NRAO ONLINE 3 Schedvin Draft CSIRO Volume- 1983

C. Boris Schedvin, History of CSIRO, draft 1986- 1987: Summary of Chapter "The Promising Skyline, Inauguration of Radio Astronomy, The Inception of Cloud Physics"

Epigraph:

> The scientific community is censorious of those who venture prematurely, and Bowen's kamikaze style [with cloud physics theories related to meteor streams] helped cast a shadow over the rainmaking programme as a whole.
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> ….In radio astronomy, the intellectual leadership was carried by Joe Pawsey, Bernard Mills, Paul Wild, John Bolton and others. Bowen's scientific judgments were sometimes found wanting, and many of his public pronouncements about the rainmaking experiments were unhelpful. Nevertheless, he provided much of the synergy behind the remarkable period of success enjoyed by the Radiophysics Laboratory, and as such he has made a distinctive contribution to Australian science and culture. [1]

In February 2010, Rob Birtles (CSIRO Records Advisor - Collections Archives) sent Goss an unpublished manuscript by C Boris Schedvin, Professor in the Faculty of Economics and Commerce, Department of Economic History at the University of Melbourne. This manuscript was a continuation of the first volume of the history of CSIR/CSIRO, published in 1987, *Shaping Science and Industry: A History of Australia's Council for Scientific and Industrial Research, 1926-1949*, published in 1987.[2] This comprehensive history of the CSIR was to be followed by a second volume covering the new CSIRO from 1949 to about 1983.[3]

[1] Schedvin, Unpublished Vol2, CSIRO, 1949/1986-1987.
[2] The authors have utilised the 1987 volume (CSIR history) in the preparation of the present book.
[3] Schedvin told Sullivan on 1 October 1986 that the draft chapters were completed three years earlier. (Sullivan archive)

Schedvin wrote Birtles on 8 July 2002 with a complex narrative explaining the delays and subsequent abandonment of vol 2. He had moved to Melbourne from Canberra in 1979 to accept a professorial position at the University of Melbourne.

> I think the reasons for the non-appearance of the second volume may be summarized as follows.
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> Incidentally, although more than 100,000 words was written for volume 2, it was only about half completed, and therein lies part of the explanation.
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> 1.The book was conceived as a single volume. It was only at a late stage that it became evident and the single volume approach would not work. The underlying issue was the vastly different sizes of the pre-war CSIR and the post-1949 CSIRO. It became virtually impossible to write the CSlRO story in the same way and in the same detail as *Shaping Science and Industry.* An entire fresh structural approach was required for CSIRO. Yet the scientific detail was still required. Neither the Organization nor I came to terms with this dilemma. The unpublished chapters you have seen were written on the CSIR model. For volume 2 they would have required significant rewriting[4] (except for the overview chapter.)
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> 2. For the scale of the project it was always under-resourced, and I had great difficulty finding suitable research assistants. The problem of time and resources became acute when I moved to the University of Melbourne in 1979 to accept a professorial position.
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> 3. The project was commissioned when Sir Robert Price was chairman (CSIRO 1970-1977). Over time those with an interest in the project moved on, and eventually there was no one left to provide support. It should be said that CSIRO was given a tough time after the Hawke government was elected [1983] and there was less money available for exotic (and some would say) unnecessary projects. Also, because the resource problems, the complexity of the project and my own lack of time, the project was taking much too long.

[4] For the radio astronomy text, this concern is confirmed. A number of radio astronomical and engineering missteps have been identified. Clearly, if Schedvin had published this second volume, he would have had this text checked by scientific colleagues before publication.

> 4. Around 1997 or 1998 I raised completion with the regime of Malcolm Macintosh [CEO of CSIRO 1996-2000]. I was advised that the Organization was interested in a new style of book, as I interpreted it along the lines of *Surprise and Enterprise* [a glossy popular anthology produced in 1976 at the 50^{th} anniversary of CSIR/CSIRO]. I was not interested in this sort of project. I was told subsequently that this was a device to "kill it off". As so much water had flown under the bridge, I don't think that this was an unreasonable position for CSIRO to take.

