JOHN MICHAEL ZIMAN

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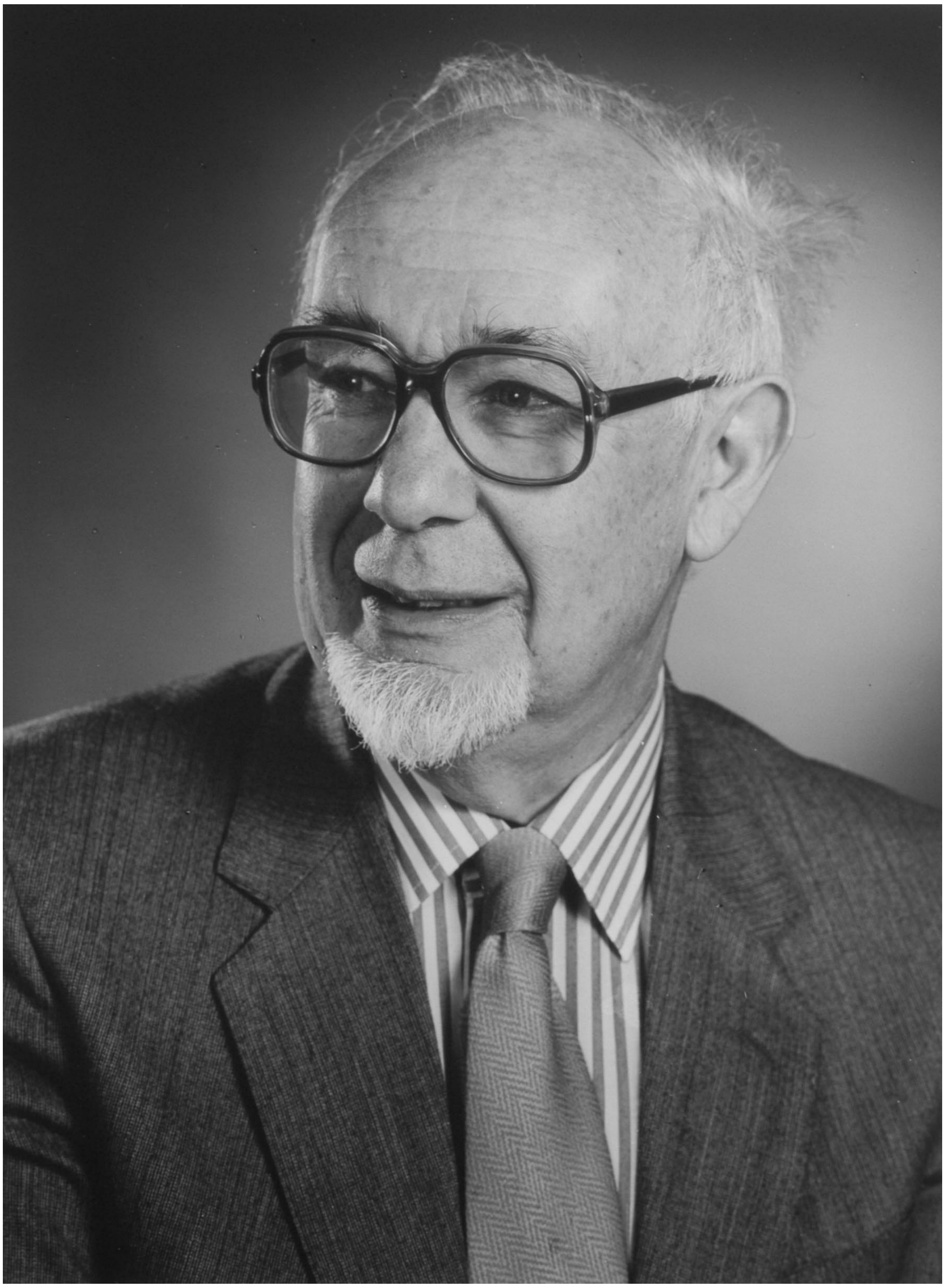

J M. Ziman

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Elected FRS 1967

By Sir Michael Berry FRS and John F. Nye FRS

H. H. Wills Physics Laboratory, University of Bristol, Tyndall Avenue, Bristol BS8 1TL, UK

John Ziman was a theoretical physicist whose work was characterized by its clarity and simplicity and was always firmly grounded in experimental reality. He developed and refined the application of quantum mechanics to the transport properties of crystalline solids, and pioneered the quantum theory of disordered solids and liquid metals. He served as Head of the Physics Department at Bristol University, and created the theoretical physics group there. In many influential books and articles he broke fresh ground in his studies of science as a collective human enterprise.

