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NITROGEN NARCOSIS

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Conducted by
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PREFACE

One of the tasks of the Office of Undersea Research, National Oceanic and Atmospheric Administration, U.S. Department of Commerce, is to study all aspects of coastal waters. One technique being used for this purpose is for scientists to live in sea-floor habitats located in shallow water. NOAA's Office of Undersea Research has one such habitat currently in operation and two more in the works. These habitats use nitrogen-oxygen or "nitrox" gas mixtures or air as the primary breathing gases. Since the nitrogen component of these mixtures is known to cause narcosis, the effect and intensity of this phenomenon and the extent to which it may influence undersea habitat operations are of great concern to NOAA. This is especially true as work is carried out at deeper depths.

The idea for the workshop started with Dr. Andrew Pilmanis, whose interest is based on a need for the current state of knowledge about nitrogen narcosis as it may apply to the upcoming habitat to be built by Catalina Marine Science Center, University of Southern California. A reprint describing the habitat is included as Appendix A.

The workshop was sponsored by the Office of Undersea Research, NOAA, and was conducted by the Undersea Medical Society, Inc. The program was organized by Dr. K.W. Kizer. Dr. Paul Linaweaver hosted the workshop, which was held at the Santa Barbara Medical Foundation Clinic in Santa Barbara, on 1983 December 15-16. The proceedings were edited by Dr. R.W. Hamilton and Dr. Kizer, with the line editing being performed by Ms. Ann Barker.

SUMMARY

The workshop subject is nitrogen narcosis, a prominent effect of the diving environment always taught in diving classes and experienced from time to time by almost all divers.

The role of nitrogen narcosis in modern commercial diving was characterized as "sand beside the road," meaning that it is not normally not a "problem" because divers stay out of it, but it is an enormous operational consideration because of the limitations it imposes to air diving. Narcosis is a primary (but not the only) reason for limiting the diving depth when using air. Scientific divers are most likely to work at depths causing prominent narcosis, as well as a few sport and commercial divers, but Navy divers rarely do.

The obvious question of, "Why this new interest in narcosis?" was answered by a description of the NOAA sponsored undersea habitat to be built at Catalina Marine Science Center and the scientific missions to be conducted from it (see Appendix). Investigators will live saturated in a nitrox (nitrogen-oxygen) atmosphere at 60 to 120 feet, and will excurse--breathing air--to do their work at depths that may extend beyond 200 feet. The concern is not only that the subsea scientists can work effectively and safely, but that the data they gather is reliable.

The biophysical mechanism of nitrogen narcosis is as yet not clearly worked out. It seems to follow the same pattern as anesthetic gases, and there is not any good data to suggest that it has a different basis. At the cellular level it acts more strongly at synapses between nerves than on nerve conduction itself; it may act by blocking conduction through sodium channels in the nerve cell membrane.

Since the big question relates to the effect of narcosis on performance, the literature on studies of this were reviewed, as well as different methods of measuring performance. Performance was divided into perception, integration (or mediation), and execution. Decrements are seen, but in general the effects of narcosis as measured are not great at reasonable dive depths; there are fewer clear effects on perception and memory than on integration and complex task performance. A diver can do a single specific job while narcotized, as long as he can concentrate only on the task at hand and can work slowly, and does not have to deal with distractions or do other tasks at the same time. But in the real world divers have to do "dual task performance," and this type of test should be used for narcosis studies.

This and other studies should try to separate the effects of narcosis itself from the other stressors. It was felt that divers should learn their work tasks (and emergency procedures) in the same context and environment where they will be used. The classical "dose-response" curves describing pharmacological effects are probably present but none of the published results reviewed showed more than the first part of such a curve; the reason

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for this was accepted as being because effects on other than performance (e.g., consciousness) intervene before the full curve can be seen.

The use of video games as measures of performance was discussed; certain of these have been shown to meet formal psychometric requirements, and they have many potential advantages over more traditional tests. They can keep, store, and analyze scores, require a modest amount of training, and are self-motivating. They can automatically match the skill of the player. The idea was discussed of using such a method to get a daily assessment of habitat diver performance; this could document both their day-to-day acclimation to the environment, as well as their ability to do reliable work.

One reason for wanting daily performance monitoring was to follow the change in narcotic level with time. This phenomenon is called by several terms, including adaptation, acclimation, acclimatization, tolerance, accommodation, and others. Other more or less definite usages disqualified several of these, and the consensus of the workshop was to use acclimation to refer to a process whereby some physiological as well as behavioral change in response to a single stressor could be established, and to use accommodation where the question of a physiological change remains open but where a behavioral change can be seen. Thus accommodation is the proper word for the improved performance seen after repeated dives or after living for a time in hyperbaric nitrox.

Regarding the existance of such acclimation or accommodation, it seems clear that performance under narcotic levels of nitrogen improves under two conditions at least. Doing deep air dives improves performance on subsequent deep dives, and performance of divers saturated in a nitrox atmosphere improves towards pre-dive levels over several (5-9) days. This improvement appears to be more a matter of "learning to cope" with the narcosis than a detectable physiological change in it. The conditioned diver plans his moves, is more methodical, and by concentrating on the task at hand can accomplish it. Narcosis diminishes the ability to do multiple tasks and monitor the environment, but a single job can be done safely and relatively effectively.

If it is possible by concentrating to perform under narcosis, what about the ability to deal with emergencies and other unplanned events? Very few accidents on record list narcosis as a primary cause (probably due to inadequate reporting) but it is often regarded as contributing. It was agreed that the diver operating in a narcotic zone should "overlearn" the responses to emergencies, as well as his work tasks. All possible support--good communication, clear procedures, simple equipment, etc.--need to be built into the program. Some researchers feel that 30-50% of the performance decrement in air diving is due to stress, and things that reduce this will improve both safety and job effectiveness. It was also suggested that some heliox breathing mix be stored in the habitat, to permit limited relief from the narcosis in an emergency.

Another established characteristic of narcosis is that individual susceptibility varies considerably. Certain operating groups might select a "resistant" diver for a particular deep job, but the Workshop agreed that this cannot be a criterion for selecting habitat scientists. There might, however, be some sort of conditioning. The group recommended that investigators set up and rehearse their experimental procedures in shallow water before a dive. It was even suggested that some of this might be done under conditions simulating depth from the narcotic standpoint, by using an appropriate mixture of nitrous oxide. The Workshop recommended that performance under these conditions as well as at depth should be monitored (at least in the first few missions) as a specific narcosis study performed as part of the habitat program.

The group showed a surprising degree of acceptance of the controversial concept that living in nitrox saturation causes improved performance on deep excursions. For each atmosphere of acclimation the diver can expect one-half atmosphere or so less narcosis on excursions. This brings up the question stated earlier about where the diver in a narcotic state stands on the classical S-shaped dose-response curve. Does the acclimation shift this curve, or flatten it? Sorting this out should be a research objective.

Other recommendations were for adaptation of video or computer testing methods and continued research on basic narcosis mechanisms, but the dominant theme was that of including ongoing performance monitoring--with a proper experimental design and using controls, etc.--as part of the project.

A collection of abstracts about nitrogen narcosis, UMS Report 61(NN)61-83 was prepared by the UMS especially for this workshop.

[An extract of this was used as an article in Pressure, 13(5) October 1984.]

I. INTRODUCTION: K.W. Kizer

Prompted by the ongoing development of a new science oriented seafloor habitat, this workshop is convened to take a fresh look at the role nitrogen narcosis might play in this and related operations. Our sponsor is the Undersea Research Office of NOAA, U.S. Department of Commerce. In looking at narcosis, we consider four main constituencies: commercial, scientific, sport, and military diving.

Our overall purpose is:

To review the pathophysiology and epidemiology of nitrogen narcosis.

To describe the effects of nitrogen narcosis and the ways that they can be assessed.

To delineate how nitrogen narcosis may differ in nitrox and air environments.

To identify priority areas of research related to nitrogen narcosis.

To do this we asked for specific topics to be covered by presentations of workshop participants, and follow this with open discussions about some of the more significant questions.

Regarding the role of narcosis in modern diving, we would like to define briefly the overall current problem with nitrogen narcosis in commercial, scientific and military diving, with particular emphasis to how it is hindering or otherwise precluding divers and other hyperbaric personnel from accomplishing their operational objectives.

We will review the history of the recognition and early experience with nitrogen narcosis, and what is currently known about the mechanism.

We will discuss methods of studying the effects of nitrogen narcosis in both the dry chamber and wet diver situations. Specific modalities such as psychomotor testing, EEG, evoked potentials, reflex measurement, etc., that have been used in previous experiments and which offer promise for future studies will be discussed, as well as new and modified methods that are adaptable and practical for the operational setting. As a special case, we want to consider whether electronic games and similar modern devices can be used in hyperbaric environments to study the effects of nitrogen narcosis.

We should also consider whether the Luria, Halstead-Reitan or modified versions of such clinical neuropsychiatric tests are possibly useful or even better than currently used psychological/psychomotor tests for assessment of nitrogen narcosis effects.

We should then review what is known about accommodation or habituation to nitrogen narcosis. Specific sub-topics to be covered should include the mechanism of adaptation, at what rate does acclimation occur, to what degree does acclimation occur, can acclimation be facilitated by pre-pressurization over-learning or other manipulation, how long is acclimation maintained after pressure exposure, are there innate factors that affect acclimation (e.g. age, sex, body habitus, intellect, etc.), what environmental variables at pressure affect the rate of acclimation (e.g. ambient temperature or lighting, frequency and duration of task performance, etc.), and whether post-pressurization activities can facilitate the retention of acclimation.

These papers are expected to be reviews of the topics, not original research results, which summarize and interpret available data and which will provide the basis for a directed discussion of the subject.

The discussions should cover personnel selection, specifically whether there are criteria that could be reliably used to select personnel for hyperbaric work based on their susceptibility to nitrogen narcosis and, if so, what are they.

We are concerned about safety; what is the role of nitrogen narcosis as a contributing factor in commercial, scientific, military, and sport diving accidents. Also, what considerations need be given to nitrogen narcosis with regard to diver casualty treatment, with particular attention to the ability of inside chamber tenders to perform therapeutic interventions, the dosage and effectiveness of life support drugs, and drug interactions.

Finally, we hope to conclude with a discussion of current and future priorities of research on nitrogen narcosis.

NITROGEN NARCOSIS: ITS SIGNIFICANCE IN MODERN DIVING: R.W. Hamilton

Introduction

The objective of this workshop is to take a fresh look at nitrogen narcosis as it affects diving operations. In particular, we are interested in the type of diving operations conducted or sponsored by NOAA. These are primarily scuba and surface supplied dives, and diving conducted by means of seafloor habitats involving saturation and excursions; limited operations using a deep diving system are carried out occasionally.

This paper reviews briefly the physiological effects of nitrogen narcosis, tells how these effects are currently dealt with in various kinds of diving, and introduces some thoughts on how narcotic limits might be extended.

Martini's Law: Review of narcotic effects

The original Greek root of the word "narcosis" means "a state of numbing," and Webster recognizes it as chemically-induced stupor or insensibility, but unfortunately the term "narcotic" to most people today has a connotation of illicitness or illegality.

Neophyte divers are taught a useful rule of thumb on how to appreciate nitrogen narcosis. This apothem is called--not entirely facetiously--Martini's Law. By being somebody's "law" it fits in with typical lessons on diving physics, but the name has a meaning perhaps more immediately appreciated than "Dalton" or "Henry." The rule states that narcosis resembles alcoholic intoxication, and that "each 50 feet of depth, breathing air, is equivalent to one dry martini on an empty stomach."

The rule is a good one, and although there are some differences between these two types of narcosis, they share some characteristics. Important points are that the affected individual is a poor judge of his condition, and that the effects have a wide variability between different individuals. The quantitative comparison is not a bad one also, if one ignores facts like body weight, size and timing of the alcohol dose, etc.; a person who is a dangerous driver after 3 quick martinis can be expected to be an unsafe diver at 150 feet.

Although effects of nitrogen narcosis resemble those of alcohol, it is not beneficial to dwell on this resemblance except for the reasons mentioned. In fact, nitrogen narcosis is more properly included as part of the continuum of inert gas narcosis that includes gaseous general anesthetics. Nitrous oxide, a general anesthetic at a partial pressure of 0.8-0.85 atm, has been shown to mimic quite well the effect of air diving (Biersner et al. 1977; Fowler et al. 1983).

The effects of narcosis are primarily euphoria, a slowing of response time, and memory impairment. Cousteau lyrically described it as "Rapture of the Deep." There is a tendency to idea fixation, loss of clear thinking, difficulty in concentrating, a disordered time sense, a delayed response to stimuli, and some impaired neuromuscular coordination (summarized in Shilling et al. 1976).

These effects are detrimental to both work and safety; paradoxically, narcosis should tend to reduce or delay the onset of panic, a major factor in many diving accidents (Bachrach, 1984).

Narcosis in modern diving: Sand beside the road

A Gulf diver recently described to me the operational effect of narcosis as "sand beside the road;" this aptly summarizes narcosis in modern diving. In most situations it is not a problem as such, but it is a major operational consideration and the cost of avoiding the problem is enormous.

Like the sand, narcosis is not a problem as long as one stays out of it, and that is the approach used by almost all diving programs. Staying out of the sand involves limits and alternative methods. Virtually every organized air diving operation has a depth limit for diving on air; other factors are involved, but most air limits are based on narcotic effects. Because circumstances vary, the limits cover a wide range; the rules may be set by the organization or by law.

Recommended maximum depths for sport diving range between 100 and 130 fsw (feet of sea water), although some organized sport dives go as deep as 190 fsw. United States OSHA and Coast Guard regulations prohibit scuba diving deeper than 130 fsw. Air diving is limited to 190 fsw, with short-duration exposures allowed to 220 fsw for up to 30 min.

In both the British and Norwegian sectors of the North Sea air diving is limited to depths not exceeding 50 msw (about 165 fsw). Canada's new regulations limit scuba to 20 msw but say nothing about a limit for air. Commercial diving companies tend to conform to the prevailing legal requirements, but a few may have more restrictive rules. The safety manual of the Association of Diving Contractors limits air diving to 220 fsw, the same as the legal limit.

What remains to be said is that in order to proceed with diving at depths beyond the air limit requires the use of a less narcotic or non-narcotic breathing gas. This usually means a mixture of helium and oxygen; neon and hydrogen are also sufficiently non-narcotic as to be useable as diluent gases. Another approach is to reduce the nitrogen partial pressure by increasing the proportion of oxygen in the mixture. This "nitrox" procedure has proven useful in NOAA operations (Miller, 1979). (The term "nitrox" refers to a mixture of nitrogen and oxygen; it may be higher or lower in oxygen than air, and of course air is a kind of nitrox.)

The other method of dealing with narcosis is just to live with it. Whatever value is used, a limit represents a hard black and white division in an area that is--basically--smoothly changing shades of gray. Tolerance to narcosis varies greatly between individuals, and some are stressed well before the limit is reached, others might go well beyond some limits essentially unaffected. In addition, the same individual becomes more tolerant with both experience and frequent diving. Still another improvement in tolerance comes from living in a nitrox atmosphere. These methods are discussed in following sections.

Another way to be less affected by narcosis at any depth is to use good equipment and techniques. This applies particularly to breathing equipment, where resistance to flow leads to CO_2 buildup and exacerbation of any narcosis that may be present.

Drugs to mitigate narcosis have been suggested and tested in the laboratory (Bennett, 1972), but there is at present no commercial application of this approach.

Variations in tolerance between individuals

The matter of individual susceptibility to narcosis--or conversely, resistance to it or tolerance of it--is a central theme of this workshop, and only a couple of points need to be mentioned here. First, a "good diver" is one who can perform the needed work efficiently and safely. There is a wide range in individual competence in any situation, and competence in diving involves much more than tolerance to narcosis.

Even so, some divers can do good, safe work in the deep air range, and one way for a contractor to get a job done is to have such a person do it. These divers are not selected by psychomotor test batteries, but by performance on the job. Making this type of selection is often at some risk and considerable expense. The diver who is seriously hampered by narcosis will not perform consistently well and will tend to be selected out. On the other hand, the person who knows his job well and performs it smoothly and apparently effortlessly will generally be able to do well despite a considerable degree of narcosis.

One reason a skilled diver can do well is that he knows how to pace himself. This may be more important where breathing equipment is limiting, but seems to be a useful technique in all types of diving work. Learning this can also be considered an element of the adaptive process, covered in the next section.

Tolerance to narcosis from frequent diving

It has long been known that frequent diving tends to increase a diver's tolerance to narcosis or his ability to perform on deep air dives (Shilling and Willgrube 1937; Shilling et al. 1976; K. Hamilton, et al. 1983). Zinkowski (1971) says:

> ... you apparently build up a resistance to its effects during consecutive exposures. Many divers have commented that after not having dived for a week or so, they are definitely groggy during their first dive to 200 feet. On the second dive the effects are noticeably less and after the third or fourth dive they are unaware of any deleterious effects whatsoever."

The feeling among divers is that it is not an "adaptation" in the sense of a physiological change, but rather is a matter of "learning to cope." On subsequent dives the diver tends to rehearse the job mentally and to pace his work. Working under stress or with excessive effort tends to increase the effects of narcosis.

Tolerance from continuous exposure

The use of saturation techniques, particularly those operations including undersea habitats, has caused people to live for days at a time in narcotic levels of nitrox (air deep enough to invoke narcosis has too high a PO_2 to permit continuous habitation). These nitrox saturations have uniformly resulted in a decrement followed by an improvement in performance, and apparent tolerance to narcosis after a few days.

In Predictive Studies II at the University of Pennsylvania (part of the Tektite program) diver-subjects at 100 fsw normoxic nitrox were slightly affected at first but soon returned completely to normal (Elcombe and Teeter, 1973). As an indication of their accommodation one subject stated, "Every afternoon the doctor locks in and we study him." A similar return toward normal function was reported in the NOAA OPS experiments at Tarrytown to a depth of 120 fsw (Hamilton et al. 1973), and observed quite independently by DeLara in Spain (personal communication; cited in the review by Miller et al. 1976).

Recovery was not so complete during experiments involving saturation at 200 fsw, the Nisat I exposure by the U.S. Navy at New London in 1976 (Harvey, 1977; Hamilton et al. 1982), and Nisahex by the Swedish Navy in 1982 (Muren et al. 1983). Here the divers were severely affected, and although many functions improved, the divers did not begin to feel fully normal again until well into decompression. The Nisahex divers had not read the Nisat report before their dive, but on reading it later one observed that their experiences were strikingly (but not surpisingly) similar to those of the Nisat divers.

Interestingly, the Nisahex divers described their accommodation in much the same manner as a diver accommodating to deep air dives, that after a day or two the pharmacological narcosis was still present but they were better able to cope with it.

Moeller (1981) feels it is not proper to refer to this phenomenon as "adaptation." Adaptation should refer to a process involving physiological change, such as those seen in adaptation to altitude or cold. No evidence of a discernable physiological or "organic" change has been reported to account for it, yet there seems little doubt that some increase in tolerance or accommodation or acclimation does occur.

This accommodation to narcosis at a single constant pressure has seen little operational use because most nitrox saturation diving has followed the NOAA OPS pattern of saturation in nitrox with descending air excursions.

Does living in nitrox reduce narcosis on excursions?

This is the critical question regarding the role of nitrogen narcosis in modern diving. The NOAA OPS procedures (NOAA Diving Manual, Miller, 1979) allow efficient no-stop excursions with long bottom times to be made to the deepest part of the air diving range. In fact, with saturation storage depths between 100 to 120 fsw it is possible to perform no-stop air excursions for long working times to depths well beyond the traditional air limits. There is limited experimental evidence that dives following this pattern result in less narcosis from the deep air excursions than would be seen in air dives to the same depth directly from the surface.

The NOAA OPS experiments (Hamilton et al. 1973) used saturation at 30, 60, 90, and 120 fsw, normoxic, and involved excursions with air to as deep as 300 fsw. The broad subjective conclusion from these exposures was that the narcosis observed on the excursions was roughly equivalent to an air dive to a depth equal to the excursion depth minus the saturation storage depth. Another way of stating it is that the accommodation equals the saturation depth. That is, an air excursion to 250 fsw from nitrox saturation at 120 fsw produced narcosis equivalent to a 130-fsw dive from the surface. While this much of an extrapolation might be a bit optimistic, that there was some improvement in tolerance is supported by the data (Schmidt et al. 1974).

A later open-sea implementation of this dive plan, project SCORE, involved saturation on air at 60 fsw and air excursions to 300 fsw in the chamber at Duke University as well as the open-sea operations (Miller et al. 1976). Excursions in the laboratory to 200 and 250 fsw showed slightly better results on performance tests than in bounce dives from the surface, but it was not possible to rule out learning effects. Living in air at 60 fsw did not prevent profound narcosis at 300 fsw. Successful excursions at sea to 250 fsw were completed without problems.

In another series of excursions performed out of the <u>Chalupa</u> habitat, PRUNE II, divers saturated at 100 fsw (normoxic) stopped a descent at 265 fsw due to subjective "impending narcosis" felt by one of them (Miller et al. 1976). A number of other less extreme laboratory exposures are reported in Miller's monograph.

The U.S. Naval Submarine Medical Research Laboratory carried out a series of relevant laboratory exposures, SHAD and Nisat (Hamilton et al. 1982). Narcosis observed on excursions in the SHAD dives were about as expected. One significant observation was that about 2.5 hours after compression to 198 fsw in Nisat I, 2 of 3 divers became nauseated. This was attributed to low oxygen, considering that the PO_2 of 0.22 atm was too low in that dense gas. The nausea was relieved some hours after raising the PO2 to 0.29 atm, but did not completely disappear until 2 days later. No excursions were performed in Nisat I.

A number of commercial nitrox saturation-excursion diving operations following the NOAA OPS pattern have been performed, but few of these are reported in the technical literature (Peterson et al. 1980; Youngblood, 1982). Typically they have involved saturations in the range of 45-100 fsw with excursions in the range 75-150 fsw. Barroom talk does not stress problems with narcosis or decompression from excursions, but there have been numerous problems with the final decompression from nitrox saturation (this has been a common problem in laboratories also). Commercial operations have not reached the depth range of significant narcosis with air.

A recent experiment by the Swedish Navy, Nisahex, involved depths similar to Nisat, but at a PO_2 of 0.4 atm none of the 6 divers became nauseated (Muren et al. 1983). Excursions were made with both nitrox and heliox to as deep as 100 msw. On excursions the Nisahex divers felt some increase in narcosis, but were nevertheless subjectively less affected than on a typical dive from the surface to the same depth. This comparison was only subjective, as no control dives were done. Despite the prevailing vague symptoms of narcosis, the supervisor felt the divers were competent enough to perform the same dives at sea (Eiken, et al. in press).

The approach of making excursions with a trimix--using a fraction of helium--as was done in Nisahex has only been studied slightly. The helium would prevent further narcosis with increasing depth, would allow for reduction of the oxygen to tolerable levels, and would reduce the gas density. The oxygen in air limits its use to something in the range of 250 fsw, due to the possibility of CNS toxicity. A further limitation that interacts with that one is gas density; to dive safely with air in the range beyond about 150 fsw requires low resistance breathing equipment. Such gas switching has the potential to cause counterdiffusion sickness unless proper procedures are followed.

Youngblood has conducted two significant deep nitrox exposures, one at the Commercial Diving Center and another at Duke University (the latter dubbed NOSEX I by the 10 subjects who were NOAA divers; the name seems to be derived from NOAA-Oceaneering Simulated Excursions; Stringer, 1982).

Additional conditions involving narcosis

There are two other situations in diving that involve narcosis but that have not been discussed here. These are the use of nitrogen or other drugs to minimize HPNS on ultra-deep dives (Bennett et al. 1981), and the possible exposure to argon in hyperbaric welding. For the latter situation it is generally considered that argon is roughly twice as narcotic as nitrogen, but more operational testing is needed (Behnke and Yarbrough 1939; Fowler and Ackles 1972).

Conclusions

Modern diving lives in a standoff relationship with nitrogen narcosis. Narcosis causes few problems because most diving operations assiduously avoid it by limiting diving depth with air or nitrox mixtures. Where deep air diving is called for it can be done best by divers with a high individual tolerance to narcosis, who are experienced and skilled in both the work they have to do and in diving itself, and who dive frequently--preferably daily--under the same or comparable conditions.

Where these techniques reach their limit the established procedure is to use non-narcotic (typically heliox) breathing mixtures, but these are expensive.

One promising method for extending the effective range of air diving is to do it with excursions from nitrox saturation, the "NOAA OPS" technique.

People living in nitrox saturation develop a tolerance or accommodation--but probably not a real physiological or pharmacological adaptation--to the nitrogen. The deeper the storage depth the longer it takes to become acclimated; 1 to 3 days are apparently enough for the range 60-120 fsw, but at least 5 to 6 days seem to be needed for the range 150-200 fsw. Accommodation to the 200 fsw depth range appears not to be complete in 5-6 days, and in fact may never be.

The presumption from all this is that the deeper the storage depth and the more complete the acclimitization, the less narcosis will be experienced on excursions.

Although the available evidence is not quite convincing enough to settle the question, this method certainly begs further research, or more specifically, operational testing.

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DISCUSSION FOLLOWING DR. HAMILTON:

Kizer: What, then, is the major significance of nitrogen narcosis in commercial diving.

Hamilton: It limits what can be done with air. One hears stories about this. A consultant to a small diving company told them they could not go to 230 feet with air and do a job safely, and then a big company came along and did it with air, and quite successfully. With experienced divers, I do not think anyone is the least bit concerned about going to 230 feet or so, but they cannot do it in most regulatory environments.

Brauer: Consider the great personal differences. I am fairly sure diving supervisors feel that you can overcome some of the strictures imposed by nitrogen narcosis by selecting individuals (a) who have had much experience, and (b) who are highly resistant. While I have never seen a formal experiment designed to address that question, one of the recommendations that might be worth considering is whether diver selection for narcosis resistance is theoretically sound and practically feasible.

Hamilton: Selection is a sensitive question. All people are created equal and therefore "we must not select them." But in the "NOSEX" experiment at Duke they had ten subjects and they went from one end to the other in sensitivity (Stringer, 1982). The same thing happened in Nisahex. There we had an old, serious Swedish diver who tried to appear totally unaffected, but he was narked; he would not admit it and he would not show it. He did not do anything that exposed the fact that he was narcotized. His performance was quite good, even so.

Kizer: We are going to get into this discussion a little later as I said, but I would like to crystallize things. Is nitrogen narcosis a major limiting factor for shallow commercial diving?

Hamilton: It is a major operational consideration. They have to select the divers, and they have to see to it that the selected divers are conditioned for the dive involved. But with that they can do it. There are other limits. There is an oxygen toxicity limit at 300 feet, and a serious breathing resistance limit (with present day equipment) even before that. Because of these other limits, and because they stay out of it, narcosis really is not a serious problem. But they have to do things to deal with the narcosis. If they want the job done, if they want the diver to be able to remember and to be able to deal with unexpected things, then they have to select the right diver who is conditioned and furnish him with good equipment.

Kizer: Dr. Pilmanis, would you summarize the significance of nitrogen narcosis from the view of the scientific community.

Pilmanis: First a question to Dr. Hamilton. Do you see the commercial deep air diving on the rise or remaining at the current level? Will we see more air or nitrox diving?

Hamilton: Right now the diving market is so competitive that they will do whatever they can to get the job, and that could mean bidding it on air when it should be done on heliox, in the range of 200 fsw or slightly deeper. This may be done by the small company, and they put in their good people--their first team--because they are not busy doing anything else. So they do it successfully. Deep air diving is not on the increase in the long run. It has been done a lot in the last 2 or 3 years because of the market. More companies are able to use air, and financially they are required to do it.

As far as "NOAA OPS" or nitrox saturation/excursion diving is concerned, there seems to be a slow but steady development of this technology, and I look for it to increase.

Pilmanis: Specifically, in the scientific field, the NOAA/USC NUR Program habitat is currently out to bid, and in 2 years it will be in the water. This habitat will have a nitrox environment somewhere between 60 and 120 feet with excursion diving as deep as the 150-200 foot range. The scientists need this national facility because they require extensive bottom times which are very difficult to attain with surface-oriented diving.

There are two important questions. The first is, could a scientist take his data, submit a manuscript to a journal, and find that an editorial board rejects the manuscript on the basis of a suspected narcosis effect on the accuracy of his results? I am not sure if that question can be answered, but it is being asked with regard to the legitimacy of doing this sort of work in that kind of environment.

The second question deals with "accommodation" to narcosis during excursion diving from saturation. This excursion diving is certainly one of the main attractions to the use of saturation diving. Those are the two main questions that we have.

You mentioned Hydro-Lab, which is an ongoing operation at a fairly shallow depth; they are on air at 47 feet. I remember a couple of years ago, Dr. William Schane, Medical Director of Hydro-Lab, said he was finding subtle narcosis problems even at that shallow depth. The effect was, for example, noticed when playing chess. It did not seem to inhibit their primary work, but there was certainly an effect; it was difficult to assess.

Kizer: How about, military diving, Captain Biersner or Dr. Carter?

Biersner: I will make a statement, which incidentally is not the official view or opinion of the United States Navy. It will be my interpretation of Navy diving activities. I do not see nitrogen narcosis as a serious problem in the immediate future for the Navy. As both Berghage and I have shown, the vast majority of diving done by the Navy today is in depths less than 100 feet and for periods less than 30 minutes using air. Most of the Navy saturation dives are for experimental or training purposes. It is extremely rare, outside of special projects, to find any operational saturation diving in the Navy. The only experimental work that is being done now involves the shallow air saturation dives at New London (NSMRL). The dives are made primarily to provide the decompression schedules to rescue submarine personnel. They are going presently to about 132 feet of sea water. I believe this depth limit is imposed by oxygen exposure limits. However, the submarine personnel are not going to be expected to "perform;" they are simply going to be in a decompression mode until they reach normobaric conditions. I do not see the Navy being particularly concerned about the effects of air at this depth. It is simply a decompression-mode "elevator" problem.

Kizer: Dr. Egstrom, I suspect that you will address this in more detail later, but would you make a comment about the significance of nitrogen narcosis for sport divers?

