with the models of telescopes, made especially for this purpose in the mechanical department of the Grunewald-Sternwarte, will form the foundation for an astronomical museum. It is requested that every photograph shall be furnished with the name of the observatory sending it in, also the exact particulars as to date and time of exposure, method of developing, name of object, and any thing of interest connected therewith. It would also be desirable to state on the backs of the photographs, if, and in what publication, any further particulars may be found concerning the same subject. Though the exhibition opens on May 1, any pictures, which owing to the distance of the observatory sending them, should not arrive by that date, can be received at any subsequent period. As, however, a catalogue is to be completed by July 1, it will be to the interest of exhibitors to see that their contributions arrive in Berlin on July 15 at latest. Particulars as to the number and extent of intended contributions should be sent as early as possible to Herr F. S. Archenhold, Grunewald-Sternwarte, bei Berlin.

THE SUN'S ROTATION.—Two methods have hitherto been chiefly employed to determine the period of the sun's rotation, namely, observations of sun-spots and determinations of the displacements of lines in the spectrum of the sun's limb. A third method, depending upon the movements of faculæ, has recently been utilised by W. Stratonoff (*Ast. Nach.*, 3344). His results are based upon an investigation of 400 photographs of the sun, taken during 1891–1894, and the number of daily angular movements available for discussion amounts to 1024, after rejecting those in which identifications on successive photographs were at all uncertain. All the facts which are brought together clearly indicate that faculæ in different heliographic latitudes move with different velocities, and that the rate of movement diminishes in passing from the equator towards the poles. In the zone 10°–19° the retardation amounts to 0°·37 per day as compared with the equatorial angular velocity, while in the zones 20°–29° and 30°–40° it is 0°·47 and 1°·0 respectively. The law of variation of the velocity of the faculæ with the latitude is much more complex than in the case of spots; from 0° to 8° the angular velocity is almost constant, from 9° to 16° it decreases very rapidly, between 16° and 25° it remains nearly uniform, while from 25° to 34° it again diminishes quickly. Similar results are obtained for both solar hemispheres. The faculæ appear to move more rapidly than the spots in all solar latitudes from 0° to 40°, as shown by the following mean values:—

Heliographic latitude.	Diurnal angle of rotation. Faculæ.	Spots.
° °	°	°
0–9	14·61	14·30
10–19	14·24	14·15
20–29	14·14	13·83
30–40	13·61	13·40

The spectroscopic measurements made by Dunér indicate that the photosphere rotates even more slowly than the spots, and the following comparison shows the relation of the surface rotation with that of the faculæ:—

Heliographic latitude.	Diurnal angle. Stratonoff.	Dunér.
°	°	°
0	14·61	14·14
15	14·24	13·66
30	13·87	13·06

So far as the available data permit any conclusions to be drawn, it thus appears that there are three distinct laws of rotation for what in all probability correspond to three different solar levels.

THE TSETSE FLY-DISEASE.[1]

FOR forty-six years the Tsetse fly has been notorious as a terrible scourge to live-stock, and the most formidable of impediments to colonisation in Equatorial and South Africa. First brought into prominent notice by the explorers Gordon-Cumming Oswell and Captain Vardon, it was described by Westwood[2] in 1850, under the name *Glossina morsitans*, from specimens collected by the last-named traveller. The genus, an ally of our common blood-sucking *Stomoxys*, contains six described African species, for all of which Tsetse appears now to serve as a common name.

The peculiarities of the fly and "fly-disease" have been made familiar by most other African travellers, Livingstone, Andersson, Chapman, Selous, &c. The Tsetse (Fig. 1) is a dipterous insect, of no striking appearance, grey, with darker stripes on the thorax, and a pale or yellowish abdomen furnished with two dark spots on the anterior portion of each segment; it is rather larger than the house-fly, but is narrower when at rest, the wings overlapping. The mouth-parts form a powerful, piercing and suctorial beak. Local in distribution, the fly occurs in numerous detached regions of Africa south of the Equator, its headquarters appearing to be along the Zambesi and its tributary the Chobe. "Fly-country" is hot, moist and low alluvial ground, along river-banks, covered with forest or scrub vegetation, and uninhabited save by wild animals. Within its sharply-defined limits, which may extend along one bank only of a river, the Tsetse swarms; it is extremely active, and eagerly attacks man or animals for the purpose of sucking blood. On man no effect is produced beyond temporary irritation, of which the extent has been very variously described, probably in accordance with the idiosyncrasy of the victims. Wild animals do not suffer; but domestic animals, which have entered fly-districts, are seized in the course of a few days with fever and wasting, and almost invariably die. Horses and dogs rapidly succumb, while goats, donkeys and unweaned calves are said by some travellers to be resistant; this, however, is not generally true of the two former kinds. Slight non-fatal attacks confer no immunity, but some native breeds of dogs enjoy partial protection, although a certain number of pups