Family background and early life

In family background John Michael Ziman stood at the junction of two strong Jewish traditions. His father, Solomon Netheim Ziman, was born of Polish, Ashkenazi parents who emigrated to New Zealand. Solomon had obtained a first-class degree in mathematics at Balliol College, Oxford, as a New Zealand Rhodes Scholar, and went on to rise to high rank in the Indian Civil Service. John describes him as a man of strong, clear, practical intellect and great purity and integrity of character, a man of affairs rather than an intellectual, who maintained an interest in science although he never did research. John's mother, Nellie Frances Gaster, came from an English Sephardi tradition, well-to-do and crossed with a brilliant Romanian line with a strong intellectual and cultural background. Her father, Angel Gaster, had been born in Romania and came to England as a young doctor, becoming a well-known Harley Street physician; he was one of the founders of the London Jewish Hospital. His two brothers were also distinguished intellectuals: Moses Gaster, the 'great man' of the family, famous as a scholar, became chief rabbi of the British Sephardi community and President of the Royal Asiatic Society, while Leon Gaster was an engineer and founder of the science of Illumination Engineering. John's mother

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studied for the Cambridge Natural Sciences Tripos, Part I, and married soon afterwards. Although she ceased to show any active interest in science, John describes her as a great reader, having inherited her father's liveliness and energy and interest in people; 'She was always, I think, much less censorious than my father, who had very high standards of behaviour, and she was unwilling to be more than amused and astonished at other people's folly and wickedness'. Thus John summarizes his cultural heritage as strongly bookish.

Although he was born in Cambridge, England, the eldest of two brothers and one sister, John was taken by his parents to New Zealand as a baby and lived there until he was 21 years old. The family had a farm near Cambridge, New Zealand, and then moved to the small town of Hamilton, a few miles away, where they lived comfortably on his father's pension and his mother's private income. The house was always full of books, sent out from England, and they read *Punch*, the *Illustrated London News* and the *New Statesman*. John was a voracious reader, always interested in science—they also had the *Children's encyclopaedia*, which he explains he must have almost known by heart by the time he was about 10 years old. He recalls the year 1933 spent in London and the joy of the Science Museum. Being brought up on a farm in New Zealand gave him a love of the open air; he was far from any Jewish congregation, and scarcely aware of anti-semitism. He writes, 'we "kept" Friday nights and Passover, but otherwise were entirely emancipated. My home life, as a child, was entirely happy—at least as far as good, loving and thoughtful parents could make it. It was simple, orderly, high-minded, liberal, tolerant, argumentative, noisy, practical and sensible—perhaps unromantic and unpoetic, but safe and secure.'

In Cambridge, New Zealand, he first went to the local village school, where he was top of the class in all subjects, and then on to the local grammar school of Hamilton. In 1941 he won a university entrance ('Junior') scholarship, being second in New Zealand, but being only 16 years old was judged by his parents to be too young to go to university. So he spent a third year in the sixth form studying, among other things, Hardy's *Pure mathematics*. He is complimentary about all his teachers except for his science teacher, who did not inspire him. He was grateful that he was not pushed scholastically and was able to get a good grounding in literary studies as well as mathematics and science.

