NOEL BRYAN SLATER

All one's life is a music if one touches the notes rightly, and in tune

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1. Noel Bryan Slater was born in Blackburn, Lancashire, on 29 July, 1912. He was the son of D. Albert Slater, FRCO, and Minnie Jane Bryan; his father normally suppressed the fact that his first Christian name was " Doctor ", given to him in accordance with Lancashire tradition because he was the seventh son of a seventh son, and so supposedly possessed of special powers. I know of no other instance of this practice, and record it as a warning to genealogists.

Noel attended Blackburn Grammar School until his family moved to Scotland where he completed his education at Fordyce Academy and at the University of Edinburgh, and here his voice acquired a faintly perceptible Scottish lilt which added to its charm. He clearly benefited by and often commented with satisfaction upon the breadth of his Edinburgh studies, and these were attended by marked success; he was elected both Arnott Scholar in Experimental Physics, and Maclaren and Ferguson Scholar in Mathematics, and he was a medallist in Mathematics and Natural Philosophy. In 1933 he went to Cambridge as a scholar, and was George Green Student of Gonville and Caius College. He was a Wrangler, won the Mayhew Prize, and received the degree of Ph.D. for research supervised by R. H. Fowler.

In 1939 he was appointed to the post of Observer at the Solar Physics Observatory in Cambridge, but he was of course quickly swept up into the war, during which he served in the team of mathematicians formed by W. R. J. Cook (now Sir William Cook, KCB, FRS) which included F. J. Anscombe, M. S. Bartlett, F. E. Brown, T. V. Davies, F. M. Goodspeed, J. Howlett, C. W. Jones, A. Lees, P. A. P. Moran, J. G. Oldroyd, R. A. Rankin, and G. J. Whitrow, among many others. Professor L. Rosenhead, CBE, FRS, who later directed Slater's work during these years, writes of " his combination of mathematical expertise and physical insight, of the meticulous patience and persistence shown in his work, and of his choice of problems which showed a bold and adventurous mind ". What these problems were, is not a matter which can be discussed here, but the published indiscretions of others more highly placed [D2] make it clear that they involved what are now called rockets, but what were then, in a deliberate obfuscation, known as UPs (" unrotated projectiles "). Slater made the most of what some foolishly thought of as an exile in Wales. He led bathing expeditions to almost inaccessible pools on the rugged Pembrokeshire coast (one of these could only be reached by descending the cliff-face on a chain, no doubt long since rusted away); he played an active rôle in a Christian study group sparked off by the publication of *The Screwtape Letters* [D9]; most importantly he won the hearts of many of the inhabitants of a small Welsh borough by entering to the full

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NOEL BRYAN SLATER (1912–1973)

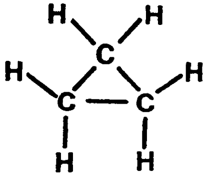
where the a_r's are amplitudes and the ε_r's are phases; it is the ω_r's which are supposed to be linearly independent. The natural " classical " view of the amplitude is that each degree of freedom in the molecule should have the same energy, and by the equipartition theory of classical statistics, this would be $kT/2$, where T is the temperature and k is Boltzmann's constant. The individual phases would be arbitrary because there is no reason to suppose that any two of these normal modes would have related phases: we may therefore take the ε_r's as " random ".

Slater worked on the hypothesis that the bond would break when the bond length L reached some critical value L_0. He therefore asked what would be the length of time from " a typical starting-point " to the first epoch at which $L = L_0$. This is " a straightforward but non-trivial mathematical problem ", for which see below. The mean-time-to-rupture so obtained could be related to a direct measure of the mean time to decay of such a molecule. Simple operations then give the rate of break-up of molecules of the same kind. These are the " unimolecular reaction rates ".

