Opinion

Diving medicine: from art to pseudo-science

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Key words
General interest, diving scholars, underwater medicine, meetings, medical society

Thank you for the opportunity to address the SPUMS Annual Scientific Meeting, probably for my last time. The acorn that a few of us planted in 1971 has grown, and you have every reason to be proud of this development. Nevertheless, I have chosen a somewhat negative topic: How diving medicine has changed from an Art to a Pseudo-science. This presentation is an opinion piece, and not a scientific review. For alternative views you will need to consult the literature on each of the points I make.

You may feel that, in the process, I am somewhat displeased with the development of diving medicine in Australia and New Zealand, but this is not so. I am proud of what SPUMS has achieved. I have affection and admiration for what you have done for my favourite subject. I see erudite university departments that will lead us into the age of science. I could never have achieved this. With my diplomatic skills, I would have ended up as road kill on the academic highway.

Hyperbaric units have flourished. I was 'it' for a few years, but I was delighted to relinquish this obligation, passing the baton from my diving unit to the hospitals in Sydney and elsewhere by the 1970s. Now they are everywhere with facilities I would give my eye teeth for, offering the sort of service that I could only dream of. The prolific educators, such as Simon Mitchell in New Zealand and John Lippmann in Australia, have done a magnificent job, taking that load off my shoulders. Thank you – you are attempting a daunting but vital task. Also, there is a reason why I shall refer often to the *SPUMS Journal* in my presentation. That is because it is the pre-eminent source of clinical material in diving medicine. Successive editors have each improved on their predecessor; I was the first, so I am entitled to make that claim.

Medicine follows fashion. When I graduated, traditional medicine was an art. Now it is a science, and I admire both, but I am concerned that in diving medicine we may have fallen between two stools. Most advances in traditional medicine were made by astute, observant clinicians. Think of Charcot, Jenner, Osler, Gower and Pasteur, and in diving medicine, Al Behnke, my friend and mentor. They applied measurements and experiments to their clinical observations and anecdotes; they practised the art of medicine. Nowadays, anecdote is not considered real evidence, and the term is often used in a derogatory manner. Now we have evidence-based medicine (EBM), the science of statistics. A science steeped in epidemiology and integral to the concept of good therapeutic trials.

In the UK, where I worked for three years in the early 1960s, these very different approaches were exemplified by two breakthroughs in endocrinology. One was an extensive, meticulous, survey of pre-diabetes in the UK. It was science as we now know it, using hundreds of thousands of subjects, producing three volumes of results, unquestionably valid. The leader of the survey team warned in a postscript against presuming that the value of the survey was in any way equated to the effort involved. Separately, Sir Charles Dent, by carefully observing a handful of clinical cases, and modifying calcium and phosphate intake, in a few pages provided a superb exposition of the whole complex subject of hyperparathyroidism. My point is that both approaches have validity, and neither is inferior.

A disclaimer – I shall not talk about hyperbaric medicine. This is a developing science, led by the activities of researchers such as Mike Bennett, Des Gorman and others. However, when you view the EBM database created by Dr Bennett and his colleagues, you will be less than impressed by the few diving medicine inclusions.[1] There are seven reports available as of this date for perusal. Possibly two of these have some clinical relevance, although I suspect only one would comply with strict EBM criteria. Diving medicine does not yet have the data necessary and available to the hyperbaric medical specialists, who essentially deal in therapeutic trials, eminently suitable to EBM.

Between the art of medicine and the science of EBM there is a pretence to science, in which its tools are misused. These are:

- Statistics, and how to lie with them
- References, and how to misuse them
- Conferences, and how to manipulate them.

I admit to being guilty of each of these sins, but it is easier to criticise others than admit one's own mistakes; I leave that to others. My problem is, how do I illustrate this pseudo-science? There are two possibilities: my preferred orientation is clinical, so I could choose a case report to illustrate the distortion of traditional medicine; or I could analyse some diving medical research reports critically, using these scientific tools.

No matter which method I use, I will offend someone, so let us do both. I will review a simple case report and critically review diving medical articles somewhat pertaining to it.

