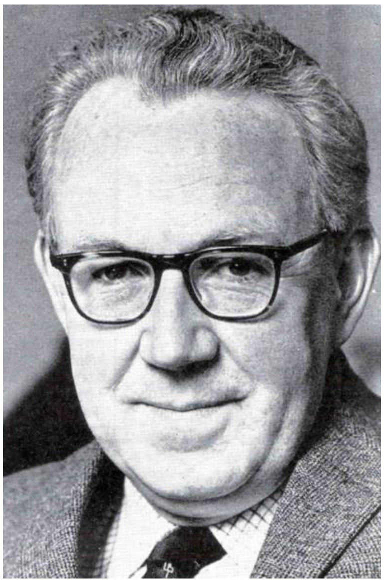
Sam Edwards

Born 1928. Professor of Physics.
Available online at www.livesretold.co.uk



Contents

1. Introduction

The following chapters were extracted and archived in 2021, with acknowledgement and thanks, from the website of the Royal Society at www.royalsocietypublishing.org. The author of the biography from which these are extracts was Professor Mark Warner. The text in italics (with tinted background) was written by Professor Sam Edwards himself.

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.

---

2. Wales and Cambridge

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.

So begin Sam's private reflections on his own life. 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 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 $n \to 0$ 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 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 ${}^{8}_{4}\mathrm{Be}$, 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.

Sam Edwards with his wife Merriell. (Reproduced by courtesy of the Edwards family.)

3. Harvard and Princeton

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 Green[2] 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 enrichening 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.

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.

---

4. 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.

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:

> 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.

> 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.

5. 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 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, 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

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:

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

Fittingly, 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 brought a house at 2 × my (standard) salary—(it is now worth 10 × 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.

6. 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 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 2πs 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):

> 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.

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:

> 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.

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 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.

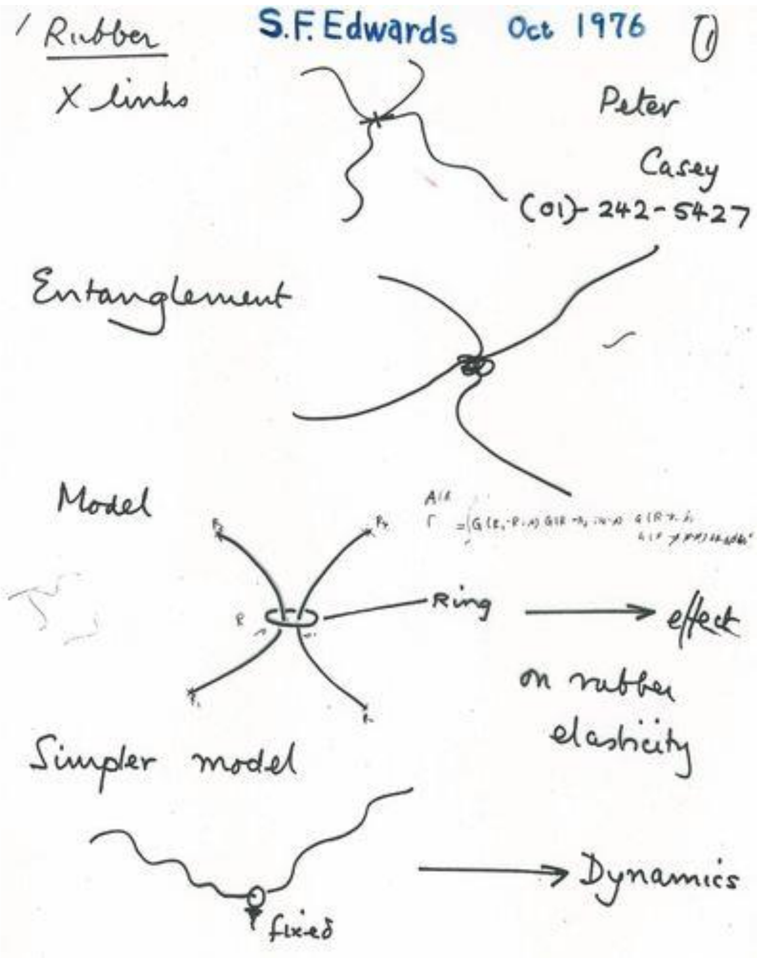
Sam continued his SG work in the late 1970s with F. Tanaka with whom, for instance, he calculated the ground state degeneracy in the Sherrington Kirkpatrick (SK) model of spin glasses. This model, proposed by his former student David Sherrington, has been widely applied to economics, machine learning, simulated annealing and so on. He used the same 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, a method that could be generalized to find the effect of such disorder on any initial density of states.

