Report Number WS: 2-28-76

THE NINTH UNDERSEA
MEDICAL SOCIETY WORKSHOP

# DEVELOPMENT OF DECOMPRESSION PROCEDURES FOR DEPTHS IN EXCESS OF 400 FEET

21-23 February 1975

Sponsored By

**F. G. HALL ENVIRONMENTAL LABORATORY,
DUKE UNIVERSITY MEDICAL CENTER**

*Workshop Chairman:* H. R. Schreiner

*Workshop Coordinator:* R. W. Hamilton, Jr.

*Edited by:* R. W. Hamilton, Jr.

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Edited by

R. W. Hamilton, Jr.

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Held at:

Wheeler Industries
Washington, D.C.
21-23 February 1975

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Undersea Medical Society, Inc.
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TABLE OF CONTENTS

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# FOREWORD

For a variety of reasons it could be said that modern diving began in the early 1960's. Among the achievements of the new technology are diving bells, saturation, light gear, unscramblers, hot water heating, and a dramatic increase in attainable depths. During the 60's commercial diving requirements for greater depths outdistanced the steady progress of military diving, and nowhere was this more apparent than in the development of techniques for decompression. By a combination of organized laboratory programs and trial-and-error modifications, the companies found a way to provide services at continental-shelf depths. The entrepreneurial nature of the diving business -- and keen competition -- helped to keep decompression procedures proprietary and confidential. But now, as the industry matures and its explosive growth slows down, greater attention is being paid to such matters as safety and efficiency. The various companies are communicating with each other, and beginning to work together to solve their common problems.

This Workshop reflects that cooperative attitude, and though proprietary barriers have by no means disappeared, some forward steps have been taken. It has been a great pleasure to be involved with this group and its work in this new spirit. I hope that it will continue.

A word about the editorial policy. The material has been assembled from the recorded transcripts, authors' manuscripts and notes, and my own notes; only a few contributors presented finished manuscripts. There are some omissions and no doubt some errors: for these I assume the sole responsibility. Housekeeping details were removed or shortened, as were some generally inconclusive comments and discussions. A few comments were lost in the transcription process, and not all of the slides presented were printed. I believe, however, that this report contains the substance of the entire Workshop.

The Workshop developed from a timely congruence of a requirement for technical data and a not-yet-completed contract. It was requested and supported by Dr. Peter B. Bennett of Duke University, using funds from the Harbor Branch Foundation. It was conducted by the Undersea Medical Society at the facilities of Wheeler Industries.

The Editor thanks the participants in the Workshop who made it a success; the sponsors; Dr. Heinz R. Schreiner for his strong chairmanship; Dr. Dennis Walder, President of UMS, Dr. C. W. Shilling, Executive Secretary of UMS, and the staff at UMS headquarters, particularly Marthe Beckett, who did the copy editing, and Claire Randlett, who supervised the preparation of final copy; E. Joseph Wheeler and his staff, particularly Patsy Jackson; Kathy and Sally at 80 Grove Street in Tarrytown, who suffered; and most of all, to all of those who came so far and contributed so much, and their institutions.

R. W. Hamilton, Jr.

February 1976

# SESSION I: INTRODUCTION

# I. INTRODUCTION

## A. PREAMBLE*

Dr. Bennett welcomed the group and said he considered that the time is ripe for a meeting of this sort. It's time for people from around the world "to come and put our views together and see if we can get a consensus of opinion" on how to approach the problem of decompression in the years ahead. He told how his laboratory had a contract with Harbor Branch Foundation to "solve the problems of decompression from depths beyond 400 feet," beginning with the 400-600 foot range. Funds from the program supported this Workshop.

Following dinner, Capt. Bornmann spoke of the U.S. Navy's programs of development in diving medicine and biomedical research in diving. "We want to increase the safety and the effectiveness of diving and diving operations at any depth." He emphasized that the overwhelming proportion of U.S. Navy dives were shallow. He also spoke of the Navy's facilities for diving research, with special emphasis on the Ocean Simulation Laboratory in Panama City, Florida, which is the new home of the Experimental Diving Unit, and on the new Environmental Health Effects Laboratory being built at the National Naval Medical Center in Bethesda, Maryland.

The other speaker was Dr. Alan H. Purdy of the National Institute for Occupational Safety and Health, who told of NIOSH's charter and responsibility for the safety and health of the working man in the United States. He explained the relatively low priority which NIOSH has given to diving by pointing out that there are many more men working in factories and coal mines than diving in the sea. But progress is being made, because the country has become concerned about undersea energy--gas and oil. A program has been started dealing with immediate applied research, standards, testing and certification of equipment, and even basic research. A provisional budget has been allocated to support diving research. This was handled through a special Task Force, which has been formed and is functioning. There is evidence that substantial funding may become available, and that the Task Force plans to ask the Undersea Medical Society, Inc., to do some future planning for us.

The meeting ended on a happy note, with Dr. Behnke speaking of the need for research, and hope for the growth of the Undersea Medical Society's influence.

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*This preamble is a summary of the introductory meeting following dinner at the Cosmos Club the night before the Workshop.

## B. WELCOME: H. R. SCHREINER

The fact that we are here together represents a milestone, in the sense that I have never, in the ten years or so that I have been associated with this field, seen such a gathering. Although I know each one of you and have worked with many of you, to have you all here from so many different parts of the world is something of an accomplishment. It is a tribute to you, because you would not be here if you did not share with me the feeling that the time has come to lay the cards on the table with respect to the safety and effectiveness of decompressing human beings, anywhere in the world.

I would like to urge you to speak out. One of the unmentioned prices of admission to this Workshop is the requirement that all of you open up. The time has come when information regarding the safety of procedures that affect human lives cannot be regarded as proprietary, cannot be hidden under the guise of company secrecy, but has to be made part of the public record. I urge that we make a signal contribution to that record.

## C. INTRODUCTION: P. B. BENNETT

With the energy crisis has come a rapid growth in deep diving. All has been far from well in the past in this area. With the navies of the world primarily interested in saturation diving, work on advanced diving decompression has been very limited. Yet this method is in great use by most commercial companies.

Unfortunately, research in decompression from bounce dives in the past has mainly been carried out by a few commercial companies who have kept their results secret. Since the research money invested was often small, few dives were made to test tables, and often such experiments were made in dry conditions, and without work. Other companies have at times purchased decompression tables which had not been adequately tested.

Decompression as a problem has been with us over 100 years, and requires the best scientific efforts possible for a solution. The best science can only be done in the open, where one's peers have an opportunity to study the data and the results and where ready exchange of views is possible. Further, the time taken to test and develop decompression tables is formidable.

In 1974, Duke University Medical Center, with a grant from Harbor Branch Foundation and using the divers of Oceaneering International and International Underwater Contractors, has taken steps to improve this situation. In our program we performed 89 3- to 4-man simulated wet working dives to 500 feet for 30 minutes. We feel we now have a good table for that dive.

Development of similar tables for other depths and times should not take so long, but time is still a vital factor. There is an urgent need for such tables offshore. It is my view that no table should be published without at least a minimal series of some 10 or 12 wet, working dives.

Release of untested computer extrapolations as a book of tables can only be regarded as very dangerous indeed, and could well result in loss of life. If all tables used in the field have to be tested--and this is now going to be enforced by law in the North Sea--then there is an awful lot of work for all of us to do.

I hope that this meeting will break the decompression cocoon and let in some air. Secrecy and science do not sit well together, especially in the health field. Our interests and certainly those of the clinicians and scientists in this room should be in the health and safety of the divers. We should think very carefully indeed before bypassing competent research, which can only result in the divers themselves being used as soldiers in the battle for oil.

## E. PRESENT STATE OF THE ART OF DECOMPRESSION RESEARCH: H. V. HEMPLEMAN

At the outset it is necessary to define the nature of the decompression problem and then to discuss attempts at solving it. There would be general agreement with the statement that the practical objective of decompression theory is to achieve the minimum time in decompression from a given exposure consistent with maintenance of a healthy state, both during the decompression and subsequently. The nature of the decompression sickness that is being avoided at depths somewhat less than 300 feet is reasonably well known, and all the various forms of decompression sickness have been well described, classified, and treatments for them discussed at great length over many years. Roughly speaking, there are three manifestations of decompression sickness, skin itches and rashes, joint pains or the bends, and central nervous system involvement generally resulting in paralysis of the lower limbs. There are, of course, other manifestations of decompression sickness, e.g., chokes and such major catastrophes, but these are only seen very rarely in quite unfortunate circumstances. On most decompression theories the boundary conditions of the problem are established by the appearance or non-appearance of mild attacks of the bends, the assumption being--an assumption, incidentally, well justified--that if one can avoid attacks of mild bends, then all other forms of decompression sickness become a rarity. It is necessary at this point to introduce a note of caution about this assumption, because Hills has shown that certain pressure-time courses predispose some animals, in his case goats, to exhibit paralysis, rather than mild bends, as the first presenting sign of decompression sickness.

Nevertheless, if one is employing conventional types of decompression pressure-time profiles, the statement remains true that avoiding mild bends will avoid virtually all other forms, certainly all other serious forms, of decompression sickness. Any serious forms of decompression sickness which do occur on rare occasions are generally due to mechanical events, such as holding one's breath during the decompression and causing burst lung.

However, this decompression review is concerned with diving deeper than 400 feet, and undoubtedly at this order of depth the first presenting sign of decompression sickness is not, for most of the dive durations being contemplated, the straightforward well-recognized limb bend pain. Over many years of experimentation at RNPL at depths in excess of 400 feet, it has become clear that unless one is concerned with short bottom times, e.g., 10-20 minutes, the first presenting sign of having exceeded the acceptable decompression limits is some form of involvement of the eighth nerve, i.e., there is partial or complete deafness of one or both ears or what has commonly been termed the "vestibular bends" or, indeed, combinations of these two ill-effects. For convenience in the subsequent discussion, therefore, I shall refer to eighth nerve disturbances as END.

Now it will be necessary to establish that we are dealing with a new end point for decompression sickness problems and it will therefore be necessary to present supporting evidence. You must realize that I am talking about decompression sickness here that comes on deep, not decompression sickness where you trail along and get a bend at 20 or 30 feet. This Workshop is concerned with diving deeper than 400 feet; therefore I won't go into "old hat" stuff about lower depths. Farmer and Thomas (2) present data (Table IE-1) giving different aspects of END. The important point to be gleaned from these cases is that no less than 20 are reported, of which the majority occurred at depths greater than 300 feet, and these represent a high proportion of the total number of cases of decompression sickness encountered from that type of diving.

This is a reversal of the picture that occurs at shallower depths, where it is fairly rare to get eighth nerve disturbance and very common to get bends. In our own experience at RNPL, when dives of more than one hour's duration were tested at depths requiring stoppages at depths greater than 300 feet, all the cases of decompression sickness encountered at deep depths were forms of END. To be more precise, all the decompression sickness that occurred in deep depths, from deep dives, were END.

Furthermore, the type of eighth nerve disturbance being encountered is undoubtedly true decompression sickness, as evidenced by the fact that when stage decompression is performed, the onset of the eighth nerve troubles does not occur until a period of waiting has been completed, comparable with that normally encountered with the onset of classical limb bend pains. For example, after two hours at an 800-ft depth, a change of pressure was made at 50 ft/min to a new lower stage at 490 feet. After a 50-minute period at 490 feet, one of two divers who performed this pressure-time profile began to complain of feeling dizzy, and this pattern of decompression, followed by a latent period and than development of signs or symptoms, is typical of true decompression sickness, as opposed to various forms of mechanical injury which may lead to a similar clinical picture. A typical latent period might be 50 minutes. The most spectacular case of END occurred following decompression from 1500 feet, when one of two divers, decompressed at a steady rate of 40 ft/hr, developed the now well-known problems of dizziness, nausea, and vomiting at a depth of approximately 1250 feet. He was decompressing continuously, the other chap was coming by stages. Thus inadequate decompression, either by a stage system or by continuous bleed, can both lead to serious eighth nerve disturbances. It is, therefore, quite apparent that the new area of concern is somehow intimately concerned with tissues innervated by the eighth nerve, or the nerve itself.

One other factor must be mentioned before proceeding to discuss underlying theoretical considerations. This concerns the oxygen partial pressure used in deep diving. If men are decompressed using 0.22 bar of oxygen, at a depth somewhere in excess of 100 meters, it becomes quite difficult to obtain a successful decompression routine

Table IE-1. Different aspects of eighth nerve disturbances (END) during decompression (2).

| | Hearing loss and vertigo | | | | | |
|---|---|---|---|---|---|---|
| Case # | 8 | 9 | 10 | 18 | 19 | 20 |
| Bottom depth, ft | 350 | 160 | 350 | 650 | 650 | 650 |
| Bottom time, min | 90 | 35 | 60 | 240 | 240 | 240 |
| Breathing mixture | $HeO_2$ + Air | $HeO_2$ | $HeO_2$ | $HeO_2$ | $HeO_2$ | $HeO_2$ |
| Onset of symptoms | 120' During Ascent | Surface + 25 Min | 110' During Ascent | 430' During Ascent | 450' During Ascent | 450' During Ascent |
| Delay in recompression treatment, min | 5 | 19 | 17 | <10 | <10 | <10 |
| Depth and time of relief | 210' +103 Min | 165' +38 Min | 165' +6 Min | 650' +25 Min | 550' +75 Min | 550' +45 Min |
| Effects | Good Recovery | Good Recovery | Good Recovery | Good Recovery | Good Recovery | Good Recovery |

Table IE-1. Different aspects of eighth nerve disturbances (END) during decompression (Cont'd.)

| | Hearing loss | | | | | |
|---|---|---|---|---|---|---|
| Case # | 11 | 12 | 13 | 14 | 15 | 16 |
| Bottom depth, ft | 350 | 450 | 450 | 350 | 350 | 450 |
| Bottom time, min | 30 | 60 | 60 | 53 | 30 | 120 |
| Breathing mixture | $HeO_2$ + Air | $HeO_2$ + Air | $HeO_2$ + Air | $HeO_2$ + Air | $HeO_2$ + Air | $HeO_2$ |
| Onset of Symptoms | 100' During Ascent | 56' During Ascent | Surface +206 Min | 95' During Ascent | 40' During Ascent | 350' During Ascent |
| Delay in recompression treatment, min | No Recompression | 300 | 66 | 17 | 21 | No Recompression |
| Depth and time of relief | No Recompression | No Relief | No Relief | 165' +16 Min | 60' +145 Min | No Recompression |
| Effects | Significant Residual | Significant Residual | Significant Residual | Good Recovery | Good Recovery | Significant Residual |

using even very slow decompression rates. We used our line of reasoning (about which I will say more later) for the decompression until the diving depth reached about 100 meters, but then any further extrapolation of the reasoning met with failure, and this despite adding considerable amounts of decompression time both deep in the schedule or shallow in the schedule, or indeed, both deep and shallow. For example, using a decompression schedule far longer than the USN Tables will not work at 0.22 bar; it only succeeds in giving a trouble-free decompression if the oxygen partial pressure is raised to 0.3 bar. When we raised the oxygen to 0.4 bar, our original extrapolation of the reasoning which occurred on 0.22 bar now became feasible at greater depths.

After several months of unsuccessful attempts to circumnavigate the difficulties encountered with lower oxygen partial pressures it has been decided that the oxygen partial pressure is not as effective at depths in excess of about 110 meters (350 ft) as it is at depths shallower than this. In other words there is some form of synergism between the oxygen partial pressure and pressure itself. Now whilst small animal work cannot be considered to apply without modification to the human situation, a similar synergism between oxygen and pressure has been demonstrated with rats at the U.S. Naval Submarine Medical Research Laboratory.

Another aspect of table development is the fact that similar sets of reasoning may give rise to the same, or nearly the same, type of decompression schedule. Keller and Buehlmann (3) say:

> "Some basic difficulties are encountered in applying the Haldane model to the problem of decompression. By introducing certain new concepts the model can be applied to all posed problems.
>
> A review is first given of the basic kinetics of gases in relation to the theory of decompression and the pertinent formulae are developed.
>
> The new concepts of "Haldane deviation", "deviation of saturation" and "specific saturation excess" are defined in order to exactly outline the systematic deviations of the Haldane formulae from the actual state of solution of gases in special diving conditions.
>
> The term "Latency period I" defines the time during which a I-fold over-saturation leads to organic damage. The analysis of this term permits a better understanding of the accidents of diving."

As you will see, they adopted a modification of classic Haldane analysis for their decompression procedures. Schreiner compares the results of applying the Keller/Buehlmann type of modifications to his own use of the Workman M values which are, of course, another form of modification of the Haldane concept.

"For a ten-minute exposure to 800 feet (244 meters) breathing 8 percent oxygen--92 percent helium, all x values sustained were equal to, or less than the appropriate M values for helium at each depth during ascent, and the same observation was made (Fig. 16, p. 34) for a 5-minute exposure to 1,000 feet (304 meters) on 8 percent oxygen--92 percent helium. We can conclude from this comparison that both Keller and Workman are employing approximately identical ascent-limiting criteria even though they are based on different conceptual views of inert gas transport." (4)

It is an important point that we all tend to obtain the same pressure-time courses from different theoretical standpoints. It is pertinent to observe that whether we adopt purely mathematical "black box" solutions to the decompression question or whether we construct a physiological model, whether we believe in extravascular or intravascular bubbles or whether we believe the system to be perfusion- or diffusion-limited, we are all tending towards similar types of decompression procedures because the facts of the situation are constraining us to do so. Thus, protagonists of the "inherent unsaturation" or "oxygen window" techniques or those who believe in a permitted safe supersaturation level in one, or many, tissues are all tending towards pressure-time profiles (for short bottom time diving at deep depths) which have a very similar decompression time course. I can say this with some authority because I have been privileged to see a number of different decompressions being used by various groups.

It becomes apparent that as the bottom time extends, the diversity of the schedules begins to increase, until we end up with the situation of, say, Professor Buehlmann's group taking 88 hours for the same dive that USN would take 240 hours to do. It is my certain knowledge that the form of reasoning which was successful for relatively short bottom times at deep depths greater than 400 feet, or for relatively long periods, as well as short periods, at depths shallower than 400 feet, will not succeed.

The reason for my total certainty that the same reasoning cannot be extrapolated is to be found in the results of our attempts to answer the old problems concerning how shallow, or how deep one can dive when the tissues of the body are saturated with gas at a constant pressure. These results were reported at the 5th Symposium of Underwater Physiology (1).

Essentially, the concept is to conduct an exposure for 24 hours at various pressures, then to see how far you can decompress without mishap. This is useful information, since it tells how far you can excurse and come back on a no-stop basis. The saturation depth (absolute pressure) is $P_1$, and $P_2$ is the depth you can go to.

Originally our thinking was so dominated by the Haldane ratio concept that we ended up with the relationship $P_1 = RP_2$, with R the Haldane ratio. This is illustrated in Fig. IE-1.

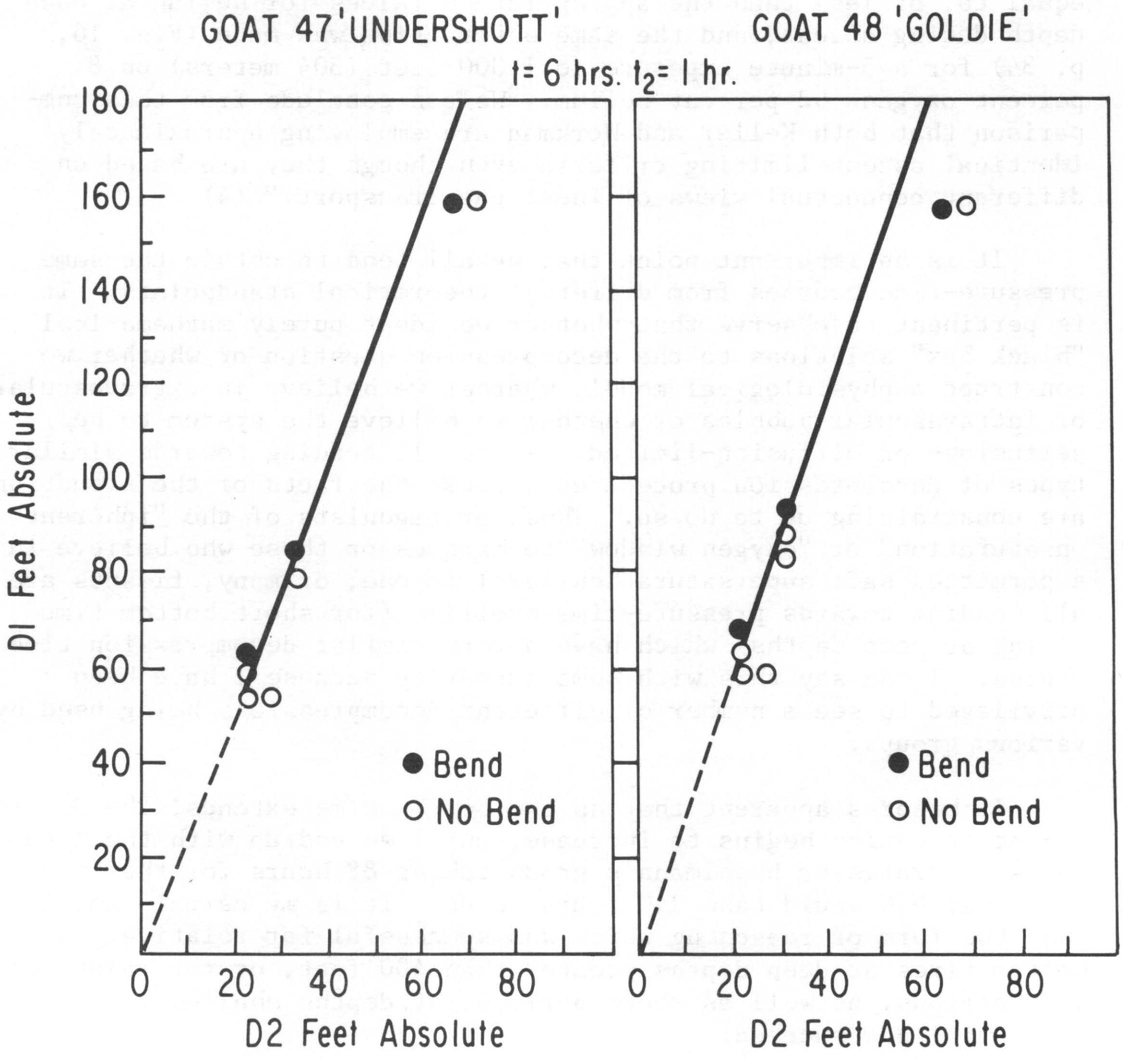


Fig. IE-1. Curve of form $D_1 = RD_2$ derived from animal experiments showing relation of saturation depths. $D_1$ = original saturation point; $D_2$ = depth to which safe ascent can be made; R = Haldane ratio.

The Barnard data yield a similar picture (Fig. IE-2), but the line is better described by the expression $P_1 = AP_2 + B$, where A represents the slope of the line and B the intercept.

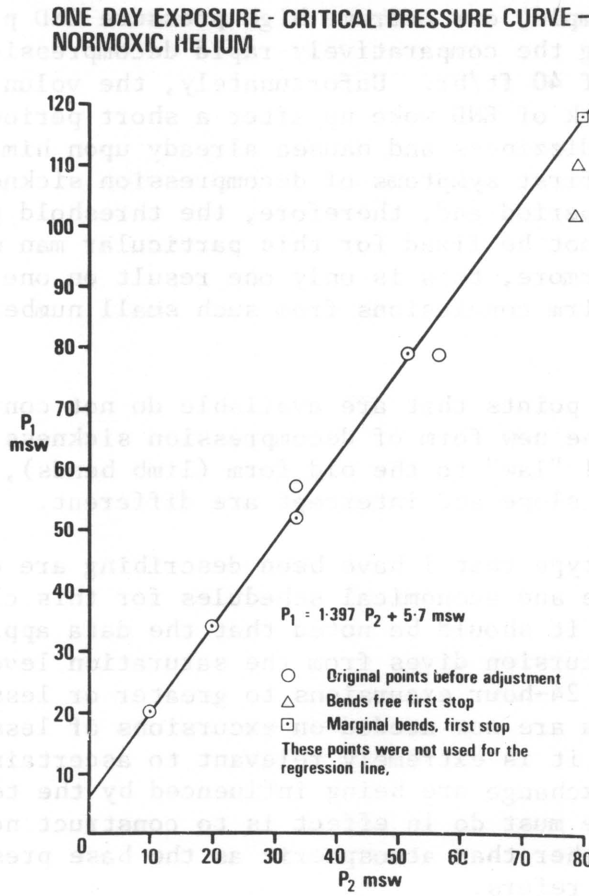


Fig. IE-2. Saturation critical pressure curve, derived from Barnard's data (1). Form of curve is $P_1 = AP_2 + B$ where B is the intercept.

Thus for both animal and human results the simple linear relationship is astonishingly accurately obeyed over quite a large pressure range. However, with the helium results it is quite impossible to extrapolate this line much beyond about 100 meters.

One reason for the apparent inability to conform to the "law" at greater pressures is that the end point of experiments at pressures up to 100 meters was limb pain, but the increase of pressure alters the nature of the first signs of decompression sickness (as mentioned before), and we now have END as the decompression sickness manifestation. There is insufficient evidence of a proper quantitative type to draw a new saturation decompression line, but we have collected data at three points, namely 110 meters, 300 meters, and 450 meters, which tend to show a new line of the form $P_1 = AP_2 + B$. This describes the new boundary conditions, but the values of the intercept B and the slope A have now been markedly altered. For example, our extreme high pressure END problem was encountered during the comparatively rapid decompression from 1,500 feet, at a rate of 40 ft/hr. Unfortunately, the volunteer who exhibited an attack of END woke up after a short period of sleep with feelings of dizziness and nausea already upon him. Presumably he developed the first symptoms of decompression sickness sometime during his sleep period and, therefore, the threshold point for END at this depth cannot be fixed for this particular man with great accuracy. Furthermore, this is only one result on one man, and to attempt to draw firm conclusions from such small numbers is mere self-deception.

However, the points that are available do not contradict the hypothesis that the new form of decompression sickness, END, follows a similar physical "law" to the old form (limb bends), but the relevant constants for the slope and intercept are different.

Data of the type that I have been describing are crucial to the production of safe and economical schedules for this class of diving. However, it should be noted that the data apply only to extremely long excursion dives from the saturation level. In fact, we are describing 24-hour excursions to greater or lesser depths, and much more data are now needed on excursions of lesser time durations. Also, it is extremely relevant to ascertain whether the dynamics of gas exchange are being influenced by the total hydrostatic pressure. What we must do in effect is to construct no-stop curves using pressures other than atmospheric as the base pressure to which the no-stop curve refers.

The other form of diving at depths greater than 400 feet is to use short bottom times with as high an oxygen content throughout the dive as possible, thus leading to the speediest possible decompression. This can probably be further aided by substitution of inert gases other than helium at appropriate lower pressure levels in the decompression. This is a somewhat haphazard form of decompression which really can only be done on a very empirical basis but, nevertheless, this type of procedure has met with some degree of success and I think that we will hear further about this type of diving later on.

Finally, there is the standard method of saturation at a depth, working at that depth, and then doing a single decompression therefrom. There is the problem of what form does the saturation decompression procedure take. We have found the USN Saturation Tables quite satisfactory to depths of the order of 400-500 feet, but thereafter we personally have never succeeded in conducting an entirely trouble-free USN type of decompression at depths of the order of 600 feet (180 meters). However, we have found that pressure-time profiles quite similar to the USN saturation decompressions will succeed if the oxygen content is raised to 0.4 bar rather than the 0.3 bar which they prefer to use.

Discussion

Dr. Schreiner: Would you elaborate on what you said about having better luck at 0.4 atm $PO_2$?

Dr. Hempleman: Using 0.22 bar we just couldn't decompress people from deeper than about 100 meters.

Also, using the staging techniques reported at the Fifth Symposium (1), we used 24-hour steps, and I would like to make the point that there is no use dealing with halftimes that would go longer than 24 hours.

Our steps were something like 10 meters to the surface, maybe 24 meters to 10--I'm relying on memory--and these worked out like clockwork. All stages were 24 hours. But when we took the whole thing deeper, around 100 meters, little hits would develop on perhaps the third stop. We couldn't seem to reduce the stops enough to get rid of everything.

It all cleared up with the increase in oxygen partial pressure.

## REFERENCES

1. Barnard, E. E. P. Fundamental studies in decompression from steady-state exposures. In: Proc. Fifth Symp. Underwater Physiol., edited by C. J. Lambertsen. (in press).
2. Farmer, J. C., Jr., and W. G. Thomas. Vestibular injury during diving. Foersvarsmedicin 9:396-403, 1973.
3. Keller, H., and A. A. Buehlmann. Probleme der Dekompressions-berechungen. Helvet. Med. Acta 28:764-774, 1961.
4. Schreiner, H. R. Safe ascent after deep dives. Rev. Physiol. Subaq. Med. Hyp. 1:28-37, 1968.

# SESSION II: SYSTEMS CONSTRAINTS

# II. SYSTEMS CONSTRAINTS

## A. DIVER WORK: ANDRE GALERNE

When I first began to work underwater, 25 years ago, the U.S. Navy Diving Manual defined deep-sea diving as work done deeper than 32 feet. As we talk here, divers are going to 300, 400, or even 500 feet of water. Some deeper work has already been performed, and there is no doubt that work will soon be done at 1,000 feet or deeper than that. Progress in research in gas mixtures and diving support equipment has been instrumental in this development.

### Equipment and Tools

Discussing diver work gives us two related topics to explore: equipment which puts the diver on the bottom, keeps him there, and gets him back safely, and equipment which the diver uses while on the bottom to do work, namely his tools. Surprisingly enough, most research has focused on the first area; diver tools have not had the same evolution. Some effort has been made in this direction in the last few years (I would cite particularly the Naval Civil Engineering Laboratory in Point Mugu, California, for its work on hydraulic tools), but it is still true that the man whose time may cost the customer thousands of dollars per hour does most of his work with an adjustable wrench.

### Environmental Constraints

Everyone knows that with proper equipment the diver can weld, cut with gas or explosives, pour concrete, place masonry and piping, rig, drill or blast. Such work can be performed with relative ease in clear, calm, shallow water. However, all this can be tremendously challenging if any one of these factors is altered. Limited visibility can change an easy task into a nightmare.

Imagine yourself with your eyes closed, in the middle of a room full of obstacles, pipes, and valves, and your object is to place a small nut on a small bolt somewhere across the room. You must do this only by touch. We can complicate this picture by lowering the temperature, which decreases your ability to think clearly, numbs your body and fingers, and inspires the overpowering and reasonable desire to go back to a more comfortable world. Add to this current which can transform your gas hose into a tow rope and turn you into a kite.

The expression, "you don't have to be crazy, but it helps" has been applied to diving. I hope at least that insanity is contra-indicated in my profession, but it is hard to find men who have sufficient imagination and skill to be effective with primitive tools and who are stoic enough to take the physical beating which divers routinely undergo.

If you assume that the diver can get the work done, but is constrained by cold, darkness and pressure effects, you can attempt to remove or reduce these constraints. Our efforts to deal with cold and darkness have been reasonably successful: we add heat and light. Our efforts to deal with pressure effects have been even more successful: an International Underwater Contractors' diver at 60 feet can work 3-4 times as long as a diver using conventional techniques, through the use of sophisticated gas mixtures.

At International Underwater Contractors we separate diving work into three categories: 1) Shallow water long-exposure dives, 2) Deep bounce short-exposure dives, and 3) Deep saturation dives.

## Shallow Water Long-Exposure Dives

In the first type of dive, inshore or offshore, air or nitrogen/oxygen mixtures are generally used. In construction diving, such as work in harbors, power plants and outfalls, we generally rely on air tables, which have been proved for a long time to be safe and easy to use.

Although recent investigations have shown an alarming increase in aseptic bone necrosis in older divers, statistical proof of cause and effect must be exclusive: there should not be another factor which might either cause the result or contribute to it. Bone necrosis is a result of blood circulation inadequacies, which can be provoked by alcohol. It is often difficult, therefore, to know if the diver has developed bone necrosis because of diving, drinking, or both. Certainly, our experience with divers for over 20 years could suggest to a statistician that diving causes drinking, or the reverse! International Underwater Contractors introduced nitrogen/oxygen mixtures with enriched oxygen into construction diving some 15 years ago, and we have experienced an absence of bone necrosis for typical exposures - down to 200 feet for up to 2 hours of bottom time. This suggests that decreasing the nitrogen partial pressure may help.

Decompression incidents in this type of dive are very rare in our company, and we have experienced only two inner ear problems in thousands of dives. It is our considered opinion that these dives with air or mixed gas in relatively shallow water have been proven to be inoffensive for the average diving population.

## Deep Bounce Short-Exposure Dives

This type of dive is performed from offshore rigs with a bell and chamber system. These dives are quite deep (500 to 600 feet) with bottom times ranging from 30 to 45 minutes. These dives are occasionally referred to as "deep and dirty" because they are characterized by: 1) quick exposure to depth and lengthy decompression, and 2) occasional neuromotor disorders during compression.

The problems in these dives are often provoked by failure of equipment such as winches, gauges, gaskets, and so on. We cannot, therefore, avoid the conclusion that an apparently safe laboratory procedure is not easily duplicated in the field. Many incidents of bends have been reported in deep bounce dives. On some jobs there have been reports of up to 30% incidence, which emphasizes the difficulty of performing safe decompression in deep water.

International Underwater Contractors has pioneered in the use of three-gas mixtures. We have had exposures at 350 feet for up to two hours, with virtually no decompression incidents. While three-gas mixtures are beginning to get attention from other diving companies, the big difference between our technique and that of others seems to be the relatively high partial pressure of nitrogen we use, both during the dive and in decompression.

I began to suspect more than 20 years ago that nitrogen is not narcotic, per se, but that its density produces a restraint on ventilation which, in turn, yields a narcotic effect. I have not had reason to change my opinion, and a little experiment which I made 15 years ago seems to confirm it.

Using a demand regulator to avoid any interference with $CO_2$ buildup in a helmet, I used a mixture of 90% helium/10% oxygen with divers at 250 feet. This mixture at that depth theoretically can induce no narcotic effect, and the density of the gas mixture is only twice that of air at the surface. I then voluntarily reduced the pressure, which reduced the flow. Within 15 minutes, the divers were experiencing tremendous narcotic effects. When full pressure was restored, the narcosis disappeared. For me, this experiment demonstrated that ventilation restrictions can produce narcotic effects.

After this experiment and for the past 15 years, we have maintained a relatively high partial pressure of nitrogen in our mixtures, generally without narcotic effect. Very recently Dr. Bennett demonstrated that modest partial pressures of nitrogen have offered excellent protection against the High Pressure Nervous Syndrome (HPNS).

Although nitrogen is more soluble in fat than helium, it has a slower uptake into the tissues than helium, and its release is also slower than helium. Accordingly, in short-duration dives, nitrogen requires less decompression than helium and has less tendency than helium to provoke decompression accidents. As you know, nitrogen absorption is increased in longer dives or saturation dives, and decompression time becomes longer. We accordingly employ nitrogen wherever feasible, and we gain two other advantages - voice intelligibility, and a heat transfer capacity less than that of helium. Use of nitrogen minimizes respiratory heat loss, and helps us to reduce diver chilling, another variable in decompression incidents.

To handle a tri-gas mix, however, flexible gas mixing capabilities are essential. International Underwater Contractors uses the Airco

Mixmaker to prepare gas for all deep dives. This device measures gas with precision and mixes it directly on site. While the Mixmaker is expensive, it brings us some operational advantages and economies. First, we can use atmospheric air for our nitrogen supply and part of our oxygen. This means we supplement with bottled helium and oxygen, but storage of nitrogen is eliminated. Second, we can simply dial the optimum mixture for the dive at hand, taking into account the specific bottom time and depth contemplated. If operational considerations change the dive profile, the mixture can be changed to optimize the decompression schedule.

Please don't let me give you the impression that tri-mix techniques and the admixture of nitrogen solve all the problems. The physiology of decompression is still imperfectly understood. Theoreticians like Dr. Schreiner, Dr. Workman, and Professor Chouteau, to mention a few, have made mathematical models embodying various assumptions about the way gas is absorbed into body liquids and tissues under pressure and the way it is subsequently released. Blood, for example, accepts and releases soluble inert gas almost instantly; other tissues become saturated very slowly. Accordingly, the calculation of decompression profiles is inexact. In fact, a bubble in the blood stream is an event governed by statistical probability rather than by obedience to any immutable law or formula. I think it would be fair to say that, while diving decompression calculation is a science, those who do it best are obliged to employ a little art as well.

## Deep Saturation Dives

The third type of diving is the saturation technique which was pioneered by Captain George Bond and the U.S. Navy and has been widely used for the past few years by the whole diving industry. In this case, helium/oxygen mixtures are used, and the technique requires very lengthy decompression. Also, saturation diving decompression is very much easier to handle and safer than we originally believed. Saturation capability has pushed the human limit far beyond what we dreamed of 10-20 years ago. We can now say that we can safely dive between 500-600 feet. The people, techniques, and equipment used are all making tremendous progress, but neither our achievements nor the price which we pay in time, money, and perspiration have reached the bottom. There is a tremendous need and the demand provoked by the energy crisis will stimulate us to go deeper and deeper.

The commercial diving industry has made great strides from its very recent infancy. There has been enormous sophistication of techniques, procedures, and operational personnel. Please reflect that virtually all of this progress has been achieved by private capital with very limited help from governments. The diving industry faced the problems and found the solutions, with ingenuity, hard work, and guts.

For a long time only a few dedicated doctors were working with divers and their problems, but now there is a more widespread collaboration between the doer and the doctor to develop procedures and new approaches in basic research. Hyperbaric medicine is not a dirty word. It is only in the past 15 years, during which time our definition of deep diving has more than doubled, that we have come to believe that a diver can have a long and fruitful life and career.

Let me close with a few words about the divers who really are the focus of our interest and who have been the willing guinea pigs in the advance of hyperbaric medicine. The British Navy Diving Manual of 20 years ago described the ideal diver as "stocky, stolid and phlegmatic." In America, these requirements were paraphrased at about the same time as "a man with a size 5 hat and a size 15 neck." The new breed of diver is an inspiration to those of us who remember the old ones. They are better educated, more highly skilled and professionally oriented. They respond to technical challenges which would have baffled an earlier generation of divers, and they accept the risk and hardship, not always without complaint, but usually with intelligence and humor.

As we demand more and more of them, let us remember that they are a unique breed, and it is they, not we, who are in the forefront of the advancing science of diving.

## B. THE SCOPE OF DEEP JOBS: H. D. WILSON

Requirements for dives of more than 400 feet come chiefly from the petroleum industry. These dives fall into four main categories: 1) drilling rig support, 2) platform installation, 3) laying, connection, and inspection of pipelines, and 4) salvage.

### Drilling Rig Support

For drilling rig support, we need equipment to handle the following types of dives: 1) inspection to determine the extent of the work required, 2) the short duration dive (anything between one minute and saturation), and 3) the saturation dive.

We have the back-to-back capability to use the first team of divers in either the short duration or the saturation mode, and if necessary, to lock in a second team to carry on in case of difficulties. If a diver is injured, for instance, we can keep on operating.

Work that might be required in support of a drilling rig includes guide wire replacement, hydraulic system repair, and cement return inspection. In the North Sea, conditions are so rough that at times the rig will drag its anchors and break off the blowout preventers, so that we actually find ourselves performing a salvage operation.

We go into saturation for any operation which requires us to dive beyond surface rescue limits. In this mode we have complete environmental control. The reason for this is that if a bell gets fouled on the bottom, we will have to go to saturation; if we do not have adequate life-support capability we may lose some divers, particularly in cold water. Temperature control is the most crucial factor in such cases.

The keys to the kingdom are our saturation rigs. My company builds all its own equipment. Temperature and humidity control and carbon dioxide scrubbing are all handled within the chamber. Temperatures can be set to within one hundredth of a degree. In saturation diving a difference of only one half of a degree may make a difference. Hot and cold water are supplied for flush toilets and showers.

### Platform Installation

The BP 40's platform, of which there are now four in the North Sea, is the largest structure of this type now in existence. In the BP 40's area in the North Sea there is oil sufficient to meet 20-25% of Great Britain's requirements. These platforms vary somewhat in size; they are 450 to 500 feet long, and weigh about 27,000 tons.

The platform is floated out by large buoyancy tanks on the bottom, which are released hydraulically. During installation, the first job for the divers is to check for hydraulic failure among approximately 100 hydraulic rams, and to deal with any problem that exists. The next job is to remove the large buoyancy spheres that hold the platform at the proper angle as it goes down. When the platform is on the bottom, all the hydraulic systems are activated. The ballast tanks are then blown and the whole buoyancy system goes to the surface. The piles are then cut off after they have been driven and routed because they show too much surface to the sea. Thanks to the advanced skills of underwater burning, our divers can cut two-inch steel at the rate of one foot per minute. Because of the severe weather conditions in the North Sea, we had to install one of these platforms in one day.

The deck chamber complex can support three teams of two divers each, and can operate on a 24-hour a day schedule. In addition to three deck chambers, there is an auxiliary bell which can be used for rescue, observation, or shorter duration dives.

This type of job costs about $300,000 a day, so the economic consequences of delay would be severe. We cannot afford to make a mistake. We have double or triple backups for each piece of equipment.

There is a separate control van for the saturation life support, the saturation diving operation, and the surface diving which is sometimes required in support of the saturation dive.

Pipeline Work

In pipeline work, divers are required to lay, connect, and repair pipeline. One method of repairing pipelines makes use of an underwater habitat to which the diving bell can mate for a dry transfer. We hire pipeline welders and send divers down with them after a few days of training, instead of trying to teach divers to be welders or welders to be divers.

Salvage

Every well that is drilled requires some salvage. The first thing to go down to the ocean floor at the beginning of an operation is a guide structure, with a blowout preventer and many guidelines attached to it. After the well is drilled, this is cut off and must be recovered.

Two years ago, a platform was knocked over in the Gulf of Mexico by hurricane Camille. To salvage this, we performed a 96-day saturation operation, involving 16,569 man hours in saturation, 316 bell dives, and 2,426 excursion dives from the bell. We recovered 6,000 tons of steel. Records that were made during this operation have yet to be broken. To cut the structure up, 979 explosive charges were set; the depth was 330 feet. We saturated the divers at 240-260 feet, using the excursion mode. We used the Navy saturation schedules except that the divers were allowed to sleep when they wanted to instead of on fixed schedules. We had no cases of bends. During decompression the divers had room to stand and move around.

Current Needs in the Diving Industry

In closing, I would like to state what I believe to be our main problems in the diving industry. First comes the need for skilled divers; training is the greatest need at present. Next, since the technique of excursion diving from saturation is the most important recent development in diving, it is necessary to determine whether it is safe to adopt a no-decompression profile for excursions, or whether some decompression is necessary. Also, a slightly faster decompression would be desirable. The Navy schedule appears to be a little over-conservative, while some of the commercial diving schedules seem under-conservative.

The toughest problem of all is the short duration decompression schedule. A 1000-foot saturation dive is easier to make than a two-hour dive at 500 or 600 feet. It is difficult to determine the optimum length for a short duration dive--30 minutes, one, one and a half, or two hours? I tend to prefer the longer times, but this makes decompression much more difficult. A very pressing problem at present is vestibular bends. We have had numerous cases, but so far, fortunately, no cases of hearing loss. As to bends, we have a lot, but we now have sufficient understanding of the bends problem to be able to stop, back up, hold, recover, and then continue with the decompression.

Commercial diving requires continuous modification of techniques as the requirements change.

## REFERENCES

1. Lawrie, J. A., editor. Manual of Safe Practices in Commercial Diving Operations. Houma, La.: Association of Diving Contractors, 1974.

## C. DIVER ENVIRONMENT: J. A. LAWRIE

The three aspects of diver environment that I have been asked to discuss are pressure, cold, and gas, which are, as we all know, the basic problems.

In no other industry, with the possible exception of space, have new technologies been applied so rapidly as in diving. During the past 10 or 12 years, new decompression procedures and other scientific developments have moved from the laboratory to the field in less than a year.

Such rapid technological progress demands that the scientific community know what is practically applicable in the field. It also demands an understanding of what the diving contractor is able to assimilate from the plethora of technological information that confronts him. The technological approach to problems must be tempered by an understanding of the practical and economic limitations of man's underwater working capability.

Keeping corporate or individual knowledge of physiologic hyperbaric data secret is now obsolescent; we have a common concern for the well-being of commercial divers throughout the world. The diving industry has achieved the maturity which demands true international cooperation.

To return to the three main problems: the diver lives under pressure, is subject to cold which is aggravated by the heat-conductive environment, and must be maintained in an artificial gaseous environment.

### Pressure

Progress in both depth and duration of diving and the development of compression and decompression procedures has been satisfactory as far as the commercial operator is concerned.

However, two points must be emphasized: 1) Time under pressure constitutes a fundamental safety hazard; equipment malfunctions, procedural errors, and environmental hazards endanger the diver's survival. Shorter decompression time may not be economically significant, but it is important to diver safety. 2) There are many problems in translating decompression procedures from the laboratory to the field. For instance, multiple gas switching imposes logistic and procedural difficulties on a rig at sea, or specialized laboratory instrumentation may not survive in field conditions, or may overtax the capabilities of the diver-operator. Consequently, optimization of a technique must sometimes be sacrificed to practicability.

Research efforts should now be directed toward the following: treatment of decompression at extended depth; hyperbaric pharmacology; etiology of osteonecrosis; problems of adequate pulmonary ventilation; and the achievement of rapid access to a deep bottom depth with a minimization of HPNS and vestibular bends.

Emergency situations may occur during deep, short-duration dives, which force the operator to convert to a saturation mode for a long job or to combat decompression sickness. There is a great deal of work to be done on the handling of bends and CNS decompression sickness. The U.S. Navy should be commended for its work in developing oxygen recompression tables. However, this does not help when a man is in trouble at 250 or 300 feet. There is much to be done here.

Cold

It is obvious that the conductivity of the water environment, enhanced by the conductivity of helium, imposes physiologic hazards and causes performance decrement.

Reliable techniques for maintaining diver temperature, such as the use of surface-generated hot water for body and respiratory heating, combined with interior heating of the bell, are generally utilized. However, operational hazards exist. Direct fire boilers are prohibited because of explosive proofing hazards, and hot water supply hoses increase the size of umbilicals, thus causing handling and storage problems and increased drag. The optimal solution would be a bell-mounted regenerable heat source, but this would require better insulation, new power sources, and possibly regenerative respiratory heat exchange.

Another hazard is the possibility of the bell being cut off from surface supply power. If this were to last 24 to 48 hours, how could hypothermia be prevented? This is not a theoretical problem; we have seen accidents of this type.

Gas

We have settled on air and helium-oxygen as the likeliest breathing gases, as well as tri-mixes, which are used by Ocean Systems and others, including International Underwater Contractors. Investigations into other gases do not appear to be justified at present.

However, there are other aspects of diver breathing gases that do require investigation. Little is known about toxic concentrations and allowable exposures to contaminants. Further work is also required in the areas of simplification of gas analysis, removal of contaminants, and inert gas conservation. In passing, I should like to comment on the difficulty of operating infrared $CO_2$ analyzers in the field.

Present breathing apparatus leaves much to be desired. As working depth increases, assisted pulmonary ventilation would be desirable. Also, simple push-pull equipment (or analagous types) for the conservation of breathing gas is needed for widespread use. Such equipment has been developed, but is not commercially available.

To conclude, we now have the capability to put a diver in 1,000 feet of sea water, but we still need to increase our knowledge of occupational health and the practical field application of scientific knowledge. In this connection, it is particularly important that scientific data be translated into the simplest form possible, because divers who are coming into the field now, with the tremendous increase in diving activity, are much less experienced and knowledgeable than the divers of six or eight years ago.

Discussion

Mr. Vann: Did you say you are going to publish some Mark VIII-A tables?

Mr. Lawrie: I think we have reached the point where we are all participants. We are increasingly prepared to share whatever information we have, because it is getting to the point where decompression is not in and of itself a big competitive factor. Yes, we are prepared to share information. Let me know what you need.

Dr. Hills: What is the maximum number of breathing mixes that can practically be handled, in say a 500-foot, one-hour dive?

Mr. Lawrie: We prefer to work from bottom mix to the point where we can shift to air. If you have mixing equipment, you can use several. In general, I would say two mixes are a reasonable maximum.

Dr. Hills: This is the kind of information we need.

## D. DIVER EQUIPMENT AND OPERATIONS: D. M. HUGHES

To begin with I'd like to discuss three subjects: 1) breathing equipment; 2) heating the diver or his breathing gas; and 3) communications. Then I will go into a more general discourse on the realities of decompression table making.

### Breathing Equipment

The most commonly used equipment is the demand type. The Kirby-Morgan band mask, which has become universally accepted, is being modified for use at greater depths. Our company uses a demand type helmet--the Rat hat--which maintains a rather positive pressure on inhalation once the regulator is triggered. It has very large orifices and supply hoses and we operate it with a very high supply pressure. It has been tested successfully at 1000 feet in the wet pot, and we believe it would operate satisfactorily at even greater depths.

Semi-closed circuit equipment is used to some extent. The Draeger gear works successfully, and as far as we know, is being used by the Italian company, SSOS. Closed circuit gear is still in the development stage. There is at present at least one commercial effort to use it in the Gulf of Mexico.

Our company does not believe that closed circuit gear is practicable for normal operations. The cost of gas is negligible when weighed against the possibility of having to abort a dive because of equipment failure. As has been mentioned, we are faced with a scarcity of experienced divers, and it is quite possible we might get into a position where we have a diver trying to use complex and sophisticated equipment which he is not adequately trained to use. Some of the currently used gear was tested in the Navy 1600-foot dive, and, according to Dr. Spaur, the gear itself was not the limiting factor.

Another type of breathing system is the BIB (built-in-breathing) system that is used in the chamber when different gas mixtures are used during decompression. We have made an attempt to build a BIB system that would conserve gas. If you change mixtures, for instance, at 200 feet, and stay on the BIB system for an hour, you use a great deal of gas. We find that gas consumption during decompression greatly exceeds gas consumption while working on the bottom. It may be that gas switching is impracticable from an economic point of view in field operations.

### Heating the Diver

Heated suits are becoming common, particularly for deeper work. At present no electrically heated suits are available that meet the requirements of commercial contractors. Suits are heated by hot water, which is a very straightforward and simple system.

As to heating the gas, there are electrical heaters in use, but our company uses the same hot water heat source that we use for the suit. Another alternative is the thermal regenerator. This is merely a heat sink or trap that is incorporated into the mouthpiece of the helmet. We have used this in Arctic conditions at 400 feet with very good results. The tender in the bell also breathes through a thermal regenerator to maintain his own body heat levels. This is a very positive system for the preservation of body heat and should be more widely used.

To heat the diving bell, we use the same hot water heat source. If the bell is insulated and you have a fairly large heater and a satisfactory means of circulating gas in the bell, you can maintain a reasonable amount of heat.

Communications

Although a great deal of helium speech unscrambling equipment is now available, our problem has to do with the application of that equipment. For instance, a $28 noise-cancelling microphone in the helmet will produce much higher fidelity than a $2.19 Japanese speaker, but it is the latter that we all use, simply because the average diver knows how to hook it up. Here we go back again to the same old problem of personnel training.

Decompression-Related Stresses

I would like to discuss briefly the way in which these factors relate to decompression. You must consider the thermal stresses that a diver working in the North Sea has to undergo. The divers go on deck in the cold to help rig up the equipment. They get cold and they perspire. Then they have to get into a cold, clammy diving suit. They get into the bell, and during pressurization the heat builds up very fast. Then the diver is in the water in a heated suit and the stand-by man is freezing to death in the bell. After the dive, during ascent, the bell sometimes gets so cold that ice forms on the inside walls. In a few minutes the divers are back in the deck chamber, where the temperature is under fairly good control.

These stresses have a direct relation to decompression schedules, with the result that laboratory schedules frequently do not work out in the field. These stresses should be simulated as closely as possible in the laboratory.

Depth Requirements

For many years the oil industry believed that divers would never be able to work beyond 400 feet, so they tried to develop remote-controlled equipment, but that did not work out well. Now we diving contractors have done too good a job of selling the oil industry, and they seem to believe that we can do anything. It is expected that we will be diving to 1500 feet this year, and there is potential use for a 2000-foot capability, since oil leases in water of that depth have actually been bought.

Mr. Wide: The welding habitat, with a regeneration system, has been used down to 450 feet. Our breathing equipment consists chiefly of surface-oriented gear, operated from the diving bell. This works well; it has been tested to 1400 feet. We also use thermal regenerators. One problem with them is that they call for too much power close to the diver. We are using between 6 and 9 kilowatts of power to heat our insulated bells. One real problem in saturation diving is how to determine when it is all right for a diver to dive again.

Dr. Hills: What about the one-atmosphere suit, Big Jim? Will it work to 2000 feet or so?

Mr. Hughes: I don't think it is the total answer, but any company that expects to do work in those water depths has to have all the capabilities it can--including divers and submersibles. The suit is a very real thing, it is not a toy.

## Operational Constraints

With regard to operations, let me say first that I don't necessarily represent the industry as a whole; others may differ with me. I want to try to describe the operational constraints that we are faced with that may affect the use of decompression schedules. This is a very involved subject, so I'm just going to hit some highlights.

We can divide the diving procedures that we use into three basic categories. One is bounce diving or short-duration diving, which generally is considered to be less than an hour bottom time. Saturation diving involves workers saturating at the depth of the work and staying there until the job is completed. Saturation-excursion diving uses vertical excursions to reach the work site, from a saturated base. And they each have different constraints, operationally.

The first concern is breathing gas. Basically we're set up with a deck chamber complex, a diving bell, and a gas source which supplies both the diving bell and the deck chamber complex; there is also a gas source on the diving bell. The diving bell has a very limited storage capability for gas; the bell gas is used for two purposes. First, it's a standby gas, an emergency supply for the diver working outside the diving bell, should his supply from the surface be interrupted. The normal method is that the diver working from the bell is supplied with gas from the surface. The on-board supply is only cut-in in an emergency. That means that the bell's on-board gas must be a gas which is suitable for breathing on the bottom. It must have the appropriate oxygen component. Second, the bell normally contains a small supply of pure oxygen, as metabolic make-up oxygen for use in the event of emergency.

The deck chamber requires whatever gases are needed for the environment during decompression. One constraint is that we must start the dive, for practical purposes, with 1 atm of air in the diving bell. So, whatever the effect that this has on the decompression computations, you have to take into account that 0.79 atm of nitrogen is there.

It gets a little complicated, because when you get to the bottom the man in the diving bell is breathing a mixture which consists of at least 0.79 atm of nitrogen, whereas the man out in the water is breathing whatever is supplied down his umbilical, which may be pure helium-oxygen with no nitrogen component. So right off you have two divers being exposed to pressure, but not breathing the same gas mixture.

Another consideration is that on a short-duration dive the diving bell is not pressurized until it reaches the bottom, which means the gas for pressurization has to come down the umbilical. That's not the case in a saturation dive. It is though, in a saturation-excursion dive; usually you pressure up on your excursion after you are in the water. The question was raised about skin absorption of the bell diver. I don't know whether that's a significant factor or not, but it obviously exists.

Discussion

Mr. Wilson: Have you had some problem with the line tender in the bell?

Mr. Hughes: Yes, the diver who got the vestibular hit on the deep dive in the Gulf was a tender. There is something to that phenomenon. Also, the $CO_2$ may be a little bit different between tender and lockout diver, but that's a little hard to predict too. We don't have very good $CO_2$ scrubbing on our bell systems.

Dr. Schreiner: What are the consequences of the tender breathing the same gas from the surface that the diver breathes?

Mr. Hughes: It's a matter of gas usage. It just doubles the consumption.

Dr. Schreiner: Is it open circuit?

Mr. Hughes: Generally. The common equipment is the open-circuit demand type apparatus, which means that the diver is generally getting exactly what comes down the umbilical.

Now, it is possible that, to enhance decompression, you want the men breathing some gas other than bottom gas at some time during the decompression. We don't consider it feasible or reasonable to increase the oxygen partial pressure until the bell is back on deck.

Another point is that whatever the oxygen partial pressure is during a dive, even if it's supposed to be a short dive, it must be an oxygen partial pressure that would be tolerable if the men become trapped on the bottom. You calculate whether the oxygen will be reduced sufficiently by metabolic consumption in a short enough period of time to prevent CNS toxicity (not pulmonary). You don't really worry about pulmonary toxicity at this point.

The next thing is the sequencing of gases. It is not practical, for example, to expect these men to breathe a different gas as soon as they leave bottom. The constraints of gas storage usually make this a little bit impractical.

If you assume that your first gas change will occur when the bell has landed on the deck, and the men can transfer into the deck chamber, how much time should you allow? Fifteen minutes is the absolute minimum and probably not a practical figure at all. Twenty-five minutes is probably a better number; a half hour is okay. You want to get them out of the bell pretty quickly if you can--it's not a very comfortable place to be.

There are several procedures that are used to leave the bottom. In some cases, the divers leave the hatch open and the ascent to the first stop is made that way, and then at the first stop they button the hatch up and prepare for lift to surface. So if you allow about a half hour from the time they leave bottom until they make their first gas switch--this is talking about fairly deep diving--then that's a satisfactory number.

Mr. Vann: What about a short stop on the way up to close the hatch?

Mr. Hughes: Your first stop is usually where the guys will want to button the hatch up; it should be long enough to allow them to do it mechanically and that's about 5 minutes.

Mr. Vann: So then the winch operator controls the decompression.

Mr. Wilson: It's kind of a team play. The big danger is you have to make damn sure that hatch is shut. The only way you can tell this is on the gauges. If you lose communications, then the surface crew doesn't pull that bell any place except to do the decompression profile from that depth.

Mr. Hughes: And for you fellows that are so exacting in your calculations, the typical decompression profile looks like this.

They come up to the first stop, make the stop, and then they do one of two things: they either overpressure to see if they have a seal, and then they start on up, or they start on up and as they bring the bell up to surface, they have to drop the bell back down again and get back on the track. So those of you who think those stops and times are exact, they're really not. And your tables should take that into account.

Next, how do you modify the mixtures that the men are breathing once they are in the deck chamber? The two most common ways are to add oxygen--which is relatively simple to do in the field, to increase the oxygen content by injection of pure oxygen--or to completely flush the chamber, and change to air. That's also relatively straightforward. But anything other than that is not straightforward and if your decompression schedules require you to add some air, so as to change the oxygen component, it's just not practical. We do not have analytical equipment in the field that will give us an analysis of the inert components of the breathing gas. The only thing we can measure is oxygen, and if we're lucky, $CO_2$.

Mr. Galerne: Helium?

Mr. Hughes: Well, you have it Andre, but nobody else does.

The other thing, of course, is that you have to be cognizant of the fire safety limits in the chamber atmosphere. That puts some pretty severe constraints on us, too, from the standpoint of decompression.

Now one area that is very controversial is the BIBS time--the time in the chamber sitting in one atmosphere and breathing another on the mask system. To follow many schedules you have to use a great deal of mask breathing.

If you look in the Duke report, you'll see that the Parent schedule, the one that Oceaneering so charitably gave to Duke, has got 6 hours of BIBS time; this was totally unsatisfactory in the field. You can get guys to sit in your research chambers and do anything for a research dive--that's for science and glory. You get them out on a drilling rig and after about the fifth time they're going to start complaining and when they start complaining you do one of two things. You either change or they change it for you, which means they just don't put the mask on. So you're depending on so much BIBS time, but you're not going to get it. And they don't generally seal very well, so you can't depend on the BIBS gas being what you want it to be anyway. There are a lot of reasons why you should not have much time on BIBS in a decompression schedule.

Mr. Vann: What is "much" time?

Mr. Hughes: This is opinion now, but I think an hour.

Mr. Vann: How about if you break it up instead of giving it all in one shot.

Mr. Hughes: Of course that helps. It's always a compromise when you include BIBS time. You want to keep it to a minimum. And you know if you give the diver a choice of wearing the BIBS for 6 hours or staying in the chamber 3 hours longer, they will usually stay in the chamber.

Dr. Youngblood: Another thing, fashion being what it is, a great many of the divers now have full beards and mustaches. And at best a BIBS will only give you 90% pure oxygen at 60 ft, but with all that hair...It's a fire hazard, too. We ran some tests at Morgan City, and found that we weren't getting even close to pure oxygen.

Dr. Smith: In our decompression procedures, when we consider we're getting pure oxygen, we assume that it's no more than 80%.

Mr. Hughes: Yes, you just can't depend on 100% oxygen. So you see all these neat schedules and computations based on 100% oxygen--no inert partial pressure at all--they just seldom ever exist in real life.

Dr. Youngblood: We found one group using a hospital mask designed deliberately not to give more than 40% oxygen.

Mr. Hughes: Linear decompression or continual descent decompression is very difficult to do in the field.

Mr. Vann: What stop time do you think is reasonable?

Mr. Hughes: The first few stops you can tolerate 1, 2, or 4-minute stops. Once you get on up in the shallower ranges I don't think any stop ought to be shorter than 15 minutes.

Mr. Vann: What increment between stops? Ten feet is the normal. We've been using five feet at Duke. Is that satisfactory?

Mr. Hughes: Well, I don't like to see it, but if there is a distinct decompression advantage in using smaller increments, then fair enough. But I think anyone would have to demonstrate that there is a very distinct advantage before you can justify deviating from your good old normal 10-ft stops. Once we've got the guys trained to figure out that the heavy line is where you stop each time, it's really kind of tough to get them to understand that now you stop half way in between.

Dr. Schreiner: To what extent is pressure being measured accurately; what is the precision of pressure measurement in bell diving today?

Mr. Hughes: There is a tendency--perhaps by government officials--to say, "You've got to have one-tenth of 1% precision gauges," and so forth. But that's a farce. You can control the deck chamber accurately enough, but for divers in the water you have a lot of factors, such as time lag in the pneumofathometer, which create big errors. I know of cases where accidents have almost happened because the operator was trying so carefully to adjust the gauge to exactly the right place that he totally ignored some disaster about to happen around him. Control should be as precise as possible, but it shouldn't become an obsession to the crew. The control on the winches isn't so precise, either.

Dr. Smith: One of the limitations is in training the crews out there; they are just not keeping up. Do you think this will be a significant limitation to field operations in coming years?

Mr. Hughes: We all joke about divers, you know, their ignorance or whatever. But commercial divers, by and large, are pretty sharp guys. They are basically conscientious and they understand what they are doing. The problem is, to a large extent, available manpower on site. Suppose we're diving down to 600 feet. Those dives are generally conducted with 5 men on board the rig. The whole diving team is 5 men. Now, somebody has to be in the diving bell. So you end up with two divers in the bell, and a topside crew of 3 on the surface. Now those three men have to run the winch, switch the gases, communicate, keep the records, handle the umbilical, and explain to the visiting engineer what they are doing. So it probably isn't intelligence or lack of it, but just operating pressures to do many things at once that make it hard for them to keep up.

Visualize this diver on the deck of the barge, with his wristwatch turned so he can see it, tending the umbilical with one arm (which develops a big left arm) and running the winch with the other arm, and talking to the tool pusher on headphones. That's typical of how these dives are run. You just don't give that guy too many variables to have to fool with. He just can't do it.

The question whether to maintain a constant oxygen percentage or a constant $PO_2$ partial pressure comes up. At one time I thought this was important. Now that good instrumentation is available, I don't think it matters much anymore, as long as what you do is appropriate for the tables. Do you have any thoughts on that, Dan?

Mr. Wilson: We go with the percentage, generally, on short-duration dives, but we run saturations, basically, on partial pressures.

Dr. Hamilton: Agreed. We print the proper percentage on the tables, so the divers can read the analyzer directly.

Mr. Wide: Beyond 120 meters we use constant partial pressure. The reason for that is because when we are going deeper, even if we start off with a short-duration dive, we could end up in a saturation situation.

Mr. Hughes: Right. For any diving deeper than 400 feet now, we call for environmental control, whether it's a saturation dive or not. For example, for the "500 x-ray" schedule, the bottom mix is 7% oxygen which is then changed to 16% by transferring into the deck chamber. As soon as this schedule hit the field the guys took one look at it and said, "What happens if we get stuck on bottom and we've got a deck chamber with 16% in it?" So they started putting about 8% in the deck chamber at 300 feet or so, so that they would have a suitable saturation environment in the deck chamber, and if the dive was completed in less than 30 minutes, they could stay with the short-duration dive schedule.

Dr. Greene: Is the cost of mix-making equipment so economically impractical that you can't use it here?

Mr. Hughes: It's another complication. I think that's the main reason we've avoided it. Who's going to sit at the mix maker? The idea of optimizing decompression by using variable gas mixtures is theoretically a good thing, but from a practical point of view, the simpler the better.

Dr. Galerne: We use the mix maker, but not to supply the divers directly. We mix the gas in advance. We use two compressors and a mix maker.

Dr. Flynn: What I'm hearing so far is that it's a personnel problem, not a physiological problem. In other words, physiologists may give you an optimal schedule, but you're not willing or able to accept it because you don't have the trained manpower to implement it. Is this a matter of economics?

Mr. Hughes: Basically, it's a growth problem. If we have three men on a rig to begin with, one of the men is not very well trained at all, one of them is maybe moderately trained, and one of them should know what he is doing. If we add two more men, they fit in the first category. So now we have three men on deck who don't know what they are doing. And they can make more mistakes than one man does. They become part of the problem. And the quarters constraints on the rigs are just unbelievable. We couldn't put more men on most of them if we wanted to and could afford it.

Mr. Wilson: Mike, you should point out that we're not in trouble. We do the job reasonably well.

Mr. Hughes: But we can only do it if the procedures are relatively simple and straightforward.

Mr. Wilson: I don't agree that there is an economic advantage to the use of a mix maker.

Mr. Galerne: We are of different opinion. For example, the mix maker gives you the best possible table for a particular location.

Mr. Hughes: I want to emphasize one thing here--that the control of multiple inert gases is impossible on a rig, and you have to use one gas--one oxygen percentage--over a wide range of depths; optimization is tough to pull off in real life.

We'd like more independence; the idea of using air with just pure helium on top, mixed on site as a very practical procedure appeals to me.

Mr. Kenyon: We are developing for Three-X, the new Norwegian company, a set of tables to 650 feet which do just that. We're very excited about the idea.

Mr. Hughes: One more thing. Rig divers don't get to do conditioning or acclimatization dives. If you do 3 dives 3 days in a row in the lab, you may not be representing the operational situation very well.

The work rates vary quite a bit. On most of the dives that we've done at Duke, the work rate that we simulated exceeds what most of our intelligent divers say they would ever be willing to do on bottom, in an operational situation. They say no experienced diver would push himself that hard.

Dr. Behnke: What is your work?

Dr. Bennett: They run, I think, anywhere from around 0.6 up to 2.3 liters a minute.

Dr. Behnke: That's heavy. When you work in the field, it will be about 1 1/2 liters.

Mr. Hughes: I imagine that would be sort of a maximum.

Dr. Bennett: I think we ought to remember that at Duke we are working with the arms and not with the legs. Much previous work has been done with the legs on a bicycle--I was making odds you've never seen a diver bicycling around the ocean floor--let's get working with the arms instead of the legs.

Mr. Edel: One case where divers do use their legs is in pipeline walking.

Mr. Hughes: You can't really predict what sort of work you are going to do on a given dive.

To conclude, the matters that I consider important in decompression table development are:

1. Safety
2. Simplicity
3. Cost-effectiveness
4. Time efficiency

Dr. Flynn: Would you expand on time-efficiency? For instance, if you have a decompression procedure that's 8 days versus 10 days, is that the order of time-efficiency that you're talking about, or are you talking about a 50% saving in time?

Mr. Hughes: At 100 grand a day, I'll take two days anytime, but the point is, we can't sacrifice any of those other things to get it. If you do, then it's not worth the difference.

Dr. Flynn: Let's say you're not sacrificing anything and you have a 16-day operation and you can make it in 14 days. You would consider that to be a worthwhile goal?

Mr. Hughes: Absolutely. For example, in a derrick barge operation, they may not be able to begin work on some new location until they get the divers out. However, I'm convinced that this whole business about our decompression tables are better than your decompression tables hasn't made me any money in a long time. I don't think it really makes for competition; I think it's a fallacy. We want to do a good job for our clients, and a safe job for our divers.

Dr. Schreiner: Thank you, Mike. I appreciate your entering into the record this distillation of many years of commercial experience. I hope it will find its way into research programs.

## E. LOCKOUT SUBMERSIBLES: D. A. YOUNGBLOOD

Lockout submersibles or diver subs as Joe MacInnis and I prefer to call them, have been around for quite a while. They have been looked upon as toys. I think they may finally be coming into their own, particularly in the North Sea where they can be launched successfully and retrieved successfully, so they can work more than one rig.

### Background

To give you a short background on the history of lockout submersibles in the United States, DEEP DIVER was designed and built by John Perry and Ed Link, launched in 1967, and I believe it still holds a record for submarine lockouts at a 700-ft level in the Bahamas. A predecessor to this submarine, the PC-3, also participated in the great atomic bomb hunt off Palomares, Spain. Another Perry ship, SHELF DIVER, which was constructed in 1968, performed the first commercial submarine lockout that I am aware of, in an oil field from the platform Molly Brown in the Gulf of Mexico.

Despite interest and investment by several large aerospace and defense corporations in the period 1965 to 1970, commercial application of lockout submersibles was rare, with the exception of a few Government-supported operations associated with oceanographic research.

### Current and Future Uses

Now offshore petroleum exploration has rekindled interest in lockout submersibles, just as it has for commercial diving in general. As I said earlier, the North Sea will probably be the proving ground for diver subs, particularly in the inspection of deep water pipeline installations where the dangerous practice of so-called "live-boating" from the surface has been employed. This has now, I think, been proscribed by the new British North Sea Regulations. It is, in fact, impractical in depths beyond 250 feet, anyway.

In regard to compression and decompression requirements, in general, we are looking toward rapid compression and relatively short bottom times with the use of diver subs, mainly because of the systems constraints associated with gas supplies available on board, and secondarily, because of the type of mission which they will be associated with.

### Design Constraints

The main systems constraint of submersibles, particularly diver subs, is the strength, weight and safety which must be balanced against payload and performance. Aside from the external pressure loads to which all submersibles are subjected, divers obviously must withstand internal pressurization at least within the dive compartment. This requirement imposes an additional weight penalty not present in ordinary submersibles. DEEP DIVER, for instance, with an overall length of 23 feet, has a dry weight of about 13,000 pounds. The PC-5C, another Perry submersible which is similar in design but is not a lockout submersible, has an overall length of 22 feet and a dry weight of only 8,600 pounds. Submarines, like aircraft and other things, are basically bought "by the pound," with a little added in for the extra engineering. So, unless there is real economic justification for diver lockout capability, it is not worth the additional cost. That is what the next few years will tell us.

Gas Supply Problems

The gas supply situation is similar to that on offshore rigs, except that it is more acute. All submersibles require gas supplies for blowing ballast tanks, and they require metabolic oxygen for their occupants, but diver subs require additional gas storage capability for pressurization of the dive compartment and for providing breathing gas for the lockout diver.

If the decompression schedules employed require gas switching during compression-decompression, it complicates diver sub design and requires additional high pressure cylinders.

In addition to the operational gas requirements, there must be a gas reserve for recompression therapy in the event that the submarine cannot be recovered by the support vessel and mated to the Deck Decompression Chamber or cannot receive additional gas supplies by way of an umbilical from the support ship while lying alongside (As an aside, let me say that the problem of not being able to recover submersibles because of high sea states is a very severe and very common one.)

A few simple calculations will show that there are only two practical answers to these gas supply problems. One is to transport divers under pressure from a DDC or an underwater habitat at a practical saturation storage depth, thus conserving compression gas, and the second is to employ closed-circuit diving apparatus for the lockout diver with a lifeline umbilical to the diver sub to use as an open-circuit, demand-type emergency system.

This, incidentally, is the type of system which we now employ on the submersible JOHNSON-SEA-LINK and have used in two successful open-sea trials of the 500-ft decompression schedules which we have developed at Duke University.

Our system is a closed-circuit rebreather, the Biomarine CCL-1000, modified to a KMB-9 mask as the primary breathing mode. This is not a free-swimming diver; he is tethered with a hard wire communication and a hose to our primary bottom mix gas supply. His primary breathing support is from a closed-circuit system, so if all goes well he requires no gas other than the pressurization gas. Should the closed system fail, he simply has to open a valve and he is on open circuit, direct from the submarine, and is a tethered hose diver.

Compression in these dives utilizes a 7% oxygen-helium mixture from an aluminum sphere container with approximately 1700 cubic feet of gas at 1900 psi, which after blowdown to 800 feet or so would leave only a few minutes of breathing time on open circuit. This is why we have chosen the closed-circuit route.

The decompression schedules we used did require a gas shift at around the 300-ft level to 16% oxygen-helium during decompression. In actual field practice this is achieved by adding oxygen to the chamber atmosphere upon commencing decompression or leaving the bottom, at a rate which will achieve the desired partial pressure upon reaching the gas shift depth. This additional process is monitored directly on an oxygen-partial pressure sensor, with readouts in the forward compartment where the dive supervisor or dive director rides in a 1-atm environment. The supervisor has an override on the oxygen supply so that they cannot exceed this. In the event of failure of the system there is a built-in breathing system which has 16% helium-oxygen available.

Power Supply

Now the next operational constraint or systems constraint in submersibles is power. An inadequate supply of power has been the primary system constraint on the design of all submersibles, whether diver subs or not. At present, JOHNSON-SEA-LINK carries about 1200 ampere hours (A·h) of lead acid batteries. This is ample for the type of scientific mission which we contemplate, but I really doubt that lead acid batteries would be adequate for efficient operation on longer cold water commercial operations in the North Sea.

Communications

Communications is a problem for us, as they are for everyone else, except that it is a little more complex. We have a round robin communication system between the diver, the diver-tender inside the submarine dive compartment, and the dive supervisor in the forward compartment.

There is no suitable helium speech unscrambler for use on submersibles. As Mike Hughes has said, there are several unscramblers which will work very well. My favorite at the moment is the Marconi Space and Defense System Unit. However, unscramblers have not been miniaturized to the extent that we can install them in a submarine. They work fine in a laboratory. They may work offshore in a control van, but to use them in a submersible they have to be small as well as rugged.

## Life Support

In life support the first thing to consider, of course, is oxygen, and the best method of oxygen replenishment at present seems to be simple addition of gaseous oxygen by way of a flowmeter. It must be remembered that a flowmeter only gives a relative readout under a pressurized situation and not an accurate one. This is backed up with an oxygen sensor with readouts in two separate places. Reliable automatic systems are available and we are installing an automatic metabolic oxygen make-up system in our second submersible.