A medical colleague died from pulmonary decompression sickness (DCS) during 'technical diving' training. He was moderately obese, middle-aged, and doing repetitive dives with reverse dive profiles (RDPs). He was using a state-of-the-art decompression meter (DCM) with an algorithm not validated for this type of diving.

Let us first tackle the pseudo-science of statistics. Death statistics are important, because if diving is a safe activity, then it does not warrant more vigorous medical examinations, fitness assessment, improved training, or safer equipment. For decades, the instructor organisations have promoted diving as a safe sport, quoting a death rate of 2–4 deaths per 100,000 divers per year. This rate was achieved with what I regard as 'creative' statistics, mainly exaggerating the denominator, that is, the number of active divers.

Monaghan, a recreational diving instructor with a doctorate in population statistics, blew the whistle on these deceptions, but the propaganda has continued.[2] Unfortunately we diving doctors have promulgated this deceit in our lectures and journals. This Society's journal, in non-peer reviewed articles, has aided and abetted this process, publishing misleading articles, often sourced from the diving industry. Such comments as "*Diving is safer than swimming and lawn bowls*", represent a selective use of inappropriate non-comparable populations that defies common sense. No one has been killed by a low-flying lawn bowl!

Several surveys from various countries balance this view with reported death rates of somewhere in the range of 15–30 per 100,000 divers per year (1.5–3 deaths per 100,000 dives).[3–5] Although wondrous, diving is a potentially dangerous activity and warrants attention to the factors that make it so. For this reason and others, I conclude that the RTSC questionnaire, widely accepted and used internationally, is not an adequate alternative to a competent diving medical. I have no problem with excitement-orientated adventurers diving, and possibly dying, as long as they appreciate the risks, and do not mislead or entice others, often younger and less capable. It is others, such as the diver described above, that concern me. Are we informing them of the real hazards, or are we acting as spruikers for the diving industry, promoting it as a safe activity? The vulnerable potential victims that come to mind include people with asthma, diabetes and other disabilities, and also diving children. The figures can be manipulated to make diving with these conditions appear safer than it really is.

Years ago, based on our diving accident cases and deaths, we concluded that asthma substantially increased the risk from diving. An asthma attack was more dangerous underwater, in the ocean, trying to get back to shore, than on land. However, our scientific brethren euphemistically referred to these reports as anecdotes. By careful selection of diving statistics, the perceived risks can be minimised. This is pseudo-science at work.

How?

- Inflate the apparent numbers of active divers – inflating the denominator, reducing the apparent significance of all pre-existing diseases.
- Exaggerate the prevalence of the disease in the diving population. How? By presuming that the incidence of asthma in divers is the same as that in the normal population. This ignores the natural selection of healthy candidates for diver training, and the demographic surveys of active divers.
- Designate as 'asthmatics' all those divers who had a past history of asthma in childhood – ignoring the expected and normal 50% reduction in active asthma as children mature. Including them reduces the apparent risk from this disease.
- Do retrospective or 'survivor' surveys. If you survey all current divers you will find that there is a zero incidence of diving deaths by shark attack, drowning and asthma – the 'healthy worker' effect.
- Dismiss the significance of asthma in the death reports. In such surveys as Project Stickybeak, a most valuable concept, observer bias is probably inevitable if there is only one assessor and no critic.

These are some of the ways pseudo-science can confuse or defuse important issues. Scepticism with statistics is healthy. You may not be able to do a discriminate function analysis, but you can at least use common sense and judgment.

Des Gorman has described the BSAC study of divers with asthma as "*a role model of how not to do such studies*". I agree. Yet it retains pride of place in most diving medical reviews on asthma. Why would reputable physicians promote this misuse of statistics? Some, especially if they identify with the diving industry, have a need to promote both themselves and their sport. Most simply re-quote figures in common circulation, even if they come from unsubstantiated information from the diving industry.