Egstrom: Before I do that, I would like to make the observation that we just finished the 1983 update on the scientific diving statistics--they come in from the various agencies. It is interesting that since the beginning of scientific diving in this country there has been what is called "the 200 foot certification." In the scientific diving community in general, the divers are staged down in essentially one atmosphere levels over a period of time. There is a significant amount of work that has been and is being done in the range of 150 to 200 feet--with some exceptions, such as Scripps, where they have instruments as deep as 230 or 240 feet. They do this on air, and from the survey so far, no one has had any operationally troublesome narcosis. As they go down, they do the job that needs to be done, they get the instruments in and out, and they have not had any accidents or anything else. I think this just points up some of the things that Dr. Hamilton has already mentioned, that there appears to be a difference between what we anticipate is going to happen and what does happen. I am not saying that narcosis is not present, but operationally it does not seem to be as much of a problem as we might suspect.

It is far more complex in the sport diving community, because the training agencies have for years established either 100 or 130 feet as the limit to which a sport diver should go on air, because of the potential problems with narcosis. For example, cave divers were going deeper and novices were being killed; now they are doing less penetration and being more careful.

Today we are seeing a resurgence of this deep diving syndrome. Deep diving courses are being offered by most of the agencies; I think this is a function of diving travel promotion and the challenge of the deep "walls." Ten years ago a wall dive was something that few people had made and there was little understanding about them. Now a number of resorts have their favorite spot on the wall, 185 feet in one location, 225 in another, and everybody at some time during that week is going to make that wall dive; as a matter of fact many trips schedule a wall dive at undetermined depths. In some places you really do not know where you are, because a lot of the depth gauges do not go as far as the people do; a problem could be developing. But once again, in terms of actual accidents that are traced back to narcosis, it does not seem to be an operational problem considering the numbers of people that are being exposed. I do think that it is an area where a good deal of education is needed and perhaps this workshop can provide some insight into the wall divers, because they will just continue to push that practice until the statistics start to rise, and then--hopefully --they will develop controls. But it would be nice if they would develop controls before they kill a lot of kids.

Kizer: So you would say at this time, for the sport diver, narcosis is not a major contributing factor for accidents?

Egstrom: In the statistics that I have looked at--and I have gone through as much data as I could--since 1979 there are only three incidents where narcosis is even mentioned as a possible involving factor; previous to that no one has ever mentioned it.

Biersner: Some problems attributed to narcosis are probably related to vestibular dysfunction--the divers lose their orientation and start going deeper instead of toward the surface. In analyzing Navy diving incidents I have found that for many drownings in the Navy, disorientation was a contributing factor.

Linaweaver: In recent litigations the question of narcosis has come up. I think there is a tendency to lump together problems that amateur divers and sport divers have had in terms of narcotic effects. There are other effects of deep air diving such as inexperience with the work of breathing at depth, the increased air comsumption at depth, as well as the narcotic effects. So I think that any blame given specifically to narcosis may be just a partial factor and all the other things have to be taken into consideration.

THE HISTORY OF NITROGEN NARCOSIS: Charles W. Shilling

History usually records what people did and when they did it; we are following this pattern here. The basis for what I have to say is a collection of 473 abstracts on nitrogen narcosis drawn from the UMS's data base (Shilling, 1983). This report is as complete as we could make it. If some of your papers are left out, please send us a reprint. Numbers given here refer to that publication. This count makes no judgement as to the quality of the paper or extent of its coverage of narcosis, only that the topic was mentioned.

The first result of our search is given in Table III-1, which lists the authors who have six or more papers included in the search.

The second item is Table III-2, the Frequency of Publication by Ten Year Intervals. It is interesting to note that the frequency of publication increased markedly in the period 1970 to 1979. If the same rate continues for the present 10 year period we will have at least 400 papers.

Table III-1. Authors on nitrogen narcosis and their publications

Authors	Number of papers
Bennett, Peter	54
Miller, K.W.	19
Rostain, J.C.	14
Hamilton, R.W.	13
D'Aoust, B.G.	10
Fowler, B.	10
Paton, W.D.M.	10
Thomas, J.R.	10
Vaernes, R.	9
Ackles, K.N.	8
Behnke, A.R.	8
Brauer, R.W.	8
Lemaire, C.	8
Roby, J.	8
Gardette, B.	7
Hesser, C.M.	7
Lambertsen, C.J.	7
Dosett, A.N.	6
Kinney, J.A.S.	6
Moeller, G.	6
Walsh, J.M.	6

Table III-2. The frequency of publication on nitrogen narcosis by 10-year intervals

Interval	Number of publications
1920-1929	1
1930-1939	6
1940-1949	1
1950-1959	11
1960-1969	74
1970-1979	264
1980-1983	111

The third approach takes a closer look at what was done in the first 30 years. The earliest papers we found were by Meyer and Hopff in 1920 and 1923 (#294 and 295). They reported on the theory of narcosis by inhalation anesthetics and by narcosis caused by inert gases under pressure. They state as a rule: "All gaseous or volatile substances induce narcosis, if they penetrate the cell lipoids in a definite molar concentration...."

The 10 year period, 1930-1939, was the first early production period with Damant (#144) in 1930 reporting that "some of the men became abnormal mentally (or emotionally) and had no memory of what they were doing before ascending." In 1932 Leonard Hill and A.E. Phillips wrote of "divers having pyscho-physiological problems...."

Behnke and coworkers in 1935 reported on "The psychological effects from breathing air at 4 atmospheres pressure" (#42). In 1938 came "Physiologic studies of helium," and in 1939, "Respiratory resistance, oil-water solubility, and mental effects of argon, compared with helium and nitrogen" (#43 and 47).

> Simulated breathing resistance was tested in a respiratory resistance machine which admitted to and withdrew from a Benedict spirometer 32 liters of gas per minute. The tested mixtures were: 86 percent argon, 14 percent nitrogen, 76 percent helium, 4 percent nitrogen, 20 percent oxygen, and air. At pressures of 1 to 4 atmospheres the resistance to the passage of these gases to and from the spirometer varied as the square roots of their specific gravities (air=1). The oil-water solubility ratio for argon is 5.32 to 1 compared with a value of 5.24 to 1 for nitrogen, and 1.7 to 1 for helium. Argon is twice as soluble, however, in water and in oil compared with nitrogen. The narcotic effect of argon is greater than that of nitrogen at high pressures of 4 to 10 atmospheres, corresponding to depths of 100 to 300 feet. At a pressure of 1 atmosphere no difference would be detected between argon, nitrogen, or helium with respect to respiratory resistance or psychologic effects. (Author's summary)

In 1937 Shilling and Willgrube reported on the first "Quantitative study of mental and neuromuscular reactions as influenced by increased air pressure" (#395). This was a mass observation. We were planning some submarine escape research to find out how long one could stay down and still come up at a steady rate and not develop decompression sickness. I overheard one of the boys say, "Well tomorrow is my turn for a cheap drunk." I said, "If they think they are drunk, there must be something we can measure." And from that came this study, which according to some people was the first attempt to quantitate nitrogen narcosis.

> Data, which include problems, number cross-out test, and light-to-touch reaction time worked at both atmospheric pressure and under increased air pressures, are presented. These data give quantitative evidence of the slowing effect that increased air pressure has upon the normal mental and neuromuscular responses. Experience in work under pressure tends to lessen this effect, and low mental ability undoubtedly enhances early and extreme failure under high air pressure. The cause of this effect is discussed. (Author's summary)

Hydrogen came into the picture by Bjurstedt and Severin reporting on the "Prevention of decompression sickness and nitrogen narcosis by the use of hydrogen as a substitute for the nitrogen (The Arne Zetterstrom method for deep-sea diving)" (#102).

Dr. Peter Bennett appears in the literature starting in 1957 and accounts for 54 of the papers on nitrogen narcosis appearing after that date.

From 1960 on we consider to be modern times and assume that all of you are familiar with the work in the field.

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DISCUSSION FOLLOWING DR. SHILLING:

Linaweaver: Dr. Shilling, in these periods that you addressed, what do you think was the motivating factor leading to the studies? There must have been something. People just did not out of the clear blue say, "I am going to study nitrogen narcosis." What, in your opinion, prompted the investigations?

Shilling: Well, in the 1930 to 1939 period I think was definitely, "Hey, this is something new and maybe it is going to bother the Navy." Behnke was Navy, the British Navy was in it also. I think they were trying to see if there was anything that was really going to be important to us. At the time we did not know enough about it to say.

In the 1960-1969 period, Peter Bennett came in, and in addition to studying narcosis itself he found problems associated with HPNS. I think they were trying to sort out that situation. I do not know that at the present time if we really know at a cellular or molecular level what is going on.

The reason for all the excitement now is the excursion from a saturation depth, where there may be nitrogen narcosis. Thus the interest relates to habitats and excursions from habitats.

Kizer: From looking at these papers, particularly 1970-1979, it seems they are included because they mention nitrogen narcosis and give some discussions of what its effects are. But most of them are not really the "contributing" sort of papers; their number correlates with the increased interest in diving and diving medicine. Many of these papers are just reviews of diving medicine and they talk about nitrogen narcosis.

Brauer: I would like to make one injection here I think it is politically important one. A major thrust during the 1960's came from the anesthesia community. In addition to whatever was done for underwater work, there has been an enormous amount of input into very basic medical, physiological, and pharmacological problems. The resurgence in the 1960's, in which I had a hand, sprang from Linus Pauling and a number of others who criticized some of the ongoing arguments and offered some new ideas.

Pilmanis: When I initiated this workshop, my biggest concern was the anecdotal aspect of this reported accommodation or adaptation. According to these anecdotes from the SCORE program and others, if you saturate at 60 feet you can dive productively to 250. I was appalled at the credence given to those ideas because there was practically no data at all. And yet funding was moving ahead as if this were proven fact.

Shilling: Well, maybe I am too critical, but I have been looking at this field in depth and I think there is a lot left to be done to give us some real proof.

Biersner: For my review I evaluated the performance work primarily based on the bibliography. I reviewed over 60 articles that in some way claimed to have assessed performance. Twenty-seven of these articles were acceptable for scientific evaluation. Only these 27 articles provided adequate data on which to judge what was being done in the experiment and also provided sufficient quantitative data to estimate the performance impairment.

When I was reviewing this material I noticed a tendency for researchers assessing performance to try to use a new technique to determine the performance effects of nitrogen narcosis. As new techniques are developed in experimental psychology, they are adopted several years later in work involving nitrogen narcosis. Therefore, single reaction time measures were used initially, followed by more sophisticated measures (such as evoked potentials) in later work. Overall, work on performance assessment and narcosis has been diminishing over the years. The peak years seemed to be between 1965 and 1975, and work has been much reduced since 1975.

Brauer: Dr. Kendig, would you share my impression as a pharmacologist that one thing that tends to be absent in the literature is any awareness of the dose-response relations. In other words, one may well find competent and sophisticated testing procedures reported, but reports of successful correlation of partial pressures with magnitudes of effects are extremely rare. I think as one continues in this, perhaps one of the things we ought to do is to recognize that this is a pharmacologic problem that ought to be dealt with by standard pharmacologic research methods which begin with establishment of dose-response relations.

Kendig: With respect to the anesthesia side of things, I found in going through the human literature in this excellent bibliography that it is very difficult to compare the reports of nitrogen narcosis with anything related to general anesthesia, because it is quite obvious that one is not dealing with anesthesia in the human reports. One is dealing with a more or less mild impairment. For any other agent we know about the anesthesia dose-response is very steep, so that the ED_{50} is not that far from ED_{90}. It is highly probable that with nitrogen one is well up on the dose-response curve and quite far from anesthesia. But I assume it is on the same continuum.

Brauer: In fact, Dr. Kendig, the problem implicit in this extremely dangerous misuse by our sports divers of the so-called Martini's Law is the question of linearity. Like all dose-response curves, this one is sigmoid, and we are impinging on the break between the flat part and the extremely steep central portion of this curve. With myself as subject, I deliberately explored the transition zone and found it to be a very dramatic border. For me it comes between 250 and 270 feet. Suddenly the symptoms become severe and threatening with a slight further increase in depth. You can then drag yourself another few feet down if you are careful and very well aware of what is going on, but it is an extremely steep and extremely non-linear thing. It seems to follow quite comfortably what we know from general pharmacology.

Biersner: The only worthwhile data I know of on this problem have been collected using the stabilimeter. These data show a curvilinear function. In most of the performance work there has been an extremely restricted range of doses used and the performance effects seems to be occurring near this margin. Most of this work was limited to exposure around 10 atmospheres and not much below 6 atmospheres. Ten atmospheres may be the level at which the curve starts to elevate dramatically. At 13 atmospheres, performance becomes extremely disorganized. Because the range of study has been limited the effects appear to be linear.

Hamilton: Our laboratory found what we feel looks like the first part of a dose-response curve in several parameters. I agree that one has a lot of trouble defining the end of the curve. We do not know whether it follows that pattern or just crashes; you cannot get down on that part of the curve because other things take over. But I think it looks very much like a dose-response curve (Schmidt, et al. 1974).

Linaweaver: That has been my view of the subject for many years, I have not found that anybody has approached human nitrogen narcosis, or even animal, from the point of view of the affect on the perception, the central correlation, and the execution. Individuals have taken one aspect of that triad and emphasized it in their research. I know of no study that has looked at the role of perception in terms of what is perceived, how it is correlated in the central nervous system and how execution is performed. Individuals historically have taken one aspect of those three and emphasized their research on it, but have not tied the three together. And until that is done I think we will still be floundering around.

Editors note: Work by Barry Fowler and colleagues at York University in Toronto is making progress in separating the elements of this triad.

Kizer: I think that is a critical thing as far as scientific divers are concerned. They are going to execute based on their perceptions; that whole issue has to be addressed. We have further comment on this later.

Reference

Schmidt TC, Hamilton RW Jr, Moeller G, Chattin CP. Cognitive and psychomotor performance during NOAA OPS I and II. Report CRL-T-799. Tarrytown, NY, Union Carbide Technical Center, 1974.

PATHOPHYSIOLOGY OF NITROGEN NARCOSIS: A SUCCINCT REVIEW: Joan J. Kendig

INTRODUCTION

It is a long-held assumption of research into anesthetic mechanisms that the phenomena of nitrogen narcosis and clinical general anesthesia are fundamentally the same. This has been the case since the demonstration by Meyer that anesthetic potency of a series from methane to chloroform was correlated with solubility in non-polar solvents, leading to the famous Meyer-Overton postulate that anesthesia consists of the solution of these gases or volatile compounds in some "lipoid" region of the central nervous system[1]. The range of substances tested for goodness of fit to the relationship was later expanded by others to include those inert gases which require hyperbaric pressures to produce anesthesia. Perhaps the earliest self-consistent series to use loss of righting reflex as a quantifiable end point was published by Miller, Paton and Smith in 1967[2]. They tested a series which included nitrogen among seven inert monatomic gases, as well as nitrous oxide and eleven organic gaseous and volatile agents. On a logarithmic scale, the points relating anesthetic partial pressure to solubility in either olive oil or benzene fit a straight line remarkably well. The same points do not fit as well when hydrate dissociation pressure instead of lipid solubility was used, placing another nail in the coffin of the alternative Pauling-Miller theory that clathrate formation rather than solution in lipid is the fundamental anesthetic process[3,4].

These and similar experiments established the fact that on this broad scale it was possible to consider hyperbaric nitrogen narcosis together with anesthesia by more potent gaseous and volatile agents as a single phenomenon to be explained if possible by a unitary theory. The rest of this review will consider more detailed comparisons of nitrogen with other anesthetic agents at the level of observations on intact human subjects and animals, isolated tissues and cell membranes. In addition, there will be brief consideration of the interaction of hyperbaric nitrogen with other factors in the diving environment which may modify its narcotic effects at a given pressure, including interactions with drugs as well as physical variables. Finally, there will be an attempt to draw some conclusions about the nature of nitrogen effects on man at both sub-anesthetic and anesthetic pressures, and to suggest some directions for further research on unanswered questions, of which there are many.

Nitrogen Narcosis: Central Nervous System (CNS) Manifestations

Electroencephalogram (EEG) and evoked responses

Initial attempts to use the EEG to monitor onset and depth of anesthesia produced disappointing results in part because of the difficulty of identifying a pattern of EEG changes unique to anesthesia and separable from the confounding effects of changes in perfusion, oxygenation, CO_2 removal, and temperature. However, within the last decade computerized power spectral analysis has permitted the extraction of a characteristic pattern of shifts in amplitude and frequency of the EEG. Leaving aside the question of epileptogenicity exhibited to varying degrees by different inhalation agents, increasing anesthetic concentrations are associated with, at light stages, an increase in total voltage and a progressive downward shift in the predominant frequency; at deeper levels amplitude declines and the EEG becomes isoelectric. The same pattern of shifts is observed with hyperbaric nitrogen[5,6]. Grossly at any rate, therefore, nitrogen appears to fall within the same pattern as other inhalation agents. However, between agents the characteristic power spectrum of the EEG differs sufficiently that limited conclusions can be drawn about the identity of the anesthetic state; it is probable that much of the EEG is produced by brain areas other than those immediately responsible for loss of consciousness.

In an analysis of the visual evoked response, nitrogen and halothane were shown to exert similar effects on latency. However, nitrogen at 8 and 16 atmospheres was more potent in reducing amplitude of the response[7]. In other studies of visual, auditory and somatosensory evoked potentials, nitrogen at pressures from 4 to 22 atmospheres was shown to behave in general like other anesthetic agents[8,9], although there is considerable variation and the correlation between these responses and other measures of anesthesia is poor.

Reversibility

No evidence has been brought forward to suggest that infrequent episodes of anesthesia by any clinical agent are associated with irreversible physiological changes. The presumption of complete reversibility should also hold for nitrogen narcosis. Nitrogen's relatively limited tissue binding would predict an intrinsic recovery time at least as rapid as that for any other inhalation agent.

Interactions with hyperbaric pressure

A common feature of all classes of anesthetic agents is the ability to suppress the hyperexcitability associated with hyperbaric pressure *per se*. Nitrogen shares this property, and its efficacy in postponing the High Pressure Neurological Syndrome (HPNS) in experimental dives has been extensively tested[10]. Inasmuch as both narcosis and HPNS can proceed simultaneously in the same animal to a point at which the animal exposed to extremes of anesthetics and high pressure wakes only to convulse[11], it appears likely that the site responsible for anesthetic antagonism to HPNS is different from the anesthetic site.

Pressure, of course, also antagonizes anesthesia produced by a wide variety of agents, including nitrogen[12]. The original demonstration of the universality of this antagonism led to the hope that it could be used to demonstrate the proposed unitary site of anesthesia. However, more recent studies have revealed that both high pressure and anesthetics are protean in their effects; the demonstration of nonlinear slopes for the pressure/anesthetic dose-response curves and differing slopes among agents has weakened the probability of a single site for anesthetic action[13]. It is possible that pressure antagonism to some or all agents is indirect at some level, and thus the phenomenon may have less to say about an anesthetic site than had been first proposed. The alternative possibility, directly opposing actions of anesthetics and pressure at some common site of action, remains a viable hypothesis. The question of single[12] versus multisite[13] anesthetic theories remains to be resolved.

Nitrogen anesthetic partial pressure does not differ significantly from that predicted from its lipid solubility, and it behaves additively in reducing the anesthetic pressure of other more potent agents such as nitrous oxide. Less lipid soluble inert gases such as neon, hydrogen and helium exert anesthetic effects which must be corrected for opposing pressure effects. An equation assuming constant linear additivity of anesthetic and pressure effects among inert gases adequately predicts the anesthetic contributions of these less potent inert gases, up to total pressures of approximately 100 atmospheres[14]. Above that pressure the equation does not fit the experimental data, for reasons which are not clear.

Cellular Effects of Nitrogen

Nerve cells

A characteristic of many anesthetic agents is that they inhibit excitatory synaptic transmission at concentrations lower than those required to block the conducted action potential. Although this property varies considerably among agents[15], it has been widely assumed to imply that blockade of synaptic transmission is a common site of anesthetic action relevant to the phenomenon of anesthesia. It is not clear that nitrogen shares this property. Although certainly the inert gas pressure required for complete block of the sciatic nerve is very high[16], in a study on the sympathetic ganglion 8 atmospheres of nitrogen was found to exert no effect on either the action potential or the transsynaptic response to single stimuli[17].

In a study on the sodium currents which underly the action potential, nitrogen was found to exert a reversible dose-dependent depressant effect at pressures of 7 and 14 atmospheres[18], close to the pressures which are associated with nitrogen narcosis. Since other more potent anesthetic agents depress sodium currents only at pressures well above the clinical range[19], this finding suggests that sodium channels may be particularly sensitive to hyperbaric nitrogen. Like all other anesthetic agents so far studied, nitrogen depressed the sodium currents in part by favoring the inactive (non-conducting) channel conformation state, an action which was reversed by the addition of helium pressure[18]. This action of pressure is the probable basis for pressure reversal of anesthetic conduction block[20], the only instance so far found in nerve cells in which pressure and anesthetics act antagonistically. In its shift of sodium channel inactivation level and its subjection to pressure reversal, nitrogen appears to act on the sodium channel in a manner similar to general anesthetic agents.

Hyperbaric bradycardia

Hyperbaric pressure per se slows the beating frequency of isolated sinus node preparations, an action that appears almost perfectly antagonized when hyperbaric nitrogen is substituted for hydrostatic pressure[21]. The outcome is that elevated pressures of nitrogen up to 140 atmospheres exert no net effect on beating frequency. When the potency of nitrogen in antagonizing hyperbaric bradycardia is compared to that of nitrous oxide, the ratio is similar to that for inducing general anesthesia[21]. In antagonizing hyperbaric bradycardia, it appears that nitrogen is acting in a fashion similar to other anesthetic agents. The membrane basis for the phenomenon is not known.

Nitrogen Effects in Model Membrane Systems

Inasmuch as the presumed site of action of nitrogen, like other anesthetics, is in some lipid region of cell membranes, it was of interest to examine inert gas effects on lipid bilayers. Hydrostatic pressure increases orderliness in bilayer membranes and raises the transition temperature for the crystal-fluid phase transition. In general, anesthetic agents exert opposing effects. However, depending on the lipids employed and the depth of the probe in the membrane, nitrogen may exert anesthetic-like effects, no effects, or an effect in the same direction as hydrostatic pressure[14]. At present, therefore, it is not possible clearly to relate nitrogen effects on lipid bilayers to nitrogen narcosis.

The alternative possibility is a site of action in some hydrophobic region of membrane proteins. In the case of nitrogen as with other anesthetics, there is little positive evidence in favor of such a site.

Interaction with Other Factors in the Hyperbaric Environment

Drugs

Inasmuch as the additivity between anesthetic agents and hyperbaric nitrogen is clearly established, it is reasonable to assume that any drug with an anesthetic or intoxicating effect will reduce the threshold for nitrogen narcosis. The primary agent of concern in this regard is alcohol. It has been emphasized that the surface actions of drugs cannot be used to predict their actions under hyperbaric conditions. Not only do sea-sickness remedies and antihistamines potentiate nitrogen narcosis, but also, unexpectedly, amphetamines[22].

Physical variables

Low temperatures decrease anesthetic dose requirements. However, the anomalous effect of temperature on nitrogen lipid solubility may lessen this effect in the case of nitrogen narcosis[23]. Any impairment of gas exchange which increases CO_2 will potentiate nitrogen narcosis[23].

Conclusions and Future Directions

From the above review, it is clear that the actions of nitrogen are broadly similar to those of anesthetic agents. To a certain extent one is justified in extrapolating the results from studies on anesthetic mechanisms with one inhalation agent to another, including inert gases such as nitrogen. However, it is also clear that distressingly little is known about anesthetic mechanisms for any agent, especially those which require a hyperbaric exposure. In isolated cellular systems, anesthetic effects on voltage-dependent and transmitter-activated ion channels are known to a certain extent, although more studies are necessary to confirm the extrapolation to inert gases. One may anticipate that the newer neurophysiological techniques of single channel analysis and reconstituted ion channels will permit molecular-level identification of the site and nature of anesthetic action on these proteins in the near future. The greatest lacuna in knowledge for all anesthetic agents is likely to remain at the level of the central nervous system. For all agents including nitrogen, the change in brain cell function which produces the anesthetic state is unknown.

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DISCUSSION FOLLOWING DR. KENDIG:

Hamilton: Did you say you could block the synapse and not the conduction?

Kendig: Larrabee and Posternak showed that when they measured axonal conduction in response to a stimulus and also conduction across the excited synapse, for most agents the synaptically evoked response was much more sensitive to anesthetics than were the ones directly evoked. It is odd, but I can not find any evidence that this is true for nitrogen. Certainly it is true in a series of papers by Carpenter.

In my own work, we find when we look at the sodium channels responsible for the conducted action potential, general anesthetic agents depress the sodium currents; with nitrogen one can get a dose-related depression at 7 and 14 atmospheres of nitrogen that is reversible after decompression.

But there is evidence that perhaps the sodium channels are more sensitive to nitrogen. Fourteen atmospheres of nitrogen shifts the slope of the curve of sodium channel availability, indicating that nitrogen promotes the inactive state of the sodium channel.

Linaweaver: How do you compare the effects of a high pressure of nitrogen with other anesthetics without eliminating the pressure effect?

Kendig: At 7 to 14 atmospheres any pressure effect that we would see would be very small.

Hamilton: Is there any "time" component to these things you just mentioned? This could account for the difference; the nitrogen might get there quicker than the anesthetic agents.

Kendig: Benzocaine will infiltrate as rapidly as one can perfuse. Ether is enough more lipid soluble so that with it there is a significant time to equilibrate, on the order of 2 or 3 minutes. If anything nitrogen is slower because the method of the experiment involves putting the preparation in a pressure chamber and the diffusion time through the chamber is probably the limiting factor. There is also a transient heat of compression.

Greer: Do your results say that there is not any nitrogen effect at 1 atmosphere?

Kendig: If there is an effect, it is a slight effect on the slope.

Hamilton: You mentioned a temperature effect. The temperature of the cells we are talking about could only go down a couple of degrees, and that is not very much of a change on the absolute scale. Where would we encounter low temperatures?

Kendig: True. If you had hypothermic human, you would be in trouble for other reasons. In small animals (e.g., rodents) where you get a marked temperature change on exposure to a hyperbaric atmosphere, you probably will get some interaction with the anesthetic effect.

Kizer: Is there any data on the effects of nitrogen on different types of synapses? Has that been looked at at all?

Kendig: It has been looked at for general anesthetic agents in a study which showed in a system that had both an excitatory and inhibitory synapse, the response to the general anesthetic was much more sensitive for the excitatory transmitter. That is, the inhibitory transmitter for an anesthetic agent is relatively little affected. This has not been done for nitrogen.

Brauer: One point implicit in some of the things you have said with regard to anesthesia and anesthetic potency is that mouse data are remarkably well transferable to man. For instance, we just obtained human data with regard to the hydrogen/nitrogen relation which seem to come exceedingly close to the results derived from mouse experiments.

Kendig: Using what as an input?

Brauer: Using in the mouse loss of righting reflex. In the case of man, the inevitable, "I recall it feels like...," as well as concordance with results of performance tests, and one subject that lost consciousness.

Kendig: Let me summarize what I have said regarding the comparison of nitrogen narcosis and anesthesia.

Nitrogen narcosis as an anesthetic is similar to other anesthetic agents. At least we can say there are not any clearly defined differences. We really do not know what anesthesia is, by nitrogen or by other agents.

1. There may be a linear relationship between potency and lipid solubility for nitrogen and all the other agents.

2. The CNS manifestations are broadly similar; all the agents are pressure reversible, and there is additivity.

3. Anesthesia dose-response curves may be non-linear.

4. CNS manifestations do differ in detail.

5. Regarding cellular effects, there is a great lack of data because of the difficulty of doing even simple experiments in a hyperbaric chamber.

ASSESSMENT OF THE NARCOTIC EFFECTS OF NITROGEN: R.J. Biersner

Formal assessment of the performance, psychological, neurophysiological and biochemical effects of nitrogen narcosis has a long history, beginning with the classic work of Shilling and Willgrube in 1937 (1). To date, this literature has not been reviewed in a comprehensive fashion, although selected portions of this work have been compiled and discussed previously (2,3). In reviewing the work on nitrogen narcosis, the principal source of information was Nitrogen Narcosis: A Bibliography with Informative Abstracts published by the Undersea Medical Society (4), supplemented with other material held by the author. In order to delimit this task, the following criteria were used in selecting work to be discussed:

1. The complete published work had to be available either through the Undersea Medical Society or the National Library of Medicine (e.g., work published only as an abstract was excluded).

2. Only work involving air or a nitrogen-oxygen mixture as a breathing medium was included. Results from hyperbaric experiments in which inert gases such as helium, argon and hydrogen were used were not included, nor were findings included from research using trimix gases. (The problems involved in differentiating the narcotic effects of nitrogen from the pressure effects--such as High Pressure Nervous Syndrome--associated with most of this trimix research are impossible with the data available currently.) While experimental work on the narcotic effects of nitrous oxide indicate that this inert gas may someday be useful in simulating the effects of hyperbaric nitrogen (5), these findings nevertheless were excluded from this analysis because a consensus does not exist regarding the validity of this approach.

3. Data on the narcotic effects of nitrogen used in association with saturation diving were not included because the extent to which these effects were mediated by adaptation is poorly understood. (Additionally, saturation dives typically use small sample sizes, making the reliability of the results questionable unless replicated.)

4. The published work had to provide a complete description of the tasks or measures that were used, as well as the hyperbaric and control conditions, and provide data adequate to determine the approximate percentage of variation in the major effects (i.e., the difference between control and experimental conditions).

5. Only work involving humans as subjects was included because such work is more common than animal research and because animal data, especially performance data, are difficult to interpret in terms of human responses.

Using these criteria, a total of 27 published works were selected for this review. The narcotic effects described in these publications were divided into four major categories--performance, underwater, neurophysiological, and miscellaneous--and are discussed below.

PERFORMANCE EFFECTS

The vast majority of work on nitrogen narcosis involves assessment of performance effects. In order to simplify the organization of this work, the performance effects were subdivided further into a modified version of the behavioral classifications used by Bachrach (6)--perceptual processes, mediational processes, and motor processes. These data are presented in Table 1.

Although the five criteria described above were useful in selecting the most carefully documented results on this topic, large differences nevertheless existed among these experiments. The major differences were:

1. Techniques -- Several different tests may have been used to assess the same behavioral activity.

2. Measurement -- The effects, even if assessed using the same test, may have been expressed differently (e.g., number correct versus number of errors).

3. Subject characteristics -- The human subjects used in these experiments varied widely in age, education, intelligence, motivation, and diving experience. (However, most were males.) Additionally, the subjects varied substantially in the extent to which they had practiced the different tests.

