the heads of the departed to facilitate the exodus of any malignant influence still lingering within, and to ensure them, by the venerated aperture, a satisfactory position in their new existence. For similar reasons the bone amulet was buried with the deceased, and sometimes it was even placed within his skull. Dr. Munro considers it hard to say for what purpose such an insertion should have been made, but, arguing from his data, the practice does not appear to me difficult of explanation. He has shown that disease was the work of a demon imprisoned in the skull; that this demon was expelled through the trepanned hole; and that its margins were thus sanctified for talismanic purposes. The unclean spirit was gone out of the man, and observation showed that, during the man's earthly existence, he did not return; but what guarantee was there that in the dim unknown region to which the deceased was passing the assaults of the evil one might not be renewed, that he might not return to his house whence he came out, and, with or without other spirits more wicked than himself, enter in and dwell in the swept and garnished abode? Surely, with such a possibility before them, it was the duty of pious mourners to offer all the protection that religion could suggest, and to defend the citadel with that potent amulet which recorded the previous discomforture of the besieger. The *post mortem* trepanning may have been such a pious endeavour to carry sacramental benefits beyond the grave, as induced the early Christians to be baptised for the dead, and, if there be truth in the deductions which have been made from the evidence, they point not only to a belief in the supernatural and in the existence of a future state, but also to that pathetic struggle of human love to penetrate the kingdom of death, which has persisted from the death of "Cain, the first male child, to him that did but yesterday suspire."

The possibility of reasonably making such deductions from a few decayed bones is a remarkable proof of the progress of anthropological science. Should any readers regard these deductions as unwarranted, they must remember that their value is dependent upon a series of facts which can here only be but very imperfectly reproduced. For these evidences in full sequence reference should be made to the paper by Dr. Munro, which forms the subject of this notice, and which will amply repay perusal. FRANK REDE FOWKE.

ANIMAL HEAT AND PHYSIOLOGICAL CALORIMETRY.[1]

THE problem of animal heat is one of the oldest problems of scientific speculation. Nevertheless it is only within recent years that we have been able to speak of it in terms of modern knowledge.

Among the earliest contributors to such knowledge we may cite John Mayow and Joseph Black. Mayow was the first to suggest that atmospheric air is not a simple element and that its "nitro-aeric particles," in combining with the blood in the lungs, produce the animal heat, while Black demonstrated that the air expired by the lungs contains "fixed air" or, as we now call it, carbonic acid.

Priestley discovered oxygen gas in 1771, but Lavoisier was the first to show that this constituent of the air is taken in by the blood in the lungs, and that its combination with the carbon, which is a regular constituent of all organic matter, produces animal heat in the same way as in all combustions. Lavoisier was the first, too, who measured the heat produced by an animal, making use of the ice calorimeter, constructed by himself and Laplace, while Crawford nearly at the same time made investigations with an apparatus similar to our water calorimeter.

Neither form of apparatus is very suitable for this purpose. Scharling, Vogel, and Hirn made use of an air calorimeter. Within the last few years Prof. d'Arsonval of Paris adopted the same principle, and I myself have worked out the theory of it, and constructed apparatus, with which I have made a great number of experiments.

The animal to be experimented upon in my apparatus is placed in a chamber surrounded by double metallic walls. The heat given out by the animal raises the temperature of the air contained between the walls, until the radiation from the outer surface causes a loss of heat equal to the amount gained by it from the animal. This state of things having been established, the temperature of the air becomes constant, the gain and loss of heat being equal. In this way the heat given out can be calculated.[1]

The chamber containing the animal is well ventilated by aspiration. If we measure the volume of the air aspired and conduct a part of it through liquids absorbing carbonic acid, the amount of this gas given out by the animal can be measured. In another series of experiments the amount of oxygen absorbed by the animal was also measured. The combination of apparatus I made use of for this purpose is a variation of the method invented by Regnault and Reiset.

I shall not weary you with a long enumeration of all my experiments. All I wish is to give a brief account of some of the results, which I think are of interest from a general biological point of view.

In the first place, I may mention my experiments on fever. The high temperature in cases of febrile disease—is it the result of greater heat production? Are we to assume that certain poisons taken into the body, or produced in it by microbes, stimulate the nervous system, or directly influence the tissues in such a way as to cause greater oxidation, and thus to produce more heat?

That is the opinion of many medical men, but it is met with the great difficulty that neither the expiration of carbonic acid nor the excretion of oxidized nitrogenous matter is increased to such a degree as to account fully for the rise of temperature. Therefore Traube, the late clinician of Berlin, proposed the theory that the rise of temperature in fever is caused, not by greater heat production, but by greater retention of heat.

