JOURNAL OF THE NEW ZEALAND INSTITUTE OF CHEMISTRY

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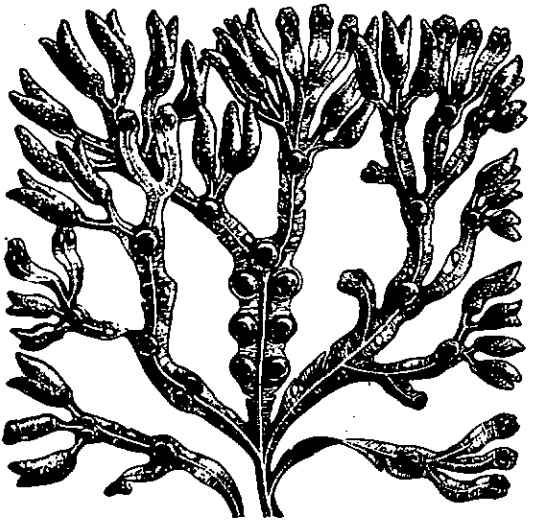
IODINE



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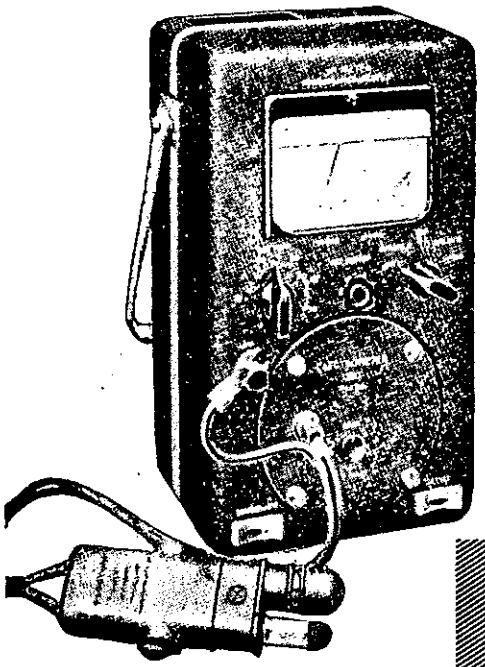

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THE SEARCH FOR TRUTH

Presidential Address Delivered by Prof. S. N. Slater at the Annual Conference, Wellington, August, 1952.

Speaking as President, I find the Annual Conference of the Institute of Chemistry a time of mixed pleasure and worry. It is in particular, as we all find, a time of renewing and extending acquaintance and in this country where our chemical population is so thinly dispersed this is one of the most important sides to the Conference. At the same time, it requires something of the mental and physical agility of a quick-change theatre artist. At one moment there is the ordeal of this address, with one's fellow chemists and their wives sitting in decently silent judgment, at another there is the Annual General Meeting, where one can be certain that judgment will be not so silent, and at still another one may be giving a formal scientific paper. This same schizophrenia that you require of the President is, I always feel, characteristic of the Institute itself. We are virtually a chemical society—a society of chemistry and industry, and an Institute of Chemistry, viewed as a professional body. I am reminded of that very old story about a visitor to an asylum who was talking to a patient and finding it difficult to credit that the man was insane until, on something being said, he adopted a most offended attitude and reminded the visitor, with some dignity, that he was talking to the Czar of Russia. Some time later the same visitor was again being taken round, and on seeing the patient, said to the attendant: "Do you mind if I talk to my old friend the Czar of Russia?" Again he found the man quite lucid but, as before, he took offence at some remark and reminded the visitor that he was speaking to the Emperor of China. The visitor ventured to remind the patient that on the previous occasion he was the

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Czar of Russia. "Oh, yes," said the patient, "but you must remember that was by another mother." This morning I was the Czar of Russia; tonight, in providing what I am told must be the hors d'oeuvre to the main dish, I appear before you as the Emperor of China. Inasmuch as both these somewhat legendary figures are personna non grata in their own countries, I must thank you for the asylum you have given me here.

I hope the slightly pretentious title of this address has not led you to expect or fear a deeply conceived and authoritative philosophical statement, the sort of thing which comes most fittingly from an elder statesman. I cannot claim age or statesmanship, and I chose the subject because it allows me to say practically anything I please about any of the hundred and one topics that might prove suitable for a Presidential Address.

As one whose prime concern is university education I might be expected to discuss the university's chief task, the cultivation of knowledge, whether it be the knowledge of good and evil, the knowledge of the thought and culture of another race, or the knowledge of the factors governing the periodicity in properties of the chemical elements. Our Institute is indeed becoming increasingly conscious of the problems involved in the education of the chemist, and a glance at the current issue of the Journal is sufficient to emphasise this point. We are also, through force of circumstance, considering the related problem of the examination of the chemist, and I hope it will not be long before we have formulated a clear statement on this matter. Again there is much to fascinate in all the many aspects of the active search for truth, where one may see the beautiful development of some unifying theory, the craftsmanlike dissection of a puzzling molecule, or even the interplay of personality and circumstance that has invested so many discoveries with warmly human biographical material. There is need, too, for the chemist to speak at times in terms of pounds, shillings and pence and of brick and mortar to bring home to the responsible administrators that the search for truth, whether it be the butterfat content of a sample of milk or the chemical structure of a new antibiotic, is a costly search and that the searchers are not machines, but men and women, often highly strung, whose creative ability is at least as worthy of an inspiring architectural environment as the activity of a factory worker. And in this troubled world should not a word be said about the claims of planned research and the political responsibility of the scientist? I have already cited enough topics for half a dozen Presidential Addresses and the social hour is fast receding from its position in

the comfortable middle distance to one on the horizon which we are likely to approach by a regrettably asymptotic route.

I shall start, then, by saying something about the technical education of the chemist. We have all been through the mill and the process must interest every chemist in prospect or retrospect. A simple problem to formulate is the length of the formal training a chemist should receive, and if we think in terms of the customary university course we must consider the Bachelor, the Master and the Doctor. Speaking in very general terms, I am still convinced that three, four and six years are reasonable times to spend on these courses, viewed as science courses. If it is argued that the content of the subject is increasing and that the course should be lengthened, I would reply 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, and that the problem of selecting what to leave out in, say, the Bachelor's course, is not greatly simplified by extending it one year. We ought still be able to deal with the real fundamentals of our subject in three years and give the student a good training in laboratory work. The one thing I believe to be absolutely essential in such a three-year course is that the third year should be spent almost entirely in the chemistry department. The student should come into the laboratory at 9 and stay there until 5 with breaks for lectures and necessary reading, and perhaps a little time in another department where he may be studying some ancillary subject. Just what we are to teach in the chemistry department during these years is a matter for constant revision. It is sometimes said that the young graduate is not as useful as might be hoped, particularly in industry, and I would agree that he is unlikely to be a good leather or freezing works chemist, or a good specialised analyst, or to be possessed of cost accounting ability.

But if the University has done its job properly he should be a reasonably intelligent embryo *chemist* and if industry does *its* job properly he should be a good leather or freezing works chemist, or a good specialised analyst in a few years' time. On the other hand, if the University has failed in its job it is unlikely that he will ever be more than a skilled technician. I am hardly in a position to say, in the words of the announcement displayed in a French hotel, "The wines of this establishment leave the traveller nothing to hope for," but I do feel in a position to say that the University can and must concern itself almost exclusively with the fundamentals of any subject it teaches. If I may quote a statement made by Professor Ingold during his recent visit to this country, we must aim at turning out graduates who can read a

statement in a book and say "That cannot be true." This is the philosophy which must guide our teaching no matter what the demands of expediency may be.

The question of altering the degree structure in such a way as to include cultural and other non-science subjects is another matter, and if a student is willing to extend his normal B.Sc. course to four years by taking such subjects, I am convinced that he will benefit greatly by it. In fact, I think on the whole I should be *more* in favour of a compulsory extension of the course in this direction than of increasing the amount of chemistry. My own feeling, however, is that neither extension should be compulsory. I believe that the newly-matriculated student, after five years at a secondary school, should already possess a sound educational background. He should have, in particular, a good command of his mother tongue, a working knowledge of another modern language, I think probably as much mathematics as his natural bent will allow him to absorb comfortably, and an awareness of the added zest for living that comes from constant refreshment at the many springs of culture. This should make him conscious in both his university years and in later life of the need to balance his professional development with that steady enlargement of his mental horizon which will ultimately stamp him as a man of general education and culture. I have said nothing of the place of science in his schooling. This need not loom over-largely. But for any student taking a university course requiring chemistry at any stage, I think this is the one science he should be reasonably grounded in. I say this because from personal observation I believe it is easier for the student to walk into, say, a Physics laboratory, knowing no physics, and grapple with that subject than it is for a student with no previous experience to start on a university course in chemistry. Such students are often completely lost and find the struggle very hard indeed. But although I look for a *grounding* in Chemistry, an advanced knowledge is not necessary. At all costs let the newly-matriculated student be already a well-educated person. If he is not, then we should look to our schools and ask where the trouble lies. Here, unfortunately, it is only too easy to see an unhealthy situation. Speaking for my own Department, it is rare for more than one graduate in Chemistry each year to be seeking a position in the schools. This year, in spite of a particularly large Honours class, I know of none. Unless teaching can be made more attractive as a profession, I see no hope of improvement in the general education of the students entering the University and ultimately our own profession. My observations as teacher and examiner lead me to believe that we cannot regard the present position as

satisfactory. The reason, of course, may be that we do not select our students on a sufficiently exclusive basis, and that this is the price we pay for making university education so readily available. Nevertheless, the general standard of intelligence amongst the students we teach seems to me to be good, and I therefore find myself forced to the conclusion that it is the training in the schools, and I might add, the home, which we must try to improve. The responsibility of providing a broad cultural background must be shared equally by the teacher and the parent.

