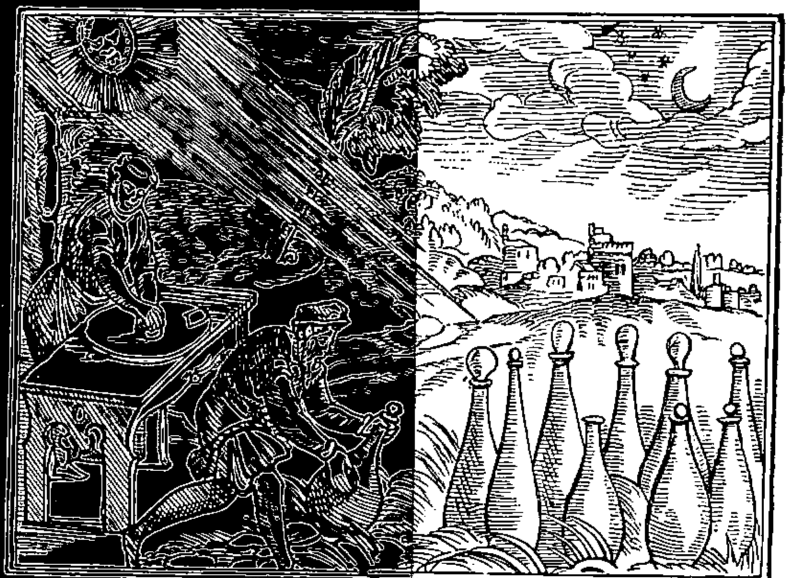
CHEMISTRY IN NEW ZEALAND

JOURNAL OF
THE NEW ZEALAND
INSTITUTE
OF CHEMISTRY



Vol. 36, No. 5, October, 1972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CHEMISTRY IN NEW ZEALAND

Journal of The New Zealand Institute of Chemistry

Vol. 36, No. 5, October, 1972

Published bi-monthly by the New Zealand Institute of Chemistry Inc. (P.O. Box 250, Wellington)

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Editor: Miss J. Mattingley, M.Sc., A.N.Z.I.C.
P.O. Box 250, Wellington.

Advertising Manager:
D. Howard, M.Sc., A.N.Z.I.C.
P.O. Box 250, Wellington.

Printed by David F. Jones Ltd., Wellington.

The Cover by courtesy of Dr. H. Keyser
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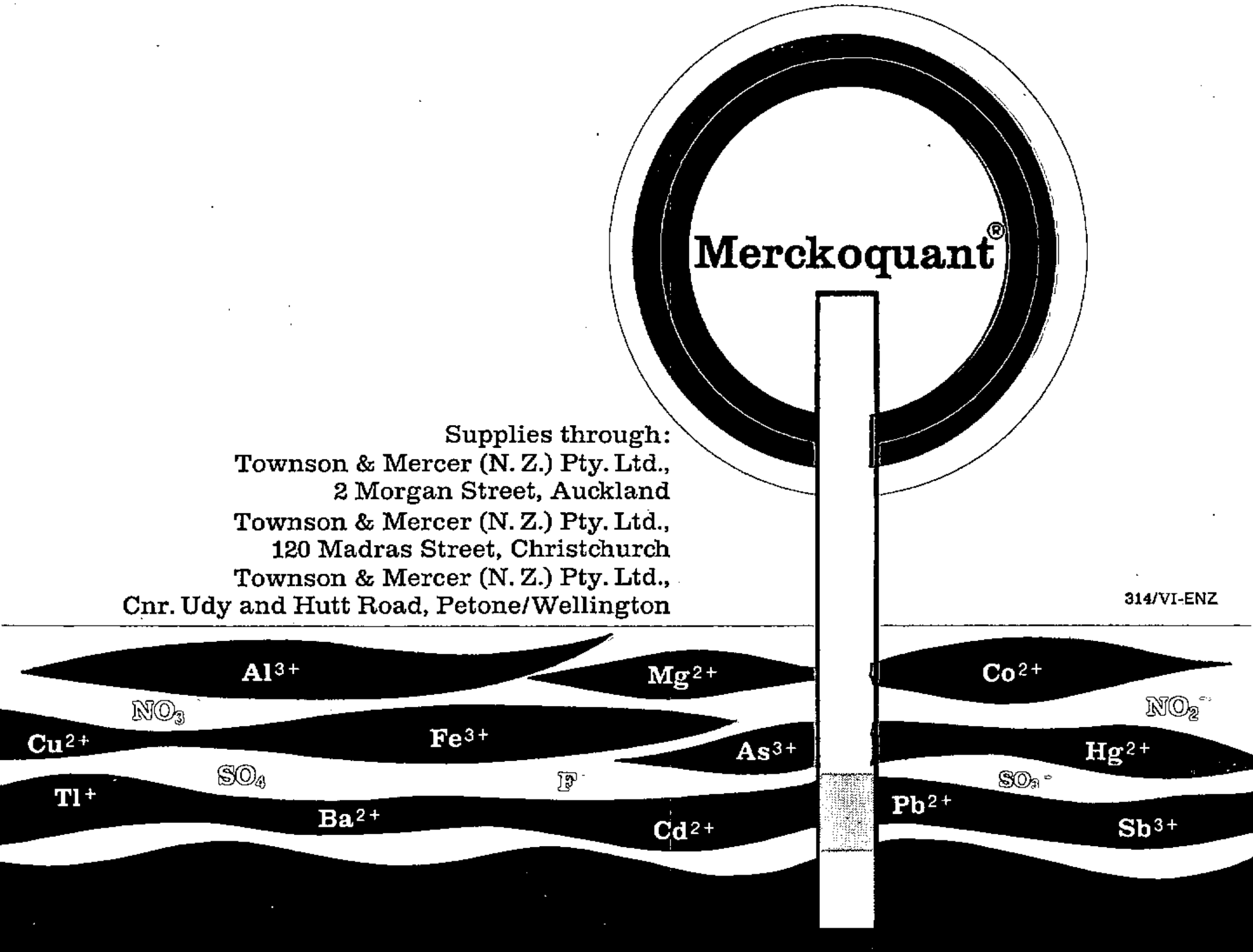
Distribution. The Registrar:
D. J. Hogan, B.Sc., A.N.Z.I.C.
P.O. Box 1926, Christchurch.

Honorary General Secretary:
Prof. W. E. Harvey, M.Sc., Ph.D., F.N.Z.I.C.
P.O. Box 250, Wellington.

Employment Officer:
E. S. Borthwick, M.Sc., A.R.I.C., A.N.Z.I.C.
P.O. Box 2091, Wellington.

The Journal of the N.Z. Institute of Chemistry has an audit circulation Certificate of New Zealand Advertisers. Latest audited circulation (March 1969) 1150.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GUEST EDITORIAL

WHERE HAVE ALL THE CHEMISTS GONE?

The large number of graduating chemists who are eligible for N.Z.I.C. membership but who are not are becoming members has been a matter of concern to your Council for some time. It is estimated that there are about 200 chemistry graduates from New Zealand Universities each year, yet our net increase each year is only one quarter of this. Since 1969 the total membership has increased from 994 to 1136, a gain of 20%. Why are the remainder not becoming members?

It appears that the Institute is not attractive to chemists at large and to most members it is nothing more than "just another subscription". For any organisation to be effective and inviting it should be meaningful to its members. Conversely, members should take an active part in their organisation. While an analysis shows that nonresearch orientated chemists comprise about half the membership it has been the University and research chemist who has kept the Institute alive and accordingly the Institute has reflected their viewpoints. The recent Council decision to make entry into the Institute more difficult for non-graduate applicants reflects the power this quarter has over Institute affairs. The onus is clearly then on all members to make sure their membership is meaningful and not vice-versa. It is clear from Auckland Branch activities that times of meetings, speaker attractiveness, luncheon meetings and symposia have a big influence on member participation. Only through members involvement with the Institute will it be truly representative and give service to its members and so promote "the science and practice of chemistry".

To encourage members to become more involved in Institute affairs, maybe it should become a statutory body, registering chemists, having a permanent secretariat and charging high annual subscriptions. Alternatively the Institute should become a registered Union. However, maybe the Institute should continue to be a reasonably unperturbed uninfluential "club". What do you think?

Auckland Branch Editor.

The death has occurred of our first Vice-President, James Eric Allan, of Hamilton. An obituary will appear in the December Journal.

PRESIDENTIAL ADDRESS TO CONFERENCE, AUGUST, 1971

W. A. McGillivray

This address was given at conference in Hamilton, August 1971. Various factors have delayed its publication, but it is still a timely comment on the aims and objects of our Institute. Dr McGillivray referred to his Presidential addresses to Branches throughout his year of office, and continued by referring to a rather unusual duty which befell him.

As my last duty as President of the Institute, I will be Guest Speaker at the 25th Jubilee Celebration of the Korean Chemical Society in Seoul. I will, on that occasion, be conveying warm greetings from the New Zealand Institute of Chemistry to the Korean Chemical Society, and congratulating them on your behalf on their first quarter century.

It is some years now since the New Zealand Institute of Chemistry celebrated its 25th Jubilee—October 1955 to be exact—and since that time we have seen considerable change in the Institute, in the profession of Chemistry, and in the various other organisations with which our Institute is associated in one way or another. Not only the scientific scene, but every aspect of our way of life in New Zealand is changing, and is likely to change increasingly rapidly as we move through the 70's. We have appraised these changes at many levels: we have attempted to plan for our future agricultural and industrial development; we are conscious of the demands which changing agricultural policies and increased industrialization will make on our resources and facilities; we have thought of our positions individually as chemists; but I doubt if we have given much consideration to the changing role of the Institute in the different environment in which it must operate in the future.

Certainly, a number of pebbles have rippled the surface and given indications of the storms to come. We have been concerned about a number of things like falling attendance at Branch meetings, but these problems were relatively easily solved—a social hour with a few drinks and a bite to eat, and enough members to justify holding a meeting were prepared to stay on and listen to a speaker. There was always some sort of group—a Standing Committee or something or other—in Wellington to handle any other matters, and whether through confidence in our officers or apathy, it did not seem very necessary for us to worry about these or the consequences of the decisions taken.

Obviously things are not quite as bad as all this. The Institute is numerically the strongest scientific body in New Zealand, and its membership continues to grow. Nearly all young chemists graduating in New Zealand consider it worthwhile joining the Institute, and older chemists think it appropriate to continue their membership. Clearly as a professional body the Institute is fulfilling a need for its members, even though only some 10-20 percent take advantage of its learned society activities. However, this does not necessarily mean that the form which has existed for the past four decades is not going to need some fairly drastic overhaul in the rapidly changing next decade.

When we first join the Institute of Chemistry, we receive along with our Associate Certificate a set of Rules and Code of Ethics of the Institute. I suppose most of us scan through these and then put them on one side. As a matter of fact I had some difficulty finding mine, and when I did find it, it opened automatically at Rules 9 and 10, the Associateship and Fellowship Rules—a hangover from the time I was on the Membership Committee.

Tonight I would like to refer to one of the Rules which tends to get flicked past as we seek out one of the later, more detailed Rules, in order to clear up some procedural point. I refer to Rule 3, which deals with the objects of the Institute, and I would like to make a few general comments on a number of these objects and the way in which we interpret them. Some of course are very general in nature, such as you would expect to find in the rules of any professional body. Our first object is to promote the science and practice of chemistry in all its branches, and the usefulness and efficiency of persons engaged therein. We exist also to raise the status and to advance the interests of the profession of chemistry and of those engaged therein. The Institute exists to promote honourable practice and, fortunately something we rarely if ever have to do, to suppress malpractice, to settle disputed points of practice, and to decide all questions of professional useage and etiquette.

These are our main objectives, and most of the other Clauses in Rule 3 really outline how we are to achieve these objectives: by admitting suitable persons to membership and conferring rights and privileges on them; by collaborating with other organizations whose objects are similar to our own; by arranging lectures, exhibitions, classes and conferences; by publishing material of various types; by circulating statistics and other information relative to any branch of chemistry; by holding examinations and issuing certificates. Finally, we have a further object, which again is seldom evoked—to accept gifts.

No one, I am sure, would disagree with these objects, particularly the last, but being intentionally couched in rather general terms, they are capable of a number of different interpretations; I would like to comment briefly on one or two that have been giving some concern to recent Councils and to some Branches.

Membership

First, this question of who should be admitted to the Institute. I think I am right in saying that when the Institute was first formed, all practising chemists, irrespective of their precise qualifications, were given the opportunity to become Associates or, in the case of those with more seniority, Fellows. Since that time the main means of admission to the Institute of Chemistry has been through a university degree with some branch of chemistry as the major subject. Until recently the requirement for Associateship in addition to this university degree, was a minimum of two years subsequent study or prac-

The President in his address to the Institute at last year's conference commented on the way in which chemistry is blamed for all the bad things which are by-products of technology. Industrial pollution, disturbances of environmental balance — all unpleasant things happen because of the applications of chemistry.

