ORIGINAL COMMUNICATIONS.

PHYSIOLOGICAL STUDIES BEARING ON PRACTICAL SUBJECTS.

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ON THE PROCESS OF CALORIFICATION IN ANIMAL BODIES.

[Read before the Physiological Section of the Medical Society of London, on Monday, January 9th, 1854.]

CAUSES OF THE ANIMAL TEMPERATURE.

EVERY writer of every age who has spoken concerning animal heat, has taken especial pains to observe on the importance of the subject, and to intimate that a full and true explanation of the causes of the animal temperature would throw a flood of light over the most profound and mysterious processes of the living world.

It has been stated in admirable and classical language, by one of the most industrious and ingenious minds that ever entered the field of medical philosophy—by Samuel Metcalfe—that amongst all the earlier nations of the world, of whom we have any record, the calorifying influence, which, emanating from the suns of the various systems of worlds, pervades all the universe, and gives motion and life to everything that moves and lives, was considered as the first of all natural phenomena, and as the grand problem of the world: that from the primitive ideas arising out of a contemplation of the effects of our own solar luminary, sprang forth, clothed in the first instance with the simplest majesty and supremest glory, those mythical representations of the nature, power, and attributes of the Great Ruler of the universe, which even in these latter days cling to the minds of all men who are not stored with the full philosophy and learning of the time.

Moreover, it is known that many of the ancient and great philosophers, descending to the subject of animal heat, and observing how independent it was in some respects of what they understood as external influences, believed that the production and effects of heat in the body were not secondary, as subjects of thought, to the production and effects of heat in the universe; and that this subtle influence pervading the living frame was the animating principle, the *πνεῦμα* or *ψυχή* of Aristotle; the *calor vitalis* of Lucretius, and the *φύσις* of Hippocrates.

From the epochs of time in which these mighty ones flourished to the present day, but few centuries have passed away without leaving on their literary scroll some essay or thought on this interesting subject. And the poets, catching at scientific facts, or inventing scientific allusions for themselves, under the mere influence of genius, have repeatedly referred indirectly to the animal temperature as a sustaining or vivifying influence.

The following expressions, says the author whom I mentioned above, "are not merely metaphorical, but vivid and faithful representations of nature derived from observation and experience. The lamp of life, the glow of health, the warm vigour of youth, the lustre of a beaming eye, the brightness of fancy, the light of reason, the fire of genius, the heat of passion, the chilliness of age, and the coldness of death; with a thousand others, which are not less philosophically correct than poetically beautiful."

The very importance indeed of the various questions connected with animal heat alarmed many of the greatest of the great ancient philosophers. The origin of the living fire, its maintenance, its departure from the body after what is called death, were all matters so mysterious and so intimately linked to subjects of another, though not a higher order, that the mind shrank from the inquiry, and the existence of heat in the body was regarded in much the same doubtful manner as the phenomenon of gravitation is regarded now, as an ultimate fact, unexplained and unexplainable.

Hence, in all probability, arose those indefinite terms

which are well known to have been applied to the animal temperature, as *calidum innatum*, and many others.

I need scarcely say, that in this day a bolder, though not less pure tone of thought prevails: that mystery now means ignorance: and ignorance the conquerable and negative foe of industry, courage, and genius. At the present time, therefore, the causes of the *calidum innatum* may freely be discussed, and even should some physiologist be able to illustrate that in these causes lie the springs and essence of life, the happier that illustration for him and for a world ready and anxious to receive it.

In remarking on the fear with which the ancient physicians approached the subject of animal heat, I wished only to speak in a general sense, for certain it is that there were here and there ancient theorists who dropped many very curious ideas on this matter; I cannot, however, do more in this place than direct all who may wish to know the opinions of the ancients on the animal temperature, to Bostock's *Physiology*, to Krau Boerhaave's treatise, entitled *Perspirator dicta Hippocrati*, and to the chapter on "Life" in Metcalfe's work on *Caloric and its Vital Agencies*.

The point to which I am anxious to call attention now is the cause of the animal temperature.

The heat of the body has at various times been referred to at least three sources:—to a mere mechanical source; to nervous influence; and to chemical combination.