Many decades ago, a number of divers with asthma gave excellent descriptions of asthma attacks induced by diving situations. I reported on these trigger factors, but did not explore them further, except for salt-water aspiration. Recently, in the *SPUMS Journal*, exercise physiologists in Colorado verified our observations and demonstrated the additive effects of triggering asthma in those who breathed against the resistance of normal scuba regulators.[6] However, I do not think they fully appreciated the value of their observations. An excellent article in the *SPUMS Journal* by Sandy Anderson described the limitations of our provocative tests for asthma in general medicine.[7] Chemical agents (histamine) have variable potency; inhalants (hypertonic saline, mannitol) depend on lung distribution, and vary in their effect with ventilation and respiratory tract anatomy. Exercise, which you would think could be standardised, has an effect that varies with the degree of fitness. Here in the Colorado study, using respiratory resistance, we have a totally standardised, safe, controllable and variable dose/

response provocative stimulus. It costs virtually nothing, and is a potential area for future research.

There are some statistics I find simply unbelievable. Consider children divers. In our journal, it was stated that 2,215 open water dives were undertaken by children without a single incident – not even ear equalisation problems.[8] Much better than the 10–30% incidence in adult trainees, especially when one considers that young age is an increased risk factor for upper respiratory tract barotrauma. Even this was usurped by the 3.5 million open water SNUBA experiences, mainly children, without a single incident – again quoted in our journal.

These miracles of statistics can be achieved only by failing to ask the right questions, or any questions, and then presuming negative responses. You should look for and document evidence before you say it is not there. That is pseudo-science, it ignores the statistical mantra: "absence of evidence is not evidence of absence". If evidence is not collected, this does not mean it does not exist.

Let us move back to the art of medicine, and the second tool of pseudo-scientists: the misuse of references. The cause of death in the diver described is not in dispute. Anyone who has seen divers die of the 'chokes' would easily recognise it, but why did he die? Chokes is a rarity in recreational divers who follow tables, do no-decompression diving and make allowance for predisposing factors, such as age and weight. The standard tables themselves include safety margins, in excess of the mathematical models on which they were originally based. It was not rare in professionals, who did prolonged dives with extended decompression – like abalone divers, divers who pushed the safety envelope. Chorinsky, Babbington and Hall come to mind. Nowadays they all would be called 'technical' divers.

Now we have DCMs that allow you to dive right to the limits of the various theoretical decompression algorithms. Many 'tech' divers, like the diver described, place complete faith in them. However, he had some predisposing factors for DCS, namely obesity and age. For long dives, we advise obese divers to reduce their allowable bottom time. This practice of adding safety factors to compensate for risk factors in air divers was supported both by the theoretical argument, and experimental observations – that adipose tissue absorbs five to six times more nitrogen than aqueous tissues. The nitrogen load is increased in fat divers.

Paul Bert first observed this when emaciated dogs endured extreme hyperbaric exposures, but succumbed to DCS from the same exposures after they had been well fed. Support for this belief has come from many subsequent animal and human studies, in diving and caisson work, in the field and during experiments. In the first edition of *Bennett & Elliott* it was stated that "*Obesity favours death after long exposures*".

However, a couple of selected references (abstracts only), discrediting the importance of fatness, are now widely quoted in dive magazines and this belief is becoming fashionable, even in some current diving medical texts. These references are used to refute numerous earlier observations. In the most recent US Navy study, it is impossible to determine the decompression stress to which the divers were subjected, and attempts to unearth the original data have been unsuccessful. I was, however, able to find some similar studies by the same authors, showing the positive relationship between weight and DCS. In a USAF report, describing altitude exposure after two hours of pre-oxygenation, the air usage during ascent was not stipulated. How these conditions influence nitrogen liberation from medium or slow lipid tissues, is totally beyond my ability to calculate, and apparently that of the researchers, as they did not clarify it.

The third and earlier report quoted as discrediting the weight/DCS association, actually supports it, suggesting that those who quote it may not necessarily have read it.

All three series employed armed forces populations, presumably homogeneous, with relatively narrow spreads of obesity, i.e., small dispersion of the weight parameter, compared with the normal population. Brian Hills had warned us of this error in his text on decompression sickness.[9] You need a wide range of fatness, or large sample numbers, to illustrate its likely importance.

