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Cover Pictures: Top: An optical micrograph of crystal structure in superconducting thin film, made at CSIRO Division of Applied Physics, and submitted by Dr John MacFarlane. Below: A photograph of a corona around the sun produced by diffraction of light by the water droplets in cirrus clouds, taken in the Brindabella Mountains 30 kilometres south-west of Canberra in May 1975.
My colleague, Boris Schedin, Professor of Economic History at the University of Melbourne and author of a history of the CSIRO entitled "Shaping Science and Industry" (Allen and Unwin, Sydney, 1987), gave a colloquium in the School of Physics a couple of months ago. I am glad to have succeeded in persuading him to allow us to publish his text (see: CSIRO: What Went Right? What Went Wrong? elsewhere in this issue); I found it interesting and thought provoking.

One of the historical facts, of which I was only vaguely aware, was the enmity, in the early days, between universities (then starved of funds by state, rather than federal, governments) and the CSIRO, which was then fairly well equipped and staffed. For a very long time, research was almost entirely the province of the CSIRO where the prevailing ethos was that of that great man, Sir David Rivett.

Later, however, under the influence of that other great man, Sir Ian Clunies Ross, the system was reformed and, by the mid-1960s, with the advent of the ARGS, university research was brought up to scratch and began to give the CSIRO a run for its money. It was more of a spirit of friendly rivalry, rather than enmity, for people of my generation. Cooperation existed and even flourished, in certain fields.

Later still, in the 1970s and 1980s, university researchers became close allies when they began to perceive the increasing plight of their CSIRO colleagues, during the many trials, tribulations, reviews and restructuring to which the organisation was subjected.

Now, as the CSIRO lurches from bad to worse under the guidance of yet another great and nifty man, I sense that whatever enmity or even rivalry their predecessors may have felt, university scientists are full of compassion and concern. Out of a keen sense of self-interest, if for no other reason. It may be their turn next! The inexorable march of economic rationalism and the implacable antagonism of politicians and bureaucrats is well on the way to bringing pure science to its knees in this country. The process is well advanced in the CSIRO, particularly in the field of physics. A great pity, because many things did go right and contributed greatly to Australia's scientific culture and international standing.

Among the things that went wrong, Schedin is probably correct in identifying the lack of early recognition of the problems of knowledge or technology transfer. In the biological areas there was no great problem. A sizeable army of agricultural extension workers was there to bridge the gap and CSIRO-developed methods, products and processes found their way to the farms. Besides, farmers were eager to adopt new techniques, especially if they were lucrative.

In physics and allied areas, not many middle-men existed for transferring new technology into industry. Furthermore, in the corporate culture of Australian industry there was, plainly, no demand for research. New technology was something that one didn't need, or one bought off the shelf, or something that was done at the headquarters overseas. Thus, much of the front-line, world-class research in the CSIRO Divisions did not lead to any directly identifiable economic benefits, at least not to Australia.

Today, there is a great deal of emphasis on technology transfer but the difficulties are still enormous. Is the 150% tax rebate "carrot" changing ingrained attitudes in industry? Even Senator Button admits that it is a very slow process. Compared with the rate of visible change in industry, the rate of change in the CSIRO has been nothing short of dramatic. While industry was getting all the carrots, the CSIRO was getting nothing but sticks. Has the Science Statement changed anything? Will any of the ethos of a premier research organisation survive? Will the quality of research be maintained? These and other such questions do not appear to concern the economic rationalists who deal with costs, not values.

What about the future? I was very glad that Professor Schedin ended on a note of cautious optimism; he obviously believes that the government's recent rhetoric means something. I hope he is right. But please read his last paragraph carefully; he says that the CSIRO will continue to have an important role in the commodity production sector. But what about manufacturing industry? I hope that the CSIRO gets there too but, just to be provocative, allow me to finish allegorically, with the following Norwegian story.
A CORRECTION AND A COMMENT

My editorial for this issue will address two quite separate topics. The first part is by way of a correction, the second is a critical comment on our community of physicists and related disciplines.

Reprinted in this issue is a letter received from the Institution of Engineers, Australia following comments I made in the August issue editorial in which I expressed my concerns at newspaper reports of the Institution’s submission to the ARC. Those reports suggested the Institution was calling for a reduction in the amount of funding devoted to basic research in Australia. I am pleased to learn from Dr Dack’s letter that this was not the case. It is particularly important since a large proportion of the funding for research in engineering schools in Australia comes from the ARC and much of that research is of a basic scientific nature, often directed towards exploring the fundamental science of engineering processes and applications.

I am happy to have Dr Dack set the record straight through his letter. I would suggest that the Institution of Engineers Australia does not also be exerting pressure on Australian industry to “place greater attention on exploiting the results of basic research for economic and social development”. Australia still maintains an extremely poor record for such research in industry. The 150% tax incentive has apparently been of marginal use in raising our effort in this area, at significant cost.

The second part of this editorial stems from my recent experience as a member of the discipline panel of the ARC. My respect for the efforts of those involved in earlier panels has increased enormously now that I have experienced the work load imposed on them and the poor support a substantial part of the physics community gives to one very important aspect of the allocation process. Most physicists and many scientists in related disciplines will have been asked to review grant applications. On occasions I understand individuals have been asked to accept a number of related grants and provide a critical review. Many, if not most, of the reviewers do their jobs well, obviously producing a considered opinion on the project and the proposer. There is a very significant proportion of those asked to review proposals, however, who do not help the granting process at all. Firstly there are some proposed reviewers who do not respond to the request from the ARC. Many reviewers in this category are themselves recipients of ARC funding for their own proposals. Some reviewers legitimately decline to act, due to conflict of interest or other related reasons. Many others provide reports which are in conflict. Those who have received proposals will be aware that there are three components to the report. The first is a set of comments confidential to the Committee, the second, reviewers’ comments to be returned to the applicant, while the third is a rating on the project and the applicant. In many cases these three sections are in conflict. Too often a high rating is given to projects which from the comments are at best good, but unexciting, extensions of existing knowledge.

Reports such as these are of limited use to the panel, while they create misunderstanding in the applicant when these sections available to them are returned. A report which, in relative terms, damns a project in one section, but gives a rating of “very good” to “outstanding” class in another section, is very difficult to accept.

Those members of the physics community invited to act as reviewers must accept the responsibility of that duty. Well-reasoned reports are essential. A response in all cases is required, even if it is to decline to act for legitimate reasons. We place great emphasis on peer review in all aspects of our life as scientists, and we must play our part in that system in an acceptable way. All those who do their best and play the role of reviewer with good purpose expect their colleagues to do likewise.

R.J. MacDonald
Honorary Editor

President’s Column continued from page 206

Johanssen is late for the cross-fjord ferry which has just pulled out as he reaches the wharf. “Yump, Yohanssen, yump”, shout the bystanders, “... in two yumps you can make it!” It seems to me that the new, reformed, reorganised revamped CSIRO is poised to jump.

Tony Klein
President


Commencing between 1 July 1990 and 30 June 1991

Senior Awards. For those holding senior academic posts, leaders in the arts and senior practising professionals who wish to study, lecture, research or carry out advanced professional training, at a recognised institution/organisation in the US. Tenable for 3-12 months. Applications for periods of less than 3 months will NOT be considered. (Study tours are tours of inspection are not supported.)


Postdoctoral Fellow (PDF) Awards. For those who have recently completed, or are about to complete, a PhD and who plan to pursue postdoctoral study at an American institution. Tenable for up to 12 months: extensions possible but without stipend.

Awards and Benefits: 8-10 awards - travel plus an all-inclusive allowance of $AUD1000 per month.

Postgraduate Student Awards. For those who have completed or are about to complete a good honours degree (or the equivalent) and who wish to study for a higher degree in America, undertake research in America which will earn credit towards an Australian higher degree, or wish to undertake a program of training and professional development. Tenable for 12 months, renewable.

Awards and Benefits: At least 12 awards - travel plus a once only inclusive allowance of $AUD6000.

All candidates must be Australian Citizens by birth or naturalisation and must agree to return to Australia to reside on completion of the award.

There are no restrictions as to discipline, although a general preference will be given to those intending to undertake work in their discipline as it specifically applies to the bilateral relationship between Australia and the United States, or to undertake comparative Australian/US studies. Up to 60% of awards will be available for proposals in four specific priority areas: Pacific Basin Issues, International Trade, Higher Education Policy Developments, and Impact of New Technologies and R&D. For further details and application forms, specifying the category of award for which you wish to be considered, contact the Secretary, Department of Employment, Education and Training (AAEF Awards), PO Box 826, Woden, ACT, 2606. Tel. (062) 76 8111.

Closing date for applications in the PDF and PGS categories: 30 September 1989. Closing date for applications in the senior category: 30 November 1989.
In the News:

400% increase in tunable UV power reported

A new UV dye from Exciton, Exalite 392E, in conjunction with the 2045 UV ion laser and 375B cw dye laser, delivers tunable cw power from 372 nm to 413 nm with peak power of 580 mW at 390 nm. This represents a fourfold increase in peak power when compared to standard polyphenyl dye.

A new UV optics option for these wavelengths is now available for the 375B.

Technology Notes:
The tunable solid-state laser comes of age

The recently introduced Model 3900 continuous-wave Ti:Sapphire laser produces higher output power over a broader tuning range than corresponding cw dye lasers. It also has all the advantages inherent to solid-state lasers: dependability, convenience, and virtually unlimited operating life.

Sapphire (Al₂O₃) is a well-known optical material. In Titanium-doped sapphire (Ti³⁺) ions substitute for a small percentage of Al³⁺ ions. The interaction of the single 3d electron of Ti³⁺ with the crystal field of the sapphire host is the source of a broad blue-green absorption band, making the argon ion laser an ideal pump source. It also gives rise to a broad emission spectrum—from below 650 nm to beyond 1 µm.

Both small- and large-frame argon ion lasers can pump the Model 3900. The former produces output powers of about 500 mW at 790 nm. However, when the Model 3900 is pumped by a large-frame ion laser such as the Model 2040, it exhibits its true power and tuning range capability, utilizing all available pump power. It produces 2.5 W at 790 nm from a 25 W pump beam. Ti:Sapphire suffers no deleterious thermal effects at high pump powers, unlike near-infrared dyes such as pyridine 1 and styril 9. This extraordinary combination of power and tunability is the source of the excitement that surrounds the Model 3900. It also suggests the future of Ti:Sapphire lasers.

Because it is a fundamentally different laser medium, Ti:Sapphire has potential both similar to and different from laser dyes. For example, the Ti:Sapphire laser may surpass, by far, the power and linewidth performance of single-frequency dye lasers. Active mode-locking can produce picosecond pulses. Frequency doubling may provide a reliable source of tunable light in the 360 to 500 nm range. And with its 3-µsec lifetime, the potential exists for Q-switching Ti:Sapphire at high repetition rates.

All things considered, the future of Ti:Sapphire is indeed bright.

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PHYSICS EMPLOYMENT IN 1988
Holding Up Well!

John R. Prescott
Department of Physics and Mathematical Physics, University of Adelaide

With this report, the surveys of employment opportunities for physicists in Australia enter their second decade. These surveys are carried out by the author for the Australian Institute of Physics. They are based on positions advertised in The Weekend Australian and in the Higher Education supplement in The Australian on Wednesdays. The general principles of the surveys were set out in considerable detail in the first report in the series (Prescott, 1980) and the data for the first decade were summarised last year (Prescott, 1988) together with comments on trends. Because these two references cover the field in some detail, the present report will be brief. In general the positions are those for which a degree in physics or applied physics or a diploma in applied physics is a suitable training even though this may not be explicitly stated in the advertisement. Most of the advertisements in The Australian call for an honours or post-graduate qualification. Positions for which a first degree or diploma in physics would be a suitable qualification are mostly to be found in the local press. For example upwards of forty positions were advertised only in the Adelaide Advertiser in 1988. Overall, these 'local' advertisements in main metropolitan dailies account for about as many positions again.

The statistics for 1988 are shown in the table and compared there with the previous three years. The most noteworthy feature of the table is the increase in the total number of positions advertised: 810, an increase of 20% over 1987, which was close to the 10-year average. This is about the same as the increase for jobs generally reported in Skilled Vacancy Surveys published by DEET (1987, 1988). It seems strangely at variance with the recent well-publicised complaints of the CSIRO Officers Association and some biologists concerned with job opportunities in that field. Whatever may be said for others, the fact remains that the market for physicists in Australia is holding up strongly. Perhaps 'buoyant' is too strong a term but there is certainly plenty of scope for employment.

