Reminiscences of 30 years of meteorological research in Australia

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Scope and introduction

In responding to the Editor’s invitation to write about research over the last 30 years ‘from my own perspective’ I have finally chosen, apart from a slight displacement in time, to take him literally. My first inclination was toward a broader perspective, but it soon became clear that this was impossible in the time and space allotted.

Thus regretfully I make no more than passing reference to a great deal of fine research with which I have had contact but in no way helped to originate, hoping that much will find its place in future articles by others. I think here of Bowen’s entry into cloud physics, the initial drive to get good physical measurements in and around clouds: of the ensuing work, experimental and interpretational, of Bigg, Mossop, Squires, Twomey and Warner, establishing the anatomy and physiology of clouds as a prerequisite for sound surgery: of Radok and Budd who brought more physics into Antarctic meteorology and glaciology: of Jensen’s pioneering numerical approach to the synoptics of the Australian region: of Denmead’s blend of micrometeorology with ecology: of Schwertfeger’s micro and mesoscale studies in Adelaide and rural South Australia: and of Smith’s elucidation of the dynamics of rotational phenomena. Research in the Bureau of Meteorology will presumably be covered in the companion article by Dr Gibbs.

Even when narrowed to Aspendale and ANMRC, I still found much too much for the space-time allowance. Forced then to contract in depth as well as breadth, I have not in general been concerned to describe details and findings of the research. These are already in the literature. I have made exception in two cases where we were fortunate enough to discover laws of quite general applicability. To say that such are rare in meteorology is not intended as a boast, for these like many discoveries were largely a consequence of time and circumstance, the laws themselves being simple enough. What is not in the literature and has been a main concern to paint here is the evolutionary picture, the background of and reasons for research undertaking with their consequences foreseen or unforeseen. By and large I have confined myself to the period 1946—1975, since others are writing about selected items thereafter. I have added a few anecdotes about my personal work and experience which illustrate the pleasures and the slings and arrows of a research career. For the autobiographical bias in what emerges, I offer the excuse that those not by nature historians can remember best what was closest to them.

Two sets of discussions in 1945, a hemisphere apart, were to determine my whole future. A committee had been founded by the Director of the UK Meteorological Office to advise on his research
program. What a list of names they were: Chapman (chairman), Dobson, Normand, Gold, Taylor, Brunt, Sutton. Swinbank and I were working on the roster for upper air analysis and forecasting in headquarters at Dunstable. We both had a pre-interest in boundary layer problems and we used to chew the scientific rag over cups of tea at leisure moments. We submitted a paper which challenged the established views on vertical transport as formulated by the last three names above. The Goliaths prepared for battle, the Davids' teeth chattered. But we were supported, quietly by Normand and Dobson and virulently by Gold, who relished an argument, took us aside to help select the best pebbles, and promised to present our paper to the Royal Society. Sad to say, there was one instance of lasting damage to personal relationships. The experience taught me much, that true scientists will always be contestants, that even the most eminent are fallible and must be ready to re-think, that in such a controversial science the give and take of criticism should extend from top to bottom without rancour.

Concurrently in Australia, Dr White of the Executive and Dr Bowen a Chief of CSIR, were advocating the need for more fundamental study of atmospheric processes. With agreement of Mr Warren, Director of the 'Bureau', (as the now Bureau of Meteorology will be referred to hereafter), a new Section was approved in CSIR and the position of Officer-in-Charge was advertised. Brunt was approached for suggestions and offered me his nomination. The challenge and prospects of greater freedom were attractive, and I had several family connections in Australia who were loud in its praise. But our independence of the Bureau would be setting an unprecedented pattern in government-financed meteorology, and I wondered whether the little flower could flourish near the great oak tree.

In the first half of 1946 I had the benefit of talking with three visitors to London: Sir David Rivett, who described the organisation and philosophy of 'his' CSIR, for whom and for which I was to develop a lifelong admiration as recorded elsewhere (Priestley 1973): Dr Bowen, who extolled the CSIR from the viewpoint of a Chief, and had started a study of radar echoes from clouds which required meteorological participation: and Mr Warren, who said he fully approved the venture and promised all possible support. Persuaded, my wife and I disembarked in Melbourne on Christmas Eve.

Early days

I was told to take two months or so to get the feel of CSIR, the Bureau, and the general scientific environment, and then prepare recommendations for a program. CSIR had formed a committee to advise on launching the venture and lead me to the starting gate. Dr Woolley, the Commonwealth Astronomer (chairman), White, Warren, Martin who was head, and Loewe a member of the Physics Department at Melbourne University. My role was cast as the study of basic processes of the atmosphere; suggestions as to what, where, and how were for me to make. Loewe alone could talk about the problems of Australian meteorology. He was one of the great scholars of the subject, and was always helpful in discussing ideas even when they lay away from his own special interests.

Early in 1947 I went for a few weeks visit to Bowen's Division of Radiophysics in Sydney. Pat Squires had been appointed to work on radar echoes. So had Eric Kraus, who had come from England via America where he had picked up Langmuir's idea of scattering dry ice into supercooled clouds to make rain. I was in time for their first demonstrably successful experiment, and won the draw for a seat in the plane for the next experiment, which was unsuccessful. Thus ended my experimental career. In the excited aftermath I came under heavy pressure to settle in Sydney and join the movement. However there was danger of becoming absorbed, possibly overwhelmed, in an area of meteorology in which I had no experience or special aptitude. Thus, perversely as some thought, the rainmaking success was a significant factor in my decision to set up in Melbourne, closer to institutions with wider meteorological interests.

