

by Prof. C. J. Chamberlain; Current bacteriological literature, by Prof. H. H. Waite; Normal and pathological histology, by Dr. R. M. Pearce; Neurological literature, by Edith M. Brace. Among original communications in the same branch of science we may mention an improvement in the technique of making blood-serum culture media, by Ernest C. Levy; and preparing sections of cochlea for microscopical examination, by M. T. Cook and H. H. Zimmermann.

Two new volumes have been added to the series of brochures published by MM. Georges Carré and C. Naud under the general title of "Scientia." The volumes are: "Les actions moléculaires dans l'organisme," by Prof. H. Bordier, and "La coagulation du sang," by Prof. Maurice Arthus. Each book comprises about one hundred pages, and shows the present state of knowledge of the subject dealt with in it.

THE seventh volume of the renowned "System of Medicine," edited by Prof. Clifford Allbutt, F.R.S., has been published by Messrs. Macmillan and Co., Ltd. The volume continues the treatment of the subject of diseases of the nervous system. In the eighth volume, which will conclude the work, this subject will be completed, and the full sections on mental diseases and diseases of the skin will be added. When the final volume has appeared, it will be reviewed with others not yet noticed in these columns.

THE additions to the Zoological Society's Gardens during the past week include a Purple-faced Monkey (*Semnopithecus cephalopterus*, ♂) from Ceylon, presented by Mrs. Osborne; a Common Badger (*Meles taxus*, ♀), British, presented by Mrs. F. Travers; a Zebu (*Bos indicus*, ♂) from India, presented by Mr. Smith Rylands; two Common Squirrels (*Sciurus vulgaris*), European, presented by Miss E. B. Sparrow; a Martinique Gallinule (*Jonornis martinicus*), captured at sea, presented by Mr. H. A. Pare; a Raven (*Corvus corax*), European, presented Mr. P. Stuart; two Tengmalms Owls (*Nyctala tengmalmi*) from Norway, presented by Mr. P. Musters; an Adorned Terrapin (*Chrysemys ornata*) from Central America, presented by Mrs. R. J. Aston; a Common Snake (*Tropidonotus natrix*) from Italy, presented by Mr. T. G. Gunn; a Common Badger (*Meles taxus*) from Siberia, a Common Hamster (*Cricetus frumentarius*), European, a Ring-necked Pheasant (*Phasianus torquatus*) from Mongolia, four Horsfield's Tortoises (*Testudo horsfieldi*) from Central Asia, two Blackish Sternotheres (*Sternotherus nigricans*) from Madagascar, a Japanese Terrapin (*Clemmys japonica*) from Japan, six Land Lizards (*Lacerta agilis*) from Central Europe, six Crested Anolis (*Anolis cristatellus*) from the West Indies, two Long-snouted Snakes (*Dryophis mycterians*) from India, a Common Snake (*Tropidonotus natrix*), two Common Vipers (*Vipera berus*), British, a Glass Snake (*Ophiostauirus apus*) from Southern Europe, deposited; two Common Wolves (*Canis lupus*, ♂ ♀) from Siberia, two Yellow-tufted Honey-eaters (*Ptilotis auricomis*) from New South Wales, two Nonpareils (*Cyanospiza ciris*, ♂ ♀) from North America, purchased.

OUR ASTRONOMICAL COLUMN.

TEMPEL'S COMET 1899 c (1873 II.).—

1899.		R.A.			Decl.			Br.
		h.	m.	s.	°	'	"	
Aug. 3	... 20	56	14	6	...	-25	43	7
4	...	57	26	2	...	26	14	6
5	...	58	37	9	...	26	44	34
6	... 20	59	49	8	...	27	14	29
7	... 21	1	1	9	...	27	43	48
8	...	2	14	3	...	28	12	29
9	...	3	26	9	...	28	40	30
10	... 21	4	39	8	...	-29	7	48

NO. 1553, VOL. 60]

MARS DURING OPPOSITION 1898-1899.—MM. Flammarion and Antoniadi contribute to *Astr. Nach.* (Bd. 150, No. 3581) the results of their observations of Mars during the last opposition of the planet. The work was done at the Observatory of Juvisy, with an objective by Mailhat of 0.26m. aperture and 3.8m. focal length. The magnifying powers employed were 145, 224, 308, 411, and 607, the best images being obtained with the power of 308. Tables are given showing the progressive diminution in the extent of the polar caps, and of the whitening of the land surface under varying degrees of obliquity of the sun's rays. Two plates accompany the paper, showing the whole of the details observed, and in the description of these several differences are noted in comparison with the data given by Lowell.

