GULSTONIAN LECTURES ON THE HEAT OF THE BODY.

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LECTURE I.—*Concluded.*

WE must admit the truth of Galen's doctrine, that the lungs not only cool, but also fan the blood. But the body at large likewise produces heat, as a result of the respiration of the tissues. This is a fact past dispute. That arterial blood contains more oxygen and less carbonic acid than venous blood, belongs to the elements of physiology. Moreover, the venous blood of any part is poor in oxygen and rich in carbonic acid, according to the functional activity of the part. The vital processes in many parts of the body are so slow that (excepting the brain, of which little is known) the glands and the muscles are the only structures which can be supposed to furnish any considerable quantity of heat. And the glandular and muscular tissues together make up the chief part of the body. Let us, then, investigate the calorific power of these two tissues separately.

First, with regard to the glands. Their temperature rises with the energy of function. At least, this is certainly true of glands whose secretions are the products of oxidation. Ludwig has shown that the temperature of the salivary glands rises 2.7 deg. Fahr. during secretion. And when we remember the manner in which the secreted products are continually carried off, and therewith heat, it is safe to conclude that the amount of heat which most glands set free is great. The clear carmine colour of the renal venous blood would seem to show that little oxidation, and therefore, probably, little production of heat, go on in the kidney. Yet even in the case of the kidney, the elaborate experiments of Alex. Schmidt tend to prove that the oxidation processes within that organ are extremely active.

Next, as to the muscles. Their activity generates heat. Heidenhain, by means of a thermo-electric apparatus applied to the muscles of frogs, found that a single contraction raised the temperature from $\frac{3}{10}$ to $\frac{1}{1000}$ of a degree Fahrenheit. The tetanic state raised the temperature more than a quarter of a degree. Now, this heat, whether simply mechanical force transformed or not, undoubtedly indicates chemical changes going on in the muscles, proportionate to its activity.

But, now, a question upon which I have already touched once or twice can be postponed no longer. The lungs produce heat, and the rest of the body produces heat. But the chemical processes which underlie this production of heat—where do they go on? in the blood, or in the tissues? Was Galen right when he attributed the animal heat to a burning of the blood? "Not one-hundredth part of the oxidation processes of the body goes on outside the bloodvessels." Such is Mayer's dictum; let us examine the grounds upon which it is based.

Mayer seeks to support his dogma by two lines of argument. The first argument, briefly put, is as follows. The pressure of the blood in the blood-vessels, from within outwards, is such as to hinder endosmosis or the passage of liquids from without inwards; in other words, the blood-pressure renders what we may call the intravasation of fluids difficult. And, therefore, do the lymphatic vessels exist, so as to carry off the results of exosmosis, the fluids extravasated from the blood. Now, the quantity of liquid flowing along the blood-vessels is 350 times as great as that which escapes from the thoracic duct in the same space of time. That is to say, only $\frac{1}{350}$ part of the blood, of the oxidating liquid, exists outside the blood-vessels in the lymphatic and other tissues. Yet the blood is deoxidised; *ergo*, not one-hundredth part of the oxidation-processes of the body goes on outside the blood-vessels. You will have observed several serious fallacies in Mayer's argument. He seems to assume that all exosmosis is filtration under pressure; moreover, he takes no reckoning of the secretions. Hence, for these and other reasons, we cannot admit the force of Mayer's first argument; which, however, I believe, Mr. Heaton has recently put in an improved form, and has still arrived at Mayer's conclusion.

Let us proceed to Mayer's second argument, based upon muscular

activity. This subject is so very important, and knowledge concerning it has increased so much of late, that we will desert Mayer's guidance in all but in principle.

