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 many 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 in 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 supersatu-
ration 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
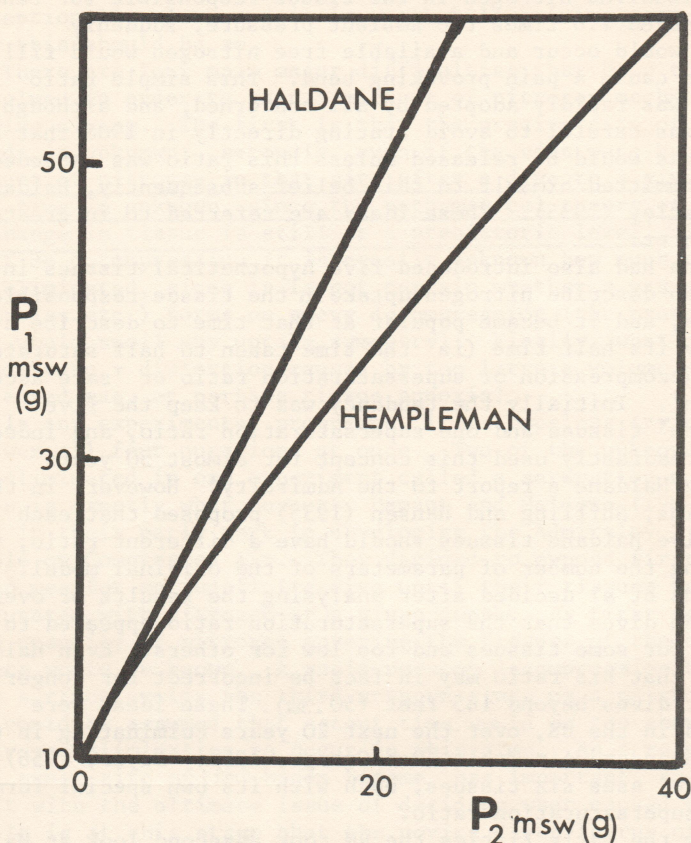(ie the ratio is lowered with increasing depth).

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 mètres 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 suspects, 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
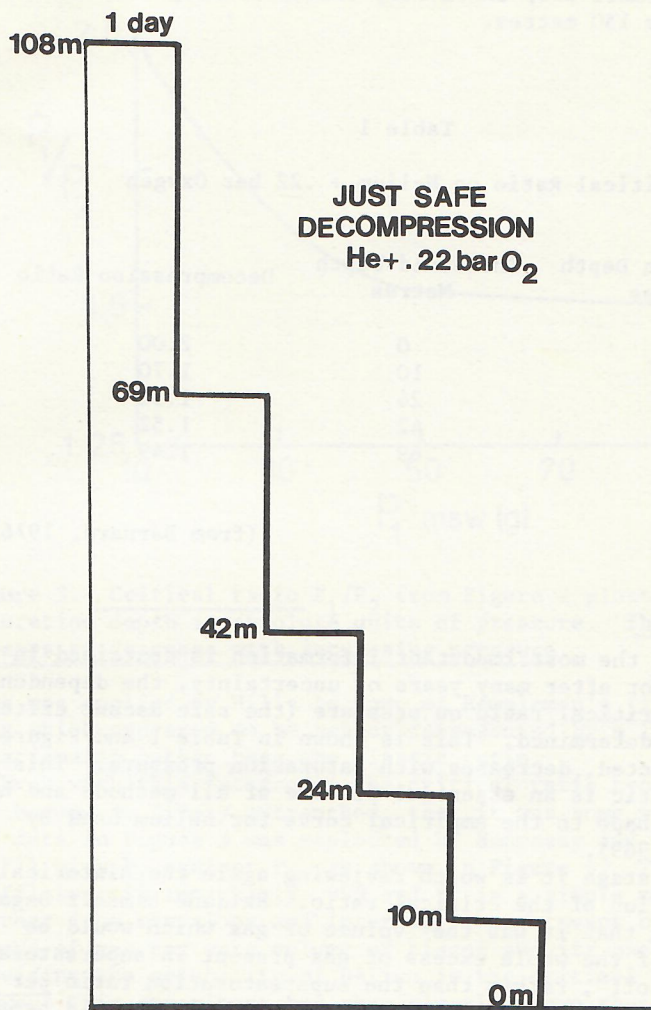at the 69 metre point because it was found that the 108 m depth

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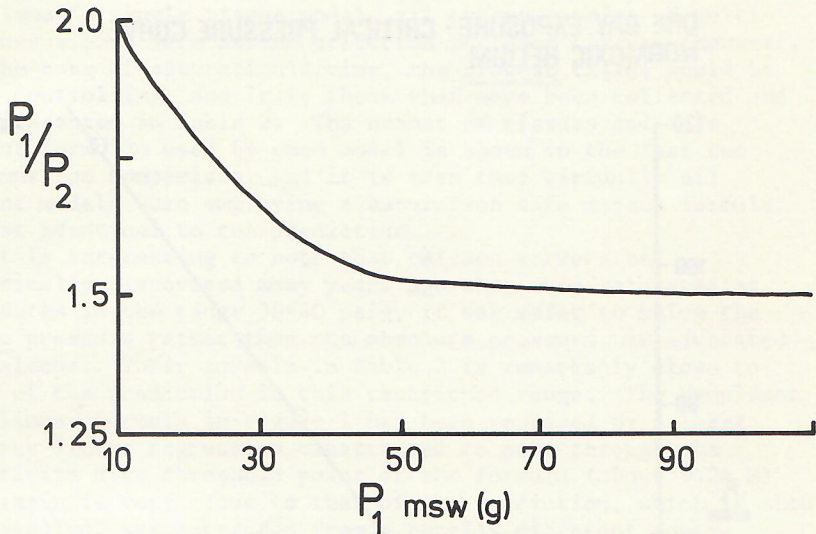
Figure 3. Critical ratio P_1/P_2 from Figure 2 plotted against saturation depth in absolute units of pressure. The ratio appears to decrease with increasing pressure.

