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PRESIDENT'S COLUMN

Employment Service Trial

At last our employment service is about to begin on a trial basis. With the help of a volunteer in each state we should be able to provide any member of the Institute who requests it with a list of the most recently advertised positions in physics. This initiative is a copy of a successful scheme used by RACI and as well as being of direct use for job seekers is also used by some departments to indicate to undergraduates the sort of positions available. Details of how to register will be given in future editions of the Physicist.

On a somewhat different tack, I am pleased to report on my experiences at the recent Council, AGM and 5th National Conference of the New Zealand Institute of Physics. These were all held in Auckland in late August. The new executive of NZIP will be based at Waitako and it is a pleasure to congratulate the new president Crispin Gardiner on his election. The new Hon. Sec is Alistair Steyn-Ross and the Hon.Treas. is Reg. Harold Round. I wish all of them the best in their important new roles and look forward to working with them as well as we have been able to work with the retiring executive.

The Conference itself was a stimulating experience with a number of absolutely first-rate speakers. Education was a major focus of the meeting and two of the plenary speakers dealt with this topic. Roy Glubber, known to many for his work in high energy physics and to others for his work in quantum optics, spoke about lecture demonstrations and showed us a number of his best. Lilian MacDermott of the University of Washington reported on the research of her physics education group, presenting some startling examples of how mistaken we can be about the understanding of basic concepts that our students develop.

In order to reduce the clash with our Congress the NZIP Conference has been moved to odd years, with the next major meeting in 1995. (However the Waikato group may hold a smaller meeting before then.) I trust that this will make it easier for both sides to boost attendance at these important cross-disciplinary meetings.

A. W. Thomas

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EDITORIAL
A Feeling of Optimism?

Taking over the Physicist from Ron MacDonald when it is in such good shape engenders a feeling of optimism but isn’t optimism out of phase with all the gloom and doom that seems to dominate discussions of Australian support for the universities and scientific research institutions? They are being pressed to move away from tedious long term aims in favour of quick fire projects which show results in the current financial year and provide much better photo opportunities. Universities and CAE colleges have suffered the indignity of needless shotgun weddings, without showing the slightest signs of pregnancy. CSIRO has been investigated and reorganised out of its mind and most of the outstanding new ARC physics projects will not be funded next year.

Our Australian reaction is to agree with Hanrahan that “We’ll all be rooned” but pessimism is not the natural attitude of physicists. Would a pessimist try to understand the stuff of which the universe is made, where it came from and how? Experimentalists who hunt for a few neutrinos against a background of billions of other things aren’t pessimists. However even if we do naturally incline towards optimism, don’t the present harsh realities make this attitude unrealistic? A young physicist, whose outstanding project was not funded, is unlikely to be cheered up even if you do point out to him that although Galileo did suffer a certain amount of personal inconvenience, his ideas eventually won out. Does it matter anyway if we are optimists or pessimists, or do we just agree with Cabell that “the optimist proclaims that we live in the best of all possible worlds; and the pessimist fears this is true”. It does matter because a pessimistic course of action tends to be self fulfilling or, in management speak, is a sub optimal goal orientation.

We have made progress in physics by continually trying to find out what is, rather than what ought to be. It is tragic to see an elegant theory kicked to death by a brutal gang of facts but it happens not infrequently. We don’t excuse wrong results by pleading that the Gods have been unkind. Even if they have been, there is not much we can do to make them more benevolent. They not only play dice with the universe, they throw the dice under the table where you can read the numbers. It is easier to whip off a quick prayer than rebuild the equipment, but we all know which approach is likely to be more successful. The prudent experimentalist would do both and so should we when it comes to funding. We are right to continue to protest but we should not expect too much to come from complaining. To a politician whingeing is associated with losing and is to be expected. In fact too much whingeing makes us appear losers even when we are not. Justice and fair play, even level playing fields, are OK for political speeches but it is pressure and numbers that produce action, which we as physicists should understand and learn to direct.

Lack of support for physics is partially our own fault. This may not be true but lets treat it as we would any theory. It doesn’t violate any basic laws. It is plausible and we can test its validity. If, on the other hand, we believe we are the helpless victims of an unjust system and abandon ourselves to weeping and gnashing of teeth we may feel better but we will not improve things, nor even find out if we are right

The physics profession includes a wide range of independent, dedicated, talented and intelligent people but we have little political influence compared with say the professions of law and medicine. Aside from the fact that trying to organise physicists politically is like herding cats, our main political disadvantage has been that we have had little direct contact with the public. If you are about to be put away for a few years for fiddling the funds or worried about being sent early to join the great majority by a defective heart valve, you naturally approach your lawyer or your medico with a degree of hope, concentration and financial generosity rarely found in someone wanting a physicist to solve a heat flow equation.

So lets be realistic, we will never have that sort of influence.

We can however all do more to countermand the unfortunate image that many people have of scientists in general and physicists in particular through the media, schools, clubs and other contacts we have. Suburban papers for example always need something to fill the gaps between the ads. They will be delighted to get something from you, especially if it is free. It is surprising the effect a few paragraphs can have. Tied to the human interest angle that you live in the area, people will read your piece who would pass over it in a daily paper. This is what we need, not preaching to the converted but spreading the word to a larger audience. The time for making the point that physics is mostly benevolent and essential for our society has never been better. The public is being forced into realising that conservation laws exist. That resources are finite both environmentally and financially. Our governments have made so many bad decisions based on political or economic “truth” that our options are becoming rapidly more limited and it is time for reality to be given a chance. Short term greed is now seen to be not a viable economic policy. We must, for the survival of a reasonable standard of living in this country, try to redress the balance between those who make and those who take. There is even a growing awareness that if you spend hundreds of millions of dollars on legal inquiries into everything that has gone wrong recently you will have no money left to ensure that some of the important things are done properly in the future.

Doing this missionary work can initially be more difficult than writing yet another paper on oscillating thingatrons but it is certainly more necessary at present. Why not do both?

When we produce this swing of opinion in a sufficient number of voters our political leaders will follow as they always do. I must stop. I didn’t mean to be this optimistic.

Jak Kelly
LETTERS

Job Prospects for Physicists

Dear Editor,

Prof. J. R. Prescott’s 13th annual survey of job prospects for physicists was summarised in the Weekend Australian (25 July) under the headlines “Blossomng disciplines offer increased scope for jobs. Science opportunities buoyant”. “Physicists can celebrate”, rang the caption to a photograph of Prescott. In the text (and in a smaller typeface) we read “in a perverse sort of way, physicists can celebrate the fact that we are better off than many and that the profession does not share other fluctuations shown by some other professions, for example, by architecture.” Recent demand for physicists is said to be at about two-thirds of the long-term average.

The demand and projected (desired?) demand for physicists has been of discussion and conjecture in the A/ANZP for years. Numbers of job advertisements are claimed to be a measure of demand, and have been used to advocate university physics courses, along with spurious estimates of an expanded future demand.

The basis for such advocacy is in need of strengthening. Some of the present weaknesses are:

• Number of ads. is a partial measure of turnover as well as of new positions, however the effects are not separated quantitatively. Turnover includes replacement of both short and long term positions. Demand and supply, on the other hand, are directly reflected by the number of physicists in employment and the level of their remuneration.

• Employment in the public sector is more stable than in the private sector. It seems likely that most physicists are in the former and that many architects are in the latter.

Comparison of physics with architecture as a career option may therefore be quite misleading.

• The “good news” messages of Prescott’s surveys may not be so good: a reduction in advertisements by one third could be disastrous once the public/private employer-type and turnover rate are taken into account.

• Is a shortage of physicists good or bad? If one is a physicist, a shortage may mean better salary, high mobility, greater prestige. If one is an employer of physicists, over-supply may be optimal. If one is a teacher of physics, increased undergraduate enrolments is desirable since it increases the demand for one’s own services. It follows that there is conflict between the personal interests of institute members, university teachers, and the “advancement” of our profession.

In part, assessment of these matters requires hard, up-to-date, data. The National Strategy Plan document, Physics: A Vision for the Future, contains estimates of the number of physicists in employment based on six-year old data. It also discusses future demand. One could take issue with the reliability of the extrapolations, for example because of events such as the recession, reorganisation of higher education in Australia, and withdrawal of cold-war inspired funding of physics in the U.S.A. and elsewhere.

Some better data might be readily available, for example present and past employment details of AIP members. Secondly, a more sophisticated analysis of supply and demand is needed, preferably based on the direct market variables of quantity and price, if the claims of physicists are to be seen as factually based and not biased conjecture. Thirdly, any conflict between member interests and the general advancement of physics deserves careful consideration whenever the AIP and its members decide to take a public role.

Either the AIP should address these issues in a comprehensive, professional manner, or it should discourage claims which are potentially misleading and self serving.

Dr Robin Pollard
Monash University

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I greatly appreciate the honour of being asked to give this lecture and am happy to be able to pay my respects to the memory of Lloyd Rees. I was privileged to have known and worked with Lloyd from the beginning of the Chemical Physics Section and benefitted greatly from his guidance at an early and formative stage of my career. There have been a number of eloquent tributes paid to Lloyd, mentioning many aspects of his life and work. I hope you will excuse me if I do not attempt to give a complete picture of his influence, but just give a partial impression from my own limited viewpoint.

Looking back to those early days, one has to be impressed by what an extraordinary thing the establishment of the Chemical Physics Section really was. The idea of forming a Section specifically to introduce and promote the most advanced modern techniques of chemical physics, for the general benefit of CSIRO and Australian science, was quite new in Australia and rare in the world at that time. Ian Wark, the Chief of the Division, showed extraordinary insight in his part in generating that idea and also in picking Lloyd Rees to lead the Section. Lloyd was 28 when he took up the position. These days most 28-year-olds are barely out of graduate school and just beginning to find their way around their profession. Lloyd at 28 had an extraordinary breadth of experience, a clear perception of the role of instrumentation in science and a clear vision of what Chemical Physics could and should be.

Dr. L. M. Clarebrough (Secretary, Physical Sciences, Australian Academy of Science), Sir Alan Walsh, and Professor J. M. Cowley, with a characteristic photograph of Dr. Lloyd Rees, after the Lloyd Rees Memorial Lecture

The financial backing was good. Lloyd knew what equipment was needed and he picked the best. First to come was an electron microscope. It was not just any electron microscope, but the best, most advanced microscope possible: number 3 off the production line of the new RCA EMU series that was the top of the line and was to remain so for the next 5 to 10 years. He had extraordinary success in hiring people over the next few years. How it was done, I don’t know, but he brought in from overseas people like Alan Walsh, Sandy Mathieson, Jim Morrison, Alex Moodie, who all quickly became distinguished in their fields, and are now all Fellows of the Academy of Science. But it is not just that these were very bright young people. It was important that Lloyd created for them an environment in which they could develop and flourish.

At that time Chemical Physics was one of the best-equipped laboratories of its kind in the world and Lloyd insisted that it could be the best in every respect. The common attitude in Australian science in those days tended to be apologetic: “Of course, Australia is a young country and we can’t hope to keep up with England or America”. Lloyd would not have a bar of that. He knew that we could not only keep up, we could lead the way. We had the best equipment but we could improve on it. We could generate new ideas and open up new areas of science.
THE LLOYD REES LEGACY

I must admit that I felt somewhat overwhelmed in that context. By comparison with Lloyd I was extremely naive, both personally and scientifically. Personally, I was a shy and awkward product of the quiet, conservative atmosphere of Adelaide. With Lloyd and Marion Rees I encountered an unknown world of sophistication. They had travelled the world, and lived in war-time London. They knew the good things of life and how to enjoy them. Not all the Chemical Physics recruits were as naive as I of course, but all of them could rely on Lloyd and Marion for warm and generous support.

About Marion, I would like to say that I have come to appreciate her even more in retrospect. With the wisdom of hindsight I have begun to realize that it is not always easy to be married to a dedicated scientist. Without disparaging Lloyd in any way, it is clear that, with that particular dedicated scientist, life could have been difficult at times when things did not go well and the frustrations mounted up. But I've never known Marion to lose that warmth, charm and good humour that were always her characteristics. I would like to express our sincere thanks to Marion for all she did for the Chemical Physics group.

