Egyptians looked like in the third millennium B.C. Of no other people at so remote an age do we know so much, and we may well bless that pious care for the

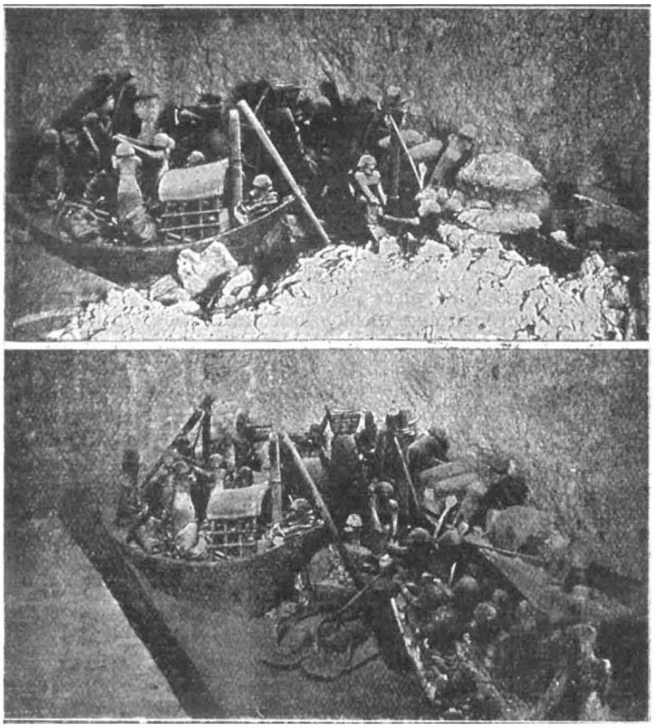

FIG. 2.—(a) Interior of Tomb as discovered.
(b) The same after removing the Débris.

ancient dead which provided them with these little representations of their life on earth.

Very few slips of any kind have crept into the text, but we notice one on p. 169, in which it is said that the names of the vases and other offerings painted on the coffins illustrated in Fig. 171 "are given in difficult hieratic writing." The names shown are in linear hieroglyphics, and are quite easy to read. The

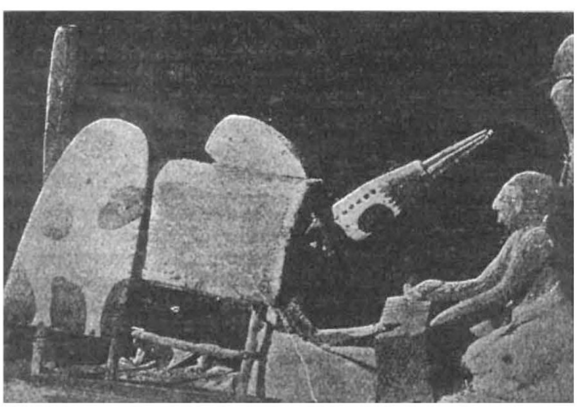

FIG. 3.—Officers playing Draughts on board ship. Model from a Tomb.

only unworthy photograph in the book is Fig. 4, in which the cliffs illustrated are by no means clear.

At the end of his preface, Prof. Garstang says that his assistant, Mr. Harold Jones, is now leaving him, to pursue an independent path as a painter. This is to be regretted from the point of view of archæology, as Mr. Jones would, as his work with Prof. Garstang has shown, have been a valuable recruit to the ranks of the excavators.

H. R. HALL.

SLEEPING SICKNESS.[1]

WHEN the campaign against malaria was commenced, our knowledge of the parasitic agent of that disease was practically complete, and in no essential particular has our knowledge of the mode of transmission changed since the discovery of the anopheline-malarial cycle. But when we consider sleeping sickness the matter is very different. Our knowledge of trypanosomes is even yet in its infancy. It has, for instance, been asserted over and over again that sexual differences exist in trypanosomes, and on this basis have been constructed developmental cycles which indeed may exist, but in proof of which the evidence hitherto adduced has been practically nil; and indeed two of the latest observers, Moore and Breinl, not only find no evidence of this sexual difference, at least in the blood, but describe two new phases of trypanosomes, viz. a so-called minute latent form, which comes into existence mainly when the ordinary forms from one cause or another have disappeared from the peripheral circulation, and resistant cystic forms, which appear when an animal is treated with atoxyl.

