XI.—Medical Thermometry.[1]

(*Continued from* vol. xlv, *p.* 441.)

In passing on to review the teachings of animal thermometry, we shall find that in this, as in every other like investigation, the first thing to be done is, to make ourselves thoroughly familiar with the standards of health before we attempt to make an estimate of the fluctuations of disease. The importance of this has been well recognised by a few observers, by Dr. Davy especially; but as yet we lack records of sufficient variety, accuracy, and frequency, to settle all points. Dr. Davy's observations, like those of many of the earlier inquirers, are not altogether trustworthy; in many places, indeed, he is evidently in error. Dr. William Ogle's paper, in the first volume of 'St. George's Reports,' is excellent; and other essays have been published by Jürgensen, in the third volume of the 'D. Archiv. f. Klin. Medicin;' by Drs. Ringer and Stewart, in the 'Proceedings of the Royal Society' for February 11th, 1869; and by Mr. Garrod, in the same for May 13th, 1869.

Many experiments have been made upon the temperatures of those lower animals which have a constant warmth, but these are scarcely available for determining human phenomena. One fact is very clearly known, namely, that in a healthy man the limits of fluctuation, under various conditions, are very narrow, and upon this certainty medical thermometry becomes possible. Men differ a little, however, in this respect, as may be seen by comparing their observations, and by testing them personally. I find, for instance, that the limits of my own fluctuations are closer than those of Dr. Ogle.

It is worthy of remark that bodily weight, as measured in the same full-grown adult from year to year, is very stable. This, like heat, depends upon the regulation of waste and supply, and is independent, as Parkes has shown,[2] of external temperature. The constancy of the bodily temperature is a consequence of the remarkable regulation of evolution and loss of its heat, and is the expression of their difference. It is one example among many of the wonderful equilibration of function in the higher animals. The daily fluctuations follow a definite course, and their limits vary with age. The average maximum of temperature in persons under twenty-five is given by Ringer and Stewart as 99·1, of those over forty as 98·8. It should have been added that temperature rises again in old age, the second

[1] *Das Verhalten der Eigenwärme in Krankheiten.* Von Dr. C. A. Wunderlich, Professor der Klinik an der Universität Leipzig, &c. &c. Leipzig. 1868.

[2] 'Proc. Roy. Soc.,' June 20th, 1867.

childhood. The highest range of daily temperature is maintained between 9 a.m. and 6 p.m. After this time the temperature falls slowly and continuously if no alcohol be taken, but if alcohol be taken with the evening meal the fall is more sudden (Ogle). Otherwise food seems to have little influence upon normal temperatures, except, perhaps, in infants.[1] Sex, race, latitudes, seasons, weather, habits of life, and idiosyncrasies, go for very little in influencing the temperatures of healthy persons, so far as we can tell at present. Nor, again, should such changes as menstruation, pregnancy, and childbed have much influence if normally performed. This is a very important point; it seems that in the latter months of pregnancy there is a slight rise, compensated by a slight fall after delivery, when even in women who subsequently go wrong the temperature is generally normal.[2] The effects of cold and hot applications and the effects of alcohol ought to be ascertained beyond a doubt, but at present our information is rather contradictory. In testing the effects of alcohol we have this difficulty, which intrudes itself into many of these experiments; that is, we can scarcely say in a given case whether the influence seen in the change of temperature is really a normal fluctuation or one indicating the initiation of a morbid state.[3] It seems settled, however, that wine and brandy have at first an influence which depresses temperature. This depression, however, in my own case, is generally followed by a rise, the duration of the fall depending very much upon the dose.

Many elaborate investigations have been made into the comparative temperatures of different parts of the body. That the temperature of the inner body is higher than that of the outer admits of no doubt, and that of the closed cavities is from half a degree to a degree and a half (C.) higher than that of the axilla. This we should expect, for whether heat be produced chiefly in the blood-vessels, as Mayer believes, or chiefly in the organs themselves—and in certain organs especially, as is generally thought—it is still true that production is within, and that loss is on the surface, the blood acting as an equaliser of the whole. The heat set free within by numerous chemical processes—by the re-combinations of aliment in the blood, and by the oxidation of the tissues themselves—must be very great, and is spent on

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[1] The ingestion of food after a long fast may, according to Jürgensen (loc. cit.), raise the temperature half a degree centigrade.

