Book Reviews

Cosmological Possibilities

Action at a Distance in Physics and Cosmology. F. Hoyle and J. V. Narlikar. Freeman, San Francisco, 1974. xii, 266 pp., illus. $15. A Series of Books in Astronomy and Astrophysics.

The fundamental laws of physics are time-symmetric, but the world is not. This is a problem that most physicists are able to ignore by confining attention to local physics and a thermodynamically determined arrow of time. But perhaps our local physics is governed by the structure of the universe as a whole, and the problem is the key to the link. *Action at a Distance in Physics and Cosmology* brings together the work that Hoyle and Narlikar have done over the last decade or so in attempting to deal with this matter.

Imagine the world lines of a collection of charged particles laid out before us with a given arrow of time. We can use any combination of retarded and advanced solutions of Maxwell's equations to relate the electromagnetic field at one time to the particle motions and fields in the past and the future. All descriptions are mathematically equivalent, yet in some sense nature seems to prefer the retarded solution: while we see outgoing fields correlated with the motion of a charge, we rarely see incoming fields so correlated. Wheeler and Feynman showed that symmetric solutions (half-advanced plus half-retarded) could lead to retarded interactions if one assumed the universe to be a perfect absorber; that is, every emitted photon must be absorbed somewhere. The advanced field of the absorber charges, "the response of the universe," acts on radiating charges to produce retarded interactions.

The natural approach to such a theory, where radiation emitted is also absorbed, is a direct particle interaction picture. Here we bring in the whole universe to the physical laws. This is the starting point of Hoyle and Narlikar's book. Of course, if the universe absorbs perfectly in both the past and the future there is again no asymmetry. Can we look to the cosmological arrow of time in an expanding universe to provide the asymmetry? Not if the big-bang Friedmann models of Einstein's relativity theory are appropriate to our universe, because these are not future perfect absorbers. However, Einstein's theory is inconsistent with the basic philosophy; if electromagnetism is an action-at-a-distance theory, gravity should be too. It is hoped that this will lead to "better" cosmological models.

To carry out this program, the authors need two ideas: first, a version of Mach's principle, different from Einstein's, that the inertial mass of a body is a function of the masses of all other bodies, and second, that the equations of physics should be invariant under changes of scale which are functions of position.

Finally, if the action-at-a-distance picture is correct it must be possible to quantize it. Hoyle and Narlikar take up the problem where Feynman gave up. They show that effects usually attributed to vacuum energy, hence to the independent existence of the photon field, can in fact be derived from the quantum response of the universe.

Not all of these interrelated themes of the book are linked with equal clarity, possibly because the reader is expected to be aware of the background material, more likely because of the "multidimensional" structure of the book. The authors have preserved the lecture style even to the extent of numbering the lectures within each chapter. This is supposed to allow the reader to dip into the book. One can only imagine the original lectures addressed to an audience in perpetual flux. In any case, the absence of an index of notation and any substantial subject index undermines the attempt.

A new theory of this character might be expected to produce new results on a dramatic scale. That this one does not is its weakness. For how can it be tested? The authors state that from an astrophysical point of view it is desirable to find new cosmological possibilities. It is not clear that the point of view is not more metaphysical. So abhorrent is the idea of a beginning in time for the universe that it must be abandoned at any cost. If the steady-state theory is no longer defensible, then the big-bang model must be attacked. In the end there is the possibility that the big bang may arise from viewing a model with no singularity in a singular conformal frame. The conclusion is that the universe is probably much larger than is usually supposed (!).

There are some occasional tangents such as an isolated remark on quantum gravity (in a completely direct particle interaction theory there should be no gravitational field to quantize). There are some absurdities: Question: how does a particle travel on a space-like curve? Answer: it goes via the universe. But the book is nicely produced with a sensible use of appendices and is a good read.

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Emotion and Bias in Science

The Subjective Side of Science. A Philosophical Inquiry into the Psychology of the Apollo Moon Scientists. Ian I. Mitroff. Elsevier, New York, 1974. xvi, 330 pp., illus. $11.50.

Ian Mitroff's *The Subjective Side of Science* is a sociological, psychological, and philosophical study of some 40 scientists who were involved with the Apollo moon program. As I see it, the question raised by Mitroff is how an objective science is possible when our best scientists seem to be anything but objective in conducting their research. Is it necessary, in other words, for all scientists as individuals to behave in accord with traditional notions of scientific objectivity if science as a whole is to be an objective enterprise? If individual objectivity is necessary, says Mitroff, we are clearly in trouble because many individual scientists seem to behave quite differently much of the time.

The great strength of this book is the convincing argument Mitroff makes in chapter 7 that science as a whole can hope for objectivity *only* if scientists, at least some of the time, behave in ways quite different from the way a traditional notion of objectivity would have them behave. Its great weakness is that Mitroff takes too much time and expends too much effort before he comes to the point.

Much of the book reports results from an intensive, tape-recorded four-wave interview study, conducted by Mitroff himself, of the scientists. Each wave of interviews took place during an interval between Apollo missions. The first wave took place just after Apollo 12 and the last after Apollo 15. The data obtained in these interviews included responses both to open-ended but focused questions and to highly structured psychological scales and semantic differentials. The scientists were asked questions about where they stood at the time of each interview on several controversial scientific questions being addressed by Apollo, such as the origin of the moon, its temperature history, and the origin of mascons and tektites. Another bank of questions was designed to discover the actual epistemologies-in-use within the group. Here the focus was on the beliefs each scientist had about such issues as the relationship between theory and data, whether the hypotheticodeductive method is actually used by scientists, and whether scientific hypotheses can ever be verified or falsified. There was quite a range of opinions on these latter issues, revealing that several epistemologies were actually in use within this group of Apollo scientists.

