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THE UNKNOWN FACTORS IN THE ILL EFFECTS OF BAD VENTILATION.

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It is one of the cardinal principles of modern hygiene that bad ventilation of dwellings, factories, shops, and cars is detrimental to health. Fresh air has in fact become almost a fetich. The open-air treatment of tuberculosis is assuming each year a more important part in medicine. The marked improvement in cases of pneumonia obtained by putting them in the open air is testified to almost universally by those who have tried it. In view of these facts, the statement which I am about to make sounds almost unbelievable. The statement is this: We have really, at the present time, no adequate scientific explanation for the health-stimulating properties of fresh air and the health-destroying influences of bad ventilation.

In such a statement it is necessary to limit the meaning to the effects of the air itself and not to include the effects of dust, the effects of deprivation of sunlight, the effects of sedentary habits, long hours of work, and other unsanitary conditions. Even when all of these elements are left out, however, it is clear that close-crowded, ill-ventilated rooms act upon the majority of persons and produce immediate ill effects. Those who are particularly sensitive are liable to headaches, nausea, giddiness, and even vomiting or fainting. To what are these effects due? It is my purpose to-day to consider briefly our knowledge, or rather our ignorance, of the subject, and to make a suggestion along a somewhat new line.

In this problem scientific progress has in the main served only to eliminate possible explanations. When Lavoisier discovered that breathing consists in the absorption of oxygen for the vital combustion, and the elimination of the carbonic acid which the combustion produces, the problem seemed to have been solved. The manifest answer was "lack of oxygen." Even at the present time it is quite common to hear the expression that "the air of a close and crowded room has been deprived of its oxygen." The analogies of the kerosene lamp smoking because of an insufficiency of air supply or of a fire in a stove extinguished by closed dampers have become firmly fixed in the popular mind. Every school child is taught that a mouse sealed up in a small glass jar dies in a short time. It is unnecessary to review scientific investigation at any length, however, to prove that none of these facts have really any bearing on the

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problem before us. It is well established that even in the closest hall packed with people the oxygen content of the air is practically never reduced by as much as 1 per cent. On the other hand, it is only necessary to go a short distance up into the mountains to come under an atmospheric pressure such as to reduce the oxygen supply much more than this amount, and yet mountain air is especially healthful. Moreover, the administration of pure oxygen to cases of tuberculosis has proved of no value, and it is extremely doubtful whether the use of oxygen in pneumonia is of any value. From such facts, and they could be almost indefinitely multiplied, it is clear that the amount of oxygen in the air has little or nothing to do with its stimulating or depressing properties in ordinary life or in disease.

Hygienists are accustomed to estimate the effectiveness of ventilation by the amount of carbon dioxid which the air in a room contains. It is quite certain, however, that a quantity of carbon dioxid tenfold greater than the highest hygienic limit may be breathed practically indefinitely with impunity. To mention only a single fact indicating this, take such respiration experiments as those carried out in a closed calorimeter in the Carnegie laboratory in Boston. The subject in such an apparatus breathes an atmosphere which frequently contains considerably more than 1 per cent of carbon dioxid. In this he remains for days, or even weeks, and performs active mental or physical work with no ill effects whatever, yet the standard which hygienists are accustomed to use in estimating the purity of air limits the carbon dioxid to amounts so small as "parts in ten-thousandth," with 0.13 of 1 per cent as an upper limit. For more than 50 years it has been recognized that carbon dioxid, in itself, has little or nothing to do with the relations of air to health. Pettenkofer's standard for estimating ventilation by the carbon-dioxid content of the air has continued to be used merely as a convenient index.

In the experiments of Brown-Sequard and d'Arsonval, published in 1887, the idea was advanced that the toxic effects of expired air are due to a volatile poison contained in the breath. Their experiments have been so frequently quoted that it is quite unnecessary for me to review them here. As the result of a long series of investigations by many workers, it seems to have been shown beyond all question that the breath contains no such "anthropotoxin." This was the conclusion reached, for instance, by Mitchell, Billings, and Bergey, while Formanek concluded from an extended review of the subject that the only substances appearing in the breath, other than carbon dioxid and water vapor, are the products of putrefaction in the mouth, particularly ammonia.

