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EARLY INDICATIONS OF BEHAVIORAL AND PHYSIOLOGICAL DYSFUNCTIONING IN DEEP DIVES

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EARLY INDICATORS OF BEHAVIORAL AND PHYSIOLOGICAL DYSFUNCTIONING IN DEEP DIVES

Third Undersea Medical Society Workshop

Co-Chairmen: Dr. Arthur J. Bachrach
Dr. Mark E. Bradley

Topic Chairmen: PERFORMANCE--Dr. Glen Egstrom
HIGH PRESSURE NERVOUS SYNDROME--Dr. Peter B. Bennett
VESTIBULAR SYMPTOMATOLOGY--Dr. Robert S. Kennedy
ELECTROENCEPHALOGRAPHY--Dr. Paul Naitoh

Participants:

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(Defence and Civil Institute of Environmental Medicine [DCIEM])

Dr. Arthur J. Bachrach
(Naval Medical Research Institute [NMRI])

Dr. Peter Bennett
(Duke University Medical Center)

Dr. Robert J. Biersner (LCDR, MSC, USN)
(Now at Naval Training Support Command [NTC])

Dr. Beverly Bishop
(State University of New York at Buffalo [SUNY/B])

Dr. Robert C. Bornmann
(Bureau of Medicine and Surgery [BUMED])

Dr. Mark E. Bradley
(Now at Naval Medical Research Institute [NMRI])

Dr. Ralph Brauer
(IMBR, University of North Carolina at Wilmington)

Mr. Kenneth Conda (Now deceased)
(Naval Medical Research Institute [NMRI])

Dr. Carl Edmonds
(Royal Australian Navy, HMS Penguin)

Dr. Glen Egstrom
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Dr. David H. Elliott
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Dr. Dorothy E. Fletcher
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Dr. James G. Holland
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Dr. John Kanwisher
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Dr. Robert Kennedy (LCDR, MSC, USN)
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Miss Suzanne Kronheim
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(Navy Medical Neuropsychiatric Research Unit [NPU])

Dr. Donald McMillan (LCDR, MSC, USN)
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Dr. Paul Naitoh
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Dr. John S. P. Rawlins
(Ministry of Defence MDG(N), England)

Mr. James R. Stewart
(Scripps Institution of Oceanography)

Dr. Robert H. Wilcox
(Naval Medical Research Institute [NMRI])

N O T E

The following write-up is a narrative account of discussions based on brief presentations by participants.

Sessions 1 & 2: 3 May 1973

Topic Chairmen: Dr. Glen Egstrom (Performance)
University of California at Los Angeles

Dr. Peter B. Bennett (High Pressure Nervous Syndrom [HPNS])
Duke University Medical Center

The Workshop was opened by Dr. Bachrach, who introduced the first speakers, Mr. Ken Conda and Mr. James Stewart, both active operational divers. Mr. Conda began the discussion of problems of operational diving and the needs for research in various problem areas. He emphasized problems of operating in cold dark waters, problems of vision, and problems imposed upon a diver by equipment, dealing primarily with hard-hat and saturation divers. Mr. Stewart, from Scripps, indicated his problem area to be primarily that of open-circuit scuba diving for scientific purposes, such as physical oceanography and marine biology. Stewart emphasized the problem of divers observing novel events and not recording them--which suggests that even scientific divers overlook effects of the environment (e.g., possible narcosis) while concentrating on their mission. Physical problems discussed by Stewart were visual restriction (as tunnel vision or perceptual narrowing), respiratory fatigue (especially during heavy work), and problems with reading dials and gauges under cold dark-water conditions. He noted that some tremors have occurred in scuba divers of their group, but not to the extent that the problem is seen in deeper dives. A major problem for the scientific diver is that observations made on the bottom are frequently lost because of the short-term memory he experiences when he hits the surface and begins to record. Mr. Stewart also discussed the occurrence of vertigo on surfacing, lasting several seconds, involving full disorientation and occasionally nausea. Some divers report the

"uglies," involving sudden onset of symptoms of lack of eye focus and vertigo.

Dr. Egstrom continued with a preliminary discussion of problems of environment and diver interaction, concentrating on the observations that adaptations made by the diver are highly specific to the equipment and to the environment. The diver's psychological readiness is specific; he is ready to accept specific, not general, stresses. Dr. Egstrom observed that the prime limitation of solving engineering problems topside is that the equipment has not been tested in precisely the environment and under the work conditions where it will be used. This presents problems of objective measurement. Methods that have been developed for "field" testing include the pipe puzzle, involving moderate exercise, and the fin-swimming ergometer. Other factors such as possibilities of narcosos, emotional stability, and repetitions of exposure in cold are obviously relevant.

Dr. Egstrom stressed the need to build a set of predictions based on sufficient testing and observation of divers under work conditions. One relatively simple and critical measurement is pulse rate (frequently used to determine changes in diver performance); if pulse rate is monitored alone, any extreme variation seen in it may be a clue that there is something that should be attended to. Dr. Egstrom observed that one of the important parameters of diver-performance assessment is the task itself; its simplicity or complexity and particularly, how well it has been learned. Comparing different kinds of tasks under such conditions as cold can provide information that will allow us to predict performance. He suggested that

possibilities exist of correlating changes in blood pressure or pulse rate with symptoms of the high pressure nervous syndrome (HPNS) such as tremor.

Dr. Bennett responded to this by talking about the need for uniformity of testing, the problem of being able to relate one's results to those obtained by others, and the need for agreement upon which test to use. Dr. Egstrom, stressing the need for standardization of assissment, pointed out that the operational needs determine one approach to problems of diver performance, while other more "esoteric" considerations, such as research into neurophysiology, determine others.

