**NRAO ONLINE 25 Pawsey and Cloud Physics 1947-1948**

Epigraph[1]:

> Under the direction of the noted radio astronomer, Dr E.G. Bowen, the CSIRO organized what could be called the world's most outstanding scientific group dealing with cloud physics and weather modification. It is probably correct to say that no other organization contributed more to practical cloud physics during the period approximately from 1950 to 1965.

[However, after numerous Australian experiments in 1957-1959, the final results of Bowen and colleagues remained disappointingly inconclusive.] The conclusion was that there was no evidence that cloud seeding influenced the mean precipitation.

**Pawsey and Cloud Physics 1947-48[2]**

During the 1947-48 trip to North American and Europe, J.L. Pawsey visited a number of cloud physics groups, especially those working on artificial rain production. He was also involved in detailed correspondence with Bowen concerning the cloud physics at RPL. For example, in a letter of 24 December 1947 to Pawsey[3], Bowen expressed frustration with the Radiophysics rainmaking group of Eric Kraus, Pat Squires and E.J. "Pat" Smith.

> Dear Joe: I continue to be worried about the rainmakers. All my attempts to help them leads [sic] to an interminable argument. I had given them to the end of the year to do something useful …

[1] H.E. Byers in "History of Weather Modification" 1974, *Weather Climate Modification,* ed by W.N. Hess, p 23-24. Also see *Sullivan, W. T., III. (2009). Cosmic Noise: A History of Early Radio Astronomy. Cambridge University Press, Cambridge, UK,* p 123.

[2] An excellent study of the RP rainmakers has been provided by 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). An additional description of RP cloud-seeding efforts has been provided by Helen Sim in an unpublished MSc thesis at the University of New South Wales (1995): *The Rise and Fall of the Rainmakers: A History of the CSIRO Cloud Seeding Experiments 1947-1981.* Reproduced by permission in NRAO ONLINE 50.

[3] NAA C3830 F1/4/PAW/1 Part 1.

> [This group was stationed at Richmond RAAF airfield. Bowen was satisfied with the performance of Squires and Smith. Bowen's frustration was more focused on Kraus. Maybe we should leave] Kraus as advisor … or whatever he likes to call himself but with no responsibility for experimental work.

The most significant visit that Pawsey made was a short visit to Langmuir and Schaefer at General Electric in Schenectady, New York, in December 1947; this group had pioneered cloud seeding starting in November 1946.[4] Pawsey described some of his visits in a report sent to Sydney from Washington, D.C. on 13 January 1948, "Informal Notes on 'Rain Making' in the US". He wrote: "I enclose [confidential] notes I have been writing on the general subject of rainmaking". The seven-page report consisted of a number of details based on conversations with Langmuir and Schaefer, with additional reports of visits to the US Weather Bureau (likely in Washington, DC), MIT in Boston and the University of Southern California in Los Angeles. "I am not attempting to give technical details, but to indicate the general perspective which I derived from these experiences having in mind the relation to work in Australia."

Pawsey's assessment of 14 June 1948[5] shows his approach to this completely new science:

> I wish to say at the outset that I see here a new and very fruitful branch of science which we are entering at the relatively early stage if we take our opportunity now. There are, I am sure, outstanding opportunities for success for those who pursue this subject with skill and perseverance. Naturally, there are also opportunities for failure for the not-so-skilful who, in this new and uncharted science, attempt blind alleys and do not turn back in time. This science differs from the well-established ones in that success should come to those who display intelligence; there is, as yet, no well-established, exclusive technique, which must be learned through years of study.
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> Nor is there a numerous and brilliant body of competitors. The outstanding workers … are Schaefer and Langmuir. Each, in own way, is an inspiration, but two men do not monopolise a science. More men and more facilities will be available [in the US] than in Australia, but that should be regarded as a challenge to us to devise simpler and better