Edwards and Doi at Sam's retirement meeting, nearly 20 years after their initial collaboration started. Reproduced by courtesy of Professor Doi.

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, 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. 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:

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.

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.



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.

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

Brownian motion in the equilibrium state, molecular motion under flow, the constitutive equation and rheological properties. 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 on the subject. Of it, Professor David Williams, of the Australian National University, wrote;

> 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.

7. Appointment to the Cavendish Chair

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.

8. Foods and Foams

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 that would later find application to foods.

9. Powders and Granular Assemblies

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 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’.

He later returned to KPZ with Moshe Schwartz. The dynamics of surfaces was revisited, for instance with Bouchaud and other colleagues. 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), 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:

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.

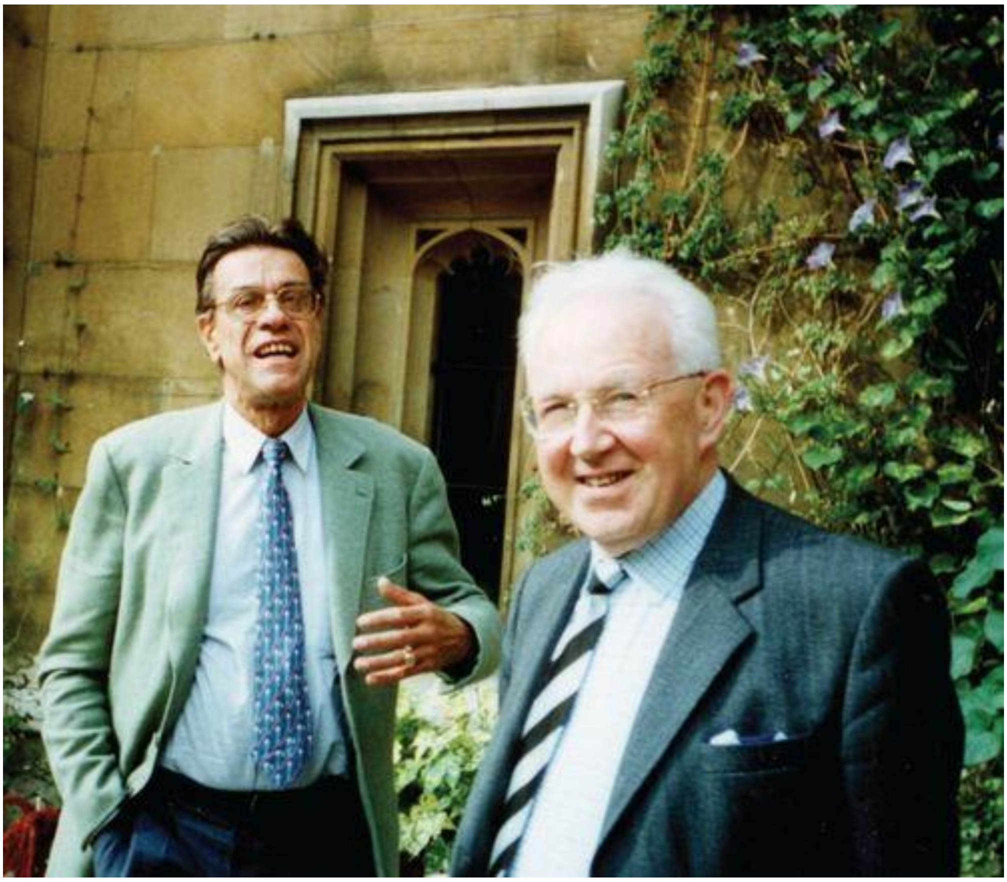
Sand and soil are classic subjects of engineering mechanics, where they tend to be treated within classical continuum elasticity and plasticity, which in this area are subject to some doubt. The connection between local forces and macroscopic stress is not unique, there is no constitutive relation, and 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.