We have, according to these authors, a cycle of the trypanosome going on in the body hitherto unsuspected, and we also have encystment of trypanosomes under injurious influences. If this be true, it shows that, unlike malaria, we know but little of the complete life-cycle of trypanosomes, for of these forms we know so far only of their bare existence. This discovery, then, opens the whole question of the life-cycle of trypanosomes, including the question also whether there are sexual forms or no. There are further questions which are equally obscure. While, in the case of malaria, shortly after the discovery of the all-essential importance of some of the anophelines in its transmission close attention was paid to the habits of these mosquitoes, in the case of tsetse-flies we know about their habits comparatively little. It is perhaps an exaggeration to say that we know now no more about tsetse-flies than we did when Bruce discovered that *Gl. morsitans* transmitted the trypanosome (*T. brucei*) of ngana, but at any rate we can sum up in a few words what we know of the habits of the fly:—(1) The only place so far discovered where the tsetses deposit their larvæ has been among the roots of banana-trees; (2) they haunt the scrub or bush along the margins of lakes and rivers, and are seldom found far from water. The reason for this distribution is unknown, though one might conjecture that it has something to do with their food supply. (3) The sources of their food supply are also very imperfectly known. Is blood a necessity for their existence in nature? That they pursue man voraciously is known, but what other animals do they feed on? Koch recently has confirmed the observation that they suck crocodiles' blood, and holds that this is their main if not sole food; and has even gone so far as to suggest that the destruction of crocodiles would cause the disappearance of the fly. The

[1] Proceedings of the First International Conference on the Sleeping Sickness held at London in June, 1907; and further paper respecting the Proceedings of the Conference.

destruction of crocodiles is a comparatively easy matter, as the eggs, sixty or seventy, can be collected from the nests, the sites of which are well known to the native. Possibly, however, aquatic birds would still furnish them with blood. The duration of their life, their breeding habits, the habits of different species, the conditions which give rise to "fly belts," are almost unknown. It is somewhat remarkable that so little is known, although many expeditions have now studied sleeping sickness; the fly, however, has surely been somewhat neglected. One fact of great practical importance has, however, become clear, viz. that clearing the jungle drives away the fly, and to this we shall return. When we consider next the mode by which the fly transmits the disease, we find ourselves in the midst of controversy.

One view is that the transmission is a mechanical one, *i.e.* the fly carries infection as an inoculating needle from one animal to another, and the known experimental facts entirely support this view; and, further, we have the fact that in Dourine this is the sole (? also by fleas) known method, the mechanical transmission being in this case by sexual intercourse, a method which, according to Koch, also takes place to some extent in sleeping sickness. Another view, that a developmental cycle goes on in the fly, is based mainly on analogy and on the alleged existence of sexual forms of trypanosomes in the blood, and more especially in the gut, of flies. We will not enter here into the wilderness of arguments, but point out the following facts. The tsetses used for experimental purposes have hitherto, almost without exception, been caught in nature, consequently, *ex hypothesi,* some of them must contain the trypanosome in the required hypothetical developmental stage. These flies have then been fed on infected animals, and it was found that when now fed on fresh animals the latter only became eventually infected if the period that elapsed since the last feeding on infected animals was not longer than forty-eight hours, a fact explained on the mechanical view by the statement that after this time no longer can trypanosomes be found in the proboscis. Now, if these flies, on the contrary, contained a developmental stage of trypanosome, this result is inexplicable except on one hypothesis, viz. that during the feeding on infected animals (an unnecessary procedure on this view) the flies completely get rid of all trypanosomes in the necessary developmental stages in their salivary glands (?) by the preliminary feeding on the infected animals. This objection could be met by keeping flies caught in nature for forty-eight hours more or less. If now they are capable of infecting fresh animals it would be in favour of the developmental view and against the mechanical one; if not capable of infecting it would negative the developmental view, provided, of course, sufficient experiments were made to allow for experimental error, &c.

We might point out in this connection a possible explanation of the difficulties encountered by some observers in obtaining positive results in transmission experiments. In the case of anophelines caught actually in native huts where the inmates were highly infected with malaria, we have ourselves in certain instances found only 3 per cent. of the anophelines infected with parasites, a remarkably low figure. Had these anophelines been caught in cow-houses, where they often abound, we consider that it would have been possible to dissect thousands and find none infected. Now, in the case of tsetse-flies, they are not found in houses, but live in the open, so that unless the flies have bitten man they will not become infected with the trypanosome (unless, indeed, they have bitten some other unknown host), and if the flies used in these experiments are collected from parts of the bush where they have not bitten man (or other host of the trypanosome), it would be quite conceivable that thousands of flies might be used in transmission experiments with negative result, and even if they had an opportunity of biting man it is still conceivable that the number of infected ones might be very small if we consider the fact of the low figure of 3 per cent. for infected anophelines found by us in certain highly malarious districts.