The Executive approved the initial recruitment of four more scientists. At that time the science of meteorology was greatly undeveloped, thin in universities almost world-wide, and my choice of research areas depended on the availability of people of sufficient quality. The research would then be built around these people. Today, as in other mature sciences, the area is generally decided first and appointees chosen accordingly. Bill Swinbank, the most perceptive young meteorologist I had met, a stimulating if at times provocative thinker, had expressed interest in joining me. If we were to work in micrometeorology another former colleague, Len Deacon, also stood out. Two impressive young men from the Bureau approached me. Their skills would have brought us diversity in suitable directions, their Australian experience helping towards our quicker development and acceptance. But Warren refused to discuss the possibility, insisting that I had not come to Australia to lure away his staff. I felt in no position to challenge him. His attitude had taken me aback and was to impose on our interrelationships a different pattern from the one I had expected. Within two years both these men had deserted meteorology for other pastures. I was left to lament my diffidence and resolve never to repeat the mistake.

The consequent restrictions to our plans decided me to propose just one main theme initially, that of turbulence and micrometeorology. It was agreed that research on immediate problems of synoptic meteorology should be left to the Bureau, but that dynamical research with synoptic relevance should be pursued when ideas and opportunity offered. I
had some such ideas to follow up and we were also
to appoint Bob James, an Australian who had
worked on dynamic-synoptics at Dunstable and sub-
sequently in anomalous radio propagation in New
Zealand. In a decision delayed until the others had
arrived, the fourth position went to Reg Taylor, who
had worked with Pasquill in wartime
micrometeorology in Queensland.

The Woolley Committee endorsed the recommen-
ded program and then self-immolated. I was henc-
eforward responsible solely to the Executive,
wherein Dr White was to become my permanent
counsellor and supporter. Apart from the obligation
to give a talk on our work every five years or so to a
general CSIRO Advisory Committee, no other
committee bore on me throughout my twenty-seven
years as OIC and Chief: a sharp contrast to condi-
tions today. At no time do I recall being given for-
term forms of reference, but was left to get on with
the job, doing my best possible. However a good
image was ever vital, for Chiefs were in permanent
competition for their relative slices of the CSIRO
cake.

Advertising, selection, notice, family upheaval,
sea travel, etc., took another nine months during
which I was virtually alone. From a desk in a dis-
used warehouse in Flinders Lane there was
everything to order from pencils and paper, through
slide rules and the most modern (mechanical) hand
computers to such instruments and practical
facilities as I could be sure the others would need.

There was one disappointment in that I had thought
to bring to Australia something of value in the syn-
optic field. With the advantage of a uniquely close
and accurate wartime network of upper air stations
in the British Isles, we had developed the technique
of level-by-level build-up of isobaric contour charts
by means of the relatively conservative mean tem-
perature (thickness) patterns. This had put us ahead
of many countries and had demonstrably reduced
our analysis and forecast errors, enforcing better
continuity in both vertical and time dimension. But
the Bureau analysts argued that, with a more open
network, the treatment of each level on its own
merits was preferable. I did not agree, and it was
many years before the question was reopened by
Warren Wilhelm at the IAAC (page 25).

Late in 1948 we moved to join two other Sections
on the CSIRO site at Highett. I had purchased some
dissued internment huts put up for disposal at
Tatura, and had them converted into makeshift of-
fices, instrument laboratory, and workshop. One af-
fternoon in 1949 we were working quite happily,
when someone announced it was 105°F in the screen
and over 120 in the offices. Somehow our concentra-
tion was sapped. But the huts served us surprisingly
well for five years until, needing more outdoor
space, we bought part of the old training racetrack
in open country at Aspendale. We had not foreseen
how soon we would be enclosed by streets and
houses, nor our disfavour with the local Council

when on cold clear nights the radio broadcast an
Aspendale minimum a degree or two lower than any
other suburb. We were now nearer to the well-
exposed micrometeorological site at Edithvale,
where much work had already been done and our
primary objective, the first measurements of tur-
bulent vertical flux, had already been achieved.
Ground heat flux measurements developed by
Deacon could be very high and caused comment in
England, as a result of which the values came to be
referred to in the literature as representative of 'semi-desert near Melbourne'. I arranged for Geoff
Richards to photograph the site at maximum
greenness and took malicious joy in showing the
slide in a talk to the Royal Meteorological Society
in London, with a number of desert fauna (genus
cow) and the Carrum butter factory clearly in the
background.

The period at Highett had seen consolidation of our
central theme with some sidelines and applica-
tions, recruitment of assistant and workshop staff,
further liaison with cloud physics and with five or six
primary industry groups in CSIRO whom we in-
troduced to the relevance of micrometeorology, and
for whom we developed special instrumentation.
Recognition of our progress resulted in growth of
our graduate strength to support the original five,
and in plans for more diversification. Our debt is
recorded here to the late Roma Mair (nee Thomas),
a wartime trainee from the Bureau, who became our
enthusiastic and enterprising maid-of-all-work and
instigated the Aspendale purchase. On her marriage
we appointed our first Administrative Officer,
Frank Tighe, who was to give outstanding service to
the Division, as it was soon to become, and never-
ailing help to me over the rest of my years in
CSIRO.

Micrometeorology and radiation

Whereas over the previous twenty years much of the
emphasis in work on the turbulent boundary layer
had been given to the diffusion of clouds of material
in a wind, our object was to return to the more
fundamental geophysical exchanges between the
atmosphere and its underlying surface. Important
questions about the shearing stress had been raised
in Deacon's classical Geophysical Memoir (1953)
and about vertical heat transfer in the Priestley-
Swinbank paper (1947). The latter had suggested
that sensitive and quick response instruments might
now be developed to allow the vertical eddy trans-
ports, including that of water vapour, to be
directly measured. If so, their relationships to the
vertical profiles of temperature, humidity and wind
speed could be established definitively in place of
past conjecture. This, then, was our first specific
target, whose attainment would carry with it a
number of other side benefits.

Our experience on the forecasting bench had
convinced us that all three fluxes would, in course of
time, become a synoptic prerequisite for forecasting work. It is rewarding that in the full numerical approach these are now mandatory, and that the relationships determined in our field work were available by the time they became needed in this context. Recognising that the earth's surface is reasonably uniform over large areas, we sought sites where advection effects were minimal. In smaller-area contexts where advection is highly relevant, knowledge of the basic one-dimensional laws would still be a prerequisite. These studies and the fine-structure instrumentation in itself (page 29) should find a host of applications in field biology, primary industry, water conservation, indeed in any human pursuits where conditions near the ground and in the ground were of importance. To use a catch-phrase of recent years, 'environmental' problems were in our thinking from the start. In the event the variety has exceeded even our early expectations, significantly, due significantly to Deacon's flair for carrying his expertise into novel contexts.