---

10. Retirement

In 1995 Sam retired from the Cavendish Professorship. 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.



Edwards and de Gennes at Sam's retirement meeting, September 1995. (Reproduced by courtesy of Professor Doi.

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 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.

X. Awards, Honours and Appointments

Awards

1974. Maxwell Medal for Theoretical Physics, Institute of Phyics.

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.

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.

197. 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.

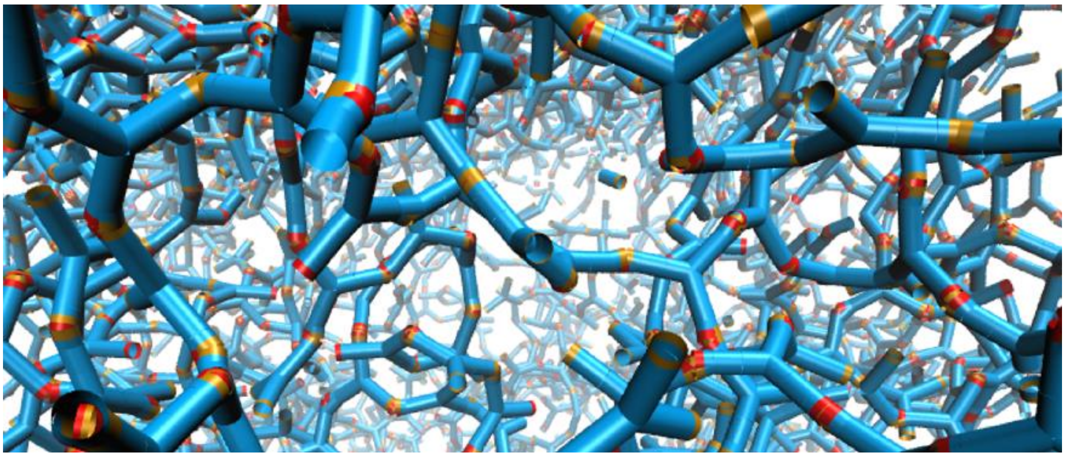
---

12. Author profile

Professor Mark Warner FRS was the author of the Royal Society obituary from which extracts have been archived, with acknowledgement and thanks, above.

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).

13. The Edwards Centre



A network of DNA nanostars.

A Virtual Laboratory

The Edwards Centre is a virtual laboratory for Soft Matter Research at Cambridge University. Its membership is drawn from academic departments across the University, including Physics, Chemistry, Applied Mathematics and Theoretical Physics, Materials Science, and Chemical Engineering. It is named after Professor Sir Sam Edwards (1928-2015). Sir Sam was one of founders of soft matter science, making fundamental contributions to the theory of entangled polymers, granular materials, and many other forms of soft matter. He was the Cavendish Professor of Physics from 1984 to 1995. The Edwards Centre was founded in 2016.