The table does give some indication of the reasons for concern of our colleagues in CSIRO. Not only are positions in CSIRO down in proportion, they are down in absolute terms too; and the ratio of temporary to permanent posts remains high. Other areas in the Commonwealth (other than Defence) are also down to a record low level. There seems to be some support for the rhetoric that claims a lack of commitment of the Commonwealth government to science. On the other hand, Defence is advertising strongly, maintaining the thrust that began in 1986 (with some slight fall-back last year). There is no doubt that the various sections of the Defence Science and Technology Organisation (DSTO) are having difficulty in recruiting. They have continued the cadetship/scholarship program, begun three years ago, and they place general advertisements at regular

Advertised Positions in The Australian

All jobs advertised in The Australian for which a degree in Physics or Applied Physics or a diploma in Applied Physics provide a suitable starting point. All subdivision figures are percentages.

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PHYSICS EMPLOYMENT IN 1988

intervals. Neither of these two classes of positions is included in the count.

University recruiting to permanent academic positions remains low. It includes five professors. Temporary positions of all sorts are similar in absolute terms to previous years. The CAEs are back in the market again after an 'off' year in 1987.

There is a significant increase in positions in commerce and industry. It is encouraging to note that most of them have a stated 'research' content. One might be tempted to conclude that this section is at last coming to realise the value of research, were it not for the fact that it does no more than restore the status quo of a decade ago.

Across the board, for some 30% of positions, a PhD was a stated or preferred requirement—about the same as for the past two years. About half of these positions were permanent. As was pointed out last year, the annual output of physics PhDs in Australia is running at about sixty per year and the net flow into Australia from overseas is upwards of thirty. It would appear that supply and demand are just about in balance. Certainly students should not be deterred from taking physics on the grounds that there are few employment opportunities.

The author attended the British Institute of Physics Education Group Conference earlier this year. Over there the complaint was that the best physics students were, "going into The City" (Australian translation - taking up accountancy). Chapman (1989) discusses this phenomenon and the reasons for it (although you may not agree with his solution). There is much evidence that the appeal of the business world is also influential in career choice in Australia these days.

As has been the practice for the past four years, a list of all positions surveyed, classified by fields, and giving the job classification, a brief job description, whether a PhD is specified, the location, whether indefinite or limited term and the month of the advertisement, is being prepared and will be sent to all Australian physics departments, to careers officers in tertiary institutions and to government employment agencies. Copies are available to interested persons from the author.

Particular thanks are due to Sam Keany, Wong C-c and Argyro Tzekas for much work in collating the data.

References


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(a) To allow people involved in, or in any way interested in vacuum or the areas of its applications to come together in a united body. (b) To allow manufacturers of vacuum apparatus, or equipment related to vacuum techniques to join that body. (c) To act as Australia's representative in the International Union of Vacuum Science Techniques and Applications (IUVSTA). (d) To spread and share existing knowledge of vacuum and to advance the state of knowledge of vacuum and the areas of its applications. (e) To encourage the teaching of vacuum in educational institutions, and provide education in vacuum in the form of seminars, lectures, demonstrations etc. (f) To collaborate with other vacuum societies or bodies where consistent with the aims of the society.

Free membership of the VSA is offered for one year Membership of the society is offered on the basis of an applicant possessing a genuine interest in any activity involving vacuum science and techniques or an interest in any field of its application. This criterion provides the opportunity for membership of persons involved in the widest possible range of vacuum practice and enables vacuum users at all levels and those interested in any areas of vacuum application to participate in the benefits of membership. These include:

(a) provision of updated listings and contact information of other members and their fields of interest (b) provision of a quarterly News Information bulletin covering both local and overseas events relating to vacuum techniques and applications (c) the organisation of seminars, conferences and equipment displays relating to vacuum technology and its field of application (d) provision of short training courses in various aspects of vacuum technology and its fields of application (e) literature on vacuum related products and processes provided by corporate sponsors of the society.

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☐ Forming
☐ Packaging
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☐ Lamps
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☐ Vacuum education

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CSIRO:
WHAT WENT RIGHT? WHAT WENT WRONG?

By C.B. Schedvin

My text is taken from Voltaire. Writing in 1732 he declared:
"The historian who ventures to write contemporary history
must expect to be attacked both for everything he has said
and everything he has not said."

After many years delving into the history of CSIRO and pre-
decessor, CSIR, I know that there is no way of living a quiet life.
There are more opinions about the Organisation than there are
research scientists; strangely, overseas opinion is less diverse.
There are also considerable differences between users of
agricultural and biological research, and users of physical and
industrial research. Then there are the perennial differences
over science policy—ranging from extreme laissez faire to
solid interventionism.

In the same vein CSIRO's reputation historically has been
volatile. It began as a small Commonwealth authority in 1926
for the purpose of lifting performance in both scientific research
and industrial efficiency. The background model was the Max
Planck Institute in Germany, although organisational and
cultural features were derived from the DSIR in the United
Kingdom. It immediately ran into opposition from state
departments of agriculture, and to a lesser extent from some
university departments. The disappointment of the universities
was that the bulk of CSIR research was conducted in-house.
A number of prominent professors had hoped that the main
responsibility of the new Organisation would combine in-
house research with grant-making. It should be said, however,
that effective collaborative arrangements were made with
some universities, notably at the McMaster Animal Health
Laboratory in Sydney, Adelaide's Waite Agricultural Research
Institute, and the Veterinary Research Institute in the University
of Melbourne.

Overall it could not be said that CSIR made a great impact
before 1959, scientifically or industrially. Useful work on
animal and plant disease was undertaken, notably the empirical
discovery of the role of minor elements—copper, and zinc—in the animal and plant physiology. CSIR picked up
some more publicity in the early 1950s with the first major
element of biological control—the devastation of prickly pear
by Cacoblastus cactorum. In fact, CSIR had little to do with
this research which had been the responsibility of scientists
attached to the Queensland and New South Wales departments
of agriculture. The reflected glory was useful nevertheless, and
the successful control of prickly pear boosted the reputation of
applied biology.

Scientifically the most significant work was D.F. Martyn's
estimation of the temperature of the E and F layers of the upper
atmosphere, and perhaps also his observation of the correlation
between solar emission and ionospheric disturbance. As a
physicist with the Radio Research Board, Martyn was funded
jointly by CSIR and the old PMG; he worked in a university
environment, under the patronage of Sir John Madsen in
Sydney's Department of Electrical Engineering. It would be
fairer to attribute Martyn's success to the university sector.

It is customary to explain the great breakthrough in the
appreciation of science in Australia, as elsewhere, to the
stimulus of World War II. Broadly this is correct, but in the
Australian case the story is rather more complex. With the
exception of the secret radar work in Sydney and the
collaborative work on optical munitions, scientists were under-
utilised during the first two years of war. Then after Pearl
Harbour there was a desperate scramble to increase production,
and scientists were used mainly as technologists—in food pro-
cessing, minerals recovery, making slip gauges, tropic proofing
and the like. The radiophysicists were engaged mainly in
adapting radar equipment for the requirements of the South
Pacific. There is no doubt that the reputation of the scientist
was enhanced, but more as a technologist than as a research
scientist. This war-time confusion has tended to persist. Still,
at the end of the war, the perception of CSIR was rather
shadowy. It was known mainly within the governing elite, to
larger pastonists and a few manufacturers.

CSIRO's halcyon years were from the end of the war to the early
1960s. At that time it burst on the public stage with one
scientific achievement after another. Its international reputation
also spread, notably among radio astronomers and the much
smaller community of wool scientists. At a time when economic
development was an article of faith, CSIRO became the new
Prometheus. A remarkable cluster of achievements from a
range of disciplines emerged at about the same time. In the
agricultural and biological group these included biological
control of the rabbit by myxomatosis, apparent conquest of
semi-arid regions by the use of minor elements and generally
the pasture revolution (not all by any means the responsibility
of CSIRO), development of a number of vaccines for animal
use, and even the aerial survey of northern Australia. The
industrial and physical group contributed radio point sources
from outside the galaxy, the classification of solar disturbances
as discrete types, fundamental measurement in micro-
meteorology, and a range of improvements in wool processing
such as SIROSET. There was even the promise of weather
modification through artificial stimulation of rainfall.

It should be said at this point that the CSIRO model seemed so
effective and its achievements so ubiquitous because there was
no effective competition from the universities until well after
adoption of the Murray Committee report. The universities
did not begin to become competitive until the mid-1960s, and then
only in some disciplines. This was due of course to the fact that
education was the constitutional responsibility of the states,
and to the refusal of the states to fund research. Until the late
1960s, therefore, CSIRO had a degree of research concentration
in a single institution characteristic of scientifically undeveloped
countries.

There is no doubt that CSIRO's reputation has slipped badly,
even within the scientific community. Crisis has been endemic
since the Birch Committee report of 1977. Funding cuts have
been imposed since the mid-1970s, sharply since the advent of
the Hawke government. There are disturbing signs of a brain
drain to greener pastures in the US and elsewhere. Chiefs of
division speak openly of low morale, and there have been
obvious failures of management. A few weeks ago one senior
CSIRO scientist even wrote that the organisation is dying.
These difficulties should be kept in perspective. CSIRO retains a high international reputation and is well regarded within the rural community and more generally. Some of the breathtaker reflects anxiety about a major philosophical shift. But among Commonwealth officials there has been a dramatic decline in CSIRO's prestige, almost a vindictive campaign against scientific elitism and alleged economic irresponsibility of scientists. Despite the rhetoric I doubt whether the Prime Minister's science statement of a few weeks ago represents any real change in sentiment. Whether the criticism of CSIRO is justified or not, the fall from grace is undoubted.

It is difficult to pinpoint the antagonism of such contemporary history, but a few points can be made. Recent problems have their origin in the 1960s. This was a period of galactic change in research structure; the disposition of research resources was much the same as in the late 1940s when post-war problems dominated. There was much criticism of a lack of co-ordination, and of never-ending research projects. There was also criticism by ambitious university researchers angered by what was seen to be a two-class system, and irritated by the fact that research productivity was not necessarily higher in the first class division.

Within this context, there were two main events that over time resulted in a change in perception. The first of these was the sharp drop in wool prices at the end of the 1960s which undermined confidence in the Organisation's primary research mission, even since 1926, and more particularly, since 1945, wool research had been the backbone of CSIRO absorbing almost half of the total funds available. This was justified at least until the end of the 1950s because so little research had been done elsewhere and because of the productivity gains to be derived from such a large industry. But by the end of the 1960s wool had fallen greatly in significance, and it appeared that the Organisation had over-invested in wool-related research. Minerals had emerged as the most significant export industry, yet the deployment of funds in this sector had only begun. The same could be said of research of environmental significance.

The point about this is that it highlighted the difficulty of changing direction after a quarter of a century set in a particular track. Industry-related divisions could not be easily reemployed. The traditional autonomy accorded chiefs of division was a further roadblock, and there was the usual vested defence of territory. Ironically, university departments with their discipline-based research programs found the process of change much easier. Academic researchers were able to jump into new projects with much greater freedom.

The other event, or more accurately conjuncture of events, was common to all public authorities. This was the combination of the oil price shock of 1972/73, and the attempts by government in 1974/75 to combat rising unemployment by lifting expenditure. After more than half a century of public sector growth, this finally tipped the balance against further growth in public expenditure. Indeed, the rhetoric turned decisively against the regulatory and interventionist activities of public authorities.

This is, of course, a very general explanation and inadequate in itself. For the present I would like to delay further discussion, and return to it specifically with CSIRO in mind. First I need to retrace my steps and answer the questions: What went right during the first thirty or forty years of CSIRO's history?

Part of the answer has been mentioned already. Until the 1960s the Organisation had close a research monopoly in the applied biological and industrial sciences. The disparity was the greatest in the 1940s and early 1950s when CSIRO was at large as the university sector as a whole. Although CSIRO housing was generally poor by contemporary standards, equipment, materials and support staff were available in reasonable quantities. A good example was the range of electron microscopes accumulated by Industrial Chemistry's chemical physics section under Lloyd Rees in 1944/45 which gave the group a headstart in developing analytical techniques.

Perhaps even more important was the advantage of a national research institute in agricultural and applied biological research. Before the war, biology was still in its formative taxonomic stage, empirically grounded, and with relatively few even low level generalisations. Given the variability of biological phenomena, advantages were conferred on local researchers. These advantages were even greater when comparatively little work had been undertaken on the biologically challenging Australian environment. This does not mean that discoveries were lining up to be picked like ripe fruit in autumn. But it does mean that the environment offered considerable potential, and that the chances of making a discovery were relatively good.