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

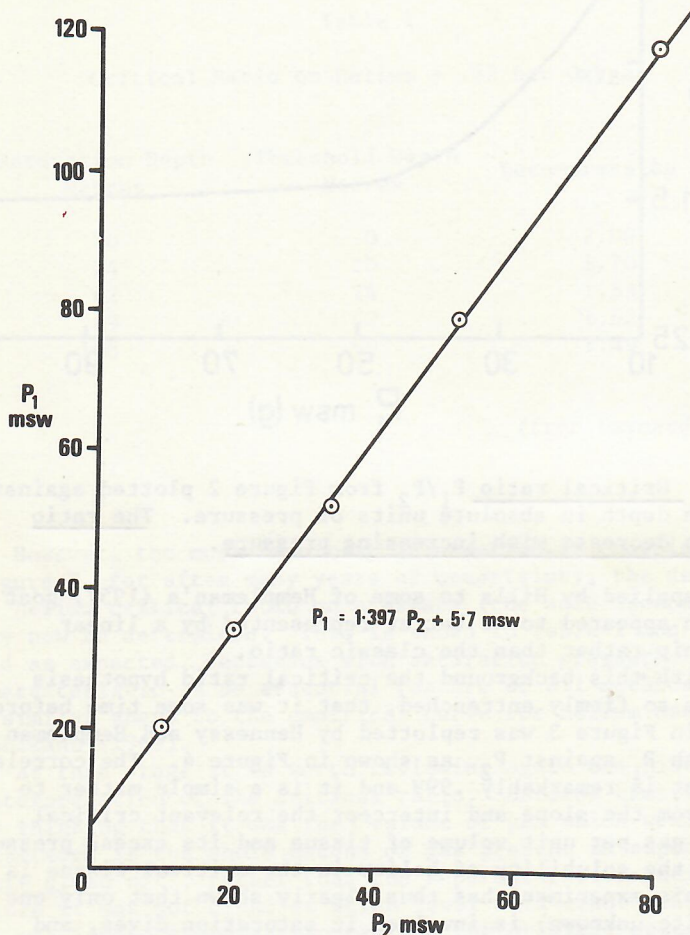## ONE DAY EXPOSURE - CRITICAL PRESSURE CURVE - NORMOXIC HELIUM

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 success-
ful 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 formula were originally published as non-
linear curves relating P_{N2}/P_2 against P_{N2} 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 et al 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) Equiv..ient 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_{N_2}/P_2 vs P_{N_2} (where $P_{N_2} = .8 P_1$), 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 $(=P_T) = \dfrac{1.385}{\overline{PS_A}}$ $\dfrac{}{1-13.7/P_T}$ where $P_T \sim P_1$ and $P_{S_A} \sim P_2$.

(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 $(=P_1/P_2) = \dfrac{400}{P_1+180}$
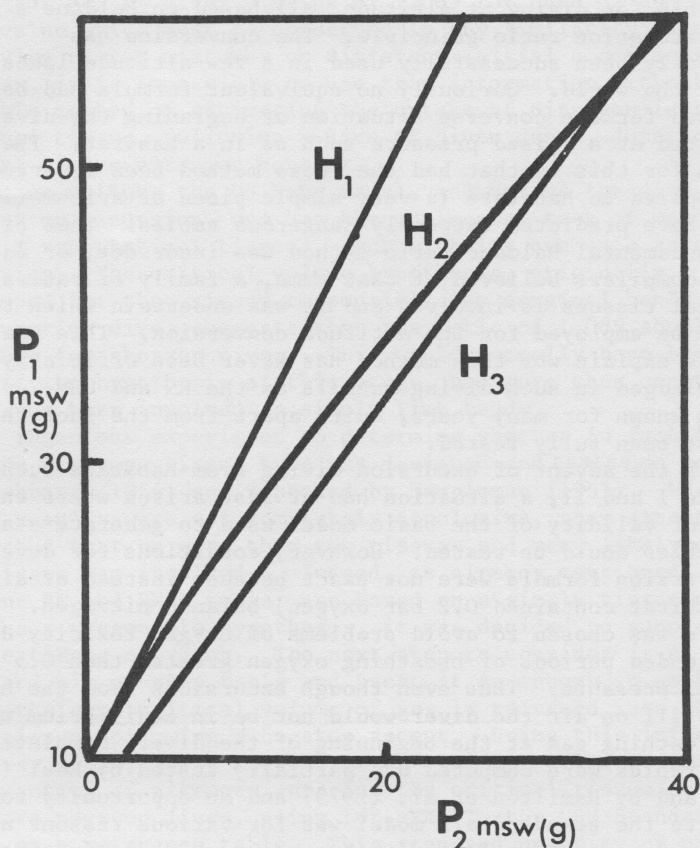passing through the no-stop threshold depth of
the formula.

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 short-comings 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_2 gives a value of 1.089 for pure N_2 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 (Boni 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 decom-
pression 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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