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CONTRASTS IN CHEMICAL STYLE: SIDGWICK AND EYRING

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In this address I will say something about two people I came into contact with early in my career, Nevil Vincent Sidgwick and Henry Eyring. They offer a striking contrast in every respect, and the contrast between them illustrates something of which I have become more and more convinced as I have worked on the history of science - that there is really no such thing as a scientific method. There are as many ways of doing good science as there are good scientists. Moreover, one can do little planning ahead in the case of a piece of scientific research; one must constantly make decisions - sometimes daily - as one proceeds with the work.

Let me make a few general comments first, before I come to Sidgwick and Eyring. Nonscientists, and indeed some scientists, often think that scientists are in some way a special breed of people. I have been lucky enough to know personally a considerable number of scientists, many of them extremely good ones, and I have read many biographies of scientists. My conclusion is that scientists are much the same as other competent people and that there are enormous differences between different scientists, even between those working in the same field.

For one thing, many good scientists would have been successful in anything they undertook to do. Quite a number of scientists did not originally intend to be scientists; J. J. Thomson (1856-1940), P. A. M. Dirac (1902-1984), and Henry Eyring, for example, originally wanted to be engineers; it is hard to believe that they would not have been good ones. Joseph Black (1728-1799) took a medical degree and practiced medicine during the same period that he lectured in chemistry. Thomas Young (1773-1829) and many others, particularly a number of chemists, also began their careers in the practice of medicine. Several scientists, like William Grove (1811-1896) and Joseph Plateau (1801-1883), became lawyers before becoming scientists; Grove, in fact, finally went back into law and became a judge.

Several scientists have won such great renown in fields other than science that they are better known for their other achievements than for their scientific work. An obvious example was the architect Sir Christopher Wren (1632-1723), who was a mathematician and a professor of astronomy at Oxford. There was also the physicist and statesman Benjamin Franklin (1706-1790) and the composer Alexander Borodin (1833-1887), who was a full-time professor of chemistry; for the most part he only composed when he did not feel well enough to do scientific work! Last but not least, there was Chaim Weizmann (1874-1952), who became the first President of the State of Israel, and who would probably not have been chosen for that position if he had not done, in Britain, some very important research in chemistry which contributed to the allied success in World War I.

Scientists, then, seem to be very much like other people who are interested in intellectual pursuits. In their general behaviour also, scientists seem just like other people. Some are generous, and the proportion of generous scientists is not obviously different from the proportion of generous people as a whole. A few scientists have been scoundrels, but again their proportion seems no greater than that in the general population Religious belief does not seem to be much affected by whether one is a scientist or not. Michael Faraday (1791-1865) was a Sandemanian, which means that he was a religious fundamentalist; one wonders, incidentally, how he would have taken to the theory of evolution. Henry Eyring was born a Mormon and rose to high office in that church. Some scientists, including Sidgwick, were agnostics; but their proportion seems about the same as that among other intellectuals.

Some scientists are highly gregarious, some are hermits, and most are somewhere in between. Most scientists are enthusiastic about discussing their ideas with others, but some fear that their ideas will be stolen by others and are secretive. Wilhelm Konrad von Röntgen(1845-1923), famous for his discovery of X-rays, is believed never to have discussed his scientific work with anyone. Oliver Heaviside (1850-1925), remembered today for the Heaviside layer in the ionosphere, retired at the age of 24 (perhaps a record for early retirement) and tried to avoid speaking to anyone during the rest of his life.

Now I come to the two men I am going to talk about, Sidgwick and Eyring. First I will say something about the differences in their personalities. Sidgwick was austere in manner and never married, while Eyring was friendly and gregarious, and loved his wife and family. Sidgwick was by no means easy to talk to, while Eyring was just the opposite. Sidgwick was an avowed atheist, while Eyring was a devout Mormon. What they did have in common was a devotion to science and a high regard for the truth. Both had a great effect on the progress of chemistry. Chemists today who may not know much about their work are greatly influenced by what Sidgwick and Eyring did, since it is reflected in the textbooks we use today.