Turning now to the Master's degree, again I prefer to tackle this course in one year. In that time an average student can be given sufficient additional theory and practical experience to make him a most useful embryo chemist. A final two years for a Ph.D., if he goes that far, makes a six-year course which I believe to be long enough. One point I have not seen raised yet is whether the professional rewards are sufficient to justify us in expecting a student to spend more time on costly university study. I do not believe they are, and it is equally clear that other professions, more attractive in this sense, are still recruiting a high proportion of our best students from the first-year classes in Chemistry. Until employers are willing to offer much better ultimate salaries to their chemists, I doubt whether we shall attract a sufficient number of first-rate people into the profession, and having attracted them, keep them in the country. There is another aspect to this problem which we must face honestly. Have we as a profession always made it abundantly clear that the chemist is a member of a highly skilled professional group, able to speak and write with clear and forceful authority in his own field, and on other matters, by his demeanour and aspect, to be respected and listened to when he offers an opinion? Has his organisation of his laboratory, his house-keeping within it, his conduct towards other members of the staff of the laboratory, office, and factory given the impression that he is a key man, to whom it is reasonable to look for advice on vital matters, not necessarily concerned with pure chemistry? These are problems we cannot escape, and unless our professional face-value is transparently clear, we must look to ourselves before complaining of inadequate recognition.

To sum up, I should say that we must look to the school for a liberal education, to the university for a balanced training in the fundamentals of scientific work, and to the members of the profession for a clear indication, by precept and practice, of the responsible position the chemist should hold not only in his immediate sphere of employment, but in the community at large.

I spoke earlier of the active search for truth, or research if

you like, and I have heard it said that with our Honours students we create too much interest in their thesis work with the result that every graduate is burning with the idea that the only possible career is one of research in pure chemistry. I think there may be a grain of truth in this. The justification, if such be needed, is that we believe our Honours graduates, trained in this way, have at least grappled with a problem and in doing so learned to do things for themselves. These, too, are precious years in the student life, and if we have stressed in this way the intellectual excitement and satisfaction that can come from a personal search for truth who will say, with all the uncertainty we face to-day, that the balance is faulty? The corrective may be a conscious attempt on the part of us all to destroy two widely held beliefs which I consider to be quite unjustifiable. The first is that there is some mysterious wall between pure and applied chemistry, on one side of which the worker may spend his days in sunny delight while on the other he must labour in the shadow, that the pursuit of pure chemistry (whatever that may be) is in itself an intellectual activity on a higher plane than that of applied chemistry and that the corresponding intellectual satisfaction is therefore so much the deeper on the one side than on the other. The truth is that in any chemical work there are problems to be formulated and solutions to be found. It is in the recognition of the actual existence of these problems, their clear definition, and the methods of tackling them that we can most clearly show our mettle. Rather than divide chemistry into Pure and Applied, I would divide chemists into fundamental and superficial, according to the approach they make to their daily problems. I suspect that whatever his field of employment the fundamental chemist will achieve satisfaction in his work and probably make lasting contributions to his subject. I like to think in terms of specific examples, and will mention two which seem to bring out these points clearly. The first will probably be familiar to you from the screening of that excellent film which tells the story of the discovery of the dyestuff Monastral Blue. You will remember that the whole intensely interesting group of phthalocyanine dyes, with all their intrinsic theoretical importance and practical application, was discovered through the careful study of a stain which appeared in certain batch preparations of a colourless organic background. If the chemist responsible had been a superficial worker he might have discovered that the stains were due to a fault in the glazing of the vessel in which the reaction took place, he might very properly have scrapped the faulty vessel, installed a new one, and gone on his unimaginative way in the knowledge that the quality of his employer's product had been safeguarded, this being the substance of the obligation he assumed.

in accepting his employment. At next week's meeting of his section of the Institute he would probably bump into the University staff during supper and envy them the interest of their ivory tower investigations. It is the fear of leading this kind of life which causes the recent graduate to prefer what he calls "pure research." But in the particular example I have chosen, you know the sequel. The chemist was a fundamental worker, a man with sufficient imagination to ask *why* the stain had appeared, and *what* it consisted of and in the answering of those two fundamental questions opened up a new field of chemistry. So much for routine works life. The second example I have chosen may not be quite so familiar. It is concerned with the sort of problem that is given the chemist already formulated. He is asked to discover a substance which will have certain properties, or behave in a certain way under certain conditions. The resulting search may be a series of dreary *ad hoc* attempts or a blazing intellectual quest. The choice is open to the chemist. The adventurous approach is well illustrated by the discovery of the freon type of liquid, used in refrigerators and for other purposes. May I give you a brief account of it, largely in Thomas Midgley's own words:

"We were looking for a compound, or group of compounds, in which would be combined certain well-known properties: b.p. between 0 and 40 degrees Centigrade, stable, non-toxic, and non-inflammable. This was accomplished in three days. As nearly as I can remember, the events occurred as follows: I was in the laboratory one morning and called Mr. Kettering, in Detroit, about something of minor importance. After we had finished discussing whatever I had called him about, he said, 'Midge, I was talking with Lester Keilholtz the other night, and we came to the conclusion that the refrigeration industry needs a new refrigerant if they ever expect to get anywhere. See him and talk it over.' Mr. Keilholtz was chief engineer of Frigidaire at that time. On leaving him, I expressed myself as very doubtful that we would be able to find a single substance suited to the task. In this frame of mind I returned to our laboratory, where I found A. L. Henne and Robert MacNary waiting to have lunch with me. We discussed the problem during lunch and I have Henne's word that my scepticism of solving the problem with a single compound intrigued his interest so much that he gave up his afternoon off and he, MacNary, and I went to the library and started work. International Critical Tables gave us a partial summary of the volatile organic compounds. The now proven mistake that carbon tetrafluoride boils at —15 degrees Centigrade struck us in the face and started us thinking about fluoride. Recognising that the International Critical Table list was very incomplete, I decided to

bring into play the periodic table. Perhaps volatility could be related to it in some way. It takes but a fraction of a second to see that it is. In fact, the only elements which need be considered are those shown in the figure. Every refrigerant ever used has been made from combinations of these elements. Inflammability decreases from left to right. Toxicity (in general) decreased from bottom to top. These two desiderata focus on fluorine. It was an exciting deduction. Seemingly no one previously had ever considered it possible that fluorine might be non-toxic in some of its compounds. Perhaps it could be—who knew? If the problem before us were solvable by the use of a single compound then that compound would most certainly contain fluorine. Plottings of boiling points, hunting for data, corrections, slide rules, log paper, eraser dirt, pencil shavings, and all the rest of the paraphernalia that take the place of tea leaves and crystal spheres in the life of the scientific clairvoyant, were brought into play."

It was in this way, as the result of an inspired analysis of the evidence, that within three days of the problem being set the answer was found and the organic fluorides firmly established as the perfect refrigerants. I am sorry that time is too short to give an account of another of Midgley's great discoveries, that of tetraethyl lead as an anti-knock additive to petrol. This discovery also stemmed from brilliant exploitation of the periodic table.

My thesis, then, is that whatever his formal occupation, the chemist must look first to himself for the intellectual rewards of his labours.

The second misconception I referred to earlier is that analytical work is the least interesting and rewarding side of a chemist's life. I would merely remind you that at every stage in the history of chemistry progress has gone hand in hand with the refinement of analytical technique. One has only to think of the effect of the introduction of the balance, with Lavoisier as its apostle, of the advances in organic chemistry that followed Liebig's perfection of his method of determining carbon and hydrogen, to realise what analysis meant to classical chemistry. And in modern times think what a different picture chemistry would present if it had not been for Pregl's contribution to micro analysis or the development of all the different aspects of chromatography and adsorption analysis. Truly, the analyst is the master and architect of the science and not its slave. It is worth remembering, too, that many of the most striking advances in analytical technique have not required elaborate apparatus or methods. It required only the lifting of a thermometer from the liquid to the vapour to

revolutionise distillation, and the utmost refinement of separation of such difficult mixtures as the amino acids resulting from the hydrolysis of proteins was achieved through the correct juxtaposition of a drain pipe, a trough, a sheet of filter paper, some fluid and a cover glass. Think too, of the sheer beauty of simplicity of form and conception of the Conway diffusion cell. If any of you are unfamiliar with its use and with Conway's book on the subject, I suggest there is a gap in your armoury which can be filled with pleasure and profit.