Schedvin pointed out to Birtles that he would give the CSIRO archive permission to share the unpublished chapters with interested users with appropriate acknowledgements; he assumed that he also was the holder of the copyright for this new text.

The draft chapters are: (1) "Modern Talisman", overview, (2) "Expanding Horizons", new endeavours such as the Division of Meteorological Physics, (3) "The Promising Skyline", radio astronomy and cloud physics at the Division of Radiophysics under the leadership of Bowen, (4)" Myxomatosis", (5) "Restoring the Golden Fleece" and (6) "Wildlife Research". Below we summarise Schedvin's view of the development and impact of the RPL on radio astronomy and cloud physics.[5] In addition, NRAO ONLINE 25 contains a description of Pawsey's interactions with Bowen, especially Pawsey's growing disaffection with the cloud seeding experiments in the 1950s. This text also contains a summary of Rod Home's comprehensive summary of the RPL cloud physics research, written in 2005, well after the official end of rainmaking in the CSIRO circa 1980: "Rainmaking in CSIRO, The Science and Politics of Climate Modification" in the anthology *A Change in the Weather, Climate and Culture in Australia* edited by Sherratt, Griffiths and Robin.

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[5] In his volume 2, Schedvin also treated the post war RPL endeavours in DME (Distance Measuring Equipment for air navigation) and an electronic project. Neither became a major factor in the post -war RPL programme.

Summary of Radio Astronomy "Inauguration of Radio Astronomy"

Based on his experience with the CSIR, Schedvin was familiar with the major players at RPL as radio astronomy began in October 1945 at Collaroy (Pawsey, , Payne-Scott, McCready at RPL and from the Commonwealth Solar Observatory Allen, Woolley and Martyn). In addition, he recognised the impact that Rivett, White and even David Martyn had on the RPL as major new discoveries were made in the period 1945 to 1960. Schedvin's insights of1986 still provide essential understanding of these events in 2020.

Schedvin:

> [During the War] Australians had made important contributions to radiophysics, but the strength of the group lay in application and engineering…..In a number of fields CSIRO modified its perceived role as an applied scientific organization, but nowhere more clearly than in radio astronomy. The laboratory was driven by disciplinary imperatives, and much of its history centres on the need for larger and more complex instruments to increase resolving power so that the most distant [radio] sources of radiation could be identified and understood.

A key component in the success of RPL in 1945 was the combined leadership of Bowen and Pawsey. Pawsey's role is emphasised in the main book. Schedvin wrote:

> Early in 1946 E.G. (Taffy) Bowen had been approached to succeed John Briton as chief of the division of Radiophysics. The approach was not unexpected. Even though he had been in Australia for only two years and the division was rich in scientific talent. Bowen had been identified by White as chief-designate shortly after his arrival in 1944. He soon became deputy chief, and in 1945 was invited to prepare a comprehensive post-war programme for the division. Bowen approached the task with characteristic enthusiasm and imagination. The broad philosophy was enunciated clearly and forcefully. [Bowen wrote in KE 8/2, "Future Programme of the Division of Radiophysics, 2 July 1945"]:
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>> When determining the policy of a laboratory it is important to

> consider carefully the distribution of effort as between fundamental and applied work. Fundamental studies are Primarily intended to make original contributions in knowledge, but they may also produce new techniques which can be applied directly to the activities of mankind. In the same way the solution of an applied problem can lead to valuable contributions to our knowledge, will advance the techniques available for fundamental work, and suggest new lines for pure research. The danger of concentrating entirely on applied problems is well known and need not be enlarged upon. It is not generally realized that a similar danger exists from concentrating on pure research, and for this reason it is important to maintain a correct balance between them.

