due to a calcified lymph gland or other cause outside the ureter, I thought it advisable to pass a bougie opaque to the x rays up the right ureter, to prove that the shadow was in the exact line of the ureter. This was done under cocaine, and I was rather surprised that the bougie passed easily without causing the slightest pain until it reached the kidney, when the patient said he felt a slight pain like the old pain he had suffered from before the stones were removed from his kidney. Another radiogram was then taken, and it was found that the shadow cast by the supposed stone lay exactly in the line of the ureter.

Operation.

The ureter was exposed by an iliac extraperitoneal incision. It appeared dilated down to the place where the shadow had been localized, but no stone could be felt. Thinking the stone might have moved during the necessary manipulations, the ureter was opened and explored, a sinus forceps being passed up to the kidney and a large probe downwards into the bladder, without finding a stone. The ureter was sutured and the wound closed, a cystoscope was then passed, as it was thought the stone might have entered the bladder; there was no stone in the bladder. Apparently the only solution now left was that the stone might have been assisted out of the ureter by the passage and withdrawal of the bougie, and escaped from the bladder while the urethra was still occluded, and lost while the man was walking back to bed.

The mystery was not solved until another radiogram was taken a week after the operation, and the shadow found still present exactly in the same place, proving that it was due to some other cause besides a calculus in the ureter. What the shadow is due to I am not prepared to say; there were several enlarged glands in the region of the ureter, but they all felt soft and not calcified. An atheromatous patch in one of the vessels might explain the case, but the shadow is rather large, and the patient is young for this.

After-History.

This has been satisfactory, the patient being the only one not disappointed by the operation. The dilatation of the ureter with canterization of the fistulae resulted in an immediate increase in the amount of urine passed by the urethra and a gradual diminution of the leak.

The mistake made in this case could, I think, have been avoided by means of a stereoscopic radiogram, and in future I intend to have this done in all cases of supposed calculus in the ureter.

In conclusion I wish to express my thanks to Dr. Watson, Radiographer to Sir Patrick Dunn's Hospital, for his skill and trouble in taking several radiograms of the case.

Arris and Gale Lectures

ON

THE PHYSIOLOGICAL AND PATHOLOGICAL EFFECTS WHICH FOLLOW EXPOSURE TO COMPRESSED AIR.

DELIVERED IN THE THEATRE OF THE ROYAL COLLEGE OF SURGEONS ON MARCH 18TH AND 20TH, 1908.

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LECTURE II.

WE have seen that the analyses of arterial blood in animals, besides being technically difficult, are not free from objection. In the case of man, the difficulty becomes a practical impossibility so far as the arterial blood is concerned. We have, therefore, attacked the problem indirectly. If a condition of diuresis be induced by drinking large quantities of water, the abundant secretion which results will, on analysis, give us some idea of the percentage of gas dissolved in the blood. When the kidney is actively secreting, the saturation of the urine must approximate somewhat closely to that of blood in the renal artery, and its study is of much interest. Our experiments (Hill and Greenwood, 1907, A) were carried out in the following manner. The observer drank at least two pints of water, and ten or fifteen minutes later entered the cylinder, the pressure then being raised a definite amount. As soon as the pressure was stationary he emptied his bladder and then, ten or fifteen minutes later, emptied it again. Samples of the urine were run into glass bulbs with rubber connexions, care being taken to drive out any bubbles of air that happened to be present. The junctions were then clamped and the dissolved gas subsequently

pumped out and analysed. By this device, samples of urine actually secreted in definite intervals, after a given pressure limit had been reached, were available for analysis.

As a preliminary, we determined the volume of nitrogen dissolved in the urine under normal conditions. This was found to be 1.14 per cent. The individual analyses differed but little, so that we are not likely to be much in error in assuming the coefficient of solubility of nitrogen in urine to be, at body temperature and 760 mm. pressure, .01.

Table Summarizing Results at Various Pressures.
(Hill and Greenwood.)

