GULSTONIAN LECTURES ON THE HEAT OF THE BODY.

DELIVERED AT

The Royal College of Physicians, London,
MARCH 1871.

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LECTURE I.

MR. PRESIDENT,—Sir, when yourself and the Censors of the College honoured me by a request that I would undertake the Gulstonian Lectures for this year, my mind at once turned to the topic of Fever. I did not forget that it is only sixteen years since Dr. Parkes, upon the same occasion as the present, chose that very theme, and handled it in a manner which still lives, I doubt not, in your memory. And when I began to think of what might be fairly looked for from him who should profess to set that subject forth in a fashion at all worthy of our present knowledge; when I considered how fever, which includes almost the whole pathology of animal heat, is itself only a portion of the physiology of animal heat; when I saw how the physiology of animal heat had always been a reflex of the current doctrines concerning heat in general: led on in this way from medicine to pathology, from pathology to physiology, from physiology to chemistry and physics, the horizon of my subject spreading ever wider and wider, I became, I must confess, afraid of it. But just as the difficulty of the subject, so is its importance. That "heat is the source of life", was taught by philosophers of extreme antiquity, before the physical sciences can be said to have existed; and we, after the experience of nearly three thousand years, seem likely to recur to an opinion not very dissimilar. Be this as it may, that which most concerns us as physicians—the doctrine of fever—is now, always has been, and always will be, one of the most attractive branches of pathology. For these reasons, I determined not to forsake my earliest purpose, hoping that I might be able to give such a view of the present state of science on this behalf as would not be altogether unworthy the attention of a learned assembly like that which I now address.

I have just spoken of the difficulties which are due to the largeness of a subject, from which, indeed, we may wander on almost every side into infinity; but throughout these lectures I shall earnestly seek to ask your attention to those topics alone which may be strictly deemed to belong to the knowledge of a physician. And, having said this, I now, Sir, feel less loath to delay our entry into the physiology of animal heat until we have made a short inquiry as to what physics have to say upon the subject of heat in general. And this, haply, may be more needful, inasmuch as the last thirty years have seen doctrines, which dated back to the very beginning of knowledge, finally given up. The old or the material theory concerning the nature of heat, born in the earliest age of Greek philosophy, lived on, under various guises, until the year 1842. Fire was a form of matter—matter in its most attenuated state: a conception which would be found less difficult by the ancients, because they did not associate necessarily matter and weight. Fire, though a substance, need not have weight—being, in the speech of fifty years ago, an imponderable element. Doubtless those four first qualities of things which were supposed to mould the universal substance into its elementary forms—the hot quality and the cold, the moist and the dry—were a kind of foreshadowing of the modern doctrine of forces. Nay, the very word, *δυναμεις*, seems to have been applied to those prime properties. Nevertheless, the material notion of heat was always uppermost. And no wonder, for of the eternity and mutability of matter the Greeks had attained to clear ideas. The eternity and mutability of force it was reserved for our own age to develop. The phlogiston of Stahl and the caloric of Lavoisier (the matter of fire, *matière de feu*, *Wärmestoff*) were merely the old fire under new names.

Physicians, Sir, must always feel deep interest in the modern theory of heat; for it was first worked out by a physician. Dr. Julius Robert Mayer, practising in the little town of Heilbronn, in the kingdom of Würtemberg, first unfolded the modern or dynamical theory of heat.

Mayer's earliest publication, *On the Forces of Inorganic Nature*, appeared in 1842. In 1845, he brought out a treatise upon *Organic Movement*; and this is, for our purposes, his most important work. You will remember that, about the same time, Dr. Joule of Manchester, working at a part of Mayer's argument, came, by way of experiment, to the very same conclusions concerning the mechanical equivalent of heat at which Mayer had arrived from the opposite direction. Mayer's process was wholly *à priori*. He starts, in his first tract, from the axiom that the cause is equal to the effect; and of his second book the fundamental fact is simply this, that nothing can come of nothing. And in both these treatises of Mayer's there is not a single experiment, scarcely a single observation of his own; so that Joule's empirical method came as a most timely support to the much more comprehensive scheme of Mayer. To be sure, the dynamical theory of heat had been vaguely hinted before Mayer's time—notably by Lord Bacon in the *Novum Organon*. But the indestructibility of force is Mayer's real thesis, of which the dynamical theory of heat is only a portion.