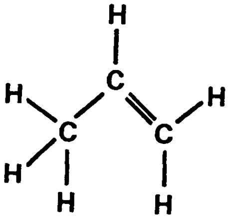
Some more detailed explanatory remarks are in order here. In the gaseous state, molecules of course collide, and one might think that as the collisions provide the energy leading to dissociation, so the gross rate at which dissociation proceeds should be proportional to the square of the concentration (i.e. to the rate of " meetings "). This is found to be true at low concentrations, but when the concentration is high one paradoxically obtains a first-order dissociation rate, that is, the dissociations occur at a gross rate proportional to c, rather than to c^2 (where c is the measure of concentration). An explanation of this phenomenon was first given by Lindemann (the " Prof " of **[D2]**), who pointed out that when c is large, collisions will occur so frequently that even when a molecule has acquired enough energy to enable it to dissociate, it may not retain this energy long enough for the " catastrophe " to occur, for almost immediately it will be " deactivated " by yet another collision. Lindemann thought of the " catastrophe " as some kind of " centrifugal bursting ", but the argument works equally well if (as is supposed here) it is the excessive stretching of a critical bond-length which constitutes the " catastrophe ". At high concentrations we will have an almost steady state, with molecules shunted up and down the energy ladder; the number of " energised " molecules present at any time will be nearly constant, and the gross rate of dissociation will be proportional to this number, that is, to c. Any individual " energised " molecule will, however, stay in that state very briefly, and normally not long enough for the " dissociation catastrophe " to occur. Moreover (and this is very important for the mathematical treatment) since even one " catastrophe " is unlikely between two collisions, the probability of any " catastrophe " at all occurring during this period can be equated to the *expected number* of " catastrophes " during the period in a modified system in which a " catastrophe " does not now lead to rupture, but allows motion to continue unperturbed.

Accordingly Slater's work applies (in the first instance) to the high-concentration situation, but in general a further condition has to be satisfied also. As he points out, when a pendulum is struck violently, the new motion starts off with a phase that is very nearly $\pi/2$, or $-\pi/2$. Similarly we must expect that the initial phases for an " energised " molecule will be atypical, and so (in this initial version of the theory) the concentration must not be so large that this atypicality of the initial phase completely dominates the behaviour during the time until the next collision. The requirement of linear independence (over the integers) for the frequencies ω is of course imposed precisely in order that, given sufficient time, the atypical initial phase-configuration will be destroyed. We complete these asides by observing that, in the notation adopted here, the molecule is " energised ", or " becomes interesting ", when $\sum a_r$ exceeds the critical bond-length, L_0.

Slater applied this theory to several such molecules; the best known was the cyclopropane C_3H_6,



and here it is the rate of " opening " of the carbon " ring " that is of interest; the rate will, of course, depend on the temperature. This is, however, an oversimplification, for when the ring ruptures, the new structure has the form



(each H has just one bond, and each C has four bonds); thus one of the hydrogen atoms has had to change its allegiance to a new carbon atom. It appeared that the critical distortion was in the relative position of this hydrogen atom, and Slater's detailed study shows that if this hydrogen atom were to move about 1.15 Ångstroms ($1\text{Å} = 10^{-8}$ cm) towards the " new " carbon with which it might become bonded, then the reaction would occur.

Rates of chemical reactions are usually written in the form

$$\text{rate} = A \exp(-\Delta E/(kT)),$$

where ΔE is the activation energy and A is the pre-exponential factor (a measure of frequency rather than energy). For his model Slater found that

$$A = 4.0 \times 10^{14},$$

in the usual units; this is to be compared with the experimental value

$$A = 15 \times 10^{14}.$$

In relation to the enormous difficulty in predicting any rates in this *ab initio* way, this is considered to be good agreement.

But as a final answer it won't really do. There is no reason to suppose that increasing the length of a bond to some critical value will inevitably cause rupture, because this will depend on where the other atoms are, and whether they act so as to push the wandering atom back. Also it is not satisfactory to suppose that the restoring forces are harmonic; one might say that it is precisely the anharmonic character of these forces which permits bond-breaking, and the assumption of a critical bond-length is one way of trying to bypass the real difficulties associated with such anharmonicity. Slater of course fully appreciated this.

There are other difficulties. If Slater's theory were correct then the energy term ΔE in the Arrhenius expression would be a natural measure of the actual energy needed to be given to the bond to get it to the critical length, and it now seems that this over-estimates the true value of ΔE.