MECHANICAL THEORY. It is difficult in this day to comprehend how the minds of intelligent men could ever have been brought to believe that the temperature of the body was produced by a presumed kind of friction betwixt the particles of the blood and the tissues over and into which they passed. Yet this doctrine really had its advocates and its day; and for more than half a century countless multitudes of squares, circles, lines, and figures, were ever ready to rise in its behalf, and to wed themselves for ever with the offspring of the paper-mill.

It is but fair to the mechanical physiologists to say, that their theory had for its basis the simple and well known fact, that friction brings out the caloric resident in many substances. But on this fact they generalised too largely, and committed a very grand mistake in connecting it by analogical reasoning with the production of heat in the body.

VENOUS THEORY. I agree with Metcalfe in assigning to the illustrious Cullen the invention of the singular hypothesis, that the animal temperature, motion, and many other of the living functions are dependent on a peculiar fluid or force resident and inherent in the nervous centres, and communicated to every part of the body through the medium of the nervous cords.

Cullen, though one of the most accomplished physicians in the history of medicine, seems, from what we learn of him from his friends and contemporaries, to have been a very doubtful physiologist. His great strength consisted in his wonderful power of describing minutely that which he saw before him; his great and dangerous weakness in attempting to fathom and to describe with equal fluency the unknown and invisible,—to theorize, in short. "He was", says one of his most admiring and intelligent pupils, Dr. Rush, "a hotbed of hypothesis"; but in his own speculations he could have had no faith, for he built them up at one minute, and destroyed them the next with as much dexterity as a skittle player sets up his nine-pins for the purpose and pleasure of knocking them down. Moreover, it does not seem that Cullen performed any leading physiological experiments, or that his opinions were based on those principles of inductive inquiry which were first taught and illustrated practically by our own immortal Harvey, and which are in this day falsely called Baconian principles, instead of Harveian.

The particular hypothesis of Cullen as to the cause of the animal heat was, as far as I can ascertain, nothing more than a mere and hasty thought. Yet he stated the hypothesis so decidedly, his words had so much weight, and the notions of Willis and of other physiologists on the uses of the nervous system were at that time making

such a noise in the scientific world, that the Cullenian idea was received almost immediately, by a host of medical writers, as the whole truth and nothing but the truth. From that time forward it had its literary and experimental defenders; and even at this moment there are many persons who subscribe to it without inquiring how it arose, on what proofs it is based, or by what other hypothesis it is opposed.

In opposition to the chimerical and extraordinary opinions of the neuro-physiologists on the generation of animal heat, I beg to offer the following arguments:—

I. That the existence of a nervous fluid or force inherent in the nervous centres is as yet unproven; that the nature of the presumed force cannot be defined by those who are the most earnest believers in its presence and importance; and that it is contrary to true science to describe visible effects as springing from an undefinable and doubtful cause.

II. That if the presence of a nervous fluid or force were proved beyond dispute, there is no obvious arrangement in the animal economy by which it could act as a direct cause of the animal temperature.

III. That there is no relationship whatever between the development of the nervous system in an animal, and the degree of heat which the body of that animal represents.

IV. That if the respiratory functions be in no way interfered with by the operation, the most serious mutilation of the nervous centres may occur, without either lessening or producing any marked variations in the heat of the body.

V. That in some pathological states where the leading functions of the brain are fully suspended, the animal heat is often greatly increased.

VI. That in other conditions, where the nervous centres show no sign of disease, the heat of the system falls, and even fails altogether.

VII. That during such states as syncope and anæsthesia, the return of the most important and marked function of the brain, viz., consciousness, is always secondary to the return of the circulation, respiration, and animal heat; whereas consciousness would be the first to return, and not the last, if the brain were the primary organ that manifested a restoration of function, and if the other organs waited for, and were dependent on, such restoration.

I could multiply these arguments against the nervous influence to a great extent, did time permit, and were such a multiplication required.