The New Zealand pulp and paper industry is based on our enormous man-made forests of exotic trees. 'Man-made' and 'exotic' are terms which raise the hackles of environmentalists . . . anything so described must upset the balance of nature and be responsible for the destruction of defenceless plants and animals. However, a recent Forest Products brochure describes how these man-made exotic forests, properly managed, are responsible for a new lease of life for native plants, birds and insects. When planting of the volcanic wastelands began in 1925 in the Tokoroa district the area was almost totally devoid of birdlife. Now, with proper management of these exotic forests, the pines have provided shelter for the growth of native ferns and shrubs; insects, berries and nectar are plentiful and the native birds have spread in great numbers into these forests.

Certain areas of the company's plantations have been set aside for public recreation. In due course further development will include signposting, tracks, etc., enabling the public to locate and enjoy these areas with ease.

Here at least is one example of an industry in which chemistry is much involved yet environmental enrichment has followed.

There must be others.

J.M.

tice in some branch of chemistry. Last year we introduced changes which essentially increased this qualifying period from two to four years, and at the same time provided for a Graduate Membership category to which young graduates could be admitted until they had gained necessary post-graduate experience in chemistry.

Other qualifications are also recognized for Associateship of the Institute, such as an Associate of another chemical institute like the Royal Institute of Chemistry, which has membership requirements at least as high as our own Insttiute. We do also provide for admitting persons who have passed an examination prescribed by Council, and have had adequate practical training in Chemistry. In general, this is interpreted as passing a University examination in some branch of chemistry equivalent to Stage 3, and passing an appropriate practical examination. With the changes introduced last year in the Assocaiteship regulations and the introduction of Graduate Membership, it was intended that entry by examination would be phased out. Persons who do not possess the above more usual types of qualification may also be admitted, if it can be established that their qualifications are equivalent to the degree in chemistry and the practical experience thereafter, but admissions under this clause are relatively rare. Successive Councils and Merbership Committees have tended to place different interpretations on this clause, and it has come to represent a large grey area in our Membership Regulations that requires clarification and definition. In tidying this up, we have to consider not only our own Institute, but equivalence and recognition of our qualifications by other chemical institutes overseas, and also our relationship to other professional bodies in New Zealand with whom we may hope to have increasingly closer ties in the future.

I think we would all agree that we would wish to see within the ranks of the Institute of Chemistry, all those who are competent to practice some branch of chemistry. In theory, none in this category who wish to join the Institute would be excluded, but in practice entry for those who have other than the fairly standard qualifications is difficult, and I suspect that some members of the Institute feel it should be impossible.

The problems of membership of the Institute have been discussed in quite a lot of detail by Council and by individual branches, and I should stress that my comments here are my own and do not necessarily represent the opinion of the majority of members or of Council. Membership of a professional body such as the Institute of Chemistry, although it has no legal significance in our case, should indicate a certain standard of professional competence, and for this reason if for no other, no one would wish to see a too open door policy towards membership of the Institute which could lead to any lowering of professional standards. At the same time I would suggest that our present emphasis on a particular University qualification may be losing relevance with our changing scientific scene. It may be denying us members who would strengthen our Institute, and it may lead, as there are indications of it doing at the present time, to an unnecessary and undesirable fragmentation of chemistry and proliferation of groups with related or overlapping interests and objectives.

There are various groups of individuals whom I think we should be encouraging into some type of membership of the Institute, and the active participation in meetings, conferences, specialist groups etc. that this implies. The growth of border sciences has been such that we now have many individuals on the fringe of chemistry using chemical techniques effectively and well, but not necessarily trained primarily as chemists. These people will have majored in technological subjects, in microbiology, veterianry science, medicine, etc., with only perhaps second year chemistry or biochemistry. These people could perhaps be admitted to the Institute under the "equivalent qualification" clause, but as I have said, this is capable of many interpretations and in any event hardly waves the welcoming flag very warmly.

Rather more controversial is the group that obtained a training primarily in chemistry but not through a university degree. I refer here to what I suspect will be the increasing number coming up with qualifications obtained through polytechnics and related institutions. There is today a tendency to assume that the more able and ambitious students will go to university, and that the better of these will come to us as graduates. If this were true, those taking up scientific employment as technicians would represent a less-capable group, and would not be able to, or would not wish to attempt to reach the standard necessary for admission to the Institute. This has been true up to a point in the past, and I think that most of us, finding a technician with above-average ability, have encourâged him or her to take a science degree at a university, generally with very satisfactory results. However, there are increasing indications that some of our more able students are, through disenchantment with the university system or for other reasons, turning to the polytechnics and related organizations for their tertiary education. Some of these people may well become better chemists than their less-able counterparts, who nevertheless went through the motions of obtaining a formal university degree in chemistry.

The worthy chemist who is a graduate in some other discipline and does not possess a Stage 3 degree in chemistry, or the person who has the equivalent of Stage 3 but does not have a full university degree, may not be particularly common today, but now is the time to decide very clearly what we want to do about these people before their numbers increase too significantly.

I feel that our Associateship and Fellowship should be guarded assiduously. We should not admit any of the people I have been talking about until we are certain that they measure up to the standards of our profession. Equally, we should not automatically pass from the Graduate Member category into to Associateship, any person just because he happens to have a degree in chemistry, irrespective of how well he has measured up to the requirements of a professional chemist after obtaining that degree. I fell we can only cope with all these situations by introducing a new class of membership below the Associateship level—probably Licentiate is the best term to use. This would be a transition grade for most, but could be terminal for some. Licentiate, irrespective of his background, apprenticeship period, during which time the Licentiate, irrespective of hs background, could be assessed for a minimum period of four years as for Graduate Membership at the present time. At that stage, the majority would become Assocaites, but others might remain Licentiates for longer and perhaps indefinitely.

All Licentiates, like their counterparts in the Royal Institute of Chemistry, should be entitled to the letters LNZIC and the status and recognition within the Institute that goes with this. They would thus be in marked contrast, and the Institute would show that it regarded them as markedly different, from local membership, which is equally open to anyone with an interest in chemistry but not necessarily with any qualifications in chemistry or with any interest in establishing a professional reputation for himself. To relegate the sort of person discussed above to local membership is effectively telling them that the Institute has no real interest in them as individuals or in their advancement in chemistry, but will tolerate them attending branch meetings if they want to. The greater status of Associates and Fellows can be adequately protected by limiting the voting and other rights of Licentiates.

Relationships with other Organizations

I touched earlier on the proliferation of scientific bodies, and it will be recalled that cooperation with other organizations with similar objectives is one of the objects for which our Institute was founded. We have the opportunity to collaborate on the one hand with multidiscipline organizations such as the Royal Society or the Association of Scientists, and on the other hand with a number of specialist societies relating to particular aspects of chemistry or of sciences which impinge on cheimstry. We have seen a quite considerable growth of these smaller organizations over recent years. It is natural that chemists working in peripheral areas should wish to associate with scientists in other disciplines, but it would be unfortunate if the stage were reached where chemists were to feel that their first loyalties lay with those specialist groups rather than with the Institute of Chemistry. One of our strengths has been the broad scope of our interests, and I think both the individual chemist and the Institute would suffer if groups like the biochemists or the chemical engineers were to operate outside the general umbrella of the Institute. The specialist groups within the Institute appear to be operating effectively and to be providing, not only for the members of the Institute but also for many non-members, the type of specialist discussion they desire in their own fields. We have increasingly admitted non-Institute members to these specialist groups to the great benefit of our own members as well as the non-members. Why not regularize this by providing a grade of membership of the Institute as a whole? At a time when other scientific groups in New Zealand and overseas are coming together to form larger organizations, it is to be hoped that chemistry, the strongest science in New Zealand, will not fragment.

Our relationship with multidiscipline organizations such as the Royal Society raises problems of an entirely different type. The Institute of Chemistry is a member body of the Royal Society, and as such naturally expects a reasonable say in Royal Society affairs, particularly as they affect chemists and chemistry. But the Royal Society is an unusual type of organization, and does not necessarily see its relationship to its member bodies in the same way as they see theirs to it.

The Institute of Chemistry naturally considers itself to be the body most competent to comment on chemical matters. When its advice is not sought, or if sought is not acted on, members of the Institute may be justified in wondering whether the best interests of chemistry in New Zealand are served by continued association of the Institute with a body such as the Royal Society, which purports to speak authoritatively, or at least is regarded by many lay people as the organization which speaks for all science including chemistry.

In particular, concern and dissatisfaction has been expressed over Fellowship selection. Chemistry, and in fact physical sciences generally, are poorly represented among the Fellows of the Royal Society. The problems of establishing a more equitable distribution of disciplines in a group with an essentially closed membership are clearly appreciated, but it does seem that the appropriate member body is the best qualified to advise the Royal Society on the most outstanding person or persons from its particular discipline to be considered for Fellowship when elections are being held. The Institute of Chemistry puts forward names each year, but with singularly little success.

This problem of selection does, however, seem to be somewhat peculiar to chemistry. Other groups seem reasonably satisfied that those of their disciplines elected to Fellowship have been the most worthy. It seems that we are not so concerned with the Fellowship policy of the Royal Society, but that there is a difference of opinion between the Institute of Chemistry and the Chemical Sciences Panel of the Royal Society on what constitutes outstanding achievement in chemistry. If we can resolve these differences (and here I think the only way is for the Institute to press for a greater say in the composition of the Selection Panel) our longstanding argument over the selection of chemists for Fellowships will disappear, but unless substantially more Fellows are elected each year with the emphasis heavily on the Physical Sciences, it will be a very long time before Chemistry is adequately represented in the ranks of Royal Society Fellows. At the same time, we must be sure that the Institute members we do nominate are the most worthy of election to Fellowship, and that we miss no chemist, member of the Institute or not, who is worthy of consideration. To put forward unworthy names, or worse, to miss promising candidates, lays open to criticism our claim to be the best organization to select suitable chemists. The onus is on branches to keep Council advised of suitable persons to nominate.

I think that particularly with the present leadership, a great opportunity exists to strengthen the Royal Society into an effective organization. The Institute of Chemistry, as the strongest and largest member body, is probably in the best position to do this, and in helping to do it, to ensure an adequate place for chemistry. The Royal Society must, however, in all things recognize the specialist knowledge in their respective fields of its member bodies, and the contribution which they can make.

Dual Function of the Institute

In our association with other organizations, it is I think important to recognize the dual function which the Institute has attempted to fulfil in the past—that of a professional body and of a learned society. In this respect we differ somewhat from other Institutes such as the Royal Institute of Chemistry, which has never set out to be more than a professional and a qualifying body. The learned society functions for chemists in Great Britain have been catered for by the Chemical Society and by the smaller specialist societies such as the Faraday Society and the Society of Analytical Chemistry. At a time when at least some people in different branches of Chemistry in New Zealand are thinking about forming their own societies, it is rather interesting to note that the Royal Institute of Chemistry and the three learned societies just mentioned are engaged at present in implementing a scheme of amalgamation. This has already been agreed to by all four memberships, and will come into operation at the beginning of 1972. Under this scheme, all the members of the individual organizations will become Fellows of the Chemical Society, and all the learned society functions will be carried out under the Chemical Society Charter. The two smaller societies, the Faraday Society and the Society of Analytical Chemistry, will be totally absorbed into the Chemical Society. The Royal Institute of Chemistry will continue to exist, but will confine itself, no doubt more than in the past, to strictly professional and qualifying activities. I would suggest that this amalgamation be watched with considerable interest, since it may well indicate a pattern for the future activities of our Institute and its relationship to other organizations. If the 42,000 chemists and others interested in chemistry in Great Britain see an advantage in coming together within the Chemical Society and in working closely with the 28,000 chemists who are members of the Royal Institute of Chemistry, such integration of effort would seem even more obvious for the much smaller numbers in New Zealand. Indeed, in attempting to carry out many of the objects set out in our Rules, such as qualifying requirements, relationships to other organizations, learned society functions and so on, we can draw heavily on the experience of other similar scientific groups.

Enlarged Responsibilities of the Institute

When we come to consider the objectives, which relate more to the promotion of chemistry, and the interests of chemists generally, we do not find so much common ground. The Institute's responsibilities as a professional body are to the profession as a whole, but chemists are engaged in a wide variety of occupations, so that there are very considerable difficulties in the way of conducting activities of common interest. The majority of chemists still do work in which a good knowledge of chemistry is essential, but chemistry ceased to be exclusively a vocational study a long time ago; increasingly we are finding chemists in positions of management or administration which draw little on their background knowledge of chemistry. Consequently the Institute has to concern itself with the very considerable variety of situations in which chemists are involved, and to do its best to assist the interetss of its different groups. I think we cater extremely well for those engaged in the more academic or fundamental lines of chemical rseearch and investigation. These groups have a greater unity of interest and are easier to cater for, but I wonder if we should not be devoting even more time and effort to those of our members who are working in greater isolation from their fellow chemists in the more applied fields of chemistry. I do not know quite what we could do here, and the same type of question is being posed at the present time, also without any clear answer, in the Royal Institute of Chemistry. However, it is as well to remind ourselves that many of the activities of the Institute are, by their very nature, of interest only to a minority of our members.