One example will suffice. Soon after its formation in 1928 the division of Animal Nutrition was drawn into the investigation of a mysterious wasting disease of sheep known as 'Coast Disease', a condition that had puzzled pastoralists since the early days of settlement in South Australia. The disease was confined to the calcareous litoral that extended from Cape Otway to the Western Australian border. This suggested mineral deficiency, but the research opportunity had not been grasped earlier because of a lack of funding and expertise.

Having eliminated internal parasites as a cause, CSIR nutritionists worked through the list in a purely empirical manner, eliminating phosphorus, iron and copper. In the meantime a Western Australian group had discovered an apparent 'cure' for a disease with the same symptoms by using an iron compound. As pure iron oxide was not responsible, the group proceeded by elimination in much the same way as the Adelaide team. Both teams were making slow progress.

Eventually a chemist in the Adelaide team employed deductive logic to conjure that cobalt deficiency might be the cause of the wasting syndrome. The deduction was based on a reading of the literature suggesting that cobalt could lead to an overproduction of red blood corpuscles in rats, and on the known fact that cobalt was deficient in calcareous soils. The conjecture was highly speculative because cobalt was not known to have a role in rumen metabolism. Much later it was shown that cobalt deficiency could be prevented by vitamin B12.

The point of the example is that it was possible to make major gains by tackling specific local problems in a systematic way. The likelihood of success was enhanced if an interdisciplinary perspective was employed, as in this case by combining veterinary, nutritional and chemical understanding. In many instances CSIR was the first scientific organisation to use the team approach on problems of the Australian environment.

There are abundant examples of this kind in the early history of biological research. The development of new vaccines, discovery of the role of sulphur and molybdenum, and even introduction of the myxoma virus have features in common. The history of myxomatosis research was retarded because it was originally controlled by a veterinarian without an understanding of insect and virus ecology, but this is a minor point.

The early success of the Organisation has also been attributed to the ethical principles of research science introduced from the outset, notably by Sir David Rivett, the first chief executive.
officer. These were no less than the classical norms developed within the English tradition as a set of ideal standards of rationality. They included an emphasis on emotional neutrality and disinterestedness; the primacy of individual endeavour and rejection of authoritarianism; universalism in the transmission of knowledge; loyalty to the scientific community; and autonomy in the conduct of research and choice of programs. Rivett placed particular emphasis on individualism, autonomy and universalism. His commitment to universalism got him into trouble on several occasions. He was strongly opposed to the use of patents, and this led to the exploitation of CSIRO technology by overseas companies and to the loss of valuable revenue in the 1940s and 1950s. It also led to the rejection of secrecy in the conduct of sensitive military research during the Cold War, which led to the controversies of 1948, the transfer of the Aeronautics Research Laboratory to the Department of Supply, and to the change in name to CSIRO in 1949.

As a footnote, I might say that the transfer of ARL out of CSIRO was a minor tragedy for industrial research. ARL was the only significant engineering research laboratory within the Organisation, and the relatively high-tech capability that has been built up would have been invaluable for postwar manufacturing development.

I don’t think that there is any doubt that the introduction of these ideals was liberating. They were not new in themselves. They were part of the intellectual baggage of a number of science professors appointed in the nineteenth century and early this century. At the University of Melbourne Sir David Masson and T.H. Laby spring to mind. The point is that Rivett was able to back up his principles with a reasonable allocation of resources. Almost for the first time groups of scientists were able to work on significant national problems without interruption and over extended periods. The freedom was conducive not only to respectable science but also to a sense of mission and esprit d’corps. Elitism was also part of the process. Generally this was elitism in the best sense, in the sense of excellence; but it did have on occasion an arrogant side.

Of course, autonomy and individualism could become ends in themselves, and thus turn into conservative forces. I will return to this shortly.

During the Organisation’s first twenty years the full expression of Rivett’s scientific idealism was heavily constrained by the emergencies of depression and war. During the 1930s the immediate needs of agricultural industry had to be given priority. This usually took the form of research on a range of pests and diseases as an indirect way of moderating the impact of low commodity prices. There was some scope for more basic work, but not much. During the war the situation was even worse for reasons that have been described. By the end of the war Rivett was somewhat disillusioned. His own rule of thumb was that the Organisation should pursue pure and applied projects in about equal measure. (The division into pure and applied then had clarity that has long since disappeared.) During the war Rivett’s rule of thumb was no more than a distant hope.

The basic thrust of postwar policy was to shift the balance in favour of more basic and scientifically respectable research. As new laboratories and groups were established, or old ones reconstituted, discipline-based teams were formed. Wool research is a good example. When it was decided to invest heavily in wool textiles research at the end of the war, a range of organisational models was considered; but the one eventually chosen was based on three independent units—one industry based (Textile Industry), and two discipline based (Protein Chemistry and Textile Physics). The new unit at Aspendale for meteorological work had a strong basis in atmospheric physics. When Sir Otto Frankel was appointed in 1951 as chief of Plant Industry, his clear mission was to introduce the disciplines of plant physiology and genetics to the rather pedestrian work of that division. Similarly, Animal Health and Production were given two disciplinary groups—animal genetics and physiology. Finally, the radiophysicists and measurement scientists were training at the lash to break away from intellectually sterile wartime work.

It is still a little difficult to explain how Radiophysics was able to justify the solar and astrophysical work. The answer is a combination of clever footwork by Taffy Bowen, the chief of division, in including cloud physics as an applied programme, the tendency to defer to the expert, and the awesome contemporary reputation of physics as the standard bearer of high intellectual endeavour. In any case, the Radiophysics Laboratory was seen as primus inter pares as far as Australian science was concerned.

There is no doubt that this strategy was broadly correct. Australian science badly needed an intellectual boost. It is difficult to see how Australia could have become a serious part of the intellectual community or upgraded its industrial technology without a decisive shift in this direction. It should be said also that the strategy was highly successful, at least on its own terms. A large number of disciplines during the 1950s and 1960s achieved world recognition. These include radio-astronomy, cloud physics, micrometeorology, plant physiology, animal physiology, atomic absorption spectroscopy, and perhaps also animal behaviour in connection with wildlife research.

However, there were inherent problems, although these only became evident gradually—and probably would still be disputed. It was assumed rather too easily that the pursuit of academic science would lead automatically to technological progress in Australia. The historical record seemed to offer overwhelming support. In particular the record of the previous twenty years with examples such as atomic fission, radar and penicillin appeared to be conclusive. Underlying these empirical observations was an anarchic theory of the growth of knowledge. The growth of knowledge was unpredictable. From this theoretical perspective it was a short step to the normative principle of autonomy and individualism.

Because it was assumed that there was no knowledge-technology transfer problem, the discipline based divisions were often located physically and industrially at arms length from consumers. I use consumers in two senses: in the sense of the applied scientific divisions and the ultimate consumers, the industries themselves. Thus, research programs were inclined to take on a life of their own, and to take scientific criteria as their point of reference. For example, the more fundamental work of the division of Protein Chemistry eventually lost contact with the textile industry; the same is true of the division of Animal Physiology with its concentration on ruminant metabolism. Examples could be multiplied, but I have taken too many risks already.

This is not a criticism of academic science as such; far from it. But it is a comment on the structural organisation of science intended ultimately for material enhancement. If the intention is technological advancement, most of the evidence from the postwar period suggests that basic research will be most productive if organised in close association with consumers. Bell Laboratories is a classic case, as are the leading inter-
national drug companies. As is well known, scientists in these laboratories enjoyed almost as much freedom as their academic counterparts; but their orientation was slightly different and the scope for technology transfer much greater.

This does not mean, of course, that all basic research should be organised in this way. I am only referring to that part of the scientific enterprise intended for industrial purposes.

I now move to a quite separate matter, one peculiar to CSIRO. I referred earlier to the paucity of university research before the 1960s, and to the fact that one of the reasons for the creation of a Commonwealth agency such as CSIRO was to fill in the gap left by the university sector. This was acknowledged explicitly in the Science and Industry Research Act which allocated CSIRO a role in scientific training and the dissemination of scientific information. Others have argued that the reference to both science and industry in the legislation could be inferred to mean that CSIRO had a roughly equal responsibility for both science and industry, although I think that this view is mistaken.

The point is that by the end of the war CSIRO saw itself as having a responsibility for both academic and industrial science. Rivett was particularly pessimistic about the prospects for the universities as other than teaching machines, and this attitude rubbed off on many of his senior colleagues. And of course in the late 1940s and 1950s the situation was grim. Lecture halls were overflowing with CRTS (Commonwealth Reconstruction Training Scheme) students, and then by the lift in the participation rate among 18–21 year-olds. There was little equipment and no laboratories to speak of; research funds were virtually non-existent.

In these circumstances it is not difficult to see how CSIRO attitudes emerged, and how de facto binary system developed. That system was that CSIRO was responsible for research and the universities for teaching. It was a situation that prevailed from the 1930s to the early 1960s, and it has to be said that it accorded with CSIRO’s self-interest when research funds were so scarce. But it was obviously an unstable situation. No university system worth the name could tolerate the persistence of such a binary structure indefinitely. Carping by university professors began in the 1930s, and intensified in the 1950s. Antagonism about domination by CSIRO permeated the early history of the Australian Academy of Science. The chairman of CSIRO in the 1950s, Sir Ian Clunies Ross, was well aware that the situation could not continue indefinitely. He was a solid advocate of reform of the university system, and probably influential in setting up the Murray Committee on which he served. It has been described as a way of getting the universities off the back of CSIRO, but I am convinced that there was more to it than that. Clunies Ross had a vision of a rational progressive and scientifically literate society, and it was this that was the primary motivating force.

After Murray, and especially after formation of the ARGC in the mid-1960s, CSIRO and the universities were on a collision course. The issue was the responsibility for basic scientific research in the national interest. This was not an either/or contest; both sectors had legitimate claims. But what was at issue was the binary or two-class system. By the late 1960s the universities had made serious inroads into CSIRO’s dominance; by the mid-1970s the two sectors were roughly on parity. Even in the absence of an economic crisis, this was bound to force a reappraisal of CSIRO’s role.

Before drawing together the threads of this discussion, there is one critical issue that has not been covered. I have proceeded as if there was no difference between the agricultural and biological, and the industrial and physical. This is a distortion. Broadly A & B research has been successful both economically and scientifically; industrially I & P research is widely regarded as unsuccessful because of the condition of Australian manufacturing. My comments will be restricted to the industrial side.

The alleged failure of industrial research has been the source of much criticism, and a basic reason for CSIRO’s difficulties over the past decade. CSIRO is portrayed as having a large I & P group of divisions since World War II, yet has not been able to arrest the decline of manufacturing. Industrialists have complained about the lack of assistance from the Organisation, and there have been equally strident criticisms of the scientifically complacent and irresponsible attitude of corporations.

Both comments are true, and both miss the point. The main points are that industrial research barely existed in CSIRO, and that that observation should not surprise.

A brief survey of the I & P group as it existed in the quarter century after the war will illustrate the point. Sydney had Radiophysics, the National Standards Laboratory, and Textile Physics. NSL provided infrastructure research for a wide range of industries and offered some technical assistance, but its mission was functional standards not industrial R&D. Before Paul Wild took up the work that led to Interscan, the main industrial connection of Radiophysics was with the rural sector through the rainmaking experiments. In Melbourne the group comprised Industrial Chemistry, Building Research, Tribophysics, Meteorological Physics, and the wool laboratories.

Only Industrial Chemistry had pretensions in relation to Australian industry, and most of these were linked to the processing of natural resources—minerals separation, dairy research, the pharmaceutical potential of natural flora, bush fire research, evaporation control. Lloyd Rees had a clear vision to establish a scientific instruments industry, ultimately with some success; obviously this was a niche activity. The division of Textile Industry was the most successful group in delivering new technology. Ironically the main linkage was to European then Japanese wooden textile manufacturers because of the absence of any significant local industry; Australian wool growers were intended to be the ultimate beneficiaries by enhancing the international demand for wool.

The plain fact is that there was so little industrial R&D because of a lack of demand. When large firms undertook research, they did it in-house. Smaller (and indeed many larger firms) were either unable to meet the cost or were content to buy off the shelf.

CSIRO was well aware of the problem as early as the 1940s. Research associations were tried and failed. In the early 1960s a survey conducted by Stewart Bastow, a CSIRO Executive member, revealed the dearth of interest among industrialists. After a lengthy delay the matter was taken up by the Goron government in the form of a research incentives scheme, but the profits squeeze of the 1970s dashed hopes once again.

