Physiological Effects of Insufficient Oxygen Supply.*

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PROMINENT among the pathological conditions which claimed attention during the war was that of insufficient oxygen supply to the tissues, or anoxæmia. The statement has been made that "anoxæmia not only stops the machine, but wrecks the machinery." This phrase is so apposite that I shall commence by an inquiry as to the limits within which it is true.

Anything like complete anoxæmia stops the machine with almost incredible rapidity. Though the breath can be held for a time, it must be borne in mind that the lungs normally contain about half a litre of oxygen, and that this will suffice for the body at rest for upwards of two minutes. But get rid of the residual oxygen from the lungs even to the extent possible by the breathing of nitrogen, and you will find that only with difficulty will you endure half a minute. It seems doubtful whether complete absence of oxygen would not bring the brain to an instantaneous standstill. So convincing are the experimental facts to anyone who has tested them for himself that I will not further labour the power of anoxæmia to stop the machine. I will, however, say a word about the assumption which I have made that the machine in this connection is the brain.

It cannot be stated too clearly that anoxæmia in the last resort must affect every organ of the body directly. Stoppage of the oxygen supply is known to bring the perfused heart to a standstill, to cause a cessation of the flow of urine, to produce muscular fatigue, and at last immobility, but from our present point of view these effects seem to me to be out of the picture because the brain is so much the most sensitive to oxygen want.

To what extent does acute anoxæmia in a healthy subject wreck the machinery as well as stop the machine? By acute anoxæmia I mean complete or almost complete deprivation of oxygen which, in the matter of time, is too short to prove fatal. No doubt many data might be quoted of men who have recovered from drowning, etc. Such data are complicated by the fact that anoxæmia has only been a factor in their condition. These data, therefore, have a value in so far as they show that a very great degree of anoxæmia, if acute and of short duration, may be experienced with but little wreckage to the machine. They have but little value in showing that such wreckage is due to the anoxæmia, because the anoxæmia has not been the sole disturbance.

Cases in which the anoxæmia has been uncomplicated are to be found among those who have been exposed to low atmospheric pressures; for instance, balloonists and aviators. Of these quite a considerable number have suffered from oxygen want to the extent of being unconscious for short intervals of time.

No scientific observer has pushed a general condition of anoxæmia either on himself or on his fellows to the extent of complete unconsciousness. The most severe experiments of this nature are those carried out by Dr. Haldane and his colleagues. One experiment in particular demands attention. Dr. Haldane and Dr. Kellas[1] together spent an hour in a chamber in which the air was reduced to between 320 mm. and 295 mm. It is difficult to say how far they were conscious. Clearly each believed the other to be complete master of his own faculties, but it is evident that Dr. Haldane was not so. I gather that he has no recollection of what took place; that whenever he was consulted about the pressure he gave a stereotyped answer which was the same for all questions; and that even with a little more oxygen present he was sufficiently himself to wish to investigate the colour of his lips in the glass, but insufficiently himself to be conscious that he was looking into the back, and not the front, of the mirror. Dr. Kellas, who could make observations, never discovered Dr. Haldane's mental condition, though boxed up with him for an hour, and went on consulting him automatically. A somewhat similar experiment was performed on the other two observers, with results differing only in degree.

Yet the after-effects are summed up in the following sentence: "All four observers suffered somewhat from headache for several hours after these experiments, but there was no nausea or loss of appetite."

Of real importance in this connection are the results of carbon monoxide poisoning. Of these a large number might be cited. Those interested will find some very instructive cases described in a volume entitled "The Investigation of Mine Air," by the late Sir C. Le Neve Foster and Dr. Haldane.[2] The cases in question were those of a number of officials who went to investigate the mine disaster on Snaefell, in the Isle of Man, in May, 1897. Of the five cases cited all suffered some after-effects, by which I mean that by the time the blood was restored sufficiently to its normal condition for the tissues to get the amount of oxygen which they required, the effects of the asphyxia had not passed off, and to this extent the machine suffered.

To sum up, then, what may be said of the permanent damage caused by acute anoxæmia, it seems to me to be as follows: No degree of anoxæmia which produces a less effect than that of complete unconsciousness leaves anything more than the most transient effects; if the anoxæmia be pushed to the point at which the subject is within a measurable distance of death, the results may take days or weeks to get over, but only in the case of elderly or unsound persons is the machine wrecked beyond repair.[3]

Chronic Anoxaemia.