[Schedvin continued:] At the fundamental end of the spectrum Bowen proposed that the division undertake a survey of the “cosmic' noise” that had been identified by Jansky before the war and was thought to come from galactic “stars” or interstellar space. In a formal sense this was the beginning of the remarkably successful radio astronomy programme, although at this early stage it was not intended to achieve more than improved understanding of the various sources of radio interference.

Within a short period, the novice radio astronomy programme had elicited the strong support of the CSIRO hierarchy. The Chairman, David Rivett, was looking for expansion into fundamental science. On 5 September 1947, Rivett wrote Bowen:[6]

What you have told me about the [radio astronomical] work of Pawsey and Bolton is not only of interest to me but also gives a good deal of comfort, for I feel that, to a great extent, the future of CSIR depends on the ability

[6] KE 8/2, Part II 5 September 1947

> and willingness of our best men to get down to fundamental problems and not be content merely with technical applications.

Schedvin provided a succinct but thorough description of the evolution of solar work at RP starting in 1945 under Pawsey's leadership. "Almost everyone in the laboratory was sharing in some new discovery. Information was shared freely and a strong sense of collegiality developed under Pawsey's guiding influence. The atmosphere was far removed from the intense rivalry that developed in the 1950s over the construction of much larger instruments [the GRT or Parkes telescope]."

Schedvin provides a more detailed description of the major role played by Paul Wild starting in 1949 in his pioneering work identifying the Type I, II and III bursts at Penrith. The evolution of the successful evolution of the work at Dapto (1950s and 1960s) is described. The culmination of Paul Wild's contributions with the radio heliograph at Culgoora (staring in 1967) concludes the story of the dominant role played by the solar radio astronomers at RPL. (See NRAO ONLINE 20 and 23). Schedvin 's concluding text on the solar work at RPL summarises the lasting impact of Paul Wild and his colleagues:

> Building on the foundations laid by optical astronomy, Wild's research was an essential bridge towards a more comprehensive understanding of the Sun. It was a pure instance of science for its own sake. In the tradition of the Radiophysics Laboratory its hallmark was technical virtuosity: the design and construction of instruments, often ingeniously improvised and of increasing sophistication, to meet the challenge of unravelling the complex nature of the radio Universe.

Schedvin then described in an extensive text the evolution of radio astronomy outside the sun: "Galaxy and Beyond". The text deals with (1) the work of Bolton and colleagues at Dover Heights (up to 1955), (2) Mills and the controversies as his 3.5 m wavelength survey at Fleurs was found to have major disagreements with the Ryle et al 2C and 3C surveys (see Chapters 35 and 36) and (3) the construction of the GRT, the Parkes radio telescope. (chapters 27, 29-32). Schedvin's summary contains some insightful comparisons of Bowen and Pawsey.

The Dover Heights summary unfortunately contains a number of misconceptions as Schedvin has apperently followed Bolton's texts with an uncritical eye (eg Bolton 1982 "Radio Astronomy at Dover Heights", *Proc Astronomical Soc of Australia*, vol 4, 1982, p. 349). Appropriately, Schedvin emphasises the remarkable success of the Cosmic Noise Expedition of 1948 on both the east and west coasts of New Zealand. Bolton and Gordon Stanley succeeded in providing accurate celestial coordinates for the radio sources Taurus A, Virgo A and Centaurus A which lead to the significant publication of 1949 by Bolton, Stanley and Slee "Positions of Three Discrete Sources of Galactic Radio-Frequency Radiation", *Nature* vol 164, p 101, July 1949.

Details of the Mills/Ryle controversy are treated in the main book in Chapters 35 and 36. Schedvin was not aware of the success of the Scheuer P of D analysis of the 2C survey, leading to conclusive evidence of evolution in the source counts even with a survey compromised by confusion.

The Schedvin text concludes the radio astronomy discussion with a sub-section "The Parkes Telescope". (See Chapter 27). The challenges raising the funds for this massive project are discussed, including the key roles played by the two US foundations, the Carnegie Corporation of New York[7] and the Rockefeller Foundation. The total cost of order A$ 2 million was unprecedented in 1961 when the telescope was opened.

