

## EXPERIMENTAL AND OPEN WATER DIVES AT COMEX, THE PRACTICAL DIFFERENCES AT 2001 FT

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### ABSTRACT

This paper describes two simulated dive experiments in 1972 which were part of the Comex deep diving research programme. During these dives, to 500 m and 610 m, particular attention was paid to the effects of High Pressure Nervous Syndrome (HPNS) on diver performance. The results from both dives showed that such depths can be attained and that, although HPNS effects were apparent, they could largely be overcome by careful control of the compression schedule. It is hoped that such experiments will enable future commercial dives to these greater depths.

### INTRODUCTION

The Comex Deep Diving Research Programme, which has been in operation since 1964, has two main objectives: a) To improve the safety and efficiency of operational diving procedures (eg compression and decompression schedules; survey of the physiological response of divers to long, hardworking dives; control of breathing mixtures). b) To try to determine the ultimate depth limit for safe diving.

Under the leadership of Dr X Fructus a highly experienced team has been carrying out a series of deep diving experiments. The programme has taken place at the Comex Hyperbaric Research Centre where four diving simulators can be operated independently and simultaneously.

The diving depth limit research programme has been divided into four phases:

- I Ultra deep test dives with a monkey.
- II 'Physalie' series tests in which divers are exposed to record depths for short periods.
- III 'Sagittaire' series tests in which the divers' ability to perform work is studied during long exposures. Wet dives are carried out.

IV 'Janus' series tests in which operational procedures and equipment are tested in simulation chambers and subsequently in the sea itself.

#### THE EXPERIMENTS 'SAGITTAIRE II' AND 'PHYSALIE VI'

After the experiment 'Physalie V', in November 1970, during which two divers descended to a simulated depth of 520 m, two major experiments were undertaken in 1972. These were 'Sagittaire II' from February 21 to March 6 which was a 100 hr saturation dive at 500 m and 'Physalie VI' in which, between May 16 and June 2, two Comex divers descended to a simulated depth of 610 m (2001 ft) and where they remained for 80 minutes.

These dives were made possible by the design of special compression schedules which minimized the symptoms of "High Pressure Nervous Syndrome". A main objective during these experiments was to continuously monitor medical, physiological and psychological parameters of the subjects.

#### High Pressure Nervous Syndrome (HPNS)

The compression programmes are based on the "Osmotic Dysbarism" hypothesis which concerns the disequilibrium existing between the saturation of "rapid" tissues (blood for example) by inert gases, and that of the "slowest" tissues, during rapid increases in pressure. It seems more than likely that a limit exists, as far as differences in pressure of dissolved gases are concerned, above which osmotic dysbarism causes problems connected with the HPNS as observed during experiments.

We determined, from the results of previous experiments, a presumed admissible gradient, as a function of depth, from which the compression speed could easily be computed.

For 'Physalie V', the initial gradient was obtained by a fast compression phase at one metre/minute, down to 108 metres. The compression was subsequently slowed down according to the theoretical programme, and interrupted by two 16-hour stops at 350 and 460 metres. To test their influence on the HPNS, periods of rapid compression at one metre/minute, from 350 to 400 metres and from 460 to 490 metres took place after the stops.

In the 'Sagittaire II' experiment, this theoretical compression curve was used uninterruptedly so as to permit a comparison of the two methods. The results obtained showed both the utility of stops at intermediary depths and the adverse effects of rapid compression at one metre/minute. In order to verify these conclusions and to check the possibility of reaching greater depths, the following method was chosen for 'Physalie VI' the permissible gradient at a given depth was reduced; all fast compression phases were suppressed (even the initial one from the surface) and a two-day stop was effected at 350 metres to reduce possible HPNS symptoms before proceeding deeper.

Tremor In HPNS the first clinical disorder observed is a tremor. This appears at the extremities of the upper limbs especially during deliberate movements and tends to disappear when the subject is at rest. Measurements carried out by means of a geophone showed that it appears around 300 metres, and tended to disappear during the 46 hour stop at 350 metres in 'Physalie VI'. This tremor increased during the compression phases. With 'Sagittaire II' at 500 metres, the tremor increased by 400% of the control value; with 'Physalie VI' it increased by 250% at 610 metres. In all cases the frequency of the tremor ranges from 8 to 12 cycles.

Muscle jerk may also appear in the extremities of the limbs and from 450 metres on, this may spread to the upper parts of the limbs sometimes reaching the muscles of the neck and face. In addition, when the tremor is pronounced, it can in certain cases generate a slight dysmetry.

Day-time somnolence, as well as tremor, was observed in the divers from 300 metres during 'Sagittaire II', and from 565 metres during 'Physalie VI'. This somnolence disappeared when the subjects' attention was attracted. During the decompression the tremor decreases, as do the muscle jerk and the dysmetry. This decrease was more rapid during 'Physalie VI'. The somnolence persisted for a long time during 'Sagittaire II', and disappeared rapidly during 'Physalie VI'.

Electroencephalograph (E.E.G.) The EEG modifications were far more pronounced during 'Sagittaire II' than during 'Physalie VI'. During 'Sagittaire II' the first EEG modifications occurred with both subjects between 300 and 315 metres, and consisted of the rapid appearance of a sleep record when the subject closed his eyes; the alpha activity was replaced after several seconds by a stage I sleep record. This vigilance instability persisted unchanged with subject B. With this same diver the theta activities also increased from 400 metres whether his eyes were open or closed. An increase in theta activities was observed with subject A at 500 metres, and in both cases was more pronounced in the anterior and middle zones. These disorders persisted during the stay at 500 metres, and were more pronounced with diver B.

The power spectrum of the EEG activities showed, in comparison with the values observed on the surface, an increase of 500% for the delta frequencies and 900% for the theta frequencies with subject A; and an increase of 900% for delta activities and more than 1,200% for theta activities with subject B. The maximum differences were observed especially the 3rd and 4th day of the stay at 500 metres. The upper frequencies (alpha and beta) were for their part depressed by 75%.

During the decompression, the modifications decreased on the whole but still persisted between 100 metres and the surface: the vigilance instability did not disappear until 12 hours after subject B left the compression chamber.

During 'Physalie VI', the first EEG modifications were

observed at 270 metres. With Subject A they took the form of a slight increase of the 6 cycles theta activities in the fronto-central zone and of a decrease in the alpha activities. Only this last modification was perceptible with subject B. These modifications tended to disappear during the stop at 350 metres but reappeared and became more pronounced during the compression from 350 metres to 610 metres, the first bursts of theta activity in the anterior zone becoming apparent with subject B from 400 metres.

From 565 metres, the anterior and middle theta activity reached a high level and at the same time a slight vigilance instability was observed especially with subject A. At 610 metres, the anterior and middle theta activity was very marked with subject A, but remained slight with subject B.