Scientifically I came to Chemical Physics from a background that we can only be regarded as naive and even primitive by comparison. The Physics Department in the University of Adelaide was primitive indeed. The staff consisted of the Professor, Kerr Grant, the Senior Lecturer Roy Burdon and a lecturer, George Fuller. Of these only Roy Burdon had any background in research. There were two pieces of scientific equipment in the Department. There was a Hilger medium spectroscope but that was being used by Keith Mather (who later became director of the Geophysical Institute in Alaska) and a Finch-type electron diffraction camera; the old type developed in the 1930's with a wine bottle mounted on top with plasticine to serve as an electron gun and vacuum joints made of flat metal plates stuck together with a thick layer of vacuum grease in between. It was a product of the poverty and ingenuity of English science in the 1930's. That camera had been bought for Roy Burdon's research on surfaces but no one had actually used it. So we got it into operation with the help of the Physics Department cadet, John Symonds (later Director of the Atomic Energy Research Establishment at Lucas Heights), and I started in electron diffraction.

The level of understanding of the realities of modern science in the Department may be judged by an occasion early in 1945. The rumour came through that CSIRO in Melbourne were planning to buy an electron microscope. The Professors’ response was, "I say, wouldn't it be great if we built an electron microscope in our workshop and beat them to it?" and he actually wrote to the CSIRO Executive offering his services if the purchase did not go through. The Department workshop at that stage included one machinist and an apprentice. Probably the Professor was the only person in the Department who did not know that the one machinist spent most of his time repairing peoples cars and the apprentice was learning that very useful trade. There was certainly no one in the Department who knew anything about any of the techniques needed to build electron microscopes. So arrived in Chemical Physics ready to begin my education in modern research techniques and Lloyd was full of the knowledge of the latest and best.

My job at Chemical Physics was to do electron diffraction. The RCA electron microscope had a diffraction adapter and that gave diffraction patterns with much better resolution than any previous instrument. So Lloyd and I started work on exploiting the high resolution. That brought us up against some phenomena that could not be explained by the simple theory that was commonly used. So that started us looking at the more complete theory, the dynamical theory of electron diffraction: essentially wave mechanics. More of that later. But the diffraction adaptor on the RCA microscope was a very limited device. Lloyd's response was that we must build our own special electron diffraction instrument that would get away from those limitations. He had the background since he had been interested in electron diffraction for many years, and he had picked up the latest techniques from England and America: hot filament electron guns and vacuum seals with special rubber rings.

Electron diffraction and microscopy was only one of Lloyd's interests in those days. He had expertise and was developing ideas in three or four of the main fields that were the basis for Chemical Physics. I hope you will excuse me for concentrating on this one aspect of his activities and influence that I know best.

So we started off in a modern new building with a modern electron microscope. Figure 1 is a picture published in the Melbourne Herald in January, 1946, showing Lloyd Rees at the controls of the microscope. John Parrant, shown in figure 2, was employed from the beginning as the person in charge of electron microscopy and was soon established as the Australian leader in that field. Parenthetically the microscope operator in Figure 2 is Robbie Beckett whom I married later that year, 1951. I may point out that Lloyd did a great service to the Section by bringing in some very intelligent and attractive young ladies from Western Australia, no doubt influenced by his own good fortune with Marion. Another was Lois Hulme, now Lois Mathieson.

So we designed our own electron diffraction cameras and they were built mainly by Jock Mills in the workshop. Figure 3 shows the first one. This picture appeared in the Melbourne Herald, January 1946.

Figure 1 Lloyd Rees at the controls of the RCA EMU electron microscope
Melbourne Herald, January 1946
Argus. The man on the left was described by the Argus reporter as the 'softy barred Mr. Moodie' and of course he remains so today. For me the only adjective the writer could find was "youthful" which of course no longer applies.

In retrospect these instruments seem very crude and poorly designed but they served a purpose at the time, and it was an important accomplishment to build instruments of this magnitude in the Chemical Physics workshop.

I was interested to read recently an article written by Lloyd in the report of a meeting of the Academy’s Science and Industry Forum, in which he talked about the origins of the Australian scientific instrument industry. This is one of the aspects of Lloyd’s career that, quite rightly, has been well publicised. He was enormously influential in putting Australia on the map in this, as in other, areas of science. Lloyd created the basis for high class instrument-building in the Division. He built up a first class workshop. He showed that it was possible to build first class scientific instruments in Australia. If it could be done in the Chemical Physics Workshop, why not elsewhere. Why not in industry? Why should Australia always be lagging the world in its scientific equipment? Why should it not produce the best for the great benefit to Australian science as well as the Australian economy?

Lloyd was very rightly proud of what his workshop accomplished in electron diffraction cameras as well as other areas. In his historic memo to the Chairman of CSIRO in 1952 he gave examples of the instruments that could and should be manufactured in Australia, and he included the electron diffraction cameras.

In retrospect it is fortunate that there were other instruments that were much more appropriate for industrial development including, of course, Alan Walsh’s multiple pass monochromator and later the atomic absorption spectrometer. These as you know have been the basis for the establishment of a major scientific instrument industry in Australia, with benefits for the science and the economy of the country which have far outweighed all the money ever spent on Chemical Physics. As it turned out, any attempt to build electron diffraction cameras commercially would have been disastrous. At about that time the developments of new techniques for use in electron microscopes soon made the electron microscopes much more useful for electron diffraction than any stand-alone electron diffraction instrument, except for some very special purposes for a few academic scientists.

So why not follow Kerr Grant’s idea and build electron microscopes in Australia? The same question keeps recurring in America today. After the success of the RCA EMU microscopes the electron microscope industry in America collapsed. The Japanese, the Germans, the Dutch, even the English put more research effort into the job and captured the market in the 1950’s. Now the Japanese and Philips in the Netherlands dominate the field.

The United States spends hundreds of millions of dollars on importing electron microscopes each year. Someone has calculated that electron microscopes represent 0.2 per cent of the adverse trade balance of the country. It will get worse because the balance of newest microscopes from Japan are becoming even more sophisticated and expensive. It would be a very difficult thing and very costly to try to compete with the well established electron microscope manufacturers overseas.

But I did not want to discuss the scientific instrument industry today. I would like to emphasize, instead, Lloyd’s contributions to what might be called pure science. (He preferred the term uncommitted science). He always believed that the pure and applied, or uncommitted and committed, science must go hand-in-hand and one must not exist without the other. That was a basic principle that he not only talked about but put into practice.

I might cite Lloyd’s attitude on wave mechanics or, more generally, quantum mechanics. In 1945 Lloyd was one of the

Figure 2. John Farrant and Robbie Beckett at the microscope, 1951
Figure 3  Alex Moodie and John Cowley with the first electron diffraction camera built in Chemical Physics
few people in Australia who knew about the developments of quantum mechanics since the 1920's. He was one of the few who not only knew about it but made use of it as a practical modern technique for dealing with problems of the physics and chemistry of solids.

I believe he was appalled to find that people in the Section had such a poor appreciation for what was so fundamental for their science. I remember he organized a lunch-time self-help study group. We studied one chapter of Eyring, Walter and Kimball's book on quantum chemistry each week, taking turns to prepare the presentation and lead the discussion.

But Lloyd was not satisfied to just use a tool like that. He questioned the basic ideas involved. He was not happy with the idea that electrons, for example, were just waves. He felt that they were actually particles, possibly guided by waves. In this idea he was not alone. Louis de Broglie, who introduced the idea of electron waves in 1923, thought the same way and so did Albert Einstein.

Electron diffraction was always quoted as being the proof of the wave-nature of the electron, but Lloyd asked me at one stage if couldn't find some way of explaining electron diffraction in terms of electrons as particles. I am afraid I failed him in that: I am no Einstein.

But the encouragement was always there to look at our subject from the most fundamental, basic viewpoint and not be inhibited by what others have done or said. In our case it provided a fine example of the interaction of instrumental development and fundamental science. The electron diffraction adapter in the RCA microscope had allowed us to see detail that prompted us to look at the dynamical, wave mechanical theory of electron diffraction. With our new home-made electron diffraction cameras we could get diffraction patterns from much smaller, thinner crystals; diffraction patterns with hundreds of diffraction spots. Then we realized that the standard wave mechanical treatment that people had used until then was completely inadequate to deal with the problem of interpreting those patterns. That gave us the strong incentive to find a new approach to the wave mechanical theory. The story of how that came about has been recorded elsewhere. But I would like to mention now how one aspect of that work tied in later with another of Lloyd Rees's interests and led to a major development in the science and practical applications of electron microscopy.

From the early days, Lloyd was intrigued by the subject of defects in solids. In 1953 he published a book, a Methuen monograph, on the subject "The Chemistry of the Defect Solid State". I can remember much earlier than that when he organized a mini-conference with J.S. Anderson who was then Professor of Chemistry at Melbourne University and Keith Sutherland of the CSIRO Physical Chemistry Section. One of the intriguing problems of the day was the non-stoichiometry of solids; that is,
why for some oxides and sulphides in particular the compositions could be variable. The ratios of the numbers of atoms of the various kinds was not the ratio of small integers as in NaCl for example where the ratio is 1:1 or CaCl2 where the ratio is 1:2. Lloyd described in his book how the variation in the ratio arises from the occurrence of defects in the crystal structures.

Later on it was found that for some metals oxides like those of niobium and titanium the composition could be almost anything. David Wadsley in the Minerals Section became involved in x-ray structure analysis of these materials. He and others found an entirely new basis for this variable composition. The structures were made up of blocks of material with metal-oxygen ratio 1:3. But these blocks were joined together with a different arrangement of atoms with a metal atom-oxygen ratio 2:5 and corner atoms with a ratio 1:2, so by putting together blocks of various sizes it was possible to get compositions anywhere in between these limits.

Then someone realized that these blocks of structure were so big that it should be possible to see them with an electron microscope. The electron microscopes of those days, in the early 1960's, had a resolution of about 6 Å or about twice the distance between the metal atoms but the blocks were 10 or 12 or more Å in size. The people who tried imaging these oxides were John Sanders and John Allpress in the Tribophysics Division. Who first suggested it, I don't know. I was at the University of Melbourne and somewhat out-of-touch by that time. But I have a suspicion that Alex Moodie was very much involved. Alex has been a catalyst for inspiration in many cases in many areas. He was always so interested, so excited about new ideas and enthusiastic about what people were doing or could do that he had an enormous influence on many of the major developments.

The pictures John Sanders and John Allpress got (figure 4) were not good enough to show clearly the structures of those compounds but they were so suggestive that the interpretations seemed obvious. When I first saw these results I tended to doubt that the interpretation could be so simple: after all, the wave-mechanical theory for diffraction from such crystals was terribly complicated. But then several things happened. One was that computers became available so that we could solve the scattering problem numerically and prove that the interpretations were correct. The next thing was that new microscopes became available so that the structures could be seen in much greater detail. Figure 5 is a picture by Sumio Iijima with the new Japanese microscope that came out in about 1969. The metal atom positions were now clearly visible with a resolution of about 3.5 Å. Currently, with new microscopes operating at 400 keV instead of 100 keV it is possible to get 0.6 Å resolution. They show directly how the atoms are arranged in crystals and show the structures of the all-important defects in the crystal structure.

Recently the study of crystals with this sort of resolution, seeing the positions of the individual atoms, has become a standard technique for many areas of solid state science. It is a major tool for research on semiconductors, on the new high temperature superconducting materials on the catalysts which are the basis of the petro-chemical industry (figure 6) and on many other types of materials.

The interpretation of the pictures is not always straight-forward. It must be accompanied by computer calculations and the only useful basis for the computer calculations has been the wave-mechanical theory we developed in the 1950's.

The role of Chemical Physics in the whole development of crystal-structure imaging has been a basic one, (and since I left Chemical Physics before that time I can talk freely). The fundamental theory of the imaging of crystals was worked out in a classical series of papers by Alex Moodie in conjunction with various people: John Sanders, John Allpress, Andy Johnson, Mike O'Keefe, Elizabeth Chidzey, Geoff Austis, Dennis Lynch.