It early became our policy to foster development of micrometeorological expertise elsewhere, especially where areas of specialisation application began to appear. Two such groups in CSIRO grew to status of high international reputation in their own right. Collaboration with one was to result in a handbook of applied micrometeorology published by UNESCO (Slater and McIlroy 1961). Another measure of interest was the Conference on Instrumentation for Plant Environment Measurements organised by David Angus at Aspendale in 1966 and attended by 180 scientists from 57 different institutions. Yet another was a regular flow of visitors from more than fifteen countries for advanced training and/or research experience in the subject.

While the applied work developed more or less as foreseen, the details of the fundamental results certainly did not. Our interest was focussed on the unstable (i.e. daytime) side of neutral conditions, since this was when the fluxes were strongest and geophysically most important. Our expectation that the eddy transfer coefficient (ratio of flux to gradient) for heat would exceed that for momentum was confirmed against the opposite prediction by G. I. Taylor; but we were surprised, and the rest of the world at first incredulous, at the closeness to neutral at which this and associated effects began to appear.

It had long been known that dampening of the wind-shear turbulence under stable conditions occurred at Richardson numbers (Ri) of the order of 0.5. The surprising discovery was that on the unstable side a significant departure from neutrality was evident with Ri as low as -0.02, and from Ri = -0.03, which could represent still quite moderate-to-strong winds, the heat flux was independent of wind speed and proportional to the 1.5 power of the lapse rate. These were shown to be the relationships applying to effectively free convection. The potential temperature profile would then obey the law

\[
\frac{\partial T}{\partial z} \propto z^{-3/2}
\]

This dual law was found to hold for at least a 20-fold range of heights and, under normal conditions of surface heating over most of the earth, these heights would be the most accessible for measurement of the gradient. Thus the new law was both powerful and practical. The temperature fluctuation records were also surprising in showing that, even very close to the ground, the effect of instability was not to augment the temperature fluctuations due to wind-shear but to create a new set of fluctuations of larger scale. These sustained their identity while penetrating deeply through the background of wind-shear turbulence. Accordingly the transfer coefficient for heat increased with height faster than that for momentum. The coefficient for water vapour was found to be effectively the same as for heat, due to the high correlation between temperature and specific humidity within the buoyant elements.

With the heat flux and temperature profile solved for most practical situations, the focus turned back to that of establishing the exact form of the wind profile under conditions of heating from below. Such was shown to require expeditions to exceptionally flat and uniform sites at Kerang and Hay, from which the profile was determined with definitive accuracy. Using the universal scaling factors developed by Monin and Obukhov, the profile under all conditions of drag and upward heat flux could be shown as a single curve or arithmetic function.

The above land-based part of the micrometeorological research had been directed by Swinbank, a full account of whose work, personality, and general contribution has been published elsewhere (Priestley 1974). After he left for America the leadership was taken over by Arch Dyer, supported by Bruce Hicks and later John Garratt. Strong liaison developed with Russian and American groups, with joint comparative field programs in the USSR and at Cobargo, New South Wales. In the international Air Mass Transformation Experiment in the East China Sea, the fluxatron (page 29) was to play a key part in providing the measurements of the large heat flux and evaporation rates.

From the start, Deacon had directed a counterpart program over sea, where both the nature of the basic problem and the techniques of obtaining data are quite different. The knowledge of the drag and heat exchange coefficients, here primarily a function of wind speed but modified by stability, was substantially improved, but obvious difficulties have prevented the same accuracy at the strongest wind speeds. Because of the importance of air-sea interactions to climatology in both short and long term, we became persistent but frustrated crusaders for more oceanographical research in Australia. Now this has been recognised it is
gratifying that an Aspendale man, Angus McEwan, has been appointed to lead it: and it is noted for the sake of history that when an early effort was made internationally to summarise understanding about the oceans, the only contribution solicited from this island continent was by Deacon and Eric Webb (1962).

Other scientists contributing to the total micrometeorological program were Taylor, McIroy, Angus and Spillane. It is impossible here to do justice to the many ramifications, some of which were major undertakings and could merit an article in their own right. A summary of our applied environmental studies has been published (Priestley 1972). Mention by way of headline should be made of frost protection of grapes, citrus and pineapples; of field studies and evaporation estimates in design and operation of the Snowy Mountains scheme; of suppression of evaporation by monomolecular films, in collaboration with Bill Mansfield, who later joined the Division; of heat balance of animals and plants; of gust structure as a function of height, important in building and aviation; of direct (Fluxatron) measurement of evaporation from snowfields, forests, and marshes; of estimated profiles of temperature in air and soil over huge areas for cable and radio communications; in studies of roughness inside coral reefs and of the dispersion of oil slicks on open water. Of late the greater community awareness of industrial pollution has turned the emphasis in micrometeorology back towards the traditional problems of turbulent diffusion.

By the early 1950s a radiation observing program had been established in Melbourne by Dr Albrecht of the University. But it languished when he left and, with micrometeorology demanding an adjunctive radiation expertise, Peter Funk was appointed in 1957. A composite recording program at Aspendale was set up within a year, the basis of the complete radiation observatory which now exists. The development of a new and weatherproof net radiometer of high sensitivity quickly followed. This in turn led to the establishment of a national calibration service, and later of a WMO Regional Standards Laboratory, for radiation instruments.

Interested colleagues around Australia set up radiation observing stations, serious errors were found over most of southern Australia of values published in the reference literature, and in 1963 we began a limited form of national radiation survey in conjunction with the RAAF. The discovery that the vertical radiation divergence at night (i.e. the contribution of radiation to the cooling process) had been grossly underestimated by all previous calculations from 'radiation charts' was to throw considerable new light on the complexities of frost, dew and fog formation.

