

BIOGRAPHICAL MEMOIRS

Sir Sam Edwards. 1 February 1928 — 7 July 2015

Mark Warner

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SIR SAM EDWARDS
1 February 1928 — 7 July 2015



A handwritten signature in black ink, appearing to read 'S. F. Edwards'.

S. F. EDWARDS



SIR SAM EDWARDS

1 February 1928 — 7 July 2015

Elected FRS 1966

BY MARK WARNER FRS*

Cavendish Laboratory, JJ Thomson Avenue, Cambridge CB3 0HE, UK

Sam Edwards was one of the leading physicists of the second half of the twentieth century. He was Cavendish Professor at the University of Cambridge, a Vice President of the Royal Society, a member of the Académie des Sciences and of the US National Academy, and a senior figure in the university and his college. He played a major role in public life, most notably as chairman of the Science Research Council (SRC), responsible for research funding in the UK. He was chairman of the British Association, chief government scientist to the Department of Energy, and chairman of the Defence Scientific Advisory Council. He was equally in demand to lead or to help set up bodies abroad, particularly the Max Planck Institute for Polymers in Mainz, Germany. Remarkably, Sam made some of his most celebrated scientific discoveries, for instance the theory of spin glasses and the rheology of high polymer melts, while serving as the full-time head of the SRC. Conversely, his scientific insights informed his leadership in advising the government. His later science was in highly applicable areas: he was an active advisor to Unilever, Dow, Lucas and many other companies that rely on research.

WALES, CAMBRIDGE AND THE USA

I was born in Swansea on 1st February 1928. I was an only child but there was a large extended working class family. Soon after my birth, my father who had found a permanent job reading electric meters, bought a house in the suburb of Manselton where I was brought up. He lived there for the rest of his life. There was no scientific history in the family but there was artistic talent on my mother's side ... This talent has re-emerged periodically in the family but not in me.

*mw141@cam.ac.uk

So begin Sam's private reflections on his own life (31).^{*1} It is the precise and laconic style that all who knew him vividly remember. Sam was a Welshman—something that was immediately obvious. He goes on:

I did well at the local schools and proceeded to Swansea Grammar School in 1939. There were several grammar schools in Swansea but 'the' Grammar School was highly selective and its entry was again divided, so I found myself in a class of the top 30 boys from a town of 180,000 people, and we were taught very intensively. It was a school dominated by science and mathematics; moreover, the maths we were taught always was close to useful applications with none of the intrusions such as set theory which no doubt are useful to pure mathematicians, but have no value to scientists. I remember the great pleasure I found when we were introduced to Cartesian Geometry which gave me an unambiguous route to solve problems, and resolved the distaste I had for Euclidean methods which always seemed based on tricks. I was quite competent at the tricks, but Descartes offered a bulldozer and I became a lifelong adherent of the Cartesian style.

Already we recognize Sam's interest in the applicability of theoretical physics as stemming from the values he encountered at secondary school. The fields he founded, or made seminal contributions to, became of ever increasing applicability. In particular, his discoveries in polymer rheology underpin the huge plastics industry. Equally, his use of powerful and systematic methods to solve problems evidently went back to the childhood revelation that Euclidean tricks could be replaced by the 'bulldozer' of Descartes. He remained a Cartesian for the rest of his life and brought this style to the fields he founded, and to the co-workers who were privileged to be guided by him.

The 17-year-old Sam Edwards entered Gonville and Caius College in 1945, as the War ended, in possession of a College scholarship, plus those from the state and the local authority. He records there being ample money left over for books and visits to France in the long vacation. Aspiring to read theoretical physics, he found he first had to take a degree in mathematics that he found heavy going (31) since he was, and would continue to be,

impatient with the insistence on real proof in Maths, that is I think intuitively, get an answer, ... then worry about the correctness of the answer. Heredity deals one a hand of cards, and you must try to win with that hand, and not try to win in a different way. This philosophy is the way forward in research but in University exams you have to play according to the examiners' rules. In the end I reached the promised land and offered six courses for examination (2 quantum mechanics, 2 nuclear theory, 2 theoretical chemistry) but also attended general relativity, atomic theory, statistical mechanics, probability and classical dynamics, but did not offer these for examination.

Again, one recognizes the man whose later flashes of brilliance and discovery of bulldozers would, for instance, set up the spin glass problem as an n -component field theory and then derive results from the form of the $n \rightarrow 0$ limit—the so-called 'Edwards replica method'.

Sam did well enough to be offered a research studentship by the Department of Science and Industrial Research that he held at Caius and he started research in 1949. He completed his degree in 3 years, something that concerned him in later life: 'People learn too much; one only really needs to be taught method; facts just take time and are usually the wrong

* Numbers in this form refer to the bibliography at the end of the text.

¹ I am fortunate to have had access to these recollections to guide parts of this memoir. At many points, we hear Sam's own candid assessments and judgements. Private discussions and recollections over the years 1972–2015 that I knew Sam are referred to as 'personal communication'.

things anyway.’ This view set him apart from his contemporaries in the Arts. He would say to research students (personal communication): ‘A PhD is a certificate that you are capable in the methods of research, an apprenticeship if you like. It is not supposed to be a polished piece of work.’

As a potential supervisor in the Mathematics Department, he found Dirac a lone figure. Hartree had migrated to computation and, although Leonard-Jones was an attractive figure, Sam’s interests settled on nuclear and quantum field theory. Other students included Paul Matthews, with whom Sam would later work, Richard Eden, with whom his friendship resumed when the Edwards family returned to Cambridge, and Abdus Salam. In the Cavendish, Bragg decided to hire four theorists, one of whom was James (Jim) Hamilton, who had worked with Heitler, and whom Sam chose as supervisor. Sam complained about not being set problems that would be of consequence, and so turned to an interest in the then new re-normalization theories of Schwinger, Feynman and Dyson. In the meantime, his first published work was on nuclear binding energies, which theory had seriously underestimated. He addressed the binding problem, through interacting α -particles, the Coulomb-unstable ${}^8_4\text{Be}$, a work that seemed to have some impact and which he discussed later with Wigner when the two were at Princeton. An interest in nuclear reaction theory led him to Wigner’s work, which he found ‘impenetrable’, but also to that of Peierls, who would later play a major role in his life.

After 2 years of research, Sam went to Harvard with a J. H. Choate fellowship in September 1951. A highly stimulating year unfolded in both personal and professional spheres. He had lunches with J. K. Galbraith and McGeorge Bundy, heard the Boston Symphony Orchestra and the touring Met Opera, and visited the libraries of Harvard. More importantly, he was taken on by Julian Schwinger, who challenged him to solve his formulation of QED that took the form of functional differential equations relating the electron and photon Green² functions G and D , and the vertex function Γ . Sam solved these equations and eventually, after a struggle, convinced a sceptical Schwinger of the validity of his solutions. It was a decisive introduction to functional methods and (Feynman) diagrammatics (‘not popular in Harvard at the time’!) that would be a continuing intellectual thread in his physics for decades to come.

Sam’s Cambridge (UK) PhD complete and its results published, Schwinger arranged for Sam to spend a year at the Institute for Advanced Studies at Princeton. It was again a culturally enriching experience, with Wagner at the Met, the complete Beethoven Quartets and the galleries in New York and Washington, but somewhat directionless academically. He discussed his Harvard thesis work with Oppenheimer, Wigner, Yang, Lee, van Hove and others, but in 1953 accepted ‘a definite and generous offer’ from Peierls to join his school of theoretical physics in Birmingham, UK. In that year, he married Merriell E. M. Bland. They originally met as undergraduates at Cambridge through the Rambling Society. Merriell received a first in maths while at Girton and Sam would always cheerfully acknowledge that she ‘has a better degree than me!’. Their happy marriage was to last over 60 years (figure 1).

The years to follow—Birmingham 1953–1958, Manchester 1958–1972 and Cambridge 1972 to his death—were to see the development of the ideas and techniques from his early days, supplemented by some radical new ideas, and their application with enormous impact on other areas, some of which were entirely new to physics.

² Sam was very particular that such functions be called ‘Green’ rather than ‘Green’s’ functions. Whatever the correctness of this nomenclature, in his honour in this memoir we shall use ‘Green’.



Figure 1. Sam and his wife Merriell. (Reproduced by courtesy of the Edwards family.) (Online version in colour.)

BIRMINGHAM 1953–1958

Peierls and his group were a great magnet for visitors; Sam met many of the world's leading theorists. He was to encounter another figure who would also be decisive in his life, Brian Flowers, later Lord Flowers and Rector of Imperial College.

In Birmingham, Sam would employ field-theoretic methods in new ways and to address new problems that would revolutionize several fields over the next decades and give other theorists important tools for further problems. In particular, the functional methods Sam brought from Harvard interested Peierls greatly and led to Sam's work on pions and heavy nuclei. The Green function methods developed there offered hope for the solution of more difficult problems. Sam went on, later with P. T. Matthews, to study nucleon–meson interactions.