[2] I have just received a letter from Dr. Wiltshire, who has given this subject his special attention. Dr. Wiltshire has never found the temperature of pregnant women above 98° F.; on the contrary, in one or two he found it as low as 96° F.

[3] Take, for instance, Dr. Richardson's admirable observations on the influence of removing a limb.

the surface, partly in radiation, partly in conduction, partly in evaporation, partly in motion.

That the regulation between production and loss should be so complete is but another way of saying that a highly complex organism exists; its existence consists in this balance between function and medium, and complexity is but another word for power of rapid adaptation; but the blood being the chief medium of equalisation, the balance of temperature must depend greatly upon its distribution. In discussing the effects of hot and cold applications upon the whole heat of the body, we must, therefore, take into careful consideration the variations in distribution of the blood. When the superficial vessels are full, loss must be greater; when they are empty, it must be less. Mr. Garrod (loc. cit.) gives a very ingenious instance of this in the cold of one side of the body, caused by the exposure of the other side to a fire. As he points out, vascular tension is lessened, the vessels of the far side dilate with those of the warmed side, and radiation from them increases. Again, while the even distribution of the blood must be an important factor in modifying the temperature of particular parts, so the means of heat dissipation must be considered likewise, for the absence or excess of evaporation, for instance, in any part will modify the temperature as much as a difference in the blood distribution. These considerations explain how hard it is to ascertain exactly the effects of exercise and of warm and cold applications. If, for instance, we take the effects of exercise, we find that a part of the force set free by chemical change is diverted from heat production into mechanical movement; heat loss is also greater, for the blood circulates rapidly over the surface, and is thus more rapidly cooled, and so evaporation from the skin is also more rapid. On the other hand, oxidation is much increased, and the very rapidity of the circulation which, on the one hand, cools the blood, on the other carries more blood-corpuscles into the lungs.

These opposing changes seem so to meet together that exercise produces but little variation in the standard temperature of the healthy man. Brain-work has no effect at all upon temperature, as I have often found in myself; in tropical climates, however, Davy says that brain-work raises the temperature considerably, even so much as 4—5° (F.); but this statement must surely be received with hesitation.

I now pass on to the effects of artificial cold and warmth, a subject of very great importance at this time, when cold applications are put in use for the diminution of fever. Without committing ourselves to the belief that the effects of such applications are the same in health and in disease, yet it is of importance for us, first of all, to know the demeanour of the normal organism under such conditions. The effects of changes in thermal conditions are by no means so simple as they seem, and the more we learn about them the harder does it appear to arrive at any distinct conclusion. In the first place, it is very difficult to isolate the quality of temperature in such applications from other qualities. In cold and warm baths, there is, not only the question of temperature, but also of the water itself; in the atmosphere we have greater difficulties still to contend with—namely, the collateral effects of pressure, of moisture, and of currents and electricity; while the comparative effects of water and atmosphere are again to be determined. Moreover, if we assure ourselves, as we may do, that there is a difference between the effects of cold and warmth of the body, yet when we come to describe the effects of either, in its various degrees, we are perplexed. The effects, for instance, of cold alone are very complex, and vary from one moment to another. Thus the first effect of cold is to reduce bodily temperature, as that of higher degrees is to increase it; but these momentary results are soon balanced by modifications of heat development within; and in the case of cold, the reaction may carry up the temperature beyond the starting point. Warmth, on the contrary, which reduces the cooling process, may, by the reduction of heat development, be followed by a positive depression of bodily temperature. We know, for example, that a cold bath often warms us, while a warm one cools us to a very perceptible extent; and it must be remembered that these reactions may be more violent in morbid than in normal states.

The duration and degree of the artificial conditions are again important elements in the inquiry; applications, moreover, to various parts of the body and to the whole body have different results. Warmth, if less than bloodheat would cool the body quickly, by dilating the superficial capillaries, and allowing rapid conduction, while, at the same time, there is further heat abstraction in the consequent increase of more than one secretion. Cold, on the other hand, suppresses secretion, prevents the rapid conduction of bloodheat by closing the superficial capillaries, and drives the mass of blood inwards upon the heat-producing organs. Hence the reaction, after a short application of cold, is often very great, and is, probably, greater in fever than in health.

