morning and swim in the afternoon.

'Camp further west near St Raphael.' Then by Thursday 27th it was 'Swim first thing. Glorious day...back to Cannes & sight see.' I remember there was a helicopter for joy rides (*Helicop Azur*) parked on the (stony) beach at Cannes. On Monday we drove through Nice to Monte Carlo, via the Moyanne Corniche, the road half way up that extraordinarily steep hillside. Those views were marvellous - 'Cap Ferat a picture laid out below.' On the Wednesday we drove into Italy, to San Remo. 'Change in state of houses as cross frontier ...drive up to La Turbie. Superb views...Monte Carlo glued to the side of the hill - Monaco Ville on its crag.' And on Friday: 'Morning glorious and sunny. Beautiful drive through Cannes to St Raphael along the Corniche de l'Estoral. Glorious blue sea, red rocks and white foam. On through real bush - mostly cork trees...to Hyères...camp practically on the beach near Giéns. Many gum trees and wattle in bloom.' That camping spot had a great feeling, but the trees that I recall at Hyères were conifers.

'Saturday: Lovely day - lay in sun - John worked on car...spring trouble'. Then on Sunday it was on through Toulon, Marseille, Arles ('Roman amphitheatre'), Montpellier, and on the Monday to 'Sète, Agde, 8 mile beach & surf, Beziers, Narbonne. Camp near beach - quite a beach. Swim.' Tuesday found us at Carcassone, as the brochures say: 'The finest preserved medieval walled city in Europe', and then it was on to stay with John's friends at Laroque d'Ohmes. 'Lovely family...glorious two days...views of snow-clad first part of Pyrenees. Played French cards.' Our hosts had essentially no English and I essentially no French but we got by. It was so interesting to see how a real French family lived. And the meals! So much food, with several litre-bottles of wine on the table - the children drinking it watered down. Each course was served and eaten separately. I listed them in the order in which they were served: 'Meals: (soup), ham, salami (with bread), sausage, egg salad, main course (rabbit, steak), potatoes, cheese, custard, fruit, coffee and wine (John says 4 litres per meal).'

On the Wednesday, near the Chateau at Foix, we took a fascinating 2½ km trip underground, mostly in a row-boat on a subterranean river, with some walking up and down steps linking different sections. 'Stalactites and stalagmites'. More cards after dinner that night and then next day 'after lunch and many farewells & much hand-shaking headed north...Toulouse...and camp near Coussade. Superb moonlight night - nightingales'. Good Friday found us continuing north. After visits to Orléans and Chatres we were back in Paris for Easter Sunday and off to Dunkerque on the Monday.

SOLAR RADIO ASTRONOMER

November 1952. My two years of study with Bailey at Sydney University, and three years with Hoyle at Cambridge, have taught me something about radio emission, and the propagation of radio waves. Now I join the CSIRO Division of Radiophysics, famous for its Radio Astronomy research, and the Chief of the Division asks me, as my first task, to calculate the coalescence of raindrops!

Yes, the Radiophysics Laboratory was known world-wide for its work in radio astronomy, but that was only one of the areas of research at the Laboratory. Knowledge and techniques developed for wartime radar were being used for many peacetime activities: navigation systems for aircraft were being developed, one of the world's first electronic computers had been constructed, and attempts were being made to make rain. What, you ask, is the connection between radar and rain-making? Well, radar of the right wavelength can distinguish between water vapour, water droplets, and ice crystals in clouds. And anyway, an ability to make rain would be so important for Australia that you didn't need much excuse to follow up promising experiments made in the USA.

The rain-making project was the brainchild of Dr E.G. (Taffy) Bowen, Chief of the Division. The rainmakers weren't aspiring to manufacture clouds, they were satisfied to make existing clouds produce rain. To this end they were studying the physics of clouds, and the ways in which rain occurs naturally. In colder climates clouds were thought to produce rain by a process involving ice crystals, and some theory of this process existed. In temperate climates it seemed that ice crystals weren't involved. Here it was thought that some initial process - perhaps just condensation, or perhaps the coalescence of droplets in collisions resulting from turbulence - produced some water droplets in a cloud that were large enough that they started to fall slowly down through the cloud. Once a droplet started falling it would collide with smaller droplets with which it would coalesce to form a larger drop, which would fall even faster. So the process would accelerate until ultimately quite large drops emerged from the bottom of the cloud.

Bowen asked me to calculate this falling and coalescing process. The project didn't appeal to me; I was interested in electromagnetic radiation. However he was the Chief, and I was a shy new recruit, so although he asked me rather than directed me, there was no question of my refusing. I suppose it took a couple of months for me to become acquainted with the physics of the process, and then to set up the equations describing what happened. There I stopped. I had no idea how to solve the equations. Bowen sent me to talk to a mathematician at the Australian National University in Canberra. I probably just demonstrated my incompetence as a mathematician. After about six months I suggested to Bowen that I wasn't getting anywhere, and I was transferred to the radio astronomy programme, to a group studying the 'bursts' of radio waves emitted by the sun.

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Roast shoulder of lamb, cooked and served in huts out in the middle of McPhail's dairy farm - that's my first memory of being a solar radio astronomer. I had travelled by train to Dapto, 60 miles south of Sydney, where I was met by John Murray. He was the chief technical person of the group, and the designer of the electronic equipment. On this occasion he was the chef, and produced this succulent baked dinner in the minute kitchen on the end of the equipment building.

Radio astronomy was under the general direction of Dr J.L. (Joe) Pawsey, the Assistant Chief of the Division. The group I was attached to, headed by Paul Wild, was studying the intense *metre-wavelength* radio emissions that are sometimes received from the sun when there are large spots on the sun. These radio emissions from the sun were first recognised in 1942, during World War II, when they interfered with British radar. Initially it was assumed that the interfering signals were produced by the enemy, and with a view to pinpointing the source by triangulation, and then bombing it, all radar operators were required to report the bearing of the interference. When J.S.Hey examined these reports he found that at any one time all the operators reported the *same* bearing, that the bearing kept changing during the day, and that it was always the same bearing as the sun! Furthermore the interference ceased at night. A great discovery.

These strong emissions from the sun are in the form of sporadic 'bursts' of radio waves. Some of the bursts last only a few seconds, some last several minutes; some of the bursts of the one or two second variety occur as part of a storm of bursts that lasts for days. It was this last type, called *noise storms,* that interfered with the wartime radar. It was also this type that Hoyle had in mind when he proposed the study that had become my PhD thesis.

In 1947 it had been found that some *outbursts* - the name given to the most intense radio bursts from the sun - started at higher frequencies (shorter wavelengths) and drifted downward in frequency over the next ten minutes or so. To learn more about these drifting bursts, in 1949 Paul Wild and his collaborators had built a *radio spectrograph.* This device scanned rapidly across a wide band of frequencies, recording the power received at each frequency. The results were displayed as contour maps with the frequency along one coordinate, time along the other, and with the contours showing the strength of the radio signal. From these maps Wild identified three different types of events from the distinctive patterns they formed in the frequency-time maps. He called these Spectral Types I, II, and III. Type I are the storm bursts already mentioned; they appear on the maps as a multitude of small 'hills'. They are narrow-band, short-duration bursts, that occur in storms lasting hours or days. Type II are the outbursts, and their weaker brethren with the same spectral properties. They appear as long ridges beginning at a frequency of around 100 MHz or so, and drifting down to perhaps 30 MHz over a period of around ten minutes. Type III bursts appear as narrow ridges across the maps. They are like extremely rapid Type II bursts, and sweep over about the same range of frequencies, moving from high to low frequencies, but all in a matter of a few seconds.

[MHz is the abbreviation for Mega Hertz, or millions of oscillations per second. A radio wave with a frequency of 100 MHz has a *wavelength* of 3 metres; one with a frequency of 30 MHz has a wavelength of 10 metres; hence the phrase *metre wavelength* radio waves.]

These initial spectral studies covered the frequency range from 70 MHz to 130 MHz and used a fairly primitive aerial and recording system that had been assembled at Penrith, west of Sydney. Because of the success of these observations a second, more permanent, spectrograph had been built on the dairy farm at Dapto. Here a range of hills to the north provided protection from the radio interference produced in the city of Sydney, and the extensive paddocks meant there was plenty of space for remote aerials - of which more anon. So picture this establishment out in the middle of these cow-paddocks: three colourful aerials - they were painted orange with green bases - and three nearby small buildings.

The equipment hut was a longish building with the aforementioned minute kitchen on one end, and a similarly-sized workshop on the other end. In between there was a small office (which I shared with Paul Wild on our visits) and the equipment room. Sleeping and eating took place in the adjoining single-roomed weatherboard hut; it had probably come there on the back of a truck. It was furnished with a table down the middle, and camp stretchers round the sides. On winter evenings the primuses from the kitchen were brought in and put under the table, and before long the air would be thick. The third building was the outside dunny, with an attached three-sided shower recess, open to the sky above. There was no water supply connected to the shower, as it was at a higher level than the water tanks (which filled by run-off from the roofs of the buildings). So the first step in taking a shower was to fill the water container, with its attached shower head, and heave it up onto the rafters above the shower stall. On winter mornings, with frost on the grass, normal people added a jug of hot water. Having been raised on cold showers I cracked hardy!

In later years that little kitchen was the great gathering place while dinner was being cooked. At day's end some of us might go across to the dairy at milking time, to collect milk for the station, or cream to take back to Sydney. Max Komesaroff, John Murray's successor, would likely get into a political discussion with McPhail, the dairy farmer. It was one of those discussions that ended with McPhail's much-quoted comment to Jewish Max 'Well, anyway, Max, we're all Christians!'

After the milk had been collected the others would adjourn to the pub in Dapto. Being pretty well a teetotaller I would instead take a walk across the paddocks, or perhaps up towards rain-forested Mt Kembla, to the railway line, where during the day we occasionally saw goods trains struggling up the incline, with two steam engines at the front, and one at the back. Post-pub Paul Wild would produce the sherry bottle in that tiny kitchen, while Max cooked Wiener Schnitzel. Conversation would be animated as Max hammered the veal steak. On the primus the butter would be burning in the frying pan, egg and flour would be scattered far and wide, and the atmosphere would be thick with fug. But it was great schnitzel!

The aerials that collected the radio waves from the sun, and fed them to the radio receivers, looked somewhat like three orange rockets mounted on green launch pads. At the base of each aerial a green hut housed a motor and drive system that turned the aerial so that it followed the sun across the sky throughout the day. The structure of the 'rockets' resembled the lattice-work that you see in steel-girder bridges, but they were in fact made of marine plywood, a non-conductor. Wire in the form of two crossed diamonds stretched round this wooden supporting structure. These *rhombic* aerials collected signals in the two perpendicular planes of polarization.

The aerials were designed by Keith McAlister, a canny Scotsman when it came to cheap aerial designs. McAlister designs, this time in steel, saved much money in the construction of the *Chris Cross*, an aerial system built at Fleurs in about 1956 and still in use (by Sydney University) in the 1980's. Keith also produced the cheap design for the 96 dishes of the Solar Ring, built at Culgoora in mid 1960's as the successor to the Dapto field station. And Keith Mac was also an actor in one of those many stories about Max Komesaroff. Keith would berate Max for taking *both* Jewish and Christian holidays!

Since they were built of timber the Dapto aerials required a good deal of maintenance, and this led to frequent visits by the Laboratory's carpenters and painters. With the carpenters in residence accommodation would be a bit tight, but the added company was a social plus. At those times, instead of spending the evenings working on the results in the little office, we would gather in the other hut and play liar dice with much hilarity. And during the carpenters' visits Shep, McPhail's sheepdog, would sometimes be seen with his head thrown back howling along to the accompaniment of one of the carpenters playing the saw.

Collecting mushrooms is something else that I associate with the carpenter's visits. They must have had a nose for them. And mushrooms remind me of the time that Joe Pawsey brought Grote Reber down to visit the Dapto field station. Grote is part of the history of radio astronomy; the second radio astronomer, and he did it all on his own, in his spare time, in his own backyard in Illinios. In 1932, the report in the Bell System Technical Journal of Janksy's discovery that the Milky Way is a source of radio 'hiss' made no impact on astronomers, but it certainly excited radio-ham Grote Reber. By 1938 he had begun constructing a 31 foot diameter dish in his backyard, and over the following years by recording the strength of the radio signal received night by night with different settings of his telescope he produced the first maps of the radio emission from the Milky Way.

In the 1950's Reber decided that a 'hole' in the ionosphere over Tasmania provided an opportunity to study cosmic radio waves at frequencies as low as a few MHz. He moved to Tasmania and worked in association with the University of Tasmania, initially, I think, as a CSIRO associate. On the occasion of this visit to Dapto, Pawsey, Wild, Reber and I were walking across the paddocks when Grote saw what we regarded as toadstools growing. He maintained they were good to eat and wanted to pick them. Joe Pawsey said 'In your country we'll take your advice about what's safe to eat. Here I suggest you take our advice.' Of course the fungi could have been good to eat; in those days Australians ate only one type of mushroom. Apparently in Tasmania in later years Grote delighted in eating all manner of local fungi.

Grote had a very enquiring mind and took a great interest in many things. And he really enjoyed a joke. He would laugh so much when telling a story that he had trouble getting the story out. In Tasmania he started experiments to see whether the sense in which a vine twists round a stick is determined by the way the sun moves across the sky. He argued that if the sense was determined by the vine following the motion of the sun across the sky, then the same vine grown in the northern and southern hemispheres should twist in opposite senses. In a catalogue published by an American nursery Grote saw a picture in which a bean vine twisted in a certain way. So he wrote and ordered seeds of those particular beans. The suppliers said they couldn't be sure about which way the beans twisted, but they sent the seeds anyway. However when the seeds arrived they were impounded by the Australian Customs because of our quarantine regulations. That didn't phase Grote, he persuaded the customs guys to grow them for him in quarantine. Hee, hee, hee!

At another time he became interested in the history of the Aborigines in Tasmania, and sent remnants from some fires for carbon dating. The answers came back giving the dates of the various fires, and reporting that in one case the fire had not yet been lit! More mirth.

Back to the Dapto field station. Open-wire feeders carried the radio signals from the three aerials to the equipment room, and there, lined up side-by-side, were three TRF (tuned radio frequency) receivers, with triple-gang tuning condensers, just like the best home radios of the day. But instead of these condensers being turned by someone twiddling the tuning knob, they were driven round and round by a motor. In sequence the receivers scanned across the three frequency bands 40 to 70 MHz, 70 to 140 MHz and 140 to 240 Mhz, repeating the process every half-second.

In synchronism with each scan across the 40 to 240 Mhz frequency range, a spot was scanned across the face of a TV tube. The brightness of this spot was varied in line with the strength of the incoming radio signal. A 35 mm camera photographed the face of the TV tube on slowly moving film so that when the film was developed we had a record of the signal strength across the band throughout the day. As a monitor, a loudspeaker was connected to the receiver output, and all day one would hear the shush, shush, shush of the ½ second frequency-sweeps. Very occasionally an audible increase in this sound would announce a strong burst and bring people running to the equipment room to peer through the peephole at the pattern on the TV tube, and to switch on the *A-scan* camera which recorded the strength of the signal as a graph - much better for quantitative analysis than the intensity modulated display, but very extravagant in its use of film.

The day to day running of the station was in the hands of Jack Joisce. Each weekday Jack took the train from his home at Woonona down to Dapto, where he collected his bicycle (stored at the station), and rode out to McPhail's. For all the years that the station operated it was Jack who made sure that the equipment was running, that the aerials were tracking the sun, and that the film had not run out. We Sydney-siders normally spent three days each week at the field station, driving down and back by car. In fact I learned to drive on Paul Wild's car, a Wolseley. Initially he had me drive it across the paddocks at Dapto, and later I drove it down from Sydney. I suppose Paul gained some return on his investment in teaching me to drive. As a teetotaller I was sometimes asked to drive him back from the Dapto Christmas parties. These became annual events, with many invitees from the laboratory in Sydney making the trip down to Dapto to celebrate with various activities, including rides across the paddocks on a horse-drawn sleigh provided by McPhail.

After I obtained my license I too bought a Wolseley, but an older, 1948, model. Well that's when it was built, but it was the prewar squarish design like the English police cars. Such an old design that both the front and back doors were hinged to the central pillar, so that the front doors opened backwards. I was very proud of that British racing-green car with number plate YE 404. Henry Rishbeth christened it the 'Shahmobile' - a dig at Ron Bracewell's use of the Cyrillic letter *shah* in a paper that Ron and I were writing at the time. One day, coming down the hill from Mt Ousley on a trip to Dapto, I let the Shahmobile coast past the 30 mph sign and scored the only speeding ticket of my driving career, an event that I celebrated in a bit of doggerel I called 'Behind Iron Bars'.

A long road and a straight road
And Shahmobie takes it fast;
A downhill and a steep hill
To Wollongong at last.

But what is this beside the road,
This pole with disc and mystic code?
A three in black, and then zero -
Can this mean we should go slow?

The next car back doth drop behind
That seemeth odd - but never mind
It's fair to coast up this next hill,
We're way out in the country still.

Then suddenly beside the door
A gentleman who is the law.
With practised skill his book appears,
Would it help to shout some beers?

Forty some years later that last little rise at the bottom of the original Mt Ousley Road is still 'out in the country'. Nowadays the speed limit there is 80 km/hr (50 mph)!

The weekly three-day visits to the Dapto field station allowed Paul to see to general supervision, and provided a great environment for examining the observational results, and attempting an interpretation. At the same time Paul gently educated this new recruit on subjects such as *aerial temperature* and *available power,* in sessions over extended morning teas, taken sitting at a table out in the sunshine. Discussion would be accompanied by Paul repeatedly stabbing his teaspoon into his teacup, trying to dissolve all the sugar he had added! It was at one of those leisurely morning teas that Paul mused 'I wonder if Type III bursts really are caused by particles travelling out from the sun at close to the speed of light', which led to the first Radiophysics Publication including my name as an author, a letter to the journal *Nature*, with authors Wild, Roberts and Murray, dated December 14, 1953, and entitled *Radio Evidence of the Ejection of Very Fast Particles from the Sun.*

Back in 1947 when Ruby Payne-Scott, Don Yabsley and John Bolton first recorded an outburst which began at a higher frequency (200 MHz), and only several minutes later appeared at lower frequencies (100 MHz and 60 MHz) they had suggested the effect might be caused by a disturbance moving out through the *corona*, the outer atmosphere of the sun, at a speed of 500 to 700 kilometres per second. (Yes, kilometres per *second*, abbreviated km/s). The concept is somewhat like running your fingers down the keys of a piano, producing a sound that moves steadily downward in frequency. Like the strings of a piano, the free electrons in the ionized gas of the corona can vibrate. The frequency of their vibrations is determined by the *density* of free electrons in the gas, i.e. the number of free electrons in a cubic metre. At the base of the corona this vibration frequency, the *plasma frequency*, is around 100 MHz. As the density decreases steadily outward through the corona this frequency of natural vibration drops, so that some 400,000 km further out it has fallen to around 40 MHz. Hence if a disturbance moves outwards at a speed of 500 km/s, setting the coronal electrons vibrating as it goes, and if these vibrating electrons emit radio waves at the frequency of their vibration, then the frequency of the emitted waves would fall from around 100 MHz down to 40 MHz over the time it takes to travel 400,000 km. At a speed of 500 km/s that is 800 seconds, or about 13 minutes, which fits the observations.

That disturbances moved out through the corona at speeds of many hundreds, or even a few thousand, kilometres per second was not *so* surprising. It was known that magnetic storms and intense aurorae sometimes occurred on the earth a day or so after bright *flares* were seen on the sun. This suggested that these solar explosions ejected either a shock wave, or a stream of particles, which travelled from the sun to the earth at speeds of this order. Further confirmation of this interpretation had come from directional observations of the radio outbursts made by Ruby Payne-Scott and Alec Little at the Radiophysics Laboratory in the period from 1949 to 1951. During outbursts they recorded changes in the position of the radio source which were consistent with outward radial motions at speeds of 600 to 3,000 km/s.

Type III solar bursts are like outbursts (Type II bursts) but have a far more rapid frequency drift. To suggest that they have a similar origin is to suggest that disturbances travel out through the corona at speeds of 60,000 to 100,000 km/s, or one fifth to one third of the speed of light. In their discovery paper on Type III bursts, Wild and his collaborators discussed this possibility. It must have seemed a fairly way out idea: they mentioned it but didn't champion it. Now Paul pointed to two independent pieces of evidence that supported the idea that the sun produced such high-speed ejections. Firstly, the cosmic-ray intensity at the earth had been found to increase suddenly about 50 minutes after some very bright solar flares. This suggested that particles travelled from the sun to the earth at about one sixth the speed of light. The second piece of evidence came from the radio observations themselves. Paul had noticed that about 10 minutes before the onset of some of the strongest of the Type II *outbursts* there was an intense cluster of Type III bursts. He suggested that such compound events could be explained as the result of an explosion occurring low in the solar atmosphere that produced two types of ejections. A series of ejections travelling outwards at a substantial fraction of the speed of light would produce the cluster of Type III bursts, and a massive ejection travelling out at around 1,000 km/s would produce the Type II burst. The time gap between the two parts of the event would be explained if neither disturbance produced (meter-wavelength) emission until it reached the corona.

Paul had me examine questions such as whether particles of near-relativistic speeds would be able travel out through the corona, or whether they would be stopped by collisions with the ionized gas. We also considered how the particles might be accelerated, finding a nuclear explosion in the lower corona unlikely, and vaguely suggesting some type of electromagnetic acceleration, with the Type III's being caused by ejected particles, and the Type II by a following shock wave. Some years were to pass before directional measurements with the Dapto interferometer provided direct proof that Type III disturbances travel at these enormous speeds. Those observations were made just after I left the solar group in 1958: the results were sent to me to report at the Paris Symposium on Radio Astronomy. Later again, when spacecraft began operating in the sun-earth environment, electrons streaming from the sun at near-relativistic speeds were encountered shortly after the occurrence of Type III bursts. And as I write this in February 2000, more than 45 years after that Dapto paper, I hear warnings on the radio of possible damage to spacecraft as ton-weight clouds of charged particles have been detected being ejected from the sun at speeds of one third of the speed of light.

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While the heat and the light from the sun are very constant, the presence of sunspots, and their related *solar activity*, i.e. eruptions of solar flares, solar radio bursts, etc, varies greatly over the 11 year *solar cycle*. The sun had been very active at the 1949 solar maximum, but by the time that I joined the solar group in 1953 solar activity was waning, headed for the doldrums of the next minimum. In the years around solar minimum sunspots do appear occasionally, so a small telescope equipped with a dark filter was used to make a daily check, and when spots were seen Jack Joisce would set the Dapto equipment recording. But for much of the time around sunspot minimum Paul Wild used the Dapto equipment for another project, studying the scintillation, i.e. the twinkling, of the radio source Cygnus A.

In the next chapter I talk about this scintillation project, and about two other projects in which I was involved in the solar minimum years. Here I'll complete the story of my association with the solar studies at Dapto.

Pin-pointing the direction of arrival of radio waves, and measuring the angular extent of sources of radio emission, have been continuing challenges for radio astronomy. As I explain in detail in the next chapter, the ability of an antenna to resolve small angles depends upon it having an *aperture* that is thousands or millions of *wavelengths* across. While single radio antennas of very large aperture have been built, the largest radio telescopes use *dilute apertures*, i.e. only part of the aperture is filled by collectors. These radio telescopes consist of separate antennas distributed across an 'aperture' which can be kilometres, or indeed millions of kilometres, across. With such dilute apertures the radio waves falling in the gaps between the individual antennas are not measured, so such radio telescopes don't provide all the information that would be available from a *filled aperture*. But since the limit to resolution is determined by the extent of the aperture they have the same capacity to resolve small angles as filled apertures of the same overall extent.

A filled aperture - for example your camera lens, an ordinary optical telescope, or a single 'dish' radio telescope - has a *single beam*, by which we mean that the radiation arriving at one spot on the film, or one spot on your retina, or accepted by one 'feed' at the focus of a dish antenna, is radiation that came from a small range of angles spread about *one* particular direction. The spread of this 'beam' (the beamwidth) is determined by the width of the aperture of the 'telescope'. A dilute aperture, with gaps in the collecting area, has several 'beams', collecting radiation coming from several different directions. Each of these beams has the same width as the beam of a filled aperture with the same overall extent, so that small angles can be discerned. However if there are radiating objects in the direction of more than one of these beams then the picture will be confused.

The extreme form of a dilute aperture is a two-aerial *interferometer*, in which the signals from just two spaced aerials are combined. With such an instrument, the wide beam that would be produced by one of the aerials alone is split into a series of contiguous narrower fan-beams. The overall width of the wide beam is determined by the size of the individual aerials, while the width of the fan-beams within the wide beam is determined by the spacing between the two aerials. When this spacing is many times the width of the individual aerials then the wide beam is split into many narrow fan-beams.

An interferometer provides the easiest means of achieving a large overall aperture and hence the ability to resolve small angles. Obviously, if there are several significant sources of radiation in the main beam of an interferometer there is confusion. However when there is just one dominant source in the main beam, such as an intense radio burst on the sun, there is no confusion, and the potential of the instrument to resolve small angles can be realised. The Dapto interferometer was devised by Paul Wild to measure the movement of the sources of Type III radio bursts across the face of the sun. It used two of the rhombic aerials that were built out in the paddocks for the Cygnus scintillation studies described in the next chapter. Open-wire transmission lines, consisting of a pair of copper wires spaced about 11 inches apart, carried the signals from the aerials back to the equipment hut where they were combined and processed in one of the solar sweep-frequency receivers. (It was the perspex spacers on these lines that I mentioned earlier as having been improved by my memories from first year engineering.)

The electrical length of the 'cables' that convey the signals from each antenna back to the mixing point of an interferometer must remain stable to a fraction of a wavelength. Furthermore, to determine the *location* of the emitting source, rather than just the angular extent of the source, it is necessary to know the *actual electrical lengths* of the cables to this same accuracy. At Dapto we set out to measure the length of each transmission line by sending a signal out along the line, and having it reflected from a short circuit temporarily placed at the aerial. This set up a pattern of standing waves on the line and we measured the location of the minimum of the standing wave pattern in a piece of the line near the equipment hut. This gave us the length of the line apart from an unknown integral number of half-wavelengths. By repeating this procedure at several different frequencies (wavelengths) this ambiguity could be sorted out. I believe that modern surveying instruments use this principle for measuring distances: they examine the interference between radiated and reflected laser beams of different frequencies.

The standing waves were measured with a detector which could be moved along a section of the open-wire transmission line near the hut. For very good reasons this type of measuring device is known as a 'buggery bar'! I recall a period when Max Komesaroff and I spent many days attempting to make these measurements. After our first efforts we managed to borrow some walkie-talkies from the Radiophysics store, and there was much 'RP1 to RP2' between one of us putting the short on or off out at the aerial, and the other measuring the position of the minimum on the bar. At least the weather was warm and sunny most of the time! (We soon found that any early morning fog changed the electrical length of the lines quite drastically). We were left to make these measurements while Paul Wild spent about six weeks at Harvard, helping the radio astronomers there design a swept-frequency solar spectrograph akin to the Dapto one. I think Paul returned to find that we had not made much progress.

Another, and very direct, method of calibrating an interferometer is to observe a very distant source of radiation of known position. After some day-dreaming about using a radio transmitter on a weather balloon, it was decided to use a near-field source, and Keith McAlister designed a pole 100 feet-high to carry a small transmitting aerial. The pole was constructed of sections of aluminium tubing and was assembled lying on the ground at Dapto. Guy wires ran from several points along the pole to three short 'legs' which protruded at right angles from the base of the pole. As the pole lay on the ground two of these legs lay transversely on the ground, while the third leg pointed upward. A block and tackle was used to raise the pole, and once it was upright, other guy wires were connected to points on the ground. My doggerel verse tells the story:

Oh Cousin Joe, 'pon my word,
'Pears the folks they haven't heard
About the pole 100 high
Supposed to point up to the sky.

They haven't heard of how 'twas raised,
Of how the cows just stood and gazed,
They've not been told it stood up oke
For just two days, and then it broke.

But what about pole number two,
That went up straight, and good, and true
Until it neared the vertical,
And then it bent, and broke, and fell.

And dare we tell of number three,
Raised with many a knocking knee,
Of how it bent the wind before,
At last succumbed and stood no more?

And now they're planning number four
Of pieces left from those before.
Oh will this break their dogged luck,
Repay them for their honest pluck?
Oh will it reach the vertical,
Oh will it stand the winds of hell?

Perhaps they weren't the winds of hell, but the locality was named 'Wongawilly', apparently Aboriginal for 'Place of the Winds', and early on one of the huts had been blown off it's foundations!

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Since we are talking of interferometers I should tell of the period that Richard Twiss spent at the Radiophysics Laboratory in about 1957. You will recall that I mentioned meeting Richard in Cambridge days. Hanbury Brown, one of the inventors of radar, and the top radio astronomer at Jodrell Bank, was a friend of Richard's and when Hanbury invented a new type of radio interferometer he had Richard develop the theory of this device. In a normal interferometer, such as the one at Dapto, the signals from the two antennas are added *phase-coherently* at the mixing point. That requires that the cables connecting the signals to the mixing point remain stable in electrical length to a fraction of a *radio wavelength.* With long cables exposed to the elements this poses a severe problem. Hanbury Brown's *post-detection interferometer* had the great advantage that the signals could be mixed down to audio frequencies (i.e. *detected*) at each aerial, before being sent along the connecting cables to the mixing point. For audio frequencies the phase-stability of the cables poses no problem. While this post-detection interferometer doesn't give information about the *location* of the source on the sky, it does give information about the *angular extent* of the source.

When Richard Twiss produced the theory of Hanbury Brown's invention Hanbury asked if the idea would also work for light waves. Achieving phase-stable transmission of light over any distance poses a severe limit to optical interferometry; if Hanbury's technique worked for light it could have great advantages. Richard told me that at first he said no, believing that quantum effects would prevent it. However after further thought he realised that it would work for light, despite the quantum effects. He produced the appropriate theory and published it, and his paper immediately became a subject of contention between leading theorists. Despite Hanbury and Richard having published the results of a laboratory experiment that established that there was a correlation in the time of arrival of photons in coherent beams of light, and indeed having gone on to use the technique to measure the angular width of the star Sirius, there were still skeptics. So while Richard was at Radiophysics he and Alec Little carried out more tests on a light-bench in the laboratory to still the criticisms. Soon after this Hanbury Brown moved to the University of Sydney and constructed the Narrabri Stellar Interferometer which was used to measure the angular widths of stars.

Richard was obviously fairly well off and enjoyed the good life. He gave the impression of working because he liked working, not because he needed to earn a crust, and he had a fairly cavalier attitude to our public service hours. On a pleasant sunny afternoon he was liable to say 'Come on Jim, we can discuss this just as well down by the pool' and off we would go to the swimming pool in Victoria Park next to the University.

On occasions Richard also took me off to dinner, either in a restaurant or at his apartment where he was the chef. In either case the food was excellent. Before dinner he would insist that I consume several sherries, and then expect me to provide coherent discussion on some esoteric point of physics. It must have been as a result of one of these discussions that my name appears as the joint author of a paper with Richard. In the previous discussions of the effect that surrounding ionized gas would have on the emission from spiralling electrons (discussions such as those in my PhD thesis), it had been assumed that the radiation was emitted in the extraordinary mode of propagation (because the circular polarization of this mode rotates in the same sense as a spiralling electron). Now a paper had appeared suggesting that gyrating electrons would also radiate *ordinary* mode emission. By way of reply Richard produced a high-powered paper calculating the emission in the two modes, and put my name as second author. I protested that I did not even understand the contents of the paper, but Richard maintained that that since I had pointed out to him the need for the paper, I should be an author!

One of the stories told about Richard concerned the watering hole, the Lalla Rookh (###), which was just across City Road from the Radiophysics Lab. Some of the solar group were in the habit of adjourning there after work. The place was always crowded. In those days of six o'clock closing there were no seats and everyone stood around shoulder to shoulder, some trying to make sure they quaffed as much beer as possible before the call of 'Time Gentleman, please.' It appears that on this day Paul Wild's elbow inadvertently bumped the glass of another drinker, spilling his beer. Richard arrived during the ensuing gufuffle and was heard to enquire in his very English voice, 'Having trouble with the natives, Paul?' (Since Paul had come to Australia from England immediately after the second world war Richard could claim him as an Englishman, but Paul used to say that he was Australian in everything except cricket!)

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Solar activity increased as we entered the next cycle, and solar observations occupied more and more of our time. The films from each day's observations had to be inspected for any new types of activity, and for good examples of the known types, which were then subjected to further study. Prints of selected specimens were pasted into albums to produce a 'rouges gallery' of events. The International Geophysical Year in 1957 ushered in world-wide reporting of solar radio bursts - the reports being used to predict disturbances to the earth's ionosphere that could affect short-wave communication. This required up-to-date analysis of the Dapto films and the job of inspection was taken over by two young ladies.

New features in the spectra of both Type II and Type III bursts, and two new spectral types (IV and V) were recognised. Then we found an intriguing new spectral form which was named, not numbered: *reverse drift pairs*. These bursts, which sometimes occurred in storms, and were sometimes embedded in bursts of other spectral types, lasted only a few seconds. On the frequency-time plane of the film they appeared as two narrow, parallel ridges drifting rapidly from low to high frequencies, i.e. in the reverse sense to the drift in spectral types II and III. The best explanation we could suggest for these *reverse drift pairs* was excitation of the coronal plasma by high speed electrons shot *in*ward towards the sun. We showed that if the emission occurred at the *second harmonic* of the local plasma frequency then the second element of a pair could be explained as an echo from lower in the corona. I left solar radio astronomy over forty years ago; I wonder what has been learned about these events in the interim?

Several more examples of compound events were recorded, where an intense group of type III bursts was followed, after some minutes, by a type II outburst. To further test the idea that the two parts of these compound events had a common origin in an explosive event low in the corona, we looked for evidence that an optical *flare* occurred at the time of the compound burst. This led to collaborative studies with the group in the CSIRO Division of Applied Physics, led by Ron Giovanelli, who were making observations of the sun at optical wavelengths.

During these years we also agonised over the theory of the process by which Type II and Type III radiation was produced. The observations seemed to fit the idea that the emission occurred at the plasma frequency, the natural frequency of vibration of the ionized gas in the corona. Many Type II bursts contained a second, almost identical, drifting band at double the frequency. This suggested that emission occurred at both the fundamental and the second harmonic of the plasma frequency. The low degree of polarization of the radiation fitted with the idea of oscillations of the plasma; gyro radiation from spiralling electrons should have had substantial circular polarization.

Theory suggested that a disturbance moving through the corona would excite waves in the ionized gas, setting it oscillating at the plasma frequency. However then the problems appeared. These longitudinal-electric waves were not the sort of waves that could travel outside the ionized gas and so reach the earth. Furthermore, even if some process did produce the type of waves that could travel to the earth (transverse-electric waves), it seemed these would not be able to travel freely through the corona. Transverse-electric waves with frequencies at or below the plasma frequency cannot travel through an ionized gas. According to theory, radiation at the second harmonic of the plasma frequency could escape freely, but it seemed that, at best, radiation at the fundamental frequency might escape along a very narrow cone directed radially outwards. This didn't fit with the fact that Type II bursts *with twin bands of almost equal strength* were observed even when the sunspots responsible were seen to be near the edge of the solar disk, i.e. the sunspots were well round the curve of the spherical sun.

Ginzburg and Zelezniakov from the Lebedev Institute in Moscow found a way round these problems. They showed that 'scattering' of longitudinal-electric plasma waves by non-uniformities of the ionized gas can produce transverse-electric waves. When the non-uniformities are the plasma waves themselves, the scattered radiation contains a component at the second harmonic of the plasma frequency as well as at the fundamental. This theory appeared shortly before I left the solar group. I did not come to grips with it sufficiently to fully understand it, in particular to understand how it explained the escape of the fundamental-frequency radiation from the sun. I think that nowadays it is the accepted explanation for the Type II and Type III radiation.

As we approached the next solar maximum Paul Wild had me write up an overall account of the studies of the Type II radio outbursts. This used all the observations that had been made over the years at the Dapto observatory; it discussed all the known characteristics of the bursts, illustrating these with examples; and it compared the results with theory and with models of the solar corona. It seems that this review, my last work with the solar group, remained the basic reference on Type II bursts for some years.

In concluding this chapter it is worth commenting on the fact that astronomy of the solar system is often seen as peripheral to the mainstream of astronomical research. The sun is the closest star and one might expect it to be studied intensively. However, like your home neighbourhood, the solar system seems mundane, while more distant parts of the galaxy, indeed of the universe, seem more alluring. Would it be too cynical to suggest that the solar system is too close for comfort - here we fear that wild speculations can be ruled out by observation? An illustration of the low status of solar studies in the scheme of things is provided by the coverage given in a recent history of astronomy in Australia, *Explorers of the Southern Sky* by Haynes, Haynes, Malin and McGee. The work of the Dapto group, extending over 8 or 9 years, and including the discovery, and direct measurement of material being ejected from the sun at a third of the speed of light, is covered in two and a half paragraphs. And this despite the fact that two of the authors were research scientists at the Radiophysics Laboratory. It seems that the solar system is often more popular with the general public than with professional astronomers, a bias that has perhaps only been accentuated by the advent of space probes.

WHILE THE SUN SLUMBERED

During the period in the early 1950's when the sun was less active Pawsey had me write a survey article on radio astronomy. He had been asked to write an article for a journal called *Research*, published by Butterworths. Perhaps he didn't have the time, or perhaps he just thought it would be good training for me to write it; in any case he gave me the job. Producing that article taught me a great deal about radio astronomy, and also about writing. And Paul Wild provided considerable assistance with both of these aspects.

Present-day radio astronomers might be amazed to know that in the course of preparing that article I read nearly all of the published material on radio astronomy. Indeed it would have been quite possible to read *every* published paper. What a contrast to the situation today! My twelve page review appeared in October 1954. I think it provided a pretty good summary of the state of the subject at that time as seen from the Division of Radiophysics. However it was written by a junior, and so I was a bit startled a few years later when David Edge referred to it so extensively in the draft of the book *Astronomy Transformed* (ultimately published by Wiley in 1976 with Edge and Mulkay as authors). Of course, on reflection, I realised that my article presented the views of the Division. The content was strongly influenced by Paul Wild, and obviously Pawsey had vetted it carefully since it was he who had been invited to write the article.

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If contemporary radio astronomers ever hear the name J.A.Roberts it is as the junior author of the paper with the odd title *Aerial Smoothing in Radio Astronomy*; authors Bracewell and Roberts, manuscript received by the Australian Journal of Physics May 17, 1954. This was the second project set me by Pawsey during the sunspot minimum. He asked me to collaborate with Ron Bracewell who was writing up some fundamental ideas about the information available from an aerial system. In fact these concepts apply to any instrument used for measuring any type of electromagnetic wave; optical telescopes used to study light, X-ray telescopes used to study X-rays, radio telescopes, etc.

When we gaze at the sky at night the stars and planets appear to be just points of light. The moon has shape; it is a disk or a crescent; indeed we can make out some details of the craters on the moon's surface. But the stars and the planets are just points of light, and this despite their being in truth so much larger than the moon. This arises, of course, from their being more distant from us. As something is moved further and further from us it occupies a smaller and smaller angle, and it is this smallness of the angle, *not the faintness of the light*, that stops us seeing the detail of the planets and stars.

If we look at Jupiter or Venus through a small a pair of binoculars we see a disk, or a crescent, like a small moon; with slightly larger binoculars we can see the bands across

Jupiter. It is because the binoculars have larger *apertures* than our eyes that they can resolve the finer angular detail that our eyes cannot. In other words it is because the light enters the instrument through holes that are wider than the pupils of our eyes. The wider the aperture of an instrument the greater is its ability to resolve fine detail. Of course a larger aperture collects more light, *but that is not the cause of the improved resolution.* If you reduce the light entering your binoculars by putting a neutral density filter in front of them Jupiter will still be seen as a disk.

The aperture of our eye - the pupil - is only a couple of millimetres across. That means that our eyes can't resolve angular detail that is smaller than about one minute of arc across. (One minute of arc is 1/60 of a degree). It happens that when Jupiter is closest to the earth it subtends an angle of just about a minute of arc, and at such times people with good eyes can just make out the disk shape. But for most of us to see the disk we need the assistance of an optical instrument with a larger aperture, such as a pair of binoculars. Binoculars with an aperture of a few centimetres should reveal detail down to angles as small as 5 seconds of arc, i.e. about one tenth of the size of Jupiter and Venus when they are near the earth.

So the greater the width of an instrument the finer the detail that can be resolved. The largest present-day optical telescopes, eight metres across, in principle can resolve detail as fine as one hundredth of a second of arc. That is fine enough to show the closest stars as disks, rather than points. However, in practice, telescope images are blurred by irregularities in the earth's atmosphere, the same irregularities that cause stars to twinkle. When it comes to resolving the angular structure of stars the largest telescopes on earth are no better than a 30 cm telescope. That is why astronomers were so anxious to put a telescope in space, above the earth's atmosphere, where there is no blurring. Large telescopes on earth are built for their light-gathering power, not their resolution. Indeed, since there is nothing to be gained by building an instrument with resolving power better than a 30 cm telescope, the newest large telescopes don't waste money on a perfect mirror. Instead their mirrors consist of many segments. They have the collecting power corresponding to their size, but their ability to resolve small angles is much less. On earth much less direct methods of imaging, akin to those developed by radio astronomers, are being used to reveal the disks of nearby stars.

So far this discussion has been about light. But similar limitations apply to imaging the angular detail in all types of electromagnetic waves. In all cases the smallest angular structure that can be resolved by an instrument depends on the width of the aperture. But it also depends on the *wavelength* of the radiation being studied. The shorter the wavelength, the finer is the detail that can be resolved. The smallest angle that can be resolved is given by the formula:

> Divide the wavelength of the radiation by the width of the aperture
> (with both expressed in the same units, say metres).
> The answer is in radians.
> (A radian is about 57 degrees, 3,500 minutes of arc, or 200,000 seconds of arc.)

Light has a wavelength of about half a *micron*, i.e. half of one millionth of a metre. The electromagnetic waves that we call radio waves have wavelengths in the range from millimetres to thousands of metres, i.e. several thousand to several billion times larger. While an optical telescope with an aperture eight metres across can resolve angular detail in the arriving light waves as fine as one hundredth of a second of arc, if the same telescope were used to study metre-wavelength radio waves then all detail about the angles of arrival of the radio waves would be blurred out over many degrees. You see why radio telescopes are far larger than optical telescopes!

All this was known in the 19th century; the formula I gave above for the smallest angle that can be resolved is called the Rayleigh criterion, after Lord Rayleigh. However radio astronomers, faced with the huge blurring produced by their telescopes, wondered if there wasn't some way around this limitation. Radio astronomers were mapping the radio emission from the sky by scanning a radio antenna along a line across the sky, and recording the strength of the signal received as the antenna was scanned. Because of the width of the telescope beam, what they recorded when the telescope was pointed in a certain direction included radiation from a wide region around that direction. So they recorded a blurred image of the actual distribution of radio emission on the sky. But some argued that if the *shape* of the beam of the radio telescope was known with sufficient accuracy, then you should be able to undo this smearing mathematically, and so deduce the true distribution of radio brightness over the sky.

One of the people who believed this was possible, and who had worked out a method for making the corrections, was Kevin Westfold, a mathematician in the CSIRO radio astronomy group. Kevin, who had recently obtained a Doctor of Philosophy degree at Oxford, was a bit self conscious about being given the title 'Doctor' when people like Paul Wild and Steve Smerd were plain mister. So Kevin conferred honorary degrees on them and always addressed Paul and Steve as Dr Wild and Dr Smerd! (Because of the war, neither Paul nor Steve had had the opportunity to gain a higher degree. Later both were awarded Doctorates of Science on the basis of their published research in radio astronomy).

Kevin Westfold and John Bolton had used Kevin's restoration process to sharpen the detail in some maps of the radio sky that had been made at Dover Heights, and the results had been published. However there were those who believed that the original blurring process had destroyed all information about the fine detail, and that these restored maps were mere figments of the imagination. Ron Bracewell was one such, and he was writing a paper to prove this. The gist of his argument was that the pattern of radio emission over the sky can be regarded as made up of an infinite number of components (*Fourier* components), and that an aerial responds to only some of these components. The aerial has no response to the remaining components, the ones that contain the finer detail. Hence there are infinitely many distributions of radio brightness over the sky which all produce the same observed (blurred) distribution. These differ from one another by containing different *invisible distributions*, as Ron called them, distributions made up entirely of Fourier components to which the aerial does not respond. Ron showed that Kevin Westfold's restored image was just one of this infinity of possible distributions.

Ron was very familiar with the idea of representing a pattern as the sum of Fourier components. He had degrees in both science and engineering from Sydney University, and a PhD from Cambridge where he had undertaken ionospheric studies under the direction of Mr Ratcliffe. From his engineering studies, and from Ratcliffe's lectures, Ron had become proficient in the use of Laplace and Fourier Transforms for solving wave propagation and electrical circuit problems. He later wrote a very useful textbook on Fourier Analysis. By contrast I had had only one formal course in Fourier *series*, and had learned just a little of Fourier and Laplace *integrals* from Stratton's book *Electromagnetic Theory*

My contribution to the paper on *Aerial Smoothing* ('smoothing' meaning blurring detail) was as devil's advocate. For example, I remembered that I had learned in pure mathematics that if the values of a 'well-behaved' function were known over a part of its range, then there were methods for determining its values over its full range. So I argued with Ron that if an aerial measured some part of the Fourier Transform of the sky distribution with sufficient accuracy, then it must be possible to extrapolate the transform and so obtain the *invisible components*. Ron's reply: 'So if you measure the shape of the surface of a road in the middle part of the road, you can predict the gutter?' He didn't believe that real-life shapes were 'well-behaved functions' in the mathematical sense.