The problem that would first and firmly establish field-theoretic methods outside their initial realm of particle physics was posed to Sam by Peierls; it was concerned with the conductivity of an electron gas in the presence of random scattering potentials. In his paper,

‘On a new method for the evaluation of electrical conductivity in metals’ (1), Sam took the Greenwood–Peierls result for the conductivity tensor and transformed it in terms of Green functions and the distribution of electrons in the presence of the scattering potential $V(\underline{x})$ over which averages need to be taken, that is, over the positions of the scattering sites.³ These sites, at positions $\{\underline{X}_\alpha\}$ give $V(\underline{x}) = \sum_{\alpha=1} u(\underline{X}_\alpha - \underline{x})$, where u is the form of the individual sites’ potentials. In Fourier space:

$$V(\underline{x}) = \int u(\underline{k}) \exp(i\mathbf{k} \cdot \underline{x}) \rho_k^* d\mathbf{k} \text{ where } \rho_k = \sum_{\alpha} \exp\{i\mathbf{k} \cdot \underline{X}_\alpha\} \quad [1]$$

For randomly placed scatterers, there is an elementary, Gaussian-like at lowest order, probability distribution $P(\rho_k)$ for the ρ_k , so an averaging of the conductivity amounts to an integration over the ρ ‘fields’, that is $\int d\rho_k d\rho_k^* P(\rho_k)$. As Edwards noted (1):

This form has the great advantage that it is essentially the same form as that of electrons interacting with the quantised electromagnetic field, and so techniques for evaluating it are already in existence. Moreover, there are none of the divergence problems of electrodynamics here and the various approximate techniques of electrodynamics can be applied with confidence.

He developed a form of diagrammatics slightly different from QED and offered a partial translation from the diagrams in one problem to those in the other. He discussed the application of these methods to phonon-scattering and to liquid metals and alloys, and in cases that are not perturbative. Disorder is represented by field-theoretic methods for the first time. In fact, Gaussian integrals over auxiliary fields are ubiquitous, often known as the ‘Hubbard–Stratonovitch’ technique, but invented in the modern context before these authors by Sam, though he stated ‘Gauss gets priority’. Sam published widely in electronic and related systems, liquid metals, transport in plasmas, liquid helium, classical and quantum semi-conductors, quantum many-body theory, localization and disorder, and alloys until the 1970s. It was a fruitful and absorbing time in Birmingham. Sam always spoke of Peierls with great warmth and admiration. He recalls (31):

Peierls was invited to a grand meeting in Italy, the lead speakers were Heisenberg, Schrödinger, Pauli and Peierls. For some reason Peierls found it impossible to attend and sent me, so the lead speakers became Heisenberg, Schrödinger, Pauli and Edwards! Still it was good to meet these great men. I got on well with Pauli, which was apparently unusual.

Flowers was offered the Chair of Theoretical Physics at Manchester and invited Sam to join him as a senior lecturer—then the standard route to a chair (31).

Much as I loved Peierls the premium was huge, so we (now plus 3 kids) went off to Manchester bought a house in Prestbury (2.5 × salary) and stayed for fourteen very productive years.

MANCHESTER, 1958–1972

Sam was keen to branch out into other fields of physics. When Flowers was head of the theoretical division at Harwell, following the arrest of Klaus Fuchs, he had suggested that Sam look for problems in the still-secret fusion project at Culham, leading to his life-long interest in interacting many-body systems—initially plasmas and electrolyte solutions, and

³ I am grateful to Professor David Khmelnskii for insights into these advances (Khmelnskii 2004).

including turbulence. Again, he was to apply the same field-theoretic methods to polymers and networks to extraordinary effect.

Turbulence was then, and remains today, a formidable, partially-solved problem that had defeated many brilliant physicists and mathematicians during the twentieth century. In his first paper on turbulence (2), Sam considered fully-developed, homogeneous turbulence with energy input at long length scales by a random (stirring) force and flow through the coupled velocity modes of the fluid until the energy was released as heat by viscosity at the shortest wavelengths. It is an ambitious paper, 35 pages long, filled with statistical, functional and field-theoretic methods imported into the subject, and employing difficult and detailed analysis. In his own, concluding words, the difficulties are that:

turbulence is an exceptional problem in that there is, in the limit of large Reynolds numbers, no external parameter which can be used as a basis of an expansion technique. In the language of QFT, it is a problem of infinitely strong coupling constant. It follows that an expansion must be based on the internal properties of the system ... [and] since the probability of finding a particular velocity at a particular point in the fluid is quite close to a Gaussian (Batchelor), the system is substantially random and the generalised random phase approximation should be applicable.

It is worth exploring Sam's approach to turbulence, since it foreshadows later polymer work that had great impact. The Navier–Stokes equation for the velocity field $\underline{u}(\underline{r})$, with its stochastic driving force F , is treated in Fourier space and is shown to lead to a Liouville equation for the distribution of velocities, $F(\underline{u}_k)$. The 'noise' F is considered to fluctuate sufficiently rapidly in time that its correlation is $\langle F_k(t)F_k(0) \rangle = h_k \delta(t)$. Moreover, Sam used the fact that its distribution is Gaussian to average the distribution over F to give $\langle F(\underline{u}_k) \rangle_F$. He then considered this in a generalized phase space with a functional probability, P , following the system in time according to Lagrangian procedures. The velocity–velocity coupling in the Navier–Stokes equation, symbolically $\Sigma M \underline{u} \underline{u}$, is the source of the turbulence which he then attempted to describe self-consistently with an effective diffusion coefficient $d_k = h_k + S_k$ and a total viscosity $\omega_k = \nu k^2 + R_k$. The turbulent diffusion, or 'eddy noise covariance', S_k and viscosity, or 'eddy damping coefficient', R_k depend, through the $\Sigma M \underline{u} \underline{u}$, on S_k and R_k themselves. This dependence leads to non-linear integral equations with the ω_k in their kernels and also involving the velocity correlation q_k , the energy spectrum. Diagrammatic and other methods of solution, self-consistently if possible, had to be invoked. The use of S_k and R_k is related to considerations of Kraichnan and of Wyld.

The work was controversial at the time, leading to disagreements: for instance, with George Batchelor about the appropriateness of using Green function methods. Many insights were gained from Sam's results, but the analysis was sufficiently complex that clean power laws were not always obtained. But he could put bounds of $-5/3$ to $-3/2$ on the likely exponents for q_k , the Kolmogorov problem, and he, in common with Kraichnan, was one of the first to address the problem of the time correlation of the velocity modes, $\langle u_k(t)u_{-k}(0) \rangle$.

Sam also attacked the problem of pipe flow turbulence with McComb (6), which he felt was most successful but never gained the attention it deserved. Nearly 50 years later he returned to the study of turbulence with Schwartz (30), with a self-consistent expansion of the non-linear stochastic field equations about an undetermined model and fixing the functions in the model by requiring that, to a desired order in the expansion, there are no corrections to certain physical observables beyond zero order. They derived the Kolmogorov result both for extreme infrared and for white driving noise.

In the mid 1960s, Edwards met with Geoffrey Gee, head of Chemistry in Manchester, to discuss the outstanding problems in his area in the hope of finding fertile new ground. The meeting would have consequences for polymer physics—to which Sam would bring remarkable new insights—and also for physics generally. Theoretical physics had not really penetrated polymer science, except perhaps for the pioneering work of Cyril Domb on the statistics of self-avoiding random walks, which he had investigated numerically on lattices and by graph theory. Sam discovered that the N -step random walk mean-square end-to-end span result $\langle R^2 \rangle \propto N$ is amended by self-avoidance to $\langle R^2 \rangle \propto N^{6/5}$. The chain self-swells and, moreover, the result was independent of the type of lattice the chain occupies. The latter insight presaged the soon-to-emerge insights of critical phenomena and the re-normalization group. Polymer physics shares this character with critical phenomena and Sam's realizations put him ahead of those in the latter field.

Gee explained that polymers, at their simplest, are long sequences of linearly connected repeating molecular units, monomers, with structure and characteristics of connection, flexible or otherwise, that depend on detailed chemistry. The problems included the distribution of monomers in a self-avoiding chain, dynamics, visco-elasticity, phase changes and solution theory, and networks. Great figures had worked from the chemical side on these problems and Sam initially felt discouraged, but there were also electrifying realizations. As he wrote (31), the problems

had all been tackled in the Fifties by Flory, Huggins, Zimm, Stockmayer, Rouse, Wall and Gee himself. The outline of the subject was all there and I was coming into it ten years late. But all these authors, impressive as their work was, did not know the advances made in field theories and I realized the long random chains of polymers were a physical manifestation of field theories. A polymer *was* a Feynman path, the density of a polymer suspension *was* a field variable. The literature had a mass of redundant complexity. A polymer is comprised of monomers but, except in glasses and crystals, can be considered as a continuous path where s is the arc length. The polymer is so long that the Fourier integral can always be used so that, just as in elasticity, one need not put atomic detail into Navier's equation, or likewise in the Navier Stokes equations of hydrodynamics. There is no need, other than a convenience on simulations, ever to consider the polymer as fictitious beads and springs.

Although late, he could revolutionize the subject.