The best example of uncritical use of references is that of Cot and the concept of dry drowning.[10] This paradox had serious treatment implications for immersion injuries. It implies that laryngospasm keeps the lungs dry as the diver dies from asphyxia. It was claimed that up to 20% of cases fell into this category. I have attended many autopsies in drowning victims, and treated dozens of near drownings. Never have I encountered a case of 'dry drowning'. Gordon Dougherty told me that in all his animal experiments using aqueous Indian ink the lungs were always stained by the dye.

But the clinical experts all agreed: dry drowning was a reality. They quoted each other, and themselves. With the help of a translator colleague, I sourced the original data, a paper by a Dr C Cot in 1931. A figure of 10% (not 20%) was of dry lungs at autopsy obtained from dead dogs, fished from the Seine. There was no reason to believe that the dogs actually drowned; dead dogs often ended up, via the sewers, in the Seine. Dead dogs with aerated lungs float head down and thus are less likely to sink, or take in more water, and the lungs can remain dry.

Extrapolating these findings to human immersion incidents is simply not warranted. Also, the Seine is fresh water. We all know that fresh water is absorbed rapidly from the lungs post-mortem, and during resuscitation of humans. The experts relied on autopsy reports in humans to make the retrospective diagnosis of dry drowning, and most of their cases were probably from fresh water. Dry drowning,

it is now agreed, is a post-mortem artefact. It arose from a failure to read and critically review the original article that was then re-quoted for 70 years.

Let me summarise some of the problems with references and pseudo-science:

- References that have not been read should not be quoted.
- All references are not equal; use your judgment. If they are misleading or incomplete they should not be used, or their use should be qualified. Otherwise, you are misleading your audience.
- Abstracts and preliminary reports may support a belief, but they are not research evidence. They are opinions, no more and no less, and not usually peer reviewed or critically assessed as regards their validity.
- Data have to be transparent and available. Salesmen and pseudo-scientists in the diving industry often claim vast numbers of dives using a certain computer, or a certain table, or specific training, or a certain technique. This is usually a retrospective guestimate, with a presumption of safety. Chase references back to their source to see how robust and documented they are.
- Internet searches can be a trap. The interesting review paper by Mouret on obesity and DCS, a paper with whose conclusions I agree, illustrates this.[11] Almost half of her references were from the Internet – from sites that are not subject to peer review, and which may not even be available for perusal a few weeks later, or in which the data may be altered retrospectively. These are unsupported opinions, no matter how much one agrees with them.
- Many recent publications fall into the trap of using only Medline-type searches. The authors have ignored the wealth of material available in textbooks, monographs, conferences or theses and any research prior to 1961. The establishment of the UHMS collection at the Duke University library will be a great help in this, as will the Rubicon Foundation collection.
- A more difficult problem to overcome is that of genuine but unintentionally misleading references.

This last point is exemplified in the condition known as 'Taravana', DCS from breath-hold diving. The subject is very topical, but the reasons for believing in the story keep changing. The paper by Bob Wong on Taravana, in the *SPUMS Journal*, is a good, comprehensive review, faithfully representing the opinions of many workers.[12] However, the whole concept really rests on the validity of the case reports – and these are few and problematic. The opinions and the explanations are numerous, but, I believe, are largely based on misleading data.

Cross, the discoverer, was a master diver and a good one. He was not a physician or physiologist. He described in *Skin Diver Magazine* some incidents from the Tuamotos. Unfortunately some avant-garde decompression modellers convinced him that he was describing DCS. Beware of ultra-specialisation. To an enthusiast with a hammer, everything looks like a nail. Cross was good enough to let me peruse the case reports some years ago. Diseases such as hyperventilation hypoxia, salt-water aspiration syndrome, inner ear barotrauma, some marine animal injuries and the causes of vertigo in diving explained most. These conditions were not known when Taravana was first observed. Cross's cases include many obvious non-DCS cases. The reason for the wide range of clinical manifestations was probably the wide range of diseases lumped together under the Taravana cloak – because they occurred in the same area. You could similarly describe a Tutukaka Syndrome, grouping the problems noted during this week of diving at Tutukaka.

