Thought of Dr Leggett

Sudip Chakravarty

The Problems of Physics. By A. J. Leggett. *Oxford University Press:1987. Pp.192. Hbk £15, $17.95; pbk £4.95, $10.95.*

To those who have followed Leggett's work professionally, this book will not come as a surprise. It is written in the style which is so characteristic of the author: complex conceptual questions are broken up, inessential pieces are removed and the ideas are finally presented with a simplicity that no one can imitate. Nonetheless, whilst reading the book I could not help asking myself how it would be received by those less familiar with Leggett's writings. I shall return to this point later. For the moment, it seems to me that no matter how one is swayed by one's own personal prejudices, the book is undeniably an exquisite presentation of frontier areas of contemporary physics. The questioning attitude can be a vital source of inspiration to young and would-be physicists. We all know that strong pressures to conform to existing paradigms come much too early in our lives.

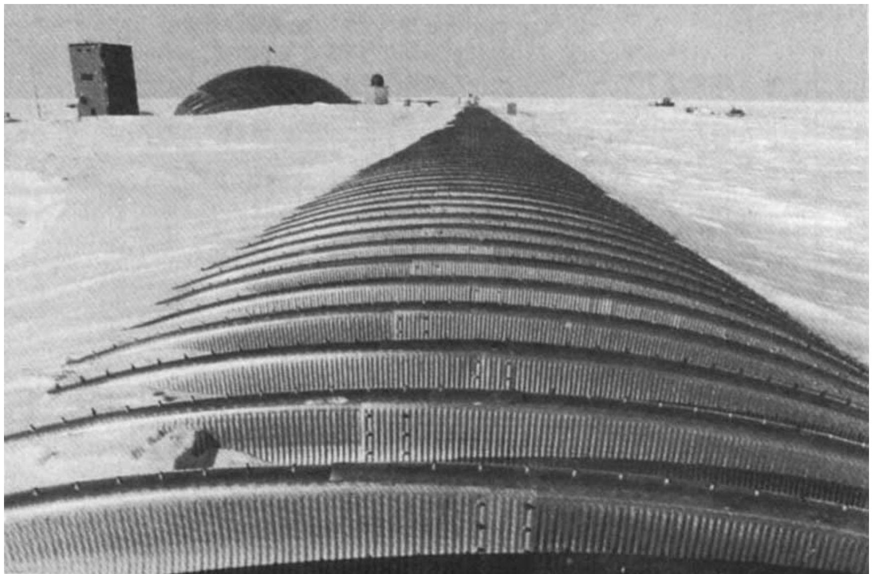
Elementary particle physics and cosmology are currently undergoing revolutionary changes. Quantum chromodynamics has ushered in a new era in which the question "What are things made of?" has reached a new level of simplicity. We have learnt that the fundamental interactions owe their form to deep concepts such as gauge invariance. We have also learnt that two of those interactions, the electromagnetic and the weak, can be unified, which has led to spectacular predictions that have been experimentally well verified. To explain these developments in a language that is understandable to a non-expert needs an unusual amount of insight, and I suspect that the clarity with which they are discussed in this book will be welcome by all.

A point that Leggett makes over and over again, and one that is worth more than a pause, is that in physics we are continually making extrapolations. Often the laws that are tested firmly on a given scale are assumed to hold on scales which are unimaginably different. Leggett illustrates the point with an almost poetic poignancy: "If we imagine a microbe confined to the surface of a microscopic speck of dust floating in the middle of St Paul's Cathedral, the microbe's problem in inferring the properties of the Cathedral, or even the earth as a whole, would be trivial compared with ours".