Dr. Bachrach was asked to define some of the parameters of diver performance and suggested it was critical to get a definition for terms that are used interchangeably, but not always correctly. He suggested that various terms be defined so that each individual reporting could achieve the kind of mutual understanding that Dr. Bennett was striving for. For example, dysfunction is a general term indicating a breakdown in normal performance. Decrement is defined as a transitory state, a decline that is potentially reversible; so, one may talk about a decrement from a normal baseline indicating some deterioration, which is impairment (also transitory), a process of change worsening from a baseline. Deficiency is a lack of a particular function, while deficit is a loss, presumably irreversible. Thus, Dr. Bachrach suggested, an impairment of hearing is a deterioration from a normal baseline. A deficit in hearing would indicate a hearing loss, presumably as a result of nerve deafness or some similar change, which is irreversible. Therefore, we can talk about a baseline with a hearing decrement or impairment which is transitory, to be reversed upon return to the surface or other change.

Deterioration is the specific process by which the change or decrement occurs, and deficit is the irreversible loss that may occur as a result of the environmental conditions that impose the deterioration and decrement. Dr. Bachrach indicated he hoped that this was not merely a semantic exercise, but that the terms deficit, decrement, and deterioration would be used a bit more clearly in research communication.

Responding, Dr. Egstrom reminded the group that it was important to recall that our monitoring interests are those that would give us an understanding of what kind of deteriorations occur that ultimately result in deficit, and "...if carried to its end point, would result in collapse." Dr. Bennett and Dr. Ackles discussed another monitoring device or technique: the evoked potential; Dr. Ackles indicated that, individually, decrease in performance did not necessarily correlate with evoked responses. Dr. Bennett wondered if the technique might be sharpened.

Dr. Egstrom returned to a discussion of different populations, relating the topic to simulated versus open-sea environments. He reported on research with scuba divers in a classroom situation, which began in a clear warm-water tank situation, then moved to an open-sea environment off Marineland pier. The substantially colder sea water, with increased turbidity and some marine life, split the population into what was classified as "steadies" and nonsteadies." The latter responded by decreased performance.

Dr. Bachrach asked Dr. Biersner to comment on SINDBAD, which has been proposed as a standardized assessment of diver performance. Dr. Biersner emphasized that SINDBAD was designed to measure basic abilities as defined

by the Fleishman factor-analytic model. These abilities are supposedly central to many kinds of complex performance tasks and impairment in any one of them should be generalizable to the family of tasks in which these abilities have significant loading. He noted that the tests included in SINDBAD I are largely psychomotor and cognitive. Little was done to test for affective characteristics directly, although this information might be obtained indirectly from the available tests--usually in the form of increased performance variability. The conveniences of the system were outlined, especially portability and the minimal interface problems that have been encountered with existing chamber complexes. The problem of helium expansion in the response module used by divers inside the chamber was eventually solved, he noted, by placing one-way valves in tubes above the module. The valves permitted helium to escape during decompression, thereby avoiding expansion of the module case, as well as loss of silicone fluid. The silicone bathes magnetic switches inside the module case and eliminates any fire or explosion hazard that might be associated with faulty activation of the switches. The advantages of the system in providing automatic timing, summing, and display of responses was mentioned, as well as plans to put these performance measures directly on computer cards from the display console. Dr. Biersner emphasized that responses to the tests presented by this system cannot usually be compared to similar tests given under normal conditions because the responses are made manually, not verbally. Pre- and post-dive control measures are, therefore, essential. The discussion about the applicability of SINDBAD to a large variety of diving conditions suggested that validation of the 26 available tests

in the current SINDBAD program would be necessary. Generalizability from chamber to open sea, which had been discussed previously by Dr. Egstrom, also came into question. What specific tests have the greatest applicability to predicting diver performance remains a crucial question.

Dr. Rawlins returned to the question of pulse rate and respiration, asking if communication directly with the diver was not a good monitoring device. A discussion among Drs. Egstrom, Kennedy, and Rawlins ensued, in which the question of verbal report of the diver was considered--whether the diver was reluctant to answer in any other way than "O.K." Dr. Egstrom questioned how often you could ask a question of the diver to get an early indicator of potential change in his performance. Dr. Rawlins indicated that you get a great deal of information from the way a man talks. This opinion was generally accepted. Dr. Egstrom gave an example of a diver in an oil patch who responded that he was well, possibly because he was unwilling to give up the dive, yet he ran into extreme difficulties that induced paralysis from his hips down, because of his unwillingness to report the pain early enough in the ascent.

More discussion ensued about such monitoring devices as the electrocardiograph (EKG), which was considered to be a complicated monitoring device that may have no more reliability as an indicator of change than simple pulse rate. Pulse rate seems to have the advantage of simplicity in transmittal and reception. A discussion of perceptual narrowing as a monitoring device for visual changes led to the conclusion that this may be a promising parameter. Dr. Bradley returned to operational parameters and their comparison with chamber environments, indicating that

there are constraints of obtaining information from a working diver, and from the pragmatic standpoint of early indicators such as pulse rate, respiratory frequency, et cetera, he wondered whether an operational situation could be assessed unless the diver were given a task that was not natural to him. He suggested that for controlled research, perhaps the chamber situation with its multiplicity of variables was a crucial environment for getting into the operational situation. Dr. Kennedy thought that the operational environment delineated the problems that should be looked at in a chamber. Dr. Bachrach indicated that researchers should simulate the ocean conditions as closely as possible, and Dr. Kennedy responded that he thought you could measure the effects of cold without going to great depth to study the interaction of cold and depth--perhaps one might want to separate one operational parameter from another. Dr. Egstrom recognized the better control situation of the chamber, but indicated less concern with individuals having problems in a chamber than those having problems in the open sea. He wondered whether generalizations made from a chamber environment are the same kinds of information that would apply to diving in an open-sea environment.

Dr. Ackles described experiments on narcosis in his laboratory, indicating that they were working on short- and long-term memory (recall storage). He expressed the belief that it's better to work on the underlying dysfunction than to depend upon the diver's verbal report. He noted that heart rate was a good measure and asked if it were possible to develop a biofeedback system to control a diver's heart rate. The diver would have information regarding his heart rate, perhaps related to resting control, and perhaps controlled by the diver himself.