Alpha activity was always present on the subjects closing their eyes, but was slightly slower (1 to 2 cycles down) and was less marked and stable than on the surface.

The power spectra of the EEG activities of the two subjects showed in comparison with the values obtained on the surface an increase of 400 to 500% for the theta frequencies, and differences in the order of 100 to 300% for the delta frequencies. A decrease of 50 to 75% was observed for the alpha and beta frequencies.

During the decompression the somnolence rapidly disappeared. The EEG modifications decreased from 400 metres and disappeared between 200 and 100 metres.

Psychometric Studies We shall deal here with only three of the tests which formed part of the comprehensive psychometric studies carried out, these are:

- (1) The peg board test which measures manual dexterity.
- (2) The visual choice reaction time test which evaluates the degree of attentiveness and vigilance.
- (3) The Rey test which measures mental agility.

Manual dexterity was little changed by the stay on the bottom. The decrease in performance during 'Physalie VI' was slight : 9% and 4%. During 'Sagittaire II' the test was repeated five times at 500 metres and showed an average decrease in performance of 8% and 13%.

Vigilance instability from 400 metres was best demonstrated by the visual choice reaction time test. With 'Physalie VI', the tests showed an increase of upto 100% in reaction time together with a marked dispersion phenomenon. 'Sagittaire II' was very different, the reaction time at first increased by 10 to 25% between 300 and 450 metres and then improved during the saturation stay at 500 metres; becoming slightly shorter on the fourth day than at the surface.

The Rey figure ordination test gave constant results in 'Sagittaire II' and showed a decrease in performance of 14% in 'Physalie VI' for the more intellectual of the two divers.

There is nothing of very great significance here and it cannot be said that the HPNS entails any very noteworthy



reduction in the psychomotor or intellectual faculties of the subjects.

#### Electrocardiograph (EKG)

Cardio-vascular adaptation to the large variations in pressure was excellent. The arterial pressure when at rest showed no modification. The EKG at rest and during effort remained normal: in particular no lengthening of the Q-T space was observed. The monitoring of the cardiac rate at rest (recordings were taken every 4 hours during the night) gave interesting results in that no bradycardia was observed at great depth. On the contrary with both experiments, the cardiac rate rose regularly from 200 to 300 metres. It decreases during the decompression but should be noted that the oxygen partial pressure was then 0.50 atmospheres.

Dynamic effort (pedalling on the ergocycle, in a prone position at high power (100 to 130 watts), was well tolerated, in spite of certain respiratory limitations. In fact the ventilation during effort was close to the maximum instantaneous flow at 600 metres (specific gravity of the mixture was 10.5 grams per litre). The maximum cardiac rates went from 135 to 145 beats per minute, just slightly higher than at lesser depths (oxygen partial pressure = 0.40 atmospheres) and had values near to those obtained for the same effort with air (oxygen partial pressure = 0.21 atmospheres).

In order to study the subjects' resistance to fatigue in relation of the depth and the duration of exposure, we measured how long they were able to hold up a 15 kilogramme weight with their forearm. This test was carried out under heart-rate and electromyographic monitoring.

With all four of the subjects no perturbation of the electrical activity of the muscles was observed, which seemed to indicate normal functioning of the motor units. Similarly heart-rates under effort are quite identical to control values.

However with one of the subjects an increase in heart-rate at the end of the effort was noticed between the first and the fourth day of bottom exposure; which probably denotes a certain degree of fatigue.

These very slight modifications prove the excellent physical and psychic condition of the subjects during the whole of the experiments.

#### CONCLUSION

It seems that the HPNS must limit the depth which can be reached by an oxy-helium diver in the same way that the nitrogen narcosis limits an air diver.

The HPNS observed for the first time in 1968 in COMEX'S Hyperbaric Research Centre seemed to constitute a barrier. Since then, the different dives carried out tend to show that this barrier can be pushed back. Indeed with these two last dives although the first symptoms of the HPNS were detected at 300 metres, the EEG modifications only became important

from 450 metres without however reaching a level necessitating the cancellation of the dive. With the two divers who reached 610 metres, the modifications of the EEG activities were relatively less marked.

Since 'Physalie VI' and 'Sagittaire II' a further dive in the 'Sagittaire' series has been carried out in which man's ability to adapt and work at 600 m was investigated. A practical application of this work took place in 1975 when the worlds deepest commercial dive was done in Labrador waters by six Comex divers who salvaged a piece of equipment at 326 m. The divers performed heavy work for a long duration at an ambient water temperature of 0°C. In 1977 the experiment 'Janus IV' will take place offshore during which divers will descend to 460 m.

We do not know what depths will be reached in the future but there seems to be no real barrier to at least 600 m.

#### ACKNOWLEDGMENTS

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## RECENT ADVANCES IN DECOMPRESSION PREDICTION

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### ABSTRACT

The paper discusses a fresh approach to the 'critical supersaturation ratio' and describes a method for converting a standard no-stop decompression table for use in a variety of other no-stop diving situations, such as excursion diving from habitats, saturation diving, diving on different gas mixtures or diving at altitude.

Some of the predicted tables are in very good agreement with published tables based on tested dives. It is shown that it is not necessary to use a multi-tissue multi-critical ratio model to generate no-stop decompression tables. Only one tissue and one safe ascent formula are needed. This enforces the hypothesis that ordinary limb bends are caused by a simple physical phenomenon - the volume of released gas in a particular critical tissue.

### INTRODUCTION

One of the more remarkable aspects of diving is the way in which basic physical laws have been adapted and applied to the physiology of man underwater. Virtually in his first few lectures, the novice learns of the laws of Archimedes, Boyle, Charles, Dalton and Henry. He may even hear of the laws of Graham and Fick and he is invariably interested in the compressibility of human tissue, thermal conductivity, refractive index and speed of sound in water. All of these aspects impinge vitally on his performance in a marine environment.

Yet, with all this impressive display of classical physics, one of the most poorly understood phenomena is the way in which nitrogen is taken up on a dive and released during decompression. It is not even certain what gives rise to ordinary limb bends, except that the victim had somehow absorbed too much gas on a particular dive.

The application of physical principles to this issue has been exceptionally slow, and it is worthwhile gaining some insight into why this should be so.

There are four main major difficulties that have to be resolved in order to gain a picture of nitrogen mechanics during diving. The first is that the precise site of limb/joint bends is unknown; secondly, even if the site were known, the amount of nitrogen in that site after a dive to a given depth and time is unknown, since the mathematical theory of gas exchange in tissue is still at a prehistoric level! Thirdly, that on decompression it is equally unknown how much nitrogen is eliminated, since it is not certain whether a gas phase (bubbles etc) forms on every decompression (the presence of gas would spoil any uptake symmetry). Finally, what causes bends pain - distortion caused by too large a volume of released gas, or perhaps tissue annoxia?