The two men contrasted sharply in their ways of doing science. Sidgwick had little competence in mathematics and made little use of it in his work. All of Eyring's work, on the other hand, was of a mathematical character. Sidgwick was a great scholar of science, by which I mean that he studied the scientific literature with great care, and was thoroughly familiar with the experimental results that had been obtained in all branches of chemistry. Eyring, on the other hand, did not pay too much attention to what had been done before; he preferred to think about science in an intuitive way and seemed to pick up experimental facts (or get his graduate students to pick them up) as he needed them to test his theoretical ideas. Sidgwick based his work on mathematical treatments that had been worked out by others, and he had the knack of understanding their implications without going into all the details; he then collated a huge mass of experimental data on the basis of his interpretation of the theories. Eyring worked the other way round; he arrived at his ideas intuitively, then formulated his theories on the basis of rigorous mathematical treatments, and finally examined the way in which his formulations fitted the experimental results.

Nevil Vincent Sidgwick

Perhaps I may tell a personal story about how I first came in touch with Sidgwick (1, 2; Fig. 1). While at school in England in the early thirties I decided that I wanted to be a chemist; and since the man who taught me chemistry was an Oxford man he thought that Oxford was the best university for me. The system at Oxford is that one must first gain admission to a college, which automatically makes one a member of the University. I again took the advice of my teacher. The outstanding chemist at Oxford, he said, was Sidgwick, who was at Lincoln College; and in fact at school I had already read his famous *Electronic Theory of Valency*

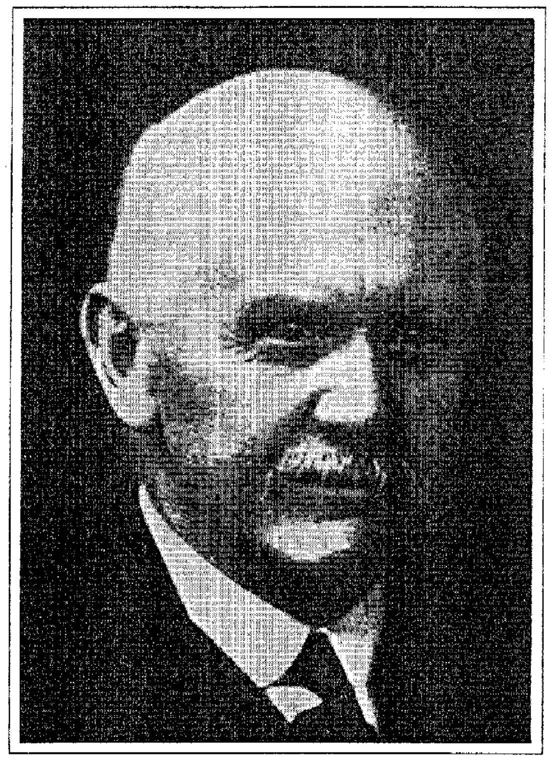

Figure 1. Nevil Vincent Sidgwick (1873-1952), from a photograph given by Sidgwick to the author in the 1940s, perhaps during World War II. He had looked much the same for several decades.

(Clarendon Press, Oxford, 1929). He added that there was also a younger man who was showing great promise - a man called Hinshelwood at Trinity College. I therefore put Lincoln College down as my first choice and Trinity as my second.

In December, 1933 I took the scholarship examination, and I still remember very vividly that when I was doing the experimental part of the examination Hinshelwood came beside me, and in his unforgettable drawl said, "Well, Laidler, how are you getting on?" I made some mumbling reply, now forgotten. A few days later I learned that I had been accepted by Hinshelwood, and as a result I went to Trinity College. There is an amusing sequel to this story, which I learned about only recently. I was in correspondence with Professor Brebis Bleaney, who became professor of experimental philosophy at Oxford in 1956 and has done distinguished work in electron spin resonance spectroscopy. It turned out that he, too, had given Lincoln College as his first choice at exactly the same time. We had both sat the same examination and had therefore been rivals, but he, too, had been turned down by Sidgwick and had been chosen by H. W. Thompson, the spectroscopist, for St. John's College. Thompson,incidentally, had been one of Hinshelwood's students and had perhaps learned from Hinshelwood the art of snatching people away from Sidgwick.