I have several times mentioned the relationship of the chemist to his work, wherein must lie the personal satisfaction or otherwise of his professional life. This link becomes of the greatest importance to the research worker, who can in fact become almost obsessed with the problem in hand. I have the feeling that many writers on the philosophy and methodology of science are not qualified by personal experience to write as authoritatively as they seem to do on this subject. They seem to me too often to tell us how theories and discoveries *should* be made rather than how they *are* made. According to these writers the scientist is a man completely devoid of partisan feeling, who can examine his own theory as dispassionately and cold-bloodedly as that of anyone else. This may be true in rare cases. I suppose Cavendish was a man of this type. But in general many of the outstanding discoveries in chemistry, say, have only been made by men who have passionately identified themselves with their work and their theories. This feeling of possessiveness, if I may call it so, has in turn provided the mainspring that has driven them forward. To take only two examples, think of the frame of mind in which Pasteur took up the study of optical activity, leading to the striking theoretical and practical developments with which his name is associated. He himself has told us that it arose from his *preconceived idea* that optical activity and asymmetry of crystalline form must inevitably go hand in hand. Again think of W. H. Perkin Junior's discovery of the small carbon rings. The investigation was undertaken against all the advice of the senior chemists he consulted. One sees here the initial stages of personal identification with a theory. Then we have the first apparently successful synthesis of the *cyclo*butane system, followed by a classical series of researches in which the other *cyclo*paraffin rings were undoubtedly synthesised. But if we look more carefully at the first *cyclo*butane synthesis, as Perkin himself did some years later, we see that the assumption of the presence of the *cyclo*butane ring was not warranted. It was in fact made against the chemical evidence. No *cyclo*butane ring had been formed. It existed only in the ferment of ideas that struggled

for practical realisation in Perkin's mind. In a very real sense I think Pasteur and Perkin were more concerned with confirming the predictions of their theories than with establishing their validity. It is also a curious and interesting fact that an hypothesis is sometimes very fruitful without being correct. A particularly good example is Ehrlich's theory of drug action, which led him to such successes as the discovery of salvarsan.

There does not seem to me to be anything reprehensible in this personal identification of the worker with his theory. What *is* important is the detailed publication of the work and its free criticism by other workers. Theirs seems to be the chief responsibility for attacking the theory, and the author will inevitably defend it. Somewhere between attack and defence we may glimpse the truth. There is no doubt that this process of attack and defence has gone on, sometimes with attendant bitterness and even tragedy. Can you not still hear the laughter that greeted Newlands when he proposed his Law of Octaves—the first prophetic statement of the Periodic Law—and the biting sarcasm of Kolbe as he castigated the unknown young man who first proposed the tetrahedral theory of the carbon atom:—

"It is indicative of the present day, in which critics are few and hated, that two practically unknown chemists, one from a veterinary school and the other from an agricultural institute, judge with such assurance the most important problems of chemistry, which may well never be solved—in particular the spatial arrangement of the atoms—and undertake their answer with such courage as to astonish the real scientists.

"Anyone to whom this concern seems exaggerated may read, if he is able, the book by Messrs. van't Hoff and Hermann on *The Arrangement of Atoms in Space,* which has recently appeared and which overflows with fantastic foolishness. A Dr. J. H. van't Hoff, of the Veterinary School at Utrecht, has no liking, it seems, for exact chemical investigation. He has considered it more convenient to mount Pegasus (apparently borowed from the Veterinary School) and to proclaim in his *La chemie dans l'espace* how the atoms appear to him to be arranged in space, when he is on the chemical Mount Parnassus which he has reached by bold flight. The prosaic chemical world has little liking for these hallucinations. It is not possible to criticise this work even half-way thoroughly because the play of phantasy therein dispenses completely and entirely with factual basis and is absolutely unintelligible to the sober scientist."

Today, although criticism is as vigorous as ever, it is usually restrained in expression. On the other hand we have another sort of personal attack to face. In the western democratic world we are

no longer always completely free to speak and write of our work, and in the laboratories of Soviet Russia the same restriction has been placed on freedom of scientific belief as has already been placed on freedom of political belief. As many of you will have read in recent months those theories of organic chemistry which are to be adhered to have been publicly defined. Other theories, of Western origin, are forbidden on ideological grounds. Such is the world we live in to-day.

Turning now to the planning of research, I find myself taking up a rather neutral position. All research must be planned at some level, and there is room for every degree of planning. It is clear that the Director of a tea research institute must direct much of the activity of his staff to attempting to improve the quality and quantity of the crop, and to searching for the cause and cure of diseases of the plant. It may be that an analysis of such work will show that it is unlikely to result in fundamental discoveries, but we must remember again that much depends on the man. It is sufficient to cite the wonderful train of theoretical discoveries that followed Pasteur in his long series of planned researches into the practical problems of the silk, wine, and other national industries of France. On the other hand I think there is a type of discovery which is most unlikely to come from any but the most untrammelled individualistic type of research, directed merely to the satisfaction of the personal interests and curiosity of the worker himself, aroused perhaps by some chance observation. The type of discovery I have in mind is that which, in the existing state of knowledge, cannot be visualised or predicted. In a self-contained community of blind men sight is meaningless and could not be predicted and I find it difficult to imagine such a community planning research leading to its discovery or an investigation into the phenomena of light. In the same way every generation lives in an intellectual sphere which is illuminated only as far as contemporary knowledge throws its light. In the darkness beyond must lie much which is unimagined. I suggest that the idea of two separate pieces of the same metal at a distance being non-explosive, either spontaneously or by detonation, but placed together exploding in a holocaust of unimagined intensity could not possibly have occurred to the most speculative thinker of a few years ago. Its discovery could hardly have been planned. The prediction of its possible experimental realisation resulted only from the free play of scientific curiosity within the mysterious depths of the atomic nucleus. It is in this sense, I think, that we must in general look to unplanned research for the greatest of discoveries, those which burst through the barriers of contemporary thought and practice.

IS SCIENCE A SACRED COW?

(Based on recollections of a talk given at a Student Congress, January, 1952.)

H. N. PARTON

(Associate Professor of Chemistry, Canterbury University College.)

"When a white-robed scientist, momentarily looking away from his microscope or his cyclotron, makes some pronouncement for the general public, he may not be understood, but at least he is certain to be believed. . . . Scientists are exalted beings who stand at the very topmost pinnacle of popular prestige, for they have the monopoly of the formula 'It has been scientifically proved . . .' which appears to rule out all possibility of disagreement. Thus the world is divided into Scientists, who practise the art of infallibility, and non-Scientists, sometimes contemptously called 'laymen', who are taken in by it."

In these words Anthony Standen, sometime Oxford physical chemist and now engaged in the preparation of a chemical encyclopaedia in the United States, threw down the gauntlet to his fellow scientists and opened the book (1) from which the title of this essay is taken. I think he has something to say which needed to be said. If his words are often a little exaggerated and his tone rather shrill, he may be forgiven because he writes as an angry man—and he is angry about claims that are often made for something called Science which make others of us, at least uncomfortable. That science has great, and deserved prestige today is not to be doubted, but it is surely a matter of concern to scientific workers that the prestige is soundly based, on real achievement and not on a pretence of infallibility. There is abroad a cult which may be called "Scientism", and statements of its devotees bring inevitably to mind the Pharisee who "stood and prayed thus with himself, 'God I thank Thee that I am not as other men are . . .'" (2) The word Pharisee has come into common use to describe a self-righteous man, and the question "Who are the modern Pharisees?" will serve to bring to focus certain tendencies in popular statements and writings of some scientific men, or more generally, of the devotees of "scientism," the worshippers of the Sacred Cow. A comfortable feeling of moral superiority is probably the worst state of mind into which anyone can sink. Critics by no means unfriendly to science have noted the appearance of just that feeling. "Having stepped into Greek's vacated place," writes Jacques Barzun. (3) "Science now occupies its position, not with

respect to size of enrolment, but with respect to educational attitude. It is now in power, and it acts disdainful, holier-than-thou, prudish. Someone once asked 'What is it that our men of science are guarding like a threatened virginity?' 'Oh,' was the reply, 'they have a Vestal interest in their subject'."

It seems worth while to examine some of the claims which have been made for Science, partly to see if they are justified and partly to remind ourselves that we are indeed, as other men are, ignorant, prejudiced, intolerant and often over-confident. These are in fact the characteristics which we would wish to deny about ourselves, for the claim is made that those who have been trained and educated in science are particularly notable for intellectual honesty, objectivity, refusal to accept authority unquestioningly, tolerance, suspension of judgment in the absence of established facts, and devotion to truth. All of us would like to think the claim is valid. Honesty compels us to admit that it has little basis.

"The scientist," says Stewart Cole (4), "is a man of integrity and faith who trusts the basic laws of nature and intelligence to lead him into the paths of truth. His loyalty to truth is unquestioned; . . . his devotion to the scientific method is unwavering; his objective is the welfare of mankind. . . ." Professor Hendren of the University of Georgia (5) tells us that "the success of the scientific method . . . is based entirely upon an absolute honesty of mind and love of truth."

