extending their zone around the central thicket formed by the mould. After some days the gelatine plates, where the moulds are accidentally or artificially developed, present an extremely curious aspect. Around each mould the colonies of the bacillus icteroides, which might be thought dead, or totally incapable of developing after so long a time, constitute a species of constellation, the more innumerable the nearer to the mould. It may be said, then, that moulds exercise a kind of radial influence, within which only the development of the icteroid colonies is possible. This radius of influence is more or less extensive according to the species of mould, and the space it occupies, but it is always perfectly regular, uniformly distributed, and equidistant from the centre represented by the thicket of the mould.

Outside this radius of influence, which is always strictly limited, the development of the microbic colony sharply ceases, and the remainder of the gelatine remains sterile until some new spore causes the formation of new mycelia, which in their turn are immediately surrounded and enclosed by a new pullulation of icteroid colonies.

It is very probable that such a faculty constitutes a specific character possessed by all the moulds in general, since the six species that I have accidentally isolated from the air of the laboratory have shown themselves, although in diverse degrees, equally capable of favouring the revival and multiplication of icteroid microbes otherwise incapable of development. Further, it is possible that there exists in Nature, especially in localities where yellow fever is installed with great vigour, some mould at present unknown, endowed with a helping power truly specific and much more notable. This strange phenomenon of parasitism, this strange form of microbic saprophytism possibly represents the principal cause of the ready acclimatisation of yellow fever on board ships.

It is, indeed, highly probable that, especially in the holds of badly ventilated vessels, the constant heat and humidity, considered in their physico-chemical effects, are the conditions which maintain so long in life the microbes of yellow fever accidentally arrived there. In the hold of vessels, notwithstanding the heat and humidity, other pathogenic microbes, as those of cholera, typhus, etc., do not prosper nor maintain their activity for a long time. In respect to yellow fever, heat and humidity and scanty aëration may therefore be considered above all as the conditions indispensable for the development of moulds, and then as indirectly favourable to the vitality of the bacillus icteroides.

This phenomenon of commensalism, very analogous to that which Metchnikoff has already some time ago indicated for the cholera vibrio, also explains, and agrees with many other well-known practical observations furnished by the epidemiological history of yellow fever, and on which I believe it is useless to dwell further.

We must then consider moulds as the natural protectors of the specific agent of yellow fever, because it is solely through their intervention that the latter can find the force to live and multiply, whereas the unsuitableness of the nutritive means or the unfavourable action of the developing temperature would render its existence altogether impossible. The intervention of this factor, apparently so insignificant, constitutes perhaps the principal cause of the acclimatisation of yellow fever, not only on board ships, but also in certain localities where it appears to find conditions extraordinarily propitious to its sad dominion. In fact one of the conditions considered indispensable to the development of yellow fever, that is, humidity, is, together with heat, the condition most propitious to the development of moulds. It is above all to the defect of ventilation and the excessive hygrometric state of the atmosphere that the unhealthiness of Rio Janeiro is held to be due. During the great epidemic of yellow fever of Montevideo in 1872 the inhabitants of houses looking north only were attacked with an inexplicable preference. Both the houses and the side of the streets looking north in Montevideo are distinguished for their truly exceptional humidity.

It is, then, probable that the factor of humidity, whether on board ships or on the coasts or the interior of countries, represents the principal coefficient of a biological phenomenon rather than that common meteorological influence, the action of which is always identical in the etiology of almost all the epidemic diseases. On the other hand, the

notable resistance presented by the bacillus icteroides towards the principal factor of natural disinfection, that is to say, drying, and its great longevity in sea water sufficiently explain the ready acclimatisation of icteroid typhus and its obstinate persistence, especially in the maritime localities afflicted by the immigration of its specific agent.

This is the point to which I have had the fortune to bring our knowledge, after a year and a half of indefatigable work, concerning a terrible disease, the most grave and most urgent sanitary problem in all America. Much more still remains to be accomplished. To-day we can recognise the specific agent of yellow fever; we have it in our hands; we have minutely studied its life, customs, necessities, relations with external agents, and with other minute beings; we have succeeded in revealing the very complicated mechanism of all the numerous diseased manifestations that it determines in the human organism; finally we have placed this malady, which up to a few months ago represented a fearful mystery, on the same level as all the other great infective diseases.

The useful results for public prophylaxis, and for clinical treatment, it is not necessary to indicate here. The principal basis of social defence against diseases is the knowledge of their specific cause.