I still clearly remember, although it was over 60 years ago, sitting in front of Sidgwick at his lectures. This was at the time when he was working on his massive two-volume *Chemical Elements and their Compounds* (Clarendon Press, Oxford, 1950). When that book later came out, in 1950, I realized that my lecture notes taken fifteen years before were very much like a precis of the book. The lectures were quite superb; meticulously prepared, they were delivered with great style. At the end of each lecture he picked up his notes and walked out; in those days and in that University there was no opportunity for anyone to ask a lecturer a question. This sounds unsatisfactory, but we had tutors whom we saw for at least an hour once a week. Since my tutor was Hinshelwood, who also was a great chemical scholar as well as a man of great originality, I did not feel deprived as far as getting help was concerned.

Nevil Vincent Sidgwick was born in Oxford in 1873, of a rather remarkable family. His father had been a teacher of classics at Oxford and later a lecturer in politics and political economy. His uncles included Henry Sidgwick, professor of moral philosophy at Cambridge, Edward White Benson, who later became Archbishop of Canterbury, and Sir Benjamin Collins Brodie, who was Aldrichian professor of chemistry at Oxford from 1855 to 1872. The Archbishop must have been a little discomfited by the fact that Brodie, Henry Sidgwick, N. V. Sidgwick's father, and Sidgwick himself were all fairly militant atheists.

Sidgwick was a student at Christ Church, Oxford, where his tutor was A. G. Vernon Harcourt (1834-1919), one of the early pioneers in chemical kinetics. He gained first-class honors in natural science in 1895 but found that his rather classically minded relatives considered a degree in science to be much inferior to one in classics. Just to impress them he stayed on in Oxford for two more years and then gained first-class honors in Greats, which covers classical literature and philosophy in the original languages, Latin and Greek.

Sidgwick later went to Tübingen University, studying under von Pechmann, and in 1901 was awarded a D. Sc. degree *summa cum laude*, for work in organic chemistry. He was elected to a tutorial fellowship at Lincoln College, Oxford, and from 1901 the College was his home until he died in 1952; he never married. From 1901 to 1916 he carried out research, mainly on the physical properties of organic compounds, but did little work of any distinction until he was in his late forties. This late development in a scientist is unusual but not unique. Sir William Bragg (1862-1942) was also well in his forties before he did anything much in science, and then, with much help from his son Lawrence Bragg (1890-1971), he pioneered X-ray crystallography.

In 1916 Sidgwick moved to the new organic chemistry laboratories, which were directed by Sir William Henry Perkin, Jr. (1860-1929). The two could scarcely tolerate each other. Sidgwick had a deep interest in physical chemistry, which Perkin thought a waste of time; Sidgwick claimed that Perkin on several occasions said to him "Physical chemistry is all very well, but of course it doesn't apply to organic compounds." Since recorded organic compounds constitute over 99 per cent of the total number of chemical compounds, this was hardly an enthusiastic endorsement of physical chemistry.

Sidgwick's later successes followed a suggestion in 1914 from Ernest Rutherford (1871-1937) that he should relate chemical properties to the new electronic and quantum theories, something that had never been done before. Just a year previously Niels Bohr (1885-1962) had published his famous work on which he explained the orbital arrangements of electrons in atoms, work that he had carried out in Rutherford's laboratories in Manchester. At once Sidgwick began to consider how chemical properties could be explained on the basis of these ideas. In 1916 G. N. Lewis (1875-1946) published his famous paper on his octet theory and in subsequent years developed his ideas in many ways. Irving Langmuir (1881-1957) also made important contributions in this field, and since he was an excellent lecturer he did much to make chemists aware of these important new developments.