So it seems that it is the laws of nature which lead us into the paths of truth, and not we who discover the laws, or perhaps invent them, or possibly even impose them on nature. Our (collective) loyalty to truth is unquestioned—by us! It is with some relief from this surfeit of self-flattery that I note that our colleagues the historians are also tinged with Pharisaism, since G. J. Renier has lately claimed that "intellectual honesty is even more important for the historian than for the scientist, for unlike the scientist, the historian cannot submit his conclusions to the test of experiment" (6). Whether this statement is really relevant to the problems of historical scholarship or not, can be left to the historians. Renier has put his finger on one of the essential advantages which science possesses—the role of experiment, and in some fields such as astronomy, repeated observations. Standen rather misses the point when he writes that "it is really uncommonly difficult to be impartial when comparing two theories, your own and the other fellow's. . . . Most scientists do this remarkably well in their own

specialised field. It is continually stressed in all scientific training that subjective feelings must not enter. . . . "

The point is surely that the impartiality of the individual scientist, and his alleged suppression of subjective feelings is not at all the foundation upon which the very real objectivity of science rests. This has been stressed by K. R. Popper (7) whose argument is as follows. "Everyone familiar with the history of the natural sciences is aware of the tenacity which characterises many of its quarrels. If scientific objectivity were dependent on the impartiality of the individual, we could say good-bye to it. It rests on different grounds and is a matter of scientific method. Science results from the co-operation of many. Two aspects are important and together they constitute the publicity of scientific method. Firstly, there is something approaching free criticism. Secondly, scientists try to avoid talking at cross purposes: they try to speak the same language. Experience is the impartial arbiter of their controversies and the important point is that it is public experience. Scientists try to express their theories in a form which can be tested by experience, and anyone who has learned the technique of understanding and testing theories can judge for himself. The fact that some will reach partial or even stupid judgments does not seriously disturb the working of the various social institutions, laboratories, periodicals and congresses, which have been set up to further scientific objectivity. The impartiality of the scientist insofar as it exists, is not the source, but the result of this socially organised objectivity of science." (Abbreviated.)

Popper does, however, idealise the situation a little when he says "there are no authorities in science." In his meaning of the term, he is no doubt right—in principle a beginner could prove a Rutherford wrong. But, in fact, the authority of Newton held up the development of certain physical theories in much the same way as the authority of Aristotle hampered mediaeval thought. Is biology entirely free from the authority of Darwin or psychology from that of Freud? It is well for us to recall that there were 13th Century thinkers who argued that new species came, not from special creation, but from natural development (8), when we contrast ourselves favourably with our forefathers. Few moderns state the case against their own views with the force and candour of Thomas Aquinas. It is quite possible, as Standen remarks "to write the history of science as one long crescendo whose ultimate glorious achievement is the present" but it is neither wise nor true to do so.

If then intellectual honesty and objectivity in science are a collective achievement rather than the characteristics of individuals,

what of toleration? Here, too, it is not easy to refute the accusation that "scientific men have suppressed and persecuted opponents of their theories and the best scientific truth can end in a rigid and mistaken orthodoxy." (Barzun). Nor does the evidence lie far in the past. Sidney Hook (9) comments as follows on a notable recent example of intolerances by scientific men. "Not so long ago," he writes, "we were confronted by the sad spectacle of scientific men evincing little faith in the logic and ethics of their own professional activity. Upon the publication of Dr. Velikovsky's book 'Worlds in Collision,' which confuses legend, myth and warranted assertions, a campaign was undertaken by scientific men not merely to refute it, which was easy enough, but to prevent its publication, which was intellectually scandalous. Nor is this the only instance of self-defeating and utterly unnecessary scientific orthodoxy. Here and there some administrator, happily no scientist, will vehemently oppose the presentation of the political-biology of Lysenko, overlooking the fact that the exposure of charlatanism is much more deadly to it than its proscription. I do not mean to suggest that scientists are under any necessity of examining seriously every crackpot theory that demands a hearing. Unless they scrulously avoid any attempt to prevent others from hearing what they may justifiably ignore, they are giving evidence of an incipient authoritarianism. . . ." Apart from such an extreme example, which is happily not representative of what scientists generally would approve, there is no question that at each period a climate of scientific opinion exists which is always tending to harden into an orthodoxy. In criticising a pseudo-philosophical "evolutionism" which has been such an orthodoxy for many decades, Popper (10) thought it necessary to make the following comment in a footnote. "Because of the tendency of evolutionists to suspect of obscurantism any one who does not share their emotional attitude towards evolution as a 'daring and revolutionary challenge to traditional thought,' I may perhaps mention that I see in modern Darwinism the most successful explanation of the relevant facts."

Toleration, then, in scientific as in other human affairs, must be defended anew in each generation. What of the role of "suspension of judgment" in scientific thinking? Is it true for example that judgment is always suspended till "all the relevant facts have been ascertained and analysed?" Firstly, such a claim is patently absurd. *All* the facts can never be obtained, and the question of what facts are relevant in a particular problem is never finally answered. Secondly, the idea that science consists of ascertaining "facts" and developing explanations (or theories) out of them by "inductive inference" is a figment of the imagination of

some logicians of the past. A cynic has said that science consists of facts in which everyone believes except those who last investigated them, and theories in which no one believes except those those who invented them. However, true, or otherwise, this epigram may be, it is clear that the relation between facts and theories in science is not a relation between empirical observations and equally empirical generalisations. Traditional accounts of scientific method, which place emphasis on universal empirical generalisations, have little to offer to the understanding of the "theoretical sciences," however useful they may be as descriptions of the natural-history phase through which all sciences no doubt pass. Science wants to make sense of its facts, and for this purpose a single observation of a carefully selected kind is more revealing than any collection of observations of how things are occurring. Carefully selected, in relation to what? Clearly to a theory which, as Popper (and perhaps before him Whewell) has shown, is prior to the facts which are collected to test it. Far from patiently awaiting more facts which are in some unexplained way to produce the desired explanation out of their very numbers, the theoretical scientist leaps to a fresh conception in terms of which phenomena may make sense and from which regularities already found or later sought for, may be deduced. A scientist is concerned, not so much with recording the way things happen, as with deciding on the merits of different ways of thinking about them, of different conceptions, models and methods of computation—in a word, different theories. Here, suspension of judgment has its place, in the sense that we arrive not at theories which we claim as true, but at those which are well-tested, and not so far refuted. The role of facts is in the testing, not in the discovery, of our theories.

The influence of an erroneous conception of how science develops and what its attitude is, has been enormous and is by no means at an end. Relativity in physics has been illogically transferred to other fields of thought and has tended to paralyse action. The Harvard philosopher R. B. Perry (11) has lately voiced a protest. "Limited by their self-imposed code . . . 'on the one hand' and 'on the other hand' . . . teachers hesitate to teach their students how to choose among opinions, and hesitate themselves to choose." They are reluctant "to be explicit on questions of value. Social 'science' no longer embraces knowledge of the good. Values are left to personal 'attitudes' and to tamper with these is to expose the teacher to the charge of indoctrination." But "thought is applied to action through decision. One cannot postpone decision indefinitely." And Perry concludes 'it is comparatively easy to raise doubts, but doubt should be regarded as the prelude to belief.

If beliefs are demolished, they should be built again or others in their place. If this is not done, the vacuum will be filled with authority, hearsay or superstition."

The characteristic belief of the modern period, the faith of modern man, has been the belief in inevitable progress. Mediaeval thinkers did not attach as much importance as the modern thoughtless to progress in means unaccompanied by improvement in ends. We have come sharply to realise that improved means are not automatically applied to better ends, and once again the metaphysical (as distinct from the scientific) problems of the nature and destiny of man are troubling us. At least we no longer fall into the error satirised by Hilaire Belloc:

> "The path of life, men said, is hard and rough,
> Only because we do not know enough.
> When science has discovered something more
> We shall be happier than we were before."

One other tendency in thinking as affected by the great achievements of science will bear a brief word. Success breeds the belief that many more, perhaps all, problems will be solved by a method so triumphant. A recent notable example of the tendency to confuse hopes with achievements is given in Professor J. Z. Young's review of the new edition of Sir Charles Sherrington's Gifford Lectures of 1937. (12) "And yet," says Young "in spite of all his science, Sherrington is still perplexed by the mystery of mind and body. Is it possible that this too is one of those mysteries that are not there?" He goes on to point out, quite reasonably, that "those who attempt to find a solution to the problem are often met with dogmatic assertion or ridicule rather than with argument."