In any experimental programme, it is hopeless trying to answer all four questions at once. Two of the unknowns can be eliminated in one experiment called a saturation no-stop dive. This is exactly what Boycott, Damant and Haldane (1908) carried out in their early work in developing the Admiralty Diving Table on Compressed Air. They decided to expose first goats and then men for an indefinite dive until their tissues were saturated with nitrogen at a given depth. By first saturating, the quantity of nitrogen entering the tissues at the chosen depth would be known. A rapid no-stop decompression back to the surface avoids the third unknown since on a no-stop return, it could be assumed that ascent time would be too short for nitrogen elimination to occur in this time. As a result the exact site of limb bends became less important, and one was left with the ultimate issue of deciding what caused the bends.

It is at this stage that the novice diver learns of the result of Haldane's experiment, culminating in the famous 2:1 ratio. Haldane found that man could be exposed to compressed air at 2 bars pressure (1.6 bars nitrogen) for periods ranging from 6 to 12 hours and thence rapidly decompressed to 1 bar (goats could tolerate a somewhat greater pressure). Any slightly deeper depth provoked marginal limb bends. He thus concluded that because the tissues appeared to be saturated after about 6-12 hours, nitrogen was being released into the tissues to form a pain provoking gas phase if this ratio was exceeded. He clearly thought that it was the physical volume of gas released into the tissue that caused bends, obviously suggesting a ratio of pressures following Boyle's law, but he implied indirectly that if the ratio was not exceeded, no gas would actually be released into the tissues, and it would leave in exactly the same way as it had entered. The reason why this was a popular idea was that it was inconceivable that free gas could be formed in the blood and tissues without some sort of overt sign, and also that it had been found experimentally that rapidly decompressed saturated water could sustain enormous supersaturations without bubble formation. Thus was



born the critical supersaturation ratio hypothesis, viz that if the dissolved nitrogen in the tissue responsible for bends ever exceeded 1.6 times the ambient pressure, suddenly bubbling would occur and available free nitrogen would fill the tissue to cause a pain provoking bend. This simple ratio approach was rapidly adopted by all concerned, and although Haldane was careful to avoid stating directly in 1908 that he felt no gas would be released unless this ratio was exceeded, he too committed himself to this belief subsequently, Haldane and Priestley (1935). These ideas are referred to in greater detail later.

Haldane had also introduced five hypothetical tissues in an attempt to describe nitrogen uptake in the tissue responsible for bends, and it became popular at that time to describe a tissue by its half time (ie the time taken to half saturate) and its decompression or supersaturation ratio or 'safe ascent criterion'. Initially the tendency was to keep the five 'Haldanian' tissues and one supersaturation ratio, and indeed the RN steadfastly used this concept for almost 50 years following Haldane's report to the Admiralty! However, in the US, Hawkins, Shilling and Hansen (1935) proposed that each of the five Haldane tissues should have a different ratio, thus increasing the number of parameters of the original model.

Hawkins et al decided after analysing the results of over 2000 human dives that the supersaturation ratio appeared to be too high for some tissues and too low for others. Even Haldane had felt that his ratio may in fact be incorrect for longer and/or deeper dives beyond 165 feet (50.3m). These ideas were developed in the US, over the next 20 years culminating in the US Navy Standard Air Table (Des Grange, 1956; Dwyer, 1956). Their model uses six tissues, each with its own special formula for the supersaturation ratio.

During the early fifties the RN took a second look at Haldane's original five tissue model, and replaced it by a single tissue model. The latter model had been put forward by Hempleman (1952), who had noted certain properties in the no-stop air curve indicated that single tissue model based upon diffusion mechanics would be more realistic. Unfortunately, choosing this new model had the effect of upsetting the hypothesis that the 2:1 ratio would still give a correct prediction for decompression. Thus began a series of experiments to redetermine the supersaturation ratio and its dependence on the depth of dive. At first it was thought that a constant pressure difference rather than a pressure ratio may be the correct formula. Thus instead of a 2:1 ratio, Rashbass (1955) proposed that a 30 ft (9.14m) pressure difference would be enough to trigger bubble formation. This was proved incorrect, or at least too large, and eventually a non-linear formula was developed by Hempleman (1969) as shown in Figure 1. The original Haldane formula is shown for comparison. It can be seen that as the exposure pressure  $P_1$  is increased, the decompression pressure must be decreased (ie the ratio is lowered with increasing depth).

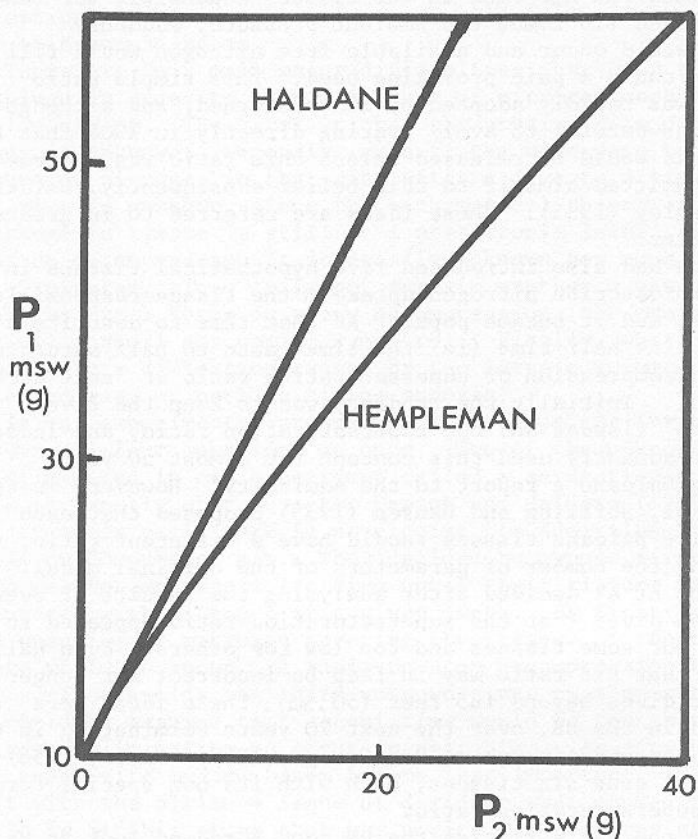


Figure 1. Empirical relationship between depths  $P_1$  and  $P_2$  on air.  $P_1$  is the equivalent depth of a saturation which can be followed by rapid ascent to a 'just safe' threshold depth  $P_2$ ; Haldane, (1908), Hempleman, (1969), MSW (g) refers to metres sea water (gauge).

