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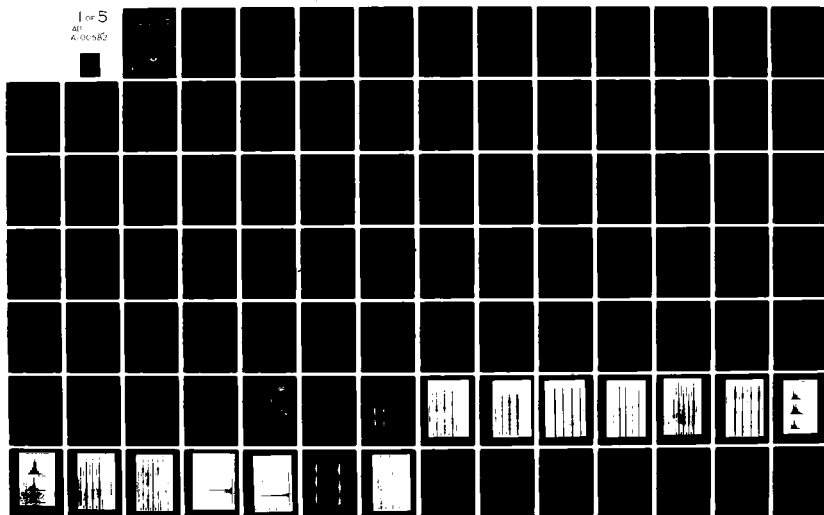
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THE TWENTY-FOURTH UNDERSEA MEDICAL SOCIETY WORKSHOP

THERMAL CONSTRAINTS IN DIVING

AD A100582

Bethesda, Maryland

3-4 September 1980

Chairman and Editor:

LORNE A. KUEHN

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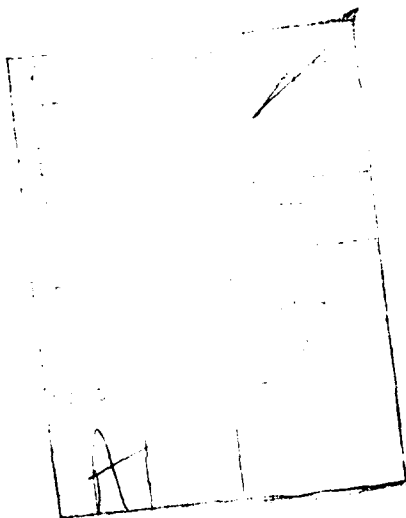
LORNE A. KUEHN

THE TWENTY-FOURTH UNDERSEA MEDICAL SOCIETY WORKSHOP

UNDERSEA MEDICAL SOCIETY, INC.
9650 Rockville Pike
Bethesda, Maryland 20014
U.S.A.

1981

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TABLE OF CONTENTS

	Page
Attendees	v
Welcoming Remarks by Chairman	vii
Acknowledgment	x
Summary of Workshop	
Introduction	1
General Physiology of Cold Water Exposure	3
Afterdrop and Rewarming	8
Shivering	12
Diver Monitoring and Thermal Technology	17
Diver Monitoring	17
Diving Technology	24
Modelling of Diving Thermal Exposures	27
The Problem of the Lost Bell Diver	29
Rewarming Treatment of the Rescued Lost Bell Diver	32
SESSION I: Physiology and Definition of Cold Exposure	
Limits for Divers	35
Predicting T_{re} from Heat Loss, by Paul Webb	36
Respiratory Heat Loss Limits in Helium-Oxygen Saturation Diving, by C. Piantadosi (presented by John Zumrick).	45
Frequency Analysis of Shiver in Humans and Its Alteration by the Temperature of Inspired Air, by Robert Pozos	55
The Effects of Cold Stress on Venous Gas Emboli, by R. Dunford and John Hayward (presented by John Hayward)	81
Cooling in Divers Following Loss of Hot Water Heating: Limits of Endurance and the Sub- sequent Rewarming Procedures, by Philip Hayes	83
General Discussion on SESSION I	101

	Page
SESSION II: Instrumentation and Techniques for Prevention of Diver Hypothermia	117
Diver Monitoring, by Frank Golden	117
The Development of Thermal Protection Equipment for Divers, by L. Nuckols	133
Evaluation of Kinergetics Breathing Gas Heaters for Use in Open Circuit Demand Underwater Breathing Apparatus, by J. Middleton	167
SESSION III: Biophysical Modelling of Hypothermia	185
A Mathematical Model of the Human Thermal System with Reference to Diving, by E. Wissler	187
Immersion Hypothermia: Validation of Computer Model and Prediction of Cooling Rates, by Sarah Nunneley and Eugene Wissler (presented by Sarah Nunneley)	213
SESSION VI. Survival of the Lost Bell Diver	253
Proposals for Survival in Isolated Diving Bells, by Lorne Kuehn	253
Thermal Considerations of Survival in Emergency Hyperbaric Situations, by Thomas Schmidt	277
An Analysis of the Lost Bell Problem, by Eugene Wissler	315
Project Polar Bear I: Testing of Diver Thermal Protection in a Simulated "Lost Bell," by Stein Tonjum	345
Design of Combination Thermal Regenerator and CO ₂ Absorption Cannister for Hyperbaric Survival, by R. W. Hamilton	359
Development and Performance Standards of DUI's Polar Bear System, by Richard Long	373
Final Workshop General Discussion	391

THERMAL CONSTRAINTS IN DIVING

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WELCOMING REMARKS BY DR. LORNE A. KUEHN, CHAIRMAN

Welcome to Washington, ladies and gentlemen. This is the Undersea Medical Society Workshop on Thermal Constraints in Diving.

Six years have passed since the Undersea Medical Society had its last workshop on thermal problems in diving. As a consequence of that workshop, the Chairman, Paul Webb, produced a paper summarizing the knowledge of cold water diving and protection existent in 1974. In the document, he pointed out that there were few new aspects of physiology presented at the Workshop that were not present in the book "Man in a Cold Environment," written by Burton and Edholm in 1955. He identified an absence of knowledge and data on operational cold water diving that could be used for modelling or for formation of new physiological concepts.

Six years have passed. We have filled that vacuum partially and now have a better grasp of the stress of cold water diving. We also have a much better technology base for diver thermal protection.

In planning for this second workshop on the same topic, we invited technologists and engineers as well as medical and scientific researchers to attend so that the information discussed will cover the entire spectrum of cold water diving protection and technology and, further, so that basic research information presented will be transferred more effectively into actual technology and hardware for use in operational cold water diving.

I have arranged the papers that are to be presented in four

sessions. The first one will be concerned with new physiological findings and limits pertinent to cold water diving that have been formulated in the last six years. For example, in this session, papers will be presented on the state of knowledge on shivering, because shivering is a topical item with implications in the fourth session. The second session will concern itself with new technology that exists in support of cold water diving, with particular emphasis on life support in pressure chambers or diving bells for prevention of hypothermia.

The third session concerns modelling which is of special interest because we are not able to conduct hypothermic experiments on subjects that lead to core temperatures below 35°C, due to ethical considerations. Modellers, using the data on mild hypothermia that comes from our experiments, the data on more severe hypothermia that is available from resuscitation of accidental hypothermic victims, as well as a large amount of data on severe hypothermia in animal experiments, can produce models of cold water diving that give us information about physiological events to be expected with core temperatures below 35°C.

These three sessions bring about a focus on the fourth session which I consider to be the most important. Scheduled for tomorrow morning, it will concern ways of enhancing the survival of the trapped bell diver which is, at present, one of the major unresolved problems in diving thermal physiology. Modelling will be particularly useful in this regard. Papers will be presented describing

experiments pertinent to this problem in Norway. We also have commercial representatives here who have designed and fabricated survival systems for enhanced survival of the cold bell diver.

The present time is most propitious for bringing together various investigators, researchers and technologists to review progress made in this research field in the last six years. In these two days we hope to arrive at a consensus of the statement of knowledge concerning cold water physiology and technology pertinent to the cold water diver.

Each session's structure consists of the presentation of three to five papers which serve as the focal point of knowledge around which the discussion will be initially based. The resulting discussion in each session, which is being recorded and annotated, will be the platform for direct interchange of views that hopefully will lead to the formation of a consensus on the topics under consideration.

Each speaker will have 15 minutes for presentation of his or her material. Several questions of clarification will be permitted for each paper but we hope that the major points of discussion are reserved for the period immediately following the papers in each session.

Thanks are due to the Workshop Attendees
for their scientific contribution to, and editing
of, this document. Thanks are also extended to
Ms. Dawn Gardham for advice and assistance in its
preparation.

SUMMARY OF THE WORKSHOP
THERMAL CONSTRAINTS IN DIVING

INTRODUCTION

Six years have passed since the Sixth Undersea Medical Society (UMS) Workshop on Thermal Problems in Diving was held in 1974 at Yellow Springs, Ohio, under the chairmanship of Dr. Paul Webb. The proceedings emanating from that conference documented the state of physiological knowledge and technological support pertinent to the exposure of divers to cold, especially under hyperbaric conditions. The review of basic physiology pertinent to this problem drew heavily on the book Man in a Cold Environment by Burton and Edholm (published in 1955) and the dearth of physiological and performance data based on real diving experience was emphasized. The technology for thermal support of the diver was also reviewed and a call was made for new thinking and work on the part of designers of diver life support equipment.

It will become clear to the reader of these proceedings in this second UMS Workshop on this topic (the Twenty-fourth UMS Workshop) that much new thinking and work has taken place since the first one. Rather than review all the elements of basic physiology and technology reported at the two-day meeting in September, 1980, this summary will present the major new findings discussed pertinent to each of the four sessions of the Workshop: General Physiology of Diving Cold Exposure, Diver Monitoring and Thermal Technology, Diver Thermal Modelling, and the Lost Bell Diver Problem. The summary will

be followed by an edited verbal transcript of the proceedings with formal papers inserted in place of the respective presentations, wherever possible.

GENERAL PHYSIOLOGY OF COLD WATER EXPOSURE

Despite considerable investigation since the 1974 UMS Workshop, the relationship between body heat loss and core temperature change in cold water exposure has not been definitively elucidated. The relationship appears to be dependent on the rate of cooling in that loss of a given amount of heat at a rapid rate causes a greater drop in rectal temperature and more shivering as compared to loss of the same heat over a longer time which results in little rectal temperature drop, no shivering and only minor complaints of mild cold stress. Although the performance decrement associated with each type of exposure remains to be determined, there has not yet been any explanation of this documented effect of rate of cooling. The effect is pertinent not only to use of the rectal temperature but is applicable to various core temperature measurement sites.

New experiments were described that documented the relationship between individual body size and fatness characteristics and cooling rate. For the same body mass and height, variation of body fat was shown to cause a large variation in rectal temperature decrease and heat loss; the fatter the individual, the less the change. For the same body fat percentage, variation of body mass and height has a large effect in rectal temperature and heat loss; the larger the individual, the less the change. People of identical skinfold thickness and sex have enormous differences in rates of cooling.

Although body fat has long been attributed to being the major

factor in the success of long distance swimmers in very cold water, new studies have shown that the major factor may be related to metabolic heat production or peripheral vascular response. People accustomed to cold water immersion become very efficient in use of the shiver response and only shiver at a level of 20-22% of their VO_2 max. Habituation of cold water personnel is also considered to be an important factor but little is known of how it is acquired and maintained. It appears to be an acquired characteristic and one that is not genetically determined, although it depends specifically on the task involved. It is a peripheral vascular physiological response.

As to recommended limits for cold exposure of divers, these were refined during the last six years; yet the question remains as to whether there should be one set of limits or a scale of limits since any one set must perforce be set conservatively to apply to the majority of the population that is to be affected by them.

The existing standards for minimum inspired gas temperature for divers have been shown by experiment to be too liberal and two papers at this workshop recommended that a new more conservative set be developed. Those proposed by US Navy investigators were based on experiments that showed that individuals breathing cold helium-oxygen mixtures at great depths did not respond metabolically to the high respiratory heat losses and suffered prompt rectal temperature decreases with very little shivering. Their proposed new standard was based on the concept that a rectal temperature change of 0.25°C per

hour can be tolerated for four hours for a cumulative rectal temperature decrease of 1°C.

Mucus formation in the respiratory tract at these new limits is not thought to be a problem. It is not known if this respiratory stress is a thermal effect or rate-of-heat-loss effect, that is, is the stimulation of mucus temperature-dependent or heat-loss-dependent? An unusual cold-induced hyperventilation was also reported. Some concern was expressed that the new US Navy limits were pertinent primarily to subjects at rest in a hyperbaric chamber environment and that more conservative limits may have to be developed for the working helmeted cold water diver.

Another set of minimum inspired gas temperature standards was recommended by English investigators concerned with the limitations to work by divers who lose access to their hot water heating supply in cold water. Although entrapment of hot water gives a certain amount of thermal protection in open circuit suits, respiratory heat losses were recommended only to a level of 200 watts, instead of the currently accepted 350 watts. The major benefit for respiratory gas heating is to prevent respiratory heat loss since, due to limitations on supply of very hot gas to the respiratory tract where it can cause burns, hot gas rewarming can only provide about 50 watts heat input to a diver. Provision of such respiratory heating markedly improves prolongation of diver work and performance at great depths. Some concern was expressed by the technologists present at the Workshop that such new limits as were proposed will be difficult to attain

with the present level of technological development of diver breathing gas equipment. Control of hot water flow and temperature to a suit or a breathing gas heater is difficult. There is a need for integration of instantaneous respiratory flow and temperatures to aid in determination of respiratory heat loss.

Although the accent on use of limits for respiratory gas heating is presently centered on the minimal limits that will prevent hypothermia, problems of hyperthermia are becoming more and more evident. Respiratory burns have already been identified as a hazard but use of gas heaters with hot water suit technology can result in divers suffering heat stress in cold water exposures such that they will encounter heat syncope on returning into a bell. The fainting usually occurs in the act of leaving the water and losing the support of the hydrostatic column. Blood pooling then occurs in the lower regions of the body and the brain loses sufficient circulatory support for consciousness. Several anecdotal accounts of this phenomenon were presented at the Workshop.

Other hyperthermic risks were identified in the deck decompression of divers in surface mounted chambers exposed to hot ambient thermal conditions in tropical regions. Several deaths have been reported due to such hyperthermia and are considered to have been preventable by use of adequate cooling measures involving shade or spray-cooling.

The limit of subjective cold tolerance is more related to peripheral cold stress rather than any rectal temperature change. Subjective cold sensation is based on afferent impulses from thermal re-

ceptors in the skin. Some concern was expressed that use of hot water suits confuses this physiological sensation mechanism. A mean skin temperature of 16-20°C can only be tolerated for 5-10 minutes.

The thermal state of the diver has an important bearing on the success of various decompression schedules and tables. Work was presented at the Workshop that documented the effect of peripheral circulation on post-dive Doppler scores of divers undergoing decompression. Divers who are cold during their bottom exposures take up less gas in solution in their body tissues than those who are warm. Consequently, on decompression, the diver who was warm at the bottom and cold during decompression has more decompression problems. An anecdotal account was presented describing divers working without hot water suits as being able to eliminate some decompression stops which were needed when hot water suits were deployed. The Workshop delegates agreed, however, that this effect of cold exposure was not to be advocated for reduction of decompression stress, since the cold divers experienced loss of capability and judgement, which was more important to their survival than the greater risk of decompression due to being kept warm.

A suggestion was made and seriously considered that diver selection be advocated to reduce the impact of specific operational problems, that is, selection of divers with particular skinfolds, or size, or body shape, or previous history may be important in significantly reducing diver hazard and risk in operational cold stress environments.

The problem of drug use in operational environments was recognized as being an important but relatively uncontrollable problem in affecting diver health and safety.

Afterdrop and Rewarming

The consideration of afterdrop of core temperature, usually denoted in terms of rectal temperature, has been defined as the continued cooling of the core during the rewarming process, and has been considered as being presumably due to the return of cold blood from the periphery to the core under the influence of vasodilation induced by the rewarming. Such continued cooling has been considered to be dangerous since the extent of afterdrop may be as great as 1 or 2°C and if the heart, which may have been cooled to temperatures less than 34°C at which point it becomes hyperexcitable, is exposed to colder temperatures, ventricular fibrillation has been suspected to be a dangerous possibility.

Several of the Workshop delegates questioned this conventional explanation. The evidence for afterdrop is based entirely on observations of rectal temperature, which is determined by cold venous blood return from the legs of a cold-exposed body, arterial blood input from the abdomen, some venous blood return from the superficial layers on the trunk, as well as the extent of body insulation provided by gluteal muscles and tissue. Golden and Hervey have shown that the rectal afterdrop can be explained sufficiently as a conductive phenomenon, that is, as being due to a time-lag in conductive heat transfer between the core and the shell. A hemodynamic explanation may be involved but it is not necessary for explanation.

In the case of good hemodynamic mixing in the core, as in the case of exercise or violent shivering, rectal temperature is consid-

ered to be a good analogue of core temperature. If hemodynamic mixing is very poor in the trunk, as in the case of nil activity or non-shiver, then a flush of venous blood from the legs on movement will cause a decline of rectal temperature and it will not be a good analogue of core temperature. Regional cooling of the legs and buttocks has been shown to have a significant effect on rectal temperature.

Golden's theoretical calculations were verified by experiments conducted on rewarmed pigs in which rectal temperature was shown to correlate poorly with central venous temperature. Arresting of the circulation by death caused an initial rise in central venous blood temperature but there was no significant change in rectal temperature during rewarming.

Since the factors that affect the temperature of any body core site are local heat production, conduction of heat to and from the site and transfer of heat by mass flow, the conductive explanation of afterdrop can also suffice to explain the initial rise of core temperature on exposure of a body to intense cold stress. A reduction of blood flow provoked by peripheral vasoconstriction as well as an increase in local metabolism, in the gluteal muscles in the case of the rectum, leads to a sudden increase in core temperature. It is also possible that this phenomenon can be accentuated by increased minor shiver.

A hemodynamic involvement with afterdrop exists when exercise provokes a movement of cold venous blood from the limbs into the core; consequently, rescue of intensely cold immersion victims

should involve as little voluntary activity as possible to minimize this exercise-induced venous return.

Such new considerations of afterdrop have brought about a removal of the stricture against rewarming of victims in hot water baths, i.e., with limbs out to reduce the chance of a rectal afterdrop and consequent chance of ventricular fibrillation. Rectal afterdrop is now considered to be a conductive artefact in rewarming of prone victims that is of little import. Death, after rescue, which still claims an inordinately large number of victims, is considered to be due to a cardiovascular cause; namely, the increase of demand on cardiac output due to movement and the removal of hydrostatic squeeze (which assists cardiac output by a factor of about 1/3). In a hypothermic body there exists a cardiac filling problem, due to a short diastole, so that an extra demand causes ventricular fibrillation and death. Such events are unrelated to rectal temperature or afterdrop and are more a factor of the hypovolemic state of the body. Such a state arises in extended immersion (several hours) because of increased diuresis associated with circulatory shifts brought on by peripheral vasoconstriction and, more importantly, by an inter-compartmental shift wherein intravascular fluid moves into the extracellular and intracellular spaces. In rewarming, this fluid shift is reversed but it does not happen immediately and the circulatory system is in a hypovolemic state.

Consequently, the practice of keeping limbs out of a hot water

bath serves no useful purpose and a faster rewarming is accomplished by having the limbs in the bath. The bath replaces the hydrostatic squeeze and thereby supports cardiac action by reducing cardiac output. A peripheral vasodilation does take place which would tend to increase cardiac output but this does not occur immediately but over a period of 15-20 minutes. Such vasodilation and loss of blood pressure is especially important in the rewarming of the older hyperthermic victim.

In summary, afterdrop is now only considered to be an important factor in the extended cooling of a diver or immersion victim due to exercise-induced venous return on coming out of a cold water environment.

Shivering

As documented in the 1974 UMS Workshop, intense shivering commences shortly after cooling begins, resulting in a marked increase of metabolic rate of up to five or six times, during the early phase of hypothermia. That report described a decrease of shivering for core temperature below 35°C, ceasing altogether to be replaced by muscle rigidity for core temperatures in the range of 30-32°C. Shivering was considered to begin again during rewarming for core temperatures above 30°C.

During the intervening years since the 1974 Workshop, much new information had been obtained on shivering which was discussed at the 1980 Workshop in considerable depth as to its efficacy and occurrence in cold water divers, particularly when in cold helium-oxygen environments.

Shivering may be an extended manifestation of the physiological action tremor that is inherent in all body muscle groups at a frequency of 5 to 12 hertz and not a new response brought into being when the hypothalamus is appropriately triggered. Even in subjects who don't appear to shiver, there is synchronized electromyographic activity in various muscle groups; as this progresses under continued stimulation, a tensing occurs in the muscles preparatory to visually noticeable shivering. The control of shiver is not considered to be entirely governed by a central hypothalamic pacemaker but in part by spinal cord generators operating at a sub-vertebral or sub-hypothalamic level.

A significant shift in shivering frequency takes place in the legs of cold water immersion victims on standing up, being due to activation of hyperactive stretch receptors after a period of relative inactivation in the cold water; therefore, shiver in cool air exposures differs from that in immersed water exposures.

The temperature of inspired air appears to have an influence on the amplitude of shiver. Temperature sensors in the mouth or respiratory tract may signal shivering by spinal cord reflex loops whenever inspired gas at one atmosphere is cold. Warm moist inspired air tends to reduce shivering. Several investigators reported that inspiration of warm air at one atmosphere pressure led to reduction of shivering by amounts from 1/4 to 1/3. When the mouth is anaesthetized, inspiration of warm air at one atmosphere fails to reduce shivering. In contrast, breathing of cold hyperbaric oxyhelium in warm gas environments does not provoke shivering even though a pronounced rectal temperature decrease is measured.

With prolonged shivering, an inability to move by the affected person has been reported and has been thought to be an effect of fatigue. It may be instead due to an alteration of electromyographic timing causing a "poverty of movement." Speculation still exists that fatigue in shivering will occur after a sufficiently long time, say eight or nine hours, with depletion of glycogen reserves, and heat generation will fall off, leading to a precipitous drop in arterial temperature.

No correlation has been obtained between rate of cooling and

shivering activity and the question has not been settled as to the efficacy of shivering of nearly-nude subjects in cold water immersion --is the heat generated quickly dissipated by convective cooling because it is produced superficially? In Keatinge's immersion experiments, lightly dressed immersion subjects experienced a rapid drop in core temperature due to shiver. Modelling of this question for air or hyperbaric gas exposures indicates that it is of benefit, especially for thermally-insulated subjects. When shivering stops or declines, the body core cooling rate should approach linearity.

The Workshop questioned the concept of decline of shivering below 35°C. The source of this information could not be ascertained and it is considered to be a speculation of Burton and Edholm in their book Man in a Cold Environment. It is possible that with prolonged cooling, fatigue sets in and shivering declines and it may be possible that, in acute immersion cooling, shivering does not occur because of a spastic response; however, violent shivering has been observed in many hypothermic victims, some with core temperatures as cold as 24°C. Since the literature contains so little reference to observations on the shivering or the metabolic response of accidental hypothermia victims, this decline of shivering remains unsubstantiated. One novel finding is a spurious form of shivering that occurs in initial cold water immersion in some subjects, which reappears on continued core cooling. Another is that people will shiver with core temperatures elevated above normothermia but with very cold skin temperatures (20°C).

In summary, shivering is considered to do two things: it increases the rate of metabolic heat generation and it increases peripheral perfusion. The former action helps to maintain core temperature and the latter phenomenon diminishes it. The net balance of thermal benefit to the individual depends on the overall convective and respiratory heat demand as well as the thermal insulation that is worn. Shivering also responds more rapidly to rate of change of temperature and direction of the thermal change.

DIVER MONITORING AND THERMAL TECHNOLOGY

Diver Monitoring

The subject of diver monitoring received considerable attention throughout all four Workshop sessions. It was readily agreed that the principal reason for diver monitoring was the enhancement of diver safety, to determine if the diver was becoming hypothermic or if his performance was being impaired or jeopardized. Such monitoring also is important in the furtherance of knowledge of diver physiology and protection. Some discussion took place as to the relevance of such monitoring in operational diving, since most divers will voluntarily end their exposure when experiencing the initial effects of hypothermia, although certain divers such as search divers or military divers may not have that option. Most surface-supplied divers experience minor peripheral cold stress in shallow water diving and are accustomed to it. The value of monitoring was seen to be most important in bounce diving, either from the surface or from a bell, in which such monitoring would be of use in determining the thermal state of a diver prior to each successive exposure. Such monitoring would enable a dive supervisor to make an informed decision as to the risk inherent in completing a set piece of a diving operation as compared with bringing a diver in and replacing him with another.

The state of such monitoring technology is considered to be poor. Hot water temperatures for suits or gas heaters may be known at the point of source but are rarely known at the bell site or at

the point of entry to the diver's suit. A risk of hyperthermia or burning exists and several cases of fainting by heat syncope have been reported.

The parameters for monitoring are several: subjective verbal comments, deep body (core) temperature, selective or mean skin temperatures, heart rate and direct heat flow measurements. It was the consensus of all Workshop delegates that no one physiological variable was an accurate and completely reliable indicator of a decrement in diver performance or development of a thermal problem. Furthermore, monitoring of several of the aforementioned parameters involves some invasion of personal privacy of the diver, either by having transducer leads affixed to him or inserted in various body orifices. Aside from the extra time and involvement that use of such transducers requires in the dressing of divers, these transducers may also adversely affect diver performance or act as irritants in the conduct of diver operations.

Because of a tendency for lack of cooperation from divers, it was proposed that use be made of environmental monitoring, wherever possible, to gather information on diver health and safety. For example, the one vital measurement for use in a hot-water-suited diver would be the temperature of the hot water on entry into the suit. If hot water is also used for heating of the breathing gas on entry into the diver's helmet, then the hot water temperature measurement also suffices for determination of respiratory thermal stress. As to limits, the measurement of the suit hot water entry temperature

should be compared to an upper limit, to avoid hyperthermia or skin burns, rather than a lower limit, since the diver can tolerate some degree of peripheral cold stress. The measurement of hot water temperature for the breathing gas heater should be compared to a lower limit, to avoid hypothermia induced by respiratory heat loss, and an upper limit to avoid respiratory burns. In the latter case, it is possible to use the hard-wired communications umbilical for transmission of the thermal information to the dive supervisor. Use of this one environmental measurement implies that should the measurement fail, then the diver involved should be withdrawn from the operation to be replaced by another, until the instrumentation channel is restored.

Instrumentation used in a helmet or on the diver umbilical was considered to be more acceptable to the diver than any transducers placed in or on his body. Several suggestions were made as to new possibilities for this purpose. The scalp is one site of the body that is always vasodilated and which could serve as a site for surface contact detection of either heart rate (by optical plethysmography) or temperature (by contact thermistor). The apparatus for such measurement could be enclosed in the diver's protective bunny cap and would require relatively little attention or special involvement during the diver's dressing procedure.

Classical diver physiological monitoring has usually involved measurement of rectal temperature, not because it is the best deep body core site but because it has historically been a widely reported

parameter and is relatively easily determined for comparison to results gathered in earlier studies. The search for an acceptable deep body site for core temperature measurement still preoccupied certain of the discussions at this Workshop. It was recognized that rectal temperature is not the preferred site, since it is a slowly-responding thermal area and one that is much affected by the "afterdrop" phenomenon. Still, its relative ease of access and the amount of experience involved in its use have shown it to be a relatively good indicator of general thoracic and head temperature during cooling but not during rewarming. Deep esophageal temperature is considered to be the best analogue of intra-thoracic temperature during rewarming, since even tympanic temperature does not correlate well with the latter during this experience. To be successful, rewarming strategies must heat the heart; therefore, the most suitable temperature analogues to arterial temperature during this exposure have been shown to be: esophageal, gastric, tympanic and rectal, in descending order of applicability.

Whatever the method of measurement for deep body core temperature measurement, it has been shown that it is not well correlated with the quantity of heat lost by the body. The heat loss-core temperature relationship is strongly dependent on rate of cooling; furthermore, there are important individual differences in cooling rate, based on amount of body fatness and habituation.

Although monitoring of core temperature is useful in determining how cold a diver is, it doesn't relate necessarily to what his

performance is or is likely to be and its use as a "gauge" of hypothermic stress is questionable. The level of unacceptable core temperature is still set at 35°C but it is recognized that this measurement value is not as important as would be a measurement of decrement of diver performance. No significant decrement in mental performance has been demonstrated for core temperatures as low as 34°C although signs of decrement of peripheral physical performance are apparent at that temperature.

The Workshop delegates gave consideration to the idea that core temperature should only be used in the sense of differential diagnosis. Is there a problem with hypothermia or not? If not, then attention should be directed to other potential diver biomedical concerns. The core temperature measurement value of 35°C is a useful indicator in that regard. If the core temperature is less than 35°C, then potential hypothermia should be considered to be a problem and the diver should be removed from the exposure. Temperature, then, may not be as important as it was once thought to be. It is the consequence of temperature that deserves consideration, i.e. respiration, cardiovascular support, fluid balance.

Measurement of heart rate was considered to be a good indicator for monitoring of diver cold stress, particularly during rewarming in a stable controlled environment. It decreases under the influence of diver cooling but is also influenced by diver work and pressure-related bradycardia. It drops precipitously on exposure of the diver to a controlled hot water bath along with a decrease of oxygen con-

sumption. It is the first body parameter to show response to rewarming; it starts to increase before any body temperature site responds. It is therefore useful in monitoring of immersion or hypothermic victims during rewarming to prevent rewarming collapse or heat syncope. If it starts to increase markedly, the bath temperature should be quickly lowered until it returns to stable lower values, at which point bath temperature can be slowly increased.

The mean skin temperature is considered to be an important parameter for diver monitoring. Indeed, some workers have proposed the measurement of the big toe temperature alone as a good parameter for correlation with diver performance and willingness to become cold. The diver sensation of thermal comfort is based upon afferent impulses from thermal receptors in the skin and such impulses are elemental in the shivering response, even for divers with elevated core temperatures.

Little agreement on the method of choice for mean skin temperatures exists among thermal researchers. Whereas various methods based on four-site, seven-site or twelve-site indices agree fairly well in steady-state exposures, no agreement exists for the case of transient exposures. All such indices were derived for use in laboratory exposures of nude subjects in cool air environments. Such indices assume that every body site considered in the weighting has the same thermal sensitivity; however, a difference in density of cold thermal receptors exists over the body, which could explain certain of the anomalies of shivering response. Mean skin temperature

indices as presently calculated may not be good representations of peripheral input. A more soundly-based physiological weighting scheme for mean skin temperature determination is warranted.

Diving Technology

The state-of-the-art in diving suit protection was reviewed in the 1974 UMS Workshop proceedings and two papers were presented in the 1980 Workshop to document advances since that time. A review was presented of the progress in active and passive protection attained by the US Navy during the last six years. The active program is centered around an individual magnesium-oxygen diver heater capable of providing 500 watts of heat over a six-hour mission. The passive program is centered around the use of the Thinsulate material, a 3M-produced hydrophobic material that maintains its insulation qualities under hyperbaric pressure, even when wet. It has a higher insulation value than foam neoprene and can serve as an insulative undergarment in both wet- and dry-suit ensembles. Copper-man studies, along with range of motion studies, have demonstrated that a suit centered around this material possesses a thermal insulation approaching that of stagnant air, which is the maximum value attainable that can still permit the diver to perform realistic tasks. The suit insulation is approximately 1 to 1.5 clo at one-atmosphere conditions, depending upon inflation. It is designed to provide passive thermal comfort for a diver operating in 5°C water over a six-hour mission profile. The major problem now to be considered with such suit technology is alleviation of overheating which is a problem common to all constant insulation suits. One solution is to design a family of such garments with varying insulations for different thermal requirements.

The future of diving suit design lies in the incorporation of regional differences in insulation to improve the physiological effectiveness of the suit or bring about a reduction of suit squeeze. As to suit material, development of materials that are partially evacuated or capable of holding a partial vacuum may lead to improved thermal insulation values that are one order of magnitude better than those of presently used materials. Control of such insulation to avoid user hyperthermia will be a major problem in development of this suit technology. Even presently used hot water suits suffer from inadequate control of the hot water supply.

A review was also presented of the best available technology for respiratory gas heating in terms of the practicality and breathing resistance presented by such technology. The new minimal respiratory gas heating limits set for this technology are such that they cannot presently be met unless use is made of "active hot water insulation." Aside from meeting the new limits, two other problems beset this technology; these are the convincing of operational divers that this technology should be used and the improvement of the control of diver's inspired gas temperature.

Open-circuit demand technology is one of the easiest ways to effect respiratory heating. Closed-circuit systems have gas heaters as well but such heating technology is fairly primitive in terms of thermal engineering. The most critical area for technological improvement is the interfacing to the helmet. Depth or pressure exposure is not a problem as the heaters are placed on the diver, close

to the helmet. Other pertinent problems of this technology are the questions of what design limits there should be for heating for thermal comfort and for the avoidance of hypothermia.

The goal of all diving technological development is equipment reliability and performance that cause diving equipment not to be the limiting factor in diver safety and productivity.

MODELLING OF DIVING THERMAL EXPOSURES

The importance of mathematical modelling of diver thermal exposure is that it permits elucidation of probable thermal and physiological behaviour associated with severe body core hypothermia. Experimental investigations in such instances are limited to animal data or data collected from treatment of accidental hypothermia victims. Human diving thermal experiments are severely limited to mild hypothermia by ethical considerations or by problems of expense. Modelling can be used to extend this experimental data.

The history of such mathematical modelling is fairly short and has come into prominence through application to astronaut exposures in space-related activities. The essence of any good mathematical model is that it be based on sound physical principles. Several such models were presented in the 1980 Workshop pertinent to the diving cold water exposure problem. In these models, arterial blood temperature was taken as body core temperature. The modelling of heat loss by convection was very important as was that of the physiological phenomenon of counter-current heat exchange. One of the models presented assumed a rate of shivering driven not only by core and skin temperatures but by other sensors as well, perhaps analogous to those considered to be intrinsic to the respiratory tract. This model also incorporated a fatigue factor for shivering with elimination of shivering within 8 or 9 hours.

The predictions of the various models were compared to limited human experimental data and were found to be in good agreement with

such data. However, the effects of shivering are sensitive to the way in which distribution of shivering and thermogenesis occurs in the body and this information still is not well known. With an ever-increasing collection of human diving thermal data, such models as exist will become more refined and definitive in the near future.

THE PROBLEM OF THE LOST BELL DIVER

Several unfortunate hypothermic deaths of divers inside diving bells that had become inadvertently lost for several hours caused attention to be focussed in the last session of the Workshop on problems of diver survival in such a situation. Six papers were presented in this session pertinent to these problems and the discussion that followed drew upon material presented throughout the whole of the Workshop proceedings.

The lost bell problem was presented as one that was analogous to the hydration problem in survival of individuals in a liferaft with a limited supply of water. Various recommendations for extending survival time were presented. These included insulating of the diver, removal of the diver from contact with the sides of the bell, adoption of a foetal position for reduction of heat loss, avoidance of any physical activity, use of all available heat retained in the bell after its "loss" and use of emergency respiratory heat reclaimers/ CO_2 scrubbers. Even the net benefit of shivering was questioned at great depths and tentative suggestions were made as to ways for reducing or eliminating this physiological response in the event that shivering was contra-indicated in terms of heat loss. The effect of shivering on the boundary layer coefficient for convective heat transfer is an unknown but presumably large factor and further indicates that shivering may be undesirable at very great depths. Any method proposed for reduction of shivering will have to take into account enhanced peripheral vasodilation which would accentuate con-

vective heat loss, although in reduction of shiver itself, there would be a concomitant reduction in respiratory heat loss.

Two theoretical papers were presented in which the survival of divers in a lost bell was modelled. One model involved the diver with and without 75% body heat protection but without respiratory heat conservation in the survival experience. The end point of this model was a body heat loss of 400 kcal, calculated to lower the body core temperature to 33°C. Depending on the depth, predictions of survival for isolated divers were of the order of one to several hours. In consideration of the possible benefit of shivering, involving only the metabolic heat generated in comparison with the associated increase in respiratory heat loss, shivering was found to be beneficial except for very deep exposures of the order of 1000 fsw and temperatures of about 5°C. This pessimistic appraisal would not pertain if the respiratory heat loss was reclaimed. A different mathematical approach also predicted short survival times for the relatively unprotected diver, but this analysis was extended to show that with proper thermal insulation (1 clo in the environment of concern) and reclamation of 75% of respiratory heat loss, survival of the diver should be possible. A 500 watt heat source and 1 clo of diver insulation (in the environment of concern) was suggested as being sufficient to provide extended survival at a depth of 150 msw and environmental temperature less than 5°C.

Two sets of experimental data were presented that had a bearing on this problem. One involved simulation of a submersible sink-

ing with occupants at one atmosphere and 5°C. This three-day experiment was terminated after 25 hours due to CO₂ scrubber failure, but the temperature in the two bells involved reached ambient environmental temperatures in approximately 5 hours, demonstrating that in uninsulated bells, the major factor in the cooling of the bell environment is the bell structure itself. The other experiment discussed was that known as Polar Bear, performed by the Norwegian Underwater Institute, in which various survival systems were tested in a lost bell simulation at 150 msw and 5°C temperature. A good sleeping bag and respiratory thermal regenerator was found to provide sufficient protection for survival for approximately 10 hours.

Two other presentations concerned the development of survival technology, one a passive insulation garment ensemble to be used with the second, a reclamative thermal regenerator/CO₂ scrubber. The emphasis on such development was cost-effectiveness in terms of design simplicity, space required and maintainability. It was considered that, if a bell rescue was not accomplished in 6 or 8 hours, then protection of more than 24 hours may be warranted, but if survival protection can last 24 hours, then several days survival is likely as long as other life support elements do not become threatening, e.g. food, carbon dioxide, etc. As much as possible, the survival equipment of a diver should be the equipment that he normally uses and wears. One example is the use of the diver umbilical for thermal insulation if it is wrapped in the bell trunk.

Rewarming Treatment of the Rescued Lost Bell Diver

Decompression of a rescued lost bell diver should only take place after rewarming has been completed and on no account should death be pronounced in the case of a body that is recovered in a presumably hypothermic condition. If the diver is conscious on rescue, then rewarming may take place in the saturation deck decompression chamber and should consist of passive rewarming to avoid the dangers of hypovolemic collapse (rewarming shock). Any manoeuver that would lead to a demand on cardiac output is to be avoided and the diver should be laid prone or horizontal with his head slightly down. If the diver is found unconscious in the lost bell, it may be preferable to leave him in it and stabilize him there until he recovers consciousness by passive rewarming. He should be moved as little as possible and an air mattress and inflatable splints may be considered for use to prevent rewarming shock. If he is alive and kept insulated, his residual metabolism will bring him back to consciousness. Once conscious and near normothermia, consideration may be given to use of warm water flushing of his hot water suit. During the rewarming, his pulse should be noted and recorded. If it starts increasing rapidly, the temperature of the oxyhelium environment should be lowered to reduce it. Once the pulse has recovered, the temperature of the bell can be raised again. This technique will prevent rewarming shock from developing.

In the lost bell incidents that have occurred, the unconscious divers have fallen down into the bell trunk and the weight of their

bodies has prevented easy or quick opening of the bell doors on rescue. It has been proposed in the diving industry that the divers lash themselves in a seated position on the side of the bell to prevent this from happening. The Workshop delegates considered that this technique could lead to suffocation of the divers due to the weight of their heads occluding their airways when unconscious. It is recommended that they not be tied up in a seated position and that consideration be given to special yokes or harnesses for prevention of the probable suffocation.

SESSION I

PHYSIOLOGY AND DEFINITION OF COLD EXPOSURE LIMITS FOR DIVERS

PREDICTING T_{re} FROM HEAT LOSS

Paul Webb

Webb Associates, Yellow Springs, Ohio

ABSTRACT

For several years we have been collecting experimental data with a suit calorimeter (a bath calorimeter in one series) to show how measured heat loss relates to changes in surface and core temperatures. Cooling has been done at widely different rates, usually carried to a voluntary tolerance limit, and also as a near-step function followed by maintenance of the heat deficit for several hours. We have seen that (1) the rate of cooling strongly affected change in T_{re} at a given level of heat loss; (2) that shivering occurred when cooling was rapid, but not when very slow and to a very large heat deficit; and (3) that differences in people's body size and fatness explained a large residual variability in the data. Predictive equations have been derived from the experimental data. The "model" which predicts T_{re} from heat loss is an imaginary sphere with a massless core, a shell with variable mass, and a surface. This peculiar sphere can be adjusted initially to represent men of different heights, weights, and fat percentages. There are other choices, such as environmental temperatures and the thermal coupling between environment and surface. The equations not only reproduce our experimental data, as of course they should, but they have also been able to predict the result of untried experiments with reasonable accuracy. Work continues.

PREDICTING T_{re} FROM HEAT LOSS

Paul Webb

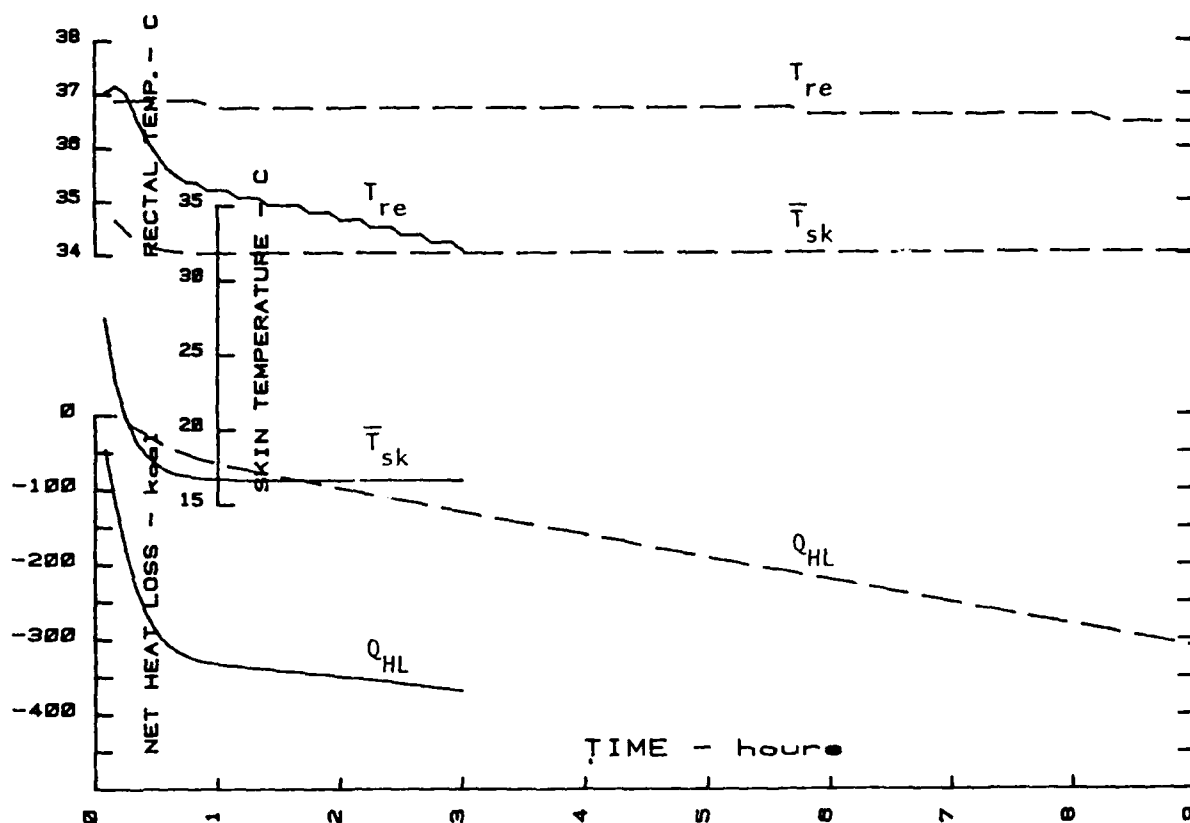
The title of my presentation concerns a goal of mine that I have pursued for many years. When we started our research we thought that all we had to do was to quantify heat loss in cold exposure, which we can do with our calorimeter. If for example we use it to remove 100 kcal from a subject's body we should see an associated change of body temperature, which we can use to determine the heat loss-core temperature relationship. This approach was not particularly successful. We have continued this work for a number of years, measuring various losses of heat and associated core temperature changes from which various relationships were formulated. Net heat loss refers to the actual loss of heat from the body corrected for metabolism of the body -- it is the residual heat loss of the body metabolism subtracted from the total heat loss.

I refer here to the abstract in your folder. It is brief and I am expanding on it. I'll show the kinds of experiments that we have done. These are experiments primarily on cooling rate-- the patterns of cooling, the man overboard situation, the downed aviator; these are examples of abrupt cooling in cold water, the most common kinds of experimental data in the literature. If you abruptly expose a man to cold water, the various parameters change in a fairly linear fashion. Temperature and net heat loss follow in steep curves of this sort. In terms of divers, we assume that they do not dive unprotected and they

have corresponding lower rates of heat loss. Some diving cold exposures last of the order of 9 hours at a mild rate of heat loss of 30 or 40 kcal/hr. These are transient or rapid functions of cooling exposures. We've done a lot of those at different rates with a lot of different people. There is one other pattern for heat loss with a rapid initial heat loss of 100 kcal or so which then decreases to a stable value which is balanced to match metabolism so the heat loss does not increase at that point. Now if rectal temperature is measured, it is slowly changing and follows the loss in body heat content and if you wait long enough it will finally come to a plateau; if you wait, you get afterdrops and other changes more indicative of acute types of exposure.

The experimental data contains graphs of varying slopes and steady state heat exposures. We have used primarily the suit calorimeter in my lab in Yellow Springs, also the bath calorimeter of Dr. Kuehn in Toronto; initially it was Al Craig's calorimeter.

How can we connect heat loss and body temperatures? Refer to graphs at end of this presentation. Three factors affect this relationship. We use rectal temperature to measure core temperature not because it is the best core temperature but because it is most widely used. One major effect is the rate of cooling. In the first graph, the effects of slow and rapid cooling in one man in different experiments are presented. The rapid cooling experiment is represented by solid lines, the slow cooling data by dashed lines --for rectal, mean skin temperatures and heat loss. The final level of heat loss is



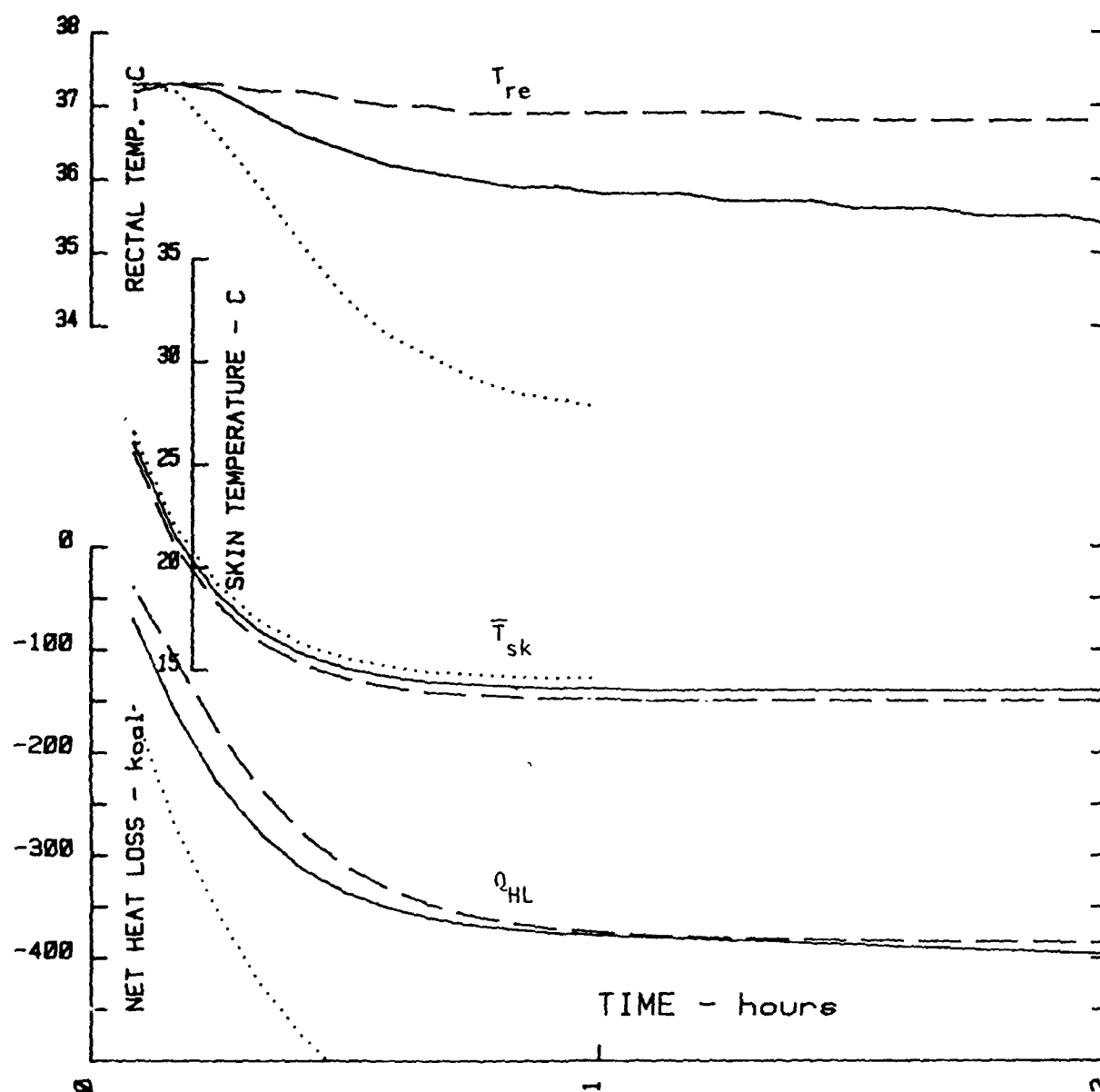
The same man undergoing either rapid cooling in 3 hours (solid lines) or slow cooling in 9 hours (dashed lines) to about the same level of net heat loss. During the first hour of rapid cooling, heat loss is about 320 kcal and T_{re} drops to the dangerous level of 35°C. In contrast, after 9 hours of slow cooling, the heat loss is about 310 kcal but T_{re} is still at 36.5°C.

The subject is 176 cm tall, weighs 72 kg, and has 18% body fat. In rapid cooling he is nude in 15°C water, head under and working at 50 watts. In slow cooling, he is well protected in cold water but losing heat at a steady rate of about 34 kcal/hour.

GRAPH 1

about the same for both experiments, about 300 kcal for two kinds of exposures but the response in the man is quite different. In the rapid cooling, the 300 kcal can be removed in one hour and the rectal temperature falls below 35°C-- an example of the "man-overboard" situation. Problems can be predicted. The experiment is in 15°C water and the skin temperature is 16°C and the heat loss fell rapidly and then slowed up. Conversely, the same-sized man who loses his 300 kcal over 9 hours is in no trouble at all. This is an important point because well-protected underwater swimmers in swimmer delivery vehicles will lose heat at this mild rate of 35-40 kcal per hour for six hours or so. They don't shiver and may only complain of mild cold stress, cold hands and cold feet, for example. There is a serious question that can be raised. Has their critical performance begun to change toward the end of this period; if so, is it hypothermia or fatigue or some combination of both? This is the big question; can the guy do the work that he is meant to do?

The other two graphs are now reviewed. Not only the rate of cooling affects the results but the degree of body fatness as well. This is not new, but I think that you will be surprised by the results. Here are three men with the same fat-free mass, about 59.5 kg. All have the same height, about 175 cm tall. The fat percentages are 30%, 15%, and 7% for the three men. The fattest fellow is hardly bothered by a nude exposure to 12°C water, head under and working; his rectal temperature drops to below 37°C in two hours. The thinnest man is in trouble in less than an hour; he will have to be lifted from the

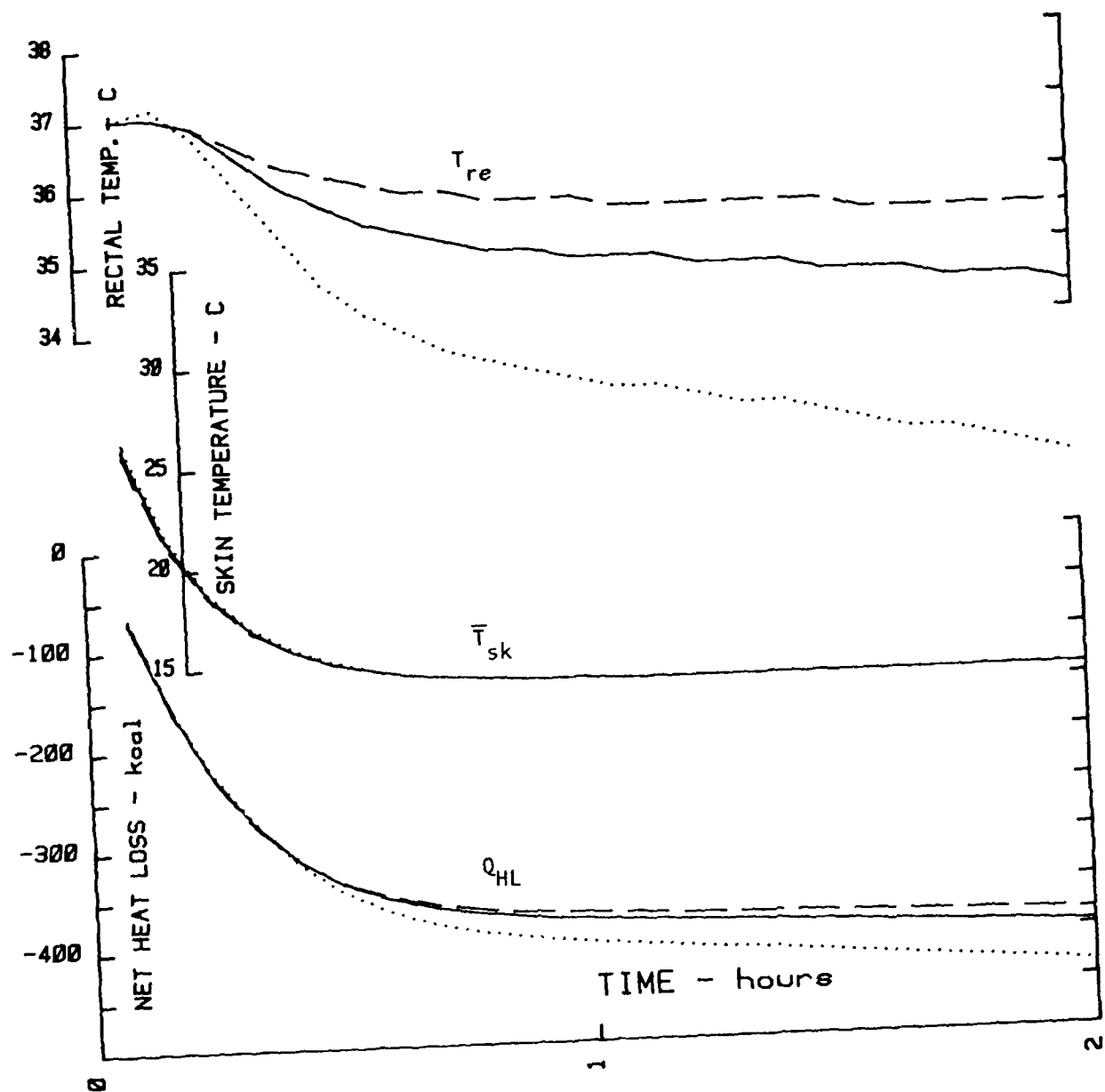


Nude men in cold (12°C) water, head under and working at 50 watts.

The three curves represent predictions for 3 men of the same heights and fat-free masses, but with different amounts of body fat.

Dashed line: Height 175 cm, weight 85 kg, 30% fat, fat-free mass 59.5 kg
 Solid line: Height 175 cm, weight 70 kg, 15% fat, fat-free mass 59.5 kg
 Dotted line: Height 175 cm, weight 64 kg, 7% fat, fat-free mass 59.5 kg

GRAPH 2



Nude men in cold ($12^{\circ}C$) water, head under and working at 50 watts.

The three curves represent predictions for 3 men of the same percentage body fat, but of different heights and weight.

Dashed line: Height 185 cm, weight 80 kg, 15% fat, fat-free mass 68 kg
 Solid line: Height 175 cm, weight 70 kg, 15% fat, fat-free mass 59.5 kg
 Dotted line: Height 165 cm, weight 60 kg, 15% fat, fat-free mass 51 kg

GRAPH 3

water. The results for the intermediate fellow are between those of the other two. So these results show that for the same lean body mass and height, variation of body fat causes the large variation in rectal temperature and in heat loss as well.

The final graph shows results pertinent for similar body fat (15%) with variation of body size (height and weight) between large and small body size. The smallest man cools most quickly, the biggest man most slowly - still functioning at the end of two hours. This should not be a surprise. If a big man loses 200 kcal, it won't bother him to the same extent as it would a little man.

These curves are drawn by my computer and pertain to cooling rate, body fat and body size. These are the inputs to the programs that generate these curves. This program could be called a "model." It consists of carefully trimmed equations that can produce our experimental data. The equations match the data. We did not start from first principles such as heat flow and geometry, etc.

The conceptual model consists of a simple sphere representing the "man." The core is the centre of the sphere and has no mass. The surface area of the sphere is that of the man and there is a shell of some kind which lies near the surface, which is not connected physically to the core except by specific thermal transfer pathways. This peculiar sphere behaves in the same manner as the equation which pertains to several years of experimental data. The graphs emphasize the importance of the differences between individuals and the cooling rate. If we only did experiments on nude men in cold water we would

mislead ourselves about the cold diver who does not dive without anything on.

Hamilton - The top line on the first graph represents the man who was cooled slowly and who has not lost much rectal temperature. He has lost 310 kcal, however! From where did he lose it and why is he in good shape and the other two are not?

Webb - That is an excellent question and I have no answer for you.

Hamilton - Did he have peripheral vasoconstriction in his limbs?

Webb - No, he hasn't lost the heat from the core; he is only mildly vasoconstricted. The data shows that he is not hypothermic. We count the calories coming off him and he only shivers in the last 15 minutes or so.

Hamilton - Did you get esophageal temperature and did it match the rectal temperature? Is this a reflection of the use of the rectal temperature site?

Webb - Yes, we measured esophageal and ear canal temperatures as well as rectal and they were all consistent in this observation. In a slow cooling experiment, the rectal temperature is a pretty good hypothermic indicator; in rapid cooling, it is a remote indicator that eventually catches up to the others. Ear canal temperature, muscle temperature, subcutaneous temperature are better. So we have this mystery. Where did the 300 kcal come from?

Hamilton - Did you measure the oxygen consumption and correct for metabolism from the heat loss?

Webb - Yes, we measured oxygen and CO₂ exchange continuously throughout the entire nine hours of exposure.

Nunneley - A similar concern can be expressed relevant to your third

graph showing the effect of body size. You show a big change in rectal temperature whereas you don't show much change in heat loss with body size. Do you have any explanation for this?

Webb - This is the most reasonable result in the three graphs. A small man losing some 400 kcal in two hours has a much greater rectal temperature decrease than a large man. The results are not expressed per unit body surface area. The smaller man does lose slightly more heat because he has a smaller surface and a better coupling between shell and core.

Nuckols - The suit designers are being saddled with the recommended 200 kcal maximum permissible heat loss that was decided on in 1976. Do we have enough information now with your model to update that criterion?

Webb - When you set criteria, you have to set them conservatively for the whole population that is affected. Do you have 60 kg men with no body fat? If so, and you set your criteria too generously, you will harm these people, whereas the bigger men will be fine. This is the problem with limits. Is there only one number like 200 kcal or is there a scale of numbers? What is acceptable to the user?

RESPIRATORY HEAT LOSS LIMITS IN HELIUM-OXYGEN SATURATION DIVING

C. Piantadosi

(formerly of United States Navy Experimental Diving Unit)

(presented by John Zumrick)

At the Navy Experimental Diving Unit, we have been interested for some time in heat loss in cold hyperbaric helium environments. Originally, this research was conducted by Dr. Kuehn and myself, and most recently by Dr. Claude Piantadosi. In a recent series of studies conducted by Dr. Piantadosi during a recent 1800 fsw dive, it became apparent that the minimum inspired gas temperature limits allowed by the U.S. Navy were too liberal, and that a new set of standards were needed. This paper describes the rationale on which this new set of inspired gas limits was derived. These new limits assume that a diver is wearing a hot water heated suit and has sufficient hot water to remain comfortable. The gas temperature limits were then computed to allow a one-degree Celsius change in rectal temperature over a four-hour mission period.

The basic heat balance equation, Equation 1,

$$S = M - (C_{sk} + RHL) \quad (\text{Eq. 1})$$

shows the relationship between changes in body heat content (S), the metabolic heat production (M), the skin heat losses (C_{sk}), and the respiratory heat loss.

During the recent 1800 fsw dive, divers breathed 15°C gas while seated in a comfortably warm environment. Oxygen consumption, as well as radiative, convective, and respiratory heat losses were measured. Based on this data, the relationship between rectal temperature and heat content was plotted and found to be a straight line, as shown in

Figure 1. From this graph, it can be seen that a change of rectal temperature of 0.25°C/hr represents a change in diver's heat content of $14 \text{ watts}\cdot\text{m}^{-2}$ ($12 \text{ Kcal}\cdot\text{m}^{-2}$).

The resting metabolic heat production, M , based on over fifty measurements in thermally comfortable hyperbaric helium environments, was $56 \pm 3 \text{ watts}\cdot\text{m}^{-2}$. These results are in agreement with figures reported by other investigators. In any thermoneutral environment, such as 33°C water, these values should be approximately the same.

As shown in Figure 2, divers wearing hot water suits are comfortable at a mean skin temperature of about 33°C to 34°C when immersed in 6°C water. In a thermoneutral environment such as this, approximately 90% of the heat loss is via the skin. Therefore, skin heat loss, (C_{sk}), for a comfortable resting diver using a hot water suit is about $50 \text{ watts}\cdot\text{m}^{-2}$.

Rearranging Equation 1 and solving for RHL:

$$-14 \text{ W}\cdot\text{m}^{-2} = 56 \text{ W}\cdot\text{m}^{-2} - \text{RHL} - 50 \text{ W}\cdot\text{m}^{-2}$$

$$\text{RHL} = 20 \text{ W}\cdot\text{m}^{-2}$$

Thus, a respiratory heat loss should not exceed $20 \text{ W}\cdot\text{m}^{-2}$ for a rectal temperature change of $0.25^{\circ}\text{C/hr}^{-1}$.

The final step in establishing new limits is to relate this respiratory heat loss to a minimum inspired gas temperature. Respiratory heat loss consists of a convective and evaporative component. As depth increases, the evaporative component of respiratory heat loss becomes increasingly small. Therefore, we will neglect this heat loss. Thus, at great depths, respiratory heat loss rates corrected for diver size is given in Equation 2:

$$\text{RHL} = \frac{\dot{V}_E (pC_p \Delta T)(k)}{A} \quad (\text{Eq. 2})$$

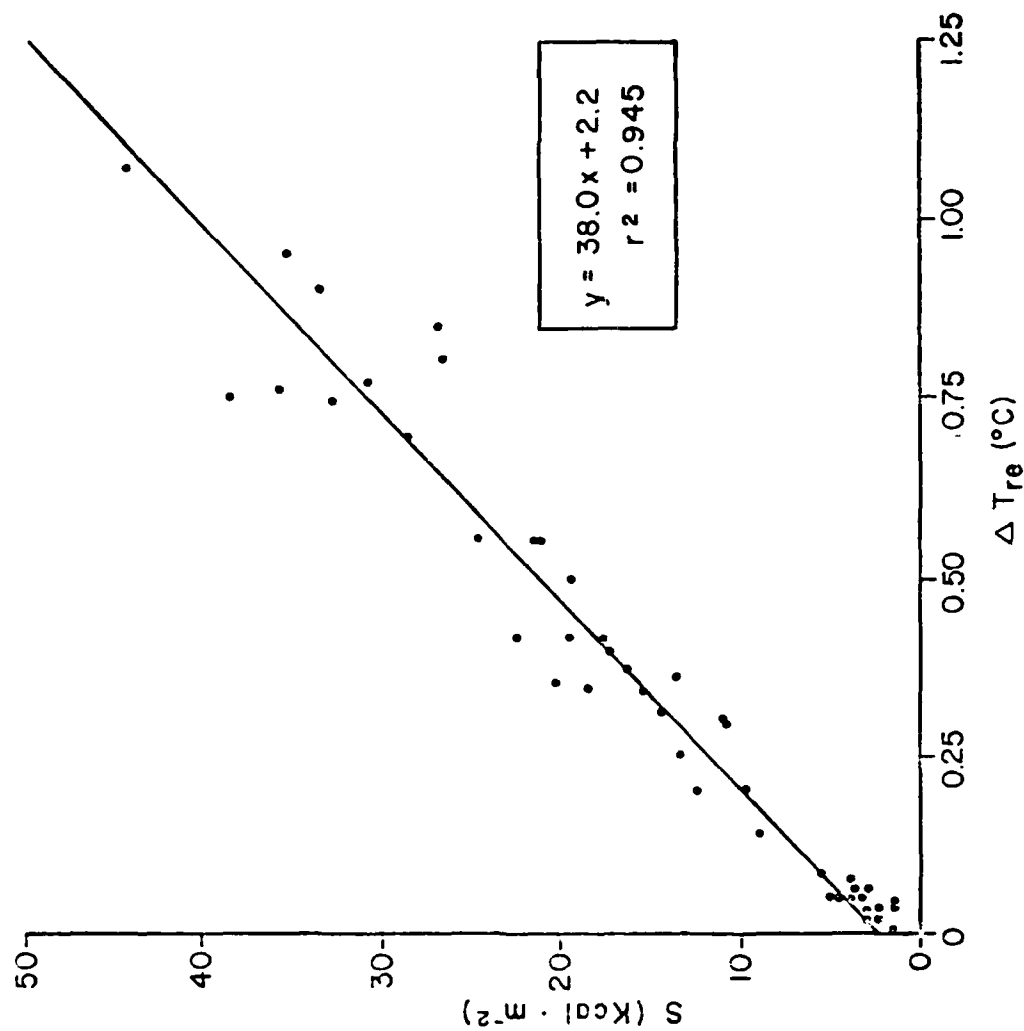
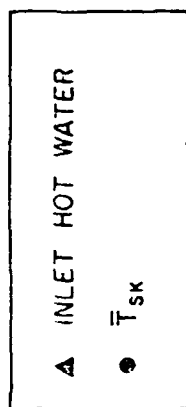
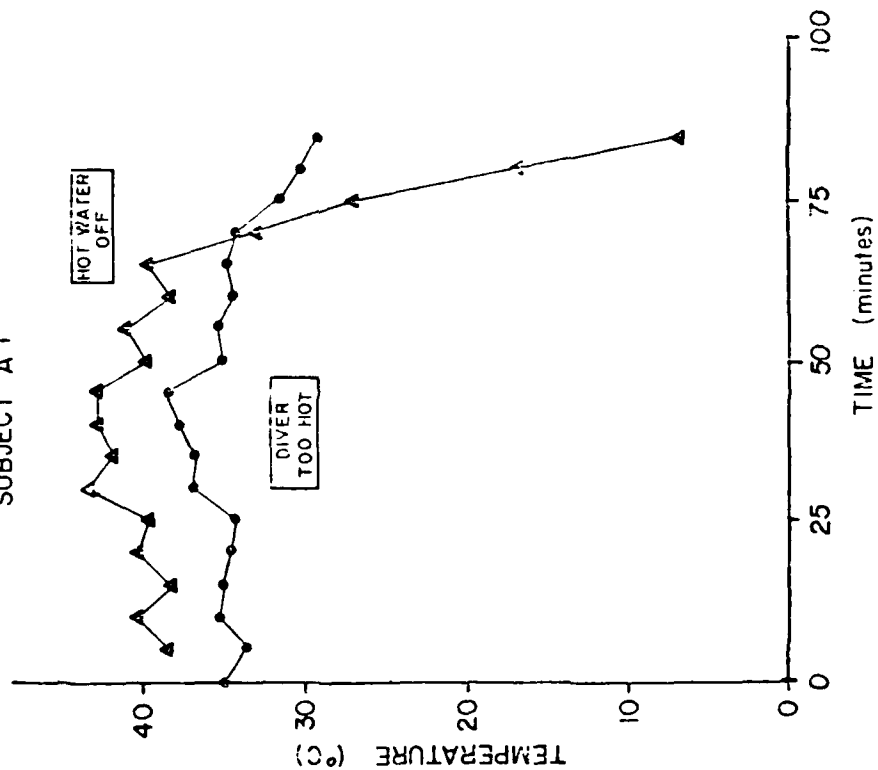


FIGURE 1. Relationship between change in body heat content (S) and change in rectal temperature (ΔT_{re}) in 4 resting subjects breathing cold helium-oxygen while maintaining normal skin heat loss.



SUBJECT AT



SUBJECT PS

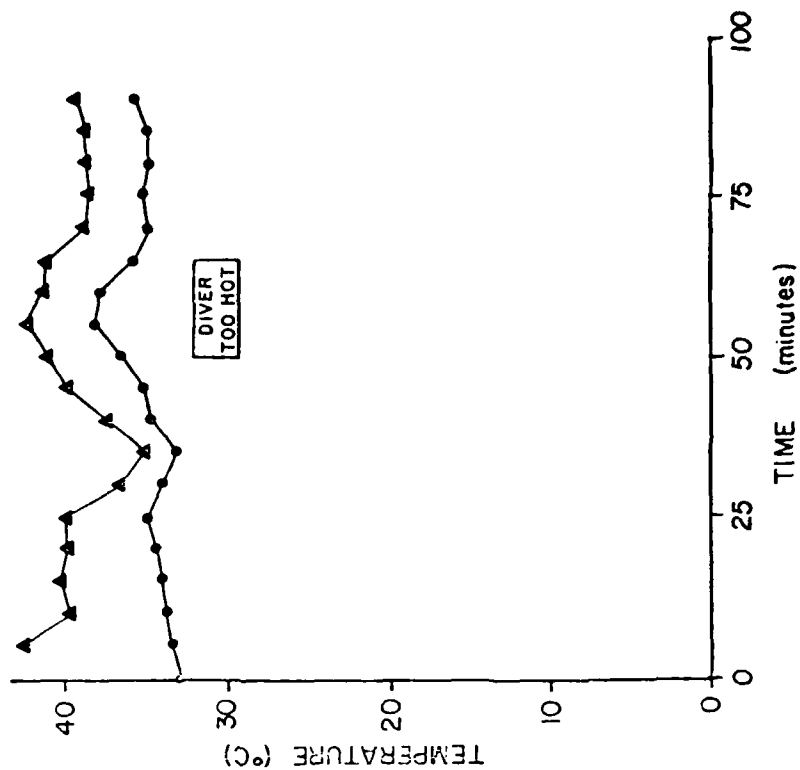


FIGURE 2. Inlet hot water temperatures and mean skin temperatures ($\bar{T}_{sk}$) in two resting subjects wearing NRV hot water suits for 6°C water temperature at 10 FSW.

where:

RHL = Convective Respiratory Heat Loss ($\text{W}\cdot\text{m}^{-2}$)

p = Gas Density ($\text{g}\cdot\text{l}^{-1}$)

C_p = Constant Pressure Specific Heat ($\text{cal}\cdot\text{g}^{-1}\text{°C}^{-1}$)

$\dot{V}_E$ = Minute Ventilation ($\text{L}\cdot\text{min}^{-1}$)

ΔT = Difference in expired (T_e) and inspired (T_i) temperature

k = Conversion constant $\text{cal}\cdot\text{min}^{-1}$ to $\text{W}\cdot\text{m}^{-2}$ (.0697)

A = Diver's surface area in m^2

Substituting into the above formula a respiratory heat loss limit of $20 \text{ W}\cdot\text{m}^{-2}$, a resting ventilation of $8\text{-}10 \text{ L}\cdot\text{min}^{-1}$ and a surface area of 2 m^2 , this equation becomes:

$$\Delta T = 72/(pC_p) \quad (\text{Eq. 3})$$

Since (pC_p) is a function of depth, if the relationship of T_i to T_e is known, then the minimum T_i can be calculated.

Figure 3 shows the relationship of inspired and expired temperatures measured with a fast responding thermistor at a variety of depth and inspired temperatures. This relationship can be expressed by the following equation:

$$T_e = 0.28 T_i + 25.4$$

Since: $\Delta T = T_e - T_i$

Then: $\Delta T = 0.28T_i + 25.4 - T_i$

And: $T_i = 35.3 - 1.39 \Delta T$

Then using this relationship, Equation 3 becomes:

$$T_i = 35.3 - 100/(pC_p)$$

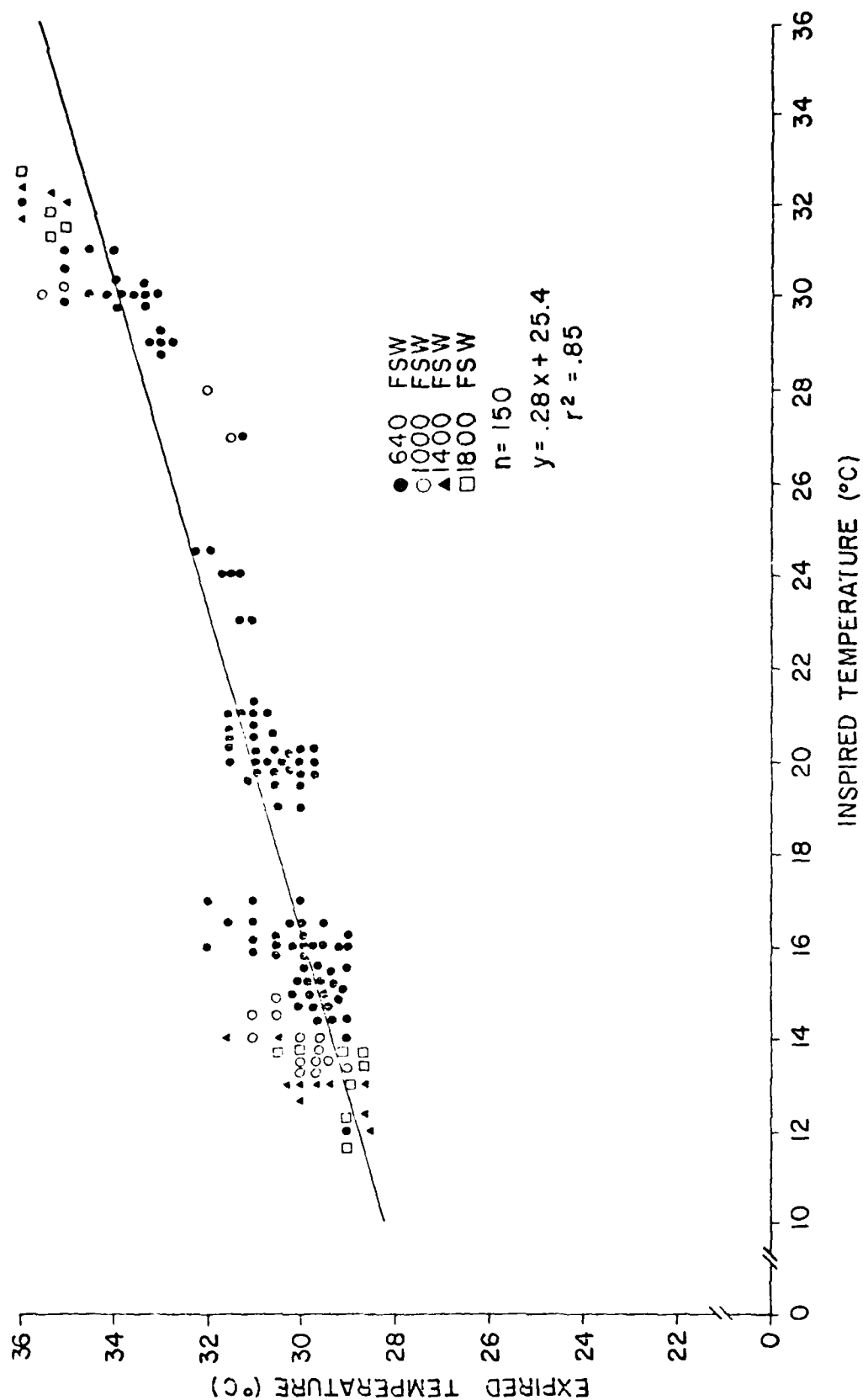


FIGURE 3. Inspired versus expired helium-oxygen temperatures over a wide range of gas temperatures and depths.

If a helium-oxygen mixture with an oxygen partial pressure of 0.4 ATA is breathed, then the minimum inspired gas temperatures are given in the Figure 4.

These limits are considerably higher than those previously followed. As a result, gas heating may be necessary at depths of 300 feet versus 600 feet for the old limits. At 1000 fsw for example, the old limits required a minimum inspired gas temperature of 13.3°C while the new limits require the gas be heated to 21.9°C.

