1 Noise-activated escape from metastable states: an historical view

ROLF LANDAUER

Historical comments by a participant in the field represent a personal viewpoint. They are not a detached and scholarly contribution to the history of science. The field of this volume, and the (hopefully) closely related field of this discussion, involves a number of 'schools', of varying degrees of distinctness, and I cannot represent them all in this brief discussion. The *Brussels School* associated with Ilya Prigogine is one example. Another group, which is much more loosely knit and less clearly identifiable, can be associated with the work and influence of the late Elliott Montroll. This work comes closer to my viewpoint, represented below, but has had public visibility through recent books and conferences (Montroll and Lebowitz, 1979; Shlesinger and Weiss, 1985; Weiss, 1983, 1986). Both of the aforementioned 'schools' have a close relationship to physical chemistry, and we will, later in our discussion, return to some of the pioneering (but long neglected) insights which arose in this area. On the other hand, our field has equally deep, or perhaps deeper, roots in electronics. We will take it upon ourselves to represent that heritage. The approach we will stress describes some degrees of freedom and their dynamics explicitly; the remaining degrees of freedom are regarded as a source of noise and of damping. It is a viewpoint which has wide applicability, but is particularly directly and accurately applicable to electrical circuits. At the very beginning we have to stress the pioneering role of R. L. Stratonovich in the development of our subject. His two-volume treatise (Stratonovich, 1963, 1967) is a landmark. Undoubtedly, much of the contents of those two volumes appeared in the Soviet periodical literature long before then. (Some of these items will be cited subsequently.) In an editorial, some years ago (Landauer, 1981), I referred to a *Stratonovich test* for the integrity of citation practices in the field. Do authors acknowledge the relationship of their work to that of Stratonovich?

Unintentional repetition in science is unavoidable; we cannot spend all of our time in the library guarding against that. This author once repeated a seventeen-year-old piece of work, unknowingly. Nevertheless, the field of this volume seems to have had more than its share of rediscovery. Originally the overlap arose, presumably, because the investigators came from different backgrounds and disciplines. In an unfashionable field it is hard to become aware of earlier literature in strange journals and unfamiliar languages. Also, in a relatively inactive field, it is hard to latch on to a meaningful citation trail. The later examples of repeated discovery, after the field became well established, are less easily explained. Repeated discovery will be a recurrent theme in our subsequent discussion.

Our subject has a diversity of roots. It has become fashionable and acclaimed within the past ten or fifteen years, but has connections to work that took place many decades ago. The concern with fluctuation induced motion, at a serious analytical level, started near the turn of the century, with contributions by Pearson, Lord Rayleigh, Einstein, v. Smoluchowski, and others. These pioneering investigations are cited by Chandrasekhar (1943). A more detailed discussion of early Brownian motion studies is given by Coffey (1985), in a review paper which in many ways complements our account. Five Einstein papers on Brownian motion have been translated and supplied with supplementary comments (Fürth, 1956). Pais's (1982) biography of Einstein*, with its marvelous mixture of scholarship and insight, provides an excellent account of the history of Brownian motion studies. We learn there that Smoluchowski's 1906 publication date for his derivation of the 1905 'Einstein' relation may have been the result of a conservative attitude toward publication. Pais also points to a more unexpected fact: William Sutherland (1905), from Australia, submitted a paper with the same result, for publication, a few weeks before Einstein's submission date. Indeed, Sutherland's paper refers to his 1904 communication to the Australian Association for the Advancement of Science, in which 'The formula obtained made the velocity of diffusion of a substance through a liquid vary inversely as the radius a of its molecule and inversely as the viscosity of the liquid.' Readers and authors of this present volume may also be amused by Pais's remarkable understatement: 'Brownian motion was still a subject of active research in the 1970's.'