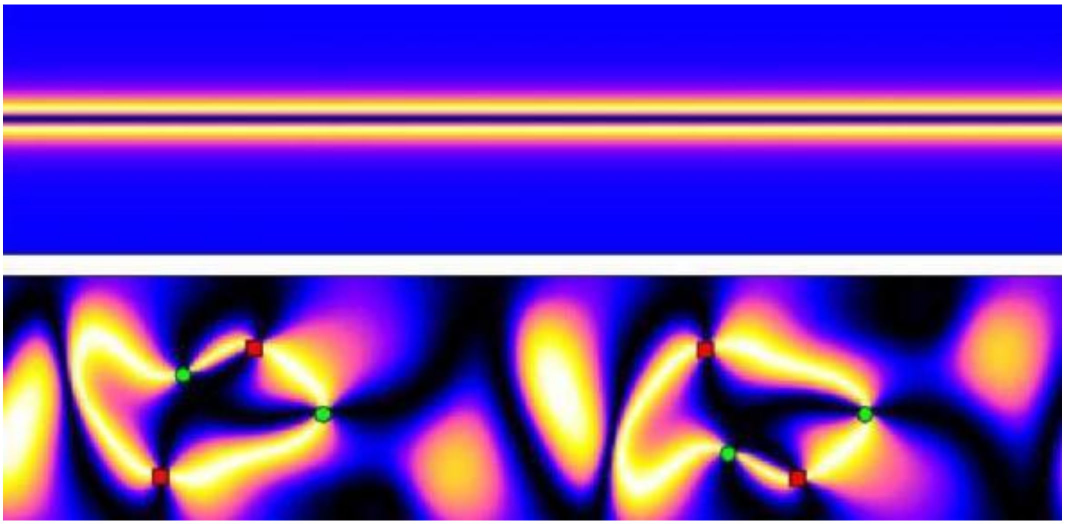
Members of the Edwards Centre for Soft Matter use experiments, theory and simulations to address the science of soft matter systems. Such systems arise in many industrial contexts, such as: foodstuffs, personal care products, paints, energy materials, and display devices. They also arise in many biological contexts.

Materials studied include:
Colloids and Suspensions
Polymers and Gels
Molecular Aggregates
Amphiphilic Systems
Granular Materials
Liquid Crystals
Biological and Biomimetic Materials
Microswimmers and Active Matter

An increasing number of the Centre’s projects address composite materials, that mix several of the above components to create new functionality in:

Soft Composites
Soft Nanomaterials

The Edwards Symposium Series



One of the activities of the Edwards Centre is the Edwards Symposium Series. The 2019 Symposium is described below:

2019 was the fourth year in the Edwards Symposium Series, funded in part by continued generous support from Unilever.

This symposium series is named after the renowned Scientist Sir Sam Edwards FRS, who was a pivotal figure in bringing advances in the physical sciences and translating them into end user applications for industry. Sir Sam's contributions to the field of soft matter ranged from polymer melts, through gels, colloids, granular materials and glasses to optimisation problems.

The Edwards Symposium Series recognises the fast evolving and diverse nature of soft matter science and each year focuses on different areas of new and emerging science.

Relevant to many industrial and biological systems, soft matter is pivotal to a wide range of disciplines and has led to innovative materials and processes for industry while also posing new fundamental problems. The Symposium builds on Sir Sam's realisation that broad classes of soft materials are governed by unifying physical principles, arising from the geometry, topology and qualitative behaviour of their microscopic components, regardless of their detailed molecular or chemical character.

Aims & Objectives

The Edwards Symposium Series highlights the latest developments in soft matter science with a particular (but not exclusive) emphasis on theoretical and mathematical models, and on how these models can inform industrial processes, materials, and design. Leading academic speakers convey their latest scientific work, aiming to foster collaborative and interdisciplinary discussions across the industry/academia boundary.

In 2019, the workshop focused on the following soft matter areas:

New perspectives on detergency
Active and driven phase separation
Rheology of dense suspensions
Environmentally sustainable plastics

These themes posed fundamental questions in basic science that were addressed by distinguished academic speakers. Their industrial relevance was reflected by the prominence at the Symposium of industrial participants whose oral presentations, posters and informal discussions informed the discussions with the hope of leading to concrete future collaborations, of benefit to both sides.