As far as carbon dioxide is concerned, lithium hydroxide seems to be the only practical answer to carbon dioxide scrubbing in helium-oxygen atmospheres and cold waters; when you are speaking of such atmospheres, cold is 70° F. The efficiency of lithium hydroxide remains remarkably stable at temperatures of down to about 35° F. This has been tested by Dr. Sin Wong of the Harbor Branch Foundation.

## Diver Heating

Diver heating is a more severe problem in lockout submersibles. The usual approach to this problem which Mike has mentioned is the open-circuit hot water suit currently used in commercial diving, and, of course, this is impossible in a small submersible. A closed-circuit liquid heated suit has potential, but again, power availability limits its practicability. Off-the-shelf electrically heated underwear combined with constant-volume dry suits could provide adequate heating if power reserves were sufficient, but they usually are not. As you know, conventional wet suits or even dry suits with wool underwear are almost worthless in a hyperbaric helium atmosphere.

The most practical answer seems to be the non-compressible wet suit, perhaps augmented by closed-loop liquid heating elements over body areas subject to the greatest heat loss, such as the head, neck, and hands. Power could be furnished by the submersible batteries or by a controlled exothermic chemical reaction in a diver-carried unit.

## Physiological Monitoring

The psychological makeup of most divers makes them prone to push themselves beyond their physical capability. The monitoring of physiological parameters is essential. This should consist, at the very least, of electrocardiograms, and the monitoring of breathing rate and body temperature. Off-the-shelf technology is available and it is a simple but costly matter of biomedical engineering to adapt these devices to submarine lockout operations.

## Emergency Life Support

This should be mentioned separately to stress the hidden pitfalls of relying upon the primary life-support system as the emergency life-support system as well. Insofar as practicable, the emergency system should be separate and should not require power or heating from external sources.

Aboard JOHNSON-SEA-LINK II we are installing a closed-circuit life-support system with independent carbon dioxide scrubbers and automatic metabolic oxygen makeup. In the dive compartment, the closed-circuit diving rig can act as an additional back-up system.

## Handling Systems

Of all the operational constraints affecting submersible operations, launch and retrieval are the most limited. This is basically a problem in seamanship, and it is a credit to the latest experimenters in this field, the Vickers group in the United Kingdom, that they turned to the marine group with experience in handling heavy gear and high seas from relatively small vessels, the deep water fishermen, particularly the stern trawlers who operate in the North Atlantic.

They adapted, or adopted, a stern trawler launch and retrieval system for their submersibles. This system, I am sure, is less than perfect, but it is a promising approach, and the North Sea is the ideal proving ground.

## Training

I am acutely aware, as these other gentlemen are, of the training problem facing commercial diving today. I find it analogous to our entry into World War II, when we had a nucleus of professional military men who had to train thousands upon thousands of non-military people when they were entering a very severe conflict.

If lockout submersibles are going to be used, the training requirements are even more severe than for other divers. I would like to stress one aspect that is often overlooked, and that is the training of the dive compartment team. It will be easy for diving operators to consider diver subs as nothing more than highly maneuverable diving bells, and they may be inclined to select divers for such operations on the basis of their bell-diving experience. This, I feel, is a mistake. Diver subs require teamwork among the entire crew, and the use of closed-circuit diving equipment demands a respect for its limitations. The general commercial diver does not have the proper appreciation, background, or most important, respect, for this type of equipment. The diver population which does have this experience, however, consists of ex-clearance divers and underwater demolition team members who are familiar with semi-closed and closed-circuit limitations, and are also psychologically adapted to small unit teamwork operations.

I would like to give you a practical example of what is required in the decompression following a lockout dive, to show why I stress the teamwork aspect. When the end of the dive period comes, the diver has to come back to the submersible. He usually does not have the same room to maneuver in locking back in as a diver from a conventional bell does, because the submarine has to sit heavy on the bottom. He may have a 2.5-ft space to re-enter the bell with his backpack on, and because he is not on umbilical, he is under a time constraint, unless he is in a saturation mode. He has to get in, secure his inner hatch, and be in perfect coordination with the pilot of the submersible, who leaves the bottom at a rapid rate while the diver supervisor commences the decompression. If at any phase of this operation someone falters, the consequences can be dire.

# SESSION III. SUB-SATURATION DECOMPRESSION PROCEDURES

# III. SUB-SATURATION DECOMPRESSION PROCEDURES

## A. DEVELOPMENT AND TESTS OF HELIOX DIVES IN EXCESS OF 100 METERS: K. G. MUELLER AND H. OSER

In recent years short-term dives in the range of 100 - 200 meters have become increasingly important due especially to oil drilling in the shelf areas of the continents. For these dives the necessary decompression times are several times longer than the bottom times, and thus become a limiting factor in the economical application of surface diving. As a solution to this problem, saturation dives with excursions to greater depths have been introduced. A reduction of the necessary decompression time in this range of depths would increase the limit of economical surface diving to greater depths. In this paper we investigate a possible reduction of decompression time by changing the type of decompression profile; part I covers theoretical development and analysis, part II the planning, performance, and results of the test dives.

A comparison of existing, successful heliox dives in excess of 100 meters reveals the following two characteristic types of decompression profiles (see Fig. IIIA-1): type m (medium pressure reduction) which is represented, for example, by the heliox tables of the Royal Navy Physiological Laboratory (5), and is characterized by a medium pressure decrease and by disagreeably long decompression times (Table IIIA-1). Type f decompression profiles (fast pressure reduction) were proposed by Cabarrou and were tested by Hartmann and Cabarrou in 1968 at our Institute. The data and experimental results are presented in the paper of Ruff and Mueller (6). Type f, in contrast to type m, shows a fast initial pressure reduction (Fig. IIIA-2) and a relatively short decompression time (Table IIIA-1). In Fig. IIIA-2, we have chosen a logarithmic scale for the absolute external pressure, $P_{ex}$, to make evident the relative pressure decrease.

In this state of development, the need for a theoretical concept to calculate type f diving profiles is obvious. In constructing this concept, the few type f test dives and the material which is condensed in systems of M values (9, 7) have to be taken into account.

We used the following steps in this research program. First, we developed a theoretical basis for type m profiles by constructing m values for a set of 15 compartments (see Ruff and Mueller [6]). The main feature of this program was that it distinguished between fast tissues where silent bubbles were allowed to occur temporarily and slow tissues where any bubble formation leads to bends or other decompression symptoms. Next, we approached the transition from the m profile to the f profile by performing a series of experimental dives (3). A detailed analysis of the f profiles will be published elsewhere. The main problem was a suitable choice of the first decompression stop. With these experimental results as background, the present investigation was started. A detailed comparative analysis

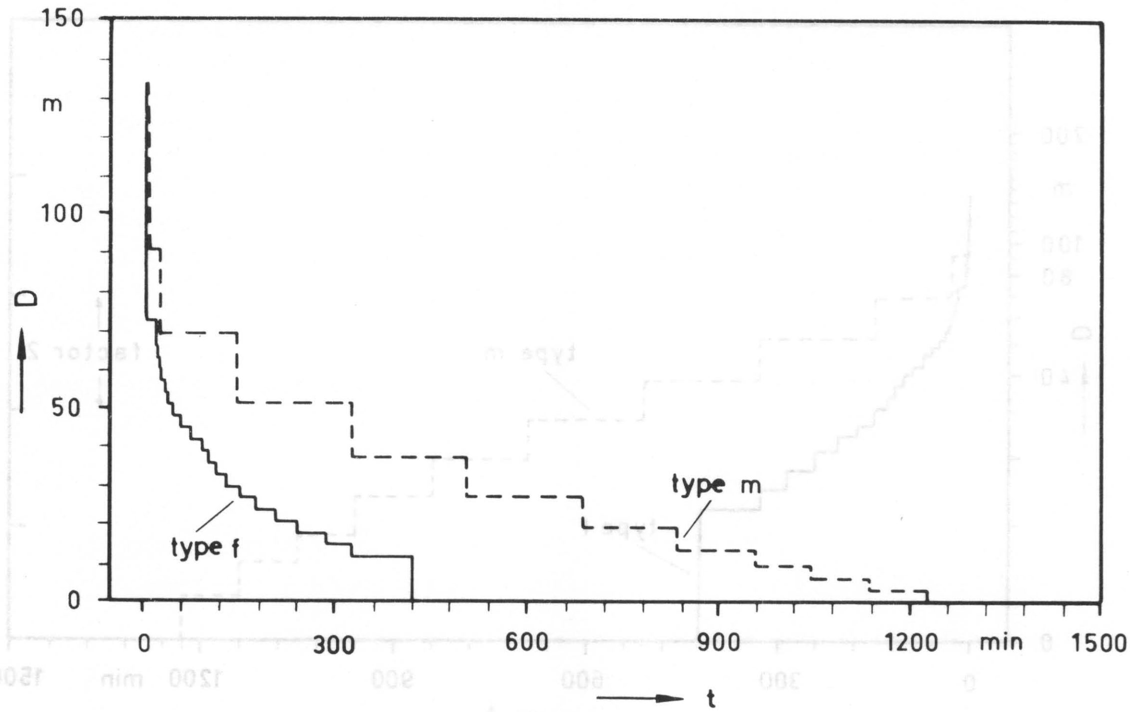


Fig. IIIA-1. Representative decompression profiles of type m (4) and type f (test dive No. 7 of this paper) in a linear scale; D: depth; t: time after bottom stay.

of type m and type f profiles provided a theoretical basis for the construction of improved type f profiles. The proposed 135 m/60 min and 150 m/30 min heliox dives have been tested successfully in a dry chamber and a wet pot.

## Comparative Analysis of the Profiles

Theoretical Concept. New profiles can be constructed by interpolation or extrapolation of tested profiles. This theoretical procedure requires a model which is more successful the closer it comes to the actual physiological processes in man during decompression. The following physiological elements enter into such a model.

1) the physiological process which is related to decompression symptoms, e.g., formation of bubbles in the tissues;
2) the physiological quantity which controls this process, e.g., the total gas pressure in the

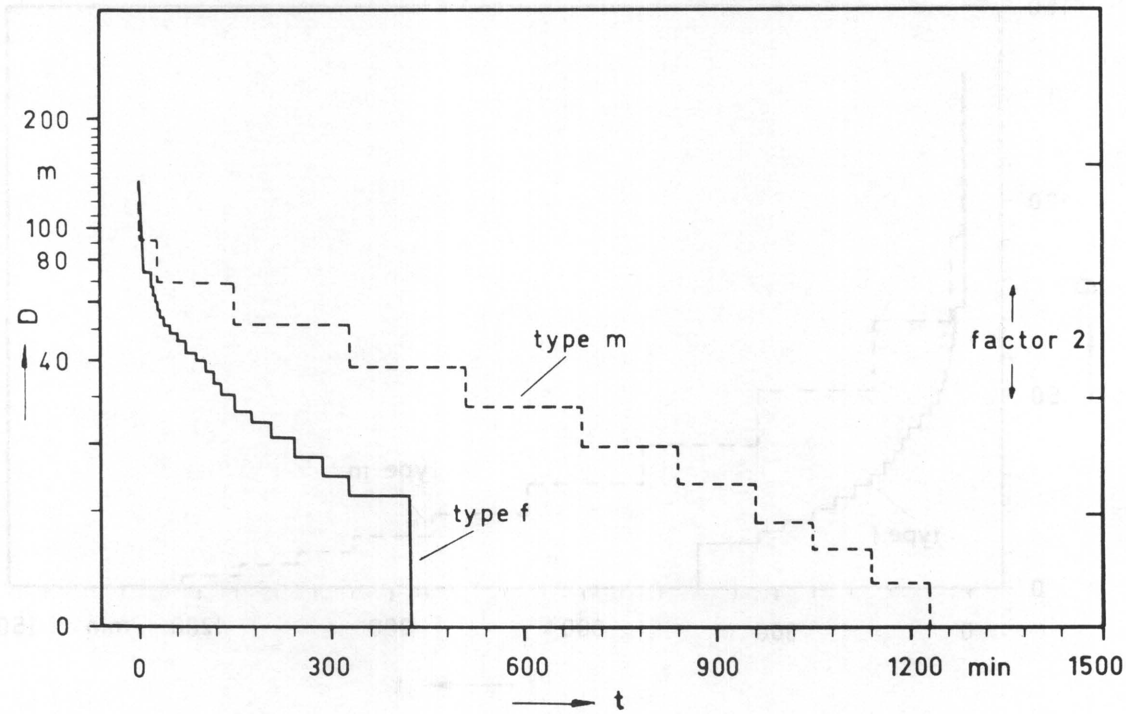


Fig. IIIA-2. Representative decompression profiles of type m (4) and type f (test dive No. 7 of this paper) in a scale, logarithmic in the absolute external pressure $P_{ex}$; D: depth; t: time after bottom stay.

tissues relative to the external pressure;
3) the limiting value of the controlling quantity;
4) the mechanism of the gas transport into the tissues, e.g., diffusion or perfusion

On the basis of these elements a model can be developed which allows the calculation of the controlling $P_{tis}/P_{ex}$ pressure-quantity during a decompression. The limiting values of this quantity can be ascertained by an analysis of successful dives.

Since Haldane's (4) original concept of the 2:1 ratio, several models have been proposed. Most of these models are equivalent to Workman's M value model (9) and only differ in the numerical values of the half times of the independent tissues (compartments) and the M values. In the following analysis we introduce a generalized Workman model with three typical sets of representative compartments.

Table IIIA-1. Typical decompression times (min) for dives in excess of 100 m as a function of depth (m) and bottom time (min)

| Depth | Bottom Time | Decompression Time | Labs |
|---|---|---|---|
| 150 m | 30' | 966' | EDU-70 (8) |
| 150 m | 36' | 308' | DFVLR-71 (6) |
| 135 m | 60' | 1252' | EDU-70 (8) |
| 135 m | 60' | 1230' | RNPL-70 (5) |
| 120 m | 60' | 1130' | EDU-70 (8) |
| 120 m | 60' | 991' | EDU-70 (8) |
| 105 m | 60' | 792' | EDU-70 (8) |
| 105 m | 60' | 751' | RNPL-70 (5) |
| 100 m | 63' | 298' | DFVLR-71 (6) |
| 100 m | 64' | 255' | DFVLR-71 (6) |
| 100 m | 60' | 250' | DFVLR-71 (6) |

No decision has been made about the actual mechanism of gas transport in this model, especially with regard to diffusion or perfusion. Our choice of compartments is demonstrated in Fig. IIIA-3, where each compartment is characterized by its half times for helium and nitrogen. Set I was introduced by Schreiner (7) and can be explained on the basis of a perfusion mechanism. In set II, only those compartments are taken into account which show identical half times for the gases He and $N_2$. Set III can be derived on the basis of a diffusion mechanism; the ratio of the half times of He and $N_2$ was taken as 1:2.7, e.g., Buehlmann (1). In a qualitative discussion we simply refer to fast tissues, $\tau_H$ (He) $\leq$ 20 min; medium tissues, 25 min $\leq \tau_H$ (He) $\leq$ 75 min; and slow tissues, $\tau_H$ (He) $\geq$ 80 min, instead of compartments.

Analysis of the profiles. An analysis of a decompression must consider several parameters which may change values during the course of decompression. We distinguish external parameters as external pressure, $P_{ex}$; gas composition; and work load, which may be set by a suitable operation of the gas supply system or by the program; and internal parameters as total gas pressure in a specific tissue, $P_{tis}$, or supersaturation $\Delta P = P_{tis} - P_{ex}$, or saturation ratio $P_{tis}/P_{ex}$, which in most cases cannot be measured directly but can only be calculated by a model.

Table IIIA-2. External parameters of representative type m and type f profiles.

| Type m Profile | Breathing Mixture | Compression Rate | Decompression Rate | First Stop | Final Stop | Total Decompression time |
|---|---|---|---|---|---|---|
| 0-135 m | 0.5 atm $O_2$, balance He | | | | | |
| 135 m | 0.5 atm $O_2$, balance He | | to first stop: 15 m/min | at 91 m | | |
| 135- 92 m | 0.5 atm $O_2$, balance He | 15 m/min | between stops: 5-15 m/min | (27 min) | at 3 m | 1230 min |
| 91 m | 0.5 atm $O_2$, balance He | | to surface: 3 m/min | | | |
| 69- 13 m | 0.5 atm $O_2$, balance He | | | | | |
| 9- 0 m | 21.0% $O_2$, 79.0% He | | | | | |
| **Type f Profile** | | | | | | |
| 0- 10 m | 21.0% $O_2$, 79.0% He | | | | | |
| 10-135 m | 8.4% $O_2$, 91.6% He | | | | | |
| 135 m | 5.4% $O_2$, 94.6% He | 0- 10 m: 10 m/min | to first stop: 9 m/min | at 73 m | | |
| 135- 74 m | 8.4% $O_2$, 91.6% He | 10-135 m: 40 m/min | between stops: 6 m/min | (15 min) | at 12 m | 428 min |
| 73 m | 28.8% $O_2$, 71.2% He | | to surface: 3 m/min | | | |
| 72- 42 m | 28.8% $O_2$, 71.2% He | | | | | |
| 39- 18 m | 49.1% $O_2$, 50.9% He | | | | | |
| 15- 0 m | 100.0% $O_2$ - | | | | | |
| 12 m | 5 min, air | | | | | |

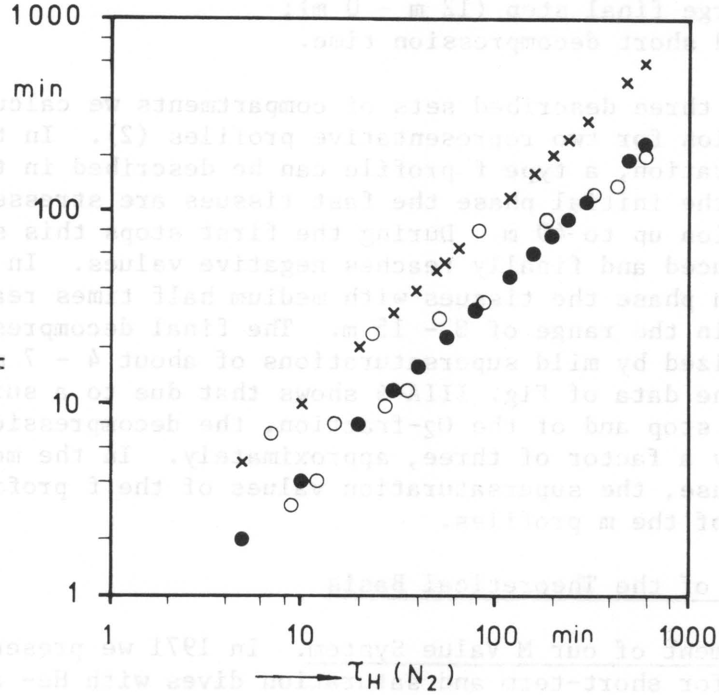


Fig. IIIA-3. Half times $\tau_H$ for He and $N_2$ for the three sets of compartments used in our analysis; o: set I, perfusion transport; +: set II; •: set III, diffusion transport.

Especially with regard to profiles of type f, we distinguish three phases of a profile:

1) the initial decompression phase where the fast tissues are stressed by high values of the supersaturation and where the slow tissues are still loaded;
2) the medium decompression phase where the tissues with medium half times are controlling;
3) the final decompression phase where the slow tissues are primarily controlling.

Comparison of the profiles. In Table IIIA-2 we have listed representative values of the external parameters of type m and type f profiles. A comparison reveals the following characteristic features of a type f profile:

1) fast compression rate (30 - 50 m/min);
2) extreme first decompression step (≃ 2:1);

3) medium initial decompression rate (9 m/min);
4) recreation phase at the first stop (15 min);
5) high values of the $O_2$-fraction:
   30% for 73 m $\geq$ D $\geq$ 42 m,
   50% for 39 m $\geq$ D $\geq$ 18 m,
   100% for 15 m $\geq$ D $\geq$ 0 m;
6) large final step (12 m - 0 m);
7) and short decompression time.

For the three described sets of compartments we calculated the supersaturation for two representative profiles (2). In the language of supersaturation, a type f profile can be described in the following way: during the initial phase the fast tissues are stressed by a high supersaturation up to 40 m. During the first stops this supersaturation is reduced and finally reaches negative values. In the medium decompression phase the tissues with medium half times reach supersaturations in the range of 8 - 15 m. The final decompression phase is characterized by mild supersaturations of about 4 - 7 m. A comparison of the data of Fig. IIIA-4 shows that due to a suitable choice of the first stop and of the $O_2$-fraction, the decompression time can be reduced by a factor of three, approximately. In the medium and the final phase, the supersaturation values of the f profiles are far below those of the m profiles.

## Construction of the Theoretical Basis

Improvement of our M Value System. In 1971 we presented a system of M values for short-term and saturation dives with He- and $N_2$-mixtures (6). The main idea was that the allowed supersaturations were constant for slow tissues and increased with depth for the other tissues in a limited range of depths. Short-term test dives (6) in 1972 in the range of 135 - 150 m led to the following:

1) the 600-min $N_2$ tissue can be neglected;
2) a slight increase of the supersaturation with increasing depth can be allowed for the slow tissues;
3) the surface value of the allowed supersaturation has to be reduced for the slow tissues;
4) the depth and duration of the first stop has to be chosen independently of the system of M values;
5) during the final phase the supersaturation of the fast tissues is not allowed to become positive again.

New Theoretical Frame. An ideal situation would be provided by a theoretical basis which could be applied to any dive with arbitrary duration and depth. Our 1971 system of M values covers the range of depths up to 100 m. Due to the results of our test dives in 1972 it appears doubtful whether a uniform system of M values can be found which includes the previous one and is applicable as well in the range in excess of 100 m. Thus, we have concentrated on finding a basis for a limited range of applicability, which could serve mainly as a tool for interpolation or extrapolation of diving tables.

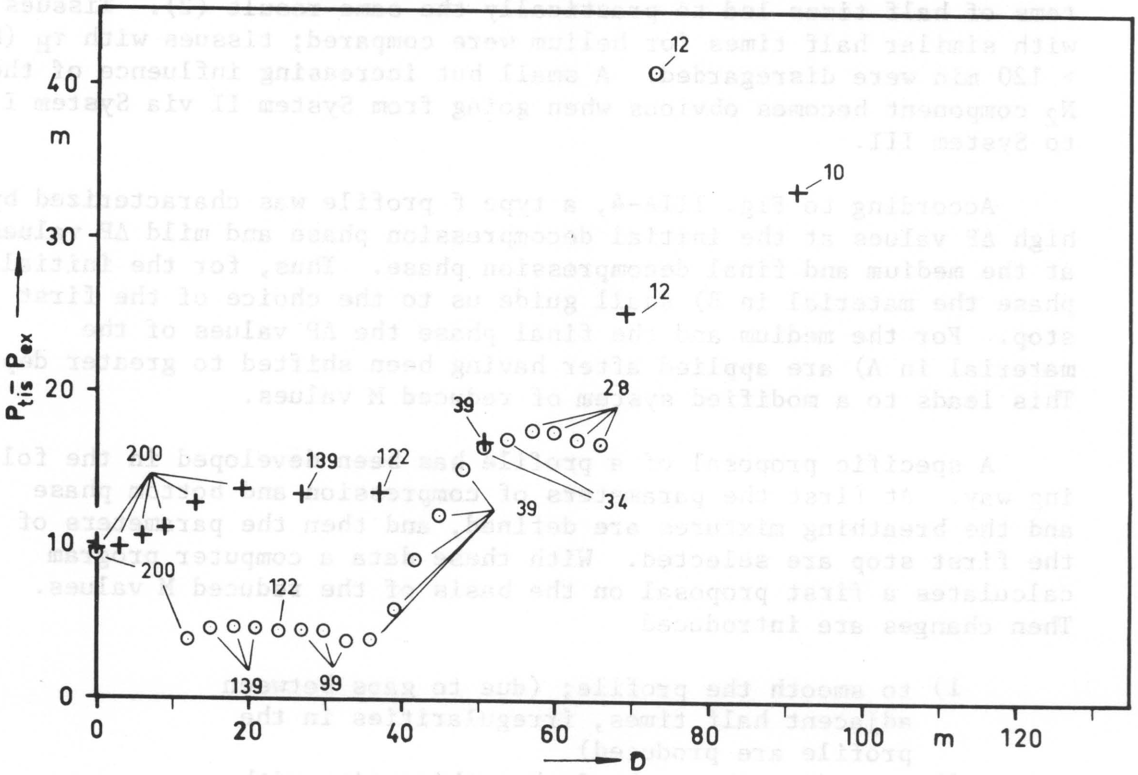


Fig. IIIA-4. Significant supersaturation values $P_{tis} - P_{ex}$ as a function of depth (D) for 2 representative profiles of Fig. IIIA-1. Compartments are characterized by their half times, $\tau_H$ in He. +: type m profile; o: type f profile

In constructing the new theoretical frame we have to regard the following material:

A) In our 1971 system of M values, the maximum $\Delta P$ values were

| | |
|---|---|
| 32 m - 17 m | for the fast tissues, |
| 16 m - 10 m | for the medium tissues, |
| 9 m - 5.5 m | for the slow tissues |

These maximum values are applicable for depths in excess of 20 - 25 m.

B) The successful type f dives described in our paper (6) show the maximum $\Delta P$ values during the initial decompression phase

| | |
|---|---|
| up to 33 m | at approx. 50 m depth, |
| up to 39 m | at approx. 75 m depth, |
| up to 42 m | at approx. 100 m depth |

C) An analysis of heliox profiles for the three described systems of half times led to practically the same result (2). Tissues with similar half times for helium were compared; tissues with $\tau_H$ (He) > 120 min were disregarded. A small but increasing influence of the $N_2$ component becomes obvious when going from System II via System I to System III.

According to Fig. IIIA-4, a type f profile was characterized by high $\Delta P$ values at the initial decompression phase and mild $\Delta P$ values at the medium and final decompression phase. Thus, for the initial phase the material in B) shall guide us to the choice of the first stop. For the medium and the final phase the $\Delta P$ values of the material in A) are applied after having been shifted to greater depths. This leads to a modified system of reduced M values.

A specific proposal of a profile has been developed in the following way. At first the parameters of compression and bottom phase and the breathing mixtures are defined, and then the parameters of the first stop are selected. With these data a computer program calculates a first proposal on the basis of the reduced M values. Then changes are introduced

1) to smooth the profile; (due to gaps between adjacent half times, irregularities in the profile are produced)
2) to redistribute the $O_2$-breathing time with respect to an optimal inert gas exchange;
3) to put air-breathing intervals into the $O_2$-breathing time.

Profiles developed in such a way automatically show negative $\Delta P$ values for the fast tissues in the final phase, as was postulated previously. By the theoretical method introduced in this section we offer a frame for the construction of a profile. Additional information about type f profiles has to be filled in. Some pieces of this information can be gained through unpublished tables; other pieces have to be achieved by test dives.

## Proposed and Tested Profiles

Our choice of external parameters is described in Table IIIA-2. In contrast to the type m profile, we have introduced the four breathing mixtures 90/10, 70/30, 50/50, and 0/100 $He/O_2$. Helium was the only inert breathing gas used, thus avoiding the problem of switching from $N_2$ to He at recompression. A computer calculation and a succeeding refinement, described above, led to proposals of 110 m/60 min, 135 m/60 min and 150 m/30 min test dives. A series of test dives resulted in slightly modified tested profiles [(2) and Table IIIA-3].

Table IIIA-3. Survey of 110 m/60 min, 135 m/60 min, and 150 m/30 min test dives.

| Date | Depth, m | Compression time, min | Bottom time, min | Decompression time, min | Number of Subjects | Remarks |
|---|---|---|---|---|---|---|
| 11.11.74 | 110 | 6 | 54 | 347 | 2 | no bends |
| 12.11.74 | 110 | 3 | 57 | 265 | 2 | 1 type-I bends after 45'; recompression |
| 13.11.74 | 135 | 6 | 54 | 433 | 2 | 1 type-I bends after 7h; recompression |
| 15.11.74 | 135 | 4 | 56 | 434 | 2 | 2 type-I bends after 30'; recompression |
| 16.11.74 | 135 | 3 | 57 | 526 | 2 | no bends |
| 18.11.74 | 135 | 5 | 55 | 452 | 2 | no bends |
| 19.11.74 | 135 | 3 | 57 | 428 | 2 | no bends |
| 02.12.74 | 150 | 4 | 26 | 319 | 2 | no bends |
| 03.12.74 | 150 | 3 | 27 | 314 | 2 | no bends |
| 05.12.74 | 150 | 5 | 25 | 300 | 2 | no bends |
| 21.01.75 | 150 | 5 | 25 | 300 | 2 | no bends |
| 23.01.75 | 135 | 4 | 56 | 428 | 2 | no bends |
| 24.01.75 | 135 | 5 | 55 | 428 | 2 | 1 type-I bends after surfacing; recompression |
| 25.01.75 | 150 | 5 | 25 | 303 | 2 | no bends |
| 02.04.75 | 150 | 3 | 27 | 286 | 2 | no bends |

Summary

We have differentiated between two types of decompression profiles, type m, with a medium pressure reduction and long decompression times, and type f, with a fast pressure reduction and a short decompression time. Except for the initial decompression phase, the $\Delta P$ values of the f profile are smaller than those of the m profile.

Our analysis of available diving information has led to a basis for the construction of new type f profiles. Heliox dives (110 m/60 min, 135 m/60 min, and 150 m/30 min) have been proposed and tested in a dry chamber and a wet pot. A series of successful and slightly modified profiles for 135 m/60 min and 150 m/30 min exist. They may provide a starting point for the introduction of type f standard tables.

A type f profile has the advantages of

1) short decompression time;
2) no occurrence of vestibular or central nervous system symptoms in the event of decompression sickness;
3) easy treatment if decompression sickness occurs;
4) and non-stressing decompression

There are possible problems connected with

1) high compression rates;
2) high $O_2$-partial pressure; and
3) high initial pressure reduction which may lead to silent bubbles.

Bone necrosis must especially be considered. An upper depth limit for the applicability of type f profiles may occur when medium tissues become governing during the first decompression phase. Oxygen partial pressure may set another upper limit.

A comparison of the $\Delta P$ values of representative type m and type f profiles of Fig. IIIA-4 indicates that for the f profile the $\Delta P$ values are far below the limiting values. On the other hand, the test dives reveal that the $\Delta P$ values are close to the limiting values. This discrepancy can be explained either by a change of the half times due to a high $O_2$ fraction which was neglected in our calculation of the $\Delta P$ values, or could indicate that the $\Delta P$ values and thus the M values alone are no longer controlling.

Discussion

Dr. Hills: I was interested in your last step of about 10 meters. Since you're using Haldane for calculating your supersaturation, how do you justify coming up from 10 meters?

Dr. Mueller: We have a basis of M values in calculating it. But in addition, we have some diving information which cannot be put into

M values, and additional information which is apart from M values. From the standpoint of evaluating profiles with M values, we see that it is not possible to transfer M values from a profile of a certain type to another profile.

Dr. Behnke: What difference does it make, for your diver to breathe so much pure oxygen?

Dr. Bennett: Oxygen is probably the key. I think here that we are in line with many tables in the commercial field today, if we use a hold to pump in a great deal of oxygen in the last stop. This in effect is performing an oxygen treatment on bubbles generated deep.

Mr. Kenyon: In reference to the possibility of an excursion after doing a final decompression hold at 10 meters (equivalent to 30 fsw), as long as you clear what we hypothesize as an M value, there is no reason why you can't jump up this way.

Dr. Mueller: There's another point in going from 10 meters to surface. If there were some problems from bends that might occur, then this would force it. So in doing a test dive it might be a good check on possible bends.

Dr. Bennett: Your depths have been to your advantage. With the kind of rate you suggest, you could run into a bends problem early, but perhaps the high oxygen you are using in the deeper depths helps you out. I certainly feel that if you were to promote this to greater depths that you would eventually run into bends with this kind of profile.

Dr. Mueller: It might be.

Dr. Hempleman: What sort of bends rates are you getting?

Dr. Oser: There were from these 14 dives, four mild bends in the knees. We got a bit weary of having bends. First of all, because we were asked not to, and second, if you have a long therapeutic time, you have not ultimately saved any time.

Third, we are worried that by giving bends we may be creating chronic problems. We treated it in a very cavalier fashion, to begin with. "So what, we get a bend, and we cure it." This attitude began to change a bit and we wondered whether perhaps we were doing a disservice to the diver by having this attitude.

Mr. Mueller: With respect to the bends I may add that the first bends occurred during the developing process and once a certain table was installed there were ten dives with only one case of bends. That means the other bends occurred during the developing of the table.

Dr. Bennett: I just wanted to say that one doesn't want to get misled; if you are going to test tables, you're going to get bends, and there's no other way of doing it. You can take the goat, the guinea pig or the night owl that Brian Hills uses, but eventually you just have to test the tables on man and it's likely your man is going to get a bend. In the short term that's unfortunate, but in the long term it's going to be to the advantage of the industry.

Dr. Hempleman: We are pioneers of the bend-no-bend approach to defining the problem. We were of the impression that if you end up with something which is prone to give you X percent bends, this is a satisfactory answer as long as the bends are mild.

Dr. Bennett: I think perhaps this is not a good answer. Let me say finally that the majority of the people in this room would only accept a table if it has <u>no</u> bends and that a table with bends is not a satisfactory table. I think that is what Dr. Hempleman is trying to point out.

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The material on which this paper is based has been reported by P. Cabarrou et al. (2).

## REFERENCES

1. Buehlmann, A. A. The use of multiple inert gas mixtures in decompression. In: The Physiology and Medicine of Diving and Compressed Air Work, edited by P. B. Bennett and D. H. Elliott. London: Balliere Tindall, 1969.
2. Cabarrou, P., U. Finkeldey, H. D. Fust, H. Krekeler, K.-G. Mueller, and H. Oser. Development and tests of heliox dives in excess of 100 m. Part 1. Bonn-Bad Godesberg: Deutsche Forschungs-und Versuchsanstalt fuer Luft-und Raumfahrt, Instituet fuer Flugmedizin, Report 75-48, 1975.
3. Fust, H. D., K. G. Mueller, H. Oser, and S. Ruff. Development of decompression tables for short term 100-200 m dives. Foervarsmedicin 9:502-506, 1973.
4. Haldane, J. S., and J. G. Priestley. Respiration. New Haven: Yale Univ. Press, 1935.
5. Royal Navy Physiological Laboratory. Oxy-Helium Diving Tables, 1970.
6. Ruff, S., and K. G. Mueller. Experimentelle and theoretische Untersuchung des Drukfallproblems. Deutsche Luft-und Raumfahrt Report 71-48, 1971.
7. Schreiner, H. R. Advances in decompression research. J. Occup. Med. 11:229-237, 1969.
8. Summitt, J. K., and R. W. Crowley. Report of experimental dives for Sealab III surface support decompression schedules. Res. Rept. 15-70. Washington: US Navy Exp. Diving Unit, 1970.
9. Workman, R. D. Calculation of decompression schedules for nitrogen-oxygen and helium-oxygen dives. USN Exp. Diving Unit Res. Rept. 6-65, 1965.

## B. HELIUM-OXYGEN DIVING IN THE SWEDISH NAVY: A. MUREN

Work on heliox diving was started in the Swedish Navy ten years ago, with the intention of developing a method for open-sea dives to 150 meters with a bottom time of at least 30 minutes, mainly for submarine rescue purposes. It has taken more time than we expected to achieve this aim, due to physiological as well as technical difficulties. As for the problem of decompression tables, it should be pointed out that instead of computing our own tables, we have adopted the rather cumbersome method of testing and modifying tables obtained from other sources. We started with the US Navy tables, and during the period 1964-66 we made 40 man dives to 100 m/60 min. The decompression time was 260-300 minutes, and the incidence of bends was nearly 20%, although the $PO_2$ during exposure was 1.5-2.0 ATA. By 1969 the decompression time had been increased to 489 minutes and a series of 56 man dives to 100 m/60 min in the wet chamber gave a bends rate of 9%. The following 30 open-sea dives from HMS Belos did not give rise to any decompression symptoms. We were, however, hesitant to go to greater depths according to this procedure.

The decompression schedules presently used, for 100- as well as for 120- and 150-meter dives, are based on tables received from Buehlmann in 1970. They were subject to minor modifications on the basis of our previous experience. The principle of decompression with air was considered to be an obvious advantage, as compared with previous tables, especially for long decompression periods. A series of 54 man dives to 100 m/60 min in the wet chamber, with a total decompression period of 460 minutes, resulted in 7% bends. These tables have been used for open-sea dives for 3 years, with no complications so far, i.e., no bends. Of course, in the lab divers performed hard work in cold water, while no work was done in the open sea.

During 1972-73, 20 man dives to 120 msw/60 min were made in the dry chamber according to the following pressure profile (Fig. IIIB-1). These dives did not give rise to any decompression symptoms. When this schedule was tried, however, in open-sea dives, we had one case of bends among six dives.

During 1974, 20 dry chamber dives were made to 150 msw/60 min (Fig. IIIB-2). Instead of using pure heliox 90/10 during exposure, as during the previous series, pressurization was made with air to 20 meters, after which helium and oxygen were added. The relation between the three gases at depth was 76% He, 15% $N_2$ and 9% $O_2$. The total decompression period was 960 minutes. Compression speed during these dives, as well as during those to 120 meters, was 10 m/min. This series resulted in 3 cases of bends, all of which were located in the knee, and appeared 5-7 hours after the end of decompression. The symptoms were quite mild, and responded quickly to treatment with USN Table 5 (oxygen, 135 minutes). The

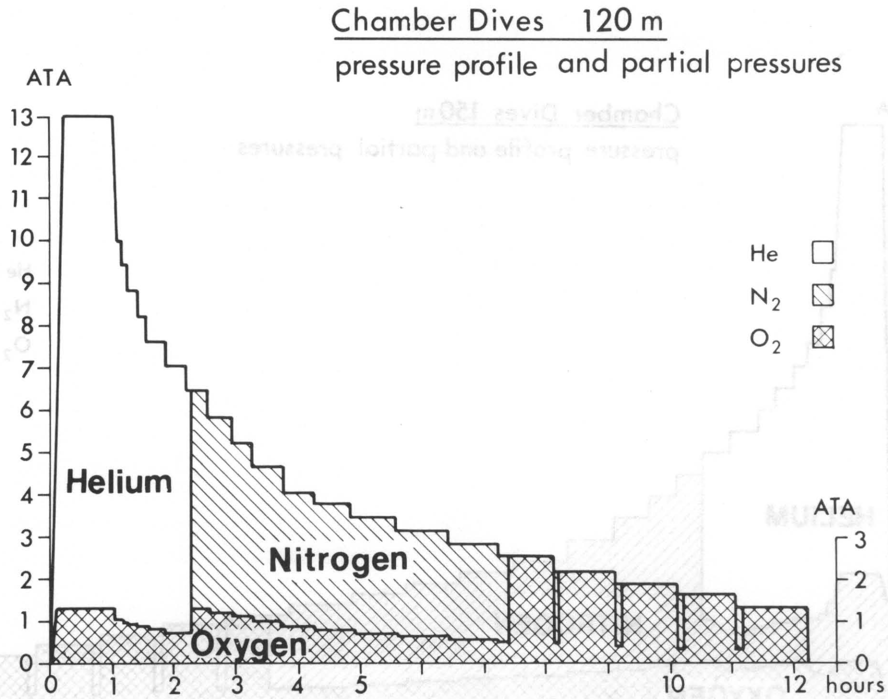


Fig. IIIB-1. Different gas components are presented separately. Actual gas mixture used during first phase (13-6.4 ATA) was heliox with 10% oxygen; during second phase (6.4-2.5 ATA), air. During last phase (2.5-1 ATA) pure oxygen was breathed, with 10-min interruptions with air each hour.

high oxygen exposure during decompression, however, did give rise to airway symptoms in the majority of the cases. This took the form of discomfort during deep inspiration, coughing, and in about 35% of the cases, pronounced symptoms including substernal pain (Fig. IIIB-3). Comparison of the peak flow values before and after the dive did not reveal any differences for those with none or moderate symptoms, whereas those with pronounced symptoms showed an average decrease of 13%. The total exposure was about 1200 UPTD units, with symptoms appearing at about 700.

A comparison of the general results of the 120- and 150-meter dives indicates that the addition of air during exposure did abolish the compression tremor to a great extent, whereas the arthralgia seemed not to be influenced (Fig. IIIB-4). Much to our surprise,

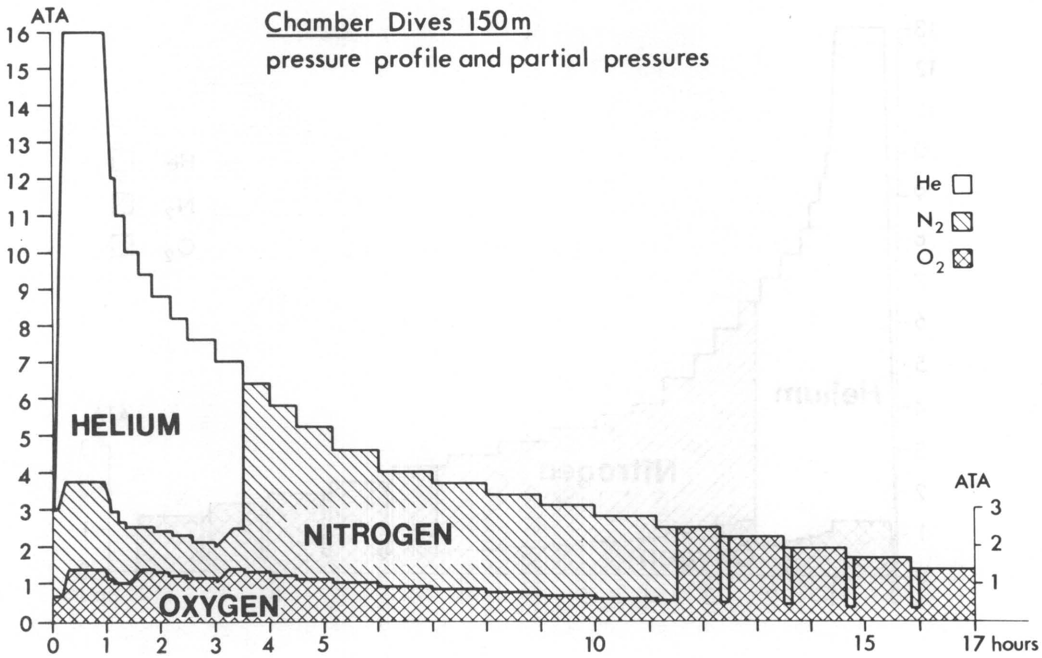


Fig. IIIB-2. Principal difference compared with Fig. IIIB-1 is that instead of heliox, a mixture of 76% He, 15% $N_2$, and 9% $O_2$ was breathed during exposure and first part of decompression.

the small amount of air at 150 meters gave rise to a certain degree of depth narcosis in half the cases. In most of these cases it was very slight, but some of the divers compared the degree of narcosis with that experienced on air at depths between 30 and 50 meters, although the actual air depth was only 20 meters. The higher incidence of pulmonary oxygen symptoms in the 150-meter dives compared to the 120-meter dives was what would be expected. In both cases the first symptoms appeared after 600-700 UPTD. As for the decompression symptoms after the 150-meter dives, these may very well have been due to nitrogen rather than to helium.

In conclusion, in the event of a submarine emergency, we have the potential to offer assistance to divers down to a depth of 150 meters. However, further work still remains to be done before we are ready for operational diving at the 150-meter level. We are considering a further increase of nitrogen admixture at depth, and a reduction of the oxygen exposure during decompression seems

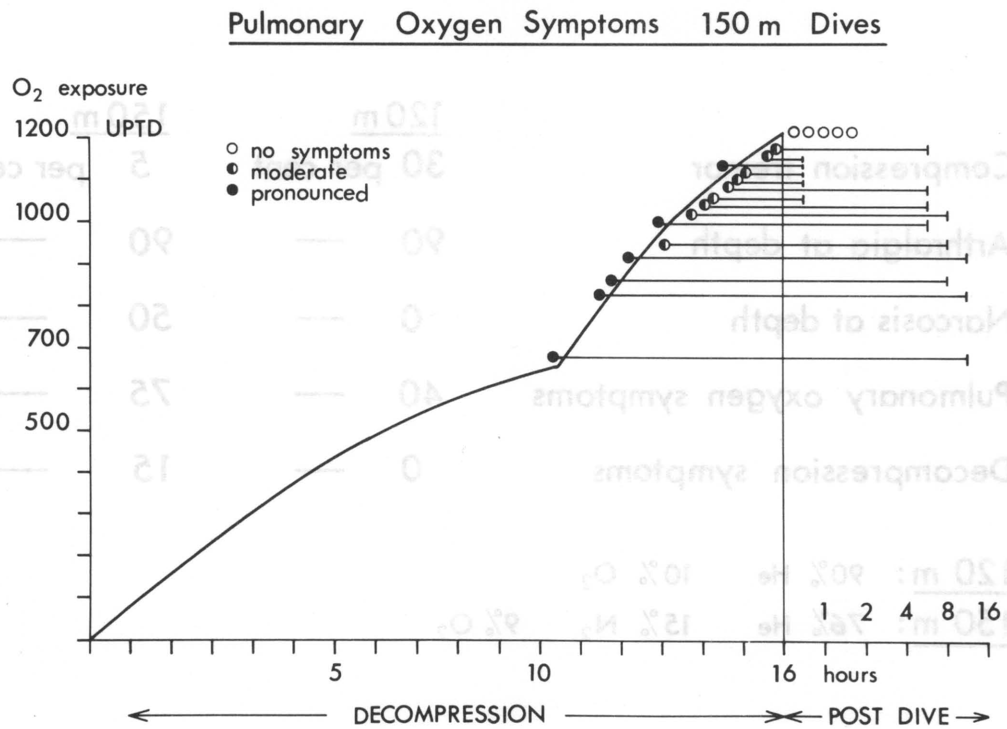


Fig. IIIB-3. Bent curve indicates gradually increasing oxygen exposure; straight lines indicate duration of symptoms.

desirable, even at the cost of increased decompression time.

## Discussion

Dr. Schreiner: Were all divers exposed to the same oxygen?

Dr. Muren: Yes.

Dr. Buehlmann: These tables were calculated in 1969, back when I had no computer, and did them myself with a slide rule. They definitely call for a long oxygen exposure; we have had to reduce the oxygen.

Mr. Kenyon: What are the criteria for your oxygen doses, and do you include time below 0.5 atm?

Dr. Muren: One minute at 1 ATA is one unit. Yes, we count the time below 0.5 atm.

**Exposure 60 min, 20 man dives each depth**

| | 120 m | | 150 m | |
|---|---|---|---|---|
| Compression tremor | 30 | per cent | 5 | per cent |
| Arthralgia at depth | 90 | — | 90 | — |
| Narcosis at depth | 0 | — | 50 | — |
| Pulmonary oxygen symptoms | 40 | — | 75 | — |
| Decompression symptoms | 0 | — | 15 | — |

120 m: 90% He  10% $O_2$
150 m: 76% He  15% $N_2$  9% $O_2$

Fig. IIIB-4. Frequency of symptoms at 120 and 150 meters.

## C. CURRENT DECOMPRESSION RESEARCH AT VIRGINIA MASON: K. H. SMITH

I come here with great humility and that is the truth. I think that many individuals who consider themselves scientists in the decompression field are kidding themselves. Despite Dr. Bennett's remarks, I think there are no diving operations in the field today that meet the criteria that he mentioned. That is, I don't believe there are bends-free tables or dive tables with proper testing, or that there can be proper testing of the profiles with the information that we have available today.

The words critical and uncritical are used, but I don't think we know what we are talking about until we define the etiology and the pathogenesis of decompression sickness. Every decompression table we make is going to have to be based on some empirical evidence, probably faulty. We're going to continue struggling in the next years as we have the past hundred years.

The RNPL 100-ft table for 60 minutes is a very "safe" table. It takes 122 minutes to decompress, and if you put a diver on that table you will find that he probably comes up bends-free. The United States Navy decompresses from the same amount of bottom time, the same depth, in 48 minutes. There are very few cases of bends from a single dive to that depth.

However, if you should take either one of those tables and run them consecutively for five days, you will induce bends. You can also hear bubbles if you listen with an ultrasonic bubble detector; if you measure the survivability of platelets or do consecutive platelet studies you will find that half the platelets have been taken out of the circulation by the fourth or fifth day of diving. Now I maintain that that is not a safe decompression.

Nevertheless we have a problem to solve. I rationalize my position by saying, "This is the best I can do", and to do this I plagiarize, compromise, and modify, and I hope that the individuals who sit in the decompression chamber throughout the decompression table that I calculated come up safely; in many cases, in most cases, they do come up quite safely.

But what's going to happen down the road 20 years from now to that individual I am loath to say.

Our basic equation for calculation of tissue tension is

$$\pi_2 = fD_1 + fR\,(T - 1/K) + (\pi_1 - fD_1 - fR/K)\,e^{-KT}$$

where $\pi_1$ = initial tension at T = 0, fsw; $\pi_2$ = final tension at T, fsw; T = step time, min; $D_1$ = depth at T = 0, fsw absolute; R = rate of change of pressure dP/dT, fsw/min; f = inert gas decimal fraction; $T_{1/2}$ = tissue half time, min; K = tissue constant = $\ln 2/T_{1/2}$

This is a very standard equation. It is based on the original Workman model, a model that has been further refined by Dr. Schreiner; it is so standard that you can find it in practically any text. It is simply a means with which I calculate, theoretically, what the gas content of a certain tissue is.

I really had trouble defining a "tissue". You and I know that no specific tissues exist, so we have to set outside limits and somewhere in between these outside limits we fill in numbers which fit our purpose.

Table IIIC-1 shows that we have chosen to use 10 tissue numbers and we have assigned different half times depending upon the mass of the gas and its diffusion characteristics, which are related to the molecular weight, directly or indirectly.

The tension of each gas is computed using its partial pressure and its half time for the tissue number in question. The tension of each gas is added and the sum is used in evaluations of that tissue. Evaluations of "safety" of the sums are based on a weighted average of the M values of these two gases, and then the average is weighted in direct proportion to the amount of tension in each of these gases in the tissue.

I took Dr. Workman's M values and plotted them (Fig. IIIC-1) so that I could see how they fell with respect to the allowable tissue over ambient allowable tension over ambient pressure. On the abscissa is the depth in feet of seawater. You see that several are grouped together quite closely. We use tissue gas tensions which are allowable at a certain depth that are very close together, and that didn't make very good sense to me.

I then took Dr. Buehlmann's published values for a constraint matrix (Fig. IIIC-2). I had to extrapolate Dr. Buehlmann's information, (Fig. IIIC-2) because Dr. Buehlmann grouped everything to three lines and had a variety of tissue half times in a single line. At any rate, Fig. IIIC-2 shows the kinds of lines that we got with Dr. Buehlmann's values. If you compare these with Workman's you see the great difference in both intercepts (the amount of gas that you can surface with, in a specific tissue) and slopes, (the change of pressure allowable in a tissue with a change in depth.)

I then straightened out these lines to put them into a more simple form so they could be used in tissue-tension ascent constraints which we could use easily and then change. Subsequent changes in the ascent constraints were arbitrary, but at least we had a systematic way of modifying them to change the ascent criteria. This was our first attempt, which we called our Mark 2. The Mark 2 M values are shown in Fig. IIIC-3. We then did a dive to 500 feet for one hour, and we repeated this dive to a total of eight exposures. We used a Draeger FG 3 to administer the gas because we were changing gases frequently.

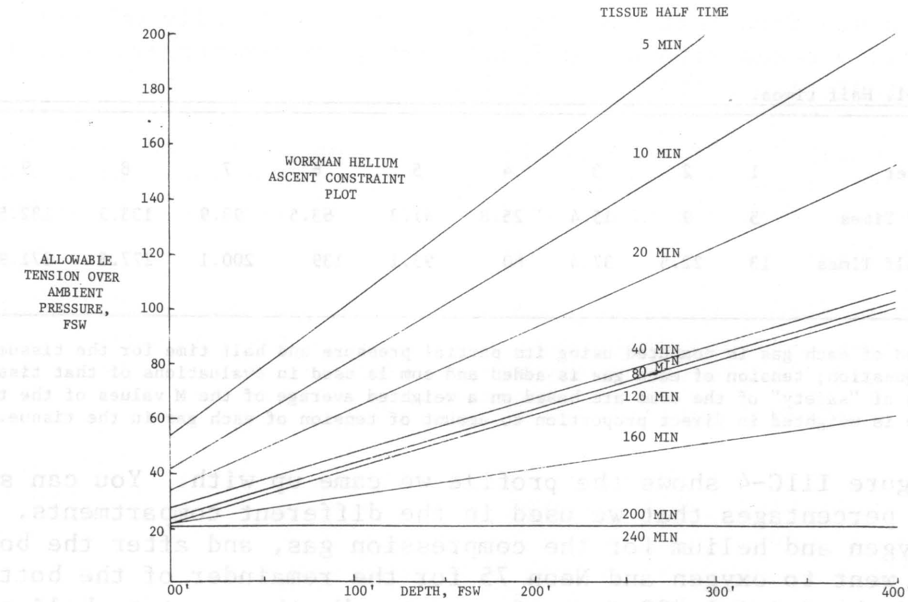


Fig. IIIC-1. Workman's M values: allowable gas tension in the various tissue compartments as a function of depth.

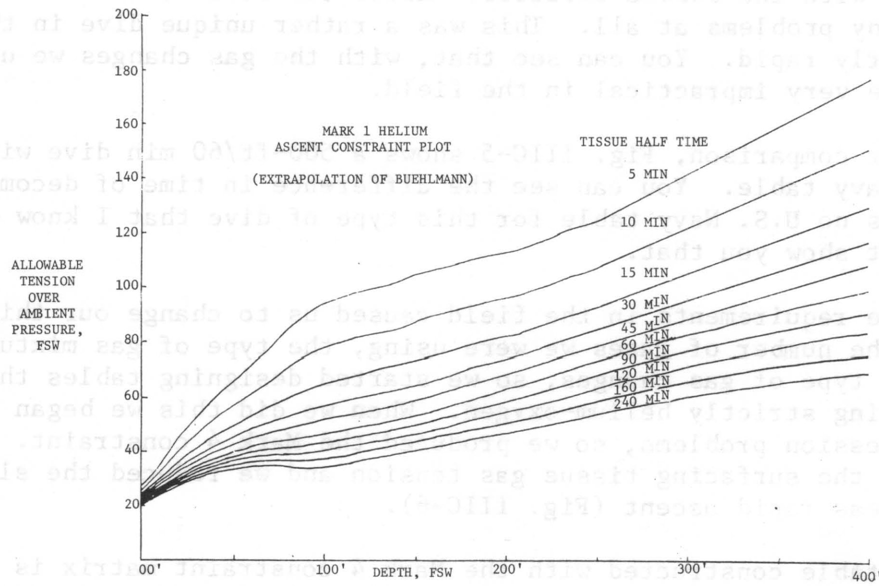


Fig. IIIC-2. Buehlmann's M values, extrapolated to allow comparison with Workman's (Fig. IIIC-1).

Table IIIC-1. Half times.

| Tissue Number | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|
| Helium Half Times | 5 | 9 | 15.4 | 25.8 | 41.3 | 63.5 | 93.9 | 133.5 | 182.5 | 240 |
| Nitrogen Half Times | 13 | 22.5 | 37.4 | 60 | 93.1 | 139 | 200.1 | 277.8 | 371.9 | 480 |

Tension of each gas is computed using its partial pressure and half time for the tissue number in question; tension of each gas is added and sum is used in evaluations of that tissue. Evaluations of "safety" of the sums are based on a weighted average of the M values of the two gases; the average is weighted in direct proportion to amount of tension of each gas in the tissue.

Figure IIIC-4 shows the profile we came up with. You can see the gas percentages that we used in the different compartments. We used oxygen and helium for the compression gas, and after the bottom time we went to oxygen and Neon 75 for the remainder of the bottom time and the initial 200 feet of ascent. We then went to helium-neon, with an increased oxygen percentage, until we reached 50% oxygen, 50% nitrogen at about 100 feet; then we went on oxygen when we reached 30 feet, 40 minutes on oxygen, 10 minutes on air.

You can see that for an hour at 500 feet the decompression is rather short, 9 hours and 56 minutes. In the 8 exposures (4 dives, 2 men each) we did not experience a single occurrence of, or manifestation of, any kind of decompression sickness. We did hear some bubbles with the bubble detector. After the dive we did not experience any problems at all. This was a rather unique dive in that it was fairly rapid. You can see that, with the gas changes we used, it would be very impractical in the field.

For comparison, Fig. IIIC-5 shows a 500-ft/60 min dive with the Royal Navy table. You can see the difference in time of decompressions. There is no U.S. Navy table for this type of dive that I know of, so I cannot show you that.

The requirements in the field caused us to change our thinking about the number of gases we were using, the type of gas mixtures, and the type of gas changes, so we started designing tables that were using strictly helium-oxygen. When we did this we began having decompression problems, so we produced the Mark 4 constraint. We reduced the surfacing tissue gas tension and we reduced the slope to get a less rapid ascent (Fig. IIIC-6).

A table constructed with the Mark 4 constraint matrix is shown in Fig. IIIC-7. These tables are used in the field; they are quite satisfactory down to 400 feet. They range from 300-500 feet. When we use this table at 500 feet for an hour we have a high percentage of bends, a high percentage being less than 50% but more than 20%.

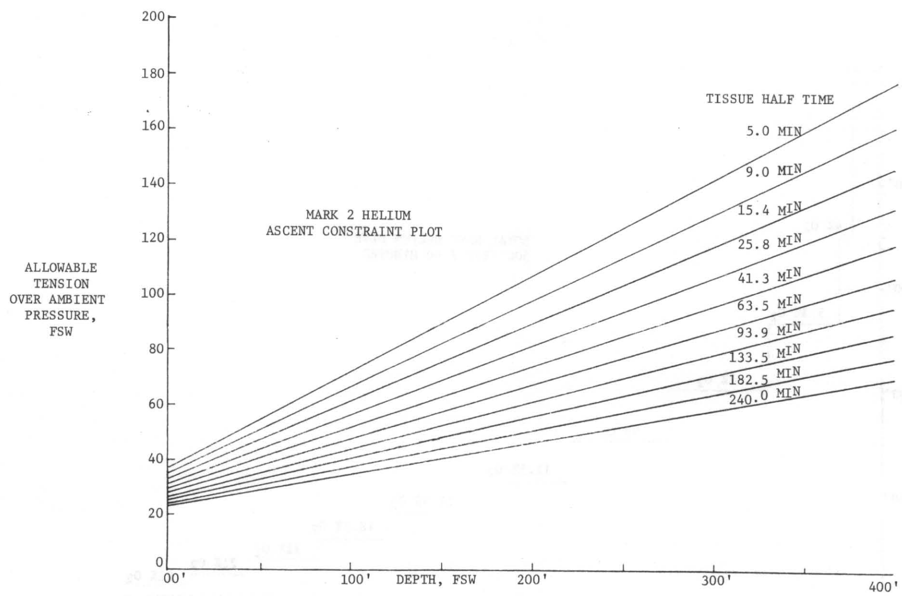


Fig. IIIC-3. New M values developed from those of Workman and Buehlmann.

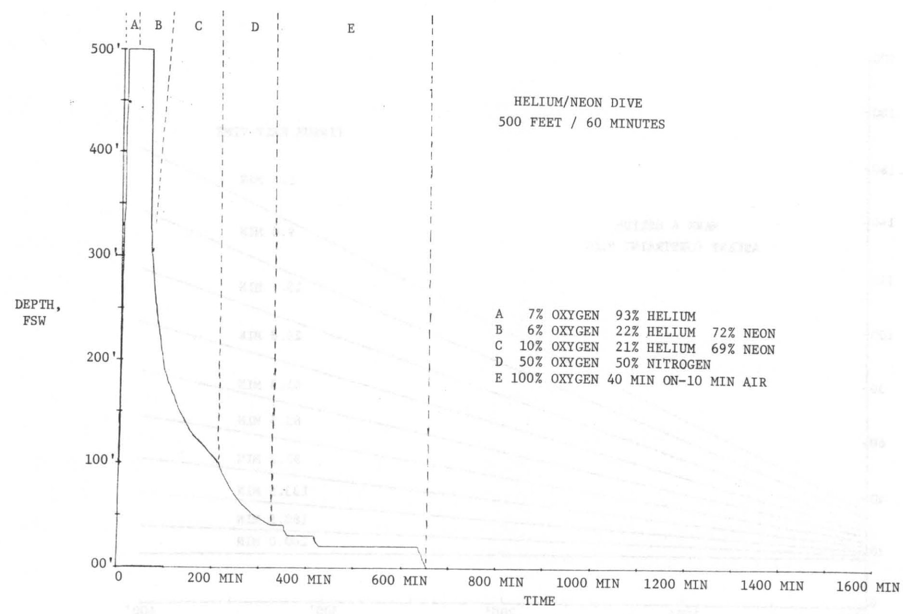


Fig. IIIC-4. Profile of 500 fsw/60 min dive based on M values of Fig. IIIC-3.

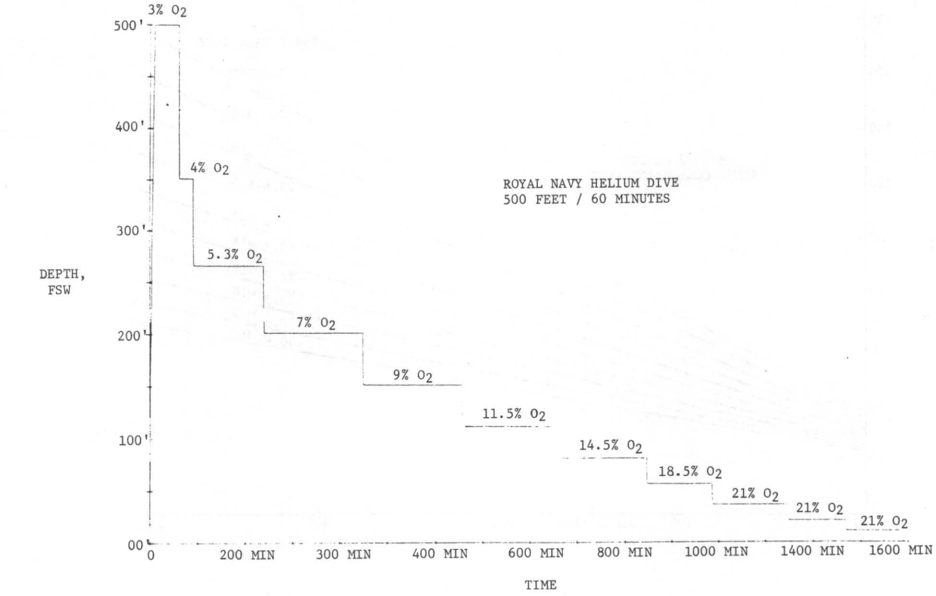


Fig. IIIC-5. Profile of 500 fsw/60 min dive using Royal Navy tables.

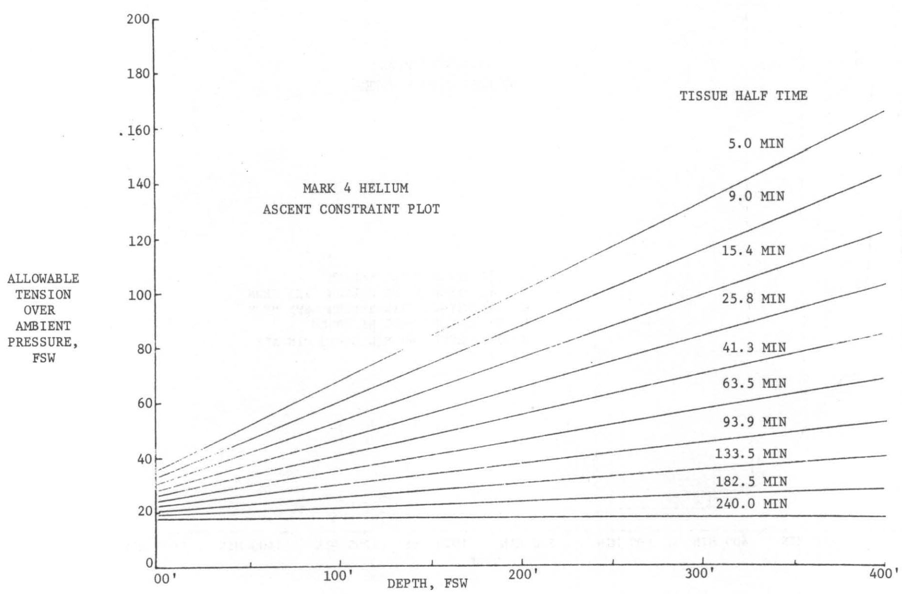


Fig. IIIC-6. Mark 4 helium ascent matrix (M values).

I hate to give percentages on a table because we can't do enough dives to make these percentages statistically significant.

Because of the problems that were experienced at the deeper depths and the requirement to go deeper than 500 feet, we came up with the Mark 6 constraint (Fig. IIIC-8). You will note that the intercept (surfacing values) for the shorter half times has been reduced. Again we wanted to slow down the initial ascent. This is where we felt we were having problems. The reason we thought we were getting our problems with the initial ascent was that we were getting a large amount of bubble formation at great depths. I use the term bubble formation advisedly. These were events which came through the Doppler instrumentation; we consider them bubbles or gas emboli.

To change the rate of ascent at the beginning of the dive we lessen the slope of the upper lines. We have a systematic function which is in the computer which will change each of these slopes and keep the distance between the lines in the same proportion. Let me say again, it is all arbitrary.

How do we get these slopes and intercepts? We set the intercept for the 5-min compartment, the 240-min compartment, and we say that a 240-min half time for helium is the longest tissue compartment that we need to use.

We then set a slope on the 5-min tissue and on the 240-min tissue and we have the computer draw out this group of lines. All of the points along those lines are a matrix in the computer, and the decompression is then calculated with respect to these limiting points.

There is very little difference between the Mark 6 and the Mark 4, even though we did change the intercept of the 5-min tissue by 9 feet of seawater. The slope was not changed very much and consequently there were some minimal changes but they were not great. They did, however, significantly change the percentage of bends or decompression sickness during the ascent from these deeper dives. A Mark 6 dive is shown in Fig. IIIC-9.

The Mark 6 parameters for descent and bottom time are as follows:

1) Descent rate is 50 ft/min
2) Bottom gas is 3% $O_2$, 11.3% $N_2$, 85.7% He
3) Mark 6 tables cover the range of 400-700 ft
4) Over the depth range, 3% $O_2$ gives an $O_2$ partial pressure of 0.39-0.67 ATA
5) The $O_2$-$N_2$ ratio of the bottom gas is the same as in air, so the mix can be prepared using helium and air
6) 11.3% $N_2$ gives an air depth equivalent to 28-72 ft over the depth range

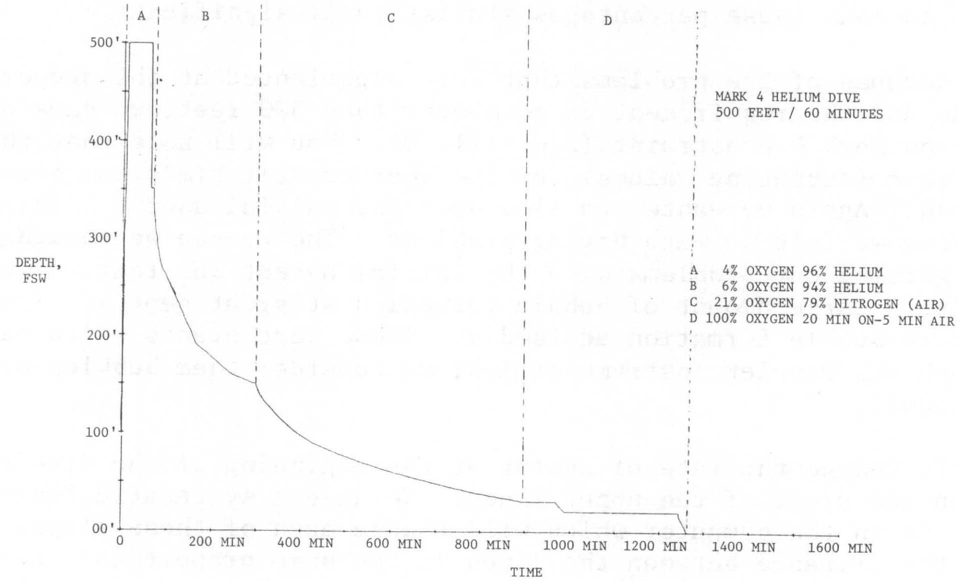


Fig. IIIC-7. Profile of 500 fsw/60 min dive based on Mark 4 matrix.

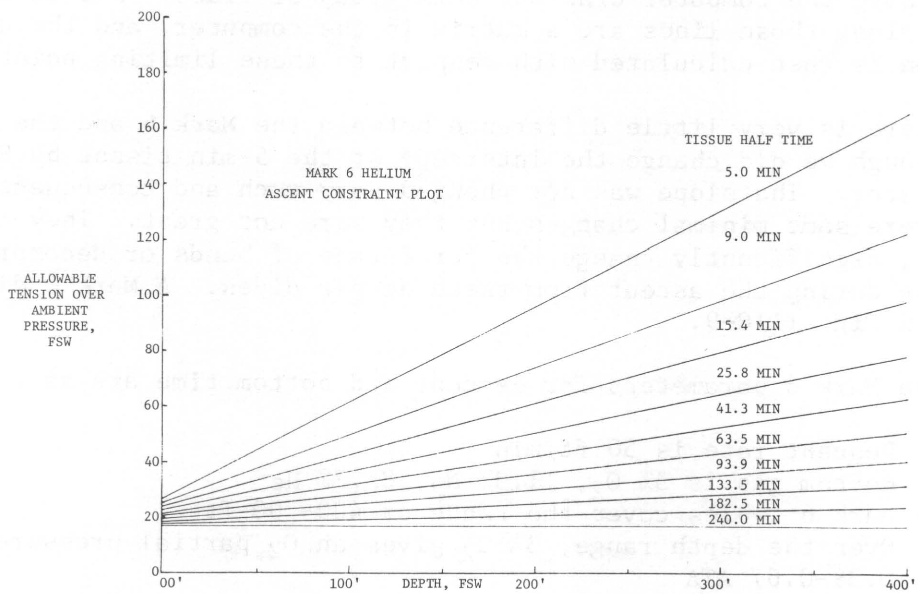


Fig. IIIC-8. Mark 6 helium ascent M values.

Table IIIC-2. Computer printout of Mark 6 dive protocol.

500. FEET 30. MINUTES MARK 6. TOTAL DIVE TIME= 10.2 HR

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| GO FROM | 0 FT TO | 500 FT IN | 10.0 MIN | (50.0 FT/MIN) | 0:10: 0 | GAS: 3% 02 | 11.3% N2 | BAL HELIUM |
| STAY AT | 500 FT FOR | 20.0 MIN | | | 0:30: 0 | GAS: 3% 02 | 11.3% N2 | BAL HELIUM |
| GO FROM | 500 FT TO | 330 FT IN | 3.4 MIN | (50.0 FT/MIN) | 0:33:24 | GAS: 3% 02 | 11.3% N2 | BAL HELIUM |
| STAY AT | 330 FT FOR | 4.6 MIN | | | 0:38: 0 | GAS: 6% 02 | 22.6% N2 | BAL HELIUM |
| GO FROM | 330 FT TO | 250 FT IN | 8.0 MIN | (10.0 FT/MIN) | 0:46: 0 | GAS: 6% 02 | 22.6% N2 | BAL HELIUM |
| GO FROM | 250 FT TO | 230 FT IN | 4.0 MIN | ( 5.0 FT/MIN) | 0:50: 0 | GAS: 6% 02 | 22.6% N2 | BAL HELIUM |
| GO FROM | 230 FT TO | 210 FT IN | 6.0 MIN | ( 3.3 FT/MIN) | 0:56: 0 | GAS: 6% 02 | 22.6% N2 | BAL HELIUM |
| GO FROM | 210 FT TO | 170 FT IN | 20.0 MIN | ( 2.0 FT/MIN) | 1:16: 0 | GAS: 6% 02 | 22.6% N2 | BAL HELIUM |
| GO FROM | 170 FT TO | 150 FT IN | 20.0 MIN | ( 1.0 FT/MIN) | 1:36: 0 | GAS: 6% 02 | 22.6% N2 | BAL HELIUM |
| STAY AT | 150 FT FOR | 6.0 MIN | | | 1:42: 0 | GAS: 21% 02 | 22.6% N2 | ---AIR |
| GO FROM | 150 FT TO | 140 FT IN | 1.0 MIN | (10.0 FT/MIN) | 1:43: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| GO FROM | 140 FT TO | 130 FT IN | 3.0 MIN | ( 3.3 FT/MIN) | 1:46: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| GO FROM | 130 FT TO | 120 FT IN | 5.0 MIN | ( 2.0 FT/MIN) | 1:51: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| GO FROM | 120 FT TO | 80 FT IN | 40.0 MIN | ( 1.0 FT/MIN) | 2:31: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| GO FROM | 80 FT TO | 70 FT IN | 15.0 MIN | ( 1.5 MIN/FT) | 2:46: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| GO FROM | 70 FT TO | 60 FT IN | 20.0 MIN | ( 2.0 MIN/FT) | 3: 6: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| GO FROM | 60 FT TO | 50 FT IN | 30.0 MIN | ( 3.0 MIN/FT) | 3:36: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| GO FROM | 50 FT TO | 40 FT IN | 40.0 MIN | ( 4.0 MIN/FT) | 4:16: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| GO FROM | 40 FT TO | 30 FT IN | 60.0 MIN | ( 6.0 MIN/FT) | 5:16: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| STAY AT | 30 FT FOR | 20.0 MIN | | | 5:36: 0 | GAS:100% 02 | .0% N2 | MASK OXYGEN |
| STAY AT | 30 FT FOR | 5.0 MIN | | | 5:41: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| STAY AT | 30 FT FOR | 20.0 MIN | | | 6: 1: 0 | GAS:100% 02 | .0% N2 | MASK OXYGEN |
| STAY AT | 30 FT FOR | 5.0 MIN | | | 6: 6: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| GO FROM | 30 FT TO | 20 FT IN | 20.0 MIN | ( 2.0 MIN/FT) | 6:26: 0 | GAS:100% 02 | .0% N2 | MASK OXYGEN |
| STAY AT | 20 FT FOR | 5.0 MIN | | | 6:31: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| STAY AT | 20 FT FOR | 20.0 MIN | | | 6:51: 0 | GAS:100% 02 | .0% N2 | MASK OXYGEN |
| STAY AT | 20 FT FOR | 5.0 MIN | | | 6:56: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| STAY AT | 20 FT FOR | 20.0 MIN | | | 7:16: 0 | GAS:100% 02 | .0% N2 | MASK OXYGEN |
| STAY AT | 20 FT FOR | 5.0 MIN | | | 7:21: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| STAY AT | 20 FT FOR | 20.0 MIN | | | 7:41: 0 | GAS:100% 02 | .0% N2 | MASK OXYGEN |
| STAY AT | 20 FT FOR | 5.0 MIN | | | 7:46: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| STAY AT | 20 FT FOR | 20.0 MIN | | | 8: 6: 0 | GAS:100% 02 | .0% N2 | MASK OXYGEN |
| STAY AT | 20 FT FOR | 5.0 MIN | | | 8:11: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| STAY AT | 20 FT FOR | 20.0 MIN | | | 8:31: 0 | GAS:100% 02 | .0% N2 | MASK OXYGEN |
| STAY AT | 20 FT FOR | 5.0 MIN | | | 8:36: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| STAY AT | 20 FT FOR | 20.0 MIN | | | 8:56: 0 | GAS:100% 02 | .0% N2 | MASK OXYGEN |
| STAY AT | 20 FT FOR | 5.0 MIN | | | 9: 1: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| STAY AT | 20 FT FOR | 20.0 MIN | | | 9:21: 0 | GAS:100% 02 | .0% N2 | MASK OXYGEN |
| STAY AT | 20 FT FOR | 5.0 MIN | | | 9:26: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| STAY AT | 20 FT FOR | 20.0 MIN | | | 9:46: 0 | GAS:100% 02 | .0% N2 | MASK OXYGEN |
| STAY AT | 20 FT FOR | 5.0 MIN | | | 9:51: 0 | GAS: 21% 02 | 79.0% N2 | ---AIR |
| GO FROM | 20 FT TO | 0 FT IN | 20.0 MIN | ( 1.0 FT/MIN) | 10:11: 0 | GAS:100% 02 | .0% N2 | MASK OXYGEN |

Bottom gas is made up by compressing part of the way with air. We have a table which tells the operational group how deep to compress with air for a certain depth of dive. The remainder of the compression is done with helium. We caution them that good mixing is absolutely imperative in the chamber. We also calculate oxygen exposure in UPTD units, and at the end of a dive the computer tells us what our UPTD is. If it's over 800 we reduce the percentage of oxygen on the bottom and throughout the dive.