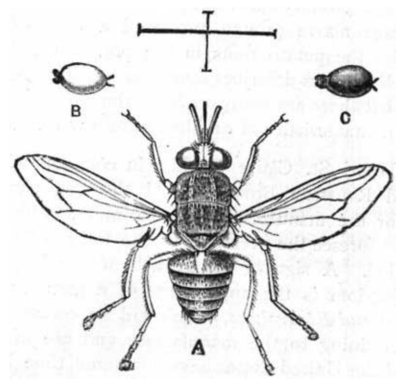

FIG. 1.—A, Tsetse fly (*Glossina* sp.), Transvaal; B, larva; and C, puparium of a Tsetse (after Bruce).

in each litter perish. Books of African travel are full of records of horses, teams of oxen, or herds of native cattle having been destroyed by entering fly-districts, and on one occasion a Masai army, proceeding to the attack of a neighbouring tribe, was effectually routed by having incautiously crossed fly-country.

For some years the accounts of fly-disease were not seriously questioned, until in 1870 a Mr. St. Vincent Erskine[1] endeavoured to show that it was due solely to change of grass or climate, and severely criticised Livingstone's account, forgetting that the proper course lay in attempting to reconcile the apparent discordance between his own and other observations. Since then Hartmann, Marno, Falkenstein and other travellers who found either the fly present and disease absent (as on the Loango coast), or the reverse, have further discredited the earlier statements, and so eminent a dipterologist as Van der Wulp,[2] in summarising the evidence, has concluded that the Tsetse is not injurious, or that its ill-effects are exaggerated. Nevertheless travellers, especially in the Zambesi district, whilst adding nothing to our knowledge, have constantly reaffirmed the connection between the fly and the disease.

One or two naturalists have indeed hit the truth, and among them Schoch, who in an ably-reasoned little paper[3] concluded that the facts pointed, not to the action of a specific fly-virus, as was originally supposed, but to the transmission of a bacterial

[1] "Preliminary Report on the Tsetse Fly-Disease, or Nagana, in Zululand." By Surgeon-Major David Bruce, A.M.S. (Bennett and Davis, Field Street, Durban.)
[2] *Proc. Zool. Soc. Lond.*, 1850, pp. 258–270.

[1] Paper read before the Nat. Hist. Assoc. of Natal, reported in *The Entomologist*, v. p. 217.
[2] *Tijdschr. Ent.*, 1884, pp. 143–150.
[3] *Mitth. Schweiz. ent. Ges.*, 1884, pp. 685–686.

poison-matter. It will be seen that this conclusion is substantially correct.

Mysterious as has been the connection between the Tsetse and the animal-disease endemic in its haunts, there has been never any reason to doubt that a properly-conducted investigation would throw much light on the subject. At last, on behalf of the Natal Government, such a research is being made by Surgeon-Major Bruce, and the results of the first three months' work are just published. They are of great interest, and are full of promise that our knowledge of this disorder will be placed at least on a level with that of kindred diseases.

Dr. Bruce, in a somewhat brief recapitulation of the characteristics and habits of the fly, adds one important new fact, of which he appears scarcely to recognise the significance. The fly investigated, which is not necessarily Westwood's species, is viviparous, giving birth to an adult larva (Fig. 1, B), which creeps about actively in search of a hiding-place, where, in the course of a few hours, it changes by the usual skin-hardening to a jet-black puparium (Fig. 1, C). Hitherto the accounts of Bradshaw and Chapman have asserted, on native authority, that the maggot lives in buffalo-droppings, and a statement of Edwards, quoted by Castelnau,[1] that the Bushmen declared and demonstrated the Tsetse to be viviparous, has gone almost unnoticed.

This fresh observation must be accepted with some reserve, as the fly has not yet been bred from the puparium. Assuming it to be correct, it is of two-fold interest; the mode of reproduction is substantially that which exists in the Pupipara, though, to judge from Dr. Bruce's account and rough figures, the newly-extruded Tsetse-larva, though equally mature, is somewhat less abnormal than that, for instance, of *Melophagus*.[2] A transitional series from oviparous forms has been described in Muscidae by Portschinsky,[3] and viviparous Oestridae are well known to occur. Nevertheless, this peculiarity of *Glossina*, which could not have been prognosticated on systematic grounds, sufficiently demonstrates the unsoundness of separating the Pupipara from Muscidae on account of developmental differences.

