ORIGINAL COMMUNICATIONS.

ON THE SPECIAL FUNCTION OF THE SKIN.

By R. WILLIS, M.D.

[Read before the Physiological Section of the Medical Society of London, May 8th, 1854.]

THERE is perhaps no function in the living body which owes less to the labours of the physiologist, than that of the extensive and highly organised structure designated the skin. The *anatomy* of the common integument is probably exhausted; its *rational physiology* has still almost to be begun. Yet that the proper action of the skin is most intimately connected with health, is universally admitted; and there are not wanting many isolated facts and experiments which go to prove that it is little less immediately necessary to general vital manifestation than the function of the lungs. Still, we are in signal ignorance of the nature of the act in the economy which the skin performs; and the best intellects in our profession have hitherto shrunk from the task of explaining in what way the elimination of a considerable quantity of water, holding in solution infinitesimal proportions of common salts and gases, is indispensable to health and life. One of the last and best authorities in physiology (Joannes Müller) avows that "the object of the cutaneous exhalation is not elucidated by its analysis"; and more impressively still, the great Berzelius declares, "That the elimination of the small quantity of solid matter which the sweat contains cannot be the end of the function of the skin." He continues: "The perspiration has been said to regulate or depress the temperature of the body when raised by violent exercise, or any excessive heat of the surrounding air. But the intimate connexion that exists between the function of the skin and the bodily health, proclaims it to have been instituted for yet another purpose, the nature of which is unknown to us." It is now more than ten years since I attempted a solution of this physiological problem; and as all my subsequent inquiries have only confirmed me in the conclusions at which I then arrived, I gladly avail myself of the opportunity afforded by the Physiological Section of the Medical Society of London to revive a subject which I cannot help regarding as both interesting and important, and now present it for the thoughtful consideration of the present meeting.

Water, it is well known, is the principal constituent of the cutaneous exhalation; sweat is composed of from $98\frac{1}{2}$ to $99\frac{1}{2}$ parts of pure water, and from $1\frac{1}{2}$ to a poor $\frac{1}{2}$ per cent. of solid ingredients, these consisting in principal part of common salt and epithelial scales. Nature, so truly economical in all her processes, we may be well assured, would not waste 99 in 100 parts of even so cheap a fluid as simple water to wash away a solitary unit of inoffensive matter; and we accordingly find physiologists in the later years of the last century, under the influence of the brilliant discovery and admirable industry of the immortal Black, arriving at what has almost up to the present hour been held a satisfactory solution of the fact, that there occurred an extraordinary elimination of simple water by the skin. The end and object of the cutaneous exhalation, it was said, was, by means of evaporation from the general surface, to regulate the temperature of the animal body.

Now, it is quite certain, that this idea has been taken up in far too wide an acceptance; and that later, more carefully conducted experiments on animal heat do not bear out the authors of the earlier series in the conclusions to which they came. The conclusion sanctioned by the experiments of Fordyce, Blagden, Solander, and Banks, is that a human being or an animal may remain for almost any length of time in an atmosphere raised to twice the heat of the body, without injury to health, and without the acquisition of a single degree of temperature.

But our countryman, Dr. Crawford, and the Messrs. Delaroché and Berger, of Paris, whose experiments have some-

times been quoted as supporting these absurd conclusions, are, without a single exception, directly opposed to them. In their experiments in the heated chamber, the French physiologists found that the health of the animals which were their subjects was speedily and very seriously affected; that few of them withstood exposure to its atmosphere for an hour without being reduced to extremity; and that, when taken out either in a dying state, or at the moment of their death, far from having retained their normal temperature, they had a temperature from $11\frac{1}{2}^{\circ}$ to $15\frac{1}{2}^{\circ}$ F. higher than it had been on their entrance. Nor was the case different when man was the subject of experiment, and inferior degrees of temperature were tried. M. Delaroché's internal temperature on entering the heated chamber on one occasion was found to be $29\frac{1}{2}^{\circ}$ R.; on quitting it, after a stay of no more than eight minutes, his internal temperature indicated as many as $33\frac{1}{2}^{\circ}$ R.; so that in this very short space of time, the temperature of the internal parts of M. Delaroché's body had actually gained 4° Reaumur, equal to 9° of Fahrenheit's scale.