I have covered a large amount of territory rather discursively, and I must bring this to a conclusion. To answer the question “What went wrong?” a number of powerful forces converged in the 1980s to cast a shadow over the CSIRO model.

1. In the context of growing disenchantment with public expenditure, in the mid-1970s CSIRO reached the limits in real terms of Treasury appropriation. Growth could no longer be used to solving problems, and hard choices had to be made. This placed pressure on the ideals of autonomy and individualism,
and in some divisions morale began to decline. The pressure on resources and demand for accountability put further pressure on traditional ideals.

2. By the early 1970s there was a widespread perception that CSIRO research was inappropriately distributed with too great an emphasis on commodity production. These views were reinforced in the 1980s with the dramatic decline in the terms of trade and the pessimism about the country's economic future.

3. Concurrently the principles of scientific autonomy and individualism were under attack by economic rationalists inside and outside the bureaucracy. The insistence on autonomy was equated with self-interest. The excellence of much of the scientific work was admitted, but the scientific indicators were used to suggest that individual scientists were the main beneficiaries, not necessarily the society at large. Australian scientists were adding prodigiously to the stock of knowledge, but little of the benefit flowed back to the local economy.

4. The bureaucrats lost any faith they might have had in the capacity of scientists to undertake the necessary reforms; they had to be imposed by changing the structure of incentives. This applied both to the private sector and publicly funded science. What had to be done was to break down the institutional barriers. CSIRO had to be forced to develop links with industry by imposing funding cuts and by making it easier to establish joint ventures and to obtain substantial private funding. Industry was to be induced to lift its game by a combination of grants and tax incentives.

5. Finally, the binary system collapsed in the 1970s, and in the 1980s CSIRO was obliged to expunge disciplinary titles from the names of institutes and divisions. The divisions of Applied Physics, Mathematics and Statistics, Oceanography and possibly Entomology are the only survivors of the old disciplinary focus.

There is much more to the story than this. CSIRO had lacked managerial expertise and leadership; the Hawke government has taken a particularly negative and short-term view of the scientific enterprise, and so on. In the time available I have concentrated on long-term and structural features.

I would like to end on a cautiously positive note. While many problems remain, I think the corner has been turned. Whatever its defects, last month's science statement recognises that there is a continuing role for publicly funded science along CSIRO lines. The Organisation is more tightly focused than at any time since World War II. There is a recognition that commodity production will remain the core of the Australian economy for many years, and that CSIRO-type research has a continuing role to play in this sector. In a sense the wheel has turned full circle to the foundation years of the 1930s.

Boris Scheldvin is Professor of Economic History at the University of Melbourne. He is the author of a history of CSIRO entitled 'Science and Industry' (Alan Unwin, Sydney, 1987). This article is based on a colloquium given in the School of Physics at the University of Melbourne on 7th June 1989.

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**WORKSHOP ON MILLIMETRE WAVES**

A workshop on radiowave propagation and applications in the upper microwave and millimetre wave bands will be held in Sydney.

**DATE:** Monday 12 February 1990  
**VENUE:** CSIRO Division of Radiophysics, Epping, NSW  
**OBJECTIVE:** The aim is to bring together those interested in propagation characteristics of the frequencies above 10 GHz and to discuss the diverse applications of these bands.

Short (one page) summaries of previous, present, or proposed work in this area will be welcomed from each participant.

For further information contact:  
Dr Bruce MacA Thomas,  
Kieran J Greene, or Lynette H Loew  
Phone (02) 868 0222

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Telephone (062) 49 4620

Closing date for applications  
6th October 1989

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**Australian Physicist Volume 26, Number 9, September 1989**
OF INTEREST

Royal Society of New South Wales Edgeworth David Medal 1989

The Edgeworth David Medal is awarded for distinguished contributions by young scientists.

The conditions of the Award of the Medal are:
- The recipient must be under the age of thirty-five years as at 1 January 1989.
- The award will be for work done mainly in Australia or its territories or contributing to the advancement of Australian science.

Nominations are called for suitable persons who have contributed significantly to science; including scientific aspects of agriculture, engineering, dentistry, medicine and veterinary science.

Each nomination should be accompanied by a list of publications and a statement clearly indicating which part of the nominee's work was done in Australia and which part was done overseas.

Agreement of the nominee to his/her nomination must be obtained by the nominator before submission of the nomination.

Where it is unable to distinguish between persons of equal merit, preference will be given to a member of the Society.

Three copies of nominations and supporting material should be submitted, no later than 30 September 1989, to the Honorary Secretary, Royal Society of New South Wales, PO Box 1525, Macquarie Centre, NSW, 2113.

The Award will be announced at the Annual General Meeting to be held on 4 April 1990, and the winner will be notified at least two weeks beforehand.

Discontent Eventually Leads to to UTS Medal

When Alan Coelho left Wymum High in Queensland he enrolled in an electrical engineering degree course. Four months later he concluded that he was really only following in his brother's footsteps and that this wasn't to be his career path. After selling furniture in Sydney for a couple of years he decided to do a six-month computer programming course. While working as a systems analyst he began to think that he should have continued with engineering. As the result of a chance conversation with a physicist when enquiring about the engineering course at the University of Technology, Sydney (UTS), he enrolled in the applied physics degree. He crammed the 6-year part-time course into 4 years and was awarded the 1989 University Medal. He is now lecturing at the University while studying the structural analysis of ceramics for his PhD.

According to Alan the most important thing is being motivated. He says it's easy to study and to go to work when you like what you are doing, and work with very good people.

Teenager Awarded Certificate of Honorary Mention

Seventeen year old Gareth Williams of Singleton, NSW has been awarded a Certificate of Honorary Mention at the 1989 International Physics Olympiad in Warsaw.

Gareth was one of a five-man Australian team to finish around 17th against 135 of the world's best high school physics students from 27 countries.

The Olympiad included a five-hour theory examination on thermodynamics and gas laws, gravitation and classical mechanics, and relativity and dynamics. A five-hour practical examination was also taken to determine the velocity of ultrasound in two unidentified liquids.

Gareth, who is currently working towards his Higher School Certificate intends studying physics at university next year but fears he will have to go overseas if he is to pursue a career in science.

Acknowledgement

Newcastle Herald, 1 August 1989.

ANZAAS 59th Congress - Global Change and the Southwest Pacific

The 59th Congress of ANZAAS will be held in Hobart, Tasmania, from 14 to 16 February 1990. The Congress will take place on the campus of the University of Tasmania which will celebrate its centenary immediately preceding ANZAAS from 10 to 13 February 1990.

The theme, 'Global Change and the Southwest Pacific', has been chosen to coincide with the commencement of the International Geosphere-Biosphere Program (IGBP) by the International Council of Scientific Unions in 1990, investigating global changes of outstanding importance for the future of humanity.

The Congress will focus on the changing global environment with special reference to the Southwest Pacific. It will encompass not only physical, chemical, and biological changes but concomitant changes in human populations, the likely consequences for society, and the implications for government and planning.

The Congress aims to bring together Southwest Pacific nations' specialists interested in global change and will include specialist ANZAAS lectures, plenary lectures by distinguished scholars, paper sessions, symposia, and workshops.

Further information on the Congress including offers of papers, excursions, travel and accommodation may be obtained from the Organising Secretary, 1990 ANZAAS Congress, University of Tasmania, GPO Box 252C, Hobart, 7001, tel (002) 20 2099.

Welsh Foundation Scholarship 1991

A scholarship is offered to a promising scholar who wishes to contribute to the study of vacuum science techniques or their application in any field.

Conditions of Scholarship

The scholarship is offered for a one-year period starting 1 September 1990. If the candidate cannot begin work as scheduled, he/she can begin within three months after 1 September. In the case of a delay of more than three months, another candidate will be chosen. The laboratory where the candidate wishes to work must approve any delay in the commencement of work.

The scholarship holder is encouraged to seek funds in addition to the scholarship but should obtain authorisation from the Chairman of the Welch Committee of the IUVSTA before accepting additional funds. Traditionally, this authorisation has been granted.

The amount of the scholarship will be approximately US$12,500, paid in three installments—one of $6,000 at the beginning, another of $6,000 six
OF INTEREST

months after he/she has started work and a third of $500 on delivery of a final report after completion of the work. A brief mid-term report is required before payment of the second installment.

Applicants are asked to make arrangements for the proposed research program with a laboratory of their choice. Because of the international nature of the scholarship, strong preference will be given to applicants who propose to study in a foreign lab in which they have not yet studied.

A form outlining the research program and signed by the supervisor in the laboratory where the research is to be carried out must be submitted with the application to indicate the agreement of the laboratory and the proposed supervisor of your studies.

Candidates for the scholarship should have at least a Bachelor's degree; a Doctor's degree is preferred.

Applications should be addressed to Dr W.D. Westwood Advanced Technology Laboratory BNR, Box 3511, Station C, Ottawa Canada K1Y 4H7
Tel (613) 763 3248. Applications close 15 April 1990.

Australian Academy of Science 1990 Pawsey Medal

The Pawsey Medal was endowed to commemorate the unique contributions to science in Australia by the late Dr J.L. Pawsey, FAA. The Medal was first awarded in 1967, and is normally awarded annually. Its purpose is to recognise outstanding research in experimental physics by younger scientists. To be eligible, candidates' dates of birth must fall no earlier than 30 September 1957, and their research must have been carried out mainly in Australia.

Nominations of candidates should be covered by nomination forms available from the Academy secretary, and should include a curriculum vitae, a list of publications, and an evaluation of the candidate's work, including comment on his or her role in co-authored papers. Proposers should ask at least two referees to forward comments directly to the Academy, to arrive by the closing date of 30 September 1989.

Nominations are confidential and should be addressed to the Executive Secretary, Australian Academy of Science, GPO Box 783, Canberra, ACT, 2601. Telephone enquiries, (062) 47 5777, Mrs Hilary Back.

Superconductivity Workshop

Two-day Workshop covering both high-T, and helium-temperature superconductivity was held on 4th and 5th July 1989 at the School of Physics, University of NSW. The event was jointly organised by the UNSW School of Physics and the CSIRO Division of Applied Physics. Staff and students from universities, CAEs, CSIRO and ANSTO made up the bulk of the 80 participants, but industry and State Government were also represented. There was one overseas attendee, from New Zealand. The meeting was the latest in a continuing series of six-monthly events on this topic.

The prevailing atmosphere at the Workshop was quite different from that of two years ago, when room-temperature superconductive, levitated vehicles and loss-free transmission of electricity were, it seemed, just around the corner. Since those heady days, hard work and dedicated scientists' endeavours have replaced fanciful flights of the imagination (and let it be said, scientists as much as anyone else were caught up in the furore which made 'superconductivity' a household word for a few giddy months).

Now there is a healthy sense of realism amongst researchers, who remain strong in number, and a much more evident spirit of co-operation amongst

Twenty-first International Cosmic Ray Conference,
University of Adelaide, January 6-19 1990

The main features of the scientific programme of this conference have been released by the Chairman of the organising committee, Professor John Prescott.

The Opening Ceremony will take place in the Elder Hall at the University at 14:15 on Monday January 8 and will be followed by the opening reception.

Invited addresses by distinguished contributors will occupy most mornings of the first week. The topics are designed to be of general scientific interest while, at the same time, related to the main theme of the conference.

Invited Speakers:

- Prof. R.D. Cannon: Supernova 1987A
- Prof. L.O.C. Drury: Cosmic ray acceleration
- Dr R.D. Ekers: Radio observations of energetic particles in other galaxies
- Prof. R. Sunyaev: X-ray astronomy: Observations of SN1987A, X-ray binaries and active galactic nuclei
- Dr D.F. Malin: Photography and the discovery of the galaxy
- Prof. F. Hatzhen: High energy in interactions
- Dr T.D. Cline: The astrophysics of gamma ray bursts

In addition to the invited speakers, there will be a Highlight Session, chaired by Professor A.E. Chudakov of the USSR, on Future Gamma Ray and Cosmic Ray Experiments in Space, with contributions from Professors V. Schoenfelder, R. Sunyaev, Ed Stone and W.V. Jones.

Professor Ed Stone of Cal. Tech., who has been closely associated with the Voyager space mission, will also describe the encounters of Voyager with Uranus and Neptune. This will include film clips and will be open to the public.