The fundamental paper setting out the principles for the computer calculations was the 1974 paper of Peter Goodman and Alex Moodie.

Yet it was ironic that Chemical Physics at that time could not take advantage of their predominant position in the theory of the electron microscope imaging of crystals. They no longer had the best available microscopes. They had made important contributions in instrumentation, certainly, including the best specimen stages. But the message of the early days of Chemical Physics had been lost. One must start with the best available equipment and build on from there.

Of course the whole situation in science has changed greatly. The best available microscopes are expensive and newer and better microscopes appear every few years. It costs a lot of money to keep up with the game. It is increasingly difficult for any one group, one laboratory or one University department to remain current.

This is where we must start talking about Facilities. Early Chemical Physics was a perfect example of what would now be called a Facility and the principles of its organization and operation are the principles that must be applied now if a Facility is to perform its proper function. Chemical Physics was organized to make available to Australia the most advanced techniques of modern solid state science. That meant getting the best possible equipment. More importantly, it meant getting together the best possible people, giving them both the freedom and the encouragement to go ahead and become the leaders in the theory and techniques of their special areas. It meant encouraging an active programme in development of the techniques and it meant, of course, an active programme of interaction with the scientific community to educate them in the value of the techniques by practical demonstrations and collaborative research in as wide a range of areas as possible.

At that time the one small Section had to fulfil this function for a whole range of techniques, with only one or two people for each broad area. Now, the Facilities are much bigger and much more specialized. Within the range of interest of Chemical Physics, there are Facilities for various kinds of spectroscopy, there are large Facilities for mass spectroscopy. For X-ray diffraction there are the synchrotron radiation facilities costing hundreds of millions of dollars. For electron microscopy common Facilities are being established because the new instruments are so varied and so expensive that they must be shared. The idea is not new, of course. It has been the basis for organization of science in a number of fields, such as particle physics and astronomy, for many years. But in the rush to set up Facilities, amidst the financial, political and organizational scrambling that goes on, it must be clearly kept in mind that the success of a Facility, in terms of scientific progress and economic value, is going to depend on the extent to which the same principles are established which governed the early days of Chemical Physics.
So it was that when we had the chance to set up a national Facility for High Resolution Electron Microscopy in Arizona in 1979 we had an excellent example to follow and we followed that example to the best of our ability in the circumstances. The circumstances were provided by the environment of the American scene in our area of science. The American electron microscope instrument industry had completely collapsed, partly because there was very little research or teaching in American universities on the fundamental science of electron diffraction and microscopy. There was a sprinkling of people, imported mostly from England and employed mostly in the big industrial laboratories, who kept the fundamental science alive, more or
less as a hobby, incidental to their main employment. America
probably bought more electron microscopes, from Japan or from
Philips, than all the rest of the world put together. Most were just
installed and used in standard modes for the solution of
immediate practical problems in biology or materials science.
Most were used poorly with only a fraction of their capabilities
because their operators had very limited understanding of the
fundamentals of the techniques.

So American electron microscope Facilities are mostly
established by buying the latest microscopes from overseas,
finding some competent electron microscope operators and
gathering together as large as possible a group of users from
industry and University departments to keep the microscopes
occupied all day with as wide a variety of projects as possible
in order to justify the purchase of the next new microscope in a few
years time.

Of course our Facility must have an effective service component.
We insist that we are a high resolution Facility which means that
we do not take the routine jobs, but specialise in the advanced
applications that can not be done anywhere else. But we exist
because we have users from a wide range of industrial,
government and University laboratories. We have an Industrial
Associates Programme. There are now 8 of the leading
companies, who have their own electron microscopy
laboratories but pay $30000 a year to be kept up-to-date, to get
assistance in using our special technique and to have access to
our special expertise. We are happy to do this: it brings in a lot
of very interesting projects.

But from the beginning we insisted that approximately half our
efforts will go into the advancement of the techniques:
fundamental research on the theory, instrumentation, data
analysis, image interpretation or whatever we feel is needed for
the advancement of the subject.

Figure 6. Electron micrograph of small crystals of
rhodium metal on a cerium oxide support, a catalyst
system, showing the relationship of the two crystal
lattices. Courtesy of Ming Pan.
THE LLOYD REES LEGACY

We have managed to recruit some of the best people. We have a core of people who are mostly concerned with the fundamental aspects and it is no accident that these are mostly from Australia and, in fact, from Melbourne, with first-hand or second-hand contact with the Lloyd Rees tradition. David Smith from Melbourne is now Director of the Facility and John Spence of Melbourne is in charge of development projects. More recently we have added to the group Peter Rez, who is English, and finally this year, Mike Scheinfein, an American. It is the Americans who have been the leaders in the application of the techniques of the Facility to fundamental studies in various areas of science: LeRoy Eyring in Solid State Chemistry, Peter Buseck in Mineralogy and Ray Carpenter in Materials Science. Our close interactions with these people and the dialogue between the developers and users of techniques has been one of the main strengths of the Facility group.

So to the extent that was possible we have tried to follow the principles that were used as a foundation of Chemical Physics. To some extent it runs counter to the practice of American science and we have apposition. Every three years we must be reevaluated. We make proposals for renewal of our grant from the NSF. We are reviewed by mail reviewers and studied by Site Visit team. Always there is some reviewer who says “Why are these people spending so much effort on all this theoretical stuff? Why don’t they get down to the job of solving real materials problems?” Our answer has to be: “we do solve real materials problems, probably more than any other Faculty of comparable size and cost, and we solve more advanced problems, more effectively than anyone else”.

At the moment we are awaiting our triennial review again. We are being ambitious. At a time when finance for research is at a low ebb and everyone is suffering from lack of support, we are asking for financing for the development of the new area of electron holography. I think our main argument must be the Japanese are currently spending at least ten times what we are asking for on this, and no one else in America has a chance of doing anything on a meaningful scale. These are the levels to which we must sink: using the bogey of Japanese dominance of new science and technology.

But now we must recognize that Facilities must be organized on an international scale. For a number of years it has been clear that any proposal for a large Facility in particle physics or astronomy must have an international component or it will not get funded. This is certainly the case in Europe and also in America. In the future I would anticipate increased interaction with the Soviet Union and Eastern Europe.

It is not quite so apparent in areas like electron microscopy as yet. The installations are costing millions rather than billions of dollars. But the trend is certainly there. The newest electron microscopes from Japan cost between 10 and 20 million dollars each, apart from housing and attachments which will increase the cost by a few more million. So far there are 4 of these microscopes in Japan and one in Germany; None in the United States; None in Australia.

I went to talk to our funding agency, the National Science Foundation people in the Division of Materials Research, about it a year or two ago. I told them what such a microscope could do. They said “That’s very interesting but where are you going to get the money from? It represents about 3 times our total annual equipment budget for the whole country!” As it turned out we decided on scientific grounds that the sort of microscope is not what we want.

Instead we decided to go into electron holography which will be only a fraction of the cost. But to do that we are joining in with a European consortium, the BRITE EUREM group, of four laboratories from three different countries. Japan is going it alone. They can apparently afford to, and on a larger scale.

But on the other hand Japan is now starting to take the lead in many ways in promoting international science. They are now considering proposals for international collaborations involving Japanese and foreign laboratories on projects, with budgets of the order of a million dollars, and many groups in America are joining in, seeking money from the Japanese government.

Appreciation of the need for international cooperation came early in Chemical Physics. Starting in the late 1950’s when international travel and international interactions became feasible, Lloyd Rees pushed very hard to get Chemical Physics to take its place on the world scene. He pushed some of us into positions in the international Unions and into participation in international conferences. International visitors started to appear in the Division.

From that time on Chemical Physics was very much part of the world scene and Lloyd himself was prominent in international organizations and became President of the International Union of Pure and Applied Chemistry. So, you may ask how does that help Australian science? The answer is that, inevitably, science has become increasingly international and will become more so.

The general trend to large-scale centralized international Facilities is inevitable but is in many ways disturbing. If one tries to maintain an effort at the cutting edge of science, a single laboratory or a single University department can do so only in an ever-narrowing range, because the costs of equipment are going up rapidly with the increasing sophistication of the equipment and research budgets are not going up in the same way. So there must be a sharing.

Individual scientists must increasingly go away from home, and go to a common Facility which inevitably is not always arranged for their convenience. They are on foreign territory, away from the familiarity of their own laboratories and equipment and their own normal contacts. It can be a daunting experience.

We are familiar with the difficulties of people coming from around the country and around the world to work with us in our Facility. There is one solution to many of the problems that has been found by one or two of our Industrial Associate companies. They hire a post-doctoral research associate to work in our laboratories on their problems. The post-doc knows the equipment, knows his way around the laboratory and knows the people to talk to. The researchers from the companies do not have to come to Arizona often. The communications are so good now, by electronic mail and FAX, that they can keep up with what is being done, probably better than if they were in the same building. It costs the company much less to support a post-doc with us than to employ someone in their own laboratory and it is much more effective in many ways.

One of my ex-students is now employed in this way by a major oil company, using a microscope for which they do not want to pay $1.5 million dollars, and wait three years. It has worked very well. He has done some very nice basic research and also he has got some practical results on their samples that induced them to modify their production line on catalysts. I would like to suggest that this idea can be extended to overseas participations. We would welcome an arrangement like that. The more good people we have around, the better we like it, and we know very...
well that young Australians are among the best trained and brightest of the young scientists.

Now a few words about the prospects for the future. From what I have heard about the Australian scene, it seems to parallel that in America. Since you all know more about the Australian situation than I do, I will confine my remarks to the American outlook. For the immediate future I do not see any evidence that major changes are likely. Government policies are set in well-defined patterns and I doubt that, even if the economic climate improves, there will be any great improvement in the support for science. In many respects the situation in science everywhere is discouraging. The scene in America can be described, I think, as confused and anything from disturbing to disastrous, depending on one's particular viewpoint. The big industrial laboratories, AT&T Bell, Exxon, IBM are changing their courses, as they say, to be more responsive to the needs of the industries. There is nothing wrong with closer ties between the research laboratories and the production line, but when the strongly applied science is built up at the expense of the pure science the balance is lost and the long term productivity must suffer.

I don't think that we are going to see the Nobel prizes coming to AT&T Bell Laboratories that we saw from the old Bell Telephone Laboratories. It is interesting to ask, incidentally, why the recent Nobel prizes, for high temperature superconductors and scanning tunneling microscopy and new developments such as Fink tips came from the IBM Laboratory in Switzerland and not from the much bigger IBM laboratories in the USA. The answer must be that the pressure of the American industrial scene, the insistence on relevance, was not felt there. It was a small backwater where it did not matter if the good scientists followed their own ideas.

That is why I venture to guess that the next Nobel Prizes to come out of industrial laboratories in America will probably come from the laboratories being set up in America by the big Japanese companies. For political reasons, these laboratories are only for pure, publishable research. They will not be limited by the immediate pressure from the production lines of the Japanese companies. They are being well funded and well supported and are attracting some very good people - many of them refugees from the big US industrial laboratories.

In the universities the situation is grim in many respects. We rely mostly on funding from federal government agencies. The State governments have never been supportive of research - at least not consciously - and now most of the States are on the verge of bankruptcy. The Federal government budgets have been static, or decreasing in terms of uninflated dollars. The situation is particularly distressing for young scientists. It is very difficult for them to get a research grant, but if they do not get research grants they can not get tenure.

This applies even for a second-rate University like Arizona State. There are so few academic positions available now that the competition for them is very great. With so many good people to choose from, a University department will pick those who can bring in most money to help the Department. The whole basis for choice gets distorted. The prime consideration is whether the appointee can bring in money. They must be able to get grants. That means they must be in a hot area, one with prospects for immediate results of commercial value; and this is so because the funding agencies must justify the use of their funds on that basis. A department cannot afford to hire someone who may be a genius if his area is not a fashionable one for funding. I am only grateful that I started in a kinder and gentler time when things were not so complicated.