Victoria University College, 1943–46

In 1943 he entered Victoria University College, Wellington, New Zealand, and in 1946 gained an MSc degree with first-class honours in physics. The final year was devoted to experimental research—in fact on the reflection coefficient of the ionosphere—as well as more mathematical courses. He remembers that his strongest influence came not from his lecturers but from a contemporary student, G. S. Bogle, with whom he argued about many mathematical and physical problems. A Long Vacation job at the Animal Research Station, Ruakura, Hamilton, on assaying pig faeces for chromium oxide (added to their diet to try to check their food intake) led to a serious interest in statistical methods, and by the time he came to his MSc thesis he was familiar with laboratory work. Nevertheless, he had learned also that mathematical physics was his real love. Thus, when he went to Oxford in 1947 it was with the deliberate intention of reading mathematics 'properly' and becoming a theoretical physicist. In fact there was a short interlude during which he worked at the Building Research Station, Garston, near Watford, where he found the work to be 'almost a parody of civil service science at its most pedestrian'.

OXFORD, 1947–54

By going to Balliol College, Oxford, as an undergraduate John Ziman was perhaps following his father's example. He was sufficiently ahead of the work to be able to read a great deal of theoretical physics, such as Dirac's *Quantum mechanics*, which was not in the curriculum. His tutor was J. S. Wet; they became excellent friends. John found Balliol an interesting contrast to Victoria University College, where they had spent much time talking physics and mathematics; at Balliol the talk in the Junior Common Room was of philosophy and politics. Although John much enjoyed it, he claims that he never became prominent in college or university life, finding it pleasant to remain a little anonymous. In New Zealand he had become a member of the Communist Party—it was during the war when Russia was the gallant ally—but he found orthodox Marxism very difficult to swallow and never convinced himself that he believed in it. Political events suddenly opened his eyes 'to the folly of it all' and before going up to Oxford he left the party for good. However, he did later become involved with the International Union of Students and then with the National Union of Students, where he was active in the work of the Oxford Committee. In 1949 he received a degree with first-class honours in mathematics.

On 14 September 1951 John married Rosemary Milnes Dixon, who had taught in girls' boarding schools before becoming an art student at the Ruskin School in Oxford, where John met her. She was an active member of the Church of England and they adopted four children: Clare Elizabeth, Matthew John (who died in an accident in 1985), Gregory Phillip and Katharine Mary. John described Rosemary as a strong enough character not to allow him to dominate her, and wrote (1967) that the marriage had been an entire success.

In October 1949 he had become a research student at the Clarendon Laboratory, Oxford, and received a DPhil in 1952. Initially his supervisor was G. S. Rushbrooke (FRS 1982). After starting on a different problem he switched to the general topic of antiferromagnetism, working essentially on his own but benefiting from the advice of K. W. H. Stevens. His first paper, published in 1951, began 'Some recent experiments…', heralding what would become a theme of much of his work, namely the close connection of theory to observations on real materials. This led to a junior lectureship in mathematics (1951–53) and a research fellowship from the Pressed Steel Co. Ltd. Having finished his DPhil he consulted M. H. L. Pryce FRS, then Professor of Theoretical Physics, saying that he wanted a new field, and on his suggestion talked to F. E. (later Sir Francis) Simon FRS, who introduced him to his low-temperature group. John then produced several theoretical papers on liquid helium, lattice conduction and electrical conduction in metals, for despite much excellent experimental work the Clarendon had no theorists on these topics.

CAMBRIDGE, 1954–64

In 1954 he moved to Cambridge as a lecturer in physics, and was appointed a Fellow of King's College. There his Oxford work on conduction blossomed into a systematic study of the transport properties of crystalline metals. In a series of papers he demonstrated that to account for experimental data it was necessary to incorporate many effects, including: the scattering of electrons by phonons (quantized lattice vibrations); the scattering of phonons by electrons; the roughness and dimensions of crystals; averaging over orientations for polycrystals; and scattering by randomly distributed isotopes. The flavour of his work in this period is captured by his comment on the effect of including phonon–phonon scattering: ‘So, in fig.5 of Walker and Fairbank’s paper, we simply raise the theoretical curve labelled “Eq.3” by a factor of 55—which takes it almost exactly into the middle of the observed data.’