We are now to look upon heat as an accidental state of matter, a mode of motion—molecular motion: in fact, a form of force. We are to regard power as indestructible, but as existing under different forms—forms which are interchangeable, and which, just as was said about the elements of old time, run a perpetual circle, multifarious, and mix and nourish all things. One fact which we shall have continually brought before us hereafter is this: that any kind of force may exist in two conditions—the tension-force and the living force of Helmholtz: tension-force, the *vis inertiae*, the force which lies latent in the *status quo*, the force present in the spring of a watch which has been wound up, the force present in a quantity of gunpowder which has not been exploded, the force present in a Leyden jar which has not been discharged: and next, living force, *vis viva*, the force made manifest by change of state—as, for example, in the cases just alluded to, the force which is set free by the uncoiling of the watch-spring, by the explosion of gunpowder, by the discharge of a Leyden phial.

There is no mistaking the great influence which this theory has exerted upon physiology. It will, I trust, be an useful clue to the whole remainder of our subject, if I read a passage from the beginning of a small paper upon fever, which Mayer published in 1862. "Let us set before our eyes", says he, "a strong and active man of middle age, whose bodily frame we may conceive to keep upon the whole a constant state, through all the changes of life, for days, weeks, months, and years. The body of this man is continually producing heat, heat which is given up to the surrounding things of lower temperature than he; but at the same time this man is performing, day by day, a certain amount of mechanical work by setting free the living force of movement. This mechanical power, however, changes into heat by friction, impulse, compression of the air; and the heat, thus indirectly generated, is the equivalent or measure of the work performed. Let us reckon together the heat directly given off from his body and the heat produced indirectly by metamorphosis of work, and the sum total will be the equivalent or measure of a chemical process which has been going on in his body at the same time. The lungs take up electronegative oxygen; the stomach, on the other hand, takes up electropositive matter in the form of food and drink; and the final result of a succession of vital processes is that these substances are thrown out again, but with a change of form and loss of force. They were taken up in a state of chemical isolation; they are thrown out in the combined or burnt condition."

It is my purpose to deal, first, with the production of animal heat; next, with its loss; and, in the third place, with the balance struck between these two processes, namely, the temperature of the body. The last topic will be most interesting to us, as physicians, bringing along with it, as it does, a discussion of the theory of fever. And, therefore, to the subject of the temperature of the body I shall devote the second and third lectures.

Let us, then, proceed to an examination of the sources of animal heat. Three theories concerning its origin have at sundry times held sway; namely, the theory of innate heat, the mechanical theory, and the chemical theory.

And, first, of the theory of innate heat, which shall not detain us long, and yet which I am loath to pass altogether by; for do we not sometimes hear it said that the old expression, augmented innate heat, reflects our modern notions of pyrexia? In a few words, then, I will endeavour to recall to your mind the nature of the ancient conception of innate heat. It has all the look of being the offspring of some such meditation as engendered the fable of Prometheus, or the old Persian faith. Ancient physiology, however, and with it the doctrine of innate heat, first took definite form in the hands of Pythagoras and his immediate successors. And the physiology of Pythagoras is of the same kind as the chemistry of Paracelsus and the geology of Thomas Burnet—a

fruit of the imagination working almost without materials, and almost without the aid of any other faculty; apt for nothing but to breed what Bacon calls vermiculate questions, and best set forth in that famous passage of the sixth *Æneid*, where we read of the spirit or breath which feeds the inward parts, the mind which agitates the mass, whence spring man and beast, and then the nature of that internal vivifying force—

"Ignes est ollis vigor et cœlestis origo
Seminibus."