As in Australia there are serious concerns about the degradation of research and education. There is a growing awareness that without the strong support of science, the country is losing competitiveness with the Japanese and, to some extent, Europe. There have been conferences and committees, resulting in reports, that go into many volumes and after a few years there appears to be some action.

In the solid state science area it was realized that one big deficiency in the US capability is in the area of materials synthesis: the techniques of making the many new types of materials which are the foundation of many high-tech industries. So after much self-study and many reports and discussions a response has been made. A fund has been set up to promote materials synthesis. In the first year this will be $85 million, although, by the time various unnamed agencies take their cuts, the amount available for open, competitive grant applications is $30 million. But this of course is essentially committed research. To what extent basic uncommitted research will benefit, if at all, is not clear. If there is not the proper balance of uncommitted research, committed research and technology, then much of the money will be wasted.

If one cares to be cynical one can say that in these times of budget cuts, money can be found for a project like this only by taking it away from somewhere else. One wonders what areas of pure science will be suffering the biggest cuts.

However I still retain some optimism based on the belief that there are good scientists and wise administrators among the government agencies and their advisory panels, who retain some sense of balance, who understand the principles that Lloyd Rees emphasized so consistently during his life time.

For the future health of our science and our community we must keep firmly in mind those principles that Lloyd Rees established many years ago. I can not do better than quote the conclusions of the lecture Lloyd gave in 1970 to the Institute of Physics: the Einstein Memorial Lecture. His title was "The Origins of Modern Technology". His thesis was that modern technology is based on pure science and he analyzed the enormous benefits that have come from the pure science of Albert Einstein. He summarized his main points as follows:

1. Pure science is an essential component of human activity and is particularly essential to technological development. While technology contributes through feedback to the development of pure science, the main stream of progress is initiated and sustained by the flow of ideas, information and understanding from pure science through to technology.

2. The initial step in the useful application of the results of pure science is the recognition of a potential exploitable result. Where a combination of uncommitted and committed research can be pursued in the one research environment then this recognition becomes much easier.

3. The results of uncommitted research can be exploited only if the supporting technology is at an appropriate stage of development.

4. Pure scientific activity contributes to the pool of scientific and technological competence within the community.

These statements may seem to us self-evident; but it is worth reminding ourselves about them occasionally. They are just as relevant today as when Lloyd wrote them in 1970 and to the extent that we keep them in mind our science and our economy may flourish. ☄

*Australian & New Zealand Physicist Volume 19, Number 10, October 1992*
New Compact, Sealed CO₂ Industrial Laser

Coherent Laser Group has recently introduced the DIAMOND, a compact, sealed and easy to operate industrial CO₂ laser.

The unique conductively cooled slab discharge geometry of the DIAMOND together with the coupling optics result in a laser one tenth the size of ordinary CO₂ lasers of similar power levels. The output beam of the DIAMOND has excellent mode quality. The minimum theoretical value of M² for the DIAMOND laser is typically 1.2 with a guaranteed maximum of 1.5. This is in sharp contrast to conventional industrial CO₂ lasers which have an M² value typically in the range 2-3.5. To define M² we take the waist size and divergence of the real beam and measure how much it exceeds the divergence for a gaussian beam having the same waist size. An M² of 1 signifies a 1:1 correlation of the real beam divergence to that of a diffraction limited beam. Higher order modes or mixtures of modes have a higher M². Consequently, the output beam of the DIAMOND can be focused to a spot that is up to four times smaller than conventional CO₂ lasers, supplying you with cutting and drilling speeds equivalent to conventional industrial CO₂ lasers with 2-3 times higher power rating. The DIAMOND has “square wave” output pulses which maximise the peak power with negligible loss of average power. The rise and fall time of the laser pulse is 50μs, which is small compared to conventional CO₂ lasers which have a rise and fall time of 0.2ms. The DIAMOND models are available with average power specifications of 75W (DIAMOND-42) and 150W (DIAMOND-64). The high peak power (75-300W for DIAMOND-42 and 150-475W for DIAMOND-64) in narrow pulses (25μs - 1000μs) at repetition frequencies up to 10,000 pulses per second produces a smaller heat-affected zone and superior edge quality. This reduces the need for secondary finishing operations.

The power supply for this laser is low voltage, allowing it to be located remote from the laser head and to offer safer operation. Both the laser head and the power supply are water cooled.

For welding, drilling, cutting or any processing, the DIAMOND can be turned on and off in microseconds. This high speed response gives the user precise control of endpoints and maximises heat spread to adjacent material or parts. The interface is a standard NC hookup that lets you adjust the power from 2% to 100%. This allows the same machine to process different materials or to be used for different processes, such as cutting or marking on the same material.

For more information, please contact Dr Narrelle Murphy at Coherent Scientific Pty Ltd 116 Burbridge Road Hilton SA 5053 Phone (08) 352 1111 Fax (08) 352 2020

Low Cost Femtosecond Ti:S Laser from

Coherent Laser Group has announced the introduction of a new, low cost, femtosecond Titanium:Sapphire laser, the “Mira-Basic”.

Mira-Basic is a reduced function version of the very successful Mira-900E, Kerr-Lens Modelocked (KLM) Ti:S laser.

Mira-Basic retains all of the essential features of the Mira-900F, ie:

- Optimally designed vibration damped, resonator baseplate
- Complete prism-compensated, femtosecond KLM cavity mechanics and optics
- Single CW alignment cavity
- Three optional pumping directions
- Pump beam translation and angular control package

The following features of the Mira-900F are not included in Mira-Basic:

- Diagnostic detector package - sync photodiode - power monitor detector - CW detector
- Control Unit - power level readout and output - CW level readout and outputs - automatic starting mechanism
- Latching, sealed covers for purging
- End bezels with Brewster windows - Thermally-compensated output coupler mechanism
- External access to prism translation
- Long wavelength operation option

Mira-Basic is fully upgradeable to the Mira-900F or Mira-900F picosecond.

CO₂ Industrial Laser from Coherent Scientific

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PRODUCT NEWS

laser later when funding permits, and is priced to make it an attractive alternative to kits offered by other manufacturers, and to the build-it-yourself option.

Package pricing including argon pump lasers is also available.

For more information please contact
Norman Jones at
Coherent Scientific Pty Ltd
116 Burbridge Road
Hilton SA 5033
Phone (08) 352 1111
Fax (08) 352 2020

Oriel Donates Optical Equipment to International Environmental Project

Oriel Corporation, a Connecticut based manufacturer of optical research systems and components, has donated optical monitoring equipment to the HAPEX (Hydrological Atmosphere Pilot Experiment) Sahel Project currently underway in Niger, Africa.

The HAPEX Sahel Project is a major international experiment concerned with the impact climatic changes have on vegetation. The equipment donated by Oriel will be used to take ground measurements of the nitrogen status and water stress levels found in plants.

Dr. Eugene Arthur, Executive Vice President of Oriel, said, "Oriel has always been concerned about our environment. Our customers are working on many aspects of environmental research, actively trying to improve our future. The results of the HAPEX Sahel Project will contribute greatly to our understanding of and ability to remotely monitor the health of crops. We hope this will lead to reduced need for pesticides and improved yields. We are pleased to be a small part of this effort."

InstaSpec™ IV CCD Detector

Oriel Corporation announces the new InstaSpec™ IV CCD Detector, the first CCD detector designed specifically for spectroscopy. Over 100 times more sensitive than linear diode array detectors, InstaSpec™ IV is the ideal detector for spectroscopic measurements of very low, fW, light levels.

Unlike the majority of presently used CCD sensors, the readout register of the UV enhanced InstaSpec™ IV sensor is along the long axis. This allows the long axis to be aligned along the wavelength axis, providing the best match of the spectrum image to the sensor area.

To achieve maximum performance with a minimal number of components, Oriel has designed the detector head to link directly to a PC interface board, eliminating the need for a large, bulky controller box, cooling water, or even liquid nitrogen. InstaSpec™ IV is so compact that when used with a laptop computer, it becomes a truly portable CCD detector!

InstaSpec™ IV's comprehensive, yet easy to use software, allows superfast kinetic measurements of spectral changes. Up to 256 spectra can be acquired at effective rates of up to 74,500 spectra per second. The data is displayed as two dimensional, overlaid, stacked, or three dimensional plots. A powerful BASIC-like script language is also included which can be used to set up automated programmed operation.

For more information on Oriel's InstaSpec™ IV CCD Detector call Lastek Pty Ltd
400 King William Street
Adelaide SA 5000
Phone (08) 231 2155
Fax (08) 231 2169

Elements

Elements is an exciting new software package that provides data and properties for the elements of the periodic table. This information has been derived from the CRC Handbook of Chemistry and Physics, 72nd Edition. The program uses the organizing power of the periodic table to access fundamental characteristics of each element.

Elements provides a brief description for each element, which can be displayed by selecting the element from the periodic table that appears on the program's main screen. One or more elements can be selected simultaneously to compare data. Elements' calculation program also enables you to determine molecular weights of compounds.

Elements provides data for each of the following fields: Atomic Number, Atomic Weight, Atomic Radius, Density, Melting Point, Boiling Point, Critical Temperature, Resistivity, Thermal Conductivity, Specific Heat, Heat of Fusion, Thermal Expansion Coefficient, Crust Fraction, Ocean Fraction, Reduction Potential, Ionization Potential, Electron Configuration, Oxidation State.

If you are unfamiliar with a field, Elements provides an on-screen definition. Elements requires an IBM compatible computer, DOS 2.0 or higher and 256KB of internal memory (RAM).

Price US$49.95 A$67.75 Available from DA Books & Journals
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The following is a biographical sketch of Lev Davidovich Landau, the USSR's greatest theoretical physicist. Increasing specialisation means his extraordinary breadth of knowledge of the subject is now unlikely to be exceeded. The sketch incorporates newly available material in books commemorating the 80th anniversary of his birth and 20th anniversary of his death.

Lev Davidovich Landau was born in Baku, Azerbaijan, a university and oil town on the western shore of the Caspian Sea, on 22nd January, 1908. His family was middle class, and Jewish, belonging to the bourgeoisie soon to be vilified in the Revolution. This must be seen in context; "middle class" was immeasurably further then from the peasantry than it is today. Landau's ethnic origins are not mentioned in Livanova's Russian biography, nor his spell in Stalin's gaols.

In addition to dates, places, works and honours, I hope to illuminate Landau's personality. How did he see himself? How did his colleagues see him? What inner life did he live? In short, who was he? The literature available, and the life he led, provide reasonably detailed answers. His father was David Lvovich Landau, an engineer from a well-off family. He was the manager of a stock company concerned with the oil business in the Baku oil fields, and was over 40 when his second child, Lev Davidovich, was born. (There was an elder sister, Sophia, who became a chemical engineer.) Landau's mother was Lyubov Beniaminovna Garkavi, 10 years younger than her husband, and from a poorer background. She graduated in 1898 from the St Petersburg Midwifery Institute, and six years later from the Women's Medical School. She met her husband when he was visiting her sister, who was having a baby. Landau's mother ran the school which her son Lev attended at the age of eight, and the young Landau would arrive with her daily by carriage. Both parents perished in World War II, in the siege of Leningrad.

As a young child he had been interested exclusively (and obsessively) in arithmetic and mathematics, concerning himself with anything else - intellectual or other - only to get it out of the way; the interest in music his parents had hoped for came to nothing. At school he excelled in mathematics and science, and had mastered the integral calculus by the time he left high school at 13. He must have been unhappy to distraction, for no 13 year old resolves - as he did - to commit suicide. Fortunately he did not do so. This was also a turbulent time for his country: during his schooling, the chaos of revolution was going on, and Baku was taken four times in the struggle.

His parents felt he was too young for University, and preferred a financial career for him. Accordingly he spent a year with his sister Sophia at Baku Economic Technicum. At his own insistence he then transferred, in autumn 1922, to science at the University of Baku. He enrolled in two departments: physics-mathematics and chemistry: when nine he had said that he wished to investigate every matter that life brought him in contact with, and to find his own solutions. Physics, the most fundamental of the sciences, was an obvious calling.