CHEMICAL THEORY. During, and long previous to the time in which the mechanical and neuro-physiologists were theorizing on the causes of the animal heat, the idea that that heat had a chemical source, and that the respiratory and circulatory systems were in some way connected with its production, was taking a slow but definite shape. Even some of the ancients had a kind of misty notion that the heat and life of the body was in some mysterious manner dependent on breath and blood. But it was not until the time of our great countryman Black, that the chemical theory obtained a due share of attention, since it was he who first pointed out the remarkable though simple fact, that the products of respiration are identical with the products resulting from the combustion of a hydro-carbonaceous substance.

From the date of that discovery to the present moment, numbers of the most eminent physiologists have advocated and have striven to prove to demonstration the truth of the chemical theory; and although the experiments of Sir Benjamin Brodie, performed in 1811, gave the theory a check, and very much elated the neuro-physiologists, the fallacies attending those experiments were soon explained; and I believe that, at the present hour, the chemical theory is generally recognised as the most feasible explanation yet afforded of the cause of the animal heat.

I do not, however, wish to affirm, though a firm believer in the chemical theory, that its correctness is *unquestionably demonstrated*; for it must be admitted that one or two

more experiments, tending to throw further objective evidence on its side, are required.

The chemical theory is based on numerous facts and observations. The following are the most important:—

I. The discovery of Black, that the products of respiration are the same as the products of the ordinary combustion in oxygen of a hydro-carbon—viz., water and carbonic acid.

II. That the agents necessary for the production of combustion out of the body are equally necessary for the support of heat in the body.

III. That in all animals, *cæteris paribus*, the degree of heat which they represent is in proportion to the size and perfection of their respiratory, circulatory, and digestive systems.

IV. That as the temperature of every animal falls on taking from it the necessary amount of the supporter of combustion or of the combustible agent, so on the other hand the temperature may, to a limited extent, be made to rise by the over administration either of the supporter of combustion or of the combustible matter.

V. That, if from disease or malformation of the circulatory or respiratory organs the free communication betwixt blood and atmosphere is lessened, the temperature of the body at large is reduced.

VI. That all those agents, which out of the body have the property of stopping oxygenation, such as chloroform, hydrocyanic acid, olefiant gas, nitrous oxide, the smoke of the lycopodon giganteum, etc., seem to produce the same results in the body; and that in proportion as they are received into the system, so is the heat of the system brought down.

To these arguments many others might be added; but I would rather make a few observations on some of those given, than bring forward more.

The first argument, viz., that the products of respiration are the same as the products of ordinary combustion, requires but little comment: except to state, that experimentalists are now in some measure able to trace a relationship betwixt the amount of the elements of combustion supplied to the body, the products realised, and the heat evolved.

The third argument, that the temperature of an animal body is in proportion to the size and perfection of its circulating organs, is, I think, abundantly proved by facts. Metcalfe, in the second volume of his work, has several tables bearing on this subject, to which I would direct the attention of all who are interested in it.

Moreover, we do not require to pass in review the special organisations of various classes of animals to have full evidence before us of the truth and importance of the argument I am now supporting. Everyday observation on ourselves and others is alone sufficient. Take any two healthy men; examine the relative development and vigour of their respiratory, circulatory, and digestive organs; and the fact will plainly, and, as I believe, invariably come out, that he who has those organs best developed and most active, will always, under the same thermometric circumstances, make and radiate the larger proportion of caloric. Indeed it seems probable, that the reason why the inhabitants of cold climates bear the transition into hot climates with such indifference, and why the inhabitants of the tropics sustain the cold of a northern land so wretchedly, depends on the development of the chest, heart, and stomach, more than on any other cause.

The Englishman with his broad chest, tonic heart, and large digestive machinery, makes a great amount of caloric, diffuses it freely, and feels none the worse, so long as the medium by which he is surrounded is cold enough to take up all the heat that radiates from his body. But put him on the equator, where there is no such coldness of atmosphere, and the condition is at once changed. He cannot breathe less deeply, for his physical conformation will not allow him to do so; he makes more caloric, therefore, than he requires; and although by the free elimination that succeeds, sometimes from the bowels and kidneys, and always from the skin,

he may manage to bear the climate, he is burning too briskly, and is always in danger of losing his life from fever and congestions, if other physical causes, such as dampness of the atmosphere occur, to prevent the due elimination of his excreted matters—his products of combustion. Hence northern persons, with small chests and indifferent circulation, not unfrequently bear the heat of the tropics much better than the hardy and the robust.