In 1919 Sidgwick applied for the Dr. Lee's Professorship at Oxford, but the appointment went instead to Frederick Soddy (1877-1965), who was to receive the 1921 Nobel Prize for Chemistry. The choice, though understandable at the time, turned out to be a poor one, as Sidgwick's teaching and later research would have made him a much better professor than Soddy, who did little research and gave indifferent lectures during his tenure of the chair. In 1922, when Sidgwick was forty-nine, he was elected a Fellow of the Royal Society, and in 1924 he was appointed University Reader in Chemistry. The title of Professor was conferred on him in 1935.

Sidgwick did not do anything highly original, but he followed the work of Lewis and Langmuir; his important contribution was to use it to explain chemical behavior. His detailed knowledge of the facts of chemistry put him into a unique position to apply the electronic theories to a wide range of chemical compounds. His work led to his book *The Electronic Theory of Valency* which appeared in 1927, when he was fifty-four. The book was soon recognized to be a scientific classic. In it Sidgwick skillfully and lucidly gave a fresh unity to the whole of chemistry, which for the most part had been presented as a large collection of isolated facts. This book had a wide influence. At once the textbooks of chemistry, even those used in high schools, began to change; even if they did not mention Sidgwick by name, they were influenced by his ideas.

In 1931 there came a great change in Sidgwick's life and attitude toward others. He was invited by Cornell University to be the George Fisher Baker Lecturer in Chemistry. This was to be his first visit to the United States, and with a prejudice that was rather typical of him he announced that he was 'sure he would not like the place.' Within a week of his arrival, however, he had completely reversed his opinion, afterwards taking every opportunity to return. On his first visit he was fifty-eight, a formidable figure, quite set in his ways. Oxford students had always been in awe of him, but the Cornell students saw him quite differently and were able to penetrate the crusty exterior, finding an amusing and kindly man underneath. They even called him 'Gran'pa,' which delighted him. They paid him the compliment of inviting him to stay at their fraternity, Telluride House, which he greatly appreciated and enjoyed. From then on he crossed the Atlantic whenever he could, becoming one of the best known British scientists in the United States; in the end he was proud to have visited 46 of the 48 continental states. (I myself, incidentally, have visited all 48 of them; the last one I got to, rather surprisingly, was Maine).

On Sidgwick's return from his first visit to Cornell, in 1932, his energies were mainly devoted to expanding and applying in much greater detail his previous formulation of the electronic theory. He labored for about twenty years on his great book, T*he Chemical Elements and their Compounds*; when it appeared in 1950 he was seventy-seven. It consisted of two massive volumes containing a total of about 750,000 words. It was written in a lively style and gave an astonishing and panoramic view of much of chemistry as it was at the time. This book also quickly became a classic. It is interesting, and rather unusual, that Sidgwick's reputation is based almost entirely on his books, and scarcely on his papers in research journals.

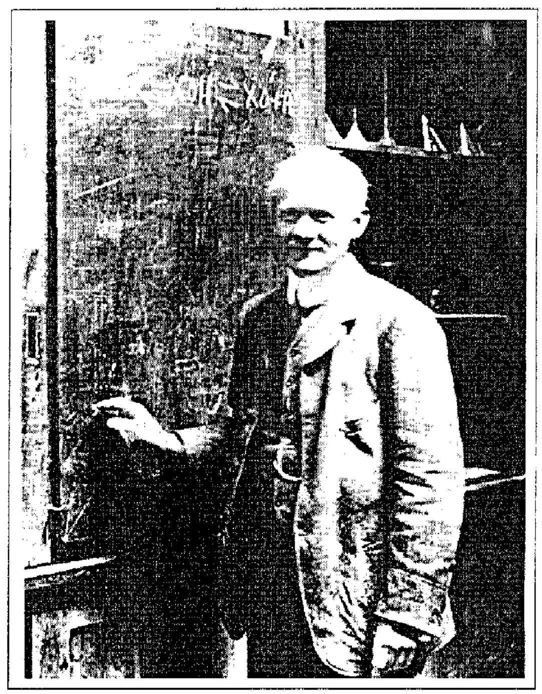

Figure 2. A photograph taken in 1910 of Sidgwick in the physical chemistry teaching laboratories at Balliol and Trinity Colleges, Oxford.

