departing at the other, having rapidly garnered their prey from the rafters."

The chapter on "A Plain Man's Philosophy" almost recalls to one the musings of Prospero on a somewhat similar isle; that on "Silences" is worthy of R. L. Stevenson; "His Majesty the Sun" brings home to one the peculiar quality of the climate of northern Australia, which makes that region a country suited to the rearing of a white race, and therefore wholly different from the economic conditions of tropical Asia and Africa.

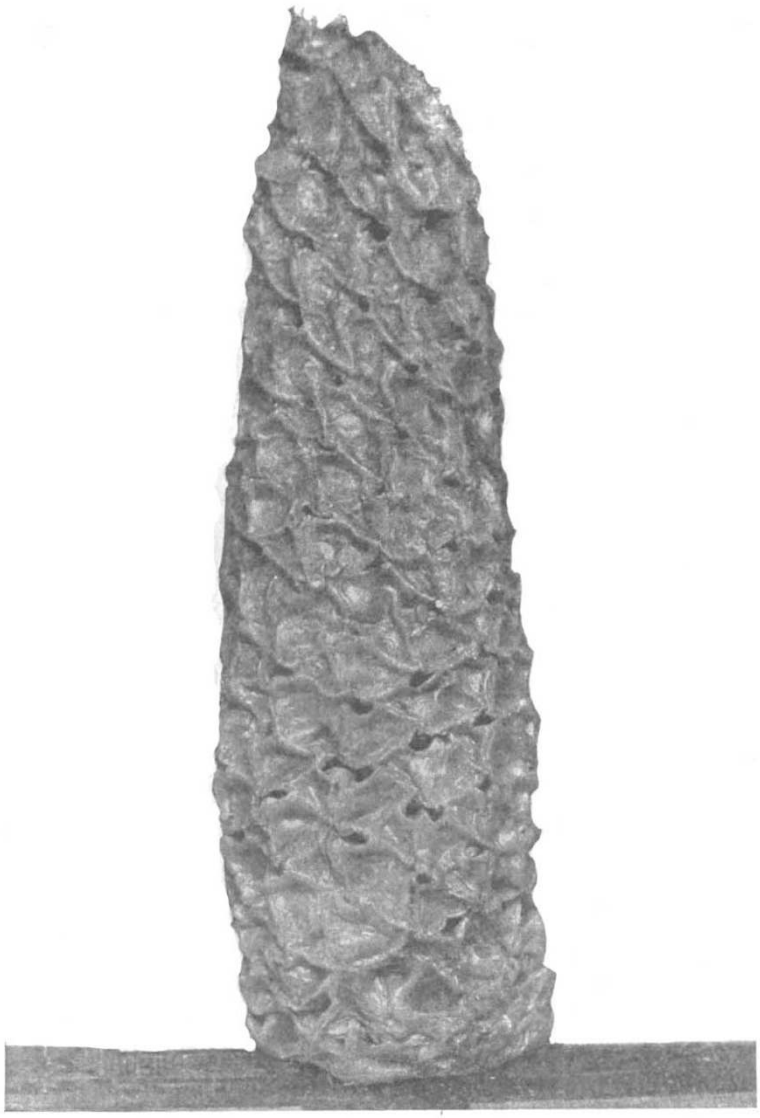

FIG. 2.—Egg Capsules of Bailer Shell. From "My Tropic Isle."

We are told much about sea-worms and sea-cucumbers, marvellous fish, so marvellous, in fact, that if there were not photographic reproductions done from the life to support the descriptions we might think the latter overdrawn. Delightfully described are the interdependent relations between the giant anemone and the painted fish (Amphiprion).

"The good fellowship between the dainty fish—resplendent in carmine, with a broad collar, and waistband of silvery lavender (or rather silver shot with lavender) and outlined with purple—and the great anemone is apparent. If the finger is presented to any part of the latter, it becomes adherent; or if the anemone is not in the mood for food, it curls and shrinks away with a repulsive demeanour. But the beautiful fish on the least alarm retires within the many folds of its host, entirely disappearing, presently to peep out again shyly at the intruder. It is almost as elusive as a sunbeam, and most difficult to catch, for if the anemone is disturbed it contracts its folds and shrinks away, offering inviolable sanctuary. If the fish be dissociated from its host, it soon dies. It cannot live apart, though the anemone, as far as can be judged from outward appearances, endures the separation without a pang.

