which can be shown with certainty on the galvanometer. Accuracy will depend, among other things, on the accuracy of the weights and balancing resistances employed, while the definition is decided by the workmanship and design of the instrument alone.

In optical instruments, resolving power is judged by the smallest angular or linear quantities which can be distinguished by their means, and definition by the relation which these quantities have to the size of the field over which they are distinguishable. If a telescope, for instance, can distinguish seconds of arc, its resolving power is 206,000 nearly, but if the field over which this resolution extends is only half a degree, the definition would be 1800 (i.e. $206,000/57 \times 2$).

Although the performance of any combination of real lenses depends on design and workmanship, it is not difficult to find the limits beyond which even perfection in both does nothing to increase resolving power.

The function of a perfect lens is to change the radius of curvature of a spherical wave surface.

FIG. 4.

Let D (Fig. 4) be the diameter of the lens where the change is effected, f the new radius of curvature, and o the geometrical focus. From every part of the wave surface at D partial waves may be supposed to spread, all of which will reach o in the same phase. Confining the attention for the moment to those rays which start from the opposite ends of a diameter of the lens, the partial waves from either end will be in opposite phases at a distance a , in the focal plane, from o if $2a \sin a/2 = \lambda/2$, where a is the angle subtended at o by D , and λ the wave-length of the light. Also, since $\sin a/2 = D/2f$, $a/f = \lambda/2D$ and $a = f\lambda/2D$. Thus $\lambda/2D$ and $f\lambda/2D$ are the least angular and linear distances from the geometrical focus at which a total absence of light can be found.

If the partial waves are received from the whole marginal annulus of the lens, in place of those from the extremities of a diameter, the value of a is slightly increased, and the image about o of a distant point of light is a bright circular area surrounded by a series of rings. (The rings are caused by recurring coincidence of the phases of the partial waves at certain distances from o greater than a .) The bright centre and rings are identical with those seen in the well-known experiment in which a bright point appears in the centre of the shadow of a disc, illuminated by a bright distant source.

If the whole area of the lens is employed the diameter of the bright centre is further increased, but the intensity of the rings is much reduced. This case, as it applies to telescopes, has been considered by Airy in a paper on "The Intensity of Light in the Neighbourhood of a Caustic," published about the middle of the last century.

In whatever manner, however, the lens is used, whether with a central stop, or with its whole area uncovered, a/f and a are the least angles and distances which separate the geometrical focus from the truly dark boundary of the image, although, owing to the rather rapid diminution of intensity from the centre to the circumference of the image, it is possible to recognise angles smaller than $\lambda/2D$ (this applies to telescopes in which a is always small), or distances less than $\lambda/2$ by the use of microscopic objectives of large angular aperture, where $\sin a/2$ approaches unity.

The appearances presented at the focus when two

objects in such close proximity are examined can scarcely be described as the images of the objects, but rather as interference phenomena which require interpretation.

A. MALLOCK.

9 Baring Crescent, Exeter.

Discoveries in Tropical Medicine.

IN NATURE of April 29, p. 549, Sir E. Ray Lankester criticises an obituary notice on Sir Patrick Manson which appeared in the *Times* of April 10. The statement chiefly objected to is that "modern tropical medicine was born the day that Manson discovered the part played by the mosquito in the transmission of *Filaria sanguinis hominis*." But all parasitologists know that it was Manson who, forty-four years ago, proved by experiment the part played by mosquitoes in the propagation of filariasis. This discovery is not only the pride of tropical medicine, but the very breath of modern medicine and one of the most glorious achievements of British science.

Strange to say, Sir Ray Lankester to-day repeats, almost word for word, a mistake he published twenty years ago in the *Times*. He says: "The fact is that Manson's 'suggestion' that the *Filaria* of elephantiasis [*sic*] is actually carried by mosquitoes from the blood of one person to that of another remains to this day a 'suggestion.' It has not been established as a fact."

