What luck!

Brian Pippard

The Physics of Chance: From Blaise Pascal to Niels Bohr. By Charles Ruhla. *Oxford University Press: 1992. Pp. 222. £30, $55 (hbk); £14.95, $27.95 (pbk).*

CHARLES Ruhla has made a brave and largely successful attempt to explain, almost without using mathematics, the various ways in which chance enters into the operations of the physical world. He has a clear and lively style, and the text is excellently translated from the original French. For those who want more details of the arguments there are mathematical appendices, but they can be ignored without losing the thread. Without saying so, Ruhla seems to have two types of reader in mind — the amateur of science and the physics teacher, and both can be encouraged to read this book, except for Chapter 3, to which I shall return.

Even the deterministic world of newtonian physics was not immune to chance. One cannot repeat an experiment exactly, because random disturbances are unavoidable. Most often these lead only to a degree of scatter about the mean behaviour, but sometimes a tiny initial disturbance may be amplified until the final outcome is totally changed. This is the phenomenon of deterministic chaos which has shaken the physicist's confidence in the predictive power of his theories. In contrast to the generation of complexity in apparently simple systems, the operations of chance can also lead to the simplification of impossibly complex problems, as in statistical mechanics. Ruhla does well to take Maxwell's deduction of the distribution of velocities among gas molecules as a characteristic example, but is unfair to earlier workers when he suggests that Maxwell created the kinetic theory of gases. Clausius, in particular, would have been the first to object (he usually was), since it was his statistical analysis of the distribution of free paths that inspired Maxwell's still more penetrating study.

With the discovery of quantum mechanics, chance usurped the very throne from which determinism had reigned, and Einstein's efforts to restore order had in due course the opposite effect. Bell's theorem, recast in a form that could be tested experimentally, shifted the Bohr–Einstein debate from the abstractions of thought experiments to the realities of the laboratory. Ruhla is an avowed disciple of Alain Aspect, whose beautifully conceived experiments on phonon correlations showed there could be no salvation for deterministic physics except by abandoning other classical concepts equally dear to the reactionary heart. In a dialogue with M. de la Palice (the epitome of naive common sense, and presumably a familiar figure to all French readers) he shows how inescapable are the paradoxes of quantum mechanics. Thus in a short book primarily devoted to physics he manages to keep in touch with deep problems of metaphysics, from Democritus to the present day.

Why, then, being so concerned to explain basic principles, does Ruhla descend in Chapter 3 to a collection of rules-of-thumb for estimating errors and significance in experimental measurements? I can only guess that he is, like all too many of his colleagues, in thrall to a tradition that considers no measurement complete unless its probable error is stated. This would be a harmless misconception if the principles of error estimation were easily understood. In truth they are difficult to grasp, and physics students at an early stage of their education are force-fed with cookbook recipes that their teachers would shrink from expounding logically. Surely the emphasis should be on devising good experiments and getting the most out of equipment, leaving the subtleties of gaussian theory to a later stage. Of course, many physicists have to take error analysis seriously; but quite as many (including myself) looking back on their research careers could say with Rutherford: "If your experiment needs statistics, you ought to have done a better experiment". I should have found no fault with Ruhla if he had explained the difference between using a known statistical ensemble to predict experimental results (kinetic theory of gases or quantum mechanics) and inferring a statistical ensemble from a limited number of measurements (theory of errors). This would have been in line with the intentions of the rest of his book; but Student's t-test is wholly irrelevant. There are other faults in this chapter, such as historical inaccuracies, and if another edition is called for I hope Ruhla will rewrite it to the high standard of the rest. □

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Patterns and processes

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Classification, Evolution and the Nature of Biology. By Alec L. Panchen. *Cambridge University Press: 1992. Pp. 403. £45, $80 (hbk); £16.95, $34.95 (pbk).*

THE battle over phylogenetic systematics has eased off, although the hydra continues to raise a threatening head — 'transformed cladism'. Some believe that the true darwinian revolution in comparative biology has only just occurred, with systematists now agreeing to view evolution as an axiom from which to deduce the methods and concepts of phylogenetic systematics. This, they think, was Willi Hennig's original goal when he outlined the principles of cladistics. But transformed or 'pattern' cladists continue to pose heretical questions. How do we know that species exist? How do we learn about descent with modification, if this happened in the phylogenetic past? How do we learn about the underlying process of evolution without having first established a pattern of relative relationships among organisms? Alec Panchen presents a critical review of these and related issues. His text is the product of a distinguished career in vertebrate palaeontology and phylogeny reconstruction, and offers an excellent introduction to comparative biology and its historical background. But by attempting to reconcile too many divergent issues, the book remains elusive, uncommitted and multi-stranded. It is uncompromising on one issue, however, and that is Panchen's answer to the question: "how do we know?"

The book "is meant to be one long logical argument", a pledge of intellectual allegiance to traditional darwinism, which indicates reservations about transformed cladists. Panchen's central thesis is that comparative biology differs from all other natural sciences by the "taxonomic statement", which is not a natural law or a logical construct but a predictive generalization about individuals, that is, about historical entities or taxa. His argument is that any generalization in biology captures some similarity and hence invariance in a continuously evolving and thus continuously changing world. Such similarity cannot be the outcome of a universal law of nature or a logical relation, both of which would be independent of time and space. In the face of continuous change, relative invariance must — potentially at least — constitute a signal for common ancestry, that is, for a unique historical process. Similarity, nevertheless, can be observed — but because there can be no theory-free observation, the theory explaining regularity of character distribution must not be allowed to influence character analysis. Otherwise, the result will be meaningless.