These experiments then prove that the commonly received opinion of the great power possessed by the animal body, in virtue of evaporation from the general surface, of maintaining its standard temperature in the midst of an atmosphere by many degrees higher than its own, is unfounded. Nor is the case different when the effects of exposure to much lower temperatures than those employed by the French physiologists are made the subject of inquiry.

In his voyage from England to Ceylon, Dr. John Davy found that the temperature of his companions rose gradually as warmer latitudes were gained; and, on arrival at their destination, that it was from 27° F. to 36° F. higher than it had been on leaving the shores of Great Britain. More than this, the temperature of the lower animals at all events (and I believe it is not otherwise with man), is influenced by the seasons of the year.

The excellent observer I have just quoted, found the temperature of the sheep to be from 1° to 2° , and even 3° F. higher in summer than in winter. And Dr. Edwards ascertained that whilst in the month of February the mean temperature of a number of house-sparrows was 105° F., in April it was 108° F., and in July as high as 111° F.; a difference of six degrees Fahrenheit between the coldest and the hottest months of the year.

It is much to be regretted that there are no corresponding observations on the temperature of the human subject. But we have now such confidence in the uniformity of the physical laws which influence the organic world, that it can be predicated with certainty that the temperature of the body of man also rises and falls with the rise and fall in the temperature of the seasons. Man, like everything else, is fashioned in harmony with the varying circumstances that surround him as the denizen of every region of the earth; and if he have the wonderful attribute of preserving his temperature nearly at the same degree under the equator and at the pole, we can now say assuredly that he does so in virtue of something very different from the greater or less activity of the exhaling function of his skin. The evolution of heat in the animal body is not, in fact, accomplished otherwise than it is in our stoves and furnaces—it is the consequence of the direct combination between the air of the atmosphere and the hydrocarbonaceous or carbonaceous matters, which in the one case constitute the food, in the other the fuel; and just as the heat of a furnace is great in the ratio of the strength and continuity of the blast, so is the temperature of an animal inseparably connected with the energy of its respiration. Unprotected by clothing, at rest, and feeding upon roots and fruit and grain, the power of the human body to maintain a temperature of 98° F. appears to be in relation with an air temperature of from 80° to 90° F.; in temperate regions, in a state of repose, and living on a mixed diet of animal and vegetable substances, we must still be well clothed if we would feel comfortable under any temperature of the atmosphere short of 64° or 65° F. In the frigid zone, again, we must be shrouded in felt and fur, and be extraordinarily

supplied with animal food—the fatter the better—if we would maintain our temperature at its proper pitch. Now, all this is accomplished by no regulating faculty inherent in the general cutaneous integument, but simply by the intensity and completeness of the act of respiration.

Dr. Crawford, in his admirable work on animal heat, long ago observed that “the heating and cooling powers in the animal body were adjusted to each other in such a manner as to produce an equal effect,” and that “the law by which animals maintain a uniform temperature, notwithstanding the variations in the heat of the surrounding air, is similar to that by which the temperatures of bodies become fixed when they have arrived at the melting or consolidating points”. Hence, the power which animals possess of generating heat is in all cases proportioned to the demand. It is increased by the winter's cold—diminished by the summer's heat. “And if animal heat depends upon a change which the air undergoes in the lungs,” as all the facts of modern physiology assure us it does, “it is evident that, all other circumstances being equal, the greater the quantity of air which is changed in a given time, the greater must be the heat produced.”

There seems to me even something absurd in the popular physiological idea of a particular system existing for the purpose of dissipating heat—heat which is the indispensable agent, if it be not perhaps the immediate cause, of every manifestation of vitality, and which it is so obviously one principal business of all the efforts of man over so large a portion of the earth's surface to economise and maintain.

