want to create and support ideas favouring high levels of reproduction. They will accept the loss of some of their children as inevitable. Conversely, the less unpredictable the environment is perceived to be, the more people will develop ideas favouring low levels of reproduction.

The hypothesis is tested, in the particular case of religion, by classifying different religious strategies as reproductively negative or positive, and then looking for correlation between broad religious traditions and such indicators of environmental stability as energy consumption and gross national product. Not surprisingly, Protestant Christians turn out to be wealthy energy-consumers with inhibited attitudes towards sex, whereas Hindus are just the opposite.

Such a bald summary does not do justice to the considerable subtlety often exercised in the interpretation of religious ideas — the perceptive discussion of celibacy is a case in point — nor to the sensitive exploration of ways in which cultural and physical evolution might in practice interrelate. I welcomed also the frank recognition that there is vastly more to religion than the biologically adaptive dimension on which the authors concentrate. But even within its own terms the main hypothesis seems to me to be thin and unconvincing. I was surprised, for example, to find no mention of the widely canvassed view that Protestant Christianity, far from being a response to an environment perceived as stable, has been a major factor in helping to bring that environment under control. In other words, it is not only the relationships between causes and effects which are uncertain in this immensely complicated field; there is also the question of which is which. □

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Hoyle: comparative excellence

P.N.R. Usherwood

Muscles and their Neural Control.
By Graham Hoyle.
Wiley: 1983. Pp.688.
£61.30, $85.85.

GRAHAM Hoyle's monograph, *Comparative Physiology of the Nervous Control of Muscular Contraction,* was required reading in the late 1950s and early 1960s when comparative neurobiology was still a fledgling undergraduate subject. At that time "according to Hoyle", took on an entirely new meaning. After almost 30 years, the revised version, *Muscles and their Neural Control,* has appeared. It is well written, beautifully illustrated and conveys a most comprehensive, if at times birds-eye view of muscle structure and function across the animal kingdom. It is a remarkable achievement, but hardly a surprising one as the author stands out amongst neurobiologists for the breadth of his research and the length of his bench career. Representing as it does a distillation of his life's work, it commemorates an era which is now sadly slipping away from us.

The book is intended for undergraduates and is at its best when describing those muscle preparations which formed the stepping stones of the author's career — there are numerous references to published and unpublished work of Hoyle and his students — but the style falters when less familiar territory is encountered. His description of muscle fine structure is closely linked to cogent functional correlates and, together with a detailed phylogenetic survey of muscle innervation, this provides a successful approach for those seeking a comparative perspective of muscle.

This account of contemporary knowledge and ideas in nerve-muscle physiology is laced with words of caution from a scientist who clearly resents the oft pejorative interpretation of the term comparative. He is also unhappy about current views on the molecular mechanisms of contraction which have emanated mainly from studies of vertebrate muscle. Past challenges to the cross-bridge hypothesis fashioned from his discovery, or rediscovery, of ultrathin filaments in some muscles have met "a very sceptical, unreceptive audience bent solely upon tracking down the nature of X-bridges". Unfortunately, whilst demanding recognition for our own discoveries we often pay scant regard to other "still small voices" crying in the wilderness of contemporary science. Admittedly authors of undergraduate texts have no choice but to clarify their messages by reducing the surrounding noise of science debate and controversy. Hoyle is no exception in this respect, but I doubt whether dissenters from the vesicle hypothesis for synaptic transmission will appreciate his unquestioning acceptance of this proposal.

In the 1950s it was still possible to pursue a successful career as a comparative physiologist; although it was a struggle to keep abreast of the literature and relevant technological development. It is no longer possible to be a "Jack of all Phyla", as a comparison between Hoyle's original monograph and this new book will clearly show. Despite this, I feel *Muscles and their Neural Control* will be as successful as its predecessor and will, in time, become a classical work. □

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Towards a new phase in physics?

Louis Lyons

Quark Matter Formation and Heavy Ion Collisions.
Edited by M. Jacob and H. Satz.
World Scientific (distributed by Wiley): 1983. Pp.586. £49, $70.

PROTONS and neutrons, the constituents of nuclei, are themselves composed of quarks, bound together by gluons. Richard Feynman is quoted as once saying that the chance of discovering anything fundamental in proton–proton collisions is about the same as that of learning about how balance wheels and mainsprings operate by hurling one watch at another. If that is so, what use can there be in attempting to study the collisions of heavy nuclei with each other?

The hope is that, with a large enough number of protons and neutrons (or equivalently, quarks and gluons), a thermodynamic approach may be possible. Furthermore, it is even conceivable that, at high enough energies, the resulting nuclear matter may become so compressed that new phenomena, not accessible in collisions of just a pair of protons, may occur. In particular, increasing attention has been given to the idea that at high densities the quarks and gluons, rather than being confined inside a single proton or neutron, could wander around freely in a quark–gluon plasma extending over the volume of the two nuclei involved in the collision. Such a phase may have existed during the first millisecond of the Universe.

Several crucial questions in this field immediately come to mind. Does such a phase transition in fact exist, and if so under what conditions? Would we expect the new phase to be attainable in heavy ion collisions (and preferably with accelerators already existing)? What would be the best experimental signatures for deducing whether such a quark–gluon plasma had been created?

Such questions were all addressed at length during a workshop held in Bielefeld in May 1982. These published proceedings give a comprehensive view of both experimental and theoretical facets of the subject at the time. They are clearly required reading for nuclear or elementary particle physicists interested in working in this field (although they would have to update their information from material presented at the subsequent conference at Brookhaven in September 1983). The rest of the scientific community, however, can probably safely wait for a few years until some of the basic questions can be answered by the experiments which are now being planned. □

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