A random flight of N links of length l , say, will have a total arc length, chemical length for polymers, of $L = Nl$. It has a mean-square end-to-end size $\langle R^2 \rangle = Nl^2 = lL$, that is, a mean-square size in each of its three dimensions of $\langle R_i^2 \rangle = \frac{1}{3}lL$. The distribution of R , by the central limit theorem for N large, is the classical Gaussian form

$$P_N(R) \propto \exp\left(-\frac{3}{2lL}R^2\right) \equiv \exp\left(-\frac{1}{2}R^2/\langle R^2 \rangle\right). \quad [2]$$

Note that l and L always appear as the combination $lL = \langle R^2 \rangle$; the local structure of the walk does not matter. If it is not freely-jointed, the l will not correspond to the chemical length a , say, but will be larger. Thus $l = \langle R^2 \rangle / L \geq a$ where $\langle R^2 \rangle$ can be measured by light scattering in an ideal solution, and L is simply the (scaled) molecular weight times the monomer length. The Flory effective step length l accounts for all the chemical structure, and indeed the ratio l/a was studied by Flory. But it was Edwards who saw there was a universality in polymer statistics. For long distance problems, one could simply use Gaussians, continuous chains

and functional analysis on any length scale including the very fine, since local structure is irrelevant. He thus came to the now-celebrated Edwards chain distribution

$$P[\underline{R}(s)] \propto \exp \left[-\frac{3}{2l} \int_0^L ds \left(\frac{\partial \underline{R}}{\partial s} \right)^2 - \frac{v}{kT} \iint ds ds' \delta(\underline{R}(s) - \underline{R}(s')) \right] \equiv \exp \left[-\frac{1}{kT} H[\underline{R}(s)] \right] \quad [3]$$

The probability functional for the entire shape $\underline{R}(s)$ of the chain, with a continuous arc (chemical) position down the chain $s \in [0, L]$, has a continuous form of a Gaussian as its first term, think of the probability of the chain as the convolution of Gaussians $\exp(-\frac{3}{2l}[\underline{R}(s_1) - \underline{R}(s_2)]^2/|s_1 - s_2|)$ from arc points $s_1 \rightarrow s_2 \rightarrow s_3 \rightarrow \dots$ and, in the spirit of the above, that structure is eventually washed out to yield Gaussians, let the intervals become small and numerous, with the distributions at small scales still taken to be Gaussian. Sam retained l and L separately, but were he to scale s by L , then an L would meet the l as required by universality. One can imagine retaining the separation was a concession to colleagues for whom his reducing the ‘redundant complexity’ was already radical enough!

The second term is no less deep: monomers at chemically widely-separated points s and s' can nevertheless randomly walk close to each other and are then repelled by a potential $v(\underline{R}(s) - \underline{R}(s'))$. There’s a second virial to this interaction, the excluded volume, v , and the potential can be represented as $v\delta(\underline{R}(s) - \underline{R}(s'))$, the delta function’s weight being finite, and encapsulating again the idea that detailed structure is not significant. Under conditions where v vanishes, we need to go beyond this two-body representation of interaction.

The functional $H[\underline{R}(s)]$ in the exponential of equation [3] is the famous Edwards Hamiltonian

$$H[\underline{R}(s)] = -\frac{3kT}{2l} \int_0^L ds \left(\frac{\partial \underline{R}}{\partial s} \right)^2 - v \iint ds ds' \delta(\underline{R}(s) - \underline{R}(s')) \quad [4]$$

where, as usual, it is $H[\underline{R}(s)]/kT$ that appears in the exponential. The kT that appears in front emphasizes that the first term accounts for the chain entropy, from the number of conformations, and encapsulates much of polymer science. The flexibility of chains drives their largely entropic response and they are often described as ‘entropic springs’. Fifty years later, this development seems to physicists to encapsulate the essence of a huge and important subject, but in the 1960s Sam’s was an entirely new view of the subject.

With a hindsight borne of experience of the fields we have already sketched above, we can immediately anticipate Sam’s approach to the self-avoiding walk problem where there is interaction over all scales in the chain—he treated it as a self-consistent field problem where the chain interacts with a mean field generated by its own density of monomers. Although the mathematics is somewhat complex, the ideas we have already met. He uses an auxiliary field to represent the potential, and indeed it is in the distribution of that field in k -space that one must first seek Sam’s replacement $v(R - R) \rightarrow v\delta(R - R)$. He deals with an infinitely long chain that starts at $(\underline{R} = 0, s = 0)$ and finds the probability density of monomers at radial positions r to follow $\tilde{p}(r) \propto r^{-4/3}$ and that the probability $p(r, L)$ that the arc position L of a chain passes through a radius r is peaked at an $r \sim L^{3/5}$, that is $\langle (\underline{R}(L) - \underline{R}(0))^2 \rangle \sim L^{6/5}$. These core results are derived from mean-field theory but are very accurate. The true exponent is $\gamma = 1.188\dots$, taking into account correlations. Sam gave, and critically discussed, the dependence $\gamma = 6/(d + 2)$ on the dimensionality d of space. It is exact in 1-D, turns out to

be exact in 2-D, is closely accurate in 3-D, is exact in $d = 4$ and remains $\gamma = 1$ in all higher dimensions. As there are more spatial dimensions to explore, chains meet themselves less frequently and return to ideality. As in critical phenomena, dimensionality alone is important, not the lattice type nor the local chemical structure, and there is a critical spatial dimension.

Raising the polymer concentration c leads eventually to chain interpenetration, beginning at a concentration which Sam identified and which was christened c^* by the French school, with interactions leading to two consequences—the osmotic pressure's dependence on concentration changes, and the chain shape and the correlation change. In the melt state at very high concentrations, chains are ideal since, with complete interpenetration, they do not know whether they are avoiding themselves, or monomers from other chains from the multitude of other chains with which they share their space.⁴

At Sam's overlapping, 'intermediate' concentrations which can be delineated into semi-dilute and semi-concentrated, he determined quantitatively a correlation length ξ beyond which chains behaved as somewhat swollen Gaussians. At the semi-dilute end, self-avoidance plays a larger role. Sam's description of the osmotic pressure is valid at the upper end and he discussed the difficulties in approaching the lower end. Given his background in field theory and his delight that Feynman weights and diagrams for polymers are real in the mathematical and physical senses, respectively, it is particularly interesting that he discussed with diagrams the interactions with interposing chains as masking, or screening, the self-interaction. The study of solution interactions and correlations was central to the approach of de Gennes and of those of his school in discussing polymer behaviour.

As an aside, and looking ahead 10 years, Sam applied to dislocation lines in solids the ideas of long, extended objects having considerable configurational entropy and, when dense, screening their mutual and self-interactions (20). In this case, the 'polymers' are the dislocation cores and the interactions being screened, and thus reducing the energy, are the elastic strain fields. This was a theory of the melting of 3-D solids, with a first order transition from a crystal without defects to one full of defects—a liquid. The avalanche is caused by the increasing effect of entropy $-TS$ in the free energy with rising temperature, along with the reduction in elastic energy per defect if there are many of them. This successful theory had the misfortune to arrive at the same time as the Kosterlitz–Thouless theory of topological defect-induced melting in 2-D and was, to Sam's regret, entirely neglected.

Immediately following his insights into polymer chains and their interactions, Sam turned to two problems that would intersect with major consequences for fundamental and applied physics and for industry: (i) first single and then multi-chain dynamics, and (ii) chain constraints by crosslinking or by topology (entanglement).

Chain motion had been modelled by Rouse as beads connected by springs and was entropic because their resistance to extension comes from their free energy rising as their entropy drops. Extended chains have fewer configurations. Sam's springs and beads tend to a continuum, as seen above. Monomer connectivity slows down diffusion from $\langle (\underline{R}(t) - \underline{R}(0))^2 \rangle \propto t$ to

⁴ This return to ideality had been anticipated by Flory and in some sense is akin to electrons in a metal such as sodium being essentially free, despite the huge number of others in their vicinity. Interactions are screened—this was Sam's view, unsurprisingly. Sam was the first to calculate chain shape and give the variation of (ideal) size with density (10), something that was confirmed by neutron scattering. Sam's interest in neutrons for soft matter research was inspired by another Manchester chemist, Geoffrey Allen (see later), with whom Julia Higgins worked closely at that time. Sam was a close collaborator with them both.

$\langle (\underline{R}(s, t) - \underline{R}(s, 0))^2 \rangle \propto t^{1/2}$. Connectivity also conveys force, as one sees in the viscosity of polymer solutions and in network, or rubber, elasticity. Both problems had already been studied, but Sam attacked them with powerful methods and they were to yield much deeper results. In understanding polymer dynamics for the problems of melt viscosity, he solved sophisticated single chain problems such as the velocity distribution and correlation of monomers, and even demonstrated that equipartition gives an energy of $2 \times \frac{1}{2}kT$ per monomer, the connectivity eliminating 1 degree of freedom (5)—this was one of several papers with Goodyear.