To say that the cutaneous exhalation exists for the purpose of cooling the body is surely to mistake an effect for a cause, to give a preference to a minor over a major consequence. Heat is lost indeed to a great extent from the surface of the living body through the action of the skin; but the loss here is not and cannot be the end. Within the tropics, such a refrigerating faculty of the skin might possibly occasionally become useful; but, wherever man feels the necessity of clothing himself, it would as obviously be useless; in temperate and cold climates, indeed, such a faculty is not only not required, but is even sedulously guarded against. Nevertheless, the function of the skin is indispensable to life in every climate of the globe—at the arctic circle, as much as under the equator.

And here a very important question presents itself. If the skin be the grand regulator of the temperature of the body, what is the effect of a partial or more general suppression of the perspiration, sensible and insensible? The effect ought certainly, if generally accredited notions are correct, to be a rise in the temperature of the body in proportion to the more or less complete suppression of the cutaneous exhalation. But what is the fact? The very opposite of this conclusion. M. Fourcault, in the course of certain experiments on the effects of covering the body of an animal with an impervious glaze or varnish, found that in every instance there was a great and obvious implication of the health; and that, if the application of the glaze was tolerably complete, the animal even perished after the lapse of a few hours. These experiments of M. Fourcault were repeated by the Messrs. Becquerel and Breschet; and they observed that the suppression of the cutaneous exhalation was accompanied, not by a *rise*, but by a *signal fall*, in the temperature of the animal which was the subject of experiment; and this, not only as regarded the surface, but the deeper parts also—a fall that went on increasing continually from the commencement of the experiment until life was extinct, when the body was found to be but a few degrees (centigrade) higher in temperature than the surrounding air. Thus, in one experiment, a rabbit, whose natural temperature was 38° cent., covered with an impervious varnish, fell in half an hour to 32.6° c., and in an hour to 24.6° c. In another case, from 38.6°, the temperature fell successively through all the intermediate degrees to 26.6° in an hour and a half, and, in the next hour and half, to 20° c., when the creature expired, the air temperature at the moment marking 17.6° c.

These are remarkable facts, generally ignored, but irrefragable, and admirably calculated to lead to a revision of accredited notions on the function of the skin.

The grand product of the cutaneous exhalation is water. The gases, salts, and epithelial scales of the perspiration, are virtually adventitious: they are not essential, and their elimination is insignificant as concerns the economy. I shall now proceed to show in what way the elimination of thirty ounces more or less of pure water by the skin in the course of the twenty-four hours is essential to life. We have just seen that the suppression of the cutaneous exhalation in the lower animal was speedily followed by its death.

The old ideas in regard to nutrition and secretion, as effected by open-mouthed or terminal capillary vessels, are no longer maintained: there is no such thing as an open-mouthed capillary vessel; every capillary artery ends in a capillary vein. But the blood still contained within its conducting channels is obviously inadequate to supply appropriate nutrient matter to the various organs and tissues of the body. In order to become immediately available to the living organism, its plasma or finer and more perfectly elaborated portions must transude the walls of the containing vessels; and nutrition is finally effected by an elective attraction between the parenchyma of the body, and the appropriate matter dissolved in the plasma. There is then the freest transudation through the minute arterial parietes; but how or by what means does the fluid thus incessantly transuded, after having bathed the tissues of the body, and probably become loaded with their effete elements, find its way back into the circulation? That it should do so is obviously even as necessary as that it should escape. The answer generally given to the question just asked is this: The exuded fluids return into the circulation; they are absorbed by the veins, in virtue of the law of heterogeneous attraction or endosmose. And this is certainly true. But the conditions of endosmose, in the sense in which that remarkable phenomenon is usually understood, are the mediate contact of fluids having different degrees of density; and no means have yet been assigned by physiologists whereby a greater degree of density should be secured in the returning current than in the outgoing current of the circulating fluid. This means, however, I find in the action of two great systems—the sudoriparous glandular system of the skin, and the general lymphatic system of the body, the end and import of both of which I regard as subservient to securing the conditions that are necessary to the return into the venous circulation of the fluids that have been shed from the arteries for the purposes of nutrition, secretion, and vital endowment.