Schedvin also discussed the fascinating issue of attempts in the 1954 to justify the building of the GRT based on practical spin-offs. Schedvin wrote:

> Special funding would also make much more visible the commitment to a programme which was not expected to have any utilitarian value. How could an applied scientific organization justify a heavy commitment to the pursuit of knowledge for its own sake? Was this not the province of the universities? Casey, as Minister-in-Charge, was keen to have a list of

[7] Schedvin has mistakenly listed the Carnegie Institution of Washington

> possible tangible benefits even though the telescope had his wholehearted personal support. White prepared such a list more in duty than conviction. Long distance radio communication might be improved, warning of magnetic and ionospheric storms might be given, the quality of radio receivers might be enhanced, and so on. In the event, the list of possible spin-offs was not needed: the telescope was accepted purely on scientific grounds.
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> The view of scientists, of course, was that distinguished intellectual achievements must be supported, and that the pursuit of basic knowledge for its own sake must bring rewards to society. The scientific community was in one of its most confident moods.

Schedvin also quoted the correspondence between Fred White and Vannevar Bush, President of the Carnegie Institution of Washington and friend of Bowen. (also see NRAO ONLINE 40). On 5 October 1954, White wrote Bush with an invitation to visit Australia, looking for support to build the GRT[8]:

> I think a visit by you would go a long way to helping us get the additional funds [from the Australian government] we need for the large radio telescope which so many of us wish to build.
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> You will appreciate that although exceptionally outstanding work has been done by Australian sciences in one or two fields, including that of radio astronomy, this is still a small scientific community. The number of people here who appreciate fully the significance of what Radiophysics has done, both in the field of radio astronomy and that of cloud physics is very limited. One consequence is that the responsibility of convincing others falls on one or two of us only who are outside the Division of Radiophysics itself. In such circumstances it is a very great help indeed if somebody from

[8] NAA C3830 A1/3/11/3 Part 2, also Bush's replies to White

> overseas such as yourself can talk to those in responsible positions. In the Government about such a project as this.
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> This work in the Division of Radiophysics is a direct consequence of our radar work during the war. Since I left the Laboratory and Bowen took charge, I have done my best to encourage Bowen and his colleagues to get into one or two quite fundamental lines in physics, in spite of the fact that CSIRO generally like to foresee some fairly reasonable application of its work. It is difficult to point to any even reasonably long term fundamental work as this must, in the long run, be of practical significance to a country such as Australia.

White hoped that Bush would help a number of bodies in Australia of the desirability of the GRT: the CSIRO Advisory Committee, the Minister for the CSIRO, Casey and especially the Australian Treasury ("a difficult matter indeed when we tackle the Treasury"). At this point White still hoped for substantial private funds from the Australian public.

On 13 October 1954, Bush replied, responding to White's invitation to visit Australia:

> I realize.. what the general problem is in Australia, and I have met similar problems in industry… on a vey more minor scale. Some of us know that it is impossible to carry on applied research in a thoroughly effective manner with the presence of basic and fundamental research with it or alongside it. But I do not think that this is generally appreciated and it is not a simple thing to expound. When one has lived closely in scientific circles for many years it is quite apparent that one of the great motivating urges of scientific men is to increase the understanding of the race and its grasp of nature, and we also know that in any group this philosophical approach should be present if it to be a scientific group of the highest order. Moreover, the urge to add in a creative way to the general understanding is very strong

> among individuals, and if there is not an outlet for this urge those individuals who feel impelled most strongly thus to contribute to the grasp of their fellow men will go elsewhere. A scientific community, then, if it is to be really effective even in applying science for practical ends, needs to have in its midst those who are reaching far ahead in their thinking and who are building the foundation for the applied work of a later generation, or even building a foundation so that man may better grasp his position in the cosmos and reason more effectively about matters of the spirit.
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> Now I know that in Australia there is just the same approach to this matter among scientific men generally as there is everywhere in the free world. The question is whether those who control the evolution of science in your country understand the nuances sufficiently well to be sure that there is an outlet and an opportunity for men of every type, so that in fact the scientific community will feel that it is fulfilling its full mission.[9]