The Mark 6 ascent parameters are as follows:

1) Ascent rate to the first gas change is 50 ft/min
2) There are brief stops at the gas changes
3) The gas is changed to a higher oxygen trimix; $O_2$-$N_2$ ratio is maintained at the ratio in air
4) A typical gas mix is 5.0% $O_2$, 18.9% $N_2$, 76.1% He
5) Ascent continues from the gas change depth and is continuous rather than staged
6) Ascent rate is constant for at least each 10-ft depth change; it is one of 25 standard ascent rates
7) The fastest safe rate is selected, using an iterative technique, by comparing the sum of the inert tensions to the weighted average of the M values for each tissue number
8) Gas is changed to air at 150 ft over a 1-hr period, by a slow flush of the deck chamber
9) Ascent continues as before, using a continuous ascent
10) On reaching 30 ft, intermittent $O_2$ breathing using demand masks is begun, 20 min on $O_2$, 5 min off $O_2$.
11) $O_2$ breathing is completed at 30 and 20 ft, with stage decompression stops at those depths
12) The unit pulmonary $O_2$ toxicity dose is computed for each dive profile; if excessive, the first gas change is changed to a lower $O_2$ content, and the table is recalculated

Brief stops occur at the gas changes for convenience if they are using mask breathing, or if the chamber is not changed by the addition of air. The gas is changed to a higher $O_2$ trimix, and then the oxygen-nitrogen ratio is maintained at the air ratio. Usually, when we are ascending and wish a higher oxygen in the chamber we add air. This also, of course, increases the nitrogen in the compartment.

Table IIIC-2 is a printout of the table shown in Fig. IIIC-9. Figure IIIC-10 shows a comparison of the dives that we have just talked about. You can see that the difference between the Mark 4 and the Mark 6 is very slight. A comparison between the Mark 6 and the Royal Naval table shows what the use of gas switching will do in a decompression profile. The helium-neon table shows an even more prominent improvement, but it is not operationally practical.

Finally, the following outline describes a treatment procedure for decompression problems encountered with the Mark 4 and Mark 6 decompression schedules:

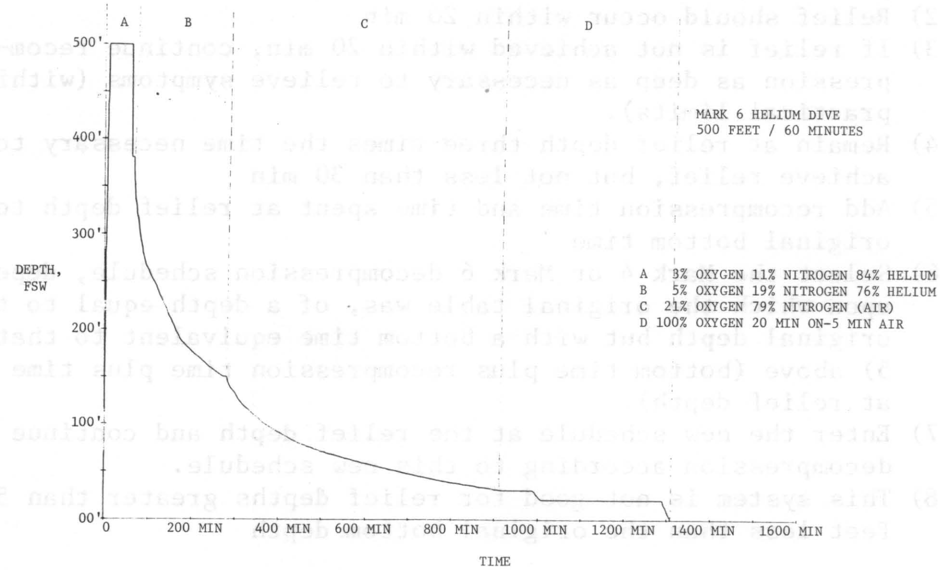


Fig. IIIC-9. Profile of 500 fsw/60 min dive computed against Mark 6 constraint. Descent rate for this dive is 50 fsw/min.

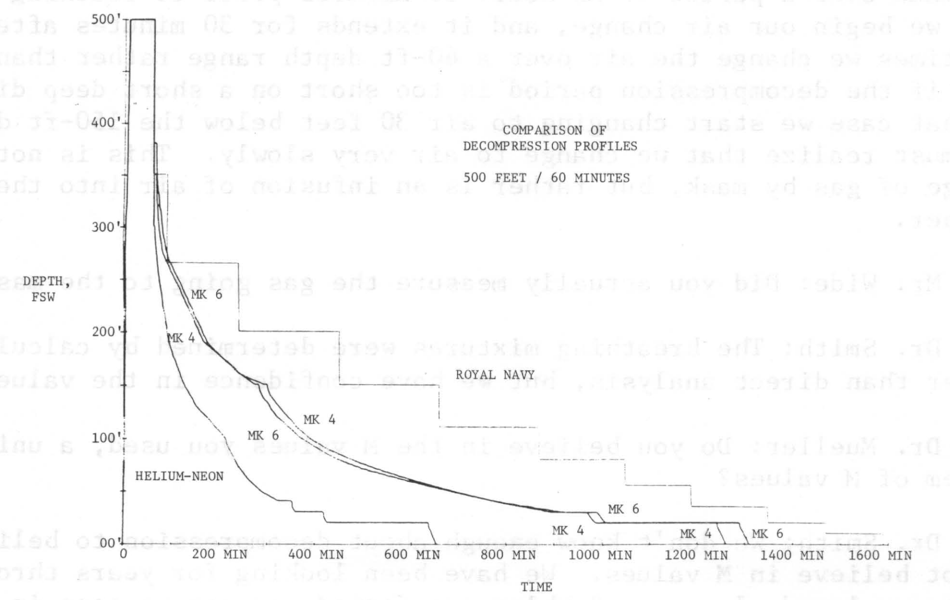


Fig. IIIC-10. Comparison of several profiles, shown individually in other figures. From left, Fig. IIIC-4, Fig. IIIC-9, Fig. IIIC-7, and Fig. IIIC-5.

1) Recompress at least 33 ft deeper than depth at which decompression problem was first noticed.
2) Relief should occur within 20 min
3) If relief is not achieved within 20 min, continue recompression as deep as necessary to relieve symptoms (within practical limits).
4) Remain at relief depth three times the time necessary to achieve relief, but not less than 30 min
5) Add recompression time and time spent at relief depth to original bottom time
6) Select the Mark 4 or Mark 6 decompression schedule, depending upon which the original table was, of a depth equal to the original depth but with a bottom time equivalent to that in 5) above (bottom time plus recompression time plus time spent at relief depth).
7) Enter the new schedule at the relief depth and continue decompression according to this new schedule.
8) This system is not good for relief depths greater than 50 feet less than the original bottom depth

Discussion

Dr. Bennett: I notice that you make the air change very slowly; is this because when you didn't make the air change slowly you ran into vestibular bends?

Dr. Smith: We had several cases of vestibular problems when we were coming from the deeper depths. Consequently our air change is now made over a period of an hour: 30 minutes prior to reaching 150 feet we begin our air change, and it extends for 30 minutes after. Sometimes we change the air over a 60-ft depth range rather than an hour if the decompression period is too short on a short deep dive. In that case we start changing to air 30 feet below the 150-ft depth. You must realize that we change to air very slowly. This is not a change of gas by mask, but rather is an infusion of air into the chamber.

Mr. Wide: Did you actually measure the gas going to the mask?

Dr. Smith: The breathing mixtures were determined by calculation rather than direct analysis, but we have confidence in the values.

Dr. Mueller: Do you believe in the M values you used, a uniform system of M values?

Dr. Smith: We don't know enough about decompression to believe or not believe in M values. We have been looking for years through Haldane-colored glasses. Bubbles are formed--as can be seen in my ultrasound paper (Section VIIA) and Haldane theory doesn't account for them at all.

Dr. Greene: I'm rather curious as to what Dr. Smith based his choice of half times for neon on? Since it was faster than helium I presume that there was some difference between the half times for helium and neon. What values did you use?

Dr. Smith: We took what was in the literature and then evaluated what had been done with neon. We will hear more about this from Bill Hamilton.

Mr. Edel: I know that you started out with one set of M values which seemed to work very well for you under the initial conditions. As you changed conditions you found that you had to modify them. This process has been duplicated by all of us. I wonder, are we looking at different modes of decompression, where under a given set of circumstances one set of M values will work, but as we increase to other depths and times or change our mode of decompression, we have to adopt a new system of M values to fit that particular requirement? Do you have that feeling?

Dr. Smith: I certainly do. That tells me that M values are not the complete answer, but are simply tools we can use.

Dr. Hills: I couldn't agree more.

Dr. Buehlmann: In the relation between helium and nitrogen half times there is a difference between you and us. You have helium half time of 5 minutes, 30 minutes for nitrogen. These are absolutely the same, but for the slowest half time you have 240 minutes for helium and a corresponding half time with nitrogen of 480 minutes. That's not our correlation. We have a ratio for these two of 2.645.

## D. CURRENT WORK AT THE UNIVERSITY OF ZUERICH: A. A. BUEHLMANN

Between 1972 and 1974 we worked on four problems: 1) the influence of physical work on saturation and equilibration with inert gas; 2) decompression after air diving at altitude; 3) blood coagulation during "critical" or "minimal" decompression, with or without bends, following oxy-helium dives; and 4) skin bends after oxy-helium and oxy-helium-nitrogen dives.

The results of most of this work are published (1, 2, 3), and I hope to publish the results of our skin bends experiments this year. We have not changed our simple model for calculating the decompressions since 1960. There are four main points: 1) The different half times were related to the perfusion rates of the compartments or tissues. It is a question of identifying the half times with different tissues. 2) For a tissue with a given perfusion rate, the ratio of half times for different inert gases is equal to the ratio of the square roots of their molecular weights. That's a very old diffusion law of the last century. 3) The same supersaturation factors of the Haldane system for different inert gases are based only on the ratio between the total inert gas pressure in the compartment or tissue and the ambient pressure. 4) The partial pressures of different inert gases in the same compartment must be added together.

We calculate saturation and desaturation with 16 half times, helium 2 - 240 minutes, nitrogen 5.3 - 635 minutes, hydrogen 1.4 - 170 minutes. The ratio of 2.65 between nitrogen and helium in the longest half times was determined by finding the minimum decompression time, using the same supersaturation factors after long-lasting and so-called saturation dives at 100 feet during which either 80% helium or 80% nitrogen was used.

If we suppose the half time to be determined mainly by the perfusion of the different tissues, it can be expected that any change of half time will depend on physical work, particularly for the slow tissues; muscles, buttocks, skin, bone, and cartilage. It is probable that during sleep the longest half times will increase. We can say saturation is faster than desaturation. Ten years ago, we calculated the decompression on the basis of a longest helium half time of 180 minutes during descent and bottom time, and 240 minutes during decompression. This method is sufficient for long-lasting and saturation dives. More specific account must be taken of physical work in determining the decompression for intervention dives with bottom times between 15 and 60 minutes.

We studied the effect of physical work on decompression by determining the "minimum decompression time" for simulated oxy-helium dives, using 82 different subjects and 173 exposures. The minimum decompression time without work after 60 minutes at 10 ATA (10% $O_2$, 82% He, 8% $N_2$) was 250 minutes. If work was performed, the minimum without symptoms was 360 minutes. That's real bottom time, without descent time. We always use the same descent time of 1 ATA per minute. The minimum decompression time without work was 250 minutes with only one case of bends in 13 divers. In Fig. IIID-1, the solid bars are with work. That means 80 watts for 10 minutes in the chamber before the dive, 10 minutes during bottom time, and in some cases an additional 10 minutes during decompression time. If work was performed, the minimum decompression time without symptoms increased to six hours. For this period there were 16 persons without bends, one with bends. These were always light bends during the final decompression, or one or two hours after the end of the exercise.

The decompression time after a 60-min dive with physical work on a bicycle ergometer with a load of 80 watts before the experiment, and as described, is approximately equal to the decompression time computed for a 90-min bottom time at 10 ATA without work. But it is not good to change from a 60-min table to the 90-min table; it is possible but it is not economical.

For the longer period of 120 minutes at 10 ATA, the minimum decompression time was 475 minutes without work and 565 minutes with work on the bottom. The longer the bottom time, the less the difference; in saturation dives there is no difference. In my opinion, these results demonstrate that the minimum decompression time must be longer after a short dive with work than after a similar dive without work. In the old days, in the '60's with Keller, we had no work or practically no work. Accordingly, the first phase of decompression, which is limited by the fast tissues, is equal whether or not work is performed. The second phase, however, must be prolonged when work takes place on the bottom, since the slow tissues, which are more fully saturated than without work, now control the rate of ascent. To take account of these considerations, we compute decompression after simulated dives with work, and all real dives in the sea, using "virtual bottom times" for the tissues with helium half times of 105-240 minutes:

| He half time, min | Virtual bottom time as a percentage of real bottom time |
|---|---|
| 105 | 120 |
| 120 | 120 |
| 150 | 135 |
| 180 | 135 |
| 210 | 145 |
| 240 | 145 |

We consider the real bottom time 100%, and increase it in the computer 20 or 35 or 45%, according to these helium half times.

In summary, it may be said that the effect of physical work in increasing the saturation of the inert gas has to be taken into account in calculating the decompression after short intervention dives. The use of virtual bottom times to do this is simple to adapt to a computer program. This method has the advantage that the spectrum of

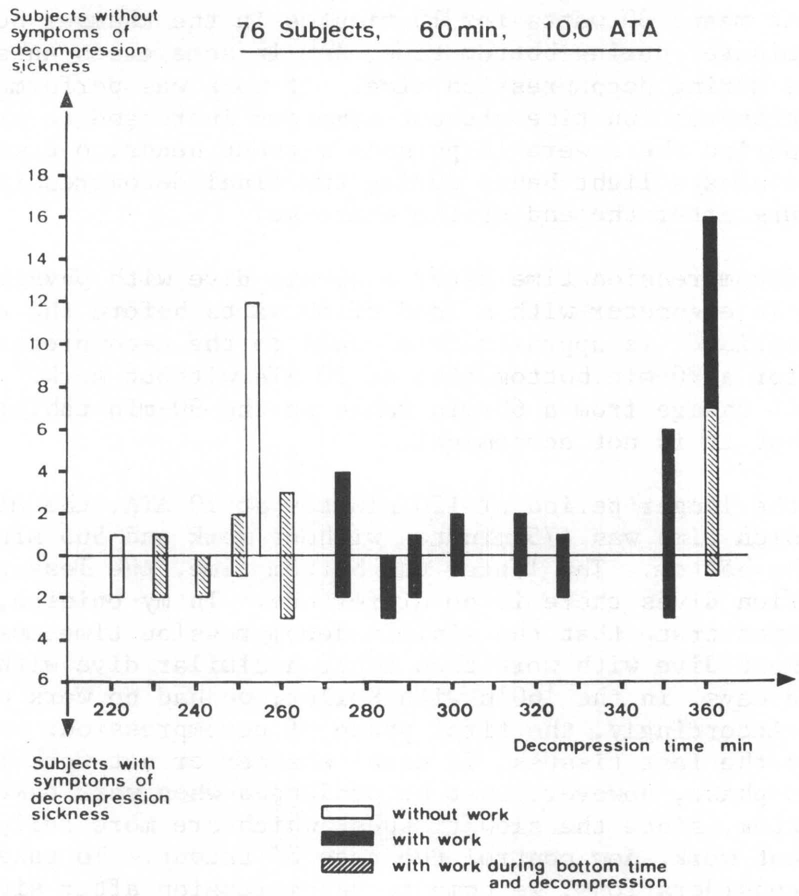


Fig. IIID-1. Experimental results, with 10% $O_2$, 82% He (2), and 8% $N_2$ (1), 60-min bottom time at 10 ATA. Number of subjects with and without decompression symptoms, as a function of decompression time, for cases with work (blank bars), with work at bottom (solid bars), and with work at bottom and during decompression (hatched bars).

half times and the supersaturation factors are constant for all dives.

Deep diving experiments demonstrate that the supersaturation factors have to be drastically reduced for all tissues. We have no theoretical concept to account for this phenomenon. But we know that the quantity of dissolved inert gas in the tissues tolerated without forming bubbles increases with the total pressure of inert gas in the tissue. We reduce the supersaturation factors for different half times in relation to the total partial pressure of inert gas. Our present experience concerning tolerable supersaturation factors is given in Fig. IIID-2. The factors for the slow tissues are well confirmed by saturation experiments up to 31 ATA. The curve for helium half times of 45 to 90 minutes is the result of the analysis of so-called vertigo bends during decompression after experiments between 5 and 31 ATA. No troubles of this kind occurred by using supersaturation factors in accordance with this curve. In the case of skin bends, the helium half times between 15 to 30 minutes are involved. After the analysis of the skin bends experiments it should be possible to produce a new version of this figure. Supersaturation factors have to be determined experimentally--that means analyzing decompression accidents. The factors for tissues with helium half times of 2-10 minutes are estimated and not confirmed by decompression incidents after oxy-helium dives.

It is interesting to compare American decompression profiles with our schedules. In 1974, Dr. Bennett gave me a schedule of a 500-fsw dive, with a descent time of 5 minutes 20 seconds, a bottom time of 25 minutes, and a decompression time of 615 minutes (7% $O_2$/93% He). This was Bennett's B table (Fig. IIID-3). The compression rate to 500 fsw in 5:20 is, in my opinion, too fast for the diver. The first part of the decompression from 500 fsw to 180 fsw (16.5 to 6.3 ATA) is practically identical with our profile. The fast initial decompression phase is followed by a very slow decompression rate from 6.3 to 2.0 ATA, but the final decompression is relatively fast, too fast according to our calculation. There is no profit as regards the total decompression time.

I must emphasize that our final decompression between 2.5 ATA and the surface is always near the limit of light bends if the slow tissues with helium half times of 150-240 minutes are leading. On this profile we have between 6 and 8% bends. For real dives we prefer a slow compression rate (1 ATA/minute), a lower $O_2$ concentration of 5% at bottom, and the simple change from oxy-helium to air, followed by 100% oxygen between 2.0 ATA and the surface. This schedule, much more convenient for operational conditions with a real bottom time of 30 minutes at 500 feet, needs a total decompression time of 720 minutes.

Peter Edel sent me a report on a series of 200-ft dives to compare oxy-nitrogen, oxy-helium, and oxy-hydrogen. Bends occurred mainly after the oxy-nitrogen and oxy-hydrogen trials, which is not surprising according to our experiences and calculations. Figure IIID-4 shows a profile using 120 minutes, 200 feet (7.3 ATA), with 3% oxygen and 97% nitrogen. Final decompression was with alternating 100% oxygen and air. You see, Mr. Edel had 472 minutes, and 3 of 6 had bends. Our profile shows a faster decompression in the middle part, and a longer final decompression between 2 ATA and surface, but the total time is shorter. And this profile was tested repeatedly during the last 15 years, so much that we are sure of it.

Figure IIID-5 shows the same but with 97% helium. And you see, with 477 minutes, there were no bends in four cases. This is practically

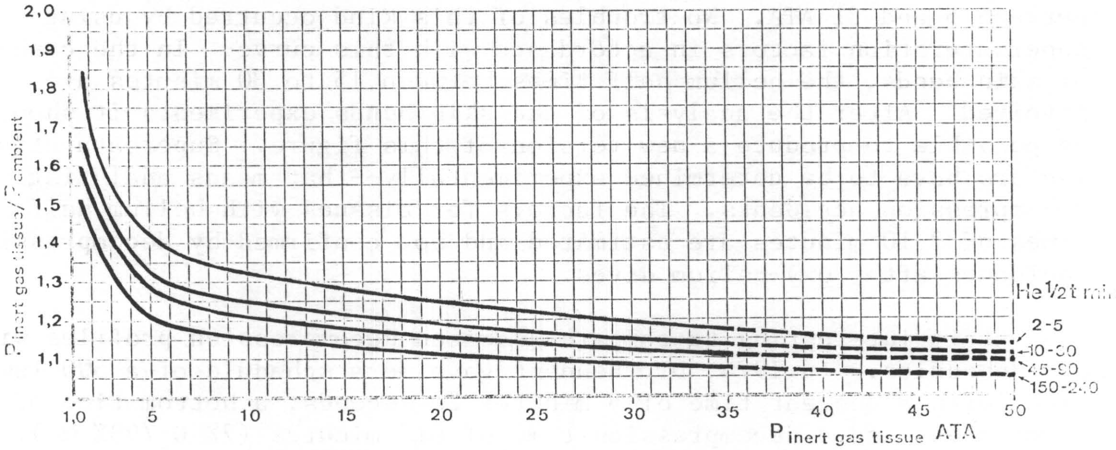


Fig. IIID-2. Supersaturation ratios as a function of depth.

the same profile as according to our calculations; no big difference in the middle part, but with 452 minutes, 3 of 4 had bends. We are very near the limit of bends with this profile, that is clear. It is very complicated, with nitrogen, helium, oxygen-nitrogen, oxygen-air, and so on.

The most interesting profile for us is with hydrogen at 477 minutes, practically the same profile as the helium one; 4 of 8 persons had bends (Fig. IIID-6). Now we calculated according to our method. We changed at 140 foot to air and then to oxygen a little later, and then we arrived at 462 minutes, but at present I am not able to say how high the percentage of bends is. We will see. These well-documented experiments of Mr. Edel's are, in my opinion, a confirmation of our concept that saturation speed is not related entirely to solubility factors in the blood, watery and fatty tissues, but is a function of the molecular weights of the different inert gases. The ratio between nitrogen and hydrogen is 3.728.

## Discussion

Mr. Edel: I'd just like to interject that the tables of mine that were mentioned were deliberately designed to provoke bends and to provoke bends at a specific point, so they did accomplish their purpose.

Dr. Buehlmann: Yes, it's clear, but for us it's very important that after helium you have bends with the same decompression as hydrogen, and that this is according to our concept of relation of half times --

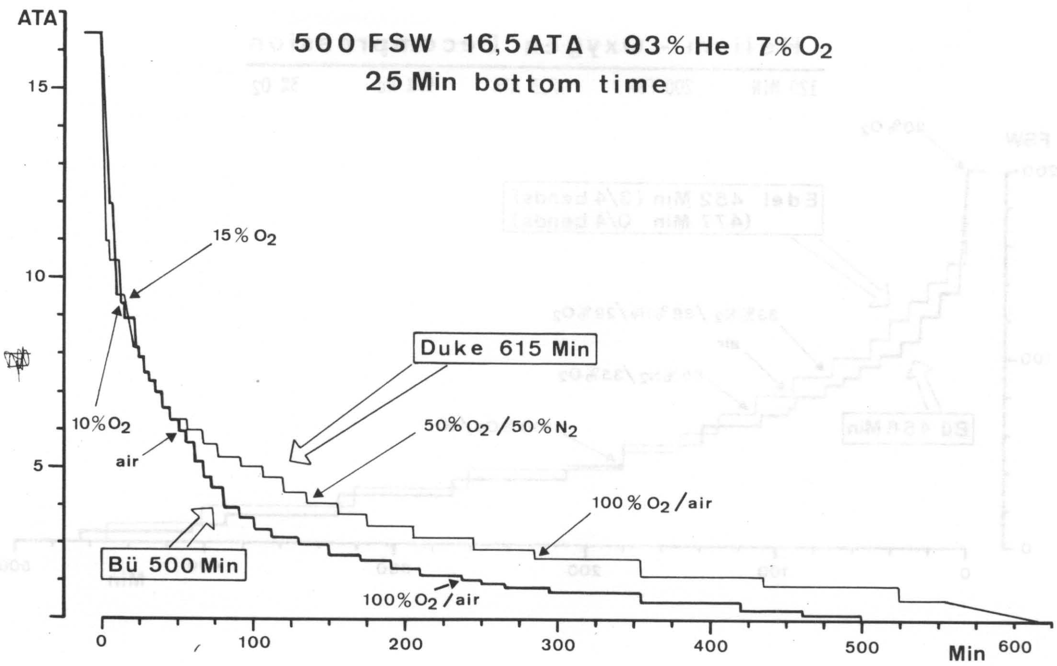


Fig. IIID-3. Comparison of Duke 500-B table with 500 minute Buehlmann.

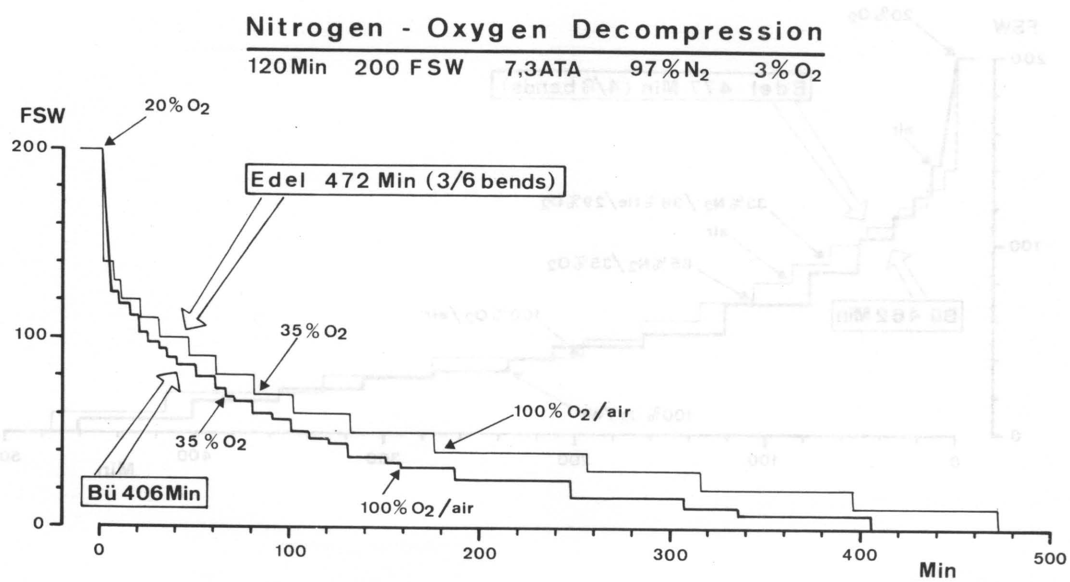


Fig. IIID-4. Comparison of Edel 200 ft/120 min table with established Buehlmann table.

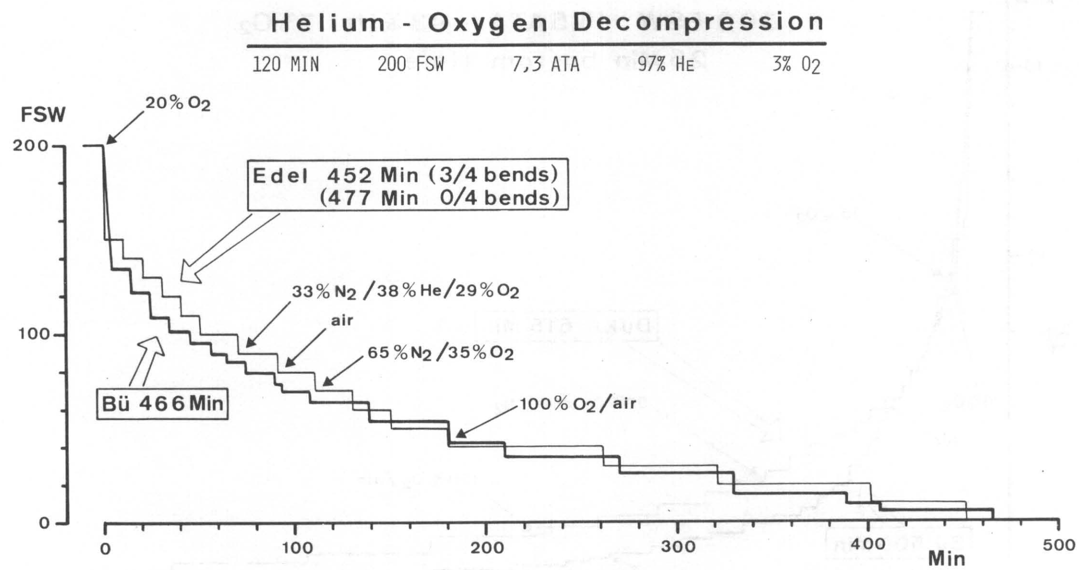


Fig. IIID-5. Similar Edel and Buehlmann tables having similar results.

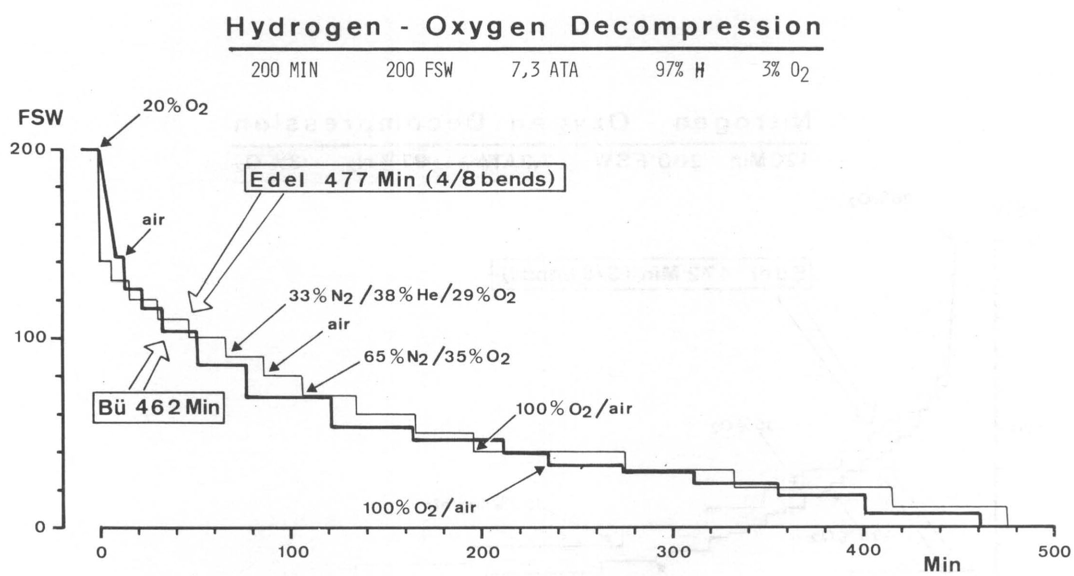


Fig. IIID-6. Edel table of Fig. IIID-6, using hydrogen in place of helium and resulting in bends. Buehlmann comparison has not been tested.

half times are related to the molecular weight, and so on.

Mr. Vann: How fast do you begin your initial decompression?

Dr. Buehlmann: One atmosphere per minute.

Dr. Behnke: Are your divers acclimatized? In other words, do you use the same group repeatedly doing dive after dive, week after week?

Dr. Buehlmann: No, we have 250 persons for experimental dives. It's not possible to make the same person dive every week. All are hobby divers.

Dr. Hills: You showed a comparison between that first Duke dive and your own dive in which Duke was deeper than you were for almost the whole profile, and one of your criticisms of the Duke method was that they were not getting rid of their gas sufficiently. In making that comment weren't you assuming Haldane? Until that's proven I don't think you can criticize what Duke was doing.

Mr. Wilson: We're running into a new phenomenon in the field that doesn't quite agree with your findings regarding hard work. For instance, better than two-thirds of our bends cases are by the line tender in the bell who is not working. The diver may well be 50-75 feet deeper, but it's the tender in the bell who seems to be getting bent. I'll cite another example. In surface diving we did over 600 dives on the west coast and we couldn't find any relationship to hard work. But there the diver did do one thing: he did do moderate exercise during his decompression in the water, which may change what you are saying a little bit, I don't know. But here are two phenomena which don't seem to agree with what you have to say about the work.

Dr. Buehlmann: Yes, I agree. According to our concept, it would be wonderful to do work during decompression and the diver in the water would be a little better off than the tender in the bell. We haven't seen any real difference in our experimental dives. But for this question our experimental dives are not real enough.

Mr. Wilson: It's becoming almost automatic with us. For instance, when the telephone rings with a little case of the bends, I'll say, "Is it the tender again?" And he'll say, "Yes".

Dr. Buehlmann: Absolutely our experience. If I check the records of the dives, tenders have more bends than divers in the water.

Mr. Wilson: Even though the diver is subjected to, say, an additional 75 feet for the entire time of his dive.

Mr. Edel: Are they both breathing the same mixture?

Mr. Wilson: Yes.

Mr. Hughes: Okay, so you have derived a way to compute where you can make the change without producing vestibular problems?

Dr. Buehlmann: Yes, but I will say this. It's a tradition to change to air at 6 ATA (50 meters). We can make it at 40 meters or even at 30 meters; for our saturation dives we make the change at 30 meters (100 feet). There are some advantages and disadvantages.

We have no theory, no concept, we have only a method of calculation. That's all, and according to our experience, this method of calculation applies, without any equal adaptation, to the variety of situations mentioned before.

Dr. Schreiner: Do you subscribe to the standard of acceptance, 12 "clean" man exposures, that was discussed by Dr. Bennett earlier? Is that your standard, or do you have a higher or lower standard?

Dr. Buehlmann: No, we have the same standard.

## REFERENCES

1. Bovin, B., P. W. Straut, R. A. Schibli, and A. A. Buehlmann. Blood coagulation during critical decompression following diving experiments with oxygen-helium. Aerospace Med. 44: 508-512, 1973.
2. Buehlmann, A. A., R. A. Schibli, and H. Gehring. Experimentelle Untersuchungen ueber die Dekompression nach Tauchgaengen in Bergseen bei vermindertem Luftdruck. Schweitz. Med. Wochenschr. 103: 378-383, 1973.
3. Schibli, R. A., and A. A. Buehlmann. The influence of physical work upon decompression time after simulated oxy-helium dives. Helv. Med. Acta 36: 327-342, 1972.

## E. DECOMPRESSION WORK AT TARRYTOWN: R. W. HAMILTON AND D. J. KENYON

### Background

In 1964 when Ocean Systems was originally organized, there was in existence at Union Carbide in Tonawanda, New York, a gas physiology and inert gas biology laboratory managed by Heinz Schreiner. It seemed logical to turn this laboratory into a diving laboratory. In fact, that was one of the motivating factors when Union Carbide formed Ocean Systems, Inc. (OSI).

Leaning heavily on Bob Workman, Chris Lambertsen, Dr. Buehlmann and V. Hempleman, and with the support of Pat Kelley, a young mathematician in the computer center at Tonawanda, Heinz Schreiner launched into decompression technology. I think he was the first person seriously to put decompression onto a digital computer, a Burroughs vacuum tube type. Although cumbersome, it was big enough to get started, and in a few developmental steps the laboratory produced what was called a pragmatic approach to decompression.*

### Structure of the Model

This concept assumes a few things, many of them familiar to you, but worth reviewing.

Inert gas is "conserved" in the body in a transport system--lungs-blood-tissues-blood-lungs. The limiting step in gas transport is assumed to be perfusion. The focus is on the inert gas; the assumption is that when you replace inert gas with oxygen, nothing is there. Gases are assumed to stay in solution. There are no bubbles to be concerned with in this model. We know, of course, that this is not strictly true.

The Haldane school offered the concept of supersaturation, which is that gas can stay dissolved beyond supersaturation, like a salt solution, in a "metastable" state and that a certain degree of supersaturation is "tolerable". This concept fitted what had been observed at the time it was introduced.

To keep track of gases, half-time compartments are assumed. They are called "tissues", but their non-physiological nature discourages the use of this term; we prefer to use the term "compartment". Originally, compartments had a common half time for all gases. Given a partial-pressure-versus-time exposure and a specific half time, each compartment acquired and lost a "gas loading". Most models assume

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*In the Proceedings of the Fourth Symposium on Underwater Physiology.

equal uptake and elimination half times.

Over a given set of compartments at each depth, a certain gas loading (expressed in units of partial pressure, such as feet of seawater) permits ascent to some definite point, usually 10 feet shallower. Ten feet is a typical staging depth.

In a given situation at a given depth, only one compartment has a limiting value. The maximum gas loading that still allows safe ascent is called an M value (M for maximum). If gas loadings are arrayed for all compartments and for a series of stage depths, we have a matrix of ascent-limiting M values.

Schreiner struggled with the use of different gases; they obviously have different properties. He defined the compartments in terms of a common fat-water solubility ratio (a function of the gas) and tissue perfusion. This approach yielded a 15-compartment model, having half times ranging to 139 minutes for helium and 416 minutes for nitrogen. It should be noted that the computer printout he was using had room for exactly 15 columns of data.

All of this produced a model that recognized some (if not all) of the physiological realities of the situation. It allowed the use of different gases, and could be tested and improved experimentally. It worked easily with a digital computer, and could handle a wide variety of profiles, gases, and oxygen levels. This system or model was designated Tonawanda II, and it is very well displayed in the Proceedings of the Fourth Symposium on Underwater Physiology.

## Results with this Model

Now let me review the results of this laboratory program over several years. First, Schreiner produced the Ocean Systems Mark VI tables in 1966. These were based essentially on the Workman model (nine compartments), with a maximum half time of 240 minutes. They were designed as long-line decompression tables, covering the range 200 to 450 fsw. They called for surfacing from 40 feet and recompression to 40 feet for oxygen breathing in the same manner as the surface decompression tables of the U. S. Navy.

The background for these tables was a series of over 80 man-dives in the 500-675 foot range, conducted in the Tonawanda Laboratory in 1965-66. These tables were produced during the course of the diving program by rewriting the program between dives. The tables and the dives supporting them have never been publicly published, but they have been quite successful commercially.

Next, our Laboratory produced the Tonawanda II model and the Mark VII tables. The Mark VI tables and the dives we had done in the laboratory involved a lot of mask breathing. To some extent to reduce diver discomfort, but primarily to avoid extensive gas logistics at sea (many, many quads of pre-mixed gas), we developed a set of tables which utilized enrichment of the chamber with oxygen (up to the limits of fire safety) and a linear or "saturation" decompression when stop times exceeded a certain limit (80 minutes, with 10-foot staging). These tables were used for successful "bounce" dives to 800 and 1000 fsw, and for a commercial dive to 495 feet and a diver lockout at 715 feet. However, the Mark VII tables have not really been put on line commercially.

The Mark VIII tables were the next ones. As diving services began to be needed to deeper than 450 feet, we were called on to extend the Mark VI tables and to redesign them for bell diving.

The "Mark VI-Extended" tables were later designated Mark VIII. These were computed on the Tonawanda II model, and were issued to the field as computer printouts instead of as printed books. By this time we had come to realize that diving tables are living things, constantly changing and growing. Mark VIII was moderately successful; they made Ocean Systems operational at 500 feet. The field results, and the follow-on Mark VIII-A tables will be discussed by Dave Kenyon in Session V of this Workshop.

<u>Neon</u>

The Mark IX neon tables are worth mentioning. As an "inert gas laboratory," before we got into diving, we had studied neon. In fact in the 1965 saturation dive we showed that Neon 75 (a mixture of neon and helium) could be breathed at 650 feet without a meaningful narcotic decrement. Many reasons compelled us to study this gas. Neon preserves the voice, reduces heat losses, and can be produced from atmospheric air, anywhere. We felt it might improve decompression. OSI considered it a potential "secret weapon."

Following development of the Tonawanda II system, Dr. Schreiner and his computer team assembled a pre-programmed IBM-360 integrated diving computation system (IDCS) which made decompression table design available to non-experts like me. Using this system, the laboratory responded to an OSI commitment for commercial diving capability to possibly 680 feet -- deeper than any of our existing tables and far deeper than any previous commercial dive.

To begin with, we had to extend the matrix. Since we were progressing into an area with very few data points, we made an arbitrary linear extension, adding 10 feet to each gas loading value at each depth to get the release value for the stop ten feet deeper. Four experimental dives were performed. These are shown in Figure IIIE-1.

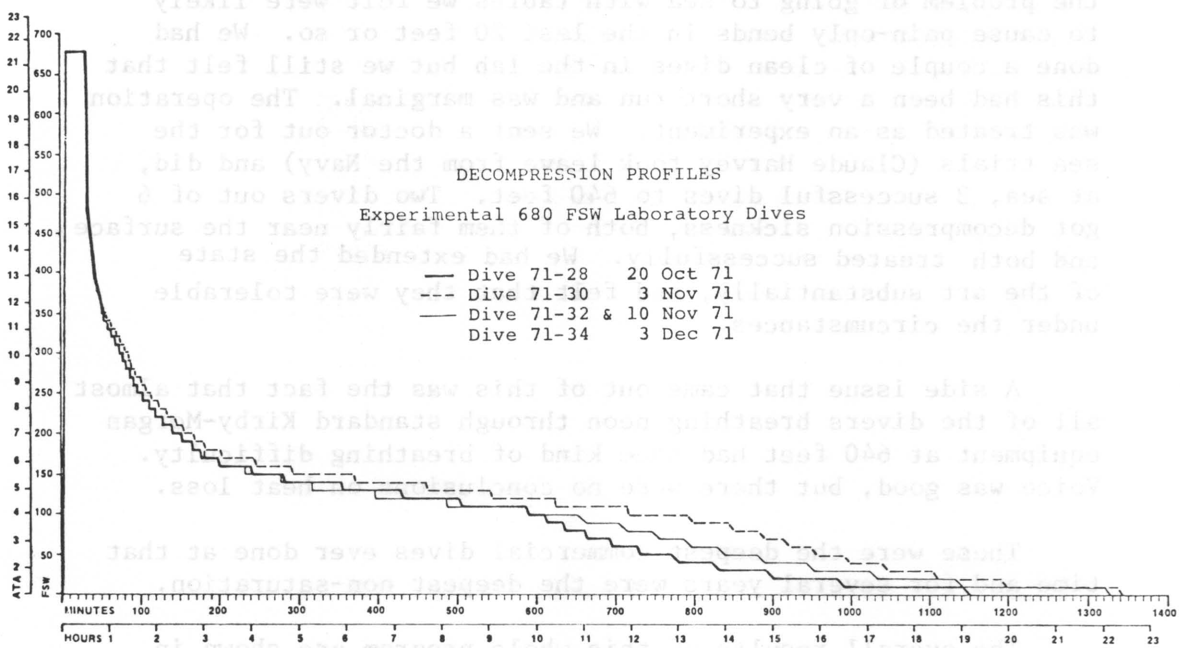



Fig IIIE-1. Profiles of experimental dives with neon. Inert gas breathed until switch to air (about 100 fsw) was Neon 75, 75% neon, 25% helium. First 2 dives resulted in pain-only bends at 80 and 2 fsw, respectively, with the last at 20 fsw.

Another arbitrary choice that we made was to reduce the ascent rate to 30 feet per minute from 60 feet per minute in the initial pull. The 60 had held on from old Navy practice, because it was easy to handle operationally and we went to 30 instead of 25 for the same reason. Abundant evidence has accrued since then to show that slowing down the initial rate of decompression was a good thing to do. A look at the profiles in comparison with more recent work shows that we didn't slow the ascent nearly enough at the deep end, and our results reflected this.

In many ways we were at the mercy of the computer. Operational requirements and practice dictated a shift to air. A series of sample runs disclosed the optimal point to be 100 feet.

When we switched inert gas from neon/helium to nitrogen, the change in driving force caused a distinct shortening of stops. This dip can be seen in the profiles. Our bends came right after the shift. We realized we couldn't do it that way, so we compromised by shifting gradually, ending up with the profile shown by the thin line.

Now, once the laboratory program was over we confronted the problem of going to sea with tables we felt were likely to cause pain-only bends in the last 20 feet or so. We had done a couple of clean dives in the lab but we still felt that this had been a very short run and was marginal. The operation was treated as an experiment. We sent a doctor out for the sea trials (Claude Harvey took leave from the Navy) and did, at sea, 3 successful dives to 640 feet. Two divers out of 6 got decompression sickness, both of them fairly near the surface, and both treated successfully. We had extended the state of the art substantially, and felt that they were tolerable under the circumstances.

A side issue that came out of this was the fact that almost all of the divers breathing neon through standard Kirby-Morgan equipment at 640 feet had some kind of breathing difficulty. Voice was good, but there were no conclusions on heat loss.

These were the deepest commercial dives ever done at that time and for several years were the deepest non-saturation.

The overall results of this whole program are shown in Table IIIE-1. We don't know how many of the dives shown were extended to the full time and depth limit.

Deep Excursion Dives

The oil companies have made it clear that they are interested in drilling in water 1000 to 1500 feet deep, even though the current state of the art is now only 500 feet. In 1973 Ocean Systems began to take steps to respond to this, by supporting a 1000-foot diving program at Tarrytown.

As you know from what has been said up to this point, the orientation of the Laboratory and Ocean Systems has been towards the short deep dive, which is what is usually required in support of offshore drilling. We have never really tried to "go deeper and stay longer," but rather to go deeper and stay as short a time as we could. Carrying on with this philosophy, we decided that the best way to provide access to the 1000-foot diving range would be by means of a combination of fast, short dives and saturation decompression. But we definitely wanted to avoid total saturation at full depth.

Table IIIE-1. Commercial helium experience as of March 1974, Ocean Systems, Inc.

| | Date Issued | Depth Range | Bottom Time Range | Approx. Number Man Exposures | Approx. Bends Incidence |
|---|---|---|---|---|---|
| U. S. Navy, Partial Pressure | NA | 30 - 380 fsw | 0 - 240 min | 366 | 2.4% |
| OSI Modified USN | 1968 | 30 - 380 fsw | 10 - 240 min | 472 | 0.0% |
| OSI MARK VI* | 1968 | 200 - 450 fsw | 5 - 80 min | 2771 | 3.1% |
| OSI MARK VII* | Sept 1972** | 400 - 1400 fsw | 10 - 60 min | 6 | 18.0% |
| OSI MARK VIII* | May 1972 | 400 - 650 fsw | 10 - 60 min | 184 | 10.0% |
| OSI MARK VIII A* | Nov 1973 | 400 - 650 fsw | 10 - 60 min | 29 | 3.5% |
| OSI MARK IX* (neon) | Jan 1972 | 400 - 750 fsw | 10 - 60 min | 18 | 38.8% |

*Developed by the Tarrytown Experimental Diving Laboratory; **experimentally verified August 1969.

One appealing thing about this concept is that it could be used out of the lockout submersible. But to have a set of pre-calculated tables for all types of diving would be very difficult. We are working on a ship-board computer which will allow the optimum decompression situation under all circumstances.

Discussion

Mr. Vann: What was your oxygen partial pressure during decompression from 1000 feet?

Mr. Kenyon: We shifted to a 10% mixture at 600 feet.

Dr. Flynn: Did you get vestibular bends on the neon dives?

Dr. Hamilton: No, these were just ordinary bends -- like knee pain at 70 feet following a shift to air at about 100 feet.

Mr. Edel: Do you regard the initial two dives (Fig. IIIE-2) as acclimatization?

Dr. Hamilton: Yes, and predisposing. They were done for training and base-line data acquisition.

Mr. Hughes: What oxygen partial pressure did you use during the saturation-type decompression?

Dr. Hamilton: Between 0.3 and 0.4 atm partial pressure. We intended to keep it at 0.4. Probably 0.5 to 0.6 would be better.

## F. THE ZERO-SUPERSATURATION APPROACH TO DECOMPRESSION: B. A. HILLS, E. L. BECKMAN, AND J. A. MOORE

There are two basically different approaches to the design of decompression tables. The more popular has involved devising a convenient calculation method and then modifying it empirically by changing the number of equations or the values of the constants in those equations, until it offered a reasonable fit to the practical data available. This is in effect curve-fitting, and can be successful for interpolation, but it tends to prove inadequate when extrapolating to a new region, such as a range of greater depth, for which there is no previous data. Sometimes physical and physiological interpretations of the calculation techniques have been offered subsequently, such as interpreting the decompression ratio of the Haldane method (1) as a metastable limit to supersaturation, or the exponential function as describing gas exchange limited by the blood perfusion rate. However other factors, such as the presence of asymptomatic gas, have not been permitted to interfere with the great advantage of this approach, mathematical simplicity.

The other approach is one of synthesizing a model from the best physical and physiological evidence available and then attempting to quantify its response to changes in the hyperbaric exposure. This method suffers from the fact that an apparently minor refinement may lead to an insuperable increase in the complexity of its mathematical description. However, we prefer to argue that it is better to approximate the quantification of the true model, if known, rather than use a calculation method which does not describe the reality of the situation simply because it is easy to handle arithmetically. Sometimes a model can have a particularly simple response to time (t) such as the $\sqrt{t}$ relationship proposed by Hempleman (4) for the first single-tissue approach to decompression sickness but, unfortunately, this tends to be the exception rather than the rule (7).

The real problem is one of determining the true model and, even then, there is the question of any individual variation in values for the key dimensions or other critical parameters. However, each feature established adds a constraint which helps to limit the otherwise infinite number of calculation methods which can be devised—each providing a different format for safely returning one diver to the surface following just one particular exposure to pressure. Only one calculation method will prove optimal and capable of extrapolation to all other exposures, and that is the one based upon the true model.

### Vital Issues

Taking the fundamental approach, there are many interesting physical and physiological questions to which one would like to know the answers, but a few cannot be avoided. Answers must be found, or assumed, for each of these before any comprehensive mathematical model can be put forward as a basis for deriving safe decompression tables.

These include:

1) The number of tissue types which can give rise to marginal bends, since this determines the number of independent conditions to be satisfied and hence the number of separate equations to be used in calculating a format.

2) What is the critical parameter determining the imminence of decompression sickness? Is it:

a) the total tension of gas in solution relative to a critical degree of supersaturation which that tissue can tolerate, or

b) the differential pressure of bubbles tending to bend a nerve ending beyond a critical threshold, and hence the local volume of gas separating from solution per unit volume of tissue, or

c) any other parameter?

This issue is also important since differentiation between the first two alternatives involves asking the further question of whether there can be asymptomatic gas present during decompression and, hence, what is the driving force for tissue desaturation during decompression?

3) Is the rate of uptake of inert gas limited by blood perfusion, permeation of one or more membranes or diffusion into a bulk of extravascular tissue and, if so, what shape does this bulk assume?

The Perfusion-Diffusion Controversy

There are many possible transport models; a few basically different types, and their simpler combinations, are shown in Fig. IIIF-1. Blood perfusion has generally been recognized as the process controlling blood-tissue exchange in most tissues (16, 17), i.e., model (b) in Fig. IIIF-1. However, this is open to question, particularly in diving, where Hempleman (5) has indicated that the bounce dive curves for $He:O_2$ and air may intersect. This has led to a re-evaluation of much of the original evidence put forward in support of the perfusion argument, but much would seem equally compatible with limitation by diffusion (11).

It now looks as though neither process can be ignored, with compromises for skeletal muscle, at least, now reducing the range of opinion on the relative perfusion: diffusion contributions to a range of the order of 2:1 (21) to 1:2 (8). It is evident, then, that I personally prefer a model something like (e) in Fig. IIIF-1.

Our latest work has included an analysis of the separation of multiple inert gases from solution in tissue (14). On the basis that bends are caused when the local volume of separated gas exceeds a critical value, the equations can be simplified for the case of breathing 20% $O_2$ to predict that decompression sickness will occur if

$$P < 0.762\ PN_2 + 0.687\ PHe - 6.0 \qquad (1)$$

where all pressures are quoted in fsw, P is the absolute pressure, while $PN_2$ and PHe are the local tensions of $N_2$ and He before decompression from the bottom direct to the surface. This expression enables the empirical time function for nitrogen uptake, $\phi\ N_2\ (t)$, to be obtained from the no-stop air decompression data of Van der Aue et al. (24) (PHe = 0). Knowing $\phi\ N_2$, it is now possible to use Eq. 1 to derive the equally empirical $\phi$He from the heliox no-stop data of Duffner (2) which involved simultaneous nitrogen washout.

It is debatable whether one should take the minimum bends depth of a 33-ft air diver as 38 fsw or 40 fsw on 80:20 $He:O_2$. In either case, Fig. IIIF-2 shows a remarkably close agreement between $\phi N_2$ and $\phi$He, both of these functions now being totally divorced from their solubilities by this analysis. Since helium has a much higher diffusion coefficient than nitrogen, the near identity of the time functions must be taken as strong evidence in favor of a perfusion-limited system.

However, when plotted against time (Fig. IIIF-3), they both show an equally good agreement with the $\sqrt{t}$ relationship of Hempleman (4), a form which is the ultimate approximation of all bulk diffusion approaches for small time-intervals (7).

These major yet apparently conflicting pieces of evidence have been interpreted (14) by a model depicting diffusion of gas through a bulk of tissue in which various regions are randomly perfused to give an overall response following a $\sqrt{t}$ relationship.

The similarity of $N_2$ and He time functions shown in Figs. IIIF-2 and 3 indicates that the only benefit in substituting helium for nitrogen is in increasing the "drop-out" depth, a parameter more dependent upon gas solubility. At least, this is the interpretation as far as decompression sickness is concerned.

However, while diffusion versus perfusion represents a most absorbing academic controversy, the eventual outcome is likely to have much less effect upon the ultimate form of the decompression profile than that of the second vital issue: supersaturation versus phase equilibration.

<u>Supersaturation versus Phase Equilibration</u>

If we return to the three vital issues and ask the same questions of the original Haldane approach, we see that it assumes five tissues, while the simple exponential function is consistent with gas uptake limited by either blood perfusion or membrane permeation--models (b) or (a) in Fig. IIIF-1, respectively. This function is derived from the simple relationship that the rate of transfer of an inert gas such as $N_2$ is directly proportional to the driving force ($\Delta P''N_2$), i.e., to the difference between instantaneous tissue tension ($P''N_2$) and the nitrogen tension in capillary blood ($P'N_2$)

where $P'N_2 = (P-P_W)\ F_{IN2}$, giving: (2)

$$\Delta P''N_2 = P''N_2 - (P-P_W)F_{IN2} \quad (3)$$

where P is the absolute pressure, $F_{IN2}$ is the volume fraction of nitrogen in the breathing mixture, and $P_W$ is a minor correction allowing for dilution of inspired air by water vapor.

In the Haldane method we see that the same functions are used to describe elimination as uptake. This mathematical symmetry between uptake and elimination assumes that the "physics" of the system is essentially the same during decompression as during gas uptake. Hence it assumes that gas has remained in true physical solution during decompression, so that the decompression ratio, or M value, represents a metastable limit to supersaturation. The Haldane calculation method therefore assumes that there is no gas present in the critical tissue(s) of the asymptomatic diver.

However there is much evidence to the contrary, ranging from X-ray data as collated by Ferris and Engel (3), ultrasonic data (22) showing bubbles soon after the conventional first long pull to the surface, electrical conductivity measurements (13), and much indirect evidence reviewed recently (10).

Cavitation by decompression is just one aspect of the general phenomenon of suppressed transformation. In all aspects other than decompression sickness, the old concept of the metastable limit as first proposed by Ostwald, a contemporary of Haldane, has been replaced by one of random nucleation of the new phase, e.g., the formation of clouds in cooling air supersaturated with water vapor or red from yellow phosphorus. If the same phenomenon occurs for gases in vivo, then gas remaining in supersaturated solution in tissue has two alternative routes (Fig. IIIF-4): to diffuse to the nearest blood or to the nearest cavity.

Decompression Philosophy

The philosophy behind decompression optimization is therefore one of changing the conditions so as to encourage the former in relation to the latter.

However, we do not know how profuse nucleation will be on any given occasion. We can only describe the best possible case (Haldane) and the worst possible case (7) where, maybe, there is one micro-

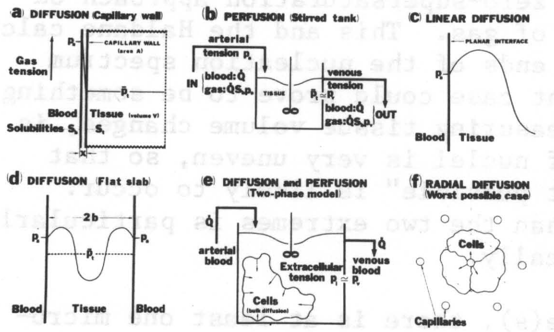



Fig. IIIF-1. Models used to describe mode of uptake of inert gases by tissue.

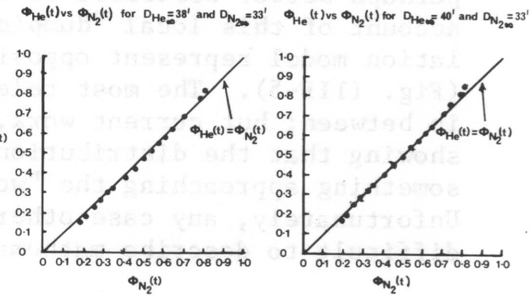
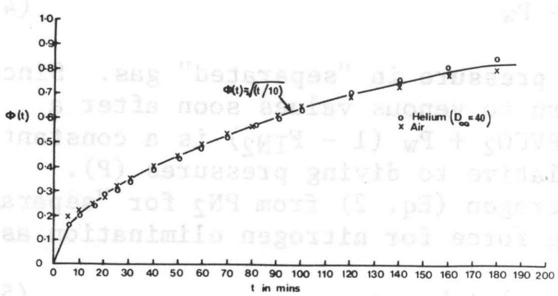



Fig. IIIF-2. Comparison of empirical time functions derived for uptake of helium and nitrogen from no-stop decompression data.



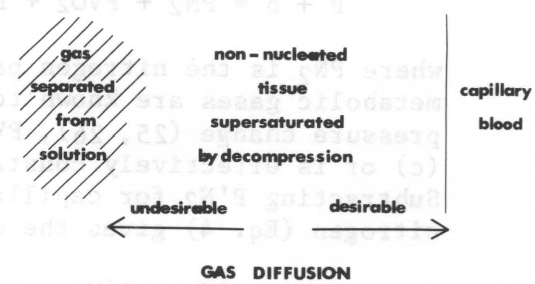




Fig. IIIF-3. Comparison of empirical time functions for uptake of helium and nitrogen with the √t relationship of Hempleman.

Fig. IIIF-4. Competition between capillary blood and cavitated tissue regions for gas remaining in supersaturated solution.

region in which nucleation is so profuse that all gas in excess of equilibrium is "dumped" into the gaseous phase within a few minutes of a reduction in pressure. This is the "worst possible" since not only does it represent the maximum local volume of gas separating from solution per unit volume of tissue, but it then leaves the minimum driving force for eliminating this gas via the circulation. This has formed the basis of the thermodynamic approach, perhaps better described as the zero-supersaturation approach on account of this local "dumping" of gas. This and the Haldane calculation model represent opposite ends of the nucleation spectrum (Fig. IIIF-5). The most relevant case could prove to be something in between; but current work, measuring tissue volume changes, is showing that the distribution of nuclei is very uneven, so that something approaching the "worst possible" is likely to occur. Unfortunately, any case other than the two extremes is particularly difficult to describe mathematically.

If, in our critical tissue(s), there is at least one microregion which cavitates as soon as equilibrium is exceeded (as suggested by conductance measurements (Fig. IIIF-6) then Eq. 3 is only valid until we have taken up the inherent unsaturation--shown for normal airbreathing by the "gas Gamblegram" in Fig. IIIF-7. Hence what will be the driving force for eliminating gas "dumped" into the nearest cavities in this "worst possible case?"

## Driving Force for Elimination During Decompression

Let us consider this region of tissue which is "phase-equilibrated" (Fig. IIIF-8). According to Dalton's Law, the sum of the partial pressures must equal the overall absolute pressure (P + b) where b is a small factor allowing for interfacial forces and tissue compliance, i.e.,

$$P + b = PN_2 + PVO_2 + PVCO_2 + P_w \qquad (4)$$

where $PN_2$ is the nitrogen partial pressure in "separated" gas. Since metabolic gases are known to return to venous values soon after a pressure change (25, 26), $PVO_2 + PVCO_2 + P_w\,(1 - F_{IN_2})$ is a constant (c) or is effectively constant relative to diving pressures (P). Subtracting $P'N_2$ for capillary nitrogen (Eq. 2) from $PN_2$ for "separated" nitrogen (Eq. 4) gives the driving force for nitrogen elimination as

$$\Delta PN_2 = PN_2 - P'N_2 = P(1 - F_{IN_2}) + b - c \qquad (5)$$

This expression is particularly interesting since it predicts that, for a breathing mixture of fixed composition, the driving force for nitrogen elimination will increase with P since $F_{IN_2}$ must be less than unity. Thus the elimination rate will decrease with further decompression with the gas phase present--exactly the reverse of the trend described by the Haldane calculation method assuming no gas phase present (see Eq. 3). Hence it is vital to know whether the gas phase is present in the critical tissue(s).

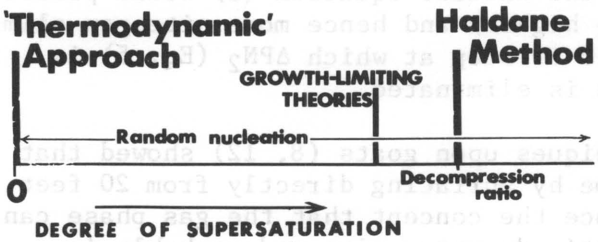



Fig. IIIF-5. Fundamental difference between 3 basically different approaches to tissue gas formation displayed as a spectrum of supersaturation tolerances.

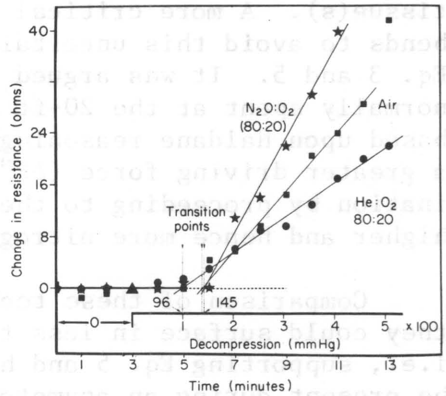



Fig. IIIF-6. Change in resistance across tails of rats that had been breathing mixture of 80% He-20% $O_2$, 80% $N_2$, 0-20% $O_2$, or air for 6 hr before death. Whatever the solubility of the gas, there is a jump at about 100-150 mm of decompression. Apparently, the gas phase is being recorded; on recompression resistance tends to return to the pre-decompression value.

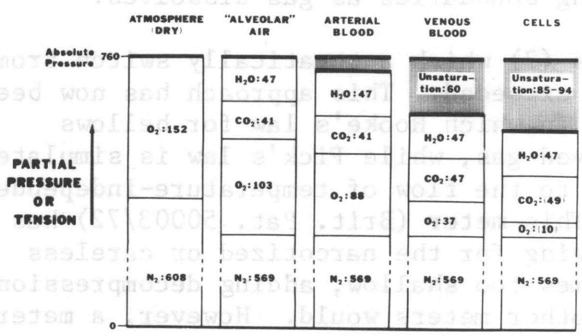



Fig. IIIF-7. A "gas Gamblegram" illustrating tensions of various respiratory gases and their totals.

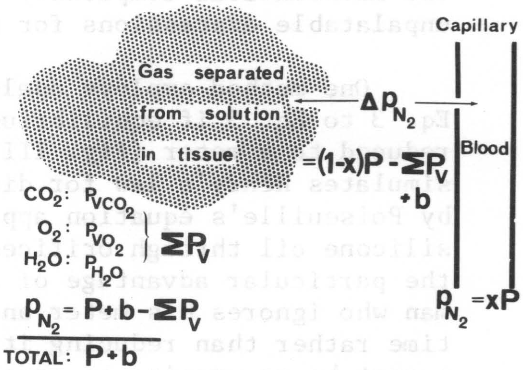



Fig. IIIF-8. Gradient for diffusion of nitrogen from "separated" gas to capillary blood.

## Critical Test

Reference has already been made to approaches involving direct searches for the gaseous phase but, although one does find evidence of gas in the asymptomatic diver, one cannot be sure that the detected gas is in potentially pain-provoking sites or even in the critical tissue(s). A more critical test used the occurrence or absence of bends to avoid this uncertainty and exploited the difference between Eq. 3 and 5. It was argued that upon reaching the end of the time normally spent at the 20-ft stop of a standard USN dive, essentially based upon Haldane reasoning, the Haldane equation (3) would predict a greater driving force ($\Delta P''N_2$ higher) and hence more nitrogen elimination by proceeding to the 10-ft stop at which $\Delta PN_2$ (Eq. 5) is higher and hence more nitrogen is eliminated.

Comparison of these techniques upon goats (8, 12) showed that they could surface in less time by surfacing directly from 20 feet, i.e., supporting Eq. 5 and hence the concept that the gas phase can be present during an asymptomatic decompression and probably is present during those computed by standard methods. It also suggests that standard Navy tables are really treatment tables, i.e., treating bubbles by keeping them below a pain-provoking volume, rather than preventing their formation as the mathematical symmetry of the Haldane calculation method would give us to understand.

## Invalidity of Exponential

If the gas phase is present, it is no longer enough simply to reduce the time constant empirically as soon as decompression commences. It means that the simple exponential is no longer the relevant mathematical function, since it cannot be derived from Eq. 5--simply, at least.

It is easy to critize the Haldane calculation method on the basis of fundamental inadequacy, but what can be recommended in its place? As soon as one introduces the gaseous phase into any tissue model, the mathematical complexity rises enormously and introduces particularly unpalatable expressions for moving boundaries as gas dissolves.

One method employs analogues (7) which automatically switch from Eq. 3 to Eq. 5 if equilibrium is exceeded. This approach has now been reduced to a meter (Fig. IIIF-9) in which Hooke's law for bellows simulates Henry's law for dissolved gas, while Fick's law is simulated by Poiseuille's equation applied to the flow of temperature-independent silicone oil through orifices. This meter (Brit. Pat. 50003/72) has the particular advantage of allowing for the narcotized or careless man who ignores his meter and comes too shallow, adding decompression time rather than reducing it as other meters would. However, a meter cannot be as precise as computed tables and, if the Haldane concept of limited supersaturation does not hold, then let us look at the opposite end of the supersaturation spectrum (Fig. IIIF-5) and consider zero supersaturation for table computation.

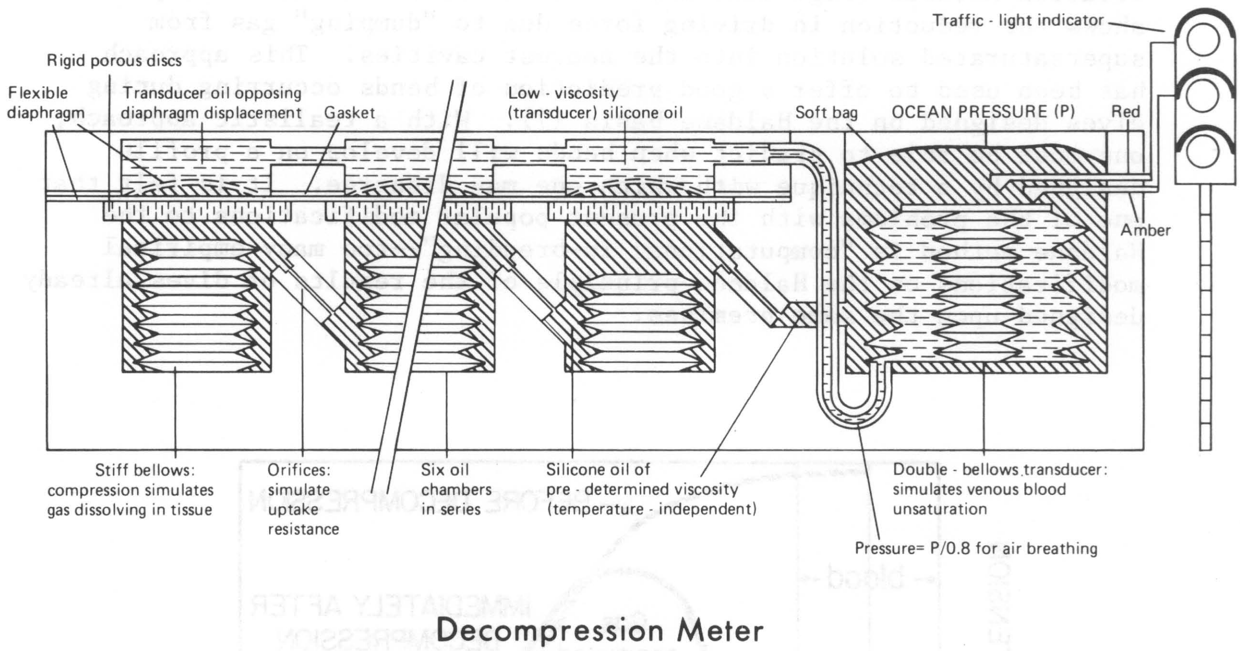



Fig. IIIF-9. A decompression meter based upon zero-supersaturation approach which allows for separation of gas phase from solution in tissue.

## Zero Supersaturation

Before attempting to compute tables, it is necessary to have a transport system to which to apply the zero-supersaturation principle. In Fig. IIIF-3, it is seen that the empirical uptake relationship is best described by $\sqrt{t}$ for time intervals up to 3 hours and that this can be derived on a joint perfusion/diffusion basis. However, it is inadequate for longer times since $\sqrt{\infty}$ is still $\infty$, so an asymptotic function, i.e., one approaching a limit, is needed to describe the system as it approaches steady state.

One such function which also fits the bounce-dive data (9) is that describing radial diffusion from a capillary. Further empirical support for this geometric form comes from the empirical modifications made by Stubbs and Kidd (23) to the volumes and resistances of their "series" pneumatic decompression meter. Hennessy (6) has shown how this model now approximates very closely to one of radial bulk diffusion.

The application of the zero-supersaturation principle to this model now gives a dual distribution, one for gas tension and the other for gas content predicted upon completion of the first long 'pull' towards the surface advocated by conventional Haldanian calculation methods (Fig. IIIF-10). The cutoff at the absolute pressure shows the reduction in driving force due to "dumping" gas from supersaturated solution into the nearest cavities. This approach has been used to offer a good prediction of bends occurring during dives designed on the Haldane basis (7). With a realistic approach, one must be able to predict when bends will develop on a profile designed by a technique with which one may disagree. It is felt that one of the problems with the present popular modifications to the Haldane method is "computational in-breeding"--too many empirical modifications on the Haldane principle of the results of dives already designed upon the same premises.

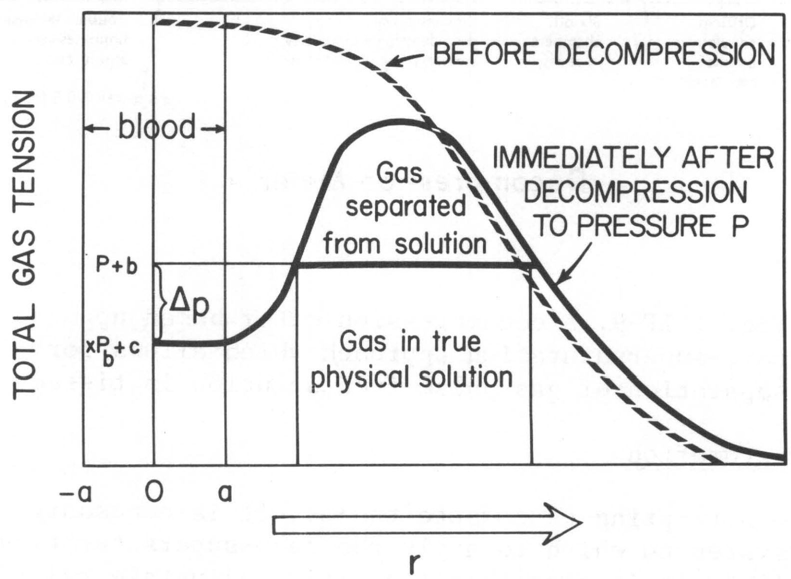



Fig. IIIF-10. Reduction in driving force for gas elimination from tissue which occurs with formation of gaseous phase.

Thus it is interesting that the conventional calculation methods offered no correlation of decompressions based upon no preconceived rationale (20), while zero-supersaturation did (7). One may disagree with what the other man does, but to have a viable hypothesis one must still be able to predict his result.

If the zero supersaturation offers a better correlation of practical data than limited supersaturation, how can it be used to compute tables?

Decompression Optimization

Optimization is simpler than analysis, since the ideal pressure (P) is given by the minimum overall pressure of potential gas (P+b) which just fails to exceed the total tension of all gases at any point in the tissue. However, if correct, gas is not actually formed, so the tension and content distributions (Fig. IIIF-10) coincide. (P+b) is then adjusted until it coincides with the peak total tension as shown in Fig. IIIF-11. This process of continual adjustment is continued until the diver reaches the "drop-out" depth (25 feet on air or 35 feet on 80:20 He:$O_2$) from which he then surfaces directly.

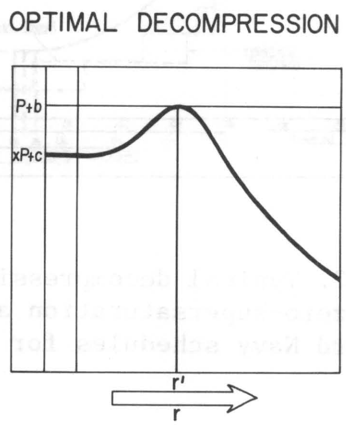



Fig. IIIF-11. Principle of continually adjusting absolute pressure of potential gas phase (P+b) to peak total gas tension in computing a decompression schedule according to zero-supersaturation approach.