By using this empirical formula, Hempleman was able to use his single tissue model to generate various diving table culminating in the RNPL Air Tables (1968, 1972) and the BSAC/RNPL Tables (1975). It was realised that the empirical formula in Figure 1 may not have any physical significance, and very likely was dependent in the first place upon the model chosen to predict nitrogen uptake. A different uptake model would in turn require a different supersaturation formula, exactly as had occurred in the development of the USN tables.

It can thus be appreciated why a certain mystique grew up around those involved with decompression diving. Divers and

medical personnel responsible for their safety viewed with suspicion the variety of theoretical attempts to link a series of seemingly unrelated facts. It appeared that as soon as a predictive model was developed, the next round of deeper and or longer diving trials would demolish the predictions, leaving the model builders cranking away at hand calculators, later to emerge with yet another variation! Evidently any model with enough parameters could by hindsight be 'bent' to fit the known facts, but its credibility also depended on good predictions outside the original range. However, air diving is limited to comparatively shallow depths and opportunities to test the range of validity of the model are essentially confined to diving at altitude or from habitats.

Before tackling this hurdle, it was decided to re-examine air and helium saturation diving in the hope that a relationship between air and helium threshold pressures might yield information on the basic processes in gas uptake and elimination in the diver.

#### SATURATION DIVING

There are a number of features of breathing helium which make comparison with air very difficult. It has long been known that the Haldane 2:1 ratio on air may be increased substantially breathing helium. For example, the diver may safely ascend from about 12-14 m following a saturation dive on 80% helium. Another uncertain factor is that for short duration dives, the effect of residual nitrogen has to be taken into account. Finally, it is also known that saturation on helium occurs more rapidly than with nitrogen. The experiment used to prove the last result is fraught with interpretation difficulties because one has to be sure all trace of residual nitrogen has disappeared. This in turn requires a knowledge of the duration of nitrogen elimination, which was only later proved to be much longer than suspected, in the vicinity of 12 hours.

In all the above experiments a formidable complicating factor is that divers may become acclimatized or 'worked-up' to large decompressions. This may give a totally false impression of the time scales of uptake and elimination of inert gas. It is perhaps worth pointing out this difficulty in greater detail. Suppose that it is required to determine the duration of nitrogen uptake in the diver, using a marginal bend as the end point. One would begin by choosing some arbitrary depth and exposing the diver to ever increasing bottom times, followed by rapid decompression back to the surface, until a marginal bend (if any) is provoked. The trial would be repeated at, say, 1 metre deeper with a corresponding decrease in the duration required to provoke a marginal bend. In the ideal case, a depth would eventually be found where bends did not appear to increase in severity with increasing bottom time beyond a certain limit. From this

data the duration for nitrogen saturation should be easily estimated. Unfortunately, however, the diver becomes able to tolerate increasingly larger decompressions during the series, which has the effect of apparently increasing the rate of nitrogen uptake. Thus one makes both an under estimate of the time for saturation, as well as an over estimate of the critical ratio for saturation dives! It was many years before this phenomenon was realised, especially as it was confounded by normal variation in a population of divers. Haldane's original estimate of approximately 9 hours saturation with 2:1 ratio has gradually changed over the years. (The saturation time is arbitrarily defined as the time required to reach 99% saturation) The USN tables suppose a 14 hour saturation and 1.5 ratio, whereas the RNPL tables employ a 10 hour saturation and a ratio varying from 1.9 to 1.4! It is now believed that the actual saturation time is nearer fourteen hours with a correspondingly smaller ratio, but certainly not as long as the 27 hour, 1.4 values used in the Swiss tables.

It can thus be seen why it is dangerous to draw conclusions from saturation data unless they are of sufficiently long duration and avoid using worked-up divers. What was really needed was a definitive experiment to determine the dependence of critical ratio on depth. Saturation diving on air with decompressions to a threshold depth other than the surface has never been carried out in the past, and would not now be realistic in view of oxygen toxicity problems. Also, in the case of helium diving, the main accent has been on the development of standard decompression tables, rather than in establishing the critical ratio. At last an opportunity arose in 1970, following the RNPL 1500 ft (457.2m) dive, when it was decided to carry out a series of experiments to design a decompression profile from saturation at 250 metres. The project was under the medical supervision of Surgeon Commander (now Captain) E.E.P. Barnard RN and was completed towards the end of 1971. Barnard reported his results at the Fifth Symposium on Underwater Physiology in 1972, the proceedings of which were finally published in 1976 and described what has since turned out to be a classic experiment in diving history.

The plan was to spend 24 hours at a pressure  $P_1$  and to determine by trial the threshold depth  $P_2$  that could just be sustained without provoking symptoms. It was intended to extend this process to ever increasing depths until the target depth was reached, with the final profile consisting of big drop, long stop stages. The gas mixture used was 0.22 bar  $O_2$ , balance helium. Since it was known that a saturation dive on 80% helium at a depth of 12-14m could be followed by direct ascent to the surface, it was assumed without verification the equivalent depth on the above mixture would be 10 metres. Complete saturation could be assured in 24 hours and care was taken to avoid working-up the divers. The result of this series of dives is shown in Figure 2. The original experimental design actually terminated at the 69 metre point because it was found that the 108 m depth



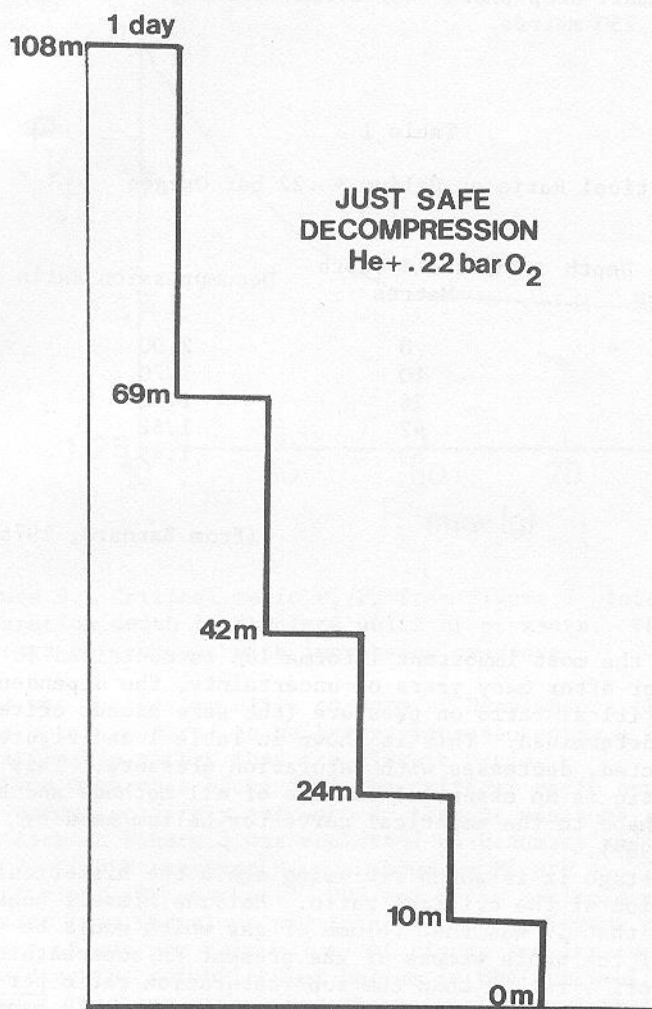


Figure 2. 'Big drop, long stop' profile on normoxic helium developed by Barnard (1976). Saturation at a given stage for 24 hours followed by decompression to a 'just safe' stop.

produced marginal limb bends and other forms of decompression sickness began to appear. This is a highly significant development and is discussed in detail by Hennessy and Hempleman (1977).