It will be recognised that this is a purely mechanistic model, and here it differs from most of its competitors. One of the best known of these (the Eyring transition-state theory) suggests that we can separate out some " reaction co-ordinate " and assume that all the other co-ordinates retain their usual statistical distribution. The potential energy when expressed in terms of the reaction co-ordinate has a " hump ", corresponding to the energy necessary to carry the molecule over this hump (this is ΔE); also the pre-exponential factor is related to the speed with which a molecule approaches the hump along the reaction path. We may say that Slater looks at one molecule, and then averages, while Eyring averages statistically first.

It proved almost impossible to do two desirable things with the Slater model. The first was, to extend it to bi-molecular collisions, where more than one type of molecule is involved. The second was, to " quantise " it. The failure in the first case arises simply from the fact that when two molecules come together from a large distance apart then we cannot use the idea of a harmonic force in any reasonable way. The failure in the second case arises because the quantum theory of vibrating molecules deals with the so-called " stationary states ", and what we need is not a view of a stationary state, but rather of a state developing in time. Slater did attempt to resolve this difficulty (see below); with recently developed techniques it might be possible, but when Slater was busy with this problem these techniques did not exist at all.

We now turn to the " straightforward but non-trivial mathematical problem ", for an account of the origin of which I am indebted to Professor Mark Kac. When Kac first went from Poland to the United States he was interested, as a (then) pure mathematician, in some problems related to the theory of almost-periodic functions, and one such was the distribution of the values of a superposition of vibrations with linearly independent frequencies. This is obviously closely linked to Slater's problem, but in fact it arose in an astronomical context, namely the " mean motion problem " of celestial mechanics. In 1943 Kac published two papers [**D6**, **7**] giving a formula for the frequency (in the probability sense) with which such a superposition of vibrations with linearly independent frequencies would cross a prescribed level " from below ". To his surprise and pleasure he received a letter from Slater telling him that his formula, which Kac thought was " entirely useless ", had opened the door to a new attack on an important chemical problem. This was the commencement of a long friendship. Nearly the same problem has recently arisen in yet another (neolithic) astronomical context [**D8**].

There are still some matters to be cleared up in the surroundings of Slater's mathematical problem (in the treatment of which, he would himself have said, earlier work by P. Stein [**D11**] was also of great value). One is, what happens when the frequencies of vibration are not linearly independent, and in particular, when there are groups of vibrations of equal frequency. This interested Slater very much and led to further original investigations by him in the theory of diophantine approximation. Another concerns the possibility of exploiting the connexion between the Kac-Slater-Stein work on trigonometric sums, and the work of S. O. Rice and his successors on similar " exceedance problems " for stationary *gaussian* stochastic processes with a prescribed covariance-function (for which, see [**D3**]). As such random functions can always be represented as a sort of Fourier integral with respect to a random measure (the Cramér-Loève representation) the two problems are in essence both trigonometric, and both probabilistic, and yet they are not quite the same.

I have not always found it easy to translate Slater's problems into probabilistic terms, and as other readers may have like difficulties, the following remarks may be found helpful. Of his own papers, those most relevant here are [**B3**, **9**, and **23**]; also the appropriate chapters in [**A2**]. Let us write $f(t, \varepsilon)$ for the right-hand side of equation (1), where

$$\varepsilon = (\varepsilon_1, \varepsilon_2, \ldots, \varepsilon_n)$$

and $n = 3N-6$. We can assume that the ωs are all distinct and positive, and that the a_r's are all positive. The vector ε can be thought of as a point on the n-torus (the cartesian product of n copies of the circle); this is of course a compact connected metric abelian group. If we increase by t' the value of the time t, the effect on $f(t, \varepsilon)$ is the same as the action of a Lebesgue-measure-preserving transformation $S_{t'}$ on the n-torus, and so, if we give to the *initial* ε normed uniform (Lebesgue) measure, then in

$$\{f(t, \varepsilon) : 0 \leqslant t < \infty\}$$

we shall have a *strictly stationary* stochastic process, but of course *not* a gaussian one, for obviously we must at all times have