Reasoning *à priori*, we should have said that the blood returning to the heart must be more dense than that which is leaving it, by the whole amount of watery fluid lost or abstracted in the course of the peripheral circulation. That which is true of the whole must also be true of the several parts: the venous currents of secreting organs, the products of which are essentially watery, must be more dense than their arterial currents. The blood of the emulgent vein, for example, must be denser than that of the emulgent artery, by the whole amount of water (less the saline and other solid ingredients of the urine) which is taken from it in its passage through the kidney. In like manner, the blood which circulates in the peripheral parts of the body must become more dense in the course of its circuit by the entire quantity of mere water which is removed from it by the extensive system of sudoriparous glands, a quantity which has been ascertained to amount to upwards of thirty ounces per diem.

On this important matter, however, we have not to rely on reasoning or inference—even of the most satisfactory kind: the greater density of the venous than of the arterial blood is now generally acknowledged by physiological inquirers, and may be set down as an absolute fact. This, indeed, the researches of Dr. John Davy alone would suffice to establish; but when we see his conclusions borne out and extended by the independent inquiries of Nasse, Simon of

Berlin, and Lecanu, there is no longer room for doubt on the subject. Taking the mean of all Dr. Davy's experiments, the specific gravity of venous blood is ascertained to be 1.053, that of arterial blood but 1.050. The specific gravity of the serum of venous blood, again, he found in the mean to be 1.026, that of the serum of arterial blood 1.022. In repeating these inquiries into the specific gravity of the different kinds of blood, I very constantly found the serum of venous blood of somewhat higher density than that of arterial blood; and even when, through imperfection of apparatus, the difference between the two seemed very slight, or was even inappreciable, I still found, on putting the two kinds of serum into an apparatus for showing endosmose, that a current of considerable force was established from the arterial to the venous side. And this is the process, in fact, which, as carried on between the fluids that transude the arteries, for the purpose of nutrition and vital endowment, and the contents of the veins in the living body, constitutes venous absorption; the condition indispensable to this, viz., the higher density of the blood in the veins or returning channels than in the arteries or efferent vessels in all the peripheral parts of the body, being mainly due to the influence of the sudoriparous glands distributed through the texture of the common integument. This view of the function of the skin will be seen to harmonise perfectly with the fact that the grand product of the action of the skin is simple water. It also explains satisfactorily the fatal effects of the entire suppression of the cutaneous transpiration, as in the experiments of Fourcault and of Becquerel and Breschet. Finally, it sheds a new light on the cause of the great amount of disturbance in the general health that follows every even partial interference with this truly vital function.

It would have been easy for me to have greatly extended this communication, and to have shown the views it involves in a variety of practical applications; but I have purposely avoided doing more than exposing an *essential idea* which has now for many years possessed my mind, and directing the attention of the intelligent members of the Medical Society of London to a subject which I feel assured is pregnant with much that is of the highest importance in the theory and practice of medicine.

Barnes, May 1854.

ON THE TREATMENT OF STRANGULATED HERNIA AFTER OPERATION.

By EDWIN MORRIS, M.D., F.R.C.S., Surgeon to the Spalding Union Infirmary.

[Read at the Annual Meeting of the Midland Branch of the Provincial Medical and Surgical Association at Lincoln, June 1, 1854.]

