produce, of causing the changes which characterise the particular disease.

I show you here photographs of a variety of such pathogenic bacteria, and you will see from them that both as regards the manner of distribution of these bacteria in the tissues of the infected individuals as also in their morphological and biological characters in artificial cultures, most of them are sufficiently distinguished from one another and from other non-pathogenic bacteria. In considering the general action of pathogenic bacteria we find that they may be grouped into (*a*) such as are entirely, so far as our knowledge at present goes, dependent on the living body of man or animals; these are endogenic bacteria or true parasites, for they do not appear to lead an existence independent of the living body: when, therefore, infection by them takes place, it takes place by direct transference from an infected individual to a new one; this is so in small-pox, in vaccinia, and in hydrophobia; (*b*) a second group comprises those which are capable besides a parasitic life, *i.e.* growing and multiplying within the animal body, to lead also an existence independent of the animal body; that is to say, they, like many other non-pathogenic bacteria, are capable of thriving in suitable materials in the outside world; such are anthrax and fowl cholera, asiatic cholera and typhoid fever, tetanus and diphtheria, and others. But also amongst these some can lead such an "ectogenic" life comparatively easily, while others do so only in a restricted sense; while, for instance, anthrax, tetanus, typhoid fever can lead such ectogenic life easily, *i.e.* growing and multiplying outside the animal body; others, like tubercle and glanders, do so only to a very small extent. The former are obviously the more dangerous to man and animals on account of their more ready distribution than the latter, of which the ectogenic existence is considerably restricted by various conditions, *e.g.* they require higher temperatures to grow at, they require a much more specialised nutritive medium than is generally attainable by them.

Time does not permit me to show you in detail the many and wonderful results obtained within a comparatively short recent period by a large number of workers, as regards the identification of many of the pathogenic bacteria, their habits of life, their mode of spread and infection; the way in which their action can be attenuated, their effects weakened, and such weakened cultures used for protective inoculations; the brilliant results achieved by Pasteur and many others in these protective and curative inoculations against anthrax, against fowl cholera, against tubercle, against hydrophobia, against tetanus and other diseases. But I will ask you to bear in mind that almost the entire study of bacteria, the exact methods first introduced by Koch and now universally used not only in regard to pathogenic bacteria, but in all other branches of bacteriology; the exact knowledge that we possess of some of the most important branches of hygiene: as the knowledge of the exact nature of contagium, its mode of spread, the means of disinfection, the methods of protective inoculation, and a hundred and one other important points have been the result of, and gained by, experiment on animals. Amongst the wilderness of misery, cruelty, and death inflicted by mankind on animals for gain, for sport, pleasure, and other similar objects, to decry, as some do, the use of a comparatively few animals for the sake of gaining knowledge of the most important and complex phenomena of life and of disease, and of securing power to apply this knowledge in the interest not only of mankind, but of the animals themselves, is apt to make one remember the words: "Ye blind guides! which strain at a gnat and swallow a camel," or the words, "Thou hypocrite! cast out first the beam out of thine own eye, and then shalt thou see clearly to pull out the mote that is in thy brother's eye."

SURGERY AND SUPERSTITION.

TO those unversed in the history of surgery it may come as a surprise that many of the appliances commonly regarded as the inventions of yesterday, are but the perfected forms of implements long in use. It is astonishing to find amongst the small bronzes of the National Museum at Naples, bistouries, forceps, cupping-vessels, trochars for tapping, bi-valvular and tri-valvular specula, an elevator for raising depressed portions of the skull, and other instruments of advanced construction which differ but little from their modern congeners. The invention of such instruments, and the skill displayed in their construction, presupposes a long period of surgical practice. We find, accordingly, that four hundred years before our era, Hippocrates was performing numerous operations bold to the verge of recklessness. Thus he was accustomed to employ the trepan not only in depression of the skull or for similar accidents, but also in cases of headache and other affections to which, according to our ideas, the process was singularly inapplicable. Strangely enough, the Montenegrins are, or recently were, accustomed to get themselves trepanned for similar trifling ailments, and it is probable that in both instances the procedure was but the surviving custom of primeval ages. That such operations were then performed Dr. Robert Munro, in his admirable article upon prehistoric trepanning in the February number of the *Fortnightly Review*, conclusively shows. His paper records a strange blending of the sciences of medicine and theology in their initial stages; for, whilst he makes it clear that during the neolithic period a surgical operation was practised (chiefly on children) which consisted in making an opening through the skull for the treatment of certain internal maladies, he renders it equally evident that the skulls of those individuals who survived the ordeal were considered as possessed of particular mystic properties. And he shows that when such individuals died fragments were often cut from their skulls, which were used as amulets, a preference being given to such as were cut from the margin of the cicatrised opening. The discovery arose as follows. In the year 1873 Dr. Prunieres exhibited to the French Association for the Advancement of Science an oval cut from a human parietal bone, which he had discovered in a dolmen near Marvejols, embedded in a skull to which it had not originally belonged. His suggestion that it was an amulet was confirmed on the discovery of similar fragments of bone grooved or perforated to facilitate suspension. When Dr. Prunieres's collection was examined by Dr. Paul Broca he pointed out that that portion of the margin of the bone which had been described as "polished" owed its texture to cicatricial deposits in the living body, and that, where these were wanting, death had ensued before the pathological action was set up, or the operation had been *post mortem*.

These discoveries led to widespread investigation, and to the production of trepanned skulls from Peru, from North America, and from nearly every country of Europe. These were not restricted to any particular race or period, but ranged from the earliest neolithic age to historic times, and included skulls of dolichocephalic and brachycephalic types.

The method of conducting the operation appears to have been to gradually scrape the skull with a sharp flint, though there is occasional evidence of its use in a sawing manner such as obtained when the ruder implement was superseded by one of metal. The process was almost exclusively practised upon children, probably on account of the facility with which it could then be accomplished, and possibly also as an early precaution against those evils for which it was esteemed a prophylactic. What the dreaded evils were was suggested by Dr. Broca, who, whilst he believed that the operation was primarily conducted for therapeutic purposes, saw behind these the apprehension of a supernatural or demoniacal influence. Readers of Lenormant's "Chaldean Magic" will remember "the wicked demon which seizes the body, which disturbs the body," and that "the disease of the forehead proceeds from the infernal regions, it is come from the dwelling of the lord of the abyss." With such an antiquated record before us it is, therefore, by no means an extravagant theory to broach, as Dr. Broca has done, that many of the convulsions of childhood, which disappear in adult life, were regarded as the result of demoniacal possession. This being granted, what more natural than to assist the escape of the imprisoned spirit by boring a hole in the skull which formed his prison. When a patient survived the operation he became a living witness to the conquest of a fiend, and it is comprehensible that a fragment of his skull taken after death from the very aperture which had furnished the exit would constitute a powerful talisman. Chaldean demons, as we know, fled from representatives of their own hideous forms, and, if they were so sensitive on the score of personal appearance, others may have dreaded with equal keenness the tangible record of a previous defeat. It is certain that to cranial bones medicinal properties were ascribed, a belief in the efficacy of which persisted to the dawn of the eighteenth century; whilst, in recent years, such osseous relics were worn by aged Italians as charms against epilepsy and other nervous diseases. When once the dogma was promulgated that sanctity and a perforated skull were correlated, fond relatives might bore

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.