If we consider the matter from another standpoint we see that again our knowledge is wanting. What is the source of the *T. gambiense?* Is it purely a man-to-man infection, as we believe to be the case in malaria, or can the fly convey the trypanosome to man from various animals? This would seem to be likely, for experimentally the fly has been proved to transmit to monkeys, so that there seems to be no *a priori* reason against thinking that the flies can transmit not only from man to man, but from man to animals and from animals to man. If this is so (but arguments can be brought against this view), then it has an important bearing on the results of isolation of the sick, for the remaining healthy population may be still living amidst infected animals, domestic and wild.

In cattle in the Congo, in sleeping-sickness areas, it is believed that the trypanosome is a different one, viz. *T. dimorphon.* Even if other reservoirs of *T. gambiense* exist, it must be admitted that the removal of the sick would remove one important source of infection, whatever proportion these bear to other reservoirs (if existent) of *T. gambiense.*

We should consider, then, that this is perhaps the most important point which requires immediate solution, and it can be determined only by a long series of laboratory inoculations.

A further point for decision, as we have seen, is the mode of transmission, mechanical or developmental, or both. This, perhaps, is of scientific rather than of immediate practical importance. Thirdly, we require a careful extended study of the habits of the fly.

But although much investigation remains to be done, we may now briefly recount what is being carried out in the light of our present knowledge.

Sleeping sickness can be detected in its early stages, first, by the enlargement of glands, *e.g.* those in the neck, an almost constant phenomenon (and the glands on puncture show trypanosomes); and, secondly, by the method recently used by Koch of examining fairly thick stained blood films on several occasions. We have thus means at our disposal of detecting early cases even when the person is to all outward appearances healthy.

(1) *Isolation.*—The removal of infected persons so far as possible to localities free from the fly, where they may be suitably treated, is certainly imperative.

(2) *Inspection posts.*—The spread of the disease to non-infected areas where the fly exists by means of infected persons should be controlled so far as possible by medical examination at inspection posts along the main routes of traffic. Although no doubt some will escape detection, yet the method is one which enables us largely to control the spread of the disease.

(3) *Treatment of the sick.*—We have in atoxyl an arsenic compound first introduced by Thomas and Breinl, undoubtedly the best drug hitherto used in combating the disease. Undoubtedly cases of sleeping sickness in Europeans have been cured by it, and lately Koch, in an extended trial of the drug, has spoken in laudatory terms of its use. He recommends the giving of half-gram doses on two consecutive days at intervals of ten days, and continuing the treatment for long periods. The method is slightly different from that advocated by the Liverpool School of Tropical Medicine when it first distributed atoxyl throughout the Congo, but Koch has

only modified the dosage, and he adds his testimony to the great value of the drug.

This method, should nothing supersede it, will thus become almost as valuable as that of quinine in the treatment of malaria. In Koch's words, "Daraus geht doch aber mit aller Bestimmtheit hervor, dass durch eine geeignete Atoxyl behandlung sehr vielen Schlafkranker. das leben gerettet werden kann."

(4) *Destruction of tsetse-flies.*—This, so far as we know at present, is not directly practicable, but the flies can be driven away by cutting the jungle. The making of clearings where the natives most frequent, such as at watering places, river fords, and around villages, will certainly be beneficial.

For the present, then, we have at our disposal methods the results of which we shall soon learn. In conclusion, it is, I think, certain that when some of the disputed points indicated above are settled the campaign against the disease will be carried out with greater efficiency because based on more certain knowledge. J. W. W. S.

WATER VAPOUR IN THE MARTIAN ATMOSPHERE.

ONE of the most telling arguments which has been used against the possibility of the planet Mars being habitable has been that spectroscopists have failed to detect with certainty the presence of water vapour in the planet's atmosphere. It now seems probable that this objection will have to be abandoned, for, in a telegram recently received by Sir Norman Lockyer, Prof. Lowell announces that Mr. Slipher has got on repeated plates—specially prepared for this research—the water vapour bands *a* and near D stronger in the spectrum of Mars than in that of the moon at the same altitude.