4. Control conditions -- Baseline measures usually were collected at 1.0 ATA before the hyperbaric exposure, although some baseline data were collected at low hyperbaric pressures. Even if conducted at 1.0 ATA, some baseline testing may have been done inside the chamber, while in other experiments baseline data were collected outside the chamber. The interval between baseline and hyperbaric testing varied widely among the experiments. Little effort was made to equate such environmental variables as temperature and noise for baseline and hyperbaric testing.

5. Experimental design -- Proper experimental design was a major problem. As noted above, practice effects and poor environmental control contaminated some of the data. Additionally, many experiments failed to counterbalance baseline and test conditions or to randomize exposure among subjects if multiple hyperbaric exposures were used (an admittedly difficult procedure in some cases).

The variability and experimental design problems that are common features of much hyperbaric performance research, and the limitations that these problems impose on interpretation of the data, have been discussed extensively by Baddeley (3) and Bachrach (6) and need not be reiterated. However, despite the diversity found among this work, some common themes emerge from the performance data presented in Table 1. The vast majority of work has been performed using air at simulated depths in excess of

TABLE 1

Performance Effects of Nitrogen Narcosis Tested in Hyperbaric Chambers

Process	Activity	Performance Test	Gas Mix	ATA	Approximate % Decrement**	Reference
Perceptual	(a) Searching for and receiving information (i.e., detect, scan, observe and inspect)	Simple reaction time				
		Visual (single light flash)	N_2O_2	4.2	NS (response time)	23
			Air	5.0	NS (response time)	23
			Air	10.1	23% (response time)	1+
	(b) Identifying objects, actions and events (i.e., identify, locate, and discriminate)	Complex reaction time				
		Visual (multiple light flashes)				
		2 - choice	Air	4.0	21% (response time)	9
		4 - choice	N_2O_2	4.2	NS(response time); NS(# errors)	23
			Air	5.0	NS(response time); NS(# errors)	
		5 - choice	Air	6.6/6.7	13%/23% (# current/30 sec)	24‡
		Auditory(1,2, or 4 digits)	Air	6.7	11% (response time)	25[1]
		Card sorting	Air	2.0	NS(response time); NS(# errors)	26
			Air	4.0	NS(response time); 118%(# errors)	26
		Signal detection (discriminate dot pattern in dot array)	Air	6.6	2% (arc sine transformation of detection percentage)	24[2]
Mediational	(a) Information processing (i.e., memorize, categorize, code, compute and translate)	Short-term memory (STM) and learning				
		Paced sequential memory (STM)	Air	6.6	6% (# correct)	24[2]
		Digit span forward/backward (STM)	Air	7.4	NS (length of series correct)	28
		Word association (STM; hour pairs)	Air	7.4		28[3]
		Easy associations			24% (# correct)	
		Difficult associations			42% (# correct)	
		Word list (learning)	Air	8.6	24% (# correct/last 6 trials)	27

TABLE 1 (Cont'd)

		Long-term memory (LTM)				
		Arithmetic memory	Air	7.0	NS (# correct)	18[4]
		Word association	Air	7.0	38% (# correct)	18[5]
		Word list (nouns)	Air	10.0	40% (# correct/cued recall) 28% (# correct/uncued recall)	27[6]
		Continuous free word association	Air	13.0	45% (# responses) 109% (response time)	17[7]
	(b) Problem solving and decision making (i.e., analyze, calculate, compare compute, and plan)	Arithmetic				
		Addition/subtraction (add 2, 3-digit nos. and subtract 1, 3-digit no. from sum)	Air Air	6.6 6.6	40% (# correct) 15% (# correct)	24[2] 24[8]
		Multiplication (2-digit no. times 1-digit no.)	Air	7.0	NS (# correct)	29[9]
			Air	7.1	30% (# correct)	30[10]
			Air	8.6	37% (# correct)	30[10]
			Air	10.1	31% (# correct) 28% (# attempt)	14[11]
		(1 digit no. times 1 digit no.; subtract product from 2-digit no.)	Air	8.0	31% (# correct) 23% (# attempt)	31[12]
		Conceptual reasoning (spatial logic)	Air	4.0	33% (time/problem)	9
		Sentence comprehension (verbal logic)	Air	7.0	NS (# correct)	18[13]
Motor	(a) Simple, discrete (i.e., activates, connects, disconnects, joins, moves, and sets)	Purdue pegboard	Air	4.0	8% (pieces assembled/30 secs)	9
		Ring and peg	Air	6.2		21[14]
		Simple			NS (total time)	
		Complex			26% (total time)	
		Minnesota rate of manipulation	Air Air	6.6 6.7	13% (total time) 13% (total time)	24[2] 24[2]

TABLE 1 (Cont'd)

	Bennett hand tool dexterity	Air	6.6	14% (total time)	24[2]
		Air	7.1	20% (total time)	30[10]
		Air	8.6	18% (total time)	30[10]
	Screw plate	Air	8.0	NS (# completed/6 min)	31[12]
(b) Complex/continuous (i.e., adjusts, regulates, synchronizes, and tracks)	Mirror drawing	N_2O_2	4.2	17% (# errors); NS(total time)	23
		Air	5.0	16% (# errors); NS(total time)	23
	Two-dimensional tracking	Air	6.7	42% (integrated error)	25[1,13]
	Adaptive two-dimensional tracking	Air	6.6	40% (rate of improvement)	24[2]
		Air	6.7	21% (rate of improvement)	24[2]
		Air	7.0	78% (rate of improvement)	29[9]
		Air	7.1	28% (rate of improvement)	30[9]
		Air	8.6	37% (rate of improvement)	30[9]
	Stabilimeter	Air	6.7	35%/46% (lateral, eyes open/closed) 34%/38% (sagittal, eyes open/closed)	24[8]
		Air	10.0	92%/103% (lateral, eyes open/closed) 89%/72% (sagittal, eyes open/closed)	32[15]

* Results are for maximum pressure exposure (in ATA) used in the experiment.
** Baseline comparison data were collected under dry surface conditions (1.0 ATA) unless noted differently. Percentage decrement was determined by the formula, $\frac{C-E}{C} \times 100$, in which C is the average performance score under comparison (baseline) conditions and E is the average score under the experimental (narcotic) conditions. These percentages have an error of ± 10% for cases in which the data were obtained from figures.
+ Linear function (5.5, 8.5 and 10.0 ATA).
‡ First trial only at 6.6/6.7 ATA; control at 1.3 ATA.
1 First dive only; average for all digits; large counterbalancing effect.
2 First trial only at 6.6 ATA; control at 1.3 ATA
3 First recall trial only.
4 Overlearned addition task.
5 Learned predive; recall at depth.
6 Control was H_eO_2 at 10.0 ATA; words learned at 1.0 ATA, recalled at 10.0 ATA.
7 Linear function (4.0, 7.0, 10.0 and 13.0 ATA).
8 First trial only at 6.7 ATA; control at 1.3 ATA.
9 First trial only at 7.0 ATA; control at 1.3 ATA.
10 Pre-habitat condition only.
11 Linear function (4.0, 5.5, 7.1, 8.6 and 10.1 ATA)
12 Low CO_2 condition only.
13 Large counterbalancing effect.
14 First trial only at 6.2 ATA.
15 Curvilinear function (2.2, 4.0, 7.0 and 10.0 ATA).

6.0 ATA. In the few cases involving lower hyperbaric pressures, the observed effects usually were either nonsignificant or of marginal significance. At the other extreme, little work has been done to determine the effects of exposure to hyperbaric air pressures in excess of 10.0 ATA. Within this limited range (6.0-10.0 ATA), the effects appear to covary linearily with pressure. (The major exception to this observation is stabilimeter performance, which has a definite curvilinear function.) The widest range of hyperbaric pressures has involved work on tests that assess perceptual and motor processes. However, these data indicate that narcosis does little to disrupt perceptual processes until extreme hyperbaric pressures are reached ($\geq$10.0 ATA). Surprisingly, tests of motor processing are perhaps the most sensitive of those tests listed in Table 1 to the performance effects of nitrogen narcosis, demonstrating a distinct covariance with hyperbaric pressure. (As noted below, these tests also are extremely sensitive to at-sea hyperbaric conditions.) More work has been done using motor tests than tests of any other process, while the least number of experiments have involved assessment of perceptual processes. While much research has been performed using tests of mediational processing, the overall results of these tests are disappointing. Variability among these test effects is more restricted than for motor tests, and the data show more random variation from a linear function than do the motor performance data. The restricted range of impairment effects for the mediational data indicates that some factor other than (or in addition to) nitrogen narcosis may be responsible for these effects. The data for long-term memory (LTM) are especially puzzling, particularly in view of the observation by Biersner (7) that LTM is highly resistant to the effects of narcotic gases. A possible explanation for this effect is that in half of this work, the test material was learned at 1.0 ATA and then recalled later at depth, indicating that state-dependency effects (8) may have confounded the data.

Another conclusion from the information provided in Table 1 is that these data do not provide support for the classic work of Kiessling and Maag (9) in which motor tasks were found to be significantly less sensitive to nitrogen narcosis than problem solving tasks. Not only are motor effects at high atmospheric pressures (6.0-8.0 ATA) similar to those effects obtained for mediational tasks, tests of motor processes are more variable across a wide range of hyperbaric pressures than are tests of mediational processes. In view of the choice reaction time, problem solving, and motor performance data presented in Table 1, serious doubt is raised regarding the validity of the narcotic effects obtained by Kiessling and Maag at 4.0 ATA. More work needs to be done to replicate these original results and to determine if other factors may have been involved in the effects that were found.

UNDERWATER EFFECTS

Despite the importance of determining the narcotic effects of nitrogen under environmental conditions that resemble those to which divers are typically exposed, little scientific attention has been paid to this problem. Interestingly, none of the work that has been done in this area has been conducted in a tank or pressurized wet pot. Every effect described in Table 2 was obtained under at-sea conditions. (However, temperatures, turpidity levels, currents, bottom conditions, and distance from shore varied among these experiments, with some of this variability being substantial in a few cases.) Additionally, most of this literature has been contributed by British researchers, with none of work resulting from American efforts. (British researchers appear to appreciate the stress and state dependency effects associated with at-sea conditions more than American researchers. These effects will be discussed more thoroughly later.)

An examinaton of Table 2 shows that the entire body of work was conducted near 4.0 ATA while breathing air, with control measures taken at 1.3 ATA, usually during descent to 4.0 ATA. These tests have been administered principally to assess mediational processes and simple motor activities. None of the tests assessed perceptual processes or complex/continuous motor activities. In view of the limited number of experiments that have been conducted under at-sea conditions, the important experimental variables (i.e., experimental and control depths and breathing gas) have been highly consistent, and the results for each test have been replicated in most cases (a rare occurrence in this literature). The high degree of experimental consistency may account for the reliability of the performance effects presented in Table 2. Nevertheless, notable differences can be seen among the various activities that were assessed at-sea, as well as between performance on these at-sea tests and similar tests described in Tabel 1 that were administered under hyperbaric chamber conditions. Information processing, at least as determined by tests of short-term and long-term memory, appears to be immune from at-sea interference (down to 4.0 ATA). However, both problem solving and simple motor activities demonstrate significant at-sea effects. Moreover, the extent of impairment is roughly equal for both problem solving and motor activities. This comparison is at odds with the data in Table 1 which show that for hyperbaric pressures between 6.2 and 8.6 ATA, problem solving performance is more than 50% worse than simple motor performance. In addition, while the impairments in problem solving activity noted in Tables 1 and 2 vary in a linear fashion with hyperbaric pressure, the combined data from Tables 1 and 2 for simple motor impairments do not conform to a linear function. On the average, percentage impairments in simple motor activity at 4.0 ATA under at-sea conditions are nearly the same as those simple motor impairments found in hyperbaric chambers at air pressures between 6.2 and 8.6 ATA. The disproportionate at-sea effects are not the result of water factors such as drag or temperature because these factors were controlled by exposing the divers to an at-sea depth of 1.3 ATA. Little data are available to account for these differential motor effects. Psychological stress is a possible factor, although anxiety has been found to impair the efficiency with which motor tasks are performed (e.g., energy expenditure) more than accuracy or time to completion (10). (The counterbalancing effect described in reference (11) of Table 2 appears to be an example of psychological stress disrupting a mediational process

TABLE 2

At-Sea Effects of Nitrogen Narcosis

Process	Activity*	Performance Test	Gas Mix	ATA	Approximate % Decrement**	Reference
Mediational	(a) Information processing	Short-term memory (STM) and learning				
		Word list (STM; nouns)	Air	4.0	NS/NS (# correct)	12+
			Air	4.0	NS (# correct)	13
		Long-term memory (LTM) Word list (nouns)	Air	4.0	NS/NS (# correct)	12+
				4.0	NS (# correct)	13
	(b) Problem solving and decision making	Arithmetic Addition (add single digits)	Air	4.0	16%/NS (# correct) 133%/142% (% error)	12+
			Air	4.0	16% (# correct) 133% (% error)	13
		Sentence comprehension (verbal logic)	Air	4.0	17%/10% (# correct) NS/NS (# errors)	12+
					16% (# correct)	13
		Picture completion (from the "Culture Fair/Free Intelligence Test")	Air	4.0	NS (#correct/3 min)	11‡
Motor	(a) Simple, discrete	Bennett hand tool dexterity	Air	4.0	16%/18% (total time)	12+
			Air	4.0	22% (total time)	13
		Screw plate	Air	4.3	16% (total time)	3[1]
		Digit copying	Air	4.0	22% (average height, last 5"9s")	11

* See Table 1 for explanation of activities.
** Baseline comparison data were collected at 1.3 ATA (at-sea).
\+ Two at-sea experiments.
‡ Not significant overall, but large counterbalancing effect.
1 5% decrement in chamber (comparing 1.3 and 4.3 ATA).

under at-sea conditions. In this case, divers who performed the picture completion test first at 1.3 ATA prior to performing the test at 4.0 ATA did significantly better on the test than divers who were administered the test in the reverse order.) Additionally, equivocal results have been obtained from two of these at-sea experiments (12, 13) in which cortisol (a biochemical factor widely associated with psychological stress) was correlated with the extent of simple motor impairment at 4.0 ATA. A more likely explanation is that the motor impairment under at-sea conditions is mediated in large part by state-dependency. Other research (8) has shown that performance on tasks learned under a specific set of environmental conditions (e.g., dry land) does not readily transfer to other, extreme environmental conditions (e.g., underwater). Under such extreme conditions, the tasks essentially must be relearned. Therefore, the additional motor impairments found under at-sea conditions (compared to hyperbaric chambers) may be the result of having to relearn these tasks under disparate environmental conditions. Surely, this intriguing observation deserves more research attention, especially in view of the implications of these findings for diver training.

NEUROPHYSIOLOGICAL EFFECTS

Table 3 presents the results of research using a variety of neurophysiological measures to assess the narcotic effects of hyperbaric air. The complex and sophisticated procedures involved in collecting and analyzing these data (often requiring the use of elaborate computer software) appears to have limited the number of researchers who have worked in this area. Additionally, none of this work has been done while the subjects were immersed in water. The limited range of hyperbaric pressures and the use of a number of diverse procedures make interpretation of these data difficult. However, a few consistencies do emerge from this work. Evoked potentials (EPs), regardless of modality (i.e., auditory, visual or somatic) appear to be much more sensitive to narcosis than electroencephalograms (EEGs). The data cited in several of the references in Table 3 showed a strong linear relationship between EPs (regardless of modality) and depth. EEGs appear to be sensitive only to gross neurological dysfunction, and are likely to add little to available performance indicators in prediction of narcotic incapacitation. If visual EPs are to be used, the data in Table 3 demonstrate that 16 cps striped patterns are the most sensitive measures of narcosis. However, interpretation of any EP data remains equivocal because the relationship of variations in EPs to other narcotic effects, especially behavioral responses, has only been demonstrated directly in a single case among the references listed in Table 3 (see Ref. 14). In this case, variations in auditory EPs were correlated highly with arithmetic performance. Fowler and Ackles (15) earlier noted that most research in this area has failed to provide adequate evidence that EPs are valid indicators of narcosis. To be valid, Fowler and Ackles state that EPs must (1) vary proportionately with the partial pressure of the narcotic gas, (2) be associated significantly with the narcotic potency of different gases, and (3) correlate with

TABLE 3

Neurophysiological Effects of Nitrogen Narcosis

Neurophysiological Variable	Stimulation Source	Gas Mix	ATA	Approximate % Decrement*	Reference
Auditory evoked potential (AEP)					
Amplitude/latency of N_1-P_2 component	60 dB click (1 cps)	Air	10.1	55%(reduced amplitude); NS (latency)	14**
Visual evoked potential (VEP)					
Amplitude/latency of general, 1-sec record	Striped pattern (16 cps)	Air	8.6	40%(reduced amplitude); NS (latency)	33+
Amplitude of 100 to 200-msec component	Striped pattern (2 cps)			NS	
Amplitude/regularity of general, 1-sec record	Striped pattern (16 cps)	Air	6.7/7.0	20%(reduced amplitude); NS (regularity)	34‡
Amplitude of 160-msec component	Striped pattern (1 cps)			59%(reduced amplitude)	
Electroencephalography (EEG)					
Alpha (amplitude)	Eyes open/closed	Air	2.0	NS	34[1]
Theta (amplitude)	Eyes open/closed			NS	
16 Hz (amplitude)	Striped pattern (16 cps)			NS	
Alpha (amplitude)	Eyes open/closed	Air	6.7/7.0	NS	
Theta (amplitude)	Eyes open/closed			NS	
16 Hz (amplitude)	Striped pattern			70%(reduced amplitude)	
Somatic evoked potential (SEP)					
Amplitude of N_1-P_2 component	Mild electric shock (1 cps)	Air	8.6	38%(reduced amplitude)	35[2]

* Baseline comparison data were collected under dry surface conditions (1.0 ATA).

** Linear function (4.0, 5.5, 7.1, 9.6 and 10.1 ATA).

\+ Linear function (3.0, 6.0 and 8.6 ATA; N = 4).

‡ One-half subjects tested at 6.7 and 7.0 ATA respectively; use first trial (dive) data only.

1 One-half subjects tested at 6.7 and 7.0 ATA respectively.

2 Pre-habitat condition only; N = 3.

behavioral measures. In most of the data cited by Fowler and Ackles, EPs failed to meet the first two of these criteria, while the little work done on behavioral correlates has lead to mixed results. The major advantage of EP technology is in determining the extent to which primary sensory mechanisms have been disrupted. Behavioral correlates would indicate the significance of these mechanisms in the overall behavioral response. A more worthwhile approach would be to determine which of the several processes underlying behavior has been impaired using the event-related potential (ERP), especially the P300 component. ERP technology shows promise of differentiating these processes, including stimulus reception, stimulus evaluation, response selection, and response execution, with P300 serving as an indicator of stimulus evaluation (16). To date, this technology has not been applied to the problem of nitrogen narcosis. The failure to use this technology in conjunction with performance measures stands as a major scientific shortcoming in this area.

MISCELLANEOUS EFFECTS

A smattering of work has been done assessing the biochemical and psychological effects of nitrogen narcosis. Surprisingly, despite the number of standard physiological measures (such as heart and respiration rates, pulmonary functions, and blood pressures) that have often been administered in conjunction with these experiments--especially during saturation exposures involving nitrogen narcosis--only in one case (13) have these measures been correlated with depth of exposure (but not with the behavioral effects of narcosis). In this case, neither heart rate nor electrocardiograms varied according to atmospheric pressure. The biochemical work has emphasized biochemical factors typically associated with psychological stress, notably adrenalin, noradrenalin and cortisol. In the first case (17), adrenalin and noradrenalin determinations made on urine samples collected at various air depths did not correlate with pressure exposure. Research cited in Table 1 (see Ref. 18) found significant elevations in plasma levels of prolactin and growth hormone, and significant reductions in urinary adrenalin and plasma testosterone, among novice divers exposed to air at 7.0 ATA. These results also provided evidence that plasma levels of cortisol and prolactin were correlated significantly with mediational task performance assessed postdive. These data were interpreted in terms of psychological stress to the hyperbaric conditions. Subsequent at-sea research to a pressure of 4.0 ATA on air showed that predive elevations in plasma cortisol levels or a heightened urinary adrenalin-noradrenalin ratio were associated with poor manual dexterity (12). A later effort (13) to replicate these plasma cortisol findings demonstrated that while predive cortisol levels rose universally among the divers, individual differences in this biochemical response were not related to measures of mediational and motor performance. (The predive/postdive biochemical assessment conducted in these last two cases underscores the emphasis on stress <u>versus</u> narcotic effects.)

None of the work that was reviewed determined the extent to which personality varied during nitrogen narcosis, although such effects have been observed during exposure to nitrous oxide (19). The absence of work in this area is most likely the result of an assumption that personality is impervious to stress (i.e., narcosis) effects--an erroneous assumption in view of the nitrous oxide data. The paucity of work on personality and narcosis is unfortunate because a variety of personality measures developed during the last 20 years may be useful in determining who is most susceptible to the effects of narcosis (even if the personality measures as such do not vary at depth). In the only research in which susceptibility to nitrogen narcosis was related to personality factors (11), a measure of psychological defensiveness was found to be correlated significantly with performance on a mediational task administered during exposure to air at 7.0 ATA. Work using other self-report psychological measures to assess the narcotic effects of nitrogen also has been limited. Only two pertinent works were found that conformed to the selection criteria defined above. In the first instance (20), subjects were asked to indicate the extent of narcosis on a single numerical scale (with the normal state being assigned the number "10"), and then to rate each of 41 adjectives describing drug states according to whether that state was more or less prevalent (from normal) under the hyperbaric conditions. Both of these measures varied significantly in the expected direction (i.e., indicative of more severe narcosis) with increased hyperbaric air pressure (4.0 and 7.0 ATA). Additionally, the single scale estimates of narcosis administered immediately after arithmetic performance were found to be correlated positively with the number of arithmetic errors made at depth. The findings that post-arithmetic performance, but not pre-arithmetic performance, was related to self-reported narcosis implies that feedback from impaired performance served as a cue for making the subjective judgements. In the second case in which subjective measures were administered, Biersner et al. (21) found that self-reported mood states did not vary significantly between surface baseline and hyperbaric air (6.2 ATA) conditions. However, the averages for moods Fatigue and Happiness across the four test conditions (two baseline and two dive conditions) were found to correlate significantly with impairment at depth on a complex motor task. Apparently, prevailing moods states are important determinants of susceptibility to nitrogen narcosis. The implications that these findings have for reducing narcotic effects through selection and intervention warrant further research attention to this sorely neglected area.

CONCLUSIONS

This review has provided a description of a variety of measures used to assess the narcotic effects of hyperbaric nitrogen, including a determination of the sensitivity of these measures to narcosis. By far, the largest number of measures that have been administered are those assessing the mediational and motor effects of nitrogen narcosis. Tests of motor processes appear to be especially sensitive to the narcotic effects of nitrogen, showing a rough linear relationship to the partial pressure of nitrogen. Of these tests, perhaps the easiest to administer and the one least susceptible to learning effects is the stabilimeter (which varies in a curvilinear fashion with nitrogen partial pressure). Tests of perceptual processes seem to be insensitive to nitrogen narcosis compared to mediational and motor tests. Measures of neurophysiological function, at least as used to date, appear to have contributed little to the literature on nitrogen narcosis. The complex technology, high cost, and sophisticated training associated with these measures simply do not justify continued use, at least as described in these previous experiments. However, event-related potentials may be useful in determining the mechanisms underlying the performance effects of nitrogen narcosis, but only if associated closely with tests that are known to assess distinct performance processes (especially dual-task measures). Work on narcosis in the underwater environment has been extremely limited in the range of nitrogen partial pressures used. While interesting, this work does more to emphasize the confounding effects of psychological stress and state-dependency on performance than to elucidate the performance effects of nitrogen narcosis. Finally, work on the biochemical and psychological (i.e., personality and emotionality) effects of nitrogen narcosis essentially is nonexistent, and the work that has been conducted emphasizes primarily the effects of stress (vice narcosis) under various hyperbaric conditions.

A number of shortcomings in this work severely limit the application of the findings. These shortcomings include poor experimental design, the plethora of assessment measures (even for assessing the same process), the diversity of experimental subjects, variable practice and learning effects, and limited range of hyperbaric pressures. Two major shortcomings are the failure to (1) account adequately for stress effects, and (2) associate narcotic effects with tasks performed by working divers. Stress effects may account substantially for the large individual differences noted in the effects of nitrogen narcosis, as well as for the restricted range of variability (for grouped data) across a doubling of atmospheric pressure (as noted for the problem solving tests). Stress effects must be accounted for by using appropriate biochemical, physiological, and self-report (i.e., mood state) measures--testing subjects at a shallow depth is an insufficient control measure. The failure to relate narcotic effects to operational performance is an oversight that must be corrected before any additional research of this type is undertaken. A task-analysis of operational performance must be conducted, and a determination made of the aptitude and skill components of these tasks. The resulting aptitudes and skills can then be associated directly with various performance measures available in the assessment repertoire. A reliable technology has been developed for making these determinations (22), and the application of this technology to diving performance is both necessary and appropriate. Only after a variety of operational diving tasks have been quantified in terms of frequency, criticality, and underlying skills and aptitudes can the true meaning of the research cited in this review be evaluated.

FOOTNOTE

The opinions, interpretations, and conclusions presented herein are those of the author only and do not necessarily represent the official views, policies, or endorsement of the U.S. Navy or any other government agency.

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DISCUSSION FOLLOWING DR. BIERSNER:

Hamilton: Would you describe a stabilimeter?

Biersner: The subject stands on a platform that is level with the floor, and as the subject moves forward, backward, or side-to-side, the device measures the degree of pitch and roll. It measures the amplitude and frequency of balance corrections.

Hamilton: It is somewhat complicated to interpret the results. You do not just read them off a dial.

Biersner: Half of the trials are done with the eyes open and half with the eyes closed. The eyes are closed in order to eliminate visual cueing and its effects on the ability to maintain balance.

Egstrom: You said that perceptual tasks are very disappointing. When you say that, are you saying it in the sense that you are not finding decrements that you would like to find?

Biersner: That is correct. One would expect different results based on the scientific literature.

Egstrom: It might not be disappointing.

Biersner: The results indicated that if divers are to be exposed to deep depths, they should try to perform tasks that require perceptual and mediational skills because these skills are least susceptible to narcosis.

I had a problem with Kiessling and Maag's original data. They were getting effects at 4 atmospheres. Other people are getting the same degree of effect at higher atmospheres that they were getting at 4, using mediation and perceptual tasks. I think there might have been something else occurring in their data. I think maybe their motor data were okay; it seemed to be consistent with what other people were getting. I think we should take another look at that work because it has not been replicated.

Kizer: But from Dr. Pilmanis's concern over a scientist's reliability, this would not be at all disappointing.

Biersner: I would not worry about the mild performance effects that occur at depths shallower than 200 feet. I do not think that performance decrements of 25% or less on most of these tests mean all that much in the operational diving world.

Pilmanis: I agree, but the job of a commercial diver turning a wrench is one thing, and the research work of a scientist doing the equivalent of sophisticated and complex laboratory work is quite another. We are talking about two different worlds.

Biersner: I think the tasks that are going to be most difficult are those involving fine and continuous motor performance, probably at 150 feet and deeper. If the diver is trying to calibrate an instrument, that performance is probably going to be impaired more than the mental capacity to use the is probably going to be impaired more than the mental capacity to use the instrument.

Brauer: However, there is another thing. I have never known anybody to test it, yet every diver knows it. Under nitrogen narcosis one's perception of peripheral stimuli is greatly reduced or nearly lost. Testing that for your purposes should be important. For instance, where the issue is observation, that must be a key component, especially underwater where one relies heavily on peripheral and incidental sensory input.

Biersner: Yes; the conventional interpretation of this effect is that it is caused by psychological stress that results in a narrowing of the attention field.

Linaweaver: You talked about a 10-20% decrement in perception. Would you consider that if this 20% decrement affected judgement, that it would be significant?

Biersner: What do you mean by "judgement?"

Linaweaver: The ability, for example, to recognize hazards: the ability to take an appropriate action in response to an occurrence.

Greer: I do not understand why a 30% decrement in cognitive ability is not important. I mean, that is the difference between pass and fail. Why do you not see that as a significant problem?

Biersner: In most typical underwater operations enough flexibility exists to allow the task to be performed over a longer (than normal) period of time.

Pilmanis: I think that is important in scientific diving. The accuracy of data gathering and the researchers' mentation is certainly involved. They have to make thousands of decisions. The accuracy of their results depends on this.

Biersner: Okay; in the case of scientists I think you are right.

Carter: The problem is that the 30% magnitude of the cognitive effect is independent of depth. Absence of a dose-response relation casts doubt that this is a nitrogen narcosis effect. Most of the dose-dependent effects were in the 10% range.

Egstrom: An important point is that when people are supposed to perform emergency procedures, the procedures often fail. It is usually related back to this big thing called stress, or that it was not "overlearned," or a lot of other reasons for it. It is possible that a significant portion of the problem could be related to the narcotic effect.

Biersner: The underwater environment selectively impairs motor performance. This effect could be related to either psychological stress or "state-dependent" learning. Baddeley interprets the effects in terms of state-dependent learning. That is, if the diver learns a task in one environment and then goes to another environment, the task must be relearned. These data have immense training implications. Divers should be trained in an environment that is as close as possible to the environment in which thay are going to work, in order to eliminate this effect.

A recent technique used to assess narcotic effects is evoked potentials (EP's). However, EP's only assess primary sensory processing prior to cognitive interpretation of the stimulus. EP's do not provide much useful data about cognitive function.

Hamilton: You did not mention the torpedo that Fowler and Ackles (1977) shot at that method.

Biersner: Fowler and Ackles (1977) described much of the EP literature on narcosis, saying that most of the EP data did not show a linear relationship to either potency of the different narcotic gases being used at a set partial pressure, nor did it show a linear relationship for a single gas as the partial pressure of that gas was increased. Also most of the EP data did not correlate these effects with performance impairments. Those studies in which these conditions had been met indicated that EP's were not a valid way of assessing narcotic effects.

Hamilton: You just said the same thing, that what these things lack is interpretation.

Biersner: That is correct, except maybe for your work. Your work emphasized "adaptation" to narcosis. My review did not concentrate on adaptation effects.

I think that if neurophysiological measures are going to be used, then we must be systematic. Event-related potentials (ERP's) should be used to assess narcotic effects so that the information processing dimension that is impaired by narcosis can be determined readily. Performance tasks that vary these information processing dimensions must be used in conjunction with ERP's. The ERP's are assessing about four or five different dimensions of information processing, and performance tasks must be used conjointly that assess each of those dimensions so that a precise determination can be made of the narcotic effects.