That chemical changes go on in a muscle during its activity, no one doubts. The venous blood of an active muscle is richer in carbonic acid, and poorer in oxygen, than in a muscle at rest. Something, then, in the muscle takes up oxygen, and something sets carbonic acid free. What is that something? is it the muscular fibre? We might be strongly tempted to answer straightway no, in view of a few facts such as I will proceed to lay before you. In the first place, Hermann has shown that the presence of oxygen is not necessary to the contraction of muscle; that it contracts in a vacuum, and also in an atmosphere containing no oxygen. He has also shown that no oxygen is stored up in muscle; or rather say, that no oxygen can be pumped out of it. Next, the nitrogenised excreta are not increased by muscular exercise. This fact has been proved, as far as the urea is concerned, by Voit and Haughton, in opposition to the earlier analyses by Lehmann. And Dr. Parkes has shown that the same holds good of the nitrogen passed off from the bowels. Thirdly, the nitrogenised excreta are not proportional to the amount of muscular work performed. I hope, sir, that you will not think that I am straying from my text, or (in respect of the subject which Dr. Parkes has chosen for the Croonian lectures this year) going over unnecessary ground, if, upon this point, I detain you for a minute with an outline of the well-known experiments of Fick and Wislicenus. The study of the muscles, more than of any other tissue, promises to shed great light upon the relation in which oxygen stands to tissue-change. This was the problem put forward by Fick and Wislicenus: Is that quantity of heat which is represented by the nitrogenised matters excreted with the urine during and after hard muscular exercise? does the heat thus estimated correspond in quantity to the heat represented by the mechanical work performed? If there be a deficit in the heat indicated by the nitrogenised excreta, obviously the work is not wholly due to oxidation of nitrogenised tissues—say, muscular fibre. These two men ascended the Faulhorn, and performed between them an amount of work equal to 270,000 kilogrammeters. During the exertion, and for six hours afterwards, they excreted an amount of urea which, reckoned as muscular fibre, would yield by combustion 212,000 kilogrammeters; thus leaving 58,000 kilogrammeters of work actually performed unaccounted for by the amount of nitrogenised tissue burnt. And I beg you to remember that no account is taken of the force given off in the form of heat; and still more, that no account is taken of the work performed by the heart and respiratory muscles, work valued at 340,000 kilogrammeters. At this rate, only one-third of the force set free was accounted for by oxidation of nitrogenised tissue. And Dr. Frankland reduces the third to a fifth, putting, as he does, the combustion-heat of muscle much lower than did Fick and Wislicenus. We speak of half a million of kilogrammeters of work done by two men in a day, and yet the amount of combustibles required for all this is really very small. To take an illustration of Dr. Tyndall's. A man of 145 pounds' weight goes up Mont Blanc; he raises 145 pounds 15,000 feet high. The heat-equivalent of 15,000 feet is twenty degrees Fahrenheit; so that this man consumes heat enough to raise 145 pounds of water twenty degrees; no more than generated by the combustion of two ounces of carbon.

It is, then, highly probable that the oxygen which is lost by the blood of active muscle is not employed in oxidation or combustion of the nitrogenous plasma of the muscular fibre. Yet, after all, it is the muscular fibre which does the work; and, moreover, the common combustion-product, carbonic acid, is set free. Many persons, putting these facts together, and taking for granted that the absorption of oxygen and the liberation of carbonic acid necessarily presuppose combustion—many persons, I say, have deemed it to be well-nigh proved that the assumed combustion must go on in the blood; that carbon is oxidised there; and that the force which this oxidation sets free is assimilated, as it were, by the muscular fibre, which is, in fact, simply the piston and cylinder which transform the heat, generated by oxidation of non-nitrogenous matters of the blood, into mechanical work. But we may not leave the discussion at this point. To the plausible doctrine which I have just stated several weighty objections must be taken. First, it is at least possible that it may be the non-nitrogenous elements of muscle which are oxidated; next, it is certainly a mistake to suppose that heat can be transmuted directly into work without the simultaneous concurrence of at least an equal amount of change of another kind. Thus, in an engine, the heat changes the physical condition of the water, and it is the altered state of the water, the expanding steam, which sets up the movement. And so in the case of the muscle: if the heat of the blood become indeed muscular motion, there must be some intermediate change going on in the muscle—some physical change, analogous to the formation of steam. This physical change in the muscle is doubtless,

