connected. The Jurassic evidence is inconclusive, while for the Cretaceous it is "rather unequivocally in favour of an active faunal interchange between South and North America". Discussing Colbert's paper, Dr. A. S. Romer (pp. 250–254) agrees about the Cretaceous and Jurassic but, although fully conscious that most hypothetical land bridges are not merely delusions but also snares to the unwary, he mildly disagrees about the Triassic. He writes: "I find myself here, after consideration of the evidence [which he carefully analyses] rather strongly inclined towards belief in the existence of a southern intercontinental connection between South America and South Africa in the Triassic. To my embarrassment; for in such a 'leftish' position I am disturbed . . . by the company (of bridge builders, radical continent shifters and Gondwanaland collectivists) which this may entail". Dr. B. Schaeffer reports on the freshwater fishes (pp. 227–234), his conclusion—from what is admittedly a scanty record—being that there is no evidence of direct interchange between South America and Africa during any part of the Mesozoic.

In a most valuable paper on "Variables affecting the Probabilities of Dispersal in Geologic Time" (pp. 177–188), Prof. D. I. Axelrod illustrates his views with special reference to the effects of climatic and water barriers on angiosperm distribution. He claims that the continents have been in their present positions since the Middle Cretaceous and probably earlier: "The floras and the faunas, not the continents, have been moving during the past ages". Angiosperms, however, are not decisive for pre-Cretaceous times, and Dr. W. H. Camp's discussion of their phytophyletic patterns (pp. 205–212), though stimulating and suggestive, carries us no farther back. Dr. T. Just deals with the older and more critical fossil floras (pp. 189–204), but he finds the evidence insufficient for the drawing of firm conclusions.

In a brief survey (pp. 213–216) of living invertebrates in relation to Mesozoic South America, Dr. P. J. Darlington, jun., demolishes the idea that they provide any relevant evidence. In particular, he denies that paussids, which have been used as evidence of former land connexions, tell us anything significant about the history of South America. Prof. A. E. Emerson presents a highly original paper on "The Biogeography of Termites" (pp. 217–225) and reaches several tentative conclusions, the first being that "it is unnecessary to postulate any great change in continental masses and their connections since early Mesozoic times".

To end on a personal note: I should confess that, despite appearances to the contrary, I have never succeeded in freeing myself from a nagging prejudice against continental drift; in my geological bones, so to speak, I feel the hypothesis to be a fantastic one. But this is not science, and in reaction I have been deliberately careful not to ignore the very formidable body of evidence that has seemed to make continental drift an inescapable inference. In recent years the weight of evidence has become less oppressive, and this symposium has left me with the general impression that a few land bridges or linkages by island stepping-stones would probably suffice for the biogeographical problems. Matching of orogenic belts on opposing sides of the Atlantic is an exercise that has not been indulged in by the participants of this symposium. I have been collecting evidence from the Pre-Cambrian of South America, Africa[3] and India[4] for several years, and I think it is now safe to say that India cannot have been where Wegener placed it relative to Africa. The evidence on the other side, however, is still ambiguous. A remarkably good 'tectonic fit' between parts of South America and Africa can be recognized, but so far no correlation by age is possible, because the South American age determinations[5] have not been correlated with the tectonics and have not been checked by isotopic analyses. No doubt this temporary frustration will be overcome, and the matter will be decided one way or the other. At least we know how the problem can be settled.

Meanwhile, there remains the most serious enigma of all: the Permo-Carboniferous glaciations. Dunbar points out that the late Palæozoic glaciations in low latitudes present "a problem still unsolved, unless we accept continental drift". But if we accept continental drift only to explain these and other still older glaciations, it becomes no more than an *ad hoc* hypothesis. As such, it may still be justified as a stimulant to research, but it may also stand in the way of progress by distracting attention from the real problem. Can the meteorologists not come to our assistance and tell us whether or not widespread equatorial and low-latitude glaciation is possible while high latitudes for the most part enjoy a genial climate? The curious feature is that the evidence, so far as it goes, suggests that it was the distribution of ice in the Pleistocene that was exceptional, not the Permo-Carboniferous distribution. Southern Africa, in particular, was repeatedly glaciated during Pre-Cambrian times. While so many contradictory voices confuse judgment, one cannot do better than commend Dunbar's wise dictum that "it is unsafe to reject, *a priori*, either continental drift or foundering of broad land bridges".