Dr. Egstrom asked Dr. Bennett to begin a discussion of the HPNS. Dr. Bennett indicated that a definition of HPNS signs and symptoms and the means for measuring them were crucial. Dr. Bachrach described the force transducers by which microtremor was measured in the Makai, Westinghouse, Pennsylvania, and Duke dives as well as the Taylor dive, which was ongoing at the time of the Workshop. Dr. Bennett defined tremor in terms of intentional tremor and postural tremor.

Dr. Brauer stated he believed that the human syndrome of HPNS was somewhat vague, but the animal situation was clearer. He said that the term high pressure nervous syndrome describes a series of phenomena that start with tremor and culminate in convulsive seizures. These events are quite reproducible in several species. Dr. Brauer suggested that the results they obtained on animals are not a factor of impaired respiratory phenomena, not CO_2 accumulation, and not a temperature phenomenon. He believes that a HPNS separates itself from the early tremors erroneously referred to as helium narcosis by certain previous workers. The same phenomena of the HPNS can be reproduced in a complete absence of helium. So, he believes that it is safe to assume that we are looking at the effects of a pressure-link; noting, however, that it may not be merely pressure as such, but that the time rate of pressure change may constitute an important element in defining the pattern of neurological change.

A series of slides illustrated data relating tremor to convulsion. Dr. Brauer demonstrated on mouse data the relationship he believed to exist between compression rate and convulsion: that absolute pressure and rate of compression are both involved in convulsion. He also was wondering

about temperature effect on tremor and convulsion threshold. His lizard data suggest that in the range of 20° to 35°C there is no temperature dependence at all, or a very minute drop in tremor threshold. Tremors, in his data, are observed and tend to come on around 60% of the pressure at which animals begin to convulse. He also suggested that in squirrel and rhesus monkeys neither respiratory rates nor heart rates show any clear correlation with the development of the HPNS.

Returning to human symptomatology, Dr. Kennedy observed that the incidence of nausea is noted frequently in deep dives. Dr. Bennett responded that in the RNPL dives to 600 or 800 ft the divers complained of being dizzy. Dr. Brauer was asked how he could detail the measurement of the tremors that they observed in the animals. Dr. Brauer responded that they did not measure the tremor; a visual observation was made of the point where intensive locomotive disturbances showed up (usually in the neck muscles) and of the foot, where visible tremors would begin to appear.

Dr. Hendricks asked Dr. McMillan to relate their experiences with the HPNS during a series of dives to evaluate the Mark II system. The dives ranged from 100 to 850 ft, with an excursion to 980 ft. Dr. McMillan indicated over 25 dives were made in the open sea to about 180 ft. During these dives no one mentioned a tremor or possible neurological problem. Later the divers made two dives to 850 ft; one included an excursion wherein the divers exited the capsule at 150 ft and swam to 945 ft. They were in the water the entire time of the excursion, yet reported no symptoms of tremor-related problems, possibly because they were concerned with breathing difficulties. In the second dive (in the capsule) the capsule operator was first to note that from 850 to 980 ft they were aware of an intention-type tremor of the extremities. At first it was more irritating than disabling.

The divers noticed the same problem to a more severe degree, including foot tremors, which remained until the capsule returned to 850 ft. Objective observation indicated the tremors dissipated within about 60 minutes on all divers. The two divers who exited at 980 ft had significant problems with breathing apparatus--the one mounting a camera was unable to operate it; their cognitive ability seemed to be hampered.

Dr. Brauer commented on some electromyographic tracings made in the course of the collaborative work at COMEX, which suggested that in subjects where marked HPNS changes could be recognized there was some evidence of suspension of reciprocal inhibition of opposing muscles involved in precise movements. He emphasized that as of now these constitute an isolated operation and testing and verification would seem worthwhile.

Discussion ensued between Drs. Bennett and Brauer about other signs of impending difficulties in diving. The question was raised about cardiovascular relationships to tremor. Dr. Brauer indicated that in the rhesus monkey no consistent blood pressure or electrocardiographic changes had been detected up to the point of actual seizures. It was suggested that the EEG may be more responsive to change and more revealing than EKG in monitoring compression.

Dr. Bennett returned to the problem of tremor and comparing intention and postural tremor. He questioned whether postural tremor might be more sensitive than any other tremor and to a degree, perhaps, even more sensitive than muscle wave function. Dr. Bachrach discussed intention tremor, particularly the condition of rigidity, indicating they haven't measured rigidity or, indeed, questioned whether rigidity is a problem in the HPNS.

He noted that in such problems as Parkinsonism the onset of tremor may actually indicate an improvement from rigidity. Dr. Kennedy asked whether postural tremor would not also have a vestibular indicator and whether both of them would not have a cardiographic component.

Dr. Bishop commented about reflex research, beginning with a discussion of the tendon jerk, which is the simplest reflex that can be elicited manually. She noted that in experimental chamber dives to 3 atm breathing a normoxic nitrox mixture, a mechanically-elicited Achilles tendon reflex was depressed, while an equivalent electrically-stimulated H response was increased. She suggested that the large motor system (alpha) in man is less susceptible than the small motor system (gamma) to the effects of hyperbaric pressure and perhaps different gas mixtures. Dr. Bishop believes that the depression of the Achilles tendon reflex in response to compression is an indication that during compression the gamma motor neurons are less excitable. The H response is primarily bypassing the gamma cells; therefore information about the large motor neurons suggests that they are hyperexcited. Something in the nervous system has either removed normal inhibitory inputs to them, she suggested, or there is some control loss.

A discussion about the meaning of these events in relation to tremor ensued among Drs. Bennett, Brauer, Bachrach, and Bishop. Dr. Bishop also observed that tremor, or any repetitive movement of that sort, is an obvious sign that control of motor systems is out of adjustment; as fatigue sets in or stress occurs, tremor becomes greater. She suggested that a careful analysis of the tremors, in addition to the amplitude of the spectral analysis, may yield much information.