To summarize this research, and to provide a modern account of the transport properties of solids, he wrote *Electrons and phonons* (1)*. The preface gives a beautiful statement of the philosophy that was also to inform his three later physics books:

> Like a chemical compound, scientific knowledge is purified by recrystallization. When first published, each new grain of fact or theory shines from a mud of irrelevant or erroneous details. In subsequent discussion the grains are redissolved, and filtered. Finally, in books and treatises, the solution is allowed to precipitate into a single crystal where each atom seems inevitably to be in its proper place. The writing of books is thus as much a part of the scientific process as watching oscilloscopes or solving differential equations.
>
> Individual scientific facts are the leaves and twigs of a great tree. They must be connected downwards, into smaller and larger branches, into the limbs, and then into the trunk itself. To visualize the tree, we must see the connexions. At each major fork, we need to comprehend in sufficient detail all that is borne above it. But a unified picture can only be made by one person comprehending the whole scene. … The recent tendency has been … numerous, short, review articles, in which the whole picture is as clear as in a jumbled jig-saw puzzle in which each piece is painted by a different artist. There is need for treatises covering, in reasonable detail, up to the level of active research, the major branches into which the subject has divided.

As elegantly argued at the beginning of chapter 1, the emphasis was on a collective, rather than a particulate, model, regarding ‘solid matter as a gas of excitations’:

> It is, at first sight, remarkable that any influence can travel through a solid body. We can imagine the passage of fast projectiles, such as energetic neutrons, tearing their way through the crystal lattice. … But besides such processes … there exist the transport properties, in which heat, electricity, and matter itself are carried through the structure, under the gentle influence of a gradient of temperature, of electric field potential, or of atomic concentration. If we insist on a particulate, electronic theory of electricity, the high conductivity of metals … is exceedingly difficult to explain. The electrons must penetrate through the closely packed arrays as though these scarcely existed. It is as if one could play cricket in the jungle.

After *Electrons and phonons*, his interest shifted towards the more subtle aspects of metals, in which the Fermi surface and its shape play a major part. The aim of a systematic study of the nearly-free-electron model was ‘to explore a simple hypothesis: the differences between the alkali metals in their transport properties are to be attributed solely to differences in their electronic structure, especially to different amounts of distortion in their Fermi surfaces’. For the noble metals, the nearly-free-electron model failed, and a more radical approach was needed, taking account of the topology of the Fermi surface.

John’s work on solid metals led to his textbook *Principles of the theory of solids* (5):

> There is a profound difference between a treatise and a textbook. A treatise expounds; a textbook explains. … This book aims to present, as simply as possible, the elements of the theory of perfect crystalline solids. It is a book full of *ideas*, not facts. It is an exposition of the principles, not a description of the phenomena. … What I have tried to do is give a self-contained mathematical treatment of the simplest model that will demonstrate each principle.

Overlapping with the work on solid metals was the research for which he is perhaps best known: the application of quantum mechanics to the transport properties of *liquid* metals (2, 3), in which he successfully explained a wide variety of experimental data. His original

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

contribution was to combine two ideas. The first, already being developed for solid metals by Morrel Cohen, Volker Heine and James Phillips, was the concept of the 'pseudopotential'. This is an approach in which the isolated ion, which would scatter electrons strongly, is replaced by an equivalent 'neutral pseudo-atom' consisting of an ion together with its screening charge. A pseudo-atom sometimes scatters quite weakly, bringing the advantage that perturbation theory can be employed. The second idea was that correlations between the positions of ions reduce the resistivity and can be incorporated via the experimentally accessible radial distribution function, representing two-body correlations. It was for this work, together with his other contributions to condensed-matter physics, that he was elected to the Fellowship of the Royal Society in 1967.

Bristol, 1964–82

John joined the Physics Department at Bristol University in 1964 and immediately started building up the theory group, whose only member at that time was the recently appointed Derek Greenwood. John had spent a year in Australia before coming to Bristol, from where he brought the brilliant autodidact Peter Lloyd, who, together with Greenwood, had a large influence on the development of metals research at Bristol. The later appointments of John Alcock, Michael Berry, Noel Cottingham, Robert Evans, Balazs Gyorffy and Brian Pollard, together with John Ziman's gentle inclusiveness and abundant personal hospitality, created a near-perfect working environment within the friendly and successful larger physics department, leading to developments not only in metal physics but also in theories of liquid state structure and surfaces; asymptotics and geometry of waves, quantum chaos, and high-energy physics were also included.