As indicating the probable transparency of the Martian atmosphere, mention is made of the visibility of the Mare Tyrrhenum as a black marking quite up to the edge of the disc. The number of canals seen at Juvisy has been thirty-six, the majority of which were large and diffuse. Those easiest seen were Boréosyrtes, Cerberus and Styx. Several observations of gemination were made, which it is thought will throw some light on the cause of the phenomenon.

PHOTOGRAPHY OF NEBULÆ AND STAR CLUSTERS.—At the meeting of May 3 of the Astronomical Society of France, M. L. Rabourdin, in the course of a paper on the history of the subject, showed some remarkably fine photographs of nebulae and star clusters, and he gives a description of them, and of how they were obtained, in the July number of the Society's *Bulletin* (*Bull. Soc. Ast. Fr.*, July, pp. 289-299). The instrument was the large reflector of the observatory at Meudon, which was kindly placed at M. Rabourdin's disposal by M. Janssen. It has an aperture of one metre and a focal length of three metres, and is thus admirably fitted for the photography of faint objects of extended area. In the same number (pp. 299-304), M. Janssen furnishes some remarks on the above paper, entering fully into the question of astronomical photography, in the course of which he suggests obtaining a photometric scale for the measurement of the brightness of nebulae by putting standard stars slightly out of focus, thus obtaining small circular discs on the plate instead of points, and then measuring the opacity of these circles.

EXPERIMENTAL INVESTIGATIONS ON TELEGENY.<sup>1</sup>

I. Introductory.

THE belief in telegeny, or what used to be known as the "infection of the germ" or "throwing back" to a previous sire, has long prevailed. It may for all we know be as old as the belief in "mental impressions," which has had its adherents since at least the time of the patriarchs. During the eighteenth century the "infection" doctrine was frequently discussed by physiologists, and since Lord Morton, in 1820, addressed a letter to the Royal Society on the subject, believers in "infection" have been increasing all over the world, with the result that one seldom now hears of breeders or fanciers who are not influenced by the doctrine, while physicians and others interested in the problems of heredity either as a rule take telegeny for granted or see nothing improbable in the "infection" hypothesis.

It must, however, be admitted that, notwithstanding the criticisms of Weismann and others, very different views are entertained by the believers in telegeny, not only as to the cause, but as to the results, of "infection." By some telegeny is confounded with simple reversion or atavism, while the better informed generally assume that "infection" invariably results in the subsequent offspring repeating more or less accurately the characters of the first or of a previous sire. In a breeders' journal of some standing there appeared recently under the heading "Colour of Animals" the following sentence:—"Greys show in breeding a great tenacity of assertion, as they are few in comparison to other colours in the Stud Book, but they reappear and no doubt go back to the Arab, and prove telegeny to be a fact" (*Live Stock Journal*, May 12, 1899, p. 588). This shows simple reversion is sometimes mistaken for telegeny. In support of the view that "infection"

<sup>1</sup> Experimental Contributions to the Theory of Heredity. A. Telegeny." By Prof. J. C. Ewart, F.R.S., University of Edinburgh. (A paper read before the Royal Society, June 1.)

tion" is commonly supposed to lead to "throwing back" to a previous sire many instances could be given, but the following from an article on telegony by De Varigny will suffice. De Varigny states that an ordinary cat which had kittens to a tailless Manx cat subsequently produced several tailless kittens to a normal cat of her own breed (*Journal des Débats*, September 9, 1897).

An extended series of experiments with various kinds of animals has led me to the conclusion that if there is such a thing as telegony it is more likely to result in the subsequent offspring "throwing back" to an ancestor of the "infected" dam than to a previous mate. This view of telegony (which has not been insisted on hitherto) will be made at once evident by an example. A sable collie crossed with a Dalmatian produced three pups which in their coloration are extremely like young foxhounds; instead of numerous small spots each has a few large blotches. According to the common view of telegony this collie, if infected, should next produce with a dog of her own breed one or more Dalmatian-like pups. If, however, the offspring of a collie and a Dalmatian are like foxhounds the subsequent offspring to a collie of the same colour and strain could hardly be expected to present Dalmatian characters, *i.e.* show numerous small spots. But if "infection" as a rule results in the subsequent offspring "throwing back" either to the ancestors of the sire or the dam, it will be extremely difficult, if not in many cases impossible, to distinguish telegony from simple reversion.<sup>1</sup>

But though "infection," if it does take place, is likely, as a rule, to lead the subsequent offspring to resemble the ancestors of the dam, it may in certain cases possibly lead to their "throwing back" to a previous sire. This result might follow if the previous sire happened to be highly prepotent. For example, Highland heifers often produce to a Galloway bull hornless black offspring indistinguishable from pure Galloways. If infected by the Galloway bull, these heifers might afterwards produce Galloway-like calves when mated with long-horned bright coloured bulls of their own breed.