[4] The CSIRO initiative with Langmuir and Schaefer had been made the year before (July 1946) when Eric Kraus (1912-2003) was on his way to Australia from the UK to join RP (See Additional Note 1: "Eric B Kraus ") from the Obituary of Kraus in the Bulletin of the American Meteorological Society November 2004: "Kraus met with Shaefer, who told him of the recent cloud-seeding breakthrough [at GE, in the laboratory]. After arriving in Australia, Kraus convinced CSIRO to try dry-ice seeding in real clouds. Plans got underway to seed several cumulus clouds from Royal Australian Air Force planes [5 February 1947, initial flight], and use radar to observe whether rain formed inside them." The RPL group of Kraus and Squires submitted a paper to Nature, published 12 April 1947, vol 159, p.489, "Experiments on the Stimulation of Clouds to Produce Rain".

[5] NAA C4659/4

experiments. It is not necessarily a handicap. The Cavendish-Rutherford tradition of "sealing wax and string" and brains [his emphasis] is a greater one than that of Berkeley [the University of California, Lawrence's group]. Langmuir and Schaefer work [sic] is in the former [Cavendish] tradition and do it magnificently ...

[A level of danger could be involved.] Dry ice was dropped 300 miles to sea [in a hurricane off Florida in October 1947]. A day later the hurricane changed its course and ravaged Florida. A thoroughly uncomfortable time was had by all ... No information whatever on whether the change of course was mixed up with the dry ice experiment as some previous hurricanes have done the same and others have not …

The theory presented by Langmuir is of outstanding importance to practical meteorologists. It provides a "heads I win, tails you lose" research opportunity. If the ideas are correct, then we have a practical method of rain stimulation in clouds additional to that depending on the provision of ice nuclei. If incorrect, then the refutation of Langmuir's ideas will provide a first class, practical investigation. It is a first class opportunity to get in at the beginning of an important investigation …

[In conclusion], if we look at the developments of the past year, two conclusions appear: … (1) the amazing extent to which dry ice seeding of clouds has given positive results, though often divergent and unexpected, (2) The lamentable failure of the experimenters to take and record [his emphasis]accurate observations … Kraus and Squire's note in *Nature*, condensed to a length suitable to that journal, is still the only scientific publication known to me on the subject of cumulus cloud seeding. I do not understand why this should be so. [Pawsey then suggested that empirical cloud seeding should be done again with accompanying measurements in the clouds; all of this would be associated with theoretical and laboratory work.]

My interpretation of the situation is that the opportunity to exploit [cloud seeding experiments] which was obvious [in February 1947] still remains. The policy should be to carry out such observations as are practicable with available equipment, and to use available facilities to obtain careful observations. [Pawsey's emphasis] Then, in parallel with this, we should try to improve the techniques and develop theory. This science of meteorology is a complex one in which we cannot control all the factors …

The objective at the outset should be a well written, detailed account of such a series of observations and its analysis. This is of fundamental importance to the science, even if no new discoveries are made.

On 16 January 1948 (footnote 3), Pawsey became directly involved with the problems of the "rainmakers". He wrote Bowen with a report of his letters to Pat Squires and the newcomer E.J.

Smith, with advice on how to break the log-jam of low productivity. Essentially there had been no publications since the *Nature* paper of Kraus and Squires, from 12 April 1947. To Pat Squires, Pawsey had written: "I had been embarrassed by having to answer the question 'what has been done since the *Nature* work?' … That we should consider whether it was to [your] advantage to continue a partnership with Kraus, after the last barren year." Pawsey continued with a tongue-in-cheek mood: "Maybe I have rushed in where angels fear to tread!" Then seriously, he continued: "One thing is essential- choice of simple experiments and then perseverance and meticulously careful observations." Relations with Kraus deteriorated. By 24 February 1948, Bowen wrote Pawsey. He appreciated Pawsey's letter to Smith and Squires, both were encouraged by the advice: "I [Bowen] am coming to the conclusion that we cannot support Kraus much longer and I have already told him (as he often says) he has attractive positions offered him, he had better accept. If he doesn't take this hint I may have to be more firm."

