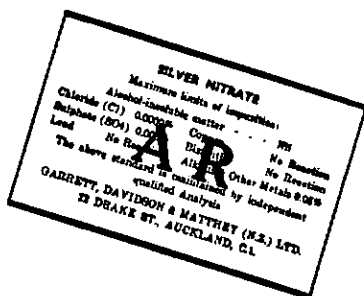
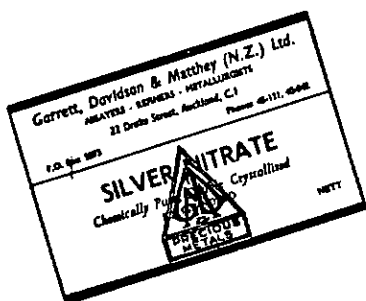


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INSTITUTE OF CHEMISTRY

Vol. 25 No. 5
October, 1961





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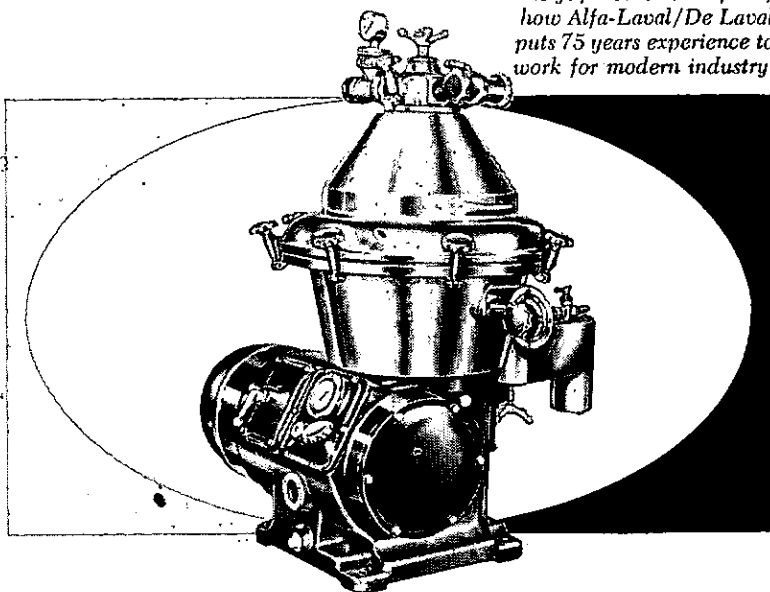
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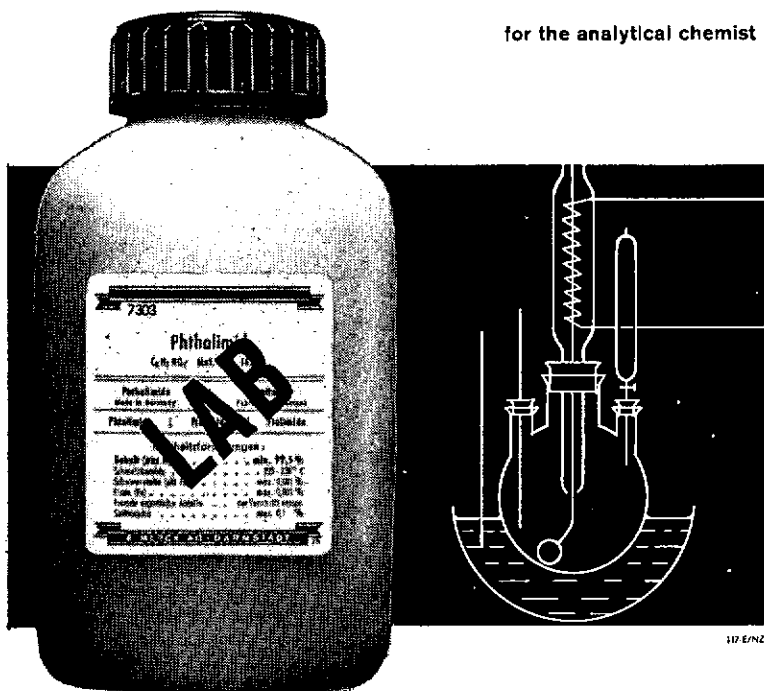
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MEMBERS ON THEIR METTLE

Two passages in this issue are related to one theme. The first (page 173) is the quotation, in the retiring President's address, from Professor Cottrell's challenging article published in the *Journal of the Royal Institute of Chemistry* for February, 1960, wherein Professor Cottrell assays analysts gravimetrically in terms of base and precious metals. The second (page 198) is the reference to Martin's methods of working with which the incoming President concludes a book review on *Gas Chromatography*. Both passages stress the importance of applying chemical theory to a problem before resorting to experiment.

A symbiosis between theory and practice is proclaimed in the mottos of both the R.I.C. and the N.Z.I.C. "Ratio atque usus"—theory and practice—says the one; "per libram et librum"—by balance and by book—exhorts the other. The older body places "ratio" first, the younger one prefers "libra". Was this an unconscious reflection of emphasis when our motto was adopted thirty years ago? The important point is that the words that link the Latin nouns are translated "and" not "or".

For amusement let us now take Professor Cottrell's metallurgical gradings of chemists quite literally, using B.D.H. Catalogue prices. Then our plumbivalent, argentivalent and aurivalent chemists are graded respectively in the proportions 1 : 60 : 1200. Before such evaluation daunts the more modest in our ranks let us remember that these metals commonly occur together and that even AnalaR lead may contain a little silver.

It should not, moreover, be inferred that the aurivalent analyst, in terms of Professor Cottrell's definition, is always superior to his colleagues. For some purposes silver, like wisdom, is more to be desired than gold, and lead than either. The aurivalent chemist who establishes that an analysis is not necessary may not be as valuable as the argentivalent or even the plumbivalent when it is unavoidable. Major discoveries have emerged from the work of men of limited background who have pursued the collection of facts with skill and integrity. The genius of a Martin lies in the formulation, from theory, of significant questions which

can be posed by experiment. Dependence only on theory, itself subject to change as new facts accumulate, can lead to error and even to dogmatism. The phlogistonists, pursuing a theory eminently serviceable in its day, pooh-poohed La Voisier. With similar *a priori* reasoning and heedless of the facts, Davy at first derided Dalton. The "ratio" of the R.I.C. motto is notorious for the variety of its meanings, some of which come perilously close to "dogma".

Most of our members will consider themselves in the silver class, and not a few will hope that they contain some gold in their composition. If the ideas that Professor Cottrell and Professor Parton enunciate are applied in chemical education, academic cupellation and quartation should in the future yield graduates of higher carat. But members of our Institute, however deeply concerned they may be with problems of chemical education, are themselves mostly beyond its refining processes. So to give this persiflage some point, before it falls too deeply into the pitfalls of analogy, we commend the addresses referred to in the hope that members will find in them a stimulus to see that "libra" is not used when "liber" can give the answer; to consider whether "ratio" can improve "usus"; and so to transmute themselves to nobler metal.

ELECTION OF TWO HONORARY FELLOWS

At the Council Meeting on August 29, the Institute conferred its highest honour upon two well-known members by electing them to the Honorary Fellowship. The new Honorary Fellows are:

Mr R. L. Andrew, who retired from the position of Dominion Analyst in 1946.

Mr A. D. Monro, formerly Associate Professor, Victoria University of Wellington, and a member of the Chemistry Department at Victoria for nearly forty years.

Both Mr Andrew and Mr Monro were foundation members of the Institute and played leading parts in its early development. Mr Monro was a member of the Publications Committee which started this *Journal*. It is proposed to publish fuller accounts of the work of the two new Honorary Fellows in the February 1962 issue.

THE TRAINING AND EDUCATION OF CHEMISTS

H. N. PARTON

(University of Otago, Dunedin)

Presidential Address delivered at the Annual Conference in Auckland,
August 30, 1961.

When a member of the Institute is honoured by his colleagues through election as President, he has a choice of subjects upon which to address them at the Annual Conference. He may choose to speak about the profession of chemistry and the state of the Institute at the moment, a theme which is perhaps most appropriate in a jubilee year or on the completion of a decade of its life. He may decide to discuss the special field of chemistry on which his own interests are focussed, or some new development in theory or practice which he thinks is of special importance to chemistry as a whole. He may choose, as I have done, to offer some comments on the problems of producing the next generation of chemists, a theme which is perhaps equally appropriate to the current producers—i.e., those of us who teach chemistry in schools and universities—and to the consumers who have to integrate our products into state, industrial or agricultural laboratories and factories.

However, before turning to my topic, I want to make one comment of the "state of the nation" type. In a recent issue of the Institute *Journal*, the Editor quoted me as saying of the Institute, when it was seven years old, that ". . . apart from providing opportunity for chemists to meet and hear each other's views on technical and general topics, admittedly a useful function, and admittedly one which the Institute performs excellently, I have been unable to persuade myself that the Institute is performing any function at all." At the time this brash utterance was made, the Institute was young (seven years old), small (about 150 members), and involved in the permanent problems created by New Zealand's difficult geography. By 1955, W. G. Hughson, reviewing the first 25 years in the Jubilee issue of the *Journal*, was able to list many significant achievements of the Institute, and today with the membership about 600, it surely justifies the claim made for it by Dr W. A. McGillivray in the same Jubilee issue, that it "is probably the strongest scientific organization in New Zealand and its Fellowship and Associateship are recognized and valued qualifications." The Institute has grown to maturity and developed many functions and we may hope that its critic of 24 years ago has also matured in judgment with the years.

Our geography will always make it an expensive business to bring the Institute Council together for quarterly meetings, but we can and do afford it, and are no longer dependent on proxies as in the early years. Our *Journal* expresses the vigour of the growing profession which supports it, and its growth to maturity early in the 1950's measures the development of the Institute it serves. It has taken the New Zealand Institute of Chemistry some three decades to reach this position. It is my sincere hope that, before another decade passes, other scientific groups will have established institutions of their own with parallel functions to ours. The geologists have recently done so. Many of our members will also be members of such sister organizations, just as they are members of the branches of the Royal Society of New Zealand, which serves different functions again. The time will come when we will see created something similar to the Australian Academy of Science, but I think that time lies some way ahead.