Pressure and Time of Exposure.	Number of Analyses.	Mean	Calculated Value, if Saturation were Attained.
+0 lb.	6	1.14	1.1
+30 lb. up to 10 min.	5	1.99	3.3
+30 lb. more than 10 min.	7	2.32	3.3
+45 lb. up to 10 min.	4	4.29	4.4
+45 lb. more than 10 min.	8	4.23	4.4
+31 lb. (during decompression)	7	3.63	3.3
+15 lb. (during decompression)	8	2.75	2.2
+0 lb. (on decompression)	6	1.99	1.1
+0 lb. 15 min. later	4	1.69	1.1
+0 lb. more than 15 min. later	3	1.13	1.1

It is evident from these results that saturation of the kidney and, *a fortiori*, of the arterial blood is practically complete in ten minutes under a pressure of three atmospheres. For a total pressure of four atmospheres the time would seem to be slightly less; on two occasions full saturation was attained in eight minutes.

Another point brought out in the table is the slowness with which equilibrium is re-established on lowering the pressure. We have always decompressed at a uniform rate of twenty minutes for each atmosphere, but the urine invariably contained an excess of gas at the end of the experiment. With rapid decompression the difference is yet more striking, as will be clear from the next table, the material for which we owe to the kindness of Lieutenant Damant and Mr. Catto.

Observations on a Diver.

Depth.	Time Below, and Nitrogen Percentage Actually Found.	Theoretical Amount.
80 ft.	Below 48 min 20 sec. Ascended in 6 min 5 sec. Sample taken 20 min. 30 sec. after ascent. $N_2 = 1.73$	1.1
84 ft.	Below 46 min. Ascended in 5 min. Sample taken 11 min. after ascent. $N_2 = 1.60$	1.1
80 ft.	Below 46 min. Ascended in 4½ min.: First sample 4 min. after ascent. $N_2 = 2.50$ Second sample 20½ min. after ascent. $N_2 = 1.38$	1.1 1.1
72 ft.	Below 44 min. Ascended in 6 min.: First sample 5 min. after ascent. $N_2 = 2.11$ Second sample 20 min. after ascent. $N_2 = 1.03$	1.1 1.1

I think, therefore, we are entitled to conclude that the saturation of arterial blood is more rapidly effected than the analyses quoted above suggest; direct proof is, however, lacking.

Naturally, results obtained from an organ the circulation through which is so rapid as in the case of the kidney, under the conditions of our experiment, cannot be transferred to the tissue liquids at large. It would be illogical to conclude that ten minutes' exposure is adequate to produce any high degree of saturation in organs the circulation through which is slow. The best way to settle the point would be by the analysis of samples of venous blood, but we have not, so far, been able to overcome certain technical difficulties in the way of carrying out such observations. Some experiments, conducted by Drs. Haldane and Boycott and Lieutenant Damant (Admiralty Committee, 1907), are of interest in this connexion. They found that for a pressure of +45 lb. rapid decompression is not, in the case of

goats, followed by serious symptoms if the exposure be not for longer than forty-five minutes. When the time under pressure has lasted two hours the results are much worse, but a further increase in the time does not greatly augment the number of accidents. This suggests that saturation is approximately attained in about two hours. Although the goat is nearer to man in size than are ordinary laboratory animals, it is, in other respects, not a satisfactory standard of comparison, but we are not at present able to cite better evidence.

The experience of caissons testifies that accidents occur more frequently when the shifts are of eight hours' duration than for working periods of six or four hours; it is not possible to say how far this is due to greater saturation in the longer shifts, because the question of cardiac embarrassment, due to fatigue, must be considered in an estimate of the factors which render decompression dangerous.

DECOMPRESSION.

Some further light will be thrown on the subject, indirectly, by studying the phenomena of decompression, to which I will now turn. If it be true that saturation depends upon the velocity of the blood stream, it follows that the rapidity of desaturation must also vary with the pulse-rate. Accordingly, the greater the velocity of the blood, the shorter the time required for the safe decompression of any animal. Since it is known that the velocity of the circulation and rate of respiratory exchange are greater in small than large warm-blooded animals, then the former should survive decompression periods too short to be borne safely by the latter.

As, by parity of reasoning, the small animals will be saturated more quickly than the large ones, we must expose the animals for a period sufficiently long in both cases to ensure that a high degree of saturation has been attained. We have experimented (Hill and Greenwood, 1906) with rats, mice, cats, rabbits, and guinea-pigs, our observations on the first-mentioned animal having been numerous. They were exposed to a pressure of +105 lb. for an hour, and then decompressed in about 5½ seconds. In all we used 85 rats (31 large and 54 small). The mortality in the latter group was 25.9 per cent., in the former 77.4 per cent. Of these animals we recorded the weights in 65 instances, so that we had some material for a statistical analysis of the relationship between body weight and immunity.