And now to pass to the consideration of what I may call chronic anoxæmia—that is to say, oxygen want which perhaps is not very great in amount, but is of long duration. The most obvious instances of men subject to chronic anoxæmia are the dwellers at high altitudes. In these the anoxæmia does not wreck the machine. On what I may call the average healthy man anoxæmia begins to tell at about 18,000 ft. At lower altitudes no doubt he will have some passing trouble, but it seems to me from my own experience that this altitude is a very critical one. Yet there are mining camps at such heights in South America at which the work of life is carried on. The machine is kept going by a process of compensation, in part carried out by a modification in the chemical properties of the blood, in which both the carbonic acid and the alkali diminish. The result, according to my interpretation of my own observations on the Peak of

* From the opening address of the President of Section I (Physiology) delivered at the Cardiff meeting of the British Association on August 24.

[1] Haldane, Kellas, and Kennaway *Journal of Physiology*, vol. liii.

[2] Foster and Haldane. "The Investigation of Mine Air." (Griffin and Co., 1905.)

[3] Since the address was written Dr. Haldane has told me of a number of victims of the same accident who were brought out alive by the search party, but in whose case the machine was wrecked beyond repair. They soon died.

Teneriffe, which appeared to be confirmed by the experiments in a partially vacuous chamber in Copenhagen,[4] was this: The hydrogen-ion concentration of the blood increased slightly, the respiratory centre worked more actively, and the lung became better ventilated with oxygen, with the natural result that the blood became more oxygenated than it would otherwise have been.

The difference which this degree of acclimatisation made was very great. On Monte Rosa in one case 15 mm. of oxygen pressure were gained in the lungs. To put the matter another way, the amount of oxygen in our lungs at the summit was what it would otherwise have been 5000 ft. or 6000 ft. lower down.

The body, then, had fought the anoxæmia and reduced it very much in degree, but at the same time the anoxæmia had in a subtle way done much to stop the powers of the body, for this very acclimatisation is effected at the expense of the ultimate reserve which the body has at its disposal for the purpose of carrying out muscular or other work. The oxygen in the lungs was obtained essentially by breathing at rest as you would normally do when taking some exercise. Clearly, then, if you are partly out of breath before you commence exercise you cannot undertake so much as you otherwise would do. As a friend of mine—who has camped at 23,000 ft., a higher altitude, I believe, than any other man—put it to me: "So great was the effort that we thought twice before we turned over in bed."

One of the interesting problems with regard to chronic anoxæmia is its effect upon the mind. Sir Clement Le Neve Foster's account of himself during CO poisoning shows loss of memory, some degree of intelligence, and a tendency to repeat what is said. The whole train of his symptoms strongly suggests some form of intoxication, and is not dissimilar to that produced by alcoholic excess. Here it may be noted that, so far as isolated nerves are concerned, there is very good evidence that alcohol and want of oxygen produce exactly the same effects, *i.e.* they cause a decrement in the conducting power of the nerve. And herein lies a part of its interest, for pharmacologists of one school, at all events, tell me that the corresponding effects of alcohol are really due to an inhibition of the higher centres of the mind; you can, therefore, conceive of the mental mechanism of self-control being knocked out either because it has not oxygen enough with which to "carry on," or because it is drugged by some poison as a secondary result of the anoxæmia.

To pass now to the results of more chronic anoxæmia, if I were to try to summarise them in a sentence I should say that, just as acute anoxæmia simulates drunkenness, chronic anoxæmia simulates fatigue.