It is now commonly believed that if there is such a thing as telegony it results from the unused germ cells of the first (or previous) sire infecting—blending with—the unripe germ cells in the ovaries of the dam. Were this possible, the subsequent progeny would in all probability in a mild way resemble the previous sire, but if this is impossible then infection—due perhaps to some obscure change in the constitution or reproductive system of the dam—is more likely to lead to more or less marked reversion to the ancestors of the dam. All my observations point to its being impossible in the Equidæ for the unused male germ cells of the first sire to infect the unripe ova. The spermatozoa lodged in the upper dilated part of the oviduct of the mare are dead, and in process of disintegrating, eight days after insemination; they probably lose their fertilising power in four or five days. There is no reason for supposing that in the Equidæ they survive longer in or around the ovary. Further, though at the time of fertilisation there may be several large Graafian follicles in each ovary containing maturing ova, all these follicles disappear long before the period of gestation is completed. The subsequent foals are developed from successive new crops of ova into the composition of which it is inconceivable any of the spermatozoa of the first sire could by any chance enter. A study of the ovaries hence tends to confirm the view that "infection" (if there is such a thing) is as likely to cause reversion to a former ancestor of the dam as a "throwing back" to a previous sire.

Having made these general observations, it will be well next to consider critically the case of "infection" communicated in the letter to the President of the Royal Society in 1820<sup>2</sup> by the Earl of Morton. Though many other instances of supposed "infection" have been recorded, Lord Morton's mare may be said to still hold the field—the theory of telegony still mainly rests on the assumption that this historic mare was "infected" by a quagga some years before she passed into the hands of Sir Gore Ouseley and produced three "colts" to a black Arabian horse. One might even go further and without much exaggeration assert that the telegony hypothesis at the present

moment mainly rests on an allegation by Sir Gore Ouseley's stud groom.

It has been generally assumed that Lord Morton's mare (a nearly purely bred chestnut Arab) was "infected" for two reasons (1) because the subsequent offspring were of a yellowish-brown colour and more or less striped, and (2) because, according to Sir Gore Ouseley's stud groom, the mane of one of the striped foals had always been upright, while in another it arched to one side clear of the neck. The presence of stripes in the subsequent offspring has never been questioned, nor yet is there any doubt that when Lord Morton in 1820 inspected the "colts" the mane in the filly was upright as in the quagga, while that of the colt resembled the mane of Lord Morton's quagga hybrid. There is, however, an absence of trustworthy evidence that the filly's mane had *always* been upright as alleged to Lord Morton by Sir Gore Ouseley's stud groom.

Were the evidence in support of this allegation satisfactory, there would I think be no escape from the conclusion that Lord Morton's mare was "infected" by the quagga. Hitherto the presence of stripes on the "colts" has generally been looked upon as affording strong evidence of "infection." Believers in telegony admit that stripes are not uncommon in Norwegian and certain other breeds of horses, but, with Mr. Darwin, they have taken for granted that they never or very rarely occur in Arabs.

I find, however, that though in Arabia dun-coloured horses are disliked and never used for breeding, stripes even in the most renowned strains are not so uncommon as is generally supposed. I have now a purely bred Arab filly of about the same colour as Lord Morton's filly, but, unlike the filly we have heard so much of, both the fore and hind legs are marked with distinct dark bars, and there are faint indications of stripes across the withers and a distinct dorsal band. The history of this filly (bred by Mr. Wilfred Scawen Blunt at Crabbet Park, Sussex, and very kindly presented to me) is well known for many generations; none of her ancestors could possibly have been "infected" by a zebra. The dun colour and stripes are doubtless the result of simple spontaneous reversion, for, unlike Lord Morton's mare, there is no history of a cross in her pedigree. This filly proves that even in high-caste Arabs of the best desert blood a dun colour and stripes may unexpectedly appear.