Another point is that the 42 m depth was originally placed at 47 m, but later altered in the second phase of the experiment.



The latter began after an attempt at achieving bend free decompression to 69 m had failed. The stages were broken up in to a small drop short stop format leading to a successful profile from 150 metres.

Table 1

Critical Ratio on Helium + .22 bar Oxygen

Saturation Depth Metres	Threshold Depth Metres	Decompression Ratio
10	0	2.00
24	10	1.70
42	24	1.53
69	42	1.52
108	69	1.49

(from Barnard, 1976)

However, the most important information is contained in Figure 2, for after many years of uncertainty, the dependence of helium critical ratio on pressure (the safe ascent criterion) may now be determined. This is shown in Table 1 and Figure 3 and as expected, decreases with saturation pressure. This characteristic is an essential feature of all methods and has a similar shape to the empirical curve for helium used by Bühlmann (1969).

At this stage it is worth reviewing again the historical interpretation of the critical ratio. Haldane himself began by thinking that it was the "volume of gas which would be liberated if the whole excess of gas present in supersaturation were given off", rather than the supersaturation ratio per se. Since it was not clear at which level this event would happen, it was found convenient to replace the original critical volume hypothesis by the concept of a critical ratio. Many years later Picard (1941), Nims (1951), Albano (1960) and Hills (1966) resurrected the original spirit of the critical ratio, but because of lack of suitable data, the critical volume hypothesis resubmerged. The crucial feature of the critical volume hypothesis is that the saturation pressure  $P_1$  should be linearly related to the threshold pressure  $P_2$ . Haldane, of course, supposed direct proportionality, as seen in Figure 1, although allowance should have been made for the presence of other gases, surface tension and tissue elasticity in the volume of released gas. This latter aspect first proposed by

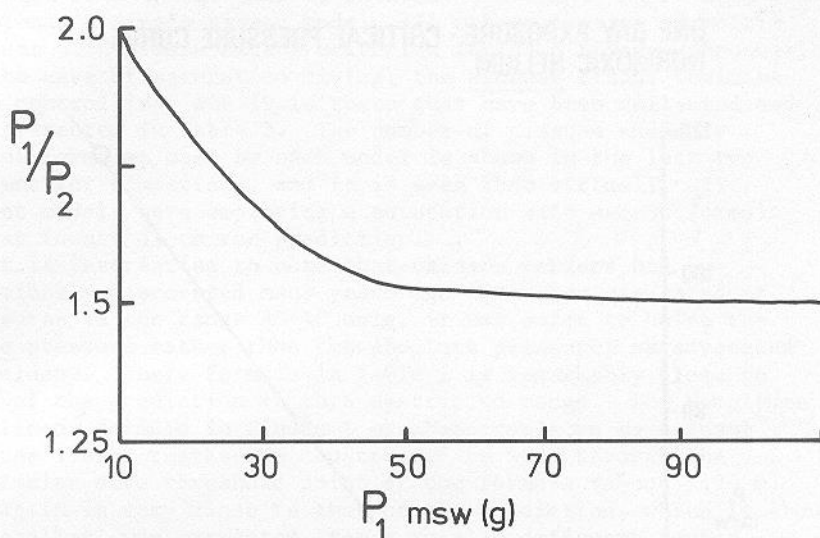


Figure 3. Critical ratio  $P_1/P_2$  from Figure 2 plotted against saturation depth in absolute units of pressure. The ratio appears to decrease with increasing pressure.

Nims was applied by Hills to some of Hempleman's (1957) goat data which appeared to be better represented by a linear relationship rather than the classic ratio.

Even with this background the critical ratio hypothesis had become so firmly entrenched, that it was some time before the data in Figure 3 was replotted by Hennessy and Hempleman (1977) with  $P_1$  against  $P_2$ , as shown in Figure 4. The correlation coefficient is remarkably .999 and it is a simple matter to extract from the slope and intercept the relevant critical volume of gas per unit volume of tissue and its excess pressure, providing the solubility of helium in the critical tissue is known. This experiment has thus clearly shown that only one tissue (site unknown) is involved in saturation dives, and that the volume of released gas is responsible for marginal symptoms. The next stage is to attempt to derive the equivalent safe ascent criterion on air, where the number of hypothetical tissues and critical ratios is prolific.

#### SAFE ASCENT CRITERION ON AIR

The critical tissue solubility is an unknown, and one must assume two extremes - a lipid or fatty tissue and an aqueous tissue. It is easily shown, Hennessy and Hempleman (1977) that the helium slope of 1.397 taken from Figure 4 converts in the case of nitrogen to 1.361 for a lipid tissue and 1.604 for an