Asiatic cholera, typhoid fever, and many other grave diseases no longer inspire the terror of other kinds, because science, founded on the study of their specific agents, can nowadays protect us against their diffusion and their acclimatisation, adopting wise sanitary measures, and effecting hygienic improvements of various kinds, from whose good effects some cities have been almost restored from death to life.

However, besides the prophylactic ideal, which hygienically speaking is far superior, because it is always better and easier to prevent maladies than to cure them, or to make them disappear when they have manifested themselves, there exists another ideal—therapeutics.

Well, given the nature of the diseased process which we have studied, I do not believe it is difficult to realise also this ideal, and I have a firm conviction that very soon it will be possible to apply to man a preventive and curative treatment of yellow fever.

THE CROONIAN LECTURES

ON

THE MEANS BY WHICH THE TEMPERATURE OF THE BODY IS MAINTAINED IN HEALTH AND DISEASE.

Delivered before the Royal College of Physicians of London,

By W. HALE WHITE, M.D., F.R.C.P. LOND.,

Physician to and Lecturer on Pharmacology and Therapeutics at
Guy's Hospital.

LECTURE III.

[ABSTRACT.]

CALORIMETRY.

DR. HALE WHITE in his third lecture dealt with the calorimetric method of studying the question whether the production of heat is increased in fever. He described the ordinary water and air calorimeters, and pointed out their defects and the fallacies to which they were apt to give rise. He then described the form of differential air calorimeter which he had devised in co-operation with Dr. Haldane, Dr. Washbourn, and Mr. F. G. Hopkins. In principle it consisted of two chambers exactly similar, in one of which hydrogen was burnt, while in the other the animal on which the experiment was to be made was placed. Each chamber had a double wall. The space between the two skins was airtight and filled with air. Each of these air spaces was connected with a manometer of special design. During an experiment, the jet of hydrogen gas was kept burning in the one chamber while in the other the animal was placed. At the beginning of an experiment a note was made of the upper level of the fluid in the manometer. During an experiment the heat given off from the animal warms its chamber, and

thus increases the pressure of the air in the air-space between the two copper walls of its chamber. This would lead to movement of the fluid in the manometer, but that the heat given off by burning the hydrogen increases at the same time the pressure in the space between the walls of the other chamber. This latter increase of pressure tends to make the fluid move in the other direction. If the fluid in the manometer is stationary the amount of heat given off by the animal is, other things being equal, equal to that produced by the burning of the hydrogen. During an experiment the manometer is kept stationary by turning up or down the hydrogen flame. The amount of hydrogen burnt is estimated, and knowing the heat of combustion of hydrogen the calories produced by the quantity used in the experiment, which equal those given off by the animal, can be calculated. Dr. Hale White discussed at length the precautions which must be taken in the construction of the calorimeter, and those which must be observed in its working. He summed up his experience on these heads in the following words:—

1. No animal calorimeter that holds out any prospect of being workable can give exactly the amount of heat produced by an animal, for when it shows an alteration in the number of calories lost it does not show what proportion of them have been lost because the radiating power has been altered, and what proportion have been lost because the production has varied; but if we know the variation in the internal temperature of the animal, its weight and its specific heat, we can often say whether or not the production of heat is increased or diminished. In other words, we can get a qualitative but not an accurate quantitative result. 2. As in man alterations of loss of heat probably depend sometimes almost entirely on variation of production, and at other times almost entirely on variations in radiating and conducting power of the skin, we must be very careful in drawing conclusions from animals to man. 3. A differential instrument is very much better than one which has been standardised. 4. But the source of heat in the differential instrument must in shape resemble the animal which is to be observed, and the instrument, before the results it yields can be trusted, be proved to register equally on both sides heat lost by radiation, conduction, and convection. I think it follows that no instrument has yet been devised which gives trustworthy results. I need hardly add that I say this with great regret, for I have for the last six years devoted all my spare time to the subject. Before leaving this subject I should like to remind my hearers of two facts that are frequently lost sight of. The first is that we are too apt to talk of fever as though the process were the same whatever the cause of the fever, while really (as I have shown in the case of man) it is extremely probable that with all animals in some fevers the rise of temperature is chiefly due to variations in the production of heat, and in others it is chiefly due to variations in the loss, and any set of experiments only hold good for the particular animal and for the particular fever under consideration. Secondly, no calorimeter can register heat which is in the body converted into some other form of energy—for example, motion. For instance, if we were to light a fire in a little locomotive, and set it running up a spiral in our calorimeter, the instrument would only register such of the heat as is lost from the locomotive by radiation, conduction, and convection; it would not show that which was produced in the engine but at once converted into motion. If any heat in the living body is converted into motion we have no means of estimating it.

