THE ROYAL SOCIETY

Introduction: science at the turn of the millennium

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1. An introduction to the Millennium Issues

The 46 articles in this collection written, largely, by relatively youthful scientists, provide various fascinating perspectives on the state of our scientific understanding, as this 20th century draws to its close. Some authors venture opinions as to the directions in which science or technology might move in the future, while others concentrate their remarks on some striking recent progress in areas of their own special expertise.

It is, indeed, a risky endeavour to attempt to predict the future. This is particularly so in science or in technology, where seemingly innocent developments can sometimes have unforeseen and irreversible effects on our society. Occasionally these are of alarming magnitude, such as with the realization that there is a vast accessible energy locked in atomic nuclei, or with the potential power and broad-ranging implications of electronic computer technology. Yet, though unreliable and difficult, attempts at futurology undoubtedly have important roles to play. Moreover, the scientists who are most directly involved in some scientific development are likely also to be those in the best position to know what its implications might be. The scientists may not get it right, of course, as was the case with Rutherford's famous remark about nuclear energy: 'The energy produced by the breaking down of the atom is a very poor kind of thing. Anyone who expects a source of power from the transformation of these atoms is talking moonshine' (*Physics Today*, October 1970, p. 33). But even if their initial opinions turn out erroneous, those opinions are very valuable, particularly when set forth in a reasoned way, accessible to the public.

These articles provide us with a broad range of expert opinion; they are clearly presented and are indeed very accessible. Most of them lie well outside that limited area within which I might myself plausibly claim some direct knowledge or understanding, yet these articles convey to me some of the feeling and excitement for many strikingly impressive recent scientific developments. As examples, there are the new and intriguing subjects of supermolecular chemistry (Gale, Millennium III),[†] of polymer electronics (Samuel, Millennium II), of potential new uses of magnetoresistance in spin electronics (Ziese, Millennium II), of organic semiconductors (Cacialli, Millennium II). There is the remarkable and ubiquitous role of organofluorines (Sandford,

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[†] In this introductory paper, references to articles in the current Millennium Issues indicate in which of the Issues I, II or III they appear.

Phil. Trans. R. Soc. Lond. A (1999) **357**, 3197–3203

Millennium III), the potential value of new methods of producing thin diamond films (May, Millennium III), the extraordinary biological structures that have arisen throughout the evolutionary history of life on Earth (Hemsley & Griffiths, Millennium III).

In many of the articles, one sees the blurring of previously established boundaries between subjects. Organic chemistry, for example, is now seen (Goodman, Millennium III) to overlap with biology and to benefit from atomic-scale engineering; the study of enzymes, so it now appears, requires genuine subtleties of quantum-mechanical physics (Sutcliffe & Scrutton, Millennium III); the biological study of vertibral skeletons links engineering with medicine (van der Meulen & Prendergast, Millennium III); the physics of electromagnetism has important implications for the study and treatment of the human brain (Walsh, Millennium III); the issues raised by environmental concerns now affect how one goes about the chemical synthesis of new substances (Macquarrie, Millennium III); the study of ice surfaces on the Earth interrelates with fundamental issues of physics on the relation between microscopic and macroscopic behaviour, and relates also to environmental questions (Wettlaufer, Millennium I).

Many of the techniques provided by modern physics are proving ever-more valuable in other subjects, such as the use of NMR spectroscopy for the elucidation of the structure and function of biomolecules (Pfuhl & Driscoll, Millennium III); femtosecond bursts of light enable chemical processes to be studied as if in 'slow motion' (Roberts, Millennium III); optical communication, via fibres and lasers, finds uses ranging from telephone cables to computers and to fundamental physics (Bayvel, Millennium II; Mosca *et al.*, Millennium II); the manipulation of coherent light in such structures as lasers and Bose–Einstein condensates has an enormous potential, and opens up the new subject of atom optics (Power, Millennium II); quantum electronics, used for over 50 years in transistors, now hints at exciting new applications in molecular biology and chemistry (Davies, Millennium II).

Electronic computers have clearly had an enormous influence on the way that science is now done, in addition to having influenced our society in innumerable other important ways (Gibbens, Millennium II). The studies of the Earth's crust (Lonergan & White, Millennium I), of its ice sheets (Sammonds, Millennium I), its climate (Saunders, Millennium I), of geophysical vortices (McDonald, Millennium I), its deep interior (Vocadlo & Dobson, Millennium I), and the chemistry of its atmosphere (Lary, Millennium I) have all made great strides, and it is difficult to see how this would have been possible without the aid of such computational devices, in conjunction with stores of observational data. Computer modelling also increasingly replaces what had previously been the necessity of actual physical tests, in many areas of biology and medicine, with important implications for the future (Kohl *et al.*, Millennium III; Kolston, Millennium III).