There was a lot more discussion along these lines with points of disagreement between us being argued out. For example, I think we finally agreed that an aerial is not *completely* blind to the so-called invisible components, but they are recorded so weakly that they are lost in the noise. As a result of these discussions I think matters were explained more clearly in the paper, and presented in a more compelling manner. So Pawsey's action in saddling Ron with an initial non-believer as co-author was really quite successful.

Some of the ideas in this paper weren't completely new, but they were set out more clearly than previously. The fact that the fine detail in the brightness distribution was irretrievably lost was clearly stated. There was also some discussion of what might be acceptable methods for guessing some of the missing information, i.e. extrapolating the measured Fourier components to 'recover' the finer detail. In later years, with the development of *synthesis radio telescopes*, guessing the missing Fourier components became top priority, and led to the development of algorithms such as *Clean* and *Maximum Entropy*. While Ron wasn't involved in those developments, he did write extensively on 'aerial smoothing' in later papers. But this paper with Ron was my only contribution to the subject (apart from later editing the proceedings of a conference on these matters). I was definitely the junior author of Bracewell and Roberts, and had no knowledge of the subject before I started questioning what Ron was saying, and so became his co-author. So I was surprised a few years ago when Ron sent me a draft of an historical note that he was writing, in which he contended that I had deleted an appendix from the paper after he had left to take up a position at Stanford University. I assured him I had no memory of this, and that it seemed an unlikely story since I was very much the new boy round the lab at that time.

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In the years around sunspot-minimum Paul Wild used the Dapto sweep-frequency equipment to study *scintillations,* i.e. the minute-to-minute variations in the strength of the signals received from some of the 'discrete radio sources'. When radio astronomers discovered that these variations were different at two well-separated observing sites on the earth, they concluded that the variations were like the twinkling of the stars, and were caused by the earth's atmosphere. In the radio case the culprit is the ionized upper-part of the atmosphere, the ionosphere. Non-uniformity of this region bends the radio waves so that they converge or diverge, thus increasing or decreasing the strength of the received signal. Scintillations were studied to learn about the ionosphere. Present-day radio astronomers are not greatly interested in the ionosphere, but for the radio astronomers of that era, who worked at metre wavelengths and often had a background in ionospheric studies, it seemed natural to use this opportunity to learn about the ionosphere.

The Dapto project studied the scintillations of the radio source *Cygnus A*, the strongest of the southern discrete sources. However Cygnus is a thousand times weaker than the intense solar bursts, so that larger aerials had to be built for the project. Each of the new aerials was a horizontal diamond, or *rhombus,* of copper wire, with sides 30 metres long, the wires being stretched between wooden poles 4 metres high. That height ensured that *interference* between the radiation received directly by the aerial, and the radiation reflected from the ground, would give the aerial a maximum sensitivity at an elevation of 15 degrees. This was the elevation of Cygnus during the two hours around transit when the observations were made.

When the non-uniform ionosphere deviates the wavefronts of the incoming radio waves the result is to produce areas of strong signal at some places on the ground, and areas of weak signal at other places, and these areas move and change. So that this pattern of signal-strength across the ground could be studied, three aerials were built, each located about 3/4 km from the equipment hut, and more or less at the corners of McPhail's property. From analysing the first records from these three aerials, Paul had concluded that the patches of strong signal on the ground were elongated north-south. To check how far they extended north-south he planned to make some observations simultaneously with the equipment at Dapto and with a portable receiver located some distance north of Dapto. And that leads me to talk about a 'school' that Pawsey organised.

Pawsey was a great physicist with a real feeling for the subject. But he wasn't a 'theoretician'. In fact he was somewhat suspicious of theoreticians. So it is interesting that at this time there were far more theoretically minded people in the radio astronomy group than there were in later years. I have already mentioned Kevin Westfold, a true mathematician, later prominent in the mathematics faculty at Monash University, and ultimately, I think, Vice Chancellor there. Steve Smerd undertook many theoretical calculations; in the early days in collaboration with Kevin Westfold. I have memories of Steve using his Brunswieger mechanical calculator, whizzing the handle round to add or multiply, then whizzing it backwards to subtract or to do division by trial and error. Paul Wild, of course, was theoretically inclined; I hear that in retirement he is still working on a unified field theory. Ron Bracewell was much interested in ideas and more competent in maths than I, and of course I had come to the Division as a theorist.

Well to give some of these theoretical types a taste of the real world, Pawsey ran a 'school' in practical matters. John Murray taught us something about receivers and amplifiers, Pawsey himself talked of aerials (his speciality), and there were other teachers whom I've forgotten. In addition we were each allocated a practical project, mine being the construction of an aerial to be used with the portable remote receiver for Paul Wild's Cygnus scintillation observations. Since these observations were to be made at a single frequency (45 Mhz), there would be no loss of lose sensitivity by time-sharing with other frequencies, as occurred at Dapto, so the antenna could be much smaller. I think it was a dipole with a single reflecting element, or perhaps a small *Yagi*. From designing and adjusting this aerial I learned much about a *Pawsey stub*, and how to adjust it so as to provide a balance-unbalance conversion from a dipole aerial to coax, and at the same time provide an earth-isolating support for the aerial.

The remote station for the scintillation project was a 'blitz buggy' with the aerial that I had organized mounted on top, and inside a 45 Mhz receiver with its output connected to an Esterline-Angus chart recorder. The truck was parked near the township of Appin, north of Dapto, and each time that recording of Cygnus took place at Dapto someone was stationed in the van at Appin to make recordings there also. Cygnus was transiting at night, and it just so happened that the 'Ashes' cricket tests were taking place in England, in the Australian night-time. That meant that the cricketing fans, Paul Wild, John Murray and Bill Rowe, could sit in the van at Appin and enjoy the cricket broadcasts whilst running the receiver. I wasn't a cricket fan, and I was much less attracted by the cold night-time stints at Appin. And now, looking back at the paper that we published, I am surprised to see that we make no mention of those Appin observations! I suppose we must have found that the separation from Dapto was greater than the extent across the ground of the patches of strong signal.

While the spaced-receiver observations didn't reveal much about the scintillations that was particularly new, those observations were really a sideline. The main purpose of the project was to use the Dapto equipment to study the *spectrum* of the scintillations, the variation of the strength of the scintillations across the frequency band. For the first time the signal from a scintillating source was recorded essentially simultaneously across a wide range of frequencies, and that turned up some unexpected features. When the scintillations were strongest the film records showed black 'ridges' sloping right across the film. So the signal was strong across the whole range of frequencies from 40 to 70 Mhz. Paul Wild argued that such systematic behaviour implied that the non-uniformities of the ionosphere causing the scintillations must be orderly structures, not *irregularities*. The current theory attributed the scintillations to completely irregular, 'random', structures in the ionosphere, and assumed that the signal received at any one point on the ground consisted of waves that had been deviated by *numerous* irregularities. Such a theory could not explain these broad-band systematic patterns in the dynamic spectra.

Wild argued that the radiation producing each strong 'ridge' must come predominantly via a single non-uniformity which 'focussed' like an imperfect lens. In fact he proposed that the inhomogeneities were density-waves travelling through the ionosphere, and that each wave-trough acted as imperfect, one-dimensional, lens. (In an ionized gas *troughs*, rather than *peaks*, cause focussing, because, unlike glass, the refractive index of an ionized gas is less than 1.) To prove his point he made calculations of the pattern of intensity expected below a wave-pattern of ionization. Of course there were no desktop computers in those days. The values of the Fresnel integral required for these calculations were read from a large Cornu spiral which Paul drew on a piece of cardboard. Jack Joisce spent many days reading values off the spiral. The calculations were very successful, and they reproduced many of the details that were found in the observed spectra.

Most of the strong ridges in the frequency-time maps sloped across the frequency range. In other words there were small, systematic delays between the times of the maxima occurring at the different frequencies. Paul ascribed this to bending (*refraction*) of the waves by a larger-scale (longer-wavelength) density wave which acted as a prism and deviated the waves by different amounts according to their frequency (called *dispersion*). (You will recall that Newton's demonstration that white light is composed of all the colours of the rainbow depended on dispersion. He split white light into its constituent colours by using a prism to deviate the different colours, i.e. frequencies, by different amounts.) The Dapto records contained numerous examples showing a series of strong ridges sloping one way across the frequency band that slowly changed over to slope the other way, and then after a period changed back to the original slope. This is just what would be expected if the smaller-scale ionospheric waves causing the focussing (the 'lenses') were riding on larger-scale waves causing the (dispersive) refraction (the 'prisms').

Additional support for these ideas came from another feature that was sometimes seen in the dynamic spectra. There would be a series of narrow, somewhat curved, and approximately equally-spaced ridges sloping across the records, followed immediately by a similar series of narrow ridges, but of the opposite slope. Because of their appearance in the frequency-time pictures Paul called these features 'ribs'. The whole 'rib' pattern occupied about the same time as one of the examples of the wider ridges going through a cycle of one sense of slope followed by the other sense of slope. Paul's interpretation was that in these cases the bending caused by the larger-scale ionospheric wave that acted as a prism was so great that the radio waves coming through the two sides of the 'prism' overlapped on the ground and *interfered.* The pattern was typical of the classical interference pattern produced by a *Fresnel bi-prism.*

I explained in the last chapter that two of the Cygnus scintillation aerials were used to form an interferometer which was later used to measure the movement of Type III radio sources across the face of the sun. At the time of the scintillation project this interferometer was operating in a rudimentary way and the interferometer records provided confirmation of some of the ideas sketched above. The fringe-pattern from the interferometer showed that as one of the strong ridges drifted past the waves arrived first from one side, then from straight ahead, then from the other side, just as would be expected for waves focussed by a lens. Unfortunately a more detailed analysis of the interferometer patterns never came to pass. A publication with Wild and Roberts as authors, and meant as Paper I, gave a description of the scintillation observations, some examples of dynamic spectra, and some discussion of the implications. I was to produce Paper II, reporting and discussing the interferometer results, and presenting Paul's theoretical calculations. Other matters intervened and this second paper was never completed. As a result neither Paul's calculations of the focussing by a density wave, nor the interferometer results were ever published. Perhaps this has contributed to the fact that what I believe to be important conclusions from these observations never seem to have been accepted by some of the other workers in this field.

SEVEN YEAR ITCH

In my case the itch started after five years. The post-war recovery of the 50's was producing a new attitude. There was more money about. Travel was in the air. Many of my friends from the Youth Hostel Association were throwing up their jobs and taking off to London to work for a year or two while they 'did' the Continent. Cambridge had opened my eyes to the world beyond Sydney, and I wanted to see more. And radio astronomy provided great opportunities for travel.

I didn't have to throw up my job. CSIRO granted me two years of unpaid leave to work at Caltech, the California Institute of Technology in Pasadena. CSIRO saw such an arrangement as a means of increasing the skills of their staff. And what made it even better was that I was awarded a Fulbright travel grant which paid my fares to California. Senator Fulbright was a clever man. After the war the United States acquired a lot of foreign currency by selling off the war-surplus equipment that it had left behind in Allied countries. Then Senator Fulbright persuaded Congress to use this money to finance visits to the US by nationals of those countries. It proved a marvellous way to promote the US in the eyes of its Allies.

Why Caltech? Well the story really starts pre-war when radar was developed in Britain with Taffy Bowen as one of the major players in the development, a contribution that was acknowledged by the British Government's post-war award shared by Watson-Watt, Taffy Bowen and Hanbury Brown. In 1942 Taffy was a member of the Tizzard Mission which carried British wartime secrets to the USA; as Taffy told it, he crossed the Atlantic carrying one of the Birmingham-developed *magnetron* transmitting valves in his coat pocket. He stayed for a couple of years at the Radiation Laboratory at the Massachusetts Institute of Technology, and as a result came to know many of the leading figures in research in the physical sciences in the US. In particular he became friendly with Lee DuBridge, the head of the MIT Rad Lab. Then, near the end of the war, Taffy moved on to Australia to become the chief of the CSIR Division of Radiophysics, and so at war's end it was he who guided the Radiophysics Laboratory into its peacetime activities. One of the activities to which the ex-radar specialists turned their attention was, of course, radio astronomy, which prospered under Joe Pawsey's leadership.

In Britain at the end of hostilities many who had worked on radar similarly turned to radio astronomy. However in the US, for the most part, this didn't happen. The leaders of the American radar effort had held university positions pre-war, and at the end of the war they returned to those positions and resumed research in their earlier chosen fields. As a result, when radio astronomy developed rapidly after the war, the US was rather left behind. In an effort to remedy this state of affairs a series of meetings were held in the US in the early 1950's. Taffy Bowen, a wartime colleague of many of those involved, and head of one of the world's major radio astronomy research centres, was included in these discussions. One of the outcomes of the meetings was the establishment of the US National Radio Astronomy Observatory (NRAO) in West Virginia. Another was a decision by Caltech, already involved with the Mt Wilson & Palomar Observatories, and with NASA's Jet Propulsion Laboratory, to add radio astronomy to its fields of interest.

It happened that the President of Caltech at this time was Lee DuBridge, the wartime head of the MIT Rad Lab. No doubt the friendship that Bowen had established with DuBridge at MIT contributed to the decision by Caltech to import someone from the CSIRO Radiophysics Laboratory to establish their new radio astronomy observatory. At Taffy's suggestion John Bolton was given the job, and John went to Caltech early in 1955. Three years later when I was considering an overseas sojourn, Taffy suggested that he arrange for me to join John's group at Caltech, a move which would broaden my experience beyond the study of *solar* radio emissions. And that's how I became a research fellow at Caltech for two and a half years. And what a grand couple of years they were!

I travelled to the United States via the Soviet Union: to say the least, a rather unusual thing to do at that time and sufficiently unusual to earn a visit from the CIA when I reached the States! However all the CIA asked me was whether I thought that publications in Soviet scientific journals gave a proper impression of what was happening in Soviet science. As if I could tell.

My trip to the US was timed to coincide with the first ever international symposium on radio astronomy, held in Paris from July 30 to August 6, 1958. This was associated with the Tenth General Assembly of the International Astronomical Union which followed in Moscow, from August 12th to 20th. I arranged to attend both these meetings, and then proceeded to plan a holiday around these events. I had loved Bavaria and my train trip from Innsbruck to Switzerland in 1950. I had also been much impressed by what I had been told of the Dolomites around Cortina d'Ampezzo by a Youth Hostel friend, Bruce Paterson. He had cycled (!) through the Dolomites and had shown marvellous slides of the area, accompanied by his own verse. And I had a friend in Oslo. Then Paul Wild suggested that I include some visits to observatories, so that I ended up leaving Sydney early in June of 1958 on an astronomical/pleasure tour.

The trip started with a four week sea voyage to Marseilles on the P & O ship SS Strathnaver. This was a one-class ship, which had appealed to my sense of values. In practice it meant that there were 1,000, rather than 500, passengers to mingle with; people you spoke with one day might not be seen again for a week. My letters home, which Mother preserved, show that I was again impressed by Perth. The view from Kings Park, looking over parklands, and the blue waters of the river, delighted me. I was impressed by the new roads being built with clover-leaf crossovers; there was nothing like that in Sydney! Then I really let myself go: 'As we left Fremantle I was reminded of the opening paragraphs of Conrad's "The Secret Sharer". The tugs turned us around in the river ... and then we very slowly slid down the harbour and out through the gates and then paused to put off the pilot. It was a calm, warm evening and we too appeared to stand at the beginning of our voyage and to measure ourselves up against what lay ahead.'

Initially what lay ahead was a blue, blue sea stretching for ever and ever and as calm as Sydney Harbour. Then we developed a steady roll 'which was quite good fun - particularly at the dances you had to exercise your ingenuity to make use of the sloping deck and mind you didn't go whizzing for the tables and chairs.' Before we reached Colombo the blue sea became very grey. We ran into the NW monsoon which was with us until we reached the Red Sea. And 'ran into' is the appropriate phrase as we were head-on to the waves. The bow of the ship would rise up on top of one wave and then plunge down, digging under the top of the next wave, sending great sheets of water over the ship. The whole place was awash. Then suddenly one day the ship was much calmer. In fact the ship had almost stopped while the crew removed debris from the decks. Before the bow had come up from under one wave the next had crashed on top of it. One lifeboat was smashed in two, and there were 44 gallon drums and bits of ship's rail all over the place. After that we proceeded at a slightly more sedate pace.

The highlight of the voyage was to see Cairo and the pyramids on a side trip taken during the day that the ship spent going through the Suez canal. The 90 mile car trip from Suez to Cairo gave me my first real sight of desert. 'Just a bitumen road running across miles of barren yellowish rocks and stones. Barren mountains to our left - a far horizon to our right. For a lot of the time there was no vegetation whatsoever, but at times there were little tufts of scrub or grass and occasionally ... leafless tress. This continued to within 10 miles of Cairo centre.' I was most impressed by new Cairo, and by the hotels where we had morning coffee (Heliopolis Palace) and lunch (Shephards). The simplicity of their architecture, the expanse of smooth, light coloured walls with a minimum of decoration, and a feeling of spaciousness and coolness was a wonderful contrast to the heat and dirt outside. The Cairo museum, with one whole section devoted to Toutankhamen and his gold coffins, was an eye opener.

Then marvel of marvels to see the pyramids and the Sphinx. 'For the last stretch we mounted camels and did the thing properly. Lots of fun - even had a trot!. ... It was a tremendous thrill to look up at the Sphinx, the pyramids and all the camels and camel drivers coming down in a cloud of dust. What a thrill to realize that I am really seeing all this. It is like a wonderful dream!' The pyramids 'are not way out in a sandy desert as I imagined. They are on the edge of the irrigated lands - where the irrigation stops, the dirt and rock begins. But the pyramids are only 10 minutes by slow camel from the green area.' I was also very surprised by the magnificent Australian gum trees that lined many of the roads. 'The gums look marvellous - they are not the dark, uninteresting box trees that we use in the city streets, but huge-trunked trees with all the colours of the peeling bark and the long thin drooping leaves.' And the train trip from Cairo to Pt Said was another eye opener as we travelled for hours down the rich, irrigated delta of the Nile. 'Green, green, green.'

Leaving the ship at Marseilles, I took a fast diesel rail-car through Aix to Manosque, and then a taxi to the observatory at St Michel. 'The countryside was thick with lush green grass; there were grapes and peaches growing vigorously...Everything gave the appearance of a great richness of the soil and a bumper harvest. Indeed this appearance continued right through Italy, and southern Germany up to France....men were playing bowls in the street - as they seem to do throughout France on any vacant spot of flat ground...I shall not quickly forget...views of fields of lavender (for scent), and haymaking...' While I enjoyed my visit to the observatoire, I wished I could speak French!

Then to Nice, and a marvellous train ride to Italy round the coast 'where the mountains come right down to the sea and the houses are perched up the hillsides...very picturesque because of the pastel shades of the countryside...This is where it beats our scenery'. But I decided that while our coastal scenery lacked this delicacy of colours, our cliffs and beaches were more magnificent! So to Florence where I was fascinated by the detail in the biblical scenes raised in bronze on the door of the Baptistery, by the fine carving in stone seen in many buildings, by the dominance of Il Duomo, and by the serenity of a sunset seen from the Old Bridge looking up the Arno. I also commented that I enjoyed many cappuccinos and that the gelato was really marvellous! However, peasant that I am, 'I found the city proper most unattractive - dirty, dusty buildings built right on the street and being kept dirty and dusty for the tourist trade. I don't understand why they can't pull down most of them and make parks and gardens to set off the really interesting buildings. A piece of greenery in Florence would be wonderful!'

'Venice is wonderful. All Europe - indeed all the world - was on holiday there. Happy people everywhere. St Mark's Square (Piazza San Marco) at 11 pm is like the Zoo on Boxing Day. People, people, people - just promenading, listening to the orchestra or sitting at an outdoor cafe. The weather's been splendid. By chance I was at Venice on the night of the annual Fiesta. Thousands of decorated gondolas converged near the Grand Canal and there was a magnificent fireworks display. On Sunday afternoon Tom Smith (from England) and I went to the Lido for a swim - just for the fun of it. At night I went to hear an orchestral concert in the open air of the courtyard of the Doge's Palace. (The peaches are just as good as last time. I just had one to wet my whistle). Brahms really sounded wonderful in the cool of the evening.'

Then, from Bolzano, 'Today I have seen magnificent views in the Dolomites. It was a wonderful day. Rugged white rocks rising to 10,000 ft - on one pass the road went to 7,000 ft - we could almost touch the bare rock peaks they seemed so close. And the ground was a carpet of vivid green grass filled with wonderful flowers - really beautiful small flowers. ... You felt you wanted to get out and hold all this delicate beauty in your hands - and such a contrast to the gigantic, massive, rugged mountains'.

Cloudy weather meant that Innsbruck, and the train trip through to Basle, lacked the magnificence of my 1950 memories. 'But in due course I reached Freiburg-im-Breisgau in the early hours of the morning and a kind railway man helped me carry my ports to the nearby Minerva Hotel.'! In Freiburg, Hannes Raben (who had spent some time with the solar group at the Radiophysics Laboratory in Sydney), and Professor Kiepenheuer, the director of the Fraunhofer Institute, made me feel very much at home. I spent the days at Freiburg hearing about their recent work in radio astronomy, and the evenings socialising in many languages. 'Most of the conversation was in German, but they would occasionally translate a joke, and they sat me beside a girl who spoke very good English. I have been surprised by the degree to which English is the universal language. At big railway stations on the Continent announcements are made in many languages (including English) and well educated people (except the French) seem to speak English as well as several other languages.'

During the World War II occupation of France, the German government had placed Kiepenheuer in charge of the Paris Observatory. Kiepenheuer's friendly personality, and the universality of science, apparently overcame the problems of working for the enemy. Roger Gallet, a Frenchman who had worked under Kiepenheuer in Paris, had later married a girl from Freiburg. Roger now lived in the US, but was visiting Freiburg when I was there and I reported: 'Gallet tends to speak French and Kiepenheuer would talk away to him in French - then his sister-in-law would say something in German, and the conversation would go on in German - suddenly they'd remember me and Kiepenheuer would swap the conversation into English.'

Freiburg is on the edge of the Black Forest and the Institute's observatory was at Schauinsland. Nearby in the forest there was a cosmic-ray observatory and one day Roger Gallet took me to see it. However en route he discovered blau beeren (blue berries) growing wild, and that was the end of the cosmic-ray expedition. We picked and ate blau beeren! On the Saturday I took a bus trip to Strasbourg where I marvelled at the figures carved in stone across the front of the cathedral. I was also intrigued to see the clock, since a copy of this clock was one of the few things of note in the Technological Museum in Sydney. Then on the Sunday I took the (electric) train to Titisee, a lovely little lake in the Black Forest, where I walked between the ever-so-orderly pine trees. Such a contrast to our bush! I was rather disappointed at the lack of a fairytale atmosphere.

And so to Paris. The Paris Symposium on Radio Astronomy was a landmark in the early post-war history of radio astronomy. Joe Pawsey, the leader of the radio astronomy group at the CSIR Radiophysics Laboratory, was the chairman of the organizing committee, which indicates Australia's status in the subject at this time. The joint sponsorship of the symposium by the International Scientific Radio Union (URSI) and the International Astronomical Union (IAU) tells something of the history of the acceptance of radio astronomy by the astronomical community. Initially radio astronomy was not understood by optical astronomers and was left to the radio science community. Over the years this attitude changed gradually, so that today radio astronomy is definitely astronomy and only the technical aspects find a home in the Radio Union.

All aspects of radio astronomy were covered in sessions of the symposium, and I would have liked to be able to attend them all. In the sessions on the Sun, I reported the work on Type II radio bursts that I described in a previous chapter. I was also a joint author with Ron Giovanelli of a paper he presented which described optical events on the sun at the time of Type II radio bursts. On behalf of Paul Wild, Kevin Sheridan and Gil Trent, I presented a paper reporting direct measurements of the movement of Type III radio bursts through the solar corona, showing that they moved at speeds of about 100,000 km per second, or one third of the speed of light. These observations, which used the newly completed Dapto interferometer, had been made after I had left Australia.

I found the conference very stimulating, and there was a very pleasant and friendly atmosphere. Since I knew of the work of many of the solar people, and my name was known to them, there was much to talk about, and many results to compare. It was also great to re-meet friends from Cambridge days, and to meet new friends. 'It has been particularly interesting to talk to Fred Haddock (University of Michigan) and Thompson from the Texas station of Harvard. Both of these people have radio spectrographs somewhat like the Dapto equipment. In fact they and others have asked me to come and visit them in the U.S. and have even offered financial assistance. In some ways it would have been quite good to go to Michigan instead of Caltech - but the advice to broaden my interests away from the sun is probably very good. (Also the climate is much more pleasant!). Also talked quite a lot with Vitkevich, the main Russian observational radio astronomer, and I'm looking forward to seeing his station in the Crimea. He also has a type of radio spectrograph.'

The French were by then one of the leading nations in solar radio astronomy. 'I was very much impressed by the French group - both by their work and their friendly attitude. We had lots of useful discussions - en Englais!' During the Symposium we visited the radio observatory at Nancay, and I recall a very French luncheon with many people around long tables and the (then) very un-Australian practice of much wine. 'I remember when Paul Wild saw it [the Nancay field station] in 1955 he commented that they put up a chateau (the living quarters) first and then started to think about aerials and equipment. Now the equipment is very impressive - and so is the heather! Fancy coming to France to see heather! I had no idea it was so brilliant ... I don't wonder the Scotch rave about it.'

After the Paris Symposium I had a day at the World Fair in Brussels and then took a KLM charter flight from Amsterdam to Moscow for the International Astronomical Union's Tenth Congress. A paragraph in my letter from Paris: 'The international situation now seems calm enough for the Moscow trip. A while back we all wondered. We have four Russians here at the Congress.'

The World Fair had as its centre piece the Atomium, 'a molecule magnified about 100,000 million times. Each "atom" was a polished aluminium sphere about 40 ft or so in diameter. There were about ten of them arranged with links to form a molecule about 100 m high. ...

By night lights blinked from numerous holes in each shiny sphere and you could philosophise on the relationship of the stars and the atoms!' I also noted that 'the British exhibit made a spectacle of British Royalty, ... and had an exhibit of Radio Astronomy.' Radio astronomy also featured in the French exhibit. The underground train that connected the centre of Brussels to the airport also merited comment, and 'I was most impressed by the use of closed-circuit T.V. to relay information to many screens scattered around the airport. Evidently they had a large blackboard at some central point and this was continuously photographed. They simply wrote the information on the board and everyone could read it.'

Our flight to Moscow was in a chartered KLM Convair. 'The Dutch crew were very nice. They had flown to Moscow once before as KLM have just begun a regular (once a week) service to Moscow.' We had a 45 minute stop at Visby, on the island of Gotland in Sweden, and then 'although we flew at 16,000 ft with clouds below us the crew had to collect our cameras to comply with Russian regulations!'

In the Soviet Union the conference delegates were the guests of the Soviet Government, who provided everything free of charge. 'We were greeted at the airport by a deputation of astronomers. There were TU 104 jets all over the place... I was struck by the fact that the greeting party and interpreters were not "in their best suits". Most ... wore open necked shirts and did <u>not</u> look clean and smart. This is typical of Moscow and Leningrad people.' At the hotel that night a band 'played very loud music of the jazz type. But the amazing part was to see these chaps in their scraggy clothes - they looked as though they were dressed for outdoor work - sitting up playing amongst the marble columns of this posh hotel! Yes, the waiters wore white jackets!'

On the drive through flat country from the airport to Moscow: 'Suddenly the road was lined on both sides by colossal blocks of flats either in the process of being built or completed. I can't convey to you the astonishing appearance of all these buildings. For about a mile, each side of the road was lined with these huge blocks of home units - about 6 or 8 stories high - all the same height - and each block as long as a city block. Furthermore there were more blocks of flats behind these. ... The amazing thing to me is that there are so many people to be housed. Where did they live before?'

Well later we saw where some of the people being re-housed had lived: in dilapidated old wooden houses that were being torn down to make way for these new buildings. These construction areas were liberally 'decorated' with what I had come to call the 'symbol of Europe, 1958'. This was a crane in the form of a tall box-girder tower, with a horizontal arm pivoted at the top, and a cable dangling down from the far end of this arm. I had first noticed this type of crane in towns along the coast near Genoa. 'They are all over Europe and here in Russia they appear in fantastic numbers on all the housing projects.' The Moscow construction sites differed in one significant way: women worked as navvies. There was much equality of the sexes, with women prominent in work mostly reserved to men in the west, whether labouring or astronomy.

'Sidelight on the news: As we drove in to Moscow a truck carrying soldiers passed us and several had bunches of wild flowers stuck in the ends of their rifles where the bayonet fits!...Before saying anything else I must emphasise how absolutely normal everything is on this side of the "iron curtain". As we walked out from the restaurant just now and gazed round the magnificent entrance hall of the hotel we said just how extraordinary it all was. Here we were leading a perfectly normal (?) life, in a magnificent hotel - but we are behind the iron curtain.' In the 29 storey Hotel Ukraine, one of the seven elaborate wedding-cake skyscrapers, 'I share a room having radio and private bathroom with Pawsey. The only complaint about the hotel is just a complaint about the way of life of these people. Time means nothing to them. We literally wait, on the average, 7 minutes for the lift to come. (No, there are no stairs. Or rather, they are locked.) Getting meals is the same - dinner to-day took one and three quarter hours. The only thing to do is relax and live the way they do - but it is a shame our time in Moscow is so short!'

Before the conference began we were whisked off for a trip to Leningrad, going overnight by train in first class sleepers. On the train, at night and in the morning, we were served tea in glasses held in silver holders. Later, in Yalta, I bought a couple of these silver holders; they are still a prized possession. 'The most amazing thing about this trip - plus all the sightseeing in Leningrad since - has been the way we have travelled without tickets. Our guides arrange everything and we just walk through!' I found it odd to have no money in my pocket.

In Leningrad the Hotel Astoria was luxurious. 'The landing on this floor is amazing. It is decorated with two beautiful vases (about three feet tall) in enamel of various colours. I guess they are worth a fortune. Then this room has a writing desk with marble ink stand and marble-backed blotter; the chairs are "antique"; the pillow on the bed is covered with a lace throw-over; - by contrast the toilet facilities are not 100%, but they're quite OK. Apparently wash basins are never equipped with a plug or other device to keep the water in. Clothes washing is somewhat of a problem.'

It was a short walk from the Leningrad hotel to the Winter Palace (the Hermitage Museum) and those among us with an interest in art were, of course, spell-bound. While I have only vague memories of the Hermitage, I often recall the visit to the Summer Palace (Peterhof) with it's golden domes and spires glittering in the sunshine, and its statues which were being regilded with gold leaf. I was left wondering at a revolution that preserved all this rich man's grandeur; it seemed so out of character with the view of the Soviet Union promoted in newspapers at home. And Peter the Great's playful fountains were all preserved. One, in the form of a mushroom about 8 feet high, had water sporadically cascading down over the roof to form a circular wall of water. The fun was to hop in and out at times when the water stopped. Afterwards I saw a chap sitting behind the shrubbery turning the tap on and off!

We visited the famous Pulkova Observatory near Leningrad, and saw their unique radio antenna nearing completion. Ninety eight pillars were arranged around an arc of a circle of radius 100 m. Attached to each pillar there was a plate 3 m high and 1.5 m wide. Adjusting screws allowed each plate to be moved and tilted so that the surface formed by the 98 plates could be made to closely approximate a section of a paraboloid. In this way radio waves coming from a specified direction in the sky could be focussed into a 'feed' horn placed at the focus of the paraboloid. The setting of each plate was adjusted by hand, with a team of students continually resetting the plates during observations.

The trip to Peterhof and Pulkova provided a delightful example of the lack of organizational efficiency in the Soviet way of life. That lack came as a surprise to me; I imagined that efficient organization was crucial to their system. In the same way, in later years, I was surprised that they lagged so far behind in developing computers; something else that would seem fairly critical to a system with central control. The day that we were to be taken to Peterhof and Pulkova we boarded the bus at the hotel at the advertised time, and sat, and sat, and sat. By then we were beginning to learn that there were likely to be delays, so we actually waited some thirty minutes before anyone decided some action was needed. Then Bart Bok, the Dutch-born director of the Mt Stromlo Observatory in Canberra, said 'I will fix', and went off into the hotel. Some time later he returned to tell us 'This bus left half an hour ago!'

The General Assembly was held in the marbled halls of the 33 storey Moscow University, the tallest of the 'wedding-cakes'. I didn't find the meeting as interesting as the Paris Symposium. As the name suggests, the General Assembly covered all branches of astronomy, while my knowledge and interest was pretty strictly limited to radio astronomy, with a particular emphasis on solar radio astronomy. And IAU General Assemblies are largely concerned with organizational matters; the reporting of scientific results forms only a small part of the proceedings. My strongest memory is of the magnificent multi-storey, marble-columned, assembly hall at the university, with its chandeliers. Each day as we entered this hall we were handed translation sets, like small transistor radios equipped with headphones. A switch on the set allowed the listener to select English, French or Russian. The translators sat in booths listening to the speaker through headphones, and their translations were transmitted to our receivers via a wire-loop round the hall. Those making speeches were expected to supply the translators with a copy of their speech ahead of time, and when they did so the system worked well. However when a script was not provided the translators had considerable difficulty. On one occasion the English translator had great trouble: he kept breaking off and starting again, and finally let out some imprecations in Russian, and stopped. After a pause, he said (in English) 'I will try again.'

Soviet physicists were renowned for their work in *theoretical* physics. In radio astronomy they were not world leaders on the experimental side, but when it came to the theory of the generation and propagation of radiation they were the leaders. Ginzburg and Shklovskii, from the Lebedev Institute in Moscow, were the two names best known to me. Shklovskii had proposed that the radio emission, and the diffuse *light,* from the Crab Nebula were both *synchrotron* emission, and the 1954 confirmation of this suggestion, when the light was shown to be substantially linearly polarized, had marked a turning point in our understanding of the nature of cosmic radio sources. Ginzburg, perhaps the deeper thinker, but a less flamboyant character, had also theorised that the radio emission from the discrete sources was likely to be synchrotron emission.. At the time of the Moscow meeting we solar radio astronomers were grappling to understand a new theory that Ginzburg and Zheleznіakov had proposed to explain much of the *outburst* radio emission from the sun. They had shown that radiation could be produced by the (self) scattering of (non-radiating) plasma waves. So it was very interesting to see these people in the flesh, though I don't recall this shy junior engaging them in heart-to-heart discussions.

'Saturday 16th (sorry, Sunday 17th, 1 am). We never seem to get to bed before 1 am in this country. In fact dinner is served from 18 to 23 hours and I have several times dined about or after 23 hours! Tonight I have been to a reception at the Kremlin! Quite something to write home about ... We were entertained in the palace in which the Supreme Soviet meets - the Grand Palace of the Kremlin. I have never seen such magnificence. Built by the Czars, of course. One room was glitteringly white with a dozen huge chandeliers down the centre - (there are chandeliers everywhere in this country) - and the most fascinating floor made of every variety of wood in wonderful patterns. Other rooms were equally marvellous and contained priceless gems of furniture, much of it inlaid with precious stones, pearl shell, marble etc. In another huge room all the walls and ceiling were covered with religious paintings ... not huge and daring such as you will see in Baroque churches in Western Europe. Each picture occupies a small part of the wall and tells a story. I suppose I should add that the supper was excellent (what else could it be in such surroundings) and Krushev was not present (pity!)'

'Sunday afternoon. Well here I am sitting in the biggest pub in Moscow and listening to Paul Robeson singing 'Ole Man River' (in English!) on Moscow radio. I suppose you knew he was here? This morning I went on a tour of Moscow - saw all the Red Square again and the fantastically long queues of people waiting to pass through the mausoleum and see Stalin and Lenin.' And they were long queues, stretching right along the wall of the Kremlin and round the corner! The roads were enormously wide, and there were lots of modern buses, trams and trolley-cars, but cars were rare, and were invariably black. There was very little advertising, but I do have a colour slide of a large billboard in the country exhorting the Soviet people to overtake the USA in the production of electricity. The lack of advertising, and of the other things that made western cities colourful, meant that Moscow seemed somewhat drab, or at least, subdued.

While visiting GUM, the big department store, I collected some interesting statistics from our guides about salaries and costs. 'The pay of a Professor is 5,000 roubles per month, while labourers get about 500 and the minimum wage that can be paid is 300. Hence the pay ratio of Professors to the poorest paid is actually greater than at home where Professors get about £3,500 p.a. and the basic wage is about £600 p.a. Against this it appears that there are more extensive social service benefits. For instance, the Government scholarships to attend universities, while superficially similar to the Australian Commonwealth Assistance grants, are evidently much more valuable.' Tomatoes cost 5 roubles/kg (¼ day's pay), shoes 250 roubles (two weeks' pay), and a smallish car 25,000 roubles (four years' pay, for a labourer).

Guides, mostly university students, were provided for all our activities, but in our spare time we were completely free to go where we liked. Unfortunately my incompatibility with the Russian language limited me to a few walks round the streets, and a few rides on that magnificent, spotless underground railway where the stations are decorated with those marvellous mosaics. I had made two attempts to learn scientific Russian, but each time had given up and depended on reading English translations of the Soviet journals. The state of my Russian was such that in the time it took our bus to pass some large sign I would just about succeed in working out the pronunciation of the letters!

'Yalta, Crimea. 22nd August, 1958. To-night I somehow feel sad. It is a combination of many things - my lack of knowledge of Russian which shuts me off from these people, but above all the realization that these people are different from us. This is a holiday resort - we really have nothing like it in Australia - it corresponds to the French Riviera ... the country is not unlike the country near Nice or Monte Carlo - the same rather steep mountains coming down to the sea. It is very pretty - more Italian than French, I guess, the architecture is rather Italian, with here and there touches of Arabic influence. After all Turkey is only just across the Black Sea.'

'We walked along the promenade this evening after dinner (eaten at 4 pm, supper 9 pm). There were great crowds promenading - especially as the cool of the evening came along. I thought back to Venice where similar crowds surged and I thought of my comment that most of those crowds could have been Sydney-siders. Here, very, very few of the people could have been. Here and there one saw a trim figure smartly dressed, but on the whole they are hefty in figure and slap-dash in dress. Also surprising was the lack of shorts - they are verboten, as some of our party discovered by trying to wear them. This contrasts strongly with the fact that they change in public on the "beach"! It is an extraordinary sight to see huge hefty women clad in a two-piece costume with neither cut nor fit.'

After the Moscow conference we had flown down to the Crimea to visit the radio astronomy establishments at Simeis, along the coast from Yalta. I have memories of enthusiastic discussions with young astronomers; female and male. And I have memories, and slides, of a variety of antennas of unusual design. One antenna was specially built to make measurements of an event that occurs each June when the motion of the earth round the sun brings the outer corona of the sun in front of the Crab Nebula. Studying the radiation received from the Nebula at this time provides information about the clumpy nature of the *solar wind*. From the Crimea the Crab Nebula lies south of the zenith, and this antenna had been constructed as a section of a paraboloid dug into the side of a north-sloping hill.

At Yalta I shared a room with Joe Pawsey and Ron Bracewell. They had just learned that their book 'Radio Astronomy' - the first textbook on radio astronomy, published by Oxford University Press in 1955 - had been translated into Russian and published without consulting them. The Soviet Union was not a signatory to the copyright convention. As compensation they were given some number of roubles that had to be spent in the Soviet Union. I think Ron later took another trip to the USSR to spend his roubles.

The evening we arrived at Yalta our bags were brought from the bus and put beside the reception desk at the hotel. Pawsey and I carried our bags up to the room, but Ron left his, saying that they were sure to be delivered to the rooms. Then, as I recall, some of the Americans complained about the quality of the accommodation and Pawsey, used to rough Australian accommodation, commented that for a couple of nights he found this quite adequate - until we found there was there was a problem with the water. None came from the tap over the basin in the room, nor from the showers, while the toilet was occupied by the maid and the plumber evidently attempting to rectify matters!

Soon after our arrival we were whisked off somewhere and did not return to the hotel until after midnight, following one of the very late and very slow Russian meals. Ron's bag was not in the room. Now Ron is a bit of a linguist and seemed pretty fluent in Russian. He called up reception from the telephone in the room and a long conversation in Russian ensued. At the end of it Joe Pawsey said 'My, that was impressive, Ron.' 'Yes', replied Ron, 'I didn't understand a word he said and he didn't understand a word I said!' Ron survived without his bag until the following morning.

Leaving the Crimea was even more memorable. We flew in a plane like a Convair-cum-DC3. After we had been airborne for about an hour the captain announced that there were thunderstorms over Moscow and for safety we were going to land at Dnepropetrovsk and wait for the storms to clear. After an hour or so in the Dnepropetrovsk airport lounge, the packed sandwiches from the plane were produced and we had our evening meal. Then towards midnight we were told that we would be taken to an hotel. This turned out to be dormitory accommodation and I recall that one American and his accompanying lady secretary opted instead to walk around the town. About 2 am we were summoned from our beds and taken back to the airport and so on to Moscow. Perhaps Bart Bok, who had left us at Yalta to meet Priscilla at the Hellespont, had been wise, as well as romantic.

As we flew into Stockholm after nearly three weeks in the Soviet Union I was amazed by the colours in the streets below. 'Look at those brightly coloured cars and the advertisements and the street signs - coloured houses - and now we land and the letters are English - many of the signs in English - people speak to us in English!' In Stockholm and then Oslo I was entertained by radio astronomers, and in Oslo also by Jan, whom I had met on the top of the Zugspitze in 1950. Then she had lived in London, but she moved to Oslo in 1956, from where she still sends us snowy postcards at Christmas.

Next it was Copenhagen with its holiday atmosphere. I spent a pleasant evening at the Tivoli Gardens. Then to Kiel where Professor Unsöld and family entertained me to dinner. Earlier in the day the younger members of his Institute for Theoretical Physics had taken me for a swim in the Baltic. 'Did I tell you that when I swan in the Adriatic at Venice I started to count up the seas that I had swum in. Pacific, Mediterranean, North, then I counted the Indian, Arabian and Red (swum in on ship!!). I missed out on a swim in the Black Sea through leaving my costume in my other suitcase which is still at Amsterdam.' On then to Hamburg. '...the train...was equipped with a Zugsekretärin to whom you could dictate your letters and she would type them! It was also equipped with telefon - radio, I presume'.

Kiel had been almost entirely rebuilt since the war, since much of it had been destroyed by bombing. Hamburg still had some bomb damage, but I found it 'quite remarkable ... for its beauty while being a great industrial port. There is a large lake in the centre of the city and this was 'decorated' with many yachts today. It was a beautiful sight. Then at the harbour there are absolutely fantastic numbers of ships and docks and cranes. I went on a hafen rundfahrt and saw at least half a dozen floating docks, and wharfs and wharfs and wharfs lined with huge cranes for loading ships. We also saw many ships being built. And the tugs - they come in tens and all richly coloured, and looking very photogenic in the sunshine.'

In Holland I visited the solar radio astronomers at Utrecht, and at Dwingeloo saw the dish used for studying the 21-cm line radiation from interstellar hydrogen. In Leiden I was entertained by Charles Seeger and Brian Robinson. Charles was an American who had worked with the Leiden radio astronomers for many years. In Moscow he had introduced me to Carlsberg beer on the top of the Ukraine Hotel. Brian, who had been a year or two behind me at Sydney University, had worked at the Radiophysics Laboratory with Chris Christiansen, and was now at Leiden building sensitive (low noise) radio receivers. He later became the head of the radio astronomy group in the Radiophysics laboratory.

And so to England, six years on from my Cambridge sojourn. ' ... my general reaction to England is not enthusiastic. ... The ... discouraging aspect is the lack of bright new things that I've been used to on the Continent. There the bomb damage was so extensive that they had to rebuild on a quite tremendous scale. Whole cities, particularly all railway stations and most trains. And they have done it. So that the outward impression - particularly in Holland and North Germany - is of a very rich country rising from the ruins. All bright, new, clean and shiny. My impression of England is an impression of decadence. True, there are some new trains which are very fine - but mostly they are the same ones as six years ago - with six more years of dirt. ... there are very few examples of the modern architecture one finds on the Continent. ... there are still quite a few wartime or post-war "migrant camps" - well that's the best way to describe them to you. Indeed the look of decadence even extends to the countryside where there are many fields of wheat which I presume have been rained-on since maturing and now look a dead brown colour - in sharp contrast to the rich golden yellow that I found so wonderful in southern Europe.'

Of course it was stimulating to return to Cambridge. I find this entry in my note book: 'Heffers - books still in same places - nuclear physics expanded. *The Planet Jupiter* B.M.Peck, Faber & Faber; *The Upper Atmosphere* Massey & Boyd, Hutchinson; *The Ionosphere* K.Rawer, London: Crosby Lockwood & Son; *The Exploration of Space by Radio* Hanbury Brown & Lovell, Chapman & Hall.' Obviously I had not seen these titles in Sydney before departure. Compared to Sydney, the bookshops in Cambridge were marvellous. While there were no warm reunions with the Cambridge radio astronomy group - the Cambridge-Sydney suspicion still held sway - someone from the group did take me out to see their new field station at Lord's Bridge and the 4C aperture synthesis telescope in which one element moved along rail tracks. By this time the Cambridge group had abandoned solar radio work to concentrate on studying the 'discrete sources'.