The concern with fluctuations reached a particularly developed form, at an early stage, in electronics. Einstein (1907) pointed out that an open circuited capacitor exhibits thermal equilibrium voltage fluctuations, which have an average energy $\frac{1}{2}kT$, and are conceivably measurable. Schottky (1918, 1922) described the fluctuations in emission limited current flow in thermionic

* For the benefit of future Einstein historians we allude here to a little mystery, unrelated to the real theme of this paper. Most Einstein accounts tell us he was born in Ulm, where his family actually stayed only briefly. Mohn (1970) tells us that Einstein's real origins were in Buchau, where his parents resided until the move to Ulm, and where Einstein's father's family had lived for many generations. Einstein was probably the descendant of Baruch Mosis Aynstein, who in 1665 started the thriving pre-World War II Jewish community of Buchau. Einstein's parents left Buchau toward the end of 1878; Einstein was born in Ulm on March 14, 1879. Mohn's description agrees with the view which this author has heard repeatedly from former members of the Jewish community of Buchau. In contrast, Specker (1979) has Einstein's father already residing in Ulm at the time of his marriage in 1876. I am indebted to H. Risken, whose own contribution appears in chapter 6 of this volume, for a copy of Specker's *Einstein and Ulm.*

diodes, due to the graininess in the current, carried by single and independently emitted electrons. Johnson (1927) and Nyquist (1928) at Bell Laboratories, a decade later, introduced us to thermal equilibrium noise arising in resistors, through their respective experimental and theoretical investigations. In this work, Nyquist adapted the standard black-body radiation discussion to an electrical transmission line terminated by a resistor. Despite the widely prevalent use of these results by electrical engineers, physicists did not develop an equivalent concern with fluctuation–dissipation theory until the mid-1950s. Kubo (1986), one of the principal exponents of this later viewpoint, has described this approach and its history in a recent account. Despite the great impact of this work on the physics community, it was, in a way, a step back from the understanding already achieved in circuit theory. In circuit theory a duality between current and voltage sources had long been recognized. The *linear response* theory, associated with the developments described by Kubo (1986), was more limited. There, fields were taken to be *the* cause of transport, and current flow was the response. Furthermore, the typical versions of linear response theory treat conservative Hamiltonian systems, and still require subtle cheating to yield dissipative behavior.

By 1938 the first book appeared on noise in electrical circuits (Moullin, 1938). The key points of understanding that had come out of the physical electronics literature were summarized, effectively and compactly, by Pierce (1948). Pierce emphasized the viewpoint of one concerned with vacuum tubes and electron beams, and can still be read with profit today.

Systems of serious engineering interest, which do not just sit there statically, but perform a useful function, are usually non-linear. In electronics, only straightforward amplification and signal propagation have limited ranges of linearity. Other functions, such as modulation, detection, heterodyning, and generation of a fixed amplitude oscillation, are strongly, and intrinsically, nonlinear. It is, therefore, no great surprise that the concern with noise in nonlinear circuits eventually received attention, particularly in connection with World War II radar development (Boonimovich, 1946; Lawson and Uhlenbeck, 1950; Rice, 1945).

After World War II the concern with noise in non-linear systems gradually broadened beyond the specialized communications and radar applications, regaining a more conceptual and statistical mechanics flavor. There were, for example, discussions whether rectification of spontaneously arising noise signals could generate power (Brillouin, 1950). The concern with diode kinetics was extended to vacuum diodes, which pass one electronic charge at a time (Alkemade, 1958; Van Kampen, 1960, 1961, 1981). By 1960 there was also a review by Lax (1960), emphasizing fluctuations about a state which is not necessarily one of thermal equilibrium.

The development of the laser acted as a further spur to the study of noise. We can cite only a few of the many key items in this field (Armstrong and Smith, 1967; Haken, 1964; Lax, 1966; Risken, 1965, 1966). Eventually the insights gained in this field were generalized, and led to analogies with ordinary thermodynamic phase transitions (DeGiorgio and Scully, 1970; Graham and Haken, 1968, 1970; Haken, 1970). Haken's many subsequent conferences and books helped to create public awareness of stochastic phenomena in systems far from equilibrium.