A page in my note-book written at the Alta Vista Hut, at an altitude of 12,000 ft., commences with a scrawl which is crossed out, then "6 Sept.," the word "Sept." is crossed out and "March" is inserted, "March" shares the same fate as "Sept.," and "April," the correct month, is substituted, and so on, more crossings out and corrections. All this you might say with justice is the action of a tired man. The other pages written at lower altitudes do not, however, bear out the idea that I was out of health at the time, and there was no reason for tiredness on that particular day. Another symptom frequently associated with mental fatigue is irritability. Anyone who has experience of high altitudes knows to his cost that life does not run smoothly at 10,000 ft. If the trouble is not with one's own temper, it is with those of one's colleagues; and so it was in many cases of gas poisoning and in the case of aviators. In these subjects the apparent fatigue sometimes passed into a definitely neurasthenic condition. At this point an issue appeared to arise between the partisans of two theories. One camp said that the symptoms were definitely those of anoxæmia, the other that they were due to nerve-strain. As I have indicated later on, it is not clear that these two views are mutually exclusive. It takes two substances to make an oxidation, the oxygen and the oxidised material. If the oxidation does not take place, the cause may lie in the absence of either or of both, in each case with a similar effect. The subject really is not ripe for controversy, but it is amply ripe for research—research in which both the degree of anoxæmia and the symptoms of fatigue are clearly defined.

So much, then, for the injury to the machine wrought by chronic anoxæmia.

Types of Anoxaemia.

Anoxæmia is by derivation want of oxygen in the blood. Suppose you allow your mind to pass to some much more homely substance than oxygen—such, for instance, as milk—and consider the causes which may conspire to deprive your family of milk, three obvious sources of milk deficiency will occur to you at once: (1) There is not enough milk at the dairy; (2) the milk is watered or otherwise adulterated so that the fluid on sale is not really all milk; and (3) the milkman from that particular dairy does not come down your road.

These three sources of milk deficiency are typical of the types of oxygen deficiency.

The first is insufficient oxygen dispensed to the blood by the lungs. An example of this type of anoxæmia is mountain-sickness. The characteristic of it is insufficient *pressure* of oxygen in the blood. In mountain-sickness the insufficiency of pressure in the blood is due to insufficient pressure in the air. But this type of anoxæmia may be due to other causes. In such cases, either caused by obstruction, by shallow respiration, or by the presence of fluid in the alveoli, the blood leaving the affected areas will contain considerable quantities of reduced hæmoglobin. This will mix with blood from unaffected areas which is about 95 per cent. saturated. The oxygen will then be shared round equally among the corpuscles of the mixed blood, and if the resultant is only 85–90 per cent. saturated the pressure of oxygen will only be about half the normal, and, as I said, deficiency of oxygen pressure is the characteristic of this type of anoxæmia.

The second type involves no want of oxygen pressure in the arterial blood; it is comparable to the watered milk. The deficiency is really in the quality of the blood, and not in the quantity of oxygen to which the blood has access. The most obvious example is anæmia, in which the blood contains too low a percentage of hæmoglobin, and because there is too little hæmoglobin to carry the oxygen, too little oxygen is carried. Anæmia is, however, only one example of this type of anoxæmia. The hæmoglobin may be useless for the purpose of oxygen transport; it may be turned in part into methæmoglobin, as in several diseases, *e.g.* among workers in the manufacture of some chemicals, and in some forms of dysentery contracted in tropical climates, or it may be monopolised by carbon monoxide, as in mine-air.

Thirdly, the blood may have access to sufficient oxygen and may contain sufficient functional hæmoglobin, but owing to transport trouble it may not be

[4] Hasselbach and Lindhard, quoted by Bainbridge.

circulated in sufficient quantities to the tissues. The quantity of oxygen which reaches the tissue in unit time is too small. Literally, according to the strict derivation of the word "anoxæmia," the third type should perhaps be excluded from the category of conditions covered by that word, but as the result is oxygen starvation in the tissues it will be convenient to include it.

The obvious types of anoxæmia may therefore be classified in some such scheme as the following:

ANOXÆMIA.

1. *Anoxic Type.*	2. *Anaemic Type.*	3. *Stagnant Type.*
The pressure of oxygen in the blood is too low.	The quantity of functional hæmoglobin is too small.	The blood is normal, but is supplied to the tissues in insufficient quantities.
The hæmoglobin is not saturated to the normal extent.	The oxygen pressure is normal.	Examples:
The blood is dark.	The blood is normal in colour.	1. Secondary result of histamine shock.
Examples:	Examples	2. Hæmorrhage.
1. Rare atmospheres.	1. Too little hæmoglobin.	3. Back pressure.
2. Areas of lung partially unventilated.	2. CO hæmoglobin.	
3. Fluid or fibrin on surface of cells.	3. Methæmoglobin.	