Dr. Anthony J. M. Garrett is in the Department of Physics & Astronomy at the University of Glasgow, Scotland, UK. He graduated in physics from Cambridge, where he gained his Ph.D. and pursued postdoctoral research. He has also researched at the University of Sydney, Australia.
Landau seems to have been consciously rationalist throughout his life. It does not necessarily lead to a happy existence; most of the problems of humanity are so complex that only the subconscious mechanisms put in place by evolution can handle them. But Landau certainly applied his tenet to his work: he seldom read a paper through, preferring instead to find the central idea and then work out, perhaps in discussion, the rest himself. Although a lengthier process, ideas are better assimilated by this means.

In 1924, at 16, he transferred to the physics department at Leningrad (until just before, St. Petersburg) University, retaining thereafter an amateur interest in chemistry. Leningrad was the Soviet Union's leading physics centre, and although at the time the work was more experimental than theoretical, it was here that Landau matured into a theoretical physicist proper. He says he only went into the University twice a week to "meet friends and see what was happening", but he devoted most of his spare time to study, and often could not sleep for turning formulae over in his mind. Most great physicists were sporadic attenders of lectures, since they were already well beyond the level expounded; university gives them, above all, time. Landau was staggered by the beauty of Einstein's conception in general relativity, later stating that such rapture on first meeting it should be a characteristic of the true theoretical physicist. Landau recognised the complementary importance of experimental physics, and experimentalists always found him most approachable; in fact he would always lay pure theory aside if asked for calculational help by an experimentalist. Later in life he vehemently declined to set up an exclusively theoretical institute.

In 1926 he simultaneously enrolled at the Leningrad Physico-technical Institute as a non-mature graduate student, and a year later graduated from the University and commenced full-time studies at the Institute under Frenkel. A fellow student was George Gamow. At this time the revolutionary papers on the new quantum physics were coming in, from Schrodinger, Jordan, Born, Heisenberg and Dirac. Landau read them avidly. He saw immediately the importance of the new work, but through inexperience was not in its forefront, though he contributed later to quantum electrodynamics. Certainly he had the ability; he often regretted not being seven years older. Nevertheless, his first four papers, published in his late teens, all concerned the new mechanics. In the second of these, he quantised the rigid rotor to find the spectra of diatomic molecules, and extended the analysis by perturbation theory to Zeeman splitting in magnetic fields. Nowadays it is a student exercise; then, a student applied the new ideas to work it out for the first time. Another paper was on quantum-mechanical damping, also studying spontaneous emission. It introduced the concept of the density matrix independently of von Neumann. All four papers appeared in Zeitschrift fur Physik. He published nothing more for three years.

In 1929 Landau won a Rockefeller Fellowship, which the People's Commissariat of Education supplemented, and went abroad to learn from the great European physicists. He took his opportunity, saying later "It was a pleasure to talk with everyone I met. Not one of them showed a trace of conceit, pretentiousness or arrogance." Although Landau also stated he met all the greats but Fermi, there is no mention in the literature of any meeting with Einstein, whom he regarded as the best of all. He met Born in Göttingen, Heisenberg in Leipzig, and then went on to Niels Bohr's Institute of Theoretical Physics in Copenhagen. This was by far the most formative part of the trip: all the greats regularly congregated there for seminars, discussion and cross-fertilization of ideas. Landau was one of the most active participants. He regarded the experience as so valuable that, once he had gained a measure of autonomy, he returned in 1933 and 1934. Doubless he would have visited more often but for travel restrictions later in the decade. He always considered Bohr his mentor, which is particularly interesting in view of their widely differing personal and scientific styles. After a brief return to Leipzig he visited Cambridge for four months, where he wrote up the idea of innate electron diamagnetism, in what became his fourth paper. There he worked with Rutherford, and met his fellow citizens Pyotr Kapitza and George Gamow, touring Britain (in a red jacket) on the back of Gamow's motor cycle. After that he went on to Pauli in Zurich where he also worked with Rudolf Peierls, then assistant to Pauli, who later married a prominent member of Landau's Leningrad circle. Landau was, of course, a competent linguist to benefit from this tour.

He returned to Leningrad in March 1931, becoming active in teaching as well as research. At this time, dialectical materialism was universal dogma in Russia. Landau did not initially perceive the seriousness with which this was taken; he, Gamow and three others fell into trouble over a satirical telegram, sent to the author of an encyclopaedia article attacking relativity as incompatible with dialectical materialism.

Nevertheless, at 24 Landau was appointed head of the theoretical division of the newly organised Ukrainian Physico-technical Institute in Kharkov, then the capital of the Ukrainian SSR. (Today the capital is Kiev.) He stayed in Kharkov five years. The Institute was an offshoot of the Physico-technical Institute of Leningrad, whose head, Joffe, put great effort into setting up such institutions countrywide. Landau, a graduate of the Leningrad Institute, was an obvious candidate.

By this stage Landau knew what he could do, and at 24 was in the enviable position of being in charge. His research flourished, and branched into diverse fields; in 1936 he published or co-authored the following papers:

Theory of Photo-Emf in Semiconductors, Theory of Monomolecular Reactions, Theory of Sound Dispersion, Transport Equation for Coulomb Interactions, Properties of Metals at Very Low Temperatures, Scattering of Light by Light and in 1937:


These are impressively varied. He also displayed an absolute mastery of mathematical techniques, so much so that his papers appeared effortless. It was said that von Neumann never solved any problem he found difficult, only problems others found difficult; but when Vitaly Ginsburg put a similar charge to Landau, he instantly responded "No, that is wrong; I did what I could". Landau had already developed an interest in the theory of matter at low temperatures, a field studied experimentally in Kharkov by Lev Shubnikov and his wife Olga Trapeznikova, who had earlier both worked in Kamerlingh Onnes' pioneering low temperature laboratory in Leiden. These were to become two of Landau's closest friends; later, Artemi Alikhanian was to become a personal confidant. Paul Ehrenfest, who had lived in St. Petersburg pre-revolution, was a frequent and valued visitor to Kharkov. Landau also became acting head of the theoretical physics department at the Institute of Mechanics and Machine-Building, primarily for lecturing purposes. In 1935 he moved over to head also the general...
physics department at the University of Kharkov. He must have been able to do with very little sleep - head of three departments, heavy lecturing demands, massive research output, and, as we shall see, the time and emotional energy for courtship and marriage. It is seldom noted that the famous can do with less sleep, or can fit it into idle moments, but it is usually so.

In Kharkov, Landau met Concordia (Cora, or Korusha) Terentievna Drobanitskaya, an attractive Ukrainian chemistry student and food technologist. Overcoming his original reticence with women, he courted her, and in 1937 they married. The Landaus had one son, Igor, born in 1946, who became an experimental physicist.

Perhaps most importantly, it was at Kharkov that Landau developed his ideas about the teaching of physics. Landau’s master plan was to write, or at least oversee, a graded series of textbooks, from school and lay texts to a course for professional theoreticians. He never completed the task, but by the time of his disablement in 1962, he and Evgeny Lifshitz had finished nearly all of the full Course of Theoretical Physics, and the first part of the Course of General Physics. For this they received the Order of Lenin, the highest Soviet honour. A Physics For Everybody volume was also completed. The nine volume, full Course of Theoretical Physics is universally known as “Landau and Lifshitz”; it has been kept up to date, and translated into English. (Among the translating team was John Bell of Bell’s theorem.) It would be easy to praise a project of this magnitude for its mere existence; but these books are genuine masterpieces. They include all pertinent facts, and never waste a word or use an inferior method. The initial Russian reviews were, ridiculously, negative; again dialectical materialism was involved. But the physicists knew better.

From 1930 on, Landau’s output was actually written by Lifshitz or a collaborator and overseen by Landau; perfectionism to the degree of self-torture was responsible. (Landau graciously declined co-authorship of many papers he inspired.)

The full Course of Theoretical Physics was what Landau uncompromisingly believed every intending theoretician should master before undertaking research. He called this the “theoretical minimum”. He also believed in a mastery of mathematical methods, so that technicalities should not obscure the physics of a problem. Landau initially examined students for the theoretical minimum himself, but his success later restricted him to conducting the initial interview and a mathematical preliminary. This test involved the evaluation of indefinite integrals expressible in elementary functions, solution of ordinary differential equations of standard type, vector and tensor analysis, and elements of complex variable theory. 43 persons passed the theoretical minimum from its inception in 1933 up to 1961; by 1988, 10 of these were Members of the Academy of Science (equivalent to FRS), and 20 D.Sc.’s.

In 1937 Kapitza, who three years earlier had been refused permission to return to Cambridge after a visit home, was able to invite Landau to head the theoretical division of the new Institute of Physical Problems in Moscow. Landau accepted, and was based there for the rest of his working life. The timing was fortuitous: factions within the Institute at Kharkov were interpreted as related to those in the local secret police (the NKVD), and most of the senior scientific staff was arrested. Landau was very aware that his sharp tongue made him an obvious target of the arbitrary purges then prevailing; though a naive still prevailed, for in 1936 Landau declared that Stalin’s “democratic” constitution would soon deprive him of power.

Unfortunately, departmental factionalism at Kharkov pursued him and in April 1938, in Moscow, he was charged as a German spy. He was only released a year later after Kapitza had risked personal intervention with Stalin, Molotov and Beria, and Landau had had to admit to lying (under torture or its threat) in his “confession”. In his cold and crowded cell, Landau trained himself to think without writing materials, but was convinced that another six months would have killed him. Colleagues report that the experience had a deep effect; “How dare they laugh!”, he exclaimed, overhearing a party just after his release.

More understandable is secrecy over Landau’s war efforts. In summer 1941 Hitler launched Operation Barbarossa, the invasion of his eastern ally, initiating what Russians call the Great Patriotic War. The Institute was evacuated 400 miles east to Kazan, where it assisted in the war effort. Landau became a member of the Engineering Committee of the Red Army. Later, four papers surfaced on detonation and shock waves. Evacuation and war work did not stop his own research, although a glance at his publications shows it slowed.

In 1941 Landau published the first of several papers for which he was to receive the Nobel Prize: a quantum treatment of the superfluid phase of helium-4 (confusingly called helium-II).Landau deduced the energy spectrum of the Bose excitations semi-empirically; it has a valley at 8-10K. The energy gap is the cause of superfluidity, and the quasiparticles existing in equilibrium in this valley Landau called rotons. This enabled him to reproduce Laszlo Tisza’s prediction of “second sound”, an extra wave mode. It was detected by Peschkov three years later. The differing theories were perceived as rivals, leading to a vigorous exchange which is summarised in Stephen Brush’s fine history of statistical physics.

Landau returned often to the mysteries of low temperatures; he refined his theory in 1947, and in the 1950’s turned to the equally enigmatic isotope, helium-3. In 1950 he and Ginzburg published a paper on superconductivity which is still much used today. Another famous discovery, from 1946, is collisionless (energy-conserving) attenuation of longitudinal waves in plasma, (“Landau damping”). It is a kinetic, velocity-space effect which cannot be foreseen from the hydrodynamic plasma equations.
It was in 1946 that the USSR Academy of Sciences, under threat of mass resignations, at last elected Landau a Member. The delay, which particularly incensed Kapitza and Fock, was clearly a result of Landau’s sharp tongue.

Landau was a member of Igor Kurchatov’s nuclear weapons team. (Another prominent figure was Andrei Sakharov.) Although Landau never worked full-time on the Soviet atom bomb, he published nothing for the three years prior to detonation of the first Soviet hydrogen bomb on 9th August 1953. That year he was also awarded the title Hero of Socialist Labour; Kapitza states in the Royal Society of London obituary that this was partially for “fulfilling government projects”. Clearly he was in the forefront of the bomb, which represented a huge comeback: less than one year separated the American and Russian hydrogen bombs, while four had distanced their atom bombs. Indeed, the Russian H-bomb was technically superior: it was of a kind Los Alamos had been unable to construct, light enough to be delivered by air. The guiding force behind the US hydrogen bomb, ex-Hungarian physicist Edward Teller, had ironically been Landau’s co-author in a 1936 paper on sound dispersion.