Beyond this physiology the Greeks and Romans did not advance, except in a few directions. Hippocrates, indeed, separated medicine from philosophy; but no Hippocrates arose to do the same good service for physiology. Hippocrates himself shunned physiological questions; the other writers of the Hippocratic treatises were of the Pythagorean school. Let us glance for a moment at the little books, on the Heart, on Fleshes, and on the Nature of the Bones. Heat is immortal, and the source of all life. The heat of the body is innate or connate, born with us. The blood is the hottest of all the humours, because the innate vital heat is seated in the blood, "where life and life's companion, heat, abideth"; hence the chest is the hottest part of the body, and the heart the hottest part of the chest, because containing most blood. The heart is the fountain of the blood and the source of the arteries, the liver being the source of the veins; and just as the vital heat of the liver tempers food into blood, so is the air, which is drawn into the heart during its diastole, tempered by the heat of the left ventricle into the animal spirits, which are dispersed through the whole body during the heart's systole. Doubtless it would not be difficult to discover in all this a dim foreboding of great truths.

The theory of innate heat survived, little changed, until the middle of the seventeenth century. From 1550 to 1650 many books upon this topic were published, written by those who were the last of their race. I have looked over four or five of these treatises—read them I cannot say, for no reading, possibly, could be more stale and unprofitable—the old, old quotations from Galen and Aristotle, hashed up for the hundredth time, and seasoned by some monstrous new hypothesis. Metaphysical physiology was run quite upon the lees; but Harvey had already appeared.

Two theories grew up to fill the void left by the death of the innate heat hypothesis, namely, the mechanical and the chemical; and, for reasons which are not far to seek, the mechanical theory was earliest ripe: nevertheless, it will be most convenient if we deal with the chemical theory first. The chemical theory of animal heat dates from the year 1777, but it had been anticipated many centuries before. I must ask you to accompany me back to the first experimental physiologist—I mean Galen. His book on the *Use of Breathing* will enable us to ascertain his views concerning animal heat. He declares that the chief use of respiration is to maintain the innate heat. He alludes to the common comparison between the heat of the body and that of a flame. "Now if," says he, "we could only find out why flames are extinguished for lack of air, perhaps we should also discover what it is which animal heat finds useful in respiration. When I see a furnace going out because it could not breathe, and then, when opened, to expire much smoke and inspire much pure external air, and, both these acts performed, to give a bright flame, I argue that it is no small use of expiration to evacuate that which in the blood corresponds to smoke; for we are a spark, and smoke and all excess of burnt material of this kind put out a fire just as water does; wherefore, they are most to be believed who teach that animals breathe for the sake of their internal heat. Now, fanning and cooling in moderation are both useful, for they both seem to increase the internal heat; and it is also necessary to expel the fuliginous matter, so to speak, which is in excess by reason of the burning of the blood. Wherefore, from fanning the source of the internal heat, and next, from moderate cooling of it, and thirdly, from the evacuation of all that which is like unto smoke, from these we collect one sum, which is the preservation of natural heat." I feel sure that I need not apologise for reading these remarkable passages. We moderns call fanning oxidation, and smoke carbonic acid.

Let us return to the middle of the seventeenth century, and there we find a young man of honourable memory, John Mayow of Oxford, with a spirit of scientific prophecy worthy of Galen, discussing the nature of oxygen exactly one hundred years before oxygen was discovered. I am tempted to string together a number of propositions, wonderfully acute, drawn from Mayow's book, but I forbear. What more remains but that chemists should prepare these things which have been so long foreseen?

Black led the way by the discovery of fixed air in 1754, and the chemical theory of animal heat at once became more precise. Black himself revived the forgotten doctrine of Galen, that the air, by passing through the lungs, undergoes the same changes as by supporting combustion; and he added the important fact that fixed air, or carbonic acid gas,

is evolved. Inflammable air was discovered by Cavendish in 1766; and, lastly, in 1774, Priestley made the greatest discovery, of dephlogisticated air.

Priestley discovered oxygen, but Lavoisier discovered oxidation. In a succession of *Mémoires* which appeared between 1777 and 1789, Lavoisier maintained the following propositions. First, the oxidation which goes on in the body suffices to account for the heat produced, or at least for the greater part of that heat. He put a guinea-pig into a calorimeter, and found that in ten hours the animal had set free heat enough to melt 341 grammes of ice at the freezing point. He collected the carbonic acid expired during the same time, and found that it corresponded to a quantity of carbon which would, by combustion, evolve heat enough to melt 326 grammes of ice: 341 parts of heat were produced, and 326 of these were accounted for by the combustion of carbon. Next, Lavoisier found that of 100 parts of oxygen inspired, only 81 parts were expired in the form of carbonic acid—that is, 19 per cent. of the oxygen is disposed of some other way; and he suggested that it went to the combustion of hydrogen, so as to form water. And in his last *Mémoire* he summed up his doctrines thus: "The animal machine is chiefly governed by three regulators; respiration, which burns carbon and hydrogen, and furnishes heat; transpiration, which increases or diminishes according as it is necessary to get rid of more or less of this heat; and, lastly, digestion, which gives up to the blood as much as it loses by respiration and transpiration."