# ONE DAY EXPOSURE - CRITICAL PRESSURE CURVE - NORMOXIC HELIUM

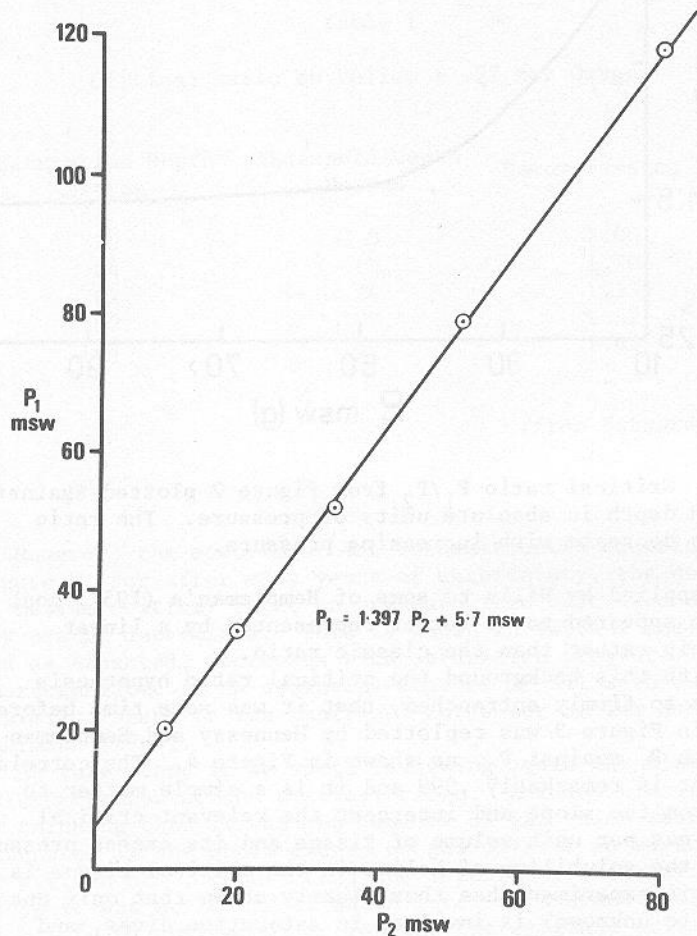


Figure 4. A plot of the saturation pressure against threshold pressure. Units are msw absolute (add 10 msw to the depth in Figure 2).

aqueous tissue. These values are very much lower than Haldane's original estimate of 2.0 and it is worth recalling that if either of these values is correct, the true rate of nitrogen saturation of tissue will be much longer.

At this point it is impossible to decide between the above two limiting ratios, since nitrogen data for deep air saturation dives

do not exist. All that can be done to examine various successful decompression models to determine which safe ascent criterion has been used in the case of saturation diving. Apart from Hempleman's single tissue model, all others consist of multi-tissues with a safe ascent criterion for each tissue. However, in the case of saturation diving, the slowest tissue would be rate controlling, and it is these that have been collected and are presented in Table 2. The number of tissues and safe ascent formulae used by each model is shown in the last two columns for comparison, and it is seen that virtually all recent models were employing a saturation safe ascent formula almost identical to the prediction.

It is interesting to note that caisson workers had empirically discovered many years ago that when saturated at pressures in the range 30-40 psig, it was safer to halve the gauge pressure rather than the absolute pressure, as advocated by Haldane. Their formula in Table 2 is remarkably close to that of the prediction in this restricted range. The Hempleman non-linear formula in Figure 1 has been replaced by a least squares linear regression constrained to pass through the indefinite dive threshold point of the formula (about 9.24 m) and again is very close to that of the prediction, which it should be recalled, was extracted from a totally different source of normoxic helium exposures.

Some of these formulae were originally published as non-linear curves relating  $P_{N_2}/P_2$  against  $P_{N_2}$  and amazingly these curves when replotted as  $P_1$  vs  $P_2$  turned out to be virtually linear! This was particularly true of the empirical curves used by Bühlmann (1969) (Swiss tables) and Des Grange (1956) (USN Standard Air Table). At first it was thought that these authors must have been aware of the linearity of their formulae. However, this was dispelled when it was noted that for example Dwyer (1956) had attempted to fit the USN safe ascent curve by a tenth power function of the critical ratio!

The formulae presented in Table 2 show clearly that there has been a steady trend over the years towards lowering the saturation ratio to a value well below the aqueous prediction of 1.6 and close to the lipid tissue prediction of 1.36. Figure 5 shows this progression, spanning virtually the entire range of formulae in Table 2. Hills derived his value of 1.766 from an analysis of goat data and used it to both correlate human diving tables prepared by Crocker (1957) and generate new tables following his hypothesis for optimal decompression. It is possible that as a result he has over estimated the rate of nitrogen desaturation in his search for best fit numerical parameters. This when coupled to an over estimate of the safe ascent ratio, could lead to an unsafe prediction, especially on longer deep dives.

Whilst it is not completely certain that a value of 1.36 is correct, an excellent opportunity to prove or disprove the preceding conclusions arises in the case of diving at altitude or excursion diving from a habitat.

Table 2  
Critical pressure formulae on air  
Slowest (saturation) tissue

Formula (bar)	Source	COMPLETE MODEL	
		No of tissues	No of critical formulae
$P_1 = 2 P_2$	RN, Haldane 1908	5	1
(a) $P_1 = 2 P_2 - .10$	Caisson workers rule		
$P_1 = 1.8 P_2$	US, Hawkins <u>et al</u> 1935	5	5
$P_1 = 1.75 P_2$	USN, Yarbrough 1937	5	5
(b) $P_1 = 1.52 P_2 + .46$	USN, Des Grange 1956	6	6
(c) $P_1 = 1.63 P_2$	USSR, Zal'tsman & Zinov'eva 1965		
(f) $P_1 = 1.38 P_2 + .52$	US, Workman 1965	6	6
$P_1 = 1.77 P_2 + .23$	Hills 1966	1	1
(b) $P_1 = 1.40 P_2 + .47$	Swiss, Bühlmann 1969	16	6
(g) $P_1 = 1.37 P_2 + .56$	RNPL, Hempleman 1969	1	1
(d) $P_1 = 1.39 P_2 + .42$	Canada, Stubbs 1971	(e)1	1
$P_1 = 1.36 P_2 + .34$	Prediction, Hennessy & Hemplemann 1977	1	1

### Notes

- (a) Equivalent to the caisson workers rule of halving the gauge pressure, rather than absolute pressure.
- (b) Derived from a  $P_1$  vs  $P_2$  replot of an empirical curve  $P_{N2}/P_2$  vs  $P_{N2}$  (where  $P_{N2} = .8 P_1$ ), which turned out to be linear.
- (c) Derived from a safe decompression to 4.3 bar after a six hour exposure at 7 bar.
- (d) A rearrangement of the original fsw formula  

$$K_T (=P_T) = \frac{1.385}{P_{SA}} \quad \text{where } P_T \sim P_1 \text{ and } P_{SA} \sim P_2.$$

$$\frac{1}{1-13.7/P_T}$$
- (e) The model actually consists of 4 tissues in series, which was shown by Hennessy (1973) to be equivalent to a single tissue.
- (f) A rearrangement of the M-value equation.
- (g) A linear regression of the psi formula R ( $=P_1/P_2$ ) =  $\frac{400}{P_1+180}$  passing through the no-stop threshold depth of the formula.