Landau resumed his research from 1953. First to surface was the paper he found most challenging, taking up Fermi’s ideas about multiple particle production in collisions. Landau analysed the expansion of a cloud of emerging particles using the equations of relativistic hydrodynamics. These were valid because the mean free path was far less than the dimensions of the cloud. He solved these asymptotically, using several tricks borrowed from other areas of physics; the whole work displays a broad outlook. He also published on quantum electrodynamics, fluid flow, and many aspects of low temperature theory. His greatest efforts, according to Ginzburg, went into an attempt to develop a theory of second order phase transitions more deep than the self-consistent field approximation. He was particularly appreciative of Lars Onsager’s solution of the two-dimensional Ising model.

The seminars at Moscow, which took place at 11am prompt on Thursdays and lasted the day, were renowned. Questions or interruptions were permitted at any stage, but with “Dau (never the formal Lev Davidovich) conducting, a conclusion would be reached. Outstanding results were entered into a “golden book”, and non-trivial problems arising into a “problems book”, a fertile source of research topics. Conclusions were by no means always favourable to the speaker, and waffle was seized on mercilessly. Landau tended to be overly influenced by his first opinion of speakers. Those who had passed the Theoretical Minimum took a commitment to report and review material at the seminar, a privilege for which in exchange they received Landau’s backing.

In 1958, on his 50th birthday, a party was held. All formalities, even minor, were strictly banned. Landau was presented with his own Ten Commandments: small marble tablets engraved with his ten most significant formulae.

Landau was by this time recognised abroad, and added many international honours to his clutch of domestic ones (though he was not permitted to travel abroad, obviously because of his knowledge of Soviet atomic secrets). These included:

1951 Member, Danish Royal Academy of Sciences (recall Bohr was Danish)

1956 Member, Netherlands Academy of Sciences

1959 Honorary Fellow, British Institute of Physics and Physical Society

Landau with Bohr and their wives outside the Landau’s home, 1961

1960 Foreign Member (equivalent to Fellow) Royal Society of London

1960 Foreign Associate, US National Academy of Sciences Fritz London Prize (USA)

1961 Max Planck Medal (Germany)

1962 brought the tragedy which ended his career abruptly at its height. On Sunday, January 7th, Landau was being driven by a colleague to Dubna. In Moscow’s northern suburbs the car braked sharply to avoid a pedestrian, slewing on the icy surface to stop in the path of an oncoming lorry. In the resulting collision Landau suffered multiple fractures, collapse of all of one lung and part of the other, severe internal damage to the abdomen, and a fracture to the base of the skull. He was rendered deeply unconscious, and in hospital was thought to be dying on several occasions. Few persons suffering such injuries would be expected to survive them; but he hung on with a tenacity belied by his physique.

During his unconsciousness, scores of academics formed a fraternity of volunteers willing to do anything the doctors suggested; at one stage they brought a respirator from the nearby poliomyelitis research institute. The best specialists were summoned to the impersonally named Hospital no. 50, in the Timiriazevsky district. Landau had inspired nothing less than love among his fellow physicists, a capacity given to few.

To minimise trauma, it was decided to repair his body before undertaking any operation on the brain. Late in February, 50 days after the crash, came tentative indications that consciousness was returning. Landau first responded to a request to blink acknowledgement. An international neurosurgical team subsequently decided it better not to operate on the brain. (Non-invasive tissue imaging, which could detect haemorrhage without risk, lay far in the future.) In early April Landau began to recover his speech, reflexes and memory, but only in July did he question where he was, and why. This highlights, as little else, what is presumed by healthy individuals; Landau’s previous achievements only heightened the contrast. Nevertheless he continued slowly to recover. Details are chronicled by Doroszynski. ❯
Sadly, it was emerging that Landau would not significantly recover his talents. He remained apathetic, and detailed thought, rather than reactive conversation or particular memory recall, was largely beyond him. On occasion he spoke of his suffering.

Late in 1962 came the announcement that Landau had been awarded the Nobel Prize in physics "for his pioneering theories concerning condensed matter, especially liquid helium". Precedent was broken by presenting the prize, not in Stockholm, but at Landau’s bedside, by the Swedish ambassador. This award cannot be given posthumously, so it is likely that Landau’s poor health catalyzed a well-deserved honour. That year he also received a Lenin Prize.

Only in 1964 could he at last return home. His physical recovery, though incomplete, was better than his mental. He learned to walk again, though suffering intense frustration. But early in the morning of 1st April, 1968 he died, following an intestinal operation.

The post-war explosion of research, with its ever-increasing specialization, led inevitably to the founding of an Institute of Theoretical Physics in the USSR, in 1964. As tribute, it today bears Landau’s name.

What of Landau’s personality? He was characterized above all by a sharp and quick tongue - he did not suffer fools gladly - and this abruptness was often likened to Pauli. Examples abound. Landau believed that genuinely talented physicists would be known and have peaked by their late 20’s (a notion he disproved by example), and this led to his famous comment "So young, and already so unknown!" At a conference he replied, after others had demurred, that the difference between Pauli and a particular philosophy professor was that Pauli understood [the uncertainty principle]. Landau’s features, at least in early photographs, were intense. Physically he was very thin, and moved angularly. His hands were never still. He chose never to learn to drive. Nevertheless he played tennis and was fond of skiing (at that time, cross-country). He was fond of travel: vacations were often spent driving with Lifshitz. He pined about his name Landau as L’ane ‘Dau: French for “the ass Dau”. He was an inveterate classifier, classifying women according to their degree of beauty and physicists on a logarithmic scale; thus a second class physicist supposedly accomplishes ten times as much as a third class physicist. This scale was already in use by 1929. Einstein alone was rated 1/2, while rank 1 included Schroedinger, Bohr, Heisenberg, Dirac and Fermi. Landau placed himself at 2 1/2, ultimately re-assessing himself at 2. In response to a question he replied: "No, I am not a genius. Bohr is, and Einstein is; I am not. But I am very talented". Undoubtedly this was to be taken literally. Those in the fifth rank he called pathological types; "pathology" was a favourite term of denigration.

Landau did not like the unexpected, and did not alter his opinion easily, although it was so rarely necessary in science as to cause no trouble. In personal contexts this was occasionally more irksome.

Landau was grateful to all correspondents who showed interest in physics, at any level, but if he detected a trace of careerism his reply was sharp. He disposed of one, enquiring in which branch best to specialize, after first giving the answer: that which interests him most. He wrote a definitive letter to one of that pestilential type who claimed to have disproved relativity:

"I must say that your manuscript is lacking in any interest. Modern physics is a tremendous science, based primarily on a large number of experimental facts. You are patently almost completely unacquainted with this science, and you attempt to explain physical phenomena, about which you know little, with meaningless phrases. It is clear that nothing can come out of it. If you are seriously interested in physics, you should not engage in discoveries, but first learn at least a little about the subject. "Modern physics is a complicated and difficult science, and in order to accomplish anything in it, it is necessary to know very much. Knowledge is all the more needed in order to advance any new ideas. It is obvious from your letter that your knowledge of physics is very limited. What you call new ideas is simply prattle of an ill-educated person; it is as if someone who never saw an electric machine before were to come before you and advance new ideas on this subject. If you are seriously interested in physics, first take time to study this science. Some time you yourself will see how ridiculous is this nonsense that came out of your typewriter..."

Although Landau is enjoying himself here, the criticism characteristically remains accurate and constructive. Above all else he was a physicist. The unravelling of nature’s mysteries meant the most to him; his humility in their face, and his sense of duty in passing on his wisdom, amply attest to this.

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1993
Mathematics In Industry
Study Group
Melbourne, Australia
15 - 19 February 1993

Please note in your diaries:
The meeting will be organised by CSIRO Division of Mathematics and Statistics in collaboration with a Melbourne-based Steering Committee with members from Monash University, Swinburne Institute of Technology and BHP Research.

The goals of the Study Group are:

- to stimulate greater awareness in Australian industry of the need for and role of mathematics
- to establish better links between industry and academic mathematicians
- to provide improved university education of mathematicians through
  1. expanded employment prospects for mathematics graduates
  2. fresh research problems for mathematicians
  3. innovative materials for teaching courses
- to provide Australian industry with high level mathematical advice on challenging problems, and to provide opportunity for industrial scientists to receive expert training in mathematical modelling.

The format of the Study Group will be basically unchanged from previous meetings.

For further information, please contact

Dr. N. G. Barton
CSIRO Division of Mathematics and Statistics
PO Box 218, Lindfield NSW 2070
Phones: (02) 413 7702, Fax: (02) 416 9317
noel@syd.dms.csiro.au
In writing he was a perfectionist, needing a collaborator. He worked on the floor or on a settle, never at a desk. As a young man he was very shy; he confessed later to despair at this, which he tried to overcome by conscious effort. He saw it as an obligation to be happy. He applied varied physical and mathematical metaphors, some quite abstruse, to other aspects of life; these were not always in jest. He held his opinions harshly, and gave unsolicited personal advice irrespective of possible offence whenever he deemed it necessary. He believed that interpersonal relationships were ultimately simple and analysable. Clearly he had no sympathy with the idea of the unconscious; one wonders what lay in his own to cause this. When young he disapproved of marriage as a "typically capitalist institution", in pushing a good thing too far. His own marriage was not a success; the most intimate biography of Landau is by a mistress, the journalist Maiya Bessarab. (This contains by far the best selection of photographs.) He adhered to communism, a basically simple creed in sympathy with his conscious rationalism; though understandably not to Stalinism. He never took Judaism seriously, and was characteristically caustic about religious belief. His "school" of physics, though meritorial, was predominantly Jewish, and he made no effort to heal the schism with Bogoliubov’s school. He was fond of literature, poetry, realistic art and cinema, but described himself as musically blind, and positively detested opera and ballet. He was uninterested in chess, another Russian passion. He was an uncompromising purist, for whom all arguments needed justifying, if only by quoting older proven ones. He was interested exclusively in an argument’s quality, and never in unsupported appeals to higher authority. Above all else Landau detested pretension; Lifshitz suggests he disliked opera and ballet because they are more contrived ways of telling a story than literature or cinema. Apparently he did not believe, or perceive, the effect of differing forms of storytelling. He was fond of history. He tried to categorise and quantify everything. The conscious rationalist always dominates.

As a physicist, he was the greatest universalist, and a Nobel laureate in his area of deepest interest. He possessed the rare capacity to inspire in his colleagues not only respect, but love. He was always accessible to all for scientific discussion, and he led from the front a profoundly successful school of physics. (In these qualities one is reminded, half a world away, of Feynman.)

While the tragedy of his loss - it is not too strong a word - left physics the poorer, his achievements are lasting. Physicists today owe a major debt to his teachings and scientific ideals.

### A Summary

<table>
<thead>
<tr>
<th>Date</th>
<th>Age</th>
<th>Event</th>
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<tbody>
<tr>
<td>1908</td>
<td>0</td>
<td>January 22nd: born, Baku, to Jewish parents.</td>
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<tr>
<td>1921</td>
<td>13</td>
<td>Completed school. Year at Baku Economic Technicum.</td>
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<tr>
<td>1922</td>
<td>14</td>
<td>Baku University: physics-maths; chemistry.</td>
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<tr>
<td>1924-7</td>
<td>16-19</td>
<td>Leningrad University: physics. First papers published.</td>
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<tr>
<td>1926-9</td>
<td>18-21</td>
<td>Leningrad Physicotechnical Institute. Reads the early quantum papers.</td>
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<tr>
<td>Oct 29-Mar 3</td>
<td>21-23</td>
<td>European travel. Meets the greats, in particular Bohr.</td>
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<tr>
<td>1932</td>
<td>24</td>
<td>Head, theoretical division, new Ukrainian Physicotechnical Institute, Kharkov.</td>
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</table>

- **1933** 25 Chair, Mechanical Institute, Kharkov, simultaneous with previous appointment.
- **1935** 27 Chair, physics dept, Kharkov University. Developed teaching ideas and seminar.
- **1937** 29 Kharkov: married Cora Drobantseva. Moscow: head of theoretical division in Kapitza’s new Institute of Physical Problems.
- **1938** 30 Imprisoned; released after a year.
- **1941** 33 German invasion. Institute evacuated to Kazan. Paper on He-11.
- **1941-45** 33-37 War work, less research. Engineering committee of the Red Army.
- **1946** 38 Son, Igor, born. Elected to Academy of Science.
- **1950-53** 42-45 H-bomb project.
- **1953-61** 45-53 Broad research; teaching; national and international honours.
- **1964** 56 Discharged from hospital. Largely incapacitated.
- **1966** 60 April 1st: died after operation.