Dr. Bachrach mentioned that tremor signatures were normal in all their sampling to date. That is, in the various open-sea and chamber dives, most of the subjects' tremor samples were normal, but were so unique to the individual that they constituted a signature easily recognizable as belonging to a particular subject. He mentioned that some sort of narcotic suppression of tremor occurred, presumably as a result of breathing gas such as nitrogen. Drs. Bennett and Brauer observed that other gases, such as urethane, will damp out tremor.

Dr. Bachrach stated that the tremor signatures previously discussed were usually normal ones and were quite different from each other. He noted that some suppression was observed, a sort of narcotic suppression of a tremor at one point. Dr. Bennett observed that suppression of tremor by narcotics is not an isolated phenomenon and occurs also with the EEG changes seen in the HPNS. Dr. Brauer noted that all of the general anesthetics seem to have this effect and in proportion to their potency. Dr. Bennett discussed the work of Shaefer of the New London Lab, who reported an indication of suppression of HPNS by nitrogen during their 1,000-ft experiment some years ago. Measurements of EEG changes were made that showed the typical increase in theta activity. During decompression the divers breathed for short periods a trimix of 3.5 atm nitrogen, 1.0-1.5 atm oxygen, and the remainder helium, at 600 ft, 400 ft, 340 ft, and 200 ft. While breathing this mixture the rise in theta was replaced by a fall toward normal values, but on returning to helium-oxygen the increase in theta activity returned.

Dr. Bennett also pointed out that there are two types of EEG changes during deep helium diving. First, there is the increase in theta activity,

which is associated with a too-rapid rate of compression and which goes through a 20-hr cycle of growth and decay. Second, there is an overall reduction of EEG activity, which becomes progressively worse with increasing hydrostatic pressure and does not ameliorate appreciably with duration of exposure.

Dr. Naitoh was asked to add further to the discussion of changes in the EEG. He questioned how many electrode positions were used during the RNPL 1,500-ft experiment, and Dr. Bennett responded that there were usually three, one on the vertex, one on the left occipital, and a ground behind the ear. He noted that the measurements by COMEX during the French deep dives were made from many more electrodes, but showed much the same EEG changes. Further, he noted that unless the compression is really rapid (e.g. 50 ft/min to 1,000 ft), one is unlikely to see overt theta activity by just looking at the recordings. However, if a frequency analyzer is used, then the theta rise and fall in general activity may be seen, even with rates of 16-17 ft/min. Indeed, Schaefer compressed at 3-4 ft/min and was still able to detect changes in the EEG. Dr. Brauer asked if the psychological task has a connection with the EEG change. Dr. Bennett responded that he did not believe it did. However, he said, we rely on the increased theta activity and its severity as a warning of possible impending loss of consciousness (the microsleep of HPNS). Tremors may be used in the same way, and if the tremors are severe, there would be a decrement in psychomotor efficiency. He noted that up to now he is not aware of any correlation between tremors and changes in the EEG.

Regarding performance decrement, if compression is far too rapid (e.g. 100 ft/min), intellectual and psychomotor performance are affected. At more normal compression rates only psychomotor performance shows a decrement, a result primarily of the tremors. Dr. Bennett then summed the current knowledge stating that the best tests of occurrence of the HPNS (besides perhaps the subjective symptoms such as dizziness and nausea) are postural tremor and EEG, measured in conjunction with an on-line frequency analyzer. He said he would regard these measurements as mandatory for deep experimental oxygen-helium diving.

Dr. Kanwisher described systems of monitoring, beginning with a consideration of the free-moving organism. He noted that if you tie an animal down, you get such a poor approximation of the natural animal that interpretation becomes problematic. He has monitored a basking shark on the end of a mile of wire, letting the shark swim around. He noted that the kind of energy needed to convey the information when you monitor free-swimming fish or divers can be a problem. Radio waves and light don't go very far, and Dr. Kanwisher believes that one has to go to sound, which also has limited frequencies with which to work. He noted the problems of basic properties of wave lengths and indicated sonar sets can never do what radar sets do. When you start sending something like EEG or EKG, you have to consider what the information is trying to say. The complexities are greater when there are sensitive analog wave forms of some sort to be sampled.

Dr. Kanwisher has been monitoring heartbeat in fish and porpoises and other marine animals in small bays. He showed a tracing of an EKG from a free-swimming cod (fish) and a record of a free-swimming diver. He indicated

that heartbeat and breathing were measures that one could easily obtain, as well as temperature and depth. He discussed the work being done in Hawaii by Dr. Strauss's group, using his monitoring equipment with free-swimming scuba divers. A demonstration by Dr. Kanwisher of inexpensive systems for telemetry ended his discussion of monitoring systems.

Dr. Bradley gave a recapitulation stressing that what we were going to measure in performance was obviously critical, whether it be performance in operational diving or chamber diving. He reviewed the comments and presentations on the HPNS and stated that on the following day discussion of the HPNS would continue to find out what the important areas were. He indicated we had been given a vivid demonstration that divers could be monitored relatively easily, reliably, and safely; the question remained unresolved as to what we were going to monitor. He agreed that heartbeat and respiration were critical measures, but stressed the need to consider additional measures, such as monitoring vestibular function.

Sessions 3 & 4: 4 May 1973

Topic Chairmen: LCDR Robert S. Kennedy (Vestibular Symptomatology)
Human Factors Engineering Branch, Naval Missile Center

Dr. Paul Naitoh (Electroencephalography [EEG])
U. S. Navy Neuropsychiatric Research Unit

Dr. Bradley opened the second day of the Workshop, indicating that the previous sessions had presented an interesting view of HPNS, with Dr. Bishop introducing some new and interesting techniques and Dr. Kanwisher providing some useful points on monitoring. He propsed to return to the HPNS to extend the consideration to monitoring, to what parameters and to operationally useful guides to the diver in the water. He asked Dr. Bennett to begin.

