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

A. Slingo

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