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C. K. INGOLD: MASTER AND MANDARIN OF
PHYSICAL ORGANIC CHEMISTRY

The Cover...

This issue is devoted to papers presented at
a symposium entitled, "C. K. Ingold: Mas-
ter and Mandarin of Physical Organic Chem-
istry," held at the American Chemical Soci-
ety National Meeting in Chicago, August 24
and 25, 1993, in commemoration of the cen-
tennial of the birth of Professor Ingold. The
symposium was organized by Professor
Derek A. Davenport, who has collaborated
in the preparation of this issue.

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INTRODUCTION

K. U. Ingold, National Research Council of Canada

The following collection of remembrances, anecdotes and comments was presented as a lecture at the 1993 American Chemical Society Fall Meeting in Chicago in a Symposium entitled "Christopher K. Ingold: Master and Mandarin of Physical Organic Chemistry" and organized by Professor Derek A. Davenport. Many of the scientific achievements of C. K. Ingold are recorded herein by his friends, colleagues, (occasional) adversaries, and modern historians. I am

C. K. Ingold

deeply grateful to all of them. However, I have deliberately refrained from contributing anything to this volume except for this introduction. It is true that I gave the opening lecture at this Ingold Centennial Symposium (1893-1993), and my title was a well-remembered quotation of my father's, "Homolysis, even between consenting adults, is grounds for instant dismissal from this Department." Nevertheless, my own memories of my father have relatively little to do with his chemical research. (On one occasion, he helped me enormously with mine by providing a single, insightful comment—not on radicals, of course—on a little problem I had in infrared spectroscopy.) Thus, a eulogy on C. K. Ingold's scientific attainments by his son would be completely inappropriate, particularly since the main contributors to this volume "have done him proud."

I think there is probably only one comment I made during the symposium that may be historically relevant and may not have been "captured." The name Robert Robinson was never mentioned in my hearing at home nor during my undergraduate years at University College London. Indeed, the first time I ever heard this name was when I was in Oxford doing my D. Phil. with Cyril Hinshelwood. If my father bore a grudge against Robinson, he certainly was careful not to pass it on to me by word or deed.

My own memories of C. K. Ingold are, I suppose, the "normal" son/father memories: playing in the garden and on the beach, bird watching together, rock climbing in North Wales, the Lake District, and Skye, and mountaineering with him in France. (Yes, we did climb several of the Charmonix Aiguilles and Mont Blanc.) Finally, there was that classic day when C. K. Ingold water-skied for the first time in his life at age

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72 on the river at the bottom of my garden. In my memory, my father was always young, active, and full of energy and enthusiasm. He always welcomed new knowledge and new experiences. He is still missed.

ABOUT THE AUTHOR

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C. K. INGOLD AT UNIVERSITY COLLEGE LONDON: EDUCATOR AND DEPARTMENT HEAD

Gerrylynn K. Roberts, The Open University

The outstanding scientific work of Christopher Kelk Ingold (1893-1970) was the focus of considerable discussion, celebration and evaluation during the year of the centenary of his birth. In addition to understanding his prolific and highly original scientific output as a pioneer in the application of physical methods to organic chemistry and, indeed, as a founder of physical organic chemistry, it is also important to examine other aspects of Ingold's career, in particular his role in shaping the institutional context in which he developed his work(1). From 1930 until his death, Ingold was attached to a major international centre, the Chemistry Department of University College London. From 1937 to 1961, as its Head and Director of Laboratories, continuing the policies of his predecessor on chemical education and the organisation of research, he developed a very distinctive chemistry department.

Ingold arrived at University College London to take up its second Chair of Chemistry with his reputation already firmly established. The outline of his education and career is well known(2). He began his tertiary education at the Hartley University College in Southampton where he took an external BSc of the University of London in 1913. This was followed by a period at Imperial College London (Associate of the Royal College of Science, 1914), three years working under the War Office for the Cassell Cyanide Company, and then three years as a research chemist for them in Glasgow. He qualified as an Associate of the Institute of Chemistry in 1919 and also gained a University of London MSc in that year. Ingold was keen to resume academic life and, in 1920, he returned to Imperial College as a demonstrator and subsequently a lecturer(3), gaining his DSc in 1921 as well as the first-ever Meldola Medal of the Institute of Chemistry (for a promising

young scientist) in 1923 and becoming a Fellow of the Institute of Chemistry in that year. While at Imperial, Ingold worked with Jocelyn Field Thorpe (1872-1940)(4); his research thrived and, in 1924, he was made an FRS at the very young age of thirty.

In that same year, he became Professor of Organic Chemistry at the University of Leeds, a position which he held for six years. In later years, Ingold would say that Leeds was where(5):

> I did my chief personal *pedagogic* research, finding out by trial and error how to present organic chemistry to students more rationally and less empirically than was formally [*sic*] the custom - as a science rather than an art. (We have done a lot more since on the integration of the branches of chemistry, but that is another story.)

It is that 'other' story with which this paper is concerned.

Early Years at UCL

The UCL chair which Ingold took up had recently been retitled from 'Organic Chemistry' to simply 'Chemistry'. Apparently innocuous, the change is actually quite significant for understanding the outlook of the department which had the famous physical chemist, Frederick George Donnan (1870-1956) (6), as its head. From the time of the reconstitution of the University of London in the early years of the century, the UCL Department of Chemistry had had two chairs: one of General Chemistry and one of Organic Chemistry. It was known late in 1929 that the organic chair would fall vacant in the autumn of 1930 and Donnan took the opportunity to restructure, proposing that both Chairs be retitled simply 'Chair of Chemistry'(7). Because they were technically chairs of the University of London, of which Uni-

versity College London was one of the federated members, such a change had to be approved by the University Senate as well as the College authorities. Speaking as the representative of Imperial College, Ingold's former collaborator, Thorpe, objected strongly, arguing that teaching and research in organic chemistry would be undermined in the University just at a time when the subject was of increasing importance to industry. Donnan was straight with the relevant University Subcommittee; its minutes noted that he proposed the change even if it might mean a temporary diminution in teaching and research in organic chemistry(8):

> ... so that the new Professor may develop the application of physico-chemical concepts to Organic Chemistry. He [Donnan] considered that twenty-five years hence wave mechanics and the electronic ideas will displace what is now known as Organic Chemistry; he [Donnan] states that no chemist exists at present who has done such work but that one man may be capable of beginning to work on these lines.

Donnan himself had promoted this view of chemistry for some years and it would seem that he clearly had Ingold in mind (9). The full Senate did not receive that part of the Subcommittee's minutes, but the extract it did receive clarified Donnan's departmental orientation more generally. It was an orientation which Ingold was to share and further(10).

> University College has every intention of furthering in every possible way the study of Organic Chemistry, and ... it is not intended to replace the Chair by a Chair of Physical Chemistry. The wish of the College is to bring organic chemistry, like chemistry, into a homogeneous science, which may, and it is hoped will, advance on a broad front, making as much use as possible of the results, methods and theories of physical and chemical science. In the past there has been too much subdivision and too much specialization, and there is now urgent need for bringing together in close relation of all these arbitrary

divisions of chemical science. The new professor will be bound to do his utmost to further the progress of organic chemistry. In order to give University College the necessary freedom for progress and advance, both as regards teaching and research in all branches of chemistry, it is desirable that the two Professors be styled simply 'Professors of Chemistry'.
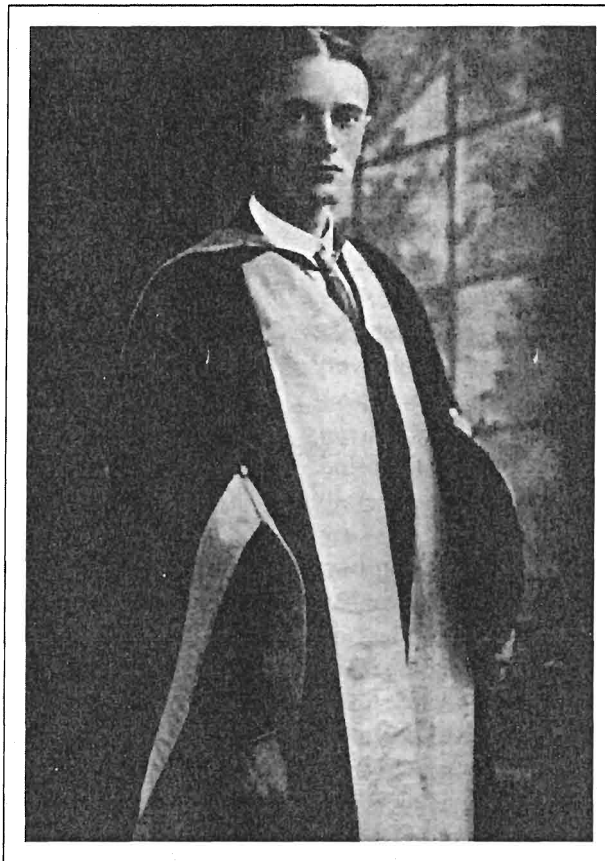
C. K. Ingold, D.Sc., 1921

It was an internationally famous department that Ingold joined, built up particularly on the inorganic and physical side by Sir William Ramsay and by Donnan who had succeeded Ramsay as Professor in 1913 and become Head of Department in succession to J. N. Collie in 1928. Donnan had been largely responsible for re-establishing the department after the First World War, and, perhaps as a consequence, by 1930, he had more or less ceased to be an active researcher himself, though he remained active as an editor and masterly author and his reputation continued to attract students. He was at the height of his powers as an organizer of science, an international figure who enjoyed close relations with industry.

Ingold's first task at UCL was to build up an independent research presence; only one student had moved with him from Leeds(11). Apparently Ingold made it clear to Donnan almost immediately that he wished to deal with the College administration on his own behalf rather than through the head of department(12). Ingold received a start-up grant of £1000 from the College, at a time when the departmental grant was only about £3000 p.a., on the grounds that the organic laboratories had been rather allowed to run down(13). By 1932, an assistant lecturer post in organic chemistry was authorized though, in a move that was to become characteristic of his later administration as head, Ingold extracted a quart of staffing from his pint of resource by appointing his two star researchers (C. L. Wilson and E. D. Hughes) to senior demonstratorships instead, arguing that there was

a very restricted field at the time, and it would be better to wait for the right person to fill the lectureship(14). It is thus hardly surprising that Ingold was successful in establishing a separate identity at University College London and by the middle 1930s, students recognized two very definite 'sides' to the department(15). At the same time, it is also remembered as having been a remarkably harmonious, open and mutually supportive department(16). Publications flowed steadily. Ingold, by contrast with Donnan, was an extremely active researcher as an individual, and was not linked in with industry in the same intimate way. In terms of research style, Ingold's students worked more as a group; that is, several of them tackled particular projects focussed on an overall goal towards which Ingold was working, a marked contrast with Donnan's *laissez-faire* style (17). At the same time, Ingold planned his researches like long-term strategic campaigns, advancing across a very broad front. However, Ingold's personal style as a supervisor was apparently similar to Donnan's(18).

As a doyen of British chemistry in this period, Donnan was an accomplished fund-raiser, especially from Imperial Chemical Industries and from the Government, and his was by far the larger of the two groups at UCL so far as senior researchers were concerned. But as Donnan approached retirement, the department entered a period of instability. Considerable funding which the department had enjoyed from the ICI Research Council came to an abrupt end on 30 June 1936 when ICI closed down its UCL laboratory(19). So when Ingold became Head of Department and Director of the Laboratories by a process of 'natural' succession in October 1937, he took over a department in much reduced circumstances (20).

An Overview of Ingold's Headship

Snapshots of the department's staffing, funding, student numbers and numbers of publications at the beginning and end of the period 1937-38 to 1960-61 provide an overview of the institutional aspects of Ingold's headship. The number of academic staff increased markedly and steadily from fifteen to twenty-seven full-time members, including five professors and five readers, plus fourteen honorary members of staff in the latter session(21). Furthermore, the academic staff were much better supported by technical staff in 1960-61, with a third of the staffing budget devoted to the latter in that academic year. The size of the establishment does not provide a full account, particularly of the research complement, as UCL was characterized by a steady stream of research visitors who brought their own funding(22).

Throughout Ingold's headship, chemistry had the highest internal grant in the College, increasing from £13,800 to £125,300(23), a long way ahead of all other departments. Although by 1960-61 physics was better-funded overall owing to much higher external grants. Interestingly, despite the fact that Ingold applied fairly steadily for funds to ICI, the Chemical Society, the Royal Society, the University of London and, in later years, to the Department of Scientific and Industrial Research, external grant income was not particularly high for the department(24). Instrumentation was the main focus of bids; an £1800 grant was received for an infrared instrument in 1937-38, while the equivalent figure in 1960-61 was £24.5k for a mass spectrometer. While an improvement on the absence of funding in 1937-38, the amount for externally-funded staff in 1960-61 was in fact very little different from that in 1935-36 when the department had enjoyed ICI funding under Donnan. The period was marked by an escalation in costs caused partly by changes in chemistry itself as it became more dependent on expensive instruments, while a push from government for expansion in student numbers, with no commensurate funding, also affected costs. Throughout the 1950s, the department experienced continual financial difficulties, as did the College as a whole(25).

Although not an avid committee-person, in addition to those committees on which he served *ex officio*, Ingold took care to participate in the key decision-making and funding committees in the College(26) and in the University. Such was Ingold's administrative style, that colleagues were shielded from financial concerns. As one department member put it, the money simply turned up(27). Ingold was a centrist in administration, taking most major decisions himself, though E. D. Hughes (1906-1963)(28) did many of the routine tasks.

Again comparing the first and final years of Ingold's headship and considering the growth achieved in staffing and funding, the continuity in student numbers(29), both undergraduate and postgraduate, is remarkable. In 1937-38, 39 students earned BScs, when 13 Ph.D.s were awarded. The figures for 1960-61 are 38 and 15, respectively. The continuity in the number of publications by department members is also striking. The department, as a whole, published 94 papers in 1937-38, of which 9 were by Ingold. In 1960-61 the deparment published 100 papers, with 7 by Ingold(30). A more detailed inspection of the intervening period shows, however, that this apparent continuity was actually a major achievement because of the extreme disruption caused

by the war which began only two years after Ingold assumed the headship.

The UCL chemistry department continued to operate throughout the war, but it was evacuated in two parts to Wales causing numerous administrative difficulties, not least in liaising with the host institutions. The BSc General Degree students went to Bangor while the BSc Special Degree students and the post-graduates went to Aberystwyth, where Ingold also went. Both parts had skeletal staffing because several academics undertook war work elsewhere. A hope of moving back to London after the first year was thwarted when the College was bombed, the department sustaining serious damage. Thus, after the war, Ingold faced the major task of rebuilding the department physically, as well as replacing a number of well-established staff who, having departed for war work, found other employment afterwards. Although it arose out of adversity, and may not have been seen as such at the time, this in fact gave Ingold an opportunity to develop his own staff team, since a number of Donnan's senior people were among those who left(31). To rebuild the staffing complement rapidly, Ingold recruited heavily from students of his own research school, sometimes in a rather informal manner(32). Of seventy new members of the teaching staff who joined the department during Ingold's headship from 1937 to 1961, forty-eight (68%) were former students of the department and thirty of those had done both their BSc and their PhD there. Even discounting thirty-two (22 UCL PhDs) of the seventy who were on the staff for only a single year as temporary assistant lecturers, internal recruitment was a marked feature of the department under Ingold's headship(33).

Ingold's Policies - Reform of the London BSc

During his tenure as Head, except for distancing himself from industry, Ingold to a large extent carried on Donnan's policies. In particular, Ingold brought to fruition the reform of the London BSc in Chemistry long advocated by Donnan and he also worked on the expansion of the Department in both the undergraduate and the postgraduate areas.

With regard to the BSc, the UCL Department had been from the 1920s in almost constant negotiation with the University of London. Under the regulations in force when Ingold took over, the syllabus and most examinations were set by the University, while the teaching was done in the federated schools and colleges. UCL sought greater autonomy at the College level. Donnan had pressed for the Colleges to set their own papers for the degree examination arguing that the University syllabus did not allow scope for the emergence of local approaches. In keeping with Donnan's view of the primary importance of physical chemistry, the overall thrust of the UCL campaign was to emphasize the importance of physics for the student of chemistry. After two years of debate, the Colleges were finally permitted in 1928 to set for a pilot four-year period two out of the six examination papers themselves, one in physical and one in organic chemistry(34). In 1931, six years after UCL's initial suggestion that all practical examinations be College-based, it was finally agreed that this could be permitted in the case of organic chemistry(35).

In October 1932, Donnan, Ingold and O. L. Brady(36) became members of a subcommittee of the University's Board of Chemical Studies to consider the revision of the BSc. It reported a year later that the London Special BSc, which was a two-year degree, after an initial foundation year known as the Intermediate BSc year for which an increasing number of school final examinations gave exemption anyway, was far too overloaded to cover both the fundamentals of chemistry and contemporary developments. Students often concentrated on the latter at the expense of the class of their degree. The proposal was that the Intermediate BSc year be dropped from the degree to allow the teaching for the Special BSc degree to be lengthened to three years(37). This change was rejected by the Academic Council of Senate to which the Board of Chemical Studies reported (38). Five years later in March 1939 (such was the speed with which the Colleges reached agreement), some modest changes in the distribution of papers among the subjects were agreed(39). Ingold immediately used the changes as an argument to win some funding for a modest expansion of the UCL chemistry buildings(40), but implementation was soon suspended owing to the war(41).

This suspension gave an opportunity for those who favoured the change to three years to reopen the issue, and it was hotly debated during the war; Ingold was active in the debate throughout. His influence on the new degree and the continuity with Donnan's emphasis is clear. Explaining the new rationale, Ingold said(42):

> A revolution has occurred in chemistry in my lifetime; and it is continuing and cannot be resisted. When I began, chemistry was almost wholly a mass of empirical observation with a little regularity, but without either reason or coherence.... Today, however, the outlook is quite changed. The whole of chemistry is bound together and rationalised by physical principles;...Thus the hit-or-miss empiricism is being limited, and a scientific method built by which

results can be achieved more quickly and with less wasted effort than before.

It is part of this development that the barriers between the conventional divisions of chemistry are breaking down to the point at which while many problems cannot be assigned to one division only, there are hardly any problems assignable to one division which cannot be assisted by the ideas associated with another.

The regulations that were to stay in force until after his retirement were put in place. The BSc Special Degree became a three-year course with students being required to take four course-years worth of ancillary subjects which had to include at least one year of mathematics and two years of physics(43). Thus physics and mathematics became central. The fourth course-year could be either botany or geology. To Ingold, the aim of the BSc was to prepare students to undertake research, preferably his own students in his own department(44). A number of chemists expressed considerable scepticism about the value of the new degree for training chemists for industry. Indeed, Ingold saw the chemistry degree as a 'foundation' for future study and resisted collaboration with either Chemical Engineering or Biochemistry at the undergraduate level(45).

The Unity of Chemistry

Ingold was very consistent in developing the educational principles outlined above. They applied to research as well as to undergraduate teaching and their implementation can be seen in his staffing policy. On the eve of taking up the headship, he made this policy very clear when making the case for his choice of Donnan's successor(46). Ingold argued that the Chemistry Department should be run as a whole, not in three separate branches of inorganic, organic and physical chemistry. Most members of the department were specialists, but their specialisms crossed the three branches and 'there is frequent transgression of boundaries'. The incumbent would be expected to give a considerable number of lectures to post-graduate students, to participate in seminars, give private information and advice to independent research workers and expert assistance to colleagues. Therefore it was necessary to have a spread of expertise in the department, both in terms of fields and in terms of techniques. Despite his view of the unity of the three branches, an inorganic chemist with a theoretical emphasis was sought, because organic chemists currently there, he argued, were actually very physical in their outlook(47).

The most important branches of theory which require to be represented in this way are quantum mechan-

ics, statistical mechanics, thermodynamics, the theory of spectra, of solutions and of reactions. Actually the representation of quantum mechanics needs improvement, but the Department is rather well provided in all the other subjects at the present time. The branches of technique at present practised include infra-red, Raman and ultraviolet spectroscopy and electron and X-ray interferometry.

So at the very start of his headship, Ingold stated his vision of the Department as an intellectual unity, but also as a wide-ranging collection not only of knowledge, but of techniques on which all individuals should be able to draw (48). And the recruiting that Ingold did was to continue to follow this pattern of balancing fields with techniques in order to make sure that new developments in chemistry were available to the research effort as well as to spread the expertise for teaching. He also tended to seek out specific individuals, as was the practice at the time.

For example, in 1945, he wanted to add an X-ray crystallographer to the staff and invited G. A Jeffrey to consider the post. Although Jeffrey, feeling .that Ingold wanted him to provide a service rather than work on the development of X-ray crystallography(49), preferred to take an appointment at Leeds, he expressed himself willing to assist in the post-war re-establishment of X-ray work at UCL(50).

On the other hand, if you intend to wait until crystallographers are a little more plentiful and there are problems such as the one you mentioned which you would particularly like tackled in relation to your own work, then I am quite free, in fact encouraged, to consider some form of cooperation between us.

Shortly afterwards, in a change of tack, instead of seeking someone in the early stages of an academic career, Ingold recruited an established scholar who would have independent research interests but might be expected to contribute to the training of a new pool of expertise. In 1947, Kathleen Lonsdale, already an FRS, joined the Department as Reader in Crystallography. Ingold very much admired her early work on the structure of hexamethylbenzene and hexachlorobenzene which was done while they were both at Leeds. That seemingly anomalous appointment, anomalous because she was a physicist not a chemical crystallographer, is explained by Ingold's policy of recruiting to have a balance of expertise to draw on, including for his own research(51).

It is considered that a general chemical research institute, having the balance and coherence at which the Department of Chemistry aims, should carry a

strong crystallographic school.... The established research activities [here] are such as to illuminate, and would themselves be enriched by, many of the interests which might be expected to develop in a strong crystallographic section.

Furthermore, it is expected that a crystallographic section, such as that contemplated, would also develop as a teaching unit, directed to the production of trained crystallographers, for which the demand is considerable, whilst training facilities are inadequate(52).

Later, when Ronald Nyholm(53) an established and innovative inorganic chemist, was recruited back to the department as a Professor to boost research in inorganic chemistry and to reform the teaching of the subject, a desideratum which had been signalled at the time of the reform of the London BSc(54), Ingold himself was keen to pursue with him some work on inorganic reaction mechanisms, by analogy with organic reaction mechanisms(55). Another strategy for bringing new techniques to the department was to send members of staff to departments elsewhere to learn them. For example, Clifford Bunton was sent to Columbia in 1948 to learn about the latest developments in mass spectroscopy(56).

Ingold, however, gave colleagues scope to pursue their own interests even when they diverged from his(57). He also took a great interest in their welfare at a personal level. In 1956, for example, he was concerned about the financial circumstances of some junior staff with young families and he feared their loss to institutions elsewhere or to academia altogether. Quoting US precedents, Ingold arranged with Sir Sydney Barrett, a Director of Albright & Wilson and former member of the UCL staff in Donnan's time, for a grant of £400 per year over three years to be used discretely to supplement the salaries of certain key staff(58).

Ingold also expounded his educational outlook in response to a request by the College to each Department early in 1944 for post-war reconstruction plans. In contrast to the modest efforts of some other beleaguered heads, Ingold submitted a twenty-page statement on the 'Future Needs of the Chemistry Department'(59). Regretting that, before the war, they could only take about 30% of their own BSc Specials on to do research, what he called for was an expansion in students, staff and accommodation(60). The argument contained an important statement of educational principle as regards research training(61):

... it has never been the practice in this particular Chemistry Department to foster 'mass-produced' research (where large numbers of students contribute to a single problem, in which methods are standard,

the results more or less assured, and the students little more than a pair of hands). No criticism is implied of this method of organizing research in some subjects (e.g. chemotherapy): it's merely felt that it does not provide the best training for students. Thus [here] each research student means a special, time-consuming, thought-occupying problem for some member of staff, who has personally to take the main burden of carrying out those parts of his students' research which demand considerable experience (e.g. the development of new technique, or the reduction of data), besides doing the more routine work of daily advice and supervision. There is literally no end, except that of exhaustion, to this work, which makes such heavy demands on the time and strength of the staff that it is felt essential, not only to increase the ratio of research students to staff, but rather to provide for an increase in research students which is proportionately smaller than the increase of staff recommended.

Among the specific requests for expansion were: the elementary lab agreed before the war, facilities for between five and eight new lecture and practical courses, plus facilities for research training for twenty more postgrads. In addition, he called for the institution of a third chair in chemistry, arguing that despite the discussions at the time of the filling of the last professorial vacancy, there was still no theoretician on the staff [though, in a characteristic piece of Ingold administrative argument, if a chair were established, he would wait for the right person to appear and not necessarily appoint a theoretician(!)]. From the point of view of staffing, a third established chair was needed, he argued, to give a proper lead to research because so much of the time of the head, at least 75%, was taken up with administration, plus consequent college and external roles, that research time was severely squeezed. In addition, five more full-time staff in other grades, adequate funding for student demonstratorships and the incentive of internal promotions, plus twelve extra non-academic staff were required to meet modern technical demands(62).

Furthermore, although divergent views have been held with the relation of academic science to industry, the view of the Chemistry Department must be recorded that, not only for the sake of its students, but also for the good of the country, it must maintain and even strengthen, its connexion with chemical industry, subject only to the principle that its senior staff should concern themselves only with the strategy of research, the tactics being primarily the business of the Department's more able old students.

Broadly, the plans were fulfilled. The third chair was finally established in the College's estimates for the

1948-53 quinquennium as part of a balanced expansion of the chemistry department to cope with the government-instigated lengthening of the curriculum and the addition of specialist courses with a view to greater research productivity and a higher output of trained scientific personnel. It was noted as part of the case that all other major departments in the country had three chairs, so UCL had in fact been behind. (Of course, in most departments, the three chairs would have been assigned to the three different branches and been run almost as separate fiefdoms.) Ingold nominated to the post his long-time collaborator who then held a Chair at Bangor, E. D. Hughes, to cover organic chemistry so that Ingold could be free to concentrate on physical chemistry. Such was the coverage of theoretical chemistry which the chair had been pencilled in to secure(63).

Thinking that he would be retiring in 1958, Ingold took steps early in 1957 to create more time for research by easing himself away from some of the administrative burdens of the headship, transferring them to Hughes(64), and thus recognizing formally a situation that existed *de facto*(65). Ingold was still very much in charge, however, and carried on with certain aspects of administrative work. Early in 1958, he fronted a case for a completely new chemistry building(66), that which was eventually opened in 1970 as the Christopher Ingold Laboratories. Again the context was one of desired expansion. Accommodation limits, he argued, meant that a maximum of 144 undergraduate chemistry special students plus 96 ancillary students and 122 research workers could be accommodated in their very outdated building which was unsuitable for running modern equipment in an instrument- dependent science. In the heady days of science expansion, he wanted to increase the undergraduate numbers to 240 specials with 375 ancillaries and a research complement of 200 (150 of whom would be postgraduates).

Here again was an opportunity for a re-statement of educational principles, which had remained remarkably constant. The UCL system, which had been in place since Donnan's time, was that all researchers attended the advanced lectures given by senior colleagues, plus the colloquia given by students and visitors at various stages of research. There were several concurrent series. (Ingold's pointed and precise interventions at colloquia were keenly anticipated by students, who both feared and admired them(67).) There was, he argued, research going on in all the main areas of chemistry and this policy of mutual support encouraged much cross-fertilization. 'Massively organized research teams are avoided as lacking educative features which diversity

can provide'. Ingold retired formally from his Chair of Chemistry and Directorship of the Laboratories in the summer of 1961, becoming Professor Emeritus but he retained a role as a special lecturer 'to keep the chair warm for a successor' as he put it(68).

After retirement, Ingold continued to expound his educational philosophy as an advisor to new universities abroad and in Britain(69). In the early years of his retirement, on the occasion of his visiting the about to be independent University of Ibadan in Nigeria, he delivered a lecture called 'The Education of a Scientist'(70). Citing a world transforming scientific revolution over the preceding twenty-five years, he argued that there were consequences for scientific education. Some scientific understanding would be necessary for all and thus reform would have to start in the schools where science and the study of modern languages should be fundamental. Thus schools would have to be less specialized. And he also argued for a year between school and university, a sort of foundation university year, where students could be weaned from exam coaching to truly independent study.

At university level, Ingold argued for what he called, in a phrase that will find resonance in the current university climate, 'live' science teaching, that would be done by active research workers who could convey the excitement and wonder of science's continual unfolding and dynamic development. The best researchers were the best teachers, he declared. Furthermore, a student had to have technique, which meant studying with a master for several years of research under personal guidance. To be really effective, the mature scientist had to work as part of a group pooling expertise as no one could expect to master individually an adequately wide field given the complexities of modern science. It was a case of cross-fertilization.

Although, in this talk he argued that educational changes were necessary, the educational views he espoused in terms of departmental organization showed a remarkable continuity with those he articulated at the very start of his headship. The UCL chemistry department in Ingold's period was unique in Britain in being organized around the concept of the unity of chemistry(71). And the reason for its uniqueness is perhaps best understood through some advice which Ingold gave to the then fledgling University of East Anglia in 1962(72).

> As a purely personal opinion on this subject, I would suggest that, while the Departments of Mathematics, Physics and Chemistry are young, each should be built up with a sense of unity by and around <u>one</u> pro-

fessor, who should be a man sympathetic towards and interested in the whole of his professed subject, even if he himself works on only in one corner of it. [sic] As the departments and staff are enlarged, it will surely be necessary to bring into some of them a second and perhaps a third professor: but this should not be done until the original professor feels the need of a colleague of equal stature; and when it is done, the big Department itself should remain single and united, with one of its professors designated as the administrative head. The reasons for these recommendations are, (a) that the fewer the frontiers, the more fruitfully and economically can the available resources in space, equipment, and money be used; and (b) that, given a wise head, the fewer the frontiers, the stronger and more scientifically effective is the collaborative spirit that can be developed.

With his truly astonishing breadth and depth of scientific understanding and strong personality, Ingold was one such wise head.

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REFERENCES AND NOTES

1. This paper is a somewhat abridged version of one which appeared in The British Journal for the History of Science, 1996, 29, 65-82. I am grateful to the British Society for the History of Science for permission to reprint. Earlier versions were presented to the Royal Society of Chemistry, Annual Chemical Congress, Historical Group Symposium H/1 "C. K. Ingold (1893-1970): Aspects of a Life's Work," University of Southampton, April 6, 1993; and to the American Chemical Society, National Meeting, Historical Division Symposium, "Christopher K. Ingold, Master and Mandarin of Physical Organic Chemistry," Chicago, August 24, 1993. I learned a great deal from participants in these symposia as well as from participants in the Royal Society of Chemistry, Perkin Division, "Ingold Centenary Symposium," which was held in the Christopher Ingold Laboratories of University College London on the date of the centenary of Ingold's birth, October 28, 1993.

2. C. W. Shoppee, "Christopher Kelk Ingold, 1893-1970," *Biogr. Mem. Fellows R. Soc.* **1972**, *18*, 349-411.

3. J. F. Thorpe to Mr. Gow, September 24, 1919. The initial arrangement was that Ingold would be given three years' leave from Cassell, J. F. Thorpe to the Rector, October 11, 1919. [Professor J. F. Thorpe, Correspondence 1915-1919, Imperial College London Archives, KC/9/7]. Ingold's enthusiasm extended to expressing willingness to accept a £100 (25%) cut in annual salary (H. B. Baker to Mr. Gow, September 23, 1919). [Professor Baker, Correspondence, 1912-1932, Imperial College London Archives, KC9/6].

4. G. A. Kon and R. P. Linstead, "Sir Jocelyn Field Thorpe, 1872-1940," *J. Chem. Soc.*, **1941**, 444-464; C. K. Ingold, Jocelyn Field Thorpe 1872-1939," *Obit. Not. Fellows R. Soc.*, 1939-41, 3, 531-543.

5. Quoted in F. C. Challenger, "Schools of Chemistry in Great Britain and Ireland IV - The Chemistry Department of the University of Leeds," JRIC, 1953, 71, 161-171, on p. 166.

6. F. A. Freeth, "Frederick George Donnan, 1870-1956," *Biogr. Mem. Fellows R. Soc.*, **1957**, *3*, 23- 39.

7. University College London, Minutes of the Professorial Board, November 26, 1929, item 58 and 28 January 1930, item 106; University College London Records Office.

8. University of London, Report of a Meeting of the Academic Council of Senate Subcommittee in Science, February 24, 1930 in University of London, Academic Council of Senate Minutes, March 3, 1930; University of London Archives, AC 1/1/30.

9. C. W. Shoppee, Ref. 2, 355, mentions a rumor that Donnan would have preferred someone other than Ingold, but this is difficult to reconcile with Donnan's person specification.

10. University of London, Report of a Meeting of the Academic Council of Senate Subcommittee in Science, 1929-30, February 24, 1930 in Senate Minutes, 1929-30, March 26, 1930; University of London Archives, ST 2/2/46.

11. Professor H. J. Shine [UCL: BSc 1945], Texas Tech University, Personal Communication, April 11, 1996, reported a conversation in which C. L. Wilson indicated that only he had made the move. It was at this time that E. D. Hughes joined Ingold from Wales.

12. C. K. Ingold to Provost, October 22, 1931; University College London Records Office, Chemistry Miscellaneous Files, File 31/3/6.

13. UCL Finance Secretary to F. G. Donnan, December 2, 1930; University College London Records Office, Chemistry Appointments, File 31/4/4. (This may of course have been merely a bargaining ploy; it would be difficult to conclude from it anything about the state of the laboratories during Collie's final years and Robinson's brief tenure.) UCL Secretary to C K. Ingold, December 2, 1930; *ibid.*

14. C. K. Ingold to the Provost [Allen Mawer], September 27, 1932; University College London Records Office, Chemistry Appointments Files, File 31.

15. Dr. D. W. Gillings, formerly of ICI Central Research Instruments Laboratory [UCL: BSc 1936, PhD 1938], Interview, March 30, 1993; Dr. T. P. Nevell, formerly of UMIST [UCL: BSc 1936, PhD 1939, DSc London 1967] and Mrs. V. Nevell, formerly a school Head of Science [UCL: BSc 1938], Interview, April 17, 1993.

16. For example, Dr. Catherine [Tideman] Le Fevre, formerly of the University of Sydney [UCL: BSc 1931, MSc 1952, DSc], private communication, July 21, 1994.

17. Dr. D. W. Gillings, Interview, Ref.15. Dr. J. H. S. Green, formerly of the National Chemical Laboratory [UCL: BSc 1950, PhD 1953], Interview, June 7,1993.

18. Dr. G. S. Hartley, formerly Director of Research at Pest Control Ltd (Fison's) [UCL: BSc 1927, DSc 1937], Private Communications, April 1,1993 and September 26, 1994. A Donnan researcher from 1927 to 1932, Hartley feels in retrospect that he was left very much to his own devices and would have appreciated more active supervision. Dr. J. L. Moilliet, formerly of ICI Dyestuffs Division [UCL: PhD 1932], Interview, April 22, 1993, felt the same. Ingold's similar style was noted by students from the 1940s and 1950s; Prof. Alwyn Davies of UCL [UCL: BSc 1946, PhD 1949, DSc], Interview, 1990; Prof. D. J. Millen of UCL [UCL: BSc 1943, PhD 1947] and Prof. J. H. Ridd of UCL [UCL: BSc 1948, PhD 1951], Interviews, May 18, 1993; Dr. D. Davenport of Purdue University [UCL: BSc 1947, PhD 1950], " Gradus ad Parnassus: The Faraday Society Discussions of 1923, 1937 and 1941," ACS National Meeting, Ref. 1, August 24, 1993. This should of course not be taken to imply that Ingold was not aware of students' work. See J. H. Ridd, "Sir Christopher Ingold," J. Chemical & Physical Society (UCL), 1973, 4, No. 2, 15. Certainly from the 1940s, E. D. Hughes acted in some instances as a kind of "gatekeeper" for Ingold as, in effect, did Ingold's wife, Edith Hilda Ingold; Prof. H. J. Shine, Interview, August 24, 1993; Dr. J. H. S. Green, Ref.17; Prof. B. Challis formerly of The Open University [UCL: BSc 1957, PhD 1961], Interview, May 24, 1993.

19. R. E. Slade, Proposals Re University College, March 11, 1936, ICI Millbank Archives, Box 96, ICHO/RES/ 0075. See Gerrylynn K. Roberts, "Dealing with Issues at the Academic-Industrial Interface in Interwar Britain: University College London and Imperial Chemical Industries," Science and Public Policy, **1997**, 24, forthcoming.

20. University College London, Minutes of the College Committee, June 1, 1937; University College London Records Office.

21. Compiled from University College London, College Calendar, 1937 and 1960.

22. During the period of Ingold's headship, 191 'registered' research visitors came for various periods; Record Cards held by University College London, Chemistry Department. Data compiled by Janet Garrod at the Open University for Gerrylynn K. Roberts, "Chemists Prosopography Project." There were also numerous unrecorded very short-term visitors.

23. Compiled from University College London Records Office; University College London, Minutes of the College Committee, Appendices, 1937-38. For 1960-61, see University College London, Minutes of the Standing Committee of Professorial Board on Departmental Grants, May 25, 1960 and April 27, 1961. As an indicator of relative monetary values over theperiod, professorial salaries roughly trebled during these 31 years.

24. Kathleen Lonsdale, from 1947, and Ronald Nyholm, from 1955, had a marked effect on grant income as they were both active in applying for external grants to support students and post-doctoral researchers as well as for the expensive equipment required by their respective researches. See University College London Records Office, Miscellaneous Grants Files, *passim*.

25. University College London Records Office, University College London, Minutes of the Standing Committee of Professorial Board on Departmental Grants, *passim*. For a specific example, see C. K. Ingold to Provost, February 11, 1955; University College London Records Office, Chemistry Miscellaneous Files, Chemistry Departmental Grant, File 31/3/37.

26. 1934-61, Academic Staff Appointments and Promotions Committee; 1934-37, Grants and Loans Committee; 1937-40, College Committee; 1943-59, Planning and Building Committee; 1950-60, Finance Committee; 1952-61, Departmental Grants Committee; 1943-50 and 1952-61, Technicians Committee. University College London Records Office, Staff Record Card for Sir Christopher Kelk Ingold.

27. Prof. D. J. Millen, Ref.18.

28. C. K. Ingold, "Edward David Hughes 1906-1963," *Biogr. Mem. Fellows R. Soc.* **1964**, *10*, 147-182.

29. BScs compiled from Pass Lists published annually by the University of London, and PhDs from annual issues of University of London, List of Higher Degrees Awarded. The BSc figures include only those taking the Special Degree, not the General Degree. Ignoring three aberrant years during the change-over in London regulations, the UC department's graduates amounted to roughly 18% of the total number of Special Chemistry BScs of the University of London, with the later 1930s and later 1950s having figures on the higher side of the average. The PhD figures fluctuated rather more from the average of 17%, with generally larger numbers overall from the 1950s onwards; "Chemists Prosopography Project," Ref. 22).

30. Compiled from University College London, Calendar or Annual Report, depending on the year. Ingold preferred to publish papers in blocks which tackled a problem from a number of different angles. See C. K. Ingold to R. E. Slade, October 6, 1941; ICI Millbank Archives,

Box 447, ICHO/CFD/4472(ii); and "Report on Grant for Apparatus and Chemicals Session 1942-43," C. K. Ingold to R. E. Slade, September 13, 1943; ICI Millbank Archives, Box 447, ICHO/CFD/4472(iii). This caused occasional frustration among research students who might have a considerable wait before seeing their work in print; Ridd, Interview, Ref. 18. This policy also allowed Ingold to minimise and dodge criticism by publishing fairly comprehensively on topics.

31. C. F. Goodeve, G. S. Hartley, R. J. Le Fevre, O. J. Walker, and C. L. Wilson all went elsewhere. Of these, Wilson was the only "Ingold" appointment. Some had already been looking for other posts before the war.

