The issue of statistical significance appears to be one which can be argued endlessly. It would be desirable to test to conventional levels of statistical reliability or higher, but the requirements of the vast number of trials makes this virtually cost prohibitive. Moreover, it has never been accomplished to date by anyone within the field of hyperbaric science. Previous testing practices in the field may reflect this difficulty. For example:

(a) Haldane tested his schedule twice.

(b) Initially, the US Navy tested their standard air schedules four times. During the 70s, commercial schedules were tested 12 times and more recent programs have used 20 to 40 tests.

These facts are reported in a paper written by Drs. Bennett and Vann of the F.G. Hall Laboratory in Duke Medical Center entitled "Development and Validation of Deep Bounce and Other Decompression Procedures In The Laboratory". (It is significant to note that DSAT (Duke University Saturation Diving) research tested approximately 500 manned dives, far more than any other tests of this nature.) Drs. Bennett and Vann went on to state that when few tests are conducted, it is essential to achieve the greatest assurance of safety. This can only occur when no decompression sickness incidents are allowed, such as in the DSAT study.

The testing of decompression procedures involves validation of a decompression table which contains many different schedules. It is impractical to test every profile in the DSAT table which has over 36,000 possibilities.

Decompression, as you well know, is highly complex. There are many variables to consider such as the diver himself, the patterns of diving, and the table design. These factors make table validation of a major problem; the medical community tried to address this as recently as 1987 in a UMS conference with no firm consensus.

Within the framework of the mathematical models, decompression sickness becomes a statistical phenomenon. As a result, it is not possible to design a practical table that is 100% safe for 100% of the people 100% of the time. This is commonly known.

To totally eliminate all risk of decompression sickness, one would have to avoid diving altogether or, once having decended, never surface. Obviously, neither alternative is practical. Furthermore, to design a testing process that would define limits for everyone, everyone would have to be tested. Every man, woman and child would have to be tested every day. (Obviously, this would no longer be a test. As a result, the number of test dives used by table developers can never be perfect.) Because people differ in susceptibility to decompression sickness, no decompression table can guarantee that decompression sickness will never occur, even though the diver dives within the table limits. All this is, of course, clear to you.

PADI feels that the diver education community shares a responsibility with the medical community to provide recreational divers (who now number in the millions versus commercial divers who number in the thousands) with the very best set of tables, both in terms of safety and utility, that current technology and available resources can produce, to accommodate the type of diving (ie. no decompression repetitive dives) these people are already doing.

The data resulting from the testing which produced the DSAT tables show a better approach to this problem than military or commercial tables. There was an obvious need for a better table for recreational scuba activity. The data tested at the Institute of Applied Physiology and Medicine in Seattle did not appear de novo but rather from a logical extension of earlier information.

As an additional point for consideration, this research was the first of an ongoing series of research projects DSAT has planned. DSAT, in close connection with the North American scientific community, is formulating a study that would extend the research recently completed. The study would investigate the effect of using the algorithms on which the recreational dive planner is based in situations where divers dive repetitively for many days such as during a live aboard boat vacation. This test has already been designed and is currently being submitted for review by a panel of hyperbaric experts from the United States and Canada. We expect the chamber phase of the test to begin within the next month.

Thank you again for your comments and suggestions. I'll look forward to furthering our communications on this and future projects.

Drew Richardson
Training Manager

The Christchurch Clinical School of Medicine,
University of Otago,
P.O. Box 4345,
Christchurch,
New Zealand.

9 November 1988

Dear Sir,

In recent issues of the SPUMS Journal and the UHMS magazine "Pressure" there has been passing reference to the new PADI (Professional Association of Diving Instructors) diving tables that are currently being introduced internationally. All these references appear derogatory and I am particularly concerned about David Davies' comment that both Brian Hills and Des Gorman condemned the findings of the research on which these new diving tables are based as being unscientific.

