may be produced and thus crepitation arise, so that actually the same secretion may produce a moist or a dry *râle* somewhat at random.

Again, in respect of some of the forms of fine crepitation the ear recognises the sounds readily, and they are described correctly as small crepitation, but the ear alone is quite unable to decide in many cases what the cause or mode of production of these sounds is. The same fine crepitant sound may have four modes of origin. It may be due (1) to the opening out of collapsed air vesicles, the crepitant *râle* of Laennec; (2) to the crepitation of emphysematous lungs, the "dry crackling crepitation" of Laennec; (3) to the bursting of minute air bubbles, the small mucous crepitation of Laennec; or (4) to pleuritic friction. According to the method of production we might call three of these "dry" *râles*, and the other "moist"; but it might be quite impossible by the ear alone to decide to which cause the particular crepitation were due.

It is dangerous for anyone, whether student or teacher, thus to mix up fact and inference in a common term, and it is fatal to clear teaching. I may make my meaning clear if I take another example. Nothing is commoner in the examination of the heart than to hear a murmur described as "mitral systolic." Now, what does this expression really involve? It means that there is a murmur audible at the apex, systolic in time, transmitted in certain directions—that is, to the axilla and to the interscapular space, associated with certain changes in the heart itself and in the character of the heart sounds elsewhere, and because of these facts we infer that it is produced by regurgitation through the mitral valve. The proper place for this conclusion would be at the end of the description of the case, where the diagnosis comes, and not in the commencement of it under the physical examination, where we have only to describe the facts observed, and not the conclusions drawn. At any rate, what is a more common experience with students than to hear them say that there is a mitral systolic murmur, the simple fact being that there is a systolic apex murmur, while their opinion that it is due to mitral disease is quite as likely to be wrong as right? This tendency therefore to mix up facts and inferences in a common term should be discountenanced.

What we require in an account of the physical examination is a statement of facts observed, not of inferences drawn. As long as the terms employed are descriptive only they are useful; as soon as they involve an inference or a theory they become dangerous, for the facts may be right and the conclusion or theory wrong. With care even beginners can easily be right as to the facts of observation, but from want of knowledge and experience they are only too likely to be wrong in the conclusions they draw, and what the beginner has to learn is how to observe accurately and ascertain correctly the facts. The correct conclusions to be drawn from these facts can only be learnt by time and experience.

The term "*râle*" thus becomes a source of confusion, first because it is used in different senses either as a special term or as a general one; and, secondly, because the addition of such descriptive terms as "moist" and "dry" imply a theory about which there may be great difference of opinion. It would be well, therefore, if we could banish "*râle*" altogether from our vocabulary. We do not want it, and a term which is redundant is often the cause of confusion.

When I find a student puzzled by technical terms in listening to the chest, I often say, "Now drop all technical terms, and describe simply what you hear." I rarely find that they have any difficulty in hearing what there is to hear, and describing it in simple language. I may then supply the proper technical term and explain its meaning.

Treated in this way auscultation is easy to teach and easy to learn, for the facts are plain; it is only the terms and theories which differ.

I will end as I began by saying that auscultation is easy enough, and that words alone make it difficult. The difficulties may be to a great extent avoided by not using ambiguous terms. I wish teachers could be all agreed on terms as well as they are on facts: of this I have no hope at present, but a step in the right direction will have been made if it is generally recognised that the chief sources of difficulty in auscultation are the ambiguous terms in use.

A METHOD OF STAINING THE MALARIA FLAGELLATED ORGANISM.

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STUDENTS of the malaria parasite may like to know of a simple method of making permanent preparations of the oval, spherical, and flagellated phases of the plasmodium.

So far as I am aware Sacharoff (*Amœba Malariae (Hominis) Specierum Variarum Icones Micro-photographice*, Tiflis, 1892) is the only one who has hitherto successfully stained the flagellated organism. This he succeeded in doing by collecting a series of drops of malarial blood on cover-glasses which he immediately placed in a moist chamber. When, by observing under the microscope an ordinary slip and cover-glass preparation of blood simultaneously made from the same patient, he had ascertained that exflagellation had taken place, he removed the cover-glasses from the moist chamber, spread out the drops of blood, and fixed and stained the films—using gentian violet for the latter purpose. Gentian violet stains the plasmodium so deeply that it completely obscures the pigment and all internal structure of the parasite. By employing the method which I shall describe, and by using weak carbol-fuchsin as the stain, a large number of preparations of the flagellate body, showing the pigment and certain details of structure, can be procured with the greatest ease and certainty, and, at the same time, an elaborate moist chamber be dispensed with.

