the expiration of this lease the islands were relet for a period of twelve years to the North American Commercial Company, on more advantageous terms, the quota of skins being fixed for the first year at 60,000, while it has since been under the regulation of the Secretary to the Treasury.

Putting aside for subsequent mention the question of pelagic sealing, it may be observed that between the years 1871 and 1875 the number of breeding seals and young on the islands was estimated by Mr. Elliott, in round numbers, at 3,193,000. In spite, however, of the fact that this observer did not recognise that only a portion of the cows were on land at any one time, the Commission concludes that this estimate is far too high, and that 1,400,000 would have been a much closer approximation to the truth. They further state that between 600,000 and 700,000 seems to be a fair estimate of the number of breeding females resorting annually to the islands between the years 1871 and 1885; while at the present time (1896-97) the number is only about one-fifth of what it then was.

As regards the decline of the Pribyloff herd, the best evidence is afforded by the fact that whereas between the years 1871 and 1885 no difficulty was experienced in obtaining the full number of 100,000 bachelor seals of the proper age before July 20, in 1896 it was only found possible to obtain 30,000 fit for killing even by continuing the drives till July 27; while in the following year, when driving was carried on as late as August 11, only 20,890 were obtained. It is largely on these data that the above-mentioned estimate of the former number of breeding animals is founded.

The life of the female seal being estimated at from ten to fifteen years, thirteen years may be taken as an average, during ten of which she is capable of producing young. On this estimate 10 per cent. of the breeding females die of old age each winter, in addition to those which perish from other causes. The stock is replenished by the annual addition of the three-year-old females. Among the young and pups the death-rate from natural causes is very high; about two-thirds thus perishing annually before they attain the age of three years, when the females are fit for breeding and the males for killing. The most important of such natural causes are the presence of a parasitic worm on the sandy breeding-grounds, the trampling to death by the ordinary movements or fights of the adults, starvation of the pups from being separated from their mothers at a very early age, destruction by the killer-whale, and drowning during the winter storms.

In 1896 the number of females with pups on the islands was about 157,000, and in the following year 130,000. In certain rookeries the number of pups had diminished from about 16,240 in 1896 to about 14,320 in 1897, indicating a decrease of about 12 per cent., the number of harems having likewise diminished by about 10½ per cent. Although precise figures are not available, the total decrease in the number of breeding females for the same period may be put down at about 15 per cent., and that of the males fit for killing at about 30 per cent.

Although the exact number to which it is safe to reduce the breeding bulls in a rookery as compared to the cows has not yet been ascertained, it is quite certain that in the Pribyloff herd there is no reduction of the former to anything near that limit. Consequently the killing carried on in the islands cannot be held responsible for the serious reduction which has of late years taken place in the numbers of the herd. On the contrary, such thinning out of the bachelors has tended to the actual increase of the breeding herd, owing to the less amount of fighting which takes place when the bulls are reduced in number, and the consequent diminished loss of life among the cows and pups owing to such fights.

On the other hand, there is every reason for believing that the waning of the herd is solely to be attributed to pelagic sealing, in which the number of females taken is very largely in excess of the males, while for each female so killed an unborn pup is also destroyed, and in the case of those which have already bred a second pup is starved miserably to death on land. Since the normal rate of increase of the breeding herd is a little short of 17 per cent., while the natural death-rate from old age is not far from 10 per cent., it follows (without allowing for other natural causes of death among the adults) that not more than about 6-2/3 per cent. of the females can be destroyed by human agency year by year without involving the ultimate destruction of the herd. This limit has been very largely exceeded as the result of pelagic sealing, in which (in spite of statements to the contrary) it is impossible to distinguish females from males until too late; and in consequence of this the Pribyloff herd has been so reduced that neither pelagic nor land sealing yields an adequate profit on the money invested. The Commission, indeed, go so far as to say that from a commercial point of view the herd is virtually destroyed. "But this," they add, "has not involved the biological destruction of the herd. Under wise protection it may regain its former numbers." That such protection (which involves the prohibition of the killing of females, and therefore apparently also of pelagic sealing[1]) may be extended to the herd while there is yet time, must be the hope of every naturalist. R. L.

INHERITANCE OF LONGEVITY IN MAN.

THE object of this paper[2] is twofold, namely:—

(1) To ascertain whether duration of life is inherited, and

(2) To exhibit natural selection at work in man.

According to both Wallace and Weismann the duration of life in any organism is determined by natural selection. An organism lives so long as it is advantageous, not to itself, but to its species that it should live. But it would be impossible for natural selection to determine the fit duration of life, as it would be impossible for it to fix any other character, unless that character were inherited. Accordingly a preliminary inquiry as to whether duration of life is inherited or not seems needful before we consider further the plausibility of Wallace and Weismann's hypothesis. The present paper shows that directly and collaterally duration of life is certainly inherited in the male line. We believe this to be the first quantitative measure of the inheritance of life's duration. Further data for the inheritance of this character in the female line, and for the study of the inheritance of "brachybioty" or shortlivedness as distinguished from longevity are now being collected. We point out in the paper and endeavour to illustrate by examples the importance of such quantitative measure of the inheritance of life's duration for actuarial practice.