Anoxic anoxæmia is essentially a general as opposed to a local condition. Not only is the pressure of oxygen in the blood too low, but the lowness of the pressure, and not the deficiency in the quantity, is the cause of the symptoms observed.

The workers on Pike's Peak, for instance, emphasised the fact that the increase of red-blood corpuscles during their residence at 14,000 ft. was due to deficient oxygen pressure. No doubt they were right, but the point was rather taken from their argument by their assertion in another part of the paper that the oxygen pressure in their arterial blood was anything up to about 100 mm. of mercury. Let me, therefore, take my own case, in which the alveolar pressures are known to be an index of the oxygen pressures in the arterial blood. I will compare my condition on two occasions, the point being that on these two occasions the quantities of oxygen united with the hæmoglobin were as nearly as may be the same, whilst the pressures were widely different.

As I sit here the hæmoglobin value of my blood is 96–97, which corresponds to an oxygen capacity of 0·178 c.c. of O_2 per c.c. of blood. In the oxygen chamber on the last day of my experiment, to which I refer later,[5] the oxygen capacity of my blood was 0·201 c.c. Assuming the blood to be 95 per cent. saturated now and 84 per cent. saturated then, the actual quantity of oxygen in the blood on the two occasions would be:

Oxygen capacity.	Percentage saturation.	Oxygen content.
0·178	95	0·169
0·201	84	0·169

Here I am in my usual health. In the chamber I vomited; my pulse was 86, it is now 56; my head ached in a most distressing fashion; it was with the utmost difficulty that I could carry out routine gas analyses, and when doing so the only objects which I saw distinctly were those on which my attention was concentrated.

In the anoxic type of anoxæmia there may then be quite a sufficient quantity of oxygen in the blood, but a sufficient quantity does not avail in the face of an insufficient pressure. Indeed, as I shall show presently, the anoxic type of anoxæmia is the most serious. We are, therefore, confronted with something of a paradox in that the most severe type of anoxæmia is one in which there is not necessarily an insufficient quantity of oxygen in the blood at all.

[5] Barcroft, Cooke, Hartridge, Parsons, and Parsons. *Journal of Physiology*, vol. liii., p. 451, 1920.

It is interesting and not uninstructive to try to calculate the degree to which the tissues are prejudiced by being subjected to various types of anoxæmia. Let us suppose that we have a piece of tissue—muscle, for instance—which normally is under the following conditions:

(*a*) One cubic centimetre of blood per minute runs through it.

(*b*) The total oxygen capacity of this blood is 0·188 c.c. of oxygen per c.c. of blood.

(*c*) The percentage saturation is 97.

(*d*) The oxygen pressure is 100 mm.

(*e*) The oxygen used is 0·059 c.c.

(*f*) The oxygen pressure in the tissues is half of that in the veins, in this case 19 mm.

My colleague, Mr. F. J. Roughton, has calculated the amount of oxygen which would penetrate this tissue from the blood in each type of anoxæmia, if the oxygen which reached it in the blood was reduced to 66 per cent. of the quantity stated above.

Measurement of Anoxæmia.

In the study of all physical processes there comes a point, and that very early, when it becomes necessary to compare them one with another to establish some sort of numerical standard and have some sort of quantitative measurements. The study of anoxæmia has reached that point. By what scale are we to measure oxygen want?

Let us take the anoxic type first. There are two scales which might be applied to it, both concerning the arterial blood; the one is the oxygen pressure in it, the other is the consequent percentage of the hæmoglobin which is oxyhæmoglobin. The important thing is that there should be as little reduced hæmoglobin as possible. The more reduced hæmoglobin there is present, the less saturated is the blood; or, as the American authors say, the more *un*saturated is the blood. They emphasise the fact that it is the quantity of *reduced* hæmoglobin that is the index of the anoxic condition. They speak not of the percentage saturation, but of the percentage of unsaturation. A blood which would ordinarily be called 85 per cent. saturated they speak of as 15 per cent. unsaturated.