He pins his faith in a successful solution on "a new language for tackling the problem," a "whole science of information theory," the development of calculating machines, and new statistical methods for studying the pattern of nervous activity. It is certainly desirable that these tools should be applied to the hypothesis that the activities we call mental can be included in the same scheme with the physical activities of the nerves (Sherrington concluded otherwise). It is well that there are investigators like Professor Young who believe, however flimsy the present evidence, that the attempt will succeed—otherwise it is unlikely to be made. But at best we can only expect to achieve a "well tested hypothesis" (Popper) and in Young's own words "the apparent

absurdity of antique ideas should act as a reminder of the relativity of our own." It is *because of* all his science, not *in spite* of it, that Sherrington declined to claim more than he achieved. Young's choice of words reveals his belief that the issue is settled, and that is certainly not a scientific conclusion. Naive realism remains dominant in much biological thinking.

What then must we conclude? Has Science become a Sacred Cow, above and beyond criticism, not only a way but The Way? If it has, then certainly its modern spokesmen have fallen below its best traditions, or some have presumed to speak in its name with little warrant. Science is above all a great adventure. It can, indeed has, allowed the display of the best of human qualities, even if its pursuit is no guarantee of their possession. If it does not lead to certainty and truth, it does lead away from error, and in that important sense, makes real progress. Perhaps we may claim the old Scottish verdict "Not Proven".

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(10) K. R. Popper. "The Poverty of Historicism". Part III. Economica, Vol. 12, 1945. p. 69.

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STOP PRESS!

CONFERENCE, 1953

This year's Conference will be held at Dunedin, where we had a very pleasant time in 1948, and the tentative dates are Wednesday. 26th August to Friday 28th, inclusive.

The Conference Committee considered that a "retreat" type of conference should prove successful, and arrangements are in hand to obtain accommodation at the University Residential colleges. The University has approved the idea subject to satisfactory internal arrangements being made. Alternative hotel bookings could be made for those desiring them.

With reference to transport, it would be possible to charter a plane flying Auckland to Dunedin, provided there is sufficient support. The time of departure could be conveniently arranged and passengers picked up en route. Normal air-travel rates would be charged.

Members are invited to write to the Conference Secretary, G. W. Broughton, Box 229, Dunedin, giving

(1) preliminary indications whether they hope to attend the Conference, and accommodation required

(2) information as to whether they are interested in air travel as outlined above.

(3) suggestions for papers.

TWENTY-SECOND ANNUAL REPORT
for the year ended 31st October, 1952.
(Slightly Abridged)

MEMBERSHIP—

Membership figures for recent years are as follows:—

	1949	1950	1951	1952
AUCKLAND	88	95	77	89
WAIKATO	-	-	24	28
MANAWATU	-	-	31	37
WELLINGTON	137	147	133	142
CANTERBURY	54	60	65	68
OTAGO	55	54	52	54
OVERSEAS	36	44	47	37
	370	400	429	455

Three Associates, Dr. L. G. Neubauer, Dr. E. B. Davies and Dr. W. A. McGillivray were elected to the Fellowship and thirty-five new Associates joined the Institute during the year.

There were seven resignations and one name was removed from the roll through non-payment of subscription.

SUB-COMMITTEES OF COUNCIL.

Committees of Council vary in many ways. Some are large and continue from year to year as a vital part of the Institute as for example the Journal Committee and the Examinations Committee. Others are set up to do a specific job and cease to function when the work is completed. e.g. Contracts of Service. Others still, are modified in the light of experience and this is the case with our Chemists' Employment Committee.

At the General Meeting held during Conference Sir Theodore Rigg thanked members of all sub-committees for their contributions towards the progress and prestige of the Institute.

Thirteen sub-committees reported to the General Meeting and a brief summary of the various reports is recorded here.

1. CONFERENCE:

Conference 1952 was held at Victoria College, Wellington from August 26th-28th. A Conference Booklet gave a resume of all papers. One evening was devoted to the Presidential address with a social hour to follow and the second evening to the gathering together of various groups of chemists e.g. Teachers, Chemical Engineers, Paint and Colour Chemists, Ceramic Chemists, Biochemists and so on.

Conference 1953 is planned for August 1953 in Dunedin.

2. CHEMISTS EMPLOYMENT COMMITTEE:

This Committee was originally set up to cater for unemployed chemists but such a group of chemists doesn't seem to exist. We get a number of enquiries from abroad often through Government Departments and advertised vacancies in New Zealand and Australia (C.S.I.R.O.) are collated and sent to members on the Employment register. It is now recommended that the full Committee go into recess and that an Employment Officer be appointed to exercise general supervision of employment affairs.

3. EXAMINATIONS COMMITTEE:

This is a hard-working Committee situated in Dunedin. Apart from preparing syllabi to cover new subjects for the Laboratory Assistant's Certificate and making the necessary arrangements for examiners and supervisors and seeing that sixteen candidates were examined in 42 papers the Committee was asked to prepare regulations to govern admission to the associateship of the Institute by examination. Five Laboratory Assistants completed the course and were awarded certificates.

4. JOURNAL:

The Auckland Editor and Committee have now completed five years in producing the Journal and the delegates from Branches have been unanimous in expressing appreciation of the good work done. At present, five issues are published each year and 725 copies are required each issue for members, advertisers, Libraries and overseas exchanges.

Unfortunately we are having to cope with ever increasing costs but this difficulty is being experienced by societies everywhere.

5. MEDICAL ADVERTISEMENTS:

This Committee was of great help to the Health Department when regulations governing Medical advertisements were newly gazetted. For some years now no cases of objectionable advertising have been reported so it appears that the regulations have been effective.

6. MEMBERSHIP COMMITTEE:

This Committee, consisting of three Fellows of the Institute examines and reports to Council on all applications for admission to the Associateship or the Fellowship. It also assists in amending rules governing admission.

This year 29 applicants for admission have been granted.

7. PATENTS:

We have an officer interested in the chemical aspects of patents. He regularly peruses the Patent Journal and draws the attention of individual chemists to patents which may concern them.

8. PROFESSIONAL STATUS COMMITTEE:

This Auckland Committee has been mainly concerned with the impact of Unionism on our members. It is also examining rules for admission, i.e. standards of admission to the various Empire Institutes of Chemistry.

9. RULES COMMITTEE:

Amendments to Rules are generally long drawn out processes when an overall revision is undertaken. Work which commenced in 1950 was handed over to a committee under the chairmanship of the President in 1951, and has occupied the Committee, Branch Committees and Council for many hours during 1952. It is a costly process both in time and money so it is hoped the new edition will serve for many years without further overhaul.

10. STANDARDS INSTITUTE:

We are fortunate that in our membership we cover such a wide variety of chemists. This in turn is fortunate for the Standards Institute for we are able to nominate representatives to a wide variety of chemical committees, (nine in all). We are also represented on the Chemical Division and a past President of the Institute is our representative and chairman of the Standards Council.

11. SALARIES:

During 1952, Council authorised its third Salary Survey. Members of the Institute responded well and the salaries committee was able to submit a report with Graphs to the General Meeting in August. The report has been published in the October Journal.

12. STANDARD METHODS OF ANALYSIS:

This work has been largely in abeyance during recent years. The Soils and Fertilizer Section is still carrying out some comparative tests. Many methods are elaborated in one main laboratory where the analysis is carried out regularly and this method is followed by other workers who have only an occasional analysis to do. Contacts are made each year during Conference particularly under the present system where groups of chemists working in one particular line are encouraged to foregather on one evening.

13. UNESCO:

We were well represented on the National Commission and have three members on a Scientific Sub-Committee of Unesco. A past-President led the N.Z. delegation in Paris in July 1951. Science abstracting of all Journals is in hand and discussion groups should shortly be formed to discuss the topic "Energy in the Service of Man."

LOCAL MEMBERS.

Local members now number about 160 and form an important group in the Institute. It was recently decided that it should be obligatory for them to take the Journal at an annual cost of 7/6. That is, their total annual subscription will be the Journal subscription and the Local Branch subscription. Local members do not pay a full subscription like qualified members and do not have a vote in Institute affairs, but otherwise all Institute privileges are available to them.

GENERAL MEETING:

A General Meeting of the Institute was held on the first day of Conference and was very well attended. On this occasion it has become customary for all sub-committees of Council to submit an annual statement of activities covering the previous 12 months.

AWARD OF PRIZES:

The I.C.I. Prize for 1952 was awarded posthumously to Mr. C. W. Brandt. The Morcom Green and Edwards Prize was awarded to Dr. W. A. McGillivray with Mr. A. L. Odell very highly commended.

OVERSEAS CONFERENCES.

Representatives who attended the American Chemical Society Diamond Jubilee meetings in New York have published reports either in the Journal or as reports to Council.

We were represented at a recent Biochemistry Conference in Paris and also at the Sydney meeting of the Australian and New Zealand Association for the Advancement of Science.

FOOD PARCELS.

We have been given every opportunity to stop this service but after due consideration of the situation and with some knowledge of individual cases our membership decided to continue sending parcels to selected names on the Benevolent List of the Royal Institute of Chemistry.

Letters of appreciation also indicate the worthwhileness of the scheme.

TIMBER PRESERVATION.

This has been a subject of great interest to a number of Societies and Government Departments over the past few years and we number in our membership Chemists who have taken an active part in the promulgation of standards and more recently in a full investigation of the effectiveness of the borax/boric-acid process for preserving timber.