In speaking of the training and education of chemists, I am implying that the two are not exactly the same. This is a commonplace of current thought about education, but the distinction is a valid one. By training I mean that part of a student's education which emphasizes that he is preparing himself for a particular profession. A good deal has been said about the teaching of chemistry at all levels, in New Zealand as elsewhere. You will find articles about it in the *Journal* of August 1952 (F. G. Soper and G. Maskell Smith), again in February 1956 (E. J. Searle, H. S. Maslen, T. A. Turney and N. L. Edson); W. A. McGillivray (in the Jubilee issue) and N. T. Clare (Vol. 19, 118, 1955) have discussed, among other aspects of the problem, the training of chemical technicians; my predecessor in the office of President, Professor S. N. Slater, dealt with the university chemist (Vol. 17, 1, 1953) in an admirable way. My excuses for developing this theme once more are two. First, at the end of this year, the University of New Zealand, from which the great majority of Institute members have graduated, terminates its life in its 91st year. In the years leading up to this climax of university reform, the Science faculties in the constituent colleges (for several years now constituent universities), have already been diverging in their degree patterns, going forward to meet the future in the ways decided as best by each alone. It is justifiable, I think, to look briefly at the past, critically at the present, and hopefully at the future, of university chemistry in New Zealand. Secondly, I have been stimulated by reading again some of the views referred to above, and particularly by Professor

T. L. Cottrell's Inaugural Address at Edinburgh, published in the *Journal of the Royal Institute of Chemistry* in February 1960. Professor Cottrell is an iconoclast, and his views are worth thinking about.

As the Institute is only 31 years old, and the number of professional chemists has increased by a factor of five during its lifetime, it is safe to assume that a large proportion of its membership has been trained in the degree pattern established in 1928. They may be excused if they assume that there never was any other pattern and question whether anything as good could be devised, let alone anything better, than that which nurtured them. The truth is that in the high and far off times when I was a student, to go no further back into the depths of historical eras, there were only two divisions of university studies in the B.A. and B.Sc. degrees called Pass and Advanced. The Pass (first year) course became Stage I in 1928, later incorporating into itself the separate Intermediates for Medicine and Engineering, which had had one paper examinations only, with Medical Organic as an extra. If you took Chemistry beyond Pass Chemistry in a B.A. or B.Sc. degree, you committed yourself to two years' work to sit Advanced, with only an internal "terms" examination at the end of the first advanced year. The B.Sc. degree involved either five Pass courses and one Advanced (equivalent to seven "units" in the later pattern), or four Pass and two Advanced (eight "units"). Advanced Courses were examined overseas (World War I had liberated Pass Courses from this incubus), owing to the prevalence of the myth that overseas examiners were necessary to "maintain the standard of the degrees".

The new degree pattern of 1928 was the 8 unit degree, comprising at least four separate subjects, at least one taken to Stage III and at least one other to Stage II. Prerequisites were almost non-existent. If a Department wanted to ensure that all its students of, say, Stage III had taken some other subject as a common background—e.g., Physics and Mathematics for Chemistry—it had to rely on persuasion. Prerequisites came in for subjects such as Applied Physics, Mathematical Physics, Applied Chemistry, Psychology, and the "medical sciences", Physiology and Biochemistry. Initially they were Stage I subjects only, but, as the sciences developed, it became more common for advanced units to be required, notably Chemistry II for Biochemistry III. The University began to move away from a science degree in which students had the widest possible freedom to present almost any combination of subjects. As compared with the pre-1928 B.Sc.

degree, the new one had increased the minimum requirement from seven to eight units, and the extra unit was at Stage II level—a very considerable increase. This made it possible for a chemist to take Physics II or Pure Mathematics II (or both), to the benefit of his capacity to deal with the rapid developments in theoretical chemistry; or similar combinations with advanced units in the field sciences. In short, advanced subsidiary units became available. Inadequate staffing, however, made it impossible to make these genuine subsidiaries. A chemist wanting Mathematics beyond Stage I had to take Pure Mathematics II, and this course was rightly planned with the needs of mathematicians in view, not the needs of chemists. For the able students, this was probably all to the good. For the less able, it raised problems. It was always a matter of distress to me in planning Stage II Physical Chemistry that I was compelled to regard it strictly as part of the base from which to advance to M.Sc. in chemistry, virtually disregarding the needs of students for whom it was a terminal course, contributing to their background for their main subjects, such as the biological sciences. Moreover, the pass standard had to be the same for all, and the Senate in its wisdom had decreed that the pass mark was to be changed from 40% to 50% without changing the pass standard. This would have been harmless enough if it were always realized, by students and by all examiners, that a candidate does not pass because his paper is worth 50%: he is given 50% because his paper is just worth a pass. A candidate who gets 80% may be three or four times as good as one who gets 50%. New Zealand has been bedevilled with percentage (numerical) examination marks, though an advance was made when the practice of giving them to candidates was abolished except at one college. I will not pursue this, as its status is that of the confessional and the Official Secrets Act.

To return to the matter of advanced units as subsidiaries to other subjects, further complications developed as greater freedom was given to the separate university colleges after World War II. In chemistry a change had been made about 1942, so that each major branch of the subject could be developed over the two advanced years of the B.Sc. degree. Previously Chemistry II was usually organic chemistry (two papers) and Chemistry III inorganic and physical (one paper each), with aspirants for Senior Scholarships taking all four papers in their final year, having already passed two of them in the previous year. One result of the change was to increase the content of Chemistry II and III. Then the individual colleges began to introduce prere-

quisites independently. For example, Biochemistry at Otago required a student taking Biochemistry III to include in his degree Chemistry I and II, Physics I and either Zoology I or Botany I. Six out of eight units were fixed by regulation. If the student intended to take M.Sc. in Biochemistry, then Chemistry III was required; that is, seven out of eight B.Sc. units were fixed. If Chemistry III had required, say, Pure Mathematics I (as it should do), the whole of a B.Sc. leading to M.Sc. in Biochemistry would have been fixed by regulation, with no freedom of choice (other than a minor one) to the student. When this point is reached, a degree has ceased to be a general degree, and the question of introducing parallel degree courses for undergraduates becomes urgent. Otago has in fact now taken the logical step, for this and other reasons, as I shall indicate shortly.

The M.Sc. degree in chemistry also underwent development. It became very difficult for a student to complete a thesis by about September and do himself justice in an examination in November, in a subject increasing rapidly in amount of material and depth of treatment. Subjects other than chemistry had long needed up to two years for the thesis. The first change was to permit the presentation of the thesis at any time subsequent to the written examination. Then Canterbury and Otago made two academic years the normal period for the M.Sc. degree, with the right to complete in a shorter time safeguarded. The result was the production of some theses which, both in quality and quantity, were adequate for the Ph.D. degree. In addition it was made possible for the degree of M.Sc. (without Honours) to be gained on the written papers only. This came near to implying that honours was obtained by thesis; if papers plus thesis equals M.Sc. plus Honours, and papers without thesis equals M.Sc. without Honours, what follows? The answer, of course, is that the equation is not valid. I have long believed that the invalidity goes much deeper. I believe strongly that it is desirable to make two separate statements about a student's quality; one about his ability as revealed by his performance in written papers which are entirely his own work, and one about his ability to prosecute a small piece of closely supervised research and produce an account of its results and their relation to other work. We have long been involved in the illogical task of expressing the sum of these incommensurables in a single statement—first class or second class, etc. (Of course in private testimonials we separate them again.) I believe it is unwise to try to classify theses into more than three grades; let us call them pass with distinction, pass, and

fail. It is possible, as some examiners do, to grade them α , β , etc. and even add finer distinctions by plus or minus. The use of a percentage scale cannot be justified at all, and to combine a percentage thesis mark with similar paper marks is surely an academic crime. I know other academic people do not agree with this, or, if they do, see no reason to do anything about it.

I have dealt, approximately and inadequately, with the past because it is the basis from which the Science faculties in the four universities, now about to be independent (though with certain limitations placed upon their independence), have planned their future degree patterns. As I see it, some of the problems are as follows.

First, the content of chemistry is increasing very rapidly. About this I agree with Professor Slater's view "that at no stage has it been possible to teach the whole of the subject in detail, that it would be folly to attempt it . . ." Moreover the advance of chemical theory enables us to include more facts within the scope of wider generalizations, thus easing the burden on the memory, and there is no point in cramming students with all the latest information, since much of it will be superseded by more significant information. We should try rather to give students a firm grasp of fundamental theory into which they can fit new information as they acquire it. This, however, raises the second problem. Fundamental theory in chemistry is increasingly embraced within the quantum mechanics of atoms and molecules, and the statistical mechanics of assemblies of large numbers of atoms and molecules. If chemistry students are to be given a firm grasp of these fundamental theories, their mathematical equipment must be increased, and they must acquire the background of classical physics, especially thermodynamics, earlier in the course than has been usual. This can be done for only a proportion of those taking chemistry as the major subject, and points to the desirability of introducing optional courses, by which the needs of students taking advanced chemistry as subsidiary to some other science can also be met.

This is the current trend, and options can be introduced in various ways. Auckland and Canterbury have introduced them at Stage III, which they divide into IIIA and IIIB (this is Auckland's terminology) both being required as preliminary to M.Sc. in Chemistry, which I understand will continue to follow the old pattern. At Otago we have been rather more drastic, and as our new system involves certain verbal changes in the description of courses which may appear confusing, I will explain

the new system briefly. The degree of B.Sc. is to be conferred as an ordinary degree (three years) or as a degree with Honours (four years). The degree of M.Sc. will be awarded on a thesis, without classification into grades of Honours, though distinction may be granted for a thesis of special merit. The new features of the system are as follows:

(1) An Intermediate of four subjects selected from seven is required of all candidates for a B.Sc. degree, and a candidate who has not passed in at least three Intermediate subjects cannot present himself for examination in any advanced subject. Limitations of this kind have existed in B.Sc. degrees for some years and are of long standing in professional courses. The old Stage I courses have been renamed Intermediate. The number of hours involved in each has been fixed to permit four subjects to become the normal first year for the average entrant with experience in Upper Sixth. By this means the tendency for courses to increase in content is checked, without alteration in the standard. (Content and standard are quite different concepts, though the latter is not completely independent of the former. Some subjects appear to be "difficult" merely because of an overwhelming amount of relatively simple material.)

(2) The concept of Honours Schools is introduced, entry to an Honours School requiring prior completion of the full four subject Intermediate.

(3) Special examinations (better called supplementaries) are introduced to balance the limitations in (1) and (2). A candidate may sit a special examination in only one subject.

(4) While students will not be prevented from proceeding to B.Sc. (Hons) following the ordinary B.Sc., most of those who desire an Honours degree will enter an Honours School as early as possible. This is desirable and in some subjects will be almost essential if a good degree is to be taken.

(5) The introduction of parallel courses required a change in nomenclature. In the ordinary B.Sc., Chemistry II and III become Chemistry Advanced I and Advanced II, a modified reversion to pre-1928 terminology. In B.Sc. (Hons), the post-Intermediate years are called Part I, Part II and Part III, the last corresponding to the old M.Sc. lecture and examination course. The differences between the Honours and ordinary courses depend on departmental requirements. In mathematics they are entirely separate classes, and Pure Mathematics Advanced I has been planned with the needs of physicists and chemists in mind, a most valuable forward step. In chemistry separation is at present

limited to physical chemistry. The Part I course is planned to permit a fairly rapid coverage of Thermodynamics, and the whole class is doing Pure Mathematics Advanced I as a compulsory subsidiary.