$$|f(t, \varepsilon)| \leqslant \sum a_r < \infty.$$

We remarked above that in the circumstances first envisaged by Slater, it is enough to work with $N(t, \varepsilon)$ (the total number of upsurges above the critical level L_0) when ε is the initial phase-vector, and t is the time to " de-energisation " (the distribution of which is known). One might think that each one of two quantities would be of interest. One is the *long-term time-average* (if it exists),

$$\lim_{t \to \infty} N(t, \varepsilon)/t \qquad (\varepsilon \text{ fixed}), \tag{2}$$

and the other is the *phase-average*

$$E\{N(t, \varepsilon)/t\} \qquad (t \text{ fixed}); \tag{3}$$

here $E\{\ldots\}$ denotes the operation of averaging with respect to ε, using Lebesgue measure (the invariant measure for the flow on the torus). An elementary but non-trivial argument (see Appendix—but the reader may like to try to supply this argument for himself) shows that the ratio $N(t, \varepsilon)/t$ is bounded above by a constant, that is, by a number independent of both t ($t \geqslant 1$) and ε. Also the strong law of large numbers (= the ergodic theorem) shows that the limit (2) does indeed exist for Lebesgue-almost-all ε, and that *it is independent of* ε *when the* ω*s are linearly independent.* Finally the stationarity shows that the phase-average (3) is always independent of t. The boundedness lemma mentioned above tells us (via the Lebesgue bounded convergence theorem) that

$$E\{\lim_{t \to \infty} N(t, \varepsilon)/t\} = \lim_{t \to \infty} E\{N(t, \varepsilon)/t\} = E\{N(1, \varepsilon)\}$$

whether we are in the ergodic (linearly independent) situation, or not; moreover in the ergodic situation this number will be the value of $\lim N(t, \varepsilon)/t$ for almost all ε. Thus $E\{N(1, .)\}$ is the vital quantity to calculate, and it is this which Kac (in the ergodic case) evaluated exactly in [D6]. It will be recalled that linear independence (and sufficient time) is needed for the effect of the atypical initial phase ε of an " interesting " molecule to " wear off " as time passes; we have already stressed this point, but mention it again to underline the way in which ergodicity (the absence of non-trivial and non-co-trivial invariant subsets) is chemically relevant.

Notice also that in Kac's evaluation of $E\{N(1, .)\}$ in the ergodic case, it is possible to regard the *individual* phases $\varepsilon_1, \varepsilon_2, \ldots, \varepsilon_n$ as *independently* distributed over the circle, so that his calculation in fact deals with a sum

$$\sum_{r=1}^{n} a_r X_r,$$

where the X's are independently and identically distributed like $\cos\theta$ (θ uniform). For n large, and suitable amplitudes a_r, we shall thus have asymptotically a gaussian situation, and Rice-type formulae should give results compatible in the limit with those of Kac. It ought also to be pointed out that exceedance-theory for stationary stochastic processes has long since outgrown its gaussian baby-clothes, and in fact there are many theorems in [D3] for which the gaussian assumption is not required, although unfortunately other restrictive assumptions normally take its place. What is exceptional about the Kac formula is that it holds for *all* values of n, and that it works even though the stochastic process has a discrete spectrum.

In later work Slater interested himself in the extension to the non-linearly-independent case, which is of importance when symmetry leads to degeneracy, and also in the low-concentration problem when the actual time from " energisation " to dissociation, i.e. the time σ to the first *up-surge*, is of importance. Obviously in an " exact " treatment this is the vital random variable. We can suppose that the time which elapses between the occurrence of a collision that makes a molecule " interesting ", and the next collision (which in general de-energises it sufficiently to make it " uninteresting "), has an exponential distribution with a parameter λ which is an increasing function of concentration. When we ask, " will the molecule dissociate during its interesting condition ", we are enquiring about a random event whose probability is