In appearance and personality Sidgwick was unusual. Figure 2 shows him as he was in 1910, and he looks rather elderly. At the time, however, he was only thirty-seven. When I first saw him twenty-four years later he looked almost exactly the same; only the depth of his collars had decreased. Indeed, forty years after that picture was taken he still looked much the same. He was always conventional in dress and invariably carried an umbrella; even in the hot California sun he would wear a felt hat, a thick English suit, and a raincoat. He cared very little about his surroundings, and his rooms in Lincoln College always looked shabby and untidy.

In his relationships with others he was very prejudiced, either completely approving or completely disapproving; in Leslie Sutton's words(2):

> In personal judgments he seemed sometimes to be carried away by the poetic ecstasy of imaginative denigration.

He would aggressively pounce on any loose or inaccurate statement and so made a few enemies; others became immune to being bitten. He made a particular point of being rude to clergymen. He was quite prepared to adjust his prejudices if confronted with adequate evidence as he did after his first visit to the United States.

In 1951, in failing health, he was determined to make what he knew must be his last visit to the United States. After undergoing an operation he returned to Telluride House at Cornell University, where the students helped him to go up and down stairs and took him for trips to see the autumn colors. He had a stroke on the ship returning to England and spent his final months in a nursing home, where he died peacefully on March 15, 1952. Throughout his adult life he insisted that he had no belief in God or in an after life.

Henry Eyring

I mentioned earlier that when I became an undergraduate, my first choice had been to go to Sidgwick's college (Lincoln), but that instead I became Hinshelwood's pupil at Trinity. A similar thing happened when I became a graduate student in 1938. Late in 1937 I applied for a fellowship which would allow me to go to an American university, asking Hinshelwood for advice. Hinshelwood was always in favor of broadening one's experience. Since I was then doing a year's undergraduate research in kinetics with him, his idea was that I should do my graduate work in another branch of chemistry. His advice was that I should give Linus Pauling (1901-1994) as my first choice, and this I did. I also had to give a second choice. My research with Hinshelwood had brought us into contact with what Henry Eyring had been doing at Princeton, particularly his formulation of transition-state theory in 1935. I had in fact myself been present at a seminar that Eyring had given at Oxford in 1937, a seminar that is still deeply engraved in my mind, because afterwards F. A. Lindemann (later Lord Cherwell), the professor of physics, was publicly extremely rude to Eyring, treating him like a stupid schoolboy who had forgotten his basic physics. I remember that afterwards Hinshelwood was extremely angry at Lindemann's behavior. I also remember that soon after I met Eyring he referred to what Lindemann had said, which he had naturally found very offensive.

Figure 3. Henry Eyring (1901-1981), from a photograph given by Eyring to the author in the 1950s.

Hinshelwood suggested to me that I should put Eyring down as my second choice. When I was interviewed for the fellowship the chairman of the committee told me that they had decided that I was successful, but mentioned that many Englishmen during the past few years had gone to work at the California Institute of Technology with Pauling; would I mind going instead to Princeton to work with Eyring, my second choice. Would I mind? Of course I was overjoyed. Thus, at a second crucial stage in my life I was given my second choice instead of my first, and I now think that this was fortunate for me. If I had been granted my first choices, Sidgwick and Pauling, my subsequent career would probably have been very different. Instead of working on kinetics, I should perhaps have concentrated on chemical structure, and I have a feeling that I might well have been a complete failure at it.

I mention these two incidents of my being given my second choice to emphasize that sheer luck does play a great role in all our lives. I have often speculated as to what would have happened to Michael Faraday, the son of an impoverished blacksmith living in the slums of London, if he had not got a job with a kindly bookbinder who encouraged him to read the books he bound, or if a kindly customer had not given him a ticket to go and hear one of Sir Humphry Davy's lectures. Faraday might well have lived in obscurity; at least he might have started his career much later. Similarly, what if Joseph Henry (1797-1878), living near Albany, New York, had not chased his pet rabbit under the village library, from there finding his way into the library, and into the world of books? Would he ever have become a distinguished scientist?