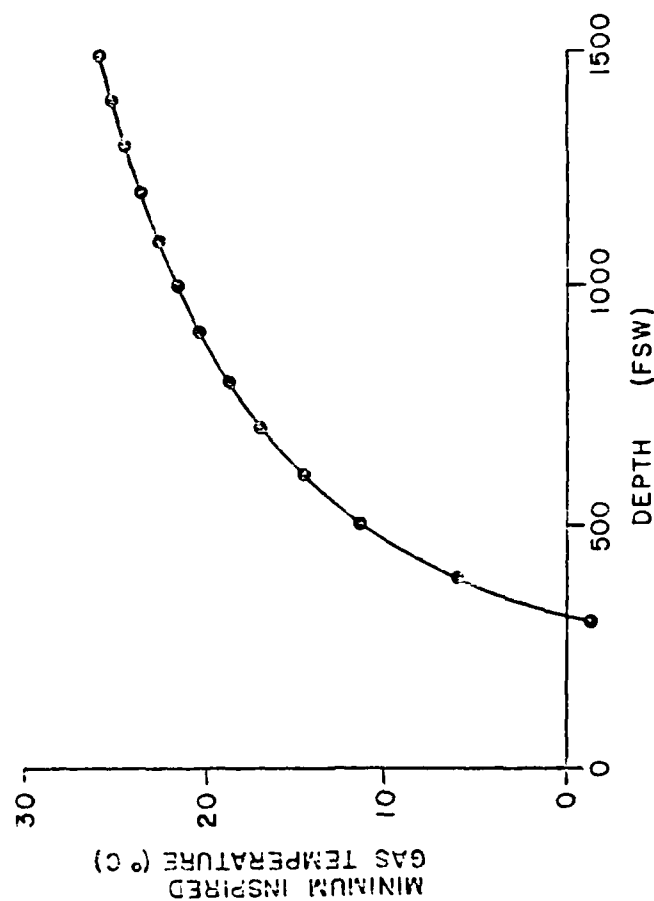
Manalaysay: One problem I have is with the skin convection heat loss that you are measuring in the chamber environment. Are you trying to get a prediction for a diver in the water? Would the 90/10 breakdown still hold in that case?

Zumrick: There is some limited data to suggest that. Skin temperatures in a hot water heated suit are about the same as those for an oxygen-helium environment. The measured heat losses using heat flow transducers are also similar.

Manalaysay: But the rates of heat conduction are different in the two environments.

Zumrick: Values of heat flow measured using heat flow transducers are similar.

Hayes: These measurements are stated to be pertinent to rest. Do you think that it is safe or logical to use this sort of expired/inspired regression system that is pertinent to rest in a dry experimental situation rather than one pertinent to a helmet situation? In helmet tests that we have done, we get a much higher expired temperature than



DEPTH (FSW)	MINIMUM INSPIRED GAS TEMPERATURE (°C)	(°F)
300	-1.0	30.2
400	6.0	42.8
500	11.5	52.7
600	14.1	57.9
700	17.0	62.6
800	18.7	65.7
900	20.5	68.9
1000	21.9	71.4
1100	22.7	72.9
1200	23.7	74.7
1300	24.6	76.3
1400	25.3	77.5
1500	25.9	78.6

FIGURE 4. Proposed minimum inspired helium-oxygen temperatures for saturation depths between 300 and 1500 FSW.

your regression curve indicates. I wonder if there is not a lot of difference between an experimental set-up that uses nice wide-bore tubing and a practical helmet system using demand valves. Your limits might need to be more conservative for such a practical case.

Zumrick: I don't know what the differences are.

Hayes: Was there one thermistor right in front of the mouth measuring the respiratory temperature breath-by-breath?

Zumrick: Yes. We derived the inspired/expired temperature relationship from the fast-responding thermistor. Some people are concerned about whether we overly quantitate heat loss by using that relationship. Initially-expired gas temperature is not the end-expired gas temperature--it's something else and probably not as warm as the end- expired gas.

Hayes: We do tend to use the two extremes. One gets a trace of breath-by-breath temperatures and the two extreme temperatures are put into the standard. A real problem of quantifying this absolutely requires some sort of integration of instantaneous flow and expiratory gas temperatures--then we can feed reliable data into that sort of equation.

Zumrick: At least in this sense we get an overestimation of respiratory heat loss.

Wissler: I would like to ask Phil Hayes how the proposed minimum temperatures compare with the temperature that causes respiratory difficulties owing to mucus formation. Are these limits high enough so that these problems will not be observed?

Zumrick: They are considerably higher than those proposed by Hoke and

Jackson. Associated with this study was another that I did, concerned with forced expirations on these individuals trying to see if 15°C inspired gas at 1800 fsw, a condition which was beginning to approach these limits, was beginning to cause any change. We did see some significant changes which were attributed to the slightly different gas properties acting on the airways rather than a real change in the lungs.

FREQUENCY ANALYSIS OF SHIVER IN HUMANS AND ITS ALTERATION BY THE
TEMPERATURE OF INSPIRED AIR

R. Pozos

University of Minnesota at Duluth

We have a hypothesis on shivering which might be of benefit to those interested in hypothermia--specifically the several mechanisms behind shiver. (See Slide One; all slides shown at end of paper.) Basically the hypothesis is that, inherent within normal human systems, there is a physiological action tremor --that as a person moves his limbs there is an increase in amplitude of an involuntary oscillation and, since this involuntary oscillation occurs during movement of the limbs, we call it physiological action tremor. Based upon various pathologies or inputs to the controlling mechanisms behind this tremor, one may see a number of things. One could possibly see that due to lesions of the cerebellum or basal ganglia one could see Parkinson and other pathological disorders. You could also see essential tremor and in the case that we will discuss today, based upon inputs into the system that controls physiological action tremor, you could also see shiver. That's basically the hypothesis which is a little different from what is classically preached, namely that shiver is something that comes about when the hypothalamus is triggered. That might be the case, but inherent in all of us right now that mechanism that is set to oscillate is already functioning.

The second slide is from Montcastle with some added caricatures. There are thermal receptors located on the skin, on the viscera

and perhaps in the respiratory system. These receptors sense differences in temperature--some for cold, pain or heat. All send signals to the hypothalamus--depending on what signal has been received by the hypothalamus and higher centers, in addition to some kind of reference point set by the hypothalamus center itself, you are seeing the triggering of different responses. We are going to discuss the influence of the hypothalamus, in terms of its control on muscles, in terms of shiver. The hypothalamus is the home of the autonomic nervous system and if this is triggered then one can trigger control to sweat glands and to the vasomotor system. I don't separate these systems easily.

The case that I would like to present here is that even though the hypothalamus is considered the home of the autonomic nervous system that is involved in the generation of shiver, one has to also include in his thinking the idea of spinal cord loops, that it isn't necessarily essential for the thermal receptors to trigger something within the hypothalamus to begin this, that there might virtually be sub-cortical or sub-hypothalamic loops that might influence some aspects of shiver at a segmental level; so, superimposed on this diagram there might be some more primitive reflex loops that are involved in the thermogenesis of shiver. Next slide (Slide Three).

This is basically our system, using surface electrodes that most of you are aware of, looking at the muscles in question using accelerometers to record the motion that we are going to be talking about. Fourth slide. As has been classically discussed, one of the first things that happen in shiver is a tensing of muscles. We have looked

at this a number of ways, for example, cold water or cold rooms. In either case, for example, immersion in the cold water of Lake Superior at 8°C, we had surface electrodes on the latimus dorsi and the masseter on the soleus and an accelerometer on the patella to look at motion. As has been reported in the literature but has not been too well quantified, first there is a tensing that develops in the neuromuscular system when the shivering is about to begin and you can see this tensing right here.

The shiver you see is the actual synchronized EMG activity that is recorded from the soleus that produces the shiver. Fifth slide.

Frank shiver is the old-time thermal physiological classification of massive shivering all over the body-- you can see the soleus, pectoralis major, trapezius and the accelerometer. Looking at the values which should be important to the modellers here, we have this in terms of microvolts and they give you the calibration. We have individuals, and I'll show you this later on, who don't have this kind of activity but they have some kind of synchronized activity, not at the same amplitude. They are shivering supposedly but not at the same amplitude. Supposedly, a non-shivering individual doesn't shiver but, by placement of EMG sensors on those muscles, we can demonstrate synchronized EMG activity. It may not be at the same amplitude as for those people who grossly shiver but there has been some increase of amplitude of synchronized EMG activity. Next slide (Slide Six).

This is shivering in cold water immersion in latissimus dorsi, quadriceps, masseter and deltoids. What is of interest is that this

individual claims not to shiver but on inspection of the deltoids and masseter, synchronized EMG activity is observed. If you take the individual out and have him relax, you would have straight lines. This is raw physiologic data that has not been treated or integrated.

The same thing is shown in the seventh slide. When we examine individuals who are in low shiver, these individuals have evidence of shiver relative to their control values. Next slide (Slide Eight).

This is an individual who has been in cold water. Same thing.

We were interested in the frequency analysis of such data (as has been reported by Stewart and others). We had two questions. One was "What was the frequency of shiver increment at these various muscle groups?" and the other was "Whether or not there is a coordination of phase, whether or not the EMG activity was synchronous?" We argued that if the phase analysis and frequency were the same then you could have a case for a central pacemaker for shiver. If the frequencies and phases were different, we could assume that the hypothalamus was involved but that much of the control was governed by spinal cord generators, perhaps more than one. Ninth slide. Another example of an individual who claims not to shiver but shows very nice synchronized EMG activity from the rectus abdominus.

Tenth slide. We take the analog data and run it through a PDP-12 computer hooked up to a Cyber 171 for frequency analysis. This is shiver for cold water immersion. Frequency and amplitude are shown in the masseter at 7 hz; something is going on as in the quadriceps at 7 hz. At the latissimus dorsi, something else is going on. Some of

these points do not coincide.

Slide 11. After cold water immersion there is a significant shift in frequency of the leg muscles when the individual is shivering after cold water immersion when standing up. The reason for this might be that shivering more than likely is due to hyperactive stretch reflexes and when you are in the water you are not activating so much the stretch reflexes of the lower limbs. When you are standing up, you might be activating the stretch reflexes to a greater extent and activate the shift in frequency. One could therefore say that when you shiver in air it is not the same thing as shivering in water based upon frequency analysis. Now looking at phase analysis, at the dominant frequencies that are found, if you look at 5 hz in the deltoids and compare them with 5 hz in the rectus abdominus, there is no synchrony in terms of firing. If there is an oscillator going on then it is not based on a central pacemaker because, if there was one, we would see a coherence in phase. Next slide (Slide 12).

After looking at phase and frequency analysis of EMG in humans, we were interested in the autonomic system and what influence the temperature of inspired air would have on the amplitude of shiver. In the old literature according to Cance and others, the temperature of inspired air was considered to have a significant effect on the amplitude of shiver. That used to bother me, but consider two facts. If the core is to be kept warm it might make good sense to have temperature sensors in the mouth or respiratory system to sense the temperature of inspired air so that there might be a signal that goes by way of spinal

reflex loops that increase shivering when the air is cold. Another fact is that in the literature on respiratory involvement with cardiovascular systems during inspiration there has been documentation of a pronounced vasodilation that occurs and is driven by the respiratory system. The respiratory tract system itself is more than a conduit for movement of air. It has a pronounced number of receptors that are sensitive to inspiration and expiration volumes and they may be tied to systems that control heart rate and vasodilation. Next slide (Slide 13).

We had a seated individual breathing air in a cold room and in frank shiver. His nose was plugged and he was breathing cold air from a tube. When he expired there was a dramatic increase based on spectral analysis of the EMG amplitude of the masseter and in the trapezius as well. We were also interested in what happened when one inspired hot humidified air. We were intrigued with rewarming of hypothermic victims and I wanted to see what would happen. Next slide (Slide 14).

This person is breathing warm humidified air at this point in the same method as discussed for the last slide. You can see that she is in shiver here and then there is a dramatic decrease in the EMG of the pectoralis major, the trapezius and the amplitude of shiver recorded from the patella. If one does spectral analysis, this is motion of the knee before the hot air— about 7 hz and amplitude 4500. After inhalation of hot air the frequency peak is about the same but a dramatic decrease occurs in amplitude (Slide 15). There are several criticisms of this research.

One is that hot humidified air might warm the heart and thereby the hypothalamus, even though we are monitoring rectal and tympanic temperatures. This is an important question. To answer this, we took three subjects and procainized the mouth. We made it numb to sensing nervation as far back as we could. Those were done last week. When these subjects breathed hot humidified air, there was no change in amplitude of shiver. So one could argue that there might be temperature sensors in the mouth or some other particular effect of procaine that controls the amplitude of shiver. Next slide (Slide 16).

This slide concerns hypothermia and alcohol. People who are drunk do not shiver as much as when sober. If you look at the person who shivers as a control, compared to himself when drunk (0.1 mg % alcohol), the pronounced shiver in the control case disappears on intoxication. This documents what is already well known, namely that a person that is intoxicated does not shiver as well. There is not a synchrony of firing of the motor units to produce nice shiver. Next slide (Not included in proceedings).

This slide documents that the dung beetle shivers and the point to be made is that even though we are concentrating our efforts on man and vertebrates, the invertebrates as well use shiver for protection against the cold. Next slide (Slide 17).

One of the interesting problems is the inability to move when the individual shivers. Some people say this is due to fatigue and others to other mechanisms. If shivering is caused by hyperactive

stretch reflexes, whether at the segmental level or at the super-segmental level or both, we should keep in mind that we are looking at the timing of the EMG signals. One may consider the timing between the antagonistic muscles even though there is a thing called tensing which might cause some of the inability to move when the individual is very cold. In some of our initial work we are seeing some shifts in terms of the timing of antagonistic EMG signals. If one takes a look at the extensor and flexor in the control situation you can see that the control signals are 180° out of phase. As the individual begins to shiver there is an attenuation of that timing so that these two EMG's line up more and more so that theoretically one could have a poverty of movement associated with hypothermia due to alteration of the EMG timing.

Webb - Do you have any idea of what the oxygen consumption or metabolism would be in relation to your EMG?

Pozos - Dr. Wittmers is the respiratory/thermal physiologist working with me and he is beginning to get these measurements. What I wanted to do was document shivering as a function of temperature of inspired air and we are now moving into those classical measurement areas of oxygen and CO₂ exchange.

Golden - Have you done anything to see if this shiver/non-shiver has any correlation with oxygen consumption?

Pozos - No, we intend to do this. The shivering is there though, indicated by the EMG signals.

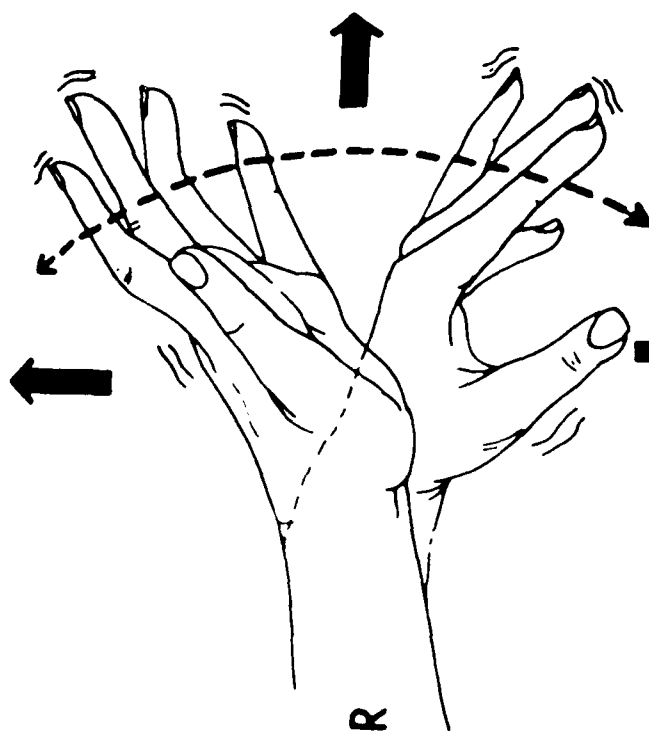
Hayward - Do you have any idea if the respiratory temperature

sensors are in the nasopharynx, the lips, the buccal cavity? Is there lip warming as well?

Pozos - Yes, there is. To answer this question, we will go to more animal and human work. These temperature sensors have been documented to drive the thermoregulatory mechanism at a spinal cord level.

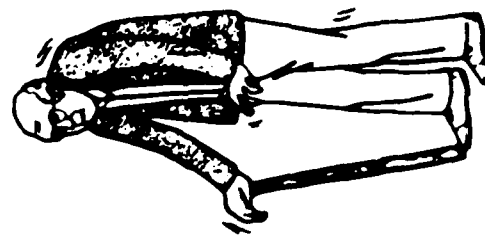


Shiver

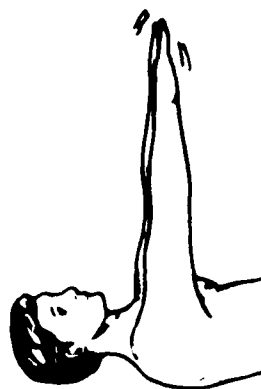


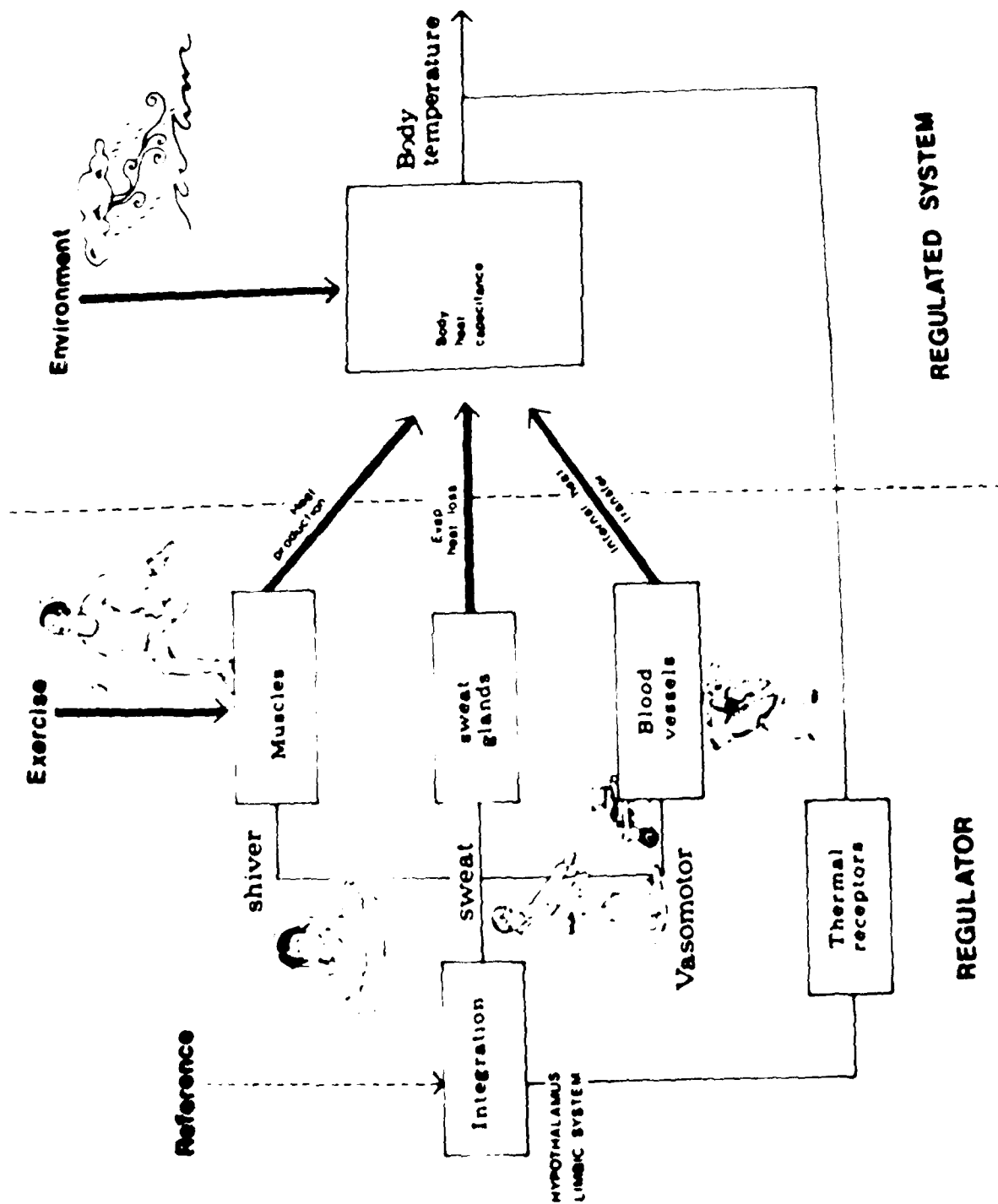
(PAT)
PHYSIOLOGICAL
ACTION TREMOR

Parkinsonian
and other
Pathological tremor

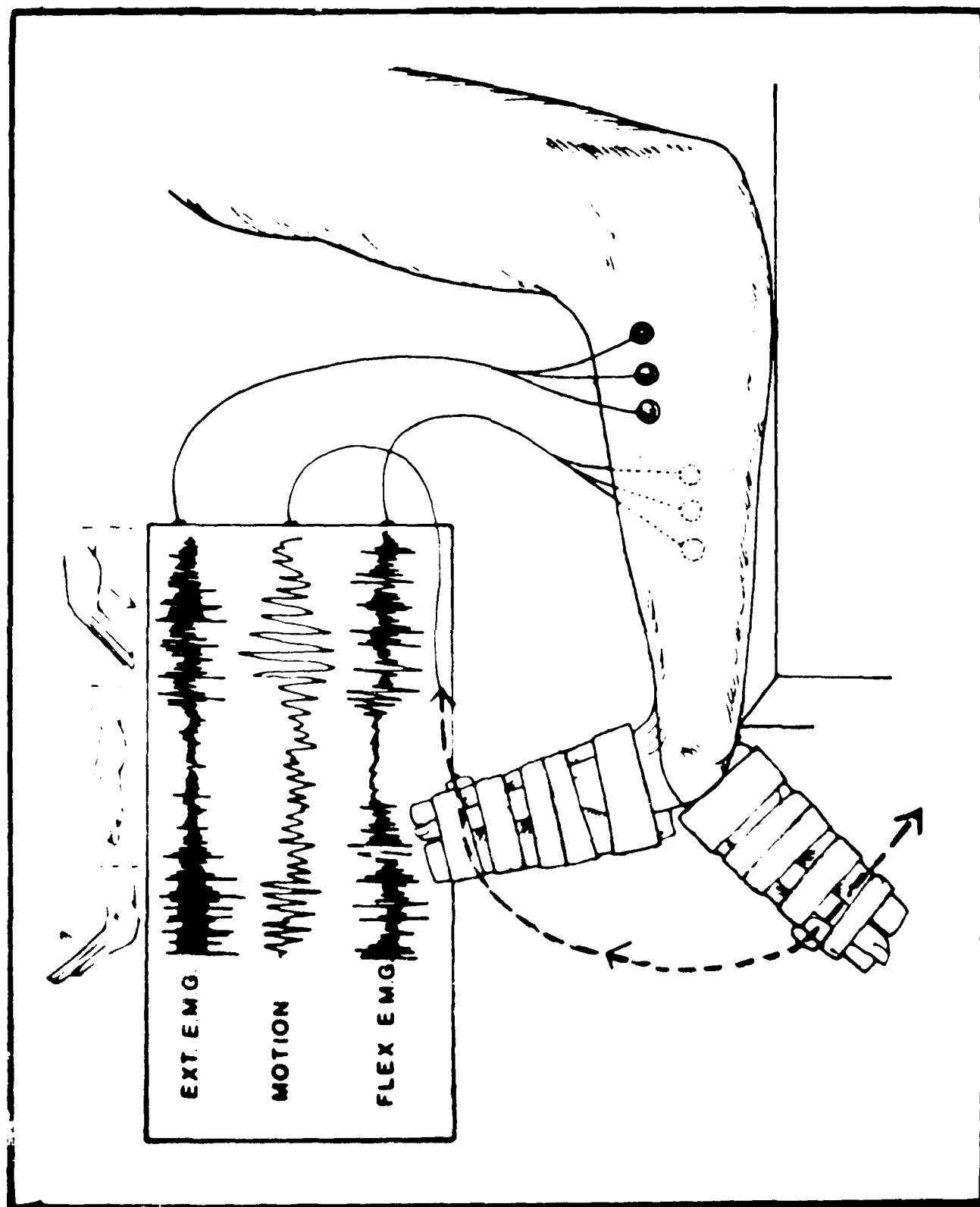


Essential tremor





SLIDE 2.



SLIDE 3.

TENSING + SHIVERING

EMG

LATISSIMUS DORSI

296.9 μv

EMG

MASSETER

200.0 μv

EMG

SOLEUS

187.5 μv

ACCELERATION

700.0 cm

← 2 sec. →

FRANK SHIVER

SOLEUS

EMG



472.2 μV

PECTORALIS MAJOR

EMG



375.0 μV

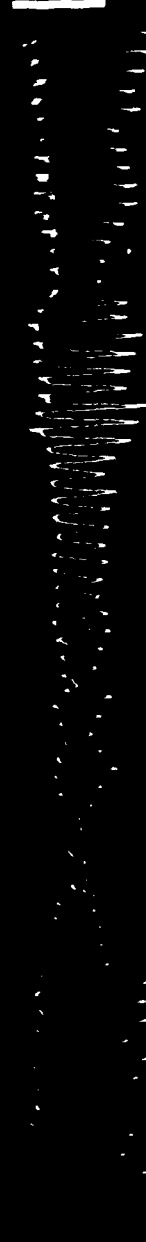
TRAPEZIUS

EMG



350.0 μV

ACCELERATION



525.0 cm/sec^2

← 2 sec →

SHIVER DURING COLD WATER IMMERSION

EMG

LATISSIMUS DORSI

3.5 μ v

EMG

QUADRICEPS FEMORIS

3 μ v

EMG

MASSETER

2.25 μ v

EMG

DELTOIDS

1 μ v

2 sec. $\rightarrow$

SHILLER

SHIVER AFTER COLD WATER IMMERSION

EMG

LATISSIMUS DORSI

35 μ v

EMG

QUADRICEPS FEMORIS

37.5 μ v

EMG

RECTUS ABDOMINIS

45 μ v

EMG

MASSETER

12 μ v

2 sec. $\blacktriangleright$

SHIVER AFTER COLD WATER IMMERSION

ENG

LATISSIMUS DORSI



ENG

QUADRICEPS FEMORIS



ENG

RECTUS ABDOMINIS



ENG

DELTOIDS



← .5 sec. →

SHIVER AFTER COLD WATER IMMERSION

EMG

LATISSIMUS DORSI

35 μ v

EMG

QUADRICEPS FEMORIS

15 μ v

EMG

RECTUS ABDOMINIS

45 μ v

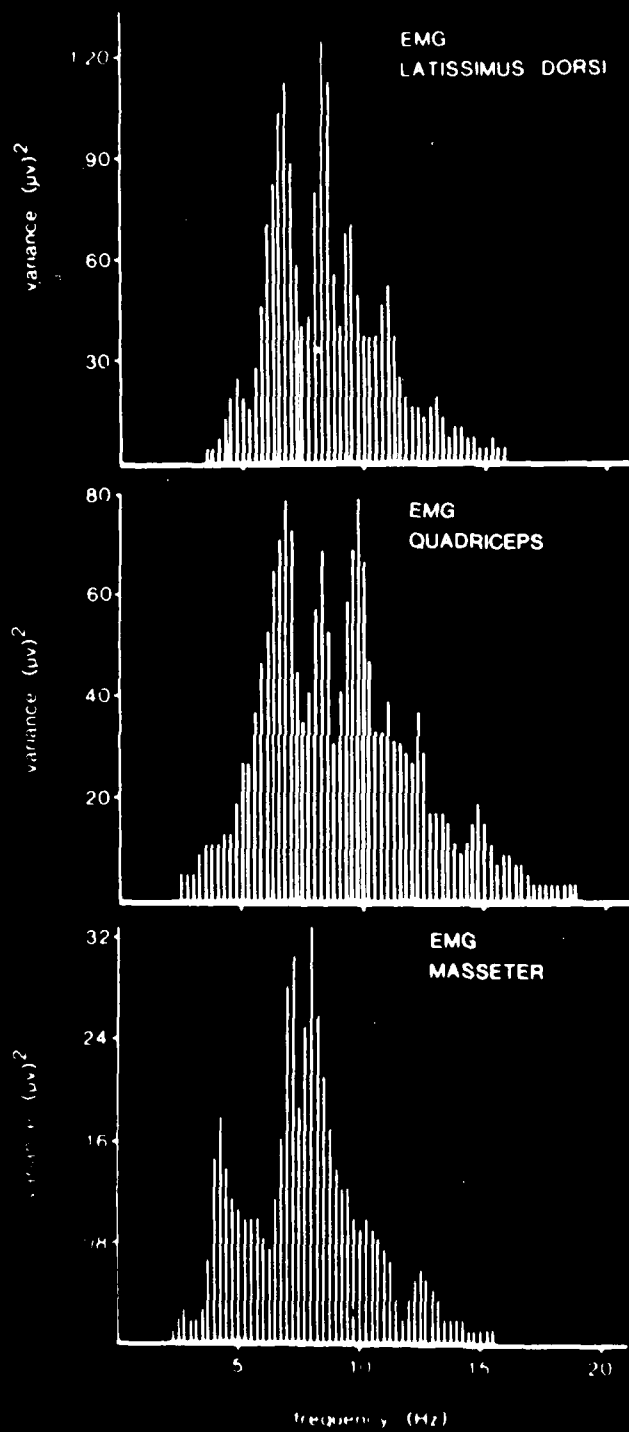
EMG

DELTOIDS

2 μ v

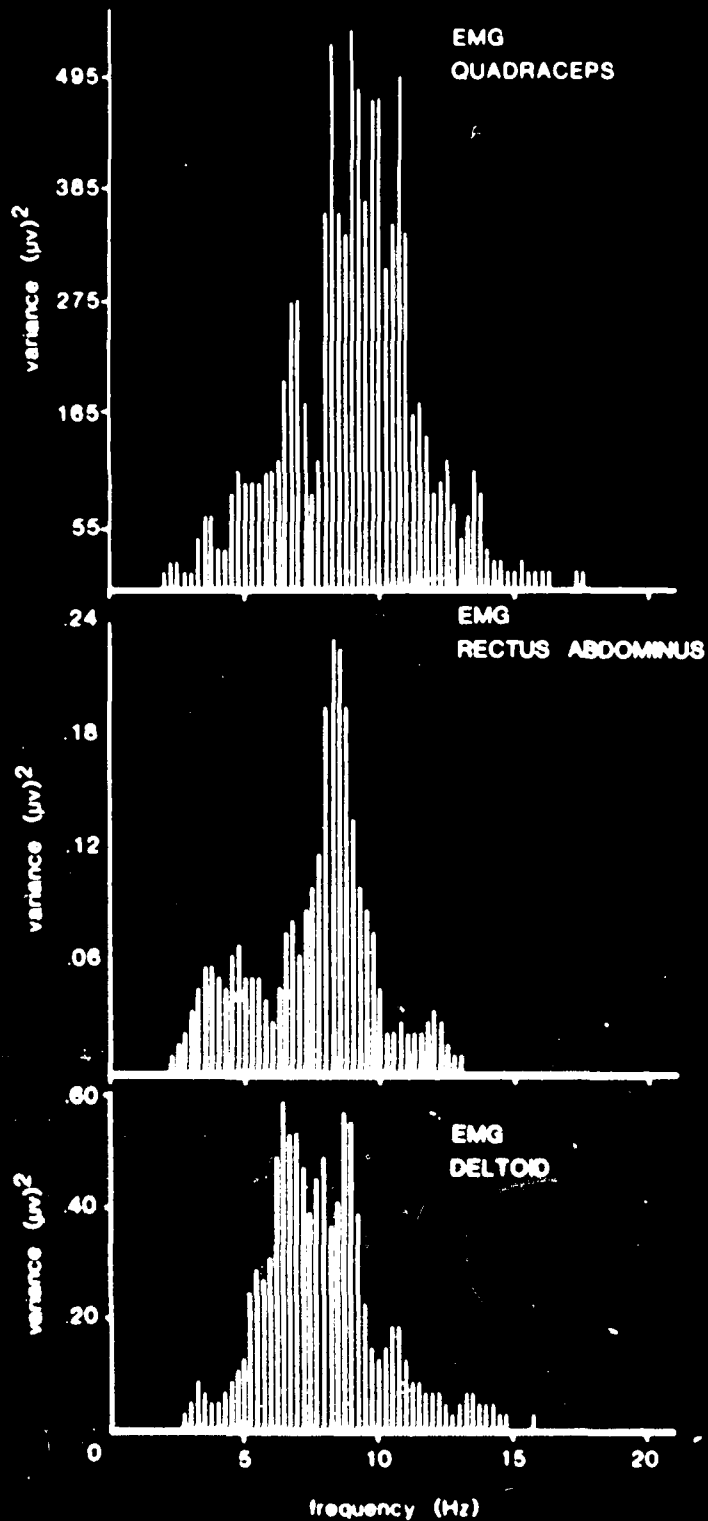
2 sec. ▶

SHIVER DURING COLD WATER IMMERSION



SLIDE 10.

SHIVER AFTER COLD WATER IMMERSION



THE EFFECT OF BREATHING WARM AIR (40°C+) ON SHIVERING

EMG

SOLEUS



EMG

PECTORALIS MAJOR



EMG

TRAPEZUS



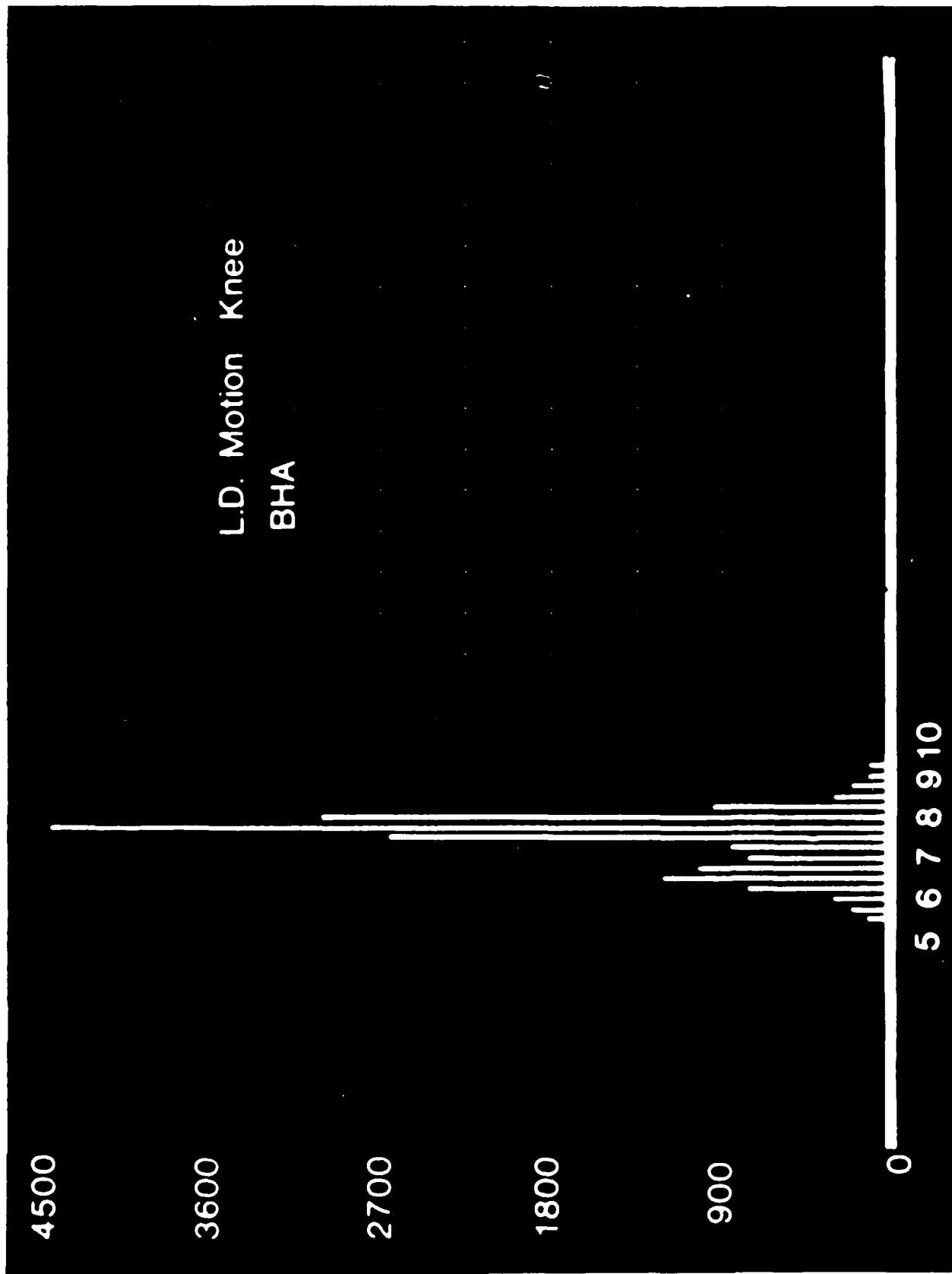
ACCELERATION

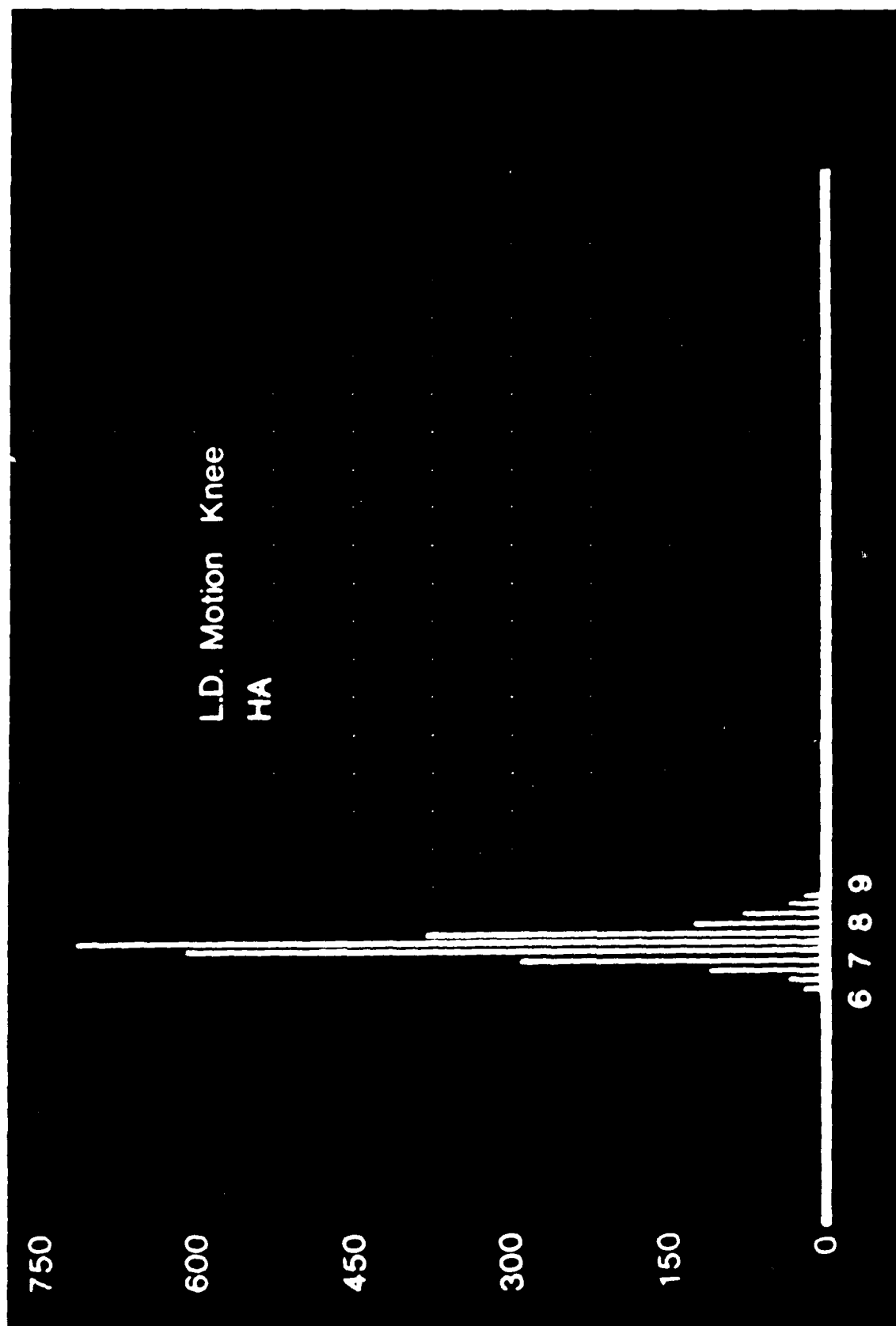
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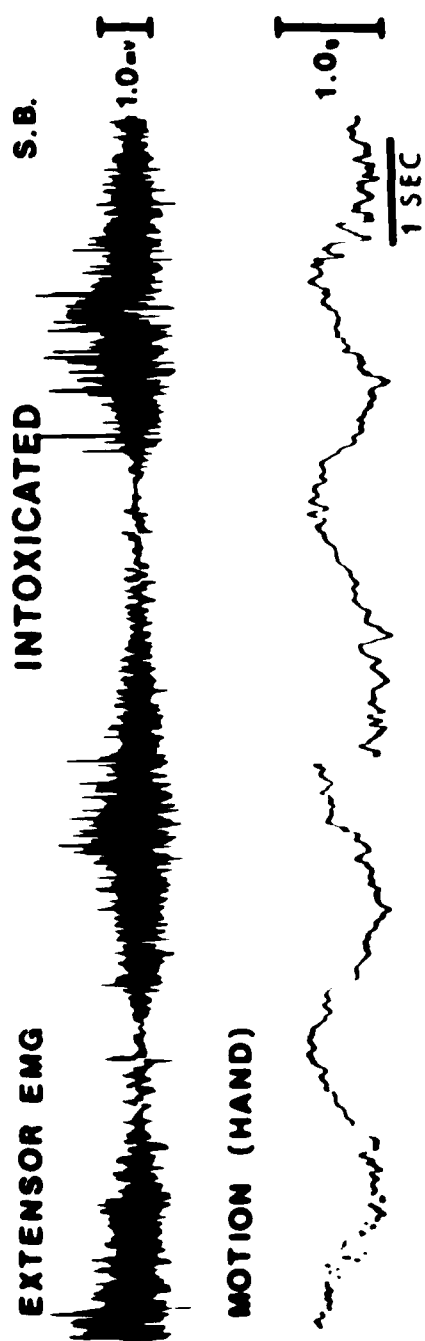
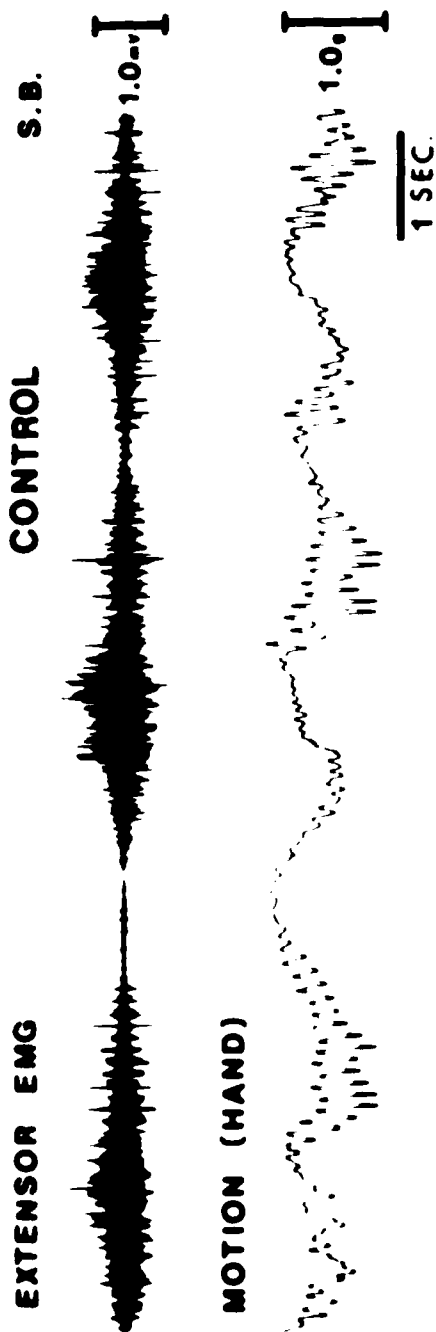


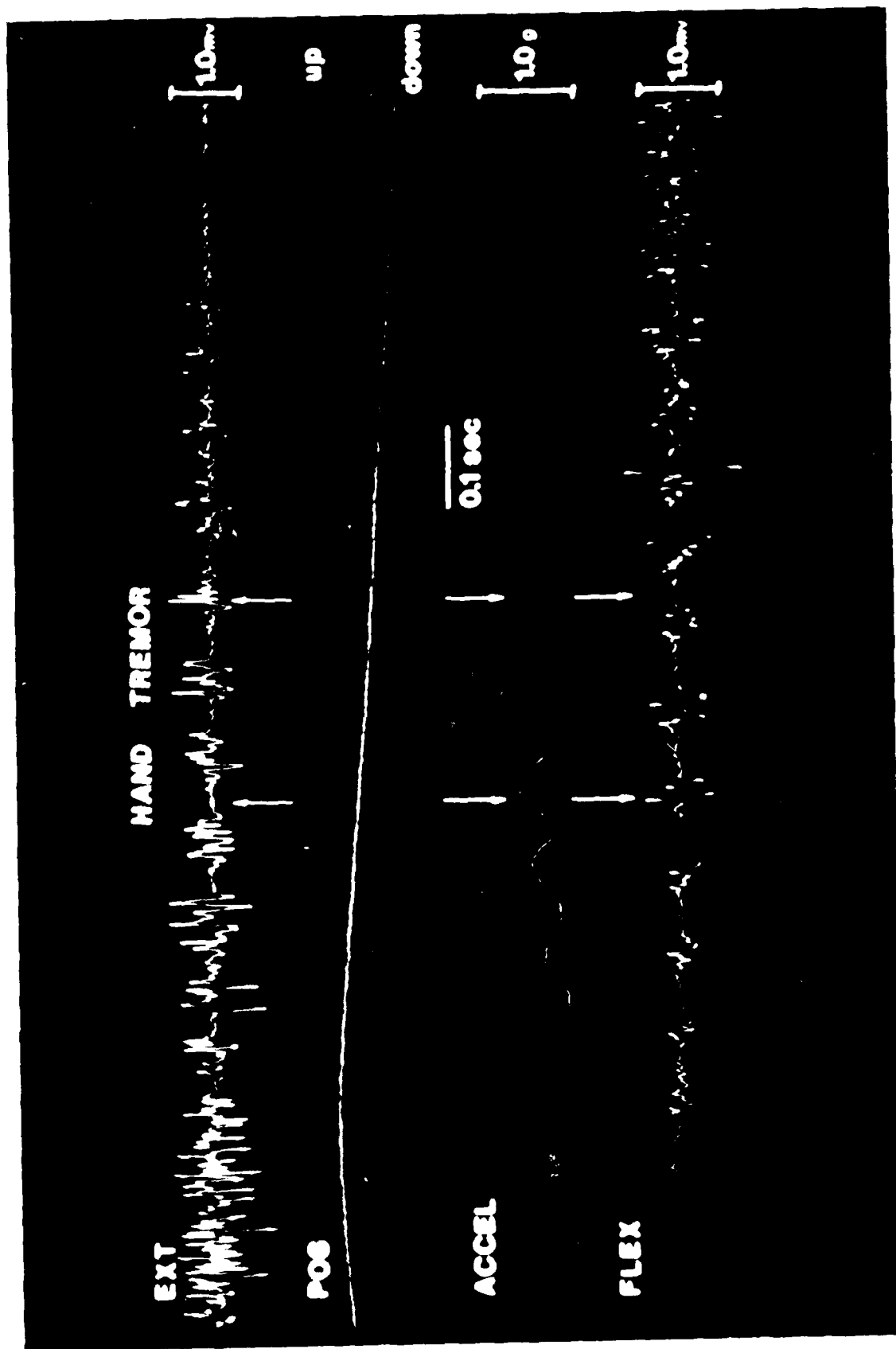
BREATHING WARM AIR

TIME









THE EFFECTS OF COLD STRESS ON VENOUS GAS BUBBLE PRODUCTION IN MAN

R. Dunford and J. Hayward

(Presented by J. Hayward, University of Victoria)

ABSTRACT

The effect of cold stress on venous gas bubble production was studied using Doppler ultrasonic monitoring. Ten subjects participated in four exposure regimes carried out at 78 feet on an underwater platform for 38 minutes of light exercise in 10°C water. Two cold exposures and two warm exposures were each followed either by rewarming in a heated bath or by endogenous heat production while insulated in a sleeping bag.

Results showed that for the cold dives compared to warm dives air consumption increased 29%, rectal temperature dropped 0.8°C at the end of the dive, mean skin temperature dropped 11°C, and cooling rate correlated with mean skinfold and endomorphy ($p < .001$). A three-fold increase in bubble counts ($p < .025$) was observed following the warm dives compared to the cold dives. The results suggest that cold stress affects peripheral circulation to inhibit inert gas uptake in the periphery. The effect of rewarming regimes on bubble production after cold dives was not conclusive.

(Note that this paper presented at the Workshop is published in the March, 1981, issue of Undersea Biomedical Research.)

COOLING IN DIVERS FOLLOWING LOSS OF HOT WATER HEATING:
LIMITS OF ENDURANCE AND THE SUBSEQUENT REWARMING PROCEDURES

Philip Hayes

Admiralty Experimental Diving Unit

There have been a number of anecdotal accounts from the North Sea about the length of time that divers can continue work once they lose their hot water heating. On the commercial side, I've heard of the odd case of work continued for an hour after hot water loss in fairly shallow water. It's difficult to get good data on these people.

The standard RN practice is that, if divers lose their heating, they are to be brought back to the bell within two minutes.

There is a very simple experiment that can be done to see what happens when you turn off the hot water heating of divers and try and keep them working outside the bell. We had the following questions to ask. What were the measured respiratory losses from somebody in this situation? What happened to rectal and skin temperatures? Were there any differences due to the wearing of different suits by the divers?

We used three suits, the standard open-circuit type of suit, the Dick Long NRV2 type of suit and a Divematics closed-circuit suit. The reason for the different suits was to see if they supported any better protection for the man when he had his hot water off.

We worked the divers very hard on a 5 minutes rest/10 minutes work cycle. The work alternated between swimming and weight lifting.

Now a brief explanation of the slides (presented at end of paper).

The first one shows the experimental set-up with a conventional open-circuit suit. A Superlite 17B helmet was instrumented to permit measurement of respiratory losses in the demand valve with a very fast microthermistor, as well as a slow speed thermistor, the latter with a time constant of about a second. This latter thermistor was slow in response but more robust and it was tested to see if it gave an approximate indication of respiratory losses. The man was instrumented with five skin thermistors and a rectal thermistor as well. Mouth pressure was monitored and in addition a mass spectrometer probe was placed inside the oronasal mask. Upstream of the gas inlet, there was a Kinergetics gas heater used in the same way as in the RN diving vessel Clansman, for better comparison to the diving performed there. There was no additional insulation for the sideblock on the helmet or anything in the way of hot water coming up the umbilical. It was a standard RN set-up. The only way we had changed it, apart from instrumenting for inlet and outlet gas and water temperature, was to put a three-way cock downstream in the water inlet from the man so that we could isolate the man individually from the water supply but still give him background respiratory heating through the Kinergetics heater. Next slide (Slide Two).

Just to set the scene, these tracings were typical of shallow water exposures during the course of the dive, eventually down to 300 m in water of 6°C temperature. For the first 15 minutes, the man would be provided with normal hot water heating. Rectal temperatures tended to be high to begin with; it's a fairly rigorous exercise to get

a man into this gear and into the water. As a consequence, he tends to have a rectal temperature that is still increasing in the water. Then we shut all his heat off. He has no heating and the rectal temperature decreases. This point here is the limit of his subjective tolerance, at about 80 minutes, and it tends to be related more to peripheral cold stress than any rectal temperature change. This diver is coming out with a rectal temperature of 36.8°C . He is absolutely miserable and is incapable of performing any task that demands any sort of normal dexterity. The small interval here is the time it takes to get the man out. The solid line defines the point at which we take them out of the water. The point I want to make is that the rectal temperature is very high with these people under a great degree of cold stress. I'll get on to mean skin temperatures in awhile. Next slide (Slide Three).

Let us now go to the 300 m situation, still looking at the same man, wearing two different suits. I'm not too interested in discussing the suits at this time, but you can see a variation in the rectal temperature. If the heat is now turned off, but with the respiratory heat maintained in one case and not in the other, you can see that the time the first one lasts is 50 minutes whereas that of the second is 10 minutes. The point is that the first diver exits largely because of peripheral cold stress. In the 10 minute case, he does not have a strong peripheral problem but with this depth and lack of respiratory heating, he is getting severe respiratory problems. You could observe frothy mucus when the helmet was off, as well as a white beard of a rather thick consistency mucus across his lips. He also experienced

breathing difficulties which is a fairly well known phenomenon. What you can show is that, provided you can give some respiratory heating to this diver, he can continue working for much longer. I don't think that this is particularly startling but it does give us some data on which to work.

Let us take a look at mean skin temperature derived from five sites, with weighting factors dependent on the areas that are presumably represented (although there may be some fallacies in that). I like to include measurement of hand and toe temperature in the areas for measurement. In this fourth slide, the hot water is off in both cases and in one case the man is wearing the closed-circuit suit and the other, the free-flooding suit. They come out at about the same sort of level, about 18 or 20°C skin temperature, maybe going down to 16°C. It seems to be the limit of mean skin temperature which they can tolerate for any length of time, maybe about 5 or 10 minutes, after which they have had enough.

The rise in skin temperature observed here is a consequence of allowing the divers to shiver for an hour afterwards. The difference in the two skin temperatures at the start of the rewarming is not a function of the different suit types. When you use a closed-circuit suit, you demand a higher temperature of water into the suit at a lesser flow rate than the open-circuit system. Our engineering capabilities are limited and we need to develop a better system for this mode. We started at a disadvantage and could not keep the level of water temperature going through the closed-circuit at the level that we

should have. However, on shutting the hot water off, the slope of temperature decrease is not so severe. Entrapment of the hot water gives a certain amount of protection relative to the open-circuit suit. Next slide (Slide Five).

I'll go straight into the respiratory losses since they appear to be a fairly important consideration in terms of time that you can go on working after you lose your hot water. Having that stopcock on the hot water supply line allowed us to measure respiratory losses with the gas heater in-line and without and then to compare the two. If we look at what happens to respiratory losses (in watts) against ventilation rate, we end up with the usual straight-line relationship. The important thing is that when you add in the heater and consider ventilation rates of about 30-35 l/min, the heater is only saving you about 10 to 15 watts, so you are saving this amount of heat transfer that would otherwise be lost with no heating at all. In terms of the level of respiratory heat loss which gave us some sort of problem (e.g., excess mucus secretion and tightness in the chest), it is about 200 watts. Contrary to the Braithwaite paper and referring to what John Zumrick said earlier, we obviously need to drop our levels of tolerable losses that the diver can put up with. The Braithwaite paper referred to a level of 300 or 350 watts. The level should be brought down to a level near 200 watts. The other point is that this sort of engineering approach for keeping the inspired gas warm is really way out of date and I appeal to the engineers here to provide us with a more workable alternative when we have to do some of the deeper diving. Next slide

please (Slide Six).

Just to touch on the rewarming aspects, the methodology employed is one of hot wet gas breathing. Just to give a good example of the sort of differences that you can have using this sort of rewarming compared to allowing the man to shiver, the slide shows the differences in rectal temperature that occurred between the two methods. I've looked at it in terms of differentials but the rectal temperatures were fairly high. I cannot get people down that low under these situations. I might actually get a subject down to 36°C if I'm lucky so I'm trying to rewarm people from this minor level of cooling. It's almost a futile exercise but the experimental system as it stands doesn't allow divers to get any colder than that.

Respiratory rewarming does help though. It's certainly more comfortable for the divers particularly at about 300 m at which you can transfer about 50 watts into the man. The men say that for the first 20 minutes of rewarming they really notice the difference. You get odd changes as well in the times that you subsequently shiver later on in the rewarming procedures. If you're inspiring warmed gas at 40°C at 300 m, the periods of shivering during rewarming appear decreased whereas with no respiratory rewarming the shivering appears more severe, particularly around the 30-minute mark. Whether this is an advantage or not is questionable. Again, as this is a workshop, my excuse for presenting a limited amount of data, aimed particularly at those who work in the field, is that they will appreciate the problems of putting microthermistors in a demand valve and not having a diver

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THERMAL CONSTRAINTS IN DIVING (U)

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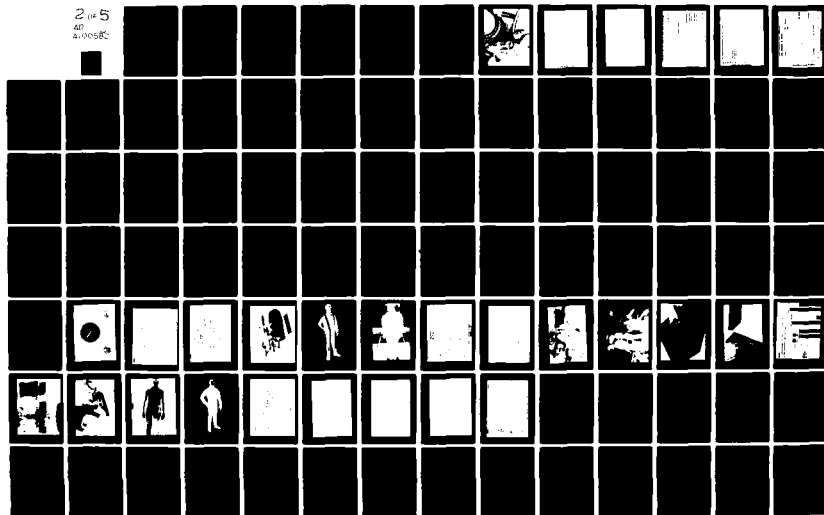
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put his tongue through it.

At this time I'll take the liberty of taking John Zumrick's data and comparing it to ours. These regressions are different due to our value of T_e being increased. The effect is to increase the slope to around 0.42 on this sort of helmet system. We are using an expired temperature which is considerably in excess of that which we had been led to expect from earlier experimental temperatures.

At 180 m and based on a loss of 200 watts, the inspired value comes out at about 9°C and if we go to the limit of what one will expect in RN diving at 300 m we are up about 27°C.

Kuehn - 180 m is roughly about 600 ft so that 9°C compares with about 14.1°C, from Claude Piantadosi's studies on such limits.

Hayes - The difference arises because I am sticking to a constant level of respiratory loss whereas Piantadosi has been using a more extensive set of equations analyzing the body as a whole.

Webb - Can you imagine that the distressing effect of respiratory cooling is a temperature effect rather than a rate of heat loss effect-- in other words, could you have very cold gas with a low ventilation causing the excessive secretion but without the large heat loss of 200 or 300 watts? I'm wondering if the stimulus for the secretion is thermal or dependent on heat loss.

Hayes - I'm not sure. Let me quote you an alternative example. Stein Tonjum told me a story about cross-country skiers. They have the same sort of respiratory problems but not to such a marked degree; however, below a certain temperature of about -20°C they are forbidden

to do anything in the way of competitive racing because they are getting the same sort of problems. Skiers have very high ventilation rates and the sport is exhausting over a length of time. I suspect that the stimulus for secretion is more likely to be a sustained high heat flow over a long period of time rather than any direct effect of temperature.

Zumrick - In our studies, the maximum rate of heat loss seen at 1800 feet was about 125 watts and about the only symptom that was described was increased salivation, which I can attribute to the mouthpiece that they were using. We didn't see any more cold-related symptoms in our exposures.

Hayes - You have to bear in mind that our RN divers also had their bodies surrounded by extremely cold water.

Zumrick - I can make a comment on that. Dr. Piantadosi found that, for an individual immersed in cold dry helium-oxygen which he was breathing, there appeared to be a very prompt and powerful response for shivering that apparently compensated for the skin heat losses but in no way compensated for the respiratory heat losses. He did another experiment on two subjects breathing warm gas in cold helium-oxygen and he still got the shivering response. It seems to be the case that the body does not respond appropriately to respiratory heat loss.

Hayes - I think that that is an important point. You just happened to depress rectal temperature a lot more with respiratory heat loss.

Zumrick - Back to your differences on the temperature of inspired and expired gas. Part of it may be the fact that within the linear rela-

tionship, you may be measuring inspired temperatures an order of magnitude less than we were and it might be the point at which the relationship breaks down; perhaps your different numbers are thereby produced.

Hayes - The reason I am pushing the point is that we have so many workers who play with models and who rely on us giving them reliable data. For the differences in the conditions they are trying to model we must offer up all available data.

Hayward - A brief comment on the last slide on rewarming data on one subject using rectal temperature as the criterion of differences between treatments. I've used rectal temperatures as have others to make assessments in this score and I now realize that, although rectal temperature is a pretty good indicator of general thoracic and head temperature in the cooling phase, during the dynamics of rewarming strategies it is a very poor indicator of what is happening in the thoracic cavity. You can get some information out of it but for definitive decision-making about rewarming, rectal temperature is inadequate. Even tympanic temperature doesn't give high fidelity with the intra-thoracic temperature as measured in the pulmonary artery of mildly hypothermic men during rewarming. Esophageal temperature is the best measurement. If you get the people down far enough, even with rewarming strategies such as respiratory rewarming, it gives a good measure of what the temperature is in central blood. As a comment for this workshop, during rewarming I hope that we will see more use of esophageal temperatures.

Hayes - I agree wholeheartedly. My restriction was that I could not

use esophageal probes in the working conditions of my subjects. When I have divers in the water I want some continuity of measurement between in-water and surface measurements and I would have all sorts of problems with what I'm doing with the divers if I used the esophageal site.

Pozos - With all this talk on inhalation rewarming we should bear in mind that with cold and the breathing of warm humidified air, there is an alteration of the hyperexcitability of the cardiac tissue. We tend to concentrate on temperatures but, as is seen from some of the work that we are doing now on isolated hearts and in hypothermic animals, when you begin to play with a cold heart at 34 or 35°C, you begin to alter the hyperexcitability period of the heart. When you start inspiring hot humidified air, you now have two variables to play with, namely how hyperexcitable is the heart and then what is the influence of selectively warming the heart during hyperexcitable periods. It's theoretically possible that you could trigger some kind of abnormal ecg depending on a number of variables. I bring this point forward for consideration in light of this discussion on warm humidified air.

Hayes - I remember seeing some time ago some pictures that Lloyd presented on his sheep-cooling experiments. He had a sheep with a low core temperature that had ventricular fibrillation. He gave it some hot gas to breathe for some time and it did not make much difference to the rectal temperature but what it did do was to spontaneously reverse the fibrillation to a more acceptable ecg. He did write something a while back in the British Medical Journal on the possible mechanisms

of heart failure related to cold. We have to bear in mind that in re-warming we are doing some strange things to the system such as putting temperature differentials across specific tissues which might be undesirable.

Hamilton - You said you measured respiratory temperatures with a microthermistor and an ordinary thermistor. Did you find that you could "operate" on the results of the ordinary thermistor and convert it to a more proper value?

Hayes - To a certain extent. The large thermistor had a time constant of about one second, the microthermistor had a time constant of about 0.12 seconds. It works out that the calculated respiratory heat loss for the large thermistor is about half that of the microthermistor. The large one will last you for the duration of the dive but small ones have a very short life expectancy.

Hamilton - Dr. Charles Johnson (Duke University) says that he has a program to convert the response of a slow thermistor to that of a fast one.

Hayes - It might be worth having that. To put in a small thermistor in the apparatus is very difficult and time-consuming.

Kuehn - In experiments that John Zumrick and I conducted, our divers kept sticking their tongues out to see if the thermistor was still there and of course it wasn't.

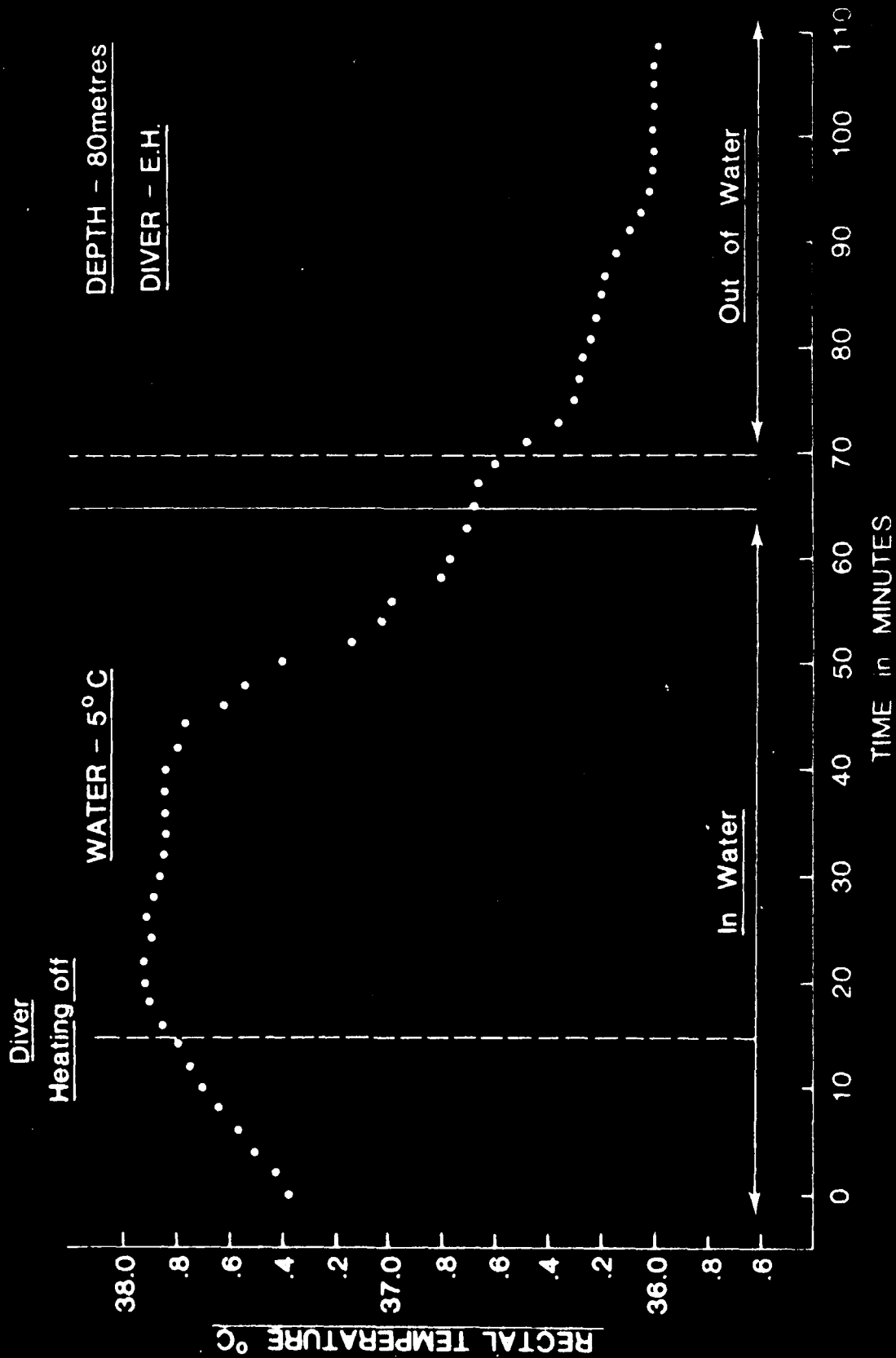
Hamilton - One point about esophageal temperature. If you are breathing cold helium, it will give a value below what you possibly could have as a body or core temperature, because the esophagus and trachea

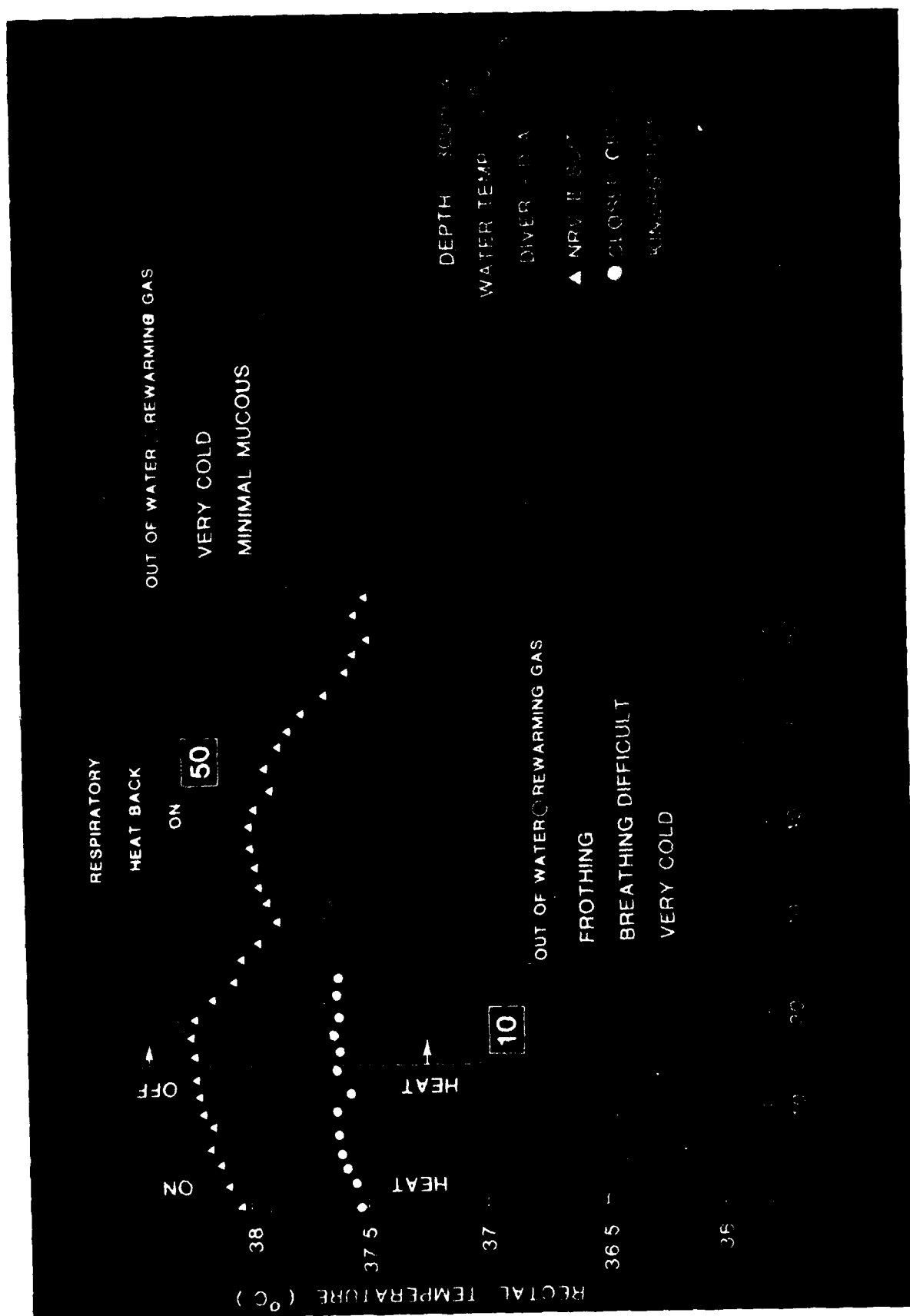
are quite close. You have to go quite deep, to about the level of the diaphragm. We made the mistake in the Polar Bear experiment of putting it where you would put the balloon for respiratory measurements, and it was totally distorted by the temperature of the breathing gas (See Tonjum, S., R. Hamilton, R. Peterson, A. Brubakk, Project Polar Bear I, NUI Report No. 2-80, 1980).



SLIDE 1.

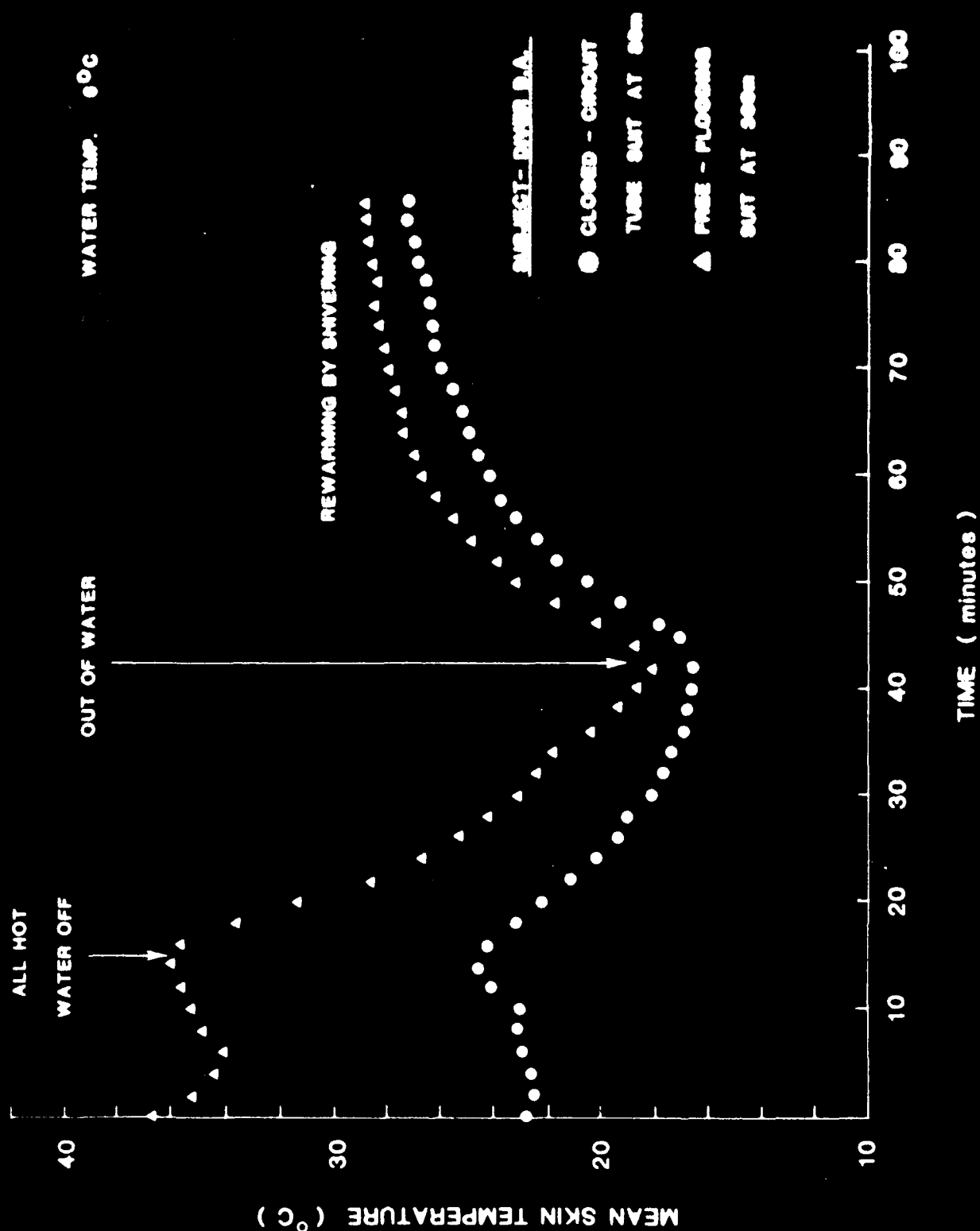
TYPICAL COOLING CURVE OF DIVER WITHOUT HEATING OR REWARMING

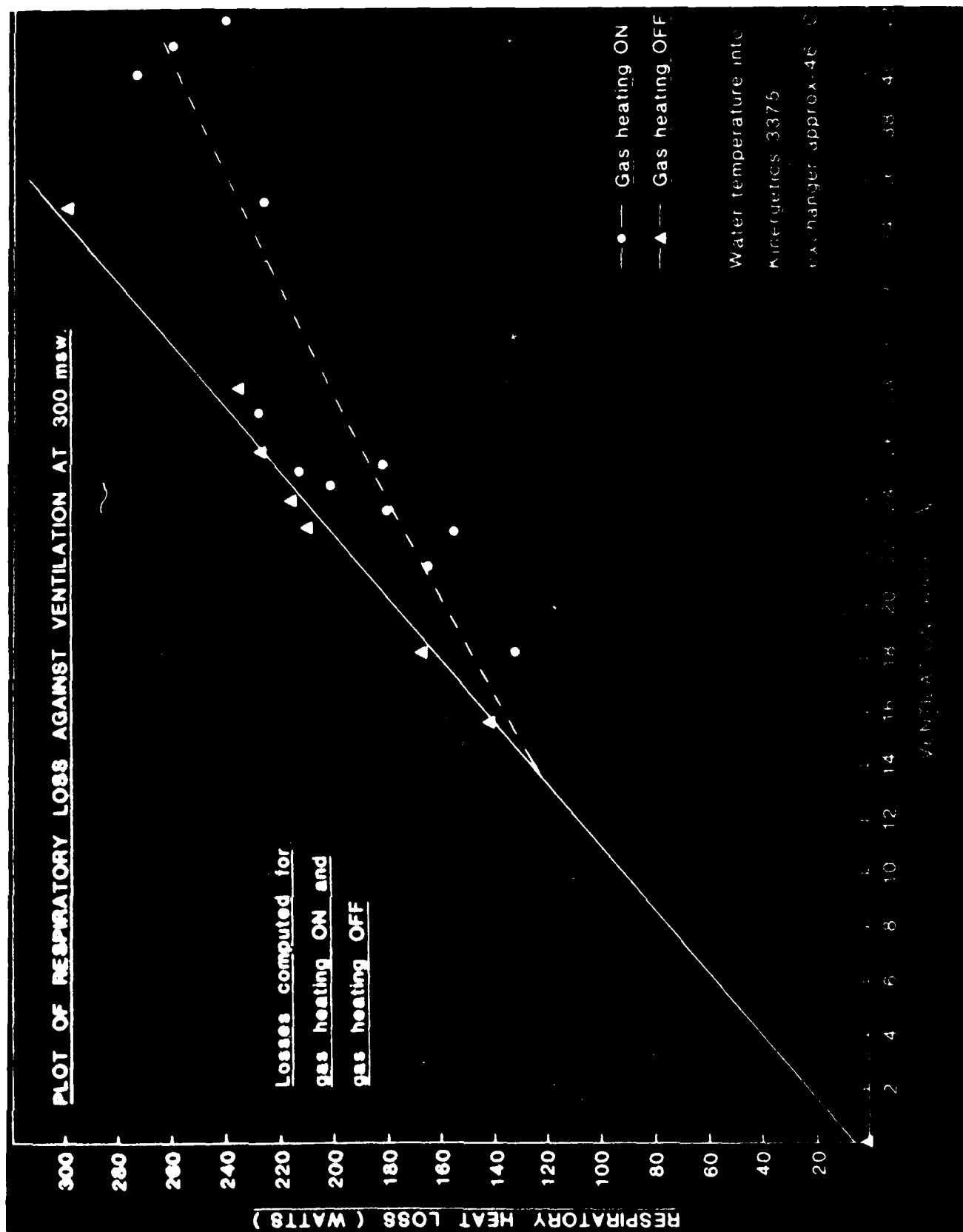




SLIDE 3.