On the other hand, the inhabitants of southern climes, whether animals or men, perish in the north. Their bodies may be elegantly shaped; but they have not such a storehouse of a stomach, such depths of inspiration, such a distributing heart as the Norseman; they cannot supply that irresistible northern robber that surrounds them, the cold atmosphere, with his full demands: their bodies pay, therefore, the forfeit of their physical incapacity—they perish from gradual cold.

In reference to the fourth argument, that the heat of the body is increased or lessened by the addition or abstraction of the elements of combustion, the evidence is of the purest kind. Is not the chilliness arising from starvation, or the withholding of food as much the indication of a declining combustion, as the cooling of the furnace when the fuel is suspended, is an indication of the household fact, that the fire is going out? And is not the cutting off of oxygen from the lungs, simply equivalent to putting an extinguisher on a taper? I need scarcely reverse matters, and refer to the increased heat of the body that follows a new supply of food, or a quickened healthy respiration.

But I wish particularly to dwell on a view which I have more than once expressed, that the mere shortening of the circulatory currents of the body leads almost invariably, and as of necessity, to an increased combustion—by causing an increased flow of blood through the parts in which circulation continues.

The mode of suppression does not, I believe, signify much, so long as it is sufficiently extensive. It may be the tying of an important artery; the suppression of the function of the skin or other excreting organ; or the infliction of a severe wound or injury sufficient to check temporarily the circulation of blood through the affected part. In any case the temperature of the body will, as a general rule, be much increased some time afterwards, and for simple and obvious reasons. For, as I before said, if circulation through one part of the body be suppressed, more blood must circulate through the remaining parts. To take an example; the circulation of the lower limbs being stopped, more blood will of necessity have to circulate in the parts superior to those limbs. Upon this the heart must receive an extra charge of blood; and should this overcharge be enormous, the central circulating organ may be, and indeed sometimes is, paralysed at once. But should the added burthen be slight, and the central organ of the circulation be healthy, it will not long stop or hesitate, but will pour a larger flood of blood into the pulmonic circuit. The pulmonic current will then of necessity be increased, and quickened respiration will result. But an increased pulmonic circulation and increased respiration cannot take place without increased chemical change or oxydation of the blood; under the influence of which the heart will be stimulated more, and the end will be that the whole of the body will give evidence of an increased combustion, which, if not checked by some means, natural or medical, will continue until the blood is so modified that it cannot flow through the circulatory channels, when death must almost inevitably supervene.

That a shortened circulation does give rise to an increased oxydation, and, secondary to that, to the increased combustion known under the name of inflammatory fever, may, I believe, be sufficiently well proved. I might refer to the slight inflammatory disposition, or febrile excitement, as it is called, which, in spite of the blood lost, not unfrequently follows the amputation of a limb, or the infliction of a severe wound.

From the careful observation of many cases which have fallen under my own care, I have reason to believe that

the heated state of body appearing in some kinds of apoplexy of the brain, arises from the cause I now mention. The case of hanging, to which I called attention a few weeks ago, where the symptoms were all of an apoplectic character, and where the brain was found congested to the last degree, is a case in point, and admirably illustrates the views now given in every particular.

In cases of intermittent fever, another good illustration occurs. In these cases, the cold or congestive stage, during which the whole of the internal organs are in an apoplectic condition, and the skin bloodless, is invariably followed by increased combustion, indicated first by the increased temperature of the body, and secondly, by the most profuse elimination. Something strictly analogous will often be seen in hysteria.

It is also known to experimental physiologists, that the tying of several arteries in an animal sets up an increased circulation through the lungs, and inflammatory fever secondarily. When Dr. Home of Edinburgh tried, many years since, to induce menstruation in cases of amenorrhœa, by compressing the femoral arteries, violent dyspœa, flushing of the face and upper parts of the body, and sweating, were marked symptoms, consequent on the compression.

Lastly, as I have briefly hinted in another place, the mere accident of taking cold is an excellent example of an increased combustion of the body, coming on from suppressed circulation.

