ATMOSPHERIC DISINFECTION, AND ITS APPLICATION IN THE TREATMENT OF DISEASE.

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For a long time, I have been expecting that some important researches would be made into the subject of the conditions in which organic germs exist in the atmosphere, as a step towards an explanation of the mode by which infectious diseases spread themselves in the subtle way they do, through the medium of the air. We know how the process of putrefaction is prevented from taking place, for example, in the familiar experiment of boiling a piece of meat in water in a test-tube, and closing it with cotton-wool. We feel as certain as we can of any phenomenon that the air is full of minute forms of life, which rapidly develop in the presence of dead organic substance; and we know that the wool prevents putrefaction by the exclusion of these. We further know that, if we put a piece of meat in a solution of carbolic acid, of two per cent. strength, putrefaction will not occur, because, as we can prove easily by experiment and microscopic observation, a solution of this kind destroys those minute organic beings which appear in the putrefying process, and which I need not specify more fully.

We have reasons for believing that the monads exist in the air only under certain conditions; for they are not present at high mountain altitudes, nor when the air is overheated. We have, it is true, a certain amount of general knowledge on these points; and we are inclined to believe that what is true of the germs which develop in ordinary putrefaction is also true of other germs; such as those of malaria, of cholera, or of typhoid fever.

The question, however, which I am anxious to discuss is this. Seeing that we can destroy the ordinary putrefactive organisms when they exist in solution, by means of carbolic acid and other substances of similar nature, may we not reasonably hope to destroy the germs when existing in the air, and so protect ourselves from their influence?

I mean that, while the surgeon is applying the principles of the antiseptic system to surgical practice, may not physicians go further, and study the extension of the system in the direction indicated? Most of us must have indulged in some such ideas, but we have no very exact knowledge at present to guide us. It is easy to explain the reason of this. We want a certain number of experiments made, of a somewhat original and laborious nature. These experiments belong not to the province of the physiologist or of the chemist, and a man would be obliged to educate himself if he undertook them.

Let me explain by an example. In one of the volumes of the Reports of the Medical Department of the Privy Council, there is an account of experiments somewhat of the kind to which I refer. The object of these was to determine whether the vapour of carbolic acid would have any effect on vaccine virus. By another series of experiments, it was shown that, if we add carbolic acid in a certain comparatively small percentage to vaccine-lymph, and inoculate the arm on one side with it, and compare it with inoculation on the other arm with plain lymph, the effect of the carbolic acid is to destroy the power of the vaccine-virus; for the inoculations with carbolised lymph produced no effect. The question next to determine was whether the vapour of carbolic acid would act in the same way; for, if it were so, we might look forward to nullifying the virus of small-pox; at least, some useful facts would be likely to come out of the above experiments. Well, the method employed was very simple. Some points charged with vaccine-lymph were fixed in the top of a bottle, and so arranged that, when the top was put on, the points came to within half an inch of a strong solution of carbolic acid. There they remained for some time, and were used to vaccinate with. The result was that the vaccination succeeded, and the conclusion drawn was that the vapour of carbolic acid has no effect on vaccine-virus. Quite a fair conclusion, you would imagine. A similar experiment was performed with sulphurous acid. There the result was more satisfactory, and it was clearly shown that the vapour of sulphurous acid has an effect. Now, I do not mention this example with the intention of reflecting on the experimenter. I do it simply to show that here was an experiment of a simple kind, which required special knowledge for its performance.

The error was, in assuming that the vapour of carbolic acid is given off by a solution under the circumstances. No more effect practically

would be produced on points charged with vaccine-virus held above a solution of carbolic acid, as described, than if they were held over the top of this table. There is no evaporation of either acid, and consequently no effect could result.

You see upon what a small matter an important conclusion may depend. Sulphurous acid is volatile, and would act; and thus, when the addition of carbolic acid to the lymph produced one effect and the supposed vapour another, there was an inconsistency difficult to explain, and not encouraging in direction of further experiments. I could give you many examples of experiments similar to this. You know how frequently we hear of rooms being disinfected, as it is called, by sprinkling them with carbolic acid, or by hanging the room with sheets saturated with the acid, as is commonly done in cases of typhoid fever or whooping-cough. It is imagined that the air of the room is purified in this way. But such is not the case, for the reason already given, that carbolic acid does not evaporate at the ordinary temperature of the air. There is another fallacy which ought to be pointed out; and that is, that we can disinfect the air by burning certain substances, such as the resinous woods. It is stated in ancient history that a celebrated physician, Aëron of Agrigentum, caused the plague to cease by burning large and numerous fires in different parts of the city. Now, it is not difficult to burn carbolic acid. It can be done in an ordinary spirit-lamp. But if we consider what takes place, we can see that our object in disinfecting in this way is not to burn, and thus destroy the agent, but to volatilise it, or convert it into the form of vapour without destruction. We can burn eau-de-Cologne, but this does not vaporise the odorous substances it contains. In the case of Agrigentum, it was the smoke of the fires which acted as the disinfectant, exactly in the same manner that it acts in a chimney when meat or fish are exposed to its antiseptic action, and are thus prevented from decomposition. Forgive me, if I seem to be dealing with very simple and familiar phenomena. My excuse is, that there is a great deal which must be undone before we can begin to make any advance towards what is true.