The early interest in noise was a result of its role as a source of confusion and error in measurement and communication, and, with limited exceptions, that characterizes the history of the subject we have presented so far. In more recent times there has been a growing awareness that noise can be *a controlling qualitative influence on the development of systems.* In systems which are multistable, i.e. have two or more competing states of local stability, noise determines the motion of the system between these competing states. Small systems, as found in molecular biology, and in the drive toward miniaturization of computer components, can show a strong susceptibility to noise. Furthermore, such noise induced transitions may be of utility, as invoked in the simulated annealing technique for optimization in the presence of complex constraints (Kirkpatrick and Toulouse, 1985). A similar application occurs in a model for the mechanism of perception and visual recognition (Ackley, Hinton and Sejnowski, 1985; Kienker, Sejnowski, Hinton and Schumacher, 1986; Sejnowski, Kienker and Hinton, 1986), in which fluctuations are invoked to allow escape from premature fixation. First order phase transitions can occur by homogeneous nucleation, followed by expansion of the nucleus through motion of the interface. The formation of a critical nucleus is a fluctuation which takes the system from one state of local stability (i.e. the original metastable phase) towards a new state of local stability. Indeed, in recent years, such nucleated transitions have become of cosmological interest in connection with the *inflationary universe* (Barrow and Tipler, 1986). In our general discussion of competing states of local stability, and stochastically induced transitions between them, it is appropriate to point out the connection to discussions of evolution and the origin of life. This, also, can be viewed as an optimization process, in which fluctuations (e.g. mutations) take us from one metastable ecology to a new one. (For citations see ref. 40 in Landauer (1987a).) Evolution can be viewed as hill climbing (or valley search) in a multi-valley 'Fitness Landscape', a concept often associated with the names S. Wright and R. A. Fisher.

Indeed, evolution bears a strikingly detailed analogy to the nucleation process. In nucleation a fluctuation is required which takes us to the formation of the critical nucleus; not just any fluctuation will do. After that the macroscopic system laws, which determine interface motion, take over. Genetic mutations are like the critical nucleus, and represent the necessary consequence of fluctuations. After that the normal biological machinery can take over and control the further time development. In contrast to the discussions cited in Landauer (1987a), which elaborate on this theme, and represent a longstanding orientation in ecology, physical scientists have been inclined to use the word *self-organization* very loosely. To apply this expression to the Benard instability, for example, to the Zhabotinskii reaction, or to the oscillations of a laser, under conditions where these temporal and spatial patterns are the only allowed behavior, seems strained. We might, with almost equal justice, refer to the revolution of an electron around a hydrogen nucleus, or the rotation of a water wheel, as self-organization.

The modern analysis of noise induced transitions between states of local stability has a number of precursors. Many escape problems, e.g. the thermionic emission of electrons from a metal surface, or the vaporization of an atom from the surface of a condensed phase, have the character of noise-assisted escape from a state of local stability. Thus, for example, the electrons in a metal, at the absolute zero of temperature (i.e. in the absence of noise), have no chance to escape. In the case of electron emission, however, the typical analysis did not greatly concern itself with the way fluctuations gave the electrons their required escape energy; it was simply assumed that gaining the required energy was not the bottleneck in the process. A pioneering paper by Becker and Döring (1935) went well beyond that and treated the formation of a critical nucleus, in the case of droplet formation, by treating the detailed statistics of atoms arriving at the droplet and evaporating from it.

The key step, however, in the treatment of escape from a locally stable state and in the treatment of transition kinetics between competing states, came through the paper by H. A. Kramers (1940). Kramers treated the escape from a potential well as a problem of Brownian motion in a non-uniform force field. Most of the papers in this volume can be considered to be descendants of Kramers' work. Table 1.1, adapted from Landauer (1987b), illustrates the diversity this field has achieved. No review covering most of these topics exists; it can be hoped that the present volume will help to correct this gap. Kramers' paper was a model scientific paper. It was compact by modern standards, but clear and very physical. It touched upon several of the most basic aspects of the area, some of which took decades to be appreciated. Yet, it left room for many later Ph.D. theses. The real question: Why did it take several decades for the flood of papers based on Kramers' work to start?

In connection with our emphasis on Kramers' paper, we need to add a qualification. R. L. Stratonovich, in correspondence, drew my attention to a 1933 paper by Pontryagin, Andronov and Vitt (1933). An English translation of this paper is provided in the Appendix to this volume. Quoting Stratonovich's correspondence:

> In this work mean exit time was exactly found for the one-dimensional case, initial point being given. If we take the point where double-well potential has local maximum (the point lies between the wells) as a boundary point and the centre of a well as an initial point, then we immediately find the mean lifetime of metastable state.