Anoxic anoxæmia, in many cases of lung affection, should be measured by the direct method of arterial puncture introduced by Stadie,[6] for the simple reason that the relation between the alveolar air and the arterial blood is quite unknown. Such, for instance, are cases of many lung lesions of pneumonia in which the lung may be functioning only in parts, of pneumothorax, of pleural effusions, of emphysema, of multiple pulmonary embolism, in phases of which the arterial blood has been found experimentally to be unsaturated. In addition to these definite lung lesions there is another type of case on which great stress has been laid by Haldane, Meakins, and Priestley, namely, cases of shallow respiration.[7] A thorough investigation of the arterial blood is such cases is urgently necessary. Indeed, in all cases in which it is practicable, the method of arterial puncture is desirable. But in the cases of many normal persons—as, for instance, those of airmen at different altitudes—alveolar-air determinations would give a useful index.

The anæmic type of anoxæmia is gauged by the quantity of *oxy*hæmoglobin in the blood. In the case of simple anæmias this is measured by the scale in which the normal man counts as 100, and the hæmoglobin in the anæmic individual is expressed as a percentage of this. This method has been stan-

[6] Stadie. *Journal of Experimental Medicine*, vol. xxx., p. 215, 1919.

[7] Haldane, Meakins, and Priestley. *Journal of Physiology*, vol. liii. p. 420, 1918–19.

dardised carefully by Haldane, and we now know that the man who shows 100 on the scale has an oxygen capacity of 0·185 c.c. of oxygen for every c.c. of blood. We can, therefore, in cases of carboxy-hæmoglobin or methæmoglobin poisoning, express the absolute amount of oxyhæmoglobin pressure either by stating the oxygen capacity and so getting an absolute measurement, or in relative units by dividing one hundred times the oxygen capacity by 0·185, and thus getting a figure on the ordinary hæmoglobin metre scale.

The Mechanism of Anoxaemia.

Perhaps the most difficult phase of the discussion is that of how anoxæmia produces its baneful results. Before you discuss whether a certain effect is due to cause A or cause B, be clear in your own mind that A and B are mutually exclusive.

Let me take an example and suppose

(1) That the energy of muscular contraction in the long run depends in some way on the oxidation of sugar;

(2) That in the absence of an adequate supply of oxygen the reaction $C_6H_{12}O_6+6O_2=6CO_2+6H_2O$ cannot take place in its entirety;

(3) That in such circumstances some lactic acid is formed as well as carbonic acid;

(4) That the hydrogen-ion concentration of the blood rises and the total ventilation increases. On what lines are you to discuss whether the increased ventilation is due to "acidosis," by which is meant in this connection the increased hydrogen-ion concentration of the blood, or to "anoxæmia"? Clearly not on the lines that it must be due to one or other, for in the above instance anoxæmia and acidosis are, to some extent, dependent variables.

I have chosen the above case because measurements have been made throughout which make the various assumptions fairly certain, and tell us pretty clearly in what sort of chain to string up the events, what is cause and what is effect. Clearly it would be ridiculous to start a discussion as to whether the breathlessness was due to "acidosis" or "anoxæmia." Each has its place in the chain of events, but I have heard discussions of whether other phenomena of a more obscure nature were due to oxygen want or to acidosis. Such discussions tend to no useful end.

Nor is this the only problem with regard to oxygen want concerning which my warning is needed. Oxygen want may act immediately in at least two ways:

(1) In virtue of absence of oxygen some oxidation which otherwise might take place does not do so, and therefore something which might otherwise happen may not happen. For instance, it may be conceived that the respiratory centre can go through the rhythmic changes of its activity only as the result of the oxidation of its own substance.

(2) A deficient supply of oxygen may produce, not the negation of a chemical action, but an altered chemical action which in its turn produces toxic products that have a secondary effect on such an organism as the respiratory centre.

Now these effects are not mutually exclusive. In the same category are many arguments about whether accumulations of carbonic acid act specifically as such or merely produce an effect in virtue of their effect on the hydrogen-ion concentration. Here again the two points of view are not, strictly speaking, alternatives, and, in some cases at all events, both actions seem to go on at the same time.

It will be evident that in any balanced action in which CO_2 is produced its accumulation will tend to retard the reaction; but, on the other hand, the same accumulation may very likely raise the hydrogen-ion concentration, and in that way produce an effect.