In Cambridge I was entertained by Henry Rishbeth, who had spent a few years at the Radiophysics Laboratory in Sydney, including a period with Paul Wild's solar group. In 1955 Henry and I had Youth Hostelled round New Zealand where we had been greeted with 'Oh, you come from Australia. You have snakes in Australia, don't you', or 'Oh you come from Sydney. You have night-life in Sydney, don't you.' The excuse for our trip to New Zealand was to attend ANZAAS, the annual conference of the Australian and New Zealand Association for the Advancement of Science, which that year was held in Christchurch. The only science I remember from the trip is some geology learned on a pre-conference excursion that took us to some of the tourist spots of the South Island, including Milford Sound, reached via the newly-opened Homer Tunnel. Another memory of this visit is of baby's prams being transported on hooks provided across the front of the trams in Christchurch! When I have spoken of this some have expressed disbelief, but on a recent visit to Christchurch we saw that today children's strollers are carried that way on the front of the buses.

Next stop on my trip was the University of Manchester's radio observatory at Jodrell Bank. Here Bernard Lovell had built an antenna in the form of a bowl 250 feet across, able to be tipped to point to any direction on the sky. The first of the really big steerable dishes, and an engineering feat of some magnitude, this was the dish that Taffy Bowen hoped to surpass in size when he built the Parkes Radio Telescope. That was not to be; the money was not sufficient; but the Parkes dish did outstrip the Jodrell Bank dish in performance. The Jodrell Bank dish remained the largest fully-steerable dish in the world until the 100 metre diameter dish was built at Effelsberg, near Bonn, in 1971. As I write in the year 2000 the Jodrell dish is still in use, after several face-lifts. The dish is pivoted between two towers which move around a circular railway track. When I first the telescope the dish was upside down; quite a strange sight. That was the way that they gained access to the focus to make changes to the 'feed system', i.e. the small antenna that collects the radio waves that are reflected from the dish surface and concentrated near the focus.

According to my letter I was impressed by something else that I saw while at Jodrell Bank. 'I suddenly saw a futuristic aircraft flying over and just pointed and said 'What on earth is that?' It was a Vulcan delta-wing bomber ... I'd seen pictures of such things in the papers, but to find them flying over the countryside as a regular thing rather took me by surprise.'

I visited the Royal Radar Establishment at Malvern, where Hey, one of the largely unsung pioneers of radio astronomy, was building a pair of dishes mounted on railway tracks. The instrument being built at Caltech, where I was to spend the next two years, was of similar design, so there was much interesting discussion. At Didcot (Harwell) near Oxford, I said hello to John Carver, spending his sabbatical year there on leave from the Australian National University, and in London I visited Hugh Michael, who had taught me to play squash at Cambridge, and now lectured in fluid dynamics at University College. 'Sidelight on London: London bus conductors now say "Hold tightly" and not "Hold tight". Also noticed the number of West Indians working in London as bus conductors, railway porters, street cleaners etc.'

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From London I took the new route 'over-the-pole' to Los Angeles. After twelve hours or so in the air we made a refuelling stop at Frobisher, on Baffin Island, near the entrance to Hudson Bay. 'The summer temperature here is supposed to reach only 40 degrees Fahrenheit, but at 3 am it was not at all bitingly cold. Cold, but pleasant.' Then on across 'the dark, lake-dotted, far north of Canada which slowly gave way to wheat fields - the checker-board pattern again. As we came down across Saskatchewan, and across the corner of Alberta and into Montana we saw wheat, wheat, and more wheat as far as the eye could see. Then suddenly we came on mountains (fairly dry) and since then have flown over mountainous regions'. Geography from the air in those days was somewhat more intimate than it is from the 35,000 feet of present-day jets. 'I'm wondering if we shall actually pass over the radio astronomy field station of Caltech which is in the Owens Valley, 250 miles from L.A.' Well we must have gone close, since a later entry says 'Just passing over Reno, Nevada. Civilization in the midst of dry ground.'

As we landed at the Los Angles airport I felt that this was the first country that I had seen that resembled Australia. There was dry grass all round the airport! But the trip from the airport up the Pasadena Freeway in the service limousine was quite unlike anything I had experienced before. 'The "limousine" is like a long car. We travelled at 60 mph for over 30 miles along these fantastic roads - at least 6 lanes, often 8. Cars everywhere, also doing sixty. My eyes were popping out on sticks. What a contrast to Moscow! Here the airport is in the built-up area and it is built-up area all the way from the airport [to Pasadena]. "All" the houses are single storey - almost all are wooden - and in some parts the shops, motels etc are gaily painted - it looks very much like Surfer's Paradise.' Well, perhaps like the

Surfers' Paradise of the 1950's, but hardly like the present day Surfers' with its masses of 30 and 40 storey apartment blocks and hotels!

CALTECH

'Of course you'll stay with us' was John Bolton's response when I had written to ask if he could recommend somewhere for me to stay when I arrived in Pasadena. While I had barely known John at Radiophysics, and had never met Letty, that was their way. From day one students and staff were treated like an extended family. And later it was Letty who took me round Pasadena to find an apartment, and then to equip it with linen, crockery, cutlery etc.

After I had been at the Bolton's for a couple of days, John announced 'We'll leave for the site at 5:30 in the morning', 'the site', or 'Big Pine', being the Owens Valley Radio Observatory in the making. It was a five hour, 250 mile drive north from Pasadena. The route passed NASA's Jet propulsion Laboratory (JPL), then climbed up the Angeles Crest Highway over the San Gabriel Mountains, locale of the 100 inch Hale telescope. Descending into the Mojave Desert, we passed by the Edwards Air Force Base, and a couple of hours after setting out John pulled into a restaurant in Rosamond. He selected one of those 'banquette' style seats typical of American restaurants, and after we had been greeted with a glass of iced water and 'Coffee?', John introduced me to the American breakfast mysteries of 'hash browns', and eggs 'over-easy' or 'sunny side up'. 'Short stacks' of hot cakes with maple syrup weren't John's forte.

Onward then through the land of sagebrush, with occasional Joshua tress twisted into fantastic shapes. John stopped to show me the colours of Red Rock Canyon, and pointed out the Alhambra Hills, where many of the Wild West movies were filmed. The Owens valley is not a sandy desert, there are just miles and miles of low, grey bushes stretching out to the bluish haze of the arid Inyo and White Mountains in the east, while the Sierra Nevada tower in the west. Joining Route 395 near the China Lake Weapons Test Center we passed the Owens Lake with its salt works, the town of Lone Pine , and the psychological cattle grids - just a series of white lines drawn across the highway.

In a cluster of houses - the township of Big Pine - we turned off along a road that winds it's lonely way eastward over Last Chance Range and into Death Valley. However after crossing the hardly-used railroad tracks that sport a sign 'ZURICH', and then crossing the Owens River (depleted of water by the City of Los Angeles), we came to the observatory turn-off. Here, in a dusty stretch of desert, two 90-foot dishes were under construction, while nearby amongst the sagebrush stood two buildings made of concrete blocks, the workshop, and the comfortable, modern, living quarters. A few hundred yards off to one side was a smaller (32-foot diameter) dish antenna that had first been erected at Mt Palomar. This was the 'site', and this was where I spent about half of the next two and a half years, in sojourns of a week or two at a time.

When I arrived the first of the 90-foot dishes had already been erected, while the second was just a 'tepee' shaped tower. Over the following weeks I watched the progress of the second telescope. The section housing the polar axis was lifted and welded onto the tower, then the declination axis was added, and finally, early one still morning, the dish itself, which had been assembled on the ground, was lifted into place by two large cranes. I found it fascinating to watch all this, and to record it on slides with my recently acquired 35 mm camera. Imagine what this culmination of four years planning meant to John Bolton, as he supervised it all very closely, often through the theodolite.

In John's design each of the 90-foot dishes could be used alone, or they could be connected together as an interferometer. One dish operating alone would collect radiation from a 'beam' almost 1 degree across, but when the two dishes operated together as an interferometer this beam would be split into a series of narrower fan-beams. Being narrower these fan-beams would allow a source of radio emission to be located more accurately. The narrow beams would also allow the angular-extent of the emitting area of a radio source to be measured. As a source moved through the fan-beams the output from the receiver would increase and decrease. For a source having a width less than the width of each fan-beam these excursions would be of the full amplitude of the source, but if the width of the emitting area was greater than the width of the fan-beams, then the excursions would be, to some extent, smoothed out.

The width of the fan-beams could be changed by observing a radio source at different times as it crossed the sky. This would change the *projected* separation between the two dishes, and hence the number of fan beams across the main-beam. (The number of fan beams is equal to the number of dish-widths across the total aperture.) To provide a greater variation in the width of the fan-beams the dishes were equipped with wheels and could be moved to a number of different 'stations' along an east-west section of widely-spaced rail track some 1,600 feet long, and along a slightly shorter north-south section of track. With the dishes further apart the main-beam would be split into a greater number of narrower fan-beams. By using the north-south section of rail track, and also to some extent also by observing the source at different positions across the sky, the orientation of the fan-beams could be changed. Hence, from a series of observations made with the antennas in different locations, and the source at different positions across the sky, crude maps of the emitting regions could be made.

While construction of the Caltech antennas was relatively trouble free, the tangled mess of a section of antenna that lay on the ground when I arrived bore testimony to an earlier accident. As the first dish was being lifted one of the cranes broke, and the dish crashed to the ground, narrowly missing killing a man. According to John, his suggestion that Caltech immediately sue the crane owner was not followed. Instead the crane owner sued Caltech for breaking the crane and ultimately Caltech found it simpler to pay rather than fight it through the courts! Before the second antenna was lifted the cranes were tested with weights.

Yellow Jackets, a type of wasp, caused another minor irritation. They swarmed in the tepee towers, and got under the welders' helmets. The pest exterminator was called in and sprayed the towers with an insecticide in an oil base. So then we slipped on the metal surfaces. Later, when the Owens Valley Radio Observatory began a preprint series in yellow covers, they naturally earned the nickname *Yellow Jackets.*

Erection of the antennas did not mean that they were complete. Power and control cables had to be run, and, inside each tepee tower, a room constructed where the observer operated. Much of the fitting out of the telescopes was done by the staff and students. A letter home dated 4th November 1958 reported: 'At present we are spending most of our time running screwed conduit down beside the railway tracks to take power and controls etc to the aerials. One of the conduits has 35 wires in it - so you can see that getting them all in and joined up correctly is quite a job. Besides this we are lining and insulating the aerial towers and installing the lighting and power, and then the driving and recording equipment in there. Quite a lot of manual and outdoor work which suits me fine. Particularly after little exercise for so long it is good - and as far as I am concerned it is like being still on holidays - there is no vast brain fag involved.' John reported back to Sydney that this supposed-theoretician had become quite proficient in manual tasks!

Al Munger (Big Al) was the site manager. He lived locally, and was a great guy who could turn his hand to most anything. The receivers were Gordon Stanley's baby. Gordon, originally from New Zealand, had worked with John Bolton at the Radiophysics Laboratory in Sydney, and had followed him to Caltech. He became the observatory director after John returned to Australia.. The rest of the scientific staff were also from outside the United States, initially consisting of Tom Mathews from Canada and Kevin Westfold visiting from Australia for a year, soon joined by Radhakrishnan (Rad) from India, Peer Maltby from Norway and Dave Morris from England.

Students included Dan Harris, for whom the observers' quarters was home, and of whom more anon; Little Al (Al Moffet), later a director of the observatory; Barry Clark, still, I think, the computer whiz kid at the Very Large Array in New Mexico; Bob Wilson, who later shared the Nobel Prize with Arno Penzias for the discovery of the 4 K background radiation; Ken Kellermann, who has made many major contributions to our knowledge of extra-galactic radio sources;and Dick Read, very strong on technical matters.

Rachel Gates was the housekeeper at the observatory. She arrived each morning after breakfast, cleaned the house and made the beds, informing me one day that she had found a scorpion in my bed. She explained that they liked the warmth! Rachel also prepared and served our lunches, and prepared dinner, which we ate about 5 pm, as I recall. After preparing our dinner Rachel then headed home to care for her husband, Tiny. Tiny dwarfed even Big Al. In the huntin' season Tiny and Al would 'go get em an elk'. I think either could have carried the carcase over his shoulder with ease. Tiny drove a road-making machine, and at that time was working on the road out to Death Valley, while he and Rachel lived in a trailer-home in Big Pine. Being used to the small caravans that we had in

Australia, I was amazed when I was invited to visit them in their three-roomed, *very* long trailer.

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I was delighted by the friendliness of California. When I first arrived Letty had greeted me with a 'Hi!', explaining that was the local custom. Then, when she took me apartment hunting 'I thought that Letty must know half the landladies. But no, that is just the way of the place. You greet one another with "Well hulloa" as if you were old buddies. And the shop assistants on the whole are most polite. I am learning to say, as they do, "Yoar wulcome."' The modernity was another source of delight - what I would now regard as conspicuous consumption damaging to the environment. Wide roads, big cars, modern apartments, things we didn't have at home. And I found Pasadena 'a rather swanky residential city....Some of the stores are as exclusive as the locality....You never see anyone in the streets or in the shops. It is like a deserted city. People travel by car and park in the parking lots provided by the stores at the rear...One shop...(Bullocks) looks like a large modern hotel...There are no display windows at all.'

I had not before experienced the like of my modern bachelor apartment at 231 So Hudson, with everything so new, and I set to and photographed myself in the apartment. Having 'a home of my own', when, later in the year, Radhakrishnan arrived to join the staff, I was able to imitate the welcome I had received at Dapto from John Murray. I invited Rad round to share a baked shoulder of lamb. Rad seemed suitably impressed - probably told me it was 'very white' of me - and didn't let on that he was a vegetarian who loved hot spicy food. Nor, by the way, did he reveal until months later that he was the son of the Nobel Laureate, Raman, director of the institute of physics in Bangalore named after him, a post Rad later occupied himself.

Rad arrived with stories of his film star idols performing on the set in London where he had worked as a sound engineer. Then he had spent a year working with radio astronomers in Sweden, and had stories of his black skin fascinating the fair Nordic people. He had shipped his VW across to the east coast of the US and then driven over to Pasadena. En route, in the Deep South, he had been treated as a Negro and refused a bed. What seemed to upset him most was that when it was realised that he was from India, and not a Negro, they gave him a bed. Then not long after he arrived in Pasadena he went walking in the swanky area of San Marino just south of Caltech and was stopped by a patrol car. No one *walks* in San Marino, and a walking black man was obviously up to no good.

Rad was as fascinated by modern America as I, and he was a bit put out when he found that while my apartment had a disposal-unit in the sink, his did not. However I think he won the competition when he had two telephones installed in his 'no bedroom' apartment. If you phoned him when he was still in his 'hit-you-on-the-head' foldaway-bed - which happened fairly often as mornings are not Rad's time of the day - then you would be answered with 'Bedside'! Rad contributed greatly to the social life in Pasadena. This was largely dominated by parties with Caltech people. Back in Sydney, apart from occasional invitations to play tennis on the Wild's grass court, my social life had been quite separate from my working life. Most of my social activities had revolved around the Youth Hostels Association. In Pasadena I found no equivalent social club, and since Caltech was an almost male bastion there was a severe shortage of unattached females.

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At Caltech Radio Astronomy formed part of the Astronomy Department, headed by Jesse Greenstein. The Department was housed in the Robinson Building, a pleasantly cool building in Pasadena's heat, but one that seemed to accumulate smog,. It was never free of the smell, and the eye irritation, of the polluted air. While part of the Astronomy Department the radio astronomers operated as a separate group, enjoying a great sense of camaraderie. This was engendered by John Bolton, who worked with staff and students, even on mundane tasks. He also introduced morning and afternoon tea - or should I say, coffee - in Pasadena, something that is uncommon at workplaces in North America. Students, research staff, secretaries and the one technician, Johnny Harriman, would gather in the workshop where Johnny would have the coffee percolating.

The morning and afternoon tea breaks were an importation of practices followed at the Radiophysics Laboratory in Sydney, but John wasn't able to duplicate some of the other features of Radiophysics. There we had enjoyed extensive drawing-office, machine-shop, and editorial and publishing facilities that were not available at Caltech. Nor was the astronomy library at Caltech the equal of the library back home. That library, shared by the CSIRO Divisions of Physics, Applied Physics and Radiophysics, contained a wide range of material, including a very complete coverage of journals and books relevant to radio astronomy. In addition the CSIRO Librarian was very familiar with the radio astronomy literature, and supervised the production each week of a set of abstracts covering the new radio astronomy material received in the library.

John Bolton's secretary, Kath Wilson, was a marvellous lady. When I phoned her a year or two back on the occasion of her eightieth birthday, I reminded her of her chagrin when Caltech decided to install 'modesty panels' on the secretaries' desks, so as to hide their legs from prying eyes! Kath, and the Astronomy Department secretary, Maggie, ragged the Australians about our pronunciation of *cake* and *bakery*. They said we pronounced them *cike* and *bikery*. I retaliated by exaggerating their pronunciation of *walk* and *talk* as *wark* and *tark*. After a month or two I think Maggie came to regard her pronunciation of such words as inelegant! Kath's husband, Olin, was an optical astronomer at the Mount Wilson and Palomar Observatories - Santa Barbara Street, as it was called. (I didn't really understand the relationship between Caltech and Santa Barbara Street, nor for that matter between Caltech and JPL - the Jet Propulsion Laboratory. These days I think that JPL, at least, is a completely independent organization.) Olin was a great character. I have a vision of him sitting in his big chair, nursing his pipe, and in his slow, relaxed way telling some amusing yarn. One such story was about discouraging his kids from watching too much television by putting a money box on the television set and making them pay for their viewing. Coming from the land of pedal wirelesses for the School of the Air, I suggested that he install a pedal generator and let them generate the electricity to run the television!

I bought a Chevrolet. That year they had broad, horizontal tail fins, so when Rad arrived in his little VW he christened it the Aircraft Carrier. The day I collected the car from the dealers it refused to move from the first stop light. It was out of petrol! That car is also remembered as the only car that I have owned that I did not wash myself. For $1 (with purchase of gas - which cost 26 cents per gallon!) the team of young Negroes at the car wash did it over thoroughly both inside and out. ($1 was also the cost of a Crooke's Radiometer, a device that really intrigues me, and that I had tried to manufacture at home in my glass-blowing period.)

The first time I drove my shiny new car up to the ski resort of Mammoth, just north of the radio observatory, I slid off the icy road. Fortunately it was into soft, deep snow, and there were enough of us to simply lift the car back onto the road. Some time later I nearly involved Rad and myself in a serious accident. After an extended sojourn at the observatory we left in the dark to drive back to Pasadena. In his usual style Rad was still cogitating over things that had puzzled us at the observatory, raising deep questions. As we drove out along the access road these matters were claiming more of my attention than is safe when driving, and I suddenly became aware of cars racing at us from the right. Without realising, I had gone through the stop sign, and was in the middle of Highway 395. Luckily my reaction was to put my foot down and get out of the way. And again, luckily, there was a bit of road across the other side of the highway to receive us.

Another drive back from the observatory to Pasadena that sticks in my mind was the occasion when I drove via San Francisco with John Shakeshaft. During a visit to the Owens Valley, John, from Cambridge, had expressed an interest in seeing San Francisco. As I had not yet visited that city I offered to drive him back to Pasadena the long way round. As we drove, the song on the car radio celebrated the Americans chasing the British 'down the Mississippi from Louisiana to the Gulf of Mexico' (in the war of 1812.). I thought it a fun song, but I suspect that John was 'not amused'. As this was mid-summer we were able to cross the Sierras by taking the road over Tioga Pass and through Yosemite National Park, a road that is closed in winter. Mid-summer meant two other things: my attire consisted of shorts and shirt, and San Francisco was freezing cold. Spring and autumn are the warmest seasons in San Francisco; in summer fog covers the Bay Area and it is quite cold. I froze. I seem to remember that I was loaned a coat and tie so that we could go to the 'Top of the Mark', the summit of the John Hopkins Hotel.

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Staff and students spent so much time at the observatory that we arranged some recreation up there. There was a tennis court in Big Pine which we hired on a few occasions. One letter reported games between Dan, Rad and myself. 'Australia managed to defeat America, then India defeated Australia, and just to round things off America beat India. So now you know who is the best player.' Once or twice we drove up Big Pine Creek and went walking in the foothills of the Sierras. On one occasion Dan and I walked through deep, soft snow on a clear sunny day. A marvellous experience, and the lunch Dan made was great too. Sandwiches of unbuttered bread with thick slices carved from the ham that Rachel had cooked for the previous night's dinner, and washed down with a carton of milk.

Occasionally we patronised the ski resort of Mammoth, an hour's drive north of the radio observatory. Not that I was ever good enough to really enjoy skiing, but Dan was keen. One such visit was on the opening day of the trout-fishing season. 'On the way up to Mammoth we dropped in to see the spectacle at Crowley Lake. There were reportedly 4,000 boats out on the lake at 7:30 am.' At New Year we followed our day's skiing with a dip in the local creek which is heated by hot springs. There we met a crowd from the Hollywood-based Edelweiss Ski Club, the only social club that I found in my Pasadena days. I went on a few of their events, including a summer-time water-skiing weekend when we camped by Lake Isabella. Talk about a desolate area; I don't think there was a tree in sight.

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In about August or September of 1959 Colin Gum came to spend a year at the Mt Wilson and Palomar Observatories and we began sharing an apartment at 1080 No Lake Avenue. Colin's arrival was a result of Harry Messel being appointed to head the School of Physics at the University of Sydney, following Victor Bailey's retirement. We laughed when Messel skited about 'killing lions in the jungle of physics', but he certainly put some life into the Physics Department. From corporate sponsors he obtained the money to start several new departments. Bernie Mills from Radiophysics, irate that CSIRO money was going into a big dish rather than into innovative aerial systems, was easily lured with the promise of money for a large Mills Cross which was built near Canberra. And an Astronomy Department was created, where Hanbury Brown came to build an optical post-detection correlation interferometer to measure the diameters of stars. Colin Gum (after whom the *Gum Nebula* is named) was appointed to a position in this new department, and came to Pasadena for further experience, and to search out a manufacturer for the 40 inch telescope planned for the new department.

We staged a few good parties in our apartment. One that I recall was when Paul Wild was visiting from Australia, and Fred Hoyle was spending a term at Caltech during the English winter. To quote my letter: 'This has been his [Fred's] custom for some years, but last year he didn't come as he had just been made a Professor at Cambridge and had to set a good example, or something...I nearly had hysterics watching Fred Hoyle and Paul lying on the floor, blindfolded, and trying to swipe each other with rolled up newspaper. They had to hold their left hands together while one asked "Where art thou, Bert?" As soon as the other replied "Here" there was a mighty swipe to try to hit him on the head before he had time to move. Fred explained that it was an old Yorkshire game, and really should be played with wooden clubs.'

It is interesting to have that letter to correct my memory which had coalesced that party with the Christmas party we hosted later in the year. Just before Christmas Colin's observing run at Mount Wilson was rained out: who'd be an optical astronomer? While waiting for the weather to clear Colin had spent some time reading *The Saturday Evening Post*. For the festive season *The Post* had suggested constructing a dodecahedron from a series of triangles made of coloured straws, the straws being connected together by threading cotton through their tubes. On his return from Mt Wilson Colin had bought the materials, constructed the device, and suspended it from the apartment ceiling by a fine thread. Then he added his own touch - a column of straws going down to the floor as an apparent support. This device greeted me when I returned from Big Pine - a structure about 2 feet across standing in the middle of our apartment, and to all appearances supported on a single vertical line of straws going down to a pedestal of straws at the base. It became the centrepiece for that Christmas party.

Colin met a tragic death in the following April. He went to Europe visiting potential telescope suppliers, and while there went skiing in Switzerland. Apparently he followed a dangerous ski-trail, and that led to his death. John Bolton's telling me of the cable from the Australian High Commission on this occasion rings in my mind along with the cable that came a year earlier reporting the death of my younger brother in a car accident back home.

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The Owens Valley radio Observatory was dedicated at a ceremony in December 1958. At that time the two 90-footers looked complete, but in truth observations were not possible until the early months of 1959 when the first dish (called #2!) went into service. Initially it was instrumented for observations at a frequency of 750 Mhz, but that was soon changed to the frequency being used by NASA, namely 960 Mhz, i.e. a wavelength of 31 cm. A solar swept-frequency receiver was also installed off-axis on this dish. By the middle of 1959 the other dish was operating at a frequency of 1420 Mhz, studying the line-radiation emitted by the neutral hydrogen between the stars. Rad was heavily involved in developing and using this hydrogen-line system.

Early in 1960 the two dishes began operating together as an interferometer, at a frequency of 960 Mhz. Initially they sat where they had been built at a separation of 400 feet. Over the following months the #1 dish was first moved to the 800 ft station and then to the 1600 ft station; finally the two dishes were repositioned on the north-south track. So, after more than five years of work, John Bolton's dream was in full operation. Then in mid-December of 1960 John amazed many people by leaving Caltech to return to Australia. It seems there had been a long-term arrangement with Taffy Bowen for John to return to the Radiophysics Laboratory to supervise the final stages of the construction of the Parkes Radio Telescope, and then to become its director. The management at Caltech did not know of these plans until the middle of 1960: they were certainly surprised, and I think somewhat annoyed. I stayed on at Caltech until March 1961, (###) when I also returned to the Radiophysics Laboratory to work with the Parkes Radio Telescope.

OBSERVATIONS AT OWENS VALLEY

When observations with the first of the 90 foot dishes at the Owens Valley Radio Observatory began early in 1959 John Bolton decided what observations would be made and we all pitched in with the observing and the analysis of the records. Then slowly different groups were assigned specific projects. In what follows I talk about projects that resulted in my name appearing on a publication. Bear in mind that some of the other observations not mentioned here lead to very important discoveries.

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Both the Cambridge and Sydney groups had made surveys of the sky with the object of detecting and cataloguing the 'discrete' radio sources. In the area of the sky that is accessible to both groups the initial surveys had shown little agreement, which led to considerable controversy. The Cambridge surveys were made with interferometers, exploiting the fact that an interferometer splits the main beam of an antenna into many finer beams, and so allows a source of radiation to be located more accurately. However, when there are several sources of radiation in the main beam there is considerable confusion. When Bernie Mills, at the Radiophysics Laboratory, made a survey with his *Mills Cross* aerial, he failed to find many of the sources in the Second Cambridge (2C) Catalogue. A Mills Cross has a single beam and so avoided the problem of confusion. However, the Cross was a new invention and was not a *filled aperture* instrument, so Mill's results were not entirely free of contention. Ultimately it was agreed that the Cambridge catalogue contained numerous imaginary sources resulting from confusion, and the Cambridge group then produced a new catalogue, the Third Cambridge (3C) Catalogue.

The 3C catalogue was based on observations made at a higher frequency, and with the records interpreted in a more conservative fashion. It contained far fewer sources and all the sources were thought to be genuine. However there remained a suspicion that some of the sources might be 'lobe-shifted', the phrase used when the fan-beam, or 'lobe', of the interferometer that is directed at the source is the neighbour of the one assumed. That, of course, leads to an error in the measured position. To help clarify the situation, and to produce a reliable list of sources for further study at the Owens Valley, John Bolton planned a survey exploiting the well-defined pencil-beam of the single dish. As a thesis project, Dan Harris was given the job of using the first 90-footer to observe all the radio sources in the 3C catalogue, and I was assigned to collaborate with him.

Later, when the two dishes were operating together as an interferometer, they could be controlled from the equipment room in the observers' quarters. However in these early days each dish was controlled from the tiny room built inside its tepee-tower base. As I relate below, the first 90-foot dish to go into service was used for solar studies by day, so that observations of other sources had to be made at night. That meant spending nights in this tiny room in the base of the telescope, with no view whatsoever of the outside world. Dials showed the direction in which the dish was pointed and the receiver output was displayed on a chart recorder.

For the 3C project we would direct the antenna to the *declination* (latitude on the sky) listed for the source, and to a *right ascension* (longitude on the sky, but labelled in hours and minutes) that was about 12 minutes earlier than the listed right ascension. Then for the next 25 minutes, as the rotation of the earth drifted the reported source through the beam of the telescope, we periodically marked on the chart the right ascension indicated by the dials. When a source was detected we measured its right ascension from the chart record, set the dish to track this measured right ascension, and then scanned the dish north and south to measure the declination of the source.

Periodically we checked the accuracy of the right ascension and declination dials by observing one of the few sources already identified with optical objects with known positions. To determine the strength of a source we compared the deflection that it produced on the chart with the deflection produced when the radio signal from a noise-tube (rather like a fluorescent-light) was coupled into the receiver input. In subsidiary observations the output of this noise-tube was compared with the signal received from the well-studied radio source Virgo A (the galaxy M87). Establishing the strength of a radio emitter in fundamental physical units (Watts per square metre per Hertz of bandwidth) is so difficult that it is rarely attempted.

The pen on the chart recorder wandered about, which made it difficult to detect weak sources. Much of this wandering wasn't caused by changes in the signal level received by the antenna, but resulted from 'noise' signals generated in the *front end* of the receiver. Thermodynamics tells us that this internal receiver noise can never be completely eliminated, but producing receivers with the least possible noise is the Holy Grail of receiver designers. At Caltech Gordon Stanley built some of the lowest-noise receivers then available: his designs were used by NASA. However even these receivers generated internal noise that was as strong as the signal collected by the 90-foot dish from the strongest radio sources (except for the sun). [The noise in the best modern receivers is almost 100 times lower than in those days!]

Because of the presence of this large internally-generated noise-signal even small changes in the receiver amplification produced large changes in the receiver output. To reduce these variations in the output the receiver was equipped with a Dicke switch - named after the inventor of the idea. In this system the input of the receiver is switched many times a second between the signal coming from the aerial and a reference signal of approximately the same strength, and just the switched-component is recorded. If the switching is more rapid than the changes in receiver amplification, then changes in the amplification of the internal noise will not produce any switched output. Great, but Dicke-switching doesn't eliminate any problems that occur before the switch, and it certainly doesn't eliminate those things that obey Murphy's Law rather than logic! Our comparison signal was initially the signal from a small antenna directed at the sky, a 'sky horn', but later we used the noise generated in a resistor, reducing its thermal noise output by cooling it in liquid nitrogen held in a thermos flask. Modern receivers rarely use a Dicke switch. Since the invention of the transistor everything has become 'solid-state', and variations in amplification are not the bug-bear they were in the days of 'valves'.

Even when variations of receiver amplification are not a problem there are intrinsic variations to cope with. Noise signals, whether they from cosmic radio sources or are generated within the radio receiver, have rapid intrinsic fluctuations. The only way to reduce these fluctuations is to average the signal over a period of time. So, as with most radio-astronomy receivers, the output of the Caltech receiver passed through an averaging circuit or *time constant.* Of course you can do only so much averaging; if the averaging time is too long you will smooth out the change that occurs when a radio source passes through the beam!

While Dan Harris and I appear as joint authors of the paper reporting the results of this survey of the 3C sources, the CTA (i.e. Caltech A) survey, it was Dan who did most of the observing. Now, as you can imagine, observing was a rather boring business, and occasionally Dan dozed off and allowed more of the sky to drift through the beam than had been intended. This led to the discovery of several new sources which appeared in neither the Cambridge nor the Sydney catalogues, the most notable being CTA 21, CTA 26 and CTA 102. Most discrete radio sources emit more strongly at the lower frequencies, but, as Ken Kellermann later showed, these particular sources are weak emitters at the lower frequencies and hence were not detected in the earlier surveys. The reason for this weakness at lower frequencies did not become known until some years later when it was realised that the sources are so dense that at low frequencies the synchrotron emission is *self absorbed.* These sources are the extremely dense cores of galaxies, objects which we now call *quasars.*

Perhaps this is an appropriate place to comment on the progression over the years of the wavelengths (frequencies) of radiation studied by radio astronomers. Before the Second World War radio communications developed in the metre to kilometre wavelength bands. *AM* radio broadcasts have wavelengths of hundreds of metres, while in the so-called *short-wave band* the waves have wavelengths of tens of metres. As the desire for aerials to be more directive assumed greater prominence (a desire accentuated by the development of radar) there was increasing pressure to develop valves that worked at shorter wavelengths. At the start of the war the British (and Australian) early warning radars used a wavelength of 1½ metres, i.e. a frequency of 200 Mhz, or 200 megacycles per second, to use the terminology of the time. During the war the pressure for smaller antenna systems for use on aircraft led to the development of equipment operating at much shorter wavelengths and by the end of the war the standard aircraft systems operated in the 10 and 3 cm bands. (To confuse the enemy these were called the S- and X-bands, phrases which have continued to confuse the non-initiated to this day!)

Apart from a few measurements of the emission from the sun and the moon made in the centimetre-wavelength range, all the early radio astronomy was at metre-wavelengths. The immediate post-war observations used ex-radar equipment, particularly the 200 Mhz (1½ m) equipment. And when it became clear that the emission from most cosmic sources was stronger at longer wavelengths, measurements were made at wavelengths out to the limit where the ionosphere began to cause trouble, at around 10 m. However the need for more precision in determining the direction of arrival of the radio waves favoured using shorter wavelengths (higher frequencies), and, for example, by 1956 Piddington and Trent at CSIRO had made a sky survey at a wavelength of 50 cm (600 Mhz). The study of the *line* emission from the hydrogen gas between the stars, radiation which has a wavelength of 21 cm (1420 MHz), also boosted the development of higher frequency systems. At the Owens Valley the initial plans for the interferometer envisaged a wavelength of 75 cm (a frequency of 400 Mhz); by the time the first receiver was installed this had become 40 cm (750 Mhz), and soon afterwards 960 Mhz (31 cm), a frequency being used by NASA, was adopted as the standard operating frequency. In subsequent years radio astronomy has moved to higher and higher frequencies. Today there are many millimetre, and even sub-millimetre, installations around the world.

To return to the Caltech survey of the 3C sources. One night, in the course of tipping the antenna from one source to the next, Dan unexpectedly encountered a strong source. He went outside to find that the telescope was pointed at the moon. This 'discovery' recalled to mind the studies of the 1.25 cm radio emission from the moon made at the CSIR Radiophysics Laboratory in 1948 by Piddington and Minnett. They had found that the change in the moon's radio emission during the lunar cycle, as it goes from new moon to full moon and back again, is much less than the change in the light. Furthermore the changes in the radio emission lag several days behind the changes in the light. While the light we see is reflected from the very surface of the moon, the radio emission comes from a layer several centimetres thick and the sun must shine on the surface for some days before heat penetrates to that depth. From their measurements Piddington and Minnett were able to infer some of the characteristics of the dust on the moon's surface long before the astronauts landed there.

I suggested to Dan that we measure the moon each night to see what variations occurred at this much longer wavelength of 31 cm. Over the next few nights we found encouragingly large changes: then we learned about *lunar parallax*. Parallax refers to the fact that if you change the position from which you view a scene then you will find that a particular foreground object now appears in front of a different part of the background. Astronomical positions of the moon and planets are given in terms of the directions to the background stars. We had set the telescope to the positions for the moon given in the astronomer's bible, the Nautical Almanac, positions that would apply for a mythical observer at the centre of the earth. However for an observer on the surface of the earth, and displaced sideways from the line running from the centre of the earth to the moon, parallax can change the direction by almost 1 degree. Furthermore the parallax changes throughout the night, as we rotate with the earth. End of that experiment.

It was while we were engaged in this 3C survey that one morning I found a note from Dan on the kitchen table lamenting that the World's Most Versatile Radio Telescope had an Achilles' Heel. During the night we had had the first rain since the telescopes were built, rain being fairly uncommon in this desert country. The cabling alongside the rail-tracks was inside screwed conduit, and raised above ground level, and supposed to be waterproof. However water had seeped into the junction boxes and caused a short circuit. The only compensation was that this rain had fallen as snow on the mountains, and after the sun came out, the silver antennas with the white snow reflected in the puddles of water made a great subject for photographs!

One of John Bolton's major motivations for building the Owen's Valley interferometer was to measure the positions of radio sources with sufficient accuracy to be able to identify the optical objects with which they were associated. The positions measured in our single-dish survey of the 3C sources were not sufficiently accurate for this purpose as the beam of the telescope was 0.8 degree wide. Our positions were meant as a finding-list, accurate enough to ensure that when the two antennas began operating as an interferometer there would be no confusion as to which *lobe* of the interferometer was directed at the source. However even before the interferometer came into operation John attempted to identify the optical counterparts of two of the stronger sources (Hydra A and Hercules A) by using a series of more careful measurements made with the single dish. Since Dan and I were involved in these observations John added our names to his as co-authors of a paper reporting the findings. John had a marvellous 'nose' for research, and often anticipated the correct outcome when, in my view, there was insufficient evidence for his conclusions. In this case I felt John was being 'optimistic' in estimating the possible errors in the measured positions and said that I wanted my name removed from the paper. Some compromise was reached about the wording of the paper and then John put my name as first author! But on this occasion John's intuition let him down; later observations showed that the suggested identification of Hercules A was incorrect.

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That an aircraft company should be involved in studying radio bursts from the sun may sound strange, but at this time essentially all US research in the physical sciences was funded by the military. Indeed, the Owens Valley Radio Observatory was funded by the Office of Naval Research. I don't know the history of the Convair-Caltech collaboration to study solar radio bursts, but Convair provided a swept-frequency receiver which was mounted off-axis on the first antenna to go into operation. Because of my experience with swept-frequency solar studies, John suggested that I should be the Caltech collaborator with this Convair group. Chet Young from Convair, Pomona (near Pasadena) was in charge, and Bill Erickson, from Convair, San Diego, whom I had met at the Paris Symposium and the Moscow IAU, was also somehow involved.

By this time swept-frequency solar observations, similar to those made by Wild and his collaborators at Dapto, were being made at two radio observatories in the US, and the frequency range being recorded had been extended up to 600 MHz. At much higher frequencies, above several thousand MHz, observations made at separated discrete frequencies suggested that swept-frequency observations were not needed: at those frequencies the solar emission changed relatively slowly with frequency and time. There remained the gap between about 600 MHz and 2,000 MHz, and this was the target of the Convair/Caltech swept-frequency project. The receiver scanned from 500 to 950 MHz. It was mounted off-axis on the No 2 dish, and operated daily from March to July of 1959. The equipment was built by Airborne Instruments Laboratory who had earlier built equipment for Harvard, following Paul Wild's visit there in the mid 1950's. Convair provided staff to service the equipment and make the observations; my role was to collaborate in the analysis of the records.

Unfortunately these first observations in this frequency range did not produce great enlightenment. Many of the spectral patterns were unlike those seen at lower frequencies. The examples that we published were dominated by very short duration bursts lasting a second or less, extending across part of the frequency range, and drifting extremely rapidly up or down in frequency. Their relationship to the types of activity seen at other frequencies was unclear.

In the early summer of 1960, in an attempt to clarify this relationship, I spent some weeks at the University of Michigan. There we compared the Convair-Caltech records with the Michigan records for times when Michigan had observed simultaneously in the range from 100-580 MHz, and sometimes also in the range from 2,000 to 4,000 MHz. I had guessed that some of the 'fast drift bursts' seen near the low-frequency edge of the Convair/Caltech spectra would appear as normal Type III bursts at the lower frequencies. They did not. At times when there were patches of broad-band continuum in the higher-frequency Convair records there would be very strong groups of Type III bursts, or compound Type II-Type III events, in the lower frequency Michigan records. However it seemed that most of the activity in the 500-950 MHz range was not closely related to events at the lower frequencies. A fairly disappointing outcome. I wonder what the state of knowledge is these days?

It is said that a picture is worth a thousand words, and a picture is often far more valuable than reams of numbers. In these solar studies, the technique of displaying the dynamic radio spectra photographically was very effective. However it was limited by the non-uniform response across the frequency band, which produced spurious bright and dark ridges running parallel to the time axis. Because of the limited range of intensities that can be displayed in printing - much less than can be displayed in a transparency - these features seem to dominate the published pictures, and make it difficult to appreciate the character of the bursts. Forty years on, looking at these imperfections in the published examples makes me wonder what modern dynamic spectra might reveal.

The Dapto equipment was provided with a cunning, but rudimentary, device to attempt to overcome the non-uniform response. A radial slit of light passed through a perspex disc which rotated with the tuning capacitors, and then fell on a photo cell. The output of the photo cell was used to control the receiver amplification. By trial and error a pattern of photographic 'opaque' was painted on the disc so as to achieve an approximately uniform response when a broad-band noise signal was injected at the aerial. It was moderately successful. I don't recall whether the Convair receiver was similarly equipped. Nowadays, of course, some automatic, computer-controlled, gain modifying system should produce gloriously uniform dynamic spectra and reveal features of the bursts that were hidden from us.

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When John Bolton visited the US the Naval Research Laboratory in Washington in March 1959 he learned that the radio emission from Jupiter at a wavelength of 10 cm was greater than expected. This prompted John to tell Gordon Stanley that Gordon and I should see if we could detect emission from Jupiter at the longer wavelength of 31 cm, the wavelength used at Owens Valley. And that led to the most exciting and interesting research of my career. Before describing this I need to give some background information.

Everything, absolutely everything, you, the chair you are sitting on, the earth, everything, is radiating electromagnetic waves because of the thermal motions of the electrons that are inside all material. The higher the temperature of the emitting material the greater is this *thermal* or *black body emission.* The emission is spread across a wide range of wavelengths, with the maximum emission occurring at a wavelength that depends on the temperature of the emitting material. As the temperature is raised the peak of this emission moves to shorter wavelengths. For very cold material, such as we find in remote space, the peak of the emission is at short radio wavelengths (in the millimetre range). For somewhat warmer material - such as the earth and the other planets, and you and I - the emission peaks in the *far infra-red,* at a wavelength of a few microns (a few millionths of a metre). For hotter bodies - a saucepan on the stove, or a meal in the oven - the peak moves into the range of slightly shorter wavelengths called *near infra-red,* wavelengths that are just longer than the wavelengths of visible light. Further heating moves the peak into the visible wavelengths (red hot), then to the shorter visible wavelengths (white hot) and so on through ultra violet light, X-rays and gamma-rays.

So for bodies at the temperature of the planets, the peak of this 'thermal' emission occurs at a wavelength that is far shorter than any radio wavelength. On the long-wavelength side of the peak the emission decreases as the inverse square of the wavelength. Hence the best chance of detecting *thermal radio emission* from planets is at the shortest radio wavelengths. At this time, on the roof of the US Naval Research Laboratory (NRL), beside the Potomac River in downtown Washington, there was a 50 foot dish with a surface accurate enough for use at centimetric wavelengths. This was a magnificent dish for those times, and the NRL radio astronomers had used it to measure the radio emission from some of the planets. At a wavelength of 3 cm they had found that the strength of the emission from Jupiter was about what would be expected from the known level of infra-red emission. Because of the fall-off with increasing wavelength, it was expected that at a wavelength of 10 cm the emission would be only about one tenth of the 3 cm emission. However the NRL observers found the 10 cm emission was about one third, rather than one tenth, as strong as the 3 cm emission. For a black body to produce so much emission at a wavelength of 10 cm it would need to be at a temperature of about 600 degrees, far hotter than Jupiter was thought to be.

John Bolton's reaction on hearing of this result was to guess that the 10 cm radio emission from Jupiter was some unusual type of radio emission, like the enhanced emission radio from the sun, or the radio emission from the *discrete sources*. No doubt the fact that Jupiter was known to emit peculiar bursts of radio emission in the short wave band - somewhat reminiscent of the sun - strengthened this idea. So Gordon Stanley and I were told to see if we could detect Jupiter at the longer wavelength of 31 cm. Looking back now it is intriguing to realise that this result from the Naval Research Laboratory was reported at the 1958 Paris Symposium on Radio Astronomy which both Gordon and I (but not John) had attended. Neither of us were particularly interested in planets at that time. Perhaps we didn't attend the session where it was reported; if we did hear the report it certainly didn't suggest to us what it suggested to John.

We detected emission from Jupiter. But the bump on the chart record when Jupiter passed through the beam of the telescope wasn't much bigger than the wanderings of the pen. So it wasn't a very accurate measurement. But detecting emission at such a relatively long wavelength had great significance. A *black body* the size of Jupiter that emitted radiation as strong as this at this relatively long wavelength would need to have a temperature of 5,000 to 6,000 degrees! At round about this same time Frank Drake and his collaborators at the National Radio Astronomy Observatory at Greenbank in West Virginia also detected decimetric-wavelength radio emission from Jupiter. At both 22 cm and 68 cm they found about the same intensity (flux density) as we did at 31 cm. Clearly the emission did not follow the 'inverse-square-of-the-wavelength' law: it was not the long-wavelength tail of the infra-red emission.

In writing up our Jupiter observations we first discussed whether the radiation could be *free-free* emission from a *corona* around Jupiter, like the corona around the sun. We concluded that to produce the observed radio emission such a corona would need to be as dense as the solar corona, and at a temperature of 100,000 degrees. While it had recently been suggested that the *solar wind* passing Jupiter would have such a temperature, it seemed that Jupiter's gravity was inadequate to capture sufficient of this wind to make this a plausible explanation.

By this time it was accepted that the intense emission from the discrete sources was *synchrotron emission*. Could the Jupiter radiation be synchrotron emission? Discovery of the earth's Van Allen belts by the first artificial satellites was red hot news. These belts contain charged particles that spiral back and forth along the *magnetic lines of force* that connect the earth's north and south magnetic poles. That is just the recipe for synchrotron radiation, but for the Earth's belts the emission is so weak that it was not detected until many years later. For Jupiter, there was some evidence of a magnetic field ten times stronger than the earth's field, evidence that came from the *circular polarization* of the radio 'bursts' observed in the 'short wave' band. So it had immediately occurred both to Frank Drake and to us, that the unexpectedly strong radio emission might be synchrotron emission from Van Allen-type belts around Jupiter. Putting in the numbers, I concluded that in the proposed Jovian belts the density of relativistic electrons would need to be thousands, or tens of thousands, of times the density of the electrons in the earth's belt. It also seemed that the energy spectrum of the electrons would need to be quite different to the energy spectrum in the earth's belt.