As to the occurrence of stripes in other breeds I could give, were it necessary, many instances. A year ago I had in my possession a light bay (or yellow dun) pony, which showed nearly as many stripes on the trunk as the Gore-Ouseley filly, and in addition had several interrupted narrow stripes on the forehead.<sup>1</sup> Moreover, the stripes on the Gore-Ouseley "colts," while agreeing with stripes occasionally seen in horses, differ in their arrangement from the stripes in the quagga. The stripes themselves are evidence of reversion, but nothing more; and seeing that pure bred horses sometimes show quite as many stripes, we are not justified in assuming that but for the dam of the "colts" having been first mated with a quagga the stripes would not have appeared.

Hence unless it is proved that the mane in the filly and colt were naturally erect, or nearly erect, the case for the "infection" of Lord Morton's mare will be lost. It may be well to quote the passage from Lord Morton's letter referring to the mane. It is as follows:—"That of the filly is short, stiff, and upright, and Sir Gore Ouseley's stud groom alleged it never was otherwise. That of the colt is long, but so stiff as to arch upwards and to hang clear of the neck, in which circumstance it resembles that of the hybrid. This is the more remarkable as the manes of the Arabian breed hang lank and closer to the neck than that of most others" (*Phil. Trans.* 1821).

I am not prepared to accept the allegation as to the manes for the following reasons:—

(1) I have had twelve zebra hybrids under observation, and in each case the mane, though erect to start with, always after a time arched over to one or both sides. The stud groom's statement, it seems to me, proves too much. If in the quagga hybrid and in all my horse hybrids the mane, sooner or later, falls to one side it is a little remarkable that in the pure bred two-year-old filly it had been always upright.

I may here mention that the hair of the mane of zebra hybrids is shed annually; it is for this reason that the mane in hybrids is never long enough to hang close to the neck.

(2) The mane in the drawing of the filly by Agassé is not

<sup>1</sup> See Fig. 36, "The Pencyuk Experiments," A. and C. Black, 1899.

<sup>1</sup> That reversion ever occurs has been questioned by Bateson ("Materials for the Study of Variation") and others, but I have already (*Nature*, February 9, 1899) proved beyond doubt that reversion can be easily induced by intercrossing distinct types, and I have recently heard of several instances of spontaneous reversion—reversion not induced by intercrossing.

<sup>2</sup> *Phil. Trans.*, 1821.

represented as upright, but as lying to one side. If the mane had remained erect during the first two years, by virtue of shedding its hairs, it could not very well have lost this habit and fallen completely over to one side subsequently, say, during the fourth year. From the mane being erect in 1820, and hanging to one side in 1821 or 1822, when Agassé's drawing was made, the presumption is that the mane of the "colts" had been cut some time before they were examined by Lord Morton.

Two years ago I had a bay Arab with a mane which was to start with short, stiff and upright; some months later it arched freely to one side, as in my zebra hybrids, and later still it hung lank and close to the neck.

(3) There is always an intimate relation in the Equidæ between the mane and the tail; when the mane is short and erect the upper third or so of the tail is only covered with short hairs, which, like the hairs of the mane, are annually shed. Lord Morton noticed nothing peculiar about the tail of the "colts," and the tail of both the colt and filly in Agassé's drawings is the tail of a high-caste Arab. This seems to me to warrant the conclusion that the filly's mane had been hogged some time before Lord Morton's visit.

It thus appears that the evidence in support of the belief that Lord Morton's mare was "infected" by the quagga is at the best far from satisfactory. The same may be said of the evidence in support of all the other supposed cases of telegony in the Equidæ—of, amongst others, Lord Mostyn's mare, referred to by Darwin ("Animals and Plants," vol. i. p. 435, 1875); of the mule-like mare in the Paris Gardens, referred to by Tegetmeier and Sutherland ("Horses, Asses and Zebras," p. 81); and of the African ass (*Equus asinus*), still in the Zoological Gardens (London), which now and then has a reddish-coloured foal, like the cross-bred foal she produced in 1883 to an Asiatic ass (*E. henionus*).

Although I am now satisfied that Lord Morton's case throws little light on the telegony hypothesis, like many others I had no very decided views on the subject some years ago, and hence when arranging in 1894 to make a collection of horse embryos, I decided to repeat, as far as circumstances permitted, what is commonly called Lord Morton's experiment. For this purpose I procured early in 1895 three zebras and a number of mares. Two of the zebras died during the winter of 1895, but the third—a handsome stallion of the Chapman variety (*E. burchelli* v. *chapmani*)—still survives and is now thoroughly acclimatised.