32. R. J. Gillespie of MacMaster University [UCL: BSc 1944, PhD 1949], "One Hundred Years of Superacid Chemistry," ACS, National Meeting, Ref. 1, August 25, 1993 and RSC Perkin Division, "Ingold Centenary Symposium," Ref. 1, October 28, 1993; Ridd, Interview, Ref. 18.

33. J. H. Ridd, "Sir Christopher Ingold," Ref. 18 and "Chemists Prosopography Project," Ref. 22.

34. University of London, Minutes of the Board of Chemical Studies, March 25, May 6, June 8, 1926; January 27 and June 8, 1928. University of London Archive, AC 8/11/1/2.

35. The UC initiative is recorded in University of London, Minutes of the Minutes of the Academic Council of Senate, October 13, 1924. University of London Archive, AC 1/1/25. For the final decision, see University of London, Minutes of the Board of Chemical Studies, June 5, 1931. University of London Archive, AC 8/11/1/2.

36. "Oscar Lisle Brady (1890-1968)," *Chem. Brit.* **1968**, *4*, 554, was then reader in Organic Chemistry at UCL.

37. University of London, Minutes of the Board of Chemical Studies, October 21, 1932; December 15, 1933. University of London Archive, AC 8/11/1/2.

38. University of London, Minutes of the Board of Chemical Studies, July 22, 1942. University of London Archive, AC 8/11/1/3.

39. University of London, Minutes of the Board of Chemical Studies, March 17, 1939. University of London Archive, AC 8/11/1/3.

40. University College London Records Office, Minutes of College Committee, July 4, 1939, Appendix X.

41. University of London, Minutes of the Board of Chemical Studies, January 26, 1940. University of London Archive, AC 8/11/1/3.

42. C. K. Ingold, "Education in Chemistry: The Aim of the Revised Regulations in Science Degrees," *Brit. Chemist*, **1953**, *35*, No 3, 2-4, on p. 3.

43. University of London, Minutes of the Board of Chemical Studies, July 22, 1942; July 14, September 21, and November 4, 1943; May 4, 1944. University of London Archive, AC 8/11/1/3.

44. C. K. Ingold, "Education in Chemistry," Ref. 42.

45. See *Brit. Chemist.*, **1953**, *35*, *passim*; Professor M. McGlashan of UCL, Interview, November 14, 1993; Professor B. E. C. Banks of UCL Department of Physiology [UCL: MSc(Biochem) 1956, PhD 1959], Interview, June 14, 1993.

46. The College's View on the New Chair, late 1936- early 1937, University College London Records Office, Chemistry Miscellaneous Files, File 31/3/5.

47. University College London Records Office, Chemistry Miscellaneous Files, File 31/3/5. As regards quantum mechanics, Ingold expressed disappointment that Edward Teller had left after only a short spell in the department as a refugee in the 1930s. See Edward Teller, "A Word of Thanks," *J. Chemical and Physical Society* (UCL), **1976**, *4*, No. 3, 16.

48. In constructing his ICI-funded research programme, Donnan had similarly sought to build a team of varied expertise across fields and techniques which could be shared. Among the principal researchers were experts in X-ray crystallography, electron diffraction, the reactions of active nitrogen, high energy discharges, colloids and surface phenomena. A current member of staff's spectroscopic expertise was also cited. "Proposed Organisation of Scientific Work at University College Chemical Department by Members of the Research Staff at Imperial Chemical Industries, October 1928, ICI Millbank Archives, Box 321, ICHO/CFD/0395.

49. Professor G. A. Jeffrey of the University of Pittsburgh, Private Communication, August 13, 1993. Jeffrey was at the start of his career. He had done post-graduate work in the late 1930s and worked during the war for the British Rubber Research Association.

50. G. A. Jeffrey to C. K. Ingold, August 27, 1945, University College London Records Office, Chemistry Appointments Files, File 31/3/9. The offer of collaboration was not, in the event taken up, see Ref. 49.

51. Prof. H. J. Milledge of UCL Department of Geology [UCL: PhD 1951], Interview, November 17, 1993. Prof. Milledge notes that Lonsdale certainly did no service work for the department and, except for some joint research with Professor Craig, her interests were quite separate.

52. "Statement of the Case for the Establishment of a Readership in Crystallography in the Department of Chemistry," University College London Records Office, Minutes of the College Committee, June 4, 1946, Appendix X.

53. "Sir Ronald Sydney Nyholm, 1917-1971," *Chem. Brit.*, **1972**, *8*, 341.

54. C. K. Ingold, "Education in Chemistry," Ref. 42, p. 4.

55. Margaret E. Farago [Baldwin, UCL: PhD 1959], "Ingold and Inorganic Reaction Mechanisms," RSC, ACC, Ref. 1, April 6, 1993 and Fred Basolo, "Base Hydrolysis of Cobalt(III) Amines," ACS National Meeting, Ref. 1, August 25, 1993. Nyholm received a UCL PhD in 1950. See also, C. W. Shoppee, Ref. 2, pp. 368-9.

56. "Professor Ingold's Application to the DSIR for equipment for the development of a special research," en-

closed with C. K. Ingold to Provost, October 15, 1948, University College London Records Office, Chemistry Miscellaneous Files, File 31/3/24. Professor C. A. Bunton [UCL: BSc 1941, PhD 1945], now at the University of California, Santa Barbara, Interview, August 25, 1993.

57. Millen, Ridd and Davies, Interviews, Ref. 18.

58. University College London Records Office, Chemistry Miscellaneous Files, Chemistry Staff and Consultancy Work 31/3/61, correspondence in October and November 1956.

59. The Future Needs of the Chemistry Department, January 31, 1944. University College London Records Office, Post-war and Finance, 1939-1945, ff. 277-297.

60. Of the 259 members of the 1929 to 1937 cohort of BSc Specials, 72 (28%) went on to UCL PhD's. Another 15 students, did an MSc or DSc. In addition, during that period, some 32 students who had done BScs elsewhere did a PhD at UCL. Thus during the period overall, the postgraduate cohort was about 40% of the size of the undergraduate cohort while UCL graduates comprised two-thirds of the PhD cohort. In fact, during Ingold's headship, the percentage of UC undergraduates staying on remained remarkably similar, although they were a smaller segment of the total postgraduate cohort. Of the 661 students who completed BSc Special Degrees in Chemistry at UCL between 1938 and 1961, 173 (26%) went on to take PhDs there by 1964; that is 46% of the 370 UCL Chemistry PhDs awarded between 1938 and 1964. [Chemistry has been taken to include Crystallography.] "Chemists Prosopography Project," Ref. 22.

61. The Future Needs of the Chemistry Department, Ref. 59, f. 282. Interestingly, as mentioned above, Ingold's own approach to post-graduate tasks, though not one of "mass-production", was to organize students to work on different angles of a particular problem in which he was interested. Some of the work would be quite routine and, on occasion, might repeat that of previous students. Several of my interviewees made this point.

62. *Ibid.*, f. 292. This was rather a contrast to Donnan's integrationist model.

63. Proposal for a Third Chair of Chemistry, n.d. [c. December 1947]; and D. B. Pye [Provost], File note of conversation with Professor Ingold, n.d. [c. December 1947]. University College London Records Office, Chairs of Chemistry Files, File 31/2/2. Still using the need for a theoretician as an argument, in 1954, Ingold negotiated the institution of a fourth chair in chemistry with special attention to theoretical chemistry. The "theoretical" qualifier was attached for tactical reasons within the University and was soon dropped from the title once the chair was taken up by D. P. Craig [UCL: PhD 1949], who was indeed a theoretician. Kathleen Lonsdale was awarded a personal chair, so there were five chairs in the department.

64. Correspondence between C. K. Ingold and B. Ifor Evans [Provost], January 28, 1957 - February 15, 1957. University College London Records Office, Ingold File, File 31/1/27. In the event, the retirement age for professors was raised to 67, so Ingold stayed on until 1961. [Provost to C. K. Ingold, June 17, 1957. *ibid.*]

65. C. K. Ingold to B. Evans [Provost], March 29, 1956. University College London Records Office, Ingold File, file 31/1/27.

66. Professor Ingold's Memorandum on Rebuilding the Department of Chemistry, February 1958. University College London Records Office, Chemistry Files, File 31/3/76.

67. Most of my interviewees have made this point.

68. C. K. Ingold to Provost, May 31, 1961. University College London Records office, Ingold File, File 31/1/27.

69. With regard to the new University of Lancaster, see "Lecture to the Lancastrian Frankland Society, October 19, 1962," Churchill College Archives Centre, Cambridge, Todd Collection, 11/18.

70. C. K. Ingold, The Education of a Scientist, Ibadan, 1963. UCL had a formal relationship on behalf of the University of London with Ibadan to help it make the transition from an institution whose students took London degrees to an independent university. Ingold wished to accept the invitation, he said, because the Department there had been largely built up by old students of UCL. Four Englishmen who did UCL BScs and received UCL PhDs in 1950 made careers at the then University College of Ibadan, as did a Nigerian who received a PhD in 1959; "Chemists Prosopography Project," Ref. 22.

71. It was also unique in its lack of preparative organic and bioorganic chemistry, which made it very different from other departments.

72. C. K. Ingold, "University of East Anglia: First Thoughts on a Plan for a Faculty of Science," September 12, 1960; The Registry, University of East Anglia. Emphasis in the original.

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THE PROGRESS OF PHYSICAL ORGANIC CHEMISTRY AS MIRRORED IN THE FARADAY SOCIETY DISCUSSIONS OF 1923, 1937, AND 1941

Derek A. Davenport, Purdue University

The Faraday Society Discussion of 1923 titled "The Electronic Theory of Valency" was a stellar affair (1). It was prompted by the 25th anniversary of the "discovery" of the electron and J. J. Thomson himself was in the chair. The opening address was given by G. N. Lewis whose classic contribution to "the ephemeral literature of science," *Valence and the Structure of Atoms and Molecules*, was at the proof stage (2). Other participants included R. H. Fowler, N. V. Sidgwick, W. A. Noyes, W. H. Bragg and, on the organic side, T. M. Lowry (the convener), A. N. Lapworth, Robert Robinson, B. Flurscheim, and J. F. Thorpe. The absence of Thorpe's former student and then colleague, C. K. Ingold, is explained by the fact that he had been married the previous week and was away on his honeymoon in Wales (3). That physicists should be interested in the nature of the chemical bond in those pre-Heitler/London days was not unusual. On August 6, 1922, for instance, Max Born wrote to Albert Einstein(4):

> A short notice in *Naturwssenschafton* about the H_2 molecule contains some results of interest to connoisseurs. But the more unequivocal these turn out to be, the crazier the whole system seems. I am not yet on the right track as questions of principle go.

Early in 1923, Born again wrote to Einstein(5):

> Then I am going to put this subject into cold storage until the question of homeopolar binding forces between atoms has been solved from Bohr's point of view. Unfortunately every attempt to clarify the concept fails. I am fairly sure though that in reality it must all be very different from what we think now.

The Discussion opened with a few rather perfunctory remarks by Sir Joseph followed by G. N. Lewis' keynote address. As H. B. Watson was later to observe in the preface to his pioneering *Modern Theories of Organic Chemistry* (6):

> Professor G. N. Lewis's conception of the sharing of one or more electron pairs by two atoms gave a new and illuminating picture of the bonds which link carbon atoms into chains and rings, and set organic chemists the task of interpreting the reactions of carbon compounds of various types in terms of the electronic structure of molecules.

In the event Lewis' address was rather low-key, by no means a call-to-arms(7):

> Since it is the purpose of such a discussion as the present one to attempt to secure agreement between conflicting opinions, or at least to secure better mutual understanding of divergent points of view, it seems to me that the introduction might well consist merely of a sketch of a few of the more controversial topics chosen from the many which our subject suggests.

Lewis here reiterated his famous statement that "I proposed to represent every [chemical] bond as a pair of electrons held jointly by two atoms." He adumbrates his "magnetochemical" theory of the chemical bond, a theory that was to prove one of the few "ephemeral" parts of his monograph in press (8). He gives no credence to alternating polarity, a matter that was to remain one of the "divergent points of view" at the close of the Discussion.

Lewis, together with Lowry and Thorpe, addressed the difficulty of pinning down some substances to a single electron structure (9):

> Rather we may state that no one of the formulae given above expresses the average state of ethylene. Some

molecules may at times approximate to any one of these and possibly other states but the average properties of ethylene would be somewhere between those corresponding to the three formulas, and I think we may state that the great majority of molecules must be nearest to the first structure, as shown by the prevalence of that stereoisomerism which can be ascribed to the lack of free rotation about the double bond.

and further (10):

I hope that Professor Thorpe will present to us in this discussion some of his extremely convincing evidence that there are tautomeric substances which are not to be interpreted by an oscillation between two distinct structures, but rather as a single loose structure of intermediate character which cannot be represented by our existing graphical methods.

These musings would seem to represent the first glimmerings of mesomerism and resonance theory.

In two papers Lowry advocated his semi-polar double bond, even for the case of the symmetrical ethylene molecules, but this came under ironic attack in the Discussion (11) and elsewhere:

The remark that *a double bond in organic chemistry usually reacts as if it contained one covalency and one electrovalency is,* with its double qualification, unexceptionable, being no more than a restatement of the fact that unsaturated compounds undergo polar additive reactions - sometimes.

Bond polarity (alternating or not) was a major point of dispute. Thus we find Thorpe pontificating (12):

Organic chemistry is, however, the most exact of sciences, because the organic chemist can build models of his compounds, ask them questions and obtain answers....In the case of the polarity theory, the main point is that its supporters are not yet agreed among themselves, and until they are there is no point in bringing forward any evidence against them. The polarity theory explains everything and predicts nothing.

To which Robert Robinson rather ingenuously replied (13):

The difficulty was that prediction and verification were usually published in the same paper and the reader could not tell which really came first, and in order to show good faith in the theory he would predict the results of some experiments now being carried out by Mr. Oxford in Manchester....

Although he was not one of the invited speakers, the most trenchant voice was that of Bernard Flurscheim

(14), a rather shadowy figure whose obituary Ingold was to write in 1956 (15):

Since organic molecules contain atoms with substantially different volumes and total number of electrons it requires courage to assume that they all share the preference shown by many authors for the number 8; and that, to gratify this preference, they will set aside the fundamental laws of electrostatics. ...To summarize, organic facts can be concordantly coordinated on the basis of varying affinity demand in conjunction with general polarity and steric hindrance. Electronic bonds, however, are confronted with serious difficulties by the facts of organic chemistry as well as by the postulates of physics.

In spite of Ingold's absence, this meeting can be viewed as the birth pangs of what Remick was later to call "The Electronic Theory of the English School"(16). In the evolution of this far from homogenous, indeed highly disputatious, "school", Lapworth, Robinson, and Lowry were at first the principal figures. But by the mid-1920's Ingold (with his idiosyncratic ally, Flurscheim) was in the thick of the fray. The lively epistolary battles that ensued, mainly in the indulgent correspondence columns of *Chemistry and Industry*, have been described elsewhere (17–20). There was to be no victor and no vanquished at this time, and when Ingold moved to University College London in the summer of 1930 he was briefly the colleague of Robert Robinson, who was about to move on to the Waynflete Chair of Organic Chemistry at Oxford. There are no reports of rancor.

Such was probably not the case in 1934 when Ingold published his magisterial review, "Principles of Electronic Theory of Organic Reactions" (21). This "reads like a legal brief as to how God should have fashioned physical organic chemistry if only He had listened to the best advice"(22). The review opens with the passage:

The electronic theory of organic reactions has now developed to a stage at which it seems desirable to give a connected statement of its principles. Actually these principles have emerged piece-meal in connection with various separate applications of the theory; but the presentation of the subject on a corresponding plan has the disadvantage that the common viewpoint tends to become obscured in what at the outset constitutes a mass of undesired detail.

Among the "undesired detail" would seem to have been much sense of historical fairness. Parts of Lapworth and Robinson's classification of reagents are silently subsumed, and their curly arrows are appropriated. The terms "kationoid" and "anionoid" are dismissed in a

footnote and the more felicitous "electrophilic" and "nucleophilic" introduced in their place. Nonetheless the review does make manifest Ingold's originality and his genius for codification, rationalization, and speculation. In his discussion of mesomerism he marshals evidence from polarizabilities, dipole moments, infrared spectroscopy, and the emerging quantum mechanics. This was an early example of Ingold's ability to bring a wide range of physical insight to bear on the problem of organic structure and reactivity. Surprisingly, there is no mention of chemical kinetics.

Not everyone was pleased by these remarkable developments. Early on H. S. Fry had protested that "the structural formula of the organic chemist is not the canvas on which the cubist artist should impose his drawing which he alone can interpret (23)" while that glorious old curmudgeon Henry Armstrong complained about 'jargonthropos' and slyly observed that "bent arrows never hit their marks (24)."

The three-day Faraday Society Discussion of September, 1937, was titled "Reaction Kinetics"(25). It was divided into two parts: Part I. - General: On the Theoretical Methods of Treating Activation Energy and Reaction Velocity and Part II. - Some Experimental Evidence Bearing on the Discussion of Part I. The program led off with talks by Henry Eyring, M. G. Evans and Michael Polanyi, and Eugene Wigner, who between them had just laid the foundations of quasi-thermodynamic and statistical mechanical transition state theory. This Discussion illustrates the Faraday Society's long-time gift for spotlighting "hot" and controversial topics. The inclusion of extensive verbatim accounts of the spoken and written discussion that ensued provides a fine sense of important science in the making.

The experimental Part II contains papers by C. N. Hinshelwood, Louis P. Hammett, and Ingold's bright young proteges: Christopher L. Wilson and Edward D. Hughes. Strangely, Ingold himself did not make a formal presentation (though he participated in the Discussion) but his Svengali-like presence can be sensed in Wilson's paper on the "Rate of Ionisation Part I. Ionisation of C-H Bonds" and in Hughes' two papers on the "Rate Of Ionisation. Part II. Ionisation of C-Hal Bonds" and on "Aliphatic Substitution and the Walden Inversion."

There is a singular appropriateness in the marriage of reaction kinetics and physical organic chemistry found in this Discussion. Although Lapworth (26, 27) and others had already made shrewd mechanistic use of kinetic rate laws, it was Hughes and Ingold who largely consummated the union. By 1937 Ingold's mechanistic sym-

bolism: $S_N2, S_N1, S_E2, S_E1, E2, E1$, etc., was established, at least in the English literature (28), and many of his neologisms had become standard. In the course of the Discussion that followed Hughes' second paper he makes a seemingly impromptu addition to the list (29):

> I hold that new words, like falsehoods, should be invented sparingly, but if chemists as a whole feel one of the former to be necessary here, then I would suggest that we may as well have two and be done with it: viz., "heterolysis" and "homolysis" defined as follows:
>
> $$A:B \rightarrow A + :B \qquad A:B \rightarrow A. + .B$$
> Heterolysis   Homolysis
>
> Here the dots represent electrons, whilst charges on A & B are subject to the balancing of the equations but are otherwise designedly unspecified. Be it understood I do not *advocate* this step .....

The disclaimer notwithstanding, the coinage quickly joined the others already in the literature.

Not everyone was enraptured by Ingold's proliferating nomenclature and symbolism. In the US, where activity in physical organic chemistry was already at a high, if not yet dominant, level, little notice was given to it until the appearance of Remick's *Electronic Interpretations of Organic Chemistry* in 1943 (16). In the preface to Branch and Calvin's *The Theory of Organic Reactions* published in 1941 we find (30):

> C. K. Ingold, whose contributions to electronic structural theory are second to none, has developed a language to express his ideas. We have given scant recognition to his language, and have even taken liberties with it. Actually, we are in very close agreement with Professor Ingold's theories.

A surprising number of Ingold's coinages have survived, though few students (and even junior faculty) are aware of their provenance.

Mention has been made of Ingold's two bright young proteges, Ted Hughes and Chris Wilson. Since the writer was the student of one and later, in Columbus, Ohio, a friend of the other, perhaps he may be permitted a few personal comments. Ted Hughes was a taciturn Welshman and, to this student at least, a rather dour figure. Undergraduates knew little of his ribaldry, his capacity for beer, and his love of greyhounds and dog racing. I took his course "Kinetics and Mechanisms of Some Organic Reactions." I still have my notes and they bring back grim memories. The lectures were meticulously prepared, and their substance was simply copied on the board with every comma and full stop firmly in

place. It was like learning chemistry from an un-droll Victor Borge. However, a vast amount of insight was packed into very few words and, retrospectively, I am grateful. When Hughes was elected to The Royal Society in 1949, I was one of many who shouldered the not inconsiderable burden of carrying him in triumph to a nearby pub.
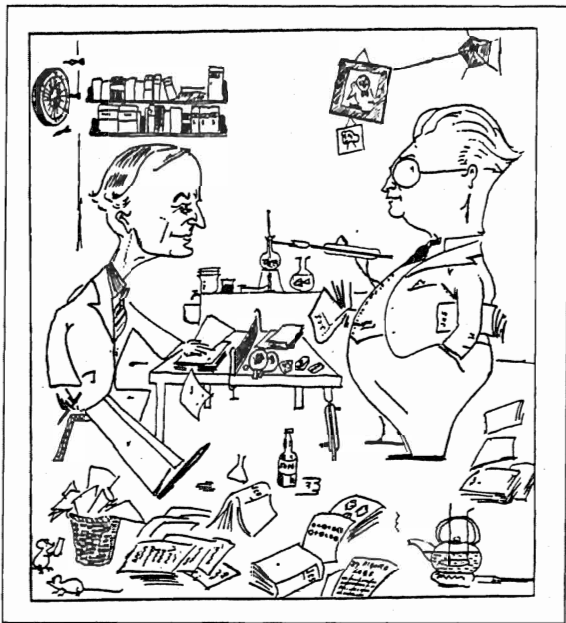
Ingold and Hughes, ca. 1948
(cartoon by J. D. H. Mackie)

At the beginning of my graduate work I was assigned the topic of aromatic sulfonation by Ingold. I was to meet with him only two or three times more in the ensuing three years. Meanwhile Hughes was still at the University of Wales, and about once a month he came riding down from Bangor (though not on an Eastern train!) to visit at University College London. The interviews were brief (so often were the progress reports) and Hughes' comments were terse and noncommittal. Finally he would say "Well then, Davenport, carry on." It was research direction by benign neglect, and it seems only appropriate that my thesis work was never to be published (31).

In comparison with Ingold's brilliance both as lecturer and researcher, Ted Hughes seemed to me at that time something of a plodder. How wrong that impression was can be gauged from the two obituary notices Ingold wrote after Hughes' premature death (32, 33). Close to a third of Ingold's papers have Hughes as a co-author, though from internal evidence it would seem that Ingold did virtually all of the writing. Ingold's hand is even apparent when his name does not appear, as in the

magnificent paper on the Walden Inversion and substitution in secondary octyl iodide. Chris Wilson always claimed that Ingold was not a particularly adroit experimentalist, while allowing that Hughes' experimental skills were exceptional (34). What is clear is that Ingold's intuitively brilliant intellectual framework for organic reaction mechanisms could not have been constructed without the butressing provided by the rock-solid kinetic rate data supplied by Hughes and his coworkers.

By comparison with Hughes, Chris Wilson, a far from taciturn Yorkshireman, had a chameleon-like charm and brilliance, coupled with a sly sense of humor. It is not surprising that at one time Ingold wrote "...he [Wilson] seems to be quite the best student I have ever had (35)." Wilson was more restless and ranged more widely than did Hughes and unlike Hughes he found Ingold's overwhelming intellectual presence at times somewhat suffocating. In spite of his steady progress through the ranks at UCL he felt he had to get away (36). World War II served to direct Wilson to ICI in Manchester, where one of his assignments was to develop expanded plastics and polymeric adhesives for the largely wooden Mosquito aircraft then under development for the R.A.F. This early experience in industrial polymers was to become a principal interest after his post-war emigration to the U.S. After stints at the University of Notre Dame and The Ohio State University, where he worked primarily in organic electrochemistry, he set up his own company and left the academic world. One can only speculate as to what might have happened had he stayed on at UCL, but somehow "Wilson and Ingold" sounds as strange to the ear as does "Burnand and Sullivan."

By comparison with the international character of the 1937 Faraday Discussion, the one held in September, 1941 was, understandably, a somewhat parochial affair (37). This was not a propitious time for an international meeting in London. It was held at the Hotel Rembrandt in South Kensington, and Professor Emeleus is thanked "for providing the epidiascope and other conveniences." The somewhat cart-before-the-horse title of the Discussion is "Mechanism and Chemical Kinetics of Organic Reactions in Liquid Solution." Besides Hughes and Ingold, only Michael Polanyi was a carry-over author from the 1937 Discussion, though Chris Wilson took a lively part in many of the discussions. The total assimilation of transition state theory in the intervening four years is everywhere apparent. Most of the authors were colleagues, or former colleagues, of Ingold who was the organizer of the Discussion. In his "Introductory Remarks" Ingold states:

We may note with gratification that all the papers have avoided, on the one hand, the tendency to be observed among physical chemists to drift obscurely into the misty regions of the general theory of reaction rate; and, on the other, the still surviving tendency among organic chemists towards that picturesque but only pseudo-physical type of speculation, which fifteen years ago might have been justifiable, but today is an anachronism.

Ingold also pays generous tribute to Kennedy Orton, one of the pioneers of detailed studies of the kinetics of reactions in solution, five of whose students participated in the program.

There is little need for specific comment on the individual papers in the 1941 Discussion. Hughes gave a classic summary of "Mechanism and Kinetics of Substitution at a Saturated Carbon" while Hughes and Ingold rendered the same service to "Mechanism and Kinetics of Elimination Reactions" and Day and Ingold to "Mechanism and Kinetics of Carboxylic Ester Hydrolysis and Carboxyl Esterification." Ingold's nomenclature, particularly his symbolism, is on full, indeed effulgent, display. It is almost a relief to find free radicals and radical cations creeping into some of the later papers. With the publication of this 1941 Discussion the Ingold school effectively shut up shop for the remainder of the war.

C. K. Ingold, E. D. Hughes, and E. H. (Hilda) Ingold

Only three papers were published, one of which was, in the mean spirit of the times, a rather savage attack on work by Jander (38).

There were to be no more Faraday Society Discussions on physical organic chemistry, recently so-named by Louis Hammett (39). The topic was no longer "hot" and was soon to enter the mainstream. It might have entered the mainstream even earlier if Ingold's plan to

write a defining textbook had not been delayed by the war. I clearly remember, sometime in 1952, Chris Wilson's showing me a substantial typescript (no doubt typed by Mrs. Ingold) from the late 1930's. He led me to understand that he had collaborated with Ingold on parts of this. There is no mention of this aborted effort (or of Chris Wilson) in the preface to Ingold's magnum opus, *Structure and Mechanism in Organic Chemistry*, published by Cornell University Press in 1953. That book is the capstone of an extraordinary era in the development of a new chemical discipline. It serves as Christopher Kelk Ingold's true memorial. By 1953, however, the center of activity had ineluctably moved across the Atlantic and the appearance, in 1969, of a second edition of *Structure and Mechanism* was, if not anachronistic, at least something of an anticlimax. But then so had been the appearance of the third edition of Linus Pauling's *Nature of the Chemical Bond* in 1960.

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ABOUT THE AUTHOR

Derek Davenport was a student at University College London from 1945 to 1950. Since 1953 he has been associated with Purdue University, where he is now Professor Emeritus. His interests have been primarily in chemical education and the history of chemistry, and he has served as chair of both the ACS Divisions of Chemical Education and the History of Chemistry. The present symposium is the sixth in a series he has organized over the years.

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TEACHING CHEMISTRY EMBEDDED IN HISTORY: REFLECTIONS ON C. K. INGOLD'S INFLUENCE AS HISTORIAN AND EDUCATOR

Theodor Benfey, Greensboro, NC

History of Chemistry had always been associated in my mind with a few lectures by that title given by Christopher Kelk Ingold during my undergraduate years in the early 40's at University College, London, which through the exigencies of warfare were given in Aberystwyth, Wales. I remember them as deadly dull, early 19th century developments in understanding quantitative relations among reacting chemicals. I imagine the lectures covered material which I began to appreciate later when I came across Ida Freund's superb book, *The Study of Chemical Composition*(1). Ingold's handful of lectures of whose details I recall nothing gave me a permanent aversion to history of chemistry as a course topic. Even though I taught history of science in various forms over a 40-year period, I never taught a course entitled "history of chemistry" or remotely resembling such a name. The closest I came to it was a set of four lectures on the development of organic chemistry I gave to a CHF-Woodrow Wilson Foundation chemistry teacher group at Princeton in the summer of 1992.

Slowly my history of science courses incorporated one chemical topic after another as my own researches and fascinations with various phases of chemical history opened up those episodes. My main interests for long were in intellectual history, the development of ideas, particularly those of organic chemistry.

Those developmental sequences in organic theory also found their way into lectures for my organic chemistry students. In fact both in freshman chemistry and the majors course in organic chemistry I incorporated intellectual history – internalist history – whenever I

was reasonably confident of my facts – and sometimes when I wasn't, when their anecdotal flavor was too delicious to ignore. It never occurred to me during those forty years to track to their source the developmental approach that was perhaps the most characteristic aspect of my teaching when compared with that of other college and university teachers in this country.

I began my teaching at Haverford College in January 1948, a year after coming to the States on a postdoctoral with Louis P. Hammett at Columbia. Hammett told me he was strongly influenced by Percy W. Bridgman to look at how scientific concepts were defined, what the operational definitions were for accurately understanding and employing those concepts and terms. The search for those definitions was an exercise in intellectual history, admittedly of a different sort. Hammett applied the approach to organic chemical practice and thereby created the extension of the pH concept to the Hammett acidity function H_0 because the operational definition of pH led to absurd values in highly acidic media. More broadly he was one of the pioneers of what we now know as physical organic chemistry.

At Haverford, my department head was William Buell Meldrum, a Harvard Ph.D. under T. W. Richards, who himself had been a Haverford graduate and at Harvard taught a course in history of chemistry. Meldrum had written what was considered an influential textbook, *Introduction to Theoretical Chemistry*(2), which dealt with many aspects of classical chemical theory – the concepts underlying our understanding of

atomic weights and the periodic table – from a histori-
cal point of view (Fig. 1).

    With Louis Hammett and William Meldrum as my
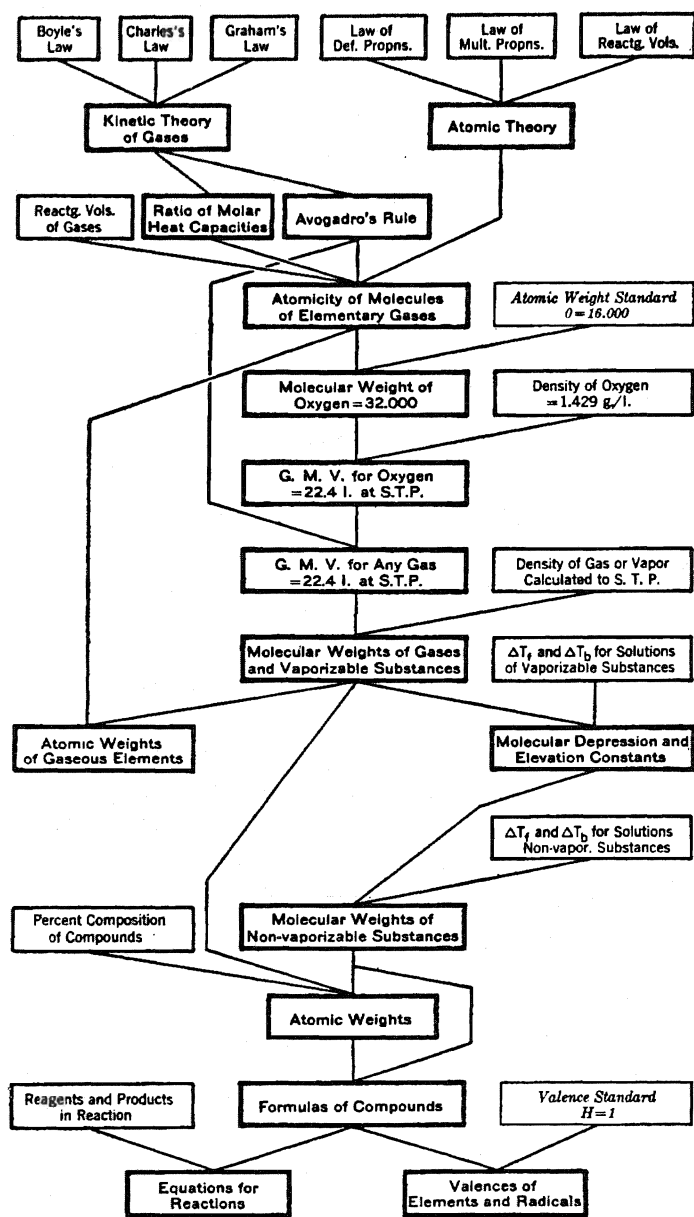chief American mentors, I may perhaps be forgiven if I

Figure 1 Fundamental Theory: Flowchart showing
historical interactions leading to atomic weights,
formulas, and equations. From William B.
Meldrum and Frank T. Gucker Jr., *Introduction to
Theoretical Chemistry* (2).

came to think that the accepted way to teach chemistry
was as far as possible to present its current concepts in
the context of their intellectual history.

That view was further strengthened by the case his-
tory approach to the sciences that James B. Conant in-
troduced as a requirement for all nonscience majors
when he returned to the Harvard presidency after his
war service in Washington(3). He had been appalled to
discover that legislators and government officials had
scant understanding of how science worked, how dis-
coveries were made. He decided that future decision
makers needed to have such an understanding and that
the way to do it was to teach key episodes of science in
their historical context.

    I attended Conant's summer school in 1949 where
I met the Harvard teachers of the various courses that
were carrying out Conant's vision—among them
Leonard Nash, Thomas Kuhn, E. C. Kemble, and Gerald
Holton. A number of the participants felt such courses
should be required of science majors also. Out of that
experience came my first foray into historical writing—
a brief paper on William Prout and Prout's hypothesis.
Several years later I worked with Leonard Nash and
wrote my own organic chemical case study *From Vital
Force to Structural Formulas*. Conant did not accept it
for his series—it seems he had attempted to do the same
himself and had decided it could not be done—but my
manuscript was published by Houghton Mifflin (Harold
Hart, series editor) and reprinted by the American
Chemical Society and most recently by the Chemical
Heritage Foundation(4).

    Finally, I was fascinated by George Willard
Wheland's *Advanced Organic Chemistry*(5), a more
philosophical approach in which he devotes for instance
forty pages to the clarification of the concept of isomer
(before arriving at stereoisomers), a topic we expect our
organic students to comprehend on the basis of one or
two lectures. Wheland, a Conant Ph.D., had been a re-
search fellow with Pauling at Pasadena from 1932 to
1936 and the following year had a Guggenheim Fel-
lowship which he spent in England, with Ingold, as well
as with N. V. Sidgwick and J. E. Lennard-Jones. Will-
iam H. Brock in his *Norton History of Chemistry*(6)
comments on the excitement generated by Wheland's
book because he pointed to fundamental philosophical
problems in chemistry.

    Only six years ago, after I gave up teaching chem-
istry to devote myself wholly to its history, did it occur
to me that the person from whom I learned my own ap-
proach to teaching was none other than C. K. Ingold
(Fig. 2).

    The lectures he prepared we copied essentially ver-
batim in our notes and memorized for that final com-
prehensive set of written examinations at the end of our
undergraduate training, which determined whether we

Figure 2 University College London chemistry faculty and students after the return from Aberystwyth, 1945.

Left to right, front row: unknown, Elfred Evans, Dan Godard, Peter LaMer, "Butch" Easty, George Kohnstam, Alwyn Davis, Everest.
2nd row: Brenda Irlam, Theodor Benfey, Stephen Awokoya, Margaret Grunau, June Revai, remaining unknown.
3rd row: Mary Hemming, G. H. Smith, unknown, Samuel Fowden, C. K. Ingold, H. J. Evans, Ronald J. Gillespie, Gareth Williams, Rimmer, Dorothy Usher (Easty).
4th row: Franceska Leake (Garforth), unknown, unknown, unknown, unknown, D. James Millen, unknown, Kathleen Winstanley, Johnston, unknown.

would be permanently labelled as having received a first class degree or a second or a bare pass. Those lectures in somewhat polished form became the Baker lectures at Cornell University published as *Structure and Mechanism in Organic Chemistry*(7).

Ingold's *Magnum Opus*

If we look in detail and from an educator's and historian's point of view at Ingold's *Structure and Mechanism* we meet up with some intriguing discoveries. The preface announces that the book is not intended as a research monograph. Instead Ingold declares, "I have been writing chiefly for the university student, and rightly or wrongly, I have adopted the policy of limitation by selection."

The first chapter, "Valency and Molecular Structure," begins with a historical section, "Development of the Theory of Molecular Structure," pointing to the key figures who helped clarify the numerical aspect of va-

lency: Dumas, Gerhardt, Laurent, Cannizzaro, Frankland, Williamson, Kekulé, van't Hoff and LeBel.

By the end of the century valency had become synonymous with the charge on an element if in ionic form and with the number of bonds. But Alfred "Werner was foremost in maintaining a clear distinction between the charge number of an ion and the coordination number."

By page 3 (2nd page of the text) I already learned something quite fascinating. Ingold talks of the 1897 "discovery of the electron by Thomson and Wiechert." Who was Wiechert—a coworker of J. J. Thomson? No sign of that. It turns out that Emil Wiechert became head of one of the leading schools of geophysics, in Göttingen, and provided some of the basic information about the earth's core, as well as developing a highly sensitive seismograph. He was also much involved in trying to interpret Maxwell's equations of electromagnetism. He measured the e/m ratio of cathode rays at the same time as J. J. Thomson.

However, John H. Heilbron in his account of Thomson in the *Dictionary of Scientific Biography* says Wiechert did not discover the corpuscular nature of elecrons because he was influenced by H. A. Lorentz' ideas about electricity, and identified the cathode-ray particle as a disembodied atom of electricity, a fundamental entity distinct from common matter. Yet Ingold made Wiechert a codiscoverer. Was his judgment more accurate than Heilbron's? Considering our present knowledge of wave-particle dualism, Wiechert's description of the electron may have been truer than Thomson's. We have now found that all of matter is as mysterious as the electron, whereas Thomson claimed that the electron was simply another piece of matter.

In the first quarter of the book Ingold laid the structural groundwork with introductory chapters on physical properties, inter- and intramolecular interaction, and classification of reactions and reagents. He also develops his terminology, the inductive, mesomeric, and electromeric effects and the associated symbolism ± I, ± M, and so on. He then launches into the heart of his subject, the mechanisms of reactions. And he chooses as his first topic not the seemingly simpler aliphatic substitution but rather electrophilic aromatic substitution for very perceptive reasons:

> If organic chemistry had so developed that we could report its position in a completely systematic way, our next proceeding would be to direct attention, for each one of the reaction types surveyed in the preceding chapter, to the principal observations and conclusions concerning its orientation, rate, extent, steric course, and mechanism. However, we shall actually proceed in a less systematic manner for two reasons. One is that the subject of chemical reactions is intrinsically dissymmetric: there can be no orientation problem where only one potential reaction center exists, and no practical question of extent in experimentally irreversible reactions. The second reason is that organic chemistry has in fact developed unevenly.

He then states that one must preserve the "historical perspective ... in order that the present position may be appraised." Now he explains why he has chosen aromatic substitution as the first area to be discussed in detail:

> Electrophilic aromatic substitution almost always involves several potential reaction centers, and therefore presents a problem in orientation; and since orientation can be studied on the basis of a minimum of previous knowledge of reaction mechanism, it constitutes a natural first step in the approach to the problem of mechanism. Moreover the study of orienta-

tion historically came first, leading naturally to the comparison of reaction rates, and then to the study of mechanism....