In Bristol, John's research took a more formal turn, prompted by the realization that non-perturbative effects in liquid metals, particularly those involving d-bands and associated resonances, required a more systematic approach to scattering theory. The appropriate idealization, for which a large literature already existed in acoustics and optics, was waves (representing electrons) diffracted by arbitrary disordered arrangements of spherical potentials (representing screened ions). This is not an exactly solvable problem, but John adapted several effective medium theories, going beyond perturbation theory; these early attempts evolved into the 'coherent potential approximation', which was later refined and widely applied with much success by Balazs Gyorffy.

Another implication of the inadequacy of perturbation theory was the need to study structure beyond the two-body correlations embodied in the radial distribution function. This led to a consideration of four-body correlations and topological restraints (important, for example, in tetrahedrally bonded glassy amorphous semiconductors). And, most fundamentally, it led John to realize the importance of long-range order and the relevance of the profound discovery by Philip Anderson (ForMemRS 1980) of electron localization by disorder. Reviewing the basic theoretical difficulties of the nearly-free-electron model when applied to liquid semiconductors, metallic vapours, metal–ammonia solutions, impurity-band semiconductors and semiconductor glasses (10), John paid the following tribute:

> Almost exactly ten years ago (Easter 1960), Professor Mott called together a little unscripted conference of … solid-state physicists, and invited us to think aloud about *liquid* metals for a change. Most of what I have to report was triggered off by that stimulus.

The move towards formalism led in 1969 to another textbook, *Elements of advanced quantum theory* (9). In the preface, he likens quantum theory to a pyramid, but

> Even this image is misleading if it calls to mind a uniformly sloping edifice up which one may laboriously clamber hand over foot. … To my mind, quantum theory is much more like a *ziggurat*, with sudden high cliffs to be surmounted before one can move freely on the next plane of abstraction. … The first great step [is] from classical to quantum physics. … The second phase is to learn the language of Hilbert space. … But when the graduate student begins … he is often confronted … by yet another barrier of mysterious symbols and concepts—*field operators*, *graphs*, *propagators*, *Green functions*, *spinors*, *the S-matrix*, *irreducible representations*, *continuous groups*….

John continued being fully active in physics through the early 1970s, and never felt disaffected or disappointed with the field as his interests shifted towards the social aspects of science (see below). His last major project was the treatise, published in 1979, *Models of disorder: the theoretical physics of homogeneously disordered systems* (13); its philosophy, almost diametrically opposite to that of *Principles of the theory of solids*, he summarized thus:

> Condensed-matter physics has expanded in recent years and shifted its centre of interest to encompass a whole new range of materials and phenomena … disordered phases … steel and glass, earth and water … are far more abundant, and of no less technological value, than the idealized single crystal that used to be the sole subject of 'solid-state physics'.

After outlining some of the major conceptual problems remaining in the field, he ends with a valediction for his career as a theoretical physicist:

> I may perhaps be forgiven if I do not take the matter further, and take the opportunity of a natural break to announce that this is, as far as I am concerned, THE END.

Science studies, 1963–2004

At King's College, where John was a Fellow from 1957 to 1964, he became Tutor for Advanced Students. He began to write for the *Cambridge Review* and later became its editor. He was now an active reformer of the university, especially in relation to the position of non-Fellows. Such activities, a friendship with the philosopher of science Norwood Russell Hanson and contacts at King's with Edward Shils and other social scientists, drew him into the beginnings of what was to become his second career. With his friend Jasper Rose he wrote (in 1964) *Camford observed* (4), an amused look at the kind of societies into which the ancient universities had evolved. He called it folk sociology. He realized that the real key to an understanding of how science worked lay as much in sociology as philosophy, an idea almost unheard of at that time. He wrote occasional reviews and articles for the *New Statesman* and in 1960 gave two radio talks ('Scientists: gentlemen or players?' and 'Science is social') that led to his book *Public knowledge: an essay concerning the social dimension of science* (1968) (6), which described a model of science built around the communication system of journals and referees. Its success brought him into contact with established sociologists of science, such as Robert Merton. From then on he was a known figure in the field, exceptional for being able to speak with the authority of first-hand experience as a theoretical physicist.