This chapter seemed to have been closed when, about a year ago, Rosenau and Amoss[1] revived the conception of the toxicity of the

breath in a new form. They condensed the vapor of the air expired by human beings and injected the liquid into guinea pigs. A second injection, the proper number of days later, showed that the pigs had become sensitized to some substance contained in the injected liquid. It is only fair to Rosenau and Amoss to say that, in stating their experiments, they have not claimed for them that they afford necessarily a complete explanation of the problem before us, but the suggestion that they may offer such an explanation is clear. Their more recent work along this line is, I understand, to be offered before this section of this congress. Unless, however, it goes much further than their earlier work, I must express my incredulity regarding its significance for the problem before us. There is nothing, so far as I am aware, in our present knowledge of anaphylaxis, to suggest that any species of animal can be sensitized to a protein produced by itself. It may well be that a small amount of albuminous material is carried by the expired air. If so, we should expect that guinea pigs might thereby become sensitized to human protein; but it is quite another thing to suppose that human beings can be sensitized to human protein. Another objection which weighs strongly, it seems to me, against this suggestion, is that the clinical picture presented by a person in a close and crowded room is altogether different from that of anaphylactic shock. If the suggestion of Rosenau and Amoss were correct, we should expect that persons in a close and crowded room would develop a coryza, conjunctivitis, and asthma. I say that we might expect this, for the reason that this is what happens in those unfortunate persons who are afflicted with hay fever, and particularly in those who, while indifferent to all other forms of dust and pollen, are susceptible to the skin and breath emanations of horses. In fact, in that form of hay fever which consists in sensitiveness to the exhalations of horses, we have exactly the sort of thing which, as Rosenau and Amoss suggest, is responsible for the ill effects of bad ventilation. It is evidence of some value against their suggestion, therefore, that the symptoms induced by the two sets of conditions are totally different.

A good many years ago Hermann suggested that in reality the ill effects of bad ventilation are due not to the chemical composition of the inspired air but to interference with the heat eliminating function of the skin. This view has been elaborated and placed upon a very firm experimental basis in recent years by Flügge[2] and his pupils. All of their experiments were carried out upon man. The subject was inclosed in an air-tight box of small size and the experiments were planned so as to contrast the effects of chemical vitiation of the atmosphere, on the one hand, with the physical changes of high temperature and humidity, on the other. It was found that a high degree of chemical vitiation (i. e., reduction of the oxygen and in-

crease of the carbon dioxid) was borne with no perceptible ill effects, so long as the air in the chamber was kept cool and dry. On the other hand, all of the ill effects of vitiated air were experienced, with even a slight chemical impurity, as soon as the temperature and moisture content rose sufficiently to interfere with the elimination of heat through the subject's skin. Particularly striking was one experiment in which one subject, placed within the chamber in a hot, moist, and chemically highly vitiated atmosphere, experienced all the ill effects of bad ventilation, although at the same time he was allowed to breathe fresh and perfectly pure air from outside, through a mouthpiece in the wall of the chamber. At the same time another subject outside of the chamber breathed through a mouthpiece connected with the interior. The skin of the latter was of course under hygienic conditions. His air supply was supposedly highly unhygienic, but no ill effects were experienced. On the other hand, the subject within the box whose lungs received fresh air, but whose skin was exposed to a hot and moist atmosphere, felt dizzy and became nauseated.

Very similar results have been reported by Leonard Hill [3] in England. Hill described one of his experiments in which he and several of his pupils were shut up in a small chamber, as follows:

> After 44 minutes the dry-bulb thermometer stood at 87° F., the wet bulb at 83° F. The carbon dioxid had risen to 5.26 per cent. The oxygen had fallen to 15.1 per cent. The discomfort felt was great; all were wet with sweat and the skin of all was flushed. The talking and laughing of the occupants had gradually become less and then ceased. On putting on the electric fans and whirling the air in the chamber the relief was immediate and very great, and this in spite of the temperature of the chamber continuing to rise. On putting off the fans the discomfort returned. The occupants cried out for the fans. No headache or aftereffects have followed this type of experiment, which has been repeated five times..

In the light of these facts we seem to be forced to the conclusion that the effects of bad ventilation are not dependent upon the air which enters the lungs, but upon the temperature, moisture content, and drafts of the air in contact with the surface of the body. They depend upon interference with heat elimination by the skin. They are due wholly, or almost wholly, to heat stagnation in the atmosphere contained in the pores of the clothing surrounding the body.

What I wish to put before you now is a question. Is this explanation satisfactory? I must confess that for me it is not. From the standpoint of hygiene, it is indeed practically proved. From the standpoint of physiology, however, the proof offered by Hill and Flügge merely opens up a new problem. In the experiment which I have quoted from Hill's report, it is clear that the ill effects experienced by the men in the small chamber were not due to a rise of body temperature, but merely to a high skin temperature. Quick relief was experienced when the electric fans were turned on and the

air in the chamber was set whirling, and yet at the same time the temperature in the chamber continued to rise and the wet and dry bulb thermometers showed a difference of only 4°. I do not know how satisfactory the hypothesis which I am going to lay before you may appear, but to me it seems perfectly evident that the effects experienced by Hill and his pupils in that chamber were not the direct result of skin conditions, but rather the result of some process in the lungs as yet dimly understood, which is influenced by skin conditions. In this connection I would remind you that Bohr,[4] whose recent lamented death has removed one of the ablest investigators in this field, devoted nearly the whole of his working life to the attempt to prove that the passage of gases, and particularly the passage of oxygen, from the lungs into the blood is not a mere process of diffusion, but involves a distinct secretory activity by the pulmonary epithelium. This view, it is true, seems to-day to be further from acceptance than at the time Bohr died. Speaking for myself, I may say that, up to a short time ago, I regarded it with entire scepticism.