It is left up to the reader to judge the exact extent to which this work pioneered. These authors, however, understood first passage time evaluation, understood

Table 1.1 *Escape from the metastable state*

Dimensions and degrees of freedom	
Particle moving in one dimension	
Many degrees of freedom	
	All on similar time scale
	Born–Oppenheimer surfaces
Infinite number of particles	
	Sine–Gordon chain, ϕ^4 chain, solitons
	One-dimensional Ising
Law of motion	
Completely Hamiltonian (molecular dynamics, MQT theory)	
Hamiltonian with noise and friction	
More general dynamics, e.g. multistable circuit, laser, ecology	
Number of competing locally stable states	
Single well, escape into unlimited range of motion	
Bistable well	
	Symmetrical
	Unsymmetrical
Many competing states	
	No symmetries (e.g. spin glass, competing states of ecology)
	All alike: particle in sinusoidal potential
	Biased sinusoidal potential
Quantization	
Classical continuous case	
Classical discrete case	
QM case	
	Quantization in well
	Escape by tunneling with or without friction
Noise	
Thermal equilibrium	
More general, e.g. $1/f$ noise	
Big occasional jumps (chemical collision, quantized radiation)	
Markovian v. correlated noise	
Noise independent of state of system v. state-dependent noise	
Time dependence	
Time independent law of motion	
Modulated barrier and/or sinusoidal excitation in well	
Damping in well, noise	
Strong, weak, or very weak	
Purpose; answer desired	
Escape probability	
Equilibration within a well	
Average escape energy	
Dwell time near top of barrier, or tunneling through barrier	
Approach to optimum well (spin glass annealing)	

bistable potentials, and understood that noise could depend on the state of the system. They also addressed motion in a general dynamical system, not just motion of a particle in a potential. The paper, however, does not seem to be widely known. *Science Citation Index* for 1986 provides only three citations of this paper. None of these were from the Soviet Union (but 1986 is atypical in that respect). None of the three listed citations relate to the full scope of the paper, e.g. to the bistable case.

A few words about the authors of this pioneering 1933 paper are in order. Pontryagin is a well known blind mathematician, particularly noted for his work on topological groups. He received the Stalin prize in 1941, the first year for that prize*. When a new prize is issued there is always a backlog of candidates, and inclusion in this first year must signify an unusual distinction. In 1962 Pontryagin received the Lenin prize, which in the intervening years had replaced the Stalin prize. Pontryagin also was decorated with the order of Lenin; an honor he shares with Pravda, among other Soviet institutions.

Let me also add a few remarks related to Pontryagin's collaborators. A book entitled *Theory of Oscillations*, with Andronow and Chaikin listed as authors, was published in the USSR in 1937, and in English translation in 1949 (Andronow and Chaikin, 1949). The second edition, published in Moscow in 1959, bears the names of Andronov, Vitt and Khaikin. It also appeared in an English translation (Andronov, Vitt and Khaikin, 1966). The translated preface to this second Russian edition, signed by Khaikin, tells us:

> The writer of this Preface is the only one of the three authors of this book who is still alive. Aleksandr Adol'fovich Vitt, who took part in the writing of the first edition of this book equally with the other two authors, but who by an unfortunate mistake was not included on the title page as one of the authors, died in 1937.

This belated correction, after twenty-two years, without real explanation, is remarkable. The fact that the English editor, in his own notes, provides no further elucidation, is even more remarkable. The year of the original appearance of this book, 1937, was at, or near, the peak of Stalin's purges; 1956 was the beginning of Khrushchev's reaction against Stalin, and 1962 its high point. We cannot but help compare Vitt's misfortune with Pontryagin's ability to earn high honors under a diversity of regimes.

Remarkably enough, Kramers' paper gained attention, at first, primarily for its development of equations for Brownian motion in a non-uniform force field, and not for the paper's real target, the escape rate problem. Actually, the equations for Brownian motion in a non-uniform force field had already been discussed by others, e.g. Klein (1922). Quite likely, the first important paper which carried Kramers (1940) further was that of Brinkman (1956), who generalized Kramers' work for a one-dimensional potential to many dimensions. A few years later Landauer and Swanson (1961), unaware of Brinkman's

* I am indebted to T. Walsh of Dublin, Ireland, expert on Soviet history, for the interpretation in terms of broader Soviet history, given in this paragraph, and in the next one.

work, repeated it but also went beyond Brinkman and discussed escape from an extremely underdamped multi-dimensional well. The next rediscovery was by Langer (1968). We can hardly afford to list the multitude of subsequent rediscoveries, and mention only one recent paper to illustrate this tendency (Barma and Ramaswamy, 1986). The authors of that paper state: 'The single particle problem has been generalized relatively recently to allow for higher dimensional potentials.' The authors append a citation to a 1980 paper, in connection with that statement. Another example of rediscovery: Kramers' basic notions were found again in 1978, under the label *Stochastic Catastrophe Models* (Cobb, 1978).