History in Chemistry Elsewhere

Since both Ingold and Conant took it for granted that the only way to understand science was to see its episodes in historical, developmental context, I wondered how general this approach was at the time of Conant's return to Harvard and to Ingold's teaching and Cornell lectures. In Aaron Ihde's bibliographic notes in his *The Development of Modern Chemistry*(8), he comments that "Organic textbooks of the past generally gave considerable attention to historical matters, but this becomes less true of contemporary texts." He then lists a "few of the principal exceptions,"among them textbooks by L. F. and M. Fieser(9), J. B. Conant and A. H. Blatt(10), C. R. Noller(11), J. Read(12), E. Werthheim and H. Jeskey(13), and P. Karrer(14). He also mentions chapters in Henry Gilman's four-volume *Organic Chemistry*(15)and in A. Todd's *Perspectives in Organic Chemistry*(16) and finally says, "Also see C. K. Ingold *Introduction to Structure in Organic Chemistry*(17) and *Structure and Mechanism in Organic Chemistry*(7); and Edwin S. Gould *Mechanism and Structure in Organic Chemistry*(18)." Karrer includes a chronology of important discoveries as well as a list of over 160 compounds detected in coal tar, arranged in order of increasing boiling point, with the name of the discoverer, date and literature reference for each. Wertheim's *Organic Chemistry* includes about 50 photographs of organic chemists (spread throughout the text in order of their birth dates!) with commentary and references to biographical information. His preface seeks to justify the incorporation of history: "Organic chemistry as we know it today is the product of human brains and human hands. It gains in interest when the human element is recognized and at times emphasized in teaching. Nor does this treatment add to the burden of memorization. Every teacher of experience knows that facts 'stick' better when coupled with an anecdote or interesting bit of history." One of his tables is a chronology of organic chemistry from 1500 (Paracelsus' synthesis of ethyl chloride!) to 1931, with references to further listings of American chemical events.

My most interesting discovery was Louis and Mary Fieser's *Advanced Organic Chemistry*(9). In their preface they announce that they have followed a topical presentation and that they tried "to do full justice to history, to modern theory, and to details of experimenta-

tion." In place of full references to the literature, which would have defeated their aim of producing a volume of "reasonable size and price," they give guidance to the original literature by name-and-date references, and biographical information about at least one member of a research group. These biographical notations are extraordinarily useful. Here is an example:

> Henry Gilman, b. 1893 Boston; Ph.D. Harvard (Kohler); Iowa State College.

In all, 400 organic chemists are similarly listed with birth place and date, doctoral institution and mentor, and career affiliations. Ingold's listing reads:

> Sir Christopher Ingold, b. 1893 Ilford, England; D.Sc. London (Thorpe); University College, London.

On looking up J. F. Thorpe's listing in the Fieser text we discover that he did his Ph.D. work with K. von Auwers in Heidelberg, who in turn obtained his doctorate with August Wilhelm von Hofmann in Berlin. That is where the trail ends. The Fiesers apologize for not having tracked down the mentors of many of the foreign chemists they list. But for those individuals, other sources are now available such as the *Dictionary of Scientific Biography,* as well as the growing number of chemical genealogies appearing in the historical and chemical-education literature(19).

E. D. Hughes obtained his Ph.D. with Kennedy J. P. Orton, whose kinetics training he then transmitted to Ingold. He also worked with H. B. Watson, student and colleague of Orton. And Orton, like Ingold's mentor J. F. Thorpe, did his doctoral work with von Auwers in Marburg. Auwers was something of a historian himself. A major contributor to stereochemistry, he wrote *Die Entwicklung der Stereochemie*(20).

Among Ingold's former students several have been active in chemical history endeavors. Included among them are John H. S. Green, the honorary secretary of the historical group of the Royal Society of Chemistry, and William H. Brock, who after training with Hughes and Ingold as an undergraduate switched to graduate study in UCL's history of science department. He has become one of Britain's leading historians of chemistry now on the faculty at the University of Leicester. He is book review editor of *Ambix* and has recently published a new and totally original history of chemistry, his Norton/*Fontana History of Chemistry*(6).One chapter in it is largely devoted to Ingold while another is devoted wholly to chemical education, from Edward Frankland and Henry Armstrong to the Nuffield, CBA, and Chem Study curricular reforms.

Derek Davenport, a post-world war II Ingold Ph.D., is another Ingold educator-historian. He has the distinction of being the only person who has directed two four-week Princeton summer institutes for chemistry teachers, one in inorganic chemistry and one in the history of chemistry.

Why the Decline - and now the Resurgence - of Chemical History?

In the half century between 1870 and 1920 there was a powerful movement to make the history of chemistry an integral part of chemistry. During that period Carl Schorlemmer(21), Hélène Metzger(22), J. R. Partington(23), and Edgar Fahs Smith(24) wrote influential histories of chemistry that included much chemical content. These books were largely "internalist" science histories, although Schorlemmer as a Marxist, looked at broader contexts also.

As far as the U. S. was concerned, what motivated Smith and his followers was the realization that many students were the first in their families to be exposed to a university and its broader cultural vistas. And many of those from rural backgrounds were particularly attracted to chemistry. Thus the introductory chemistry course could aid in this broader educational task by showing the historical setting of the science content they needed to learn.

By Conant's time the focus was very different. Conant was concerned that the nonscientist who would take future leadership positions in the nation had no understanding of science. He therefore instituted attractive science courses for nonscientists. But science majors were exempt. Thus the historical-cultural emphasis moved from the science courses to the nonscientists' exposure to science. The new textbook writers for chemistry majors no longer needed to become literate about historical context. They used the "excessive amount of information to be learned" as their rationale for eliminating references to the human side of chemistry. Ingold's success in systematizing organic chemistry may have contributed to the trend because he helped turn organic chemistry from a series of seemingly unrelated reactions to a coherent science, many parts of which can now be taught deductively.

Recently. however, there has been a resurgence of interest in showing to science students the human side of chemistry as seen for instance in the large number of applicants for the 1992 Princeton institute in chemical history and in the formation and growth of the Chemical Heritage Foundation. There have also been a num-

ber of new chemical history texts, such as William Brock's, already cited(6), John Hudson's *The History of Chemistry(25)*, and David Knight's *Ideas in Chemistry: A History of the Science(26)*. One reason may be the belated self-consciousness of the chemical community in the face of the public's suspicion and criticism of all things chemical, the concern to demonstrate the significant roles played by the chemical sciences in the intellectual, cultural, and economic development of the last two centuries.

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ABOUT THE AUTHOR

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C. K. INGOLD'S DEVELOPMENT OF THE CONCEPT OF MESOMERISM

Martin D. Saltzman, Providence College

Charles W. Shoppee, in his obituary notice for Sir Christopher Kelk Ingold (1893-1970), wrote(1):

> ..he was a chemical genius, an excellent physicist, and a talented mathematician, able to employ and to combine all aspects of organic, inorganic, and physical chemistry.

Ingold's enormous output totaling 443 papers covered a vast array of subjects and set the research agenda for physical organic chemistry for several decades. His *Chemical Reviews* paper "Principles of an Electron Theory of Organic Reactions(2)" published in 1934 was for many years the only source for understanding the pioneering work he and other British chemists had done on the interpretations of reactions mechanisms. This paper will deal with one aspect of his work: his anticipation of a development of the concept of mesomerism, which he derived from both physical and chemical evidence. Linus Pauling (1901-1995), using quantum mechanics, developed resonance theory at the same time.

Ingold realized that the Lewis theory was a first step in relating structure and reactivity, but that it did not go far enough. As Ingold wrote(3):

> Lewis discovered the material constitution of the covalent bond. But he could not describe the forces involved, because they were of a nature unknown in classical physics.

Lewis's pioneering paper "The Atom and the Molecule(4)" appeared in 1916; just a decade later Ingold was questioning whether certain types of structures represented by the Lewis theory were adequate.

G. N. Lewis (1875-1946) provided little leadership to the organic chemical community in how to apply his ideas to the problems of structure, reactivity, and mechanism. In 1919 Irving Langmuir (1881-1957) elaborated upon the foundation established by Lewis and introduced the octet and covalent bond concept into chemical thinking. Through his many papers and lectures Langmuir was to have more of an impact than G. N. Lewis on organic chemists. In the US there were only isolated attempts by chemists such as James Bryant Conant (1893-1978) and Howard Lucas (1885-1963) to use Lewis theory. The most fertile ground for the
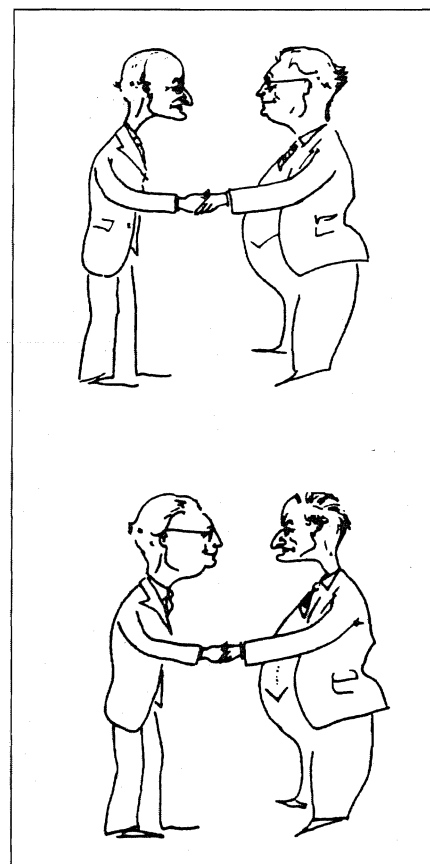

Mesomerism: Ingold metamorphoses into E. D. Hughes. (cartoon by J. D. H. Mackie)

Lewis-Langmuir theory, however was in Great Britain. Langmuir lectured on his work at a British Association meeting in Edinburgh in 1921, and in 1923 Lewis attended a symposium sponsored by the Faraday Society at Cambridge devoted to the use of the electron pair and octet theory.

Major strides in the use of the Lewis-Langmuir theory were made in the early 1920's by Thomas Martin Lowry (1874-1936), Arthur Lapworth (1873-1941), and–most importantly–by Robert Robinson (1886-1973). Not until approximately 1925 did Ingold begin to make use of the Lewis-Langmuir theory in his work. Even though his was a somewhat late and shaky start, because of a bitter polemic with Robinson(5), he easily surpassed his rivals in his understanding and use of the new theory.

The positive attitude of the British toward electronic theory may be attributed to several factors. One is the history of the consistent success in attempting to utilize electronic theory in organic chemistry, whereas the American chemical community had been plagued with failures. The second factor is the type of problems that British chemists initially chose to study. Much of the focus was on understanding conjugated systems in general and aromatic systems in particular, for the chemistry of these systems was a "hot" subject in the 1920's. Finally, there was an accepted tradition of theoretical speculation in British organic chemistry and of an openness to new ideas.

Robinson introduced the concept of the mobility of electron pairs in conjugated systems in 1926 (6). For example, he represented the reaction of methyl iodide with an aminocrotonic ester as follows:

$$CH_3-C\!\!=\!\!C-COC_2H_5 \quad \cdots\!\!\rightarrow \quad CH_3-C-C-COC_2H_5$$
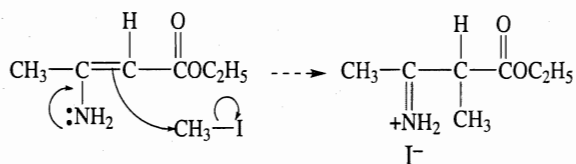
An often cited example is the ionic reaction of hexatriene, which is represented as the following doubly charged structure:

$$\overset{\oplus}{CH_2}\!\!=\!\!CH-CH\!\!=\!\!CH-CH\!\!=\!\!\overset{\ominus}{CH_2}$$
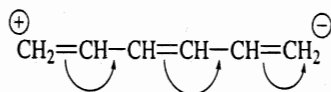
Ingold was later to name this the "electromeric effect," the term being borrowed fro the American chemist Harry Shipley Fray (1878-1949). In 1926 Robinson published a detailed general theory of reactivity in conjugated systems. He not only invoked the use of electromeric ef-

fects but also suggested how polar effects can influence substitution in conjugated systems.

By 1923 Lowry had already introduced a concept of the double bond as one containing both covalent and ionic attributes or a mixed bond to account for its reactivity in various systems(8). He believed that the polarization was only of a temporary nature and existed at the time of reactions. Lowry was also the first person to realize that formal positive and negative charges must be included in the structures of certain molecules, such as amine oxides.

Colin Russell, in his classic study of valence, summarizes the contributions made by Robinson and Lowry(9):

> Thus the covalent bond was no longer conceived as a rigid entity with two electrons symmetrically shared between the atoms. A molecule was subject to strains and stresses unimagined twenty years previously, and the valency bonds were more dynamic than static, responding to the demand of a reagent to assist a reaction.

Ingold's late start in this field was due to his use of the concept of partial affinity as being the major determinant in the course of reactions. Between 1924 and early 1926, Ingold relied upon the theoretical framework as developed by Bernard Flurscheim (1874-1955), who had studied with Johannes Thiele (1865-1918) and Alfred Werner (1866-1919). He advocated the use of residual affinity as the basis of his theories. A substituent could exert a strong or weak affinity demand on the carbon to which it was bonded, and this effect would be carried along a chain in an alternating manner. Implicit in Flurscheim's theoretical framework was the notion that affinity was continuously divisible and was partly bound and partly free. In 1924 Ingold was a rising star in British chemistry, having just been made a Fellow of the Royal Society and appointed Professor of Organic Chemistry at Leeds University. He may have sought to enhance his stature by engaging in studies of aromatic substitution, a subject of intense interest. His interpretation was contrary to that of Robinson; and over a period of approximately two years Ingold and Robinson engaged in bitter and at times personal polemics concerning the mechanism of aromatic substitution. By late 1925 Ingold knew that Robinson was correct and started to use many of Robinson's ideas concerning the mobility of the electron pair in conjugated systems. The 1926 paper (10), "The Nature of the Alternating Effect in Carbon Chains, Part V," which Ingold published with his wife Edith Hilda (1898-1988) as the co-author, was

to be the beginning of the series of events which shall form the major focus of this paper.

Ingold attempted to place himself into the mainstream of chemical science by showing that his previous theoretical framework for interpreting aromatic substitution was compatible with the views of Robinson. In this paper several insights are presented that had been previously overlooked by others. For example, in discussing the depressive effect of the acylated substituent as compared to an alkyl group with regard to o,p-directive power, he proposed the following structures for the aclyoxy and carboxyamido groups and commented(10):
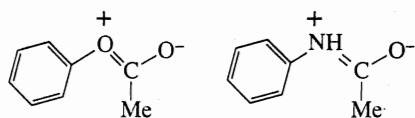

..a modernized interpretation of which would involve the recognition of a betaine-like phase (or a condition corresponding with partial conversion into such a phase), the bond joining the oxygen atoms being an electrovalency. In the carboxylamide group, the nitrogen atom will necessarily constitute the positive end of the betaine linking, and thus forms arise which...would be expected to diminish the tendency to o,p-substitution.

Thus, we can see from this quotation the crucial idea that polarization by the electromeric effect may have a permanent aspect. The actual state of the molecule may be intermediate between the two structures, an anticipation of the concept of mesomerism and resonance.

Ingold also offered a rationalization for why certain reagents such as phosphorus pentachloride, iodine trichloride, and sulfuryl chloride will produce a positive chlorine ion in aromatic substitution, whereas we usually expect a negative chloride ion from these reagents. To understand this, Ingold proposed that the chlorine in these compounds separates as a neutral or positive group that would then seek a negative center. As Ingold pointed out, "...phosphorus pentachloride readily chlorinates anisole in the p-position, but has no action on nitrobenzene(10)."

In a footnote Ingold indicated that measurement of the conductivity of phosphorus pentachloride in nitrobenzene produces such a low value that it would not be compatible with an ionic structure such as $(PCl_4)^+(Cl)^-$. In describing the structure of phosphorus pentachloride he stated "the bonds by which the labile chlorine atoms are held might be termed semi-polar single bonds and written to avoid use of signs such as 1/2$^+$ and 1/2$^-$(10)." The semi-polar bond idea had been developed by Lowry, although Lowry's use of it was

not well understood by his contemporaries. Structures for sulfuryl chloride incorporating these ideas are reproduced below(10):
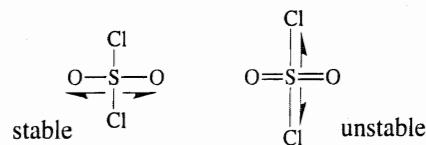

Sulphuryl chloride may be regarded as having two electromeric forms, the stable one containing two semipolar double bonds (X), and the other only one semipolar double bond and the two semipolar single bonds (Y). It is the latter which contains the labile chlorine, and it would be in this form rather than the first which is concerned in chlorination by sulphuryl chloride.

Polarization in conjugated systems may have a permanent aspect. The molecule as it was represented by the Lewis structure up to this point may not be the "normal" state. Ingold was to refer to this as "electronic strain," an entirely novel idea in 1926. The inductive effect had been previously described in 1923 by Lewis (11) and applied by Lucas(12) in 1924 and 1925 to problems of orientation in alkenes; but the idea that permanent polarization could exist in conjugated molecules had not been proposed before 1926.

Fritz Arndt (1885-1969) in many ways anticipated Ingold's concept of mesomerism through his analysis of the properties of pyrones and thiopyrones, first published in 1924. The Lewis structures for these molecules do not explain their characteristic reactions.
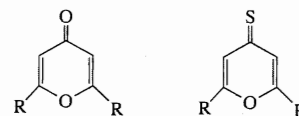
Arndt proposed that an additional zwitterionic dipolar structure (below) could be written for these molecules. The actual state of the molecule was an intermediate or Zwischenstufe, as Arndt termed it.
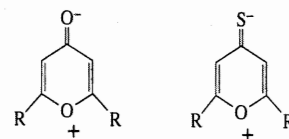
Campaign(13) has presented an excellent account of Arndt's work and the reasons for the general lack of recognition of this early anticipation of the resonance(mesomerism) theory. A letter to Campaign by Arndt in 1957 contains the following statement(13):

It should be noted that Sir Robert Robinson, one of the first pioneers of electronic theory, formulated in 1925..the potential shifts in pyrones and similar systems..In doing so he made a more detailed use of electronic theory than I had at first done; but he did not speak of a definite intermediate state of the molecule.

There are no references to Arndt's work in the early Ingold papers; whether Ingold knew of his work is a matter of conjecture. Ingold probably independently conceived of the idea of intermediate states.

A hallmark of Ingold's work was the use of physical methods to substantiate his theoretical speculations. Having presented the possibility of conjugate polarization, he proposed in the *Annual Reports* of the Chemical Society for 1926 a test of his hypothesis. This test was to measure dipole moments of certain molecules, a technique that had recently been developed by Peter Debye (1884-1966). Ingold proposed that, if a dipole moment were to be found for dimethylaniline, it would confirm permanent polarization existed in this molecule. Nitrogen in the dimethylamino group should have a weak inductive effect, drawing electron density from the aromatic ring; but it should also exert a stronger "tautomeric electron-repulsion," as Ingold termed it. In 1934 Ingold wrote(2):

> ...it then had to be shown by the use of vector addition principle that the direction of electric moment associated with the saturated aliphatic linking of the group becomes reversed in the corresponding aromatic combination:

$$\text{Alkyl} \dashrightarrow NR_2 \qquad \text{Aryl} \dashrightarrow NR_2$$

> The first test of this kind was completed (for the group NH$_2$) in 1928 by K. Hogendahl...with definitely positive results.

There are two orienting effects in aromatic substitution, according to Ingold "originating in the combinations of inductively propagated electronic strain (I,--->) and tautomeric or electromeric transformations $(T, \frown)(2)$. Ingold sought to determine how these effects would influence reactivity in various positions, i.e., *o*,*p*- versus *m*-. At that time it was known from experimental observation that, whatever the substituent effect, it was most prominent at the *o*- and *p*-positions. To understand fully what these effects were, Ingold realized that he must know not only the isomer distribution but "relative velocities of the total substitutions ($o + m + p$) of different

substances(2)." Since such data was not available, Ingold and Shaw in 1927(14) derived partial rate factors for aromatic substitution which they termed "coefficients of activation." The method of competitive reactions was used to obtain the data for the rate factors. An analysis was presented for the four possible cases: +I-T; -I-T; -I+T; +I+T. The most interesting are the latter two, where the inductive and tautomeric effects compete in one case and reinforce each other in the other. Ingold assumed that in the -I+T case, as for example the halobenzenes, the *o*- and *p*-deactivation is less pronounced than that for the *meta* position. Thus, by default, the *o*- and *p*-positions become more susceptible to attack, though at a slower rate, than would be found in the pure +T case. That tautomeric effect in this instance is very weak but, coupled with enhanced deactivation of *m*-positions, leads to the observed outcome.

In another case of -I+T, *i.e.*, the phenoxide ion, we find some of Ingold's most original thinking. Perhaps the most important statement concerning this case is as follows(14):

> ..that whilst inductive effects give rise to an essentially permanent not necessarily constant, molecular condition, the tautomeric effect (apart, possibly, from a small permanent residuum) is essentially temporary in character, assuming much greater importance at the moment of attack by a reagent than in the quiescent period preceding it.

These remarks were accompanied by the following representations:

+I (*o,p*)   −I(*m*)   +T(*o,p*)
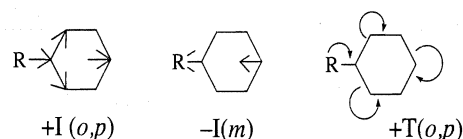
Further clarification of the inductive effect in aromatic substitution was made by Ingold and Vass in 1928 (15). They proposed that there was operating, in addition to the I effect, what he called a direct effect D(15).

> Since its genesis is also that of the inductive effect (-I), its variation as between different directing atoms will be the same as that of the inductive effect. Because, however, it is propagated, not through the bonds, but through space, its distribution in the ring will be, not in the order *o*,*p* > m, but in the order *o* > *m*.> *p*.

The following nitration data was presented to substantiate the argument.

	PhF	PhCl	PhBr	PhI
ortho	12.7	30.1	37.6	41.1
para	87.4	69.9	62.1	58.9

Iodine having the largest +T and smallest -I, it followed that -D would favor more of the *o*- than the *p*- product. In the case of fluorobenzene, fluorine having the smallest +T and largest -I, the -D effect will result in least favored attack at the *o*-position. To Ingold's mind the inductive effect was transmitted both through bonds and through space.

By 1928 Ingold had almost completed his assessment of electronic effects in aromatic substitution. He would later classify some of these as permanent or polarization effects and some as temporary or polarizability effects. This is summarized in a table Ingold included in his 1934 *Chemical Reviews* paper (2).

Electronic Mechanism	Electrical Classification	
	Polarization	Polarizability
Chemical inductive (---▶) (I)	Inductive	Inductomeric *
Tautomeric (⌒) (T)	Mesomeric	Electromeric

While Ingold was developing his ideas, a concurrent revolution was occurring in physics: the development of quantum mechanics. This revolution in physics would rapidly become the basis for theoretical chemistry and the whole new field of chemical physics in the 1930's. How these ideas related to the problems that organic chemists were grappling with seemed totally incomprehensible to most organic chemists. There were, however, a few exceptions, these being Ingold and his fellow countryman Neville Sidgwick (1873-1952). M. J. S. Dewar in his 1949 text *The Electronic Theory of Organic Chemistry* sums up the problem best(16):

> The application of the new quantum mechanics of Heisenberg and Schrödinger to chemistry in general, and organic chemistry in particular, has been hampered by two facts. Firstly, the mathematicians and physicists who have devised and elaborated the new theory have naturally had little or no knowledge of chemistry, and they have, moreover, been mainly interested in the quantitative application of the theory in a few simple problems and not in its qualitative application to more complex cases. Secondly, chemists have felt that the new theory must be unintelligible without a more detailed knowledge of mathematics than they possess, and many have also clung

to the older ideas of atomic structure because they found them easier to visualize.

Quantum mechanics provided a theoretical justification for the Lewis theory, which, in turn, fostered the initial impetus for the mechanistic revolution in organic chemistry. A passage from a letter of Linus Pauling to G. N. Lewis written on March 7, 1928, shows this connection(17).

> You have no doubt seen London's recent paper in the *Zeitschrift fur Physik* and have observed that the results which he derives from the quantum mechanics regarding the sharing of electrons are in the main equivalent to the rules which you had previously postulated. It is, of course, your prerogative to point this out; but, in the belief that you would probably not do so, I have taken the liberty of referring to the fact in the first part of a note concerning some further developments of the theory which has been sent to the *Proceeding of the National Academy* (a copy is enclosed).

Ingold realized that quantum mechanics was to play an important role in organic chemistry. As Shoppee has indicated, Ingold possessed the skill to gain an understanding of the subject which he did in 1932 while on leave at Stanford University, recovering from a serious illness. A letter dated April 2, 1932, from Ingold to Lewis written on Stanford stationery indicates that Ingold was planning to visit Lewis at Berkeley and was prepared to present, as Ingold phrased it, an "informal talk" during his visit (18). Ingold had first met Lewis in 1922 in London, when Ingold was a lecturer at Imperial College in the department headed by his mentor J. F. Thorpe (1872-1940). Ingold described this meeting to Lewis in a letter of April 12, 1927, "...though I do not think either of us realized what a large influence you would have on our chemical lives (19)." Ingold was now prepared to try to translate some of his ideas on a quantum mechanical basis.

An anticipation of what would come later can be seen in a 1929 paper in which he discussed the reasons for the existence of stable free radicals. Ingold proposed that it is the result of the ability to distribute charge and made the following apocryphal statement as to why this charge is redistributed(20).

> To this the best answer we can make is to refer to the microphysical equivalent (the exclusion principle) of the macrophysical law, which militates against the continued existence of intense, highly localised charge. In our view, this is also the ultimate cause of tautomeric change.

The first significant use of wave mechanics was made by Ingold in 1933. In a paper (21) entitled "Significance of Tautomerism and of the Reactions of Aromatic Compounds in the Electronic Theory of Organic Reactions" Ingold returned to a discussion of the general inductive and tautomeric effects with regard to which is time-dependent and which is time-independent. In this paper the inductomeric effect, the time-dependent inductive effect, was introduced. This type of polarizability, Ingold argued, occurs during the course of a reaction(21).

> On the other hand, a general inductive effect of the same kind persists even in hydrocarbons when the practical issue is reactivity towards an electrophilic reagent, as illustrated by the orientation rules (Markovnikoff) for the nitration of paraffins. Clearly, the polarisability of carbon is the important factor here, and thus, despite a zero moment, a condition such as the following may obtain during reaction:

In dealing with the time-independent tautomeric effect Ingold introduced the term mesomerism, which he equated with "between the parts." Why does the mesomeric effect exist? Ingold considered this by analysis of all of the possible types of structures for a molecule, for example, such as in enamines. Two unperturbed states are presented and then a third state which was "expected to have lower energy than the unperturbed states, and therefore to correspond to the most stable condition of the system."
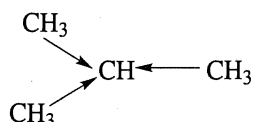
> Where there is an exact equivalence of unperturbed states, the degenerate state must be symmetrically related to the unperturbed states, whilst in other cases the relationships, although not identical, must be similar; hence, the term mesomeric = between the parts...

Ingold offered evidence for his views from observations that had been made of the chemical properties of certain systems. Various physical measurements such as dipole moments and spectroscopy were readily available by the 1930's. An example cited was that of the nitro group, in which, if the Lewis structure $-N^+(=O)-O^-$ were the correct representation, one would expect "a large dipole moment oriented perpendicular to the line of the nitroxyl groups just as have quinol ethers (21)." The actual measurement is a value close to zero. This is

what would be predicted if the nitro group has a mesomeric structure which Ingold represented as
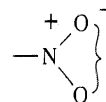
Spectroscopic evidence reported in 1933 showed that in sulfur dioxide the two oxygen atoms are equivalent, a result that could only be rationalized by mesomerism. A further insight by Ingold is given by this statement near the end of the paper(21).

> In general, the fall in energy arising from a degeneracy of given order will be increased when the order of degeneracy is raised by the participation of further unperturbed states.

An example is provided by the case Ingold had examined in 1929 of the stability of the triphenylmethyl radical, where "owing to the large number of possible positions for the free valency, the energy of degeneracy becomes comparable with the energy of the homopolar linking (20)." Aromaticity was also interpreted in terms of this wave mechanical degeneracy.

Ingold had developed his ideas by a combination of deductions from chemical and physical measurement as well as from ideas from wave mechanics. In a series of papers appearing in the premier volume of *The Journal of Chemical Physics*, Linus Pauling and his coworkers George W. Wheland and J. Sherman introduced the resonance theory(22) which they based on both chemical and physical data, as well as on quantum mechanics. As Ingold stated in 1934, "Pauling describes the phenomenon under the name resonance, which as is well known, is based on the mathematical analogy between mechanical resonance and the behavior of wave functions in quantum mechanical exchange phenomena(2)." Ingold's mechanical presentation of the concept of mesomerism led to confusion as to what he meant by mesomeric states. In the minds of some of his contemporaries these mesomeric states, equated with tautomeric structures, would interconvert much more quickly than would ordinary tautomers. The use of structural formulas created the problem that these were thought by many to be real states of the molecules. Ingold argued that "they are of the nature of intellectual scaffolding, and only the mesomeric state is real"(23).

As an example Ingold cites the reduced basicity of aniline by a factor of 106 over that of aliphatic amines. The confusion that arises is the misconception that the reduced basicity can be accounted for by postulating that

there is a tautomerism occurring between the uncharged and the three dipolar structures.

The uncharged structure would exhibit a basicity of the same magnitude as that of the aliphatic amines, whereas the dipolar structures would show no basicity at all. Tautomerism would require that aniline must exist almost entirely in the betaine form to account for the lower basicity, something Ingold viewed to be an untenable state of affairs. The only rationale is to discard altogether the idea of unperturbed forms as molecular states and to adopt in its place the concept of a state distinct in properties from any of those originally assumed. Thus, mesomerism and tautomerism are different, and one must ascribe to the mesomeric state something more than a titular position in the physics and chemistry of unsaturated structures (23). This confusion Ingold hoped to dispel indicates a problem for the modern reader of his papers. Indeed this situation is best seen from a footnote in D. Stanley Tarbell's essay on valence in organic chemistry(24):

> One of us, as a graduate student in 1934-1937, remembers clearly the reaction of some classical organic chemists to the "new-fangled theories" of Ingold and others; these were considered not entirely respectable intellectually, and it was said that the Ingold nomenclature was difficult and obscure. This was not confirmed by a reading of Ingold's *Chem. Rev.* article in 1934, although the presentation was more formalistic than seemed necessary. Ingold's presentations improved with time, the 2nd ed. of *Structure and Mechanism* being the best.

C. K. Ingold was the dominant figure in physical organic chemistry during the decades of the 1930's, 1940's, and 1950's. His work was the most frequently cited in the first generation of physical organic textbooks. For example, Waters' *Physical Aspects of Organic Chemistry* from 1925 contains 45 citations to Ingold's work(25). In Watson's *Modern Theories of Organic Chemistry* of 1937 there are 14 references(26). In Hammett's *Physical Organic Chemistry* of 1940(27) there are 51 references—six more than from Hammett's own work. Finally, Remick's *Electronic Interpretation of Organic Chemistry* of 1943 (28) contains 83 Ingold references. Even on the basis of this one small aspect of Ingold's work, there is little doubt of the greatness of the man, who almost single-handedly revolutionized the interpretation of structure, function, and mechanism in organic chemistry.
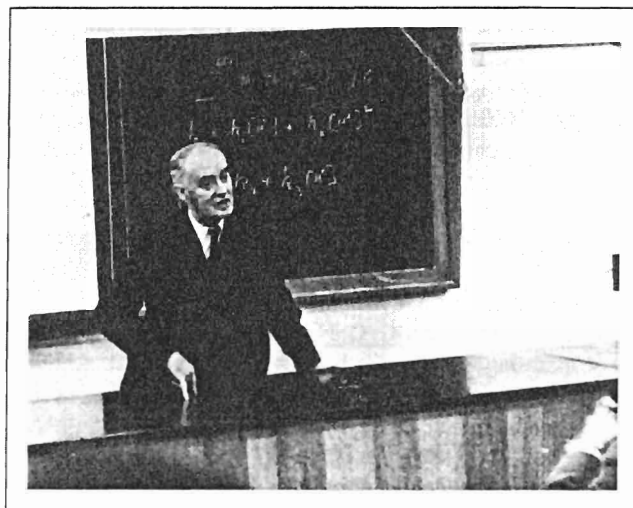
The Lecturer

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PHYSICAL ORGANIC TERMINOLOGY, AFTER INGOLD

Joseph F. Bunnet, University of California, Santa Cruz

The preeminence of Christopher Ingold in physical organic chemistry during its formative quarter century, say, 1925-1950, was based in part on his experimental contributions but even more on his perception and organization of principles and his communication of the new knowledge to his fellow chemists. That communication, in *Annual Reports of the Chemical Society* for several consecutive years in the late 1920's, in review articles published notably in 1929, 1934, and 1941, and in a major book(1), as well as in experimental papers, required the employment of good terminology for lucid expression of those principles.

My present purposes are: to recognize characteristic Ingold terminology; to discern the origin of his terms, whether original or adopted; and to recognize why some terms endure in the working language of chemists while others wither, drawing in part on my own experience in the invention of terms. I do not claim to present an exhaustive catalog of Ingold's terms.

The intellectual foundation of physical organic chemistry, as it developed under Ingold's leadership, was G. N. Lewis' recognition, in 1916, that a covalent bond consists of a pair of electrons shared between the atoms joined by that bond. Acceptance of his ideas was slow, no doubt in part because Berkeley was then two weeks in travel time distant from England, where Ingold and the other principal founders of the field were located. At an influential 1923 Faraday Society discussion, Lewis apparently convinced a number of other participants of the validity of his concepts in his role as lead-off speaker and perhaps as well in informal discussions at that meet-ing. His book on chemical bonding (2) gave guidance and inspiration to all persons seriously interested in organic mechanisms and reactivity.

Before 1925 significant insights were published, notably by Arthur Lapworth, Thomas M. Lowry, and Robert Robinson, while Ingold was busily at work in other areas, especially small ring chemistry, but also certain physico-chemical phenomena(3). Ingold's attention to chemistry of the sort most often associated with his name might be said to start with a 1925 paper(7) on the directive influence of the nitroso group in aromatic substitution. That and some of his other early studies were however interpreted in terms of then-current alternate polarity theory (8).

Terms for Electronic Effects

These effects were characterized as "electronic strain" in Ingold's early recognition of them(9,10). His understanding of electronic effects materialized gradually. In his 1934 review(11) they were presented in a table reproduced here as Figure 1. Virtually the same table appears in both the 1953 and 1969 editions of his book.

ELECTRONIC MECHANISM	ELECTRICAL CLASSIFICATION	
	Polarization	Polarizability
General inductive (→) (*I*)	Inductive	Inductomeric
Tautomeric (⌒) (*T*)	Mesomeric (*M*)	Electromeric (*E*)

Figure 1. "The four polar effects, their electrical classification, and their electronic mechanism," as outlined in Ingold's 1934 review(11).

The two "electrical classification" categories, polarization and polarizability, refer to what today would generally be called ground state and transition state effects, respectively.

***Inductive* effects.** What we today term the through-bond inductive effect was clearly recognized by G. N. Lewis in his 1923 book(12), but he did not use the word "induce" or any variant thereof in discussing it. Lewis took the decreasing acid strength in the series chloroacetic, β-chloropropionic, γ-chlorobutyric, and δ-chlorovaleric acids as evidence that the effect is dampened as it passes through a saturated chain. Ingold, in 1926 papers(9,10,13), utilized the idea in discussion of experimental data, but also without using "induce" or any variant of it. It appears that he first employed the term "inductive effect," together with the symbols +I and -I, in his section of the *Annual Reports* for 1926(14).

The term *inductive effect* was more an adoption than an invention. Lapworth in 1922 published "A Theoretical Derivation of the Principle of Induced Alternate Polarities"(8). He(15,16) and Robinson(17) used variants of *induce* in additional papers, with meaning somewhat like that intended by Ingold. Thus Ingold's *inductive effect* resembled usage current at that time.

In Lewis' discussion(12) of what Ingold later called the inductive effect, he acknowledged that the effect might be transmitted in part through space, as well as through bonds. In 1929, Ingold(18) recognized explicitly the through-space possibility, calling it the *direct effect (±D)*. Like Lewis, he said "the operation of this effect seldom becomes obvious." The direct effect does not appear in his 1934 table of electronic effects (Fig. 1) but is discussed in nearby text(11). Since Ingold's death in 1969, experimental evidence indicating operation of what is now generally termed the *field effect* has been obtained(19). The effect as recognized today is however not quite the same as that postulated by Lewis or as originally conceived by Ingold. They visualized that an electronegative substituent, such as chlorine, would exert a through-space attraction of electrons so as, for example, to increase the dissociation constant of a carboxylic acid by stabilization of the negative charge of the carboxylate ion. Recent evidence(19) indicates, rather, that the effect stems from the interaction of the dipole of the substituent-to-carbon bond with an electrical charge or dipole generated during formation of the final or transition state(20). In certain carboxylic acids of rigid structure having the negative end of a C-Cl dipole closer than the positive end to the carboxyl group, a chlorine substituent is actually acid-weakening(19).

***Tautomeric* effects.** In my first earnest study of Ingold's scheme of electronic effects (Fig. 1), I was surprised by his characterization as "tautomeric" effects that had more to do with resonance than with tautomerism, as the latter had been taught to me. From Professors Leland Pence at Reed College and Ralph Helmkamp at the University of Rochester, I had learned in the early 1940's that tautomerism was a condition of facile interconversion of isomers, sometimes isolable, sometimes constantly in equilibrium with each other. I had also learned about the phenomenon of resonance(21), and particularly that a "resonance hybrid" is steadily in-between the canonical forms, *not* oscillating back and forth.
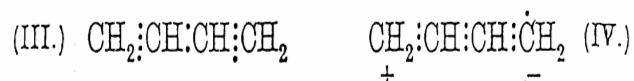
(III.) $CH_2{:}CH{:}CH{:}CH_2$ $CH_2{:}CH{:}CH{:}CH_2$ (IV.)
                                                              $\overset{+}{}$ $\overset{-}{}$

Figure 2. Polarized and nonpolarized forms of butadiene, from Robinson (1922)(17)

In the 1920's the distinction between tautomerism as we now know it and the yet-to-be recognized resonance was not clearly perceived. Thus Kermack and Robinson(17) wrote in 1922, referring to the electron-dot structures III and IV for 1,3-butadiene shown in Fig. 2,

> in III ... the system is probably an oscillating one, the terminal carbon atoms becoming in turn feebly electropositive and electronegative ...We suppose ... that the reactant takes advantage of these momentary manifestations of polarity ...

The following year Lowry(22) described the anion from acetoacetic ester as a "tautomeric ion." He also discussed the "tautomeric" ions of carboxylic acids, in which "the distinction between the single and double bonds disappears"(23). G. N. Lewis said, in his address at the 1923 Faraday Society Discussion(24):

> While there can be no question that tautomerism often represents a rapid equilibrium between two or more distinct substances, to each of which an orthodox formula may be assigned, apparently this is not always true. I hope that Professor Thorpe will present to us ... some of his extremely convincing evidence that there are tautomeric substances which are not to be interpreted by an oscillation between two distinct structures, but rather a single loose structure of intermediate character ...

In view of the widespread description as *tautomerism* of systems more or less recognized to be in a condition that we now call resonance, it is not surprising that Ingold(14) chose to call a mode of electronic interaction of a substituent group with a reacting system, in

which an electron pair of a double bond between two atoms shifts so as to become an unshared pair on one of the atoms so joined, or the reverse, a *tautomeric* "type of disturbance." He retained the term *tautomeric* in writing his 1934 review(11) and even in his 1953 book(1). In the second edition of the book, however, *conjugative (K)* succeeded *Tautomeric (T)* of his earlier writings.

