

CYRIL N. HINSHELWOOD

## Chemical kinetics in the past few decades

*Nobel Lecture, December 11, 1956*

The task before me is to speak of the evolution during the last few decades of the branch of physical chemistry which treats of the intimate mechanism of chemical reactions. The nature of the present occasion seems to call for a survey taken from a rather more personal point of view than is normally desirable or proper. Since the Swedish Academy of Sciences has done me the great honour of bringing me to Stockholm to receive their high award, it is perhaps suitable that I should give some account of the lines of thought and of the accidents which have determined the course of the work in which, with others, I have taken part.

Nobody, I suppose, could devote many years to the study of chemical kinetics without being deeply conscious of the fascination of time and change: this is something that goes outside science into poetry; but science, subject to the rigid necessity of always seeking closer approximations to the truth, itself contains many poetical elements.

There are many ways in which the mysteries of chemical change might begin to impress themselves on a young chemist. I will mention one: it belongs to practical life and is a real example. Explosives have to be tested for stability and this is done by measuring their decomposition at a suitably high temperature. Phenomena of great interest appear, revealing a remarkably large influence of temperature, a great acceleration of the rate if the solid liquefies, and various striking catalytic effects. Many major problems present themselves, which can be roughly summed up as those involving the dependence of chemical change upon the energy and the environment of the molecules. It was obvious that these problems offered many mysterious and fascinating aspects. In fact they prove to contain more than enough for a lifetime's study.

In the early twenties one of the central questions in physical chemistry was that of unimolecular reactions, in which interest had been newly roused by the idea that such reactions were provoked by the absorption of radiation. A curious circumstance is that much theory was centred on the category of unimolecular reactions in the gas phase, of which in 1920 no authentic ex-

ample was actually known. The much discussed decomposition of phosphine proved to take place on the vessel walls, a fact linking up with the renewed interest in heterogeneous catalysis of gas reactions which the work of Langmuir on reactions with heated filaments had awakened. Against this background work in Oxford became directed to two ends. First, the discovery of new homogeneous reactions, and in particular those of single molecules, and secondly, the study of the factors which made the mechanism of heterogeneous catalysis often the preferred one.

The study of the heterogeneous reactions revealed one principle, which now seems trite enough, but was very stimulating in the days when molecular mechanisms of chemical changes were largely wrapped in mystery, namely that the affinity of the solid catalyst for part of the reacting molecule opens a reaction path with lower activation energy. Examples of homogeneous changes multiplied, and a new field opened up when thermal decompositions of organic vapours such as acetone, ether and acetaldehyde proved a rich source of unimolecular reactions.

Like the famous example of the nitrogen pentoxide dissociation, investigated by Daniels and Johnston in 1921, all these reactions eventually proved to have mechanisms of a much more complex character than was at first supposed. Two major findings have, however, stood the test of time. First, there was the unmistakable evidence that molecular collisions play the all-important role in communicating the activation energy to the molecules which are to be transformed and, secondly, there was the recognition of the activation energy (derivable from the Arrhenius law of temperature dependence) as a dominant, though by no means the sole factor determining reactivity in general.

These things helped to give shape and coherence to the whole subject of chemical kinetics. While the development of these themes was being pursued, a new one entered to add complexity to what was already complex enough, the theme namely of chain reactions.

The principle of chain propagation had been enunciated by Nernst to explain large departures of photochemical reactions from Einstein's rule, and the possibility of its intervention in thermal reactions had been suggested by Christiansen. The first clear evidence that chain processes play a major part in thermal reactions in the gaseous phase came from the investigation of the unexpected properties of such reactions as the formation of water from its elements, and the oxidation of hydrocarbons like ethylene. Not only were the relations between rate and concentration inexplicable in terms of simple

applications of the mass action law to non-chain processes, but the reactions showed a quite surprising dependence upon the size of the containing vessel, the presence of what should have been chemically inert gases, and the presence of sensitizers or inhibitors.