Here again there was a diverse spin-off in industry, building design and construction, deterioration of materials, health and comfort, e.g. in the ability to make a survey of the radiation balance of the human body and other isolated biological objects with a single instrument. Tragically, Funk died suddenly in 1964 on his way to an international instrument comparison. The services for information, standards, calibration and observatory were shouldered by Barrie Collins. But the impetus of research was halted, until the appointment of Garth Paltridge and of Martin Platt brought renewed momentum, new directions, and a major reference work (1976). Dr Platt is to contribute an article bringing us the cutting edge of current radiation research.

With Ian McIroy to the fore, the Division had worked on the control of plant growth by radiation and micrometeorological factors. For this purpose welcome crops of potatoes had been grown in the Aspendale backyard, and barley at Mount Derrimut in a collaborative experiment with Melbourne University. Paltridge took this further with comprehensive growth models of pasture and trees. He organised a comprehensive study on wheat with the Victorian Department of Agriculture at Rutherglen in 1971. Some forty meteorological variables were recorded hourly for most of a seven-month period, supported by detailed growth measurements. I remain unsure whether this unprecedented data bank has been used to full effectiveness.

**Dynamical meteorology**

Shortly before leaving for Australia I had done some research entitled 'dynamical control of atmospheric pressure'. A paper had been accepted by the QJRMS. No proofs reached me, and it was published with so many errors that nobody, least of all myself, could bear to read it. Meteorologists were still puzzled by the Jeffreys paradox' that the surface pressure tendency in terms of integrated geostrophic wind divergence implied that cyclones and anticyclones should move around with speeds of hundreds of miles per hour. The paradox was resolved only when the geostrophic was replaced by the gradient wind based on trajectory curvature.

The work was elaborated in two further papers. A fourth, on thermal steering, was rejected by both QJ referees. The Society in error sent me their reports, with signatures intact! Sutcliffe and Sawyer have since been my good friends, but I took a little joy in successfully returning the compliment to one of them two years later. Though never so recognised, my approximation was clearly the forerunner of the 'balance equation', developed a decade later to rescue the early numerical forecasts from massive errors deriving from the same paradox. What I have continued to regard as my best piece of work was thus almost totally ignored, and remains so. Others who have felt the same may find solace in this story.

Indirectly, however, there was fruit. Pondering on Jeffreys in Flinders Lane, I recalled his classical
conjecture, that cyclones and anticyclones were not just disturbances but necessary agents in the general circulation. This could now be put to firm test. The poleward momentum and heat fluxes could be directly calculated at radio wind-sonde stations with near-perfect records: they were, and Jeffreys was vindicated. I was fortunate here in getting a start on two American pairs, Starr-White and Bjerknes-Mintz, in thus opening the study of largest-scale transfer processes (climatic stresses, as Bjerknes christened them). The disparity of resources meant that they soon caught up, but a significant difference of approach developed. My calculations were made from observed radio winds, thereby providing values for the contribution from the mean meridional circulations. This they sacrificed, unwittingly at first, gaining wider coverage by reading winds from analysed maps. It seemed to me that they could also be making a systematic error in the eddy flux, but in conversation in 1951 one of them told me I was wasting my time and his. He had submitted a paper in which his readings were confined to my locations, and had then got results identical to mine. When asked whether he had fitted his contours to the observed winds he paused, thanked me, and wrote off to withdraw the paper. Homer can nod, indeed.

But the writing was on the wall. With Sandy Troup, we turned to specific aspects, the fluxes across the high pressure belt, the contributions from jet-stream conditions, which our approach could illuminate the better. After this, we yielded the race. The approach was later resumed by Brian Tucker, who found interesting relationships in the interdependence of flux, latitude, and structure and maturity of depressions. Troup was to branch out on his own, braving the difficulties of that most elusive of phenomena, the Southern Oscillation.

It was anticipated that there must soon be a growth of meteorological interest in convection research. Oddly, cloud physicists had been slow to face up to this. Our microstructure records showed convection in miniature, providing an impetus and an advantage. A series of papers culminated in one with Keith Ball, a formal treatment of plume rise through an environmental temperature gradient, showing *inter alia* that penetration height through an inversion is proportional to the \( \frac{1}{2} \) power of the source and \( -\frac{2}{3} \) of the inversion strength. This, our second discovery of a 'law', is usually credited to Morton, Taylor (G.I.), and Turner, though their paper appeared the year after ours. The two works were quite independent, a hemisphere apart, so that it is interesting to note that all four surviving authors are now in Australia.

I also published a working model along novel lines of a plume bent over by the wind. Further progress in either theory was impossible: there were no data good enough to test the predictions! Now that there are data in plenty, formulae are fitted to them but still carry the imprint of our theories. Kevin Spillane has made new applications, e.g. to underwater heat discharge, among his many contributions to environmental aspects of power generation.

Ball was to become a highly original researcher. His most productive period began with a paper on energy changes in disturbing a dry atmosphere and two papers on katabatic winds, explaining the exceptional gusts and lulls along the Antarctic coast and the angle between wind and slope on long fetches. Another set out the formal and physical similarities and differences between long waves, lee waves and gravity waves. Strong physical perception underlay his work on the control of inversion height by surface heating. A theory of fronts accounted for the weakening of surface warm fronts with decreasing latitude. Ball then worked for a year as Honorary Fellow at the ANU, and subsequently for two years on leave of absence as Assistant Director of Research in Dynamical Meteorology at Cambridge. Here he suffered severe head injuries in a street accident. It was the Division's and meteorology's tragedy that our two brightest hopes of that era, Ball and Funk, were to be cut short at the full crescendo of their careers.