EVOLUTION OF MEAN SKIN TEMPERATURE DURING COOLING AND REWARMING





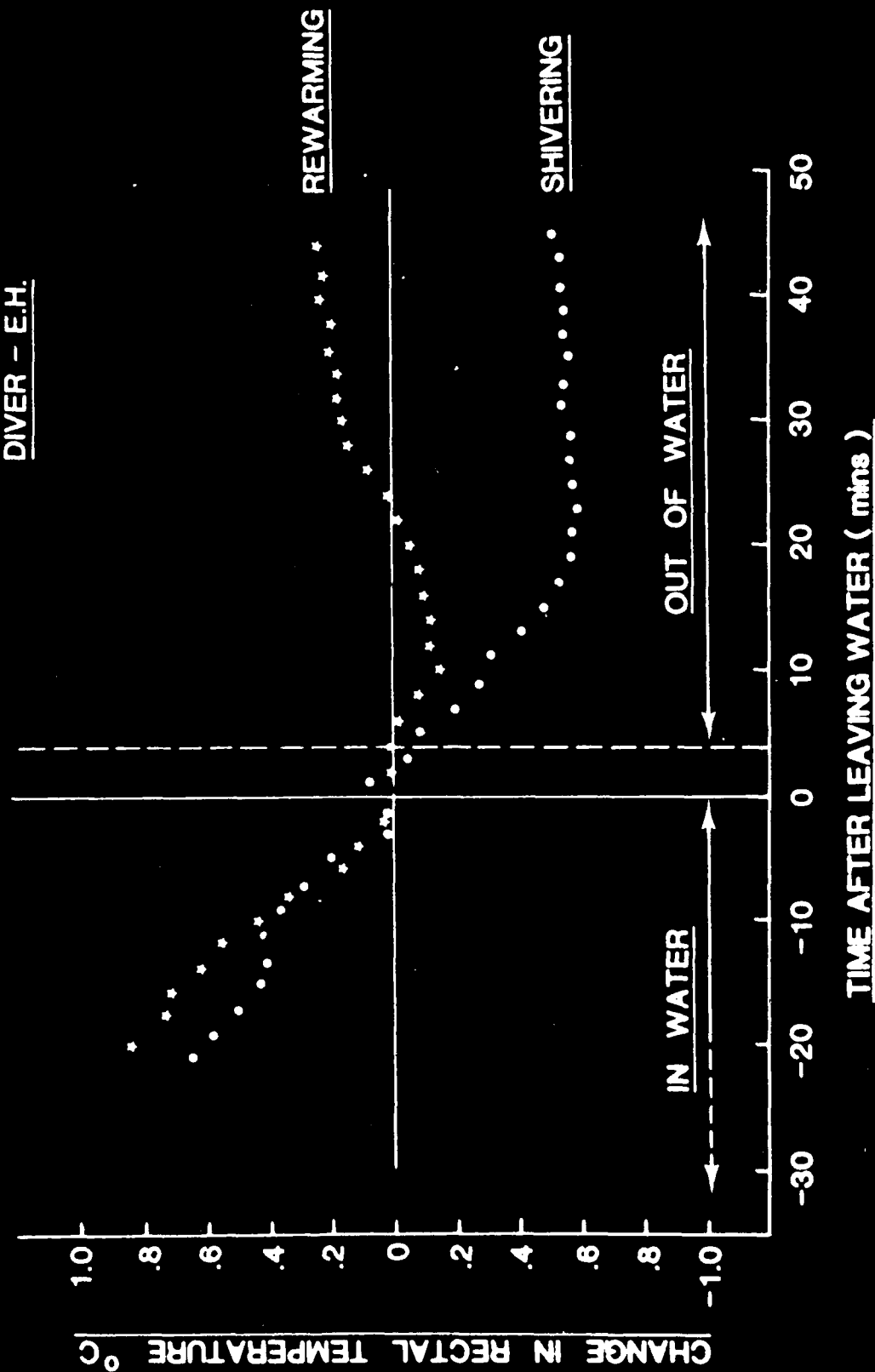
TYPICAL EVOLUTION OF RECTAL TEMPERATURE

DURING REWARMING AND SHIVERING

WATER - 6° C

DEPTH - 300metres

DIVER - E.H.



GENERAL DISCUSSION ON SESSION I

Kuehn - We've heard an elegant series of papers this morning that have shown that during the last six years a lot of new operational data has been obtained on cold water diving. It is now incumbent on us in this discussion to examine how we can relate some of this basic and operational data to the technologists. We should concern ourselves with modifications for decompression schedules, heat loss limits for diving, as well as respiratory aids for cold exposure and respiratory heat loss.

I have a number of questions that I would like you to consider. The first is "How do the inspired gas temperature and density interact with equipment design in setting respiratory resistance?" If inspired gas temperature cannot be kept as high as desired, what does cold gas breathing do to airway function in normothermic men at depth? We heard a lot about this question this morning. The third question is to establish a consensus on what is the effect of the currently available thermal protection such as suits on lung mechanics and pulmonary exchange. We need new information here. We already discussed in John Hayward's paper the influence of cold water during decompression schedules. It was suggested that cold divers may suffer less bends but at what cost of performance? This suggestion deserves more debate. As Paul Webb brought to our attention, there is still the question to answer "How much heat loss is critical to the cold water diver?" Which physiological parameters are important and what are the magnitudes that we should set to

these parameters to form limits? These are the questions that I would like you to address so that we can establish some degree of consensus as to what the answers should be.

Long - You are showing the need for higher inspired gas temperatures at greater depths, i.e., the minimum levels we have are not high enough. The current state of monitoring of the diver is very poor. Some people only know what the hot water temperature is on leaving the surface; some know what it is on leaving the bell; only a few know what it is when it's at the diver. The United States Navy diver is the only one that I know to be monitored in this way.

One of the problems that we had in the initial phases of introducing hot water suits into the field was that we had six divers who were told to enter cold water for awhile but who came in saying that they were sick and dizzy. On taking their masks off, they took one breath and passed out. No one was ever able to isolate the reason for this. They were using our gas heaters which were designed not to have too high a gas temperature. In talking to the divers, we found out that they were new and the supervisors were somewhat new, with what I call the hotwater shower syndrome --they kept turning the temperature up. I sincerely believe that these people were forcing themselves into hyperthermia. If you raise the skin temperature into the suit, you can create any skin temperature that you wish. Then you eliminate that method of losing metabolic heat. Then if we raise the inspired gas temperature, we can no longer lose heat that way except by evaporation.

Hypothermia is our friend compared to hyperthermia. We can live with the first and it is difficult to live with the other carried to the extreme. I mention this word of caution, because when it comes to application in the field, we do not have as yet a way of knowing what the diver's thermal state is. If we exceed thermoneutrality, the margin for error is very small. If we drop below thermoneutrality, then you become hypothermic and our margins are big.

Kuehn: You are suggesting that the unexplained consciousness that was observed may be hyperthermic in origin?

Long: No, only that as we proceed into this subject, we do so carefully. The problem is to avoid over-heating the diver arbitrarily. Unless heating supplies are controlled, the divers may wind up with problems that are hard to identify and, in the process of identifying the problem, some people may be lost.

Hamilton: Would you repeat the scenario that involved this unconsciousness?

Long: We had cases of divers entering the water for a number of minutes, as high as twenty minutes. The data in the field is often inaccurate in this respect. The diver would complain of heavy breathing, in excess of that normally expected in his work and would claim that he was a little dizzy. He comes back inside the diving bell trunk, takes off his mask, takes one breath and passes out. This phenomenon of taking one breath and passing out is something that I have no explanation for. After a short coma, the diver recovers. The divers had not used gas heaters before but they were using them on these dives. They knew

what the temperature was in the bell but they did not know the temperature at the diver. The temperatures were correct in the suits but the hot water flow rates were high. When you use a higher flow rate, you elevate the skin temperature in the suit by a considerable amount. My analysis of the limited data available is that the diver was driven into hyperthermia. I can relate further some of our earlier tests in which we did drive our people into hyperthermia.

Webb: The loss of consciousness sounds like heat syncope, fainting from heat exposure, and not the hard-driving hyperthermia of huge body storage. In other words, a lot of people are unused to heat exposure and when they first start to work in a warm environment, they don't really like it and react by fainting. The fact that he waited until he got in the bell and took his mask off is that he felt safe. He didn't have to keep his muscles going. He was back in the bell, he's home. This is the time at which he would relax and collapse. I'm just trying to relate the experience in warm climates to the situation you described.

Hayward: Also, on assuming a vertical body posture on getting out of the water, the circulation of blood pools in the lower part of the body and the brain is deprived of oxygen.

Hamilton: The divers are usually immersed to some extent.

Zumrick: By the time they are in the trunk, they're nearly out of the water.

Greene: The problem is amplified by the fact that the divers are vasodilated due to the heat stress of the hot water suit and when they

stand up in a vertical gradient, it's a perfect case of blood pooling.

Hamilton: Do they have to be in the bell? Do they pass out before getting to the bell?

Long: We didn't have anyone experience that. We only had six cases and they were all in the bell. Some were in the process of climbing in and others were all the way in.

Webb: The loss of circulatory support when you come out of the water must be a big part of this. I have a question for John Zumrick. Do you set your respiratory limit in terms of keeping the respiratory heat loss at about 25% of the metabolic heat production?

Zumrick: No. Claude Piantadosi was saying that we could have a net heat loss of 20 watts/m² so he was keeping it constant, assuming a resting diver state. There was a further extension in his paper in which he developed the rationale that, as the individual exercised, he would not lose more heat using these limits.

Webb: Phil Hayes' limit was to keep respiratory heat loss below 200 watts total, to avoid bronchorrhea.

Zumrick: Our scenario involved a resting diver in a NRV-type hot water suit with the suit temperature maintained within a comfortable region. In terms of the previous statement, we found that one can raise the skin temperatures to abolish heat loss in that suit but the diver usually complained of being too warm. If we then said, "Bypass the hot water used, do what you need to make yourself comfortable", we come back to a skin temperature of about 33°C or so and there was in fact a heat loss from the skin at that point.

Vorosmarti: I have a question about that. You didn't do any work on exercise. You say the diver is regulating himself. What happens if he starts exercising? Self-regulation doesn't really work very well based on subjective feeling, at least with cold stress, but how about with heat?

Zumrick: It seems that subjective responses of heat or cold are dependent on skin temperature and, for a high level of exercise, the skin temperature climbs, and they feel warmth. During our exercise periods of divers in cold water, what they will usually do during the high levels of exercise is to by-pass some hot water to cool down a little. Subjectively they will feel warm with exercise.

Vorosmarti: What you're saying is that Piantadosi's limit was set for the respiratory heat loss so that if he does exercise he is not going to overload himself because you're trying to keep the skin temperature constant while holding the respiratory gas temperature constant.

Zumrick: We know the diver can do that, adjusting himself to these parameters. Then how much respiratory heat did he lose?

Hayes: The problem with the skin temperature worries me slightly. We quote mean levels for all of these limits in terms of skin temperature. When you use a hot water system you get incredible regional variability across the body and I don't really know how we're supposed to get a mean level out of these. We had levels much higher than anticipated in terms of a mean skin temperature. The problem might be that we were weighting all the areas wrongly anyway. I don't know what is required in terms of regional differentiation in temperature to

provide the person with a feeling of comfort. This is a whole research project in itself.

Zumrick: As to how to measure mean skin temperature, we have seen different individuals measure skin temperatures different ways: four points, five points, seven points, twelve points. Lorne Kuehn and I have had experience in this. Many of these indices don't agree.

Kuehn: They don't, especially in transient exposures, which is what you usually have in cold water diving. When you do get to the steady-state case, the various mean skin temperature equations tend to plateau very close to each other. Most of these weighting formulae have been derived for experiments in relatively cool air environments and are not at all intended for application to the diving case.

I would like to come back to Dick Long's question at the start of this Discussion Period. Dick, as one who has worked in the operational environment, what parameter would the divers like to see measured to assess hypothermia in the bell or before and after water excursions? What parameters would you like us to suggest? Clearly rectal temperatures are not practical or desirable in the field and yet when we look at such techniques as radio-pill temperatures, there is not enough justification for using those as good core temperature indicators. Is there a one-number parameter that can be assigned to a hypothermic diver? Perhaps we need some performance indicator telling us that a man is showing a decrement in performance and should terminate his exposure.

Long: One of the things that we can reasonably assume, based on past

experience, is that the water in the suit directly above the skin is equal to the temperature of the skin. Which one controls the other is a question for you folks to address. If I were to ask for instrumentation, the only place we can use instrumentation and protect it is inside the helmet. You can run wires to the helmet and put sensors in it. Any place else involves more difficulty, with the exception of the United States Navy practice of putting a temperature probe in the water inlet on the umbilical. The sensor never touches the diver. Therefore, whatever you use must be extremely robust and be put on when the diver puts on his equipment. He cannot take extra time to put something else on and adjust it. He doesn't even have time to do what he does now, let alone to add another step to the dressing procedure. Paul Webb and I have discussed whether or not you monitor the environment or the diver. Paul feels that for good reasons we should not concentrate on the environment. We considered using an optical plethysmograph for heart rate and taking temperatures off the scalp which is usually always dilated. Insulating the head is not difficult. The ear phones can go in the bunny cap which can also contain sensors for heart rate and temperature. That kind of thing is practical and acceptable but tomorrow it may be different. The people in the field will tolerate this. When you go beyond that, involving strapping something on the chest or swallowing something, inserting something, the divers will rebel and the equipment will become broken.

Vorosmarti: I have a question about why you would want to monitor a diver. Is there a problem in the field with divers getting hypotherm-

ia? People start getting cold skin temperatures and then they quit. I can see a problem if a guy is stuck in an environment that he cannot get out of but somebody who loses his hot water supply to his suit - is it worth worrying about monitoring him? He is going to get back in the bell as soon as he can.

Long: Your military people may not have that choice.

Vorosmarti: That's true, but in that case it doesn't matter whether you monitor them or not. We may be wasting a lot of time on discussing monitoring when there is not much use for it.

Hamilton: I'd like to reply. If you have a situation where a diver does not get properly rewarmed after a first dive, then he may be in danger on a subsequent exposure. This diver is at risk. The monitoring, however, can be done in the habitat.

Wissler: Apparently the boilers that provide hot water don't always work perfectly, and occasionally a diver does not receive a sufficient supply of hot water. If he has been working in the water for 30 minutes or so, the question that he has to answer is "Do I get out, or do I try to finish this job?" If you don't have some mechanism for monitoring him, you can't provide him with much useful advice.

Long: The operational procedure in such a case is that the diver must leave the water environment.

Wissler: Yes, that is true if the hot water supply is totally lost, but what if the temperature falls to 90°F from 110°F?

Tonjum: I would like to say something to Dick Long. If you could monitor the temperature of the hot water supply to the suit then I think

that would suffice for your requirement.

Long: I agree. One way that we developed to do this was to measure the temperature in the tube that comes up the back of the neck. If the diver is wearing a bandmask, the temperature can be measured at the earphone site if the hot water supply runs near the earphone. You can use the same wire to measure temperature as is used for diver communication.

Tonjum: Another comment I have is that although you know when you are cold and shivering, Keatinge has shown in his paper on the urine temperature of professional offshore divers that they were not even aware that they had a thermal problem. You can quit your exposure when you are shivering but it is also possible to have hypothermia without shivering and without knowing it.

Hayward: I think that you may not know that you have core hypothermia in the case of breathing helium-oxygen without respiratory heating, but in the case where you lower the hot water temperature slightly, and the diver continues working, the question is should we monitor the core temperature in that case? I would say, from what we've seen and the results of others, that you really don't need to. The core temperature would not fall to a point of any danger. The limit would still be set over another hour or so by the subjective tolerance of the diver to mild cold stress. None of us have demonstrated in our experiments very much core cooling, even with fairly strong cold stimuli. So with a mild cold stimulus I believe that you would get some peripheral hypothermia that the person would tolerate for awhile but not enough core

hypothermia, with skin cooling only, that it needs to be monitored.

Wissler: I have a question for Phil Layton. Didn't you do some experiments several years ago in 25°C water on people wearing Unisuits and observe that core temperatures really came down?

Layton: The core temperatures didn't drop.

Manalaysay: We immersed people at 25°C and also at 33°C initially and we found that some of our divers had as much rectal temperature drop at 33°C as at 25°C. I spoke to Keatinge about this and he said that he had seen unpublished cases in which people were having ventricular arrhythmias and not knowing it. They would have urine temperatures in the 33°C range and feel comfortable. The divers that I hauled out of the tank in 33°C water were claiming thermal comfort but their rectal temperatures had dropped as much as 0.7°C.

Pozos: A question I have for people in the field is whether or not their divers take drugs before diving or if there are other complications. I know we are considering the fit 70 kg white Caucasian types but I'm sure some people are on various medications such as amphetamines or barbituates. These will have a pronounced effect on temperature regulation whether in hot or cold environments. Is this a problem?

Long: Drugs offshore is a very serious problem for divers but we don't know how to control it. The drugs vary widely and are basically mood-altering.

To come back to thermal awareness, we see in the exposure of subjects in submarine escape training tanks, where the men are in the water a long time and where the water temperature is only slightly

cool, a slow rate of cooling but with a rate of exercise so we don't see any shivering. They are in fact hypothermic; however, I'll come back to Tonjum's suggestion. If we could monitor the water temperature going into the suit, that would solve that problem. If you are using a hot water respiratory gas heater you would know what respiratory gas temperature as well so you wouldn't have to measure the temperature of the gas.

Pozos: To follow my earlier point, many mood-altering drugs, whether prescribed by a physician or not, do have significant, sometimes paradoxical effects. In terms of whether or not there should be monitoring, we should remember that the divers may be on pep pills or whatever which will significantly change any of the parameters that we have discussed so far.

Long: I agree, but the problem is considered to be an uncontrollable one, despite the care taken by diving supervisors.

Hayes: In reference to the Keatinge paper that we've talked about I think that we might be getting too worked up about it, particularly in regard to the urine temperatures. The experiments involved only a few people and I am not sure about the reliability of urine measurements under these conditions. To come out with a paper like that is fine but it could appear to a commercial outfit as a radical state of affairs and we ought to see some backup data on this point before we say that divers are getting cold all the time without realizing it. To amplify on what Stein Tonjum has been saying, if you do measure the water temperature then if it decreases the simple remedy is to deploy

your backup system or pull the diver out of the water. We can prevent all those problems by using backup engineering. We can try and remove the threat completely by having this one simple measurement.

Tonjum: We did some surface experiments to check up on Keatinge's data on urine temperature and we measured it in relation to rectal and radio-pill temperatures as well as a deep body core temperature probe and the correlations were all good. It correlated best with the radio-pill temperatures.

Hayes: You did this in nice controlled scientific conditions; you can never be sure of what divers make of urine temperature measurements.

Wissler: I'd like to return to respiratory heat loss. From a philosophical point of view, it bothers me that minimum inspired gas temperature should be based on a specific set of assumptions such as the ones that come from EDU. I don't know what assumptions one should use in defining minimum inspired temperature, but it does seem that those assumptions should be less specific than the ones that pertain to a specific mission in which the United States Navy is interested. I have a question. How difficult is it to maintain inspired gas temperatures above any of the proposed minimum values? Is this a difficult engineering problem?

Vorosmarti: I think that the current generation of gas heaters will handle gas temperatures up to 27°C. If you have to go above 27°C, the gas heaters require a major re-design.

Middleton: In the studies that we did on Kinergetics breathing gas heaters at 1000 fsw, we found that we could not get quite that high in

temperature and the LBT heater that is used in association with the Mark II Mod I system has less capability than the Kinergetics heater. So, meeting the inspired gas temperature standard is fairly hard.

Hayward: As has been pointed out by Paul Webb, we know that one of the biggest variables in the susceptibility of the hypothermic diver is the individual characteristics of the diver, that under any one cold stress, be it a failure of the hot water system or the respiratory gas heating or whatever, the response varies greatly with individuals, based on their fatness and body size. It seems to me that one of the practical measures that could be considered is that, when you suspect that there are dives taking place that run more risk of hypothermia due to one reason or another, there should be limits put on the physical characteristics of the divers. Has there been any attempt to say that "We should not let our diver do this sort of operation unless he has a skinfold thickness of this particular size and a certain degree of mesomorphy?" Individual characteristics should be one of the criteria in the selection of divers to dive or not to dive in certain situations.

Hayes: When we did the 300 metre dive, we were using three British Navy divers, two of whom had routinely dived in cold water in wet suits for about six weeks before, while the other was a nurse who happened to be a saturation diver as well. The nurse was worse off in all these conditions. He didn't like respiratory cooling or general cooling and he had a far worse time than the other divers. They were of variable height but they all had about the same skinfold thickness. It struck

me that the previous history of the divers was an important factor in determining how susceptible they are to these cold conditions.

Hayward: As well, I would add that if you control past experience then these physical thermodynamic considerations would be (in a rating system) more important than the "experience" in determining safe limits. One may "cop out" earlier due to lack of experience, but in terms of rate of cooling, if that's what your eventual problem is, I think physical characteristics would be more important than past experience.

SESSION II

INSTRUMENTATION AND TECHNIQUES FOR PREVENTION OF DIVER HYPOTHERMIA

DIVER MONITORING

F. Golden

I want to take a brief philosophical look at diver monitoring. There are four questions that we should ask ourselves.

- 1) Why should we monitor?
- 2) What should we monitor?
- 3) How should we monitor it?
- 4) What does it mean when we do monitor it, that is,
how do we interpret the results?

The why is fairly straightforward. We are obviously interested in diver safety, firstly to determine if the diver might be developing hypothermia insiduously or, secondly, for diver safety, to see if the diver's performance is being impaired in any way that may result in errors of commission or omission that consequently affect his safety in that sense. The second reason why we monitor could be put in the general category of "It's nice to know," that is, it furthers our information on the cooling response of the diver and certainly when involved in the development of thermal protective garments, it's nice to have some baseline data on which to base our technology. It remains open to question if these two reasons are sufficient justification for encumbering the divers for the reasons that were presented in the last discussion by Dick Long. So if I can go to the

second question of what to monitor, I think that we can take the word how as well because, once deciding what to monitor, the how follows closely on.

The simple answer is that one would like to monitor core or deep body temperature; one would like to monitor skin temperature, or heart rate or, with increasing degrees of sophistication, you can start talking about heat flow discs. But in the end it boils down to how do you interpret the data that you monitor. What does it all mean? I think that there is an enormous variability in the human response to a cold stress so that if we, based on the knowledge that we have gathered on a small number of experiments on a fairly select group of human beings, lay down a set of arbitrary limits of skin temperature or core temperature, then we are going to be excessively restrictive on some people and too permissive on other people. Let me give you some examples. This is where I can delve into some of the things that can be shown at a workshop and not elsewhere.

You're all familiar with the figure that shows rate of change of rectal temperature against the reciprocal of mean skinfold thickness. It was first produced by Keatinge in 1961 but if you read his paper you find that his data was plotted on rates of change of rectal temperature for the first 30 minutes of immersion. Now those of you who have done immersion experiments know that, in the first 30 minutes of immersion of very fat subjects, there is very little change of rectal temperature. In many of these subjects the

rectal temperature increases and that is the change that you measure so what we did is to modify this approach. In 23 subjects, 15 males and 8 females, we looked at the effect of the reciprocal of mean skinfold thickness on the rate of change of rectal temperature once this steady-state rate of cooling had occurred, during the last 20 minutes of immersion. The two regression lines that we got showed that here you have people of identical skinfold thickness and of identical sex with enormous variation in rates of cooling. It's something we've all observed and know. What could be happening? Is it possible that this difference in cooling would be accounted for by variations in rate of metabolic heat production? Is this person here shivering much harder than this person here with the metabolic heat production compensating for the difference in rate of change of rectal temperature? Well, if that was the case, we would expect to find a significant regression between the rate of cooling and the shivering activity.

When you look you see in fact that there is no significant relationship at all. That in itself is interesting and one would like to ask Bob Pozos, what in fact does shivering contribute to somebody immersed in cold water? Does it help at all or is the extra heat produced by shivering quickly dissipated because it is produced superficially? I don't know the answer but it is interesting that it is not shivering that is accounting for these differences. It isn't subcutaneous fat thickness because that is the same for the subjects. There is another factor.

Most of you are aware of the standard answer of why it is that long distance swimmers can swim for long periods in cold water. It all goes back to the paper by Pugh and Edholm published in 1950 in the Lancet. One of the swimmers is a physiologist, one is a long-distance swimmer. The answer was that this one man could survive because he had a subcutaneous "wet suit", i.e., a substantial layer of subcutaneous fat, built in here. A few years ago in 1977, I had the opportunity of looking at some long distance swimmers in the international swimming race in Lake Windemere where some of these original experiments were done.

This is what I found. The current European long distance swimming champion in 1974 was a Dutch boy 17 years of age. This thin man spent over six hours in the water of 15°C and romped in ahead of everybody else.

A thin young girl became a record-breaker for swimming the English Channel even though she was too young to enter the international competition at Lake Windermere, being 15 years of age with the minimum age limit at 16 years. She was not fat.

I could tell you a lot about these kinds of observations. In 1978 I went back with some colleagues, Ian Hampton and Doug Smith*, and I measured skinfold thickness of all 34 competitors this time,

25 males and 9 females. The mean skinfold thickness (SFT) in millimeters of the males ($\pm$ S.E.M.) was 12.2 ($\pm$ 0.85) and the percentage body fat was 18.0 ($\pm$ 0.98). You will agree that these are of a pretty normal range. These are the females; SFT = 19.5 ($\pm$ 2.17), Fat = 31.9 ($\pm$ 2.28)%. The results are biased by three rather buxom lasses from Jersey. The point I'm trying to make is that the simple answer as to why long distance swimmers can swim so long isn't simply a question of body fat. It may be related to heat production although the absence of a significant relationship between metabolic heat production from shivering and rate of cooling make this unlikely. It's just that there is another possibility which we haven't identified perhaps akin to that which Suki Hong and some of his colleagues have found in runners and nonrunners in Hawaii. It may be related to a peripheral vascular response of some sort.

I took an opportunity of measuring parameters on three long distance swimmers, two of whom were outdoor swimmers in cold water while the third did all his long distance swimming in a heated indoor pool. The measurements included the heights, the weights, the mean skinfold thickness and the percentage of body fat of the three people. Two were reasonably similar and one was a bit fat. I then immersed these three subjects in water at 12°C until their body core temperature fell to 35°C.

The two men who trained outdoors in cold water had 10% and

19% body fat and cooled at 3.8°C and 1.2°C/hr respectively, while the third subject, with 9% body fat and who trained in a heated pool, cooled at only 0.6°C/hr. Both of the swimmers who were accustomed to cold showed very little metabolic or shiver response, although their core temperatures fell to 35°C. Likewise they showed little or no subjective distress at that temperature, whereas the swimmer unaccustomed to cold shivered violently, cooled more slowly and subjectively was very distressed. That's my point. Here you have people used to cold exposure, they can tolerate a body temperature of 35°C, they can obviously tolerate a skin temperature at 12°C or so without showing a subjective response, without showing any significant metabolic response, whereas this man who was unaccustomed and gave a violent shivering response did manage to maintain his shivering in some way and lasted much longer.

Hayward: Doesn't that suggest that there is a correlation between shivering metabolism and cooling rate?

Golden: It would suggest that there is but, when you look at the correlation for a larger number of subjects, you don't find it.

Nunneley: What was his subjective state after two hours?

Golden: He was dreadful. He was almost crying and was very glad to get out. He didn't stay until he was down to 35°C.

Nunneley: So the others stopped sooner because you pulled them out, not because they quit?

Golden: Oh, they would have stayed longer. They said that they felt better than they do when they are swimming.

Hamilton: Haven't we seen this before today, that the people who feel cold have the increase in metabolic rate? This is what we see here.

Golden: If I can move on slightly. We did follow these three subjects in their swim in open water and measured their deep body temperature by radio pill in the stomach and their oxygen consumption intermittently while swimming in Windemere. The water was much warmer (17.4°C in 1978; normally it is around 15°C). During the static immersion in water at 12°C , the two cold-accustomed swimmers shivered at 26% and 22% of their VO_2max , whereas the swimmer unaccustomed to cold shivered at 50% of his VO_2max . When they were swimming at Windemere in much warmer water, the first two exercised at a rate of 58% and 48% of their $\text{VO}_2\text{ max}$ and the other was exercising at a rate of 72% of his $\text{VO}_2\text{ max}$ and in fact he had to quit. His body core temperature had fallen to 35°C after about 2 1/2 hours of immersion. He had finished the swim but as he was swimming in an international race the following day we pulled him out so he would not over-exert himself.

If we look at a group of students, the 23 subjects that we looked at earlier for whom we correlated the rate of cooling and mean skinfold thickness, when you look at these 15 males and 9 females and look at their shivering response expressed as a percentage of VO_2max you find the males at 48% and the females at 43%. So what I am saying is that people who are accustomed to cold water immersion only

shiver at about 20 - 22% of VO_2max . The differences in subjective sensation is really the point I'm trying to drive home.

The question I want to ask is "Are divers habituated to cold?" I don't know the answer. From what I heard this morning it sounds as if there is a random mix across the spread of divers or is it that the people we use in experiments and have access to in experimental laboratories may well have been habituated but have lost their habituation in some way?

I'm not sure of the answer. It would be nice to know if they are habituated and for how long can they maintain their habituation? Of these three swimmers, the fatter one was tested almost five months since his last cold water immersion, so it really looked as if he had maintained his habituation for quite a period of time. It appears to be an acquired thing rather than an inborn thing. I have no scientific evidence and only anecdotal evidence for this statement. If you interview long distance swimmers and talk to them, you find that at the start of the swimming season they swim throughout the year, and in the winter they swim in indoor pools to stay fit and maintain their competence, but before the start of the outdoor swimming season they start an habituation process. They do some cold dips of short duration and gradually build up the duration until they can tolerate it quite comfortably. It sounds as if it is an acquired thing rather than an inborn thing but as I've said I've no scientific evidence for that statement. So the conclusion from that data on swimmers is that core temperature is useful in telling you how cold a

diver is but it doesn't necessarily relate to what his performance is likely to be so there is not much point in monitoring it. Those long distance swimmers here could have performed quite adequately although their core temperatures are down at 35°C. So it is nice to know but it is not such a valuable indicator as a man's performance. The next question is "What else can we find as a good indicator or a good thing to monitor to give us some indication of cold stress in a diver?" Well, the obvious answer is heart rate. One would imagine that if someone was suffering from cold stress, it should be reflected in heart rate.

I would like to tell you about another swimmer, someone who is cold adapted. We measured his rectal temperature and his heart rate during 30 or almost 40 minutes of immersion in water of 12.5°C temperature and then we rewarmed him. Initially his rectal temperature fell as did his heart rate. He was a little bit anxious before the start because this was a new venture for him but he very quickly settled back-- he had a heart rate of 42-43 in water at 12.5°C. Quite happy; the heart rate moved up towards the end. One would like to have some oxygen consumption measurements to go with this. As things were happening very dramatically, his temperatures were falling precipitously. We wanted to get him out quickly and we couldn't mess around getting another sample that would be "nice to have".

This man was only shivering at 7-13% of maximum oxygen uptake. He was not showing anything like the metabolic response one

would expect. So core temperature, heart rate, oxygen consumption - they are not really good indicators, but perhaps they are; perhaps this man can perform his task quite well. I'm not really sure what these things are telling us. Of interest is the heart rate in the rewarming; you can see it dropping precipitously; as you put the man in hot water, his oxygen consumption comes down. The point I want to make is that his heart rate turns before the core temperature turns and starts going up. The value of monitoring heart rate might well be in the rewarming phase to try and prevent this rewarming collapse that people are likely to get or the heat syncope that was referred to this morning. An earlier indicator of that is the pulse rate increasing and then you know you must slow that man's cooling rate just as you do with a geriatric hypothermic patient in whom you monitor the central venous and peripheral blood pressure and the heart rate as well. You must balance these; you must govern the rate of rewarming to prevent occurrence of a hypovolemic collapse.

So that's the value of measuring the heart rate-- just during the rewarming and not during the cooling period. If the man is working hard, doing some driving task, his heart rate is going to be determined by his work rate rather than by his cold stress.

We also examined the changes in heart rate, relative to the last reading before leaving the water, for nine subjects. Their mean heart rate prior to leaving cold water, with a core temperature of 36°C (they had spent six hours in water of 3°C in a submarine escape

suit), was 69.6 (± 3.5). When they left the water to stand up to go to the hot water bath for rewarming, this increased to 115 (± 3.2). During the hot water bath their heart rate went right down and as they started to rewarm peripherally the heart rate started to rise again so the heart rate is the best indicator in the rewarming, much better than core temperature, of an impending heat syncope or circulatory problem, of terminating your rewarming or slowing down the rate of rewarming.

So what does all this mean? Let's try and sum it up. I think that there is no physiological variable that we can measure to give us an accurate indicator of diver performance or development of a problem. So I presume that one must go back to working on subjective sensation but subjective sensation is based primarily on the afferent impulses from the thermal receptors in the skin. If we are warming the skin by hot water suits, we're interfering with the one selective indicator we have that something is going wrong. I think that we might be better in fact just monitoring the temperature of the water going into the suit and, instead of fixing a lower level of water temperature, we should fix an upper limit of water temperature. Now the habituated man can set the water temperature to any level that he likes but the unhabituated man that is likely to have troubles must be stopped from boosting his suit's temperature and confusing his hypothalamus and getting himself into problems. We therefore may want to consider setting an upper limit to suit heating and a lower limit to respiratory gas heating. This may be the best way we can

monitor divers for their safety.

Kuehn: Thanks, Frank, for a most interesting paper that summarized the concerns for diver monitoring that we discussed this morning.

Long: I'd like to add something. There is already in the old military suits a system for doing such monitoring. We've taken it out of the newer ones because most divers did not understand what it was for. If you look at hot water suits, some have three knobs. In those that do, the second and third knobs control water flow to the front and back parts of the suit respectively. The purpose for putting these in was to take into consideration that exact same thing. Take two divers in the water, one working harder than the other; he could divert the flow from the front and rear portions thereby dropping the temperature of the suit by a number of degrees, not so that he was uncomfortable, but just to reduce water protection in these areas. The decrease in protection was not in the hands, where he needs it, but in the chest region. The difficulty in controlling hot water to the suit from an industrial viewpoint is quite difficult. If you can get the diver to cooperate you don't have to control the water temperature at all, though I agree with the concepts of limits that you presented.

Hayes: I noticed in your example, where people were resting and exercising, that the rectal temperature of the person trained in warm water had a rate of fall of rectal temperature which, while resting, was the lowest, and the highest when he was exercising.

Golden: No, that was not the case. The person who was unaccustomed

to swimming in cold water had the lowest rate of change in static immersion but in swimming he had the highest rate of change.

Hayes: Yes, that struck me as being very specific for the sort of chore that you were performing. Your level of habituation reflects the sort of task you have given them.

Golden: That's right. If I can just recap on this point on the effect of exercise brought up - by John Hayward's paper, if you recall - when the subject was static in water at 12°C, his shivering was at 50% of VO_2max and his rate of change of shivering was 0.6°C/hr. When he was in water 5°C warmer, his metabolic heat production was 72% of his maximum VO_2max , yet his rate of change of temperature was 1.7°C/hr, more than twice as much, even though he was exercising much more. So the people who were habituated to cold water had core temperatures that fell much more rapidly when they were static but, when they were exercising, their rectal temperatures remained fairly level. It does suggest that part of the habituation process depends on a peripheral vascular physiological response which you develop with habituation, of which we know nothing. The exciting thing is that though you can't do too much about your subcutaneous fat, there is a physiological response that you can acquire. It would be nice to see how habituated divers are and into which category they fall. We would be wrong to restrict them to certain core and skin temperatures. We would be taking men, who were perfectly capable of working longer, out of the water on reaching these limits.

Hayward: With regard to diver monitoring, Frank, you and others have

pointed out that monitoring of core temperature doesn't seem to be too compelling in respect of telling them that there is danger. Isn't there an argument to be made in some situations of telling divers that there is no danger? I can see a situation in some sorts of diving when they do not have hot water suits and are suffering some peripheral cold for some period of time. They are getting signals from their cortex saying "I'm feeling cold" along with some low-level shivering and they've heard all about hypothermia. They may be asking themselves "Do I have dangerous core hypothermia?" If they do ask that question, would they not impair their performance by the very uncertainty about core hypothermia? If you were monitoring the core and could say that there was no hypothermia, would not this information feedback to them be of significance in causing them to put up with peripheral cold stress? Would there be any argument for that use of core temperature?

Golden: In light of what we heard this morning and my own experience; when you take somebody with a core temperature of 36.8°C who is feeling very cold, do you reassure him by saying "No, you're not suffering from hypothermia; stay where you are" or do you get somebody who is habituated to cold whose temperature is down at 35.5°C and say to him "You must come out because you are cold?" As long as the man is doing his task and doing it well without running into any physiological danger, I think that that is the thing we should be looking at.

Hayward: For the person who will perform well at 35°C and who is

habituated, presumably there is for him some core temperature lower limit. Probably for him we have to consider 32°C or 33°C as the temperature at which he will get cardiac abnormalities or perhaps under 34°C there is some central nervous system performance decrement. We have some evidence that you have to be under 34°C before you get electroencephalograph changes of any significance. So where would you set the core temperature limits? From what you are saying, it would certainly be under 35°C. In regard to tolerance, it can be looked at in different ways. The core temperature shouldn't matter until it is less than 35°C. That's what I was getting at. In the 35°C person who is still performing and who doesn't feel bad, I would agree that he should be told he is safe - but down to what level?

Tonjum: I would say that when you get a reduced core temperature, it should show up in reduced work performance. That should probably count more from a commercial view. We should have a better appreciation of the amount of performance decrement with reduced core temperature. The important thing is not whether the man is at 35.5°C but how well is he functioning?

Hayward: We tried to look at the effect of cold core, mainly cold brain, the control centre, as being impaired by temperature without people doing anything other than mental tasks. We found that, down to 34°C, we had no significant impairment from brain cooling per se. Any impairment was peripheral, from muscle cooling or from sensation of cold arising to the cortex and interfering with control activity. That's the only evidence that I have.

Golden: I think that the only value for core temperature is in the differential diagnostic point of view. You have a diver who has come back into the bell and collapsed unconscious. One of your differential diagnoses is hypothermia. When you can forget that, it is very reassuring. You can then concentrate on something else. From the point of view of withdrawing a diver from the water or keeping him there, I think core temperature is spurious. We are going the wrong way.

Wissler: If you have a diver in the water and his core temperature falls rapidly to 35°C, you are going to pull him out. Isn't there useful information in that measurement, regardless of how he feels?

Golden: I would challenge that as well in the sense that if the man said he was perfectly well, we're quite happy. Perhaps my monitoring probe had slipped out and I wasn't getting a true reading. I wouldn't believe what I was getting.

Wissler: Implicit in my statement was the idea that you are making an accurate measurement. If you don't have confidence in your measurements, then certainly you are correct. However, if you have some confidence in your measurement procedure, and if you see the temperature fall to 35°C and continue to fall, aren't you going to pull the man out of the water?

Golden: I think that would concern me if I was there.

THE DEVELOPMENT OF THERMAL PROTECTION EQUIPMENT FOR DIVERS

Mr. L. Nuckols

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First of all, I'll be outlining the U.S. Navy program that was taken and initiated approximately two or three years ago to develop cold water systems for both active and passive divers using mixed gas as well as air for breathing gases. I'll talk a little bit about the evaluation techniques that were used in the development efforts. Since we've all been talking about models I'd like to show how the use of thermal models was very helpful in this development effort. First slide please.

A propaganda slide. Two or three years ago, the Naval Coastal Systems Centre in cooperation with the Navy Experimental Diving Unit began a development effort on thermal protection systems for the Navy. Next slide (Slide Two).

We broke rapidly into a two-phase program. The starting point was to design a system to be all things for all diving modes. This was broken into an active heating system and a passive heating system, primarily looking in the former case at mixed gas operations and in the latter at the special warfare applications, that is, long duration, shallow depths, this type of thing. I'll talk briefly on the active system. I will say that there are a number of heating systems that have been developed to the prototype stage; one has gone

on to its own development track itself, that is, it has gone on to its own program for use in a swimmer delivery vehicle heating system. We are also working with heating distribution garments and also a control system, working with Webb Associates, linking up the heater and the diver to supply the proper amount of heat.

The passive system is presently in the development stage looking for approval for service use, the present plans calling for that toward the end of this fiscal year. Next slide (Slide Three).

As I said, I will mention one of the heating systems that has been evolved, this being a magnesium-oxygen heater. Basically it involves the reaction of magnesium chips with oxygen to give off heat, a very efficient means of heating. This particular device is a multi-diver heater. We are looking in the neighborhood of 500 watts for a single diver heater and the existing prototypes are being built to that level. Next slide (Slide Four).

This was one of the first prototypes. You are looking at a combustion chamber that includes magnesium chips, an oxygen supply, and a battery pack that is used for circulation of water. I won't elaborate on this any further but will move on to the passive area. Next slide (Slide Five).

This is the present system that is being evaluated for fleet use, primarily in the special warfare application. This particular garment is a passive one; its major features are a compression-resistant outer garment material. Its primary feature is a thermal undergarment that provides the maximum amount of insulation that is

attainable, assuming that the diver is going to work at some level of effort.

It is made of a commercial material known as Thinsulate that we have evaluated in a series of material studies. We have concluded that, from the standpoint of compression resistance of materials, specific insulation values, and water resistance, it shows very promising features for a diving thermal undergarment. It does have thermal boots of double thickness because this is a body area that is very difficult to keep warm. Next slide (Slide Six).

Some of the evaluation efforts that went into the development of this particular passive garment: Approximately three years ago, a large-level investigation characterizing the thermal properties of various dry suits in hyperbaric environments was begun. Particular use was made of a thermal mannikin to measure the thermal insulation of various garments. We were able to put the thermal mannikin in a hyperbaric chamber and with his instrumentation we were able to measure thermal insulation values, watch how they change with depth and also with various gases in the suit. Next slide (Slide Seven).

This is typical of the types of results that we were able to see from that. Of course, everyone is aware that due to the compression of foam neoprene you get the rapid drop-off in insulation as you leave the surface. This also shows the effect of placing mixed gas or heliox within the suit and the dramatic reduction in insulation values that you will get. Along with this investigation we had a thermal model which we developed and were able to validate. The

straight lines show the model prediction and various data points show what the actual measurements were. From this investigation, we have a thermal model from which we are able to predict, knowing the thermal properties of various materials and components that are in the passive system, the insulation value for a particular passive garment. Next slide (Slide Eight).

I put this in because we are all concerned with the thermal properties of garments and their thermal behavior. However, there is a human engineering factor, a range-of-motion area, one that we had to look at strongly. This is a series of exercises that we went through in the evaluation of various garments to compare relative mobility of the garments. These particular motions were developed for comparative mobility by Glen Egstrom. We continue to use them and one reason for mentioning them here is that I feel that there is a need for standardization in our suit development efforts. For all those involved in this kind of work, if we are expecting to compare our results with those of others, it is important to consider standardization in our testing procedures. Next slide (Slide Nine).

We also had to do a number of open-water dives. In this particular case we were looking at buoyancy control in a particular garment; of course this is a very critical area and one of the major safety concerns with passive systems. Next slide (Slide 10).

One of the difficult problems in the human engineering side is "What is the application? What is the diver going to be doing?" We

had a number of tests in which the divers were basically going through various mission scenarios for a particular application. Here we are concentrating primarily on the combat swimmer application. Next slide (Slide 11).

As I mentioned, there was an extensive material study in conjunction with the Navy Clothing and Textile Research Facility (NCTRF) at Natick, Massachusetts. We went through a number of experimental and on-the-shelf materials characterizing properties such as compression resistance, specific insulation values and water resistance properties. This particular material is the M400 Thinsulate which has been chosen for the passive garment. We also looked at, as the next slide (Slide 12) will show, various compressive-resistant foam materials, open cell foams; this one is high density urethane foam. From that and from a number of reports on this particular investigation we were able to conclude that with the open cell foam you are very much concerned with the water resistance, of being able to protect this particular material from any leakage or sweating in the suit. This next slide (Slide 13) characterizes the two final candidates that were used in the investigation for the thermal undergarment, comparing their water resistance properties and thermal insulation values to each other and to foam neoprene, used in wet suits. Taking a look at both final candidate materials, they show higher insulation values than foam neoprene in a dry state; however, as soon as you wet out the material, and all dry suits are not characteristically dry, you will observe a very rapid drop in insulation values with the

urethane foams. However, the data we have collected show that the M400 Thinsulate will still have insulation values higher than the foam neoprene, even if the suit is completely flooded.

There is one problem with this. The Thinsulate material is basically hydrophobic in that it will resist water; however, if you flood your suit, then go to 100-200 feet of water, you will force the water in. This is an area of concern. Another major feature and benefit of the water resistant property of this material is the fact that, if you wet out your suit, you will not become negatively buoyant immediately as you do with some dry suit undergarments. Next slide (Slide 14).

Another area was that of glove development. This is something that we haven't talked about, namely protecting of extremities. We went through a number of investigations with the Army Research Institute of Environmental Medicine to characterize the insulation values of various liners to be used inside a dry glove that was developed by NCTRF as an Arctic fuel handlers glove. We selected the best, insulation-wise. Then you have to consider what effect this system will have on the dexterity of the subject. Next slide (Slide 15).

This is another area that I feel needs some type of standardization. Dr. John Brady now at NMRI developed a series of tests for measuring finger sensitivity, manual dexterity, etc. In this particular case, they were doing it bare-handed but we went through this series of five or six tests to characterize the relative mobility and sensitivity of various prototypes. Next slide (Slide 16).

Finally of course, the thermal properties of the garments were most important. Here we were using the 12-point weighting system using the heat flux transducers that have thermistors at each location. Again this is an area that I feel needs standardization. We're using a 12-point system, how well does it compare to others? I know Lorne Kuehn has some data comparing the 12-point technique to other methods. Next slide (Slide 17).

The last area is that of models which were used in the early development of the garment. We have some thermal physiological criteria that we started out with, based on the 1976 meeting; however, to the engineer trying to interpret this in engineering terms, it really results in a trial-and-error basis without some type of modelling. We worked very closely with Gene Wissler, with his thermal model, and we obtained some early estimates of the amount of insulation with it that would be required for six-hour missions in 50°C water. We were also able to characterize types of endurance that you could get at various other ambient conditions. The question that then arises is validation of that model with actual testing data. The next slides (Slide 18) show comparisons of the experimental data with the use of the passive system with the model. These tests were conducted last May and June on an average of four subjects. These are actual measurements showing the ranges of data obtained on three typical scenarios. We had one case where the diver was completely at rest (full circles), another where he was going through a 6/4 work/rest cycle at a 50 watt work level (triangles), shown here, and the final case

involved an incremental workload from 50 to 150 watts for the first 30 minutes after which he rested (open circles), shown here. Next slide (Slide 19).

These are the model predictions. The type of information that we supplied to Gene Wissler for this type of analysis was the actual insulation value of the garment (determined from an earlier test), the ambient temperature and the metabolic levels. The thing to observe here is that the model is more sensitive to transients than is the actual case. We did observe that following these initial transient conditions, we were able to get very good agreement. The model itself is measuring arterial temperature whereas we were measuring rectal temperature. The next two slides (Slides 20 and 21) show a comparison of mean skin temperatures. Again, with the three different cases of resting, moderate exercise and heavy exercise for an average of four subjects. This example is the continuous exercise at 50 watts. This is the heavy exercise and then the diver quits; this is a reduction in mean skin temperature, due perhaps to some type of sweating during the early exercise level. The next slide (Slide 21) shows the predictions obtained from the model itself again showing the relative trend observed with the model, namely that of the initial transitions not being shown. I won't describe here the model and all of its features; Gene Wissler will be talking about that later. Next slide (Slide 22).

In summary, the point that we are making is that we have a passive garment that exhibits the maximum insulation that is attain-

able and yet will permit a diver to do some realistic task. This shows how, with the passive system, you have insulation values that don't change with depth. This has been confirmed in pool tests as well as in chambers at 70 fsw. Compare that with the drop-off in insulation values of wet suits and the various foam dry suits. Perhaps more important than the development of this system is the development of these tools, the modelling tools, that are available now. We have good confidence in them and with the data that we are collecting; we feel that we will continue to update and provide more confidence in the use of such models.

Kuehn: Are the suits that you are examining representative of the potential insulation technology that is possible at this given time?

Nuckols: I guess it's difficult to say that we've reached the state-of-the-art but I think that we have. Those insulation values that we recorded in the laboratory for the thermal undergarment are approaching that of stagnant air. To improve on this you are going to have to go to some type of vacuum system. Getting to the insulation value of stagnant air is approaching the limit unless you get into some very elaborate vacuum-sealed system.

Kuehn: I take it then that further improvements in diving suit technology will come from the active systems?

Nuckols: That's correct. As soon as you go to mixed gas, and I think that I mentioned that the passive system is intended for use in the air mode, when you go to mixed gas the thermal insulation value of Thinsulate approaches that of stagnant helium, which is far infer-

for in insulation value. There you would have to go to some type of active or supplemental heating to withstand the durations that we are talking about, six-hour missions in 5°C water.

Greene: One thing you haven't done to reach the state-of-the-art is to vary the amount of insulation over the body surface area according to its needs.

Nuckols: That's correct. There were some early efforts, and those were more subjective than based on the model, in varying the thickness in the undergarments to compensate for squeeze, say in the legs. This could be explored, to optimize the thickness of insulation material.

Wissler: Given the current state-of-the-art, do you think that one could build one of the Conox heaters that would produce one kilowatt over 24 hours?

Nuckols: The reaction rate of the magnesium-oxygen heater is controlled very accurately with the supply rate of oxygen. This is what we intend to use for the control system.

Wissler: Do you have hardware now that will produce 1.5 kilowatts for six hours, that is, produce 9 kilowatt-hours?

Nuckols: Yes, 1.5 kilowatts was the multi-diver specification for the early prototype. The units built now produce 500 watts for a single diver.

Hayward: Is there any information as to at what water temperatures some divers would have heat stress problems in that insulated suit?

Nuckols: We have looked at the problem of overheating in this suit,

using the model. That is one of the problems with a constant-insulation suit. If you design to withstand the extremes at the cold end, you will have an overheating problem at reduced temperatures. The way we plan to handle this is to have a family of insulating garments for the various ambient temperature ranges. When I say "family", I'm talking of two or three garments to cover the range of metabolic and water temperatures.

Hayward: So at this time you have not defined a water temperature zone appropriate to the suit that you described to us?

Nuckols: The insulation value for the suit now is nominally 1 to 1.5 clo, depending on the inflation that is in the suit. That was designed for the six-hour mission in 5°C water.

Hayward: But for the 15°C water, would that present any heat stress? Are experiments being done to predict this possibility?

Nuckols: Model predictions have been made in this area. Perhaps John Zimrick could comment because he is planning a series of dives in this area, to characterize this garment in varying environments of different water temperatures and metabolic rates.

Kuehn: We at DCIEM have tried to develop a vacuum insulation for divers and I was very disappointed at the experimental results from the samples we sent you for testing. What is your prognosis for this technology? Do you see vacuum insulation being feasible?

Nuckols: I guess the way I saw the samples you sent me was that you have a trade-off. If you can't support the vacuum, essentially when you pull a vacuum you are compressing the material. The compression

of the material causes its insulation to approach that of solid materials. Until we can develop spacer materials to support this vacuum, I don't think we are there. Until that happens I would put this possibility in the elaborate category.

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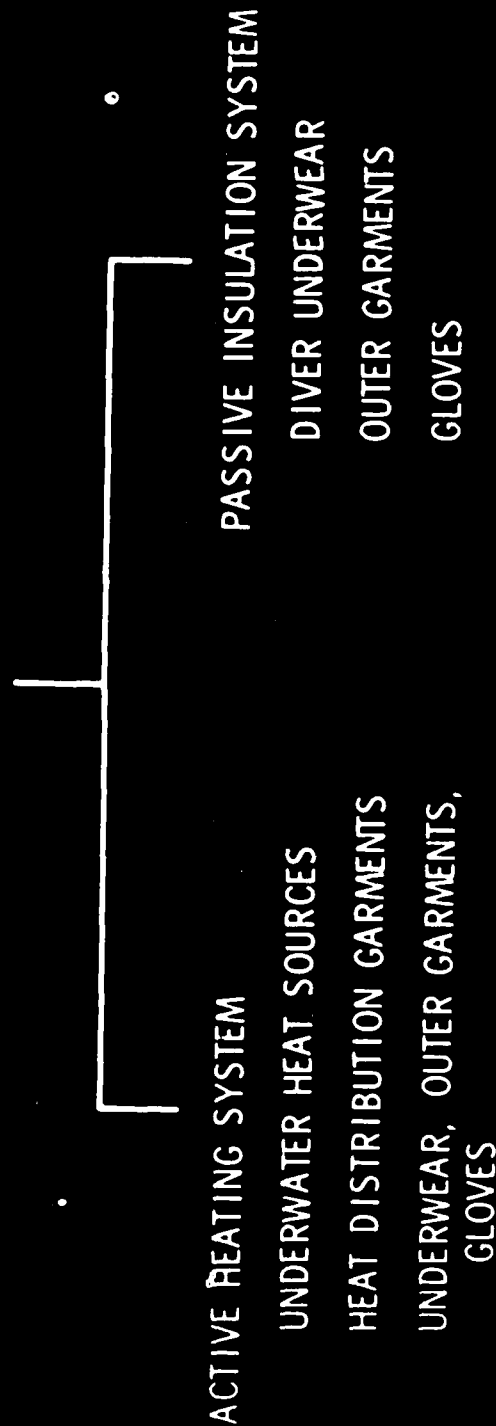
NCSC

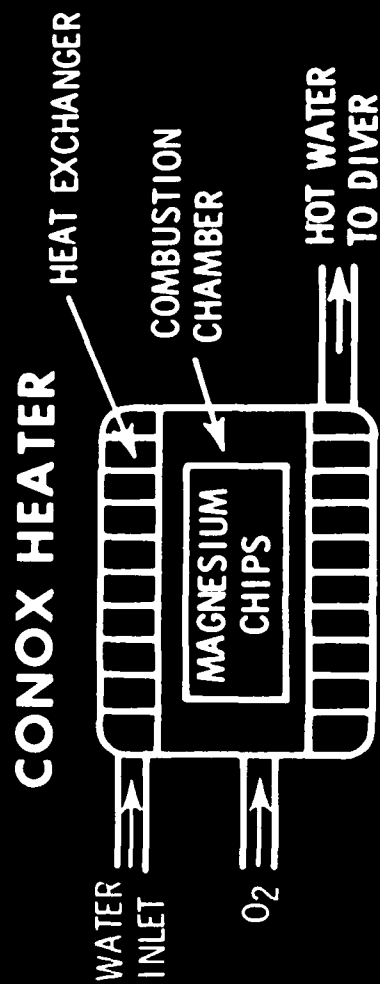


NAVAL COASTAL SYSTEMS CENTER—PANAMA CITY, FLORIDA

SLIDE 1.

U.S. NAVY DIVER THERMAL PROTECTION PROGRAM





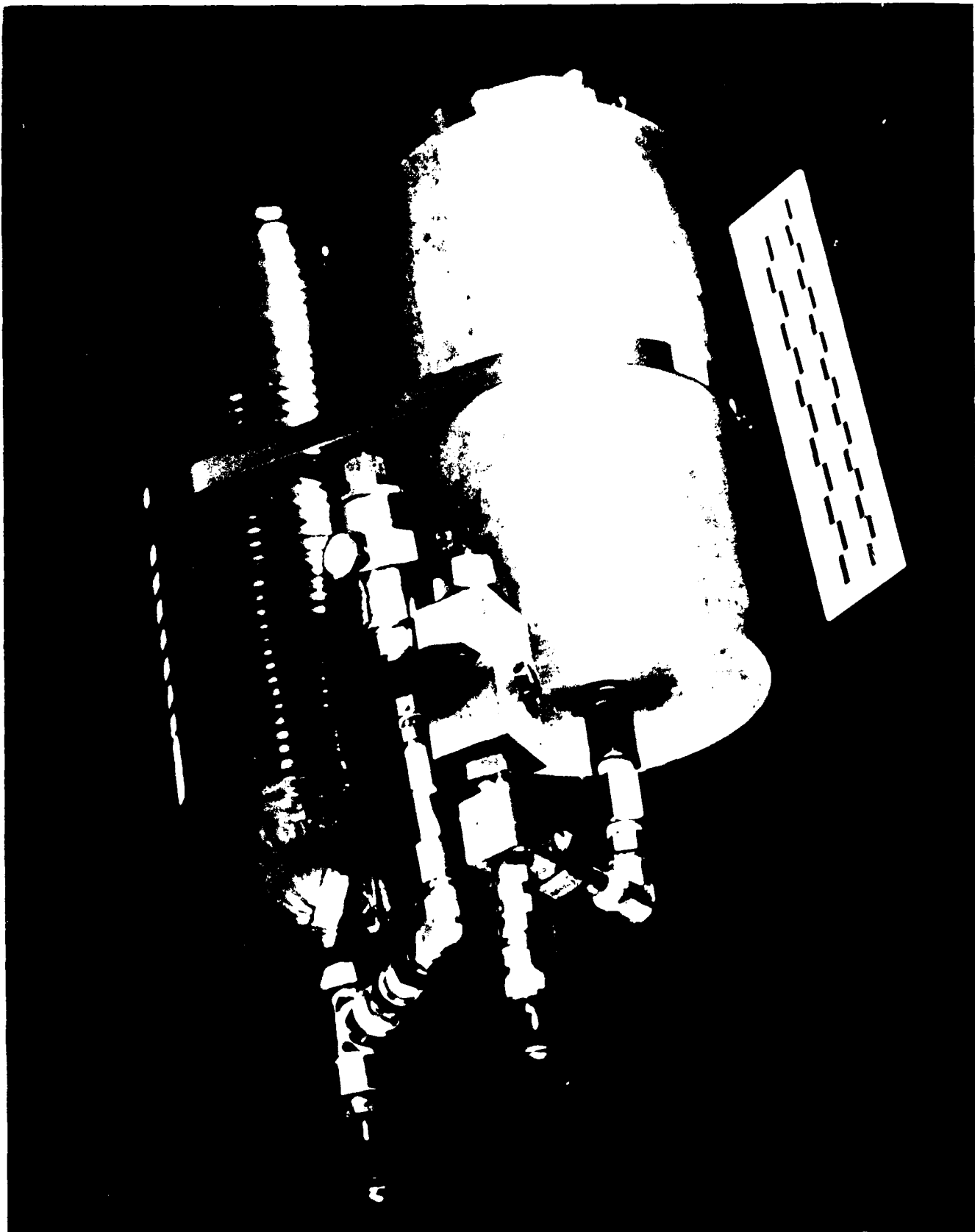
CURRENT EMPHASIS - DEVELOP UNIT FOR COMBAT SWIMMER OPERATIONS

POWER OUTPUT 1500 WATTS

DURATION 6 HOURS

OXYGEN REQUIREMENTS .34 Kg/Hr
(.75 Lbs/Hr)

SIZE 30 x 35 x 45 cm
(12" x 14" x 18")





SLIDE 5.



SLIDE 6.

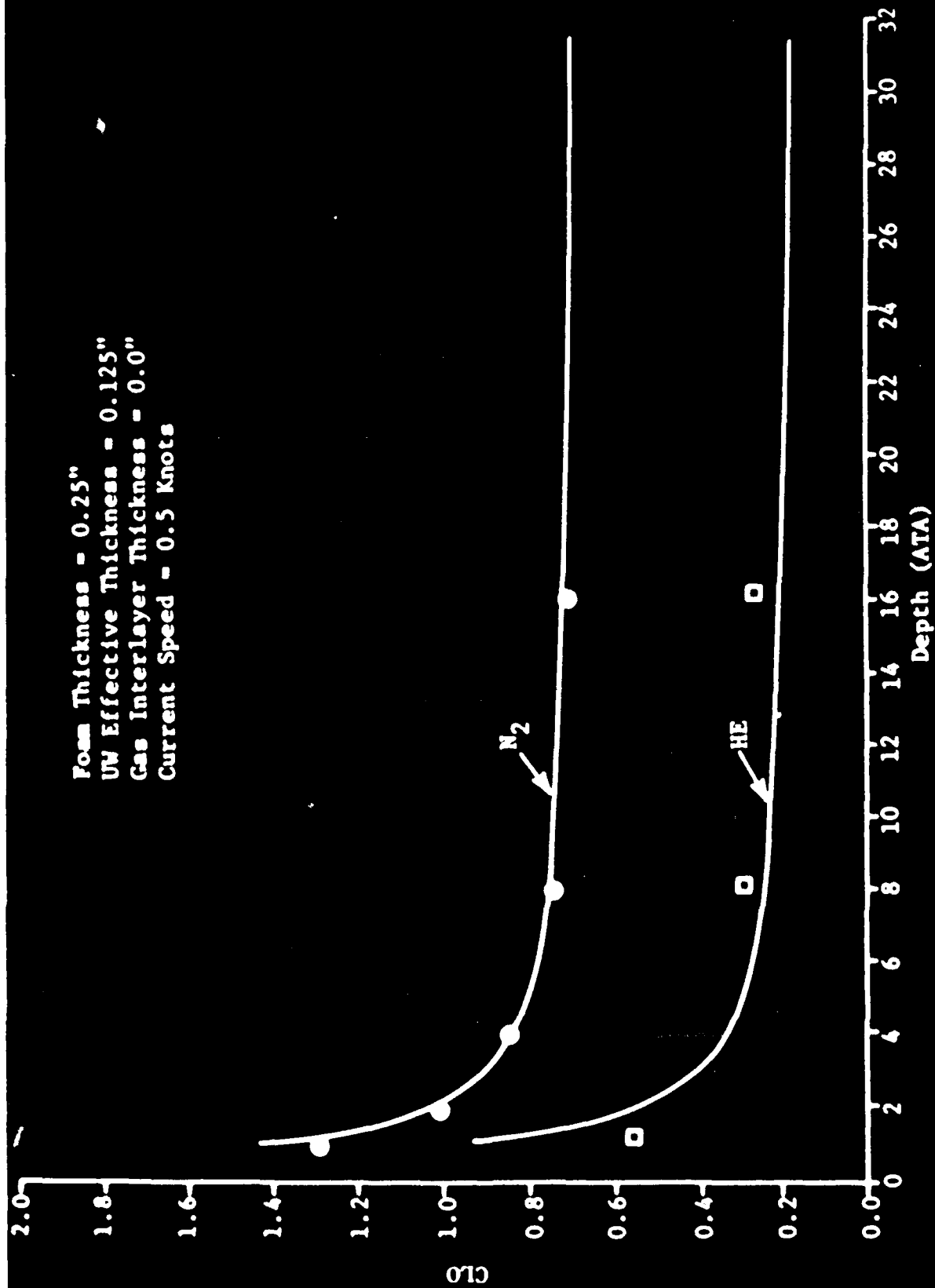
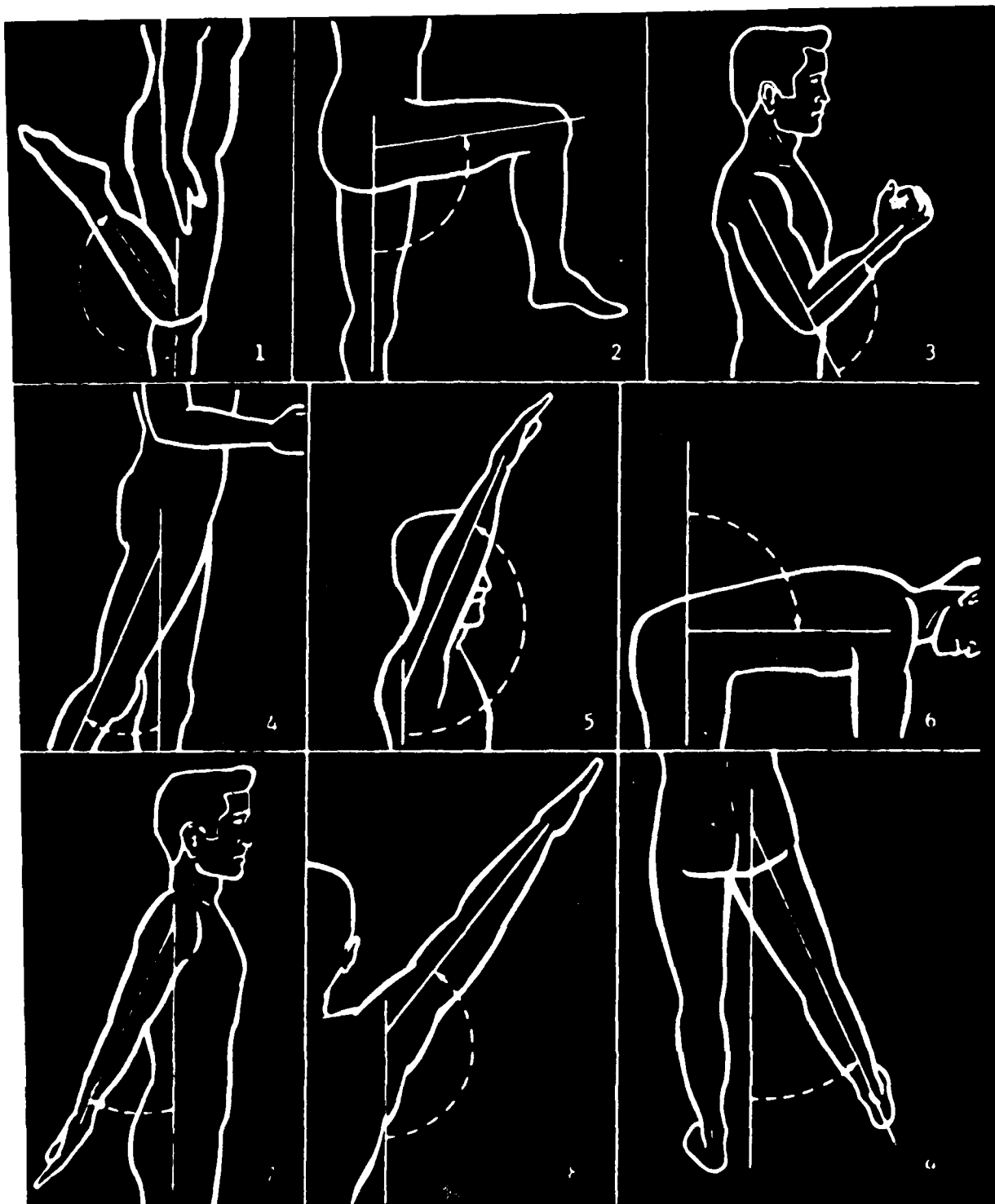


FIGURE 8. PREDICTED AND MEASURED VALUES
 INSULATION VS. DEPTH
 CONFIGURATION II-C



Knee Flexion
Hip Flexion
Elbow Flexion

4. Hip Extension
5. Shoulder Joint Extension
6. Trunk Flexion

7. Shoulder Joint Extension
8. Shoulder Joint Abduction
9. Hip Abduction



SLIDE 9.

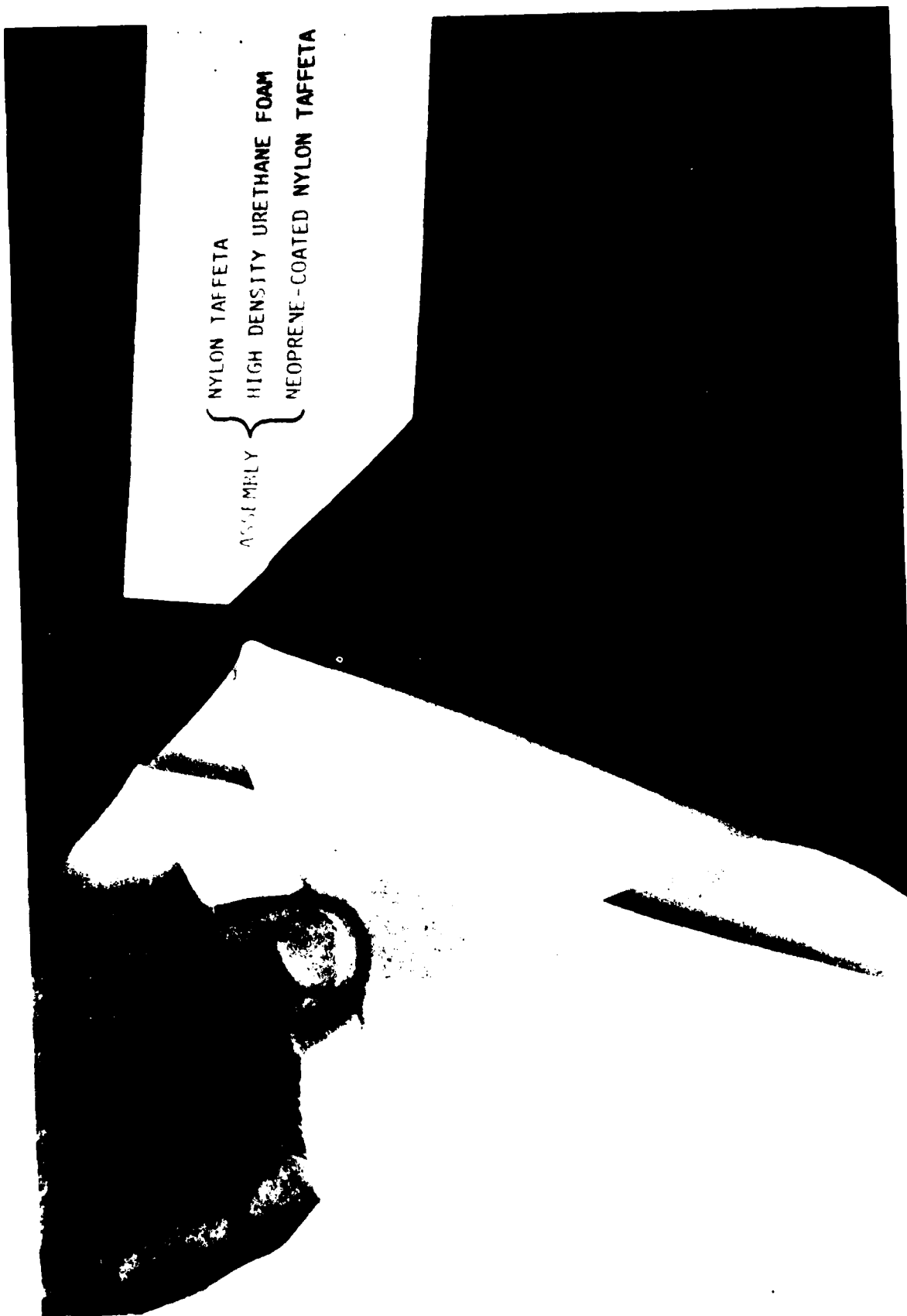


SLIDE 10.



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SLIDE 11.