The very careful investigations of Leibermeister into the effects of cold baths are well known: the general conclusion from them, and from those of Kernig[1], is that on moderate use

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[1] 'Exper. Beiträge zur Kenntniss der Wärmeregulirung beim Menschen,' Dorpat 1864. See also articles by Weissflog, 'Arch. f. klin. Med.,' ii, 570, and iii, 460.

of cold baths great abstraction of heat corresponds to great heat production, and slight abstraction to slight increase of production. Liebermeister's observations were taken in the well-closed axilla. I have myself made repeated observations on the cold bath, and find that deep long plunges into cold water have no effect in lowering my own temperature. I have tried this during cold weather in baths and rivers, never with a depressing effect, but sometimes with slight resulting elevations on reaction. Applications to exposed parts are more effective. Brown-Séquard found[1] that a hand immersed in cold water lost very much heat, and was long in recovering itself, while the temperature of the whole body was either unchanged or a little elevated. Flowing water, probably, abstracts heat more rapidly than still water. I do not find that cold-sitz baths of ordinary duration affect my own general temperature at all, but in some persons such baths lower the whole temperature even 1·5° C., which depression is followed by a rise to, perhaps, 0·5° beyond the normal. Warm-sitz baths, if of higher temperature than the blood, seem to cause a slight general rise, which, after the bath, is followed by a rapid recovery of equilibrium.

Binz[2] makes the interesting and useful observation that ice applications upon the abdomen cause a decided fall in a thermometer placed under the abdominal wall, the rectum temperature remaining unchanged. The influence of drinks needs further research, in the interest of clinical medicine. I have several times noticed that the favorite prescription of ice for suction is followed by depression of temperature and lowered resistance. In one case of heart disease, with nausea and sickness, the free use of ice was followed by a depression from 37° (C.) to 36·4°, and the general condition of the patient was also lowered.

Such are some of the conclusions to which we are led by investigation of the temperature of healthy persons; many points remain as yet very uncertain, and many statements need revision and extension; still we have gained something like a firm foundation for the consideration of the causes of fluctuation of temperature in disease.

In passing from the observations of thermometric changes in health to the like changes in disease, we are met by an old question, but by one which the thermometer is helping us to answer. What is disease? Modern pathology teaches us by many instances that disease does not consist in evil spirits or essences, in humours of the blood, nor in affections of the solids,

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1 'Journal de Physiol.,' i, 497.

2 'Beob. zur innern Klinik,' 1865, pp, 150—164, quoted by Wunderlich.

but in a loss of equilibrium. The body is an equilibrium mobile, and, like all such systems of forces, it tends, when disturbed, to regain equilibrium. The object of modern pathology is to learn in what ways equilibrium may be disturbed, and of modern medicine to learn how to help the system to right itself. In this inquiry medical thermometry takes a very high, if not the first place. We see by thermometrical records that there is no border line between health and disease; that the same influence affecting a stable individual and an unstable one may set up little disturbance in the former, but may shake the latter so evidently that his state will be called one of disease. Disease is therefore, an evident disturbance of equilibrium, and whether such disturbances in their degrees be called disease or not depends a good deal upon the observer. A certain amount of external disturbance, which amount varies for different individuals, is necessary for the actions and reactions of health; less than this fails to keep the equilibrium mobile in play, more than this throws it off its centre. The thermometer is in a large number of cases the most delicate test we have for variations of functional tension; in those cases, at least, where the loss of balance affects the regularity of nutrition, it gives us a new sense in enabling us to investigate the manner in which disturbance begins and the manner in which it rights itself. Hitherto we have supposed that the external influences we have examined were no more than compatible with normal movements; cold and hot baths, alcohol, exercise, and the like were supposed to act upon healthy organisms, and to exert no more pressure than such organisms could meet. Let us now suppose that these influences are slightly intensified so that their pressure is a little greater than the resistance of the organism, we shall then observe the first trembling of the balance, the first irregularity in the combination of functions, the first stage, that is, in the institution of disease.

