seems to be the first time that the designation of "Nyanza" has been applied, without any qualification, to the separate Lake Tanganyika. I perceive that "F.R.G.S." associates Captain Burton with this "Nyanza;" but such a name was never given to it by its discoverer, neither is it generally known by any other designation than that of "Lake Tanganyika:" whether or not it should properly be called the "Lake of Tanganyika" is of no moment.

I come now to the consideration of Dr. Livingstone's claim to the discovery of the sources of the Nile, which will be best given in his own words: "I think that I may safely assert that the chief sources of the Nile rise between 10° and 12° south latitude, or nearly in the position attributed to them by Ptolemy, whose river Rhaptus is probably the Rovuma." On this "F.R.G.S." acutely remarks: "Here two different problems are attempted to be solved at once—the one touching the sources of the White Nile, and the other those of Ptolemy's Nile;" in which remark he is no doubt substantially correct. Into the question of Ptolemy's sources of the Nile, on which subject "F.R.G.S." and I differ widely, I need not now enter: what I have here to do with, is the question of the chief sources of the Nile. And in order to decide whether Dr. Livingstone has really discovered these sources, it is, in the first instance, requisite to define the limits of the basin of the Nile, so as absolutely to determine where the sources of the river can or can not be situated. As those limits were approximately determined in a paper "On the Nile and its Tributaries," communicated to the Royal Geographical Society in 1846, and published in the seventeenth volume of the Society's Journal, I cannot do better than reproduce the portion of it relating to this particular subject.

After describing the physical character of the table-land of Eastern Africa, of which Abyssinia forms the northern extremity, and its rivers as far as they were then known,—on which subject I need not dilate, as the substantial correctness of my views is now established,—I proceeded in these terms:—

"All the streams of the plateau or western counter-slope of the Abyssinian chain are affluents of the Nile, and their easternmost branches take their rise at the extreme eastern edge of the table-land, which is the limit of the basin of the Nile, and the watershed between its tributaries and the rivers flowing E. and S.E. towards the Indian Ocean. On the seaward side of this watershed, the declivity being much more abrupt and its extent much more limited, the rivers must necessarily be of secondary importance. Thus, proceeding from the N., we do not meet with a stream deserving of name until we come to the Hawash; and even that river is, near Aussa, lost in Lake Abhebbad before reaching the ocean. The river Haines of Lieutenant Christopher, which is the next in succession, appears, in like manner, not to have sufficient power to reach the sea, at least not at all times of the year. Further to the S. we find the river Gowin (*i.e.* Wabbi-Giweyna) or Jubb, possessing a substantive character as an ocean stream; but this river, during the dry season, has at its mouth a depth of only two feet. At a short distance to the S. of the equator is the Ozay, which river, though said to be of great extent, has very little water at the entrance. Further S. the same law appears to prevail, as is exemplified in the Lufji or Kwavi (Quavi), the Livuma [Rovuma] and the Kwama (Quama) or Kilimane (Quilimane), which rivers rise on the eastern edge of the elevated plain in which Lake Zambre or N'yassi is situate, and flow into the Indian Ocean. *Here, however, the southern extremity of the basin of the Nile having been passed,* the larger streams of the counter-slope no longer join that river, but take their course westwards into the Atlantic, belonging in fact to a distinct hydrographical basin."

What I thus wrote three-and-twenty years ago requires now but little modification. The erroneous identification of Lake Zambre with N'yassi was simply adopted from Mr. Cooley's learned and valuable paper in the fifteenth volume of the Society's Journal, which was then our only authority on the subject. I also followed him in his alteration of the spelling of the name "Zambre," which in my paper was printed "Zambeze," with the explanatory note, "This name is usually printed Zembere, Zembre, or Zambre. It is the Lake *Maravi* of the maps." Though even this was wrong; for Nyassa is properly Lake Maravi, and Tanganyika is the Great Lake, or Zambre. The blending of the two together by Delille and D'Anville was the primary cause of the long-existing misapprehension of the subject.

In my paper from which the foregoing extract is taken, when speaking of the lakes and swamps of the Upper Nile as then

known, I added in a note, "May not Lake Zambre ('Zambeze'), or Nyassi, be the continuation of this series of lakes? In this case it would be simply the upper course of the Nile."

Acting on this suggestion, Professor Berghaus, in 1850, laid down Mr. Cooley's "Nyassi, or the Sea" as the head of the Nile; but, as I pointed out to him, he had under any circumstances carried the river too far south, because the Chevalier Bunsen and I had in the previous year come to the positive conclusion, on the reports of the Church missionaries at Mombas, that Zambre (now Tanganyika) and Nyassa were two separate lakes, a conclusion which every fresh discovery only tended to confirm.

The Cuama and Quilimane mentioned by me were all that we then well knew of the Zambesi, the great western extent of which river only became revealed to us through the former explorations of Livingstone. He thus absolutely closed the basin of the Nile in that direction; though the fact of his having done so was not then demonstrable. When he wrote to Lord Clarendon in February 1867, as he says in his present letter, he "had the impression that he was then on the watershed between the Zambesi and either the Congo or the Nile." His present determination of the want of connection between the Chambeze and the Zambesi, and of the western and north-western course of the former river, has proved the soundness of his impression of February 1867.