As an Institute we can only be interested in a general way and in seeing that the necessity of full chemical control is realised.

MANPOWER.

Our Institute has agreed to co-operate in the compilation of a register of scientific manpower in New Zealand. This is being organised by the Department of Scientific and Industrial Research and is on a voluntary basis.

FINANCIAL.

The Institute's finances are on a sounder basis following the increase in subscriptions which was effective for this financial year. Due to this increase and to the increase in membership our subscription income has increased by almost 50 per cent. On the expenses side, the Registrar's salary has been increased to £200, while other administration expenses are in line with those of the previous year.

The net cost of the Journal is now almost £50 per issue. On the Balance Sheet overdue subscriptions show a substantial rise to £160 while cash balances including the National Savings Account total £1235, subject to the payment of the sundry creditors. Following the transfer of £75 from the general funds, the Trust Fund now stands at over £600.

MEETINGS.

There are now six Branches of the Institute throughout New Zealand and each Branch holds meetings at approximately monthly intervals over a period of approximately eight months. A list of papers read before Branches will be published in a future issue.

THANKS.

The mere list of members serving on Council, on Branch Committees and on sub-committees of various kinds shows that the Institute has a large percentage of its membership in voluntary and regular service. We know these members are not seeking thanks but Council wishes to express appreciation of the way they "run" the Institute.

In particular we would again thank Mr. Joiner for inscribing all Certificates.

For the use of the Council room we are indebted to the Department of Scientific and Industrial Research and on numerous occasions we have been granted facilities by Victoria University College.

For and on behalf of Council,

S. N. SLATER,
President.

W. G. HUGHSON,
Hon. General Secretary.

NOTE—The Balance Sheet and Financial Statement will appear in our next issue

DR. M. M. BURNS

The appointment of Dr. M. M. Burns to the post of Principal of Lincoln College honours a member of our Institute who is respected both for his scholarship and for his interest in professional affairs.

Dr. Burns attended Rangiora High School, and graduated from Canterbury College with first class honours in botany. He also studied advanced chemistry. He was a Senior Scholar in botany and a Post-Graduate Science Scholar to the Macaulay Institute for Soil Research, from which he completed Ph.D. from University of Aberdeen with a thesis on chemical and ecological aspects of the moorlands.

In 1934, he was granted a Commonwealth Fund Fellowship in agriculture to Cornell University, New York, where he undertook two years' post-graduate work in the plant sciences and agronomy and conducted research on the exudation of nitrogenous compounds from legume nodules.

After his appointment in 1936 as plant physiologist in the Soil Bureau of the D.S.I.R., he surveyed and reported on the citrus and tung oil industries in the United States and in New Zealand. In 1937, he transferred to Lincoln College as Senior Lecturer and head of the Soil Science Department. There he engaged in teaching and research on soil fertility problems and acted as tutor to the students in degree courses.

From 1948 until last year, he was Director of the newly formed Fertiliser Manufacturers' Research Association for which laboratories and research facilities have been established at the Otara Research Station, Otahuhu. (An account of the work of this Association is given on another page.)

Dr. Burns was elected an Associate in 1937 and a Fellow in 1943. He served for several years on the Canterbury Branch Committee, and was Chairman for 1943. From 1939 to 1943, he served as Canterbury Delegate to the Council.

THE N.Z. FERTILISER MANUFACTURERS' RESEARCH ASSOCIATION

This Research Association was established jointly by the Department of Scientific and Industrial Research and the Fertiliser Manufacturers to provide a link between agricultural research in New Zealand and overseas and the fertiliser industry and to conduct research on problems of production, analysis, distribution and use of fertilisers, especially phosphates, in agriculture and on materials used in their manufacture.

The research unit which is housed at the Otara Research Station, Otahuhu, and commenced work in 1951 with Dr. M. M. Burns as Director, comprises new and well-equipped chemical laboratories, including one devoted to work with radio-active phosphorus, glasshouse space and associated facilities for studies with fertilisers on a pot and field plot scale.

The main problems under investigation are:—

(a) Raw Materials:

The suggestion of Hendricks that there may be a correlation between the CO_2 evolved from a rock phosphate and the phosphate which goes into solution is under investigation. Both products are presumed to be derived from the readily decomposable carbonate-apatite (collophane) mineral fraction of the rock phosphate.

The superiority for direct application to the soil of the North African rock phosphates which contain much collophane over the Pacific Island rock phosphates which contain traces of collophane suggest that an investigation of the phosphate minerals present in the various rock phosphates available to this country is justified.

The project listed is introductory to full scale investigations which will call for collaborative work with the Dominion Laboratory and the Soil Bureau.

(b) Types of fertilisers:

The present world shortage of sulphur has directed attention to manufactured phosphates which do not require the use of sulphuric acid; to the rock phosphates for direct applications to the soil and the various additives which can be added to straight superphosphate.

Chemical tests and pot tests using selected indicator plants are under way to test the effectiveness of the fertilisers now in production with these other products.

The high standard of effectiveness of superphosphate under a wide range of soil conditions makes the problem of finding satisfactory alternatives very difficult, but trials conducted to date have shown that some of the fused products, including fused calcium-magnesium phosphate, give very satisfactory results on some of the acid soils.

In a study of the manufacture of superphosphate by using sulphuric acid of different strengths at fixed volumes, it was shown that the amount of rock phosphate which was decomposed was directly related to the amount of sulphuric acid present and that better conditioned products could be made by diluting normal superphosphate with ground rock phosphate than resulted from the use of weaker acid than that normally used.

(c) Production of fertiliser mixtures:

The increasing use of chemicals for weed control and for the control of pests in pastures has led to problems of incorporating these materials in fertilisers. Wherever possible these chemicals are added to the fertiliser during manufacture, but work done with the sodium salt of 2-4D used in

weed control has shown that the addition of the chemical in the early stages of manufacture of superphosphate leads to a loss in effectiveness of the hormone. This has been overcome by adding the hormone to the matured fertiliser.

(d) Fertiliser Analysis:

The analysis of fertilisers for various constituents is a major part of the chemical work in the fertiliser industry.

Investigations are under way on various methods for determining nutrients in fertilisers with the objective of establishing efficient rapid methods of analysis.

(e) Radioactive phosphorus studies: P_{32} is being used to:

1. Check the relative efficiency of various phosphate fertilisers as a source of phosphorus for plants.
2. Check on the relative effectiveness of the same fertiliser in powdered and granular forms.
3. Check on the available phosphate reserves in various soil types.

NEWS AND NOTES

During September, the Otago Branch was addressed by Dr. K. L. Sutherland of Melbourne on "Physical Chemistry and Industry", and also by Prof. E. C. Dodds on "The Paths to Stilboestrol". A symposium on current research was presented on another evening by members of the Branch and the following papers were read:—

G. W. Blackman: Oxidation of Benzene by Peroxide.
A. J. Ellis: Positive Halogen Complexes.
A. D. Campbell: Quantitative Estimations—Determination of Nitrogen.
G. K. McIndoe: A New Dehalogenation Method.

Dr. W. E. Harvey, of Auckland, has returned after four years' post-graduate study overseas, having spent two years at Cambridge under Prof. Todd studying nucleotides, and two years in Sweden under Prof. Erdtman in research on nukatin and associated natural products from wood.

On November 1st a number of members of the Auckland Branch availed themselves of the opportunity of a visit to the cement works at Portland, near Whangerei. The visitors were very hospitably entertained by Mr. O'Rorke and the trip was a great success.

Prof. Chapman, Professor of Botany at A.U.C., gave the Auckland Branch a most interesting and informative lecture on "Sex Hormones in Plants" at its November meeting.: He described some excellent collaborative studies carried out by research terms comprising workers in the various sciences.

After the Annual General Meeting of the Auckland Branch had disposed of the more general business, certain members wanted to discuss some aspects of the proposed Auckland Drainage scheme. Those members not particularly affected by the problem volunteered to bring in supper and the business of the evening continued without a break until a late hour. The main problems confronting members of the Branch were: (1) The attempt by certain antagonists of the present drainage proposals to create confusion in the public mind by asserting that all information and evidence placed before them by professional "experts" is ill-founded and unconsidered or prejudiced, and (2) the disregard the engineers planning the project appear

to have for the fact that sewage disposal is a biochemical process, and their apparent continued disregard of the need to utilise the services of competent chemists in their deliberations. The meeting set up a committee to discuss the problems and to make recommendations to the Branch Committee for further action.

Mr. R. C. Selkirk has recently accepted a position with Dominion Breweries Ltd., Otahuhu, after spending several years on timber preservation and toxicology at the Dominion Laboratory, Auckland.

Mr. Leslie H. Davis, M.Sc., A.N.Z.I.C., Scientific Liaison Officer for the Department of Scientific and Industrial Research in Australia (with offices at Melbourne) has returned to New Zealand to take up the position of senior chemist, Dominion Laboratory, and will work with the Division of Occupational Health, Health Department, in the field of industrial hygiene.