On producing fever in animals by injection of various putrid substances, I found that at the beginning of the fever, heat production is not increased, that the loss of heat is diminished, and that the difference between the normal loss and that observed in the period of rising temperature is sufficient to cause the febrile rise. When the temperature reaches its highest point the amount of heat given out rises and comes to its *normal* rate. Finally, when the fever begins to subside, during the period of falling temperature, the loss of heat is greatly increased.

All this is in perfect accordance with Traube's theory. Nevertheless, I cannot say that heat production is *never* augmented in fever. I have not yet been able to make many experiments on man. There are two great difficulties in the way, and the greatest is the impossibility of making a strict comparison between the heat production in fever and that in the normal state, except in cases of the regular intermittent type. Malaria, once so frequent in several parts of Germany, nowadays, thanks to hygienic improvements, is very seldom met with. So I have been able to make only two experiments on an individual afflicted with intermittent fever, some on invalids with abdominal typhus (typhoid fever), some on cases of pneumonia, and others on cases of fever caused by the injection of Koch's tuberculine during the short time when such injections were practised in the hospitals of Erlangen. In these cases I found a small but real augmentation of heat production, and therefore I am inclined to suppose that the question is not yet solved. Perhaps there are two causes able to raise the temperature in fever, one of them prevailing in some cases or types of fever.

Most of my studies were conducted with a view to explain the connection between heat production and other physiological functions, and the influence of external circumstances on it. Higher animals, mammals and birds, maintain their own temperature nearly at the same degree, even when the temperature of the surrounding air changes within large limits. Is this *regulation*, as we call it, caused by adaptation of heat production to the greater or smaller loss, or are there means to keep the loss constant in spite of the changing difference between the animal and surrounding objects?

On measuring the heat production of the same animal in cold and warm air, I found that it is smallest in air of medium temperature, *i.e.* about 15° C., becoming greater in lower and in higher temperatures. Thus an animal produces and loses nearly the same amount of heat in air at 5° as in air at 25°. In this case regulation of the animal temperature can be effected only by changes of the co-efficient of emission of heat from the skin, caused by changes of circulation. But for longer periods

[1] Paper by Prof. Rosenthal of Erlangen, read before the Biological Section at the Edinburgh meeting of the British Association for the Advancement of Science.

[1] For a fuller account see my papers in: *Archiv für Physiologie*, 1889, and in *Sitzungsber. d. K. preuss.* Akad. d. Wissensch. 1888-1892.

that regulation is insufficient. In winter time we use thicker clothing, we need more food, and if the cold is very great, we produce more heat by muscular action. In accordance with that experience, I found that animals produce more heat in winter than in summer. If nourished with the same food, sufficient to maintain their weight constant in winter, they do not oxidize the whole in summer, and therefore they gain in weight. It is remarkable that similar changes were observed by Dr. Karl Theodor, Duke of Bavaria, in the amount of carbonic acid expired by a cat, in the case of which he measured the expiration of this gas during five months.

Many experiments have been made to find the combustion heat of our food-stuffs. For want of direct animal calorimetry, physiologists used these data for calculating the heat produced by living beings; but as my experiments show, there is frequently no exact accordance between the two.

Richly nourished animals produce less, sparely nourished ones more, heat than the calculation gives. Between the two cases there is a third one in animals *sufficiently* nourished, viz. such as take in so much nutriment as serves to maintain their weight unchanged for a long time. In this case only the amount of heat produced is really equal to that calculated upon the combustion of the constituents of food. But also in this case variations are observed, caused by change of temperature, muscular motion or other circumstances, so that only the middle figures correspond exactly to the theoretical value.

Thus, if a well-nourished animal is starved the heat production remains unchanged from three to four days, the animal burning its stored-up materials and losing much of its weight; only then is it suddenly reduced to a lower amount. If now food is given again, heat production remains small, the weight increases, and then, three or four days later, the heat production increases and reaches its former amount.

If a sufficiently nourished animal takes in all its food once a day, the heat production varies very regularly in the twenty-four hours. Two hours after the meal it begins to rise, comes to its maximum point between the fifth and seventh hour, falls suddenly between the eleventh and twelfth hour. In the second half of the period the changes are small, the minimum point being usually in the twenty-third hour.

Similar changes go on in the expiration of carbonic acid. But after the meal it rises much more rapidly, and therefore comes earlier to its maximum point. Thus the ratio between heat production and expiration of carbonic acid is not a constant. This is true not only in the daily period. The variations are seen to be still greater when we compare different animals, or the same animal at different times and in different states of nutrition.