The data Mitroff obtained in his interviews are rich, but the small number of subjects in his study did not allow him to ask some important questions of his data, such as what explains the variation in epistemologies-in-use, in degrees of commitment to theories of one kind or another, in aggressiveness, and in hostility that he finds within his group of scientists. Mitroff's goal in the analysis of his interviews seems restricted to showing that scientists do not always, or even frequently, conform to what he calls the "Storybook image" of science, according to which scientists are rational, emotionally neutral, universalistic, willing to share ideas openly, disinterested, and impartial. In his study, and in the analyses of almost any sociologist of science I can think of, scientists are often emotionally committed to an idea or theory, particularistic, self-interested, secretive, partial, and biased. It is not new any more to say these things about scientists, and certainly Robert Merton is no believer in the Storybook image of science as Mitroff tries to suggest. In addition, while demolishing the straw man of Storybook science. Mitroff quotes from his interviews excessively, which makes parts of the text drag considerably.

Even though Mitroff's assertions that scientists neither are objective in the traditional sense nor share a common epistemology are not new, they are not trivial. It is essential to know those two facts in order to realize that a new philosophy of science must come into play if we are to agree that science can in some sense ever be objective. Mitroff suggests such a philosophy of science in chapter 7, a philosophy that combines elements of what Mitroff calls the Kantian and Hegelian "Inquiring Systems" (IS) with the traditional Lockean and Leibnizian IS's:

> Science advances through the process of scientists of widely differing persuasions (types and degrees of commitment) thrusting their opposing conceptions and commitments at one another. Through this process science not only subjects its results to severe (but not crucial) tests but also exposes the underlying commitments of its practitioners.
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> It is important to emphasize that in this process commitments alone do not make for the objectivity of science. It is the presence of intense commitments coupled with experiments (Lockean IS), seemingly impersonal tests, arguments (Leibnizian IS), evidence, and general paradigms that make for the objectivity of science. Science, as opposed to other systems of knowledge, is distinguished by the fact that, if not in theory then in actual practice, it has learned how to make use of strong determinants of rationality (testing, evidence, etc.) plus strong emotional commitments [p. 249].

There is much more that is worthwhile in this book about the Apollo scientists, their personalities, their research roles, their views of the Apollo program, and their notions about the moon as a symbol than can be discussed in this review. On balance the book is worth the price.

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Galileo's Thought

The Natural Philosophy of Galileo. Essay on the Origins and Formation of Classical Mechanics. MAURICE CLAVELIN. Translated from the French edition (Paris, 1968) by A. J. Pomerans. MIT Press, Cambridge, Mass., 1974. xxvi, 498 pp., illus. $25.

Galileo. A Philosophical Study. DUDLEY SHAPERE. University of Chicago Press, Chicago, 1974. xii, 162 pp. Cloth, $9.75; paper, $2.95.

Although long a hero of modern science, Galileo became a touchstone for historians of science in 1939 when Alexandre Koyré's *Études galiléennes* showed how to analyze the Scientific Revolution philosophically and thereby how to treat science historically. In the 35 years since then, Galileo has become something of a minor historical industry. Almost all of his major and important minor works have appeared in English translations (the most recent example being Stillman Drake's translation of *Two New Sciences*, University of Wisconsin Press, 1974), and one can count on at least a few scholarly studies each year. It is a further mark of Koyré's influence that, with few exceptions, the secondary literature has followed his mode of analysis (if not always his specific conclusions) and addressed the issues he raised.

The two works under consideration here may serve as good examples. Though quite different in scope, depth, and quality, they agree with each other and with Koyré in viewing Galileo's role in the history of science as a conceptual one, to be examined and grasped within Galileo's works themselves by detailed textual and conceptual analysis and by reference to a similarly textual past. Despite the chronological ordering of the material discussed, structual patterns take precedence over developmental ones, as Galileo's science is reduced to its essentials and then ranged in place between Aristotle's and Newton's. Galileo the man, especially the struggling young professor who could seldom make ends meet and was always on the lookout for a better-paying position while he taught subjects he disliked, or the witty man of letters who enjoyed a good argument perhaps as much as the search for truth, plays no role in either Clavelin's or Shapere's Galilean world.

Nonetheless, if it is Galileo's thought that one is interested in, one can hardly find a better guide than Clavelin's study. In a full, at times even wearying, tour of Galileo's science and its philosophical environs, Clavelin begins with an unusually lucid and informative account of Aristotle's doctrine of motion, emphasizing its ontological dependence on the mover and its cosmological underpinnings in a hierarchically ordered universe. Against the backdrop of this account, Clavelin is able to show that, for all the technical and critical ingenuity displayed, the medieval science of motion as pursued at Oxford and Paris retained its full Aristotelian commitment.

Galileo abandoned that commitment. He did so, argues Clavelin, in chronological stages and at two philosophical levels. His *De motu* (1592) opened the