Roberts and Stanley are the authors of this paper on Jupiter and we don't acknowledge that anyone else assisted with the observations. However Rad must have been involved since one of the Rad stories concerns observations of Jupiter made with the No 2 antenna alone, not the interferometer observations that I'll describe in a minute. The Caltech dishes have *polar axes*, i.e. they turn about axes that are parallel to the axis of the earth. That design makes it easy to track a source across the sky as the earth rotates. However it is mechanically awkward: in the Caltech case the polar axis is tipped from the vertical by 63 degrees. To allow the telescope to point to different declinations (latitudes on the sky) the dish itself is mounted on a second (declination) axis at right angles to the polar axis.

Now imagine all this on top of a tepee-shaped tower, and think of the problem of providing limit switches to prevent the dish from hitting the ground or the tower. Some radio sources, sources with declinations similar to the latitude of the telescope, passed almost overhead and could be tracked safely for many hours. But sources with southern declinations rose above the horizon for only a short period of time (if at all). The compromise on the Owens Valley dishes was to allow tracking for eight hours (four hours either side of transit) for sources that passed nearly overhead, and to restrict the tracking of more southern sources to only two hours (one hour either side of transit).

Jupiter fell in the restricted area, but it wasn't very far south and so, in truth, it could be tracked safely for several hours either side of transit. So we shorted out the limit switches and tracked it for several hours. Oh, yes, it was done with John's knowledge. You simply connected a clip lead across some terminals in the base of the tower and were then required to be on your guard and carefully monitor the telescope position. Well of course it happened. Rad managed to run the dish into the base of the telescope, and the top of the jack-screw made a good-sized dint in the mesh of one of the surface panels. John wasn't at the observatory at the time so Rad pleaded with Al Munger to straighten it out before John came back. Once it was fixed I suppose Rad felt it was safe to tell John.

Our publication reporting those April to June 1959 observations of Jupiter concluded by indicating the need to measure two things: the angular size of the radio emitting region, and the polarization of the radiation. If the radio emission was coming from radiation belts surrounding Jupiter, or even from a corona, then the radio emitting region would extend out beyond the disk of the visible planet. The polarization of the radiation would provide information about the way the radiation was produced: *free free* emission from a corona should be unpolarized, synchrotron emission should be linearly polarized.

The polarization of radiation is something that has intrigued me since I first learned about it. Lach Harper and I used to discuss it at great length. I still delight in showing people the effect of twisting their Polaroid sunglasses round whilst viewing the blue sky in a direction away from the sun, an effect that demonstrates the linear polarization of this scattered sky-light. As I explained in an earlier chapter, synchrotron emission, the emission from relativistic electrons spiralling in a magnetic field, is predominantly linearly polarized. How could we determine whether the emission from Jupiter was linearly polarized?

Following the (then) usual custom the *feed horn,* that collects the radiation reflected from the surface of the dish, accepted only one plane of linear polarization. In principle, therefore, all we needed to do was rotate the feed horn and see whether the strength of the signal received from Jupiter changed as the feed was rotated. OK in principle, but the signal from Jupiter is so weak that we could not measure it to an accuracy of better than about 30%. So if we oriented the feed first for maximum response and measured that to 30%, and then turned the feed through 90 degrees and measured that response to 30%, we would have measured the difference - the polarized component - with an accuracy of 60% of the total Jupiter emission. Jupiter is not as highly polarized as that, so we had to wait until we had a more sensitive method of detecting polarization. That came in 1960 when the two dishes were connected together as an interferometer.

Back in Dapto days I had learned about using an interferometer to measure polarization from reading an article by Marshall Cohen in a special radio-astronomy issue of the *Proceedings of the Institute of Radio Engineers* (Vol. 46, 1958). If a pair of antennas that respond to *opposite* polarizations are connected together as an interferometer there will be no response to unpolarized, i.e. *randomly polarized,* radiation. For randomly polarized radiation there is no *phase coherence* between opposite polarizations. However such an interferometer will respond to polarized radiation of an appropriate type. In the system we used each antenna responded to radiation with the electric field in one direction only (one linearly-polarized component), and the feed horns on the two antennas were oriented so that the directions accepted by the two were at right angles. This formed a system that had no response to unpolarized radiation, but responded to suitably-oriented linearly-polarized radiation. It had its greatest response when the maximum electric field of the incoming radiation was at 45 degrees to the two accepted polarizations. To make a complete determination of the linear polarization of the radiation we needed a second measurement with both feed horns rotated through 45 degrees. (Such a system of *crossed linears* also responds to circular polarization - of which more later).

You see the great advantage of this system for detecting small percentages of polarization; it responds to only the polarized component, you don't have to take the difference of two measurements each having has large errors. A similar result can be achieved with a single dish by using a feed with two probes, one for each of the two perpendicular polarizations, and separate receivers for each probe - or a switching system. We used such systems later at Parkes.

For the Caltech Jupiter measurements we wanted to first observe Jupiter with both feed horns accepting the same linear polarization, then turn one horn through 90 degrees to measure just the polarized component. When I told Al Munger that we wanted to be able to rotate the feed on one of the dishes during the observations his response was: 'If I go to the auto junk yard and get a ring gear and pinion from a automobile starter motor then I reckon I could organise that.' And he did, and I was impressed. I think an electric clock motor was used to drive the feed round. We also cooked up an indicator showing the orientation of the feed, and for that we needed some matt, black paint. Just too many Australian *a's* in that for the man in the store to understand me. I finally had to get Dan to tell him what we wanted!

Rad and I made the first polarization measurements when the two antennas were linked together as an interferometer, sitting where they had been built, at a separation of 400 feet. We found the radiation from Jupiter was about 25% linearly polarized, with the maximum electric vector approximately parallel to Jupiter's equator. We took this as strong support for the notion that the radiation was synchrotron emission from a radiation belt. True, George Field had proposed an alternative, that the emission came from non-relativistic electrons, so that the radiation was *gyro* or *cyclotron* emission. But according to this theory the polarization would change rapidly with frequency, and soon afterwards measurements at other frequencies eliminated this possibility.

In April 1960 the No 1 antenna was moved out to the 800 foot station and this allowed Radhakrishnan and Dave Morris to measure the angular spread of the source of Jupiter's radio emission. Recall that the main beam of an interferometer is split into narrower fan beams. As a radio source moves through these fan beams the receiver output varies up and down. While the fan beams are wider than the emitting region these variations are of the full strength of the radio source. However if the fan beams are narrower than the emitting region these variations are, to some extent, smoothed out, and their amplitude is less than the full strength of the source. The width of the fan beams is determined by the separation of the antennas; the larger the separation the narrower the fan beams. But what counts is the *projected* separation of the antennas, i.e. the perpendicular distance between the rays travelling to the two antennas.. As a source is carried across the sky by the rotation of the earth this projected separation, and hence the width of the fan beams changes. The projected separation is least when a source is low down near the horizon, and greatest when it is highest in the sky, at which time the projected separation of the antennas is equal to their actual separation. So the fan-beams are widest when a source is rising or setting and decrease to their narrowest when the source is highest in the sky, i.e. *at transit.*

When the Owens Valley antennas were 400 ft apart the fan beams were always wider than Jupiter's emitting region and the variations were of the full strength of the source. However, with the antennas 800 ft apart Rad and Dave found that the amplitude of the variations changed as the planet moved across the sky. Near rising and setting, when the projected baseline was much less than 800 ft, the variations were of the full amplitude of the source. But as the source moved towards transit, and the increase in the projected-separation of the antennas narrowed the fan beams, the amplitude of the variations decreased. When Jupiter reached its highest point in the sky the amplitude was only about 50% of the full amplitude. From this we concluded that the radio emission from Jupiter comes a region about three times the size of the visible planet, which was certainly consistent with the notion of emission coming from a radiation belt surrounding the planet.

TRIPPING ROUND FROM PASADENA

'I arrived back about 8 pm on Wednesday after driving 550 miles that day and a total of nearly 3,500. I was at the Grand Canyon on Wednesday morning at 7,000 feet up and with snow lying on the ground. I had lunch at the Colorado River on the border between California and Arizona at a place called Needles...Height above sea level 590 feet and surrounded by desert that must stretch at least 200 miles in all directions. What's more the surrounding country is all higher (up to 3,000 feet) so that it really is a hot hole!'

Early in March 1959, when the Convair swept-frequency solar equipment began operating at the Owens Valley, Chet Young decided to visit the Harvard solar radio astronomy station at Fort Davis in Texas. John Bolton suggested that I join him in this visit. Chet flew over, but I made a holiday of it, exploring, and falling in love with 'The Land of Little Rain - South West USA'. I started the trip by heading eastward on the San Bernadino freeway and reported that 'I was amongst traffic almost as thick as Parramatta Road at peak hour for over forty miles - yes all doing 55 to 65 as a steady speed. Again there were those weird and rugged mountains through the murk. The smog was really thick and it was hard to see the mountains at all. Then in all this murk - hardly the place for agriculture one would think - the freeway cuts through "the largest vineyard in the world".'

'The highway out of Palm Springs rejoiced in the name of the Araby Highway. Very appropriate. The whole scene reminded me very much of the trip from Suez to Cairo. Those same desolate, eroded mountains always with this strange haze on them ...Towards the town of Indio there are groves of date palms - "The only place in the western hemisphere where dates are grown commercially." Where is the western hemisphere anyway?'

'So to the (California-Arizona) border. What a surprise. It was like crossing between two European countries. There were guards and places for the trucks and cars to pull in for inspection. I fully expected someone to appear and demand "*Passeport.* Have you anything to declare?" In fact they did make an inspection, in search of citrus with scale, or any plants.' After an overnight in a motel in Salome I was again commenting on those 'weird mountains, eroded and desolate, rising out of smooth rolling country which I guess is made up of material eroded from these mountains. Breakfast at Aquilla - another patch of green. An old timer told me that until a few years ago this was just "jack rabbit pasture"...some large companies have taken up large tracts here and are irrigating with underground water and growing lettuce and small grains on a large scale.' Which reminded me of the signs one saw in Pasadena proclaiming 'Everything you will eat tonight was brought to you by truck.'

North of Prescott, Arizona, 'Here were these marvellous red rock mountains right before my eyes. They were really true and not just exaggerations of Kodachrome....So to the

Meteor Crater. It is the largest known - the second largest is in Central or Western Australia somewhere. Of course it was not a surprise to me as I had read a lot about it and seen pictures of it. But it is truly tremendous - 570 feet deep and 4,000 feet across. Boy there must have been a bang when the hunk of rock came crashing in from outer space and made that hole.'

That night I slept in a wigwam - some enterprising person had built concrete motel units shaped like wigwams - and 'next day I went on to the Petrified Forest...logs, and bits of logs, scattered over the landscape - but they are not logs of wood - they are made of stone, and exceedingly pretty stone...The shattered logs are quaint, but when they are cut and polished they are really things of beauty.' And then 'the Painted Desert which extends for a hundred miles to the north. I admired the rainbows of colour...miles and miles of cliffs...like a layer cake in multicolours.'

'I forgot to mention two points on the way from the Forest to the Painted Desert. One was Newspaper Rock, which is a rock carved with signs by the early Indians. It is certainly cute - but I'd hate to get the newspaper in this form! Besides being even harder to handle than the Herald (!) it is quite impossible to read! The other point of interest was that I crossed the Atchison, Topeka and Santa Fe railway!' And of course I was travelling on Route 66.

I 'left the super highway (speed limit 70 mph) and took a dirt road up to the red mountains. It was interesting to see the Indians with their sheep. The country is very dry, but they shepherd their sheep and apparently find enough for them to eat. The women do the shepherding, and they dress in very colourful shawls and headgear. Earlier in the day I had been twiddling the dial on the radio and had found a very strong station, playing rather odd music and speaking in a strange tongue which was not Spanish (very common on the radio from Mexican stations). Besides, some words, such as "Carnation Milk", came out in English. It was not till I heard the word "Navajo" (pronounced Navaho) that I was sure that this was the Navajo Indian radio that I had read about in the National Geographic Magazine.'

'Back to the highway and...on and on over rolling country with the highway stretching far ahead over hill after hill until on the top of one hill there was Albuquerque below and high snow-capped mountains behind it. So I rolled down to this 100,000 person city, crossed the Rio Grande and soon found Chet Young who had flown out that afternoon. After a night in a motel (Chet stayed in the Hilton at Convair's expense!) we headed south and east past 'Truth or Consequences', then 'south again to Alamogordo, near the White Sands. From there we climbed for 30 miles or so to a height of 9,000 feet and enjoyed a day amongst pine trees at the Sacramento Peak solar optical observatory (run partly by the Air Force and partly by Harvard). We were amused to learn from the notice that we civilians did not get our meals free, but could "subsist by re-imbursement".'

Next day we drove past the truly White Sands, which are gypsum crystals, then south into Texas to El Paso on the Rio Grande, with Mexico across the river. 'Now I saw the first country that I had seen that looked anything at all like New South Wales. Fairly flat grass country that could, I guess, have been on the western slopes...the trees are of quite a different variety , of course. In fact, in places there are whole forests of yuccas...Here there were lots of skunks on the road - dead. Rabbits and skunks - we saw about one every ten minutes of driving.'

'We spent two days at Fort Davis talking to Alan Maxwell and Dick Thompson - the former from New Zealand and the latter from England. Yes, there are some American radio astronomers! Then we went on to see Carlsbad Caverns - limestone caves like Jenolan but on a far grander scale....all told we descended over 800 feet and walked three miles underground. At the end we came up by elevator. I was disappointed by the first part of the tour because there were very few stalactites or stalagmites....But then we came to three rooms that were truly magnificent. They were simply jam-packed with stalactites and stalagmites (the tites come down, as the guide remarked). These three rooms beat anything that I had seen before.'

That Sunday afternoon I left Chet Young at Carlsbad and drove 300 miles north through flat dry grassland that 'looked like a cross between western NSW and Cambridge...It was on this stretch that I did the best average of the trip, 73 miles in 62 minutes, and I was never more than 5 mph over the speed limit!' There were lots of pieces of tumbleweed blowing across the road. 'I thought that they looked like a lot of tiny kangaroos...all hopping as they went.' Santa Fe that night - 'the capital of New Mexico, but it is not nearly as big as Albuquerque....The old town with its narrow and twisted streets is quite European in character and quite unlike any of the west coast of America which is completely modern...a very historic city mixing memories of the Spanish era, Indian wars and the civil war....Just north of Santa Fe is Los Alamos where the atomic bombs were (and I suppose are) made. Along with the district near Alamogordo where they tested the first ones, this country looks very suitable for atomic bombs and not much else.'

Heading westward now, I crossed the Continental Divide - the tail end of the Rockies - at just under 8,000 feet near 'the flourishing city of Durango....surprised to find such a city out here where the roads had very much resembled Australian roads and there had been very little sign of settlement. While waiting for my car to be greased I talked with one of the locals in a cafe and learned that the prosperity is due to oil and uranium.'

'As usual I was running late and arrived [at Mesa Verde] just before sunset so could only see the houses from a distance. Mesa Verde - the green flat-topped mountain - is a plateau with very deep gorges cut into the sandstone, very like the Blue Mountains. It is in the caves in the sides of these canyons that the cliff dwellers...built their houses of mud bricks...Very convenient, you see, you don't have to build a roof, and you get some of your walls built for you too! The houses were built side by side as tenements...at another place there is what is claimed as the world's largest apartment house - built in the 12th century!...these Indians suddenly left their cities in the 13th century....they are really not sure why'.

And so to Four Corners and Monument Valley, which I look back on as the highlight of the trip. Four Corners is the only place in the US where four states meet, Arizona, New Mexico, Colorado and Utah. And those wedding-cake sandstone castles of Monument Valley, that have featured in so many photographs of the south-west US, are just south of the Four Corners. 'It is true desert - mostly sand - and little habitation. On the skyline are the weird shapes - the monuments - and below, the muddy San Juan River lies at the bottom of this tortuous gorge which twists and turns on itself in a series of hairpin bends - the goosenecks....Mexican Hat - the settlement is named for a rock formation which makes a wonderful Mexican sitting under his huge, wide-brimmed hat....and there are the monuments themselves...red sandstone rocks that have been left sticking up above the desert as if they had been constructed as monuments by some prehistoric builders. I drove to various ones and admired and photographed them'

'The Navajo reservation stretches for perhaps one hundred and fifty miles to the south....the roads in that direction are anything but good. I had nearly one hundred miles of terrible dirt road and by the time I got out I was very glad that it was bitumen from then on. Though it is only fair to add that parts of the main highway that I travelled - US 66 - which is the main east-west road in the south - were no better than some of the bad parts of our roads. Parts of it are magnificent, but there are parts which are no wider than our main highways and with a surface which can only be described as bad.'

'I slept that night at the Grand Canyon and the next morning came the climax of the trip - sunrise over the Grand Canyon. It is useless to try to describe the Canyon. It is too immense and too stupendous to give you any real idea of it. The bottom is over a mile below you! But the fascination of the view in the early light is really not due to the immensity of it, but to the myriads of side canyons which run into the main canyon. Each of these stands out in the morning light and makes a wonderful "texture study". Well that is the best place to leave you - gazing at those mile deep gorges and wondering about the power of the Colorado River that cut that mighty valley.'

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We had a road map of the USA spread out on the floor. Rad waved his hand over the whole of the eastern part, where the roads made rectangles on the map, and said 'Forget that flat part'. It was September '59, and we were planning a trip in Rad's VW. In 18 days we travelled up through the western part of the US and into Canada, then back down a little further east. Over 6,000 miles, crossing 10 states, and visiting 13 national parks. My memories of this trip are of the previously-unheard-of early risings by Rad as each morning we left our overnight motel and drove 100 miles before breakfasting in some cafe; of the cafe proprietor who asked about the expansion of the universe; of Rad telling of his father's speculations about the colour of Crater Lake; of that magnificent view of the Oregon coast from Cape Foulweather and the view of Peyto Lake from the Banff-Jasper Highway; the big bear that loomed over the VW; my resembling Rad after the oil heater exploded in the Grand Tetons; the wonder of Bryce Canyon; Rad kicking the lever to access the emergency petrol tank on the VW that had no fuel gauge; and of the Vdub limping home on three cylinders.

Those are the memories that stand out, but I have found a letter that I wrote home describing the trip. There are eleven pages of 'onion skin' paper, closely typed in 'elite' typeface with narrow margins. There must be over eight thousand words. So I can't resist quoting some of it. Starting from the radio observatory we headed north 'up the east side of the Sierras, past Mammoth, and...the huge expanse of Mono Lake and proceeded to climb over a spur of the Sierras. What a view from the top! The whole huge lake stretched out below, with the mountains running away into the distance.'

Through 'sparsely populated high valleys. Here and there a little cattle raising. Green grass. Then we are into Nevada - no speed limit, gambling, easy divorce laws - and the lowest population of any of the 48 states (I'm not sure about the 49th). Carson City, the capital of Nevada, shows us the first of the gaming and then it is Reno with thousands of slot machines. Lots and lots of places like the penny arcades in Sydney with thousands of "one armed bandits", each with its patron madly pushing in nickels, dimes, quarters, halves or even silver dollars. Equally madly they pull the handle after each coin and search to see if the machine has paid them anything. What an absolutely soulless thing. The racket of the machines is almost deafening.'

My writing about Virginia City drew on Irving Stone's *Men to Match My Mountains*. 'Virginia City was the town built at the Comstock Mines...the greatest silver mines in the US. I should quote you the fantastic millions that were taken out of the mines - it helped to build San Francisco and to rebuild it after the fire. It is also said that it helped to win the civil war....Today the "city" is just a few streets staggered up the side of the mountain, with some of the old shops, houses and saloons left to give some idea of the rip-roaring life of the mining days. We duly visited the *Bucket of Blood Saloon*'.

'Lake Tahoe...is a magnificent blue lake - and huge! It would be a wonderful place for a holiday....We headed back into California crossing the new super highway that is being constructed into Squaw Valley...where the winter Olympics are to be held next year....At this point we passed many memories of the Donner party...one of the many bands of people who set out to come to California by wagon train in the second half of the last century....they took a short cut that turned out to be a nightmare...had to cut their own road through the bush over mountains and down canyons....The snows beat them and most of them would never cross the Sierras. It is a terrible story of starvation during the winter, of feuding and fighting, of eating one another, and of tremendous courage by some of the party and by those who sought to rescue them.'

Obviously the accounts in *Men to Match My Mountains* had a big effect on me! But the story from that book that I recall without referring to letters concerns the construction of the transcontinental railroad, built from both ends and ceremoniously joined by driving a golden spike. According to Stone many famous people - Stanford, Huntington, Hopkins, Crocker - made millions from this venture. The government paid them so much per mile for building the track, the price rising as the grade increased. These entrepreneurs redrew the maps of the country so as to make the grade steeper and be paid more!

We reached 'Lassen Volcanic National Park at the time of day that became fashionable for us to make the national parks - sunset...The road rises to 8,500 feet, and the mountains in the garish light of the sunset were some 2,000 feet above us. We saw lots of deer, squirrels and chipmunks running across the road.' That night we 'got talking to the chap and his sister who were running the motel and cafe, cum general store. They told us that the spot for the University of California (Berkeley) radio telescope was not far away. I was completely staggered when this chap asked me if there was something in radio astronomy that corresponded to the red shift in optical astronomy, and so allowed us to study the recession of the galaxies (expansion of the universe). Here was a chap way out in the middle of nowhere, running a cafe, and yet he was sufficiently acquainted with, and interested in, these things to be able to ask such a question.' (And as an aside about memory: I have often recalled this event, but in my recollection it was the girl who asked the question!)

'Next day we headed up US 99 and really saw those road makers at work. Huge yellow carryalls crawling over the mountain - parts of the mountain missing as they carved out a new route for the road' - a common enough sight here in Australia in the 1990's, but quite unknown in the 1950's. 'Soon we entered Oregon...Logs and logs. Huge logs on trailers. The logs so large sometimes that only three logs make a load. Logs in the water - hundreds of them, and lumber mills everywhere.'

And so to Crater Lake. 'A tremendous blue lake in a crater 6 miles in diameter. Rugged peaks all round the rim. What a wonderful cacophony of colours with the deep blue of the water and the reds and oranges of the evening sky. There are reds and yellows on the rocks, too...The lake is 2,000 feet deep and the craggy rim rises to 2,000 feet above the lake...Rad was very interested to see the actual colour of the water...Rad's father had apparently seen this colour and theorised on the cause of it.'

'We reached the [Oregon] coast at Florence...There were wonderful stretches of surf and sand, then the cliffs came right down to the sea and the waves crashing against the rocks made a wonderful sight in the sun...the high point was at Cape Foulweather, near sunset, where we looked south over miles and miles of breaking waves curving away down the coast.' High point indeed; I often project the slide I took that afternoon so as to recall that view. 'We went down through tall timber to the confluence of the Hood and Columbia Rivers...The river is dammed further down...we took a quick look at the locks...but spent most of our time looking at the fish ladders and at the fish going through the counting station. The ladders are a whole series of shallow steps with water running over them to form a cascade that the fish can swim up - with just a bit of a kick to get over each step. At the top they have to pass through a smaller opening where they pass in front of a white screen. A chap sits in a small observation room and counts and types each fish as it passes.

They count for 55 minutes out of each hour and usually count for between 8 and 16 hours a day. Quite a job! There had been over 10,000 fish in the last few days - mostly salmon.'

'On down the valley with lots of logs floating in the water now. At one point we raced to get ahead of a train and stopped to photograph it. It stretched as far as we could see when only half of it had passed us. We guessed that it must have been nearly two miles long!' Heading north into Washington State we went 'through the Mt Ranier National Park. We quite agreed that it was appropriately named...we never did see the mountain....We spent another wet day going round the Olympic National Park. We saw even less this day, though we did photograph an impressive truckload of just three logs.'

'Wednesday night we made Canada. There were no difficulties about the border crossing, but Rad had to sign a form to say that he'd be willing to be deported if he didn't get his American visa renewed OK in Vancouver. We haven't discovered why you have to go outside of the US (in this case to the American Consul in Vancouver) to have the visa renewed...As to why they didn't give Rad a decently long visa in the first place...according to the Consul in Vancouver it is some sort of a fight (or arrangement) between the US and India that they are only allowed to give Indians a visa for 6 months at a time.'

In Vancouver, at the University of British Columbia, we saw the totem poles. 'I was surprised to learn that they are peculiar to the peoples of the north-west coast.' I remember the totem poles, but I don't remember writing the following: 'Rad put into considerable words the thoughts we had been having about the differences between the people there and here (i.e. at UBC and Caltech). He started by looking round at all the girls and commenting that we were obviously at the wrong university (there are no girls at Caltech - at least as undergrads). We went on to agree that the girls there (at UBC) seemed still to have that freshness of youth, a smile, a light in the eyes, that is so lacking in So California. Their dress is more pleasing - they show more taste in the selection of make-up and clothes....Rad summed it up in part by saying that they (boys and girls) looked more like university students and less like Beatniks, than their southern brothers and sisters!' These comments resonate with me now: I have much the same thoughts as I look round in a bus or train. Where are the smartly dressed, attractive girls, full of fun, with 'a smile, a light in the eyes' that were on every train that I took in my youth?

'Friday...Near Penticton...the White Lake Radio Observatory, otherwise known as the Dominion Radio Astrophysical Observatory...their Kennedy 84 foot antenna had just been tilted for the first time....That evening we were invited to John Galt's place and Rad and I commented that it was the first home that we had visited in this country and not been offered drinks! I don't know whether this is a comment on US v Canada, California v Canada - or whether it is merely a statistical fluctuation. Anyway we had a very pleasant evening there.' (In case you are wondering, offering drinks was very *un*common in the society that I was used to in Australia.)

'Now we were headed south...back into USA for lunch' and 'the Grand Coulee Dam - claimed as the greatest man-made structure...It is hard to get the correct impression of the size of this wall of concrete which towers above and stretches across, dwarfing a whole town. The dam is almost a mile long and the water pouring over the spillway makes a waterfall twice as high as Niagara! There are 15 acres of snow-white foaming water, topped by a wall of green "glass"...15 feet thick.' It was hard to believe that that smooth wall of water going over the top was not solid glass.

'Next day it was Northward Ho...through the rain. We...entered Canada again...following up the valley of the Kootenay...just before dark we had our first fun with a bear. We had both been looking forward to bears as one of the high points of the trip, and there was one standing by the road - a big black one. We had been warned that they can be dangerous and so obeyed the instructions and stayed in the car. He came and wandered around the car, and while we were both adjusting our cameras I suddenly heard an imprecation from Rad damning the bear. Rad had opened the small front window (quarter window) and poked his camera out, and the bear had taken a swipe at it and knocked it out of his hand! Fortunately he hit it into the car, and there appeared to be no real damage. Just a lot of water from one very wet paw.'

'Now we had 18 miles of the worst road in the dark. Mud and gravel - the section of the road that they are still building - then half way between Banff and Lake Louise we joined the razzle dazzle new Trans Canada Highway and were soon warm and dry in the Lake Louise Motel. Next morning found Rad outside chasing the elk in his pyjamas. He was after a photo...but it was still early and the light a bit feeble...And so to our first view of Lake Louise. What superb reflections of the autumn tints in the mysterious greenish blue waters of the lake. There were still clouds swirling around the mountains, but we did get occasional views of the peaks.' When the mountains cleared later in the day the scene was just like the picture that hung in the hall of my childhood home. My Father had visited Canada and Alaska before I was born, and that picture in the hall certainly captured the peace and tranquillity of those reflections in Lake Louise. 'That morning we went...over Kicking Horse Pass...to Yoho National Park...to see the spiral railway tunnels...the Canadian Pacific...climbs up like a corkscrew! I had seen such a railway before - in fact I had travelled on it - in Switzerland down near the St Gothard tunnel...we took a side road...to see Emerald lake. The lake was superb. Such a green as one does not expect in a lake, and blending so well with the coloured leaves of the trees in the fall.'

'That afternoon was grand. The sun came out and we had sunshine and blue sky and majestic mountains...time was getting short but we drove north on the Banff-Jasper Highway as far as Lake Peyto. This is a magnificent new highway and that drive certainly stands out in my memory. After the rain it was wonderful to see the sun come out and the sky turn blue...And what mountains! Huge blocks of rock. You could see the way that these great mountains had been heaved up - the layers of rock just ran up into the air and then stopped...We turned round at Lake Peyto which I think is my favourite of the lakes that we saw. A deep blue, it nestles at the feet of all those mountains, and from our vantage point we looked out over it and to a tremendous panorama of mountains to the north.'

'It was nearly sunset as we came to Vermilion Lakes to the north of the town [Banff] and the reflections of the mountains and the sunset were something that I shall long remember. There were beaver ponds here too. We saw lots of trees that the beavers had eaten through to make their dams, but we didn't manage to see them at work.' Then, headed for Calgary: 'In 50 miles we were out of the Rockies and onto the prairies. It was a very sudden change. Of course the mountains that we had been in are not as high as the Sierras...but in some ways they are more dramatic.'

'We headed south on some rotten roads' to the 'International Peace Park. In the Canadian section - Waterton Lakes National Park - we saw lakes, waterfalls, and mountains, but somehow it seemed tame compared to Banff. Then we crossed the border at Chief Mountain - a huge block of rock - and entered Glacier National Park. We climbed across the Continental Divide for the fourth time on the "Going to the Sun Highway"...This was scenery on the grandest scale. Colossal peaks enveloped in clouds, waterfalls everywhere....probably the more impressive because the weather was not the best and the mists and clouds added to the awe of these tremendous mountain passes.' We 'skirted around the top of a huge cirque with, as Rad said, a waterfall at every turn. And here we met our third bear. He was a big brown one and was standing on the wall beside the road, surveying the mists in the tremendous valley below us. When we stopped he came and stood up beside the car with his paws on top and Rad took close-ups of his face from below at a distance of one foot! He seemed friendly enough but we took a little more care this time! I thought that he would have scratched the duco, but there didn't appear to be any damage.'

'What a contrast the next day as we went on south around the shores of Flathead Lake (named after the Flathead Indians). Here there was dry grass and I felt much more at home! It was a beautiful blue lake - yes, there was sunshine for us to enjoy it...I was very surprised when we passed the headwaters of the Missouri way up here at a place called Trident...Soon we turned south and slept that night at the entrance to Yellowstone National Park...Yellowstone brings memories of myriads of displays of steam issuing from the ground, of hundreds of little geysers, wonderful displays of colours in the hot springs - greens, yellows, reds, and orange - truly amazing. There were wonderful pastel tints in the white terraces at Mammoth Hot Springs - wonderful colours and such a variety. We saw boiling mud pools, though many of them were too wet to plop nicely; we saw Old Faithful, who was certainly faithful, though hardly majestic.'

'The bears were great fun. Here they really put on a show. They would come and stand up beside the car and one huge one really did scratch the car a bit. There were several with cubs, and we saw one cub holding on to the side of a car actually pull himself up off the ground so that he could look in through the window. They are such friendly looking chaps with very beguiling expressions on their faces...We crossed the Continental Divide five times in Yellowstone, and at one point passed a lake on the Divide which flows both ways!'

'We slept that night at Signal Mountain in Grand Tetons National Park...Alas, it was raining, and we didn't see the Tetons. However I provided a little amusement for Rad.' There was an oil heater in the cabin where we stayed. During the night it went out, and produced great clouds of evil-smelling smoke. 'I turned it off...and waiting dutifully until the fumes disappeared to avoid an explosion...I dropped a match into it when I thought that it was completely safe. There was a whoof, and soot went everywhere - particularly on me...Rad thought that the startled look on my face, the soot covering me, and my assurance that I was all right, was the best bit of slapstick comedy that he had seen for a while!'

'That day we hurried to Salt Lake City. We were behind time and the VW was down to three cylinders. She was still going well, but we were anxious to get to Salt Lake City and get some advice. We went down the western side of Wyoming, across the corner of Idaho, and into Utah. We crossed at least three mountain passes, the last one near the Utah border taking us to 7,800 feet. There were lots of beautiful autumn tints in the trees on the hillsides...Many of the higher mountains were snow capped and this added greatly to the effect of a pale blue sky with white clouds, misty-bright sunshine, the autumn tints of the leaves and soft green of the grass.'

'Then we were in the Mormon capital...It was too late for the VW service place, but they still attended to us! It turned out that Rad had one of a certain number of VW's that were fitted with extra long valve guides. These didn't prove a success as the carbon collects on the ends of the guides and ultimately stops the valve from seating. Then the valve burns out and you are one cylinder short...that's the way we were, and that's the way we drove home, including over 600 miles in about 14 hours on the last day.'

A quick look at Salt Lake City, including 'the outside of the temple (Mormons regard their temples as places of worship and not tourist attractions, and they are open only to "Mormons in good standing"), and the tabernacle with its famous organ. We learned a little about the Mormon religion...They certainly must have had the courage of their convictions to make that trek west and establish this city in the desert.' Then 'we put miles behind us and slept that night in a very comfortable cabin in Bryce Canyon National Park.'

'What a thrill the next morning to walk to Sunrise Point and gaze down on that entrancing valley. How to describe it? Shall I say that it was a fairyland full of enchanting pink and white castles? Or shall I say that some army of sculptors had in ancient times carved myriads of fantastic figures from this pink and white limestone? Whatever I say can't convey to you the impression that place made on me. It is out of this world. Perhaps pictures of it will give you some idea of the jungle of columns and pinnacles that fill the valley - perhaps they will show you the amazing colours of these weird rocks - but really you will have to experience it yourself to understand its fantasy, to appreciate the millions of columns and steeples that cram the valley, and to marvel at the exquisite beauty and yet grotesque grandeur of the place.'

'Next stop was Zion National Park. In its own way this is just as fabulous, but for me it was dwarfed by Bryce canyon. Zion is completely different. No myriads of pinnacles here but enormous smooth walls of pink sandstone...Then it was on again through desert and over mountains until slowly the land flattened...And then we were in Las Vegas. Millions of brilliant lights in the desert. Lots of fancy buildings and again huge rooms full of one-armed bandits and people pouring money into them. I must say it is beyond me. Onward once more through Barstow, San Bernadino and finally home.'

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As I have related, in the early summer of 1960 I spent a period working with the solar radio astronomers at the University of Michigan. This was in those flat lands of the mid-west that Rad had dismissed with a wave of the hand. I drove to Ann Arbor via Boulder, Colorado, another centre of solar studies, crossing the Continental Divide at something over 10,000 feet. Did I ski there? I know that I took my skis and left them at Boulder for Hal Zirin to sell for me. At that time I assumed that I had spent my last winter in North America and would return to Australia in the following September. In the event I stayed on until March (### ?) of '61.

Ann Arbor was delightful: a riot of rich green and such a contrast to the deserts of the west. Even the surrounding flat cornfields attracted my camera. What's more the University teemed with girls. One lass, Christine, became well known to solar radio astronomers. A student of music, and later a concert pianist, she spent her summer vacations working for Fred Haddock in the radio astronomy department. Her intellect matched her beauty; I'm sure she could have succeeded in any walk of life. I worked with the Michigan radio astronomers comparing the Convair/Caltech spectrograph records of solar radio bursts with records in other frequency ranges made simultaneously at Michigan. After several weeks spent poring over the records we produced a joint report, but the seeming lack of connection between events in the different frequency ranges was rather disappointing.

Leaving Ann Arbor I drove to Toronto, Canada, to meet my sister Helen and her friend Edna, who had flown over after tripping round Europe. Together we drove through eastern Canada - Ottawa, Montreal, Quebec, New Brunswick - and then, with memories of that song that my cousin Benedicta used to sing,

> Riding down from Bangor
> In an eastern train,
> After weeks of hunting
> In the woods of Maine,

we drove on down through the US to New York. Here we showed what country hicks we were. Staying overnight downtown in the YWCA, we left most of our belongings in the car parked outside. The morning revealed a smashed window and numerous things missing, including both my camera and Edna's. Seeing a pair of 'New York's Finest' walking nearby I approached them to report the incident. That nearly caused another incident - you don't approach New York policemen from behind! When they learned that we had left clothes hanging up in view in the car they implied that we had asked to be robbed. Unfortunately, nowadays, that is also the situation in Australia, but it was quite a new idea to us then.

I left Helen and Edna in New York, or was it in Washington? - left them with the keys to my car and to my apartment. While they drove RKN 254 across the States and installed themselves in my apartment in Pasadena, I went on to Europe. I flew to Paris on a MATS (Military Air Transport Service) plane. That was the way you travelled on a project financed by the Office of Naval Research, so it was obviously an official trip. However the basic reason for the trip escapes me. The only records I have are a few scrappy notes, started with enthusiasm on the 26th July while sitting in an outdoor cafe in Paris, but trailing away after that. I visited the University of Bonn radio observatory at Stockert, and the notes record 'Kann ich Frühstück essen um halb acht? "Yes, sure you can" and the whole place collapsed in laughter'. Also 'Tennis this afternoon with the crowd from the observatory. Best since Australia.' Peter Mezger was a keen tennis player.

I recall that I attended a conference about the ionosphere in Kiruna, just inside the Arctic Circle in Sweden. Unfortunately it was too late in the season for the midnight sun! In Holland, in the castle of Nyenrode, I attended a residential summer school in astronomy. I did learn some astronomy, but other matters dominate my memories of that school: the bread and chocolate 'decorettes' served at breakfast; the marvellous Apfel Strudel sold in the local village; and sailing on a nearby lake with Don Mathewson. Don, on leave from the Radiophysics Laboratory while spending a period at Jodrell Bank in England, was also a summer-school student. I seem to remember that our afternoon of sailing ended with my sailing the boat back alone as Don had decided that a girl in another boat was better company!

Sailing with Don in Holland was the forerunner to our sailing together on Sydney harbour in the early days of the Parkes Radio Telescope. For a season we shared the ownership of a small centre-board dingy, a VS (Vaucluse Senior). It was strictly a boat for racing; the mainsail and jib had to be 'raised' with the boat lying on its side on dry land. No ropes - sorry, sheets - were going to add to the air resistance on this boat. The boat might have been optimised for racing, but we weren't, and when a strong southerly happened along we usually spent a good deal of time in the water. One day when the southerly arrived the bolts that held the rudder assembly to the stern of the boat pulled through the timber. That left us wallowing in the water until a rescue boat came to give us a tow, and during that time I became so cold that I could not have cared if several sharks had come and eaten me.

Another memorable moment from that European trip concerned the flight home. The MATS flight left from somewhere in Scotland. Of course, in those days aeroplanes had propellers; jets were not yet the standard. As we raced down the runway for take-off, the pilot suddenly reversed all the engines and slammed on the brakes, coming to screaming a halt with a few yards to spare at the end of the runway. He then announced that he wasn't satisfied with the engine performance, and we returned to the terminal to wait for some hours while matters were adjusted to his satisfaction. Better that way than he became dissatisfied in the middle of the Atlantic!

THE PARKES RADIO TELESCOPE

Did we have streamers when the Caltech crowd farewelled me at Longbeach for what turned out to be my last journey by ocean liner? If so, it would have been unusual: ships leaving Longbeach weren't farewelled the way they were in Sydney. There was touch of Australia when the ship reached Honululu and I teamed up with two Queensland girls and an American guy to rent a red convertible. We put the top down and drove off to spend the day exploring the island, and at the Sacred Falls the two Queensland girls just whipped off their shoes and walked through the mud - irrepressible Aussies. A few weeks later I was home, being somewhat taken aback by the shabby appearance of Parramatta Road, and wondering, as I listened to my Mother speaking, 'Does my Mother really speak like that?' We might have ragged the Americans about their accents, but after two and a half years of American twang I found Australian voices rather strange. Then it was back to the Radiophysics Laboratory; but no longer as a *solar* radio astronomer. Now I shared an office with John Bolton, director-designate of the Parkes Radio Telescope-in-the-making.

The Parkes Telescope was Taffy Bowen's creation. He wanted the Radiophysics Laboratory to have a bigger and better dish than the 250 foot dish being built at Jodrell Bank by the University of Manchester. Joe Pawsey, the pioneer of radio astronomy in Australia, and the head of the radio astronomy section, instead favoured further development of interferometer systems. The Laboratory had been a world leader with innovative antenna systems, such as the Mills Cross and the Chris-Cross. Pawsey believed the future lay in that direction. A large dish, like the Parkes Telescope, has the advantage of versatility; it is relatively easy to mount receivers of different frequencies, and accepting different polarizations, at the focus. However to pin-point the direction of a source of radio waves, and to map the extent of areas of radio emission, requires aerial systems that are far larger than the largest dish that can be constructed.

Unfortunately this conflict of ideas split the Radiophysics Laboratory. When Taffy Bowen's proposed Giant Radio Telescope won the day, both Bernie Mills and Chris Christiansen left. They moved to Sydney University where they could pursue their interests in innovative antenna designs. Perhaps that wasn't too bad - the youngsters setting forth to make their own mark in the world? Joe Pawsey was the tragedy. With the Parkes telescope nearing completion, and John Bolton returning from Caltech as Taffy Bowen's man to run the new telescope, Pawsey decided to accept a position as the head of the recently created US National Radio Astronomy Observatory at Green Bank, West Virginia. However, on an inspection visit before taking up the appointment, he suffered a brain tumour which left him partly paralysed. He battled on as head of radio astronomy at the Radiophysics Laboratory, using his last few months to edit a special radio astronomy issue of the *Proceedings of the Institute of Radio Engineers (Australia).* But what a sad ending for a man who contributed so much to Australia's prestige in international scientific circles.

Would Australia would have made a greater contribution to radio astronomy if the available resources had been devoted to developing multi-element antenna-systems rather than to building the Parkes Radio Telescope? That is a matter of opinion. One thing is clear, there would never have been sufficient funds to support both, and it was Taffy Bowen's passion for a big dish that drove him to raise funds for that project. He persuaded the Carnegie Institute and the Rockefeller Foundation to each donate $250,000 to the project, provided that the Australian Government matched their donations dollar for dollar. The Australian Government was then persuaded to come to the party, and the Parkes Telescope was born.

The Parkes dish was immensely successful and is still doing excellent work forty years later; certainly a tribute to the care taken with its design. Taffy was critical of the design and construction of the Jodrell Bank dish, the first very large dish ever built. He was determined to learn from the Jodrell experience, and spent more than 10% ### of the available money on design studies. Freeman Fox of London, incidentally the designers of the Sydney Harbour Bridge, had the main design contract. Harry Minnett, a pioneer of millimetre-wave radio astronomy, and in later years a chief of the Division of Radiophysics, spent several years in London working with Freeman Fox, acting as the liaison with Bowen.

Thanks to the movie, *The Dish,* many people now know that the Parkes Radio Telescope is a colourful sight - but let's hope they don't think that people behave the way the actors behaved in the movie! The telescope sits out in the middle of sheep paddocks, in a flat valley 15 miles north of the town of Parkes, in the *Goobang Valley*, as Dick McGee liked to remind us. Remoteness from sources of electrical interference but easy access from Sydney, some local technical facilities, and a sufficient area of flat land for possible future interferometry were some of the requirements that led to the choice of this site. The area is far enough from Sydney to escape most of the radio interference associated with that city and is shielded from local Parkes town interference by a range of hills. The railway workshops in Parkes provided technical facilities and a three-times-a-day air service linking Sydney and Parkes provided easy access. The air service was such an advantage relative to the five-hour drive we had experienced from Pasadena to the Owens Valley that John Bolton stipulated air travel from Sydney as the norm. Those opting to travel by car were required to take recreation leave for the extra time involved.

On my first visit to Parkes I saw some of the ribs of the dish being lifted and connected to the hub. At the Owen's Valley I had watched the whole dish lifted, but the Parkes dish, 210 feet (64 metres) in diameter, was too large to be lifted as a unit. Instead this dish was built in situ, following a trial erection on the ground to make sure that everything fitted. On my next visit I watched as the spray painter gaily walked out along those ribs with no safety line, but carrying the spray gun weighed down by the hose which connected the spray gun to the compressor on the ground!

When the dish structure was complete and the reflecting mesh surface installed, John gave me my first job with the telescope. To establish the shape of the dish a special theodolite was mounted at the centre of the dish and used to survey a series of 'targets' distributed over the surface. John asked me to analyse these survey measurements so as to determine how closely the surface approximated a paraboloidal shape, and hence to determine which adjustment screws should be turned to improve the dish shape. Norm Broten, a radio astronomer from the Canadian National Research Council, joined me in this task. Norman spent several years at the Radiophysics Laboratory, partly to study the Parkes telescope in preparation for the construction of a similar instrument in Canada. This job of analysing the survey results was soon taken over by Don Yabsley, and for many years thereafter, working in collaboration with measurement specialists from the CSIRO National Measurements Laboratory, Don continued surveying and improving the surface of the telescope.

Once the dish could be tipped John had me work on testing the 'pointing' of the telescope, i.e. checking that the radio beam pointed in the direction shown by the indicators on the control desk. As I explained in connection with the Caltech radio telescopes, the rotation of the earth causes all celestial bodies to move across the sky in just the way that we see the sun move across the sky during the day. The problem of tracking this motion was simplified for the Caltech telescopes by adopting the practice traditionally followed with optical telescopes: the dishes were *equatorially mounted.* In other words the main rotational axle of each telescope was set parallel to the rotational axis of the earth. Such an axle is called a *polar axis*.

Using an equatorial mount simplifies the problem of tracking a source against the rotation of the earth, but having the main rotational axis tipped from the vertical causes considerable mechanical problems. For a structure as large as the Parkes Telescope an equatorial mount was considered undesirable. Instead it is mounted *alt-az*, i.e. it tips in altitude and rotates in azimuth. The symmetry of this form of mount reduces the mechanical problems. Tipping in altitude and rotation in azimuth are achieved by mounting the dish on a horizontal axle that is housed in a structure that turns about a vertical axis. At Parkes this structure is called the *turret*: it is indeed like a very large gun turret. The dish and turret structure, weighing 1,000 tons, is supported on four 21 inch-diameter rollers. These roll on a 23 inch-wide flat track, 37 feet in diameter, that sits on the top of a three story tower.