$$P = E\{e^{-\lambda\sigma}\};$$

here σ is the time needed for dissociation to occur, $e^{-\lambda\sigma}$ is the probability that the molecule will remain interesting throughout this period, and the final averaging indicated by $E\{...\}$ is to allow for random fluctuations in σ. By another ingenious use of the ergodic theorem for strictly stationary random processes Slater showed, in the important Chapter 9 of [A2], that

$$P = P_\infty (1-E\{e^{-\lambda\tau}\}). \tag{4}$$

Here τ differs in an important way from σ; the latter was the time from energisation until dissociation (supposing the interesting condition to last long enough), so depends upon the initial phase, whereas τ denotes *the time between one up-surge and the next* when an interesting molecule is protected from collisions which might de-energise it. This identity (4) involves the assumption that the initial phase is effectively uniformly distributed, and it is based on the long-term averages considered in the ergodic theorem, which are phase-independent because of the linear independence of the frequencies†. In another language, (4) is a theorem about strictly stationary " point-processes " which are " orderly ".

When the concentration is high, λ will be large, and P becomes P_∞; this corresponds to the earlier solution described above, and exclusively studied in Slater's

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† There is a conflict here with Slater's remark (quoted above from [B3]) about the pendulum. If the initial phases are " atypical " then so is σ, and (4) fails. It would be interesting to try concentrating the initial phase on $(\pm\pi/2, \pm\pi/2, .., \pm\pi/2)$, giving equal probability to each one of these 2^n possibilities. The use of the ergodic theorem has the appearance of " giving us something for nothing " precisely because it bypasses the particularities of the concrete special case. When using it, even for an ergodic system, we must always enquire whether the concealed price paid (the use of the invariant measure) is within our means.

earlier papers e.g. [**B3**]. But in (4) he now had the key to a study of the problem at arbitrary levels of concentration, and it is to this important question that much of his later work is devoted. Evidently (4) can be used if and only if we know enough about the distribution of the time τ between consecutive up-surges. In most of the existing work by statisticians (see e.g. [**D3**]) the process is required to be gaussian and to have a covariance function which decays to zero (neither condition holds in the chemical problem); also the " point-process " of up-surges is studied by them only in the limit, as the level-to-be-surged-over L_0 tends to infinity. Such limit theorems are irrelevant here, because in the chemical problem we have an absolute upper bound $\sum a_r$ above which the oscillations cannot take us, so the door to asymptotics is firmly closed in our faces. However, in most cases of interest the critical level L_0 will not be much less than $\sum a_r$, and then Slater noted that the up-surges across the level L_0 will occur just before the really high maxima (i.e. those nearly equal to $\sum a_r$).

Because of the ergodicity, we can study the statistics of the successive " gaps " between such high maxima (i.e. between up-surges) by choosing any convenient initial phase ε, and then investigating $f(.,\varepsilon)$; obviously the choice $\varepsilon = (0, 0, \ldots, 0)$ is the most convenient one. Now the high maxima of

$$f(t, 0) = \sum a_r \cos(\omega_r t)$$

will occur at those t-values at which *all* of

$$t\theta_1, t\theta_2, \ldots, t\theta_n$$

are near to integers, where $\theta_r = \omega_r/(2\pi)$. When $n = 2$ (the simplest non-trivial case) we can without loss of generality change the time-scale so that $\theta_1 = 1$, and then we find ourselves interested in those integers m for which the fractional part of $m\theta$ ($\theta = \theta_2$) lies in some specified small convex neighbourhood of zero. If these integers are $m_1, m_2, m_3, \ldots$, when arranged in increasing order, what we need to know is something about the differences (or " gaps ")

$$m_2 - m_1, m_3 - m_2, \quad m_4 - m_3, \ldots.$$

Specifically we need to know what gaps can occur, and how frequently each size of gap occurs. Of course the answers will depend on θ and on the small convex neighbourhood which we have specified.