In dynamical research on the synoptic scale, James had published a number of papers, but without creating any continuing theme. On his resignation we decided that the presence of a thoroughly experienced synoptician in the Division, without breaking our understanding with the Bureau, would give our younger men a greater awareness of this central part of meteorology. Andrzej Berson had been an independent co-discoverer, with Eady and Charney, of baroclinic instability, though robbed of his share of credit by his paper being just a little later and less sharply pointed. He had a finer 'feel' for synoptic meteorology than anyone else I have known, excepting only C. K. M. Douglas. Like Douglas, he had difficulty in passing it on to others in easily digestible form, and many 'others' were not wise enough to seek it: but those who did were rewarded, as acknowledged e.g. by Paltridge and Platt in their work on radiation. Reg Clarke and Spillane were later to add to the synoptic educational role in the Division and in interaction with research groups outside it.

Berson made a wide range of research contributions, on problems concerning Antarctic circulation, the equatorial trough and monsoons, the quasi-biennial oscillation, the interpretation of radar echoes, and convective systems. But his most sharply focused efforts lay in studies of structure and propagation of cold fronts and sea breezes in South Australia and Victoria, in collaboration with Troup and Derek Reid. This work emphasised the important role of differential heating over land and sea. For a curious phenomenon at the leading edge, the term 'Berson nose' became a byword. He organised and led an expedition to Mount Gambier,
our first outside micrometeorology, thereby laying the basis of what became an important tradition.

Clarke, then in the Bureau, had joined the expedition for two weeks. He had a prior interest in sea breezes and had himself conducted field studies in Canberra, with one assistant, and on the Esperance ‘Doctor’ assisted by his brother. After joining the Division he was to develop these studies further with expeditions to the Renmark and Coonalpyn Downs areas. One outcome was a thermal-dynamical model of the sea breeze system: another, a typical cross-section normal to a cold front, based on the best data then available.

I was concerned at the time about the scale ‘handover’ problem in the general circulation, particularly about mesoscale contributions to vertical momentum transfer in the westerlies. Clarke’s cross-section indicated that fronts could be important agents in this transfer. I suggested a new expedition objective, measurement of mesoscale vertical velocity and associated transfer. Our calculations suggested that this should be just practicable, and Clarke expanded the objective to a full field study of the planetary boundary layer in the westerlies. The Wangara Expedition (1967) remains today as the definitive study in its field. Cooperation of the Bureau, headed by Bob Brook, provided much needed extra manpower. The huge data set was published in 1971 and has formed the material for scores of papers in international journals, whose number is still mounting. The fact that the expedition totally failed to confirm the expectation which had prompted it is a typical quirk of research evolution.

A later expedition (Koorin 1974) with similar objectives relating to the sub-tropical easterlies produced another unique data set, also well used, perhaps not so comprehensively. Owing to advection effects on meso and larger scales there has been difficulty in isolating some of the sought phenomena, such as thermal wind and nocturnal jet.

Clarke was to spend three years in charge of ANMRC before his expeditionary ardour again found outlet, after retirement, in the ‘Morning Glory’ of the Carpentaria region. With Roger Smith and his team of Monash students, and the experienced Derek Reid, there has been dynamical elucidation and fine photography of this spectacular phenomenon, of considerable general scientific interest as an unidural bore. And, though trespassing beyond my term, I note a recent return to major expeditionary study of cold fronts, now on a multi-institutional basis and including aircraft support and concurrent synoptic participation.

To complement our field and paper work we had twice in the sixties advertised unsuccessfully for someone to do laboratory modelling of atmospheric processes. One day in 1969 a young stranger in duffel coat and bow tie strode into my office to ask if he could name Aspendale as host laboratory in his application for a QE II Fellowship. With strong commendation from G. I. Taylor, McEwan’s application was successful. The two-year Fellowship gave opportunity to introduce him gently to meteorology, and then he joined up. Here was a rock on which to anchor a new activity, an originator and brilliant experimental tester of new ideas about long-misunderstood processes. Peter Baines was already in line for recruitment via a CSIRO Overseas Fellowship, to be followed by the first of the new Queen’s Fellowships in Marine Science. These appointments were to usher in a greatly expanded program of phenomenologically-oriented dynamical research, with Peter Manins, Alan Plumb, and Brian Sawford added to form a highly viable group. New concepts and formulations, laboratory modelling, and some ad hoc field observation programs, have brought much light to bear on internal waves and tides, fronts and southerly busters, convection, rotating systems, the quasi-biennial oscillation, katabatic winds, and the planetary boundary layer. Only the foundations and earliest work belonged to my own period as Chief. This and my length limitations prohibit a detailed account here, but it is much hoped that one will be written, now or soon. Suffice it for me to express delight at seeing all this young strength which, added to that in ANMRC, Monash and elsewhere, augurs a vintage era in Australian dynamical research on atmosphere and ocean.

Collaboration with the Bureau and the growth of internationalism

The most productive scientific relationships between institutions are generally those established informally between individuals. However my task here is to chronicle those which had some measure of formality.

Very early we began to hold tripartite seminars with the Bureau and the University. Occasionally there was a speaker from overseas, but international comings and goings were rare in those days.* The first big breakthrough was the 1956 Canberra UNESCO/CSIRO conference on ‘Climatology of the Arid Zone with Special Reference to Micrometeorology’, to which came Brooks (F. A.), Burgos, Drummond, Dzerdzevski, Emberger, Geiger, Monteith, Ramdas and Thornthwaite. The conference tour, planned as a demonstration of aridity, was a complete flop as such. The Murray was miles wide in flood, and the hospitality of the City of Broken Hill went far to emulate it. The delegates enjoyed the contrast. Such conference hospitality remained unparalleled until 1974, the First Special Assembly of IAMAP/IPASO, which far more of my readers will remember.

During Mr Timcke’s directorship, the Bureau had formed Climate Consultative Committees in each

* Contrast my own single overseas visit in the first ten years, with fourteen in my last eight years as Chief.
State. CSIRO officers served on several of these, and in Victoria we were able to extend the scope significantly in areas where our work was most relevant, e.g. soil erosion and water conservation. We also served with Bureau representatives on committees for the Australian work in Antarctica and, after the foundation of the Academy in 1954, on committees for the IGY and the newly formed SCAR, SCOR and COSPAR, and National sub-Committees for Meteorology, Oceanography, Hydrology, etc.