Table of Results of Rapid Decompression.

Weight in Grams.	Recoveries.	Deaths.	Totals.
Below 124	23	13	41
Above 124	6	18	24
Totals	24	31	65

From this the coefficient of correlation was calculated to be 0.625 ± 0.095 . Using the method of contingency, the coefficient of mean square contingency proved to be 0.544 ± 0.0978 , so that there can be little doubt that the connexion between small body weight and immunity from decompression symptoms is more than a coincidence. On considering the results a little more closely, it will be evident that their interpretation is not so simple as might appear at first. Thus, the rats not only differed in weight, but also in age, and we must inquire how much of the immunity was due to small body mass and how much to youth. Unfortunately, it is difficult to obtain large numbers of rats differing in weight but not in age. If we test rats against other animals, a new source of error is introduced, because, as I shall try to show, specific differences exist. Perhaps the course least open to objection is to compare rats with mice, since the last-mentioned error is not so great as in the employment of other animals. We find:

	Died.	Survived.	Percentage Mortality.
Rats	6	10	37.5 ± 8.16
Mice	3	27	10 ± 3.69
Difference ...	—	—	27.5 ± 8.96

It would seem, therefore, that full-grown mice are less affected than young rats, and the suggestion—it is little more—is that, in these animals, the weight factor is more important than the age factor.

Our observations upon larger animals tend to confirm these conclusions, and the following will serve as an example:

An old cat and two half-grown kittens were exposed to +100 lb. for thirty minutes, decompression being effected in ten seconds. The cat was found dead on opening the chamber, while the two kittens survived.

Our results do not, however, admit of exact formulation, and I may remark upon the differences between various kinds of animals. Thus, it by no means follows that an animal of Species B will survive a rate of decompression found to be safe for an animal of the same mass, but belonging to a different species, A. We have found, for instance, that guinea-pigs bear rapid decompression ill even when young and light animals are used. It has seemed to us that herbivorous animals differ from flesh eaters in this way, but further evidence is required.

The importance of the preceding considerations is obvious. Results obtained on animals can only be transferred to man with great caution. Although, as we have seen, laboratory experiments have rendered, and will still render, priceless services to our knowledge of caisson disease, such work must go hand in hand with clinical observations and a direct study of human beings exposed to compressed air. This affords another proof—if such be needed—of the impropriety of segregating physiologists in institutes out of touch with applied medicine.

Bearing in mind these reserves, we must inquire how far the researches described elucidate the distribution of cases of illness among compressed air workers. Snell gives the following table of his observations at the Black-wall Tunnel (1896, p. 154):

Ages.	Number of Men Examined and Passed.	Number of Men taken ill whose Ages are Recorded.	Proportion of Illnesses to every 100 Men Passed.
15-20	55	0	0
20-25	145	15	10.3
25-30	152	37	24.3
30-35	91	19	20.9
35-40	61	14	22.9
40-45	38	10	26.3
45-50	3	5	166.0
	545		

Unfortunately the first column of this table gives not the total number employed, but only those who submitted themselves to medical inspection, which was not at first compulsory. But if the men examined by Snell were a fair sample of the workers, which is not an unreasonable assumption, they can be used as a measure of the age-distribution in the whole class. If there be no special liability to illness at any particular age, then those in each age-group who suffer ought to be n/N multiplied by the total number of cases, where N is 545 and n the numbers in each division of column 1. In this way the next table was obtained.

Ages.	Actual Number Affected.	Theoretical Number.
15-20	0	10.09
20-25	15	26.61
25-30	37	27.89
30-35	19	16.70
35-40	14	11.19
40-45	10	6.97
45-50	5	0.55

The odds against so great a discrepancy as that shown being a result of random sampling are more than a million to one, while an analysis of v. Schrötter's Nussdorf

statistics leads to a similar result. We cannot, of course, say whether these differences depend more upon age itself or upon the increased body weight of the older men; it is possible that a circumstance associated with changes in the tissues as time passes may reduce the two factors to a single one.