Kramers' discussion, in the very heavily damped case, pointed out that escape over a potential barrier was controlled by diffusive motion over the top of the barrier. The motion of injected minority carriers, through the base of a bipolar transistor, is a similar diffusion controlled escape over a barrier. The transistor literature found its own way to an understanding of that problem (Shockley, 1950), without reference to Kramers. Eventually the transistor related literature became extensive, with many variations on the basic problem, including time-dependent cases, non-uniform mobilities, and trapping phenomena. The transistor literature and the statistical mechanics community concerned with escape from the metastable state have continued on their independent courses with few cross-citations. The simplicity and power of the viewpoint used in the transistor literature has been stressed elsewhere (Büttiker and Landauer, 1982, Appendix, p. 138; Landauer, 1983).

Active kinetic systems, following their own law for time evolution and influenced by noise, can show great similarity to the motion of a particle in a noisy force field. In many cases an exact equivalence can be found. This was understood in connection with the treatment of electronic oscillator synchronization, in the presence of noise, in the mid 1950s (Kuznetsov, Stratonovich and Tikhonov 1954, 1955; Raevskii and Khokhlov, 1958; Stratonovich, 1958). These ideas were subsequently explored in greater detail by Stratonovich and other Soviet colleagues (Kuznetsov, Stratonovich and Tikhonov, 1965; Stratonovich, 1967). Stratonovich (1958) treated a problem equivalent to Brownian motion in a sinusoidal potential, with an additional spatially uniform force, and this discussion was expanded by Tikhonov (1959). There have been numerous independent rediscoveries of this analysis since that time. In 1978, for example, papers by two separate groups published results virtually identical to the Figure 3 of Tikhonov (1959).

I asked Stratonovich to explain the relationship between the three names that occur on a number of our citations: Kuznetsov, Stratonovich and Tikhonov. These papers are identified in the journals as coming from Moscow State University. Stratonovich writes:

> At that time P. I. Kuznetsov was the senior professor and the head of the Chair of Physical Department of the Moscow University. I was the junior member of his Chair.

V. I. Tikhonov was the friend of Kuznetsov. He never was the member of the Moscow University. Afterwards he became my friend despite the age gap: he was nine years older than I. Tikhonov was interested in the theory of fluctuations. Because of acquaintance and contacts with him I got interested in the fluctuation theory and began to work on this subject. Tikhonov stated problems. Following his advice, I wrote my first book *Topics in the Theory of Random Noise* (Russian version of the book was published in 1961).

The series of papers (which we have not cited in its entirety) started only after Stratonovich came on the scene. With awareness of Stratonovich's later work, I cannot but help guess Stratonovich was, in fact, the key participant. Clearly, however, such a speculation by someone far from the scene, unfamiliar with the language, and who has never met the participants, must remain a guess.

R. Landauer (1962) returned to the subject of active multi-stable systems (without awareness of the Soviet work), in an analysis of the stability of information in very small, active, memory circuits. The particular vehicle used in that analysis was a bistable Esaki diode circuit. That circuit, once, was a serious candidate for memory and logic applications. Landauer (1962) was anticipated in an earlier qualitative discussion (Landauer, 1961) of 'structures which are in a steady (time invariant) state, but in a dissipative one, while holding on to information'. (The occurrence of the words *structure* and *dissipative* in this quotation is not in the order which subsequently became popular.) In a narrow technical sense Landauer (1962) can be considered to be an amalgamation of Kramers' work and that of Van Kampen (1960, 1961). Nevertheless, it took a dozen years after Landauer (1962) appeared before these notions were again taken up in the basic science literature. Indeed, as late as 1984, a set of papers (Knessl, *et al.*, 1984; Knessl, Matkowsky, Schuss and Tier, 1986; Matkowsky *et al.*, 1984), rediscovered one of the key points of Landauer (1962), that Kramers' view of escape from the metastable state could be extended to systems in which there is a discrete ladder of states, rather than a continuum. (As in a number of other rediscovery cases discussed in this paper, the rediscovery included a good deal not present in the earlier version.) The engineering literature turned out to be more receptive than the more basic areas. See, for example, Lindsey (1969), which demonstrated the influence of Stratonovich's (1963, 1967) work. The Esaki diode paper (Landauer, 1962) analyzed a system in which the fluctuations are not simply determined by a temperature, but depend on the state of the system. The paper explained that in such cases questions about relative stability cannot be answered by a simple appeal to the noiseless, deterministic, equations of motion, e.g. by a version of Maxwell's equal area rules. This same point is also contained in equation (4.49) of Stratonovich (1963) and in much of the earlier Soviet literature we have cited. The point is totally explicit in some of the cases discussed in Stratonovich (1967). The general understanding of this point, elsewhere, came more than a decade later, typically associated with the labels *multiplicative noise, external noise* and *noise induced phase transitions.*

