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Introductory Remarks to the Closing Session [and Discussion]

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Introductory remarks to the closing session

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After listening to the papers and the discussions during the meeting, it seems obvious empirically, whatever one might think *a priori*, that hyperbaric physiology and medicine in fact is a 'subject', in the sense of catching the scientific imagination, and recruiting new workers. Perhaps there is some analogy with another new subject, toxicology; there was a doubt there, too, whether it constituted a coherent area of work, but again, experience showed that, despite widely different backgrounds, toxicologists have increasingly found it valuable to meet together both nationally and internationally, and to undertake jointly matters such as training. One can also see a need for maintaining the tradition of skill in hyperbaric work; it takes a considerable time to acquire, and the occasions when it will be needed are not predictable. Even with present practice, there is a need: Dr Macdonald asked at one point whether, bearing in mind the neurological disturbances that very deep diving seems to cause, together with the other physiological stresses, there was a comparable clinical or industrial procedure that exposes men to a comparable experience; Dr Bevan raised the question whether the addition of an anaesthetic gas (N_2) to a helium–oxygen mixture introduced a new principle, that of giving a drug to enable an industrial task to be done. As regards future work, there was also discussion as to the relative values of work on animal and on human subjects. In all such issues, accurate answers can only be hoped for if there exists a body of informed opinion, with direct experience, to give advice. Perhaps, too, the courage of the submariner, of the diver, and of such experimental subjects as we saw in Dr Bennett's ATLANTIS IV film, is of a kind that those exposed to it never quite lose their interest in such work. The preliminary remarks by the chairmen of the sessions, and the discussions, have all endorsed its importance. The pattern of work is sure to change, and Surgeon Rear Admiral Barnard pointed to its links, not only with biophysics, physiology, zoology and pharmacology, but also with work on man's interaction with machines, ergonomics and occupational medicine.

To survey progress and what needs to be done, one can construct a table (table 1) that very briefly lists the major problems met as one goes deeper, the practical solutions offered, their practical success, and the extent of scientific understanding behind these solutions. Two other areas that should be mentioned because they need attention are general medical care of the diver, including neuropsychiatric care, and the question of any long-lasting effects of high pressure, for which there is still insufficient evidence. Table 1 shows how practical solution has run ahead of scientific understanding. For oxygen poisoning and decompression sickness, advance in such understanding could transform diving practice of all types. Oxygen poisoning resembles radiation sickness in that for both one can at one extreme name highly reactive molecular species, and at the other describe a well known phenomenology, yet the biochemical links between are still obscure. Many of the solutions indicated have resulted from sophisticated engineering and tecchnological advance and much more may be expected. But life below the surface remains dangerous, and equipment failure can still produce acute respiratory hazard,

CO_2 intoxication, severe 'bends', or thermal danger. The interaction between high pressure and anaesthesia has aroused considerable interest, and pressure reversal has been proposed as a test of 'true' anaesthesia. Diver selection has hardly been explored. It is clear that there is considerable individual human variation in the response to oxygen, in liability to decompression sickness, and in the effects of pressure; Professor Naquet's suggestion that, just as astronauts are selected, divers should be too, offers considerable potential when the principles of selection can be established.

Table 1. Survey of progress

problem	*solution offered*	*practical success*	*scientific understanding*
optimal respiratory pressure	apparatus design	reasonable to around 350 m s.w.	partial
'burst lung' in submarine escape	screening training equipment design	reasonable	partial
respiratory gas density at raised pressures	helium	reasonable to 300–400 m s.w.	good
CO_2 accumulation	equipment for removal	good	good (except for mechanism of narcosis)
oxygen poisoning	avoidance of excess O_2 tension	reasonable, but gross practical constraint	poor
decompression sickness	avoidance by slow decompression	reasonable, by empirical adjustment, but gross practical constraint	inadequate on nucleation, gas movement, and pathogenesis of lesions
inert gas narcosis	helium	good	partial (as with anaesthesia in general)
high pressure nervous syndrome	palliated by (*a*) adaptation (*b*) drugs or gases	partial	poor
hypothermia or hyperthermia	heating control systems	reasonable	good
communications	'unscramblers'	partial	partial

Even if the future course of hyperbaric work is uncertain, it is hard to believe support will not be found when one considers that there are problems of such scientific generality as the biological effects of pressure, anaesthesia, and nucleation, together with the continuing need by navies and by industry for at least a maintenance of a body of expertise, and the existence of a very considerable field of amateur diving. The discussion made it clear that for some of the work needed, the facilities required called for a major national centre; but there was an equal stress on the need for diversification, particularly in university centres, so that the subject could be exposed to cross-fertilization by other sciences.