Slater answered these questions in full by proving his impressive, and indeed rather startling, *Three-Gap Theorem*: there are never more than 3 different possible gaps, and they can be expressed in the form

$$\alpha, \beta, \alpha + \beta.$$

His theorem also tells us the asymptotic frequency of occurrence for each of the three gaps. For example, when $\theta = \sqrt{5}$, and when we ask for the fractional part of

$m\theta$ to lie in $(-0.1, +0.1)$, the first 200 gaps turn out to be

9	4	4	4	5	4	4	4	5	4	4	4	9	4	4	9	4	4	9	4
4	4	5	4	4	4	9	4	4	9	4	4	9	4	4	4	5	4	4	4
9	4	4	9	4	4	9	4	4	4	5	4	4	4	5	4	4	4	9	4
4	9	4	4	4	5	4	4	4	5	4	4	4	9	4	4	9	4	4	9
4	4	4	5	4	4	4	9	4	4	9	4	4	9	4	4	4	5	4	4
4	9	4	4	9	4	4	9	4	4	4	5	4	4	4	5	4	4	4	9
4	4	9	4	4	4	5	4	4	4	5	4	4	4	9	4	4	9	4	4
4	5	4	4	4	5	4	4	4	9	4	4	9	4	4	9	4	4	4	5
4	4	4	9	4	4	9	4	4	9	4	4	4	5	4	4	4	9	4	4
9	4	4	9	4	4	4	5	4	4	4	5	4	4	4	9	4	4	9	4

The Three-Gap Theorem settles the questions at issue when $n = 2$, but of course the molecules we want to study are more complicated than this, and when $n > 2$ the complete solution is not yet known. Slater however was able to show (i) that the number of possible gaps is always finite, and (ii) that in general there appears to be a subset of size n of the possible gaps, in terms of which all the others can be expressed linearly, with positive integer (or zero) coefficients. He also indicated how the asymptotic frequencies for the possible gaps might be found, and he made many numerical studies up to $n = 7$ (an example with 7 basic gaps and 36 combination-gaps). These studies confirmed (ii) so far as they went, and Slater found a partial proof of (ii) without, however, being able to establish the *positivity* of the representation in terms of the n basic gaps. Thus the n-Gap Theorem awaits complete proof. Slater's final comments on this problem are in **[B25]**, but **[A2, B5**, and **B23]** should also be consulted.

From such " gap " studies, and in particular from computations of the statistical moments of gap-length, Slater was led to suggest the use of a probability-density of the form

$$\tau e^{-\tau} \tag{5}$$

for τ, in suitable units. This, together with (4), then makes possible a concentration-dependent treatment of reaction rates.

4. Some of Slater's other interests have been touched upon in the above remarks, and these have not here been exhausted, as reference to the bibliography will show; in particular one should mention that at the time of his death a substantial joint paper with T. C. T. Kotiah had just been completed in the field of queuing theory (characteristically linking up with *Partitio Numerorum*) following an earlier note by these authors on a similar topic.

5. But these latter investigations were side issues when compared with the other great intellectual adventure of Slater's life. When Eddington died on 22 November, 1944, the controversial book *Fundamental Theory* **[D4]** existed in draft state only. A " final draft " was seen through the press by the generosity of Sir Edmund Whittaker, but this was only *one* draft, and there were many. Slater realised the importance of urgent action, and took steps to ensure that a veritable mountain of manuscript in Eddington's study should not be disturbed or so much as dusted until he had had time to go to Cambridge and sort it. When he got there he found odd bundles of sheets from numerous different drafts heaped up in a towering pile, sometimes one way up and sometimes the other, and with little to indicate their interrelationship. After the war Slater devoted himself to the separation of these into variant versions of sections of the book, representing different and competing strands in Eddington's thought, and finally, in his own book **[A1]**, Slater published a sort of *variorum* edition modelled quite consciously on the similar activities of New Testament scholars. No-one was sure about the scientific status of *Fundamental Theory* at that time, and one is even less sure now. What is important in the long term about Slater's own achievement is that it provides us with an authentic snapshot of the competing concepts simultaneously present and wrestling for expression in the mind of a great creative genius. There are not many comparable instances (if any) of this sort of thing. Far too often (Newton's *Opticks* is said to provide one example, and the papers of Mendel another) we are presented with the mock-reporting of experiments described as if they are exploratory, when in fact they have been framed either in the light of an earlier experimental programme not reported upon, or on a hypothesis which is then made to appear as if it had been deduced from the experiments. If this can happen in experimental work, it is all the more true in theoretical studies. The unsuccessful ideas are thrown away, and we are only told about the one which happens to give the right answer. Even in the context of Eddington's own work such questions have been raised by Sir Harold Jeffreys **[D5]**. But in the case of *Fundamental Theory*, we can now see for ourselves how such a speculator actually speculates. The thanks for this must go to Slater, not merely for his patient labour of love in disentangling things, but also for the Napoleonic stroke which made it possible.