Also, future work must differentiate the effects of psychological stress, narcosis, and state-dependent learning as the performance impairments that have been obtained.

Kizer: How much of this data can you extrapolate to the science model?

Biersner: I would be reluctant to make any statement unless I had done a task analysis of the performance required in the situation. A procedure is available for relating discrete performance measures to task analysis data so that these measures can then be used to predict specific job performance effects under these conditions.

Kizer: So you are saying there is no data base on which to answer this question.

VIDEO GAMES AS HUMAN PERFORMANCE TESTS FOR REPEATED MEASUREMENTS IN HIGH PRESSURE NITROGEN: Robert C. Carter

Abstract

Diving researchers are urged to consider using videogames as human performance tests. Videogames are contrasted with computerized tests or other electronic devices for performance measurement. The psychometric properties required of performance tests are summarized, and evidence that some videogames meet the requirements is cited. Some characteristics which distinguish games that are unsuitable for use as tests are discussed. Comparisons are made between the traits measured by games and by traditional tests. Previous attempts to use videogames in diving research are mentioned, and the possible relevance of games to known effects of high pressure nitrogen is discussed.

Introduction

"Anything that can be done on paper can be done on a television screen, and more." So claims a proponent of videogames as human performance tests (1). Computer-controlled videogames measure skills, and plenty of them! In this review we will consider whether these facinating electronic diversions can supplement or supplant more traditional performance tests. Attention will be given to measurement of performance effects of high-pressure nitrogen.

Videogames, Computerized Tests, and Other Electronic Devices

Let us begin by distinguishing three related innovations: videogames, computerized traditional tests, and non-traditional computerized test devices. Videogames are intrinsically rewarding. They are easily understood by a new player yet they have sufficient range to challenge the most experienced and skilled devotee. They invite development of game-playing strategy and tactics. They give immediate feedback so that players know where they stand relative to their goal. Successes and failures come in rapid order; videogames think as fast as we do. Videogames are active. They present us with a stream of auditory and visual information rather than passively requiring us to read or reason to discern the challenges they present. They exhibit kinematic continuity. This continuity provides an opportunity to exercise skills for dealing with moving objects.

Computerized versions of traditional tests can be generated by a computer and presented on a video screen. These tests are unlike video games. They are usually not intrinsically rewarding. They ordinarily have verbal instructions, and are applicable to a limited range of ability. They typically involve thinking about abstract symbols, and provide few opportunities for strategic or tactical innovation. They preserve the traits measured by the original non-computerized versions of tests (e.g. 2, 3). Computerized tests have advantages over their paper-and-pencil predecessors. Computers offer new possibilities for making tests that adapt to the ability level of the subject, so less testing time is needed. Furthermore, computers are a much better medium than paper and pencil for testing memory because of they can precisely control the presence or absence of stimuli. Computers have advantages, too, for measuring time of response and controlling the timing of stimuli. Computers are very capable of producing visual images for testing spatial abilities. A very important advantage of computers is that they can present alternate forms of tests for repeated measurements. Alternate forms are made by randomly sampling items from the population of items that meet the specifications of the test (4). Wonderful as they are, we mention computerized tests here only to differentiate them from videogames. Computerized tests should be as applicable to diver performance as their traditional predecessors.

Another category which should be distinguished from videogames is non-traditional tasks which employ a computer. Examples of these are a device for measuring rate of acquisition of information (5), a pen which senses complex patterns of pressure during writing or drawing (6), or computerized devices for measuring standing steadiness or tremor (7). Such computerized performance measurement techniques are neither videogames nor computer versions of traditional tests, and we mention them only in passing.

One characteristic of videogames is that they can be played repeatedly - with enjoyment. This characteristic suits them for our purposes. Diving experiments involve repeated measurements.

Psychometrics of Repeated Measurements

To be useful for repeated measurement a videogame (or any other test) must produce scores with four properties. When administered to a control group, the test must give scores with variances (among subjects) that do not change appreciably from one occasion of measurement to another. The mean score of control subjects should be approximately constant, or have a constant rate of change across occasions. Plots of the subjects' results as a function of time should be roughly parallel to each other (no criss-crossing of plots). Finally, differences in level of performance among subjects should be large compared with measurement errors. This list of requirements was a quick summary of the psychometrics of repeated measurements. The first requirement is not too stringent, but the others are important considerations which are usually not verified (but should be). Now, let us see how videogames fare when examined for these properties.

Videogames as Tests

Evidence on the psychometric suitability of videogames for repeated measurement of human performance comes from a few recent studies. The videogames studied include ATARI Air Combat Maneuvering, Breakout, Racecar, Slalom, and Antiaircraft Gunnery (8); Phantoms Five, a bomb dropping game (9); ATARI Air Combat Maneuvering (10); ATARI Air Combat Maneuvering, Breakout, Surround, Racecar, and Slalom (11); ATARI Touch-Me (12); and ATARI Racecar, Pong, Basketball, Antiaircraft, Air Combat Maneuvering, and Breakout (12).

The results of these pioneering studies with videogames are encouraging. Once the subjects have become familiar with a game, the chances are excellent that it will be psychometrically suitable for repeated measurement. Some games are not suitable performance tests though (e.g. ATARI Surround).

The best videogame performance test examined to date, in terms of psychometric suitability for repeated measures, is ATARI Air Combat Maneuvering. The price one must pay for attaining good test properties of this game is about two hours of familiarization practice by the subjects. Many traditional tests also require extensive practice before becoming suitable. The distinct advantage for videogames is that subjects don't mind the warmup - they enjoy it.

Weeding out Games that are Not Good Tests

There are some things to look for which distinguish games that make poorer performance tests. Chief among these is the element of chance. Random variation of scores contributes to popularity of a game because it allows less skilled players to beat the experts, sometimes. Random variation of scores ruins a performance test. Tests in which the effect of a player's actions is determined in part or whole by random factors should be eliminated from consideration. The use of a random number generator in the computer code for game scoring is a bad sign. Games which have a randomness fault are sometimes obvious.

Another thing to look for when eliminating games not to be used as tests is adaptation of the games to the skill of the player. For example, the computerized adversary in ATARI Basketball plays harder if you are ahead, so your score does not represent your skill. Adaptive games may be okay if the score reflects the level at which the computerized adersary must play to keep up with you (i.e. higher scores for better play).

One last problem to look for when selecting videogame tests is interface equipment quality. Game manipulanda are not necessarily built to the high standards required of scientific measuring devices. Manipulanda (e.g. joysticks, game paddles, push buttons, etc.) of the same type differ from one to another, they change with use, and they fail at the least convenient time. Joysticks seem to be the most susceptible to failure. Custom-built input devices should be considered. If standard input devices are used, spares should be kept handy, and the devices should be tested before and after each use for change of response (range of movement, resistance to movement, electrical output associated with every possible position of the manipulandum, etc.).

What do Videogames Measure?

What skills and abilities are measured by videogames? One aspect of this question is, do videogames measure skills and abilities measured by traditional tests? Shannon, Krause, and

Irons (9) showed that individual differences on the Phantoms Five videogame (aerial bombing) were related to individual differences on traditional tests of Visualization, Flexibility of Closure, and pattern recognition. (Well-established ability factor names (12) are capitalized). Performance on Phantoms Five was found not to be related to Speed of Closure, grammatical reasoning, Number Facility, or reaction time.

Kennedy, Andrews, and Carter (13) showed that ATARI's Touch-Me game measured memory abilities also measured by a traditional digit-span test.

An experiment by Dunlop and Bilodeau summarized by Jones (1) compared the aptitudes measured by five videogames and 13 well-chosen traditional tests. About two thirds of the variability of videogame scores was due to reliable differences among subjects. Half of this reliable variability in game performance was due to mental abilities measured by traditional tests (e.g., Flexibility of Closure, Speed of Closure, Spatial Orientation; to a lesser extent, Spatial Scanning and Visualization; and barely if at all, Number Facility, Perceptual Speed, Logical Reasoning, or Verbal Comprehension). The remaining half of the reliable individual differences in videogame performance may be due to manual skills and physical abilities measured by traditional tests. For example Kennedy, Bittner, and Jones (14) showed that ATARI's Air Combat Maneuvering game measured the same skills as a conventional compensatory tracking test. Nonetheless, the possibility remains that videogames measure some abilities not measured by traditional tests. Ability factors remaining after traditional test variance was accounted for tended to be unique to each game.

Relevance of Videogames to Nitrogen Narcosis

In another presentation at this meeting, Robert Biersner has summarized the known human performance effects of high pressure nitrogen. Commonality between what is measured by videogames and abilities affected by nitrogen narcosis includes memory, tracking and perceptual abilities. It is possible that videogames could be used to measure effects of nitrogen not measured by traditional tests.

Use of Videogames in Diving Research

Use of videogames as performance tests has been attempted in the deep-sea diving context, but not often. David Styer showed that the ATARI Touch-Me game equipment could take pressure (to 1000 fsw). However, the Touch-Me game wasn't used in an experimental dive because of lack of availability of spares. David Youngblood actually used videogames during a series of nitrogen narcosis experiments at the F.G. Hall Laboratory for Environmental Research of Duke University in 1982. Dr. Youngblood found that the videogame manipulanda had reliability problems, discussed above, which forced a premature end to the videogame part of his research. Hence, equipment problems have been a major limitation to the usefulness of videogames in diving research.

Prospects

Some videogames have been shown to meet the rigorous psychometric standards of human performance tests. The games can be used to measure some skills and abilities measured by traditional tests, and perhaps to measure previously undefined human abilities. This alone would not command our attention to videogames as performance measurement techniques. The unique contribution of videogames is their capability to sustain alert, interested performance across repeated measures. Given the necessity for extensive familiarization of subjects with performance tests, and the repeated measurements called for in most experimental diving protocols, videogames would have been invented for performance testing if they had not first been introduced for entertainment.

NOTE

We are especially indebted to Drs. M.B. Jones and W.P. Dunlop for sharing previously unpublished results with us. The videogame research reported here is largely attributable to Dr. R.S. Kennedy, who planted the intellectual seeds.

The opinions expressed herein are those of the author, and should not be ascribed to the U.S. Navy or any other Agency of the U.S. Government.

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DISCUSSION FOLLOWING DR. CARTER:

Brauer: What about the games that adapt as you play?

Carter: Research shows that some games are just not suitable for performance testing. One is that many of them bring in the element of chance; the scores are determined to some extent by random factors. Another thing that makes some of these games unsuitable is that the test adapts to the level of performance of the player, without changing the score. For example, computer basketball by Atari; the better you play, the harder the computer plays against you. Your score does not really reflect your ability. If you had a numerical measure of how hard the game is playing against you it would be quite acceptable. With this particular example, your score does not reflect that very well.

Biersner: Is that a problem with Moeller's tests?

Carter: No, because the scores on his tests are higher if you are performing at a more difficult level. Moeller's tests adapt when you are presented with a more difficult problem; because you are a more skilled performer, you get a higher score. Whereas in Atari basketball, you play harder and it just plays harder defense. It is okay to have a test be adaptive, but make sure that the adaptation is reflected in the score. This is something to watch out for when you are shopping for video games to use as performance tests.

There are other assorted reasons why some video games might not be suitable. Sometimes the top end on the score is not high enough. With practice, everybody scores near the maximum. One general problem with video games that look like good candidates is that the game manipulanda--the joy sticks, etc.--have not been built to the standards of scientific instruments. Two joysticks may not give you the same response. Furthermore, they may change over time.

Solutions to that general problem are to get custom built manipulanda or get a whole pool of these things and cast away those that are out of specifications. In either case, it is very important, for our purposes, to check the games regularly during the experiment.

Pilmanis: How about under hyperbaric conditions?

Carter: Some games have been used under hyperbaric conditions. Dave Styer and David Youngblood exposed games to pressure. Lt. Styer (at NEDU) tested an Atari game down to 1,000 feet and brought it back and it worked just fine, down and up. There are some articles on testing electronic devices under pressure. As long as you take those kinds of precautions, I would not anticipate any problem with the equipment. Let me reemphasize that you want to be sure that interface manipulanda are consistent from one occasion of measurement to another.

Kendig: Have you only tested the arcade type of games, not the thinking ones like "nuclear power plant" where you have to gauge fuel consumption?

Carter: None of the research I know of has dealt with that game. I have not conducted any of this research. I just happened to be at a place that was at the hub of this research on video games. Incidentally, most of the video game research has involved home games, not arcade games. I guess you could put together games that might involve these things. They might mot be interesting enough to keep somebody going. One could invent a vocabulary game, but some people might not be interested in doing that.

Returning to available evidence on video games, we want to look at where these abilities overlap with some of those that have been shown to be affected by high pressure nitrogen. Memory, tracking, and the perceptual abilities have been found to be affected. The mental abilities do not show a dose-response curve, however.

Biersner: I would not say that these abilities do not show a dose-response effect; these responses just have not been measured yet.

Carter: Anyway, that is the known overlap. There may be additional overlap and additional effects of high pressure nitrogen that we do not know about yet. And there may be additional abilities measured by video games that are not measured by traditional tests. These new abilities might be affected by high pressure nitrogen.

Of the total variance measured by video games, about two-thirds is reliable. About one of those two-thirds matches up with abilities measured by traditional tests. The other one-third of the reliable variance is not measured by traditional tests. There is an opportunity to get at something with video games that is not measured by traditional tests.

Biersner: Any idea of what that might be?

Carter: We would just be guessing. In the paper I mention that some manual skills might be involved in responding and there may be possibilies for response integration and strategic planning. There is a definite "strategy effect" in playing some of these games (and in some other tests as well), the matter of how you plan your game. This can have a big effect on score that is not necessarily due to one's ability at the moment.

Just to summarize, the main contribution of the video games would be that they maintain a person's motivation to do the task over a long series of repeated measurements.

Kizer: Take a game like Simon. A lot of these involve both a motor component and an integrated or cerebral component. Can the video game separate out the motor component from whatever thought process is going on and try to get at what is being preferentially affected by the environmental conditions?

Carter: There are two approaches I can think of for that. One approach is to use more than one video game where one of them has this motor-cerebral mix and another seems to require only the motor response. One can separate, using statistical regression analysis techniques, a particular person's ability to do that kind of motor response, and what is left over would represent the integrated or cerebral capability.

The approach that I favor is to use video games with multiple response (and if possible input) media. The subject responds to the game using different kinds of responses. If the narcosis effect varies radically with response media then you would guess it is the response pathway that is being affected. Whereas if the effect is consistent across response media, then you would say mentation is affected. That assumes you have more control over the game then most people have over their home video arcades. You need a laboratory in which alternate games can be implemented. One game might have a visual stimulus whereas the other one would have an auditory stimulus. Voice and manual responses could be used. The best situation would be to have multiple modes of stimulation, multiple modes of response, with essentially the same task. That is the best way to untangle this Gordian knot.

Brauer: What about diurnal variation in performance?

Carter: Well, actually video games are especially well suited to this question because if you are going to study diurnal variations you really need to get a long series of data. You need to study an individual or individuals in depth. A time series analysis is needed with 50 or 60 observations on a person at a minimum. To get that kind of series you need engaging tasks, and a video game meets that requirement.

To answer your question, you would have to do the experiment to find out. Then that effect would have to be accounted for in experiments on nitrogen narcosis.

Brauer: I am just thinking of typical diving experiments where the time of day is not quite so much at your disposal as it might be. If some performance function were highly sensitive to this, such effects might overlay some of the fluctuations we have seen.

Linaweaver: I think from a practical point of view with our work at Catalina, diurnal variations may be quite important because to these people at the deeper depths day and night may not be too distinct.

Carter: I would say video games combined with time series analysis would give you an excellent approach to studying the diurnal cycles.

Pilmanis: In the scientific situation we are most concerned about the finer points of mentation; these people have to be able to think very precisely. The type of game you are talking about may require a little more strategy.

Carter: I am not sure I would want to say that these things always require thought. That is the joy of video game playing. It can be effortless.

Egstrom: Are not the learning curves on these video games typically very long? You will not reach a learning plateau very early, so it would seem to me that you would always have a learning progression taking place that would interfere with your ability to be able to prepare a score.

Carter: We have found that even the traditional tests have learning curves that go on and on forever. What you are interested in doing is not so much reaching an asymptote, because if you try to get that asymptote you will just keep on going and going. What you want to do is get up off that strong non-linearity at the beginning of the learning curve to where scores gradually increase linearly. Then you can predict where your next observation should be if there is no experimental effect. This was what I had in mind when I said that some of these games have characteristics that lend them to use as performance tests. Some of them quickly achieve the slow linear improvement with practice that makes them suitable as performance tests.

Biersner: A dimension of these tests that has to be considered is whether or not individual differences stabilize early. Aside from the mean reaching an asymptote (or gradual linear slope) fairly early, you want to ensure that the relative positions of the individuals within the sample remain stable. Those tests that show instability among individuals are probably assessing the "strategy" skill mentioned earlier. The relative position of individuals may vary because they are using a number of different strategies to solve the problem. If the number of strategies is limited then individual differences stabilize early. However, those tests that stabilize early are probably assessing simple mental functions or abilities, whereas test of complex mental abilities are probably going to take longer to stabilize because a number of problem-solving "strategies" will be assessed by these tests.

Egstrom: It seems to me that one of the real advantages of what Dr. Carter is suggesting is that you could come up with a series of video games that have essentially the same learning curve characteristics, and that you could actually use that curve as the baseline. Variations from the rate at which you would be able to learn would be attributable to the effects of the variables being studied. In other words, it is the "alternate form characteristic," that may be better suited to the video game than a great many of our paper and pencil tests.

Biersner: That is a problem with the five types of tests that I was describing. These tests are not assessing the maximum ability of some individuals. These tests are too insensitive to delineate an impairment. Therefore, a conservative estimate of the narcotic effect is obtained. The tests used by Moeller avoid many of these problems because the adaptive feature of these test permits an assessment at maximum performance.

Pilmanis: How does competitiveness enter into that?

Carter: People will compete on video games, but it is harder to get them to compete to do well on the traditional tests. If you want to get them all up to some high level of performance (relative to their particular capability) you can give feedback and induce social pressure. Let the members of the group know that they are competing to be the best. I do not know that anybody has ever solved the problem of how to insure that people are doing their best.

Shilling: Did I hear you say that you have tested at least one piece of equipment under pressure and it works all right?

Carter: I think these are common problems with many kinds of electronic instrumentation that we think about taking under pressure. There are some general rules of types of equipment checks you would want to do before betting your experiments on a piece of equipment. I would recommend that you pressure test any equipment first. However, in pressure tests conducted up to now on video games, the equipment has come through well.

Hamilton: The fact that it is a video game does not limit what one can test, it only limits what is available at this time off the shelf.

Carter: And these results are based on the on the very limited sample of what is available. It is great that we have about ten video games represented in this summary of literature, but there are hundreds available.

Brauer: Are you taking your screen inside, or are you expecting your people to stand at the window and look out?

Carter: There are a couple of different approaches. My inclination would be to have them stand at the port looking out, or use a projector TV where all you have on the inside is some kind of screen and what goes through the port is a beam. There are different ways to skin that cat, depending on your situation. Anyway, you do not need a TV inside the chamber.

Greer: I should think that that would be important. Pressure is one thing, but 2 weeks of 100% humidity and salt water is another. If the only thing that goes through the penetrator is the manipulator and/or keyboard, I think you are better off.

Carter: You would want to assess the kind of situation and figure out the best way to get the information in and out.

Pilmanis: The approach we are taking is to put the instrument in a one atmosphere case. There will be a computer in the habitat anyway, and the same screen could be used.

Hamilton: I would like to make a general comment on performance measurements related to Captain Biersner's talk. We--here I mean most of us who try to measure performance in diving--have tried to implement tests to assess a dive environment. The dive may be for another purpose, with the test thrown in to keep the troops entertained while they are on the bottom, and also because we want all the information we can get from the exposures. We are really not in the business of trying to evaluate the test itself. In past experiments we have picked "off-the-shelf" tests. A lot of these tests that are in the books--the Bennett Nut and Bolt test, the Purdue Pegboard, the Stroop, and many others--have a special purpose. They were designed to detect a particular individual characteristic, like left hand vs. right hand coordination (the Purdue pegboard) or dyslexia or something like that with the Stroop (that is the test where a word stating a color is printed in another color of ink, and the subject has to tell what color it is rather than what it says).

These tests are often designed with the purpose of assessing some kind of psychological or psychiatric characteristic of the individual that has nothing to do with different environments. In fact, if the tests do the jobs they are designed for, the results will be independent of the environment. The only test difference is supposed to be between individuals, and yet we have tried to use them to test the effects of the environment on the same individual. In some ways the characteristics that were stated as those of the video game are exactly what we are looking for; they are not designed to test some particular psychological or psychiatric characteristic of the individual.

One more thing. I have been at odds for some time with this "number correct and number attempted" measure on a test such as an arithmetic test. Scoring such a test presents an absolute dilemma to the investigator, and is very susceptible to the "strategy effect" just mentioned. This test can be as difficult to score as an EEG. Our lab (mainly Ted Langley) found a rather simple solution to this, where we could get a single quantitive number that was extremely well correlated with what we presumed to be the effect of the environment. That is a "paced arithmetic" test, where the numbers are presented at a constant speed. In use, the investigator finds out what speed to present problems so the subject can get about an 80% score. Then when the environment changes that score goes up or down, but only one score is involved. We used 35 mm slides sequenced by a timer, with the answers written on a pad by the subject.

Carter: Paced arithmetic is a very popular test these days. Its popularity has been rising for the last few years. One thing that is assesses that the usual arithmetic test does not, though, is something some people find adversive: pacing. Subjects get all flustered, they miss one problem and that makes them miss the next one which comes before they are ready.

Hamilton: That is a test under stress. That kind of thing can be tested with the video game mechanism much easier than some of the things that we have had to rig up in order to do it. It gives an absolute score, and is easy.

Biersner: These tests allow you to assess more accurately the true capability of the individual at the time.

Egstrom: Because we make highly specific adaptations to whatever the demands are, we still generate tasks that are essentially non-relevant to the thing that we are trying to measure. I wonder if part of the problem could be that if we are looking at the effect of narcosis from the academic point of view, one kind of test might be more appropriate, but if we look at it from the operational point of view, we might need a different test. Generalizing from the academic to the operational test may be a non-sequitur of sorts; you can not really get there from here.

Biersner: Tests with much operational significance but which are not used much in work on narcosis are those that assess dual-task performance. These tests measure the ability to perform two tasks simultaneously, a component of many video games. I suspect that in many diving situations much dual-task performance is occurring. The diver receives both verbal and visual input while simultaneously working on a manual task. Divers who operate submersible vehicles must operate rudders and elevators while receiving navigation information and trying to remember where they were five minutes ago in order to estimate rate of travel.

Greer: Dr. Carter, how far are you from being able to make recommendations on equipment? If, say, two years from now, we put the habitat down in Catalina, are you going to be able to tell us what hardware to take off the shelf? Or software?

Carter: Most of this work that I reported was done around 1980-1981. It was done mostly by people who were not really thinking about diving. They had their fling with showing what video games measured. They had some theoretical points that they were interested in. I do not think there will be much more progress in looking at video games as performance assessors, unless somebody with a particular application in mind does it. Analyze the jobs, say what abilities you want to measure, find video games that measure that, and then adapt the equipment to your needs. I think you will have to do it for yourself.

Kizer: That might be one of our recommendations, or at least something that could come out of this may be the recommendation to develop this sort of hardware and software. It will require someone to propose it and work it up.

Carter: You may decide that the abilities measured by games described here are not the things you are interested in. Then you are off on a chase of trying to find a video game that measures what you are interested in. It may be a holy grail. There is no guarantee that there will be an intrinsically motivating test that also measure what you are looking for. But you are right, people should write up proposals.

Shilling: It has been said several times now that we really need to analyze the tasks to be performed. Is this something that can be done fairly readily, for example, for your habitat activity?

Pilmanis: I think so.

Shilling: Then after the task has been formulated we must determine what a person has to do, or what abilities one has to have to be able to do it. Then comes the selection of appropriate tests to assess these people. Do you look on this as a very difficult task?

Carter: I would say the technology exists to do it.

Biersner: This type of work is not much fun, but the data are necessary in order to ensure the validity of the performance measures that are to be used.

Brauer: And it is something that should be done out of the water, before going into the field.

Egstrom: Except there is a basic problem with that. Remember that the thing we are trying to assess is how well our scientists are able to do observational research. We want to know that the observer has been conditioned to respond maximally at that depth and under those conditions. Also, if possible, what the error factor will be.

Shilling: That is what was worrying me when I asked my question. Everybody seems to say that this is easy. I do not think it is all that easy. But Dr. Pilmanis says he can tell the testers what it is that his observers have to be able to do.

Pilmanis: We can grossly, but not in detail. But we know of the typical kinds of things researchers do.

Hamilton: Somebody used a word a moment ago, "conditioning." That is what it is all about, this so-called acclimation. It is a matter of conditioning to operate in the environment.

Egstrom: In any of the adaptive mechanisms that I understand, you have physiological correlates that go along with that. You also may have behavioral correlates.

Brauer: You have one other thing which nobody has mentioned. Anybody who has observed groups of people under these conditions must be aware that a given level of narcosis in a single diver sitting outside all by himself is one thing, but the same level of anesthesia or narcosis or intoxication in a group of five people interacting makes for an entirely different thing. It has implications rather remote from anything we have said so far. Divers may not be supposed to have a sense of humor, and yet many of us have seen situations of groups under effective nitrogen partial pressures in which everybody was having a marvelous time but not getting much of the job done.

Egstrom: I wonder sometimes how much of that is based on anticipating behavior. That is, you know you are supposed to be narked and a little happy and decide to get with the program. That is typical social party behavior. If it is time to have a good time, you go in and have a good time, and it may or may not depend on the alcohol you ingest.

Shilling: Let me give an example about selection from my own experience. They were originally selecting sonar operators by saying "You look like a good man, would you like to be a sonar operator? We will train you." Well, without exerting much genius it occurred to me that there must be some characteristics here that were important. Nowadays it is all visual, but in those days, auditory and pitch discrimination seemed to me to be important criteria. Well, I got the Seashore records (developed by Seashore at Northwestern University) went down to the waterfront and said to the skippers, pick me your best sonar operators. I unerringly gave them a list of the good ones and the bad ones, based on the old Seashore method. So then we made a nice electronic device that would score them, and they were then selected on the basis of ability. I know of one submarine that was saved with everybody on it because a good sonar operator had been selected and knew how to do something. It gives one a sort of nice feeling. I think somewhere along the line, all of these things have to be analyzed to find out what are the crucial components of the situation.

ACCLIMATION TO NITROGEN NARCOSIS: A REVIEW: R.W. Brauer

BACKGROUND, HISTORY AND TERMINOLOGY

The depth of narcosis caused by a given partial pressure of nitrogen or other "inert gas anesthetic" is not constant but tends to decrease with prolonged exposure. This phenomenon has been called by a variety of names: "adaptation", the term which occurs most commonly, but which in English speaking countries has been preempted for an entirely different range of biological phenomena (multi-generation adaptation to a given stress factor by a population on the basis of genetic change); "habituation", which stresses the similarity of these effects to those associated with habituation to certain other CNS depressant drugs; or "tolerance", a term which is semantically wrong since it refers to the end result rather than to the process. Here, I shall prefer to use two other terms which seem to me to conform better than any of these with current general physiologic usage, namely 1) "accommodation", a noncommital term implying precisely what is actually happening without inferring any mechanisms and 2) "acclimation", (i.e. physiological or biochemical changes increasing tolerance of an individual to a single stressor and acquired as a result of exposure of that individual to this particular stress factor), a term which expresses a perceived resemblance between the phenomena described here and phenomena observed in a number of other stressful environments, including high altitude, high or low temperatures, and high pressures. Since this latter term implies some hypothesis about what is going on, it will become appropriate only later on when we shall have examined the data on hand to see whether the implied comparison is useful.

Credit for first recognizing phenomena of this type for what they were should go to Shilling and Willgrube (23) who (shortly after the description of nitrogen narcosis by Behnke and his co-workers (1)) pointed out that the severity of nitrogen narcosis usually was greatest at the beginning of a dive and decreased as time under pressure lenghtened. Somewhat later, Case and Haldane (3) also called attention to the fact that with continuous exposure to air at 300fsw the degree of narcosis induced not only did not grow worse with time but that instead the divers showed some degree of improvement of performance with increasing time under pressure.

THE BASIC PHENOMENON

Since that time numerous reports have appeared dealing with uncomplicated nitrogen accommodation phenomena ((4),(6),(7),(8),(10),(13),(14),(15),(16),(25),(26)), so that a general, albeit I am afraid a rather episodic, description of these events is now possible. All of these reports start from the point that exposure of subjects to nitrogen at partial pressures from 3 to about 8 ata entail recognizable changes in alertness, short term memory, postural stability, and ability to perform any but the simplest kinds of tasks, as well as objective changes in characteristics of the electroencephalogram and of the time pattern of sensory evoked potentials. Accommodation to nitrogen narcosis, then, is made evident by decreases in the intensity of all of these changes as subjects continue to be exposed for periods of days to the same nitrogen partial pressure. If the conditioning nitrogen pressure is not too high, the performance of the subjects improves steadily and after the lapse of about a week may reach levels indistinguishable from their performance in air at 1 ata ((4), (2), (6)). There have been suggestions that, if the conditioning nitrogen partial pressures are pegged at a level producing "very definite" evidence of narcosis, the progression may not be smooth and unidirectional, but rather waver a bit (9), and that, if the nitrogen partial pressures are increased even further "to the point of quite severe narcosis", prolonged exposure may not result in recovery to normal well being and performance but rather in progressive deterioration ((8), (9)).

The only dissenting voice in this chorus affirming the existence of acclimation to nitrogen narcosis is that of Moeller ((18), (19)) who contends that this phenomenon is an artifact, only observed when subjects were hypoxic or when other complications led to misinterpretation of the data. I feel that this contention should be rejected for several reasons: A) It is not correct that acclimation to nitrogen narcosis cannot be observed in smoothly run experiments, some of which have gone to considerable lengths to avoid hypoxia (e.g. ((4), (7),(8),(10)). B) The fact that acclimation phenomena have been observed in animal experiments ((2),(7),(29)) eliminates the possibility of its being a subjectively established artifact. And, C) The nitrogen partial pressure used in Moeller's own experiments which failed to yield measurable "adaptation" was 198fsw, nearly 6 ata, substantially higher than used in any of the successful acclimation experiments. This may correspond to the observations of (9) suggesting that the dose-response relations for acclimation to nitrogen narcosis may not be simple but entail deterioration with little accommodation at excessive nitrogen partial pressures.