Other electronic effects. Ingold and Shaw wrote, in 1927:

> Whilst inductive effects give rise to an essentially permanent (not necessarily constant) molecular condition, the tautomeric effect (apart, possibly, from a small permanent residuum) is essentially temporary in character, assuming much greater importance at the moment of attack by a reagent than in the quiescent period preceding it.

These views were repeated in 1928(25) and 1929(26). It will be noted that, despite a nearly rigid I/T permanent/transient distinction, he sensed that each might obtain to some extent in the time domain assigned to the other. In his 1934 review(11) two virtually new categories of electronic effect were introduced: *mesomeric (M)* for initial-state conjugative interaction of groups with an unsaturated system and *inductomeric (I)* for inductive-type electron shifts in response to the demands of attacking reagents (Fig. 1). Also, what had been termed *tautomeric* was renamed *electromeric (E),* with *tautomeric* being used to designate conjugative shifts both in the initial state and at reagent demand.

Fry(27), an American chemist, attempted in 1921 to explain numerous phenomena in terms of electronic tautomerism, defined as tautomerism involving electromers in dynamic equilibrium. One might liken that to resonance between canonical forms, but Fry's electromers were vastly different from canonical forms that might be proposed today. In his 1927 *Annual Reports* article(25), Ingold discussed "tautomeric (electromeric) transformations," acknowledging Fry. His utilization of *electromeric* was therefore precedented.

In 1934 Ingold employed an apparently novel term, *mesomeric* effect, to designate a molecular condition that had previously been recognized, although he had not emphasized it. The term is well-conceived; literally, a *mesomer* is a molecule in an intermediate state, or in more recent usage, a resonance hybrid. Some authors today use *mesomerism* for a condition of resonance. *Inductomeric* was also a late arrival in Ingold's set of electronic effects.

In discussing the inductive as contrasted to the inductomeric effect, Ingold wrote(11):

The terms 'polarization' and 'polarizability' have quantitative meaning in relation to a molecule, since each may be measured by certain accurately definable magnitudes.

Those were stated to be respectively dipole moment, μ, and polarizability, α, as evaluated from optical measurements. His inductomeric effect pertained however to through-bond electron shifts, whereas polarizability as evaluated from optical measurements relates to the general phenomenon of distortion, by a nearby ion or dipole, of an electron cloud somewhat loosely associated with its nucleus or nuclei. Intermolecular attraction due to dispersion forces is largely dependent on the polarizabilities of the molecules or ions involved. Deep in his 1934 review(28) Ingold acknowledged this point.

Synartetic Acceleration **versus** *Anchimeric Assistance.* Wagner-Meerwein rearrangements had attracted Ingold's attention even in the 1920's(18) and received significant experimental attention in his laboratories. In 1950-51, he proposed the terms *synartetic acceleration* and *synartetic ion(29-31)*. The former pertained to acceleration of solvolysis in substrates such as isobornyl chloride, attributed to participation by the electrons of a suitably juxtaposed C-C single bond in loosening the C-Cl bond, and the latter to the nonclassical ion so formed. Soon thereafter, Winstein(32) proposed *anchimeric assistance* for the same meaning as *synartetic acceleration*. Inasmuch as in the 'fifties and 'sixties the study of solvolyses was fashionable, especially in the US, and the research of the charismatic Winstein was the paradigm for the fashion, Winstein's term prevailed. In my judgment, Ingold's term was a good one, Winstein's unnecessary

Lapworth(33) in 1925 suggested the terms *anionoid* and *kationoid* for the meanings now conveyed by *nucleophilic* and *electrophilic,* respectively. *Anionoid* and *kationoid* were utilized in succeeding papers by Robinson(34) and by Lapworth and Robinson(35), to cite two examples. Ingold was doubtless aware of those terms, but he did not use them. He regarded classification on the basis of similarity to cations or anions to be unsound(36).

In a 1929 presentation of "the principles of aromatic substitution(26), Ingold used the term *electron-seeking reagent,* not so much to distinguish such a reagent from others but rather in a context in which it was pretty much taken for granted that species effecting aromatic substitution were electron-seeking. The understood presumption that aromatic substitution was facilitated by electron supply to the site of reagent attack prevailed in other Ingold papers in those years, without

being specifically stated. A 1934 Ingold paper(37) on modes of addition to conjugated systems, however, included the term *electrophilic* for a reagent that "demands a centre of high electron-availability." By 1935, Hughes and Ingold(38) were writing of *nucleophilic* and *electrophilic* reagents without special explanation.

Reactions. The familiar reaction descriptions, *substitution* and *elimination,* gained their present connotations surprisingly late. As late as 1933, in reports on the decomposition of
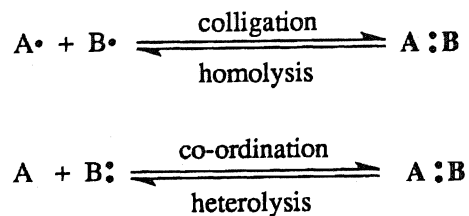
$$\{R^1R^2CH\cdot CR^3R^4\cdot NR'R''R'''\}^+ X^- \rightarrow R^1R^2C{=}CR^3R^4 + NR'R''R''' + HX \quad (A)$$

$$\{R\cdot NR'R''R'''\}^+ X^- \rightarrow RX + NR'R''R''' \quad (B)$$

Figure 3. Ingold's representation of reactions both called *elimination*(39)

quaternary ammonium salts(39) (Fig. 3) and tertiary sulfonium salts(40), Ingold and coworkers characterized as "elimination reactions" two competing routes that we know today as elimination and substitution. Evidence was presented that both modes of reaction might occur either by bimolecular reaction of X⁻ with the 'onium ion (pathway A2 or B2) or by unimolecular heterolysis of the 'onium ion to form a carbocation, which then coordinates with X⁻ to form RX (B1) or cedes a β-H to X⁻ to form the alkene (A1). The numerals 1 and 2 in these descriptions were intended to symbolize reaction molecularity(39).

Two years later, papers by Hughes and Ingold(38,41-43) utilized, without special comment, the terms *substitution* and *elimination* with their modern connotation as well as the now familiar symbols S_N1, S_N2, S_E1, S_E2, E1, and E2. The symbols used in 1933 mainly to delineate within one research paper different reaction mechanisms were transformed into symbols that served, besides that purpose, as general representations of reaction type and mechanism: A1 became E1; A2 became E2; B1 became S_N1; and B2 became S_N2.

With respect to both reagents and reactions, the change in terminology between 1933 and 1935 is remarkable. What factor was chiefly responsible? Did the new terms result from increasing influence by Hughes? Did conversations with American chemists in California(44) stimulate formulation of the new definitions and symbols?

The convenient and widely used terms, *heterolysis* and *homolysis,* were suggested by Ingold(45,46), seemingly as an afterthought following a Faraday Society Discussion. Ingold also proposed sensible nomenclature for the reverse of these processes, presenting the following diagrams:

$$A\bullet + B\bullet \xrightleftharpoons[\text{homolysis}]{\text{colligation}} A:B$$

$$A + B: \xrightleftharpoons[\text{heterolysis}]{\text{co-ordination}} A:B$$

Of the two terms for combination of species, *co-ordination* is familiar from inorganic chemistry and is utilized by Ingold for essentially the same meaning. *Colligation* is novel, a convenient, logical term, but one little used by radical chemists(47), even by C. K. Ingold's radical son Keith, who uses *combination* instead(48).

Symbols for Mechanisms

The genesis of E1, E2, S_N1, and S_N2 has already been discussed. Another group of Ingold mechanism symbols pertains to ester hydrolysis and related reactions, which had been of interest to him from the 1920's. That carboxylic esters may be hydrolyzed by action of either aqueous acid or alkali was common knowledge. Research in his laboratories and from other sources showed that the carbon-oxygen bond broken might be either
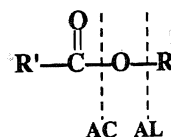
$$R'{-}\overset{\overset{\textstyle O}{\|}}{C}{-}O{-}R$$

$$AC\ \ AL$$

Figure 4. Alternative C-O bond cleavage in ester hydrolysis

that from the bridging oxygen to the carbonyl carbon (AC) or to the first carbon of the alkyl group (AL), as shown in Fig. 4. Moreover, the actual breaking of that bond might occur either unimolecularly in the ester or its conjugate acid, or bimolecularly thru attack of hydroxide ion or water on the relevant carbon atom. Unimolecular acyl-oxygen fission forms an acylium ion intermediate, while unimolecular alkyl-oxygen fission generates an alkyl carbocation.

In a 1941 review Day and Ingold(49) used a newly-devised set of symbols to represent the various possibilities. The symbols started with A or B, for acidic or basic, and ended with 1 or 2, for unimolecular or bimolecular. In between was a single or double prime (' or "), the single prime indicating acyl-oxygen scission and the double prime alkyl-oxygen scission. Thus, common saponification (which involves bimolecular attack of hydroxide ion at acyl carbon) was symbolized by B′2, while unimolecular scission of the alkyl-oxygen bond of the ester conjugate acid was respresented as A″1,

etc. This system allowed eight mechanism types to be represented by compact symbols, and facilitated representation of the various mechanisms in discussions. (For B″2, there was at that time no known example.)

In his 1953 book(1) Ingold modified these symbols to increase their clarity. The single prime was replaced by subscript AC and the double prime by subscript AL. Thus, B′2 became $B_{AC}2$, and A″1 became $A_{AL}1$, and so forth. In their 1953 form, the symbols relieve one from having to remember whether single or double prime stands for acyl-oxygen scission.

Some Other Terms

Of career-long interest to Ingold were rearrangements that he called, as many others do, *prototropic* and *anionotropic* rearrangements. Lowry wrote of *prototropy* in a 1923 paper(23) and, according to Ingold(50), in 1925 spoke *of ionotropy,* which implies *anionotropy* as well as *cationotropy*.

A convenient term frequently used in treatment of rate data for electrophilic aromatic substitution is *partial rate factor*. It is the part of the total rate constant for a substrate with more than one reactive site that pertains to reaction at one particular site; for example, one-sixth the total rate constant for nitration of benzene is the partial rate factor for one benzene carbon atom. It was introduced in a paper co-authored by Ingold, Lapworth, Rothstein, and Ward(51). Which co-author was principally responsible for initiating the term is unclear.

Related Terms, Proposed by Others, that Did Not Survive

In 1920, Lapworth(52) proposed words to describe the effects of substituent groups on acid dissociation constants:

> The writer would propose the term 'basylous' for a group such as -NH$_2$ which tends to lower the acidity of a molecule of which it forms but a part, and the term 'acylous' for a group such as CH$_3$CO which has the opposite effect.

These terms were used in 1923 by Lowry(53), but Lapworth did not make much if any use of his own invention in subsequent papers. For example, they were not used in a 1927 discussion(35) of the *ortho-para* ratio in aromatic substitutions; substituents were instead treated with respect to their "attraction on electrons." Lapworth was, in the early 1920's, much more interested in his "principle of induced alternate polarities"(8).

Lapworth proposed(33) in 1925 the terms *anionoid* and *kationoid* for reagents of opposite polar character, for the meanings now generally conveyed by *nucleophilic* and *electrophilic,* respectively. I must confess to finding these terms attractive when I first encountered them in the 1940's. Ingold eschewed them on grounds that they overemphasized the charge character of reagents(36). Without denying the validity of that criticism, I suggest that *nucleophilic* is defective to a similar extent in that it implies nucleus-seeking whereas a nucleophile actually seeks an atom with a real or virtual empty valence-shell orbital.

In a 1925 paper, Robinson(54) proposed terms related to the capacities of substituent groups to contribute or accept what we now call π-electrons to or from an unsaturated system; see Fig. 5. The terms are *crotenoid* for a system in which the substituent has contributed π-electrons to the unsaturated system, and *crotonoid* for the opposite. The structures in Fig. 5 are those of Robinson, in a newer notation, but intellectually equivalent to his. These terms were used to some extent by others(55) as well as by Robinson(34); indeed, he extended that terminology style to embrace *butadienoid, quinonoid, semi-crotonoid* and *allyloid*. Bearing in mind that he also characterized reagents as *anionoid* or *kationoid,* one speculates that some readers may have been annoyed. The ideas expressed by *crotenoid* and *crotonoid* could also be conveyed in Ingold's terms for electronic effects, which prevailed. In modern usage, *quinonoid appears* now and then, and perhaps *butadienoid*.
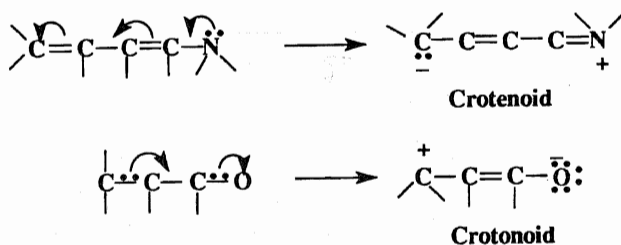

Crotenoid

Crotonoid

Figure 5. Robinson's 1925 depiction of *crotenoid* and *crotonoid* systems.

Some Terms Suggested by Bunnett

The following experiences will aid in the assessment of what characteristics in a newly proposed term contribute to its longevity in the language of chemists. Some of mine survived, whereas others died.

Names for Charge-Carrying Substituent Groups. Incidental to a review of aromatic nucleophilic substitution reactions(56), Roland Zahler and I introduced the

term *ammonio* for the -NH$_3$$^+$ substituent group, and suggested that substituted ammonio groups be named according to the same principle. In a later article(57), the proposal was extended to substituted sulfonio groups, for example, *dimethylsulfonio* for -S(CH$_3$)$_2$$^+$. In April, 1954, this suggestion, as well as the one below on naming substitution reactions, was submitted to Mary Alexander, Chair of the Committee on Nomenclature of the American Chemical Society Division of Organic Chemistry. She passed it on to Austin M. Patterson, who had in those times a regular column on nomenclature in *Chemical and Engineering News*. He reported it benignly(58). Shortly thereafter, H. J. Barber(59) disclosed that he had independently proposed the same nomenclature. *Ammonio* and *sulfonio* are now recommended by the IUPAC Commission on Nomenclature of Organic Chemistry(60).

Cine-Substitution. Zahler and I recognized(61) several kinds of substitution reactions that surprisingly introduced the entering group at a position *ortho* to the leaving group. These included prominently the von Richter reaction and some reactions of aryl halides with strong bases that we now recognize to occur via aryne intermediates. For such reactions, we proposed the term *cine-substitution,* from the Greek *cine,* to move. This term has gained wide usage.

Names for Substitution Reactions. My proposal, published by Patterson(62) in October, 1954, was that the name of a substitution reaction comprise these parts: the name of the incoming group, the syllable "de," the name of the departing group, and the suffix "ation," with occasional elision of vowels or introduction of consonants as occasionally necessary for euphony. I used this terminology in my own papers on aromatic nucleophilic substitution, but probably more important to its acceptance was reproduction of my proposal, with favorable comment, in the 1954 Editorial Report on Nomenclature of The Chemical Society(63). Also helpful were utilization of such names in an important monograph(64) and recommendation of them by IUPAC(65). These names are now widely used, and even abused(66).

The S$_{RN}$1 Mechanism. In 1970, Jhong Kook Kim and I(67) found evidence that certain reactions that might appear to be aromatic nucleophilic substitutions actually occur by a radical mechanism previously recognized for aliphatic reactions by other chemists(68). Inasmuch as a characteristic step in this then-unusual mechanism is the fragmentation of the radical anion of the substrate, to form an aryl radical, we suggested the mechanism be symbolized S$_{RN}$1, by analogy with the well-known S$_N$1. (The step, ArX$^{\bullet}$ — —> Ar$^{\bullet}$ + X$^-$, resembles the rupture

step of S$_N$1, except that the reactant and the organic product are one electron richer.) Since 1970 there has been quite a bit of study of such reactions, for which the symbol S$_{RN}$1 is generally used.

Names for E2 Transition States. In a 1962 review of bimolecular olefin-forming elimination reactions(69), I presented arguments that the E2 transition state need not be synchronous, in that rupture of the C$_\beta$–H bond may be much more advanced than of the C$_\alpha$–X bond, or *vice versa*. In an extreme case of the former, C$_\beta$ has much carbanion character, while at the other extreme C$_\alpha$ has much carbonium ion(70) character. I called these extremes *nearly carbanion* and *nearly E1*.

That concept received favorable attention from mechanisms chemists in the 1960's. In a review written in 1967(71), I advocated, on the indirect advice of a classical scholar at the University of Canterbury, New Zealand(72), for the transition state extremes instead the terms *paenecarbanion* and *paenecarbonium,* these being derived from the Latin *paene,* "almost." These terms have received little use, however. A major reason is that my concept of a variable E2 transition state was subsumed into a more general variable transition state theory proposed in 1970 by More O'Ferrall(73), which is now widely employed. The terms themselves may, however, have been defective, as I speculate below.

The Usual Mechanism of Aromatic Nucleophilic Substitution. In 1951, Zahler and I(74) proposed that the usual mechanism involves two steps: the nucleophile attaches to the site of substitution, and then the leaving group departs. However, we passed up the opportunity to propose a special name or symbol for this mechanism. Later, Parker(75,75) proposed for it the convenient symbol S$_N$Ar, which is now generally used(77).

Treatment of Acid/Base Equilibria in Moderately Concentrated Sulfuric Acid *via* Linear Free Energy Relationships. Hammett's H$_0$ acidity function(78) was a pioneering achievement, but his method actually was accurate for but a limited range of bases. As other acidity functions were evaluated for other classes of bases, the field became complicated. In 1966 Olsen and I showed, however, that H$_0$ alone was sufficient for accurate treatment of such equilibria for bases in general, if it were employed in a new way(79), which was of the character of a linear free energy relationship (LFER). Our treatment involved plotting data against the parameter (H$_0$ + log [H$^+$]). A few years later Cox and Yates(80) developed another LFER, related in principle but different in approach; in their method, data were plotted against parameter X. In magnitude, X and (H$_0$ + log [H$^+$]) are, however, very similar(81). Perhaps because

their parameter is simpler(82), and also because they were diligent in publishing a series of papers on their treatment, their modification is today more often used than our original method.

Attachment **and** *Detachment.* For several years I was a member of the IUPAC Commission III.2, on Physical Organic Chemistry, the main concern of which was terminology in the field. In developing a general system for naming transformations(65), we limited the term *addition* to its common use in describing the bonding of *two* moieties, e.g., to an alkene or a carbene, and advocated that the bonding of *one* moiety to a single site be called *attachment.* Conversely, *elimination* connotes the release of two moieties, and *detachment* for the release of one from a single site. These usages have not yet become general; thus, the S_NAr is still sometimes referred to as an *addition/elimination* mechanism; it is better called an *attachment/detachment* mechanism.

Hydron. Commission III.2 also dealt with a common irregularity in the language of physical organic chemists, namely, that *proton* was used both for $^1H^+$ and for H^+ of whatever isotopic nature. That led to such nonsensical concepts as the kinetic isotope effect in proton transfer reactions. Rigorously, the proton is H^+ of mass unity, and no isotope effect is measurable if all atoms of an element are of the same isotope. Accordingly, Commission III.2 proposed(83) *hydron* as the term for H^+ of whatever isotope, restricting *proton* to $^1H^+$. Chemists, have however, been slow to relinquish the familiar if irregular use of *proton* for all monohydrogen cations.

Perspectives

Ingold's numerous contributions to the terminology of physical organic chemistry tell us something about Ingold the scientist. Another aspect is what significance they had for the development of the field. Finally, from consideration of Ingold's contributions to terminology as well as those of others, including my own lesser efforts, we may gain some insight into what factors determine whether a new term will become part of the basic language of the field or will wither on the page.

What Ingold's Terms Tell about Ingold. First, an orderly mind is revealed. One senses that from his study of various questions, he was able to discern features he wished to stake out in his mind, somewhat as a prospector might mark a discovery of gold. For that purpose, attaching definitive labels was helpful. It appears that his terms first served that private purpose. Some of those "private" labels appear in early publications on various topics; for example, A1, A2, B1 and B2 pathways for

decomposition of quaternary ammonium salts *(vide supra).*

Second, they indicate something of his quality as a teacher. Often he perceived dichotomies and chose terms or symbols that would effectively contrast differing behaviors. Thus, *inductive* vs. *tautomeric, nucleophilic* vs. *electrophilic,* S_N1 vs. S_N2, and E1 vs. E2. For his purposes as a teacher, introducing terms that effectively contrast one feature from another was useful. Relabeling A1 as E1, B1 as S_N1, etc., as mentioned, served pedagogical purposes.

We have also seen that Ingold often chose terms that were part of the current language of the field, giving them however more precise definitions. *Inductive, tautomeric, electromeric,* and *prototropy* are examples. Noteworthy, however, is his introduction of *nucleophilic* and *electrophilic* in place of *anionoid* and *kationoid,* which had been proposed years earlier(33) for the same meanings. *Elimination* and *substitution* had been in the language of chemists for many years, but Hughes and Ingold(41,43,84) in effect redefined them in the way we understand them today.

Significance of Ingold's Terminology. Its significance can be evaluated with respect to two standards: how valuable were his terms in years immediately following their development, and what use do they enjoy in the current language of scientists?

Ingold's terms were indeed important in the former sense. Besides his original research contributions, Ingold was an intellectual systemizer and a teacher. In times when many others had only murky perceptions of new developments, he got them straight in his own mind and repeatedly offered his understanding to his fellow chemists in review articles as well as in research papers. He was a teacher not only to traditional students, but also to his contemporaries. He became the effective leader in reconstruction of the intellectual outlook of organic chemists; terminology that helped organic chemists in the classical tradition to grasp the new concepts was important to his mission.

Now about the endurance of his terms: of those in Fig. 1, *inductive effects* are part of the standard language of physical organic chemistry. *Mesomerism is* considerably used to designate conditions of resonance. The others are seldom used today. Why? In large part because two structures of theory that were little known in the years 1926-34, transition state theory and quantum mechanics, are now foundations to the field. Those other terms no longer seem needed.

Most of the rest of the Ingold terms that I have discussed are central to modern physical organic chemis-

try, in both the thinking of chemists and in their language. As mentioned above, *synartetic acceleration* did not however survive. *Colligation* has not been much used, nor are Ingold's symbols for the various carboxylic ester hydrolysis mechanisms.

Factors That Affect Whether a New Term Will Endure. The following thoughts are based mainly on consideration of Ingold's terms, as well as the few that I have suggested.

That it represent a new substance or concept. Examples include several terms introduced by Woodward and Hoffmann(85) in connection with orbital symmetry principles, as well as benzyne, ferrocene, and, it appears, fullerene. From Ingold's terminology contributions, S_N1, S_N2, E1, and E2 endure in part for the same reason. Prior to 1933(39), what we now call aliphatic nucleophilic substitution reactions and olefin-forming eliminations were all more or less assumed to be of bimolecular character.

That it state concisely an idea otherwise cumbersome to express. A good example is $S_NAr(75)$ to designate what I had been calling the intermediate complex mechanism of aromatic nucleophilic substitution. Another is $S_{RN}1$, which seems more efficient than longer terms such as "substitution reactions which proceed via radical anion intermediates"(86). Terms of that sort had been used by Kornblum, who continued to use them, eschewing $S_{RN}1$, long after the latter had come into general use(86,87).

That it replace an existing term generally regarded as deficient. Ingold's set of electronic effects (Fig. 1) might be cited in this regard, although they supplanted not only the terminology but also the concepts of alternate polarity theory.

That it be uncomplicated, easy to remember, pronounce, and spell. That *synartetic acceleration* did not survive was perhaps due in minor part to this factor; I have trouble remembering how to spell *synartetic*. Possibly the seemingly greater acceptance of the Cox-Yates than of the Bunnett-Olsen LFER stems in part from the seemingly greater simplicity of X than ($H_0 + \log [H^+]$).

That terms in a set to contrast two or more concepts be quickly distinctive. Nucleophilic vs. electrophilic is a good example, as also are S_N1 vs. S_N2, and E1 vs. E2. Deficient in this regard are *paenecarbanion vs. paenecarbonium;* although intended to represent extremes, the first three syllables, which receive the main accent, are identical. Robinson's *crotenoid* and *crotonoid* are deficient in much the same way.

Diligence in employment of the term, a very important factor. One reason so many of Ingold's terms

are part of the modern language of physical organic chemistry is that he used them repeatedly in experimental reports and reviews. (It also helped that his experimetal reports were scientifically substantial.) That caused them frequently to enter the minds of scientists keeping abreast of modern developments. Lapworth was not diligent in using his *acylous* and *basylous,* even in his own later publications. Robinson was more faithful in using *crotenoid* and *crotonoid,* as well as *anionoid* and *kationoid,* which he favored; but Robinson turned his research focus away from reaction mechanisms toward synthetic chemistry. After 1930, he published little in physical organic chemistry. Here, as elsewhere, the principle "out of sight, out of mind" is relevant.

Official adoption. That my scheme for naming substitution reactions *(vide supra)* was generally adopted was no doubt assisted by its endorsement by The Chemical Society(63). On the other hand, IUPAC recommendation of schemes for naming elimination, addition and insertion transformations(65) has had little apparent effect. Systems (not discussed above) recommended by Commission III.2 for naming transformations of those types have found little use. Official adoption had nothing to do with furthering use of the numerous terms proposed by Ingold.

Fashion. This is a major factor. That *anchimeric assistance* prevailed over *synartetic acceleration* in the usage of the throng of American solvolysis chemists of the third quarter of this century was considerably the outcome of fashion; Winstein was in vogue with that crowd; Ingold was not, as discussed above. Fashion powerfully influences the choices chemists make as to what to study and what language to use in reporting their results.

Epilog

In studying and evaluating with the wisdom of retrospection the vocabulary, and to some extent the scientific contributions, of great figures like Lewis, Lapworth, Lowry, Robinson, and Ingold, one feels more than usual humility. Had I been a scientist at the time, would I have been able to make even one of the conceptual advances that they made?

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COMING EVENTS

ACS National Meeting, April 13–17, San Francisco, CA. [see *Chem Eng. News* for more details]

ACS Great Lakes Regional Meeting, May 28-30, Loyola University, Chicago, IL. History of Chemistry Symposium, Dr. Elsa L. Gonzalez, Chair.

ACS National Meeting, September 7–11, Las Vegas NV.

INGOLD, ROBINSON, WINSTEIN, WOODWARD, AND I

Derek H.R. Barton, Texas A&M University

The hero of this article, Sir Christopher Ingold, was born in 1893 and died in 1970, nearly 25 years ago (1). Ingold had a profound influence on the development of physical organic chemistry, especially in the seminal work carried out in collaboration with the late Professor E.D. Hughes.

Ingold was the only son of William Kelk Ingold. For family reasons, he took his B.Sc. as an External Student of London University at Southampton in the South of England in 1913. He proceeded promptly to Imperial College and began his collaboration with Jocelyn Field Thorpe (for many years Professor of Organic Chemistry at that institution). After a detour into industry, Ingold returned to Imperial College in 1920 and then left in 1924 at the age of 31 (in the same year as his election to the Royal Society) to become Professor at Leeds. He returned to London in 1930 as Professor at University College and stayed there until his retirement in 1961 and as Emeritus until his death in 1970.

Ingold was clearly a boy genius who could turn his mind to any aspect of chemistry. His early work with Thorpe led to the discovery of the Thorpe-Ingold Effect (2), which still remains an important experimental generalization even though the explanation given by conformational analysis is different from that first offered.

From early days in 1922, Ingold was fascinated by the problem of the structure of benzene (3). From 1925 onwards, he added electrophilic aromatic substitution to his interests (4), as well as alternating effects in carbon chains. Robinson was shown to be in error in some of his arguments (5). It was Leeds versus Manchester, later to become Oxford versus University College, London. In a few years, thirty-two papers appeared in the *J. Chem. Soc.* on these aspects of organic chemistry.

Ingold became interested in the titration curves of dibasic acids and published three papers on the subject (6) in 1928-1929 with R. Gane. These papers eventually led me to my first interview with Ingold. It was 1946 and I was interviewing with a Fellowship Committee at Senate House in London. Ingold was chairman. He asked me what I intended to do. l was planning to apply the results of Gane and Ingold to determine the distance apart of two carboxyl groups (or other acidic functions) in triterpenoids. Ingold brightened up at once and started to talk about his work. However, I detected a misquotation of his own work. I quickly pointed out his error. He seemed surprised, but not impolite. The interview promptly ended and on reflection, I felt that it was I who had made the error in correcting the words of the great master. However, I got the fellowship and published an impressive looking article on the subject (7).

The move to London brought the first meeting between Ingold and E. D. Hughes. This meeting was prolonged from 1930-1944 and renewed (when Hughes returned from his stint in his native Wales) from 1948-1963.

While still at Leeds, Ingold had been interested in elimination reactions. As he put it in his own words, "my purview expanded to embrace the whole of organic chemistry, a process that conceptually was complete by 1927." Robinson had exactly the same confidence in his own universality.

At University College, the interest in elimination reactions was harmoniously interwoven into the famous substitution reactions of the S_N1 and S_N2 classification. The Golden Years of collaboration with Hughes had begun. To S_N1 and S_N2, mechanisms were added in a harmonious paradigm–E1 and E2. Organic chemists rarely made kinetic studies on preparatively significant

organic reactions at that time. Hughes and Ingold opened a new era of precision (8). One of the noteworthy accomplishments was a revelationary clarification of the hitherto mysterious Walden Inversion.

While on sabbatical leave at Stanford in 1932, Ingold decided to write a summary on the principles of an electronic theory of organic reactions. This duly appeared and created a lot of attention. Ingold wrote clearly, using the simplified nomenclature that is still in use today. As Ingold modestly put it (9):

> The new work made it inescapably clear that the old order in organic chemistry was changing, the art of the subject diminishing, its science increasing: no longer could one just mix things: sophistication in physical chemistry was the base from which all chemists, including the organic chemist, must start.

Robinson was infuriated by this article. It was a "clash of the Titans." Ingold in one short review article had replaced the abstruse and sometimes obscure language of Robinson by a much simpler vocabulary. He had also shown the value of the precise kinetic studies carried out in collaboration with Hughes which justified the Ingoldian quote above.

The American organic chemical scene of the time was also not always appreciative. Ingold and Robinson could be put in the same basket and labeled "the English Heresy." Since Hughes was also involved, it would have been more accurate to call it "the Anglo-Welsh Heresy."

However, over the next ten years, American organic chemistry changed and by 1948, University College had become the mecca of American professors on sabbatical leave and of many Fulbright scholars. It was as if Ingold were a reincarnation of Allah. His lectures were indeed like sermons. He spoke with wonderful clarity and precision. He started with the proposition, followed by the evidence sometimes provided by a mysterious Finkelstein (Saul Winstein whispered to me once, "Who is he?"); but all was illuminated as we approached the beautiful revelation of truth. A slight descent to a conclusion that Ingold was indeed a "prophet of science" completed the lecture. Could one ask questions after this? Not an English audience; Robinson never attended. American audiences in later years did.

I remember once at Manchester at a Chemical Society meeting, Hughes, Ingold, Winstein, and I were all on the program. Hughes and Winstein had an interesting discussion. Winstein asked Hughes what he would call a reaction which was neither S_N1 nor S_N2. Hughes thought about this heresy for a few moments and then replied, "A bastard." Everybody within hearing distance laughed.

The Golden Years of Hughes and Ingold were interrupted by the Second World War. University College Chemistry Department was sent off to the wilds of Wales at Aberystwyth. The more prosaic Imperial College never left South Kensington. I was there until the end of 1944. They were exciting, nonchemical times!

At the end of the war, the precise kinetic measurements resumed, especially on elimination reactions (10). In 1946 Hughes and Ingold published a seminal paper on steric effects and the Wagner rearrangement (11). Later, work on the solvolysis of bornyl and isobornyl chlorides led to the postulate of synartetic acceleration involving synartetic ions (12). At the same time, Winstein was studying the same phenomenon under the name of anchimeric acceleration. Hughes and Ingold were probably surprised to realize that they had an American rival worthy of their own genius. The same applied to the relationship between Robinson and Woodward. It was very dificult for Sir Robert to realize that another intelligence, fully the equal of his own, had entered upon the alkaloidal scene. R. B. Woodward liked to tell the following story about the structure of strychnine. In 1947, Woodward and Robinson had dinner together in a New York restaurant, Sir Robert being the host. As the meal progressed, Woodward told Robinson that he had been thinking about the structure of strychnine. Since Robinson had a group of fifteen or so workers concentrated on this subject, he had to admit that the problem was interesting. Woodward asked Robinson what he thought about the true—as later revealed—structure of strychnine and wrote it on the paper table cloth. Robinson looked at it for a while and cried in great excitement, "That's rubbish, absolute rubbish!" So ever after, Woodward called it the rubbish formula and was indeed quite surprised to see, a year later in *Nature*, that this was also the formula deduced eventually by Robinson. Woodward should have sent the table cloth for publication!

Back in London, Hughes and Ingold started to produce a memorable series of papers on the nitronium ion and its role in aromatic nitration. All the available physical tools were brought to bear upon the identification of the nitronium ion. This work, which undoubtedly had its inception in the wartime fabrication of explosives, is presented in one of the many papers published in 1950(13).

Hughes and Ingold were also the first to systematize the mechanisms of hydrolysis of esters. Six different mechanisms involving acyl-oxygen and alkyl-oxygen cleavage were presented. The first pioneer-

ing paper was in 1939 (14), while a more general discussion was given in the memorable year of 1950 (15).

Ingold showed, in general, a fine perception of the importance of the reactions on which he worked. A reaction which was not quite in that category was the benzidine rearrangement, a fascinating problem by the standards of the day. Earlier work by Ingold in 1933 had established that the rearrangement of hydrazobenzene to benzidine was intramolecular. A short note in 1941 correlated the benzidine rearrangement with electrophilic aromatic substitution (16). A deeper analysis in 1950 (17) gave rise to a general understanding in 1964 (18). Sadly, the last paper tells us that Ted Hughes had already left the scene, slain by a cancer.

Ingold never forgot his love for the structure of benzene. He returned to the problem when deuterated benzenes became available in the 1930's. Suitable deuterium substitution destroys the symmetry of the molecule and allows a much more profound analysis of the first excited UV band. To present all his post-war work on this subject, Ingold filled a complete monthly issue of the *J. Chem Soc.*–a feat never equaled by any other author. Winstein, who was very impressed by this work, once asked me whether Ingold really understood the theory of what he was doing. I assured him that Ingold was a superior brain and that he did. The work was summarized in Ingold's second Presidential Address to the Chemical Society (19).

When in 1961 Ingold changed from the status of Professor to that of Emeritus Professor, he did not loosen his grip on the frontiers of chemistry. His last twelve publications demonstrate well the majesty of the man. In 1965 there appeared his fourth paper on octahedral substitution in cobalt complexes (20)–a contribution to inorganic chemistry. In the following year, we see the monumental proposals (21), still in general use today, on the specification of molecular chirality. In 1967 and 1968, Ingold returned to the benzidine rearrangement with four papers of his usual high quality. He summarized it all in an Israeli publication (22).

Then in 1968, he also published a paper on electrophilic substitution in organomercury compounds which was followed a year later by two additional papers, the last with M. D. Johnson (23). His final publication was a true monument to his genius. It was the second edition (first edition in 1953) of his book *Structure and Mechanism in Organic Chemistry*. It seems that he then laid down his pen for the last time.

C. W. Shoppee has paid a well-merited tribute to Ingold (1):

> If we stand back from the detail and look down a vista of some 50 years, the conclusion is inescapable that Ingold had extraordinary imagination, insight, initiative and ingenuity, that he possessed one of the greatest intellects in chemistry in the 20th century, and that he added a new dimension to organic chemistry. *Amplius alteris providebat.*

I fully concur with these words and so one can return to the rumors current in the late fifties and in the sixties. One heard the question, "Why has Ingold not received a Nobel Prize?" Another question, more American this time, was posed, "Why have Ingold and Winstein not shared a Nobel Prize?"

Winstein was born in 1912. His *Biographical Memoir* (24) shows that, like Ingold, he was a boy genius. Before he started his Ph.D. at Cal. Tech., he had taken an A.B. and then an M.A. at UCLA. This produced eight publications in collaboration with the late Professor W. G. Young *before* he became a graduate student. After the Ph.D. with Professor H. J. Lucas and a year at Harvard as National Research Fellow, he started his academic life as an Instructor at Illinois Institute of Technology. R. B. Woodward told me once that Winstein, who was nominally collaborating with P. D. Bartlett at Harvard, in fact, spent most of his time discussing chemistry with R. B. The discussion was surely mutually beneficial. Winstein was a much better practical chemist than R. B. After one year in Chicago, Winstein was appointed Instructor at UCLA. He rapidly climbed the academic ranks and six years later, in 1947, he became a Full Professor. He gained rapid fame for his work on neighboring group participation, on intimate and solvent-separated ion pairs, on anchimeric assistance, and on aromaticity. He died in 1969, one year before Ingold, from a heart attack. Neither man spared himself in his personal crusades for truth. So why was the Nobel Prize not awarded in time?

The answer surely lies in the machinations of Sir Robert Robinson, for whom there is an excellent biography (25). Sir Robert, as will be realized from remarks already made, was strongly opposed to any recognition of Ingold. Robinson had a brilliant career and spent 25 years in Oxford (1930-1955) as Waynflete Professor. He received the Nobel Prize in 1947 "for his investigations on plant products of biological importance, especially the alkaloids." Sir Robert had a special friend in Stockholm, Holger Erdtman, Professor at the Royal Institute of Technology. One can surmise that Sir Robert's candidature was discretely aided and perhaps Sir Christopher's was not.

In any case, Sir Robert's autobiography (26) says it all. In Chapter XI (p. 184) under "Development of an Electronic Theory," Sir Robert modestly wrote about himself, "The development of these ideas constituted, in the writer's opinion, his most important contribution to knowledge."

About myself, all has already been written (27). After the four Titans I have described, I was the lesser being who knew both some chemical physics and a reasonable amount of natural product chemistry. At the right moment in time, this produced a seminal article (28) and nineteen years later in 1969 a Nobel Prize shared with Odd Hassel. I was fortunate, for Sir Robert gave it his reserved approval. I plan to be alive in the year 2000 to celebrate 50 years of conformational analysis!

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THE BEGINNINGS OF PHYSICAL ORGANIC CHEMISTRY IN THE UNITED STATES (1)

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What is this physical organic chemistry that we emphasize in celebrating the centennial of the birth of C. K. Ingold? Some will say it is the study of organic reaction rates and mechanisms, others will include structural organic chemistry, and still others will bring theoretical chemistry under the tent. Whatever it covers, the general recognition of physical organic chemistry as a discipline in its own right is hardly more than sixty years old. However, there have been a great many workers studying reaction rates and mechanisms, structural and theoretical organic chemistry for more than one hundred years.

Another question is who are the real practitioners of physical organic chemistry. I chose to distinguish between what I call *organophysical* and *physical organic* chemists. I look at the first group as physical chemists, who, for one reason or the other, decided that the study of organic compounds needed their physical or theoretical tools to solve structures and/or to interpret reaction rates and reaction mechanisms. Some of the early key players on this side of the fence included G. N. Lewis, Linus Pauling, Henry Eyring, L. P. Hammett, K.S. Pitzer, and Richard Ogg.

The second group I take to be those who started with a more solid organic chemistry background and borrowed what they needed from the physical side to investigate mostly reaction rates and mechanisms. Included here are such worthies as J. B. Conant, F. C. Whitmore, Howard Lucas, M. S. Kharasch, Frank Mayo, W. G. Young, G. W. Wheland, Paul Bartlett, Frank Westheimer, and Saul Winstein.

The difference between these two groups of players is in the way they use organic compounds. The physical side almost invariably works with compounds off the shelf, or donated to them by their organic colleagues. The organic side, trained in the classical mode with a wealth of synthetic experience, has in their hands the power to synthesize compounds that they can use to test particular predictions, or otherwise be critical to understanding structural elements relevant to the reactions they are studying.

I justify my definition of physical *organic chemistry* on the basis of the immense vitality that the organic chemists have brought to the field, and I choose in what follows to trace the course of the development of the field primarily and chauvinistically from the organic side. This approach greatly cuts down the task of covering the subject and leaves what I regard as the most interesting part of it.