In 1960 the government had approved submissions from the Bureau and the Academy (the adhering body to SCAR) that an International Antarctic Analysis Centre should be established in Melbourne. The IAAC was able to demonstrate the viability and operational usefulness of the first hemispheric analyses, large as the data-gaps were. Subsequently the operational activity was absorbed into national services, the Centre itself shifting emphasis in that ‘Meteorological Research’ replaced ‘Analysis’ in its title. It was a pleasure to serve throughout the nine years as Academy representative on the Steering Committee, to hold many discussions with the truly international personnel which included Bureau officers and Berson and Troup from CSIRO, and to acquire growing respect for the qualities shown by Henry Phillpot as its leader.

IAAC/IAMRC was a harbinger of bigger things to come in ‘global collaboration’. This concept is more nearly achieved, in the full sense of both words, in meteorology than in almost any other walk of life and certainly than in any other science. Fired by John Kennedy’s addresses to the UN Congress in 1961 and 1962, ICSU formed its Committee for Atmospheric Sciences. This committee began immediately to develop the conception of, and outline a plan for, a truly global experiment to exploit the advances in satellite and computer technology. In 1964 WMO followed by forming an Advisory Committee to advise it on its scientific and educational functions. It was a somewhat awesome responsibility to serve throughout on each of these committees as the only member from the southern hemisphere. All this activity came together in the 1969 ICSU/WMO Agreement which formalised the concept of GARP. A new joint committee (JOC) was formed to organise the work. I was to serve for four more years, and Tucker has carried on since and into WCRP to preserve our hemispheric tradition. Though perhaps too soon for final judgements, there are many who feel the global experiment to have been successful, a potential milestone in meteorology particularly for southern countries.

One great personal disappointment marked this era. In the WMO Advisory Committee I had suggested that the sea surface temperature data lying idle in national archives be pooled and analysed on a historical, month-by-month basis. I saw this as a step towards understanding and eventual prediction on the monthly and seasonal scales, a unique source of material for research into short-period climatic variability. The suggestion was enthusiastically endorsed by the AC and accepted by WMO, who christened it the Historical Sea Surface Temperature Project. But somewhere along the corridors of international bureaucracy the project seemed to falter. Subsequent individual efforts have gone some way to remedy the shortfall.

In Australia, Mr Dwyer had died and Dr Bill Gibbs has succeeded as Director, and he and I had held discussions on our roles in the now fast-expanding meteorological scene. At Sir Frederick White’s suggestion we circulated a ‘Prospectus’ (1967) in government and scientific circles to create a wider awareness of the growth and intricacy of meteorology, and hopefully to promote interest in some of the newer universities. It caught the attention of Professor Karmel, first Vice-Chancellor of Flinders, and helped to bring about the Institute there. With Bruce Morton’s initiative at Monash, the overall university scene was growing stronger.

Before us all lay the question of what to do in Australia to exploit the forecasting and climatic promise of the new numerical-hydrodynamical approach. Uwe Radok and Dick Jenssen in Melbourne University had started the ball rolling with some pioneering studies in the Australian region, and Clarke had made the first hemispheric forecast while in Princeton, but it was clear that the total requirement called for full-time involvement on the Commonwealth scale. Three scientists in the Bureau (Ross Maine, Doug Gauntlett and Bob Seaman) and two in CSIRO (Barrie Hunt and Bill Bourke) were already earmarked and preparing for this eventuality. It was agreed in principle between us that the main responsibility should rest with the Bureau: that CSIRO would plan to give considerable back-room support, meanwhile putting all their influence in the political arena behind the Bureau’s submission for the necessary resources. But the decision, when it came, was that the government would provide the funds on condition that CSIRO were active partners. On this basis the IAMRC was discontinued and CMRC came into existence as a joint venture which, following successive reviews with minor changes of name and structure, is still continuing. My own belief, which some others do not share, is that on this occasion the government was wiser that its scientific advisers.

But there was a price to pay. A heavy overseas travel program had allowed me to be a close observer of the development of this area of meteorology in several countries, particularly the UK and USA. Conspicuous had been the ‘non-acceptance syndrome’ among the traditional forecasting profession with its concomitant symptom of lack of feedback. Knowing this, we might ourselves have found prophylactic measures,
but such a hope was not realised and the lesson had to be learnt again here the hard way.

The early lessons were hard in other ways too. Techniques proven in the northern hemisphere gave far less practical response in the data-sparse and alien climate of the southern hemisphere. Another constraint was the severe limitation imposed by local computing resources. New approaches were needed and were found by the CMRC under the leadership of Tucker, who had transferred from the Bureau position of Assistant Director (Research). Tucker showed fine judgment in choosing and grouping the men for the various tasks, in building a sense of corporate entity into the Centre, whose later successes owe much to his foundations. In the case of Bourke's spectral technique he was prepared to sponsor and back an 'outsider' which was to go on and win handsomely.

Some improved and some quite new methods were devised for turning cloud pictures and other satellite data into numbers which could be put into the numerical analyses (Neil Streten, Troup, Graeme Kelly). Routine regional prognoses were started using a filtered baroclinic model (Maine). Numerical methods were extended to the full hemisphere (Gauntlett, Seaman) on a quasi-operational basis, and were applied to problems of observational network design (Seaman).

By the late seventies substantial returns were showing up as an improvement in accuracy of regional and hemispheric circulation predictions. Significantly, these advances were achieved with a largely Australian-made technology. Several of its elements, notably the spectral method, have been adopted by other national weather services and research institutions. More recent essays into fine-mesh modelling give promise of contributions in the smaller scale and the shorter term (Lance Leslie, Gauntlett).

Work on the general circulation (Hunt) has shown that the numerical method can incorporate factors contributing to the variability of climate, such as ocean forcing, volcanic dust, solar variations, internal oscillations. Complementary observational studies have been carried out by Neville Nicholls and Streten, and at last substantial investigations of sea surface temperatures as correlated with subsequent Australian rainfall have come about. There is good reason to believe that the best benefits are still to come, to forecasting in both shorter and longer term and to our basic understanding of the global scale of dynamics.