Dr. Bennett indicated there was still a great deal to be done in both humans and animals. One of the basic important operational questions is excursions--just how far and how fast one can make an excursion before the physiological consequences become serious. He referred to Captain Bornmann's work that shows you can move from 1,000 ft to possibly 1,400 ft making a no stop dive; but in order to do this, you must be able to compress quickly. Current evidence indicates the deeper you go, the slower you go. Since there's a need to go faster, we should study this particular area for optimal rates of compression for different dive depths. With regard to mixed gases, there is a need for more work on the effects of suppression of the HPNS by nitrogen or nitrous oxide. While it's evident it can be done, there are obvious grounds for caution in going about it. Dr. Bennett thought it would be wiser to pick a level such as 1,000 ft, from which people were known to return safely (even with some symptoms of HPNS), and then try to suppress the HPNS effects at that point.

He said he was a little cautious about going too deep too fast because one could end up with oxygen toxicity effects, convulsions, and other potential brain damage. He indicated we don't know what is causing HPNS at this stage; more animal research is indicated to find out what is happening, as well as some biophysical investigation of nerve, cerebellum, and brain stem, for example. He stressed caution about HPNS, repeating that perhaps some animal histology is an important first step in basic investigation. He also felt that some depth recording was indicated from the brain and brain stem, cerebellum, and other areas.

Dr. Brauer agreed that there is much basic work to be done, but felt we were making some headway toward describing the neurology of the diver. He believes that within the next 2 or 3 years, as the technology advances, we will have an indication of what we're dealing with in the HPNS. We should begin to look at what kinds of serious operational problems we might expect to encounter--we have yet to see a serious enough motor impairment in deep diving to view this as an operational hazard. High pressure nervous syndrome symptomatology is useful as a diagnostic tool, but operationally it does not matter too much until we go to more serious stages. One of the most ominous changes that Dr. Brauer believes occurs in human subjects is what should be described as a failure of arousal. As we go deeper, we must be concerned in the area of lapse of attention or vigilance. As far as research design goes, he suggested that while hunting for HPNS in human subjects he might be very strongly tempted to give reserpine and see what modest pressures could produce in terms of symptomatology: such pressures have produced severe HPNS-like symptoms in

animal models with reserpine. He's worried about convulsive problems. He suggested that increasing the partial pressure of nitrogen in the diver's breathing mix or adding some anesthetic agent to the breathing medium, may well be a means of coping with the convulsive syndrome. Perhaps one might want to go to helium and nitrous oxide because it takes little nitrous oxide to suppress the symptoms, or perhaps one might wish to go to hydrogen to find the right balance between narcotic potency and respirability. He pointed out that there are problems in handling hydrogen, but there may be some advantages as well including 20-30% increase in threshold of symptoms. He felt that the future will rely on neuropharmacology for some of the answers needed to determine what kinds of dive profiles and breathing mixes will be used.

Dr. Brauer returned to the matter of monitoring and asked again what it is we wish to monitor, agreeing that monitoring of the entire motor disturbance is a thing most likely to give us ongoing information of the development of the syndrome, wondering whether any of the methods so far has been specific enough to give definitive results. He said he would like to have Dr. Bachrach comment on that one, noting also that Dr. Bishop might suggest that instead of looking for tremor, which may not be sufficiently relevant, we might look at alterations of evokation of movements in myographic studies on opposing muscles. Perhaps a combination of myographic monitoring with the performance of some of the local motor tasks might not be a bad way to go.

Dr. Bishop responded by agreeing that monitoring has come up as a major problem, and that it is possible to get some of the simple physio-

logical information from the diver in shallow and deep dives with the present technology. At least, we should get feedback from the diver during the actual dive, heart rate and respiratory information, as well as information on oxygen consumption and ventilation patterns. In any case, one can still get the pattern of breathing, which will provide a lot of information about the respiratory system itself in terms of motor activity.

Dr. Bishop returned to the verbal report, to what the diver himself has to say, and to consideration of the diver as a subject who is trained to attend to his own sensory input. She indicated she had talked to the divers Mr. Conda and Mr. Stewart, who appeared to be cooperative and anxious to see divers trained to attend to alterations in their sensory inputs. Recognizing that divers are concerned about aborting a dive, they still could be trained to report what is occurring as objectively as possible. That means we should attend to what the sensory signals are: indicating what the divers should be watching for and how to report on them. Each of the special senses might be looked at; for example, vision has come in for a great deal of attention, although it's hard to tell whether a problem of the environment or the sensory system is involved--presumably because of the degraded visual situation underwater. The question of tunnel vision or perceptual narrowing has come up in the discussions.

Dr. Bishop indicated that the earliest element to be blocked is the peripheral retina whenever oxygen is limited or the CNS involvement comes about. She believes it's probable that every diver has a different sen-

sory limitation. Instead of expecting one sign to be the clue, there will be individual differences, some having vestibular signs, some visual problems, and others different types of sensory disturbances. Her emphasis was to separate sensory disturbances from motor disturbances in terms of our own thinking and in the design of tests. She suggested we might monitor peripheral events of the motor system; also, that it would be worthwhile to analyze tremor in more detail, not just in terms of the grossness, but in terms of a single motor rate discharge during the tremor as well. For example, is this in particular types of muscle fibers or in a particular muscle group? Just on the basis of documenting this phenomenon, she thought we could learn more about which part of the nervous system is involved. These are examples of the things that could be done with the peripheral nervous system. Central nervous system activities are much more complex. A good deal of work has been done on the EEG; we must ask if this is practical as a monitoring system. In terms of autonomic function, she has heard very little about events other than heart rate; there are other autonomic nervous system functions that should not be ignored. Dr. Bishop indicated she hadn't given much thought to what signals might be monitored: it might be important to look at other events in the autonomic nervous system--for example, pain receptors are very sensitive.