NYLON TAFFETA

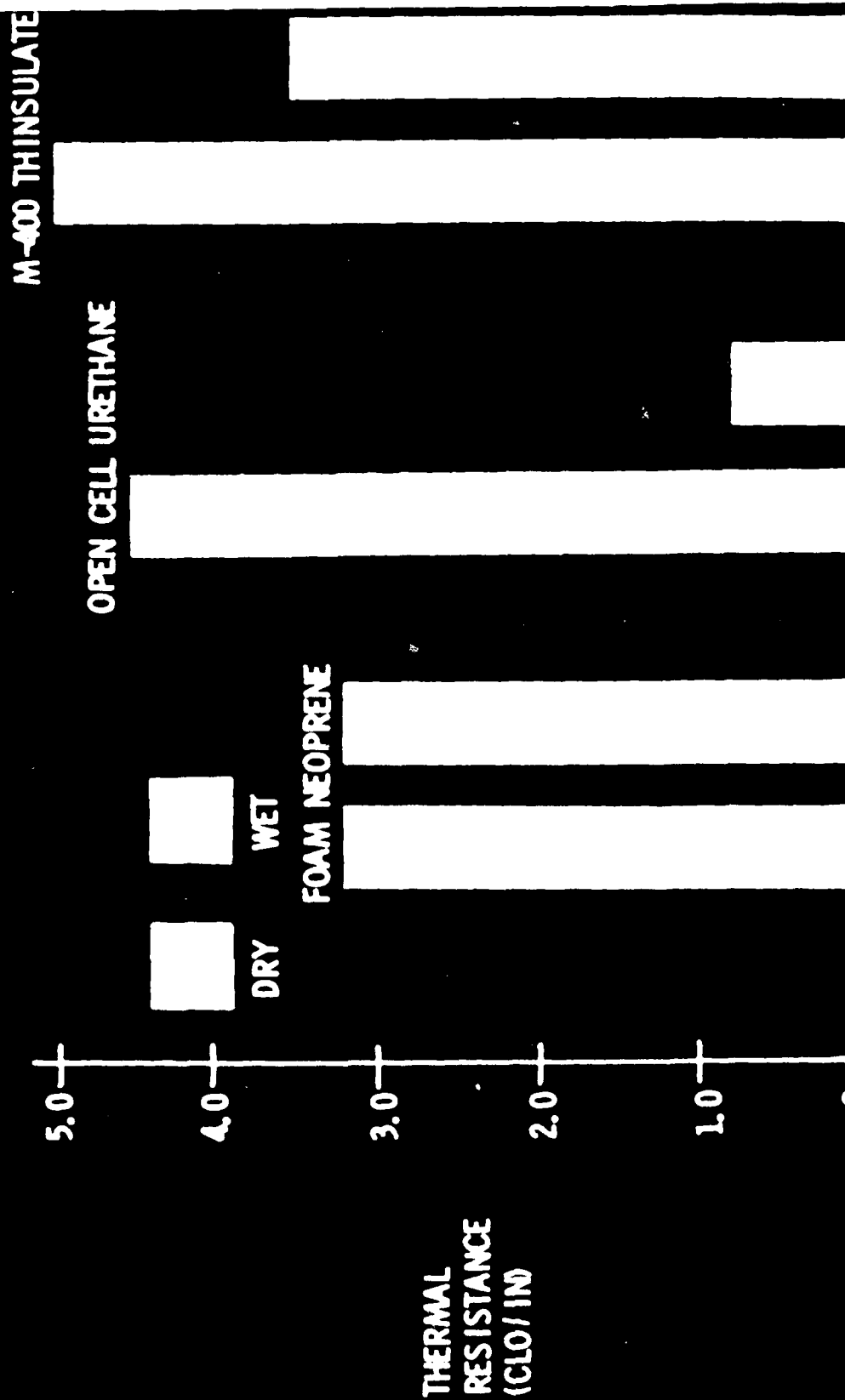
HIGH DENSITY URETHANE FOAM

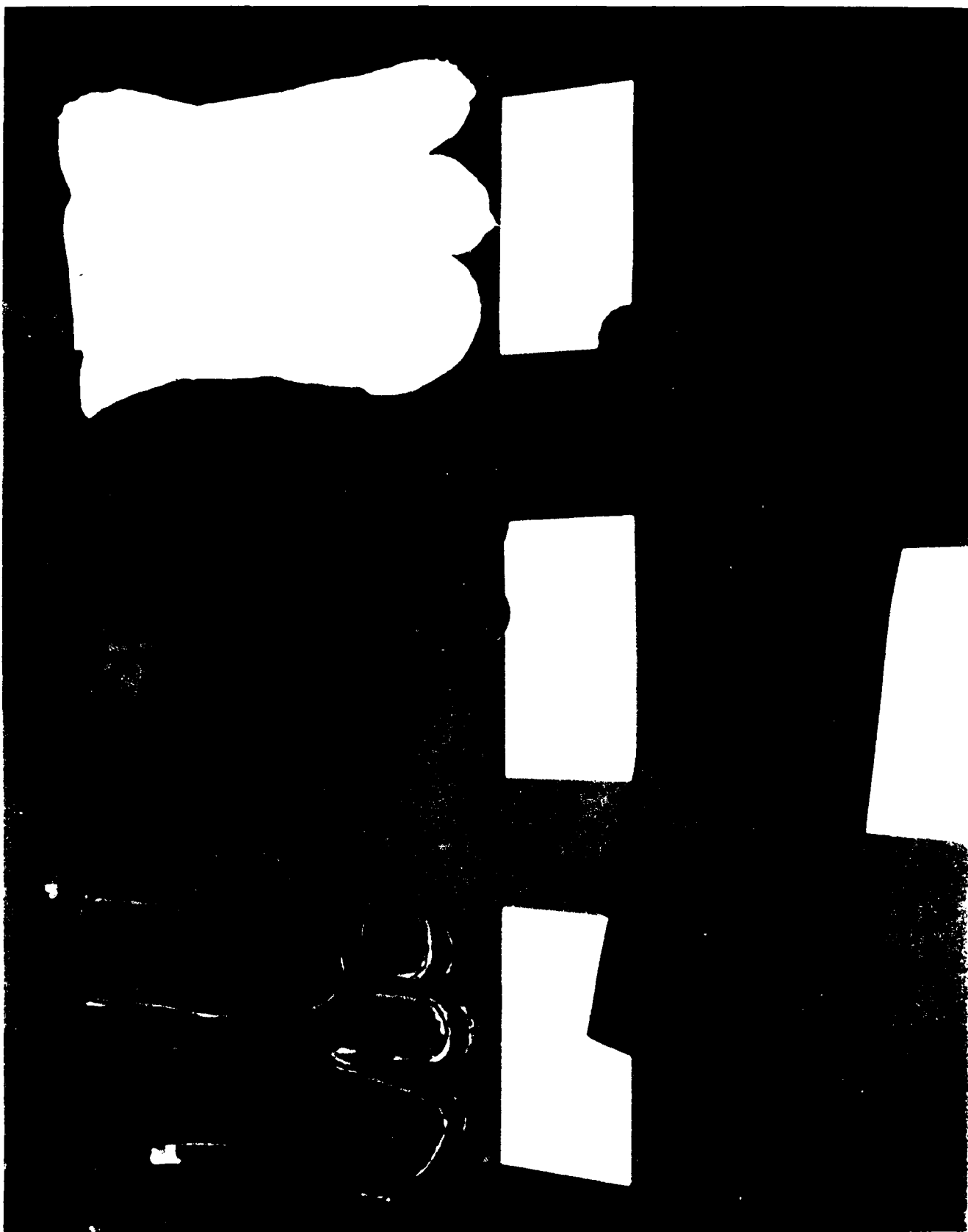
NEOPRENE-COATED NYLON TAFFETA

ASSEMBLY

NCSC

COMPARISON OF SPECIFIC THERMAL RESISTANCE OF DIVER GARMENTS BEFORE AND AFTER SUIT FLOODING



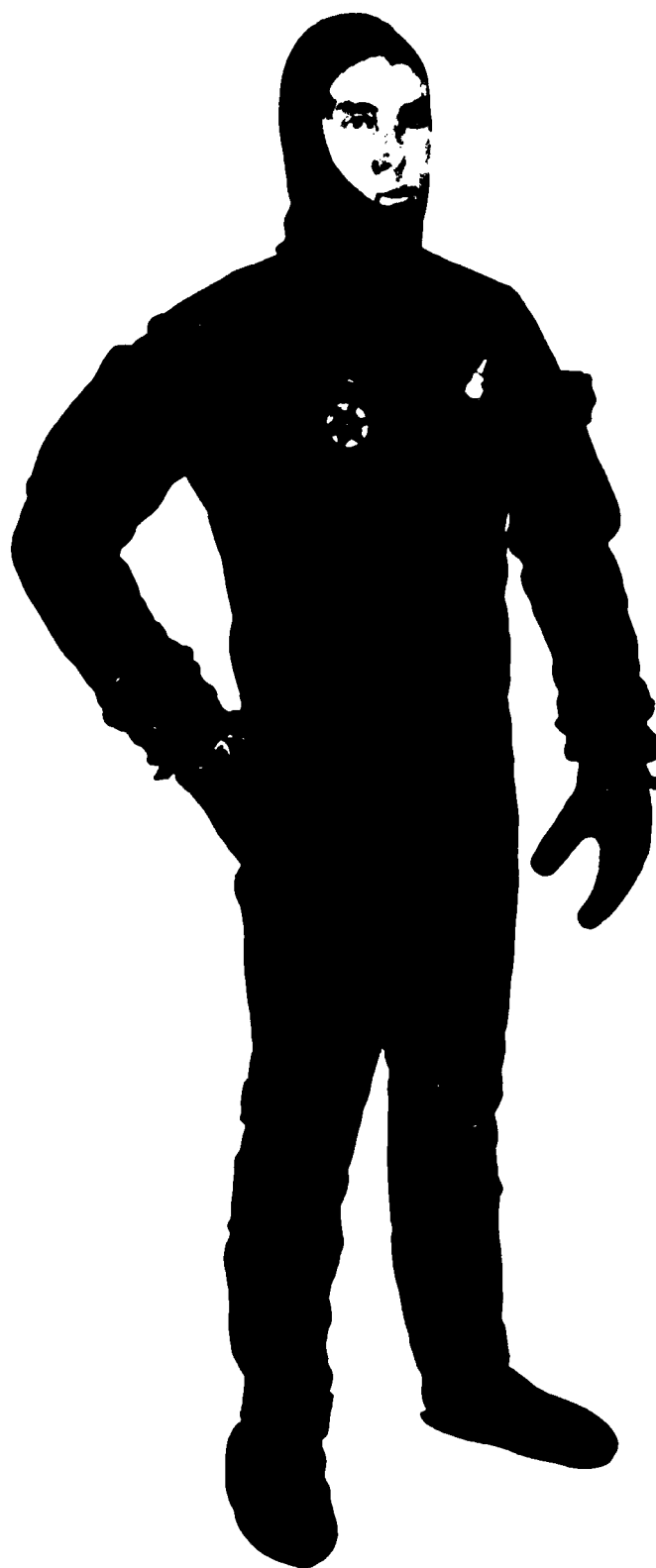




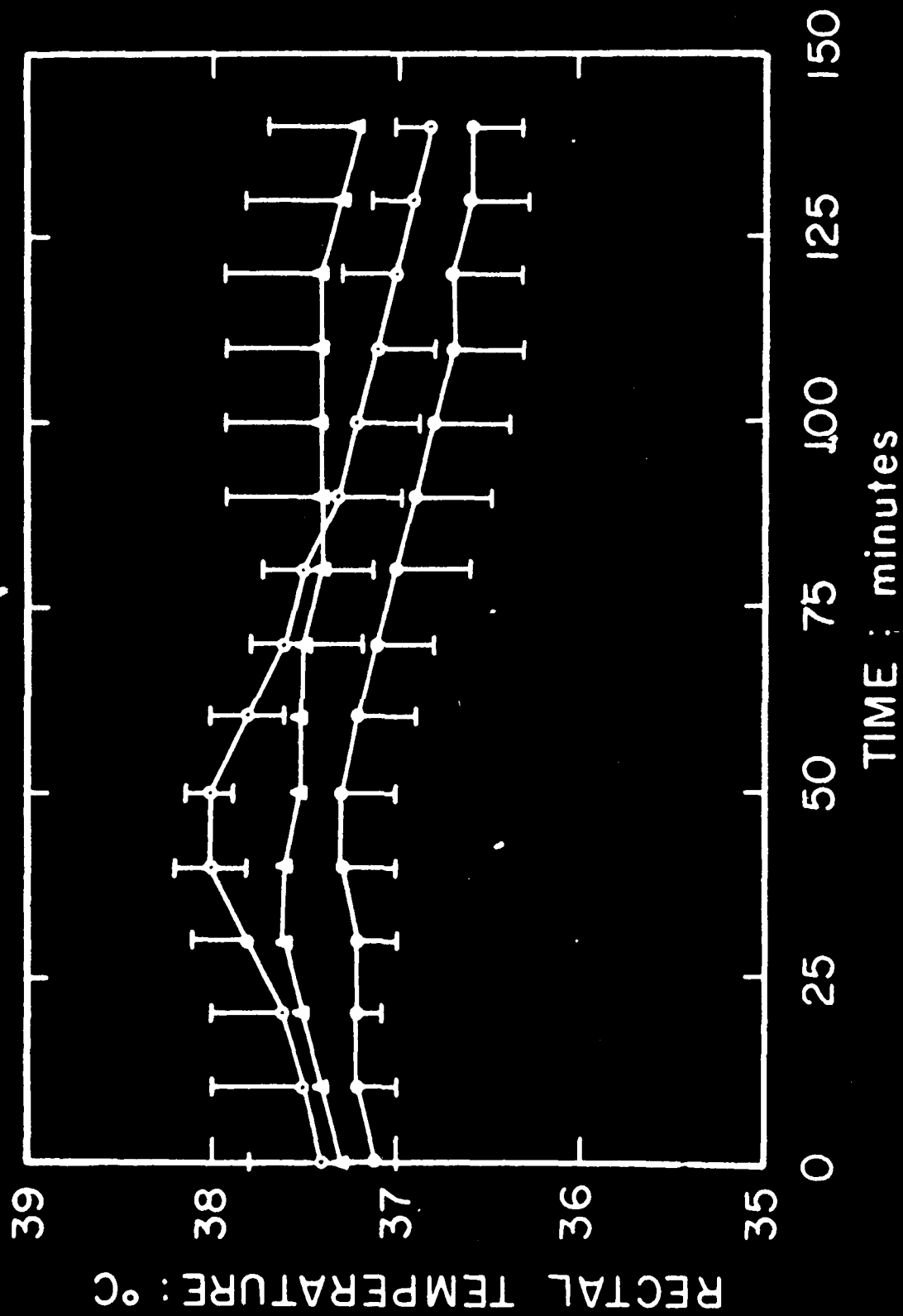
SLIDE 15.

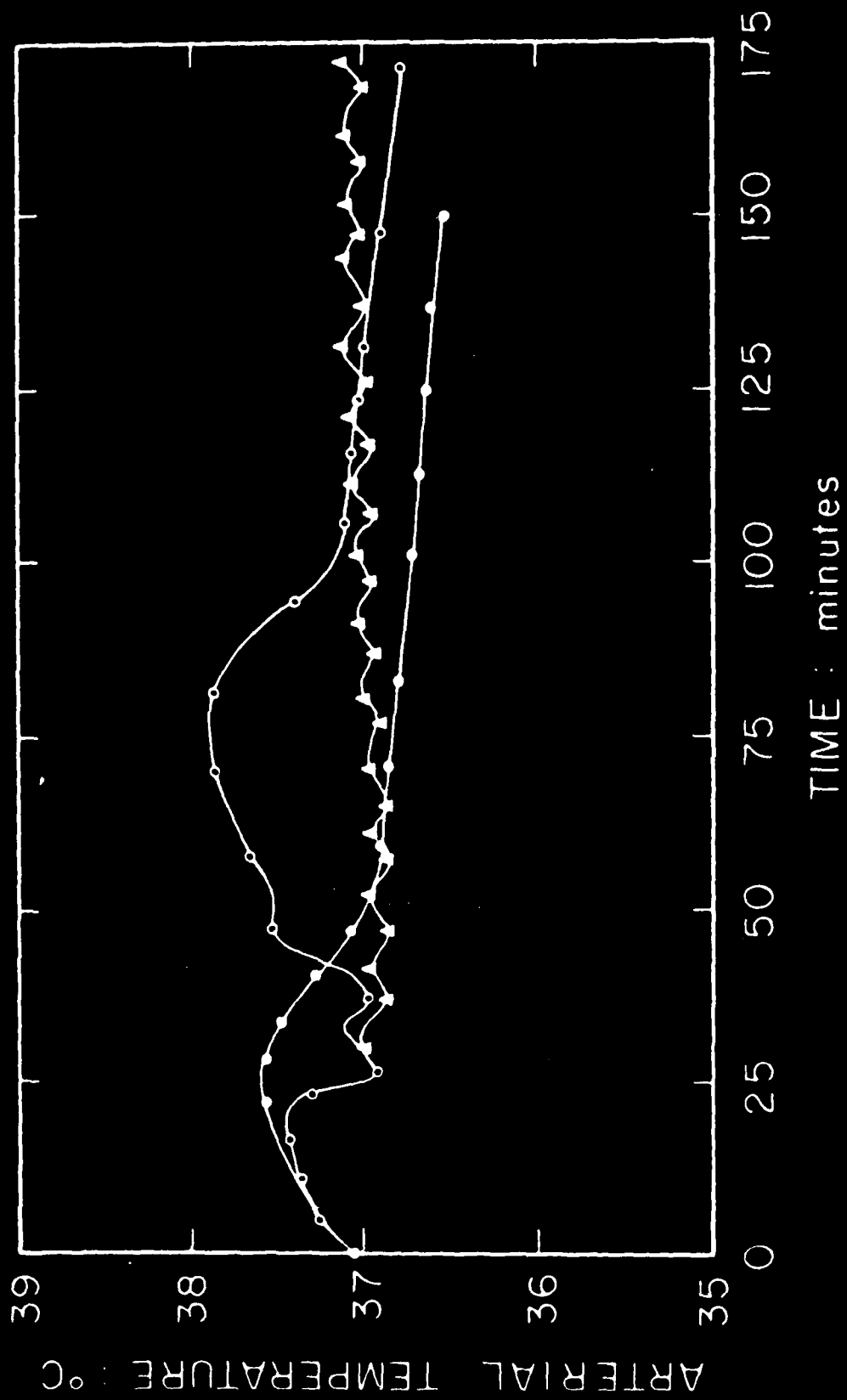


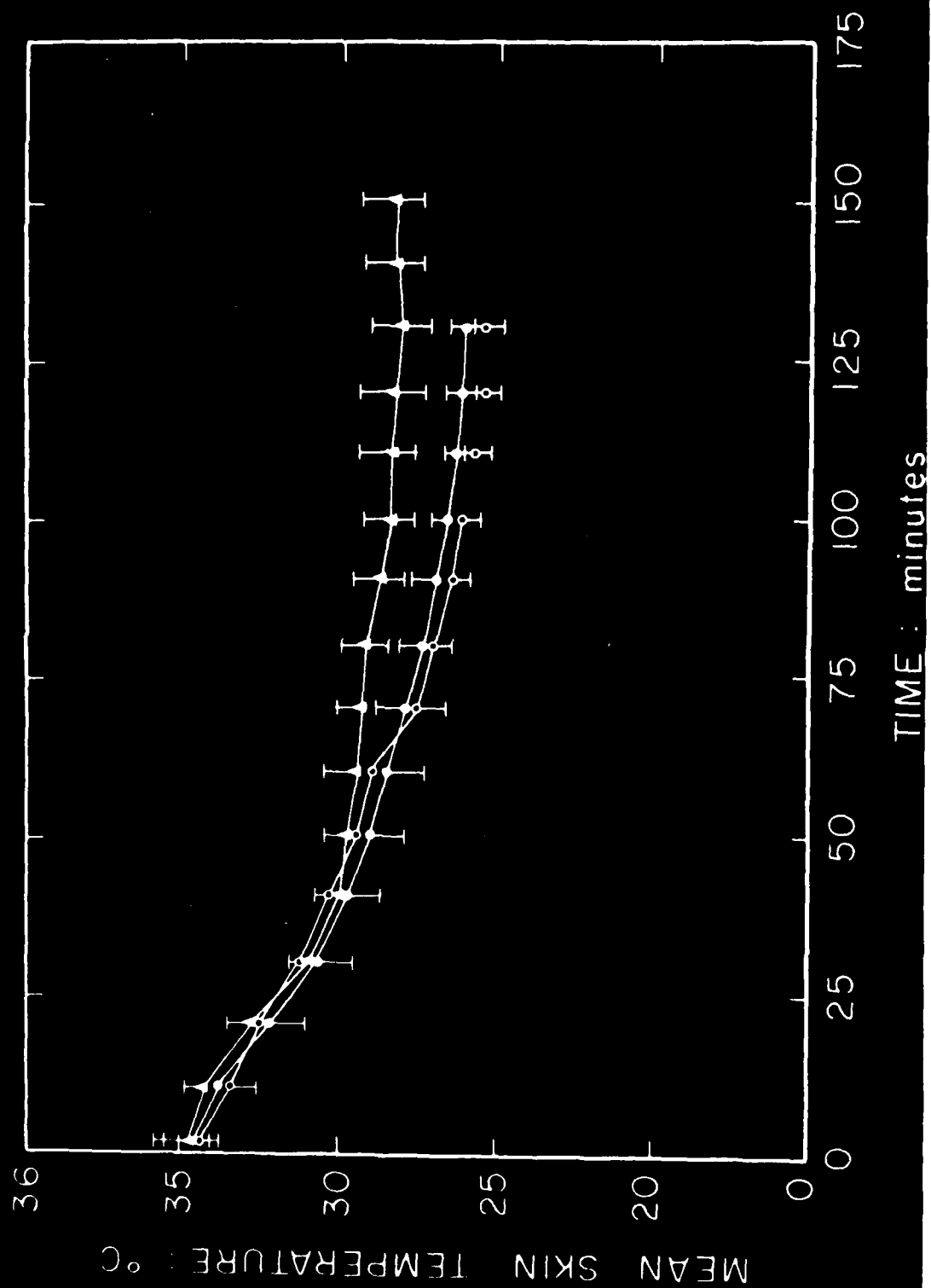
SLIDE 16.

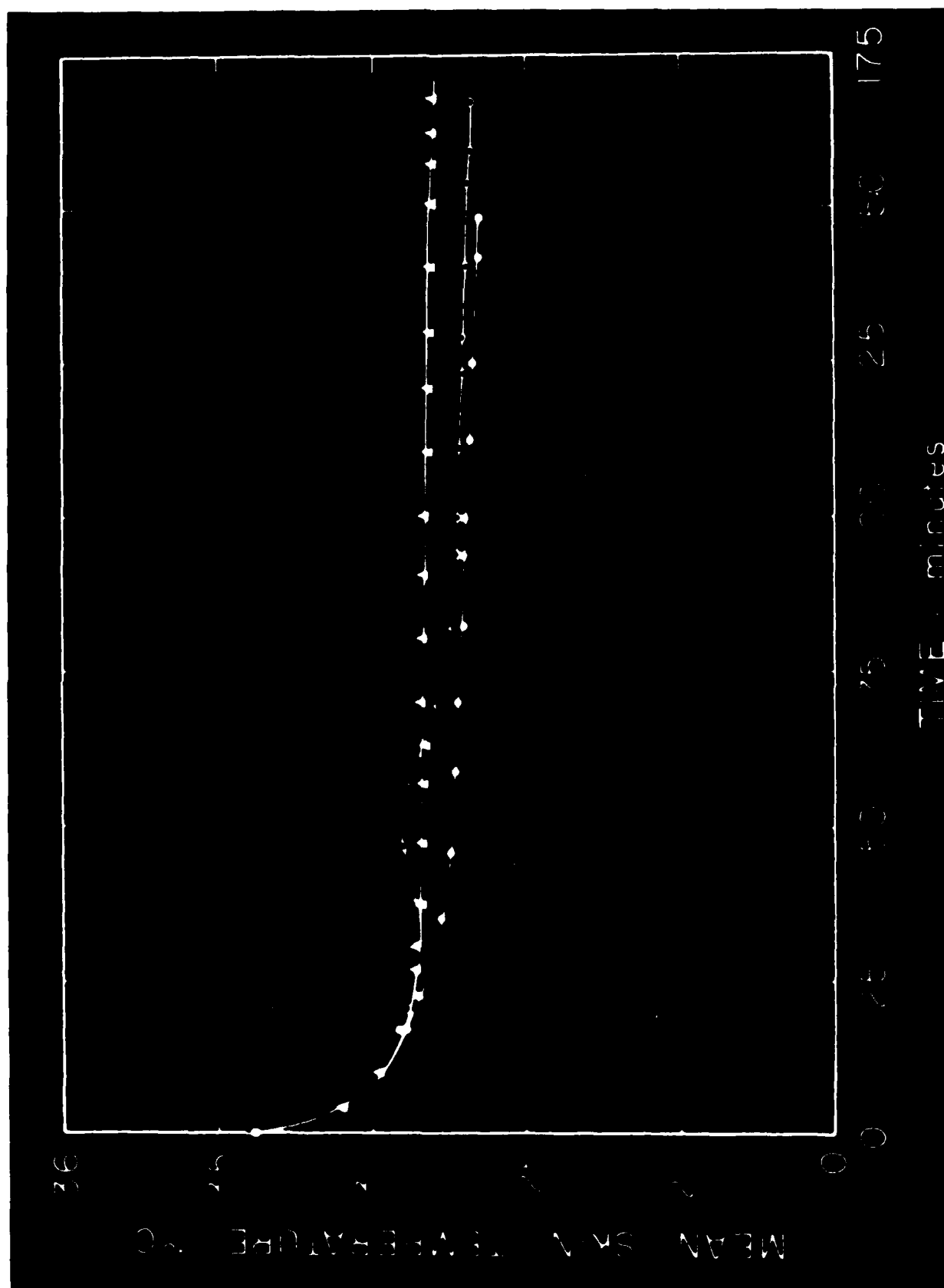


SLIDE 17.









SLIDE 21.

INSULATION OF DIVER'S SUITS

	1/4 IN. WET SUIT	1/4 IN. DRY SUIT WITH FOAM UNDERGARMENT	DTP PASSIVE SYSTEM
 IN AIR	1.8 CLO	2.5 CLO	2.5 CLO
 SURFACE	.75 CLO	1.2 CLO	1.2 CLO
 100 FSW	.25 CLO	.75 CLO	1.2 CLO

NAVAL COASTAL SYSTEMS CENTER—PANAMA CITY, FLORIDA

EVALUATION OF KINERGETICS BREATHING GAS HEATERS FOR USE IN
OPEN-CIRCUIT DEMAND UNDERWATER BREATHING APPARATUS

J. Middleton

United States Navy Experimental Diving Unit

In December of 1977 we were conducting our yearly deep dive, this time to 1500 fsw, and during the decompression phase we evaluated the Kinergetics breathing gas heater. There are several on the market right now and the reason that we picked the Kinergetics heater was that an evaluation had been done by Barry Miller of NCSC. He had done an evaluation of several commercial breathing gas heaters and one that had been designed by the engineers at the lab. The end result was that the Kinergetics heater was the most efficient and the most expensive. The lab tried to take the best aspects of this heater and several others to come up with something better but they weren't successful. This may sound like an advertisement for Kinergetics but it is not. The heater is simply very effective with a good reputation in the commercial industry.

I might start off by saying that I'm glad that somebody else has problems with fast-responding thermistors besides myself. We went through a lot of trouble with them on this particular study and could not get any of them to last with any degree of reliability. Our divers were too rough on them.

What I have to say will support the papers of John Zumrick and Philip Hayes at this Workshop as well as demonstrate what a breathing gas heater can do and what its limitations are.

We were searching for answers to three problems in 1977. One was to determine if the heater in a man-mode would do what Kinergetics said it would; secondly, we wanted to find out if it was practical because we have to consider that it's another piece of hardware that the diver has got to maintain, set up and use; and finally, we wanted to see if it would affect breathing resistance. We hope to come out soon with a new set of performance goals for diving equipment. This is an attempt to get to the point where the diver life support system is not the limiting factor in his performance. We want what he can physically do to be the limiting factor and not his equipment. Consequently, any equipment that can affect the work of breathing must be tested. What we did was to evaluate the Kinergetics breathing gas heater in conjunction with what we call a Mark I Mod S mask, basically the same as what the commercial industry uses as either a DSI 18B (the Bob Kirby version) or the U.S. Divers KMB-10. There are two versions of breathing gas heaters designed and built by Kinergetics. This one, the model 3375-2 (holding up a heater) is a counter-flow heat exchanger. It is more efficient than this other one, the model 3375-3 (holding up another) which has parallel flow. The way this Kinergetics heat exchanger operates involves a series of internal wire mesh screens that are made of a proprietary copper-nickel alloy and do the heat exchanging with the gas. The water simply circulates around the outer perimeter. We used the counterflow heat exchanger in 1977 because it was the most efficient. John Zumrick helped me with the computer programming on the tests and he is now going to evaluate the parallel flow heater in basically the same test on our next saturation dive. The parallel

flow heater takes two gallons a minute rather than one gallon a minute to get the same level of performance according to manufacturer's specifications. The manufacturer states, and we have found it generally to be true, that given 35°F or warmer water you can count on 95°F output from the heaters, regardless of work rate by the diver, assuming that you've got 110°F water flowing to it. The counterflow design presents more problems than the parallel flow because of configuration as you will see in the slides. The parallel flow exchanger can be stuck in line with the supply to the diver's hot water suit. Since it is in series in the hot water umbilical, nothing has to be diverted.

This was one rationale for our evaluating the parallel flow heater. We have an operational requirement now to use a heater where the counterflow unit is not practical. I expect results to be pretty much identical to what you will see here. First slide please (See Figure one at end of paper).

We mounted the Kinergetics heater to a standard SCUBA backpack. It would normally be worn on a diver's bailout bottle. The gas umbilical came in and at the umbilical hookup to the heater we sensed temperature of the breathing gas in and out with probes. We also placed a pressure transducer across this loop because we were concerned about the pressure loss in the heater. If breathing resistance did go up and we sensed a high loss here, we would know what the problem was. We also put a pressure transducer in the oronasal cavity, which is standard procedure for us, to measure respiratory

4P. We had an inspired gas temperature probe, which was a slow probe, about 1 second response. The hot water supply came in counter flow fashion. We sensed the hot water temperature in and the temperature out; the hot water was simply dumped overboard. Now the way that this particular set up was done involved using one of Dick Long's hot water flow splitters. We had the hot water umbilical from the surface T'ed off so that one gallon per minute went to the heater and the rest to the diver's hot water suit. This same thing could be accomplished in the parallel flow unit without the splitter by simply putting it in line with the hot water umbilical which is logistically more practical.

It is interesting that the hot water came in on a 50 foot umbilical and we only saw about a 1-2°F drop in the umbilical from the supply point in the chamber. The gas supply was chilled down as close as possible to ambient water temperature; the diver was in 40°F water. We made a make-shift heat exchanger that we installed in our wet pot and the best we could do was to get the gas into our heater at 50°F. Next slide.

The dive was done over two days of decompression. The first day was at 1200 fsw, the next day at approximately 1100 fsw. Table 1 summarizes the results. We put three divers in the water each day. They performed on a bicycle ergometer, working on a 6/4 minute work/rest cycle. Each cycle consists of an increase in work rate by 25 watts where we went from 25 to 150 watts. It's important to note that a diver is working a little harder than stipulated by these

rates when he is sitting on a vertical ergometer like this one. He has to concentrate on holding himself on the ergometer (although he is held in place with straps). Balance is a factor as well as the pedalling requiring that his legs move through the water. So when we talk of 150 watts on a bicycle ergometer, the diver is probably putting out in excess of 200 watts.

Our gas temperatures into the heaters were right around 50°F and our gas out of the heater was between 95 and 96°F. It was very consistent. Our inspired gas temperature was in the 75-77°F range. The reasons for this are obvious. The ambient water temperature was 40°F and we put 110°F water to the heater, giving us 95-105°F gas out of the heater. You can see that this is the chamber hot water manifold temperature so we are losing only a couple of degrees through the hot water umbilical. During this first test we had five gallons per minute being pumped with four gallons per minute to the suit and one gallon per minute to the heater. The divers complained during our first day of test that the hot water suits were ballooning with the four gallons per minute flow rate. Next slide.

On the second day we dropped our flow rate. We were at 1100 fsw. The flow rate was set with 3 1/4 gallons per minute to the suit and 3/4 gallon per minute to the heater. I was concerned that this would affect the heater detrimentally so we bumped up the hot water temperature by 4 or 5°F. The divers ended up doing a fair amount of by-passing a lot of their flow because they were too warm. Table 2 shows that the incoming gas temperature was around 50°F into the heater and the temperature of the gas coming out of the heater in-

creased almost 10°F. This and subsequent unmanned tests determined that this particular heater is quite insensitive to pressure and flow rate as long as you have got around 110°F water supply to the heater. You can reduce the flow rate by 1/4 to 1/2 gallon per minute and not really affect the performance of the heater. The interesting thing is that we stayed around 75°F inspired gas temperature the first day while on the second day, even though the temperature coming out of the heater was 10°F higher, we were still seeing inspired gas temperatures around 75-76°F. That told us that the losses between the heater and the mask were tremendous. You would expect that. The heater, while they are small and very efficient, was losing a lot of what it worked so hard to gain. The gas coming out of the heater is dropping in temperature before it can get to the diver. There is not much that can be done about this without severely complicating the system.

One thing that we did want to look into, and I've built a little mock-up here to show you, was a mask sideblock heater. I believe that Bob Kirby and Bev Morgan are now marketing a sideblock heater. Theirs is more extensive than this. It is a jacket that goes around the sideblock and is extended around the intermediate pressure tube and over the second stage itself.

For this approach to help insulate the breathing gas between the heater and the mask, a method of directing hot water out of the heater and into the sideblock heater jacket must be devised. The way we are thinking of dealing with this is to build a heater where the

gas exit was concentric with that of the hot water exit (parallel-flow type heater) . In this mock-up you can see that the gas supply is concentric with a whip reinforced with a piece of tygon tubing. That way the water can be ducted up around your whip to the sideblock. The hot water is then dumped over the side block. We think that this will be an effective means of further heating the gas. Based on Claude Piantadosi's work, gas temperatures of 75°F in 40°F water are obviously better than breathing 40°F gas but it is not enough to be really considered an active form of diver heating. Whether or not we have problems with the inspired gas becoming too hot with the sideblock heater is not yet known. This seems to be the quickest, most inexpensive method of further heating a diver's breathing gas. Based on John Zumrick's work and Phil Hayes' work, any heating is better than nothing at this particular point. We did develop, for the Mark 2 Mod 1 Deep Dive System, a sideblock heater that was used with a very early model respiratory gas heat exchanger that was quite inefficient. We developed the quick disconnect fittings for making all this into a practical hook-up system that can be fitted in a couple of seconds. So there is not really a large logistical problem with using a sideblock heater. The problem is to do the test and find out how much additional heating we can get and what kind of apparatus we will need to control the diver's inspired gas temperature.

Nuckols: Where there is insulation, there is also buoyancy. By insulating the gas hose and sideblock, are you getting to the point where the diver is going to be buoyant?

Middleton: When I say insulation, that is a misnomer. We mean that we are insulating with hot water. The hot water will be dumped into the sideblock jacket and form a warm-water insulation around the sideblock. I feel that we are going to do more in just keeping the gas warm than actual heating by this method. This is not an insulation that is compressible or incompressible. It will all be flooded so that there will be no buoyancy change.

Kuehn: If I understand you right, this present system you have does meet the Braithwaite heating requirement limits but, at the current state of technology, it does not meet the Piantadosi limits.

Middleton: That's right.

Kuehn: With the changes that you are proposing for active hot water "Insulation", you have a chance at meeting the Piantadosi limits.

Middleton: That's right. We are not that far away from the Piantadosi limits right now. With the sideblock jacket we may get there. Another thing that I might mention is that breathing resistance was not affected at all; in fact, we found that the Kinergetics heater almost served as a plenum. There is significant volume inside the heater, even with all the elements, so that it serves as a plenum rather than a restriction, and the pressure drop across the heater at 150 watts, with the diver somewhere in the vicinity of 75-90 rmv, consuming 3 litres of oxygen per minute, was

of the order of only 1/2 psi.

Long: I don't want to take up your time on this point but I can show you how we managed to achieve a system that had both counter-heat flow and in-line design in one unit. It's not really a problem. You can do away with the T and the higher flow rate. I can show you how to do that.

Hayes: Even that heat exchanger that was passed around is supposed to work in a counterflow system. You are supposed to pass gas and water flows in opposite directions; however, the Royal Navy doesn't use it correctly either. It's impossible to get the correct configuration because you have a great deal of slack piping. The findings are the same; however, I can't get hot gas to the man even when correct.

Long: You'll also find tyvon tubing placed over the outside of the hose. This will decrease the temperature loss of the gas without any active insulation derived from passing water through it. You could improve the insulation by putting a jacket on it.

Hayes: I don't think you can't use any heat exchangers at all for the Royal Navy. I think you can't use them at all.

Long: I've seen divers working without heaters for a long time. It is not a critical requirement with short in-water times.

Long: You'll find that uniformly in the commercial diving field that there is disrespect for this position. They don't want gas heating and they don't think that they need it. Their divers are acclimatized to the cold, etc. They don't want the increased complexity and

weight of heaters. They simply do not believe that it is necessary.

Kuehn: How deep are such dives, typically?

Long: The dives are in excess of 500 fsw and, in the Cognac job, they were at over 1000 fsw. There are so many problems experienced there that have a more dramatic every-day impact, when the gas is cold, when you really don't care.

Hamilton: What I was told was that the reason that they didn't need heaters was that the gas hose was bundled with the hot water hoses. This would amount to a very long parallel heat exchanger, so that the gas was warm when it got to the diver. But was it warm enough to meet any of these specifications that we have talked about?

Middleton: I really don't know. I would have to see the set-up but I have my doubts that it would work.

Long: There is a lot to be said for the integrated umbilical in which everything is held together in one conduit, holding the gas temperature up.

Middleton: You were not talking about a composite umbilical but just a bundle?

Hamilton: I don't know.

Middleton: A composite umbilical could make a difference but a bundle probably wouldn't.

Hamilton: The word used by Taylor Diving was "bundled together" but I don't know if that meant taped or whatever.

Wissler: When I was in Aberdeen, I saw the Comex umbilical. It has

all of the tubes inside one outer sheath, so you should get fairly decent heat transfer there. To change the subject, is the Rat Hat being used anymore?

Long: Oh yes. We're sharing the standard equipment. These helmets have a small heat exchanger right in the back of the neck.

Wissler: Is this a passive device?

Middleton: No. They have the passive respiratory heat exchanger inside the hat on the mouthpiece but they also have a small heat exchanger built into the back of the hat for use with the hot water suit.

Hayes: Ramsey Pearson told me of a system used by the French in which they use electrical heaters on the helmet, but there appears to be little information available on this system.

Long: One of the problems they had with that system was the shorting out of elements which were sitting in the gas supply. This would lead to potential gaseous contamination. Two different versions were built.

Middleton: It seems to me that if it is determined that the respiratory heating is indeed necessary then hot water heaters using open circuit demand is one of the easiest ways to effect it. We have installed in the U.S. Navy Mark 14 push-pull being developed now, another version of a Kinergetics heater but we have not really tested its effectiveness. It is a constant-flow rather than a demand system. In any type of helmet, the need for heated gas for a constant-flow-recirculated helmet is not quite as great as for an open-circuit system. You

don't have the same instantaneous flow rates, so the cooling is not as dramatic.

Kuehn: I would like to ask Dick Long, in follow up on Jim's presentation, if this type of technology depends on hot water supply from the surface?

Long: No, we have closed-circuit systems that have gas heaters in them as well, as heaters are really a very primitive technology and their problems are easy to solve. The only real problem is the interface one between the breathing helmet type and the exchanger. The recognition of the need for the exchanger is most important. In terms of technology we have been ready to do it for years. The question then becomes, and it is a legitimate point of discussion for this group, namely at what temperatures should the limits be? People like myself need your input to that design criterion because we can make that gas temperature any value that you want.

Kuehn: Do you think that there is any depth restriction on a bell that is going to be a problem?

Long: No, depth is not important, since we always put the heaters directly on the diver some place, as close in to the helmet as possible. We are using the hot water for the active support of the diver anyway so we prefer to use the hot water to supply the heater in parallel with the diver to keep the logistics simple. The actual engineering of such systems is relatively straight forward and simple. The great problem in this area is getting input from yourselves as to what the temperature ought to be. Then, on the operator's side, the

problem is how to avoid hyperthermia and how to recognize this problem. It is a greater problem than the operators feel that it is, since the divers don't complain. They won't complain because they don't want to be put on the beach and become part of the unemployment problem.

Middleton: Do you think that a simple heater, in-line with no additional active or passive insulation or heating, outside of the heater, may still fail to meet the respiratory temperature limits that we want?

Long: I would say that they would fail, without question.

Middleton: So more work will have to be done if the temperature limits are raised. I've got copies of this report to pass around.

Hamilton: A point of clarification. You mentioned the difference between the constant-flow and demand systems and you referred to the rather high heat flow of the demand system. But, in using the analogy of the CO₂ cannister which is designed to hold one breath, if you've got one full breath in the plenum, then you have a dwell-time that is much longer. The demand system should then be much more efficient instead of less, shouldn't it?

Middleton: Yes and no. There is another factor to be considered. The fact of the matter is that all this is lost between the helmet and the backpack in a closed-circuit rig with the cannister on the back.

Hamilton: If it dwells in that unheated space for one breath, then that is why it gets cold.

Middleton: Yes, but in a closed-circuit rebreather you do get

effective gas heating by the cannister. You don't have heat losses due to gas expansion across the side block or demand valve.

Hamilton: Because it's shorter?

Middleton: More so because it is insulated because of the inherent design of closed-circuit rebreathers. The gas also doesn't have to be expanded a couple of times as it does in an open-circuit demand band mask.

Hamilton: That's true; it is already expanded. As a matter of fact if you have one breath there and another one in the pipe, that breath sits and gets cold between breaths.

Greene: Jim, are you doing any work with heating on semi-closed rigs of the sort that combat swimmers use?

Middleton: Like the Mark 15, for instance? Not right now. Obviously their profiles call for mission durations that involve times in excess of what the rig can actually do in cold water. That problem is not being approached right now. There is not a short-term solution to that problem.

Nuckols: Well for one thing you are talking of shallow water and another is that, rather than mixed gas, you are talking of N_2-O_2 .

Middleton: Well the thing is, which probably won't mean anything to anyone other than Navy personnel: if the EOD version of the Mark 16 UBA flies - you are all familiar with the Biomarine CCR-1000 rebreather, well our Mark 15 and 16 UBAs are basically the same rig - we may have a partial answer to the problem Mr. Greene addressed. The Mark 16 has a lexan cannister, and we have found for the first time a CO_2 cannister with a duration that is independent of water temperature.

It is solely dependent on depth. Up to this point we lost so much heat from the cannister directly to the water, that depth did not really affect the life of the cannister. Now we're finding that we have a cannister that is so well insulated by the lexan that we can test this cannister at 35°F and 90°F and at all 10°F increments in between with no change in cannister duration. So in answer to your question, if the Mark 16 cannister turns out to work, it could be a cure-all for that kind of mission as long as it's shallow. Of course I am referring here to cannister duration rather than diver heating since inspired gas temperatures on this UBA will be increased only slightly over previous rigs for the reasons previously discussed.

Kuehn: Contrary to the situation that we had in 1974 in which it seemed that the technologists were not keeping pace with the scientists, in that the scientists were defining the problem but the technologists had not produced the equipment that could extend cold water diving, the situation now seems to be reversed. It is now necessary that the scientists define the limits for the technologists to meet in diving equipment design. The technology for passive thermal protection and for respiratory gas heating is well in hand. It is the limits that now need development. With that observation, I will now close this session.

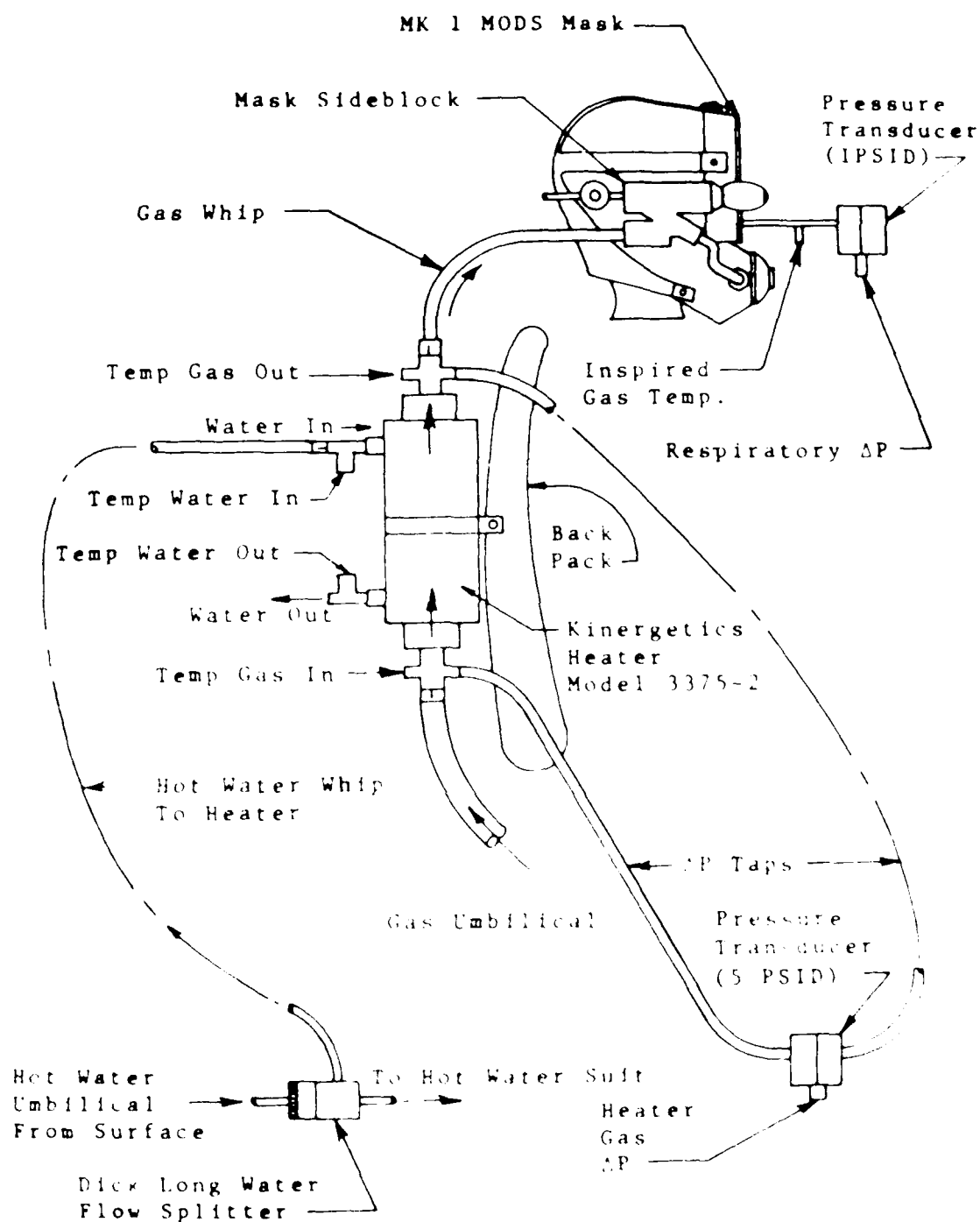


FIGURE 1 KINERGETICS TEST SET UP

TABLE 1

TEMPERATURES °F

Diver No.	Gas Into Heater	Gas Out Of Heater	Inspired Gas	Ambient H ₂ O	H ₂ O Into Heater	H ₂ O Out Of Heater	Chamber H ₂ O Manifold
1	50.1	96.0	77.5	40.0	110	105.0	112.0
2	48.7	96.9	74.5	40.0	110	97.8	112.0
3	48.3	95.4	74.6	40.0	110	100.0	112.0

NOTE: (1) Divers depth: 1250-1220 fsw

(2) Hot water umbilical flow rate: 5 GPM with 4 GPM to suit
and 1 GPM to heater

TABLE 2

TEMPERATURES °F

Diver No.	Gas Into Heater	Gas Out Of Heater	Inspired Gas	Ambient H ₂ O	H ₂ O Into Heater	H ₂ O Out Of Heater	Chamber H ₂ O Manifold
1	49.6	104	76.7	40.0	115	108	115
2	51.8	101	76.0	40.0	115	106	115
3	49.1	104	77.3	40.0	114	106	115

NOTE: (1) Divers depth: 1150-1120 fsw

(2) Hot water umbilical flow rate: 4 GPM with 3.25 GPM to
suit and 0.75 GPM to heater

SESSION III

BIOPHYSICAL MODELLING OF HYPOTHERMIA

Kuehn: This session is concerned with diver modelling. As you all know, in the research in support of cold water diving, we are restricted from doing vital physiological experiments with humans below 35°C core temperature. It is only in recourse to animal experimentation or to data collected in treatment of accidental rewarming that we can observe the unique physiological consequences of very low core temperatures. It is a responsibility of the mathematical modellers to take this animal and accidental victim data, plus our limited experimental data, and to build upon it various models of human hypothermia that can be applied to the very cold hypothermia problems encountered in diving, such as that of the lost bell diver or that of the long-term open-water dive. With that introduction, I'm going to call on two eminent modellers to present their work to us this afternoon, first Gene Wissler talking about his general mathematical model of the human thermal system (which he will also refer to in the next session in his presentation pertinent to the lost bell problem) followed by Dr. Sally Nunneley who will present the application of this model to the surface immersion problem. Questions pertinent to both papers will take place after Dr. Nunneley's presentation.

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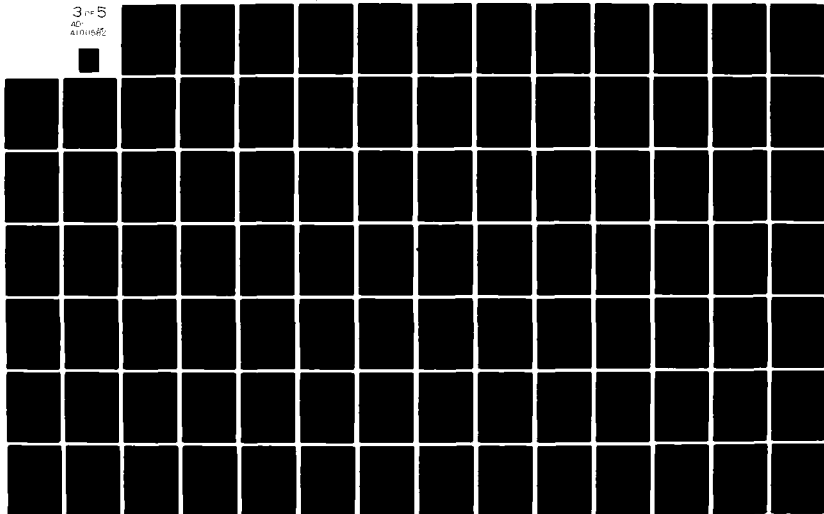
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A Mathematical Model of the Human Thermal
System with Reference to Diving

by

Eugene H. Wissler
The University of Texas at Austin

It is generally difficult and expensive to conduct experimental studies on human subjects under conditions involved in diving. Increased value can be derived from such studies when the results are related to a theoretical model. The purpose of this paper is to describe a mathematical model which has been developed to predict human thermal response under a variety of conditions.

At the present time, the model is being applied in three different ways. The first application involves use of the model to predict behavior under conditions outside of the range of experimental study, after prior validation for the conditions that have been studied. The model is also useful for evaluating physiological phenomena which are not measured easily; for example, the importance of countercurrent heat exchange during cold exposure can be assessed theoretically. A third application is the analysis of situations for which no experimental data are available. Use of the model to analyze the "lost bell problem" will be discussed in a companion paper.

Mathematical models can take many forms. The simplest model consists of a single energy balance,

$$S = M - C - R - E - Re \quad (1)$$

in which S = rate of storage of heat,

M = metabolic rate,

C = convective surface loss,

R = radiation loss from the surface,

E = rate of evaporative loss, and

Re = rate of heat loss through the respiratory tract.

In 1947, Machle and Hatch [1] improved on the simple energy balance by introducing the concept of core and shell temperatures, which are often represented by the rectal and mean skin temperatures, respectively. In such models, the stored heat content is usually expressed as a linear function of the core and shell temperatures.

About the same time (1948), Pennes [2] solved the steady-state heat conduction equation for a cylindrical region with uniform heat generation and showed that the resulting temperature profile provided a reasonable approximation for profiles measured in the forearm. Pennes' model served as the basis for Wissler's first model [3] in which six cylindrical elements were used to approximate the entire human thermal system. Wyndham and Atkins [4] developed a similar model at approximately the same time.

Mathematical modeling came of age during the Apollo program when Stolwijk and his associates [5] developed a computer program capable of predicting the thermal response of astronauts during extravehicular missions. Although their relatively simple model was adequate for analyzing changes resulting from heat stress, its predictive ability for cold stress was somewhat lacking [6]. Exposure to a cold environment results in large internal temperature gradients which amplify errors owing to use of an insufficient number of temperatures to describe adequately the temperature field within the body. A manifestation of this problem is the general lack of agreement between mean body temperature and any fixed linear combination of rectal and mean skin temperatures for hypothermic subjects. Furthermore, factors which are relatively unimportant in

warm subjects can be of great significance in cold subjects; as examples of this one can cite the perfusion rate in muscle and the magnitude of counter-current heat exchange between large arteries and veins. Consequently, a model which is capable of describing thermal response during the kinds of cold exposures likely to be encountered in diving will necessarily be rather complex.

The model used in this study can be described best by discussing first the physical basis for the model and then discussing physiological control equations. Physical factors which must be evaluated properly include the following:

1. Temperature is a function of position and time.
2. Since heat conduction is influenced by geometry, the human form must be adequately represented.
3. Thermal properties vary with position throughout the body.
4. Heat generation owing to metabolic reactions varies with position and time depending on the kind and intensity of exercise and whether the subject is shivering.
5. Heat transfer from regions of high temperature to regions of low temperature occurs by conduction and convection in circulating blood.
6. Countercurrent heat exchange can occur between adjacent arteries and veins.
7. Heat transfer occurs in the respiratory tract.
8. Heat transfer between external surfaces and the environment occurs by convection, radiation and evaporation.

Shown (on p.224) is the geometric arrangement on which the model is based. Fifteen cylindrical elements represent the head, thorax, abdomen and proximal, medial and distal elements of each arm and leg. Within each element, temperature depends on radial position and time.

Each major element is further divided into cylindrical shells, within which appropriate physical properties are assigned. Illustrated in Fig. 2 are assignments which might be used for an extremity. Note that certain properties, such as thermal conductivity, density, and specific heat, vary with position but not with time, while others, such as the metabolic and perfusion rates, also vary with time.

Heat generated by metabolic reactions either remains in the element and increases the temperature, is conducted to the surface and transferred to the environment, or is carried to a cooler region of the body by circulating blood. Metabolic heat generation rates are assigned to the various regions in a manner that is appropriate for the level and kind of exercise being performed. Heat generation in the basal state is greatest in major organs, such as the liver and brain, and rather low in inactive muscle, bone, fat, and skin. However, during exercise the rate of metabolic heat generation in the active muscle mass increases several fold. Since the model permits one to specify the rate of heat generation in each region of each element, it is possible to analyze temperature changes that accompany exercise involving different muscle groups. Heat generated by shivering is treated in an analogous manner.

Since transport of heat by circulating blood is such an important factor in thermal physiology, the cardiovascular system is represented in a rather detailed manner. Each geometric element contains three vascular components -- an arterial pool representing large arteries, a venous pool representing large veins, and the capillary beds. The density of each vascular component varies with radial position, and the flow rate of blood through each component varies as the metabolic rate and temperature change.

Exchange of heat between blood and neighboring tissue is possible for each component. In the case of capillary exchange, it is assumed that blood enters

the capillaries with the temperature of arterial blood which supplies the region and leaves with the local tissue temperature. Arterial and venous exchange are defined in terms of heat transfer coefficients which reflect the density of arteries and veins in the region.

The rate of heat transfer through the respiratory tract is determined by the temperature, humidity, density and specific heat of inspired gas and the ventilation rate. Expired gas is assumed to have the temperature,

$$T_{ex} = 0.32 T_{in} + 24.0 \quad (2)$$

and to be saturated with water [7]. It is also assumed that the walls of the upper respiratory tract are well perfused and, therefore, heat transferred through that pathway is either provided or received by arterial and venous pools in the head and thorax.

Coupling between an individual and the environment is defined in terms of boundary conditions for the model. Heat transfer at external surfaces can occur by convection, radiation and evaporation. The thermal flux owing to convection, q_c , is expressed as

$$q_c = h(T_s - T_{env}) \quad (3)$$

in which h = a heat transfer coefficient that depends on the physical properties and velocity of the environmental fluid, and

T_s and T_{env} = temperatures of the surface and environmental fluid, respectively.

Radiation is defined in terms of the Steffan Boltzmann equation,

$$q_r = \sigma \epsilon (T_s^4 - T_{wall}^4) \quad (4)$$

in which σ = Boltzmann constant

ϵ = emissivity, and

T_{wall} = mean wall temperature.

The rate of evaporation is determined by the rate at which moisture is carried to the surface, either as sensible or insensible perspiration. However, when the sweat rate exceeds the rate at which water can diffuse away from the surface, moisture accumulates on the skin.

Given an adequate physical description of the human thermal system, one must incorporate into the model the following physiological considerations.

1. Local perfusion rate depends on the local metabolic rate and thermal state of the subject.
2. Heat stress of sufficient intensity causes vasodilation in the skin and sweating.
3. Cold stress of sufficient intensity causes vasoconstriction and shivering.
4. Venous return from peripheral regions is directed through superficial or deep veins depending on the thermal state; this controls the amount of countercurrent heat exchange between large arteries and veins [8].
5. The ventilation rate depends on P_{O_2} and P_{CO_2} at peripheral and central receptors, temperature, and intensity of exercise.

With the exception of the control equations for shivering, which will be discussed in considerable detail, the various control equations will be discussed only briefly. Thermal receptors located in the skin and in the hypothalamus generate afferent signals which determine the cutaneous perfusion rate, sweat rate, and shivering rate. In addition, humoral factors exert an influence on the perfusion rate in muscle and on the ventilation rate. Therefore, concentrations of key chemical constituents are computed also.

As the metabolic rate changes in various regions, the demand for oxygen

changes accordingly. This is taken into consideration in the model by including oxygen, carbon dioxide, and lactic acid balances in the set of equations which describe the system. Local perfusion rates in muscle are continuously adjusted to maintain the end-capillary oxygen tensions within the physiologic range. However, the response to increased metabolic demand for oxygen is not instantaneous, and a switch from the aerobic to anaerobic metabolic cycle may be necessary during the early phase of vigorous exercise. The resulting production of lactic acid, which changes the pH of blood, affects both the oxyhemoglobin equilibrium relationship and the ventilation rate [9].

When physiological variables can be measured directly, as is the case for metabolism owing to shivering, sweat rate, and ventilation rate, correlations based on experimental data are generally available. Unfortunately, correlations proposed by different investigators do not always agree with each other, and one must evaluate the available studies carefully.

Our most complete evaluation to date has been conducted for shivering data and correlations published by 5 investigators [10-14]. The results of this evaluation can be summarized as follows: (1) Although a given model might describe a specific set of data quite well, it often fails when tested against data taken under different conditions. (2) Predictive equations based on mild exposures and low metabolic rates owing to shivering are generally incapable of describing severe exposures. (3) There is definitely a dynamic component in the shivering response [15,16,17]; this is illustrated most clearly by the sharp increase in metabolic rate that occurs during the first five minutes of immersion in cold water [10,11,18].

When it became apparent that there were serious defects in all of the proposed control equations for shivering, a new model was developed. This model

can be described either as a first-order dynamic model or as a fading memory model, depending on which formulation the reader prefers, they are completely equivalent, except for a slight difference in behavior during periods of rapidly diminishing cold stress.

The first formulation is defined by the following equations:

$$M = S_1 + S_2 + S_3 \quad (5)$$

in which S_1 = contribution owing to $\dot{\bar{T}}_s$

S_2 = contribution owing to reduced T_c and $\bar{T}_s$, and

S_3 = contribution owing to $\dot{T}_c$.

The individual contributions are defined below.

$$\begin{aligned} \dot{S}_1 &= A_1(-\dot{\bar{T}}_s - 1.5) - \beta S_1 && \text{for } -3.5 < \dot{\bar{T}}_s < -1.5 \\ &= -\beta S_1 && \text{for } -1.5 < \dot{\bar{T}}_s \\ &= A_2(2.0) - \beta S_1 && \text{for } \dot{\bar{T}}_s < -3.5 \end{aligned} \quad (6)$$

According to this model, there is a definite range of $\dot{\bar{T}}_s$ in which the control of shivering is sensitive to $\dot{\bar{T}}_s$. If $\bar{T}_s$ does not decrease at a rate greater than 1.5°C/min, S_1 does not increase. On the other hand, as the rate of cooling increases, the ability to discern a difference in rate decreases, and we assume that the maximum stimulus is obtained for a rate of cooling greater than 3.5°C/min. The manner in which S_1 increases to a maximum value and then decreases again to zero when the surface temperature decreases exponentially from an initial value of 33°C to a final value of 13°C is shown in Fig. 1. This response agrees reasonably well with the observations of Hayward, Timbal and others who have measured oxygen consumption rates during an initial period of cold immersion.

The second factor is defined as follows

$$\dot{S}_2 = B'[F_2(T_c, \bar{T}_s) - S_2] \quad (7)$$

in which F_2 = an integrated stimulus owing to decrease of both T_c and $\bar{T}_s$.

For prolonged exposure, S_2 determines the shivering rate and becomes approximately equal to F_2 when F_2 changes slowly. When $35.0 < T_c < 36.8$,

$$F_2 = [A_2(T_c - 41.0)(\bar{T}_s - 41.8) - A_0] G \quad (8)$$

Aside from the multiplicative factor, G , which will be discussed later, F_2 is equal to Hayward's expression for the rate of shivering in this range of T_c . When T_c and T_s are such that $F_2 < 0$, we set $F_2 = 0$. Note that as T_c increases, F_2 decreases for a given value of $\bar{T}_s$. Comparison of computed and measured metabolic rates for a number of cases suggests that the factor $A_2(41.0 - T_c)$ should not decrease indefinitely as T_c increases, but should be bounded below by the value $A_2(41.0 - 36.8)$. Accordingly, when $36.8 < T_c$,

$$F_2 = [A_2(-4.2)(\bar{T}_s - 41.8)] G \quad (9)$$

Hayward did not limit the factor $A_2(T_c - 41.0)$, as we have done.

One of the striking features of Hayward's observations was the fact that shivering continued at nearly maximal rate for approximately 8 minutes after rewarming was initiated. Then M began an exponential decay toward zero with a time constant of approximately 3 min. This feature was incorporated into the model by specifying that $\dot{S}_2$ could have negative values only when $F_2 \geq 0$, or $-0.1 < \dot{T}_c$; otherwise, $\dot{S}_2 \geq 0$. Hence, during an interval when T_c and $\bar{T}_s$ are both decreasing, F_2 increases and S_2 follows; but during rewarming, the model does not permit S_2 to decrease as long as F_2 is positive and T_c continues to decrease. As soon as the integrated stimulus vanishes or the central temperature begins to increase, S_2 begins to decrease exponentially with a time constant, $1/\beta'$.

The third term, S_3 , accounts for the effect of decreasing T_c on M , as follows.

$$\begin{aligned} S_3 &= -c(\dot{T}_c + 0.01) \text{ when } \dot{T}_c \leq -0.01 \text{ c/min} \\ &= 0 \quad \text{otherwise.} \end{aligned} \quad (10)$$

Although other models ignore this contribution, there is ample experimental evidence to justify its inclusion [12,15,16]. Indeed, it appears that the increase in M which Nadel, et al [12] observed when lightly shivering subjects ingested ice cream should have been attributed primarily to the rate of change of T_c , rather than to the decrease in T_c .

The factor, G , which appears in the integrated stimulus term, F_2 , was added when it was observed that metabolic rates continued to increase during prolonged exposure to cold even though T_c and $\bar{T}_s$ were relatively stable. Hence, G , defined as follows

$$G = 0.6 + 0.4 (t/t_f) \quad (11)$$

in which t_f = length of exposure, was included in F_2 . Although this term has not been related to lowering of a particular temperature or combination of temperatures, it could represent the contribution of thermal receptors other than those in the hypothalamus and skin; others have presented evidence to support the existence of such receptors. This factor was incorporated into the model by relating G to the change of stored heat content, which can continue to decrease even after T_c and $\bar{T}_s$ have become relatively stable.

Two other factors which limit one's capacity for generating heat by shivering during prolonged exposure to severe cold were incorporated into G . The first is a factor which decreases linearly from unity to zero as the central temperature decreases from 35°C to 33°C. Although the experimental data base which supports this factor is small, several authors have stated that such a decrease does occur [19,20].

The second factor accounts for fatigue which occurs when the duration and intensity of shivering exceed one's ability to sustain the activity. Although fatigue is a complex phenomenon, the dominant factor seems to be depletion of glycogen in active muscles [21,22,23]. Since the number of moles of ATP produced per mole of glycogen is much smaller for anaerobic metabolism than for aerobic

metabolism, glycogen stores are depleted rapidly when $\dot{V}_{O_2}$ exceeds 50 percent of $\dot{V}_{O_{2,max}}$ and the endurance time for such activities is rather short. By postulating a relationship between the rate of glycogen consumption and work load, one can develop a theoretical equation for endurance time.

Gleser and Vogel [22] assumed that the rate of anaerobic metabolism is related exponentially to the work load; for steady work their assumption yields the following relationship between endurance time, t_f , and load, L .

$$\ln(t_f) = B - AL \quad (12)$$

in which A and B are constants. They claim that Eqn. 12 fits their experimental data better than the "general hyperbolic" equation,

$$\ln(t_f) = A \ln(L) + B \quad (13)$$

which was proposed earlier by Grosse - Lordemann and Muller [24]. The conditions studied by Gleser and Vogel involved work loads for which $\dot{V}_{O_2}$ exceeded 50 percent of $\dot{V}_{O_{2,max}}$, and endurance times were less than 5 hours.

Although there were considerable differences between subjects in Gleser and Vogel's study, they observed that these differences could be reduced greatly by correlating endurance time with relative work load, $L/\dot{V}_{O_{2,max}}$, rather than with absolute work load, L . This concept is reinforced by the studies of Petrofsky and Lind [25,26] who observed that fatigue resulted from one to four hours of lifting at a rate which required at least 50 percent of the maximum $\dot{V}_{O_2}$ for the specific lifting task. They reported that maximum $\dot{V}_{O_2}$ for lifting varied from 2.0 to 3.0 l/min, depending on the weight lifted, although $\dot{V}_{O_{2,max}}$ for these subjects averaged 3.7 l/min while bicycling. Hence, they also concluded that relative work load is a better determinant of endurance time than absolute load.

Fatigue owing to shivering generally involves two factors not present in the preceding studies. One is that the metabolic rate owing to shivering is not

constant, but tends to increase with time of exposure. Therefore, Eqn. 12 is not directly applicable to shivering. The other factor is that shivering metabolic rates are relatively low; a reasonable maximum value of $\dot{V}_{O_2}$ for shivering is $1.4 \text{ l}_{O_2}/\text{min}$, which is less than one-half $\dot{V}_{O_2, \text{max}}$ for a physically fit individual.

Even though the metabolic rate for shivering is relatively low, it cannot be maintained indefinitely. Beckman and Reeves [27] observed in a 1965 study that 24 male Navy personnel immersed to the neck in 75°F (23.9°C) water had to leave the water within 12 hours, although only 2 left because of excessive decrease in core temperature. Two others left the water because of nausea and headache owing to hypoglycemia, and 13 left because of severe persistent muscle cramps. Beckman and Reeves commented that,

"The implications of such tonic spasms are far-reaching in terms of survival. The pain was intense and incapacitating. Furthermore, spasms of a large group of muscles limit the body's ability to generate heat. The occurrence of muscle cramps would therefore signify the limit of thermal balance and the onset of a critical heat loss and drop in body temperature."

Colin and Houdas [28] also observed that in some cases man's ability to tolerate immersion in cold water may be limited by fatigue, rather than by loss of body heat.

Since several experimental studies implicate fatigue as the factor which determines survival time for certain kinds of cold exposure, it was deemed necessary to develop an endurance equation which is applicable to shivering. This was done by attempting to relate endurance times for various activities, including bicycling, lifting, and shivering, to a relative work load, M_r , defined as follows:

$$M_r = (\dot{V}_{O_2} - \dot{V}_{O_2, r}) / (\dot{V}_{O_2, \text{max}} - \dot{V}_{O_2, r}) \quad (14)$$

in which $\dot{V}_{O_2, \text{max}}$ = maximum $\dot{V}_{O_2}$ for the activity, and $\dot{V}_{O_2, r}$ = resting $\dot{V}_{O_2}$.

The relative load represents the ratio of the metabolic rate in working muscle to the maximum metabolic rate in those muscles which are active in the specific exercise.

As a starting point, one can use the model proposed by Gleser and Vogel for metabolic rates in the range of 50 to 110 percent of $\dot{V}_{O_2, \max}$ (endurance times from 3 to 300 minutes). Unfortunately, this equation does not have the proper form for extrapolation to relatively small metabolic rates because the endurance time does not approach infinity as the load approaches zero. An alternative formulation which has the desired property in the limit of vanishing load can be constructed by combining the model of Gleser and Vogel with the model of Grosse - Lordemann and Muller to obtain

$$\ln (t_f) = B - A_1 L_r + A_2 \ln (L_r) \quad (15)$$

When the data of Gleser and Vogel [22] are fitted with an equation of the above form ($A_2 = -1$), the resulting equation yields values that differ by only 2 percent from those computed using Gleser and Vogel's equation.

As Gleser and Vogel noted [22], endurance equations of the above form have a physical interpretation. If fatigue occurs when the glycogen concentration in active muscle falls below a certain level, one can obtain an endurance equation from the glycogen balance. Let R = rate of depletion of glycogen per unit mass of muscle and G = glycogen concentration; then

$$\frac{dG}{dt} = -R \quad (16)$$

Gleser and Vogel assumed that

$$R = K_1 e^{A_1 L_r} \quad (17)$$

a form which does not approach zero as L_r approaches zero. However, the expression

$$R = K_2 L_r e^{A_2 L_r} \quad (18)$$

does have the correct behavior for small work loads.

If the initial glycogen concentration is G_0 , the concentration after a period of exercise, t , will be

$$G = G_0 - \int_0^t R \, dt \quad (19)$$

The endurance time, t_f , is the value of t for which $G = 0$ in Eqn. 19.

For the special case of steady work, R is constant, and

$$G = G_0 - Rt \quad (20)$$

Hence,

$$t_f = G_0/R \quad (21)$$

Substituting the rate expressions given in Eqns. 17 and 18 into Eqn. 21, one obtains

$$t_f = (G_0/K) e^{-A_1 L_r} \quad (22)$$

and

$$t_f = (G_0/K) e^{-A_2 L_r / L_r} \quad (23)$$

respectively. These equations are equivalent to Eqns. 12 and 15. Implicit in this analysis is the assumption that glycogen stores in active muscle are not replenished during the exercise period; this is consistent with the fact that there is no known enzyme present in muscle which can mobilize glycogen by transforming it into glucose [21].

When the load, L_r , varies with time, the endurance time must be evaluated implicitly using the equation

$$G_0/K_2 = \int_0^{t_f} L_r e^{A_2 L_r} \, dt \quad (24)$$

Two parameters, (G_0/K_2) and A_2 , must be specified in the above formulation. Experimental data from the papers mentioned previously were used to estimate values for these parameters. Table I summarizes these data, which are plotted

in Fig. 2. Also shown in Fig. 2 is a curve (+ - +) based on data reported for weeks 3 through 10 of Gleser's study. After determining the regression line for the equation

$$\ln (t_f L_r) = A + BL_r \quad , \quad (25)$$

L_r was linearly related to M_r using work loads which produced 75% of $\dot{V}_{O_2, \max}$; the resulting equation is

$$t_f = 41.47 e^{-6.44 M_r / M_r} \quad (26)$$

Table 1. Published Endurance Times for Various Tasks.

Exercise	Ref.	$\dot{V}_{O_2}$ (l/min)	$\dot{V}_{O_2, \max}$	M_r	t_f hours
Running	29	3.08	3.63	0.83	0.96
"		2.20	2.53	0.85	0.85
"		2.22	2.79	0.77	1.36
Bicycling	30	2.6	3.4	0.75	1.50
"		2.2	2.8	0.74	1.42
Lifting	26	1.4	2.5	0.48	4.00
"		1.8	2.5	0.66	1.00
Shivering	31	0.6	1.4	0.27	0
Shivering	27	0.72	1.4	0.38	10.
Bicycling	22	2.1	3.1	0.65	1.8

It can be seen in Fig. 2 that Eqn. 26 predicts somewhat shorter endurance times than others have observed. Although it is known that training tends to increase an individual's endurance time, this does not appear to offer an

explanation for the relatively short endurance times of Gleser's subjects because they were put through a moderate physical training program before the endurance experiments were performed. Even though one should probably assign a high weight to Gleser's data, it is felt that a relationship which predicts longer endurance times would be more appropriate for use in the model. Two factors enter into this judgment; one is that fatigue may not result as rapidly from near maximal shivering as from other more vigorous forms of exercise, and the other is that when using the model to predict survival times, it is probably best to err on the side of times that are too long. Accordingly, the following equation was used to define the endurance times for steady exercise.

$$t_f = 18 e^{-4 M_r / M_r} \quad (27)$$

The graph of this function appears as the solid line in Fig. 2.

When work load varies with time, the endurance times are predicted by Eqn. 24, which becomes

$$18 = \int_0^{t_f} M_r e^{4M_r} dt \quad (28)$$

Equation 28 is the form used in the computer model.

Hayward's model and the proposed model were both compared with published experimental data for nonfatiguing exposures. In general, it can be said that both models do an acceptable job of predicting quasi-steady-state metabolic rates owing to shivering. Although the definition of acceptable accuracy depends on the use being made of the model, a reasonable criterion is that the error in total metabolic rate should not exceed ten percent of the total rate. Shown in Figs. 3 and 4 are measured and predicted metabolic rates owing to shivering. Also shown are broken lines between which the error in predicted value is no greater than ten percent of the total metabolic rate for a resting subject. The data shown in these figures establish that both models yield acceptable values.

with the modified version yielding slightly better accuracy than Hayward's original equation. The largest differences between measured and calculated values occur for subjects exposed to air. Paradoxically, in one case (Timbal, $T_{\text{air}} = 15^{\circ}\text{C}$), predicted metabolic rates are larger than the measured values, and in the other case (Raven, $T_{\text{air}} = 5^{\circ}\text{C}$), the predicted values are smaller. It is not apparent how the model could be changed to reconcile the differences in both cases.

The principal difference between the new model and Hayward's model is the addition of terms which account for changing skin and central temperatures. It appears that the new model describes quite well the transient burst of shivering which occurs during the first five minutes of severe cold exposure. The new model also accounts for the persistent shivering which Hayward observed during the rewarming period of his experiments. In addition, it predicts the transient burst of shivering that accompanies a rapid decrease in T_c in a lightly shivering subject. Although agreement between measured and predicted metabolic rates for Nadel's experiment [12] is not as good as one might have hoped, it is not unreasonable when one considers the difficulty of making such measurements. The computed five-minute average rate of 1.5 Kcal/min compares favorably with the value, 2.1 Kcal/min, obtained from Nadel's paper.

In addition to having valid control equations for shivering, a useful model must treat vasomotor responses with good accuracy. It is necessary to consider two aspects of vascular response. One is vasoconstriction which sharply reduces the perfusion rate in peripheral regions, and the other is venomotor action which shifts venous return in the extremities from superficial vessels near the skin to deeper veins which lie adjacent to arteries. This redirection of venous return brings into effect countercurrent heat exchange between arterial and venous streams, thereby reducing the temperature of arterial blood before it enters the capillaries of cool distal tissues and simultaneously rewarming venous blood

before it returns to the heart [8].

Although there is little doubt about the occurrence of such responses, they have not been evaluated experimentally with good precision. However, it is generally agreed that thermal receptors in both the skin and hypothalamus affect vasomotor action. In the computer model, equal weight is given to the decrease in central temperature and the decrease in mean skin temperature (evaluated with allowance for regional variation in the density of cutaneous thermal receptors). Furthermore, it is assumed that vasomotor action occurs promptly in response to imposed thermal stress and that near maximal vasoconstriction can be induced by exposure to relatively mild cold stress. Since even severe cold stress by itself does not result in lactic acid accumulation owing to insufficient blood supply to support metabolism, local humoral factors are allowed to moderate vasoconstrictive drives which would result in excessive anaerobic metabolism. When vasoconstriction becomes very strong, an increased fraction of the cardiac output is directed to the abdominal region in order to maintain sufficient total blood flow to prevent unrealistic values of P_{O_2} and P_{CO_2} . This aspect of the model is supported by the experimental observations of Raven, et al [32], although alternative mechanisms for maintaining cardiac output have been proposed.

Validation of the model rests on satisfactory agreement between computed and measured responses for conditions which involve exposure to environmental cold. A comparison of computed central and mean skin temperatures with experimental data obtained from divers wearing a new dry suit under development at the Naval Coastal Systems Center in Panama City is included in Lew Nuckol's paper [33]. Results from a more extensive evaluation of the model's capability for predicting thermal response during immersion in cold water are presented by Dr. Nunneley [34]. This paper also illustrates use of the model to predict

survival times for accidental cold immersion.

The purpose of this brief discussion of the model has been to present the most important physical and physiological features of a mathematical model which has been developed to analyze thermal responses during diving. A more complete discussion of the equations and the method of solution is available in other papers [35,36]. Application of the model for predicting survival times for divers who are trapped in an isolated personnel transfer capsule is discussed in another paper presented at the workshop [37].

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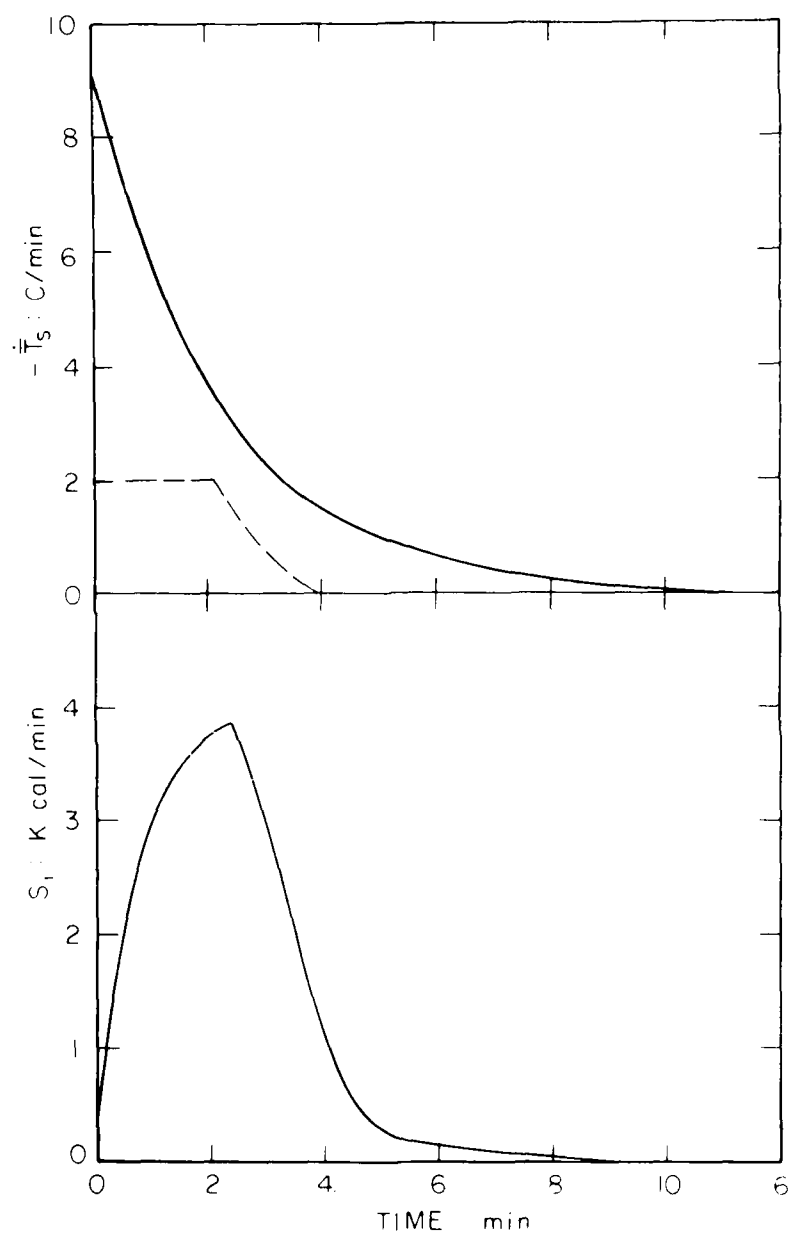


Figure 1. Variation of S_1 when the surface temperature decreases exponentially from an initial value to a final value.

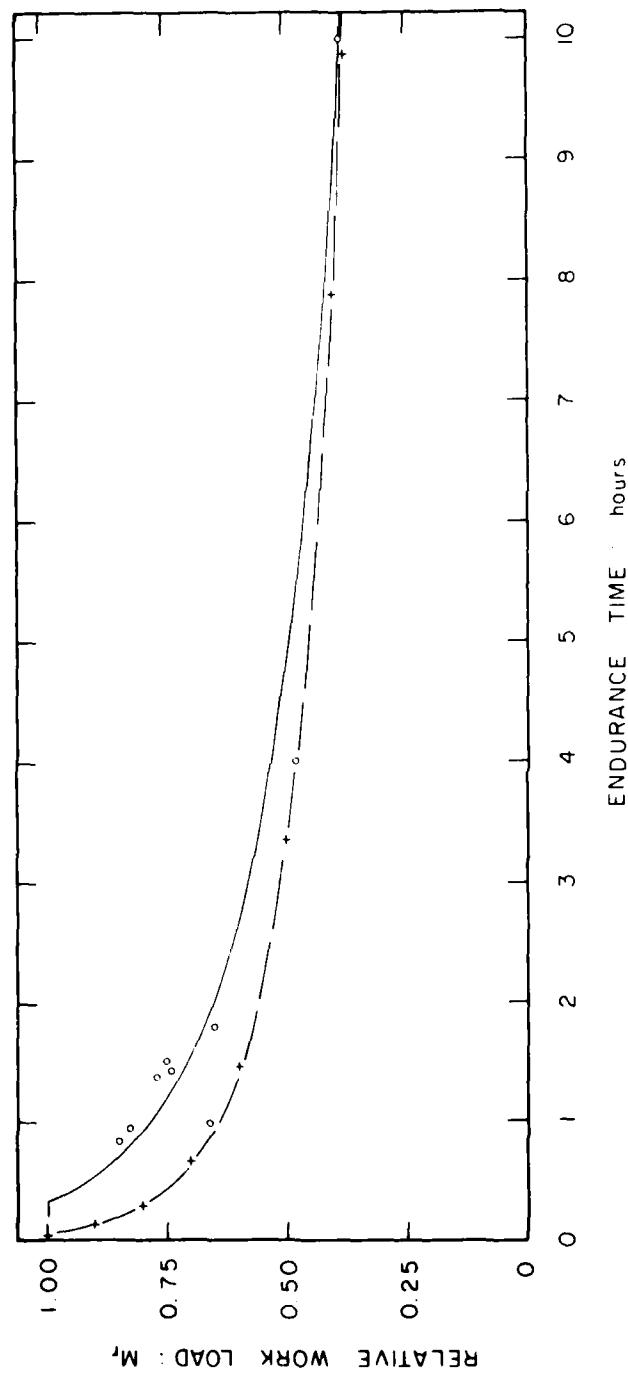


Figure 2. Relationship between relative work load and endurance time. The solid line shows the correlation used in the model (Egn.27) and the dashed line is a correlation based on Gleser's data[22].