It is surprising that a naturalist of repute, who writes frequently on matters medical, should make this mistake. Surely Sir Ray Lankester must have come across books on parasitology written within the last twenty or thirty years. How can he say that the agency of the mosquito, in the dissemination of filariasis, has not yet been established? Was it not surmised by the Chinese ages ago? Was it not suggested by Bancroft of Brisbane (Queensland) in 1877? Was it not independently and experimentally proved by Manson, in China, that very same year? Only one thing remained uncertain for some years, and that was the actual method by means of which the young filariæ, after reaching a certain stage of development within the body of their insect host, left the mosquito to invade man; but this was fully established and admirably demonstrated between 1899 and 1900 by Manson, Low, Bancroft, Annet, and Dutton, James, Noë, Grassi, myself, and others. Dr. Low's beautiful celloidin sections of infected mosquitoes showing the worms, either quiescent between the massive thoracic muscles of the insect, or actively migrating to the latter's mouth parts and fixed while passing beneath the cephalic ganglia, gliding down within the labium, or escaping through a rent between the labella, at the distal extremity of the labium, have been exhibited repeatedly, not only at the Royal Society and elsewhere in this country, but also, on several occasions, in France, Italy, and Belgium. Moreover, they have been photographed and even reproduced in colour by skilled artists, and both photographs and drawings have appeared in books on tropical medicine and parasitology published since 1900.

Sir Ray Lankester refers to two other vexing questions, namely, the discovery of the part played by mosquitoes in the propagation of the intermittent fevers and that of tsetse flies in the transmission of certain trypanosome diseases of animals and man such as nagana and sleeping sickness. With regard

to malaria, he says: "The man who actually discovered the fact of the carriage of malaria germs by a mosquito and the particular species (*Anopheles maculipennis*) so concerned, as well as important facts as to the multiplication of the malarial parasite in the gnat's body, is Sir Ronald Ross." This short statement includes several mistakes. In the first place, *Anopheles maculipennis* is not the only anopheline concerned in the transmission of malaria, because several species belonging to the genera *Anopheles*, *Myzomyia*, *Pyretophorus*, *Myzorrhynchus* and *Cellia* play an active part in the transmission of the intermittent fevers, within their respective habitats. Then again, *Anopheles maculipennis* was unknown to Ross in India for the simple reason that it is not to be found anywhere within the Indian triangle. *Anopheles maculipennis* is a Holarctic species ranging over North America and throughout Europe and extending round the shores of the Mediterranean Sea and its islands, but otherwise absent from both the Oriental and Ethiopian regions.

Sir Ray Lankester states that Prof. Laveran, the discoverer of the malaria parasites of man, had already previously suggested mosquitoes as the carriers of paludism. I can adduce much older evidence to prove that in malarial stations the natives long suspected the mosquito as the probable cause of infection, just as the tick was suspected of being the carrier of relapsing fever and the body louse the vector of typhus, because the name of the dread "Fever-fly" is inscribed in cuneiform characters on a Babylonian clay tablet of thousands of years ago, now preserved in the British Museum. But Sir Ray Lankester ignores Manson's brilliant interpretation of the "flagellating" malarial parasite, looked upon by the Italians as a form of degeneration; by Manson as the prelude to a further all-important developmental stage outside the body of man. He overlooks the fact that Ross's investigations were inspired by Manson, and that Ross was all along instructed, aided, and supported by Manson. I need but quote Ross's own words in the paper he sent to the French Academy of Medicine, January 24, 1899: "*Pour éviter tout commentaire erroné, qu'il me soit permis de déclarer ici que mes travaux ont été entièrement dirigés par Manson, et que j'ai eu l'assistance de ses conseils et de son influence à toute occasion.*" During the whole period of Ross's work in India, I was almost daily at Manson's house, where I had the privilege and good fortune of being able to follow step by step the unfolding of one of the most wonderful chapters of tropical medicine. I was allowed to read the correspondence, examine the specimens sent by Ross to Manson, and discuss every detail. I do not wish to minimise in any way the importance of Ross's work. Humanity and science are greatly indebted to Ross for his splendid researches and no one appreciated this more than Manson, but all the world knows that Manson was the man at the helm; Ross himself has stated it quite frankly and honourably in his writings.