Some one thousand contributed papers will be presented in regular and poster sessions under the themes: Origin and Galactic phenomena, Solar and Heliosphere, and High Energy. The conference will conclude with Rapporteur Sessions in which the main new result for each area will be summarised and the ongoing state-of-the-art reviewed.

A comprehensive social programme of evening and weekend activities has been arranged (bearing in mind the holiday season) and daytime activities with particular attention to accompanying persons.

A special reduced Observer Registration rate of $190 (regular rate $350) is available for those who, while not working directly in the field of cosmic rays or a closely related field, would nevertheless like to attend the Conference. This entitles the Observer to take part in all Conference activities but does not include a copy of the Proceedings.

Full details, including registration and accommodation information are available from:
Elliservice Convention Management, PO Box 753, Norwood SA 5067 or from the Conference Secretariat (08) 228 5996, Fax (08) 224 0464.
Asian Physics Education Network

Establishment
ASPEN was established in 1981, as a result of the recommendations of the UNESCO Consultative Committee Meeting held in Khon Kaen, Thailand, November, 1981.

Objectives
The objectives of ASPEN are:
- to contribute towards efforts in promoting the overall development of university physics education in the Asian region.
- to establish a programme of cooperation amongst members in physics education and related areas
- to establish effective channels of communication.
- to disseminate information on physics education and related ideas.

There are in all, about 20 countries in our region that participate in ASPEN.

Participating in ASPEN meetings (of which there are several a year) is a marvelous way for you to represent Australia overseas, and to make professional contact with our colleagues in the Asia-Pacific.

The next two meetings are:

Computer Software Development for Physics Instruction
6-9 December 1989
Department of Physics
Faculty of Science
Chiang Mai University, Chiang Mai Thailand

Physics Education Through Experiments
23-27 April 1990
Tianjin - China

If you have an interest in these meetings, or want to know more about ASPEN or participate in its activities please contact
Prof. Geoffrey I. Opat
Victorian State ASPEN
School of Physics
University of Melbourne
Parkville VIC 3052
Australia
COLD FUSION
A Learning Curve?

Lindsay Davis, Leader, Neutron Scattering Group
Australian Institute of Nuclear Science and Engineering

In 1866 it was discovered that palladium metal can absorb thousands of times its own volume of hydrogen gas. In 1932 deuterium and the neutron were discovered (and before that radioactivity, atomic structure, quantum mechanics, energy mass equivalence, etc). By 1939 we understood the proton-proton fusion energy process in our sun. In 1951 man “kind” produced uncontrolled nuclear fusion in the (heavy) hydrogen bomb. Since then probably hundreds of billions of dollars have been expended trying to control fusion of (say) deuterium nuclei with a net, useful energy output.

But if we stop this headlong rush for a few seconds we must admit we do not need fusion energy yet. For example, at the present rate of usage of the world’s non-renewable energy sources, fossil fuel (coal, oil) will last several hundred years, uranium (through nuclear fission plus breeding) several thousand years and deuterium (through nuclear fusion, when it works) tens of billions of years. (One part in 7000 of our water is heavy.) Even if we save our fossil fuel for chemical feedstock (for plastics, etc) we have plenty of uranium and we could run our cars on hydrogen.

But need fusion energy or not, we want it! I just hope that as scientists we will take on more responsibility for managing our creations. But I digress from our headlong rush. In fact, we already have a renewable source of fusion energy emanating from the sun. At the centre of our sun positively charged hydrogen nuclei (protons) have temperatures of tens of millions of degrees Kelvin. This gives them a reasonable Probability of almost touching by quantum mechanical tunneling through their long range coulomb repulsive barrier and fusing via short range nuclear forces.

Over the past few decades the major research effort in fusion has been directed at attempting to mimic the sun’s thermonuclear (“hot”) fusion. Plasmas of light nuclei and electrons of 10^18 deuterons per cm^3 must be heated to tens of millions of degrees Kelvin while being confined magnetically (or inerterially) in order for fusion to occur. I should note in passing that the number of deuterons per cm^3 in palladium would be 10^24 and we might accelerate two nuclei into one another or press them together with huge pressures or a combination of these forces to fuse them together. The difficulty (not the only one!) is getting a net, useful energy output from fusion.

Current wisdom indicates we will have fusion energy available in several decades. A good possibility might be that the final machinery will be modelled on, for example, the Joint European Torus, which has dimensions of tens of metres and costs ~2 billion dollars and confines hot plasma magnetically. It is difficult to compare such a colossus with a tiny glass electrolytic cell with a palladium cathode and conducting heavy water electrolyte.

On March 23 of this year, Professors Fleischmann and Pons (F & P) from the University of Utah (U of U) effectively did just that! Rather cheekily they claimed to the media to have achieved nuclear fusion in their cell (not a prison one yet) at room temperature. Such a circus would have been laughed out of town had it not been for the fact the both are highly respected electrochemists (Fleischmann is a “retiree” from Southampton University).

Dr Steven Jones and colleagues (Jones) from Brigham Young University (BYU) in the geographic vicinity of U of U, had also been working along similar lines to F & P. Apparently Jones had thought the BYU team and F & P would publish at the same time and there is a lot of mud in the water which I could spend this article on stirring. Suffice it is to say that the U of U managers were over-zealous to protect the patent potential of the F & P “discovery”. If you are interested in soap operas the movie comes out next year.

Jones claimed to have generated fusion with his electrolytic cell, too. But whereas the F & P heavy water electrolyte only contained lithium deuterioxide the Jones heavy water had a “soup” of iron, nickel, palladium, calcium, lithium, sodium and titanium metal salts with a pinch of gold cyanide for good taste. Jones was suggesting that “cold” fusion could be contributing to the heating of the earth’s interior. Jones was measuring very few neutrons (several standard deviations from background) but at the correct energy for the reaction

\[ d + d \rightarrow ^3\text{He}(0.82 \text{ MeV}) + \eta(2.45 \text{ MeV}) \]

F & P on the other hand reported thousands of neutrons per second and a corresponding number of tritium atoms detected per second generated by the reaction.

\[ d + d \rightarrow ^4\text{He}(1.01 \text{ MeV}) + \gamma(3.02 \text{ MeV}) \]

The real strength of F & P is their electrochemistry and they reported significant excess heating above “break even” in several cases.

But let’s hear what the 91 Australian scientists who attended the ANSIE colloquium on May 19 thought. Dr Roger Gammon, the Scientific Secretary, had arranged for eleven speakers to represent six universities, ANSIE, CSIRO and BHP-Utah.

Dr Boldeman (ANSTO, nuclear physicist) set the scene. He pointed out that a third nuclear reaction

\[ d + d \rightarrow ^4\text{He}(0.08 \text{ MeV}) + \gamma(23.8 \text{ MeV}) \]

occurs (normally) with a probability 10^{-8} that of the two reactions I mentioned above which occur with about the same probability. One might also get the reaction

\[ p + d \rightarrow ^4\text{He}(0.005 \text{ MeV}) + \gamma(3.49 \text{ MeV}) \]

if hydrogen is present.

His comment about the F & P Neutron detection system was that it was “extremely primitive” and he “would not put too much credence on their neutron emission” results. The Jones neutron detector had “some very nice qualities but had a very low efficiency”. Dr Boldeman described a Hungarian colleague who detected 0.4 neutrons per sec as being “very reliable” and an East German, Dr Zeitliger, who got 200 n/hour (allowing for 10% efficient detector) as having “superduper equipment”. ANSTO was collaborating with a large number of scientists who wished to use their neutron detection facilities which
would get down to -0.015 n/sec. Dr Mark Florence (CSIRO, electrochemist) thought F & P's most amazing omission was how they had treated the electrode. He thought deuterium and oxygen ignition was responsible for the F & P meltdown and reminded us that F & P claimed they exceeded 1:1 D:Pu. In his opinion F was a brilliant electrochemist.

Dr Guy White (CSIRO, Applied Physics) pointed out that in the seventies hydrogen (H) in palladium was "the flavour of the month". More than 80% occupation of the (octahedral) interstitial sites in face centred cubic palladium (Pd) by H (or D) induces superconductivity in Pd. At this concentration the pure beta phase has the same structure as Pd but is expanded 10%. Pd is "almost a magnet" and introducing only one iron atom in 1000 or 10,000 produces a (magnetic) polarisation cloud with an (unusually large) magnetic moment per Fe atom of ~11 Bohr of ~11 Bohr magnetons. As F & P mentioned H and D are very mobile in Pd and move between fairly shallow potential wells (at the octahedral sites). I might chip in that quasielastic neutron scattering indicates the H or D "atoms" (or p and d) spend ~several picoseconds "rattling" at each octahedral site before spending ~0.1 psec or less "jumping" to a neighbouring site. Whether a tetrahedral site is involved in this jump is not known.

Professor Noel Hush (Sydney University, theoretical chemist) pointed out that Sir Charles Frank had suggested cold fusion as far back as 1947 (in Nature). He discussed why F & P had rushed into press and knows Professor Fleischmann very well and says he is a "very distinguished electrochemist". He pointed out that the iron coating of the Jones Cathode would passivate the surface and explains why the reaction stops after ~8 hours.

Dr Peter Krug (Sydney University) proposed using a diamond anvil to reach pressures ~1 million atmospheres and planned to repeat the Italian "Frascati" experiment and subject titanium shavings to 50 atmospheres of D$_2$ gas at temperatures down to 77 K. Again I will chip in to point out that the Italians talked to F & P before the U of U managers clamped down. The "Frascati" experiment seems to give neutron "bursts" often when there is a pressure or temperature change. Dr Brotherton-Ratcliffe (Flinders University) pointed out that the cathode and anode bend significantly and could touch. Dr Terry Quickenden (University of Western Australia, chemist) pointed out that either constant current or constant voltage could produce "meltdown" if either the resistance increases or there is a short circuit (electrodes touching). His is the only group to attract funds ($20,000 from University of Western Australia) by May 19. Dr Trevor Ophel (Australian National University, physicist) was not a believer! Dr David Raszell (Melbourne University, physicist) had to "hunt around for half a day to find a beaker" and Dr Dom Schwinkens (consultant for BHP-Utah, electrochemist) gave a report on the Los Angeles Electrochemical Society meeting of May 8. At that meeting F & P showed a graph of a cell's temperature which "was constant at 333: 1/2°C for two months then increased to ~48°C with excursions to ~60°C for two days and then returned to 33°C". They admitted they should have measured He (and they since have, and have found He) and they would re-measure the gamma ray emission. Professor Alan Oates (Newcastle University, metallurgist) has been working on hydrogen in metals for a quarter of a century. He pointed out that one can get H or D into metals using compression (better at low temperature eg, Frascati), chemical overpotential (F & P and Jones), ~10 Torr monatomic H (D) but you must avoid recombination so. How much H (D) gets into the metal lattice is determined by the enthalpy term of the metal-H interaction. In some systems expansions of ~20% "powder" the metal. There seems to be a minimum distance of 2.1A between H occupied interstitial sites for all metal H systems. His recommendations are to use titanium in preference to Pd and perhaps increase the temperature to get higher jump frequencies and reduce the effective minimum distance of 2.1 Å. To induce non equilibrum effects he would try using ion beams, membrane gradients and phase transformations.

Finally, I summarise by pointing out that excess heat, helium, tritium, neutrons have all been reported but there is no consistency and my theory (which is not for this article) is as good as anyone else's (that bad, huh!). For those who have been unsuccessful either learn the tricks or give up! My learned judgement (for the Editor) is that many learned scientists with many learned judgements cannot all be right, nor can they all be wrong! Have you learnt anything?

The University Of Melbourne

LECTURER (CONTINUING) IN THE
SCHOOL OF PHYSICS

Applications are invited for the above position from experimental physicists with a proven research record and interests which overlap or complement one of the following fields:

Condensed matter physics
Detector development for particle physics
Modern optics and electronics

Information about the school is obtainable from the Head Professor A.G. Klein, telephone (03) 344 5420, fax (03) 347 4783.

Applicants should be willing to participate in the supervision of honours as well as postgraduate students. A genuine interest in the undergraduate teaching program is essential.

This position will be available as from 1st January 1990. Salary is in the range $31,259 - $40,622 per annum.

Closing Date: 30 September 1989.

Position Number: 6400051.

Further printed information regarding details of application procedure and conditions of appointment is available from Ms M. Petras on (03) 344 7546. Written applications should reach The Director, Personnel Services as stated below. Applications, in duplicate, including names and addresses of at least three referees and quoting the relevant position number should be addressed to The Director, Personnel Services, The University of Melbourne, Parkville, Victoria 3052.