Writing *Models of disorder* had taken several years, during which his career moved decisively into social science and he did no other original physics research. By the time it was published he felt he was no longer an 'active' authority in the field. Thus it was that in 1982, although he could have stayed had he wished, he took early voluntary retirement from Bristol, eight years in advance of the normal date. As it happened, he also had a private need to move from Bristol to London, in his own words,

> in order to live with my second cousin, Joan Solomon (*née* Diamond) who was the daughter of my mother's favourite cousin, whom I had met occasionally (and not quite unromantically) in my student days, whom I met almost accidentally again in 1975, who had in the meantime married, had four children, and divorced, and with whom I (and she reciprocally) fell hopelessly and, as it has turned out in a further twenty years, incurably in love.

In London, Lord (Brian) Flowers FRS, then Rector of Imperial College, arranged for John a Visiting Professorship in the Department of Social and Economic Studies. This gave him an academic base and other facilities, which he used several times a week. But after a year Joan took up a post in science education at Oxford, so they moved to the beautiful old cottage she owned in the country village of Oakley between Oxford and Aylesbury. However, John bought a flat near Notting Hill Gate and continued to spend a couple of days a week in London.

In 1986 he was approached by the Chairman of the Economic and Social Research Council to set up the Science Policy Support Group (SPSG), to analyse and advise on science policy. Although nominally a part-time job, in practice it occupied him fully. It took him into the centre of the research system. But it was also quite stressful, so that it came as a relief when he was able to leave in 1992 (aged 66 years) and retire more completely to the country. He would go to Bristol every few weeks to visit his wife, Rosemary, and his daughters, who lived there with their young families.

After the success of *Public knowledge*, which had appeared in 1968, John used a course of lectures he had given for the science faculty at Bristol as the basis for *The force of knowledge* (1976) (11). This set out, in elementary terms with historical case studies and many illustrations, the various aspects of science as a social institution. Then in 1978 came *Reliable knowledge*, which set out to explore the grounds for belief in science. In his final book, *Real science: what it is and what it means* (2000) (19), John Ziman undertook a systematic reworking of his analysis of science as a collective human endeavour. There were four major themes, admirably summarized by Peter Lipton (Lipton 2003):

> The first theme is *naturalism*, a view about how metascience [the science of science] should be done. According to Ziman, science should itself be studied empirically, with priority given to causal rather than logical analysis. This is one of the reasons why he is sympathetic to sociology and somewhat dismissive of philosophy. But Ziman's attitude is complex: he is also repelled by some of the sociology … [but] fascinated by questions that are essentially philosophical. What is the proper cognitive attitude towards our best scientific theories? Are we entitled to believe they are true? These questions, spurned by the sociologists, are ardently embraced by the philosophers.
>
> The second theme may be called … *socialism.* As Ziman shows, any account of science that relies on the model of the lone heroic scientist is doomed to failure, because science is by its nature a social process. No account of scientists' interactions with nature that leaves out their interactions with each other can do science justice … one fundamental example is the distribution of criticism and trust. On the one hand, the sustained criticism essential to scientific advance … will be provided by competition between research groups. On the other hand, the structure of trust in the testimony of other scientists is also a necessary condition for science, because almost every bit of science—from theory to data to the contents of the bottles of chemicals—is known only on the basis of what other scientists say.
>
> The third theme is *evolutionary epistemology*. Ziman cannot bring himself to endorse the view that science is straightforwardly in the truth business, revealing the real joints of a largely invisible world. Various

considerations stay his hand. There are the great sceptical arguments of the philosophers, especially David Hume's corrosive argument against the possibility of justifying any non-demonstrative inference. If Hume is right, there can be no reason to believe any scientific claim that goes beyond the actual data. Another consideration is the strong norm of scepticism within science itself. Then there is the 'path dependence' of scientific development, in which the form that new models take is strongly constrained by contingent features of their predecessors; this is combined with the opposite phenomenon, the extensive conceptual change displayed by the history of science. All of these present serious challenges to the view of science as revelatory of the truth about the world, however spontaneously most scientists embrace it.

