

Cuckows' Eggs

WILL you kindly allow me space to thank Prof. Newton for the trouble he has taken in replying to my inquiries, although I must confess I am still unconvinced?

My omission of the name of the eminent oölogist in my last letter was entirely accidental, for I had no purpose in concealing it, but rather the reverse. My quotation was from a letter of Mr. Hewitson's, in the *Field* of March 17, 1868.

Mr. Newton mentions the eggs of the Black Cap Warbler and the Tree Pipit, as some indication of the existence of a condition which I doubted in my sixth question. I have not found the eggs of the Black Cap vary more than this, that in some the ground colour was of a warmer tone than in others. The eggs of the Tree Pipit, I freely admit, do vary greatly, but their variations are all confined to different shades of nearly the same colour—viz., purple; ranging from purplish red on the one side, to bluish purple on the other, but these variations have, nevertheless, so much similitude that there is no difficulty in at once recognising them.

Mr. Newton says: "If the eggs in question were not cuckows', what birds laid them?" My reply is, simply, that they were laid by the birds in whose nests they were found. It seems to me far more likely that an egg laid by a certain bird should vary slightly from the rest of her eggs in the same nest, than that another species should lay eggs varying to the extent mentioned by Dr. Baldamus—viz., from vinous red to greenish blue, olive green, plain brown, &c., or even pure white, or light blue green, mentioned by Degland and Gerbe, as quoted by Mr. Newton.

Mr. Newton will excuse me for saying that I did not refer to the German authors mentioned by him in the footnote to his letter, excepting where quoted by Dr. Baldamus, for unfortunately I do not possess a knowledge of the German language, and am therefore unacquainted with their writings.

The doubts I have expressed, and still feel, have nothing personal in them, but only apply to the theory and the evidence on which it is supported. It does seem to me singular that these extreme variations of colour in the eggs of the cuckoo should only have been remarked in Germany. They do not appear to have been observed in Britain. Mr. Newton does not say he has found them himself, and admits that the evidence on which these German eggs are pronounced cuckows' might have been more satisfactory. Mr. Hewitson says "few eggs differ less," and Mr. Dawson Rowley has remarked, in a letter to the *Field*, "I believe few men have taken with their own hands so many eggs of *cuculus canorus* as myself;" and yet his experience does not confirm the theory, but the contrary.

I cannot help feeling that we still want more positive information on this point. Were all the varied eggs alleged to be cuckows' really laid by that bird? I can easily conceive an enthusiastic naturalist, with a favourite theory to maintain, imagine when he takes out of the nest of the hedge-sparrow, or tree pipit, an egg rather larger than the rest, but marked and coloured in a similar manner, that it is that of the cuckoo. I hold, however, that nothing less than *positive proof* that it was deposited by a cuckoo will suffice. I admit this may be difficult to obtain, but it is not the less necessary. A dogma like the one in question must be based on evidence that is not only unimpeachable, but above suspicion, and this I think the advocates of the theory have not yet furnished.

May I ask you to be good enough to allow my orthography of the word "cuckoo" to remain? With all deference to so high an authority as Prof. Newton, I prefer and always use the common mode of spelling the word to the one adopted by him, as better representing the call-note, from which the name is derived.

W. J. STERLAND

January 17

Dr. Livingstone's Discoveries

IN the conclusion of a letter which has lately appeared in your journal on the subject of Dr. Livingstone's recent letters, Dr. Beke gives the opinion that the river and lake chain which forms the main part of the great traveller's latest discoveries, is the head stream of the Nile. Though I am unwilling to differ from such an authority as Dr. Beke, yet there appear to me to be considerable difficulties in the way of his conclusions.