An equal opportunity employer.

Australian Physicist Volume 26, Number 9, September 1989
Facit Introduces New Laser Printer with a Full Range of Accessories

Featuring a full range of accessories offering total flexibility and expandability, the new Facit P6060 workstation laser printer can be tailored to suit a very wide range of applications.

With its 0.5Mb memory capacity, a selection of six fonts and HP Laserjet Series II emulation, the standard P6060 is highly suitable for word processing.

The compact unit is based on a new "engine", that promises an excellent machine life of 300,000 pages. Print speed is 6 pages per minute.

Users can easily expand the font range by selecting from some twenty HP-compatible font cartridges or by means of downline loading. Facit offers three soft font packages complete with the necessary software. Forms, logotypes, etc., can also be downloaded and stored in the P6060 as overlays.

Printer memory can be expanded to 1.5, 2.5 or 4.5 Mb for applications involving large amounts of graphics and text, such as desktop publishing. Both serial and parallel interfaces are supplied as standard.

The printer can also be equipped with a sheetfeeder comprised of two 220-sheet bins plus one bin of 50 envelopes.

The Facit P6060 is one of seven new products recently added to the Facit range of workstation printers, a range based on three technologies - laser, inkjet and impact matrix.

For more information on the Facit P6060 laser printer, please contact Elmasco Instruments Pty Ltd:

Sydney (02) 736 2888
Melbourne (03) 879 2322
Brisbane (07) 875 1444
Adelaide (08) 344 9000
Perth (09) 470 1855

Digital Storage Oscilloscope Bends High Performance, Portability

Low cost, high performance and portability meet in the model 400 digital storage oscilloscope from Gould. The unit features a sampling rate of 100 Msample/s on both input channels for single-shot capture with 8-bit amplitude, 10-ns time resolution. For continuous waveforms, the instrument uses random time sampling at an effective frequency of 500 MHz, giving 2-ns time resolution.

Thanks to its small size, light weight, and ability to operate from ac and dc power sources, the scope is a highly versatile instrument for a wide range of laboratory, production and field applications.

A powerful range of trigger capabilities is included. As an aid to capturing the meaningful part of a signal, trigger delays of up to 5000 seconds are available with 20-ns resolution, so that maximum use can be made of the high timebase sweep rates. In addition, the waveform leading up to the trigger point can be viewed using a preretrigger facility with a range of zero to 100%.

On-screen cursors enable time and voltage measurements to be made quickly and accurately. Post-storage magnification in both X & Y directions is also available for rescaling and comparison purposes.

The model 400 digital storage scope measures 135 mm high by 277 mm wide by 389 mm deep and weighs 5.5 kg. The instrument costs $3595 excluding sales tax.

For further information on the product please contact any Elmasco Instruments office on:-

(08) 344 9000 Adelaide
(07) 875 1444 Brisbane
(03) 879 2322 Melbourne
(09) 470 1855 Perth
(02) 736 2888 Sydney

New Tektronix Software Automatically Characterises Signal Noise and Timing Jitter

Software for characterizing signal noise and timing jitter using Tektronix digital oscilloscopes and waveform digitizers is being introduced for use in design and manufacturing test and measurement.

Tektronix i-Pattern Software is optimized for telecommunications applications. It features timing and voltage histograms for digital circuit design and verification, real-time signal display for monitor-mode troubleshooting and adjustment verification and user-definable masks for pass/fail manufacturing testing and incoming inspection.

The software runs on MS DOS-compatible controllers such as Tektronix PEP 301 equipped with Tektronix Guru or National Instruments-compatible GPIB cards. It supports all Tektronix 11000 Series, 2400 Series and 2230 digitizing oscilloscopes, as well as Tektronix 7D20 and Sony/Tektronix RTD 710A programmable waveform digitizers.

Tektronix i-Pattern Software is used for measuring and conducting statistical analysis of signal noise and timing jitter. Real-time waveform data is stored in the controller memory for current and future analysis. For example, waveform regions can be recalled from memory and selected.
with cursors, analyzed, and compared to normal distribution.

In addition to showing cursor readouts on the controller screen, 1-Pattern Software displays readouts of the number of waveforms acquired, number of hits (samples within the user-selected region), mean value, standard deviation and peak-peak value in the selected region, and the percentage of hits within 1, 2 and 3 σ (standard deviation) of the mean value. Measurements can also be shown using either voltage or timing histograms.

Data can be displayed in a standard 2-D plot with colour (X-Y plot, with colour used to indicate intensity level), or in two types of 3-D displays: waterfall (an X-Y-Z histogram), and a “solid-fill” waterfall.

The software’s live waveform display enables a user to look at a sequence of acquired waveforms (up to 40) in a “variable-persistence” mode. In this mode, acquired waveforms remain on screen longer than usual for viewing and inspection. This mode is especially useful for the digital communications field.

**BWD Powerscope 881A**

Tech-Rentals now offer the recently released BWD POWERSCOPE 881A for rental. The 881A is an upgraded version of the 881 oscilloscope, which has found wide acceptance both nationally and internationally particularly in the power industry.

Although the POWERSCOPE was designed to meet the needs of the power industry, it can be used in any environment that requires a multi-channel display with a high voltage capability. Featuring four differential input channels, capable of measuring 1600 volts peak to peak with the standard probes provided, and an optional single ended input fifth channel, the 881A can meet the most demanding of requirements.

The sensitivity of channels 1 to 4 is 20 mV to 200 V/div and the maximum 3 dB bandwidth is 30 MHz, with the gain being adjustable in eleven steps. To enable fast rise time measurements, the vertical amplifiers have a 120 ns delay incorporated. Channel 4 has a D.C. offset provided, in the range 20 mV to 200 V per division. Channel 5, which is selected as an alternative to channel 4, has a sensitivity range of 100 mV to 10 V/div, in 6 steps, and a maximum 3 dB bandwidth of 50 MHz.

The output of each channel is available singly and as a composite signal via BNC connectors on the rear panel.

The time base on the POWERSCOPE ranges from 2 us to 1 s/div in 21 steps with a vernier between steps, which extends the slowest speed to 5 s/div. A 10X magnifier increases the time base speed to 20 ns/div. Triggering can be affected on any one of the channels 1 to 5 and a low pass filter ensures stability in the presence of noisy signals.

A unique feature of the POWERSCOPE is its ability to measure the phase angle between any two signals being displayed. It does this utilizing intensified markers and a 3 digit LED display. Measurements can be made in the range of 1 to 359 degrees of signals in the band 15 to 2000 Hz. The phase marker pulse can also be used to trigger the time base.

The POWERSCOPE will operate from a wide range of AC and DC voltages, weighs 10 kg and can accept a variety of optional current and high voltage probes.

For further information on the BWD 881A POWERSCOPE please contact your nearest Tech-Rentals office:

- **Melbourne** (03) 879 2266
- **Sydney** (02) 736 2066
- **Brisbane** (07) 875 1077
- **Perth** (09) 470 3644
- **Adelaide** (08) 344 6990
- **Canberra** (062) 57 4983

**New High Performance CPW Microwave Probes Provide Low Loss from DC to 40 GHz at Exceptional Value**

Tektronix answers the industry’s need for economical high performance microwave probing with the introduction of the TMP9600 series RF microwave probes.

Designed to be compatible with existing microwave probe stations including those manufactured by Alessi, Cascade Microtech and Design Techniques, Tektronix TMP9600 series microwave probes operate from DC to 40 GHz. They feature return loss greater than 10 dB and insertion loss less than 2.5 dB at 40 GHz.

Tektronix TMP9600 series microwave probes are precision adapters, converting coaxial input into ground-signal-ground, coplanar waveguide footprints that interface to hybrid microwave circuits, MMICs or microwave packages. Tektronix in-house thin film hybrid capability includes twenty years of experience in precision and high frequency thin film processing to build tapered coplanar transmission lines on low loss dielectric substrates. Contact from the probe to the device-under-test is made with photolithographically defined contact bumps. Probes are available in discrete pitch increments of 100, 150, 200 and 250 microns. Input to the probe is through a female K Connector. A female OS-50 connector is available as an option.

The Microwave Technology Organisation’s goal with this introduction is to provide a high-performance low-cost microwave probing solution for the cost sensitive manufacturing environment. In addition, the TMP9600 series microwave probes provide low insertion loss characteristics ideally suited to making noise parameter measurements. Unlike####################################################
L E T T E R S

IE (Aust) Responds

Dear Professor MacDonald,

In response to your editorial in the Australian Physicist (August 1989), I wish to make the following comments:

1. The Institution of Engineers’ submission to the Australian Research Council concerned high-priority research areas (about $10 million) for 1991, over and above the funding allocated by the discipline committees of the ARC.

2. To extrapolate the Institution’s recommendations for this limited area to a case “calling for a reduction (to zero) of funding going to basic research in Australia to applied or strategic research and to development” is clearly a mis-representation - one that I am sure you did not intend.

3. The Institution has never argued for a reduction in the nation’s basic research. Rather, it has emphasised to Government the need to maintain this effort, but to place greater attention on exploiting the results for economic and social development.

4. I am enclosing a copy of the Institution’s submission for your information.

5. As you said in your editorial, “current bickering exhibited in the public arena between scientists and engineers ... can be of no benefit to our image in the community”. In the interests of mutual understanding, therefore, I would be grateful if you would set the record straight in your next issue.

(Dr) Michael R.J. Dack
Director Public Affairs

Erroneous Credit!

Dear Editor,

In the July issue of the Australian Physicist I was erroneously credited with the authorship of an item entitled ‘ACT Branch Response to the Government’s Statement on Science Policy’, which began on page 171. As I did not write this article would you please find the correct name of the author and make a note of it in some future issue of the AP.

Tim Davis
CSIRO, Materials Science and Technology

Ed. Note. Apologies. The ACT Branch Response was unattributed.
Prompt Critical

Particle Physics for Non-Particle Physicists

When Bob Delbourgo reviewed Paul Davies' fascinating paperback 'Superforce' he concluded by recommending it to convey to non-physicists what physics is all about. He could have included most of us Average Busy Professional Physicists (ABPPs) who want to know what's going on without first ingesting a graduate course in gauge theory, quantum gravity, strings and the like. There are some other good books around, like "The Cosmic Onion" by Frank Close which impart some of the ideas, but hardly enough to satisfy an ABPP with the inquiring mind we are all supposed to have.

Well, now we have just the very ministr酌ger we ABPPs always wanted to let us appreciate where the big money goes in the world of physics. "Particle Physics - A Los Alamos Primer" is an attractive book describing in some detail the theoretical and experimental developments leading towards the elusive TOE: the Theory Of Everything. It is a well-illustrated collection of articles and lecture notes which address a great many of the questions likely to come from an ABPP who has no idea of the difference between a Cabibbo angle and a Weinberg angle, or even what the heck they are.

Not a lot of the questions raised in such popular books as "Superforce" or Hawking's "A Brief History of Time" are answered, but in many cases enough material is given to allow one to get a bigger idea of what's going on. I never realised, for example, how the dimensional analysis methodology I was taught many years ago has been a cornerstone in the efforts over the past couple of decades to unify all fundamental forces. On the other hand I was disappointed that the Los Alamos Primer threw no light on Hawking's concept of imaginary time. One can expect too much. After all, the book is primarily about particles rather than time.

Granted that the dozen or so authors of the notes and articles have all a major stake in the building of yet bigger and more expensive particle accelerators, their descriptions of the machines and the need for them make fascinating reading. What a thrill it will be to pick up tomorrow's newspaper and read that the Higgs boson has been discovered, or the first goldstein, familion or sparticle has been unmasked to send the theorists to their Crays in a frenzy of further creativity.

Apart from a couple of minor tyrants (which I've lost because I didn't mark them at the time) "Particle Physics - etc" seems to be fairly free of errors that I could detect. All the daggers seem to be in the right places and the numbers of indices appear to be balanced properly. There is something dimensionally funny with the preamble to an equation on page 176, but maybe I haven't yet got the point.

To cut a long review short, "Particle Physics - A Los Alamos Primer" will assuredly be at my side when I read the next book on the nature of the universe. A 280-page hardback, it was edited by Necia Grant Cooper and Geoffrey B. West, published by Cambridge University Press and available at the reasonable price of $A34.

Colin Keay
Book Reviews Editor

New Books Received

Siderius Nuncius, or the Sidereal Messenger

Superstrings: A Theory of Everything?