The first person that I am aware to define physical organic chemistry, by that name, was Louis P. Hammett of Columbia, who made an indelible impression on studies of reactivity by organic chemists. Hammett's book of the title *Physical Organic Chemistry*(2), published in 1940, gave validity to the idea that here was an important new discipline and many organic chemists happily clambered aboard the ship. I think some liked the name, because it looked like they were a higher calling of organic chemists, just as some of my physical chemistry colleagues really like to be known as chemical physicists.

Great and influential books often have great prefaces. The one written by G. N. Lewis for his monumental book *Thermodynamics*(3) is classic in which he likened the development of chemistry to the building of a cathedral. The preface to Louis Hammett's book begins(2):

> It is one of the commonest occurrences in the development of science that necessary subdivision of the field leads to a temporary neglect of phenomena lying on the borders between specialized fields. Sooner or later the deficiency becomes too patent to overlook, and a new specialty makes its appearance. Something of this sort has been happening in the last

two decades on the borderline between physical and organic chemistry. For a time it was almost a point of honor with both physical and organic chemists to profess ignorance of the other's field, and it remains a useful defense mechanism, if any is needed, to excuse the fact that specialization entails limitation as well as intensification of knowledge. Meanwhile there has grown up a body of fact, generalization, and theory that may properly be called *physical organic chemistry*. The name implies the investigation of the phenomena of organic chemistry by quantitative and mathematical methods.

He later goes on to say in a lovely paragraph, as pertinent today as fifty years ago:

> A physicist colleague once mockingly referred to this kind of investigation as the study of soapmaking, whereas any respectable chemist must today busy himself with the chemistry of the nucleus. The remark underestimates both the theoretical and practical significance of soapmaking. Soap is by no means a negligible factor in human civilization; I am not sure that we know more about the fundamentals of soapmaking, which is to say ester hydrolysis, than we do about the nucleus; I think the theoretical problems involved are quite as interesting; and I am convinced that an understanding of the mechanism by which complex naturally occurring substances, the enzymes, accelerate hydrolysis would lead to a great advance toward the interpretation of the phenomena of life.

Louis Hammett might be affronted by my suggesting that he was really an organophysical chemist, but he and his coworkers certainly didn't synthesize many compounds. In the logical extreme, I suppose Sir Christopher Ingold might belong over the fence in the organophysical group, because he didn't do much synthesis either, despite the fact that he received his DSc with J. F. Thorpe. Perhaps he indeed he could, but the mechanisms of the reactions of methyl, ethyl, isopropyl and *tert*-butyl compounds were so poorly explored in his time that there was not much need to go a lot farther than the nearest shelf.

Having defined the field to my own taste, I turn to the question of how and when did physical organic chemistry get started in the United States. In what follows, I will ignore what it was called and focus on organic chemists who were doing what resembled physical organic chemistry as we think of it today.

In Melvin Calvin's recent autobiography, *Following the Trail of Light, A Scientiffc Odyssey*, you will find the following statements about his book with Branch (5), here somewhat abridged(4).

Actually, Hammett's book was published a year earlier and he had published a substantial review (6) of his ideas on reactivity of organic compounds six years before that.

Calvin goes on to say:

> The publication of *The Theory of Organic Chemistry* in 1941 by Branch and me was the *beginning of theoretical organic chemistry in the United States* (emphasis supplied). Our book in effect organized all of organic chemistry in terms of electronic theory. Physical organic chemistry at Berkeley and the United States depends on Gilbert Lewis's electron-pair bond and Branch's evolution and development of that concept and stimulus not only for structure but for reactivity as well. The publication of the book made Berkeley one of the foremost centers in the United States for theoretical organic chemistry.

These statements ignore a lot of history. To be sure, Berkeley did become a center of physical organic chemistry, but less from the Branch and Calvin connection, than from the efforts of Streitwieser, Jensen, Dauben, Noyce, and so on who had very different roots. Indeed, the Branch and Calvin book, while excellent, in as far as it went, was substantially less influential than Hammett's book. Even more important with respect to the beginning of the field, Hammett himself recognized in his preface that what he recognized as physical organic chemistry had been going on at least two decades earlier.

What about before that? Stanley and Ann Tarbell, in their very useful book, *Essays on the History of Organic Chemistry in the United States, 1875-1955* (7) divide organic chemistry in the United States into three different periods. They take physical organic chemistry *per se* as starting in 1914, but they also define a period of studies on reaction rates and mechanisms from 1876-1913. Actually, the first reference they give is to R. B. Warder of the University of Cincinnati in 1881, the pioneer in the quantitative study of alkaline hydrolysis of esters. Warder showed the rate to be proportional to the product of the concentrations of ester and hydroxide ion (8). He also measured the changes of rate with temperature (9).

The Tarbells also comment on E. Emmett Reid of Johns Hopkins University. Reid lived to be 101 and was active to the end, writing a book, *My First Hundred Years* (10). His early research was on acid-catalyzed amide reactions (11-13), but he was particularly well known for his work on divalent sulfur compounds (14).

Arthur Michael (15) (Tufts and Harvard Universities) was another in this period who did what we could

call physical organic research. Michael was a firm believer in the application of thermodynamics to organic chemistry (16), but his efforts in this direction were couched in terms not easy to understand today and, at times, there seems to be confusion of enthalpy and free energy, as well as hand-waving. Among other things, Michael showed that the order of esterification of alcohols with trichloroacetic acid followed the order: methyl > ethyl > n-propyl > isobutyl and found that tertiary alcohols were slow(17).

Julius Stieglitz(18) (University of Chicago) made several contributions to organic mechanisms. In an 1899 paper(19) on imido compounds, he shifts gears and espouses the probability of carbonium ions as reaction intermediates; a concept he said Ostwald rejected out of hand. Another worker on organic reactivities was James F. Norris(20) (MIT), whose name endures in the Norris Awards of the Northeastern Section of the ACS. Norris apparently was the first to isolate triphenylmethyl carbonium salt, but he failed to recognize that carbonium ions might be involved in the formation and hydrolysis of *tert*-butyl chloride(21). Much of the early Norris work on reactivities was rather qualitative, but he did develop some useful ways of studying relative rates, such as of carboxylic acids with diaryldiazomethanes (22), although these rates were measured by gasometric rather than spectroscopic means.

Another pioneer that most of us know little about was Solomon F. Acree (Johns Hopkins University). The Tarbells(7) compare his work between 1904 and 1914 on kinetics and reaction mechanisms favorably with that of England's Arthur Lapworth, whom many regard as the father of physical organic chemistry. If you read some of Acree's very substantial output of papers, you will see that he was a person far ahead of his time with intelligence, style, and clarity. Among other things, he studied the mechanism of oxime formation, and suggested steric crowding to account for the difference in rate between acetone and diethyl ketone(23). His mechanism for oxime formation is quite modern, except that he had hydroxylammonium ion attacking the carbonyl compound in the rate-determining step. More than twenty years later, J. B. Conant(24) made a similar mistake for diazo coupling with phenols even though he had the benefit of, and used, the electronic theories of G.N. Lewis.

Acree(25) also studied the metathesis reactions of halides with alkoxides and other nucleophilic reagents. In this work, he examined a number of mechanistic possibilities involving ionic intermediates and clearly defined a number of intelligent questions as to what needed to be found out. Acree also considered the importance of association and dissociation in the reactivity of metallic alkoxides(26,27). Furthermore, he had some rather forward-looking ideas on enzyme mechanisms and recognized the possibility of an enzyme-substrate complex(28). Acree had a monumental polemic with Julius Stieglitz that matched, if not surpassed, the invective of the Ingold-Robinson battles some twenty years later(28). Acree clearly was a person who deserves a much better place in our consciousness as a really great, very early, physical organic chemist.

In their chapter about early studies of rates and mechanisms, the Tarbells omit three individuals whom they cover in other chapters and were enormously influential. One is J. U. Nef(29) (University of Chicago), who both introduced the idea of carbene intermediates and then oversold it by believing that carbenes were more common intermediates than they actually are(30). There also was Moses Gomberg(31) (University of Michigan) and his renowned work on the triphenylmethyl radical(32) which, it turned out was vigorously opposed by James F. Norris(33) of MIT. Last of the three was E. P. Kohler(34) (Harvard University), a person not heard so much about today as thirty years ago. Kohler discovered 1,4-addition of Grignard reagents to α, β unsaturated carbonyl compounds as the result of adding phenylmagnesium bromide to benzalacetophenone(35). Kohler was an extraordinary teacher and spawned a number of brilliant doctoral students. His greatest contribution to physical organic chemistry was James B. Conant of whom more will be said shortly. The unusual reasons for Kohler's success as a teacher are movingly described in a short biography written by Conant(34).

I think it is fair to say that US physical organic chemistry got off to a very good start even prior to 1920, at which point Hammett suggests it began to emerge as a recognized discipline. The Tarbells divide their essays on US physical organic chemistry after 1913 into the periods of 1914-1939 and 1940-1955. The latter of these two periods is clearly the start of the relatively familiar modern physical organic era of Bartlett, Westheimer, Winstein, and so on that will not be covered here.

The great figures in the field between 1920 and 1940 seem to me to be Louis Hammett, Howard Lucas, Frank C. Whitmore, Morris Kharasch, and James B. Conant.

Hammett did wonderful work on the physical chemistry of acids and bases, particularly acidity functions(36-38) and relationships connecting structure with reactivity (6). Both of these were absolutely vital to the success of the emerging discipline, but he did not himself beget a lineage of students to carry on behind him, even though many others used and extended his ideas.

Howard Lucas(39) was a very perceptive and kindly man who only had a master's degree (Ohio State). Legend has it that he was hired by A. A. Noyes to teach organic chemistry at Caltech, but not bother the other chemists by doing odorous organic research. In the then, and still, yeasty Caltech environment, Lucas thrived and went on to start a substantial line of distinguished workers in physical organic chemistry. Lucas had two particularly outstanding students. One was W. G. Young, who is best known for his work on allylic rearrangements. The other was Saul Winstein, who had an enormous impact on the study of carbonium ions and neighboring group effects. Lucas started Winstein down the path on the stereochemistry of neighboring group effects in the course of his own studies of bromonium, chloronium, and iodonium intermediates(40-42). He was one of the very few chemists elected to the National Academy of Sciences without a PhD.

Lucas wrote a pathbreaking elementary organic textbook (43) published in 1936, which was probably the first to introduce, at that level, modern ideas of valence and thermodynamics to organic chemistry. An early chapter of this book details how the free energy changes in the reactions of hydrogen with halogens might be relevant to understanding the corresponding reactions of methane with halogens. Lucas also illustrated the use of bond energies to calculate the enthalpies of halogenation of methane. The treatment predates those of Morrison and Boyd(44) and of Roberts and Caserio(45) by more than twenty-five years. I remember my own professor of beginning organic chemistry telling our class that he did not understand the first chapter of the Lucas book, that we would not understand it either, and so he was going to skip over it.

Frank C. Whitmore(46) was an extraordinarily dynamic individual who brought Penn State to the forefront of organic chemistry. His specialty was hydrocarbons and particularly rearrangement reactions. He published a famous paper in 1932 correlating much of what was known about organic rearrangements(47). He told me that the referees were badly split on the merits of the paper. One thought it was unintelligible; one thought it was just right; and the third thought it was what everyone already knew!

Whitmore deserves enormous credit for his research on organosilicon compounds. He started this work because he was interested in seeing whether trimethylsilylmethyl chloride would behave in the same way as neopentyl chloride in S_N1 and S_N2 reactions(48). This line of work was carried on later by his able student, Leo Sommer. Whitmore published a very unusual

advanced chemistry book, that he said could be characterized as a one-volume *Beilstein* for practicing chemists(49). It is full of strange reactions and compounds that apparently just struck Whitmore's fancy.

Morris Kharasch(50) was another dynamic individual who, like Whitmore, was rather more devoted to exploratory organic chemistry than to the applications of physical theory. He is best known for his development of much free-radical chemistry. With his student, Frank Mayo, Kharasch tackled the existing mass of conflicting information as to the direction of addition of hydrobromic acid to alkenes. Kharasch and Mayo were the first to show that there is competition between normal and the anti-Markovnikoff additions, with the anti-Markovnikoff addition occurring by a free radical chain mechanism(51).

I think the evidence is very good that James Bryant Conant was by far the most influential of the early physical organic types. Conant had so many different careers that it is hard to believe that he could be great shakes as a researcher. However, between 1918 and 1933, in just fifteen years, he not only did some very important studies, but he launched the careers of Louis F. Fieser, Lyndon Small, Robert E. Lutz, W. R. Kirner, and A. H. Blatt, as well as three truly great physical organic chemists: Paul D. Bartlett, George Willard Wheland, and Frank H. Westheimer; each, in his own way, a master of the discipline.

Bartlett in his biography of Conant says(52):

> I came to think of Conant as the most truly intelligent man I ever knew. For him, objectivity seemed to be a natural state of mind, rather than something for which one must strive. The habit of viewing the world as it revealed itself, rather than as he might wish it to be, was fundamental to Conant's professional, political and administrative life.

As I mentioned earlier, Conant did his Ph.D. work with E. P. Kohler at Harvard. This came about in an unusual way. When Conant was a student at Roxbury Latin School, he came under the influence of a superb science teacher, Newton Henry Black, who gave Conant very special attention and, in fact, arranged a life plan for Conant which included, not only undergraduate work at Harvard, but also graduate work with the great atomic weight chemist at Harvard, Theodore William Richards. However, before Conant graduated, he did some undergraduate research with Kohler, then newly arrived from Bryn Mawr College, that siphoned him off into organic chemistry. Conant said that Black never forgave him for departing from the plan, although later he and Black wrote a book together called *Practical Chemistry* (53).

It is not stated whether part of Black's plan was for Conant to marry T.W. Richard's daughter, but he did.

At the time Conant started his graduate work on cyclopropane compounds, Kohler had not previously worked in this area and Conant, in his thesis(54) which I read many years ago at Harvard, made a quite good survey of the chemistry of cyclopropane and also commented at length on its resemblance to double-bond chemistry. This survey is not much reflected in his two papers with Kohler published in 1917(55, 56). The research described there is not on simple cyclopropanes, but rather on highly substituted cyclopropane derivatives related to Kohler's beloved and much-studied benzalacetophenone. In these compounds, the three bonds of the cyclopropane ring were nonequivalent and Conant found conditions by which each could be opened selectively. Judging from his thesis and his papers with Kohler, it is clear that Conant had extensive and superior training as an organic chemist.

Many years later, when Conant was President of Harvard, he became quite disturbed about the way the Soviets were excoriating Pauling about resonance, and he offered to give his views on the dangers of mixing political ideology and science at a seminar at MIT. He started his seminar by saying that "Some years ago, I did my thesis work on cyclopropanes and it appears as though nothing has been done in the field since." This caused my colleagues and students much merriment, which Conant clearly did not understand. Later, I sent him some reprints and had a subsequent note from him conceding that indeed some progress had been made.

There was nothing in the Kohler and Conant papers about electrons or mechanisms, but Conant clearly had his ear to the ground because, in 1921, he published a paper(57) drawing on the, as yet unpublished, electronic theories of G.N. Lewis(58) to account for nucleophilic additions to carbonyl groups on the basis of the dipolar character of carbon-oxygen double bonds. Conant's first paper specifically on reaction mechanisms had to do with the formation of mustard gas from ethylene and sulfur dichloride(59). It is interesting that his student, Paul Bartlett, had an important research project on the mechanism of the hydrolysis of mustards during World War II(60, 61).

Conant developed a strong interest in the relationship between thermodynamic and rate properties and, in his work with Fieser and later others, investigated the reduction potentials of quinones as a function of structure(62-64). Conant was very versatile and broad ranging. Of particular importance, to physical organic chemistry, besides the reduction potential determinations, were his studies of:

1. Superacid solutions, in which he specifically uses that terminology, although the work involved acetic acid as the solvent(65).
2. Reactivity of organic chlorides to displacement by iodide ion in acetone, which turned out to be a useful procedure in qualitative organic analysis(66-68).
3. Kinetics and mechanism of coupling of diazonium salts with phenols (24), although as pointed out earlier, Conant postulated incorrectly from the kinetics that the diazotic acid and unionized phenol reacted in the rate-determining step. The currently accepted mechanism was established later by Wistar and Bartlett(69).
4. Kinetics of semicarbazone formation and the demonstration that, with a mixture of carbonyl compounds, one product could be the result of kinetic control, but another product could predominate at equilibrium(70).
5. Relative acidities of a wide range of weakly acidic hydrocarbons by determining whether or not a particular hydrocarbon salt would react with an another acidic hydrocarbon(71).
6. The steric effects of strategically placed alkyl groups that substituted for aromatic groups in stabilizing free radicals(72).
7. The effects of high pressure in influencing the rates of polymerization(73).
8. Attempts to synthesize highly hindered alcohols, such as triisopropylcarbinol, by Grignard addition to carbonyl groups and, in consequence, the discovery that reduction by Grignard reagents could be strongly competitive with addition, when addition would lead to extensive steric crowding(74). F. C. Whitmore, who had been a student at Harvard at the same time as Conant, liked to say, "Conant was made President of Harvard because he tried to attach four isopropyl groups to a single carbon." Triisopropylcarbinol was not made until fifteen years later(75). Paul Bartlett carried on Conant's work and successfully prepared tri-tert-butylcarbinol as well a number of other highly branched alcohols(76,77).
9. Hemoglobin and chlorophyll; where, over the years, Conant applied the skills and knowledge he had honed as a physical organic chemist(78-88). Thus, he became perhaps the first of the now prevalent breed of physical bioorganic chemists.

This recital of Conant's research interests should leave no doubt that he was indeed a physical organic chemist with a skillful blend of organic and physical talents as well as a keen eye for important problems. Yet along with these achievements, Conant started a formidable dynasty of scholars through his students, Paul Bartlett, G. W. Wheland, and Frank Westheimer. Each went different ways.

Wheland was a postdoctoral fellow with Linus Pauling at Caltech and, although he published some experimental papers from the University of Chicago, he is best known through his wonderfully scholarly books.

The first covers the theory of resonance(89); the second goes over the same subject in greater detail(90); and the third, a detailed and thoughtful advanced organic chemistry book(91), deals with stereochemistry and reaction mechanisms.

Frank Westheimer has been enormously influential through his work with Kirkwood(92) on modification of the Bjerrum electrostatic analysis of the relative strengths of carboxylic acids by taking into account the structure of the solvent; with Mayer on the first true molecular mechanics calculations(93), the mechanism of the chromic acid oxidation of alcohols and the H/D kinetic isotope effect(94), and, perhaps even more, for his exemplary and pathbreaking applications of physical organic chemistry to the mechanisms of enzyme-catalyzed reactions(95).

Compared to Wheland and Westheimer, Bartlett has been the most deeply rooted in organic chemistry. Bartlett's paper(96) on the synthesis of apocamphanyl chloride for the purpose of determining whether inversion is required for nucleophilic substitution and whether or not carbocations are energetically most favorable in the planar state is one of the most important papers ever published relating to physical organic chemistry. The reason is because this paper illustrates vividly the power of combining imagination with talent for organic syntheses and a sound knowledge of physical principles. Bartlett has been very broad ranging in his interests in physical organic chemistry. Among other things, he has been concerned with free radicals(97, 98), carbocation chemistry(77, 99-101), reactions of singlet oxygen(102), sulfur chemistry(103), cycloaddition reactions (104) and other bridgehead compounds(105, 106).

A wonderful example of the warmth and humor of Bartlett style is contained in his early-on analysis of papers in the carbocation field, *Non-Classical Carbonium Ions* (107). In his comments on H. C. Brown's famous polemic entitled *Strained Transition States* (108), Bartlett concludes by offering a multipart exam for the reader that deftly skewers many of Brown's arguments.

It should be clear from the above that general recognition of physical organic chemistry as a distinct discipline of chemistry in the United States came rather slowly. This was particularly true in the Midwest universities, except for the University of Chicago. I believe that it reached its pinnacle in the '60s to '80s and now seems on a rather steep decline as its experimental practitioners fade away, either through age, or by moving into subdisciplines such as bioorganic and organometallic chemistry, as well as materials science and photochemistry. One enormous effect that the discipline has had has been to revitalize organic synthesis by both providing interesting compounds to make and also by providing deeper understanding of how synthetic reactions work. Another very important effect has been on inorganic chemistry, on the one hand, by example through its successful use of a combination of kinetics and structural theory and, on the other hand, by greatly broadening the study of organometallic compounds.

Much of what is left of physical organic chemistry, as judged by what is currently accepted for publication in the *Journal of the American Chemical Society*, is concerned with theoretical calculations at various levels of sophistication aimed at demonstrating the power of quantum mechanics and molecular mechanics to predict chemical phenomena. Such calculations are impressive in their fantastic use of number crunching with the aid of Cray or massively parallel computers. However, in my view, the results often seem sterile because, while lots of numbers are obtained, little or no qualitative understanding is provided of what those numbers mean.

Another problem is increasing specialization. Fifty years ago, there were many problems that just could not be tackled by any means whatsoever. The instrumentation revolution has made many changes in what we can study. As one example take conformational analysis of 1,2-disubstituted ethanes. There was not much an organic chemist could find out about conformational equilibria in aqueous solution fifty years ago. With NMR, an enormous amount of useful data can be obtained and one can easily become a specialist in data collection, analysis, and interpretation of such conformations. The result is formation of still another subspecialty of physical organic chemistry. The problem for each of us is how much we have to know about the various subspecialties to keep reasonably abreast of the field, much less worry about how much we need to appreciate the early work of Solomon F. Acree.

History shows that, like other sciences, chemistry progresses in cycles. New techniques, new classes of compounds, new instruments and new theories restore vitality to fields that seem to have leveled off. My prediction is that continuing application of physical organic principles laid down over the last seventy-five years or so to biochemistry will keep physical organic chemistry alive for a long time, even though it may well become better known as *biophysical organic chemistry*.

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"PLUS COMMODE ET PLUS ELEGANT": THE PARIS SCHOOL OF ORGANIC REACTION MECHANISMS IN THE 1920'S AND 1930'S.

Mary Jo Nye, Oregon State University

Research on organic reaction mechanisms was an important aspect of the development of physical organic chemistry in the decades of the 1920's and 1930's. The experiments and ideas of Christopher Ingold were instrumental in defining this field of research, indeed in establishing the very vocabulary and thought patterns required for talking about structure and mechanism in organic chemistry.

Much less well-known than Ingold's school, or than competing groups like Arthur Lapworth's and Robert Robinson's laboratory at Manchester, is another research school in organic reaction mechanisms centered in the Ecole Normale Supérieure in Paris during the 1920's and early l930's. It was directed by the the French chemist Robert Lespieau. Among the most important students and collaborators in this school were Albert Kirrmann, who taught at Strasbourg and Bordeaux before returning to Paris in the 1950's, and Charles Prévost, Kirrmann's closest friend, who returned to Paris in 1937, after having taught at Nancy and Lille. Kirrmann and Prévost were perhaps the most influential figures in organic chemistry in Paris from the mid-1930's through the mid-1950's.

The aim of this paper is to summarize briefly some of the principal aspects of this Parisian research school, as it existed in the 1920's and early 1930's. A short history of the Paris school is important in order to understand the character of physical organic chemistry in France between the wars. This history also is important, as I note in the conclusion of this paper, for the light it sheds on contrasts, as well as similarities, between the experimental practice and theoretical style of chemistry in France and in England.

Robert Lespieau and the Reform of French Chemistry

Robert Lespieau (1864-1947), director of the chemistry laboratory of the Ecole Normale Supérieure from 1904-1934, was born into a family with strong military ties. The family had considerable political connections, including links with Armand Faillières, at one time President of the French Republic(1). Lespieau brooked family tradition in 1886 by entering the Ecole Normale Supérieure rather than military schools or the Ecole Polytechnique(2).

His intellectual interests lay in the physical sciences, especially chemistry, but he found absolutely uninspiring the chemistry taught by protégés of Marcellin Berthelot in the late nineteenth century. Until 1891 the stubborn Berthelot and his acolytes still were writing water as HO, and, as late as 1898, Berthelot continued to write benzene as C_2H_2 (C_2H_2) (C_2H_2) rather than as a structural or ring formula. There is not the least use of structural formulas in Berthelot's discussions of isomerism in a 1901 text on hydrocarbons(3).

Successfully learning how to negotiate the perils of this outmoded French chemical notation on his examinations, Lespieau passed the predoctoral exams in physical sciences and then spent two years working with the more up-to-date organic chemist Charles Friedel, who had succeeded Adolphe Wurtz at the Sorbonne(4). To his interest in the new physical chemistry of the late

nineteenth century, Lespieau added a passion for the recent stereochemistry of the Dutch chemist Jakobus van't Hoff and the non-academic French chemist Achille LeBel. In 1892 Lespieau presented a lecture at Friedel's laboratory on the kinetics of tautomerism, using the hypothesis of the tetrahedral carbon structure. This approach led him into a doctoral thesis which included the preparation of stereoisomers(5), and when he completed his dissertation in late 1896, his public defense attracted a considerable audience because he was regarded as a representative of "current chemistry"(6).

The Practice of Organic Chemistry at the Ecole Normale, 1904-1934

By 1904 Lespieau was appointed director of the chemistry laboratory at the Ecole Normale Supérieure, and he radically changed its direction. At the time he was appointed, the best work in French organic chemistry was being accomplished mostly outside Paris, by chemists who had been hostile or indifferent to Berthelot's tutelage, notably Paul Sabatier at Toulouse and Victor Grignard at Lyon and Nancy. They shared a Nobel Prize in 1912 (7).

Under Lespieau's directorship, the primary research program at the Ecole Normale chemistry laboratory became the synthesis and study of nonsaturated organic compounds, along with the application to hydrocarbons of physical methods like François Raoult's techniques using boiling points and melting points. Most of Lespieau's students worked on research topics having to do with isomerism, unsaturated hydrocarbons, or catalysis, often focussing on reactions associated with double and triple bonds (8). His laboratory was open to the students of his colleague Jean Perrin in physical chemistry, and by the 1920's, to students of Albin Haller in organic chemistry, Georges Urbain in general chemistry, and André Job in inorganic chemistry. Collaborating with his former student Maurice Bourguel, Lespieau became one of the first chemists in France to apply Raman spectroscopy to organic analysis (9).

In 1922, the title of Lespieau's position became "professeur de théories chimiques" (10), perhaps the first professorship of "theoretical chemistry" anywhere. There was not a chair of theoretical chemistry in England, for example, until 1931 (11). That Lespieau wanted to have this "theoretical" title demonstrates his self-consciousness about the need for chemical "theory," not just chemical empiricism, in order to renew French chemistry. He had the sense that he was putting organic chemistry at the Sorbonne and the Ecole Normale on a new and different track. By 1922, too, he had two new

students, Kirrmann and Prévost. who soon were among his best recruits, Let us turn, now, in some detail to the two of them and their work.

## Kirrmann, Prévost, and the Theory of "Synionie"

Albert Kirrmann (1900-1974) entered the Ecole Normale in 1919 as part of a special group of five Alsatian students permitted to enter *hors concours* because of their status as Alsatians whose province now had been restored to the French fatherland. Another student in this special group was Alfred Kastler, who was to receive the Physics Nobel Prize in 1966 for his optical methods of exciting atoms in laser work (12). Kirrmann, like Kastler, taught at Bordeaux after completing his doctoral degree in Paris. But Kirrmann returned to Strasbourg, where he held a chair in organic chemistry for almost two decades. Protestant by faith, he was nonetheless deported to Buchenwald during 1943-1945 (13). He moved to Paris in 1954, where he ended his career as Professor of organic chemistry, laboratory director, and associate administrative director at his *alma mater*, the Ecole Normale (14).

Charles Prévost (1899-1983), the son of a Parisian engineer, like so many ambitious young French intellectuals, first thought himself to have a vocation as a mathematician after he passed the baccaulaureate (15). But introductory exercises in the chemistry laboratory completely reoriented him, and when Robert Lespieau examined him for the certificate in chemistry, Prévost found himself offered a place in Lespieau's laboratory for doing research.

After completing his doctoral thesis, Prévost taught at Nancy and, beginning in 1936, at Lille. The next year he returned to Paris in a specially designated position at the Sorbonne, where he finally received a chair of organic chemistry in 1953. During the Second World War, Prévost remained in Paris, continuing to teach under the Vichy regime. He soon joined the Resistance (16).

As young men who were preparing their doctoral theses in the mid-1920's, Kirrmann and Prévost were influenced both by physicists and chemists (17). The physical chemist Jean Perrin presided over the jury for Kirrmann's thesis defense (18). By the time Kirrmann and Prévost were independently defending their doctoral theses, they had been collaborating in research for several years and had reached the opinion that, while the development of theoretical organic chemistry had not yet arrived at a mathematical stage, the time had come for a qualitative general theory of organic chemistry modelled on physical mechanics (19).

How had they arrived at such a conclusion? In the mid-1920's, Kirrmann and Prévost both were working on problems on reactions of unsaturated hydrocarbons, in particular allylic and conjugated systems. Kirrmann's research topic became focussed on brominated aldehydes and the so-called "abnormal" reactions in which, for example, these aldehydes behave as if they were acid bromides (20), i.e.

$$RCH_2 - \underset{\underset{Br}{|}}{C} = O \quad \text{rather than} \quad RCHBr - CH = O$$
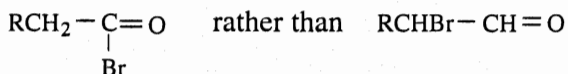
Prévost took for his research project the preparation of derivatives of the alcohol erythritol $C_4H_6(OH)_4$ and the study of the mechanism of bromine fixation. Like others, he was struck by what appeared to be anomalies. Thus he obtained a 1,4 dibromide ($CH_2Br-CH=CH-CH_2Br$) instead of the 1,2 dibromide of butadiene. Similarly, instead of obtaining the expected 1,2 dibromide form of divinyl-glycol, he obtained a 1,4 form (21).

In thinking about these reactions, students in Lespieau's laboratory in the mid-1920's were inclined to speculate about intramolecular mobility and migrations not only of atoms or groups within the molecule, but also of electrons which were part of the molecular architecture. These ideas came to them largely through lectures by André Job, who updated students and colleagues on what was happening at the Solvay Institute chemistry conferences in Brussels in 1922 and 1925, the first Solvay conferences in chemistry.

In Job's lectures, and in his own work, Job emphasized reaction dynamics due to inter- and intramolecular "mobilities," as he put it, including the production of unstable intermediate compounds having an electronic or ionic character. We are led to suppose with the English chemist Thomas Martin Lowry, he said, that the double bond is a polarized bond, and that an organic acid is similarly polarized (22):

$$O = O \text{ is } \underset{-\quad +}{O = O} \quad \text{acid: } R'\overset{-}{C}\overset{\displaystyle O}{\underset{\displaystyle O}{\diagdown}} \quad H^+$$
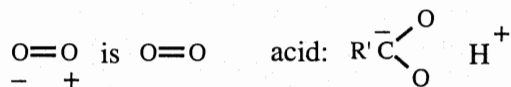
Lowry, the holder of a newly created chair in physical chemistry at Cambridge University who attended the Solvay conferences, spoke in Paris in March, 1924, on aspects of the theory of valence. He returned to Paris in December, 1925, to lecture on his hypothesis of semi-polar double bonds in organic compounds. These occasions were meetings of the Société de Chimique de France and the Société de Chimie Physique (23).

In speaking to his French audience, Lowry praised the 1916 memoir of the American chemist G. N. Lewis as a "turning point in the history of chemistry" because of its "plausible theory" of the electronic origin of the different types of chemical affinity and its clear differentiation between two kinds of valence: ionic and covalent. It is customary in mineral chemistry, Lowry said, to consider reactions that occur between ions without attaching any importance to ionization in organic chemistry, except for the formation of salts from organic acids(24).

> My opinion is otherwise. I consider that 1) certain organic reactants which are the most active are already ionized. 2) Others owe their activity to a possibility of ionization, for example the influence of a catalyst. I conclude that in organic chemistry exactly as in mineral chemistry, reactions take place almost always between ions.

Lowry concluded with a discussion of what he called "dynamic isomerism" which might be of three kinds: transfer of 1) a radical; 2) a hydrogen ion or proton; or 3) an electron. The latter two categories he called "prototropy" and "electrotropy "(25).

There is no question that Kirrmann and Prévost were influenced by Lowry's theories for explanation of reaction mechanisms. Another important influence was Georges Dupont, a frequent visitor from Bordeaux, with whom they talked at length in the Paris laboratory and who succeeded Lespieau as director upon Lespieau's retirement. Dupont published a paper in 1927 in which he attempted to combine the electron octet theory of valence and Bohr's hydrogen electron model with classical concepts of stereochemistry. Dupont adopted without reservation Lowry's application of ionic radicals in hydrocarbon chemistry (26).

At the time they were writing their doctoral dissertations, Kirrmann and Prévost conceived the idea of developing a general theory of reaction mechanisms in organic chemistry, using principles and notation that Prévost was using in his thesis. They were concerned to take into account the classical tradition of stereochemistry, in particular the notion of the carbon tetrahedron, which commanded pride of place in French organic chemistry. They also wanted to develop ideas compatible with their mentor Jean Perrin's radiation theory of chemical activation. As Prévost and Kirrmann put it, their goal was to construct a conceptual edifice "plus commode et plus élégant" than the array of theories presently available in organic chemistry" (27) (i.e., than theories mostly appearing in England and in the United States).

Their joint effort was published in the form of three memoirs in the Bulletin of the Société Chimique de France in 1931 and 1933. The joint publication would have appeared earlier, had it not been for the fact that the manuscript was in press at the time that a fire destroyed the printing house (28). Somehow they recovered their stamina and their text.

In their introduction to the memoirs, Kirrmann and Prévost took pains to state that theirs was not simply a "compilation of . . . memoirs published . . . abroad on the question." They stressed that this was a "purely theoretical work" and that it was new in character, while building on previous hypotheses. They also suggested that chemists' work was providing challenges and guidance to problems which now must be tackled by physicists (29). This notion of collaboration, by the way, was a frequent theme of Lowry.

Their starting point was the fundamental hypothesis that the reactions of organic chemistry are reactions of ions, i.e., that at the moment of reaction, the sections ["tronçons"] of the hydrocarbon molecule possess localized electric charges (30). Physical proof exists for this hypothesis: for example, that with the exception of saturated hydrocarbons, molecules with pronounced chemical reactivity have a permanent electric moment. This was a subject on which Kirrmann published a review paper in the *Revue Générale des Sciences* (31).

They further argued that the ionized molecule is the "active molecule" referred to in Jean Perrin's recent theoretical work on the energy of activation, and that the energy required to cause the ionization must come from circumstances exterior to the molecule, for example, through the presence of another molecule or catalyst, or from radiation (32). (The radiation theory of activation, by the way, was just then disappearing from the chemical literature.)

The theory they offered rested on a new and more general law, they claimed, than Lowry's notion of "prototropy," which referred to hydrogen or proton migration. They introduced a new term, "synionie," which does not imply the sign of the mobile radical. In particular, Prévost and Kirrmann had in mind a principle (or law) and a theory which would cover, in addition to H^+, the case of hydroxyl (OH^-) and halogen (especially Br^-) migration which they had studied in their research on alcohols and brominated hydrocarbons.
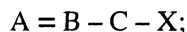

For the molecule of form $X-A-B = C$, the departure of negative X creates the ions:

$$X^- \quad \text{and} \quad {}^+A-B = C$$

but this form becomes

$$\begin{array}{ccc} + & - & + \\ A & - B & - C \end{array}$$
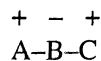
by induction, which is also the activated form of

$$A = B - C - X;$$

so we say that $\quad X-A-B = C \quad$ and $\quad A = B-C-X$ are two "synionic isomers" (33). An example from experimental work is the existence of the synionic isomers:

$$CH_3-CHCl-CH=CH_2 \quad \text{and} \quad CH_3-CH=CH-CH_2Cl.$$

They also defined what they called a state of "métaionie" in which none of several structural formulas corresponds to the "real" structure of the reacting molecule. The multiple structural formulas for benzene are examples of their "métaionie" (34).

The general theory of ionization and the new terminology were meant to cover and coordinate three different types of ionic reactive states: the allylic (involving carbon, hydrogen, oxygen, and sulfur), cyanhydric (involving nitrogen), and ring-chain or cyclization (35). In addition, Prévost and Kirrmann stressed the problematic character of the electron doublet representation, an "image, certainement fausse," they said, because the doublet does not really have a fixed position, but corresponds "to an average position in the oscillations of which the molecule is the seat." It was through spectroscopy focussed on the infrared spectrum and the Raman effect that they thought the true movement of electrons could be made precise in what they called a future "electro-geometric synthesis" (36).

The "French" and "Anglo-Saxon" Schools of Theoretical Organic Chemistry

The joint memoirs of Prévost and Kirrmann self-consciously presented a general theory of organic chemistry which constituted an application of physical methods and principles to the problem of organic reaction mechanisms. However, the language-system devised by Prévost and Kirrmann was not adopted by chemists in general, and that part of their notation that was new was not used outside France. Indeed, there was very little interest in their work inside France.

In 1934, the year after the publication of the last installment of the Prévost-Kirrmann theory, there appeared in *Chemical Reviews* the paper which was to set up a system of explanation-language for organic reaction mechanisms that became dominant in the next decades. This was Christopher Ingold's "Principles of an Electronic Theory of Organic Reactions," in which the "nucleophilic"/"electrophilic" classification and notation were introduced(37).

Curiously, the Ecole Normale school of chemistry now repeated the history of Berthelot's generation of French chemists, in which many French chemists had persisted in the language and theory of "equivalents" not in use elsewere. Now Prévost's and Kirrmann's "ionic" theory was everywhere else the "heterolytic" theory; their "synionie" was the S_N1 or El group of reactions. The elsewhere ubiquitous curly arrow and electron octet hardly appeared in French chemical papers and textbooks. Many organic chemists in France found even the Kirrmann-Prévost approach too speculative and radical. Victor Grignard wrote in 1935 in his introduction to the multivolume *Traite dé chimie orqanique(38):*

> As for the new electronic theories, they are not sufficiently developed for serving as the basis for speculations in organic chemistry, despite all the promises they offer to chemists. In this text, they will . . . remain discreetly in the background; and it is still the very fruitful conception of Le Bel and van't Hoff which will constitute the surest guide for us.

Prévost and Kirrmann did little to develop further the theory of "synionie" in the 1930's and 1940's, although Prévost returned to these kinds of issues after an international meeting at Montpellier in 1948 in which Christopher Ingold participated and after a set of four lectures by Ingold in Paris. Prévost remained convinced that the S_N1/S_N2 categories were too rigid and did not adequately describe the nuanced spectrum of electronic mechanisms (39). (Here Prévost reminds the historian of Georges Buffon's objections to Linnaeus' definition of biological species in the eighteenth century.) Prévost's criticism ignored the fact that Ingold's classification was not meant to be a rigorous set of inferential principles, but only a representation of extreme types (40).

Ingold's *Structure and Mechanism in Organic Chemistry* appeared in 1953. It became one of the fundamental handbooks of organic chemists–a piece of classic literature for the discipline of physical organic chemistry. It included both in its text and in its network of citations a history of developments in physical organic chemistry. As in Ingold's paper of 1934, this history made no reference to Prévost and Kirrmann, nor to any French theories of reaction mechanisms (41). Prévost and some of his protégés were bitter about this, and they often referred to an "Anglo-Saxon School" of chemistry that ignored French contributions to the theory of reaction mechanisms (42). They nonetheless professed admiration for the work of Ingold, whom Prévost and Kirrmann cited as early as 1931 in their joint memoirs (43). In 1954 Prévost proposed Ingold's name to the Nobel Chemistry Committee in Stockholm (44).