The question is therefore now narrowed to this:—Do the united streams of the Chambeze and the Lufira, under the name first of Luapula, and then of Lualaba, flow into the Nile or into the Congo? I am of opinion that they join the former river, and that the explorations of Dr. Livingstone have established the correctness of the views I have long entertained, and especially those enunciated in the *Athenaeum*, No. 1,969, of July 22, 1865, on the first announcement by Sir Samuel Baker of his (unconscious) discovery of the main stream of the Nile under the name of "Albert Nyanza," and consequently I believe we are at length enabled to strip the veil from the Nile Mystery.

CHARLES BEKE

Bekesbourne, December 1

Food of Oceanic Animals

UNDER the above head, in a note which appeared in *NATURE* of the 16th Dec. p. 192, Mr. Gwyn Jeffreys "calls the attention of physiologists to the fact that plant-life appears to be absent in the ocean, with the exception of a comparatively narrow fringe, known as the littoral and laminarian zones, which girds the coasts, and of the 'Sargasso' tract in the Gulf of Mexico." He then proceeds to say that, "during the recent exploration in H.M.S. *Porcupine* of part of the North Atlantic, he could not detect the *slightest trace* of any vegetable organism at a greater depth than fifteen fathoms. Animal organisms of all kinds and sizes, living and dead, were everywhere abundant, from the surface to the bottom . . . some of them being zoophagons, others sarcophagons, none phytophagons." And, lastly, after asking "whence do oceanic animals get that supply of carbon which terrestrial and littoral or shallow-water animals derive, directly or indirectly, from plants?" and "can any class of marine animals assimilate the carbon contained in the sea, as plants assimilate the carbon contained in the air?" Mr. Jeffreys sums up his conclusions on the subject in the following words:—"At all events, the usual theory, that all animals ultimately depend for their nourishment on vegetable life, seems not to be applicable to the main ocean, and consequently not to one-half (*sic*) of the earth's surface."

As Mr. Jeffreys has been constituted an authority on deep-sea exploration, and now claims the view above cited as original, I must be permitted to point out that he has either forgotten what, at one period, he professed to have read and acquiesced in, in one of my writings; or that, for some unaccountable reason, he now repudiates both my opinions and those which were once his own.

As the entire absence of plant-life, even in its primitive phases, in the deeper abysses of the ocean, and the process whereby the nutrition of the lowest animal forms is secured in the absence of even the rudimentary digestive apparatus which is observable amongst the higher Rhizopods, were fully discussed by me in my "Notes on the Presence of Animal Life at great depths in the Ocean" (p. 27), published in 1860; in my work on "The North Sea-bed" (pp. 131-2), published in 1862; in a note which appeared in the *Annals and Magazine of Natural History* for August 1863 (p. 166); and more recently in two papers contributed by me to the *Monthly Journal of Microscopical*

Science, No. 1, pp. 32-33, and No. 4, pp. 231-2-3, published in the present year,—it will, I think, be admitted that Mr. Jeffreys can hardly claim originality in his statement.

But to prove that Mr. Jeffreys was well acquainted with my previously published observations on the subject, I invite attention to two distinct statements of his which appeared in his Reports on Dredging, and were published in the *Annals and Magazine of Natural History*, on the respective dates given below:—

Annals, Nov. 1866, p. 397.

"Dr. G. C. Wallich, in his admirable and philosophic treatise, with which all marine zoologists and geologists are, or ought to be, familiar, believed," &c. &c. "As to the accuracy of his statements, no reasonable doubt can be entertained."

Annals, Oct. 1868, p. 305.

"Coccospheres and Foraminifera cover the bed of the Atlantic at enormous depths. The occurrence, therefore, of such organisms on the floor of the ocean, at great depths, does not prove that they ever lived there. I should rather be inclined to believe that they dropped to the bottom when dead, or after having passed through the stomachs of other animals which had fed on them."

It thus becomes manifest that Mr. Jeffreys had studied my writings, but that the opinions entertained by him in 1866 became revoked in 1868; whilst those held by him in 1868 were in turn superseded by views formed and published in 1869! This circumstance is the more significant, inasmuch as Dr. Carpenter, in his "Official Report on Dredging," for 1868 (p. 181), actually singles out the opinion published by Mr. Jeffreys, as above, in the autumn of the same year, as an authoritative illustration of the want of credence which my discoveries had met with!

With regard to Mr. Jeffreys' new division of oceanic animals into *zoophagons* and *sarcophagons*, I have nothing to urge beyond my avowed inability to discern any physiological difference between creatures that are zoophagous and those that are sarcophagous. It only remains for me to express my belief that, up to the present period, I have stood alone in maintaining, against Ehrenberg and others, that plant-life, even of the lowest types, becomes extinct at depths exceeding four or five hundred fathoms; and in endeavouring, by a series of observed facts, to prove that the nutrition of the Foraminifera and certain other oceanic Rhizopods is effected by a special vital process, which enables them to eliminate and apply to the formation and sustenance of their body and shell-substance, through their surfaces only, the materials which exist in the medium in which they reside.