Lavoisier's theory we still accept as fundamentally true. In the words of Mayer, "the sole source of animal heat is a chemical process, in specie an oxidation process"; that is to say, a process by which complicated substances containing much tension-force are taken up by the body, and other simpler substances containing much less tension-force are cast out: the loss of tension-force being equal to the sum of force which the animal sets free. And this living or free force is, in the case of an animal at rest, of one kind only, namely heat.

You will have anticipated me when I go on to remark that the oxidation-theory is not so simple as at first it seems. Explanations and reservations must be made; to which I now ask your attention. We will discuss in order the following propositions: that the combustion-heats of the compounds burnt in the body are not the same as the combustion-heats of the elements of those compounds; that the oxidation of much of the combustible of the body is incomplete: that oxidation will not account for all the heat which the chemical processes of the body set free.

First: the combustion-heats of the compounds burnt in the body are not the same as the combustion-heats of the elements of those compounds. The body does not burn pure carbon and hydrogen; and the heat yielded by oxidation of any kind of food cannot be estimated from the heat yielded by oxidation of an amount of carbon and hydrogen equal to that contained in the given kind of food. Nor, on the other hand, can the total heat set free be calculated from the quantity of oxygen contained in the final oxidation-products, water, and carbonic acid. Hence the chemical problem becomes extremely complicated; neither is chemistry at present able to solve it. Most organic products are already oxidised, and their oxygen seems not to behave like free oxygen. In like manner, carbon and hydrogen, in composition, afford different amounts of heat in different compounds. These facts have been well illustrated by Berthelot. He found that sixteen grammes of oxygen, employed in converting carbon into carbonic acid, carbonic oxide into carbonic acid, hydrogen into water, and formic acid into carbonic acid and water; he found, I say, that the same quantity of oxygen, employed in each case, afforded respectively 47,000 units of heat, 69,000, 69,000, and 96,000. And, reversing the problem, he found that the same quantity of carbonic acid, produced by complete oxidation of carbon, carbonic oxide, formic acid, and marsh-gas, gave 94,000, 69,000, 96,000, and 210,000 units of heat respectively. For physiological purposes this difficulty has been surmounted by Dr. Frankland, in a paper published in 1866. Dr. Frankland experimentally ascertained the heat evolved by combustion of different kinds of food. He mixed about two grammes of a given substance with nine grammes and a half of chlorate of potash and a little oxide of manganese: he then fired the mixture in a calorimeter. The result of these investigations showed that most heat was evolved by fatty and oily kinds of food; next in order came starchy and saccharine matters; and lastly, muscular flesh. At the top of Dr. Frankland's list stands cod-liver oil, of which one gramme evolves 9,107 units of heat, that is to say, combustion of one gramme of cod-liver oil would suffice to raise more than 9,000 times that weight of water one degree centigrade. Scarcely inferior to cod-liver oil stands the fat of beef. Next come butter and the cacao-bean. Bread, the sundry kinds of meal, rice, and arrow-root form the next series, generating less than half the heat of cod-liver oil—from 4,400 to 3,800 heat-

units. The yolk of egg and sugar stand next. Ham, mackerel, beef, veal, Guinness's stout, and potatoes, evolve from 1,000 to 2,000 heat-units. And last of all come Bass's ale, the white of egg, milk, carrots, and cabbage, rating at not more than one-fifteenth to one-twentieth of the combustion heat of cod-liver oil. All the substances of which I have just spoken were in their natural condition; that is, not dried.