There is one fact to which I wish to direct your attention, as it is one of great practical value. Supposing that we make a mixture of carbolic acid and water in the proportion of one of acid to fifty of water, and boil it. What do you imagine will be the effect? Will the carbolic acid evaporate quickly as the water boils, and leave the water, as is the case with benzoic acid, acetic acid, and some others? or will the water boil away, leaving the acid, as would be the case with sugar or non-volatile substances? This question can only be answered by experiment, and is one which is neither chemical nor physiological; at least, neither the chemist nor the physiologist could answer it from previous knowledge. It is a curious and singular fact that, as the water boils away, the carbolic acid evaporates almost, if not exactly, at the same rate; so that, if we evaporate twenty-five drachms out of fifty to which one drachm of carbolic acid had been added, we should find that the twenty-five drachms remaining would contain half a drachm of acid. At least, this is true for all practical purposes; if we are making experiments with antiseptic agents in the form of vapour, as was attempted in the case of the vaccine-points which I have mentioned, it is clear that we must first ascertain what physical laws which regulate the change from the fluid on solid state of the substance to the state of vapour. That is clearly the first step, and this one, as I have said, which is rather laborious, and is viscid to discuss.

Supposing, now, we boil some water containing what is a voluminous carbolic acid obtained in the way indicated, and keep it at its boiling temperature of 75° for a given period, at the end of which time, we compare it with the contents of another test-tube which had been boiled in air, and kept in air under similar conditions of temperature; what difference shall we find? In the latter case, at the end of two days, putrefaction has made active progress; in the former case, the signs of putrefaction are absent. (The method of examining the fluids is simply that of taking a drop on a slide, and placing it under a quarter-inch objective. The experiment is so simple, that anyone can repeat it.)

Let me explain all that I think is required to test the antiseptic value of vapours. A tin vessel holding about a pint is arranged so that it can be heated, and kept at the same temperature by means of a jet of gas, for any length of time. (See Fig. 1.) It is of this sort. Above this, a tin tube, five or six inches in diameter and two feet long, is fixed, so that the vapour of fluid heated in the vessel passes into it. A pint of water containing carbolic acid in any proportion will evaporate in about twenty-four hours, and thus the chamber is filled with a constant stream of carbolic vapour, and we can keep one or more test-tubes suspended in it for any number of days, the solution being replaced as required. Thus day by day we can remove a tube, and compare the state of its contents with a standard tube

¹ Read at a meeting of the East Sussex District of the South-Eastern Brahm.

exposed to the air. By this simple arrangement, I have been able to make observations extending over several weeks, without any important variation in the conditions.

You may reasonably ask what is the practical use of this kind of work. I will tell you in a few words. We are in a position to test clinically

the value of the antiseptic system in the treatment of a class of diseases which hitherto have been treated on general principles, as they are termed, and by constitutional remedies.

Amongst these diseases are, notably, whooping-cough, diphtheria, and phthisis. Of the treatment of the first, viz., whooping-cough, there is a summary in a recent number of the *Archiv für Kinderheilkunde*, by Jacob Cassel, which illustrates in a singular way the ignorance which prevails respecting the method of treatment by inhalation, a method which really depends upon an accurate knowledge of the conditions which must be observed when any particular substance is converted into vapour. These most important details do not seem to have presented themselves as deserving of consideration; and when the results of observations by one author are contrasted with those of another, the actual method employed is not inquired into or questioned. That care which the surgeon bestows upon details when following out the principles of the antiseptic system must be observed with even more attention when we are using the vapour of an antiseptic agent, than when we are employing the same substance in the fluid condition.

THE PYREXIA AND COLD BATH TREATMENT OF ENTERIC FEVER.

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THE recent discussion on the above at the Medical Society of London, and the article in the *BRITISH MEDICAL JOURNAL* of March 1st, lead me to offer the subjoined remarks on the pyrexia of enteric fever and cold applications to the skin derived mainly from observations on Europeans in hot climates.