The second aim of our paper seems to us, perhaps, to have the greater scientific importance. In the presidential address at the Oxford meeting of the British Association we were told that no one had seen natural selection at work. In a criticism then published by one of us, it was suggested that every one who had examined a mortality table had seen natural selection at work. Now the meaning of natural selection is absolutely simple. All individuals die, but some, better suited by their constitution and characters to their environment than others, survive longer, and so are able, or better able, to reproduce themselves, and to protect for a longer period their offspring. To assert that natural selection does not exist, is to assert that the whole death-rate is non-selective, or is not a function of the constitution and characters of the individual. Looked at from this standpoint the existence of natural selection really becomes a truism. All that remains when we desire to see it at work is to determine the relative amounts of the selective and non-selective parts of the death-rate for individuals living under the like environment. If, therefore, individuals living under much the same conditions are dealt with, the determination of the selective and non-selective death-rates is a measure of the quantitative amount of natural selection. Now we can answer this problem in two ways. First we may take any organ, and determine whether the death-rate is a function of the size of this organ. This method, adopted by Prof. Weldon, would be the direct and best method, if the results were not apt to be screened by other factors. In the first place we have to hit upon some organ upon which vitality largely and sensibly depends; and this is not easy, for constitutional power of resisting the attacks of disease may depend upon, not one organ, but on the complex relationships of a system of organs, and in the next place the whole problem is rendered difficult by changes due to growth. In the second method we do not attempt to select any organ whatever, but select individuals having any general

[1] The writer takes this opportunity of mentioning that, misled by a summary of some of the evidence given before the Paris Commission, he was inclined in the "Royal Natural History" to pronounce pelagic sealing more humane than seal-killing on land.

[2] "Data for the Problem of Evolution in Man. II. A First Study of the Inheritance of Longevity and the Selective Death-rate in Man." By Miss Mary Beeton and Karl Pearson, F.R.S., University College, London. Received May 29. (Abstract of a paper read before the Royal Society, June 15.)

resemblance in their constitution, or in the whole complex of organs and characters, and correlate their fitness for surviving. Now relations or members of the same family are precisely such individuals. If there were no selective death-rate there would be no correlation between the ages of death of, say, brothers. If there were no non-selective death-rate, we ought to find that the correlation between ages of death of brothers takes the value determined for the coefficient of heredity in brothers, *e.g.* the '4 of stature, fore-arm, cephalic index, eye colour, &c., Actually we find it to be something sensibly less than '4. Our investigation shows that, in round numbers, about 80 per cent. of the death-rate is selective in the case of mankind. To that extent natural selection is actually at work. Combined with the quantitative measures of heredity already published, or obtained if not yet published, we can safely conclude that Darwin's theory of a progressive change due to natural selection combined with heredity applies even to mankind to an extent which can be quantitatively measured. The next stage must be an experimental one. Various types of life ought to be submitted to ordeals of a kind like to those which occur in nature, and the correlation between the powers of resistance to these ordeals existing in members of the same family or brood determined. We shall thus be able to ascertain under a variety of circumstances the relative proportions of the selective and non-selective death-rates. A careful inspection of the characters of the longer-lived families may possibly enable the trained biologist to select some organs or characters to which a direct application of Prof. Weldon's method can be made, and thus enable us to distribute, so to speak, the total selective death-rate previously discovered among its chief factors; but here it must be remembered that relationship of organs may be quite as important as absolute size. The present paper is merely a preliminary study of the selective death-rate in man; but one may venture to express a hope that in a comparatively few years, if enough workers can be found for the experimental side of the subject, we shall no longer hear natural selection spoken of as hypothetical, but rather its quantitative measure given for various organisms under divers environments.

THE CAUSE AND PREVENTION OF MALARIA.[1]

I HAVE the honour to address you, on completion of my term of special duty for the investigation of malaria, on the subject of the practical results as regards the prevention of the disease which may be expected to arise from my researches; and I trust that this letter may be submitted to Government if the Director General thinks fit.

It has been shown in my reports to you that the parasites of malaria pass a stage of their existence in certain species of mosquitoes, by the bites of which they are inoculated into the blood of healthy men and birds. These observations have solved the problem—previously thought insolvable—of the mode of life of these parasites in external nature.