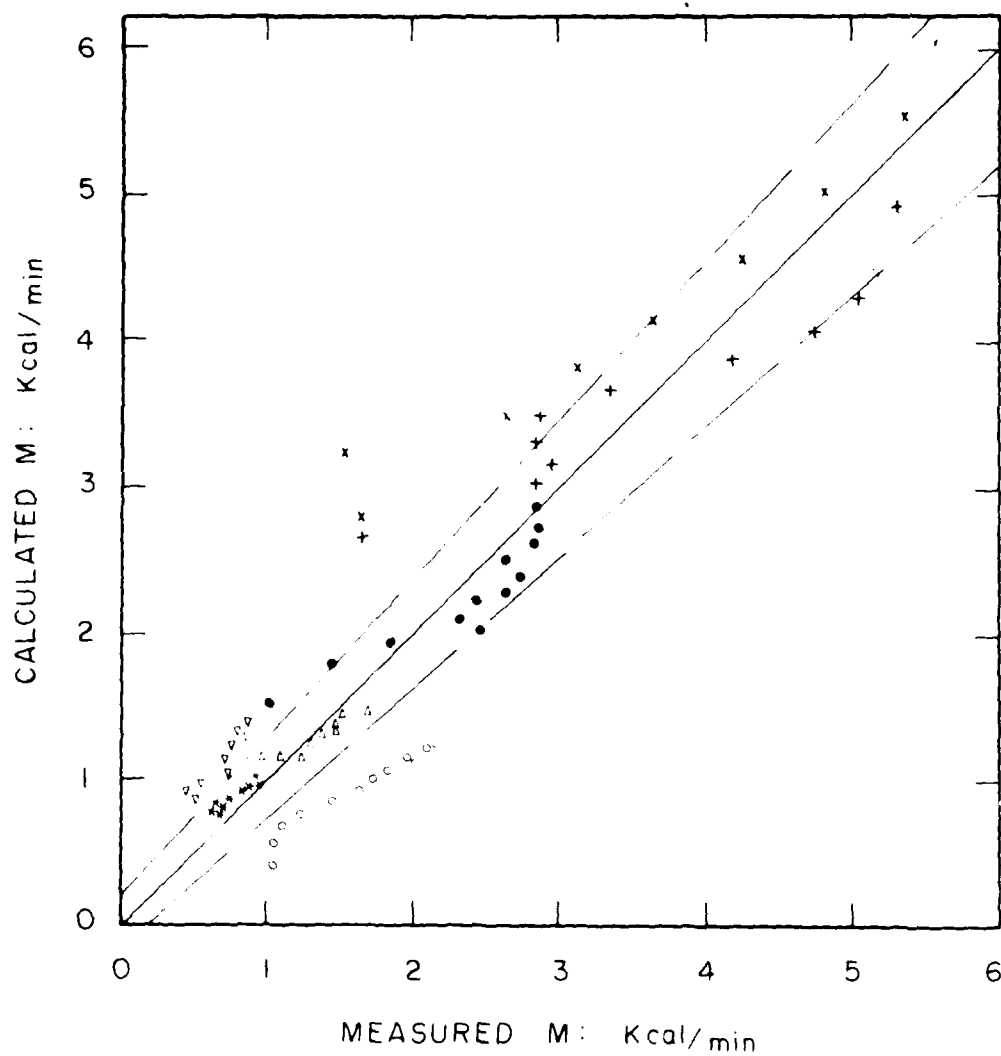


Figure a. Comparison between metabolic rates computed using Hayward's correlation and corresponding measured values reported by the following investigations:
 X - Hayward, Fig. 1 [10]; * - Timbal, 28°C water;
 + - Hayward, Fig. 2 [10]; ∇ - Timbal, 15°C air;
 Δ - Timbal, 28°C water [11]; • - Rochelle [14];
 o - Raven [13].

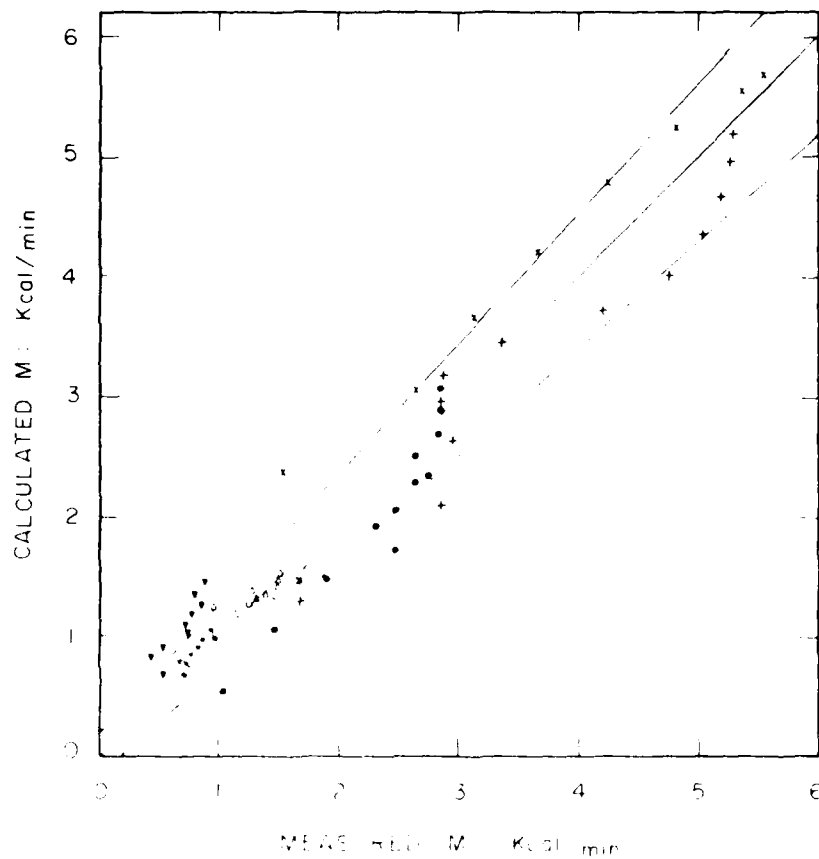


Figure 4. Comparison between metabolic rates computed using the new model and corresponding measured values. Symbols have the same meaning as in Fig. 3.

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IMMERSION HYPOTHERMIA:

VALIDATION OF COMPUTER MODEL AND PREDICTION OF COOLING RATES

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ABSTRACT

Protective clothing for persons immersed in cold water is usually evaluated with human subjects. Such experiments are necessarily few in number and limited in scope; a computer model can assist in clothing design and evaluation while minimizing the need for human studies. We describe here validation of the Wissler model against experimental data for water temperatures of 5-18°C and subjects who were nude, lightly clothed, or wearing anti-exposure suits. Nude subjects provide the severest test because of the large thermal gradients and high rates of temperature change. The model performed reasonably well on these short-term experiments.

It was then used to produce graphs showing core temperature isotherms for given time-temperature conditions. Problem areas included 1) possible suppression of shivering due to hypothermia and/or fatigue, and 2) selection of exposure limits in terms of acceptable core temperatures.

INTRODUCTION

Provision of adequate thermal protection for men immersed in cold water is an important, continuing problem in diving, surface activities and military aviation. Since water is an excellent heat sink, substantial amounts of clothing insulation may be required to prevent progressive hypothermia during immersion. However, insulation which protects in water also creates unwanted warmth and awkward bulk in an air environment; it is therefore desirable to wear the minimum clothing required to provide the necessary protection. The problem is complex because heat loss is a function of multiple factors involving environment, clothing, and physiology.

The effectiveness of immersion ensembles is generally evaluated using human volunteers, but such experiments are necessarily few in number and limited in scope. Subjects rarely represent the full range of characteristics of the user population. Ethical considerations limit experimental hypothermia, dictating end-points which tell little about real threats to survival. These factors handicap extrapolation of experimental results to emergency conditions. Furthermore, a small change in clothing or scenario can render the test data inapplicable.

Computer modelling now provides rational means of analyzing the complex interactions which govern heat loss. Current knowledge of human thermoregulation has been incorporated into several mathematical models (3,9,10,12,13). This paper describes the process of validating one such model and applying it to the assessment of clothing ensembles.

MODEL DESCRIPTION

The model was developed by Wissler and is described in greater detail in another paper at this workshop and elsewhere (12,13). The body is subdivided into 15 cylindrical elements (Fig. 1), each composed of radial layers with appropriate physical and physiological properties to simulate bone, muscle, viscera, fat and skin. The various elements and layers are linked by the circulatory system (Fig. 1), which includes a central thoracic pool and arterial flow through a sequence of elements; in each cylinder some blood is routed to the capillaries of various layers, while the remainder continues to the next element.* Venous return may be via deep or superficial veins. Heat is transmitted from warmer to cooler regions within the model by conduction between adjacent radial layers and by circulatory convection among layers and elements. Clothing is specified by adding radial layers over appropriate regions to represent various garments. Thermal status of the model is assessed from the following output variables: mean skin temperature ($\bar{T}_{sk}$), core temperature as represented by thoracic arterial temperature (T_{ar}), and metabolic rate ($\dot{M}$), which indicates shivering and/or exercise. (Fig. 2)

A crucial problem in modelling hypothermia is designation of realistic shivering levels. Heat production peaks at 4-5 times the resting level when $T_{re} \approx 35^{\circ}\text{C}$, but there is evidence that shivering diminishes with further cooling and disappears at $T_{re} \approx 28^{\circ}\text{C}$ (8). Boutelier supports this conclusion and adds that shivering may also be reduced by fatigue in moderate but prolonged exposures (2). Re-

* Fig. 1a

duction of shivering due to either factor would speed the development of life-threatening hypothermia, as illustrated in Fig. 2, which shows model T_{ar} both with and without shivering attenuation. Also shown is a line extrapolating T_{ar} from the early, moderate cooling phase ($T_{ar} = 35-37^{\circ}\text{C}$); such a linear extrapolation is too severe and invalid because it fails to allow for the decrease in outward heat flow as body temperature drops. All model runs described below incorporate diminished cooling and attenuated shivering at $T_{ar} < 35^{\circ}\text{C}$.

MODEL VALIDATION

The model has actually evolved over several years, and previous applications established its capacity to predict thermal exchange in divers wearing wet suits while working in cold water (report in preparation). The severe nature of the accidental immersions addressed here required additional validation. We therefore decided to test the model against experimental results for subjects who were either nude or wearing light, water-permeable clothing, as well as subjects wearing ventile anti-exposure flying ensembles.

Although the esophageal temperature at the level of the heart (T_{es}) provides the best available estimate for T_{ar} , temperatures are more often measured in the rectum (T_{re}) and auditory canal (T_{ac}). Both T_{re} and T_{ac} are subject to bias from direct effects of environmental extremes on local tissue temperature, but their rates of change during sustained cooling provide a good estimate for the rate of change of T_{ar} . Several reports from the literature were used to validate the model, as described below (references in parentheses):

1) Nude Subjects, $T_w = 10^\circ\text{C}$ (5)

The most rigorous test of the model's performance was provided by experiments involving nude immersion and rewarming of eight subjects. Figure 3 shows good agreement between the computed T_{ar} and measured core temperatures during the entire 60-minute cooling period, with slopes nearly parallel during the linear phase. Metabolic rate ($\dot{M}$) also shows good agreement between computed and measured values. Notable is the sharp initial peak in $\dot{M}$ associated with a burst of shivering upon water entry with the attendant sudden fall in T_{sk} . The $\dot{M}$ declines as T_{sk} approaches T_w and the rate-dependent afferent signal diminishes. Thereafter, progressive heat loss causes a compensatory increase in $\dot{M}$. The model shows some aberration during the first 10 minutes of rewarming, when its core temperature falls sharply due to a sudden increase in the perfusion of chilled peripheral regions. Since the level of shivering depends strongly on core temperature, computed $\dot{M}$ during early rewarming reflects the variations in T_{ar} . Although there is room for improvement in the simulation of vasomotor action during rapid rewarming, we believe that the overall performance of the model on this rather demanding test is quite satisfactory.

2) Light Clothing, $T_w = 5-18^\circ\text{C}$ (4)

In a study designed to provide information about survival times following boating accidents, Hayward measured T_{re} and $\dot{M}$ of 12 resting subjects who wore cotton shirts and pants and kapok life vests during immersion in water of 4.6, 10.5, and 18.2°C. Observa-

tions were also made on 12 subjects who swam in 10.5°C water. Since Hayward used both male and female subjects, the variance of the measured values must have been rather large. Furthermore, expired gas was conducted to the analyzer through a tube 30 m long, which may have damped transient changes to some degree. Despite these limitations, these results still provide a useful test of the model.

In simulating these experiments, the cotton garments were modelled by surrounding each element with a layer of still water which provided $R_T = 0.06$ clo. Measured and computed changes in T_{ar} and $\dot{M}$ are presented in Fig. 4. There is reasonable agreement between predicted and observed values. As before, the slope of T_{ar} parallels that of T_{re} during the last 20 minutes of immersion. Both the computed and measured $\dot{M}$ exhibit the transient peak which is typical of sudden immersion in cold water. During the last half of the immersion period, the computed $\dot{M}$ is approximately 25% larger than the corresponding measured values. Hayward noted that the $\dot{M}$'s were approximately 25% below Keatinge's results under similar conditions (7); therefore, the model output appears quite reasonable. Finally, it is interesting to see that at $T_w = 10.5^\circ\text{C}$ the swimming subjects cooled more rapidly than still subjects, a result which agrees with the observations of Keatinge (7).

3) Ventile Suits in Sea, $T_w = 10-12^\circ\text{C}$ (6)

Hayward et al also studied a variety of immersion suits, including two ventile garments, the Beaufort Mk 10 (2 layers) and the Hansen suit (single-layer), each worn over rather light "waffle-

weave" underwear. Twenty male subjects were studied in both suits and while wearing the light cotton garments described earlier. Immersions were in ocean water at 10-12°C and lasted until T_{re} fell 1.5-2.0°C, about two hours in suits and one hour in control clothing. Figure 5 shows computed and measured changes in core temperature; the model successfully reproduces the observed rate of cooling during the linear phase (last 80 minutes). Computed $\bar{T}_{sk}$ also agrees well with the single measured site, the subscapular area. It therefore appears that the assumed insulation value of 0.33 clo adequately represents these ventile ensembles. Furthermore, the model successfully simulated the effect of the control clothing for longer immersion and at a different T_w than for the earlier experiments.

4) Ventile Suits in Tank, $T_w = 0^\circ\text{C}$ (1)

The fourth set of data used to validate the model was published by Andrae, who studied subjects wearing a two-layer ventile garment over heavy underwear. Subjects underwent two experiments, continuous immersion at $T_w=0^\circ\text{C}$ and transient (5-minute) immersion followed by raft entry with exposure to air at $T_{db}=0^\circ\text{C}$ and $V=10$ m/s. Measurements included T_{re} and $\bar{T}_{sk}$, and exposures lasted 30-120 minutes, depending on the severity of the conditions. The ensemble was modelled as a dry suit with a thermal resistance of 1.2 clo in air and 0.7 clo in water.

Computed and measured core temperatures are presented in Fig. 6 for two cases, 1) continuous immersion, and 2) raft entry following 5 minutes in the water. In both cases, results obtained using the

model closely resemble actual observations. The model reproduces the transient rise in core temperature at time 0-20 minutes followed by a nearly linear decline as exposure continues. There is good agreement between the slopes of the computed and measured curves during the linear cooling period. The $\bar{T}_{sk}$ (not shown) also showed good agreement. The clothing ensembles studied by Andrae appear to provide twice the thermal insulation of the ventile suits studied by Hayward et al. The difference probably reflects the heavier underwear worn by the Swedish subjects. This conclusion is supported by the difference in $\Delta T(=\bar{T}_{sk}-T_w)$ at $t=60$ minutes: Andrae's ΔT was 30°C while Hayward et al reported 10°C (6).

Specifying only the environmental temperature, air or water speed, thickness and insulation value of the garment, and the subject's weight and metabolic rate owing to exercise, it is possible to reproduce with good accuracy the transient state profiles for T_{ar} , T_{sk} , and M , including the contribution from shivering. Although further testing is desirable to establish confidence limits for the model, we believe that the above-described results justify the applications which are presented in the following section.

INITIAL MODEL APPLICATION

Our eventual goal in applying the model to accidental immersion in cold water is the development of relatively simple instructional materials which will allow medical or life-support personnel to select clothing ensembles which are appropriate for given environmental conditions and durations of exposure. A major difficulty

is that of defining acceptable risks and correlating them with various levels of chilling. Since terminal events in severe hypothermia, such as impairment of consciousness and/or cardiac failure, appear to be directly related to core temperature, we selected T_{ar} as the variable for defining cold exposure limits. The serious medical consequences of deep hypothermia have been reviewed elsewhere (2,7). The subtler effects of mild to moderate hypothermia are of greater interest in approaching operational problems. We elected to study the following three exposure zones:

1) Safe, $T_{ar} < 36^{\circ}\text{C}$

This zone is thought clinically safe and preserves self-rescue efforts. It can be studied in the laboratory using human volunteers, and has been proposed as a criterion of protective clothing for divers working routinely in cold water (11).

2) Hazardous, $T_{ar} = 35-36^{\circ}\text{C}$

This zone is also safe and subject to laboratory study. This degree of hypothermia is thought to affect mental performance and limit self-help activities.

3) Probably fatal, $T_{ar} < 35^{\circ}\text{C}$

In this region, drowning becomes likely as consciousness fades. Shivering diminishes, and at $T_{ar} < 35^{\circ}\text{C}$, the risk of cardiac arrhythmia becomes significant. Since core temperatures below 35°C are not subject to experimental study, thermal events in this zone cannot be modelled with confidence. The small amount of available data will be discussed below.

Insulating properties of clothing ensembles are known for dry (air) environments, but immersed clo values represent estimates. Based on the validation runs described earlier, three sets of clothing properties were selected to represent typical aircrew ensembles: a) Lightweight, permeable clothing (e.g., ordinary flight coveralls): 0.06 clo in water, 1.0 clo in air; b) Ventile suit (1-2 layers) with single layer of light underwear: 0.33 clo in water, 1.0 clo in air; c) Ventile suit (1-2 layers) with heavy, thermal underwear: 0.70 clo in water, 1.2 clo in air. Two types of exposure were simulated for each ensemble, 1) continuous immersion and 2) raft entry after transient (5-minute) immersion.

The computer model was used to outline safe, hazardous, and fatal zones for $T_w = 0 - 20^\circ\text{C}$, and results for ensembles a and b appear in Figs. 7 and 8. These are only preliminary data, and much work is required before guidance can be issued to the field.

The designation of $T_{ar} \leq 35^\circ\text{C}$ as "probably fatal" can be checked against data from two sources, the criminal experiments conducted at Dachau and reports from shipwrecks. Molnar analyzed these data and developed a time-temperature curve representing "a limit of tolerance (survival) which probably few men can exceed and many cannot approach" (8); this appears as a dashed line in Fig. 7. Also indicated is the exposure of victims from the 1963 sinking of the "Lakon-ia;" of 200 persons afloat in life jackets on calm seas, only 87 were alive when picked up after 3 hours (2). Hayward's curve for

"incipient death" has been added for comparison (5). These data clearly support the contention that fatalities are possible in any exposure where T_{ar} falls below 35°C. All of the calculated isotherms for T_{ar} (Figs. 7 and 8) show a characteristic curve which is flat at low T_w 's (very rapid cooling) and then rises steeply at higher T_w 's. This inflection is clearest in subjects with least protection and becomes much less important in subjects wearing immersion suits or using rafts.

The alternative to use of a mathematical model such as the one discussed in this paper is to construct a multivariate statistical model based on a large number of experimental data, which are not now available nor are likely to become so. The type of model described here can be applied in several ways other than that shown. It allows assessment of the thermal effects of modifications in clothing, equipment or scenario, and may thus be used to minimize the need for studies on human subjects. It can assist in optimizing the design of laboratory experiments and field studies. The model could also be used in real time to help make rational decisions on termination of hazardous search and rescue efforts.

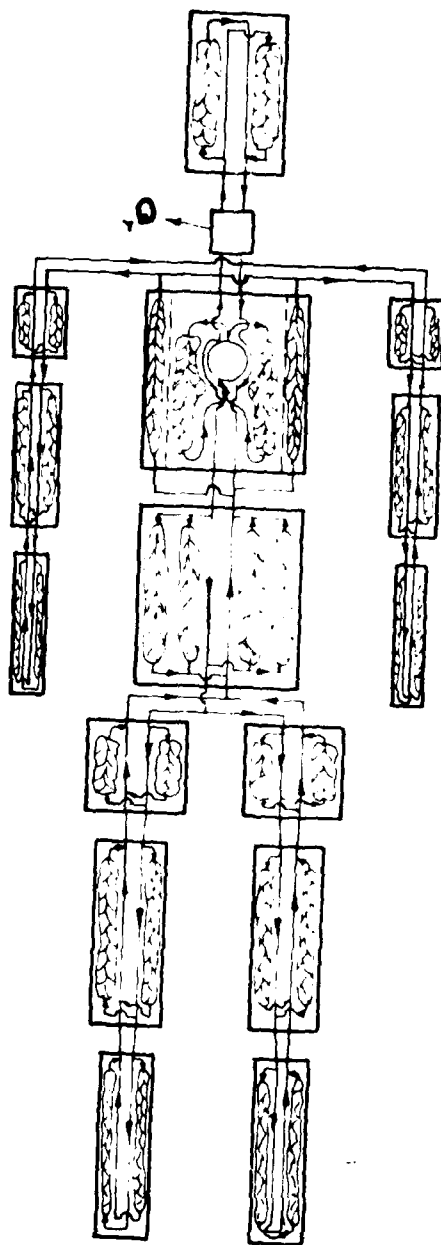
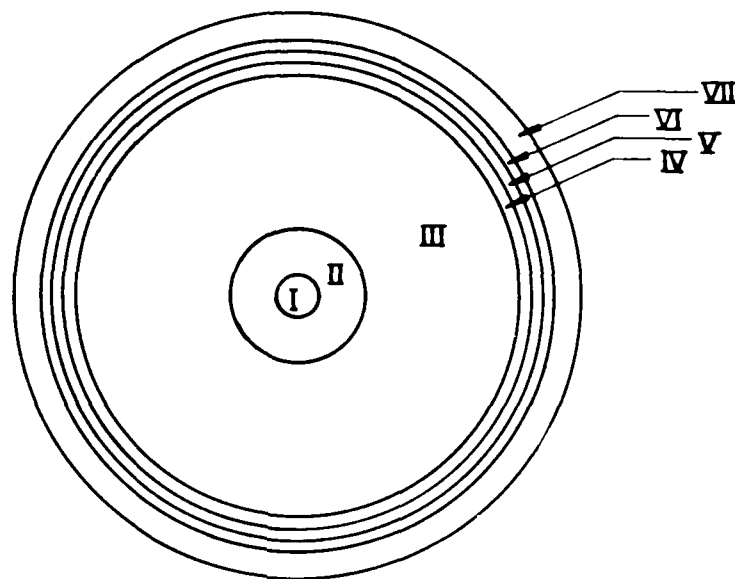


Figure 1. Schematic representation of the geometric arrangement of major elements connected by the circulatory system.



- I . LARGE ARTERIES AND VEINS
- II . TISSUE , SMALLER ARTERIES AND VEINS
- III . MUSCLE
- IV . SUBCUTANEOUS FAT AND SKIN
- V . LIQUID HEATED GARMENT
- VI . GAS SPACE
- VII . INSULATING OUTER GARMENT

Figure 1a. Typical arrangement of properly regions within a major element.

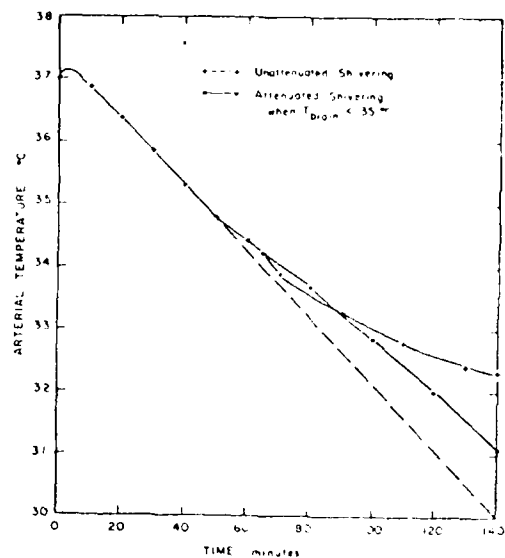
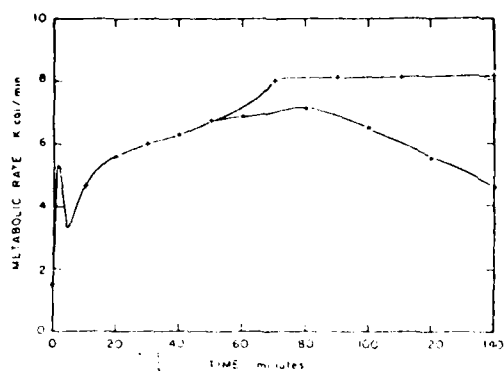


Fig. 2 Effect of shivering on computed T_{ar} . Subject wears permeable clothing (0.06 Cl_{18}) and $T_{\text{a}} = 5^\circ\text{C}$. Dashes show linear extrapolation from early phase.

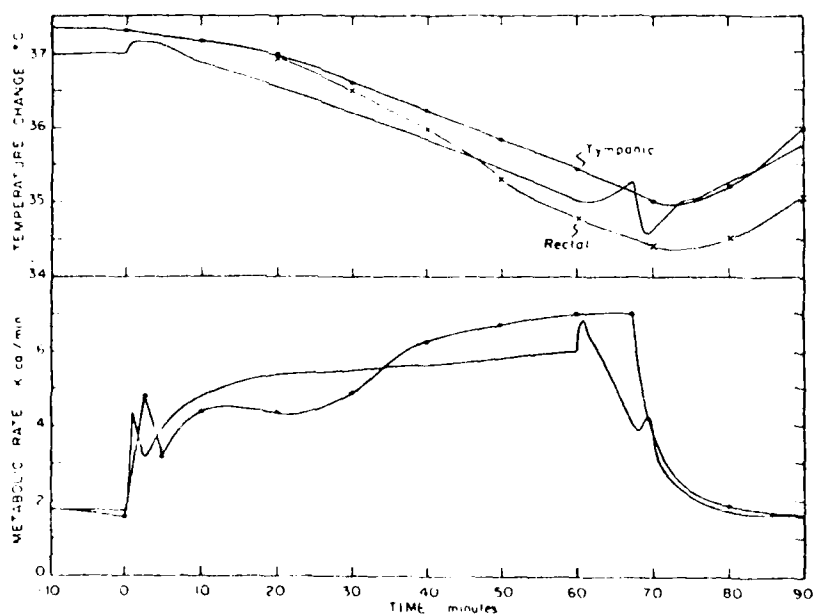


Fig. 3 Comparison of model output (continuous lines) with experimental data (dots and X's) from Hayward et al. (5). Subjects ($n=18$) were immersed nude at $T_{\text{a}} = 10^\circ\text{C}$ for 60 min, then were rewarmed at $T_{\text{a}} = 28-40^\circ\text{C}$.

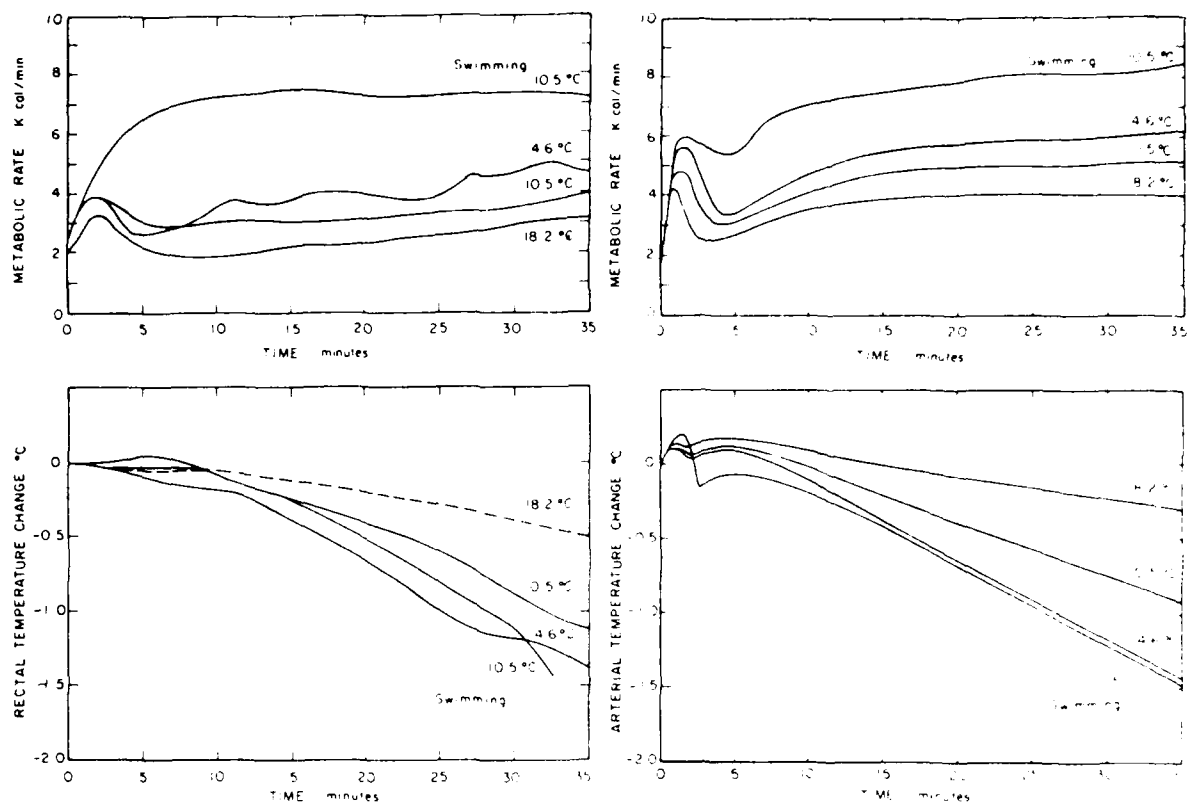


Fig. 4 Results of study by Hayward (4). Subjects (n=12) were immersed wearing cotton trousers and shirt at T_w shown. Subjects sat still except where indicated.

Computer simulation of study by Hayward et al. (4).

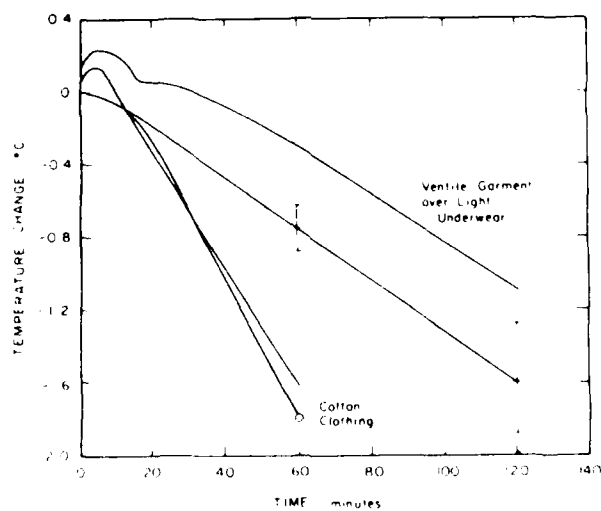


Fig. 5 Computed change in T_{re} and measured change in T_{re} ($\bar{X} \pm SD$ shown at 60 and 120 min) from a study of ventile immersion suits; controls wore cotton slacks and shirt (6). $N=20$, $T_w = 10-12^\circ\text{C}$.

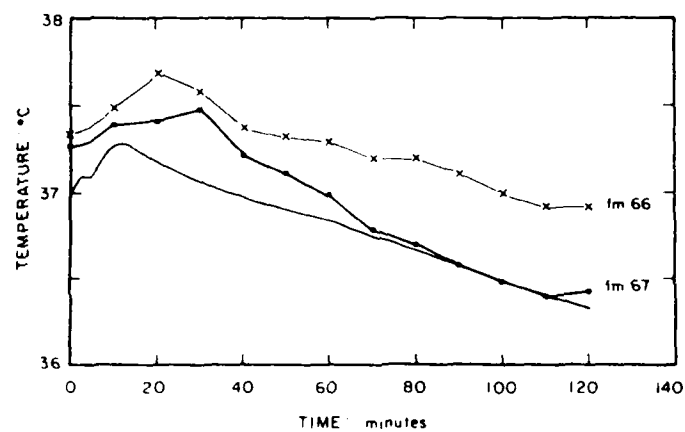
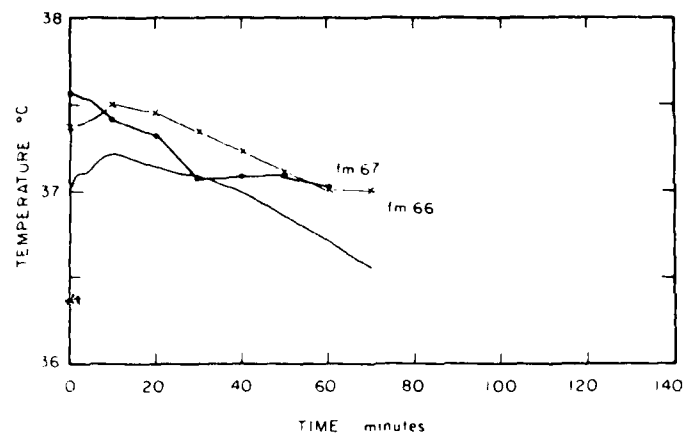


Fig. 6 Computer T_{re} (continuous line) and measured T_{re} (dots and Xs) from Andrae's study (1). Line "fm 66" includes 7 subjects wearing two-layer ventile suit (see text). Line "fm 67" (not modeled) includes 3 subjects in a heavier ventile suit. Both suits were worn over heavy thermal underwear. Upper section shows continuous immersion to the neck ($T_{db} = 11.8^{\circ}\text{C}$) while lower part shows transient immersion (5 min) followed by raft flotation ($T_{db} = 0^{\circ}\text{C}$).

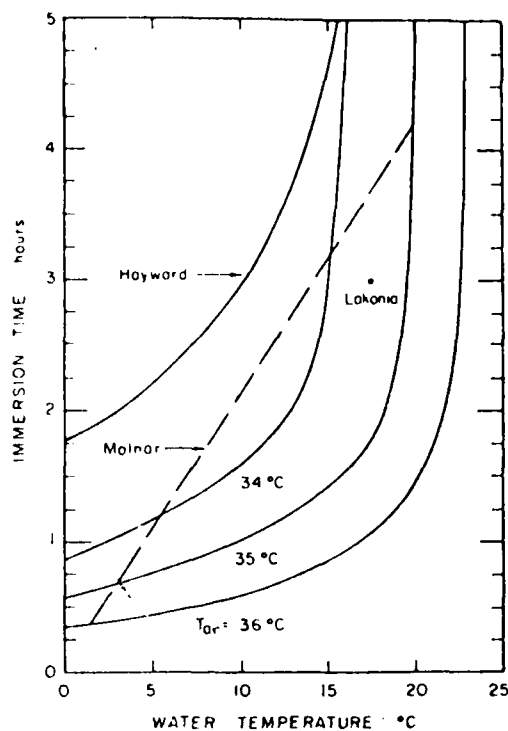


Fig. 7 Predicted immersion ensemble b, time to reach designated arterial temperature (T_{ar}) at various water temperatures. Subject is wearing light, water permeable clothing (ensemble a, see text). Dashed line represents survival limit found by Molnar (8). "Lakonia" point is from a 1963 sinking (see text) (2). "Hayward" curve shows his prediction of "incipient death" (5).

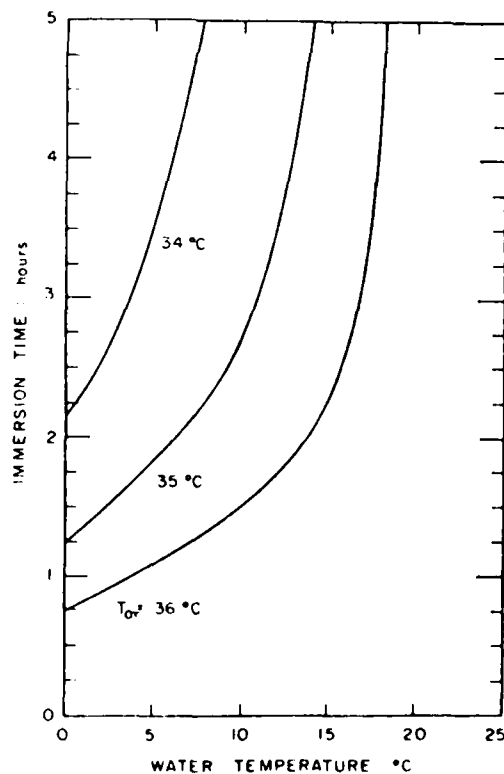


Fig. 8 Predicted immersion time to reach designated arterial temperature (T_{ar}) at various water temperatures. Clothing consists of a ventile suit with 1 layer of light underwear (see text).

Hamilton: Very early in the slides, you showed the difference between when you do and do not take into account the fact that the temperature gradient is less. It didn't seem to help very much. It seemed that you would get a more dramatic change in that heat loss curve.

Nunneley: It's still a relatively short-term exposure. There would be a more pronounced difference if you extended the analysis.

Hamilton: Body temperature is water temperature. There is no gradient after a while, at least no gradient between the skin and the water. The gradient is in the body.

Nunneley: There is still a big gradient between core temperature and water.

Wissler: In order to maintain a temperature difference, you have to transfer heat; and in this case, the heat is produced metabolically. When you stop producing heat, the gradient diminishes. It's like Ohm's Law, where the current corresponds to the rate of heat transfer and R corresponds to the resistance. If you turn the heat generator off, you also reduce the gradient.

Hayward: It's interesting that the question of the ongoing cooling rate in relation to possible fall-off in shivering at some point is where you can't usually measure it. In the US Coast Guard pamphlet on hypothermia and other texts on cooling rate, they generally say that when the shivering is impaired you will have an accelerated cooling rate. It's there in the general text books, which is wrong. When the shivering falls off, the cooling rate should approach lin-

earity, which your model shows. One slide which does go down experimentally below 35°C is this one out of the Dachau report which I use to argue for probable linearity of cooling rate. These are results from those people cooled during the Second World War. Where temperature is measured, you see this linearity in cooling, supporting with your model.

Nunnely: Do you think that the physical condition of these subjects might have affected their shivering ability?

Hayward: I don't know about their shivering, but their cooling rate was faster than those of normally-fed people with a little more fat. One of the reasons that early predictions of survival time seemed short in the cold was because the people involved were low in fat. My point is that this is experimental data that does fit with your results.

Golden: Where does this business about shivering rate disappearing below 35°C come from? It's what one would expect from a Q_{10} effect and if you look at the anaesthetic literature on hypothermia you get it with anaesthesia. So you find that most people quote Burton and Edholm, page 213. If you go there, you will find an ill-defined paragraph where they refer to metabolic activity waning below 35°C and they don't say where this came from. In that paragraph they quote Dill and Forbes and it sounds as if this was their source in 1941. This was data extracted from some work done by Talbot who was experimentally cooling patients in the treatment of malignancies and schizophrenics. Such cooling lasted 3 or 4 days at core temperatures

of 28°C. Sure enough, if this data is plotted from Dill and Forbes' paper, you obtain a curve that is eye-fitted to Table I. If you go to Table II, which contains blood-gas measurements, you find wide variations between expired oxygen and PaO_2 which indicates a tremendous diffusion problem, as would be expected in such patient treatment --being hypothermic and flat on your back would cause much A.V. shunting --I think that's where the concept of shivering declining below 35°C originated and it is erroneous in fact. Just to support that, if you look at people rescued from water whose temperatures are below 35°C, you find that they are shivering violently. I attended to a young lad with an auditory canal temperature measured with the zero gradient (Keatinge and Sloan, 1975) thermometer on the Y-axis against the time of day. This chap arrived in the emergency ward and I saw him about 15 minutes later. When I measured his temperature, it was 29.5°C and he seemed to be doing well so I left him alone in the bed, covered him with blankets, and he shivered violently. I could see the rise of temperature from 29.5°C without any active heating is about 6.0°C/hr. So maybe with very prolonged cooling, you find that fatigue sets in and shivering declines, and in acute exposures such as in immersion rescue, they may not be shivering because they are spastic from the cold.

Hayward: I agree with your observations and would like to add that I've never seen average people stop shivering in experimental hypothermia, even when below 35°C down to 32 or 33°C. I've been down there myself and kept shivering. I've always been reluctant to agree

about this slowing down of shivering below 35°C. I question this choice of temperature.

Nunneley: I've always wondered if it would be due to stiffness or muscular inability to shiver.

Pozos: The muscles go into tetany more than shiver.

Nunneley: How much heat is given off in that case?

Pozos: We will be doing these experiments, but you do get simultaneous motor unit contraction.

Hayward: In trunk musculature, which is much warmer than hands, they are still at a temperature at which they could continue shivering.

Wissler: One reason we emphasize that this is a feature of the model is that we realize that there is a great uncertainty about this, and we are looking for guidance and experimental data to improve the model. I've looked at 50 or so papers dealing with accidental hypothermia, and very few of the physicians who treat elderly or young people even comment on whether or not the victim was shivering. No one seems to measure oxygen consumption rate and it is very difficult to find good experimental data to provide guidance for this particular aspect of the problem, in spite of the fact that it is extremely important. We welcome any help that we can get from you.

Hayward: I understand that in one of the Scandinavian countries, Norway or Denmark, someone was cooling conscious humans who were shivering down to the low thirties °C. Do you know anything about that?

Pozos: What would your model predict about initial shivering, which

then stopped? We have a number of medical patients who do that. They are in cold water shivering for five minutes and then they stop shivering. There is some minor fasciculation, as Frank Golden was saying, and some kind of motor unit activity, but it is not coordinated and is not frank shiver. What would your model predict in that case?

Wissler: I'm going to duck that question. The problem is that shivering does two things. It increases the rate of heat generation and it also increases perfusion. One helps to maintain central temperature and the other hinders the maintenance of central temperature. This is similar to the case of swimming. It is not clear what is going to happen.

Nunneley: Unless you have some external insulation, shivering could be a counter-productive activity.

Wissler: If you look at some of Keatinge's cold immersion studies, you will see that his rather fat subjects laid in cold water and were happy for a long period of time, several hours. Finally they reached a threshold at which they began to shiver, and then he observed a rapid drop of core temperature. With those subjects, shivering was not helpful.

Schmidt: Are we saying that in some cases the cessation of shivering is due to hyperventilation and tetany? For instance, where hyperventilation is absent you maintain the shivering, but if you hyperventilate to the point of tetany, you lose your shiver?

Hayward: My experience with that is that it would not be the reason why some individuals stop shivering after a few minutes. As Bob

Pozos has pointed out, there is a large individual variation, particularly when there is no core drive for shivering metabolism. The data that Dr. Wissler has used is for the average person; well, we have people who have shivered at first more than what the data are in the equation and others who shivered very little. We are taking an average stance. It wouldn't predict well for those subjects who don't shiver. My colleague Eckerson is one of them. He won't shiver; for the first few minutes, yes, and then for the next 15-20 minutes in light clothing, no shivering, until he gets some core cooling and then he'll start to shiver. It seems that there is enough individual variation that this parameter would be hard to model.

Pozos: I'd like to back this up by telling you about our evaluation of some life jackets. We put one student in and he shivered for no more than five minutes and we kept the emg records on his body. No shivering. He repeated the trial two weeks later because we considered that this first result was due to fatigue. The same results were obtained. We've seen this in two subjects now. Shivering for the first five minutes and then no more.

Hayward: Until you get him colder! I've not met anyone that I can't make shiver, sooner or later.

Hayes: Question for Gene Wissler. Although all the models used to date use mean skin temperature calculated according to some area weighting scheme or other, it's a physical symbol for skin temperature that we put into this model. What do you think would happen if

we actually put in a physiological skin temperature? In other words, different areas exhibit different sensitivities to the same temperature. We always assume when we calculate a mean skin temperature that every area has the same sensitivity. What do you think would happen if we had a physiological weighting system for skin temperature and substituted that into your equation? What do you think would happen to the evaluation of shivering then?

Wissler: I'm not sure what happens to the relation. I think that this is something that should be taken into consideration but the necessary experimental data are not currently available. There is certainly a variation in the density of cold thermal receptors, and something that reflects this should be used as the input variable, rather than just physical skin temperature.

Hayes: Perhaps it would account for some of the anomalies with regard to shivering.

Wissler: I think one set of experimental observations that are pertinent to this subject are those made by Timbal, who measured shivering rates for persons who were either immersed in cool, but not cold, water or exposed to cold air. He observed that he had to use different correlations to represent the two sets of data. He was able to develop equations which represented each individual set of data rather well. As I recall, he had to add a term to the air equation to describe the water-immersion experiments. One explanation for that may be that mean skin temperature per se is not a good representation of the peripheral input.

Hayes: I was quite surprised to see that somebody had actually done it with sweating as a controlled output. You can think of shivering as a controlled output as well. I think that it was Nadel who had some sort of physiological weighting system for sweating and that seems to be very good at predicting sweat loss.

Kuehn: I'd like to put a question first of all to Gene Wissler and then Frank Golden. In modelling, which I have done, both as a user and a modeller, the question came up as to how does one model the afterdrop? What is the afterdrop? Is it a unique physiological event or is it simply the momentum of a cold wave passing into the core? Could you comment on that, Gene, from the point of view of your model, and then, Frank, could you comment on your thoughts on this.

Wissler: My wife always accuses me of answering a question with a question, and that's what I'll do now. What are you measuring when you measure rectal temperature? What does this temperature represent? Rectal temperature is responsive to the cold venous return coming from the legs, arterial blood in the abdomen, and venous return from superficial layers of the trunk. How do you put all of these temperatures together to obtain something that corresponds to the measured rectal temperature?

Kuehn: But you do this in your model. You have equations for this.

Wissler: Well, I used to. Now I compute arterial temperature and take the position that it is up to the experimentalists to measure it.

Golden: I stand here with an apology as I'm neither a modeller or a mathematician and the work that I'm showing now was done in conjunction with Dr. Joyce Wheeler, an applied mathematician from Cambridge. Professor Hervey did the computing involved. The question we approached was that from a lot of evidence on human and animal work we felt that the afterdrop in rectal temperature was a straight conductive phenomenon, a time-lag in the conductive heat transfer between the core and the shell. We had supportive evidence from animals and human experiments to support this. The final solution was that if this was true then we should be able to reproduce it mathematically.

We have a much simpler model than that just presented. Our model consists of a cylinder of infinite length with a stirred core of uniform temperature and constant heat production and a variable insulating surrounding shell. One then can plot the temperature gradient through the shell in a series of concentric circles. The beauty of this model is that one can vary the heat production or the thickness of the shell and look at the results that were produced.

We had to make a couple of assumptions. The first was that this was a physiological model, which by virtue of its metabolism and insulation, could stay in thermal balance in water at 20°C. One started off with the cylinder in water at 20°C in a steady state.

In such a model, there is the temperature in the core, the temperature at the surface and the temperatures at each of the con-

centric layers in the insulating shell. With time, as cooling progresses, the cold wave moves in from the surface and you get a steady linear fall throughout the layers.

In the rewarming, the reverse happens. At the start of the re-warm, the core-to-surface gradient is steady and as you rewarm there is a time-lag before the warm temperatures move through. Meanwhile the core temperatures continue to fall.

There is a time lag before core temperature falls. Heat production remains constant and there is no stimulation of shivering. (This has been done in a later model which shows the initial kick as you would expect.) When you transfer the model from cold water into hot water, you get a typical afterdrop occurring. We are only looking at conduction heat transfer. There is no heat transfer by mass flow.

If you put in data from two human beings, a fairly thin athletic male and a reasonably fat female, you get an the afterdrop for each particular case.

What we are saying is that the afterdrop in rectal temperature is purely conductive and is not related to any hemodynamic mechanism. There may well be a hemodynamic mechanism involved but it is not necessary to involve it or use it to explain afterdrop.

So in answer to the question of rectal temperature and what influences it, in our opinion, if you have good mixing in the core, as there is when someone exercises or shivers heavily, well then, the rectal temperature represents core temperature and is representing the thermal gradient out through the gluteal muscles. But if there is not good mixing in the core, as in the case when someone sits still in a cold room or is not shivering violently, when they stand up there will be a venous return from the cold legs which causes a drop in rectal temperature. If the man was shivering violently and had his legs in cold water, then the man would not get an extra-afterdrop on standing up. It would continue changing as determined by previous events.

Nunneley: Are you familiar with David Anton's work at IAM? He had people sitting in life rafts. At first he terminated his experiments when the people sitting on a cold water surface got a rectal temperature of 35°C. Then he started measuring ear temperature and found that when rectal temperature was near 35°C, ear temperature was near 37°C. He proceeded to push these experiments using ear temperature for deciding the termination point and he was getting rectal temperatures as cold as 32°C before ear temperature fell to 35°C. It was a case of regional cooling which enormously influenced rectal temperature.

Zumrick: In the cold gas studies that we did, in which we assumed there to be core cooling, we saw absolutely no afterdrop.

Golden: Was there any shivering?

Zumrick: Nobody shivered violently during the 1400 and 1800 fsw exposures.

Golden: I think you have to get a good mixing to get a reasonable comparison. In the start of an experiment you don't get a very good correlation between the various core temperatures that you are measuring but when the body really starts shivering you find that the methods correlate nicely.

Wissler: I'm not sure what you mean by good mixing. When you talk about very cold exposures you have strong gradients throughout the body. Peripheral temperatures are much lower than central temperatures. I think that it is a very complex phenomenon and my answer is that there are influences due to circulation, some due to conduction and I don't think that you can make a blanket statement that covers all cases.

Webb: I agree with the conduction explanation of the afterdrop in many cases. Our model does the same case. The circulation is purposely fixed in the simple sphere that we are using. It's just a matter of the remoteness of the site of the probe that you are using, whether that probe is in the rectum or some other part of the body that is not strongly influenced by arterial flow. The rectal probe is about as far away from the action as you can get. Conductive lag is all you need to explain things like afterdrop and the initial rise also.

Wissler: How do you explain Timbal's experiments in which he immersed

subjects in 24, 26 and 28°C water and he got increases in rectal temperature of 0.5°C on entry into the water. Conduction certainly doesn't explain that.

Golden: No, but the local increase in metabolism in the gluteal muscles may do this.

Wissler: The subjects were not shivering.

Golden: They were not shivering frankly but emg sensors may have shown that shivering was occurring.

Wissler: When oxygen consumption rates were measured, they didn't change.

Kuehn: When a man goes into cold water, we always see a rise in rectal temperature of 0.3°C or so. Are you proposing a conductive explanation for this?

Golden: No. It's a combination of peripheral vasoconstriction and increased local metabolism. If you assume that with peripheral vasoconstriction that there is a reduction of blood flow from the core to the shell and if at the same time the heat production in the core is slightly increased, well then you must get a rise in temperature.

Webb: You don't even have to increase the heat production where the rectal probe is. It stops losing heat so fast and that temperature readjusts before you get heat outflow again.

Wissler: Is this not what I am saying? It's a circulatory phenomenon as well, rather than a pure conductive phenomenon.

Golden: Well I have another experiment to describe on this point. The data involves 10 pigs, and I now have data on 32, showing the

changes during the last 10 minutes of exposure in cold water and the first 10 minutes in hot water. We measured the central venous temperature in the right ventricle, right atrium and the proximal one or two cm in the inferior vena cava. We obtained the esophageal temperature, the rectal temperature but ignored the gastric temperature because there was a variable amount of gastric dilation between animals and the gastric data was all over the place. In actual fact, the mean temperature at time zero was 31.6°C with a standard error of $\pm 0.2^{\circ}\text{C}$. What we saw was a pretty normal afterdrop in rectal temperature during the first 10 minutes of rewarming but, in the central venous blood temperature, there was no sign of an afterdrop at all. In actual fact, while the rectal temperature was continuing to fall, the central venous temperature was rising. The esophageal temperature was halfway between the two. This was the control experiment.

In the next experiment the animals were re-immersed and during the second cooling they were now killed by an intra-cardiac injection of KCl and we saw what the effect was of arresting the circulation. We got an immediate rise of central venous blood temperature although the animal was in cold water throughout this period. He was not rewarming and was continually cooling while lying in the cold water. Arresting circulation produced an initial rise in central venous blood temperature and after a few hours it began to turn and come down again due to many reasons, ventricular fibrillation and insulation due to the lungs. This was also reflected in the esophageal

temperature. The rate of fall of rectal temperatures was largely similar in alive and dead animals, when the circulation is present and when it wasn't. There was a slight difference due to the contribution of the circulation in these animals. It may be that there was something wrong in my experimental model, with its cardiovascular state, but nevertheless it does show that the central venous temperature was rising as the rectal temperature was falling and here we have the rise of venous temperature with a continued fall in rectal temperature. One can deduce from this that the factors that affect the temperature in any core site in the body are local heat production, conduction of heat to and from the site and transfer of heat by mass flow to and from the site. Now here when you remove the mass flow phenomena you are left with the heat production and conduction factors. Local heat production arises from some local muscular activity while post-mortem events go on but the conduction phenomena is the predominant one. The rectal temperature goes on a bit longer.

Hayward: This is an important issue among those of us who argue about afterdrop and I have data in humans that corroborates what Frank Golden has said. When you talk about afterdrop, number one, talk about which site, because you can have an afterdrop at the rectum for one reason or another and not somewhere else. Humans show much the same pattern for rewarming as Frank showed for pigs. When you look at the different sites, certainly he has shown very nicely the conductive heat loss accounting directly at the rectal site,

especially for the so-called afterdrop. There is a middle position that I think is important, that you can probably enhance that afterdrop at that site by stimulation of circulation. There has been an argument that putting a person in the warm bath enhances peripheral circulation. It may or may not, depending on how long the person is in the bath. But one of the other variables that increases the rate of afterdrop, which is this flow of heat down a gradient in a dead animal model system, is exercise. Frank had some interesting thoughts on this at a meeting in Germany recently on post-rescue collapse, where people have been in cold water and get up on a deck and then die. Frank was talking about the hemodynamic problems in hypovolemic people, that shock contributed to their passing out. It may also be that people, who have been holding still fairly well in the cold and somewhat hypothermic and who suddenly have this afterdrop when they got out on the deck, the physical activity, especially in the legs when climbing, would result in a hemodynamic factor, which probably by convection increases the afterdrop. So Frank was saying there that people who were rescued from the water should probably be rescued in a horizontal position. I would add that they should be rescued with as little voluntary physical activity as possible to minimize the chance of any exercise-induced venous return which would accentuate the afterdrop. It's not yes or no with regard to the hemodynamic factors but the main one, as shown nicely by Frank Golden, is conductive cooling.

Wissler: How well does the pig represent the human, as far as

structure is concerned, for this kind of experiment?

Golden: Well, not awfully well, since you do not have the same limb volumes. I don't think that that is the issue at hand, however. Here you can quite clearly see that you can get a fall in rectal temperature in a peripheral core site while the central venous blood temperature is rising. Whether you've got large limbs or small limbs is not immaterial but it is not important. I do agree that the pig is not the ideal model.

Hayward: But it is not that far off. I've just said that what Frank has shown for pigs looks just like what I've shown in terms of gastric, heart, tympanic, esophageal and rectal temperatures for humans. Some of you know about this. I was very concerned about core temperature sites that you want to get at in rewarming strategies. Rectal temperature is very interesting but we don't care about rewarming the rectum; it is the heart that must be rewarmed. In one study we had monitoring of heart temperature with a Swann-Ganz catheter through the heart into the pulmonary artery to get a valid temperature of what we really want to know. I wanted to compare that during various rewarming regimes to other sites such as rectal, tympanic, gastric and esophageal. This was a person who was cooled down to 35.5°C and brought out at this point and put in a sleeping bag where he rewarmed himself as hypothermic people do by shivering only. What he showed is similar to what we've seen from Frank, namely that during rewarming, the rectal temperature is a very poor criterion of what is going on at sites that you're more interested in. This slide shows a typical afterdrop of

rectal temperature. The subject has been shivering for half an hour in a sleeping bag and the rectal temperature is back to what it was when removed from the cold stress while other temperatures are higher. This spread of temperature at different sites is repeated with other rewarming methods, in this case inhalation rewarming with water saturated air. Again, rectal was the slowest tissue to rewarm followed by tympanic, gastric, esophageal and arterial. The site of the low esophagus seemed to be a very good analogue of heart temperature. There was a faster rewarming of these core sites with inhalation than with shivering only. The fastest rewarming rates are in the bath. The most dramatic rise is shown for pulmonary artery temperature and esophageal temperature in the bath. I've often wondered why after 30 minutes of lying in a bath while measuring rectal and tympanic temperature we were getting the temperatures rising, but not back to normothermia, and yet the people were starting to sweat on their foreheads. Some of you may have seen this. These data show that at that time if you are measuring esophageal temperature and pulmonary artery temperature, they are quite different from rectal temperature. In the bath, and I don't have enough data to confirm this, it looks to me as if you have a greater afterdrop initially. Is this a hot bath enhancement of afterdrop at the heart site? The main point is that if you wanted to find a site for measurement of afterdrop, and if you are interested in strategy of rewarming for divers, you should stay away from the rectal site for making definitive comments on strategies or on determining where we

are in the rewarming schedule, just as Frank was pointing out for pigs. So pigs and humans are basically very similar.

Greene: The immersion results you showed-- was that whole body immersion?

Hayward: Yes it was. Whole body immersion from the neck down. Legs were in. Another experiment would be legs out. I'm calling on people to volunteer in future studies, although hopefully without the need for pulmonary artery monitoring. I think that the deep esophagus, even with such rewarming strategies as donation of heat by the respiratory route, seems to give a high fidelity with the pulmonary artery temperature if you've got it down far enough. I had it just above the diaphragm.

Webb: So you're getting some liver input from that point, apparently. It's warmer there than near the heart.

Hayward: I don't know if the liver is warmer in hypothermic people.

Harnett: A question for Gene Wissler. In your model, is heat production spatially distributed in some way?

Wissler: In distributing heat production due to exercise, you have to specify what fraction of the metabolic rate over resting is produced in the arms, the legs or the trunk. As far as shivering is concerned, we distribute it also.

Hayward: Another question for Gene Wissler. Have models been effective in predicting the central temperature fate of people wearing the hot water suits or some of the new suits with Thinsulate and incompressible layers? Has the model been applied to some of these

situations to see how well it fits, if indeed there is any data on cooling in these suits? Have the models enabled us to see where to go next in suit design?

Wissler: Well, Lew Nuckols presented four slides earlier that showed agreement between mean skin temperature, measured and computed, for the Thinsulate garment. This was done also for the rectal temperature and arterial temperature for three different cases. One involved subjects who rested for four hours in 4°C water, one involved alternate periods of exercise and rest on a 6/4 minute schedule at 50 watts, and another involved the 6/4 exercise schedule at 50, 75, 100 and 150 watts followed by 1 1/2 hours of rest.

Hayward: That would be good prediction of early cooling in fairly mild hypothermia. Have you extended it for predictions of cooling rates down to 32 or 33°C core temperature if the subjects had to stay beyond some undesirable period?

Nuckols: The subjects had to stay as long as six hours in some experiments, although with the suit design we hope that we have more than six hours.

Wissler: The garment in question was better than a one-clo garment and these people could easily shiver a bit and come to equilibrium.

Hayward: So the model has been used to verify the effectiveness of different suits. That was the first part of my question. The other part was to ask if the model has revealed shortcomings of various designs that should be changed

Wissler: I don't think so.

Nuckols: I don't think that we're far enough along in that. I think it would be interesting to apply your model to varying insulation values over the body to see what effects that these would have.

Wissler: I have some reservations about that. One needs to look at local skin temperature and heat fluxes and compare those values with computed values in order to validate the model for that kind of detail. That has not been done.

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SESSION IV

SURVIVAL OF THE LOST BELL DIVER

Kuehn: I consider this session to be important because it addresses a topical and timely problem, namely the lost bell diver. There will be six presentations before the discussion period in which I hope we will be able to bring together many of the points raised in the first three sessions.

PROPOSALS FOR SURVIVAL IN ISOLATED DIVING BELLS

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I have the task of presenting the first paper on proposals for survival in isolated diving bells. I find it surprising that it has taken us, as thermal researchers, so long to come to focus on the lost bell diver problem. On close inspection, the problem is one that is hazardous for human survival. At DCIEM, our interests in this particular problem started with considerations for survival of submersible occupants, stimulated largely as a consequence of the Sea-Link disaster of 1973. Because of that unfortunate incident and fears for survival of the occupants of the Canadian Forces Diver Lockout Submersible SDL-1 if exposed to a similar hazard, we initiated a series of calculations for the heat required for survival during a three-day sinking and did some experiments to test these calculations in 1975 which were reported in the literature a year or two later. First slide, please (Slides situated at end of paper).

The simulation described here took place in Halifax Harbour at a

depth of 50 fsw in the SDL-1. There were three people on board: myself as the physiological observer in the diver lockout chamber--a bell about five feet in diameter--and a pilot and co-pilot in the front chamber, a vessel about seven feet in diameter. The purpose of the experiment was to simulate a normal six-hour mission of the submersible followed by as long a period of time as we could maintain operation of the life support systems. The dashed line here represents the fall of the major 120 volt power system during the six hours of normal running followed by the survival simulation. During the latter simulation, the main power supply was turned off and we were only using the emergency power supplies for operation of CO₂ scrubbers and lights. Next slide (Slide Two).

This showed what happened to the temperature in and on the submersible during its cooling. This data should also pertain to the cooling of a bell that is lost or captured underwater. There is a period of 300 minutes or six hours for most of the bell surface temperatures to decrease to ambient environmental temperatures. In Halifax Harbour at that time of year, the water temperature was about 5 or 6°C. The gas temperature during the running period was warmer due to the functioning of the main power supply, driving various instruments, but on its termination of operation in the survival phase, there was a quick decrease in gas temperature to rather coolish values in both one-atmosphere gas volumes in both the front and diver lockout spheres. Within a period of 400 minutes or so, we were at ambient environmental temperatures. This indicates to me that the

major factor during the heat loss for enclosed occupants would be the characteristics of heat loss from the bell surface to the water surrounding the bell. The diver's body or the occupant's body would be minor factors in the heat loss equation looking at the bell and its contents as a whole. Next slide (Slide Three).

This shows radio-temperature pill measurements for core temperature measured during the 25 hours of the experiment. Although it may be difficult to see here, there was a normal circadian temperature rhythm for the three occupants. One had an elevated core temperature maintained throughout the experiment. The other two, the pilot and myself, did have an overall core temperature drop of 1°C during the experiment while wearing about 2.5 clo of thermal insulation. Remember the background thermal stress was the cold shell of the vessel structure and ambient one-atmosphere air at 10°C . Next slide (Slide Four).

This shows the physiological temperatures on myself. Rectal temperature is the top line, radio pill is the next one. Note how the matching of these two curves shows that the radio pill can be taken as a fairly good analogue of core temperature. It follows the rectal temperature very nicely, about 0.3°C lower, and during various periods of activity you do see periods of temperature increase which suggest that the two probes are tracking together very nicely. These are two different core temperature appraisals. The radio pill, which has engendered some degree of concern lately, does indicate core temperature in the long-term hypothermia exposure. Throughout the

experiment, there was a fall of skin temperature gradually as shown here. Next slide (Slide Five).

In this next slide we see the mean skin temperature according to the Ramanathan index slowly decreasing, mean body temperature decreasing, against a more-or-less constant measurement of heat flow from the skin surface as detected by heat flow discs - about 70 kcal/(m² hr). Next slide (Slide Six).

The clothing insulation was about 2.5 clo. Due to air movement and physical motion, air flooded through the clothing, bringing its insulation down to 2.2 clo. The use of heat flow discs does allow one to appraise the shell insulation, which in this case was about 0.3 clo. This value slowly increased, due to intense vasoconstriction throughout the experiment.

The seventh slide shows the reason for the termination of the experiment at 25 hours instead of three days as we initially hoped. This was due to the failure of the power supplies operating the CO₂ blowers at the end of one day. You can see the consequent build-up of CO₂ with a termination point of 1% CO₂. We terminated the experiment at this point, realizing that, for a submersible occupant, we had one day's survivability before things became difficult from the viewpoint of hypothermia and hypercapnia.

In extending these calculations to the bell situation, a task that we tried to do early last year and later this spring, we realized that Tom Schmidt's model, just being published at that time, offered us a very pessimistic appraisal of just how much time a diver

would have in a bell that was stranded for some time in the sea. Tom's calculations, which he'll present in a paper immediately following this one, indicated that there was only a survival period of about three to five hours in the water column of 400 to 1000 fsw, given the current conventional state of diver clothing that does exist in most bell operations.

Sir John Rawlins in correspondence with me suggested that the bell problem is analogous to that of the lifeboat at sea in which there is a limited supply of water with the occupants of the lifeboat trying to maintain their survival by conservation of the water. In the bell, the analogue of the water is heat; the heat is what has to be conserved. One philosophical point came up. Is it better to use the heat that is residual in the bell as quickly as possible so that it is not dissipated by the driving force of conduction of the bell to the water or is it wiser to conserve it and hang on to it for as long as possible?

Next(slide missing), I put together eight items that I suggest should be considered as survival actions for maintenance of diver physiology until the point of rescue. In most scenarios of the trapped bell diver, immediately on failure of the system, the bell power supply or the sinking of the bell, the diver is still wearing his hot water suit and an attempt should be made to retain as much hot water in the suit as possible to gain those extra few minutes of protection that are residual there. Dick Long may speak to this point later on, concerning the development of a suit that has valves

in it for retention of the hot water supply within the suit, giving the occupant several more minutes of heat, rather than having it be washed out into the bell environment itself or into the sea if the diver is in the water at the time of the bell failure.

Isolation of the diver's body immediately from the bell structure is important. The bell structure is the major driving factor in the heat flow equation which results in the diver's body becoming hypothermic. It is important to move the diver's body off the bell structure quickly. This can be very difficult in the small confines of most bells. The one in which we did our experiments was five feet in diameter and that's typical of most bells. These bells usually hold two divers. So it is very difficult to remove them from contact with the bell structure and we have to look at ways to do this. Such means as sitting on coiled umbilicals or extra diving clothing would be sensible.

Any excess or unnecessary movement must be avoided. There is no benefit to any isotonic or physical exercise in terms of survival. The longer the person avoids moving, the longer he will survive. This is expedited by the adoption of foetal position. Although the experiments have not yet been conducted in hyperbaric environments, John Hayward has shown that, in the immersion environment, the foetal position does extend markedly the chances of recovery of an immersion victim. I'm not sure of the exact figure in John's experiments but I recall a value of 50% increase of cooling times or prolongation of cooling for people in the foetal position which significantly reduces

surface area of exposure.

One of the most important ways to extend survival is respiratory heat reclamation and Bill Hamilton and others will speak to this later. The use of either a passive or active respiratory heat exchanger to get back as much heat and water loss as possible from that contributed to the inhaled cold breathing gas is essential to survival.

Enhancement of insulation is necessary. There are now two commercial manufacturers of survival bag systems that have been appraised in at least one experiment. These are novel systems but may require some modifications for the confined bell environment. Whether or not they can be easily deployed in a bell environment remains to be seen. The bell may fall on its side or be without lights; however, there are now survival bag packages for sale and various laboratories have tested and will test these systems in the near future. There is talk of getting insulation values up as high as 16 clo and perhaps we can talk on this point later - about what 16 clo in a one-atmosphere air environment means in terms of deployment in anoxylhelium environment at 1000 fsw at 4°C temperature.

For provision of dexterity to the diver in the case that he has to move or adjust his life support facilities in the event of rescue or assist the rescuers (in which case he may be fairly hypothermic) heat producing rubefacients can be considered for local application to the hands. There is some debate as to whether or not the trapped bell diver should be involved in effecting his rescue process.

Lastly, I suggest that consideration be given to the prevention of shivering at great depths. I suggest that the coupling of the oxyhelium hyperbaric atmosphere to human skin, even through clothing, is such that shivering may be a negative factor. It may be only at relatively shallow depths that one has any major positive benefit from the massive shaking of the muscle groups. Next slide please.

Enhancement of insulation. The bell environment is usually very wet and when the bell falls or capsizes, there will be water entry through the bell door. There is the consideration of keeping the clothing as dry as possible. Occlusion of the gas-filled spaces in clothing with water will lead to less insulation; donning of as much clothing as possible will diminish this effect. Use of radiation-barrier garments should be considered as well as the use of anhydrotics to remove any residual water that is in the clothing. Urine collection devices used within the clothing also reduce this hazard. The occupant should not need to leave his survival bag or ensemble for such tasks as urination and defecation. Next slide.

Respiratory heat exchangers are of two types, passive and active. The active ones are the most efficient but have the disadvantage of requiring power input. At DCIEM we have been looking at the use of passive exchangers for reclamation of heat and water from each expired breath to the next inspired breath. The disadvantage of these devices is that of higher respiratory resistance. The next slide shows the design of the DCIEM exchanger. It consists of a sandwich of conductive and non-conductive elements in which the heat

is trapped radially in each conductive element with non-conductive elements serving as a barrier to longitudinal heat flow out of the exchanger into the exhaled gas. If the exchanger is insulated, you have a heat trap and a water trap that restores energy to the diver on the next inspired breath. Next slide.

This slide shows the principle of operation. In a normally conductive solid, you will have the temperature in the exchanger coming quickly to a linear drop axially down the length of the exchanger, whereas if you have a series of small heat traps, like small waterfalls, which is what the conductive elements are analogous to, you will have much better heat retention. There is better retention of temperature in the forefront of the exchanger. The next slide shows work at the lab which has resulted in efficiencies of the order of 80% or so, which matches fairly well the efficiency of the Kinergetics passive heat exchanger. So here are two exchangers, one commercial one and the DCIEM prototype, which have similar efficiencies for reclamation of respiratory heat. Next slide.

The important thing to appreciate in much of this technology is that we have to have it not only efficient but also economic. The device must also be compact and corrosion resistant. One problem with our DCIEM exchanger is that we have about 10 hours of use of a passive exchanger before it becomes completely occluded with mucus or respiratory moisture to the point where efficiency drops off markedly. A low respiratory ΔP is also important. As the exchanger pores are occluded with moisture, we wind up with a very inefficient

device. Next slide.

The heat-producing rubefacients that I found in the literature that can be applied to the bell diver problem are those containing lithium/aluminum, lithium/magnesium or aluminum/magnesium to be used only in the event of rescue scenario and not to be applied except in that scenario. I don't think aerosols have a place in this problem because of their volatility. Next slide.

I went to the literature as well to find ways that can modify or prevent shivering. Some of these suggestions do not pertain at all to the hyperbaric environment. This is certainly true for the first case. At one atmosphere, inhalation of 100% oxygen does ameliorate shivering as do antipyretic drugs, magnesium ions, injection of insulin, and inhalation of 1 to 5% CO₂. Respiration of low levels of CO₂ as high as 10 or 15% can repress shivering. This would be an inevitable consequence of the bell diving environment in any case since the diver probably will have a poor reclamation process for CO₂. There will be an intrinsic shivering modification because of that but how much do we wish to have? And of course we have the possibility of going to barbiturates.

Much of this later topic is speculation and should only be presented at a workshop such as this. I realize that it is incumbent on me to talk about experiments. We plan to conduct an experiment at DCIEM similar to that conducted by Stein Tonjum at the Norwegian Underwater Institute to examine some of these possibilities. The experiment will take place next year and will include some considerations of

just how much carbon dioxide is necessary for suppression of shivering and just what that will mean for retention of heat in the working environment.

Zumrick: Are you prepared to speculate at what depth in an oxyhelium environment that shivering will not be beneficial for maintenance of thermal balance?

Kuehn: I think that it is similar to the situation in immersion physiology where at very cold temperatures some authors have speculated that the shivering is of little benefit because of the coupling with the water. The heat produced by the shivering metabolism may not be beneficial against an accentuated conductive or convective heat loss. That water temperature of concern is around 5°C at one atmosphere. In terms of diving, one would have to translate that to an oxyhelium hyperbaric environment, via the convective coefficient, which Paul Webb has defined about 12 years ago. This coefficient is a means of comparing different convective environments. Based on this coefficient, I think that the depth at which shivering is contra-indicated in cold oxyhelium is at depths of 1000 fsw.

Pozos: The point about many of the regimes that you mentioned to prevent shiver is that most of these methods are associated with a pronounced peripheral vasodilation, leading to accentuated heat loss. So on one hand you want to minimize shiver but in the process you will promote a massive vasodilation. We've experimented with CO₂ to a certain extent and we got a pronounced vasodilation. Some of our subjects turned a beet-red color in a very cold environment.

In these experiments they are shivering when they start breathing various mixtures of CO₂. This saving of heat loss in one direction will promote another.

Kuehn: That's right. Experiments have been conducted to see how these two effects balance each other off. We would have to look at this experimentally in terms of depth of exposure to determine at what depths shivering is a negative factor.

Wissler: Was your bell insulated on the outside?

Kuehn: No, it wasn't. The bells were part of the submersible and made of standard half-inch high-tensile strength steel.

Long: In the case of the hot water suit inside a diving bell, remember that a diving suit does not heat the diver. In fact, it cools the diver. All it does is control the rate of cooling to the point at which he can eliminate his metabolic heat. His skin temperatures are 93°F and the core is something higher than that, so in fact he is being cooled. If you put a lot of hot water inside his suit, you are going to cool him faster. Potentially, if you were in a warm bell and you filled his suit with water, you would cool him faster than if you took the water out. There is some merit in what you say, if you have water in the bell. That latent heat in the water would be passed out as part of the sensible heat of the system.

Kuehn: That's the point I was trying to make. If the diver has hot water in his suit, he shouldn't just flush it and dump it. He should take it into the bell with him. In your literature, you mention 20 minutes of extra survival time based on this concept.

Long: In the case of the NRV suit, it is intended to prevent thermal shock in the water as opposed to hypothermia.

Kuehn: We've tested your suit at DCIEM expressly for that purpose and although we didn't get quite 20 minutes thermal protection we did get a beneficial difference in temperature drop in cold water exposures. My point for the diver is that if he has this suit on, he should use the hot water, rather than flush it.

Hayes: Just a practical question. Did you have any problem using a rectal probe for 25 hours?

Kuehn: No, I didn't, although I did not have a bowel movement in the course of the experiment. I've talked to people who have used it for longer periods of time. In the case of bowel movements, it is simply removed and re-inserted after.

Hayes: I was more curious about general discomfort.

Kuehn: No, the probe was not uncomfortable.

Webb: We routinely use rectal probes for 48 hours in my laboratory. There is no problem.

Hamilton: The divers' attitudes about rectal probes are not in agreement with that viewpoint. They dislike rectal probes out of proportion to the discomfort involved.

Golden: I'd like to come back to this question of shivering. Obviously you're right, if there is a high heat flow from the surface of the body, shivering is then undesirable because it will increase the heat flow. It contributes very little to the body heat state. If you can insulate the surface of the body from the surrounding en-

vironment, obviously shivering will be beneficial. You reach a state, then, where if you have one of these survival bags which insulates the diver to a degree from his environment, is it a wise thing now to stop the diver from shivering? I don't know the answer, but I think that the most important point of suppressing shivering in somebody like that is that you are reducing his respiratory ventilation and his concomitant respiratory heat loss.

Kuehn: That's an interesting point but I don't want to steal Stein Tonjum's thunder since he plans to talk about shivering in the survival bag in his tests in Norway earlier this spring. I don't think that we have 16 clo of insulation in the bag and I don't think that the bag occupants are that well protected in hyperbaric helium. Perhaps we can come back to this point after Stein has talked to us.

Hayward: Did you consider that your simulation, for as long as it went, was fairly representative of these types of accidents in terms of the thermal situation?

Kuehn: Yes, I did. In a sense it was pessimistic in that we don't have three-day survivability. It probably will take of the order of three days to effect a rescue in most difficult environments, such as the Arctic. In the North Sea where you have operational systems nearby, well then you may have 24 hours turn-around, but even that may be pessimistic, in terms of Tom Schmidt's paper, which we will hear shortly.

Hayward: In looking at your data, there seems to be no evidence of hypothermia over that time. You talked about some ongoing slight

temperature decrease but really, in my estimation, the core temperatures at the end of that one day were those of a normothermic person who is inactive for that length of time. I see no significant evidence of hypothermia there. Did you shiver at all?