In addition to progressive improvement of performance at constant pressure, it has been found that protracted exposure to effective partial pressures of nitrogen also entails a recognizable increase of the subjects' ability to work effectively under still higher nitrogen partial pressures, attained typically in some type of excursion dive to depths where men who had not undergone such preliminary nitrogen conditioning were much more seriously affected (14).

A number of workers ((18),(19),(21),(22),(29)) have carried out observations involving brief relatively deep bounce dives on nitrogen, separated by a number of days, up to 28, finding that performance tests and performance indicators during the second dive of a series were less impaired than during the first. The very short time required for this type of conditioning distinguishes it sharply from the much longer time required for the true acclimation process. This form of improvement in performance under elevated nitrogen levels is said to persist for many weeks after the first exposure (19), again a trait that seems to be in contrast with what we know about nitrogen accommodation resulting from prolonged exposure to moderate levels of nitrogen narcosis. I tend to agree with these observers in feeling that anxiety and relief of anxiety ((18),(19)), and "learning to cope" with nitrogen narcosis were the primary factors involved in this type of accommodation to nitrogen, and I suggest that these phenomena should be kept separate from the ones we are considering here, although it should be borne in mind that they can be superimposed upon and complicate interpretation of phenomena associated with true nitrogen acclimation. In line with this, it has been pointed out the extra nitrogen exposure in the form of bounce dives does not accelerate the process of nitrogen acclimation (8).

TIME COURSE

Only a very limited volume of data is available to give somewhat sharper definition to the time course of the phenomena at issue. Acclimation to nitrogen narcosis has been observed in all experiments in which subjects have been exposed for periods ranging from two to 26 days of continuous exposure to nitrogen partial pressures between 2.7 and 4.0 ata ((4),(6),(7),(10)). When EEG changes were used as the prime test parameter, nitrogen related changes at 2.71 ata nitrogen partial pressure reached their peak intensity after two to four days and diminished slowly thereafter. In another experiment (27), 10 men were exposed for nine days to 6.7 atm of air. "Some of these divers could adapt to the narcosis and showed significant improvement towards the end of the study." In another study, excursion dives from nitrogen saturation indicated that there is "less nitrogen narcosis in excursion dives from nitrogen saturation when dives take place after a "few" days in saturation rather than at the beginning" (14). During a two weeks' saturation dive, two of the three subjects showed "steady adaptation" as evidenced by progressive decrease in the alterations in evoked potential (14). The most complete experiment bearing on this point was probably that conducted by Coler and associates (5) in the AMES Crew Simulator where divers were exposed to 100 ft. of normoxic nitrox for two weeks with repeated tests for short term memory, EEG changes, and the like. Adaptation started at five days, and by the eighth or ninth day the results of their tests had returned to levels indistinguishable from those observed in the same subjects in air at 1 ata. In all of these experiments the acclimation did not become significant (i.e. measurable) until two or three days under nitrogen at pressure had passed, and leveled out, át what was perceived to be the maximum accommodation level under the given circumstances, within five to nine days.

DEACCLIMATION

The published data are remarkably silent regarding the opposite phenomenon, i.e. the dissipation of nitrogen acclimation once the subjects return from a high pressure environment to sea level air. The only specific reference to phenomena associated with this period are several references to "rebound", described by Langley and co-workers (14) as occurring immediately on return of his subjects to the surface. This is said to be less severe after 10 days of acclimation than during a relatively brief bounce dive, suggesting that such "rebound" is linked to the severity of nitrogen narcosis at the time of decompression and hence negatively correlated with the level of acclimation achieved.

DOSE-RESPONSE RELATIONS

The structure of the simulated diving experiments, which have furnished all of the data on nitrogen acclimation in man, does not lend itself to well controlled dose-response studies. Only by comparing the results of different dives can one seek to develop at least a general sense of the effects of varying nitrogen partial pressures during the acclimation period upon the development of increased tolerance to nitrogen narcosis. Thus, it has been reported that exposure to 2 ata of normoxic nitrox does not induce acclimation (19). Acclimation has been reported, on the other hand, for exposures to as little as 2.7 ata (19) and in some subjects to as much as 7 ata (26) of nitrogen. Acclimation to levels of nitrogen causing "well marked" effects of nitrogen narcosis (more than 5 ata) has been reported to follow an unsteady, wavery course, while even higher levels are said to produce deterioration with no clear cut evidence of accommodation (to those levels of N_2) (9). Another report using performance and EEG criteria confirms these findings, stating that acclimation to 4 ata N_2 was "quite feasible", while acclimation to 7 ata was achieved by "some but not all" of the subjects (26).

Using performance during bounce dives to greater depths as the criterion, it has been shown that a week's acclimation to 2.7 as well as to 4.6 ata of nitrogen resulted in improved performance under 7.5 to 10 ata of nitrogen ((4),(6),(10),(16)).

All told, the data currently available indicate that effective acclimation to nitrogen narcosis requires partial pressures no greater than 2.7 ata, that accommodation can be attained to 4 but not invariably to 7 ata of nitrogen, and that effective acclimation to nitrogen at pressures as low as 2.7 ata improves performance in bounce dives to nitrogen partial pressures as great as 10 ata. The data do not allow one to say anything about a possible correlation between severity of conditioning exposure and degree of tolerance achieved, i.e. about possible dose-response relations.

INDIVIDUAL DIFFERENCES

A number of reports comment on wide individual differences, not only in the inherent susceptibility of divers to nitrogen narcosis but also in their ability to accommodate during prolonged exposures. Thus, it is suggested ((12),(13)) that "by careful choice of highly intelligent young men in excellent condition one can select individuals who can acclimate very adequately to conditions in which others might be seriously incapacitated as a result of nitrogen narcosis." Again, it is reported that while "all subjects" could accommodate to 4 ata N_2, only "some but not all" could compensate for 7 ata during extended sojourns (26).

So far as we can tell, there are no published data relating to man which would allow one to determine whether these undoubted individual differences are merely reflections of individual differences in narcosis susceptibility at the outset, or whether they reflect differences in the ability of individuals to acclimate. Working with rats subjected to 270 ft. dives of several hours' duration every three days ((27),(28),(29)), investigators were led to conclude that "maximum acquisition and retention of tolerance as measured by improvement in operant conditioning situations involving multiple reinforcement varied with individual thresholds but was not affected by number or frequency of exposures." This suggests that inherent resistance to N_2 narcosis may prove to be the primary determinant of tolerance levels attained as a result of nitrogen acclimation.

SUMMARY OF RESULTS OF HUMAN STUDIES

While the data on accommodation to nitrogen narcosis derived from real or simulated human diving experiments are, in the nature of that kind of study, largely episodic rather than rigorous and quantifiable, they do allow one to perceive the general outlines of the phenomena involved: One can discern two types of event, one resulting from very brief exposure and conferring limited benefits which persist for some time, the other requiring much more prolonged exposure but producing more tangible increases in narcosis tolerance, dissipated, presumably, at about the same rate as they were acquired. Very tentatively, the former of these has been associated with "learning to cope" and reduction of anxiety, and would seem to be akin to the ability of highly trained divers to function effectively at nitrogen partial pressures which would incapacitate less experienced workers. The second kind of accommodation has some of the hallmarks of acclimation, a process whereby the resistance of an organism to a single specific stress factor is increased progressively. It would appear to follow a fairly well defined, if not necessarily unidirectional progression, reaching an optimal tolerance level after a number of days. It shows - albeit very hazily - some dose dependence: a threshold of nitrogen partial pressure below which accommodation fails to take place can be recognized; on the other hand, there is no information to suggest whether the level of tolerance ultimately achieved by exposure to higher nitrogen pressures varies with the severity of the conditioning challenge. The tolerance developed extends not only to the nitrogen partial pressures to which the individual was acclimated but also to greater partial pressures and thus appears to reflect a true increase in tolerance to the narcotic effects of nitrogen. It is clear that there are important differences in the degree of tolerance that can be achieved by different individuals, and that prolonged exposure to nitrogen levels to which an individual cannot acclimate entails deterioration rather than progressive acclimation. Only animal data are available to suggest that the observed differences represent differences in inherent resistance to nitrogen narcosis rather than differences in the ability to acclimate.

ANIMAL EXPERIMENTATION

Thus, the results of the human studies define the basic phenomenon and help to formulate a series of questions, but, at least as of this date, fail to provide testable quantifiable answers to most of the questions raised. It seems to me this is the kind of situation that calls for data of a kind that can most readily, if not exclusively, be derived from animal experimentation.

Several communications have dealt with the effect of brief N_2 exposures upon the susceptibility of conditioned operant behavior in rats to nitrogen narcosis ((27),(28),(29)). These experiments, however, seem to bear primarily upon the "learning to cope" type of nitrogen accommodation rather than upon the phenomenon of nitrogen acclimation of primary interest here. I shall, therefore, report only on two other investigations concerning N_2O acclimation in mice which seem to me to contain answers to many of the questions raised above, and to point to experimental approaches to most of the remaining ones.

Koblin et al (17) maintained CD-1 mice for up to three weeks in air containing 0.5 ata N_2O - half of the ED-50 for N_2O anesthesia in this strain. The animals accommodated themselves well, gaining weight at a rate only slightly below control animals maintained in air. At intervals, animals were removed to determine ED-50 for anesthesia, and it was found that the partial pressure of N_2O corresponding to this value rose progressively for the first two weeks, and then leveled out at about 120% of the control ED-50. Slopes of the dose-response curves before and after N_2O acclimation were indistinguishable, suggesting that the acclimation process added little variability to the N_2O resistance values of the population. On return to air, the extra N_2O tolerance was dissipated in about five days. Cross tolerance tests showed that N_2O acclimation also entailed increased resistance to pentobarbital and to ethanol, indicating that acclimation was to the narcotic effects of N_2O rather than to N_2O as a chemical species. Changes in brain lipid composition could not be detected.

We ((2) and R.W. Brauer and W. Hinson, unpub.) studied similarly treated mice using resistance to high pressure convulsions as the test parameter. N_2O acclimation greatly reduces HPNS convulsion threshold pressures (by as much as 40% after two weeks in 0.5 ata N_2O). The process follows a time course identical to that for the progressive increase in anesthesia resistance - indeed, a plot of our ΔP_c against Δ ED-50 of Koblin et al at corresponding points of the acclimation process is linear. Recovery to normal convulsion threshold pressures, likewise, follows a time course very similar to the return to normal of anesthesia tolerance except that in this case there is a substantial overshoot to higher than normal mean convulsion pressures on the fifth and sixth days. Finally, it could be shown that if the high pressure challenge of the N_2O acclimated mice was carried out not in pure heliox but in heliox containing 0.5 ata N_2O, convulsion threshold pressures of the fully acclimated mice returned to values indistinguishable from those of control mice compressed in pure heliox. In a sense, therefore, it seems proper to conclude that N_2O acclimated mice in an N_2O free medium were suffering from nitrous oxide withdrawal symptoms, strengthening the similarity of this type of conditioning to dependence developed upon habitual use of other CNS depressants.

These two sets of results establish that acclimation to inert gas narcosis is a progressive phenomenon with a well defined pseudo-exponential time course completed in about two weeks in the mouse, that it is reversible with a slightly more rapid time course and possible overshoot effects, and that it does not seem to contribute greatly to population variance in characteristics related to narcosis susceptibility. In addition, the data show that these effects entail cross-tolerance with other anesthetics, chemically unrelated to N_2O, and modifications of hydrostatic pressure tolerance, all indicative of generalized changes in CNS excitability. Finally, the data suggest that the state of N_2O acclimation achieved implies a measure of dependence upon the anesthetic which hints at significant parallels with alcohol dependence and other similar states.

Obviously, these are data on nitrous oxide and not on nitrogen, and on mice and not on men. Yet, I think they have an excellent chance of being qualitatively transferrable to the situation of interest here. Unlike some other types of CNS effects, narcosis seems to lend itself well to mouse-to-man transfer of conclusions: Relative narcotic potencies we estimated ten years ago for nitrogen and hydrogen in the mouse have just been shown to fit as closely as may be the situation in man: mouse estimate (net anesthetic potencies) H_2: N_2 as 0.24:1.0; human estimate: H_2:N_2 as 0.23 to 0.25:1.0! Furthermore, such data as we have for the course of N_2 acclimation in man fit effortlessly with the much more detailed quantification possible in the mouse experiments with N_2O acclimation. Thus, while I am quite certain that much human experimentation will be needed to provide the accurate numerical data needed to make these phenomena useful in diving practice, I am equally convinced that animal experiments of the type quoted provide the only practicable means of formulating and answering the basic questions raised by these fascinating phenomena, a full understanding of which will be necessary to allow effective planning of the human experiments.

SOME QUESTIONS FOR FUTURE STUDY

I would like to close by listing as a basis for further discussion the key questions regarding nitrogen acclimation as they present themselves to me at this time.

1. Individual Differences -- Two questions occur here: A) Do differences in inherent nitrogen susceptibility represent stable individual characteristics, or do they fluctuate from day to day so that ranking men with respect to this factor is useless? (I suspect the answer to this question can be extracted from human experience now at hand). B) Does the ability to acclimate to nitrogen narcosis vary independently of inherent susceptibility to this effect, i.e., will ranking of susceptibilities in unacclimated subjects allow one to predict ranking of susceptibilities in acclimated subjects? (This lends itself to mouse repetitive exposure - correlation studies analogous to those we have carried out regarding HPNS seizures).

2. Dose-Response Relations -- What is the relation between partial pressure of the narcotic gas during acclimation and the magnitude of improvement in narcosis tolerance at the end of the acclimated period? To what extent can one increase the ultimately attainable tolerance by superposing successive acclimation steps at increasing levels of the narcotic gas made possible at each step by the preceding acclimation? Finally, under this heading falls further exploration of the observations of references ((8),(9)) suggesting that at excessive nitrogen partial pressures successful acclimation is not

possible. It remains to be established whether this reflects non-linearity of the dose-response curves, or deterioration incidental to excessive levels of narcosis, and perhaps amenable to relief by multi-step acclimation procedures. (All of these, it seems to me, need to be explored in animal experiments before designing human studies to test and, if appropriate, to exploit the concept of staircase acclimation).

3. Cross-acclimation -- Koblin et al have shown that N_2 acclimation confers a degree of cross-tolerance to other CNS depressants. Can this be reversed, i.e. can one achieve N_2 acclimation by suitable drug treatment without the inconvenience of prolonged sojourn in high pressure nitrogen? (Again, this calls for animal experiments; transfer to the human situation seems worth pondering on this basis but may well be fraught with grave ethical and legal problems. Still - if they exist, may they not apply with equal force to N_2 acclimation as such?).

4. Deacclimation -- How rapidly does it proceed in man? Does it entail overshoot phenomena of practically important magnitude? Is there a problem with withdrawal symptoms when one attempts to maximize N_2 acclimation?

And, finally,

5. Basic Mechanisms -- A tantalizing subject, probably well beyond the objectives of this workshop. I happen to be struck by the continuity between N_2 acclimation and high pressure acclimation suggested by our own data. Others will focus on the similarity between these phenomena and drug habituation phenomena. Again, as implicit by the term nitrogen acclimation, by which I have chosen to designate these phenomena, I am impressed by their resemblance to temperature acclimation with all its implications of change in brain composition and brain chemistry. This list could readily be lengthened, reflecting individual points of view, but should suffice to indicate that 45 years after the original discovery of the phenomenon of nitrogen acclimation, there remains a wide and exciting field for future investigation.

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DISCUSSION FOLLOWING DR. BRAUER:

The first few minutes of discussion focused on terminology; whether a person under the conditions described is said to have adapted, acclimated, or accommodated. All agreed that adaptation should not be used in this context, but should be reserved for genetically determined multigeneration changes. It was the further consensus that accommodation is the correct term when performance improvement is not related to physiological or biochemical change; when physiological and/or biochemical changes are involved, the correct word is acclimation.

Kizer: Well, given that, the Undersea Medical Society ought to perpetuate the usage of those terms.

Kendig: I have a question on the surprising linearity between the convulsion threshold and the anesthetic threshold. Keith Miller has data suggesting that there is a window in the threshold of convulsive and anesthetic antagonism such that one may have anesthetized convulsive individuals. He suggests that the HPNS site and the anesthetic site are different. Are your data in conflict with that?

Brauer: No, indeed. I am concerned with what I perceive as qualitative differences in the clinical appearance of the anesthesia elicited at high pressure and at low pressure, as, for instance, during the recent hydrogen dives or in our bolus injection studies with nitrogen. We have seen the same thing Moeller saw. You can produce the sleeping mouse that wakes up to have a convulsion and goes back to sleep again with barbiturates just as easily as other ways. I was hoping these two would segregate under the acclimation conditions we have used, but so far they have refused to oblige me.

Kendig: Thanks for complicating my life.

Brauer: Yes, I agree--this is a Chestertonian tragedy, the killing of a beautiful idea by an ugly fact. I was looking forward to these things segregating out but they so far at least do not.

Egstrom: Dr. Brauer, if this anesthetic effect is so predictable, then why are the performance decrements so variable? Just as an example, everyone says rather liberally that CO_2 has beeen the villain of the piece and yet we have a number of mechanisms that can change pH (presumably this is the ultimate problem). This would say to me that the problem we have of quantifying narcosis for human acclimation is going to be very, very, difficult.

Brauer: I have a couple of answers to that. One of them is that human physiologists and pharmocologists for perfectly good reasons work on the worst possible part of the dose-response curve. By contrast I can afford to work on the most sensitive part, so I get all the benefit of a curve that is steep, whereas you must try to get the human data down there in the flat part where variability is greatest. I wonder whether in trying to get valid human data you might not be able to push up a little further on that curve. I think you might get rid of some of the noise that I think has terribly little to do with how good or how bad the experiment is, but rests simply in the nature of the dose-response curve. This is one part of the problem.

The other one is that animals can be standardized a great deal more than divers. I have not touched upon the genetic aspects of this problem of variability. For instance, strains that have the same anesthesia resistance may show as much as a 50% difference in hydrostatic pressure required to reverse that anesthesia. To those of us who are concerned with deep diving and the use of nitrogen or hydrogen to alleviate HPNS, where you have high pressure and anesthesia superimposed, these are enormously complicated phenomena. The linearity of pressure/nitrogen interaction may hold quite nicely for anesthesia, but it simply does not exist for the high pressure phenomena. I think probably those are going to be the least important in relation to anesthesia as such, as you encounter it in shallow-water diving.

Kendig: The convulsive thresholds?

Brauer: Yes, for instance the relationship between total pressure and PN_2 at convulsion onset.

May I pose another question? I get the feeling that individuals who have had the experience of narcosis can do a far better job of telling you where they are--replicating the depths--than we can by any of the tests that I know of. I think my old friend George Bond would buy that enthusiastically. He and I compared notes at a time when both of us had been doing a fair amount of deep air diving. I remember that I used to calibrate my depth gauge by reference to subjective sensory phenomena that for me showed up regularly at 270 plus or minus 10 feet. George had a different indicator system that he used. I forget where his came in, but he too perceived it as a reliable and reproducible index. As you talk to people, you get the feeling that experienced divers can separate and assess levels of anesthesia to a very substantial degree.

Hamilton: You can pick the difference between 250 and 280 feet, but what about the difference by your method between 200 and 250? Can you also make that distinction?

Brauer: No.

Hamilton: So you are on a level curve until you go over the edge. If it were linear you would have something.

Kizer: What you are saying goes for any intoxicating thing.

Brauer: I agree. This once again brings up the point that if we want to improve our measurements and comparisons, we may have to push just a little deeper than we are willing to push operationally. I think perhaps by doing that, we might gain very substantially, and I think we do not need to go anywhere near the point I use with the animals, where when they fall over, they fall over!

Egstrom: "Pushing" has another context. If the experience [in the NOSEX dive] at Duke is anything meaningful, they pushed. They lived at 200 feet for 9 days, and I think they had either 4 or 5 bends.

Hamilton: They lived at 165 feet, and did excursions to 200 feet every day. That is the same thing you did at CDC. In Nisahex they lived at 200 feet for 6 days, as had been done in Nisat.

With the Chairman's permission I would like to review briefly the Nisahex experiment. This experiment was conducted at the Swedish Naval Diving Center in 1982. Overall aspects were presented by Muren and colleagues (1984), at the Eighth Symposium [on Underwater Physiology], and subjective details will be given in a subsequent report to be published by the Swedish National Defense Research Institute (Eiken, et al. in press).

Six divers were compressed to 60 msw, with stops of 2 hours at 40 meters and 4 hours at 50 msw. They did excursions during the course of the dive. PO_2 was 0.4 at storage depth.

Standing steadiness, the so-called statimetry Captain Biersner mentioned earlier, showed a lot of effect at first (Figure VII-1). Youngblood saw the same thing at Duke, a rather dramatic initial performance decrement that recovered in a couple of hours. The biggest recovery in the Duke experiment was in a matter of an hour or two. In Nisahex there was a slow but steady change or improvement throughout, after the first spike. Statimetry is fairly objective and not very motivational. Training should have no bearing on it.

They were bored because they could not entertain themselves. The first couple of days they were rather fumbly and they could only do one thing at a time. Given a list of three things to do, they would do the first one adequately, then come back and say, "What was the second thing?" They could do things and actually did them well. They took blood samples, spun them down and separated them. They did a lot of things that required a certain amount of training and knowledge. The two who handled the blood samples were newly graduated medical students, so they knew what they were doing. They were really quite comfortable after they learned to cope. To do something new, though, they had to think it through carefully, then do it slowly and methodically. They could not be in a hurry.

The subjective feelings of the divers is shown in Figure VII-2. The divers felt mildly drunk. They described it "like a Midsummer day, when you have been drinking all day long and are pleasantly high, rather than at a party where you just get smashed." They stayed this way for about 5 days. Then they began to taper off. The most stoic and professional of the bunch never admitted that he was narcotized, but there was no doubt that he was. Yet he did not do things that exposed his narcosis. He kept himself extremely formal and "straight." Some of the younger subjects really enjoyed it. After the experiment, when they read the report of Nisat they said "that's the way it was." What they experienced was really quite close to what was recorded from the Nisat experience (Hamilton, Adams, et al. 1982). They really did not get over it; they did not get over the numbness of the lips. They could not fully enjoy watching TV or reading until decompression was well underway.

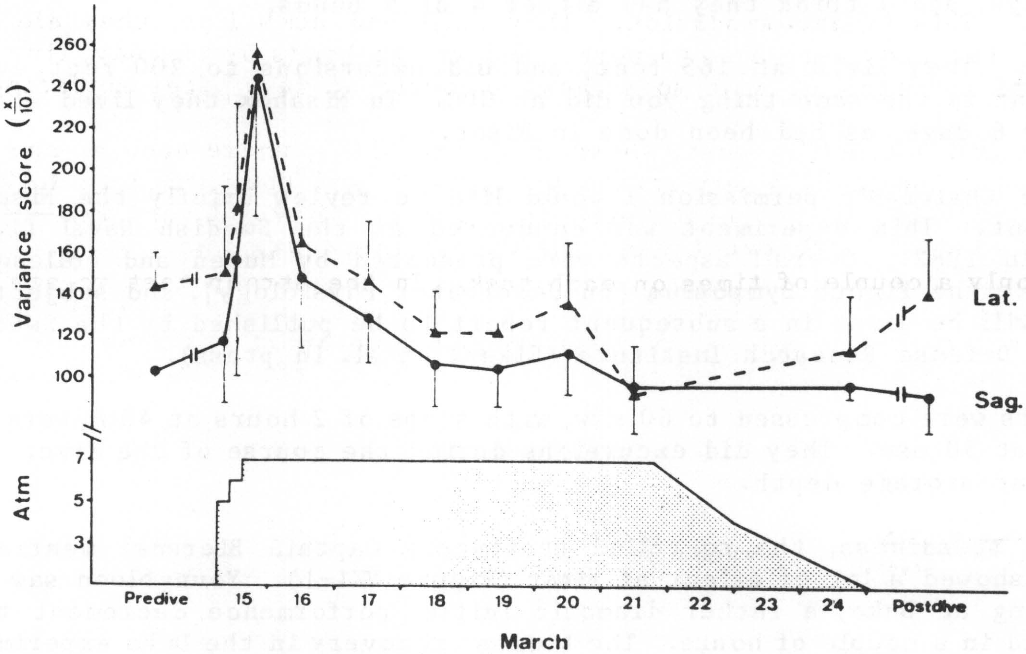

Figure VII-1. Standing steadiness in Nisahex. Front-to-back (Lat.) and side-to-side (Sag.) scores are shown. 4 divers are included in the 5 and 6 atm measures during compression, 6 divers in all others.

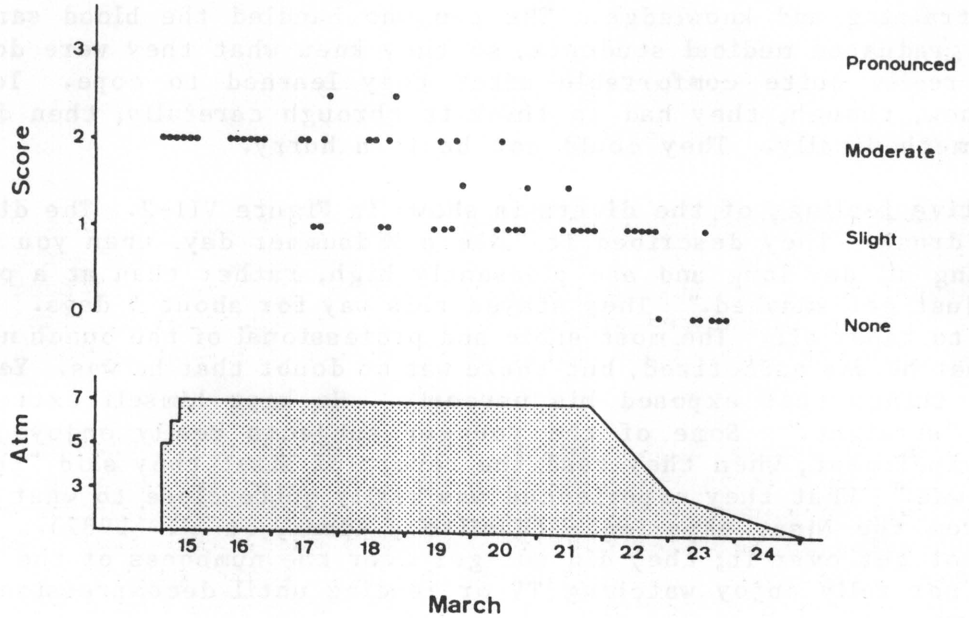

Figure VII-2. Subjective narcosis of Nisahex divers. Divers assessed their feelings on scale shown each day under pressure.

Biersner: That would be accommodation.

Hamilton: This is accommodation. They still had numb lips, they did not enjoy TV, but they were able to work. And we had bends by the way; five out of six had symptoms of some sort, most a couple of days after surfacing.

They also did a tracking test, shown in figure VII-3, where each of the two divers had a handle of a two-person pantograph, and they tried to follow a track so they had to cooperate with each other. They did very well. They showed a gradual improvement over the dive. There was almost no pre-dive workup, only a couple of times on each task. In the Stroop test we saw the same thing, a little disturbance at the beginning, gradual slight improvement over the course of the dive, and then about the same after the dive was over. The RAT is retinal readaptation time following a light flash.

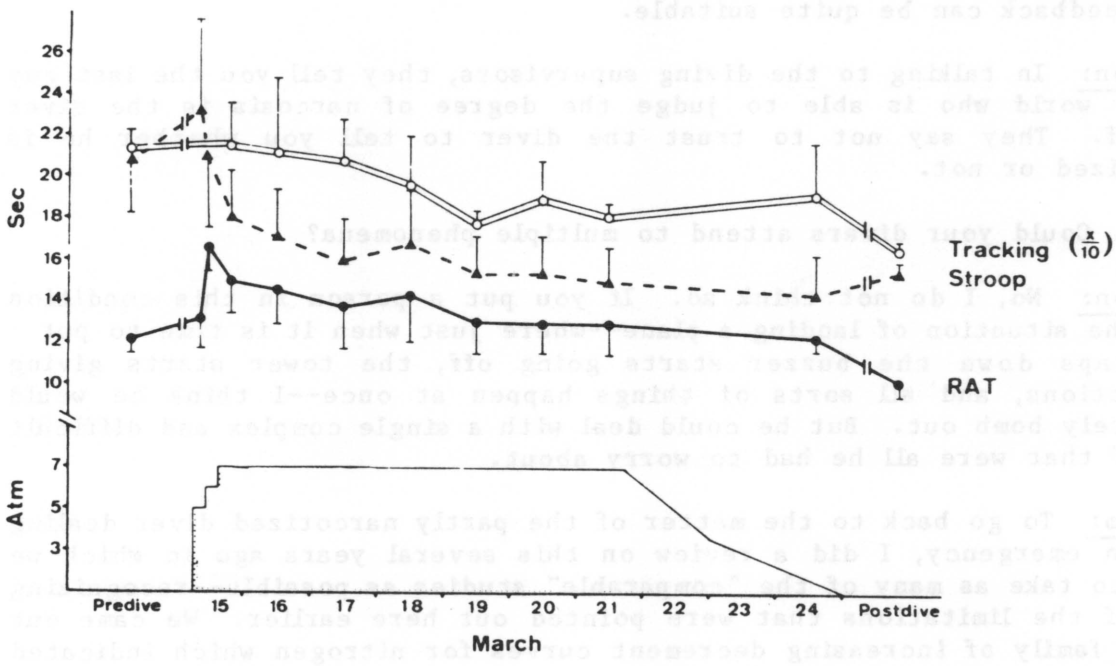

Figure VII-3. Nisahex performance tests. Tracking was a cooperative effort of pairs of subjects using a 2-handle pantograph; score is time to complete task (x 1/10 sec). Stroop is time to name 20 colored words. RAT (retinal readaptation time) is time following light flash before eye begins tracking again.

Egstrom: To me this also tells another story, the old adage that half the strength of the giant is in the knowledge that he is a giant. Some people will start to feel very uncomfortable and feel that they have control and in fact they are going to be under control, and then there are others who are going to say, "I am not comfortable here," and their performance will degrade in the opposite direction. We have seen some wide variations even in simulations, where we would trick people, such as to setting up a situation where they would think they were doing something that they were not doing. If they believed they were doing it, we would get one whole different kind of data profile. And if we did not tell them anything, just let it happen, then we would get essentially a normal kind of decrement. They would believe, for example, that they were at 100 feet of sea water when all they were getting was a lot of hot air moving back and forth and banging on their ear drums. They would come up with some really bizarre differences. What they believe is happening to them has a lot to do with their response.