Moreover it shows that the Tsetse, unlike most blood-sucking insects, such as the flea or mosquito, is absolutely dependent for its continued existence upon the food taken in the imaginal state, and, unless it is capable of feeding upon other matters than blood, which, though unlikely, should not be disregarded in the inquiry, its life is bound up with that of the indigenous mammalia. And this both confirms and explains the observation made by Livingstone, Selous and others, that it is constantly associated with large game, such as the buffalo, and ceases to frequent districts from which they retreat.

Fly-disease or Nagana (a Zulu term, aptly signifying to be low or depressed in spirits) is due, according to Dr. Bruce, to the presence in the blood of a flagellated infusorian. This hæmatozoon (Fig. 2) is of elongate form, about 10–20 μ long by 2 μ wide, furnished with a membrane or "fin" running along one side of the body, and a flagellum at one end.

It is intimately allied to, if not actually identical with, *Trypanosoma evansi*, the hæmatozoon of "Horse Surra." At present Dr. Lingard, the leading authority on that Indian animal-disease, hesitates to regard the two complaints as the same, because Surra does not attack cattle. But when it is recollected that Nagana pursues a much slower course in cattle than in horses, and that wild game are immune to it, just as African sheep are to anthrax, the objection does not seem very formidable.

The hæmatozoa of Nagana make their appearance, which is signalised by a rise in temperature, in the blood after an incubation period of 7 to 20 days, swimming actively among, and apparently "worrying" the corpuscles. With the progress of the disease they increase in numbers and, at the time of their host's death, may amount, in the dog, to 310,000 per cub. mm. of blood! Neither reproductive nor any other stages of the parasite are yet known, nor has it been found in the blood of any wild animal, inoculation of which (the best test for the presence of the hæmatozoon) has hitherto failed to produce disease.

Dr. Bruce has demonstrated that it is possible repeatedly to feed Tsetse on a healthy dog without producing disease in that animal—that is, the flies possess no specific venom; but that, if allowed to draw blood from a diseased animal or the carcase of one, they will communicate Nagana to any healthy animals on which they are subsequently fed, and the same result is obtained by inoculation of diseased blood, or, in dogs, by feeding them on the flesh of an animal dead of Nagana.

Thus far is the cause of the disease ascertained, as is the fact that the Tsetse can serve as a transmissive agent; but the natural source, other than diseased animals, which are not known to occur in a wild-state, whence the flies obtain the parasite is still undetermined, nor is it proved that, unlike malaria, the disease cannot be acquired by breathing the air of the fly-country.

Repeating an old experiment of Captain Vardon's, Dr. Bruce has shown that a few hours' sojourn in a fly-district is sufficient exposure to induce the disease in a horse, which is prevented from eating and drinking there; but to complete the proof that the flies are indispensable as carriers of infection, it has further to be shown that domestic animals, if protected from their bites, can remain in such a region with impunity. As yet Dr. Bruce has not been able to make the experiment, but it may be observed that the concurrent testimony of many travellers, that animals can safely cross a fly-country on nights when the insects are inactive, goes to prove that the infection is not air-borne.

If Dr. Lingard's very voluminous report on Surra[1] be compared with the one under consideration, the points of identity between the two diseases will be found to be remarkably numerous, though not quite universal, and the fact that two such investigations are in progress by workers in touch with each other ought materially to quicken and extend the results arrived at. The complete life-history of the Surra parasite has yet to be

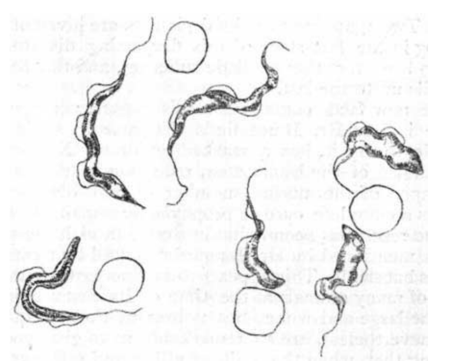

FIG. 2.—Hæmatozoa of Nagana in the blood of a horse (after Bruce).

published, and in discussing the ætiology of that complaint, Dr. Lingard attaches much importance to the eating of grass from swamps and marshy ground, and the drinking of stagnant water, and but little to fly-infection, which he considers to occur chiefly when healthy and diseased horses are crowded together. Such infection is, therefore, regarded as being of the purely transmissive form of fly-inoculation found in anthrax and possibly other septic diseases.