Should Prof. Lowell's further researches confirm it, this result is one of the most important links in the remarkable chain of evidence for a habitable Mars. The photography of the canals was a great step forward, but the presence of these features was unconvincing unless it could be proved that the water to fill them in their proper seasons was available. Similarly, the seasonal increase and decrease in the dimensions of the snow-caps were thought to be conclusive evidence for the presence of water until the frozen carbon dioxide theory was advanced, although this theory left unexplained the ill-defined edges of the disappearing snowfields. But, so far as our present knowledge goes, it is difficult to see how carbon dioxide is able to produce the intensification of the water-vapour bands in the spectrum of the planet's atmosphere.

For many years, in fact since the actual existence of permanent features on the planet's surface was established, this question of water vapour—of the existence of a substance capable of producing clouds and mists—has been one of the chief points of contention among areographers. So far back as 1863 Sir Norman Lockyer, in a communication to the Royal Astronomical Society (Memoirs, vol. xxxii., p. 179, 1863), describing his observations of Mars during the opposition of 1862, stated that "although the complete fixity of the main features of the planet has been thus placed beyond all doubt, daily—nay, *hourly*—changes in the detail and in the tones of the different parts of the planet, both light and dark, occur. These changes are, I doubt not, caused by the transit of clouds over the different features." The drawings accompanying the memoir illustrated the changes mentioned, and confirmed the suspicions of cloud effects noticed by Secchi in 1858. But the assumption that these effects were caused by clouds and mists entailed the assumption of the presence of water vapour in the planet's atmosphere, and the spectroscopic evidence for this has hitherto been too indefinite. Suspected by Huggins and Vogel in 1867 and 1873 respectively, its presence was negatived by the subsequent spectroscopic researches of Campbell and Keeler, but now it appears certain, from this latest result from the Lowell Observatory, that water vapour is one of the concomitants of the Martian atmosphere.

In his recent book, "Is Mars Habitable?" reviewed by Dr. Lockyer in NATURE for February 13 (p. 337), Dr. Russel Wallace insisted on the absence of spectroscopic evidence as a strong argument against the presence of water vapour. This objection is now removed, and once more it becomes reasonable to suppose that the Martian surface is, at least to some extent, supplied with that compound which, to terrestrial minds, is one of the essentials of habitability. At the same time, the theories advanced by Prof. Lowell to explain the remarkable variety of appearances and changes from season to season, disclosed by his wonderful observations, have received support worthy of their brilliant conceptions.

WILLIAM E. ROLSTON.

NOTES.

IN an announcement in last week's NATURE it was stated that Prof. Kamerlingh Onnes had succeeded in liquefying helium. It should have been stated that the gas was solidified, no intermediate liquid stage being observed. The demonstration was made in the presence of Prof. H. A. Lorentz and Prof. J. P. Kuenen, both of the University of Leyden. The method adopted is described by the Leyden correspondent of the *Daily Telegraph* (March 10) as follows, and is the same as that used with success by both Sir James Dewar and Prof. Olszewski. The only noteworthy point is the large amount of helium used for the instantaneous expansion. "To make this experiment," Prof. Onnes says, "I placed a tube with thick sides, containing a thinner one for extra protection against external warming influences, in a vessel filled with liquid hydrogen, at $-434°$ F., and in this tube about one and a half gallon of helium was compressed under 100 atmospheres. On allowing expansion to a lower temperature a cloud appeared, which increased as the expansion *in vacuo* continued. Out of the nebulous mass a white flocculent substance gathered in the inner tube, where—although the tube was well closed—it evaporated within twenty seconds. Some solid substance, however, was left, the pressure in the tube meanwhile rising to one atmosphere, and when the valve was opened and the pressure was reduced this substance exhaled almost immediately, no sign of liquefaction being observable. The substance which remained at a temperature of $-434°$ F. was solid helium." We are glad to be able to print the telegraphic message sent to Sir James Dewar by Prof. Onnes on March 5, and Sir James Dewar's reply to it:—Prof. Onnes to Sir James Dewar, Royal Institution, London: "Converted helium into solid. Last evaporating parts show considerable vapour pressures, as if liquid state is jumped over." Sir James Dewar to Prof. Onnes, University, Leyden: "Congratulations. Glad my anticipation of the possibility of the achievement by known methods confirmed. My helium work arrested by ill-health, but hope to continue later on."

THE council of the British Association has nominated Prof. J. J. Thomson, F.R.S., as president of the association for the meeting to be held next year in Winnipeg,