Mr. Davis is one of the few scientists still on the active list of the Department of Scientific and Industrial Research who were on the staff of the Dominion Laboratory when it was transferred from the Internal Affairs Department to the newly-formed Department of Scientific and Industrial Research in 1926. He was the first permanent scientific liaison officer in Australia for the Department, and has held the position since 1945.

L. H. DAVIS

Mr. H. A. L. Morris, having returned from a most interesting and instructive sojourn at Massachusetts Institute of Technology, is now working in the laboratories of the Dairy Division of the Dept. of Agriculture, Auckland.

Mr. G. Dingley, chemist to Newdick Bros., biscuit and cake manufacturers until they closed down recently, is now with the Dominion Compressed Yeast Co., Auckland.

Dr. K. L. Sutherland, of Melbourne, spoke to the Auckland Branch in September on "Froth Flotation Methods."

Mr. W. J. Blackie, who was elected a Fellow in 1935, is at present on leave in New Zealand before proceeding to his new post of Director of Agriculture in Hong Kong. Educated in New Zealand and at Yale University, U.S.A., Mr. Blackie was appointed Government Chemist in Fiji in 1929, and was promoted to the position of Deputy Director of Agriculture for Fiji, six years ago.

ITEMS FROM THE MINUTES OF COUNCIL-IN-PERSON

Held at Victoria College, Wellington,
27th and 28th November, 1952.

Present—Prof. S. N. Slater (in the chair), Dr. H. E. Annett, Vice-President, Messrs. G. S. Lambert (Auckland), R. E. R. Grimmett (Waikato), Dr. R. M. Dolby, (Manawatu), A. P. Oliver (Wellington), F. H. G Johnstone (Canterbury), O. H. Keys (Otago), W. G. Hughson (Secretary), H. K. Palmer (Registrar) and, by invitation, Messrs. B. E. Jackson, I. G. McIntosh, L. H. James and Dr. L. G. Neubauer.

Amendments to the Rules—Much time was taken up with a very detailed revision of the rules which are shortly to be reprinted, with all the amendments. One important change is the establishment of the office of a second Vice-President, to be held by the immediate past President. The rules relating to examinations and the admission now to be redrafted by a committee consisting of Messrs. Slater, James, Hughson and Oliver, then submitted to a solicitor for perusal and to the Branch Committees before being reprinted.

Annual Report and Balance Sheet as printed in this issue were approved. It was decided that £100 be set aside for reprinting the rules and list of members.

Annual Grant to Branches to be increased to £5 per annum.

Honoraria granted were as follows: Hon. Secretary 25 guineas, Hon. Editor, 10 guineas.

Overseas Visitors—Prof. Slater was asked to communicate with Prof. Emeleus, F.R.S., of Cambridge, inviting him to New Zealand.

Duties of the Registrar are to be as follows—

Correspondence. To arrange for the regular clearing of the Institute's Post Office Box, and to acknowledge all letters addressed to the Registrar, to the Institute or to the Hon. General Secretary. He shall answer fully such matters as he can and will refer other letters to the appropriate officer of the Institute. He will keep a register of all letters received and despatched. In general he will act as a clearing house for the receipt and despatch of all correspondence, circulars, etc.

Addressograph. He will keep the addressograph plates up-to-date in accordance with information furnished by Branch Secretaries and individual Members. He will arrange for the addressing of wrappers and envelopes as required.

Applications. He will record the receipt of all applications and will undertake all the written work in connection therewith.

Finance. He will keep the financial books and records of the Institute and will prepare the annual accounts for submission to the Auditor. In particular he will keep a subscription record for every Fellow and Associate. He will write receipts for all moneys received and will bank such moneys promptly to the credit of the Institute's Bank Account.

Council Meetings. He will attend all Council Meetings held in Wellington and will keep such notes of the proceedings as will enable him to check the Secretary's Minutes.

Filing. He will keep the current correspondence files.

General. He will carry out such further duties as may be mutually agreed on from time to time.

Journal. He will be responsible for supplying the addressed Journal wrappers to the printers, and for supplying back numbers of the Journal.

Employment Committee—It was decided, in view of the small number of unemployed chemists, not to reappoint this Committee but to leave its work to an Employment Officer. Mr. E. S. Borthwick was appointed.
Journal—No Journals are to be sent to Local Members who are unfinancial. Hon. Local Members are to receive free Journals. Mr. G. M. Wallace was appointed Assistant Editor of the Journal.
Standard Methods of Analysis—It was resolved that this Committee be disbanded and thanked for its work.
Conference, 1953—This will be in Dunedin in August with Mr. G. W. Broughton as Secretary.
Membership.—The following new Associates were elected: W. H. West, M.Sc., Abels Ltd., Newmarket, Auckland. F. N. Fastier, M.Sc., D.Phil. (Oxon.), A.R.I.C., Medical School, Dunedin. (Re-election) Miss Sheila Merrick, B.Sc., (Lond.), Griffin and Sons, Lower Hutt.

The following resignations were accepted: D. S. Cumberbeach, L. Osgerby, I. H. Skipworth.

The name of A. Goldstern has been deleted for non-payment of subscription.

Leave has been granted to A. J. Beckwith, D. S. Letham and R. M. Allison.

LIST OF OFFICERS

for the year 1st November 1952—31st October, 1953.

President:	Dr. H. E. Annett, 15 East St., Claudelands, HAMILTON.
Vice-president:	Dr. H. O. Askew, Cawthron Institute, NELSON.
Hon. Gen. Secretary:	W. G. Hughson, P.O. Box 250, WELLINGTON.
Auckland Delegate:	G. S. Lambert, P.O. Box 9029, Newmarket, AUCKLAND.
Waikato Delegate:	R. E. R. Grimmett, Rukuhia Soil Research Station, Private Bag, HAMILTON.
Manawatu Delegate:	Dr. A. T. Johns, Grasslands Division, P.O. Box 623, PALMERSTON NORTH.
Wellington Delegate:	Dr. L. G. Neubauer, Dominion Laboratory, WELLINGTON.
Canterbury Delegate:	Dr. M. L. McGlashan, Canterbury University College, CHRISTCHURCH.
Otago Delegate:	O. H. Keys, P.O. Box 562, DUNEDIN.
Editor of Journal:	S. G. Brooker, P.O. Box 9012, Newmarket, AUCKLAND.
Past President:	Professor S. N. Slater, Victoria College University, WELLINGTON.
Registrar:	H. K. Palmer, P.O. Box 250, WELLINGTON.
Assistant Secretary:	A. P. Oliver, P.O. Box 250, WELLINGTON.

AUCKLAND BRANCH

Chairman:	G. S. Lambert, P.O. Box 9029, Newmarket, AUCKLAND.
Secretary:	A. G. Frieberg, 42 Nottingham St., Westmere, AUCKLAND, W.2.
Treasurer:	R. C. Selkirk, 62 Arney Road, Remuera, AUCKLAND.
Committee:	G. Dingley, L. W. Jagger, Dr. H. Bloom, G. M. Wallace.

WAIKATO BRANCH

Chairman:	R. E. R. Grimmett, Rukuhia Soil Research Station, Private Bag, HAMILTON.
Secretary-Treasurer	E. P. White, Ruakura Animal Research Station, Private Bag, HAMILTON.
Committee:	J. L. Grigg, F. E. Mason, Miss M. P. Bartrum, Dr. R. H. Jackman.

MANAWATU BRANCH

Chairman:	Dr. A. T. Johns, Grasslands Division, P.O. Box 623, PALMERSTON Nth.
Secretary-Treasurer	Dr. R. P. Newbold, Grasslands Division, P.O. Box 623, PALMERSTON Nth.
Committee:	Dr. R. M. Dolby, Dr. J. C. Dacre, E. C. Jessop, Dr. W. A. McGillivray.

WELLINGTON BRANCH

Chairman:	Dr. L. G. Neubauer, Dominion Laboratory, WELLINGTON.
Secretary-Treasurer	Miss H. E. Barr, Dominion Laboratory, WELLINGTON.
Committee:	T. A. Rafter, H. J. Wood, C. G. Mason, C. L. H. Stonyer.

CANTERBURY BRANCH

Chairman:	Dr. M. L. McGlashan, Canterbury University College, CHRISTCHURCH.
Secretary-Treasurer	F.H.G. Johnstone, P.O. Box 325, CHRISTCHURCH.
Committee:	Dr. H. N. Parton, M. S. Carrie, R. W. Cawley, J. Vaughan.

OTAGO BRANCH

Chairman:	O. H. Keyes, P.O. Box 562, DUNEDIN.
Secretary:	J. Rogers, School of Mines, University of Otago, DUNEDIN.
Treasurer:	A. D. Campbell, Chemistry Dept. University of Otago, DUNEDIN.
Committee:	G. W. Broughton, C. L. Carter, D. F. Nelson, D. H. McLean.

SUB-COMMITTEES 1/11/52 — 31/10/53.