### Other Biographies & References

- A bullet denotes a complete book on Landau.

- Landau - A Great Physicist and Teacher (A. Livanova; English translation: Pergamon 1980)


  Collected Papers, ed D. ter Haar; Intro p(xiii) (1965; Pergamon)

  Bird of Passage (R. Peiers; autobiography, Princeton 1985)


  * The Man They Wouldn’t Let Die. Alexander Dorozynski, Seeker and Warburg (1966)


  Obituary by Kapitza and Lifshitz


  Landau’s Attitude Towards Physics and Physicists. V.L. Ginzburg, Physics Today, May 1989, p54


  Australian & New Zealand Physicist Volume 29, Number 10, October 1992
Table 1 The editor of the Collected Papers, Dirk ter Haar, has suggested a formal organisation of Landau's contributions into the following groups of topics: 1. low-temperature physics; 2. solid-state physics; 3. plasma physics; 4. hydrodynamics; 5. astrophysics; 6. nuclear physics and cosmic rays; 7. quantum mechanics; 8. quantum field theory; 9. miscellaneous. Out of these main topics we shall address here only several papers from groups 6 and 8. They deal with what has often been regarded to be a peripheral aspect of the author's interest, which was included neither in the citations for the Nobel Prize (1962) nor the Fritz London Award (1960) - though grossly embraced in that for the third international prize, the Max Planck Medal (1960) - i.e. the problems of quantum field theory and high-energy physics. Table 1 lists the place of these papers within Landau's scientific life.

<table>
<thead>
<tr>
<th>Year</th>
<th>High-energy physics &amp; quantum field theory</th>
<th>Astrophysics and nuclear physics</th>
<th>Solid-state physics &amp; low-temperature physics</th>
<th>Miscellaneous</th>
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</thead>
<tbody>
<tr>
<td>1930</td>
<td>Quantum electrodynamics (1)</td>
<td>Nuclear reactions</td>
<td>Diamagnetism</td>
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<td>1932</td>
<td></td>
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<tr>
<td>1934</td>
<td>Bremsstrahlung, pair creation</td>
<td>Star theory</td>
<td>Susceptibility</td>
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<tr>
<td>1936</td>
<td>yy-scattering</td>
<td>'Neutronic' stars</td>
<td>Superconductivity</td>
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<td>1938</td>
<td>Cascade theory (2)</td>
<td></td>
<td>Magnetic domains</td>
<td></td>
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<tr>
<td>1940</td>
<td>Nuclear forces</td>
<td></td>
<td>Helium-II</td>
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<td>1942</td>
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<td>1944</td>
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<td>pp-Scattering</td>
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<td>Turbulence</td>
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<td>1946</td>
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<td></td>
<td>Explosive detonations</td>
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<tr>
<td>1948</td>
<td>Two-photon system e+e^−</td>
<td>Nuclear reactions</td>
<td>Superconductivity</td>
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<tr>
<td>1950</td>
<td></td>
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<td>Plasma oscillations</td>
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<td>1952</td>
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<tr>
<td>1954</td>
<td>Multiple particle production (3)</td>
<td>Limits of quantum field theory (4)</td>
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<tr>
<td>1956</td>
<td>Beyond quantum field theory (5)</td>
<td>CP-Conservation</td>
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<td>1958</td>
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<td>1960</td>
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* Akademik L.B. Landau. by A.A. Abrikosov, Nauka, 1965 (no English translation) Material from this source has not been incorporated
* Pages from Landau's Book of Life. Maiya Bessarab. Moscow Worker Press, 1971 (no English translation)
* Proceedings of the Landau Memorial Conference, Tel Aviv, Israel, 6-10 June 1988, eds. E. Gotsman, Y. Ne'eman & A. Voronel (Pergamon 1990)

### Selected Works

Course of Theoretical Physics Vols I-IX; Pergamon. "Landau and Lifshitz"
Shorter Course of Physics Vols I & II (with Lifshitz); Pergamon
What is Relativity? (with Yu.B. Rumer) Oliver and Boyd 1960
Physics for Everybody (with A.T. Kitaiorodskii). Moscow 1963
Collected Papers (see above). Not all of Landau's papers are included in this volume: see its Appendix B, p.833

* The author thanks Sir Rudolf Peierls, Professor David Shoenberg and Dr. D. ter Haar for information and suggestions, and accepts responsibility for any inaccuracy.
Beyond Southern Skies

The giant Parkes radio telescope celebrates its 31st anniversary this month, and its story has at last appeared in the bookstores for all Australians to enjoy and to marvel at the fortunes of its creators. For much of its productive life it has led the world in performance, prompting its first director, John Bolton, to remark that "The Parkes telescope is probably one of the most successful research instruments ever built". But research was not everything the Parkes radio telescope played a crucial role in the rescue of the Apollo 13 astronauts and the success of many other space missions. Its productive life is far from over, and to this day it attracts top-ranking overseas radio astronomers to our country.

Before overwhelming ourselves in a tide of nationalistic pride, we need to remember that half of the brains that inspired it were British and half of the money that paid for it came from America. Even so, the chronicle of the Parkes telescope's conception, construction and operation is an inspiring tribute to the men and women who placed Australia at the forefront of the science of radio astronomy.

This newly published account of the Parkes radio telescope is titled "Beyond Southern Skies" and was written by Peter Robertson, well known as the editor of the Australian Journal of Physics. He has spent many years painstakingly gathering material and taping interviews in order to present a definitive history. It helps to explain why this book has been so long coming: after all it is over a quarter of a century since the story of Parkes' older sister at Jodrell Bank came off the presses.

The wait has been worth it, if only for the memories it awakens. The book records that Australian funding for the Parkes telescope was very difficult to raise because it was considered that such a project would not bring any clear economic benefit to Australia. Times have not changed. The driving force for building the Parkes telescope, Welshman Taffy Bowen, lamented that he was "surprised to find how scared the rugged Australians are of going one better than anyone else in the world".

The book is divided into three sections, each dealing with an essential aspect. The first, dedicated to Australian Joe Pawsey, traces the history of the radiophysics division of the C.S.I.R.O. from the days of its birth out of secret wartime radar research. The second, dedicated to Taffy Bowen, describes the planning and construction of the huge dish antenna. And the third, dedicated to Yorkshire-born John Bolton, is an often thrilling account of the achievements of the Parkes instrument.

A few minor errors of fact have crept into this otherwise splendid book. I believe that common-anode radar operation was made possible by the transmit-receive switch developed in only six weeks at the start of World War II by John Banwell (J. Insta. Elect. Engrs, Pt IIIA, 93 (No.3) S45-S51, 1946). Pawsey and Minnett would have learned about it through the close cooperation which existed between Britain and Australia in radar development. Another slip attributes the first laser to the Bell Labs and not the Hughes Laboratories. Also, visual observers in the southern hemisphere, mainly in New Zealand, noted that the Sputnik I carrier rocket was drifting in a separate orbit from the Sputnik itself days before the Jodrell Bank radio telescope located it. Apart from one meaningless sentence: 'The binary system lies in a rate of 86 times per second', the book appears to be free of typos.

"Beyond Southern Skies" is a fine Australian production, well written by Peter Robertson, printed in Melbourne on quality paper and excellently bound. It is published by the Cambridge University Press and costs $59.95. Good value for any library or personal collection. Every high school in Australia should have a copy to help inspire intelligent young people and convey a very positive message about Australian science.

R.A. Lewis
Department of Physics
University of Wollongong

Quantum Mechanics on the Macintosh

S. Brandt and H.D. Dahmen
Springer-Verlag, Berlin, 1991
x + 306pp., DM 98
(hardcover, with disks)

This is a course on quantum mechanics consisting of a textbook and interactive computer programs, originally published two years ago as Quantum Mechanics on the Personal Computer for use with an IBM compatible platform. It was reviewed in this Journal in volume 28 (1/2). This book is adapted for use on Apple/Macintosh machines.

Firstly some negative comments. Many will find the user interface a bit off-putting. Anyone used to the very slick software available elsewhere will wonder why the authors didn't make more use of animation, of the mouse, of menus rather than question-and-answer input. The reason is that these programs were written before all these things were so widely available on Personal computers. It is a drawback common to all pedagogical software - it will start to look dated very quickly.

Reviews

Hot Carriers in Semiconductor Nanostructures: Physics and Applications
Jagdeep Shah (Ed)
Academic Press, San Diego, 1992
xvi + 508pp., $US99 (hardcover)

Hot carriers are electrons or holes pushed (commonly by photoexcitation or by a strong electric field) out of thermal equilibrium with the crystal lattice. In semiconductor devices hot carriers dominate high field transport and so become increasingly important as device size decreases and speed increases. Several books treat hot carriers in bulk material but this is the first to specifically deal with them in nanostructures. The central question is how the non-equilibrium carriers shed their excess energy. For single quantum wells in GaAs (the system almost exclusively treated), the answer is largely by the electron-LO phonon interaction (although several processes are involved on the fs, ps, and as timescales).

Theoretical decay times were at first much shorter than experiments indicated; the creation of "hot phonons", which return energy to the electrons, is thought to be the chief reason although other processes, such as screening, play a role. Phonon confinement in quantum wells (its nature, not its occurrence) is an interesting unresolved issue. The book is well presented and displays an unexpected unity given its conference origin. The numerous authors strive to begin at an elementary level and conduct the reader through to the latest research results. Both pure and numerical (Monte Carlo) theory are presented as well as fundamental (optical) and device-oriented (transport) experiments. This book is certainly of value to those interested in its subject.
BOOK REVIEWS

Slightly more worrying is the idiosyncratic language of abbreviations which control the programs, reminiscent of older operating systems. Modern users (particularly on the Mac) have come to expect more user-friendliness. This shouldn’t matter; but it means that if you or your students want to use this book, you will have to invest a lot of time learning how.

But, all that being said, the basic idea behind the book(s) is superb. Quantum mechanics is absolutely basic to so much of modern physics, and students need to develop the same kind of familiarity with it as they have (ideally) with classical mechanics.

It is often argued this is not possible: that Quantum Mechanics is intrinsically anti-intuitive: that students understand classical mechanics in a way they will never understand quantum mechanics because they experience it in the everyday world. This is nonsense. The Newtonian description of classical physics was also once considered anti-intuitive. Yet generations of students have developed Newtonian intuition by observing nature carefully and by doing hundreds of simple calculations to cement their understanding. Up till now they have not been able to do this with QM because the subject has so much difficult mathematics.

And that is where Brandi and Dahmen make their invaluable contribution. This book is a treasure-house of simple exercises that students can work through, and see the results of calculations. There are other books starting to come out also filling this need: but theirs was one of the first and certainly the most comprehensive to date.

J.D. Johnston
School of Physics
University of Sydney

Geometry and
Theoretical Physics

J. Debrus and A.C. Hirshfeld (Eds)
Springer-Verlag, 1991
x + 323pp., DM78 (hardcover)

The subject of Geometry is now so general that it has been incorporated into much Mathematics. Theoretical Physics has been increasingly, if not totally geometrised. In fact, the capacity of humans for the inner mental visualisation of 'space' has unleashed powerful intuitive and systematic mathematical methods in physics. One is reminded of such names as Riemann, Minkowski and Einstein (space-time) and Hilbert, Dirac and von Neumann (state space), and their influence.