Here, of course, he means the reality that a cosmologist faces. But then did not Newton also make such an extrapolation when he successfully described the motion of the planets? How are we able to make such extrapolations in physics? What guides us? What are the tools of present-day cosmology? How can we describe the first 10^{-35} seconds after the Big Bang? It almost sounds fantastic to a layman, and yet cosmologists are making steady progress in finding answers to these questions. New concrete concepts and tools of particle physics are being used successfully. The idea of broken symmetry, borrowed from condensed-matter physics, is exploited to propose how, as the Universe expanded, and cooled, the various symmetries were progressively broken, and the Universe evolved to the state that we live in. The scheme, however grand and elegant it may sound, is not without its difficulties. Almost all versions of this scenario predict unobserved debris of the Big Bang — monopoles, cosmic strings and so on. It is only recently that those pursuing the concept of an inflationary universe have begun to come to terms with such difficulties.

Leggett then moves on to physics on an entirely different scale, a scale that we are familiar with in our everyday life, the so-called condensed-matter physics. The subject matter in this area is extraordinarily diverse, so some important developments are sampled. In this respect I sometimes wonder how old-fashioned the textbooks are, and how naive the views of some of my colleagues are as to what condensed-matter physics is. Leggett has strived marvellously to dispel some of the antiquated notions in common currency. The sample that he provides is truly representative of the modern developments, and even experts will find the exposition insightful. But what is perhaps more important is the underlying view that the author holds. As he says, it is not uncommon to hear "that there are no new laws of nature to be discovered by studying condensed matter as such, since all behaviour of such matter follows, in principle, from the behaviour of its atomic or subatomic constituents; and second if this is so, then the study of complex matter cannot be as 'fundamental' as the study of the constituents themselves — indeed, that it is really rather a trivial occupation by comparison".

Leggett then goes on to expose the fallacious nature of this view, and in this in my opinion he has done a much-needed service to the community. His view that the most important advances in this area come about by the emergence of *qualitatively* new concepts is a point that even the practitioners of the field sometimes fail to recognize. Some of these concepts such as voltage, temperature, entropy and so on are so ingrained in our education that we fail to recognize their true role in the development of the field. Others, such as broken symmetry and universality classes, have found such wide applications that some of us tend to forget where they came from. I leave it as an amusing exercise to



Hidden depths — the Amundsen–Scott Station at the South Pole. The picture is taken from International Research in the Antarctic, *which is based on reports from working groups of SCAR, the Scientific Committee on Antarctic Research. Published by the International Council of Scientific Committees with Oxford University Press, the book costs £25, $39.95.*

the reader to trace, for example, the origin of what is now known as the Higgs mechanism. The author also correctly notes that, when it is a matter of many interacting degrees of freedom, we often run into totally unexpected effects which are difficult to predict ahead of their experimental discovery, such has certainly been the case for superconductivity and superfluidity. And how about the newly discovered high-temperature superconductors? Those who believe that condensed-matter physics is an arid occupation of simply solving many-particle Schrödinger equations should take a lesson.

As some of us know, Leggett has been interested in the problem of quantum measurement for some time. Indeed, in recent years, he has played a major role in inspiring condensed-matter experimentalists to perform a number of beautiful experiments to test quantum mechanics on a macroscopic scale. It therefore does not come as a surprise to see a large amount of space devoted to the subject. In fact, the whole book betrays an uneasiness about quantum mechanics. However unorthodox the point of view of the author may be, I am sure that any serious reader will find his discussion of Bell's inequality delightfully simple and yet forceful. The same is true for his account of the 'cat paradox'. The question that he wishes us to consider is "whether, and if so where and how quantum mechanics breaks down in the face of increasing complexity...".

However, there are certain aspects of the book that I found disturbing. First, there is a tendency throughout the text to throw out a few questions, for example "why should nature seem to be described by Lagrangian field theory, and therefore submit herself to the stringent constraints imposed by it? Is the formalism universally valid, in fact, even when it gives rise to severe paradoxes?... Or are these questions themselves 'meaningless'?". Although these questions are not in themselves 'meaningless', they seem to lose their impact if they are not seen in context.