coincident with a chemical change, albeit not an oxidation-process, as we have already seen. In fact, Hermann believes that the contraction of living muscle and the *rigor mortis* are alike in kind. The highly complex nitrogenous plasma of the fibre breaks up, under "irritation", into the firmly contracting myosin and other compounds, chiefly acids, which render active muscle sour. The contraction of the myosin is the contraction of the muscle; by its change of physical state, force is set free, both mechanical force and heat: but nothing can come of nothing. The force set free must have been previously absorbed by the muscle from the blood—probably when the myosin undergoes a synthesis back into the original muscular plasma. Hermann suggests that oxygen may take part in this synthesis, and that carbonic acid is produced, together with the paralactic and other acids, of which I spoke, when the plasma breaks up again and contracts. Now, if this be so, then the absorption of oxygen and the liberation of carbonic acid do not form part of the same, but of opposite, processes—there is no combustion. During rest these processes may be of equal activity, but during work they are not so; and it is quite possible that the observation of Pettenkofer and Voit, that oxygen is stored up by the body at night-time, may have something to do with the regeneration of muscle. Combustion takes no part in these changes; but we need not wish to deny that the soluble by-products of the formation of myosin are, as much as possible, oxidated in the blood of the muscle, and that any surplus is carried to the lungs and oxidated there. If, then, by oxidation we imply combustion, or degradation of the amount of tension-force contained in any substance, Mayer's doctrine, so far as muscle is concerned, may still be true, "that the hearth of the oxidation-process is the channel of the blood-vessels"; and again, "not one-hundredth part of the oxidation-processes of the body goes on outside the blood-vessels"—for muscles do not oxidate in this sense of the word.

Let us pass on to consider whether there be any direct proof of the oxidation of the blood; and for this purpose we can employ the observations of Estor and Saintpierre, Hoppe Seyler, and Alex. Schmidt.

Estor and Saintpierre found that the quantity of oxygen contained in the blood of different arteries was not always the same. For instance, when the carotid blood contained 21 per cent. of oxygen, the renal blood contained only 18 per cent., the splenic blood 14, and the femoral only $7\frac{1}{2}$ —that is to say, the blood of the femoral artery contains only one-third as much oxygen as the carotid blood contains. This seems to imply that the farther the blood is taken from the lungs the less oxygen does it possess. Estor and Saintpierre conceive that the change which goes on in the blood of the arteries is the same as that which goes on in the blood of the capillaries; namely, an oxidation of some constituents of the blood; and also, that the capillaries increase the venosity of the blood just in proportion as they foreshadow its current, and thereby allow more time for the oxidation to go on in.

Hoppe Seyler admits the facts of Estor and Saintpierre, but draws a different, nay, an opposite, conclusion from them. When he put two ligatures, at some distance apart, upon an artery, enclosing blood, he found that the blood did indeed become venous, but that the venosity proceeded from the wall of the artery. This observation led him to the belief that the blood does not, as Estor and Saintpierre thought, oxidate matter within itself, but that it gives up oxygen to the tissues around, and that the blood, which is close upon the walls of the vessels, loses its oxygen first, as might be supposed. From this and other experiments, Hoppe Seyler concludes that the oxyhæmoglobin of the blood is not the direct means of oxidation; and also that, in vertebrata, no oxidation-processes go on in the blood.

But Hoppe Seyler's views are greatly at variance with the inferences which we may fairly draw from the important experiments of Alex. Schmidt—experiments which seem to leave no doubt possible that venous blood, *e.g.*, the blood of suffocated animals, does contain a large quantity of some material which combines with free oxygen very speedily. Schmidt also found that the venous blood of active muscle contains much more oxidisable matter than the blood of muscle at rest. And herein, probably, lies a partial explanation of the fact, that the venous blood of contracting muscle contains less oxygen than that of quiet muscle. Considerations such as these are closely connected with a topic which we shall have to discuss when we come to the theory of fever—I refer to what has been called the pyrogenic, or fever-making, power of healthy blood.