ARTHUR HOLMES

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THE FARADAY SOCIETY, 1903–53

By DR. F. C. TOMPKINS

ON February 14, 1903, a small meeting took place in the rooms of the now defunct Faraday Club at St. Ermin's Hotel, Westminster, and a new scientific society was inaugurated. A little later "The Faraday Society" was formally founded, having as its objects the promotion of "Electrochemistry, Electrometallurgy, Chemical Physics, and Kindred Subjects". The Society celebrated its fiftieth anniversary this year on April 16 at the Royal Institution, which houses the famous laboratories where Faraday's researches were carried out.

In 1903, as to-day, there were dangers, both financial and otherwise, in creating yet one more society and one more journal. Indeed, Sir William Ramsay viewed the prospects with marked disfavour, for he could not find sufficient time to read the stream of scientific literature already appearing. Despite this, the project made a good start under the presidency of Sir Joseph Swan, ably supported by such eminent men as Prof. Crum Brown, Prof. F. G. Donnan, Lord Kelvin, Sir Oliver Lodge, Dr. Ludwig Mond, Lord Rayleigh, Sir James Swinburne and others. Within a year there were 254 members, with

F. S. Spiers as honorary secretary and editor of the *Transactions*. To these men we must attribute the coining of the phrase 'chemical physics' to describe the borderline field between chemistry and physics. It is no contradiction that in April 1951 the objects of the Society were re-defined as follows: "to promote the Study of Sciences lying between Chemistry, Physics and Biology", for the founders had wisely included that indefinite phrase "and Kindred Subjects". The Society could, therefore, adjust itself as a living organism to its environment. At its inception, the *Transactions* fulfilled the need of a publication catering for developments in the study of metals, both in the more academic and in its technical aspects, as well as one for reporting those studies recognized to-day as the province of physical chemistry.

The first volume of the *Transactions* appeared in 1905; in its 366 pages there are thirty papers, and the path along which the Society was to travel was, even then, clearly defined. The contributions were grouped, usually in pairs, covering different aspects of a common topic. The reading of the papers was followed by a discussion, and the importance of having a permanent record of the comments and criticisms of other scientists was appreciated. The earlier volumes of the *Transactions* contain such a record and it remains a constant source of historical interest and value. The second object which has been carefully nurtured through the subsequent years was the encouragement given to scientists from overseas to present papers; thus we find that, of the thirty papers in the first volume, one originated in the United States and another in France.

The Faraday Society, unlike many other scientific societies, has never held regular meetings devoted to the reading of papers on unrelated topics; rather it has sought to find a new approach to the technique of scientific discussion. The Society can point to the success of its endeavours by reference to its own General Discussions, the hundredth of which is being held in this, its jubilee year. The first, on "Osmotic Pressure", was held in January 1907 in London, and since that time two or more have been held yearly. The purpose of such Discussions was clearly defined—to secure a survey of the topic under discussion from the varied points of view of scientists working along different lines of attack; to focus general interest in a subject with the object of extending its development or applications; to afford an opportunity of bringing forward new facts and theories for criticism and appreciation; and finally to map out possible new lines of investigation. The Society is justly proud of its General Discussions, for even in this era of conferences they still retain a unique character. It had been accepted that the important aspect was critical appraisal of a large proportion of the papers, and practically the whole time of the meeting is therefore devoted to this. All the papers are published in full in the form of preprints two or three weeks before the meeting, so that they may be studied at leisure. A high standard of criticism is consequently attained, and the recognition of the value and quality of the verbal discussion has resulted in authors setting themselves a high standard for their contributions.

The success of these meetings and the achievement of their objects depends primarily on the choice of subject and the timing of the Discussion. In the main, faith has been placed in the national characteristic of compromise—the programme, though limited and well defined in scope, is so arranged that it is of value and interest to the physicist, chemist, biologist, geologist, etc.—that is, to scientists of different disciplines and experiences who bring different points of view to the common theme, and so effect a cross-fertilization of ideas, a synthesis and a stimulus to originality. Moreover, overseas contributions are particularly invited in order to obtain a representative cross-section of authoritative scientists, irrespective of nationality. The Society has, indeed, been fortunate in obtaining their constant support in its endeavour to make these meetings truly international in character.