Discussion

H. V. Hempleman (*A.M.T.E. Physiological Laboratory, Gosport, U.K.*). One of the criticisms made of human diving trials is that such small numbers are used that it is difficult to draw useful conclusions.

Although the small numbers of subjects undoubtedly prevent firm decisions on some issues, there are features of this type of experimentation that often compensate. For example, it is common for the decompression time to exceed three weeks of continuous pressure reduction. This represents an extraordinarily severe test of the adequacy of any decompression procedure, despite the small numbers of subjects. Or again, in the 1970 dive to 457 m s.w., the two volunteers had such markedly different responses to the raised pressure (for example e.e.g, tremor) that one could unequivocally state that there will be a similar, or greater, spread of responses in a population of divers.

A. G. Macdonald (*Department of Physiology, Aberdeen University, U.K.*) The title of this discussion meeting was Diving and Life at High Pressures. Consider the curve in figure 1, which is the average profile of the ocean floor, and let us assume that free-diving man might achieve a working depth of, say, 1000 m (100 atm). It is clear that the bulk of the ocean floor will remain out of range of a free diver and will only be explored and exploited by manned and unmanned vehicles.

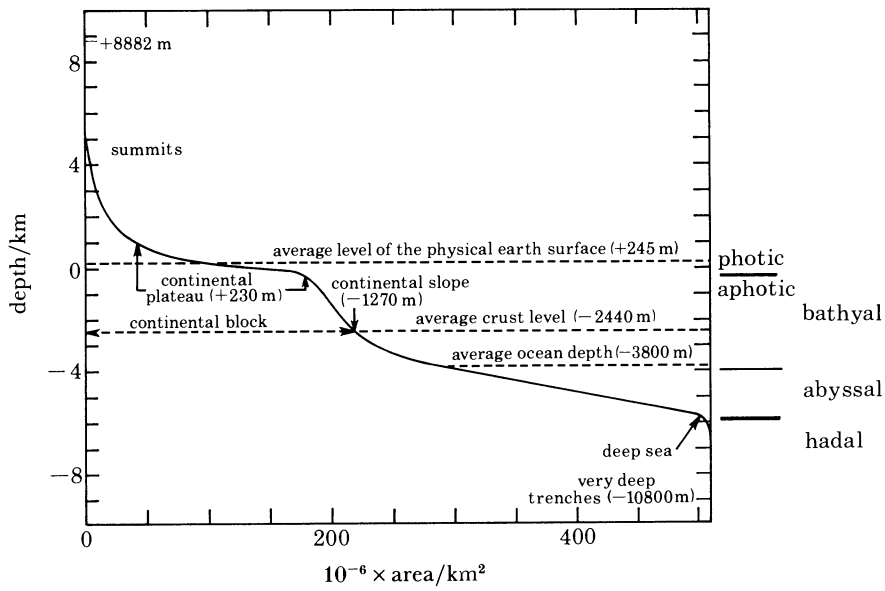

Figure 1. Hypsographic map of the Earth's surface: area against height or depth from sea level.

Animal and bacterial life in the ocean depths are clearly adapted to high hydrostatic pressure and there exists a body of knowledge concerned with the nature of those adaptations. At a biochemical level, enzyme–ligand interactions stabilized to function at high pressure are the main focus of interest (see, for example, Hochachka *et al.* 1975). At a physiological level there exists a quantitative description of the pressure tolerance of deep sea amphipods living at depths down to the abyssal plain (see Macdonald & Gilchrist 1982). Bacterial pressure tolerance is assessed by growth studies, and both the bacterial and amphipod experiments have required the development of special pressure-retaining collecting equipment (reviewed in Macdonald & Priede 1983).

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Macdonald, A. G. & Priede, I. G. (eds) 1983 *Experimental biology at sea.* London: Academic Press.