The problem of climate variability is much the hardest meteorology has ever faced. Besides the above, Australia has contributions to make with Bill Budd's studies on sea surface temperature, Barrie Pittock's statistical attacks, and venturesome new approaches via thermodynamics (Paltridge) and statistical mechanics (Jorgen Frederiksen). There now appear two needs if the required support for work in this area is to be gained and held. One is to keep to the hard middle path between undersell and exaggeration, the other to bring more mutual correlation to the various approaches. And WCRP must emerge from its soul-searching to emulate GARP in giving leads and providing shape to the international effort.

**Stratospheric and chemical studies**

Visiting Dobson before we left England, Swinbank and I had learnt that there had been sporadic measurements but no continuing ozone program in the southern hemisphere. We intended to take this up when the opportunity arose. In addition to gaining information on the general circulation at high levels, there was a more immediate synoptic motive. In Britain a change in stratospheric ozone at times provided the first harbinger of a depression approaching from the Atlantic. Any such signals here would be even more valuable in view of the sparsity of data to the west and south.

Two Dobson spectrophotometers, some years in disuse, were made over to us after much persuasion. Learning, refurbishing and calibration of the old and supply of a third and new instrument took much longer than expected. Routine observations were finally started at Aspendale in 1955, and at Brisbane and Macquarie Island the following year in time for the IGY. The network today comprises six stations observing total ozone and its vertical distribution, under supervision and standards control from Aspendale, now the WMO Regional Centre, with cooperation at outstations from the Bureau and the Antarctic Division. With ten stations in the hemisphere our network, linking with Antarctica, provides the only cross-section.

A much improved body of knowledge about structure and circulations of the stratosphere has been built up, due largely to the interpretative skill of Rangnath Kulkarni, leader of the ozone program for the last twenty years, and of Barrie Pittock, who initiated the vertical soundings. Joining later, Ian Galbally pioneered the study of ozone interaction with the earth's surface, so completing the cycle of creation, transport (mass and diffusive), and destruction. Synoptic rewards have not materialised, at least not yet. But there have been major benefits in three contexts unforeseen at the outset. The first was the proliferation of nuclear testing which placed a premium on our knowledge about stratospheric circulation and diffusion. The second, from Galbally's work, was the identification of photochemical smog in Australia and the start of a new generation of urban pollution studies.

Cracks in the knowledge of ozone photochemistry had first appeared when Chapman's theory, accepted for a generation, was successfully challenged in 1965 by Hunt, then working in the Weapons Research Establishment in Salisbury, S.A. For a while Hunt could claim the reputation as the world authority in ozone photochemistry, until a
growing recognition of its full complexity (by no means yet sorted out) took the matter beyond his interest. World-wide concern erupted when an American scientist predicted that the advent of commercial supersonic aircraft in the stratosphere would drastically reduce the ozone amount through interaction with the exhaust gases. The Australian Academy of Science convened a Working Group to provide the public with a balanced viewpoint. Their report in 1971, relatively reassuring but admittedly based on incomplete knowledge, was mercilessly and not altogether scrupulously attacked. There is consolation that, after a decade of subsequent intensified research, overall scientific opinion today would demand no major revision of its main conclusions. The major consolation at the time, the third of the benefits of our 1950 initiative, was that we had considerable expertise on which to draw, and 15 years or so of good southern hemisphere baseline stratospheric ozone data, against which the relevance of future variations could be judged. Following its report, the Academy made repeated recommendations to government for support of stratospheric research and monitoring. But the only substantial development seems to have been the winding down of the Balloon Launching Facility. CSIRO has soldiered on from its own resources.

From about 1960 onwards Dyer and Hicks had taken holidays from their work in micrometeorology to think about the stratospheric behaviour of radioactive debris and supplement our knowledge on a more ad hoc basis. This set of studies, including an examination of the effects of the Bali eruption on subsequent radiation records, was able to deduce successive global circuits of expanding discrete clouds in the stratosphere with systematic but seasonally variable drifts in latitude. Peter Hysom complemented this work with measurements of stratospheric water vapour, radiatively active and useful as a tracer in formulating the first model of stratospheric circulation. On the side, Hicks and Helen Goodman made significant studies of the distribution of radioactivity, dust content and dust fall-out across latitudes from New Guinea to Hobart.

The most important outcome of the supersonic brouhaha however had been to show that, while knowledge of stratospheric motions was fair, ignorance of atmospheric chemistry was intense, and in a much more general sense. There had been growing awareness of this for some years, and a rising concern about global pollution and its possible impact on climate. For example, a main barrier to advance in ozone photochemistry was recognised as the complete ignorance about natural existing distribution of nitrogen oxides. Galbally was to tackle this and related problems including the man-made origin and history of atmospheric carbon tetrachloride with conspicuous success. And as the role of anthropogenic 'ozone bogy' shifted from supersonic aircraft exhaust to the halocarbon gases, their study was taken up at Aspendale by Paul Fraser.

Meanwhile, our contributions to global CO$_2$ studies had originated from an entirely different direction. David Parkhurst, who liked to describe himself as a nanometeorologist, worked with us for some years on questions of leaf shape, stomatal effects, etc. Graeme Pearman, originally of similar interests, had turned with Garratt to studies of CO$_2$ uptake by crops, so acquiring skills and experience of extremely accurate CO$_2$ measurement. I was concerned about the almost total absence of southern hemispheric CO$_2$ data in the global climatic context, and suggested that they might move in this direction. The slightest encouragement was enough. Within weeks cooperation with airlines had been arranged, and in less than a year data on vertical distribution from ground into the stratosphere, and seasonal variation, were rolling in.