Looking back, say, twenty-five years, to the time when the theory of strong electrolytes represented a new powerful approach to replace the classical theory of dissociation, we need only turn the pages of the 1921 Discussion volume to illustrate this point. Britain was represented by papers from Bell, Hartley, Wolfenden and Wynne-Jones, while from overseas came such distinguished men as Bjerrum, Brønsted, Fajans, Harned, Hückel, Onsager and Scatchard, and written contributions were received from Debye, McInnes and Randall. Scarcely a name which would spring naturally to one's mind when considering this topic is absent; and how aptly was the time selected. The Debye–Hückel theory appeared in 1923 and Onsager's extension three years later, but their work was still hotly debated. It is no exaggeration to attribute the general acceptance of these theories and the opening up of the modern attack on the problem of short-range forces in solution to this meeting. Again, two years later, in 1929, the overseas members and guests who attended and contributed to the Bristol meeting on "Spectra in Relation to Molecular Structure" included Raman of India, Badger, Birge, Mulliken, Taylor, Wood and others from the United States, Bonnino from Italy, Victor Henri from Switzerland, Grimm, Herzberg, Hund, Schaefer from Germany, Errera from Belgium, and representatives of the best work from Canada, France, Poland and the U.S.S.R.—a veritable galaxy of stars. The Society has done much for the consolidation of theory, the co-ordination of different lines of approach and the stimulus to further work, by timing these Discussions to coincide with the upward surge of interest and of development.

It is dangerous—and often invidious—to pin-point any one contribution as being that from which a marked advance in knowledge and development of the many varied aspects of physical chemistry has evolved. But one may sometimes reflect that, had not a contribution appeared at that particular time, then the road of advance would indeed have been more arduous and the progress along it slower. It is of interest, therefore, to browse through the pages of old Discussion numbers to find whether they record such papers of especial significance.

In 1921, on the subject of chemical kinetics, there was the first suggestion by Lindemann (now Lord Cherwell) of the modern concept of the nature of unimolecular reactions; this led to the well-known Hinshelwood–Lindemann theory and sounded the approaching doom of the old radiation theory. But, almost as if it were in compensation, after the destructive criticism of an application of radiation theory in one Discussion, there appears in a later one a concept of major importance in guiding the spectroscopist and the photochemist in a memorable contribution by J. Franck containing the first enunciation of that broad generalization, the Franck–Condon

Principle. Again, although the Society cannot claim to have published the original concept of chain reactions, its first application to hydrocarbon reactions is given in the 1926 Discussion volume, on "Photochemical Reactions", in a contribution by this year's president, Prof. H. S. Taylor, of Princeton. At another meeting seven years later, Rice proposed his chain mechanism of the thermal cracking of hydrocarbons. Although this concept has now been modified and extended, it is difficult to assess the immense value of this one contribution to many industrial developments. As is always true of any outstanding advance in fundamental theory, its value grows when quantitative deductions are made possible. The recognition of the guiding principle—that of the stationary state—is clearly stated in a paper by the outstanding German chemist, Bodenstein, in the 1931 Discussion. Two years later, Paneth described the synthesis of the antimony analogue of Bunsen's cacodyl radical at a meeting of the Society, and in the same year there were the enunciation of the Franck–Rabinowitch effect and the detailed applications by Hückel and Lennard-Jones of quantum mechanical theories and the birth of the modern language of molecular orbitals and the like.

In quite different fields, the Discussion volumes record equally significant and important discoveries, each of which has largely affected our modern outlook; thus in 1930 Donnan applied to organisms his ionic membrane equilibria; a 1932 Discussion reports H. S. Taylor's concept of activated adsorption and the theoretical contributions of Lennard-Jones on the application of potential energy relationships to adsorption—and there are many more.

Although its publications contain the record of many such important advances in particular branches, yet the Society has never attempted to contract its wide field of interest. The whole of the region lying between physics, chemistry and biology is deemed its province, so that the range covered is possibly unsurpassed in scope and breadth in any similar series of publications. At first there was a bias towards the more technical and applied aspects, with Discussions on fluxes, slags, igneous rock formation, soils, refractories, cements, corrosion, electroplating, passivity and hardening of metals, to list only a few. The later publications are concerned with the more fundamental problems as exemplified by meetings devoted to kinetics, photochemistry, radiation chemistry, electrode processes, spectroscopy, molecular structure, adsorption, chromatography, solid reactions, dielectrics, luminescence, crystal growth, etc. In addition, the growth of colloid science has always been one of especial concern to the Faraday Society. More than any other single body it has been responsible for the development of, and interest in, this subject in Great Britain. Inspired by the enthusiasm of Sir William Hardy, this interest has been constantly maintained, particularly by the efforts of Sir Eric Rideal.