"However, it is safe to assert that the association between the stolid anemone and the painted fish—only an inch and a half long—is for their mutual welfare, the fish attracting microscopic food to its host. And why should one anemone greedily seize a fish and another find pleasure in the companionship of one of the most beautiful and delicate of the tribe?"

The account of the development of the Bailer shell (Melo or Cymbium) from "a few drops of translucent jelly—as free from earthly leaven as a dewdrop" to a very large and capacious bowl-shaped shell, emitting egg-clusters sixteen inches long and twelve inches in circumference is most interestingly told. Other chapters of biological value and great literary charm are entitled "Some Curious Bivalves," "Barrier Reef Crabs," "Insect Ways," "Swifts and Eagles," "Socialistic Birds"; besides those which describe Hamed, the pearl-fishing Arab of Jeddah, and the black Australians with their superstitions, their quaint ways and dialect, their fine physical development and naïve charm of manner.

MALARIA IN INDIA.

IT is usual to preach nowadays that plain speaking does more harm than good; but I have reason to believe that some very plain statements which have appeared in NATURE and elsewhere on the subject of Indian medical research and sanitation have had a converse effect. It is now a great pleaure to learn from the third number of the quarterly publication of the Government of India, called *Paludism,* that that Government has set aside the sum of 500,000 rupees (about 33,000*l.*) for an Indian Research Fund to study medical and sanitary problems. No other Government possesses such a magnificent opportunity to add to the common stock of knowledge on such subjects. It rules an immense population; it draws a great revenue; and it is served by hundreds of well-trained medical officers. We are not yet informed as to the details of the allotment, but funds are the sinews of science as of war, and the Indian Government will certainly never regret the step it has taken.

For a number of years past increasingly good scien-

tific medical work has been done in India, and the number of *Paludism* referred to adds to it. Major W. H. Kenrick, special malaria officer, Central Provinces, studies the effect of malaria on birth-rates and death-rates—a difficult subject, which has been considerably neglected, though it lies at the basis of prevention. He compares thirty-four healthy villages possessing a spleen-rate of only 4 per cent. and a total population of 19,064, with thirty-three "hyperendemic" villages with a spleen-rate of 80 per cent. and a total population of 10,825. The *birth-rate* in Britain is, I understand, not much affected by season; but in healthy Indian villages it seems, curiously enough, to be highest in October and November, which the author attributes to increased frequency of conception after the gathering of harvest in the first months of the year. In malarious areas, however, these are also the most feverish months, owing to the accumulated effect of the untreated autumn infections, and the result is that the most favourable conception period is delayed until June–July and the corresponding maximum birth-rate until March–May. Yet the *total* birth-rate is not much influenced, even by severe *endemic* malaria, though it is much reduced by *epidemic*, that is, exceptional malaria.

The reason for this probably lies in a consideration discussed in section 31 of my book on the prevention of malaria. In regions of high static (*i.e.* constant) malaria-frequency, nearly all the children are rendered comparatively immune at puberty, so that there should be comparatively little sickness among the *adults*—enough only, perhaps, to delay conception among the women without stopping it entirely. But in epidemic times the frequency of reinfections is sure to be so enormous (see below) that the comparative immunity will be overcome, and the sickness will suffice to reduce as well as to retard the birth-rate.

It would be interesting to ascertain by such good studies as those of Major Kenrick how far malaria checks delivery as well as conception. Regarding the *death-rate*, he finds that during the three years 1908, 1909, 1910, they were respectively 24, 22, 23, in the thirty-four healthy villages, and 38, 32, 44 in the thirty-three hyperendemic ones (plague and cholera being absent in all), and concludes that malaria-frequency of over 80 per cent. spleen-rate measure causes an increase of from 10 to 15 per mille of total death-rate. Nothing shows better the enormous anti-human effect (as it may be called) of malaria in the tropics; the single disease, benign as it is, may cause a death-rate nearly equal to the whole death-rate of London due to all causes together.

Colonel J. R. Adie, special malaria officer, Punjab, found Plasmodia in 38 out of 150 British soldiers at Delhi Fort in November, 1910, and in 29 out of 71 children there. Yet all these were undergoing "prophylactic quinine treatment" at the time. This confirms what Malcolm Watson and others have observed elsewhere. In fact, I am beginning to believe that quinine is of little use in regions of very high malaria-frequency, for reasons to be given presently. It is surprising that the military authorities have not tried a more radical preventive measure at Delhi Fort long before this, in preference to allowing such an expensive article as a British soldier to be rotted by malaria in this manner. Colonel Adie also gives a good example of the errors of inadequate sampling. The 150 soldiers were examined in five successive batches of thirty each, and the percentages found infected were respectively 10, 36, 36, 20, 23, the mean being 25 per cent. Yet important conclusions, quite disregarding such error, have been previously based in India on even smaller samples.