While this alt-az design reduces the mechanical problems, you are then left with the difficulty of tracking celestial objects across the sky. That is no problem with a modern computer: in a fraction of a second it can calculate the altitude and azimuth corresponding to any given *right ascension* and *declination* (longitude and latitude on the sky). However in the 1950's digital computers were in their infancy, and most alt-az mounted radio telescopes achieved the equatorial to alt-az conversion by using strange-looking mechanical devices - interconnected mechanical analogues of the two mounting systems.

The Parkes telescope used a new system devised by Barnes Wallis, an innovative designer of airships, aircraft, and other structures, and perhaps best known for proposing the bouncing bombs used by the *Dam Busters*. Running up through the centre of the tower, and the turret, there is a column which has separate foundations so that it is completely isolated from the radio telescope structure. On top of this column there is a small, equatorially-mounted guider. This instrument (the *master equatorial*) is driven accurately in right ascension and declination, and then a servo drives the alt-az mounted radio telescope to follow the guider. The servo is operated by a light beam that shines from the back of the radio telescope (inside the turret structure). A flat mirror on the top end of the guider telescope reflects this light beam back to error-detecting photo cells on the radio telescope. Any misalignment detected by light falling on the photocells causes the appropriate alt-az motors to be energised so as to correct the misalignment. Many of us have fond memories of Jack Rothwell (from Associated Electrical Industries) who had the job of correcting the teething problems in this servo system. You would often see Jack standing motionless, sucking on his pipe, watching the telescope going through its paces, as he considered how to improve the performance.

To ensure that this system points the beam of the radio telescope in the direction indicated by the master-equatorial readouts two things are necessary: the mirror on the end of the guider telescope must point in the direction indicated by the master equatorial dials, and the axis of the error-detecting system must be aligned with the radio beam. The pointing of the guider telescope was checked by using it to observe stars of known position. When some cover-plates are removed from the radio telescope it is possible to observe stars through the guider, with the radio telescope then acting like a cumbersome telescope-dome. It was fortunate for me that just as we started on this job John Whiteoak arrived to spend some time at the Radiophysics Laboratory. John had just completed his PhD in optical astronomy at Mt Stromlo observatory, so he knew something about stars and optical telescopes. Under John's guidance I undertook my first, and only, optical observing. Pretty fundamental stuff too, measuring star positions.

Once the master equatorial was adjusted, we had to determine the misalignment between the error-detector axis and the radio beam. To do this we followed a radio source across the sky, continually measuring the (apparent) position of the source as indicated by the master equatorial readouts. A misalignment causes the measured position to change as the source moves across the sky. To understand this think of the way that the sun moves across the sky in the course of a mid-summer day in a place like Sydney. During the morning it is in the east, and then in quite a short time near midday it moves round to the west as it passes nearly overhead. Radio sources that pass nearly overhead have a similar rapid change of azimuth near *transit*. With the Parkes system if, for example, there is an *altitude* misalignment error, then before transit there will be an error in right ascension. As the source passes nearly overhead at transit this error will change to an error in declination, and after transit it will change back to an error in right ascension *but of the opposite sense*. By following radio sources through this transition the misalignment error could be determined and the error-detector system then adjusted to remove the error.

It was while John Whiteoak and I were engaged in these 'pointing' observations that a reporter from one of the Sydney Sunday papers visited Parkes - in those early days the Parkes Telescope excited quite a lot of media attention. In the resulting newspaper article the main items of interest seemed to be my red jumper, and the lunch basket that we took to the tower with sandwiches for a midnight snack!

Determining the positions of radio sources on the sky was central to John Bolton's interests. Over the years he carried out or organized many observing programmes at Parkes to measure the positions of radio sources and so identify their optical counterparts. For him, monitoring and improving the 'pointing' of the telescope was a continuing project. I was heavily involved in the early observations made to determine these 'pointing errors', but I slowly moved on to other projects.

A few words about observing at Parkes in these early days might be in order. Because the *light* from the sun is so dominant compared to the light from any other cosmic source, optical astronomers work only at night. Not that sunlight would enter the telescope *directly* during the day; the problem is *scattered* sunlight. Even the sunlight scattered by the molecules of the air is bright enough to spoil the observations. In the radio band, the emission from the sun is not as dominant, and some daytime observations are possible. However at Parkes we soon found that sensitive observations could be spoiled by solar radio emission scattered from the tripod legs that support the focus cabin. So most observations were restricted to night-time, with the daytime mainly reserved for maintenance and improvement of the telescope systems. Teams of astronomers coming up from Sydney would be allocated a series of nights, or half nights, and would spend the remaining part of the 24 hours catching a little sleep and reducing their records in preparation for the next night's observing.

The job of controlling the telescope during observations was handled by 'drivers'. They were mechanical or electrical technicians by day, and worked half-nights on rotating-shifts as telescope drivers. John Bolton felt that these technicians, many of whom had worked on the telescope during its construction, had a far better understanding of the telescope than the radio astronomers who came up from Sydney for short periods. The driver sat at the impressive-looking control desk. On the desk there were easily-read, numerical displays showing the direction the was telescope pointing, the standard time, the sidereal time, the wind speed, etc. They were 'digital' displays, not dials, but the whole system was purely mechanical; computers and digital recording were in the future.

Movement of the telescope was controlled by four large knobs; one pair of knobs for the master equatorial, and one for the radio telescope itself. During observations the radio telescope was slaved to the master equatorial with the movement of the radio telescope being controlled by moving the master equatorial. Driving to a new position rapidly when the telescope was slaved in this way presented a challenge. When the master equatorial is driven rapidly the radio telescope lags behind, and large errors build up in the servo system.

If there are large errors at the end of the drive then the radio telescope overshoots the final position and oscillates back and forth for many seconds, wasting observing time. John Bolton, I, and some of the drivers, enjoyed the challenge of varying the acceleration and deceleration of the master equatorial in such a way so as to make a move in the shortest possible time while avoiding any overshoot. In later years, when computer control of the telescope became available, Paul Rayner had some trouble in developing a computer programme that imitated these driving techniques.

In the 'slaved' condition the speed couldn't exceed 2½ degrees per minute, and to move the telescope a large distance across the sky it was quicker to 'break lock' and drive the radio telescope and the master equatorial separately. In that mode the master equatorial could move at 60 degrees a minute and soon be in the new position. Meanwhile the radio telescope, limited to 15 to 20 degrees a minute, would be set lumbering round towards the altitude and azimuth appropriate to the new right ascension and declination. These alt-az values would have been read from a book of conversion tables that had been calculated with that pioneering CSIRO electronic computer CSIRAC ###. On the control desk a model showed where the guider and the radio telescope pointed. Once the model indicated the two were sufficiently closely aligned the driver would push an 'auto-lock' button, locking the two together. Thereafter the radio telescope would follow the guider, provided the guider was not driven above 2½ degrees per minute.

The outputs from the radio receivers were displayed on chart recorders. Apart from determining the observing programme, the astronomer had the job of annotating the chart with information such as the date, time, name of the source being studied, telescope position, etc. A hand-held button could be used to make a mark on the chart at the moment the desk readouts indicated that the telescope passed through a certain sky position, a system that was soon superseded by having the markers triggered automatically from the read-outs. The observer also had the job of keeping the ink flowing properly through the pens on the chart recorders which was a perpetual headache that led to many dyed hands! There was plenty of space to write on the chart paper, and some of John Whiteoak's frank comments written during the pointing observations lightened the heart of those of us who came to reduce the records the following day!

The telescope designers provided an electric typewriter on the control desk that kept a log of telescope operations. Every few minutes the solar and sidereal time, telescope coordinates, wind speed, etc were printed on a paper roll. We soon dispensed with the typewriter, but we had a bit of fun with it first. Most of the characters being printed were numbers, not letters. Now the 'upper case' of the numbers on a typewriter keyboard are those odd symbols like %+)(&£. So if you depressed the 'caps lock' the printout looked like something mad scientists might understand; a lovely display to 'impress' visitors. And we had visitors in those early days. One Sunday each quarter was designated Visitors Day and the public streamed through the telescope in their hundreds while the technicians and astronomers showed them around and talked about the telescope and its uses.

When regular observations began I was involved in several programmes in which we first checked the position of the source we wished to study, and then made a series of 'on-off' measurements to establish the strength, and sometimes the polarization, of the source. To help us follow this routine Doug Milne constructed a timer box, using a telephone uniselector. Different lights indicated when to move the telescope on or off the source, while other lights indicated when the feed should be rotated. Crude, but useful, it was the forerunner of the very successful computer programme - another Paul Rayner effort - written years later when computer control of the telescope movement, and digital recording of the receiver outputs, allowed the entire operation to be automated.

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The coming of the Parkes Telescope changed the way that radio astronomy was organized in the Radiophysics Laboratory. Over the years a number of groups had formed in the Laboratory. Each of these groups, with considerable input from Joe Pawsey as the overall leader of the radio astronomy programme, had initiated the construction of its own system of antennas and receivers, and used these to pursue a programme of study largely determined by the leader of the group. At the time that the Parkes Telescope was funded these scattered groups included one studying *solar bursts*, one concentrating on the *slowly varying component* of solar radio emission, three groups with different antennas studying *discrete sources*, and another group studying the 21 cm *line emission* from the hydrogen in the space between the stars. Only one of these groups survived the coming of the Parkes Telescope. Paul Wild, with Pawsey's backing, and with some financial assistance from the Ford Foundation, constructed the Solar Ring at Culgoora, near Narrabri. This is the site of the present day Australia Telescope, and some of the dishes of the Solar Ring can still be seen there.

While Taffy Bowen was responsible for the creation of the Parkes Telescope, he had no plans to use it for his own research. John Bolton was appointed as OIC of the observatory, and soon moved to Parkes to devote the rest of his life to this task. The observatory operates like an optical observatory, with the telescope and facilities provided and maintained by the observatory, and astronomers allocated use of the facilities for designated periods. Initially use was restricted to Radiophysics staff, with a few outside collaborators. However nowadays it is used by astronomers from the world over, with telescope time apportioned by a Time Allocation Committee. Observing periods range from a day or two to a week or two and during that time the observers stay in the comfortable observers' quarters. To avoid electrical interference these quarters are located at the other end of the property, half a kilometre from the telescope. The intervening stretch can be traversed by car, bicycle or on shanks' pony; on a clear night the choice of walking at the start a second-half shift provides a marvellous view of the stars.

The accommodation and facilities for observers at Parkes were of American standard with all meals and housekeeping provided. Luxury after the old field stations, but somehow losing some of the relaxed atmosphere of those simpler establishments. There was intense competition to use the telescope, and a calculation of the hourly cost of providing the telescope facilities suggested that telescope time should not be waisted in frivolity. However in the early days things were a bit more relaxed. Apart from the ping pong table being well used there were other bits of fun. I recall an incident with a dead snake, which I think originated with Doug Milne. John Bolton hauled me over the coals for putting it in someone's bed. He thought the lady making the bed could have had a heart attack. There are stories of Marc Price parading round the control room during observing sessions playing his tuba, with Dick McGee calling him a 'Goddamn Yank'. Dick promoted the idea that the regular scientific sessions that discussed results from Parkes should be called Meetings of the *North Goobang Philosophical Society*, a title not entirely appreciated by the hierarchy. Marc then had a buddy of his in Colorado make a leather insignia for the society which still hangs on the wall of the Observers' Quarters at Parkes.

And talking of the observers' quarters, I should tell of the coffee percolators. These were those aluminium ones with glass knobs on the lids. Soon the glass pieces fell out, and they were replaced with brass knobs turned on the local lathe. This made the lids so heavy that it was essential to hold the lid in place when pouring the coffee. Some VIP's visiting with Taffy Bowen were not warned of this necessity with disastrous results. New coffee percolators appeared with great dispatch!

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In *Parkes, Thirty Years of Radio Astronomy* (ed. Goddard & Milne, CSIRO Australia 1994) Marc Price relates how he came from Colorado to Parkes. He was the first of a series of scholars from the Australian National University who undertook research for their PhD at Parkes, with John Bolton acting as a joint supervisor. After returning to his homeland for some years Marc later spent a period as Director of the Parkes Telescope. Ron Ekers, currently the Director of the Australia Telescope National Facility, of which the Parkes telescope is one element, was another such student. He returned to Australia to take his present position after spending some years as Director of the U.S. Very Large Array in New Mexico. I'll talk later of some Jupiter observations we did together at Parkes. I worked quite closely with was another ANU student, Beverley Harris. Her thesis project, a study of the *radio spectra* of the discrete sources, involved measuring the strength of the radio-source signals at many frequencies. For those observations we made good use of Doug Milne's uniselector sequencer that I mentioned above. While they were students Marc, Ron and Beverley each spent some years residing at Parkes.

After earning his PhD at Caltech, Ken Kellermann came to spend some time at the Radiophysics Laboratory, making observations at Parkes. We scared Ken with stories of snakes and spiders, which he at least pretended to believe. Ken earned a reputation for his keenness as an observer. According to one story he was so keen for telescope time that he arranged to swap some of his allocated time with two different groups in exchange for their times!

On Christmas Eve in 1965 I answered the telephone at home to be asked 'Is that Dr Roberts?' 'Yes, speaking.' 'Is that *the* Dr Roberts?' At which point the penny dropped and I recognised Rad's voice. A few years after I had left Caltech to return to Australia Radhakrishnan had started planning to sail a trimaran from England to Australia. He had the boat built in the UK, and named it *Cygnus A*, after the radio source. The crew for the leg from the UK to Puerto Rico were all Caltech radio astronomers - Rad, Dan Harris and Dave Morris. Dan left ship in Puerto Rico to take a position at the Arecibo Radio Observatory, while Rad and Dave continued the voyage with a non-radio astronomer as the third crew member. Rad had invited me to sail the leg from Fiji to Australia, but sailing on a 20,000 ton ocean liner had made me realise how inadequate were mankind's attempts to master the ocean. The best we could manage was to float something out in the middle of this great expanse and leave it to the mercy of the elements. No, sailing across an ocean in a small boat was not my idea of fun. (In fact the sailors did encounter a serious storm on that last leg - it was getting close to the cyclone season.)

Yachts arriving in Sydney from overseas were required to fly a yellow quarantine flag and to contact Her Majesty's Customs as soon as they arrived. Rad moored at the Cruising Yacht Club in Rushcutters Bay and telephoned Customs, where a voice told him: 'Don't worry mate. She'll be right.' He was interrupting a Christmas Eve Party! However when he continued to fly the yellow flag, with the yachts assembling for the Sydney to Hobart Race starting on Boxing Day, the Yacht Club insisted that he somehow get the customs to come and inspect the yacht. Rad and Dave joined the staff of the Radiophysics Laboratory and spent some years using the Parkes radio telescope.

Many of these overseas visitors wanted to experience something of Australia and I enjoyed holidays with several of them. Marc Price and I travelled by the *Sunlander* train from Brisbane to Cairns; quite a journey. From Cairns we took the tourist (steam) train up to see the Barron Falls. Looking at the falls, Marc, who had acted as a guide in the caves near his home in Colorado, said: 'You know, if this was back home, we would put a fence round it, floodlight it, and charge two dollars admittance.' (Call it $25 now, I suppose.) Marc and I then spent some days on one of the Whitsunday Islands, where I seem to remember he used his ukulele to bat balls in the swimming pool, to the detriment of his uke!

Another holiday with overseas visitors working at Parkes involved Jan Hogbom. Jan was from Sweden, but had earned his PhD and spent some years working at Cambridge. At this time he was based in the Netherlands. When Don Yabsley, Jan and I were stayed in an apartment at Coolangatta Jan announced that he would make coffee 'the way we make it when we go fishing in North Sweden'. Evidently the coffee in North Sweden is strong. Jan completely filled the top of the percolator with grounds, so that when the grounds became wet and swelled, our North Swedish coffee went everywhere.

PARKES MARKS ITS DEBUT

In these memoirs the only Parkes programs that I will discuss are those with which I had some contact. Of course there were many other important programs. For an overall discussion of the early observations you might consult Peter Robinson's book about the Parkes Telescope, *Beyond Southern Skies* [Cambridge University Press, 1992].

In December 1962, some nine months after the Parkes Telescope commenced operation, a NASA-sponsored conference on *The Physics of Non-Thermal Radio Sources* was held in New York. As John Bolton was unable to attend I was sent to give a report on the work in progress at Parkes and I began with the words: 'Of the current observing programs on the CSIRO 210 ft radio telescope at Parkes, the program to measure the polarization of radio sources has produced the most unexpected results. Three polarization studies were scheduled for the first observing quarter - observations of Jupiter, a search for linear polarization in the emission of discrete sources, and observations to confirm the Dutch findings of polarization in galactic radiation. Of these three, the studies of discrete sources have proved so fruitful that they have taken precedence over the other programs.'

When the Parkes Telescope came into operation my priority was to use it to continue the Jupiter studies that we had initiated at Caltech; to exploit the advantage of the larger size of the Parkes dish which collects more than five times the energy collected by one of the Owens Valley dishes. And there had been a development in the Jupiter studies. At Caltech Dave Morris and Glenn Berge had discovered that the direction of maximum linear polarization of the radio emission (the *plane of polarization*, as it is called) tilts as the planet rotates, tipping about 10 degrees one way and then 10 degrees the other way. They interpreted this to mean that Jupiter's magnetic axis is tilted about 10 degrees from its rotational axis. We have much the same tilt between the two axes here on the earth.

In addition to continuing the Jupiter studies I was also interested to search for polarization in the radio emission from other radio sources. I have already told of my interest in polarization. I have also spoken of my frustration that astronomers are limited to *observing* celestial objects: it therefore behoves astronomers to measure *every* characteristic of the emissions from these celestial objects, and polarization is a measurable property.

By this time it was believed that the radio emission from the 'discrete sources', and the background radio emission from the Milky Way, must be synchrotron radiation. Since synchrotron radiation is expected to be substantially linearly polarized there had been numerous attempts to find linear polarization in these radio emissions. Pawsey had initiated several such searches at the Radiophysics Laboratory, none of which had been successful. Some support for the synchrotron theory had come from the 1954 discovery of strong linear polarization in the diffuse *light* from the Crab Nebula. This discovery was made following Shklovskii's suggestion that both the light and the radio emission from the nebula were synchrotron emission. However until 1957 no polarization had been detected in the *radio* emission from the Crab Nebula, nor in the radio emission of any other cosmic source suspected of being a synchrotron emitter. Polarization had been measured in the variable radio emission from the sun, and in the *bursts* of radio emission from Jupiter, but these emissions were not thought to be synchrotron emission, and the polarization measured was mainly circular, not linear.

The first detection of polarization in cosmic radio emission thought to be synchrotron radiation came in 1957 when observers at the US Naval Research Laboratory measured 7% linear polarization in the 3 cm radio emission from the Crab Nebula. In 1959 Soviet observers found 3½% polarization in the same source at the longer wavelength of 10 cm. In 1960, at Caltech, we found more than 20% linear polarization in the microwave emission from Jupiter, and in 1961 the Dutch group measured linear polarization in the radiation from the Milky Way background. The first detection of polarization in the radio emission from an *extragalactic* source came early in 1962 when the group at the US Naval Research Laboratory measured 8% linear polarization in the 3 cm radiation from the famous Cygnus A. Significantly they found that the degree of polarization was much less at the somewhat longer wavelength of 10 cm. This suggested that perhaps appreciable polarization would be found only at the shortest wavelengths, which might explain the failure of the earlier attempts to detect linear polarization, all of which had been made at longer wavelengths.

In Peter Robinson's book about the Parkes Telescope referred to above, he reports that searching for linear polarization was included in a list (prepared while I was at Caltech) of research topics proposed for the new telescope. However no means for measuring polarization was provided until John Bolton had a 'feed rotator' constructed and installed at the focus of the telescope early in 1962. I understood that John's action sprang from the success of the polarization observations of Jupiter that we had made at the Owen's Valley, when Al Munger had used that ring-gear from an old automobile starter-motor to enable us to rotate the feed. You won't find John Bolton's name as an author of any of the papers reporting the very successful polarization studies made at Parkes. However John contributed greatly to the success of those studies, making many improvements to the feed rotator over the years. I particularly recall the time he had the inspiration to add sectors to the reflecting plate that surrounded the feed. These additions, which changed the shape of the reflector from square to circular, produced a dramatic reduction in the spurious variations which occurred whenever the feed was rotated, even when the antenna was directed at blank sky.

The first Parkes detection of polarized radio emission from a source other than Jupiter was made by Ron Bracewell (visiting from Stanford University), Brian Cooper and Tom Cousins. This was during some unscheduled observations early in 1962, when an experimental 10 cm receiver was being used to test the performance of the dish at this short wavelength. (The dish had been designed to work well at wavelengths of 21 cm and longer. These tests showed that it still worked well at a wavelength as short as 10 cm). Strong linear polarization was found in the radio emission from the peculiar southern galaxy, Centaurus A.

Marc Price and Brian Cooper then made measurements of Centaurus A at longer wavelengths. Not only did they find linear polarization at these longer wavelengths, but they found that the direction of the linear polarization changed systematically with wavelength, the change in angle being proportional to the square of the wavelength. This is an example of the *Faraday Effect.* Under certain conditions, when linearly polarized electromagnetic waves travel through materials that have different properties in different directions (*anisotropic* materials), the plane of polarization rotates as the wave travels through the material. In this case a magnetic field permeating the ionized gas between the stars causes the gas to be anisotropic. As radio waves travel through this gas the plane of polarization rotates through an angle which is greater at longer wavelengths, the increase being proportional to the square of the wavelength.

Soon after the detection of linear polarization in Centaurus A, Frank Gardner and John Whiteoak discovered linear polarization in other radio sources, and by July 1962 they were reporting detections for eight sources. Suddenly a new area of study had opened up. Linear polarization in extragalactic sources was at last being detected, confirming the synchrotron origin of the emission, and measurements of the Faraday rotation were providing information on the number of electrons and the strength of the magnetic field along the path from the source to the earth. And incidentally the discovery of strong Faraday rotation explained the earlier failures to detect polarization at longer wavelengths; the radiation was depolarized by variable Faraday rotation across the face of the source.

In reporting Gardner and Whiteoak's polarization studies to that 1962 New York meeting I said there was 'still considerable room for doubt' as to just where the Faraday rotation was produced. However the results for 17 sources showed that the effect was larger for sources seen through the thicker parts of the Milky Way, suggesting that the rotation occurred as the radio waves travelled through the ionized gas between the stars in the Milky Way. While polarization measurements from the Naval Research Laboratory, Caltech, and Jodrell Bank were reported at the conference they were not sufficiently extensive to shed further light on this problem. Dennis Sciama, whom I had known at Cambridge, where he was one of Bondi's students, was championing an alternative theory, attributing the rotation to ionized gas between the galaxies of the local cluster. As there seemed to be no evidence to support this idea I thought he was just complicating the issue. Over coffee I asked Dennis whether he was really motivated to understand where the rotation occurred, or was just interested in championing his own theory. I believe his answer was along the lines of 'A bit of both'!

These polarization discoveries ushered in a long series of observations at Parkes, in which Frank Gardner and John Whiteoak studied the polarization, and the Faraday rotation, of extragalactic sources. Meanwhile, as I relate in the next chapter, Max Komesaroff and I were studying Jupiter. A pattern soon developed where Frank and John would be allocated the telescope for most of the night, and Max and I would take over for the period when Jupiter was in the beam.

I also started a collaborative program with some of the Sydney University people to observe the polarization of the galactic background. In studying extended areas of radio emission, such as the galactic background, you are faced with a problem in establishing a 'zero point'. No matter where the telescope was pointed on the sky, rotating the feed produced a quasi-sinusoidal variation of the receiver output of the sort expected when receiving linearly polarized radiation. Some of this variation was genuine, resulting from receiving polarized radiation from the sky; some was caused by polarized radio emission from the ground sneaking in over the edge of the dish; and some of the variation was simply spurious. When studying small-diameter sources it was easy to remove the unwanted components by subtracting a measurement made at a nearby off-source position. But for the galactic background radio emission, which comes from an extensive area covering a large part of the sky, things weren't so simple. However I didn't have to worry about this problem for very long. Don Mathewson was also interested in the polarization of the galactic background, and soon John Bolton, wisely, if somewhat to my disappointment, suggested that I had enough on my plate and that Don should take over this project. In collaboration with Doug Milne and others, Don pursued the subject with enthusiasm, even, I think, after he left to become the Director of Mt Stromlo Observatory.

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It must have been early in 1962 that Cyril Hazard, a radio astronomer from the University of Manchester who was spending a period at the University of Sydney, walked up the road from the School of Physics to visit John Bolton at the Radiophysics Laboratory. Cyril showed us recordings he had made at Jodrell Bank of the *occultation* of the weakish radio source 3C 212. Occultation is one of those delightful words that litter the astronomical literature; for *occultation* read *eclipse*. You will be familiar with the idea that in its path across the heavens the moon occasionally passes in front of the sun and causes an 'eclipse'. Obviously at other times the moon must pass in front of other stars and planets and 'discrete radio sources'. Cyril had realised that observing lunar occultations of discrete radio sources could provide valuable information. Timing the disappearance of a radio source behind the moon, and then its reappearance, provides a means of accurately locating the source on the sky. Furthermore the patterns of decrease and increase of the received radio emission as the edge of the moon passes over the source can delineate the size and shape of the source. In the case of 3C 212 the pattern Cyril had recorded as the source disappeared behind the moon was a classical *diffraction pattern,* showing that the angular size of the source was very small indeed.

Having had some success with this technique at Jodrell Bank, Cyril now came to tell John of predictions that one of the stronger radio sources in the sky, 3C 273, would be occulted a number of times during 1962, and that for several of these occultations the source should be above the horizon of the Parkes Radio Telescope. He asked if he could use the Parkes dish to observe these events. I must say I was excited by Cyril's results, and I was surprised when Joe Pawsey, who had come into the office during the discussion, seemed unimpressed. Had Pawsey tried the technique himself and become aware of the difficulties?

Cyril observed the occultations of 3C 273 at Parkes and the observations were extremely successful, largely, it must be said, thanks to John Bolton's assistance. In the papers reporting the observations John's name does not appear as an author, but it was he who made sure there were no problems. As he delighted in telling, for one of the occultations he ground away a part of the telescope so that it could be tipped the extra few degrees needed to be sure of observing the emergence of the source from behind the moon. Cyril Hazard was a man of ideas, but his execution of these ideas could sometimes attract that natural nickname of 'Hap Hazard'. Some years later, in Puerto Rico, Cyril invited me to witness an occultation they planned to observe at the Arecibo Observatory. I arrived at the observatory to find everything very quiet. There was none of the frenzied activity one would expect in advance of an attempt to record a once-in-twenty-years event. It turned out that Cyril had forgotten to convert Universal (Greenwich) Time to local time and that the occultation had already occurred!

There was no formal report of Cyril's observations of the 3C 273 occultations given at that December 1962 New York meeting: the paper reporting the results was still in preparation. However news of the observations had travelled by the grapevine and Al Moffet referred to them in his presentation, which gave me the opportunity to show the slides I had brought of the recordings of the August and October events. Analysis of the occultation records defined the position of the source on the sky, and the shape of the radio emitting area, with great accuracy. The optical counterpart was identified immediately: like the radio source, the optical object had a peculiar shape. It looked like a star with a jet issuing from it. One more example to add to the four cases already known of very small diameter radio sources whose optical counterparts looked like stars - the so-called 'radio stars'. But less than a month after the New York meeting Maarten Schmidt recorded the spectrum of the optical counterpart and showed the object to be the core of a galaxy far beyond the Milky Way, a galaxy with a redshift of 0.16, the first recognised *quasar*. A rapid re-assessment of the spectra of the other four 'radio stars' led to the recognition that they were also galactic cores with high red-shifts, with 3C 48 the most distant at a redshift of 0.37.

Anyone interested in the history of science would be fascinated to read a paper presented at the New York meeting that does not appear in the Proceedings. In a long presentation Jesse Greenstein discussed the optical spectra of the four previously known 'radio stars'. For each of them he managed to find an explanation of the spectral features in terms of normal Milky Way stars. The paper is not mentioned in the published proceedings of the meeting, and the lengthy paper Jesse submitted to the Astrophysical Journal was withdrawn after Maarten Schmidt's discovery. Fortunately for the history of science, Maarten has assured me that he has preserved a copy of Jesse's paper.

After the New York conference I spent Christmas in Pasadena staying with John and Mary Whiteoak. John had just left the Radiophysics Laboratory to spend a couple of years at the Mt Wilson and Palomar Observatories. Whilst there, amongst other things, he extended the Palomar Schmidt Sky Survey further south. John Bolton had found the original survey, made with the 48 inch Schmidt telescope on Mt Palomar, invaluable for identifying the optical counterparts of radio sources whose positions had been measured with the Caltech interferometer. He now felt the need for a similar, deep, wide-field, survey of the southern sky to help identify sources studied at Parkes. There would be no large Schmidt telescope in the southern hemisphere until the coming of the UK Schmidt at Siding Spring, and the ESO Schmidt in Chile, some ten years later. In the interim he arranged for John Whiteoak to produce this Southern Extension of the Palomar survey, covering the area of the sky near the southern horizon at Palomar. This area had been omitted from the original survey because, at such low altitudes, differential refraction in the earth's atmosphere causes some degradation of the images.

My visit to Pasadena was a great opportunity for me to renew friendships, particularly with Radhakrishnan. There was much discussion about their latest studies of Jupiter, and their other polarization studies. Rad was so fired up that he insisted that I spend the morning of Christmas Day in discussions with him at Caltech, before we went to enjoy Christmas dinner at the Whiteoaks! After Christmas I continued on to Europe, spending New Year with John Murray and family in Leiden, where John was spending several years working with the Dutch radio astronomers. This was one of the coldest European winters on record; I recall a completely snow-covered landscape as I travelled by train from Paris to Leiden, and in Leiden it was bitterly cold. I still own the ear muffs that I had to buy to survive. And then on the plane to London, en route to attend a conference in England, I found news from home in the *Times*: the strange and tragic death of Gib Bogle, after a New Years Eve party at the home of Ken Nash.

Studying Jupiter at Parkes

We began measuring the radio emission from Jupiter as soon as the Parkes Telescope went into operation in the first months of 1962, and you will learn in a later chapter that we were still making improved measurements of Jupiter at Parkes 13 years later. 'We' were Max Komesaroff and I, with various other collaborators at different times. I had first met Max in Dapto days when he replaced John Murray as the main electronics technician. He had been destined to be an optometrist, following the profession of his father, and had graduated in that discipline, but I don't think he ever practised in the profession. Before coming to CSIRO he had been with the Bureau of Mineral Resources, flying magnetometers over the Northern Territory, surveying for minerals. When Pawsey employed Max for the Dapto job I am sure that he saw him as a future 'research officer', but since a PhD or the equivalent was the minimum requirement for appointment as a research officer, Max was employed as a technical officer. By the time the Parkes telescope appeared on the scene he had become a research officer on the basis of his research output: later he earned a Doctorate of Science.

Our collaboration in studying Jupiter led to a long friendship which was very rewarding to me, at both the personal and scientific levels. Max worried about things - whether it was the state of society or the possibility that something might be forgotten during the observations. His concerns certainly helped avoid some disasters at Parkes when I was inclined to rush ahead without thinking. However our night-time observing stints at Parkes were hard on Max, as he had trouble sleeping by day. Our lunch-time walks from the Sydney laboratory were much more relaxed times, but even then most of the talk would be about the social problems of the day, rather than light-hearted gossip. Interesting, then, that so many of the funny stories told round the lab were stories about Max - stories that Max had originally told about himself.

I was surprised to find that the earliest observation of Jupiter at Parkes that we have reported in print was a recording of an occultation of Jupiter by the moon on April Fools Day in 1962. That was two weeks before the first occultation of 3C 273 was observed at Parkes. I suppose it was as a result of Cyril Hazard's visit to John Bolton that I came to investigate whether the moon would occult Jupiter, and hence planned these observations. However, again to my surprise, I see that in John Bolton's article *Radiophysics in Exile* (Proceedings of the Astronomical Society of Australia **8**, 381,1990) he says that I attempted to measure an occultation of a Mills, Slee and Hill (MSH) source at Caltech.

We recorded the occultation in an attempt to define the extent of the Jovian radio emission at wavelengths longer than those studied at Caltech. Now a lunar occultation might seem to provide a cheap means of obtaining high resolution, but occultations suffer from one great disadvantage: if there is a problem you can't just go back and repeat the observation. And since any change in the receiver output during the disappearance or reappearance of the radio source will be interpreted as structure of the radio source being eclipsed, you are at the mercy of interfering signals and receiver instabilities. There is the added problem that the radio emission from the moon is often much stronger than the radio emission from the object you wish to study. This was certainly true in the case of Jupiter. To cope with this latter problem we attempted to keep the radiation received from the moon constant by having the telescope track the edge of the moon accurately during Jupiter's disappearance and reappearance.

To limit problems from ignition interference, aircraft are banned from flying in an area surrounding the Parkes Telescope. In addition there are gates on the local road that can be closed to exclude cars at times of critical observations. These gates were closed during the later 3C 273 occultation observations, but I doubt whether they were closed for our Jupiter attempt. However we did take anti-cockatoo precautions! The occultation occurred near sunrise, a time of the day when galahs enjoyed skylarking round the focus cabin. To avoid having the birds sit on the feed aerial, or fly through the beam at the critical time, Doug Milne rode up on the structure of the telescope, attached with a safety belt, and repeatedly struck the steel tubing with a hammer to scare the birds away!

Our records of the disappearance of Jupiter behind the moon, and of its reappearance, suggested that the extent of the region around the planet that emits radiation at a wavelength of 75 cm was similar to the extent of the region emitting radiation at 20 cm, as measured by the Caltech interferometer. However the radio signal from Jupiter was sufficiently weak (about fifty times weaker than the signal from 3C 273) that some of the problems mentioned above made our results rather uncertain, to say the least.

The other studies of Jupiter that we made at Parkes were very successful. During 1962 and 1963 we made extensive measurements of the strength and linear polarization of Jupiter's radio emission at wavelengths of 6, 10, 11, 21, 74 and 100 cm, establishing the way these quantities varied throughout the 9 hours and 55 minutes of Jupiter's rotation. We quickly confirmed Dave Morris and Glenn Berge's Caltech discovery that the plane of polarization rocks back and forth during a rotation, evidently as a result of a misalignment between the magnetic and rotational axes. However with the larger signal collected by the Parkes Telescope we could define this variation more accurately and we found a new feature: the polarization plane tilts more rapidly as it goes anti-clockwise, and less rapidly as it returns. There is something asymmetric about Jupiter's radiation belt, implying that the magnetic field is not symmetric about the centre of the planet. The magnetic field of the earth is almost symmetric: it is approximately *dipolar*, which is the simplest, and symmetric, form of a magnetic field. The earth's field has some higher-order (multipolar) components but they are weak. In Jupiter's field these higher-order components are much stronger, giving it a more complex asymmetric structure.

Our measurements also showed that the strength of the received radio signal varies as the planet rotates - a feature which had been hinted at by the earlier measurements. As I will explain, this variation is another result of the misalignment between Jupiter's magnetic and rotational axes.

The way that electrons and other charged particles move in a dipolar magnetic field had been studied for many years, particularly by those interested in the aurora, but there was renewed interest in this theory when the first artificial satellites discovered the 'radiation belt' surrounding the earth. Electrons spiral round the *magnetic field lines*. Those are the lines that become evident when iron filings are placed near a bar magnet. The field of a bar magnet is approximately dipolar, and you will recall that the lines curve round into the two *poles* of the magnet. In a dipolar field the spiralling motion of the electrons is symmetric about the *magnetic equator*, the imaginary plane at right angles to the magnet through its centre. The spiral of an electron's motion is steepest as it crosses this magnetic equator, and the spiral becomes flatter and flatter as the electron moves away from the equator. Ultimately the electron is moving round a circle, in a plane at right angles to the magnetic field. Then from this *mirror point* the electron spirals back through the magnetic equator, to a matching mirror point in the other hemisphere. At the same time as it performs this spiralling, mirroring motion the electron slowly drifts round the magnet.

Electrons that are moving in flattish helices as they cross the magnetic equator mirror soon after crossing the equator. Hence throughout the entire length of their trajectory the motions of these electrons are almost parallel to the magnetic equatorial plane. As a result the synchrotron emission from such electrons is strongly beamed towards the magnetic equatorial plane, and the emission is highly linearly polarized with the net electric field parallel to the magnetic equatorial plane. (These electrons are described as having small *equatorial pitch angles*.) By contrast, electrons that cross the magnetic equator in steep helices (i.e. with larger pitch angles) travel a long way from the magnetic equator and mirror at high *magnetic latitudes*. Because of the curvature of the magnetic field lines at higher latitudes large parts of the trajectories of these electrons are steeply inclined to the magnetic equator. Hence their emission is not so highly beamed towards the equatorial plane, and the net degree of linear polarization of their emission is less.

The 10 degree tilt between Juiter's magnetic and rotational axes causes the Jovian magnetic equatorial plane to tip up and down past the earth as Jupiter rotates. In other words, as Jupiter rotates we receive radiation that was emitted at different magnetic latitudes. The beaming of the radiation towards Jupiter's magnetic equator thus explains the variation in the strength of the emission that we receive. From measurements of this beaming, and the already known degree of linear polarization, Max and I were able to deduce something about the steepness of the spirals followed by the electrons in Jupiter's radiation belt, that is about the *distribution of pitch angles* of the electrons. To make these deductions we used the calculations of Kip Thorne, these days an eminent relativist and cosmologist. While

Kip was still an undergraduate at Caltech, during a vacation spent working for Jesse Greenstein, he computed the synchrotron emission that would be emitted by possible radiation belts around magnetic stars - a set of calculations that was published as a 30 page article in the Astrophysical Journal Supplement Series. Using these calculations we showed that the degree of linear polarization and the beaming that we measured implied the presence of two fairly distinct groups of relativistic electrons in Jupiter's radiation belt. Electrons in the first group move in flattish spirals at the magnetic equator and consequently mirror at low magnetic latitudes; those in the other group have a wider range of pitch angles with some electrons mirroring at high magnetic latitudes.

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As part of our Jupiter observations we also attempted to measure any *circular* polarization of the microwave emission. In an earlier chapter describing synchrotron emission from spiralling electrons I explained that when the emitting electrons have a range of pitch angles the right-handed circularly polarized radiation from some electrons tends to cancel the left-handed radiation from other electrons, leaving the radiation predominantly linearly polarized. However there is a small residual circularly polarized component, and I now realised that the magnitude of this residual is dependent on the strength of the magnetic field. So a measurement of the circular polarization could provide a means of measuring the strength of the magnetic field. In an appendix to our Jupiter paper I produced a rough theory of this effect, which prompted Legg and Westfold to later publish a thorough-going analysis.

For our 1963 attempt to detect circular polarization we used a feed design being used by NASA for some spacecraft observations they were making at Parkes. There were two separate feeds, one that accepted right-hand polarized radiation and one that accepted left-hand polarized radiation. We would install the right hand polarized feed, make a measurement of Jupiter, stow the dish, change to the other feed and make another measurement, and so on. Even with someone riding up in the aerial cabin to make the changes, quite some time elapsed between making the left- and right-handed measurements. As a result, the measurement of the difference was not very accurate. The best we could do was set an upper limit of 3%.

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My next encounter with Jupiter was prompted by a report that Glenn Berge and Dave Morris had used the Caltech interferometer to show that the centroid of Jupiter's microwave emission was displaced sideways by 0.4 radii away from the rotational axis of the planet. Perhaps this explained the asymmetries that we had observed. At Parkes we could not measure the *absolute* position of Jupiter with sufficient accuracy to check this conclusion. However, if the centroid of the radio emission was displaced sideways from the rotational axis then the position of the radio source should wobble as the planet rotates. In the course of one rotation the centroid should move back-and-forth sideways through a total of

0.8 Jupiter radii. I thought that we should be able to detect such a movement. In November 1964 Jupiter was only ½ degree away from one of those very small diameter radio sources that Dan Harris had found at Caltech, CTA-21. I felt sure that by making repeated measurements of the separation between Jupiter and CTA-21 we would be able to detect any such a wobble in the position of the Jupiter radio source.

At this period Max Komesaroff was spending a few years at the University of Maryland, so I teamed up with Ron Ekers to make these observations. As I have mentioned, Ron was one of the Australian National University students who gained their PhD from studies made at Parkes: by this time he was an ANU postgraduate scholar working at Parkes. Making these observations with Ron led to many long discussions as we tried to understand just what form of asymmetry of the Jovian magnetic field would produce the asymmetries observed in the radio emission.

In the acknowledgments in our paper that reported these observations we thanked the telescope drivers for their diligence, as well we might! For four hours each night, for six successive nights, we followed a procedure spelled out in the paper:

> The telescope was set to the declination of CTA-21 and approximately 45 sec ahead of the source in right ascension, and the source allowed to drift through the beam with the telescope stationary. The telescope was then immediately set to the declination of Jupiter and 45 sec ahead in right ascension and Jupiter allowed to drift through the beam. This sequence was repeated three times. Then a pair of declination scans through Jupiter were made at +/- 0.25°/min, followed by a similar pair of declination scans through CTA-21. One such set of three right-ascension drifts and a pair of declination scans through both sources occupied approximately 20 min. Successive sets were made with the feed set to accept the E vector in position angle 75° (perpendicular to Jupiter's axis of rotation) and in position angle 345° (parallel to the axis of rotation).

We didn't find any detectable wobble in the position of Jupiter's microwave centroid, and our measurements ruled out any sideways displacement of Jupiter's magnetic centre from the planet's rotational axis as large as had been suggested by Berge and Morris. Before we went to print there was a lunar occultation of our comparison source CTA-21. Rupert Clarke and Bob Batchelor recorded this occultation at Parkes and so obtained an accurate position for CTA-21. This allowed us to convert our measurements of the relative position of Jupiter and CTA-21 to actual positions of the Jupiter radio source, and the position of the source lay very close to the centre of Jupiter.

Jim Warwick, from Boulder, Colorado, studied the *bursts* of radio waves emitted by Jupiter in the short-wave band. To explain the variation in the occurrence of these short-wave bursts as Jupiter rotates Jim had suggested that the magnetic centre was displaced substantially from the planetary centre. So the Berge and Morris report had been music to his ears. When he heard of our result he questioned whether it was fundamentally possible to measure positions to an accuracy of 1/50 of the beamwidth of the telescope, as we claimed. The answer is 'Yes', by having a very stable system and a good *signal to noise ratio*. That paper by Bracewell and Roberts might have spelled out the fact that you can't uniquely decipher *structure* in the emitting source on scales less than the beamwidth, but you can certainly locate the centroid of a source to a small fraction of the beamwidth.

When we told Glenn Berge and Dave Morris of our result Glenn re-interpreted their observations, suggesting that instead of indicating a displacement of the centroid of the radio source, the observations indicated a substantial (3%) circular polarization of the Jupiter radiation. By the time that we set out to test this re-interpretation Radhakrishnan and Dave Morris had reached Australia in Rad's yacht and had begun working at the Radiophysics Laboratory. This meant that Dave Morris could join Max Komesaroff (now returned from Maryland) and me in testing Glenn's re-interpretation. Our observations, made in December 1966 and February 1967, formed part of a larger study (which also involved Frank Gardner and John Whiteoak) which was mainly aimed at searching for circular polarization in quasars. By then the Parkes Telescope was equipped with feeds that could be switched rapidly between accepting left-handed and right-handed circularly polarized radiation, thus providing a far more sensitive system than the one we had used in our earlier attempt to any measure circular polarization of Jupiter's radio emission.

We didn't find any circular polarization in the quasar radio emissions, but we did measure circular polarization in the Jupiter emission. It was much less than the 3% reported by Glenn Berge, and it varied as the planet rotated. It reached about 1% right-handed when the southern magnetic pole was tilted towards the earth and changed to about 1% left-handed when the northern magnetic pole was tilted towards the earth. This immediately showed that Jupiter's magnetic field is of the opposite sense to the earth's field - a result earlier suggested by the polarization of the short-wave bursts. It was more than 3 years later (1970) before these results were sent for publication. By then Ernie Seaquist from Toronto had also measured a smaller degree of polarization than reported by Glenn Berge. Also by then Gleeson, Legg and Westfold, and Jim Clarke (of whom more anon), had made calculations of the circular polarization of the synchrotron radiation emitted by shells of electrons trapped in dipolar magnetic fields. From these calculations and our measurements we were able to conclude that the magnetic field in the radiation belt, i.e. about two Jovian radii *above* the planetary surface, was between ½ and 2 Gauss. That is about ten times stronger than the earth's field. It wasn't until several years after this that the Pioneer 10 space-probe flew past Jupiter and measured the magnetic field *in situ*, a measurement that confirmed these deductions.

TORONTO

When my feet became itchy again, and I dreamed of another period overseas, I naturally thought of spending a few years at the Canadian National Research Council in Ottawa. In the early days of Parkes, Norm Broten from NRC had spent some years at CSIRO. In fact, at the opening of the Parkes Radio Telescope Canadian Norman had helped unfurl the Australian flag from the roof of the aerial cabin! We had become good friends. Now NRC had a brand new radio telescope, located in the Algonquin Provincial Park north of Toronto. Designed by the designers of the Parkes Telescope, it is a smaller, but more accurate model, for use at shorter wavelengths. I proposed to John Bolton that CSIRO grant me leave for a couple of years so that I could take a job with the NRC. While John supported the idea of a 'sabbatical' he suggested that I might gain wider experience if I spent the time at a university, rather than at another government research institute. John knew Don McCrea, the director of the David Dunlap Observatory and head of the Astronomy Department at the University of Toronto. With John's recommendation I was able to secure a two-year appointment at the university, with the grand title of Visiting Professor.