Will you allow me to show how it seems equally, if not more probable, that Dr. Livingstone, whilst he has ascertained the sources of the Nile, has also the merit of being the discoverer of the head streams of one of the great rivers which flow to the Atlantic, perhaps of the Congo. The Chambeze, the head stream

of the lake chain in question, has its rise somewhere in the eastern part of the great plateau or ridge which skirts the whole side of Africa, next the Indian Ocean. Dr. Livingstone crossed it in lat. $10^{\circ} 34'$ south; from this it flows first westwards to Lake Bangweolo, then north to Lake Moero. The position of Lake Moero can only be determined as yet by reference to that of Lunda, the capital of the kingdom of the Cazembe, twelve miles beyond which town the lake is said to begin. Portuguese travellers are the only Europeans who are known to have previously visited this town, and the two routes from which we can assign it a position on the map, are those of Dr. Lacerda in 1798, and of Major Monteiro in 1831. These two travellers, with their escorts, have passed over almost the same route from Tete on the Zambesi to the Cazembe. From the former traveller there remain two astronomically fixed positions in the middle of this route, and the latter has published a volume which contains the distances and directions of his journey, but no astronomical positions. The route of Monteiro then, justified by the now ascertained position of Tete at the beginning, and by the positions formerly determined by Lacerda for its middle course, gives the place of the Cazembe town of Lunda, at its termination, in lat. $8^{\circ} 40' S.$, lon. $28^{\circ} 20' E.$

Dr. Livingstone describes Lake Moero as beginning twelve miles below this position and extending for fifty miles to northward. Since he proceeded north from Cazembe town along the eastern shore of Lake Moero, in his attempt to reach Ujiji in the end of 1867, the great bulk of this lake must lie to westward of the meridian of Lunda. The centre of Moero would then be in the latitude of the south end of Tanganyika, and at about 120 miles to westward of its longitude. Dr. Livingstone has seen the river at its outflow from this lake and also at the point where it emerged from the "crack in the mountains of Rua," when, according to his own observation, the river turned to north-north-west to form Ulenge, a third lake or marsh in the country west of Tanganyika.

This north-north-westerly direction would carry this river quite out of the line of Tanganyika or of the Albert Nyanza; besides, both of these lakes appear to be closed in on the western side by high mountains.

The levels of the river also appear to present a great obstacle to its joining the Nile lakes.

Leaving the Valley of the Loangwa, Dr. Livingstone tells us that he ascended to a great plateau which extends for 350 miles square, southward of Tanganyika. This table-land is at an elevation of from 3,000 to 6,000 feet above the sea. The valley of the Chambeze crosses this plateau from east to west, and the river descends from it into the great valley of the Lakes Bangweolo and Moero, not far west from the point where it was crossed by Dr. Livingstone. The valley of the Chambeze is no doubt one of the greatest hollows in this plateau, and so the bed of the river here may be taken to be at the lowest general height of the plateau given by Dr. Livingstone—that is, 3,000 feet, or 200 feet above the Tanganyika. From the point at which the Chambeze was crossed, its course is for perhaps 200 miles westward to Lake Bangweolo, and in this part of its flow from the plateau to the valley the fall of the river must be considerable. Between Bangweolo and Moero the course of perhaps 120 miles to northward seems to be through a more level part of the valley. Still, here there must be another descent to Lake Moero. According to the Portuguese traveller, Monteiro, the kingdom of the Cazembe extends on the east and north-east to the land of the Auembas, apparently the same as the Luwemba of Burton and Speke on the south-east of Tanganyika. His country is described as low and flat, and this would seem to be confirmed by the absence of current in the marshy rivers visited by the Portuguese to the east of Cazembe's town, and also by the Lake Liemba of Dr. Livingstone, which he has found to be the termination of a long river-like arm of Tanganyika, stretching south-south-east to the north edge of the before-mentioned plateau. Lake Moero, then, cannot be above the level of Tanganyika, else its outflow would surely be over this level country, and not through the mountains to northward. From Lake Moero the river flows on through a "rent in the Mountains of Rua." In passing through this gorge, it appears certain that the river must have a further and rapid descent, lowering its bed still more beneath the level of Tanganyika.