Fig. 1.

Thirty or forty strips (3 inches by $1\frac{1}{2}$ inches) of thick blotting paper, each having an oblong hole (1 inch by $\frac{3}{4}$ inch) cut lengthways in its centre, are prepared; they are then slightly but sufficiently moistened with water and laid in rows on a sheet of window glass.

Fig. 2.

A patient in whose blood the crescent form of the malaria parasite abounds is selected. His finger is pricked and a minute droplet of blood, the size of a large pin's head, is

expressed. A clean microscope slip is then breathed on once and the droplet of blood immediately taken up by lightly touching it with the centre of the breathed on surface of the slip. The blood is now rapidly and somewhat unevenly spread out with the needle so as to cover an area of about $\frac{3}{4}$ inch by $\frac{1}{2}$ inch. Immediately the slip is inverted over the blotting-paper cell and pressed down sufficiently to secure thorough apposition of the slip to the paper, without at the same time bringing the blood into contact either with the moistened paper forming the wall, or with the glass forming the floor of what is now a very perfect moist chamber. The rest of the paper cells are rapidly covered with blood-charged slips prepared in the same way. In from half to three-

quarters of an hour the slips are removed and dried by gently warming them over the spirit lamp—blood surface away from the flame. When dry the films are fixed with absolute alcohol, a few drops being poured on each. After five minutes the alcohol is dried off and a few drops of weak acetic acid (10 per cent. to 20 per cent.) are laid on the film and left there long enough to thoroughly dissolve out all the hæmoglobin. The slides are then washed in water and dried. They may now be stained with various reagents. So far, I have obtained the best results from weak carbol-fuchsin (20 per

cent.) and prolonged staining. The stain is dropped on the slip and covered with a watch-glass; after six to eight hours it is washed off, the slide dried and a cover-glass applied with xylol balsam. The staining, though sufficiently intense, is fairly transparent.

Most of the slides will show numbers of spheres and several or many well-stained flagellated bodies. Very few crescents remained untransformed. If the slips are removed and dried in from five to ten minutes after being placed on the blotting paper cells only crescents, ovals and spheres will be found; if they are left for three-quarters of an hour to an hour free flagella and what Ross calls spent pigment may be found, the latter sometimes enclosed in phagocytes. Occasionally flagellated bodies are also found partially included in phagocytes.

The beautiful microphotographs here reproduced, which I owe to the courtesy of Professor Greenfield and to the skill of Mr. Richard Muir of the Pathological Department,

Edinburgh University, were made from slides prepared in this way. The drawings are faithful representations of the same or similar plasmodia by Mr. Max Colhoun. Together these illustrations afford some interesting information about the evolution and structure of the flagellated body.

In the crescents (Fig 1 and A), in the ovals (Figs. 2 and B), and in the early stages of the sphere (Figs. 3 and C), it can be seen that the melanin pigment lies in or on a central body which takes the fuchsin stain less intensely than does the peripheral portion of the organism. It is also evident that at a later stage (Figs. 4 and D) the lightly staining central mass expands considerably, and that subsequently (Figs. 5 and E) the flagella are formed at the expense of this

central mass, and that for a time these flagella lie inside the delicate cyst wall which forms the outer covering of the parasite. Later still (Figs. 6 and F) these preformed flagella are projected through the cyst wall and exflagellation is completed.

It will be observed that in some of the flagellated bodies (Figs. F and G) the central mass of the organism is of considerable bulk, larger, apparently, than the crescent bodies from which they originated. In others (Figs. H and I) the body of the parasite is much smaller. This discrepancy in

size is more apparent than real. I believe that when the blood film chances to be very thin the plastic body of the parasite flattens itself on the slide and so, becoming a mere disc, appears larger than when, being in a thicker film it preserves its spherical or approximately spherical form; the same thing can be observed in ordinary preparation of fresh malarial blood. It will also be noted that in some (Figs. H and I) the flagella are of uniform thickness throughout, whereas in other (Figs. F and G) their free extremities are sometimes bulbous (Fig. G), sometimes expanded (Figs. 6 and F) into a large, leaf-shaped or reniform swelling. This is confirmatory of what has been described as occurring in the living flagellum, although expansions so considerable as that shown in Figs. 6 and F have not hitherto been described.