In May 1948 Bowen had a slight sense of optimism about the activities of the "rainmakers". On 5 May, he reported to Pawsey that: "I have taken the drastic step of doing some of the work myself and, as a result there has been almost as much flying [and thus cloud seeding] during April [1948] as there was during January, February and March." He described the types of clouds in which dry ice was dropped: "An empirical result which is beginning to emerge from the work of the past few months is that there is a critical temperature between -6 C and -8 C above which the dry ice process doesn't work very well … the Squires-Smith combination can yet lead the world in rainmaking."

Kraus was not mentioned. On 8 June 1948[6], Pawsey responded to Bowen. He was pleased to see that the efficiency had improved as Bowen took a "hands on" approach. "[Your observation] indicating that rain will not form in clouds of higher temperature than -6 C seems to be forming the basis for a very nice investigation …"

On 18 May 1948, Bowen continued his response to Pawsey's efforts in providing advice concerning the rainmakers' problems. "You will have gathered by now that I have given Kraus up as hopeless. His approach to everything is entirely emotional and he is as unstable as ever." Kraus left RPL in 1949 under pressure from Bowen[7], remaining in Australia until about 1961, working first for the British Commonwealth Pacific Airlines as an Operations Research Officer and then from 1950 to 1961 for the Australian Snowy Mountain Authority. Details of his later career are provided in the Additional Note 1: Eric B Kraus.

[6] NAA C3830 F1/4/PAW/1 Part 2

[7] Bowen wrote Kraus on 22 April 1949 with the report that the CSIRO Executive would allow him a three month extension from end March to end June to "wind up your affairs" at RPL. His resignation was to take place from 30 June 1949. (CSIRO, ATNF archive). He had been at RP for almost three years.

**Epilogue: Pawsey's Perception of Cloud Physics in the 1950s. Did this Play a Role in the Bowen-Pawsey Disaffection of 1960?**

Discussions between Pawsey and Bowen in 1947-1948 presaged a growing conflict in the 1950s: Pawsey's increasing scepticism about the foundations, execution and efficiency of cloud seeding. In 1947-1948, Pawsey played a role, visiting centres of research in the US and providing guidance in the affairs back in Sydney. During the course of the 1950s (Chapters 36-41 and NRAO ONLINE 38-47) as the GRT planning developed, we have detected a growing disaffection between Bowen and Pawsey, leading eventually to the break of 1960-1961 (Chapter 48). We suspect that Pawsey's doubts about the cloud physics programme, especially the cloud-seeding aspects, played a role. For example, during the long overseas trip of 1957-1958 (Chapter 37), there were almost no discussions of cloud physics topics, in contrast to the trip of a decade earlier. As we have seen in Chapter 48, the main factor in the break of 1960 were the different philosophies of Bowen and Pawsey concerning the scientific direction of RPL and in particular to the exclusive emphasis on the GRT programme. However, it is likely that the evolution of the cloud physics group was a secondary factor.

Bowen did not receive the same level of international scientific recognition achieved by Pawsey. For example, Pawsey was elected to fellowship in the Royal Society of London (Chapter 32) in 1954, while Bowen was elected 21 years later in 1975, four years after his retirement from CSIRO. Arthur Higgs told Woody Sullivan in Sydney in 1978 that Bowen had a long term resentment after Pawsey's election to Fellowship of the Royal Society. .

On the other hand, as Schedvin has pointed out (Schedvin, 1986-1987, unpublished history of CSRIO, draft edition from CSIRO archives, "The Promising Skylines", history of the Division of Radiophysics since 1949), Bowen "was one of the great **entrepreneurial chiefs** (our emphasis) in the history of CSIRO". (See NRAO ONLINE 3**)**

Clearly, the scientific and entrepreneurial leadership combination was a major factor in creating the world wide, renowned position of the RPL. Both leaders were essential; but by 1960 the contradictions were too extreme, leading to Pawsey's move in early 1962 to Green Bank (Chapter 50).