In 1934, the Colloid Committee of the Faraday Society was formed. Broadly conceived, with representation from other interested societies, it received generous help and guidance from the Council of the Society, and one General Discussion every two years was devoted to a subject of interest to colloid chemists. Using the organizing facilities of the Society and with its financial backing, the Colloid Committee went forward, and it is possible to trace the emergence and growth of colloid science in the publications of the Society. Again an initial trend towards aspects having technical value gave place to more fundamental problems; for Discussions on soils, textile materials and the like were followed by others on colloidal electrolytes, the electrical double layer, and the size and shape factor in colloidal systems. More recently, the Society has again taken a lead, this time in fostering the growing interest in biophysics. So long ago as 1930, a Discussion was held on "Colloid Science applied to Biology"; "The Properties and Functions of Membranes, Natural and Artificial" was one of the topics in 1937, and "Modes of Drug Action" in 1943. A pattern was slowly emerging and was thrown into relief in 1948 when the Society, always responsive to modern developments, decided to enlarge the scope and activity of the original Committee by reconstituting it as the Colloid and Biophysics Committee of the Faraday Society, and also to devote one Discussion yearly to topics of interest to that Committee. Such Discussions held over the past few years include "The Physical Chemistry of Proteins", "Lipo-proteins" and "The Optical Methods of Investigating Cell Structure". The Committee also initiates informal meetings on similar subjects of importance to biophysicists and colloid scientists, summaries of which are published in the *Transactions*.

Nevertheless, although the aims and objects of the Society can be well appreciated by a study of its Discussions volumes, the *Transactions* published monthly contribute no less vigorously to the life of the Society. It has been termed the foremost journal in Europe devoted solely to physical chemistry. Although only a hundred and fifty or so papers appear yearly, the lack of size is in part a measure of the high standard required of the contributions.

If the international character of the Discussion meetings is emphasized by the fact that 50 per cent of the papers may be contributed from overseas and that, of the audience of two to three hundred, eighty–ninety are resident outside Great Britain, then, although contributions to the *Transactions* are predominantly from the United Kingdom, yet some six hundred members out of the total of more than 2,200 are from outside the British Isles—some from the Dominions and Colonies, others from all the countries of Western, and some from Eastern, Europe, and many from the United States and the South American continent. On its fiftieth birthday, it is appropriate, therefore, that the president should be Dean H. S. Taylor, who for the past twenty years or more has done so much to enhance the reputation of Princeton University as one of the foremost schools of physical chemistry.

The Society has seen many changes: the original objects with their bias towards electrochemistry, metallography and metallurgy in its more technical aspects have changed, but its efforts in this field have not been unrewarded, because this interest has been taken over by what may be affectionately termed its daughter society, the Institute of Metal Finishing, formerly the Electrodepositors' Technical Society. There is no doubt that other similar changes and broadening of interests will be initiated in the future; perhaps the formation of further daughter societies encouraged by the expanding interests of science may be anticipated.

The Faraday Society has been well served by its members. Because of its generous benefactors, particularly Colonel J. J. Bourke, its financial problems, although not absent, are often less serious than are those of many other scientific bodies, and

because of the enthusiasm of its first secretary, F. S. Spiers, whose name is perpetuated by the Spiers Memorial Lectures (the seventh of which will be delivered this year by Prof. J. H. Hildebrand), and of his successor, G. S. W. Marlow, the Society has never lost its initial impetus.

Its purpose remains to serve the changing needs of those interested in the sciences lying between chemistry, physics and biology, and to foster the international co-operation of scientists. Inspired by this purpose, a General Discussion on "The Reactivity of Free Radicals" was held last year in Canada at the University of Toronto. It proved a stimulating innovation; forty-five members travelled from Great Britain to meet in conference some eighty Canadians and sixty Americans. Those members will long remember their enthusiastic welcome in that great Dominion; they returned richer in knowledge and experience, with an increased appreciation of the high quality of the scientific contribution which Canada is making. The future may see this as the first of many Faraday Discussions to be held in other parts of the Commonwealth, the United States and Western Europe. The Society may hope thereby to contribute still further to "the glory of holding aloft among the nations the scientific name of England", to quote the words of Tyndall in appraising the genius of the man whose name the Society has been proud to assume.