Let us pass to the second reservation required by the oxidation theory: namely, that the oxidation of much of the combustible of the body is incomplete. Complete combustion of nitrogenised compounds results in carbonic acid, water, and free nitrogen. Now, very little of the nitrogen taken in with the food passes off in the form of gas. That is to say, the combustion of nitrogenised matters is incomplete; urea, uric acid, kreatinin, and other substances urinary and faecal, which are still combustible, carry off the nitrogen. And along with the nitrogen they carry off carbon and hydrogen imperfectly consumed. Dr. Frankland has given us information upon this point also. Complete oxidation of one gramme of hippuric acid produces heat equal to 2,280 kilogrammeters, or more than is afforded by dried beef and albumen. Urea gives 934 kilogrammeters; uric acid, 1,108. One-third of the muscle and albumen, taken as food, passes off as urea, imperfectly burnt: an amount of albumen which gives, by complete combustion, say 50 heat units, gives, by combustion in the body, only 42.

Lastly, oxidation will not account for all the heat which the chemical processes of the body set free. This fact has been especially insisted upon by Berthelot. He has referred to the large quantity of heat which hydration sometimes sets free, exemplified by adding water to potash, sulphuric acid, or alcohol; and similar processes may go on in the body. He believes, moreover, that heat is set free when oxygen is absorbed by the red corpuscles of the blood in the pulmonary circulation, a process probably allied to a liquefaction of the gas; for which reason Berthelot estimates the condensation heat of 16 grammes of oxygen at four or five units. A man is said to consume 746 grammes of oxygen in twenty-four hours: this would produce by liquefaction about 200 heat units, equal to 85 kilogrammeters, which after all is a small quantity. The heat evolved when hydrogen is condensed by spongy platinum is an illustration of the case in point. Again, Berthelot has shown that a mere rearrangement of the elements of a compound, without the presence of free oxygen, may generate heat; a fact illustrated by the fermentation of grape-sugar, whereby water, alcohol, and carbonic acid are produced, the carbonic acid escaping in the form of gas. I think that we shall see enough hereafter to make us suspect that some of the chemical processes of the body, whereby carbonic acid is set free, are processes of the same kind. And I will just add that perhaps we may be led to believe that much of the oxygen of the blood is first of all employed in synthesis of the higher animal compounds, which become oxydated indeed, but not thereby burnt, or degraded in their tension force—rather the reverse; the combustion or liberation of tension-force being a subsequent process.

We have now considered all the chemical processes known to go on in the body, by which the tension-force of the ingesta is turned into the living force of heat. I feel that the details, into which latterly I have been compelled to go, may have been wearisome. I hope that this will be less the case with the discussion of a question which now arises—Where do these chemical processes go on? The blood is oxydated in the lungs, and reduced in the rest of the body. Which of these respirations—the pulmonary respiration or the tissue-respiration—is concerned with the production of animal heat? or are both so concerned?

Lavoisier raised the question, but gave not a distinct answer. Yet, throughout his last *mémoire*, it is obviously assumed that carbonic acid is formed both in the lungs and in the general systemic circulation: possibly some carbonic acid, he suggests, is formed in the alimentary canal and introduced into the blood along with the chyle. Spallanzani soon afterwards showed that snails, immersed in nitrogen, continued to exhale carbonic acid; thus affording confirmation of Lavoisier's doctrine concerning the formation of carbonic acid in the body at large as well as in the lungs. Brandis, in an essay upon the *Force of Life*, published in 1795, formulated the Lavoisierian opinion thus: That the animal heat is produced by constant small combustions of the carbonised fibres of the body, and therefore is not produced in the lungs alone; nor are the lungs warmer than the rest of the body; nor, he adds, is heat produced in the blood alone. Blumenbach taught a similar doctrine.

But dissentients were not slow to appear. In 1801, Autenrieth maintained that oxidation went on almost wholly in the lungs; the watery vapour and carbonic acid are formed in the lungs; the heat generated at the same time and place, is distributed by the blood to the rest of the body; the oxygen absorbed by the arterial blood, does, he says, but little. This anti-Lavoisierian doctrine was supposed to be established by the experiments of John Davy in 1814. Davy found the blood in