Also fundamental was his recognition of the role of ‘quenched’ (immutable) degrees of freedom in polymers, soon to be applied in spin glasses and random matrices. The notion and terminology he employed has since become ubiquitous in physics. Normally one integrates over *all* varying quantities that the partition function (or statistical weight) depends upon, and then takes the logarithm to find the free energy

$$F_L = -kT \ln \langle (Z_m)_m \rangle \quad [5]$$

where m is the quantity chosen to be fixed, and might, for instance, be $m = \underline{R}_L - \underline{R}_0$, the end-to-end separation of the chain; $\langle Z_m \rangle$ is the statistical weight (or partition function) of the chain obtained by summing over all configurations that the chain is able to explore, *subject to this constraint*. But the free energy F_L is that of an unconstrained chain since in that case the average is taken over all the m values in finding $\langle \dots \rangle_m$ of the Z_m . In contrast, the actual free energy of a chain with the constraint m , is simply $F_m = -kT \ln(Z_m)$. One could now instead average this free energy over the distribution of all realizations m , giving the quenched free energy

$$F_Q = -kT \langle \ln(Z_m) \rangle_m. \quad [6]$$

The former F_L represents a liquid of chains while the latter quenched average yields the free energy F_Q of a solid, where the choices of m are fixed (quenched) at the moment of solidification and do not thereafter change.

Consider a deformation gradient $\underline{\lambda}$ applied to the solid $\underline{\lambda} = \partial \underline{r} / \partial \underline{r}^0$, where the material points \underline{r}^0 move to \underline{r} under deformation. The end-to-end vector for a particular chain takes a new (fixed) value $\underline{R}_L - \underline{R}_0 = \underline{\lambda} \cdot (\underline{R}_L^0 - \underline{R}_0^0)$, that is, the constraint is now m_λ , say, with superscripts 0 representing the constraint at formation, which for simplicity we take to be the reference state.⁵ Knowing the chain span distribution set in the reference state, m , the averaging is now

$$F_Q(\underline{\lambda}) = -kT \langle \ln(Z_{m_\lambda}) \rangle_m. \quad [7]$$

Thus, the partition function is constrained at m_λ but the averaging is over the m at formation.

The free energy $F_Q(\underline{\lambda})$ is linearly dependent on T , the signature of a purely entropic response, and so $F_Q(\underline{\lambda}) = kT f(\underline{\lambda})$, the chains reducing the entropy of their distribution of shapes purely because of geometry, encapsulated in the $f(\underline{\lambda})$, here a simple function, the

⁵ If there is a difference between the formation and reference states (by, for instance, heating etc.) then there is a simple additive factor to the free energy, but the principles are the same. This procedure, of taking the end-to-end vector to transform affinely with the macroscopic distortion, ignores certain fluctuations of the chain end anchoring points, but does not materially change the result.

invariant $I_1(\underline{\underline{\lambda}}^T \cdot \underline{\underline{\lambda}})$ (see below). We have already seen this entropic behaviour in the first term of the Edwards Hamiltonian equation [4].⁶

These deeper ways of looking at an already classical subject, rubber elasticity, allowed Sam to dispose of a spurious logarithmic term in the free energy that had been much argued over in the literature. But Sam's purposes were much deeper: constraining the ends of chains by crosslinking them to other chains, that is, the formation of a solid by vulcanization, is not the only additional constraint that is quenched. A chain with no free ends, if it is entangled with its neighbours, is permanently so knotted—see figure 2 for a sketch in Sam's hand (his figure 2.21) of crosslinks making an entanglement a permanent constraint (4). He also introduced Gaussian invariant ways of expressing topological permanency, his figure 2.22. Note that he had to hand-write equations into some papers, much increasing the scope for errors.

The quenched entities are not simple vectors such as $(\underline{R}_L^0 - \underline{R}_0^0)$, but are now topological invariants and present a much more formidable challenge to specify and then to average over their logarithms.

Let us digress onto a subject about which Sam felt very strongly: the $\underline{\underline{\lambda}}$ tensor 'lives' partly, from the left, in r -space (the target space) and partly, from the right, in r^0 -space (the reference space). The Cauchy–Green combination $\underline{\underline{C}} = \underline{\underline{\lambda}}^T \cdot \underline{\underline{\lambda}}$ is thus of $r^0 r^0$ character (the outer parts of $\underline{\underline{\lambda}}^T \cdot \underline{\underline{\lambda}}$) and is therefore automatically invariant under rotations of r -space. But the free energy must also be invariant under rotations of r^0 -space and therefore $f(\underline{\underline{\lambda}})$ can only be a function of the combination of the elements of $\underline{\underline{C}}$ that are invariants too. These invariants are

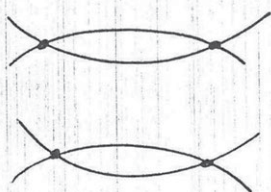
$$I_1 = \text{Tr}(\underline{\underline{C}}), \quad I_2 = \text{Tr}(\underline{\underline{C}}^T \underline{\underline{C}}), \quad \text{and} \quad I_3 = \text{Det}(\underline{\underline{C}}). \quad [8]$$

The rubbers deform at constant volume, $I_3 = 1$, and the elasticity of ideal chains is simply $f(\underline{\underline{\lambda}}) = I_1$, the term 'ideal' meaning non-entangled, flexible, or at least long. Experimentally, there are deviations from this law, attributed to entanglements and other smaller effects, and empirically $F(\underline{\underline{\lambda}})$ is fitted to elaborate expansions in terms of invariants. There was no basis for doing so and no understanding derived from the fit. For the huge elongations, by factors of up to $\lambda \approx 10$ which can be inflicted on a rubber, such strategies were of limited use. Sam had objections to proceeding blindly, as he saw it, though rigorously since the I_i are invariants. Something as complex as entanglements and large amplitudes demanded a molecular-based approach, even if the complexity of the result was such that one could not necessarily see how to reduce difficult results to invariants. A paper he wrote with Stockmayer (9) obtained an interesting $f(\underline{\underline{\lambda}})$ for a material intermediate between rubber and glass: the $f(\underline{\underline{\lambda}})$ (31),

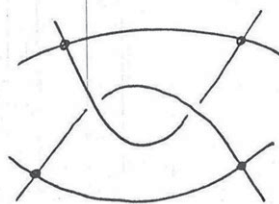
had no relation whatever to the invariants used by Mooney and Rivlin and I realized that the only way forward for elasticity and visco-elasticity was to create a plausible model and then solve it. Guessing gets one nowhere, but even now the literature is clogged with quite useless sums of the invariants which have no physical significance whatever.

⁶ This law for the rubber free energy has the same status as the perfect gas law, but where the $f(\underline{\underline{\lambda}})$ depends on shape rather than volume, and one should think of rubber more as being more like a gas rather than a solid or nearly-flowing liquid. This insight, and that of averaging outside the log rather than inside being the essence of what it is to be a solid (equation [6]), were two delights I received from Sam as I embarked on my PhD with him in 1973 in his early Cambridge days.

Finally there are the topological constraints. These express the fact that chains cannot pass through one another, for example



is different,
permanently,
from



(2.21)

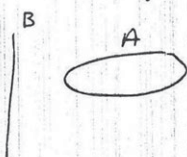
It has been shown (Ref. 4) that this can be expressed by a series of invariant expressions, the simplest of which is

$$I = \iint (d\underline{R}(s_1) \times d\underline{R}(s_2)) \cdot \underline{\nabla} |\underline{R}(s_1) - \underline{R}(s_2)|^{-1}$$

This rather formidable looking expression is just the 3 dimensional generalization of angle swept out, and will be familiar in the theory of Ampere and Gauss concerning the interaction of electric currents via magnetic fields. If for example we consider two curves $\underline{A}(s)$, $\underline{B}(s)$, and take one to be an infinite straight line, the other a circle

$$I = \iint (d\underline{A} \times d\underline{B}) \cdot \underline{\nabla} |\underline{A} - \underline{B}| \quad (2.22)$$

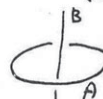
= 0 for



Similarly $I = 0$ for



= $\pm 4\pi$ for



= $\pm 8\pi$ for



$I = \pm 4\pi$ for

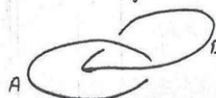


Figure 2. A sketch in Sam's hand of crosslinks, entanglements and Gaussian invariants of topological constraints (4). Reproduced with permission from Springer.

This wish to have a molecularly-founded description of exceedingly complex and non-linear processes, for instance large deformation in the face of topological constraints, set Sam, and the large community that followed him, apart from, for instance, the applied mathematics mechanics community. In a 6-year period of astonishing inventiveness and productivity, Sam and his group addressed the quenched statics and the dynamics of interacting polymers—melts

and networks—which required specifying and then dealing with the constraints and finally averaging logs over them.