It was Manson who first clearly grasped the problem, it was Manson who planned the *modus operandi*, it was Manson who chose the man who should carry out his ideas and do the work, and, when failure threatened, as in the case of the Italians, when they attempted to solve the problem, it was again Manson who saved the situation, by suggesting that the researches be continued with the *Plasmodium* parasites of passerine birds. Indeed, the life cycle studied and unravelled by Ross was that of *Plasmodium danilewskyi*, a blood parasite of sparrows and not that of any of the malarial parasites of man. It was in Italy, by Profs. Grassi, Bignami, and

Bastianelli, that it was actually proved that the malaria parasites of man go through exactly similar transformations and migrations as those of *Plasmodium danilewskyi*; not, however, in any of the *Culicinae*, as Ross had proved for the bird parasites, but in a different subfamily of mosquitoes, the *Anophelinae*. Finally, the experiments carried out by Manson in London and by Dr. Low and myself in the Roman Campagna, in 1900, put the last brick in the structure of proof and were especially important in proving that, under natural conditions, the intermittent fevers cannot be contracted in any other way than through the stab of *Anopheline* mosquitoes. However, it is only right to say that Ross's experiments were undertaken at Manson's request, for the sole purpose of elucidating the etiology of human malaria, that Ross began by using the *plasmodium* parasites of man, and that the first mosquitoes he infected and examined were "dappled-winged" mosquitoes, that is to say, in all probability, *Anopheline* mosquitoes. Great discoveries are seldom made by a single man. Ross would never have done this work had he not come across Manson, and probably Manson might have had long to wait for the establishment of his theory had he not found Ross.

With regard to sleeping sickness, Sir Ray Lankester is no better informed. Sir David Bruce did certainly prove that "nagana," the horse disease of Africa, is caused by the same kind of parasite—a trypanosome—discovered by Evans, eleven years previously, in "surra," the horse disease of India, and he repeated very fully the experiments previously made by Dr. David Livingstone and by others to ascertain whether the African natives were right in suspecting the tsetse flies as carriers of the infection, but, unfortunately, Bruce gave wrong interpretations to his own experiments and to those of others, contending that the fly acted merely as a passive carrier, "just as a vaccinating needle"—these are his very words.

It was Prof. Aldo Castellani who first demonstrated the true causative agent of sleeping sickness—a trypanosome which he found not only in the blood, but also, and chiefly, in the cerebro-spinal fluid of sleeping-sickness patients. At the same time, Prof. E. Brumpt of Paris and I, independently, simultaneously, and some months before the publication of Bruce's work, incriminated a tsetse fly as the carrier of sleeping sickness. Prof. Brumpt suggested *Glossina morsitans* as the probable vector, basing his belief partly on Castellani's discovery and partly on his own extended field work in the French Congo. I incriminated the dusky tsetse fly (*Glossina palpalis*), basing my opinion on a careful study of the peculiar topographical distribution and other striking epidemiological features of the disease, on analogy with the better known epidemiology of nagana, and on the bionomics and distribution of the then known species of tsetse flies. At the International Conference on Sleeping Sickness (London, 1907) and at several meetings of the British Medical Association, I endeavoured to prove that the "sexual" dimorphism noticed first by me in the trypanosomes of sleeping sickness (specimens forwarded by Dr. Low to the School of Tropical Medicine) and a critical study of Bruce's experiments on both nagana and sleeping sickness showed that the respective trypanosomes go through a necessary cycle of development (sporogony) within the body of their definitive insect-hosts, analogous to that of malaria parasites in the body of mosquitoes. Six years later (1909) Klein's careful researches in Africa fully proved the justice of my interpretations.

LOUIS W. SAMBON.