According to Ziman, although full-blooded realism might be indefensible, science nonetheless generates reliable knowledge. Scientific models are imperfect maps. … Ziman exploits a biological analogy with natural selection to illuminate the way in which these maps evolve. Scientific models are generated and then the 'fittest' are selected by mediated confrontation with the physical environment that they purport to describe. The biological analogy is of course not perfect. For example, scientific variation is not blind. But the analogy nevertheless illuminates diverse aspects of scientific development, and it shows both why science should not be expected to yield perfect truth and why it should be expected to generate increasingly reliable maps of reality.

The last of Ziman's themes to be visited in this review is the emergence of *post-academic science*. The distinction between academic and post-academic is not precise, but it is roughly the difference between blue-sky research and work strongly constrained by interests of funding bodies, public and private. In Ziman's view the importance of this change in the dominant culture of science is difficult to exaggerate: 'In less than a generation we have witnessed a radical, irreversible, world-wide transformation in the way science is organized, managed and performed.'… Post-academic science is science done with a focus on utility, application and value for money. It is often large-scale and transdisciplinary. It marks a change in the social structures of science, so we ought to expect consequent changes in the epistemology and in the kinds of models that the process yields.…

Ziman obviously thinks that practising scientists ought to care about metascience. Is he right? Gaining a better description of how science works is certainly no guarantee of better science, although one hopes that at least it will not make things worse, in the way that thinking too much about juggling sometimes leads to dropping the balls. … As Ziman emphasizes, the move into post-academic science has both costs and benefits, and thinking about what the move entails might help us to decrease the costs and increase the benefits.

John Ziman always had many irons in the fire. His interest in social responsibility in science led him to consider how science as a social institution should be approached in higher-educational practice, and resulted in *Teaching and learning about science and society* (1980) (14) and *An introduction to science studies* (1984) (16). He became naturally involved in the topic of the public understanding of science, first in the working party set up by the British Association on 'Science and the media', and later on the 'Bodmer Committee' of the Royal Society. The latter was, in some ways, a stressful experience, because he found himself at odds with Walter (later Sir Walter) Bodmer FRS. John argued that there was a serious issue as to whether science should be presented to the public and 'understood' by them solely on the terms of the scientists. He argued for at least some research on the question, and in due course obtained £$\frac{1}{2}$ million for a research programme. This happened to be the time at which they were setting up the SPSG, so it became their responsibility to choose projects and manage them. John felt they did this very well, but he certainly did not enjoy the experience of dealing with colleagues whom he felt to be quite blind to what people in general think about science.

He did not, originally, want to be involved in the administration, management and financing of science, but in writing about science as a social institution he did get interested in how it fitted in to the larger picture of change taking place in the scientific world. So in the SPSG he established a centre where academic sociologists could come together and take seriously what he regarded as the real issues of science policy. For example, in 1989 they ran a NATO

Advanced Studies Institute on 'The research system in transition', a central theme for John's thinking, as we have already seen. His Bernal Lecture at the Royal Society on 'The collectivisation of science' in 1983 (15) and his Medawar Lecture on 'Post-academic science' in 1995 (18) reflect similar themes.

John Ziman described his involvement with the International Centre for Theoretical Physics (ICTP) at Trieste as one of the most agreeable and instructive experiences of his life. In about 1966 he had been invited by its founder, Abdus Salam FRS, to run a course there on solid state physics, and for the next 16 years he would go to Trieste for a few days or weeks each year to run a series of 'Winter Colleges' on condensed matter physics. As a member of the Board of ICTP he played a major part in defining its policy during that period. However, the real influence of ICTP on John's thinking was in relation to science in developing countries, for which indeed it mainly existed. His sympathy and concern led him to think and write on the subject—he gave the Rutherford Memorial Lecture in 1968 (8) in India and Pakistan—and to arrange many personal trips to developing countries in Latin America and South-East Asia, and to external examining in Nigeria and Ghana.