Shakespeare, as always, had something wise and interesting to say about that sort of thing (3):

> There is a tide in the affairs of men,
> Which, taken at the flood, leads on to fortune;
> Omitted, all the voyage of their life
> Is bound in shallows and in miseries.

I never made a fortune, but have been fortunate in my career, having been washed along by the tide, avoiding by sheer luck the shallows and miseries that a career sometimes leads to.

I worked with Henry Eyring (4-8; Figure 3) from 1938 to 1940. He was born in 1901 in Colonia Juarez, Mexico, of American parents. After studying mining engineering at the University of Arizona, he went to the University of California at Berkeley, obtaining a Ph. D. degree in physical chemistry under George Gibson in 1927. He taught for a period at the University of Wisconsin (9) and always enjoyed telling that he had been fired from the chemistry department there, as a result of a disagreement with the chairman, J. Howard Mathews (1881-1970). From all accounts Mathews was a difficult man with rigid and old fashioned ideas, and it is easy to see how he and Eyring could never have agreed. Eyring was required to conduct a laboratory course, and Mathews first ordered him to paint the floor, which even in those days was an unusual assignment for a member of the academic staff. Eyring complied, and was not pleased when Mathews, after inspecting the job, said that it was perhaps good enough for a first coat. Eyring later remarked to a colleague that the department would never amount to anything as long as it was run in the way it was. That remark got back to Mathews, and within an hour Eyring was fired. In those days, of course, one could not grieve – or rather, if one did, one did it alone.

Figure 4. Samuel Glasstone (1897-1986), from a photograph given by Glasstone to the author in the 1960s. After a distinguished career in physical chemistry, with several books to his credit, Glasstone became a nuclear engineer, working at the Los Alamos Scientific Laboratory until 1969 and then for the U. S. Atomic Energy Commission at Oak Ridge, Tennessee. He received several awards for his work on nuclear engineering and published books in that field also.

In 1929-30 Eyring spent a year in Berlin collaborating with Michael Polanyi on the construction of the first potential-energy surface for a chemical reaction. In 1931 he was appointed professor of chemistry at Princeton. He had discovered to his surprise that he was officially a Mexican citizen and became a naturalized American citizen in 1935. In 1946 he went to the University of Utah as Dean of Graduate Studies and professor of chemistry, remaining there until the end of his life.

When I arrived at Princeton to work with him in 1938 he had three years earlier made what was perhaps his most important contribution to science, the formulation of transition-state theory. The theory was still highly controversial, and his main interest at the time was to apply it to problems other than gas reactions. My work with him was first on reactions on surfaces, about which I knew a fair amount because of my work with Hinshelwood. We devised a way of dealing with the partition functions of surfaces and of surface layers and were able to show that the theory is quite satisfactory in interpreting the rates of surface reactions. We also looked at a number of reactions in solution, and they seemed to fit in also. After I had been at Princeton for a year Samuel Glasstone (Fig. 4), then in his early forties, came over from England and joined Eyring's research team. Glasstone was already well known for a number of very lucid books he had written on physical chemistry. (I still refer to them from time to time, as they are excellent on the basic concepts of thermodynamics, statistical mechanics, X-ray scattering, and so forth.) Glasstone also had made a name for himself in electrochemistry and had worked on overvoltage. Overvoltage was still something of a mystery, and Eyring, Glasstone, and I worked on the application of transition-state theory to it, with very successful results. At the same time, the three of us decided that the time was ripe for a book on transition state theory. Eyring, never much of a writer, left the actual writing to Glasstone and me; but he contributed enormously to it by his lengthy discussions of the subject matter and his penetrating criticisms of what we had written.