1. CONFERENCE COMMITTEE 1953:
   O. H. Keys (Chairman), P.O. Box 562, DUNEDIN. (R.I.C. Representative).
   G. W. Broughton (Secretary), McLeod Bros. Ltd., P.O. Box 229, DUNEDIN.
   Dr. R. Gardner (R.I.C. Representative) together with other Members of the Otago Branch Committee.
2. EMPLOYMENT OFFICER:
   E. S. Borthwick, 39 Northland Rd., WELLINGTON, W.2.
3. EXAMINATION COMMITTEE:
   O. H. Keys (Chairman), Dominion Laboratory, P.O. Box 562, DUNEDIN, C.1.
   C. R. Edmond (Secretary), C/o Dr. R. Gardner, 41 Dowling Street, DUNEDIN.
   G. B. Beath, H. G. Woolman, A. D. Campbell and J. W. McChesney.

4. JOURNAL EDITORIAL COMMITTEE:
   S. G. Brooker (Editor), P.O. Box 9012, Newmarket, AUCKLAND.
   G. M. Wallace (Assistant Editor), Dominion Laboratory, Durham St., W. AUCKLAND.
   A. G. Frieberg (Business Manager), 42 Nottingham St., Westmere, AUCKLAND.
   D. Whillans, G. W. Stace, Dr. H. Bloom.
5. MEDICAL ADVERTISEMENTS COMMITTEE:
   L. H. James (Convenor), Dominion Laboratory, Sydney Street, WELLINGTON, N.1.
   N. H. Law.
6. MEMBERSHIP COMMITTEE:
   W. A. Joiner, Dr. R. Gardner, Dr. L. H. Briggs.
7. PATENTS OFFICER:
   E. S. Borthwick, 39 Northland Rd., WELLINGTON, W.2.
8. PROFESSIONAL STATUS COMMITTEE:
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   J. Ricketts (Secretary), 113 Valley Road, AUCKLAND, S.2.
   F. H. V. Fielder, D. Whillans.
9. STANDARDS INSTITUTE OF NEW ZEALAND:

9. 1. *Representatives on N.Z. Standards Institute Council*—
   G. A. Lawrence, JOHNSONVILLE.

9. 2. *Chief Representative for all Standards Institute Affairs*—
   M. L. H. Stewart, P.O. Box 2091, WELLINGTON.
   (C. L. H. Stonyer, 26 Brees Street, LOWER HUTT—acting in Mr. Stewart's absence).

9. 3. *Representatives on special Standards Institutes Committees*—
   Chemical, Insecticides, Domestic Refrigeration and General—
   M. L. H. Stewart.
   Roadmaking Materials and Methods—Road Testing Material—
   J. B. Hyatt, Dominion Laboratory, WELLINGTON.
   Electro-Plating and Protective Metal Finishes—
   Dr. R. Gardner, 41 Dowling Street, DUNEDIN, C.1.
   Rodent Poisons, Refrigeration and Timber Preservation—
   C. G. Mason, P.O. Box 632, WELLINGTON.
   Metal Containers—Paints—
   J. M. C. Tingey, 20 Cavendish Square, WELLINGTON, E.5.
   Textiles—
   Dr. L. F. Story, Woollen Research Association, University of Otago, DUNEDIN.

10. U.N.E.S.C.O. REPRESENTATIVE—
    J. A. D. Nash, Dept. Scientific and Industrial Research, WELLINGTON,

11. FOOD PARCELS OFFICER—
    Dr. J. K. Dixon, Soil Bureau, 54 Molesworth Street, WELLINGTON, N.1.

12. SALARIES COMMITTEE—
    Dr. J. K. Dixon, Soil Bureau, 54 Molesworth Street, WELLINGTON, N.1.
    J. L. Mandeno, 1 Pine Terrace, WELLINGTON, W.3.

BOOK REVIEWS

Chemistry of Carbon Compounds. Edited by E. H. Rodd. Vol. 1, Part B. Pages 779 to 1462. 1952: Elsevier Publishing Co. Ltd., Cleaver-Hume Press, London. Pre-publication price £5; after publication, £5/15/-. It is a pleasure to receive this volume which completes the survey of the aliphatic division, including carbohydrates, proteins and enzymes, and comes to hand six months after 1A. This is commendably prompt, especially as the book under review contains references up to March, 1952. About 20 per cent of the references are to 1945 or later, showing that the work is well up-to-date. This is certainly a very useful publication and any criticism can only be about minor points or matters of emphasis. It is rather surprising to find the fats dismissed in 2½ pages while sixteen pages are allotted to the dihydroxy aliphatic acids. Under lecithin there is no reference to the important fact of its insolubility in acetone. A few errors were noted: triolein has not been found in butter fat and its occurrence is unlikely (p. 1027), and on p. 872 the anilide of acetoacetic acid is referred to where the β—phenyl imine is intended —an error which has been taken over from Richter and shows the close descent bf one work from the other. The printing and binding are very good.

Kurzes Handbuch der Chemie (Concise manual of Chemistry). Prepared by Waldemar Koglin. Vol. 1 432 pages. 1951: Vandenhoeck and Ruprecht, Göttigen. D.M. 39.50 (£3/10/-). This is a more elaborate handbook than any available in English but is yet within the scope of the library of the smallest laboratory. It is planned to give all those who cannot afford or do not have ready access to the more elaborate and expensive compendiums such as the International Critical Tables, all the information they can reasonably need in a compass of five volumes of about 440 large octavo pages each to be issued at six-monthly intervals. In the first four volumes it is proposed to give data on about 30,000 different compounds, while volume 5 will contain collected table of certain values together with the usual mathematical tables, lists of indicators, analytical factors, etc. To give an idea of the nature and scope of the work,the data on acetic acid cover 4½ pages and include full tables of vapour pressures, viscosities, azeotropes, liquid-vapour compositions for mixtures with water, heats of fusion and evaporation, specific heat, conductivity, electrolytic dissociation, melting point at various pressures, densities of solutions, surface tension of solutions, solubilities, refractive index, van der Waal's constant and other minor items. It will be seen from this formidable list that nothing has been overlooked. The printing is excellent and an acid-proof binding encourages bench use. Altogether, this work can be thoroughly commended. Two points may be mentioned in conclusion: an English index included in the last volume would be a help, especially as many of the compound are listed under unusual names such as benzenkarbonal for benzaldehyde; and secondly, why do authors of these works like to bolster up the apparent number of compounds listed by numbering cross-references in the general scheme?

Synthetic Methods of Organic Chemistry by W. Theilheimer. Vol. 6, 401 pages, 1952: S. Karger Ltd., Basle. 54.1 Swiss francs. This volume contains abstracts of papers published mostly in 1949 and 1950 with supplementary references from 1951. It is published in English which is an advantage and contains 911 abstracts, more than in any previous volume in keeping with the growth of organic chemistry. Though it is the first of the second series of these volumes it contains references to some methods which have been used in the period under review, but have been abstracted from another author in an earlier volume. It should be mentioned for those unfamiliar with this series that it consists of abstracts of organic methods classified according to bonds formed or broken, and according to the agent used. It can thus be a great help in finding in the recent literature methods of achieving desired steps in synthesis. The printing and binding are commendable.

both occupy important executive positions in their industry. They should be thus be well qualified for the task they have undertaken.

The manual is an attempt to bring within the scope of one volume the basic principles of the processes of manufacture in the paint and varnish industry. The subjects dealt with include materials handling, factory planning and lay-out, processes of manufacture and plant employed some engineering principles and production planning. There is a section which deals broadly with the raw materials and finished products of the industry, a rather lengthy section on management and personnel problems and a chapter on costing procedure. The section on Research and Development is, in keeping with the rest of the book, written for the production man rather than for the chemist.

However, all chemists in the paint industry should read the book. It deals in a very practical way with many of the technical problems encountered in paint and varnish manufacture.

The book is well illustrated with photographs, diagrams, charts and plans of plant lay-out. At the end of each section is an extensive bibliography, but all references are to American literature. The binding, printing and the quality of the paper are all good. The book is adequately indexed.

—J.L.M.

BOOK REVIEWS

Chemische Technologie. Vol. 3. 911 pages. 1952: Carl Hanser Verlag, Munich. Subscription Price, DM 71. The first two volumes of this series were discussed in our issue for June, 1952; this one is the first of two dealing with organic technology. It covers the following topics: coal and peat and their products; petroleum and its products; synthesis of methanol and isobutanol; wood as a raw material and the manufacture from it of charcoal, sugar, and paper; aliphatic chemicals and intermediates; synthetic fibres; and the Fischer-Tropsch process.

Messrs. John Wiley and Sons, New York, have recently issued Vol. 2 of the well-known *Organic Syntheses* under the editorship of R. T. Arnold (119 pages, $3.50) and Vol. 2 of *Biochemical Preparations* which is published in a similar style. It is edited by Eric G. Ball (109 pages, $3.00).

Paint and Varnish Production Manual, by V. C. Bidlack and E. W. Fasig, 1952, New York, John Wiley and Sons Inc. $6.50. London, Chapman and Hall Ltd. 52/-. ix + 288 pages.

Publication of this book has been sponsored by the Federation of Paint and Varnish Production Clubs, of which the Editors are Past Presidents. Their names are well known in the American technical literature and they

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