And now, sir, that we have ended our survey of the chemical theory concerning animal heat, I will proceed to the third, or the mechanical, theory. Harvey was the unconscious founder of the mechanical school of physiology; for he had shown the hydraulics of the circulation to be so beautifully simple, that men sought for mechanical explanations of all the functions of the body. But the mechanical theory of animal heat was not well established until nearly a century after Harvey. If we turn, for example, to the famous Borelli, whose book *De Motu*

Animalium, or rather the second volume of that book, was published in 1681, there, in the 225th proposition, we read as follows: "The violent movement of the heart is the effective cause of the heat of the body, and not the reverse; that is to say, the heat of the body is not the productive cause of the movement of the blood." Borelli had previously striven to prove that each contraction of the heart is able to overcome a resistance of equal to 180,000 pounds—an adequate source of friction-heat surely. You will remember that my colleague, Dr. Black, in the Croonian lectures which he delivered in this place, pointed out the reason of Borelli's great mistake. Five years after Borelli, Stahl also taught that the blood is heated chiefly by motion, and chiefly, moreover, by motion through the lungs; and hence those animals which breathe most have hottest blood. But what did Borelli, Stahl, and Hoffmann, mean by this motion which was of power to generate animal heat? They did not mean motion of the blood in mass along the vessels; for, as Borelli declares, "I believe that the origin of heat is by no means to be sought in the motion as mere motion: the heat does not depend upon friction of heterogeneous particles of blood". In fact, by motion they meant a turbulence of certain inflammable particles existing in the blood—possibly a dim conception of those molecular movements which are supposed in the present day to underlie the phenomena of heat.

Haller tells us that, towards the close of the seventeenth century, when more regard was had than previously to the solid parts of the body, and when the authority of chemical causes began to decay, a notion sprang up that the heat of the body was produced by motion in the sense of friction. Malpighi's discovery of the capillary vessels and of the blood-corpuscles rendered this explanation easy. The blood rubs against the walls of the vessels, and the corpuscles rub against each other, so that animal heat became a mere topic of the circulation of the blood, and is so treated of by Haller in his *Elements*, where he thus sums up: "So far, at all events, it is most probable that the blood is heated by motion", that is, by friction. In fact, the use of the corpuscles is to increase the friction, and the iron which the corpuscles contain is serviceable to the same end, for is there any other body which becomes so hot by friction as iron does?

Before we leave this doctrine, you will allow me to refer to one more particular. The physiology of the last century, as represented by Haller, did not need a respiratory function. All the animal machinery could be explained excellently well without what seemed to be an unnecessary process; and yet the fact remained that animals did breathe, and also quickly died if they did not breathe. Then respiration simply distended the lungs, and so enabled the pulmonary circulation to be carried on. But what when animals continued to breathe exhausted air and yet were suffocated? The air, in that case, might be supposed to lose its elasticity, so that the lungs collapse, nevertheless, and the circulation is arrested. We also believe that air which has been breathed loses its elasticity or tension-force, but hardly in the gross sense of the word elastic as understood by Haller.

The mechanical theory of heat at the present day resolves itself into a matter of the transformation of muscular work into heat. Undoubtedly the whole force exerted by the heart becomes changed into heat, in consequence of the resistance which the blood-vessels oppose to the pressure of the blood. But, nothing can come of nothing. The heart does not create all this motive power out of nothing. The heart, like any other muscle, is only the means by which the conversion of force is brought about. The heart abstracts tension-force from the blood, and returns it to the blood as motion; but the heart is not consumed. The power of the left ventricle has been estimated at 56,000 kilogrammeters per twenty-four hours. But, if we take the much lower estimate of Dr. Playfair—38,000 kilogrammeters for the whole heart, seeing that the combustion-heat of muscle is 660 kilogrammeters per *gramme*—a simple calculation shows that, if the heart's force were due to combustion of its muscular tissue, it would be wholly consumed in about two days. The results at which we arrive with regard to the voluntary muscles are thus corroborated.