My second concern is to do with an unnecessary defensiveness, which perhaps stems from a particular type of sociological tension in the physics community today. Thus Stephen Hawking may proudly announce that the end is perhaps in sight for theoretical physics, but we know, all too well, what the fate of such prophecies is. We do not need to put up a straw man to conclude that ".... far from the end of the road being in sight, we are still, after three hundred years, only at the beginning of a long journey along a path whose twists and turns promise to reveal vistas which at present are beyond our wildest imagination". □

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Climatological quirks

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A Climate Modelling Primer. By A. Henderson-Sellers and K. McGuffie. *Wiley: 1987. Pp.217. £28.50, $53.95.*

CLIMATE modelling is very much in the news at present. Our understanding of the physical mechanisms responsible for such important natural variations as the ice ages and the El Niño/Southern Oscillation has improved through advances in both observational techniques and numerical modelling. At the same time, models have been used to predict the systematic changes expected from increasing concentrations of greenhouse gases liberated by anthropogenic activities, as well as the short-term effects of a nuclear war.

There is clearly a need for an introductory text which describes climate models and their application to a wide range of problems. The authors of this book have identified a gap in the literature, but their account is far too idiosyncratic to serve as a useful introduction and in many places it is actually misleading. For example, what little information there is on the physical basis of climate is scattered through the text, so the reader is in no position to understand the material on feedback processes and climate change which occupies much of the first chapter.

Three-dimensional models form the basis of serious climate research, yet they receive less attention than much simpler models. As justification, there is the extraordinary and incorrect assertion that in 1986 there was "a series of occurrences of apparently correct results being generated for the wrong reason".

Some quirks are simply juvenile, such as the attempts at humour in the dedication and acknowledgements, and the glossary definition of infinite as "quite a lot — really an awful lot". Others are more serious. There is a tendency to reproduce material from other sources without explanation, as in the section on cloud prediction in Chapter 6. In this and other places, the reader is swamped by unnecessary detail, while the underlying physics remains unexplained. Such detail does, however, lead to a useful bibliography.

The book is summed up by the 'spaghetti diagram', which purports to show the many interactions between elements of the climate system and which leaves the boxes blank for readers to fill in for themselves. As an alternative text, I would recommend *An Introduction to Three-dimensional Climate Modeling* by Washington and Parkinson, which, although its remit is more restrictive, does at least have the stamp of authority which comes from authors who are acknowledged experts in this field. □

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Static action

Peter Dunnill

Immobilized Cells: Principles and Applications. By J. Tampion and M.D. Tampion. *Cambridge University Press:1987. Pp.257. £30, $59.50.*
Immobilised Enzymes and Cells. By A. Rosevear, J.F. Kennedy and J.M.S. Cabral. *Adam Hilger, Bristol/Taylor & Francis, Philadelphia:1987. Pp.248. £37.50, $102.*

IT IS now 80 years since Michaelis of enzyme kinetics fame described the adsorption of an enzyme to charcoal, and 30 years since the systematic study of enzyme immobilization for practical application began in earnest. Success with attachment of enzymes to supports encouraged studies of whole-cell immobilization, though immobilized cells had been used for a century, largely unconsciously, in fields such as sewage treatment and vinegar production.

There are now dozens of methods of enzyme and cell immobilization and any guidance on appropriate procedures is very welcome. As their titles imply, these two books take quite different approaches, with one focusing just on immobilized cells and the other considering both enzymes and cells. For newcomers to applied biocatalysis, the latter approach is particularly valuable because the first decision to be made is whether to use whole cells or enzymes.

That choice might be more straightforward if scientists could agree on what they mean by the use of the word 'cell'. Thus on page 1 Tampion and Tampion define an immobilized cell as "a cell or remnant thereof that by natural or artificial means is prevented from moving independently". Aside from cell organelles, however, a remnant of a cell effectively functions as an enzyme catalyst lacking the organization needed for, say, co-factor recycling. Rosevear *et al.* refer to such systems as "dead cells", but the European Federation of Biotechnology Working Party disapproves of this term. Equally it dislikes terms such as "living", but welcomes "viable", "non-growing" and "respiring" in appropriate circumstances. At present we don't understand why immobilized cells are often much more stable as catalysts than free cells, so