Whether or not we are able to do anything formally for those chemists who find little of interest in at least some of our current activities, it does seem likely that our membership individually may come to rely more on the Institute as a professional body to protect and further their interests. Hard selling by certain technologies has led some employers to believe that individuals trained in other disciplines are better suited to their requirements than the chemists they have traditionally employed. Much of our professional recognition, the improved conditions of employment and remuneration, have stemmed from the relative scarcity value of chemists, but there are now indications that we may be approaching a period of over-supply. Many employers still fail to distinguish between the qualified chemist and the chemical technician. These factors may operate against some of our members in the not too far distant future.

At various times over the last few decades, professional workers have become concerned at the increasing power of organized labour and its apparent determination to use this power to the limit to further its sectional interests, whether these are directly related to the welfare of its own members or not, and with little concern for the implications of these actions on the social and economic interests of the community at large. It does seem that we are now in or approaching another such period. The work carried out by certain chemists in industry comes fairly close to what may be regarded by some unions as the area of activity of their members. This has led in the past, and possibly in the immediate future will lead to pressure being exerted on some chemists to join industrial unions may limit the effectiveness of chemists their normal work and certainly no interest in their welfare as professional workers. Worse still, the restrictive practices of some Unions may limit the effectiveness of chemists and the way in which they perform their work on the industrial scene. The number of chemists affected may be relatively small, but I suggest that the Institute as a whole, if it is to carry out its objectives, must be prepared to act immediately to protect the interests of these individuals, and if necessary to assist them in forming their own Guild, registered under the appropriate Act, or in taking some other action to avoid being forced into industrial unions and industrial awards against their wishes and best interests. We may have to think about registration of chemists in the same way as engineers, architects, etc., are registered. This is a real possibility, although it present difficulties and may affect our membership deliberations.

Carrying this a stage further, the objectives of the Institute cover not just qualified chemists but, if these people are to perform as effectively and efficiently as possible, must include the increasingly important group of technicians and other unqualified people with whom these chemists work. Many of these technicians are under pressure already from trade unions, and some have in fact already been forced to join the unions or to form their own industrial organization. This type of activity, I suggest, is again not in the best interests of the profession of chemistry and of those engaged therein, and the Institute has, I feel, a wider responsibility than just to its own members. It should be prepared in the industrial field to assist technicians and related workers whose efficiency and usefulness to their chemist associates may be endangered by current union attitudes and activities.

Overproduction of chemists

I do not think that this need be a matter of great worry to the Institute at the present time. Whether or not there is over-production of chemists will depend not so much on the numbers we train here in New Zealand, but on policies followed overseas, particularly in Great Britain, the United States and Australia. In any event, we are talking not just about chemists but about well-educated people, and this is something which can not be over-produced. It may mean that chemists will move increasingly into non-traditional areas of employment, something which many of us on the fringe of chemistry would warmly welcome. To suggest any diversion away from chemistry into other disciplines could be much more disastrous for the country and wasteful of key manpower.

Social Responsibilities

To fulfil its objects, the Institute may become involved in guarding the public image of the chemist. The relatively poor public image of scientists is still a matter which concerns most scietnific organizations, but it is perhaps the chemists who seem to appear most frequently in the worst possible light. Unfortunately the press and the mass news media seem convinced that the public prefers bad news to good news, and they delight in playing up any situation, particularly where science or scientists are involved. These instances are on the increase, and sooner or later some of our members are going to require assistance from or the protection of the Institute. Where the profound benefits to society of the numerous advances in chemistry are discussed, it seems that credit goes to the team of medical scientists, to the dairy scientist, to the agricultural scientist, but let something go wrong, let some problem arise, and the umbrella title seems to disappear—it is the chemist who is named by the news media. The area that springs to mind immediately here is the rather long overdue attention currently being given to problems of environmental pollution. The chemist is seldom personally responsible, but we must admit that chemistry and chemical technology have been a major factor in creating some of the worst offending industries, and the chemist could be a convenient person to blame. The best defence is for chemists and for the Institute of Chemistry to be taking and to be seen to be taking the initiative in measures to control and to prevent environmental pollution.

This is, of course, only one aspect of our responsibility for the consequences of our science. Wider thinking on this subject led a couple of years ago to the formation in Britain of the British Society for Social Responsibility in Science. Many Institute members will have seen the publications of this group and will know something of its activities. Its thesis is the obvious one that science is not something performed in a vaccum in isolation from the society in which it is done, and that therefore both the scientists themselves and society must appreciate the nature and importance of the interrelationships which exist between science and society. Groups like this new society tend to concentrate on major issues such as atomic explosions, chemical and biological warfare and the like, which fortunately have little or no relevance to the New Zealand scene at present. But there are many other areas where consideration should be given to the consequences of scientific decisions or scientific actions on the community. There is increasingly, too, a need to explain science and the results of scientific investigation to lay people whose actions and perhaps livelihoods they may affect. We have, I think, as chemists all experienced this type of problem and perhaps stood by and not corrected an exaggerated or perhaps misrepresented press statement on some issue.

I think the Institute should be awake to these problems, and if we are to carry out our objects should keep in close contact with organizations such as the Society for Social Responsibility in Science, or perhaps it would be more appropriate to encourage an organization like the Royal Society to do this on behalf of all sciences.

Conclusion

I have touched on a number of different subjects which I think are of some relevance to the Institute of Chemistry. I do not intend to be unduly critical of the Institute. I think it has achieved a great deal in the past for its members and for the profession of Chemistry generally. It is in a very strong and healthy position. But I do think the next decade will see much greater change in the environment in which it must operate than we have been in the past forty years, and I would suggest that the Institute should be looking now very carefully at itself, its relation to its members, and to other organizations, to ensure that it is in the best position to meet the challenges of the next decade. The first step I would suggest towards this is a more complete involvement of our whole membership in the activities of the Institute and in the running of the Institute. It is too important an organization for these activities to be left to a small group. No matter how willing and able these people may be, they represent the views of only part of our membership.

What I am essentially suggesting is that our Institute and our members individually need to awaken themselves much more to what is going on around them. To too great an extent we tend, I suggest, to wait until after the event to make what changes may be necessary in our Rules or in our organization to cope with a new situation. Instead, we should be to a much greater extent a part of the action ,and in fact we should, not only as individuals but as a professional body representing the major science in New Zealand, be directing the course of the action. Other professional bodies are fulfilling this role much more effectively than we are, and while the involvement of many professional bodies in activities such as the type of training to be provided for those entering the profession may be resented at times by the Universities and the Polytechnics, it does nevertheless represent a healthy attitude and ensures that those actively engaged in the particular professional do have an opportunity to feed back to the training groups their particular problems and their ideas of how training should proceed. The vocational aspects of chemistry should not be over-estimated, but they never-to the academic members of our Institute to represent us on educational matters.

We require a considerable degree of understanding and cooperation between the different groups of chemists that make up the Institute. There are clearly academic matters which are of little interest or concern to some of our members, but they nevertheless affect theless exist, and we should not just leave it the future of our profession. Likewise, our more industrial members face problems which are completely foreign to our more academic members. They are nonetheless important to our Institute, and perhaps even pressing of urgent solution.

The challenges and opportunities that lie ahead on all fronts for any professional body such as the Institute of Chemistry, and for all those engaged in any scientific pursuit, are tremendous; their solution demands the active participation of all groups within the profession, irrespective of how far these individual groups may at this stage appear to be removed from the particular problems or the type of decision which must be made.

ATOMIC FLUORESCENCE SPECTROSCOPY

F. J. Aggett

Atomic absorption spectroscopy has now been generally accepted as a useful, rapid, and reliable method of analysis for trace quantities of materials. Indeed it is probably no exaggeration to state that this method has revolutionised those branches of science which depend extensively on trace element analysis. It is by nature specific and generally free from spectral interferences but not, of course, from chemical and physical interferences. However, while sensitivities are generally high, the analyst is frequently forced to determine quantities which are uncomfortably close to the sensitivity or detection limit of the atomic absorption method. This affects both the reliability of the results and the time and effort necessary to obtain them. In the late 1960's it was mooted that atomic fluorescence spectroscopy might provide greater sensitivity while still maintaining the inherent selectivity of atomic absorption. The main basis for this belief was the knowledge that in fluorescence the intensity of the signal is proportional to the intensity of the radiation incident on the sample and that small emission signals can be measured with greater confidence than small differences between two large signals as required in atomic absorption.

The purpose of this article is to review the recent development of atomic fluorescence and indicate areas of analysis where the technique may prove superior to atomic absorption spectroscopy. Readers seeking a review of the fundamental principles of atomic fluorescence are referred to the recent review of Winefordner and Elser[1] and references therein.

FIGURE 1 illustrates the basic design of an atomic fluorescence spectrometer. The essential difference between this arrangement and that used for atomic absorption is that the source-sample axis is perpendicular to the sample-detector axis, an arrangement which permits detection of the emission signal without interference from the incident light. A number of modifications of this design have been used; these have recently been reviewed by Winefordner and Elser[1] and will not be discussed in detail here. While it is true that some of these variations result in greater sensitivities either by utilising multiple-radiation of the sample or by more efficient collection of the emission signal the great advantage of the basic system illustrated in Figure 1 is that it requires only two lenses in addition to the normal accessories of an atomic absorption instrument. Thus conversion to the fluorescence mode is both rapid and inexpensive.

Since the fluorescence signal is proportional to the intensity of the source it is hardly surprising that considerable effort has been expended on the development of light sources of high intensity. Three types of source have been investigated.

1. Continuous sources such as xenon arc lamps.
2. Hollow cathode lamps.
3. Electrodeless discharge tubes.

Sensitivities achieved with xenon arc lamps are relatively poor[2] because the emission intensity at a specific wavelength is normally low. TABLE 1 contains some typical date obtained with a 150 watt xenon arc lamp using a Unicam SP900A spectrophotmeter. This data was published in 1966. Although xenon arc lamps were frequently used in early studies on atomic fluorescence they are seldom used at the present time. However, in one fairly recent study[3] in which a 900 watt lamp was used for the determination of copper, iron and lead in hydrocarbon fuels the detection limits are almost comparable with those obtainable by atomic absorption spectroscopy.

High intensity hallow cathode lamps when used for atomic fluorescence also give detec-

TABLE 1*

ATOMIC FLUORESCENCE WITH A 150-WATT XENON ARC LAMP

Element	Detection limit in p.p.m.	
	Atomic Fluorescence	Atomic absorption
Cadmium	0.25	0.05
Cobalt	1.5	0.5
Copper	1.0	0.15
Iron	5.0	0.5
Lead	20	1.0
Magnesium	2.0	0.02
Manganese	0.3	0.2
Silver	0.35	0.13
Zinc	0.6	0.04

* Data extracted from Reference 2.

tion limits comparable to and perhaps a little better than those obtained by atomic absorption spectroscopy. Unfortunately, to the best of this author's knowledge these lamps are no longer manufactured. One or two groups of scientists overseas are currently investigating the use of pulsed hollow cathode lamps. In these systems the lamp is pulsed to produce short emission bursts at 100-200 ma. Although preliminary information indicate that a considerable increase in sensitivity is obtained with pulsed lamps it is still too early to judge their overall effectiveness. Some indication of the sensitivities obtainable with pulsed hollow cathode lamps is given by the data in TABLE 2 which has been extracted from Reference 4.

TABLE 2

ATOMIC FLUORESCENCE WITH PULSED HOLLOW CATHODE LAMPS

Element	Peak current (ma)	Wavelength (nm)	Detection Limit (p.p.m.)
Copper	190	324.7	0.003
Iron	190	248.3	0.1
Magnesium	112	338.3	0.001
Silver	140	285.2	0.002

Without doubt the electrodeless discharge tube provides the most intense light source currently available and some quite remarkable detection limits have been reported with these[5]. The early reported successes of these sources no doubt raised the hopes of those interested in the development of atomic fluorescence. Unfortunately, experience has shown that these light sources are not particularly stable and have unpredictable lifetimes. Consequently they are unlikely to be useful for routine analyses.

To sum up, hollow cathode lamps (preferably pulsed or high intensity) currently appear to be the best available sources for atomic fluorescence, but the increase in sensitivity over atomic absorption is in most cases insufficient to warrant a change from the latter technique on the basis of sensitivity alone.

The nature of the flame used for atomisation also influences the sensitivity of atomic fluorescence. Since the detection limit is based on signal: background ratio a low flame emission background is clearly advantageous. A reduction in flame background can be achieved by lifting the outer diffusion zone of the flame above the optical axis, a process known as separating the flame. Atomic fluorescence detection limits with separated flames are commonly three to five times lower than those with conventional flames[6] although ten-fold increases in sensitivity have been reported by T. S. West and co-workers.[7, 8] Similar increases in sensitivity are not normally achieved when separated flames are used in atomic absorption spectroscopy because the signal recorded is the difference between the total signal in the presence and absence of analyte. Flame background contributes equally to both signals.

Non flame atomisers such as the carbon filament device initially developed by T. S. West are also very effective in increasing sensitivities in atomic fluorescence[9, 10] However, they are equally effective in this respect when used for analysis by atomic absorption. At the present time the potential of non-flame atomisers has not been fully established. Research to date indicates that they are more prone to suffer from interference problems than are flames and consequently more research in this direction is needed before they can be accepted with confidence. At the University of Auckland the current research programme includes investigation of the mechanism of atomisation and interferences with non flame atomisers. Results to date indicate that knowledge of these basic processes will lead to more effective use of these devices.