In the ordinary B.Sc., optional courses are offered in chemistry. The "organic option" is the traditional course: organic and physical (including structural) courses in the second year (Advanced I) and a course in each established branch in the third year (Advanced II). The "inorganic option" has inorganic and physical courses in Advanced I, and three courses in Advanced II, one in each branch. The latter option provides a suitable subsidiary for geology, and the whole option is more suitable than the other for post-primary teachers. This latter profession differs from all other pursuits of chemistry graduates in that we actually know what, out of all the range of chemical knowledge which could be taught, they will need in their professional life. In the foreseeable future, it involves far more inorganic than organic chemistry, and in the laboratory far more experience with inorganic materials. So long as these traditional divisions of chemistry remain (and I would like to see the end of them), the "inorganic option" seems to me a useful one. B.Sc (Hons) candidates do both options.

(6) In the Honours School the "unit" pattern of the last thirty years is substantially modified. Stress is laid on advancing the principal subject at a faster rate than is desirable in the ordinary degree. Honours students pursue more of a common pattern of subjects, though an "elective" subject is available. Moreover subsidiary subjects are not necessarily identical with full "units" of the ordinary degree.

While a Part III student will not present a thesis, his laboratory work will be a research project of the traditional M.Sc. type, but limited in time in the way common before World War II. Much of the value of the M.Sc. thesis, which has contributed greatly to the training of New Zealand chemists, can be retained by an investigation stopping at the end of the second term (or perhaps three weeks later). The student could then be given a limited time to write a report, and prepare a short lecture on his work. The report would not be marked, except pass or fail, for the reasons I have outlined earlier.

The third major problem is undergraduate laboratory work in advanced chemistry. I have somewhat nostalgic memories of the unorganized and almost unsupervised laboratory work of my own student days, when we were in the chemistry department

from 9 a.m. to 5 p.m. and later, except when we were away for lectures or laboratory classes in other subjects. It took us 25 hours a week to do about the same number of experiments as are now done in a third of that time in organized, supervised classes, but we did have time to repeat the determination of C and H in oxalic acid five or six times to get a reasonable result, using a gas-fired furnace which made excellent toast when at full bore. We spent a long time analysing cement gravimetrically and preparing and standardizing volumetric solutions. Quantitative analysis remains basic to the solution of problems in chemistry, and while pH meters replace indicators, and optical methods have transformed many kinds of analysis, it is still necessary for chemists to learn some of the traditional techniques. But how much of them? There is no clear-cut answer. Professor Cottrell, however, has some interesting ideas. He came to his chair at Edinburgh from industry, and looks at both the university and industry with a keenly critical eye. He remarks, for example, that "so-called experiments in chemical education are usually not experiments at all; they are simply changes of teaching method or material, without any vigorous attempt to follow up the results of the change. This means that most of the existing knowledge about the education of chemists is at the anecdotal level." This is true and applies to education more generally. He holds further that we should not set out to train new chemists to do exactly what older chemists are doing at present. He believes, as I do, that "the end of chemistry is its theory." When he says that "a high proportion of honours university graduates in chemistry are not very good chemists," he will probably find an echo from industrial laboratories. But when he gives his view on *why* they are not very good chemists, he probably shakes the walls of chemical orthodoxy inside and outside the university laboratories. For he says "when faced with a problem of a type he has not met before, he [the chemistry graduate] resorts to the purest empiricism, untinged by chemical theory . . ."; he goes on ". . . an even more depressing failing [is] a tendency to do experimental work rather than look up the literature . . . experimentation is a time-consuming and expensive business which should be resorted to only if it is certain that the result is needed and that it is not already known. One might say that if the chemist who can make a measurement (or carry out an analysis) accurately is worth his weight in lead, and the chemist who can improve the method is worth his weight in silver, the chemist who can show that the measurement is not necessary is worth his weight in gold."

To quote further, "The student spends most of his time in laboratory exercises, and . . . has to memorize a large amount of information for reproduction in examinations. Since the laboratory course consists largely of experiments (. . . really manipulative exercises) in which the method of working out the result is prescribed, he has little experience in the understanding and application, as distinct from the memorizing, of chemical theory." Cottrell criticizes the role of manipulative work ("most chemists do not do titrations"), meeting the objections which are no doubt rising in your minds with the comment "there are parts of chemical research where manipulative skill of a very high order is required of the chemist himself but this skill is usually too specific for it to be worth while training all chemists for it in an undergraduate course." He is thus led to advocate "a drastic reduction in the time spent in laboratory work," proposing its replacement by practical exercises in calculation, being careful to avoid calculations becoming as much a formality as practical work.

I have time for two comments only on these stimulating opinions. Some of Cottrell's points are met by the research project traditional in the New Zealand M.Sc. degree. This is why we will keep it in Part III of the B.Sc. Honours degree at Otago. My other comment relates to the traditional divisions of chemistry. In reviewing the symposium on the Kekule-Couper centenary (published under the title *Theoretical Organic Chemistry*), the Faraday Society reviewer (V. Gold) observed: "Reading this book, one is forced to ponder again how much chemical progress in this country is retarded by the strict segregation of the established sub-divisions of chemistry in so many of the chemistry departments of our universities, and alas also in the minds of some of their inhabitants." This is equally true of theoretical and practical work. For the latter, I think chemists have much to learn from chemical engineers. In particular the concept of unit operations could well be applied in chemistry courses. Many operations such as vacuum distillation, optical rotation measurements, fractional distillation, and chromatographic separations, are first introduced to students in a laboratory designated Organic Chemistry. To the minds of the students, these laboratory operations become inevitably Organic Chemistry (I hesitate to refer again to Gold's addition "also in the minds of some of their inhabitants"). These misconceptions continue till finally we find some students believing that infra-red spectroscopy and nuclear magnetic resonance spectroscopy are also "Organic Chemistry". Lest my friends who belong to the Cult of Carbon feel affronted at these references, let me

say that if I dared to reconstruct chemistry, I should probably eliminate Physical Chemistry entirely.

Classical Physical Chemistry has become General Chemistry, and some of it is done in the Intermediate year. More of it is incorporated into the theory of analysis. In fact one of the awkward features of the traditional three divisions of Chemistry is that in the laboratory there are four: inorganic, organic, physical, and analytical. The Americans have overcome this by devoting the second year of a four-year undergraduate course to the theory and practice of analytical chemistry, and by so doing can also offer a valuable subsidiary course to scientists other than chemists who want a general training in laboratory methods, which chemists can offer better than anyone else. With us, analytical chemistry has tended to be fused with inorganic, to the detriment of both, especially inorganic chemistry. The renaissance of inorganic chemistry, which is an outstanding feature of recent decades, has required careful consideration of university courses.

What of applied chemistry? Courses under this name were added to the B.Sc. degree in 1946 when the chemical engineering degree was introduced. They were initially taught at Auckland, Canterbury and Otago. Only the Otago course survived continuously from this beginning and it has provided both extra analytical work and some experience with materials of industrial importance. The course is now being developed in the direction of unit operations. Moreover, a second year of applied chemistry will be offered as one of the optional courses of the final year of B.Sc. Honours. Victoria University has introduced applied chemistry with special reference to instrumental methods, including those used in nuclear chemistry. Canterbury has modified its chemical engineering degree, but does not offer a separate applied chemistry in the B.Sc. degree.

In 1959 the Senate of the University of New Zealand received and referred to the University Grants Committee a report entitled "The training of Technologists in the Universities of New Zealand", based on a memorandum by Dr F. J. Llewellyn and Professor J. Packer. The Parry Committee endorsed the view that the training of professional technologists requiring the usual university qualifications should be done at the University. It recommended the introduction of more of the applied sciences, and in addition such technological subjects as food, wool, leather, and building technology. There is no doubt that development of such technological courses will take place in the near future. I

propose only to comment on them in relation to the training of chemists. Quite apart from the setting up of new degrees, as for example in Food Technology at Massey College, it is useful to see how subjects could be grafted on to the B.Sc. course, so that graduates in chemistry, whose first requirement is an adequate course in chemistry and its main subsidiaries (which I regard as mathematics and physics), can have an introduction to some technological specialty. I should say right away that the addition of more subjects to the B.Sc. course inevitably produces time-table problems, at least so long as we continue to allow students to go ahead with advanced courses while repeating others they have failed in, and in addition permit them freedom to "shop for a degree", or, to change the metaphor, to proceed to a degree by a random walk. In the B.Sc. (Honours) degree in chemistry at Otago, we have one subsidiary course described simply as "an approved course". This will usually be taken along with Part II chemistry, that is in the third year, and can be any course at all, in principle, which the Faculty of Science and the Professorial Board are prepared to approve. This year "approved courses" have been as follows: (1) A course in advanced Physics, not quite the whole of Physics Advanced I (Physics II); (2) Pure Mathematics Advanced II; (3) one paper of Biochemistry Advanced I; (4) Geology Advanced I. These are orthodox science courses, except that the whole of a unit is not necessarily required. We could, however, approve a course in Metallurgy, provided by the Mining School, or a proposed course in Industrial Management, if it proves satisfactory for the needs of future managers. If the demand for chemists with some metallurgical training grows, we could then add a more advanced course in metallurgy as one of the optional courses in the final year (Part III) of B.Sc. (Honours).

This is perhaps a convenient point at which to elaborate a little on the B.Sc. (Honours) course in chemistry as we have conceived it. I indicated earlier that following a four-subject Intermediate, the second year is devoted to Part I chemistry (which is 50% greater in content than Advanced I), and subsidiary Mathematics (Pure Mathematics Advanced I). The third year consists of Part II chemistry and the "approved course". In the final (fourth) year, the Part III course will be varied from the traditional M.Sc. course, which involves four papers, all of them compulsory, and with some variations in the nature of the fourth paper. We intend to offer seven optional courses of which each student will do four. Each course will involve about 39 lectures

(which fits a 26-week year conveniently). Though we do not at present contemplate compulsion, I hope that all candidates will take the course in Quantum Chemistry, as it will deal with bonding, which is fundamental to all chemistry, and which chemists should no longer be satisfied to deal with in a qualitative way based on doubtful analogies. It is often said, with justification, that advances in a science should enable courses to be shortened, because our much better understanding of certain aspects of the subject allows us to get down to the fundamentals of the science and thus link things together. Unfortunately for chemistry, its fundamentals reside in physics, and to get down to fundamentals means to go deeper into physics than many chemists like to do. Teachers of physical chemistry have always been troubled with the amount of time needed to lay the foundations of thermodynamics, quantum theory and statistical mechanics before grappling with chemical problems in terms of these theories. The other courses proposed are Inorganic Chemistry, Physical Chemistry, Statistical Thermodynamics, two courses of Organic Chemistry, and one of Advanced Applied Chemistry. As I remarked, other options, such as an advanced course in Metallurgy, can be added if a demand appears, and as the research project can be done in any field of pure or applied chemistry which staff members can offer, a student opting for a metallurgy course would probably do his project in that field, if the Mining School found it practicable. These ideas are at the moment only possibilities, but they illustrate the way in which the training of chemists can be widened without sacrificing adequacy in basic chemistry.