Kuehn: Yes we did, toward the end of the experiment. Our analysis was that we had a 1°C drop over the experiment in two of the subjects, taking the circadian rhythm into account. Also, we were not completely inactive and we were wearing about 2 clo of insulation. We felt that this was the start of a slow incipient hypothermia which would become dangerous over a three-day period.

Hayward: You stressed the close coupling of the skin to high heat loss out to the water in that environment, but in comparison to the water situation in relation to suppression of shivering, there is a vast difference between the cold stress in that documented case and anything in water, other than in a survival or exposure suit. I'm not convinced from the data that you've presented that there is any case for suppression of shivering.

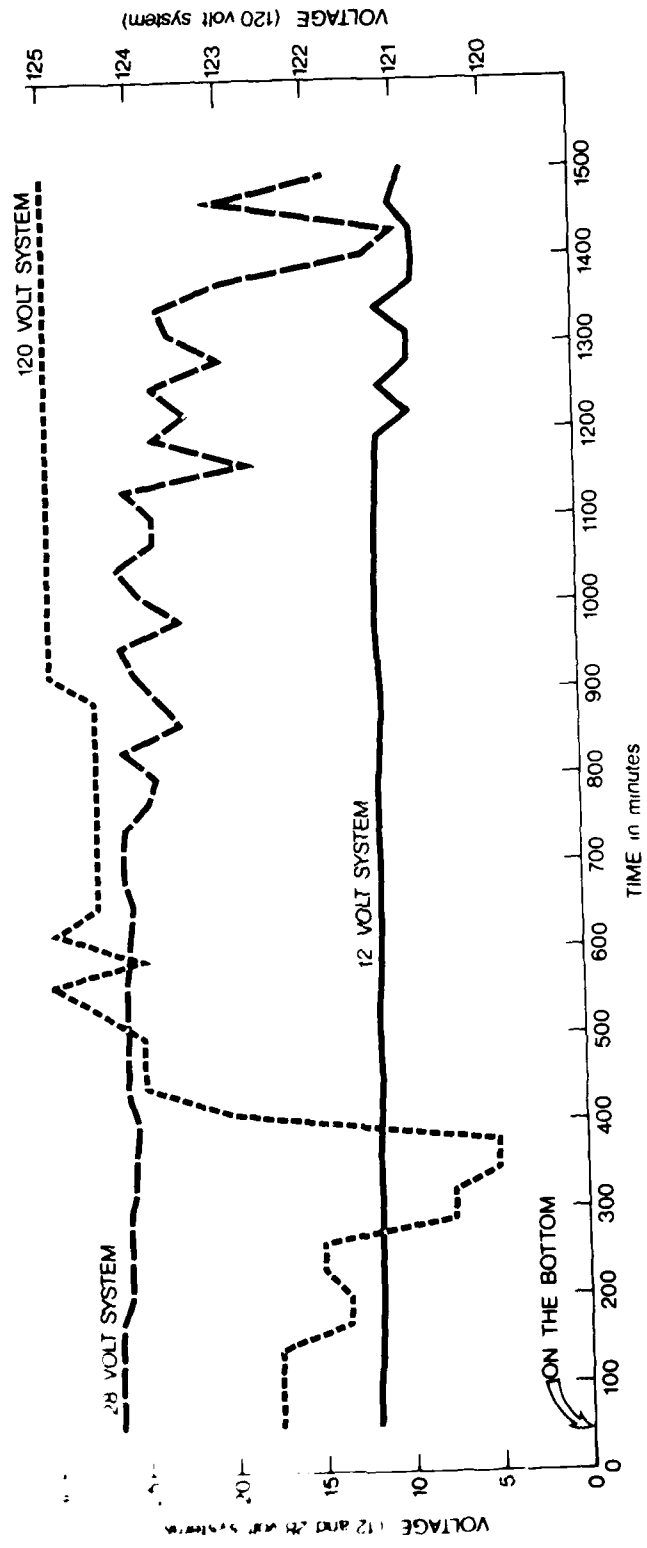
Webb: Let me help answer the objection. Lorne said at least once that the experiment was conducted at one atmosphere.

Kuehn: That's right. The environment in the submersible was dry and at one atmosphere.

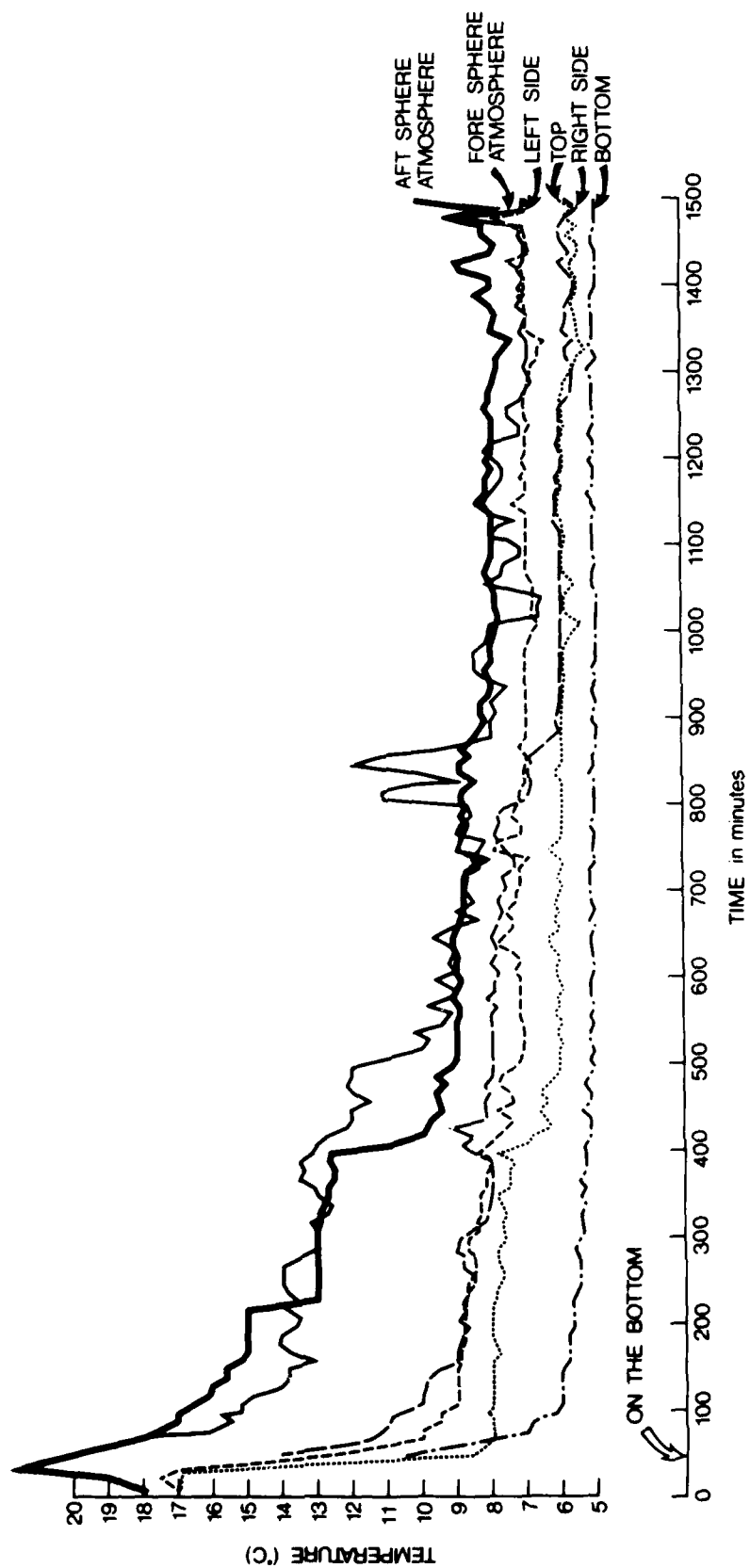
Webb: The thermal stress in such a case is very small and the data indicate that they had mild cold stress.

Kuehn: The one-atmosphere submersible experiment is not representative of what we are going to hear in the later papers in this

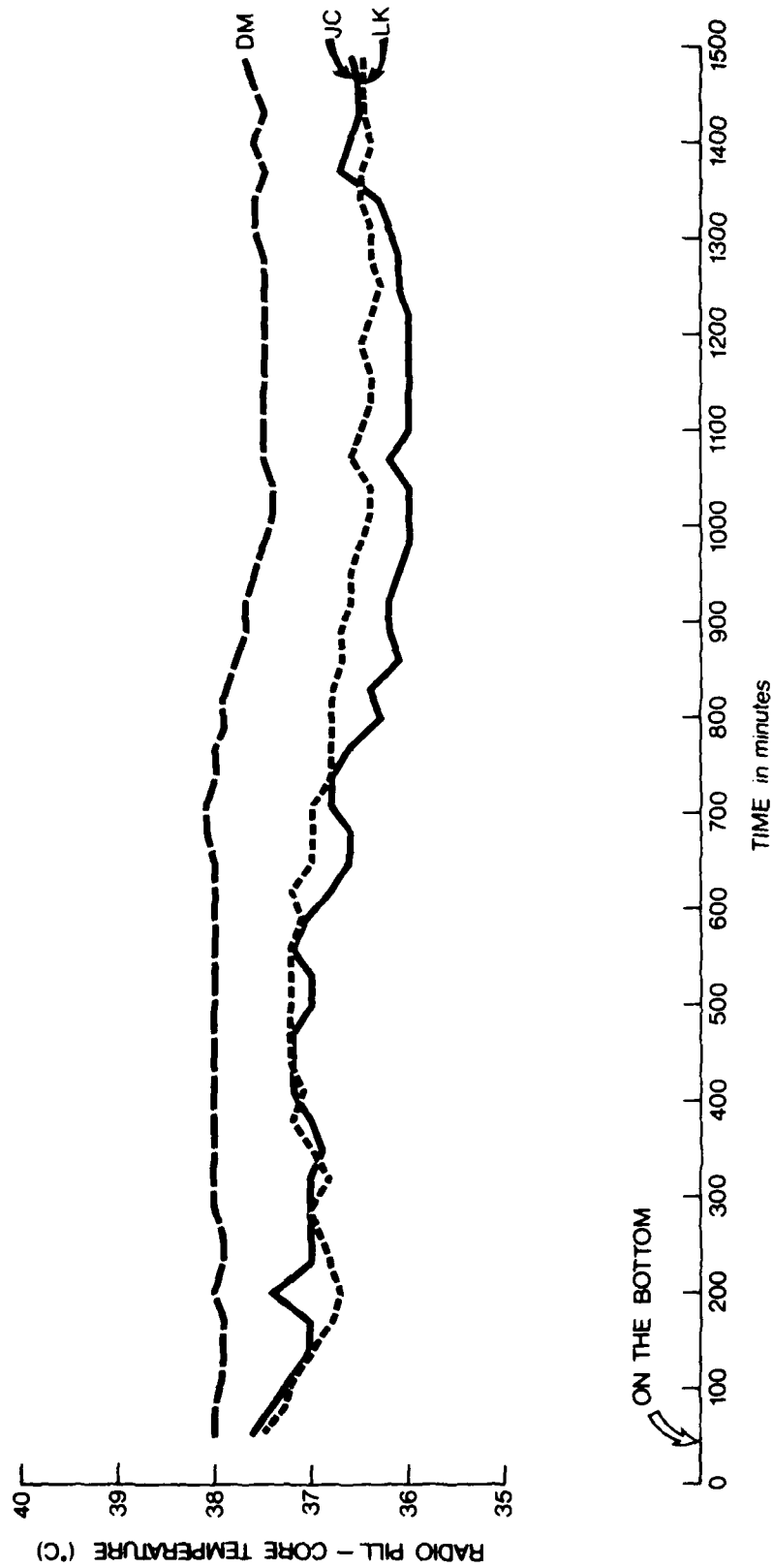
session. I would like now to introduce Tom Schmidt, who to my knowledge has been the first modeller to attack this problem back in 1976. He revamped his model in 1978 and now he is going to tell us of his recent work on projections for survival in emergency hyperbaric environments.



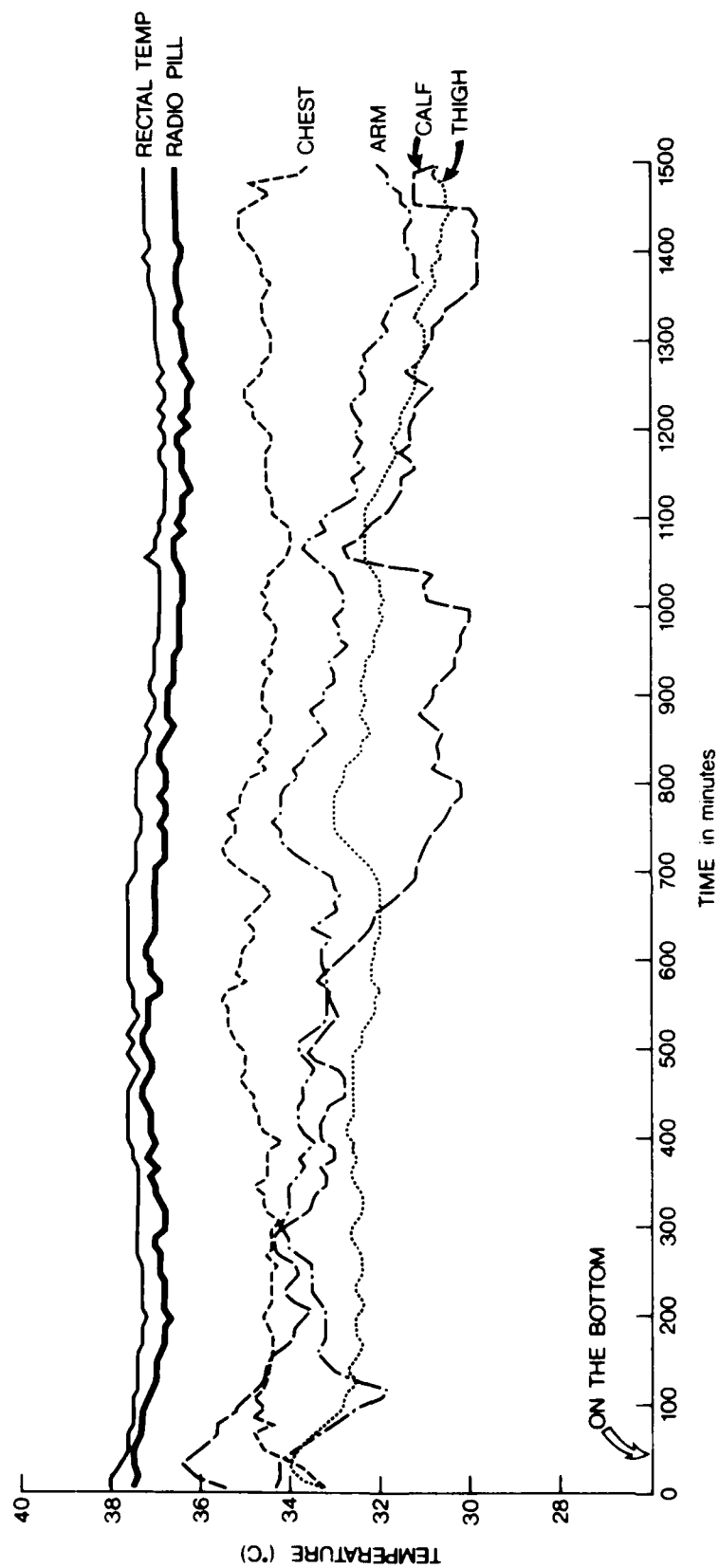
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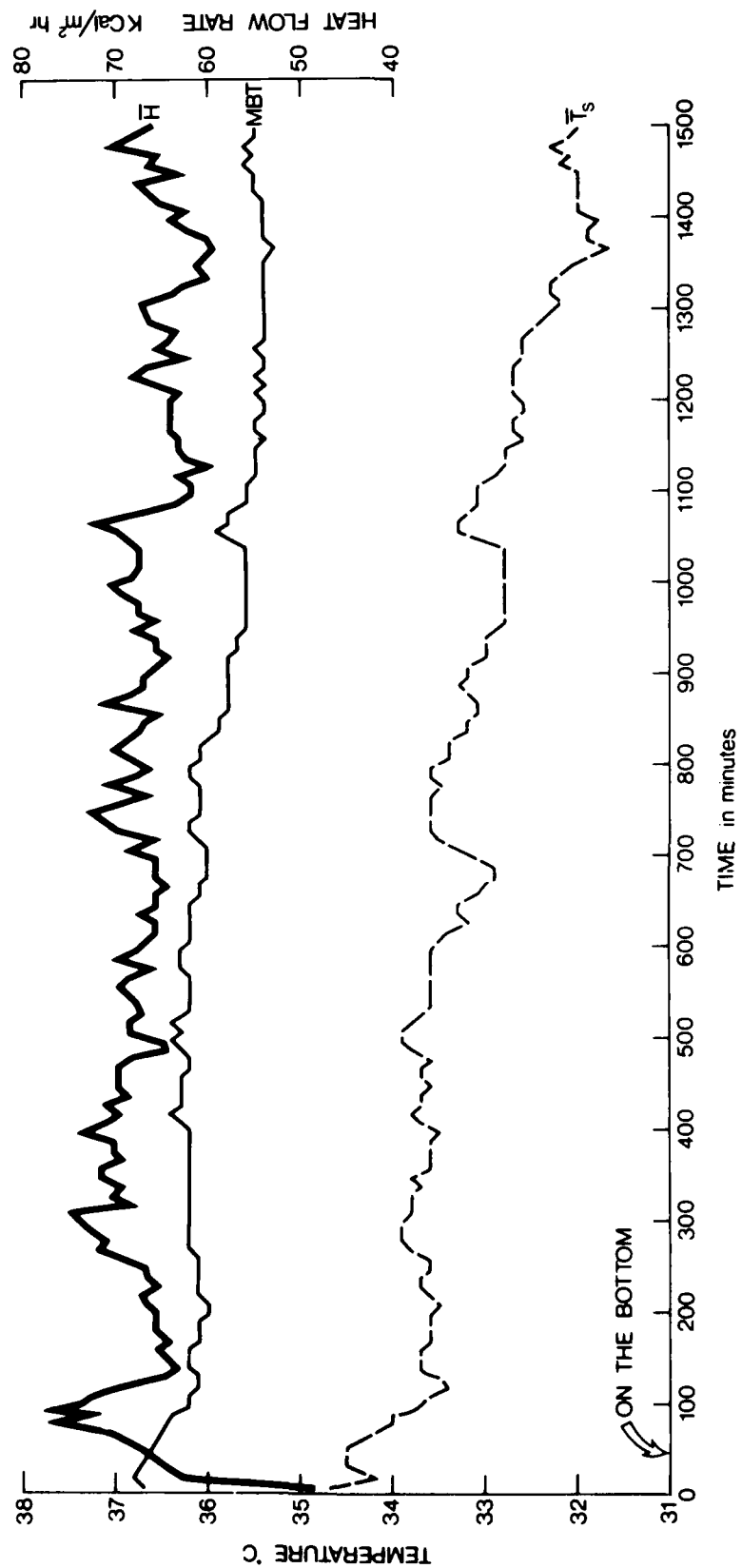
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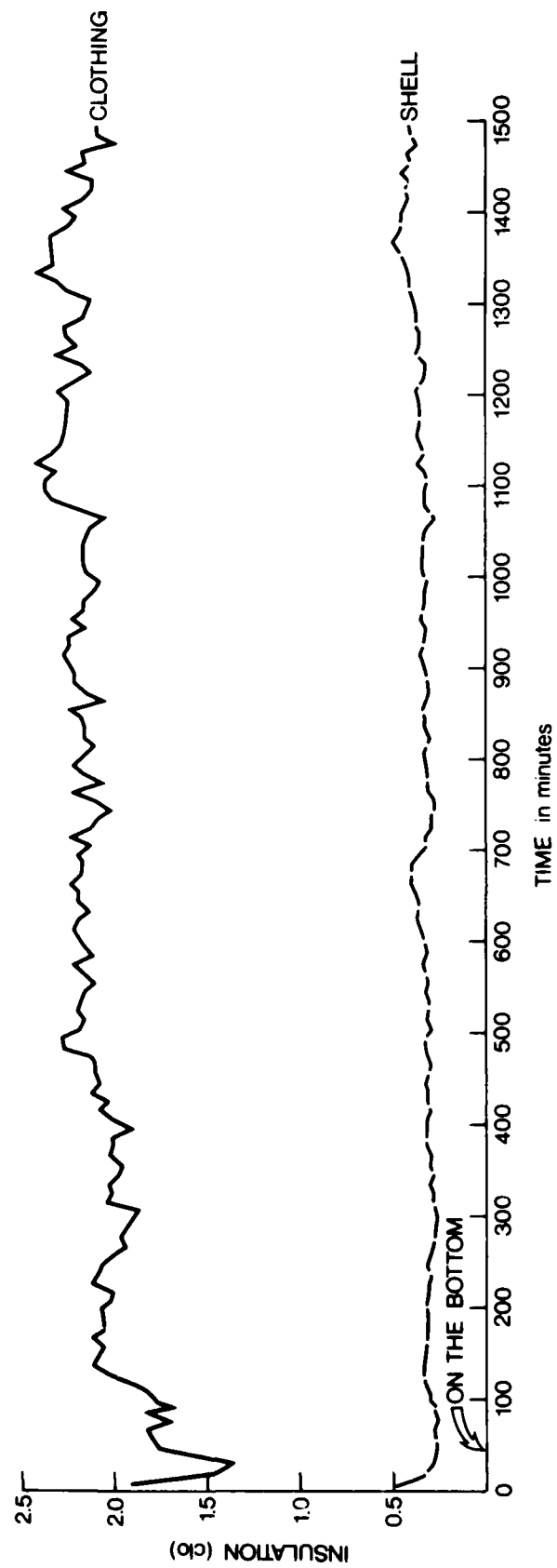
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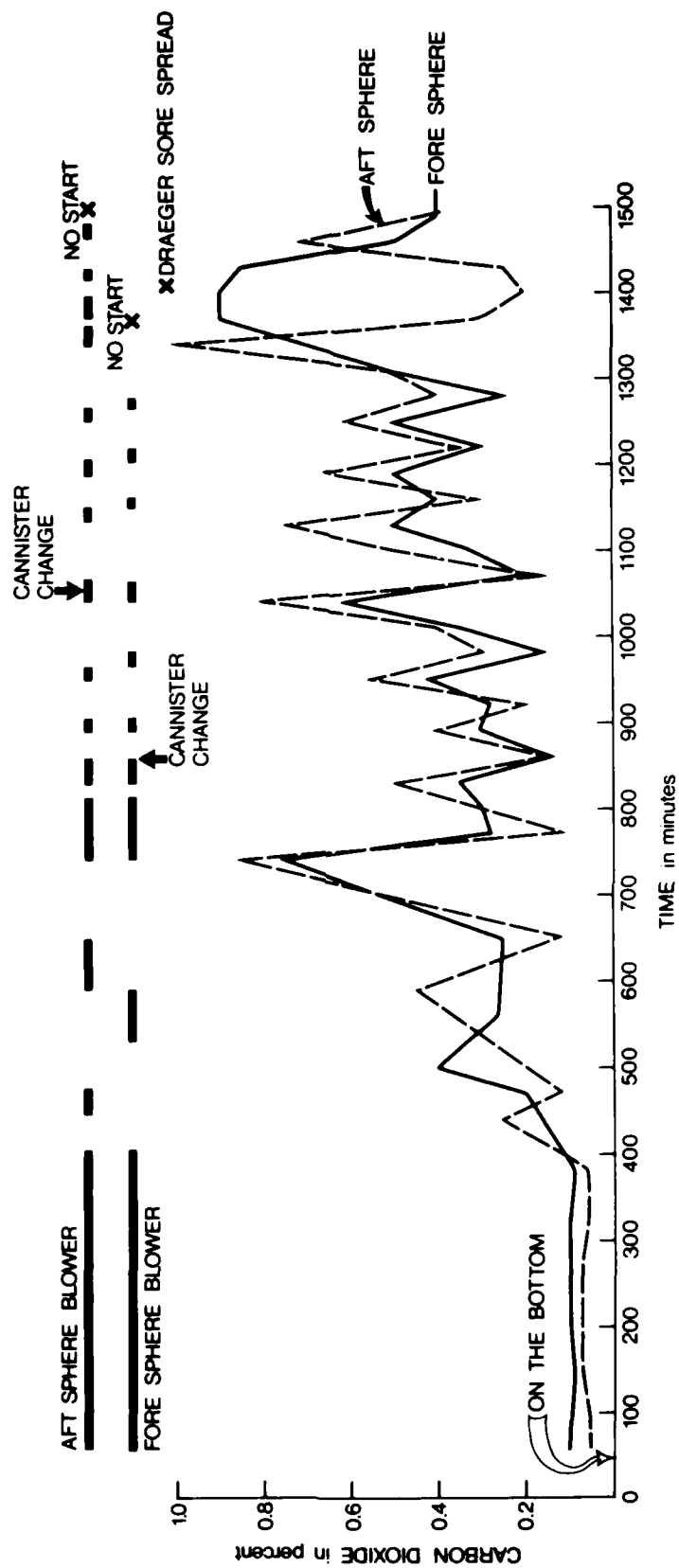
SLIDE 4.



SLIDE 5.



SLIDE 6.



SLIDE 7.

THERMAL CONSIDERATIONS OF SURVIVAL IN EMERGENCY HYPERBARIC SITUATIONS.

T. C. Schmidt. Senior Research Engineer.

Lockheed Missiles and Space Co.,

Advanced Systems Division,

Ocean Systems Group

INTRODUCTION

The ability of any gas to transport heat out of (or into) the body increases with increasing ambient pressure, and the convective heat transfer characteristic of helium is significantly greater than air. Thus, in hyperbaric helium environments, the temperature requirements for thermal comfort approach body temperature, and the range of comfortable temperature narrows. This will be referred to as the Regime of Thermal Comfort. Likewise, the minimum temperature to preclude possible fatal hypothermia also increases with increasing ambient pressure, and will be referred to as the Regime of Sustainable Cold Stress. Finally, with the onset of shivering, for certain combinations of temperature and pressure, the increase in respiratory heat loss due to the increase in respiration rate attendant with the increase in oxygen consumption, may exceed the increase in metabolic heat generated. In this case, the body's involuntary defense mechanism of shivering will hasten, rather than retard, the onset of hypothermia. Thus, there also exist Regimes of Beneficial and Detrimental Shivering. The temperatures and pressures of each of these regimes will be presented, based upon either empirical data, or theoretical calculations (modeling) supported by limited empirical data.

THE REGIME OF THERMAL COMFORT

As shown in Figure 1, in helium based environments, the temperature required for thermal comfort increases with increasing depth or pressure, and there is a narrowing of the range of thermal comfort (Lawrie, et al , 1974; Webb, 1975). Not only does this pose a more difficult temperature regulation problem at the higher pressures, but body heat loss can become a serious problem in cases of a lost or stranded SDC, PTC, or diver lockout submersible, or in emergency evacuation of the diver support platform while divers have a decompression obligation. For this reason, a knowledge of what temperatures as a function of pressure would be life sustaining for extended periods of time is desirable.

THE REGIME OF SUSTAINABLE COLD STRESS

In an earlier attempt at calculating heat losses, resultant core temperatures dropped with time, as did consequent "survival" times in cold hyperbaric helium environments (Schmidt, 1976). A conversion error was made such that the respiratory and total heat losses were too high and the "survival" times excessively short. Although the error was corrected prior to presentation, the published preprint had already gone to press. The correct results are represented here, along with maximum voluntary exposure limits in cold hyperbaric helium reported on since then by Kuehn and Zumrick (1978).

The approach taken (Schmidt, 1976) was as follows:

- (1) Conductive, surface evaporative and latent respiratory losses were neglected;
- (2) Radiative losses were calculated using the derivation from the relation for plane grey surfaces as developed by Nevins, et al (1965);
- (3) Convective losses were calculated using the Nusselt, Grashof and Prandtl formulations for free convection around vertical cylinder body approximations as developed by Nevins, et al (1965); and
- (4) Sensible (non-latent) respiratory losses were calculated as:

$$(V)(C_p)(T_{exp} - T_{insp}) \text{ (density @ P, T inspired)}$$

For physiological parameters that might be representative of long term sustainable cold stress the following values were used:

- (1) Metabolic heat production = 250 Kcal/hr (approx. 2 1/2 X typical metabolic rate in hyperbaric environments);
- (2) Respiratory minute volume = 15 LPM (approx. 2 1/2 X that sitting at rest at thermal comfort);
- (3) T skin mean = 82 deg F (27.8 deg C -- approx. 6 deg C less than normal);
and
- (4) T exp = 95 deg F (35 deg C -- approx. 1-2 deg C less than normal).

To approximately account for good thermal protection that might be reasonably available (e.g., long underwear plus dry suit, etc.) the convective losses as calculated for the unprotected condition were reduced by 75 percent.

The resultant losses, for the unprotected and dressed situations at various pressures and temperatures were then summed over time and compared to the correlation between heat loss, rectal temperature (T_{re}) and symptoms, as presented by Webb (1976), shown in Figure 2. For a maximum allowable body heat loss, a value of -400 Kcal was chosen. This would be expected to result in a T_{re} of slightly less than 33 deg. C, at which point sensory, motor and cognitive function would be significantly impaired, cardiac irregularities may begin to appear, and improper rewarming techniques might prove fatal.

The results are shown in Figures 3 thru 6. The curves are the visual best fit for points at each 10 deg. F interval. The "critical temperature" shown in the left of each figure is that temperature at which the calculated "survival time" appears to become indefinite, and in essence, represents a temperature at which heat loss would stabilize at around -400 Kcal, and T_{re} would stabilize at around 33 deg. C. The range of known thermal comfort is also shown on the left of each figure. The horizontal bars are the $\pm$ S.D. values of voluntary exposure limits as reported by Kuehn and Zumrick (1978) for unprotected subjects.

As shown in Figure 5, the voluntary exposure limit at 820 FSW was longer at the warmer temperature (25 deg C) than the colder temperature (20 deg C), as would be expected. However, at the colder temperature, the limit at 870 FSW, which would be expected to result in somewhat higher thermal stress, actually resulted in voluntary exposure limits not only longer than the lower pressure at the same temperature, but longer than the lower pressure at the warmer temperature. However, the longer exposure at 870 FSW was terminated at a decrease in T_{re} of almost 1 deg C, while for the shorter duration at 820 FSW, decreases of only .25-.33 were sustained.

In Figure 6, the warmer (25 deg C) exposure at 1000 FSW was tolerated longer than the colder one (20 deg C), and the exposure at 870 FSW was tolerated longer than the one at 1000 FSW at the same temperature. The lower temperature exposure at 1000 FSW, if involuntarily extended would have penetrated the critical exposure curve for the unprotected condition, while the warmer exposure was above it. In terms of stress physiology, at 1000 FSW, the warmer exposure was tolerated to a decrease in T_{re} of about 1/2 deg C, while at the colder temperature the exposure was terminated as soon as the initial T_{re} rise was dissipated, i.e. no net decrease below normal was tolerated.

It is difficult to extrapolate too much meaning from voluntary exposure times per se. One factor most common in all of the exposures was that at termination, the mean body temperature was still decreasing. That measure included, however, a skin temperature component, which in most cases was also still decreasing, possibly indicating that the heat conservation processes still had additional capability to further reduce the rate of heat loss.

However, as shown in Figure 7, at 20 deg C, in the longest duration exposures (approximately 2 hours at 700 FSW and 3 hours at 870 FSW) the fall in T_{re} appeared to have plateaued, or appeared to be approaching a plateau. However, for the 1000 FSW exposure, the cold stress was to an extent such that no decrease in T_{re} below normal was tolerated, with the exposure being terminated within 1 hour.

In Figure 8, the critical temperature as a function of pressure for the case of no thermal protection is shown, along with the above mentioned exposures at 20 deg C. As can be seen, the 700 FSW exposure which appeared to have resulted in a plateauing of the T_{re} was right at the calculated critical temperature; the 870 FSW exposure which appeared to have been approaching a plateau at the termination of the exposure was about 1.5 deg C below the calculated critical temperature; and the 1000 FSW exposure in which the cold stress was such that no decrease in T_{re} was tolerable was in fact about 3.5 deg C below the critical temperature.

Although the critical temperature curve is based upon a theoretical attainment of a thermal equilibrium state at a heat debt of -400 Kcal, and, according to Figure 2, a T_{re} loss plateau at -1 deg C would indicate only a 150 Kcal heat debt, Webb (this workshop) has shown that the correlation between T_{re} and heat debt incurred is actually a time (rate) dependent function, while Hayward (this workshop) has shown that T_{rectal} may not be indicative of T deep arterial blood, the physiological parameter of primary concern. As stated in 1976, these curves are only theoretical and are based upon numerous assumptions and approximations, and are only an attempt to at least get a feel for an important aspect of diver safety that had (at that time) otherwise been left entirely to conjecture.

THE REGIME OF BENEFICIAL SHIVERING

The question of net change in heat balance due to the onset of shivering in hyperbaric helium environments has been alluded to, but has not really been quantified. Many references simply state that the increased metabolic heat produced will tend to be negated by the increased respiratory and surface convective heat losses, and the beneficial vs detrimental effects of the onset of shivering remain unresolved with any certainty or unanimity. Since the attendant losses are a direct function of ambient pressure and temperature, for certain ambient regimes there would be expected to occur a net heat gain, while at others a net heat loss. An analysis was made to attempt to define this regime of beneficial shivering.

The approach taken was to compare the additional heat generated to the additional heat lost, between typical resting and shivering activity levels, at various ambient pressures and temperatures.

It has been reported by Raymond (1977) that divers shivering but otherwise resting have an O_2 consumption rate of about $150 \text{ Kcal/m}^2/\text{hr}$. For a 1.8 m^2 individual, this would give a value of 270 Kcal/hr . By comparison, for a resting individual at thermal comfort, performing no external work, metabolic heat production is around 72 Kcal/hr (Seagrave, 1971). Likewise, Webb (1975) gives a comparison of $72\text{-}108 \text{ Kcal/hr}$ resting vs $180\text{-}270 \text{ Kcal/hr}$ shivering, and a third source (Shilling, et al 1976) gives a comparison of 93 Kcal/hr resting vs $278\text{-}557 \text{ Kcal/hr}$ shivering. These values are summarized in Table I. Although normal metabolic rates typical of hyperbaric chamber occupations are typically closer to 100 Kcal/hr , for this analysis values of 72 Kcal/hr resting vs 270 Kcal/hr shivering but otherwise resting, were used.

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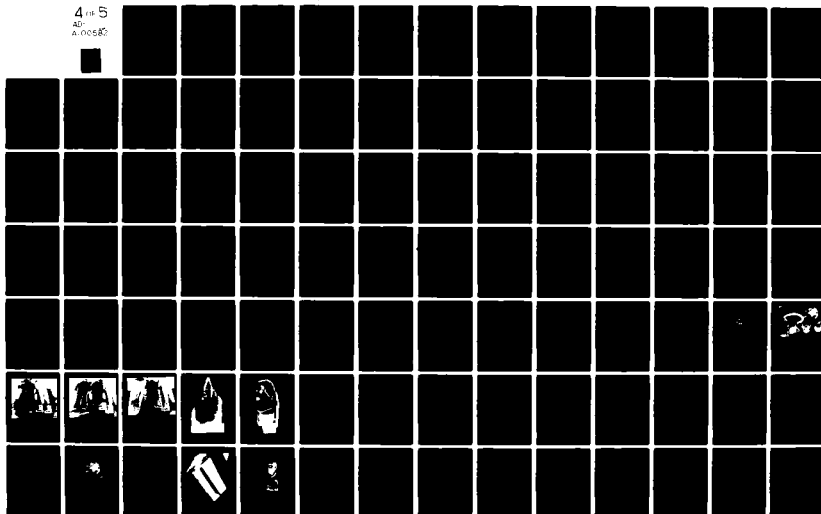
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The difference in O_2 heat equivalent is 198 Kcal/hr. However, not all of this may be realized as available internal heat if external work is performed (i.e. bodily movement attendant with shivering, increased ventilatory work, etc.). Although this will be very small, since the efficiency of shivering muscles is given as 0.1 to 0.15, to be maximally conservative, only 87.5 percent of the 198 Kcal/hr was credited to available heat, or 173 Kcal/hr. By contrast, if work is performed by coordinated muscle movement, up to 25 percent may leave the body as external work.

To calculate the respiratory heat losses it is necessary to determine the representative respiratory minute volume. Figure 9 shows the relation between alveolar ventilation, endogenous CO_2 production, and the partial pressure of CO_2 in alveolar gas. At lower values of CO_2 production, an alveolar CO_2 partial pressure of around 40 mm Hg is maintained, up to about 70 percent of maximum capacity, at which point ventilation rate increases out of proportion to further increases in CO_2 production.

Figure 10 shows the resultant plots of alveolar ventilation vs oxygen consumption for R.Q.'s of 0.8 and 1.0, to maintain alveolar PCO_2 as derived from the dashed line relation of Figure 9. Also shown are the pulmonary ventilation rates as a function of oxygen consumption as given by the U.S. Navy Diving Gas Manual (Milwee, et.al. 1969) and by Flynn, et al (1974). The former shows a direct linear relation such that pulmonary ventilation rate is about 24X oxygen consumption rate, while the latter gives a ratio of 26, with a correction factor from saturated to dry conditions, as a function of pressure, which assumes that the expired gas is saturated at 37 deg C. At 1 ATA, this gives a dry corrected value of 24.4, while at 1000 FSW the correction is 25.95.

As it was not desired to have to assume representative respiratory frequencies in order to convert from alveolar to pulmonary ventilation rates, and since we will be within 70 percent of maximum capacity, it was decided to simply use a value for pulmonary ventilation rate of 25 times oxygen consumption rate, determining the oxygen consumption rates from the previously determined metabolic rates of 72 and 270 Kcal/hr, using an in-vivo Kcal oxygen equivalent for an average mixed diet of 4.825 Kcal/liter. This approach yielded a resting ventilation rate of 6.2 LPM, a shivering but resting ventilation rate of 23.25 LPM, and a net difference between the two of approximately 17 LPM.

The sensible heat loss due to the increase in respiration rate was calculated using the specific heat and density values for a 3 percent oxygen-helium mixture at inspired temperature and pressure. Although the oxygen composition might typically be anywhere between one to five percent for the range of pressures under consideration, for any variation of oxygen composition within this range, the increase in density is offset by the decrease in specific heat, such that the density times the specific heat is essentially constant at any given temperature and pressure.

The other variable required for the calculation of sensible respiratory heat loss is the difference between the inspired and expired gas temperatures. It has been well documented that expired gas temperatures are, in general, progressively lower as the inspired gas temperature is lower.

A relation prevalent in the literature is:

$$T_{\text{exp}} = 24 + 0.32 T_{\text{insp}} (\text{deg C}) \text{ -- Varene, et al , 1973; In Webb, 1975.}$$

This is illustrated in Figure 11, along with other reported data, including two relations presented at this workshop, namely:

$$T_{\text{exp}} = 25.4 + 0.28 T_{\text{insp}} (\text{Piantadosi and Zumrick}), \text{ and}$$

$$T_{\text{exp}} = 26 + 0.42 T_{\text{insp}} (\text{Hayes}).$$

Also shown are the relations derived from least squares analyses performed on the data reported by Hoke, et al (1976) in their study of respiratory heat loss and pulmonary function during cold gas breathing at high pressures, which includes 120 values of inspired vs expired gas temperatures at various work loads, for both cold and warm inspired gas temperatures, throughout the range of surface to 1000 FSW. The equations obtained are shown in Table II.

Note that from surface thru 800 FSW there appeared to be an increasing trend in expired gas temperature, which did not hold, however, for the 1000 FSW data. The 850 FSW data was not analyzed. As the T_{exp} vs T_{insp} values were also reported for five different work rates from 0-120 watts, a similar analysis could be performed to see if there was any trend in T_{exp} as a function of work (and therefore ventilation) rate. Although one would intuitively suspect that increasing work and ventilation rates might result in a decreasing T_{exp} , this analysis was not performed.

The relation used was the one derived from the combined data of Hoke, et al ,

$$T_{exp} = 29.25 + 0.196 T_{insp} \text{ (deg C).}$$

The increase in sensible respiratory heat loss in going from a resting to a resting but shivering state was calculated by multiplying the increase in respiratory minute volume (17 LPM) times the specific heat of 3 percent helium-oxygen (1.044 cal/gm deg C) times the difference between inspired and expired gas temperature (as determined by the eqn. $T_{exp} = 29.25 + 0.196 T_{insp}$) times the density of 3 percent helium-oxygen at inspired temperature and pressure (Table III). For a given composition the value of specific heat is fairly independent of pressure and temperature.

The additional heat losses(Kcal/hr)for each case, due to the increase in respiratory minute volume, is given in Table IV.

If we take the increase in metabolic heat available (173 Kcal/hr), and subtract the value lost thru increased ventilation (Table IV), the percent of increase in metabolic heat still realized as a net heat gain is shown in Figure 12. As can be seen, the heat loss due to increased ventilation exceeds the increase in metabolic heat produced at around 40 deg F (4.4 C) at 1000 FSW, and (if extrapolated) at around 30 deg F (-1.1 C) at 800 FSW.

This data is in fair agreement with the prediction by Tauber, et al (1969) that respiratory heat loss would exceed metabolic heat production at 850 FSW in 4.2 deg C water, and the data of Hoke, et al (1976) which showed the percentage of metabolic heat production lost thru respiration at various depths and work loads approaches 100 percent for cold gas breathing (0-7 deg C) as the pressure approaches 1000 FSW.

Comments on the approximations and assumptions made in this analysis are as follows:

(1) The respiratory heat loss due to heating the water component of the gas, the latent component due to saturating a non-saturated gas, and maintaining saturation between inhalation and exhalation temperatures were neglected--i.e. only the heat loss component due to sensible heating of the dry gas component was considered. For the situation under consideration, the neglected components are extremely small. For the case of inhalation of a completely dry gas, the latent component, which would normally be dominant at 1 ATA, decreases significantly with increasing pressure. For example, for the case of 6000 FSW, 70 deg. F (21.1 C) ambient, the latent component would be 4-5 percent of the sensible component, but would be only about 1 percent of the additional heat generated. Also, a stranded bell or submersible would quickly become saturated, eliminating this consideration, and the remaining losses due to heating the water component of the gas and maintaining saturation between inhalation and exhalation temperatures are likewise exceedingly small.

(2) The increase in surface convective heat loss due to the onset of shivering was neglected. For the case where the diver is dressed and/or there is already forced convection due to the movement of gas by a CO₂ scrubber, etc., the increase in additional convective loss is not significant.

(3) An important variable is the relation between exhaled and inhaled gas temperatures. As was seen in Figure 11, although there is a definite decrease in expired gas temperature with decreasing inspired gas temperature, the reported data as to exactly what this relation is, are not in close agreement. In the foregoing analysis, the more conservative relation was used.

(4) Finally, the analysis was based upon the assumption that cold induced hyperventilation does not occur, i.e. that ventilation rate is that required to maintain alveolar PCO_2 at "normal" levels of 40 mm Hg. If we were to allow for an "overventilation factor" of say 50%, which would result in an alveolar PCO_2 of about 25 mm Hg (low, but still not in the "problem area" of hypocarbia) then the ambient temperature required for a net heat gain is about that of the USN Minimum Safe Inspired Gas Temperature Limits as calculated by Braithwaite (1972).

DISCUSSION AND SUMMARY

Figure 13 shows the proposed regimes of:

- (1) Thermal comfort,
- (2) Sustainable cold stress unprotected,
- (3) Sustainable cold stress with passive thermal protection such that the convective heat loss is reduced to a value of 25% of that unprotected,
- (4) Beneficial shivering for a "normal" $PACO_2$ of 40 mm Hg,
- (5) Beneficial shivering for a moderate level of overventilation, and
- (6) Detrimental shivering.

The regime of thermal comfort has been well documented. The proposed regime of sustainable cold stress unprotected appears to be supported by the actual exposures reported on by Kuehn and Zumrick (1978), assuming that the T_{re} plateau, and approaching plateau, observed at 700 and 870 FSW respectively at 20 deg C ambient temperature are actually representative of states of equilibrium cold stress, and are not artifacts or interim stages of progressive hypothermia. In this regard, whether the level of oxygen metabolism representative of this equilibrium can be sustained for extended periods of time is of critical importance. The model of Wissler (this workshop) uses a level of oxygen metabolism (and heat generation) that within a certain period of time leads to fatigue and the cessation of shivering, quickly followed by the rapid onset of hypothermia. Indeed, the level of metabolism of the aforementioned subjects of Kuehn and Zumrick is not known, nor do we know what level of shivering can be maintained for long periods of time without the onset of "shivering fatigue".

Allowing for conservative values of T_{exp} vs T_{insp} and a moderate amount of over-ventilation, it appears that as long as the ambient temperature is at least that of the temperatures of the USN Minimum Safe Inspired Gas Temperature Limits (1972), the onset of shivering should prove beneficial in warding off the onset of hypothermia. However, for pressures and temperatures bounded by a line constructed between the points (800 FSW/0 deg C) and (1000 FSW/4 deg C) we begin to enter a Regime of Detrimental Shivering. In essence, it is where respiratory heat loss will surpass metabolic heat production, and regardless of the amount of passive body thermal protection, unless supplementary respiratory thermal protection is also provided (such as that described by Hamilton, this workshop) the onset of hypothermia will progress very quickly.

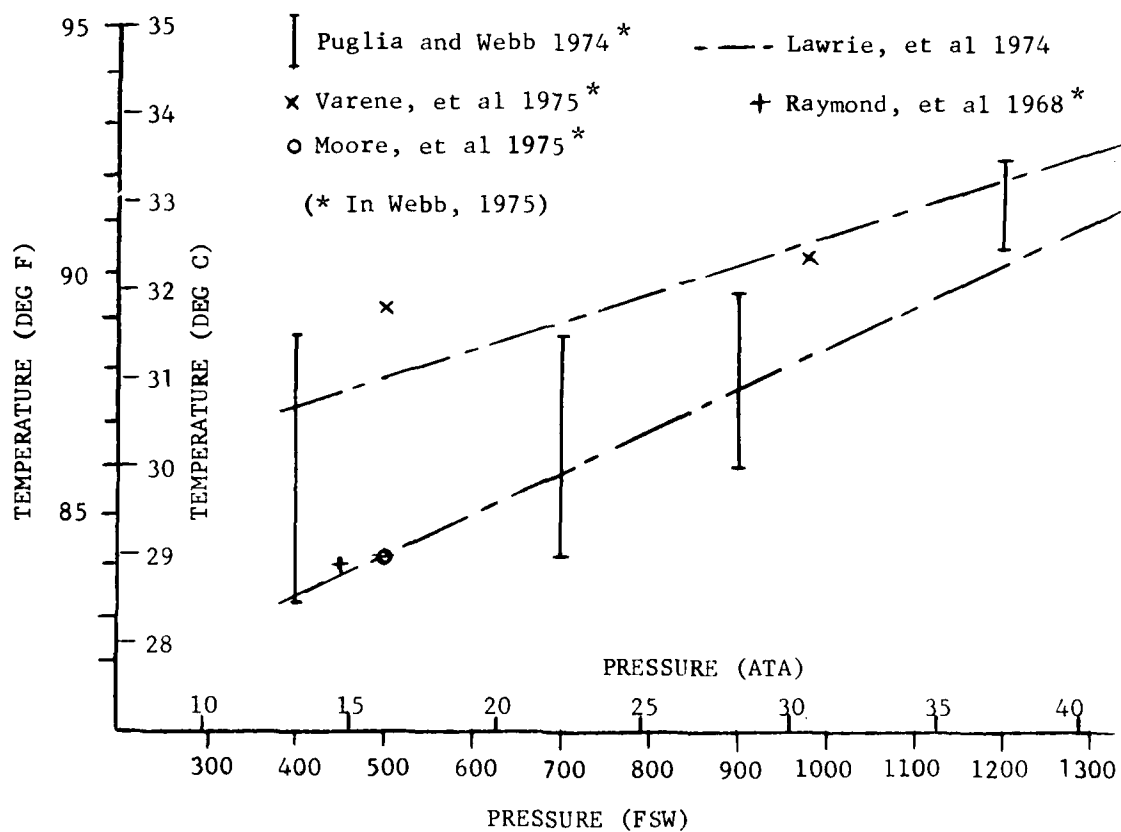


FIGURE 1 - TEMPERATURE OF THERMAL COMFORT IN HYPERBARIC HELIUM

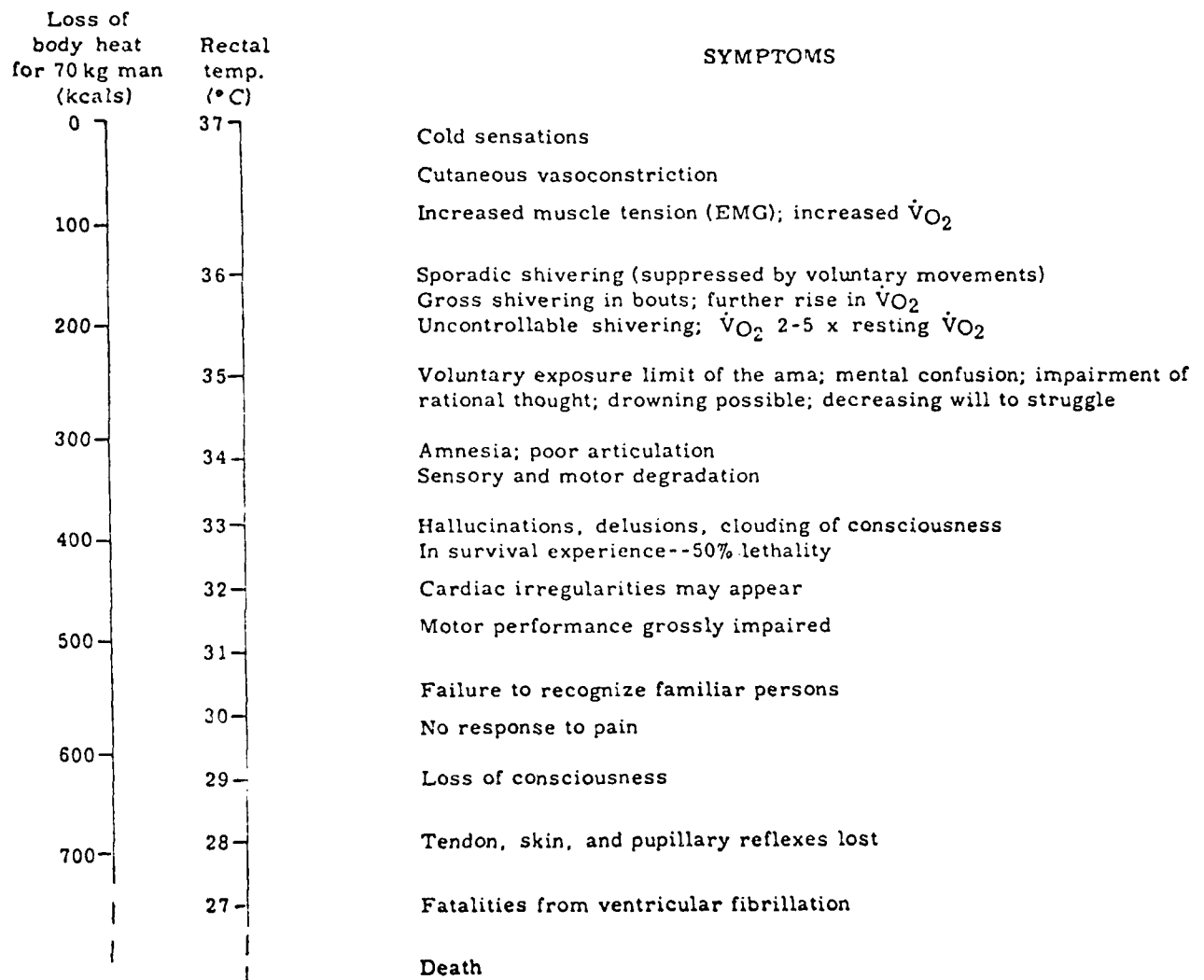


FIGURE 2 - BODY HEAT LOSS, T RE AND SYMPTOMS (WEBB, 1976)

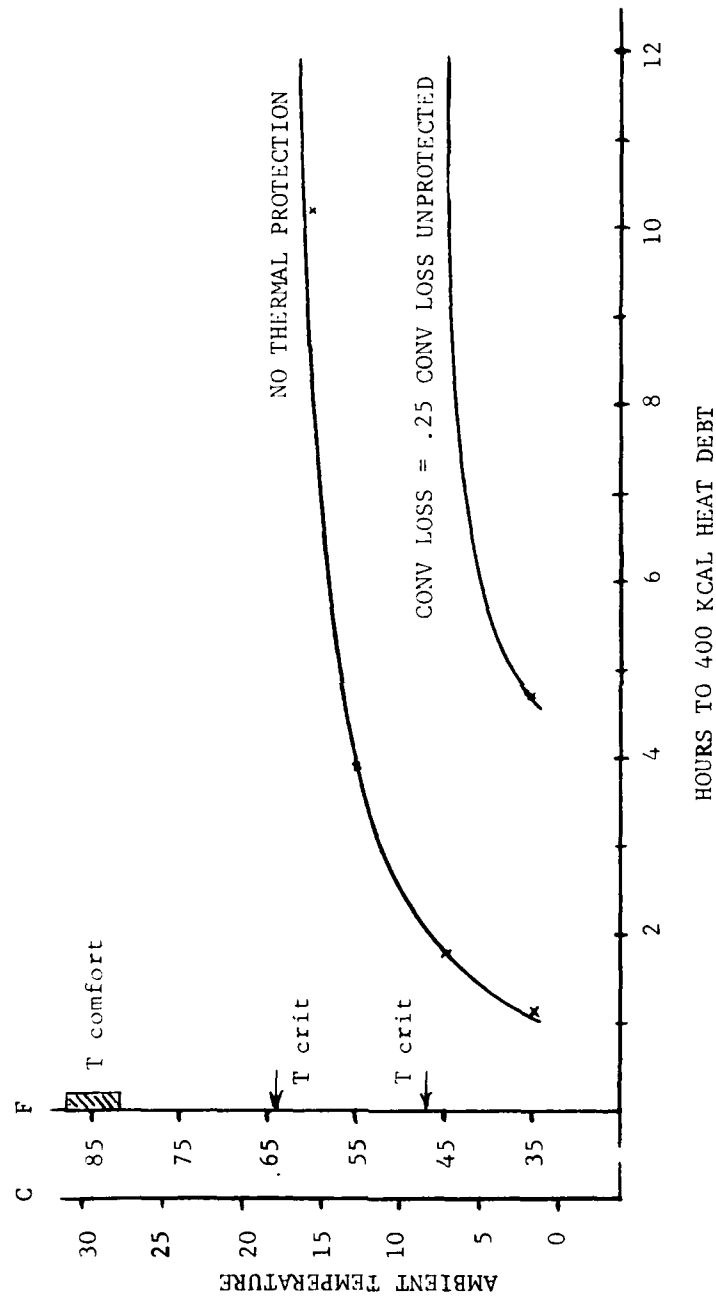


FIGURE 3 - CRITICAL TIME/TEMPERATURE FOR 400 FSW HE/O2 ATM

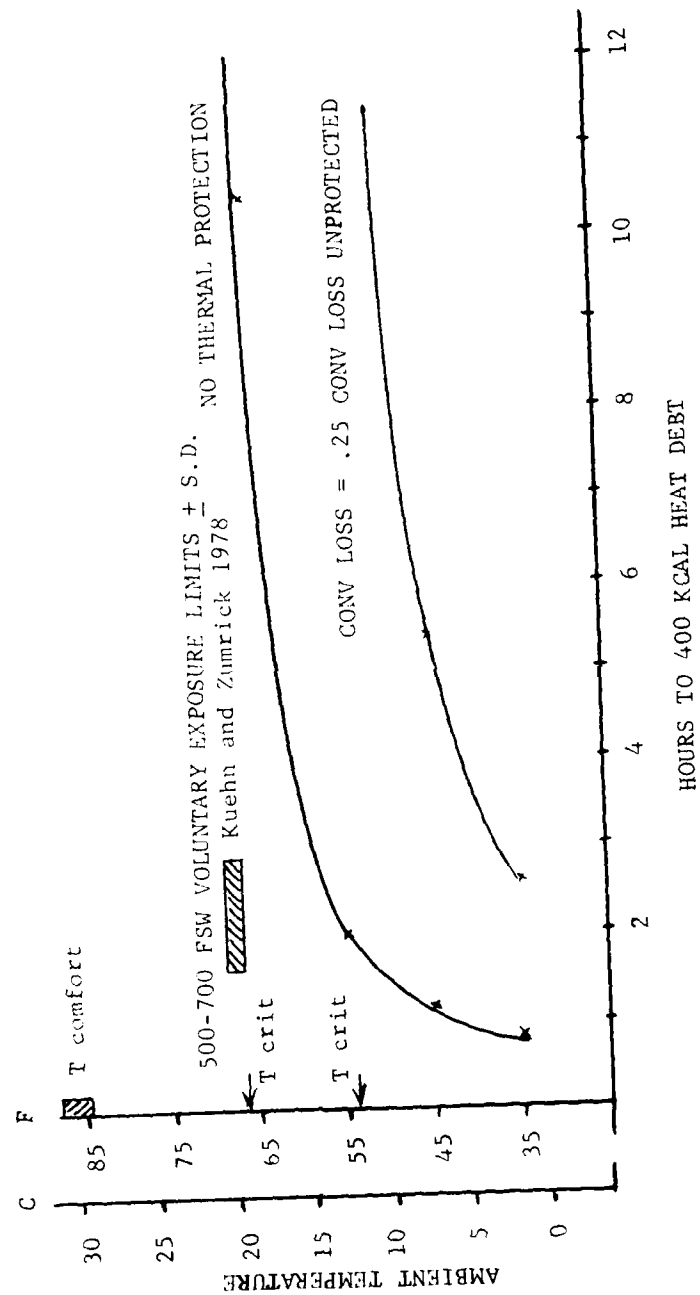


FIGURE 4 - CRITICAL TIME/TEMPERATURE FOR 600 FSW HE/O2 ATM

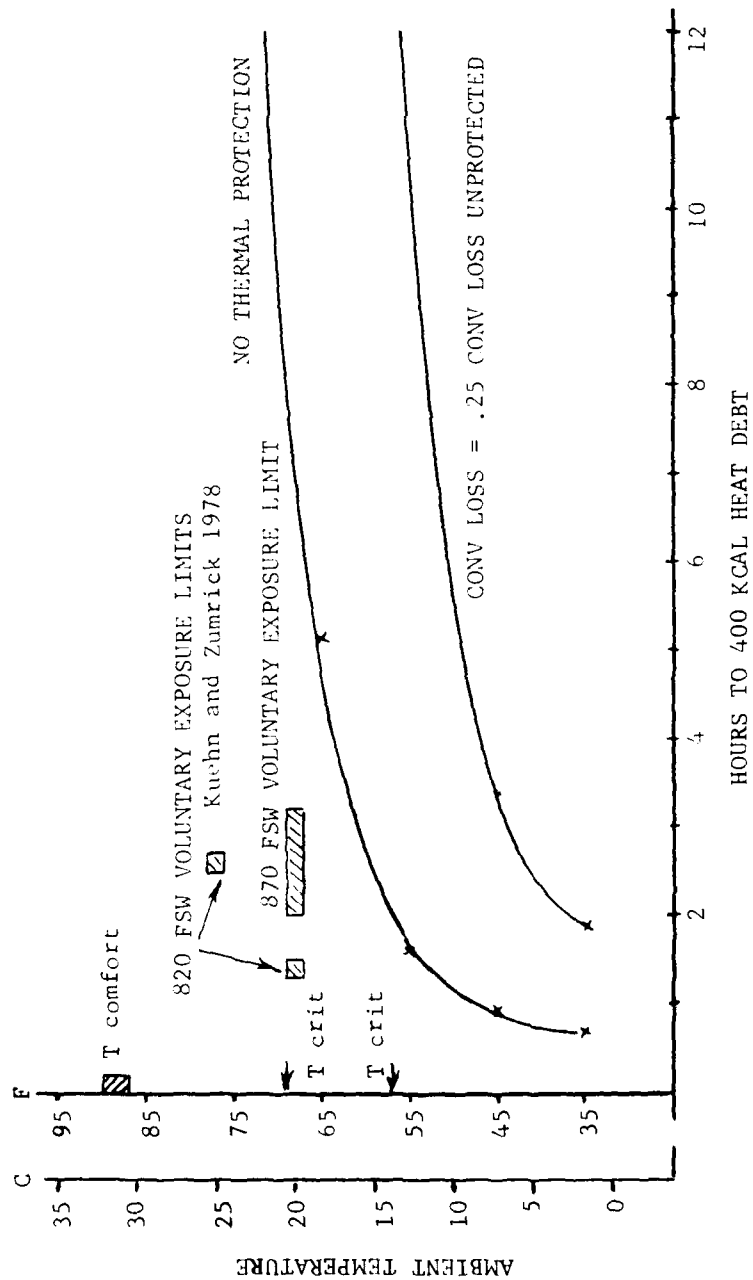


FIGURE 5 - CRITICAL TIME/TEMPERATURE FOR 800 FSW HE/O2 ATM

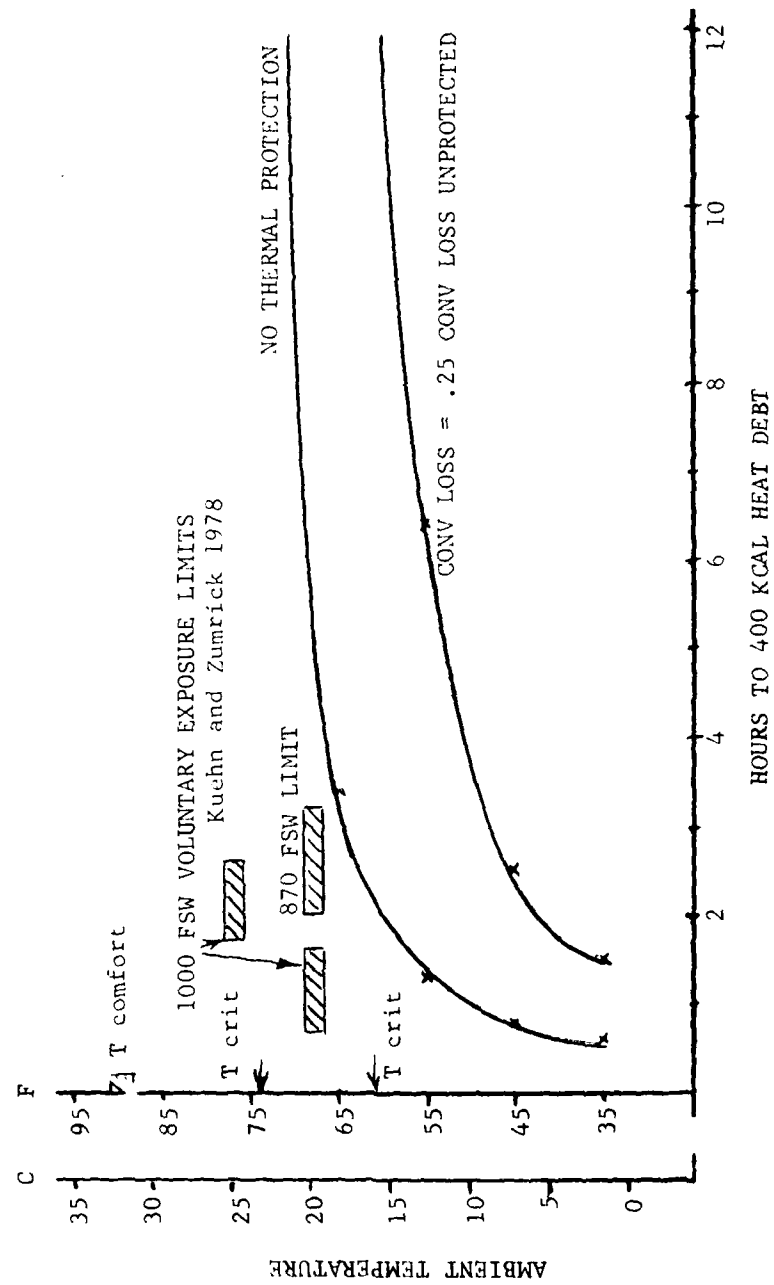


FIGURE 6 - CRITICAL TIME/TEMPERATURE FOR 1000 FSW HE/O2 ATM

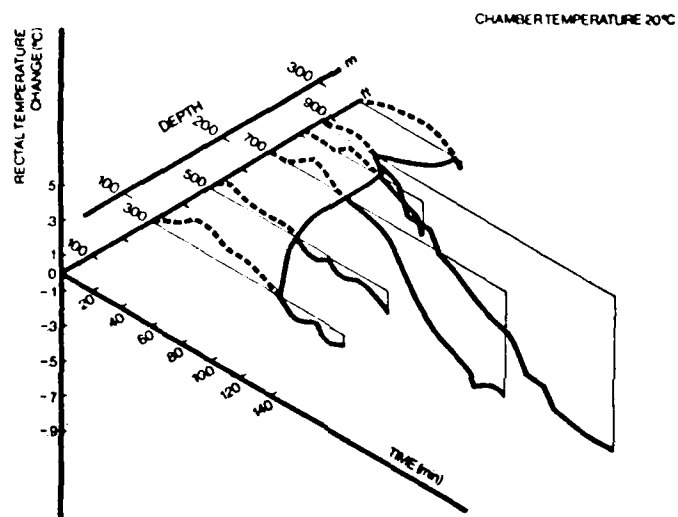


FIGURE 7 - THREE-DIMENSIONAL GRAPH OF VARIATION OF AVERAGED SUBJECT MEAN RECTAL TEMPERATURE VS TIME AND DEPTH (PRESSURE) FOR AMBIENT OXY-HELIUM ENVIRONMENTAL TEMPERATURE OF 20 DEG C (KUEHN AND ZUMRICK, 1978)

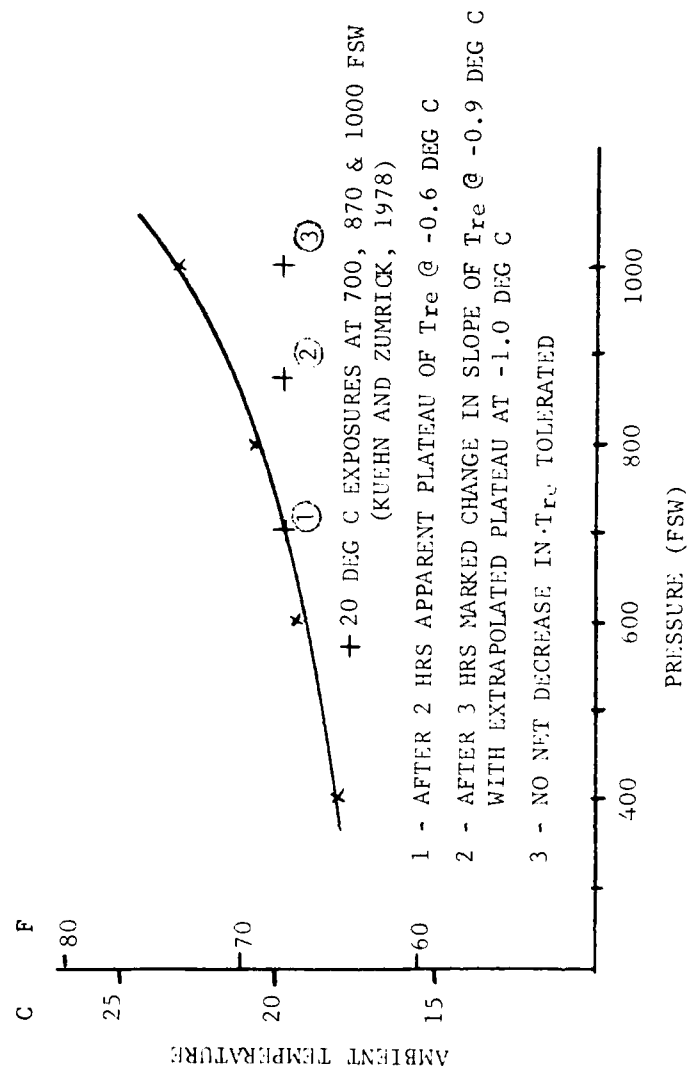


FIGURE 8 - PLOT OF CALCULATED CRITICAL TEMPERATURES AS A FUNCTION OF PRESSURE FOR THERMALLY UNPROTECTED PERSONS IN HYPERBARIC HELIUM AND POINTS OF THERMAL TOLERANCE EXPOSURES FOR THERMALLY UNPROTECTED INDIVIDUALS AT 20 DEG C AMBIENT TEMPERATURE.