But a more intimate relationship of the fly and the parasite, at least in Nagana, suggests itself. In 1884, Dr. Manson showed[2] that the mosquito is the intermediate host of filaria, and applying his observations to the building-up of a working hypothesis as to the life-history of the malaria parasite (Laveran's hæmacytozoon) outside the body, he suggested, in 1894, that the mosquito also served as host for that form, and that the "flagellation-stage" assumed by a certain number of the parasites in drawn blood was an incipient change designed for life in the mosquito. In a course of lectures,[3] in process of being delivered as we write, for the unpublished text of which we have to return him our warm acknowledgments, this theory is developed in greater detail, and is supported by the observations of Surgeon-Major Ross, that examination of the blood imbibed from a malarial patient by mosquitoes shows, shortly after its extraction, that not a few, but the majority of parasites undergo flagellation. That the destiny of the flagella is still untraced is due to the extreme difficulty of the observations.

Dr. Manson's theory still awaits its final proof or disproof,

1 *Compt. rend.*, 1858 (1), pp. 984–986.
2 Leuckart, *Abh. Naturf. Ges.* Halle, 1858, pp. 145–226; and Pratt, *Arch. Naturg.* 1893, i. pp. 151–200.
3 Osten Sacken, *Berl. ent. Zeitschr.*, 1887, pp. 17–27.

1 "Report on Horse Surra," vol. i. (Bombay, 1893); and "Summary of further Report on Surra." (Bombay, 1895).
2 *Trans. Linn. Soc.* (2), II. pp. 367–388, pl. xxxix.
3 "The Goulstonian Lectures on the Life-History of the Malaria Germ outside the Human Body," delivered before the Royal College of Physicians of London, March 1896, by Dr. Patrick Manson.

and has been developed to a stage in which, failing the latter contingency, it is likely to be widely accepted. Assuming it to be correct, man occupies to the parasite the position of what he terms an "optional host," in which parasitism is neither necessary for nor inimical to the continuance of the species.

Another case of such possible relationship may be mentioned. In 1889, it was shown by Drs. Smith and Kilborne [1] that the so-called "Red-water" or "Texas fever" of cattle is a disease of malarial type due to the presence in the red blood-corpuscles of bodies presenting a certain similarity to Laveran's parasite. In this disease the sole form of infection known is by the agency of cattle-ticks (Ixodidæ), in which, notwithstanding their undoubted transmissive part, we believe the entozoon has hitherto not been detected in any stage.

Without attempting in any way to prescribe a line of research which Dr. Bruce is unlikely to overlook, it is impossible not to foreshadow the interest which will attach to an examination of the evolutions of the parasite in the body of the Tsetse, an examination which may end by showing that the insect, even if it possesses no specific virus in the older sense, may play an essential part in the economy of the hæmatozoon.

The symptoms, course, and pathology of Nagana are treated very fully by Dr. Bruce in a series of clinical cases, accompanied by charts indicating the variations of temperature, and the percentages of red blood-corpuscles and hæmatozoa. Suffice it here to say that the red blood-corpuscles may be reduced to one-third of their normal amount, and in one dog, at the point of death, bore to the parasites the proportion only of ten to one. He finds it invariably fatal in the horse, ass and dog—perhaps not necessarily so in cattle—in which it runs a much slower course. Two graphic and painful pictures are given of a donkey and a dog in the last stage of this distressing disease, and one is glad to learn that there is little suffering, and that the appetite rarely fails up to the last.

Of the new facts contained in this report, perhaps the most welcome is that Dr. Bruce finds that arsenic, so far as he has been able to try it, has a marked action on Nagana, causing disappearance of the hæmatozoa, reduction in temperature, and maintenance of the normal number of red blood-corpuscles. That it is a complete cure or prophylactic remains to be shown. The same result has been found in Surra, though the proportion of cases mentioned by Dr. Lingard as cured by treatment with arsenic is but small. This appears to be due partly to the extreme debility of many animals at the time of its first administration, and to the large and sometimes poisonous doses required. Its effects, nevertheless, are so remarkable as to give good ground for hoping that, when the limits of utility and safety of the drug, especially as a preventive, or in the early stages of disease, are determined, the trivial addition of a supply of arsenic to the traveller's outfit will free the African colonist of, perhaps, his greatest source of anxiety.

In Surra attempts have been made to treat the disease by inoculation or injection of filtered serum from affected animals. They have not proved successful, nor, while admitting the importance of the fact that the range of forms attacked by it and Nagana is limited, is there any *à priori* reason why they should succeed. The class of diseases here noticed, of which malaria may be taken as the type, is not caused by bacteria; and though it is known that the vitality of hæmatozoa is affected by alterations in the medium in which they live, as by the administration of quinine or arsenic, the ordinary methods of research and antitoxic treatment employed in bacteriology do not appear to be applicable to them.

