fact that anthropologists ask the questions "What is man?" and "Whence his coming, becoming, and going?" as related to the particular combination of "rational objectivity" and "participatory oneness" achieved through the Greco-Christian synthesis. Thus, while not ignoring the essentially destructive consequences of European expansionism for the Australian Aborigines, among others, he sees the development of anthropology not as solely, or even primarily, related to this form of imperialism, but rather as related to what he calls the "search for otherness." This contemplation of cultural otherness was born out of participatory values, as exemplified in the Pauline injunction to go out among the peoples of the world with understanding, and rational values, as exemplified in Plato's *Republic*, which utilizes logic to construct a cultural alternative. Burridge sees it as producing intellectualizations concerning the question "What is man?" rather than the question "Who am I?" and as encouraging the exploration of the self through comparison with other, unlike selves rather than through "received tradition and personal insights." As he puts it (p. 232):

> The opposition between rational objectivity and participation in oneness has always informed the substance of anthropological thought and investigation. Given the Christian synthesis, the lineaments of God were to be sought in those whom He had made in His image. Attempting to "save the data" by seeing it as God—the epitome of the rational—might have seen it, through the device of a rationally objective intellectual construct, has always been qualified by the exhortation to engage the participatory values.

Burridge encourages us to look to the early and continuing dialectic interplay between rational objectivity and participatory oneness, rather than to a lineal descent from the scientism of the Enlightenment, for an explanation of the "European signature" distinctive of anthropology. He further argues that were anthropology a product only of the imperialistic and rationalistic trends in Western civilization it would not be characterized by the assumption of a moral imperative in the relationship between observer and observed which exhorts us to preserve each kind of cultural otherness and to value and understand it in its own terms. This imperative is, in Burridge's view, rooted in the Christian tradition.

After delineating these ideas in an initial chapter, Burridge explicates them utilizing familiar problems in anthropology posed by the Australian materials. The second chapter begins with a historical survey of the published material on the Aborigines, then moves to a discussion of the general character of Aboriginal life which builds toward characterizing certain "themes" or aspects of world view inherent in this kind of life. This view depicts Aboriginal man as like everyman in the moral and practical problems he faces except as his options for solution are shaped by cultural and natural circumstances. The third chapter addresses the question "What kinds of relationships do Aborigines and their cultures bear to other kinds of people and their cultures, and how do we express them?" (p. 85). Unlike the preceding chapter, it focuses on those studies informed by the assumption that Aboriginal life is different from other kinds of human life, and examines problems in the task of attempting to conceptualize the differences. This provides Burridge with a context for discussing some of the problems inherent in various classificatory and evolutionary frameworks that have characterized anthropological thought at various times. His critique of evolutionary theory is particularly interesting in terms of his general thesis. He objects to much of the work done under the evolutionary banner as disengaged from the moral imperative, and so from the reach into otherness, and as motivated by professionalism and careerism. The fourth chapter takes its departure from studies of Aboriginal social organization, but more importantly addresses the issue inherent in the fact that the way a human society is seen to be organized is a function of the assumptions held by the observer concerning the purposes of such an organization. He compares the impasse reached in the so-called Murngin controversy by contemporary social anthropologists to that reached by Medieval angelologists concerning the spatial properties of angels in that both stemmed from failure to examine basic assumptions and were only further confused by appeals to rationality and empiricism. The fifth and sixth chapters, on religion and culture change respectively, similarly point to difficulties in understanding these phenomena which have arisen from a failure to fully embrace the "otherness" of the Aborigine because of a dependence on our own logicality.

Burridge is not sanguine about the future of anthropology. He sees its distinctiveness being gradually eroded by professionalism rather than humanism, by incursions of scientific technique in place of intellect, and by a loss of concern for the distinctive quality of other cultures through mathematically based structural descriptions. Since he believes our intellectual progress to be moved by the alternating swings of a dialectic, however, there is some hope that the field might yet be saved as current trends run their course, and Burridge can count the book a success insofar as it moves its reader into a greater awareness of whence he came intellectually and where, possibly, he might go.

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Wartime Scientific Efforts

OR in World War 2. Operational Research against the U Boat. C. H. WADDINGTON. Elek Books, London, 1973 (U.S. distributor, British Book Centre, New York). xvi, 254 pp., illus. $7.25.

This firsthand account of the British antisubmarine operations research effort during the second World War was written in 1946. The book was set in type, but, in Waddington's words, it

> . . . turned out to be a ghost edition. Change in the political climate, the fall in temperature towards the Cold War, led the Security Authorities to withdraw permission for publication. Their reasons were not, of course, explicitly stated, but so far as I can recollect our correspondence, they did not object to the publication of any of the particular factual data mentioned. . . . They had come to the conclusion, however, that the way in which these facts had been synthesized into an account of a coherent scientific study, of all the factors which contribute to the effectiveness with which part of the military forces perform their tasks, would itself be of potential value to other military forces.

This book would have been much more valuable if it had been released for publication in 1946. If it could have been published in 1951, it would have been a valuable adjunct to Morse and Kimball's *Methods of Operations Research*, published that year. Now, of course, its publication can have little effect on the development of operations research, though it adds greatly to the history of that early period.