Vernon (1907) has recently proved that the solubility of nitrogen in fat or oil is more than five times as great as in water. We have extended these observations by exposing olive oil and water to +100 lb. air pressure. We found that the difference is considerable, but that the oil becomes very slowly saturated. For example, after an exposure of two hours twenty minutes, the water was almost fully saturated, containing 7 per cent. nitrogen, while the oil held 19 per cent. only. Relatively, the proportion of adipose tissue tends to increase with age, so that the increase in weight is peculiarly unfavourable because of the enhanced solvent power for gas. Perhaps we may say that bulkiness is always an additional source of danger, especially so when the condition is due to fatty accumulations. Whether Vernon's discovery will throw light upon the specific differences to which I previously alluded is not yet known; but, combining it with our experiments, we may say that young, spare men, possessing a vigorous circulation, should always be selected for compressed air work, and that we may expect this to be more important in caisson operations than for divers. With *very short* exposures, it may even be that young men are relatively at a disadvantage, provided their seniors be organically sound.

I should add that this partial immunity of small and young animals is also borne out by the observations of Bert, as will be found on comparing the results of his experiments on sparrows, rats, and dogs.

METHODS OF DECOMPRESSION.

We note that age, weight, and species are all influential in the determination of appropriate decompression times, and we also know that the weight factor is itself complex, requiring a study of the bodily structure, with particular reference to its proportion of fat. We now come to the practical question as to the way in which decompression should be managed.

Uniform Decompression.

That this process must be slow is manifest; its details, however, require careful examination. At first sight, it would appear that since desaturation is effected by the same agency as saturation, namely, by the circulating medium, which intervenes between air and tissues, compression and decompression are, in a sense, interchangeable. We have seen that the tissues are uniformly and gradually saturated; it is natural to suppose that they should be gradually and uniformly desaturated. This train of reasoning has been adopted by most of the authors following Paul Bert, although, as I shall point out, Bert himself had considered another aspect of the problem. In consequence, slow, uniform decompression has been generally advocated. Broadly speaking, we may say that the statistics of caissons and diving show that frequency of accidents has varied inversely as the time occupied in decompression. When we come to details, it is not possible to arrive at satisfactory conclusions. Experience shows that there is often a considerable difference between the time laid down by the authorities for "locking out" and the actual practice of the workmen. Heller, Mager, and v. Schrötter, who were in medical charge at the Nussdorf Works, remark on this with some bitterness. They say (p. 471):

Unfortunately during our work we were able to convince ourselves how difficult it is, without possessing special authority, to carry out the necessary arrangements, with the result that well thought-out and energetically urged advice remains merely a pious expression of opinion, in that it is frustrated by external factors and opposition.

When, therefore, we read that decompression at a certain rate was attended by many accidents, we require to be sure that the prescribed rules were actually obeyed—an assurance not often afforded.

In those cases in which uniform decompression at the rate of twenty minutes for each atmosphere of positive pressure was actually effected, no serious accident has, I

think, been recorded. In Hersent's experiment, the only symptom described was in the case of a man who experienced a few "picotements" after decompression from +68.27 lb. He was under the pressure one hour and was decompressed in two hours three minutes; a similar condition resulted after exposure to +71.16 lb. In our own experiments we had mild symptoms on three occasions—once "bends" in the arms; once slight pain in the knee; once hyperaesthesia associated with some purpuric eruption over the chest. As a matter of fact, the interpretation of these very trivial accidents is simple. From what has gone before it will be clear that an active circulation is a prerequisite for the quick discharge of dissolved gas. In the first experiments we remained motionless during decompression, so that the return of blood from the limbs, joints, and adipose tissues was not aided by the expressive action of contracting muscles, and it was in the second experiment of our series that I suffered from "bends." Subsequently I regularly flexed and extended my arms and legs, also massaging the chest. Once I omitted to flex the knees, and on that occasion had subsequently pain and stiffness in those joints. Hill was only once affected. He forgot to massage his chest and subsequently had pain and a purpuric rash in that part alone. It may be urged that no satisfactory evidence exists that accidents occur when a uniform rate of decompression is maintained. It is, however, manifest that the experiments are certainly not numerous enough to justify any assertion that decompression at the uniform rate of twenty minutes per atmosphere is necessarily safe.

Discontinuous Decompression.