All these activities and promises were ripe to find their place in a new major undertaking, as was the particulate research in the Division of Cloud Physics. Australia was in an excellent position in terms of geography, and now also in terms of scientific expertise, to make a major contribution to the concept of a global atmospheric monitoring program which was taking shape in international thinking. At my request Galbally and Sean Twomey had given preliminary consideration to the structure and content of such a contribution. When the responsibility was accepted by WMO I had no difficulty in persuading Dr Gibbs that we should join in formal recommendations to establish a baseline station. A coincidental holiday touring the 'apple isle' assured me that Tasmania could provide a most appropriate site. However, this in the event would have to depend on intentions of other countries: for example, any intention relating to a station in the southwest of New Zealand could persuade us to think of alternative Australian locations, say in the tropical easterlies. Our recommendations resulted in a government decision in principle, that a Baseline Air Monitoring Station be set up and managed by the Department of Science, with individual scientists from CSIRO playing a collaborative and scientifically supervisory role. This decision opened a new chapter, whose events and progress are recorded in the official reports and will doubtless form the basis of other present and future contributions to this Magazine.

Instruments and introspections

It should be clear that many of our interpretational advances referred to above could be achieved only against a background of practical work, on many fronts, in the laboratory and the field. This in turn depended on new instrumental ideas and developments. To none of these did I make any personal contribution. Indeed some of the inventors felt that my inadequacy in this regard robbed them
of full recognition for the quality of their work. However I took care that 'second opinions', always obtained before making any important judgment, were extended to third and sometimes fourth in instrumental contexts.

Among the instruments and facilities designed and developed at Aspendale, I recall without prompting the following (capital letters are used where christenings took place, names in brackets are the parents): ground heat flux plates (Deacon), quick-response wet bulbs (Swinbank), wind tunnels for model studies and anemometer calibration (Deacon), the Atmospheric Fine Structure Recorder (Swinbank, Deacon, Taylor, McLlroy) later automatized into the Evapotron and Fluxotron (Taylor, Dyer, Hicks), the Energy Partition Evaporation Recorder and lysimeters (McIlroy), the net radiometer including miniaturisation (Funk), the radiation calibration laboratory (Funk, Collins, Paltridge), the net long-wave radiometer (Paltridge), the 6-month Long Term Field Recorder (Summer), the Buoy, and orange peel (Deacon, Sheppard, Webb). Many of these found wide use by other scientists, the long-term field recorder and net radiometer were patented and manufactured under licence in Australia, with good sales here and overseas. Our group of instrument makers, led successively by Messrs Harris, Sim and Karau, contributed ideas as well as skills to these developments.

The Summer buoy provides the cue for some final introspections. A study was published in this Magazine in 1964 wherein the very scrappy records of sea surface temperature offshore from Coffs Harbour to Maria Island were correlated with land rainfall in adjacent months. A joint program was later set up with the Bureau and the CSIRO Division of Fisheries and Oceanography to improve the quality and continuity of data and extend the study to a ring of stations around the continent. The buoys worked reasonably well, through difficulties largely associated with access and anchoring, the program was not very fruitful. Fortunately the remote sensing era has supersedes.

Other project failures or dead-end initiatives have not been recorded here. Such are an inevitable part of any original research program and, provided they are not too frequent or expensive, there need be no heartburnings. Ineffectiveness in representation is quite another matter. Though we did make quite crucial contributions to the FGGE program in the form of planning, design and supply of buoys, and subsequent analysis, I regret my failure to secure a larger real national allocation of funds, from which we stood to gain so much. I also failed to convince those who drew up the CMRC Agreement, and the subsequent ones for ANMRC, that the Officer-in-Charge could be equally well appointed as a Bureau or as a CSIRO officer. Many of the early troubles and some of their aftermaths might have been avoided if this had been agreed. But certain administrative considerations were allowed to prevail.

The principle that administration in science be designed to help the scientist was given position of primacy by Rivett of CSIR. It has been sad to observe its decline together with the enormously increased share of the cake now gobbled up in non-productive activities. I can claim to be one who resisted to the last. But the Chiefs, by number and nature, were never fitted to form a united front on any issue. Just before he retired, Rivett had written to a colleague: 'Like you I am unhappy about the future. The main danger as I see it is that people will knuckle under to the bureaucratic regime and, by avoiding fight and seeking comfort, they will gradually reach a condition of tolerant acquiescence in what they formerly knew to be wrong. A generation will arise that knows not freedom and will be content to do without it. Then some day an old battle will be fought over again'.

My comment concluding the 'Dynamical meteorology' section applies in a more general sense. At no time before has Australian meteorology been so rich in total talent, and my greatest pride is to have played a part in bringing this about. The future then should be highly productive, provided that authority and the institutional environments are sympathetic, the profession responsible and united. The international world of meteorology has liberally recognised our achievements and potential. But I worry that, within Australia, our image has (or images have) failed to keep pace. The finest hour of our public image came with the early television programs by Bob Crowder and Tony Powell: it went when they went. Of the image in the eyes of our bureaucratic and political masters and fellow scientists in the establishment, suffice it to point to the surfet of reviews to which our institutions have been subjected of recent years; and to the fact (as of September 1981) that not one member of any reviewing committee, nor of the standing Meteorology Policy Committee, has been a proficient meteorologist familiar with all the interconnections within the subject. This has been referred to as the 'Amateur Review Syndrome', and their reports are redolent of haste as well as inexperience. I know no other science of comparable scope and complexity on which such an indignity could have been imposed. Yet there has been no concerted protest.

Sydney Chapman, only part-time a meteorologist but always a close observer, gave this view in a ceremonial address in America shortly before he died: 'The astronomer in his quiet observatory or study, the physicist before his experiment in the laboratory, might well marvel that men can be found to face such complicated and urgent tasks as confront the meteorologist. But there are men who will tackle immense difficulties, doing their best possible'. Throughout this article I have used the word 'meteorology' as Chapman did, in its
practical, scientific and literal sense; knowledge of the atmosphere. My final failure has been to convince some fellow meteorologists that this is what they are: and of the need to set differences aside and put unanimity and conscious effort into the cultivation of a better image, if the science of the atmosphere is to gain a place in the sun.

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