Certainly, as a detailed report on the use of operations research in antisubmarine warfare, it is of considerable historic interest, since it expands and

illuminates the scanty public records of the parallel U.S. effort. The book reminds me again of the great difficulty OR workers had, in wartime, to pass on their findings to other OR groups. Waddington mentions several studies his group had completed—on depth charge depth settings and on radar disappearing contacts, for example—that our group in Washington had laboriously to redevelop. Vice versa, Waddington's book shows no awareness of any of the work our group was carrying out which would have benefited his group. Detailed comparative study might produce a convincing demonstration of the debilitating effect of overzealous secrecy on the waging of a war—or the development of science.

The book goes into detail on most of the technical problems encountered in antisubmarine warfare—depth charge attacks, radar and visual search, convoying, maintenance, and so on. It reemphasizes the fact, often forgotten nowadays, that successful operations research keeps close to actual happenings and does not lose itself in esoteric theories or computer simulations.

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Biological Uses of NMR

Nuclear Magnetic Resonance (N.M.R.) in Biochemistry. Applications to Enzyme Systems. RAYMOND A. DWEK. Clarendon (Oxford University Press), New York, 1973. xviii, 396 pp., illus. $25.75. Monographs on Physical Biochemistry.

The early chapters of this book present in a pleasantly readable, not excessively mathematical, form the basic principles of magnetic resonance spectroscopy, with considerable emphasis on relaxation phenomena. The effect of unpaired spins plays a large role in this initial discussion of basic principles, as seems reasonable given the focus of much of the subsequent material. There is a good, brief presentation of interactions that influence the chemical shift of resonances (particularly those of importance in proteins). A short chapter is devoted to extrinsic shift probes. This basic section comprises about 20 percent of the book. The last two chapters deal with some examples of the use of quadrupolar nuclei and some useful instrumental techniques. The large midsections present the application of these techniques to a number of biochemical systems; the discussion is organized according to resonance technique used rather than biochemical system studied.

The general principles of nuclear magnetic resonance (NMR) are well and clearly presented. As the authors themselves point out, this book cannot serve as a text for beginners, nor in general is it sufficiently detailed or sophisticated to give the exact theoretical recipes one will need to use in research. (Probably no book of reasonable length could.)

The application of techniques to biochemical problems discussed relies heavily on the use of nuclei with unpaired electrons to influence the NMR parameters of nearby, nonparamagnetic nuclei. (This is not primarily a book on electron spin resonance spectroscopy, though the effects of motion in the ESR spectra of spin labels are briefly discussed.) These techniques allow one to obtain considerable structural and dynamic information. The particular interests (for example, lysozyme) of the Oxford group receive (not surprisingly) considerable attention. Overall, however, this is a good presentation of these aspects of NMR spectroscopy applied to biological systems.

My major disappointment is that the book as a whole does not present a truly balanced view of those aspects of NMR that have potential for productive application to biological problems. To me the approach is too much "here are some techniques; how can we use them in biochemistry?" rather than "here are the biological problems; how can we solve them?" In many cases I find the emphasis misplaced. For example, I think the solution of protein structures in solution by extrinsic shift probes will tell us relatively little not already revealed by the x-ray diffractionist. Indeed, complexation of parts of the protein with the paramagnetic ion may alter the three-dimensional structure as much as, if not more than, the intermolecular forces of crystallization. Further, I think studies of other nuclei hold great promise. Time has already proved wrong the judgments expressed (p. 153) on the utility of ^{13}C as a probe of protein structure and function (^{13}C NMR has been productively used when this nucleus is incorporated not only in the ligand but also specifically, as well as in natural abundance, into the protein itself). ^{31}P spectroscopy has been usefully applied, ^{15}N is in the wings, and I shall be surprised if we do not see important utilization of ^{205}Tl as a magnetic resonance mimic for potassium. In short, NMR has much more to offer toward the solution of problems of central importance to biology than this book would indicate.

Nevertheless, this is a work of considerable scholarship. And, for its discussion of some of the ways of using NMR techniques to study some aspects of protein structure and function, it merits high praise.

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Lesser Moments in Astronomy

The Planets. Some Myths and Realities. RICHARD BAUM. Halsted (Wiley), New York, 1973. 300 pp., illus. $8.95.

This book by a British amateur astronomer has an engaging theme: the false starts and dead ends of planetary astronomy. Eight episodes are presented, most of them hinging on flawed observations performed before the modern era in astronomy.

Some of them are not very well known, such as the naked-eye observation in 1921 by the astronomers W. W. Campbell and Henry Norris Russell and—remarkably enough—Captain Eddie Rickenbacker of a bright point of light, not a planet, seen near the setting sun on one day only. They were admiring a sunset from a verandah of Campbell's house at Lick Observatory. A very bright and brief nova outburst, or a sungrazing comet on an unusual trajectory? No one knows.

There are also some details of 19th-century astronomy which are inadvertently wry, such as the publication in the *Times* (of London, it is true) on 14 October 1846 by William Lassell of the discovery of the rings of Neptune. Lassell is the discoverer of Triton, Hyperion, Umbriel, and Ariel: moons of Saturn, Uranus, and Neptune. Despite such rapid publication and confirming observations by other astronomers, there is no reason at all to believe in the reality of rings around Neptune, and Baum holds that this "discovery" had psychological rather