Biersner: Feedback has significant effects on performance. Fowler and Ackles did the only work that I found in my review in which divers were asked to estimate the degree of narcosis. If the divers were provided with performance scores under the narcotic conditions, a significant correlation was found between subjective estimates of narcosis and performance impairment. This correlation indicates that the divers were judging the degree of narcosis based on the feedback they had received following performance of the task. Under operational diving conditions, I suspect that feedback can be quite suitable.

Hamilton: In talking to the diving supervisors, they tell you the last guy in the world who is able to judge the degree of narcosis is the diver himself. They say not to trust the diver to tell you whether he is narcotized or not.

Kizer: Could your divers attend to multiple phenomena?

Hamilton: No, I do not think so. If you put a person in this condition into the situation of landing a plane--where just when it is time to put the flaps down the buzzer starts going off, the tower starts giving instructions, and all sorts of things happen at once--I think he would absolutely bomb out. But he could deal with a single complex and difficult task if that were all he had to worry about.

Egstrom: To go back to the matter of the partly narcotized diver dealing with an emergency, I did a review on this several years ago in which we tried to take as many of the "comparable" studies as possible--recognizing some of the limitations that were pointed out here earlier. We came out with a family of increasing decrement curves for nitrogen which indicated that if the task was simple and well-learned you would have the least decrement, if it was moderately difficult or less well learned you would have more, and you would have the most if it was difficult and poorly learned. You were literally in a significant portion of the decrement curve with the different tasks and training. The breadth of the curve is frightening, across the study.

The thing that struck me most clearly was that if, for example, you are going to have an emergency procedure, it had better be simple and well learned to expect it to work under conditions of an increased partial pressure of nitrogen of any significance. Because if it is difficult or if it is not well learned, you can anticipate that it is not going to work for you.

Hamilton: Several of the people I have talked to in this situation felt that having close topside monitoring of what they were doing made things a great deal better. What I mean is the ability to get a little coaching, and to ask a question if they forgot where they were, this sort of thing.

With regard to how we might deal with a complicated emergency situation in this condition, we can look at what you have to do in flying. In an emergency a pilot is not supposed to think. He (or she) is supposed to have an over-learned set of responses that he does initially, then after that he is supposed to do what is in the book. He is even supposed to take the book out and read it rather than try to remember.

Biersner: The Navy pilot refers to the NATOPS manual and follows the NATOPS procedures based on the symptoms that are present. My guess is that if we would adopt the same sort of procedure, it might work.

Hamilton: One problem with that approach is cost. To do the amount of procedural development and training to put the diver to the same response level as the air crew could be prohibitive. However, diving is not as complicated and does not require such quick responses.

Egstrom: It is interesting that if you look at the acclimation rate for pressure, temperature, or whatever the environmental variable under consideration, there is rather dramatic evidence that somewhere around 2 to 3 weeks plus or minus a bit you get up to around 80-85 acclimation, but to get the rest of it takes a much longer period of time. It may be that you have to do is store your people 2-3 weeks before your mission. Put them in a topside chamber, let them get acclimated, and then send them down to do the work. Some of the early accidents that occurred in the commercial diving business indicated that the largest number of accidents occurred on the first and second days on the job or when they went to a new set of environmental conditions.

Brauer: I am fascinated by these acclimation data. I have been waiting for them. What about the scaling problems? How do you translate "mouse time" into "people time?" For some things we know how to do this. You do it in terms of life span, specific metabolic rate, and so on. I have not tried to do that, but with the question you are posing it might be worth doing. In my mice it took 15 days to reach maximum acclimation. I would not be very surprised if in man it took substantially more days, but I do not know how many more days.

Hamilton: Like Dr. Egstrom just said, you have a family of levels of performance for the different kinds of tasks, and they are going to peel off. You will reach some of them in the first day.

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DISCUSSION TOPICS:
PERSONNEL SELECTION CRITERIA FOR WORK IN HYPERBARIC AIR OR NITROX:
Paul Linaweaver

The objective of my discussion topic is to consider whether there are criteria currently available that could be used reliably to select personnel for hyperbaric work based on their susceptibility to nitrogen narcosis. I think our discussion has shown that, at least within the depth range we are talking about, that we do not have a good grasp on what has been done in the past in testing.

A variety of test methodology has been used that comes up with the statement that there is a certain decrement. But do we have any testing that predicts an individual's susceptibility to nitrogen narcosis, and will those tests predict or measure acclimation, accommodation, or "resistance." I think the answer is no.

So how do we get them? Some of the research has pointed out areas that we should be looking towards in terms of coming up with selection criteria. It is important to be able to predict an individual's ability to function in the normal state. When I say normal state, I mean his normal hyperbaric state of doing his work. We also want to be able to predict or measure how he is going to perform under stress.

As a first step we have to identify what skills we expect might degrade; I do not think much has been done here. Nobody has looked at other than the gross work product, which involves perception, correlation or mediation, and then motor response. Usually it will be a combination of all three. From the tests that have been used, the end result was usually related to the pre- and postexposure level. But we know little about the reproducibility among different individuals, nor about an individual himself. How does the single individual vary on a day-to-day basis? We are not sure we are measuring the nitrogen narcosis effect? We have no quantitative idea of the additional effect of other stresses, nor of the input of an individual's personality and behavior.

Stresses include physiological effects other than those due to nitrogen and its narcotic effect. The effect of carbon dioxide has been mentioned. Carbon dioxide can influence the narcotic effect of a variety of anesthetic agents. Ed Lanphier years ago demonstrated that divers alter their CO_2 sensitivity in the hyperbaric environment. There was some question, in a chicken and egg routine, whether this lack of response to CO_2 was nitrogen mediated. I think Lanphier pretty well showed that the nitrogen influence on CO_2 response was not significant. He attributed the effects primarily to the respiratory system, specifically the work of breathing and the effect of dead space.

Consider the effect of breathing resistance on CO_2, and how it may effect narcosis at the depths we are talking about. In clinical medicine, an individual with chronic obstructive lung disease tends to "give in" to the hard work of breathing required to maintain a normal CO_2 by tolerating an elevated carbon dioxide. He can nevertheless function well. With our divers living at depth, could the small but measurable decrement that we saw be a result of giving in to the added work of respiration--to the density of the gas--and therefore allowing a subtle increase in the carbon dioxide? This may lead to increased carbon dioxide levels, but it may also have an effect on the so-called dose-response curve. As Lambertsen has pointed out, with any increase in the arterial PCO_2 there is a given increase in cerebral blood flow. The increased gas density and resulting increase in retention of CO_2 may indeed deliver increased nitrogen to the end organs involved. So should our testing involve some sort of assessment of an individual's CO_2 sensitivity?

What effect is age going to have on all of this? Our scientists are not going to be young bucks. Our primary investigators are probably going to be people my age.

What is the effect of hypothermia? One of the things we are concerned about is chronic hypothermia, because of the possible heat loss in the habitat environment (even though it will be nitrogen, not helium) as well as from repeated excursions. What effect is cold going to have on performance? Will our predictive testing have to include an assessment of this?

What can we do in terms of a practical test exposure? Can we take every single guy that wants to go out there and subject him to a depth at which we can expect a decrement (off the top of my head, say 10 atmospheres), or can we use a suitable substitute such as nitrous oxide for predictive testing? That would be a form of simulation. Or can we use other types of drugs for this prediction, as Dr. Brauer pointed out? So my discussion turns out to be a series of "what if's," and "where do we go", and consideration of what sort of testing is reasonable, practical, and truly predictive of an individual's susceptibility and ability to perform at the depths of interest.

DISCUSSION FOLLOWING DR. LINAWEAVER:

Kizer: Do you think we have to know the theoretical mechanisms, or do you think the data base is sufficient to guide or design those studies?

Biersner: Some anecdotal evidence exists that intelligent individuals are more resistant to these effects than less intelligent individuals.

Shilling: We said that in our paper in 1937.

Biersner: Edmonds has made this observation as well. However, direct evidence of this relationship is not available in the research literature. In most research in which Navy divers have been used a correlation might be done between the GCT (General Classification Test--a measure of verbal intelligence) and susceptibility to narcosis.

Kizer: Between the lines I heard you saying that there is some anecdotal data but that the scientific data are really not of very much help.

Shilling: Dr. Ed Lanphier is really convinced that he can set up a selection criteria for CO_2 retainers, and I think the literature bears out that CO_2 does have some potentiating effect on nitrogen narcosis. So it might be worth setting up criteria such as Ed's for this.

Linaweaver: Basically, that is a simple test to perform and there are two ways to do it. You can expose individuals to increased levels of inspired carbon dioxide and measure the ventilatory rate; that is the "CO_2 sensitivity" test. A more accurate method is to control arterial PCO_2 and measure the ventilation rate, but this is more difficult to do.

Egstrom: Most of your aquatic oriented people are CO_2 retainers, to a degree. They tolerate higher CO_2 because they have changed respiratory patterns because of breath-holding and controlled breathing patterns. If we say that experienced divers are better able to tolerate narcosis, then we must also say that CO_2 retainers seem to be better able to tolerate narcosis.

Linaweaver: But our scientists may be divers of lesser experience.

Egstrom: I think that is true, they will be much less tolerant. In fact, I think it may take a bit of training for some of them to become comfortable as CO_2 retainers. One of the concerns I have is that people who are used to higher levels of CO_2 are more comfortable with it, a little more tolerant, and consequently are less stressed by the initial environmental insults of diving. For a novice this could come down to a CO_2 retention problem that would make him uncomfortable. This will have a stress effect. At the same time we may be putting him into a physiological state where he is going to be more susceptible to narcosis.

Carter: If I understand the question you are addressing, some of the technology associated with personnel testing could help you quite a bit. Two questions that come up in selection decisions. (1) Can selection devices help us improve the performance at all, and (2) Does that balance the cost of the testing program?

In that first question, whether you can improve performance, three factors are important. One is the strength of the relationship between whatever you are measuring as a test, and your criteria. Typically we have a weak relationship, correlations of 0.4 or 0.5 if you are lucky. As a second factor, what proportion of your people would be satisfactory in a nonselected population. If just about everybody would be satisfactory in a nonselected population, then the selection process is probably not worth it. If almost no one would be satisfactory in a nonselected population, then you have to do an awful lot of testing to find that good guy, and the cost can be prohibitive. So you want to be dealing with something where a moderate number of people would be successful.

A third factor is, what proportion of the applicants can you afford to throw away. If you have to take just about everybody who comes in, then do not bother with testing.

Pilmanis: In our situation we will not have a choice.

Hamilton: We are not at the graduate school admissions office. We have trained scientists in which there is a big investment. We are not going to "select" these people. We have to make it possible for them to go down there and do the job the way it is.

Brauer: The kind of people who are willing to work at this kind of thing are the same kind who are liable to want to work up in the mountains, where you have exactly the opposite physiological problem. It can be a dilemma.

Pilmanis: We do not have the luxury of selection. Whoever qualifies, we have to accept, unless there is a medical or safety hazard. We have to accommodate them, and in some manner enable them to do their work. In addition, we probably will not have more than 5 days allowed for training.

Brauer: I think that they could very well be preconditioned for a situation like yours.

Egstrom: One thing that would probably be worthwhile is to identify the experiment that answers the question you raised, put it up against a subject population, and then generalize later. But I think not to make an effort to get selection criteria is probably being less than responsible.

Pilmanis: I think that selection criteria are probably much more important for other sectors, in commercial diving and military diving, where they can have the luxury of selection.

Hamilton: There is an unofficial selection process that takes place in the commercial industry. The people who do not like to be cold or can not stand CO_2 or whatever tend to select themselves out.

Greer: Does anyone have any theories about the number of candidates who get washed out of diving programs, military or commercial, because they are unusually susceptible to narcosis?

Hamilton: There are a few. There surely are not very many.

Biersner: The Navy does not have any data on this matter.

Greer: There is a great range of susceptibility to narcosis, but I just have not seen that as a selection criterion.

Brauer: Everyone says "Well, of course, the old experienced diver, the master diver who has been in this for a while, is highly resistant to that. I am not sure that is an axiom.

Greer: I am not sure that is an axiom either, but we have just been talking about what we are going to do to select out these people among our scientist saturation diver candidates who are unusually susceptible to narcosis. Is that really a problem? It certainly is not a big problem in the industry.

Biersner: The Navy does not have that luxury. The Navy has to admit everybody that applies (who is qualified medically) because of a severe shortage among the senior diver classifications. Secondly, Navy divers do not perform much work at narcotic depths and when they do, they breathe helium-oxygen mixtures.

Linaweaver: Should there be a difference in terms of the people who are going to be users of the system and those who are operators of the system, those who are expected to react in an emergency, take charge, and see that the scientist users get out of whatever mess they are in. Should there be separate criteria for these people compared to the scientists?

Kizer: Maybe you will have to determine what these criteria are, which no one seems to be able to decide on in the first place.

Linaweaver: Do we know what skills are required to do the jobs that we have been asked to look at? Can we identify those skills and determine among those skills, which skills affect the individual's ability to do whatever he is down there to do. In other words, is this individual going to be able to perform this experiment meaningfully, and the bottom line, is it going to be accepted as science?

[Further discussion of selection criteria followed. Pilmanis noted that the Navy accepts anyone who is qualified medically and would not present a safety hazard. The group debated whether different criteria are necessary for users and operators, i.e., those who would be responsible for reacting in an emergency and taking charge, as opposed to the scientists. It was agreed that for the scientists, scientific skills are the first consideration. Pilmanis reiterated that the habitat will be open to all types of marine research, both observational and manipulative.]

Pilmanis: The inquiries we have gotten so far are along the same lines that a typical marine biologist is doing with scuba in shallow water. This includes everything from surveying to setting up a field experiment. There are also a lot of behavioralists. The bottom time required for observation of fish behavior, etc., is very long, and it is almost impossible to do this sort of work from the surface. We are also encouraging experimental science which also requires a long down time. I suppose the analogy would be terrestrial biologists.

In addition, however, the way the habitat is designed it becomes the lab and office for the field data. In other words, when they return to the habitat they can process their data. They will have a computer. One of the problems with Hydro-Lab right now, and most of the previous scientific habitats, has been that whatever you can throw in a suitcase is your lab. One of the basic concepts of our habitat is that it is the next generation of equipment. It will enable the scientists to do more in saturation than in the past. It will require more thinking. To go down and collect specimens or take a picture is one thing. It is quite another to go down and set up an experiment and follow it in detail for 10 days.

Linaweaver: Are we looking to set up criteria to predict an individual's ability to perform, or do we want to develop something that says he has performed? Let us use an anology. When you sterilize a surgery kit, you put in an indicator that says the thing has reached a certain temperature. After it is autoclaved and opened up, if the indicator is colored you can assume that this pack is sterilized. Maybe what we want is a performance test that can show how well he performs while he is down there. He met our standard test that measures the following parameters, etc., and he did not degrade beyond the point at which we have said he is not competent. Is this what we are looking for? This cannot be accomplished without pretesting.

Kizer: So you have to develop a pretest.

Egstrom: You are putting an on-board computer into the habitat on the presumption that they are going to be able to operate it.

Pilmanis: That is correct. We designed the equipment based on an assumption that people could use it.

Egstrom: You have a piece of equipment that would make a most adequate testing vehicle to test narcotic decrements and acclimation.

Kizer: That is why we have a particular interest in the video games. This could provide daily scores, and it will also provide entertainment.

Biersner: It is similar to the problem of trying to determine if a driver is inebriated. A proposal has been made to have drivers perform a reaction-time task which, if performed correctly, could unlock the ignition. This procedure is based on the assumption that reaction time is related to the complex psychomotor abilities involved in in driving an automobile. The data on narcosis, however, indicate that this technique would result in a high false negative error rate. Many narcotized divers would be able to perform a simple reaction task and even a complex reaction time task and still might not be able to perform a complex scientific task under narcotic conditions.

Egstrom: I am suggesting that if you know that they are going to use the computer to process data, then obviously they are going to use their computer to process, let us say a data package on the surface. That is their baseline. They now go down and they process the data package on the bottom, and if they now have a 15% error in the use of that device, it may be that somebody else should be operating the computer.

Kizer: Even before that, assuming there is an on-line monitor, if you see that they have a 15% error rate, what do you do to correct it? Do you pull them up and tell them that they are washed out? If you let them stay down 2 weeks and they blow their whole experiment, that creates different problems.

Pilmanis: You have put a question mark on their data at that point.

Egstrom: Which may resolve itself over a period of time, too. You probably would learn as much about acclimation processes and narcosis as you would about error factors.

Pilmanis: It has been suggested that the first year of missions have extensive physiological testing. But do not lose sight of the original reason for building the habitat, i.e., it is not for medical testing or engineering that the scientist is there. All he is interested in is his research. The rest are just vehicles to get him there safely.

Egstrom: But he must want to know that his research is accurate.

Kizer: I wonder if a compromise position could be that for the first year you structure the types of studies that people do. Limit it to simple studies.

Brauer: What defines the depths to which you go? Probably all of our problems would disappear very quickly if the depth limits were 140 feet with a habitat at 80. Are we scraping the margin of nitrogen narcosis? How good is your reason for setting the depths, or are you including that as precisely one of the experimental variables?

Pilmanis: That question was rigorously gone over in the beginning of the program. The science community was asked: "Where do you want to work?" That question was answered and the equipment was designed according to those guidelines. The equipment is capable of storage depth to 120 feet on nitrox, which is equivalent to 155 feet on air. Excursion dives can go beyond that. How deep is an open question. NOAA currently restricts us to 130 feet. However, if scientific need dictated that somebody needs to go deeper, that can probably be approved. Are these depths a nonproblem?

Kizer: I am not sure I have heard anything today that says that is not a problem for your mission.

Brauer: Certainly excursions going substantially beyond this could fairly swiftly entail problems. If NOAA is going to be conservative and let them excurse for no more than 20 feet it would not be. But if they stick with their reasons for doing excursion diving, them we are obviously talking about 300 foot depth potentials, and now we are looking at real problems.

Pilmanis: We are really talking about two areas, one is safety and the other is the ability to do the work in the presence of narcosis. Such things as using scuba for excursions to 200 feet where your navigation becomes extremely important are a safety consideration. If narcosis is affecting somebody's ability to use the navigational systems, then you have a real problem.

Shilling: Are you going to use normoxic nitrox?

Pilmanis: Excursions will be on air, habitat gas will be normoxic or with slightly more oxygen.

Hamilton: There will be a big effort to make it possible to make multiple excursions so they would not have to stay in the water all that time, but on occasions they will. For that you may have to use nitrox for the breathing gas, or some other method. This can be dealt with.

Regarding acclimation to narcosis, one thing you might consider is making the mission 2 weeks long instead of 1 week, so they will have a little more time to get with it. As it is, they are going to be just about ready to go to work when it is time to decompress.

Pilmanis: It has been said that the maximum point of acclimation is reached in 5 to 9 days. The only reason that we are projecting 7-10 day missions is that the Hydro-Lab experience suggests that as a reasonable time period. Why do they limit their missions to 7 days? It is comfortable. People just feel that is long enough to do what they need to do. After 7 days they start getting distracted and are not as productive. However, there is a big difference between the design of our habitat and Hydro-Lab. Hydro-Lab is very small and the four people in it are very cramped. Environmental control is very limited. Our habitat is designed to much be larger with a shirt-sleeve laboratory/office/home environment. I would think that the users will find that they can stay longer and still be productive. Perhaps they will stay longer and do research on acclimation.

Biersner: A behavioral test battery could be designed to assess the specific skills required of a scientist working at depth--to determine if these skills are impaired.

Linaweaver: I think we have a moral responsibility in this specific project to contribute good data to that body of knowledge. At the outset, the first evaluations of the system as a unique experiment, we must produce sound, valid data. And second, I think we should have some sort of ongoing monitoring for the scientists themselves so that they can establish that what they have done has been meaningful work. So I think we have two goals. And from what I hear you say, the second goal is relatively simple. The first goal may be more difficult.

Kizer: I would certainly second that.

Brauer: It is easy to design those tests. But I would like to ask Dr. Biersner whether having designed them it will have any predictive value relative to what you need. I do not think you could guarantee that, could you?

Biersner: The test battery that would be developed essentially duplicates the performance tasks that are to be accomplished. It would be a job sample. I think the test battery would help to improve the scientific validity of the work they are performing, but this information will not contribute much to the overall problem of assessing the general performance effects of nitrogen narcosis.

Kizer: But that has to be an ongoing process. Each mission has to have that built into it.

Carter: I do not think you would want to eliminate a person on the basis of that testing. You just want to give the person that information during the dive. Afterward, the report should say this experiment was done by a person who showed the following level of competence in this setting.

Hamilton: Is there any possibility of requiring that before each day as part of a set procedure but also in the guise of safety, have everybody take a test to make sure they are ready to go out on a dive?

Brauer: I think operationally any test that does not take the motivation of the subjects into account is going to be worthless. One of your problems is going to be not just to set up a dummy task, but also to set up dummy motivation. I am sure a number of you have done diving operations in which you had to go deeper than you really liked because something had to be done. And it can be done, and done reliably. I have photographs I have taken at 340 feet. I do not think I would like to take many, but you can do it. And I think this is going to enter into your reliability problem as a key component if you have a highly motivated chap going down. You can put up with a fair amount of narcosis and still function.

Kizer: By and large I would suspect that these people are going to be highly motivated.

Kendig: There is one other thing that could be done from information generated at this workshop, and that is since this is presumably a rational and intelligent group of people, one can submit to them the kinds of tasks that have shown to be most effective. To say that their best chance of getting reliable data is to do the kind of experiment they have done before often, that is more or less automatic and does not require any fine analysis from the data.

Pilmanis: That can be injected in the proposal process.

Kizer: This is what I was getting at before, that maybe you should structure your studies, for the first period of time at the least, with criteria like that built in. You are going to want only those sorts of studies done until you have a better index of whether this is really a problem or not.

Hamilton: This not a new experience. This kind of work has been done and so we are not really sticking our necks out that far.

Pilmanis: There is one difference from the past. In many previous habitat operations, the goal was "try it and see if it works." Once you have proven the method works, you walk away from it. The method of saturation diving for science has been proven. Now, we want a productive and routine operation on a long-term basis rather than a one-time survival situation. Therefore, the research data must be a lot more accurate, more credible.

Linaweaver: Could I make a summary statement to see if everybody buys it for this particular discussion?

(1) Because of the depths we may be operating at initially, we need not develop a specific "selection" test for the personnel in the experiment. We have a reasonable feeling that they can operate safely and adequately at this depth without going into an extensive pre-selection testing.

(2) We also agree that because this will be unique exposure in terms of temperature, depth, and excursion depth, that we owe the scientific community to develop some testing methodology to determine what decrement, if any, does occur.

(3) That some sort of individual performance record be established during each experiment on the experimenters as part of their daily routine, at whatever frequency is determined, to give some indication of the quality of their work.

Carter: It may not be feasible to select even if you wanted to. One would have to consider the success rate without selection, the number of applicants, and the strength of the relation between the selection test(s) and job performance.

Pilmanis: I agree with the first two statements, but I am a little uneasy about hanging a score card on each scientist each day.

Greer: How else are we going to answer the concern you expressed at the beginning of the meeting? How are we going to be sure if the data are acceptable if we do not know whether the people are competent?

Kizer: Until there is a track record, you may have to have a surveillance mechanism.

Hamilton: Can we not say we should establish a performance record. How able are these people to perform over the course of the dive?

Egstrom: The scientist should be able to demonstrate when he comes on board, before he goes down, that he has, in fact, overlearned whatever those skills are that he is going to be using and should be willing to be tested on that. He is going to do it as a normal part of his work anyway, so he should be willing to be tested on that as he goes through.

Biersner: The jobs the diver is going to perform will be sampled, and tests constructed to assess the ability to perform these jobs. These jobs will have to be well learned so that test reliability will be high.

Egstrom: I think you will find that some folks will not have overlearned the techniques that they are anticipating using down there unless they are forced into it. They may have their graduate students and technicians doing it; individually the scientist may not have hands-on learning until they get into it.

Hamilton: Some people will not have seen the apparatus until they get there.

Kizer: So what the criteria are for at least the initial studies needs to be clearly specified in the RFP's (requests for proposals).

Hamilton: Time is going to be limited in the habitat, so you are really obligated to make sure you get the maximum out of everybody who goes down there.

Pilmanis: Absolutely. In fact the mechanism is there for the preplanning through the RFP process. This is also done to some extent with Hydro-Lab right now. It can be done.

Hamilton: This is beyond nitrogen narcosis, this is just good management.

DISCUSSION TOPICS:

NITROGEN NARCOSIS AS A CONTRIBUTING FACTOR IN DIVING ACCIDENTS:

Glen Egstrom

Reviewing the literature revealed a great deal of information as to what narcosis effects are and what can be anticipated with changes of exposure, but when it comes down to identifying narcosis and the role in accidents, it appears that there is a real dearth of material. In the URI fatalities statistics for the period 1979 to 1982 or 1983 there were cases involving narcosis, but the words used to describe those cases were "estimated causes." In other words, the people were deep enough when they had their problem that an "estimated cause" could have been narcosis. There were three cases in 1976-1979; one of these was at 250 feet and the other two were in excess of 130. It is clear that the involvement as far as the accident is concerned has been largely speculative.

Let us take a look at how we have dealt with the problem. We have essentially established a series of limitations for various levels of certification. Within the University of California system and in most of the scientific diving community, an individual who wants to work deep starts off by first becoming qualified at one additional atmosphere of pressure, taking a number of dives, and then progressively working his certification down an atmosphere at a time to the limit, which right now is 200 feet. There are certain circumstances where scientific divers go even deeper than that.

I spoke with Jim Stewart, diving officer at Scripps, on the history of their deeper dives since 1950. They have a significant number, in our colder California waters. Every year (he did not know the exact number) they dive to the 230 foot range and put in instruments. They make routine placements, yet they have never had a problem of not getting the instruments placed effectively.

Jim Stewart has made a number of dives on air to 300 fsw--while making body recoveries, not placing instruments. There is one place in La Jolla Canyon at about 300 feet depth that seems to collect the bodies of people who drown in the Canyon. Jim has been in there repeatedly and had no injury, no difficulty, no problems associated with narcosis.

The La Jolla Canyon flushes periodically and drops all of the residual material at the base of the canyon; they have meters and instruments set in the sides of the canyon walls so they can try to predict when the canyon is going to flush. The instruments are reasonably complex and require dual task responsibilities in placement. Again, they have not had problems.

It occurred to me when I was going through the conversation with him that maybe we have another example of what I call the "Friday at 5 syndrome." We know that Friday at 5:00 o'clock there are significantly larger numbers of people on the freeways and highways who have been indulging in alcohol. By 5 to 7 on a Friday afternoon you can be reasonably certain that the TGIF folks have done their trick, and yet there does not appear to be a significant rise in the number of accidents not consistent with increased traffic. Perhaps, like the Friday drivers, the deep divers exert additional amounts of self control, knowing that they are going into a deeper diving profile. For example, I tend to become more introspective and focus more strongly on what is going to be done at depth. Work is done much more mechanically and probably quite considerably slower, but typically with few mistakes. I dive 200 feet minimally 10 to 12 times a year and do so with little difficulty.

One of the things I think we need to address is what kinds of accidents would appear most likely to happen at such depths. In other words, considering the mechanisms, what kind of accidents would we anticipate? I think the work that Captain Biersner has reported will help answer that question.

Other factors that need to be considered are the complexity of the tasks and the degree of focus or concentration that is utilized. What it seems to boil down to, from a functional or operational management issue, is that the acclimation of the individual to the job must be effective. One must ensure that individuals have "overlearned" the skills they are going to use so that they can reproduce them with a minimum amount of cognitive energy. They should also operate in a millieu where the stress of things like visibility and temperature are minimized.

I believe there is a real lesson to be learned in a four-person sport diving accident referred to as the "Farnsworth Banks" case. There were four young people who apparently identified themselves as experienced divers, even though three of the four had only been certified three months and the fourth had been certified for about 5 months. They were going to make their first deep dive at a place called Farnsworth Banks. In the preparation for that dive they had apparently decided the night before that it was going to be a 100 foot dive. For whatever reasons, it appears that each of the two buddy teams went deeper than 100 feet and it appears that they also did run low on air while apparently trying to make an ascent to the surface. There were several other compounding problems, a weight belt inside the crotch strap and some other things that contributed to the problem. As nearly as we can tell from the air remaining in their tanks, they probably got within 40 to 50 feet of the surface before they turned around and fell back down to the bottom. They were recovered in 135-150 feet of water. One of them apparently had a spear gun and was doing some spear fishing with his girl friend, while the other two were just looking around, apparently failing to pay attention to their air resources. Consequently, when the time came to go to the surface they did not have the wherewithall to be able to make it. One of the tanks had 36 psi and then one had 52 psi of air remaining, which means that they did not break the surface--but it also means that they could not have taken that much air out of the tank at the bottom.

DISCUSSION FOLLOWING DR. EGSTROM:

<u>Biersner:</u> Some personality test data substantiate this observation. Divers breathing nitrous oxide become more introspective and concentrate on the immediate task. Under normal conditions, divers attend not only to the

immediate task but attend as well to other peripheral events. During narcosis, sufficient mental resources exist only to attend to the primary task. At higher levels of narcosis, even the primary task will become impaired. This is the rationale for using "dual task" tests--to be able to assess the effects of narcosis on secondary (i.e., peripheral) tasks.

Kizer: Is this a process of sensory screening?

Biersner: More of a cognitive screening process. The diver recognizes that mental resources are limited by narcosis and dedicates these available resources to the primary task. For diving under highly dangerous conditions, such as during combat or in shark-infested waters, this limited attention effect could be a problem.

Brauer: I have done over the years probably 10 or 12 dives to and beyond 300 feet. I think I would fully subscribe to your analysis. It appears to me that once you decide that something needs to be accomplished, you will focus on the task and have a fair chance of bringing it off. The real danger from narcosis comes not while you are doing your task, but afterwards, once you have decided the task is done and you are on your way back. Because at that point that mechanism may no longer function, and you run the danger of missing cues, of returning faster than you ought to, and so on. Once again, once you are aware of that danger it is often avoidable; in terms of the very deep dives, the danger from narcosis starts when the mission is accomplished.

Biersner: The behavioral effect being described as "accommodation" may be simply learning to focus on the primary task during repeated exposures to the narcotic conditions.