We will not take our history past 1970. After that the field became very active, and historical perspective takes time to develop. But we will point out the connection of some early work to a few of the subjects in Table 1.1, which became fashionable later on. One of these is the *statistical mechanics* of sine–Gordon solitons. We have in mind a set of particles, strung out along a line, with each particle in a biased sinusoidal potential, i.e. with a spatially uniform force added to the sinusoidal potential. The particles are subject to damping and to thermal fluctuations. The particles are coupled to each other, so that adjacent particles tend to be in alignment in their motion along the sinusoidal potential.

Consider such a chain of particles, in which all the particles are aligned, lying along the bottom of a particular trough in the tilted sinusoidal potential, analogous to a chain lying along a trough in a tilted washboard. This is a metastable state; after all, one of the potential valleys next to the occupied one is at a lower energy. To make a transition to chain configurations of lower energy we must first cross a saddle point, i.e. form a critical nucleus of the 'new' phase. This nucleus consists of a portion of the chain which has been transferred to the adjacent lower valley. The formation of the critical nucleus is, of course, noise-activated escape from a many-dimensional potential well (Brinkman, 1956; Landauer and Swanson, 1961; Langer, 1968). This problem was faced first by the dislocation theorists, concerned with the behavior of kinks in edge dislocations (Hirth and Lothe, 1968; Lothe and Hirth, 1959; Seeger and Schiller, 1966). (Strictly speaking, the dislocation theory problem differs slightly from the sine–Gordon soliton problem. The difference arises from the fact that the dislocation theorists invoke an attractive force between kink and anti-kink which represents elastic interactions in a *three-dimensional* medium.) Some of the dislocation theorists knew about Kramers, but not about Brinkman (1956), and Landauer and Swanson (1961). The dislocation theorists developed their own picture of this multi-dimensional process. Finally, McCumber and Halperin (1970) solved a problem of this type (in the treatment of resistive fluctuations in thin superconducting wires) by combining the correct approach to the saddle point configuration, with the kinetics of Brinkman (1956), and of Landauer and Swanson (1961). The correct saddle point configuration had, of course, already been discussed by the dislocation theorists and also by Langer (1967), in a paper which preceded Langer (1968). The synthesis provided by McCumber and Halperin (1970) was also provided in the dislocation theory literature by Petukhov and Pokrovskii (1973). In the mid-1970s, the statistical mechanics of sine–Gordon solitons became widely recognized and fashionable. For review articles written after the initial burst of this activity see Bishop, Krumhansl and Trullinger (1980), and Büttiker and Landauer (1982). The extent to which even conference organizers, as well as authors of books and review articles, have managed to ignore the early dislocation theory work is truly amazing. In fact, the dislocation theorists had pioneered not only in the statistical mechanics of sine–Gordon solitons, which is our concern here, but also in the earlier deterministic theory. One of the key participants in all this, Alfred Seeger, has provided his own historical observations (Seeger, 1980a, b, 1983, 1986). Seeger's historical accounts, however, emphasize the deterministic case, which is not our principal interest.