Dr. Bachrach, responding to Dr. Brauer's question to him about monitoring, said he held a particular brief for tremor because he believed it was one of the more valuable precursors of a seriously pending problem. He referred to the review that he and Dr. Bennett had done, which was to be published in the June 1973 issue of Aerospace Medicine. They had gone through all of the deep dives reported in the literature to document HPNS

symptoms and found there was a great deal of verbal report from divers and observers about trembling, convulsions, and other similar HPNS symptoms, such as loss of vigilance. He agreed with Dr. Brauer that tremor is a sign that you're on the way to some serious neurological event. Dr. Bachrach said he'd like to comment on some of the research indications and then on the monitoring of these events.

The theoretical implications of the study of tremor, such as those advanced by Brumlik and his group, and by Lippold, impinge on what the discussion has covered. Lippold considers tremor to be a "hunting" mechanism superimposed on a whole reflex control muscle, getting into some of the basic reflexes. Brumlik is a major exponent of the BCG (ballistocardiographic) hypothesis. In correspondence with Dr. Bachrach, Brumlik has suggested that he believes tremor to be ballistocardiographic in origin, which may be interesting to study in deep diving. Dr. Bachrach said the Japanese are exploring the microvibration hypothesis (MV) which says that tremor is just a contraction of the individual muscle fibers, which suggests that it really is not basic to blood osmolality or cardiac response. In his laboratory at NMRI they are moving to a similar thing in motor unit tremor zone research in animals (primates). dealing with some of the questions that Dr. Brauer raised. Dr. Bachrach discussed the data on the Pennsylvania dive, again touching upon some of the effects of nitrous oxide and nitrogen, which Dr. Bennett has described as tremor with a "narcotic suppression," a tremor that appeared on the 1,250-ft dive. Dr. Bachrach went on to describe the types of tremor, suggesting that the difference between postural and intentional tremor is not at all clear.

not all that clear. He dealt with the various ranges of tremor such as the normal range of 8-12 Hz, in which postural tremor would appear; he stated you can also get a tremor from 8-12 Hz in alcoholism and thyrotoxicosis with a frequency that is normal, but an amplitude out of line. He suggested that these are the sort of things that neurophysiology can begin to pull together as some of the theoretical aspects of tremor measurement. He also said that the work on cold water required that a finer discrimination be made between shiver and tremor.

With respect to monitoring, Dr. Bachrach suggested several approaches. Using devices such as the NMRI Mark III intentional tremor device or the RNPL postural tremor device (each has been used successfully in several deep dives), Dr. Bachrach suggested that one might mount the devices in a Personnel Transfer Capsule (PTC), where intentional tremor and postural tremor can be recorded from a diver as he's being compressed. The intentional tremor data could be coupled with postural tremor accelerometer recordings; these data could then be combined with in-water accelerometer recordings. We can differentiate tremor from shiver so that possible effects of cold water might be discriminated. The problems of wet accelerometry are obvious, including the medium itself, which is a different measurement environment from that of a chamber.

Dr. Bachrach stated that from a research standpoint, there are many questions relevant to the whole neurophysiological problem. He indicated that Saltzman in one of his descriptions of an operational dive said, "by force of will" the diver overcame his tremor and went to work. Dr. Bachrach said one of the divers on the Duke dive had the most marked

tremors of those sampled, but was the most skilled as far as performance went. (He was a professional diver from Oceaneering International.) Dr. Bachrach said we've got to be very careful because we can get into the niceties of research and say "this diver is not really a trembler." This diver was really a trembler, but when it came to getting down to work, perhaps he employed Saltzman's "force of will"; whatever it was, he could overcome the tremor and perform. Although he was successful at performing, the basic question remains: how much physiological wear and tear is this creating for the diver? Monitoring is feasible for real-time analysis on board; ideally the spectral analysis techniques taken on a baseline normal run could be stored and compared with real-time dive data. Dr. Bachrach returned to the comment about the individual signature in tremor, suggesting that individual baselines are specific and unique to each individual and can be used as the subject's own control for monitoring changes in a particular diver. He believed that for monitoring impending collapse and deterioration, tremor is one of our most promising techniques.

Dr. Bishop responded that it was interesting that Dr. Bachrach mentioned shivering versus tremor because it has been well established that shivering is mediated by the way of the reflex arc--in other words, by way of the small motor neurons with a spindle loop. This was why she was asking to analyze tremor. Is it mediated by way of the gamma spindle loop, or is it mediated directly to the motor nerve? One has to have a much better idea of where in the brain tremor initiates. Comparing the tremor in divers against the tremor of disease and against the

tremors induced by gross abnormality would be a fascinating approach. Dr. Bachrach said that at 1,250 ft they did get an extrapyramidal type of tremor, not prolonged, nor marked, but certainly there. He said that this illuminates the problem we're all wrestling with: is it hydrostatic pressure causing the effects, the rate of compression, the gas mixture, or a combination of these? The most promising lead so far, perhaps, is that of hydrostatic pressure. Dr. Bishop thought the diver should be reassured that tremor does not have to interfere with his voluntary efforts at all. She noted, all you have to do is to watch persons who have developed tremor because of disease: every muscle in their bodies can be involved, and yet often these people can do art work or voluntary work perfectly all right. They can adapt. Dr. Bachrach indicated that the transducers used in the various dives did not appear to be monitoring cerebellar effects; this might be crucial. Dr. Bishop said that the tremor may have the cerebellum as one of its components, and Dr. Brauer responded that they were impressed in their own research with animals by the fact that the tremors were just as pronounced when at least 90% of the cerebellum was destroyed. He felt we should look very closely at the extrapyramidal system especially because the earliest paroxysmal effects begin to appear at the implants in the base of the basal ganglia. Dr. Bachrach said that it always bothers him when people talk about "early" symptoms. He referred to work on hyperbaric oxygenization in which an author in Ledingham's book says twitching of the lips is one of the first symptoms of oxygen toxicity. Dr. Bachrach noted, "If that's a first symptom, we're really in trouble, and we have to develop techniques that

will pick up changes early enough to do something about them." Dr. Parent observed that extrapyramidal possibilities are mentioned and asked if any effects of rigidity were noted. Dr. Bachrach responded that in testing some neurological patients with an earlier model of the NMRI Mark III tremor device at Barrow Neurological Institute in Pheonix, rigidity was such a problem in some cases that the development of tremor was considered to be an improvement. Yet rigidity has not been found in any of the divers on which tremor measurement has been made. Dr. Bennett and Dr. Bishop discussed muscular response, such as quantifying spasticity in relating changes in muscle and pain to tremor. Dr. Bradley noted in a semi-jocular fashion that he thought it was wonderful that there's been so much interaction in the diving community in the past 6 or 7 years because for a long time the English were the only ones who had tremors. Dr. Bennett said that work has progressed very quickly. Earlier efforts dealt with such problems as hypercapnia, hypocapnia, oxygen difficulties, and the like. People were dealing with breathing difficulties, and central nervous system problems were not as well discussed.