Bush declined White's invitation to visit Australia; he was not convinced that a series of lectures would have major impact on convincing the Australians to support fundamental research. White thanked Bush on 25 October 1954: "I appreciate your pertinent and kindly remarks about our situation here. There are fortunately a few senior men who see the need to support basic research quite clearly and I am assured of their assistance in promoting the activities of our Division of Radiophysics."

Schedvin continued this theme in concluding remarks on the radio astronomy activities up to the mid-1980s:

> In a symbolic sense radio astronomy had become CSIRO's scientific flagship. The support for radio astronomy has been criticized because of its lack of direct economic and social significance. In an Organization expressly designed to foster the linkage of science and industry, it has been argued

[9] White to Bush and Bush to White, NAA A1/3/11/3 Part 2

> that expenditure on activities such as radio astronomy has been a waste of scarce resources. In an ideal world the activity might have been more appropriate in a university, although there is also a strong case for its location in a national institute because of the high cost of the facilities which are used internationally.

Schedvin pointed out that RPL was one of the largest divisions of CSIRO. In 1949-1950, the RPL budget represented about 7 per cent of the total budget of CSIRO. However, by 1969-1970 this fraction had fallen to 4 per cent (radio astronomy was about one half of the total RPL budget).

Schedvin ended the summary of the impact of radio astronomy with a mixed assessment, criticism followed by praise:

> (1) The success of the [RPL] group encouraged over-confidence in the economic benefits to be derived from progress in physical science. Although radio astronomy itself was not expected to have a utilitarian spin-off (except in solar-terrestrial interactions), the view was held long after the evidence indicated otherwise that advance in basic physical understanding would benefit Australian industry by some osmotic process. While the radio astronomers were building international scientific reputations, the domestic electronics industry languished under competition from technologically more sophisticated European and Japanese imports. In other words, hard thinking about suitable structures for the transfer of knowledge (including the development of knowledge in a usable form) was neglected.
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> 2. In any organization, however utilitarian in orientation, some individuals and groups must be allowed to climb mountains, and radiophysics offered one outstanding opportunity.

Schedvin provided a summary of the construction and opening of the GRT on 31 October 1961. The schism of RPL with the departures of Mills and Christiansen to the University of Sydney and later Kerr to the University of Maryland in the US had a major impact:

> The pursuit of scientific distinction by a large proportion of a group makes it difficult for a single laboratory to satisfy all reasonable claims for equipment and advancement. The risk of instability will be increased if equipment costs are high and if the field of science in question is expanding, creating opportunities elsewhere.

As described in the main book, the two new groups at the University of Sydney (Mills at the School of Physics, leading later to the Molonglo Radio Telescope near Canberra and Christiansen taking over the former RPL field station at Fleurs as it became the Fleurs Synthesis Telescope) flourished , producing new science and many new active participants in science and engineering. The return of John Bolton in early 1961 to RPL was a key event in the continued flourishing of new science enabled by the versatile GRT (chapter 31). Parkes catalogues of the southern sky were made at 75cm, then 20 cm, and even 11 cm. An arc sec position of the first high redshift quasar (3C 273, chapter 32) was determined by a visitor to the University of Sydney, Cyril Hazar at Parkes during three lunar occultations in 1962 (May, August and October), published in *Nature* in March 1963 (Hazard, Mackey and Shimmons, *Nature* vol 197 p 1037) followed by the redshift determined by Maarten Schmidt with the 200 inch telescope at Mount Palomar (*Nature*, vol 197 p 1040). [10]

Within a few years, radio detections of quasars at Parkes was a major result: at 75 cm the fraction of the radio sources associated with quasars was 15 per cent and at 11 cm 40 per cent. Numerous redshifts were determined using the Lick Observatory and Mount Palomar in California and Mt Stromlo. In chapter 32, we describe the early years of observations at Parkes.