After the Second World War, students in French chemistry were to feel that they had lost step with progress in theoretical chemistry elsewhere, especially in the understanding of organic reaction mechanisms and in the application of quantum mechanics to chemical problems. Raymond Daudel began a theoretical chemistry group in 1942, with the patronage of Louis de Broglie, Frédéric Joliot, and A. Lepape, but Daudel was hard-pressed to interest theoreticians in quantum chemistry rather than in quantum field theory or in neutron and high-energy physics. In the CNRS, work which would have been called quantum chemistry elsewhere was carried out in the section for applied mathematics until the 1970's (45).

In 1948, the year that Christopher Ingold lectured on reaction mechanisms in Paris, Edmond Bauer, who was Jean Perrin's successor at the Sorbonne, organized a conference on the quantum theory of the chemical bond, funded by the CNRS and the Rockefeller Foundation. Charles Coulson, Christopher Longuet-Higgins, Michael Polanyi, R. W. Hill, and L. E. Sutton came from England to give papers, as did Linus Pauling and Robert Mulliken from the United States. In 1959 Guy Ourisson renovated the study of organic chemistry in France by organizing the Groupe d'Etudes de Chimie Organique (G.E.C.O.), in order to facilitate contact with chemists abroad and to rethink the theoretical foundations of modern organic chemistry (46). That this was a project still sorely needed in the late 1950's is indicated by Micheline Charpentier-Morize's reflections upon her doctoral thesis defense in 1958. Prévost presided over the jury. When she referred to the possible existence of a "π-complex" in order to explain reactivity, Prévost exclaimed: "Madame, if I have one reproach for you, it is that you know the modern theories too well (47)."

It is striking how isolated French chemists were from what we may now call the mainstream during the decades of the 1920's through the 1950's. Scarcely any French scientists studied abroad. Albert Kirrmann was unusual in his decision to spend a year in Munich in 1930, but he had, after all, been born a German in Alsace. J. C. Smith notes in his history of Oxford University's Dyson Perrins Laboratory, directed by Robert Robinson in the 1920's and 1930's, that there was a great mixture in Robinson's lab of ages and nationalities among the twenty students each year, but never, until 1947, a French person (48). This French insularity contributed to the closure of the boundaries of the research school associated with Lespieau's laboratory at the Ecole Normale Superiéure and to its exclusion from the wider disciplinary history of physical organic chemistry and theo-

retical chemistry. Staging international conferences in Montpellier and Paris helped overcome some of this isolation but by no means solved the problem.

Finally, to come back to the title of this paper, and to a characteristic tradition or style in French science, the Paris school defined theoretical chemistry as a kind of chemistry (and a kind of science) that aimed ultimately at abstract knowledge in the purest sense. Historically, in the development of theories about reaction mechanisms and activation in the 1920's, research groups at London, Manchester, Cambridge, and Leeds led the way with imaginative, visually graphic mechanisms based on ideas of corpuscular motions. In contrast, the Ecole Normale school of chemistry tended to avoid visual, corpuscular imagery in favor of more generalized and abstract systems based firmly in empirical descriptions of ionization or radiation. That is, the French school valued most highly the theory that was "plus commode" and especially "plus élégant."

I conclude with Albert Kirrmann's statement of the agenda for organic chemistry in the early 1950's. Fundamental problems of organic chemistry lay, he said, in three areas: the accomplishment of organic synthesis; the elucidation of molecular structure; and the inquiry into reaction mechanisms. The study of mechanisms, Kirrmann wrote(49):

> . . . is, more than that of synthesis, deeply impregnated with the spirit of pure science, of which the unique goal is to know and not to create . . . . It is still the case that the achievements of chemistry, in the face of problems of extreme complexity, inevitably are of a qualitative order on the whole, and they appeal exclusively to the *esprit de finesse*. Physics, in contrast, since it is able to limit its parameters, has more easily attained the quantitative stage, and the *esprit géometrique* suffices, on the whole, for it to progress on its way.

So we see that Kirrmann and his colleagues in theoretical chemistry at the Ecole Normale Supérieure still continued to anticipate a future stage of chemical progress when theoretical chemistry, like theoretical physics, would be characterized by *l'esprit de géometrie*. Perhaps neither Kirrmann nor Prévost really ever surrendered the youthful aspiration of the French intellectual to be a mathematician.

ACKNOWLEDGMENTS

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2. G. Dupont, "Robert Lespieau. 1864-1947," *Bull. Soc. Chim. Fr.,* **1949**, [5], *16*, 1-9, on p. 1.

3. *Ibid.*, citing H. Moissan, *Ann. Chim. Phys.,* **1890**, *19*, 266; M. Berthelot, *Bull. Soc. Chim. Fr.,* **1890**, *4*, 809; M. Berthelot, *Compt. Rend. Hebd. Acad. Sci.,* **1891**, *112*, 829; M. Berthelot and E. Jungfleisch, *Traité élémentaire de chimie organique,* 4th ed., Dunod, Paris, 1898; M. Berthelot, *Les carbures d'hydrogene,* Gauthier-Villars, Paris, 1901, Vol. III, 120.

4. Friedel became internationally known for the synthetic method called the "Friedel-Crafts Reaction" using aluminum chloride as a catalyst in the introduction of an alkyl or acyl group into benzene. James Mason Crafts was an American professor from MIT working with Friedel in 1877 at the Sorbonne. Crafts later became president of MIT.

5. R. Lespieau, *Notice sur les travaux scientifiques*, Gauthier-Villars, Paris, 1910, 10-12. The thesis concentrated on compounds of the propylene and propanol series; he prepared two dibrominated propylenes and found stereochemical isomerism in one of them: $CH_2=CBr–CH_2Br$.

6. Report on Lespieau's thesis, dated October 24, 1896. AN F17/24392.

7. Grignard was recognized for the method of using alkyl magnesium halides in organic synthesis; and Sabatier, for the method of catalysis of hydrogenation and other reactions by finely divided nickel. See M. J. Nye, *Science in the Provinces: Scientific Communities and Provincial Leadership in France, 1860-1930*, University of California Press, Berkeley, 1986, for studies of the work and careers of Sabatier and Grignard.

8. C. Mauguin, *Amides bromosodes (*1910); H. Pariselle, *Etude d'une glycérine en C4* (1911); H. Duffour, *Derivées complexes de l'iridium* (1912); M. Viguier, *Aldehyde tétrolique* (1912); G. Dupont, *Gamma-Glycols acétyléniques* (1912); G. Vavon, *Reductions catalytique en presence noir de platine* (1913); M. Bourguel, *Carbures acétyléniques vrais* (1925); M. Faillebin, *Hvdrogenation d'aldehydes et kétones en presence de noir de platine* (1925); R. Lespieau, *Notice*, 1925; and R. Dulou and A. Kirrmann, "Le Laboratoire de Chimie

de l'Ecole Normale Superiéure: Notes historiques, *Bull. Soc. Amis Ecole Normale Superiéure,* **1973,** *54,* 12.

9. G. Dupont, "Robert Lespieau," *Bull. Soc. Chim. Fr.,* **1949,** 3.

10. AN F17/24392 (Robert Lespieau) and AN AJ16/5738 (Georges Dupont).

11. This was the chair of theoretical chemistry held by John Lennard-Jones at Cambridge University, who pursued a very different research agenda from that of Lespieau. Christopher Hugh Longuet-Higgins became professor of theoretical chemistry at Cambridge, succeeding Lennard-Jones, in 1954.

12. A. Kastler and Y.Noel, in *Hommage á Albert Kirrmann,* courtesy of Ecole Normale Supérieure (ENS) Bibiothèque des Lettres; W. Sullivan, "Dr. Alfred Kastler, 81, Nobel-Prize Winner, Dies," *New York Times,* January 6, 1984. Sullivan says there were five students admitted *hor concours;* Kastler says there were eight.

13. See C. Prévost, "Notice nécrologique. Albert Kirrmann," *Bull. Soc. Chim. Fr.,* **1957,** ler partie, 1451-1454.

14. *Ibid.* Dupont was director of the Ecole Normale, but since professors of letters and sciences alternate in assuming the directorship, Kirrmann became vice-director, not director (p. 1452).

15. C. Prévost, *Notice sur les titres et travaux scientifiques,* Société d'Edition d'Enseignement Supérieure, Paris, 1967, 7.

16. See P. Piganiol, "Charles Prévost," *Ann. AEENS,* **1985,** 46-48, on p. 47.

17. See Kirrmann's remarks in *Hommage à Albert Kirrmann,* 1970, and especially on Job, A. Kirrmann, *Notice sur les titres et travaux scientifiques,* **1954,** p 8, typescript of 20 pages, ENS. Courtesy of ENS Bibliothèque des Lettres. Eugene Bloch taught physics at the Ecole Normale from 1913 on and became professor of theoretical physics and celestial mechanics at the Sorbonne in 1930. He succeeded Henri Abraham, one of Lespieau's old friends.

18. Ref. 13, p. 1452.

19. C. Prévost and A. Kirrmann. "Essai d'une théorie ionique des réactions organiques," (2eme memoire), *Bull. Soc. Chim. Fr.,* **1931,** [4], *49,* 1309-1368.

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22. A. Job, "Mécanismes chimiques. Conférence faite au Collège de France. (Laboratoire de M. Professeur Charles Moureu), le 10 mars 1923," in A. Job, *Formes chimiques de transition,* J. Perrin and G. Urbain, Ed., Société d'Editions Scientifiques, Paris, 1931, pp, 125-164; *ibid.,* "Les réactions intermédiaires dans la catalyse," rapport presenté au deuxième Conseil Solvay de Chimie tenu à Bruxelles du 16 au 24 avril 1925," pp. 165-193; diagrams on pp. 172-174.

23. T. M. Lowry, "Nouveaux aspects de la théorie de la valence," *Bull. Soc. Chim. Fr.,* **1924,** *35,* 815-837 and 905-921; T. M. Lowry, "Le dispersion rotatoire optique. Hommage à la memoire de Biot (1774-1862)," J. Chim. Phys., **1926,** *23,* 565-585; T. M. Lowry, "Preuves expérimentales de l'existence des doubles liaisons semi-polaires," *Bull. Soc. Chim.Fr.,* **1926,** *39,* 203-206. There is an account of the December 11, 1925 lecture, which notes discussion afterwards. Dufraisse, Mauguin, Tiffeneau, and Job were among those present.

24. T. M. Lowry, "Nouveaux aspects de la théorie de la valence," *Bull. Soc. Chim. Fr.,* **1924,** *35,* 817.

25. Ref. 24, esp. pp. 816, 827-828, 834, 836; and 908-909, 917.

26. G. Dupont, "Sur la théorie électronique de la valence. Essai de répresentation stéréochimique des éléments," *Bull. Soc. Chim.Fr.,* **1927,** [4], *41,* 1101-1137, quotation on p. 1107.

27. Ref. 19, p. 1368.

28. The three memoirs are: a) C. Prévost and A. Kirrmann, "Essai d'une théorie ionique des réactions organiques. Premier memoire," *Bull. Soc. Chim. Fr.,* **1931,** *49,* 194-243; b) Ref. 19; and c) "La tautomérie anneau-chaine, et la notion de synionie. Troisième communication sur la théorie ionique des réactions organiques," *Bull. Soc. Chim. Fr.,* **1933,** *53,* 253-260; quotation from 1931, p. 194.

29. *Ibid.,* p. 196.

30. They distinguished the molecule "in repose" from the molecule in action. They also suggested use of circled + and – symbols to indicate "tendency" to charge rather than actual charge. As we will see later, the "curly arrow" and the δ+/δ– notation, not their symbols, were to accomplish this notation. See Ref. 28, pp. 201, 208-210.

31. A. Kirrmann, "Le moment électrique des molécules," *Revue Générale des Sciences,* **1928,** *39,* 598-603. He cites work including that of Paul Langevin on the theory of the Kerr Phenomenon. He also calculates that the average calculated dipole length is on the order of tenths of an angstrom, compatibile with the estimated distance of an angstrom between atoms, for example, in hydrogen chloride (p. 602).

32. Ref. 28a, 197, 200; Ref. 19, 1324, 1339.

33. Ref. 21, pp. 199-120.

34. Ref. 28a, pp. 224, 240.

35. Ref. 28c, p. 253.

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38. V. Grignard, Ed., *Traité de chimie organique,* Masson, Paris, 1935, Vol. I, pp. x-xi.

39. Conversation with Professor Georgoulis, Paris, June 11, 1987; see also Ref. 15, p. 13.

40. See D. W. Theobald, "Some Considerations on the Philosophy of Chemistry," *Chem. Soc. Rev.,* **1976,** *5,* 203-213, on p. 213.

41. C. K. Ingold, *Structure and Mechanism in Organic Chemistry*, Cornell University Press, Ithaca, NY, 1953.

42. Prévost refers to the "Anglo-Saxon School" often, for example in "La valence et l'enseignement," *L'Information Scientifique,* **1951**, *6*, 14-18, on pp. 15-16: "We must keep those teaching methods which are good .... in guarding against our falling into the exaggerations of the Anglo-Saxon School which from the start sacrifices descriptive chemistry, the solid part, to 'up-to-date' [original in English] theoretical chemistry which is less definitively established." Pierre Piganiol wrote in his obituary notice for Prévost, "After the war ... the foreign schools [of chemistry] took the lead, and British 'fair-play' [original in English] proved more a myth than a reality in this regard." (Ref. 16, p. 47.)

43. Prévost and Kirrmann cite Ingold's experiments providing evidence of the inadeqacy of Lapworth's and Robinson's theory of alternate induced polarity in their 1931 memoir, p. 1320. Their reference is to C. K. Ingold, *J. Chem. Soc.,* **1930**, 1037.

44. Letter from Ingold to Prévost, dated July 29, 1946; and a letter from the Secretary of the Nobel Committees of the Swedish Royal Academy of Sciences to Prévost, January 16, 1954. Copies of these letters were given me by Constantin Georgoulis. Georgoulis completed a doc-toral thesis at Paris in 1960 on the kinetic study of reaction schemes for allylic transpositions, under the director of Prévost. Another of Georgoulis' teachers was Paul Job, the cousin of André Job.

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THE JOHN HUNT HOUSE MUSEUM

located in the village of Rumford in the city of East Providence, RI, is the repository for a wide collection of items—photographs, laboratory notebooks, packaging materials—from the former Rumford Chemical Works, founded by Eben Horsford and a business collaborator in the mid-nineteenth century. The museum is maintained by the East Providence Historical Society, PO Box 4774, East Providence, RI 102916, Edna Anness, Curator.

BASE HYDROLYSIS OF COBALT(III) AMMINES

Fred Basolo, Northwestern University

I was pleased to be invited by Professor D. A. Davenport to present a paper at the symposium C. K. Ingold: Master and Mandarin of Physical Organic Chemistry to honor Professor Sir Christopher Ingold on the centennial year of his birth. The chemistry community recalls that he was one of the giants of physical organic chemistry, but few chemists realize he also made an important contribution to physical inorganic chemistry. As the token inorganic chemist on the symposium program, it is my job to review briefly the inorganic papers of Ingold and coworkers and to present the saga of this research done at University College London (UCL) and that done independently at Northwestern University (NU). The research involved had to do with the kinetics and mechanisms of ligand substitution of octahedral metal complexes, specifically of cobalt(III) ammines. The two research groups agreed on several of the experimental observations made, but they parted company when it came to the interpretation of the second-order rate law for the base hydrolysis of cobalt(III) ammines. It is primarily this difference that is discussed in this article.

It is not known to me what prompted Ingold to take an interest in the kinetics and mechanisms of ligand substitution of octahedral metal complexes, but perhaps it was because of the influence of the late Professor Sir Ronald Nyholm who had left Australia to join the faculty at UCL. This would be reminiscent of my arrival in 1946 at NU and being able to persuade Professor Ralph G. Pearson, then a physical organic chemist, to join forces and become a physical inorganic chemist. In fact, the initial paper on this subject by Ingold was coauthored

by Nyholm. This paper was concerned with stereochemical changes accompanying ligand substitutions of cobalt(III) complexes, and it was indicated that kinetic studies would be necessary to determine the mechanisms of substitution (S_N1 or S_N2). Nyholm was not comfortable with studies of kinetics and mechanisms, being primarily interested in the syntheses and characterizations of metal complexes, so he did not tend to coauthor the Ingold kinetics papers.

Ingold published five papers on this subject during 1953-56, and finally summarized their research in a small (52 pages) book, the *Weizmann Memorial Lectures(2)* in May, 1958. Ingold states in the preface of the book:

> The object of the lectures recorded in this book was to point out that the first attempts are being made to start the development of a corresponding extension of scope (beyond mechanisms of organic reactions) in the very much more diversified field of inorganic chemistry, which hitherto has been essentially restricted, as organic chemistry used to be, to the study of structure.

He was certainly correct that the time had arrived when inorganic chemists could address the questions of mechanisms of ligand substitution in octahedral and square planar metal complexes(3) using approaches similar to those of organic chemists to probe mechanisms of substitution at tetrahedral carbon. Who was better qualified to initiate such studies than Ingold, one of the founding fathers of physical organic chemistry who invented(4) the now classical nomenclature and symbolism S_N1 and S_N2?

Ingold's first paper(1) on the subject stressed the importance of stereochemical changes accompanying ligand substitution in the assignment of reaction mechanisms. For substitution at tetrahedral carbon it is well known that an S_N2 reaction takes place with inversion of configuration. The situation is not as simple for S_N2 reactions of octahedral systems, and Ingold described this in terms of *edge displacement* (Fig. 1).
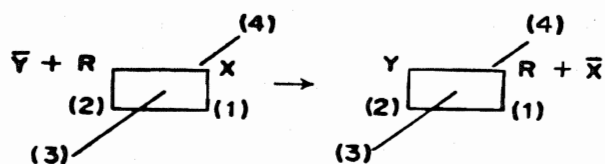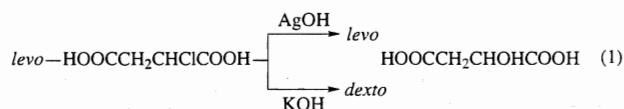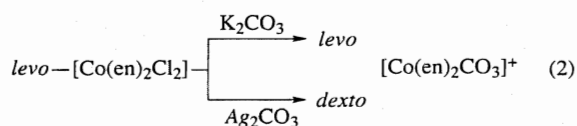

Figure 1   Edge displacement pathway of an S_N2 octahedral substitution.

According to this description the stereochemical change that takes place does not depend on groups Y, R, and X directly involved, but on the position of a marker group A. If A is at (1) the change is *cis* → *trans,* if at (2) *trans* → *cis,* and if at (3) or (4) either D → L or L → D. This corresponds to a *trans*-attack via a pentagonal bipyramid intermediate(5), whereas a nonedge displacement process with retention of configuration corresponds to a *cis*-attack. Walden(6) in 1896 discovered an inversion, now given his name, in one of the steps of the reaction of chlorosuccinic acid to form malic acid (Eq. 1).

$$levo-HOOCCH_2CHClCOOH \xrightarrow[KOH]{AgOH} \begin{matrix} levo \\ \\ dexto \end{matrix} HOOCCH_2CHOHCOOH \quad (1)$$

Bailar(7) in 1934 discovered what he termed the first "Walden inversion" reaction of inorganic chemistry (Eq. 2).

$$levo-[Co(en)_2Cl_2] \xrightarrow[Ag_2CO_3]{K_2CO_3} \begin{matrix} levo \\ \\ dexto \end{matrix} [Co(en)_2CO_3]^+ \quad (2)$$

Ingold(1,2) suggested it is misleading to refer to this as a Walden inversion because it implies a geometrical inversion of octahedral valencies analogous to that of tetrahedral valencies. This one example can serve to illustrate the greater complexities of displacement reactions in octahedral systems over those at tetrahedral carbon

Furthermore S_N1 reactions for octahedral substitution also permit a choice of pathways that account for a variety of stereochemical changes. Research groups at

both UCL and NU independently arrived at the same interpretation of rearrangements that may occur during ligand substitution of octahedral systems by an S_N1 mechanism (Fig. 2).
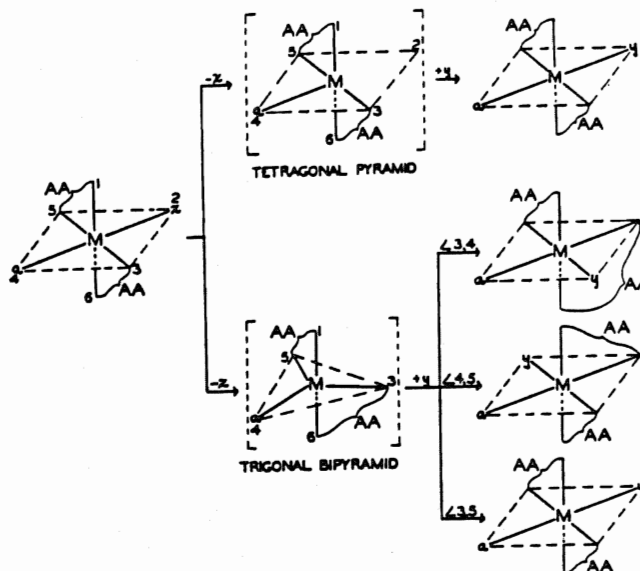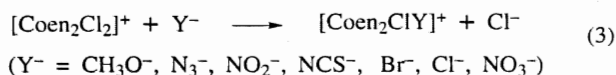

Figure 2   Possible pathways of an S_N1 octahedral substitution.

The rearrangements proposed for S_N2 and for S_N1 mechanisms were used with modest success in an attempt to account for observations reported earlier by the "father of coordination chemistry," Professor Alfred Werner(8). More quantitative spectroscopic data came from the UCL laboratory(9) and from our laboratory(10). Authors of our paper included first names, considered trivial by some English standards; and we later learned that this publication was called by Ingold the "Fred, Bob, and Ralph" paper. The final conclusion of both research groups was that the approaches used could account for stereochemical changes of reactions of octahedral cobalt(III) complexes, but the information obtained was not as diagnostic of mechanism as it was for reactions of tetrahedral carbon compounds

The second paper in this series by Ingold(11) reported details of a kinetic study of some reactions in methanol of $[Coen_2Cl_2]^+$ (Eq. 3)

$$[Coen_2Cl_2]^+ + Y^- \longrightarrow [Coen_2ClY]^+ + Cl^- \quad (3)$$
$$(Y^- = CH_3O^-, N_3^-, NO_2^-, NCS^-, Br^-, Cl^-, NO_3^-)$$

Methanol was chosen as a solvent because it has much less tendency to coordinate to cobalt than does water,

which was known(12) to complicate such a study. The summary of this investigation is illustrated in Fig. 3.
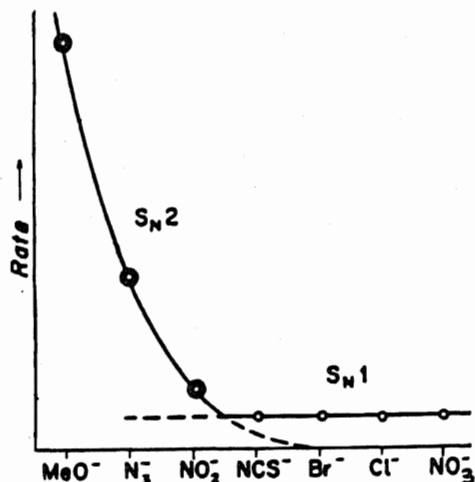

Figure 3 Rates of substitution of one chloride ion from *cis*-[Coen$_2$Cl$_2$]$^+$ with different reagents in nonbuffered methanol solutions (Eq. 3). See text for assignment of an S$_N$1CB mechanism to the reagents CH$_3$O$^-$, N$_3^-$ and NO$_2^-$.

The experimental results showed that for some reagents (NCS$^-$, Br$^-$, Cl$^-$, NO$_3^-$) the rate is first-order in concentration of substrate but zero-order in concentration of reagent. The S$_N$1 mechanism of substitution was assigned for these poor nucleophiles. A second-order rate law (Eq. 4)

$$-\frac{[Coen_2Cl_2]^+}{dt} = k_2\,[Coen_2Cl_2^+]\,[Y^-] \quad (4)$$

was observed for the reagents CH$_3$O$^-$, N$_3^-$, and NO$_2^-$. It was suggested that these stronger nucleophiles react by an S$_N$2 mechanism. Later(13) we were able to show that the reactivities of N$_3^-$ and NO$_2^-$ are due to their greater proton basicities which produce catalytic amounts of CH$_3$O$^-$ (Eq. 5).

$$CH_3OH + Y^- \underset{}{\overset{fast}{\rightleftharpoons}} CH_3O^- + HY \quad (5)$$
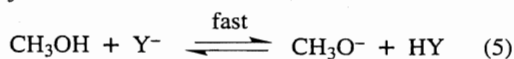
As mentioned near the end of this article, had the UCL group buffered their solutions they would have noted that the rates of reaction are zero-order in concentrations of either N$_3^-$ or NO$_2^-$. Here then is the beginning of the saga of the polemic between the research groups at UCL and at NU. What follows is a brief account of our work on the base hydrolysis of cobalt(III) ammines, leading finally to experiments that conclusively disprove the S$_N$2 mechanism proposed by Ingold.

Cobalt(III) complexes are often water-soluble, and at pH less than 3 or 4 some of the complexes slowly react with water to form an aquo complex (Eq. 6).

$$[Co(NH_3)_5Cl]^{2+} + H_2O \overset{kH_2O}{\longrightarrow} [Co(NH_3)_5H_2O]^{3+} + Cl^- \quad (6)$$
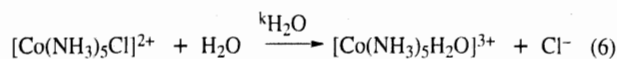
At higher pH the rates are much faster (even as much as 10 times faster than aquation) and the rate law is second-order (Eq. 7).

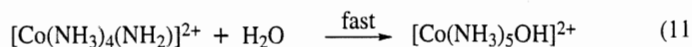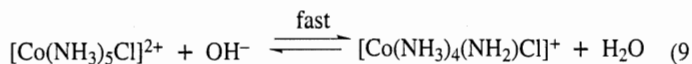$$-\frac{[Co(NH_3)_5Cl]^{+2}}{dt} = k_{OH} - [Co(NH_3)_5Cl^{+2}]\,[OH^-] \quad (7)$$

Such a reaction (Eq. 8) is referred to as *base hydrolysis*(3).

$$[Co(NH_3)_5Cl]^{+2} + OH^- \overset{kOH^-}{\longrightarrow} [Co(NH_3)_5OH]^{2+} + Cl^- \quad (8)$$
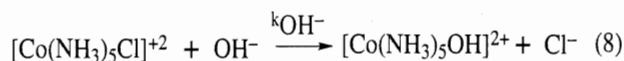
Since OH$^-$ is the strongest base possible in water and since it is a strong nucleophile, it is understandable that Ingold would suggest the reaction takes place by an S$_N$2 pathway. In spite of this, at NU we were aware of work by Brønsted(14) and the suggestion by Garrick(15) of an alternative mechanism also consistent with the observed rate law (Eq. 7), along with other qualitative observations in our laboratory that prompted us to investigate further the mechanism of base hydrolysis of cobalt(III) ammines.

Brønsted(14) reported that the hydrolysis of [Co(NH$_3$)$_5$NO$_3$]$^{2+}$ is independent of pH below 3; however, that of [Co(NH$_3$)$_4$(H$_2$O)NO$_3$]$^{2+}$ is dependent on [H$^+$] even below pH 3. He suggested that [Co(NH$_3$)$_5$NO$_3$]$^{2+}$ does not dissociate in acid to form its conjugate base, whereas [Co(NH$_3$)$_4$(H$_2$O)NO$_3$]$^{2+}$ is a sufficiently strong acid to form rapidly equilibrium amounts of the conjugate base [Co(NH$_3$)$_4$(OH)NO$_3$]$^+$ which is much more substitution-labile than its parent aquo complex. Garrick(15) reported in 1937 that the second-order rate law (Eq. 7) for the base hydrolysis of [Co(NH)$_5$Cl]$^{2+}$ was consistent with what is called an *S$_N$1CB mechanism* (substitution, nucleophilic, unimolecular, conjugate base) (Eqs. 9, 10, 11).

$$[Co(NH_3)_5Cl]^{2+} + OH^- \underset{}{\overset{fast}{\rightleftharpoons}} [Co(NH_3)_4(NH_2)Cl]^+ + H_2O \quad (9$$

$$[Co(NH_3)_4(NH_2)Cl]^+ \overset{kOH^-}{\longrightarrow} [Co(NH_3)_4(NH_2)]2^+ + Cl^- \quad (10$$

$$[Co(NH_3)_4(NH_2)]^{2+} + H_2O \overset{fast}{\longrightarrow} [Co(NH_3)_5OH]^{2+} \quad (11$$

Garrick did not report any attempts to test his proposed mechanism, but we felt it worthy of being investigated.

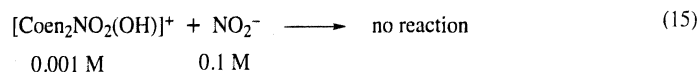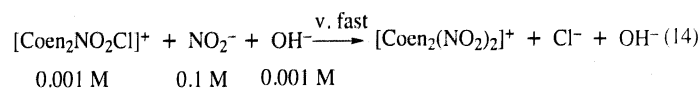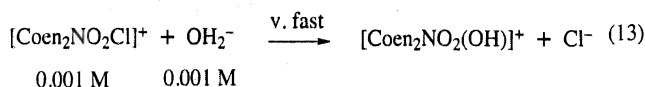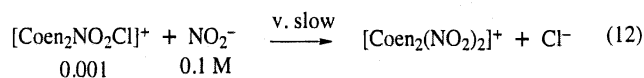Although a reaction mechanism, like any scientific theory, cannot be proved correct, it can be disproved.

For example, this S_N1CB mechanism requires that 1) the complex have a $pK_a > 14$, 2) the complex undergo H—D exchange in D_2O faster than its rate of base hydrolysis, 3) OH^- not be a good reagent if the complex has ligands with no N-H bonds, and 4) the reaction be specifically OH^- catalyzed. All four of these requirements stood up to the tests applied; had any one of them failed. the mechanism would have had to be discarded or modified.

1) A $pK_a = 11$ was reported(16) for $[Co(NH_3)_6]^{3+}$ and a $pK_a > 14$ for $[Co(NH_3)_5Cl]^{2+}$ was indicated because it could not be determined in water. 2) Adamson and I(17) determined that the rate of H-D exchange of $[Co(NH_3)_5Cl]^{2+}$ in D_2O is about 10^6 times faster than its rate of base hydrolysis. 3) We found(18) that OH-concentration had no effect on the rates of solvolysis of three different pyridine complexes $[Co(x-py)_4Cl_2]^+$ which do not have any N—H bonds. 4) The rate of release of Cl^- from an acidified water solution of $[Coen_2NO_2Cl]^+$ is independent of the concentrations(12) or even the presence of added NO_2^-, N_3^-, or NCS^-.

In spite of all of our efforts to test the S_N1CB mechanism, Ingold stated(2):

> Before summarizing the main points of evidence (reported in support of S_N1CB), let me clear away the irrelevancies which have been introduced by remarking that no conclusion can be reached by altering the constitution of the complex by changing the ligands or the metal and discussing the consequent changes in absolute rate. There are so many factors affecting rate, most of which cannot be relatively weighed, that it is always possible plausibly to rationalize observations of this character. I know of no such significant observations that cannot quite reasonably be discussed on the basis of mechanism S_N2.

Confronted with the cleverness of Ingold in rationalizing our experiments in support of an S_N2 mechanism for base hydrolysis of cobalt(III) ammines, we were driven to the necessity of proving that the S_N2 mechanism is wrong. This was achieved(19) by using dimethyl sulfoxide as a solvent and observing the following reactions of $[Coen_2NO_2Cl]^+$ (Eqs. 12-15).
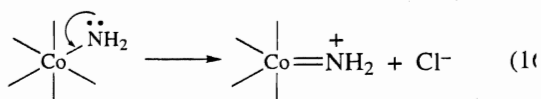
$$[Coen_2NO_2Cl]^+ + NO_2^- \xrightarrow{\text{v. slow}} [Coen_2(NO_2)_2]^+ + Cl^- \quad (12)$$
$$0.001 \quad\quad 0.1\ M$$

$$[Coen_2NO_2Cl]^+ + OH_2^- \xrightarrow{\text{v. fast}} [Coen_2NO_2(OH)]^+ + Cl^- \quad (13)$$
$$0.001\ M \quad\quad 0.001\ M$$

$$[Coen_2NO_2Cl]^+ + NO_2^- + OH^- \xrightarrow{\text{v. fast}} [Coen_2(NO_2)_2]^+ + Cl^- + OH^- \quad (14)$$
$$0.001\ M \quad\quad 0.1\ M \quad\quad 0.001\ M$$

$$[Coen_2NO_2(OH)]^+ + NO_2^- \longrightarrow \text{no reaction} \quad (15)$$
$$0.001\ M \quad\quad 0.1\ M$$

If the S_N2 reaction were correct then the role of OH^- would be to attack cobalt and form $[Coen_2NO_2(OH)]^+$ (Eq. 13). Clearly that is not the role of OH^-, because $[Coen_2NO_2(OH)]^+$ does not react with NO_2^- (Eq. 15). However, a mixture of $[Coen_2NO_2Cl]$, NO_2^-, and OH^- readily afford $[Coen_2(NO_2)_2]^+$ which must occur without the intermediate formation of $[Coen_2NO_2OH]^+$ required, were this to involve an S_N2 pathway. The role of the OH^- here is in keeping with an S_N1CB mechanism. Although these experiments cannot prove the S_N1CB mechanism is correct, they surely prove the S_N2 mechanism is wrong. Just prior to the publication of these results in 1960, I gave a guest seminar at UCL and visited with Ingold in his office. There behind closed doors we discussed our differences, and I stressed our unpublished findings in DMSO solution that disproved his S_N2 mechanism. He was courteous, kind, and listened carefully; but at the end he looked at me and said, "...but, Fred, DMSO is not water." Fortunately, three years later Green and Taube(20), using ^{18}O-labeling in much the same experiment, showed it was not OH^- that attacked the cobalt as required by an S_N2 mechanism, but instead the labeled-O in the product $[Co(NH_3)_5OH]^{2+}$ was derived from H_2O. A few years later a series of competition experiments(21) provided the ratio of products $[Co(NH_3)_5Y]^{2+}/[Co(NH_3)_5H_2O]^{3+}$ for the reactions of $[Co(NH_3)_5X]^{2+}$ in water solution in the presence of OH^- and Y^-, where $Y^- = NO_2^-$, N_3^-, NCS^- and $X = Cl^-$, Br^-, I^-, NO_3^-. The results were consistent with an S_N1CB mechanism. Hundreds, perhaps even thousands, of papers have been published on the base hydrolysis of metal ammines which report results in keeping with the S_N1CB mechanism, perhaps somewhat modified because of the presence of ion pairs(22).

Although Ingold never did state in print that the S_N2 mechanism was disproved, his former student, co-author, and then faculty colleague at UCL, Professor Martin L. Tobe, who continued to do elegant research on this problem, did state(23):

> Probably the closest approach to the correct mechanism is that developed by Basolo and Pearson, who used a suggestion by Garrick that the role of hydroxide was essentially catalytic and that it served to generate a dissociatively reactive entity by removing a

proton from the substrate...., although it ought to be borne in mind that base hydrolysis might take place by more than one mechanism.

One issue to address briefly is why the conjugate base $[Co(NH_3)_4NH_2Cl]^+$ is so much more reactive than are corresponding complexes $[Co(NH_3)_4YCl]^+$ ($Y^- = Cl^-$, OH^-, NCS^-). We suggested(24) this is because NH_2^- is such a strong base that it stabilizes the cobaltonium ion and promotes a dissociative reaction (Eq. 16),

$$\begin{array}{c} | \\ Co \\ | \end{array} \overset{\cdots}{NH_2} \longrightarrow \begin{array}{c} | \\ Co \\ | \end{array} = \overset{+}{N}H_2 + Cl^- \qquad (16)$$

similar to stabilizing the carbocation believed responsible for the high reactivity of α-haloethers (Eq. 17).

$$CH_3\overset{\cdots}{O}CH_2-Cl \longrightarrow CH_3\overset{+}{O}=CH_2 + Cl^- \qquad (17)$$

Furthermore, efficient NH_2^- to Co π- donation requires formation of a trigonal bipyramid intermediate which leads to stereochemical change during reaction that is observed experimentally.

Finally, reference is made to Fig. 3 and the indication that the strongly nucelophilic reagents N_3^- and NO_2^- react with $[Coen_2Cl_2]^+$ by an S_N2 mechanism. We repeated these studies in methanol solution and also added the reagent acetate ion. Our results(13) in buffered solutions showed conclusively that the rates did not depend on the concentrations of the entering ligand, but only on the concentrations of CH_3O^- rapidly generated by these basic reagents (Eq. 5). Thus the role of CH_3O^- in methanol solution is similar to that of OH^- in water and in DMSO, and the results support an S_N1CB mechanism(3).

To my knowledge, the final words in print by Ingold on this subject are contained in his booklet on his *Weizmann Lectures* given in May, 1958. There he states(2):

> ...yet it must be confessed that the first five years (1953-58) of published work consciously directed to the disclosure of mechanism in substitution at octahedral cobalt by the correlated study of kinetics and products has yielded little in the way of fully proved conclusions.. ..I do not dare to hope that the interpretations I have suggested are correct in their entirety. But I do believe them suffciently to think that they are more likely to be modified than scrapped.

Modified they were, but scrapped not because the outstanding insight Ingold possessed about kinetics and

mechanisms of chemical reactions was something very much needed by inorganic chemistry and inorganic chemists. Shoppee in his biographical memoirs wrote(25):

> Ingold had extraordinary imagination, insight, initiative, and ingenuity . . . he possessed one of the greatest intellects in chemistry of the 20th century, and he added a new dimension to organic chemistry.

Pearson, my former colleague and collaborator on research dealing with the base hydrolysis of cobalt(III) complexes, states(26):

> I wholeheartedly agree with these remarks (above of Shoppee) and I will add that he also was a great stimulus to inorganic chemistry. The very fact that he interested himself in the subject of inorganic reaction mechanisms added great stature to the topic. Both Basolo and I recall the sparse audiences to which we would lecture prior to 1953, and the crowds we drew after Ingold's entry into the field.

I strongly second everything stated by Pearson, and hasten to add that the polemic we, as yet untenured faculty at NU, had with Ingold focussed attention on our research and helped make NU one of the best departments of inorganic chemistry in the USA. Needless to say, Pearson and I are very grateful to Ingold and to Nyholm for helping us get off to a good start with our careers in inorganic chemistry.

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MEDIUM EFFECTS OF MICELLES AS MICROREACTORS AND THE SCOPE OF THE HUGHES-INGOLD SOLVENT THEORY

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In 1935, Hughes and Ingold developed a qualitative treatment of solvent effects upon S_N reactions(1,2). An increase in solvent polarity should favor reactions in which charge increases in transition state formation and disfavor those in which charge decreases, or in which an existing charge is dispersed. Effects due to changes in net charge are the more important. The theory focuses attention on the mechanism and charge-type of reactions and provides a useful guide for a large number of nucleophilic reactions, provided that specific effects such as ion-pairing and hydrogen bonding are considered(3). This qualitative approach, with its emphasis on mechanism, can be contrasted with quantitative treatments based on linear free-energy relations, such as the Grunwald-Winstein equation and its descendants(3a, 4). It also differed from theoretical treatments that involved calculations of interactions of ions and dipoles in continuous dielectrics(5).

The meaning of polarity was left open, although Ingold drew distinctions between polar, protic, solvents, which form strong hydrogen bonds, especially with small anions, polar aprotic solvents, which solvate cations, and apolar solvents. These distinctions form the basis of quantitative treatments, such as that based on the solvatochromic scale of Kamlet, Taft, and Abraham, which sets out to quantify the factors controlling polarity(6). In many reactions, the role of specific solvent interactions, *e.g.*, hydrogen bonding to anions or anionic leaving groups, has been identified(1,3,6).