The very techniques of computer modelling have also undergone vast improvements, such as in ionic modelling (Wilson, Millennium III), and new mathematical ideas, such as topology optimization, appear to have broad potential application (Sigmund, Millennium II). The interplay between computer modelling and purely mathematical reasoning has importance both in the valued traditional applied-mathematical areas, such as fluid dynamics (Bowles, Millennium II), and in the newer ranges of mathematical/computer-aided research such as in biology, medicine and finance (King, Millennium II; Wilmott, Millennium II).

There is a very significant issue which must be raised, however, namely whether, or under what circumstances, such modelling is to be trusted. Issues of this nature should by no means be overlooked, and their careful consideration is an important matter (Stark, Millennium II; Wilmott, Millennium II). Moreover, there is always the danger that too much reliance on direct computational modelling may cause one to overlook the consequences of simple mathematical reasoning, such as may come about by use of symmetry arguments (Guest, Millennium II).

Computers may themselves significantly change in the future, of course. There are many relevant new technological ideas available, as well as those that have now become standard. Magnetism has played a crucial role in computer memory, and will continue to do so, but will perhaps be relevant in quite novel ways (Cowburn, Millennium II). Moreover, there are other ideas such as the use of optical fibres (or even light-beams directly) (Gibbens, Millennium II; Bayvel, Millennium II), and perhaps there will be relevant new uses of atom optics (already employed widely in CDs) (Power, Millennium II). Furthermore, new kinds of semiconductor or other microelectronic devices (Davies, Millennium II; Ziese, Millennium II) may well play novel roles. In the future, these might be of organic and/or polymeric nature, and perhaps even biological (Cacialli, Millennium II; Samuel, Millennium II).

Looking farther into the future, there is now great interest in the potential possibility of quantum computers which would make use of large-scale quantum coherence and quantum entanglement to achieve computational actions of a fundamentally different character from those that have been performed so far (Mosca *et al.*, Millennium II). As yet, in the study of quantum computation, purely theoretical issues have held the stage, and there are many fascinating new questions that have been raised. Accordingly, theoretical developments have far outstripped their practical means of implementation; yet some impressive but limited progress has been made on the experimental side. It may be that radical new insights will be needed before true practical progress can come about. One possible area of advance might, for example, be in the collective quantum behaviour of charged particles (Lee & Schofield, Millennium II). In the related subject of quantum cryptography, however, practical devices are already at hand (Mosca *et al.*, Millennium II).

Moving from the tiny scales, where quantum effects most usually make their mark, to astronomical and cosmological dimensions, we find, still, that we can by no means ignore the extraordinary effects of quantum mechanics. Remarkably, the consequences of the intrinsically quantum-mechanical phenomenon of maser action have been observed in astrophysical situations (in fact, some 35 years ago). The source of these effects was subsequently explained, and such things are now used as an astrophysical observational tool (Gray, Millennium I). Moreover, the subject of cosmology, which is concerned with the largest distances of all, cannot be studied theoretically in a modern and satisfactory way without addressing the role of quantum mechanics. The reason for this is that the structure of the early universe—which means the Big Bang and what came immediately afterwards—depended crucially on a subtle interplay between space-time geometry and quantum fields, and ultimately upon the still missing theory of quantum gravity. Here the scope for theoretical speculation is large, and a wealth of yet-untested ideas play important roles in discussion. There is impressive observational support for an early extremely high-density state of the universe, closely in accord with the predictions made by Friedmann in the 1920s, and by Gamow and others in the late 1940s, using Einstein's general theory of relativity.

This is what is now referred to as the 'standard model of the Big Bang', but particular modifications of this scheme called 'inflationary cosmology' have also become popular in recent years (Magueijo & Baskerville, Millennium I, Garcia-Bellido, Millennium I). Sophisticated quantum effects are essential to such schemes. These issues are linked with the vexed question of the dark matter that is known to constitute, by far, the major part of the mass-density of the universe, but whose actual nature remains an issue of mystery, speculation and controversy (Moore, Millennium I).