One of the problems associated with comparing atomic fluorescence with atomic absorption is that few comparisons have been reported in which the same instrument has been used for both techniques. Now this is extremely relevant to an analyst making a decision in favour of one of these techniques. TABLE 3 contains a few examples where a valid comparison can be made. The atomic fluorescence data in this table is confined to systems in which hollow cathode lamps were used as radiation sources. Comparative tables such as that presented by Winefordner and Elser[1] are misleading since they make comparison between the techniques where the measurements have almost invariably been made on different apparatus under quite different conditions.

TABLE 3

COMPARISON OF DETECTION LIMITS

Element	Detection Limits (p.p.m.)		Atomiser
	AFS	AAS	
Arsenic	0.5	1	Carbon filament
Beryllium	0.003	0.04	Carbon filament
Cadmium	0.00003	0.002	Carbon filament
Calcium	0.0001	0.001	Carbon filament
Cobalt	0.01	0.03	Flame
Copper	0.005	0.02	Carbon filiment
Gold	0.001	0.04	Carbon filiment
Iron	0.002	0.01	Carbon filiment
	0.02	0.03	Flame
Lead	0.003	0.02	Carbon filament
	0.02	0.12	Flame
Magnesium	0.00003	0.0004	Carbon filament
Nickel	0.003	0.02	Flame
Silver	0.0005	0.001	Carbon filament
Zinc	0.0001	0.001	Carbon filament

There are two areas in which atomic fluorescence may prove extremely useful irrespective of sensitivity relative to atomic absorption.

Several elements (e.g. zinc) have only one effective atomic absorption line. In analysis by atomic absorption this restricts the range of concentrations which can be nebulised. Samples containing relatively high concentrations of these elements must be diluted prior to nebulisation in order to produce a convenient absorbance reading. However, in atomic fluorescence the emission signal is linearly related to the analyte concentration over a very wide range (1000-10,000 fold)[4 11] so that in this respect there is less restriction on the concentration of material which may be analysed directly. Where this situation exists in atomic absorption atomic fluorescence may well provide a more convenient and less time-consuming means of analysis.

But perhaps the feature of atomic fluorescence which will lead to its wider acceptance is the relative ease with which the technique can be adapted to perform automatic simultaneous multi-element analyses. One such instrument has been designed by Mitchell and Johansson[4]. In this instrument independently operated pulsed hollow cathode lamps are used as radiation sources and the fluorescence radiation from the different elements is rapidly and successively isolated by means of a rotating filter-wheel. The data in Table 2 were obtained with this instrument.

One final comment seems relevant. In order to supersede an existing technique any new technique must be demonstrably superior. The acceptance of atomic absorption as an advance over spectrophotometry for a wide variety of analyses illustrates this point very clearly. Furthermore, at the present time atomic absorption provides a very adequate means of analysis in many situations and since individual analysts have built up a considerable degree of expertise in this technique it seems unlikely that there will be a major changeover to atomic fluorescence.

However, a series of situations will undoubtedly arise in which serious consideration will indicate the advantage of atomic fluorescence over atomic absorption. Accordingly it is anticipated that atomic fluorescence will become accepted in a more gradual manner than was atomic absorption a decade ago.

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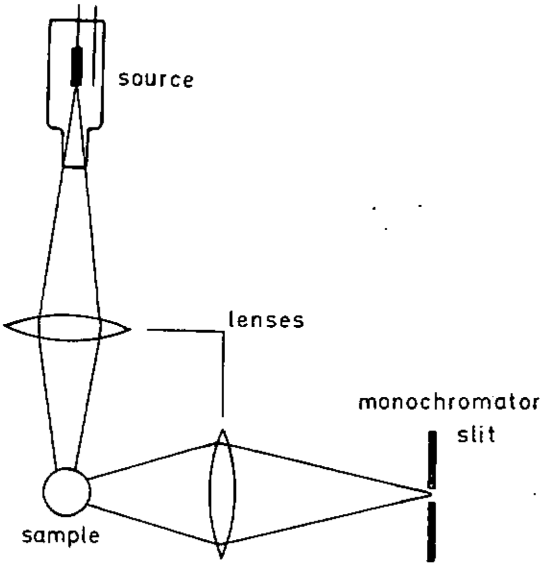

Fig. 1 Arrangement for atomic fluorescence spectroscopy.

COMMENT

DO YOU BELONG TO THE INSTITUTE OR DOES IT BELONG TO YOU?— THE SPECIALIST GROUPS

The Institute has members engaged in marketing, sales, administration, manufacturing, teaching, quality control, research and many other occupations. Within these categories, our members have a diversity of chemical interests—biochemistry, electrochemistry, chemical engineering, organic chemistry, analytical chemistry and so on, as well as a lot of non-chemical interests, some science-based and others with hardly any scientific content.

Regardless of the lack of scientific or chemical content of their jobs, however, many choose to become and remain members of the Institute. Is it because the letters F.N.Z.I.C. or A.N.Z.I.C. are valuable to them, because they wish to support a society whose aims are allied to chemistry, and therefore to their jobs, or is it to meet other members of similar interests?

We have several specialist groups within the Institute, and it is open to any member to join these. Membership is free, and the Institute supports them by providing addressograph plates and stationery (if required). Groups have been established in Geochemistry, Electrochemistry, Biochemistry and Organic chemistry.

These groups cover only some of the interests of members, and the question arises —what about the rest? Is the Institute doing enough for all its members, especially for those whose interests are now largely nonchemical?

Would you be interested in a group concerned with marketing, for instance? There is no reason why such a group should not be formed, and if prospective members feel that one would be valuable, it could easily be done. It only needs someone to take the initiative.

C.L.H.S.

THE ROLE OF EXPERIMENT IN THE CHEMICAL REVOLUTION

A lecture delivered to the special Otago Branch Meeting to farewell Professor H. Parton on February 22nd, 1972.

Alan E. Musgrave

Introduction

It is a great honour for me to address this meeting to mark the retirement of Hugh Parton from the Mellor Chair of Chemistry at Otago. Ever since I received the invitation, I have been wondering what I have done to deserve it. The answer is, of course, that I share Professor Parton's interest in the history and philosophy of science. Unlike many professional scientists, who regard philosophy of science with suspicion, Professor Parton has made no secret of his interest in it. The University will shortly be publishing a collection of his essays and lectures on the philosophy and history of science, and I am sure we are all looking forward to reading it.

It is also no secret that Professor Parton and I are both admirers of the views of Karl Popper in this field. Popper is, of course, famous for his new view of the relation between experiment and theory in science, a view which has recently been labelled *falsificationism.* So I though it would be appropriate if I said something about this tonight. In doing so, I shall be touching upon one of the central issues in contemporary philosophy of science. For it would be no exaggeration to say that some of the most interesting work in recent philosophy of science centres around the critical discussion and elaboration of Popper's views.

But philosophical discussions without examples or case-studies are apt to appear a little abstract and irrelevant, especially to working scientists. As my friend Imre Lakatos once put it, modifying a famous saying of Kant's: "Philosophy of science without history of science is empty—and history of science without philosophy of science is blind." So I shall illustrate my talk with a case-study. Actually, 'illustrate' is not quite the right word—having embarked on the case-study, I got so interested in it that most of my lecture will be concerned with it.

Since this is a meeting of an institute of chemistry, there was really nothing for it but to choose my case-study from the history of chemistry. And what could be a more appropriate case-study than the marvellous story of the origins of modern chemistry in the late eighteenth century? The great name is, of course, Lavoisier who was responsible for what has aptly been called the Chemical Revolution. This, then, explains my title: The Role of Experiment in the Chemical Revolution.

I make no apology for treading the well-worn ground of the chemical revolution once more—the story is such a marvellous one that it will bear retelling. However, I am rather nervous that it is me who is retelling it. For it will soon become apparent that my knowledge of chemistry is both amateur and armchair. So it is a little like putting my head into the lion's mouth—you being the lion. However, according to Popper science progresses when scientists stick out their necks, by proposing bold conjectures, and get their heads chopped off by their colleagues who criticise their conjectures. So admirers of Popper, like myself, should not mind getting their heads chopped off too. I hope, therefore, that you will correct the chemical errors I am bound to make. In this way I shall learn something, even if you don't.

Phlogiston theory

Let me begin by reminding you of the salient points of the phlogiston theory, the theory which was overthrown in the chemical revolution. The theory began from the obvious fact that when things burn they give off something—the something was called phlogiston (denoted by ϕ). Thus:

Charcoal burned in air → Ashes + ϕ (to the air).

When air was saturated with ϕ, or "phlogisticated', it would not support combustion.

The theory also explained metallurgy:

Ore heated with charcoal → Metal (Ore + ϕ) + ϕ (to air) + Ashes.

Hence a metal is a more complicated substance than its ore. It is also, since it contains ϕ, a combustible substance. When it is heated it gives up its ϕ to form acalx (oxide):

Metal heated in air → Calx + ϕ (to the air).

Thus metallic calxes are basically the same thing as metallic ores, except that ores ordinarily contain impurities. A calx can be 'reduced' back to a metal by heating it with charcoal:

Calx heated with charcoal → Metal (Calx + ϕ) + ϕ (to air) + Ashes.

The theory also explained acidity, since when some substances were burned they produced fumes which yielded acids when combined with water. Thus acidification was the removal of ϕ from an 'acid base':

Sulphur — ϕ → Vitriolic Acid Fumes.

Phosphorus – ϕ → Phosphoric Acid Fumes.

One mark of a good scientific theory is that it unifies apparently distinct phenomena. The phlogiston theory did this with the phenomena of combustion, metallurgy, calcination of metals, and acidification. Perhaps this is why Priestley called it "the greatest discovery that has ever been made in the science".

But of course, the phlogiston theory is false. Indeed, it is the precise reverse of the truth. This was what the chemical revolution taught us—so let us now see how that revolution was brought about.

The Chemical Revolution — a popular account

According to a popular account, the chemical revolution was brought about by two simple experiments of Lavoisier's.

The first experiment (1772) disproved the phlogiston theory. Lavoisier burned phosphorus in air, and noticed that the volume of air decreased while the weight of the phosphorus increased. This disproved the phlogiston theory, which demanded that the weight of the burning phosphorus should decrease since ϕ was being released from it. It also proved that combustion involves the 'fixing' in the burning substance of the air or some part of it.

The second experiment (1774) proved the oxygen theory of combustion. Lavoisier calcinated (or oxidised) a metal in air until the formation of a calx ceased. He then reduced the calx back to the metal and collected the oxygen given off. The weights tallied, and the original air could be recovered by mixing the residue left by the calcination with the oxygen produced by the reduction. It was this discovery of oxygen which dealt the death-blow to ϕ.

So runs a popular account. A cursory glance at history reveals a few strange facts which should make us suspect that matters were not quite so simple.

For one thing, the fact that the volume of air is reduced, while the weight of the burned substance can increase, was well-known to phlogiston theorists. For another, oxygen was first discovered by phlogiston theorists. And for a third, great chemists like Priestley, Cavendish, a dnKirwan, retained the phlogiston theory long after these two decisive experiments had been performed.

One can say, of course, that these men were too dogmatic, or tradition bound, or plain stupid, to see that phlogistonism had been overthrown. But this will hardly do—as we shall see, they had some good reasons to resist Lavoisier's new theory.

So perhaps we should take a closer look.

Lavoisier's First Experiment (1772)

Why didn't Lavoisier's first experiment overthrow the phlogiston theory? The reduction in the volume of air in which things are burned had been explained quite simply —air saturated with ϕ just takes up less room than ordinary air.

But what of the weight increase? This could be explained in many different ways by phlogiston theorists. But how could the release of ϕ be consistent with a weight-*in*crease? To see this, we have only to realise that the ϕ-theory alone does not entail that there will be a weight-loss. To deduce such a prediction from the ϕ-theory, we need several further premises:

(P) Combustion = the release of ϕ.
(1) ϕ has weight.
(2) Nothing weighty is added to a burning substance.
(3) If a weighty substance is released, and nothing weighty is added, then the residue will weigh less.

From these four premises we can deduce: *Prediction*: The residue of combustion will weigh less.

Thus Lavoisier's experimental report contradicted this prediction, and with it the conjunction of the four premises from which it was derived. We must either reject Lavoisier's report, or reject at least one of the four premises.

Lavoisier, of course, chose to reject the phlogiston theory (P), together with premise (2). And he simultaneously proposed the new theory that combustion is the 'fixing' of a 'quantity of air' in the burning substance.

But others chose to retain (P). And they explored pretty well every other possibility there was. Early on, experimental reports of weight-increase were contradicted by reports of weight-loss, and were therefore denied. But gradually, as techniques improved, this option was closed. Other ϕ-theorists denied premise (1), and said that ϕ had negative weight or levity. This caused some amusement at the time, and has been an unfailing source of amusement to historians of science ever since.