The reaction  $2\text{H}_2 + \text{O}_2 \rightarrow 2\text{H}_2\text{O}$ , from one point of view perhaps among the most elementary in chemistry, proved to offer a wealth of complex and intriguing behaviour -not all details of which are even yet fully understood. Victor Meyer had thought it an interesting reaction to study, but found it, apparently, intractable. Bodenstein showed that at certain temperatures the union took place on the walls of the vessel. The work in Oxford started from the simple idea that in the region of temperature between that used by Bodenstein and the inflammation temperature homogeneous processes must come into play.

The now well-known phenomena of the lower and upper explosion limits were observed. Semenov, then in Leningrad, had just explained the sharp transition from negligibly slow reaction to inflammation (a "lower limit") of phosphorus vapour by the theory of chain branching. It was soon clear that the lower limit with hydrogen and oxygen was of a similar nature. The branching was shown here to be controlled by the deactivation of chain carriers, at the vessel wall, and the "upper limit" was proved to depend upon the removal of chain carriers by three-body collisions in the gas phase.

The study of the hydrogen-oxygen reaction was the first point at which the work in Oxford came into close contact with that of Semenov. Our indebtedness to his ideas was at once recognized, and the early exchanges opened friendly relations between Semenov and myself which have lasted ever since. I should like at this point to mention also my personal debt to an earlier great pioneer of chemical kinetics, Max Bodenstein, who himself plays a part in the curious history of the hydrogen-oxygen reaction.

The phenomena involved in the initiation, propagation and termination of chains are now among the commonplaces of physical chemistry. Each process may be separately influenced by the conditions, so that the variety and complexity of the experimentally observable effects can no longer be wondered at. The detailed explanation of numerous examples continues to be of importance especially in connexion with the control of the polymerization reactions widely used in the modern plastics industry.

The importance of thermal decomposition reactions of organic molecules in connexion with the fundamental theory of chemical reactions has already been mentioned. Once chain processes were discovered it was obviously

necessary to know whether they were involved in these thermal decomposition processes. If so, the rather elaborate theoretical studies for which some of the results had been used would need considerable revision. A decision on this point was not quite simple. Work on the occurrence of free atoms and radicals, notably by R. W. Wood and Paneth, did indeed suggest the possibility of their intervention in such reactions as the ether decomposition, and Rice and Herzfeld formulated theoretical reaction schemes on this basis. On the other hand, tests for the actual presence of free radicals in the reacting systems which Patat and Sachsse made by the application of the o-p hydrogen method seemed at first to negate the idea.

Definite evidence was at length forthcoming when Staveley and Hinshelwood began to study the influence of nitric oxide on these reactions. The idea which initiated the work was simply that an odd electron molecule such as NO might well produce some interesting effects. Quite how interesting the effects would prove was not altogether foreseen. The pronounced inhibition caused by relatively minute quantities of this substance in a whole series of reactions which could conceivably involve the intervention of alkyl radicals left no doubt that the radical chain mechanism was in fact operative.

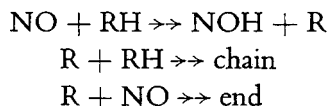
The working out in full detail of the mechanism of the large number of types of chain process now constitutes an extensive and elaborate chapter of chemistry which can absorb the attention of many workers for quite a long time to come.

The prevalence of chain mechanisms greatly complicates the problem of establishing a really complete and detailed theory of unimolecular reactions. Although a chain decomposition is often enough initiated by a unimolecular step, the observable kinetic relations are so much complicated by the varying possibilities for propagation and termination that definite conclusions about the details of the initiating process itself are very hard to arrive at. Some reactions, it is true, have been shown to be unimolecular without the participation of chains, but they are rare, and although the study of them is of great importance it deals with a small minority of the examples presented by Nature.

Great interest, therefore, attaches to the following problem. With increase in the amount of nitric oxide added to a system such as decomposing butane the rate of reaction falls not to zero but to a definite limit. This limiting rate has been widely regarded as that of a unimolecular non-chain reaction, that is to say, the dissociation of the butane into its stable products without the liberation of free radicals. If this view is correct the butane decomposition,

as well as a large number of other examples, can be used in the study of unimolecular reactions, provided that enough nitric oxide is added to suppress the chains.