The overall scheme is that, by not exceeding equilibrium conditions to this point, he does not cavitate. However, upon reaching this "drop-out" depth he surfaces directly and does precipitate the gas phase, but to just below the pain-provoking volume. The overall idea is that the diver uses the greater depths (higher $\Delta PN_2$ values in Eq. 5) to eliminate the excess gas which his critical tissues would not tolerate at the surface. This leads to a distribution of decompression time: much longer is spent at the deeper stops and further stops are added deeper than used conventionally. However, "dropping out" from deeper than advocated in conventional tables generally leads to a reduction in total decompression time; see Fig. IIIF-12 for profiles of equal bends rate as found on goats.

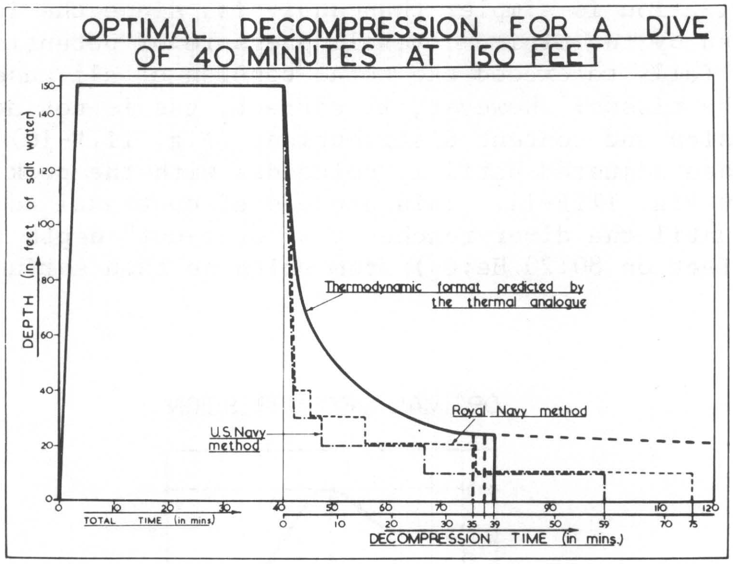



Fig. IIIF-12. Typical decompression profile derived by zero-supersaturation approach compared with standard Navy schedules for same exposures.

Schedule Computation

This phase of the work, converting the fundamental physical and physiological reasoning into practical schedules, has been coordinated by Dr. Edward L. Beckman under a project at Texas A & M University for the last two years. It has relied heavily upon the superb computer at their campus at College Station and, in particular, upon the ability of Mr. Jim Moore to "handle the beast."

The basic model is that of a capillary surrounded by an annulus of tissue in which the total gas tension is estimated at each of twelve different radial locations at 1-min intervals of dive time. Capillary blood tensions are taken as venous values to give a conservative estimate of the total (c) contributed by water and the metabolic gases. This must be less than the absolute pressure P (see Fig. IIIF-11) and lower again than the lowest pressure for cavitation (P+b).

While metabolic gases are conservatively estimated as contributing c throughout the tissue, the change in inert gas tension $\Delta p(r;t)$ at radial location (r) and time (t) after a blood step change of $\Delta P$ is given (7) by

$$\frac{\Delta p(r;t)}{\Delta P} = 1 - \pi . \sum_{o}^{\infty} \frac{[J_o(r\alpha_n).Y_o(a\alpha_n)-Y_o(r\alpha_n).J_o(a\alpha_n)].\exp[(a\alpha_n)^2(Dt/a^2)]}{[J_o(a\alpha_n)/J.(b\alpha_n)]^2 - 1} \quad (6)$$

where $(a\alpha_n)$ is given by

$$J_o(a\alpha_n).Y_1(b'\alpha_n) = Y_o\ (a\alpha_n).J_1(b'\alpha_n) \quad (7)$$

where a is the capillary radius and 2b' is the intercapillary distance (see Figs. IIIF-10, 11), while D is the diffusion coefficient.

Despite their apparently offensive appearance, these expressions have proven particularly conducive to numerical analysis using Legendre quadrature.

## Steps in Computation

The steps in computing any profile can be enumerated as follows:

1) Determine the tension of inert gas at each of twelve different radial locations covering the range a to b' by applying the principle of superposition to Eq. 6

2) Select the highest value and add (c-b) to obtain the near-peak total tension of all gases less the factor b

3) Adjust the absolute pressure P to equal this value

4) Repeat this procedure at 1-min intervals until the "drop-out" depth for that particular inert gas is reached.

## Constants

This single-tissue version of the zero-supersaturation approach has the great advantage that it needs only three constants:

1) A value for $(D/a^2)$ which has the dimensions of $(\text{time})^{-1}$; this is needed in Eq. 6.

2) A value for (b'/a) - a dimensionless index of vascularity

3) A value for the small pressure correction (c-b)

## Schedules

Taking values of $D = 1.32 \times 10^{-12}\ m^2\ s^{-1}$, (b'/a) = 5.0 and (b-c) = 7.7 fsw, schedules have been computed ranging from 30 min at 200 ft on air to 30 min at 500 ft on 93% He + 7% $O_2$, using the same three constants for all tables. The profiles for 15 min at 250 ft on air and 30 min at 500 ft on 93% He + 7% $O_2$ are given in Figs. IIIF-13 and 14, with the helium data given in more detail in Table 1.

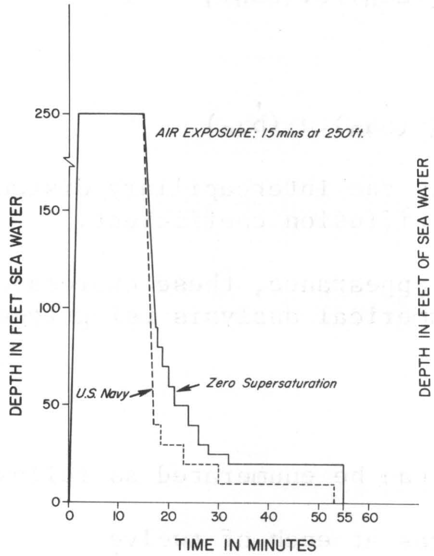



Fig. IIIF-13. Schedule for 15-min exposure at 250 ft on air computed by zero-supersaturation approach, using only three constants.

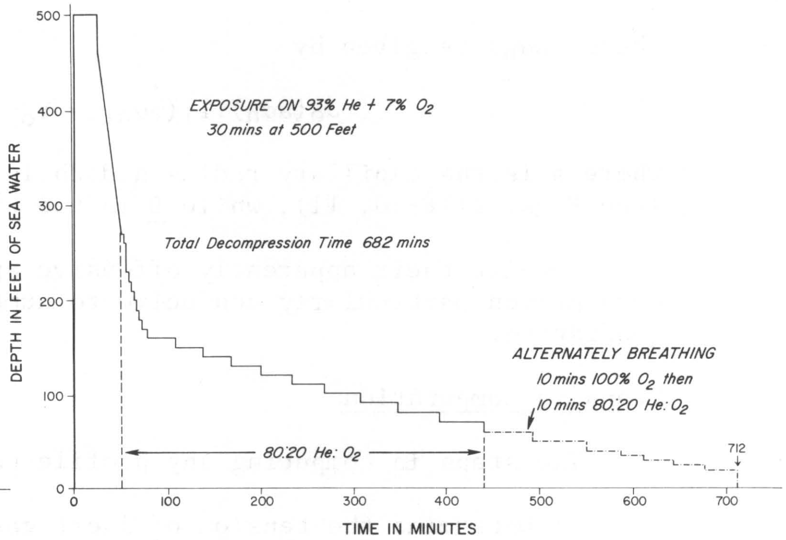



Fig. IIIF-14. Schedule for 30-min exposure at 500 ft on 93:7 He:$O_2$, using the same three constants.

These profiles represent a much more exacting test than the Haldane approach where there is no limit to the number of tissues, and no limits to the ratios and half times allotted to those tissues. Anyone can make any calculation approach the data when given an effectively infinite number of degrees of freedom. The great advantage of the zero-supersaturation approach is the ability to give reasonable profiles for just three constants and hence to extrapolate from dives such as 30 min on air at 200 ft, to 30 min on He:$O_2$ at 500 ft without a prior bank of data on which empirical methods rely for adjustment of constants.

The latest refinement introduced since obtaining the schedules shown in Figs. IIIF-13 and 14 allows for dual optimization of both depth and breathing mix during the exposure.

Dual Optimization

A cumulative oxygen toxicity index (COTi) has been derived (15) which gives an indication of the imminence of the toxic manifestations of oxygen based upon the entire oxygen history of the exposure. This differs from the approach of Lambertsen (19) in so far as it allows for regression of the effects of the early changes as they recede

Table IIIF-1. Schedule for an exposure of 30 min at 500 ft computed by the zero-supersaturation approach.

| | Stop Depth, fsw | Rate to Stop, ft/min | Stop Time, min | Cumulative Times Arrive, min | Cumulative Times Leave, min |
|---|---|---|---|---|---|
| 93% He | S | – | – | – | 0 |
| 7% $O_2$ | 500 | 100 | 25 | 5 | 30 |
| | 460 | 20 | 0 | 32 | 32 |
| | 270 | 10 | 1 | 51 | 52 |
| | 260 | 10 | 1 | 53 | 54 |
| | 230 | 10 | 1 | 57 | 56 |
| | 220 | 10 | 1 | 59 | 60 |
| | 210 | 10 | 1 | 61 | 62 |
| 80% He | 200 | 10 | 1 | 63 | 64 |
| | 190 | 10 | 2 | 65 | 67 |
| | 180 | 10 | 3 | 68 | 71 |
| 20% $O_2$ | 170 | 10 | 6 | 72 | 78 |
| | 160 | 10 | 32 | 79 | 111 |
| | 150 | 10 | 28 | 112 | 140 |
| | 140 | 10 | 29 | 141 | 170 |
| | 130 | 10 | 31 | 171 | 202 |
| | 120 | 10 | 32 | 203 | 235 |
| | 110 | 10 | 35 | 236 | 271 |
| | 100 | 10 | 37 | 272 | 309 |
| | 90 | 10 | 40 | 310 | 350 |
| | 80 | 10 | 43 | 351 | 394 |
| | 70 | 10 | 47 | 395 | 442 |
| Alternate | 60 | 10 | 51 | 443 | 494 |
| 10 min | 54 | 10 | 57 | 495 | 552 |
| oxygen, | 40 | 10 | 30 | 553 | 583 |
| 10 min | 35 | 5 | 29 | 584 | 613 |
| 80% He, 20% $O_2$ | 30 | 5 | 31 | 614 | 645 |
| | 25 | 5 | 32 | 646 | 678 |
| | 20 | 5 | 35 | 679 | 714 |
| | S | 10 | - | 716 | - |

Total decompression time = 696 min, dropping out from 20 ft; 660 min, dropping out from 25 ft.

further into the history.

A means has now been found to link the computer program giving the instantaneous value of this index with that computing the imminence of decompression sickness. Thus we can now offer integration of two programs so that we print out both the optimal depth at any time and the optimal point to switch mixes or alternate between two mixes of different oxygen composition.

Animal Analogue

The last, and rather novel approach being pursued is that of using a small animal as an analogue, similar to the way that miners used canaries to detect toxic gases. At Duke, I found that kangaroo rats tend to bite their tails when given inadequate decompression, and some of these animals are more sensitive than certain divers over the no-stop curve, for exposures up to 2 hours, at least. This concept is now being pursued; these rats may not only provide a good experimental animal, but could provide a "living meter."

Discussion

Dr. Schreiner: I am still chewing over the pearl divers, the divers that got you and LeMessurier into this some 15 years ago.

Dr. Hills: The Okinawans. These people have been diving there for 100-120 years. There were some 900 of them operating out of one of the ports along the northern coast. They were diving to 300 feet on air - only one hour per dive - two dives per day - six days per week, and 10 months per year. This was real diving. I could see no evidence of any medical, mathematical, physiological, or chemical advice ever having been given to these people. By killing maybe 2,000 divers they came up with useful procedures by what I regard as the pure empirical approach. I'm not advocating we repeat that for helium. But what is so interesting is that as people nowadays are becoming more empirical in their approach to helium decompression, they're coming very much back to what these Okinawans did on air. That is, they are using much more time deeper and then dropping out from what I would advocate is 25 feet on air or 35 feet on helium. This, I expect, will form the gas phase, but to just below pain-provoking dimensions.

Dr. Buehlmann: How many dives have you confirmed to 500 feet-30 minutes?

Dr. Hills: We don't have any money. None.

Dr. Buehlmann: You will have many troubles. Our profile is absolutely the same, but we change to nitrogen and then it is working. But with this time, 682 minutes, you will have bends.

Dr. Bennett: I think it's a little short perhaps. One thing, we got into this in a big way as we found the $O_2$ is high, certainly, and the table is a little short in the tail. We found, try as we may, we can't drop out from 35 feet. We can't even drop out from 25 feet. We still need that extra bit in the tail. I don't think we can say why. Maybe we don't have our constants quite right, but with an overall population and the variability of individuals we need that slightly softer and longer tail.

Dr. Hempleman: Could it be that your worst possible case isn't the worst possible case, in the sense that there might be a bubble there before any desaturation starts?

Dr. Hills: Yes, this is a sort of academic problem: do we have cavities present the whole time or not? But it doesn't really matter whether we have nucleation or whether we have cavities already present. The thing is, they won't grow until we exceed equilibrium. So as far as the calculation is concerned, and the profile, it's a purely academic argument.

## REFERENCES

1. Boycott, A. E., C. C. Damant, and J. S. Haldane. The prevention of compressed-air illness. J. Hyg., Camb. 8, 342-443, 1908.
2. Duffner, G. J. Recent studies of helium exchange in diving. In: Man's Dependence on the Earthly Atmosphere, edited by K. E. Schaefer. New York: Macmillan, 1958.
3. Ferris, E. G., and G. L. Engel. The clinical nature of high altitude decompression sickness. In: Decompression Sickness, edited by J. F. Fulton. Philadelphia: Saunders, 1951.
4. Hempleman, H. V. Investigation into the Decompression Tables. III. A New Theoretical Basis for the Calculation of Decompression Tables. Medical Res. Council, RN Pers. Res. Comm. Rept., 1952.
5. Hempleman, H. V. Decompression procedures for deep, open sea operations. In: Proc. 3rd Symp. Underwater Physiology, edited by C. J. Lambertsen. Baltimore: Williams and Wilkins, 1967.
6. Hennessy, T. R. Inert Gas Exchange in Tissue. Ph.D. Thesis, University of Cape Town, 1973.
7. Hills, B. A. A Thermodynamic and Kinetic Approach to Decompression Sickness. Adelaide: Libraries Board of S. Australia, 1966.
8. Hills, B. A. Relevant phase conditions for predicting occurrence of decompression sickness. J. Appl. Physiol. 25: 310-315, 1968.
9. Hills, B. A. The time course for the uptake of inert gases by the tissue type responsible for marginal symptoms of decompression sickness. Rev. Subaqua. Physiol., 1: 255-261, 1969.
10. Hills, B. A. Vital issues in computing decompression schedules from fundamentals. I. Critical supersaturation versus phase equilibration. Int. J. Biochim. Biometeor. 14: 111-131, 1970a.
11. Hills, B. A. Vital issues in computing decompression schedules from fundamentals. II. Diffusion versus blood perfusion in controlling blood: tissue exchange. Int. J. Biometeor. 14: 323-342, 1970b.
12. Hills, B. A. Limited supersaturation versus phase equilibration in predicting the occurrence of decompression sickness. Clin. Sci. 38: 251-267, 1970C.
13. Hills, B. A. Concepts of inert gas exchange in tissue during decompression. In: Proc. 4th Symp. Underwater Physiology, edited by C. J. Lambertsen. New York: Academic Press, 1971.
14. Hills, B. A. Decompression after inspiring various inert gases simultaneously or sequentially. RNPL Rept. 1975a.
15. Hills, B. A. A cumulative oxygen toxicity index allowing for the regression of effects at low inspired oxygen partial pressures. USN Rept., 1975b.
16. Jones, H. B. Gas exchange and blood-tissue perfusion factors in various body tissues. In: Decompression Sickness, edited by J. F. Fulton. Philadelphia: Saunders, 1951.
17. Kety, S. S. Theory and applications of exchange of inert gases at lungs and tissue. Pharmac. Rev., 3: 1-41, 1951.
18. Kety, S. S., and D. F. Schmidt. The determination of cerebral blood flow in man by the use of nitrous oxide in low concentrations. Am. J. Physiol., 143: 53-56, 1945.

19. Lambertsen, C. J. A Quantitative Method for Calculating Cumulative Pulmonary Oxygen Toxicity: Use of the Unit Pulmonary Toxicity Dose (UPTD). Philadelphia: University of Pennsylvania, 1970.

20. LeMessurier, D. H., and B. A. Hills. Decompression sickness: a study of Torres Strait diving techniques. Hvalradets Skrifter, 48:54-84, 1965.

21. Renkin, E. M. Blood flow and transcapillary exchange in skeletal and cardiac muscle. Proc. Internat. Symp. Coronary Circulation and Energetics, edited by G. Marchetti and B. Taccardi. Basel: Karger, 1967.

22. Spencer, M. P., and S. D. Campbell. Development of bubbles in venous and arterial blood during hyperbaric decompression. Bull. Mason Clin. 22: 26-32, 1968.

23. Stubbs, R. A., and D. J. Kidd. A Pneumatic Analogue Decompression Computer. Instl. Aviat. Med. Rept. 65-RD-1. Toronto: Canadian Forces Medical Service, 1965.

24. Van der Aue, O. E., E. S. Brinton, and R. J. Kellar. Surface decompression and testing of decompression tables with safety limits for certain depths and exposures. USN Exp. Diving Unit Res. project X-476, Rept. 1, 1945.

25. Campbell, J. A. Changes in the tensions of $CO_2$ and $O_2$ in gases injected under the skin and into the abdominal cavity. J. Physiol., 59:1-16, 1924.

26. Van Liew, H. D., B. Bishop, P. D. Walder, and H. Rahn. Effects of compression on composition and absorption of tissue gas pockets. J. Appl. Physiol., 20:927-933, 1965.

## G. THEORY AND DEVELOPMENT OF SUB-SATURATION DECOMPRESSION PROCEDURES FOR DEPTHS IN EXCESS OF 400 FEET: P. B. BENNETT AND R. D. VANN

This paper will discuss the results of a unique research program started at Duke University Medical Center in early 1974 in support of the health and safety of the diving industry. The program is unique because all of the data is open and is being published and made freely available, in an effort to break down the commercial secrecy maintained by many commercial companies, who kept their tables and data carefully guarded secrets. This has led to much expensive duplication of data, and the failure to permit free scientific discussion of results precludes satisfactory solutions to the problems of the divers of the company concerned, or the problems of decompression sickness in general. Further, many so-called schedules offered for sale by companies are merely computer extrapolations of a few tested tables. These may only have been tested in a dry pressure chamber and in non-working divers, or on 2 or 3 men at the most.

The incidence of decompression sickness in offshore work at depths greater than 400 ft is thought to be 10% or greater. The tables utilize a considerable amount of oxygen, and numbness of fingers and toes or shortness of breath are reported symptoms of mild oxygen toxicity. The result of continual exposure of sensitive lung tissue to toxic or irritant levels of oxygen could lead to many lung diseases in later life, and such damage should be regarded as at least as dangerous as aseptic bone necrosis or decompression sickness. Further, such tables require the use of so much oxygen that additional oxygen cannot be used in the treatment of decompression sickness without the risk of acute toxicity.

With the boom in offshore oil exploration and production has come an accelerated need for work at greater depths. This has led to the realization that much of the basic knowledge which will permit computation and testing of decompression schedules at depths greater than 400 ft has yet to be done. The regulations now applicable in the North Sea and Norwegian waters, to be followed soon by other waters, require evidence of the safety of decompression tables before such tables may be utilized. There is a vital need for a publicly available body of theoretical data and tested tables, if unnecessary injury to the life and limb of offshore divers is to be prevented.

The research to be described seeks, therefore, answers as to how a man may best be safely decompressed. The work combines an unusual combination of private, university, government, and commercial research funds, and it is undertaken in pressure chambers actually situated in the hospital of a major university medical center, so that specialized treatments are immediately available if required. Credit must go to the Harbor Branch Foundation, Florida, for a research grant which has made the expensive and time-consuming research possible, and to Oceaneering International, Inc., which provided us with proprietary information, divers, and support costs; recently, International Underwater Contractors, Inc. has helped as well.

Such support has enabled two dives per week to be made to 500-600 ft by 3 or 4 men, with one of the men working with his arms ($\dot{V}O_2$ 0.60-2.33 liter·min$^{-1}$STPD) in standard commercial diving equipment in cold water. At the present time some 100 simulated oxygen-helium working dives have been made at depths of either 500 or 600 ft, with a bottom time of 30 min and a compression rate of 100 ft/min inclusive.

Some criteria were set for the testing of tables. These included avoidance of central nervous system decompression sickness (DCS), DCS deeper than 60 ft, vestibular DCS, optimal use of oxygen without toxicity, minimal use of BIBS (Built-in-Breathing Supply) and simplicity of table design, the latter achieved by minimizing the number of gas mixtures and the use of staged rather than linear ascent.

The number of tests required before a decompression table could be considered satisfactory proved difficult to determine. However, an arbitrary standard was agreed upon: not less than 12 dives without decompression sickness would be required.

We decided to standardize the bottom breathing mixture at 7% oxygen, 93% helium. However, in the current 600-ft series the mixture is 7% oxygen, 10% nitrogen, 83% helium; the nitrogen is being utilized to suppress the signs and symptoms of the High Pressure Nervous Syndrome, which is evoked by a compression rate of 100 ft/min (2). It is probably best that the 500 ft-profiles also standardize to this mixture.

Schedule Development

The program started by evaluating an Oceaneering International table for a dive to 500 ft with a 30-min bottom time, including a compression of about 5 min. This table will be called "the parent" (Fig. IIIG-1). The table was calculated, like the majority of commercial decompression tables, on Haldane philosophies (10), with a ratio and tissue half-time matrix evolved through a combination of experience in operational diving and utilization of such other knowledge as was available to individuals who were neither scientists nor clinicians.

As may be seen from Fig. IIIG-1, the table utilized oxygen to the full, as do most commercial tables, with 8% oxygen at depth and an increasing percentage of this gas with decreasing

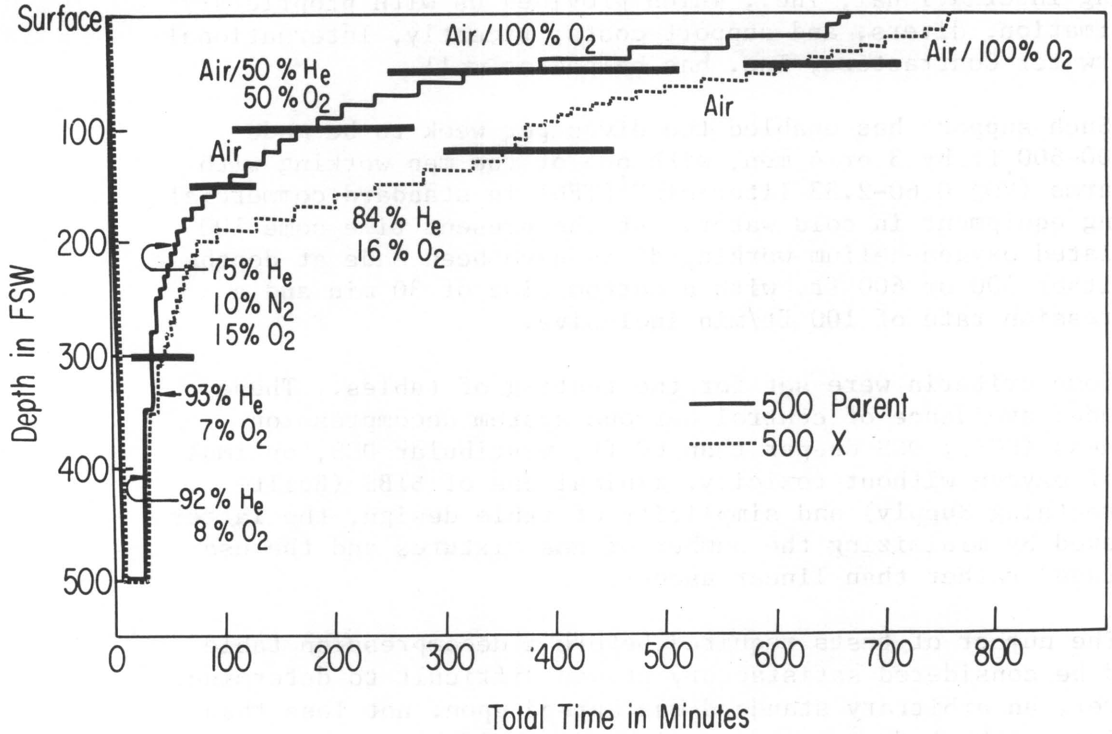



Fig. IIIG-1. Pressure-time plot of the parent table calculated on Haldane principles, and the final product, 500 X-ray, calculated with a combination of Haldane and zero supersaturation concepts.

depth. Nitrogen was incorporated from 300 ft to increase the helium desaturation gradient further, and air was used from 150 ft with alternating air and oxygen from 50 ft. The total time of this table was 666 min, with a decompression time of 636 min (Table IIIG-1).

This table had two problems, vestibular DCS on the air change at some 160 ft, and DCS Type I at 30 to 50 ft. Initially, therefore, in an endeavor not to be forced into any specific computation hypothesis, the air shift was changed to 130 ft to reduce the risk of the nitrogen causing rapid growth of bubbles formed deep, and deep stops were extended, which produced Tables B, C, and D, which were essentially the same. Table B had only a 20-min bottom time, as did Table A, and no DCS occurred in 12 dives. However, with Tables C and D

Table IIIG-1. Parent table, to 500 ft with 30-min bottom time, using 6% $O_2$/94% He.

| Gas | Depth | Travel Time | Stop Time | | Elapsed Time |
|---|---|---|---|---|---|
| 6% $O_2$/94% He or 8% $O_2$/92% He† | 500-230 at 50 ft/min | 3 | - | | 3 |
| | 350-280 at 25 ft/min | 3 | - | | 6 |
| 15% $O_2$/10% $N_2$? 75% He | 280 | - | 4 | | 10 |
| | 280 - 250 | 3 | - | | 13 |
| | 250 | - | 2 | | 15 |
| | 240 | - | 2 | | 17 |
| | 230 | - | 3 | | 20 |
| | 220 | - | 3 | | 23 |
| | 210 | - | 4 | | 27 |
| | 200 | - | 4 | | 31 |
| | 190 | - | 4 | | 35 |
| | 180 | - | 6 | | 41 |
| | 170 | - | 10 ** | | 51 |
| Air linear decompression | 160 | - | ↑ 25 ↓ | Begin slow bleed upon arriving at 160' | 61 |
| | ↓ begin air at 150 ft | | | | 76 |
| | 140 | - | 15 | Resume stage decompression at 140' | 91 |
| | 130 | - | 15 | | 106 |
| | 120 | - | 15 | | 121 |
| | 110 | - | 15 | | 136 |
| 50% $O_2$/50% He mix with air breaks | 100 | - | 20 | | 156 |
| | 90 | - | 20 | | 176 |
| | 80 | - | 30* | | 206 |
| | 70 | - | 40 (20-20*) | | 246 |
| | 60 | - | 40 (30-10*) | | 286 |
| $O_2$ with air breaks | 50 | - | 70 (20-10*-20-20*) | | 356 |
| | 40 | - | 80 (20-10*-20-10*-20) | | 436 |
| | 30 | - | 90 (30*-20-10*-30) | | 526 |
| | 20 | - | 90 (30*-20-10*-30) | | 616 |
| | 20-0 at 1 ft/min | - | 20 (10*-10) | | 636 |

Decompression schedule:

| | |
|---|---|
| 0-700 at 150 ft/min = | 1:20 |
| Stop | :30 |
| 200-400 at 100 ft/min = | 2:00 |
| Stop | :30 |
| 400-500 at 100 ft/min = | 1:00 |
| | 5:20 |

Ventilate lungs well at stops

Elapsed time includes stop time; all travel time from 1st stop is 10 ft/min.
* = air time; ** Note:

1) Complete 170-ft stop and go to 160 ft in 1 min
2) On arrival at 160 ft begin 25 min of linear decompression at a rate of 1 ft/1.25 min (5 ft/6.25 min)
3) When crossing 150-ft mark, begin air changeover
4) Continue linear decompression until arriving at 140 ft
5) Resume stage decompression according to schedule, remembering to add 1 hr to 20-ft $O_2$ stop. †Note: Trimix should be started on board at 350 ft or as soon thereafter as practical.

and a 30-min bottom time, four bends occurred, three in the wet diver and one in a tender. Two of the wet divers were at 100-110 ft, and the other was at the surface. The dry tender incident occurred at 15 ft.

At this time we felt it was advantageous to apply a Haldane matrix to the decompression tables tested so far, to see if additional information could be obtained. Calculations were made of the ratio $\frac{N_2 + He}{\text{ambient}}$ (10) for 16 tissue half times, namely 5, 10, 20, 30, 45, 60, 90, 120, 180, 240, 300, 360, 420, 480, 540, and 600. These were printed out for each stop on one piece of paper, and from this the highest or limiting ratio was selected for each depth to give a "back-bone" of limiting ratios (Table IIIG-2). In addition the $\Delta P$ in fsw was calculated and the PHe and $PN_2$ for each half time. However, our analysis relied primarily on the ratios. The limiting ratios for tables A through O are shown in Table IIIG-3, together with the oxygen toxicity indicator UPTD (Unit Pulmonary Toxic Dose, ( 4 )), total time of the table, number of exposures, and incidence of DCS. Columns G and H in Table IIIG-3 are separated because they involved larger differences between stops than the conventional 10 ft.

We will not discuss the many variations in stop times utilized in the different tables here. However, in developing each table, the ratio and the possible need for reduction to a lower value in areas believed responsible for supersaturation leading to DCS were kept in mind.

We also obtained assistance by using Doppler bubble indicators from probes placed over the heart. In this Haldane-type series of tables, a good correlation was obtained between the incidence of Doppler bubble noise and bends.

As bends developed, ratios were reduced, and the table recalculated and tested with total times of approximately 600 min. However, DCS occurred consistently, mainly in the 20-50 ft depth ranges. These limb bends occurred almost without exception in the wet working diver.

By the time Table J (in Table IIIG-3) was reached it seemed apparent that despite many changes in the ratios, DCS appeared with striking consistency at the 20-to-50-ft stops. The one region of consistent ratios which had not been changed was between 200 and 300 ft where ratios between 1.13 to 1.14 were evident.

Accordingly, the ratios for Table J from 500-200 ft were reduced significantly, so that in the 200-300 ft depths the ratio was between 1.06 to 1.08. Nine man-dives were made (3 wet) with no DCS at the 20-50 ft depth ranges. However, a vestibular bend occurred on the air change at 140 to 130 ft.

Table IIIG-2. Tissue half times and ratios of matrix for parent table

| | 5 | 10 | 20 | 30 | 45 | 60 | 90 | 120 | 180 | 240 | 300 | 360 | 420 | 480 | 540 | 600 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 0 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 |
| 500 | 1.28 | 1.15 | 0.86 | 0.68 | 0.52 | 0.43 | 0.32 | 0.26 | 0.20 | 0.17 | 0.15 | 0.13 | 0.12 | 0.12 | 0.11 | 0.11 |
| 350 | 1.43 | 1.36 | 1.07 | 0.86 | 0.66 | 0.54 | 0.41 | 0.33 | 0.25 | 0.21 | 1.19 | 0.17 | 1.16 | 0.15 | 0.14 | 0.13 |
| 280 | 1.18 | 1.28 | 1.13 | 0.95 | 0.75 | 0.63 | 0.48 | 0.39 | 0.30 | 0.25 | 0.22 | 0.20 | 0.18 | 0.17 | 0.16 | 0.15 |
| 250 | 1.05 | 1.20 | 1.12 | 0.97 | 0.79 | 0.66 | 0.51 | 0.42 | 0.32 | 0.27 | 0.23 | 0.21 | 1.19 | 0.18 | 0.17 | 0.16 |
| 240 | 1.04 | 1.20 | 1.15 | 1.00 | 0.82 | 0.69 | 0.53 | 0.44 | 0.34 | 0.28 | 0.25 | 0.22 | 0.20 | 0.19 | 0.18 | 0.17 |
| 230 | 1.01 | 1.18 | 1.16 | 1.03 | 0.85 | 0.73 | 0.56 | 0.47 | 0.36 | 0.30 | 0.26 | 0.23 | 0.22 | 0.20 | 0.19 | 0.18 |
| 220 | 0.99 | 1.16 | 1.18 | 1.06 | 0.89 | 0.76 | 0.59 | 0.49 | 0.38 | 0.32 | 0.28 | 0.25 | 0.23 | 0.21 | 0.20 | 0.19 |
| 210 | 0.97 | 1.13 | 1.18 | 1.09 | 0.92 | 0.80 | 0.63 | 0.52 | 0.40 | 0.34 | 0.29 | 0.26 | 0.24 | 0.23 | 0.21 | 0.20 |
| 200 | 0.96 | 1.11 | 1.19 | 1.11 | 0.96 | 0.84 | 0.66 | 0.55 | 0.43 | 0.36 | 0.31 | 0.28 | 0.26 | 0.24 | 0.23 | 0.22 |
| 190 | 0.95 | 1.09 | 1.20 | 1.14 | 1.00 | 0.88 | 0.70 | 0.59 | 0.45 | 0.38 | 0.33 | 0.30 | 0.27 | 0.26 | 0.24 | 0.23 |
| 180 | 0.94 | 1.06 | 1.19 | 1.16 | 1.03 | 0.92 | 0.74 | 0.62 | 0.49 | 0.41 | 0.36 | 0.32 | 0.29 | 0.27 | 0.26 | 0.25 |
| 170 | 0.91 | 1.00 | 1.14 | 1.15 | 1.06 | 0.96 | 0.79 | 0.67 | 0.53 | 0.44 | 0.39 | 0.35 | 0.32 | 0.30 | 0.28 | 0.27 |
| 160 | 0.91 | 0.96 | 1.10 | 1.13 | 1.08 | 0.99 | 0.84 | 0.72 | 0.57 | 0.48 | 0.42 | 0.38 | 0.35 | 0.32 | 0.30 | 0.29 |
| 150 | 0.85 | 0.91 | 1.04 | 1.10 | 1.09 | 1.02 | 0.88 | 0.77 | 0.61 | 0.52 | 0.46 | 0.41 | 0.38 | 0.35 | 0.33 | 0.31 |
| 140 | 0.84 | 0.88 | 1.00 | 1.07 | 1.09 | 1.04 | 0.92 | 0.82 | 0.66 | 0.56 | 0.50 | 0.45 | 0.41 | 0.38 | 0.36 | 0.34 |
| 130 | 0.84 | 0.87 | 0.97 | 1.05 | 1.09 | 1.07 | 0.97 | 0.87 | 0.71 | 0.61 | 0.54 | 0.49 | 0.45 | 0.42 | 0.39 | 0.37 |
| 120 | 0.85 | 0.87 | 0.96 | 1.04 | 1.10 | 1.10 | 1.01 | 0.92 | 0.77 | 0.66 | 0.59 | 0.53 | 0.49 | 0.46 | 0.43 | 0.41 |
| 110 | 0.85 | 0.88 | 0.96 | 1.04 | 1.11 | 1.12 | 1.06 | 0.98 | 0.83 | 0.72 | 0.64 | 0.58 | 0.53 | 0.50 | 0.47 | 0.44 |
| 100 | 0.56 | 0.64 | 0.78 | 0.91 | 1.03 | 1.08 | 1.06 | 1.00 | 0.87 | 0.76 | 0.68 | 0.62 | 0.58 | 0.54 | 0.51 | 0.48 |
| 90 | 0.54 | 0.58 | 0.70 | 0.82 | 0.97 | 1.04 | 1.07 | 1.03 | 0.92 | 0.81 | 0.74 | 0.67 | 0.63 | 0.59 | 0.55 | 0.53 |
| 80 | 0.86 | 0.83 | 0.83 | 0.88 | 0.99 | 1.06 | 1.11 | 1.09 | 0.99 | 0.89 | 0.81 | 0.75 | 0.70 | 0.65 | 0.62 | 0.59 |
| 70 | 0.52 | 0.58 | 0.66 | 0.74 | 0.86 | 0.94 | 1.02 | 1.02 | 0.95 | 0.87 | 0.80 | 0.74 | 0.69 | 0.65 | 0.61 | 0.58 |
| 70 | 0.85 | 0.81 | 0.80 | 0.84 | 0.93 | 1.01 | 1.09 | 1.10 | 1.04 | 0.96 | 0.88 | 0.72 | 0.77 | 0.72 | 0.69 | 0.65 |
| 60 | 0.50 | 0.53 | 0.60 | 0.67 | 0.77 | 0.86 | 0.97 | 1.01 | 0.98 | 0.92 | 0.86 | 0.80 | 0.75 | 0.71 | 0.68 | 0.65 |
| 60 | 0.80 | 0.74 | 0.74 | 0.77 | 0.86 | 0.95 | 1.07 | 1.12 | 1.09 | 1.03 | 0.96 | 0.90 | 0.85 | 0.80 | 0.76 | 0.73 |
| 50 | 0.05 | 0.18 | 0.37 | 0.49 | 0.63 | 0.76 | 0.92 | 0.99 | 1.01 | 0.97 | 0.92 | 0.86 | 0.82 | 0.78 | 0.74 | 0.71 |
| 50 | 0.60 | 0.48 | 0.49 | 0.55 | 0.65 | 0.76 | 0.91 | 0.98 | 1.00 | 0.96 | 0.91 | 0.86 | 0.82 | 0.78 | 0.74 | 0.71 |
| 50 | 0.03 | 1.12 | 0.24 | 0.34 | 0.48 | 0.60 | 0.78 | 0.87 | 0.93 | 0.91 | 0.87 | 0.83 | 0.79 | 0.76 | 0.72 | 0.70 |
| 50 | 0.84 | 0.70 | 0.58 | 0.58 | 0.64 | 0.73 | 0.89 | 0.98 | 1.04 | 1.03 | 0.99 | 0.94 | 0.90 | 0.86 | 0.83 | 0.79 |
| 40 | 0.05 | 0.17 | 0.29 | 0.36 | 0.47 | 0.58 | 0.76 | 0.88 | 0.97 | 0.97 | 9.94 | 0.91 | 0.87 | 0.84 | 0.80 | 0.78 |
| 40 | 0.60 | 0.48 | 0.43 | 0.45 | 0.51 | 0.60 | 0.76 | 0.87 | 0.96 | 0.96 | 0.94 | 0.90 | 0.87 | 0.84 | 0.80 | 0.78 |
| 40 | 0.03 | 0.12 | 0.21 | 0.28 | 0.38 | 0.47 | 0.65 | 0.78 | 0.89 | 0.91 | 0.90 | 0.87 | 0.84 | 0.81 | 0.78 | 0.76 |
| 40 | 0.60 | 0.45 | 0.38 | 0.38 | 0.43 | 0.51 | 0.66 | 0.78 | 0.88 | 0.91 | 0.89 | 0.87 | 0.84 | 0.81 | 0.78 | 0.76 |
| 40 | 0.04 | 0.13 | 0.22 | 0.28 | 0.37 | 0.47 | 0.66 | 0.80 | 0.95 | 0.99 | 0.99 | 0.97 | 0.94 | 0.91 | 0.89 | 0.86 |
| 30 | 0.77 | 0.70 | 0.58 | 0.53 | 0.52 | 0.56 | 0.68 | 0.80 | 0.93 | 0.97 | 0.98 | 0.96 | 0.93 | 0.91 | 0.88 | 0.86 |
| 30 | 0.04 | 0.17 | 0.29 | 0.33 | 0.38 | 0.44 | 0.59 | 0.71 | 0.86 | 0.92 | 0.93 | 0.92 | 0.90 | 0.88 | 0.86 | 0.84 |
| 30 | 0.60 | 0.48 | 0.43 | 0.43 | 0.44 | 0.48 | 0.60 | 0.71 | 0.86 | 0.92 | 0.93 | 0.92 | 0.90 | 0.88 | 0.86 | 0.84 |
| 30 | 0.01 | 0.07 | 0.18 | 0.25 | 0.33 | 0.40 | 0.57 | 0.71 | 0.91 | 1.00 | 1.03 | 1.03 | 1.02 | 1.00 | 0.98 | 0.96 |
| 20 | 0.77 | 0.70 | 0.57 | 0.52 | 0.50 | 0.52 | 0.61 | 0.73 | 0.90 | 0.98 | 1.01 | 1.02 | 1.01 | 0.99 | 0.98 | 0.96 |
| 20 | 0.04 | 0.17 | 0.28 | 0.32 | 0.36 | 0.41 | 0.52 | 0.65 | 0.83 | 0.93 | 0.97 | 0.98 | 0.98 | 0.97 | 0.95 | 0.93 |
| 20 | 0.60 | 0.48 | 0.43 | 0.42 | 0.42 | 0.45 | 0.54 | 0.65 | 0.83 | 0.92 | 0.96 | 0.98 | 0.97 | 0.96 | 0.95 | 0.93 |
| 20 | 0.01 | 0.07 | 0.18 | 0.26 | 0.33 | 0.39 | 0.53 | 0.68 | 0.91 | 1.04 | 1.11 | 1.14 | 1.14 | 1.14 | 1.13 | 1.11 |
| 10 | 0.77 | 0.56 | 0.47 | 0.48 | 0.51 | 0.57 | 0.72 | 0.89 | 1.18 | 1.35 | 1.44 | 1.47 | 1.48 | 1.48 | 1.46 | 1.44 |
| 0 | 0.19 | 0.28 | 0.33 | 0.38 | 0.44 | 0.50 | 0.66 | 0.84 | 1.14 | 1.31 | 1.40 | 1.44 | 1.46 | 1.46 | 1.44 | 1.43 |
| 0 | 0.19 | 0.28 | 0.33 | 0.38 | 0.44 | 0.50 | 0.66 | 0.84 | 1.14 | 1.31 | 1.40 | 1.44 | 1.46 | 1.46 | 1.44 | 1.43 |

500 ft for 30 minutes, helium-oxygen.

Table IIIG-3. Haldane-type tables.

| | Depth | Parent | A | C/D | E | F | Depth | G | H | I | J | L | N | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| He/$O_2$ | 500 | 1.28 | 1.27 | 1.27 | 1.27 | 1.27 | 500 | 1.34 | 1.34 | 1.27 | 1.09 | 1.01 | 1.01 | 1.01 |
| | 450 | | | | | | | | | | | 1.26 | 1.26 | 1.26 |
| | 400 | | | | | | | | | | 1.18 | | | |
| | 350 | 1.43 | 1.32 | 1.32 | 1.32 | 1.32 | 330 | 1.25 | 1.22 | 1.32 | | 1.15 | 1.15 | 1.15 |
| | 300 | | | | | | | | | | 1.16 | 1.13 | 1.13 | 1.13 |
| | 290 | | | | | | | | | | 1.15 | 1.10 | 1.10 | 1.10 |
| | 280 | 1.28 | 1.14 | 1.14 | 1.14 | 1.14 | 305 | 1.22 | 1.21 | 1.14 | 1.12 | 1.08 | 1.08 | 1.08 |
| | 270 | | 1.14 | 1.14 | 1.14 | 1.14 | | | | 1.14 | 1.10 | 1.06 | 1.06 | 1.06 |
| | 260 | | 1.13 | 1.13 | 1.13 | 1.13 | | | | 1.14 | 1.08 | 1.06 | 1.06 | 1.06 |
| | 250 | 1.20 | 1.13 | 1.13 | 1.13 | 1.13 | 255 | 1.16 | 1.15 | 1.13 | 1.06 | 1.06 | 1.06 | 1.06 |
| | 240 | 1.20 | 1.13 | 1.13 | 1.13 | 1.13 | | | | 1.13 | 1.06 | 1.06 | 1.06 | 1.06 |
| | 230 | 1.18 | 1.13 | 1.13 | 1.13 | 1.13 | 235 | 1.16 | 1.16 | 1.13 | 1.07 | 1.07 | 1.07 | 1.07 |
| | 220 | 1.18 | 1.14 | 1.14 | 1.14 | 1.14 | | | | 1.13 | 1.07 | 1.07 | 1.07 | 1.07 |
| | 210 | 1.18 | 1.14 | 1.14 | 1.14 | 1.14 | 215 | 1.17 | 1.17 | 1.14 | 1.07 | 1.07 | 1.07 | 1.07 |
| | 200 | 1.19 | 1.15 | 1.15 | 1.15 | 1.15 | 200 | 1.16 | 1.16 | 1.14 | 1.07 | 1.07 | 1.07 | 1.07 |
| | 190 | 1.20 | 1.15 | 1.15 | 1.15 | 1.15 | 190 | 1.15 | 1.15 | 1.15 | 1.07 | 1.07 | 1.07 | 1.07 |
| | 180 | 1.19 | 1.14 | 1.14 | 1.10 | 1.05 | 180 | 1.15 | 1.15 | 1.15 | 1.07 | 1.06 | 1.06 | 1.08 |
| | 170 | 1.15 | 1.11 | 1.11 | 1.06 | 0.99 | 170 | 1.10 | 1.12 | 1.12 | 1.08 | 1.06 | 1.06 | 1.07 |
| | 160 Vest* | 1.13 | 1.11 | 1.11 | 1.02 | 0.96 | 160 | 1.11 | 1.11 | 1.10 | 1.08 | 1.06 | 1.06 | 1.07 |
| Air | 150 Vest** | 1.10 | 1.10 | 1.10 | 1.01 | 0.95 | 150 | 1.11 | 1.11 | 1.10 | 1.08 | 1.07 | 1.07 | 1.09 |
| | 140 | 1.09 | 1.09 | 1.09 | 1.00 | 0.95 | 140 | 1.09 | 1.09 | 1.09 | 1.08 Vest* | 1.07 | 1.07 | 1.09 |
| | 130 | 1.09 | 1.09 | 1.09 | 1.02 | 0.97 | 130 | 1.07 | 1.12 | 1.10 | 1.07 | 1.07 | 1.07 | 1.08 |
| | 120 | 1.10 | 1.11 | 1.10 | 1.05 | 0.99 | 120 | 1.07 | 1.12 | 1.09 | 1.07 | 1.08 | 1.08 | 1.09 |
| | 110 | 1.12 | 1.13 | 1.12 | 1.09 | 1.02 | 110 | 1.09 | 1.12 | 1.08 | 1.06 | 1.08 | 1.08 | 1.08 |
| | 100 | 1.08 | 1.08 | 1.14** | 1.11 | 1.05 | 100 | 1.09 | 1.11 | 1.09 | 1.05 | 1.08 | 1.08 | 1.08 |
| | 90 | 1.07 | 1.07 | 1.16 | 1.15 | 1.09 | 90 | 1.11 | 1.11 | 1.08 | 1.05 | 1.08 | 1.08 | 1.08 |
| | 80 | 1.11 | 1.11 | 1.18 | 1.18 | 1.14 | 80 | 1.13 | 1.12 | 1.09 | 1.06 | 1.08 | 1.08 | 1.09 |
| | 70 | 1.10 | 1.10 | 1.19 | 1.20 | 1.18 | 70 | 1.16 | 1.15 | 1.09 | 1.09 | 1.08 | 1.08 | 1.08 |
| | 60 | 1.12 | 1.13 | 1.24 | 1.25 | 1.18 | 60 | 1.11 | 1.10 | 1.10 | 0.99 | 0.98*$O_2$ | 1.09 | 1.09* |
| $O_2$/Air | 50* | 1.04 | 1.05 | 1.13 | 1.15* | 1.12 | 50 | 1.14 | 1.03 | 1.02* | 1.04 | 1.00 | 1.06 | 1.06*$O_2$ |
| | 40 | 0.97 | 1.00 | 1.06 | 1.09 | 1.03 | 40 | 1.03* | 1.02* | 1.01 | 1.04 | 0.98 | 1.09 | 1.09 |
| | 30** | 1.03 | 1.03 | 1.09 | 1.13 | 1.14 | 30 | 1.06 | 1.07 | 1.08 | 1.03 | 1.15 | 1.26 | 1.22 |
| | 20 | 1.14 | 1.10 | 1.15 | 1.19 | 1.29* | 20 | 1.24 | 1.25 | 1.26 | 1.19 | 1.20 | 1.31 | 1.59 |
| | 16 | | | | | | | | | | 1.30 | 1.31 | 1.45 | |
| | 15 | | 1.38 | 1.27* | 1.33 | | 15 | | | | | | | |
| | 10 | 1.48 | | 1.48 | | 1.63 | 10 | 1.60 | 1.61 | | | | | |
| | 8 | | | | | | | | | | 1.57 | 1.58 | 1.74 | |
| | 5 | | | | | | 5 | | | | | | | |
| | S | 1.46 | 1.56 | 1.62* | 1.96 | 1.66 | S | 1.60 | 1.61 | 1.63 | 1.75 | 1.76 | 1.74 | 1.76 |
| Total Time | | 636 | 575 | 585 | 624 | 749 | | 629 | 570 | 617 | 742 | 796 (4 hr post*) | 971 | 849 |
| $O_2$, UPTD | | 838 | 755 | 697 | 721 | 986 | | $O_2$ 783 | $O_2$ 788 | 819 | 842 | 922 | 847 | 886 |
| Problems | | | - | 4 | 1 | 1 | | 1 | 1 | 1 | 1 | 1 | - | 3 |
| No Exposure | | | 12 | 20 | 12 | 6 | | 3 | 3 | 6 | 9 | 12 | 3 | 21 |

The air change was therefore dropped to 100 ft and Table 500 L resulted from this modification (Table IIIG-4). Four 3-man dives were made, with DCS occurring 4 hr post-dive in one subject, and an oxygen convulsion at 60 ft in another. The table was the longest tested until that time, with the highest UPTD, which accounts for the toxicity, as does the greater time on helium-oxygen rather than air, which induces a higher cerebral oxygen tension (1).

Attempts to achieve a table with the same ratios in a reasonable time but without DCS or oxygen problems continued, with reductions of the oxygen breathing time and depth. However, it became apparent that the oxygen had to be reduced significantly or there would be no leeway for the application of an oxygen treatment; if required such treatment would induce either pulmonary or central nervous system toxicity. If a surfacing ratio in the 1.7 region was to be kept, which by Haldane standards seemed remarkable high, the only way would be by developing a table for well over 1000 min. A change of hypothesis was needed.

We decided to change to the concept of diffusion and nil-supersaturation described in further detail later in this paper. A large number of tables was generated on this basis, using different combinations of diffusion constants and nil decompression constants for both helium and nitrogen (Table IIIG-5). Calculations of the generated table were also made by Haldane methods (Table IIIG-6) for comparison with past tables, and special note was made of the surfacing ratio and the depth of the first stop, as shown in Table IIIG-5.

As may be seen, the first tables in Table IIIG-6, P and Q, produced DCS post-dive with decompression from the last stops at 25 and 30 ft at a rate of 5 ft/min to the surface. Thus, for Table R in Table IIIG-6, the constants were changed for helium to values considered very safe, which produced a very long table. The constants of this table would then be adjusted to decrease the time until DCS occurred. On this basis Tables R and S of Table IIIG-6 did not produce DCS, but Table T again caused DCS some 8 hr post-dive.

Since these tables involved a 5 ft/min decompression to the surface from the 25-ft stop (a feat certainly impossible with Haldanian methods of decompression), we thought that the common problem of post-decompression DCS in this series might be resolved by less drastic decompression from the last stop.

Accordingly, the surfacing ratios were decreased, and air and oxygen, instead of air alone, were introduced, with the air change at 60-65 ft and pure oxygen from 30 ft. However, post-dive DCS continued, and unlike the earlier Haldane tables, was not selective for the wet working diver.

Table IIIG-4. 500 Lima. (Experimental diving schedule only; not for general circulation)

| Gas | Depth | Stop time | Ascent elapsed time (includes stop time) | Total time of dive |
|---|---|---|---|---|
| 7% $O_2$/93% He | 500 | 30 | 0 | 30 |
| | at 50 ft/min (1 min) | | | |
| | 450 | 1 | 1 | 31 |
| | at 25 ft/min (4 min) | | | |
| | 350 | 5 | 6 | 36 |
| | at 20 ft/min (1 min) | | | |
| | 330 | 2 | 8 | 38 |
| | at 20 ft/min (1 min) | | | |
| | 310 | 3 | 11 | 41 |
| 20% $O_2$/80% He | 300 | 3 | 14 | 44 |
| | 290 | 3 | 17 | 47 |
| | 280 | 3 | 20 | 50 |
| | 270 | 3 | 23 | 53 |
| | 260 | 3 | 26 | 56 |
| | 250 | 4 | 30 | 60 |
| | 240 | 4 | 34 | 64 |
| | 230 | 4 | 38 | 68 |
| | 220 | 5 | 43 | 73 |
| | 210 | 5 | 48 | 78 |
| | 200 | 5 | 53 | 83 |
| | 190 | 8 | 61 | 91 |
| | 180 | 10 | 71 | 101 |
| | 170 | 10 | 81 | 111 |
| | 160 | 10 | 91 | 121 |
| | 150 | 15 | 106 | 136 |
| | 140 | 15 | 121 | 151 |
| | 130 | 20 | 141 | 171 |
| | 120 | 20 | 161 | 191 |
| | 110 | 30 | 191 | 221 |

Table IIIG-4. 500 Lima. (Experimental diving schedule only; not for general circulation) (cont'd)

| Gas | Depth | Stop time | Ascent elapsed time (includes stop time) | Total time of dive |
|---|---|---|---|---|
| Air | 100 | 40 | 231 | 261 |
| | 90 | 50 | 281 | 311 |
| | 80 | 60 | 341 | 371 |
| | 70 | 90 | 431 | 461 |
| | 60 | 60(25*-10-25*) | 491 | 521 |
| | 50 | 70(10-25*-10-25*) | 561 | 591 |
| | 40 | 90(30-25*-10-25*) | 651 | 681 |
| | 30 | 35(10-25*) | 686 | 716 |
| | 20 | 60 | 746 | 776 |
| | at 1 ft/min to surface | | 766 | 796 |

Decompression schedule: 0-500 ft at 100 ft/min = 5 min
(Duke, 17 June 1974)
Safe non-saturation diving capability to 1000 ft; 500-ft depth, 30-min bottom time (inclusive compression); gas = 7% $O_2$/93% He. Total dive time: leave surface at time 0; arrive bottom at 5 min; leave bottom at 30 min. * = air time. Four 3-man dives (12 exposures) were made, with one bend in a wet diver 4 hr post-dive, and one $O_2$ convulsion at 60 ft.

Table IIIG-5. Diffusion/Nil Supersaturation Tables

| Schedule | Depth, First Stop | Last Stop To Surface | Surfacing Ratio | He Constants | $N_2$ Constants | Total Time | $O_2$, UPTD | Bends Incidence |
|---|---|---|---|---|---|---|---|---|
| POPPA 1 | 220 | 25 Air | 2.41 | 42/190 | 34/190 | 915 | 721 | All 3 divers, no bends, no bubbles |
| POPPA 2 | 220 | 30 Air | 2.49 | 42/190 | 34/280 | 880 | 714 | TC, knee pain 4 hr post-dive<br>DM, mild shoulder 1½ hr post-dive |
| QUEBEC | 250 | 30 Air | 2.76 | 42/190 | 32/330 | 826 | 735 | DF, both knees 5 hr post-dive<br>CM, onset depth 7 ft, right knee<br>PK, bubbles (wet) |
| ROMEO | 250 | 25 Air | 2.22 | 39/230 | 32/330 | 1017 | 823 | 6 clear |
| SUGAR | 250 | 25 Air | 2.38 | 40/210 | 32/330 | 976 | 772 | 3 clear |
| TANGO | 250 | 25 Air | 2.49 | 41/190 | 32/330 | 865 | 710 | DF, slight bubbles on surface<br>DF, knee pain 8 hr post-dive, 2 clear<br>SN, knee pain 8 hr post-dive, 2 clear (wet) |
| UTAH | 220 | 25 Air and $O_2$ | 2.34 | 41/190 | 32/330 | 863 | 713 | JB, 2-3 ft, left knee (wet), 8 clear |
| VICTOR | 220 | 25 Air and $O_2$ | 2.36 | 41/210 | 32/330 | 833 | 713 | CM, 10 ft, both elbows (wet), 5 clear |
| VICTOR (2) | 210 | 25 Air and $O_2$ | 2.43 | 41/210 | 32/330 | 856 | 742 | JM, both knees at 6 ft (wet), 10 clear<br>DF, both knees 8 hr post-dive |
| VICTOR (3) | 210 | 20 Air and $O_2$ | 2.30 | 41/210 | 32/330 | 875 | 787 | JM, 1 hr post-dive, left knee, 5 clear |
| WHISKEY | 210 | 15 Air and $O_2$* | 2.22 | 41/210 | 32/330 | 688 | 712 | JB, 42 min, right knee, 1 clear<br>DM, 3½ hr, both knees (wet) |
| X-RAY | 310 | 10 Air and $O_2$* | 2.24 | 41/210 | 32/330 | 757 | 780 | 20 clear |

*Alternating air and oxygen.

Consequently, the deep and shallow halves of two unsatisfactory tables were intuitively modified and spliced together. By employing a Haldane model between 310 and 190 ft, an additional 29 minutes of decompression time was added. This table was 500 X, which has produced no decompression sickness in 22 man-dives, including two from Harbor Branch's lockout submersible. Twenty-two decompression tables were tested on 173 man-exposures in the 500 ft/30 min series. The evolution of the shape of the decompression profile is shown in Fig. IIIG-1.

Initial attempts to find constants to fit 500 X-ray were unsuccessful, and the 600 ft/30 min dive series began using the Haldane algorithm in which the ratios were adjusted to simulate 500 X-ray. After four unsuccessful tables were tested, the zero supersaturation/linear diffusion algorithm was again resorted to with an educated guess at the constants. The three tables which were calculated had a bends incidence of 10%. A second attempt to find the constants which fit 500 X-ray was successful except at the break point between the fast and slow ascents. The values determined were 155 min for helium and 220 min for nitrogen. Because the constants which most closely fit 500 X-ray do not reproduce the break-point behavior, it has been necessary intuitively to adjust this point. The break must occur deep enough to give adequate decompression but not so deep as to cause additional uptake of inert gas.

As an initial physiological hypothesis, the diffusion-limited tissue was taken to be articular cartilage having blood and bone on opposite faces. Changes in environmental gas composition were assumed to be transmitted instantaneously to the blood/cartilage interface. It has recently been noted, however, from the work of Simkin and Pizzorno (1), that the synovial fluid between blood and cartilage acts as a flow-limited tissue in the transport of tritiated water. Synovial fluid would also introduce a time delay in the transmission of an environmental inert gas change to the cartilage. It is believed that a calculation algorithm which includes such a delay will reproduce the safe break-point behavior found empirically in Table 500 X-ray.

Finally, we would like to show a Haldane analysis using the computational method we used up to Table J. Figure IIIG-3 shows some of the early dives, including the parent table and Fig. IIIG-4 shows the later ones, including X-ray. Note particularly the high surfacing ratios of the later, more successful dives, as compared with the parent table. Table S, though long, is a safe table. The reason the high surfacing ratios can be tolerated is the softer ratios in the middle range, where X-ray hovers around 1.1 and others were upward of 1.3.

Table IIIG-6. Haldane Analysis of Table IIIG-5.

| Depth | Bends | | No Bends | | Bends | | | | No Bends |
|---|---|---|---|---|---|---|---|---|---|
| | P | Q | R | S | T | U | V | W | X |
| 500 | 1.18 | 0.93 | 1.18 | 1.15 | 0.99 | 0.99 | 0.99 | 0.99 | 0.01 |
| 380 | 1.17 | 1.07 | 1.12 | 1.15 | 1.15 | 1.16 | 1.15 | 1.15 | 1.08 |
| 370 | 1.17 | 1.08 | 1.12 | 1.15 | 1.15 | 1.15 | 1.15 | 1.15 | 1.08 |
| 360 | 1.17 | 1.10 | 1.12 | 1.15 | 1.15 | 1.15 | 1.15 | 1.15 | 1.09 |
| 350 | 1.17 | 1.12 | 1.13 | 1.15 | 1.15 | 1.15 | 1.15 | 1.15 | 1.10 |
| 340 | 1.17 | 1.13 | 1.13 | 1.15 | 1.15 | 1.15 | 1.15 | 1.15 | 1.12 |
| 330 | 1.17 | 1.14 | 1.14 | 1.16 | 1.16 | 1.16 | 1.16 | 1.16 | 1.14 |
| 320 | 1.18 | 1.15 | 1.16 | 1.18 | 1.18 | 1.18 | 1.18 | 1.18 | 1.16 |
| 310 | 1.19 | 1.16 | 1.18 | 1.19 | 1.19 | 1.19 | 1.19 | 1.19 | 1.16 |
| 300 | 1.20 | 1.17 | 1.19 | 1.20 | 1.20 | 1.20 | 1.20 | 1.20 | 1.13 |
| 290 | 1.21 | 1.18 | 1.20 | 1.21 | 1.21 | 1.21 | 1.21 | 1.19 | 1.10 |
| 280 | 1.22 | 1.19 | 1.21 | 1.23 | 1.23 | 1.23 | 1.23 | 1.17 | 1.08 |
| 270 | 1.24 | 1.21 | 1.23 | 1.24 | 1.24 | 1.24 | 1.24 | 1.16 | 1.06 |
| 260 | 1.25 | 1.22 | 1.24 | 1.25 | 1.25 | 1.25 | 1.25 | 1.16 | 1.07 |
| 250 | 1.26 | 1.19 | 1.23 | 1.24 | 1.24 | 1.27 | 1.27 | 1.15 | 1.08 |
| 240 | 1.28 | 1.20 | 1.22 | 1.20 | 1.20 | 1.28 | 1.28 | 1.15 | 1.09 |
| 230 | 1.30 | 1.20 | 1.21 | 1.18 | 1.18 | 1.30 | 1.30 | 1.17 | 1.11 |
| 220 | 1.28 | 1.15 | 1.14 | 1.13 | 1.13 | 1.28 | 1.28 | 1.19 | 1.11 |
| 210 | 1.25 | 1.00 | 1.09 | 1.08 | 1.08 | 1.27 | 1.27 | 1.20 | 1.12 |
| 200 | 1.26 | 0.96 | 1.03 | 1.03 | 1.03 | 1.28 | 1.28 | 1.21 | 1.12 |
| 190 | 1.24 | 0.94 | 1.01 | 1.01 | 1.01 | 1.24 | 1.27 | 1.21 | 1.06 |
| 180 | 1.05 | 0.94 | 0.99 | 0.99 | 0.99 | 1.06 | 1.09 | 1.08 | 0.96 |
| 170 | 0.95 | 0.94 | 0.98 | 0.98 | 0.96 | 0.97 | 1.00 | 0.97 | 0.94 |
| 160 | 0.93 | 0.95 | 0.98 | 0.95 | 0.94 | 0.95 | 0.95 | 0.93 | 0.93 |
| 150 | 0.92 | 0.96 | 0.95 | 0.94 | 0.94 | 0.94 | 0.92 | 0.92 | 0.93 |
| 140 | 0.93 | 0.97 | 0.94 | 0.94 | 0.95 | 0.94 | 0.93 | 0.93 | 0.94 |
| 130 | 0.94 | 0.99 | 0.95 | 0.95 | 0.95 | 0.95 | 0.93 | 0.93 | 0.95 |
| 120 | 0.96 | 1.00 | 0.96 | 0.96 | 0.97 | 0.96 | 0.95 | 0.97 | 0.99 |
| 110 | 0.97 | 1.03 | 0.98 | 0.98 | 0.99 | 0.98 | 0.96 | 1.03 | 1.04 |
| 100 | 1.00 | 1.05 | 1.00 | 1.00 | 1.00 | 1.00 | 0.97 | 1.05 | 1.07 |
| 90 | 1.02 | 1.08 | 1.02 | 1.02 | 1.03 | 1.02 | 0.99 | 1.10 | 1.11 |
| 80 | 1.05 | 1.11 | 1.95 | 1.05 | 1.07 | 1.05 | 1.02 | 1.14 | 1.15 |
| 70 | 1.09 | 1.15 | 1.08 | 1.08 | 1.10 | 1.09 | 1.06 | 1.17 | 1.19 |
| 60 | 1.12 | 1.19 | 1.18 | 1.18 | 1.19 | 1.18 | 1.14 | 1.20 | 1.23 |
| 50 | 1.24 | 1.13 | 1.24 | 1.28 | 1.31 | 1.29 | 1.25 | 1.24 | 1.27 |
| 40 | 1.31 | 1.39 | 1.27 | 1.31 | 1.36 | 1.34 | 1.36 | 1.31 | 1.35 |
| 35 | 1.38 | 1.46 | 1.30 | 1.35 | 1.40 | 1.37 | 1.14 | 1.36 | 1.39 |
| 30 | 1.45 | 1.01 | 1.31 | 1.36 | 1.72 | 1.39 | 1.41 | 1.42 | 1.44 |
| 25 | 1.85 | | 1.69 | 1.76 | | 1.72 | 1.75 | 1.50 | 1.51 |
| 20 | | | | | | | | 1.60 | 1.77 |
| 12 or 15 | | 2.76 | 2.29 | 2.38 | 2.49 | 2.34 | 2.38 | 1.84 | |
| 10 | 2.41 | | | | | | | 2.22 | 1.95 |
| 5 | | | | | | | | | 2.24 |
| S | | | | | | | | | |
| Total time | 915 | 826 | 1017 | 976 | 865 | 863 | 833 | 688 | 757 |
| $O_2$ UPTD | 721 | 735 | 823 | 772 | 710 | 713 | 742 | 712 | 780 |
| Problems | 1 | 2 | - | - | 2 | 1 | 1 | 2 | - |
| Exposures | 6 | 3 | 6 | 3 | 6 | 8 | 6 | 3 | 20 |

Marked areas indicate probable areas of supersaturation when compared with Tables R and S. DCS occurs either when the gas bubbles occurring at around 200 ft expand close to the surface (together with further supersaturation) or when supersaturation increases toward the surface as for Q and T resulting in an over-high surfacing ratio. Apparently, 1.12 is satisfactory at 200 ft, 1.26-1.28 is not.

As with the previous Haldane tables, a study of the data shown in Tables IIIG-5 and 6 indicated that one variable, the depth of the first stop (220-210 ft), had not been changed unduly in the series from P to W. The two tables which gave no trouble both had a first stop at 250 ft. Accordingly, Table X-Ray was constructed (Table IIIG-7) with the first stop at 310 ft; this table consisted of parts of two tables, which will be explained later. Thus the decompression in the early phase (to 200 ft) was Haldanian in character, but with lower ratios (Table IIIG-7) so that an ascent slower than that of the parent table was made. The latter part of the table was calculated on diffusion and nil/supersaturation principles. This would conform to the hypothesis that perfusion factors are initially involved in desaturation of gas while diffusion is involved in the latter stages. This seems realistic, since once the blood has removed the gas from easily accessible tissues with a good blood supply (short half times), diffusion is required to desaturate gas from areas poorly perfused by blood.

Twenty man-dives were made with Table X-Ray without DCS (Table IIIG-7). The Haldane ratios are shown in Table IIIG-6. Further, the UPTD of 780 did not produce oxygen toxicity problems.

The total time of 757 min compares very favorably with the U.S. Navy time for the same depth, (1017 min) and Ocean Systems MK8A (989 min). We understand that these latter tables utilize considerable amounts of oxygen in the last stops at 20 and 10 ft, and DCS is experienced, though to what extent is not known. A variation of Table X-Ray is available which utilizes only air and therefore dispenses with BIBS and the use of oxygen, should this be desireable.

The UPTD concept has not been entirely satisfactory for determining pulmonary oxygen toxicity limits, because it does not take the presence of inert gas which prevents atelectasis and provides some protection against toxicity into account. However, it may be said that, in general, for tables under 1000 min, it is advisable to keep as close to 700 UPTD's as possible, to prevent numbness of fingers and toes and shortness of breath, which may require many days before recovery.

To conclude this general introduction of the methods employed at Duke in developing the 500 X-Ray table, and before we consider the diffusion and nil supersaturation concepts in greater detail, we suggest that Haldanian tables generate gas early in the decompression due to too little time during the deep stages and too shallow a depth for the first stop. Such tables are made effective in practice by using considerable amounts of oxygen near the surface to treat the bubbles generated deep as they expand during the last 30 feet to the surface in accordance with Boyle's Law. In the Haldane diffusion/nil supersaturation methods for

Table IIIG-7. 500 X.

| Gas | Depth | Stop time | Ascent elapsed time (includes stop time) | Total time of dive |
|---|---|---|---|---|
| 7% $O_2$/93% He | 500 | 30.0 | 0 | 30 |
| 16% $O_2$/84% He | 50 ft/min (2.4 min) | | | |
| at 300 ft on BIBS | 380 | 3.6 | 6 | 36 |
| until chamber | 10 ft/min (7 min) | | | |
| makeup | 310 | 1.0 | 14 | 44 |
| 16% $O_2$/84% He | 300 | 3.0 | 17 | 47 |
| | 290 | 3.0 | 20 | 50 |
| | 280 | 3.0 | 23 | 53 |
| | 270 | 3.0 | 26 | 56 |
| | 260 | 3.0 | 29 | 59 |
| | 250 | 3.0 | 32 | 62 |
| | 240 | 3.0 | 35 | 65 |
| | 230 | 3.0 | 38 | 68 |
| | 220 | 4.0 | 42 | 72 |
| | 210 | 4.0 | 46 | 76 |
| | 200 | 4.0 | 50 | 80 |
| | 190 | 16.0 | 66 | 96 |
| | 180 | 36.0 | 102 | 132 |
| | 170 | 34.0 | 136 | 166 |
| | 160 | 36.0 | 172 | 202 |
| | 150 | 39.0 | 211 | 241 |
| | 140 | 42.0 | 253 | 283 |
| | 130 | 46.0 | 299 | 329 |
| Air | 120 | 23.0 | 322 | 352 |
| | 110 | 13.0 | 335 | 365 |
| | 100 | 8.0 | 343 | 373 |
| | 95 | 20.0 | 363 | 393 |
| | 90 | 11.0 | 374 | 404 |
| | 85 | 13.0 | 387 | 417 |
| | 80 | 16.0 | 403 | 433 |
| | 75 | 18.0 | 421 | 451 |
| | 70 | 22.0 | 443 | 473 |
| | 65 | 26.0 | 469 | 499 |
| | 60 | 33.0 | 502 | 532 |
| | 55 | 41.0 | 543 | 573 |
| | 50 | 27.0 | 570 | 600 |
| 100% $O_2$ with air breaks | 45 | 10.0 | 580 | 610 |
| | 40* | 5.0 | 585 | 615 |
| | 40 | 15.0 | 600 | 630 |
| | 35* | 5.0 | 605 | 635 |
| | 35 | 15.0 | 620 | 650 |
| | 30* | 10.0 | 630 | 660 |
| | 30 | 15.0 | 645 | 675 |
| | 25* | 10.0 | 655 | 685 |
| | 25 | 15.0 | 670 | 700 |
| | 20* | 20.0 | 690 | 720 |
| | 20 | 15.0 | 705 | 735 |
| | 10* | 10.0 | 715 | 745 |
| | 10 | 10.0 | 725 | 755 |
| | at 5 ft/min to surface | | 727 | 757 |

Schedule of Duke V7; 500-ft depth, 30-min bottom time (includes compression). Descent from 0-500 ft at 100 ft/min = 5 min. Leave surface: time 0; arrive bottom: 5 min; leave bottom: 30 min. Total exposures on this table = 20 (one 2-man and six 3-man dives), with no hits.

calculating Table X-Ray, the very low Haldane ratios in the head of the table require 3 times more time before the air change at 130 ft (299 min) compared with tables which resulted in vestibular DCS, such as Table J (Table IIIG-8) and the parent table (Table IIIG-1), yet the total decompression times are remarkably similar, because the times close to the surface are shorter than in Haldane-type tables, since theoretically at least there are no gas bubbles to treat. The Doppler measurements appear to confirm this statement, as does the fact that the wet working diver using the Haldane tables was preferentially affected by DCS as a consequence of his taking on more gas due to more efficient perfusion than the resting divers. With the nil supersaturation tables where perfusion was not the problem, the DCS was, as would be expected, random between working and non-working divers and no bubbles were heard in the blood with the Doppler.

## Decompression Table Computation

During the early 1950's, Hempleman (5) noted that for dives requiring no decompression, the dive depth was proportional to the square root of the bottom time. This, he saw, would be the case if diffusion instead of blood flow was the process limiting gas transport in tissues susceptible to pain-only decompression sickness. In 1963, with the aid of a digital computer, A.F. Wittenborn (9) analyzed the diffusion of dissolved gas between blood and a slab of tissue of finite thickness. He was able to show that upon return to the surface after a no-decompression dive, the same volume of gas in excess of ambient pressure was present in the tissue regardless of the depth of the dive. In 1966, as a part of his thermodynamic theory of decompression sickness, B.A. Hills (6) introduced as the criterion for safe ascent from a dive the concept of zero supersaturation, in which the tension of dissolved gases in susceptible tissue is never allowed to exceed the ambient pressure. By combining Wittenborn's gas transport model with Hills' safe ascent criterion, an algorithm can be constructed which may be used in the computation of decompression schedules.

An important characteristic of this algorithm is that only one constant is required for each inert gas used on a dive. Thus, a decompression schedule for an air dive depends on one arbitrary constant, and a decompression schedule for a dive employing helium/nitrogen/oxygen mixes requires two arbitrary constants. While this may appear to be a small number of unknowns when compared to a matrix of M values or ratios, predicting a safe decompression table for a mixed gas dive is nonetheless no easy task.