The delicacy of the regulating actions of the living organism has been shown by a very telling experiment carried out under the direction of Donders, by two of his pupils. The sympathetic nerve in the neck of a rabbit is cut on both sides with the well-known effect of paralysing the vessels of the ear, and allowing the ears to be charged with an excess of blood. The consequence of this is that cooling goes on from the surfaces of the two ears with abnormal rapidity, and the general temperature of the animal is thereby lowered.[1]

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[1] Compare the well-known experiment of killing an animal by varnishing its skin, death being due to the increased radiation of heat from dilated superficial vessels, so that the animal is rapidly chilled down.

The organism thus affected, and deprived of its normal means of regulating the radiation from the ears by and through the sympathetic nerves, meets it by an increased tissue waste, and the animal recovers its normal temperature at the cost of a definite loss of weight. But the new position of equilibrium is less stable than the normal one, and consequently the animal has less power of rapid adaptation to external changes. Changes, therefore, which would have affected its temperatures momentarily and secretly, when the sympathetic was intact, are now met less promptly, and the thermometer shows their influence in larger curves of deviation. Here we find ourselves carried by insensible gradations from health into disease, the normal balance of the system is deranged, and impressions from without are met with less resistance. In health thermometrical variations are limited and constant, in disease they are inconstant and excessive, because the system cannot adapt itself to incident forces with its former readiness. The study of these marches, this no-man's-land which lies between that which on one side is certainly health, and on the other as certainly disease—is full of interest and of practical value. The variations of the normal curve of health set up by drugs are most important and suggestive in therapeutics, and the variations of the curve caused by excessive external impressions are equally important and suggestive on the side of ætiology. Many impressions, again, which cause no direct disturbance of the healthy curve, set up considerable disturbance indirectly. Prolonged and severe exercise causes in myself no change whatever in temperature, unless it be unduly prolonged, when a rise takes place, not immediately, but on the evening following the exertion. So it was with me again, after exposure to a severe heat. Happening one day to be on the premises of Messrs. Fairbairn and Company, I discovered a large oven for heating sand. I had no means of telling its temperature, but I was assured by bystanders, who knew less than I did about the heat regulations of the human body, that no one could remain in it. I fancy the temperature was not more than 300° Fahr. at most, and after the first half minute of immersion I found the air not intolerable. I staid in it for three minutes without moving my temperature a tenth either way, but five hours after, in the evening, my temperature rose, and I was able to detect in myself a mild sub-remittent fever-curve for the four following days. I had "caught no cold," for on leaving the oven I put on a thick over-coat, and returned home in a closed carriage. These secondary fevers following severe strains upon the regulation, but strains no more than were met at the time, are very curious, and scarcely explicable, except on the supposition of an excessive drain upon the nervous reservoirs. This brings us face to face with one of the chief physiological questions of the day. How far is the nervous system directly concerned in the development of heat? That the nervous system, by means of its vasomotor filaments, exercises a constant control over the contents of the arterioles, and thus over the radiation of heat from the skin, and over its formation inside the body, is well known and sufficiently intelligible. But is there anything more than this? Have the nervous centres the power of giving off energy directly as heat, instead of giving it off as movement, or, to put it otherwise, can the nerves, in connection with the intimate parts of tissues, exercise actively or passively such a dissolving or relaxing influence upon the tissues, as to increase heat in proportion to an excessive waste? Sir Benjamin Brodie's strong opinion that the source of animal heat was to be found in the nervous system is well known, and although his views and his modes of expressing them are not such as to fall in with more recent ways of thinking, yet we may admit that the convictions of so able an observer are not without significance. Dr. Parkes, in his definition of fever, directly refers the heat to increased tissue change, and the increased tissue change to the action of the nervous system. It certainly seems clear, as nutrition goes on in animals which have little or no nervous system, that there must be in the world nutrition and oscillations of nutrition which are not dependent upon that system. On the other hand, it may be, and it probably is, a condition of farther complexity, that particular parts should yield up their autonomy to some central regulating power, or combination for mutual advantage would be impossible.