Finally, I must turn to the education of chemists, or more widely of science graduates. A former Vice-Chancellor of Adelaide, A. P. Rowe, who was at one time head of Telecommunications Research Establishment, has in the course of a book (*If the Gown Fits*) and various articles, dealt many shrewd blows at university complacency. In one of his articles, he wrote of "the importance of a broadening of education so that graduates go out into the world with some knowledge of how their profession fits into the pattern of society, and so that they are at least not less educated than they would have been if a university education had not channelled their minds along paths of specialization and the absorption of factual material. . . . There is no lack of lip service to the idea of a broadening of education in universities, but a rigid departmentalism and the professor-god myth prevent anything much from being done." (The myth referred to is hardly part of New Zealand's culture pattern.)

I have argued earlier, and Professor Slater argued before me, that we have never been able to teach the whole of chemistry and we should not try; and I have indicated some ways in which concentration of studies can be achieved within chemistry, and room can still be left for some other valuable vocational training. But how can we graft on something called education to all this training? The only answer I can give is that if it is a matter of "grafting on", it cannot be done. We can only rely on conceiving our courses in so broad a way that they will have educational value as well as training students in the limited sense. What do we mean by education? Let me quote some remarks from an article shortly to be published by Professor A. J. Danks.

The subject matter of course work done in the University may suffer several fates as the years pass and come to obtrude between the ex-student and his memories of lectures, texts, discussions and the rest. Some may keep professional knowledge well nourished by practice in their occupations. But the narrowly vocational material will often sink out of the conscious memory if it is not used. Techniques will become dim, and the burden of facts and information may be pretty well cast aside. But something will be left, and for want of a better term let me call it cultivation of the mind. This will be an amalgam of beliefs and attitudes; it will include an appreciation of methods of thought and of intellectual honesty and consistency; it will embrace tolerance, scepticism and ability to perceive resemblances and analogies. This enrichment, for that is what it is, is perhaps the best thing that serious work in some field of study can bequeath to those who have done it, whether it has been done in or out of the University.

A writer in the *Economist* (24/6/61) expressed this same view of the primary function of a university education in these words:

The University . . . exists primarily to give the country's ablest young men a pause of three or four years to think, to learn, and to reflect; to acquire intellectual (and perhaps social and aesthetic) standards which will enable them to see through bogus arguments quickly and to develop valid ones with conviction; and to realise that the frontiers of knowledge are neither certain nor static. The core of this process is the mastery of the basic theoretical principles of one or two subjects, and some acquaintance with those of others. The method consists as much in mutual discussion between the undergraduates as in formal or informal tuition. In this connection the really able men play a part out of all relation to their numbers, for they teach each other, form a serious link between dons and undergraduates, and set the intellectual pace for undergraduate life in the University as a whole.

Holders of this view of a university education are inevitably suspicious of "practical courses", and of courses which are claimed to be "relevant" to the needs of a specialized and intricate society, because they are planned with the immediate needs of that society in mind. Courses meant, with the best will in the world, to be relevant to today's needs may be entirely irrelevant to tomorrow's. As the *Economist* writer put it, "to use a military metaphor, those who argue for direct relevance are always committing their main forces to action against enemies which may not exist, or whose strength may be wildly misjudged."

I find myself returning to Professor Cottrell and will leave with you these final ideas of his with which I fully agree. We must plan our courses so as to give our students three things: a firm grasp of fundamental theory into which they can fit new information as they acquire it; a critical approach so they can appreciate the significance (or lack of significance) of new information; and an interest in chemistry so that they will seek to extend their knowledge of it themselves. It is by some such criteria that our success or failure is to be judged.

A very recent visitor (Dr K. L. Sutherland, now of the Colonial Sugar Refining Company) asked me whether our chemistry degree courses were designed for any other purpose than the training of university lecturers. There is more truth in the implied criticism than I find comfortable. I can only answer it in terms of the three objectives I have given. If our courses go a reasonable distance towards achieving *them*, they should produce the kind of graduates we need for the varied work which has to be done, including, of course, university teaching.

FLOUR

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Flours can be made by crushing or grinding many seeds. However, to most of civilization flour implies wheaten flour. Leavened bread is traditionally made by mixing flour, water, salt and yeast into a dough, allowing the mixture to ferment so that small gas cells are formed, then baking to fix the structure. The pre-eminence of wheat over other seeds for breadmaking purposes is due to the coherent properties of the dough which is formed on wetting the flour. Rye has this property only to a limited degree and the other cereals lack it entirely. Some other seed flours have coherent properties but are less suitable in other ways. The coherence is necessary, of course, for the cellular structure of bread to be developed. Our relative ignorance of the mechanisms of doughing, fermentation and baking is a fine example of how little we yet understand the common things around us.

To the investigator there are two obvious ways of looking at flour. These we may picturesquely describe as the microscope and the test-tube methods. Each alone may take us some way towards understanding but it is necessary that both techniques be used in parallel for full understanding to be obtained. The wheat grain is a highly ordered structure quite amenable to descriptive histology and its detailed structure has been known for a considerable time. The distribution of some nutrients within the grain has been studied because of its importance to milling technology and nutrition. However, it is only recently that the structure of flour has been looked at in detail.

Flour particles are anything but uniform in size and shape. This variation has two causes. Flour is produced by crushing the endosperm of the wheat grain which itself is not uniform in structure, the cells being smaller near the periphery and larger near the crease of the grain. These box-like cells contain essentially a mixture of large and small starch granules embedded in a proteinaceous matrix. This intracellular structure is itself broken into irregular fragments in the milling process, so that variously sized chunks of cellular structure exist together with isolated large and small starch granules and protein fragments in the flour.

The discovery by a German worker in recent years that the protein of the flour particles occurs in two distinct situations has gone far towards explaining the mechanism of air-classifica-

tion of flour, a modern development in milling whereby high-protein and low-protein fractions of flour may be produced. Part of the protein occurs as irregular particles filling the interstices between the starch granules. Hess termed this "zwickel" (wedge) protein. The other part of the protein lies as a film on the surface of the starch granules, and Hess called this "haft" (fixed) protein. These fractions may be separated by flotation methods, as the wedge protein is less dense than the starch granules.

Air-classifying of flour aims at separating the finest particles of the flour from the remainder by a system similar to a cyclone. The fine fraction consists mainly of small starch granules and wedge protein particles. A mid fraction of rather larger sizes can be obtained, which is chiefly large starch granules and has low protein content, whilst the largest sized fraction consists of chunks of endosperm having characteristics similar to those of the original flour. The complex air-classifying system is necessary since sieving cannot fractionate flour; in effect, the finest particles cling to the coarser ones and do not pass through the appropriate screens. For full economic advantage to be taken of air-classification it is necessary to break down the endosperm structure more than normally, while retaining the starch granules intact. This is accomplished by a turbo-milling process in which a flour-air mixture is passed through the space between two counter-rotating, pin-studded plates. Probably the structure is broken by turbulent buffeting of the particles.

The endosperm structure has been formed during the ripening of the grain from a system in which we may imagine the starch granules to be suspended in a solution of protein and other substances. Ripening is physically a desiccation and the properties of the mature grain seem to be influenced greatly by the rate and temperature at which this desiccation takes place. "Hard" wheats have a vitreous structure which is probably continuous throughout the endosperm, whereas "soft" wheats contain air spaces produced by shrinkage of the cell contents on drying. Whether or not such air spaces will form may depend on the quantity of protein in the grain and on its "quality". As we shall see later, many other things are influenced by this mysterious "quality" factor of the protein. Occasionally wheat ripens to a mottled state when there are patches of vitreosity in an otherwise soft grain. This we can possibly look on as a borderline state when ripening conditions did not lead to one or the other definite condition. Vitreosity of wheat is, however, largely the concern of graders and millers and has no direct interest for the baker

except that, as stated above, vitreosity is generally associated with higher content or quality of protein.

We have now to turn from the microscope to the test-tube and discuss the physical and chemical properties of flour. There are three major types of compound to consider; carbohydrate, protein and lipid.

Carbohydrate is largely in the form of the starch granules, although there are also free sugars, on which the yeast initially feeds, and pentosan gums which have been shown to have a considerable influence on the baking properties of flours. Like the starches of most other plants, wheat starch contains both straight-chain amylose and branched-chain amylopectin molecules. Surprisingly, the distribution of these two types of starch in the two sorts of starch granules does not appear to have been looked at. It has been shown that the two sorts of starch granule start to form at different stages in the growth of grain, so that we might well expect some difference in composition. It has also been shown that amylose and amylopectin are formed separately, not one from the other.

The proteins of flour, which account largely for its unique properties, have a long history of investigation. Gluten, the protein mass which is easily obtained by kneading dough under a stream of water, was the first protein for which a preparation was published (in 1745). In the late years of last century gluten and the flour proteins were the subject of much investigation, the names of Kjeldahl and Osborne being particularly associated with this work. Osborne classified the proteins into several water- and salt-soluble fractions and into gliadin, soluble in aqueous alcohol, and glutenin, insoluble in all but acid and alkali. This classification has remained, through periods of favour and disfavour, to the present time. Gliadin is sticky, easily wetted, but structureless, whereas glutenin is not sticky, is not so easily wetted and has a gelatinous quality which enables it to absorb a considerable amount of water.

The years following 1920 saw many attempts to fractionate gluten by the partial precipitation procedures of physiological chemistry which were then the vogue. By vigorous stirring gluten was dispersed in either aqueous acid or solutions of urea or sodium salicylate and from such "solution" fractions were precipitated by stepwise additions of neutral salts. The results were in all cases disappointing since, although a progressive change in properties of the precipitated fractions was found, there were no sharp changes in properties indicating clear separation of com-

ponents. These results naturally led to speculation on the nature of gluten; whether it was a system of very many components or of a few components with considerable interaction between them.

Although theoretical grounds have been published for deciding that fractionation by solubility and fractionation by precipitation must give identical results, the author returned to fractional solubility in working on gluten structure, and from the properties of the resulting fractions there emerge reasons for the superiority of the method in this instance. If dry gluten is allowed to stand in dilute aqueous acid, part of the protein disperses in the acid and part remains as an insoluble gel. The recovered protein fractions can be recombined to give a hydrated mixture closely similar to the original gluten. The properties of the gel depend strongly on the concentration of salts in the medium. As salts are reduced to very low concentrations by repeated washings or dialysis the gel swells and eventually reaches a stage where it becomes mechanically unstable and disperses, though not molecularly, in the acid. The protein gel is rapidly rendered soluble in acid by oxidation or reduction, both of which are considered to break disulphide linkages between protein chains. Examination of the dry gel protein by X-ray diffraction showed no ordered structure and it must therefore be presumed to be a random three-dimensional network of peptide chains with both disulphide and ionic linkages present.