OBITUARIES

Prof. J. W. McBain, F.R.S.

On March 12, one of the great pioneers in the field of what is now termed 'colloid science' passed away. The era of regarding colloids as an interesting field for those who desired to prepare chemical substances in curious and unexpected states was just beginning to pass when, as Leverhulme professor at the University of Bristol, James W. McBain commenced what proved to be his main interest in subsequent years at Leland Stanford University, California, namely, a systematic investigation of the properties of soaps and their solutions. We are indebted to McBain not only for the elucidation of the complex series of phases which these systems can exhibit but also for the concept of the 'micelle', both neutral and ionic, which had the most profound influence on subsequent developments in the fields of colloidal electrolytes. While the actual shape of the micelle in any particular system is still a matter of controversy, we may note that McBain recognized two distinct forms, the spherical and the lamellar. Indeed, he postulated the existence of the latter form, re-discovered in 1947, as early as 1925.

The contributions of the McBain school to the thermodynamic properties and physico-chemical behaviour of these molecularly associated colloidal electrolytes would alone justify the inclusion of his name among the fathers of the science.

McBain's researches in molecular association were not confined to aqueous systems. Apart from detailed studies on gels and jellies in non-aqueous systems, a great deal of pioneer work on what is now termed 'solubilization', for example, of water-insoluble dyes and hydrocarbons by means of soaps, has come from his laboratory; and we are indebted to him for an understanding of the factors operative in what he termed 'co-solvency', which is of great importance in blending.

McBain was one of the first to recognize that the uptake of a gas by a porous solid might involve quite a number of distinct and separate processes taking place simultaneously, and, to cover them all, introduced the term 'sorption'.

On the technical side we are indebted to McBain for many interesting and valuable research methods, such as the McBain sorption spring balance, the McBain air-driven centrifuge, as well as his ingenious methods of skimming off the Gibbs layer in a Langmuir trough, and evaluation of the osmotic coefficient by observations on the depression of the dew point.

McBain's contributions to colloid science consist not only in numerous papers to scientific journals; a number of valuable text-books have also emanated from his pen. These books are distinguished for their clarity of exposition, their style and their broadness in outlook on the topics under discussion. His last book, published in 1950, is designed to give the author's concept of the domain of colloid science. It is interesting to note that he does not limit the field to the properties of interfaces, but considers organization and form as equally important.

On retirement from Leland Stanford, McBain spent five years as director of the National Chemical Laboratory in India, a period which he completed only last year.

McBain had many friends, and he kept them. His character is most clearly revealed by his answer to the query put to him by the writer as to what impelled him to accept Pandit Nehru's offer of the Indian appointment. He replied that he was so impressed by the signs of starvation and low economy of many of the Indians that he thought it necessary for him to try to do something about it, and he believed that the infiltration of science and scientific methods was the only way by which anything radical could be accomplished.

Eric K. Rideal

Father A. Gatterer, S.J.

Father Alois Gatterer, S.J., who died on February 17, was a natural scientist of the best nineteenth-century tradition, student and teacher by turns throughout his career. Born of Austrian stock in 1886, he was educated in Carinthia and there entered the Society of Jesus at the age of nineteen to continue his studies in rhetoric, philosophy and theology. He was ordained as priest in 1915 and joined the staff of the faculty of theology in the University of Innsbruck; there he also studied chemistry and physics, graduated and was appointed to the staff. He was released from lecturing in philosophy in order to pursue his scientific studies. At the age of about forty he studied physics for a year at Oxford and developed an interest in spectroscopy, which he pursued on his return to Innsbruck.

In 1930, when he was forty-four, he was invited to join the staff of the Observatory at the Vatican, where he established and developed a spectroscopic laboratory, largely with the aim of analysing a valuable collection of meteorites bequeathed to the Observatory by a French mineralogist. This laboratory has gradually become excellently equipped, with the aid of grants from Pius XI and his successor.

One of Father Gatterer's first tasks was to purify carbon rods for electrodes: a new method of doing this was developed, and then early tests on meteorites showed up the weakness of the atlases of spectra then available. During 1937–49 one of the main tasks of his laboratory was the preparation of