In contemplating in fresh preparations the evolution of the living, crescent-derived sphere into the flagellated body, one sometimes fancies that one can see the flagella wriggling

about inside the limiting membrane of the parasite. In fact one can sometimes almost see that the agitation, which is so characteristic a feature of this stage of the evolution of the parasite, is confined to the central portion of the organism, and is not shared in, except in a passive way, by the cyst wall; and that it is entirely owing to the vigorous movements of the imprisoned flagella. From the extreme transparency of the flagella and the rapidity of their movements, one cannot be absolutely sure of this in the unstained living specimens, but in these stained preparations it is distinctly evident that this fancy is well founded. In the preparation from which Fig. 5 was drawn, and also in that of which Fig. E is a photograph, the flagella can be readily made out surrounding the central mass and lying inside the limiting membrane of the organism.

G.

Looking at these photographs and drawings, I do not see how it is possible any longer to contend, as so many Italian writers have done, that the flagellated phase of the malaria parasite is an involution or degeneration form; that the flagella are comparable to the protoplasmic projections sometimes seen in scorched blood corpuscles; or that they are anything other than a distinct phase in the evolution of the living malaria parasite.

H.

I have not attempted to make similar preparations of the flagellated bodies of the benign, tertian, or of the quartan parasites. I have no doubt, however, that by selecting the stage of fever in ordinary tertian or quartan attacks their flagellated forms could be equally easily demonstrated, and that probably some interesting details bearing on the question of species would be made out.

When working at these preparations I took the opportunity to repeat Marshall's and Ross's experiments on the influence of water, and of exposure to the air, in inducing exflagellation. I can confirm their statements. I found that breathing once on the slip before taking up the droplet of blood was a better way of supplying moisture than Marshall's plan of placing a droplet of water on the coverglass; if the amount of moisture be too great, as it is apt to be by Marshall's method, the parasites are killed. I also repeated Ross's experiment of pricking the finger through vaseline, and not removing the grease film surrounding the droplet of blood till one hour thereafter.

I found that Ross's statement, that under these circumstances exflagellation did not take place, was perfectly correct; for when I spread the blood out, at the end of an hour, and at once dried and then fixed and stained it, only crescents, and

perhaps a few ovals were found. When, however, the blood, after breaking the vaseline film surrounding it, was exposed to the air for a minute or thereby, and then mounted on a slip and coverglass in the usual way for making fresh blood preparations, in many instances, after a short time, exflagellation was seen to occur.

THE CROONIAN LECTURES

ON

THE MEANS BY WHICH THE TEMPERATURE OF THE BODY IS MAINTAINED IN HEALTH AND DISEASE.

Delivered before the Royal College of Physicians of London,

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LECTURE IV.*

[ABSTRACT.]

CHEMICAL STUDY OF HEAT PRODUCTION.

THE breaking down of any chemically complex substance, such as a proteid or a fat, always leads to the liberation of energy. Various observers have, by burning these substances in a calorimeter, calculated the amount of heat set free by their destruction. In such a case the whole of the liberated energy appears as heat. Some of the most careful and recent results are Stohmann's.¹ His figures represent the amount of heat necessary to raise 1 gramme of water from 17° to 18° C. He found that the heat value, expressed in gramme-calories, of the substances enumerated was as follows: Most albumens, 5,298 to 5,941; meat, 5,662; albuminoids, 4,650 to 5,961; chondrin, 5,130; animal fat, 9,509; olive oil, 9,500; carbohydrates, 4,381 to 3,714; cane sugar, 3,955.

These figures are of great value, but we cannot, of course, calculate the heat production of an animal from its diet. In the first place we do not know what proportion of any meal is absorbed from the gastro-intestinal tract; secondly, not all the energy set free in the body is manifested as heat—some takes the form of mechanical work, and therefore in calculating the heat value of any particular diet we should have to deduct the energy which in the body takes the form of motion, but this is very difficult to estimate accurately, and therefore the deduction to be made from the total heat value of the food would be uncertain; and, thirdly, the food may be stored up in the body and not broken down for some time. The value of figures, like those of Stohmann's, which show the amount of heat produced by the breaking down of the albumins and fats of the body lies in the fact that we can,

* Abstracts of Lectures I., II., and III. were published in the BRITISH MEDICAL JOURNAL of June 26th and July 3rd.