TABLE I - TYPICAL METABOLIC RATES FOR RESTING AND SHIVERING STATES

<u>THERMAL COMFORT AT REST</u> <u>KCAL/HR FOR 1.8 M²</u>	<u>COLD STRESS/SHIVERING</u> <u>KCAL/HR FOR 1.8 M²</u>
93 (a)	278-557 (a)
72-108 (b)	180-270 (b)
72 (c)	270 (d)

(a) Shilling, et al 1976, p.231

(b) Webb, 1975

(c) Seagrave, 1971

(d) Raymond, 1977 -- shivering but otherwise resting

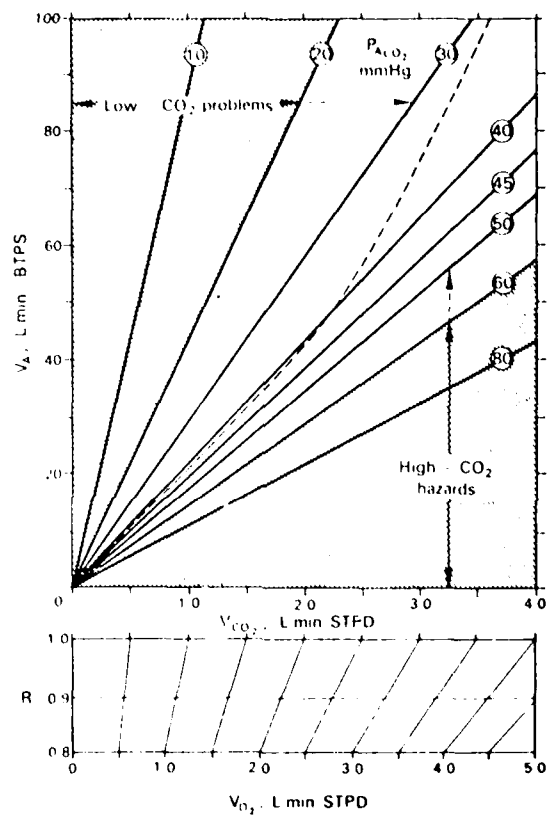


FIGURE 9 - RELATIONSHIP DETERMINING P_{ACO_2} AS A FUNCTION OF ALVEOLAR VENTILATION AND CO_2 PRODUCTION (LANPHIER, 1969)

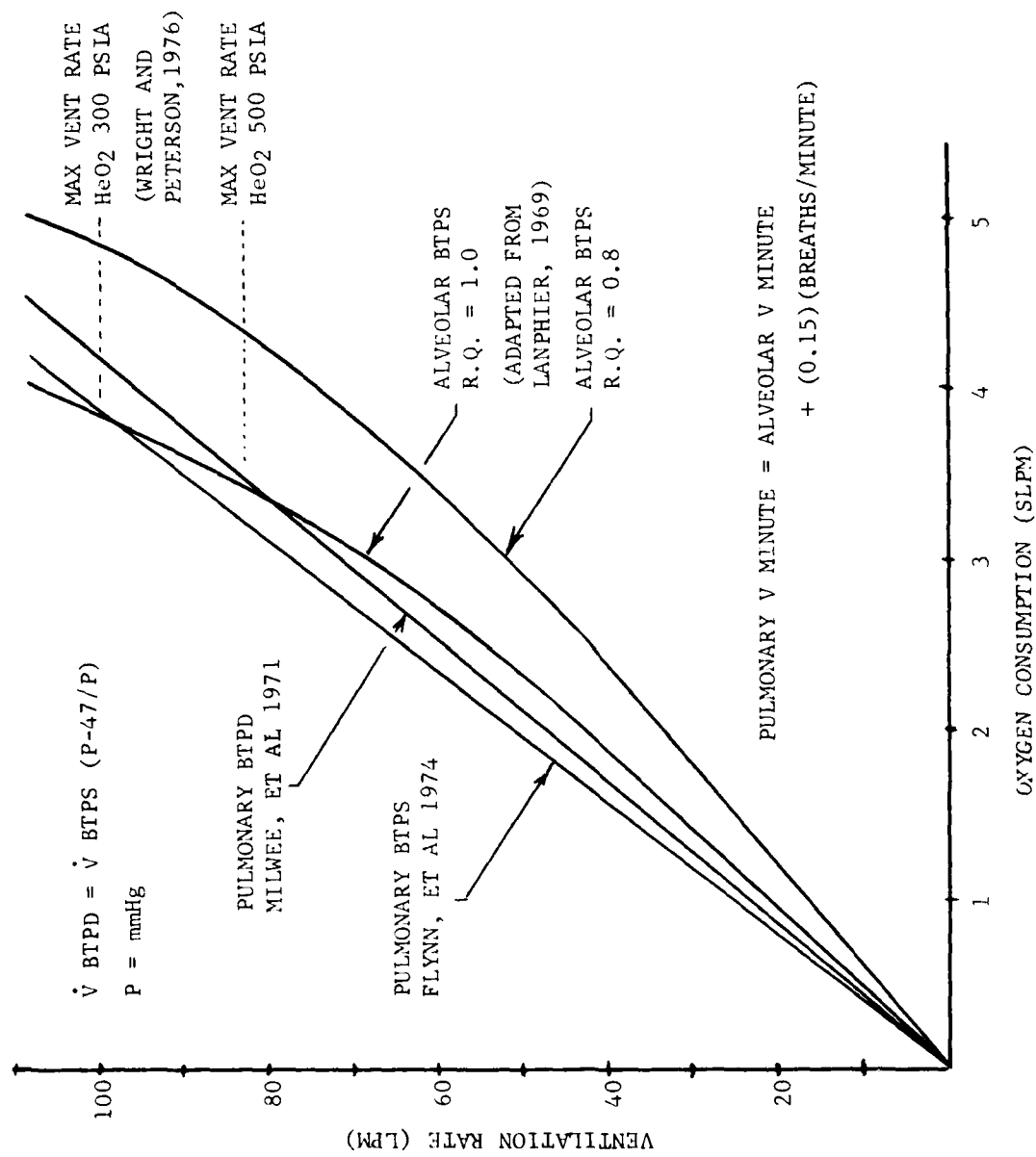


FIGURE 10 - RELATION BETWEEN RESPIRATORY VENTILATION AND OXYGEN CONSUMPTION RATES

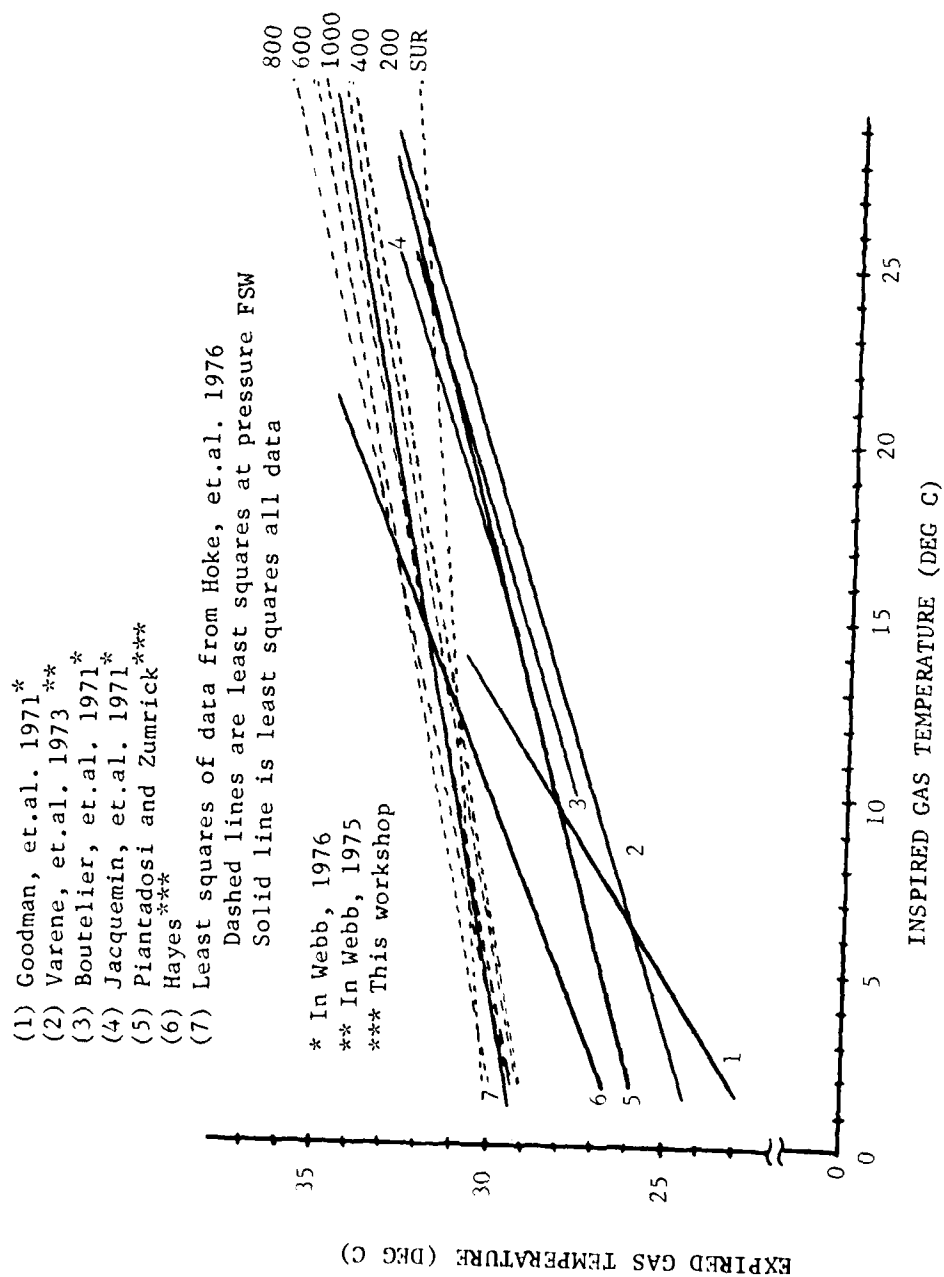


FIGURE 11 - RELATIONS BETWEEN INSPIRED AND EXPIRED GAS TEMPERATURES

TABLE II
LEAST SQUARES INTERCEPT AND SLOPE (DEG C)
OF INHALATION/ EXHALATION GAS TEMPERATURES
OF DATA PRESENTED BY HOKE, ET. AL., 1976

SURFACE	ITCP = 29.98 slope = 0.0914
200 FSW	ITCP = 28.87 slope = 0.1917
400 FSW	ITCP = 28.85 slope = 0.1985
600 FSW	ITCP = 29.73 slope = 0.2023
800 FSW	ITCP = 28.97 slope = 0.2783
1000 FSW	ITCP = 29.10 slope = 0.2147
ALL DATA	ITCP = 29.25 slope = 0.196

TABLE III
DENSITY OF 3% O₂-HE (GM/LITER)

	40 DEG F (4.44 C)	50 DEG F (10.0 C)	60 DEG F (15.6 C)	70 DEG F (21.1 C)	80 DEG F (26.7 C)
400 FSW	2.762	2.708	2.657	2.607	2.559
600 FSW	4.025	3.947	3.872	3.800	3.730
800 FSW	5.280	5.178	5.080	4.985	4.894
1000 FSW	6.527	6.401	6.281	6.164	6.051

TABLE IV
INCREASE IN RESPIRATORY HEAT LOSS (KCAL/HR)
DUE TO A 17 LPM INCREASE IN VENTILATION RATE

	40 DEG F (4.44 C)	50 DEG F (10.0 C)	60 DEG F (15.6 C)	70 DEG F (21.1 C)	80 DEG F (26.7 C)
400 FSW	76	61	48	34	21
600 FSW	110	89	69	50	31
800 FSW	145	117	91	65	41
1000 FSW	179	145	112	81	50

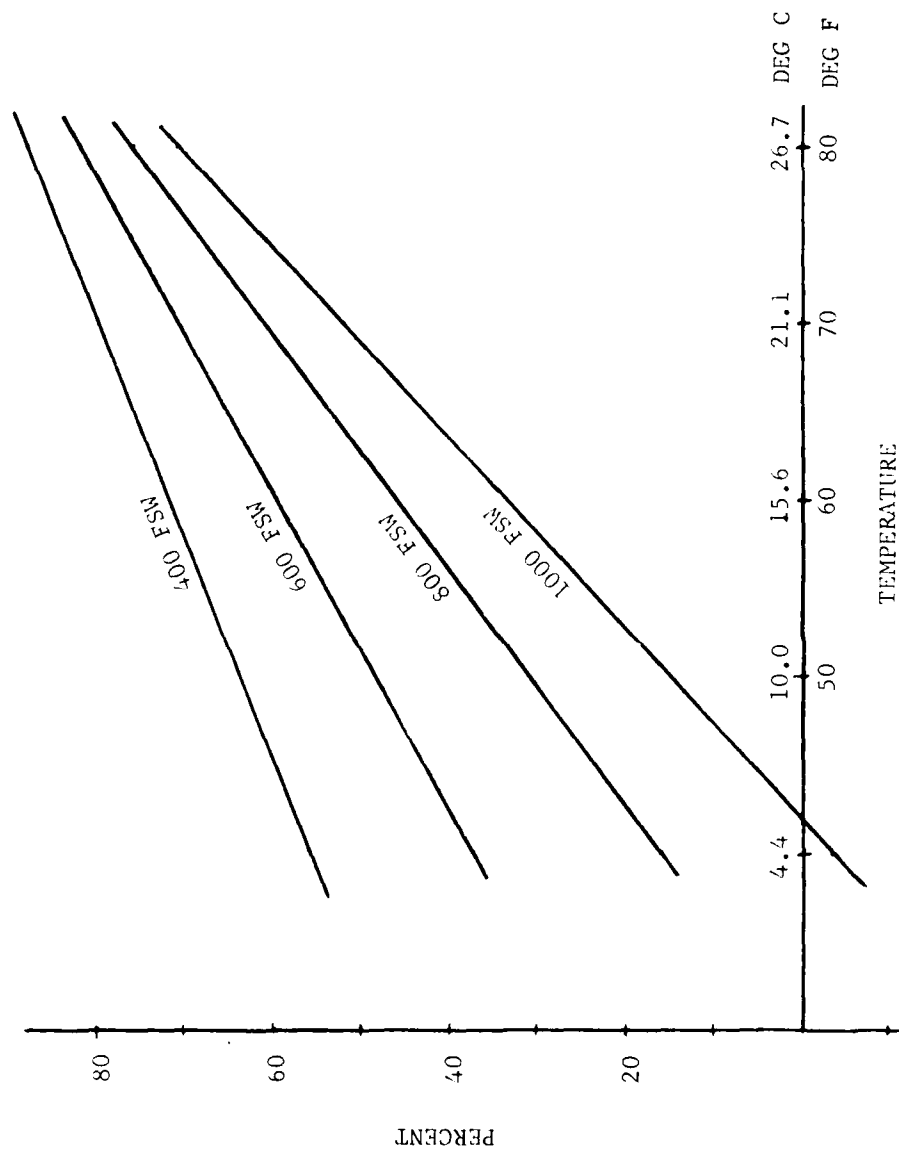


FIGURE 12 - METABOLIC HEAT GAIN LEFT AFTER SUBTRACTING
HEAT LOST DUE TO INCREASED VENTILATION RATE

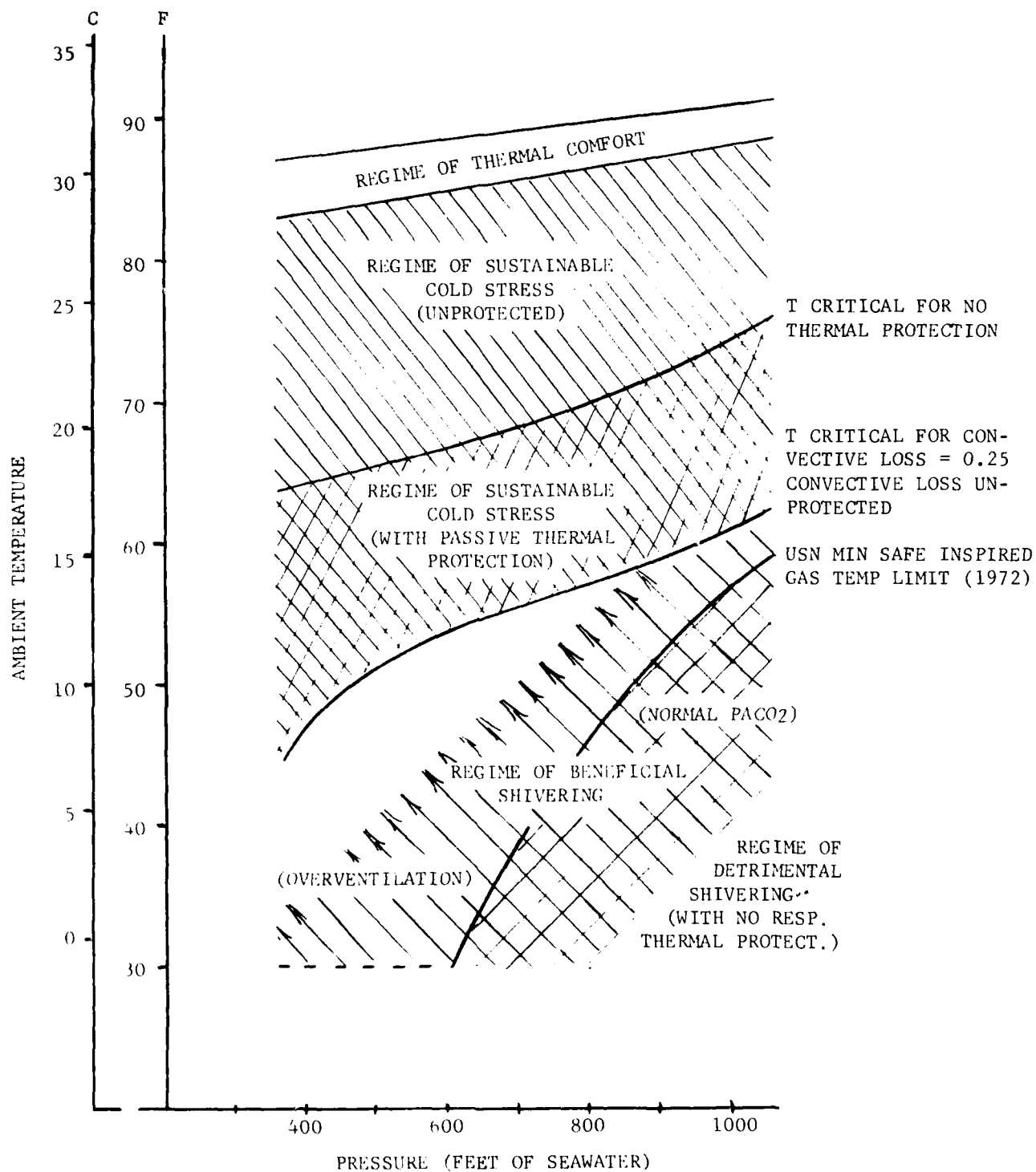


FIGURE 13 - REGIMES OF (1) THERMAL COMFORT, (2) SUSTAINABLE COLD STRESS UNPROTECTED, (3) SUSTAINABLE COLD STRESS WITH PASSIVE BODY THERMAL PROTECTION, (4) BENEFICIAL SHIVERING FOR NORMAL PACO₂, (5) BENEFICIAL SHIVERING FOR A MODERATE LEVEL OF OVERVENTILATION, AND (6) DETRIMENTAL SHIVERING UNLESS RESPIRATORY THERMAL PROTECTION IS PROVIDED.

Webb: Can you explain your estimate of survival time? What do you mean by that?

Schmidt: I calculated my losses at that particular temperature --a 400 kcal loss.

Webb: I was curious to know why you thought that there was external work in shivering.

Schmidt: To be conservative.

Webb: There is no external work in anything unless you're shoveling coal and that all stays on board as heat.

Schmidt: There is also work in moving the gas.

Hamilton: Most of the energy involved stays on board.

Webb: This last part of your paper was saying that you will not lose so much from respiratory heat exchanged that the shivering is diminished greatly.

Hamilton: Unless you hyperventilate, as was his last point!

Webb: And if you have a gas heat exchanger, you block that, so that shivering can't help.

Hamilton: In your earlier curves in which you considered insulation protection, did you consider respiratory protection as well?

Schmidt: No. That was only body or convective protection.

Hamilton: So that curve could be pushed a great deal further yet if you have respiratory protection.

Schmidt: That's right.

Kuehn: Could you speculate on the convective body heat loss with

respect to this point?

Schmidt: Well, I was going to comment on that during your presentation. Are we going to have a still environment or are we going to have the blowers going for CO₂ removal? This will introduce a forced convective component. How much more is the shivering going to increase the gas velocity with regard to the heat losses? Again, we will have some sort of thermal protection. The men will not be nude; this will cut down the convective losses.

Kuehn: That was the point that Frank made earlier but I think that in the worst case you must assume that you can have active ventilation in the trapped bell situation.

Nuckols: How much insulation was worn by those divers in the exposures that were described?

Kuehn: The divers were effectively nude in the experiments which were done in 1976 and 1977.

Nuckols: There were two curves for each graph, one protected and one unprotected.

Schmidt: Those are my curves. What we were doing is using the unprotected dive calculations as the upper limit to the survival time.

Nuckols: What protection level did the others have?

Schmidt: Five years ago, I assumed that the diver was wearing dry underwear in a dry suit and I considered that this would reduce convection losses by about 75% as compared to the nude case.

Nuckols: This determination is based on heat loss rather than resistance values.

Schmidt: That's right.

Hayward: Could you describe again your thoughts on the effect of hyperventilation over that of the ventilation required for shivering? Was it CO₂ buildup?

Schmidt: As I've never put people in cold baths, you have a better understanding of overventilation beyond normal demand in this situation.

Hayward: I couldn't see a problem with the sort of cold-induced hyperventilation that is familiar in initial immersion in cold water but I can see that of CO₂ accumulated; then, yes, you would have a hyperventilation there.

Zumrick: In fact, we have seen that in certain body types. Individuals who have low amounts of body fat and very poor insulation will sometimes ventilate out of proportion to their intake of oxygen.

Hayward: I've never seen that when measuring RQ's and oxygen consumption in subjects at the water surface and I would play down the chance of ongoing cold-induced hyperventilation in the hyperbaric environment. I could see a CO₂ buildup cause and that is a possibility in that environment. Another question that I would like to ask is, in your curves estimating survival time to temperature and depth, one for example at 1000 fsw and at 35°F, you had an estimated survival time with protection of around two hours. This is a devil's advocate view but I find that very hard to imagine. With a dry suit on and dry underwear as well, it is known that in 0°C water, when subjects wear survival suits, they last six hours or more without losing more

than 1°C core temperature.

Schmidt: The difference is respiratory heat loss.

Hayward: Right, but in your calculations, with shivering only, and inducement of higher ventilation rates, you haven't shown that extra heat loss offsets the advantage from shivering.

Schmidt: Well, we will also have convective surface losses. We are having total losses which are above and beyond what we are generating metabolically. We are in a negative situation. The calculations say that within two hours we will have a negative heat debt accumulated to 400 kcal or a core temperature of 33°C.

Hayward: You would estimate survival time as the time required for the core to drop to 33°C. Is that it?

Schmidt: Yes. The basis for these survival curves was the time taken for the core to drop to 33°C, assuming that this temperature was analogous to a heat debt of 400 kcal, which was a value taken to be conservative. The persons would not be able to take care of themselves and, if improperly rewarmed, they could have problems.

Hamilton: It's not proper to use the term "survival" on that curve. The term should be "incapacitated" because these people have a long way to go yet, if handled properly.

Hayward: Wouldn't Paul Webb say that the loss of 400 kcal could come from areas that did not result in a core temperature of 33°C?

Webb: Well, the chart that Tom Schmidt used was published in the Proceedings of Fifth Underwater Physiology Symposium and I had my fingers crossed when it was published. It was intended for illustra-

tion purposes and I have changed my mind as to the relationship between rectal temperature and heat loss, particularly in this situation where the heat loss is steady and relatively slow compared to the case of cold water immersion. Nevertheless, the bell is a severe situation and I don't dispute the general tenor of argument that, in this hyperbaric environment, heat loss is desperately fast and extremely hard to insulate against. This reflects what we are going to hear later.

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An Analysis of the "Lost Bell" Problem

by

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Several recent fatal accidents have called attention to the fact that divers operating under saturated conditions in a personnel transfer capsule (PTC) are critically dependent on heat provided from the surface. When that supply of heat is interrupted, the temperature in the capsule decreases rapidly to a level only a few degrees warmer than the temperature of the ocean. Since conventional diving practice involves the use of hot water suits which provide only minimal passive thermal protection when the supply of hot water is interrupted, divers are subjected to extreme cold stress when the umbilical connecting the PTC with the surface support vessel is severed. Background information relating to this problem is contained in other papers presented at the workshop by Kuehn [1], Schmidt [2], and Long [3].

Conditions surrounding an accident can vary greatly depending on the specific circumstances. Although the method of analysis presented in this paper can be applied to many different cases, detailed results will be presented for one set of conditions which are typical of those existing in the North Sea. The following assumptions summarize these conditions:

1. There is complete loss of contact between the PTC and the surface support vessel.
2. The bell contains two divers.
3. The bell is 1.7 m inside diameter, has a steel wall 20 mm thick, and is insulated on the outside with a 22 mm thick layer of foam.

4. The atmosphere within the bell is 20 ata heliox initially at a temperature of 28°C.
5. The temperature of the ocean is approximately 2°C.
6. Survival for at least 24 hours is necessary.

Several experimental studies relating to the lost bell problem have been published. Kuehn, et al [4] reported results from a 24-hour survival test conducted in the Canadian Forces Submersible Lockout Vehicle, SDL-1. This test involved submersion at a depth of 40 feet in the 4°C waters of Halifax Harbour. The atmosphere within the vessel was 1 ata air and on-board power was used to operate CO₂ scrubbers. Both environmental and physiological variables were monitored during the test. Although none of the subjects became hypothermic in this test, it must be recognized that conditions were milder than those expected in a typical lost bell accident; just changing the breathing gas from 1 ata air to 20 ata heliox greatly increases the rate of heat loss, even though other factors remain unchanged. However, this test is valuable because it could serve as a prototype for a direct experimental study of thermal changes occurring during a lost bell accident.

Tonjum, et al [5] tested several personal survival systems in a 16 ata, 90 percent helium environment. The gas temperature varied from 2 to 6°C, and it was found that only the heaviest arctic-type bags allowed subjects to remain in the chamber more than a few hours.

While the studies reviewed above provide much useful information, they do not answer several important questions. In particular, the individual studies tend to provide only fragmentary results relative to the lost bell problem, and a rational analysis is necessary to assemble these partial results into a comprehensive study from which valid conclusions can be drawn. The purpose of

this paper is to provide the necessary theoretical framework. Hopefully, this analysis will be helpful in defining the true severity of the problem and in developing procedures which enhance the probability of survival in future accidents.

In analyzing this problem, one must consider a number of factors, each of which potentially affects in a significant way the final conclusions. When there are no external sources of heat available, the divers must generate metabolically all of the heat which is transferred to the sea. If there is sufficient thermal resistance between the divers and the sea, they will be able to maintain body temperatures within an acceptable range; otherwise, generalized cooling will occur. The following factors must be taken into consideration.

1. Excessive loss of heat becomes life threatening to the divers.
Enhanced heat generation owing to shivering, and vasomotor action serve to reduce the rate of bodily heat loss.
2. Heat transfer from the body can be reduced by increasing the thermal resistance of the diver's garment.
3. The gas space which surrounds the divers can provide an appreciable thermal resistance between the external surface of the garment and the interior surface of the bell. However, natural convection and heat transfer by radiation tend to reduce the resistance of the gas space.
4. If the bell is warm when contact with the surface is lost, several hours will pass before the walls cool to nearly the temperature of the sea. Even after the wall cools, it still provides some thermal resistance. Rigid foam applied to the exterior of the bell can significantly prolong the cooling time and enhance the thermal

resistance of the wall.

5. Although it is generally small, there will be some thermal resistance at the bell-sea interface.

The manner in which each of these factors is incorporated into the complete analysis will be described before typical results are presented.

The most complex aspect of the analysis involves evaluating the physiological responses of divers when subjected to prolonged, severe cold stress. This is accomplished using a comprehensive mathematical model of the human thermal system as described in a companion paper [6]. The reader should consult that paper for a detailed discussion of this aspect of the problem.

In all cases divers will be wearing some kind of thermal garment. The worst case is probably represented by an unheated hot water suit and the best case by the heavy "survival bags" currently employed by some diving contractors. Any useful model must be capable of describing the garments that are likely to be used. Since the form of thermal energy balance used for regions within the body is also applicable in regions occupied by clothing, one can simulate a garment by simply adding shells outside of the body to represent various layers of clothing. The great flexibility provided by this approach makes it possible to analyze the suitability of a variety of garments for a particular application.

Considerable progress has been made during the past five years in characterizing the thermal properties of various diving suits. The current state of knowledge in this area is summarized in a series of papers [7,8,9] presented at the 1978 Winter Annual Meeting of the American Society of Mechanical Engineers. Interested readers should refer to the original papers for complete information. Two significant conclusions which can be drawn from these papers are: (1) the thermal properties of a given garment can be defined in terms of its constituent

materials [9], and (2) the required properties for many materials are now available.

The thermal resistance of a given clothing layer depends on the thickness, thermal conductivity, and surface area. For compressible materials, such as the closed-cell neoprene commonly used in wet suits, the thickness and thermal conductivity both depend on the ambient pressure. Furthermore, the thickness and thermal conductivity of most clothing materials change when the material is squeezed. The effect of compression on the thickness and thermal conductivity of closed-cell foams is described rather well by theoretical equations. Open-cell foams and fibrous materials are unaffected by pressure changes, but they do compress when squeezed. Unfortunately, there is no theory which describes the variation of thickness and thermal conductivity when a porous material is subjected to an external compressive force, and each material must be evaluated experimentally. An extensive series of tests on commercially available foam and fibrous materials was conducted during 1978 at the Navy Clothing and Textile Research facility in Natick, Massachusetts [7]. It was found that all materials which had an uncompressed density less than 0.12 gm/cm^3 compressed to less than one-half of the original thickness when subjected to a compressive stress of 2 psi, with most of the change occurring during the first 0.5 psi. Higher density foams compressed to approximately 70 percent of the original thickness under the application of 2 psi.

Another point that needs to be made about the thermal properties of garments is that in an atmosphere of helium and oxygen instead of compressed air, the thermal resistance of a garment decreases markedly owing to the fact that the thermal conductivity of helium is approximately six times that of air. In a series of tests conducted using a heated copper mannikin, it was observed that

the thermal insulation of dry suits in helium was approximately one-third to one-half of the value for the same suit in air [9].

By adding heat at an appropriate node in the suit, one can simulate actively heated garments. The model allows one to specify the volumetric flow rate and heat transfer coefficient on each element for hot water suits. Both flooded suits and tube suits have been successfully simulated.

Although helium has a relatively high thermal conductivity, the thick layer of gas surrounding the divers still provides an appreciable thermal resistance. However, this resistance is reduced by convection which always occurs to some extent. Even when the divers are quiet and no blowers are in operation, free convection occurs driven by the temperature gradient that exists across the gas space. This is a complex phenomenon, especially for irregular geometries, but a wealth of empirical information exists in the technical literature. Jacob [10] summarizes results from experimental studies of free convection near plates, cylinders and spheres suspended in an infinite region and between parallel plates. Beckmann [11] studied free convection between concentric horizontal cylinders, and Bishop, et al [12] and Weber, et al [13] measured heat transfer rates between concentric and eccentric spheres, respectively. Mack and Hardee [14] carried out a theoretical analysis of free convection between concentric spheres in the limit of small temperature differences. All of the experimental studies support the notion that the rate of heat transfer varies as $\Delta T^{1.25}$ for small ΔT and as $\Delta T^{1.33}$ for larger ΔT 's, depending on whether the flow is laminar or turbulent.

The experimental data reviewed above provide adequate justification for assuming that the rate of heat transfer across the gas space is defined by the

following equation.

$$Q = A_g \text{KHTC } \Delta T^{1.25} \quad (1)$$

in which A_g = external area of the garment, and

KHTC = a constant which depends on the geometry of the system and the properties of the gas.

In this analysis, the magnitude of KHTC is estimated using experimental data for eccentric spheres. Weber, et al. [13] showed that the rate of heat transfer between vertically eccentric spheres can be expressed as

$$Q = Q_{\text{cond}} (k_{\text{eff}}/k) \quad (2)$$

in which Q_{cond} = rate of heat transfer owing to conduction alone through a material having a thermal conductivity, k , and

k_{eff} = an effective thermal conductivity which accounts for the effect of natural convection.

The thermal conductivity ratio, k_{eff}/k , is generally defined in terms of a Rayleigh number, $N_{\text{Ra}} = N_{\text{Gr}} \cdot N_{\text{Pr}}$, in which N_{Gr} = Grashof number, $g\beta L^3 \Delta T / \nu^2$ and $N_{\text{Pr}} = C_p \mu / k$. For both concentric and vertically eccentric spheres, the correlation between k_{eff}/k and N_{Ra} has the form [12,13]

$$k_{\text{eff}}/k = 0.228 N_{\text{Ra}}^{0.228} \quad (3)$$

The relationships discussed above suggest that the value of KHTC in Eqn. (1) depends on the geometry of the system and the properties of the gas. When the composition, pressure and temperature of the gas are specified, KHTC depends only on geometric factors. Assuming that the bell is a spherical vessel having an inside diameter of 1.7 m leaves only the equivalent diameter and vertical position of the divers as adjustable geometric parameters.

For inside diameters within the range, 100 to 120 cm, and vertical eccentricities no greater than 75 percent of the difference in radii, the value of KHTC falls within the range, $0.078 \leq \text{KHTC} \leq 0.112 \text{ Kcal}/(\text{min} \cdot \text{m}^2 \cdot ^\circ\text{C}^{1.228})$. Hence, a reasonable value to use for KHTC is $0.092 \text{ Kcal}/(\text{min} \cdot \text{m}^2 \cdot ^\circ\text{C}^{1.228})$. Although most of the reported results in this study were computed using $\text{KHTC} = 0.092$, other values were used to evaluate the degree to which conclusions are affected by the value of KHTC. The outcome of this evaluation will be discussed later.

Even though natural convection is the principal mechanism for transferring heat across the gas space, radiation provides a parallel pathway which is not completely negligible. Numerous studies have established that the Stefan-Boltzmann law is directly applicable to the analysis of radiant heat transfer between individuals and their surroundings. Therefore, this aspect of the problem presents no difficulty.

Heat transfer within the wall of the PTC (including any rigid foam applied to the exterior of the bell) is important for two reasons-- it determines the initial rate of cooling within the bell and it makes a non-negligible contribution to the total thermal resistance. Although one could postulate accidents in which the PTC is cold when the umbilical is severed, it is probably more reasonable to assume that the chamber is warm. Hence, the divers have a brief period of time during which they can initiate emergency procedures in relative comfort. The duration of the initial cooling period is determined by the thermal diffusivity of the chamber wall, the temperature of the sea, and the heat transfer coefficient between the chamber and the sea.

Personnel transfer chambers come in various shapes composed generally of spherical and cylindrical sections. For the purpose of this analysis, it is

assumed that the chamber is a spherical steel vessel having an inside diameter of 1.7 m, wall thickness of 1.7 cm, and exterior covering of 2.2 cm - thick syntactic foam. Transient state heat conduction equations are solved for the steel and foam regions, with appropriate boundary and initial conditions. At the interior surface of the shell, the thermal flux is assumed to be uniform and equal to the rate of heat transfer across the gas space divided by the area of the wall. An explicit finite-difference method is used to compute temperatures in the wall. Since the thermal resistance of the foam covering is much larger than that of the steel shell, one can assume that the initial temperature is uniform throughout the steel shell and decreases linearly through the foam covering.

The rate of heat transfer between the external foam surface and the sea is determined by the temperature difference and the ocean current on the bottom. Even when the bell itself is spherical, it is difficult to evaluate the external heat transfer coefficient because of the complex structure which surrounds the bell. In the computer model, the effect of external structures is ignored and the ocean current on the bottom is assumed to be 0.1 knot. Fortunately, the thermal resistance of the bell - ocean interface is small compared to other resistances, and errors resulting from these assumptions should not affect the overall accuracy of the model.

The factors discussed above were incorporated into a comprehensive computer program which was used to analyze a number of specific cases. Results for three cases chosen to illustrate the importance of reducing loss of heat through the respiratory tract are presented in Fig. 1. For each case, the arterial temperature in the thorax is shown in the upper panel, and the mean skin and interior wall temperatures are presented in the lower panel. The times of

occurrence of three events - onset of shivering, loss of 200 Kcal of sensible heat, and collapse owing to fatigue - characterize each case. Shivering, which is always the first of these events to occur, is associated with a rapid decrease in arterial temperature. The time at which a loss of 200 Kcal occurs is indicated by the arrow placed above each arterial curve, and collapse produces a sharp decrease in arterial temperature near the end of exposure.

Eff refers to the efficiency of a regenerative heat exchanger located in the breathing gas line. The case marked $\text{Eff} = 0.0$ represents an unprotected diver, wearing only an unheated, hot-water suit (0.2 clo) and breathing gas at the temperature of the chamber. For this case, shivering begins in less than 2 hours; loss of 200 Kcal occurs in approximately 3 1/2 hours; and collapse occurs approximately 6 1/2 hours after the loss of power. If the only protective device is a regenerative heat exchanger which returns to the diver 75 percent of the excess sensible heat content of his expired gas, all three events are delayed somewhat, with collapse occurring approximately 8 hours after the accident. The maximum benefit derivable from heating inspired gas is illustrated by the upper curve which corresponds to breathing 44°C , saturated gas. This protective measure extends the survival time to approximately 12 hours, but the diver still develops a level of shivering thermogenesis which cannot be maintained indefinitely. These results lead to the conclusion that only heating the breathing gas provides insufficient protection. Nevertheless, some respiratory heating is essential to prevent the excessive secretion of mucus which occurs when cold dense gas is inhaled [15].

If the thermal resistance of the garment is 0.5 clo, which is representative of a rather heavy dry suit in heliox, and a regenerative heat exchanger is used in the breathing gas line, characteristic times result which are similar to those for the maximum respiratory heating case shown in Fig. 1. However, there

are differences between the two cases. When hot gas is inhaled, central temperatures tend to remain high even though the skin cools; and when a heavier garment is worn, higher skin temperatures are maintained while the central temperature decreases.

Since the value of KHTC is not well established, results were computed using a range of values in order to determine how sensitive the conclusions are to the value used. These results are summarized in Fig. 2, where the times required for onset of shivering, loss of 200 Kcal, and collapse are shown for different values of KHTC. It is assumed that the divers are wearing 0.2 clo garments and using regenerative heat exchangers which conserve 75 percent of the excess heat in expired gas. Increasing the value of KHTC has relatively little effect on the thermal response of divers. When the value of KHTC is doubled from 0.092 to 0.184, the time of collapse decreases from 500 to 410 minutes. Decreasing KHTC has a larger effect than increasing it; when KHTC is halved to 0.046, the time of collapse increases to nearly 600 minutes. This is easily understood in terms of the thermal resistance of the gas space, which is inversely proportional to KHTC. When KHTC has its standard value (0.092), the thermal resistance of the gas space is approximately one-third of the total resistance. The garment and the insulated wall of the PTC provide comparable resistances. This is illustrated by halving the thickness of insulation on the outside of the bell, which results in the characteristic times designated by + in Fig. 2. Since the thermal resistance of the wall is somewhat greater than that of the gas space for the standard case, halving the resistance of the wall reduces the characteristic times more than halving the resistance of the gas space. Quantitative results will be presented in

the next section to substantiate these statements. The relevant point to be made now is that the value assigned to KHTC is at most of comparable importance to values assigned to thermal properties of the garment and wall. Increasing the thermal resistance of the garment or wall to levels which provide adequate protection for the divers will reduce the relative resistance (and importance) of the gas space. Hence, uncertainty about the value of KHTC should not invalidate the results of this analysis.

Recent efforts to increase the probability of survival in present day systems concentrate on providing better garments for the divers. Results presented in Fig. 3 show how the characteristic times are affected by the thermal resistance of the garment. Values represented by the three lines were computed assuming that KHTC = 0.092, the garment has arms and legs, and the regenerative heat exchanger has an efficiency of 0.75. For these standard conditions, it appears that a 1 clo garment should just be adequate to allow long term survival. Shivering should begin approximately 6 hours after the accident, but the intensity would not be sufficient to cause fatigue. For reference, note that a stationary cylindrical shell having an inner radius of 17 cm, a thickness of 3.6 cm, and a thermal conductivity of 5×10^{-4} cal/(sec x 6m x °C), which is approximately 40 percent greater than the conductivity of pure helium, has a thermal insulation of 1 clo = 0.18 Kcal/(hr x m² x °C).

Also shown in Fig. 3 are characteristic times for two cases in which the garment has the form of a sleeping bag rather than a conventional suit. This reduces the rate of heat transfer from the arms and legs and results in a moderate increase in the characteristic times, as illustrated for the 1 clo garment under standard conditions. The second special case was defined to evaluate the effect of serious errors in the assumptions concerning the thermal

resistances of the gas space and PTC wall; both values were reduced to 25 percent of their standard values. Results plotted in Fig. 3 show that a 1 clo garment provides inadequate protection under these conditions, and collapse would occur approximately 9 hours after the accident. Henceforth, these will be referred to as the "worst case" conditions.

In designing a system which permits long term survival in an isolated bell, one must take two related factors into consideration. Sufficient thermal insulation must be provided between the divers and the sea, and an adequate source of heat must be available. Under steady-state conditions, these two factors are related to the difference between the mean skin temperature and the temperature of the sea by the following equation.

$$\bar{T}_{\text{skin}} - T_{\text{sea}} = Q R_{\text{total}} \quad (4)$$

in which Q = rate of heat transfer,

$$R_{\text{total}} = \sum_{i=1}^4 R_i = \text{total thermal resistance, and}$$

R_i = resistances of the garment, gas space, PTC wall, and bell-sea interface.

Computed values presented in Table 1 provide information about the relative importance of the four resistances for three different systems in which the garments have resistances of 0.5, 1.0, and 2.0 clo. For each system, temperatures of the skin, outer surface of the garment, inner and outer surfaces of the wall, and the sea are tabulated. Also shown are the relative thermal resistances of the components.

Table 1. Temperature and Relative Thermal Resistances for
Three Garments Used Under Standard Conditions.

	Skin		Garment		Wall _{in}		Wall _{out}		Sea
0.5 Clo									
Temp	24.3	-	16.5	-	11.0	-	3.1	-	2.7
R_i/R_T		0.36		0.25		0.37		0.02	
1.0 Clo									
Temp	27.9	-	14.3	-	9.5	-	3.1	-	2.7
R_i/R_T		0.54		0.19		0.25		0.02	
2.0 Clo									
Temp	29.7	-	11.1	-	7.7	-	3.1	-	2.8
R_i/R_T		0.69		0.13		0.17		0.01	

As the thermal resistance of the garment increases, the relative resistances of the gas space and PTC wall decrease; the resistance at the external surface of the bell is never very important. In a system consisting of a 1 clo garment with gas space and wall resistances as defined for standard conditions, the garment provides one-half of the total resistances. If the resistances of the gas space and wall are reduced to 25 percent of their standard values, the total resistance decreases to 65 percent of the original value. However, if the resistance of the garment is 2 clo, a similar reduction in the resistances of the gas space and wall reduces the total resistance to 77 percent of the original value, which should still be adequate. Therefore, to be reasonably safe a passive system should include a garment which has a thermal resistance of at least 2 clo.

The alternative to increasing R_{total} in Eqn. (4) is to increase Q through active heating. Prototype units have been built for several emergency heat sources, but none is commercially available at the present time. Nevertheless, it is constructive to define basic characteristics which an efficient actively heated system would have.

It is apparent from Eqn. (4) that both a source of heat and thermal insulation are required to maintain a sufficient temperature difference between skin and sea. Therefore, a survival system which has minimal insulation requires a large heat source; and a system which relies on metabolic heat requires excellent insulation.

Previous theoretical and experimental studies [16,17,18] have established that 10 Kwatts of power are required to heat an entire bell, but this is way beyond the capability of any proposed emergency heat source. By supplying heat as close to the skin as possible, one maximizes the thermal resistance between the source and the sea and, thereby, achieves the largest possible temperature difference per watt of power supplied.

These considerations suggest that it would be best to heat the divers using a closed-circuit, hot water suit worn under the survival garment. If the divers are already wearing closed-circuit suits, they would only have to switch to the emergency hot water supply. If they are wearing suits normally used in the open-circuit mode, adaptation for closed-circuit operation would be required.

For the purposes of this study, it is assumed that the divers are wearing free-flooding hot water suits which have been modified for closed-circuit use. This assumption was used because experimental data are available to define parameters and validate the model for simulating actively heated garments. However, the conclusions should not be strongly dependent on assumptions about

the kind of heated garment.

It is also assumed that the divers control heat input to the suit to minimize energy requirements. In the model this was accomplished by regulating the inlet water temperature so that the divers are cool, but not shivering. Whenever shivering occurs, the inlet temperature is increased gradually until shivering ceases. It is also assumed that the divers use regenerative heat exchangers to minimize respiratory heat loss.

The first system to be discussed is the conventional hot water suit, worn without an external garment. A thermal insulation of 0.07 clo was assigned to the crushed foam outer shell of this garment. When standard assumptions are made about the thermal resistances of the gas space and PTC wall, the power requirement is 250 watts per diver. Using the worst case assumptions, the heating requirement becomes 1 Kwatt per diver.

Several sleeping bag arrangements were studied also. If the divers are protected by one - clo sleeping bags, the heating requirement is only 120 watts per diver under standard conditions and 250 watts per diver for the worst case. The heating requirement for a 0.5 clo sleeping bag for the worst case is approximately 600 watts per diver. Hence, it appears that the power requirement is reasonable if the divers are provided with a moderately heavy outer garment.

Some fairly definite conclusions can be drawn from the results of this analysis. These are summarized below.

1. The thermal resistance at the external surface of the bell is small and assumptions about conditions on the bottom are not critical.
2. In a small bell, the resistance of the wall can be significant, but the effect of windows and hatches needs to be investigated. It

appears that adding foam insulation to the outside of the bell is worthwhile; not only does it provide insulation in the steady-state mode, it also prolongs the initial cool-down period.

3. The thermal resistance of the gas space is smaller than other resistances, but still significant. Since it is small, errors in evaluating KHTC should not invalidate the analysis.
4. Heating the breathing gas either actively or with a passive regenerative heat exchanger prolongs survival times, but it does not provide sufficient protection by itself. Aside from thermal balance considerations, some heating of inhaled gas is essential to prevent excessive mucus formation.
5. A one-clo garment will provide marginal protection if assumptions concerning the thermal resistances of the gas space and wall are correct. However, such a system allows little room for error.
6. A two-clo garment, especially in the sleeping bag configuration, should provide adequate protection.
7. Active heating alone does not appear to be feasible. A heater capable of supporting two divers wearing only closed-circuit hot water suits would have to supply a minimum of 0.5 Kwatt, and a more realistic estimate is 1 to 2 Kwatts.
8. If the divers are protected by one-clo sleeping bags, a 0.5 Kwatt heater should be adequate even under worst case conditions.

It appears that the heavy sleeping bags which are commercially available today may provide adequate protection. A desirable margin of safety could be provided by adding a small heater which supplies 0.5 Kwatt to the divers through a closed-circuit system.

The purpose of this paper has been to provide a rational basis for discussing the lost bell problem. Relevant factors have been defined and evaluated quantitatively using the best information available in the technical literature. Even though some of these factors are subject to considerable uncertainty, final conclusions resulting from this study should provide a sound basis for developing effective survival systems.

ACKNOWLEDGMENT

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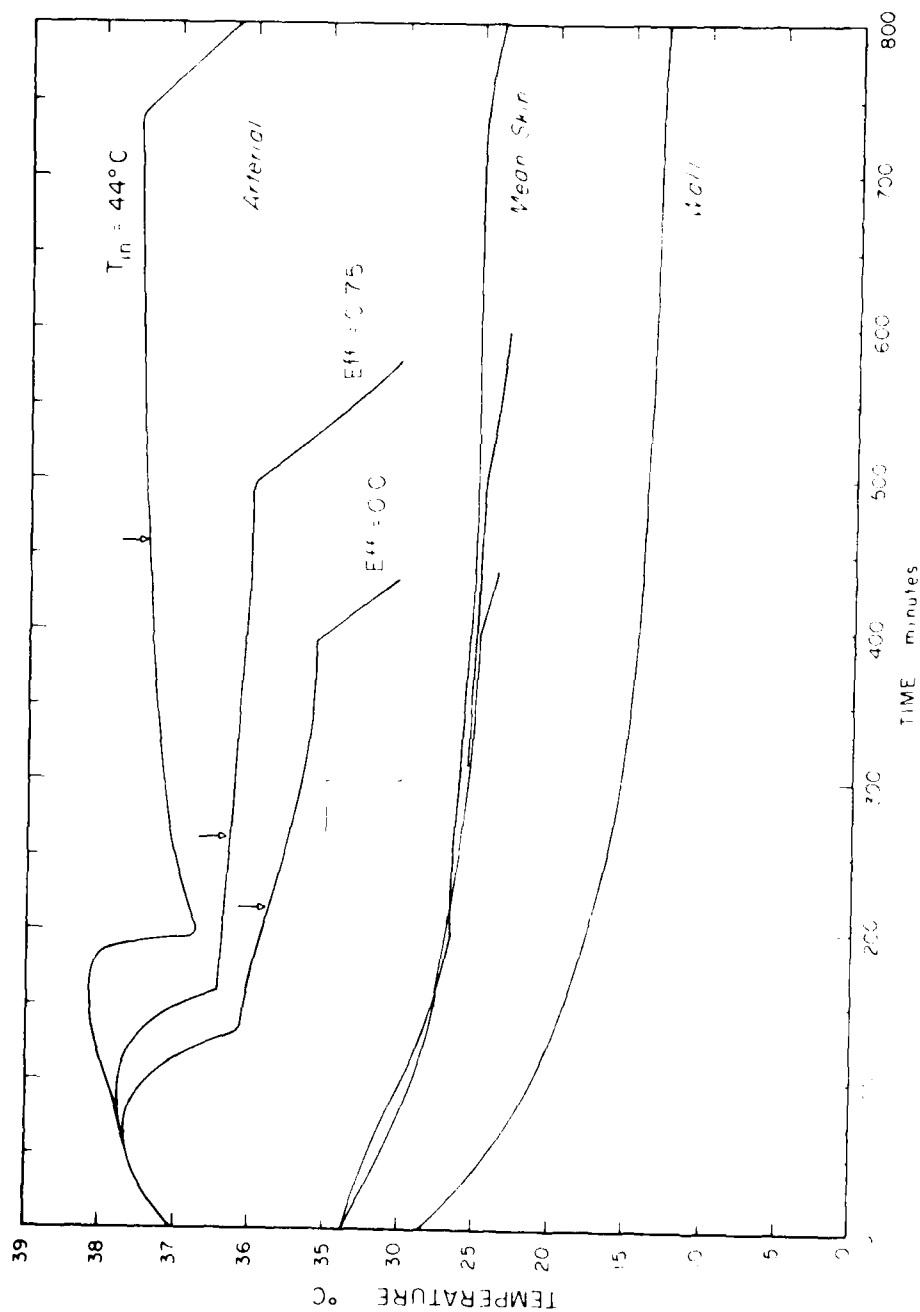


Figure 1. Computed values of the arterial, mean skin, and inside wall temperatures for three cases chosen to show the importance of heating breathing gas. Efficiency of regenerative heat exchanger in the breathing gas line, and arrows mark the loss of 200 Kcal of heat.

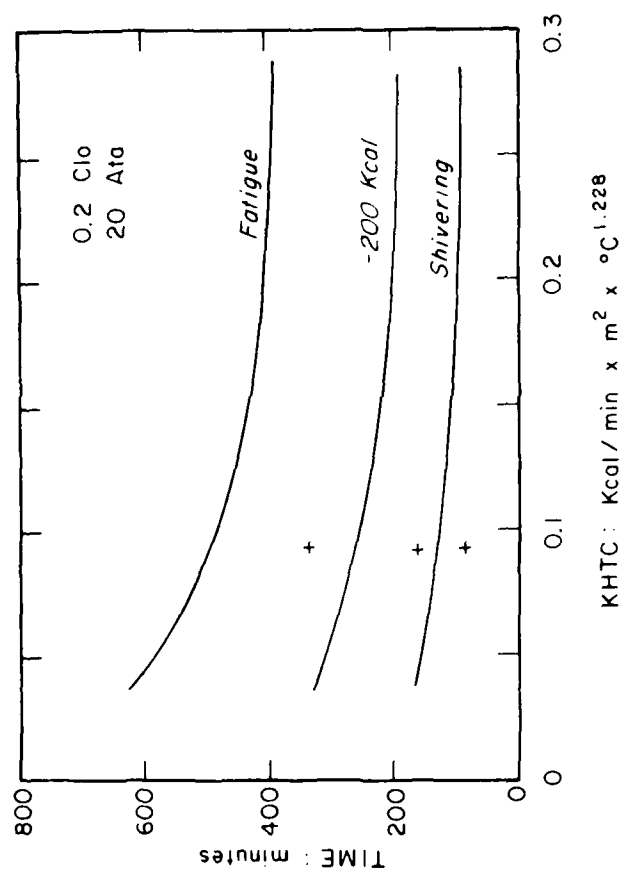


Figure 2. Characteristic times computed for different values of KHTC. The thermal resistance of the gas space is inversely proportional to KHTC.

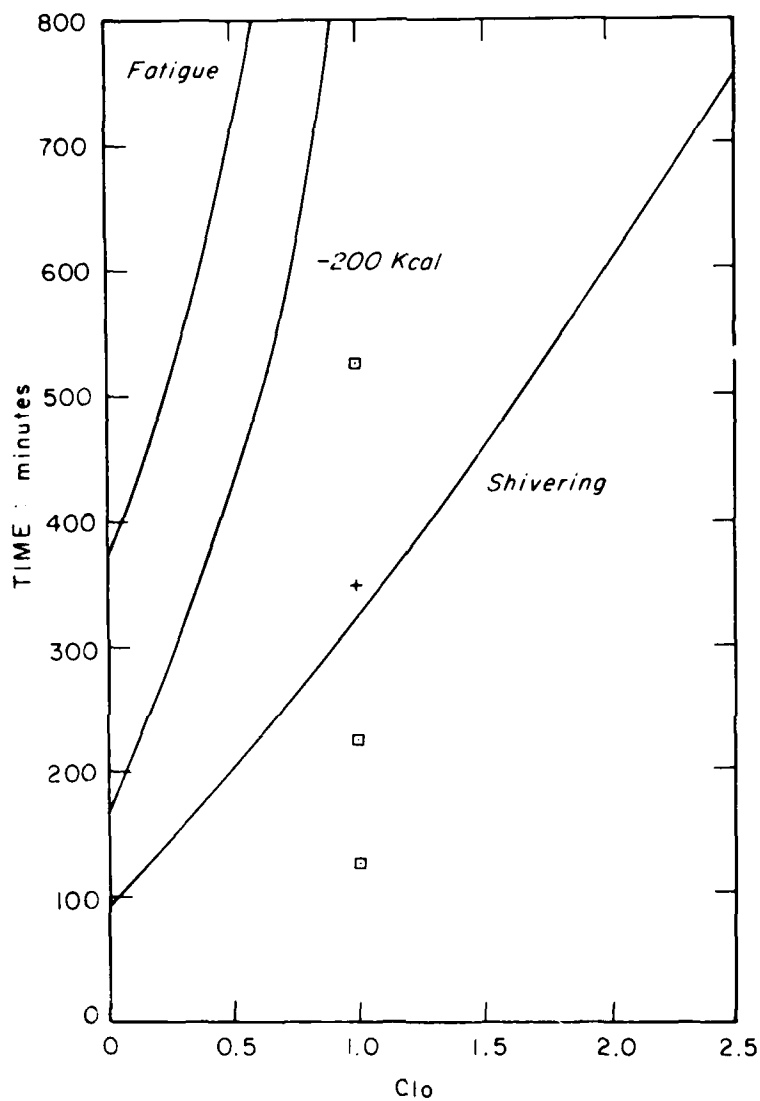


Figure 3. Characteristic times computed for different garments under conditions which are defined more fully in the text. Individual points refer to use of a sleeping bag; + and □ denote standard and worst case conditions, respectively.

Hayward: If with the regenerative heat exchanger but with core temperature around 37°C, they wouldn't be shivering, would they?

Wissler: Yes, they would, even with core temperatures as high as 38°C. Skin temperature would be down to 25°C. I guess the only thing to do, if you don't believe it, is to go into the laboratory and carry out some experiments like this.

Webb: The point about fatigue of shivering after a long time is a very interesting one. I really can't think of any specific data. There is surely fatigue if you shiver hard for a very long time. Whether that fatigue leads to cessation of shivering is unknown.

Wissler: Well, I don't know, either. I can only quote two references, neither of which I have access to. One is a report by Ed Beckman to the effect that you can double your metabolic rate owing to shivering and maintain it for about eight or nine hours. The other one was a French study by Timbal. It was similar although the metabolic rate was somewhat higher and the period of time was somewhat shorter. This is something that really needs to be investigated. If, in fact, that's what happens, then I think in many cases where the diver has inadequate passive protection, that's going to determine the endpoint.

Kuehn: Could you speculate on the benefit of shivering in hyperbaric environments?

Wissler: It's clear that it's of value because when it stops, there is a precipitous drop in temperature.

Kuehn: How do you treat the difference in boundary layer coefficient

with shivering?

Wissler: I multiplied it by an empirical factor.

Hayward: Would you comment, Lorne, on your feeling that shivering should be inhibited, after these last two papers.

Kuehn: Experiments conducted in the laboratory will be the proving ground of whether or not shivering should be inhibited at certain depths in the survival situation. I think that the effect of shivering on the boundary layer coefficient for convective heat transfer is a relatively large factor and that it may be desirable to eliminate or reduce shivering at great depths.

Nuckols: That factor of K_{htc} seems to be so insensitive in terms of convection. Is it saying that it really doesn't make any difference what garment the diver has on?

Wissler: No. The diver is wearing a 0.2 clo garment, so you have that resistance plus the resistance from the outer clothing surface to the inner surface of the bell. The second resistance is affected by both radiation and convection. We are only talking about the convective part.

And then there is the resistance across the syntactic foam. What we are saying is that this one is about 20 or 30% of the total resistance. If you reduce it to 10%, it doesn't make that much difference. If you reduce the resistance of the foam on the outside of the bell by a factor of two, then these points come down to here, again making some difference, but by itself, it's not a critical factor.

Hamilton: When you calculated clo, are you doing so for helium or air? If you take a value for air, the value for helium is only about 1/4 as much.

Wissler: What I am using is the clo value that exists in the environment in the bell (heliox at 20 atm). A value of 0.2 clo in this environment would be about 1.0 clo in air.

Harnett: Were the calculations done with the 15 segment model that you described yesterday?

Wissler: Yes. I took that basic program and added on the simulation for the wall.

Harnett: It seems to me that the effects of shivering are going to be sensitive to ways in which you model the distribution of shivering and thermogenesis throughout the body -- peripheral perfusion effects, peripheral tissue temperatures. Some of these details need to be further understood to really know what the diver's experience is going to be like.

Wissler: That's true. That was the reason that we spent as much time as we did describing our model and its validation under conditions of severe cold stress.

Pozos: You're assuming that the individual is shivering at the same time as you are giving him warm air. What happens if the warm air inhibits the shiver?

Wissler: I'll just have to plead ignorance. My shivering model is the one I described yesterday, which is based on data obtained under normal conditions. If you provide a subject with very warm

fully-saturated gas, then I don't know what that does to shivering. Maybe John Hayward can tell us.

Hayward: It does reduce the shivering thermogenesis by about 1/4 or 1/3.

Zumrick: I'm wondering if the hyperbaric environment changes the body's response to cold because in two subjects, an admittedly small sample, Claude Piantadosi had people in a cold chamber breathing gas in from a warm chamber, attempting to reduce respiratory heat losses to a minimum. In doing that, and comparing those same two subjects to themselves in a similar environment when breathing cold gas, he saw that there was no real change in oxygen consumption between the two exposures.

Webb: Evidently the cold stress was continuing. What John is thinking of during the rewarming is a transient situation where the surface temperatures are allowed to rise as well as the inhaled gas. This fellow here is still in his cold environment.

Hayward: In my case we were locally warming the whole head region, including perhaps the hypothalamus, whereas Bob might feel that it is thermal receptors in the mouth. By whatever mechanism, we've observed, at Simon Fraser University, about a 1/3 reduction in the shivering metabolism, with the skin being the same temperature, with 42°C inhaled.

Wissler: I think that the overall consideration here is that this person is subjected to a severe cold stress. The added heat provided through the respiratory system is not sufficient to compensate for

severe stress. The details of the model may be incorrect, but the net result is probably not going to change markedly if you change some of those assumptions. If you are not providing enough heat to maintain the person, eventually he will cool to the point where the drive for shivering will increase again and the same thing is going to happen.

Hayes: We've played with rewarming people under hyperbaric conditions and there appears to be less shivering when you use the rewarming gas. Skin temperatures that we had are comparable, if a little lower, than the curve of Gene Wissler. The important thing is that over the ensuing time that you breathe the hot gas (although I haven't done it for too long), it shortens the time that you are shivering violently. Maybe you start off at 40 l/min and if you just allow them to shiver for the next hour, they'll go from 40 to 20 to 10 l/min after 30 minutes. If you look at the amount of shivering that occurs during active rewarming, you stop after about 10 minutes, but then you get blips of activity. As the gradients in the body are restored, you go through further shivering periods, then it ceases for awhile, then more shivering etc. It appears to be a very random phenomenon.

In reference to John Hayward's observation of people shivering with elevated rectal temperatures, I can show you people in water certainly who will shiver at a rate of oxygen uptake of 40 or 50 l/min with a rectal temperature of 37.5°C and mean skin temperature

around 20°C. I think we can all agree that shivering responds more rapidly to rate of change of temperature and direction of core temperature change. So you could have somebody with a rectal temperature of 38°C while you're cooling the skin, breathing 40 l/min, and you also can have somebody at 34.5°C rectal temperature who has just done the turn with a rising mean skin temperature of 16°C and he will have a ventilation rate of 10 l/min.

Hayward: At those skin temperatures, yes, but I think that the data that were shown were for mean skin temperatures at 25°C. I was just wondering, at that not-so-cold temperature, about the shivering.

Hayes: What we can go back to again is the convective heat coefficients found at the skin surface at any one point determining the likely rate of cooling at that surface. If you're in water or if you are at 20 bar, say, then that capacity to remove heat is far more for any fixed values of surrounding skin temperatures. I don't know how the body does it but you get the impression that it samples a gradient somewhere.

Kuehn: We have three papers left and they are experimentally oriented. The first is by Stein Tonjum who will describe his Polar Bear experiment that took place earlier this year in January and which triggered so much interest in this work.

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PROJECT POLAR BEAR I

TESTING OF DIVER THERMAL PROTECTION IN A SIMULATED "LOST BELL"

S. Tonjum

Norwegian Underwater Institute

I hadn't intentionally planned to present this but fortunately Bill Hamilton has some slides so I will proceed. The background for the experiment was the North Sea accident in which a diving bell was lost and recovered after some 18 hours. Both divers were dead because of hypothermia. There was a need for some protective gear in the diving bells but there wasn't anything available at that time. There was one prototype system made by Kinergetics and we knew that some of the diving companies used a heat reflective suit in the bell for possible protection in the lost bell situation. Our test conditions were at 16 bars, corresponding to 150 msw, in heliox and a gas temperature of 4-6°C. First slide, please (Figs. 1 & 2, end of paper).

We used the dry part of the wet pot of the NUI chamber as our site for the lost bell simulation. To get the gas temperature down to 5°C, we had to put water in the water portion of the wet pot. Three of our own research people and one medical doctor acted as subjects. Next slide (Figure 3 at end of paper).

We tested different systems of diver protection, survival bags and suits and respiratory regenerators. This is a prototype from Comex - a CO₂ scrubber/regenerator prototype as well as the Hamilton CO₂ scrubber/regenerator, a Norwegian prototype and we also intended to test as well one prototype from DCIEM but it wasn't functioning,

due to corrosion problems. Next slide (Figure 4 at end of paper).

This is the heat reflective suit used in some diving bells which is supposed to be used for surface immersion protection. We used the Comex thermal regenerator with this suit. The diver wearing this suit also used a thin wet suit beneath it along with protective gloves and boots.

All the divers were to go into the cold chamber and sit for 15 minutes before dressing in their survival gear but we saw quickly that it would be too long to wait so we used instead a time of only 5 to 10 minutes before entering the "survival phase" of the experiment. The man in the reflective suit shivered uncontrollably immediately on entry into the chamber and never did stop shivering in the experiment. He lasted only 77 minutes. Next slide (Figure 5).

This shows a sea survival suit used by the Northsea Offshore Industries for sea-surface protection. We combined it with a "wooly bear" undergarment. This diver also used the Hamilton prototype CO₂ scrubber/thermal regenerator. He lasted about two hours and four minutes. These exposure times were voluntary exposure periods. We asked the divers how they felt every 15 minutes in relation to a symptom list. Both divers mentioned so far gave up because they were "freezing to death". After they came into the warmer chambers they immediately went to sleep and slept for about two hours each. Next slide (Figure 6 at end of paper).

We then tested a Norwegian synthetic sleeping bag with a heat reflective internal layer and combined it with the Hamilton CO₂

scrubber/thermal regenerator. The diver subject here had been one of the ones exposed before who had been rewarmed. With this system and a "wooly bear" undergarment, he lasted for about 8.5 hours. During the whole dive, we had problems with our communication system and we didn't know what the core temperature and diver reactions were. The external monitors removed this subject after 8.5 hours but the diver thought he could have lasted for a longer period. Next slide (Figures 7 & 8).

This is the fourth system, the Kinergetics prototype survival bag that is supposed to hang inside the bell. The other subject systems lay on the floor of the chamber. This subject wearing only a thin wet suit lasted 10.5 hours and gave up because of the cramped position in the bag. His feet in particular were uncomfortable. The breathing regenerator that he was using gave him *inspiratory gas that* was too warm and he started to take it off periodically, alternating one-half hour on with one-half hour off. Inspiratory temperatures were measured at more than 40°C. In the last 1.5 hours of the test period, he did not use his breathing regenerator. The investigators were not aware of this situation.

All the systems were used with different hand and feet protection. During the exposures, we measured rectal temperature and foot and hand temperatures as well as esophageal temperatures. A new deep body temperature probe (in which heat flow through a disc was balanced) was used for core temperature measurement. Expired gas temperatures were assessed with a respiratory gas thermistor inside each breathing mask.

Some conclusions now. The suit systems tested would not last 24 hours in the conditions tested, i.e., 150 msw and 4-6°C gas temperature. The two bag systems were promising and we concluded that these could keep the divers alive for 24 hours. Passive systems can protect a diver for 24 hours, if he uses a thermal regenerator to take care of his respiratory heat loss.

Schmidt: After the divers came back into the warm chambers, how long did they continue shivering?

Tonjum: Not very long. About 30 minutes.

Wissler: It seems to me that the the lower portion of the big Kinergetics bag is not as efficiently designed as it might be. The reason is that in order to maintain the necessary temperature difference across a resistance, you have to multiply the heat transfer rate per unit area times the resistance per unit area. If you are transferring a given amount of heat through a large surface area, then the rate of heat transfer per unit area goes down, and the temperature difference you can maintain across that resistance decreases as well. What you want to do is force that heat out through the smallest possible surface area, which is what you do in the fairly tight-fitting sleeping bag.

Tonjum: We've thought about such consequences and have suggested combining the two bag approaches. A gas-inflatable layer could be included to act as a type of mattress. We have to do something with the regenerator as well since it gave too hot a gas supply above 40°C.

Hayward: It is my feeling that if a person can last for 24 hours, then it is very likely that the thermal situation is not limiting and that the limitations that relate to rescue time may be other factors. How do you feel about that? When you say 24 hours, do you mean $24 \pm X$ hours or is it likely that once an individual lasts 24 hours, he might go on to 48 or 72 hours?

Vorosmarti: It would depend on the life support in the bell.

Hayward: Right, and what are those other long-term limiting factors? The heat from the thermal regenerator, the food supply, the CO₂ scrubbing?

Schmidt: When you mentioned that, once you get to 24 hours, you should be able to go on indefinitely, if you look at the curves I came up with, assuming that we don't lose shivering from exhaustion, once you get to 12 hours, it looks like you are going to keep going.

Tonjum: Another point is that we should not ask "why should we keep the bell support systems going for 72 hours" or "why should we get the bell up to the surface within 24 hours?" The figure of 18 hours that was used for bell surfacing in the example I gave you involved as many things going wrong as could go wrong.

Hayward: Obviously you are not going to wait a long time and are going to rescue the bell as quickly as possible. I was just curious how you felt about such things.

Tonjum: I think if the men are stable at 24 hours, they can last another 12 or 24 hours.

Kuehn: The next two presentations will be given back-to-back without

any question period separating them. Bill Hamilton will be first with his considerations of the design of a thermal regenerator and CO₂ scrubber for hyperbaric survival, the one I believe that was tested in the Polar Bear experiment.

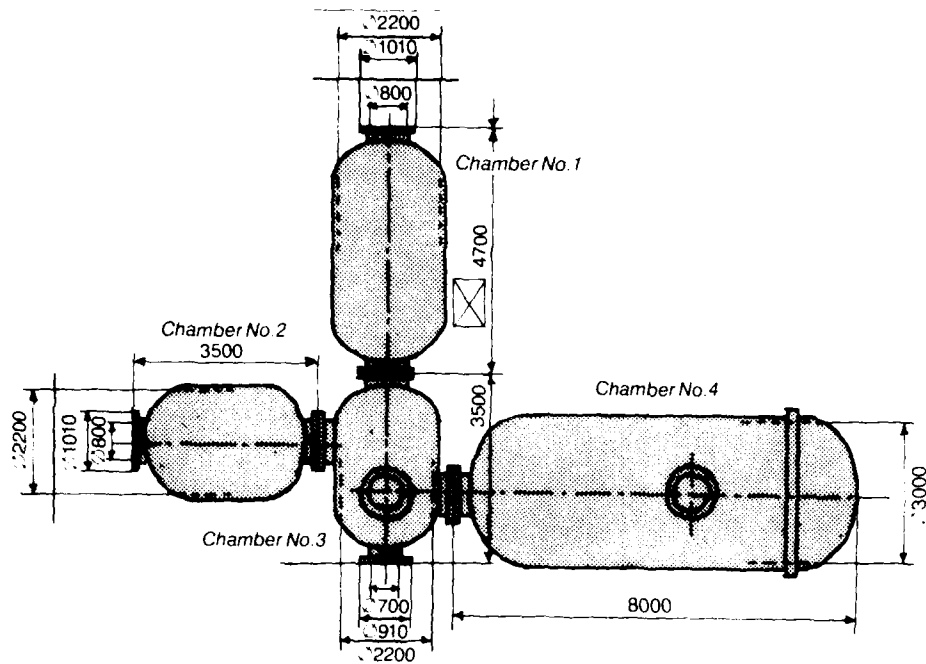


Figure 1: NUI hyperbaric chamber complex. Chamber 4, right, was used for the experiment and divers lived in Chamber 1.

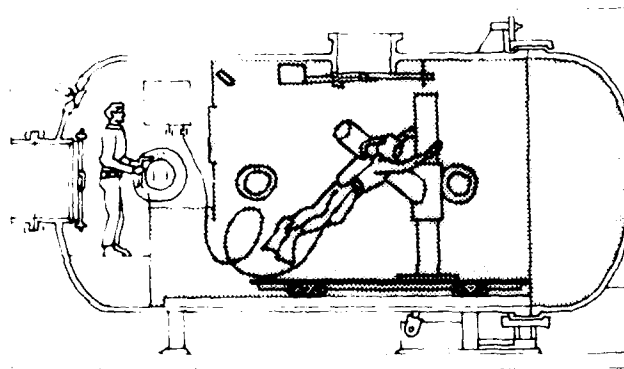


Figure 2: Chamber 4, showing the Lanphier-Morin baffle and the dry area at left which was used for the Polar Bear experiment.

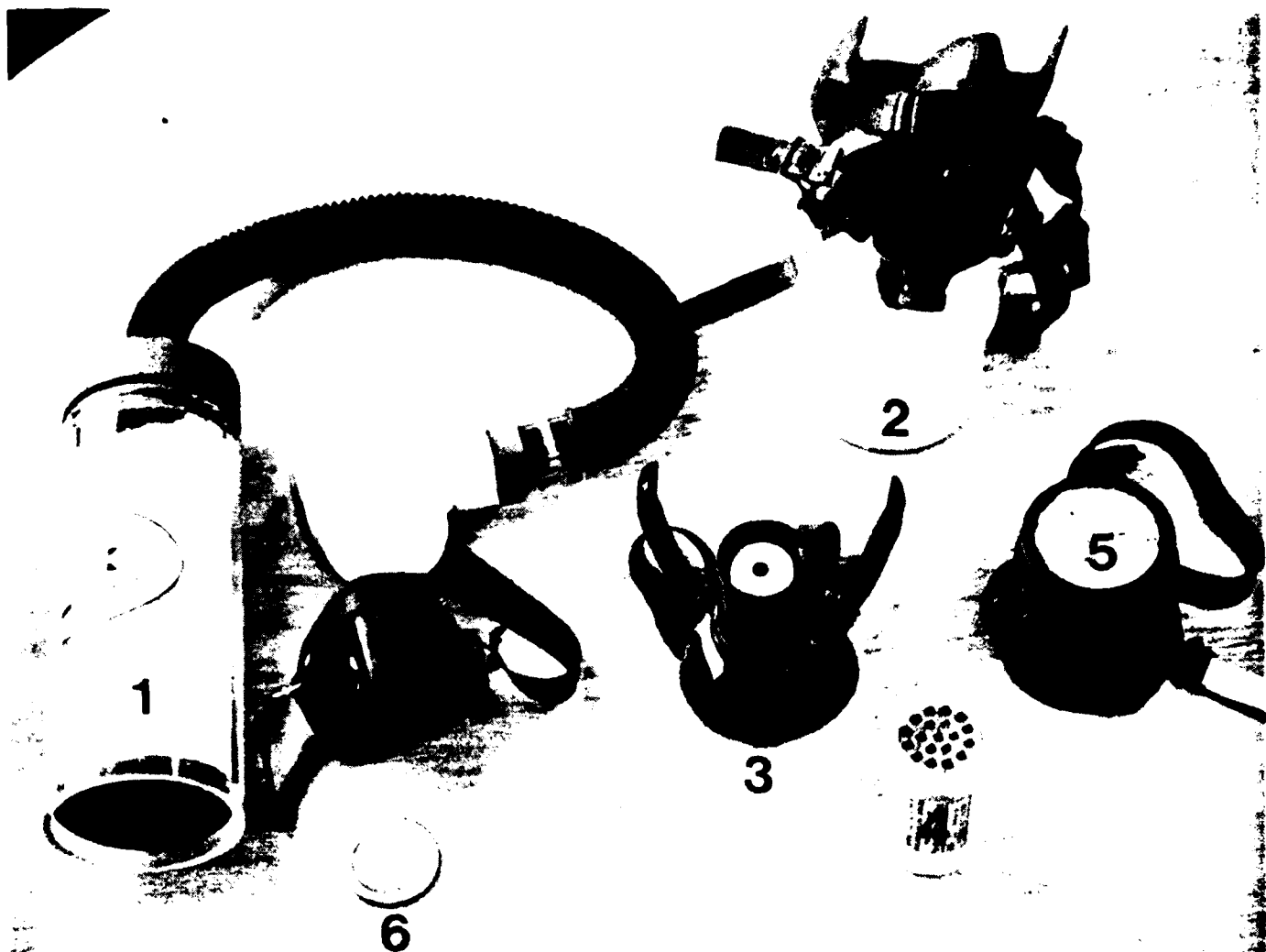


Figure 3 : Respiratory protective equipment

No. 1 is a prototype respiratory regenerator from Comex Pro with a screen from the unit aside (No. 6), No. 2 is the Hamilton prototype regenerator, No. 3 the early model Kinergetics, No. 4 the prototype from DCIEM, and No. 5 the prototype from Bennex.



Figure 4: System II

Based on heat reflective outdoor survival suit. This system consists of the thin wet suit (right), heavy mittens with wool liners, heavy socks and thermal boots, a knitted ski hood, and the reflective suit and hood (left). The CO_2 absorber and "heat sponge" is shown in the upper left.



Figure 5: System III, based on water survival suit. The thin wet suit is to the right, with the thermal boots. The wooly bear, its hood, booties and mittens are in the center; the survival suit is at left. The Hamilton respiratory regenerator is at the top.

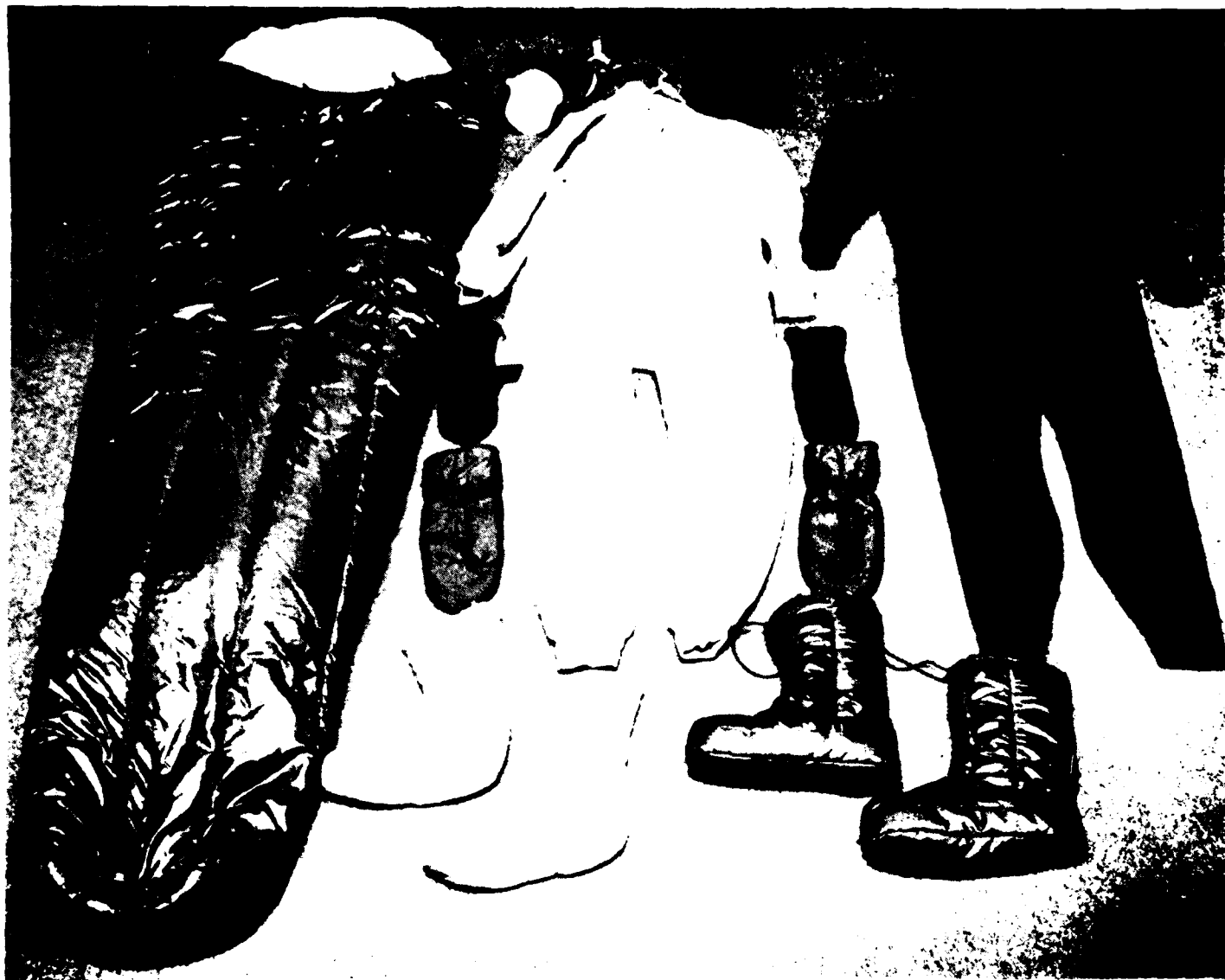


Figure 6: System IV based on a sleeping bag. At right is the wet suit and thermal boots. The wooly bear is in the center, with the ski hood, mittens, liners, and socks. The bag is at the left, and the Hamilton regenerator is at the top.



Figure 7: System 1. Suspended bag diver survival system (Kinergetics). While in this system, the diver also wore a thin wet suit, thermal boots and socks, but no mittens or hood.



Figure 8: System I
The vest-hood combination (Kinergetics) which houses
a breathing device inside.

DESIGN OF A COMBINATION THERMAL REGENERATOR AND CO₂ CANNISTER
FOR HYPERBARIC SURVIVAL

R.W. Hamilton
Hamilton Research Limited

Let me start with a little bit of history. In 1977 I was on a study group that was concerned with deep underwater welding. We were considering an underwater welding chamber on the sea floor, in which divers would work. There was a finite possibility that the chamber could become isolated, disconnected from surface support. I made the suggestion that sleeping bags be put in the chamber and that provisions be made for respiratory protection as well, possibly from a small cannister rebreather. The diver would breathe in and out through the absorbent material in the cannister, using it as both a thermal regenerator (Fischel, 1970) and CO₂ absorber. Other provisions were made for dealing with this emergency so nothing was done about the idea at the time.

This was some five years after Dr. Evan Lloyd of Edinburgh had published information on a similar device which he recommended for use in rewarming of Scottish hill walkers who had exhausted themselves in 20 temperatures and were in hypothermia (Lloyd, 1973). He developed a system which consisted of a small pediatric anaesthesia CO₂ absorbent cannister, a to-fro device, which has a small CO₂ cartridge on it similar to those used for making seltzer water. When CO₂ is discharged into the Sodasorb, it generates heat, enough to warm the oxygen from a small supply bottle which is rebreathed from a rubber bladder. It turns out that a device like this but without the CO₂ charge, designed by Dr. C.E.G. Pugh, was used on the first ascent of Mount Everest, 1953. So this is not new technology! In

fact, Dr. Lloyd in 1979 had recommended using a device like this for diving, but I was not aware of his letter (Lloyd, 1979).

You just heard from Dr. Tonjum about "Polar Bear", held at the Norwegian Underwater Institute last January, and the "Tupperware Special" (Tonjum, et al., 1980). When preparing for the Polar Bear experiment I built a provisional model of the idea. I used a piece of my wife's Tupperware and some assorted gear I had (including an old Air Force oxygen mask) to make this little breathing rig, shown in Figure 1. The basic idea was to use the mass of Sodasorb as the element in a respiratory thermal regenerator.

Here is what I thought has to be considered. First, we had to minimize the dead space. Figure 2 shows a sketch of a cross section of the Tupperware unit; it uses a radial or annular flow pattern with only about 15 cc of dead space in the cannister itself.

Next, we wanted a low breathing resistance. This also had the corollary effect of providing a maximum cross-sectional area and a short path length through the absorbent. This was a benefit of the radial flow cannister design. It seemed like no more than a half kilogram of material would be good to start with, because we could not be sure what the effect of the dead space within the absorbent would be.

We needed an even distribution of the gas through the cannister, a common problem in cannister design, but frankly I didn't know much about how to deal with that at the time. One factor is that the material has to be kept tightly packed so that it doesn't settle out and leave channels where the gas can get past the absorbent. Further, the cannister had to be

light, easy to fill, easy to use, and so forth, and preferably it should have some kind of see-through capability so that one could see when the absorbent is exhausted by its change of colour.

Well, apparently this design worked, but we don't know quantitatively how well. The divers felt that it was warm. We have made a demonstrator model available for you to try. You can see that you really do get nice warm, moist gas through the thing. Even in the cold heliox, it comes through feeling warm. It is almost oppressive, the hot moist gas. I was sweating quite a bit while wearing it for 15 minutes earlier this morning.

We had several problems. Mechanically, the cannister fell apart frequently, and this was quite a problem for the Polar Bear subjects. It couldn't be reassembled inside the chamber because we needed some special pliers, so it had to be passed out and this caused delay. Even so, we were gratified by the insistence of the subjects in wanting it back in a hurry. They did not want to be in the chamber without it. One of the important aspects of this particular rig was the unusual comfort of the aviation mask. I have never seen a diving mask as comfortable as this one. It is of course important that any mask fit well and cover the face with a good seal.

One incomplete aspect of the Polar Bear testing is that we did not consider CO_2 absorption at all. We did not make any measurements of CO_2 , we were only trying to conserve heat. Another interesting point is that the material did not turn blue when it was used at pressure in the cold environment. In one case, we found out we were using a soda lime that did not have an indicator dye. We changed to a type with an indicator, but

even that in the second usage did not turn blue. We do not know the reason for this. It may be due to the pressure, but that effect is only expected to be about 10-15% (Middleton, 1978). I have heard that you get very little indication of such material when it is cold but have no reference for that effect. We know it was not too cold to absorb CO_2 , however, because the material got warm and gave off heat. When one expires into this device with humidified, body temperature gas it starts working, picks up CO_2 and humidity out of the breath, and gets warm; it returns this heat and moisture to the next breath.

The unit holds 320 gm of Sodasorb. I tested it at sea level after the dive. With it full of fresh, indicating Sodasorb I breathed on it for four hours and monitored the CO_2 . It removed the CO_2 from my breath and was quite warm for three to four hours, after which the CO_2 began to come through. The breakthrough was about 2.5% after four hours and as high as 4% in another half hour. This meant that it was not absorbing much CO_2 by that time. The Sodasorb got very blue. Also, at the end of the test the cannister was only slightly warmer than body temperature.

The tests convinced us that the concept would work. The next step was to design a system for commercial use.

For this I joined up with Dick Long of Diving Unlimited International, San Diego. As you will hear next, Dick's objective was to design a practical complete system for diver thermal protection in an isolated bell. For the cannister we wanted a shape that was flat so that the unit would fit comfortably on a diver's chest inside his sleeping bag. The Tupperware unit used polyester/wool to contain the absorbent and act as a filter. We found out that the cannister of the Biomarine CCR-1000 diving rebreather

uses a polypropylene plastic that is a tough, porous sponge and is available in various thickness and pore sizes. The trade name is Porex (Glasrock Industries, Fairburn, GA). The material is rigid and about 1/16 inch thick, with a pore size of 250 microns. It keeps the Sodasorb in place, acts as a filter, and passes gas with almost no resistance. Porex is used for both the outer wall and the inner plenum of our cannister.