That part of the gluten that was soluble in acid has been further split, again by fractional solubility, with a mixture of chloroform and methanol. Part gave an absolutely clear solution in the organic solvent mixture and has proved to have properties quite unlike most proteins, being somewhat lipid-like in character. This is ascribed to a relative lack of ionizable groups and high content of non-polar groups in the protein. It is not this lipid-like character itself which concerns us, however, but the solubility behaviour in water, acid and salt solutions. This protein, in fact, has no solubility in the accepted sense but forms two-liquid phase systems with salt solutions and in some circumstances with acid. A curve similar to the phenol-water phase diagram is easily obtained and by manipulation of the salt and acid concentrations in the aqueous phase three humps can appear in the curve. With more care, conditions can be chosen whereby a closed circle appears as part of the curve, similar to the well known nicotine-water system. The properties here are interesting, for a mixture of definite composition may be homogeneous at low temperature,

separatê into two layers on warming and again become homogeneous at a higher temperature. At temperatures above the maxima of the curves, the protein and aqueous phase are miscible in all proportions. Some complex lipids give similar two-liquid phase systems with water, but the gluten preparation discussed is completely free of lipid, although it contains about 10% of carbohydrate. The remainder is accounted for as amino acids.

We are now in possession of two facts which may explain the difference between fractional solubility and fractional precipitation when applied to gluten. First, there is an insoluble protein present which is not in true solution in the dispersions used for precipitation. Secondly, part of the protein forms liquid phases in aqueous media and from such liquid systems distinct fractions may not be obtained. I mentioned above that fractional precipitation was the vogue in protein chemistry during the nineteen-twenties. There is always some method particularly in vogue in most branches of chemistry and the method now in favour in protein chemistry is electrophoresis. This has been difficult to apply to the gluten proteins since the need for sufficient salt to suppress molecular interactions and the precipitating effect of even low concentrations of salts in solutions of gluten proteins appear mutually exclusive. However, these problems are being overcome and further advance in knowledge of the molecular structure of gluten may well come from application of this method.

We are still not in a position to answer clearly the question from the 1930's—"Is gluten a system of many components or of a few interacting components?" A tentative conclusion from the two fractionations mentioned above is that gluten is a giant molecule, the framework being the gelatinous protein, with the other proteins and lipid linked to it so that there is a continuous molecular structure throughout a piece of gluten. The mechanical properties of this structure may easily be modified by change of pH and salt content, mechanically and by oxidation, all of which processes may occur during bread making. Therefore, rather than attempt further fractionations or structural studies I have concentrated on the effects of some of the changes mentioned.

The fractionation by mixing with dilute acid has proved a good tool for investigating changes in dough structure, once the more important experimental variables had been recognized and controlled. The mixing of the dough is of great importance in baking, for too little or too much mixing alike cause a deterioration in the quality of the loaf produced. The optimum amount of mixing

varies for different flours and there are differences in the tolerance of flours to more or less mixing. These properties of mixing time and mixing tolerance can be determined comparatively easily with laboratory mixers which record the amount of work performed on the dough piece, and the results are frequently used as a guide to the required treatment in the bakehouse. The two experiences are not exactly parallel, however, and no clearly recognizable part of the mixing-work curve necessarily corresponds to the baker's assessment of dough requirements. Different mixers seem to do different things to the dough piece; principally compression, extension and shearing occur in varying combinations.

The proportion of gel protein to be found in gluten from dough at various stages of mixing has been determined, using four types of mixing. Since the physical difference between, say, hand-kneading and the use of a Z-arm mechanical mixer appears so great, it was a surprise to find that all four mixings gave similar results. There was a sharp fall in the proportion of gel, for about the first two minutes, followed by a rise for five or ten minutes to a maximum. There was then a decline, steep where the mixing appeared severe and slight where the mixing appeared gentle. The earlier observations of the structure of flour are where we look first for an explanation of the observations. The initial, falling part of the curve probably represents the freeing of the shell of "fixed" protein from each starch granule, so that in the earliest stages this will be washed away with the starch granules in preparing gluten for examination, and the protein of the gluten is chiefly that of the "wedge" protein. At the minimum of gel content the "fixed" protein has been transferred from the starch surface to the surface of the "wedge" protein particles but has not been bound into the gel structure. The increasing proportion of gel protein found probably reflects this binding of the two lots of protein into a single structure. However, the protein structure is mechanically unstable and there is an increasing breakdown with time, so that the overall effect is that the gel content reaches a maximum and then declines.

This speculation concerning what happens during mixing of dough has been tested in a series of experiments in which pieces of dough taken from the mixer at different times were rapidly frozen and dried. A range of mixing speeds was used. In microscopical preparations the disappearance of protein from the surface of the starch granules was followed. The same amount of work was required to reach the minimum gel content in every case, so that this phase is independent of time, as would be

expected for a mechanical transfer of material. The middle phase, from minimum to maximum, was dependent on both amount of work and time, but more largely on the amount of work. The final phase also depended on both amount of work and time but was more largely time-dependent. If we assume that molecular processes are time-dependent then these additional observations support the theory. The most exact test of such a theory can be made only by physical methods such as X-ray diffraction. Such examination of experimental doughs has yet to be carried out.

It was mentioned earlier that a preparation of the gel protein showed no ordered structure, other than that of the polypeptide backbone, when examined by X-ray diffraction methods. Several workers in the past five years have examined whole gluten by this technique and have found evidence of oriented structures, but only when lipid was present in the gluten. It is suggested that leaflet structures of lipid and protein layers are formed. Certainly both glutenin and the gel protein have been shown to have affinity for lipid and it may be that lipid-protein interaction structures are of considerable importance in determining the properties of dough. In the chemistry of dough all the possible combinations of protein, lipid and carbohydrate seem to occur. A considerable part of the lipids has already been shown to contain galactose and other sugar components, whilst some protein fractions contain bound carbohydrate. The presence of carbohydrate in lipids or proteins is not remarkable, for many animal tissues contain such assemblages. The chemical nature of the linkages involved is open for speculation and investigation. Protein-lipid interactions are, like protein structure itself, a realm of chemistry where salt linkages and Van der Waals forces are at least as important as covalent linkages and crystalline structure. These interactions between the fundamental components bring problems right at the start of investigations since it is difficult to separate all the lipid of flour free of protein and similarly all the protein free of lipid. The lipid-like protein and the highly polar lipids which are known to be present must be a major cause of this difficulty.

The lipids of flour are a mixture of considerable complexity. It is estimated that about one-third is truly lipid of the endosperm cells whilst two-thirds is derived from wheat germ by migration during milling. When vitamin E was being isolated and identified, squalene and similar hydrocarbons were identified as constituents of wheat germ oil. The hydrocarbon fraction which is separable from flour oil may be of similar type. The sterol fractions have

similarly been studied in the germ oils of wheat and maize, the component sitosterols being first separated into alpha, beta and gamma fractions. The alpha fraction was further separated into alpha 1, alpha 2 and alpha 3 fractions. There has been little work on these since the last war and only *beta*-sitosterol seems well characterized and identified. So far as the methods of analysis can be relied on, there are mono-, di- and tri-glycerides present in the flour lipids, together with free fatty acids. The titratable free acid of the lipids, generally assumed to be free fatty acids, is of technological interest since there is an increase on storage of flour and the value is sometimes used as an index of deterioration. The remaining 50 to 60% of flour lipid we may refer to as "complex" lipid. Early workers have claimed the presence of choline and ethanolamine phosphatides and of phosphatidic acids, but the evidence is sometimes rather doubtful. Several groups are at present working on the structures of these complex lipids, in most cases separating them by partition chromatography or countercurrent distribution. Many peaks are found in the elution curves but few identifications have been made yet. There are many building units in each molecule, hence there is scope here for much "pure" chemistry.

The role of the native lipids and of added lipid in baking is very variable and there are many absolutely conflicting reports in the literature. It is concluded that samples of flour vary considerably in this respect and until much more is known the subject does not lead to profitable discussion.

Having looked at the three predominant classes of compound and the way they are arranged in flour, we are in a position to consider the bread baking process, the ultimate use of most wheat flour. In the presence of yeast, enzyme kinetics are extremely important and in many instances are the controlling factors in deciding the final state of the dough and bread. Yeast in dough does two things. Most obviously, it leavens the mass, creating bubbles of carbon dioxide which cause the dough to rise or "prove" and so form the open texture we associate with bread. To the baker it has another important function—it matures or ripens the dough, bringing it to the physical state best suited for the production of a well-risen loaf of good texture. A dough which is under-mature or "green" gives bread with a coarse cellular structure and poor volume, amongst other symptoms. Over-maturity is associated with uneven cells and again poor volume. There are also obvious differences in the colour and texture of the crust.

The state of maturity of the dough may be varied by altering the duration of fermentation, the proportion of yeast and its food supplies, by mechanical treatment and by treatment with oxidizing or reducing agents. Long ago it was common practice to store flour for a period of weeks or months before use, to mature it. Nowadays there is widespread use of chemical oxidation, either by treatment of the flour at the mill with a gas such as chlorine dioxide or by addition to the mix in the bakery of potassium bromate or sodium chlorite, to quote two examples. Only trace quantities of these "improvers" are used, of the order of 10 to 20 parts per million by weight.

Investigations of flour "improvement" and dough "maturity" are probably the most interesting fields of cereal chemistry being carried on today. Several theories have in the past been proposed and discarded, but it now seems clear that the major action of these oxidizing agents is on the gluten protein and in particular on the disulphide linkages which are involved in its structure. Enzymic processes have been eliminated as the major pathway of oxidation, though it is probable that there is more than one reaction mechanism. It is not clear whether oxidation of the protein occurs directly or via other smaller molecules containing SH groups. Two such compounds, glutathione and lipoic acid, have been isolated from flour and the latter observation accords with the faster or more complete reaction of the protein in the presence of the flour lipids. This could, however, be a matter of orientation. The major uncertainty is whether disulphide linkages are being formed or broken by oxidation of the protein, either being possible. Another alternative is that lability of the protein due to S-S interchange mediated by a few free S-H groups could be seriously hindered by oxidation of the S-H groups. There is a large field of work here for both the analytical protein chemist and for him who would synthesize simpler model compounds and observe their properties.