Egstrom: We are not saying that there is not impairment, we are simply saying that by exerting more control, you compensate for that additional impairment. Therefore, you have no net loss on your performance.

Biersner: An impairment probably could be demonstrated if dual-task measures were used.

Pilmanis: I want to emphasize a point. There is a world of difference between the type of diving that Jim Stewart does in the Scripps Canyon, and the type of scientific work that we are pointing at with the habitat. To go down and service an instrument, or take a picture of a bug is one thing. You can focus on that single task. However, to go down and do rigorous experimental science is quite another. The overwhelming reason for that scientist to be down there at all is to take his brain down there and use it. That is a different ball game, at least in my mind.

Biersner: If multiple (i.e., dual) tasks are involved, performance is going to be impaired.

Linaweaver: I would like to make a comment on the level of experience. In talking about the scientific community, maybe about 1 to 5 percent of the 2,000 scientific divers do reach these depths. How do we get a scientist to the level of being an experienced diver so that he can handle these emergencies? What we need to address is accident potential in the inexperienced diver.

Shilling: I want to make the point that there are very few histories. UMS is doing a book on diving accident case histories. We have some 720 collected so far, but not a single one of those has any reference to nitrogen narcosis as the cause or even suggested cause of the accident.

Biersner: In the limited cognitive resource model that I just described, the diver, by concentrating on the primary task, may fail to monitor depth, time, air pressure, and other events that are involved in maintaining a safe dive.

Pilmanis: That is probably true. At the USC Catalina Chamber we have 415 files on diving accidents; not one is directly attributed to narcosis. However, in how many cases were mistakes made due to some level of narcosis? I can not answer that question.

Shilling: I am convinced that many of the accidents are caused by narcosis; but we do not report it that way; we should start reporting it.

Greer: I published one case history which I attributed directly to narcosis. A young fellow was at 230 feet in company with three experienced divers who were working, and I think he became narcotized.

Biersner: I used to look at Navy accidents. The typical scenario was that they would run out of air and not know it. That is where narcosis comes in; they are not monitoring their bottles. I do not know what they are doing. Sometimes they would not even flick on their reserve. What they would tend to do is panic and start going deeper. They would lose their orientation and think they were going toward the surface while they were actually going deeper. When they ran out of gas, they would drown. There is case history after case history of this with military divers.

Egstrom: Paul's question asking how could we put these scientists into the position of becoming experienced divers is a good point. I would suggest using the graded depth program. People do not start off being 200-foot divers, they work themselves down in some progression to the point where they have a chance to accommodate effectively.

There is also the question of what other kinds of measures might be dealt with; one of them would certainly appear to be task organization. If the task is more organized, simpler, less involved, better learned, then we would anticipate fewer accidents coming along. From the references that I was able to go through, it would appear that you would want to minimize dependence upon recall.

The further point is that if we recognize that accommodation is taking place, it seems only logical that we should try to build in some insurance by seeing to it that the person who is going to be doing the work would go through this accommodation process before being put to the task.

Biersner: Much of the research data indicate that performance learned on the surface is not remembered well at depth--that information is retained best if it is learned under the conditions in which it is to be used (i.e., learned at depth).

Egstrom: Agreed. We did a series of studies on recognition and recall and found that those tasks that were learned at depth were better reproduced at depth than were those same tasks if they were learned on the surface and reproduced at depth. We were working down to 100 feet.

Kizer: If you learn it at the surface and go down and relearn it at depth, is learning greatly facilitated or do you have to relearn it completely?

Egstrom: I do not know of any studies on it. Are you aware of anybody that has actually studied that pattern?

Biersner: No; but I suspect divers would have to totally relearn the task at depth. Baddeley has demonstrated that the tasks must be relearned at depth.

Kizer: That would be important for operations. But if what you are talking about is learning it at less than 100 feet, then you are not really talking about narcosis.

Egstrom: It appears again from the literature and from the experience of a number of people in the scientific diving community, that we really need to consider the thing that Dr. Linaweaver mentioned, of bringing the scientists up to a level of experience. First, consider water temperature and visibility. It appears that we get greater learning effects in dirtier, colder water than we get in clear, clean water. And this is one thing I believe is operating in the sport diving community that may get people in trouble. People go down in the Carribean; they do their wall dives, their 150-and 200-footers, and everybody is happy as a clam and they come back without incident. Here in Southern California they go out to Catalina Island and try to follow that same profile. I believe many get themselves into some difficulty due to the changed environmental conditions. We do see accidents that are occurring in these circumstances but, as I mentioned, they are not being attributed to nitrogen narcosis and I think they might very well be. There have been studies that have indicated in open water tests that performance decrements are seen as shallow as 60 feet in cold, dirty water. It is not particularly life threatening, but there are in fact measurable decrements.

Shilling: When you say dirty water, do you mean seaweed and all kinds of pollution?

Egstrom: We mean limited visibility.

Shilling: From whatever cause.

Egstrom: Yes, and so the temperature variations, the visibility issue, the emotional set point, may very well make a difference. For example, the anxiety increase in a number of the studies have been identified with additional decrements of up to 18 to 22%. Whether that is potentiating the narcosis or whether it is a different issue does not make any difference, functionally. The problem is, we do have performance decrements on that order of magnitude that are being attributed to how comfortable a person is.

Biersner: The performance improvements demonstrated by Moeller appear to be attributable to coping effectively with stress. These data show substantial performance improvements--usually around 30 to 50 percent--can be obtained by reducing stress.

Egstrom: Again, if we look at the way that much of these data have been gathered we see that there have been multiple stresses, by definition, operational involvments in those experiments and in the experiment design. It is just that the stressor the experimenter tried to control was the gas tensions for nitrogen. But in fact, it is very difficult to eliminate all the other stress issues. This should be kept in mind when looking at the cause of underwater decrements.

Brauer: If one wants to focus on the role of narcosis in this, is not the obvious control of this to perform the same operation under air diving and under heliox diving conditions? I recognize that normally that is not what one would do, but surely if one wants to ask about the role of narcosis, that is a clear cut way to control it.

Egstrom: I was quite surprised to find I have only four case histories on deep dive incidents and these are very limited in information. One of them was at 70 meters for three minutes, a clearance diver on the verge of unconsciousness. He had a PCO_2 above 65 mmHg. Another one was an amateur diver at 35 meters who exhibited signs of narcosis and then lost consciousness in the presence of a PCO_2 of 68 mmHg. And another professional diver lost consciousness at 69 meters. There is no other information in the abstract that tells you what that was all about. Yet another diver lost consciousness at 40 meters. In both of these two cases it was later judged that both showed CO_2 retention characteristics, that they were both "CO_2 retainers." One was 65 milimeters mercury and the other was 57.

Hamilton: Those are not dangerous levels of CO_2, those are normal.

Egstrom: 65?

Hamilton: Well, normal for someone working in a diving suit. Trained or not, when you breath on diving equipment CO_2 will be high, but those levels are normally encountered.

Linaweaver: Another case not reported in the literature involved an experienced diver who was used to diving on helium and oxygen in accordance with company regulations when they go below 170 feet. He was hired by another company to work at 190 feet on air, and he had not been to 190 feet on air for many years, since he had been working with heliox. He went down, worked hard, became confused and disoriented, kept complaining that he was not getting enough air. He kept cranking up his air (he was in a hard hat) and had an embolism and died as a result. I feel sure that narcosis had an influence on that dive and may have contributed to his feeling of dyspnea breathing on a closed rig. But again, here is an experienced diver who is not currently experienced with air, because it was the first dive at that depth on air for many years.

Egstrom: I realize that Captian Biersner does not like anecdotal records, but sometimes they are all we have.

There is a well accepted understanding within the scientific diving community, and I believe also among people in the Navy, that if you have not dived deep, then you should expect to have a more difficult time.

We have had to institute into our own program a ruling that a person who holds the 200 foot certification, to maintain the certification has to go 6 times a year to the depth of the certification. If they have not been there for several months, they move themselves back one "layer" and dive there before they jump back to 200. Progressive depth exposure appears to benefit performance.

Biersner: The question remains, what is mediating this effect?

Egstrom: The point I am making is that the data would support that you are better off if you have been diving deep regularly then you are if you have been diving deep only on an intermittent basis. We do not have good information on the acclimation, how long it lasts and what time periods we are talking about.

Biersner: I was wondering if Paul might tell us about the deep air dives made by Navy divers near San Clemente Island?

Linaweaver: An ordinance group out of Long Beach worked at San Clemente Island and they routinely made 200 foot dives with scuba. They definitely had to work up to it, both for decompression as well as narcosis. They had to get special permission to dive at that depth. They had a scheme of working people up from shallow dives; the people involved were very reliable. They had good leaders. They were the first to recognize, probably before the Navy as a whole, that you have to do "workup dives." This was in the late '50's and early '60's in the Polaris program. They did a "popup" in 200 feet of water. They did hard manual work as well as reasonably complex assignments; they attached explosive mechanisms and fuses. They were able to do these tasks after being worked up in terms of doing the job at depth. They were very successful, and had very few incidents of either decompression sickness or accidents in what was very deep diving.

Kizer: The Hawaiian divers know that very well. I was involved in a number of cases of decompression sickness, and although decompression was usually the problem, they would certainly talk about narcosis as well. And they were routinely making dives of up to 300 feet. These were diving fishermen.

Brauer: Glen, there is one other group of people that it might be worth to check into and that is the French Corailleur. They dive for coral in the Mediterranean. They habitually dive very deep. In fact, there used to be a special heavy-walled air bottle that could be charged to higher pressures, and that were known as France's Corailleur bottles. I would be surprised if the French underwater sports people do not have access to some kind of records on these people. They have been in business for a long time. Certainly they are one group of people who are semi-controlled and still habitually do very deep diving.

Egstrom: I believe that the lack of case histories is due to the reporting mechanisms and the fact that we really do not have any way to quantify the effect of narcosis on a particular dive. There do appear to be differences in performance decrements for whatever reasons. The colder, dirtier water off Southern California is going to have a greater effect than cleaner, warmer water. I think we might be well advised not to try to translate experiences in clearer, warmer water to dirtier, colder water and expect that the decrements will be similar; I do not believe they will be.

I think the narcosis issue is that we continue to do what we have been doing, and that is to avoid it. We should limit ourselves to depths and tasks that mimimize the effects. We recognize the need but do not have a handle yet on the limits. I hope that is something that will come out of Catalina, that we will get some cold, dirty water experiences over extended periods of time.

Biersner: Given that a narcotized diver is functioning with limited mental resources in which safety is likely to be ignored, then the diving situation has to be structured so that safety factors are monitored by topside personnel or by personnel in the habitat. This would require rigging the diver with sensors so that safety factors could be monitored.

Pilmanis: Due to a variety of constraints and requirements, our habitat operation will have procedures and equipment that do not necessarily follow commercial or military diving standards. For example, control of the system will be in the habitat rather than on the surface. Scuba diving will be allowed. We would like to keep it all hose diving, but because of the nature of the work, some of the people have to have free-swimming capabilities. A good way to go would be rebreathers, but financially and because of training requirements, this is out of the realm of possibility for the foreseeable future. Thus, with scuba we have to accept "running out of air" as a potential hazard. Because of saturation, it is even a worse hazard than diving from the surface. We are trying to do what you are suggesting. Navigational aids, training techiques, etc., will be used to make the system as safe as possible.

Biersner: The more external monitoring that can be incorporated into the diving situation, the lower will be the risk of having an accident.

Pilmanis: I think that is an important point.

Linaweaver: Can we give people experience in a synthetic way before the actual operational exposure? For example, by having them--at their own laboratory--breathe nitrous oxide equivalent to whatever narcosis is expected, to have them learn their tasks or whatever they have to do, under narcosis but at one atmosphere with all the safety in the world.

Hamilton: One current investigator who is really trying to find out what the mechanisms of nitrogen narcosis are, Barry Fowler of the University of York, Toronto, in two as yet unpublished papers concludes that narcosis does not disrupt, rather it slows down the mental processes. It is only when the input gets ahead of one's ability to process it that we see disruption. (Fowler, et al, 1983; K. Hamilton et al, 1983. See references after Hamilton paper.)

Biersner: That is consistent with the limited mental resources model that I mentioned earlier.

Hamilton: Narcosis does not go in and switch around all the connections in your brain, it simply squeezes the amount of information you can process. His work is interesting because he has done good laboratory work to show exactly what we have concluded here. If you draw a "three box model," where you have the learning, the long-term memory, and the recall, Fowler shows that the disruption is in the recall process. This requires that you embed the information when you put it in, or use a recognition situation rather than a pure memory task or a pure recall task. He concludes that if you employ recognition-cued processes you can function well under narcosis. This he supports by laboratory work what we have said here.

Biersner: The diver has a choice--do many tasks slowly or a few tasks normally. The experienced diver recognizes this choice more readily than the novice diver, and therefore appears to do better under narcotic conditions.

DISCUSSION TOPIC:
NITROGEN NARCOSIS AND DIVER CASUALTY MANAGEMENT:
Kenneth Kizer.

Let us now address the role of nitrogen narcosis in diver casualty treatment, as a logical step beyond the session on accidents. In general, treatments are conducted at shallow depths, so narcosis should not be much of a problem. It can be a great problem at the 165-foot level, but even at the 60-foot level some people have trouble integrating the kind of data involved with patient management. I see this as somewhat analogous to the science situation, where you are not programmed to do the task. You have to integrate data from the patient and then make management decisions based on what is coming in. Those are multi-factorial inputs.

We can divide casualty treatment into three general areas. First is the ability to perform therapeutic interventions in the chamber - e.g., things like putting in i.v.'s, endotracheal tubes, chest tubes, bladder catheters, taking blood, and doing general examinations. Another area is determining the dosage and effectiveness--and possible alteration--of life-support drugs. Lastly, we could consider recompression chamber design changes, such as we were talking about before. Can the chamber or the treatment facility design be altered to improve casualty care where narcosis may be a factor.

There is really no data on this, except for some observations we have made in treating patients, particularly at 165 feet. In that case, one becomes extremely task oriented. In other words, you know to intubate someone or put in an i.v., but that becomes all you can focus on; that becomes the task. The patient may be really deteriorating, but you are so tuned in to trying to accomplish that one task that you can forget about everything else regarding patient management. On the outside looking in, you wonder why it is taking so long for your corpsman or your diving med tech to do what on the surface is a very simple problem. He accomplishes it very well at the surface. We sometimes get around this problem by doing things on the surface before recompressing, although that is not always possible. We frequently have to do procedures at depth.

I remember one very graphic situation, a classic mistake. A patient came in to the chamber intubated from a local emergency department, but they had not filled the cuff with water. So when he was recompressed, the cuff shrank and he extubated himself so he had to be reintubated at 165 feet. The first physician could not do it, and I got called in at 1:00 a.m. to intubate the person. People who routinely had no problems could not do it. There were some logistic problems, being in the chamber and down on the floor, and it was not like doing it in the emergency room. But the main point is that they really could not function as well as usual at that depth. Although there is no hard data on the subject, I believe experience tells us quite clearly that narcosis can be a problem during recompression treatments.

DISCUSSION FOLLOWING DR. KIZER:

Casualty treatment

Egstrom: One continuing problem is that typically the emergency procedures are the least well practiced, and most frequently are not reinforced in that particular environment. If you assume you are going to put a paramedic type in the chamber and he will be able to handle the problem, it may be a false assumption. It appears that these people should be given some type of acclimation if they are going to be called on to give medical support.

Kizer: Let me change my hat a bit. In a few months we hope to address the issue of state regulations about diving medical technicians and hyperbaric chamber operators; i.e., we hope to write state standards for what training these people need. Issues that have to be addressed are (1) what is their scope of practice; (2) how much overtraining do they need, given these sorts of considerations; and (3) what is their ongoing continuing education program.

Brauer: Is there any justification for associating these questions, which are obviously significant medical problems, with nitrogen narcosis? Or is their inclusion in our discussions merely going to perturb the advice we might give in the matter and take attention away from the physiological factors under consideration.

Kizer: It is my perception that they are very real nitrogen narcosis problems. Where you are very intoxicated at 165 feet, and even at 60 feet, there are performance decrements, particularly when you are called upon to make decisions in the chamber as to what you are going to do.

Pilmanis: I would like to make three points. First, at the Catalina Chamber we routinely maintain a second physician available by phone. We do not allow our physician to go in the chamber unless absolutely needed. Instead paramedical people are sent inside. Then, if the physician does go inside, we have another one standing by on the phone to verify decisions. For example, pronouncing someone dead is a rather serious decision to make at 165 feet. Secondly, another factor that complicates the situation is that the patient may be narcotized also. We have seen, for example, "paraplegic" patients giggling and having a great time. The patient examination process is compromised by this. You must look past the narcosis manifestations to see the symptoms of air embolism or decompression sickness. Third, in particular with the habitat program, the engineering planning was based on the worst case situation with respect to depth. Because the British Treatment Table 71 goes to 230 feet, we set that as our present limit.

Egstrom: Maybe I misunderstood, but did you say earlier that you made some compromises that are going to essentially isolate the habitat?

Pilmanis: Yes, the control is in the habitat. There will be five scientists and one staff technician. The staff technician will control the mission.

Egstrom: There will be surface monitoring?

Pilmanis: There will be monitoring of gases and shutoffs on the surface. There will probably be TV cameras, but the control will be in the habitat. We separate monitoring and control. The control in the commercial industry is on the surface. We do not have time to go into the reasons behind that.

Hamilton: To begin treatment, you need information. Consider a patient at, say, 230 feet. You first have to make a diagnosis. One of the other divers will have to perform an examination. Here is the problem--you cannot trust him to remember what he is saying. He has to be told from someone else, step by step, what to do. It could be by another person standing there with a checklist; it does not absolutely have to be from the surface, but you cannot depend on human memory either to produce the right procedure to begin with, or to remember the observations. This needs to be programmed into the plan.

Kizer: For example, one can sit there looking through the porthole, by microphone telling the medic would he please check something, and he is sitting there scratching his head saying I do not remember what that is. Yet at the surface he could tell you very easily.

Hamilton: Count on the fact that to get the information you have to tell the diver how to do it, and he has to tell you right then what he observes. He can see perfectly well, and he can hear, and he can make all the observations that you need. But do not count on him to remember, or for big judgements or integration. That has to be done by someone who is not narcotized.

Egstrom: Performance decrements on emergency procedures are far more important than the scientist getting the wrong count on his bugs. Your emergency procedures have to go way up on the priority list.

Kizer: I do not think you can rely on topside monitoring for this. If you have a situation where someone has a pneumothorax which is progressing to a tension pneumothorax, the attendant on the scene has to make that decision; does the patient need a chest tube or not?

Hamilton: We could take a lesson from aviation. The pilot in a fast-moving emergency is comparable to the narcotized diver. The pilot has a definite procedure to follow; he has a few things memorized, and the rest of the things he looks up. If he has a flameout on takeoff, he does not have time to look up very much in that circumstance. So he has a memorized set of procedures. He does not stop to think whether they are the right ones or not at that point. He just does a, b, c, and d, right in a row when in the stressful situation; he does not rationalize.

For diagnosing a pneumothorax, I strongly recommend you make sure the diver acting as a medic has somebody to talk to. We are not doing "remote medicine" here. Our crew is isolated in a way, but we have good communication, and we have good topside support. We have a medical team over here, 100 miles away, but available. We do not want to put the divers in the situation where they have to make these decisions, because it will not be done right.

Kizer: Do we need the physician or topside medical attendant on site, standing by on a moment's notice, or can he be in Santa Barbara?

Hamilton: The guy topside is a paramedic, somebody who knows--when not narcotized--how to do these things. He does not have to be a diving doctor, he just has to be able to do the things you expect.

Linaweaver: I can assure you that if an individual doing CPR has to do anything else, do not count on much. Hugh and I had a case where I was doing a CPR at 165 feet. That was all I could do. Hugh, topside, had the responsibility to stay stop or keep going. Only he could assess whether or not ventilation was being accomplished; I could not. The ability of the one doing CPR to assess and do multiple jobs is over. In our case, we had two paramedics and were trading off. The only time I could do anything else was when I was resting, and then I was not much good.

Pilmanis: Should heliox be provided for this particular instance?

Hamilton: I think it would be safe to use it for a reasonable period of time. Decompression is a problem, but it can be handled. You would not have any immediate counterdiffusion problems.

Pilmanis: There are some differences between a shore chamber and the habitat. You cannot just look in the port like you can at a chamber sitting in a building. This thing is 120 feet under water. The outside person can not just walk up and look in to see if you are doing something right or wrong. Yes, you can communicate, but remote communication has its limitations.

Biersner: I think it might behoove the industry to begin to bring consistency to this whole process. The Navy is now doing this on board nuclear submarines (which are isolated and cannot communicate), using computer-assisted medical diagnosis; eventually this process will be extended to treatment. On surface ships (where there is communication), diagnosis and treatment can be done remotely. A remote diagnosis and treatment capability could be developed for the diving industry, but first treatment and diagnosis data must be pooled from those who are working in the area and then integated to determine optimum protocols. In essence, some standardization has to be imposed on this process.

Linaweaver: That is a good point. We could start with communications, e.g., standard phraseology. We need a glossary of terms to be used uniformly and all mean the same thing. The air traffic controllers can talk to a guy from Chili or a guy from Korea coming into LAX because they use standard phraseology. Even if the transmission is only partially received, the individual recognizes what was said.

Kizer: Advances in offshore emergency medical services systems development in general are just now becoming realized. But they are not anywhere to be seen in the diving casualty treatment sphere. The systems development is directed at people on platforms; it includes communications, personnel, transportation and all the components in any systems development. Biersner's point is very well taken.

Biersner: A system such as I have described will provide the onsite physician with treatment options that may have been ignored in the stress of the moment.

Drug action under pressure

Kizer: I would like us to address the effectiveness and what dosage changes need to be made in life-support drugs in the hyperbaric environment. Dr. Kendig, are there changes in conductance and that sort of thing?

Kendig: I can refer you to what is probably the best source of relevant information, the UMS workshop on drugs under pressure held a few years ago (Walsh, 1980). There is a discussion by Mike Walsh on amphetamines, which one would predict to have an antinarcotic effect, if anything. Perhaps as a result of the complex interactions of stress, giving amphetamines to somebody in a stressful situation seemed to make the narcotic effect worse.

One can predict certain classes of drugs will potentiate narcosis, that is, those drugs that themselves (at sea level) usually have a sedative effect. That prediction will be based on the known additive efforts between one sedative drug and another. Nitrogen in hyperbaric pressure is sedative, so one would predict that any barbiturate is going to have an increased effect at depth. Any anesthetic, possibly even a local, might have a slightly enhanced effect.

Kizer: That is a very real concern. When you give Lidocaine for dysrhythmias under pressure, do you use standard doses, increased doses, or what?

Kendig: I suspect the interaction would be slight. The effect of nitrogen on either the cardiac conduction or the central nerve conduction is going to be slight at these modest pressures. The nitrogen interaction with the massive overdose of Lidocaine used for a nerve block is certainly not going to be effective. I think experiments would have to be done to sort out the antiarrhythmic effects. I do not think they have been done.

Kizer: For lack of better data, would you use standard dosages of your basic life support drugs in the hyperbaric treatment environment.

Kendig: That would certainly be the first thing to do.

Hamilton: You do not refrain from using the drug if the drug is needed.

Kizer: No problem with that. The question is, if you use the same dosage and do not get a response, does that mean you need to alter your dose?

Hamilton: Nicodemus at NMRI has worked out guidelines for anesthetic agents, has he not? Whether or not they are all exactly right, at least you have some idea of where to start. But this is not really our problem here.

Kizer: Most of the research has been on anesthetic agents, not your basic life support drugs.

Kendig: We have a better handle on those because of the known interaction between anesthetics and hyperbaric pressure and/or nitrogen narcosis. What categories are the life support drugs?

Kizer: Antiarrhythmic drugs such as adrenalin.

Brauer: I want to raise a question. You all know that even under normal circumstances, any use of a drug by a physician is in fact a pharmological experiment, hopefully tempered by a great deal of experience and therefore carried our with a measure of assurance. Under our conditions, the experimental character becomes a bit more obvious, and the use of the drug less avoidable. I wonder whether one could give the physician confronted with that situation some guidance about how to carry out that experiement most effectively with a minimum of risk to his patient. Perhaps one could come up with some thoughts on that, some advice as to how to proceed by watching the patient, how to change the dose if it does not get an effect, and so forth.

Linaweaver: In that regard, could we just make a general statement that drugs should be used with caution and that the drugs should be given in small, multiple doses rather than in single bolus form, until the patient's reaction is seen.

Brauer: Perhaps we could even go one step further, by rethinking the specific drugs we are concerned about, and making some "time" guidelines, including for example the frequency of successive doses. With some drugs you want to let a fair amount of time elapse. With epinephrin, obviously you do not want to allow very much time to elapse. Maybe one could come up with some guidelines that could be used until we have better data.

Chamber Design

Kizer: The last part of this was regarding recompression chamber design. I know that taking care of patients in the Catalina chamber is different from taking care of patients in the standard Navy or commercial double-lock, where you work on the floor or against the bulkhead and there is no room to maneuver. But could recommendations be made as to how the interior of chambers should be configured to optimize these sorts of management problems. Given the problems with narcosis and accomplishing simple motor tasks as well as integrating information, what should be the optimal design of the inside of the chamber.

Pilmanis: Minimizing the complexity of all tasks inside the chamber is the objective. For example, drawing on the current electronics technology for better and smaller equipment that can be worn by the inside personnel, freeing their hands. Fiber optics is another area which might be explored. There are definitely areas for equipment improvement in chambers; however, people tend to accept the most readily available equipment.

Brauer: What about lighting? In the systems I have seen, lighting is frequently the bugaboo. If you have a partially narcotized medic, then increasing the light intensity available in his work field may be one of the more effective things you can do to.

Carter: Let me offer a couple of thoughts about light. As you pointed out, the overriding constraint is cost. If you can afford an optimally designed chamber, great. But if something is available and it is that or nothing, we take the small, dim, chamber. I am somewhat pessimistic about optical conduits. The places I have seen them installed there was just not enough density of light to do reading comfortably, or medical jobs. I think lighting for diving chambers is a specialized problem that does not have a big constituency. The lighting companies are not out there fighting each other off to solve your problems, because there is not any money in it. And so I think it will take a special effort from within the diving community to design lighting systems for chambers.

Pilmanis: It can be done. Technologically, none of this is really a big problem. It is all cost.

Biersner: Environmental stressors, such as noise or thermal extremes, can be distracting and these distractions can particularly impair the performance of anybody who is narcotized and has limited mental resources available. These stressors should be reduced. Fatigue, too, is a stressor that can compete for these limited resources and impair performance. Care should be taken to ensure that adequate work-rest protocols are implemented to avoid fatigue. This should be true as well for the physician who is treating an injured diver under narcotic conditions.

Brauer: I think you have just brought up the point we have neglected and perhaps should not. I am under the impression that a given effort of supporting a patient carried out under partial narcosis entails an extraordinarily high cost in terms of fatigue, and that the fatigue problem needs to be addressed. We have not addressed it at all in our discussion. As I recall it, once the immediate task is over, the letdown is very steep, and the ability to restart becomes more and more compromised. That may be physiological or it may even have a pharmacologic element. I do not know of any work that specifically addresses the question of fatigue and light narcosis.

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GENERAL DISCUSSION
K. W. Kizer

Pilmanis: I would like to get back to the topic of narcosis accommodation during excursion diving from saturation. It is of major concern to our program; particularly, we need to know whether we will have enough time to accommodate. Perhaps there is not enough known, but I would be very concerned if this meeting ended without addressing it more extensively.

Brauer: We have addressed it a little bit in relation to the acclimation study. There are reports there that indicate that in excursion operations the ability of individuals to perform during fairly deep excursions increased as the prior time of sojourn at "effective" nitrogen levels increased. That represents one of the few things on which there seems to be a consensus, in what little literature there is. But we do not know exactly how effective "nitrogen time" is in improving the ability of divers to perform at greater depths in excursion situations. What data there is is based on typical nitrogen narcosis-type data, which means it is 100% episodic.

Egstrom: The point has been made that the linear function of narcosis is a myth, and there are some dose-response situations where we can anticipate that there is going to be a dramatically increased narcotic effect. In 100 foot excursions from the habitat--which could be 100 feet deep--you may be getting close to a place where the slope of the curve increases sharply. How much do we know about where the dose-response curve changes?

Brauer: There are two ways of looking at the acclimating effect. You either view it as subtracting effective depth from the depth to which you go, or you can do it factorially by saying that the effectiveness of narcosis will be decreased at each level. In the first case, if we are thinking of that depth-effects curve, the curve will have been flattened. We do not have that information, but I think it would be relatively easy to get it experimentally.

Kendig: On animals?

Brauer: Yes, exactly. I think that is transferrable.

Kizer: Note that as a possible priority for future research.

Pilmanis: I am familiar with the anecdotal information. It is difficult to base operations and long-term planning on anecdotal information. How solid is the evidence that accommodation is, in fact, taking place, and how extensive is that accommodation? Some of the statements in the "gray" literature have made some fairly sweeping comments, such as "you are essentially normal at 200+ feet if you have been saturated at 60." I find that a little hard to believe. That has tremendous impact on operational questions if it is indeed true.

Kizer: How good are those data and how much data are there?

Hamilton: "Not very," is the answer to both questions. Not very much data and not very good data. Most of the data is really subjective. It is not anecdotal, which implies third party; it is subjective, with the scientist standing there asking the diver how he feels. It is not from a barroom, but it is still subjective. I think one could make a distinction between "anecdotal" and "subjective".

Brauer: It is anecdotal in the sense that comparisons are being made based on recollection rather than on a quantitative basis. The tools that we have available for this measurement by and large do not lend themselves to the kind of qualitative comparison you need to get real answers. In order to get answers, I can conceive readily of animal experiments. But those can only look at much more advanced stages of anesthesia, and therefore do not address the question of the lesser stages that are relevant to diving. Still the question of whether this represents a displacement of the [dose-response] curve or the changing of its slope should be amenable to such an approach, and could give us significant guidelines. The two types of change carry very different implications.

Pilmanis: Perhaps one of the first areas of future research should be to define that curve.

Brauer: Define the nature of the displacement in the curve during excursions, as a result of acclimation. This acclimation is one of the concrete tools we have to relieve narcosis effects. The second one is much trickier. In addition to that acclimation displacement, there is the matter of experience--learning to cope, or accomodation, whatever you want to call it--that is associated with even brief exposure or "having been there." I think that is totally real, but I do not know how to assess it. That one you do not do by animal experiment.