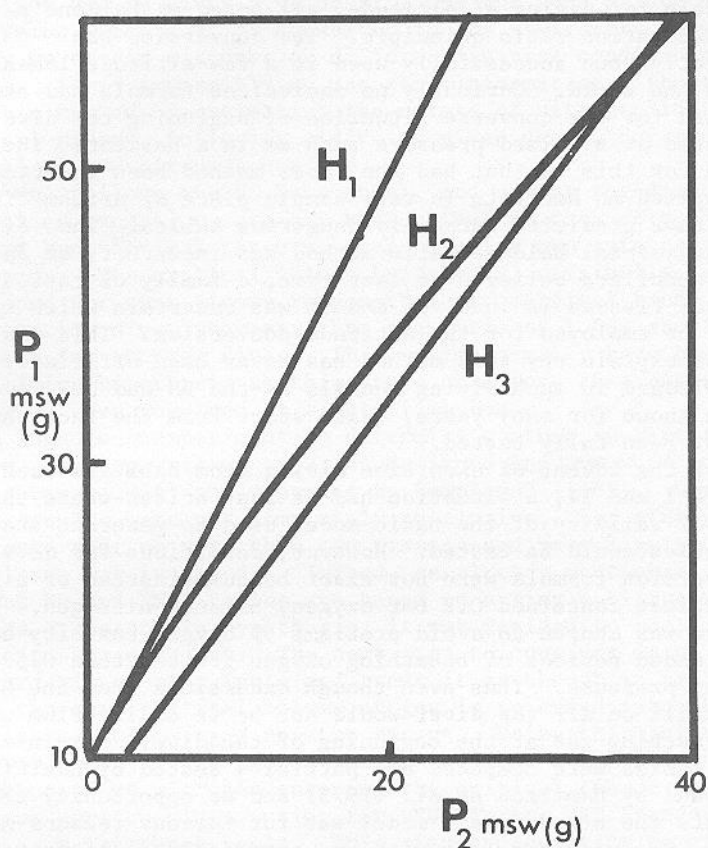


Figure 5. The progression of the safe ascent criterion on air;  $H_1$ , the Haldane linear formula, slope found too steep especially on long deep dives;  $H_2$ , the Hempleman formula, slope less steep but still passing through the origin;  $H_3$ , the Hennessy-Hempleman formula derived from helium data and allowing for tissue elasticity and surface tension in the gas phase. All the formula in Table 2 are in the range between  $H_1$  and  $H_3$ , except for a small area near the origin.

#### OTHER FORMS OF AIR DIVING

The only useful variations of standard air diving are diving at altitude or as excursions from habitats. These forms of diving have only recently come into vogue, and the shortcomings of standard air tables are immediately apparent. The latter were designed for situations where the diver, saturated in air at 1 bar, begins and ends his dive at the same pressure.

In 1967 and 1970, Cross published a series of reader's suggestions in *Skin Diver* on a method to convert a standard

air table for diving at altitude, all based on Haldane's supersaturation ratio principle. The conversion has apparently been successfully used in a few altitude lakes around the world. Curiously no equivalent formula had been proposed for the converse situation of beginning the dive saturated at a raised pressure such as in a habitat. The reason for this is that had the Cross method been inverted and applied to habitats (a very simple piece of arithmetic) it would have predicted extremely dangerous tables! Thus either the fundamental Haldane ratio method was incorrect, or as most table compilers believed at that time, a family of ratios and critical tissues is involved and it was uncertain which tissue should be employed for the altitude conversion. This would perhaps explain why this method has never been officially acknowledged in such diving manuals as the RN and USN, even though known for many years, quite apart from the fact that it has not been fully tested.

With the advent of excursion diving from habitats such as Tektite I and II, a situation had at last arisen where the range of validity of the basic model used to generate standard air tables could be tested. However, conditions for developing a conversion formula were not exact because instead of air, the habitat contained 0.2 bar oxygen, balance nitrogen. This mixture was chosen to avoid problems of oxygen toxicity caused by extended periods of breathing oxygen greater than 0.5 bar partial pressure. Thus even though excursions from the habitat were still on air the diver would not be in equilibrium with his breathing gas at the beginning of the dive. Completely fresh tables were computed and partially tested by Edel (1969, 1970) and by Hamilton *et al.* (1973) and an opportunity to validate the standard air model was for various reasons not taken. One was thus left with two apparently distinct methods to generate air tables!

To compare a dive at altitude or from a habitat with a standard air dive, care must be taken to define the exact basis of similarity. If the dive involves decompression stops, or is a repetitive dive, then a meaningful comparison may not be possible. This is because the rate of elimination of gas during decompression or following several recompressions may be quite different to that of uptake or indeed from one dive to the next.

There is only one situation which may be compared directly without such difficulties of interpretation. This is a single 'one off' no-stop dive, which is not repeated within a sufficiently long period of time, so that all memory of the previous dive has vanished.

In this case the basis of similarity is obviously that the volume of gas which would be released on a no-stop dive at altitude or from a habitat must be identical to that released following an equivalent standard air dive. The critical volume of gas which the tissue can just tolerate is contained in the safe ascent ratio for air in Table 2. The limiting ratio of

1.361 for 80% N<sub>2</sub> gives a value of 1.089 for pure N<sub>2</sub> and it is this value which must be conserved in any similarity situation involving no-air mixtures or different initial conditions. However, it is impossible to use this information without a reliable method of estimating the uptake of nitrogen in the critical tissue, following a dive of given depth, duration and initial tissue nitrogen content.

If one follows the US Navy model, and unless the dive is a saturation excursion, one has the unpleasant task of monitoring all six hypothetical tissues, each with its own safe ascent criterion. The critical volume hypothesis, originally developed for that tissue responsible for marginal bends in a saturation dive, suggests that the ratio of 1.089 should be the same for shorter dives. This would normally have been a simplifying hypothesis at this stage, had more than one type of tissue been involved in simple limb bends.

An ingenious experiment to determine whether in fact there is more than one tissue type was designed and tested on goats by Rashbass (1954) and repeated by Hempleman (1961). Whilst their results were not completely conclusive, they strongly suggested that no more than two tissues and very likely only one tissue was involved. Indeed, as already mentioned, the current RN and RNPL tables are based on a single tissue model. Thus as a reasonable hypothesis, it was decided to adopt a single tissue approach. The next step to consider is a standard air no-stop table and treat it as though in each entry the same critical volume of gas is released into the same tissue following a no-stop ascent. Using this information Hennessy (1976, 1977) showed how an estimate could be made of the quantity of nitrogen entering the critical tissue on each of these no-stop dives, using for example the USN Standard Air Table as a reference table. This standard quantity of nitrogen was then used to make a prediction of the actual decompression that could safely be sustained following an air dive of the same duration but now commencing from altitude or a normoxic or air habitat. In the case of diving at altitude, the predictions were in turn compared with various well known rules, such as the Cross Corrections, those appearing in the RN Diving Manual, RNPL Air Tables (1972) and Swiss Tables (Boní et al., 1976).