I do not believe that colds are ever taken from mere exposure of the whole body to a low temperature; for, although the body does certainly perish when surrounded by intense cold, it perishes because the medium by which it is enveloped robs it, by simple conduction, of more caloric than it has the power of generating. It does not perish, then, from taking a cold; but what is called a cold is produced by the body being surrounded by something (by some moist envelope, as a general rule) which prevents, for a great length of time, the eliminatory function of the skin; or by a limited portion of it being exposed to a low temperature. The circulation is thus obstructed over a large surface; more blood must play through the heart and lungs; increased combustion must succeed; and, if some other eliminatory channel does not perform double duty, inflammatory fever is a necessary result. Not uncommonly in these cases, another eliminating organ does perform double duty, and so saves the system. A man, sleeping in a damp bed, feels for a time exceedingly uncomfortable and chilly, and finally obtains relief by frequent calls to micturate, or occasionally by free evacuations from the bowels. Some months since, I designated this increased combustion, arising from suppressed eliminatory function, "*the body consuming its own smoke*". This is a homely term; but it describes well and truly what I would like to convey.

It would be possible to enlarge greatly on the fifth argument in favour of the chemical theory; viz., that if, from disease or malformation of the circulatory or respiratory organs, the free communication betwixt blood and atmosphere is lessened, the temperature of the body at large is diminished. Indeed, the chemical theory of animal heat would stand on very firm ground, if based on this argument alone.

Take, as a first illustration, the simple matter of the decrease of temperature from the abstraction of blood; as a second, the general chilliness of persons whose bronchial tubes are always loaded with a superabundant secretion—asthmatic persons; as a third, the decrease of the animal temperature during sleep, when the heart and the respirations are invariably reduced; and, as a fourth, those cases where, from patency of the foramen ovale, the blood current through the lungs is modified, and where the heat of the body, as an effect of that modification, is constantly below the natural standard. The mind of the pathologist will at once revert to many other illustrations.

In the history of pathology, three or four instances have been met with, in which, during patency of the foramen ovale, the blood current has passed for many years from the left to the right auricle, thus making, I had almost said,

two pulmonary circuits to one systemic, instead of two systemic to one pulmonary, as is usually the case. Such instances have been recorded by Meckel and Corvisart; but the symptoms of the cases are not described by those authors. However, in the *Dublin Journal* for 1848, a similar case is supplied by Dr. Mayne, and all the particulars are stated.

The symptoms presented by the patient were strictly peculiar, and lasted for years. Taking them all in all, they were identical with those which we all feel during intense heat of the weather, when the hot medium surrounding us will not carry off all the caloric which our bodies are prepared to radiate. The patient, says Dr. Mayne, had the greatest dislike to change her position. She suffered from dyspnoea and palpitation of the heart on taking exercise: and one remarkable symptom was *constant sweating*, which, unlike that of hectic, was unceasing; so that her linen was at all times saturated with moisture. She had also but little desire for nourishment. The bowels were torpid, and she seemed as incapable of mental as of bodily exertion. She died suddenly, after taking an unusual amount of exercise.

As cases of cyanosis, where the blood passes from the right to the left side of the heart without being properly arterialised, give evidence of deficient calorification, and afford a negative proof that the amount of caloric generated in the body is in proportion to the free union of blood and atmosphere, the peculiar case related by Dr. Mayne gives affirmative proof of the same fact. The arterial blood, passing twice through the lungs, and diffused through the venous as well as through the arterial system, could not but produce those effects in the system described by Dr. Mayne—effects which all happen when venous blood becomes arterial in character, as it is in the tropics, and even in the temperate zones, when the summer heat is very great. It is unfortunate that Dr. Mayne did not note the temperature of this woman's body; though I do not myself imagine that he would have found it much increased, because the profuse elimination from the skin would equalise the animal temperature, as it does in cases where the body is exposed to extraordinary heat, as in the experiments of Blagden and Fordyce, and in operations attendant on the drying of cloth-prints in the heated chambers of printing establishments.