I have also reviewed the available evidence on these tables and written a lengthy report to New Zealand Underwater Association (NZUA)/PADI in New Zealand expressing my own concerns at the lack of scientific validity. Whilst my efforts have been acknowledged and appreciated, NZUA/PADI have chosen to proceed with the marketing of these tables in conjunction with their parent body despite the expressed concerns. Is this also the Australian experience?

If so, what should we be doing about it as a professional body? It would seem to me that a completely new set of tables is being introduced to sport diving on the basis of inadequate scientific validation. Brian Sayer of NZUA/PADI recently informed me of new major trials that are underway, and I understand that Dr Des Gorman has offered also to test these tables in the laboratory facilities at Adelaide. Is this not putting the cart before the horse? Should not tables be fully validated before their release rather than afterwards? We have had numerous examples of this in recent years what with the Huggins Tables, the Bassett Tables and so on. In fact the whole issue begs the question of what is appropriate scientific validation of a table. Weathersby and his colleagues at the US Naval Medical Research Institute (NMRI) have suggested that this can only be done statistically.

Perhaps the pages of the SPUMS Journal are an appropriate vehicle to allow PADI and others to express their views on such an important topic. I personally remain firm in my assessment that, as they stand, these tables lack scientific validity.

On a personal note I adopted the Canadian Defefence and Civil Institute of Environmental Medicine (DCIEM) tables for my own use early in 1987 since the overall evidence, as I understand it, is that these are currently the most conservative repetitive dive tables available. Of course, even with these tables the old maxim of 'one longer and/or one deeper' still applies.

F. Michael Davis
Senior Lecturer in Anaesthesia

REVISITING KEY WEST SCUBA DISEASE

19 Otahuri Crescent,
Greenlane,
Auckland 5,
New Zealand.

30th January, 1989

Dear Sir,

Robert Wong presents a case report of a diver suffering a systemic illness with major effects localised to the lung characterised by breathlessness, a reduced carbon monoxide diffusing capacity and a fine granular pattern chest X-ray (*SPUMS J* 1988; 18: (4) 124-125). The diagnosis of Legionella pneumophilia is made solely on clinical grounds supported by serology.

The serological response is worthy of comment in that a polyclonal response is shown with 4 fold rises in Gp.1, Gp.3, Gp.4, and Gp.6. I think this is far more likely to be a general stimulation of the immune system such as may occur after many infectious and non-infectious illnesses, rather than infection with several serotypes of Legionella, or cross-reactivity between these sub-types. A 'diffuse granular' chest X-ray is an unusual appearance in Legionella infections, but is seen frequently in hypersensitivity lung disease or adult respiratory distress syndrome both of which may occur as a consequence of aspiration. I suspect a transbronchial lung biopsy could not be justified in view of the patients improvement, but would have provided valuable data.

In the early investigation of Legionella pneumophilia the organism was isolated from stored frozen autopsy lung obtained from a diver who died in the late 1950s of a pneumonic illness. I have not been able to locate the reference to this however.

I think the case for Legionella pneumophilia is unproven on the available data.

I would be interested in Carl Edmonds views and also those of an Immunologist.

A.G. Veale,
Secretary/Treasurer,
NZ Chapter SPUMS .

JELLYFISH ENVENOMATION; WHAT DIVING MEDICAL PHYSICIANS SHOULD KNOW

International Consortium forJellyfish Stings,
MSO Box 5695,
Townsville,
Queensland, 4810

January 27, 1989

Dear Sir,

I write to correct what may be an ambiguous statement in my paper (*SPUMS J* 1988; 18: 118-121), under the sub-heading "Analgesia", on page 120. The possibly misleading statement reads "It" (i.e. pain) "is also unquestionably relieved by the specific antivenom for *Chironex*".

It is important for your readers not to misinterpret this statement to imply that the *Chironex* specific antivenom is beneficial for the pain of any jellyfish sting. Our present understanding, based admittedly on only a relatively small