During 1895 I only succeeded in mating the zebra with one mare, and hence there was only one hybrid born in 1896. During the last two years, however, quite a number of hybrids have made their appearance, and the dams of several of the hybrids have subsequently produced pure-bred foals. The time has hence come when some of the results of the experiments may with propriety be communicated to the Royal Society.

"II. Experiments with West Highland Ponies." By Lord Arthur Cecil, Orchardmains, Kent, and J. C. Ewart.

The first mare mated with the zebra was a black West Highland pony (Mulatto), set apart for the telegony experiments by Lord Arthur Cecil. The better bred West Highland ponies are supposed to have descended from "Armada" horses, and are hence perhaps related to Mexican and Argentine horses, so often dun-coloured and partially striped. Mulatto's hybrid (Romulus, born August 12, 1896) is, on the whole, more a zebra than a pony both mentally and physically. He is especially remarkable in being more profusely striped than his sire (the zebra Matopo) in having a heavy semi-erect mane, which is shed annually, and in having a mule-like tail from the upper third of which the longer hairs are periodically shed. The body colour of the hybrid varies from a dark orange colour to a mouse-dun; the stripes, of a reddish-brown colour, on the head are dark brown or nearly black on the trunk and limbs.

In the number and plan of the stripes, the hybrid agrees more closely with the Somali zebra than with any of the Burchell zebras. Over the brow, e.g. there are narrow rounded arches instead of somewhat broad pointed arches as in his sire, the neck and trunk have quite double the number of stripes found in the sire, while over the croup in the position of the "gridiron" of the mountain zebra there were at birth irregular rows of spots, which in course of time blended to form somewhat zig-zag, narrow, transverse bands. The ears are nearly as large as in the sire, while the eyelashes are longer and distinctly curved. In his movements the hybrid resembles his

sire, and like his sire he is always on the alert, very active and suspicious of unfamiliar objects. Further, in his call he agrees far more with his sire than his dam. In being profusely striped, Romulus differs greatly from the quagga hybrid bred by Lord Morton, in which the stripes were fewer in number than in many dun-coloured horses.

Mulatto's second foal arrived in July 1897, the sire, Benazrek, being a high-caste grey Arab horse. Like Lord Morton's colts, Mulatto's foal by the Arab horse, in make, action and temperament, agreed with ordinary foals, but it differed from the majority of foals in presenting quite a number of *indistinct* stripes—subtle markings only visible in certain lights. These stripes differed but little from the body colour, which varied from dark bay to brown. Though few references have been made to the occurrence of stripes in foals, they are, we find, far from uncommon. As is well known, Mr. Darwin once bred a striped foal by putting a cross-bred bay mare to a thoroughbred horse. This foal was for a time marked nearly all over with obscure dark narrow stripes, plainest on the forehead, but also distinct over the croup ("Animals and Plants," vol. i. p. 60).

There is no figure of Mr. Darwin's striped foal, but from the description given there can be little doubt that the markings were more abundant than in Mulatto's second foal. In this foal (as in Mr. Darwin's) the stripes became more and more indistinct, and by November they had almost vanished. Unfortunately the foal died when about five months old, and hence it is impossible to say whether any of the stripes would have persisted. It will be evident that Mulatto's second foal helped but little to clear up the vexed "infection" problem. Mulatto missed having a foal in 1898, but she recently produced at Knole, Kent, her third foal. The sire (Loch Corrie) of this foal belongs to the Island of Rum section of the West Highland ponies, and closely resembles Mulatto. The third foal has about as many stripes as the second. As in the second, they are most distinct over the croup and hind quarters; and as in the second, they differ both from the markings in the previous sire, the zebra, and from the markings on the hybrid Romulus.

This third foal, which was born on May 6, 1899, seemed, like the second, to lend some support to the "infection" hypothesis. Fortunately, since it made its appearance, two other West Highland mares have had foals to Loch Corrie. These foals put all doubt as to the nature and significance of the stripes on Mulatto's second and third foals at an end.