Home (2005, see reference to R. Home's article "Rainmaking in CSIRO: the Science and Politics of Climate Modification", footnote 2) has made a convincing case in comparing the friction between "applied" and "fundamental" research at RPL, that is cloud physics and radio astronomy. On p 72 of "Rainmaking in CSIRO, the Science and Politics of Climate Modification" Home wrote:

> …The net result was that from the mid-1950s virtually all the projects that had once been identified as constituting the "applied" side of the research program in

Radiophysics had been more or less abandoned [in the post-war era such as aircraft navigational aids]. More and more, radio astronomy and cloud physics became the only fields of research being pursued. Both continued to make rapid advances, and both continued to enjoy very strong support from the CSIRO Executive, especially from Fred White who became the organisation's chief executive officer when it was restructured in 1949 and later its long-serving chairman. The radio astronomy group, in particular, was one of the top two or three groups in the world in what had become a fast-moving and glamorous new field of scientific inquiry. Given, however, that the radio astronomers' work continued to be viewed as "fundamental"[8], the Division's overall research profile could only meet CSIRO's general guidelines for balancing fundamental and applied research if the cloud physics work was redefined as "applied", although it had initially been classified unambiguously as fundamental research. This was now plausible to do -- but only so long as the rainmaking side of the work, rather than the long-term investigations of cloud dynamics that had initially been envisaged, was seen as paramount. In other words, in addition to the sheer excitement engendered by the prospect of making rain when it was needed, there were clear institutional and political reasons for rainmaking to be the principal focus of the cloud physics group's research.

The point is worth emphasising, because - responding, no doubt, to the eventual collapse of the rainmaking program - histories of the cloud physics project have tended to lay heavy emphasis on the group's achievements in fundamental research, and to portray the rainmaking work as a mere appendage to this. Indeed, according to the most recent account [written in 2005], the scientists quickly became disillusioned with the rainmaking work and persisted with it only because the powers that be insisted they do so. To be sure, the growing number of calls for assistance from drought-affected communities around Australia was a distraction for the group since they saw their role as doing the underlying research that was necessary, not becoming service providers. Moreover, they were not yet confident about their techniques and in any case did not have the resources to respond adequately to appeals for help. But the fact is that Bowen, enthusiastically supported by White, even though he was fully aware of the political necessities that constrained research in CSIRO, deliberately wound down other areas of potential "applied" research and left the Division with just its two research programs in radio astronomy and cloud physics. His doing this testifies to the confidence

[8] On a number of occasions in the 1950s, Bowen and White did make the point in interviews that solar radio astronomy had a practical application due to solar storm outbursts on the behavior of the ionosphere, i.e. solar-terrestrial behavior. From the view point of 2020, there is some suggestion of a "fig-leaf" aspect to this claim.

> he and White had in the viability of the "applied" side of the cloud physics research - that is, the rainmaking - and to their primary commitment to this aspect of the work.

In Additional Note, we describe an unfortunate event of 1953 as Bowen proposed an implausible connection between meteoritic dust and rainfall. By 1954, Bowen complained to Fred White about Pawsey's opposition to this theory, as shown in Additional Note 2: "… Joe lined up against me ..." This exchange is a likely example of an event that may have signaled a growing animosity between Bowen and Pawsey.

The end of cloud seeding occurred in stages, starting in 1970-1971 as White and then Bowen retired. The new chief of RPL, Paul Wild, moved the cloud physics project out of the Division of Radiophysics. A decade later, "the rainmaking side of the work was officially abandoned, for both scientific and economic reasons." (Home, 2005, p. 76).

Home (2005, p. 77 and 79) summarised the impact of the cloud-seeding programme over the three plus decade era:

> The fact that [cloud seeding] was pursued so tenaciously is testimony to Bowen's and White's continuing belief in the ability of the program to deliver long-term benefits to the nation. The early work had provided a secure underpinning for all subsequent research by demonstrating beyond all doubt that clouds could be induced to yield rain. What remained to be done was to work out how to induce them to do so in ways that changed the existing, unsatisfactory patterns of rainfall across the country.
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> Perhaps one could argue that the rainmaking group was unlucky in having such extraordinary success with its first seeding experiments, in that this gave rise to unrealistic expectations as to what could be achieved. They were certainly unlucky in that one of the major conclusions that subsequently emerged from the research they did was that not all clouds occurring over continental Australia were potential sources of rain and that, in fact, only a small proportion were.