R. W. Brauer (*University of North Carolina at Wilmington, U.S.A.*). It has been approximately 15 years since I had the privilege of presiding over a conference, entitled 'Barobiology'. The point of view of that conference focused on the implications of the biological effects of hydrostatic pressure within the framework of the biology of marine organisms; clearly, that is one appropriate way of looking at the subject. A second view is that taken today, in which the focus has been largely on the implications of phenomena associated with exposure of air breathing organisms to high pressure environments and their bearing upon the occupational medicine of divers. A third, which has been relatively neglected in the context of both of these conferences, it seems to me, has been the bearing of some of the phenomena we have heard described at these meetings upon human medicine in the form of providing powerful disease models, which benefit from both the reversibility of the majority of the changes we impose, and the precision and replicability with which hydrostatic pressure can be applied as an agent to modify important bodily processes. Altogether, I tend to share Surgeon Rear Admiral Barnard's view that it is the substance of what we investigate and what we discover, rather than the name of the field, that should be the essence of what we propose to defend, and that it is here rather than in any single specialized application of the data that we should seek the key to convincing our peers in the scientific community that the work we are doing is worth supporting. It seems to me that on the whole we have been singularly unsuccessful in convincing our colleagues in adjacent fields, who sit on the panels that adjudicate distribution of funds, of the importance of what we are doing when it is not directly related to somebody helping to cap an oil valve. It is here more than anywhere else that a concerted effort might be in order, to see whether we cannot convince our colleagues of the fact that consequences of great scientific and medical and practical significance can be expected to flow from continued exploration of this type of biological phenomenon and this type of medical event.

A. O. Brubakk (*NUTEC, Bergen, Norway*). The main purpose of diving research is to establish methods and technology for commercial exploitation of the oceans. This requires that divers work effectively and perform highly complex tasks. In the near future, technological improvement primarily will take over only simple work, while the complicated and difficult tasks will continue to be tackled by divers.

During this discussion, the divers' safety, both on a long and a short term basis, have been mentioned. I think that safety should be one consideration, but emphasis should also be put on the general working conditions of the divers and possible ways to improve them. Technical research is needed to improve thermal protection of the diver and breathing equipment. We also need more information about the working diver and his responses to different working conditions. This would require methods for monitoring the diver as well as access to diving sites and diving operations. Too much of the knowledge that could improve all diving is regarded as a company secret. Ergonomic studies of the diver at work are needed. In this field much of the methodology is lacking, but studies could lead to improved design of procedures and equipment, and it would help us to select the optimal use of new technology.

Just as important as applied research are several basic research topics. I therefore agree with the Chairman that more basic research will have to be done to explain h.p.n.s. and decompression problems. In addition to this we also lack some understanding of the effects of chemicals and drugs under pressure.

All this research will cost money. But if we are serious about the need for further exploitations in the sea, it will be money well spent, and in the long run it will probably be cheaper than many of the short term solutions used today. We probably all agree that what is needed are long term commitments to research programmes of high quality. It seems that only the oil companies can afford such commitments at present. I therefore suggest that the oil companies try to define their goals and problems under a more general heading, in a similar way to the U.S. decision to put a man on the moon. The research teams could be selected by competition and the optimal approach would be a coordinated effort where several teams cooperated towards a common goal, with a possibility of doing both basic and applied research.

D. H. Elliott (*Shell U.K. Ltd*, *London*, *U.K.*). It is often said that research into the physiology of diving should be supported by industry. This has become more important since the withdrawal of other funding, especially that of the United States Navy.

Industrial support is generally agreed in principle. It is easy to implement where research can produce some commercial advantage for the sponsor. It is less easy for more fundamental studies or when the results may be applied to the diving industry as a whole. Most of the research that we have been discussing falls into this category. As is well known, for instance to the researchers at the Norwegian Underwater Technology Centre, there are a number of conscientious oil companies and diving contractors who sponsor a significant amount of research and development. The problem to be solved is how to convince the other members of these international industries that they should contribute. An industrial initiative to sponsor a research programme to improve the effectiveness of divers at all depths was initiated last February. It remains to be seen from this how effectively we in industry can persuade our colleagues to support that initiative.

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