But this view about the limiting rate is not without uncertainty. The alternative view is that the limit is determined by a steady state in which the nitric oxide starts as many chains as it stops



The arguments on the two sides are clear enough. That the limit corresponds to a molecular reaction is suggested by the facts

(a) inhibitors as different as nitric oxide, propylene and isobutene give just the same limit (though quite different amounts of them are needed to reach it) ;

(b) the addition of certain foreign gases such as sulphur hexafluoride and carbon tetrafluoride accelerates the "residual" reaction in presence of nitric oxide by precisely the same absolute amount as they accelerate the much more rapid reaction occurring in the absence of the inhibitor - a fact suggesting that the uninhibited chain reaction and the residual reaction are independent entities;

(c) the kinetics of the "residual" or "maximally inhibited" reaction show certain peculiarities which they share with other unimolecular reactions (notably the decomposition of nitrous oxide and the dissociation of di-*tert.*-butyl peroxide), known not to involve chains at all. The kinetic form would not be easy to interpret by the hypothesis of an NO-induced chain reaction - though it would be rash to assert that this could not be done with suitable ingenuity.

The argument on the other side is based chiefly upon the fact that deuterium exchange in such systems as  $\text{C}_2\text{H}_6$ - $\text{C}_2\text{D}_6$  proceeds to about the same proportional extent whether the main decomposition is damped down by nitric oxide or not.

Although Rice and Varnerin reached this conclusion Wall and Moore had found that the exchange was reduced by the addition of an amount of nitric oxide which was not sufficient to slow the decomposition to its limiting rate, and Stephen and Danby found that nitric oxide would in fact inhibit all the gas phase chain reaction in the simple system  $\text{D}_2 + \text{H}_2 \rightarrow\rightarrow 2\text{HD}$ .

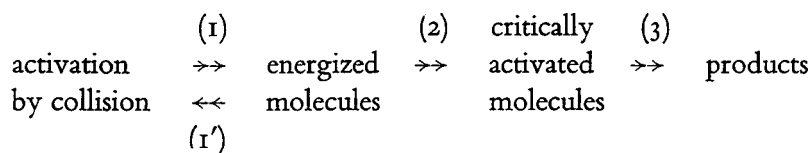
At present my own support is for the view that the residual reaction is molecular, and that the experiments on deuterium exchange have other explanations. This may, of course, prove to be erroneous. If so, Nature has played a curious trick in arranging the coincidence whereby such varied agents can both start and stop chains with efficiencies which remain in a constant ratio. If this coincidence really does occur, then there is also a very puzzling problem to be solved in explaining the kinetic relations actually found with the maximally inhibited reactions themselves.

It is a rather strange position to defend a certain view because the stronger arguments seem to be in its favour, and yet clearly to realize what arguments there are on the other side. Here, ironically enough, I may be wrong for maintaining that there is a non-chain component in some reactions which until the advent of the nitric oxide method could not be certainly proved to involve any chains at all. There is perhaps a moral in this to which I shall refer at the end. In the meantime all kinds of detailed experiments are still going on to resolve this problem.

Turning now to another question which has exercised attention from the very beginning, there are still puzzling and intriguing aspects about the value of the constant  $A$  in the equation which relates the rate constant of a unimolecular reaction to the activation energy

$$k = Ae^{-E/RT}$$

The processes by which the isolated molecule undergoes its first split, whether this is to be followed by a chain or not, seem to present a special interest both to theoretical and practical researchers. As we know, communication of energy by collision initiates a sequence which may be written as follows



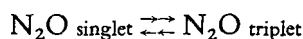
At low pressures  $I$  determines the rate. At higher pressures  $I$  and  $I'$  are nearly in equilibrium. According to the simplest theoretical picture of the process, evolved by Rice, Ransperger, Kassel and others and more recently greatly refined by Slater, the critical act in the whole sequence is 2 which

consists simply in the accumulation into one special bond of enough energy to rupture it, The mathematical analysis of this idea leads to the conclusion that  $A$  is of the order of magnitude of a molecular vibration frequency.