The escape from a very underdamped potential well has already been mentioned in our discussion of the multi-dimensional well, and has become a subject of considerable attention recently. We refer the reader to Büttiker (Chapter 3, Volume 2) for a description of the modern work. A particle in a well without damping and fluctuations will just stay at its initial energy, and cannot escape. As the coupling to the reservoir, and therefore the noise and damping, is increased, the chances for a change in energy, and therefore for escape, will improve. At sufficiently high damping the particle will execute diffusive motion over the barrier, and in that range further increases in damping will decrease the escape probability. All this was clear already to Kramers (1940). It seems, however, to have escaped the attention of many theoretical physicists, starting with Chandrasekhar's (1943) otherwise definitive review paper. The continuing failure to understand the point may, in part, have been an unfortunate and unintended consequence of the elegant viewpoint introduced by Langer in the late 1960s, and reviewed in Langer (1980), which invoked analytic continuation, and the imaginary part of the free energy, to calculate escape rates. That, however, introduces no real kinetics. The physical chemists were more perceptive. After all, molecules in a gas, going through some internal rearrangement which involves barrier crossing, need to be jostled by other molecules to get the necessary energy. This has been understood, at least, since the early 1920s, as reflected in the work of Lindemann (1922) and of J. A. Christiansen (1921, Ph.D. Thesis, University of Copenhagen. Note: this author has not inspected Christiansen's thesis and has copied this citation from other sources). It is, perhaps, no great surprise that Kramers, a collaborator of Christiansen (Christiansen and Kramers, 1923), later returned to a more analytically definitive discussion of the subject (Kramers, 1940). In recent years theoretical physics has finally caught up with the physical chemists, though there are also a few older papers in the physics literature which appreciated the behavior of very lightly damped systems (Bak and Lebowitz, 1963; Iche and Nozières, 1976; Landauer and Swanson, 1961; Landauer and Woo, 1971; Lee, 1971).

Christiansen (1936) published a perceptive paper which treated the successive steps in an activated chemical reaction very much like steady state current transport through a set of series resistors, with the size of the local conductivity representing the level of response to a gradient in chemical potential. This is the approach which the transistor community rediscovered (Shockley, 1950) and which this author has repeatedly advocated (Büttiker and Landauer, 1982, Appendix, p. 138; Landauer, 1983). Unfortunately, Christiansen's paper had little impact on the literature that followed it. Lifson and Jackson (1962), for example, derived an effective diffusion constant for particle motion in a periodic field. This is a problem which becomes very simple when handled by the series resistance methods of Christiansen (1936). Furthermore, via the Einstein relation, it becomes equivalent to the problem of drift in a weakly biased sinusoidal potential, a special case of the problem already treated in Stratonovich (1958) and Tikhonov (1959). Nevertheless, Lifson and Jackson (1962) was still ahead of many others in its correct and clear treatment of this problem. The subject was revisited on many later occasions, e.g. by Keck and Carrier (1965). At a much later stage, Festa and d'Agliano (1978) rediscovered the result of Lifson and Jackson by a most elegant and complex method. Gunther, Revzen and Ron (1979) responded to this by correctly and succinctly pointing out the simplicity of the problem, but presumed they were the first authors to understand that. A still later return to the question, without awareness of this long history, is exemplified by Derrida (1983). Again we stress that we emphasize the continued rediscovery episodes not in an attempt to blame particular authors, but only to characterize the overall evolution of the subject.

I do not want to overdo the case for the perception of the physical chemistry community; they, in turn, largely ignored Kramers (1940). For example: three otherwise very authoritative texts on unimolecular reactions (Forst, 1973; Robinson and Holbrook, 1972; Slater, 1959) do not cite Kramers (1940) in their comprehensive bibliographies.

Our account has emphasized the prevalence of unintentional rediscovery in this field, without even remotely exhausting my list of relevant case histories. Rediscovery is not limited to this field. Elsewhere (Landauer, 1978), I have described the history of the internal field in dielectric theory; over many decades, different scientists gave us the same answer to that problem. It does seem, however, that the huge volume of work done in modern science has made the communications process ineffective and has increased the probability of unintentional reinvention. In fact, we have positive feedback in our publication system: the flood of publications makes any one item invisible. As a result, the author has to repeat the message in closely related manuscripts, thus helping to cause the flood.

Is unintentional rediscovery a serious problem, aside from the obvious credit questions? I believe that it is; it is a symptom of an inefficient method of doing science. In addition to the costs of repeating the work, even the repeated publication, in itself, is a costly process. It ties up referees, editors and authors, and permits the existence of journals which cost of the order of one-thousand dollars for an annual subscription. As scientists, we all tend to turn to our sources of financial support and plead: Give us more money to do a better job. But an even larger number of scientists is likely to give us an even more inefficient process. It may be in order for the scientific community to put its house in order, and first learn to be more effective at our current level of support.

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