ALTHOUGH the provincial surgeon is not often, I am happy to say, called upon to perform the operation for the relief of strangulated hernia, it behoves us all to be well acquainted with the necessary measures and caution to be taken for the safe reduction of the incarcerated bowel when the painful necessity occurs, and to be thoroughly conversant with the most approved and judicious plan of treatment after the operation itself has been successfully done. It is not my intention to detail the operative procedure, or to enumerate the anatomical difficulties that may arise; inasmuch as such a proceeding would be presumptuous in me, and a waste of valuable time. I propose therefore to take the management of a case of hernia immediately after operation. It will be necessary therefore to give briefly the treatment which others have formerly recommended, before explaining that of some modern surgeons and my own. The circumstances that I have devoted a great deal of my time to the consideration of this all-important subject, and also that I have had some experience, more than usually falls to the lot of the country surgeon, must be my apology for introducing this interesting point to your notice.

Upon referring to the writings of the late Sir A. Cooper,

than whom a greater authority on this subject does not exist, I find that he says in his *Lectures on the Principles and Practice of Surgery*, "In five or six hours after the operation, give a little sulphate of magnesia or castor-oil. When a patient has two or three motions, the surgeon thinks he will do well; but the danger is not over, and it will be necessary to keep up a free discharge from the bowels by opening medicines, or the patient will die. The history of a case is generally this: in four or five hours after the operation, the patient has a motion, and in the course of twenty-four hours, two or three, and he will be supposed to be doing well. On the following day there will be no motion, the abdomen is tense and tender to the touch, and vomiting comes on. Were I to tell you how many I have known do well for three days, and then die after all, I should enumerate a long list. Great danger is to be apprehended for some time afterwards, and even those who have two or three motions within twenty-four hours often die." Such was the opinion and practice of the most eminent man of his day, viz., to give a purgative five or six hours after the operation; a practice which was universally adopted, and which doubtless, in a great measure, caused the long list of deaths which Sir Astley was capable of enumerating.

Mr. Lawrence, too, of St. Bartholomew's, who stands second to none in his knowledge of this particular branch of surgical pathology, states in his *Treatise on Ruptures*, a work of great research, affording a mass of valuable practical information: "When the operation is finished the patient should be placed in bed, and allowed to remain there quietly for some time; a little tea, thin gruel, or other diluent being given if it is desired. The pain is lessened and the vomiting ceases after the operation; sometimes the bowels are spontaneously relieved, and a considerable abatement of the symptoms in general ensues. More commonly it is necessary to solicit the action of the intestinal canal by aperients and injections. If, therefore, the bowels should not have been relieved in three or four hours, a few grains of calomel may be given in a pill; or two pills may be administered, consisting of calomel and the compound extract of colocynth in equal parts. The sulphate of magnesia may be given afterwards, in the dose of two drachms or of one drachm, in the infusion of roses, or in a mixture of mint-water and common water, and this should be repeated every three or four hours until the bowels are freely relieved. We must repeat those or similar means, and persist in their employment until the canal is completely unloaded." He also adds in a note at the bottom of the same page, that "M. Velpeau recommends the exhibition of purgatives after the operation, and dissents expressly from the opinion of Dupuytren and others, that these remedies increase the probability of inflammation." The work from which I have just quoted was published in the year 1838. Whether Mr. Lawrence still holds the same opinion, or pursues the same practice, I am unable to say.

I wish to confine my remarks more especially to the consideration of *purgatives*, and to endeavour to clear up the disputed point whether it is better to administer them immediately after the operation for strangulated hernia, or to wait and leave nature to take her own course. My attention was first directed to this important point by the following interesting case.

CASE. A gentleman, a valued friend of mine, aged 76, went to Brighton for change of air and sea-bathing. During the time he was there, he was attacked with the symptoms of strangulated hernia. The late Mr. John Lawrence was sent for (a man distinguished for his ability as a surgeon, and for whose untimely fate many will mourn); and, after having tried the various means usually had recourse to, he was unable to reduce the rupture. The operation was proposed, and submitted to without a murmur. The constricted bowel was relieved, and the more urgent symptoms immediately subsided. No *purgative*, and, indeed, no medicine of any kind was given, and it was seven days after the operation before the bowels were relieved, when an evacuation took place spontaneously, and without the least inconvenience or pain. No inflammatory action took place, and the