The Metallic Bond and the Structure of Metals
V. K. Grigorievich
Nova Science Publishers, Commack, NY, 1989, x + 311 pp, US$94.00 (hardcover)

Exactly Solved Models in Statistical Physics
R. J. Baxter

Heavy Ion Interactions in Statistical Physics
C. Signorini, S. Skorka, P. Spolaore, A. Vitturi (Eds)
Springer-Verlag, Berlin, 1988, x + 330 pp, DM59 (hardcover)

The Metaphysics of Quantum Theory
Henry Krips
Oxford, Clarendon Press, 1987, iv + 257 pp, $75.00 (hard cover)

This is another in a long line of books that attempts to provide a direct interpretation of quantum mechanics (QM). By a direct interpretation of QM I mean one which describes a 'quantum world' or QM metaphysics and selects some features of the present mathematical formalism of QM as providing the basic descriptors of the denizens of that world. Past proposed direct interpretations have included the obvious ones, particles (e.g. Lande, Popper) and waves (e.g. early Schrodinger), and of course the combined particle-plus-pilot wave (e.g. deBroglie). Next comes the more outriderish alternatives: latencies/potentiality (e.g. Margenau), quantum potential (e.g. Bohm-Bub), smear as such (e.g. Maxwell). All these direct interpretations, and a host of other twists and permutations upon them, take some part of the QM formalism literally and then attempt to deal with the host of difficulties and bizarre consequences which subsequently arise. Krips' book is in this tradition.

Of course it is not the only interpretative tradition. Neither Bohr nor Einstein, e.g., would have a bar of any of these direct interpretations. They both believed that QM called for a much more radical approach. Thereafter, however, they diverged in their views, Bohr re-working science as a semantico-cognitive enterprise and Einstein looking for a deeper theory to which QM would be but a flawed approximation. And there are still other views, e.g. the quantum logic tradition. For diverse reasons none of these approaches has proven wholly satisfactory (though each has changed our understanding of both QM and science in general forever). And so attention shifts back to the bizarre alternatives.

Krips re-introduces the idea that QM systems are to be "associated with fields, indeed probability fields, which pilot them; an idea which was, I argue, prematurely given up in the face of Einstein's criticisms. I shall take it that these fields are not described in terms of potential functions as they are in classical theory or in terms of metrics as they are in general relativity, but in terms of state-vectors or, more generally density operators", (p. 2).

In addition, Krips introduces the notion that QM quantities may be objectively indeterminate in value. These indeterminate quantities are, Krips insists, wholly new QM properties, they are not merely expressions of
BOOK REVIEWS

ignorance or some funny kind of QM statistical distribution across the usual deterministic quantities (pp 34–5). Thus Krips’s QM world or metaphysics. Krips adopts the principle (Bohr): a quantity Q in QM system S at t has a determinate value if the density operator for S at t is diagonal in the eigenvalues of Q (and the possible values are given by the eigenvalues). In addition, Krips argues that the converse ‘only if’ condition, if adopted as well, would lead to a contradiction (chapter 4). On this basis Krips argues that he can resolve the measurement problem without reduction of the wave-packet and also the standard paradoxes of QM, e.g. Einstein–Podolsky–Rosen and Schrödinger’s cat. One could look on this as deriving from a bag of logical tricks. The solutions to the above problems, e.g., rest on admitting that, besides objectively indeterminate properties conveniently present at the right times, “the only restriction on a pair of incompatible physical quantities… is to preclude both of them simultaneously having a particular value with certainty… (QM) does not preclude them both simultaneously having a determinate value” (p 102, Krips’s italics). So in Krips’s world position and momentum are sometimes objectively indeterminate and at other times can be simultaneously determinate. As Krips shows, actual inconsistency is strictly avoided as long as one adheres carefully to guidance by the QM mathematics. But of course Krips’s problems do not stop there. One has to be equally ingenious in dealing with n-body QM systems—where the reduced density operator for a component may be diagonal in Q but not the density operator for the n-body system—the problem of degenerate quantities of the Kochen–Specker mapping theorem associated with them, Bell’s theorem and its associated non-locality and so on. Krips is driven to various measures in order to cope with these central features of QM, in addition to the introduction of indeterminate quantities. He concedes, e.g., that a degenerate operator refers indiscriminately to the (possibly infinitely) many distinct non-degenerate quantities which its eigenvalues can be used to define and that “we should give up the equality between the probabilities of measurements registering particular values and the probabilities of the measured quantities possessing those values” (chapter 9). Whatever the merits of this position, there is a particular merit to Krips’s book worth noting, viz. its scholarly rigour. Krips’s chapters are filled with careful analyses of the traditional arguments, analyses which not only reveal their usually suppressed premises but which point out with particular clarity and care their connection both to the mathematical formalism and to their conceptual–metaphysical consequences. A wealth of recent technical argumentation surrounding the problems mentioned above is explored by Krips with care and clarity, and backed up by ten useful appendices on the von Neumann formalism, which should make the book of interest to physicists and philosophers alike quite independently of one’s convictions concerning the interpretation problem. On the other hand, there is a converse to Krips’s scholarly care, namely a certain scholarly narrowness. Krips often pursues his technical issues without regard for the larger scientific literature in which they are embedded. Thus, e.g., the revival of the pilot wave theory in recent years by deBroglie, Vigier and others goes unnoticed, as does Bell’s recent vigorous defence of it. Equally undiscussed is the related Bohm–Bub quantum potential theory, recently extended by Bohm, Hiley and others. There is no discussion of the profoundly non-classical character of bounded QM properties, like spin, in contrast to their unbounded counterparts like position and momentum, nor of their puzzlingly deep connection to relativistic theory. And so on. In a certain sense logical finesse is substituted for physical appreciation. But then this is perhaps one legitimate and distinctive contribution which a theoretically literate philosopher may make to physics.

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Following the award of a Generic Industrial Research and Development Grant by the Department of Industry, Trade and Commerce, the Optical Sciences Centre, in collaboration with the Plasma Research Laboratory, is embarking on the design and fabrication of planar waveguides and devices for application to optical telecommunications networks. A postdoctoral or research fellowship is available for a suitably qualified person to work on the theoretical design, which will also entail close collaboration with the experiment development. Further details can be obtained from Dr J.D. Love (phone (02) 49 4691, fax (02) 49 46911, fax (06) 42 49 1884). Applicants should be recent PhD graduates in physics, applied mathematics or electrical engineering, with appropriate analytical and numerical experience. The fellowship is tenable for three years only and is available from 1 December 1989.


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Group Structure of Gauge Theories

L. O’Raifeartaigh
Cambridge Monographs on Mathematical Physics
Cambridge University Press, Cambridge, 1986 ix + 172 pp, $32.00 (paper back)

Relativistic quantum field theory has had a long and chequered history. After the brilliant post-war successes of quantum electrodynamics there came nearly twenty years of the doldrums when it was thought that the experimentally revealed complexity of the strongly interacting particles necessitated new theoretical frameworks.

The last fifteen years have seen a renaissance of field theory in the description of the fundamental particles and their interactions, in the guise of renormalisable, spontaneously broken nonabelian gauge theories. This has come about partly through better theoretical understanding of the nature of these theories (spontaneous symmetry breaking and the Higgs mechanism), partly via the allocation of some of the difficulties to particular groups of practitioners (existence and consistency to the constructivists, confinement and the explanation of the hadron spectrum to the lattice modelers), and in no small measure through experiment (for example the evidence for the quark substructure of the hadrons, and for the W and Z bosons). There is a vast literature on physical and geometrical aspects of gauge
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theories: renormalisation, confinement, phenomenology, topology, instantons, and so on. O’Raifeartaigh’s monograph takes up topics not usually emphasised either in the original literature or in texts: namely, their group theoretical structure, and the structure of their spontaneous symmetry breakdown.

After introductory material on group theory, and detailed exposition of the gauge field and matter content of the ‘standard model’ of strong and electroweak interactions based on $SU(3) \times SU(2)$, the stage is set for the treatment of grand unified models (‘GUTS’) beyond the standard model, and the issue of their spontaneous breakdown. Once the existence of a larger GUT group is seen to be at least plausible on renormalisation group grounds (a leap of faith perhaps in that an extrapolation over several orders of magnitude in energy is involved), then a multitude of possibilities enter.

O’Raifeartaigh describes the simplest candidates and gives general ‘ground rules’ for building GUT models to test; for example the proton lifetime should not be too small—perhaps $10^{30}$ years for the reader to get to the end of this review without being fatally irradiated by decays within the body; $10^{15}$ years is the current experimental bound!

No GUT could work without spontaneous symmetry breakdown, and the strength of the book lies in the last two chapters devoted to strata, orbit structure and the minimisation of the group-invariant potential function (for the scalar fields). As the author mentions, much of the work in this field is pioneering, and there are few firm results. O’Raifeartaigh himself, and others like L. Michel, have made many original contributions.

Mathematical readers may be uneasy with any treatment of gauge theories which does not bring out their intimate connection with differential geometry. However, the central concerns of the book are with the classical and one-loop structure of the candidate GUT’s themselves, rather than with topological considerations (however important these may be in other contexts). In fact, given the concern with spontaneous symmetry breaking, the appropriate language might be the space of all connections, and associated mathematical constructs. In this light the innocuous-looking field redefinition used to implement the Higgs mechanism, $\phi = \phi_i + \phi_i$, is seen to be rather ill-defined geometrically.

A more satisfactory formulation has been given by Vilkovisky (and also deWitt), and the resulting effective action turns out to have formal advantages (gauge invariance) over the naive version.

This is a reasonably-priced book for theoretical physicists and applied mathematicians working in field theory. It may also form a useful source of additional background material for teaching group theory and classical field theory. It has comprehensive references and suggestions for further reading, and a glossary of terms in addition to the index. I found a glance through the problems at the end of each chapter instructive. Graduate students may even be able to do them!

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Acoustooptic Devices and their Applications
L.N. Magdich and Y.Ya. Molchanov (translated from Russian by D. Parsons)

This book is written in the style of a review article, with copious reference to the published literature. As such it will be of value to the active researcher in acoustooptic devices as well as to users of these devices (e.g. for information processing or photo-excitation studies of atoms and molecules involving modulated laser beams). However, it could be rather disconcerting to the uninstructed, and I do not believe that the book is entirely suitable for higher level undergraduate students in spite of the publisher’s assertion in this regard.

The volume begins with a theoretical overview (chapter 1) of the acoustooptic interaction, which refers to the diffraction of light by an ultrasonic wave in its medium of propagation. These ideas are then adapted to the design principles of corresponding devices for controlling optical radiation in their appropriate settings (chapters 2-5). Chapter 6 deals with the author’s area of special interest, on methods of fabrication of piezoelectric transducers. The final chapter examines the characteristics of some promising acoustooptic materials for visible and infrared radiations, with a useful table of various physical properties relevant to such devices. It is unfortunate that this English translation of the original 1978 Russian edition has only recently appeared in print. As a consequence the admirably comprehensive bibliography is somewhat out-of-date.

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Speakable and UnSpeakable in Quantum Mechanics
J.S. Bell
Cambridge University Press, Cambridge, 1987. xii + 212 pp, $30.50 (paper back)

This book contains twenty-two reprinted articles from physicist John Bell. The articles focus around the clarification of basic issues in the interpretation of quantum mechanics (QM): locality, Bell’s theorem and the Bell inequalities; no-hidden-variable theorems; the place of the de Broglie-Bohm pilot wave theory.

Bell’s writing is marked out by its clarity and stylishness. Pretty examples, witty anecdotes, erudite quotations and humorous asides abound. All serious students of physics should read them, first to
convince themselves that interpretational questions can be treated clearly, hard-headed and in a substantive way. Second, to realise that traditional issues are alive and highly relevant (and that great men do not necessarily have everything right) and thirdly to see how elegantly good science can be presented.

A good example of Bell's writing is essay 16 in this collection, "Bertelmann's Socks and the Nature of Reality". The focus here is on the QM statistics of the spin correlation situations typified by the Bohm spin half version of the Einstein-Podolsky-Rosen (EPR) Gedanken-experiment. Bell offers a lucid explanation of why precisely these statistics must conflict with any causally local theory of those situations. There is of course nothing anti-classical or anti-local about them being correlations, it is the particular correlation value yielded by QM that clashes with classical theory. Bell sets up the problem with the perfect anti-correlation of Bertelmann's decidedly non-pedestrian pedant haberdasher.