The enzyme chemistry of yeast growth and multiplication has been studied more by the brewing industries than by bakery chemists, though it is no less important to the bakery trade. The commercial production of yeast has reached the stage where a consistent product is received by the baker and there is little incentive to investigate further. Although many enzyme systems may be demonstrated in flour, only two chiefly concern us. In most flour there is present *beta*-amylase activity to assure a continuous supply of sugars for the yeast to feed on, but there must be a sufficient number of starch granules broken open in the

milling process, so that enzyme attack will be possible. As in so many phases of cereal technology, a balance is desirable. With too few broken grains there is insufficient yeast growth unless sugar is added to the dough, while with too many broken grains the water-absorbing property of the starch is unduly high. The situation is quite different when we consider *alpha*-amylase activity. Flour from sound, ripe wheat contains little of this enzyme and in some cases additions of malt are made to increase the activity, so that a desirable degree of softening of the breadcrumb may occur. In effect, *beta*-amylase nibbles at the ends of the starch molecules, so that there is little reduction in molecular size and hence change in properties. *Alpha*-amylase, however, cleaves the molecules near the middle and attacks the parts again and again, so that there is a rapid reduction in molecular size and a drastic change of properties. This process is termed dextrinization. This change is very noticeable when flour containing an excessive amount of *alpha*-amylase activity is baked into bread. The crumb is very sticky and difficult to cut cleanly or, in the extreme case, the middle of the loaf may be structureless owing to the lack of starch gel.

The formation of this starch gel is the major change during the baking of bread. In dough we have starch granules embedded in layers of hydrated protein surrounding the gas cells. When the temperature is raised the starch is gelatinized, using the water which was held by the protein. The gas cells swell until the structure "sets" due to the absorption of water by the starch. Thus bread has two complete structures, one of starch and one of protein. This is easily demonstrated by digesting with amylase, protease or both. Only when both enzymes are used does bread disintegrate. There are many other baked goods which have only protein or only starch structure. The carrying of water for the swelling of the starch in the oven is the major function of the protein in dough, and in this respect it may be replaced by other water-holding colloids. Loaves made in such a way are clearly inferior to normal bread so that the importance of the protein in forming the texture and flavour of bread is emphasized.

A discussion of breadmaking would be incomplete without mention of the "continuous processes" which are recent developments. Flour, fat and a liquor containing the other ingredients are metered continuously to a premixer and then pumped into a "developer". Here the dough is intensively mixed for a short period under pressure and then extruded straight into the baking tins, to be proved and baked in the normal way. The process

from flour to bread takes little more than an hour, compared with 5 to 6 hours for the conventional processes, and is economical in space and man-power. Many of the difficulties of putting dough through handling machines are avoided and faults in the bread may be rapidly corrected by adjustment of the controls. However, the capital cost is high. This process provides a challenge to the scientists of the industry since it appears to contravene several of the established theories of dough behaviour, and it is for that reason that it has been mentioned. We have much to learn from it.

This essay has covered in a fairly general way the problems which confront the scientist in the bakery trade, one of the older chemical industries. Naturally, more emphasis has been put on those fields in which the author has worked. The bakery trade is rapidly becoming completely mechanized so that it is now common to talk of "bread factories" or "plants" rather than bakeries. The discussion has not been entirely concerned with chemistry but it is the nature of most technologies that a number of complementary scientific disciplines are involved and that these cannot be arbitrarily separated.

The natural and fall-out induced radioactivity of wheat is currently the subject of discussion and experiment, but it seems likely that we shall continue to eat bread, baked from flour, milled from wheat, for a long time yet. It is also likely that we shall go on arguing whether white or wholemeal flour is better for us!

INSTITUTE PRIZES

At the meeting of Council on August 29 the Institute Prizes for 1961 were awarded as follows:

I.C.I. PRIZE: Awarded to DR W. A. MCGILLIVRAY, Dairy Research Institute, for outstanding research work on the biochemistry and metabolism of Vitamin A. This work was largely carried out while Dr McGillivray was on the staff of Massey College.

CHEMICAL ESSAY PRIZE: Awarded to DR P. MEREDITH of the Wheat Research Institute, Christchurch. Dr Meredith's Essay, "Flour", is published in this issue of the *Journal*.

MORCOM GREEN, EDWARDS PRIZE: Awarded to DR J. R. L. WALKER, Cawthron Institute, Nelson. This award was made for chemical studies on the nature of substances which determine the characteristic flavours of cheese. This work was largely carried out at the Dairy Research Institute.

INSTITUTE OFFICERS

The President of the Institute for the ensuing year will be DR F. B. SHORLAND, Director of the Fats Research Laboratory, Wellington. As a result of the postal ballot MR W. G. HUGHSON, Dominion Laboratory, Lower Hutt, was elected Vice-President and President-Elect.

THIRTY-FIRST ANNUAL REPORT

For the year ending July 31, 1961

OFFICERS:

The list of Officers for the year was published in February 1961 issue of the Journal, p. 19.

INTRODUCTION:

The general activities of the Institute have followed the pattern of earlier years, but two matters deserve special mention. No Conference was held during the period under review, but members were fortunate to be able to enjoy visits from an unusually large number of outstanding overseas chemists.

MEMBERSHIP:

Membership of the Institute, has, during the period covered by this report, changed as follows:

	<i>Associates</i>	<i>Fellows</i>
New members	56	6 (4 elected from
Resignations	5	- associates
Deaths	1	1

Membership figures for the last three years are as follows:

	<i>1959</i>	<i>1960</i>	<i>1961</i>
Auckland	97	105	111
Waikato	33	29	32
Manawatu	45	51	58
Wellington	179	177	182
Canterbury	68	79	81
Otago	63	61	71
Overseas	62	52	66
	<hr/> 547	<hr/> 554	<hr/> 601

COUNCIL SUB-COMMITTEES:

JOURNAL: The *Journal* is now firmly established under the Editorship of Mr N. T. Clare and a committee of the Waikato Branch. The cost of the *Journal* during the period under review changed very little from the cost in the previous 12 months.

EXAMINATIONS: The Education Department courses leading to a National Certificate in Chemistry appear to be rapidly decreasing the demand for the L.A.C. qualification and it appears that this phase of the Examinations Committee work will be coming to an end in the not too distant future. The Examinations Committee has for a number of years been drawn from members of the Otago Branch, but at the request of that Branch the Committee will be re-constituted in another centre. It seems that in the future the Committee will be concerned largely with Examinations for the Associateship, which may be expected to assume greater importance when the National Certificate is well established.

PROFESSIONAL STATUS AND SALARY COMMITTEE: No salary survey has been carried out for some time but a new survey is now being undertaken and it is expected that the results will be available before the end of 1961. The need for a new survey has been appreciated for some time but the timing for the survey is a matter of some importance if the results are to be useful for more than a short period.

OTHER SUB-COMMITTEES: The Membership Committee, the Employment Officer, the Honorary Librarian and the Institute's Representative on the Standards Council continue to play a vital part in the smooth running of the Institute. These groups carry out an increasing amount of work requiring much specialised knowledge.

INSTITUTE PRIZES:

Prizes for 1960 were awarded to the following:

I.C.I.—Professor H. Bloom.

Morcom Green, Edwards—Dr H. P. Rothbaum.

Chemical Essay—Dr J. C. Dacre.

OVERSEAS VISITORS:

This year the number of overseas chemists to visit New Zealand was unusually high. The Institute was fortunate that a number of outstanding chemists who visited Australia for the Conference on Natural Product Chemistry were able to extend their itineraries to include brief visits to this country. With the financial assistance of the D.S.I.R., the University of New Zealand, the individual University institutions and the Chemical Society tours were arranged for Professors A. W. Johnson, E. Lederer, C. Djerassi and A. J. Birch and Dr H. W. Thompson. Professor N. A. Sorenson paid a short visit to New Zealand in May, 1961.

"ROYAL" TITLE:

For a number of years there have been suggestions that it would be desirable for the Institute to seek permission to use the word "Royal" in the title. During the year this matter was raised again and after a full discussion Council decided that no action should be taken.

FINANCIAL:

The balance sheet (*see August Journal, p. 152*) shows an excess of expenditure over income of about £30, a satisfactory year's operations when the expanding range of Institute activities is considered in relation to a relatively stable and modest income from subscriptions. The auditor comments on the healthy state of the Institute's finances.

H. N. PARTON, *President.*

W. E. HARVEY, *Hon. General Secretary.*

BRANCH NEWS AND NOTES

AUCKLAND BRANCH

Mr R. S. Jebson, Branch Secretary, has transferred from Kempthorne Prosser Ltd., to Dairy Research Institute, Palmerston North.

Professor L. H. Briggs has been awarded a Commonwealth Visiting Fellowship to be taken up at Aberdeen University.

Dr D. Hall and Dr R. C. Cambie have not resigned from their positions at Auckland University, as stated in the August Branch News. Dr Hall is taking sabbatical leave in the United States. He has attended an X-ray crystallography conference at Denver, Colorado, and is now working in the Department of Chemical Crystallography at the University of Pittsburgh, Pennsylvania. Dr Cambie has been awarded a Pressed Steel Research Scholarship for doctoral research in the Dyson Perrins Laboratory at Oxford University.

Mr J. L. Hoare has been awarded a N.Z. Wool Board Research Fellowship for three years doctoral research at the University of Leeds.

Mr G. Dodson is shortly leaving to work under Mrs Hodgkin in the X-ray Crystallography Department at Oxford University.

Mr S. B. Tricklebank has left to work under Professor Bockris at the University of Pennsylvania.

Dr G. A. Wright, 1958 Rhodes Scholar, has recently returned to take up a position as lecturer. For the past year he has been working with Professor Bjerrum in Copenhagen.

Mr R. N. Seelye and Dr B. R. Davis will shortly be returning after two years' doctoral research in the Dyson Perrins Laboratory at Oxford University.

OTAGO BRANCH

Guest lecturers addressed both the June and July Branch meetings. Mr T. A. Rafter, Director of the Nuclear Science Institute, gave local members much food for thought in his address on "What we have lost and what we have gained in the nuclear age". "Recent theories on protein synthesis" was the title of a lecture from Dr J. W. Lyttleton of the Plant Chemistry Division, D.S.I.R., and this was regarded by the audience as a most interesting sequel to Sir Alexander Todd's nucleic acid discourse of last year. It is quite obvious that branch members welcome the policy of bringing more guest lecturers to address local meetings.

CANTERBURY BRANCH

Dr A. Fisher has returned to the Chemistry Department, University of Canterbury, after a year at the California Institute of Technology on a post-doctoral fellowship.

Mr D. E. Moffat has transferred to Standard Vacuum Oil Co., Wellington.

Dr J. W. Lyttleton, Plant Chemistry Division, Palmerston North, gave a stimulating address to the Branch on recent theories of protein synthesis. Dr Lyttleton's visit to the South Island was at the joint invitation of the Canterbury and Otago Branches, an arrangement which is proving very successful.