During the summer of 1911, it was my good fortune to take part in an expedition which spent five weeks on the summit of Pike's Peak.[5] There, under almost ideal conditions for the scientific investigation of the effects of low barometric pressure upon man and under the inspiration also of continuous association with Dr. J. S. Haldane, of Oxford, I was forced to the conclusion that Bohr had been upon the track of a great scientific truth. Much of the previous work of Haldane and his collaborators had tended to support the view of Bohr regarding the activity of the lung epithelium. The results of our observations on Pike's Peak, both upon ourselves and upon others, under a barometric condition of only 450 millimeters of mercury, seemed susceptible of no other explanation than that of pulmonary activity. The results of our investigations will soon be in print and available to those interested. I shall lay stress, therefore, upon only a few points. These are, that the symptoms of mountain sickness are in nearly all essential points the same, or closely similar to, the effects of bad ventilation. Mountain sickness, however, is not due to any skin effect, but is wholly due to an insufficient supply of oxygen to the tissues because of the low air pressure. In a newcomer on Pike's Peak the lips are blue, the temper uncertain; headache, nausea, vomiting, and even fainting are common. After four or five days, however, a healthy subject becomes adjusted to the low barometric pressure so that these symptoms disappear, and an ordinary condition of health returns. Our experiments indicated clearly that the principal reason for this return to health lies in the development on the part of the lungs of the power to secrete oxygen actively from the air of the lungs into the blood. It was very noticeable that in newcomers on the Peak even a few minutes in a room, not excessively close to the senses of a regular inhabitant, was sufficient to precipitate an attack of acute

mountain sickness. If such persons went outside in the cool and breezy open air, the attack was often relieved or prevented. The chill to the skin stimulated the lungs to secrete.

In the earlier work of Haldane and some of his collaborators there are a number of facts which suggest that one of the effects of cold upon the skin is to stimulate the pulmonary epithelium to secretory activity. Taking all of these facts into account, it is logical to conclude that the lungs are normally the seat of a more or less active process of oxygen secretion, and that the effects of warm moist air upon the skin are to inhibit this process in the lungs. In a sense, such an explanation brings us back to the point from which we started, namely, to the idea that in a close and crowded room the body is insufficiently supplied with oxygen; but this insufficient supply, according to the explanation which I have stated, is due not to an insufficiency of the oxygen in the air, but to an inhibition (probably by a hormone) of the power of the lungs to utilize this oxygen.

Lack of time prevents my discussing this topic in detail. The subject needs investigation along new experimental lines rather than a rehearsal of old data. Reference should be made, however, to the recent paper of Douglas and Haldane[6] on "the causes of absorption of oxygen by the lungs," in which improved methods are described and the secretory theory strongly supported. It is also to the point to mention certain evidence along an entirely distinct line which I have been collecting for several years past and hope soon to publish. My experiments show as clearly as vivisectional methods can that, contrary to the calculations of Zuntz[7] and of Krogh,[8] the blood stream is insufficient to transport all of the oxygen taken up by the lungs during vigorous physical exercise. The conclusion seems to be inevitable that Bohr was correct when he claimed that under certain conditions a considerable utilization, or at least disappearance, of oxygen occurs in the lungs—a pulmonary combustion, so to speak.

Along these lines, it seems to me, and along these lines alone, there appears to be hope of reaching an adequate explanation of the ill effects of bad ventilation and the stimulating influences of fresh air.

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[1] Rosenau and Amoss; Journ. Med. Research, 1911, XXV, p. 35.

[2] Flügge, Zeitschr. f. Hygiene, 1905, XLIX, p. 363.

[3] L. Hill, Journ. Physiol., 1910, XLI, p. 3.

[4] Bohr. Skand. Arch. Physiol., 1909, XXII, p. 228.

[5] Report of expedition to Pike's Peak (soon to appear in Phil. Trans. Royal Soc.).

[6] Douglas and Haldane, Journ. Physiol., 1912, XLIV, p. 305.

[7] Zuntz, Zeitschr. f. klin. Med., 1912, LXXIV, h. 3 and 4.

[8] Krogh, Skand. Arch. Physiol., 1912, XXVII, pp. 126 and 227.

For recent reviews of literature, see Crowder, Arch. Intern. Med., 1911, p. 85; and Sewall, Journ. A. M. A., 1912, LVIII, p. 174.