The Ingold school examined S_N reactions of nonionic and cationic substrates and nonionic and anionic nucleophiles in testing the Hughes-Ingold solvent treat-

ment and showed that it fitted a large amount of data, although for S_N2 reactions in aqueous ethanol the presence of two potential nucleophiles was a complication(1,2).

Ingold pointed out that solvent effects are similar for aliphatic and aromatic substitutions by anions(1). Spontaneous reactions that involve charge dispersion, *e.g.*, anionic decarboxylations(7) and solvolyses of dianionic aryl phosphates(8) are inhibited by an increase in solvent polarity, especially for protic solvents where hydrogen bonding stabilizes the initial state, and solvent effects are often very large. The original theory is therefore a useful qualitative predictor of kinetic solvent effects for many organic reactions.

While this work on the relation of solvent effects to mechanism was in progress at University College, Hartley, at the School of Pharmacy, was examining properties of association colloids, for example, micelles, which are self-assemblies of surfactants (detergents), induced by operation of the hydrophobic effect(9). (Hartley's work in colloid science was started with Donnan at University College). He elucidated the source of the "colloid error," which is the effect of synthetic and natural colloids on acid-base indicator equilibria, and showed that cationic micelles increase deprotonation of weakly acidic indicators and neutralization of carbocationic dyes by OH^-, and anionic micelles have the opposite effect. The treatment was extended to equilibria involving variously charged indicators in anionic, cationic and nonionic micelles. If micelles affect equilibria they must also affect reaction rates, and Hartley's rules for indicator equilibria were

later shown to apply to reactions involving H_3O^+ and OH^- and other anions. For example, reactions of OH^- with nonionic substrates are speeded by cationic, and inhibited by anionic, micelles(10).

At first sight, there appears to be little connection between Ingold's ideas on kinetic solvent effects and Hartley's work on micellar effects on indicator equilibria, and to make the connection we must consider the structure of micelles and the way in which they control reaction rates and equilibria.

Micellar Structure

Surfactants (detergents) are amphiphilic ions or molecules that have ionic or polar head groups and apolar, hydrophobic residues, typically *n*-octyl to *n*-octadecyl groups(9,10). In dilute aqueous solution surfactants are monomeric, but above the critical micelle concentration (cmc) they associate to form micelles, which, for dilute surfactant, are approximately spherical and contain about 10^2 monomers. Micelles are thermodynamically stable, but monomers and incorporated solutes enter at near diffusion rates(11), so that equilibrium is maintained between monomer and micelle, and, for relatively slow thermal reactions, between reactants in water and in micelles.

Hydrophobic interactions cause the apolar groups to associate in a hydrocarbon-like core, with polar or ionic head groups at the surface in contact with water. Micellar structure depends on a balance between hydrophobically-driven association of the apolar groups and head group repulsions which, for ionic micelles, are reduced by association with counterions(10,12).

Ionic micelles attract counterions to their surface and repel co-ions, and they also incorporate nonionic solutes. As a result, reactant concentrations in the interfacial surface region can be much higher than in water, which increases rates of bimolecular reactions of counterions(13). Micelles also influence rates of spontaneous reactions, and we can treat them as submicroscopic reaction media, distinct from bulk solvent. It is convenient to regard micelles and water as pseudophases(10,13).

Polarities, or apparent dielectric constants, of micellar surfaces are estimated by examining spectral shifts of probes on their transfer from water to micelles on the assumption that spectral relations with properties of bulk solvents are applicable to submicroscopic interfaces(5,10,13-15). Based on this leap of faith, we conclude that these interfaces are less polar than water and much more polar than alkanes. Head groups are exposed to water, but its apparent activity is lower than in bulk water(16). We can then ask whether the Hughes-Ingold solvent theory can be applied to reactions in micelles or other association colloids, but we first have to separate the overall reaction rate into the rates in the two pseudophases.

A typical cationic surfactant is a salt of the cetyltrimethylammonium ion, $C_{16}H_{33}NMe_3X$ (CTAX, X=Br, Cl, OMs). Sodium dodecyl sulfate, $C_{12}H_{25}OSO_3Na$, SDS, is the most studied anionic surfactant, and these surfactants were generally used in the work discussed here.

The Pseudophase Kinetic Model

Application of the pseudophase model to reaction rates and equilibria in association colloids requires estimation of reactant concentrations in the colloidal pseudophase, *i.e.*, analysis of the transfer equilibria(13). The problem is simple for spontaneous reactions, where we consider only transfer of the substrate. When it is fully transferred to the micelle the rate constant of the overall reaction is that in the micelle. The first order rate constant as a function of micellized surfactant (detergent) concentration, $[D_n]$, is given by(13):

$$k_{obs} = \frac{k'_W + k'_M K_S [D_n]}{1 + K_S [D_n]} \quad (1)$$

where k'_w and k'_M are first order rate constants and K_s is an association constant.

Micellar rate effects upon spontaneous reactions follow the predictions of the Hughes-Ingold solvent theory on the assumption that micellar surfaces are less polar than water. For example, for S_N1 and S_N2 hydrolyses of alkyl halides and arenesulfonates $k'_w > k'_M$(17). There is also a charge effect of the micelle, because of charge asymmetry at its surface, so that for an S_N1 reaction of a nonionic substrate the forming positive charge in the transition state interacts unfavorably with cationic head groups. This unfavorable interaction is absent in anionic micelles, so they inhibit S_N1 reactions less than cationic micelles. In S_N2 and similar solvolyses negative charge tends to develop on the organic residue and inhibition is lower in cationic than in anionic micelles. These generalizations also apply to hydrolyses of acyl and sulfonyl chlorides(17,18). The more S_N1-like is the transition state, the higher is the rate in anionic as compared with cationic micelles.

Spontaneous reactions that involve dispersion of an existing charge, *e.g.*, decarboxylations(19) and hydrolyses of aryl sulfate monoanions(20) and aryl phosphate dianions(21), are faster in cationic micelles than in water, as predicted by the Hughes-Ingold solvent theory.

In some cases, rate enhancements are by factors of 10^2 or more, due to changes in initial state hydration and interactions of micellar head groups with anionic transition states.

It is more difficult to analyze micellar effects upon nonspontaneous reactions because rates now depend upon concentrations of both reagents at the micellar surface(13). In some cases, the transfer equilibrium of a reagent between water and micelles can be estimated directly. This approach has been used for some ionic and nonionic organic nucleophiles, *e.g.*, amines and aryloxide ions(22) and for chloride and bromide ions(23). In other cases, *e.g.*, for reactions of OH⁻, ionic concentrations are calculated by theoretical treatment of micelle-ion interactions(13,24). Provided that these concentrations can be estimated, we can write k'_w and k'_M in terms of the local concentrations of a nucleophile, Nu, in water and at the micellar surface and the second order rate constants, k_w and k_2^m

$$k'_w = k_w[Nu_w] \qquad (2)$$
$$k'_M = k_2^m \, Nu_M \qquad (3)$$

where $[Nu_w]$ is the molarity in water and NU_M is the <u>local</u> molarity in the watermicelle interfacial region, not the molarity in terms of the total solution volume(13).

Micellar effects upon S_N2 reactions of Cl⁻ and Br⁻ with methyl naphthalenesulfonate fit the Hughes-Ingold solvent theory and k_2^m/k_w is in the range of 2-3 with several cationic micelles(23,24). Bimolecular reactions of nonionic nucleophiles with carboxylic esters and 2, 4- dinitrofluorobenzene are, as predicted, slower at micellar surfaces than in water(13,22).

For most bimolecular reactions of hydrophilic nucleophilic anions, *e.g.*, OH⁻, with a variety of organic substrates, values of k_2^m are similar to, or slightly lower than, those in water(13). Some of the substrates are very hydrophobic and transfer from water to micelles lowers their free energies, which, of itself, inhibits reaction.

Provided that transfer equilibria between water and micelles are taken into account, micellar rate effects on many bimolecular nucleophilic reactions are as predicted for a medium that is somewhat less polar than water. The charge on the micellar surface markedly affects transfer equilibria, especially of hydrophilic ions, but has little effect upon free energies of activation at the micelle-water interface. This generalization also applies reasonably well for reactions involving electrophilic cations, e.g., H_3O^+, where overall reaction rates are increased by anionic micelles(13).

The situation is completely different for reactions of electrophilic anions, *e.g.*, oxidations of sulfides by IO_4^- and HSO_5^-(25) and reactions of alkenes with Br_3^- (26). Cationic micelles concentrate these anions in the interfacial region, which should increase reaction rates; but rate enhancements are very small, or nonexistent(25, 26). Based on estimation of reactant concentrations in the micellar pseudophase, k_2^m is smaller than k_w by two to three orders of magnitude. This behavior is very different from that seen for reactions of nucleophilic anions.

Although ionic micelles repel co-ions, they have a small, but finite concentration in the interfacial region at the micellar surface, and it can be estimated, based on a theoretical model(27). This calculation has been made for IO_4^- in micelles of SDS, and values of k_2^m for sulfide oxidations are lower than k_w by factors of 3-5(25b). This small inhibition is as expected because of the lower polarity of an SDS micelle relative to water(14). This striking difference in kinetic behaviors of cationic and anionic micelles can be ascribed to interactions between micellar head groups and the transition state. In sulfide oxidation, an electron is transferred from sulfur to the anionic oxidant with development of positive charge on sulfur(25,28). Interaction of this charge with a cationic micellar head group strongly inhibits oxidation, but this unfavorable interaction disappears for oxidation at the surface of an anionic micelle, although the overall reaction is slow because of the very low concentration of IO_4^- at the anionic micellar surface.

The same unfavorable charge effect applies to bromination by Br_3^- at cationic micellar surfaces where the forming bromonium ion interacts unfavorably with the head groups(26). The reaction of Br_3^- cannot be examined in solutions of SDS where equilibrium between the brominating agents favors Br_2.

Conclusions

Rate constants in the interfacial region at surfaces of aqueous micelles depend upon the properties of this region as a submicroscopic reaction medium and the mechanism and charge type of the reaction(13,17-24). For bimolecular reactions of nonionic nucleophiles, decreases in the second-order rate constants, relative to those in water, are consistent with the Hughes-Ingold solvent theory and the lower polarities of micellar surfaces relative to water(22). Similar considerations apply to S_N2 reactions of halide ions with methyl arenesulfonates which are slightly faster at cationic micellar surfaces than in water(23,24b).

The theory also predicts the lower first-order rate constants of S_N1 and S_N2 hydrolyses of nonionic substrates at micellar surfaces relative to water. However,

because the interfacial region is electrically asymmetrical it is necessary in S_N1 reactions to consider interactions between the dipolar transition state and the micellar head groups(17). This problem of electrical asymmetry also complicates analysis of micellar rate effects upon oxidations or brominations by anionic electrophiles(25,26).

I do not believe that Ingold ever considered the extent to which association colloids might affect reaction rates and equilibria, and systematic study of reactivity in these systems required the availability of readily usable spectrophotometers for rate measurement(10). The success of the Hughes-Ingold solvent theory, as applied to micellar rate effects, shows how well-considered ideas on the factors that control reactivity in one set of conditions, or one type of reaction, can generally be applied over a much wider range of conditions. The generalizations that Hartley developed to explain micellar structure and effects on indicator equilibria(9) have also stood the test of time; and the two sets of ideas come together to explain the behavior of association colloids as reaction media.

ACKNOWLEDGMENTS

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A biography covering details of Ingold's early life, education, and professional accomplishments, together with extensive information about Hilda Usherwood Ingold and E. D. Hughes, has been published since the ACS Symposium: K. T. Leffek, *Sir Christopher Ingold, A Major Prophet of Organic Chemistry*, Nova Lion Press, 980 Kentwood Terrace, Victoria, BC, Canada, V8Y 1A6.

A PERSONAL HISTORY OF THE BENZIDINE REARRANGEMENT

Henry J. Shine, Texas Tech University

What I have to say today amounts more to a personal account rather than an honest-to-goodness history of the benzidine rearrangements. Interest in the mechanism of these rearrangements spans this century from the early 1900s to the present day. Prominent in studies of the rearrangements and their mechanisms during these ninety years is the name of the man whose memory we honor today, C. K. Ingold, associated also with his long-time colleague and collaborator E. D. Hughes and their younger coworker Derek Banthorpe. Also prominent in these studies are the names of Paul Jacobson, Michael Dewar, Robert Carlin, Miroslav Vecera, and George Hammond and me. I don't know how or why Carlin and Vecera took up the benzidine rearrangements. Carlin, of course, was much interested in the Fischer indole reaction, and that is connected with benzidine rearrangements, as you know. Possibly, that's how the rearrangements snared his interest. I would guess that Ingold's attraction and that of Dewar developed from their much broader interests in the electronic theories of organic reactions and the mechanisms of all molecular rearrangements. As for me and George Hammond, our interest in benzidine rearrangements was quite unplanned and entirely accidental. This is how it happened.

In early 1948 I went to Iowa State College as a postdoctoral fellow with Henry Gilman. I went there with an arrangement, that I had proposed, to study under his guidance the mechanism of Grignard reactions, particularly those which I called anomalous (enolization, condensation, and reduction of carbonyl compounds) and which I had been studying with E. E. Turner at Bedford College in London during the years 1945-1947. My arrangement with Henry Gilman broke up in the fall, 1948, and I became an independent research associate supported by the Department of Chemistry. In return for that support, I became an instructor of a quiz section in sophomore organic chemistry when the section's graduate-student instructor left Ames on short notice. During one of the quiz-section periods a student asked how the benzidine rearrangement, mentioned only briefly in his book, occurred (Eq. 1) this equation shows two products, as is correct.

$$(1)$$
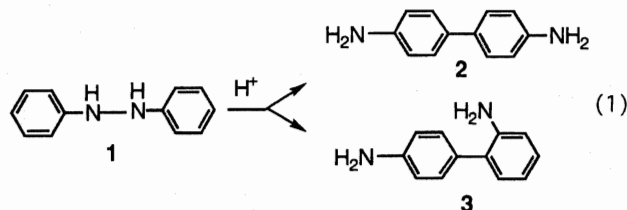
The student's book would have shown only the major product, benzidine, **2**). Of course, I had no idea, at all. George Hammond had joined Iowa State College in the fall, 1948, bringing to the Department a wonderful freshness and vigor in organic chemistry. I felt sure that George would know the answer to the benzidine question and promised the student that I would find the answer for him. It turned out that Hammond knew no more than I about the rearrangement, and he suggested that I search it out in the literature and report on it at one of

the Tuesday evening seminars he had started after arriving at Ames. I don't remember if I ever did answer the student's question, but it was responsible for leading me and Hammond into parts of our life-long research commitments. Search of the literature turned up Dewar's π-complex theory of the rearrangement and the earlier of his two experimental works in it(1). Hammond noticed that in that work Dewar had assumed, but not verified, a first-order dependence on acid, and recommended that I should check it out myself. At the time, I was devoted to studying anomalous Grignard reactions, measuring the quantities of gaseous alkenes and alkanes they gave rise to, and trying to tie them to a mechanism of reaction(2). I knew nothing then about electron-transfer, radical ions, and esr, so the task I had set myself of solving the mechanism of Grignard reactions was, in fact, close to hopeless; but I didn't know it. Nevertheless, I accepted Hammond's challenge to repeat Dewar's work on hydrazobenzene. Fortunately, Dewar had discovered the clever use of Bindschedler's Green and titanous chloride in titrating hydrazo compounds, so it didn't take me long to learn how to measure the rate of disappearance of hydrazobenzene. The work was finished in two or three months, and I remember telephoning Hammond at home on a Sunday morning, after all of the acid-dependence had been plotted out, to tell him: "George, it's second order." Hammond's reply was: "I'll be over soon." Neither of us, I believe, had any idea that our work would keep us busy, separately, for years, or that it would trigger a new interest by Ingold. I am not certain of that triggering, in fact; but experimental work on the rearrangement seems to have begun again in Ingold's laboratory in the mid 1950s after a lapse of 20 years. Certainly, Hammond and I were unaware that Carlin was already at work (at Carnegie Tech in Pittsburgh), measuring rates of rearrangement with the more laborious but informative spectroscopic analysis of reactant and product concentrations. Carlin's very nice work not only confirmed my finding of second-order acid-dependence for hydrazobenzene, but also disclosed the mixed first- and second-order acid kinetics for *o*-hydrazotoluene, a key finding that was used later by Ingold in establishing the foundations of further understanding. Hammond continued work on acid-catalyzed benzidine rearrangements for some years at Iowa State and, later, at Caltech. I left the work on departing Ames and going, (coincidentally, and before Hammond), to Caltech in 1949 for postdoctoral work with Carl Niemann on kinetics of chymotrypsin-catalyzed ester hydrolysis. I had no thought at all of ever returning to the benzidine reaction. How that happened is told later in this story.

First, it is time to turn to Ingold. The bulk of Ingold's experimental work on the benzidine rearrangement was published in the early 1960s. In that period, with his collaborators, Hughes and Banthorpe, he was intimately concerned with the kinetics and the effect of acidity and hydrazoarene structure on them. We must go back to 1933, however, for Ingold's first publication of experimental work on the benzidine rearrangement, and we shall see the very curious lapse in his attention to the kinetics between that time and the 1960s.

In 1933 Ingold and Kidd published their proof of the intramolecularity of the benzidine rearrangement. Their proof lay in rearranging a mixture of two similar hydrazobenzenes, 2,2'-dimethoxy- (**4a**) and 2,2'-diethoxyhydrazobenzene (**4b**) and showing that only **5a** and **5b** were formed; that is, a crossed benzidine product was not obtained (Eq. 2)(3).

4a, X = OMe   **5a**, X = OMe
4b, X = OEt   **5b**, X = OEt
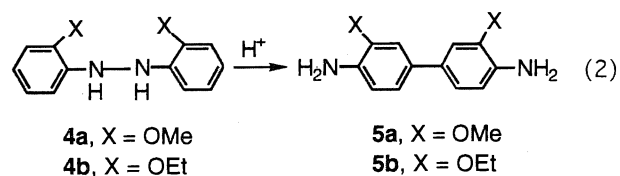
Evidence for intramolecularity was already available among the many examples of rearrangement of unsymmetrical hydrazobenzenes in Jacobson's tabulations(4). Ingold acknowledged this, but felt that the better test would be with symmetrical hydrazobenzenes, such as **4a** and **4b**, which, in the understanding of the time, could not separate into halves of different polarity, and thus bias recombination, a possible flaw he saw in Jacobson's examples. An important criterion of the mixed rearrangement test was that the chosen compounds should rearrange at similar rates. Under the conditions chosen by Ingold and Kidd for rearranging **4a** and **4b**, the rates were too fast to be measured, so that the method was used of measuring the amount of product each gave within a specified time. In that way **4a** was found to rearrange about six times faster than **4b**. Here we find the enigma, which unfortunately can never now be solved, of how Ingold overlooked in later years what appears to have been known to him about benzidine kinetics in 1933. In the paper of 1933, and in commenting on the need to know the relative rates of rearrangement of **4a** and **4b**, Ingold wrote(5):

Measurements indicating the speeds of benzidine conversion have been recorded by Holleman and van Loon (*Proc. K. Akad. Wetensch*. Amsterdam **1903**, *6* 262)[6] and by Biilmann and Blom (*J*., **1924**, *125*,

1719), but under conditions corresponding to those of our experiments on simultaneous isomerization, it was not possible to obtain satisfactory direct measurements of velocity, owing to the rapidity of the reactions.

Now, the attempts by Biilmann and Blom to correlate rates and acidity failed(7). But, Holleman and van Loon were explicit about the dependence on acidity in the formation of benzidine (**2**) from hydrazobenzene (**1**), and said so with their Eq. 3 (modified slightly here):

$$d[2]/dt = k[HCl]^2 \qquad (3)$$

(3) Furthermore, they included a drawing of N,N'-diprotonated hydrazobenzene, leaving no question as to how these earliest of kineticists saw the catalysis of rearrangement(6). It is possible that Ingold and Kidd overlooked the significance of these findings because they were very much more concerned with products. A second criterion for a successful mixture experiment was that the rearrangements be uncomplicated. Each component must give only a benzidine and none of the other possibilities in the family of products. Rearrangements of **4a** and **4b** satisfied that criterion, except that each also disproportionated. It was, in fact, the disproportionation and a way of avoiding its complications that led Ingold and Kidd to choose the conditions of rearrangement, the conditions that prevented them from measuring rates directly. That is, rearrangement of a mixture of **4a** and **4b** was carried out in dry ethanol containing hydrogen chloride. Addition of dry ether to the solution after rearrangement precipitated the hydrochlorides of the two benzidines but left the disproportionation products in solution, thus effecting the clean separation desired. The fact that disproportionation occurred impressed Ingold and Kidd. They noted that(5):

> We know of no previous record of this reaction, although van Loon (*Rec. Trav. Chim.*, **1904**, *23*, 162) observed the formation of a mononuclear amine from a hydrazo-compound in the absence of an external reducing agent.

Here, we find the second part of the enigma: The paper in *Recueil* that Ingold and Kidd refer to is, in fact, one by Lobry de Bruyn and Wolff(8) on Tyndall effects. van Loon's paper occurs earlier in the issue(9), and while he does indeed report the formation of mononuclear amine, the emphasis of this paper is on the relative yields of benzidine (**2**) and diphenyline (**3**) obtained from **1** and on the rearrangement's kinetic order in acid. van Loon's paper is, in fact, the detailed report of the work described briefly earlier in the *Proceedings*(6), to which Ingold and Kidd also referred(3). van Loon's full paper(9) pre-

sents the kinetic method in great detail (quantitative assay of **2**, with time, as its dihydrogen sulfate) and under a variety of conditions, and ends again with the kinetic equation, Eq. 3. It seems extraordinary that Ingold could have overlooked this. Further, van Loon was quite explicit about the mononuclear amine. It was obtained along with azobenene, but only when rearrangment of hydrazobenzene was carried out at elevated temperatures and in relatively concentrated acid. van Loon recognized that the formation of azobenene meant that aniline should be formed. He went on to isolate it, gave the disproportionation reaction for its formation, and characterized it as its derivative benzanilide(9).

How could Ingold have glossed over van Loon's work? We shall never be able to answer that question. Could he have relied on the reading by the less experienced Kidd? The question haunts us because of how Ingold wrote eight years later about the role of acid in the benzidine rearrangement. I refer here to the paper by Hughes and Ingold on the "Theory of Aromatic Substituents and Rearrangements with Special Reference to the Benzidine Change"(10). This paper is concerned particularly with the difference between Robinson's and Ingold's views of the rearrangement and may well have been written in response to Robinson's sudden (apparently), awakened interest in the benzidine rearrangemen(11). Robinson represented rearrangement with a flow of electrons in protonated hydrazobenzene(11). Ingold, however, went to great pains to distance himself from the idea that protonation preceded N-N bond scission. He and Hughes said (2):

> We may collect these points as follows: (a) A nearby proton (at least one) disturbs the electronic system of the benzidine (sic) molecule; (b) therefore, this proton (at least) is present in the transition state; that is to say, its coordinates enter into the specification of the state; (c) but its covalent bonding with nitrogen does not precede the N-N heterolysis.

In the same paper, Hughes and Ingold present drawings of electronic structures that could represent states between hydrazobenzene and benzidine, and they say of these structures (Scheme 1)(13):

> As we do not know yet whether one or two adding protons are included in this state (the *p*-benzidine transition state) we shall omit them ...

It is hard to understand how Ingold could say this eight years after having referred(3) to van Loon's papers. No mention is made of these papers in the analysis by Hughes and Ingold(10).

Scheme 1
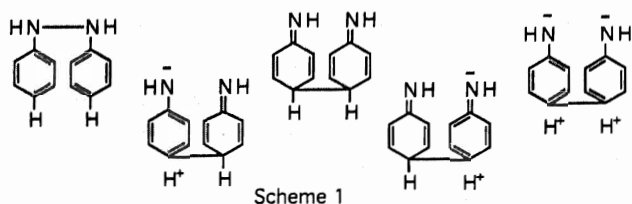
If I seem to dwell at unseeming length on van Loon's papers, it is not for the purpose of uncovering a flaw in Ingold's benzidine work, but to set the following historical point. None of the writers, not just Ingold, of the early years appears to have paid any attention to van Loon's second-order acid dependence. Had they done so, in fact, the history of the benzidine rearrangements would certainly have taken a different turn, and it is certain, too, that neither Hammond nor I would have had any part in it.

It is remarkable that as early as 1918 the Robinsons were writing about the mechanism of the benzidine rearrangement in connection with their interest in the Fischer indole reaction and its role in their preparation of tetraphenylpyrrole(14). They ruled out the possibility, voiced occasionally at that time, that *p*-aminodiphenylamine (i.e., *p*-semidine) was an intermediate in the formation of benzidine, by showing that *p*-semidine was unaffected by being heated with concentrated hydrochloric acid. They fitted the mechanism of the benzidine rearrangement into a generalized scheme covering other "enimic to enamic" rearrangements without being specific about protonation.

I am not aware that Robinson engaged directly in any experimental work on the benzidine rearrangement. Yet, remarkably, his address as President of the Chemical Society in 1941 was devoted largely to the mechanism(11). In that address he formulated the, now, curious alternating flow of electrons, to which I referred above, beginning with monoprotonated hydrazobenzene, from which Hughes and Ingold distanced themselves as noted above. Insofar as this history is concerned, Robinson's monoprotonation(15), which was continued by all writers up to 1950, is all the more remarkable, because he, like others, referred to Jacobson's exhaustive summary without, apparently, reading or heeding its last paragraph. Robinson called attention to Jacobson's summary in the following way(16):

> Paul Jacobson, who devoted a large part of his experimental work to the study of the benzidine and semidine transformations of substituted hydrazobenzenes has published a summary of everything known on the subject in 1922 (*Annalen*, **428**, 76) together with a discussion of the course of the

reactions. In view of this it is unnecessary to repeat the historical account....

In Jacobson's historical account van Loon's kinetic work is recorded but second-order acid dependence is not disclosed. Jacobson's failure to state the kinetics of rearrangement explicitly is another benzidine enigma. Possibly, he neglected the specifics of the kinetics because of his lack of kinetics training to which he refers. Certainly, had Jacobson dwelt on the kinetics and their meaning, the history of the rearrangements must have been different. This is what Jacobson said(17):

> Ein weiterer Einblick–besonders auch in Bezug auf die Art, in welcher die als Hervorrufer der Umlagerung wirkenden Säuren an dem Vorgang teilnehmen–dürfte in erster Linie durch reaktionskinetische Erforschung der Umlagerung zu erwarten sein, auf die ich wegen Mangel an Zeit und Schulung verzichten musste. In dieser Richtung liegen einstweilen (ref. 2) nur einige in Holleman's Laboratorium durchgeführte Versuche von van Loon (ref. 3) vor; aus ihnen wird der Schluss gezogen dass die Geschwindigkeit, mit der verschiedenen Säuren die Umlagerung von Hydrazobenzol bewirken, durch ihre Ionization bestimmt wird.

If only Robinson had followed up Jacobson's lead, and had found van Loon's paper, how different the history would have been.

It was left to Hughes and Ingold to call all of us to order by pointing out in 1952 that Hammond and I in 1950 had only re-discovered what van Loon had reported in 1904(18). I remember feeling chagrined at reading that 1952 review, because it made me feel that I had missed the boat in 1949, and that the more perceptive English group had caught it in 1952. In fact, the failure of Hammond and me to find van Loon's work indicates how peripheral was our interest in the rearrangement in 1949. I made no further literature search after finding Dewar's paper(1), relying on his account of the rearrangement; and even after I took up benzidine chemistry again in the late 1950s, I still failed to read the literature carefully, so that I was unaware until going over Ingold and Kidd much, much later that, in some way, Ingold himself had then missed the same boat. Strangely, Carlin, a very careful experimenter, referred to van Loon's work, not for its kinetic import but for information on products(19). In turn, Dewar(20), in support of his π-complex theory of the rearrangement, referred to this information on van Loon's products in Carlin's paper, but apparently did not check into van Loon's paper itself. The consequences for the π-complex theory, based therefore on monoprotonated hydrazoarenes, were se-

vere. That story has been recounted elsewhere and need not be retold here(21).

Between 1933 and 1957(22) Ingold published no experimental work on the benzidine rearrangement. During those years appeared an outpouring of papers on mechanistic organic chemistry, particularly on nucleophilic substitution, elimination, and aromatic substitution, especially nitration. Interest in the benzidine rearrangement appears to have been peripheral until its grand resurgence in the early 1960s. The paper in 1957, bearing also the name (C.A.Bunton) of one of the contributors to this Symposium, established the relationship between rate of rearrangement and acidity function. It established also that acid catalysis was specific rather than general. This paper, then, was the forerunner of the series of 15 experimental papers in the *Journal of the Chemical Society* spanning the years 1962-1964, that were to follow from the group of Hughes, Ingold, Banthorpe, and coworkers. These papers reported the detailed kinetics and product studies of rearrangements of a variety of hydrazoarenes, chosen so as to illustrate the scope in kinetics in benzidine rearrangements, then being uncovered at UCL. The UCL group was primed by Carlin's discovery of mixed order (1.6) acid catalysis to show that some hydrazoarenes rearranged under second-order acid catalysis (e.g., hydrazobenzene) and some by first-order catalysis (e.g., 1,1'-hydrazonaphthalene), while others could exhibit both orders, depending on the acidity of the solution. The UCL group found the solvent, ring-deuterium isotope, and salt effects that characterized the course of protonation at nitrogen and the polar nature of the transition state in rearrangement. Discovering the duality of acid catalysis must have been particularly pleasing to Ingold in that it resembled an earlier duality in mechanistic pathways he had discovered, namely the S_N1/S_N2 and E1/E2 reactions. The series of investigations included thermal rearrangements, whose kinetics I had studied myself a little earlier and to which I refer later. The series represented the completion of the development of the polar-transition-state (PTS) theory of rearrangement, the beginnings of which can be seen in the paper by Hughes and Ingold in 1941(10). Part XV of the series, called "A Collective Discussion," is 37 pages long(23). It summarizes the results and meaning of the preceding parts in the series and is also an analysis of other theories and their shortcomings. The massive undertaking was designed to support the PTS theory, showing how it accounted not only for ways in which substituents affected the kinetics of rearrangements, but also, with the exception of the *p*-semidine rearrangement (of

which more is said later), the types of products formed. This, then, was the end of Ingold's search for an answer to the mechanism of the benzidine rearrangements; that is, the establishment of the PTS theory. Further experimental work continued at UCL but mainly in the hands of Derek Banthorpe. In all, the series reached to Part XXVII in 1973(24). Ingold's participation in the last papers was small, there being three papers bearing his name, in 1967(25) and 1968(26). It is sobering to recognize that the benzidine papers were published when Ingold was in his 70s and after he had offically retired in 1961. Ingold and I were corresponding about the rearrangements in 1967. On June 20 of that year he wrote(27):

> When I wrote the other day, I had not seen Banthorpe (who is largely concerned with biosynthetic work now-a-days)(28) and did not know (or had forgotten) that, during the year in which I have been withdrawn from the benzidine problem, he has been going on with it......we have more rate-product studies than I told you of .... I don't think the new data alter my general appraisal of the position, as I wrote it in my letter (June 7). But as the area of consistency is larger than I thought it was, I feel rather more assurance than I did.

Thus, the benzidine story for Ingold came to an end essentially in 1964 with the PTS theory, in his 71st year(29).

In July, 1963, the IUPAC meeting on Pure and Applied Chemistry was held in London. I had been looking forward to seeing the UCL group and talking about the rearrangements; but Hughes died in June, 1963, Banthorpe was on holiday while the IUPAC meeting was on, and Ingold's time was sought by many international visitors. At a pub lunch, our only meeting, he asked, "What is the remaining question to be answered in benzidine rearrangements?" Even after 20 years I was still somewhat intimidated by the residue of awe inculcated in me in my two undergraduate years at Aberystwyth(30) and sensed that Ingold was asking a question to which he already had an answer. My answer was pragmatic: to find how disproportionation fitted with rearrangement. His answer, which indeed he had had all along was: to know what the transition state was like. This was the question he had posed at the "Symposium on The Transition State" in Sheffield in April, 1962, at which he displayed his now famed drawing of a transition state hidden in the clouds (Fig. 1)(31). In the printed version he wrote(32):
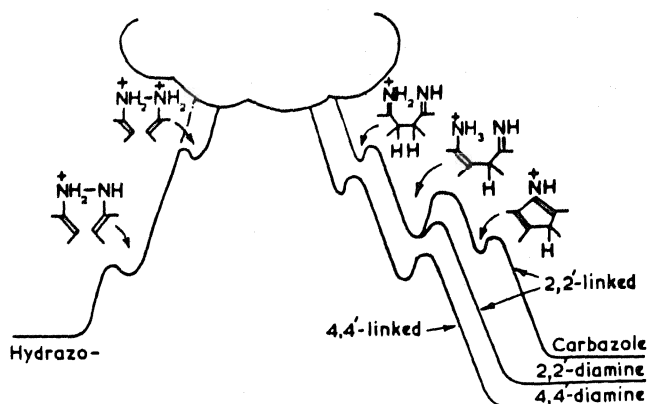
Figure 1. Reproduction (with permission of the Royal Society of Chemistry) of Fig. 8, Ref. 31, showing Ingold's cloud over the transition state of the rearrangement of hydrazobenzene.

Fortified by these reminders of partly solved problems, let us now face an unsolved one, that of the mechanism of the benzidine rearrangement, the transition states of which must be like nothing else in this world.

He summarized what was then known and outlined how it could be explained with polar transition states. He concluded(33):

> I do not change my view that the problem of the transition state of the benzidine rearrangement is unsolved. I have offered certain tentative interpretations only in order to show how it may be possible to fit known facts into a picture. To establish the unique correctness of the picture is another matter.

The unique correctness, from his point of view, became the PTS theory. According to the theory, the transition state of each rearrangement became polar by the heterolysis of the N-N bond of the hydrazoarene. Heterolysis for first-order-acid cases was initiated by monoprotonation at one of the nitrogen atoms and led to two dissimilar halves of the PTS, one bearing positive charge and the other being neutral. Which half bore the charge depended on the relative basicities of the two nitrogen atoms and the relative capabilities the rings had to delocalize the charge. These relative conditions were controlled by substituents in the rings. In second-order-acid cases the dominant role in causing heterolysis was attributed to the first of the two protons. Nevertheless, pre-equilibrium diprotonation was called for by the solvent deuterium kinetic isotope effect and the acidity function dependence at high acidities, which

the UCL group had discovered. An important criterion was that both protons were in place in the transition state. Again, heterolysis was such that one half of the TS bore two positive charges and the other half had the character of a primary amine. In the PTS theory, product type (benzidine, diphenyline, semidine) was determined by the dispositions of polarity in the rings, the dispositions being affected by the placement and nature of substituents. Examples of Ingold's TS drawings(23) are shown in Scheme 2(34).

Scheme 2
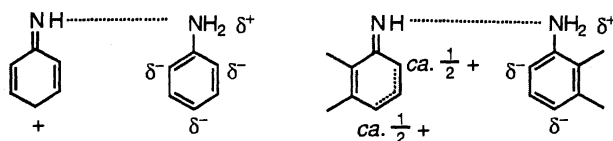
The PTS theory was the most comprehensive of all of the theories that had appeared on the mechanism of the benzidine rearrangements. The earliest of theories proposed, for example, by Tichwinsky(35) and by Stieglitz(36) called for scission of the N-N bond into two fragments, either with or without prior protonation, and the recombination of the fragments. This intermolecularity was essentially disposed of by Jacobson, who quoted thereto his 65 examples of rearrangement of unsymmetrical hydrazo compounds, not one of which had given evidence of crossed products(4). Jacobson came down on the side of intramolecular rearrangement without understanding fully how it occurred. The formation of benzidine from hydrazobenzene was described by him as nothing more than an exchange reaction in which two *para* hydrogens changed place intramolecularly with a single bond. It is interesting to see Jacobson's diagram for this exchange and compare it with our modern representation of a sigmatropic shift. Jacobson represented the *para* hydrogens as migrating to the nitrogen atoms at the time of forming the new single bond. Analogously, the semidine rearrangement was represented as the intramolecular replacement of a *para* hydrogen by a phenylimino group, the hydrogen migrating at the same time to the second nitrogen atom. The role of the acid was not explained, however, nor was protonation shown. Jacobson was candid about his interpretation, moreover, acknowledging that the objection could be raised that it was not a clarification but only a rephrasing of the facts. It was this open appraisal of his position that he followed with his observation, that is quoted earlier, on the need for kinetics.

Some years before Ingold's interest in the benzidine rearrangements was reawakened, a simple and very clever explanation for them was given by Dewar. This

came to be known as Dewar's π-complex theory of the rearrangement. It was first proposed in 1945(39) and incorporated a year later in a general theory of a number of related rearrangements(20). The π-complex theory was initially very attractive. The basic idea of it was that the N-N bond of a protonated hydrazoarene broke heterolytically and led intramolecularly into a complex of the two halves. These could then rotate, depending on their structure, to the conformation favoring product formation. Dewar's earliest presentation(39) of this idea is shown in Scheme 3.

Scheme 3
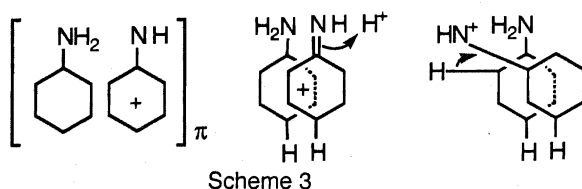
The theory assumed at the start that monoprotonated hydrazobenzene rearranged(20,39), and this assumption was maintained in Dewar's experimental work by his failing to measure acid dependence in the rearrangements of hydrazobenzene itself and three other hydrazobenzenes(1). The finding of second-order acid dependence by Hammond and me in 1950(40), and a little later by Carlin(41) was bad news for the π-complex theory. This was unfortunate, in a sense, because an intrinsically clever idea received the criticism due only to one of its faulty props. The story of the succession of flaws in parts of the theory has been described already(21). The theory received a pummelling because of them but continued to be promulgated by Dewar after being shorn of faults. As far as I can tell, the last of Dewar's writings on the π-complex theory of the benzidine rearrangement was in 1965(42).

A major supporter of an intermediate-complex (π- or molecular) theory was Vecera, who published a substantial amount of experimental work, initially on the chromatographic analysis of products(43) and later on the mechanism of rearrangement; but the sparse attention paid to his work by others does not reflect its wide scope. Possibly, the reason is that the publications were mainly in Czech and in German. The last of the series of papers, published, this time in English(44), was aimed specifically at expressing the difference of opinions and conclusions the Czech group had from those of Ingold and his coworkers, which had been laid out in great detail in the summary paper(23) mentioned earlier. The Czech view was that the major driving force for rearrangement was the cleavage of the N-N bond and the resonance (delocalization) energy of the fragments. It

was irrelevant to consider the way in which the N-N bond cleaved (that is, homo- or heterolytically) because complexation via overlapping π-orbitals accompanied cleavage. The essence of the Czech view, then, was in opposition to the PTS theory, first in its view of two-proton catalysis and next in favor of one calling for some intermediate complex. It was not that the group had compelling new evidence to support an intermediate-complex theory, but that it was, for them, the better way of understanding the rearrangements.

Here, then, a sort of plateau was reached in the years 1964-1966, in which the choice of PTS versus intermediate-complex theories became a matter of opinion. Dewar and Marchand put it aptly in 1965(42) in saying that kinetic evidence could throw no light on the nature of intermediates in the reactions.

Now, the PTS and π-complex theories had one thing in common, namely that each had one view and one only of all of the rearrangements. In the PTS case, all rearrangements were one-step in type; that is, concerted in today's terminology. In contrast, the π-complex theory showed all rearrangements as being two-step in type; that is, a π-complex was formed in a rate-determining step and, after the appropriate rotational adjustment, collapsed in a fast step to product.

Distinction between these two possible pathways could not be made with any of the great body of experimental evidence that had so far been assembled. It was made only later with work on heavy-atom kinetic isotope effects (KIE) at Texas Tech. I can finish this account, then, by returning to where benzidine rearrangements and I parted company in 1949.