The simplest, though not quite exact, method of measuring malaria-frequency is by observing the frequency of enlarged spleen (spleen-rate). But as it is easy to estimate roughly at the same time the degree of the enlargement, I have advocated for several years the additional computation of the average size of spleen and average degree of enlargement found. Thus in Mauritius in 1907–8, in 30,137 children examined by a number of workers at my suggestion, we estimated that the average size of the spleen was 2·54 times the normal, the whole number of children affected being 34 per cent. of the total number examined. The table of details showed a strongly marked positive correlation between the average spleen and the spleen-rate, as exhibited in contiguous columns; but I thought that owing to several sources of error it would not be worth while to work out the relation further. This, however, has now been ably done by Major S. R. Christophers, with the aid of my figures and some of his own. He finds that the very interesting and simple linear relation $A = 1 + 0{\cdot}05S$ holds where A is the average spleen estimated by my rules and S is the ordinary spleen-rate. This is certainly a much simpler function than was to be expected; but I will not discuss it at present, as a more detailed paper is promised.

I have no space to mention several other good papers and notes, largely entomological, in the number of *Paludism*. It is doubtful whether the entomologists will entirely accept the classifications of some of the Culicidæ suggested by James and Liston in the second edition of their book. The printing and appearance of the number leave much to be desired.

Years ago, in 1898, while infecting birds with Proteosoma by the bites of *Culex fatigans*, I made some observations which showed that such experiments might easily be utilised for studying questions of immunity and pathology in malaria. My work was interrupted and never resumed; but one of the most important sidelights was the following. Out of five sparrows which originally contained a very few Proteosoma, four showed a much more copious infection a week after being subjected to the bites of heavily infected mosquitoes. At the same time the infection of these birds was not so copious as in the case of most of the originally uninfected birds which I had previously dealt with (see my paper in the *Indian Medical Gazette*, vol. xxxiv., January, 1899). This obviously suggested (*a*) that fresh bites of infected mosquitoes will cause a severe recurrence even in subjects already infected; but that (*b*) this recurrent infection, though severe, will not be so severe as an original infection, probably owing to the previous establishment of partial immunity. I have often, fruitlessly, urged the continuation of this line of work. Major Christophers now reports ("Scientific Memoirs, Government of India," No. 46) two more similar experiments which, though they are not very convincing, tend to confirm the possibility of such reinfection. But he also gives eighteen experiments which strongly suggest that the severity of the infection in the birds depends largely on the number of, and degree of infection in, the mosquitoes—as already probable for theoretical reasons.

The importance of these points is rendered very manifest by my quantitative studies in epidemiology (see article in NATURE, vol. lxxxvii., p. 467, last paragraph but two), by which it is shown, for instance, that if 50 per cent. of the people are constantly affected under constant conditions, then about 63 per cent. of them will probably be infected or reinfected every four months. In such circumstances it will be extremely difficult to keep down the fever by quinine alone, a fact which explains the failures complained of in *Paludism*, pp. 7 and 34, and elsewhere. It must

therefore be combined with mosquito reduction when the malaria-frequency is high.

Dr. C. A. Bentley has published a very good report on malaria in Bombay and its prevention. He concludes that, as was manifest from the first, the principal measure must be the reduction of the carrier, *N. stephensi*. The cost should be about 100,000 rupees a year. Our studies of malaria are, then, advancing into fine detail; but I agree with Colonel King, C.I.E., that practical preventive measures should not be postponed until we actually become quite omniscient. That will imply a considerable loss of life in the meantime. We have already waited twelve years in India.

R. Ross.

THE LONGEVITY OF ANIMALS.[1]

ONE of the most satisfactory results of the reorganisation of the Zoological Society of London is the series of papers dealing with important aspects of animal life which are now issuing from the pens of the society's officials. Instead of being content to amass, as formerly, a mere collection of as many strange beasts as possible for the inspection of the idle and curious, the officials are now directing their attention to many important points concerning the life of animals which could not have been examined, except where such unrivalled opportunities exist for their prosecution. Thus the Gardens bid fair to become a centre for important studies, while the health of the animals improves as the results are brought to bear on their treatment and housing.