Other ϕ-theorists, believe it or not, denied premise (3), which looks almost tautologous to modern eyes. Careful argumentation was necessary to show that this overturned the principles of sound, that is, Newtonian, physics.

The most promising option was to deny premise (2), and say that something weighty was added to burning substances as ϕ was released. Some of the earliest explanations of the weight-increase, proposed even before the ϕ-theory, were in this vein. Rey said that air particles, made heavy and sticky by the heat, were added to the tin calx whose weight-increase he noticed. Boyle said fire particles were added to it, and concluded that heat had weight. Lavoisier's first experiment seemed to make it clear that what was added, as ϕ was released, was the air or some part of it. Lavoisier himself flirted with this version of ϕ-ism in 1772, before arriving at his rival view. Priestley took it up later.

So Lavoisier's first experiment did not disprove the phlogiston theory (P). Its real significance was that it suggested to Lavoisier a *rival theory*: I shall return to this point later.

What did Lavoisier do next? He first refuted Boyle's theory that the weight-increase of calxes was due to particles of fire which became mixed with them. Lavoisier weighed the sealed container in which the calx was formed before and after the experiment, and detected no increase. He concluded that the weight-increase of the tin calx must come from *inside* the container, from the air contained it. Boyle had opened the container after the experiment, for he weighed only the tin.

This had no direct bearing on ϕ-ism—but it did strengthen its rival. Lavoisier's theory, as it stood in 1774, needed strength. Its great weakness arose from the fact that combustion ceases long before the air in which it is carried out is exhausted. We have seen how ϕ-theorists explained this. But Lavoisier could explain it only by sup-

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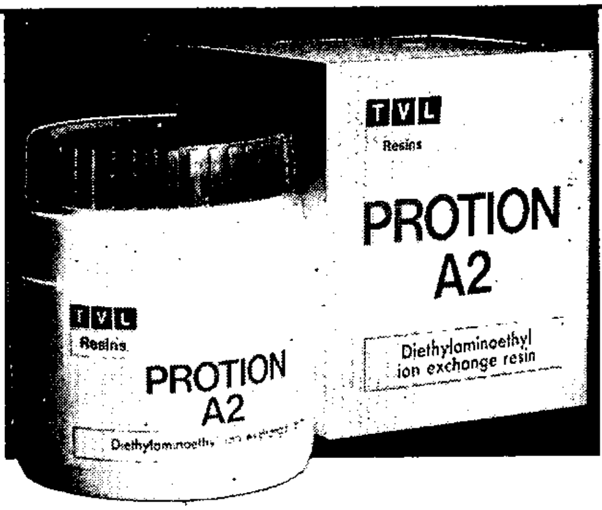
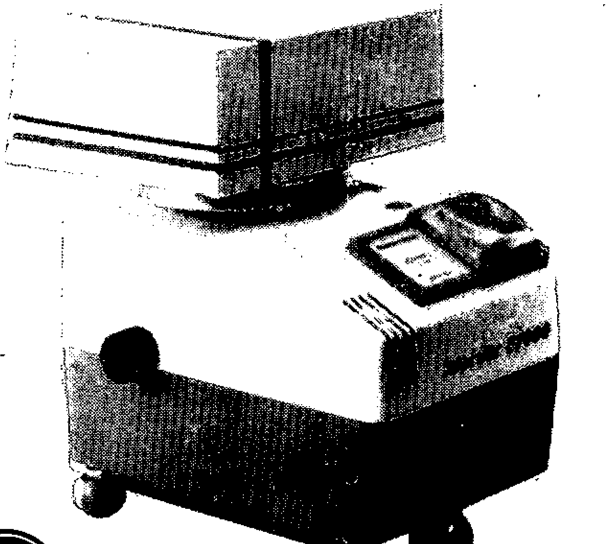




posing that not all of the air combines with a burning substance. Lavoisier predicts, in other words, that the air is a mixture of two substances, only one of which will support combustion.

In 1774 this was mere guesswork—moreover, it seemed to be contradicted by the facts. It follows from Lavoisier's hypothesis that when a calx is reduced back to a metal, the part of the air which supports combustion should be given off. But the reduction of calxes always yielded 'phlogisticated' or 'fixed' air, which would not support combustion.

Lavoisier said that this happened because *charcoal* had to be added to calxes in order to reduce them. The charcoal combined with the part of the air released from the calx, and rendered it incapable of supporting combustion. This again, was just guesswork. Moreover, Bayen announced in February 1774 that he had reduced a calx of mercury back to mercury without needing charcoal, and had still obtained 'fixed air'. A fine mess indeed! But Lavoisier soon got help from a strange quarter.

Priestley and Lavoisier discover oxygen (1774)

Joseph Priestley was also concerned about Bayen's report. According to the ϕ-theory, to which he subscribed, the reduction of a calx is the addition of ϕ. So one ought not to be able to reduce a calx at all without charcoal, or some other source of ϕ.

He obtained some of the mercury calx, repeated the experiment, and to his great surprise refuted Bayen. The calx *was* reduced, as Bayen had claimed, but 'fixed air' was not given off. What *was* given off?

At this point Priestley takes a trip to Paris, and tells Lavoisier that he has refuted Bayen. Lavoisier repeats the experiment, gets confused, and announces that what is given off is both common air and "better" than common air. Moreover, he does not mention Priestley, who in the meantime has discovered that what is given off is a new *species* of air.

Priestley calls it "dephlogisticated air", since it supports combustion better than common air. Priestley, a little piqued at being ignored, gently sets Lavoisier right. Lavoisier amends his paper before it can appear in the official publication of the Academy, again without mentioning Priestley! In the amended version he concludes what he had earlier predicted—that what is given off when the calx is reduced without charcoal is "the purest part of the air" (which he later called oxygen).

Lavoisier's discovery of oxygen had its comic aspects. But if we ignore these, it was a triumph. Just as he had predicted, the mercury calx reduced without charcoal had yielded oxygen:

Mercury Calx (Mercury + O) → Metal + O.

But did the experiment refute ϕ-theory?

Priestley did not think so. He agreed that dephlogisticated air had been collected, and not phlogisticated air—this, he said, was because no charcoal had been used in the reduction. But he still insisted that ϕ must have been added to the calx to produce the metal. And where had the ϕ come from? It must have been present in the calx all along, but in an uncombined state. Heating the calx releases the dephlogisticated air it contains, and makes the ϕ combine with the calx:

Mercury calx (i.e. calx, dephlogisticated air, and ϕ) heated → Mercury (Calx + ϕ) + dephlogisticated air.

According to Priestley, then, this partiular calx of mercury is peculiar because it is always *impure*—some ϕ is always mixed with it, and this is why it can be reduced without charcoal.

Why was Priestley so stubborn? Because he had performed some experiments with lead calxes which proved that they also contained ϕ, and disproved Lavoisier's theory that any calx reduced alone would yield only 'dephlogisticated' air. Priestley had reduced lead calxes without charcoal, adn had obtained 'fixed air':

Lead calxes (i.e. Calxes and ϕ) → Lead (Calx + ϕ) + ϕ.

What could Lavoisier make of these experiments? According to him, a calx reduced without charcoal should yield only oxygen. So Priestley's samples must be impure—his lead calxes must contain some charcoal, or other source of carbon, which combines with the oxygen to give off 'fixed air' (CO_2):

Lead Calxes (i.e. lead oxides and C) → Lead + CO_2.

So in 1775 there were experiments to refute both theories. And both sides explained them away by invoking impurities in the samples. In 1775 honours were even!

Of course, honours did not stay even. Lavoisier's followers were able to show that Priestley's lead oxides must have contained lead carbonates. But this happened only much later—in 1775 Lavoisier's theory was in a pretty bad state.

Lavoisier himself was not much concerned about Priestley's experiments. Instead of bothering with them, he concentrated on extending his theory, during 1776 and 1777, to cover acidification. All acids, he says, consist of an 'acid base' combined with oxygen. Oxygen is therefore the 'acid-generating principle' (that is what the word means).

But Lavoisier's extended theory was in even more trouble than the original.

Cavendish's discovery of hydrogen (1766)

To see why, we have to go back a little to 1766, when Henry Cavendish had isolated an 'inflammable air' (hydrogen) released when metals were dissolved in acids. Phlogiston theorists were delighted with this—since for them all metals contained ϕ, what could Cavendish's 'inflammable air' be but pure ϕ! As one of them said:

> "We have long talked of phlogiston without knowing what we talked about, but now . . . we can pour that element out of one vessel into another, and can tell how much of it by accurate measurement is necessary to reduce a calx to a metal . . . In short, this goddess of levity can be measured and weighed like other matter." (Matthew Boulton).

So phlogistonists were delighted with Cavendish's discovery—but it brought only trouble for Lavoisier.

First of all, the 'inflammable air' was only produced when metals were dissolved in acids—metallic calxes did not produce it. The ϕ-theory explained this nicely—pure metallic calxes contain no ϕ, and therefore cannot yield any. But how could Lavoisier explain it? How *was* the 'inflammable air' produced?

Secondly, according to Lavoisier's theory, when 'inflammable air' burns an acid compound should be produced. Lavoisier kept trying to detect it, and failing (he tried in 1774, 1777, 1781, and 1782). When inflammable air was exploded in air, it just seemed to disappear into thin air (to coin a phrase)!

The third difficulty was an experiment first performed by Priestley in 1782. If 'inflammable air' is pure ϕ, then a calx heated in it should readily reduce to a metal. According to Lavoisier, the calx should merely release its oxygen. Priestley heated lead calx in 'inflammable air'—sure enough, the volume of gas diminished rapidly as the calx was reduced. A direct confirmation of phlogiston theory, and an equally direct refutation of Lavoisier's.

Lavoisier was in a mess—but salvation was at hand. And once again, the hands which saved Lavoisier were those of this great rivals, Priestley and Cavendish.

The Composition of Water

In 1781 Priestley exploded 'inflammable air' and 'dephlogisticated air' together, and noticed that the walls of the vessel "became dewy". Cavendish repeated the experiment and found that the dew was pure water. In June 1783 Cavendish's assistant told Lavoisier of the experiment. Lavoisier was overjoyed, repeated it the same day, and announced Cavendish's results the next day (without mentioning Cavendish). His outstanding difficulties disappeared.

The combustion of hydrogen did indeed produce a compound with oxygen, namely *water*.

The hydrogen given off when metals were dissolved in acids came originally from the *water* used to make the acid. Metallic calxes, being already oxidised, did not decompose the acid, and hence they gave off no hydrogen.

Finally, Priestley's crucial experiment was erroneous. The calxes did release their oxygen upon heating them in hydrogen. But the oxygen combined with the hydrogen to produce water. Priestley had not noticed the water because he had confined the hydrogen over water. But Lavoisier could now predict that if he confined the hydrogen over mercury, he would detect the water produced. This prediction was also confirmed, ironically enough, by Priestley himself in 1785.

Lavoisier deduced a further prediction from his theory. Metals should oxidise in water as well as in air, and hydrogen be given off. He confirmed this prediction in 1783.

He was so encouraged by his success that he began to make fun of phlogiston theory. Phlogiston is, he says, an "imaginary thing" which "accommodates itself to every explanation into which it is pressed". Sometimes it is heavy, sometimes light; sometimes 'free fire', sometimes combined with some other element; sometimes it can pass through vessels, sometimes it can be confined in them. "It is", Lavoisier concludes, "a veritable Proteus which changes its form every minute".

Most chemists began to agree with him. Even Priestley almost admitted defeat in 1785. The chemical revolution was effectively completed by the time Lavoisier published his *Elements of Chemistry* in 1789.

It will come as no surprise if I say that phlogistonism was not completely finished—Proteus changed his form once more before he died! Cavendish developed a new version of the theory in order to account for the production of water when hydrogen is exploded in oxygen. For Cavendish, hydrogen was not pure ϕ, but 'phlogisticated water'. This theory could explain Lavoisier's key results. But it was too late. Soon only poor Priestley was left in the phlogistonist army. Mind you, he could still strike some pretty hefty blows—as when he refuted Lavoisier's theory that 'inflammable air' can only be produced from water by producing it from burning charcoal. This was cleared up in 1801 when it was shown that the 'inflammable air' was not hydrogen but carbon monoxide. Priestley's other hefty blow was to question Lavoisier's theory that all acids contained oxygen by claiming that hydrochloric acid does not. This was never cleared up, since Lavoisier's theory of acids is just false (as Davy showed). But Priestley's last stand also failed, and Lavoisier's defeats came too late to be phlogiston's victories.

So much, then, for my case-study. In the chemical revolution one false theory was overthrown, and another one put in its place. And there is no doubt that Lavoisier's theory, though false, was a *better* theory than phlogistonism. In other words, the chemical revolution was great progress. Let me now return to the philosophical question: what was the role of experiment in bringing about this progress?

Experiment and Theory

Many things have been said about the relations between experiments and scientific theories—and most of them are wrong.