During 1949-1951 I had a postdoctoral fellowship with Carl Niemann at Caltech. Failing to get a teaching position, I joined the research laboratories of the U. S. Rubber Co., in Passaic, NJ, in late 1951, and spent three happy years there, until the fall, 1954. The urge to "do my own thing" took me to the small and unknown Texas Technological College (now Texas Tech University) in September, 1954. The head of the (then) Chemistry and Chemical Engineering Department, Joe Dennis, had decided views on building a research and graduate department. It was not a publish-or-perish department and there was no such thing as tenure (that came later, and I received tenure by "grandfathering"); but there was a moral obligation to begin a research program. One of my first efforts in this direction was to study the oxygen oxidation of alkenes in acetic anhydride that I had discovered at U. S. Rubber(45). The reaction encompassed a Criegee rearrangement and that led me, at Texas Tech, to study the decomposition of acetyl peroxide in

cycloalkenes. Out of that came the discovery that, as unstable as it may be, the acetoxy radical could be trapped by an alkene(46). At the same time I returned to the benzidine rearrangement and Jacobson's wonderful summary(4). In that summary Jacobson listed other agents, beside mineral acid, that cause benzidine rearrangements; and I was struck by his reference of the, until then, sole observation of a rearrangement caused by heating with base. This was the work of Meisemheimer and Witte, who had rearranged 2,2'-hydrazonaphthalene by heating it in alcoholic sodium hydroxide(47). A search of the literature showed, however, that Krolik and Lukashevich had pinpointed the rearrangement as being thermal and not in need of the base(48). I decided then to begin studying the mechanism of that rearrangement. My notebooks show that I began the work on February 4, 1955, first with hydrazobenzene, whose study was fruitless, and next on March 3 with 2,2'-hydrazonaphthalene in ethanol. The first-order kinetics were soon uncovered and led to further study with a series of solvents(49). Thereafter the work was taken over by my first graduate students, John Trisler and Robert Snell(50). It was never my intention to return to acid-catalyzed rearrangements, because I was now becoming enmeshed in a new field for me, radical ions and esr spectroscopy. Yet, when papers began coming out from Ingold's group and the battle lines between them and Dewar were being drawn, I was attracted to join the fray. I did not begin a systematic study in the sense of the UCL one. At that time, I felt more in tune with the π-complex than the PTS theory, but began checking out the cases that were crucial to arguments for the both of them, but which had never been investigated properly. In time, for example, it was found by Tommy Chamness with 4,4'-di-*tert*-butylhydrazobenzene(51) and by Jim Stanley with 4,4'-hydrazobiphenyl(52) that these bulky hydrazo compounds rearranged and by second-order acid catalysis, contrary to what Dewar was saying about them and their consequent role in the π-complex theory.

Insofar as the PTS theory was concerned, we took up at Texas Tech the only class of rearrangements, the *p*-semidine, that had been deliberately excluded from consideration by Banthorpe, Hughes, and Ingold(53). We thought it odd that the *p*-semidines had not been examined at UCL. The reason for the exclusion was their acceptance(53) of an earlier, faulty proposal by Hammick and Munro(54), that heavy-metal ions were necessary to effect *p*-semidine rearrangements. The reason for this proposal was that it was thought, also erroneously, that all the rearrangements listed by Jacobson

had been carried out by starting with an azoarene and reducing it with metal and acid to the hydrazoarene, which then rearranged. Hammick and Munro tried to find whether *p*-semidine rearrangements could occur without following that procedure, but unfortunately chose a rather complicated case as the test, the rearrangement of *p*-ethoxyhydrazobenzene. The *p*-semidine product of that rearrangement is so sensitive to oxidation that Hammick and Munro failed to isolate it and concluded, therefore, that it had not been formed, thus supporting the idea that a metal ion should have been used. But, Charles Baldwin and Harvey Harris showed at Texas Tech, that, if handled properly, the rearrangements of *p*-ethoxy- and *p*-methoxyhydrazobenzene took place in the absence of heavy-metal ions(55). Ingold and I corresponded about the *p*-semidine rearrangement. On May 22, 1967, I wrote about our work on the *p*-ethoxy- and *p*-methoxy cases at Texas Tech(56):

> If the *p*-semidine rearrangement turns out to be a valid intramolecular acid-catalyzed one, for which assistance by a metal ion is not needed, I would say that this rearrangement would best be interpreted as going through an intermediate other than the common place ones. I do not see how the breaking of the N-N bond and formation of the *p*-C-N bond could be concerted and so be represented with one transition state. Perhaps I am not being sufficiently imaginative over this. The *p*-semidine rearrangement (if intramolecular, etc.) would be similar to the quinamine rearrangement, and I have the same feeling about requiring an intermediate in that one(57).

Ingold replied on June 6(60):

> You were right to feel that we cannot get rid of the para-semidine problem as easily as Hammick and Munro wanted to. However, I find no great difficulty in picturing a transition state with an N-to-para polar bond of about 3A long. There would be plenty of strength in such a bond and direction is no problem.

Ingold's letter included tables of rate data, including those for *p*-methoxy-and some other *p*-substituted hydrazobenzenes. Nevertheless, I was disappointed in the UCL group for writing an updated history, with the *p*-semidine rearrangement as a chief new point(25), without acknowledging Baldwin's and Harris's work, and for Banthorpe and Cooper's publishing the *p*-methoxy work as the first recorded one-proton conversion of a *p*-semidine(61).

Very little more of consequence was done at Texas Tech with acid-catalyzed rearrangements in those years. Work was carried out on photo-benzidine rearrangements, however, since that fitted in more with my inter-

ests in radical ions and esr(62). Thus, acid-catalyzed rearrangements were abandoned at Texas Tech in the late 1960s. I felt then that nothing new could be done in solving the benzidine mechanism until someone came along with an entirely new approach. It was certainly not in my thoughts that it would come along at Texas Tech. But, that is what happened and also in a more-or-less accidental way. I met Prof. C. A. Bunton(63) by chance at a meeting of the American Chemical Society and we fell into a discussion of benzidine rearrangements. Bunton had continued his interest in them since leaving England but not in a major way(64). Our discussion turned to one's not being certain that N-N bond breaking was rate-limiting, and that measuring the nitrogen KIE would give the answer. At that time I did not know how to go about measuring a nitrogen KIE, so Bunton suggested that I solicit the help of Prof. A. N. Bourns at McMaster University, a recognized expert in measuring KIE with isotope-ratio mass spectrometric data. That suggestion bore fruit, and eventually with the help of one of Prof. Bourns' former co-workers, Dr. Peter Schmid, the nitrogen KIE was at last obtained(65). When the nitrogen KIE result was published I heard from an old friend, Prof. Harold Kwart, who pointed out that he could measure heavy atom KIE without needing to use isotope-ratio mass spectrometry, but, instead, by using whole molecule-ion quadrupole mass spectrometry(66). We undertook to do that with the rearrangement of hydrazobenzene, but this time to do it properly; that is, on the separated products, benzidine and diphenyline. At about the same time, I solicited the help of another old friend from Gordon Conference days, Dr. Clair Collins, an expert in measuring ^{14}C KIE, at Oak Ridge National Laboratories(69). Our collaborative results were, to me, astonishing. They showed that benzidine formation was a concerted process whereas diphenyline formation was not(70,71). It may seem odd for me to be saying that I was astonished, because as soon as the results were obtained we could see that is exactly what they should have been. Benzidine formation is a [5,5]-sigmatropic shift, with which the KIE were consistent. That being so, the diphenyline formation could not be concerted because it is a [3,5]-sigmatropic shift, forbidden by orbital-symmetry constraints from being concerted, unless antarafacially.

The hydrazobenzene results led us at Texas Tech to go further with *o*- and *p*-semidine and *o*-benzidine rearrangements (in the naphthalene series), with thermal rearrangements, and with the disproportionation reaction. Successful characterization of pertinent examples encouraged us to extend the mass spectrometric and scin-

tillation techniques to solving questions about other rearrangements, such as the Claisen and quinamine, and to continue now with Diels-Alder reactions. Thus, the question that an undergraduate asked over 40 years ago, and which affected my research life so dramatically, can now be answered more-or-less reasonably.

It is amusing now to look back and wonder how all of us, who were the major workers in the benzidine rearrangements, could have missed the connection between them and the blossoming of ideas in pericyclic reactions. Perhaps we were too far in the woods to see the trees.

The first papers by Woodward and Hoffmann on orbital-symmetry control of pericyclic reactions appeared in 1965(72). At about that time Ingold's interest in benzidine rearrangements was diminishing, and it may be understandable that he did not see or comment on their relationship to sigmatropic shifts. It would be interesting to know why others in the field failed to see or comment, too. For my own part, I can believe only that I was not sufficiently alert. The first workers to suggest that benzidine formation may be a [5,5]-sigmatropic shift were, in fact, not benzidine practitioners at all, but those very active indeed in Claisen rearrangements. From H. Schmid's laboratory in Switzerland came a succession of truly beautiful papers on these rearrangements, crammed full of splendidly worked out examples. Schmid's group applied frontier molecular orbital treatment to those rearrangements and noted that benzidine formation might be included among [5,5]-sigmatropic shifts in charged systems(73). Schmid went no further than that. A more extensive recognition that benzidine formation was an allowed [5,5]-sigmatropic shift, whereas other products come from rearrangements that are formally forbidden, is to be found in the 1971 book by Alder, Baker, and Brown(74). I was not aware of this perceptive writing until almost 20 years later and can only admit to another lapse in alertness. It is apparent that Alder and his colleagues must have written about the rearrangements some time before 1971(75). In a brief review of the rearrangements(77) I described Schmid's view(73) but even then did not appreciate fully that the answer to the benzidine rearrangements lay in it.

A summary of the KIE results for the rearrangements of hydrazobenzene, 4-methoxyhydrazobenzene (an allowed *p*-semidine rearrangement), 2,2'-dimethoxyhydrazobenzene (a benzidine rearrangement), 2,2'-hydrazonaphthalene, N-2-naphthyl-N'-phenylhydrazine (each *o*-benzidine rearrangements), 4,4'-dichlorohydrazobenzene (an *o*- and a

p-semidine rearrangement with loss of Cl), and the disproportionation of 4,4'-diiodohydrazobenzene has been given earlier and need not be repeated here(21). After that summary, however, the nitrogen and carbon KIE for the rearrangement of hydrazobenzene were re-measured and are given here in Table I. The motive for remeasuring these KIE lay in our intention of measuring KIE for rearrangements of $[1-^{14}C]$- and $[1,1'-^{13}C_2]$hydrazobenzene and our wanting to have for comparison contemporary measurements for $[4-^{14}C]$- and $[4,4'-^{13}C_2]$labelling. We recognized that the earlier KIE, determined on benzidine and diphenyline themselves(70,71), might be quantitatively wrong.

The 1,1'-positions of hydrazobenzene are not directly involved in bond breaking and forming, so that ordinarily one would not make these positions candidates for KIE measurements. We had found, however, that labelling the C-1 and C-ß positions of allyl phenyl ether, also not formally involved in bonding, gave rise to significant KIE in its rearrangement to 2-allylphenol(78). The KIE arise from the motion of these atoms in the six-atom array of a [3,3]-sigmatropic shift. We felt, therefore, that analogous, coupled motion of the 1,1'-positions of rearranging hydrazobenzene might be discernable with KIE measurements. Our data for both benzidine and diphenyline formation are given.

Table I. KIE for Rearrangement of Hydrazobenzene (**1**) to Benzidine (**2**) and Diphenyline (**3**)(79)

Isotope, Position	KIE[a]	
	2	**3**
$1,1'-^{13}C_2$[b]	1.0035 ± 0.0010	1.0000 ± 0.0026
$1-^{15}C$	1.0051 ± 0.0017	0.9988 ± 0.0017
$4,4'-^{13}C_2$[b]	1.0127 ± 0.0011	1.0121 ± 0.0008
$4-^{15}C$	1.0121 ± 0.0008	1.0010 ± 0.0010
$^{15}N,^{15}N'$[b]	1.0410 ± 0.0009	1.0367 ± 0.0009

a) All measurements were made on the bis(trifluoroacetyl) derivatives.
b) For two atoms.

The results in Table I from labelling at the 1,1'-positions show KIE for forming **2** but none for forming **3**. That is, the coupled motion of the 1,1'carbon atoms, anticipated for the ten-atom array in a [5,5]-pericyclic reaction, is detectable. In contrast coupled motion for diphenyline formation cannot be detected. The KIE at the 1- and 1,1'-positions for **2** are very small and not precise. By this I mean that the logarithmic ratio (*eq.* 4)

$$\ln(k^{14}C/k^{12}C) \; \ln(k^{13}C/k^{12}C) \, 1.9 \qquad (4)$$

expected for ^{14}C and ^{13}C KIE (single atoms) is not met by our results. The precision of our measurements, for

^{13}C probably, was, we think, not good enough. Yet, the motion we sought was found. The results for labelling at the *para* carbon atoms confirmed our earlier finding, that formation of **2** is concerted while formation of **3** is not. The new measurements for the para carbons do fit eq. 4, whereas our earlier ones with **2** itself did not(71). It is notable that the nitrogen KIE (Table I) are not much different from each other. They tell us that there is not much difference in the breaking of the N-N bond in the transition states for the two products, but for a reason still unknown, 16% (the yield of **3**) of the molecules do not follow a pericyclic path. Nevertheless, they do not enter an intermolecular path either, and perhaps, after all, a π-complex prevails for this pathway. Thus, here we may have the pathways of both earlier theorists, the concerted (Ingold) for **2** and π-complex (Dewar) for **3**.

We now know, then, that the benzidine rearrangements follow the dictates of sigmatropic reactions. This still does not lift Ingold's cloud completely from obscuring the transition state(s). We do not know the geometry of the TS in concerted rearrangement, and we do not know what maintains intramolecularity in non-concerted ones. Perhaps this is as far as we shall get—until someone comes along with a newer approach.

One of the puzzles of benzidine formation is that benzidine and not 2,2'-diaminobiphenyl (**6**, an *o*-benzidine) is formed. Formation of **6** is an allowed reaction, a [3,3]-sigmatropic shift. That type shift occurs in the rearrangement of hydrazonaphthalenes, both under acid catalysis and by heating(67,80). At one time it was suggested, briefly, that benzidine was formed by a succession of migrations, first to the *ortho* and next to the *para* positions(81). That suggestion was made when the so-called "cartwheel" mechanism of the nitramine rearrangement was popular. Had the double migration, now recognized as double [3,3]-shifts, prevailed, it would have been remarkable that it could compete almost exclusively with aromatization by loss of protons leading to **6**. Later, my colleagues and I were able to exclude

(6)
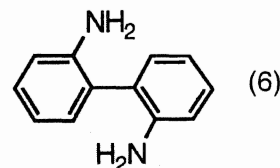
the double migration by finding no KIE for forming **2** from $[2,2'6,6'-^{13}C_4]$**1**(84). We do not really know why **6** is not formed, although I have suggested earlier that either the bent cyclohexadienyl shape of the rings in the diprotonated TS does not allow for the close-enough

approach of the *ortho* carbons, or that charge and elec-
tron distributions, by analogy with spin distributions in
the aniline cation radical, call for preferred bonding at
the *para* positions(71). If the latter is correct, it will
correspond with Ingold's PTS.

I will end this personal account by returning to van
Loon and my own most recent and perhaps last experi-
mental work on the benzidine rearrangement. In the
many separations of **2** and **3** that accompanied measur-
ing the KIE, Dr. Subotkowski perforce had the ratios of
2/3. These were uniformally 84:16(79), rather than the
70:30 so often quoted(85). In his first report, with Lobry
De Bruyn (see Ref. 6), and in his full paper(9), van Loon
recorded that in 0.1 N HCl and 50% ethanol 84% of
benzidine was formed. He did not report the amount of
diphenyline, that being too difficult to obtain. I take my
hat off to van Loon who was 50 years ahead of us in
kinetics and 90 years in yields. The same hat I doff to C.
K. Ingold and M. J. S. Dewar, whose originality in ideas
set them apart from all other practitioners and goaded
us into action.

ACKNOWLEDGMENTS

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      about his work on the rearrangement of hydrazobenzene,
      communicated by Prof. C. A. Lobry De Bruyn on No-
      vember 29, 1902, as mentioned by Holleman in the sec-
      ond communication. The earlier communication reports
      the yield of benzidine obtained (as the sulfate) under
      various conditions. In 0.1 N HCl it was 84%, a result of
      interest to us in our work in 1992 (Ref. 79). Further, van
      Loon freed his hydrazobenzene from remaining
      azobenzene by reduction with zinc dust and ammonia, a
      procedure used still. In Lobry De Bruyn's communica-
      tion it is reported, too, that the rate of rearrangement
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      gin with monoprotonation but include others showing
      diprotonation. It is not clear whether Robinson knew that
      two protons were required initially in hydrazobenzene's
      rearrangement.
16.   Ref. 11, p. 220.
17.   Ref. 4, p. 121: "Further insight – particularly regarding
      the way acids cause the rearrangement – should be an-
      ticipated primarily from kinetic studies of the rearrange-
      ment, which I must forgo because of lack of time and
      training. For the time being (Ref. 2), the only completed
      effort in this direction is by van Loon (Ref. 3), from
      Holleman's laboratory; from it the conclusion is drawn

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that their ionization determines the velocity with which various acids cause the rearrangement of hydrazobenzene." Jacobson's Ref. 2 is to an announcement, by Stieglitz and Curme, Jr. in *Berichte* **1913**, *46*, 912, of other rate measurements . Reference 3 is to van Loon in *Rec. Trav. Chim.* **1904**, *23*, 62 and to Lapworth in *J. Chem. Soc.* **1908**, *93*, 2188. Lapworth's work is a brief qualitative description that the speed at which benzidine sulfate precipitates from a solution of hydrazobenzene in ethanol, acidified with a little sulfuric acid, is inversely proportional to the amount of added water. Ref. 2 is a report of first-order kinetics in the thermal decomposition of hydrazobenzene in ethanol into azobenzene and aniline. Stieglitz and Curme refer briefly to van Loon's work and to their intention to submit later their recent measurements of rates in the acid-catalyzed benzidine rearrangement.

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28.   Banthorpe's switch to biosynthetic work became complete some years later. In response to my enquiring for some additional information about the preparation of D$_3$PO$_2$ in the UCL work, he replied in a letter of February 20, 1979, that he had disposed of his laboratory notebooks and gave me helpful information from memory. A clean sweep had been made, evidently.

29.   An indication of his diminishing interest in benzidines may be inferred, also, from his visit to Texas Tech on May 25, 1964, from Vanderbilt, where he was a visitor for several months under the NSF Senior Foreign Scientist Fellowship Program. He wrote, on March 19, 1964, that "in order not to bring coals to Newcastle I think I had better lecture on something else than benzidine rearrangements (which we can still talk about privately). I am warmed up just now on organo-metallic substitutions and could keep going for an hour on that." He lectured on "Substitutions on Organo-metallic Compounds." In 1964 the air journey required changing planes at Love Field. Ingold had a four-hour lay-over and spent the time in the comfortable mezzanine lounge available then, writing on the next edition of his book.

30.   Ingold was a remote figure to me, and maybe to other undergraduates, at that time (1942-1944). I met him and Hughes at the beginning of my two years at the interview all new undergraduates had to have with them before embarking on course work. I blotted my copy book by showing up late, after all interviews were finished, having gone walking on the sea front to kill time. Ingold was testy with me about that. I cannot remember talking to him again except in some group meetings that were held when the students and faculty tried, but failed, to make an arrangement to hold classes during the long summer vacation. Ingold would walk along the long promenade at Aberystwyth but be oblivious to passing students. Hughes had more personal contact with undergraduates, it seems, especially during laboratory periods. Ingold taught, of course, and very well. In contrast, Hughes was a terrible lecturer, reading from his meticulous notes and continuing his reading each period from where he ended the time before.

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knowledged having heard about the work at Texas Tech.
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"Benzidine Rearrangements. XIII. The Role of
Reductive Scission. Reactions of N,N'-
Dimethylhydrazobenzenes in Acid Solutions," *J. Org.
Chem.*, **1975**, *40*, 703-710.

63. Clifford A. Bunton was a lecturer at UCL when I was an
undergraduate, in Aberystwyth, so that my acquaintance
with him went back many years.

64. C. A. Bunton and R. J. Rubin, "Benzidine Rearrange-
ment in the Presence and Absence of Micelles. Evidence
for Rate-Limiting Proton Transfer," *J. Am. Chem. Soc.*,
1976, *98*, 4236-4246.

65. H. J. Shine, G. N. Henderson, A. Cu, and P. Schmid,
"Benzidine Rearrangements. 14. The Nitrogen Kinetic
Isotope Effect in the Acid-Catalyzed Rearrangement of
Hydrazobenzene," *J. Am. Chem. Soc.* **1977**, *99*,
3719-3723. The final answer took a long time to come.

On October 4, 1974, Bunton wrote: "I will be very in-
terested to know how your ^{15}N isotope work comes out.
Our mechanistic ideas would be very well clarified if
we knew with a degree of certainty that N-N bond break-
ing was rate limiting." A letter of mine to Bunton on
November 12, 1975, says, "We have our samples sealed,
waiting for mass spectrometry." The samples at that time
were, in fact, the mixed hydrochlorides of benzidine
and diphenyline. We did not think far enough ahead to
separate the products, wanting to know only if we could
find a nitrogen KIE for their formation. Thus, the an-
swer we obtained was really for the disappearance of
hydrazobenzene. At that time, Peter Schmid was, in fact,
a postdoctoral worker with Dr. Keith Ingold in the Na-
tional Research Council laboratories in Ottawa. Ingold
graciously agreed that Schmid could return to McMaster
for a month to make our measurements.

66. I had known Harold Kwart since 1951 when I worked
for, and he was a consultant for, U. S. Rubber. Our fami-
lies became friends and we would meet periodically, par-
ticularly at Gordon Conferences. Nevertheless, I had not
kept up with Kwart's work well enough to know what
he had been doing with mass spectrometry and KIE. He
had, in fact, pioneered the technique of measuring iso-
topic ratios with the selected ion monitoring (SIM) mode
of a quadrupole mass spectrometer. He worked with a
very old Hewlett Packard instrument that belonged to
the Chemical Engineering Department at the University
of Delaware. My impression, on seeing it, was that
Kwart's group was keeping it going with "baling wire"
repairs. The first nitrogen KIE on separated benzidine
and diphenyline were measured with that instrument. At
the time we, in Lubbock, did not know enough about
the technique and its demands on sample character to
send Kwart anything but benzidine (a solid) and
diphenyline (an oil), themselves. Therefore, it was very
difficult to handle diphenyline, something we were bliss-
fully unconcerned about in Lubbock. As we learned,
when we repeated the work ourselves years later in Lub-
bock with our own quadrupole mass spectrometer, the
early results were correct in finding KIE but quantita-
tively wrong. My collaboration with Harold Kwart con-
tinued until his mass spectrometer became inoperable.
At the same time, Kwart became increasingly ill with
stomach cancer. We continued our contacts, neverthe-
less, even during his final days in the Sloan-Kettering
Hospital in New York. We spoke almost daily by tele-
phone, always about chemistry, and, on learning that I
was planning on trying to measure KIE in photochemi-
cal reactions, his last conversation with me was to ad-
vise me to read up on the Russian work on magnetic
isotope effects. Kwart died on March 31, 1983. When
Kwart's mass spectrometer became unusable, he recom-
mended that I seek the help of Prof. Joe San Filippo, Jr.,
then at Rutgers University. San Filippo had one of the
best HP instruments and had it programmed to handle
SIM data beautifully. We collaborated for a few years

until he begged off because the type of samples we were sending, bis(trifluoroacetyl) derivatives of amines, were deadly to his electron multipliers. Our being cut off from a mass spectrometer finally enabled us to get our own, with Texas Tech University funds, not the high-class model that San Filippo had, but a lesser one that could do the job, however. San Filippo kindly gave us his computer program for handling the thousands of SIM scans. When we updated the computer facility for the mass spectrometer and found that it worked on Pascal rather than HP Basic, we found two hot-shot undergraduates in the Computer Science Department to make the translation for us. Thus, after some years of wandering in the mass spectrometry deserts we were settled in our own promised land. Some further forays into measuring KIE with isotope ratio mass spectrometry were also made. Initially, in 1985-1986, (that is, some years after our work with Peter Schmid in Canada), we had to use a commercial laboratory, Kreuger-Geochron, for the measurements. These were successful for nitrogen KIE, but not ^{13}C KIE, in the rearrangement of N-2-naphthyl-N'-phenylhydrazine(67). The cost of commercial analyses was high, and, although Kreuger-Geochron generously dropped its price for us to $45/sample, we had to abandon them. I learned a lot about the technique from Mr. Harold Kreuger by letter and 'phone discussions. Fortunately, Texas Tech's Department of Geosciences acquired a state-of-the-art VG-SIRA 12 isotope ratio mass spectrometer, which Dr. Yilmaz learned to use, and in that way obtained nitrogen KIE in a Smiles rearrangement with naturally-abundant starting material (68)

67. (a) H. J. Shine, L. Kupczyk-Subotkowska, and W. Subotkowski, "Heavy Atom Kinetic Isotope Effects in the Acid-Catalyzed Rearrangement of N-2-Naphthyl-N'-phenylhydrazine. Rearrangement is Shown to be a Concerted Process," *J. Am. Chem. Soc.*, **1985**, *107*, 6674-6678. (b) *Ibid.*, **1987**, *109*, 1286; a correction.

68. I. Yilmaz and H. J. Shine, "Heavy-Atom Kinetic Isotope Effects in the Base-Catalyzed Smiles Rearrangement of N-Methyl-2-(4-nitrophenoxy)ethanamine," *Gazz. Chim. Ital.*, **1989**, *119*, 603-607.

69. Again, because of our inexperience we sent Collins samples of benzidine and diphenyline, and again the answers were qualitatively right but quantitatively wrong. Collins thrilled us with his phone call about the first benzidine measurements: "Henry, you have a carbon KIE." Following the collaboration with Collins we turned to using scintillation counting. Texas Tech did, in fact, have a counter. Clair Collins died July 27, 1988.

70. H. J. Shine, H. Zmuda, K. H. Park, H. Kwart, A. G. Horgan, C. Collins, and B. Maxwell, "Mechanism of the Benzidine Rearrangement. Kinetic Isotope Effects and Transition States. Evidence for Concerted Rearrangement," *J. Am. Chem. Soc.*, **1981**, *103*, 955-956.

71. H. J. Shine, H. Zmuda, K. H. Park, H. Kwart, A. G. Horgan, and M. Brechbiel, "Benzidine Rearrangements.

16. The Use of Heavy-Atom Kinetic Isotope Effects in Solving the Mechanism of the Acid-Catalyzed Rearrangement of Hydrazobenzene. The Concerted Pathway to Benzidine and the Nonconcerted Pathway to Diphenyline," *J. Am. Chem. Soc.*, **1982**, *104*, 2501-2509.

72. R. B. Woodward and R. Hoffmann, "Selection Rules for Sigmatropic Reactions," *J. Am. Chem. Soc.*, **1965**, *87*, 2511-2513.

73. Gy. Fräter and H. Schmid, "Thermische Umwandlung von Penta-2,4-dienyl-phenyläthern in 4-(Penta-2,4-dienyl)-phenole; [5s,5s]-sigmatropische Umlagerungen," *Helv. Chim. Acta*, **1970**, *53*, 269-290. The possibility that the quinamine rearrangement is a sigmatropic one is also pointed out in this paper. See Ref. 59. 74. R. W. Alder, R. Baker, and J. M. Brown, *Mechanism in Organic Chemistry*, Wiley-Interscience, London, 1971, pp. 273-274.

75. Dr. Alder told me(76), in fact, that he included this view of benzidine rearrangements in his University lectures for some years before the book was written.

76. Meeting of Dr. R. W. Alder and H. J. Shine, University of Bristol, July 19, 1989.

77. H. J. Shine, "Aromatic Rearrangements," in *MTP Intl. Rev. Sci., Org. Chem.*, Ser. I, H. Zollinger, Ed., **1973**, *3*, 65-103; see p. 84.

78. L. Kupczyk-Subotkowska, W. Subotkowski, W. H. Saunders, Jr., and H. J. Shine, "Claisen Rearrangement of Allyl Phenyl Ether. 1-^{14}C and ß-^{14}C Kinetic Isotope Effects. A Clearer View of the Transition Structure," *J. Am. Chem. Soc.*, **1992**, *114*, 3441-3445.

79. W. Subotkowski, L. Kupczyk-Subotkowska, and H. J. Shine, "The Benzidine and Diphenyline Rearrangements Revisited. 1-^{14}C and 1,1-^{13}C$_2$ Kinetic Isotope Effects, Transition State Differences, and Coupled Motion in a 10-Atom Sigmatropic Rearrangement," *J. Am. Chem. Soc.*, **1993**, *115*, 5073-5076.

80. H. J. Shine, E. Gruszecka, W. Subotkowski, M. Brownawell, and J. San Filippo, Jr., "Heavy-Atom Kinetic Isotope Effects in the Acid-Catalyzed and Thermal Rearrangements of 2,2'-Hydrazonaphthalene. Transition-State Differences in their Concerted Rearrangements," *J. Am. Chem. Soc.*, **1985**, *107*, 3218-3223.

81. S. Brownstein, C. A. Bunton, and E. D. Hughes, "The Intramolecular Rearrangement of Phenylnitramine and the Benzidine and Semidine Changes," *Chem. Ind. (London)*, **1956**, 981. The intervention of stepwise migrations was part of the earliest speculations on mechanisms of benzidine rearrangements, but not, of course, in the sense of the double migration described here. That is, as mentioned earlier in the discussion of Robinson's work, the *p*-semidine was thought by Chattaway(82) to precede benzidine formation. Analogously, Chattaway proposed that the *o*-semidine might also be an intermediate step on the way to *o*-benzidine, although, he noted, that product had hitherto escaped detection. Fry(83) also made an early speculation that either the *o*- or *p*-semidine

rearrangement preceded benzidine formation. It is interesting that each of these speculations was made as a by-analogy addendum, to a discussion of another piece of chemistry, the diazoaminobenzene rearrangement in Chattaway's case and the Crum-Brown and Gibson Rule in Fry's.

82. F. D. Chattaway, "The Transformation of Diazoamido-into Aminoazocompounds and of Hydrazobenzene into Benzidine," *Proc. Chem. Soc. (London)*, **1902**, *18*, 175-177.

83. H. S. Fry, "Die Konstitution des Benzols vom Standpunkte des Korpuskular-atomistischen Begriffs der positiven und negativen Wertigkeit. I. Eine Interpretation der Regel von Crum Brown und Gibson," *Z. Physikal. Chem.*, **1911**, *76*, 385-397.

84. L. Kupczyk-Subotkowska, H. J. Shine, W. Subotkowski, and J. Zygumnt, "Bond-Forming Carbon Kinetic Isotope Effects in Benzidine Rearrangements," *Gazz. Chim. Ital.*, **1987**, *117*, 513-516. This paper is dedicated to the memory of Prof. Gabriello Illuminati (1922-1986), whom I met first in 1948 when we were both postdoctoral fellows with Prof. H. Gilman at Iowa State College.

85. The ratio of yields 79:15 is also reported by Vecera for rearrangement in 50% ethanol 0.1 M in HCl at 20. C (Ref. 86, p. 1996).

86. M. Vecera, L. Synek, and V. Sterba, "Über die Umlagerung Aromatischer Hydrazoverbindungen IV. Untersuchung der Sauren Katalyse und des Einflusses des Mediums und der Temperatur bei der Umlagerung von Hydrazobenzol," *Coll. Czech. Chem. Commun.*, **1960**, *25*, 1992-2004.

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Henry J. Shine is a Paul Whitfield Horn Professor of Chemistry at Texas Tech University, Lubbock, TX 79400. He retired from full-time activities in January, 1996, but continues teaching and research with a modified-service appointment. His research interests are in free radical and ion radical reactions, and in using heavy atom kinetic isotope effects in solving mechanisms of organic reactions. He is the author of "Aromatic Rearrangements."

Sir Christopher, returning from Buckingham Palace

BOOK NOTES

American Chemists and Chemical Engineers, Vol. 2. W. D. Miles and R. F. Gould. Gould Books, Guilford, CT, 1994. X + 297 pp. $20.

This inexpensive volume of short biographies of American chemists continues where the first volume (published in 1976 by the American Chemical Society) left off. Included in this volume are 258 chemists and chemical engineers, 252 being men and 9 women, representing both industry and academia. There are also some biographies of people who were not chemists or chemical engineers but made contributions in science or industry. Those who have died since the appearance of Volume 1, such as Vincent du Vigneaud, Louis Fieser, R. B. Woodward, Kasimir Fajans, and Ralph Oesper, make their appearance here. A short perusal through some of the biographies reveals many interesting facts. For example, Louis Fieser (1899-1977) served on the Surgeon General's 1964 committee on the health effects of smoking. Murray Raney (1885-1966) was a nonchemist who developed the "Raney" nickel catalyst in 1924 to hydrogenate cottonseed oil. Francis Garvan (1875-1937) was the lawyer who engineered the US takeover of the German patents on dyes after World War I and enabled the US chemical industry to become independent. Samuel Fisk Green (1822-1894) completed a vocabulary of chemical and physical terms in Tamil, the language of Sri Lanka(then Ceylon), and completed a translation of chemical texts into that language. All the biographies are written in simple, direct prose that describes the life and accomplishments of each chemist clearly and succinctly. The book itself is well bound with a detailed index, and each entry has a list of additional biographical references. In perusing the volume, I found only one minor fault. The criteria for inclusion listed in the preface are somewhat confusing, as if there were typographical errors that were not noticed. But this is a minor fault, since a simple overview of the biographies gives an indication of the criteria. Overall Gould and Miles have put together an excellent and extremely useful reference work in the history of chemistry and chemical engineering. *Peter J. Ramberg, Dept. Chem., Ohio University, Athens, OH 45701*

Thinking About Matter: Studies in the History of Chemical Philosophy, John Hedley Brooke. Variorium Ashgate Pub. Co., Brookfield, VT, 1995.

The "Collected Studies Series" of the Variorum Press has already made available several collections of the papers of noted historians of chemistry. The volumes of Maurice Crosland, Trevor H. Levere, and Allen G. Debus are now joined by one containing ten papers of John Hedley Brooke, who has a high reputation for his studies of 219th-century chemistry, especially in relation to religious and philosophical issues.

The papers reprinted in the Variorium volumes are reproduced exactly as they first appeared, without revision or even correction of misprints. All retain their original pagination to which the volume index is keyed. Since revision is not permitted, the author faces the challenge of selecting, arranging, and introducing a series of samples of his work that form a coherent sequence and give a rounded picture of the themes of his research. Brooke has achieved this quite successfully, though inevitably the collection remains less coherent than a monograph would be.

A short preface, written specifically for the volume is appropriately followed by "Chemists and their contexts: some recent trends in historiography" (originally published in 1991), which doubles as an introduction to the remainder of the articles. Brooke makes the case for his own approach to the history of chemistry as an alternative to the traditional emphasis on celebrating the precursors of recent achievements. He has been concerned instead to relate chemical research to the philosophical issues that have often arisen in connection with its, though his exposition is not specifically philosophical in idiom and he criticizes attempts to make history fit into a philosophical model of how science should be done. He is interested in discussions of such general issues as the nature of matter, the composition of mol-

ecules, and the differences between organic and inorganic substances. These are the central themes of the studies included here.

The second article in the collection is the only one not devoted to a 19th-century topic. "'the sower went forth': Joseph Priestley and the ministry of reform" (originally published in 1984 and revised in 1990) is an elegant discussion of Priestley's theological outlook as a framework for his scientific research. This is followed by "Davy's chemical outlook: the acid test" (1980), which subtly analyzes Humphry Davy's complex and varying views about acidity, arguing that he was not a consistent advocate of the theory that ascribed acidity to the presence of hydrogen. A similarly subtle analysis, alert to the ironies of a complex issue, is presented in "The superiority of nature's art? Vitalism, natural theology and the rise of organic chemistry" (1989). In this article, Brooke argues that vitalism and mechanism can in fact be rather difficult to discriminate, and that ambiguous theological implications can be drawn from each position.

In the following papers, on organic chemistry in the early 19th century, Brooke enters a series of further caveats against historical interpretations that he considers too simplistic. In "Wohler's urea and its vital force?—a verdict from the chemists" (1968), he persuasively debunks the mythology that has surrounded the artificial synthesis of urea in 1828. Brooke argues that, at the time, Wohler's synthesis was more noted for its implications for the problem of isomerism than for breaching the supposed barrier between inorganic and organic substances. "Berzelius, the dualistic hypothesis, and the rise of organic chemistry" (1992) is a comprehensive study of the role of analogies between the inorganic and organic realms in Berzelius's chemistry, especially focusing upon the theory of electrochemical dualism. Brooke proposes that this was a approach, albeit one that was soon superseded. "Laurent, Gerhardt,

and the philosophy of chemistry" (1975) deals with controversies as to how much could be deduced about molecular structure from the reactions of organic substances. "Organic synthesis and the unification of chemistry—a reappraisal" (1971) closes the circle of these studies by proposing that analogical arguments between organic and inorganic realms were more important in creating a common conceptual framework for the two branches of chemistry than were the achievements of organic synthesis.

The final two papers continue the theme of redressing naive expectations of episodes in the history of chemistry. "Avogadro's' hypothesis and its fate: a case-study in the failure of case-studies" (1981) argues against the supposition that Avogadro's hypothesis w2as unreasonably neglected by chemists for fifty years after it was advanced. Brooke argues that the meaning of the hypothesis was not exactly clear, and its potential utility for chemists at the time very limited. Finally, "Doing down the Frenchies: how much credit should Kekule have given?" (1993) takes issue with John Wotiz's claim that Kekule suppressed his degree of indebtedness to Laurent for the idea of the ring structure of benzene. Brooke shows that the structure was not given the same meaning by Laurent and by Kekule, and that the ethics of acknowledging priority cannot be decided unambiguously.

Each of these papers is fully worthy of reprinting, and study of the collection will richly reward anyone interested in the history of chemical theory in the early 19th century. Brooke argues his points with an elegant style and resourceful use of an extensive knowledge of the primary literature. Although the studies are independently conceived and remain quite distinct from one another, taken together they display a remarkably subtle and discriminating historical intelligence. *Jan Golinski, Department of History, University of New Hampshire, Durham, NH 03824.*
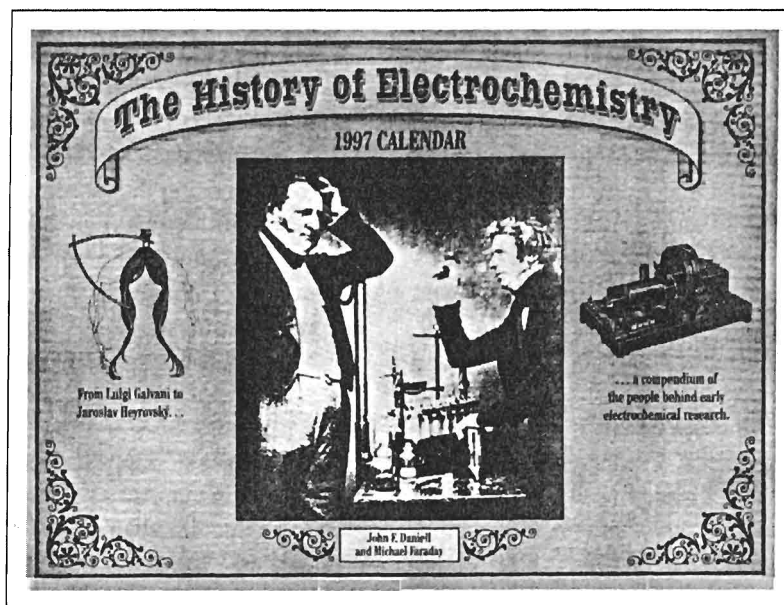
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