My departure for Canada was set for April of 1967. However it was delayed somewhat when I became engaged! Gina and I married a few months later, and left for Canada in August. We travelled by a somewhat circuitous route spending a few days in each of Bangkok, Istanbul and Vienna, before attending the triennial General Assembly of the International Astronomical Union in Prague. The 8 mm movies that I took on this trip capture the contrast between the frenetic action in the cities of the East and the calm of staid Vienna. In the streets of Bangkok and Istanbul there are masses of people and vehicles, bearers carry tables on their backs as they push their way through crowds, flames flare as food is cooked in the street. In Istanbul masses of gold glitter in the covered markets and a cook frenetically stretches pastry to make Turkish bread. Then follows the sedate grandeur of Viennese palaces with almost deserted streets.

Yes, we were in Prague near the start of that 'false spring', just one year before the tanks rolled in. I attended the sessions of the conference at the university, and wondered at the lifts which ran on a continuous belt up over pulleys at the top, to descend down the other side. Meanwhile Gina delighted in the marvellous ancient buildings of the city - Wencelas Square, the castle, and the ornate Charles Bridge dating from the 14th century. And I recall a wonderful evening when we sat under the stars and listened as a symphony orchestra played Smetna's *Moldau*, while that river itself flowed quietly by behind us. After Prague we spent a few days in England before flying to Canada to become 'landed immigrants', a necessary formality for my employment. And we managed to fit in a visit to the World Expo in Montreal, before I finally fronted-up at the University of Toronto.

Toronto was a city with roughly the same population as Sydney, but 15 miles from downtown Toronto, at Richmond Hill, you found the David Dunlap Observatory in rural surroundings. At that distance from downtown Sydney you were in the city of Parramatta, the actual centre of Sydney's population sprawl. Bungalows on quarter-acre blocks were not the norm in Toronto. High rise apartment buildings downtown slowly gave way to two-storey houses with basements as you moved into the suburbs. The compactness of the city, and the flatness of the lakeside land, no doubt contributed to Toronto having such an excellent grid of public transport. Buses, trams and underground trains were all interlinked at *undercover* interchanges - a godsend in the winter.

We moved into an apartment tower in the newly-built St James Town. Huddled in one corner of this complex, dwarfed by the surrounding residential towers, stood a little tailor's shop, a remnant of yesteryear. He had refused to sell. The complex was only a few subway-stops east of the university but the view from our 13th floor apartment (superstitiously numbered 1401!) was semi-rural. A large cemetery opposite abutted the Don Valley beyond. It was on the balcony of this apartment that Gina had her first experience of snow, and she danced around catching the flakes as they came drifting down. Then the whole cemetery became a Christmas card, and we went over to the slopes on the far side of the Don Valley to join the local the kids in tobogganing down the snowy hillside on sheets of corrugated cardboard.

The apartments had parquet floors with underfloor electric heating, and the 'hydro', was included in the rent. In Toronto 'hydro' meant electricity, most electricity still being water-generated, although some did come from nuclear stations. Those parquet floors caused us a headache as the occupants of the unit above were apt to arrive home late at night and drop their hard-heeled shoes on the floor. As our lease guaranteed us 'quiet living' we expected that the building owners would be required to come to our rescue. However, when we consulted a lawyer he laughed and told us that 'quiet living' had quite another meaning: an Olde English phrase meaning that the owner guaranteed that he alone would attempt to collect rent from us. There was nothing for it but to pay the penalty for breaking the lease, and find other accommodation.

We moved out of the apartment on New Years Eve. There wasn't time for our goods and chattels to be delivered to our new abode that day, so they stayed in the pantechnicon over New Year while we camped in our snow-bound house in North Toronto. When our frozen belongings were delivered on 2nd January, with warnings not to turn on the TV until it thawed out, we were fascinated by the ice crystals in the olive oil. The house was surrounded by snow until the following April when, as the snow began to melt, we first set eyes on our lawn and garden. Then, as the snow finally disappeared, we watched rhubarb and raspberry canes appear, red-winged blackbirds and cardinals come to feed, and squirrels begin using a telephone-cable above the back fence as their highway.

A city under snow provided sights quite new to me, whether it was the parking-meters half submerged in white fluff, or the 'tuckshop' trucks in the snow selling sandwiches and hot drinks to workers on construction sites, and to students round the campus. From my office on the 13th floor of the Physics tower (also numbered 14!) I looked down on a construction site. Can you imagine working on a job like that when the land is covered with snow? I spoke of this once to an engineer at the radio observatory. He told of tools being dropped while working on the structure in the winter; tools that weren't recovered until the following spring. Living through snowy winters was fun for a few years, but I wouldn't care to live that way for ever. There were times when the cold really hurt. I recall one morning when Gina set off for the subway but returned after walking only a few blocks. Although wearing two pairs of gloves and all her thick clothing, her hands were so cold she could not feel her fingers.

Some aspects of the winters were truly marvellous. Niagra Falls in winter was a fairyland. The trees were covered in frozen mist from the falls and long icicles hung from every lightpole. When we had an ice-storm in Toronto, followed the next day by sunshine, the glistening icicles were something to behold. However ice-storms can be very destructive as the weight of the ice can break powerlines and trees. In a recent letter from Norm Broten he tells of being without power in Ottawa for seven days as a result of such a storm. Other wintertime memories include Gina skating on the ice-rink in front of City Hall; my building hi fi speakers in the basement of our house at Felbrigg Avenue with the assistance of our landlord and neighbour, a retired builder; the photo in *The Toronto Star* of a pheasant on a local airport warming his tail feathers in the exhaust from an executive jet; and Christmas parties at the director's house at the David Dunlap Observatory, where Don McCrea introduced us to egg-nog. The mention of egg-nog reminds me of the LCBO - the Liquor Control Board of Ontario. The only 'bottle shops' in Toronto were the government owned LCBO outlets, and there you filled in an order form, and signed a declaration that you were over the age of 21. We found it amusing to watch old greybeards making this declaration.

One winter we stayed beside Lake Kashagawigamog - lovely name isn't it? Such peace and serenity. The frozen lake stretched into the distance, the dark fir trees were decked with snow like a Christmas card. The whole atmosphere was one of placid contentment. As we wandered along the lake-edge, watching bubbles carried along in a little stream of water flowing under the surface of the ice, suddenly two skidoos came roaring round a bend in the lake to remind us of man's ever-present threat to tranquillity.

Spring brought that marvellous change when leaves appeared on seemingly dead trees. We went to watch the maple syrup being collected. The old practice of placing buckets at individual trees had by then been discontinued; plastic tubing now connected the weeping trees to a central collection point. Bubbles of air in the syrup marked its slow flow through the tubes, reminding us of the bubbles we had seen moving under the ice. The squirrels thought these bubbles were such fun that they would chew through the plastic tube, a habit that forced the farmers to change to opaque tubing.

As cities, Sydney and Toronto have much in common, but Toronto is not as fortunate as Sydney in its natural surroundings. Much of Sydney is perched on bushland hillsides above extensive waterways or long stretches of sandy beaches: Toronto is on flat land beside the lake. But Toronto has scenic areas both within the city and beyond. One of our first visits was to the marvellous High Park on the western edge of the city, and there I nearly caused Gina a heart attack. We had been driving on a small side-road in the park where we had seen no other cars. As we joined the main road through the park I inadvertently began driving on the left-hand side of the road. It is easy to stay on the right-side when there is other traffic to remind you, but when you are on your own it is easy to slip into old habits. As we drove down the road a car approached from the other direction and Gina apparently said: 'You are driving on the wrong side of the road'. I heard it as: 'He is driving on the wrong side of the road' and continued on my merry way. Both cars were moving at low speed and the other car went round me. However as the next car approached Gina made it quite clear to me that *I* was on the wrong side!

In the summers we enjoyed some pleasant camping trips. To the north we went as far as Muskoka, camping beside the lake, but we didn't venture as far north as Algonquin Park. As the radio telescope was on the northern edge of this provincial park I went there, staying in the observers' quarters, to be awoken in the night by bears raiding the garbage bins. Wives were not permitted to stay at the observatory, so during one of my observing trips Gina stayed at a lodge in the south of the park. As this was at the very beginning of the season she enjoyed the serenity of canoeing alone on the lake, watching the beavers building their dam. Because of the ban on wives staying at the observatory, Ernie Seaquist and his wife decided to camp in the park near the observatory for one of Ernie's observing trips. A ripping sound in the middle of the night, accompanied by the appearance of a large paw through the sidewall of the tent, secured them permission to stay in the observers' quarters for the remaining nights of that visit.

South of Toronto we went camping in the Adirondaks in northern USA. En route we visited Bob Chisholm (###) who lived in Kingston, on the eastern end of Lake Ontario. Bob (###) had spent a year at the University of Sydney, working with Chris Christiansen. He took us to his basement to show off his model trains. Every station was named after a Sydney suburb. When Gina said that he had spelt Kirribilli incorrectly Bob (###)would not believe her until he consulted his Sydney street directory! The Adirondaks were great. We even learned how to pronounce them; earlier we had been saying A<u>dir</u>ondaks, placing the emphasis on the second, instead of the third, syllable. Three notable memories from our Adirondak trip: the chipmunks rotating salted peanuts in their little paws as they carefully removed the salt; the squirrel who stole a block of chocolate from a neighbour's tent and then sat on a branch and carefully unwrapped the silver paper; and our complimenting a cafe owner on her sign 'No tipping', only to be told: 'Oh, please don't take any notice of that. That's for the Inland Revenue.'

When the American Astronomical Society met in Victoria, on Vancouver Island, we took the train across, and broke our journey at Banff to rent a car and visit places in the Rockies that I had enjoyed on that trip with Rad, in Caltech days. On this visit we had time to travel the full length of the marvellous Banff-Jasper Highway, stopping wherever we saw other cars stopped to watch a moose, or admire a great view, and at the Athabasca Glacier to take a snowcat ride and marvel at the shades of blue revealed in the holes in the ice. With overnight stays in a cabin on the mystic green Lake Maligne, and again at Lake Moraine, and with visits to Lake Louise, and Yoho National Park, it was a great week. In Toronto Gina had come to love feeding peanuts to the squirrels in University Park (wearing gloves after she was warned of rabies!); at Yoho it was the turn of the ground squirrels, who surrounded her begging for their share. Then, on our continuing westward journey, while we waited at Kamloops for a bus to take us to the radio observatory at Penticton, we enjoyed watching seven small tortoises sitting on a log over a lake.

Back in Toronto the 'fall' brought those magnificent colours of the leaves. Impossible to describe, hard to believe, and easily missed as they last for only a week or two. Then there would be masses of dead leaves on the ground and Gina could run through them kicking them into the air.

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Radio astronomy at the University of Toronto was a joint project of the Physics and Engineering Departments, and my appointment was to both of these departments. This association with engineers taught me that while physicists tend to identify 'effects' by the name of their discoverer (Zeeman effect, Raman effect), engineers are inclined to use descriptive names (magnetic splitting, resonant scattering). My duty to the Engineering Department was to attempt to teach electromagnetism to first year students. This subject was required of all first year engineering students, whatever branch of engineering they intended to pursue. Fine. But the students had already nominated their preferred branch of engineering, and been separated into classes on that basis. I had a class of about one hundred who wished to be civil or mechanical engineers, and a fair proportion of them had no interest whatsoever in electromagnetism. I did the best I could, leavening the lectures with sparks from Wimshurst machines etc., but those lectures were not enjoyable for either the teacher or the students.

I had expected that my years of experience would make me the local expert in radio astronomy, and looked forward to teaching graduate students in the Astronomy Department. But there is nothing like teaching a subject to make you learn it, and Ernie Seaquist, who had been undertaking this task for the last several years, was the expert. In research you learn what is necessary for your project, but your knowledge of other aspects of the subject can be quite superficial. Preparing my lectures on radio astronomy was a true learning process for me. I had the idea that my notes might form the basis for a book on radio astronomy; I saw a need for a thorough-going textbook, covering the subject in some depth, an updated version of Pawsey and Bracewell's volume published 12 years earlier. I wrote out my lectures by hand, and they were photocopied for the students, but I didn't have the time to go the next step. I heard that Alan Barrett at MIT was also working towards a textbook at this time, but his didn't materialise either. After this the subject became too large to be covered in depth in a single volume.

When the astronomy students wanted me to undertake a serious discussion of the imaginations published by Velokovsky, writings of which I was but vaguely aware, I was somewhat non-plussed. And I was amazed to be told that Velokovsky referenced our Jupiter studies to support his strange notions! In 1967 Toronto was experiencing the student unrest associated with the hippie period that boiled over as riots at Berkeley. Students wanted to determine what they were taught, and sat in judgment on their instructors. A far cry from my student days. I recall that the Department was concerned that the number of students wishing to undertake the PhD course in Astronomy was far greater than the number of jobs that would be available. In the old days the university would simply have restricted the intake of students. At Toronto the students had to be given the facts and allowed to make their own decision. Most stayed on; I hope they received a broad enough education that they could find employment in some other field.

Soon after arriving in Toronto I was called on to give a talk to the local astronomers and naturally I chose to speak about the radiation belts round Jupiter. With the encouragement of Don McCrea, and the cooperation of the workshop, we constructed a motorised model of Jupiter and its radiation belts. It later graced the entrance foyer of the Physics tower. A wooden sphere about a foot in diameter represented the body of the planet, while enamelled copper wires, laboriously formed into a series of helices, represented the spiralling electrons. The ends of these wires were inserted into a series of holes which had been milled into the sphere in such a way that the tilt of 10 degrees between the rotational and magnetic axes of Jupiter was properly represented. The finished object looked rather like a lady in hairs curlers, but when driven round by a clock motor it clearly showed the side-to-side tipping of the radiation belt. The short piece of 8 mm movie that I have of the model is quite amusing.

As a research project for Jack Winzer, one of the graduate students, I suggested a project that arose from a discussion that I had had with Jan Hogbom on one of our Parkes visits. If you have ever compared contour maps with maps in which the height of the land is shown by colouring, then you will appreciate this project. Because they were not able to *photograph* the radio sky, radio astronomers made *contour* maps of the radio emission. They recorded the strength of the radio emission along a series of scans across the sky, and from these measurements constructed maps in which contour lines joined places of equal radio emission. Now the features that dominate such a map are the *steep* areas; there the contours lines lie close together and attract the eye. By contrast, the features that dominate an optical photograph are the *bright* areas. Problem: How to make images of the radio sky that, like an optical photograph, emphasise intensity of emission rather than *rate-of-change* of intensity of emission?

It is all very simple these days with computers and digital recording. However digital recording was not available at the Algonquin Radio Observatory at that time. We used an *instrumentation tape recorder*, an analogue device that can record at one speed and play back at a very different speed. We scanned the telescope across the sky and recorded the strength of the received radio signals with the tape running at the slowest possible speed. Recording scans across the five-degree-square region of sky that we surveyed took many hours of telescope time, spread over two periods of several days each. Afterwards, in the laboratory, we played the tapes back at the highest possible speed (16 times the recording speed), and used the output signal from the tape to control the brightness of the spot of an oscilloscope (TV screen). As we played the tapes back we drove the spot across the screen to follow the pattern that the telescope had followed across the sky during the recording. A camera attached to the oscilloscope took a time exposure of the screen and thus we produced a 'radio photograph' of the region. The playback occupied about ten hours, which required a very stable (solid state) oscilloscope.

We ended up with an interesting 'radio photograph' of the Cynus X region. In our published paper we demonstrated the relative advantages of the two types of display by presenting a contour map, made from the same observations, alongside our 'photograph'. Of course the process we used to make the photograph was very awkward, and there was little opportunity to apply calibrations and corrections after the observations were recorded. As a consequence there were many imperfections in the image. Nowadays digital recording and computer processing produce artefact-free images which can be displayed as black and white images, or in 'false colour', where different colours represent different intensities of radio emission.

For his MSc thesis project, another student, Jim Clarke, computed Jupiter models, extending the calculations of the radio emission from electrons spiralling in a dipolar field that had been made earlier by Ortwein, Chang and Davis. While I suggested the project, Jim was advised on computational methods by two of the theoreticians on the staff, Maurice Clement and Robert Roeder. (In our household, mention of the Roeders brings memories of our visit to their house, when Dagma greeted us at the front door accompanied by their Siamese cat, Andromeda. I am afraid that Gina ignored Dagma and lavished her attentions on Andromeda!) Jim Clarke's calculations for the first time included the degree of *circular* polarization of the radiation. The calculations were made for a thick shell of electrons, and included the effects of shadowing by the planet. In later years I used a modification of Jim's computer code to calculate models to fit the observations of Jupiter that Glenn Berge, Carl Bignell, and I made with the US Very Large Array.

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A few months after we arrived in Toronto Jocelyn Bell discovered pulsars, a discovery that has changed the face of astronomy. Because the Cambridge radio astronomers wanted to understand the significance of their discovery before making it public news of the discovery was suppressed until the following February. When the announcement came it triggered a flurry of activity at radio observatories around the world. Had I still been at the Radiophysics Laboratory in Sydney no doubt I too would have been caught up in this observing spree. However the Algonquin Radio Observatory was not a place for studying pulsars. Its forte was the centimetric wavelengths, whereas pulsars are strong in the metre wavelength range. In any case much of my time had to be devoted to the preparation of lectures.

Pulsars are produced when massive stars exhaust their supply of nuclear fuel and explode, producing a dazzling light that lasts for weeks. Viewed from the distance of the earth this bright flash looks like a brilliant new star - a *super nova.* The outer shell of the star is thrown off and expands out through the interstellar gas at speeds of thousands of kilometres per second. The core of the star collapses, and can become a neutron star, an object as massive as our sun, but only 10 or 20 km in diameter. Like many things in the universe, neutron stars are almost beyond our imagination. The material of these stars is not composed of atoms. Under the intense gravitational pressure the atoms have collapsed; the electrons that surround the nucleus in normal atoms have been forced into the nucleus where they have combined with the protons to form neutrons. These neutrons are packed tightly together to produce material so dense that a piece the size of a pinhead weighs nearly a million tonnes!

The collapse of these stars causes them to spin more rapidly, in the same way that the spin of a skater increases when she retracts her outstretched arms and legs. In the case of a neutron star the contraction is so great that after the collapse the stars can be spinning *many hundreds of times per second.* An absolutely incredible idea - something as massive as the sun - 300,000 times more massive than the earth - spinning hundreds of times a second. The contraction also increases the strength of any magnetic field possessed by the star. As a result neutron stars can have magnetic fields *tens of billions* of times as strong as the magnetic field of the earth. Perhaps it is not so surprising then that some of these stars emit beams of intense radio emission, beams which sweep across the sky as the star spins. They are radio lighthouses. If the earth happens to lie in the path of such a beam then the earth receives a pulse of radio waves each time the beam passes us. The resulting steady sequence of radio pulses is the signature of a pulsar.

At Toronto my only foray into the pulsar field was to collaborate with Greg Fahlman, one of the graduate students, in writing a short note to the journal *Nature.* We pointed out a fatal error in one of the proposed theories of the origin of the pulsar radio emission. But pulsars were certainly a hot topic when I attended the Texas Symposium on Relativistic Astrophysics held in Dallas in the summer of 1968. It is from there that I recall an over-breakfast discussion between John Bolton and Tony Hewish. It was PhD student Jocelyn Bell who noticed the strange, periodic pulses in the Cambridge scintillation records. However Tony, Jocelyn's supervisor, made the point that it was his knowledge and experience that allowed him to see the significance of the discovery, and therefore he was entitled to much of the credit. John's ready agreement left me a little disturbed, but I must say that apportioning honours in such cases is certainly difficult.

At the hotel where the symposium was held Gina delighted in asking directions of the uniformed doorman who stood at the main entrance. She loved his Texan accent! She was also amongst those who visited the Nieman Marcus store, close by the hotel, and returned carrying a parcel containing a 'Newton's Pendulum', a set of heavy balls hung from V-strings as one-dimensional pendulums. We found it fascinating to watch the transfer of momentum between the balls when one, two, or three of the balls were displaced sideways and allowed to fall back. I doubt if any of the conference delegates splurged on the featured Nieman Marcus' *Christmas Special for the couple who already have everything*. That year it was 'His and Her Submarines'!

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A notable advance in radio astronomy techniques occurred while we were in Toronto. As I explained earlier, an *interferometer*, made by connecting together separated aerials, has the same power to resolve angular structure in arriving radio waves as does a filled aperture having the same overall extent. However to achieve the full power of this technique the connection between the aerials must be *phase coherent*, i.e. it must preserve the (relative) phase of the radio waves arriving at the different antennas. It is the difference in the *phase* of the radio waves at the different parts of an aerial system that carries information about the direction of arrival of the waves.

The normal practice in interferometry is to mix the radio signal from each aerial with a local-oscillator so as to reduce the frequency to an intermediate frequency, or IF. These IF signals will preserve the phase differences of the radio waves provided the local-oscillator signals used at the aerials are phase coherent. So you are faced with two problems: to provide phase-coherent local oscillators at the aerials (e.g. by transmitting a signal out from a central point in a phase-preserving manner), and to return the IF signals to a central point in a manner that preserves the phase differences. When the separation between the antennas is very great these become formidable problems.

I remember that when I was at Caltech, in other words ten years before my Toronto sojourn, Gordon Stanley talked of the possibility of using 'independent local oscillators' in interferometers. I think the idea came from the Jet Propulsion Laboratory. The notion was this: Even if the oscillators at the individual aerials are completely independent, provided they are very stable, their phase difference will change very slowly during an observation. If the actual phase difference can be established by other means, e.g. by observing radio sources of known position, then the need for a connection between the local oscillators would be eliminated. I took this science fiction with a grain of salt, but obviously those close to the action saw it as a serious future possibility.

It came to pass while we were in Toronto, and these days it is an everyday affair under the title of Very Long Baseline Interferometry, or VLBI. The baselines used not only bridge continents, they include satellite-to-earth baselines. The stable local-oscillators are usually derived from Hydrogen masers, with a little assistance from Rubidium standards, and the IF signals are sent back to the mixing point on magnetic tapes! Universally transmitted time signals are now sufficiently accurate to allow the tapes to be synchronized to a few cycles of the intermediate frequency, and then in the processing stage a little computerised searching allows them to be aligned to the required accuracy.

There was a certain rivalry between the US and Canadian groups who were developing this Long Baseline Interferometry. Each wished to be the first to use the technique. In the event the Canadian group won by a nose. However, as I delighted in pointing out to these high-powered groups, they were both beaten to the punch by the lowly Jupiter observers at the University of Florida. In January 1967, apparently unaware of the US and Canadian Long Baseline projects, the Florida group used a system with independent local oscillators to study the angular extent of the sources of the radio bursts emitted by Jupiter. Of course for these observers, working at frequencies near 20 Mhz, the technical problems were much less severe than for the groups working at higher frequencies. However, with a little assistance from the WWV time and frequency transmissions, they were able to operate with independent crystal-controlled oscillators at the two antennas, and tape record the outputs for later correlation.

The Canadian Long Baseline Interferometry group was led by Alan Yen, from the Engineering Department at the University of Toronto. Alan was a delightful guy. While entertaining us to yum cha at his favourite Chinese restaurant, he would regale us with stories of his sailing exploits on Lake Ontario. According to Alan, should you have the misfortune to fall into the lake at the start of the sailing season your life expectancy is but a few minutes! While we were in Toronto Alan took a set of Canadian LBI equipment to Australia and ran an interferometer between the Parkes and Algonquin radio telescopes. The experiment was very successful, proving that the long baseline interferometry technique would work between continents, and making ground-breaking astronomical measurements. However on his return Alan told us that when he had first arrived in Australia he wondered whether the project was doomed to failure by the language barrier. They had been greeted at the airport by Bob Batchelor, who speaks rather rapidly with a good Aussie accent. It seems that Alan's initial reaction was: 'If they all talk like this it will be hopeless.'

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During our second winter in Canada we drove down to Ithaca, in the northern part of the US state of New York, where I had some discussions with the radio astronomers at Cornell University. En route we came round a bend in the road to find ice yachts screaming across a frozen lake. They were heeling over in the wind so that their hulls lifted high in the air and they skated along on one outrigger and the tailskid. I stopped the car and we sat and watched as the boats raced past us and then turned and went far out over the lake to make a circuit and come racing past again. I thought 'How I'd love to try that'. The only negative was the noise, as they bumped across the uneven surface of the ice. Then later on, as we drove into the city of Ithaca, we saw that supercilious goose that waddled across the frozen pond with head held high, trying to look *so* important, until his feet slipped from under him, as pride went before a fall. I captured him nicely with the movie camera.

Cornell University had been one of the early players in radio astronomy in the USA. Back then it had been in a small way, but now the University operated a dish 1,000 feet in diameter, located on the Caribbean island of Puerto Rico. In the course of our discussions Frank Drake offered me a year's appointment at the telescope. In truth, I think they were having trouble finding radio astronomers willing to spend time there. I just laughed, saying I was on leave from CSIRO, and due to return to Australia in August. But back in Toronto, in the depths of the Canadian winter, the back of *The Toronto Star* often featured a full-page colour advertisement declaring the Caribbean to be 'The Closest Place to Paradise'. Gina asked: 'Did you give Frank Drake the right answer about that job at Arecibo?'

A Year in 'Paradise'

As we emerged from the air conditioned interior of the plane, and were engulfed in the hot, moist air of San Juan, I recalled my first visit to Puerto Rico, in 1965. Then I had arrived first class, and been welcomed with a delightful daiquiri. Taking the new Qantas flight from Sydney to Mexico City (with an overnight in Tahiti at the airline's expense!), I had then enjoyed a weekend stopover in Mexico City, before continuing on by a Pan Am flight to San Juan. Quite a direct route on the map! The trip was to a NASA sponsored conference on the planets, where I presented the opening review of the session *Jupiter, as observed at short radio wavelengths.* The return trip to Australia took in a number of radio astronomy centres in the southern USA.

I made the airline bookings for that trip on the assumption that I was paying the airfare. Then CSIRO decided to pay my airfare and a per diem, and I was surprised to learn that CSIRO officers on duty travelled first class. The admin. section found that they could change only some of my flights to first class, which meant that I was able to make a direct comparison of economy and first class travel. After the trip I wrote to the chairman of CSIRO suggesting that paying first class airfares was a very expensive way of providing free drinks. (In those days economy class passengers had to pay for alcoholic drinks, which were supplied free of charge to first class passengers.) Fred White wrote back to say that he disagreed; he believed that on a long trip the extra legroom in first class was well worth the extra cost. Some years later CSIRO, along with all government departments, adopted economy class travel.

That planetary conference was held at the Dorado Beach Hotel, a luxury enclave on the outskirts of San Juan. I believe it was owned by the Rockefellers. From the airport, the taxi took me through the poorer areas of San Juan, past wooden shacks and dilapidated houses so typical of tropical climes, then through huge guarded gates, past the private golf course, to the hotel, a group of single story buildings. My memory is of a hotel of fabulous tropical luxury beside a white sandy beach, with the 'yellow birds' of the song stealing sugar from the breakfast table beneath the palm trees, and of balmy evenings when limbo dancers performed while we drank exotic rum punches. So it was intriguing to discover a letter that I wrote home at the time.

'I'm not in love with Puerto Rico. As a tropical paradise it falls far short of Lindeman Island. Sure it's tropical. Much stickier than Tahiti. I arrived late at night, cheesed off by the lack of efficiency of Pan Am, short of sleep, and this hot moist air did nothing to make me sparkle. It was midnight before I was in the expensive hotel where the conference was held - $24 per day without lunch. Well I expect the world for that. I'd never paid such a sum before and I expected to walk on ermine, eat off gold plates, and really be served like a king. In fact the room was about equal to a Californian $5 motel and the service stank. Added to this I couldn't sleep and so spent half that first night preparing what I was to say at the conference the next day. Dragged myself out of bed the next morning and pulled back the "drapes" and there was white beach, white foam and palm trees straight outside my window! Did a lot to raise my spirits.'

'Other good things about the place:- the fresh pineapple juice, the rum punch at a cocktail party (you wouldn't approve!), a steel band and limbo dancer that performed one night. That steel band was great, I felt I wanted to get an oil drum myself. Oh, and the little yellow birds that came and sat on your breakfast table, particularly if you left the lid off the sugar!'

There is also this comment: 'I naturally had an eye to see whether I might like to try for a job here for a couple of years. Half an hour in Puerto Rico convinced me I didn't. I couldn't stand the atmosphere - this terrific contrast of rich and poor. Fabulous hotels for rich American tourists and subsistence living for a lot of Puerto Ricans. Although apparently a small fraction of the Puerto Ricans are really rich.'

In my conference review I was able to compare some preliminary calculations of the synchrotron emission expected from electrons trapped in a magnetic field with the first map of the radio emission from around Jupiter. The calculations were by Ortwein, Chang and Davis from UCLA (###); the map was made by Glenn Berge with the Caltech interferometer. This comparison of theoretical and observational *maps* added weight to the conclusion that Max Komesaroff and I had reached from comparing theoretical predictions and measurements of the beaming and the linear polarization of the *total* microwave emission: there must be two fairly distinct groups of relativistic electrons in the Jovian 'Van Allen belts'. Electrons in the first group move in flattish spirals as they cross the magnetic equator and consequently mirror at low latitudes. Their radio emission is like an equatorial doughnut, viewed more or less edge on. The other group move through the equator in relatively steep spirals and mirror at high latitudes to produce two smaller-diameter doughnuts of emission, displaced from the equator towards the north and south magnetic poles. My analysis for this conference was a forerunner to work in later years when I was myself involved in detailed mapping of Jupiter's radio emission, and in calculating models to fit those observations.

After the conference I stayed a few days at the Arecibo (Radio) Observatory, in rooms provided for visiting observers. The Arecibo Telescope was then, and still is, the largest 'dish' telescope in the world. It is an amazing structure. To visualise the telescope you need to imagine a strange landscape of peaked limestone hills. Draped across a small valley in these hills - a limestone *sink-hole* - is a grid of steel cables, which hang from three supporting towers standing on hills round the rim of the valley. Mesh, supported on this grid of cables, forms the telescope 'mirror', a bowl 1,000 feet, or 1/3 km, across.

Above the centre of the bowl, suspended from other cables attached to the towers, is a circular rail-track, (###) feet in diameter. Hanging beneath the rail-track, supported by wheels which run on top of the track, there is an inverted arch. And then hanging from the bottom of this upside-down arch there are two 'carriage-houses' which can move along the arch. From the undersides of the carriage-houses hang long 'line antennas' that point down at the dish. These collect the radio waves focussed on them by the reflector, or, in the transmitting mode, distribute the transmitted power over the reflector surface. The structure hanging from the rail-track weighs 1,000 tons! By rotating the inverted arch on its supporting circular track, and moving the carriage-houses along the arch, the two radio beams can be steered over a circular patch of sky 38 degrees (###) across, centred on the zenith. It is a science-fiction structure and, at night, the view of the telescope from the control building is truly amazing.

At the entrance to the telescope site there were guards, accompanied by Alsatian dogs, and looking very sinister in their black uniforms. In truth they were gentle guys who idled away their hours on duty by carving figures into the limestone cliff beside the guardhouse. Posting the guards was the result of the FBI uncovering a plot to blow up certain large US installations on the island, including the telescope. Administratively, Puerto Rico is a strange mixture. It is a US territory, and Puerto Ricans have US citizenship. There are as many Puerto Ricans living in New York as in P.R.. Communications, foreign affairs, defence, etc are administered by the US. However Puerto Ricans have their own government; they don't pay US Federal taxes; Spanish is the official local language; all distances are in km, and speed limits in km/hr (despite the fact that the cars are American with speedos in mph!). In some ways you might think Puerto Ricans have the best of both worlds. However there has always been some agitation for independence from the US, and at this time the *independistas* were quite active.

An enduring memory from this visit to P.R. is of being taken on a trip to the south of the island by three Australians: Don Campbell, Ron Wand and Ron's wife, Judy. Don and Ron were PhD students from the University of Sydney undertaking research at the Arecibo Observatory under a scheme arranged by Harry Messel. They drove me down '...the Rio Grande de Arecibo. Quite spectacular scenery with the muddy river far below flanked by canefields; banana plantations spread up the hillsides (they really are steep) with lush green vegetation everywhere. Judy got a great kick out of the mangoes that seem to grow everywhere' and could just be picked up from the ground under the trees. That letter didn't mention the dramatic change that you witness as you cross the divide when driving south from Arecibo. The island is about 170 km long east-west, and 60 km wide north-south. Arecibo is two-thirds of the way along the north coast. The road from Arecibo to the south rises to a height of about 1,000 metres, at which point you suddenly pass from lush rain-forest to an arid landscape. The change is so dramatic that the area on the southern slope is called a desert.

In Ponce, on the south coast, we bought 'enough pasties and cakes for lunch for four for 80 cents', and then went snorkelling at an isolated beach on the west end of the island, reached by a dirt track through the canefields. Well snorkelling is not really the right word for what I did at Puerto Real, for while they had a spare mask for me, there was no spare snorkel, so that my under-water time was quite limited. Nevertheless I found the coral and fish truly impressive, and the water was delightfully warm. 'Judy, in her two piece costume, was a lot of interest to the locals - they are not used to this sort of thing.'

In the evening I was taken to Phosphorescent Bay at Parguera. 'The wake of the boat lit up brightly. You would see fish swim off as they heard the boat coming, giving a bright flash in the water. They...hauled a bucket of water on board for you to splash your hands in, and of course the Aussies leaned over the side and splashed in the flashing water. Really something I will remember. We thought they should have taken us swimming there. You would be safe from sharks and barracudas - you'd see them coming.' It certainly is something to remember; something I have seen nowhere else. When Gina and I lived in Puerto Rico we took, or directed, all our visitors to Phosphorescent Bay. So what a sadness to learn recently from Ron Wand that the bay is no longer phosphorescent. It seems the glowing creatures have been killed by pollution.

Of the journey back from P.R. on the occasion of this 1965 visit, I recall only my stay in Gainesville, Florida, with Tom and Glenna Carr. At the University of Florida Tom led a group studying the radio bursts emitted by Jupiter in the 20 Mhz (15 metre) range. Tom and Glenna had spent a few months in Australia when Tom worked with Bruce Slee, the Jupiter burst expert at the Radiophysics Laboratory. On the social side of this visit I remember my namesake, Jim, a sleek, black animal who would hop into any car with an open door. Jim was an otter, and lived like one of the family in the household of Tom's brother, a zoologist at the university. Jim was great fun.

On the scientific side the memories are of technical matters. The radio bursts from Jupiter are just brief pulses of radio emission, very like the pulses of radio emission produced by lightning, or by lots of man-made devices such as electric motors, (and nowadays, by computers). How to be sure that the pulse of radio emission recorded by your antenna comes from Jupiter and not from some nearby source of interference? If you could establish that the radio emission came from the *direction* of Jupiter it would be a great step forward. But recall that the accuracy with which an aerial system can pin-point the direction of arrival of radio waves is limited; the smallest angle that can be measured is, in degrees, 57 times the wavelength of the radiation, divided by the overall length of the aerial system. At a wavelength of 15 metres, to discriminate as coarsely as one tenth of a degree, requires an aerial system almost 10,000 metres (10 kilometres) across. So the Florida group were exploring methods of connecting aerials to one another in ways that preserved the phase-differences of the signals, even when the aerials were separated by tens of kilometres.

They wondered if they could use the long-distance telephone lines to make the connections, and as a test sent a 1 kHz signal down the telephone line. Thus they discovered some of the mysteries of *single sideband, suppressed carrier* radio telephony. The 1 kHz signal came out the other end at 999 Hz! In carrier telephony the signal to be transmitted is used to modulate a high frequency carrier, so producing two sidebands, with some residual carrier signal. In the single sideband, suppressed carrier system all that is sent down the line to the receiving end is one sideband, thus achieving a considerable saving in bandwidth. At the other end the telephone company restores the carrier with a locally produced signal. While this local carrier is nominally at the same frequency as the original carrier, for speech, a change of a few Hz in frequency is neither here nor there. And, of course, phone companies aren't concerned with phase. That spelt the end of that idea for the radio astronomers. As I have related in the previous chapter they then went on to develop an independent local oscillator system, and became the first in the world to use the technique now called VLBI.

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When Gina and I moved from Toronto to Arecibo in 1969 our first task was to find somewhere to live. Not an easy task as there were no estate agents, no local paper, and we were not fluent in Spanish. While English was the lingua franca at the observatory, Spanish was the local language. Gina, who is fluent in Italian and interested in languages, expected to pick up Spanish easily, and later, to the interest of all in the queue, she would practise her Spanish in the (US) Post Office. But perhaps Spanish and Italian are too similar; perhaps the humidity beat her; in any case, she never became fluent in Spanish. For my part I am hopeless at languages and since English was used at the observatory I 'got by' with the few words of Spanish that I had learnt in Caltech days. That led to some social isolation at the observatory: at lunchtime, those of the foreigners who were fluent in Spanish fraternised with the local staff, leaving the rest of us to our own devices.

For our house search we used a phrase book to construct a note which we left at any apparently unoccupied house near a beach: as some compensation for the rotten climate we were determined to live by the sea. 'Estar este casa por alquiler? Por favor telefonear El Radar.' 'Is this house for rent? Please telephone the Radar', for that was the local name for the Arecibo Telescope. Meanwhile, we stayed in the visitors' quarters at the telescope. One evening, to escape from the confines of the telescope, we took the 25 minute drive down the winding road to the town of Arecibo and ate in a North-American-style restaurant. After dinner we returned to the rented car to find that we had no car keys. We asked in the restaurant if anyone had seen them. No. No one had seen any keys. So, there we were in a strange place, 10 km from our beds, knowing few words of the local language, and with a car that we couldn't drive. We couldn't see the keys in the car, but nonetheless set out to break into it, and soon a bystander came along with advice: we should buy a knife from the little supermarket nearby. He was right; the knife did the trick. However there were no keys in the car. Back to the restaurant to find a telephone to ring the observatory and ask someone to come and rescue us, and as we entered the restaurant we were greeted with 'Are these your keys?'

No one responded to those notes that we left asking house owners to contact El Radar, but from some locals we learned where to find the owner of a beach cottage that we had seen at

Caracoles beach. We visited him in his town house with its elegant garden courtyard, surrounded by the usual steel grillwork, and learned that he was the advertising manager for Ron Rico. This was the local rum, and an important product as sugar is the main crop of the island. 'You come from Sydney. Wait.' And he produced a copy of the *Sydney Sun* newspaper showing himself and his daughter launching Ron Rico rum on the Australian market! Ron Rico, at $1.20 a bottle, wasn't a bad drop, so when we returned to Sydney I looked for it in the shops. No one had heard of Ron Rico so the launch could not have had the desired result.

So our first Puerto Rican home was a holiday cottage right beside Caracoles Bay. Sitting on the verandah you enjoyed a view of blue water, golden sand, and palm trees; an apparently idyllic location. The house, like most of the better Puerto Rican houses, was built of concrete with a flat concrete roof, tiled floors and metal louvres for windows. On those louvres Geckos lay in wait for unsuspecting insects. The house was definitely styled for short term visits. As I recall there were really just two rooms. The main room, dominated by a tiled bar, had a kitchenette at one end. Behind that was the bathroom, an amazing place where the shower head was straight above the toilet! In the grounds, under the palm trees, there were two old caravans - well rusted by the sea spray - which were to provide extra space. But in the event we lived there for only a few weeks.

All Saints Day ushered in the North Atlantic storms, and brought great waves crashing over the limestone headlands of our little bay. Waves began washing against a large dead tree that lay across the front of the house, below the verandah. I thought the tree was saving the house from being undermined, and with unusual prescience said we should get some chain and attach the tree to the house so that it would not float away. Next morning the tree was gone, and the undermining commenced. We suggested to the owners that they have a truckload of rock placed in front of the house as a protection, but they acted as if they didn't care. However they said *we* could do it if we wished. We tried. But the truck had to drop the rocks *beside* the house, and they were huge. Even with the help of local youngsters, organised by Patty Heiles who was fluent in Spanish, we were unable to move many rocks down to the front of the house. We gave up and moved out. The undermined verandah actually survived that storm, but I have seen a photo of the place taken a year or so later showing no verandah, and waves washing against the front wall.

Our new address was Km 1.6, Carretera Dos, which is to say 1.6 km along road number 2. Instead of milestones the main roads had markers every hectometre (0.1 km). This house was much larger, raised high above the yard, with a wide verandah enclosed with steel grill work, to keep the poor at bay. We called it The Fortress. Again the concrete construction with tiled floors and metal louvres. While the interior was, I suppose, a stark contrast to the timbered, carpeted, air conditioned interiors of North American homes, it was equipped with many of the facilities of a such a house, including two bathrooms. And it stood in stark contrast to the wooden shanties which were the homes of most Puerto Ricans.

Here our surroundings were 'jungle'. Coquis - little green frogs - sat in the bathroom and called 'Koe Kee, Koe Kee'. In the evenings fireflies came dancing down the hillside behind the house, and huge cane toads came to sit on the steps leading up to the verandah. The crowing of roosters woke us in the mornings, and Reinitas - the little yellow honeyeaters - stole sugar from the breakfast table on the verandah. In the heat of the day the pods on a large tree by the gate exploded open to spread their seeds afar. Each afternoon in the dry season the man from the nearby village sang as he rode by on his way home, his horse loaded down with green fodder for his cows. In the wet season thunderstorms each afternoon turned Carretera Dos into a raging torrent.

A mongoose lived behind the house. The mongoose was introduced to Puerto Rico to do what St Patrick did for Ireland:- eradicate the snakes. It did that, but apparently also killed the beautiful Puerto Rican parrots. Fortunately, many other colourful birds survived, including the humming birds. They came to the feeder that Gina put on the verandah, and one day they attempted to sample the hibiscus printed on the material of the dress she wore! But the Reinitas were our favourites. I have movie film of them that I made while reclining in a hammock on our verandah. After helping themselves to sugar from the bowl on the breakfast table, they take wonderful long baths in the water bowl, splashing water everywhere.

In Puerto Rico the density of population is almost 300 times the density in Australia, but it is only in the towns that there is a sense of crowding. In much of the countryside houses are not in evidence. Sugar cane covers the flat lands, while the hilly areas are lush with tropical growth. In the mornings, stylishly dressed young ladies emerge from tracks through this thick growth and wait beside the road for the bus to take them to work. From where we lived we could see no other houses - something that had repercussions when we returned to live in Sydney, as I'll relate later. Our nearest neighbours lived in a small village round a bend in the road, in houses with dirt floors, their water supply a pump in the village centre. But they did have television. They were Negroes. In contrast to many of the other Caribbean islands Puerto Rico has few Negroes, and here blacks are definitely at the bottom of the ladder. Skin colour is all important, with the light-skinned Spanish at the top of the pile, and so anxious to stay pale that they wear shirts or blouses when swimming.

Ferdinand, the Puerto Rican owner of our house, was a concrete contractor - no doubt a good business considering the wide use of concrete in new houses. He had served as a photographer in the US army, and considered himself almost a North American. He would apologise for his wife, saying 'You know, my wife, Louisa, she's a Puerto Rican'! When we first moved into the Fortress, Louisa came and cooked a meal for us. The creme caramel was delicious. Whether from Louisa or elsewhere, Gina learned to cook *pollo con ariz* - chicken with rice, heavily dosed with garlic - a common cafe meal. We still call this Puerto Rican chicken.

A tropical climate doesn't encourage rapid activity and we soon had to learn to live with *mañana*. When would our furniture arrive from the boat. 'Mañana'. With the same reply on the following days we rapidly became exasperated. Later, when our refrigerator stopped working, and the repairman would come mañana, Gina finally blew her top. Acting like an American she kept demanding action until they gave in, and came to fix it.

Social life in Arecibo was rather limited. Gordon Pettengill, the director of the observatory, broke the ice with a welcome party at their home. Our eyes boggled at the array of liqueurs they had to offer, and they explained that these resulted from trips to the nearby duty-free island of St Thomas, one of the Virgin Islands. The computer engineer at the telescope, who had his own light plane, would occasionally take any of the wives who wished on a day trip to St Thomas. When later Gina made such a trip she reported that on the return flight the plane was almost overloaded with bottles. Gordon, from MIT, was spending his second period at Arecibo, exploiting the capabilities of the telescope in planetary radar. Both Gordon and Pam, his English wife, were keen birdwatchers and told fascinating stories of bird-watching trips to South and Central America.

We made a few visits to the movie theatre in Arecibo when English-language films (with Spanish sub-titles) were being shown. The Spanish-language funnies shown before the main feature would have the locals in stitches, while we sat there glum-faced. On several occasions we tripped down to the south coast to enjoy the sights that I had been shown on my earlier trip. Particular favourites were the phosphorescence at Parguera, and the amazingly gaudy fire station in Ponce, 'the world's most photographed fire house', originally built as an exhibition hall for the 1883 fair. Norm Broten, visiting from Canada for an observing session at the telescope, came along on one of these trips when we witnessed a solar eclipse, or was it a spectacular comet? Both are associated in my mind with the south coast. There were also trips to San Juan to take visitors to admire El Morro, the sixteenth century Spanish fort, or sometimes just for Kentucky Fried chicken, which Gina had discovered in Canada! And once the University of Puerto Rico, in San Juan, hosted a concert by Pablo Cassals; a marvellous concert, and extremely popular. Indeed the crowds were so large that there was almost a riot as people attempted to leave after the event. When the traffic police blocked lanes of traffic that could have proceeded safely there was a cacophony of blaring horns, and then the Latin temperament took over and the traffic simply disobeyed the directions of the police.

Once a week Gina made contact with North America when she drove to the Remey Air Force base, 45 km west of Arecibo on the north-west tip of the island. The radio telescope had been built in the days when most US scientific research was funded by the military, in this case by the Air Force. For this reason the observatory staff had access to facilities at Remey where Gina would shop North-American-style at the PBX, swim in the pool, and generally enjoy being in the USA again. En route she often stopped at a stall overlooking the bay at Guajataca to enjoy a *paragua*, a drink made with shaved ice.