In a recent paper Dr. Chalmers Mitchell has collected the available information supplied by records in the Gardens concerning the length of life and viability of mammals and birds.

On first glancing at this paper we are struck by the immense amount of information collected, and, on the other hand, by the small amount of knowledge which we really possess on so important a subject.

This deficiency is not likely to be easily eliminated, since our powers of ascertaining the actual length of life of any wild animal are, and must always remain, strictly limited. Sometimes an individual animal becomes abnormally marked, so that we observe and watch it for a period, or the unnatural life of a captive affords us material for estimating the longevity of the wild race; but such isolated observations can at the best only be regarded as approximate, and our knowledge of the longevity of the bulk of wild animals must always remain meagre.

Even our knowledge of the longevity of common domestic animals is far below the standard which might be expected. Domestic fowls are said by Dr. Mitchell to be capable of living for thirty years, yet, owing to commercial reasons, few members of the farmyard flock reach five years. It is the same with cattle. Although their potential longevity is, according to Dr. Mitchell, about thirty years, we habitually kill all our beef cattle and bulls under five, and a vast majority of our cows under twelve years. The same principle holds with all domestic breeds, the tendency being to speed up the processes of life to such an extent that the career of the organism is concluded at an artificially early date.

On the other hand, there are methods which may eventually yield a considerable increase of knowledge, and one of these is used by Dr. Mitchell, who has tabulated the numerous records kept in the prosectorium of the Zoological Society, and has calculated the average and maximum longevity of a large number of mammals and birds which have lived and died in the Gardens. From these it appears that the average duration of life of any species in the Gardens is as a rule remarkably below the maximum duration, so that, to the majority of animals, captivity, even under the care of experts and in spite of the resulting protection from enemies, is anything but conducive to great length of life. Even, however, after allowing for the undoubted shortening of life resulting from captivity, the potential longevity of mammals in general appears to be surprisingly low, and it may be some satisfaction to know that the possible duration of life in man is probably greater than that of any other mammal, excepting, possibly, the large whales.

In this respect birds seem to be fully equal, if not superior, to mammals, amongst which those who live longest are certain of the larger carnivora and ungulates. For instance, the potential longevity of lions is between thirty and forty years; a polar bear lived to thirty-three years in the Gardens, and the largest ungulates may reach fifty years.

Both whales and elephants are popularly supposed to be creatures of high potential longevity, but as regards the former, the officials of the Zoological Gardens are naturally not in a position to offer any information. As regards the latter, it appears that their reputation has been wrongly acquired, since for them Dr. Mitchell estimates one hundred years as being the probable limit, and twenty to thirty years a fair average duration. On the other hand, there are amongst birds several groups which equal or exceed such figures. A raven has been known to reach sixty-nine years, an eagle sixty-eight, while more than one parrot has been recorded to have survived to close upon or more than a century. It appears that some birds of prey may also reach 100, and that herons, swans, and geese have a high potential longevity. The ostrich, to judge by its size, ought to live as long as any other bird, but thirty-five years is considered to be an extreme age for it.

The most difficult mammals to keep in captivity are probably the insectivorous bats. For these the maximum duration of life in the Gardens has so far been only five months, but the failure to keep them alive is undoubtedly due, not to their being naturally short-lived animals, but to their great delicacy under artificial conditions.

These bats commence to breed at relatively so late a period of their life and produce so few young at a time or in any single season that the majority of them must in nature reach an age of at least five years, that is, if they are to keep up their numbers and without making any allowance for the undoubtedly high death-rate which is always prevalent amongst wild animals. Dr. Mitchell has calculated the death-rate for the London sparrows at at least 50 per cent. in a stationary population; in the mixed assemblance of vertebrates in the Zoological Gardens it has been observed to be 28 per cent., both of which figures are very much above that of human beings.

Weissman has sought to establish a correlation between longevity and reproduction, but Dr. Mitchell refuses to accept his interpretation. He believes, not that longevity has become adapted to reproduction, but that the rate of reproduction has been adapted to average specific longevity. In any case, the death-rate amongst prolific wild animals maintaining a stationary population must be stupendous; for instance, if a mouse produces only two litters a year of six young in each, then if all survive to maturity there will be fourteen mice where before there were only two, and if the population is to remain stationary

[1] "On Longevity and Relative Viability in Mammals and Birds: with a Note on the Theory of Longevity." By Dr. P. Chalmers Mitchell, F.R.S., Secretary to the Zoological Society of London. From the Proc. Zool. Soc. Lond., 1911. Published June, 1911.