The great Newton said that experiments could prove theories to be true. Since everybody worshipped Newton's science, they admired his philosophy of science too. No theory was thought respectable unless it could be claimed that it had been "deduced from experiments". The story of the chemical revolution is full of claims like this: experiment proved that all inflammables contain phlogiston; experiment proved that phlogiston is the same as hydrogen; experiment proved that all acids contain oxygen; and so on. No wonder people got confused by these conflicting claims: in 1771 the *Encyclopaedia Brittanica* thought the phlogiston theory had been proved; by 1810 they had changed their minds, and now it was the oxygen theory which had been "incontestably proved".

The fact was, however, that all the theories which were supposed to have been proved were *false*. Moreover, by elementary logic, no finite number of *particular* experiments can establish that a *general* theory is true. The poor philosophers had often pointed this out, but everybody ignored them, because the great Newton had said otherwise.

One not so poor philosopher, Francis Bacon, had a different idea. Bacon said that even a single experiment can prove that a theory is true *provided it is a crucial experiment between two rival theories.* For a single crucial experiment can disprove one of the two theories, and thereby prove its rival.

The fallacy here was first pointed out by Duhem. We can never assume, said Duhem, that there are only *two* possible theories. We cannot argue that because one theory is refuted by experiment its rival must be true. Both of them might be false. Bacon had been misled by the analogy with proof by *reductio ad absurdum* in mathematics.

So we have to give up the idea that experiments can prove theories to be true. But perhaps they can prove them to be false? After all, a single experiment can *contradict* a general theory, even if no finite number of experiments can *logically imply* one.

There are two important arguments against this idea, one proposed by Duhem and the other by Popper. Duhem's argument was that since we never test a single generalisation in isolation, but always in conjunction with other assumptions, we can always blame any failure of a prediction on one or more of the other assumptions. We have seen a good example of this in the way in which the phlogiston theory was retained in the face of the fact that calxes increase in weight, by modifying one or more auxiliary assumptions.

Popper's argument is an even more radical one. It begins from the apparently finicky point that *experiments* cannot contradict theories at all—strictly speaking a theoretical statement can only be contradicted by another statement. So it is statements *about* experiments, or experimental reports, which can contradict theories. But now, says Popper, all statements, even statements describing the results of experiments, are actually *interpretations of what we see in the light of hypotheses which we hold.* Thus, even so-called experimental data are riddled with hypotheses—and since the hypotheses which riddle them might be false, the data might be false too. This means that the scientist, faced with a clash between some theory and an experimental report, need not always *accept* the report—he may try to criticise it, or rather the hypotheses presupposed by it.

Our case-study is full of excellent examples of this. I will remind you of only one of them. Priestley reported that lead calxes yielded 'fixed air' (carbon dioxide) on being reduced without charcoal. This report presupposed the hypothesis that the calxes were pure ones, and contained no charcoal themselves. Lavoisier's followers criticised this hypothesis, and managed to refute Priestley's refutation!

So it seems that experiments can neither conclusively prove theories, nor conclusively disprove them. So what *do* they do? Did Lavoisier's beautiful experiments play no role at all in the chemical revolution? Was his victory merely an irrational change of fashion, where chemists jumped on his bandwaggon because it promised something new? Some philosophers of science, impressed by Duhem's and Popper's arguments, seem to think that this is what scientific revolutions are like. But they are wrong. Experiments played a key role in the chemical revolution, as I shall now explain.

Experiments were not the *only* thing, however. Indeed, the first and most crucial step in the chemical revolution was not a new experiment at all, but a new theory.

And this point cannot be emphasised too strongly. Before Lavoisier devised a new theory of combustion, phlogiston theory reigned in splendid isolation. And *theories which reign in isolation reign supreme.* Scientists will not reject a theory because of experimental difficulties before they have another one to put in its place. The mere invention of a rival theory gives them a *choice* which they did not have before—and they make the choice on the basis of experiment.

Lavoisier was well aware of this. In 1775 he admitted that phlogiston theory gave a nice explanation of the facts. And then he said that it would be "shaken to its foundations" because he had invented a different way of explaining the facts. Lavoisier was aware, in other words, of the desirability of inventing alternative theories *before* existing ones have been overthrown. What Lavoisier knew, many have forgotten. Scientific speculation is still being discouraged, especially in "settled fields"—as if the fields could ever be *un*settled, and progress made, without it.

So we now have *two* theories—and the story of the chemical revolution is the story of the battle between them. The battleground was the laboratory, and Lavoisier won. Why did he win?

If we compare the performance of the two theories, a significant difference emerges. Lavoisier predicted all sorts of strange new facts—the compound nature of the atmosphere, the production of a compound when hydrogen burned, and so on. These predictions were eventually confirmed. But while Lavoisier's theory *ran ahead* of experimental discoveries, his rivals struggled to account for them by elaborating new versions of phlogistonism. Each new version only succeeded in accommodating the refutations of the old—they predicted few, if any, new facts of their own.

Moreover, successive versions of phlogistonism were *incoherent*. Phlogiston began as an imponderable substance—then it was identified with hydrogen—then, in Cavendish's hands, it became an imponderable once more. This contrasts with the smooth development of Lavoisier's theory out of the first vague hunch of 1772 up till 1779. Even the apparent refutations of it produced by his rivals brought little change in its basic principles. Instead, Lavoisier predicted how they would turn out to be erroneous and his predictions were remarkably successful. Again and again, he managed to turn an apparent defeat into a victory.

To sum up then. Lavoisier's theory performed better because it predicted new facts, including some which overthrew the refutations of it, and because it developed coherently. Who could blame chemists for preferring it to Cavendish's version of phlogistonism, even though that final version could account for almost all the experiments?

Some of you may be wondering why I have not mentioned the fact that Lavoisier's theory was much simpler than the phlogiston theory. The reason is simple—it was not. But it is getting late, so I shall not pursue that particular strange fact.

Instead, I shall end by emphasising one important point. The point is this. It took a considerable time for the superiority of Lavoisier's new theory to become apparent. Its superiority was not obvious as soon as it was proposed, and no *single* experiment made it obvious. It is only *with hindsight* that we can pick out those few experiments which were crucial. This means that we should not condemn those who resisted the new chemistry—they just backed the wrong horse, which can happen to the best of us. Even more, in resisting Lavoisier they played a key role—only their ingenious refutations of Lavoisier enabled him to have some of his greatest victories. For theories, like boxers, cannot really show their prowess unless they get into a fight.

So, it was experiments, devised in the light of a new theory, which taught us that phlogiston does not exist. But learning such lessons takes time. Battles between rival theories are protracted ones. They are not like duels, to be settled with a single decisive shot. This conclusion is neither surprising nor disappointing. For when you think about it, it is only a fancy way of saying that science is a pretty interesting business.

SIR MALCOLM BURNS
K.B.E., C.B.E.

The Institute extends its congratulations on the recent award of a Knighthood to one of its members—Dr. M. M. Burns, the Principal of Lincoln College and a past-president of the Institute. The award recognises a near lifetime of distinguished service to science and to agricultural education.

Sir Malcolm was born and educated within the rural community of Rangiora-Cust and graduated from Canterbury College in 1932 with First Class Honours in Botany. The award of a Post Graduate Scholarship in Science took him to Aberdeen where he gained his Ph.D. in Soil Science. This was followed by two years at Cornell as a Commonwealth Fund-Harkness Fellow.

Returning to New Zealand in 1936 he spent a short time in D.S.I.R. before being appointed lecturer in soils at Lincoln. From Lincoln he became Director of the Fertiliser Manufacturers' Research Association at Otara, Auckland, 1948-52, but returned to Lincoln in 1952 as Principal, a position he has held ever since.

As Principal he has guided the destiny of the College for 20 years through its greatest period of expansion. This period has seen the student roll trebled, new buildings erected, new courses instituted and the scope and depth of research greatly increased. It was natural that a man of his ability would be called upon to serve in many other capacities. For example, the Department of External Affairs appointed him to report on and make recommendations for the establishment of Faculties of Agriculture in the University of Malaya, the University of North East Thailand and the College of Agriculture of Western Samoa. He is a member of the N.D.C., and for some time chairman of its Physical Environment Committee. He was a member of the Advisory Council of the D.S.I.R. from 1955-62 and Chairman for three years. He serves on many other advisory committees and Trust Boards.

Besides being a Fellow of the N.Z. Institute of Chemistry, he is a Fellow of the Royal Society of N.Z., the Institute of Agricultural Science and the American Association for the Advancement of Science.

But it is primarily at Lincoln College that Sir Malcolm has made his great contribution, displaying the foresight and leadership that has strengthened the position of that institution in the eyes of the country and the world. He has been ably assisted by Lady Burns, herself a graduate and person of high intellect. Their three children—one M.B. Ch.B., and two Ph.D.s—continue their heritage of science.

Sir Malcolm Burns

PEOPLE AND PLACES

From our branch Editors.

Auckland

University of Auckland

Professor A. D. Campbell Otago University, recently visited Auckland and gave a lecture at the University on "Organic Analysis Today".

Dr. Ken Schofield, Reader in Organic Chemistry at the University of Exeter, is visiting the Chemistry Department for three months. He is a specialist in the field of physical organic chemistry, and is well known for his research on the mechanism of nitration.

A Research Dinner was held in the University Chemistry Department on 9 August. Staff, research students and advanced undergraduates attended. The address was given by Dr. K. Schofield.

Professor G. T. Mills has resigned from his post as Head of the Department of Biochemistry at the University of Auckland. Professor Mills and his wife Dr. E. E. B. Smith are at present on study leave in Britain, where Professor Mills has been appointed to a position as Senior Regional Scientist.

Professor J. O'M. Bockris, Flinders University of South Australia, addressed the Auckland Branch on Electrochemical Technology as the Basis for a Pollution Free Future. The audience of 70 members and guests were given a lively account of the future energy economy of the world, and the role of electrochemical storage batteries and fuel cells in avoiding the pollution inherent in the internal combustion engine. Professor Bockris's remarks featured on the front page of the New Zealand Herald of 29 August.

N.Z.F.M.R.A.

Dr. A. C. Herd has recently joined N.Z. Fertiliser Manufacturers Research Association as a research scientist.

Auckland Technical Institute

On 19 and 20 September Mr L. H. Boulton delivered a lecture entitled "Tools of the Modern Chemist" to sixth and seventh form pupils from Auckland Secondary Schools. Mr J. K. Johnanesson arranged a working demonstration of modern instruments to illustrate the lecture.

During the winter term Mr J. C. Bowles conducted a highly successful course entitled "Computing for Chemists". Certificates were awarded to the successful students. The course will be conducted again next year.

An Applied Microbiology course has been organised by Mr R. B. Page, Senior Tutor in microbiology during 1972. Should demand warrant, the course will be available again in 1973. Those interested should contact him directly.

Manawatu

Professor J. G. T. Sneyd, University of Otago, addressed a meeting of the branch on "Cyclic Adenosine Monophosphate and Hormone Action".

A meeting of the branch was held at New Plymouth, at Ivon Watkins Dow Ltd. cafeteria. Visitors to New Plymouth were shown around the I.W.D. plant before the meeting. Dr. R. T. Gallagher, Massey University, spoke on "The Isolation and Structure of Dothistromin". The meeting was very successful and enabled the committee to maintain contacts with the Taranaki membership.

A special meeting of the branch for sixth and seventh form secondary pupils in the Massey area was held on August 16th. There was a large attendance of pupils and teachers. Dr E. N. Baker, of Massey University, spoke on "Architecture of Molecules". Afterwards, the pupils were shown some of the research equipment in operation. The evening was very successful, and engendered much interest.

New Zealand Dairy Research Institute

Dr W. A. McGillivray, Director of the D.R.I., has departed for Britain at the request of a large dairy company to advise on the setting up of research and development facilities similar to those of the D.R.I. He will then move on to Mexico City to represent New Zealand at the Meeting of the International Union of Nutritional Sciences, and will finally join the New Zealand delegation at the International Dairy Federation meeting in Tokyo.

Dr R. C. Lawrence, Supervisor of Fundamental Research, will also form part of the New Zealand delegation at the International Dairy Federation meeting.

The Processing Hall has now been renamed the Product Development Centre, and plans are well advanced for additions to the building to provide more facilities for large scale experimental production.

Mr J. A. Kavanagh has recently arrived from Wisconsin, U.S.A., and taken up his appointment as Research Officer in the Whey Products section.

Massey University

Professor T. Murachi, Professor of Biochemistry at Nagoya City University, Japan, who is the treasurer of the Japanese Biochemical Society, visited the department and gave a seminar on "The Structure and Function of the Enzyme, Bromlain (from Pineapple)".

Dr. J. C. Hawke has returned from a year's study leave spent at the Department of Biology, University of York, working on lipid biosynthesis by chloroplasts isolated from developing leaves.

Dr. C H. Moore has arrived from the Medical Research Council Laboratory of Molecular Biology, Cambridge, to take up a position as Senior Lecturer in Biochemistry.

Dr. J. W. Tweedie, from the University of Wisconsin, has taken up his position of Lecturer in Biochemistry.

Mr M. J. Boland's paper "Properties of a Protease from Actinidia Chinensis (Chinese Gooseberry)" won the student paper competition at the N.Z.I.C. Conference.

Wellington

Victoria University

Professor J. N. Smith presented a paper at the Insecticide Resistance Symposium at the 14th International Conference of Entomologists, held at Canberra in August.