In response to a question by Dr. Bachrach about questions or comments from the panel, Dr. Hendricks responded we must consider what possible difficulties operational divers will encounter by pushing capabilities to 1,500, and perhaps 2,000 ft, with a need to determine physiological inputs and limits. Captain Bornmann responded by saying that 850 has been the official Navy operational goal. However extension of that capability deeper is a valid research and development objective.

be going to 1,500 ft in the next few years. Miss Kronheim observed that industry also seems to be using a much faster compression rate than does the Navy. She asked if there had been any monitoring of the industrial type of dives in the open water with fast compressions, and if such data might appear in the review that Drs. Bachrach and Bennett had done. Dr. Bennett said that commercial people do use fast compression, but they are not very much involved in saturation diving systems, preferring to go down very fast to the bottom to work for an hour or so and then come up. The comment was made that the diving industry people are primarily interested in demonstrating they can work with depth effectively, and if physiological inputs could increase that work-effectiveness, they would be interested; otherwise, probably not--and certainly not for a research or academic interest. Dr. Brauer observed that the industrial/Navy/academic interfaces that might be so valuable have never developed. He noted, however, that the Taylor dive (ongoing at the time of the Workshop) was a Navy-Taylor diving cooperative enterprise, which might not have been possible 10 years ago. Dr. Bennett observed that a meeting in Houston put together standards for divers in commercial diving, sports diving, and scientific diving, as well. So, while at the present time we do not know what the standards will be, some safety standards have to be developed. The hope is that the commercial diving companies will begin more measurements to get the information to modify the standards that will be imposed upon them. There may be some physiologic inputs because of that imposition.

Dr. Bachrach asked Dr. Kennedy to moderate a section on the cerebellar, vestibular, and labyrinthine systems. Dr. Kennedy said vestibular prob-

lems may be induced directly or indirectly by diving and in some cases, the relationship is uncertain. For example, lesions, which can induce vertigo and other vestibular symptoms, have been found within the vestibular structures directly due to the decompression. Differential caloric irrigation also may incur vestibular effects directly. "Indirect" effects he believed, include perceptual problems created by the environment (e.g. spatial orientation problems, "Which way is up?"), which are a separate problem. Uncertain vestibular involvement may include conditions like the inversion of stomach contents while swimming downward (which is not, strictly speaking, a vestibular problem, but a result of the gravity of the environment) and other problems which occur underwater because of neutral bouyancy, et cetera. Motion sickness may be occasioned in the boat prior to diving (which may have hazardous consequences, assuming the diver dives while nauseated), but it also is not in the same category of vestibular problems as those related to decompression sickness.

He noted there were 300 studies, done in the past 100 years or so, that deal one way or another with direct vestibular symptoms and other studies that deal with indirect and uncertain events. A review of these studies is provided by a bibliography available from the Naval Medical Research Institute. There are about 100 studies of some importance dealing with the perceptual problems and most of these do not deal specifically with underwater problems per se. Rather, they deal with the kind of problems that occur in environments that are impoverished or distorted from the standpoint of sensory inputs. Vestibular problems, he observed, have occurred in places such as submarines and caissons; they have occurred in breath-hold dives from compression and decompression; and they can occur just after

decompression, as well as long after. They can occur in man and animal. Residual neurological effects and vestibular deficits have been reported in divers and caisson workers. Signs and symptoms of the direct type of vestibular problem include such events as dizziness, disorientation, gait defects, vertigo, and nausea, as well as symptoms that are potentially useful to identify migraine. Other symptoms are parasympathetic disturbances: vomiting, cold sweating, drowsiness, and salivation, as well as an increase in ear symptoms--perhaps even sudden deafness. Body sway seems to change as a function of depth.

Dr. Kennedy cited evidence from Bradley, Vorosmarti, and others that vertigo has occurred in 12 to 40% of all divers. Reports in the literature indicate that divers as well as caisson workers show ranges of 0 to 51% with ear problems, and 0 to 28% with vestibular problems. Not all people have addressed themselves to these particular symptoms; therefore the symptoms have not been observed or reported in many dives. He reported on 2,500 dive accidents from the period 1945 to 1970 and suggested that vestibular decompression sickness was a significant event causing perhaps as high as 30% of decompression symptoms. Dr. Kennedy was asked why this is now an emerging problem with decompression sickness; he responded that it is not an emerging problem--it has been around for a long time, but in recent years has not been recognized. A discussion ensued about the need for research on vestibular and cerebellar problems, with suggestions as to use of such techniques as electronystagmography. Dr. Carl Edmonds from Australia discussed briefly the uses of electronystagmography and some of its limitations, particularly in certain types of dives.

Dr. Paul Naitoh was introduced and began the session on Electroencephalography. He said that the early signs of the HPNS in human subjects are varied and somewhat differently characterized by each investigator. It has been anticipated that electroencephalograms (EEGs) might offer a better means to detect those early signs than other physiological information, since they reveal most directly the activities of the central nervous sytems (CNS). Unfortunately this expectancy has not yet been fully confirmed.