I wish next to call attention to another aspect of the problem which was considered by Bert. He (p. 959) exposed a dog to a pressure of 10 atmospheres for a time not definitely stated, perhaps about an hour. Decompression was effected in the following way: The pressure was rapidly lowered to 8 atmospheres and maintained at this level for fifteen minutes; it was then diminished to 6 atmospheres and another pause of a quarter of an hour occurred. This plan was followed throughout the decompression; after each drop of two atmospheres there was a pause of fifteen minutes, the whole process occupying one hour and ten minutes. On opening the cylinder the animal seemed well, but two or three minutes later gave vent to cries of pain. It subsequently moved the left forefoot as if the limb were tender, but on the following day appeared quite well. Two days later the same animal was exposed to 10 atmospheres for fifteen minutes, and decompressed uniformly at the rate of eight minutes per atmosphere. It exhibited neither immediate nor remote symptoms.

Another dog was exposed to 10 atmospheres for seven minutes, decompressed to 6 atmospheres in two minutes, and after a pause of twenty-eight minutes, decompressed in two minutes to 3 atmospheres. There was then an interval of twenty-six minutes, subsequent decompression being carried out in less than half an hour. No symptoms followed.

In summarizing his results, Bert (1878, p. 975) writes:

I have remarked no great differences between the cases in which decompression was effected continuously at the rate of eight or ten minutes per atmosphere and those in which it was carried out by means of sudden jumps, with intervals of repose. The facts are not, however, sufficiently numerous to permit of deciding in favour of one or other of these methods.

This other method—of discontinuous or staged decompression—has been reinvestigated by Drs. Haldane and Boycott, with the assistance of Lieutenant Damant, and they have repeated and amplified Bert's experiments on this point. The results of this research have been published in the report of an Admiralty Committee on Deep-water Diving (1907), and are accompanied by an interesting theoretical discussion.

In the first place some *a priori* considerations suggest that time might be saved by employing the method. From the point of view of *relative* increase in volume, the same effect is produced by falling from 8 to 4 atmospheres, from 4 to 2, and from 2 to 1. In each case there is a doubling of the initial volume. Further, since the calibre of the vessels is unaltered, even taking into account the absolute volume of the gas, it is clear that bubbles formed at 2 or 4 atmospheres are respectively one-half and one-

quarter of the size of bubbles representing the same weight of gas liberated at 1 atmosphere, absolute pressure. In other words, both considerations tell in favour of the view that a fall from 4 to 2 atmospheres of total pressure can be safely effected in the same time as a fall from 2 to 1 atmosphere. The state tending to cause a liberation of bubbles is the *relative* difference between internal and external gas pressure, which we see to be the same in both cases; the danger to life of the bubbles, when formed, will depend upon their absolute size, which is actually less in the former than in the latter case.

Following out these ideas, it has been proposed to substitute discontinuous decompression for the method usually advocated. The following example, quoted from the Addenda to the Diving Manual, which embodies the recommendations of the Admiralty Committee with respect to this point, illustrates the practice advised:

If a diver has been working at a depth of 144 ft. for between 45 minutes and 1½ hours, he is directed to ascend rapidly to 50 ft. and then pause 10 minutes; he next comes to 40 ft. and again waits 10 minutes; at 30 ft. he stops 20 minutes, at 20 ft. 30 minutes, at 10 ft. 35 minutes, and then leaves the water, the whole ascent occupying 108 minutes. By the method of uniform decompression the ascent would have been performed continuously in about 126 minutes. Had the exposure been longer, over 1½ hours, the same plan would have been followed, but the total time allotted greater—178 minutes.

In attempting to form a judgement as to the respective merits of the two systems, it is necessary to distinguish carefully between the cases of long and short exposure. When the diver has been exposed to the higher pressure for an absolutely short space of time, the advantage of staged decompression is, I think, considerable. We have seen how slowly the tissues are saturated with gas, hence it is of benefit to descend rapidly at first, because the tissues are not nearly in equilibrium with the higher pressure, so that no risk of embolism is faced. On the contrary, if we decompress at a uniform rate—from, say, 6 to 3 atmospheres—the subject, who was not saturated to the full pressure of +5 atmospheres, will, on arriving at +2, be almost saturated at that lower pressure. If he had descended rapidly to +2, he would have begun a further descent with a smaller quantity of dissolved gas requiring liberation.

For short exposures, therefore, the method is, so far as I can see, a distinct improvement. The question is somewhat more difficult if we suppose the subject highly saturated with gas, as would be the case after an absolutely long exposure to the maximum pressure attained.