All physics students increasingly need geometric skills. For example, entering students should have preparation in Euclidean geometry, trigonometry and vectors, but this is decreasingly the case. Later year students need vector calculus, however surely the day has come when a knowledge of differential forms, exterior differential calculus, and group theory should also be common place. Graduate students, particularly in theory, have their special needs also.

The present volume is the result of a graduate school held at Bad Honnef in February 1990 under the auspices of the German Physical Society. There were 11 courses delivered by 15 lecturers (authors), as well as a memoir (in German and English) on the scientific work and ideas of Wolfgang Pauli, presented by his former student, K. Bleuler. As you might expect the courses are variable in quality and importance.

The aim of the set of courses was to give the student those techniques required to understand gravitation, gauge field theories, and related subjects. It begins with a course on Fibre Bundles, which is applied in subsequent courses to various subjects. These include: deformable media, fermions and gravity, braided group statistics, infinite dimensional algebras and field-theory, anomalies in quantum field theory, the index theorem, Wess-Zumino consistency conditions, the PCT theorem, and knots. Most of these were very short courses.

I feel that the course on Fibre Bundles was unnecessarily abstract, dry, and short, totally devoid of illustration. On the other hand the only long course (by Hohn, Lemke and Mielke) on Fermions and Gravity, is a model of clarity and insight. It may well become a standard reference to the understanding of Fermions in quite general space-time manifolds. For this article alone the book is worth it.

The book is sufficiently good, and sufficiently inexpensive for you to have one in your library. However, inspite of the general title, it will not solve all your geometry problems.

G.L. Opat
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University of Melbourne

Polarization of the Vacuum and a Quantum Relativistic Gas in an External Field

A.Ye. Shabad
vi + 236pp., US$80.75 (paperback)

In a quantum treatment of an electron in a magnetic field $B$, the motion perpendicular to the magnetic field lines is quantized, with the energy level separation determined by the cyclotron energy $\hbar B/m$. Near the critical magnetic field $B_c$, defined such that the cyclotron energy is equal to the rest energy $m^2/c^2$, relativistic quantum effects are necessarily important. There is an analogous critical electric field, which is defined to have the same energy density as $B_c$. In the presence of such fields familiar physical processes become strongly modified and, more interestingly, some otherwise forbidden effects become allowed, and intrinsically new effects appear.

Otherwise forbidden processes that become allowed include the decay of a single photon into an electron-positron pair, the inverse process of annihilation of a pair into one photon, and the splitting of one photon into two photons. Intrinsically new effects that appear include the birefringence of the vacuum and spontaneous pair creation in a superstrong electric field.

The monograph by Shabad contains a detailed review of the contributions of Shabad and his co-workers at the Lebedev Institute to this field. The text begins with a 20-page introduction, which provides a readable overview for anyone with an interest in the field, and which is complemented by a comprehensive list of detailed references. The body of the book includes three chapters on dispersion, the role of psrotium and the response tensor for an electron gas, all for the case of a superstrong magnetic field. The magnetic fields in neutron stars are close to the critical field, at least for radio pulsars, and in this context many of these effects are of direct interest. The application to pulsars is discussed briefly at the end of chapter 3.

The various processes mentioned above are described using the polarization of the vacuum. The basic idea, which dates from the work of Weisskopf in the 1930s, is that a photon spends part of its time as a virtual electron-positron pair, and the magnetic or electric field affects this virtual pair thereby affecting the dispersion of the photon. The vacuum itself becomes analogous to an anisotropic material medium, in that the radiation splits into two modes, neither of which have phase speed equal to the speed of light. Near the threshold for pair production, the photon state becomes strongly coupled to the state of a real bound electron-positron pair, and a photon can evolve continuously into a pair in an inhomogeneous magnetic field. As in other contexts, the polarization tensor has an anthermitian part that may be used to treat dissipation processes such as one-photon pair creation. All these and other processes are discussed in detail. There are also five Appendices.

The presentation is in the traditional Russian style of theoretical physics, with at most terse explanations of the algebra.
BOOK REVIEWS

It will be hard-going even for knowledgeable readers to fill in the mathematical details. It is Volume 191 of the Proceedings of the Lebedev Institute, edited by N.G. Basov. As with many Russian translations, the equations have been copied directly from the original. No date is given for the original Russian publication; the latest reference I found was a preprint dated 1986. (Shabad went into politics as a supporter of Yeltsin about that time.)

This book gives such a complete coverage of the field that it is likely to become a standard reference.

D.B. Melrose
University of Sydney

Basics of Interferometry

P. Hariharan
Academic Press, San Diego, 1992
xiii + 213pp., US$39.95 (hardcover)

Optical interferometry was long regarded as an esoteric method confined principally to national standards laboratories. Today, however, it is a commonplace tool which has wide applicability in fields as diverse as microscopy, non-destructive testing and interferometric sensing. There are of course many texts available, including the excellent "Optical Interferometry" (Academic Press, 1983) by the same author and one immediately wonders if we really need yet another book on interferometry.

Most texts on the subject follow a fairly predictable pattern: an introduction which presents the theory of coherence followed by a detailed analysis of the classic interferometric configurations. In most cases it is assumed that the reader is already familiar with classical optics and the emphasis is strongly on the physical optics side. A scientist or engineer who is not an optician and who wants to know whether interferometry can be used to solve a particular problem will not find the answer in the standard textbooks.

'Basics of Interferometry' is intended for this non-specialist market. By setting out the basic concepts of interferometry in a non-theoretical way it will assist the reader in deciding whether optical methods are appropriate for a particular application. The key concepts in each chapter are highlighted, again making it easier for someone to quickly find the relevant material. Most chapters include examples and problems (with fully worked solutions) which illustrate specific applications.

Once you have decided to use optical techniques in a particular application you are faced with the problem of actually putting the optics together. This is also addressed by 'Basics of Interferometry.' There is even a chapter called Choosing an Interferometer which includes information on suppliers of optical equipment. Hariharan assumes that the reader is familiar with the basics of geometrical optics but includes practical details about aspects of interferometry which are rarely mentioned in standard textbooks. There are appendices, for example, describing how to set up a Twyman-Green interferometer and how to align a Mach-Zehnder interferometer.

In summary, this is a unique book on interferometry which is ideally suited for the scientists and engineers who are not professional opticians but who may want to use interferometry as a tool. Professionals will also find this book useful, since it concisely summarizes a great deal of information about all aspects of interferometry. I highly recommend it.

W.J. Tango
University of Sydney

Reliability Problems of Semiconductor Lasers

P.G. Eliseev
viii + 305pp., US$87 (paperback)

This book will be of interest to a wider readership than its title might suggest. It presents a very complete discussion of the defects in semiconductor lasers which arise from the materials, the structures, the fabrication processes and the long and short term evolution of these defects under different operating conditions. The aim of this discussion is to account for the limitations that these static and evolving defects have on the reliability of the semiconductor laser in optical communications and optical information storage/retrieval systems, where longevity of the device is a prime concern. The outline on reliability functions and the statistics of device failure will be of value to systems' engineers wanting to know what a laser lifetime of 10^9 hours really means, statistically.

Much of the discussion, of growth defects, impurity defects, dislocation defects, and degradation defects, is relevant to semiconductor devices in general. The defects generated by the high optical power density within and at the surface of the active region of the laser are the additional topics that would not be covered in more general books on defects in semiconductor devices. The author presents results from numerous experimental studies and the physical models developed to agree with these. Many of the models are phenomenological. Articles referenced date up until 1986 and come from the international literature, not just the Soviet literature.

Deborah Kane
School of MPCE
Macquarie University

New Books

Physics of High Temperature Superconductors

S. Maekawa and M. Sato (Eds)
Springer-Verlag, Berlin, 1992
x + 437pp., DM118 (hardcover)

Exposure Criteria for Medical Diagnostic Ultrasound: 1 Criteria based on Thermal Mechanisms

National Council of Radiation Protection & Measurement, Bethesda MD, 1992
xi + 278pp., US$25 (paperback)

High Energy Radiation from Magnetized Neutron Stars

P. Meszaros
University of Chicago Press, 1992
xiii + 531pp., US $39 (paperback)

Electromagnetic Processes in Dispersive Media

D.B. Melrose & R. McPhedran
Cambridge University Press, 1991
xxii + 407pp., A$99 (hardcover)
CONFERENCE & MEETINGS

1992

November 12-14
16th Scientific Meeting of the Australian Society for Biophysics
University of NSW, Sydney
Prof. Hans Coster, Department of Biophysics, University of NSW
PO Box 1, Kensington NSW 2033, Phone (02) 6974583, fax (02) 663 3420

November 25-27
Australian Acoustical Society Annual Conference 1992, Ballarat, Victoria
John Upton (Convener), Phone (03) 370 7666 or (03) 370 7166,
Fax (03) 370 0332, Geoff Barnes, phone (03) 720 1206, fax (03) 720 6952

December 1-5
ICPE Conference on Physics Education for Development, Philippines
Prof. Geoffrey I. Opat, School of Physics, The University of Melbourne,
Parkville VIC 3052, Australia. Phone (03) 344 5121, fax (03) 347 4783

December 11
Dr. Geoffrey Fletcher's Festschrift Symposium on
"Electrons in Solids - The 1990s and Beyond".
Department of Physics, Monash University, Clayton VIC 3168
Phone 61-3-565 3630, fax 61-3-565 3637

1993

January 11-29
6th Physics Summer School: Modern Perspectives of Many Body Physics,
Canberra, Australia
J. Mahanty, Dept of Theoretical Physics, R.S.Phys.S.E., ANU,
GPO Box 4, Canberra ACT 2601 Australia
Phone (06) 249 2952, fax (06) 249 4676

February 3-5
8th Conference of the Australian Optical Society
University of Sydney, NSW 2006 Australia
Chair of the Organising Committee: Dr Brian James, School of Physics,
University of Sydney NSW 2006. Phone (02) 692 2599, fax (02) 660 2903
E-mail: james@physics.usyd.edu.au

February 7-11
AMC 93 - The 29th Australian Applied Mathematics Conference
Hahndorf, Adelaide Hills, Adelaide, Australia
Conference Secretary, 29AMC, Department of Applied Mathematics
The University of Adelaide, GPO Box 498, Adelaide SA 5001
E-mail (preferred) to: amc93@maths.adelaide.edu.au

February 9-12
WAGGA 93 - 17th Australian Institute of Physics Condensed Matter Physics
Meeting. Charles Sturt University, Wagga Wagga, New South Wales, Australia
Contact: John Bell, Dept. Applied Physics, University of Technology, Sydney
Phone (02) 330 2213, fax (02) 330 2219 Message (02) 330 2206
E-mail: wagga@phys.uts.edu.au

February 15-19
The First Australian - Asian Conference on Radiation Science and Nuclear Medicine,
at Macquarie University, Sydney, Australia
A/Professor Ron Cooper, Convenor, ANSIE/IAIRR Sub-committee

February 15-19
Nuclear and Particle Physics Group (NUPP) School
Victor Harbour, South Australia
Contact: Prof. A. W. Thomas Phone (08) 228 5113, Fax (08) 224 0464
E-mail: athomas@physics.adelaide.edu.au

March 7-19
Japan Physical Society International Spring School, Tokyo
Contact: Prof. S. Homma, INS Tokyo, Fax +81-424-64-9480

March 29 - April 1
The Physical Society of Japan - 48th Annual Meeting
Tohoku University, Sendai, Miyagi-ken, Japan
The Physical Society of Japan, Room 211, Kakiz-Shinko Building,
3-5-8 Shiba-koen, Minato-Ku, Tokyo 105, Japan
Phone 81-3-3434-2671, fax 81-3-3432-0997

April 14-16
OzCUPE: The first Australian conference on Computers in University Physics Education
Dr. Ian D. Johnston, School of Physics, University of Sydney, NSW 2006
Phone (02) 692 2637, 692 2537, fax (02) 660 2903. E-mail: idj@physics.usyd.edu.au
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