A curious anecdote should perhaps be recorded here. In **[D4]** Eddington made a serious mathematical error in formula (3.8) of section 3; this was discovered by Slater after Eddington's death and noted in his 1947 review of **[D4]** in the *Philosophical Magazine*. The mistake was an unfortunate one, because it destroyed Eddington's *post factum* prediction of the rate of recession of the galaxies, observed at that time to be 560 km/sec/megaparsec. Eddington's value had been 572.4. After the error had been noticed, Eddington's value had to be reduced by multiplication by $4/9 = 0.444$. Six years later a mistake was also discovered in the *observations*; in fact measurement of the distance to the Magellanic Clouds by a new technique changed its value drastically, calling for an adjustment to the basic Cepheid procedure used to measure the distances of the galaxies, and so in effect doubling the scale of the

Universe overnight; this meant that the observational figure 560 had to be halved, bringing it into agreement with the (corrected) Eddington formula once again! Slater was quick to point this out [C3], but the incident, dramatic though it was, may have made him feel that a subject where theory and observation were both so little to be trusted was not deserving of much more than the enormous effort he had already put into it, and after the publication of his book **[A1]** he does not seem to have intervened publicly in the matter again. As time passed, more and more adjustments were made to the observational value for the recession, which was gradually eroded until today, according to some authorities, the galaxies crawl sluggishly outwards at a mere 50 km/sec/megaparsec. It may be that further adjustments could be made in the rather loosely articulated structure of *Fundamental Theory* so as to keep theory and observation in step " no matter what ", but, if so, Slater prudently eschewed such further tinkering, and he reminded the reader of [A1] that

> " Eddington's theory loses much of its awesome nature if it is regarded as *a* theory (not as *the* theory!) . . . ";

on that note we may take leave of this controversial topic.

It is of interest that there was another occasion on which Slater put his head into the lion's mouth, to see what of value might be found there. This excursion is surveyed in the last chapter of **[A2]**, where in " Quantum harmonic oscillator models " he attempts to reconcile reaction rate theory with quantum theory, but employs J. E. Moyal's statistical version **[D10]** of the latter, and the further extension of the same by Bartlett and Moyal **[D1]**. This is seen by some as involving a paradox, for the Moyal theory works with a joint statistical distribution for the coordinates q_i and momenta p_i, while in traditional quantum theory these are thought of as being in principle not comeasurable. Slater was not entirely satisfied with the results obtained in this way, but apart from the relevance of this work to the chemical issue, physicists may see it as of methodological interest, in that Moyal's approach is here put to work on a concrete problem.

6. I have already mentioned Slater's remarkable gifts as a musician, surely inherited ones (his father was an organist with an outstanding capacity for making and teaching music), but I have not done full justice to these. His powers were extraordinary, and it is said that he could play most, if not all, of Beethoven's piano sonatas and Bach's " 48 " from memory. Mary Rankin writes " A piano was a magnet to him and he often dropped in on people who were fortunate enough to possess one when he did not—and played regardless! He once got into trouble at a Welsh pub because he insisted on playing his kind of music, which was not the kind the hearty drinkers at the bar wanted to hear. He refused to stop, and was asked not to come again. Another (perhaps apocryphal) story concerns his own ' grand ', reputed to have cost £1. He left it in Cambridge during the war, and when he returned, found that it was in the garden, serving as a hen house."