When the body is at rest, the movements of the heart, chest-walls, and other parts, are wholly converted into heat. It is only when the body is performing work, that is to say, imparting motion to external objects, or to itself as a whole, that any of the force set free in the body is not manifested as heat. The friction of the circulation is no gain of heat to the body: whether the ingested tension-force pass directly into heat, or whether there be an intermediate stage of muscular movements, makes no difference. The heat produced by friction within the blood-vessels was once thought to be illustrated by an observation of Bernard's, to the effect that the blood of the renal vein is warmer than that of the renal artery; the excess of heat within the vein was thought to be wholly due to friction, inasmuch as the venous blood is of a bright carmine colour, supposed to indicate the small amount of oxidation which goes on in the kidney. But the experiments of Alex. Schmidt,

to which I have already alluded, show that oxidising processes within the kidney are not slack. The observation of Onimus is more to the point: that the ligature of a large artery instantly raises by one degree the temperature of the blood hurled against the obstruction.

Before leaving this part of our subject, I may just remark, with regard to the actual amount of heat produced by the animal machine, that Helmholtz reckons that a man of 180 pounds' weight, under ordinary circumstances, generates in twenty-four hours an amount of heat which would raise 11,000 pounds of water one degree Fahrenheit. In other words, the heat which he sets free would raise a weight of water equal to his own sixty degrees.

The second division of the subject, the ways by which we lose our heat, shall not keep us long. Cold ingesta, air and food, are an obvious cause of loss of heat to the body. The same may be said of radiation and conduction from the surface. Evaporation of water from the skin and lungs abstracts a large quantity of latent heat. Radiation and evaporation from the skin are very variable quantities, dependent upon the state of the skin as to vascularity, and upon the state of the atmosphere as to heat and moisture. But, the evaporation from the lungs is much more constant, inasmuch as the air within the lungs is almost invariably saturated with watery vapour. The frequency of the respiratory movements is the chief means by which loss of heat from the lungs is regulated. The remaining causes of loss of heat are less important. Liquefaction of the solids of the food probably absorbs some heat; also certain processes of hydration and dehydration, such as Berthelot has pointed out. The separation of carbonic acid from the blood; the synthesis of the more complex molecules of the body, hæmoglobin and the plasma of muscle and nerve; also the casting off of imperfectly oxidated matters, epithelium, milk, sebaceous matter, kreatinin, hippuric acid, and so on; all these must be means by which the actual or potential heat of the body is diminished.

But when we pass from the animal at rest to the working animal, we light upon a new and active source of loss of potential heat. Yet, inasmuch as contracting muscle generates heat at the time of contraction, and also the cardiac and respiratory movements are increased, the actual temperature of the body is raised by exercise. And just as work is a loss of potential heat, so this formation of heat by exercise is a loss of potential work. To paraphrase Mayer once more:—"The production of work and heat are antagonistic; hourly experience teaches this. To go far, we start slowly; haste makes waste. The workman seeks to lessen sweating, thereby to husband his strength; no horseman likes his horse to become warm. In common life we say, no work should be carried to the sweating point; in scientific language, the increased production of heat is at the expense of the power to work. With the same consumption of force, it is the cool phlegmatic man who labours to the greatest profit."

And, lastly, with regard to another form of force—electricity—Paul Bert has made the interesting observation upon the torpedo, that repeated discharges of electricity lower the temperature of the body. Potential heat is lost as free electricity.

THERAPEUTIC MEMORANDA.

THE EFFECTS OF SANTONINE.

THE instance recorded by Dr. Sieveking, of santonine producing urticaria, is, I should think, unique. I have frequently administered it for the *ascaris lumbricoides*, which infests, principally, I believe, the jejunum, with unexceptional success, and generally order it to be taken on an empty stomach, in a small quantity of a semi-fluid vehicle, such as arrow-root. I am not aware, however, that it possesses any efficacy in *ascarides*, as Dr. Sieveking has stated. It is said that santonine is only soluble in the alkaline secretions of the small intestines, where its active and bitter principle acts as a vermicide. In several instances patients, both children and adults, have complained sometimes of nausea and giddiness, as well as a yellow tinge imparted by vision to surrounding objects, similar to that assumed by the salt itself when exposed to the light for any time.