In his letter of 30th May, 1869, from Ujiji, which has the brevity of a telegram, Dr. Livingstone says: "Tanganyika, Nyige Chowambe (Baker's) are one water, and the head of it is 300 miles to south of this. The western and central lines of drainage

converge into an unvisited lake west or south-west of this." If the expression "one water" here means that these two lakes are united by an extension of one into the other, and not by a river, then it is evident that the river and lake chain under consideration can never flow up to join either of them after having passed down through the rent in the Mountains of Rua; if it means that these lakes are joined together by a river, still the small difference in height between that computed for Lake Tanganyika by Mr. Finlay, of 2,800 feet (afterwards so curiously confirmed by Livingstone's height of Lake Liemba), and that found for the Albert Nyanza by Baker, would not give a sufficient lowness to the latter lake to allow this river to flow down to it through the five degrees of latitude which separate its outfall from the Mountains of Rua, from the southern end of the Albert Lake. Dr. Livingstone's statement in his letter above quoted from Ujiji, that the head waters of the Tanganyika and Albert Lakes are 300 miles south of that place, is not at all opposed to the view that the Chambeze River and its lake chain may join the Congo, for the streams which flow into his Lake Liemba may rise at this distance from Ujiji. In this case the sources of the Nile would be side by side with those of the Congo; and the man who has the claim to be called the greatest explorer that the world has ever known, has the double honour of having solved the two greatest of African problems.

74, Strand, W.C.

KEITH JOHNSTON, Jun.

Physical Meteorology

ASSUMING with your correspondent that there is an ascending current in the heart of a cyclone, no doubt latent heat will play its part. I presume, however, your correspondent does not imagine that air is ever actually heated by such means to 370° F.

Suppose, for instance, that two cubic feet of saturated air, both at thirty inches pressure, but one at the temperature 32° F. and the other at the temperature 90° F., become mixed. The cubic foot at 32° F. will contain 2·37 grains of vapour, that at 90° F. 14·50 grains. Hence, after mixture the average weight of vapour in unit of volume will be 8·43 grains. This would saturate a temperature = 71·7° F. But this is greater than the mean between the two temperatures or 61° F. There will, therefore, be hardly enough heat to keep the mixture at 71·7° F. and prevent deposition.

On the other hand, we cannot imagine the temperature of the mixture to fall as low as 61° F.

The temperature of the mixture will therefore, I presume, be greater than 61° and less than 71·7°.

B. STEWART

Veined Structure in Ice

FEW men have had better opportunities of examining glacial phenomena than Mr. Whymper, and his explanation of the veined structure is certainly an ingenious one. I venture, however, to doubt whether it can be regarded as generally satisfactory, although, possibly, it might explain some isolated cases.

The following, which, so far as my experience goes, are common facts in glaciers, appear to me difficult to reconcile with his explanation.

(1.) One common case in which the veined structure becomes conspicuous is after the glacier has been pressed into a narrower channel than has been occupied by its *névé*. The structure planes are then roughly parallel to the *sides* of the channel. Dr. Tyndall has pointed this out in his "Glaciers of the Alps," p. 387, and I have frequently observed the same thing myself. Three instances occur to me at this moment: one on the Gorner Glacier, under the Gorner Grat; another in the middle part of the Glacier de la Pilatte (Dauphine); a third on the upper part of the Mer de Glace. Did I search through my note-book I have no doubt I could find plenty more. If now, say in the second example, the veined structure was due to the crevasses in the ice fall below the Col du Sélé, surely its planes would hardly be twisted through a right angle in the comparatively short distance intervening between the ice fall and the rocky spur from the Crête des Boeufs Rouges which causes the "nip." Moreover, if the planes have been turned by the unequal motion of the centre and sides of the ice stream, ought we to find them so uniform in direction as they now are, often extending with a very general parallelism over the greater part of the glacier?