First, we turn to the averages of logs, $\langle \ln(Z_m) \rangle_m$, where m is the set of quenched constraints, for instance knots confining chains to each other or, in simple networks, the fixed chain spans. Recognizing that

$$(Z_m)^n = \exp(n \ln(Z_m)) = 1 + n \ln(Z_m) + \dots, \quad [9]$$

then $\ln(Z_m)$ is the term at $\mathcal{O}(n)$ in the expansion of $(Z_m)^n$ (4). Each of the n factors of Z_m is the statistical weight of an m -constrained system. For instance, the α^{th} term in $(Z_m)^n = \prod_{\alpha=1}^n Z_m^\alpha$ for chains with constraint m is

$$Z_m^\alpha = \int_m \delta R^\alpha(s_\alpha) \exp \left[-\frac{3}{2l} \int_0^L ds \left(\frac{\partial R^\alpha(s_\alpha)}{\partial s_\alpha} \right)^2 \right] \quad [10]$$

Sam called the α instances ‘replicas’. The functional integral over the chain complexions is subject to the constraint m . The averaging of $(Z_m)^n$ over formation conditions and constraints m_0 is then simple:

$$\left\langle \prod_{\alpha=1}^n Z_m^\alpha \right\rangle_{m_0} = \frac{1}{Z} \int_{m_0} \delta R^0(s_0) \exp \left[-\frac{1}{kT} H [R^0(s_0)] \right] \prod_{\alpha=1}^n Z_m^\alpha \quad [11]$$

where

$$Z = \int_\delta R^0(s_0) \exp \left[-\frac{1}{kT} H [R^0(s_0)] \right] \quad [12]$$

The Edwards Hamiltonian functional $H [R^0(s_0)]$ is used for brevity in the exponent. In the numerator, one integrates over chain freedom limited by m_0 , but the normalizing denominator Z has no such constraint. Taking the limit $n \rightarrow 0$ and extracting the terms in n then yields the averaged log. Considerable liberties have been taken with the ordering of the average and the limit of this unusual n -component field theory. Sam also employed his replica method in his spin glass papers with P. W. Anderson, to be discussed below. Its use provoked controversy, but also stimulated many others to establish and scrutinize the method’s validity, including the questions of symmetry-breaking in replica space (14) and of obtaining minima (or maxima) in the free energy.

The grand result that illustrates the approach to the problem as an n -component field theory, albeit with $n \rightarrow 0$, is

$$\left\langle \prod_{\alpha=1}^n Z_m^\alpha \right\rangle_{m_0} \propto \int_{m_0} \delta R^0(s_0) \int_m \prod_{\alpha=1}^n \delta R^\alpha(s_\alpha) \exp \left[-\frac{1}{kT} \sum_{\alpha=0}^n H [R^\alpha(s_\alpha)] \right] \quad [13]$$

There are $n + 1$ replicas, the 0^{th} differing only in the constraints over which it is integrated. This result underpins all Sam’s celebrated attacks on polymer network elasticity—linkage, entanglement, slip links, etc.

Sam was initially attracted to Gaussian invariants to express fixed topology, m , when summing over the configurations of chains in the presence of others with which they may be topologically related. Several interesting papers on statistical mechanics with such invariants appeared, but it was clear to Sam that, in dense systems of chains, a given chain has an

enormous number of neighbours⁷ and hence an enormous number of complicated invariants—a cruder and more powerful method was required (see the extract from the decisive paper (3) in figure 3). We have the pleasure of seeing the figures hand-drawn by Sam himself, just as his co-workers and colleagues would see them at the blackboard. His Figure 1 shows other chains as dots passing through a plane, with the chain in question threading them and being constrained. His Figure 2 shows a workable model of the topologically-localized chain—it is localized about a central path, and hence for a dense system of chains a collection of non-local constraints have taken on a local character. The penalty for deviations from this reference path $\mathcal{R}(s)$ is entropic (wider excursions of the chain from $\mathcal{R}(s)$ have fewer complexions), and can be considered as a harmonic, entropic potential. When the material is strained, it is $\underline{\lambda} \cdot \mathcal{R}(s)$ that is deformed by $\underline{\lambda}$ and $\mathcal{R}(s)$ is now penalized for deviations from $\underline{\lambda} \cdot \mathcal{R}(s)$. The resulting free energy of small distortions is classical, but the crosslink density, which normally scales the shear modulus in the expression for the free energy density we have seen before, $F(\underline{\lambda}) = \frac{1}{2}\mu f(\underline{\lambda}) = \frac{1}{2}\mu I_3(\underline{\lambda})$ (with the latter form for small strains), now depends on the chain density that arises from Sam's estimate of the entanglement density. For small strains, the 'entanglement rubber' (crêche rubber, ultimately creeping, as Sam noted) has the classical form in $\underline{\lambda}$, but at high strains $f(\underline{\lambda})$ is complex, as one expects.

This masterly and prescient paper (3) set the scene for 'tubes', 'pipe constraints', 'primitive paths', 'reptation', a term coined by de Gennes in 1971 for chain dynamics, the work of Doi in 1974, and ultimately the grand synthesis of dynamics with constraints for a theory of polymer rheology by Doi and Edwards.

In 1973, at the end of Sam's time in Manchester, there appeared two important papers with Grant on the diffusion and viscosity of chains in a field of topological constraints represented as a tube, following on from his paper of 1967 and that of de Gennes in 1971—see Figure 1 of their first paper (7). In that paper, they first obtained the vividly named 'reptation' $t^{1/4}$ law for the diffusion of a chain along a tube, $\langle (\underline{R}(s, t) - \underline{R}(s, 0))^2 \rangle \propto t^{1/4}$. It corresponds to Rouse motion $\langle (S(t) - S(0))^2 \rangle \propto t^{1/2}$ along the curvilinear coordinate S along the tube which is itself a random walk in space; in other words, the position $\underline{r}(S)$ in space along the tube has a mean-square spatial separation varying as $\langle (\underline{r}(S) - \underline{r}(S'))^2 \rangle \propto |S - S'|^{1/2}$. Folding the square-rooted former result into the left-hand side of the latter gives the $t^{1/4}$ law. The second paper with Grant (8) addresses the ultimate goal, the effect of entanglements on the polymer melt viscosity which underpins most of the plastics industry. Using the above dynamics they had already established, they calculated, from correlation functions, the molecular weight (M) dependence of the shear viscosity η , obtaining $\eta \sim M^3$, already a triumph, but not quite the experimental exponent 3.4.

Tubes, and more complex aspects of melt rheology, would recur later in the 1970s with Doi, and replicas would be revisited in spin glasses with Phil (P. W.) Anderson.

As the Manchester years drew to a close, Sam was drawn into public affairs, with some negative impact on his output from 1973, at least as compared with Manchester, despite which the two main areas for which he is best known were carried out while working for the government. His wider public service started with the Physical Society (now the Institute of Physics, IoP), of which he became vice president between 1970 and 1973 (31):

⁷ Chains are ideal in the melt and so, with N monomers, have an extent $R \sim aN^{1/2}$. They thus span a volume $V \sim a^3 N^{3/2}$, whereupon there must be a number $\sim N^{1/2}$ other chains, each of chemical volume $V_c \sim a^3 N$, sharing this spanned extent with the test chain.

3. The effective realization of constraints

The proposal to be put forward will have some resemblance to Einstein's model of a solid. Let us consider a long segment of the chain, shown in figure 1 in a section along it;

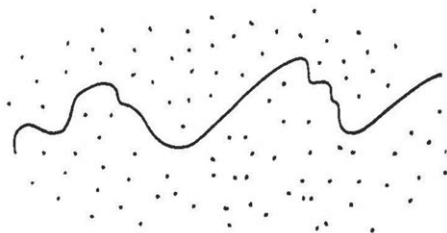


Figure 1.

the rest of the chains cut by the section appear symbolically as dots. Let us suppose for the moment that the rest of the chain is fixed; then this segment will be hedged in by the other chains. Let us suppose at some initial time it has an initial configuration $\mathcal{R}(L)$, shown as a broken curve in figure 2; then it will take up various other configurations as time goes

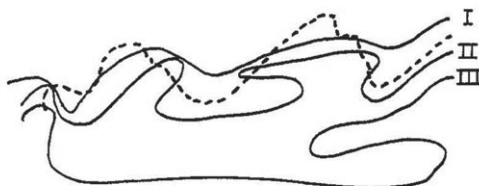


Figure 2.

by, but configuration I for example is a more likely one than configuration II, whilst III is most improbable, it being very unlikely that there is no intervening chain in the region

Figure 3. An extract from Sam's 1967 paper (3) on statistical mechanics of polymerized materials, with figures in his own, unmistakable hand. (Reproduced with permission.)

I found the Institute was virtually giving its journals away, in particular I split the Proceedings of the Physical Society into several parts renamed *The Journal of Physics*, *A*, *B* etc., and after a year charged the same for each section as for the single *Proc. Phys. Soc.* All the commercial firms were doing this, so why not the Institute, whose finances were thus transformed.

Representing the Physical Society, Sam was able to play a founding role in the Condensed Matter Division of the European Physical Society, delivering more members from the UK than from the rest of Europe put together. Sam also joined the Physics Committee of the Science Research Council,⁸ becoming its chairman and therefore also sitting on the Science Board of the SRC; he eventually became the Board's chairman too. The latter position also made him a member of the Council and brought him into frequent contact with his old friend and

⁸ Now the EPSRC, the then SRC funded science research in UK universities and central facilities, including research collaborations and facilities abroad.

colleague, Brian Flowers, who had called him to Manchester, but who was now the Chairman of the SRC.