When we meet with excessive temperatures in disease, we ask ourselves two questions; first, is the excessive temperature in the particular case an evidence of lessened molecular stability throughout the body; or, on the other hand, is it the evidence only of an abnormal distribution of blood? For example, in the flushed cheek which follows an injury to the cervical sympathetic we have only to do with an increased conduction consequent upon an increased exposure of blood on the surface.

But in ague, or in hectic chills, we find a rising thermometer coincident with a recession of blood from the surface, so that in this case we have to seek a different explanation. Or if we apply a thermometer to an inflamed knee-joint, we may be at a loss to know how much of the increase of temperature to attribute to afflux of blood, and how much, on the other hand, to an abnormal rate of tissue combustion.

Again, in cases of hæmorrhage into the mesocephalon, we commonly find increased disengagement of heat on the palsied side for many days or weeks, while the axilla of the other side appears rather, as we should expect, to be cooled down a little below the normal standard. A like result may be obtained by division of the lumbo-sacral plexus or of the brachial plexus, which is followed by increase of temperature in the respective limbs.

Or, as both Bernard and Schiff have proved, by section of one lateral half of the spinal cord in the dorsal region, we may raise the temperature of the leg on the same side, while that of the opposite limb is lowered. Schiff explains this latter phenomenon by supposing that section of one half of the cord sets up irritation in the other half. All these changes seem to be due only to variations in the distribution of the blood, and not at all to excessive tissue combustion; the effect of section of a nerve upon parts beyond it must be paralytic, and as this would directly cause lessened tissue activity, we explain the increased heat by remembering that this lessened tissue activity affects, among other parts, the contractile tissues of the arterioles, so that a dilatation of these ensues, with corresponding influx of blood. Irritation, on the other hand, if moderate, by causing contraction of the arterioles, diminishes blood supply, and so by loss of radiation makes sensible temperature lower. In sleep, therefore, when the great centres are at rest, and the arterioles thus at liberty, radiation increases, while the opposite states of mania or delirium, when the centres are discharging, and the arterioles therefore contracted, radiation is much diminished, and prolonged external cold is borne with impunity.[1] These instances of inequalities of distribution with ineffectual "regulation," fail, however, to explain those rises of temperature which exceed the common standard of the innermost parts. Rises of temperature which exceed 38° (C.) can scarcely be explained on the supposition of mere "vasomotor" palsies, but must be due to increased liberation of heat from the tissues. In hemiplegia, indeed, I have never found an increase of temperature to persist at a level of more than 1° (C.) above the other side, and as this can be accounted for by a paralysis of the arterioles, if we are sure that such a paralysis exists in their muscles, as well as in the voluntary muscles,[2] we need not assume that there is any excessive tissue combustion; indeed it has been shown by several independent observers that vascular palsy is, if anything, unfavorable to active combustion. Fever set up in animals in which paralysis of a region of vessels has been induced, has less effect upon the thermometer in such a region than in corresponding

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[1] The dry harsh skin of the maniac is well known.

[2] Bernard, indeed, believes that in hyperæmia of the ear, after division of the cervical sympathetic, the increased heat is due in some measure to increased tissue change. But he seems to me to base this belief upon insufficient grounds.

regions elsewhere. I cannot but think, therefore, that Dr. Handfield Jones,[1] and other writers who hold his views, are attributing too much to the vasomotor nerves and centres when they look to them as the means of any elevation of temperature above 38° (C.). I have had the advantage of some discussion on this subject with Dr. Jones, and I find that he is quite disposed to regard certain possible vasomotor centres in the encephalon as sufficient for the liberation of such high degrees of heat as we see in sunstroke and in acute rheumatism, a liberation which not unfrequently reaches 41·5° or 42°. But such temperatures as these seem to me to be far beyond any known results of the section of vasal nerve branches.[2]