It sometimes is of this magnitude - a refreshing but not too common example of Nature's conforming to a simple pattern of behaviour. (Incidentally, a good many of the "inhibited" paraffin decompositions, whose questionable status as primary unimolecular processes has been mentioned, conform very well.

In other examples  $A$  exceeds any likely vibration frequency by several powers of ten. Here it would certainly seem that the simple picture of a critical concentration of energy into one bond is inadequate, and I believe that, as a general proposition, this conception must be widened. The whole idea of the energy relations in unimolecular reactions has had quite an eventful history. First, the idea that the activation energy could not be communicated by collision had to be disposed of. Next, until it was realized that energy could be, as it were, mobilized from numerous internal degrees of freedom (in the process indicated by 2 of the above scheme) there were misgivings about the adequacy of possible rates of energy transfer (and several writers even entertained the possibility that energy conservation did not apply in such processes). Then came the rather oversimplified view I have just mentioned, which is in course of being widened, so that we may now envisage many alternative states of energy distribution from which the final transformation may take place.

Another interesting set of problems has arisen out of the scheme of things set out above. Process 1 is certainly rate-determining at the lowest pressures, and process 2 takes over at higher pressures. Where one rate-determining process gives way to another the characteristic plot of the first order rate constant,  $k$ , against the pressure shows a change of slope. In several examples there are very strong indications that such transitions occur in more than one region of pressure. In other words, there are still further changes in the nature of the rate-determining process. The obvious interpretation is that process 2 involves further steps or even that process 3 may be collision-dependent. In the example of the decomposition of nitrous oxide there is a considerable probability that the extra process introduced into the sequence is that of



These matters are still subjects of research.

I must turn now to a quite different field of investigation, that namely of the relation between reactivity and structure. Here, perhaps, we may distinguish two main groups of problem. The first is one of a rather broad, indeed almost philosophical character. In the equation for a reaction rate constant

$$k = Ae^{-E/RT}$$

$$\text{or } \ln k = \ln A - E/RT$$

In  $k$  is the kinetic analogue of a free energy,  $E$  of a total energy and  $\ln A$  of an entropy.  $E$  does indeed represent the energy required to lift molecules to the transition state,  $A$  is concerned with the probability that all other requisites for reaction are fulfilled. Structural changes in reacting substances can affect both  $A$  and  $E$ . Sometimes, as is beautifully exemplified by the benzylation of a whole series of aniline derivatives containing different substituents, only  $E$  is affected. Sometimes, however, there is a deep inner correlation between the changes in  $E$  and the changes in  $\ln A$ , the completer understanding of which is now being sought, and it is to be hoped may reveal some more of the fundamental harmonies of Nature.

The other great groups of problems is that of the relation between changes in rate, whether expressed through  $E$ ,  $\ln A$  or both, and electron displacements within molecules, the theory of which has been largely developed by organic chemists, and only very partially interpreted in terms of the quantum mechanical theory of valency.

Perhaps as typical of the interesting relations in the structural field I may mention one or two examples. The oxidation of hydrocarbon vapours, a very typical chain reaction, offers a rather remarkable one. The oxidation rate for  $n$ -octane is many powers of ten greater than that of ethane, and detailed investigation shows the major influence governing the variations to be almost astonishing stabilising influence of methyl groups which strongly discourage attack at any point of a hydrocarbon chain adjacent to them. This is indeed an elegant problem for theoretical structural chemistry.

Here are two more such problems for modern valency theory which experiment presents. In a reaction involving a benzene derivative the rate is influenced by substituents in the ring, but to widely varying extents in different examples, and intriguing questions arise about the factors determining the ease of transmission of the substituent influence to the reaction-centre. The benzylation of aniline is slowed down hundreds of times by a nitro-group, while the dissociation of nitrobenzaldehyde bisulphite compound differs little in rate from that of the parent substance.



When several substituents are introduced into aniline their effects on the free energy of activation of the benzylation are almost strictly additive: in other examples the additivity is not so nearly so evident, offering once again an interesting exercise for the theoretical chemist.