The theoretical arguments are not, I think, so convincing as they appear at first sight. Consider one point: It is suggested that, on falling from 6 to 3 atmospheres of absolute pressure, if the gas freed does form bubbles they will be too small to cause danger. But suppose the excess is not so liberated? The tissues are charged with, approximately, 6 per cent. of nitrogen, and the normal carrying power of the blood has fallen to 3 per cent., so a considerable excess might be retained in the tissues to give trouble later. An obvious answer to this is that the blood may become supersaturated and dissolve more than its theoretical 3 per cent. Without remarking that the same argument applies to the tissues also, this would only help us if we assume that the blood passing through the lungs is at once brought into equilibrium with the alveolar partial pressure of nitrogen. Now, I do not think this suggestion rests upon an adequate basis of fact. To assert that it is a necessary consequence of the known mechanism of respiratory change—whatever that may be—is merely to beg the question.

Hill and I have more than once observed air bubbles in the aorta, and once in the pulmonary veins. Either those bubbles had passed through the pulmonary circulation, or the blood as a supersaturated solution had so passed, unless, indeed, the circulation had been instantly arrested at the moment of decompression, which is not, I think, usual. It would seem, therefore, to be rash to assert that instant equilibration occurs in the lungs.

The question cannot, however, be otherwise determined than by means of direct experiment. The Admiralty Committee has published the following observations (p. 72):

Stage Decompression, 75 lb. Positive Pressure.

Length of Exposure.	Number of Exposures.	No. Symptoms.	Bends.	Severe Symptoms.
15 min. ...	34	29	5	0
30	6	4	2	0
30	14	14	0	0
120	13	9	4	0
120	19	15	3	1
180	10	8	2	0

Uniform Decompression.

Length of Exposure.	Number of Exposures.	No. Symptoms.	Bends.	Severe Symptoms.
15 min. ...	36	19	13	3 together with a death.
30	6	1	4	1
30	14	7	7	0
120	13	4	7	2
120	19	10	3	5 and a death.
180	10	5	5	0

The most valuable observations are those for exposures of not less than two hours, since the Committee have advanced some reasons for thinking that in such a time a high degree of saturation is attained. If we club together the cases of "bends" with the unaffected animals, which is perhaps best, since the criterion of a bend is rather uncertain, we find that for 42 cases of staged decompression 1, or 2.38 per cent., showed dangerous symptoms; with uniform decompression 8, or 19 per cent. of accidents occurred in the same number of observations. The actual numbers are very small; but, on the hypothesis that the case comes under those discussed by Pearson in a recent memoir (1907), the probable error of the difference can hardly be more than ± 1 per cent. We are entitled to say, therefore, that the results show a definite advantage in favour of the discontinuous method, but it is necessary to be a little cautious. I have drawn attention to the specific differences which are so remarkable in work of this kind. Hill and myself have been struck by the distension of the stomach and intestines associated with rapid decompression in herbivorous animals. It has seemed to us that this distension may be a factor in decompression effects. Thus it is possible that some mechanical obstruction may be offered to the return of blood from the abdominal viscera, which would markedly interfere with the process of desaturation.

Naturally, this does not account for the difference observed in goats treated by the two methods; but it is evidence that results obtained on these animals are not necessarily applicable to man and may be very variable under different conditions.

This is, to some extent, borne out by our own experiments on rabbits, although they are not sufficiently numerous to be of definite significance.

Rabbits exposed to +100 lb. for One Hour.

	Uniform Method.		Staged Method.	
	Normal.	Paralysed or Dead.	Normal.	Paralysed or Dead.
A. Total time of decompression 2 min.	6	13	0	0
B. Total time 4 min. ...	15	1	5	3
C. Total time, 5 min. ...	7	0	1	0
D. Total time, 6 min. ...	1	0	1	0

The details of one experiment are interesting:

Four rabbits were exposed to +105 lb. for one hour and decompressed uniformly in four minutes. In the afternoon of the same day these animals were once more submitted to +105 lb. for one hour thirty-five minutes, and again decompressed uniformly in four minutes. No symptoms whatever were produced. The animals were kept under observation for a week, during which period they remained well. Seven days after the first experiment all four were again exposed to +105 lb. for one hour fifteen minutes; decompression was

then effected in stages, calculated from the corresponding times allotted in the Admiralty recommendations, the total time being, as before, four minutes. As a result, one rabbit was found paraplegic, with incontinence of urine on removal, and soon went into convulsions and died. A second animal became paralysed, after an attack of convulsions, while the two others remained normal.