In view of a tendency in the reports, both of Dr. Bruce and Dr. Lingard, to dwell in detail upon the clinical features of the respective diseases, it cannot be too strongly urged that, when once the pathogenic nature of the hæmatozoon has been established, these inquiries, in order to progress to a fruitful issue, must be conducted on zoological lines. The mode of reproduction, distribution and general bionomics of the hæmatozoon, and, in the event of its possessing more than an accidental connection with the Tsetse, the economy of that insect, these are the essential subjects of research: and little light will be thrown on them by any amount of laboriously-compiled clinical and pathological details.

It is to be desired that Surgeon-Major Bruce's further and more complete reports shall be republished in England, or at least made easily accessible to the many persons interested in African colonisation. WALTER F. H. BLANDFORD.

[1] *Ann. Rep. U.S. Sec. Agric.*, 1889, pp. 88-91: 1890, pp. 92, 93, and 105-110; and following year.

THE ACTION OF LIGHT ON THE IRIS, DEMONSTRATED BY A NEW PUPILOMETER.

BROWN-SÉQUARD observed that, in the iris of batrachians and fishes, separated from the rest of the eye, the pupil contracts at the approach of a candle, a fact which he attributed to the direct action of the light on the muscular tissues of the iris, the nervous elements having already lost, as he thought, at the times of his experiment, all irritability. We may also ask if the iris of the living eye responds to the direct action of light.

This problem cannot be approached directly, because of the mobility of the eye and the extreme variability of the pupil.

My new pupilometer, constructed by the well-known engineer, Mr. Ph. Pellin, consists of a series of three tubes of increasing diameter, commencing with the ocular tube; the first is provided with a screen perforated by a very small hole, and with an adjustable frame which may be removed or brought near in a manner to fix the eye at the required distance (12·8 mm.), of the anterior focus of the eye. The last tube is closed by a ground glass, 10 cm. in diameter; on the surface of this glass appear black and white circles with numerical graduations. All the peripheric zones of the ground glass which are not perceived by the retina illuminate the iris. In this manner I am able to distinguish the effect produced upon the pupil by suppressing the illumination of a portion of the iris by means of opaque rings of blackened copper successively arranged upon the glass, the apertures of which are precisely equal to the apparent surfaces of the pupil, and then suddenly removed. For the retina nothing is changed by changing these rings, since the opening of each ring equals precisely the apparent surface of the luminous admission; for the iris, on the contrary, all is changed, since the opacity of the interposed rings prevents the luminous rays from reaching it.

The experiment made under these conditions proves that there is almost always a dilatation of the pupil when the iris is withdrawn from the light. The process may be described as follows. The subject is requested to indicate the largest concentric circle that he is able to distinguish on the luminous background, whereupon I place against the background an opaque ring, the opening of which equals precisely the said concentric circle; after a few moments I remove the ring, and then the subject generally remarks the coincidence of the apparent surface of his pupil with a concentric circle of much greater diameter. I have noticed but two exceptions to this rule where the result was a contraction instead of enlargement. The dilatation varies from $\frac{1}{8}$ to $\frac{1}{73}$ of 1 mmq. for 1 mmq. of iris withdrawn from the light, and such dilatation has generally been observed to be greater for the dark than for the light iris.

The great majority of dark eyes which prevails in southern latitudes is perhaps a provision of nature to thus protect the eye from the effect of too abrupt changes of luminous irritation.

In any case the variations of dimensions of the iris are much less when it is the iris alone which is subjected to light than when the iris and the retina are influenced together. It may be shown, for instance, that if 1 mmq. of the centre of the retina be withdrawn from the light, the iris is capable of increasing in size from 1 to 16 mmq.

If, with the pupilometer of Robert Houdin, we observe the pupil of one eye while the iris of the other is obscured, we remark on the said pupil a dilatation from half to quarter of a millimetre in diameter; this goes to prove that the action of light on the iris is due, in part at least, to a reflex of cerebral origin; but, on the other hand, we do not yet possess sufficient knowledge of the anatomy of the iris to enable us to say whether these variations are due to the direct action of light on muscular elements (as supposed by Brown-Séquard) or to the action of nervous centres yet unknown belonging to the iris.

I have studied the influence of a coloured disc on the pupil, allowing the coloured light to strike the retina, in which cases I remarked that the more luminous the colours the less the dilatation of the pupil: yellow and green, for instance, cause a greater dilatation than red or blue. Again, I have remarked just the contrary. The same contradictions, which may be explained by the fatigue of the eye, are manifest when we examine the isolated action upon the iris of rings cut from the same block as the aforesaid coloured disc.

I have also endeavoured to find with the new pupilometer the