Fittingly (31), considering the science that would follow, it was Anderson who

came to give a seminar at Manchester and asked me why I showed no interest in The Plummer Chair currently advertised in Cambridge. My view was that Manchester had treated me really well and to apply for a post elsewhere was quite inappropriate. However, if Cambridge really wanted me, it could offer me the Chair. I thought that would be the end of that, but in fact Cambridge did offer me the Chair and after some heart searching I accepted and we (now with 4 children) moved to Cambridge in 1972 where we bought a house at $2\times$ my (standard) salary—(it is now worth $10\times$ a professor's salary; times were really better then) and Gonville and Caius offered me a fellowship which I still have, since after a while a Caius fellowship lasts for life.⁹

CAMBRIDGE, 1972–2015

The first year in Cambridge, 1972–1973, was a busy one with continuing research problems, his students still working on their PhDs moving to Cambridge, and with teaching new courses. An important course was statistical mechanics for the third, and thus final, year students (Part II of the Natural Sciences Tripos). Sam records in his notes (31) that

Mott had instituted the teaching of theoretical physics and I lectured on statistical mechanics and found two students, Jacob Klein and Mark Warner, who followed my lectures in great detail and visited me often to sort out things that worried them (and usually worried me too).

It is fascinating to hear of the encounter from the other side. I certainly recall being worried about the lectures since they presented statistical mechanics in a formidable and uncompromising way, with difficult notions expressed in very difficult mathematics.

A second problem was that the hastily handwritten notes were riddled with errors. I focused my reading and analysis by correcting these errors, as best I could, using red ink which could cover the entire page. Finally presenting Sam with my efforts, I suddenly thought, 'this is not the way to start a relationship with a senior and famous physicist'. Things did not come to an end, but progressed according to most people's memories of scientific interactions with Sam—he respected those who engaged intellectually and valued their engagement, however junior they were. The upshot for me was a PhD place with him in London (see below), and for Jacob Klein an experimental position with David Tabor. Both of us were to become professors in polymer-related research, at the Cavendish and in Oxford, respectively. Sam actually worried with us, we now see.

A third difficulty with Sam's approach to students was to fly through the development of some topic, saying along the way for economy of writing and of time 'Using constants that absorb the units ...'. Now, by this he did not just mean dimensional constants, but also the 2π s etc., which for learners was a minefield. So, everything could be set equal to 1, though he drew the line at $i = 1$. However awful his teaching style could be, Professor Michael Gunn notes (personal communication):

⁹ Sam was a fellow of Gonville and Caius College, Cambridge, until his death. He was president (senior fellow) of Caius for a time before retirement.

Of course that [attitude to detail] summed up an attractive aspect of Sam—complete impatience with irrelevant detail in a drive to get to the kernel of the problem formulated in the most economical manner.¹⁰

Brian Flowers resigned as Chairman of the SRC in 1973 to become Rector of Imperial College. By then, Sam had much experience of the Council and of other administrative responsibilities such as in the IoP and the University Grants Committee. He had been elected FRS several years before (1966) and was a natural successor to Flowers. He was approached for a 4-year (1973–1977) tenure and accepted, Cambridge giving him unpaid leave of absence. He started the new position in September of 1973, assured that the duties would not require five days a week, an assurance so empty that he had to rely on the Cavendish functioning on Saturdays to maintain his research interests. He had negotiated a first-class season ticket on the comfortable, if rather slow, Cambridge–London train, giving him two hours of uninterrupted time each day that he exploited to the full. He had a notebook in which he would write during long business meetings, most people admiring his dedication to detail but, to those with a sharp eye, it was clearly theoretical physics being developed during the duller moments.

An immediate and difficult problem was that the two national high energy accelerators were past their prime. Major parts of such machines are their foundations and shielding and so new machines on existing sites were much cheaper than green field developments. Two major facilities were nationally desirable, a synchrotron radiation source and a spallation neutron source. Both were studied and he secured funding for them. In astronomy, Sir Bernard Lovell planned a major new radio telescope, the initial cost of which seemed reasonable in spite of its vast size (31):

However, I had heard stories from the Atomic Energy Authority of wild escalations of costs in major engineering projects so I set in motion a full costing from its designers who came up with a firm quotation enormously greater than anything HMG would contemplate. The Astronomy Board abandoned the project and I was then arraigned by a Parliamentary Select Committee for ‘nugatory expenditure’ i.e. I had spent £0.5m on a project which was not carried to fruition. Since I had saved the country £15m I thought this was unreasonable, and so in the end did they.¹¹

He moved the UK’s Isaac Newton telescope from ‘its absurd position at Herstmonceaux’ to the Canary Islands, which involved much negotiation with Spain. The UK Infrared Telescope went to Hawaii, which involved somewhat easier negotiations. Sam’s SRC chairmanship was recognized by a knighthood in 1976. What is astonishing about the periods of such onerous and responsible duties outside academia is that Sam remained active at the forefront of science. His originality was undiminished—he established two new fields and applied radical models and techniques to them.

The SRC years taught Sam much about how the UK government works and how to find pathways through rules to achieve an objective, leading him later to serve the government

¹⁰ Michael Gunn also recalls two more aspects of Sam that are in great resonance with what has been described so far: ‘I think the other things I learnt from him were: “not to be frightened of inventing mathematics if you need to”, and “don’t be overpowered by a desire for precision, if you can’t do the integral with the functions that arise, stick something in of the same shape where you can get a feel for what is happening”.’

¹¹ This author recalls the day, after being supervised by Sam during the morning, when Sam said he was off to be roasted at the House of Commons. He did not seem unduly worried and undoubtedly knew he could be fierce in battle with the committee.

as chairman of the Defence Scientific Advisory Council and chief scientific advisor to the Department of Energy.

During the SRC years, fruitful Saturday mornings were spent with Phil Anderson at the Cavendish Laboratory. Anderson shared with Sam his puzzlement about a dilute, random alloy of iron in copper that appeared to have a phase transition, but without macroscopic ferro- or anti-ferromagnetic order in its low temperature state which is reached by some mysterious transition from the high temperature state, which is also random. The metallic electron gas mediates an interaction between spins, here on the iron, that oscillates with distance between the magnetic ions (the so-called RKKY interaction). This means that the coupling J_{ij} between the i^{th} and the j^{th} ions is also random. These materials were quickly named ‘spin glasses’ (SGs) since they were evidently quenched at low temperatures to a random but fixed, that is glassy, phase of spins. Sam introduced (11) an imaginative, new order parameter to describe a random, but frozen phase

$$q = \langle \underline{s}_i^{(1)} \cdot \underline{s}_i^{(2)} \rangle \quad [14]$$

The order parameter represents the correlation of a spin at time t_1 with itself at time t_2 . At high temperatures, $q = 0$ for long enough time differences, but in the SG phase for $T < T_c$, the spins are locked and correlate with themselves: $q \neq 0$. He then gave a Curie–Weiss treatment to obtain q as the solution of $q = \coth(\rho q) - 1/(\rho q)$ with $\rho = 2J_0^2/3(kT)^2$ and J_0 representing the root mean-square value of the J_{ij} . The order is $q \sim (T_c - T)^{1/2}$ near T_c .

Having located the transition, the detailed thermodynamics was required, specifically, the specific heat, with a cusp at T_c , and the susceptibility. One has to average $\ln(Z_{\{J_{ij}\}})$ over realizations of the random fixed $\{J_{ij}\}$ distributed with a probability that was taken to be Gaussian with variance J_0 . Of course, replicas were needed for the $\langle \ln(Z) \rangle_{J_{ij}}$, as was a variational fit to quadratic forms in exponents of the less tractable quartics that arose from averaging. The self-correlation q was now between replicas α, β of the same realization of the $\{J_{ij}\}$, rather than the spins at different times, but takes the same sense $q = \langle \underline{s}_i^\alpha \cdot \underline{s}_i^\beta \rangle \neq 0$ for $\alpha \neq \beta$ when $T < T_c$.

I recall at the time there being almost shock at the direct, some would say brutal, analysis that led to such concrete results in a little-known, deeply mysterious field with a new phenomenon. For most researchers, this was their first exposure to the replica method, and certainly to Sam’s style of scything through intractability. The SG papers were then, and subsequently, widely studied, provoking much theory, development of the replica trick and experiment. Sam recollected (31):

I recalled my paper on network dynamics which avoided the replica method and worked through the spin glass problem dynamically and EA-II [(12)] agreed completely with EA-I. As a bonus one could see that the total number of states of the spin glass is properly extensive. A final paper [(13)] suggested to me by Phil, was to do the calculation in quantum theory, modelling the iron ions as rotators. This came up with the interesting though quite believable result that quantum fluctuations can wash out the spin glass transition according to the value of the moment of inertia of the model ion. (EA-I had thousands of citations, EA-II a few and EA-III had essentially none: An unappreciative world!).