Figure 3 shows the prototype of the second generation cannister. We felt that about 1 kg of material would be appropriate and could be configured to have a minimum amount of dead space; this concern for dead space is necessary because of the to-fro operation. One thing required in order to have a low dead space is that the unit has to be close to the user's mouth and the tubes have to be short. It has the size and shape of a cigar box. The sides are made of Porex, and down the middle is a plenum of Porex with the mask connected at one end. In this model the plenum has a triangular cross section, but in the production version a cylindrical Porex tube is used. In testing the unit shown in Figure 3 we put thermistors all through the material to determine the flow pattern from the distribution of the reaction heat.

In a sea level breathing test of this early unit we found it would heat up primarily at the far end. We had to figure out some way to distribute the gas evenly. We made a small anemometer from a sheet of thin aluminum, and by forcing compressed air through the device we found out where the gas was exiting the tube. With these tests we found that the flow could be distributed evenly by tapering the plenum smaller towards the far end.

What we ended up with, used in the production model shown in Figure 4, is a cylinder of Porex with a plastic cone inside. The cylinder and cone are shaped to distribute the gas evenly along the length of the tube, and this design has the added benefit of minimizing the dead space. We confirmed the proper flow pattern of this design by long duration breathing tests.

For help on cannister design we turned to the anaesthesia literature, which is more extensive than the diving literature in this respect. One of the widely used "circle" type anesthesia cannisters (i.e., not a to-fro) has a simple design; it is a short cylinder with openings in the ends which have diameters about $2/3$ the diameter of the main cylinder. An anesthesia machine has two of these in series, such that when the first one is exhausted the second is moved up and a new one is inserted in the second position.

The aspect copied directly from this design is that the path length along the wall, which the gas would tend to follow preferentially, is about 50-75% longer than the direct path length straight through the absorbent, so we designed our little box with the same relative dimensions.

During the design of this cannister we resisted making the size large enough to go 24 hours because of the feeling that the dead space might be too large. The present model holds a little more than 1 kg; figuring 250 cc/min of CO_2 or 15 liters per hour for a person very much at rest and an absorbing capacity of 250 liters/kg it should last 16 hours or so at high efficiency. We can expect a reasonably high efficiency because we can tolerate a much higher breakthrough CO_2 percentage in the survival situation. High Performance Sodasorb has an efficiency 2 or 3 times better

than the standard type when cool, and is recommended. It should account for any reduction in absorption efficiency.

If the person is shivering at a high rate his CO_2 production will be greatly elevated and the device will not last nearly as long. But if we have a survival bag plus this device, then the man should not be shivering, at least not violently or for a long period of time.

We tested the unit a couple of times for 12 hours; the subject was exhausted at the end of 12 hours and the material was almost exhausted.

In this application we can expect a relatively high efficiency from the Sodasorb. For one thing, the efficiencies used by either the diving or anaesthesia schools are based on "breakthrough." They consider the absorbent exhausted when the effluent gas is 1/2% CO_2 . We don't really care so much about breakthrough, since our main objective is to conserve the heat and attenuate the CO_2 to some extent. Remember that the expired CO_2 is diluted by the bell's volume and that it has to pass through the absorbent again as it is inspired. Also keep in mind that we are concerned with survival at rest, not excellent performance in hard work. Lorne Kuehn (this workshop) stopped his experiment at 1% CO_2 ; I would not be too concerned with slight buildup of CO_2 as long as the ventilation wasn't unduly increased. The problem Schmidt spoke of, the loss of heat due to ventilation, would not apply so much to the diver wearing this rebreather. A CO_2 of several percent (sea level equivalent) would certainly be acceptable in a survival situation. We figure that even when this thing is in pretty bad shape it will still do some good. When it is totally exhausted it still acts as a passive thermal regenerator, and divers would

be advised to continue wearing it.

To review, we were concerned about dead space; there are three such spaces: the CO_2 dead space with the material working and with it exhausted, and the oxygen dead space. The important one is the oxygen dead space. Sodasorb has a void space of 47%, and the cannister volume is a little over one liter. If we add the mask, plenum, tubing and anatomical dead spaces it gives us a theoretical dead space of about 600-700 ml. That would be too much dead space for a sedentary person who might go to sleep. However, there are a couple of things that help us. First, I would advocate that the PO_2 in the bell should be at least 0.4 bars. I can't visualize why one would use less, and it would normally be more; this is in our favour.

More important, the gas flow pattern is not as one might expect for a pure "anatomical" dead space. It is not at all necessary to displace this entire volume in order to exchange gas. Considering it as a "physiological" dead space, we ran some tests by taking a large syringe and filling it with nitrogen, "exhaling" it through the cannister and "inhaling" again, and then analyzing the gas that was in the syringe to determine how much fresh air we were getting with each breath. Even at a syringe stroke or "tidal volume" as low as 100 ml we got some fresh air taken in. We found the dead space to be 26 to 33% of the "tidal volume," the latter ranging from 100 to 700 ml. What is happening is that gas flow through the device does not behave as square wave.

It has been shown that at high frequencies ventilation can be adequate with a stroke volume less than the anatomical dead space (Slutsky, et al., 1980). This is not exactly the same model, but it demonstrates the point

that one need not consider the "anatomical" dead space a barrier, as our tests confirmed. The important thing is that we do not find a stagnation or a rebreathing of the total dead space. A substantial amount of fresh air is added to the breath each time, even with a very small tidal volume. Because of these results we now consider it entirely safe to build a larger cannister.

Incidentally, the Norwegian Underwater Institute plans additional tests of this device in its Deepex experiments planned for late fall 1980.

An important thing about this device is that it should be worn in the lost bell situation at all times after it begins to cool down, even if the Sodasorb is exhausted. For this size cannister I would suggest using two per man and changing after 6-8 hours, because the material does recuperate if it sits quietly. The reaction proceeds to the inside of the granules and more absorption is possible after some delay. Of course CO_2 absorbent material from the bell scrubber could also be used to refill a cannister.

Another possible application of the to-fro cannister could be to take advantage of a dead space from which CO_2 is scrubbed, to act as a barrier to high oxygen. If one had to survive in a high oxygen environment for several days, such as a submarine trapped with 6 bars of air inside, a device that would increase the oxygen dead space without increasing the CO_2 dead space could help to reduce the pulmonary oxygen exposure to a tolerable level. This device will do this if an extension is added to the dead space beyond the cannister. It is a far-out possibility, but worth considering.

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Photograph Courtesy of Øivind Christensen

Figure 1. The "Tupperware special" cannister made from a polyethylene refrigerator container. It has a perforated tube down the center of the cylinder and a polyester/wool liner inside the outer wall. Gas passes radially outwards and exits through a hole in the bottom (see Figure 2.)

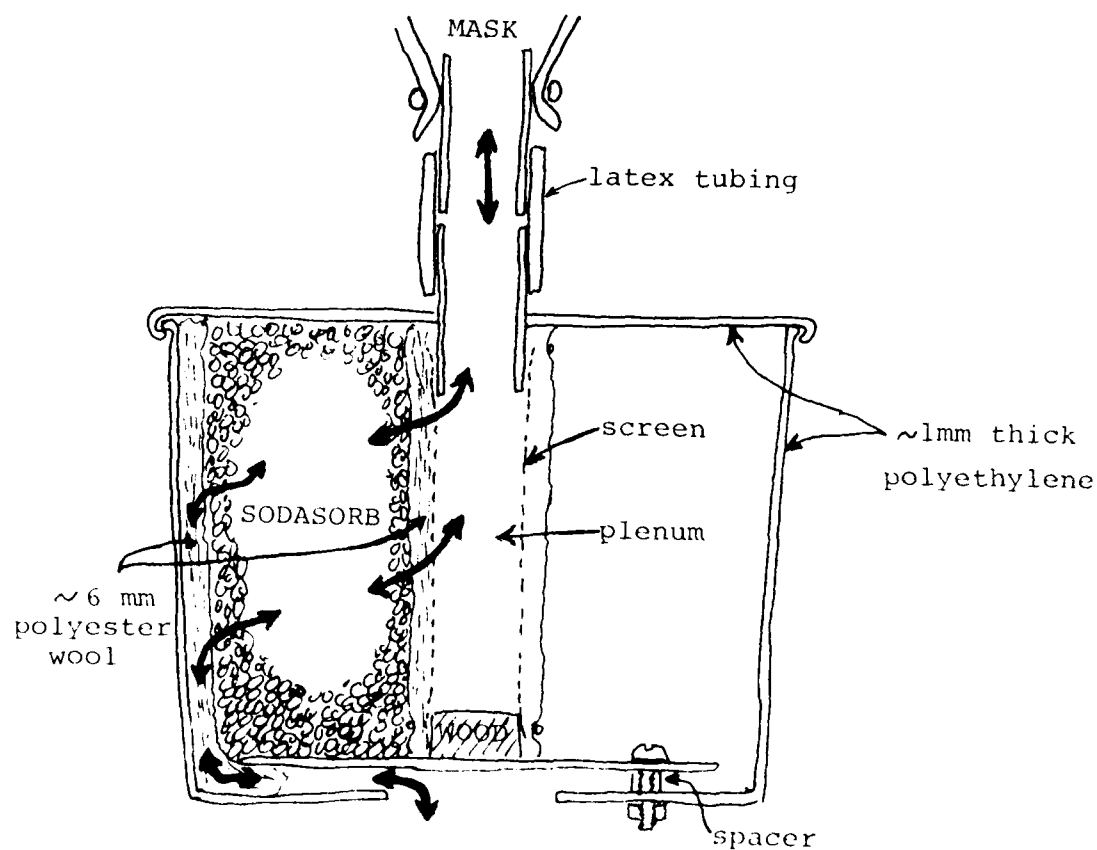


Figure 2. Sketch of a cross section of the "Tupperware" cannister. The heavy arrows show flow pathways for inspired and expired gas. The crude connections to the mask came loose at all possible places during the experiment.



Figure 3. Prototype-test model of second generation cannister. Both the sloping sides and the triangular plenum in the center are made of 1/16" Porex, with 250 micron pores.



Figure 4. Production version of cannister. The small sides are Porex, the large flat front is clear acrylic plastic and the ends, back and corners are stainless steel.

DEVELOPMENT AND PERFORMANCE STANDARDS OF DUI's

POLAR BEAR SYSTEM

R. Long

Diving Unlimited International

For the sake of time, I'm going to oversimplify my presentation. We have been in the business of underwater survival for quite some time. We have recognized the need for a bell survival capability for some time as well. The problem was real and was there but it was low on the priority list because we had not lost anybody. It was like the traffic light on the corner: one is not put up until there are enough accidents to make it worthwhile. Now, because of the impetus brought on by the NUI Polar Bear experiment, where the issue was forced and for which the entire industry is grateful and in their debt, the oil companies and government agencies are responding to the issue.

We basically started off by using the recommendations in the back of the NUI report as the prime design criteria for our system. We had a basic jump on other people because a lot of thinking was done by the other people before our involvement. Our basic design criterion that was used was that the survival bag must be kept very simple. The economics were going to be the prime governing charact-

eristic. The economics in so far as cost were "Could people afford to buy it and use it and replace it, if it needed to be replaced?" Also, the economics in space were important. The bell is a very small place with a lot of things crowded inside. Diving companies resist greatly anything else being put in. There is no place inside to put a survival bag.

Another point was the safety hazard of safety devices. You can only put so many whizos in there until pretty soon the diver has no room to get in. You can hang things in there but they bang their heads, etc. One of the other points is that the bag is a piece of safety equipment that is not used every day. It will not be maintained and will be hung out of the way until the day on which it is needed and then it has got to work.

This system that I'm about to show you is not the best system that we could produce from the engineer's or physiologist's standpoints. The other considerations weighed more heavily in bringing this about. So we have here one Polar Bear survival bag. You have to be careful about pulling the release cord when handling it because of the amount of material we have packed inside.

First of all, we put in a pair of boots - Polar Guard Boots, that come about so high, similar to the ones used in the Norwegian experiment. We chose a parka instead of a vest so that the arms could be covered to expedite the arm warmth when the diver puts his arms outside of the survival bag to turn valves, etc. In the parka, remembering that we are trying to be cost-effective, we have big

pockets in which the hands can be kept although we find most users prefer to put their hands on the exterior of the CO₂ scrubber because of the warmth there. Inside the pockets, we also put two or three sponge bags for urine collection, similar to the ones used in deep submergence. They are well developed, inexpensive, can be rolled up and hold about one litre of urine.

The bag that was used is the biggest one that we could get, the reason being the size of divers, wearing not only the parka but also the CO₂ scrubber/thermal generator. The experiments in Norway used a bag rated to -10°F; we chose one rated to -25°F for several reasons. One reason was that the tests in Norway involved divers wearing "wooly bear" underwear over a 1/8 inch hot water wet suit liner plus boots and gloves inside a sleeping bag. No one will have "wooly bears" in a diving bell, because they probably would be stolen and there is no room for them anyway. We assumed that they will not be available and the diver will only have a hot water suit plus liner at the time of isolation of the bell. The bag has 9.5 inches loft in its new state and it comprises a lot of volume by itself. The tests on this bag were confirmed by copper man tests at the University of Kansas by the bag's manufacturer. All such bag statistics have been similarly verified. On the outside of the bag system we have used a large plastic bag with a slit down the front end. We tried to find a way to find a way to make the bag waterproof since it is rare that bells do not have water in them. Stitching of bags cannot be sealed to be leakproof. The cheapest solution was to use a bag like this

Coming back to economics, there are only going to be about 100 of these systems sold. You have to take the cost, amortize the research and development over that, and you are not going to run a quarter million dollar dive under those terms.

So our design was to give the diver the best design we could, one that is controllable to a certain extent, through making it more conductive, by bypassing the internal gas layers. We also feel that, from the preliminary data that we have, that we are going to lose about 25% of our insulation in taking this course in packaging. That will come off our 4 clo. No one knows what the long-term storage effect is going to be, least of all ourselves.

One of the problems we had in the early days of the tests was the loss of the facemask off the face when moving in the bag. Adjusting the mask was difficult. Therefore, the head harness for the mask has two parts. A lot of work on the scrubber was to make it more compatible with the whole system rather than work on the efficiency of the scrubber itself. We wanted the ducting to be very short and light, since if things hang on your neck for 24 hours while you are sitting up, every bit of weight reduction is important. We found that we could support the cannister from the head with the strap and that it was more comfortable. The mask is flexible and we have two more straps to hold it on the face. When wearing the system, it feels like they are almost independent, as if there was only a hose between them. This mask was chosen because the inlet and outlet orifices were high on the mask as opposed to low so that any mucus or

whatever would collect in the bottom and not occlude the orifices.

Again coming back to the NUI report, we tried to apply their recommendations in a way that the product can be useful in the field. There is a non-return valve here. The breathing resistance is so low in the cannister and, because there are no valves inside, we had to put a sponge in here to get a little back pressure against that return valve. If you press on the nose, as if you were clearing your ears in a facemask, any mucus or water goes down into this bag and doesn't dribble down on you and wet your clothing. If the bag gets full, you can replace it.

Hamilton: There is a lot of moisture in there because all the moisture from your breath is captured. It's not the same as bronchorrhea that you get from being cold. It's not mucus, but water.

Long: We also say that the cannister is worn outside the parka and I'll talk about that in a moment. There is a strap that goes around here and we put that on because it can be used inside the chamber. You may have a breakdown of some kind in the bell, or even a chamber with short BIBS feeders. Then you can put this kind of device on. The helmet is held tight on the face and the cannister is tied to your chest. You almost have to take it off to see how your blue line is doing.

We also felt that the divers would not read instruction books so we wrote the instructions right on the devices on the exterior surfaces.

In order to prevent the CO₂ from migrating into the cannister,

Bill and I developed a cheap means for viewing the sodasorb. It was a double plastic bag with a bar seal here. You put the thing inside, seal it and roll it over and seal it again. The bar seal has a piece of lime here with a series of X's cut in the plastic (seen on close inspection), the purpose of which is that there is very little gas transfer into it unless there is a pressure differential. If we have a differential, it forces itself in there and equalizes and you do not have to worry about crushing your cannister during diver compression/decompression. It costs only a small amount of money.

The pile of material for the system was very large. We tried vacuum packing and it didn't get very small. We had to come up with an involved packing process which brings it down to this size. It took three guys and two days to do this initially. A minimum of two days packing is still involved but we don't think repacking will be as difficult. The problem is to reduce the sleeping bag size the first time. The system goes inside a waterproof container. It will take gas in through the edges but it is spray tight. A lot of synthetics have been used inside so we don't have to worry about cotton and mildew or rotting. We put velcro strips on the back of the bag with interruptions for belt straps. By putting this thing on the floor, if you press on it you can force it into the shape of the bell surface. By using stainless steel or tygon fasteners you can tie it back.

We thought about special ways of undoing the package but we came up with this release cord system, which is easily pulled. With

and keep it off the wet bell surface by a mountaineer's air mattress. It is inflatable with a foam inside to protect it against internal convection currents. The mattress is 20 inches by 72 inches and at the top of it we've put on some grommets and ties so that you can slip it inside the plastic bag or tie it up to the piping and put yourself up inside of it.

A number of people want to use a harness to put the diver in an upright condition so that nothing covers the bell trunk. We chose not to use a hammock situation, not because it is not better, which it would be, or because of its expense, since it is cheap to do, but because it is impractical. There is not enough room inside the diving bell to hang two guys. You can only hang one guy, probably the stronger one.

Again we are using Polar Guard. We looked at hollow-fill insulation but we are concerned about long-term storage and the effect of water on hollow-fill. Also its resistance to long-term compression is less, although Polar Guard is not so good either.

When you put all this stuff together, and you work it out, it comes to 16 clo insulation. If you take 16 clo and look at the work done at Natick on helium impermeation, then in hel'ox it will be reduced by 75% to 4 clo. If you assume that we will not lose any respiratory heat or anything from the CO₂ absorbent, that tells you the guy can survive forever. But there are problems with this appraisal.

There is no test data. Such testing is extremely costly.

an eye to cost and performance, we came to this.

We wanted to put the CO₂ scrubber inside the bag so that whatever heat was lost would be picked up in the bag and retained. When we put it all on a subject, we were quite concerned with oxygen inside the bag. What we did was to take a Teledyne sensor, strapped to the outside of the device and also took a little tube from the side of the mask. The first time we did the experiment, we put the sensor inside the parka and we got values for oxygen below 16%. The experiment was stopped and the subject was changed with the CO₂ scrubber outside the parka but inside the bag. We found that, in 20 minutes, we went from 20% oxygen down to 18.5% and that became a steady state reading. This experiment was repeated several times. The lowest oxygen we saw was at the very beginning, about 17% in the first five minutes, which we put down to the effects of early heavy breathing due to getting in the bag, zipping up, and having a higher rate of exercise. By conducting these experiments, we felt that we had adequate oxygen coming in around the facemask and through the outer garment itself.

I want to underline that there are two words not used in any of our literature or advertising. You don't see the word "survival" because we don't know that you will and the other word is "24 hours" because we don't know that there is that much time. Based on the data we have and calling the various experts around the country and taking advantage of a lot of experience, we applied our best design to this product. We feel that you have 35% more insulation here than

anything else we know of in the marketplace. Now we have to do a lot more testing and we have to get more cooperation from those who will be diving, like NUI.

We don't know how good a survival garment it is. We don't know of the condition of the diver when he gets inside. No one has done these experiments. NUI, again whom I want to commend for their efforts, openly say that their experiment was a beginning. We are not claiming any more than this. We have made an open offer and I extend it again at this place. If anyone is running any tests anywhere and wants to use one of these things, in part or in whole, let us know. We are already doing that with two different groups now. The only two things that we ask are that we get copies of data and that you do an adequate job training the subjects on how they are supposed to be deployed. We have had equipment before put in tests with a verdict of failure yet the equipment was not deployed properly. We will be glad to assist you where possible and where our limited finances permit.

I want to say a word about 48 hour survival. I share Dr. Tonjum's feeling that what we want to do is get the bell out of its predicament as quickly as possible. In the two diving accidents in which people died, the recovery sea conditions were ideal but the divers were still lost. The self-help systems employed by diving companies themselves include secondary lift and recovery procedures as well since they do not want to depend on outside assistance. If they can get themselves up, they will probably do so within six

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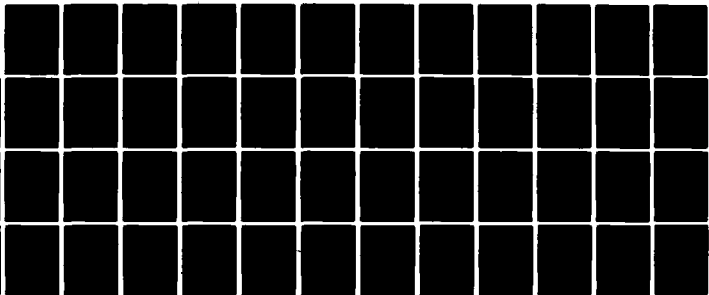
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hours. If they haven't recovered themselves, then it will require outside resources. If it is not recovered in six or eight hours, then we won't have them up even after 24 hours. If we have adverse sea conditions or difficulty in finding the bell or any one of a number of conditions, there may be a need to go longer than 24 hours.

There is another problem. The more safety equipment that we put in there, the more the diving companies and the divers rebel. I would, therefore, like to make a suggestion for your consideration. The equipment that the man uses every day should be, as much as possible, his survival equipment. He should be wearing his survival equipment. It should be his uniform of choice rather than one that he is forced into, such as pilots in poopy suits. You can put them in, if you have a gun.

Permit me to share some generalized thinking with you. If we maximize the insulation on the outside of the diving bell-- we use the insulation we do now, because we can sustain a 10,000 BTU/hr heat loss from the bell, because the hot water will be easily put in. If we have no insulation, the figure rises to a 100,000 BTU/hr requirement. That's why we do what we do today; there is no grand design. We can simply supply 10,000 BTU/hr easily. If we maximize this approach with survival in mind, by putting more insulation outside and compensating for the increased buoyancy, if we in turn, after helping the divers do everything that they can to help themselves, if they then would take their umbilicals down and wrap them around the trunk because the trunk is the single greatest area of heat loss in

the bell, if we make a mattress that blows up, similar to one on our bag, say three to four inches thick, designed optimally with a hole in the middle, like a doughnut, to permit opening of the bell door: now the divers would sit on a cushion on top of the mattress and we have cut off or slowed down the biggest single heat leak to the sea. There now remains only the conduction of heat through the bell walls.

If we then use something like a non-return-valve suit which gives you some time to get back into the bell after your hot water is cut off, it will stop the cooling of the outer regions of the body. It is not designed to prevent long-term hypothermia. Underneath that, instead of a thin wet suit, which does very little in terms of thermal protection, we use a Thinsulate garment and we wear it inside the garment; then as we compress and decompress in the bell it affords thermal protection. It is also comfortable to wear. That should be a liner for the hot water suit and our clo factor would be improved. Now we put on a regenerator/ CO_2 scrubber which is going to reduce the dependence on gas heating from the outside. By doing these different things we have lessened the temperature in the bell necessary for diver thermal comfort, say from 85 or 90°F to something lower. It is not inconceivable of bringing the bell BTU requirement down to 3000 BTU/hr. We have already developed a simple exothermic bell heater that is inside a cylinder 12 inches long and 12 inches in diameter with domed ends that will supply approximately 1,440,000 BTU. If this could be metered out at 3000 BTU/hr, this would give you 48 hour protection, with no sleeping bags involved, with only the

Polar Bear system. We supply two of these. The guy is wearing one and he transfers it off to the other one. If you go beyond 24 hours, you still have the CO₂ absorbent that is inside the bell cannister. You can open the used individual cannister and replenish it with sodasorb from the bell cannister. This permits longer-term survival. This is one approach that is possible with today's technology to go 48 hours and beyond. I also believe that this package is acceptable to the diving company and to the diver, because we have done away with a lot of garbage.

Some other possibilities for use of this CO₂ scrubber include use in air decompression chambers when the air is very cold. You are not going to bring a guy back from hypothermia but you can prevent him from going deeper into it. There is a lot of surface decompression done in 54-inch chambers exposed to windchill. Submersibles; the O₂ control permits use in high oxygen concentrations. We have reconfigured the device on several occasions in which we put a non-return valve in here and by inverting or changing direction, we can scrub inhalation only or exhalation only. If you are in a hot environment, you don't want to use this in a to-fro mode, but in an inhalation-only mode unless there is a high ambient concentration of CO₂.

One other thing. One recommendation is that there be some basic fundamental understanding of the problems of hypothermia, its recognition and first aid treatment, in the field today. There is none and there is none being offered. I would suggest that the UMS

or NUI or someone else develop a 4-hour first aid course, just as for CPR, on hypothermia and its first aid. The divers, diving supervisors and medical technicians should be exposed to this. The University of Aberdeen could do a good job on this, because of their location.

Another one is in relation to this Thermal Workshop. I've learned a lot and I've been impressed by the proceedings. As you know, there is very little transfer of information from this room and group to the field. Today, divers are out there using what they had years ago and your existence is of no value to them. Even though we have thermal guidelines for diving, they are for scientific use only. There is no method by which they could be applied in the field by a diving supervisor, the chap on-site who makes decisions on dressing and exposure. This chap has nothing to go by, even though the rules request it. The U.S. Navy Diving Manual is suited to the use of the U.S. Navy.

In 1979, at the meeting of the Informal Thermal Group at the UMS Scientific Meeting, DUI posed the development of some thermal guidelines for divers to be used by divers themselves. We are keen to do this and would like to work with some qualified people on it. I would like to suggest that this become an active project of some investigator here or some agency that could lead to a published UMS guide book, the sale or proceeds of which could cover its costs of publication. I see it being pocket-book sized, which the diving supervisor could pull out, with first aid advice.

Let me give you some examples of what I mean. The guidelines should be written to the level of understanding of a diving supervisor, very much like the one done for recognition of decompression sickness in "Your Diving Buddy", written by Dick Clark. Someone in this room gave me that information yesterday. Those are practical things that can be used in the field today and which are desperately needed. I have an appreciation for your art, but it's only an appreciation and not an in-depth understanding. The guys in the field don't even know that you exist.

The kinds of things needed by them are indicated by the following examples. When you are rewarming a guy who starts to sweat, you terminate the sweating. That's what the diving supervisor needs to know and understand and he doesn't need any instruments to determine it. Paul Webb told me also that in active diver heating or should I say, thermal conditioning, the passive side of that is a real problem. If you bring the diver out and give him a cup of coffee, you may want to send him back in the cold water. Is it safe to send him in? Does he have a thermal debt? Is he rewarmed totally? Can we expect the same performance in the next dive? One idea is simply to check to see if his testicles have come down. If they have, his body is rewarmed. That's the kind of thing that we can apply. Another is the case of the diver with uncontrollable shivering. Pull him out of the water! But no one has bothered to say that and the divers need to know it.

In conclusion, I would like to thank NUI in particular for

their work, because, without that, this new product would not exist. Thanks also to Dr. Hamilton for working with us. This was an ideal way in which a physiologist gets together with a mechanic to develop the physical art as applied in the field in producing something which is needed and can be used. If what you do doesn't improve the practice in the field, you have not accomplished fully what you set out to do. We would stand ready to do the same thing with anybody to help advance the state of the art. Lastly, if any of you get a chance to come to San Diego, we've got a neat boat and abalone galore. We're always looking for ways to play hooky.

Kuehn: Thank you very much, Dick. We have about ten minutes for questions relevant to the presentations of the last three speakers.

Greene: I know Dick speaks for himself and his company when he invites people to be involved in field management, but in saying we would like to have active input, are you speaking on behalf of a larger group, like the Association of Diving Contractors, with which the UMS could communicate?

Long: As most of you know, I was on the Board of Directors of the Association of Diving Contractors and many diving industries are beginning to realize the need of interdependency on one another. There are people who are willing to get involved. The answer to your question is both yes and no. But if we can get things started and advertise the fact that we have successfully done them, that will encourage others.

Hayward: I didn't hear the value of the inspired gas temperature on

the Hamilton CO₂ scrubber/thermal regenerator.

Hamilton: It gets up to 45-50°C in about 20 or 30 minutes and it is then stabilized in a normothermic room. I have no idea what it would be in a diving bell. We need to measure it.

Long: We know that it's quite warm. We know that if we remove the sponge out of the bottom, we will start to pass particulate gas out of the bottom and then we will only be partially scrubbing on the exhaust side and 100% on the inhalate side. This will cut down on the temperature of the gas.

Manalaysay: Supposing you get beyond 42 or 43°C inspired gas temperature, especially if you don't have 100% relative humidity, that could be damaging to the respiratory tract.

Long: The inhalate from the device is always 100% relative humidity. It's raining inside.

Manalaysay: If you stayed below 42°C and with 75-100% relative humidity, you would be fine.

Long: We are at 100% relative humidity, as measured up here in the mask and again, we need data to verify this equipment. That is true for the bag as well. If we say our bag loses so many watts, to do that, we put a mannikin inside along with an electric blanket and some temperature sensors. I don't think you would accept that compared to laboratory testing.

Hamilton: I think that you are fooled by the heat that the scrubber generates. It is a trivial amount of heat compared to what you are dumping if you exhale into the helium environment. Don't be misled

by the 15 watts that it produces. A lot of people are impressed by its feeling warm but that's not much heat from the absorption of CO₂.

Vorosmarti: That's not the problem, Bill. The problem is that, if it gets too hot, you are going to start doing damage to the upper respiratory tract. We're not worried about the heat but about the chance of burning.

Hamilton: That's the concern in anaesthesia cannister design, how to get rid of the heat. We're lucky that the heat is about right. It doesn't get hot enough to do any damage.

Long: The temperature can be made to drop. It takes a couple of minutes for this to be noticeable because of the latent heat on the sodasorb.

Middleton: But all of your observations are at one atmosphere conditions.

Wissler: It is possible to derive some simple equations which characterize the behaviour of a breathing gas heater designed to recover the heat of absorption of CO₂. When sodasorb is the absorbent, 13.5 kcal of heat are released per gm.mole of CO₂ absorbed. If we assume that the metabolic equivalent of O₂ is 5 kcal/l of O₂ at 1 ata and the respiratory quotient is 0.9, then the ratio of the heat of absorption (Q_A) to metabolic rate (Q_M) is

$$Q_A/Q_M = \frac{VO_2 (0.9)(13.5)/22.}{VO_2 (5.0)} = 0.11$$

Hence, recovery of all of the heat released when CO₂ is absorbed is equivalent to increasing the metabolic rate by 11 percent.

It is also instructive to compute the increase in gas temperature required to carry the heat of absorption as sensible heat. If ΔT = the difference between the temperature of the gas leaving the absorber and the temperature of the entering gas, and the minute volume is $\dot{V}_g$, a simple energy balance yields

$$\begin{aligned}\rho C_p \Delta T &= \text{rate of heat release} \\ &= 0.11 \dot{V}_{O_2} (5,000)\end{aligned}$$

using the previously obtained result. If the gas is essentially helium,

$$\begin{aligned}\rho &= 0.2 \text{ P gm/l} \\ C_p &= 1.0 \text{ cal/(gm.}^\circ\text{C)}\end{aligned}$$

in which P is measured in ata, then assuming that $\dot{V}_g / \dot{V}_{O_2} = 20$, we obtain

$$\Delta T = 137.5/P (^\circ\text{C})$$

Such a large increase in temperature would only be realized in a device similar to the one shown before. Of course, loss of heat from the surface of the device will reduce ΔT also.

Hayward: What about cost of this total package, since you stressed economy?

Long: In small amounts and for the deluxe package, about \$1465. If you buy them in lots, they are cheaper than that. Anything else on the market is over \$2000. We are trying to disseminate this technology to the guys who need it.

FINAL WORKSHOP GENERAL DISCUSSION

Kuehn: We will now take up questions in a final discussion, pertinent to all topics that have been presented.

Ackles: If afterdrop is not a real phenomenon why is everyone concerned about it in rewarming? Is it a rectal artefact or is it a true concern in rewarming?

Golden: This is something that I found very surprising when we did the animal experiments, namely that the central venous temperature did not fall in anything like the relationship shown by the falling rectal temperature. That immediately implied the question of what did it mean in terms of the therapeutic view in managing the hypothermic candidate? If I can digress a little, everybody's thinking was affected by the Dachau experiments which implied, although the experimenters did not say so -- this was done by Burton and Edholm -- that the cause of post-rescue death in hypothermia was a return of cold blood coming back from the periphery into the central circulation and stopping the heart. This was taken up by everybody subsequently and carried on in methods of treating old people which have been aimed at reducing the magnitude of the afterdrop. If the work that I have done with Hervey and the work that John Hayward has shown us is true, then the afterdrop in rectal temperature is not something which we should worry about from a therapeutic view. So that nice neat explanation of post-rescue death goes out the window, but it does raise the question "What is causing these people to die?" Our feeling now, for a variety of reasons, is that it is

associated with a cardiovascular cause. We think that if you have somebody who has been in the water for an hour or more, shivering continuously, immersed up to his neck, and you then come along in rescue and lift him out of the water vertically, say via a helicopter sling, or up the side of a ship, in which his physical effort is increased, you are increasing the demand on his cardiac output and removing the hydrostatic squeeze, which was helping his cardiac output by about 34%. In hypothermia, from all the work done by Berne et al on dogs, which showed that the coronary circulation which is predominantly filled during diastole, is decreased by anything which increases the heart rate. In hypothermia, you already have a coronary filling problem, because the diastolic period is already short, due to a long slow systole and slow relaxation. Nevertheless, in hypothermia there is sufficient cardiac filling to satisfy the myocardial demands for oxygen. If you pace a hypothermic heart, and this is well known clinically or in animal work, you will immediately put the animal into ventricular fibrillation if the heart rate exceeds a critical level. Well if you look at this situation of a survivor who is immersed up to his neck for a long time, when you lift him out of the water, the hydrostatic support of his venous return will be removed, which perhaps was the factor which was keeping his cardiac output viable. So it is possible that this explains why 20% of people rescued collapse and die on rescue or shortly after.

Hamilton: Is that when they die rather than when they are rewarmed?

Golden: No. Post-rescue death is a complication of drowning or

hypothermia. The drowning is fairly obvious; it is either delayed primary death or secondary drowning or cerebral edema secondary to the drowning. The hypothermia death is the immediate post-rescue collapse or a rewarming death. The 20% people of which I spoke are the post-rescue collapse and rewarming deaths. It appears to be a cardiovascular problem related to the hypovolemic state of the individual because of his prolonged cold exposure and these other complicating myocardial problems. It has nothing to do with his rectal temperature. This is why we have recorded instances of people who die in the post-rescue phase when they were fully conscious on rescue, with core temperatures obviously above 32 or 33°C, or people who die when their rectal temperatures are around 28 or 29°C. The rectal temperatures are immaterial and have been confusing us for years.

Manalaysay: Has this post-rescue death been seen in people who are not immersed, say the Scottish hill hikers?

Golden: Yes. If you read Pugh's very good account of the deaths of the Boy Scouts on the Four Inns March, you find that one of these people, when carried down the hill in head-up position, went into an epileptic seizure and died.

Schmidt: Is it possible that the hypovolemia is due to sea-water aspiration and not the cold exposure?

Golden: No, it is due to two factors; first it is due to the increased diuresis associated with the circulatory shifts associated with peripheral vasoconstriction and second, more importantly, there

is an inter-compartmental fluid shift that occurs and the intravascular fluid moves into extracellular and intracellular fluid. In rewarming, that shift will recover, but not immediately on rescue. The shift has occurred and the fluid is not in the vascular compartment. That's the problem.

Ackles: The basic question I asked about the afterdrop has not been answered. From the therapeutic point of view, does it make any difference, on rewarming of persons in a hot bath, that you have the limbs in or not?

Golden: My feeling on that is that it does not matter. This is how the Nazis got confused with their experiments because by a sort of serendipity they put people in a hot bath and they found that those put in the hot bath recovered. The hot bath does two things. It replaces the hydrostatic squeeze to the peripheral circulation although not as well as if one is in the vertical position and also by applying heat to the skin, it inhibits shivering and reduces the demand on cardiac output. This was a serendipitous finding which fitted in with the theory of the cold blood and confused people over the years.

Pozos: There is decreased demand when the subjects are in the hot water but you have an increased peripheral vasodilation. Isn't there going to be an increased cardiac output because of this?

Golden: That's another thing that one hears, namely that one of the disadvantages of rapid rewarming in a hot bath is that you cause a peripheral vasodilation and cause an increased cardiac demand. But

you see from the slides I showed you on heart rate, that does not occur until 15 or 20 minutes into the rewarming when you suddenly see the heart rate rising, presumably associated with the occurrence of peripheral vasodilation. There is a time lag for the heat to move through the tissue to reverse the peripheral vasoconstriction in the cold tissue. The skin warms and you can see in the pig experiments that the cutaneous temperatures were rising and the central venous temperature rising along with it but the intervening muscle and subcutaneous temperatures were much cooler so the blood coming back from the skin bypassed the muscle bed of the shell, and went straight into the central venous circulation. So there is a time lag before peripheral vasodilation will occur. This is one of the problems in chronic or geriatric hypothermia. You find these old dears collapsed in their houses with body temperatures of 26 or 27°C, you bring them into intensive care, you start to rewarm them, you have no problems with them until they get to 32 or 33°C and suddenly they go. Their blood pressure collapses from hypovolemia shock and they get a cardiac arrest. So you have to monitor the central venous pressure very carefully and the peripheral arterial pressure as well and you govern your rate of rewarming on the balance. When you see that the pressure is falling, well then you must stop the rewarming there and then, to let the pressure recover and continue rewarming later.

Hayward: Dr. Mills has said in Alaska that if you know what you are doing, you can rewarm in any way you want. Frank Colden knows what he is doing. The danger is in the field. What are you monitoring to

know that you should back off on your rate of rewarming? It is a danger. I agree with Frank that in humans in 20 minutes of bath rewarming you see a halving of peripheral resistance, indicating vasodilation and an increase in heart rate.

Could I just follow on in the definition of afterdrop? I would not like to leave with the impression that afterdrop is only a rectal artefact. It is not an artefact, it is a rectal fact. What we mean is who cares about rectal afterdrop? What we care about is thoracic and brain afterdrop. The word "after" refers to "after the cold exposure." If you go from the cold exposure immediately to a warm environment, with core rewarming and good insulation, that's one thing, and you may not see, in a good shiverer especially, any cardiac afterdrop if you are measuring at that site, but don't forget that in the field you may have a diver out of the water onto land or a raft and he will still experience cold stress. It is after the rescue but it is in that situation in which they may have impaired shivering and vasoconstriction as well as trauma that there can also be an "afterdrop" of the core temperature. Let's not confuse the situation in which you remove the cold stress completely on rescue with this case in the field. Remember that there is a hiatus in time between the precipitating incident and the beginning of appropriate rewarming therapy. This is what we should talk about as afterdrop. It is possible in the tents, in the bush, that you rescue a man from the cold but even there the thoracic area of some individuals would continue to drop. How do you feel about that, say a downed aviator

Golden: We do know that the heart, if cooled, will keep beating as long as respiration is controlled all the way down to 9 or 10°C.

Hayward: In a hospital. I'm talking about the field where you do not have hospital control and management and you do have environmental cold stress.

Golden: So what does the hospital manage to control that keeps the heart insensitive to cold until it's down to 9 or 10°C? Respiration and cardiovascular support and fluid balance. The point I'm getting at is that it's not the temperature that is important. It is the consequence of the temperature that really is the important thing. When we are talking of treating people, it is not the temperature correction we should be primarily concerned with. It is the support or therapy of these other systems which have been influenced by temperature initially but for which the temperature is now incidental. If you support these other things, the temperature will follow on eventually and come back. Lie the man down and do first aid peripheral squeeze with inflatable splints to increase the venous return.

Tonjum: I agree totally with Frank Golden. In my experience from the clinical side, we have many old people exposed to cold stress. What we did was to put them under blankets with intravenous fluid. We are talking here of hot bath rewarming but if you can do that, you surely can use intravenous fluids. That's probably more important and will compensate the hypovolemia that will occur.

Golden: You want to be careful that you do not put it in too fast because that will upset the central circulation as well.

Tonjum: If you don't use too much and use slow rewarming, there will not be too much danger.

Kuehn: I'd like to turn this whole conversation to a practical point that came up over lunch. I realize now that we perhaps should have included a session in the Workshop devoted entirely to rewarming considerations. This is a good time to have an attempt at that. In the event of rescue of people in bells who are hypothermic and who must experience decompression before being brought back to a one atmosphere pressure, how are we to apply rewarming to those persons, given the decompression constraint? The remarks on rewarming so far are well suited to immersion or mountain hypothermia at one atmosphere. When you have a cold comatose body in a bell under hyperbaric pressure, what do you do? In our last Workshop session we talked about the modelling of the developing hypothermia of the lost bell diver and how to prevent it from happening. What should we recommend, though, to the situation of treatment of the hypothermic lost bell diver? There is the constraint of the long decompression delay before he is in medical hands. What should the diving doctor do for him before he gets back to one atmosphere?

Hamilton: You would do the same thing as you would at one atmosphere before beginning decompression. Decompression should take place only after rewarming has been completed. The rewarming has to be done in a chamber and the doctor will have to be locked in.

Kuehn: Was this ever done in the field?

Hamilton: Well, we have some cases when it wasn't done!

Long: We have a case right now and permit me to say this as a neutral party. In one case, a bell was lost for 17 hours and brought back up. A lot of things had gone wrong but for the sake of this discussion we will only talk about the thermal aspects. The divers had fallen down into the trunk and the bell door had to be jacked up. The medical doctor in the decompression complex reached up into the bell, felt each man, and as I was told by someone who was there, put his hand on the side of each neck and on the arm of each diver and could find no pulse. The smell coming out of the bell was very foul. The doctor then pronounced them dead and the bell was ordered to be decompressed to the surface. The uproar caused in the diving community was incredible.

Kuehn: Was the bell decompressed explosively or was it brought up on a schedule?

Long: It is my understanding that it was brought straight up.

Kuehn: Well, in that case the divers were surely dead at the surface.

Long: The diving community was very upset. I know there are no guidelines for this kind of thing. It is a very touchy subject, since the investigations are not over. It isn't important as to who was at fault. It is important as to what we do next time.

Kuehn: I like Bill Hamilton's comments about getting the man rewarmed in the bell. That's not going to be very easy because of the small confines and limited access.

Hamilton: If you have a bell, you have a mating deck chamber at

the surface. It's rare to dive without this support. You can assume that, easily or with some effort, you can mate the bell to some chamber and take the people out into the deck decompression chamber for treatment. You have to get rewarming accomplished before you decompress. There is no hope of successful decompression of a hypothermic person. Moving of the patient should be done as gently as possible.

Hayward: What level of hypothermia are we talking about?

Hamilton: I would say that anyone who is debilitated to the point of unconsciousness should not be decompressed. Anyone who is conscious and cold is not nearly so bad off, but if it were my body at 36°C, I would prefer that you wait before you decompress me. The decompression procedures we use are for warm 37°C thermoneutral bodies and they work badly enough for these cases. With what we know now, the person has to be completely rewarmed before beginning decompression, unless there is some overwhelming extraneous concern.

Kuehn: Well this is important, because many of us have had subjects at depths as great as 300 fsw in cold water experiments and have initiated decompression when the subjects were down as low as 36°C without due concern for what this might mean.

Hayes: I think that all you can do is be certain that you do not decompress a comatose or semi-comatose diver. The only job that you can perform on him while he is in the bell is to try to remove him from any subsequent cold stress. You make a reasonable job of insulating him. When you actually manage to lock the lost bell on to some

with no immersion suit? He is brought out of the water in a semi-conscious state with a 30°C core temperature. We know that the rectal temperature will decrease even in a bath. If you don't have a bath but have him on the floor of a wet raft, or on the deck of a ship, he can experience post-rescue collapse. Could not there be, in that instance, loss of heat, not only from the rectum to other tissues but even from the thorax and head to the environment and other tissues? So afterdrop is true in all sites in the worst possible situation.

Golden: I really do believe, and I would appreciate other people's comments on this, that it does not matter what the temperature is, whether it's 29 or 30 or 31 or 32°C or whatever. The thing that is going to kill the man is the state of his cardiovascular system. Admittedly, cold will eventually affect this system but there are so many other things going on which have a much more important effect that will produce arrest.

Hayward: But if a man who is hypovolemic and hypotensive and is laid out horizontal, you say that will get around the post-rescue collapse. I bet you will find many people who are rescued unconscious and who are horizontal all the time in water situations, or even in air situations where hydrostatic squeeze has not been involved, who will die after rescue has occurred. They may die during transport when there is not a suspension of the ongoing cooling of critical core tissues such as the heart. The myocardium is cooled so that it cannot maintain its rhythmicity.

saturation system, then it is a toss-up as to whether you actually turn the saturation system into any sort of intensive care system or whether you should just leave him for as long as you can to restore his own thermoneutrality basically on his own. It would be rare that this situation would arise and that a medical officer is placed in that position.

Long: We do know in the diving industry that in the next year or two we are going to have this situation again. Unless someone takes a great deal of courage to come forward and tell us what to do, we are going to do what we have done in the past and pay the same price. I realize what a horrible responsibility it is for the medical doctor, but someone has to step forward and say "This is what you do, Sir."

Manalaysay: Some of the guidelines that have been proposed for dealing with a prolonged drowning victim in cold water should be applicable here. The initial evaluation is not really that adequate and the presumption that just because a body is cold and without a pulse does not mean that the body is dead.

Kuehn: You have to be warm and dead in this business to be really dead.

Zumrick: At the US Navy Experimental Diving Unit, we get a lot of calls from people with various problems and we have some of our own cold exposures that may require rewarming. In general, we end up fielding more calls in which people, trying to react aggressively to a situation, end up making the situation worse. If you have a diving

complex with saturation capability, you can stay at depth until the hypothermia problem is solved.

Golden: Can I translate the points that I was making earlier into practical advice: If you have somebody with a core temperature between 30 and 34°C, and chances are that you won't even be able to determine this in the field, you have got a cold diver. He is still alive and unless his temperature goes down further, you know he can survive his current temperature. The first thing is to prevent further heat loss, to prevent that temperature from going lower. He must be insulated. The next thing that we know that kills these people is the cardiovascular stress that I mentioned. You must avoid any maneuver that would increase demand on his cardiac output. Lie him preferably horizontal or slightly head-down and you insulate him and that prevents the requirement to increase his heart rate because of these other things. These things, then: you know he is alive, keep him at that temperature, insulate him to prevent heat loss and do not make any cardiac demand on him.

Kuehn: Would moving the body from the bell to the deck decompression chamber impose any demand on his cardiac system that carries the risk you speak of, Frank?

Hamilton: You don't have any choice, though.

Golden: How long are we talking about, the interval between his recovery and this movement taking place?

Hamilton: Well, you get the bell and bring it up on deck. That may take one hour after the attachment of a cable to the bell. These

people are cold and unconscious in the bell. After mating the chamber, you have got to pry the bell door open and get someone in there to get them out. You may have a horizontal mating or a vertical mating. You have a problem. One of the things you don't do with a hypothermic person is to move or disturb him. Yet you have to make a decision as to whether we can blow up an air mattress and plug in his hot water suit for treatment in the deck decompression chamber or in the bell. This will be decided on the facts of the case. But John Zumrick is right, don't do anything if you can avoid it. If you can stabilize him in the bell, that might be the best thing to do.

Golden: I agree that you don't do anything aggressive, because if you do, you know that you will run into all sorts of monitoring requirements and you don't even know his temperature. If he is alive and you can insulate him, then his residual metabolism will bring him back.

Hamilton: Unless he has been in immersion and has the drowning complication.

Golden: That's a complication. Let us look at the simple case first. If you wish to move him from the bell to the chamber you may find that it is just a simple requirement of putting on inflatable splints on his lower limbs that will give him some circulatory support while you transfer him into the main chamber. But that's a practical point and I'm afraid that I'm not the best qualified to answer it.

Kuehn: I think your advice on the risk of movement and the use of

splints should be made known to the diving doctors who may be faced with this problem.

Zumrick: What we are talking about is fundamentally different from what has been advocated many times, i.e., very aggressive treatment for people with low body temperatures. I've always been disturbed by the general medical textbooks in which various hypothermic cases have been reported as aggressively treated.

Golden: We are dealing with different situations though. If I was dealing with immersion hypothermia victims and a hot tub was available, I would put them into it. If there is no doctor around and you are giving practical advice to laymen, the victims should be laid horizontal and tilted head down and insulated. You may lose a few that you may have saved had you been a doctor, but you will do little harm this way.

Hamilton: In diving emergencies, the doctor himself may be the biggest problem because you may get a doctor who knows less than the divers about diving illnesses.

Tonjum: I agree that you should not be too aggressive but I have a feeling that if you are speaking of insulating divers you are bringing back a hypothermic environment in which they have been many hours. The deck decompression chamber will have a temperature of 30 or 32°C. If they can be brought into this chamber and covered with blankets, that should be enough to cause slow rewarming.

Long: Your recommendation then is to move them to the main chamber, put them in a bunk and cover them with blankets rather than treat

them in the bell?

Tonjum: No, I didn't mean that. I was speaking of the degree of insulation. A warm gas temperature and some blankets should be enough to cause rewarming.

Golden: Let's add some monitoring advice. Monitor the pulse because you have nothing else. Monitor it and keep a record of it. If it starts increasing, drop the temperature very slightly and keep him at the temperature he is at. When his pulse has recovered, then you can bring the temperature of the bell back. Continue this until he has recovered. It will prevent rewarming collapse.

Schmidt: Would you want him to breathe through a DUI Hamilton scrubber/thermal regenerator so that he gets warm 50°C gas?

Hamilton: If you can get warm heliox into the bell, you don't need to do that. Warm heliox is like a water bath.

Golden: But you don't want it too warm for the reasons that I have given you. You want to monitor the pulse and if it climbs you know he is getting peripheral vasodilation and hypovolemic shock. Drop the gas temperature then.

Hamilton: The message here seems to be "Just don't be too much in a hurry."

Golden: If the man is alive, don't kill him by your interference.

Long: One of the questions I would like to ask then because I'm certain that this is a planned course of events: Ways are being considered to lash lost bell divers into their seats in a vertical position so that the bell doors can be opened on rescue. Most bells

have bottom doors and a few have side mating. It is not impossible to roll one over but most chambers cannot handle side-mating. So the occupants know that they are going to be there a long time and are at the final stages of their consciousness. They know they will be unconscious on rescue and they fit themselves into their seats and strap themselves in an upright position but leaning forward. If I am the safety officer and I know that I am recovering my divers after 24 hours and that they are going to be very hypothermic, have I increased my chance of killing my divers by forcing them to sit in that upright position, just so I can get the bell door open? What I could do is lie them on the bottom of the bell, pick it up and roll it over on the deck and bring it back slowly so that they are stacked on top of each other. Then get in there and get the door open after mating with the chamber.

Kuehn: Could we have a response to this query from those of us who have been studying rewarming? Is that a good way to find the victim? Slumped over and seated?

Golden: I think that that is a disastrous way to do it. They could suffocate.

Hamilton: But it is disastrous to have them lying on top of the hatch, which you can't open. You have to compromise.

Long: So it would be this group's feeling that it was a good thing to do? Relax the guy in the vertical position? We won't have a blood pressure problem?

Greene: How are they going to lash themselves in a vertical

position?

Hamilton: They do that before they pass out.

Long: They put a harness in there and tie themselves up before passing out. They may have to sit in there till the end of their time, till they are rescued or they die.

Kuehn: This position does not permit the carrying out of some recommendations to increase their survival by reducing heat loss. When wearing a survival bag, you want the diver to have as little contact as possible with the bell.

Long: Assume that they have a survival bag adapted to the harness. They wouldn't put the harness in there if they didn't have means for using it with the bag. We're talking about the divers awaiting rescue. One diving company is going ahead with the harness idea and it would be very useful if I could get back to them with some information from this group. They are going to insist that the divers be in these chairs in their harnesses and when they are recovered unconscious, they are going to be slumped forward. If you had a hypothermic victim that was hypothermic for many hours and you have your choice of having him lie horizontal or vertical, in which position will he last longer?

Golden: I think that if the diver is unconscious and sitting that way that he will be suffocated long before he dies from hypothermia. He is just going to choke.

Long: Then that is a reason why we should not do that!

Hamilton: Can't we get these people to put themselves into a posi-

tion such that the hatch can be opened and still not be sitting straight up?

Long: But that is the recommended solution to the problem and they see nothing wrong in doing it.

Hamilton: Well, let's tell them that we are not sure that their solution is the best procedure and that we do not know definitely what the consequences would be.

Pozos: A brief intermission, please. The University of Minnesota will sponsor a meeting next summer on the identification of the accidental hypothermia victim and the treatment thereof. Some of these points can be taken up there.

Long: The diving company I have in mind is going to proceed on its intended action in the next few months. I feel honour-bound at this point to forward this information on to them.

Pozos: Some of us could do some experiments quickly to get some information on this problem.

Long: If the diver is going to fall over and choke himself, we have no choice but to lie him down. To lash him in a vertical position is an easy thing to do and unless we have a good reason not to, that's what they are going to do. I would say from what you told me, they should not do it.

Vorosmarti: A diver does not have to be lashed in vertically. He can be lashed in another orientation.

Long: He is in a 66-inch diameter sphere with another diver. By virtue of the size, he doesn't have much choice.

Vorosmarti: The chair doesn't take up more space if it is vertical or semi-reclining. His head must be back and supported. Has anyone explored the various angles of reclination that may be possible?

Long: It's well worth your time and effort to investigate a different position other than lying over the bell door and sitting in a vertical position, leaning forward. We need to explore a third alternative. That's all I need to go back and tell them to reconsider.

Hamilton: Everybody agrees that if they can lie down horizontally and not cover the hatch, then the problem is solved.

Kuehn: But in this case of lashing, could not the head be lashed up by tying the helmet or bunny cap back.

Golden: A collar or yoke would probably help, but you would still have the problem of the tongue. I imagine that in large bells, the divers could lie down on the floor but I can see the problem in small bells.

Long: Well, you've kindled some thinking and given me some ideas.

Hamilton: They don't have to be prone; they could be curled up-- as long as the head is down.

Long: The real problem is that we have to be able to get the door open, not where he is sitting.

Golden: I would like to come back to the vertical position thing again. I may have over-complicated the issue. Certainly, immediately on rescue, the vertical position is bad; but if you have somebody

in a survival situation who has cooled progressively over a period of hours, then he is peripherally vasoconstricted and will remain so. The vertical position is not then as critical as the post-immersion rescue when the peripheral hydrostatic support is suddenly removed. Nevertheless, keeping the man strapped vertically is undesirable with the major disadvantage being one of potential asphyxiation. When his head falls over, he is going to suffocate. Don't over-emphasize the blood pressure point of view. That is an immediate rescue or rewarming problem.

Manalaysay: In addition to the monitoring of the pulse, in some people there is no problem to the monitoring of the neck veins. These will give you a good idea of what is happening in the volumetric status and, while not always possible, it is plural.

Long: When you say monitoring of the neck veins, how would you do that?

Manalaysay: Just look at the neck veins; cardiac action should be evident. It is easy to teach someone this technique.

Long: That's the kind of thing we would like for our guidelines. You said it and I heard it, but I don't know what you said.

Manalaysay: You must look at the neck vein as a manometer, and see how high the cardiac pulsation comes. If the diver has distension beyond his clavicles, he is not that hypovolemic and you're probably safe.

Hamilton: That's a good point. It needs to be there but you can't always use it, say when the guy is fat.

Vorosmarti: In a couple of slides you showed the man whose temperature initially went up in the dive, in the uninsulated bag in which he continued to experience a decrease of 0.5°C in rectal temperature over a two-hour period.

Hayward: These people were warm on leaving the dive and lost about 0.2 or 0.3°C . What we did was to have the bath water temperature at about 40 or 42°C . We made sure that any heat loss they had from the periphery was such that they felt good. After about an hour in the warm bath, not having had any significant core hypothermia, they complained of too much warmth at 42°C so we allowed the bath to drop down to a thermoneutrally-comfortable temperature. The effect of general inactivity is to let the core temperature drop.

Hamilton: It looks like you've shown what we would think intuitively to be the case, but which has not been documented in the literature so far, namely that the person who is warm while he is working and cool while he is being decompressed is the one who has the problem. The one who is cold when he is in the uptake mode on the bottom does not have the most bubbles. It is not the cold during the dive but the cold during the decompression that causes the problems.

Hayward: Cold and perhaps lesser activity are the factors that interrupt gas uptake and these are the important factors. I think that it is the skin and not the core temperature that is driving the peripheral circulation. Those things need to be looked at both during the uptake and loss phases.

Hamilton: I've been trying to figure out a variable to stick into de-

compression computation to account for cold. Core temperature doesn't do it. The perception that a diver has of cold is much better correlated with how much effect the cold has on his decompression.

Hayward: The perception that a diver has of cold is highly correlated with how cold his skin is.

Long: I can give you some field data that will support your point. When diving first started in the North Sea, divers were brought in from the Gulf of Mexico to dive in wet suits to assist in the laying of gas pipelines. They had to dive through pipes and were on the bottom about 40 minutes or so. By the time they were ready to do any work they were very cold and had to be brought up. Because they were so cold and the water was so shallow (about 150 feet), they could use surface decompression. The chambers were very cold, so the divers were brought out as fast as possible. The 10 and 20 foot stops were eliminated and there were no bends; however, the job wasn't done. The divers then put on hot water suits and were brought up on the same protocol. The first of these divers was quickly bent and one of the first things that was considered was that hot water suits bend divers. When they went back to the standard decompression schedules, they didn't have a problem any more.

So the problem in decompression is a two-fold one. If divers are cold initially when they first go in and vasoconstriction occurs right away, they don't take up that much gas. If they wear dry suits and work hard, their core and peripheral temperatures are high; then if they are forced to go through long decompressions in which they become

cold, particularly at the 90 foot stop in in-water decompression, then we begin to see very severe decompression problems and schedules have to be extended.

Hayward: We should emphasize here that in our experiments we were applying a very strong cold insult. Does one need this level of peripheral cold stress accompanied by core cooling of this magnitude to get this benefit of an apparent lesser uptake of inert gas during the dive? I think that there must be milder cold stresses, perhaps with skin temperatures of 25°C or so, that produce enough vasoconstriction response to give a similar effect on post-dive bubbles.

Long: I would suggest that the reduction in diver capability and the increased potential for making poor judgement rule against that. We are better off to keep the diver as warm as possible.

Hayward: Could I ask a question about the mildly hypothermic person who looks and acts and feels very cold, someone who is at 36 or 36.5°C and needs decompression? A hot water suit is available; but should he be rewarmed during decompression? At what level are we saying "Don't give him that type of aggressive heating" unless we can monitor very carefully? I agree with Frank Golden that treatment should depend very much on who is there, a diving supervisor or physician or whatever. That's important in the deeper levels of hypothermia. Do we have any right to say "Look we know this guy, he is only down to 36°C, we would like to give him the pleasure of rewarming in a hot water suit?" Are we saying that in no circumstances that should be done?

Kuehn: No, not at our place. The divers ask for it and seem to prefer it.

Hayward: But that's you who knows that they are safely in hand and mildly hypothermic. But what about that safety officer who doesn't know?

Tonjum: You are speaking of two different situations. You have one case of slow cooling and one of rapid cooling. In rapid cooling, there should not be any problem with warming on decompression.

Long: I'll tell you what will happen in the real world. The guy will come in and if he is conscious at all and in his hot water suit, they will lock him on to the main lock and open the bottom door. The guy will be in the suit and they will plug it in and turn it on and

they will flood the dickens out of him and he will love it.

Hamilton: If he can say "That's what I want", it won't hurt him.

Golden: I think I agree with that. We are talking about slightly different situations as compared to the immersion victim. I'd be afraid to refuse the diver!

Hayward: I would like to have that on the record because it was sounding as if the treatment was not to give the cold diver anything as aggressive as that.

Long: You'll never have the choice. You will never have that level of control.

Kuehn: The thermal investigators working with divers don't consider hypothermia a problem till the core temperature is down to about 35°C.

Hayward: I agree. I've taken subjects down as low as 33°C and rewarmed them in a bath. But how is his judgement in a bath? How does the safety officer make this judgement? Does he look for consciousness or a certain level of consciousness and go ahead and flood the suit?

Long: It's out of our hands and in the hands of the diving safety officer.

Hamilton: But it's not out of our hands. We are discussing right now what to tell them.

Long: That's a different story. Today they will take things in their own hands and treat as they see fit.

Vorosmarti: If the guy is conscious, even though he is not making

much sense, but if he is conscious, I don't see any problem with rewarming.

Golden: With one exception. That is the people who have been in a disaster situation and the bell is recovered and locked in and you have medical help available.

Vorosmarti: That's right. I'm talking about the guy who's been in the water and flooded out and who has had an acute problem of hypothermia.

Golden: It is the chronic case with hypovolemia who presents the problem.

Hayward: Are all divers who get cold in bells suffering "chronic" hypothermia? What is the dividing line in terms of physiology? I'd assumed that from the data I've seen on diver cooling at this Workshop that it is fairly slow. Is it chronic hypothermia?

Vorosmarti: In the practical situation, most are acute, because they have been operating from a bell in a hot water suit and lose power.

Hayward: But they don't reach the unconscious level of hypothermia. How long does it take to get there?

Vorosmarti: I don't think hypothermic unconsciousness would happen in the bell situation unless it is lost.

Kuehn: It is 6 to 8 hours before you get hypovolemic changes -- of the order of half a day.

Golden: That's correct.

Long: We are much more apt to see what you are describing in a bell diver who loses hot water five minutes before the end of a job. He

just gets it out and the bell is not lost. If we have lost a bell, then we have a gradual chronic hypothermic problem and a different problem. The diving supervisor recognizes that and is not going to do anything without checking with everybody.

Hayward: I thought that what was being referred to was two ways to get to deeper levels of hypothermia, say 30°C: fast (acute) or slow (chronic). There is a lot of discussion on the rewarming strategies for each. For five minutes of cooling, I wouldn't even call that acute; that's skin hypothermia!

Long: Thirty minutes, an hour, the tender and everybody freeze away, they can't move and are incoherent in speech but they are still alive.

Hamilton: You must remember that we are dealing with a tradition that has been built up with only cooling in air. You have got to cool a person a long time in air to get him to 30°C. Even in water.

Hayward: I still want to know how long it takes to get a man to 30°C in a diving bell.

Hamilton: Does it matter how long it took to get there as against the fact that he is there?

Hayward: I think that it does matter that he is there but I thought that the point was being made that the slow versus fast way of getting there, acute versus chronic, did matter. Correct me if I am wrong.

Tonjum: I think that it does matter. We've just been speaking of chronic hypothermia as requiring more than six hours but experiments

done by Phil Hayes on the loss of water supply at 300 msw showed that his subjects dropped down to 30°C probably within an hour.

Hayward: I would call an hour acute.

Hayes: The problem with that is if you're at depth and lose your hot water, you are likely to drown in your own mucus before you die of hypothermia. You are going to be very cold in two hours or less if respiratory problems do not arise!

Golden: Can I just try and clarify John Hayward's problem? We were talking about two different situations, acute and chronic. Dismiss the chronic, that's the bell disaster situation with people in their sleeping bags. In spite of those precautionary measures, they are still in imbalance and are cooling and when you get them to the surface, you have the chronic situation. The other problem is the acute situation. In one case we have the chap who has just lost his hot water suit supply and he is awfully cold and in the other case we have the man Phil Hayes was talking about, a man as cold as 30°C. For the man who has just lost his heating, you have to rewarm him, because he will punch you if you don't. For the man Phil is talking about, you are back to a 30°C unconscious individual who should suffer no active interference. Although this second case is acute, you don't rewarm him, not because his temperature doesn't demand it, it is rather his state of consciousness and the difficulty of actively interfering with somebody who is unconscious.

Kuehn: I think that summary is a good one on which to close that topic because time is important. Some people have to leave shortly.

I would like to leave about another 10 minutes or so for extraneous questions before I give you a small task prior to our departure.

Golden: One last comment, particularly for you, Lorne. For people who are interested in the shivering at one atmosphere and the influence of CO₂ on shivering, may I recommend the avalanche literature. These people who are buried by snow use plenty of oxygen but there is a big buildup of CO₂ and they get hypothermic very quickly because of the inhibition of shivering by CO₂. They are extremely hypothermic when rescued after a few hours of being buried by snow. The literature is primarily Swiss and I'm thinking primarily of the Institute of Avalanche Research in Davos, Switzerland. They have a book of conference proceedings now with lots of good clinical examples of people who were recovered in these situations.

Kuehn: Thanks, Frank. I will make a point of reading that literature.

Hayes: I have a comment and a question as well. We seem to have spent 95% of the time talking about saturation diving and 95% of all diving is performed in water less than 30 msw. I think that we have a good view of the physiology of surface types of diving, in an immersion situation at one atmosphere. The models seem to fit and most of the criteria are applicable to the bounce diving approach. I'm not so sure that we understand the physiology so well for the saturation diver and the problems associated with warm skin and cold gas, and the like. It worries me if we try to apply the same sets of standards and criteria to cover the whole sphere of diving. To say

tomorrow that the result of this Workshop is an update of the various limits for all diving is worrying . It may well be the case for one sphere of diving but I don't think that it will work for all of diving. I'm not suggesting that the limitations are divided into two parts but some regard should be given to the fact that we almost have two different physiological animals when we get to the hot water suit situation.

Kuehn: An excellent point. It is true that we have not spent much time on the energetics of chamber thermal comfort. One reason that we haven't is that it has been well reviewed in the past and in view of many of the new things that are happening in subsaturation exposure, say bell exposures, which seem to be very topical, I wanted to concentrate on them, but I agree we shouldn't forget the other problems of chamber thermal comfort.

Long: The saturation diver's problem will soon be solved because so many people are working on it. The other problem is there and it will be the worse of the two because the other has received so much attention. I second your comments and want to come back to the field guidelines I mentioned because it is a start in the right direction.

Kuehn: To follow on Dick's comment, I want to recommend that we all independently communicate with him on improvements to the guidelines. As a Workshop, we should also endorse the training scheme that he has proposed for a potential UMS activity. Are there other comments and pertinent comments to make to any of the sessions?

If not, Paul Webb has left me one charge before we depart, a charge that he himself made six years ago in Yellow Springs in the first UMS Workshop on this subject. I want to ask each individual

who is a thermal researcher, or who wants to become one, to comment in two areas: first, what his next thermal research area is or will be and second, what would he really like to do if he had sufficient resources. Two different questions. So I propose that we go around the room and ask each person to indicate what his next planned thermal activity is and next time, we go round, what should we be doing. In the next six years we will have had another one of those Workshops. We have made so much progress in the last six years but, as we have heard, there is so much more to be done. What are we planning to do in 1980-81 and what should we do before 1985?

Hayward: With respect to this Workshop, what I would like to do is get some data on the duration at which various levels of shivering can be sustained in the general population. How long can individuals maintain certain levels of shivering heat production which seems to be very important for models in terms of long-term survivability in bells or hypothermic environments?

Harnett: Well, I have difficulty in separating your two questions because if I don't get support, I can't do anything. I would like to do a study of comparing rewarming schemes on non-human primates, subject to deep hypothermia, specifically looking for the appearance of acroteric core sites of interest during various rewarming schemes. I'm also interested in the sensitivity of peripheral blood flow to temperature and variations in rewarming schemes. I would like to look for imbalances in the electrolyte concentrations between the peripheral and central blood. There is some concern about the shock

trousers or inflatable splints. There are other explanations for hypothermic collapse that involve electrolytes.

Schmidt: I'm not an active thermal researcher per se so I really couldn't comment on the first question but as far as what needs to be done, well, as John Hayward has said we need to find out what level of shivering and oxygen metabolism can be maintained for 12 or more hours. The model that I presented assumed 2.5 times normal resting rate. Gene Wissler's model assumed that we would reach physiological fatigue at certain levels of shivering and we really don't have good numbers on this and it's something that we have to know.

Layton: I'm a virgin in the field but I have an interest in respiratory physiology. I will be undertaking a project to examine in detail both the pulmonary and cardiovascular effects of cold gas inhalation and warming with heated gas. We have to look at other things besides core temperature and whole body temperature plexus. It may be that some of the pulmonary and cardiovascular parameters are not only acute in nature but are very serious and need to be looked at.

Libber: Well, we don't do any research in our office but we have been supporting research in cold water immersion for a number of years and we are recently getting involved in something that is related, namely the effects of dry cold stresses on our Marine Corps personnel. We are interested now in the interaction between altitude, hemorrhage shock and cold. Not much research has been done in this area and it is a very practical problem.

Pozos: Assuming that I have a number of lifetimes, I'd like to quantitate shiver. People have used that term very loosely in the last two days and my goal is to quantitate it and show a relationship with the various kinds of neuromuscular activity associated with hypothermic stress and oxygen use with CO₂ production. In addition, I would like to characterize the skin and other thermal receptors that might be involved in maintenance of the amplitude of shiver and any alterations in the electrical activity thereof. Finally, to see whether or not what happens at one atmosphere is related to what happens in a diving bell.

Zumrick: It's clear that over the next year we will probably be looking at various combinations of diving gloves to use with the 6-hour garment that has been developed and characterizing the ability of the glove to permit people to perform things. In addition, we will be characterizing that suit in the failure mode in various types of water so that we can predict how it will function on a great variety of people. There will also be some studies going on, looking at regenerative heaters as they influence respiratory heat loss at depth. There will be some study to test the new Kinergetic heaters and tie up some of the loose ends on the new neoprene inspiratory gas standards. Those are the things that are going on at EDU. There is more interest in what is going on with the respiratory heat loss at depth. As opportunity presents itself, we will look into some aspects of this.

Golden: My ambition is to try to follow up on the immediate post-

immersion collapse and death and to look at the cardiovascular changes occurring at that stage. I'm also involved in swimming experiments at the moment and would like to pursue this adaptation thing a little further.

Long: Our next area of work, being mad inventors, will be to introduce a line of products that will use Thinsulate, taking full advantage of its unique properties while trying to minimize some of the disadvantages that we see in it. I believe we will be successful. What I would like to do, if I had the money, would be to produce the field guidelines for the diver and to move the information that we have to an applied basis, much as we have done with Polar Bear, so that the divers in the real world get the benefit of the knowledge and expertise that we have here.

Tonjum: We plan this year to provide the data for various heat loss models and we feel that this could be quite important. If the models are verified we could use them for novel situations. We would also like to look at mental performance in mild hypothermia. Another thing would be to collect as much data as we can on divers in actual saturation to know what is going on. We are limited to the number of divers we have offshore but we will try to get data from them.

Manalaysay: We will be looking at the results of varying pressure and depth and cold stress on catecholamine levels in divers as well as asymptomatic hypothermia as I think that this is an area that needs some attention.

Hayes: I think we might be venturing into the area of trimix, at

least for awhile. I'm going to try to measure various heat transfer rates in trimix. Other than that I'd like to work, as Frank Golden has mentioned, on the various bits of Burton and Edholm that worry me as well. It's basic physiology but I'd like to sort out the few things that might be pertinent to diving such as rates of change of storage and what that means.

Nuckols: Speaking not only for myself but for the Coastal Lab, we will be continuing the development of the 6-hour passive system and the continued development of the heaters. As John Zumrick pointed out, we want to continue an extensive effort in passive glove development. Working with Gene Wissler, we want to more fully characterize what level of insulation is required for various environments and metabolic levels. Personally, I have a small effort in the respiratory area where I am looking at the respiratory tract in a strictly engineering manner, as a mechanical heat exchanger, in which I'm measuring the convective heat transfer exchange coefficients so that you can then look at the sensitivity of flow rates, gas density and composition on the heat transfer and penetration in the lungs.

Vorosmarti: The general Naval Medical R and D Command is pledged to provide specifications for equipment design and since half the people in this room get money from my program or are actively engaged in cooperation between the different programs and laboratories, they are engaged in doing this-- the provision of specifications to design engineers of equipment and navy diving.

Kuehn: Well, Paul Webb has left me with some notes to pass on to

Vorosmarti: His model goes both ways.

Long: That's terrific if it's in his computer. My guy is in the field and the hyperthermia problem is not in the university, it's in the water. In identifying it, giving us field guidelines or knowledge that will be imparted to the diving supervisor and diver, this is what we need. Overheated divers have been treated in cold showers and survived— without knowing how close they were to death.

Vorosmarti: I'm not so sure that we need a lot of research on the hyperthermia side to solve some of the problems that may be coming up, in particular, your last example. I think the problem there is the training of the chamber operators to do their job properly.

Long: Train them with what? Who has got the text book?

Vorosmarti: They should know the consequences of their chamber actions, what happens when valves are turned, etc. If the wrong valve is turned and the guy is heated, no amount of medical research helps you because you know the guy is going to cook if you raise his temperature. I think a lot of the hyperthermia side is training of the supervisors in running the chambers and following temperature guidelines. I'm not saying that we shouldn't do anything, however.

Long: I'm not challenging the logic or truth of what you have said but in the real world, they don't know it, they don't see it and they'll do it again. Our chambers are more crude than yours. I'm more concerned about the guy sitting in a dry suit in the Thinsulate garment in a diving chamber on an exposed deck. He has got a problem. Maybe "research" is a poor word and I should have used "work

you. He states that he wants to get into long and slow cooling experiments with a performance end-point, plus quantitative measure of heat loss, metabolism and body temperatures. What he would like to do, if he didn't have any constraints, is to develop a monitoring method for warning of performance decrement associated with general hypothermia.

Of course, you must have gathered by now what my next goal is going to be, trying to survive the shivering criticism that I have engendered. You'll be hearing more about that because it is very much on my mind.

Is there anything now that we are missing? We've talked among ourselves. Is there anything that none of us is trying to do but which should be done? That's the second question that I posed. Do we have all our bases covered?

Long: If someone isn't doing it and it seems that no one is, I see that hyperthermia is becoming a problem because we are getting too good at diver heating. I would suggest that this subject is a worthwhile challenge. What is the effect of having it in the water, in the bell, etc?

Pozos: I think that somebody should look into the subject of drug interaction with hypothermia. In the real world, this is a real problem.

Schmidt: Dick Long mentioned hyperthermia and I just wanted to mention that Gene Wissler, who is not here now, has an excellent model for this.

study, report, etc."

Hamilton: I think that we have to keep in mind whether we are dealing with survival or diver comfort. I've spent a lot of time in the last 20 or 30 years worrying about survival as an aviator and in the diving business. We tend to lose sight of the fact that a parachute is not a very good airplane, but it does the job. It gets you to the ground safely. We need to keep in mind that maybe we should use a guy's ability to shiver and use his ability to vasoconstrict as survival tactics. We don't know enough on these points. To save having to put survival bags in bells and to know what needs to be done, this is necessary. Survival bags won't be used in bells in the Gulf of Mexico. I bet not many are sold there, because there is no authority to do it. They don't recognize the problem but the diver who happens to be there in a lost bell at 1000 fsw will wish he had one. If he knows what to do and if his topside people do, then he may survive. If we can bring this information together, and put it into training, that would be great. We need to know more and disseminate it better.

Schmidt: You've mentioned the Gulf of Mexico and, ironically, this may be a place in which they need the Polar Bear system as much as any other. If you look at the insulation thickness that you can put on a bell or hyperbaric chamber, taking into account the seasonal temperature variations in the North Sea, you can put a goodly amount of insulation on it that will markedly cut down the heat loss at colder temperatures, whereas in the Gulf you can't put that much insulation on in the summer and in the winter you are very vulnerable

in the lost bell situation because of the minimal insulation.

Hamilton: This is a bigger problem that just hasn't come up.

Long: That is a severe problem for guys decompressing in South African waters. They are brought up in a bell and sit in the bell mated to an exposed deck chamber and decompress. They sweat terribly in there and the temperature increases greatly.

Vorosmarti: That's a common sense problem! If they don't pour cold water over the bell or keep a tarpaulin over it to keep it out of the sun, then what good is 10 million dollars of research on hyperthermia?

Long: But no one knows. No one tells the oil company or the diving company that that is not what to do. They persist in their actions.

Vorosmarti: I'm not sure that this is a UMS problem. There are some standards in the US Navy for chamber use in hot climates. It is in an NSMRL report and studies are now being done to validate the limits.

Hayes: In terms of the fatalities from hypothermia and hyperthermia, it may very well be that on the hyperthermia side, it is a great lack of common sense. If I remember rightly, there have been eight deaths throughout the world in diving this year due to hyperthermia. That takes into account these hot water diving areas.

Kuehn: Well I would like to thank all of the speakers and observers for coming to this Workshop. I found it a great education myself. We have come a long way since 1974 and we have a much better grasp of some of the basic physiology and certainly the technology base is

very good compared to what it was six years ago. Yet when you look at the various remaining problems that have been brought up in discussion we have a longer way to go in the true understanding of the physiology of saturation and sub-saturation diving thermal problems. The technology itself, although embryonic and presently satisfactory, must improve to meet the demands of the deeper and longer dives planned for the future.

I want to thank again the Undersea Medical Society and Dr. Shilling and Dr. Paul Webb in particular, for organizing the basis of this Workshop and certainly the United States Navy for its financial support in getting us all here at one time. We try to have an informal UMS Thermal Committee meeting once a year and I was just telling Gene Wissler before he left that we don't have to have one this year because this Workshop has been it! We've had most of the players in the game here and I think I've seen an advance in the consensus of knowledge in this field formulated right here. I want to assure you as Chairman that I will go through the tapes and papers and try to produce a document that reflects these concerns. It should serve as a reference for the next three to four years at which time I hope the UMS does host another Workshop to review newer and more important problems than the ones that we have now been looking at.

So thanks again for coming and making this an enjoyable meeting.

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