It would, of course, be idle to draw far-reaching conclusions from these few experiments; they simply indicate that the advantages of staged decompression are not so overwhelming as might be supposed. I may remark that the advantage of shortening the decompression time which has been urged in favour of the staged method is non-existent for moderately long exposures. For high pressures the time demanded is more than twenty minutes an atmosphere. Thus, for +91.5 lb. the time recommended is 238 minutes, for +86 lb. 228 minutes, provided the exposure in each case be of more than an hour.

I have criticized these recent researches at this length in the hope of making it seem probable that a decisive opinion in favour of one or other method, as applied to man, can hardly be pronounced in the absence of more evidence. I desire, however, to express my admiration of the care and precision with which this Committee's experiments were conducted. One thing at least is clear: their results emphasize the importance of not hastening the later stages of decompression. I do not think it can be doubted, whatever fresh discoveries may be made, that the uniform method will require modification in this sense. It is, however, only just to those who are of a different opinion to say that we possess no satisfactory evidence of serious results following the adoption of uniform decompression at the rate of twenty minutes per atmosphere.

I am conscious that the attempt to describe so much within the limits of a couple of hours has resulted in some loss of focus. To me the history of the whole subject seems to throw light upon the mutual obligations of the clinician and the experimenter, to show that each is powerless without his fellow.

If you are satisfied that the subject is of interest and that modern researches have distinctly advanced our knowledge and contributed to the preservation of human life my object will have been fully attained.

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MEMORANDA:

MEDICAL, SURGICAL, OBSTETRICAL.

A CASE OF BELLADONNA POISONING.

THE following case of poisoning due to one application of linimentum belladonnae (P.B.) to the lumbar region of a healthy male seems worth recording. The patient, a strongly built male of about 30 years, consulted me for pains in the neck and lower part of the back. His temperature was 98.6°, pulse 60, urine healthy; in fact, nothing abnormal about him except these pains. I looked upon the case as one of so-called muscular rheumatism, and ordered lin. bell. to be rubbed into the back night and morning. This was on a Sunday. The next morning I was sent for urgently and informed that at 9 o'clock on the previous night he was well rubbed with the liniment, and at 10 was quite off his head, and remained so during the night, getting no sleep whatever. The patient was lying in bed with a startled, hunted expression upon his face, eyes bright and shining, pupils widely dilated, mumbling and muttering speech, dry tongue and skin, trembling lips and hands, delirious, pulse 120 compressible and running. He vomited several times, contents undigested food. After bowels and kidneys were made to act well and opium gave him some sleep he began to improve. Hypodermic injections of pilocarpin nitras had an excellent effect in quieting the pulse and making him more comfortable. That night the head symptoms were much better and in two days he was practically well, the muscular pains having also departed. Monday was a blank day to him. The amount of liniment taken from the bottle was 3ij. The skin over the back was sound and normal in appearance. I have used the liniment extensively for many years and never before have I seen any poisonous symptoms develop.

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TOXIC EFFECT OF PETROL FUMES.

THE note by Dr. Box in the BRITISH MEDICAL JOURNAL, April 4th, leads me to record the following case. The note of what took place is by the patient's mistress, who is a trained nurse.

The chauffeur took off half the cover of the inspection pit, leaving the other half in position. He got into the pit and proceeded to empty the petrol tank of the car. About ten minutes later a lad working near by heard a snorting and puffing sound, and found the chauffeur lying unconscious on the bottom of the pit, his clothes being saturated in petrol. When he was lifted out his face and hands were blue, he was foaming at the mouth, and no pulse could be felt. Eyes open and turned upwards, so that the pupils were not visible. After a short time he began to struggle, beating the floor with his feet, throwing himself from side to side and fighting with his hands. The pulse gradually returned and the blueness passed off. He retched several times but did not vomit. After about an hour he became violent, trying to bite himself and the attendants. He gradually came round, but could not answer questions or speak intelligently, and was extremely irritable and resented all assistance.

When I saw him he had come round and was comparatively well, except for headache, and burning pain wherever the petrol in which he had been lying had soaked through his clothes. Blistering occurred in various places and there was much reddening of the skin; except for these conditions he was quite well next day.

Yateley, Hants.

ALEX. S. PETRIE.