In these cases I have noted that none of the parasites were expelled, the remedy having been taken on suspicion of their presence, or after their expulsion, which sometimes occurs spontaneously or after the administration of some other drug.

Lacock, Wilts.

J. H. CRISP.

LECTURES ON DERMATOLOGY.

DELIVERED AT

The Royal College of Surgeons of England.

By ERASMUS WILSON, F.R.S.,

Professor of Dermatology in the College.

LECTURE V.

ECZEMATOUS affections are entitled to the fullest share of our attention, as being the most numerous and probably the most important family of diseases of the skin, and as including all the more common forms of cutaneous disorder. Thus we have seen eczema represented as a simple inflammation of the skin, manifesting its operations by an erythematous, a papulous, a vesicular, an ichorous, a pustulous, and a squamous lesion, each of these lesions being accompanied by one or other of the rest in a greater or less degree. Next, we have seen the papular type of eczema in a separate form, and the pustulous type of eczema also in a separate form—the former of these receiving the name of lichen, and the latter that of impetigo. Then I have endeavoured to explain that the chronic stage of eczema, when the principal remaining lesion is exfoliation of the cuticle in small fragments or scales, has received the name of psoriasis and pityriasis; and, moreover, that an eczema resulting from the presence of a parasitic animalcule, the *acarus scabiei*, is denominated scabies. In this manner we have had presented to us six terms of common use in relation with dermatology; namely, eczema, psoriasis, pityriasis, scabies, lichen, and impetigo. Every one of these terms represents a well-defined form of disease; and, although all the forms of disease so represented are of the nature of eczema, and are members of the group of eczematous eruptions, yet each has peculiarities of its own, which render their separate consideration a matter of convenience.

It may be remembered, however, that I took occasion to except from the eczematous family two forms of lichen; namely, lichen planus and lichen urticatus, both of which are sufficiently well marked to take a position by themselves, and in some future classification to be allied with some other group; for example, lichen urticatus with urticaria, and lichen planus with a group founded on the follicular pathology of papulæ. The distinction between follicle and non-follicle opens up a very interesting field of inquiry in relation with cutaneous disease. It may be stated in general terms that the papillary portion of the skin cannot be inflamed without participation, to a certain extent, on the part of the follicle; but the reverse of this proposition is not equally true, for folliculitis in a great variety of forms may exist without a similar participation of the interfollicular papillary layer. The evidence of this fact has been before us in the instance of polymorphic eczema, in scabies, and especially in eczema papulosum and lichen. In eczema papulosum, the pathological state of the follicle is one of simple hyperæmia and consequent infiltration, and therefore comes under the denomination of *dry eczema*; but we also find in relation to the follicles a state corresponding with *moist eczema*. If an excoriated surface in eczema ichorous may be closely examined, the follicles may be seen giving exit to a transparent lymph in considerable abundance; and in the purulent stage of the exudation the excretion may be opalescent and puriform; so that the same language that we have heretofore held with reference to eczema in general may be applied separately to the follicles, which in the eczematous family present us with an erythematous, an ichorous, and a purulent form of folliculitis.

With the exception of eczema papulosum and lichen, the state of the follicles is most prominently brought before us in impetigo, which in its pathognomonic form is a purulent folliculitis. It is this pathological structure that gives to impetigo its limited and fixed character. It is never so extensive as common eczema, but evinces a disposition to develop itself in small circumscribed blotches—impetigo figurata; and it is very commonly met with as an affection of single follicles—impetigo sparsa—particularly in the region of the scalp and upon the face, where the follicles are larger than elsewhere. As the term "catarrhal inflammation" has sometimes been applied to eczema by way of illustration, we might with equal truth make use of the term "catarrhal folliculitis" in reference to impetigo as a similar illustration.