The last on the list of arguments for the chemical theory of animal heat has reference to *anæsthesia*. To enter fully into this argument, I should be obliged to go over the whole ground of debate, in relation to the *modus operandi* of narcotics in general; and should be forced to refer to Dr. Snow's theory of their *modus operandi*, and to use it as the basis of mine. After so full and able a description of the theory as that which its originator has given at the last Physiological Meeting of the Medical Society, and in his published essays, I feel that but little is required of me. That all the agents which have the property of producing *anæsthesia* have the property also of stopping oxidation, and even of suspending ordinary combustion, must be admitted by every experimentalist, and may be proven by any one who does not grudge the time and labour necessary for making a few simple experiments. It is curious, moreover, that those agents which possess the greatest power in suspending ordinary combustion, exert the quickest influence on the animal body in producing *anæsthesia*.

If, then, these truths are taken, and are coupled with Dr. Snow's carefully proven facts, that, during *anæsthesia*, all the products of respiration, viz., those expired from the lungs, those formed in the blood, and those eliminated by the kidneys, are diminished; and that, in proportion to this diminution—undeniably a sign of decreased chemical action—the temperature of the body invariably falls; if, I say, these facts are all borne in mind, I think the inference is clear and just, that the decreased temperature in the *anæsthetic* state is an effect of decreased chemical action.

When I sat down for the first time to the writing of this paper, I did so with the intention of introducing into it the histories of several experiments relating to the animal temperature which I have been conducting with considerable care during the past three years; but, on entering upon the

task, I soon felt the necessity of discussing the subject first in a general way, as I have now done.

For future occasions, therefore, I reserve many special points, such as the extent of the oxidising process in the production of animal combustion; the union of oxygen with other blood constituents than carbon and hydrogen; the parts of the body in which the chemical changes occur that lead to the evolution of heat; the question of the uses of the animal temperature.—Is it a mere chemical result running off from the body as fast as it is formed; and doing nothing? or is it, in truth, *an animating*, if not *the animating* principle?

These, and indeed many other questions connected with the animal temperature, commend themselves much to all who desire earnestly to see a sound practice of medicine based on a sound physiology. I believe there is no scientific medical subject in which practical, pathological, and physiological observations fuse more kindly than in this; and that, if we could brush away difficulties, and see clearly the full meaning of the *calor vitalis*, we should view with unclouded sight many of the most obscure points in pathology, and derive much solid information on the treatment of every disease.

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LECTURES, DELIVERED AT THE LOCK HOSPITAL, LONDON.

By HENRY LEE, Esq., F.R.C.S., Surgeon to the Hospital.
No. III. (Nov. 22ND, 1853.)

ON THE MEANS BY WHICH THE SYPHILITIC POISON ENTERS THE CONSTITUTION.

PHYSIOLOGISTS have usually recognised three modes by which absorption may take place:—

I. That by which foreign substances find their way directly into the blood through the coats of the blood vessels. This mode of absorption occurs when poisonous substances are applied to an internal vascular and membranous surface, or when they are introduced into a wound, or when they are forced through the epidermis by friction on the surface of the body.

II. The absorption of the chyle from the mucous surface of the intestines by the lacteals. When the chyle is thus absorbed, it has been supposed by many physiologists that various other substances may be taken with it into the system.

III. Absorption by the lymphatics. This is supposed to occur either in the ordinary growth and renovation of the frame, or when parts are removed and not at the same time replaced, as in ulceration.

To these three modes of absorption, by all of which extraneous substances have been supposed to enter the circulation, we may add a fourth means by which the system may be influenced by the action of some kinds of poison:—namely, that in which a direct local effect is produced upon the nerves of a part, and through them upon the brain, (and consequently upon other organs), without the poisonous material being taken into the blood.

Examples of this mode of the action of poisons are afforded by the effects of the juice of the leaves of the aconite, and of the infusion of tobacco, as illustrated in some of Sir B. Brodie's physiological experiments.

In all those instances the action of the poison commences immediately upon its application.

But there are a very remarkable, and, to us, most interesting class of cases in which this does not happen. In these a certain interval must elapse, and a certain morbid process must be gone through before the poisons can produce their specific actions upon the general system. They are, therefore, with peculiar propriety ranked among the morbid poisons.

The deleterious materials of which these essentially consist, or the secretions in which they are contained, may be applied to the living body in any quantity, and for any