In conclusion, the coupling of CSIRO cloud physics and radio astronomy began with the transition of radar research from 1939-1945 to the post-war transition to non- military research. For a period of a few decades, the "applied" nature of rainmaking served Pawsey and his group, providing a "fig leaf" for "fundamental" research in radio astronomy.

**Additional Note 1: Eric B. Kraus (1912-2003)**

Details of the fascinating life of Eric Kraus are provided in two thorough Biographical Memoirs: H.P. Hanson in *Meteorologishe Zeitschrift*, vol 13, no.4, p 345, 2004 and P.J. Lamb and J.M. Prospero in *Bulletin of the American Meteorological Society*, Nov. 2004, p. 1783. An interview with Chester Norton was carried out on 26 October 1987 and 6 November 1987 in the series "University Corporation for Atmospheric Research -UCAR, American Meteorological Society". Finally in the Niels Bohr Library and Archives, American Institute of Physics, Kraus has deposited an autobiographical sketch of nine pages (MB 2014-1583).

Eric Kraus (1912-2003) was born in the Sudentenland of Czechoslovakia. Before the war he started a PhD in Bergen, Norway, working with the famous meteorologists Jacob and Vilhelm Bjerknes. He was in the Royal Air Force in WWII (awarded a Royal Air Force Cross) and moved to Australia in 1946 after being recruited by Bowen to join RPL.

In the Lamb and Prospero memoir, details of Kraus's recruitment and research in Australia in the 1946-1949 are summarised:

> … Eric accepted a three-year contract with the Commonwealth Scientific and Industrial Research Organization (CSIRO) in Australia. Prior to moving to Australia, Eric embarked on a preparatory Australian-funded tour of leading meteorological research institutions in Norway and the United States. While in the United States, he visited Irving Langmuir and Vincent Schaefer in Schenectady, New York, where they were carrying out their early research on dry-ice cloud seeding. After his arrival in Australia, he immediately began field experiments on cloud modification to enhance rainfall. This collaborative work with Pat Squires drew on Eric's familiarity with Tor Bergeron's theory of rain formation from subfreezing clouds and from Eric's discussions with Schaefer and Langmuir. Initial results on a few clouds were spectacularly successful. This success was reported in *Nature* [1947) and widely publicised in the news media. Hopes were raised among CSIRO executives and Australian politicians that cloud seeding would transform large areas of arid Australia into a land of milk and honey. However, it soon became obvious that seeding with dry ice did not work unless natural rain was imminent from the targeted clouds. Eric's public expression of these reservations in *Penguin Science News (1947)* and elsewhere had a negative effect on efforts by CSIRO executives to increase the funding for cloud seeding. That situation, in turn, produced increasingly acrimonious relations with CSIRO administrators. As a result, Kraus's three-year contract with CSIRO was not renewed. Thus, not only was Eric among the first to experiment with

> weather modification, he also was one of the first to be caught in the conflict between the scientific process and the political agenda that often is associated with this controversial issue. Other scientists later would become similarly entrapped.

In his autobiography, Kraus provided details of his disagreement with Bowen.

> [After the first successful February 1947 cloud seeding], Squires and I could not repeat their initial success in subsequent seeding experiments. Gradually it became obvious the seeding with dry ice did not work, unless natural rain was already close. The hopes and expectations to produce water for Australia's parched interior were pipe dreams. My public expressions of these reservations [in the press] ... interfered with efforts by the CSIRO Executive to get more money for cloud seeding. Relations with the Chief [Bowen] became increasingly acrimonious and when the contract with CSIRO ended [in 1949], it was not renewed.