D. C. Weatherburn Ph.D, currently at the University of Queensland has been appointed Lecturer in Inorganic Chemistry and will be arriving later this year. His main areas of specialisation are co-ordination chemistry and analytical chemistry.

Chemistry Division, D.S.I.R.

Dr. G. J. Leary, leader of the Organic Chemistry Section at Chemistry Division, spent ten days in August visiting wood and general chemical laboratories in Melbourne, Sydney and Canberra.

Mr P. L. Cropp has joined the Forensic Section after completing research work for a Ph.D. at Victoria University. At Victoria he studied, under the supervision of Professor Smith, the metabolism of organophosphorus pesticides. At Chemistry Division he will further the work of the Forensic Section on the determination of blood type from blood stains.

Work recently commenced on a new Minerals building at the Gracefield campus.

Soil Bureau, D.S.I.R.

Dr. M. Fields, the Director of the Soil Bureau has been elected a Fellow of the Royal Society of New Zealand.

Dr. K. R. Tate has been awarded a Nuffield Travelling Fellowship. He will be leaving New Zealand with his family in March 1973 to spend a year at the Macaulay Institute for Soil Research in Aberdeen, Scotland.

Nuclear Science Institute

Professor A. W. Fairhall from the University of Washington, Seattle, is visiting the Institute for a year. He is an authority on the distribution of radiocarbon in the atmosphere and oceans, and will study its occurrence in sea foams with particular reference to the biological cycle in the ocean.

Amongst those attending the Institute's annual conference were Dr. T. A. Rafter, and Dr. N. E. Whitehead who gave a paper on the use of proton inelastic scattering as an analytical method.

Dr. H. C. Sutton visited Australia in late August to attend the 6th A.I.N.S.E. conference on Radiation Chemistry. He lectured on his work on peroxy radicals in aqueous solution and was invited to chair one of the sessions. He also gave a lecture on "Radiation Curing of Paints" at the O.C.C.A. convention at Wairakei in July.

Geochemistry

Dr C. J. D. Adams addressed the Geochemistry Group during the Institute's conference on the subject of Rb/Sr and K/Ar dating.

Computing Services

Recently a computing facility has been established at the physics and Engineering Laboratories of D.S.I.R. to service D.S.I.R. branches in the Hutt Valley. For part of the day it is linked as a batch terminal to the Government Computer Centre, and this has resulted in a considerable improvement in time for the isotopic thermodynamic and theoretical physics calculations being carried out by staff of the Nuclear Science Institute.

Canterbury

Mr C. J. Harrison has transferred to Christchurch, where he is Factory Manager of I.C.I. (N.Z.) Ltd.'s formaldehyde plant.

Mr G. J. McCort has transferred to Ivan Watkins-Dow, New Plymouth.

Reverend D. R. Bastaing is at present studying in U.S.A.

Mr P. J. Melting, a winner of the Royal Society of N.Z.'s Secondary School Science Competition in 1971, has chosen membership of the N.Z. Institute of Chemistry for three years as part of his prize.

Dr. W. B. Earl, of the Dept. of Chemical Engineering, University of Canterbury, has been awarded an Erskine Fellowship to look at chemical engineering design teaching and will visit universities and companies in U.S.A., Canada, Europe, U.K., Japan and Australia. He will also present papers at the 4th International Chemical Engineering Congress in Prague, 11-16th September, one on behalf of Dr. R. B. Keey, and will participate in the 1st Pacific Chemical Engineering Conference in Kyoto, Japan, 9-14th October.

Mr A. E. C. Cutten has taken up a postgraduate scholarship at the Royal Post-Graduate Medical School, Hammersmith Hospital, London, after completing his B.Sc. (Hons) degree in Biochemistry at Lincoln College.

Mr W. H. Swallow has joined Chemistry Division, D.S.I.R., Christchurch, after completing his Ph.D. at the Chemistry Department, University of Canterbury.

Otago

Mellor Visiting Professor

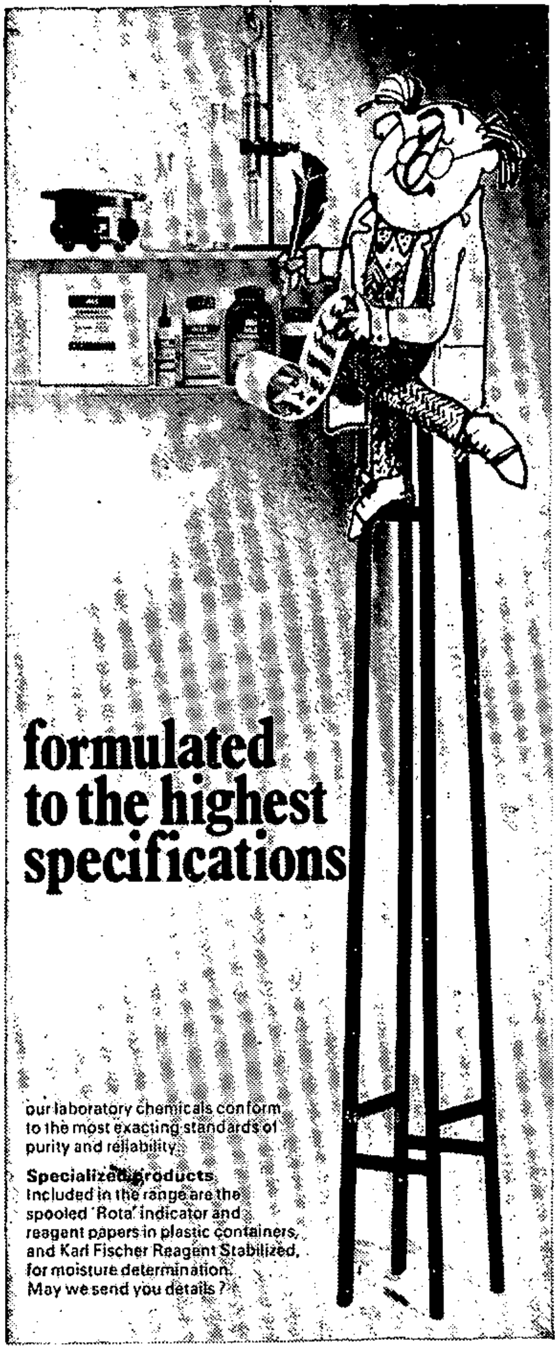
Mellor Visiting Professor L. G. Hepler arrived in the Chemistry Department, University of Otago in early August. Professor Hepler is Professor of Chemistry at the University of Lethbridge, Lethbridge, Alberta, Canada, and is author of two books and approximately ninety papers, most of which are in the field of thermodynamics.

Research Fellow in Textile Chemistry

Dr. Elizabeth Finnimore has joined the Faculty of Home Science as Research Fellow in Textile Chemistry. Dr Finnimore will be working with textile chemists Dr P. Barber on the synthesis of polyamide, polyester and other polymers which incorporate carbohydrate rings in the polymer chain.

Visit to Aluminium Smelter—Tiwai Point

The local branch arranged for a conducted tour of the aluminium smelter on Saturday 23 September.



BOOK REVIEW

Fundamental Aspects of Inorganic Chemistry
B. Chiswell and D. W. James.
John Wiley and Sons Australiasian Pty. Ltd.
1969 237 pages $A4.59

Fundamental Aspects of Inorganic Chemistry is a well-written treatise on inorganic chemistry with a physical bias sufficiently rigorous to be challenging for a beginner and yet not overpowering. The authors present an introduction to atomic structure, bonding, coordination chemistry and other topics from the quantum mechanical standpoint. The basic ideas are developed in the first four chapters with the fourth chapter acting as a bridge to the remainder of the book. In the latter chapters the principles that have been developed are applied to specific topics.

Chemical bonding is discussed almost exclusively in terms of molecular orbital theory with the importance of minimum potential energy being stressed as the criterion for a chemically stable system. Molecular orbital diagrams are used for both simple homonuclear diatomics and more complex examples such as the octahedral cobalt (III) hexammine complex.

The chemistry of solutions and dissolution of substances are discussed from the basis of thermodynamics. All calculations, as is typical of the book in general, are in SI units.

In chapter six, periodic relationships, including conduction which is related to packing of atoms and bonding, are discussed. In addition, periodicity is applied to energy of vaporization, electrical conductance, ionization and the chemistry of oxides and halides.

The book concludes with a moderately detailed description of the elements in the second period, including the diagonal relationships between the second and third periods. After a general introduction the elements in the second period are each treated separately.

This text is suitable for advanced secondary and Stage I students who are looking to extend themselves. In addition certain portions of the book could well supplement the theoretical chemistry taught at higher levels of university chemistry courses.

In this book the authors do all that they set out to do. One minor improvement could be the addition of questions at the end of each chapter.

J. C. Bowles.

LETTERS TO THE EDITOR

Despite suggestions made from time to time, no letters have been received.

FROM I.U.P.A.C.

NONENCLATURE, SYMBOLS, UNITS AND THEIR USAGE IN SPECTROCHEMICAL ANALYSIS—I: GENERAL ATOMIC EMISSION SPECTROSCOPY

The purpose of this document is to propose a consistent nomenclature for workers in spectromhemical analysis. Many of the terms have already been defined in several nomenclature documents, especially those developed by IUPAC, IUPAP, and ASTM. The fact that many of the symbols, units, nomenclature, and definitions previously recommended are repeated in this document demonstrates that nomenclature of a specific field, i.e., spectrochemical analysis, is deeply rooted in the general nomenclature of chemistry and physics. However, the adaption of a general system to a specialized field required a careful selection of general terms and the addition of new ones. The tentative version of Part I was published as **Tentative Nomenclature Appendix** No. 1 (December 1969) to the **Information Bulletin.** After careful revision the Commission on Spectrochemical and other Optical Procedures for Analysis of the Analytical Chemistry Division has now produced a definitive version for IUPAC.

Included in Part I are the following:

General Recommendations and Practices; Terms and Symbols for Physical Quantities in General Use; Terms, Symbols, and Units Related to Radiant Energy; Terms and Symbols for Description of Spectrographic Instruments; Terms and Symbols Related to Analytical Procedures; Terms and Symbols Related to Fundamental Processes Occurring in Light (Excitation) Sources; Photographic Intensity Measurements (Photographic Photometry). A statement by ASTM on Principles of Nomenclature Standardization is reprinted as an Appendix. A second Appendix deals with Application of the Concept of Optical Conductance.

Available as a reprint from: IUPAC Secretariat, Bank Court Chambers, 2-3 Pound Way, Cowley Centre, Oxford OXA 3YF, U.K. (29 pages) U.S. $1.20 (£0.40) Cash with order, postage free.

DEFINITIVE RULES FOR NOMENCLATURE OF STEROIDS

This work originates from a discussion held at the CIBA Foundation in London in 1950 between representatives of many schools. The proposals were subsequently taken over by IUPAC and amendments and additions made over a number of years. Tentative Rules were formulated by the IUPAC-IUB Commission on Biochemical Nomenclature and published in **IUPAC Information Bulletin** No. 33 (1968). These Rules have been studied further by the Commission and a Definitive version produced.

The areas of steriod chemistry covered are:

Fundamental Carbocycles, Pentacyclic and Hexacyclic Modifications, Derivatives, Stereochemical Modifications, Shortening of Side-chains and Elimination of Methyl Groups, Ring Contraction or Expansion, Ring Fission, Modification by Bond Migration (**abeo** System), Heterocyclic Modifications, Steroid Alcohols. An appendix gives Guidelines for Steroids containing Additional Rings.

Many of the principles in these Rules have become universally accepted in diterpene and triterpene chemistry; also to some extent for sesquiterpenes and for several groups of alkaloid.

Available from:
IUPAC Secretariat, Bank Court Chambers, 2-3 Pound Way, Cowley Centre, Oxford OX4 3YF, UK. (ii + 38 pages) US $1.50 (£0.503) Cash with order, postage free.

AUSTRALIAN & NEW ZEALAND SOCIETY FOR MASS SPECTROMETRY

The second conference of the Society will be held in Melbourne at The Victorian College of Pharmacy on February 12 through 14, 1973.

The organisers are calling for papers in the areas of organic and physical chemistry, GC-MS, data processing, isotope analysis, instrumental techniques and ion optics. Notice of intent to present a paper is required before August 31.

The conference plenary lecture will be delivered by Dr. Dudley Williams of Cambridge University. Further details on the above and on such items as accommodation, travel, the trade display are available from the Secretary, Mr. P. R. Sadesky, Perkin-Elmer Pty. Ltd., P.O. Box 216, Glen Waverley, Victoria 3150.

6th International Conference on Erythrocytes

The German Scientific Academy in East Berlin has sent us a large volume of some 600-odd pages containing the papers and discussions delivered at this conference.