One of the dams is of a brown colour, the other is nearly black. Though neither the brown dam nor the black has ever seen a zebra, both foals are marked in very much the same way as Mulatto's, and some of the stripes in one of the new foals look more like persisting than the stripes on Mulatto's third foal. Hence, in order to account for the markings on Mulatto's foal to the grey Arab, and on her foal to the black West Highland pony, it is unnecessary to fall back on the "infection" hypothesis.

"III. Experiments with Shetland, Iceland, Irish, Thoroughbred and other Ponies." By J. C. Ewart.

An effort was made to cross four Shetland ponies with the zebra stallion, but I only succeeded in obtaining one hybrid. The dam (Nora) of this hybrid closely resembles, except in size, the Island of Rum ponies—she is a small edition of Mulatto. Her first foal, by a black Shetland pony, was of a dun colour, and nearly as striped as Sir Gore Ouseley's filly; her second is the most zebra-like of all my hybrids; her third closely resembles her sire, a bay Welsh pony. For some time after birth there were faint indications of stripes over the hind-quarters of this foal, but now it is a year old there are no markings or any other suggestions of a zebra. It is not a little suggestive that the foal bred from this pony before she was mated with the zebra was distinctly striped, while the subsequent pure bred foal has no stripes.

Of five Iceland ponies put to the zebra only one produced a hybrid. This hybrid was faintly striped, and showed less of the zebra than any of the others. The dam, a prepotent yellow and white (skewbald) pony, had first of all a light bay foal to an Iceland pony. Her third foal, by a bay Shetland stallion, is a skewbald, and in the size and arrangement of the brown patches closely resembles the dam. There is no hint whatever that the Iceland pony has been "infected" by the zebra.

Several Irish mares were put to the zebra, and two of them (bays) have first produced hybrids and subsequently pure bred foals. A cream-coloured Irish-Canadian mare unfortunately died before her hybrid foal was born. One of the bay mares

had a bay hybrid richly striped; the other a hybrid with but indistinct stripes. The subsequent foals—one by a chestnut thoroughbred horse (Tupgill), the other by a hackney pony (Mars Royal)—are bays, not only devoid of stripes, but affording no indication whatever that their dams had been previously mated with a zebra.

Although I experimented with seven English thoroughbred mares and an Arab mare, I only succeeded with one—a small chestnut. This mare produced twin hybrids last summer; she has this summer a foal to a thoroughbred chestnut horse (Lockstitch). One of the twins died soon after birth, the other, richly but unobtrusively striped, in its colour and make strongly suggests his dam. The chestnut mare's new foal neither in make, colour nor action in any way resembles a young zebra nor a zebra hybrid. In 1897 a bay mare by a bay Arab horse (Hadeed) was for some months in foal to the zebra. Since she miscarried in 1896 she has had two foals to a thoroughbred horse (Lockstitch). Neither of these foals in any way suggests a zebra. In this case the unused germ cells of the zebra had presumably a better chance of reaching the ovum from which the first of the two pure-bred foals was developed than is usually the case.

Attempts were made to cross Welsh, Exmoor, New Forest, Norwegian and Highland ponies with the zebra without success, and though a cross-bred Clydesdale has twice had a hybrid, she has not yet produced a pure-bred foal. The experiments, as far as they have gone, afford no evidence in support of the teleology hypothesis.

#### INVESTIGATIONS ON MOSQUITOES AND MALARIA.<sup>1</sup>

I HAVE the honour to report the results of my observations since my arrival here on December 21, 1898.

Major Ross, I.M.S., first demonstrated and explained to me his method of dissection of the mosquito and the structures normally met with. From prepared specimens he then showed me the bodies met with after feeding these mosquitoes on birds infected with the proteosoma and the change day by day which they showed, ending with a demonstration of the germinal threads in cysts in the stomach wall, as seen in the fluids of the body and in cells in the salivary gland.

On my arrival there were in the laboratory, in test-tubes, series of mosquitoes fed on birds infected with proteosoma on the night of November 30, December 10, December 12, December 15, and December 20.

Of each of these series Major Ross dissected specimens for me after killing the mosquitoes with chloroform, and again demonstrated in these the same bodies that he had already shown me in prepared specimens; pointing out and demonstrating as he went on that in the older mosquitoes it was possible on cutting the thorax to observe the nature of the contents both of the coccidia in the stomach and of those of the cells of the salivary glands.

The points showed to me I readily observed.

From series of mosquitoes before mentioned I day by day examined both those which died and others I killed, and was as readily able myself to repeat the observations and in the earlier series to trace the changes in the size and nature of contents of the coccidia.