Another hypothesis, which is, perhaps, an idol of the chemical cave, is, that although mere tides of blood are not sufficient to account for any large fluctuations in the heat-curve, nevertheless an increased tide of arterial blood must be followed by an increased oxidation of tissue, and thus by a great increase of temperature. This phase so often repeated, and evidently regarded as the most transparent of explanations, seems to me to be without meaning. Let us look at it more closely. If an increased tide of fresh blood passing into dilated channels is a cause of increased oxidation of tissue, it cannot be that the tissue remains as before. New activities can only be commensurate with new molecular aggregations, and we must expect to see one of two things, either that the tissues grow, or that they are disintegrated.[3] Now in abnormal tissue growth we are accustomed to see not only increased blood-tide but also increased work, and we regard the blood supply not as a cause but as a consequence. Tissues, like horses, may be supplied with fluids but cannot be made to drink; and in any case we should expect, from such a process as this, to find tension increased and heat made latent rather than manifest. The alternative notion is that the increased supply of oxygen may disintegrate the tissues and thus evolve heat. This is not only contrary to what we know of the action of oxygen upon normal living tissues, but it is also unseen in the tissues of the rabbit's ear and other parts when flooded with blood. All experimenters are agreed that no breaking down occurs in such parts unless a wound be inflicted. In all probability, therefore, the activity of the tissues is just as much or as little affected by the over supply of blood as the

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[1] For an interesting treatment of these inquiries, see Professor Rolleston, 'Quart. Journ. of Science,' April, 1870, and Dr. Handfield Jones' excellent volume on Nervous Diseases, lately republished.

[2] A most interesting case on the converse side of the question was lately read by Dr. Greenhow, at the Clinical Society. In this case the temperature fell as low as 84° F., coincidently with atrophy of the encephalon.

[3] Mere isomeric changes can scarcely be accompanied by much heat liberation.

appetite of the whole man is affected by the serving of four legs of mutton upon his table in place of one.

Another argument against the sufficiency of vascular palsy to increase the disengagement of heat may, I think, be derived from the daily experience of all of us, who constantly meet with extreme instances of vascular palsy, apparently of central origin, in neuralgic and like conditions, but without the concurrence of that important rise in axillary temperatures which we see in inflammation.

Observation seems to lead to some such solution as this of the problem of heat fluctuations, and of the allied problem of the trophic influence of nerves, namely, that each part, in taking its place as a member of a system, for this benefit gives up its own independence and enters into a fellowship with the remaining parts; as each part then becomes more and more special the more must it depend upon the supplementary functions of other parts, and upon the integrity of its relations with those other parts. Such a combination becomes possible by means of the nervous system, the centres of which act as banks to meet and to equalise demands. In such a composite body no part is sufficient for itself, but by entering into a community of parts it gains this, that in case of extraordinary demand it is backed up by the rest of the system. Hence we have a chronometric arrangement of functions, one organ or set of organs retiring into rest while another or others are in activity.[1] This periodicity and its manifestations in the nervous system are familiar enough. Cut a part away from communication with the rest, so as to throw it upon its own unaided resources, and it fails, as we see in numerous instances of local deterioration dependent upon section of a nerve trunk. I cannot now, of course, carry out these views of the Equilibration of Function in detail, but I have said enough to show how the nervous system in a complex animal gains an almost absolute command over the activities of all and sundry of its parts, and so becomes the equaliser of tension and a reservoir of force, like the fly-wheel of an engine. To say this is to say that the nervous system commands the liberation of heat. A part which of itself would be unable to resist a certain external influence[2] is enabled to do so with the additional force indirectly given to it by the rest of the body through the mediation of the nerves and the ganglia, or centres of tension. This prompt support we call the regulating action of the organism.

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[1] To put my meaning otherwise, all organs have a rhythm of activity and rest. During activity waste predominates, and tension is lessened in them and in the associated nerve centres. During rest repair predominates, and tension is increased in the organs and in the centres.

[2] We do find, in fact, that with increase of complexity we have increased power of adaptation, and in low animals the power of resisting external changes is therefore comparatively small.