Noel Slater was one of my own best and closest friends. From the day we met, which is easily fixed because it was on the occasion of E. A. Milne's presidential address to this Society, he was a tower of strength in times of trouble, and also a splendid person to share good fortune with, when it came. He would listen to and discuss with great insight any mathematical topic, or indeed any other topic of whatever provenance. He was a staunch Christian, and supported the faith of many. With his wife and three children, we his other friends mourn the loss of an irreplaceable part of ourselves.

While in his devoted analysis of Eddington's work we may see Boswellian traits, Slater in himself was far more of a Johnson, in temperament as well as build. When one reads the book about Eddington, his own personality merely hovers in the wings, but in *Unimolecular Reactions* he writes with a delightfully uninhibited gusto. As a young man he dreamed of enriching Statistical Mechanics, by adjoining to it a sister subject, Statistical Kinetics, and in that book, the writing of which gave him so much evident pleasure, he may be said to have gone far towards achieving his ambition. But it is more than a solemn study of a scientific problem. It is a revelation of the man himself, and it is perhaps the only book on chemistry which can be warmly recommended as bedside reading for a mathematician.

A cherished possession of mine is a copy of **[B9]**, carrying the manuscript dedication " Without rigour, Noel ". In this review of his work I have been less than rigorous in the treatment of many issues, but fortunately there is a safeguard; his book. Read it.

APPENDIX

" *This perhaps exasperating statement* . . . "

In **[A2]** Slater uses the above phrase with reference to the fact that " the phase-average frequency is also the phase average of the asymptotic frequency ", and he points out that the proof of this fact, as noted here in section 3, is a direct consequence of the uniform boundedness in t_1, t_2, and ε of the ratio

$$N(t_1, t_2; \varepsilon)/(t_2-t_1) \; (t_1+1 \leqslant t_2),$$

where $N(t_1, t_2; \varepsilon)$ is the number of zeros of

$$g(.,\varepsilon) = f(.,\varepsilon)-L_0$$

in the interval $t_1 \leqslant t \leqslant t_2$. But the uniform boundedness is not very easy to establish. Here is one method; it is slick, and while it lacks " purity of method ", people fortunately do not worship that quality so much as they were once used to do.

First note that, because a time shift merely results in a change of ε, and because $N(.\,,\,.)$ has obvious properties of finiteness, positivity, and additivity, it will suffice to prove that

$$M(\varepsilon) = N(0, 1; \varepsilon)$$

is uniformly bounded in ε. Suppose then that this is not so; it would follow that we could choose $\varepsilon^1, \varepsilon^2, \ldots$ to make $M(\varepsilon^j) > j$ for each positive integer j. Now the n-torus is compact metric, and so in a suitable subsequence (to which we restrict ourselves below) we shall have $\varepsilon^j \to \varepsilon^*$ on the n-torus.

Next consider $g(., \varepsilon)$ as an entire function of the *complex* variable t. For $\varepsilon = \varepsilon^*$ this will have at most finitely many zeros in any compact t-set, and so we can construct a simple polygon Π avoiding these zeros and looping once positively round the segment [0, 1]. The integral

$$J(\varepsilon) = \frac{1}{2\pi i} \int_{\Pi} g'(z, \varepsilon)/g(z, \varepsilon)\, dz$$

(where g' is the z-derivative of g) is equal to the sum of the zeros of $g(., \varepsilon)$ which lie inside Π, counted multiply, and so is not less than the actual number of such zeros. We need not worry about the risk that one of the zeros lies on Π, because we shall only consider $\varepsilon = \varepsilon^*$ and $\varepsilon = \varepsilon^j$ for suitably large j, and then all is well because $|g(., \varepsilon^*)|$ is bounded away from zero on the compact t-set Π, $g(., .)$ is continuous, and $\varepsilon^j \to \varepsilon^*$. We now have the desired contradiction because (i) $J(\varepsilon^j) \geqslant M(\varepsilon^j) > j$, (ii) $\varepsilon^j \to \varepsilon^*$, and (iii) $J(.)$ is continuous at $\varepsilon = \varepsilon^*$, and so (being integer-valued) is constant in some neighbourhood of ε^*.

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