In theory, the inert gas constants are not at all arbitrary and can be found from experimental measurement of man's tolerance to changing pressure. The best examples of such data are the

Table IIIG-8. 500 Juliet. (Experimental diving schedule only; not for general circulation)

| Gas | Depth | Stop time | Ascent elapsed time (includes stop time) | Total time of dive |
|---|---|---|---|---|
| 10% $O_2$/90% He | 500 | | | |
| (Begin $O_2$ makeup) | at 50 ft/min (2 min) | | | |
| | 400 | | 2 | 32 |
| | at 25 ft/min (2 min) | | | |
| | 350 | 1 | 5 | 35 |
| | at 25 ft/min (1 min) | | | |
| | 325 | 1 | 7 | 37 |
| | at 25 ft/min (1 min) | | | |
| 20% $O_2$/80% He | 300 | 1 | 9 | 39 |
| | 290 | 2 | 11 | 41 |
| | 280 | 3 | 14 | 44 |
| | 270 | 3 | 17 | 47 |
| | 260 | 3 | 20 | 50 |
| | 250 | 3 | 23 | 53 |
| | 240 | 4 | 27 | 57 |
| | 230 | 4 | 31 | 61 |
| | 220 | 5 | 36 | 66 |
| | 210 | 5 | 41 | 71 |
| | 200 | 5 | 46 | 76 |
| | 190 | 6 | 52 | 82 |
| | 180 | 7 | 59 | 89 |
| | 170 | 8 | 67 | 97 |
| | 160 | 10 | 77 | 107 |
| | 150 | 10 | 87 | 117 |
| | 140 | 15 | 102 | 132 |
| Air | 130 | 20 | 122 | 152 |
| | 120 | 20 | 142 | 172 |
| | 110 | 30 | 172 | 202 |
| | 100 | 40 | 212 | 242 |
| | 90 | 50 | 262 | 292 |
| | 80 | 60 | 322 | 352 |
| | 70 | 60 | 382 | 412 |
| 100% with air breaks | 60 | 60(25*-10-25*) | 442 | 472 |
| | 50 | 35(10-25*) | 477 | 507 |
| | 40 | 70(30-25*-10-25*) | 542 | 577 |
| | 30 | 55(10-25*) | 602 | 632 |
| | 20 | 90 | 692 | 722 |
| Air | at 1 ft/min (20 min) | | | |
| | to surface | | 712 | 742 |

500 Juliet schedule, Duke University, June 5. Safe non-saturating diving capability to 650 ft; 30-min bottom time (includes compression). Descent from 0-500 ft at 100 ft/min = 5 min. Leave surface at time 0; arrive bottom: 5 min; leave bottom: 30 min. Ascent rates: 500-400 ft, 50 ft/min; 400-30 ft; 25 ft/min; 300-20 ft, 10 ft/min; 20-0 ft, 1 ft/min. Total exposures on this table = 9 (three 3-man dives). One vestibular-type hit at 130-ft air switch, in a wet diver.

no-decompression limits which were determined for air by Van der Aue (8) and for 80/20-helium/oxygen by Duffner (3, see also Workman, 11). Figure IIIG-2 presents this data and a theoretical approximation to it. However, since both the air and helium/oxygen data were gathered from different groups of men and because certain important information was not included in the publication, it has been possible to estimate only a wide range for the inert gas constants. The nitrogen constant appears to fall between 165 and 330 minutes and the helium constant between 90 and 210 minutes.

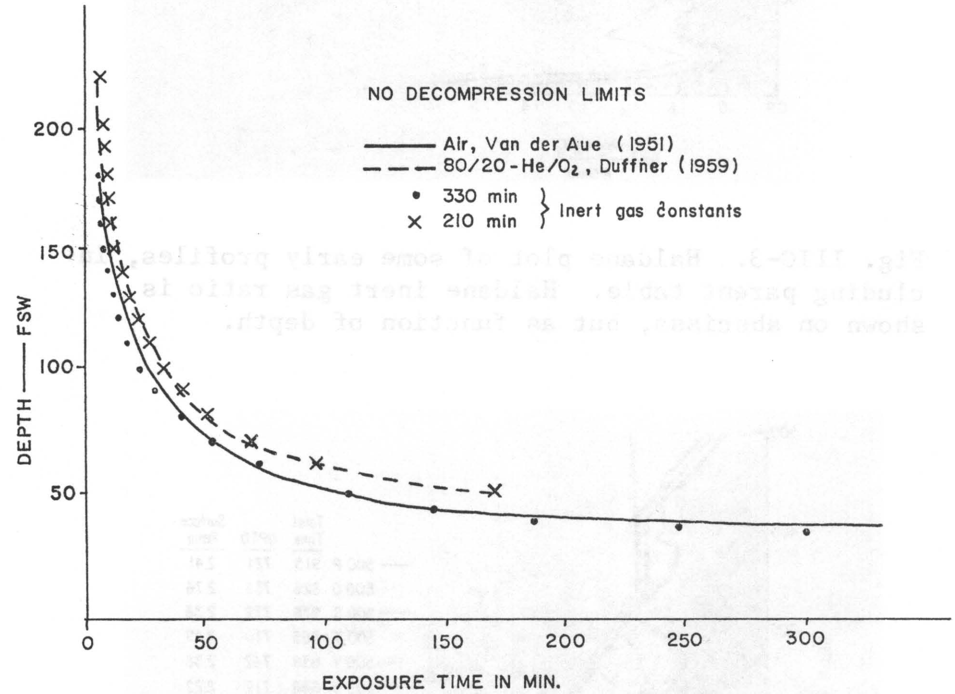



Fig. IIIG-2. Estimation of inert gas constraints from no-decompression data. Data from (3,8).

There is a second way in which the constants can be determined from experimental measurement. The algorithm predicts that if the inspired oxygen partial pressure is held constant during decompression from a saturation dive, the rate of ascent to the surface will be constant. Thus, the maximum safe rate of ascent for an inert gas at a given inspired oxygen partial pressure defines the value of the inert gas constant.

During the 500 ft/30 min dive series, all the constants tried with the zero supersaturation/linear diffusion algorithm produced decompression tables having a 10% bends incidence. Then, shortly before a dive, as luck would have it, the computer broke down and no satisfactory decompression table was available.

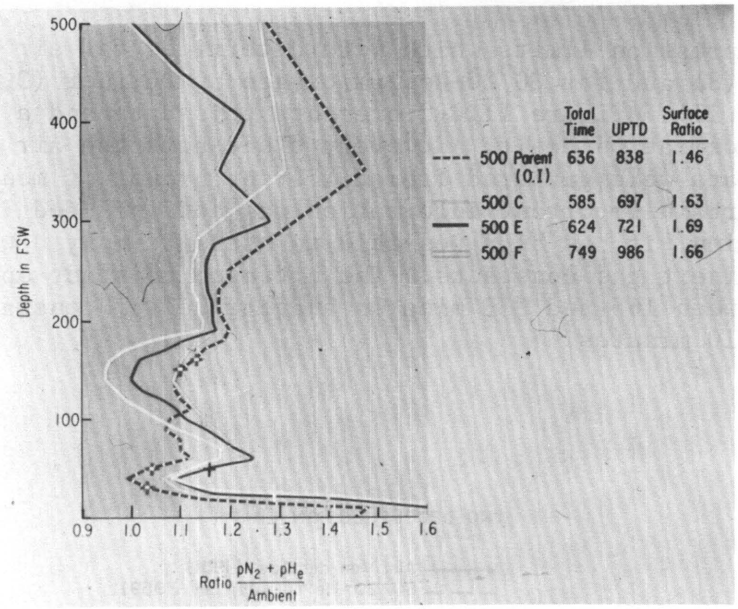



Fig. IIIG-3. Haldane plot of some early profiles, including parent table. Haldane inert gas ratio is shown on abscissa, but as function of depth.

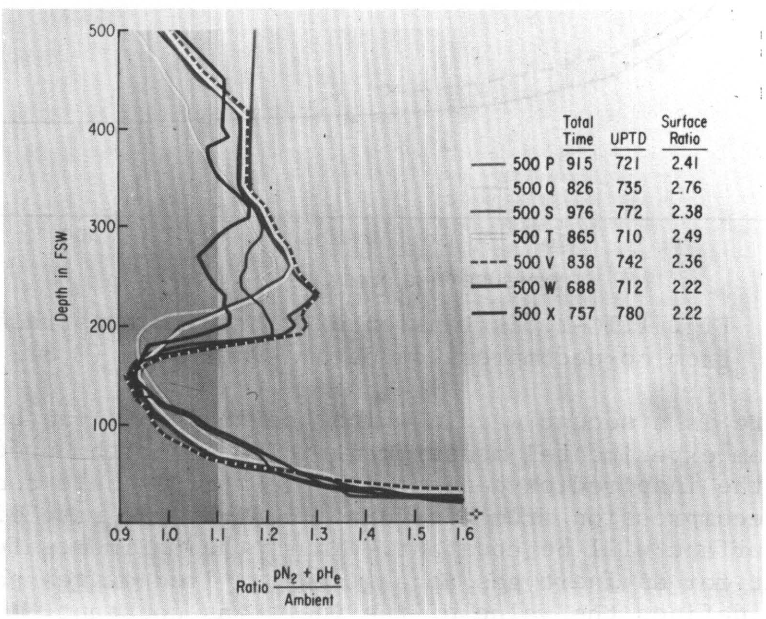



Fig. IIIG-4. Same plot as Fig. IIIG-3, for later dives (including X-ray).

Discussion

Dr. Mueller: I think now we must face the problem that we have several competing profiles which produce no symptoms and we don't really know how to evaluate them. Namely, what criteria shall we use? For example, for these dives--500 feet and 30 minutes--our lab has proposed a decompression of 300 minutes, without symptoms and it is checked. Now, what additional criteria, besides no symptoms, shall we apply to evaluate these different proposals? We could say we have no detected bubbles, no supersautration, or we can look at the UPTD also. But I think now we have no real catalog of criteria.

Dr. Bennett: Well, we do in fact. I did explain in my address initially, that we set for ourselves certain criteria: no bends, no oxygen symptoms, and the UTPD must be under control. We also have diffusion constants which we know will work and not produce bends.

Dr. Mueller: But we proposed a table that is half the time of yours, with no bends, no oxygen toxicity. What criteria do we use to compare ours with yours? Both have no symptoms, no bends, nothing.

Mr. Vann: What kind of oxygen do you use?

Dr. Mueller: We didn't calculate UPTD.

Dr. Bennett: I'd be interested in knowing what the oxygen symptoms were. To get a time like that using Haldane calculations you must have used considerable amounts of oxygen.

Dr. Oser: We only had one fellow suffering a sort of oxygen toxicity, pulmonary. He had a kind of bronchitis before the dive.

Dr. Bennett: But this was using mixture switching of some sort, wasn't it?

Dr. Oser: Yes, we use mask breathing throughout the decompression.

Mr. Hughes: There are a lot of wonderful and magic things you can do to accelerate decompression which are not practical logistically or operationally. This was a very important criterion in the Duke program, that every gas switch had to come at a time when it was not only desirable from a decompression standpoint but was operationally possible. No BIBS breathing, for example, or very minimal.

Mr. Kenyon: I want to point out regarding Vann's last slide, he's receiving some very high ratios at the tail end of the dive. This point perhaps wasn't made clear, ratios of 2.2 to 2.8. Haldane himself said a maximum of 2.0. They were using a 600-minute tissue as the evaluation. Therefore, I interject the point that the 600-minute tissue really exists.

Dr. Hills: But you're pre-judging completely on a Haldane basis.

Mr. Kenyon: I am using Haldane to prove Haldane.

Dr. Behnke: On the vestibular disturbance and the switch to air: did you get this disturbance at the same level if you did not switch to air? Was it associated with the switch to air, and was it clearly vestibular. Did you have true nystagmus, or just dizziness? The switch to air and dizziness is one thing--that's a very common occurrence. But I would say in several thousand dives with a switch from helium to air, we have never had what you describe as a vestibular disturbance. The switch was slow, I would say reasonably within five minutes. The switch was between 100 and 150 feet, but no deeper.

Dr. Bennett: Yes. It's definitely associated with the switch to air. We got severe vertigo, nausea, and nystagmus. There is no doubt it is vestibular; it's very real and very frightening.

Dr. Peterson: I wanted to ask Brian Hills what he has based the $O_2$ toxicity regression equations on. Do you have some experimental evidence?

Dr. Hills: Yes, we do. This is something we did about eight years ago at RNPL. To plus or minus 10% you can use the principle of superposition. It deals with convulsions only. Using superposition and elementary algebra you can account for earlier changes. I have a paper and can post you one if you want it.

Dr. Spaur: I would like to make a comment on the use of work in table testing. I don't think much of the argument about whether leg or arm work is better. Divers pace themselves, for two reasons: one, because they don't want to work that hard; and two, because quite often the rigs won't support them if they work up at 3 liters. But for the Navy helmets and rigs, we would like not to have the rig limit the diver. So, we use the maximum sustained work that they can do, about 3 or 3-1/2 liters oxygen consumption. This has to be leg work, because it's more than a man can do with his arms.

# REFERENCES

1. Bennett, P. B. The Aetiology of Compressed Air Intoxication and Inert Gas Narcosis. Oxford: Pergamon Press, 1966.
2. Bennett, P. B., J. Roby, S. Simon, and D. Youngblood. Optimal use of nitrogen to suppress the High Pressure Nervous Syndrome. Aviat. Space Environ. Med. 46:37-40, 1975.
3. Duffner, G. J., J. F. Snyder, and L. L. Smith. Adaptation of helium-oxygen to mixed gas SCUBA. U.S. Navy Exp. Diving Unit, Washington, D.C., Res. Rept. 3-59, 1959.
4. Feld, J. N., and J. M. Clark. Practical aspects of oxygen tolerance and oxygen toxicity. Quantitation of Unit Pulmonary Toxicity Dose UPTD. Institute for Environmental Medicine Rept., University of Pennsylvania, 1974.
5. Hempleman, H. V. Investigation into the decompression tables. III. A new theoretical basis for the calculation of decompression tables. Med. Res. Council, RN Pers. Res. Comm. Rept, 1952.
6. Hills, B. A. A Thermodynamic and Kinetic Approach to Decompression Sickness. Adelaide, Australia: Libraries Board of South Australia, 1966.
7. Simkin, P. A., and J. E. Pizzorno. Transynovial exchange of small molecules in normal human subjects. J. Appl. Physiol. 36:581-587, 1974.
8. Van der Aue, D. E., R. J. Keller, E. S. Brinton, G. Barron, H. D. Gilliam, and R. J. Jones. Calculation and testing of decompression and use of oxygen. U.S. Navy Exp. Diving Unit, Washington, D.C., Res. Rept. No. 1, 1951.
9. Wittenborn, A. J. An analytical development of a decompression computer. In: Proc. Second Symp. Underwater Physiology, edited by C. J. Lambertsen and L. J. Greenbaum, Jr. Washington, D.C.: Nat. Acad. Sci., Nat. Res. Council, 1963.
10. Workman, R. D. American decompression theory and practice. In: The Physiology and Medicine of Diving and Compressed Work, edited by P. B. Bennett and D. H. Elliott. London: Bailliere, Tindall and Cassell, 1969.
11. Workman, R. D., and J. L. Reynolds. Adaptation of helium-oxygen to mixed-gas SCUBA. U.S. Navy Exp. Diving Unit, Washington, D.C., Res. Rept. 1-65, 1965.

# SESSION IV: SATURATION DECOMPRESSION

## IV. SATURATION DECOMPRESSION

### A. CURRENT WORK AT ALVERSTOKE: H. V. HEMPLEMAN

This material was to have been presented by Dr. Vorosmarti, but he was unable to make the trip. It will be presented in greater detail at the Sixth Symposium in San Diego.

I wish to add to material covered in Session I -- we have done exposures to estimate excursion possibilities and we have some results on the sensitivity of these dives to oxygen. The actual conditions under which we did these dives were standardized, and we answered a number of the points that have arisen at this meeting.

First of all, work was done on every dive during the decompression. People were deliberately put on a rowing machine and rowed, at intervals, so that there were standardized attempts to do work during the decompression. In real life people will be asked to exercise their bodies during the course of a decompression, and therefore it would be foolish to produce procedures which did not allow this. This shows it is quite possible to have successful decompression procedures with fairly heavy work during the decompression.

Another point about helium half times was mentioned. During this technique, saturating at 24 meters and ascending to 10 meters, and so on, we did attempt to see whether we could shorten the time of the bottom stop. In other words, was 24 hours there (at 10 meters) really necessary to forget the fact that you've just done a jump, a big pressure change? This should give us some idea of how quickly the tissues take to saturate. I can tell you that 12 hours is inadequate to enable us to forget the preceding step, so some half time longer than 2 hours must be involved in the elimination of helium during the course of decompression when it is done this way. We found that 24 hours is definitely enough, so we think that 18 hours, about half-way between, is about right. So we would say, as a tentative sort of statement based on these dives, that a 4-hr half time would be overestimating helium transport, and a 2-hr half time would be underestimating it.

This is based solely on normoxic oxygen topped off with helium. There are no complicating aspects as to what the nitrogen is doing or alternations in oxygen partial pressure to confuse what the oxygen is doing. The object of the experiment was to cut out a considerable amount of uncertainty. This is done at the expense of not achieving, necessarily, a practical result, but one hopes that it will be eventually helpful to practical people.

When discussing decompression schedules, many people ignore the possibility that they may not be dealing with a symmetrical situation. Hills referred to this from the point of view of gas elimination. A decompression may produce separated gas, and this upsets the symmetry.

But there are other factors that would upset this symmetry. It is quite clear that if you consider the population as a whole, some people take up gas rather more rapidly than others, and it is quite clear that without gas separation some people would eliminate gas more rapidly than others.

The other thing we've been doing is considering half times for nitrogen and using nitrogen as a substitute for helium. We have started to repeat these helium experiments on nitrogen. We use normoxic oxygen and top off with nitrogen and are decompressing on that. We've taken 48 hours as our bottom time to begin with.

We have done 48 hours at 20 meters gauge and come to 10 meters gauge; that gave trouble after 2 or 3 hours. We have come from 20 meters gauge to 15 meters with the same group of people and that was trouble-free. So we're hoping to get some similar idea of $P_1 = AP_2 + B$ type of result for the air and nitrogen value. We are carrying on with this terribly tedious process, and we hope it will supply useful data for subsequent computations.

Other work at RNPL concerned with decompression and worth mentioning is pathological analysis. We had a number of animals that had decompression sickness, some of them serious decompression sickness, but apparently these animals had recovered and were reasonably normal. One of them was thought by the veterinary surgeon possibly to have a limp in one of its hind legs.

As a colleague in this investigation, we had a man who had been appointed by the United Nations to look into the goat as an animal for use in the Third World, as a source of meat and milk. He's been working with these animals many years and therefore knows the ins and outs of them. He has been sectioning them for us--brains and spinal cord--and we have come up with a finding that confirms Elliott and Hallenbeck, that there is no damage at all in the brain but that on some of them there is extensive damage in the spinal cord.

These particular animals appeared normal. We are hoping to go on like that to give animals less damaging decompression profiles and see whether we pick up trouble from that procedure.

The dives are submarine escape profiles, very short runs to depth and back. We compress them in about 20 seconds to something like 800 feet, hold them at full pressure about 3 seconds, then return them to atmospheric pressure at 8.5 feet per second--about the terminal velocity of a man coming up through the water. Many of them get rather severe decompression sickness. They are treated when it is definite they've got decompression sickness.

Discussion

Dr. Bennett: That's essentially the same kind of profile that Hallenbeck and Elliott used; in fact, that is a "choke-type" profile.

Dr. Bornmann: Is this a unique naval exercise, or a common type of exposure that is seen in human diving?

Dr. Bennett: It may be of interest to submersible operators. The important point is, even though you don't have decompression sickness, you might have spinal cord injury. That could make a lot of difference to what happens in the diving industry.

Dr. Purdy: Have any divers who have been injured in other types of accidents had their spinal cords examined?

Dr. Hempleman: Not that I know of.

Dr. Bennett: In the United Kingdom it is part of Medical Research Council policy, when a diver is killed, to remove the brain and if possible the spinal cord for examination. Detailed instructions are given in the second edition of the Bennett and Elliott book.*

Dr. Smith: We had a case of decompression illness, and three weeks later he committed suicide. His brain and spinal column were examined, and evidence of a severe infarct was seen at about the level of C6.

Dr. Behnke: There is some spinoff here that might be of value to the Air Force. Chokes are common in altitude decompression, and it might be worthwhile to look for spinal damage in those who have experienced them.

Mr. Galerne: Do you think the vestibular problem can also be attached to a problem with the spinal cord? We have a problem with the round window, you know.

Dr. Hempleman: I don't believe there is any connection. Our animals were essentially symptomless. One had an odd gait.

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*P. B. Bennett and D. H. Elliott, editors. The Physiology and Medicine of Diving and Compressed Air Work. London: Bailliere Tindall, 1975.

## B. RECENT U. S. NAVY EXPERIMENTS IN SATURATION-EXCURSION DIVING: E. T. FLYNN AND W. H. SPAUR

### Introduction

In 1966, Larsen and Mazzone (8) demonstrated that the no-decompression curve for air diving could be greatly extended if the dives were made from a shallow saturation depth rather than from the surface. Their finding was the natural sequel to the observations of conventional decompression research that tissues could tolerate greater supersaturation at depth than at the surface. The concept of saturation-excursion diving was quickly extended to deep helium-oxygen diving, where the combination of deep diving systems and the desire to minimize decompression requirements made it both feasible and desirable (9, 14). In 1969, the U. S. Navy developed no-decompression repetitive excursion diving tables for saturation depths ranging from 150 feet to 850 feet. These tables were computed by Bornmann (2, 3) using Workman M values (15) and were extensively tested at the Experimental Diving Unit (10, 11, 12, 13). No decompression sickness was observed in 1123 excursion dives. For a 300-ft saturation dive, these tables allowed an excursion dive to 400 feet for 100 minutes followed by a no-decompression return to the habitat at 300 feet. A comparable 100-ft helium-oxygen dive from the surface has a no-decompression limit of 35 minutes.

The experiments to be described in this paper were undertaken to determine whether the no-decompression limits of the 1969 tables could be extended significantly. Evidence that such an extension might be possible was provided by the recent work of Barnard (1). Although these authors were not studying excursion dives per se, analysis of their data on staged saturation dives indicated that considerably greater levels of inert gas supersaturation than were allowed by the present tables could be tolerated. The experiments described in this report support this conclusion.

### Method

Twenty-five U.S. Navy divers ranging in age from 27 to 38 years served as subjects. Prior to each dive, each subject received a thorough physical examination which included the following:

1) Chest X-ray
2) Long bone X-rays
3) Air conduction audiogram
4) SGOT, SGPT, LDH, CPK, Alkaline Phosphatase, Aldolase
5) Platelet count
6) Vestibular function studies

The latter included measurement of positional nystagmus, optokinetic nystagmus, balance, pendulum tracking, and nystagmus induced by ice-water irrigation of the external auditory meatus. The details of these tests and their significance are discussed by Braithwaite et al. (4).

A total of five saturation dives was performed. Five subjects were used on each dive. Dives 1, 2, 3, and 5 had a saturation base depth of 300 feet. Dive No. 4 had an initial saturation depth of 300 feet, followed by saturation base depths of 600 and 1000 feet. Excursion dives on dives 1, 2, and 3 were from 300 to 400 feet. On dive No. 4, excursions were made from 300 to 400 feet, and from 600 to 750 feet. On dive No. 5, excursions were made from 300 to 450 feet.

On each dive, an initial compression to 14 feet on a 79% helium, 21% oxygen mixture was made to establish a $PO_2$ of 0.3. Compression thereafter to the base saturation depth of 300 feet was performed on pure helium at 5 ft/min. During the saturation phases the $PO_2$ was maintained between 0.30-0.32, the carbon dioxide partial pressure was not allowed to exceed 4 mmHg, and the temperature and humidity was maintained at a level comfortable to the subjects.

Several hours after the initial compression to 300 feet, each subject received an air conduction audiogram and an objective measurement of balance on a statometer (4). These measurements served as controls for the subsequent excursion dives. The statometer measurements were confined to lateral sway. Statometer signals were integrated and recorded at 10-s intervals. Measurements were obtained for 3 minutes with eyes open and 3 minutes with eyes closed. The mean $\pm 2$ SD of the 10-s integrations under the two conditions was obtained and used to compare the subjects' balance following the excursion dives. On dive No. 4, these control measurements were repeated at the 600-ft saturation depth.

All five subjects made each excursion dive simultaneously. The entire chamber complex was compressed on pure helium at 30 ft/min. During compression the $PO_2$ was increased to 0.4 and maintained as close to that level as possible throughout the remainder of the excursion. Carbon dioxide partial pressure, temperature and humidity were controlled as described previously.

During the excursion dives, both dry and wet exercise was performed. In the dry, the subjects pedalled a Monark bicycle ergometer at work rates of 360 and/or 720 $kg \cdot m \cdot min^{-1}$. Ten-minute work periods were separated by 5-min rest intervals. In the wet, graded exercise ranging from mild to severe was performed on an electromagnetically braked upright or horizontal bicycle ergometer. The upright bicycle, in which the subject was in a sitting position, was used in dives 1-4. The horizontal bicycle, in which the subject was in the swimming position, was used on dive No. 5. All wet work sessions consisted of from one to four 10-min work periods separated by 5- or 10-min intervals. of rest.

For the in-water phases of the excursion dives the Mark I open-circuit band mask or the Mark 10 Mod 4 closed-circuit UBA were used. The helium-oxygen supply to the band mask was selected to provide a $PO_2$ of 0.4 at the excursion depth. The Mark 10 oxygen control system was set to maintain a $PO_2$ of 0.4. In actuality, the $PO_2$ ranged from 0.4-0.55, depending on whether the oxygen solenoid valve fired 1, 2, or 3 times during a control cycle.

In all wet work sessions, heart rate was monitored continuously from the output of precordial disc electrodes. When the Mark 10 was used, respiratory rate, mouthpiece pressure, and oxygen consumption were also obtained.

Twenty minutes prior to decompression, all exercise sessions were stopped and the subjects returned to the dry chamber. ENG electrodes for the measurement of horizontal nystagmus were attached and the resistances checked. Decompression to the saturation base depth was then performed at 60 ft/min. For 15 minutes immediately following decompression the subjects were monitored continuously for the development of nystagmus. Each subject then received an audiogram and an objective assessment of balance on the statometer (4). In some experiments, the subjects were also monitored for incipient bubble formation using a precordial Doppler probe. A second 10-min period of electronystagmography followed these tests. These objective examinations and careful clinical observation comprised the immediate monitoring for the development of decompression sickness. In an effort to detect subtle or more long-range effects of decompression sickness, serum enzymes (SGOT, SGPT, LDH, CPK, alkaline phosphatase, aldolase), and platelets were also obtained at periodic intervals throughout each dive.

Decompression from the saturation depth followed the Navy standard saturation decompression schedule.

## Results and Discussion

Dive Number 1. The dive profile is shown in Fig. IVB-1. Five excursion dives from 300 to 400 feet were made with exposure times of 2, 2.5, 3, 3.5 and 4 hours. The first excursion began 24 hours and 10 minutes after the saturation base depth of 300 feet had been reached, and subsequent dives were separated by a saturation interval of at least 24 hours.

No clinical signs of decompression sickness were observed following any of the five excursion dives. No spontaneous nystagmus was observed in the ENG monitoring periods, and balance as determined by the statometer was unaltered from 300-ft control values. An occasional mild (20 dB) depression in auditory threshhold was noted in some audiograms. This effect was transient, however, and appeared to be related to the noise of compression and decompression, as it was not present on the subsequent tests in which noise protectors were worn. No bubble signals were detected by the precordial Doppler, although technically good signals were not obtained in all subjects. Serum enzymes and platelets remained unaltered.

The dive extended the apparent no-decompression limit for a 100-ft excursion dive from 100 minutes to 240 minutes.

Dive Number 2. In view of the favorable results obtained with dive No. 1, dive No. 2 was undertaken in an attempt to extend the duration of a 100-ft excursion to 8 hours. The dive profile is shown in Fig. IVB-2. Six excursion dives from 300 to 400 feet were made with exposure times of 4, 4.5, 5, 6, and 8 hours. The first excursion began 24 hours after the saturation base depth had been reached. Subsequent excursions were separated by an interval varying between 18 and 20 hours.

No symptons or signs of decompression sickness either by clinical examination or by objective evaluation with audiograms, electronystagmography, statometer balance tests, and Doppler monitoring were encountered following any of these excursions. There was, however, a small rise in serum aldolase and a small decrease in platelet count in the later phases of the excursion testing. Aldolase increased from a mean control value of 6.8 to 9.6 following the 5-hr excursion, decreased to 9.1 by the completion of the 8-hr excursion, and returned to control values during the saturation decompression. Platelets were normal following the 5-hr excursion but decreased from a control value of 247,000 to 179,000 following the 8-hr excursion.

Dive Number 3. Encouraged by the results on dive No. 2, dive No. 3 was undertaken to determine (1) whether the safe no-decompression excursion dive time from 300 to 400 feet could be extended to 24 hours and (2) whether multiple excursion dives between 300 and 400 feet could be performed without regard to exposure time or saturation interval. The dive profile is shown in Fig. IVB-3. Twenty-two hours following initial saturation at 300 feet, a 12-hr excursion to 400 feet was conducted, and 14 hours following this dive, a 24-hr excursion dive to 400 feet was carried out. No clinical, objective, or laboratory evidence of decompression sickness was apparent. An interval of 22 hours was allowed to elapse, then four 2-hr excursions to 400 feet, separated by a 1-hr interval at 300 feet, were

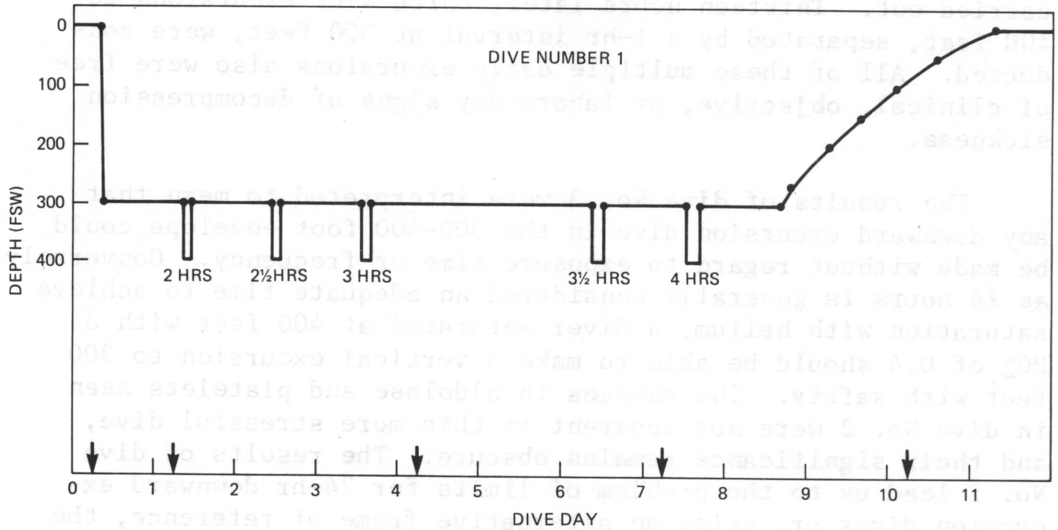


Fig. IVB-1. Profile for dive No. 1. Arrows indicate days on which blood was drawn for measurement of serum enzymes and platelets.

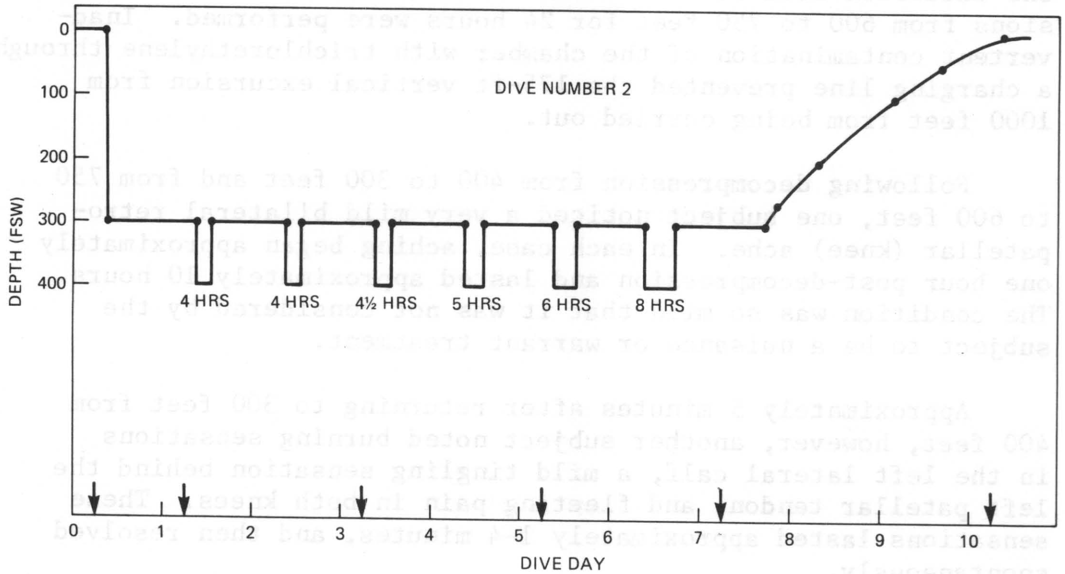


Fig. IVB-2. Profile for dive No. 2.

carried out. Thirteen hours later, three 4-hr excursions to 400 feet, separated by a 1-hr interval at 300 feet, were conducted. All of these multiple daily excursions also were free of clinical, objective, or laboratory signs of decompression sickness.

The results of dive No. 3 were interpreted to mean that any downward excursion dive in the 300-400 foot envelope could be made without regard to exposure time or frequency. Conversely, as 24 hours is generally considered an adequate time to achieve saturation with helium, a diver saturated at 400 feet with a $PO_2$ of 0.4 should be able to make a vertical excursion to 300 feet with safety. The changes in aldolase and platelets seen in dive No. 2 were not apparent in this more stressful dive, and their significance remains obscure. The results of dive No. 3 lead us to the problem of limits for 24-hr downward excursion dives or, using an alternative frame of reference, the problem of vertical excursion dives from saturation.

Dive Number 4. Dive No. 4 was undertaken: 1) to ascertain whether five unacclimatized subjects could safely make the same 24-hr no-decompression excursion to 400 feet as the subjects in dive No. 3; 2) to determine whether a 24-hr no-decompression excursion from 600 feet to 750 feet was possible; and 3) to determine whether a direct vertical excursion from 1000 feet to 825 feet was possible. The dive profile is shown in Fig. IVB-4. One excursion from 300 to 400 feet for 24 hours and two excursions from 600 to 750 feet for 24 hours were performed. Inadvertent contamination of the chamber with trichlorethylene through a charging line prevented the 175-ft vertical excursion from 1000 feet from being carried out.

Following decompression from 400 to 300 feet and from 750 to 600 feet, one subject noticed a very mild bilateral retropatellar (knee) ache. In each case, aching began approximately one hour post-decompression and lasted approximately 10 hours. The condition was so mild that it was not considered by the subject to be a nuisance or warrant treatment.

Approximately 5 minutes after returning to 300 feet from 400 feet, however, another subject noted burning sensations in the left lateral calf, a mild tingling sensation behind the left patellar tendon, and fleeting pain in both knees. These sensations lasted approximately 1-4 minutes, and then resolved spontaneously.

Approximately 15 minutes post-decompression to 600 feet in the first excursion, the same subject developed bilateral posterolateral and lateral aching knee pain. The right knee was affected to a greater degree than the left, but both were described by the subject as mild. The pain was still present

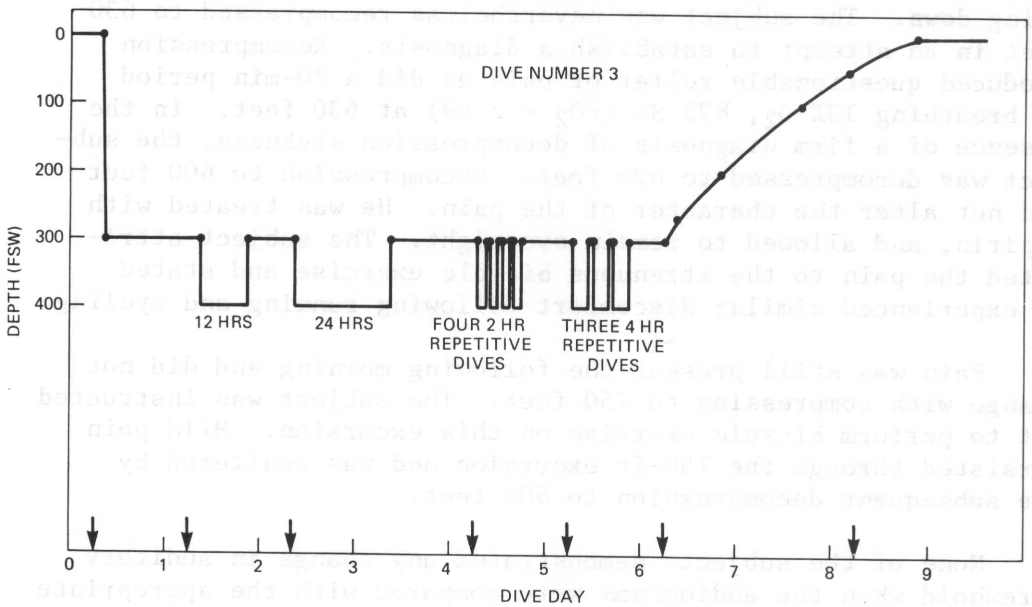


Fig. IVB-3. Profile for dive No. 3.

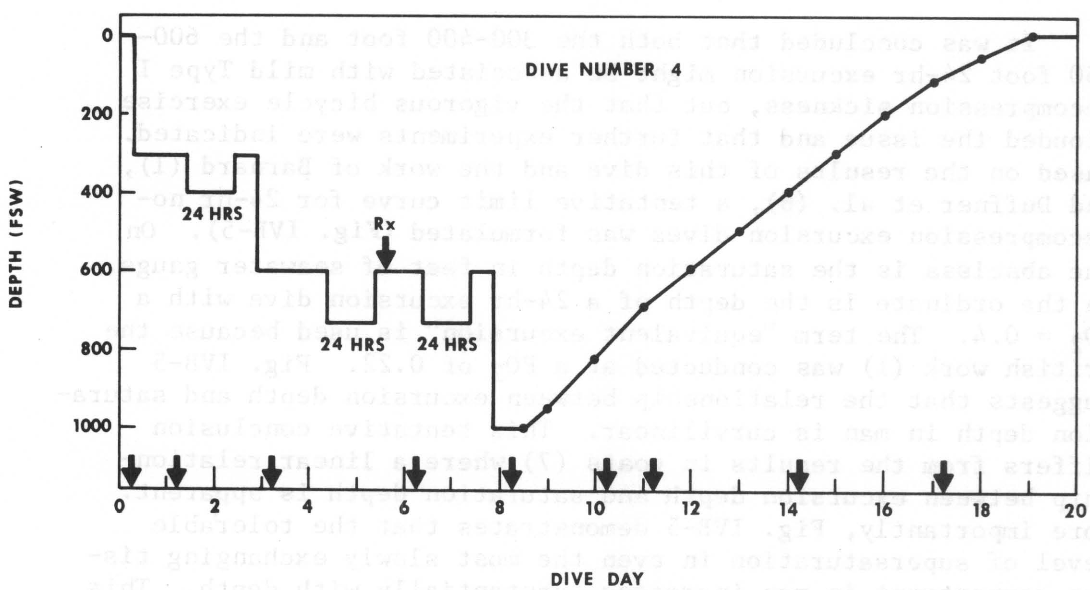


Fig. IVB-4. Profile for dive No. 4.

2 hours and 15 minutes after reaching 600 feet, although substantial relief could be obtained by extending the legs or by lying down. The subject was nevertheless recompressed to 630 feet in an attempt to establish a diagnosis. Recompression produced questionable relief of pain as did a 20-min period of breathing 13% $O_2$, 87% He ($PO_2$ = 2.69) at 630 feet. In the absence of a firm diagnosis of decompression sickness, the subject was decompressed to 600 feet. Decompression to 600 feet did not alter the character of the pain. He was treated with aspirin, and allowed to remain overnight. The subject attributed the pain to the strenuous bicycle exercise and stated he experienced similar discomfort following running and cycling.

Pain was still present the following morning and did not change with compression to 750 feet. The subject was instructed not to perform bicycle exercise on this excursion. Mild pain persisted through the 750-ft excursion and was unaltered by the subsequent decompression to 600 feet.

None of the subjects demonstrated any change in auditory threshold when the audiograms were compared with the appropriate 300- or 600-ft pre-excursion dive controls. Similarly, no spontaneous nystagmus or significant alterations in balance from control values were noted following any of the excursions. No changes in serum enzymes were apparent.

It was concluded that both the 300-400 foot and the 600-750 foot 24-hr excursion might be associated with mild Type I decompression sickness, but that the vigorous bicycle exercise clouded the issue and that further experiments were indicated. Based on the results of this dive and the work of Barnard (1), and Duffner et al. (6), a tentative limit curve for 24-hr no-decompression excursion dives was formulated (Fig. IVB-5). On the abscissa is the saturation depth in feet of seawater gauge. On the ordinate is the depth of a 24-hr excursion dive with a $PO_2$ = 0.4. The term "equivalent excursion" is used because the British work (1) was conducted at a $PO_2$ of 0.22. Fig. IVB-5 suggests that the relationship between excursion depth and saturation depth in man is curvilinear. This tentative conclusion differs from the results in goats (7) where a linear relationship between excursion depth and saturation depth is apparent. More importantly, Fig. IVB-5 demonstrates that the tolerable level of supersaturation in even the most slowly exchanging tissue compartment in man increases substantially with depth. This conclusion is supported by the work of Buehlmann et al. (5), but disagrees with the concept outlined by Workman (15).

It should not be concluded that $\Delta P$ values derived from Fig. IVB-5 can be used to control saturation decompression. The important difference between the degree of supersaturation tolerable during excursion dives and that tolerable during

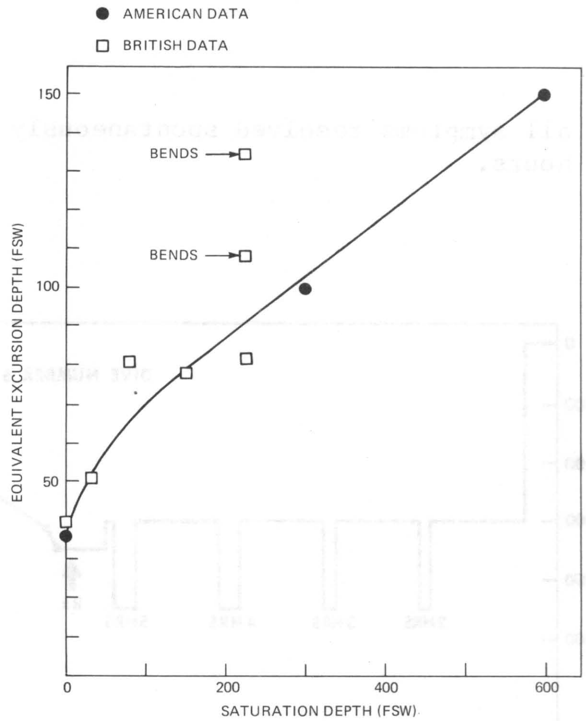


Fig. IVB-5. Relationship between saturation depth and depth to which a 24-hr no-decompression excursion can be made. Open boxes indicate data of Barnard (1) corrected for the difference in excursion $PO_2$ of 0.22 to 0.4. Solid dots are data of Duffner and Snyder (6) and present data.

saturation decompression is well recognized. During U.S. Navy saturation decompression, the calculated ΔP in a 240-min tissue at the 300-ft stop is 24.6 feet. Following a 24-hr excursion to 400 feet and return to 300 feet, the calculated ΔP in the same tissue is 87 feet. The physiological basis for this disparity is not well understood.

Dive Number 5. The impending move of the Experimental Diving Unit from Washington, D.C. to Panama City, Florida gave us the opportunity to perform only one more dive. Since the existing Navy table provided for a 60-min excursion from 300 to 450 feet, it was decided to determine whether this limit could also be significantly improved. The dive profile is shown in Fig. IVB-6. Four excursion dives were made from 300 to 450 feet with exposure times of 2, 3, 4, and 5 hours. The first excursion dive began 23 hours after arrival at the saturation depth and subsequent excursion dives were separated by a saturation interval of 21-22 hours.

Following the 2-hr excursion, all subjects were well both by clinical and objective examination. Approximately one hour following decompression from the 3-hr excursion, however, one subject noticed a diffuse superficial tightness in the anterior chest. The chest discomfort was mild and was not affected by motion or deep breathing. There was no associated dyspnea, tracheal irritation, or cough. It was decided not to recompress and all symptoms resolved spontaneously over the course of several hours.

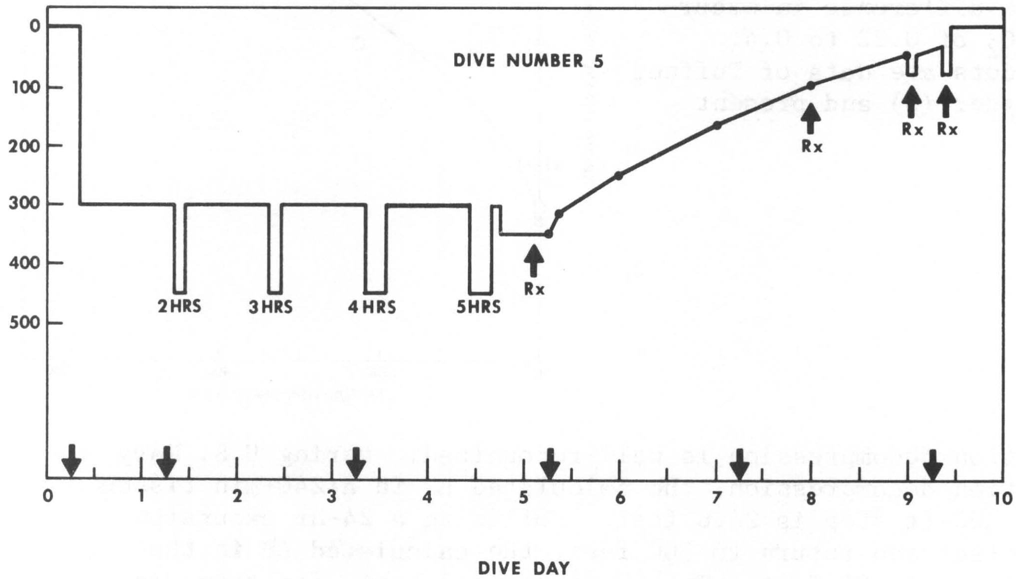


Fig. IVB-6. Profile for dive No. 5.

One hour after ascent from the 4-hr excursion, the same subject again noted tightness in the chest, this time accompanied by pruritus of the anterior chest and abdominal wall. There was no dyspnea, cough, tracheal irritation, or rash. All symptoms resolved spontaneously over the ensuing seven hours.

One hour following ascent from the 5-hr excursion, this subject again noted the onset of pruritus on the anterior chest and abdomen. One hour later this progressed to include the axillae, the medial aspect of both arms, and the lateral aspects of both thighs. The pruritic areas were tender to palpation and walking was somewhat painful. There was no visible rash, discoloration, or cutaneous swelling. The remaining four subjects were normal, and audiometric, ENG, and balance examinations on all five subjects were normal. Platelets and serum enzymes also remained normal.

Two hours and twenty minutes following decompression from 450 feet, the subject was recompressed to 350 feet. Seventy-five per cent of the discomfort was relieved by this procedure. Ninety-five per cent relief was obtained after 20 minutes of breathing a mixture of 21% oxygen, 79% helium ($PO_2$ = 2.44) by mask. A second 20-min period of breathing this gas mixture produced no further change and the subject was allowed to remain overnight at 350 feet. No symptoms were present upon arising in the morning.

Theoretically, the 240-min tissue contains the same helium tension after a 6-hr exposure at 450 ft as it does following saturation at 400 feet. On the basis of dives 3 and 4, therefore, this tissue should be able to return to 300 feet safely after a 6-hr exposure at 450 feet. The fact that difficulty was encountered following a 3-hr exposure and that these symptoms progressed in extent and severity with subsequent 4- and 5-hr exposures indicates that tissues faster than 240 minutes control the decompression. This is consistent with the experience obtained in dives originating at the surface.

Reducing the overall decompression requirements of an operation by the use of excursions was not one of the goals of the present research. It is, however, an important topic of discussion. The two critical factors governing whether saturation decompression time can be saved are the depth of the excursion and the interval of time which must be spent at the saturation depth following an excursion before beginning saturation decompression. The deeper the excursion and the shorter the interval, the greater is the saving of decompression time. The safe interval has been widely discussed, but subjected to essentially no systematic investigation. On dive No. 3 of this series an interval of nine hours was followed by symptom-free decompression from 300 feet. On dive No. 5, however, an interval of 14 hours was followed by the development of decompression sickness in one subject at 86, 38, and 29 feet during the saturation decompression. This was the same subject who developed cutaneous decompression sickness following the excursion.

Discussion

Dr. Schreiner: Tell us more about the trichloroethylene incident. Did you do any toxicological followup?

Dr. Flynn: We followed divers very extensively and were not able ever to demonstrate anything that was related to the trichlorethylene. Liver functions all remained normal. There were some enzyme changes later on in the dive.

Parenthetically, I might add, we were trying to look at what effects adding nitrogen might have on balance at 1000 feet. I calculated out the possibility of counter-diffusion. After compression from 600 feet to 1000 feet, we had a subject standing on a balance board breathing from the mask, and gave him 2 atm of nitrogen and the balance, helium-oxygen. One subject took a tail spin right off the balance board. The next subject became very dizzy and the third subject said, "This gas doesn't smell right." That's when we discovered that we had contaminated our entire breathing system with trichlorethylene. In fact, what we were doing was anesthetizing the subjects with trichlorethylene and trying to measure their balance at 1000 feet.

Dr. Smith: Did you monitor with a Doppler on these?

Dr. Flynn: We attempted to monitor with a Doppler on every one of these dives, but we had so many technical problems, both with the equipment and getting adequate signals from the subjects, that I don't want to draw any conclusions whatsoever. On the subjects that we had good signals from, I don't believe we ever heard anything.

Dr. Schreiner: I would like to ask the group whether this experience is consistent with the Haldane or any other numerology. Here we have a situation where we can secure data that are not obtained on the fly, so to speak. This is not as dynamic a system as those that were discussed earlier, and consequently, gas loading information ought to be reasonably reliable.

Dr. Bennett: At those depths you may have a gas phase without enough expansion to cause tissue damage and pain. You may well have a simple Boyle's law relationship, but in insufficient volume to cause pain.

Dr. Spaur: After excursions of 3, 4, and 5 hours, the diver complained at exactly 1 hour and 5 minutes after coming back to the base saturation depth. Whatever the nature of the complaint, the time it took to develop wasn't dose-related, as you might expect it to be.

Mr. Kenyon: I have done a quick analysis on the 240-min tissue compartment using a classical Haldane approach, trying to determine what kind of gas tension they had for that tissue as they entered the habitat, so we could compare it with another exposure that we had that was similar. These are given in Fig. IVB-7.* The dotted line represents the EDU 6-hr excursion, and return to habitat with no stop, clean.

In Access I we were coming from saturation at 500 feet, following a previously asymptomatic series of excursions, and we got hit at 285 feet. In Fig. IVB-7 you can see the difference in inert gas supersaturation which was in the final compartment, the 240-min compartment, for EDU as they came into the habitat. It peaks at about 54 feet of seawater, which

*This slide was shown at the UMS meeting in May, 1974.

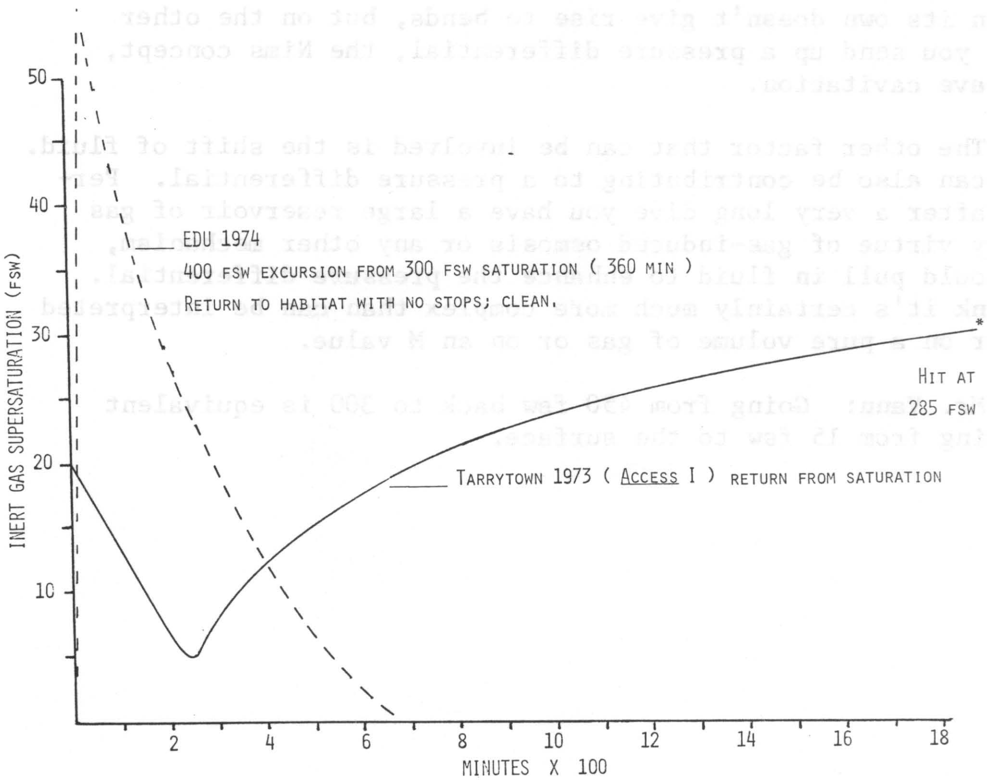


Fig. IVB-7. Gas loading analysis of 240-min tissue following 2 dives. EDU dive (dotted line) involved a clean return to 300 fsw from 6-hr excursion to 400 fsw. Solid line shows saturation decompression from 500 fsw, with a hit at 285 fsw.

is 1.6 meters of over-supersaturation; we sustained a hit with only 30 fsw of supersaturation, below our M value for that point. Our diver may have been predisposed by the excursion, but we think we have another explanation based on time. We have what appears to be a "time-dependence of M values," or something like that.

Dr. Smith: Several individuals are working on a time-dose relationship, time-amount of supersaturation. Brian D'Aoust is working on bubble formation in fish exposed to very slightly supersaturated water. Time is important; this is a good case for the time-dose phenomenon, where Tarrytown had a moderate amount of supersaturation for a long period of time and EDU has a lot for a short period of time.

Dr. Schreiner: I've just been informed that if I don't stop this conversation, the group, collectively, will have given Russ Peterson's talk.

Dr. Hills: It has been suggested that on these excursions you wouldn't form sufficient volumes of gas. The volume of gas on its own doesn't give rise to bends, but on the other hand, you send up a pressure differential, the Nims concept, you have cavitation.

The other factor that can be involved is the shift of fluid. That can also be contributing to a pressure differential. Perhaps after a very long dive you have a large reservoir of gas and by virtue of gas-induced osmosis or any other mechanism, you could pull in fluid to enhance the pressure differential. I think it's certainly much more complex than can be interpreted either on a pure volume of gas or on an M value.

Mr. Vann: Going from 450 fsw back to 300 is equivalent to going from 15 fsw to the surface.

## REFERENCES

1. Barnard, E. E. P. Fundamental studies in decompression from steady-state exposures. In: Fifth Symp. Underwater Physiol., edited by C. J. Lambertsen, (in press).
2. Bornmann, R. C. Decompression schedule development for repetitive saturation excursion helium-oxygen diving. USN Deep Submergence Systems Project, Res. Rept. 1-70.
3. Bornmann, R. C. Helium-oxygen saturation-excursion diving for U.S. Navy. In Fourth Symp. Underwater Physiol., edited by C. J. Lambertsen. New York: Academic, 1971.
4. Braithwaite, W. R., T. E. Berghage, and J. C. Crothers. Postural equilibrium and vestibular response at 49.5 ATA. Undersea Biomed. Res. 1:309-323, 1974.
5. Buehlmann, A. A., H. Matthys, G. Overrath, P. B. Bennett, D. H. Elliott, and S. P. Gray. Saturation exposures at 31 ATA in an oxygen-helium atmosphere with excursions to 36 ATA. Aerospace Med. 41:394-402, 1970.
6. Duffner, G. J. and H. H. Snyder. Effects of exposing men to compressed air and helium-oxygen mixtures for 12 hours at pressures of 2 to 2.6 atmospheres. USN Exp. Diving Unit Res. Rept. 1-59, 1958.
7. Hempleman, H. V. Investigation into the decompression tables: further basic facts on decompression sickness. Royal Naval Physiological Laboratory Rept. No. VIII, 1957.
8. Larsen, R. T., and Mazzone, W. F. Excursion diving from saturation exposures at depth. In: Third Symp. Underwater Physiol., edited by C. J. Lambertsen. Baltimore: Williams and Wilkins, 1967.
9. Schreiner, H. R., and P. L. Kelley. Computation of decompression schedules for repetitive saturation-excursion dives. Aerospace Med. 41:491-494, 1970.
10. Summitt, J. K., J. M. Herron, and E. T. Flynn. Repetitive excursion dives from saturated depths on helium-oxygen. USN Exp. Diving Unit Res. Rept. 2-70, 1970.
11. Summitt, J. K., J. M. Alexander, E. T. Flynn, and J. M. Herron. Repetitive excursion dives from saturated depths on helium-oxygen mixtures. Phase II - saturation depth 200 feet. USN Exp. Diving Unit Res. Rept. 6-70, 1970.
12. Summitt, J. K., J. M. Alexander, and E. T. Flynn. Repetitive excursion dives from saturated depths on helium-oxygen mixtures. Phase III - saturation depth 300 feet. USN Exp. Diving Unit Res. Rept. 7-70, 1970.
13. Summitt, J. K., J. M. Alexander, and E. T. Flynn. Repetitive excursion dives from saturated depths on helium-oxygen mixtures. Phase IV - saturation depth 500 feet - saturation depth 600 feet. USN Exp. Diving Unit Res. Rept. 8-70, 1970.
14. Summitt, J. K., and J. W. Kulig. Saturation dives, with excursions, for the development of a decompression schedule for use during SEALAB III. USN Exp. Diving Unit Res. Rept. 9-70, 1970.

15. Workman, R. D. Calculation of decompression schedules for nitrogen-oxygen and helium-oxygen dives. USN Exp. Diving Unit Res. Rept. 6-65, 1965.

## C. CURRENT WORK AT THE INSTITUTE FOR ENVIRONMENTAL MEDICINE: R. E. PETERSON AND K. GREENE

In the past, the Institute for Environmental Medicine (IFEM) has not directly engaged in human decompression experimentation. We have been interested users of decompression procedures, have contributed to decompression theory, particularly in the use of high inspired oxygen tensions and inert gas alternations, and have tried to assist the advance of decompression techniques through the establishment of the International Decompression Data Bank. However, we have contracted with other facilities to test the nitrogen saturation schedules devised at IFEM for the Tektite I and Tektite II operations. The 1200-fsw helium saturation schedule devised for Predictive Studies III, while untried elsewhere, was broken by several long stops for experimental purposes, so that it was also not thoroughly evaluated at IFEM as a decompression procedure. It is likely that this practice will be changed shortly.

We are now planning another collaborative helium study, this time to 1600 fsw, with the experimental protocol calling for excursions from deep saturation to as much as 400 fsw deeper to study the effects of absolute depth and compression rates on human performance. Practical experience in non-saturation diving beyond 600 fsw is extremely limited and beyond 1000 fsw is almost non-existent. The approach that what works at one depth will work at deeper depths could be taken in this case. However, the exposures that we desire would necessitate prohibitive decompression times if based on conventional methods, and there is sufficient evidence to indicate that increased depth allows for more rapid decompressions. Thus we must both develop and test our own procedures for use in the 800-1600 fsw depth range.

This need for new decompression procedures arose concomitantly with a great interest in decompression among some of the staff and graduate students at IFEM and has been incorporated into an effort to look into decompression theory in general. We believe our ideas are applicable to both saturation and non-saturation diving. Because of our current needs, however, most of our work has been directed toward non-saturation helium decompression.

As diving has developed, with exposure depths becoming deeper and exposure times becoming longer, the parameters used to calculate decompression schedules for shallower and shorter exposures have proved to be inadequate for calculating schedules for the more extreme conditions. The reason for this is of great interest to us. One explanation is that the time spent in either a supersaturated or phase-separated condition, whichever you believe pertains, is of importance. Even if decompressions from deep or long exposures are calculated to allow no greater magnitude of supersaturation than in decompressions from shallower or shorter exposures, the time spent with that degree of supersaturation is much greater. It seems reasonable that this should increase the probability of decompression sickness when some set of allowable supersaturation values is employed in a depth range or time frame greater than the one in which it was proved.

Dr. Peterson: Figure IVC-1 is a diagram of one of the dives that Ed Flynn presented, a 750-ft excursion from a 600-ft saturation depth. The solid lines show the desaturation of a 20-min and a 160-min tissue compartment with a step change in depth from 750 feet gauge to 600 feet gauge. What we are interested in is the difference between the dashed line (ambient pressure) and the solid lines (calculated compartment gas partial pressure). We are interested in the hatched area, the difference between the solid and dashed lines, summed over time.

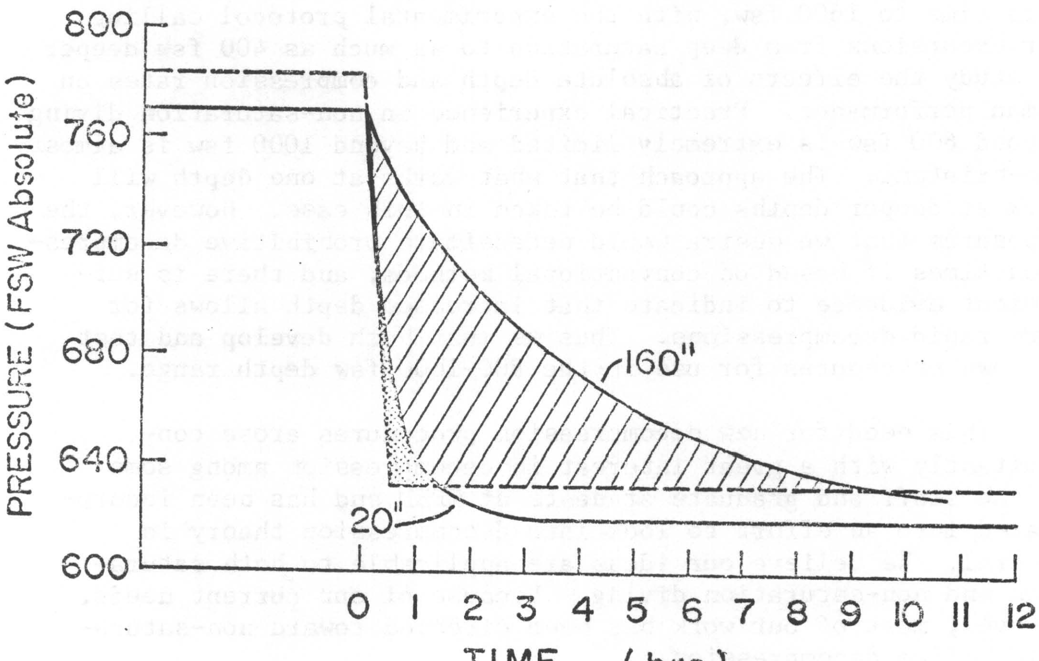


Fig. IVC-1. 750-ft excursion from 600-ft saturation depth. Desaturation of 20-min and 160-min tissue is shown by solid curves; dashed line is ambient absolute pressure.

While the approach of considering both supersaturation and time in the calculation of decompression schedules seems to have a fundamental importance, it was not until we became aware of data demonstrating the relationship between absolute depth and the depth of 24-hr, no-decompression excursions that a complete scheme fell into place. We were quite surprised when we learned from Dr. Flynn of the completely safe, 24-hr, no-decompression excursions from saturation at 300 fsw to 400 fsw and from saturation at 600 fsw to 750 fsw made at the Experimental Diving Unit. These data, together with data produced at the Royal Naval Physiological Laboratory, of a similar nature but for shallower depths and with some incidence of decompression sickness, seemed to provide an excellent means for weighting supersaturation or determining supersaturation with equivalent effects at different depths.

Figure IVC-2 is quite similar to Dr. Flynn's Fig. 5 (Fig. IVB-5 in paper IVB of this Workshop) but without the RNPL points.

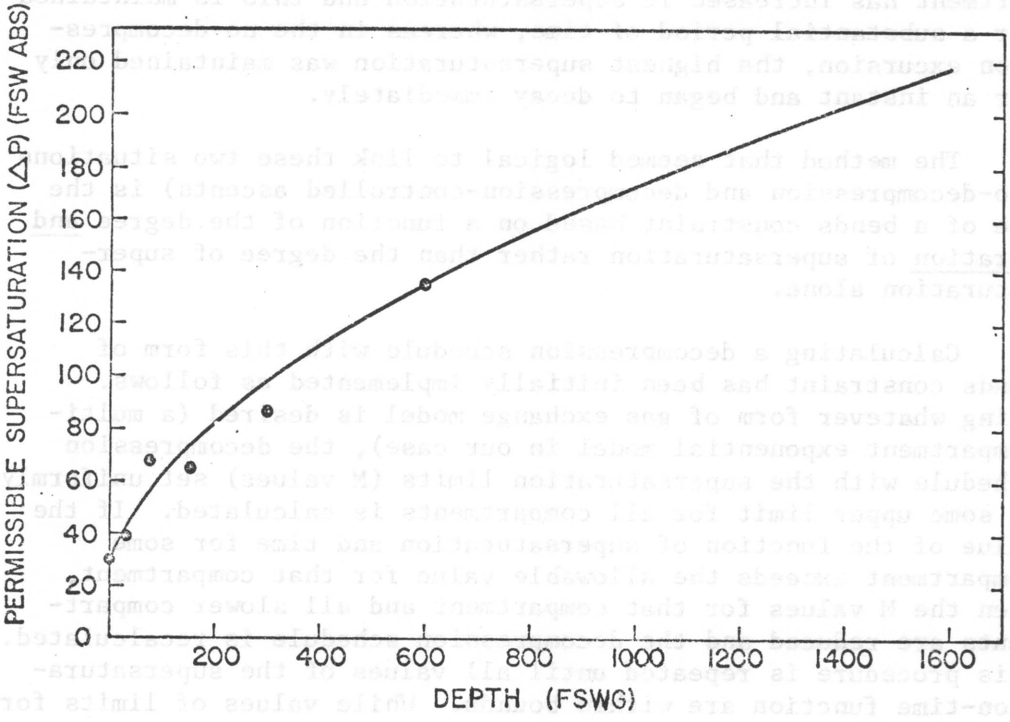


Fig. IVC-2. Permissible supersaturation as function of depth. Individual points represent data derived from single-step decompressions for 24-hr oxy-helium exposures (See paper IVB).

We fitted a power function to the points. Our curve also suggests a tolerable supersaturation of 136 fsw at 600 feet. This agrees with Kenyon's calculations (in the Discussion section of IVB) of the EDU excursions. Using a standard Workman-type decompression calculation with an M value (allowable supersaturation plus the absolute pressure) based on this information, the decompressions one gets for excursions from saturation depths of 600 fsw or greater are quite rapid. However, a supersaturation of 136 fsw is based on no-decompression ascents, and we are reluctant to apply supersaturation values from no-decompression experience to the calculation of decompression-controlled ascents.

Figure IVC-3 shows another exposure, a 400-ft excursion for an hour with a decompression-controlled ascent, similar to the Access Program at Tarrytown. We have plotted the 20-min and 160-min tissues. The small graph on the upper right corner shows the growth of certain functions of time and supersaturation--the hatched areas. The particular function that is plotted is just the absolute supersaturation integrated over time. This is a much different situation from the no-decompression change, however, because with the decompression the slower compartment has increased in supersaturation and this is maintained for a substantial period of time, whereas in the no-decompression excursion, the highest supersaturation was maintained only for an instant and began to decay immediately.

The method that seemed logical to link these two situations (no-decompression and decompression-controlled ascents) is the use of a bends constraint based on a function of the degree <u>and duration</u> of supersaturation rather than the degree of supersaturation alone.

Calculating a decompression schedule with this form of bends constraint has been initially implemented as follows. Using whatever form of gas exchange model is desired (a multicompartment exponential model in our case), the decompression schedule with the supersaturation limits (M values) set uniformly at some upper limit for all compartments is calculated. If the value of the function of supersaturation and time for some compartment exceeds the allowable value for that compartment, then the M values for that compartment and all slower compartments are reduced and the decompression schedule is recalculated. This procedure is repeated until all values of the supersaturation-time function are within bounds. While values of limits for both supersaturation and supersaturation-time functions must be extracted from extensive analyses of exposures previously performed, we have already tried to calculate schedules with this method, using arbitrary limits. The results appear to be quite reasonable. As greater and greater decompression debts are produced by an exposure, the M values are reduced more and more, with the M values for the slower compartments being reduced to

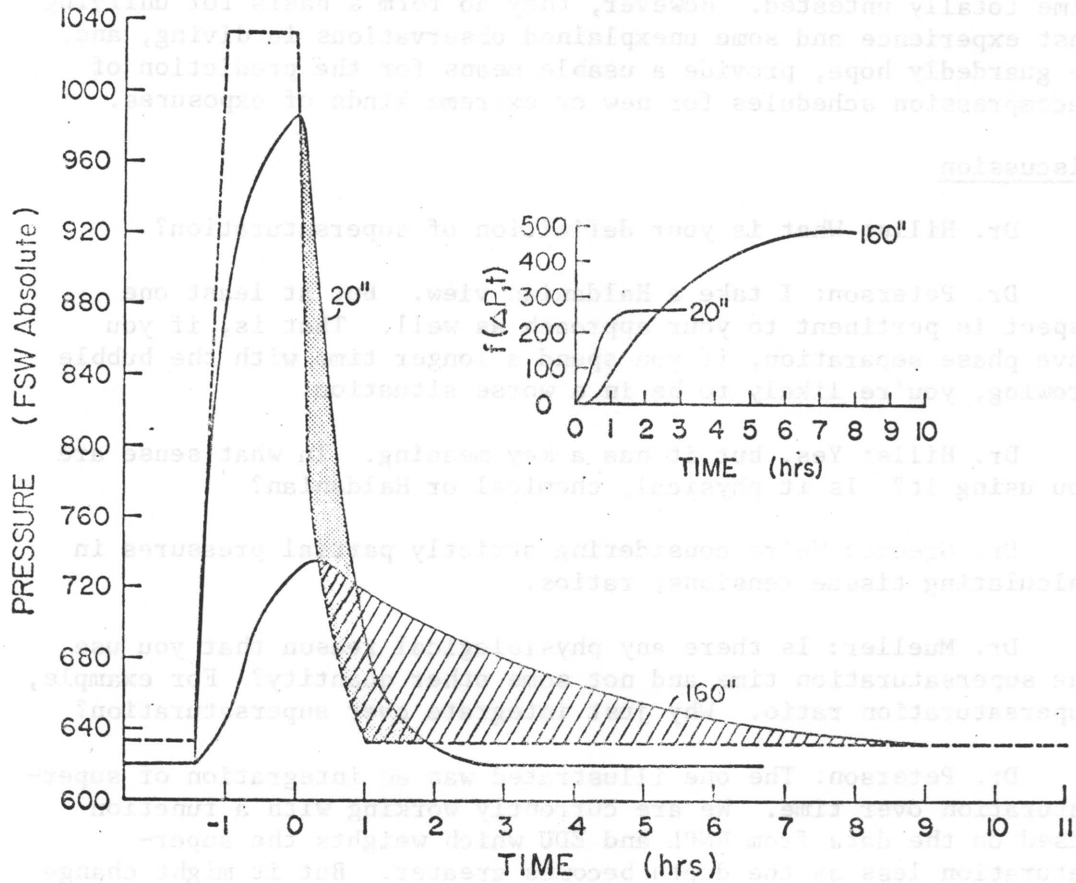


Fig. IVC-3. 400-ft excursion for 1 hr with a decompression-controlled ascent, for 20-min and 160-min tissue compartments. Growth of hatched areas with time is plotted in small graph.

the greatest extent. The effect of this progressive M value reduction is to considerably prolong decompression times for more extreme exposures. This M value reduction and geometric increase in decompression times parallel what has happened empirically in diving, that is, the extension of the longest half times and the reduction in allowable supersaturation to produce longer and safer decompression schedules as the exposures have become more extreme.

Another interesting aspect of this scheme is its prediction of the impact of oxygen on decompression. With higher inspired oxygen tensions, decompression from any exposure and the final resolution of any supersaturation would be faster. Thus if the allowed supersaturations were equivalent, a decompression with high oxygen should be safer than a decompression with lower oxygen because the time spent in a supersaturated state is less.

Without doubt, these concepts are speculative and at this time totally untested. However, they do form a basis for unifying past experience and some unexplained observations in diving, and, we guardedly hope, provide a usable means for the prediction of decompression schedules for new or extreme kinds of exposures.

Discussion

Dr. Hills: What is your definition of supersaturation?

Dr. Peterson: I take a Haldanian view. But at least one aspect is pertinent to your approach as well. That is, if you have phase separation, if you spend a longer time with the bubble growing, you're likely to be in a worse situation.

Dr. Hills: Yes, but it has a key meaning. In what sense are you using it? Is it physical, chemical or Haldanian?

Dr. Greene: We're considering strictly partial pressures in calculating tissue tensions; ratios.

Dr. Mueller: Is there any physiological reason that you use the supersaturation time and not some other quantity? For example, supersaturation ratio. Why just integrate over supersaturation?

Dr. Peterson: The one illustrated was an integration of supersaturation over time. We are currently working with a function based on the data from RNPL and EDU which weights the supersaturation less as the depth becomes greater. But it might change tomorrow, as we learn more.

# SESSION V: OPERATIONAL EXPERIENCE

# V. OPERATIONAL EXPERIENCE

## A. OPERATIONAL RESULTS OF TARRYTOWN LAB'S WORK: D. J. KENYON

Deep diving in recent years has seen an unparalleled growth. The need for working dives to 650 feet is becoming a standard requirement. Where once long-line diving was commonly used, the work is now performed with a team of from two to four divers working from an advanced diving system. New decompression procedures had to be developed to replace existing shallow-water surface decompression diving, and to extrapolate to deeper depths which were not covered by existing tables. Before we can set out to develop a set of decompression procedures we must consider four limitations that bind us to a practical solution.

The first limitation is that the breathing gas supplied to the diver must ideally contain a partial pressure of oxygen of from 0.5 to less than 2.0 atm. Next, the nitrogen partial pressure in the diver's breathing gas must be ideally in the range of from 0.8 to 4.5 atm. Three, we must eliminate all excess inert gas which has been absorbed into the body in such a way as not to cause injury. Four, during the process of decompression we must adequately protect the diver from extreme exposure to the environment.