Many attempts, he continued, have been made to make calorimetrical observations on parts of the human body, and as a rule the results are of little value; but lately Rumpel¹ has published some results which are so very interesting that it will be well to give an account of them. His experiments were all carried out on an arm placed in an air calorimeter for it. From what I have already said it follows that I do not think the results have more than a rough qualitative value. Rumpel shows, by putting first the clothed and then the unclothed arm in the calorimeter, that the loss of heat is greatly diminished by clothing, and he says that Rubner, working with a thermopile, has shown that if the radiation of heat from the naked skin be taken at 100, with a vest on it is 73; shirt and vest, 60; waistcoat, shirt, and vest, 46; and coat, waistcoat, shirt, and vest, 33; and if a guinea-pig's hair

is cut off its loss of heat increases 33 per cent. In the next series of experiments a contrast was made between the arm clothed with dry clothes, that clothed with wet clothes, and the naked arm:—

No. of Experiment.	Naked Arm gives off Calories per Hour.	Arm Clothed with Two Layers of Dry Flannel gives off Calories per Hour.	Arm Clothed with Two Layers of Wet Flannel gives off Calories per Hour.
1	7.92	4.25	—
2	7.41	—	7.45
3	8.39	—	7.96

These figures strikingly confirm the well-known fact that wet clothing is of no use in keeping the body warm. Indeed, it is worse than no clothing at all, for, in addition to the loss by radiation, there is the loss of heat from the evaporation of the water in the wet flannel, which in each of the above two cases was 15 calories, so that the total loss of heat was in the second experiment 7.45—15=22.45; and in the third experiment it was 7.96—15=20.96. The arm lost, therefore, when covered with wet clothing about three times as much heat as it would have done if it had been naked. The next experiments proved that the loss of heat in water is much greater than that in air at the same temperature:—

Temperature of Air or Water Centigrade.	Calories Lost per Hour when the Arm is Naked in Water.	Calories Lost per Hour when the Arm is Naked in Air.	Calories Lost per Hour when the Arm is Clothed but in Air.
11.5	157.0	12.5	8.4
15.2	103.0	10.9	7.6
20.9	81.0	7.8	5.6
24.7	54.0	7.9	5.0

If to the figures representing the loss of heat to the air we add the loss which is due to the evaporation of the perspiration, it works out that at least eight times as much heat is lost in cold water as in air. As might be expected, cold water is worse than wet clothes. In one experiment in which the arm was clothed in wet clothes of a temperature of 20.6° C. the loss per hour was 29.8 calories, but in a bath of a temperature of 20.9° C. the loss per hour was 81 calories.

Another experiment confirmed the well-acknowledged fact that the lower the temperature of the air the more value has clothing in saving heat. When the temperature of the air was 6.6° C. the number of calories given off hourly by the unclothed arm was 14.27, by the clothed arm 10.69, a difference of 3.58; when the temperature of the air was 29.6° C. the number of calories given off by the unclothed arm was 4.92, by the clothed arm 4.23, a difference of 0.69. In another experiment it was shown that a linen vest was of no use in saving warmth.

REFERENCE.

¹ Ueber den Werth der Bekleidung und ihre Rolle bei der Warmeregulation, *Arch. f. Hyg.*, Bd. ix, s. 51.

PROFESSOR TH. W. ENGELMANN, of the Utrecht University, has, it is stated, accepted the invitation of the German Government to accept the Chair of Physiology rendered vacant by the death of the late Professor Du Bois-Reymond at the University of Berlin. His departure from Utrecht is a great loss for the University to which he was attached for twenty-five years.

THE CHARTERED NURSES' SOCIETY.—The first report of this Society shows that it is doing a sound, remunerative business on the co-operative principle. The balance-sheet is not issued, but in the report it is stated that all the working expenses have been met, leaving a small profit. The fees are such as will suit only the pockets of the wealthy; but, conceding that the nurses supplied are of the best, those who engage them will know how much they will have to pay for their services.