Most of astronomy, on the other hand, does not deal in matters of such alarming uncertainty, and there is little direct appeal to quantum-mechanical principles. Yet many puzzles still abound, and there is much to learn of great theoretical interest. This even applies close to home, to our immediate neighbour, the Moon (Dunkin & Heather, Millennium I) and, a little more distantly, to other members of our Solar System (Coates, Millennium I). These bodies are close enough that direct space exploration is possible, and this has so far provided a wealth of surprising, but still very incomplete, information. For these neighbours, at least, one need not depend exclusively upon remote observation and theoretical modelling.

Yet for most of astronomy, astrophysics and cosmology, one is not so lucky, and progress is indeed made by insightful theoretical modelling, in conjunction with increasingly sophisticated observational technique. There is always the possibility that in some distant regions it may become necessary to invoke a physics that lies *beyond* that 'conventional physics' which our measurements and understandings here on Earth have led us to believe in. Indeed, such speculations are occasionally brought forward to explain some of the more violent observed objects in the universe, such as quasars and gamma-ray bursters. But a clear consensus of opinion tells us that this is not necessary (black holes being now regarded as 'conventional'), *except* perhaps in studies of cosmology and the early universe. Only there does it appear that we may have to turn to a physics that might reasonably be called 'unconventional'.

There has been no attempt, in the selection of articles presented here, to try to summarize the various ideas, put forward in recent years, aimed in such 'unconventional' directions. All the serious approaches are, of necessity, highly mathematical in their nature, and they do not lend themselves, easily, to accessible description. They must, at least to a close approximation, accommodate both Einstein's general relativity and the quantum theory of fields, and ultimately a re-evaluation of the very basis of space-time geometry (Majid, Millennium II), though all specific suggestions remain speculative, as of now. Also, the strikingly successful features of the standard *particle physics* model of electromagnetic, weak and strong interactions must, as well as gravity, be incorporated within any proposed comprehensive scheme. Since the most rudimentary ingredients of this model (electrons, quarks) are particles of a 'spinor' nature (so that a rotation through 360° does not restore its state to the original, whereas through 720° does), an appropriate algebraic framework for handling spinors is required. One such approach (Lasenby *et al.*, Millennium II) appeals particularly to the original algebra of Clifford, which also has numerous other applications.

Earlier in the 20th century, the speculations of mathematical physicists sometimes achieved astonishing results, such as the epoch-making advances of Dirac and others. Yet, success normally occurs when the new steps are taken not too far from the experience of observation. We may ask how far we are to trust those speculations that now appear to take us a good deal farther from what can be confirmed by experiment. Many serious questions remain with all such later schemes and it is not really clear that any of the suggestions for progress at a fundamental level are likely to survive far into the 21st century (Kent, Millennium II).

2. Fundamental physics in the 21st century?

To end this introduction, I will make a few comments of my own, concerning the likely directions in which physics, at the fundamental level, might move forward in the next century. In my own opinion there is no doubt where the major gap in our present understanding lies. It is in finding the appropriate union between the two great revolutions of 20th century physics: Einstein's general theory of relativity and the quantum theory. In this respect my views do not differ from those usually expressed, but my particular perspective on this question does differ very markedly from what might be considered to be 'conventional'.

Most theoreticians working in this area would take the view that what we seek is a 'quantum theory of gravity' in which gravitational theory, and therefore the very structure of space-time, must conform itself to the established rules of quantum (field) theory. There are many ideas aimed at achieving this, the most popular being (super) string/M-theory, in its various forms (see Greene 1999), and the approaches to quantum gravity according to the schools of Ashtekar and Hawking. While all of these involve impressive degrees of originality, and represent very powerful intellectual achievements, they remain 'conventional' in the sense that they make no attempt to shift from the standard quantum rules. In all these schemes it is Einstein's theory that must give way, to be accommodated within an unbending quantum formalism. It is my own strong opinion that the marriage between these two great theories must, on the contrary, be a much more even-handed affair. The degree of experimental confirmation of Einstein's theory is now as impressive as that of quantum field theory, according to the astonishing observations of Hulse and Taylor (cf. Taylor 1998), concerning the binary pulsar PSR 1913+16. But the real reason for expecting that quantum theory, no less than general relativity, must bend its very rules is that these rules lead one essentially into paradox, when they are employed too far from the situations to which quantum theory has been successfully applied.