Sam continued his SG work in the late 1970s with F. Tanaka with whom, for instance, he calculated the ground state degeneracy in the Sherrington–Kirkpatrick (SK) model of spin glasses. This model, proposed by his former student David Sherrington, has been widely applied to economics, machine learning, simulated annealing and so on. He used the same



Figure 4. Edwards and Doi at Sam's retirement meeting, nearly 20 years after their initial collaboration started. Reproduced by courtesy of Professor Doi. (Online version in colour.)

replica methods to solve the random matrix problem to obtain the classical semi-circular law for the density of eigenvalues in the presence of noise (15), a method that could be generalized to find the effect of such disorder on any initial density of states (21).

A major achievement of network theory was the full treatment in a general replica space of entanglements, excluded volume, assumptions of classical theory, boundary conditions, etc. In a monumental paper with Deam (14), Sam set out all the major problems in networks and then rotated to new bases in replica space, breaking symmetry and laying the ground work for others to follow in SG theory. Work continued in networks, for instance the representation of sliding knots by 'slip rings' constraining a chain and sliding to the next crosslink as deformation is imposed (23). Sam felt that this paper gave the least trivial results in the literature, and was most closely related to experiment.

Sam concluded this major intellectual phase as other important problems inspired him (31):

I decided that so many clever people had gone into spin glasses that the time had come to leave this and localization and stick to my latest adventure with a collaborator of tremendous ability. This was Masao Doi. Again Geoff Allen hosted him at Imperial at first until we both went on to Cambridge. Although de Gennes had made a breakthrough in the idea of reptation, he did not follow it on to a full theory of visco-elasticity. This, I suggested to Doi, could now be done and he started on it with great enthusiasm and skill, and during my last year in London, wrote a series of four papers leaving me breathless keeping up with him and casting me very much in the role of the experienced man who advised and revised.¹²

Doi was already experienced in the field, having published in 1974 a paper on the implications of de Gennes' tubes, representing entanglements, for visco-elasticity (Doi 1974; figure 4).

We are fortunate to have the notes of the encounter where the problem was fleshed out for the first time (figure 5). A full theory of the melt rheology of flexible polymers was developed in four papers directed respectively at Brownian motion in the equilibrium

¹² Geoff Allen was Sam's successor as Chairman of the SRC.

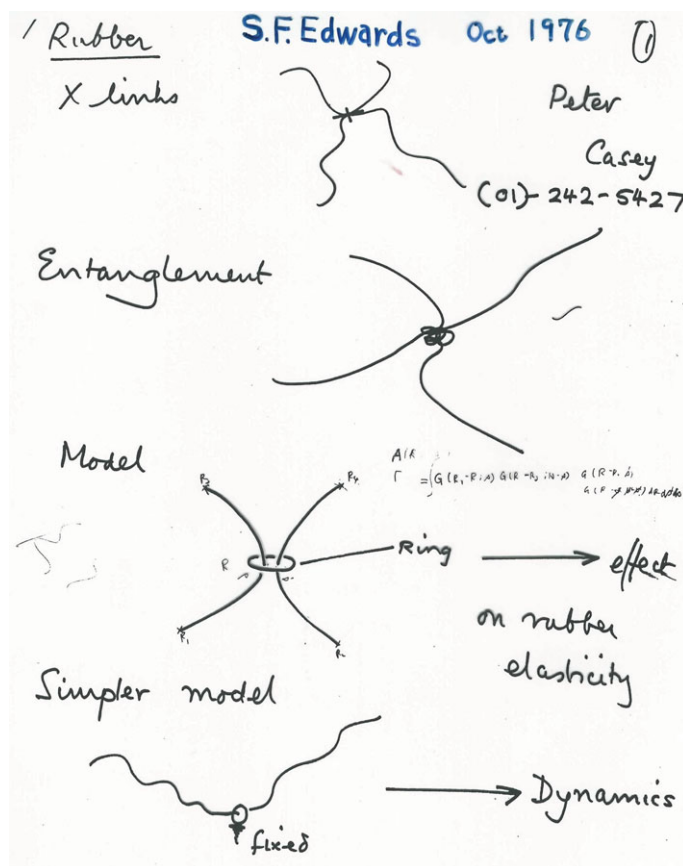


Figure 5. Notes, kept by Doi, sketched by Sam during their first encounter. Peter Casey was one of Sam's private secretaries. In the top right, Sam is noting for Masao how meetings will be arranged. Reproduced with thanks to Professor Doi.

state (16), molecular motion under flow (17), the constitutive equation (18) and rheological properties (19). Two more papers with Doi were directed at the dynamics and rheology of rods. One needs ideas of tubes for the constraints, and the related concept of the primitive path meaning the path taken by a chain were it to be pulled tight from its ends. Also central are the motion of chains and thereby their disengaging with the tube and releasing stress, the loss of the tube's macroscopic deformation as the chains that make it up are themselves disengaging, and finally the calculation of the resultant stresses as functions of time, of imposed strain history and of strain geometry. The achievement of these goals in this grand scheme was a major triumph. The work of Doi and Edwards underpins modern polymer dynamics generally and rheology in particular, the latter being at the heart of one of our principal technologies—the production, processing and moulding of plastics. The papers had the intellectual impact of the SG papers and their wider impact was even more considerable. There followed a body of work, over the succeeding 40 years to the present, of extensions and refinements of the

basic ideas—tube renewal, the motion of stars and pom-poms, how polymers that can reform behave, and so on. Experiment has been very sophisticated and seriously testing, offering differing flows, complex dynamically-imposed strains, the use of tools beyond rheology such as NMR, field gradient NMR flow realizations and single chain dynamics and birefringence, as well as the testing of predictions in practical applications. With its extensions, Doi–Edwards theory is undoubtedly one of the most successful and influential physics theories of the second half of the twentieth century. It was the synthesis and crowning achievement of Sam’s many polymer works, going back to 1965, which set out and solved a hierarchy of problems that were ultimately also very important to technology. In 1985, following another extended visit by Doi to Cambridge, they published their monograph (26) on the subject:

It is a really superb monograph. Physics relies so heavily on such books that explain what is important in a precise and readable fashion. Even if Sam had never published anything else, his contribution to the community through that book would be worth a 100 careers of lesser mortals.¹³

Brian Pippard resigned from the Cavendish Chair in 1982 and Sam was appointed to that position in 1984. On his appointment, Sam took on the role of head of department for the next 5 years. Unlike Pippard, Sam had been deeply involved in national and international science politics for many years, as discussed above. He had a very wide range of contacts in government and industry and used these and his experience of government to begin a major expansion of the scope of the Laboratory’s activities to remarkable effect.

During the Pippard era, the number of staff members remained roughly constant. New initiatives were needed and this was brought about largely through Sam’s vision during his term. The funding pressures on the university, with the gradual erosion of support for research and the universities, meant it was a major challenge to find the means of significantly increasing the numbers of academic posts, despite the ‘new blood’ scheme initiated by the government to regenerate research and teaching activity within universities.

Sam fully appreciated the gravity of the situation. He realized that government and the research councils could not be relied upon to provide the resources for new activities. Rather, the way to do new things was to become much more closely associated with the needs of industry and to enhance the support it could provide to the research programme. This was also attractive to government, who were keen to promote research that would be of benefit to industry. Often, matched funding from the research councils and government could be obtained, as well as studentships through a variety of incentive schemes. During Sam’s 5-year period as head of department, new groups were created, in Microelectronics led by Haroon Ahmed (1983), in Semiconductor Physics led by Michael Pepper (1984), Optoelectronics by Richard Friend (1987), Polymers and Colloids by Athene Donald (1987) and in the Interdisciplinary Centre for High Temperature Superconductivity, a collaborative effort between a number of departments (1987). These initiatives grew largely out of the activities of the Physics and Chemistry of Solids Group, much of the stimulus being provided by Abe Yoffe in encouraging many of the best graduate students to exploit the opportunities for innovation in these new disciplines. All of these new activities had strong industrial connections and resulted in a major increase in the staff of the Cavendish Laboratory, mostly through research posts funded by industry and the research councils.

¹³ Professor David Williams, Australian National University, private communication.

During the 1980s and 1990s, Sam continued, alone and with co-workers, to write on further aspects of networks and dynamics, but Sam's influence would extend to two more major areas. Foods are frequently networks, or even weak glasses, and Sam saw the potentiality for physics to understand the big challenges of a major technology. He had input both for academics, by starting a major research commitment at the Cavendish Laboratory, and for industry; for instance, he was an international consultant and advisor at Unilever.

Foods are also often foams, either empty or filled with liquid, with either closed or open cells as in meat, apples, cornflakes, bread. The modulus, failure and dynamics of such foams determine, among other things, feel, freshness, edibility and release of flavour. Filling with fluid can turn cell wall bending in response to imposed strain into stretching, and hence a modulus change of the material. This crossover from classical bend foams to newly-considered stretch foams changes the scaling of properties with the density of cell wall material. There can be fluid-induced fracture and/or flow in response to deformation—the induced pressure rise is also complex. Such properties were developed in a paper (27) that would later find application to foods.