1. On the assumption that the important and characteristic feature of enteric fever is the glandular lesion (solitary and agminated) of the small intestine, I think it must be conceded that the pyrexia is not an accurate exponent of its extent or degree; or, in other words, we cannot gauge the severity of the attack by the thermometer alone. The

instances of so-termed "walking disease," in which the individual keeps about with the lesion in progress in his intestine until some complication reveals the hitherto hidden disease-action, must have had no marked febrile disturbance. Cases occur in which the fever and early systemic disturbance run very high, and the symptoms point to marked severity of the attack; yet about the fourteenth day (or even before) the abdominal symptoms have gone, and it is clear that the usual destructive phases in the glands in severe cases have not been passed through. Again, cases occur in which the thermometer-chart is never high throughout (at no time over 104° , and generally nearer 102°), but the attack proves fatal, about the middle of the disease, from general exhaustion, and the *post mortem* examination reveals very extensive lesions in the gut; and such cases appear to be far from uncommon in India. We may have enteric fever fatal, with only a moderate pyrexia from first to last, in the tropics, the charts reading considerably under the European range; and we may have the temperature declining from day to day to death. Comparatively speaking, the pyrexia does not often kill, though it may assist other conditions thereto. The pyrexia would appear to be an exponent less of the extent and degree of the gland-lesion than of the impressibility and sympathy of the system to the progressing changes; and, upon the whole, it seems that the younger the individual, the more pronounced will be the pyrexia and general symptoms, and the less the possibility of gauging the gland-lesion by the temperature records.

2. Tropical enteric fever shows considerable deviations in temperature from the type-chart of the European disease, as in the following summary of many records. The gradual ascent from health is reached between the third and seventh day of pyrexia; the intermediate period is irregular from first to last, with considerable diurnal fluctuations; the sudden fall from this period is not uncommonly absent; the later undulations during the day are occasionally supplanted by a gradual decline, with no great range from day to day, extending to the termination of the case; and even when the fall is sudden and well marked, a gradual merging into the normal body-heat may be observed in the place of the diurnal undulations. In fact, the divergences from Wunderlich's chart are numerous, the approximations few; and an arbitrary decision of a special fever-range in this disease, and of the diagnostic certainty of the thermometer, would exclude many tropical cases with the characteristic intestinal lesion. But the variations in the temperature are not merely between one day and another, but no less observed in the range during the day. The evening reading of the thermometer is not always an exponent of the highest fever-point in the twenty-four hours; the evening temperature is, as a rule, higher than the morning, though exceptions are far from uncommon. From observations at 6 A.M., noon, 4 P.M., and 8 P.M., the temperature has been found to be undulatory; and about noon the highest fever-point is generally found. One day is no exponent of another. One day may show a regular ascent from early morning to evening, followed by a remission to the next morning, though this decline is not necessarily present; another day may show the highest point at morn, gradually declining to evening; on a third, the temperature, commencing with the morning remission, may advance 2° to 3° Fahr. towards noon, and then recede in the evening to a point higher than the morning or under it; on a fourth, it may, commencing with the morning remission, continue to decline to about 10° A.M., rise at midday and remain so until evening, or decline; or declining from midday may again ascend in the evening, thus remitting twice in the day, or even three times in the twenty-four hours, should the nocturnal decline follow. The fever acme may be reached before noon, at noon, in the afternoon, or late in the evening; though when it will occur and how (whether by gradual ascent or through immediate depressions) it seems impossible to say. The fact of these undulations probably explains the want of accord between tropical enteric charts and European charts, based on morning and evening records only. If the daily remission took place early in the day in the European disease, instead of late in the evening as it is supposed to do, the result would be, with the present method, divergencies from the type-chart and an approximation to the chart of the tropical disease; or if the highest temperature reached during the day were recorded, instead of the evening reading, in the tropical disease, there would occur a closer approximation to the European chart, the explanation lying in the variation of the daily *fastigium* as to hour. These undulations are very curious, and have been already recorded in the *Indian Medical Journal*.

3. Pyrexia, however, is invariably accompanied by waste of tissue and vital force, and with, when marked, minute textural changes in the body generally; and to keep it within 101° Fahr. in the morning, or 104° Fahr. in the twenty-four hours, is unquestionably important, especially in such a disease as enteric fever with disintegration going on, closely allied to septicæmia and marked accumulation of effete products in the blood. Yet it must, I think, be allowed that it is only, as a rule, in