No pedantic pedagogy, this; since these anti-correlations have a causally local explanation (originating, no doubt, in Bertelmann's social pedagogy). The socks then reappear to carry our intuition smoothly across the classical assumptions from which are derived the experimentally testable statistical inequalities of the Clauser-Horne-Shimony type. Bell explains this beautifully and provides a generalised derivation of the relevant formulæ, which now generally and deservedly bear the title of the "Bell inequalities".

It was Bell's genius to have abstracted the essentials of Bohm's spin version of the EPR argument, formulation in the context of the hidden variables debate, and on that basis to have provided an elegantly simple argument for the conditions under which the QM description parts company with a Furry-type factorisable description (first given by Furry in 1936) appropriate to a definite, physically independent reality for each of the system components.

Furry makes the minimal alteration to the QM formalism which is possible, simply introducing to the usual QM apparatus a factorised expression for the calculation of probabilities. Bell, by contrast, does away with all reliance on the QM formalism per se, except that part of it which it shares in common with certain very general assumptions of the probability calculus. This is what provides Bell's argument, with its greater strength and generality, not only with respect to Furry but equally with respect to von Neumann and related no-hidden-variables 'proofs'. It led to a fundamental critique of those arguments and once again all this is explored and explained with elegant clarity across several essays. Precisely Bell's success in this regard has served to focus attention on the issue of locality as a, even the, primary feature of QM for interpretation.

In this last remark I alert the reader to a certain narrowness of focus in these essays, a narrowness made seductive by its tacitness and elegance, a narrowness made dangerous by the corresponding narrowness of the current interpretational debate at large. In essay 16, e.g., Bell does not alert the reader to the extent to which the QM notion of intrinsic spin is a strange, decidedly non-classical property to begin with, quite aside from its appearance in these special correlation situations. Here is a property whose components do not commute and yet which shows only specific, finite values when 'measured'. Indeed, it is, one of the few features of observables with a finite value, discrete spectrum. Nor does Bell alert the reader to the deep, if not clearly understood, connection between this peculiar property, statistical mechanics and relativity theory. In short, while what is shown is shown with elegant simplicity, it is only a small, highly selective part of the complete story.

Nor do I think Bell is penetrating enough in his critique of Bohr. At the close of essay 16 Bell complains that he cannot understand Bohr's reply to EPR. But he makes no attempt to penetrate the relevant part of Bohr's theory of the operational presuppositions of descriptive concepts. And Bell's preoccupation with locality focuses his questions down simply on to whether the Bell inequality rejects non-locality. This evidently prevents him from noticing, e.g., that Bohr's mere appeal to the exchange of quanta cannot in itself explain even why the measurement of one observable should randomise the outcomes for observables corresponding to non-commuting operators, let alone explain the peculiar non-classical QM conditional probability distributions, among them those which Bell so clearly presents.

These wider considerations suggest that there is some deeper physical, and related methodological and conceptual, insight which as yet eludes our grasp. Here inserting Bell's essays back into the history from which they emerge may help our focus, if not provide a complete solution. Bell-type arguments about 'local' theories of composite systems, as well as the no-hidden-variable arguments, ultimately derive from the dispute between Bohr and Einstein over the interpretation of QM. This dispute was focused on the disturbance conception of measurement and on QM discreteness and separability. It was not in itself focused on hidden variables or locality.

One significance of Einstein's arguments, culminating in the EPR paper, was that it convinced Bohr that the disturbance doctrine was insufficient to explain the phenomena. The focus for Bohr then shifted to wholeness or, equivalently, inseparability. The Bell notion of non-locality is still an attempt to set a causal sequence into a space-time setting, something Bohr would reject arguing that physical circumstances (quantised interaction) make it conceptually incoherent to formulate a workable notion of non-locality. Even so, between the EPR argument also distorts the basic thrust of Einstein's criticisms of QM, which concerns the ambiguity of QM specifications. He argued that differing QM states must be assigned, e.g., to one component of an EPR composite system, depending upon the choice of the kind of measurement made on the other component. But if two or more mutually distinct descriptions are descriptions of the same physical reality, then none of those descriptions can in themselves be complete. It was precisely this form of argument which Schrödinger responded with such vigour in 1935, and Furry extended in 1936. For Schrödinger the 'entanglement' of quantum systems was its most basic and most unorthodox feature and he drew attention to the difficulty of reconciling this wholeness with relativity theory.

One could respond to these problems by attempting to augment the existing quantum theory in some way, the so-called 'hidden variables' program. (As Bell remarks, calling these parameters 'hidden', as opposed to simply 'additional', is a "piece of historical silliness".) But Einstein's own response was to conclude that QM was much more radically incomplete and to search for a deeper theory. It was out of this exploration of wholeness that Bohm, Einstein's student, developed the spin version of the EPR system. This has led both to the Bell arguments, and to Bohm's own 'hidden variables' interpretation of QM from which Bell began his own attack on the hidden variables proofs and his defence of the deBroglie pilot wave theory.

Bohm's own hidden variable theory nicely illustrated QM wholeness, for the quantum potential which Bohm invoked to provide a 'deterministic' specification of measurement results,
nonetheless must be re-specified for each different QM representation of the measured system, just as Einstein argued.

This discussion mentions only a few aspects of the full QM riddle. We shall eventually have to understand QM wholeness in all its multifarious aspects. Meanwhile, Bell’s essays focus some of the issues involved with exquisite sharpness.

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**Apology.** The following two reviews were partially transposed in the August issue.

Theoretical Physics on the Personal Computer
E. Schmid, G.Spitzi and W. Lösch
Springer-Verlag, Berlin, 1988
xiii + 211 pp, DM 79 (hardcover)

This book is accompanied by a disk for use on IBM or compatible PC/AT computers. All the programs are in FORTRAN 77 and are written for use with the IBM Professional FORTRAN compiler, although in a preface the authors indicate that a disk with modified graphics software for the Microsoft FORTRAN 77 compiler is available from them. The authors discuss briefly their choice of FORTRAN, with a recognition that it may be criticised. I am one who would question its choice as the basis of a text book for teaching new generations of physicists.

As a text book it is quite thorough, starting with chapters on mathematical techniques for numerical differentiation and integration, progressing in difficulty and sophistication from the trapezoidal rule to the Newton-Cotes and Gauss-Legendre methods. In each case the limitations and inherent errors are emphasised. (The authors recommend the use of double precision arithmetic for all these numerical methods and this is done with all programs in the book.) A fairly detailed discussion of the programming steps required to implement the mathematics in FORTRAN is included in all chapters. Theoretical physics problems considered in later chapters cover a considerable range, including classical mechanics, quantum mechanics, electron optics and heat.

The program supplied for the first problem allows the user to vary at run time only four of the eight parameters involved—the program must be edited to vary the other four. Perhaps this is deliberate, to force students to come to grips with changing the programs themselves, but it is inconvenient for anyone wanting to look at or use the program quickly, with a minimum of effort. The electron optics chapter confines itself to the simple (symmetrical) electrostatic lens, but a consequence is that the graphics rapidly become boring, all equipotential plots having essentially the same shape, regardless of the parameters, such as the lens tube diameters.

If you want to teach numerical methods in theoretical physics to third of fourth year students using FORTRAN this book is worth considering as a text. Personally, I would have preferred the programs to have been written in Pascal or a modern version of Basic, such as True Basic.

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Quasielastic Neutron Scattering
M. Bee
Adam Hilger, Bristol and Philadelphia, 1988
xii, 437 pp, £60.00 (hardcover)

This clearly written, readable book is about the low energy dynamics of molecules (including atoms) in condensed matter. Diffusing or reorienting molecules give time dependent 'quasielastic' neutron scattering (QENS). This is a kind of doppler broadening of a spectral line that would be purely elastic except for the relative motion of the molecule and neutron during scattering. The energy transfer to or from the neutron is typically less than several millielectron volts (meV). The time scale of interest is longer than picoseconds and each 'jump' is over interatomic distances or fractions of a nanometre. 'Cold' neutrons with energies of several meV or less and de Broglie wavelengths near interatomic distances make sensitive probes for such investigations.

The author has developed the scattering laws for just about every model of dynamic behaviour studied by QENS. The bulk of this work has been on molecular orientational disorder and this accounts for about a third of the book. The potential energy barriers to rotation arise from intermolecular interactions, information about which is of fundamental importance to chemistry.

The first third of the book is on neutron scattering theory and the special facilities at the Institut Laue Langevin (I.L.L.) in Grenoble, France. The high fluxes of cold neutrons there allow special high resolution equipment to provide usable intensities down to resolutions of tens of nano electron volts (with the spin echo spectrometer). This has helped accelerate the development of QENS over the last decade.

The remainder of the book gives detailed examples which might induce physicists, material scientists and biophysicists to develop new areas of QENS.

Of current interest may be the diffusivity of hydrogen in palladium. The proton of the hydrogen atom spends several picoseconds vibrating at each octahedral interstitial site in the palladium before jumping to a neighbouring site in a tiny fraction of a picosecond.

Common garden variety (and supercooled to -20°C) water molecules have a similar 'rattle and jump' motion it appears, although the jump time taken to get to a new site is over hydrogen bonded with neighbouring water molecules is closer to the 'rattle' time.

Studies of adsorption of molecules like benzene in zeolite 'cage' structures, chain motion of large polymer molecules in solution or in the melt and local conformational motions of biopolymers are also of potential interest to the reader.

The huge nuclear spin incoherent scattering 'power' of hydrogen has been used to advantage in QENS in many of the problems tackled so far. There is still a lot of interesting physics to be sorted out using QENS and I can highly recommend this volume to the would-be participant. Congratulations to the author!

R. Lindsay Davis
Australian Institute of Nuclear Science and Engineering

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Prof. G.J. Opal, School of Physics, University of Melbourne, Parkville, 
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Sept 25-27  SSA IMACS 1989 Biennial Conference on Modelling and Simulation, ANU. 
John Jakeman (062) 49 4742 or David Green (062) 49 4020.

Sept 25-29  ACOLS '89 — Incorporating the Australian Spectroscopy Conference, Australian 
Warren Lawrence, Flinders University (08) 275 2029

Oct 3-14  Sectional Meetings of the Physical Society of Japan. 
Yoko Iwayanagi, Physical Society of Japan, Kikai-Shinko Building, 
3-5-8 Shiba-Koen, Minato-ku, Tokyo 105, Japan.

Conference Manager, Institution of Engineers, Australia, tel (062) 706 549

Oct 10  Remote Sensing Meteorology & Climatology (Co-sponsored by AMOS and AIP.) 
C.M. Platt, CSIRO, Aspendale Vic. (03) 586 8665

Dr Richard Thompson, IPS (02) 269 8613.

Conf. Manager IE (Aust) 11 National Circuit, Barton 2600.

Dec 3-6  14th Australian Conference on Optical Fibre Technology, Brisbane. 
IE (Aust) (02) 327 4822.

Ms Ann Whittaker, (02) 887 8204.

Dec 6-9  Asian Physics Education Network Regional Workshop II on 
Computer Software Development for Physics Instruction. 
Dr Samran Lacharojana, Head, Physics Dept. Chiang Mai University, Chiang Mai, 50002

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Secretariat, Department of Physics and Mathematical Physics, 
University of Adelaide, 5001.

Jan 15-Feb 2  3rd Physics Summer School on Nuclear and Particle Physics, Canberra. 
B. Robson, ANU (062) 49 2971.

Jan 29-Feb 2  9th AIP National Congress, Perth. 
Dr R.A. Anderson, Dept. of Physics, UWA, (09) 380 2738.

Feb 4-9  Chaos in Australia Conference, University of New South Wales 
Gavin Brown, University of NSW, PO Box 1, Kensington, NSW, 2033

Feb 5-7  6th Gascous Electronics Meeting, Armidale NSW. 
M.P. Fewell, Physics Dept., University of New England, Armidale, NSW, 2351 (067) 73 2388

Feb 5-9  Conference on Solar, Terrestrial and Space Physics, Melbourne. 
Dr Essex, Physics Department, LaTrobe University, (03) 479 2644.

Feb 6-9  5th International Symposium on Acoustic Remote Sensing of the Atmosphere 
and Oceans, New Delhi, India. 
Dr S. Singal, Ctr- NPL, New Delhi, 110012.

Feb 6-9  Fourteenth Australian Institute of Physics Condensed Matter Physics Meeting. 
T.J. Bastow, CSIRO, tel (03) 542 2777, fax (03) 544 1128.

Feb 12  Workshop on Millimetre Waves, Sydney 
Dr Bruce Thomas, Kieran Greene, or Lynette Loew, tel (02) 868 0222

April 1-6  Government, Engineering and the Nation, Canberra. 
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