I remember very vividly one of the—always very friendly— arguments we had. Glasstone and I were on one side, Henry Eyring on the other. We broke off for lunch, and Glasstone and I had ours together. As we continued our discussion, we decided that Henry was right after all. When the three of us met again, we admitted to Eyring that *he* was right, but were rather taken aback when he told us that he had decided that *we* were right. The argument than continued, but with the opposing parties reversed, and soon we saw the funny side of it, and could not continue for laughing. Unfortunately, after so long, I cannot remember exactly what sides we were taking at the various times, although I do remember that it was a rather subtle point about the temperature-dependence of an equilibrium constant expressed with respect to concentrations rather than pressures. Needless to say, that problem is now one with which I have no difficulty; having an argument like that, with people like that, does straighten out one's thinking.

Our book, *The Theory of Rate Processes* (McGraw-Hill, New York), came out in 1941. Three of the chapters in it, on electrode processes, reactions on surfaces, and reactions in solution, comprised essentially my Ph. D. dissertation, submitted in the spring of 1940. I remember that after I took my oral examination, the examiners remained closeted together for such a long time that I felt sure that I had failed. When they came out and I was told that I had passed, I asked Henry why there was such a delay. "Oh," he said, "they weren't arguing about you; they were arguing about absolute rate theory" (as transition-state theory was then called.)

Eyring had a friendly disposition and was always happy to discuss his scientific work with anyone who would listen. He was always full of ideas, many of them wrong, but he always welcomed criticism; and his suggestions could always be turned into sound scientific treatments. In a formal sense, Eyring was not a good university lecturer. I have mentioned that Sidgwick was always well organized, but Eyring was just the opposite. He tended to go off on tangents, talking about something that had perhaps just occurred to him but which did not have much to do with the subject of his lecture. But his graduate classes at Princeton consisted of only a handful of students, and he did not mind at all if we interrupted him with a comment like, "Henry, we've no idea what you are talking about;" he would grin cheerfully and get back to his subject. In the end we all learned a very great deal from him. It had been realized, however, that he would be poor at teaching undergraduates, and I believe he never did so.

Science and its History

In expressing my great appreciation for receiving the Dexter Award, I should emphasize again the enormous role that good luck has played in my career. I got a wonderful start by having C. N. Hinshelwood (10-12; Fig. 5) as my tutor while I was an undergraduate, and I am sure I derived my interest in the history of science from him. Hinshelwood's work has in some quarters been underrated, and I should like to say a few words about that. In the 1920s and 1930s Hinshelwood did some very original work on explosions in gases and on reactions on surfaces and in solution. This work, in my opinion, was worthy of a Nobel Prize. His Nobel Prize, however,

Figure 5. Cyril Norman Hinshelwood (1897-1967), from a photograph taken by the author in 1961.

was not awarded to him until 1956, and by that time his work was of much less originality. Also, he had not kept up well with the latest advances. This was largely because he had many other responsibilities, such as running a large physical chemistry laboratory. When he won the Nobel Prize there was some criticism, because many people were only aware of his later work. I think, however, that if we consider his achievements as a whole and the influence he had on the growth of physical chemistry, the award was fully justified.

Like Sidgwick, Hinshelwood was very much a scholar in the field of chemistry, and he knew the subject through and through. I saw him for an hour or so every week for three years during term time, and we covered every aspect of chemistry. I remember doing with him such specialized topics as the organic chemistry of the anthocyanins. Hinshelwood had a deep knowledge of the history of science, and naturally a lot of that rubbed off on me. I had to write an essay for him every week on some chemical subject, chosen by him, and then read it to him. Today this ancient custom seems old-fashioned and amateurish, but I assure you that it was effective. He listened attentively, and any error of fact, syntax, or grammar was politely but firmly pointed out at the end; naturally one strove to make these criticisms unnecessary by very carefully checking what one had written. I still remember vividly, although it was sixty years ago, reading to him an essay on the decomposition of hydrocarbons and mentioning the work of W. A. Bone, who was then active in the field. I wrote that Bone had obtained certain results, which he had interpreted in terms of a free-radical mechanism. For once Hinshelwood broke his rule of not interrupting, and exclaimed, "What! Old Bone! Old Bone doesn't believe in free radicals." That short statement taught me two important lessons on writing the history of science. First, check your references properly, and second, do not assume that a scientist drew the conclusion that we today would expect. History must describe what happened, not what we think ought to have happened. I am sure that I derived my initial interest in writing about science and its history from Hinshelwood's influence. I also learned much about scientific writing as a result of my association with Samuel Glasstone in writing *The Theory of Rate Processes*. I feel remarkably fortunate to have been so closely associated with those two remarkable men.