These limitations are interacting; they must be accounted for when developing new decompression procedures. The first limitation, oxygen, is the most obvious, but it is generally the most difficult to manage. The upper limit of 2.0 atm is due to possible central nervous system oxygen poisoning. The upper limit is also important to prevent pulmonary oxygen toxicity over a long decompression period. The use of the Unit Pulmonary Toxicity Dose (UPTD) as developed at the University of Pennsylvania can help estimate whether a given $PO_2$ exposure will cause pulmonary problems, but there are difficulties in applying it. Although 2.0 atm was mentioned as an upper limit, an optimal level is between 0.5 and 1.5 atm $PO_2$.

Because the oxygen is quickly metabolized, we assume that the partial pressure of oxygen in the breathing mixture is not added to the inert gases in accounting for decompression calculations. Therefore, the greater the oxygen content in the breathing gas the less the inert gas that will be absorbed by the body while at bottom. It also follows that during decompression the greater the oxygen content in the breathing mixture the greater the differential between body and ambient inert gas levels.

Though simplistic in its reasoning, this has been one of the tricks most widely used by the commercial diving companies to reduce decompression sickness with resultant shorter decompression times. Ocean System's successful Mark VI decompression tables have employed the technique in procedures with the use of up to 1.5 atm

$PO_2$ during bottom work, an air flush at 150 fsw, and oxygen breathing at 40 fsw. The only time the $PO_2$ drops below 0.5 atm is during the decompression from 50 to 40 fsw, a very brief exposure.

The rigid adherence to a minimum $PO_2$ of 0.5 atm is therefore not totally arbitrary and is further substantiated by others. Dr. Hempleman and Dr. Buehlmann have also found that the higher levels of $PO_2$ during decompression are necessary and compatible with safe diving. When our group has attempted to eliminate oxygen breathing from any deep bounce dive in the hopes of providing greater comfort to the diver, we have had great difficulty, leading us to suspect that oxygen acts not only as a mechanical aid in decompression but as a prophylactic agent against tissue hypoxia.

When diving deeper than 400 fsw it is necessary to shift to an intermediate gas at some point between bottom and the point at which there is an air changeover. Ocean System's Mark VIII decompression tables were designed with a 100-fsw air shift depth and an intermediate gas shift at 300 fsw. These tables specify a mandatory change to 16% ($PO_2$ = 1.6) at 300 fsw for all decompressions from bottom depths of from 400 to 650 fsw. Experience has shown that this requirement is difficult to manage operationally for short bottom-time tables and for decompressions that are shallower than about 500 fsw.

We corrected the problem by delaying the divers' shift to intermediate gas until 45 mintues had elapsed or 300 feet, whichever came first. This provided sufficient time for the bell to be surfaced and mated to a deck decompression chamber. The transfer from the bell to the entry lock of the deck decompression chamber provides the ideal situation for a change of gas mixture and thereby eliminates the need to add oxygen or to use mask breathing while in the bell.

The second limitation ($PN_2$ between 0.8 atm and 4.5 atm) sets the groundwork for the nature of the decompression and therefore seriously affects the third limitation (inert gas elimination). The nitrogen limit of 4.5 atm (equivalent to air at approximately 160 fsw) is arbitrary--and many people will set other limits--and is due to narcosis. The increased nitrogen partial pressures have been found by the divers to be stabilizing agents against tremors when compressing rapidly (100 ft/min) to the bottom. This has been tried by Bennett in 1,000-ft trials and also at our lab in the Access series. Nitrogen also improves voice and thermal comfort. The lower limit of 0.8 atm is that part of the initial air that is present in the bell when pressurizing begins. Any decompression table must account for this initial air in the bell. The lockout diver does not receive this additional amount of nitrogen if a pre-mixed lockout gas is utilized.

The results of the initial Mark VIII decompression tables were not good. The Mark VIII was not verified in the laboratory--it was originally designed as a contingency for Ocean Systems, in case they were to go into water too deep for the Mark VI's. No instructions were furnished with the Mark VIII and no clear-cut guidelines for treatment of decompression sickness occurring at depth were available to the operational people. As it turned out, Ocean Systems did get into deep water.

Initially, the Mark VIII tables worked reasonably well, with a sporadic bends incidence of perhaps 3 to 6%, and we were pleased. In November 1973 we were called in to solve a problem of something of the order of 50% bends incidence, close to one case in every other dive. These hits were at depths of 40 to 50 fsw. They were diving in very, very cold water. They were using a cramped, unheated chamber, and I suspect the temperatures in the chamber during the helium shifts were close to 50°F. Intolerable, yes, but this was the situation.

We printed up another set of decompression tables, now known as the Mark VIII Revision A, complete with hastily done operational procedures, gas requirements, and treatment tables. I was aware of the problems of the Mark VIII in not adequately accounting for the increased nitrogen partial pressure present in the bell and in the intermediate mix in the entry lock. I had used for the Mark VIII calculations a bottom gas concentration of 5% $O_2$, 5% $N_2$, and the balance helium. I had used a calculated mixture of 16% $O_2$, 5% $N_2$ and the balance helium for the intermediate gas mixture. Because there were no instructions for the Mark VIII tables, pre-mixed gas had been obtained based upon the old Mark VI instructions, which allowed around 10% nitrogen in the mixtures.

It looked to me as though the unacceptable dives with these mixtures may have resulted from excessively high partial pressures of nitrogen. The Mark VIII Revision A tables were calculated using a new matrix which included the experience gained during the NOAA-sponsored advanced nitrogen saturation studies. The Mark VIII Revision A worked, and worked well. In spite of small chambers, lack of $CO_2$ scrubbing and extreme cold, there were no decompression problems. Except for one minor symptom treated with a short $O_2$ table, up to the time of our departure from Union Carbide we had not received any indications of problems. However, I would like to see what the Mark VIII A would do with the stringent Duke criterion of heavy work.

The fourth and final limitation is the protection of the diver from extreme exposure to the environment. This has been fairly well eliminated by the use of hot water suits, and the lockout submersible decompression chamber, and its associated entry lock/main lock decompression chamber.

Diving with advanced diving systems calls for the use of premixed gases for blow down of the bell and the entry lock as well as the breathing gas mixture. On one of my many trips at sea I had the opportunity to discuss the problem of low gas supplies with the divers. One of them mentioned that they occasionally pressurize the bell with air for 20 to 30 fsw to conserve on mixed gas. I was quick to comment that the decompression regimes that they were utilizing were not developed for such large partial pressures of nitrogen and that they might be compromising final decompression.

But in thinking about this problem, I came up with a new idea, a scheme which I called a "hybrid" decompression. By utilizing the bell as a mixing chamber during compression, air at, say, 150 fsw can provide the optimal $PO_2$ and $PN_2$ for any dive from 150 fsw to virtually any depth. By pressurizing the bell to 150 fsw on air and to bottom depth on pure helium the appropriate mixture is provided to the diver. Although a pre-mixed gas must still be supplied to the diver, this mixture is easily prepared by mixing air with helium on a percentage basis. Using this method, the percentage of air--of oxygen and nitrogen--drops as depth increases, but the partial pressure stays where you want it.* (We live in a percent world with a partial pressure need.) This approach definitely reduces the problem of getting mixtures to the diver for a variety of situations.

Tarrytown Labs developed for the Norwegian company Three-X a set of "hybrid" decompression tables spanning 150 to 650 fsw with normal bottom times of from 20 to 60 minutes and contingency profiles of 90 and 120 minutes. The advantages to such a system are as follows: First, gas costs are far cheaper and gas logistics are much simpler. Our analysis shows that there should be a saving of about one-third on cost of compressing the bell and furnishing this pre-mix to the lockout diver, for typical North Sea depths of 300 - 500 feet, and assuming no cost for air. At greater depths the air fraction becomes smaller and reduces the saving somewhat. However, when the depth and duration of the dive require transfer to a larger chamber at some intermediate depth, the savings pile up. This is particularly true with the very large chamber systems being used today. Second, decompression is more exact since the $PO_2$ and $PN_2$ are always the same. Also, if for some reason an abort is necessary, at 150 fsw or less, the diver can be immediately brought back to the surface without any decompression obligations or loss of expensive helium mixes. It's a complete "no-decompression" abort; they do not blow down immediately to depth. They hold at 150 and we ask the divers if they are okay. If there are any difficulties they can bring them right back on a no-D air schedule. They don't have to add any oxygen or anything like that, they just come right back. As an example of the problem this avoids, I know of a case where, when they attempted to commit the divers to saturation, they blew the

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* Experience in the laboratory tests and early field use of this procedure resulted in a change to 120 feet for the initial compression with air.

chambers down to about 160 feet or so on pure helium as is standard practice for a saturation dive (with the one atmosphere of air). A problem came up, so they immediately returned. The chambers were large and decompressed slowly, and by the time they reached the surface the divers were flaked out. The mixture appropriate for saturation was not compatible with life at the surface. We laugh about this incident, but it could have been serious.

If an abort is required deeper than 150 feet, we can use the appropriate decompression table at the depth of abort since all tables use the same compression procedure. Additional advantages of the hybrid approach are that the diver is warmer in the bell and in the transfer lock, and speech is more intelligible. We were highly impressed by the quality of speech at 600 fsw. We could even understand Norwegians who didn't speak English too well.

The increased nitrogen content in the pressurization mix has been shown to appreciably reduce tremors experienced during compression. This has been shown by Bennett, and again in the Access series. During decompression, the switch to air is safer. We believe that the air shift problem that causes dysfunction right after the switch is related to the rapid change between a very high helium mixture and a very low helium mixture. The hybrid approach involves such a high content of nitrogen that when the switch occurs you avoid the abrupt change.

There are, however, two special considerations with the hybrid approach. The first is the need to use an additional umbilical line for the diver lock-out gas. It is not safe--ever--to use pure helium and breathing gas in the same hose. It must be impossible in normal operation to put helium into a breathing line.

The second consideration is an overall increase in decompression time. A 600-fsw dive for 30 minutes requires 25 hours of decompression. Initial tests of the hybrid decompression tables to 600 fsw for 30 minutes resulted in no decompression problems. Further studies of a more quantitative nature are scheduled sometime early this year in our laboratory at Tarrytown.

Discussion

Dr. Schreiner: Is there any comment from the industry on the practice of taking a bell to 150 feet of air and topping it off with pure helium?

Dr. Buehlmann: It's a very old technique--go down in the chamber with air to 50 meters and then with pure helium to the bottom. That's cheaper and nice for the divers, but if you dive deeper than 150 meters (500-600 feet) the profit is not so big. According to our experience, up to 150 it is comfortable--comfortable for the money, comfortable for the diver, comfortable for the decompression and security of decompression. But deeper than 500 feet, it is not so good. The difference in decompression time increases. I have an example in the second edition of the book of Bennett and Elliott. You have a longer decompression time than with oxygen-helium mixes.

Dr. Bennett: In the Duke program we do the same thing. We actually are working with 10% nitrogen, not so much nitrogen as Dave has suggested, but we find it works very well. As Dr. Buehlmann says, it's not very new. The Russian Zaltsman quoted that kind of technique in 1961; they have been using that for quite a while and find it very efficient and are able to work effectively at 525 feet with those mixtures.

Mr. Wilson: There is one danger that everybody should be aware of, gas stratification inside the chamber. We ran into this about two years ago, so now we have divers use fans to mix the gas. On one we found that $PO_2$ was like, zero!

Mr. Kenyon: This is a problem. However, we have used an MSA "air mover." We just put the helium through an air mover, it acts by a Venturi to suck the air through and mixes it adequately with the incoming gas.

Dr. Hempleman: To illustrate how layering can take place, one easy technique if one wants to replace the air in the chamber completely with helium, is to use a balloon filled with helium. It of course goes to the top in an air environment, and if you slowly let the helium in at the top, you can see the interface between the helium and the air slowly sinking to the bottom and the balloon goes down with it. The air will go clean out the bottom of the pot with practically no loss at all.

Dr. Buehlmann: Two remarks. First, we have no problems with mixing, but we use two inlets. As to 50 meters, that's not a mystic number, we can make it 40 or 30 meters.

Dr. Hamilton: We have some reservations about blowing down to 150 feet on air. Is there some feeling we should use less air, say 120 feet?

Mr. Wilson: Yes, 120 feet.

Mr. Galerne: Going to 150 feet, you are going too deep on air.

Mr. Hughes: I think generally it's been found that the reaction of the partial pressure of 150 feet of air at greater total depth is more extreme than what would be produced by 150 feet of air alone. Dr. Bennett has information on that.

Dr. Bennett: I think certainly 150 feet is a little much. You're going to get people who are going to be quite narcotic with that amount of partial pressure. I like the idea of putting in air, but I would suggest a somewhat lower partial pressure.

Mr. Kenyon: We are considering a lower partial pressure, and this shows the need for a laboratory study here.*

Dr. Greene: Have you tried any of these mixtures on breathing apparatus?

Mr. Kenyon: No, and this is what we hope to do in the laboratory. I believe we're going to have to come down if this is a problem.

Dr. Schreiner: You said that at the time of your departure from Union Carbide you had no reports on field problems with the Mark VIII Revision A. Do you have any reports on how often they were used, how many man-dives?

Mr. Kenyon: As of March 1974 there were 29 man-exposures done with the Mark VIII A, in one of which there was a slight knee pain treated by Table 5; so that was one out of 29 man-exposures, 3.5%. A summary chart is given in Dr. Hamilton's paper (IIIE of this workshop).

Dr. Bennett: I picked up a comment a few weeks ago, that these particular tables had done quite well until they went to the North Sea, but they have not done so well in the North Sea. The incidence of bends is considerably higher. Whether this was because of temperature problems or what, I was unable to determine.

Mr. Kenyon: Well, you might know a lot more than I do about this. I apologize to this body that I cannot submit the Mark VIII tables for public review. It has been the strict policy of OSI management not to divulge M values and tables. I am no longer working for OSI, and the responsibility for the technology has been transferred to Dr. Lambertsen.

I would like to point out my own openness. I think an experience like this deserves some study and I want to be as truthful as possible on the subject of the notorious Mark VIII decompression tables. How did Ocean Systems get into the position of having to use untested decompression tables, with no instructions for their use and no contingency for therapeutic treatment? Was there pressure on Ocean Systems by the oil company to do a job that was promised? Was I completely naive and egocentric in thinking that we know all

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* The laboratory study resulted in a decision to compress to 120 feet on air.

there is to know about diving and there would be no problems?

Or was this indeed the standard at the time, to release new decompression technology? I think a little of every one of these factors played a part in this story. I don't necessarily think that Ocean Systems is alone in this dilemma, but instead of burdening myself with the crazy problem of what to do I would like to add my input to the theme of this workshop. How do we determine when a diving procedure is ready for use at sea, and who makes this judgment?

## B. THE LUDION PROCEDURE FOR OPERATIONAL SATURATION DIVING: X. R. FRUCTUS AND PETER WIDE

In 1967, COMEX started saturation diving in a manner oriented towards the operational. In effect, we developed the Ludion procedure which, in scale with the depth (and pressure), maintained the living level of the oceanauts above their working level. This manner of operating, applicable to 300 meters and more, presented such advantages from the point of view of safety that it was immediately implemented on our various underwater worksites each time it was necessary to keep men in saturation for jobs of long duration.

It was in the spring of 1967 that we undertook LUDION I, 45/100 meters, and in the autumn, LUDION II, 85/120 meters, with Ocean Systems' physiologists, Bill Hamilton and Heinz Schreiner. The latter calculated the intermediate and final decompression tables, since we still lacked quite enough confidence to use our own calculations. Very rapidly, however, we acquired the confidence to develop our own procedures of ascent with a very appreciable improvement in time and in comfort. The term LUDION refers to a Cartesian diver.

In 1968 we completed operation JANUS I to a depth of 150 meters in the ocean, using the drill ship Astragale in the Mediterranean. The first team of two men lived five days at a pressure of 90 meters and using the COMEX LUDION procedure executed 10 dives on the wellhead that had been placed at 150 meters. The following team lived six days at bottom pressure and accomplished its work in twelve dives. This team was decompressed following a table of the French Navy. Learning from this first experience, we prepared operation JANUS II, co-financed by ELF and CNEXO.

Starting in February, 1970, six oceanauts began training at the hyperbaric laboratory of COMEX, specifically in the large CNEXO sphere, the famous hydrosphere 5 meters in diameter which permits simulated bell dives. Living depth was fixed at 200 meters. Medical observation, ergonometry, and psychometry showed that man's adaptation to these depths could be almost perfect.

This long preparation permitted us to minimize the uncertainties of the ocean operation, accomplished from the Astragale in the bay of Ajaccio. A wellhead was placed at 253 meters. The hyperbaric chambers were pressurized to 200 meters. The dives were carried out with remarkable precision, and in one week 34 hours of work were accomplished by only two men. During the next-to-last dive, the two oceanauts were able to work 3 hours and 10 minutes in the water without returning to the diving bell. The decompression was accomplished in 96 hours, as planned. Figure VB-1 shows the JANUS series.

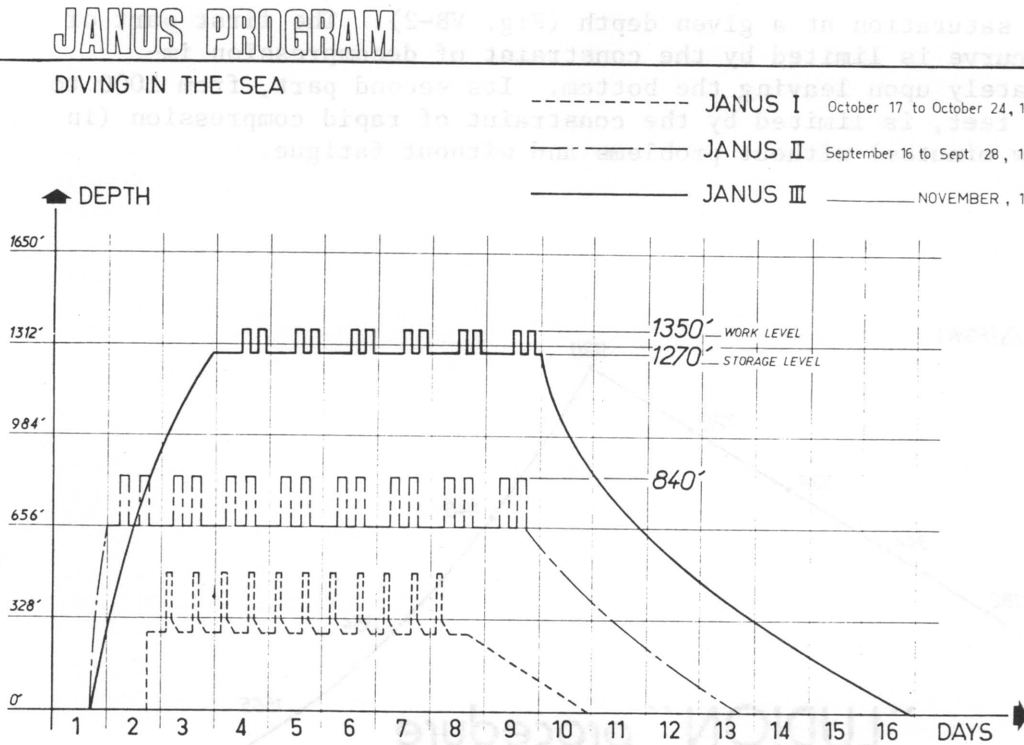


Fig. VB-1. Schedule of three JANUS operations, with their respective LUDION procedures.

Operation JANUS III represented the extreme possibilities of the LUDION procedure. We had to adopt a more moderate delta depth for its application on our worksites afterwards. A new experimental attempt served to show the risk that existed in passing certain limits. JANUS III consisted of diving on a petroleum wellhead at 180 meters depth. The bell could descent that far, but the utilization pressure of the deck chamber could not exceed 12 bars; therefore we had to undertake a LUDION 180 - 120 meters. This excursion dive implied a decompression table that we calculated in the most conservative fashion. Despite this, of the eight divers who trained for this operation in the COMEX hydrosphere, three incurred vestibular accidents (fortunately cured by an immediate recompression and drugs).

The difference between 120 and 180 meters proved too great, and our technicians resigned themselves to finding deck chambers pressurizable to 14 bars so that the excursion dives would only be 40 meters. As such, they were safe, on condition that their duration did not exceed two hours, with at least an 8-hr surface interval. However, to be operational, we have come to adopt delta depths, permitting excursion dives without time limitation, starting from saturation at a given depth (Fig. VB-2). The first part of the curve is limited by the constraint of decompression immediately upon leaving the bottom. Its second part, from 1000 to 2000 feet, is limited by the constraint of rapid compression (in a few minutes) without problems and without fatigue.

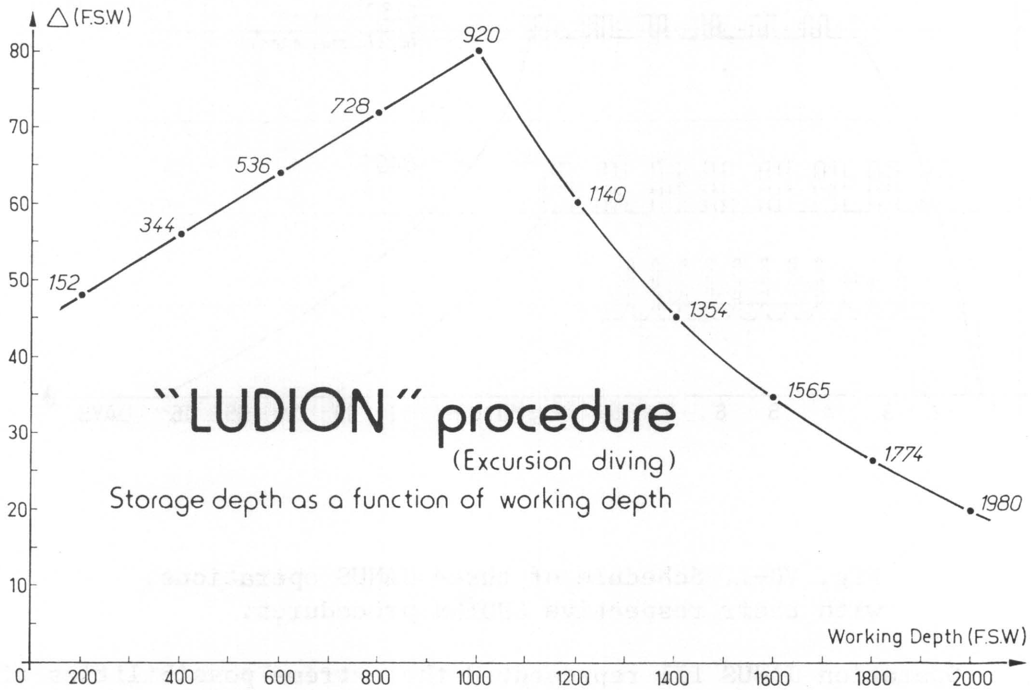


Fig. VB-2. Storage depth as a function of working depth.

We took into account at the time of the experimental dives in preparation for JANUS III that coming and going between 395 and 415 meters promoted signs of HPNS in the three subjects, who tired quickly. So we decided to keep the oceanauts at 415 meters, which greatly improved their condition and their performance. Afterwards, they could support pressure differences of 40 feet very well. Thus, at great depths, the LUDION procedure encountered new limits. It must not be abandoned because of all this, since, thanks to LUDION, the saturated diver can move in three dimensions without risk or decompression accident.

However, the decompression, if it is well controlled, proves less limiting than the compression. Let's take two relatively extreme examples:

1) The diver must work on a 66-ft-high structure at a depth of 200 feet. The bell will be lowered to the storage depth of

152 feet (LUDION 152/200), and is therefore at 48 feet above the bottom. Within this delta depth of 48 feet the diver will be able to work without time limitation. If it is necessary to work at the top of the structure, 18 feet higher, one can lower the bell to this level, decompressing the saturated divers through 18 feet (200 - 66 = 134 feet and 152 - 134 = 18 feet). This may only be done if they are working at the top of the structure at the beginning and not at the end of the excursion dive. One can then continue the work at various depths down to the 200-ft bottom and return to the storage depth of 152 feet upon re-entering and ascending in the bell.

2) The diver must work on a structure of the same height (66 feet) but at a depth of 1600 feet. The bell will be lowered to the storage depth, which in this case is 1565 feet, 35 feet above the bottom; and the diver must remain within this delta depth of 35 feet (without time limitation). Any work at the top of the structure will have to be done in the course of another dive. The bell will be lowered to 1534 feet. From there the diver cannot be compressed as far as the bottom (that is, through 66 feet) without dizziness or fatigue. He must stay within the zone from 1534 to 1565 feet, decompression from 1565 to 1534 feet presenting no problems.

One can see that the LUDION procedure gives a certain flexibility, permitting adaptation to each underwater working situation on structures of large vertical dimensions. The hundreds of saturation dives that we have completed in 1974 have been accomplished with the LUDION procedure.

Mr. Wide: Discussions at this Workshop show that nobody has completely agreed on the decompression populations used by others and that a vast research program is still ahead of us. We will give you a short resume of our ideas.

To base the criteria on a decompression curve on bends incidence is not sufficient. The infra-clinical signs are important. Disturbances in blood-circulation in the skin could be detected by thermo-vision (infrared scanning). Bubbles in the circulatory systems could be detected by Doppler or ultra sound. Studies on blood and urine are imperative to judge the presence of stress factors. They should be done when the diver returns to surface, after 24 hours, and 3 to 6 days after the dive. A drop of blood platelets more than 15 to 20% of normal and then their return to normal values is significant. The increase of fibrinogen is more or less significant but the presence of fibrinogen degradation products in the urine could cause some worry. Finally, the blood enzymes could reveal LDH (lactic dehydrogenase), but especially CPK (creatine phosphokinase) which rises well above normal levels in muscular bends. This is not well understood.

We have seen many decompression schedules in the 150-meter region and with 30-min exposure times. But what makes us sure that the shortest ones are the best? Even if the bends incidence is low....who tells us that a diver who surfaces after an 8 to 12 hours decompression could dive again, again and again and again, and could continuously do so for periods of, say, 2 to 3 weeks (which is the actual operational requirement)? One is concerned about UPTD, but nobody has spoken about the diver's pulmonary condition, his diffusion functions or his alveolar perfusion after these really long breathing periods under hyperoxia.

A few words about COMEX experience on this subject: we have 150 meters - 30 min tables using a 8/92 heliox mixture on the bottom. We use air from 54 meters and oxygen two-thirds of the time from 12 meters to the surface. We take 11 hours. We have no joint bends, but we have had two cases of vestibular problems arriving at the first stop.

In 1966 we had the same problems for the 180-meter tables after a 30-min exposure with a 6/94 heliox mixture on the bottom. Dr. Fructus personally tried the tables with 12 hours decompression and with very much oxygen. He and the other divers surfaced without bends but with aching muscles. At that time he did not make any biological examinations. They would probably have been surprised by the CPK content in their blood. Anyhow, the vestibular accidents were considered too grave for the divers. Consequently, the first phase of the decompression has been prolonged, as well as the table as a whole.

To avoid hyperoxia during long periods, we used saturation decompressions also for subsaturation dives, with a $PO_2$ of 0.65 ATA--without BIBS--all the way to the surface. This is comfortable for the divers. We have no bends but require 26 hours decompression for a 1-hr bottom time. Under the influence of the new theories of diffusion we are recalculating the tables. But for 20 hours of decompression time we had two knee bends on six divers and also a 30% drop of platelets on two divers with no clinical symptoms.

In saturation decompression we have achieved good results. The tables are relatively rapid in the beginning but have been modified a little after a vestibular bend from a 610-meter dive. Decompression times are 200 meters in two days, 300 meters in six days, and 500 meters in eight days.

We keep $PO_2$ between 0.5 and 0.6 ATA during the whole decompression, again without BIBS. We have slight joint bends in one case out of four, for dives deeper than 300 meters. We still have some work to do, as muscular bends exist after very deep dives (400 to 610 meters). The two divers from SAGITTAIRE IV (they spent 50 hours at 2000 feet), showed during decompression a considerable rise of CPK in the blood.

Our work sites between 60 and 260 meters have no decompression problems, and the divers recover very quickly even after saturations of up to one month. In the zone of 300 to 400 meters we are modifying the tables due to biological studies we have carried out. For the very deep dives we are not for the moment calculating operational tables.

We accept the value of mathematical decompression models but as long as the physiological and biological criteria are not precisely defined we are careful. Two examples:

1) What is the meaning of M values? Basically, they represent the supersaturation limit from which bubbles can be identified to occur. But if the bubbles are identified by other than just bends, the M values may cover: (a) their formation, more or less favored by different factors in the organic liquids (protein molecules, circulating lipids, surface tension of plasma, etc.); (b) if they are formed without being detected, the capacity of the lungs to eliminate them, as shown in the works of Spencer from Seattle and de Guillerm from the French Navy in Toulon; (c) if the bubbles circulate having passed the lungs, they will cause blood, vascular and tissue reactions.

2) The second example is what do the slow tissues signify? This purely theoretical definition covers perhaps two other things. Storage in tissues with bad perfusion, and a lung already blocked by bubbles from tissues with medium half times (40 - 120 minutes). This is the most dangerous case, in our opinion.

In other words, there are changes between the monophasic (dissolved gas) decompression state and the biphasic state (free gas in the capillaries). The M values and tissue half times are different. The problem is that we never know exactly when the decompression is monophasic or biphasic. To us, it looks like the monophasic decompression is perfect, but rare.

Our decompression studies on man and on animals are done more and more to study several precise objectives:

1) Bubble detection in the blood circuit, which is done with Doppler and focalized ultrasound.

2) Pulmonary function: a measure of CO conductance to evaluate diffusion or a measure of the alveolo-capillary perfusion using the $N_2O$ method.

3) Blood reactions or bubble stress: platelets, fibrinogen, enzymes.

4) Circulatory disturbances (skin bends): check with AGA thermo-vision, cartographing within 1/10 of $C^o$.

5) Biological reactions after dives and between dives.

6) Measurement of oxygen toxicity.

7) Studies of tissue reactions, lungs and muscles (this is done on dogs).

8) Smooth muscle activating factor (SMAF), such as or including bradykinin, serotonin, histamine. Delicate research, but promising. This is done in cooperation with Dr. Chryssanthou.

This research seems so important to us that we are slowing down the very deep diving a little during 1975-76, as well as our studies on HPNS. This is so that we can concentrate on the problems with several scientific teams from the university (Varene), the French Navy (Broussolle) and a new team of young scientists at COMEX.

Discussion

Dr. Schreiner: This matter of decompressing without real knowledge of whether you have a gas phase present or not still haunts me, and I know that it haunts a lot of other people.

Dr. Hills: Ideally we do want to prevent it, gas phase formation, but the trouble is, we don't know when we really do. We try techniques by which we can try to pick up the very early answer. So far the best one we have is Doppler. With that we can pick up about 40-micron bubbles at the velocity in the large vessels, but when you get to the peripheral arteries they have to be so large that they are larger than the vessels themselves. We are also looking into conductivity--impedance changes. This is not likely to be a useful field technique.

If we use the direct methods, are we looking at the bubbles which really cause us the trouble, anyhow? Our big problem is we don't know the tissue that is giving us bends.

Mr. Galerne: What kind of gas do you use in the JANUS profiles?

Dr. Fructus: Heliox, with 0.42 ATA oxygen.

Mr. Vann: The problem is, what is your end point? If it's only bends, then you can come up a lot faster, but you have to ignore the changes in enzymes, platelets, etc. If zero supersaturation is a relevant safe ascent criterion, then the fast tissues which make up the bulk of the body, muscles, etc. are being inadequately decompressed, and you are forcing gas out of solution. Therefore, in order to get out and have a normal subject, you have to come out very slowly, even perhaps slower than the normal saturation procedure. This may be a real limiting end point, to have a normal man at the end of the decompression.

Dr. Hempleman: There are three thoughts that might bear on some aspects of this problem. First, it has been shown theoretically that the heart, operating on normal blood, can cause cavitation. Two, work on joints has shown that you can pull a joint, and can actually see gas in the joint in an X-ray. And Walder has shown that there is sufficient uranium in the diet to create enough energy for nucleus formations. These are three of many spontaneous sources of gas cavities present physiologically; it might be that you can never operate monophasically.

Dr. Hamilton: He's not here to defend himself but I would like to make a comment on Dr. Chryssanthou's very interesting work. He shows dramatic increases in survival of mice with his anti-SMAF drug (smooth muscle activating factor), something apparently released in the decompressed animals.

But he uses death as an end point. He's studying the chokes-type bends, possible in aviation but rarely seen in diving, and this is why smooth muscle drugs protect these animals. He's studying massive embolism. I'd be very interested to see how this works on a larger animal, whether you can get any reduction in bends or other than lung decompression sickness.

Mr. Wide: We are now running a series of experiments on dogs, with tables calculated to give a 50% bends rate.

## C. U.S. NAVY OPERATIONAL EXPERIENCE: W. H. SPAUR

The U.S. Navy has enormous experience in deep experimental diving, but has not conducted many deep operations at sea. The MK I Deep Dive System operations to 1000 feet planned for the summer of 1975 should again give the Navy a deep dive system at sea.

In 1970, the MK I Deep Dive System conducted two series of dives off the California Channel Islands to perform an operational evaluation of the MK II semi-closed circuit underwater breathing apparatus. These open-water operations included working dives to 850 feet.

In July 1972, the MK II MOD 0 Deep Dive System conducted an open-sea dive series which concluded with an excursion to the Navy open-sea record of 1010 feet from a base saturation depth of 850 feet. The breathing apparatus used was a demand USN MK I Bandmask with inspiratory gas heater.

EDU's deepest dive has been to 1600 feet (49.5 ATA) conducted at the hyperbaric complex of Taylor Diving and Salvage Co., New Orleans. Seven days were spent at 1600 feet performing immersed exercise studies with MK 10 MOD 4 closed-circuit underwater breathing apparatus and modified KMB-9 Bandmask and other physiology studies. Seventeen days were spent at 1000 feet or deeper. The decompression lasted 19 days and the total duration of the dive was 32 days.

The 19-day decompression was done according to the Navy Standard Saturation Decompression Schedule. Four of the six divers suffered fairly severe bilateral knee pain between 106 feet and the surface. The chambers were recompressed four different times, at 10, 5, 5, and 3 feet, and high oxygen partial pressure breathing was utilized a total of six times. The knee pain was not completely relieved on any occasion but the residual discomfort was slight at final surfacing. Ten feet was our maximum recompression.

Two of the divers who had experienced knee pain remained in New Orleans five days and then boarded a commercial airliner for Washington, D.C. Both divers developed knee pain between New Orleans and Atlanta. It subsided during the ground period at Atlanta and then recurred on the flight to Washington, D.C.

This episode only illustrates our experience performing very deep dives. First, knee bends develop even on the relatively slow Navy saturation decompression schedule. Second, the relief of pain is not complete with treatment nor are recurrences prevented as decompression is continued. And third, wherever those bubbles reside, they must be constructed of marble to last five days after the completion of the dive and then cause pain at altitude.

Efforts to develop short-bottom-time dives deeper than 400 feet have not been required of us during the last four years. Our last attempt to develop non-saturation decompression tables was in 1970. Four-hour bottom-time dives to 400 and 650 feet were attempted for use with the MK I Deep Dive System. The four-hour bottom time was chosen as a reasonable time to perform a military or salvage task. A saving of from 30 to 40 percent of the decompression time compared to a saturation decompression was anticipated.

Dr. Workman's M values were used by Dick Buckles at NMRI to compute tables for 240 minutes at 400 and 650 feet. When the decompression reached the depth where the 240-min half time controlled, the Navy Standard Saturation Decompression was assumed.

The 400 ft/240 min schedule had a total dive time of 82 hours. The ascent reached the saturation decompression schedule at 200 feet, 8 hours and 45 minutes into the dive (Fig. VC-1). Three 400-ft dives with five subjects on each dive were completed, using the computed decompression table. One case of knee-pain-only decompression sickness developed at 180 feet and responded to recompression to 240 feet and high oxygen partial pressure breathing.

The first 650 ft/240 min decompression schedule had a total dive time of 115 hours and met the standard saturation schedule at 360 feet, seven hours into the dive (Fig. VC-2). Because of decompression sickness, two more 650 ft/240 min schedules were computed. The three different schedules were tested, each with a yet slower ascent, and each resulting in significant decompression sickness. On each ascent, recompression back to 650 feet was necessary for adequate decompression sickness treatment.

The decompression sickness cases were as follows:

1) During the first ascent, at 470 feet, one subject developed retrosternal chest pain, low back pain, profuse sweating, tinnitus, hearing loss, vertigo, and nystagmus. We examined him very carefully for lung problems--chokes--but concluded we had a high thoracic or cervical spinal hit, plus vestibular involvement.

2) During the second attempt, at 450 feet, two subjects had profuse sweating, dizziness, nausea, hearing loss, and nystagmus. We recompressed to 650 feet and the low-frequency hearing loss gradually recovered.

3) On the third 650-ft schedule, one subject developed moderately severe sacroiliac pain which prevented ambulating. This developed at 390 feet. He did not recover completely until some hours after recompressing to 650 feet. I don't think any of these labyrinthine hits would have recovered with shallow, say 50 feet, recompression.

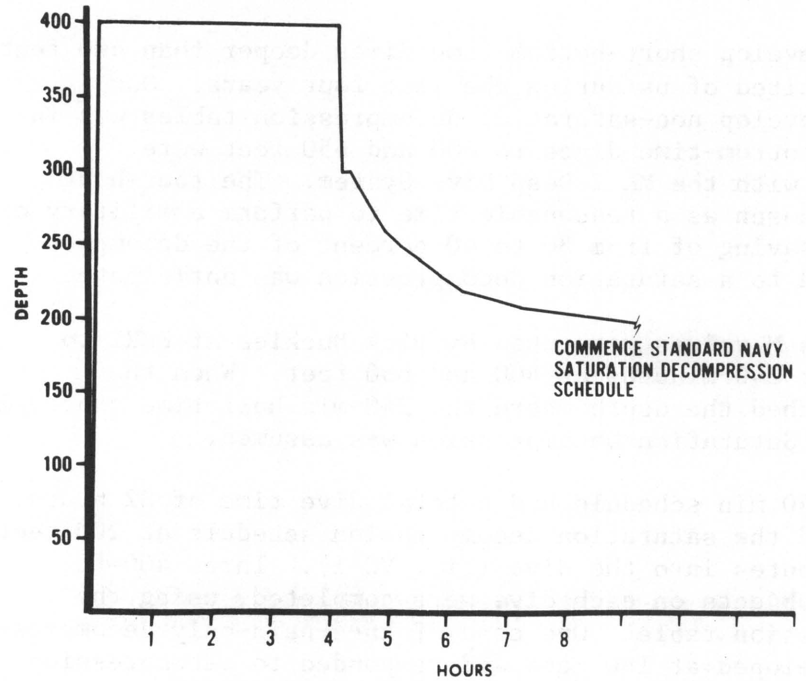


Fig. VC-1. Dive schedule, 400 ft/240 min, with total time of 82 hr.

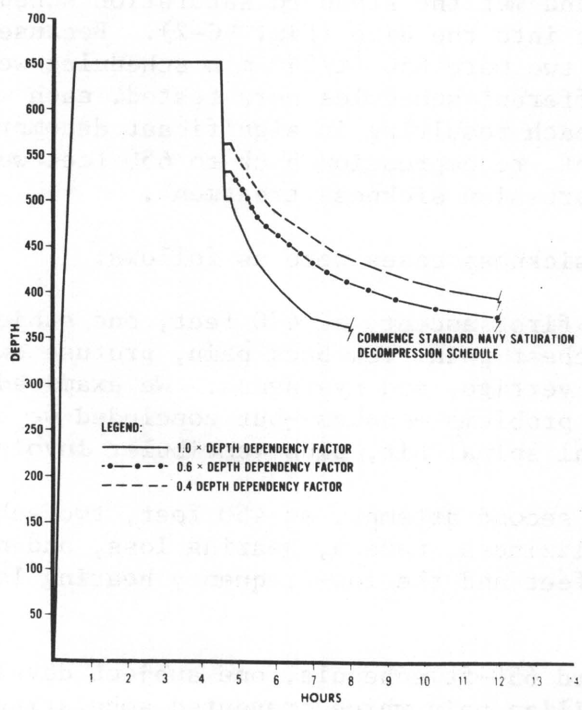


Fig. VC-2. Dive schedule, 650 ft/240 min, with total time of 115 hr.

To compute the two slower ascent 650-ft decompression schedules, the depth dependency factors of the M values, $\Delta M/\Delta$ 10 feet, were reduced by a factor of 0.6 and then 0.4. This first table had a total dive time of 115 hours, the second 120 hours, and the third 131 hours. The $PO_2$ was 0.5 on the bottom, 0.3 to 0.32 in the DDC.

We decided that these decompression tables could not be further modified and still realize a significant saving in decompression time over the Standard Saturation Dive Decompression Schedule. Now that the excursion tables have been extended, a basic saturation depth of 550 feet with excursions to 650 feet for unlimited bottom time only adds another day to the decompression as compared to these dangerous tables. The attempt to use the M value model in this fashion did produce two pieces of information. First, it is probably a dependable model to create labyrinthine decompression sickness and second, it establishes that a rapid ascent of 200 feet from 4 hours at 650 feet is not possible.

It is my personal opinion that saturation decompression procedures or saturation-excursion procedures should be used for Navy divers deeper than 400 feet and should be considered as the diving procedure of choice for dives deeper than 300 feet requiring significant work on the bottom.

## Discussion

Dr. Behnke: Were the defects permanent?

Dr. Spaur: No sir, they absolutely recovered. But I'm not sure what influence round window rupture has, or whether it was involved here, that may be why some cases have done badly. I can't be certain because we have only three cases. These people did these dives in the afternoon and then they were taken back to 650 feet and they did not receive a single foot of decompression again until the next morning at 06:00. They had 12 to 16 hours at depth eating potato chips and relaxing, sleeping.

Mr. Galerne: When we had vestibular hits we recompressed the men to full depth, but we solved nothing.

Dr. Spaur: I am sure that if they ruptured the round window on the way down there is nothing that recompression could do for them.

Dr. Flynn: I want to get back to the Access dive program. Dr. Hamilton, how do you envision the Access concept, with the storage depth significantly shallower than the working depth, fitting into the overall scheme of things? The way I look at this, if you have a job which requires 30 minutes or an hour, then the most efficient way would be to devise some kind of non-saturation dive, do the job and then come up.

As the job becomes longer, it seems to me you would want to come closer and closer to the work site. For instance, using the Navy concept, you could saturate at 100 feet above the work site and then make unlimited excursion dives of several hours, then come back up and start in decompression.

Whereas in the Access concept, let's say you have to excurse 400 feet, to 1000 feet, where you only have a 30-min bottom time and require 4 hours of decompression to get back to 600 feet. To do an 8-hr job would take you 16 excursion dives.

Dr. Hamilton: I wouldn't do an 8-hr job with that technique. The Access concept is based on the need for a half-hour bottom time. I mean, in no way would we try to do an 8-hr job with it, we'd use USN excursions.

As part of the Access program we proposed the "Re-dive," shown in Fig. VC-3. The idea is this. You really never know when the

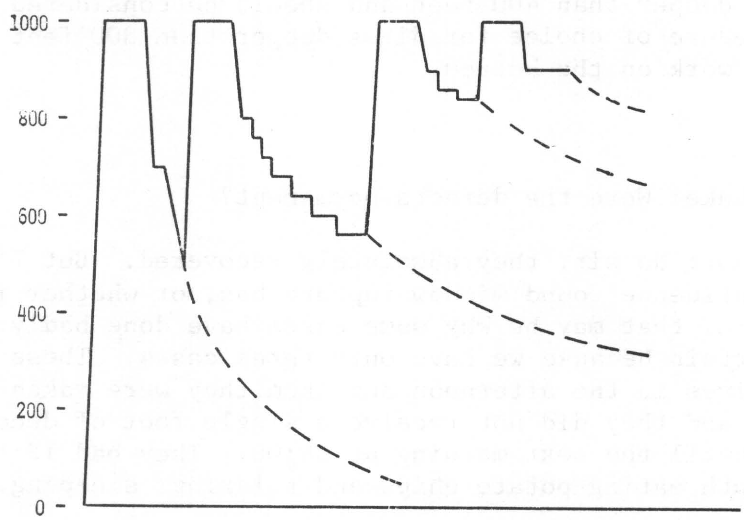


Fig. VC-3. Illustration of re-dive principle. A dive team performs a short job and begins an appropriate decompression, but it may be called back to the bottom for several successive short work periods before being fully committed to full saturation decompression.

next dive is coming; as Mike Hughes has said, in rig diving you are on standby condition all the time. When they are ready for you to dive, they've got a $50,000-a-day rig sitting there idle until you can get a man down there on the job. So a two-day saturation compression, just to get ready for the dive, is expensive.

Now, when we get much beyond 1000 feet we probably will have to accept that. For 800 to 1000 or perhaps 1200 feet we think it's possible to use a reasonable but yet fast compression, go down and do a job, and start back. The idea is then, if another job is needed, the diver can go back to the bottom from wherever he is in his decompression. It's usually a half-hour job, and it may be a day or two before he has to dive again. In the meantime he can start back, and he's starting back with the minimum practical decompression for that situation. He's only committed to a saturation if he's been at this awhile.

Dr. Flynn: Okay, but what is the storage depth concept? I've drifted away from Access. Basically, it's an excursion concept just like yours, but we use shorter times and deeper excursions, with stops.

Dr. Hamilton: Let me make a comment on something you implied but didn't call attention to. To do this kind of work day in and day out with a 400-ft excursion is going to cost a lot of compression gas. You are really much better off if you can handle a total saturation. You have a lot less risk and fewer problems and you don't need the gas for taking the bell up and down.

The thing that we're trying to get people to start thinking about, year after next, is that this ought to be all programmed on a computer that's there on board, because a stack of tables four feet high wouldn't cover all the situations that you might have. But you can program the optimal situation for an unknown length of work and an unknown excursion distance, and you compute the best situation.

Dr. Flynn: I'm still confused. Let me give an example: Let us say that you have what appears to be a half-hour job at 1000 feet. You compress at a very fast rate, using nitrogen to cover you, etc. You get on the bottom, the diver does the job, thinks he finished it and starts up. But as he's going through the 600-ft part of the profile which you labeled storage depth in Access II, he has more work to do, so the diver goes back down and starts working at it again.

Then he'll finish that, say, in the next 30 minutes, so it would be sort of like an excursion dive and then he starts back up. But he would continue to go back onto the surface if that job held, right? But in what situation would you ever expect a diver to be stored at depth for any period of time?

Dr. Hamilton: It's very likely that he knows he is going to have to dive again. He's waiting for them to get something or fix something. Is this reasonable, Mike?

Mr. Hughes: You've said, let's bring the guy back up as far as we can until he becomes obligated to some fairly slow ascent rates. Let's get him as far up as quickly as we can, until we've reached some arbitrary point--he's really starting to enter a sort of a saturation decompression. Then let's hold there and see what happens.

Dr. Flynn: Why hold there? Why not just continue on up?

Mr. Hughes: You could, but you also have the kind of situation where you know--based on the way this sort of problem has been solved in the past--that what's going to be needed is a series of 3 dives, probably spaced 8 or 10 hours apart because you are waiting for the guys on the deck to do something. So in that sort of a situation you may well want to take advantage of this because you end up back at a fairly shallow saturation depth.

Mr. Wilson: We've been operating with this technique for a long time. What we tell the customer is, we're "go" on a short duration dive, and then if something goes wrong during the diver's decompression, then we'll recommit the diver. We go to the bottom again, then take a look at the entire time and pick a depth or a number of depths. Then we'll hold there, start the saturation decompression, recommit him if necessary and just keep right on going like this. Just last month in the North Sea we operated for 25 days like this - back and forth - back and forth.

We are always attempting to get the diver as shallow as possible so his exposure under pressure will be as little as possible.

Dr. Youngblood: While we've got so many of the Navy experts here, I would like to ask a question which has puzzled me for a number of years on the standard Navy saturation decompression--the holds from 2400 to 0600 and from 1400 to 1600. Are these physiological or operational?

Dr. Spaur: The saturation decompression used in the Navy is a traditional one that was started during the SEALAB series; at a 4-ft per hour rate they had some bends and decided to have some holds. There used to be a hold at 450, 350, 250, 150, maybe, in the beginning. And then they said, "When would the medical officer like to sleep?", and it was from midnight to 6:00 in the morning. They gave him a nap in the afternoon, and they changed the rates near the surface to 5, 4, and 3 ft/hr instead of having a lot of 4-hr holds.

The changes were purely arbitrary, but they worked out very well, because we don't often have hits in the middle of the night, and because of these scheduled holds, for reasons I'll discuss in the treatment section, it makes treatment less of a hassle.

I am constantly getting people calling and saying, "Are those stops really necessary?" And the answer is, "You can come up at any rate you want, but the U.S. Navy saturation is based on 4 ft per hour, with holds." Do you have anything to add to that, Bob?

Captain Bornmann: It was just an attempt to regularize the schedule each day, and it worked pretty well.

Dr. Flynn: I think it was also predicated on the idea that circulation tends to slow down at night. Dr. Buehlmann certainly agrees with this.

Dr. Youngblood: I'd like to ask Dr. Buehlmann, we have been running his saturation decompressions from 1000 feet at 0.8 atm oxygen--which we feel is too high--and we would like very much to use this type schedule at 0.5 of 0.6 $PO_2$. Do you believe this is acceptable?

Dr. Buehlmann: Yes. I would like to comment on the matter of the transition from non-saturation to saturation decompression in deep diving. We agree with the experience of the U.S. Navy. I refer to dives in the range of 700 feet we did in 1966. With four hours of bottom time we are near 40 hours of non-saturation decompression. There is perhaps a little profit, but long-lasting exposures so deep--longer than 4 hours or 6 hours--are practically saturation schedules. Now for the short dives, 15 to 20 minutes, we have a profit with non-saturation decompression.

Mr. Vann: What was the oxygen you maintained and do you maintain it constant all the way to the surface?

Dr. Buehlmann: At that time, 0.8 atm.

Mr. Vann: Did you have any oxygen toxicity?

Dr. Buehlmann: Lung and breathing irritation yes, but not the nervous system. If we make it lower oxygen, the table is longer but the difference is not big.

Dr. Flynn: Have these schedules been published at all?

Dr. Buehlmann: In the Bennett book.*

Dr. Greene: Several people have mentioned the numbness and tingling of fingers and toes as a symptom of central nervous

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*Buehlmann, A. A. Decompression theory: Swiss practice. In: The Physiology and Medicine of Diving and Compressed Air Work, edited by P. B. Bennett and D. H. Elliott. Baltimore: Williams and Wilkins, 1975.

system oxygen toxicity. I am not familiar with that as any standard symptom. Would someone care to clarify that?

Dr. Bennett: I don't think anyone has said it is central nervous system toxicity. If you are involved in long duration breathing at relatively high oxygen partial pressures, for example, 0.8 atm, as we have done at Duke from time to time, you will get numbness of fingers and toes which is our indication of peripheral vascular shut-down or something like that. We don't know the mechanism.

Dr. Hamilton: How quickly did your people recover from that?

Dr. Bennett: It depends. Some individuals will recover very quickly, others more slowly. One man took several weeks to get back to normal.

Dr. Hamilton: In 1965 I experienced this after a 650-ft dive. Robert Stenuit who had had it before, told me, "You will not be sure whether you just become accustomed to it or whether it has gone away. Eventually it will go away." It was several weeks before I could be sure I didn't have that sawdust feeling at the ends of my fingers.

Dr. Youngblood: Fifty percent of the people on the Duke 1,000-ft dive had over two weeks of numbness.

Dr. Flynn: It is a common experience during treatment when a subject is breathing elevated oxygen pressures on deep dives. They complain of this not too long after they start breathing oxygen. In my experience it generally goes away rather promptly. The phenomenon is not uncommon.

Mr. Hughes: We all recognize the benefits of high oxygen. We have established that 0.8 atm is too high and 0.3 is probably less than optimal. We are very interested in elevating that oxygen partial pressure for saturation decompression as high as possible and still remain in safe limits. Is it possible to get an opinion on what is considered to be a reasonably safe maximum limit?

Dr. Hills: We are working on an empirical approach. We have only tested it for a convulsion. We do not yet have much experience. We feel it is plus or minus 10%. It is crude but it is much better than nothing.

Mr. Flynn: In the SHAD experiment at New London they did 26-day exposures at 50 feet and 60 feet on air and they were not able to demonstrate any pulmonary changes whatsoever. However, all four divers at the end of this exposure had a greatly reduced exercise tolerance. They could not walk long distances without becoming extremely fatigued and certainly could not do any heavy exercise. Recovery took many days--a few weeks. The only change that was apparent was in the hematology. The divers started out at normal hematocrits of roughly 45 and at the end of the exposure had gotten down to about 30 percent. No changes were apparent up to about ten days, and then after that the crit went down linearly. They aborted the dive when it went through 30% and it went down for about four more days before it showed a reversal.

Dr. Hamilton: This seems to be a new aspect of an old phenomenon, in that it looks like a mirror image of the increase in red blood cells induced by exposure to high altitudes. It is probably a normal adaptive process, following the same mechanism. My guess is that it may become a consideration where exposures last longer than about a week.

These experiments will be reported in detail at the 6th Symposium in San Diego next July.

# SESSION VI: TREATMENT

# VI. TREATMENT

## A. DECOMPRESSION SICKNESS IN THE BAY AREA RAPID TRANSIT PROJECT (BART): A. R. BEHNKE

The Bay Area Rapid Transit (BART) project was the first large scale, compressed-air tunnel operation in California and the largest industrial enterprise undertaken in the Bay Area. The objective was to construct an automated rail service over 75 miles of duo-rail tracks. The special problem was the pressurized construction of six miles of underground tunnels below metropolitan streets, in contrast to the non-pressurized installation of the Trans-Bay tube. This special project, which represents a remarkable advance in underwater engineering, required that sections of tunnel, each weighing 10,000 tons, be lowered into a man-made trench across the Bay between San Francisco and Oakland. The pressurized tunnel work, therefore, contrary to the usual location under a water bed, was carried out beneath busy, commercial thoroughfares.

A centrally located medical facility which housed three large recompression chambers was established in San Francisco to provide unified, around-the-clock services for the various contractors engaged in the project. The features of the service consisted of a comprehensive physical examination which accorded with newly formulated State regulations governing work in compressed air,* provision of central facilities for recompression therapy, and medical care of industrial illness and injury. Noteworthy in the physical examination was a radiologic survey of the long bones based on experience in the United Kingdom. This paper will summarize the results of treatment of decompression sickness, exclusively utilizing recompression and oxygen inhalation in accord with recent U.S. Navy approved Treatment Tables 5 and 6.

### Decompression Sickness

Incidence. From November 1967 to July 1968, there were 35,269 man-decompressions at pressures from 9 to 16 psig, and 18 cases of decompression sickness involving 16 of 284 workers (Table VIA-1). Noteworthy are the relatively low pressures (11.5 to 13.9 psig) associated with post-decompression symptoms following workshifts of six hours.

Subsequent to August 1969 there have been an additional 45,091 man-decompressions and 120 cases of decompression sickness (DCS). The total of 138 cases involved 85 men out of about 400 shift-workers and collateral personnel. There was a high incidence of decompression sickness above 29 psig despite curtailed hours of work (4 hrs at 29.5 psig and 3 hrs at 35-36 psig) (Table VIA-1). During the initial week

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*Chiefly the Washington State Tables (7).

Table VIA-1. Decompression sickness in relation to pressure and decompression time.

| Shift, hr | Pressure, psig | No. of Cases | Time, min | Decompression* Pressure Level | Total Time | Range |
|---|---|---|---|---|---|---|
| 6 | 11.5 - 12.5 | 3 | 2<br>6 | 4<br>0 | 8 | 6 - 13 |
| 6 | 13.0 - 13.9 | 13 | 2<br>12 | 4<br>0 | 14 | 6 - 30 |
| 6 | 14.0 - 14.9 | 3 | 2<br>16 | 4<br>0 | 18 | 16 - 22 |
| 6 | 16.0 - 16.9 | 15 | 3<br>34 | 4<br>0 | 37 | 33 - 63 |
| 6 | 17.0 - 17.9 | 5 | 3<br>59 | 4<br>0 | 62 | 33 - 90 |
| 6 | 18.0 - 18.9 | 1 | 3<br>83 | 4<br>0 | 86 | - - |
| 6 | 20.0 - 20.5 | 2 | 3<br>68 | 4<br>0 | 71 | 28 - 113 |
| 6 | 21.0 - 21.9 | 8 | 3<br>104 | 4<br>0 | 107 | 68 - 129 |
| 4 | 29.5 - 30.0 | 71 | 3<br>14<br>135 | 30 to 14<br>14 to 4<br>4 to 0 | 152 | 143 - 168 |
| 3 | 35.0 - 36.0 | 17 | 3 | 36 to 20<br>20 to 4<br>4 to 0 | 180 | 180 - 185 |
| TOTAL | | 138 | | | | |

***Pressure is reduced uniformly, in time indicated, to designated pressure levels; from 29.5 - 30 psig, pressure is reduced in 3 min to 14 psig, then to 4 psig in 14 min, and finally to 1 ATA in 135 minutes. Note that almost all of decompression time is from 4 to 0 psig.**

of exposure to the elevated pressure, there were 46 cases of DCS out of 570 manned shifts, despite the extended decompression used (Table VIA-2). By contrast, in the Lake City Tunnel (Seattle, 1964-1967) Dr. Sealey reported an incidence of DCS of only 1.7 percent in 3,000 man-shifts (8). The same decompression tables, adopted by Washington State and later by California, represented a radical departure from previous methods of decompressing tunnel workers, in that 1) a single daily work shift was stipulated in accord with U.S. Navy experience, and 2) a potential 8-hr work day in compressed air was apportioned between work at tunnel pressure and the decompression time accorded the work shift.

The lack of acclimatization may well have been an important factor in the high incidence of DCS (29.5 - 36 psig); some data from Walder's systematization (9) are shown in Table VIA-2.

Classification and Clinical Aspects. Of the 138 cases of DCS, 131 were classified as bends (Type I) and 7 were in the serious Type II category. With reference to symptoms, pain predominantly in one or both knees was reported 114 times, pain in the upper trunk and arms 22 times, and extreme fatigue, 3 times. A typical, diffuse macular rash was observed in 8 patients. The potentially serious and at times critical Type II cases were characterized by visual, nervous, and cardio-respiratory disturbances (Table VIA-3).

Selective Susceptibility of Workers. Of the 85 workers afflicted with DCS (out of a population of about 400 shift-workers and collateral personnel), 52 men had one attack and 33 men sustained two or more incidents of DCS. A Type II case was disqualified from further exposure in compressed air. Data on two susceptible workers are presented in Table VIA-4. One worker was treated six times for pain in one or both knees. He denied having had DCS on previous compressed air projects. Ten of the 33 susceptibles had a second attack of bends within 48 hours of the previous disability.

These findings are similar to those reported by Sealey for the Lake City Tunnel Project (8). Eighty-five men in this project were treated for Type I bends on 210 occasions; about one-third of the workers accounted for three-fourths of the cases. The most susceptible worker received 12 treatments on different occasions.

There are no criteria from our examinations to separate men who are susceptible from those (about 3 in 4) who are seemingly resistant, or at least do not report for treatment. In regard to such parameters as age, weight, and blood pressure, both systolic and diastolic, there is no significant difference between a susceptible and non-susceptible population. These data tend to support the innocuousness of temporary, although painful, impairment characteristic of Type I DCS. Recompression therapy with oxygen did not appreciably alter blood pressures and pulse rates.

Table VIA-2. High incidence of decompression sickness in unacclimatized men despite short work-shift (4 hr) and extended decompression time.

| Date (1969) | Pressure, psig | Decompression, min | Man-Shifts | No. of Cases DCS | % Man-Shifts |
|---|---|---|---|---|---|
| 5/6 | 29.5 | 153 | 81 | 6 | 7.4 |
| 5/7 | 29.5 | 153 | 96 | 9 | 9.4 |
| 5/8 | 29.5 | 153 | 95 | 6 | 6.3 |
| 5/9 | 29.5 | 153 | 85 | 12 | 14.1 |
| 5/10 | 29.5 | 153 | 14 | 4 | 35.0 |
| 5/11 | 29.5 | 153 | 15 | 2 | 13.3 |
| 5/12 | 29.5 | 153 | 26 | 0 | 0 |
| 5/13 | 29.5 | 153 | 82 | 4 | 4.9 |

Role of acclimatization in reduction of DCS incidence (Walder's data)

| No. of Compressions | 1 - 5 | 6 - 10 | 11 - 15 | 16 - 20 | 21 - 25 | 26 - 30 | 41 - 45 |
|---|---|---|---|---|---|---|---|
| % DCS Relative to No. Man-Shifts | 7.3 | 3.6 | 1.3 | 0.44 | 0.89 | 1.3 | 0.44 |

Role of acclimatization is evident from Walder's data on UK tunnel workers.

Table VIA-3. Type II (serious) cases of decompression sickness (DCS)

| Tunnel Job, Age, and Relative Weight* | Pressure, psig Shift Time, DC Time | Elapsed Time,** hr | Signs, Symptoms | Response to $O_2$ Therapy, Total Time |
|---|---|---|---|---|
| Shifter, 25 yr RW 113 | 13.5 2000-0200 DC(6 min) | 3.7 | Blurred vision, headache, nausea, vomiting, mottled skin, leg pain | Relief 26 psig, slow resolution of symptoms 229 min |
| Electrician, 36 yr RW 117 | 13.0 0800-1200 DC(6) 1330-1400 DC(6) 1430-1700 DC(5) | 3.2 | Ataxia, slurred speech, extreme fatigue | Relief of all disability at 26 psig, 155 min |
| Inspector, 38 yr RW 111 | 21.0 0305-0420 DC(16) 0525-0825 DC(15) | 2.3 | Scotomata, ringing in ears, nausea, vomiting | Relief 26 psig, 215 min |
| Miner, 34 yr RW 110 | 21.5 1400-2000 DC(103) | 4.0 | Dizziness, nausea, vomiting, pain both knees, viral infection(?) | After 175 min $O_2$ therapy, dizziness persists 2nd recomp. Total 397 min |
| Miner, 25 yr RW 86 | 30 2400-0400 DC(168) | 7.0 | Scotomata, numbness lips, arm, pain both knees | Relief 26 psig, 150 min |
| Miner, 28 yr RW 101 | 35.5 1600-1900 DC(180) | 3.9 | Blurred vision, rash, chest pain | Relief after 83 min at 26 psig, 225 min |
| Miner, 40 yr RW 120 | 29.5 1600-2000 DC(153) | 8.8 | Blurred vision, headache, chest pain, syncope, pain both legs | Relief 90 min at 26 psig, 295 min |
| | | | Weakness: 2nd recomp. | 120 min |

Type II cases involve neurological, respiratory, or cardiovascular involvement.

*Relative Weight: scale weight divided by avg. for age and height x 100;
**Time elapsed between onset of symptoms and recompression therapy.

Table VIA-4. Data on two patients who had repeated attacks of decompression sickness (DCS).

| PATIENT NO. 1 | Date | Pressure, Shift, Decompression | Symptoms, Time of Onset, hr | Interval from Decomp. to Therapy, hr | Oxygen Recomp. Therapy, time | Blood Pressure Before Therapy | Blood Pressure After Therapy |
|---|---|---|---|---|---|---|---|
| Age: 31<br>Rel. Wt.: 116 | 3/30 | 21.5 psi<br>1400-2000<br>103 min | Pain knee (R)<br>4 hr | 4 | Relief 15 min<br>26 psi<br>(165 min) | 150/105 | 150/110 |
| | 4/11 | 30 psi<br>0800-1200<br>143 min | Pain knee (L)<br>2 hr | 8.2 | Relief 25 min<br>26 psi<br>(155 min) | 150/88 | 140/88 |
| | 4/14 | 29.5 psi<br>0800-1200<br>143 min | Pain knee (L)<br>2 hr | 8.5 | Relief 5 min<br>26 psi<br>(157 min) | 142/90 | 138/88 |
| | 4/16 | 35.5 psi<br>0800-1100<br>180 min | Pain knee (R)<br>0.5 hr | 6.8 | Relief 10 min<br>20 psi<br>(155 min) | 138/88 | 140/90 |
| | 4/18 | 35.5 psi<br>0800-1100<br>180 min | Pain knee (R)<br>2.5 hr | 8.8 | Relief 40 min<br>26 psi<br>(155 min) | 140/80 | 126/88 |
| | 5/11 | 29.5 psi<br>2400-0400<br>153 min | Pain knee (R)<br>0.5 hr | 4.3 | Relief 12 min<br>15 psi<br>(90 min, 15 psi) | 138/100 | 128/100 |
| PATIENT NO. 2<br>Age: 26<br>Rel. Wt.: 102 | 4/5 | 21.8 psi<br>0200-0800<br>113 min | Pain knee (R)<br>3.5 hr | 8.8 | Relief immediate<br>(70 min, 15 psi) | 140/80 | 110/80 |
| | 4/16 | 35.5 psi<br>0400-0700<br>180 min | Pain knee (R&L)<br>immediately | 3.1 | Relief 9 min<br>15 psi<br>(90 min, 15 psi) | 92/54 | - |
| | 5/7 | 29.5 psi<br>0400-0800<br>153 min | Pain knee (R)<br>6 hr | 10.5 | Relief 45 min<br>15 psi<br>(115 min, 15 psi) | 132/96 | 126/74 |

Data from A. R. Behnke, Medical aspects of pressurized tunnel operations. J. Occup. Med. 12:108, 1970.

A procedure for selection of diving personnel resistant to decompression sickness (and highly susceptible as well) based on tests in a low-pressure chamber was reported in 1944 by Welham, Blanch, and Behnke (10). The three groups tested (poor, fair, and good) with reference to their resistance to decompression sickness were homogeneous with respect to leanness (body specific gravity), age, height, weight, and cardio-vascular response to step-up exercise.

Oxygen Recompression Therapy

Reference has been made to 10 of the 33 DCS repetitive patients who had a second attack of bends within 48 hours following recompression therapy. Apart from relief of pain, it is not clear to what extent bubbles are reduced in size or dissipated by relatively low-pressure oxygen therapy. The 75 out of 85 patients who were able to return to work for their next regular compressed air shift lead one to believe that if 'silent' bubbles or gas nuclei persisted following oxygen therapy, such a condition did not render this substantial group of workers susceptible to another attack of bends.

It was possible in some patients to shorten oxygen treatment (Table VIA-5) to about one-half of that stipulated in Navy Oxygen Table 5 (total time, 135 minutes). Although such abridgment appeared to be satisfactory, it is questionable in view of possible bone infarction (which may be painless), whether or not shortening of the prescribed minimal time of 135 minutes is advisable. The longest duration of oxygen therapy was more than 6 hours.

Table VIA-5. Range of recompression time on the BART project, min

| Treatments | Time Range |
|---|---|
| 24 | 60 - 90 |
| 16 | 91 - 120 |
| 75 | 121 - 170 |
| 12 | 171 - 220 |
| 9 | 221 - 300 |
| 1 | 360 |
| 1 | 397 |

Based on earlier experience, oxygen therapy was simplified by maintaining essentially two pressure levels: one at 25 to 27 psig and the other at 15 psig during the entire course of treatment. Since patients, particularly those who had "made the rounds", were extremely tired, they were allowed to sleep through some of the 'air' intervals. Oxygen was breathed from a demand system with open-circuit discharge into the chamber. This called for periodic chamber ventilation in the interest of fire safety. The noise attendant upon periodic chamber ventilation was objectionable.

Systematic tests at the Experimental Diving Unit by Workman, Goodman, and Bornmann emphasize the value of U.S. Navy treatment Tables 5 and 6, with some modification for treatment of air embolism (Table 6 A). These tables evolved from earlier investigations at EDU and the Harvard School of Public Health. Figure VIA-1 shows the "Anlage" of oxygen recompression therapy to emphasize that higher pressures above 26.7 psig (equivalent to 60 ft) may be required for treatment of spinal cord injury as well as a high partial pressure of oxygen (3).

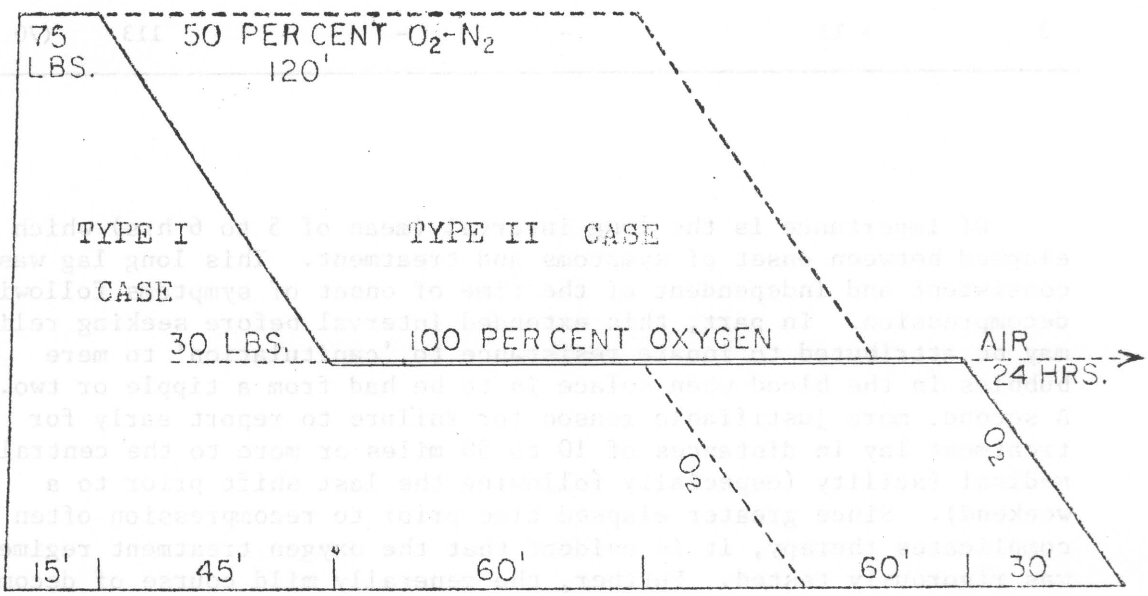


Fig. VIA-1. "Anlage" of oxygen recompression therapy which incorporated features of relatively high pressure with maximal tolerated oxygen. Prolonged exposure in hyperbaric air was utilized in therapy of patients with residual symptoms following the initial oxygen treatment (3).

In the BART treatments, minimal recompression (i.e., not higher than 26.7 psig, equivalent to 60 ft) on oxygen was highly satisfactory despite the long delay of 5 to 6 hours (average) before patients reported for treatment (Table VIA-6). With few exceptions, workers were able to resume their next regular workshift. This experience is again similar to that reported by Sealey (8).

Table VIA-6. Oxygen recompression therapy, BART project.

| Cases | Symptoms Time of Onset, hr | Elapsed Time, Onset Symptoms to Therapy, hr (Mean) | (Range) | Recompression Time, min (Mean) | (Range) |
|---|---|---|---|---|---|
| 29 | < 0 - 0.5 | 5.8 | (1.3 - 11.2) | 144 | (90 - 275) |
| 46 | 0.6 - 2.0 | 4.7 | (0.5 - 13.0) | 150 | (90 - 255) |
| 38 | 2.1 - 5.0 | 5.6 | (0.8 - 13.7) | 140 | (60 - 365) |
| 23 | 5.1 - 12.0 | 5.3 | (0.3 - 10.0) | 165 | (95 - 397) |
| 2 | > 12 | - | - | 113 | (70 - 156) |

Of importance is the long interval (mean of 5 to 6 hrs) which elapsed between onset of symptoms and treatment. This long lag was consistent and independent of the time of onset of symptoms following decompression. In part, this extended interval before seeking relief may be attributed to innate resistance to 'capitulation' to mere bubbles in the blood when solace is to be had from a tipple or two. A second, more justifiable reason for failure to report early for treatment lay in distances of 10 to 50 miles or more to the central medical facility (especially following the last shift prior to a weekend). Since greater elapsed time prior to recompression often complicates therapy, it is evident that the oxygen treatment regimen was rigorously tested. Further, the generally mild course of decompression sickness may be attributed in part to shorter hours of work at the higher pressures and the prolonged decompression time stipulated in the Washington State Tables. The gratifying result from curtailed hours of work and extended decompression is, that there is no evidence of residual disability from the compressed air exposures after a period of five years.

## Improvement in Recompression Procedure

1. Maintain minimum pressure at 15 psig and complete therapy at this pressure. Oxygen is well tolerated at this level and is in fact compatible with maximal exercise. Oxygen decompression at the 40-ft stop has long been standard practice in the U.S. Navy. Theoretically,

the effectiveness of gas transport diminished proportionately to diminution of pressure from 2 to 1 ATA. Further, the ineffectiveness of oxygen inhalation at ground level compared with elevated pressure is well known from the occasional disastrous result in treatment of altitude decompression sickness at ground level.

2. Position and need for better circulation. The supine position is preferred by patients and Balldin has reported greater nitrogen elimination supine compared with the sitting position (1). Balldin's supine subjects, however, were awake and the role of sleep (which prevails during the course of uncomplicated therapy) has not been determined, but some feel it retards gas elimination. Some striking results have been reported by Balldin (2) which show nitrogen elimination and prevention of decompression sickness are facilitated when subjects were immersed in warm water to head level, sitting position. Immersion in water prevents inward diffusion of ambient inert gas and the neutral buoyancy attending water immersion permits increased circulation (hydrostatic effect) as well as a type of exercise conducive to minimal cavitation effect.

3. Air intervals. The extension of eupneic oxygen inhalation time attending periodic inflation of the lungs with air has long been known, but the reason for the increased tolerance conferred by the short 'breaks' (prevention of atelectasis, enzyme recovery) has not been ascertained. In Type II cases or under conditions of residual disability following initial therapy, air intervals with intermittent oxygenation can be extended for a period of days at levels of 40-50 feet.

4. Adjunctive (plasma replacement) therapy not required in the BART cases. Note that in 128 Type I and in 7 Type II patients, invasive therapy in the form of plasma, dextran, or saline infusions was not required, nor were drugs administered. There is currently the danger of iatrogenic error and as a result of overzealous treatment, Type I cases have been transformed into patients who require intensive care. This topic requires extensive analysis.

5. Need for higher pressures in treatment of spinal cord injuries and auditory-vestibular involvement. Again, this topic requires extensive documentation which is not presented in this paper. Prolonged exposure in air at 40 to 50 feet (over a period of days) is recommended for patients whose disability, notably paresis of the lower extremities, persists.

## Improvement in Decompression Procedure

Here are a few comments on tabular data included in this paper, relevant to the main theme of this Workshop.