Powders and granular assemblies are the last major areas to which Sam gave a modern, theoretical physics impetus. First, with his student Wilkinson, came powders in the sense of random deposition of particles on a surface with their rolling to stable locations. Their aim was for the entire statistical and dynamical specification of the surface. Their 1982 paper (24) is now widely cited as a start of a new topic with an extension to non-linearity proposed by Kardar, Parisi and Zhang. Sam did not think that the KPZ extension described powders any better than his and Wilkinson's did, but theirs was 'an attractive mathematical problem, rather similar to Navier–Stokes turbulence, but without the difficult convergence problems of hydrodynamics' (31).

He later returned to KPZ with Moshe Schwartz (28, 30). The dynamics of surfaces was revisited, for instance with Bouchaud and other colleagues (29). There were other papers with Wilkinson that set new directions and of which Sam was very fond; for instance, on the understanding of 3-D packing of an assembly of grains working backwards from slices though it (stereography) (22, 25), and on the dynamics of smaller grains diffusing as they bounce through a fixed assembly of larger grains.

Packing indeed plays a central role in the mechanics of granular assemblies: (i) it determines contacts and the transmission of forces between grains through arches—in general paths with a lower dimensionality than the assembly of grains; (ii) it leads to jammed or arrested states that are determined by marginal stability; and (iii) it gives large numbers of microscopically different states with the same macroscopic character, inviting the use of statistical mechanics to describe collective properties, albeit in these unpromising non-ergodic systems. Bouchaud and Cates, in an incisive review (2004), regard these three features as Sam's seminal and founding insights for the physics of granular materials. But, first in Sam's words (31):

I share an office with the other physics professors in Caius College one of whom is David Tabor. One day he came in and said he had a question: if you measure the pressure executed by a sand pile on a planar surface, where will the maximum pressure come? Now I always think of related physical systems to the one in question to see if any are simpler. I know that a heap of twigs could have its middle removed and not collapse, and indeed one could do this also with gravel. All systems can form arches, sometimes enough to cover caverns, but always such that the stress is sent outwards. So I answered, the pressure is zero at the edges, rises to a

maximum and reaches a minimum under the apex. David replied that that was what Brian Briscoe at Imperial had indeed found and was looking for a simple explanation. I soon realized that instead of sand a pile of coins could be solved analytically under some very reasonable assumptions, and given exactly the same result as above. I was joined at this time by Robert Oakeshott who was interested in packing problems and after a brief conference paper we set about a general theory in which some mechanism of shaking or tapping allowed the grain to explore all the different configurations they could take, subject to a total volume V , and the number of ways this could be done led to an entropy, S , and therefore to an intensive variable $X = \partial V / \partial S$ which I named the compactivity X . The reversible accessibility of granular states whose existence is essential for the compactivity concept to be valid was found by the Chicago group of Syd Nagel and since then many investigators have been able to confirm the ideas in simulations.

In the period 1989–2008, Sam produced 51 papers on the ‘statistical mechanics’ of granular material mechanics, 13 of them as sole author and one of those being at the age of 80.

Sand and soil are classic subjects of engineering mechanics, where they tend to be treated within classical continuum elasticity and plasticity, which in this area are subject to some doubt. The connection between local forces and macroscopic stress is not unique, there is no constitutive relation, and the equilibrium state is undetermined. Sam assumed grains would settle until there was a marginal coordination, meaning a minimum number of contacts, and then move no more. But the state is fragile—a sufficient tap could lead to a large reorganization, at least in frictionless systems—the jury is perhaps still out for grains with friction. A small tap leads instead to small rearrangements of the inter-grain forces, a ‘random walk in force space’—Sam’s compactivity is an analogue of temperature, $T = \partial E / \partial S$, in a micro-canonical ensemble, and likewise depends on a uniform measure of states in the Edwards ensemble. Is the measure indeed uniform? One is in new territory with such arrested systems that lack the microscopic reversibility of thermal systems. Granular mechanics from this physics view point has become a huge field started by a man in his 60s, and continuing a long career of founding new fields. His bold step of applying the notions of statistical mechanics to granular materials has led to an avalanche of papers on the subject (32).

In 1995 Sam retired from the Cavendish Professorship (figure 6). About 100 of his closest colleagues, many of them former students, met in Corpus Christi College, Cambridge, to enjoy splendid lectures, dinners and discussions within and beyond science. One life-long colleague was Pierre-Gilles de Gennes whose own work in polymers was so close to Sam’s. For many years, and even well into his own retirement, de Gennes would visit Cambridge a few times a year. There would be sumptuous dinners in Caius, or more intimate dinners in the Edwards’ home. If there was rivalry, it was purely intellectual, with the greatest of warmth in the private sphere. Sam firmly declined de Gennes’ suggestion to those organizing his retirement meeting that we collectively present him with a hot air balloon journey. He opted rather for the complete works of J. S. Bach on CD, and a large collection of video discs of opera—two life-long loves of his that were perhaps kindled, as we have seen, during his Harvard time.

In 2016, three long-standing colleagues of Sam organized the first of an annual series of Edwards Symposia in his memory at the Isaac Newton Institute in Cambridge. About 100 friends and colleagues attended and about 100 others, unable to attend, were there in spirit. Many sent this author insistent messages not only about Sam’s pivotal role in their science, but also about the encouragement and generous recognition he had always given them—from



Figure 6. Edwards and de Gennes at Sam's retirement meeting, September 1995. (Reproduced by courtesy of Professor Doi.) (Online version in colour.)

their starting point as students, and following on during their careers. His personal affection, both from him and given in return, was a consistent theme expressed to me.

The aims of this series of meetings reflect those of Sam over many decades—the exploration through theoretical physics of complex phenomena in, broadly-speaking, soft matter. An essential element is the translation of the insights gained to the problems of industry. But equally important, as in Sam's own science, is the identification of and posing of problems by industry for theoretical physicists. The attendees at the first Edwards Symposium were from both camps and the meetings are as much to build links to industry as to advance fundamental science. Sam would have been pleased by this conjunction.

AWARDS, HONOURS AND APPOINTMENTS

Awards

- 1974 Maxwell Medal for Theoretical Physics, Institute of Physics
- 1982 Ford High Polymer Prize, American Physical Society
- 1984 Davy Medal for Chemistry, Royal Society
- 1986 Gold Medal, Institute of Maths
- 1987 Guthrie Medal, Institute of Physics
- 1990 Gold Medal, British Society of Rheology
- 1993 LVMH Science pour l'Art Prize (Paris)
- 1995 Boltzmann Medal, International Union of Pure and Applied Physics (IUPAP)
- 2001 Royal Medal, Royal Society
- 2005 Dirac Medal, International Centre for Theoretical Physics (ICTP), Trieste

Honours

- 1975 Knighthood
 Honorary Degrees from the universities of Loughborough, 1975, Edinburgh, 1976, Salford, 1976, Bath, 1978, Birmingham, 1986, Strasbourg, 1986, Wales, 1987, Sheffield, 1989, Dublin, 1991, Leeds, 1994, Swansea, 1994, East Anglia, 1995, Cambridge, 2001, Mainz, 2002 and Tel Aviv, 2006.
- 1996 Honorary Fellow of the French Physical Society
- 1997 Honorary Fellow of the Institute of Physics and the French Physical Society
- 1997 Honorary Member of the European Physical Society

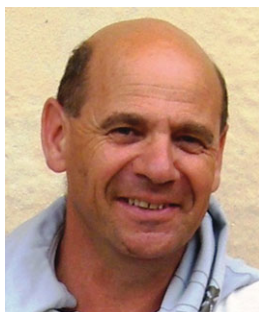
Appointments

- 1966 Fellow of the Institute of Physics, the Royal Society of Chemistry, the Institute of Mathematics and the Royal Society
- 1970–1973 Vice President, Institute of Physics
- 1980–1981 President, Institute of Mathematics
- 1982–1983 Vice President, Royal Society
- 1987 Foreign Member of the Russian Academy of Sciences
- 1989 Foreign Member of the Académie des Sciences
- 1996 Foreign Member of the National Academy of Sciences, USA

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I am grateful to many colleagues for their recollections of Sam Edwards. The principal sources are given in the text. Permissions to reproduce extracts from Sam's work and photographs of him are also acknowledged in the text. The portrait was taken in around 2000 and provided by the family.

AUTHOR PROFILE

Professor Mark Warner FRS

Professor Mark Warner FRS, Cavendish Laboratory, Cambridge, knew Sam during his time as an undergraduate, as his graduate student, while a research fellow and later as a faculty colleague, from 1972 until his death in 2015. Mark Warner is one of the founders of the field of liquid crystal elastomers, which has yielded many exotic phenomena that are now confirmed experimentally. For this he received the Maxwell Medal and Prize of the Institute of Physics and a von Humboldt Research Prize. In 2003 he was awarded the Agilent Technology Prize by the European Physical Society (the former Euro Physics Prize).

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