For diving following equilibration at altitude (about 12 hours) the conversion formula takes on a particularly simple form:  $D_T = 2 D_A / (P_A + 1)$ , where  $D_A$  is the actual depth dived,  $P_A$  the barometric pressure at altitude (bars) and  $D_T$  the theoretical depth to be used in the tables. Using this simple formula, it became apparent that the RN, RNPL and Cross rules appeared to be too conservative. On the other hand if a dive is made within 12 hours of reaching altitude, it was shown that these rules then appeared to be unsafe! If this is correct then such an obvious anomaly in these rules has passed unnoticed because of limited use. The predictions were in fair agreement with the Swiss tables, which were really unsuitable for comparison purposes because they were designed

to be used by divers making rapid excursions to high altitude lakes by helicopter followed by another helicopter flight post-dive.

Recently, an extensive set of tables has been computed by Bell and Borgwardt (1977) based on extrapolations to altitude of the six tissue USN Standard Air Model of Des Grange. Significantly, the above formula turned out to be in extremely good agreement with their prediction for a single no-stop dive.

The Cross correction also has a particularly simple form for the case of diving following equilibration at altitude, viz.  $D_T = D_A/P_A$ , and as previously noted, this formula predicts a dive which is more conservative than that of the former formula. This observation appears to be supported to a limited extent by the general opinion of users of the Cross formula in such places as California, Mexico and South Africa, but really needs to be scientifically tested under controlled conditions.

In the case of a no-stop air dive from a habitat, the predictions agree surprisingly well with the air excursion tables from normoxic habitats appearing in the NOAA (US National Oceanic and Atmospheric Administration) Diving Manual. These were computed and partially tested in both wet and dry conditions by Hamilton *et al.* (1973). The latter tables were generated by eleven hypothetical tissues, each with its own safe ascent criterion based on an empirically modified nitrogen model developed by Workman (1965). This vast number of variables was thought necessary because of the need to predict in addition the saturation decompression from the habitat following the excursion dive. Yet, by using an extremely simple concept of a critical volume of gas and a single tissue type, Hennessy (1977) showed that the entire set of NOAA excursion dives could be economically generated with more than sufficient accuracy. This fact shows that a multitude of tissues is largely irrelevant and deceiving! In particular there is a danger of over-parameterization of the model which may lead to a form of numerical instability. This phenomenon can occur when using an essentially incorrect model to predict dives outside its immediate range of applicability. When it is discovered that the model fails in this case, an attempt is made to save it by adding still more tissues and safe ascent formulae. This will in turn generate a further failure when the new model is extrapolated to yet another longer and or deeper dive.

It remains to point out that the NOAA tables may still give rise to decompression sickness if used repetitively. The tables were designed essentially for a single no-stop downward or upward excursion from the habitat. If the habitat is at the surface then the dives may be evidently repeated at 12 hourly intervals, as in a standard air table. However, at greater saturation depths it is likely that gas released on a previous dive may take much longer than 12 hours to dissipate, and subsequent dives may become unsafe. In addition some of the entries in the table are excessively long on high partial



pressures of oxygen as to constitute a risk of lung damage (in fact a caution to this effect is being prepared by NOAA).

If gas is released on these marginal no-stop dives, it is not difficult to show that to a first approximation the time for its resolution is proportional to the ambient pressure in the habitat. Thus a dive which takes for example 8 hours to lose the memory of released gas following a standard no-stop dive from the surface will require not less than 24 hours if the habitat is placed at 20 metres. This aspect would demand that the safe depth of excursion be reduced if repeat dives are to be carried out. Finally, the decompression from the habitat could not be safely commenced until such gas had been completely resolved.

These shortcomings of excursion diving tables serve to show that there is considerable scope for further research and that the design and development of optimal schedules under various physiological and operational constraints is a non-trivial problem. The overall optimisation philosophy reduces to a set of well defined multi-parameter set of constraints, complicated by a poorly defined mathematical model of gas uptake and elimination, except in particular cases such as no-stop dives.

#### MIXED GASES

One of the more interesting aspects of the critical volume approach is to examine the way in which a mixture of several gases will combine in a single no-stop dive. Such knowledge would open the way to the use of multiple gas switching, a technique pioneered by Keller and Bühlmann (1965). A formula for such a mixed gas critical volume was developed by Hennessy (1975) and used to examine the effects of no-stop diving, oxy-helium and air, with various levels of presaturation of each inert gas. Recently an opportunity to actually test the prediction arose in a trial devised at RNPL using a trimix of 40% He, 40%  $N_2$  and 20% oxygen. This gas is a particularly popular mixture with divers being free of defects of air and oxy-helium up to depths of 75 m, such as nitrogen narcosis, high gas density, thermal loss and helium speech distortion. The test dive consisted of a series of 25 minute 75 m exposures each followed by direct ascent to a first stop which was held for six hours under observation for signs of decompression symptoms. Once again, a single tissue model was used to predict the uptake of nitrogen and helium and the critical volume of gas was used to determine the marginal decompression 'pull' that could just be sustained. Remarkably this first safe stop was correctly predicted to within about 2 m of the final just-safe value in the vicinity of 10 m. This experiment actually supported not only the critical volume hypothesis for a gas mixture but also the single tissue approach and the nitrogen and helium uptake model.



## FURTHER DEVELOPMENTS

The overall approach in this paper has been to concentrate solely on the no-stop dive. This simple dive has provided great insight into comparing different decompression tables and converting these to a variety of other common situations. However, real diving almost invariably consists of a repetitive dive, as for example in the case of a shallow saturation storage depth, where a diver may typically complete an 8 hour welding shift, 8 hours off, 8 hours on and so on. At deeper storage depths, on helium plus 0.4 bar oxygen, he may make small upward and downward inspection excursion on this mixture about an oil well christmas tree or other deep installation. On top of this sort of profile there may also be the occasional deep excursion to retrieve an object from the ocean bed. The range of amplitudes and frequencies of such dives is evidently highly complex and very little is known about such situations.

As a first step, RNPL has recently released through the CIRIA Underwater Engineering Group certain excursion diving tables which are special subsets of the general case, and the latest ammendment to the USN Diving Manual sets out limited multiple excursion dive tables. Yet, it is a disturbing thought that by the time a complete understanding of decompression is at hand, oil wells and divers may have become as unworked as Cornish tin mines! Fortunately this cloud of uncertainty does not hang over shallow water maintenance, scientific and sports divers, where it is probable than when thermal insulation, convenient transport and communication become adequately developed for general underwater use, there will be renewed pressure to avoid tedious decompression. Here the most useful development would be repeat diving tables in the form of a digital calculator, capable of accepting any combination of exposures and initial conditions. Enough is now known to achieve this target in the next few years. Perhaps the ultimate goal will be an electronically controlled carburettor which will dose the diver with an optimal variable mixture of oxygen and inert gases to minimise his decompression!

However, the real gain from decompression (and compression) research will undoubtedly be in aiding the unravelling of fundamental physiological processes.

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