Perhaps I am beginning to indicate too many questions and too few answers, but to do this at least re-creates the general atmosphere of this whole subject.

I am now going to turn briefly to a still more mysterious field, that of the living cell.

With a subject like chemical kinetics whose ramifications and details multiply without end, the individual worker is always confronted with the choice whether to repeat, perfect, amplify and consolidate, or whether to press forward into the completely unknown. In one respect at any rate a small group in Oxford has pursued the second course. The direction of this further exploration has been to seek an understanding of some of the kinetic phenomena of the living cell.

In some chemical systems, specially suitable for illustrating the elementary principles of kinetics, simple one-stage systems are met with, though less rarely than might have been hoped. In most of the systems, easily accessible to experiment reactions proceed by more or less complicated combinations of steps. In the living cell the multiplicity of reactions is not just an unfortunate complication, but the essential condition for the phenomena of life to be possible at all.

Suppose we liken individual chemical reactions to so many simple musical themes each played on a different instrument, then the operation of a living cell depends upon the combination of all these elements into a symphony. Knowing something of the theory of the simple elements can we find something about the rules of composition of the symphony?

To discuss properly the idea contained in this question would require not a small part of one lecture, but a whole series of lectures. All I can do is to indicate certain facts and observations the following up of which brings us into contact with the major question.

Bacterial cells (and some other unicellular organisms) grow in very simple media at defined rates. They synthesize, by long sequences of reactions, all the complex components of their structure. When the chemical environment is changed the growth rate alters. Gradually, however, by what is called an adaptive process, the cells undergo an internal reorganization in such a way that the rate, which may initially be very small, rises to a steady maximum.

The detailed study of this process gives interesting information about the way in which the chemical reactions are linked into what we term the reaction pattern of the cell.

Inhibitory substances, such as drugs, frequently impede growth very seriously, but gradually the cells appear to learn to overcome this inhibition. Detailed study of this process shows it to be capable of a kinetic explanation about which, needless to say, some biologists are sceptical but which helps to build up the picture of the cell's internal economy, and to show how much this is determined by kinetic principles.

There are many other problems which can be envisaged in terms of the reaction pattern of the cell. Cells contain numerous specific enzymes. The way in which these are built up as the cells multiply has some curious aspects, of which the explanation is certainly linked with the mode of coordination of the reaction pattern. When cells are provided with nutrient they grow and divide. The division seems to be determined by the moment at which a more or less critical amount of deoxyribose nucleic acid has been built up. Here is a fact relating kinetic phenomena closely to structural considerations.

I can not give you a full and coherent account of these things, but I mention them to show that there are plenty of mysteries left to explore, and also to suggest that kinetics can play a part in explaining some phenomena of that fascinating entity, the living cell. Even if I am wrong about this, it will have been worth the trouble to prove it.

One of my colleagues, Professor G. Temple, has drawn an interesting distinction between the "classical" and the "romantic" in scientific research. The former is exemplified by the demonstration of clear-cut and unquestionable mathematical propositions, or the devising and application of crucial logical tests of alternatives, the latter by the exploration of the unknown and the formulation of provisional hypotheses from incomplete data which need continual revision as the search proceeds. There is little doubt to which class the study of chemical kinetics belongs. Indeed some of us may well feel in retrospect like Dante

*Mi ritrovai per una selva oscura  
che la diritta via era smarrita.*

I found myself in a dark wood,  
where the straight way was lost.

And, indeed, I mentioned earlier an example of a situation where one or other of two quite respectable lines of argument must eventually prove to be in error. Is there a moral to be drawn? I am not sure, though we may perhaps console ourselves with the famous words of Lessing:

*Nicht die Wahrheit, in deren Besitz irgendein Mensch ist oder zu sein vermeinet, sondern die aufrichtige Mühe, die er angewandt hat, hinter die Wahrheit zu kommen, macht den Wert des Menschen.*

Or as an English writer has said "To travel hopefully is a better thing than to arrive".