1. Current decompression tables for tunnel workers do not totally prevent decompression sickness (Table VIA-7). Although total time (Washington State Tables) appears to be adequate, there is insufficient

Table VIA-7. Incidence of decompression sickness in which decompression followed stipulations of the Washington State Tables.

| Pressure | Man-decompressions | Decompression Sickness | Rate/1000 man-DC |
|---|---|---|---|
| 13 - 17 | 10,000 | 12 | 1.20 |
| 18 - 20 | 11,000 | 33 | 3.00 |
| 21 - 25 | 13,000 | 90 | 7.00 |
| 26 - 29 | 1,600 | 24 | 15.00 |
| 30 - 34 | 3,000 | 51 | 17.00 |
| 9 - 16 | 35,269 | 18 | 0.51 |
| over 16 to 29.5 | 44,521 | 70 | 1.57 |
| 29.5* | 515 | 43 | 83.50 |
| 35.5** | 55 | 4 | 72.70 |
| 16 - 20 | 455 | 1 | 2.30 |
| 21 - 25 | 508 | 1 | 1.90 |
| 26 - 30 | 1,310 | 23 | 17.60 |
| 31 - 35 | 1,288 | 28 | 21.70 |
| 36 - 40 | 607 | 7 | 11.50 |

* = 4-hr work shift; ** = 3-hr work shift. Top panel data from Lake City Tunnel, Seattle, 1964-1967; middle panel from BART Project, 1967-1969; lower panel is data from Milwaukee, 1971-1972.

time accorded nitrogen elimination from rapidly desaturating tissues. Further, a great deal of time is spent in decompression between 4 and 0 psig. Hempleman's 'Blackpool' tables for work shifts not in excess of 4 hours, tend to remedy these objections (6). In these tables, however, the decompression time accorded work shifts in excess of 4 hours at higher pressures is inadequate to prevent disability.

2. During decompression, if prolonged (say, up to 3 hours), gas transport from the lower extremities may be severely retarded as a result of relative immobility and restricted circulation during the course of card games, with workers in the sitting position. A promising if not striking advance in decompression practice is the extension of the principle of water immersion elucidated by Balldin (2) to include the supine position in water which would permit moderate exercise under conditions of neutral buoyancy to limit the effect of stress causing cavitation.

3. Immersion in water with the worker enclosed in a fire-resistant garment (head excluded) would render oxygen inhalation safe. A novel gain from this procedure, if the body were enveloped by oxygen, would be percutaneous elimination of inert gas in place of inward diffusion of nitrogen in an ambient air atmosphere*. A prototype oxygen decompression table has proved highly satisfactory in tests under stringent conditions but limited in number (Table VIA-8).

Table VIA-8. Prototype oxygen decompression table for tunnel workers

| Tunnel Pressure, psig | Work Shift, hr | Δ $PN_2$, psia | Fraction* to lose | Calc. DC time, min Total | Per hr work** |
|---|---|---|---|---|---|
| 20 | 6 | 26 | 8/26 | 64 | 10 |
| 30 | 6 | 33 | 15/33 | 105 | 18 |
| 36 | 6 | 35.2 | 17.2/35 | 117 | 20 |
| 40 | 4 | 36 | 18/36 | 120 | 30 |

*From a 120-minute (t ½) tissue; permissible $PN_2$ assumed to be 18 psia; **for a continuous or intermittent work shift.

*The importance of percutaneous diffusion of helium has long been known (4) but measures to circumvent inward cutaneous diffusion of helium and facilitate percutaneous elimination of helium from body tissues have not been implemented in diving. The potential of accelerated decompression via water immersion and envelope oxygen constitutes a challenge to systematic investigation.

4. Current decompression schedules for compressed air workers are not satisfactory unless work shift time is curtailed at higher pressures so that pressurized tunneling becomes economically prohibitive (Table VIA-7). Oxygen is effective but rigid precautions must be taken with a relatively undisciplined worker population to prevent a tunnel conflagration. A practical measure, highly successful in diving, is recourse to the saturation habitat with excursion exposures to work shift pressures. It is unlikely in pressurized tunneling that habitat pressure will exceed 15.6 psig (equivalent to 35 ft) and such pressure would permit 6-hr work shifts at 40 psig (equivalent to 90 ft). Conservative decompression without oxygen supplementation, from 15.6 psig to normal pressure, would require about 10.5 hours.

Summary

The BART patients (128 Type I, 7 Type II cases) were successfully treated with minimal recompression and oxygen inhalation without recourse to invasive (infusion) therapy or drug supplementation. Overzealous (iatrogenic) therapy may pose a hazard in current practice.

It was mandatory in the BART regimen that a doctor be in the chamber with the patient or patients throughout the entire course of treatment. It was thus possible to obtain a detailed history and necessary background information, and to conduct definitive medical examination at therapeutic pressure levels. Notably, there was no delay in recompression on oxygen when a patient was brought in for treatment.

## REFERENCES

1. Balldin, U. I. Effect of ambient temperature and body position in tissue nitrogen elimination in man. Aerospace Med. 44: 365-370, 1973.
2. Balldin, U. I. The preventive effect of denitrogenation during warm water immersion on decompression sickness in man. In: Proc. First Annual Scientific Meeting European Undersea Medical Society, edited by C. M. Hesser and D. Linnarsson. Stockholm, 1973. Foervarsmedicin 9:239-243, 1973.
3. Behnke, A. R., and L. A. Shaw. The use of oxygen in the treatment of compressed-air illness. Nav. Med. Bull. 35:61-73, 1937.
4. Behnke, A. R., and T. L. Willmon. Cutaneous diffusion of helium in relation to peripheral blood flow and the absorption of atmospheric nitrogen through the skin. Am. J. Physiol. 131:627-632, 1941.
5. Gray, J. S. Constitutional factors affecting susceptibility in decompression sickness. In: Decompression Sickness, edited by J. Fulton. Philadelphia: Saunders, 1951.
6. Hempleman, H. V. The new decompression tables, Appendix 1. In: A Medical Code of Practice for Work in Compressed Air. CIRIA Report 44, Feb. 1973.
7. Sealey, J. L. Safe exit from the hyperbaric environment. Medical experience with pressurized tunnel operations. J. Occup. Med. 11:273-275, 1969.
8. Sealey, J. L. Minimal recompression, hyperbaric oxygen treatment of decompression sickness in tunnel workers. In: Hyperbaric Medicine, Proc. Fourth Int. Congress, Sapporo, Japan 1969, edited by J. Wada and T. Iwa. Baltimore: Williams and Wilkins.
9. Walder, D. N. Some problems in working in a hyperbaric environment. Ann. Roy. Coll. Surg., England, 38:288, 1966.
10. Welham, W., J. J. Blanch, and A. R. Behnke. A procedure for selection of diving and aviation personnel resistant to decompression sickness based on tests in a low pressure chamber. Exp. Diving Unit Rept. 1, 1944.

## B. U.S. NAVY INCIDENCE AND TREATMENT OF DECOMPRESSION SICKNESS IN SATURATION DIVING: W. H. SPAUR

The U.S. Navy has enormous experience in deep experimental saturation diving, but less experience in open-sea saturation diving than a number of commercial diving firms have. The diving procedures and decompression schedules now in use have evolved over a ten-year period of experimental diving and still need continued study. Though U.S. Navy decompression is slower than many others, it has not achieved bends-free diving, nor have we been able to perfect completely adequate treatment procedures.

The currently used saturation decompression schedule has remained unchanged since 1969 and is as follows:

Navy Standard Saturation Decompression Schedule

| | |
|---|---|
| 1600 - 200 feet | 6 ft/hr |
| 200 - 100 feet | 5 ft/hr |
| 100 - 50 feet | 4 ft/hr |
| 50 - 0 feet | 3 ft/hr |

Decompression is carried out for 16 of every 24 hours, according to the following table:

| | |
|---|---|
| 2400 - 0600 | Stop |
| 0600 - 1400 | Travel |
| 1400 - 1600 | Stop |
| 1600 - 2400 | Travel |

LCDR Thomas Berghage, MC, USN has tabulated the USN saturation dives recorded at the Navy Safety Center for a 24-month period.

Years 1972 and 1973

| Saturation Dives | Number of Man-Dives | Cases of Decompression Sickness |
|---|---|---|
| Shallow, less than 300 feet | 238 | 4 |
| Deep, greater than 300 feet | 27 | 3 |

The overall incidence of decompression sickness was 2.6% for this period. The 11% incidence of decompression sickness in saturation decompression from depths greater than 300 feet is similar to data on saturation decompression prior to 1970 when the schedule was slightly different. For this 24-month period, saturation dive cases represented 22% of the Navy's decompression sickness cases. This figure is more acceptable when compared to other information compiled by LCDR Berghage. The saturation dives in these two years represented almost 1000 man-days under pressure, or 20% of the U.S. Navy divers' total time under pressure in all types of diving. The Navy logs approximately 64,000 dives annually.

Most of the Navy's deep saturation dives have been conducted at the Experimental Diving Unit, Washington, D.C. These dives have been performed for training or to study saturation decompression, saturation-excursion procedures, breathing gas mixtures, underwater breathing apparatus characteristics, or diving physiology. Therefore, the decompressions often followed multiple excursions and many days of exercise studies. It has been the habit to work the divers at near maximum sustained work levels in swimming, pedalling a bicycle ergometer, or weight lifting. The bottom times are typically about seven days.

At EDU we usually have five subjects on each saturation dive. With a 11% incidence of decompression sickness on deep dives, we only arrive at the surface on schedule in about half of the dives. Unless very near the surface, we usually recompress all the subjects if one needs treatment. Therefore, a true incidence of decompression sickness on saturation decompressions cannot be determined.

The decompression sickness is always manifested as knee pain, the left knee being the greater complainer, according to LCDR Berghage's data. The pain is gradual in onset, usually over several hours, and typically occurs while still under pressure. Knee bends requiring recompression have occurred as deep as 520 feet during ascent from a 1000-ft dive. We have seen calf pain, also. There has never been a case of decompression sickness other than pain-only on the Navy Standard Saturation Decompression Schedule.

The historical files of the Experimental Diving Unit were analyzed by LCDR Berghage to assess the adequacy of various treatment regimens. His analysis indicated that 59% of the decompression sickness had a depth of onset less than 50 feet; 81% of the cases had a depth of onset less than 100 feet; and 95% of the cases had a depth of onset in 150 feet of water or less. Only 35% of the treatments in these saturation dives resulted in complete relief. The time at treatment depth to achieve complete relief was generally about twice as long as it takes to achieve complete relief in non-saturation diving.

The guidelines for treatment of bends in saturation decompression appear in the Saturation Diving Section of the 1973 U.S. Navy

Diving Manual and are as follows:

1) Recompress in increments of 10 feet at 5 ft/min until improvement is indicated by the diver. Note: An initial recompression to depth of complete relief is not necessary unless the recompression is 30 feet or less. A recompression depth increase of more than 30 fsw is usually not necessary and may result in increasing pain.

2) During recompression and at treatment depth, a treatment mixture may be given by mask to provide an oxygen partial pressure of 1.5 to 2.5 atmospheres. Pure oxygen may be used at treatment depths of 60 feet or less.

3) Interrupt the mask treatment every 20 minutes with 5 minutes of breathing the chamber atmosphere.

4) Remain at the treatment depth a minimum of 2 hours.

5) Resume the Standard Saturation Decompression Schedule from the treatment depth.

This method is not recommended for the treatment of decompression sickness occurring under pressure except in saturation decompression rates similar to the Navy Standard Saturation Decompression Schedule.

Our experience at EDU led us to offer these guidelines for treatment. Rather than an immediate attempt to recompress to the depth of relief, recompression 10 to 30 feet deeper usually caused prompt partial relief of the knee pain. With high oxygen partial pressure breathing and holding at the treatment depth, the remaining discomfort gradually resolved. If we attempted deeper recompression to resolve the residual discomfort, the divers sometimes complained of increasing pain as we went deeper; this pain could not then be relieved by any maneuvers. Our experience also seemed to be that any attempt to decompress after treatment at a rate faster than the saturation decompression rate caused increasing pain. Once recompressed, the decompression was best started over from the treatment depth at the saturation decompression rate. Our final conclusion was that recompression of 10 to 30 feet followed by a hold at the treatment depth for some hours was less traumatic than greater recompression followed by ascents more rapid than the saturation decompression rate.

## Discussion

Dr. Smith: Did you examine those individuals in a detailed fashion when they said they had knee pain?

Dr. Spaur: With most of the dives we have a physician in the chamber. The hits are usually under pressure but they're all given a kind of an orthopedic examination where you kick it, crank it, twist it and squash it. But they are not given a good neurological examination.

Dr. Smith: It is the opinion of some individuals that most pain-only bends may very well have a neurological control, but it's just not diagnosed.

Dr. Spaur: The time to achieve complete relief was about twice as long as it takes during regular diving recompression. And we only had about 35% treatment success, if you look at complete relief.

Dr. Hempleman: Have you got any speculations why they get worse when you recompress them?

Dr. Spaur: No, I don't have any idea at all. I just had some bad experiences taking people up and down several times and then having them limp off into the sunset when the dive was over, and they usually limp for about five days. If you are shallow, between 30 feet and the surface, sometimes you treat with an ordinary thing like a Table 5.

Dr. Bennett: Isn't it possible that if the bend is associated with local ischemia, and if you pump the oxygen up higher, you're going to make that ischemia worse?

Dr. Greene: In experiments with ischemic tissue, hyperbaric oxygen doesn't decrease the blood flow, it only does that in normal tissue.

Dr. Youngblood: Having just seen this phenomenon repeatedly and having experienced it myself, I agree.

Dr. Spaur: Ed Flynn made a dendritic treatment guide for Mike Hughes, like the choice diagram that appears in the little treatment handbooks and in the new saturation manual. It's better than the one that's in the big handbook. It recommends that once you pass 100 feet you recompress with helium--these are hits under pressure in DDC - SDC systems, short duration dives. When you pass 100 feet, you have the option of going until you get relief, but you mustn't end up at 300 feet with 250 feet of air in your chamber. In that kind of diving you would recompress to the depth of relief and be prepared to recompress to the depth of the dive. When we did our 650-ft "vestibular" program, we went all the way back to 650 feet and got very nice resolution, from 430 and 450 feet. They didn't really respond completely until we got back to 650 feet. We stayed overnight and then began saturation decompression the next day. I have read dive reports where a crew may get a vestibular hit and recompress 30 feet or 50 feet or something like that. I think that's absolutely wrong, you should open up the stops.

Dr. Smith: A few months ago Dick Strauss had, in Science, an article about the dangers of recompressing with helium when in a nitrogen environment. Did you agree?

Dr. Greene: I agree with Dr. Spaur that it's advisable to push back to the depth of the dive, if necessary. This gas exchange problem is a very thorny issue I'd rather avoid; I don't know the answer. But with a vestibular hit you're better off just planning on going very deep once you get back to helium.

Dr. Bennett: One thing, if you have vestibular bends you should be recompressed as fast as you can. Speed is essential.

Mr. Hughes: After experiencing some vestibular problems, we contacted several people and asked, "What gas should we use as treatment gas?" We got two answers, oxygen from one person and helium from another. We immediately used trimix as a treatment gas. With regard to recompression, our company, and all of the other companies in the industry, were ill-equipped to handle a saturation decompression from the depth of the dive.

We may have had a deck chamber which could be raised to the depth of the dive, but it had absolutely minimal life-support capability; a saturation decompression might have been darn near as dangerous as a vestibular problem. So we were faced with a very tough situation. We--I mean the whole industry--have upgraded our field equipment so that we now have a saturation decompression capability for emergency purposes, at least, even with the non-saturation type systems. Now we can go back much deeper than we did before and expect to get the guys out reasonably healthy.

Dr. Smith: Do you treat an auditory problem the same way that you would a vestibular?

Dr. Spaur: I would say so.

## C. TREATMENT EXPERIENCES AT DUKE UNIVERSITY: D.A. YOUNGBLOOD

When we started preparing for the decompression program at Duke, we decided that it was a medical experiment in a hospital environment, and that we would try to improve current treatment procedures as well as decompression tables. We were looking for field applicability -- operational simplicity -- in our treatment procedures, and required that they be useable by non-physicians. In addition to operational simplicity, we knew that if we were to learn anything new, we had to do it the same way enough times to find out for sure if it was right or wrong.

Our Duke protocols were based very much upon the Navy's. The first change we made was to alter the definition of Type II (serious) decompression sickness so that it did not include pain occurring under pressure.

We recompress at 5 feet per minute, in increments of 30 fsw, looking for significant relief. I was happy to see the Navy change, a few years ago, from "complete relief" to "significant relief". In this type of dive, where the hit is in the shallow end of the table, we experience mostly "saturation" type bends - knee pain, sometimes shoulder and arm pain, or shin bends. The diver is a model of denial. He always thinks he's hit his knee or rolled over on his arm in his bunk. When he gets to the slower stages and can feel the change of intensity with pressure changes, he finally admits to himself that he is bent.

After one atmosphere of recompression, we begin high oxygen partial pressure breathing. To maintain our independence from the Navy, we say 1.2 to 2.8 ATA of oxygen instead of 1.5 to 2.5. This is a minor point, but it simplifies gas logistics. You need a slightly fewer number of quads of treatment gas to cover a greater spectrum. Even after you have shifted to air and begun recompression, if you have not achieved relief by, say, 130 feet, you then shift to helium for further recompression.

We were cognizant of the problem that helium diffuses faster than nitrogen, and you can expect, perhaps theoretically, some enlargement in the bubble. But remember, you still have the option of further recompression, hopefully to reduce the size of the bubble faster than it grows from the diffusion into it. You can only do that if you shift to helium. Once we achieved a depth of significant relief, we stay there for one or two cycles and we have 25 minutes on, and 5 minutes off treatment mix. If it's 60 feet or shallower, we're on pure oxygen. We use 25 minutes and 5 minutes instead of 20 and 5.

As the program progressed, to along about the Sierra and Romeo tables* which were quite long and had high UPTD dosages, we were beginning to get all the way to the surface and have simple pain-only decompression sickness occur post-dive. But since divers had been exposed to so much oxygen, they were beginning to complain on Tables 5 and 6, which were our standard tables for simple bends occurring after surfacing. I tried to modify those tables by decreasing some of the oxygen exposure and found that that was unsuccessful. So we went back to the standard Navy Tables 5 and 6 despite the fact that we were having some troubles with the high oxygen. But in a treatment situation you have to accept the oxygen; it's a drug, and most drugs have side effects. We feel that the benefits outweigh them.

We have only had two Type II or Central Nervous System decompression sickness problems during our experiments at Duke, and one of those is questionable. The first was a case where the diver had what we thought was Type I. It was pain in his arm and he was recompressed and got some relief and then had a slight recurrence. Then a medical officer locked in and did a neurological examination, at about 60 feet. He detected some questionable numbness in the forearm and what he interpreted as a definite weakness in grip in that hand.

Simultaneously, the diver developed a sudden onset of supra-orbital pain. Our protocol for serious decompression sickness in this situation was to recompress back to 165 feet and/or to the depth of relief, assume we'll hold there, and go on a treatment mix breathing schedule and start intravenous infusion. Our rationale for this is first, that a person in this situation may go into shock, and also we're trying to train some of our divers as emergency medical technicians. We wanted to establish the philosophy of getting a life line in while they still have adequate veins that a relatively untrained person can get an i.v. started in. In this case we started lactated Ringer's and a slow dextran infusion. The fellow got complete relief. I had assumed, because the orders were so written, that this recompression had been on helium.

I didn't find out until some six hours later that he had not been recompressed on helium. This answered a question that I'd always wondered about, whether or not one could use the modified Buehlmann schedule on air in place of Table 4. I don't say this gives proof, but I would welcome the opportunity to try it again. If anybody else makes that mistake I would like to hear about it. As most of us who were in the Navy know, Table 4 causes about a 25% bends incidence in tenders, which makes it a questionable treatment table. The Buehlmann schedule is practically equal in time to Table 4, and has the added advantage -- particularly if you can do it with heliox instead of air -- that you can go to any depth for significant relief and stay for any length of

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*See paper IIIG of this workshop.

time and do whatever adjuvant procedures are necessary and then begin a standard decompression. We are hoping to prove it for field use because it will greatly simplify things for diving supervisors offshore. Table VIC-1 shows the parameters for this decompression schedule.

Table VIC-1. Duke Saturation Decompression, 1000 ft on 0.6 ATA $O_2$

| Depth, ft | Gas mix, Ascent rate |
|---|---|
| 1000 feet | change to $He/O_2$ |
| | 2.5 ft/min (60 min) |
| 850 feet | |
| | 12 ft/hr (46 hrs) |
| 300 feet | |
| | 8 ft/hr (26 hrs) |
| 100 feet | |
| | 4 ft/hr (10 hrs) |
| 60 feet | change to air |
| | 2 ft/hr (30 hrs) |
| Surface | |

* Total time 113 hrs (4 days and 17 hrs); $O_2 = 0.6$ ATA.

We also had a case of vestibular decompression sickness on a dive which had an air shift at 110 feet. The air shift consisted of diving through a doggy door -- a plastic flap -- from a heliox atmosphere into an air atmosphere. About 10 minutes after the switch the diver developed a sudden onset of severe vertigo, which he described as the chamber wheeling around him. He was nauseated and vomiting and had nystagmus -- horizontal nystagmus.

We had discussed repeatedly what to do in the event of vestibular decompression sickness. Dr. Joe Farmer, at Duke, has done a lot of work on vestibular problems, and his feeling, along with some of you, was that one should recompress to the depth of the dive. I held out as the devil's advocate -- I felt that immediate recompression to a reasonable depth might work better offshore, because the diving supervisor wouldn't waiver in his decision, but would do the recompression promptly. So we compromised and concluded that in the case of a vestibular hit we would recompress immediately to 100 feet beyond the depth of onset.

This is what we did. We transferred him back into the chamber with helium-oxygen, and recompressed him with helium to 100 feet. His nystagmus improved to about 15% as much as he had at the beginning.

One of the other things we had agreed to try on an experimental basis -- based on some of the work I think the French have done -- was the use of diazepam or valium. This tranquilizer is reported to have a specific affect on the vestibular end organ.

Within 20 minutes after the valium was given intramuscularly, the nystagmus was gone. We put him on treatment mix when he got to 100 feet for 2 cycles and brought him out on a Buehlmann saturation decompression. Within 4 hours after surfacing we did a follow up electronystagmogram and he was the same as baseline.

I'd like to make another point. First, we ran into the oxygen toxicity phenomenon of numbness in the fingers and toes. We had two people who were exquisitely sensitive to this, apparently. One diver was doing 500-ft dives on Monday, again on Friday, and then again on the next Wednesday. He was getting progressive finger numbness which would go up to the first joint and then recede over the weekend; then he would make another dive and it would go up a little further and then recede partly and after about a week of this, we took him off of diving status. His arterial pH was normal. He was also a snuff user. He had used great quantities of it while he was in the diving chamber. He stopped it, and after that he eventually got so that he was not as oxygen sensitive. In fact, he made a 500-ft lock outside a submarine back in December and had no problem on that table.

I would welcome any comments regarding the use of dextran 40 or diazepam. We have written dextran in as an evaluation, but I feel it is safe enough for emergency medical technicians to use offshore if they are able to start intravenous infusions.

## Discussion

Dr. Smith: There are problems with any drug given in a hyperbaric atmosphere: first, you don't know what the action is, and also you don't want to mask the symptomatology. Does valium desensitize the end organ? If it is a cochlear infarct -- because of a bubble, you want to get rid of the infarct, and if you're just desensitizing it with the valium, it's not what you want to do.

Dr. Youngblood: I'm not sure I agree. My feeling is, if it achieves nothing more than symptomatic relief, it's probably worthwhile. The patient is faced with 2 or 3 days of decompression; if you can relieve his vertigo, you've done something for him, especially if he can then take oral fluids instead of intravenous, and be able to eat within a day. My understanding is that these cases take days or weeks to recover, and if you can get symptomatic relief right away you avoid a lot of secondary problems.

Dr. Behnke: I am concerned about the late effect of oxygen. We do not know as we didn't know with radiation, what the effect is on the life span of the mammal that breathes oxygen at therapeutic levels for 2 hours every day. These studies will have to be made. With regard to use of oxygen in therapy, there is no reason to drop that pressure below 15 psi, no need to stage below 15. You can get the beneficial effect, then come out in 5 minutes.

Now, the residual case, the individual who's been treated but who still has a little pain. In the old days we put him back at 30 feet or 35 feet, possibly 40 feet, left him there overnight, then brought him up slowly. Most of the residual cases we had cleared up with this treatment.

For the problem cases, why not then have an oxygen-nitrogen mixture, say 50-50, ready as a standard treatment mixture and then you could go to 65 psi. This gives you the advantage of pressure and you can drop anytime down to half this level without changing the pressure of nitrogen. You get the added effect of pressure. You can remain there two hours and then drop down. If this isn't high enough, you would go then to helium. You have no recourse but to go to a higher pressure and the only way you can work at a higher pressure and stay there, is to use helium.

Dr. Youngblood: I find that diving supervisors offshore have never heard of the problem of the ruptured round window, and this can really lead to a serious problem. The younger untrained group of divers will not abort a dive because of the difficulty in clearing. They set themselves up for a Valsalva-type rupture of the round window. It seems possible that they might not feel it until they have finished their job -- which is quite often very short -- and leave the bottom. Then they feel the symptoms on the way up. The diving supervisor has to assume a vestibular problem. They end up in a 3-day saturation decompression, at the end of which it's too late to correct it surgically. I would like to see this information disseminated to the field as rapidly as possible.

Capt. Bornmann: How do you diagnose it?

Dr. Flynn: I think you are in a dilemma here. I don't think there is any way to diagnose round window rupture.

Dr. Youngblood: No, there is no way to diagnose it, but the most common cause of this is difficulty with clearing and excessive pressure in the Valsalva maneuver. I know about one case where a diver could only clear one ear, and went all the way down to 100 feet until something happened. The divers need to be told what the hazards are and how to spot it when it happens. If they recognize it during compression, then they should abort the dive and get the fellow to shore.

Mr. Galerne: It can happen on the way up, too.

Dr. Hills: I would like to make a comment relevant to treatment, possibly of Type II bends. We managed to develop the technique for making ultra-microbubbles. This is bubbles down to 40 microns in diameter, for any gas which we could inject into the carotid artery of a guinea pig. We developed a cranial window in which we could look at these microbubbles as they entered the cerebral circulation. They went into the arteries which go over the surface of the brain before they dive deeper. We found that as the bubbles came in we saw dilation of the artery as had been reported, but as soon as another bubble came in behind it, the first bubble would slow down. Eventually, they would come up together, more bubbles would come in and, in a very characteristic pattern of coalescence, they'd all pop together and there was then a long column of air, and this simply wouldn't move at all.

What was totally unpredictable to me was that when we recompressed by going to 200 feet for one minute and coming right back to the surface, all this had cleared up. It was quite remarkable. I would have thought that you had just pushed it together and upon decompression it would have come back. When we excised these brains and increased their gas content using compressibility measurements, we found that we had cleared about 90% of the gas.

We then cut a little vein during the treatment, and found that we were physically pushing the gas out of the venous system. This is rather interesting because what this would indicate is if we have a cerebral embolism, there's a possibility that just a rapid compression to something like 200 feet and back might prove a useful treatment. I met a Russian team who said that they do this routinely. If they get a Type II hit they compress to 300 feet and then back. They've done it purely empirically, but it is certainly compatible with what we have seen in this window.

Another interesting point is that normally with air you cannot reverse the coalescing process. However, if you're breathing oxygen you can actually reverse it, and more fluid seems to get pulled into these arteries. It certainly is another factor in favor of $O_2$ treatment.

Dr. Schreiner: How do you explain that?

Dr. Hills: I don't. This is just an observation: there are reasons which you could develop such as the gas dissolving more rapidly with $O_2$ breathing. If you inject bubbles of pure $O_2$, with air breathing, you also find that the bubbles clear much faster, that you can reverse the process which we were never able to reverse on air without recompression. We can't yet say this is a definite treatment procedure, because we did find that two of the guinea pigs died under this treatment. There may be other factors, and of course your lungs may be holding back bubbles.

Dr. Smith: I would not expect to see bubbles in the arterial system. You are talking more about embolism than decompression sickness.

Dr. Hills: We were thinking about the matter of submarine escape, because what was indicated of course is that you take a few breaths of pure $O_2$ before you come up so if you do get cerebral gas embolism let it be $O_2$ and not air.

Mr. Hughes: I've heard of several situations where a man has ceased breathing on the bottom and the reaction of the diving team in every case is to commence decompression immediately and at the same time administer artificial respiration. In none of these cases did the man survive. Maybe commencing decompression is not the thing to do. I have never seen anyone say not to. If this group of people agree that that is a very poor procedure, then I think it is time the operational people were informed that they should not do this.

Dr. Flynn: What were the circumstances surrounding this cessation of breathing?

Mr. Hughes: I had one case where a man vomited for some unknown reason and probably strangled or something and by the time he was recovered in the bell, he was no longer breathing.

Dr. Schreiner: You could have drowning. You could have electric shock, or laryngospasm. You could have a number of things. Clearly, when you stop breathing, when you stop exchanging gas, you have a body loaded with inert gas, and if you decompress it, it is like explosive decompression. There is no way that the gas can come out except by breaking down the supersaturated state physically, and forming a gas phase -- bubbles.

Dr. Youngblood: I preach that you don't bring a person off the bottom in this type of situation.

Dr. Flynn: Cardiopulmonary resuscitation is the problem here, not decompression. You have to have oxygenation first, then worry about decompression. Unless the other diver is trained to do this, you are out of luck anyway.

Dr. Youngblood: In the diving situation you have high oxygen, and this greatly improves your chances of survival.

Dr. Schreiner: For what it is worth, my opinion is not to decompress until there is no hope.

Dr. Bennett: I agree with that. I think we ought to add another corollary to that in terms of oxygen convulsions. Don't just rush to the decompression valve when a man is convulsing. It's a very tempting thing, but with a diver thrashing around, that's the worst thing you can do.

Dr. Behnke: You can sometimes defibrillate with a sharp blow to the chest, and this is helpful to begin circulation as well. It's not a perfect solution, but little can be lost by trying.

Dr. Nome: I agree.

Dr. Flynn: Possibly the best compromise is to seal the bell and try to surface it and lock it on, then make a transfer under pressure to the deck chamber. In the meantime, the diver in the bell is doing what he can to maintain circulation and respiration. You could then put your best medic into the chamber to carry on. The main point is, don't decompress a man who isn't breathing!

## D. TREATMENT EXPERIENCES IN THE NORTH SEA: TOR NOME

I'd like to begin by telling you about a problem I had this summer. A young Scottish diver with about one year of commercial experience and three years before that of sports diving had been working about two weeks on a barge in the Echofish field. I think this was his fifth or sixth dive. They were doing surface-oriented diving with oxygen decompression in the deck chamber. Diving depth was 170 feet and the work was fairly strenuous -- I think he was cutting. During his bottom time he experienced some headache, not very serious, just a slight headache. He remained on the bottom and did his work for his designated 30 or 33 minutes. On his way up he felt nauseous for a few minutes and he was wondering what he should do. He was afraid he'd throw up in his mask. He came to the surface, went through the decompression procedure, resurfaced with no problem, went to his cabin and had a hot shower, and went to bed. One hour after surfacing he woke up with a bad headache which got much, much worse and he became nauseous, vomited, and could hardly walk. His comrade managed to drag him and sluice him into the chamber which was then pressurized to 60 feet. They stayed there for just about 3 minutes, got no change, so they went down to 165 feet. They phoned Dr. Robin Cox. After 10 minutes at 165 feet, the diver came to and felt perfectly okay. Due to the fact that it only took 10 minutes at 165 feet to get relief of symptoms, he was then brought back to surface on Table 3, I believe. It took 9 or 11 hours to reach the surface.

He reached surface with no problems, had another hot shower, went to bed again and woke up after a total of 4 hours after surfacing the second time. This time he had an increasing headache and was sick to his stomach but didn't throw up. They recompressed him to 60 feet, gave him oxygen, and after about 15 minutes all symptons were gone. They phoned Dr. Cox again and got approval for the oxygen treatment table -- a reasonable decision. Symptoms had gone away after 10 or 15 minutes so he surfaced again. This time he did not take a shower. He felt clean now.

One hour afterwards, a worsening headache put him back into the chamber; 60 feet on oxygen relieved his symptoms again. This time Dr. Cox said to use the long oxygen treatment table. Relief of symptoms had been no problem. Every time he had been taken down to 60 feet and given oxygen it had relieved his symptoms. So he was given the long oxygen treatment - the 285 minute table - came to surface and again felt fine. Two hours afterwards it recurred again!

It was decided to take him down to 165 feet and start him on Table 4, and at that time it was decided that a doctor should go out on the barge to examine the man and provide some on-site medical advice. This was so far away from Great Yarmouth that Robin phoned me. It was a Friday afternoon and I agreed to go out. I planned to go out late Saturday night because he was due at 30 feet sometime Sunday morning. Two hours afterwards, however, they phoned me again and said "something's happened. The diver is in bad shape, he's in distress. His blood pressure is falling, his pulse rate is going up, his face is pale. Could you come immediately?" I managed to get out there at 12:00 Friday night.

When I came, the diver felt better. I went in and examined him and found nothing except a slight resistance in the neck at about 45-50 degrees. Everything else was normal.

I phoned Dr. Cox, discussed what I had found and decided there was nothing else to do but put up the dextran drip. So we did that. Anyway, we waited 3 hours and there was no change in the diver's condition. He had a slight residual headache and could not put his chin down on his chest. So after having thought awhile, I went in and gave him another bottle, plus 8 mg of decadron.

Six hours later there was no change in this man's condition. He had residual headache and had a slight resistance when trying to move his head. And at that stage, about 12 hours after I got there, I started wondering if it could be anything other than decompression sickness. Could it, for example, be a case of cerebral hemorrhage, as one of the things nobody thinks of.

If that were the case, my big problem was, if this guy should deteriorate upon reaching the surface, what should I do? Should I then recompress searching for the depth of relief, or should I try to differentiate before I went that far. So I had a couple of spinal needles sent out and I phoned up Alverstoke and talked to Jim Vorosmarti, and asked if there is anything that says I couldn't do a spinal tap under pressure? He promised to phone me back if he found any contraindications for this. And I heard nothing, so I assumed he hadn't found anything. In fact, Jim had been trying for 4 hours that night to reach me, and I was very happy I didn't know that.

Anyway, the diver's condition did not deteriorate. I got him out, took him back on the helicopter to the hospital in Stavager, did a spinal tap, and low and behold, there was blood in the spinal fluid. Later on he was sent to Aberdeen, and they did an angiogram and found a small aneurysm, and it was operated on. Three weeks after that, he came to the diving dock in Aberdeen and said - "When can I start diving again?"

Well, this is just one problem one might encounter in treating divers. I think one very important point to us who have this for a sideline or a hobby, is that most of the time the illnesses we are called on to treat are ordinary illnesses in a peculiar environment. Decompression illnesses are mostly treated by the diver supervisors themselves. I've been on call, for diving in the Norwegian part of the North Sea for the last 3½ years and during that time I've only actually gone out on the helicopter to treat decompression sickness 4 or 5 times. I've heard about treatments and prolonged treatments afterwards, and a few times I've been telephoned and asked what should they do and I've just more or less put my seal on what they had started to do, and in most cases, this has been okay.

We have been talking now a great deal about the development of decompression tables for dives in excess of 400 feet, and this is very important for diving. But for me, as a diving medical doctor, I am primarily interested in the safety of divers, and I must admit that the diving table is not the most important thing. Last year in 1974 there were 10 divers that lost their lives in the North Sea. And as far as I can tell, in none of the cases were the problems due to decompression tables. There were decompression accidents, to be sure. Two divers lost their lives because the drop weight fell off the bell and the bell surfaced within 1½ minutes from 120 meters. The diver who was out was pulled up by his umbilical and the tender was thrown out and both these divers died. Decompression was a factor, but not the procedures. I examined the one body that was recovered a couple of hours afterwards and he was full of gas all over. Another case was due to a fatal misunderstanding about what a gas bottle contained. In this particular diving firm they were used to adding oxygen to the helium bottles. They placed the heliox bottles in the left-hand corner and the helium bottles in the right-hand corner. They had to transfer some gas bottles to another barge. During that transfer the mistake happened. The diver got suited up at 100 feet. They did something else I don't like -- they pressured up the bell with air. They were diving at 70 meters. From 100 feet and down they used the breathing mix, the diver in his suit and through his mask, and the tender on the BIBS. The mix was supposed to be an 84-16 heliox mixture.

At 100 feet pressure, 70 meters depth, the tender had suited up the diver, opened up the heliox mixture, and took a mask himself. He turned around to see how the diver was doing and saw him keeling over, and then he lost consciousness and was only able to yell out something so that topside decided something must be going on down there. When everything became very quiet they said something must be terribly wrong, so they started hoisting up the bell. When they got the bell on the deck they found two divers in the trunk and they pulled them out. The tender came to within half a minute but the diver was dead. Cardiac massage and mouth-to-mouth resuscitation failed to revive him. He was put under pressure and taken down, but to no avail.

The diagnosis of the autopsy was asphyxia. The tender said that he remembered seeing the diver keel over, and then remembered seeing it get lighter through the port holes, and then he saw that he had lost his BIBS, so he grabbed hold of that and went under again. That bottle contained, you guessed it, only helium.

Another diver died because he was swimming around on the surface, got seasick, started vomiting in his mask and when the autopsy was performed they found vomit in his trachea. One died because he was too old, or he was having illnesses that would normally have prevented him from diving. A couple of years ago, in fact, during a diving medical examination, the diving doctor said "You have high blood pressure, and ECG changes, and you must never dive again." So the diver bowed and said, "Thank you very much. I'm very glad for these examinations that you can find that we are suffering." So he went out of the door and went straight to another diving firm. And a couple of weeks afterwards he was down on the bottom again. He suffered a stroke shortly afterwards. I don't know who you can blame for that, where a diving firm is willing to employ a man on his word that he has been examined two months ago; the diving firm never asked for the actual examination papers.

When you compare 10 deaths to the total number of divers in the North Sea, which for the moment is someplace between 700 and 800, you get a percentage of mortality of about 1.5%. A paper by Mark Bradley gave the mortality rates among commercial divers as 667 divers out of 1,000,000 a percentage of 0.667% mortality per year.

With regard to regulations and things like that, in Europe there has been a committee working for about 1½ years. It's called the European Diving Technology Committee. It is a daughter of the diving safety committee of the Society for Underwater Technology. This committee was instrumental in getting out the Principles of Safe Diving Practice (CIRIA). The European Diving Technology Committee is trying to get together information about the requirements and standards of what should be used in diving, and things like that, about what medical qualifications one should use for divers, how to train them, what diving schools should teach, and so on. We are trying to standardize things so that these regulations will become almost the same in all the countries of Europe.

One big problem is that the current regulations apply only to UK ships or on fixed installations or at 500 meters, around that. Whereas on foreign barges -- and most are foreign barges -- if they are 500 meters away, the regs do not govern them. The dive with the pure helium, which was not very successful, was subject only to the regulations of Panama. I have no knowledge of what kind of regulations Panama might have governing deep diving in the open sea.

One other problem is a concern in the training and experience of divers. Due to the tremendous increase in the diving population, the standard has gone down appreciably. Whereas earlier a deep sea diver had 6 or 8 years of experience, now an experienced man with supervisory responsibility may have 1 or 2 years of experience. In the future it will just become worse, because everything is increasing. The situation of the doctors is not much better, because physicians have to be trained and educated, and also must learn to take it easy and not believe too much in what they've been told in medical school.

Getting back to decompression sickness, if a diver has a problem on a rig, what should we do? We take him down to pressure where there is relief or, if it's saturation, we don't primarily seek relief, just add a little bit of pressure. Then, we would want the medic on the rig to go in and perform a pretty good medical examination and then get out of the chamber and telephone us and tell us his findings. He should also be able -- under our instructions -- to go in and put up a drip or give an injection or what have you and hold things together until a doctor can get out there, if that's what's called for.

I have one or two more remarks to make, mostly questions. One is, why do divers frequently faint when they are diving and working? This is something that happens very often. When a diver locks out from the bell and does his work, quite often, a few minutes later, the people topside can hear him starting to increase his breathing. They'll tell him to slow down, stop what he is doing and return to the bell. But mostly he will not hear what they say. Why, I don't know. I think he is so far gone already he does not accept the information that is coming down and shortly afterwards the breathing rhythm changes and he has fainted. Why this happens, I don't know. I think it must have something to do with $CO_2$. I don't see how it can be due to hypoxia. As you all know, "hyperventilation" at pressure is the opposite of "hyperventilation" at one atmosphere. I'm also concerned about what we are to do if a diver has an accident. I'm not talking about decompression sickness. I'm talking about a physical accident like having his arm crushed. Well, there are two possibilities: if it's so bad you just have to amputate it, it's easily done. If it's not that badly injured, and can be saved, how will that affect the decompression schedule for that diver? What kind of decompression table would you say the diver should use? Will you use a normal decompression saturation table or do you have to take into account the lower perfusion rate?

Another point, which is covered in more detail by Andre Galerne, is what to do if a diver requires immediate surgical assistance. Not just a diving physician who goes in and examines the diver and puts up a drip or something like that, but a surgeon with a surgical skill who can do difficult operations. It may happen that these things are required while the diver is under saturation. One way is to have a system where the diver can be transported under pressure from the place where he is to a bigger system on shore, where he can be locked in and the surgical team then marches in and does the operation.

I'm an internist, and we are notoriously suspicious of surgeons. We don't believe that it's necessary to operate quite as often as surgeons say, but even so, there are instances where one must operate immediately. I would think that there might be cases where it would be much better to have the surgeon and anesthetist brought out and put into the chamber with the divers, instead of putting up the immense logistics problem of trying to transfer the diver under pressure.

Discussion

Mr. Hughes: I've heard about this problem of divers fainting on bottom, and I can say that of thousands and thousands of dives we've made, we've never had a man faint on bottom.

Mr. Wilson: Let me describe a little scenario. The diving bell goes to the bottom with two men sitting in it, at atmospheric pressure. And the first thing that they do is look at the job out of the port hole. And they get ready to pressure up. But before they pressure up, they see if the equipment is all right. So they pick up their masks and take a few quick breaths. The object is to get out of the little hatch at the bottom of the bell, to get to the job and do the work as quickly as possible. For every minute that they spend there, they are going to have to pay for it in decompression. So they are ready to pressurize. They reach up and they turn the valve on. Then they grab their noses, they hold their breath, they pop their ears -- the $CO_2$ in the blood at this moment is going up, up, and up. The rush of gas is going through the lung on a one-way street.

Now, when they reach the bottom, the diver puts on his mask. Then he holds his breath to see if he has a face seal. He's still holding his breath, popping his ears, all the way down and now he has this mask on, and he's still holding his breath. I'd like to see a number on his $CO_2$ blood level. And then he pops out of the bell very rapidly and swims or walks to the job, going like hell. And then he passes out.

We came up with a workable solution. We hold the man for at least 30 seconds in the bell before he locks out, to get a little equilibrium. We've had no more problems ever since. So I think this is about right. At least it works.

Dr. Youngblood: There is a phenomena which is fairly well known in aviation, that's often confined to executive pilots that work hard all week and are used to having two or three martinis at night. They get three days off, get in their private airplane and since they are very obsessive-compulsive personalities, they abstain from alcohol and end up -- on about the third day -- in a relative hypoglycemia. They are used to having alcohol and it affects the liver so that they get this hypoglycemia. Then when they go on instrument conditions and are under stress, they get a hypoglycemia low enough to cause problems.

Hypoglycemia may also happen to divers. They tend to drink heavily when they are on the beach. They are quite often drunk when they get on the helicopter, and yet may be called upon to make a dive within a few hours. They generally do not eat very much before a dive. Or, they may be awakened out of the bunk and they take one cup of coffee with two spoonsful of sugar and then go. They are already hypoglycemic and then they get a reactive hypoglycemia from the high carbohydrate load, plus the stress.

A situation happened in Oceaneering recently, which Mike doesn't know about. It was a new team staging out of Singapore. The diver had been up for three nights and days en route, traveling to the job. He had no solid food intake and I don't doubt he'd had considerable alcohol. He landed on the helicopter and two hours later was at 450 feet, locking out of the bell. He did his job and fainted as he was coming back into the hatch.

Dr. Nome: This thing happens much too often for abstinence from alcohol and hypoglycemia to cover more than a few incidents. I personally observed one of these incidents when I was marooned out on a rig by bad weather. I had been called out because everybody thought they had scabies. It turned out to be body lice. Anyway, the weather calmed down enough for diving before the helicopter could operate. The diver was carrying an explosive charge to put on a mud riser, against a 2-knot current. He wasn't able to do it. He started hyperventilating. He was told to slow down and return to the bell. He didn't hear, fainted and was pulled back. I think it has to do with $CO_2$.

Dr. Hamilton: I'd like to emphasize something Dr. Nome said, but which may have been missed. That is the paradoxical nature of the "hyperventilation" which happens to a diver breathing on a demand rig. He is trying to ventilate at 45 breaths per minute, but instead of the low $CO_2$ which hyperventilation implies to a physiologist, he is probably rebreathing his dead space and actually has a high $CO_2$. We have some limited laboratory evidence that this is the case.

## E. THE NORTH SEA HYPERBARIC CENTER: ANDRE GALERNE

The high price of oil exploration in the North Sea has received attention for some years and we are noticing that an equally high price is being paid by the divers who work there. The North Sea takes an average of 12 lives per year, one a month. It is proper to view this circumstance with concern. It is even more proper that this circumstance be met with action. I would like to share my concepts with you, and tell you about the actions which my company, IUC, and its working partner, Conoco, are taking.

The industry has made a fair effort to come to grips with the problem: individual firms have undertaken proprietary research, and more and more, they have participated in joint efforts. This encourages me to think that this is a mature industry. Now, however, the offshore oil industry has encountered some new difficulties in the North Sea: perenially foul weather with extremely short storm-warning time, heavy seas, very cold water, and a generally adverse geographical location. We also have new government involvement to deal with--there is very strong pressure on the diving community from that quarter.

Five or six of the best diving companies in the world are sharing the North Sea work. They have all had their headaches and are beginning to improve the situation. However, we have come to the realization that we will not be able to work in the North Sea without some kinds of diving accidents. Our company, IUC, has a perfect diving safety record (that is, zero fatalities), and we mean to maintain this record. So, when we sat down to sign a contract with Conoco for diving in the North Sea with a 14-man crew, we chose to be careful.

Conoco shared our concern for safety; it was one of the reasons they had come to us and we agreed on a number of safety guidelines and some very high standards for supervisory personnel. But the most significant single step we took was to resolve to build a hyperbaric hospital to service our divers. This decision was made in recognition of the fact that there has been no good answer to the question of treating major trauma, or for that matter, a simple appendectomy while a diver is in saturation. When recompression is the answer, as in major bends problems, this can be treated best aboard the rig. But where surgery is involved and decompression may take several days, a hyperbaric hospital is the only answer.

Right now, there are 600 divers working in and around the North Sea, and thousands more to come. Some of the exploration and most of the pipelaying will require saturation techniques. The North Sea hyperbaric center provides a starting point for treatment, if not a total answer. There are still many unknowns to be solved:

1) How can pneumothorax best be detected?

2) What drugs can be administered and tolerated under pressure?

3) Might acupuncture be a suitable anesthetic?

Other considerations have also motivated Conoco, practical considerations. Although I am sure an oil company would not hesitate to immobilize a rig if it were a question of a diver's life or death, the rig's daily expenses average $50,000. Suppose for a moment that a diver must have medical attention within the DDC, and recuperate there for a 10-day period of decompression before drilling operations can recommence. The shutdown costs could run to half a million dollars. So the humane solution, to get the diver off the rig and into a properly equipped and staffed hyperbaric hospital, is also the practical solution. Right now, we are working with our colleagues at other diving companies to build a universally adaptable transfer capsule so that divers on all the rigs can be transferred under hyperbaric conditions. We have installed a very roomy saturation system, capable of working under pressures to 1,000 feet as a surgical and medical treatment room. This is the chamber system originally made by Airco and used for the first 1000-ft saturation in the U.S.

Our intention is to work in close cooperation with the Dundee hospitals and universities to prepare nurses and surgeons to work under hyperbaric conditions. Many obstacles remain, many answers need to be found, much research remains to be done. How do we keep a diving system like this aseptic? Can surgery be performed if the surgeon has HPNS? All of these subjects will require study, energy, and money. We think it is warranted.

More research is required, and I would like to announce that the North Sea Hyperbaric Center is available immediately for hyperbaric research, personnel training, or any other worthwhile activity on a straight-cost basis. I hope that in other parts of the world, similar institutions can be established, so that when a diver has a medical problem, he will find a place and people ready to help him. The diving community has found a common reason to share some of their competitive "trade secrets." We have recognized (sometimes reluctantly) that if we fail as a group to render better service to our customers, then we will fail individually as well.

In the beginning, we did not recognize the need for cooperation; we had our ego problems. This seems to be the case for the medical profession right now. There are some doctors who have been willing to cooperate with each other in the field of hyperbaric medicine, but not many, not enough. I would like to convince this group of the need to build medical teams for research into and treatment of diving problems. The undersea world is the last frontier on this planet and we must have your help to make it a safe place in which to work and live.

# SESSION VII: SELECTED RESEARCH TOPICS

# VII. SELECTED RESEARCH TOPICS

## A. DECOMPRESSION STUDIES WITH ULTRASOUND: K. H. SMITH

Many people do not believe in ultrasound. There has not been a sufficient amount of convincing information with regard to it, and ultrasound units have been used by some individuals without much success. But one should not condemn the process because one is unable to hear something on a certain type or number of exposures, whether that is due to the individual's lack of experience or the equipment's lack of development.

One thing we need is some money to spend for equipment development to assist in the detection of a potential decompression problem. We have done a lot of work with humans in the chamber monitoring for bubbles, monitoring peripherally in superficial veins; contrary to what Brian Hills says, superficial veins do work very well. You can detect gas emboli going through venous circulation very well, from radial veins, brachial veins, femoral veins, sub-clavians, a variety of fairly superficial veins. The most useful place on a human, though, using the Doppler, is with a chest or precordial probe, which attaches to the body and can be focussed on the pulmonary artery. This is the confluence of the venous return from the heart and therefore manifests the entire body.

Figure VIIA-1 and subsequent figures are data from a series of exposures we have done on two goats. These animals were implanted with ultrasonic bubble detectors on the vena cava.

The ordinate is bubble counts on a semi-log plot. We have developed a counter which uses a many-band filtering process, bands which run from 500 cycles up to approximately 3,000 or 4,000 cycles. This is the range which will pick up most of the emboli. The abscissa shows the time frame in minutes. Note particularly when the first bubble occurs, the number of bubbles, and what happens to this population of emboli over time.

The table used was the U.S. Navy 220-ft profile for 20 minutes, an exceptional exposure profile. We used it because it is a standard of comparison. We used a standard rate of ascent, 60 ft/min, and the total dive time was 62:40. One goat is represented by a star, the other by a circle.

The objective was to determine with successive dives, day after day, what the nucleation or bubble formation rate would do. Some people say that nuclei are random. Others say they exist in the body all the time and after one day of bubble formation some of those nuclei would be eliminated and consequently, you would expect fewer bubbles the next few days.

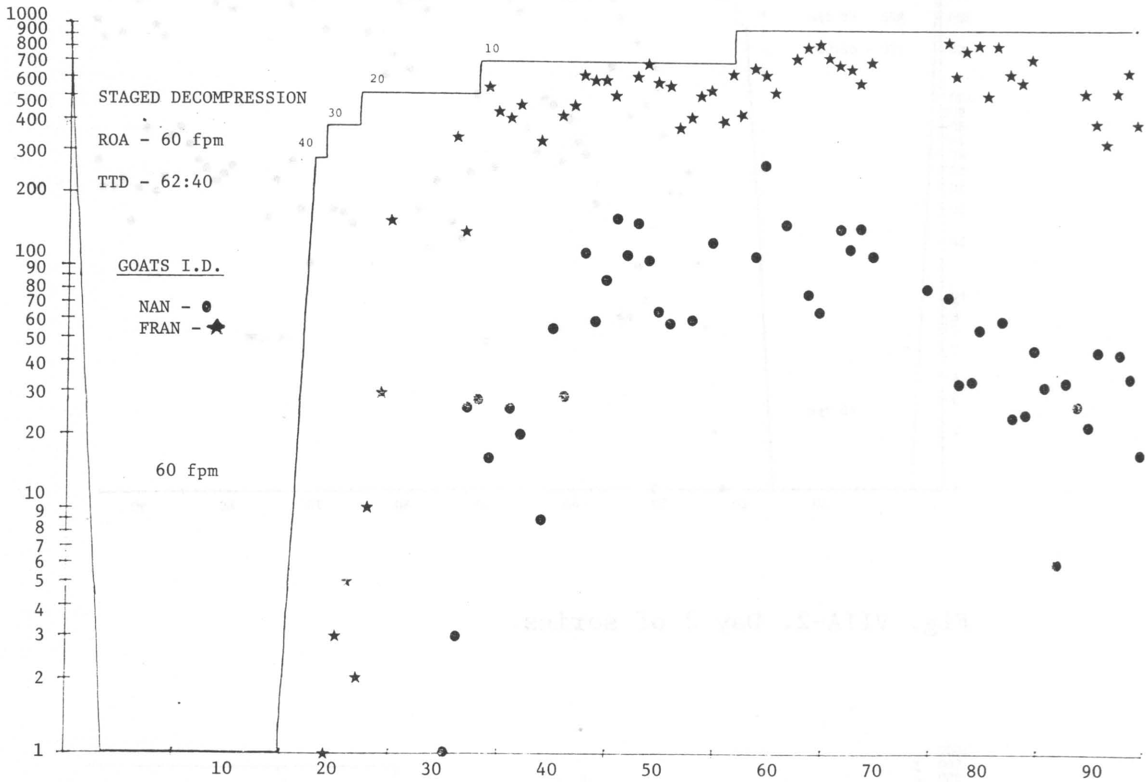


Fig. VIIA-1. Bubble counts on 2 goats resulting from dive using USN Exceptional Exposure Table for 220 ft/20 min (solid line). Ordinate shows bubble counts, abscissa time in minutes. Day 1 of series.

Figures VIIA-2 through 5 represent 8 successive dives with these two goats. Day 2 (Fig. VIIA-2) looks about like the first day. You can see that on day 3 (Fig. VIIA-3) the goats are manifesting later bubble formation. Figure VIIA-4 shows the fifth day and dive 6 and 7 were practically the same. On the eighth-day (Fig. VIIA-5) dive, one animal was bent, the one represented by the dots. After 83 minutes total time, after having been on the surface approximately 20 minutes, we recompressed and the count decreased to near zero. More experiments will have to be done to determine if there is really an effect of day-to-day diving. Our results may explain why there is a divergence of opinion about this matter.

Next we wanted to determine, in addition to the effect of consecutive day compressions, whether there was an effect of rate of ascent. In particular, we were interested in the hypothesis that the initial rate of ascent and first jump is too fast and too far, and consequently

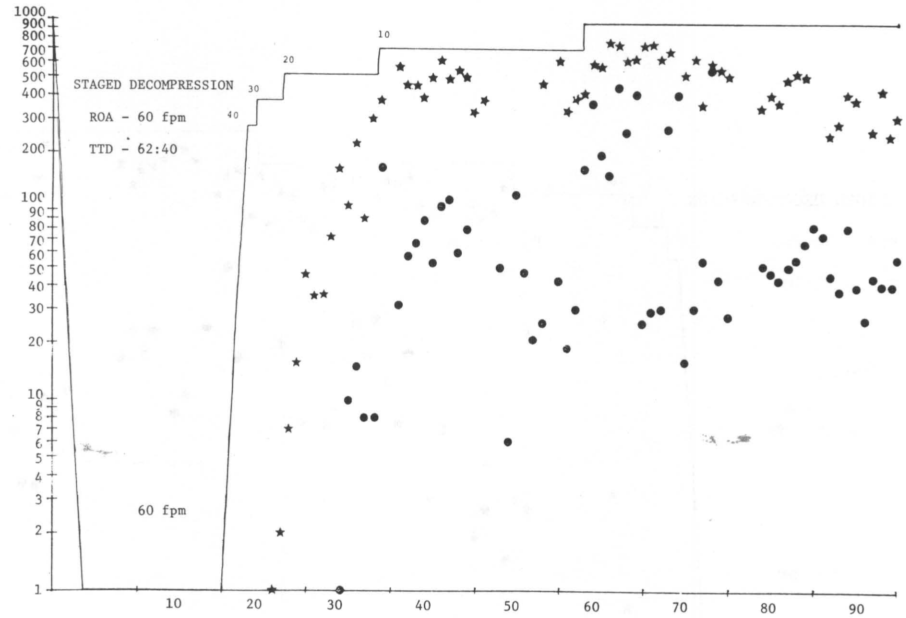


Fig. VIIA-2. Day 2 of series.

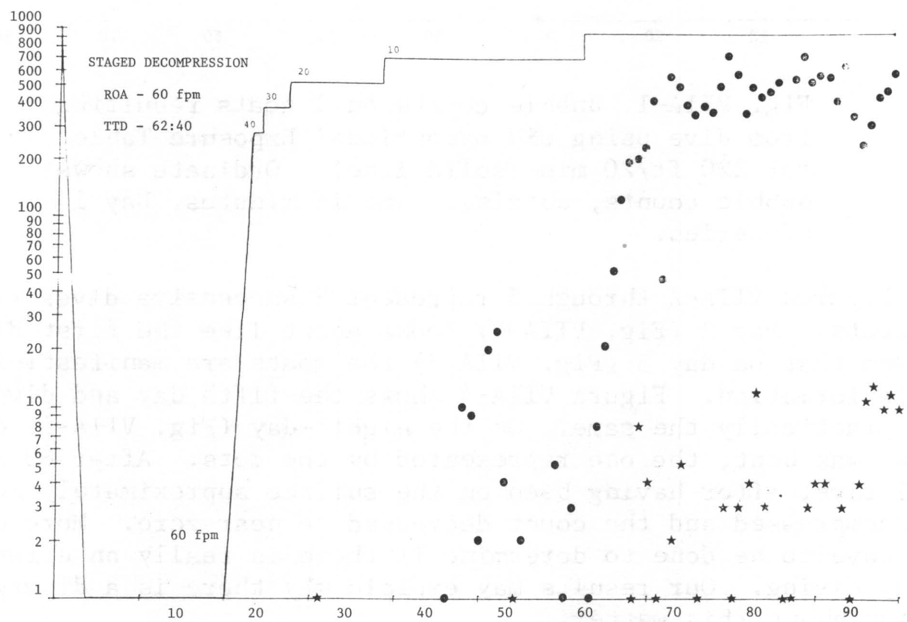


Fig. VIIA-3. Day 3 of series. Note that bubbles appear later.

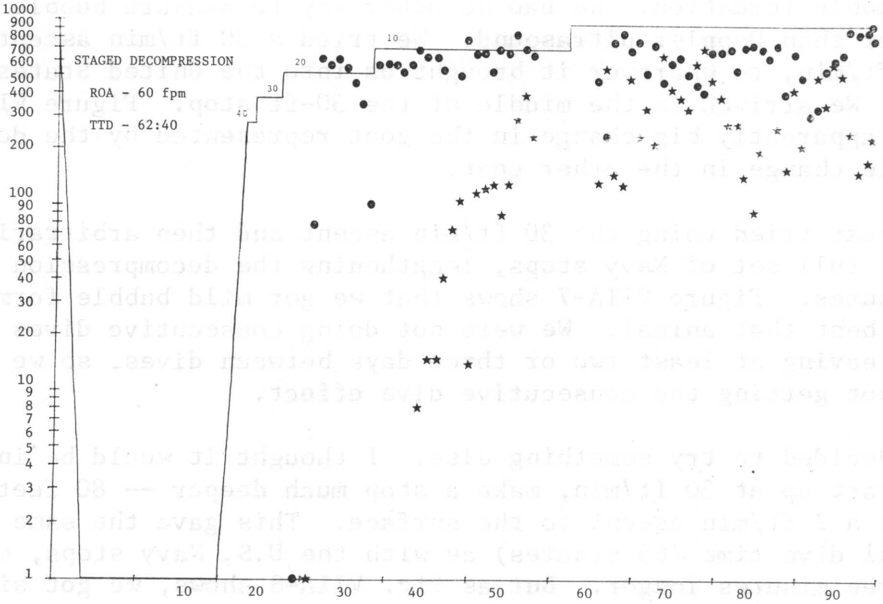


Fig. VIIA-4. Day 5 of series.

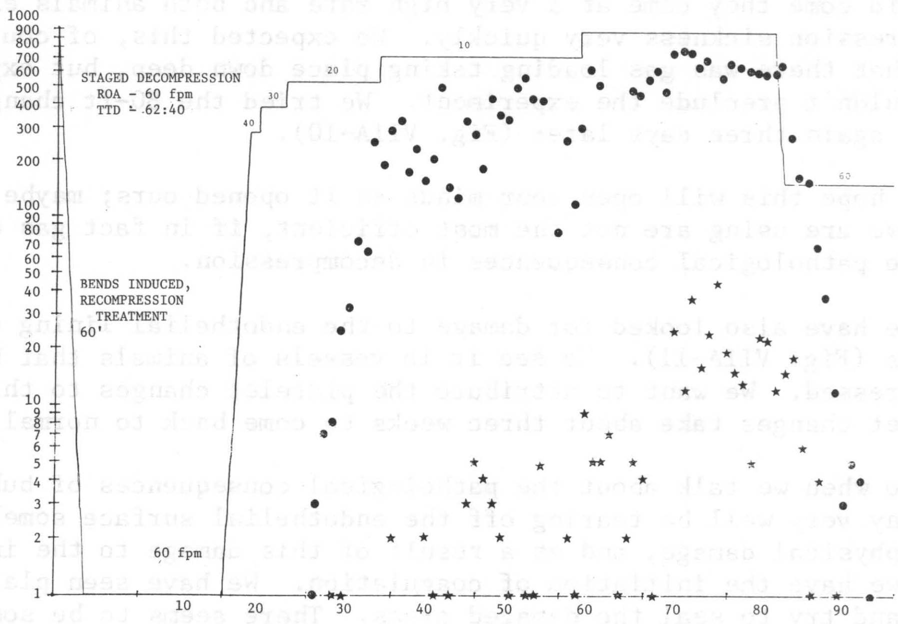


Fig. VIIA-5. Day 8 of series. Goat represented by dots showed bends; both animals were recompressed. Note disappearance of bubbles following recompression.

causes bubble formation. We had no other way to measure bubble formation other than Doppler ultrasound. We tried a 30 ft/min ascent rather than 60 ft/min, to wherever it brought us into the United States Navy profile. We arrived in the middle of the 30-ft stop. Figure VIIA-6 shows an apparently big change in the goat represented by the dots and a possible change in the other goat.

We next tried using the 30 ft/min ascent and then arbitrarily using the full set of Navy stops, lengthening the decompression by some three minutes. Figure VIIA-7 shows that we got mild bubble formation and even bent that animal. We were not doing consecutive dives then. We were leaving at least two or three days between dives, so we feel we were not getting the consecutive dive effect.

We decided to try something else. I thought it would be interesting to start up at 30 ft/min, make a stop much deeper -- 80 feet -- and then make a 2 ft/min ascent to the surface. This gave the same approximate total dive time (65 minutes) as with the U.S. Navy stops, or only about three minutes longer. But as Fig. VIIA-8 shows, we got significantly fewer bubbles.

Then we tried a single ascent rate all the way to the surface, forgetting any model and going straight to the surface. It turned out to be 5.165 ft/min. Figure VIIA-9 shows we didn't get any bubbles until several minutes after we arrived on the surface, and then when they did come they came at a very high rate and both animals experienced decompression sickness very quickly. We expected this, of course, we knew that there was gas loading taking place down deep, but expecting it shouldn't preclude the experiment. We tried the 80-ft change of ascent again three days later (Fig. VIIA-10).

I hope this will open your minds as it opened ours; maybe the rates we are using are not the most efficient, if in fact gas emboli do have pathological consequences in decompression.

We have also looked for damage to the endothelial lining of blood vessels (Fig. VIIA-11). We see it in vessels of animals that have been decompressed. We want to attribute the platelet changes to this. Platelet changes take about three weeks to come back to normal.

So when we talk about the pathological consequences of bubbles, they may very well be tearing off the endothelial surface somehow, doing physical damage, and as a result of this damage to the intimal layer we have the initiation of coagulation. We have seen platelets go in and try to seal the damaged areas. There seems to be some initial proliferation following the laying of platelets on the intimal part of the vessel, and these cells and platelets, if the damage is severe enough, can perhaps occlude the entire vessel and cause ischemia.

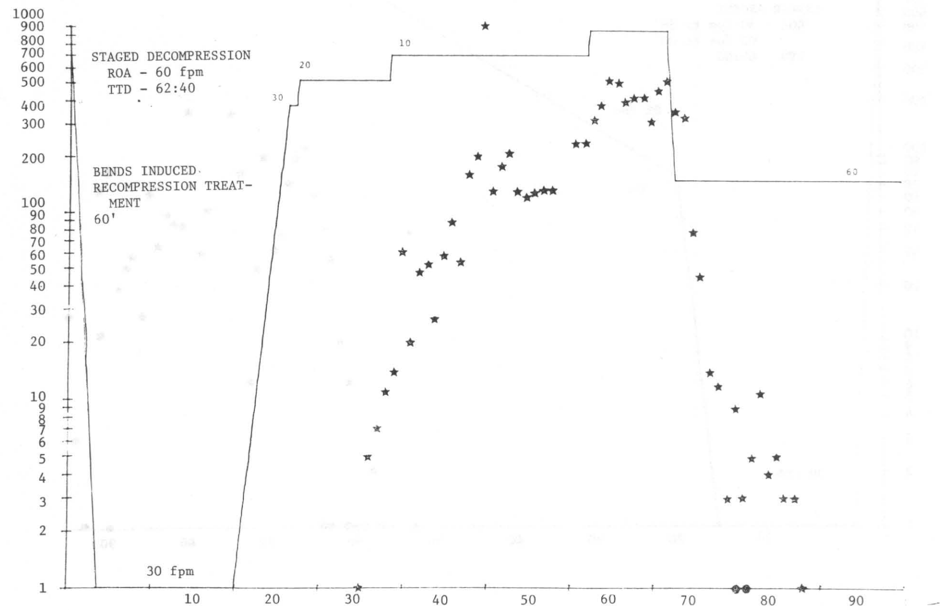


Fig. VIIA-6. Same exposure as previous figures, after a few days without diving. Rate of ascent was 30 ft/min until profile intersected USN table at 30-ft stop.

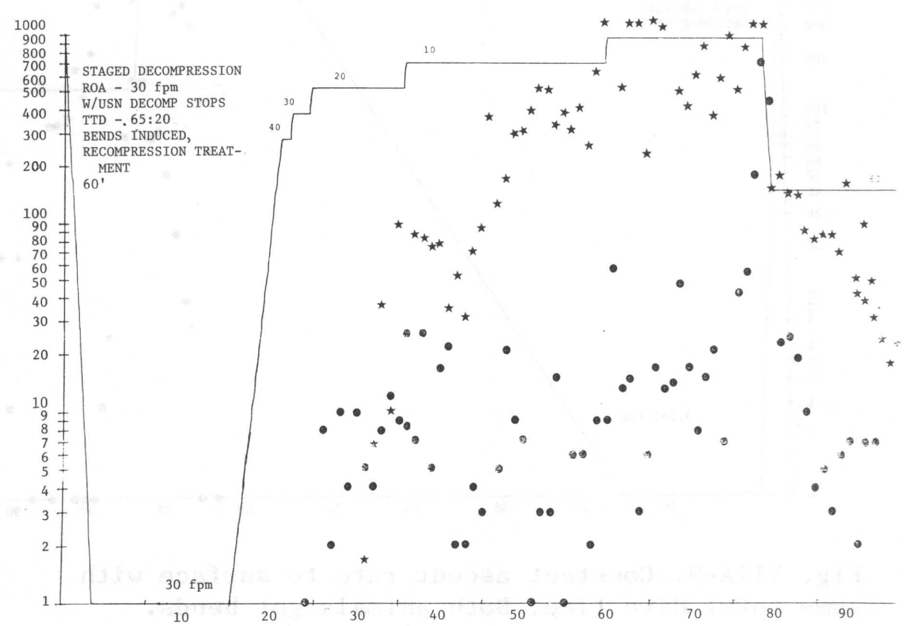


Fig. VIIA-7. Profile using 30 ft/min ascent rate combined with normal USN stops.

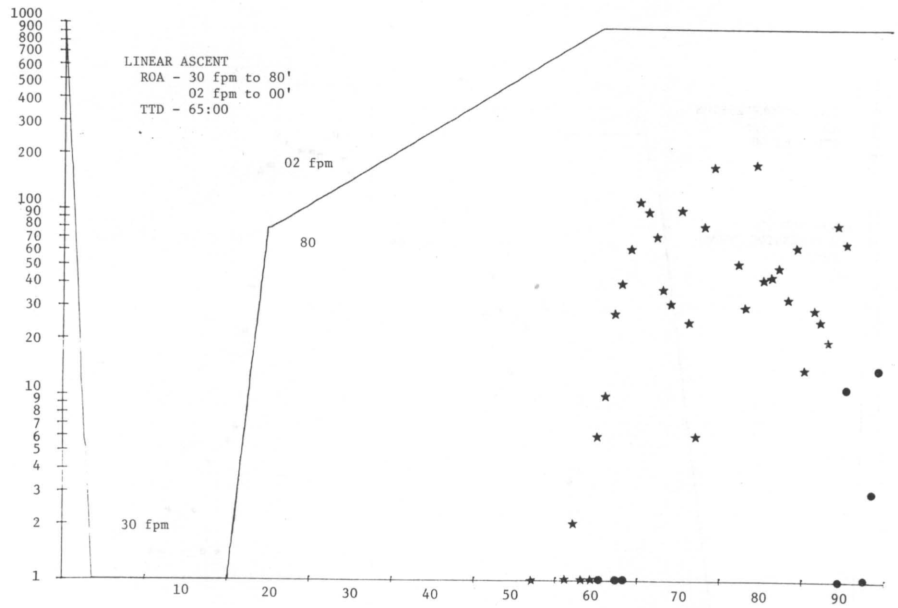


Fig. VIIA-8. Use of 30 ft/min ascent to 80 ft, with 2 ft/min ascent to surface; this profile required same total dive time, but resulted in fewer bubbles.

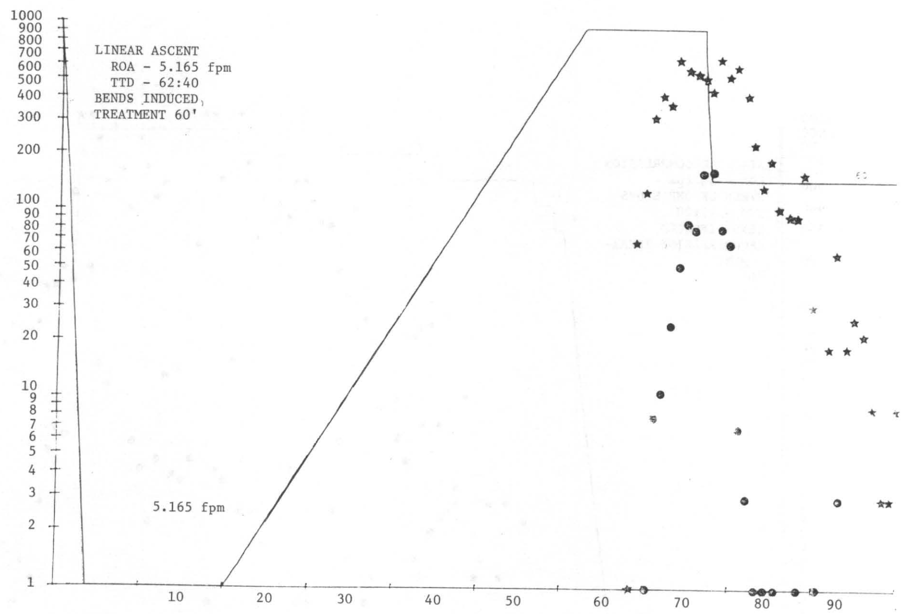


Fig. VIIA-9. Constant ascent rate to surface with same total dive time. Both animals got bends.

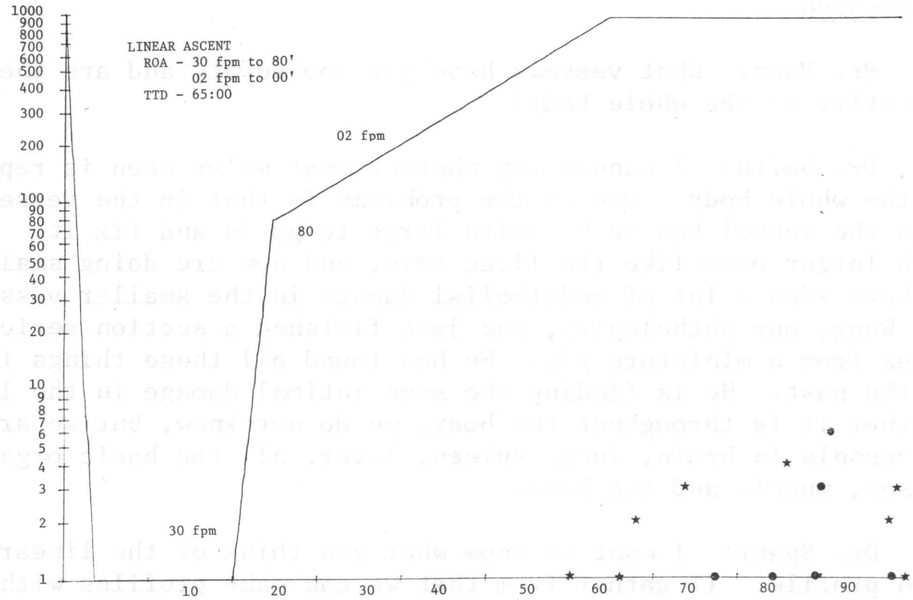


Fig. VIIA-10. Same profile as Fig. VIIA-8, repeated 3 days after exposure depicted in Fig. VIIA-9.