MANAWATU BRANCH

Dr E. L. Richards was in Australia for three weeks recently, representing Massey College at the Poultry Nutrition Conference in Sydney, and visiting biochemical laboratories in various parts of the country.

THE REGISTRY**Honorary Fellows**

(Elected August 29, 1961)

- ANDREW, Robert Leslie, F.R.I.C., Wellington. (Retired, formerly Director and Dominion Analyst, Dominion Laboratory.)
- MONRO, Alexander Donald, M.Sc., A.R.I.C., Paremata. (Retired, formerly Associate Professor, Chemistry Department, Victoria University of Wellington.)

Fellows

(Elected August 29, 1961)

- CLARK, Percival James Comfort, M.Sc., Dominion Laboratory, Private Bag, Petone (Dominion Analyst).
- HOLLAND, Harold Cecil, M.Sc., Ph.D., W. Sutherland & Co., Auckland (Managing Director).

Associates

(Elected August 29, 1961)

- ALET, Iiwi-Ret (Miss), M.Sc., Chemistry Department, Victoria University (Junior Lecturer).
- BRIGGS, Michael Harvey, B.Sc.(Liverpool), M.Sc.(Cornell), Biochemistry Department, Victoria University (Lecturer).
- BROWNE, Gavin Fair, M.Sc., Standard Vacuum Oil Co., Petone (Industrial Chemist).
- FRANCE, John Terence, M.Sc., Central Laboratory, Auckland Public Hospital (Biochemist).
- SEGAL, Wolfe, M.Sc., Ph.D.(Melb.), Chemistry Department, Victoria University (Lecturer).
- TURNER, Alan Arthur, B.Sc., Shell Oil (N.Z.) Ltd., Wellington (Senior Chemist).
- WILSON, Margaret Nellie, B.H.Sc., B.Sc., Home Science School, University of Otago (Assistant Lecturer).

Leave of Absence

- HALL, D. For one year.
- HOARE, J. L. For three years.
- MEREDITH, P. For one year.

Resignations

- BOON, Wanda Lois. ELPHICK, B. L. HARRIS, A. C. RUSSELL, Louise.

The address of Mr W. Boustridge was incorrectly recorded in the June Registry. Mr Boustridge is Works Chemist, British Imperial Paints, Auckland.

OBITUARY

James Murray

The University of Otago and New Zealand science have suffered a severe loss in the death in a motor accident, on June 24, of Dr James Murray. In a time of excessive specialization he was a man of unusually wide diversity of attainment. His work on the borderline of chemistry and botany would have developed fruitfully in the way which has so often occurred in the sciences. He had completed the long apprenticeship needed before work of significance to two sciences can reach the stage of publication, and his wide knowledge of chemistry combined with his enthusiasm for certain aspects of botany promised a cross fertilization from which results of great value could be expected.

He was dux of Otago Boys' High School in 1941, and won a University National Scholarship. He graduated M.Sc. with First Class Honours in Chemistry from the University of Otago in 1945 and was appointed Assistant Lecturer in Chemistry. Awarded a National Research Fellowship in 1951, Dr Murray went to Cambridge, where he gained the degree of Ph.D. in Chemistry. His principal research interest has been in the chemistry of New Zealand plant products and he has published some 15 papers on this field. From his chemical investigations of New Zealand lichens has stemmed a keen interest in their botany. In 1959 he was awarded a Nuffield Foundation Travelling Fellowship in Science, for study in London at Imperial College and the British Museum. He visited some of the great herbaria in Europe and on returning to New Zealand embarked on the preparation of a series of papers on New Zealand lichens, of which four have been published. He also began the even more important task of writing in collaboration with the British Museum a world monograph on the lichen genus *Sticta*.

At the time of his death, Dr Murray was Senior Lecturer in the Department of Chemistry. Here and in the University generally, he won the friendship of colleagues and students alike. Those research students who worked under his personal supervision came quickly to appreciate his quality, and equally quickly became his friends. He gave freely of his wide knowledge and ability to other investigators. Indeed unselfish co-operation was a key note of his character. He read easily in several languages and was deeply interested in all aspects of library work. The task of making the literature of science more accessible to research workers was close to his heart, and his work for the chemistry library was an expression of this.

Dr Murray married, in 1949, a member of the staff of the chemistry department, and there are three children. He was a man of great vitality and, despite his many activities, never lacked time to share the enthusiasms of others. The University community, and the community at large, can ill afford to lose a man of such high ability and character at the height of his creative capacity.

BOOK REVIEWS

AN INTRODUCTION TO TRANSITION METAL CHEMISTRY, LIGAND FIELD THEORY, by Leslie E. Orgel. Methuen and Co. Ltd., London. 1960. 180 pages. Price, 25s.

Ligand Field Theory suffers from the disadvantage that it has to be either understood as fully as possible to give any satisfaction or studied only in a qualitative manner. Orgel, however, has achieved a good balance of theory and general qualitative principles. For this reason the book will become very useful as a guide to teachers of Ligand Field who do not wish to go too fully into the theory.

The book covers stereochemistry, stabilities, spectra, bonding in coordination chemistry, and special types of complexes involving unsaturated hydrocarbons as ligands. Orgel shows how the theory can be applied to transition metal compounds. A large number of tables and diagrams are helpful in summarizing data and enabling one to visualize what is happening to the *d*-orbitals of the transition metals.

There are some minor faults in the book, such as its title which is partly a misnomer in that very little transition metal chemistry is discussed. The book is concerned with the application of a theory to examples where it works and for this reason one must not use the book as a substitute for a knowledge of chemical properties of the transition metals. Some of the explanations of a ligand field function, for example Δ , the ligand field energy, are scattered throughout the book. It would be useful to have had them collected together in an appendix.

—J.E.F.

THE THEORY OF TRANSITION METAL IONS, by J. S. Griffith. Cambridge University Press, England, 1961. 455 pages. Price, 95s.

This book is written primarily for the theoretical chemists, but has a great number of uses for the practical inorganic chemist. Griffith describes in detail the theory of the Ligand Field and its application to the transition metals. He devotes the first six chapters to the mathematical techniques required for the theory and excepting for a reference to certain aspects of quantum mechanics the book is self contained. The last six chapters deal with the applications of the theory to the properties of transition metal ions, such as paramagnetism, spectra, paramagnetic resonance, and electron pairing, followed by a very useful appendix of tables of mathematical functions which are used throughout the book.

The first chapter is well worth reading by itself as a clear, concise definition of the metals and their properties.

Griffith approaches the subject by first discussing the theory of the structure of an atom and free ion and then showing how this theory can be extended to the structures of complex ions as formed in transition metal compounds. This approach helps to point out that there is really little difference excepting that the *d*-orbitals in a free ion break up in a complex into different sets of orbitals which then can be treated just as ordinary orbitals.

The author's use of Dirac's *ket* and *bra* notation in his mathematical section in connection with vectors in quantum mechanics could give some difficulty to those wishing to study the book as it is first necessary to

learn this little-known notation. However, once that is mastered the book, together with its complementary volume by Orgel, reviewed above, should prove a great contribution to understanding of the fast developing field of transition metal chemistry.

—J.E.F.

INDEX DES HUILES SULFONEES ET DETERGENTS MODERNES, (Index of sulphonated oils and modern detergents), Volume III. By the late J.-P. Sisley. 560 pages. 1961. Paris: Editions Teintex. Price, 80 New francs.

Today the chemist is faced with an ever increasing number of products known by trade names and any works which light the way through this confusion can be very useful. Therefore the three volumes under the above title are assured of a welcome. They deal with the nature, manufacture and trade-names of surface active products of all countries. The third volume (possibly the last as Dr Sisley died before its publication) begins with the formulation of products by the mixing of individual surfactants and "building" them with mineral salts, fatty acids, talc, starch, etc., into commercial products for various purposes including toilet and beauty aids. The manufacture of surface active agents is then discussed in a well documented review covering 210 pages. Finally there is a list of about 2,500 commercial products with notes on their composition, physical properties, type, manufacturer and application. Other lists classify surfactants by type and manufacturer.

It should be emphasized that the information given is largely complementary to that in Volumes I and II so that the three together form what is, in the reviewer's opinion, the most valuable work in print on surface-active agents and detergents.

—S.G.B.

CAHIERS DE SYNTHESE ORGANIQUE. Methods and Tables of Application. By Jean Mathieu, Andre Allais, and Jacques Valls. Volume VI, 418 pages, 1960, 110 New francs. Volume VII, 310 pages, 1961, 100 New francs. Volume VIII, 234 pages, 1961, 75 New francs. (10 francs extra for cloth binding). Paris: Masson et Cie., 120 Boulevard Saint-Germain.

These volumes continue a series which has been reviewed before in this journal. The whole domain of organic synthesis has been divided up according to type of reaction, e.g., Volume 6 deals with transposition of carbon atoms under four subheadings:

- (1) Structural rearrangements of carbon skeletons.
- (2) Migration of functional groups from one carbon to another.
- (3) Migration from carbon to a hetero-atom.
- (4) Migration from one hetero-atom to another hetero-atom.

After a general discussion of the reaction and its mechanism, particular cases are discussed in detail with full references. Volume 7 discusses intramolecular ring closures, and Volume 8 intermolecular ring closures. These volumes can be highly recommended as a useful addition to the literature of organic chemistry. French chemical literature deserves a much wider circulation in New Zealand than it enjoys at present.

—S.G.B.

GAS CHROMATOGRAPHY. Edited by Henry J. Noebels, R. F. Wall and Nathaniel Brenner. The Academic Press Inc., New York and London, 1961. 463 pages. Price, 115s.

The volume contains the proceedings of the 2nd International Symposium of Gas Chromatography held at Michigan State University under the auspices of the Instrument Society of America.

In 1952 James and Martin described for the first time equipment using gas chromatography which they used for the separation and analyses of mixtures of fatty acids and amines. The amounts of these constituents were determined by automatic titration in association with a recording potentiometer. Although gas chromatography techniques have not changed in principle since 1952, the scope has been greatly enlarged and attention has been paid to the many factors involved. Factors affecting the degree of separation of constituents have been studied and the conditions for operating at maximum efficiency have been more closely defined. Detectors of hitherto unbelievable sensitivity have been developed. As a method for the detection, identification and quantitative estimation of constituents, gas chromatography has proved invaluable and its use has been extended to all compounds that can be volatilized to some extent in a stream of inert gas. In more recent times the utility of gas chromatography has been enhanced further by subjecting substances as they emerge to mass spectrography, infra-red and ultra-violet spectrography or to some chemical tests to verify the nature of the constituent as it emerges. Gas chromatography has also been worked in association with counting equipment to determine more or less automatically the amount and activity of the individual components in mixtures of radio-active substances. The advances mentioned above are well covered in this volume.