Biersner: The data in the excursion dives are not all that poor, but many were with subjective tests. There have also been evoked potentials taken, both visual and somatosensory. There have been a number of performance tests. Hamilton and colleagues have looked at that problem. They had some procedural problems in administering the tests under those conditions, but I think the tests were as appropriate as any tests that have been administered to date. (Now that does not mean I am enthusiastic about any of the tests that have been done.) They are as appropriate as anything else that has been done on excursion dives. But the problem you run into are small n's, extremely small. Some of the n's dropped down to two or three.

Hamilton: We never had large numbers in any of our experiments.

Biersner: Small sample sizes make for unreliable data. Unreliability becomes an even more serious problem because of variations in the diving protocol--such as with the NISAT dives when the oxygen partial pressure was increased and performance improved. Was this improvement due to the increased oxygen, or to acclimation.

Hamilton: That happened in NISAT, and also there was a big dip in performance on the third and fourth day. Nisahex saw none of that. We do not think that dip was typical, we think it was unique to NISAT. And I personally am not satisfied that we know the cause.

Biersner: I think the data are stronger showing that acclimation or accommodation is occurring during saturation-excursion dives. Even Moeller will acknowledge that acclimation/accommodation may be occurring under these conditions.

Hamilton: We had to make a lot of compromises with Moeller to get him to agree.

Egstrom: I think that we can agree that for Andy's functional mission, one of the early objectives will have to be to establish tolerance tables for subsequent missions. It is not likely that you can get the information you need from data we have been able to identify.

Pilmanis: That is a valuable statement.

Brauer: Assuming you get that information during your first few dives, how transferrable is it to the next situation? A second question, is it possible to rate or rank people with regard to their susceptibility to the pharmacologic effects of nitrogen. I have no information on whether that rating, when obtained on 13 November also holds on December 15? That should be one of the early things to establish. In Catalina's case, where they cannot eliminate everyone who is sensitive, they would still be well served if somebody could tell them, "we have looked at these people and Joe is highly susceptible to the pharmological effects of anesthesia and Bill is highly resistant." They could then structure their missions to take that into account. I personally have a suspicion that when that information is in, it will turn out to be a reasonably stable characteristic. Certainly it seemed to be so in the series of hydrogen exposures I just saw at Comex. There we had six subjects, and right after the first relatively shallow exposure did a rating of the subjects. The rating held beautifully, with our most susceptible subject collapsing at 240 meters unconscious, and our least susceptible subject coming through with flying colors and performing.

Hamilton: How did you make your assessment?

Brauer: From interviews. We gave them sheets with basically two questions, (a) How did you feel, are you narcotized or not? and, (b) Can you compare it?

Brauer: Our assessment was based on the way they described their comparisons.

Hamilton: This workshop has decided that exposure to one narcotic agent will help condition for another. Certainly nitrous oxide is used as a model for narcosis. Would it be helpful in training? Should we recommend that the divers go through routines while breathing a nitrous oxide mix?

Biersner: As you may know from my research, I am an enthusiastic endorser of using nitrous oxide to simulate nitrogen narcosis. However, I interpret narcosis in terms of an environmental stressor--similar to the stress induced by heat or cold--and physiological strategies can be adopted that will reduce the stress effects. Some colleagues and I have tried to address this issue.

First, we did some animal work to determine if narcosis is indeed a stressor. The technique used was called "conditional taste avoidance." Using this technique, thirsty rats are given a choice between saccharin and water solutions. A rat will normally drink the saccharin solution when given this choice. After establishing a stable saccharin response, nitrous oxide was added (using a chamber especially built for this purpose). The rats then began to drink the water solution they normally least preferred of the two solutions. This effect indicates that nitrous oxide was a stressor. We then pre-exposed the rats to nitrous oxide without associating (pairing) the gas with either saccharin or water. When the nitrous oxide was then paired with the saccharin response later, the rats did not avoid the saccharin solution. These results appear to show that the rats learned to accommodate to the stressor.

A similar procedure was then followed using human subjects, using the protocol developed by George Moeller. The subjects practiced several self-paced tasks for three days on mormobaric air. On the fourth day half of the subjects breathed nitrous oxide and the other half breathed normal air. On the sixth day, both groups were pressurized on air to 185 feet of sea water. The assumption was that the group that breathed nitrous oxide would have accommodated to the stress effects of narcosis, and their performance at depth would not be as impaired as the group that breathed normal air. However, the performance of the two groups was impaired to nearly the same extent. The two groups performed virtually at the same levels.

Hamilton: I would quibble with the design: it could be you did not give them enough time to accommodate.

Biersner: I do not think we gave them a potent enough dose of nitrous oxide, or we should have explored them for longer periods or more repeatedly.

Hamilton: It might work to compare that with someone who had not been through this 5-day protocol on air, getting rid of the stress part of it.

Biersner: Using self-reported estimates of their emotional states, we found that the divers who had breathed nitrous oxide were significantly more disturbed emotionally prior to being exposed to hyperbaric air that those divers who had breathed only air. This suggests that pre-exposure to nitrous oxide may have added to the stressful effects of nitrogen narcosis.

Hamilton: Thus compensating for any possible accommodation. Did they know they were being exposed to nitrous oxide or was it "blind?"

Biersner: The experiment used a single blind procedure--the subjects did not know what gas they were breathing. However, I was one of the subjects, and I knew what I was breathing nitrous oxide.

Brauer: How much nitrous oxide did you use?

Biersner: It was a 25% mixture. This mixture induces a mild narcotic effect. I think we needed at least two more exposures. I think what we did in one exposure was shock them and make them feel they could not cope. That is what transferred to the subsequent condition. One exposure to nitrous oxide was sufficient to be stressful, but it was not enough for the subjects to learn to accommodate to stress.

Hamilton: What about the idea of using nitrous oxide to try to preaccommodate?

Pilmanis: That would be difficult on a population coming from all walks of life, from all over the world, even though they all will be certified divers.

Kendig: I would urge that if it is going to be done at all, that it be done under your control, under standard conditions, and with results clearly noted. If candidates have to do this at their own institution, as a precondition of coming, chances are you may lose people just from the test.

Kizer: But if you do it at your institution it will require more time.

Pilmanis: Also, it would be after the fact; they would already be there. They have spent the money, time, and effort, and now you might wash them out on a test.

Brauer: If you do that (a) it should be done in the swimming pool, and (b) it should be done with your subjects wearing helmets, because a certain proportion of them are going to experience vestibular disturbances.

Pilmanis: What advantage does that have over just putting them in the chamber and pumping them down.

Hamilton: That is not the "state" that you are trying to accommodate to.

Brauer: That simulation of depth narcosis with nitrous oxide can be done easily, you can get your exposure under conditions where half of your divers should still be in perfectly good shape, and where the physical environment will be simulated more closely than you can do in the laboratory. This would not be a bad test situation as well.

[Here followed a discussion of the conditions of the early exposures with the conclusion that early missions not over at 70 or 75 fsw saturation with excursions to 130 fsw would be relatively benign and would provide a considerable experience base to use in planning deeper missions. This depth on nitrox is equivalent to 85 fsw breathing air.]

Hamilton: Thus it is a non-problem in the first habitat location and for the first missions. But you will want to excurse to 200-250 feet, so you have to work toward getting ready for that.

Pilmanis: You are talking about the safety issue. What about the other aspect, of accurate data collection?

Hamilton: Justifying whether data are valid still needs to be dealt with. But it is not nearly as serious in the range shallower than 100 feet as it is going to be at twice that depth.

Pilmanis: I think what we are hearing is that there has to be an in-house ongoing narcosis research program piggy-backed on the mission. This seems not only possible, it is imperative. Incidentally, this has always been included as part of the program planning.

Biersner: You should plan for the experiments and do as much baseline testing as you can before the divers are exposed to the narcotic conditions.

Pilmanis: We must identify the mission tasks and training on the tasks, and be specific about what they are going to do. Then we can get a handle on how much those tasks are affected by narcosis.

Hamilton: You are wasting time and money if you just send them down and let them make all their decisions. They will not be able to think. They have got to have what they want to do pre-planned, even at 80 feet.

Pilmanis: In the Hydro-Lab operation nobody dictates to them at all in any way. You can not tell a scientist how to do his work.

Hamilton: They do not have to be dictated to, they have to have good plans.

Kizer: Your RFP has to specify what sorts of tasks are appropriate; maybe the next year you can include other things as you get more experience. It has to be structured from the outset.

Hamilton: That is true whether or not narcosis exists. As I understand it, this workshop is saying that rigorous planning will be a requirement, even though the precedence for this approach is minimal in the scientific diving community. We want to break from the Hydro-Lab pattern as you have described it.

Brauer: I would like to register what I perceive to be a dissenting vote from what appears to be considered appropriate diving practice for scientific divers. The industrial diver works in a jungle of structural elements under conditions of poor visibility, and may be guided by hand and foot during the time he is underwater. On the other hand, one of the major reasons for putting scientists in the water is to get the benefit of their trained gifts of observation and interpretation. In biological and also geological work many of the important observations are spontaneaous and require prompt and intelligent responses from an observant investigator free to make ad hoc decisions. Thus, I would judge that any investigator worth putting into the habitat at all should be in a position where he is able to function as a true investigator with a high level of spontaneity. If he cannot do that he should not be there. It seems to me that this concept is basic; it seems also that this concept is built into the operating pattern in the minds of the organizers of this facility.

The specific question before us is to what extent the applicability of this concept can be jeopardized by nitrogen narcosis, and if it is, what advice can we give to miminize risk while retaining operational credibility.

I suggest that operating rule number one should be that whenever it becomes truly necessary to strip the diving scientist of his spontaneity and direct him from the surface, that particular dive should be considered as probably inadmissable and should be reviewed to determine whether it can really be justified. If a task requiring scientist intervention is at hand but the task or the operating conditions are deemed beyond reach of personnel who retain their competence, I think the program should consider substitution of diving techniques other than air.

Hamilton: Let me clarify what I said. I tried to make two points. First, if scientists are to use this habitat, they have to have a plan. When you went down to do the work you mentioned, Ralph, you knew what you were going to do when you got there. You had thought it through, you knew the effect of narcosis, and you had mentally prepared yourself to deal with it. Many of our scientists will not have your degree of experience, so planning becomes all the more important.

Second, when an operation gets off the plan we want topside to be available to help and advise. We also want the people to be trained in the exceptional activities as well as the routine.

Kizer: What I am hearing is that at first there needs to be some structure, at least until experience reveals the limits of data validity and that sort of thing.

Shilling: When you are planning you are in a way like a professor in a university. The scientists in a new setting are like graduate students, and you help them plan their total effort.

Pilmanis: We cannot specify to the investigators what they are to do.

Shilling: But you ought to know whether or not it can be done under water.

Hamilton: As a professor you are not going to let a graduate student flail around in the lab for 3 years and end up with nothing.

Pilmanis: This will be a national facility that costs taxpayers a lot of money, and it should not be used for something that can be done out of a skiff. That was an assumption from the beginning. Our question is, to what extent or in what manner can we assure that someone working from the habitat has accurate data. What type of research needs to be done to get a handle on that?

Hamilton: This is an operational situation. The problem prevails even if you do not have narcosis. You still have the basic problem of gathering data according to the scientific method, but under additional stress and with limited resources (visibility, etc.) even if you do not have the narcosis. The better planning you do, the better your results will be.

Greer: Did we not address this yesterday? We concluded that some kind of ongoing test of effectiveness that could begin before the mission and during a mission would be desirable.

Kizer: We did not specify what that test would be. We agreed that video games may be a good tool, but to make it happen properly someone will have to address that with a proposal in the usual research paradigm.

Kizer: Further, I felt we agreed that nitrogen narcosis is not just a matter of academic interest, but is a matter of considerable operational significance to both scientific and commercial operations.

Egstrom: There is a great deal of variability, depending on what other stressors are common with the narcosis.

Kizer: Let me try to summarize our comments. The simple statement that narcosis is something that needs to be dealt with, is important. We can probably simplify it to say that the current major significance of nitrogen narcosis for commercial diving is that it provides a depth limit for what can be done with air. The main impact is economic. In the scientific sphere, we also have limits from the safety point of view, and there is question of validation of their ability to do the work. With commercial and military diving we are mostly concerned with performance, the specific outcome of the work, whereas with scientific diving we have information handling and integration phenomena as well as outcome. But there is a fundamental difference there.

One of the straightforward things that we have done is to come to a consensus of the proper terms to describe this phenomenon of progressive time-dependent improvement in performance while exposed to hyperbaric nitrogen. Accommodation is learning to cope; acclimation is the more specific term, describing accommodation associated with or being due to demonstrable biochemical and/or physiologic changes. Adaptation, the word most commonly used, is more properly reserved for the multi-generation genetic process. The data on accommodation is largely anecdotal, but I must admit my perception of this phenomenon is greatly expanded over what it was 2 days ago. There is no doubt that further studies are needed.

Nitrogen narcosis is one of several stressors in diving, along with anxiety and hypothermia and other things, that combine to impair performance under water. Studies are needed to separate out the specific effect of nitrogen narcosis from the other concomitant effects of anxiety, cold, etc. The literature on the effect of nitrogen narcosis suffers in reliability because it does not distinguish the particular effects of nitrogen narcosis from other dive-related stressors. It could be said to be largely uncontrolled, with non-uniformity of study techniques or paradigms and non-uniformity of outcome measurement; there are a host of other experimental design problems. In essence the existing literature provides an insufficient data base to answer the questions about observational reliability in scientific diving. We need to know more about the benefits of state-dependent learning.

We could also say that nitrogen narcosis is a potentially useful tool to analyze brain behavior, although that would not have much bearing on operations. Hyperbaric nitrogen has effects similar to other general anesthetics, but its precise cellular and axonal effects are poorly understood. It may be a blockage of cellular channels.

Kendig: There are two major directions in research. In the near term one of them is going to lead to delightful, clean, exciting results. There is a good probability that within a year or two the major ion channels in nerve cell function that are appropriate to anesthesia, the sodium channel and one or more of the types of potassium channel, will be biochemically characterized, isolated, purified, and reinserted into membranes. The old question of whether it is the lipid bilayer or some portion of the protein itself that is controlling is going to be settled.

The second is the more thorny question which will be with us for a long time, that is the question of what is anesthesia and what is narcosis at the CNS level. That one will be more resistant.

Kizer: The future studies on nitrogen narcosis, especially with regard to scientific diving, must lookk at each of the three specific components of performance, the perception phase, the intermediate information processing or conceptual programming phase, and then the ouput or execution phase.

We mentioned that video games offer considerable promise in a study for the managment ot narcosis. However, they need to be carefully selected, and ideally they should be specifically designed for what we want to assess.

Biersner: I think a major advantage of video games is that they eliminate motivation as a variable. They hold motivation constant, allowing a reduced motivational effect on the data. That is, everybody can be highly motivated. The work of Lambertsen with highly motivated college students, showed little performance impairment on a number of tasks.

Kizer: I think we agreed that current data do not allow one to predict reliably which individuals will be particularly affected by nitrogen narcosis, and offer little basis for establishing personnel selection criteria.

Nitrogen narcosis by itself seems not to be a significant cause of sport diving accidents, but it probably plays a contributing role in many cases. This should not be confined to sport diving, but diving accidents in general. At this point, it is a contributing factor, but significance as an isolated factor is not defined.

[The balance of the discussion dealt with the mechanics of producing this report.]

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UNIQUE DESIGN OF THE NEW NOAA/USC SATURATION DIVING SYSTEM

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Abstract

Under a cooperative agreement, NOAA and the University of Southern California (USC) have designed, and are currently constructing an underwater habitat/saturation diving system as a national facility to provide extended time and depth capability to marine scientists. It is expected to be in operation in early 1986, and to be initially located at the USC Catalina Marine Science Center on Santa Catalina Island, 26 miles offshore from Los Angeles, CA.

The state-of-the-art design of this system will enable scientists to perform in situ research in depths of 50 to 200 FSW in relative comfort, with up-to-date equipment, and with on-campus academic support.

1. Introduction

Marine researchers using scuba from the surface quickly become aware of the limitations of this mode of diving. Generally, the nature of scientific diving is such that extensive bottom times are required. The primary scuba limitations restricting the bottom time are: (1) limited breathing gas, (2) in-water decompression requirements and (3) cold exposure. The vast majority of past scientific research projects have been done in less than 40 feet of water. The primary reason given for the use of this relatively shallow depth has been the restricted bottom times associated with deeper diving.

The concept of saturation diving was developed by Captain George Bond, USN, in the 1950's and has since then been applied in the military, the commercial diving industry and in scientific diving.

Since the early 1960's there have been built a number of habitats for specific use by scientists. The most prominent include Tektite, Helgoland, La Chalupa, Aegir, and Hydrolab. If the measure for success is length of time in operation and number of scientists saturated, then Hydrolab is in a class by itself. In over 10 years of operation the program has saturated over 500 scientists, and is currently the only operational habitat for scientific use. The success of Hydrolab rests, to a great extent, in its relatively low cost and simplicity of operation.

Today, as a mode of support for marine research, habitat saturation diving has become an accepted approach.

In 1980 a new national habitat saturation diving program was initiated: The NOAA National Undersea Research Program at the University of Southern California NOAA has a legislative mandate to establish programs for the assessment, protection, development, and utilizatoin of U.S. coastal zone resources. NOAA's Office of Undersea Research has established the National Undersea Research Program to address these problems through the use of such state-of-the-art technology as manned and unmanned submersibles, saturation diving, surface-supplied umbilical diving, and conventional scuba. As a part of the National Program, the Institute for Marine and Coastal Studies of the University of Southern California has designed a movable habitat-based saturation diving system to be initially located at the Catalina Marine Science Center, 26 miles off the coast of southern California on Santa Catalina Island. Construction of the habitat is due to begin in 1984, with the first scienctific mission scheduled for 1986. One of five programs within NOAA'S National Undersea Research Program, the NUR/USC project is designed to support scientific studies at depths of 40-130 FSW, using saturation diving techniques. This will be the first such system to routinely support undersea research in temperate waters, and the first time that this scientific technology will be available on a university campus, enabling aquanaut-scientists to work in an academic atmosphere under an established, on-going, fully operational marine science program.

2. The NUR/USC Habitat/Saturation System

The underwater laboratory system will consist of the habitat itself, a ballasted baseplate secured to the ocean bottom, a life support barge (LSB) moored over the site, a personnel transfer capsule (PTC), satellite "Way Stations" (similar to wet bells) for emergency shelter and to extend excursion range, and a deck decompression chamber (DDC). The habitat will be a 9 ft diameter double-lock chamber, 12 ft wide, 16 1/2 ft high, and 40 ft long. It will be always positively bouyant and will be winched down and secured to a ballasted baseplate. It will also have the capability of being surface-towed to new locations.

The habitat is divided into 3 compartments (Figure 1): (1) the "wet porch" (not a pressure vessel), (2) the enterance lock, and (3) the main lock. The wet porch will be the diver entry and exit area, always open to ambient water pressure. This compartment will contain the diving gear, the diver rewarming tub, the umbilical hookups and a sample sorting tray. The lock will serve as the hygiene and changing area. It will contain the toilet, shower, environmental control unit, a control panel and a "wet" lab area. The main chamber will be a dry and comfortable living and work area in which most of the non-diving time will be spent. It will contain the galley, the sleeping area, the primary lab area, the computer, a control panel, 2 large observation ports and 2 environmental control units.

The habitat will have the capability of decompressing divers either on the bottom or on the surface. Two rectangular doors will be used for both the main chamber and the lock. The internal pressure rating of the habitat will be 230 FSW and the external pressure rating will be 120 FSW. Normal operating storage (saturation) depth will be between 50 and 120 FSW. Excursion diving (using scuba or tether) from the habitat will initially be limited to a depth of 130 FSW. With demonstrated safety and scientific need, this depth limit may be extended in the future.

The horizontal excursion range will be extended by the use of "Way Stations" (Figure 2). These strategically placed support refuges will be supplied by umbilicals from the habitat with gas, power, communication, and hot water. The habitat is designed to house 6 aquanauts: 5 scientists and one staff technician. An anticipated average saturation duration is 7 to 10 days. However, longer times will be possible if scientific need exists.

The habitat interior will have environmental control to create a comfortable "shirt-sleeve" condition. The habitat breathing gas will be nitrox (a N O mixture with oxygen partial pressure between .21 and .50). The excursion diving will be on air. Nitrox is used at saturation depths below 50 FSW to reduce the possibility of pulmonary oxygen toxicity from the long exposures. The reduced oxygen atmosphere also essentially eliminates fire hazard during saturation. The exterior of the habitat will have thermal insulation and the interior will be maintained at a comfortable temperature and humidity. There will also be a diver re-warming tub in the wet porch.

The LSB will be moored above the habitat and will supply electrical power and life support functions to the habitat via umbilical connections. There be an operational monitoring capability on the barge. Fresh water, waste disposal and electronic connections will exist from the shore to the barge when possible. The deck chamber and handling system for the PTC will be mounted on the barge.

The PTC will normally be positioned adjacent to the habitat on the ocean bottom. It will be used for (1) emergency pressurized evacuation of aquanauts to the surface chamber and (2) decompression of staff divers. When used for these functions, it will be raised to the surface and mated to the deck chamber on the LSB.

During the 2 year habitat fabrication phase, the NUR/USC "Interim" Program is conducting undersea research in the same areas and depths that will be available to scientists when the habitat is completed. This interim program utilizes tethered diving and a portable Way Station (Figure 2). This equipment significantly extends the operational capabilities of the surface-based scientist/diver. The Way Station is a dry refuge supplied by umbilical from a support vessel. This support includes low pressure and high pressure air, power, communication, and hot water. The Way Station is positively bouyant and is attached to a clump weight by a cable. Is is winched toward the surface by the diver for oxygen decompression. The use of oxygen greatly reduces decompression times. In addition, the decompressing divers are out of the water (except for their lower legs) and reasonably comfortable. The use of the Way Station allows access to depths in the 60 to 130 FSW range with bottom times as long as 3 to 4 hours. The hot water reduces the diver's thermal problems in these temperate waters. The communications permits expanded data acquisiton capabilities.

3. Summary

The new NOAA/USC National Undersea Research Program is constructing a habitat saturation system. The purpose of this system is to provide to marine rearchers with depth and time capabilities much greater than the use of surface oriented diving. The system design has benefited from the experience of past saturation systems in the military, the commercial diving industry, and other scientific habitats. This aspect has been blended with new technology to produce what will be a sophisticated yet cost-effective, diving system.

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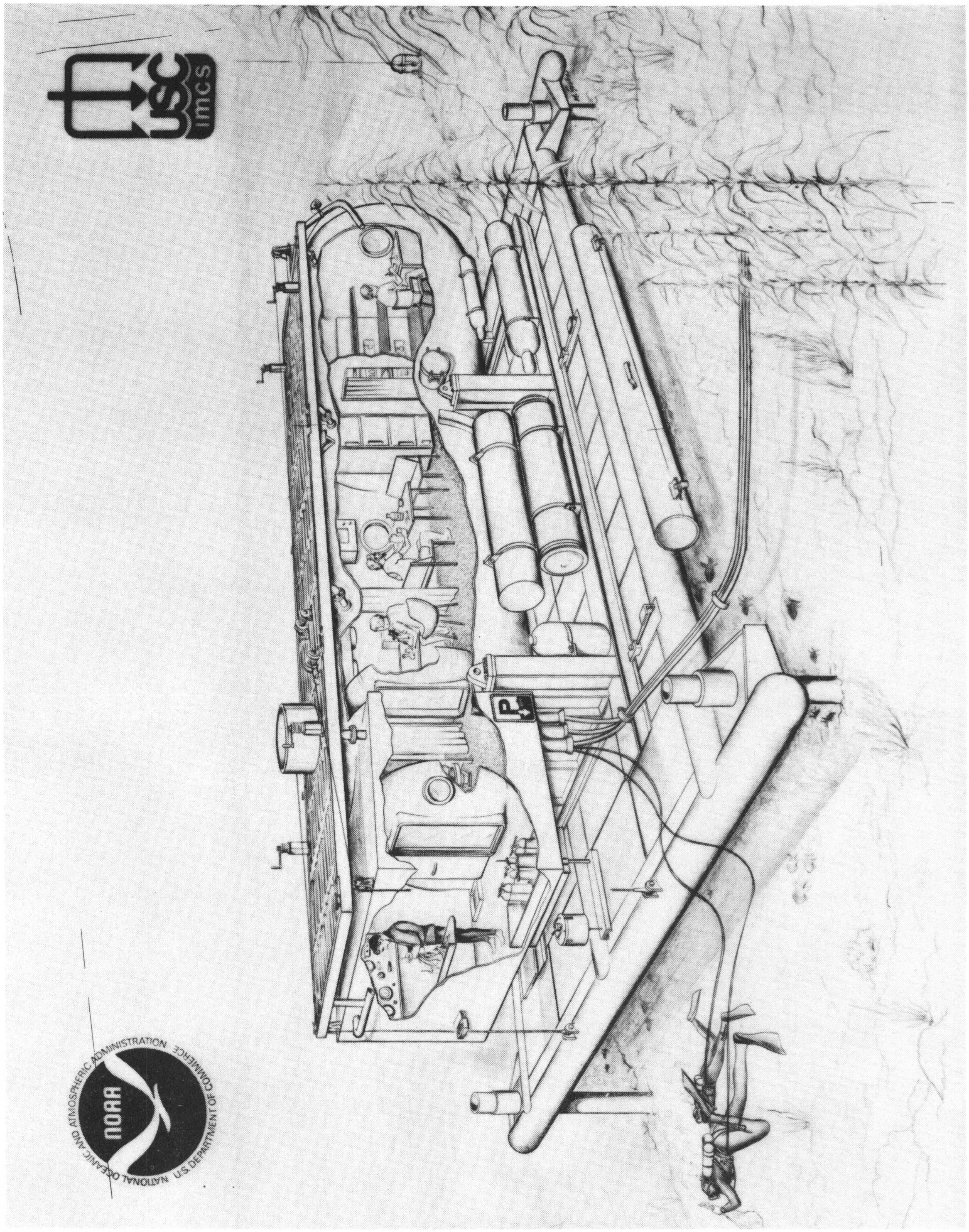
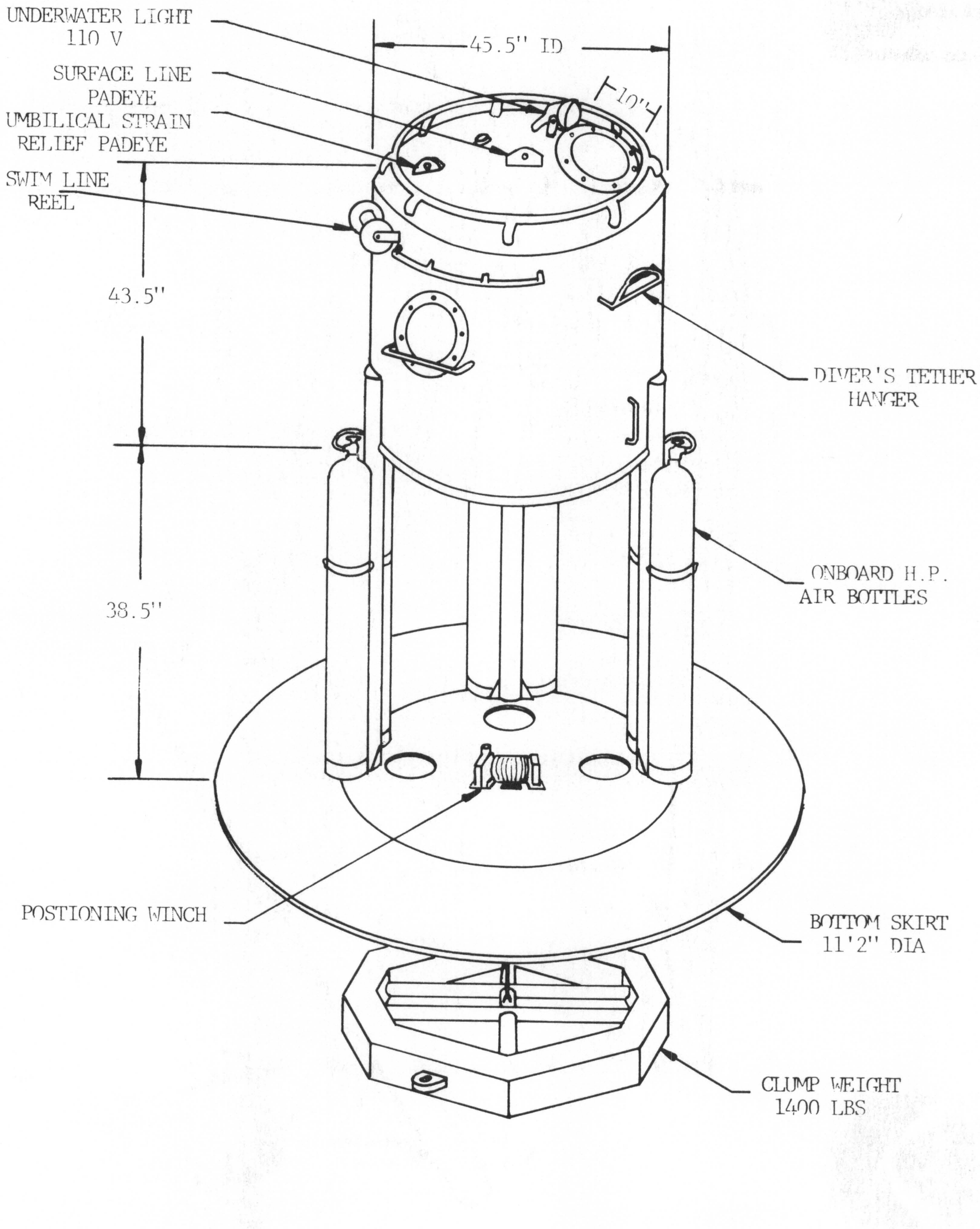
This is contribution number 89 from the Catalina Marine Science Center.


USC
imcs
NOAA
NATIONAL OCEANIC AND ATMOSPHERIC ADMINISTRATION
U.S. DEPARTMENT OF COMMERCE


UNDERWATER LIGHT
110 V
SURFACE LINE
PADEYE
UMBILICAL STRAIN
RELIEF PADEYE
SWIM LINE
REEL
45.5" ID
10"
43.5"
38.5"
DIVER'S TETHER
HANGER
ONBOARD H.P.
AIR BOTTLES
POSTIONING WINCH
BOTTOM SKIRT
11'2" DIA
CLUMP WEIGHT
1400 LBS