By such researches we are enabled to examine more exactly what chemical changes are going on in the animal system. The materials afforded by food are all oxidized at last, and leave the body in the form of carbonic acid and nitrogenous matter like urea. What in a longer period is burnt in such a way, we can, with a certain degree of exactness, make out by chemical examination of the constituents of food on the one hand, and of the excretions on the other. We can make up, in such a way, a balance account for gain and loss of the animal, like the balance account of a merchant. But such an account gives no exact knowledge, because we have no means of completing it by taking an inventory. We are, as regards the living body, in the same position as a political economist, who knows the amount of goods imported into and exported out of a country, but does not know what has become of the goods stored up or used up in the country itself. Therefore political economists do not now regard the mere balance of trade as being so important as they formerly thought.

Physiology, like all branches of science, begins with a mere description of processes observed. With the progress of our knowledge, reason tries to connect these processes one with another, and with those going on in lifeless nature. What we call *understanding* is nothing else than knowing such connections. Now in the case of bodily income and expenditure, it is easy to observe that all materials going out of the system are more oxidized than those taken in as food, and reason tells us that the combination of these food materials with the oxygen inspired must be the source of animal heat. Hence, we have no doubt that the amount of heat produced must correspond to the amount of chemical processes going on during the same time. But these processes we cannot observe directly; we can only observe the final products carbonic acids and others, when they leave the body. But by some of the processes heat may be produced or absorbed without any visible change of the body as a whole, viz. by solution of solid matter, by splitting highly complex substances into more simple ones, by forming sugar out of starch or glycogen out of sugar. Considering this, we need not wonder that for a long time it was impossible to answer the question whether there is any other source of heat production in animals besides oxidation. Only long continued calorimetric measurements have enabled me to fill up this gap.[1] This done, I thought it possible to discover also something about these inner processes, by comparing, hour for hour, the heat production with the excretion of carbonic acid, and with the absorption of oxygen.

If the ratio between the heat produced and the carbonic acid expired changes, this cannot be explained otherwise than by the fact that different chemical substances are burned. Each substance, according to its chemical constitution, gives out, when oxidized, a certain amount of carbonic acid, and produces a certain amount of heat. But in the system it is a mixture of different substances which come to be oxidized. This mixture changes, not only in animals differently nourished, but also in the same animal in different periods of digestion. After a rich meal, what comes into the circulation first must be that part of the food that is easily and rapidly digested and easily and rapidly absorbed. Such substances are the proteid matters. Later, the other constituents of the food, especially fat, come to the tissues, where they are burned. Now *fats*, for the same amount of carbonic acid, produce far more heat than proteids; so, during the first hours of digestion the afflux of oxidizable matter to the tissues being very great, both heat production and expiration of carbonic acid increase, but the latter in a far higher degree than the former.

The animal body may be compared, as Prof. Huxley so well says, to an eddy in a river, which may retain its shape for an indefinite length of time, though no one particle of the water remains in it for more than a brief period. But there is not only the difference between the animal eddy and the eddy of the river, viz. that the matter which flows into it has a different chemical composition from the matter which flows out of it, but in addition, matters which make up the eddy in a given time, change, if I may so say, their chemical value, combine with or separate from each other, without any visible change of the whole system.

The study of heat production is of the greatest value. No doubt, the study of the vital processes becomes more complicated when we take into account the invisible internal changes occurring in the body. But simplicity is not the highest aim in scientific inquiries; the highest possible exactness is that to which we must aspire. Happily, the history of science shows that after trying several ways to solve complex problems, we find that one of them leads to a higher point of view, whence things appear in all their completeness, simplicity and distinctness. Towards such a point of view my researches are but the first step. Let us hope that the united forces of many physiologists will shorten the time necessary for the completion of the work.

MAGNETIC PROPERTIES OF LIQUID OXYGEN.[2]

AFTER alluding to the generous aid which he had received both from the Royal Institution and from others in connection with his researches on the properties of liquid oxygen, and to the untiring assistance rendered him by his co-workers in the laboratory, Prof. Dewar said that on the occasion of the commemoration of the centenary of the birth of Michael Faraday he had demonstrated some of the properties of liquid oxygen. He hoped that evening to go several steps further, and to show liquid air, and to render visible some of its more extraordinary properties.

The apparatus employed consisted of the gas-engine down stairs, which was driving two compressors. The chamber containing the oxygen to be liquefied was surrounded by two circuits, one traversed by ethylene, the other by nitrous oxide. Some liquid ethylene was admitted to the chamber belonging to its circuit, and there evaporated. It was then returned to the

[1] See also my address delivered to the general meeting of the German Association of Naturalists at Bremen, 1890.

[2] Abstract of Friday evening discourse delivered at the Royal Institution by Prof. Dewar, F.R.S.