Another group of cases were described, ostensibly to clinch the Taravana concept, by Paulev – then by Wong himself – in submarine escape training 'breath-hold' divers. They developed DCS. True, they did, but they are not breath-hold divers in the same sense. Although these divers do conduct multiple breath-hold dives to escort their submariners to the surface, that is not all they do. They also breathe compressed air, just like scuba divers. The breath-hold divers often have to wait for the submariners, who sometimes take time to prepare themselves. Where do they wait? In small compressed air bubbles (blisters) built into the escape tank at various depths, or in submerged bells. So it is breath-hold diving, but it is combined with compressed air breathing. Unless you had experienced this type of diving, you would not be aware of this, or its importance, and it is not highlighted in the published reports.

The final truth about this possible disease may come from the recent Ama work, but I wonder why these divers, who have been exposed to 50 years of intense study without Taravana being reported, suddenly have cases of DCS being reported by the same authors who misled us regarding submarine escape divers. These cases need to be independently and critically reviewed. The message? Before you explain or model anything, you must first verify that it exists.

The next tool of pseudo-science is the consensus conference. The diver described above died after a series of repetitive dives with reverse profiles (RDPs), with decompression based on a contemporary computer algorithm (DCM), which had never been validated for this type of RDP diving exposure. He would never have got away with doing these dives decades earlier. He would have been protected by old-fashioned protocols advising no-deco dives, adding safety margins and avoiding RDPs. Conventional practice had been circumvented by a confidence in computers and a popular but incorrect interpretation of a consensus conference.[13]

I will not discuss DCMs here, but I will refer to the consensus conference about which I have previously expressed the strong view that it was held to influence behaviour, but in the absence of evidence. A sort of verbal meta-analysis, but without data. Ed Lanphier warned us "*Truth should not be determined by voting*".

How did we move from the art of medicine to a pseudo-science of consensus? The original workshops of the Undersea Medical Society were superb. Closed clinical meetings, attendance by invitation to a small group of experts from various disciplines and experienced clinicians who exchanged information, tried to understand and clarify problems, considered possible solutions and proposed avenues for productive research. There was no posturing and no lobbying. Indeed, there was no audience. There was no imposition or coercion in the workshops or in their publications. If there was disagreement, we agreed to disagree.

In the early 1990s, market forces intervened and the workshops mutated into commercially orchestrated meetings. Examples of these in diving medicine include the workshops on the terminology of decompression illness, asthma, DCMs, and RDPs. The main problem with these conferences is an implied obligation on delegates to reach consensus. Rarely do they agree to disagree, which for me is the basis of intellectual freedom. There is often a predetermined position, and although opposition may be recorded in the transcripts it is often overridden in the conclusions. A powerful chairman or the more eloquent delegates may impose their views. Whoever wields the pen that defines the recommendations, wields the power. These are what are quoted henceforth, not the voluminous transcripts, which are rarely read. There is an added bonus for the conveners, implying expert status, without their actually doing any original work in the field. There are exceptions: consensus conferences that actually contribute to medical knowledge.

In conclusion, please remember that I have been guilty of *all* these pseudo-science misdemeanours during my career.

I wish to acknowledge two groups of diving contributors. So many of our Australasian achievements have evolved from non-experts, part-timers and enthusiasts: Bob Thomas, Doug Walker, Noel Roydhouse, Alan Sutherland, Jack Barnes and many others. These are the unsung heroes of diving medicine. Most of all, I acknowledge the divers who have shared a spectacular world, given life-long friendships, and described their illnesses, which I translated into medical terminology, some agreeing to be clinical guinea pigs. They made my professional life much fuller, more productive and colourful.

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Dr Carl Edmonds (right) receiving a presentation at the SPUMS ASM 2007. Dr Mike Bennett, on behalf of the Society, presented him with a beautifully embossed leather-bound copy of the fourth edition of Edmonds et al, *Diving and Subaqautic Medicine.*