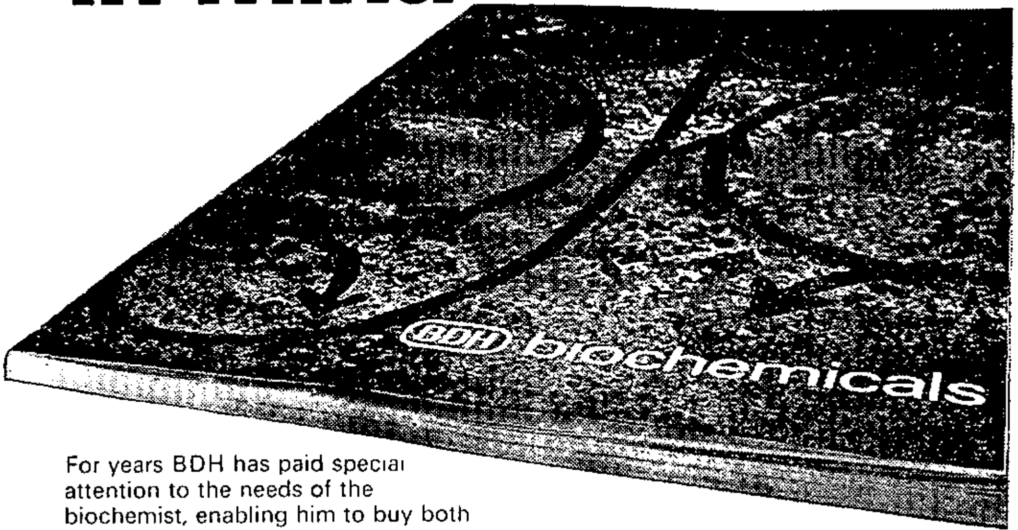
Our librarian, Mr S. G. Brooker, Box 9573, Newmarket, is looking for the best place to locate this, since we do not keep books, but only journals. Failing any other suggestions, he proposes to give it to the Medical Library in Auckland. Any other suggestions should be sent direct to the honorary librarian at the above address.

CHLORINE: AN ANNATATED BIBLIOGRAPHY— a selection of 488 abstrated references arranged chronologically from 1824 to 1971, according to the year published and, within each year, alphabetically by author. Additionally, a subject index and an author index are provided.

The bibliography was prepared by Dr. Ralph G. Smith and the Department of Occupational & Environmental Health, Wayne State University, Detroit, Michigan.

Copies are available from The Chlorine Institute, 342 Madison Avenue, New York, New York 10017 at U.S. $6.00 including postage at book rate.

Designed with the biochemist in mind



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the decision maker!

Integration, a feature in the atest Varian Techtron atomic absorption spectrophotometers, Models 1100/1200, provides precise, unambiguous readout of data and eliminates operator error.

The Models 1200 (digital readout) and 1100 (meter readout) are simple, compact and ideal for high volume routine analyses.

Other features include:

☐ readout: Model 1100 has 10 cm clear, open scale meter giving linear presentation; Model 1200 has four digit readout up to 9999.

☐ scale expansion from 0.3 X to 50 X in concentration mode.

☐ peak readout system measures peak absorbance — this feature is especially useful for short-duration signals such as those encountered with the carbon rod atomizer.

☐ choice of two integration periods (3 or 10 seconds); integrated results can be displayed indefinitely.

☐ curvature correction permits direct linear readout in concentration units over the entire 0-2.0 absorbance range.

☐ automatic zero adjustment corrects for baseline drift.

☐ push-button controls for instant selection of operating modes — absorbance, concentration, % transmission, flame emission.

☐ push-button flame ignition with safety-interlocked nitrous oxide-air changeover

These features allow for simple and highly efficient operation in the routine laboratory situation.

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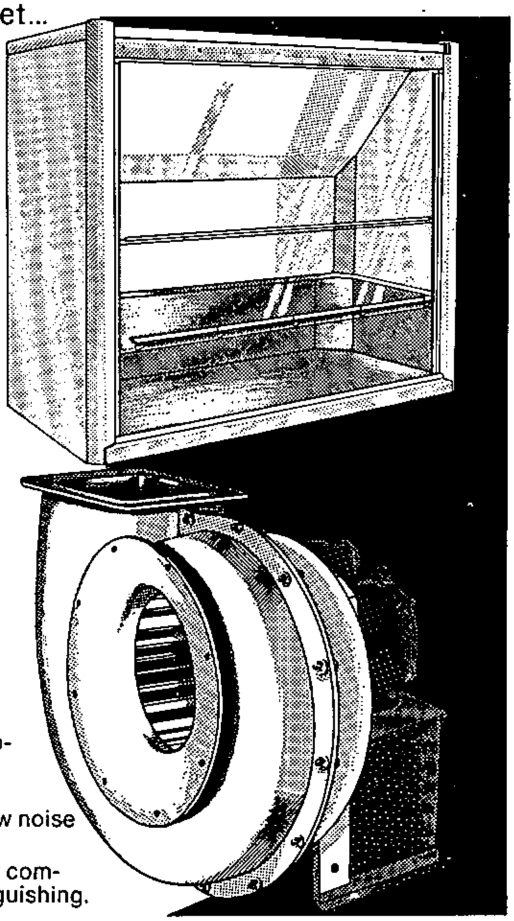

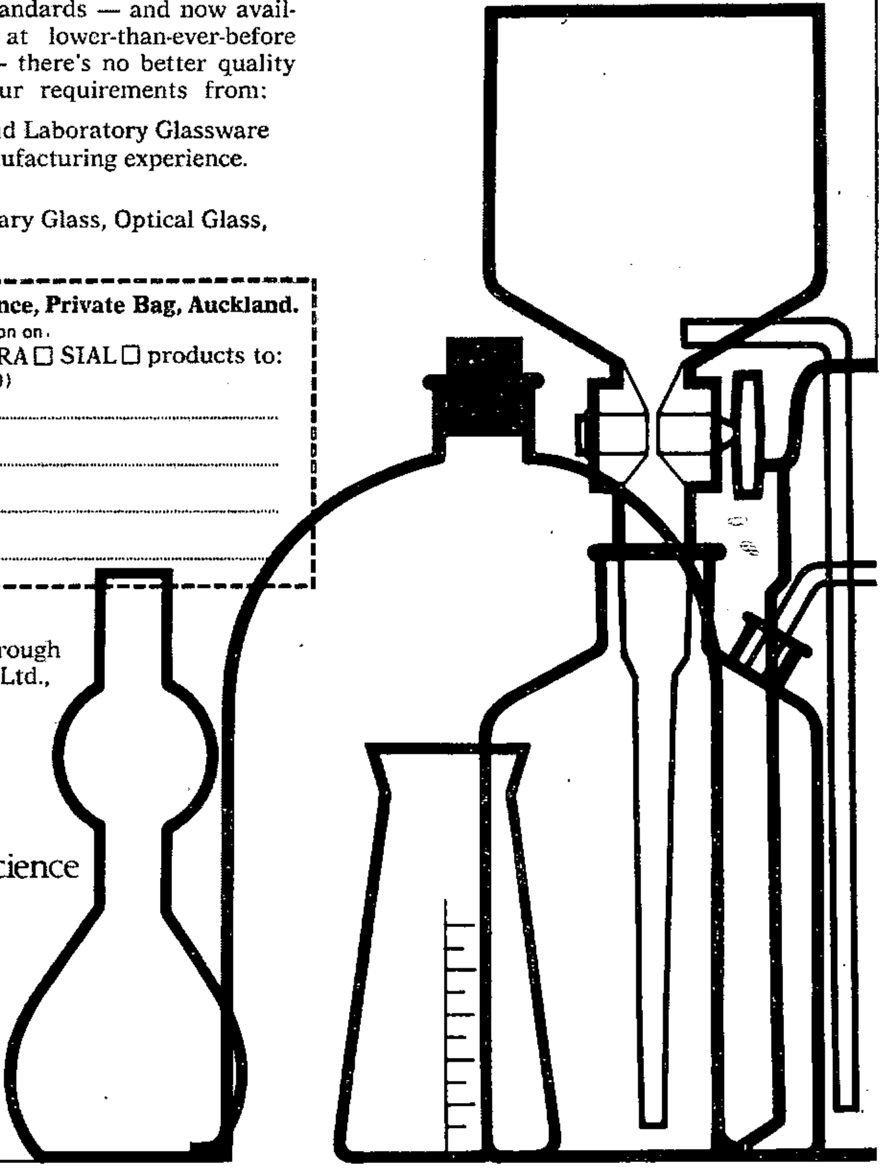

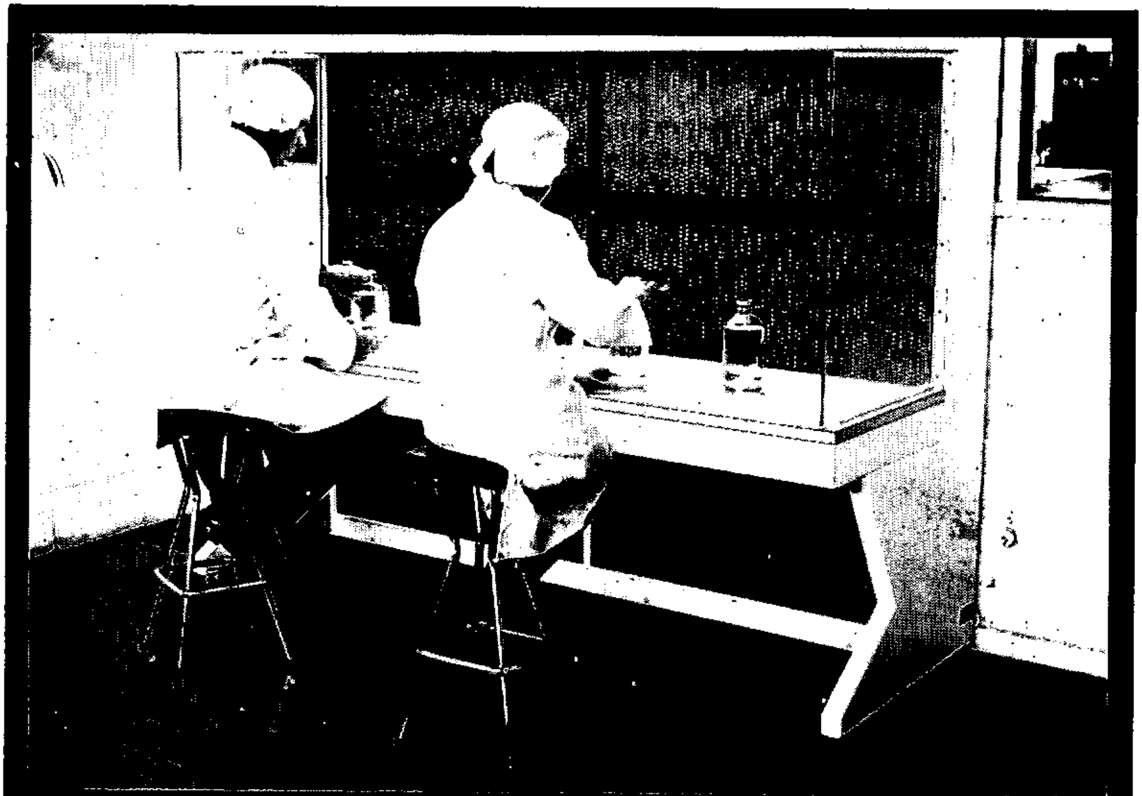

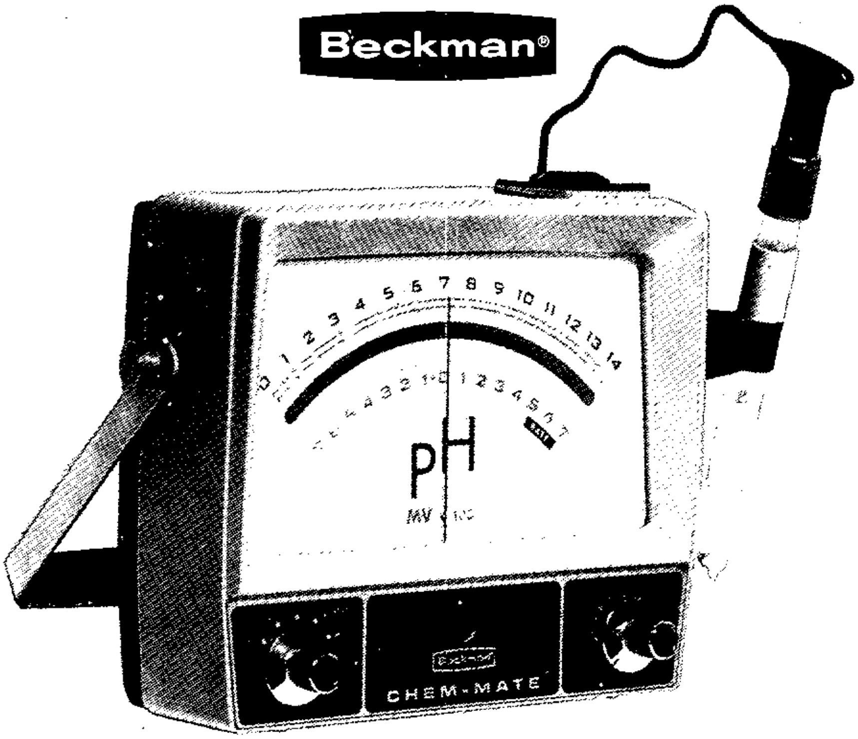
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