When we snorkelled at the beach called Sardinares we were surrounded by hundreds of fish, packed almost as densely as sardines, though they were somewhat larger than the ones in tins. Another delightful spot to visit in calm weather was the beach at Caracoles, near our abandoned beach house. Gina would collect shells and 'sea glass' to use in her art work; 'sea glass' being her name for pieces of broken bottle, ground by the sand to acquire a matt surface. And it was at Caracoles that I used a whole spool of 8 mm movie film to record the attempts of a hermit crab to find a better shell as his home. He would position himself beside a potential home, turning it so that he could quickly slip from his present shell into the new one. However it seemed that each time the new home failed to come up to his expectations and he made a rapid return to his original home. On these coastal visits we often encountered only one or two other people. While many of the visits were made on weekdays (because I was working nights or weekends), it still seemed surprising that in a country with such a high population density there were so few people.

Other memorable scenes from P.R. include cows up to their stomachs in water-lily-covered ponds, with the back of each cow decorated by a white cattle egret, their handmaidens, as Gina called them. And yuccas, with the spiky tips of their leaves decorated with upended empty egg-shells, presumably in imitation of the white flowers that appeared in season. Driving to the observatory at night, when the only lights visible were in one or two small shop-cum-bars, one had to be careful not to run over the dogs that slept on the still-warm bitumen surface of the road. Cane toads also frequented the road at night, sitting in the middle of the road, all facing into the breeze so as to collect insects. Of course some regarded it good sport to squash the toads.

The weather on this northern side of Puerto Rico was not pleasant. It was always sticky, and in the wet season there was a thunderstorm every day. Since the telescope was used round the clock it was sure to be in use when the inevitable thunderstorm occurred and caused a power failure. Uninterruptable power supplies were not yet the norm, but the observatory did have a standby generator. However instead of changing over to the generator before the thunderstorms hit, they mostly waited until a lightning surge took the power out. That meant the computers went down and the air conditioning failed, and after the standby generator was brought on-line it took another hour or so to get everything working again.

Two events associated with these thunderstorms stick in my mind. One day John Rankin was up on the telescope structure making adjustments near the focus when the storm struck. He had quite a hair-raising(!) experience. Fortunately no harm was done. The other occasion was when I was crossing the road that separated the telescope building from the office block. I was carrying a deck of several hundred computer cards containing the recorded pulsar arrival-times. There was no shelter over the roadway and as I ran across through the teeming rain I managed to drop the deck of cards. Cards went floating off in the torrent that rushed down the gutter. Fortunately we managed to retrieve them and found that after drying them off they could be copied.

Not all Caribbean islands have such a rotten climate. On the recommendation of the Pettengills, we spent a week's holiday on the nearby island of St Martin, and there the weather was great. Is that because there are no high hills? St Martin is an intriguing island, half is owned by the French and the other half by the Dutch. We enjoyed the many isolated sandy beaches, and often recall watching as the pelicans effortlessly rode on the wind, circling the bay with hardly a wing flap, and then suddenly diving to catch a fish below the surface. Their efforts to become airborne again were somewhat less gainly.

The telephone service in Puerto Rico earned a comment in that letter that I wrote on the occasion of my 1965 visit. I reported that the telephone from the observatory to the town 'goes via a radio link to an Air Force base 60 miles away. The service is hopelessly unreliable and its costs $1.00! This is so "Un-American" - as also is the lack of cleanliness in the quarters here.' The communications system hadn't advanced much by the time of our 1969/70 sojourn. Our house had no telephone, so, as a safety measure for the times when I was working at night, we were provided with a two-way radio for Gina to contact the telescope. But the hilly terrain meant that direct contact was not possible; messages had to be relayed via Sam Harris, the telescope engineer, who lived in the valley next to ours. Contact between the telescope and Cornell was also by radio, with one or two 'scheds' per day, chosen at times when it was hoped that the ionosphere would be cooperative, and the local static not too bad. Contact with Cornell was quite important as the telescope was ruled with a firm hand by Tommy Gold and Frank Drake from Ithaca. While Gordon Pettengill was the resident director of the telescope, my impression was that he had little autonomy.

As I mentioned Gordon's specialty was planetary radar. *El Radar*, the local name for the telescope, was quite accurate. The telescope had been built as a radar establishment designed to study the Earth's ionosphere by examining the echoes returned when powerful pulses of radio waves were transmitted upward from the antenna. Traditional methods of studying the ionosphere used radar transmissions in the *short-wave* band. Such signals penetrate through the lower part of the ionosphere but are reflected when they reach a height where the electron density is great enough: where the plasma frequency, determined by the local electron density, is equal to the transmitted frequency. By varying the transmitted frequency through a range in the short-wave band the heights of layers of different density can be determined, and so the structure of the lower part of the ionosphere can be investigated. However this technique provides *no information about the ionosphere above the densest layer.*

The Arecibo Telescope was designed to transmit higher-frequency signals that penetrate completely through the ionosphere. While such high frequency signals are not *reflected,* a process that depends on the *coherent* motion of electrons, a small amount of the signal is *incoherently scattered* by the electrons at every point along the path through the ionosphere. The Arecibo radar is so powerful that these weak scattered signals can be detected. In this way the ionosphere can be examined throughout its entire height.

Indeed the radar is so powerful that echoes from the moon, and from some of the planets, can be detected, so that *planetary radar* soon became an important activity at the observatory. Initially, passive radio astronomy - the study of radio waves naturally emitted by heavenly bodies - was a very secondary role for the telescope. But when Gold became head of the astronomy department at Cornell things changed, and by the time of my Arecibo sojourn, *radio,* as distinct from *radar,* astronomy occupied about half of the telescope time. The radio astronomy section was headed by Carl Heiles, a hydrogen line specialist visiting from the University of California, Berkeley.

Pulsars, discovered while we were in Toronto, were the hot topic at Arecibo. The large collecting area of the telescope offered a great advantage for the study of the weak, brief, radio-pulses received from these 'radio lighthouses'. In addition, the observing techniques that had been developed for the radar projects at Arecibo were readily adapted for pulsar studies. Up to this time radio astronomers had dealt with continuous signals, and, for the most part recorded the output of their receivers on paper charts. Radar astronomers dealt with pulsed signals, and at Arecibo the signals were recorded digitally on magnetic tapes, often under computer control. Furthermore there was a CDC 3300 computer for off-line processing; quite an advanced system for the time.

Monitoring the *arrival-times* of the pulses received from pulsars became a high-priority study. Any slowing-down or speeding-up of the rotation of the star should be revealed by such timing studies, thus providing an insight into the physical processes occurring in the star and its immediate surroundings. Furthermore, by monitoring the arrival times of the pulses over a long period any movement of the star can be detected, since any such movement will change the distance from the star to the earth, and hence the travel time of the pulses. The annual movement of the earth around the sun also changes the distance to the pulsar and hence affects the arrival times. So, by monitoring the arrivals over one year the direction of the pulsar relative o the orbit of the earth can be measured.

At Arecibo timing techniques were already well developed for the radar studies. The planetary radar astronomers had also produced accurate *planetary ephemerides*, that is, detailed information about the motion of the earth and the other planets around the sun. Obviously an accurate knowledge of the motion of the earth was essential for interpreting any study of the arrival-times of the pulses. Combine these technical advantages with Tommy Gold's strong interest in the theory of pulsars, and you see why pulsar timing studies became big-time at Arecibo, with Tommy Gold largely calling the tune from Ithaca. I was asked to join the group studying the arrival-times of the pulses, and I worked closely with two bright PhD students, John Rankin and Dave Richards. John was the programming expert. He went on to become a pulsar specialist (and adopted the persona of Joanna). I believe that David later changed his career direction to became a *physician* rather than a *physicist.*

I had been used to the system at Parkes where a project was allocated exclusive use of the telescope for several days on end, or perhaps shared-use with one other project. Arecibo operated on a different system. The restricted sky-coverage of the telescope meant that any one object could be viewed for only about 3 hours per day. Furthermore some projects required observations every few days. As a result many of the projects were allocated just a few hours of telescope time on any one day. For the pulsar-timing project we would take over the telescope shortly before the pulsar under study came into the beam and then madly scramble to follow the instruction sheet so as to change over all the cabling to suit our project rather than the preceding one, and also to ready the computer for the timing program.

Our main target in the timing observations was the pulsar in the Crab Nebula. Yes, the Crab Nebula again. It was thought that pulsars must be *neutron stars*, the hypothesised, super-dense, collapsed remains of stars that had exploded as super novae. Tommy Gold was a champion of this theory. The Crab Nebula, that diffuse patch of luminosity expanding outwards at 1,000 kilometres a second from a star that had exploded in 1054 AD, was the best-known remnant of a super-nova explosion. So did the Crab Nebula contain the remains of the star that had exploded, and was it now a collapsed star operating as a radio lighthouse? It took some searching to find it, and it was not the first pulsar to be found in the remnants of a super-nova explosion. The honour for that discovery belongs to the Sydney University group who used their Mills Cross antenna near Canberra to discover a pulsar in the Vela super-nova remnant, a pulsar that figures large in the history of the understanding of pulsars.

The pulsar in the Crab Nebula was discovered by observers at the US National Radio Astronomy Observatory at Green Bank, in West Virginia. The pulsar is much weaker than the first pulsars detected, and it spins much more rapidly, 33 times a second. The first pulsars that were discovered spin only about once per second. For some years the Crab pulsar was the fastest spinning pulsar known, although nowadays pulsars spinning many hundreds of times a second are known. There were conflicting theoretical models for the way pulsars would lose energy, and thus slow down, and it was thought that timing of the pulses could discriminate between these theories. As the rate of slow-down was expected to be greatest for the fastest pulsars, timing observations of the Crab Nebula pulsar were a must. They began at Arecibo in November 1968, only a month after the pulsar was discovered, and by February 1969 the rate of slowing down had been measured. Then the target became the rate-of-change of the rate of slowing down, a number supposedly crucial for the theories.

The Crab Nebula pulsar is so weak that it is normally not possible to detect individual pulses; the discovery of the pulsar hinged on the fact that it occasionally emits super-strong pulses. But once the period was known approximately it was possible to add together large numbers of the normal, weak pulses and so produce a detectable signal. At Arecibo the incoming signal was sampled every 32 microseconds, so that there were more than 1,000 samples across one pulse period. Initially the 'adding-together' was done off-line by cross-correlating the recorded signal with a train of spikes spaced one pulse period apart. (This train of spikes is Ron Bracewell's 'Shah function' that inspired Henry Rishbeth's naming of my first car!). Later, when the period was known more accurately, tens of thousands of pulses were added in real-time in the computer. Off-line, an arrival time was determined for each of these averaged pulses by cross-correlating the observed profile with a template of the expected pulse shape. I should add that by the time that I joined the group in September 1969 all these observing and analysis procedures were well established.

To reach the earth the radio signals from pulsars must travel very long distances through the (extremely low density) gas between the stars. Because this gas is partly ionized it has several important effects on radio pulses. In empty space radio signals and light travel at the same speed, *the speed of light*. In ionised gas the highest frequency radio signals travel at essentially *the speed of light*, but lower frequency radio signals travel at somewhat slower speeds. [The 'phase' speed of radio waves in an ionized gas is actually greater then the speed of light - the refractive index is less than 1 - but the 'signal' speed is less than the speed of light.] As a result of the different speeds of travel for radio waves of different frequencies the waves that leave the pulsar at the time that its radio beam is pointing towards the earth become spread out in space. The highest frequency waves get ahead and arrive here first; lower frequency waves arrive after a delay, a delay that increases steadily as the frequency decreases, in fact increases proportionally to the inverse square of the frequency, i.e. the square of the wavelength. For the Crab Nebula at a frequency of 400 Mhz the delay is about 1½ seconds. Now in our timing observations the arrival times of the pulses were being measured to an accuracy of tens of *micro*seconds, so that a delay of a second, which might vary with time, posed a serious problem. It might seem that this difficulty could be overcome by observing at higher frequencies. However pulsars emit much less power at higher frequencies, and for the Crab Nebula pulsar 430 Mhz was the highest frequency that we could use. Our only recourse was to make accurate measurements of the delay by making timing observations simultaneously at several frequencies.

Another effect of the interstellar gas arises from the cloudy structure of the gas. This non-uniformity causes radio waves to travel from a pulsar to a point on the earth by a variety of slightly different paths, an effect called *interstellar scattering*. Even for waves of a single frequency the travel-times along these different routes are slightly different. As a result the pulse that we receive lasts longer than the time for which the sweeping radio beam is pointed at the earth. The scattering in the interstellar gas, and hence the *pulse smearing*, is much greater at lower frequencies. The smearing increases with decreasing frequency proportionally to the inverse fourth power of the frequency (the fourth power of the wavelength). For the Crab Nebula pulsar the smearing is so great that at frequencies below about 100 Mhz the pulse is smeared over the whole period between pulses. The source is no longer a 'pulsar'. It is an ordinary continuous 'discrete source'; indeed it is a radio source that was catalogued before pulsars were discovered!

from Cornell by non-participants in the observations. I worked with very pleasant people at Arecibo, and my association with some of them continued long after Arecibo days, but somehow there wasn't that feeling of camaraderie in the workplace that I had enjoyed elsewhere. That feeling of 'family' is engendered by management and would be difficult to create when much of the management came by two-way radio from several thousand kilometres away. I suppose, also, that by this time the cult of the individual, putting personal aspirations well above community good, a cult which so bedevils our present day society, had already gained sway in the US.

Our route back to Australia from Puerto Rico took us through the southern US. We first flew to Lubbock, Texas, where we visited Bill Sandel, who had been Gina's boss when he had worked in Sydney in earlier years. Draped across the streets of Lubbock was the message 'EAT MORE PORK'. It transpired that this was not an exhortation to help the local farmers; rather the intention was to devastate the opposing football team, which had a pig for its mascot! After visiting the Sandels we rented a fancy car and drove across the south-west to Pasadena, giving Gina a chance to enjoy some of the desert sights that had so enthralled me on my trip to Fort Davis in Caltech days. Then in Pasadena friendships were renewed, and science discussed, and on our last day in the US we were to visit Disneyland. Gina being a great admirer of Disney this was high on her list. 'But Disneyland isn't open on Mondays' they told us. We had to change our flight home and stay another day!

Ringing the Changes at Radiophysics

With the threat of World War II Sydney University agreed to the construction of a radar laboratory in the grounds of the university on the understanding that the building would be vacated within one year of the ending of hostilities. Well, I suppose the *cold* war was *hostile*, and it was still in progress when the Radiophysics Laboratory finally moved out in 1968! The new laboratory was built near Epping, in a suburb called Marsfield, a suburb that until then had been mostly orchards and chicken farms. Evidently a plebiscite of the staff, held while we were in Toronto, had favoured this area of Sydney. I am afraid the location didn't suit me. With the old location of the Laboratory I had been able to live in Randwick or Coogee and enjoy a swim before breakfast, drive to work in ten minutes, drive to the airport in ten minutes and drive to the city in ten minutes: and back then you could park at those places, too. When we returned from Arecibo in December of 1970 the main question was where we should live.

The Narrabeen-Newport area seemed to provide the best hope of a near-beach address with reasonable driving time to work. To help us search for a home in that area Steve Smerd had arranged rental accommodation for us at Elanora Heights. After several week-ends of searching we had found what seemed to be a fairly suitable house when Gina announced that she felt she couldn't live with neighbours so close at hand! It is true that we had just returned from a year in a land with a population density 300 times the population density in Australia, yet where we had lived in Puerto Rico there were no neighbours to be seen. Gina wanted a similar situation back here. I then discovered that a 5-acre *bush-block* in the Dural/Kenthurst area sold for about the price of a house-block at Newport. So off we went and rented in Dural to search in that neighbourhood. That is how we came to spend the next ten years in a house in the middle of an almost-square 5-acre bush-block near Round Corner, Dural. A delightful spot, with a little creek and a dam, lots of birds and trees. However hot as Hades in summer and a very long drive to the beach or the city.

Not only had the Radiophysics Laboratory moved, but there were soon big changes in the management. Taffy Bowen, who had been chief of the Division of Radiophysics since 1945, retired, and Paul Wild became the new chief. Paul then gave me the job of overseeing the library, editorial, and photographic services. Perhaps the appointment was in part to allow my promotion to a pay level where the job description required more than just research: each of the three services that I 'supervised' functioned perfectly well under their individual heads, without any intervention from me. However I did become heavily involved on the editorial side. Paul had me read and criticise, or arrange for another member of the research staff to read and criticise, every manuscript that was prepared for publication. Since its inception the Division had vetted all publications before they left the Laboratory. Presumably this practice had its origins in wartime secrecy but was then continued as a means of ensuring a high standard for Laboratory publications. While my comments and suggestions about manuscripts did not always endear me to the authors - I do tend to be a critical character - I hope that some of the suggestions had the desired effect of improving our publications. After I, or my substitute, was satisfied about the scientific content of the article, Marie Vickery, the head of the publications section, used her thorough knowledge of English to ensure that the grammar was correct and the meaning clear. Over the years Marie made a tremendous contribution to the clarity of our writing.

Overseeing these laboratory services involved me in a very sad episode. Ken Nash, a gifted photographer, and a wonderful person, had been the head of the Photo Lab for as long as I could remember. It was to Ken that we went with special requests for the processing of those films from Dapto. I remember at one time he organized a system of Paul Wild's concocting to 'compress' the time dimension on the films, so as to produce records of Type II bursts of a suitable shape for publication. A 5 to 1 reduction of the time dimension was accomplished by driving the original 35 mm film through the enlarger at 5 inches per minute while the image from the enlarger was recorded on another film that was moving at 1 inch per minute.

During the time that I was his supervisor Ken lost his wife after one of those horrible battles with cancer. He was devastated, and holed up in his house, refusing to come to work. I went to see him, and must have seen a copy of *The Songs of a Sentimental Bloke* by C.J.Dennis on his bookshelf. That became the topic of our conversation. I had long been a fan of C.J.Dennis, with a particular liking for *The Play*:

> Doreen an' me, we bin to see a show -
> The swell two-dollar touch. Bong tong, yeh know.
> A chair apiece wiv velvet on the seat;
> A slap-up treat.
> The drarmer's writ be Shakespeare, years ago,
> About a barmy goat called Romeo.

and especially the ending

> '*Peanuts or lollies!*' sez a boy upstairs.

As I was leaving Ken insisted on giving me the book, inscribing it to me, saying that he had no use for it. I left with a great sense of foreboding: without his wife Ken obviously felt that life was not worth living. A few days later he shot himself.

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At the time of Taffy Bowen's retirement the Division of Radiophysics was undertaking research in cloud and rain physics, and in radioastronomy. As Paul Wild was not interested in the cloud and rain physics programmes these areas were parcelled off as a separate division, to leave the Division of Radiophysics with just radio astronomy. Now radio astronomy is one of those subjects pursued for its own sake, because people are driven by the desire to know and understand, because they thirst to know the whys and the wherefores of the universe around us. It is not research undertaken with the aim of making short-term 'useful' contributions to society. In the longer term such studies revolutionise our lives, but they are not undertaken for that purpose. At this time the government was more and more requiring CSIRO to pursue research projects that produced short-term, monetary returns. This put the Division under pressure to add some 'useful' research to its activities and so Paul Wild examined numerous possible projects. He settled for a collaboration with the Department of Civil Aviation to produce a *microwave landing system.*

Landing guidance at airports was being provided by a system called ILS, for 'Instrument Landing System'. This rather-dated system used metre-wavelength radio waves. The international aviation authorities wanted to develop a more sophisticated system, working in the microwave band, and having the ability to guide planes on curved, as well as straight, approach paths. The Department of Civil Aviation was anxious that whatever system was selected internationally should be suitable for Australian conditions, and should be able to be produced in Australia at a reasonable cost. They were concerned that if a system was developed overseas with little Australian input then Australia could be held to ransom.

I acted as secretary for an extensive series of technical discussions between staff from the Department of Civil Aviation and the Division of Radiophysics, discussions which eventually led to the landing system called *Interscan,* devised by Paul Wild. I listened to all the discussion, asked enough questions to make sure I understood, and then summarised the salient points. However I didn't become a very active player in the discussions. Thinking back now I wonder if, when he asked me to take on that secretarial job, Paul might have hoped that I would make a bigger contribution to the project. But Paul could not have been disappointed by the contribution made by Dennis Cooper, a new young man in Harry Minnett's antenna group. Dennis not only knew his antennas but loved the politicking that became the main game when Australia attempted - and ultimately succeeded - in having *Interscan* adopted internationally ahead of the US system. (The US system was basically the Fourier Transform of *Interscan*, but Paul's system had advantages in implementation.) Some years later Dennis became Chief of the Division of Radiophysics.

I lack some of the qualities essential for a role of that type. I might have an exaggerated faith in the correctness of my own ideas, but I don't have the confidence to sway others to do my will. Indeed, I rather dislike people who dominate others, and determine what they should do. Yet surely that ability to dominate, even if in a gentle manner, is an essential ingredient of leadership. A driving ambition is probably also essential. However I doubt whether a leader needs a 'touch of ruthlessness', which I believe is what John Bolton suggested might be desirable when he was discussing with me his possible successors at Caltech. [I might add that years later, when attempting to re-organise radio astronomy in the Radiophysics Laboratory, another chief offered me the position of assistant-chief in charge of radio astronomy. Fortunately sense prevailed; in preliminary discussions we both realised that it would not work. My interests are in understanding the how and why of things, not in managing people.]

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The big event in radio astronomy during our absence in North America, the discovery of pulsars, had set the hounds running at Radiophysics. In a short time the Cambridge discoveries had been confirmed, new pulsars discovered, and new properties of these remarkable stars revealed. Both Max Komesaroff and Radhakrishnan were involved, and made major contributions to the understanding of pulsars. Max and Rad proposed what became the accepted model to explain the sweeping of the plane of polarization, a model which I assume was based on their previous experience with that other spinning magnet, Jupiter.

When I returned to Radiophysics John Bolton expected that I would become a major player in the pulsar work. Of course I had been involved in pulsar studies at Arecibo, but there I had not been able to pursue the aspects of pulsars that interested me, and I had missed out on the deep discussions about emission processes that had taken place at Radiophysics. With Max, Rad, and Jon Ables already heavily involved in the field I felt like an outsider. So I opted to continue with the project that we had commenced before I left for Canada, attempting to detect circular polarization in the emissions from extra-galactic radio sources. As it transpired , Dick Manchester, at that stage at the University of Massachusetts, soon afterwards returned to Radiophysics to take a leading role in the world of pulsars.

In our earlier circular polarization studies we had successfully measured circular polarization in the radio emission from Jupiter, but had failed to detect circular polarization in the radio emission from any extra-galactic source. In the interim others, including Ernie Seaquist from the University of Toronto, had reported success in this search. So from 1971 to 1973 various groupings of us, including overseas visitors Jean-Claude Ribes and Francois Biraud from the Meudon Observatory in Paris, and Rob Roger from the Dominion Astrophysical Observatory in Canada, searched for circular polarization in the radio emission from extra-galactic sources, covering a whole range of frequencies from 0.63 to 8.9 GHz.

The 1.4 GHz measurements used a polarization system that had been devised by John Brooks, John Murray and Rad to study the Zeeman effect in the 21 cm Hydrogen line emissions. While Rad wasn't involved in our observations, he and I had many long discussions about the properties of circularly polarized radiation. Neither was Max Komesaroff involved in the observations at this stage, but he also contributed to these discussions. It requires careful thought to be sure of the *sense* of circular polarization, i.e. to be sure whether the electric vector rotates in a clockwise (*right-hand*) or anti-clockwise (*left-hand*) sense. The definition is relative to the *direction in which the wave is travelling*, and refers to the electric vector on a *stationary* plane at right angles to the direction of travel.

We discussed questions such as:

- *At any one instant* the directions of the electric vector at points *along* a circularly polarized wave form a helix. For a right-hand polarized wave is that helix right-handed or left-handed?
- Does an antenna in the form of a right-handed helix radiate right-hand or left-hand polarization?
- Does an antenna that radiates right-hand polarized radiation receive right- or left-hand polarization?
- After reflection from a plane surface at right angles to the direction of travel a right-hand polarized wave becomes left-handed. So consider a black body emitter connected to a right-hand radiating antenna located at one end of a plane-walled box. The right-hand polarized radiation emitted by the antenna is reflected from the wall at the other end to become left-hand polarization. Is this radiation now re-absorbed by the emitter, or does it go on for ever being reflected back and forth in the box? And so on.

At Parkes we developed techniques for measuring very small degrees of circular polarization, ending up with a system where the errors were determined solely by the system noise. Under those conditions the large size of the Parkes dish gave us an advantage over others. For the stronger sources our measurements had errors of only a few 1/100ths of 1%! We found definite circular polarization in eight of the sixty six sources that we studied. All the sources with circularly-polarized emission were quasi-stellar objects, i.e. the enormously energetic cores of distant galaxies. The greatest degree of circular polarization we measured was just over 0.3%.

Some of the observers who had claimed to have measured circular polarization in the radiation from extra-galactic sources had reported that the degree of polarization changed appreciably over periods of a month or two. It was hard to believe that the emission from a huge galaxy could change so rapidly, and since these measurements were all being made near the limits of possibility these reports of changes cast doubt on the reality of the measurements. Our measurements, with their higher signal/noise ratio, confirmed that the circular polarization of some of the stronger sources changed with time, in some cases by a factor of two in a few months. And we were encouraged by the fact that, for the few sources that we had in common, the best measurements made by others at about the same time as our measurements gave similar values to ours - that is, after the Dutch group agreed that all their measurements had the wrong sense!

Shortly after our first results appeared in the journal *Nature*, Ryle and collaborators published measurements made at Cambridge which showed no significant circular polarization for any source, including some for which we had measured significant polarization. They suggested that the sources weren't circularly polarized, and that we and others reporting positive results had simply underestimated our errors. In fact, because we had a larger collecting area, our measurements were more accurate than the Cambridge measurements. It turned out that they had been plain unlucky; several of the sources for which we had measured significant polarization happened to be in a lowly-polarized state at the time that they made their observations. When their results and ours were plotted as a series in time - (with the sign of their results reversed, since, like the Dutch, they had also made an error in the sign!) - the whole made a convincing series.

My interest in measuring circular polarization was as a means of determining the strength of the magnetic field in the source. As I explained in discussing our earlier Jupiter observations, the degree of circular polarization of synchrotron emission depends on the strength of the magnetic field in the emitting region. So we used this theory and our measurements of circular polarization to make estimates of the strength of the magnetic field in these distant sources, finding values that were reasonably consistent with rather less-direct estimates of the field strengths. But then Pacholczyk threw a spanner in the works. He suggested that the circular polarization might not be the intrinsic circular polarization of synchrotron emission, but might result from the conversion of linear polarization into circular polarization.

Anyone who has studied physics will have met a *quarter-wave plate* which converts linear polarization into circular polarization. These devices are made from materials, such as crystals, which are *anisotropic*, meaning that the properties of the material are different along different directions through the material. Ionized gas in a magnetic field is an anisotropic medium. I have already mentioned that when linearly-polarized radio waves travel through such a gas the plane of polarization can be rotated. But in addition, under suitable circumstances, linearly polarized radiation travelling through such a gas can be partially converted to circularly polarized radiation. Since synchrotron radiation has a high degree of linear polarization, and since the radiation reaches us by travelling through the ionized gas between the stars which is permeated by weak magnetic fields, the question had naturally arisen as to whether the circular polarization we were observing could be the result of linear-to-circular conversion. For the gas between the stars, however, the chance that the conditions are suitable for conversion is so remote that we had dismissed conversion as the source of the observed circular polarization. But now Pacholczyk and others showed that when waves travel through a gas of *relativistic* electrons in a magnetic field there is a much greater chance of linear-to-circular conversion. They suggested that the observed circular polarization might result from conversion in the very region where the synchrotron emission is produced. This uncertainty about the origins of the circular polarization cut the ground from under my enthusiasm for studying the circular polarization of quasars. As far as I know, to this day, the origin of this circular polarization is still an open question.

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In the case of Jupiter there seemed to be no such problem of interpretation. There the synchrotron emitting electrons are not sufficiently relativistic to cause problems: the assumption that the circular polarization is the intrinsic polarization of synchrotron emission seems well founded. With this in mind some of the circular polarization group now turned their attention to Jupiter, using the improved techniques that we had developed to make more accurate measurements. We followed the variation of the circular polarization of Jupiter's radiation throughout the 9 hours and 55 minutes of the planet's rotation, and we found what we called a 'saturation' effect.

As I explained earlier, the 10 degree tilt between Jupiter's magnetic and rotational axes causes Jupiter's *magnetic equatorial plane* to sweep up and down past the earth as Jupiter rotates. In other words the earth travels through a range of Jovian magnetic latitudes. When we plotted the measured circular polarization versus Jovian magnetic latitude we found that as the magnetic latitude increased the degree of circular polarization initially increased steadily, but then the rate of increase slowed dramatically. By 5 degrees magnetic latitude the degree of circular polarization had reached 0.6%, but in the next 5 degrees it increased by only another 0.2%, to reach 0.8% at a latitude of 10 degrees.

The calculations that Jim Clarke had made for his Toronto MSc thesis actually contained a prediction of this 'saturation' effect that we had found. Kip Thorne's earlier calculations had shown that fitting both the degree of linear polarization and the beaming of the radiation required the presence two groups of mirroring electrons in Jupiter's radiation belt, one group crossing the magnetic equator in flatter helices, and one in steeper helices. Jim Clarke's calculations showed that at low magnetic latitudes most of the circular polarization is contributed by the electrons in flatter helices and that the degree of circular polarization of the emission from these electrons increases rapidly with increasing magnetic latitude. However, because the radiation of these electrons is highly beamed towards the magnetic equator, their contribution to the emission decreases rapidly with increasing latitude. Above about 5 degrees most of the circular polarization comes from the less-highly-beamed emission of the steeper pitch-angle electrons, and the degree of polarization of this component increases less rapidly with increasing latitude.

The range of magnetic latitudes that we experience during a Jovian rotation depends on the angle between Jupiter's rotational axis and our line-of-sight to the planet. When we made these observations in 1973 our line-of-sight was almost at right angles to Jupiter's rotational axis, and so the range of Jovian magnetic latitudes that we experienced during a Jovian rotation varied between about -10 and +10 degrees. As Jupiter makes its 12 year orbit around the sun the angle between our line-of-sight and the rotational axis varies by ±3 degrees. This happens because Jupiter's rotational axis is not perpendicular to the plane in which we move round the sun, *the plane of the ecliptic.* It is tipped at about 87 degrees. As a result of this effect, by 1975 the range of magnetic latitudes experienced during a Jovian rotation had become -7 degrees to +13 degrees. As this offered the chance to explore what happened at higher (northern) magnetic latitudes, Max Komesaroff and I teamed up again to make a set of measurements that defined this 'saturation' effect even more clearly.

Comparison of our observations with Jim Clarke's calculations, showed that the model with the two-component pitch angle distribution that had been found to provide a good fit to the observed linear polarization and beaming, also had a fall-off of circular polarization with magnetic latitude that matched our results closely. For this model to also match the *magnitude* of circular polarization that we measured, the strength of the magnetic field in the radiation belt at the magnetic equator needed to be 0.3 Gauss.

The Pioneer 10 and 11 space probes had recently looped around Jupiter measuring the strength of the magnetic field and the density of relativistic electrons along the trajectory of the spacecraft. From these measurements Van Allen and collaborators had produced models of the magnetic field, and of the distribution of the relativistic electrons, around Jupiter. These models had confirmed the radio astronomy discoveries that the magnetic field has the opposite sign to the earth's field, that the magnetic axis is tilted about 10 degrees to the rotational axis, and that the magnetic field departs from a dipolar form. And according to the Pioneer model the magnetic field had a strength of 0.3 Gauss at about 2.4 Jovian radii above the surface of the planet. This fitted well with the east-west extent of the radio-emitting region deduced from interferometer measurements.

So far so good. In addition it seemed that the number of relativistic electrons measured by the spacecraft would about account for the observed intensity of the radio emission.. However the spacecraft deductions about the pitch angles of the electrons and the energy distribution of the electrons were both at odds with the deductions from the radio results. To attempt to resolve this conflict I examined the range of two-component pitch-angle models that would fit the radio data. I found that the observed beaming and linear polarization could be matched by a broad range of models, but the variation of the circular polarization with magnetic latitude restricted the range of possible models to just the sort of pitch-angle distributions that radio astronomers had proposed. I then calculated the degree of linear polarization of the radio emission that would be expected from the Pioneer model and found it would be about twice what is actually observed. After I published news of this disagreement I heard nothing from the spacecraft people. However in a 1981 paper Imke de Pater appeared to solve the discrepancy. Amalthea, one of Jupiter's smaller satellites, circles the planet within the radiation belt, and de Pater showed that the distribution of pitch angles would be different inside and outside the orbit of Amalthea.

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When Martin Ryle and his collaborators had expressed doubts about the reality of our Parkes measurements of the circular polarization of extra-galactic sources I had written to Martin giving more details of our observations. To help establish the credibility of our measurements I had also sent our latest high signal/noise observations of Jupiter. The Cambridge measurements of circular polarization were made with their 5 km Telescope, and as a result of this correspondence I realised that this Cambridge telescope was capable of mapping Jupiter in some detail. The extent of Jupiter's radio-emitting region in the polar direction is determined by the distance that the spiralling electrons travel away from the Jovian magnetic equator before mirroring, and this in turn is determined by the pitch angles of the electrons as they cross the equator. Hence maps that delineate the polar (north-south) extent of the radio emission provide information about the pitch angles of the electrons at the magnetic equator. It seemed that the Cambridge 5 km Telescope could map the Jovian radio emission in sufficient detail to help to resolve the conflict between the conclusions about these pitch angles reached by the radio astronomers and the spacecraft investigators. I stressed to Martin the desirability of making these observations, adding that I would love to spend a period at Cambridge, joining in the making of the observations, and their interpretation. After all the itch to travel was there again, and Gina is an Anglophile with a great wish to live in England!

The Cambridge group had already mapped the radio emission from Jupiter with their earlier synthesis instrument, the One Mile telescope. While these observations had not produced maps of sufficient detail for my purpose, they had given the Cambridge group the experience of mapping a rotating object with a synthesis instrument. Mapping with a synthesis instrument requires the combination of observations made on different days when the antennas are at different separations, and at different times on each day as the source moves across the sky giving different *projected* separations of the antennas. Combining observations in this way (partially) fills the gaps between the antennas; it also adds some north-south components to the baselines. For a source that does not vary over time this process is fairly straightforward. However because Jupiter rotates on its axis observations made at different times are likely to be observations of different faces of the planet. To synthesise a map of one face of Jupiter the observations that are combined, although made on different days, and when Jupiter is at different points in its movement across the sky, must none-the-less be made at times when the planet has the same face towards the earth. This poses quite a problem.

For their earlier One Mile Telescope observations of Jupiter the Cambridge group had made three 12-hour observations at each aerial spacing, then sorted the observations and recombined them in such a way as to produce three maps of three different faces of the planet. Each map was blurred over 120 degrees of longitude, or one third of a planetary rotation. Maps having a similar east-west rotational blur but made with the 5 km Telescope might have delineated the north-south structure of the emission with sufficient detail to define the pitch-angle distribution of the electrons. However it seemed sacrilege to throw away the high east-west resolution provided by the 5 km Telescope. Furthermore, there was some concern that the multipolar structure of the magnetic field might produce features which, if smeared in longitude, would obscure any narrow north-south structure.

To make maps with effectively no rotational blurring, would have required 1,200 days of observations with the 5 km Telescope! Even to make maps with 30 degrees of rotational blurring, which it seemed might provide a good test of the pitch angle distribution, would have needed 24 days, and Martin Ryle was reluctant to commit so much time to the project. We finally agreed on a compromise model-fitting plan. Observations covering the whole 12 hour period were to be made on a small number of days, and these observations were then to be compared directly (i.e. in the Fourier Transform domain) with the predictions of models. For any one orientation of the planet there would be insufficient observations to synthesise a good map, but it was thought that the information content would be great enough to discriminate between models with different pitch angle distributions. The existing model calculations were to be extended to include the non-dipolar components of the magnetic field as measured by the Pioneer spacecraft. In any case there was clearly a need to develop the theory for the non-dipolar case, and I assumed that in Cambridge I could find someone who would show me how to do it.

Incidentally, in the course of these discussions, Ryle pointed out that using the full complement of maps made at all longitudes, and the principles used in computed X-ray tomography, a *3D* picture of the radiation belts could be constructed. Max Komesaroff was so taken by this idea that he proposed the construction of a special-purpose synthesis instrument for mapping planets!

Neither CSIRO nor Cambridge had money to support my participation in this Jupiter project so Martin Ryle made an application for a British Science Research Council Fellowship to support me. The decision on this application was not made until December 1977, when the application was turned down. At that point my involvement with the project ceased. However in September 1977 Sidney Kenderdine and Bruce Elsmore had taken advantage of the favourable location of Jupiter, and the configuration of the 5 km Telescope, to make observations of five different faces of Jupiter, with longitude blurring of 72 degrees, or one fifth of a rotation. Producing maps from these observations required much processing and they were not published until after we made our VLA observations, which I describe in a later chapter. In fact the most exciting advance made in the in the mapping of Jupiter in 1977 was made in the Netherlands where Imke de Pater, as her PhD project, used the Westerbork synthesis telescope to map not only the linear, but also the circular polarization of the emission. The maps of circular polarization showed major asymmetries, another result of the multi-polar structure of the magnetic field.

It is interesting to wonder whether I would have found someone in Cambridge who could have put me on the right track to calculate models that included the effects of the *non*-dipolar components of the magnetic field. During the correspondence with Ryle I extended the dipolar calculations made by Jim Clarke, writing my own Fortran program based on

Jim's code, [and in the process correcting an error in the code that allowed for the fact that radiation emitted behind the planet is blocked off]. Over the following years I attempted to discover how to extend these calculations to include the effects of the non-dipolar components of the magnetic field. I was not successful, with the result that the models that I calculated for comparison with our 1979 VLA observations were only dipolar models. In a 1981 paper in the *Journal of Geophysical Research* (Volume 86, page 3397) Imke de Pater developed a multipolar model. I found this paper complex , and have never struggled with it sufficiently to appreciate the details.

INTERSTELLAR SCINTILLATIONS

Pulsars are renowned for the regular spacing of their pulses. Indeed, some of the pulsars rival the best man-made clocks for accuracy in keeping time. By contrast, the *power* of the pulses often varies enormously from minute-to-minute. As I mentioned in the chapter about Arecibo, much of this variation in power is not produced in the pulsar; rather it is 'twinkling', caused when the radio waves travelling from the pulsar to the earth pass through the cloudy (non-uniform) ionized gas between the stars. The density gradients in these clouds bend the radio waves from their straight-line path, and as a result waves arrive at any one place on the earth after having travelled slightly different distances. Since these waves are no longer 'in phase' they *interfere*, sometimes producing strong signals and at other times weak signals. In other words the pulsar 'twinkles'.

For a source of radiation to twinkle (*scintillate* is the technical term) the *angular width of the source* must be sufficiently small. If a source of radiation is spread over too wide a range of angles it will not scintillate. This truth is encapsulated in that old adage: 'Stars twinkle, but planets don't'. Stars are just points of light, but planets have perceptible angular widths. You can understand why the angular extent of a source of radiation affects scintillation by thinking about the pattern produced when light from a torch is shone through a piece of 'ripple-glass' onto a sheet of paper, or think of the pattern of bright lines that you see on the bottom of a swimming pool when the sun shines through ripples on the surface of the water. If you move the torch (or the sun!) sideways you will see that the pattern of light and dark moves sideways. Now imagine that there are many small lights side-by-side. Each light will produce a pattern of bright-and-dark bands that is slightly shifted from the bright and dark bands produced by the neighbouring light. If the small lights are spread over a sufficient area then all the dark bands will be filled up with light bands and the pattern of bright-and-dark will disappear. In other words, if the light source (or in our case, the source of radio waves) is spread over too wide a range of angles, then the source won't not scintillate.

The scintillations that we studied at Dapto during the sunspot minimum years were the result of the incoming radio waves being deviated in passing through the ionosphere of the earth. At low frequencies many radio sources are affected by such *ionospheric scintillations*: sources can be as wide as one degree across and still exhibit ionospheric scintillations. Other scintillations arise when the radio waves from cosmic sources are deviated as they travel through the ionized gas streaming out from the sun, the solar wind. Only sources such as quasars, which have a very small angular spread, exhibit these *interplanetary scintillations*: the presence or absence of interplanetary scintillation can be used to set limits to the angular size of a source. For *interstellar scintillations*, caused by gradients in the ionized gas between distant stars, the restrictions on angular extent are much more severe. It seems that the only sources which exhibit interstellar scintillations are the pulsars, stars which have collapsed down to a diameter of 10 or 20 km, and being many light years distant from the earth, have angular widths of only one million, millionth part of a degree, or less.

Of course once it was realised that pulsars were affected by interstellar scintillations people looked for such scintillations in the radiation from other radio sources. Jon Ables and I searched for scintillations in the *line* radiation from OH-masers. A maser - the name standing for **m**icrowave **a**mplification by the **s**timulated **e**mission of **r**adiation - is the radio version of a laser; and, by the way, masers were invented before lasers. But the cosmic masers were Nature's invention. The OH-masers operate in the gas around certain stars, amplifying the natural radio emission from a part-molecule (or *radical)* which consists of one oxygen atom and one hydrogen atom hooked together. That combination is so chemically active that it doesn't last long on the earth. However the gas in space is so diffuse that encounters between molecules, and hence chemical reactions, are extremely rare, and so OH gas is relatively common.. The emission from a maser is concentrated into an extremely narrow beam and this led Jon Ables and I to wonder whether the OH-masers might be of a small enough angular extent to scintillate. However our search revealed no evidence for interstellar scintillations.

The big project that Jon and I undertook was to study the *dynamic spectra* of the scintillating pulsars. In the Dapto studies of ionospheric scintillations new features of the scintillations were revealed by recording the dynamic spectrum, i.e. the power received over a *range* of frequencies as a function of time. These recordings showed that when the scintillations were strong they were often strong across at least a two-to-one range of frequencies and so must result from an ordered convergence of the waves. Such behaviour could not be explained by the then-accepted theory that the scintillations resulted from waves arriving randomly from many directions. At times the Dapto spectra had almost-parallel, approximately equally-spaced ridges sloping across the frequency-time maps, a pattern that is typical of the interference between just two sets of rays. Paul Wild suggested that in these cases a 'wave-like' structure in the ionosphere was acting as a huge prism. Radio waves coming through the two sides of the prism were being bent towards one another and converging at the ground to produce this classical 'double-slit' interference pattern.

In 1970 Ewing and his collaborators at MIT reported that they had been recording the dynamic spectra of pulsar scintillations, and in some cases had found 'drifting bands' in the spectra. They suggested these bands must also be the result of interference between just a few beams. To quote their paper: 'The supposition that the scintillation pattern is produced predominantly by a few, perhaps two, beams is borne out in several cases by the simplicity and regularity of the patterns seen. The CP 1919 data of Figure 1 have a sinusoidal frequency structure, which would be expected on the basis of the two-beam model.' Now when I had first heard of the strong scintillations of pulsar signals I had recalled the Dapto scintillation studies, and the conclusion that the strong scintillations could not result from random scattering. Naturally, when I saw these systematic patterns in the dynamic spectra of pulsars I was fascinated, and wanted to see more.

The examples of dynamic spectra published by the MIT group showed two or three bands across the frequency range. To clinch the argument that these patterns were interference patterns caused by just a few beams we needed observations showing more of the pattern, including more bands. That suggested having more channels across the range of frequencies covered. While the MIT group had made some observations with a 413-channel auto-correlator, the outstanding examples of drifting bands had been recorded using filter-banks with only 40 or 50 filters. The Radiophysics Laboratory had recently constructed an auto-correlator with 1,024 frequency channels. It seemed to me that this instrument offered the possibility of making records of the drifting bands that would thoroughly test the idea that they were part of a simple two- or three-beam interference pattern. As Jon Ables was one of the designers of this device I talked with Jon about using the correlator to record the dynamic spectra of scintillating pulsars.

Jon is an amazing guy. If you want to know anything at all Jon is likely to know the answer: it can be some esoteric aspect of number theory, the functioning of the auto-focus on your camera, the theory of computer programming or some aspect of relativity. He reads extremely widely. And he is full of ideas, and is amazingly inventive. Unfortunately he decided that modifying the 1,024-channel correlator for pulsar studies would be difficult. Furthermore the new correlator was much in demand for other observations. By comparison there was only limited demand for several banks of filters that were available. Each of these banks contained 64 filters, the filters in the different banks having different bandwidths. It seemed that by selecting an appropriate filter bank, and an appropriate observing frequency for each pulsar of interest, we should be able to record dynamic spectra that would show 5 or 6 maxima across the frequency range. So over the period from June 1974 to April 1976 Jon and I made filter-bank observations of pulsars at seven different frequencies ranging from 340 MHz to 5 GHz.

For two of the pulsars that we studied the spectra seemed to be dominated by the interference between bundles of rays arriving from just a few different directions. The spectra of these pulsars often contained several approximately equally-spaced, parallel ridges, usually broken up by another set of approximately equally-spaced, parallel ridges with a different slope. That is just what would be expected if the incoming waves reached us from predominantly just three directions, with the waves from one of these directions being appreciably stronger. Interference between the stronger waves and each of the two sets of weaker waves would produce a pattern of interference fringes in the frequency-time display, with the ridges in the two patterns having different slopes. I later demonstrated this with Richard Lovelace's computer program TWINKLE which I discuss later.