In the interview with Chester Newton (University Corporation for Atmospheric Research, 28 October 1987), Kraus simply stated: "I resigned from CSIRO because of that quarrel with Bowen."

After 1961 his career was in the US. He was at Woods Hole Oceanographic Institution until 1966. He then moved to the University of Miami where remained until 1981 as Director of the Institute of Meteorology and later the Rosenstiel School of Marine and Atmospheric Science. In 1977, he established the Miami-NOAA Cooperative Institute for Marine and Atmospheric Studies. Kraus moved to Boulder Colorado from 1981 to 1987 at the NOAA Cooperative Institute for Research in Environmental Sciences (CIRES). He died in Ashland Oregon in 2003.

As was the case for Pawsey, Eric Kraus was a close friend and colleague of Walter O. Roberts (1915-1990), the founding director of the High Altitude Observatory, and President of UCAR in Boulder from 1960 to 1973.

**Additional Note 2. Bowen's Meteoritic Dust and Rainfall Imbroglio of 1953**

Byers (see Epigraph -- reference described in footnote 1))

> An interesting side issue arising from the Australian research which set the world community of atmospheric and planetary scientists astir was Bowen's hypothesis [published by Bowen in *Australian J Physics*, 1953, vol 6, p 490, "The Influence of Meteoritic Dust on Rainfall"] that meteoritic dust served to nucleate clouds and stimulate rain at periods related to meteor showers … It was concluded that meteoritic dust particles are active in the atmosphere as ice nuclei … Against the onslaughts of meteorologists, planetary physicists and statisticians, the Bowen hypothesis did not stand up well. Little is heard of the idea today, although it was a startling theme when it was introduced, and it occupied much time and energy in efforts to substantiate or disprove it.

Even Bowen's archenemy, D.F. Martyn, (See NRAO ONLINE 7) attacked Bowen the following year in the same journal (Vol 7, p 358). Martyn was succinct: "... Bowen had produced no real evidence of the existence of the worldwide phenomenon he describes; that the various rainfall peaks he selects are due to one or two local cyclones; that it is possible to "establish" peaks on any calendar date by following his methods. Even Bernard Lovell wrote Bowen a letter, suggesting that his hypothesis was "extremely improbable". (Home, 2005, p. 75).

Schedvin (1986-1987, unpublished history of CSRIO, draft edition from CSIRO archives, "The Promising Skylines", history of the Division of Radiophysics since 1949) has used graphical language to provide commentary of the lasting damage done by Bowen's unsubstantiated meteorite dust hypothesis:

> The point of the story is not that the hypothesis failed. The progress of science depends on the courageous development of testable hypotheses, and most will fail. But the scientific community is censorious of those who venture prematurely, and Bowen's **kamikaze style** [our emphasis] helped to cast a shadow over the rainmaking programme as a whole.

By 1954, the controversy between Bowen and many colleagues concerning the role of meteorite dust continued. In a letter from Bowen to White[9] from 18 August 1954 (from the ASLO office in Washington, DC) mainly concerning the opposition to his theory, Bowen complained to White about his critics:

[9] CSIRO archive, G23/20.

Coming now to Cornish [Allan W Cornish -1910-1995-a prominent Australian meteorologist], I am afraid he is making a bit of a fool of himself. I forgot he is on the Academy [Australian Academy of Science] and as they have already sat in solemn judgement and decided I am a charlatan and probably fiddled the data anyhow, he has to defend the situation … I don't know what Cornish's findings will be but I guarantee he'll sit on the fence. He will not be able to show the results have no significance. But he certainly won't have the guts to say they are. With D.F. [his arch enemy David Martyn], Priestley, Swinbank [10], **Joe** [Pawsey, our emphasis] lined up against me, it looks as if I'm going to have fun.

[10]C,H.B.Priestley (1915-1998, Chief of the Division of Meteorological Physics for 26 years) and W.C. Swinbank (1913-1973), prominent meteorologist and collaborator of Priestley.