As indicated by 1975 references listed, it is no longer feasible for even the specialist to keep pace with the vast output of literature in this field. This bound volume of 34 papers, therefore, serves a very useful purpose in providing the chemist with a bird's eye view of this rapidly advancing phase of chemistry. The book is well organized and well printed. The papers indicate that much of the present day development in gas chromatography is coming from chemists in industrial firms, particularly those firms which are manufacturing gas chromatography equipment. Of special merit is Dr James's paper dealing with "Development of an Idea". This gives the reader an account of the period preceding the publication dealing with the first gas liquid chromatography apparatus. Its main use, however, is the insight which it gives into Martin's genius and method of approach in developing gas chromatography. The experiments which he carried out were preceded by a great deal of thinking so that all the useless experiments were discarded and only the critical experiment carried out. One cannot help feeling that the development of gas chromatography might have been long delayed if it were not for the genius of this man.

—F.B.S.

KINETICS AND MECHANISM, by A. A. Frost and R. G. Pearson. Wiley, 1961. Price, 11 dollars.

This is a second edition of a book published in 1953, and, although the reviewer has not seen the first edition he can unhesitatingly recommend this revision. The book is to be praised for dealing with the relationship between kinetics and mechanism. The best known books on mechanism use kinetic data wherever helpful, but seldom show the limitations of the technique, and conversely many texts on kinetics are more concerned with deductions of formulae than with the application of these to real data.

Frost and Pearson are clear in their aims. Kinetic information is frequently imperfect, it needs to be rationalized, and interpreted, and also supplemented by as much other information as can possibly be applied to the problem. A mechanism is a mental model devised to explain facts—facts not only about rate processes, but also about the overall stereochemistry, and the nature and lifetimes of intermediates in a reaction.

The book has four hundred pages, of which fifty are devoted to the treatment of data, fifty to the theory of gas reactions (including collision and transition-state theory), one hundred to reactions in solution, and one hundred to the discussion of the mechanism of specific reactions. There are also sections on rapid reactions, and on chain reactions.

Throughout the book the emphasis is on the application of theory to experimental results, and it is quite exciting to see this process so frequently applied to results of work done in the last ten years. The usual gap between the textbooks and the original literature has been considerably narrowed here by the incorporation into the text of much relevant literature of 1959 and 1960.

The selection of reactions for detailed consideration is good, and the authors have managed to avoid the well-trodden paths without losing balance of treatment. Some of the reactions discussed are: nucleophilic substitution in alkyl halides (briefly); hydrolysis of ethylene oxide and related compounds—a good section since the contentious topic of neighbouring group effects is aired; hydrolysis of lactones and esters—the H_0 function; aldol condensation; and the decomposition of ditertiary butyl peroxide.

The book is well produced and printed, and only a few minor errors were found. It can be recommended as a good source of material information about kinetics and the mechanisms of organic reactions.

—B.E.S.

BOOKS RECEIVED

BIOCHEMICAL PREPARATIONS, Vol. 8, edited by Alton Meister. Wiley, New York, 1961. 146 pages. Price, 6.25 dollars.

The latest volume in this series of preparations of compounds of biochemical importance is nearly fifty per cent. larger than most of the earlier volumes. In addition to 34 prescriptions (each independently checked and with footnotes incorporating the checker's experience), this volume contains an index for volumes 1 to 8 inclusive—about 1,000 entries covering 180 individual preparations. There is also a list of 86 "compounds of biochemical interest" which have appeared in the 40 volumes of *Organic Syntheses*.

**WOOL RESEARCH ORGANISATION OF
NEW ZEALAND
(Incorporated)**

•
CHEMIST

The Wool Research Organisation of N.Z. (Inc.) wishes to engage a well qualified chemist, with an interest in microbiology and enzyme chemistry, for research relating to the production of slipe wool.

This is an opportunity to join a large new research organisation and to study wool, particularly in relation to skin structure and composition, depilatory substances and enzyme de-woolting.

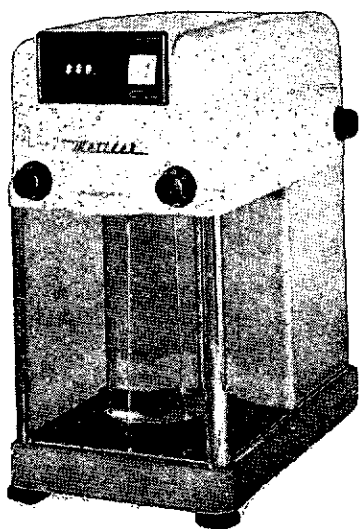
Initially, these investigations will be carried out in the laboratories of another research institution at Wellington. In about four years' time the appointee will be transferred to the Organisation's laboratories which are to be built at Lincoln, near Christchurch.

Salary will be, according to qualifications and experience, up to £2,300 a year. Superannuation is available. Financial assistance will be provided towards removal expenses involved in taking up appointment and subsequent transfer to Lincoln.

Applications stating details of age, qualifications and experience should be addressed to:

**The Secretary,
Wool Research Organisation of N.Z. (Inc.),
P.O. Box 5115,
WELLINGTON.**

Applications close 15th November, 1961.



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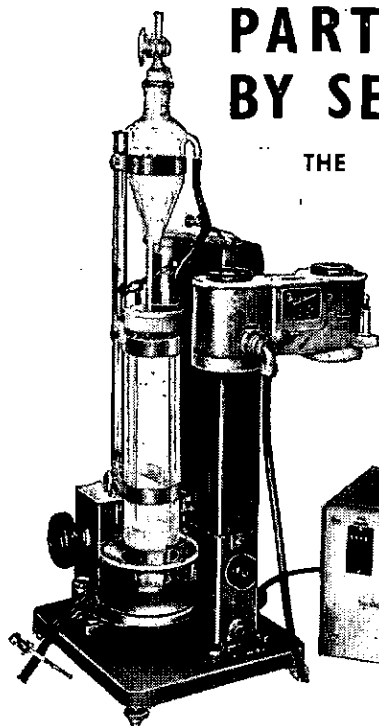
PARTICLE SIZING BY SEDIMENTATION

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- ★ Saves time Duration of test: 6 hours for full range 5μ to 7μ .
- ★ Saves labour Recording equipment: releases operator for other work.
- ★ Easy operation.
- ★ Simple maintenance.
- ★ Balance sensitive yet robust: readings by an integral optical system.

British Patent No. 712,434 and U.S. Patent Application No. 320,996.

NOTE—This apparatus is under consideration by the BSI for possible adoption as British Standard.

For particle size determination within the range 5μ to 7μ . If, however, the largest size is not greater than, say, 20μ , the lower limit can be extended to about 2μ provided the temperature is controlled within fairly narrow limits and agglomeration or flocculation does not take place.

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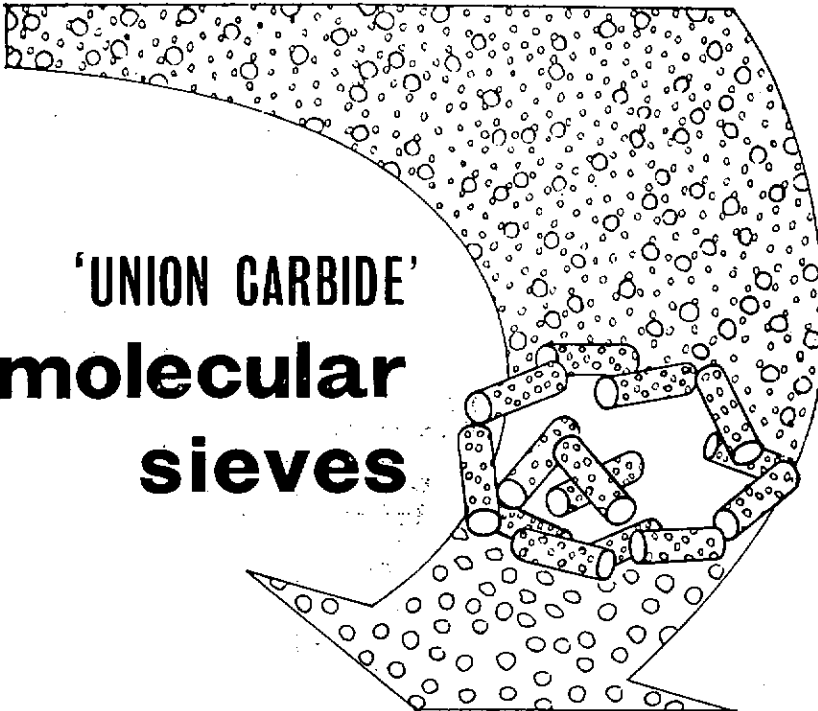
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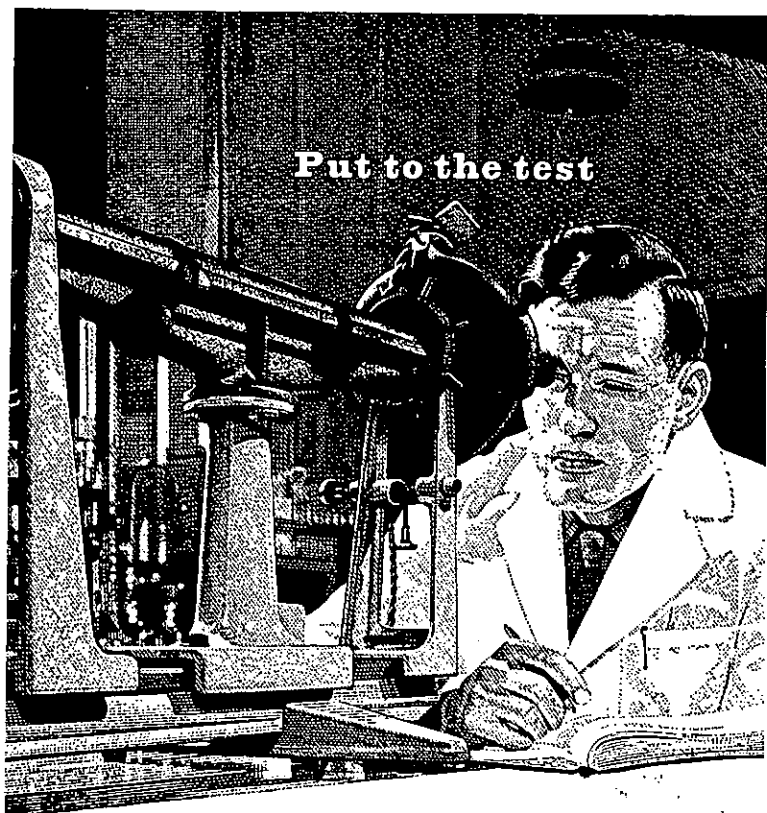


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