It is rather fashionable at present to say that "the whole question of acidosis and anoxæmia is in a hopeless muddle." To this I answer that if it is in a muddle, I believe the reason to be largely because schools of thought have rallied round words and have taken sides under the impression that they have no common ground. The "muddle," in so far as it exists, is not, I think, by any means hopeless; but I grant freely enough that we are rather at the commencement than at the end of the subject; that much thought and much research must be given, first, in getting accurate data, and, secondly, on relating cause and effect, before the whole subject will seem simple. No effort should be spared to replace indirect by direct measurements. My own inference with regard to changes of the reaction of the blood, based on interpretations of the dissociation curve, should be checked by actual hydrogen-ion measurements, as has been done by Hasselbach and is being done by Donegan and Parsons.[8] Meakins also is, I think, doing great work by actually testing the assumptions made by Haldane, Priestley, and himself as regards the oxygen in arterial blood.

The Compensations for Anoxaemia.

For the anoxic type of anoxæmia two forms of compensation at once suggest themselves. The one is increased hæmoglobin in the blood; the other is increased blood-flow through the tissues. Let us, along the lines of the calculations already made, endeavour to ascertain how far these two types of compensation will really help. To go back to the extreme anoxic case already cited, in which the hæmoglobin was 66 per cent. saturated, let us, first, see what can be accomplished by an increase of the hæmoglobin value of the blood. Such an increase takes place, of course, at high altitudes. Let us suppose that the increase is on the same grand scale as the anoxæmia, and that it is sufficient to restore the actual quantity of oxygen in 1 c.c. of blood to the normal. This, of course, means a rise in the hæmoglobin value of the blood from 100 to 150 on the Gowers scale. Yet even so great an increase in the hæmoglobin will increase the oxygen taken up in the capillary from each c.c. of blood only from 0·031 to 0·036 c.c., and will, therefore, leave it far short of the 0·06 c.c. which every cubic centimetre of normal blood was giving to the tissue. So much, then, for increased hæmoglobin. It gives a little, but only a little, respite. Let us turn, therefore, to increased blood-flow.

In the stagnant type of anoxæmia the principal change which is seen to take place is an increase in the quantity of hæmoglobin per cubic millimetre of blood.

This increase is secondary to a loss of water in the tissues, the result in some cases, as appears from the work of Dale, Richards, and Laidlaw,[9] of a formation of histamine in their cells. Whether this increase of hæmoglobin is to be regarded as merely an accidental occurrence or as a compensation is difficult to decide at present. Roughton's calculations rather surprised us by indicating that increased hæmoglobin acted less efficiently as a compensatory mechanism than we had expected. This conclusion may have been due to the inaccuracy of our assumptions. I must therefore remind you that much experimental evidence is required before the assump-

[8] Donegan and Parsons. *Journal of Physiology*, vol. lii., p. 315, 1919.
[9] Dale and Richards. *Journal of Physiology*, vol. lii., p. 110, 1919. Dale and Laidlaw. *Ibid.*, p. 355.

tions which are made above are anything but assumptions. But, so far as the evidence available at the present time can teach any lesson, that lesson is this: The only way of dealing satisfactorily with the anoxic type of anoxæmia is to abolish it by in some way supplying the blood with oxygen at a pressure sufficient to saturate it to the normal level.

It has been maintained strenuously by the Oxford school of physiologists that Nature actually did this; that when the partial pressure in the air-cells of the lung was low, the cellular covering of that organ could clutch at the oxygen and force it into the blood at an unnatural pressure, creating a sort of forced draught. This theory, as a theory, has much to recommend it. I am sorry to say, however, that I cannot agree with it on the present evidence. I will only make a passing allusion to the experiment which I performed in order to test the theory, living for six days in a glass respiration-chamber in which the partial pressure of oxygen was gradually reduced until it was at its lowest—about 45 mm. Such a pressure, if the lung was incapable of creating what I have termed a forced draught, would mean an oxygen pressure of 38–40 mm. of mercury in the blood, a change sufficient to make the arterial blood quite dark in colour, whereas, did any considerable forced draught exist, the blood in the arteries would be quite bright in colour. Could we but see the blood in the arteries, its appearance alone would almost give the answer as to whether or no oxygen was forced, or, in technical language, secreted, through the lung-wall. And, of course, we could see the blood in the arteries by the simple process of cutting one of them open and shedding a little into a closed glass tube. To the surgeon this is not a difficult matter, and it was, of course, done. The event showed that the blood was dark, and the most careful analyses failed to discover any evidence that the body can force oxygen into the blood in order to compensate for a deficiency of that gas in the air.