Cloud physics and rainmaking- 1947 to 1980

In NRAO ONLINE 25, we have discussed in detail the evolution of the complex story of RPL's work on cloud physics and rainmaking, including an epilogue that provides an overview of the period 1947 to 1980. Here we summarise Schedvin's detailed texts of 1983. As we discuss in NRAO ONLINE 25, Rod Home's text of 2005 "Rainmaking in CSIRO- The Science and Politics of Climate Modification" has the advantage of insights gained by observing the impact of the RPL cloud physics efforts from a vantage point two decades later.

In this brief summary, we emphasis a few points made by Schedvin in his 1983 draft text.

Bowen was anxious to balance the basic research of the division (radio astronomy) with an "applied problem of major potential significance and also because of the opportunity of using radar to study the process of rain-formation". The pitfalls of the projects were the same as experienced by rain-making

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[10] Hazard, Jauncey, Goss and Herald (2018, *Publ Astronom Soc Australia* , vol 35 p 6 " The Sequence of Events that led to the 1963 Publications in "*Nature*" of 3C 273, the first quasar and the first Extragalactic Radio Jet" provide a description of the complex saga of 3C 273 in 1962-1963. Schedvin's assertion that 3C 48 was the first quasar ("The first quasar [3C 48 was discovered before the giant Parkes telescope was commissioned…") is incorrect. 3C 48 was recognised after the redshift of 3C 273 (0.158) was discovered by Schmidt. Greenstein and Matthews presented their redshift for 3C 48 (0.367 in the same issue of *Nature*, vol 197, p 1041).

endeavours all over the world. Examples were “caution in predicting the outcome has been followed by periods of over-optimism and premature claims of success”. The basic problem was understanding “the complexity of the interacting systems” in the atmosphere. Statistical tests involving cloud seeding over target areas compared to nearby control areas (no seeding) were often controversial.

An example of a controversial exercise occurred in 1955 in a concerted attempt to test the efficacy of silver iodine as a cloud seeding agent (silver iodine had a crystal structure similar to ice crystals). The project was a four year experiment on a randomized basis sponsored by the Snowy Mountains Hydro-Electric Authority (SMHEA). From the beginning CSRIO and SMHEA disagreed about the results. Bowen claimed a 20 per cent increase in rain. He was quoted: “There is no shadow of doubt that we can make large quantities of rain from certain types of cloud.” SMHEA and their advisors did not agree. The commissioner of SMHEA was uncertain of the magnitude of any possible increase in participation. Even the Prime Minister of Australia, Menzies, had doubts.

A following large scale experiment in Tasmania in 1964-1970 was planned carefully and produced results that were more acceptable to the scientists. “This encouraging result suggested that cloud seeding [autumn -winter] might be an economic proposition in the special conditions of the Tasmanian highlands….Tasmania offered unusually favourable conditions for seeding, and the state was heavily dependent on hydro-electric power.”

The future of the cloud physics group at RPL was on the line in 1970-1971. White, the original driving forces behind CSIR’s push into atmospheric physics in 1945, had retired as Chairman of CSIRO in 1970 after 11 years. Bowen retired in 1971; the new chief Paul Wild separated cloud physics and radio astronomy. A Division of Cloud Physics was created in 1972 with Jack Warner as Chief. This division was part of the Environmental Physics Research Laboratories under C.H.B. Priestley as chairman. A make or break trial was organised in order to clarify the economical impact of cloud seeding in a major agricultural area, Wimmera region of western Victoria. The carefully planned trials occurred in August to Nov of 1975 to 1978; Very few days offered ideal conditions with minimal seeding. Schedvin concluded: “This dramatic failure brough CSIRO cloud seeding to an abrupt end. The result highlighted the dangers of predicting the number of seeding opportunities from synoptic weather information….. Once again, the intrinsic difficulty of the cloud seeding enterprise was underlined.