The MIT group had also concluded that such patterns indicated the dominance of ray bundles coming from just a few directions. However this was contrary to the generally accepted notion that waves arrived more or less equally from many directions, and the MIT deduction seemed to have been ignored. So we tried to present a strong case to support their conclusion. We made a two-dimensional Fourier analysis of the dynamic spectra and demonstrated that they were indeed dominated by just a few Fourier components, i.e. just a few 'wavelike' patterns. Unfortunately the best proof, to whit excellent records, eluded us. Indeed, as the best evidence to refute an alternative explanation proposed for the drifting bands, we felt it necessary to cite a spectrum recorded by others, namely by Manchester and Taylor. The alternative explanation, put forward in 1973 by Shishov, and by Rickett and Lang, attributed the drifting bands to frequency-sensitive dispersion by a 'foreground' prism of ionization. That could explain a frequency drift in the brightenings, but it could not explain why a series of brightenings would occur along approximately equally-spaced lines in the frequency-time plane.

Processing these observations and preparing the results for publication occupied a considerable time. We had to write (Fortran) computer programs to produce pictures showing the strength of the radio signals over the frequency-time plane, and also to compute the two-dimensional Fourier transforms of these dynamic spectra. Quite apart from information on the systematic patterns in the spectra, our observations provided a body of statistical data concerning the bandwidth and duration of the brightenings in the frequency-time plane. These are important parameters for any theory of scintillations so we also needed to process the observations to produce these statistical parameters. As a result of the time needed for this extensive analysis, and because of our other commitments, the paper reporting our observations was still in draft form when (as I relate in the next chapter) I went to spend calendar year 1979 at the University of California at Berkeley.

The near-final draft of the paper that eventuated by the middle of 1979 was widely discussed with researchers in the U.S. who were studying scintillations. As a result, by the time the paper was submitted for publication in 1981 it already contained responses to some of the criticisms that had been levelled at it! As referee for our paper the journal chose a firm believer in the theory that waves arrive at the earth randomly from many directions. Indeed he was an author who had recently championed the alternative proposal I mentioned above that attributed the drifting bands to frequency-dispersive refraction by a large-scale prism, a process cannot explain the presence of several, approximately equally-spaced lines of brightenings in the frequency-time plane. In reporting the referee's comments on our paper the editor said: '... is very critical of the interpretation of the data...although he believes that a brief description of the observations, combined with a correct interpretation, would make a good paper. After discussion with me concerning the proper course of action, he proposes that, in view of the quite different conclusions that he believes follow from the data, he should become a joint author, writing a section using the arguments outlined in the detailed comments.'

Needless to say we found this a rather bizarre response. We replied restating our disagreement with the referee about the interpretation of the observations and noted that his 'detailed comments' had not altered our view as to the correct interpretation. Ultimately the paper was sent to another referee and, after revision, was published in 1982.

Three years later the original referee published a paper which repeated the dispersive-refractive explanation for the drifting bands, but then acknowledged that to explain the quasi-periodicity: 'the intensity pattern may result not only from the superposition of a large number of waves...but also from the interference of a small number of wavefronts....' In an article published two years later he further said: 'When pulsar intensity variations were studied as a function of their radiofrequency and time, remarkable quasi-periodic patterns became evident. The random speckle patterns caused by small clouds were being superimposed by refraction through larger clouds to produce interference fringes', and then cited his paper from two years back as the evidence. So here he was admitting that these patterns must result from the interference of waves coming predominantly from just a few general directions, but seeming to claim this as his idea, and making no reference to Ewing et al., or to the fact that we had defended their suggestion against his attacks.

To complete this sorry story. In 1987, Fiedler and his collaborators found striking variations in the radio power received from some very compact extra-galactic radio sources and suggested these variations were caused by refraction in the interstellar ionized gas. They weren't called scintillations because the time-scale of the variations was so much longer, many days to months. Evidently the structures in the interstellar gas causing these variations were of a much larger scale than the structures causing the pulsar scintillations. Romani *et al.* then proposed that each of the events observed by Fiedler *et al.* could be explained by the movement across the line of sight of a single cloud of ionized gas, a single smooth enhancement in the gas density, acting as a poor lens.

I had learned of a computer program, appropriately called TWINKLE, written by Richard Lovelace from Cornell. This calculated the distribution of power across the 'ground' below a *thin phase-screen*, the simplest approximation to the interstellar medium. The input to the program was the distribution of the phase of a wave across a plane over which the amplitude of the wave was constant. The output was the distribution of power across a second plane parallel to the first (the ground). Richard Lovelace was kind enough to send me a copy of this program and after Michael Drinkwater, a vacation student from Sydney University, made some modifications, I used the program for calculating the scintillation patterns resulting from various patterns of phase across a screen. In particular, in a few seconds of computer time, I repeated the calculations that Paul Wild and Jack Joisce had laboured on for months at Dapto thirty years earlier when they had calculated the intensity pattern produced by a sinusoidal modulation of the phase, such as might be produced by a density-wave in the ionized gas.

I used the modified Lovelace program to show that the detailed predictions of the simple Romani et al. model were not consistent with the observations of Fiedler et al. However if one assumed that there were smaller-scale fluctuations in density superimposed on the larger-scale enhancement, then it was possible to match the observations. When a paper giving these results was sent to the journal the editor reported that the referee 'recommends against acceptance unless the author can show the single cloud model to be invalid'. Well of course the paper had demonstrated that a smooth, single cloud did not fit the observations, but that a single cloud with small perturbations would fit the observations.

This was the same journal and editor as earlier and so I assumed that it was the same referee and (foolishly?) wrote back to the editor recalling the history of the drifting bands. The editor did not reply.

Of the over fifty papers that I submitted for publication during my time as a radio astronomer only two were rejected by a journal. The earlier case was in Dapto days shortly after it was recognised that the intensity of cosmic rays received at the earth sometimes increases suddenly soon after a large 'flare' on the sun. Ken McCracken from the University of Tasmania drew my attention to the fact that these increases did not begin simultaneously at different points on the earth. For a very large event of this type that occurred on February 23, 1956, I collected data on the onset times reported at 20 different cosmic ray stations. When these times were plotted on a map of the world contours of equal-arrival times could be drawn through them, suggesting an ordered progression in the arrival of the particles at different points on the earth. The first particles arrived over eastern Europe, with no particles being detected in Siberia, Japan, New Zealand or North and South America until some 6 to 9 minutes later, and then it was a further 10 minutes before particles arrived in Northern Canada. At Paul Wild's suggestion I prepared a paper which showed these results and suggested that they did not fit the then-current theory of the non-simultaneous onset. When the paper was sent to the journal the referee recommended rejection on the grounds that 'the author was not aware of current developments'. The referee wasn't named, but in the next mail I received a stack of preprints from one of the leading groups studying cosmic rays. As far as I could see they just re-iterated the previously suggested explanation.

It seems to me that in neither of these cases was rejection the proper course for the journal to choose. If a referee finds a demonstrable error in a paper, then I think it proper for the journal to refuse publication. As I recounted, my first solo paper was wrong because of the inconsistent neglect of relativistic terms. Had the referee detected that error then it would have been proper for the journal to reject the paper. (In the event the error was not detected until the paper was in print.) But when the facts being presented are not in dispute, and a referee recommends rejection because something in the paper is contrary to the referee's belief, then I think the proper course is for the journal to publish the paper and let there be discussion about the suggested interpretation. I would also suggest that if a paper submitted for publication draws conclusions contrary to the conclusions drawn by someone else, then it is surely foolish to choose that other as the *sole* referee.

BERKELEY

Brian Robinson closed a 1978 meeting of the radio astronomy group by saying: 'David Williams from Berkeley wants to come to Radiophysics for a year, and since he is an expert on low noise FET receivers we'd like him to come. As there are no funds to offer him a position, David has suggested that someone from Radiophysics might like to exchange jobs with him for a year. If anyone is interested in such an exchange then let me know.'

The study of *line radiation* was the speciality of the Berkeley radio astronomers: they had recently commissioned a millimetre-wavelength synthesis instrument for these studies. As this was Brian's area of interest and expertise I thought he might go himself. However it transpired that none of the specialists in line-radiation at the Radiophysics Laboratory were in a position to spend a year at Berkeley at this time. That set me wondering if I might possibly be considered for an exchange. I was no expert on line-radiation, but my feet were very itchy after the collapse of my Cambridge proposal, and I felt sure that I could find something worthwhile to do with the new Berkeley instrument. Just to be in a new environment and discuss scintillations and circular polarization with interested people would be very stimulating.

Perhaps it helped that Jack Welch, the head of the Berkeley group, had once been a planets man? In any case the exchange was arranged, and David Williams and I swapped not just jobs, but also houses and cars, for the calendar year of 1979. The Radiophysics Laboratory and the Roberts perhaps did better out of the exchange than Berkeley and the Williams. While the Williams no doubt enjoyed our modest house in the middle of five acres of Australian bush, we were ensconced in quite a large house in El Cerrito, perched on the hills on the eastern side of San Francisco Bay. And I didn't make much contribution to the projects of particular interest to the staff at Berkeley, although, as I shall relate, scientifically it was a very fruitful year.

San Francisco Bay provided yet one more climate: very pleasant in the spring and the autumn, sorry, the fall; icy in winter, but with no snow; and cold again in summer because of the thick sea-fog that fills the bay. Spring and fall are the warmest seasons in the Bay area: in summer it is often quite cold because of the fog. However, in summer, just poke your head over the top of the hills behind Berkeley and you find yourself in 100 degree heat. A truly dramatic change. Of course at any time of the year there is a dramatic change in the scenery as you cross over that ridge. The hillsides facing the bay are covered with houses, while the country beyond the ridge is national park. Deer from the park wander over to the humans' side of the hill and browse on shrubs in the gardens; an event to be guarded against by garden lovers, but a delight to Gina!

Even if some aspects of the climate in El Cerrito weren't to my taste, the city offered many advantages as a place of residence. We were close to all the facilities of a large city, yet on the edge of national parks. Down the hill from the house there was a good shopping centre with supermarkets and restaurants. The City of El Cerrito provided an excellent library and a swimming pool. A few miles to the north, at Richmond, there was an even larger shopping complex, including an ice-skating rink that Gina patronised. Ten miles to the south, beyond Berkeley, lay the city of Oakland. And from there San Francisco was just across the four-miles-long Bay Bridge.

The BART provided another link between El Cerrito and San Francisco. The **B**ay **A**rea **R**apid **T**ransit System was a very new item for California - a suburban railway. You have perhaps heard the story that in the 1920's the Los Angeles' streetcars were bought up and closed down by the car manufacturers, or was it the oil companies? - the story may be apocryphal anyway. The oil crisis - at this time there was still a 55 mph national speed limit to conserve fuel - had helped change public sentiment, and had led to the construction of this state-of-the-art railway linking the cities on the eastern side of the bay with San Francisco and the west, via a tunnel under the bay. The trains were fully automated, and driverless - well someone sat at the controls in case of problems. Magnetic-stripe tickets were purchased at automatic dispensers where their value could later be 'topped up'. And all was spanking new and clean; but greatly underused. As I travelled on the BART from El Cerrito to Berkeley each day I was amazed at the emptiness of the trains. Such a contrast to the crowded trains in Sydney!

In San Francisco we marvelled at the new Hyatt Regency Hotel with hanging gardens and glass fronted elevators *inside* its huge enclosed foyer; we enjoyed the cable-car ride to Fisherman's Wharf; even more we enjoyed the huge Golden Gate Park where everyone, but everyone, put on their roller skates, or rode their bicycles; and we liked to watch the sea otters off Cliff House as they lay on their backs to crack mussel shells.

On Christmas day we set out to walk across the Golden Gate bridge. We only made it half-way before turning back, but it was well worth it if only to appreciate the majestic construction of the bridge, and the massive cables that support it. Those wonderful Muir Woods, with their massive redwood trees, lie just across the Golden Gate. Our visits there reminded me of my visit twenty years earlier when John Shakeshaft from Cambridge was visiting the Owens Valley Radio Observatory and we had driven up to San Francisco. I have a photo of John in Muir Woods. He is standing beside a slice through a very old redwood. Dates marked on the cross-section of the tree identify tree-rings which grew in the years of various historical events. The year 1066 is marked for the Battle of Hastings, but in my photograph John is pointing to the nearby ring that grew in 1054, the year of the Crab Nebula supernova.

At Easter time we were visited by my sister Alice and her husband Norman, who had been enticed to see the US and Canada by the offer of cheap airfares. We drove them to Yosemite, Kings Canyon and Sequoia National Parks, then crossed over the Sierras at

Bakersfield to go out through China Lake to Death Valley, then south again through Barstow and San Bernadino to Disneyland. Leaving them there to continue their travels we drove back up that marvellous coast road from LA to San Francisco, stopping to admire 'Heart's castle' at San Simeon, the arts and crafts of Carmel, and the windswept cypress-trees at Point Lobos.

As we started out on this trip the William's little Datsun performed well, but when we came to hills it required repeated pumping of the accelerator pedal to coax it to the top of each hill. While we stayed overnight in Yosemite we left the car at the service station, and next morning were assured that it was 'fixed'. However we had not travelled far from the valley when the problem recurred. Dropping in to the next roadside service station we were told: 'Oh, it will be the in-line gas filter. They are supposed to be changed at each service, but they never do it.' And it was. Just a small plastic filter costing a couple of dollars and taking a minute to replace. This experience had an aftermath when I returned to Australia. One Sunday John Murray and I were driving to Parkes in a laboratory Holden when we had trouble climbing the hills crossing the Blue Mountains. It seemed so like our Datsun problems that I said: 'We need a new in-line petrol filter.' We had some trouble locating one on a Sunday in Lithgow, but once we found one our problems were over. Ah, the benefit of experience!

It was great to see Yosemite partly under snow; my previous visits had been in summer. I was again spellbound by Half Dome, an image originally impressed on my memory by Ansel Adam's photographs. And we were intrigued by the raccoon tearing the ranger's house apart; the ranger said it would be 'moved to another area'! In Sequoia and Kings Canyon, where there was even more snow, we walked and drove beneath those mammoth trees, but failed to find the tree that I had driven through in Caltech days.

From the snows of the Sierras to the heat of the desert. It is amazing that in a direct line it is only 80 miles from the 14,200 ft Mt Whitney, the highest point in the 48 states, to the lowest point at Death Valley, 282 ft below sea level. There the April heat was already too much for Gina. She took refuge in the Furnace Creek Visitors Center while I showed Alice and Norman memories of my trip in Pasadena days, a trip taken as a guest of Rotarians. The Devil's Golf Course and Zabriski Point were highlights, though for me Ansel Adam's photograph of Zabriski Point, with its long shadows, is probably a more enduring memory than the real thing! Gina did enjoy seeing Disneyland again, but we had a disturbed night at Anaheim as a police helicopter kept circling overhead directing a searchlight down into our neighbourhood. One more joy of modern living.

When the General Assembly of the International Astronomical Union was held in Montreal that August we took the opportunity to renew friendships from Toronto days. Starting out on this trip we were headed for San Francisco airport on the BART when the train came to a halt in the tunnel under San Francisco Bay. Then came the announcement: There had been a small earthquake. No trains would proceed until damage was assessed. Fortunately there was negligible damage, and we were soon on our way again. Fortunate. Ten years later that double-decker approach-road from Oakland to the Bay Bridge came crashing down in a quake, squashing cars. At Toronto it was again train-travel from the airport to the city, but what a contrast to the BART! Not new and shiny, but absolutely packed, with many people standing. In Toronto the public transport is so convenient that it is very heavily used.

My memories of the Montreal IAU General Assembly are of people rather than science. I recall dining in a Russian restaurant with a group that included Yuri Pariskii from the Pulkova Observatory. Yuri, whom I had met at the 1958 General Assembly of the International Astronomical Union in Moscow, had been one of the first radio astronomers from the Soviet Union to visit Australia. In the early 1960's he came here to an astronomical meeting, and stayed a few days with us at the Randwick flat that I shared with Don Salmon. At the dinner in Montreal Yuri insisted that our vodka should be accompanied by sliced cucumber prepared with salt in the proper Russian fashion. The restaurant did not seem to know the recipe!

That dinner was also the occasion when Morrimoto was serious - one of the few times that I remember that happening. Morrimoto is such a character that he quickly dispelled that stereotype image of the formal Japanese gentleman that was shown in cartoons. He spent several periods working at the Radiophysics Laboratory in Sydney, initially in my solar days. His English was excellent even then, but he would come into the cafeteria, sit down beside the best looking girl he could see, and say: 'You are very beautiful. I do not speak English'! On a later visit, when he was using the Parkes Telescope, he would fascinate the onlookers at breakfast by putting tomato sauce on his rice bubbles. Japanese and Anglo tastes are rather different.

At a talk he gave at the Montreal meeting Morri had the audience in stitches when he slapped ordinary opaque photographs -instead of transparencies - on the overhead projector declaring: 'This is our telescope, this our receiver', etc., and then followed this act by showing a detailed circuit diagram labelled in both Japanese and English - this time as a transparency that we could see - and then suddenly declaring 'There is an error', at which point he altered some of the Japanese! But at that dinner in the Russian restaurant he spoke of the problems facing Japan, of the country's inability to produce sufficient food to feed its citizens.

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Back to Berkeley. The University of California has numerous campuses or are they campi? Berkeley is one. UCLA (the University of California at Los Angles) is another, and so is UCSD, the University of California at San Diego. It was to San Diego that I went to visit Barney Rickett, an expert on scintillations. As I flew down the coast past Los Angeles I was amazed at the changes in the landscape in the twenty years since my time at Caltech. So much of the coast between LA and San Diego was now covered in buildings. Such a contrast to the time when I had tripped down the coast by car with Kevin Westfold and his family. I shuddered, and resolved that we must not let it happen to our coastline near Sydney. But I am afraid that while I did do a little voluntary work with conservation groups after I returned home, I was not the powerful Messiah that it would have needed to stop the sprawl. Our coastline has suffered the same fate, with houses, houses, and more houses replacing the trees and rocks of the bushland that covered those hills in our young days.

In discussing pulsar scintillations with Barney Rickett and his associates at San Diego, and in speaking about scintillations at Berkeley, I attempted to convince my listeners of the truth of the conclusion reached by Ewing *et al.* at MIT, and supported by the dynamic-spectral studies that Jon Ables and I had made at Parkes. The simplicity of the patterns seen in the dynamic spectra of some pulsars, and the appearance of a series of nearly-parallel, approximately equally-spaced lines in these spectra, must mean that interstellar scattering does not cause the radiation from these pulsars to arrive randomly from many directions. Rather the radiation must come from predominantly only a few directions. Certainly at San Diego I did not seem to be successful in my mission.

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The Berkeley radio observatory is more than two hundred miles north of San Francisco, at Hat Creek, close to Lassen Volcanic National Park. It was there, you will recall, that on that trip with Rad I was so amazed by the cafe proprietor who asked if there was a radio-astronomy equivalent of the optical *red shift* that allows us to measure the expansion of the universe. This is mountainous country, part of the Sierras, and so before my first visit I needed to purchase warm clothing. It was fortunate that padded ski-parkas made of synthetics had become cheap imports from Asia. Jack Welch commuted to the observatory by flying his own light plane, and on a later visit Jack flew me there. But on this first visit I drove up with Carl Heiles. Carl, who had headed the radio astronomy group at Arecibo when I was there, was now back 'home' at Berkeley. En route to the observatory we stopped to purchase some essential accompaniments to Carl's observing stints: popping-corn and Jack Daniels! And that night I was introduced to the art of popping corn, in a saucepan on the stove; a saucepan that proved far too small as the corn grains expanded enormously and cascaded over the top.

Carl specialised in studying the 21 cm 'line' radiation emitted by the hydrogen gas that lies in the space between the stars. At this time he was studying the *Zeeman splitting* of this line. When atomic hydrogen is immersed in a magnetic field the 'line' that it emits at a frequency of 1,420 Mhz, i.e. at a wavelength of 21 cm, is split into two components. The frequency separation between the two components is proportional to the strength of the magnetic field, so measuring this frequency difference provides a means of measuring the strength of the magnetic field in the interstellar gas. In practice the frequency difference is less than the frequency width of the components, which would seem to make the measurement a bit tricky! The saving grace is that the two components are circularly polarized, one right-handed, and the other left-handed. So Carl's project consisted of measuring the frequency difference between the right- and left-hand circularly polarized components. While he was concerned with the circular polarization of line radiation, and our group at Radiophysics with the circular polarization of broad-band continuum radiation, the techniques for separating the circularly polarized components are the same. So as we clambered over the Hat Creek 70 foot Blau Knox dish we found much to discuss about our equipment and measurement techniques.

Carl Heiles wasn't the only face from Arecibo that I found at Berkeley. Don Backer was there, continuing the study of pulsars that he had begun as a student at Arecibo. During the year, when Linscott and Erkes made the amazing claim to have detected pulsed signals coming from M87, a galaxy beyond the Milky Way, Don arranged for us to use NASA's Goldstone Tracking Station in the Mojave Desert to check this claim. I forget what happened. Did we arrive at Goldstone to find that our planned observations had been displaced by higher-priority spacecraft observations? Certainly no publication resulted. I do recall having a long telephone conversation with Linscott on the east coast, a sign of the way communications were reducing the size of the earth. After I returned to Australia I collaborated with a group of radio astronomers from the University of Tasmania who brought specialised pulse-detecting equipment to Parkes to search for these pulses. We found none, and the story died a natural death.

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On the upland 'Plains of St Augustine' in New Mexico, the US National Radio Astronomy Observatory (NRAO) was at this time building the largest radio telescope in the world. The Very Large Array, or VLA, was to have 27 dishes, each 25 metres in diameter, and able to be moved along a 'Y' of rail tracks, with each arm of the 'Y' 21 kilometres long. You might have seen shots of this impressive array in the opening scenes of the movie '2010', the sequel to '2001, A Space Odyssey'. The synthesis arrays at Cambridge in England, and at Westerbork in the Netherlands, had made marvellous maps of radio sources, but those east-west arrays provided little north-south resolution when mapping objects near the celestial equator, such as the planets. The 'Y' design of the VLA overcame this limitation. Furthermore this was a far more ambitious array, of far greater extent and therefore with much finer resolution, with many more dishes able to be used at many locations along the rail tracks, and capable of operating at many different frequencies. Here was an array that could map the radio emission around Jupiter with sufficient north-south resolution to delineate that predicted narrow band of emission along the magnetic equator, and so test our deductions about the pitch angles of the electrons in the radiation belt.

In its completed form the VLA was to have so many dishes that it could produce a reasonable radio map of Jupiter in the 'snapshot mode', i.e. using it as though it were a filled aperture instrument, and without using earth-rotation synthesis. However the VLA was not completed until 1981, and when the NRAO invited applications to use the partially-completed instrument in 1979 only 17 of the 27 dishes were in operation. To grasp the opportunity to map Jupiter with this partially-complete instrument I needed to concoct an observing strategy that included earth-rotation synthesis. In other words, to (partially) fill the gaps between the dishes it would be necessary to combine observations made when Jupiter was at different points across the sky, when there would be different *projected* separations between the dishes.

As I explained in connection with the planned Cambridge observations, synthesis observations of Jupiter are complicated by fact that the planet rotates. Observations made at different times on the same day will map different faces of the planet. To synthesise a map of one face of Jupiter it is necessary to combine a series of observations made on different days when the planet is at different points in its daily progress across the sky, *but nonetheless has the same face towards the earth.* And each of these individual observations must be of short enough duration that rotation during the observation doesn't cause significant blurring. With the VLA, to ensure that any blurring was less than the synthesised beamwidth the individual observations had to be no longer than 20 minutes. Hence to synthesise an unblurred-map of one face of Jupiter required the combination of a series of 20 minute observations made at carefully selected times on different days. Programming such scattered observations was hardly likely to be popular with the operators of the new telescope. However the alternative, of making observations from rising to setting on selected days, and later splitting the observations into 20 minute intervals, and then combining those with the same face towards the earth so as to make 30 different maps of Jupiter, corresponding to every 12 degrees of planetary rotation, would have required 30 twelve-hour periods of the new instrument's time!

I submitted a proposal to map four different faces of Jupiter by making a series of 20 minute observations at appropriately selected times during the first two weeks of November 1979. While this posed a bit of a headache for telescope programming, the Time Allocation Committee was sufficiently intrigued by the project that they granted the time. There were 21 observing periods, each of 66 minutes duration. Each period contained two 20-minute observations of Jupiter centred on times when each of two faces of the planet 15 degrees apart in rotation were towards the earth, while the remainder of the 66 minutes was used for calibrations. The 21 observations were in two groups: 10 mapped one pair of faces 15 degrees apart in rotation, while the other 11 mapped another pair of faces, also separated by 15 degrees, but located 105 degrees further round the planet.

Glenn Berge, who had made the first radio maps of Jupiter, was still at Caltech and was persuaded away from his commitments to Caltech's millimetre-wave interferometer for long enough to join me in this project. Glenn is not a man to do things in haste, and, at times, his deeper consideration of matters kept us out of trouble. And not only did Glenn contribute scientifically, but when I spent time with him in Pasadena, processing the observations, Glenn entertained me as a guest in his house. As the third member of the team we had the good fortune to be joined by Carl Bignell. Carl had been a student when I was at Toronto, and was now at the VLA as the contact person to assist users of the new instrument. As you can imagine, Carl's intimate knowledge of this embryo instrument was invaluable for our rather unusual project.

Being a planet, Jupiter moves across the sky at a slightly different rate to the stars and galaxies. Fortunately the VLA software that tracks the position of a source during the observations had allowed for tracking the planets. Other corrections had to be applied after the event: corrections for the day-to-day change in the distance to, and hence the angular size, of Jupiter; corrections for the rotation of the plane of polarization of the radiation as it travelled through the earth's ionosphere; etc. And these corrections were in addition to the normal ones which apply the results of the various calibrations made during the observations. With a project of this nature, making the observations is merely the start. Many months of processing, first at the VLA itself, and later at Caltech and then at Radiophysics, were required before we were able to produce maps showing the distribution of the total intensity and linear polarization of the radio emission round Jupiter. Unfortunately we were not able to map the circular polarization as the calibration of the system was not yet sufficiently accurate. We were now well-and-truly into the computer age, so when we did produce maps it took just a few keystrokes to display the maps either as contours, as *grey-scale* radio-photographs with the brightness of the radio emission shown by the brightness in the picture, or in *false-colour*, where different colours indicated different intensities of radio emission.

When my exchange with Berkeley finished at the end of 1979 Gina flew home and tells stories of successive New Year's Eve celebrations on the way as the plane crossed the international date line. The story has always seemed rather odd to me as I thought the date line worked the other way round. Still it is a good story. Meanwhile I drove down to Pasadena to spend some weeks at Caltech working on the Jupiter results with Glenn Berge. For the sake of old memories I drove across the Sierras and down via the Owens Valley Radio Observatory - not that Big Al, Rachel, or any other faces from the past were still around. It turned out to be a trying drive on icy roads in a rented car that was not supplied with chains, and after spending the whole day concentrating to avoid slipping, I arrived at the observatory with great relief, looking forward to a restful dinner. I was devastated to learn that the plan was to drive in to Big Pine for dinner. A restaurant in Big Pine was something new since my Caltech days, and to contemplate unnecessary driving in that weather seemed nuts.

It turned out to be a good dinner, with interesting conversation. Steve Gull and John Skilling from Cambridge were at the Owens Valley searching for 'holes' in the 4 degree background radiation. It had been predicted that this all-pervading, cosmic microwave background, believed to be a remnant of the Big Bang, would be slightly less-bright in the areas around clusters of galaxies. Steve and John were searching for evidence of this. At this time they were also promoting the *maximum entropy method* for improving images made from incomplete data. This maximum entropy method, or MEM, had been introduced to the radio astronomy community by Jon Ables from Radiophysics. Steve and

John, believing MEM to be far superior to the popular 'CLEAN' method for improving imperfect images, were promoting it. They handed me their business card: they had sold their software for the process to the Cambridge police to help them decipher imperfect photographs of car number plates!

Oh yes, the icy road did prove a problem. After dinner, as we drove back to the observatory, we executed a 180 degree skid, but I must say that Steve handled it perfectly.

The raw maps produced from synthesis observations are called 'dirty' maps. They are the maps that would have been recorded by a single antenna with the same overall size as the synthesis array (the overall extent determines the width of the beam), but an antenna having a rather irregularly-shaped main beam surrounded by various *sidelobes*. For our observations with the incomplete VLA the responses in these sidelobes were as much as 25% of the peak response in the main beam. So our dirty maps weren't pretty. However they did show numerous features. The radio emission from Jupiter's atmosphere, the so-called 'thermal' emission, appeared in the maps as a fuzzy disk coincident with the visible disk. That is, it appeared in the maps of total intensity: being unpolarized this component did not appear in the maps of linearly-polarized intensity. The polarzized (synchrotron) emission was seen to come from a region extending out to four times the width of the visible disk in the equatorial direction, and to about half that distance in the polar direction. Running across the disk, more-or-less along the equator, there was a narrow band of emission joining two bright peaks just off the edges of the disk at either side. And at either side of the disk bright crescents of emission passed through these bright peaks and extended up towards the poles.

The narrow band across the disk, with the bright peaks at its two ends, is evidently the edge-on view of a thin 'doughnut' of emission. It is synchrotron emission from a group of electrons that cross the magnetic equator in flatter helices and mirror before they travel far from the magnetic equator. Because we see the doughnut edge-on the radiation that we receive from either end of the doughnut comes from quite a long path through the doughnut; hence the bright peaks. By contrast, across the disk radiation is received from only the short path through one 'wall' of the doughnut; emission from the other side of the doughnut is blocked by the planet.

For one of our set of maps Jupiter's magnetic axis was almost in the plane of the sky. In those maps the band across the disk is almost straight along a diameter of the disk. For the other pair of maps the magnetic pole in the northern hemisphere was tipped towards the earth and in those maps the doughnut is tipped below the equator in the middle of the disk, as you can see in the picture (###). The asymmetry of the emission in these maps is also obvious in the picture. Because of the non-dipolar form of the magnetic field the doughnut is twisted: to the right of the planet this twist aims the highly-beamed synchrotron emission south of the earth so that we receive a weaker signal.

The crescents at either side of the disk represent the emission from electrons that cross the equator at steeper angles and travel a long way from the equator before mirroring. Again the brightest parts of this emission occur where we receive radiation from the greatest depth through the shell, i.e. when we look through the edges of the shell just off the planetary disk.

By the time of our observations it was common practice to use the 'CLEAN' process to make dirty maps look prettier and, as I've mentioned, the Maximum Entropy Process was also coming into use. Using these processes necessarily involves guessing some of the unmeasured Fourier components. However if the processes are used conservatively they don't add significant components corresponding to spacings larger than the overall extent of the antenna system. In other words the 'prettified' maps have no more resolution than the 'dirty' maps; in fact they often have somewhat less resolution. We used 'CLEAN' to make better-looking maps of the Jupiter radio emission, and also tried out the Maximum Entropy Method. However when we came to compare our observations with theoretical predictions we made the comparison with the 'dirty' maps, rather than using the ones with the guessed components. We convolved the theoretical models with the 'dirty' beam and then compared the results with our dirty maps.

After leaving Berkeley and spending a few weeks working on the data with Glenn Berge at Caltech, I returned to CSIRO and began computing synchrotron models to compare with our maps. To reproduce the asymmetries in the maps the models needed to include the effects of the non-dipolar character of Jupiter's magnetic field. However I hadn't discovered how to compute the motions of electrons in a non-dipolar field. I had to be satisfied with using a revised form of Jim Clarke's code for calculating symmetric, dipolar models, and then add a bit of handwaving about the asymmetries. For one of our sets of observations the maps were reasonably symmetric - the main asymmetry was behind the planet - so it seemed that a dipolar model might provide a reasonable test for this case.

In making the models I started by assuming that the pitch angles of the electrons crossing the magnetic equator followed the law that we had found fitted the measurements of the beaming, and the linear and circular polarization of the *total* emission from the planet. Initially I assumed that the pitch-angle distribution was the same at all distances from the planet, and that the number density of electrons varied with distance from the planet in the way that had been deduced from the Pioneer space-probe observations. The resulting maps came close to fitting the observations. They didn't quite fit the shape of the crescents at either side of the disk, but by having the distribution of pitch angles change somewhat with distance from the planet, having more electrons in flatter helices nearer the planet, I achieved a very good fit.

By the time that our results appeared in print in 1984 the VLA had been completed and Imke de Pater and collaborators had used it to make maps of the radio emission from

Jupiter at a number of wavelengths. With twenty seven dishes available they were able to use the 'snapshot mode', and by then the calibration of the VLA was sufficiently accurate that they could include circular polarization in the mapping. When it came to matching models to their observations de Pater (Journal of Geophysical Research Volume 86, page 3397, 1981) adopted a much more ambitious approach than ours. We had just attempted to find a distribution of electrons which would produce the observed radio emission. De Pater modelled the acceleration (by inward diffusion) of electrons captured from the solar wind, taking account of pitch-angle scattering by whistlers, and absorption both by the inner satellite Amalthea, and by the small particles that orbit Jupiter in the Ring. And she took account of the non-dipolar character of the magnetic field. In her model the electrons outside the orbit of Amalthea have a near-isotropic distribution of pitch angles. In diffusing past Amalthea less of the flat helix electrons are lost by collisions and this produces the steep pitch angle distribution closer to the planet that is required to fit the observations. In this way she seemed to be able to resolve some of the conflict between deductions from the spacecraft and radio observations. However I gather that she was unable to resolve the disagreement concerning the energy distribution of the electrons.

Living off Immoral Earnings

The Parkes Radio Telescope was designed to have a life of ten years. As I write, it has just celebrated forty years of service! Surely a testimony to the fundamental soundness of its design which has allowed many improvements to be made over the years . However by its tenth birthday the telescope was no longer *the* outstanding radio telescope in the world, and CSIRO radio astronomers had begun dreaming of a new instrument. Some dreamed of a smaller, but more precise dish, to study radio waves of much shorter wavelength: millimetre waves. For some the dream was of a southern 'synthesis' telescope, with an extent of kilometres; a series of dishes that could operate together to 'synthesise' the images that would have been recorded by a dish kilometres across. Money was the trouble. The 'string-and-sealing-wax' days of radio astronomy were past. Now it was 'big science', needing big money, and there was no Taffy Bowen, no charismatic character with contacts in high places, to secure the necessary funds.

The breakthrough came when the proposed new telescope was designated a 1988 Bicentennial Project with the title 'The Australia Telescope'. The Australia Telescope has three elements: the 'Compact Array', which is an earth-rotation synthesis instrument consisting of six dishes, each 22 m in diameter, on 6 km of railway tracks at Culgoora, near Narrabri in northern NSW; a seventh similar dish instrumented for millimetre waves, and located at Mopra, near Coonabarabran; and the Parkes Radio Telescope. The Compact Array, the Mopra dish, and the Parkes dish can each be used alone, but they can also be used in combination, and in combination with other radio telescopes around Australia, in other countries, or in space, for Very Long Baseline Interferometry.

The Australia Telescope was a compromise, adopted by the astronomical community after extensive discussions. There simply was not the money for a 'world beating' instrument and the Compact Array is a smaller version of a type of instrument pioneered overseas; in fact quite a modest version compared to the VLA in the United States. As a result, I'm afraid the Australia Telescope proposal did not fire my imagination. It didn't give me that sense of pride that had come from being associated with the Laboratory that had led the world with those earlier instrumental developments that were either completely new in concept, such as the solar spectrograph, the Mills Cross, the Chris-Cross, and the radio heliograph, or the largest in the world, like the Parkes Radio Telescope. And from a personal point of view the new telescope would not be capable of tackling the problems of particular interest to me.

In practice the Australia Telescope has been extremely successful. As a National Facility, constructed and operated under the auspices of CSIRO, it is used by astronomers from around Australia and around the world. In many ways it is the radio-equivalent of the Anglo-Australian optical telescope. Neither were in themselves new concepts, but both were state-of-the-art examples of instruments of their type, incorporating novel techniques, and both have proved extremely productive workhorses for the study of the southern skies.

My lack of enthusiasm for the Australia Telescope project puzzled Dick Manchester, who was by then the leader of the radio astronomy group. Since this was the instrument we would be using in the future, he expected we would all be enthusiastic to be involved in perfecting the design of the new instrument. However, apart from my general disappointment with the concept, I realised that I would not be at Radiophysics for long after the telescope came into service. Even if it met its schedule for completion in 1988 (which, of course, it didn't), I would then have less than four years to my 65th birthday and retirement. Dick did persuade me to chair the group that considered the design of the Long Baseline Interferometry system, a project that raised some interesting problems. Max Komesaroff was much more enthusiastic about the new instrument. He argued strongly that the design of the compact array should ensure that it could measure very small degrees of circular polarization. Strangely, as he demonstrated, this requires that the basic polarizations accepted by the aerials are 'crossed linears', rather than the conventionally chosen 'opposite circulars'. Max won the day, but the telescope had been in operation for nearly ten years before this facility to measure extremely small degrees of circular polarization was put to the test and came up trumps.

Ever since I had commenced work at CSIRO I had contributed to the Commonwealth Government superannuation scheme at the higher rate, which allowed for retirement at age 60. The decision to contribute at that rate must have been made at the recommendation of my parents: in my younger days I never imagined that I would lose my passion for science and consider early retirement. However, in 1986, when CSIRO announced a scheme of 'voluntary early retirement', meant as an inducement to older, less productive, staff to leave, so as to allow new staff with the fire and imagination of youth to be employed, well things looked different. I seemed to have become a one-man band trying to work alone on the theory of interstellar scintillations, and I wasn't going to be productive in isolation. I needed the interaction, the discussion, the criticism of my ideas, that come from collaborative work. When Max Komesaroff announced his intention to apply for early retirement that settled the matter. With Max gone I would feel very isolated.

So in July 1987, a few months after my 60th birthday, I left CSIRO with what seemed a generous retirement package - even if the present-day retirement packages are rather more generous. Under the scheme, in addition to my superannuation pension, I received a lump-sum of money from the superannuation fund, and a second lump-sum based on my years of service. The lump-sum from the super fund could be used to purchase another annuity from the fund, but the fund itself advised that if I took the money, I could buy a more favourable annuity from an insurance company. So I took the money.

I had grown up to think that living off investments was, well, almost immoral. You should *work* for your living. And I had assumed that in retirement I would live on my government pension. That, I presumed, was financed from taxes. In those days it probably was. Now I found myself a capitalist, and proposing to live off immoral earnings! Since I knew nothing of investing I consulted numerous 'advisers'. If I had taken the advice offered by any of them (except for the Commonwealth Bank adviser who said I should invest in their debentures) then I would be considerably worse off financially today. These 'advisers' were simply incompetent; they could not even think in percentages, they had to use examples. They had a recipe: invest one third in shares, one third in property and one third in cash. Perhaps as a long term plan that is sensible, but when the stock market was at a record high, and interest rates were also extremely high, it was clearly stupid to invest in shares. Keep your money in cash, benefit from the high interest rates, and when the share prices drop, buy shares. Of course, for me, that is wisdom in hindsight. It was just my innate conservatism that protected me from the stock market crash that came a few months after my retirement. I had insisted that the money go into capital guaranteed products.

Those financial advisers did not like annuities, particularly whole of life annuities with no return of capital at death. They liked playing with money - preferably someone else's money - and they didn't like the idea of losing control of money by giving it to an insurance company. I didn't have a home computer, but I laboured with a hand-calculator to compute the return that would be needed from some other investment to match the returns offered by the annuities. When I found that to match the annuity payments being offered by the largest insurance companies you would need a 14% annual return on your investment, guaranteed for the whole of your life expectancy, I didn't hesitate. I suppose you should hesitate if you come from a family with a short life expectancy, or if you judge that the life expectancy of the insurance company might be less than your own. That latter possibility seems much more likely these days since the companies have 'demutualised'. Now serving their stockholders, rather than their policy holders, their investment policies are far less conservative. As a footnote: Some years after my retirement the government began using tax-advantages to entice people to buy annuities. Then the advisers began recommending annuities, and the insurance companies supplied the advisers with computer programs that demonstrated the effective interest rates earned by these investments, programs that made the calculations that I had made, but in a fraction of a second, not over several weeks.

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Because my retirement came suddenly it was a shock. A gradual tapering-off of work would have been far preferable. I immediately missed the company, the daily interaction with workmates. And I missed, and still miss, the fund of knowledge that was available from colleagues. Nowadays when I wonder about some technical question there is no one to ask. At the Radiophysics Laboratory I had only to raise the question at the morning-tea table and someone was sure provide the answer. And I can no longer talk with Max. Max, who had so looked forward to his retirement, was diagnosed with cancer only a month or two after retiring, and he died within six months. One more of my friends and colleagues who suffered the aftermath of smoking, although Max, like all my work colleagues, had stopped smoking as soon as the dangers were publicised.

To my great surprise I did not miss science, and I have not been an avid follower of the progress of astronomy since I retired. In retirement I have come to appreciate just how slow is our progress in the understanding of the big questions. When you are 'in the game' every little fragment of new knowledge seems important. But to the outsider it is only the big steps in qualitative understanding that seem important, and they don't come often, or easily. I do occasionally wonder what people believe nowadays about some of the questions that concerned us. What are the current beliefs about the acceleration and radiation processes involved in solar radio bursts? How different are the present day models of Jupiter's radiation belts from the ones that we proposed? What are the current beliefs about interstellar scintillations? I should summon up the energy to visit the Radiophysics Laboratory and search out these matters in the literature.

Since I retired I have spent a day or so a week as a volunteer with various groups. As a general comment I would say society is making very poor use of the talents of retirees. In most organisations the volunteers are given the unskilled tasks that the regulars find boring. Furthermore, at least in my experience, those organisations that recruit volunteers for other organisations, attempting to find openings that use the skills of the volunteers, are woefully unsuccessful. The only examples I have seen where the capabilities of volunteers have been used to their full extent is where the volunteers have had sufficient self-confidence and assertiveness to essentially take over a project and run it.

My experience in such an esoteric subject as radio astronomy, particularly with a leaning towards the theoretical side, is perhaps not the best preparation for being useful in the general community. However I had expected that 'society' would have made more use of my knowledge and experience than has been the case in practice. Hands-on exhibits have always seemed to me a powerful means of teaching science, so I thought I might find a role in helping to design exhibits at the embryo Power House Museum. In reply to my letter to the Director I just received an invitation to become a volunteer and show people around the completed museum. Was the fact that I just accepted this rebuff another example of my failing to be pushy enough? For one semester I supervised a class of first year students performing experiments in the Physics Laboratory at the University of Technology Sydney. As the University liked to keep such jobs for their post-graduate students they felt unable to offer me a second semester. No doubt that policy helps post-graduate students gain experience, and earn a few pennies, but perhaps the students gained more from my supervision than they would have from that of a recent graduate.

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Photography had been a hobby of mine for many years, and in retirement I was able to devote more time to it. One of my photographic pastimes had been making abstract pictures by taking close-ups of tree barks. Now I extended this to photographing small sections of discarded, painted, rusting metal. I could be found haunting car junk-yards, or the edges of bushland, looking for examples of such rubbish. I took colour *slides* which have a brilliance not matched by colour *prints*. But a slide is viewed for a fleeting moment, and so, to have something I could view over a period of time, I made Cibachrome prints of about thirty of my slides. That gave me a better chance to judge the quality of my 'art' and at the time I thought some of pictures were rather good. I am less impressed by them now!

Then I saw an exhibition of pictures in which *still life* objects, such as beautifully shaped pots, were shown in unusual - unreal - colours, and I found these pictures beautiful. The *false colours* in these pictures brought to mind the false-colour images being used in science. Since the advent of computers false colour has been in common use as a means of displaying two-dimensional information, such as infra-red satellite images of the earth, or maps of the radio emission from astronomical objects, such as our VLA maps of Jupiter. In this technique, instead of making a contour map, or a black-and-white image in which the degree of blackness represents the intensity of the infra-red or radio emission, colours are used, with a palette of colours being chosen to represent the range of intensities from the weakest to the strongest. I thought that if I had a computer I might be able to produce images similar to the pictures that I had seen in this exhibition. I imagined starting with black and white photographs featuring interesting shading, for example suitably lit vases, and replacing the black and white tones with a suitably chosen palette of colours.

Taking advice from the computer specialists at Radiophysics I bought one of the 486 series computers, which were reasonably new at that time. As I intended to photograph the screen of the monitor I chose a relatively expensive monitor. I didn't have to splurge on a scanner as I could arrange the occasional use of someone else's. Shortly before I bought the hardware the project took a side turn when I saw an advertisement for some quite cheap image-manipulating software that featured *posterized* and *solarized* colour images. I knew the difficulty of making such images in the darkroom, so I purchased these programs. Now, instead of my writing a program to convert monochrome images to colour as had been my intention, I scanned *colour* photographs into the computer and proceeded to experiment with all the devices available in these programs, negating one or several colours, solarizing, swapping colour planes, posterizing at various levels, sharpening, adding textures of various sorts. There were endless variations. For example, with solarization the level at which the display changed from positive to negative could be set anywhere between 0 and 256, separately for each of the three colours! The marvellous thing was the speed of these experiments compared with darkroom experimentation. In a few seconds I could try something, decide the result was unattractive, throw it away and start again. Compare that with the hours needed in a 'wet' darkroom, and with the cost of colour materials.

I amused myself over many months. Most of the results weren't particularly attractive, but just occasionally by chance I found something I liked and photographed it. The number of pixels that could be displayed on the monitor was limited, so my images weren't as sharp as a normal 35 mm film image, but were acceptable. I did attempt to use these programs to undertake the original proposal of converting a black-and-white image to false colour. The technique was to first convert the black-and-white image to a three colour representation, and then adjust the response curves of each of the three colours separately. Not a very satisfactory technique, and I did not produce anything of great beauty.

It is now three or four years since I did anything with this project and in the interim technology has advanced by leaps and bounds. Commonplace computers now run at ten times the speed of my computer, they feature loads of memory, and come equipped with high quality colour printers. Should I become enthusiastic again I will need to update the computer.