The problem I refer to is what is called the *measurement problem*, which has to do with the fact that the procedure that one adopts in order to treat 'quantum systems' does not appear to apply to large objects. This procedure is the evolution of a quantum state according to Schrödinger's equation (or equivalent), whereby superpositions of different alternative happenings must be treated as co-existing simultaneously, and this equation states that such superpositions should persist indefinitely. However, in a quantum measurement, one alternative or another must be the result (quantum state reduction), and this cannot be achieved simply by the Schrödinger-evolution of the state alone. I have given reasons (Penrose 1996) why I believe that quantum superpositions must become unstable when the distortions of space-time that they induce, according to Einstein's general relativity, become significant. (The lifetime of a superposition of two quantum states, each of which would be stationary on its own, can be calculated as approximately $h/2\pi E_{\mathrm{G}}$, where h is Planck's constant and E_{G} is the gravitational self-energy of that mass distribution which is the *difference* between the expectation values of the mass distributions of each individually.)

This instability occurs at a much earlier stage than one might expect, and I have suggested an experiment that would test whether this instability is a real effect (Penrose 1998). If such an effect is confirmed, 'quantum state reduction' would indeed be seen as a *real physical process*, and this would have broad implications for physics in a number of different areas. In my opinion, it would also make quantum mechanics more 'usable' in subjects like biology, where as things stand it is difficult to apply the quantum rules in situations where there is no clear-cut distinction between the 'quantum system' and the 'measuring apparatus'. Certain speculations (cf. Hameroff & Penrose 1996) concerning the biological nature of consciousness also would depend critically upon a positive result from such an experiment.

None of the present popular suggestions for *uniting* space-time structure with quantum theory makes any serious attempt to accommodate quantum state reduction; so for this reason alone I am sceptical of their eventual success unless they can undergo substantial changes. I would also give little credence to current popular theories (such as inflationary cosmology) that attempt to account for the structure of the early universe, since the ultimate unknown appears to be this same missing 'quantum gravity' theory. It is quantum gravity, in some form, that ultimately governs the very uniform structure of the Big Bang space-time singularity, but it also must govern the very non-uniform singularities in black holes. Evidence coming from the existence of the *second law of thermodynamics*, and other observational facts, indeed points to a gross time-asymmetry in the appropriate union between general relativity and quantum mechanics. This, again, is not a feature of any of the existing proposals for that union, and this provides an additional reason for my personal scepticism.

These remarks may seem like comments of despair; but I prefer to see things in a more optimistic light. We can learn from existing attempts at a solution to this problem of unification, and enough evidence may be thereby gained so that someone may pick up the correct trail before too long. It is my expectation that there will indeed be a new major revolution, and it should come sometime in the next century. This would be both very exciting and fundamentally important to science as a whole.

I am grateful to the Institute for Theoretical Physics, University of California, Santa Barbara, for support under NSF contract PHY94-07194.

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AUTHOR PROFILE

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Roger Penrose is the Emeritus Rouse Ball Professor of Mathematics at the University of Oxford and also Gresham Professor of Geometry, Gresham College, London. He has a part-time appointment as Francis and Helen Pentz Distinguished Professor of Physics and Mathematics, Penn State University, USA. He was born in Colchester, Essex, UK, in 1931, attended University College School, London, was awarded a BSc degree at University College, London, and a PhD degree at St John's College, Cambridge. He held several posts in the UK and the USA, most particularly at Birkbeck College, London. He was elected a Fellow of the Royal Society of London in 1972 and a Foreign Associate of the United States National Academy of Sciences in 1998. He has received a number of prizes and awards, including the 1988 Wolf Prize, which he shared with Stephen Hawking for their understanding of the universe, the Dannie Heinemann Prize, the Royal Society Royal Medal, the Dirac Medal, and the Albert Einstein prize. His 1989 book, *The Emperor's New Mind* became a bestseller and won the 1990 (now Rhône-Poulenc) Science Book Prize. His latest books are *Shadows of the Mind* (1994), *The Nature of Space and Time* (1996) with Stephen Hawking, and *The Large, the Small and the Human Mind* (1997).

He has research interests in many aspects of geometry, having made contributions to the theory of non-periodic tilings, to general relativity theory and the foundations of quantum theory. He has contributed to the science of consciousness. His main research programme is to develop the theory of twistors, which he originated over 30 years ago as an attempt to unite Einstein's general theory of relativity with quantum mechanics. In 1994 he was knighted for services to science.