The CSIRO announced in late October 1981 that seeding would be discontinued…. (Schedvin, 1983, section "Western Victorian Experiment)

Schedvin concluded with a discouraging summary:

> It would be inappropriate, however, to regard the history of rainmaking experimentation as a total failure. To be sure, the economic benefits have been negligible against a substantial cost in scientific manpower and materials. In the late 1940s the challenge was irresistible, and the CSIRO group was an early leader in the design and conduct of experiments. Along the way a great deal of information and expertise was accumulated. With hindsight it is clear that the degree of difficulty was underestimated, particularly by Bowen who repeatedly raised false expectations. Bowen should have accepted….that the group did not possess an adequate understanding of the complex underling mechanisms. These deficiencies were compounded by Bowen's persistence with unorthodox and unaccepted statistical procedures. Considerable interest remains, however, in the potential of cloud seeding in special circumstances [such as in Tasmania]…..We know beyond doubt, however that the grand vision of the 1950s, of large-scale weather modification is not within reach….

Conclusion – Schedvin's Assessment of Bowen and Pawsey

Schedvin explicitly addressed the issue of conflicts and complementarity between Bowen and Pawsey during the era 1945 to 1960:

In many respects Pawsey was the intellectual lifeblood of the laboratory. He possessed exceptional analytical ability to distil a physical problem to its essentials, and a special capacity to devise ingenious technical solutions to practical problems, a combination of qualities which encouraged the style of the Cavendish Laboratory at Sydney. ….[H]is quiet scepticism and intellectual power won deep respect within the laboratory. While Bowen and Pawsey often disagreed and there was a certain amount of tension between them, the two men were complementary. Bowen was a leader of unbridled enthusiasm and entrepreneurial flair which often led him to attempt more than could be achieved with available resources, a tendency counterbalanced by Pawsey's caution and scientific judgment. Bowen was usually inclined to build up an excess of steam; Pawsey kept the laboratory on the rails scientifically. The loss of Pawsey and the others [Mills and Christiansen and later Kerr] meant that a major rebuilding programme was necessary at the moment when the giant telescope was available for mapping the southern sky [in 1961].

…..To conclude with a brief comment on the man who led the Radiophysics Laboratory for such a long period, there is no doubt that Taffy Bowen was one or the great entrepreneurial chiefs in the history of CSIRO. In his twenty-five years at the helm, he enjoyed the implicit trust of the Executive. Whether selecting the design of radio telescopes, speculating on the effect of meteor dust on rainfall, estimating the efficiency of silver iodide, or predicting rainfall[11], he was rarely out of the scientific limelight. Many of his pronouncements attracted fierce controversy, and some of his peers judged him harshly; but his energy, self-confidence and engineering

[11] See NRAO ONLINE 25, Additional Note 2. "Bowen's Meteoritic Dust and Rainfall Imbroglio of 1953". Home (R.W. Home, "Rainmaking in CSIRO: The Science and Politics of Climate Modification," pp. 66, 2005, in *A Change in the Weather: Climate and Culture in Australia*, eds. T. Sherratt *et al*. (Canberra: National Museum of Australia Press) wrote: "While this controversy was not directly linked with the rainmaking work of Bowen's Division, it surely had an undermining effect on the general credibility of the rainmaking program with which Bowen was so closely identified." Bowen continued to promote the hypothesis for some years.

instinct were undiminished. In radio astronomy the intellectual leadership was carried by Joe Pawsey, Mills, Paul Wild, John Bolton and others. Bowen's scientific judgments were sometimes found wanting, and many of his public pronouncements about the rainmaking experiments were unhelpful. Nevertheless, he provided much of the energy behind the remarkable period of success enjoyed by the Radiophysics Laboratory, and as such he has made a distinctive contribution to Australian science and culture.