the carotid artery always warmer than the blood in the jugular vein; he found the left ventricle of the heart always warmer than the right. Becquerel supported Davy; he also showed that the blood in the aorta is warmer than that of the vena cava superior; and from these facts he deduced the more general statement that arterial blood is always hotter than venous blood. The observations were correct, but the generalisation was faulty. As early as 1817, Mayer (not Julius Mayer, but a writer in Meckel's *Archives*) explained that the lower temperature of the blood in the jugular vein was due to its superficiality and feeble current. In 1849, Claude Bernard maintained, what is almost a contradiction of Davy's observation, that the blood which enters the lungs is warmer than the blood in the left side of the heart; a remark tending to show that the lungs cool the blood. But, in 1857, my friend and colleague, Mr. Savory, made some experiments which led him to the conclusion that, when breathing is not interfered with, the blood of the left ventricle is warmer than that of the right. How are these contrary opinions to be reconciled? Apparently by the fact that neither of them is universally true. You will understand that I am now speaking of the heat of the blood on the two sides of the heart. Colin, in 1865, out of a number of experiments, found that twenty times the temperature of the blood within the right and the left heart was equal; thirty times the right was warmer than the left; fifty times the left was warmer than the right. Much depends upon the state of the skin (the chief cooling organ), of the muscles and viscera (great calorific organs). In animals thickly furred, and with large abdominal viscera, the right heart contains hotter blood than the left. The reason of this is clear. Still more recently, in 1868, Jacobson and Bernhardt, experimenting with thermo-electric needles upon living rabbits, found that the left heart was warmer than the right heart by $\frac{1}{2}$ to $\frac{3}{4}$ deg. Fahr.; but the temperature of the heart, and that of the blood in the heart, are not quite the same thing. To sum up: on the whole, there is reason to believe that the blood on the left side of the heart is mostly a little warmer than the blood on the right side. And this is the fact which was depended upon to prove that oxidation and the production of heat go on chiefly in the lungs.

Some opponent of this doctrine has made the poor remark that, if all the animal heat were generated in the lungs, they would be consumed thereby. But Julius Mayer endeavoured to show that, under these circumstances, the blood in the pulmonary veins would not be more than $\frac{1}{2}$ deg. Fahr. hotter than the blood in the pulmonary artery. The data afforded by the most recent physiology would reduce the difference to $\frac{1}{3}$ deg. Let us bear in mind that it is estimated by Volkmann that twenty-one million *grammes* of blood pass through the lungs of an adult man daily. The heat which he would liberate, during the same space of time, would suffice, according to Helmholtz, to raise 2 $\frac{1}{2}$ million *grammes* of water one degree centigrade. So that, supposing the capacity for heat of blood and water to be the same, each systole of the heart would raise the temperature of the blood no more than $\frac{1}{3}$ deg. centigrade, say $\frac{1}{2}$ deg. Fahr. Therefore, there is nothing unreasonable in the supposition that the whole heat of the body might be produced in the lungs, if we assume that the heat is carried off from them as soon as generated.

Anyhow, the formation of heat in the lungs must needs be considerable. The cold air which we inspire is reckoned to carry off from four to eight per cent. of the whole body-heat; to say nothing of the heat lost by the evaporation of water. And yet, as we have seen, the lungs are not cooled by this waste of heat. Heat, then, is set free in the lungs; but how is it set free? I have already referred to the suggestion that the condensation of oxygen in the red corpuscles evolves heat; but we found no reason to believe that the quantity of heat thus produced was anything but very small; and an obvious set-off against this source of heat is the rarefaction of carbonic acid. It is just as hard to believe that oxidation of the lung-tissue can generate much heat. And then there is the friction of the pulmonary circulation, which we may estimate at 44,000 heat-units of a *gramme*. But when all these causes of heat are put together, there remains a deficit which almost compels the conclusion that some chemical change goes on in the pulmonary blood over and above the mere condensation of oxygen.

[To be concluded.]

STATE HONOURS TO MEDICAL MEN.—Dr. Mattei, Professor of Obstetrics at Pisa, Dr. Marcacci, Professor of Pathological Anatomy in the same place, Dr. Fallani, Assistant Clinical Physician of the S. Maria Nuova Hospital at Florence, and Dr. D'Ancona, have been made Knights of the Order of the Crown of Italy. Professor Michelacci, Professor Pellezzari, and Dr. Giacometti (chief of the army sanitary staff), have been promoted to the rank of officer of the same order; and Professor Vannoni of Florence has been made commander.