Another major activity (1973–90) was with the Council for Science and Society (CSS). The start was a letter from Paul Sieghart, a former barrister, suggesting the need for an institution of this kind. John found him a fascinating personality, although he did have some mixed feelings about him, and became his closest associate in setting up the CSS. After Lord (Michael) Swann FRS retired from being its Chairman, John became Chairman, with Paul Sieghart as Deputy and Jerry Ravetz as Secretary. The CSS produced pioneering reports on the social consequences of scientific and technological innovations; John was very proud of what they accomplished, feeling that they were filling an important gap that was being ignored by the 'official' bodies such as the Royal Society and the British Association.

John became an experienced editor of scientific review journals. In the 1970s he was Honorary Editor of *Reports on Progress in Physics* and for many years was one of the two editors of *Science Progress*, which tried to fill the gap between semi-popular journals such as *New Scientist* and *Scientific American* and the professional ones such as *Nature* and *Science*. He writes that on two occasions he was involved in negotiations that might have led to his becoming Editor of *Nature*.

In the late 1960s John had been frustrated by the problems of getting Soviet participation in the ICTP work, and he persuaded John Maddox to publish in *Nature* 'Letter to an imaginary Soviet scientist' (7), which raised these issues directly. As a result he received a letter from Zhores Medvedev, the best-known scientific dissident of his day and, not without misgivings, was instrumental in getting *The Medvedev papers* (Medvedev 1971), which had originally circulated as samizdat, published in the West. In a foreword John argues with passion that, in spite of the author's not being in a position to give permission, nevertheless we must publish his book; this is Ziman at his most persuasive and eloquent. He thus became involved with the groups fighting for the Jewish Refusniks, who, at odds with the authorities, were trying to leave Russia for Israel. Characteristically, he did not entirely identify himself with this particular movement but nevertheless did go to Moscow for an exciting weekend with one of us (J.F.N.) to attend the Azbel seminar in the Brailovsky apartment (Brailovsky himself was in internal exile) and gave active help. There followed a CSS working party on 'Scholarly freedom and human rights' and a book with Paul Sieghart and John Humphrey, *The world of science and the rule of law* (17). In these he brought together the norms of academic science and the legal principles of human rights, showing the dependence of the scientific enterprise on freedom of speech. Talking to Ronald Keay, then Executive Secretary of the Royal Society, he found it rather shocking that the Society, charged with official responsibilities in representing science to the Soviet Academy, for example, had practically no information at all about the socio-political state of science in the countries of Eastern Europe.

Although, like all politically aware scientists of the Cold War years, John Ziman was concerned about nuclear weapons, he had little taste for strongly ideological movements like the Campaign for Nuclear Disarmament (CND), and his involvement with the Pugwash movement was brief. Instead, again characteristically, he set up a working party in the CSS on ‘Military R&D’, which brought in some of the ‘real actors’ from the Ministry of Defence and gave a public account of how the system actually worked. On the same theme, in about 1989 he became Chairman of the Board of what became the International Security Information Service (ISIS) and continued to try to inform the public and MPs of the facts behind arms control.

Ziman was a prolific writer and broadcaster, much in demand for book reviews and articles. Ranging from single paragraphs to extended essays, these occasional publications numbered more than 400. Witty, shrewd, wise, scholarly, graceful and humane, they all deal with significant issues raised by the scientific life. A single quotation must suffice, from a broadcast talk on ‘Models’, which concludes:

> What are these models of ours but charming toys that we may bring out of the cupboard for game after game of competitive, creative play? It’s only when we come to speak, and think, of life and love and things of the heart and spirit, that we must not be deceived by mechanical metaphors—by models that are too sharp and simple to represent the clouds, the perfumes, the music, the voices, the symbols, the visions that are the true stuff of reality.

Recreations and family matters

John was warm-hearted, kind and thoughtful. He was also resolute, and wise in many ways. He felt he was a fairly domesticated person; certainly he enjoyed books and especially the cultivation of his garden, first in Bristol and later in Oakley. Indeed, before Rosemary died in 2001 he would return to Bristol to tend her vegetable garden. He found woodwork satisfying and developed a passion for walking, not only in the English countryside but also in many countries abroad, with occasional photography. He married Joan on 27 October 2002 and he died, after complications after an infection of the heart valves, on 2 January 2005.

Acknowledgements

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