The spectrum of spontaneous EEGs, alpha blocking, and averaged evoked responses (including the contingent negative variations) were evaluated in past saturation dive experiments. These analyses have only partially fulfilled the anticipated value of the EEGs in revealing early signs of the HPNS. This apparent insensitivity of the EEGs may have been a product of the policy of limiting recording to short periods of wakefulness.

Dr. Naitoh suggested that the EEGs would be best utilized in a saturation dive where aquanauts would be available not only during awake and active periods, but also during sleep. He further suggested that the physiology of the HPNS would be profoundly benefitted from 24-hour recordings of the aquanauts during a saturation dive. Such intensive and long-term monitoring of EEGs would contribute to diving safety by continuously evaluating the states of consciousness and behaviors of the CNS to exotic breathing gases in hyperbaria. In addition, this method of recording would also expose the altered ultradian cycle of the basic rest-activity cycle as well as the circadian cycle of the CNS, both of which have a direct impact on human behavioral effectiveness.

Dr. Naitoh stated that with recent technological advancements, it is no longer true that the continuous multichannel monitoring of the aquanaut's physiology is cumbersome and expensive. He recommends that future research include long-term recordings of EEGs, as well as recordings of other physiological variables (such as electrocardiograms, electromyograms, and respiration), to be performed continuously during awake and sleep periods throughout the entire saturation dive.

Dr. Naitoh then introduced Dr. Robert H. Wilcox of the Behavioral Sciences Dept., NMRI, to report on their sleep studies in diving situations. Dr. Wilcox described two different classes of investigations: daytime nap-sleep breathing standard diving gases at atmospheric pressure, and regular night sleep of divers on deep chamber excursions. In both, the changes observed were in what is known as "Stage 3" and "Stage 4" sleep, defined by characteristic amounts of high-amplitude, low-frequency waves in the EEG. Stage 4 sleep seems to be especially necessary for the organism, as it occurs relatively early in the sleep period and is the first to be made up after sleep deprivation; it can also be characterized as our deepest sleep in terms of slowness of electrical activity in the brain and also of reduced physiological parameters such as heart rate, respiration rate, and body temperature.

In the nap-sleep study performed by Behavioral Sciences investigators, 7 of 8 subjects showed an appreciable decrease in their proportions of Stage 4 sleep when breathing a standard helium-oxygen gas mixture as opposed to a nitrogen-oxygen mixture. Chamber dive recordings were obtained on two occasions: at Duke University, a single subject showed total sup-

pression of Stage 4 sleep at 870 ft compared to his shallow control; at the Experimental Diving Unit in Washington, D.C., three subjects monitored for at least one night each at 1,000 ft all showed decreases in both Stages 3 and 4 sleep compared to their own shallow controls. These studies seemed to indicate a definite effect of deep diving on Stages 3 and 4 sleep, at least some of which may be attributable to the helium-oxygen breathing gas alone.

Dr. Wilcox emphasized the usefulness of sleep studies in terms of their relative freedom from subject bias; he discussed their importance in terms of the possibility of reduced diver efficiency for waking tasks and the danger of Stage 4 rebound pressure building up with several days of abnormal sleep profiles. He concluded with a plea for standardization of methodology of EEG recording, citing differences in electrode placement, type of gel used, and the danger of dessication of electrodes during recording.

Dr. McHugh was asked to discuss the biochemical changes associated with HPNS. He reported that in a study of a saturation-diving-training program which consisted of academic work, shallow pool and pier dives, and a saturation dive and decompression, significant increases in trainees' serum cholesterol was noted prior to an academic examination. A marked drop in serum uric acid was noted during decompression from the saturation dive.

In general, elevations in serum cholesterol were related to the trainees' motivation and specific diving experience. The decline in serum uric acid during decompression could not be accounted for by alterations in uric acid

production or renal urate excretion and appears to be related to a decompression-induced alteration in cell membrane permeability. There are alterations in renal function during decompression that are manifested as a diuresis of dilute urine. Interference with free water clearance and abolition of regulation of extracellular fluid volume and osmolality through antidiuretic hormone release also occurs during decompression. Dr. McHugh indicated that many of these renal deficits are explicable on the basis of increased water permeability of the distal convoluted tubule and collecting duct possibly due to low levels of adrenal cortico-steroids.

Dr. Biersner stated that although divers initially regard blood letting with some misgivings, careful explanation of the personal benefits which may result--especially the discovery of conditions associated with heart disease--usually gains their cooperation. The relationships which Dr. McHugh and he have found between blood measures (including cholesterol and serum uric acid) and emotional states and successful performance among trainees for saturation diving was briefly presented. For example, trainees who are more highly motivated during training have significantly higher cholesterols corrected for either age or ponderal index. It has also been found that divers with exceptional experience--testing semi-closed breathing equipment, serving as experimental subjects, and so forth--have lower serum uric acid levels. It was mentioned that divers would probably be more receptive to having blood drawn than being rigged with electro-physiological devices which may interfere with movement and dexterity. Blood analyses are also more reliable than most electrophysiological measures, take less time to perform, and have proven validity.

As a preliminary to closing the meeting, Drs. Bachrach and Bradley enumerated some of the approaches that might be taken for information gathering. Two sets of parameters that were discussed are listed in the following table: the first, Operational Parameters, are for immediate problem-solving, and the second, Research Parameters, deal with foundations for unique problem-solving.

Operational Parameters	Wet	Dry	Research Parameters		
Heart rate	X	---	Monosynaptic reflex	X	---
Respiration	X	---	Vestibular functions	---	X
Communication	X	X	Flicker fusion	---	X
Attentiveness	X	X	Evoked potential	---	X
Appropriateness	X	X	SINDBAD	---	X
Speech analysis	X	X	Ball bearing	---	X
Diver training			Tremor	---	X
and reporting	X	X			
Tremor	X	X			
EEG (Waking)	X	X			
EEG (Sleep)	---	X			
EMG	X	X			

Following a brief discussion of these parameters, the Co-Chairmen thanked the participants for their contributions to the two days of provocative informal discussion, and the meeting was adjourned.