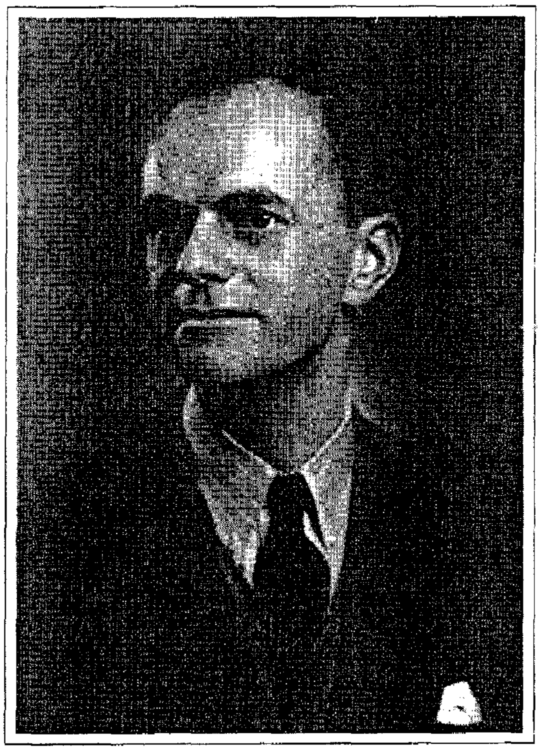

Figure 6. Keith Laidler (b. 1916), from a photograph taken in 1941, the year of publication of *The Theory of Rate Processes*. Photograph by Karsh of Ottawa.

In particular I learned from both Hinshelwood and Glasstone the most important precepts about writing, which were stated by Sir Peter Medawar and which I slightly modify as follows:

> Correctness, cogency, and clarity, these three:
> But the greatest of these is clarity.

Let me end with a brief comment on these three characteristics. Correctness, of course, speaks for itself; we must get everything right. There is much more to the truth than that, however; we can put forward perfectly correct information but end up with nothing but a big lie. That great historian Lord Macaulay made a very shrewd comment about this. He was concerned with the matter of selecting the appropriate material when one is writing history, and wrote(13):

> He who is deficient in the art of selection may, by showing nothing but the truth, produce all the effects of the greatest falsehood.

This is part of what is meant by the word cogency: we must *select* our material in such a way that the reader is left with a correct impression of the truth.

Finally, in writing about science the greatest of the virtues is clarity. It will be obvious that clarity is important, but we should be aware of some curious problems that may arise. Let me tell a little story, a true one. In my early days of teaching I was once told by a student that the students in my class understood my lectures very well. Then she spoiled everything by adding, "None of us can understand Professor X at all; but then, *he* is very brilliant." For a few seconds I was a little taken aback. Here was I, working hard to make my lectures clear, only to be regarded by the students as half-witted. I soon recovered and have continued to try to express myself as clearly as possible; but I am puzzled, and also a little concerned, by the fact that quite a few people seem to think that a person who speaks or writes obscurely must be very clever, something I know to be untrue. There are several books about science for the general public which I think are written very obscurely, which have nevertheless sold well. Do some members of the public say to themselves, "I can't understand a word of this book, so it must be a good one, and the author very clever?" The truth is that there is no correlation between obscurity and brilliance.

There is a great need for the public to know more about science, since science and its technical consequences enter so much into our lives. Writing about the history of science is one of the best ways of informing the public, and there is room for much more to be done.

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