Yet the body is not quite powerless. It can, by breathing more deeply, by increasing the ventilation of the lungs, bring the pressure of oxygen in the air-cells closer to that in the atmosphere breathed than would otherwise be the case. I said just now that the oxygen in my lungs dropped to a minimal pressure of 48 mm.; but it did not remain at that level. When I bestirred myself a little it rose, as the result of increased ventilation of the lung, to 56 mm., and at one time, when I was breathing through valves, it reached 68 mm. Nature will do something, but what Nature does not do should be done by artifice. Exploration of the condition of the arterial blood is only in its infancy, yet many cases have been recorded in which in illness the arterial blood has lacked oxygen as much as, or more than, my own did in the respiration-chamber when I was lying on the last day, with occasional vomiting, racked with headache, and at times able to see clearly only as an effort of concentration. A sick man, if his blood is as anoxic as mine was, cannot be expected to fare better as the result, and so he may be expected to have all my troubles in addition to the graver ones which are, perhaps, attributable to some toxic cause. Can he be spared the anoxæmia? The result of our calculations so far points to the fact that the efficient way of combating the anoxic condition is to give oxygen. During the war it was given with success in the field in cases of gas poisoning, and also special wards were formed on a small scale in this country in which the level of oxygen in the atmosphere was kept up to about 40 per cent., with great benefit to a large percentage of the cases. The practice then inaugurated is being tested at Guy's Hospital by Dr. Hunt, who administered the treatment during the war.

Nor are the advantages of oxygen respiration confined to pathological cases. One of the most direct victims of anoxic anoxæmia is the airman who flies at great heights. Everything in this paper tends to show that to counteract the loss of oxygen which he sustains at high altitudes there is but one policy, namely, to provide him with an oxygen equipment which is at once as light and as efficient as possible—a consummation for which Dr. Haldane has striven unremittingly. And here I come to the personal note on which I should like to conclude. In the pages which I have read views have been expressed which differ from those which he holds in matters of detail—perhaps in matters of important detail. But Dr. Haldane's teaching transcends mere detail. He has always taught that the physiology of to-day is the medicine of to-morrow. The more gladly, therefore, do I take this opportunity of saying how much I owe, and how much I think medicine owes and will owe, to the inspiration of Dr. Haldane's teaching.

The University Problem in London.

A REUNION of the Old Students' Association of the Royal College of Science was held on Tuesday evening, September 14, at the Imperial College Union, South Kensington. The president (Sir Richard Gregory) was in the chair, and the principal feature was an address by Prof. H. E. Armstrong entitled "Pre-Kensington History of the Royal College of Science and the University Problem in London." The address was devoted largely to an autobiographical sketch of Prof. Armstrong's early experiences at the Royal College of Science, dating back to the summer term of 1865, when he became a student of its forerunner, the Royal College of Chemistry, then situate in Oxford Street. Prof. Armstrong's reminiscences of Hofmann, McLeod, Tyndall, Huxley, Frankland, and others were all delightful to listen to, but especially of the first-named did he evidently cherish an affectionate memory. A discussion followed, in which the chairman, Dr. M. O. Forster, Prof. Whitehead, Prof. Morgan, Prof. E. W. Skeats, Dr. G. T. Moody, and Mr. S. W. Hunt took part. Some remarks of the lecturer referring to examinations seemed to be regarded as a polemic against this feature of university life, and called forth a certain amount of timid apology and some vigorous defence. Prof. Armstrong, briefly replying, explained that he did not object to examinations, but to the London system; what he was anxious to see was a series of institutions established in London which would make education effective. It was for each one to consider what was the best system, but so long as each school attempted to do everything there could not be success. The part of the address referring to this subject is subjoined.

I have now fifty years' direct experience of the London University system. Throughout this period the talk has ever been of examinations, never of learning—London has been without educational ideals. Under the system methods of teaching have been