Let the external influence be excessive, that is to say, let it be an injury, and although all other organs fall into abeyance during the abnormal demand at the point affected, though we cease for the time to think, walk, digest, or make our ordinary secretions, nevertheless tension is soon lessened and molecular vibration increased throughout the system, and we see a liberation of energy as heat. Such, as it appears to me, is the part played by the nervous system in heat production, a part which is acquired rather than essential, but which in complex animals has gained pre-eminence. In considering these views we must also bear in mind the following propositions, to which I hope the reader will assent, namely, that the potency of a nerve depends only upon its central connections, and that the quality of a nerve depends only upon its peripheral connections. The old terms "motor nerve" and "sensory nerve" are open to this objection, that they seem to signify some difference of endowment in the nerves themselves, whereas the difference is only one of distribution; and it is much to be regretted that more terms of the same deceptive kind are creeping into use, such as the terms "trophic nerve," "inhibitory nerve," and the like. Every nerve is probably trophic, for it is the medium by which molecular balance is preserved between the ganglia or force reservoirs and the tissue-elements, to which it is distributed. Its effects upon a cell are of the same order as those of external impressions upon the system; there may be deficient stimulus, or there may be healthy balance of action and reaction, or, again, there may be irritation with molecular disintegration. In this latter case an abnormal disengagement of heat is to be found.

The interesting thermo-electric observations of Schiff in course of publication in the 'Archives de Physiologie Normale et Pathologique,' if hereafter established, will prove how constantly an impression upon the terminations of afferent nerves is followed by disengagement of heat in the centres, and it is probable that an abnormal disturbance of the centres would be seen in abnormal disengagement of heat at the periphery. If we can conceive any cause acting upon the nerve-centres so as to destroy their tension, the resulting liberation of energy may as well be converted into heat as into motion. Intermittent fever, for example, as a periodic discharge of tension with disengagement of heat, may be a true parallel to epilepsy, which is a periodic manifestation of a discharge of tension in the form of motion. It is a curious and interesting fact that recent observations lead us to the medulla oblongata as the centre of gravity, so to speak, in both cases. This centre, which seems to be the knot of life in more senses than one, is the point about which the manifold parts of the organism are centred. Here the nerves coming down from the head and special senses meet the great strands coming up from the trunk and limbs, and here they both are put into connection with the lungs, heart, and stomach. About this point their functions seem to revolve, and it is remarkable that as Schroeder van der Kolk, like Dr. Nothnagel and many recent writers, found a centre of convulsion in the upper medulla, so likewise the observations of Professor Heidenhain lead him to the medulla as the centre of pyrexial disturbance.[1] The passage of blood at different temperatures through the medulla probably exercises a modifying influence over the root of the vagus, and thus over the action of the heart; but this supposition has not explaining power enough to solve the phenomenon of pyrexia.[2] It would seem rather that changes in the medulla, radiating in several or in all directions throughout the body, influence in some way the tension of the tissues by way of the nerves; in states of its activity increasing their tension and concealing heat, in states of its paresis losing hold upon them and liberating heat. Thus any expenditure of force at any point is first met by the derivation of force from all other parts, which accordingly subside for a corresponding period. When the expense, however, becomes greater than can be met even by this coalition, the tension of the nervous reservoirs is lessened, the tension of all tissues throughout the body is likewise lessened, and they disintegrate with liberation of energy as heat.

In the case of specific fevers, we seem rather to have an influence acting directly upon the medulla in the first place, and lowering its tension. This is followed by slackened tissue tension throughout the body with disengagement of heat, or, as often in children, with a discharge of tension in the form of motion or muscular convulsion.

Postscript.—To return to a point which I omitted when discussing the technical details of thermometry, I may say a word concerning the trouble and expense which are caused by the disappearance of the index in pocket instruments. To prevent this, the natural resource seemed to be to narrow the neck of the instrument, and this was done about two years ago by Mr. Reynolds in several instruments. The disadvantages of this device, however, more than compensated the imperfect protection it gives. Fortunately, another plan has been now adopted by Mr. Reynolds, by which the index, if lost, can easily be restored by anyone without interfering with the accuracy of the indications. The plan is a very simple one, and consists in the retention of Prof. Phillips' chamber at the upper end of the instrument.

T. Clifford Allbutt.

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[1] '*Vide* Tageblatt der Versammlung Deutscher Naturforscher in Innsbruck,' 1869. Published by Wagner, Innsbruck.

[2] *Vide* Dr. Rutherford's admirable article, 'Journal of Anatomy,' 1868-9, p. 402.