I also examined a large number of mosquitoes caught about the laboratory, and others which had been raised from larvæ. In no case did I find either coccidia in the stomach wall, germinal threads in the body fluids, or in the cells in the salivary gland; nor did I find "black spores" in them.

Major Ross informed me that his published results were based on observations made in the hot season when the temperature was 80° F., or more; and that now I should find the changes considerably slower, as it was the cool season, but that the sequence of events was the same.

My observations on the mosquitoes fed on December 20 and December 15 showed that this was the case, and that the coccidia advanced more slowly than the published results indicated. He also informed me that mosquitoes fed less readily and more difficulty was met with in rearing them to a spore-bearing age.

<sup>1</sup> Dr. Daniels's Report to the Secretary of the Malaria Investigation Committee of the Royal Society, London, on the results of observations made by him in Calcutta in conjunction with Major Ross, I.M.S. Dated Calcutta, January 23.

These difficulties the use of the incubator was only partially successful in obviating.

On the evening of January 1, following exactly in Major Ross's lines, I commenced a repetition of his main experiment. Two mosquito nets, free from rents, were taken, and in them were released a large number of grey mosquitoes reared from larvæ.

In the one, four birds were placed; in three of them on December 31 I had found proteosoma in large numbers, and in the fourth a moderate number.

In the other net two birds, in whose blood no proteosoma had been found, were placed; these two died two and three weeks later, and no pigment was found in their organs, and repeated examinations of their blood had failed to show proteosoma.

On January 2 none of the mosquitoes had fed, and on January 3 only two in the first net and eight in the second. On January 4, a warm night with a minimum temperature of 59.2° F., sixty-three mosquitoes were found gorged with blood in the morning, and were caught in separate test-tubes plugged with wool and placed in the incubator. Eighteen in the other net, where the non-infected birds were placed, the control series, were similarly collected; these were caught in the same manner and treated in the same way.

On the following two evenings, with minimum temperatures of 60.7° and 63.2°, sixty-eight and forty-six mosquitoes were fed on the infected birds and were kept for the preparation of specimens. Twelve mosquitoes were fed on the non-infected birds, and were used as additional controls so as to bring the number of the control on Blue Jay with numerous Halteridia.

On the third day the sixty-three, with the exception of those killed for examination or dead, were released inside a clean net free from mosquitoes, and birds free from proteosoma were also placed in it.

In the morning all mosquitoes found inside were collected, and most of them had fed well; the minimum temperature was 63.2° F.

This is the method Ross employs to re-feed the mosquitoes. If infected birds are used, you get a younger generation of coccidia; so I used sterile birds. The method works fairly well in warm weather; but there is always some loss, as the full number are not collected again in the morning. As the process is repeated over and over again, this loss becomes serious, the more so the longer the period required for maturation. In a frequently repeated process of this kind there is always the possibility of an outside mosquito getting in.

The mosquitoes were not fed on the following night, as they were full of blood; but most of them voided it during the night, and many died next day.

The remainder were given the opportunity of re-feeding every night after this; but a spell of cold weather ensued with minimum temperatures of 44° F.-49° F.; only on one night did it exceed 50° F., and on these nights few fed well or at all, and there was a consequent continued heavy mortality, only one being alive on the tenth day, and that subsequently escaped in the night.

This method of feeding is very unsatisfactory in exceptional weather of this kind; the mosquitoes in the day are kept warm in the incubator, and rapidly digest their food, whilst at night the cold renders them torpid and they do not feed.

The control mosquitoes were treated in exactly the same manner and fed on birds free from proteosoma. The last died on the thirteenth day.

The results of the two series are as follows:—

Sixty-three fed on proteosomal birds.

Forty-nine examined, three reserved for sections, one too much decomposed for satisfactory examination.

Ten not accounted for, lost in the nets.

Of the forty-nine examined, two were killed on the first day—that is, under twenty-four hours, and possibly under twelve hours, after they had fed. No coccidia were found in these. Two more were examined the following morning, under thirty-six and possibly under twenty-four hours after they had fed; no coccidia were found in these.

In two examined about 4 p.m., the minute pigmented coccidia were found; that is, under forty-six and possibly not more than thirty-four hours after they had fed on the infected birds.

The remainder were examined on the following days, the largest numbers, eighteen, on the fourth day and twelve on the seventh day, as on these two days those numbers died.