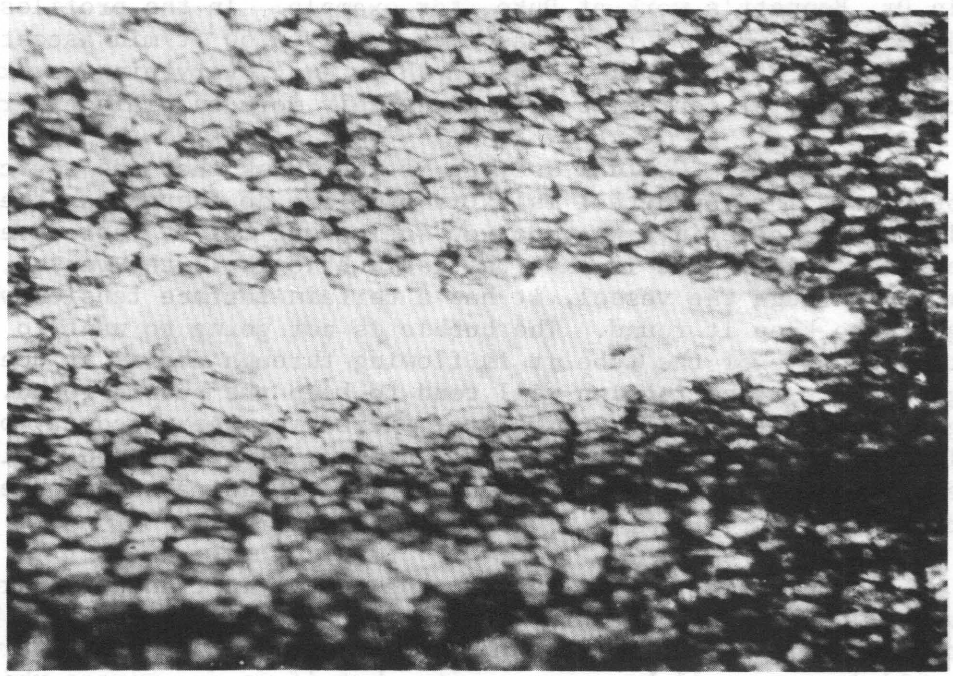

Fig. VIIA-11. Portion of vascular endothelium of pig subjected to decompression, showing damage presumed to be caused by intravascular bubbles.

Discussion

Mr. Vann: What vessels have you looked at, and are they representative of the whole body?

Dr. Smith: I cannot say whether what we've seen is representative of the whole body. One of the problems is that in the vessel preparation the vessel has to be quite large to go in and fix it. We started with larger ones like the iliac vein, and now are doing smaller vessels; we have seen a lot of endothelial damage in the smaller vessels. Dr. Wong, our pathologist, has just finished a section series on the lungs from a miniature pig. We had found all these things in the bone in the past. He is finding the same intimal damage in the lungs, also. Whether it is throughout the body, we do not know, but we are looking at vessels in brain, lung, spleen, liver, all the basic organs, heart, tissue, muscle and the bone.

Dr. Spaur: I want to know what you think of the linear decompression profile. (I gather from that we can make profiles with a ruler instead of a French curve.) Would you enlarge on what you think this might do for us in the end?

Dr. Smith: I think that Brian Hills may have been right for a long time. We are causing bubbles by the rapid and quantitative initial ascent. By reducing the initial rate and amount of the initial jump we can have a much more satisfactory profile. This is being proven in Dr. Bennett's work at Duke, for example. In the profiles we run we just cannot make 60 ft/min ascents, or even 50 ft/min ascents without problems. Maybe we will have to throw away everything we "know", not completely, of course, but we might rely more on plain experience.

Dr. Hills: Could I ask you if you think that the source of the bubbles which you detect is extravascular; do they break the endothelial lining and come in? What do you feel about this? There are several possibilities. One is that the gas bubble is in the vessel, and as it moves through the vessel, it has a certain surface tension which is going to keep it round. The bubble is not going to want to conform to the shape of the tube it is flowing through and as it does try to grow the surface tension will tend to keep the bubble round. As it goes along it collects platelets, increasing the viscosity of the blood. This restricts the flow or slows down the flow, and you could very easily get hypoxia in the nearby surfaces, and cause endothelial damage.

Another alternative is that the gas is in transport through the vessel wall, by diapedesis. Particulate matter is engulfed by an endothelial cell and is carried through the cell in an active process. It could just as well be a gas cavity, but if we decompress while that gas cavity is being carried, it certainly will grow and rupture the endothelial cell.

Mr. Hughes: Do you think that bubbles in the very early stages of ascent are necessarily the inert gases involved in the diver's breathing mix?

Dr. Smith: By all means. A thought that has been around for some time is that $CO_2$, because of its rapid equilibration, is very consequential in initial nucleation; that could be a factor, but inert gas moves in quickly.

Dr. Flynn: What kind of decompression sickness occurred in those animals that were affected?

Dr. Smith: Pain.

Dr. Flynn: The slides show that the lungs were receiving a tremendous number of emboli. How do we go from that to the development of limb pain? Are the emboli getting through? Why don't we then take our Doppler and move it to the other side of the circulation and look at what is coming out of the heart?

Dr. Smith: That has been done with sheep. Bubbles are rarely detected downstream of the lung, only when there are tremendous showers of bubbles on the right side and then only a few are seen. It has been demonstrated that the growth time for a bubble is quite rapid, from, say, 10 to 50 microns; a matter of microseconds. By the time two or three seconds are passed all bubbles have probably reached 50 microns. They are growing quite rapidly. As far as the relationship of these venous bubbles to limb bends, I think it is indirect, and not predictive in any way, except that when we do see limb bends we very frequently have a large number of bubbles in the circulation. There is not necessarily any connection whatsoever.

Dr. Flynn: Are you saying then that the development of limb bends and pulmonary embolization are parallel processes and not necessarily interrelated to one another? In other words, the pulmonary embolization is not a causative factor in limb bends, but both of these are proceeding along simultaneously and independently?

Dr. Smith: I would say that is probably right.

Dr. Greene: First, I would like to know if you could give us some comparative information on shallow and deep, short-duration helium diving, and also have you ever observed a bubble during saturation decompression in either animals or humans? Because helium is so much less soluble than nitrogen, we have wondered if you'd see the same sort of bubble formation.

Dr. Smith: Recently at Duke University they were running some NOAA OPS trials, basically to study saturation-excusion ascents on air.

There were bubbles picked up in the saturation ascent. This was in the same individual who manifested bubble formation on some of the excursions, also.

Dr. Bennett: There were no bends in the individuals with bubbles.

Dr. Smith: On deep helium diving, we certainly do have bubble formation. On a 500-ft dive, for instance, we would start getting bubbles, at 280 feet or so. We have done no studies with shallow helium dives.

Dr. Buehlmann: Your slides are a wonderful confirmation for our calculation method. An ascent rate of only 10 minutes per ATA, but not slower, is good. If you have too slow a rate of ascent you will increase the rate of bends. And, exceptional dives of the U.S. Navy -- 200 feet, 2 hours -- are not good.

Capt. Bornmann: We agree with Dr. Buehlmann's comment. As a matter of fact, in 1957 when these tables were published, we said the same thing.

Dr. Bennett: I would endorse what Dr. Buehlmann says. Certainly the Duke experience is the same. When we use the Haldane-type computation method, we do in fact have a correlation between bubble sounds and bends, but only with the Haldane method. With the nil supersaturation-diffusion method we have not been able to detect bubbles with the precordial transducer. We can get showers of bubbles when we squeeze the leg muscle, for example. I do not think we have enough information to ascertain what the bubbles really mean.

Dr. Hempleman: Many years ago, Dr. Behnke put forward the silent bubble theory. Many experiments have been done since then that show that silent bubbles really exist.

Professor Walder has put Dopplers on saturation divers at RNPL for some three or four years. All we have succeeded in doing in saturation diving is to show you can make silent bubbles unsilent with the Doppler. We got bubbles, and sometimes there was a bend and we got bubbles and there wouldn't be any discomfort at all, and nothing at all on the instrument and a bend, and so forth. We got all possible permutations and combinations. From the point of view of the normally accepted end point, it became sort of a pointless theory. I am not intending to destroy your technique at all, I am just saying that as we had it, it was meaningless.

Dr. Behnke: I would like to cite a reference that is relevant to this discussion, the work by Catchpole and Gersh, from Fulton's book on

Decompression Sickness.* It may clear up a lot of the questions that have been raised, and I'd like to issue a plan to this group. Regardless of how they get there, bubbles seem to be the problem. More studies should be made to observe where the bubbles are. Experiments have been done in which circulation to the hand has been cut off and the hand has been placed in a vacuum and decompressed to a few millimeters of mercury, which greatly distended the hand, but did not cause pain. One can demonstrate bubbles in fat with a pressure cup. We have reason to believe that the bubbles which cause symptoms and the bubbles which cause pain are intravascular. Let's find out where the bubbles are that cause the problems.

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* Gersh, I. and H. Catchpole. Decompression sickness: physical factors and pathologic consequences. In: Fulton, J. F., ed. Decompression sickness. Caisson sickness, diver's and flier's bends and related syndromes, p. 165-181. Philadelphia, W.B. Saunders Co., 1951.

## B. USE OF OTHER GASES: HYDROGEN AND NEON: P. O. EDEL

Let us begin by pointing out that research with neon has been covered in the Workshop by Drs. Hamilton and Smith and Mr. Kenyon. This paper is concerned with the use of hydrogen and other gases.* Since hydrogen is the lightest of all gases, we might expect it to offer the lowest breathing resistance in a laminar flow system. This should promote more rapid diffusion of $O_2$ and $CO_2$ within the gas exchange units of the lungs at depth. On the basis of the solubility and diffusion coefficients one would expect the uptake and elimination rates in bodily tissues to be more rapid for hydrogen than nitrogen, and possibly even more rapid than helium.

We've had helium and nitrogen around for a long time, so that one is a little nervous introducing a new gas with which we have no experience. On the other hand, if we can have another gas with different perfusion and diffusion constants, we might obtain data to add to the theories that we have.

Despite the potential advantages of hydrogen-oxygen as a breathing mixture for deep diving exposures, investigators were discouraged from experimenting with hydrox because of the explosive and flammable characteristics of this mixture for a broad range of hydrogen/oxygen ratios. However, work done by the Bureau of Mines (3) and by Dorr and Schreiner (4) has demonstrated that oxygen percentages as high as 3 or 4 percent in hydrogen are completely safe with respect to explosive and flammability hazards. A mixture of 3% oxygen - 97% hydrogen could be utilitized as shallow as 200 fsw (7 ATA abs) where the oxygen partial pressure in the mixture at that depth would be equal to the oxygen partial pressure in air at sea level. Brauer's work (2) where mice were exposed to mixtures of oxygen with nitrogen, helium, and hydrogen at elevated pressures indicated that the narcotic potency of hydrogen was less than that of nitrogen but greater than that for helium. However, convulsive seizures occurred at significantly higher pressures with hydrogen as opposed to helium.

To begin with, we might adopt a perfusion-limited model and use what we know about nitrogen to deal with hydrogen, or we might adopt a diffusion-limited concept and model hydrogen after helium. If we take the solubility coefficient we'll wind up with a limiting half time coefficient somewhere between nitrogen and helium. This should give us an indication of which of the models more nearly represents what's going on in the body.

In the early 1940's, Zetterstrom designed a cracking plant for shipboard use to produce a mixture of 72% hydrogen - 24% nitrogen -

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* The works referenced in 6, 7, and 9 at the end of this paper contain additional information on uses of neon.

4% oxygen which was used in a series of dives to a maximum depth of 360 fsw. Mixtures of 96% hydrogen - 4% oxygen were used for deeper open-sea tests to a maximum depth of 520 feet. The final dive of this series resulted in Zetterstrom's death, caused by inattentiveness of a crew member which permitted the diving stage to be raised to the surface from the maximum depth of 520 fsw without halting for the required decompression stops (1, 10).

To pursue hydrogen experimentation, we had to develop a system for injecting oxygen directly into hydrogen cylinders. The system is initially purged and pressurized with helium, after which the oxygen and hydrogen cylinder and manifold valves are opened. The helium in the lines is forced into the hydrogen cylinder by oxygen which then enters the hydrogen cylinder. The oxygen stream is allowed to continue until a differential pressure transducer (referenced at a pressure identical to that initially in the hydrogen cylinder) indicates the desired pressure increase. The hydrogen and oxygen cylinder valves are then closed and the piping system is purged again with helium.

In 1967, in Hydrox I, two successful manned exposures were made to a depth of 200 fsw for 10 and 20 minutes in a single-lock chamber 33" in diameter, 8 feet in length. The small size of the chamber permitted very rapid changes in the chamber atmosphere and provided data on atmospheric control in later experiments. In these tests the chamber which initially contained air was pressurized with pure nitrogen to provide an atmosphere of 97% nitrogen - 3% oxygen which would be safe in the event there was any accidental contamination from the hydrogen system. The hydrogen system was completely independent from the main breathing mixture supply and included an overboard dump terminating in a flashback arrestor outside the building.

Pressurization to 200 fsw required two minutes. The diver breathed 80% helium - 20% oxygen during compression to 100 fsw. At that point, he shifted to 97% helium - 3% oxygen and upon arrival at 200 fsw switched to the separate hydrogen-oxygen system. In this exposure, the diver lifted a 40-lb weight 1½ feet 200 times during the period spent at 200 fsw. At the end of the exposure period at 200 fsw, the chamber was rapidly purged, first with pure nitrogen and then with air to provide an emergency breathing medium during decompression. In this dive as in the subsequent exposure to the same depth for 20 minutes, the diver was switched to a 96% helium - 4% oxygen mixture as the ascent commenced. After one minute the diver was then shifted to an 80% helium - 20% oxygen mixture. This mixture was used during decompression until arrival at the 60-fsw stop, where the diver was shifted to pure oxygen for the remainder of the decompression. During decompression, mild bends occurred in the left elbow at the 40-fsw level. The pain diminished rapidly after arrival at the 40-ft level and decompression to the surface continued as programmed. Pain recurred during pressure reduction and diminished rapidly during the time spent at each decompression stop.

Project Hydrox II was initiated in 1974 at Michel Lecler, Inc. in Harvey, Louisiana, with funds provided by the Office of Naval Research and Bureau of Medicine and Surgery. In addition, a scientific team from the Naval Submarine Medical Research Laboratory participated in the program, to increase the spectrum of data developed in this twenty-four manned-dive series. Four test subjects were involved in the test; each subject was exposed for a 2-hr period at 200 feet for a total of 6 times during the experimental series, twice on each of three breathing mixtures (97% nitrogen - 3% oxygen, 97% helium - 3% oxygen, 97% hydrogen - 3% oxygen).

The basic operation (with respect to chamber atmospheric control and use of breathing mixtures) was the same in this series as in Project Hydrox I. The pressure profiles were made as identical as possible (except for some changes in the time distribution in the decompression stops required by the differences in decompression obligations resulting from the three inert gases in question), to provide a basis of comparison between the inerts and derive ratios of tissue half-saturation time for hydrogen as compared with helium and nitrogen. Profiles for nitrogen, helium, and hydrogen (oxygen mixtures) are shown in Figs. VIIB-1-3.

Studies made in connection with Hydrox II included performance measurements, biochemical surveillance, respiratory gas analysis (with a mass spectrometer), pulmonary function, speech, and bubble monitoring with the Spencer Doppler flowmeter.

Performance measurements showed substantial impairment with nitrogen-oxygen mixtures -- especially when only 3% oxygen was being used -- but no changes from control during the helium-oxygen and hydrogen-oxygen exposures. Biochemical observations showed no contraindication to the use of hydrogen-oxygen mixtures within the scope of the exposure studied. In both programs the speech studies showed no apparent differences between speech in hydrogen-oxygen as compared with helium-oxygen mixtures. It appears that an unscrambler which would work well for helium-oxygen would work as well for hydrogen-oxygen, if the oxygen percentage was the same in both mixtures.

The studies with the mass spectrometer showed no inert gas "bursts or spikes" like those which had been previously observed by Schaefer and Dougherty (8). Pulmonary function studies with hydrogen showed that ventilatory ability at 200 fsw was improved 32% compared with helium and 152% compared with nitrogen. The results of direct on-site monitoring, later review of the tape recording, or subsequent spectrographic analysis indicated that no bubbles were detected with the experimental Doppler flowmeter unit positioned over the pre cordial area, during any of the hydrox, heliox, or nitrox dives, although the overall bends incidence during the program was 50%. This might suggest that bubble formation may have been extravascular and hence, beyond the ability of the instrument to detect.

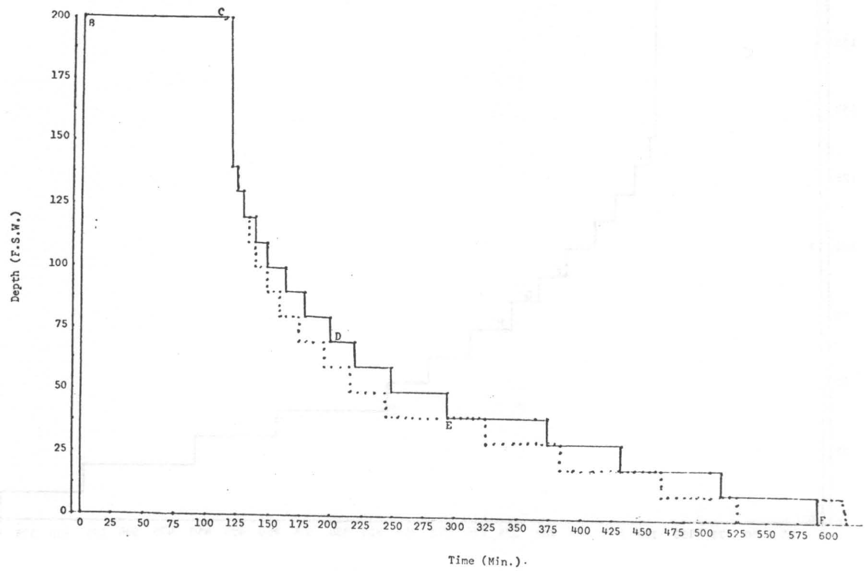


Fig. VIIB-1. Nitrogen-oxygen decompression schedule. A-B: Air; B-C: 97% $N_2$/3% $O_2$: C-D: Air; D-E: $N_2/O_2$; E-F: $O_2$, with air breaks. Table 1 = dotted lines; Table 2 = solid lines.

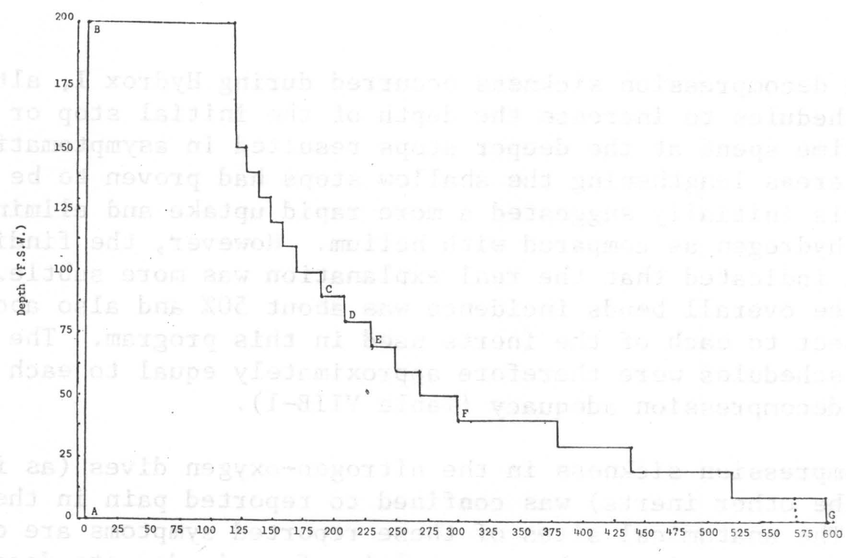


Fig. VIIB-2. Helium-oxygen decompression schedule. A-B: 80% He/20% $O_2$; B-C: 97% He/3% $O_2$; C-D: $N_2$/He/$O_2$; D-E: Air; E-F: $N_2/O_2$; F-G: $O_2$, with air breaks. Table 1 = solid lines; Table 2 = dotted lines.

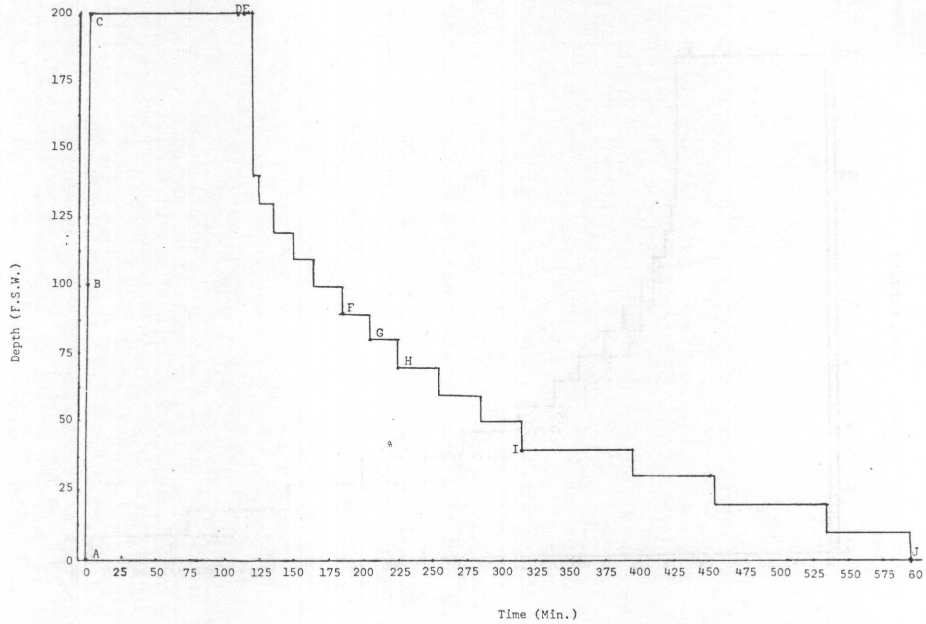


Fig. VIIB-3. Hydrogen-oxygen decompression schedule. A-B: 80% He/20% $O_2$; B-C: 97% He/3% $O_2$; C-D: 97% H/3% $O_2$; D-E: 97% He/3% $O_2$; E-F: 80% He/20% $O_2$; F-G: He/$N_2$/$O_2$; G-H: Air; H-I: 65% $N_2$/35% $O_2$; I-J: $O_2$, with air breaks.

When decompression sickness occurred during Hydrox I, alterations of the schedules to increase the depth of the initial stop or increased time spent at the deeper stops resulted in asymptomatic exposures, whereas lengthening the shallow stops had proven to be ineffective. This initially suggested a more rapid uptake and elimination time for hydrogen as compared with helium. However, the findings of Hydrox II indicated that the real explanation was more subtle. In the program the overall bends incidence was about 50% and also about 50% with respect to each of the inerts used in this program. The decompression schedules were therefore approximately equal to each other in terms of decompression adequacy (Table VIIB-1).

Decompression sickness in the nitrogen-oxygen dives (as in the case of the other inerts) was confined to reported pain in the knee joints. The anatomical sites of these reported symptoms are characteristic of decompression sickness resulting from inadequate decompression in the slowest tissues. The sole exception in the entire program occurred in the initial dive of this series (Schedule 1) where a case of elbow bends occurred at 40 fsw. Decompression in subsequent exposures was altered to provide additional time at the deeper stops, which was successful in the elimination of symptoms at this site. Calculations showed that tissue half times of 360 to 480 minutes would control

Table VIIB-1. Decompression Sickness: Project Hydrox II

| Mixture | Decompression Schedules | Total Decomp. Time, min | Subject | Bends? | Treatment Needed? |
|---|---|---|---|---|---|
| $N_2$-$O_2$ | I | 407 | PG* | YES | NO |
| " " | II | 472 | PG* | YES | NO |
| " " | II | 472 | PM | NO | NO |
| " " | II | 472 | PM | NO | NO |
| " " | II | 472 | ME** | NO | NO |
| " " | II | 472 | ME | YES | NO |
| " " | II | 472 | SA | YES | YES |
| " " | III | 493 | SA | YES | YES |
| $H_2$-$O_2$ | I | 477 | PG | NO | NO |
| " " | I | 477 | PG | YES | YES |
| " " | I | 477 | PM | NO | NO |
| " " | I | 477 | PM | NO | NO |
| " " | I | 477 | ME | NO | NO |
| " " | I | 477 | ME | YES | YES |
| " " | I | 477 | SA | YES | YES |
| " " | I | 477 | SA | YES | YES |
| He-$O_2$ | I | 477 | PG | NO | NO |
| " " | I | 477 | PM | NO | NO |
| " " | I | 477 | ME | NO | NO |
| " " | I | 477 | SA | NO | NO |
| " " | II | 452 | PG | YES | NO |
| " " | II | 452 | PM | NO | NO |
| " " | II | 452 | ME | YES | NO |
| " " | II | 452 | SA | YES | NO |

* Symptoms of dizziness and nausea at 200 fsw; ** Symptoms of dizziness, nausea and vomiting at 200 fsw.

decompression on the final ascent to surface pressure where the schedules were designed to promote bubble formation.

As in the previous case, the helium-oxygen schedules were overly conservative in terms of decompression adequacy, with the exception of the final ascent to surface pressure. When using Schedule 1, the bends incidence was zero. In the second schedule, the time at the 10-fsw stop was reduced by 25 minutes, which resulted in bends in 3 out of 4 dives. A 50% bends incidence could therefore result from a decompression schedule with a 10-fsw stop time somewhere between the two schedules used. The analysis of the decompression schedules indicated that a half-saturation time for helium of 240 minutes would control decompression on the final ascent to surface pressure (in combination with a nitrogen half-saturation time of 480 minutes) and a surfacing value of 50 fsw in Schedule 2 (Table VIIB-2). This gave a bends incidence of 50%.

Table VIIB-2. $N_2$+He Surfacing Tissue Tensions from $H_2$-$O_2$ Dives

| | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| $N_2$, half-time values | 300 | 300 | 300 | 360 | 360 | 360 | 480 | 480 | 480 | 720 | 720 | 720 |
| He, half-time values | 150 | 180 | 240 | 150 | 180 | 240 | 150 | 180 | 240 | 150 | 180 | 240 |
| $P_{N_2}$, Schedule 1 | 20.1 | 20.1 | 20.1 | 22.5 | 22.5 | 22.5 | 22.9 | 22.9 | 22.9 | 22.8 | 22.8 | 22.8 |
| $P_{He}$, Schedule 1 | 16.6 | 20.3 | 24.7 | 16.6 | 20.3 | 24.7 | 16.6 | 20.3 | 24.7 | 16.6 | 20.3 | 24.7 |
| P $N_2$+He | 36.7 | 40.4 | 44.8 | 39.1 | 42.8 | 47.2 | 39.5 | 43.2 | 47.6 | 39.4 | 43.1 | 47.5 |
| $P_{N_2}$, Schedule 2 | 21.3 | 21.3 | 21.3 | 21.8 | 21.8 | 21.8 | 23.4 | 23.4 | 23.4 | 23.1 | 23.1 | 23.1 |
| $P_{He}$, Schedule 2 | 18.7 | 22.4 | 26.6 | 18.7 | 22.4 | 26.6 | 18.7 | 22.4 | 26.6 | 18.7 | 22.4 | 26.6 |
| $PN_2$+He | 40.0 | 43.7 | 47.9 | 40.5 | 44.2 | 48.4 | 42.1 | 45.8 | 50.0 | 41.8 | 45.5 | 49.7 |

Calculated using hypothetical tissue half-saturation times of 300, 360, 480, and 720 minutes for $N_2$, and 150, 180, and 240 minutes for He.

In the hydrogen-oxygen dive series, only one schedule was used which resulted in a 50% bends incidence. Analysis of the hydrogen surfacing values was complicated by the use of other inerts in the breathing mixture during decompression. However, it appeared clear that the slowest half-time tissue value for hydrogen was greater than that for helium and less than that for nitrogen. This value appears to be between 300 and 360 minutes, with a surfacing M value slightly less than that for helium and considerably more than that for nitrogen. Or, it might be stated that it is clear that a diffusion-limited model is not controlling here.

We believe the difficulty in providing for the initial stages of the decompression profiles in the earlier experiments was due to the use of helium-oxygen breathing mixtures at the initial decompression stages. Under these conditions, the hydrogen was being eliminated at a comparatively slow rate while the helium was being introduced into the bodily tissues at a much more rapid rate. The resultant overall decrease in the sum of the tissue tension values was slower than provided for by the calculations, increasing the overall time requirements for the deeper stops. We anticipate that the overall decompression time requirements for hydrogen-oxygen following saturation exposures would be increased, as compared with helium, by approximately 1/3.

A great deal of further research on hydrogen-oxygen mixtures is urgently needed. However, on the basis of our knowledge today, there appears to be no drawback to the use of hydrogen-oxygen in the 400 - 1000 ft range. On the positive side, a great many practical and physiological advantages would result from the use of this mixture. The reduction in breathing resistance would increase the comfort and working ability of the diver using this mixture. Hydrogen is the only suitable inert gas for deep diving that is available without limit; it can be taken from the water in which it is used. Considering the probability that our supply of helium will be depleted in the next quarter of a century, hydrogen may be invaluable and perhaps even essential in assuring continuity of man's exploration and exploitation of the hydrosphere.

Discussion

Dr. Schreiner: I've always wondered about hydrogen because its diffusion properties are so close to those of helium and yet its molecular properties -- it's a reasonably polarizable gas -- ought to make it more narcotic than helium, and its fat/water solubility ratio is almost identical with that of nitrogen and argon. Hydrogen is really a gas that, from the molecular point of view, straddles the fence. It does not surprise me that it acts with an apparently larger half time than you would ascribe to it if you were to think of it as being like helium, because it partitions between fat and water just like nitrogen -- if perfusion is limiting its transport outward, then it would come out at a much slower rate than helium, more like nitrogen. This will help us all begin to realize now that both perfusion and diffusion are involved in gas transport mathematics. Hydrogen clearly has a mission to play from the research point of view, because of its unusual properties. It may well be the rosetta stone of translating these two phenomena into a homogeneous and coherent model or or description of inert gas transport.

Dr. Hills: You bring up a very good point. It seems as if hydrogen is in between diffusion and perfusion. This is going to increase the mathematical complexity over what it would be if it were one or the other.

Mr. Edel: Yes, I compared it to a pure diffusion model and it obviously didn't fit there; then I compared it with a pure perfusion model and it didn't fit there either. It seemed to be lying right about the center of the two, as near as I could tell.

Dr. Hempleman: Hydrogen, of course, is not an inert gas in the sense that the others are. It might be ionizing, for example, or acting in a hydrogen electrode system. I think it might be difficult to be sure what you are doing with hydrogen.

Mr. Edel: Well, this was considered and we tried to take a broad-spectrum look at the biochemical side, but up to now we have been able to detect no changes at all. We did take pH measurements, because of the French experiments. We couldn't make these under pressure. They showed no changes at all. In the French experiments the animals did die at pressure. We used hydrogen with dogs, which were saturated at 1000 feet. They had a 39-hr time on bottom at 1000 feet, plus a considerable number of days afterwards during decompression, and they had no ill effects at all. This exposure was in excess of some of the French experiments, where the animals died. The explanation would appear to lie elsewhere, rather than with hydrogen.

Dr. Hempleman: I'm just saying in regards to the kinetics of it, it's well know that explanations of narcosis or HPNS depend on membrane phenomena, so if you have such things as membrane phenomena with nitrogen, it's 10 to 1 they're going to come off with hydrogen! Also, it has possibilities of taking part in an actual metabolic-type exchange and this might render your kinetics a little bit more difficult than you think.

Mr. Wilson: Have you ever had an explosion?

Mr. Edel: We had one mixing explosion. It was at that time when I managed to persuade a gas company to take over the mixing procedures.

Dr. Schreiner: There are obvious dangers with hydrogen, but I would say that when we launch rockets to the moon that contain several thousand tons of liquid hydrogen, the technology is here to handle it safely.

Mr. Keyon: Our laboratory went through an equivalent exercise in arriving at half times for neon mixtures. We based it on some of Dr. Powell's pig experiments. We began with a couple of assumptions. First, that each inert gas acts independently during uptake and elimination but that they are summed together to form a total inert gas partial pressure. Regardless of whether it is a nil supersaturation or a diffusion model, all the gases do the same thing when they are combined. Second, similar symptoms exhibited by different subjects should appear with equal total inert gas partial pressure, regardless of the inert gas breathed. Then we looked at the last compartment, which according to Dr. Schreiner's work behaves as if it were 100% fat. For that compartment, we have the fairly well accepted 240-min half time for helium, and something on the order of 480 or 500 minutes for nitrogen -- this is consistent with Tektite, and confirmed by the NOAA OPS experience.

Dr. Bennett: Let me make one comment about neon -- and Dr. Hamilton's work will bear this out. If we are going to choose another gas we may as well choose hydrogen, at least for going deeper. Neon becomes a respiratory problem fairly shallow (700 ft), and does little to put off HPNS. I think most of us will see a mix of nitrogen with helium as the solution to HPNS in the very deep ranges.

# REFERENCES


1. Bjurstedt, H., and G. Severin. The prevention of decompression sickness and nitrogen narcosis by the use of hydrogen as a substitute for nitrogen. Milit. Surg., 103:107-116, 1948.
2. Brauer, R. W., S. M. Goldman, R. W. Beaver, and M. E. Sheehan. $N_2$, $H_2$, and $N_2O$ antagonism of high pressure neurological syndrome in mice. Undersea Biomed. Res. 1:59-72, 1974.
3. Coward, H., and G. Jones. Limits of Flammability of Gases and Vapors. Bureau Mines Bull. 503, 1952.
4. Dorr, V. A., and H. R. Schreiner. Region of non-combustion, flammability limits of hydrogen-oxygen mixtures, full scale combustion and extinguishing tests and screening of flame-resistant materials. 2nd Summary Rept., Contract N00014-66-C-0149. Tonawanda, N.Y.: Ocean Systems, Inc. DDC No. AD689545.
5. Edel, P. O. Report on Project Hydrox II. Final Rept. to ONR, contract N00014-73-C-0233. Harvey, Louisiana: Michel Lechler, Inc. 1974.
6. Hamilton, R. W., Jr., and C. H. Hedgepeth. Use of neon mixtures for short-duration dives in the 500 - 700 foot depth range. In: The Working Diver, 1974. Columbus, Ohio: Battelle, 1974.
7. Hamilton, R. W. Jr., M. R. Powell, D. J. Kenyon, and M. Freitag. Neon decompression. Final Rept. to ONR, contract No. N-00014-74-C-0424. Tarrytown, New York: Union Carbide, 1974.
8. Schaefer, K. E., and J. H. Dougherty, Jr. A new physiological sign of impending decompression sickness: nitrogen bursts in the expired air during nitrogen breathing in decompression. Aerospace Med. Assoc. Preprints, 1973.
9. Schreiner, H. R., R. W. Hamilton, Jr., and T. D. Langley. Neon - an attractive new commercial diving gas. Dallas, Texas: Offshore Technology Conference, 1972.
10. Zetterstrom, A. Deep sea diving with synthetic gas mixtures. Mil. Med. 103: 104-106, 1948.

# SESSION VIII: FINAL COMMENTS

# VIII. FINAL COMMENTS

## A. CRITIQUE OF THE WORKSHOP: P. B. BENNETT

A gathering like this might be expected to reveal many diverse views and opinions, and it's quite hard to assess exactly where we stand. I think there are a number of things that can be said. This meeting might well be a milestone because people have come forward and revealed their information. I regret that of the commercial people involved, two major U.S. companies, Ocean Systems and Taylor Diving, are not very actively involved. I think it's their loss.

In terms of the science, maybe the best way to summarize what we've learned would be to follow a dive profile, covering various points that have come up. In terms of a profile, compression is the first factor. I have come to the conclusion that for dives deeper than 400 feet, the idea is to get down fast, and therefore rapid compression is an objective.

The U.S. Navy has not indulged in the practice of fast compression, and we've had one or two other opinions favoring slower compression rates, but in general, and certainly in the commercial field, rapid compression is required. If you are going to have rapid compression you eventually require some narcotic, be it nitrogen or hydrogen, to try to ameliorate or supress the signs and symptoms of HPNS. When we get into greater depths this is going to cause further problems. I think there is a need for more work with deep trimix decompression, with perhaps more nitrogen or hydrogen.

Regarding theories of decompression computation, I think most work centers around two different theoretical concepts, diffusion and perfusion. They start perhaps as separate entities but tend to come together later. Where they come together, and how much is perfusion and how much is diffusion, remains to be seen.

Certainly the first ascent, the long pull to the first stop, appears to need modifying in the majority of the profiles. One should use a slower ascent to the first stage -- 60 ft/minute is perhaps not ideal.

Dr. Hills: Might I interrupt here, Peter. I think perfusion-diffusion isn't the real issue. The bubbles make more difference than perfusion-diffusion. I don't want it to be misquoted as the "diffusion" approach, because really it's the two phase versus the single phase approach. This is the major issue.

Dr. Bennett: Well, as I say, bubbles or no bubbles, the consensus seems to be that you should spend more time deep if you wish to improve you decompression. We've covered the methods for determination of bubbles. It is my belief, and it has not been changed by what I've listened to in the last two days, that bubbles and where they occur are related to the type of computation you are using. The bends still occur, so what the bubbles really mean in relation to bends, I don't think we have found out yet. The effect that bubbles have on the blood, and other changes that we've mentioned, I think are important. We have got to keep a broad view of as many of these variables as we can.

The use of oxygen has plagued all of us in decompression. Those involved with the Haldane-type of computation use a great deal of oxygen and even in the diffusion model, oxygen is still used to try to abbreviate the table length.

We haven't heard very much about inert gas washout studies in connection with the development of decompression or the relationship of inert gas washouts to bends incidence or the use of oxygen. Al Behnke has been on this for years; he worked with pink string and sealing wax, and he keeps telling us that we have the most elegant mass spectrometers and other equipment and we ought to get on it, instead of producing so many mathematical models. I think he's right. I think there is a need for that kind of work and I haven't heard very much mentioned in the last two days. There is perhaps a reason for this. Washout studies can best be done using shallow dives, and some workers who are prominent in those studies are not, unfortunately, at this Workshop. I wish we could have had them all.

Again, we haven't heard too much about the action of oxygen on inert gas washout, in terms of peripheral or vascular shutdown, perhaps for the same reason. It's assumed that high oxygen is dumping gas and is therefore good. As an aside, at Duke, we've compared 80-20 helium-oxygen, oxygen alone, air, and oxygen-air interrupted breathing, and we found that helium-oxygen was the best gas in an air decompression for dumping the nitrogen, not pure oxygen. I think that we could do with more of that kind of research. In terms of treatment, I was a little disappointed by the treatment sessions. We heard some good individual case reports, but little in the way of critical assessment, which we need, as well as some guidelines to the methods that we should really be using. It would be good if we could agree on them, so that when medical officers go out to the rigs they would be in agreement about what they are going to do. We should perhaps be thinking seriously about what kind of medical workup we would like divers to have, how thorough it should be, and we could then make recommendations to our commercial colleagues as to what they should be doing.

I had hoped that we might see more actual tables laid out so that we could look at them and play with them and study them in more detail, and in this way come up with some further answers.

I wish we knew more about adaptation or recovery between dives. How soon can or should a man make the next deep dive? We are very absorbed with developing a table but often at this stage are interested only in developing a table for a dive, forgetting that the men may have to make several dives in a relatively short period.

The data is extremely slim on no-decompression limits using diffusion-type calculations.

Dr. Hills: Once again, you have automatically linked zero supersaturation with diffusion. Now, you can maintain zero supersaturation equally well with perfusion- or diffusion-limited control.

Dr. Bennett: Not in our kind of model. I'm merely commenting that I think that kind of information would be very valuable.

Mr. Vann: To be a little more specific, I think you are talking about the no-decompression dive limits. That includes the limit for air and the limit for helium-oxygen mixtures, from which you can saturate and then return to the surface without decompressing. That's a very important point; it sets the critical volume of gas that can be liberated and not give pain. Work should be done on the same group of people for both air and helium, and Doppler should be used. On the other end of the scale you have the ascent rates from saturation dives for different constant partial pressures of oxygen. These are more or less "model-independent" and can really be analyzed from any model that you choose.

Dr. Hempleman: It looks as if we're going to fight the next war with the same technology as the last one. When we move deeper than about 400 feet -- and that's what this Workshop is about -- the form of presenting decompression sickness, except in an isolated case, changes from the familiar "bubble in a ligament." It really has changed, but we haven't heard much development of that.

Dr. Bennett: We have in fact learned one or two things about vestibular bends. One, they are associated with a very rapid initial ascent. If you make the first pull slower, it does seem to reduce the incidence of vestibular bends. Second, vestibular bends seem to be provoked by the air change. The incidence can be reduced by making the change slower.

Another thing we didn't get into very much, is the fact that a number of bend cases have been associated with taking hot showers.

We haven't considered dysbaric osteonecrosis very much in terms of the type of profiles we've been considering, but it is a very relevant and important aspect. In particular, does the existence of bubbles -- which we can detect by ultrasound -- provoke this disease?

One rather mechanical aspect is that of dive record keeping. An enormous amount of diving is going on and will go on in the future. I would hope that we would have access to the details of commercial diving -- the dives they do, the bends they get, the equipment they use. Can equipment such as depth-time recorders be worn by divers, with the records kept and analyzed by the company? This would seem like a good idea. So often we produce tables which seem good; they are sent out for a field test and the word comes back that they're useless. We hear they are no good, but we have no idea whether the diver did what he was supposed to do. I know there are constraints on divers in the field that we in the lab can hardly imagine, and they have to be lived with. But we really need to know what is actually done. At the moment we don't know.

The question of the utilization of drugs in decompression either before a dive or in terms of treatment, was treated only briefly. Should we at this stage be thinking of drugs as a method for preventing decompression sickness? We don't really know what decompression sickness is. Perhaps there are grounds for research here.

I would like to leave you with one thought. Though imperfect, the meeting was more successful than perhaps I even hoped for. I charge this group with the task of having another meeting like this, in perhaps two years.

## B. CRITICAL NEED FOR ADDITIONAL KNOWLEDGE AND EXPERIENCE: H. R. SCHREINER

Our intent now is to put together the outline of an agreement on what needs to be done and how we should go about doing it: what additional knowledge, information, and experience do we need to know to advance the art of decompression for operational diving between 400 and 1,000 feet?

What we critically need, are procedures for the same, simple, cost-effective and time-efficient exposure and return of divers in the ocean. Why have we not met this objective?

I submit that in part we do not have these procedures because the nature of the art of decompression and the diving business encourage secrecy and proprietary activity. And intuitively, I feel that no two laboratories, no two diving organizations, conduct dives in exactly the same way. The only internationally recognized standard that exists today is the U.S. Navy Air Diving Tables. There are other standards, but these are the only widely used procedures. If somebody says he decompressed according to table so and so, using the standards in the book, it is likely that the same situation applies elsewhere and the data so obtained can be compared.

One thing that hasn't happened that must happen to get to our key objective is a standardization of what we do.

Typically, what might happen is this. A laboratory looks at a new table -- say 500 X-ray -- and says, "Hey, this is a great table, let's try it. But we can't really afford to go at the rate they went, and we need a different kind of a work package, and really we can't shift to air at 150 because of logistics, etc., etc." What happens is that this particular procedure is not very unlikely to be repeated precisely anywhere else. And then you say, "In our hands this procedure didn't work." So, unless we come to some kind of understanding of how to evaluate and test decompression procedures, we will forever sit around and compare notes that do not jibe.

Now, I would like to invite you to discuss the kinds of parameters that ought to be standardized to make it possible to compare experiences on an inter-laboratory basis.

Mr. Wilson: I would like to see this group agree on some sort of "receiving standard", whereby the commercial field information can be used for analysis in a meaningful way. ADC has offered medicinal data and bone X-rays, toward a data bank. This might be one approach.

Dr. Hempleman: In the United Kingdom, you are constrained to live with certain standards if you are involved in human experimentation. For example, under no circumstances could we breathe more than half an atmosphere of oxygen for a continuous and long period of time. The word"long" has not been defined. Undoubtedly, the 0.8 atmosphere used by Professor Buehlmann and his collegues would not be permitted, even if the standard were agreed to, and even if I, as an investigator, thought it was a good thing.

Dr. Flynn: I think history shows that breakthroughs come when people do things which are radically different, or have what other people consider to be "crazy ideas." We cannot squelch that form of thinking to meet a short-term goal.

Dr. Schreiner: Maybe I am a dreamer, but I envision a group that can agree on minimal standards of compression and decompression rates, oxygen, nitrogen, you name it, so that any laboratory that wishes can say, "This research dive was conducted in according to a certain basically acceptable standard." We know what the rate of compression was, and the maximum oxygen. We know a number of things that we can't even get out of the record sometimes.

Dr. Shilling: It's true. A lot of the material that's published in the Undersea Biomedical Research journal lacks those details.

Mr. Edel: I wonder if maybe Dr. Shilling hasn't raised the critical point. Rather than set a standard for the experiment, if we could set a standard for <u>reporting</u> the experiment.

Dr. Hills: Can't we also have definitions? What do we mean by "bottom time"? I would take the U.S. Navy's definition of bottom time. This is something on which we ought to be able to agree. And what about units? Are we going to go metric? I hope so.

Dr. Schreiner: Clearly, when I speak about standards or definitions, I am speaking of things that will apply some time in the future. Obviously, the work that has been done to date has been done in a nonstandard fashion. It has been documented and is being made available.

One second constructive step we can take until the ideal day when all data are interchangeable, is to have each laboratory engaged in this business select their most elegant decompression procedure, and let us see if we cannot get some 10 or 12 of these decompression tables on the table. These should have enough information attached to them so that it's reasonable to expect someone else to be able to repeat the tables, to repeat the performance. Then all of these laboratories and groups and companies, to the extent that they have the means to do so, should try to repeat the results.

We need to have some ground that is solid under our feet, though that can be interpreted differently. Andre Galerne thinks he has a pretty good fix on it and Ocean Systems thinks they have a two or three percent bends rate; that is solid ground.

Dr. Behnke: The thing to do is to state precisely the conditions under which dives are performed, including the numbers, so you know what is behind the results.

Dr. Flynn: I would like to make the point that this forum is a perfect opportunity to see whether this will be put into practice.

We have heard from the German group, the Swedish group, the Swiss group, and the American group. All seem to agree that we should not ascend at a very fast rate, but once we arrive at the first stop we have two choices: one group says it is best to come up as shallow as possible and then stay long, and the other group says exactly the opposite.

In all cases, all these groups have presented dives at roughly 500 feet for 30 minutes. So, if those schedules could be promulgated as a part of this Workshop, it would be a perfect opportunity for other groups to test these procedures.

Mr. Hughes: I subscribe to that view wholeheartedly. I would like to comment on your statement about standard procedures. You cannot sit in a room like this and expect everyone to agree to a standard for anything. These are all very creative people. They do not like to have standards set for them; that is true of scientists as well as diving contractors. But, you can produce a standard and have them challenge it. Once a good standard is produced, what you find is that a lot of them will begin to follow it to have some continuity between experiments. So, I suggest that someone produce them, whether there is any group consensus or not.

Secondly, there are some fairly well established biomedical tests to determine if a decompression was satisfactory. If you just specified that a standard decompression study included a standard set of biomedical tests, that would be a step in the right direction.

Dr. Spaur: I think the sometimes minor variations in tables are not as significant as we would like to make them out, and that the variation in human response among individuals on separate occasions and the variation among different individuals in general, is a great deal wider than the difference in some of these nicely run tables. I do not think you are going to find fine enough discriminations between decompression stops, Navy or otherwise, to make them worth chasing after.

Dr. Schreiner: You are talking about biological noise or random noise that would occur regardless of how long or carefully you follow a particular procedure.

Dr. Flynn: Getting back to the matter of data reporting, let me cite Dr. Buehlmann's report in Aerospace Medicine on the 31-atmosphere dive and the 36-atmosphere excursions.* Someone who is interested in the decompression aspects of those excursions has nothing but a graph to go on in terms of the profile. There obviously are some stops on the way up to 31 atmospheres but there is no mention in that report, at least that I can find, of what the decompression actually was. This is part of the problem of analyzing such dives.

Dr. Schreiner: Dr. Buehlmann, if someone responsible were to write to you, and ask for detailed clarification of the circumstances surrounding one of your published decompression procedures, would you not impart to the questioner the full information he requires?

Dr. Buehlmann: It is no problem. We have our reports separated. It is possible.

Dr. Bornmann: One thing you are not addressing is who is going to pay for this. All this information is available, but the questioner has to pay the cost of getting it. He may have to go to EDU and sit there and look through the logs. If a person wants information from Dr. Buehlmann, he may have to learn to speak Swiss, but that is his responsibility. You can't expect the experimenter to go back over what he did five or ten years ago and do an exhaustive analysis for something _you_ want, unless it fits in with his goals.

Dr. Schreiner: I was hoping that as a result of this Workshop, we could all walk away from here thinking we have a common goal. If I had the task of fashioning safe, simple, cost-effective and time-efficient procedures to send working divers to 500 or 600 feet for 30 to 90 minutes, I would want to run the Duke 500 X-ray and the DFVLR tables, for example, in our laboratory on at least a dozen different individuals under exactly the same conditions. For the first time I would be in a position to really reproduce somebody else's decompression work.

A basic, fundamental aspect of any science is the replication and verification of results by different laboratories. It is about time we faced up to that. My remarks are intended to draw attention to this deficiency. I have no means, intellectual or financial, to make it happen, but I can encourage all of you to make it happen.

Dr. Bennett: Could we not agree -- we certainly have moved in that direction -- that 500 feet for 30 minutes might be a starting point. Can we begin by sharing our results for this table?

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* A. A. Buehlmann, H. Matthys, G. Overrath, P. B. Bennett, D. H. Elliott, and S.P. Gray. Saturation exposures at 31 ATA in an oxygen-helium atmosphere with excursions to 36 ATA. Aerospace Med. 40:394-402, 1970.

Dr. Peterson: I have been associated for the last couple of years with the International Decompression Data Bank, located at the University of Pennsylvania. The Data Bank's objectives are to aid in the development of decompression theory, to promote open reporting of decompression experimentation, to ensure that that reporting is in a complete form, and to make the information accessible to everybody.

The development has been quite slow and has tended to concentrate on laboratory dives, due in large part to the fact that the records the companies keep of their field dives are either incomplete or in such form that it is very difficult to transcribe them into a uniform format. The staff has been small and the going has been very slow.

I would strongly support this group in what you are suggesting, that some uniform recording format be adopted and that a minimum body of information be included in each report, so that all necessary information for the duplication or complete analysis of the experiment will be available. Now, as part of this Data Bank effort, we have developed a recording format. It is quite complex, because we were interested primarily in being able to cover all experimental situations and not just to record profiles; we also wanted to include experimental information of various sorts, such as observations, environmental conditions, and so forth. From the aspect of recording or reporting an experiment, the format is quite good. It does seem somewhat complex when relating it to field dive records. A simpler form or use of only parts of this one would be satisfactory.

I think we could, as a starting point, suggest what the minimum information required might be. A brief list of parameters is given in Table VIIIB-1, grouped into essential and desirable categories.

Dr. Hamilton: When a technical paper contains a lot of tabular data that may be of interest to only a few readers, it is a common practice for the author to deposit this data in the Library of Congress, with a proper reference in the paper. Those who need the tables may then order them for a small fee.

I propose that authors reporting decompression experiments deposit the decompression details with the International Decompression Data Bank in the same manner. This can be done now with no new programs or new fundings.

Dr. Schreiner: In may ways this would be a good first step. We can use the standards presented by Dr. Peterson, submit data to the International Decompression Data Bank, and reference it in publications and reports.

Dr. Peterson: From our experience the problem would not be at the receiving end, but rather with the people who are actually doing the dive and putting it down. If things are done right there the first time,

Table VIIB-1. International Decompression Data Bank Reporting Requirements

Primary gas-loading analysis data:

1. Date of dive and starting time.
2. Diver/subject identification.
3. Time-depth-(pressure)-gas profile.
4. Repetitive situation.
5. Occurrence and type of decompression sickness and whether treatment was used.

NOTES:

1. Essential to prevent duplication of data.

2. Name not necessary if some other consistent identification, such as a number, is used. Date of birth should be supplied with name.

3. UNITS MUST BE STATED and defined if necessary. If term ATA is used, it should be defined in mmHg. Compression rate and rates between stops should be given, as well as definition of bottom time. Gas composition must include oxygen (fraction, percentage, or partial pressure), breakdown of inert components, and shifts.

4. Any dive or dives during preceding 48 hours should be referenced.

5. Depth and time of occurrence should be given.

Secondary information needed for interpretation:

6. Facility and location.

7. Individual data: size, sex, age, physical condition, etc., was diver cold, and if so, when?

8. Environmental data: wet or dry, lab or field, water temperature, $CO_2$ level, equipment used (breathing equipment, bells and chambers, thermal protection, etc.).

9. Work level, type, and duration.

10. Decompression method: table used, on-line computer, etc.

11. Monitoring and testing performed: laboratory analysis, ultrasonic bubble detection, etc.

12. Signs and symptoms, whether or not related to decompression sickness, and diagnosis or description (e.g., oxygen convulsion, mechanical injury, blowup, etc.)

Prepared forms available from: Dr. R.E. Peterson, International Decompression Data Bank, 14 Medical Labs Building, G2, Philadelphia, PA 19174 the rest is quite simple. We encourage automatic data recording.

Dr. Schreiner: To sum up this topic, the consensus is that each laboratory will present data in sufficient detail for the experiments to be repeated in other laboratories. It has been suggested that this material be submitted to the International Decompression Data Bank, directly or preferably after review by journal editors. It then becomes incumbent on the Data Bank to store the data and make them available to all.

Now, I would like to poll you about what you consider to be the major thrusts that ought to be undertaken to improve deep diving. Where should the total, collective resources of the diving research community be aimed in the next few years?

Dr. Hempleman: I would say oxygen is a good one to start with, to decide what levels of oxygen are permissible and for what periods of time. Also, this Workshop is on work deeper than 400 feet, but I don't think that the medical or treatment capability is equal to that. I would like to see that improved.

Dr. Flynn: I think what we are dealing with here can be put, roughly, into six different categories: First, we have an inner ear-vestibular-auditory type of decompression sickness; spinal cord decompression sickness; pulmonary decompression sickness ("chokes"); cutaneous decompression sickness; and limb bends pain. Later there can be osteonecrosis.

As far as I have been able to determine, we have treated all of those as though they were one single syndrome, governed by the same overall gas exchange model. We now have to define the pathological entity that we are dealing with. It is false to assume that the same kinetics apply to each of these individual situations. Take limb bends and pulmonary decompression sickness as an example: Kent Smith has shown us that the lung receives a tremendous number of emboli on certain schedules, but the subject develops limb bends. He is not quite sure what the relationship is between those two phenomena. So if we are going to talk about limb decompression sickness, let us find out what the relevant considerations of that particular entity are. I think these are individual problems that we are dealing with and not necessarily part of the same disease process.

The second thing I would say is, we are doing too much nit picking about different kinds of computational methods. We have seen many profiles in the last two days, and they all look the same for all intents and purposes. I find it very hard to believe that one of them will have 50 percent bends and the other would cause zero bends. We need some uniform testing to separate out the various models.

Dr. Hempleman: I also wonder why we are getting different presenting forms of decompression sickness with the changing depths and gas.

Further, I would like to assure that in the long term the decompression procedures that are being pursued are not harmful to those who are subjected to them. It really is a thorny problem to know what is "harmful", and in what respect it is harmful. In this context, in the first Underwater Physiology Symposium, there was no mention of bone necrosis. In the 1971 printing there were shrieks about "bone" as though it was going to stop diving and caisson work stone dead. The furor has since lapsed a bit. Fortunately, it is not the problem we thought it might be three or four years ago. Are we perhaps doing the same again with another body system? We have changed our presenting signs; have we changed something else?

Dr. Mueller: I think we should concentrate our efforts on the question: What is a safe dive? What does it mean?

Dr. Schreiner: That is a very good point. The normal, traditional arbiter of success has been, is there pain, and can you walk out under your own power. That is no longer sufficient. We have to distinguish between an acute and a chronic sign of success. An individual can be decompressed and meet every conceivable standard of normalcy afterwards within a day or two or a week, but some of the subtle things that may be happening may not be measurable for years. So, to some extent we will have to rely only on what we can measure in a reasonable period of time. I can see no way -- philosophically -- of writing a standard of safety that requires us to wait five years to determine if we have done right today.

I think we will probably have to approach it as follows. We will first have to inventory the many signal changes that are observed with great frequency in conjunction with dives. We see changes in enzyme levels or in coagulating properties, or in bubbles -- things that have been mentioned here. I think we have to ask in each case, is it really related to the decompression procedure? We then try to aim research to find out what if any cause and effect relationship exists between the observed abnormality of the signal and the preceding pressure exposure. If we can correlate them, then we can say they are possibly harmful.

Dr. Smith: The whole thing is ridiculous unless we know what the etiology of decompression sickness is, and of course, this necessitates a definition of decompression sickness.

Dr. Hills: What really are the tissues, and what are the half times? Any other aspect is basic research into treatment. Treatment has been looked at clinically, but I think there is also a lot of laboratory work that can be done in terms of treatment.

Dr. Schreiner: May we ask Suzanne Kronheim to give us some wisdom from her vantage point, that of monitoring a lot of ONR-sponsored research.

Ms. Kronheim: I think it is very important to find guidelines for the type of decompression work that you want to get done. As Dr. Hills suggested, no one, to my knowledge, is at this time working on what tissue half times really are from an experimental point of view, yet these are being used as the basis for developing decompression tables.

Few are doing studies on inert gas uptake and elimination. This is the foundation, but information here is sorely lacking.

I also agree that we should look further into the problem of deciding what is a "safe dive", and what are the oxygen criteria.

Dr. Schreiner: To move to another matter, that of appropriate testing, it has been suggested that 12 exposures is enough. Is there a consensus that 12 manned exposures on the given profile constitute at least preliminary approval or preliminary acceptance of the safety of the table?

Dr. Spaur: I don't think we accept that. I think it's a little bit more involved. I don't think that I would accept only 12 man-dives on a table, unless we were testing a series of tables and did 12 dives on each. If you were doing one single dive, a new dive, like your 500 for 30, I don't think that I would ask the Navy to promulgate that as a Navy table after only 12 clean dives, particularly on a deep dive.

Mr. Hughes: Some years ago, I sent a questionnaire out to a lot of authorities on decompression, and basically said, "What is an acceptable criterion for bends incidence?" I got all kinds of answers and one of the answers was that I was very naive to ask that question. I still don't have an answer. To twist your question just a little bit, let's suppose that 12 man-dives don't constitute acceptance of a schedule, but does one bend out of 12 constitute a reasonable schedule for this sort of dive? At what point do you have what you consider to be a schedule acceptable to put into the field?

Dr. Hempleman: You should really do a sort of statistical sequential analysis. Which means you go on until you get a bend, perhaps. If you do get a bend, then you got one out of so many. The confidence which you can put on that, statistically, has certain limits. If you've got that bend and you've got certain confidence limits on it, you must then go on further to bury the effect of that bend so that you can increase your confidence. On the whole, if you get 10 or 12 different people and do something to them and the response is similar -- you get away with it -- it looks like a reasonable proposition. You just don't live long enough to get perfection in some of these things.

The next thing you know, somebody like Kent Smith quite rightly will say, "What about the platelets?" So, what about the platelets?

Mr. Hughes: My question then would be -- suppose that we have 200 exposures on a given schedule and that we averaged one out of 12, or, say, 8½% bends incidence. Is that acceptable?

Dr. Flynn: You have to decide that yourself. You have to decide what incidence of bends you're willing to accept, what level of bends is acceptable to you in your diving situation. Then when you turn to your experimental work, you can compute from the binomial probability distribution, what the probability is of being within your acceptable incidence with a specific number of hits, out of a specific number of dives. For example, if you have a goal of 5% hits, and out of 15 dives you've got 2 hits, you know this is unacceptable. You have less than a 1% chance of getting 2 out of 15 if the true incidence is less than 5%. That's the approach that we take.

Capt. Bornmann: May I ask Dr. Purdy if he is contemplating setting up such a standard for NIOSH?

Dr. Purdy: I really would await the recommendations of scientists and industry, some sort of consensus. I wouldn't boldly say it's got to be less than 0.2% or else; I think that would be pure foolishness. We're living in an empirical world.

Dr. Bornmann: I'm glad to hear you say that. The scientists can argue about this, and argue forever, but it's very foolish for the government to set up such an iron-bound arbitrary recommendation.

Dr. Purdy: I think we are in agreement with that.

Mr. Kenyon: We had a 10% bends incidence on the first Mark VIII table. Now 10% may seem okay for provisional tables, but it wasn't acceptable because we got a 50% bends incidence in a different application of the same tables. (We revised them!)

Mr. Wide: We aim at between 2 and 3% bends rate as acceptable for incorporation of tables. We don't reach it normally, but that's our goal.

We have started to use thermovision to study skin bends. Preliminary results indicate that we can see a skin bend earlier with thermovision.

Mr. Wilson: We keep using "percentages" of bends cases. I would like to get a clarification on this. If you do have two or three people in a chamber and one man gets the bends, is it considered one case or what? The other crew members will follow along through the therapeutic decompression? I'd like to get these numbers in perspective.

Mr. Kenyon: You're right, that's a real problem. The term "man-exposure" is good when the bends incidence is very low (less than 5%). Generally, because the problems occur after surfacing, every diver has gone through the same profile. However, when the bends incidence is higher and the incidents are occurring at depth, then you have to use "dive" criteria. Your actual incidence in terms of man-exposures may be double, if the bends incidence is high and you have not substantiated the fact that all divers have surfaced.

Dr. Schreiner: What really matters is how many event-free dives (man-exposures) you conduct.

Mr. Edel: This might be a possibility for three categories. We have clean dives, we have dives resulting in decompression sickness, and then, perhaps, a third category, incomplete dives which would not fall in either column.

Mr. Hughes: Let me make a suggestion on how you can get more data of 500 feet, 30 minutes. Exposures are expensive to do in the laboratory. We do those, paid for by the oil industry, all the time. We do them in the field. We are also doing them under a program of development right now at Duke University. There are exposures being made which are not being paid for by the NIH and the scientific community, which could be used to gather a great deal of scientific information. Either of these types, field or laboratory, could be better utilized.

For years I have proposed that some scientists come with us and make some tests and try the people and try the equipment, but very few come.

Dr. Schreiner: If nothing else has happened during the last two days, the fact that the three of you (Wilson, Hughes, Galerne) representing the diving industry are taking the attitude that you are, on the record, is probably the most helpful thing that has happened. The record will show (in case you forget when you get home) that you have given an open invitation for researchers to work with the end users of their basic information.

I think the time has come to bring this to an end. From where I sit, I think this has been useful. I hope that you share this view.

I think we have, for the record, not only a compilation of the contributions of the various laboratories to the state of the decompression art, but more importantly, we have a commitment from these laboratories to work freely in the interchange of decompression information.

We have agreed on the value of standardizing the way in which decompression information is generated and reported. We are committed to report decompression results and this may be done through the International Decompression Data Bank.

We have a commitment from the representatives of the diving industry here to work with the basic scientists in the development of safe and effective decompression procedures.

We also have an express commitment from the industry to the notion that fundamental research is required and is very important.

We have itemized a number of these areas where our knowledge is so thin that we must redouble our efforts to strengthen the scientific infrastructure under what we are doing. These include tolerance to oxygen and oxygen limits, basic work on gas transport and the different "tissues" that cuase different types of decompression sickness. We need uniform testing of different models.

Specifically, though not exactly a new subject, we need more knowledge of the etiology and pathogenesis of decompression sickness in all its manifestations. We need to know when a dive is safe, in both the long and short term, and we need ways of assessing decompression adequacy. We need to know more about treatment.

We have not been able to agree, with respect to testing tables and procedures, just what constitutes adequate evidence that a table is safe or ready for field use.

We have agreed to collect representative 500 ft/30 min tables (given in VIIIC).

Finally, we recognize that the Undersea Medical Society is looked upon to assist agencies here and elsewhere in defining the future needs of this technology.

I would like to thank every one of you. I would like to thank the Undersea Medical Society, and our sponsors Duke University and the Wheeler organization, for bringing us together.

## C. COMPARISON OF PROFILES: R. W. HAMILTON, JR., AND H. OSER

Prompted by several requests made during the Workshop, we have consolidated several typical tables into a single chart, Fig. VIIIC-1. Additional information is given in Table VIIIC-1.

We should mention at the outset that this chart is designed to facilitate general comparisons of the profiles. A number of details from the available materials were not clear, but in our judgment the effort required to approach perfection was not warranted. We had to take liberties with some formats to simplify the comparison, and we had to make a few assumptions, e.g., rate of ascent between stops. There may be errors, for which we accept full responsibility and offer our apologies. Neither the authors nor the UMS are, of course, responsible for actual use of any of these tables. Most of the tables are elements of ongoing table development programs and are subject to continuous change and modification. These do not necessarily represent the latest thinking of any laboratory or investigator--they should be considered as representative samples and nothing more.

For more convenient comparison we converted all charts to fsw (feet of seawater, 1 fsw = 1/33 of a standard atmosphere.) We regret the necessity for this, since the trend is toward the use of SI units.

For bottom time we followed general U.S. practice, and considered the 500/30 table to include about five minutes of compression and about 25 minutes of working time. In any event we tried to begin the timing of decompression at the point of departure from the bottom for all tables.

Since all eight tables presented have completely different gas regimes, minute details of the profiles become moot.

In fact, since we could not include data on <u>results</u> of the use of each table, the entire chart could be considered an exercise in geometry. Some results were presented earlier in the Workshop. Particularly well tested is the Duke 500 X-ray, with 25 clean dives (See Section III, G). Very minor bends have been encountered in field use of this table. The DFVLR table has produced good results in the laboratory (See Section III, A); however, that laboratory feels this is the deepest table using this approach which is practical. Professor Buehlmann's table has proved satisfactory in both laboratory and field, but has subsequently been modified.

Some experience is available on table <u>sets</u>, if not this specific table. The IUC and OSI Mark VIIIA are in occasional field use but are not without bends (See Section V, A). No bends have been reported from field use of the very conservative 3X Hybrid dive plan. Dr. Hill's table has not been tested.

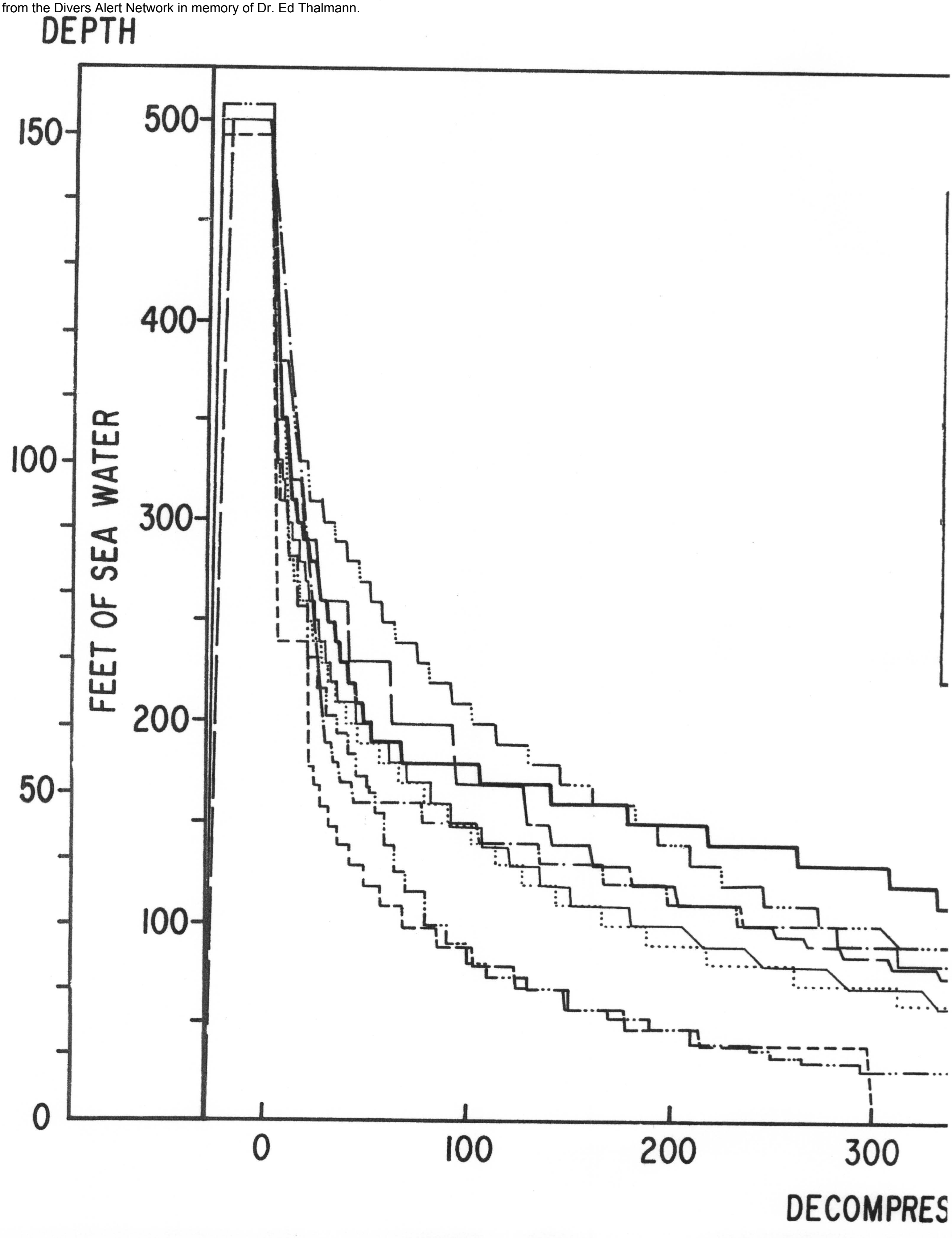
DEPTH
150
100
50
0
500
400
300
200
100
FEET OF SEA WATER
0
100
200
300
DECOMPRES

Figure VIIIC-1

Comparison of representative 500 ft/30 min decompression profiles

| Line | Table | Total time, min |
|---|---|---|
| – – – – – – – – | DFVLR | 300 |
| ——··——··—— | Buehlmann, 1974 | 499 |
| —·——·——·— | Hills | 675 |
| ━━━━━━━━ (heavy solid) | Duke 500 X-ray | 727 |
| ····················· | IUC | 790 |
| ———————— (thin solid) | OSI-Tarrytown Mark VIII A | 959 |
| ——  ——  —— | Subsea-Virginia Mason SSM-7 | 1170 |
| ——···——···—— | 3X-Tarrytown Hybrid | 1437 |

Additional data on these tables is given in Table VIIIC-1.
(From Undersea Medical Society Workshop Report WS 2-28-76.)

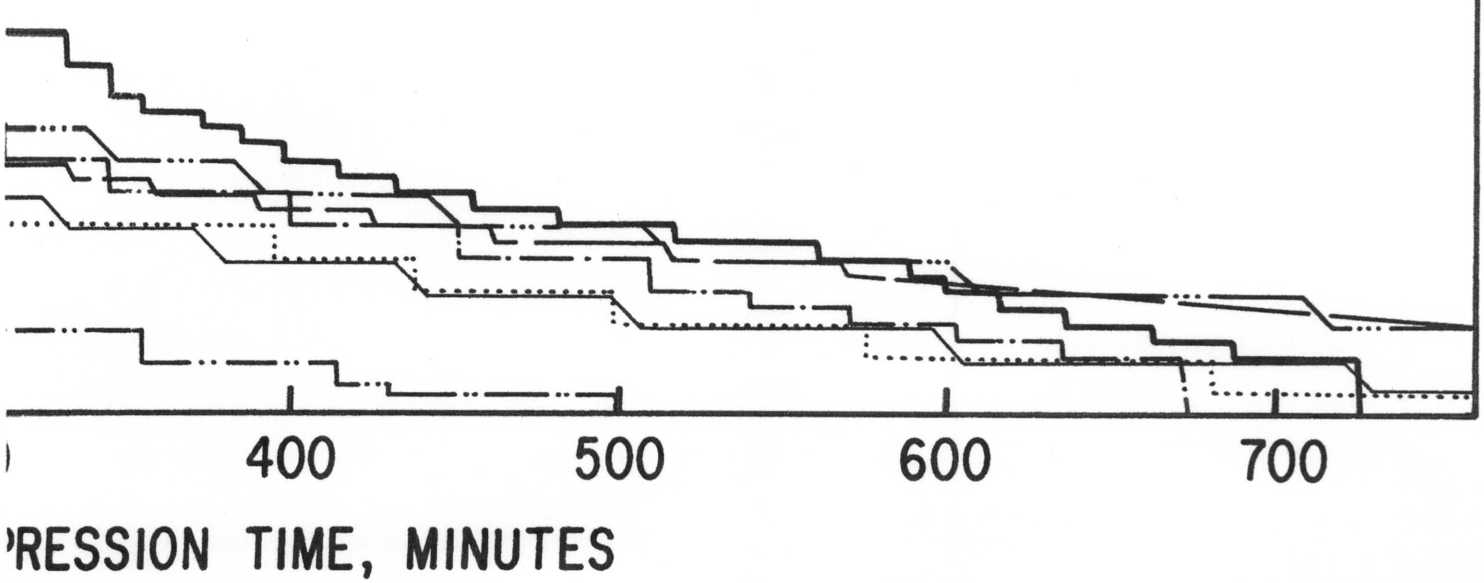

Table VIIIC-1. Characteristics of the tables in Fig. VIIIC-1.

| Table | Bottom mix, % $O_2$ | Bottom mix, % $N_2$ | Intermediate mix, % $O_2$ | Intermediate mix, fsw | Shift to air, fsw | Oxygen, fsw | Oxygen, regime | Total time, min | Remarks |
|---|---|---|---|---|---|---|---|---|---|
| DFVLR | 10 | 0 | 30<br>50 | 240<br>130 | none | 50 | continuous | 300 | entire decom on BIBS |
| Buehlmann 1974 | 7 | ≃2 | 10 | 460 | 165 | 33 | continuous | 499 | |
| Hills | 7 | 0 | 20 | 280 | none | 70 | alternating with 20-80 $HeO_2$ | 675 | |
| Duke 500 X-ray | 7 | 0 | 16 | 300 | 130 | 50 | air breaks | 727 | |
| IUC | 8 | 22 | none | | 130 | 50 | air breaks | 790 | |
| OSI-Tarrytown Mark VIII A | 5 | <10 | 16 | 200 | 100-80 | departing 60 | air breaks | 959 | |
| Subsea - Va. Mason SSM-7 | 6 | 0 | 10 (0.6 atm) | 150 | 60 | none | none | 1170 | linear from 45 fsw |
| 3X-Tarrytown Hybrid | 6 | 23 | 14 (0.5 atm) | 250 | 150-100 | departing 60 | air breaks | 1437 | linear from 30 fsw |

Report WS: 2-28-76.

Several of the tables call for excessive oxygen, or may be all right at 500/30 but excessive for deeper or longer tables on the same dive plan.

Although it is incomplete, this comparison chart is a good beginning, and represents something of a breakthrough in the general matter of table secrecy. We hope by the time of the next workshop on this subject that channels will have been opened to the International Decompression Data Bank, and that the interesting variety of profiles can be related to copious and well-documented results.