(2.) If the veined structure is the result of healed crevasses, how are we to explain the great number of these plates of different coloured ice on glaciers which are not remarkable for very nume-

rous crevasses. For example, on the Roseg Glacier, near Pontresina, these plates of blue and white ice alternate with each other for at least several hundred yards as you walk up the glacier, and are commonly only an inch or so thick. I have in my notebook a diagram of a piece to exhibit the weathering of the two kinds of ice, in which are shown five plates, three blue and two white. One of the former is about an inch thick, and all the rest are thinner. Each of the white is about half an inch, and I remember that this was a fair sample of most of the ice near. If, then, the crevasses, from whose healing this platy structure has resulted, were formed simultaneously or in close succession, how are we to explain the thinness of the white portion, its layers being, if anything, thinner than the blue? Crevasses are not usually so near together as this, and if they were not thus formed it is probable that the plates would be so produced as to be, for about as far as one could trace them, parallel one to another, so accurately that my diagram looks like a bit from a cliff of midland lias?

Cambridge, Jan. 7

T. G. BONNEY

Personal Equation of Astronomical Observers

IN the number for November 18, 1869, of NATURE, "J." asks if an experiment has hitherto been tried to ascertain the value of the personal equation of astronomical observers. The fact is, that it has been tried in different manners, as by Mr. Wolf in Paris, and Mr. Hirsch in Neuchâtel, but first of all by Dr. F. Kaiser, Astronomical Professor, and Director of the Observatory of Leiden. The apparatus of Prof. Kaiser was first constructed in 1851, but was afterwards highly improved, so that it is fitted equally well for observations with or without the chronograph.

A description of the method and apparatus of M. Kaiser is to be found in the "Archives Néerlandaises des Sciences exactes et Naturelles," vol. i. p. 194, and of the improved one in the reports and communications of the Royal Academy of Science of the Netherlands (Verslagen en mededeelingen der Koninklijke Academie van Wetenschappen), Second Series, vol. ii.; the former is written in French, and titled: "Sur la détermination absolue de l'erreur personnelle dans les observations astronomiques;" the latter, in German: "Uebereinen neuen Apparat zur absoluten Bestimmung von persönlichen Fehlern bei astronomischen Beobachtungen." H. VON DE STADT, Ph.D.

Amheim, Netherlands, January 3

Anatomical Lectures to Female Medical Students

I HAVE great pleasure in hearing that the Professors of Anatomy in Scotland have not all forgotten that women ought to be treated with some degree of chivalry.

Professor Struthers, of Aberdeen, and Professor Bell, of St. Andrews, hearing that the five ladies who are studying at the Edinburgh University are excluded from the opportunity of studying anatomy there, have severally offered their services as instructors. Many a lady will rejoice that the numbers of those willing and ready to help in the good cause of fuller knowledge for women are increased by two professors, who have bravely come forward with much moral courage and chivalrous feeling.

Edinburgh, Jan. 22.

A NON-MEDICAL WOMAN

NOTES

THE Physical Section of the Academy of Sciences at their last meeting recommended Professor Kirchhoff, of Heidelberg, to fill the place of correspondent of the section, vacant by the death of Principal Forbes. The other candidates were MM. Ångström, Billet, Dove, Grove, Henry (of Philadelphia), Jacobi, Joule, Lloyd (of Dublin), Riess, Stokes, Tyndall, Volpicelli, and Sir William Thomson.

IN our statement last week that "the Senate of London University has proposed to establish a Faculty of Science," "London University" should have been "University College, London;" the fact being that the Senate of the University of London—in advance of every other university of the kingdom—established a Faculty of Science *ten years ago*; constituting, at the suggestion, and with the advantage of the advice, of the ablest men of science in this country, a scheme for graduation in science, which has continued in efficient operation from that time to the present. And we may add that in the new building of the University the