My results have been accepted by Dr. Laveran, the discoverer of the parasites of malaria; by Dr. Manson, who elaborated the mosquito theory of malaria; by Dr. Nuttall, of the Hygienic Institute of Berlin, who has made a special study of the relations between insects and disease; and, I understand, by M. Metchnikoff, Director of the Laboratory of the Pasteur Institute in Paris. Lately, moreover, Dr. C. W. Daniels, of the Malaria Commission, who has been sent to study with me in Calcutta, has confirmed my observations in a special report to the Royal Society; while, lastly, Prof. Grassi and Drs. Bignami and Bastianelli, of Rome, have been able, after receiving specimens and copies of my reports from me, to repeat my experiments in detail, and to follow two of the parasites of human malaria through all their stages in a species of mosquito called the *Anopheles claviger*.

It may, therefore, be finally accepted as a fact that malaria is communicated by the bites of some species of mosquito; and, to judge from the general laws governing the development of parasitic animals, such as the parasites of malaria, this is very probably the only way in which infection is acquired, in which opinion several distinguished men of science concur with me.

In considering this statement it is necessary to remember that it does not refer to the mere recurrences of fever to which people previously infected are often subject as the result of chill, fatigue, and so on. When I say that malaria is communicated by the bites of mosquitoes, I allude only to the original infection.

It is also necessary to guard against assertions to the effect that malaria is prevalent where mosquitoes and gnats do not exist. In my experience, when the facts come to be inquired into, such assertions are found to be untrue. Scientific research has now yielded so absolute a proof of the mosquito theory of malaria that hearsay evidence opposed to it can no longer carry any weight.

Hence it follows that, in order to eliminate malaria wholly or partly from a given locality, it is necessary only to exterminate the various species of insect which carry the infection. This will certainly remove the malaria to a large extent, and will almost certainly remove it altogether. It remains only to consider whether such a measure is practicable.

Theoretically the extermination of mosquitoes is a very simple matter. These insects are always hatched from aquatic larvæ or grubs which can live only in small stagnant collections of water, such as pots and tubs of water, garden cisterns, wells, ditches and drains, small ponds, half-dried watercourses, and temporary pools of rain-water. So far as I have yet observed the larvæ are seldom to be found in larger bodies of water, such as tanks, rice-fields, streams and rivers and lakes, because in such places they are devoured by minnows and other small fish. Nor have I ever seen any evidence in favour of the popular view that they breed in damp grass, dead leaves, and so on.

Hence, in order to get rid of these insects from a locality, it will suffice to empty out or drain away, or treat with certain chemicals, the small collections of water in which their larvæ must pass their existence.

But the practicability of this will depend on circumstances—especially, I think, on the species of mosquito with which we wish to deal. In my experience, different species select different habitations for their larvæ. Thus the common "brindled mosquitoes" breed almost entirely in pots and tubs of water; the common "grey mosquitoes" only in cisterns, ditches and drains; while the rarer "spotted-winged mosquitoes" seem to choose only shallow rain-water puddles and ponds too large to dry up under a week or more, and too small or too foul and stagnant for minnows.

Hence the larvæ of the first two varieties are found in large numbers round almost all human dwellings in India; and, because their breeding grounds—namely, vessels of water, drains and wells—are so numerous and are so frequently contained in private tenements, it will be almost impossible to exterminate them on a large scale.

On the other hand, spotted-winged mosquitoes are generally much more rare than the other two varieties. They do not appear to breed in wells, cisterns and vessels of water, and therefore have no special connection with human habitations. In fact it is usually a matter of some difficulty to obtain their larvæ. Small pools of any permanence—such as they require—are not common in most parts of India, except during the rains, and then pools of this kind are generally full of minnows which make short work of any mosquito larvæ they may find. In other words, the breeding grounds of the spotted-winged varieties seem to be so isolated and small that I think it may be possible to exterminate this species under certain circumstances.

The importance of these observations will be apparent when I add that hitherto the parasites of human malaria have been found only in spotted-winged mosquitoes—namely, in two species of them in India and in one species in Italy. As a result of very numerous experiments I think that the common brindled and grey mosquitoes are quite innocuous as regards human malaria—a fortunate circumstance for the human race in the tropics. And Prof. Grassi seems to have come to the same conclusion as the result of his inquiries in Italy.

But I wish to be understood as writing with all due caution on these points. Up to the present our knowledge, both as regards the habits of the various species of mosquito and as regards the capacity of each for carrying malaria, is not complete. All I can now say is that if my anticipations be realised—if it be found that the malaria-bearing species of mosquito multiply only in small isolated collections of water which can easily be dissipated—we shall possess a simple mode of eliminating malaria from certain localities.

I limit this statement to certain localities only, because it is obvious that where the breeding pools are very numerous,

[1] Report from Major Ronald Ross to the Secretary to the Director General, Indian Medical Service, Simla. Dated Calcutta, February 16.