Kensington, Dec. 21

G. C. WALLICH

Colouring of the Cuckoo's Egg

As I see Professor Newton has, in his very interesting paper on Dr. Baldamus' theory of the colour of Cuckoo's eggs, noticed my "stigmatising" the Doctor's theory as "wild," in my "Birds of Somerset," will you be kind enough to allow me space for a few lines on the subject? Although it is with great diffidence that I venture to differ from Professor Newton, I still cannot help considering Dr. Baldamus' theory as "wild," not perhaps as it appears under the manipulation of Professor Newton, for he seems to me to have pruned and pared it down so nicely that there is but little of the original left; and I think he would not much differ from me in my opinion as to the wildness of the theory, if he had to accept all the allegations in Dr. Baldamus' paper published in *Naumannia*.^{*} For instance, compare the following passage in Professor Newton's paper in No. III. of *NATURE* with some passages from Dr. Baldamus' paper:—"Having said thus much, and believing as I do the Doctor to be partly justified in the carefully-worded enunciation of what he calls 'a law of nature,' I must now declare that it is only 'approximately,' and by no means universally true, that the Cuckoo's egg is coloured like those of the victims of her imposition. Increase as we may by renewed observations the number of cases which bear in favour of his theory, yet, as almost every bird's-nesting boy knows, the instances in which we cannot, even by dint of straining our fancy, see resemblances where none exist, are still so numerous as to preclude me from believing in the generality of the practice imputed to the Cuckoo. In proof of this I have only to mention the many eggs of that bird which are yearly found in nests of the *Hedge-Sparrow* in this country, without ever bearing the faintest similarity to its well-known green-blue eggs. One may grant that an ordinary English Cuckoo's egg will pass well enough, in the eyes of the dupe, for

that of a Titlark, a Pied Wagtail, or a Reed Wren, which according to my experience are the most common foster-parents of the Cuckoo in this country; and indeed one may say, perhaps, that such an egg is a compromise between the three, or a resultant, perhaps, of the three opposing forces; but any likeness between the *Hedge-Sparrow's* egg and the Cuckoo's so often found alongside of it, or in its place, is not to be traced by the most fertile imagination. We must keep, therefore, strictly to the letter of the law laid down by Dr. Baldamus, and the practice imputed to the Cuckoo is not universally, but only approximately true." This certainly is very different from Dr. Baldamus' own statement:—"If Mr. Braune, the forester of Griesland, had not cut this large Willow Wren's (*Shippolais*) egg (as it seems) out of the ovary of the Cuckoo, which was killed as she was flying out of the Willow Wren's nest; if Count Rödern, of Breslau, was not a reliable authority that this apparent Redstart's egg was taken out of the nest of the Redstart (*Ruticilla phoenicurus*); if M. Halricht had not taken this large Tree Pipit's egg out of the nest of a Tree Pipit (*Anthus arboreus*); if I myself had not taken out of the nests of the Red-backed Shrike (*Lanius collurio*) this reddish and this green-greyish peculiarly marked Cuckoo's egg, one might indeed entertain doubts whether this variously-coloured collection—these green eggs, with and without markings; these on white, grey, green, greenish, brownish, yellowish, reddish, and brown-reddish ground; these grey, green, olive/green, ash grey, yellow brown, yellow red, wine red, brown red, dark brown and black; these spotted, streaked, speckled, grained and marbled eggs could one and all be the eggs of our Cuckoo! And yet this is indeed the fact!" How different this from the much more cautious and limited statement of Professor Newton, first quoted, which would entirely sweep away some of these varieties, especially those resembling the eggs of the Redstart or the Hedge-Sparrow, for the eggs of these two species do not differ much from each other, and what might be said of the eggs of the one would apply equally to those of the other; yet these are two of Dr. Baldamus' selected species, for, a little further on, he gives a list of the various species from the nests of which Cuckoo's eggs have been taken resembling those of the foster-parent. Of the eggs of the Redstart he says:—"These four specimens, which were found in the nests of *Ruticilla phoenicurus*, are all of a light-green ground colour; two of them have the larger and more or less brownish spots, which on one of them form a zone; the third has similar markings, but only sparingly scattered over the whole surface, whilst the fourth is without any marking at all—herein it is identical with one in the possession of Dr. Dehne, which is uniformly light-greenish blue, without any markings whatsoever."

Of the single specimen of the egg resembling that of the Hedge-Sparrow, No. 15 in his list, he says:—"One of the most interesting of the Cuckoo's eggs is a beautiful blue-green one, which was taken out of the nest of *Accentor modularis*, without any markings, and which even to the shell, the grain, and the size (bis auf Shale, Korn, und Grösse) is like a very dark egg of the Hedge-Sparrow." On reading this quotation from the statement of the facts on which his theory is founded by Dr. Baldamus in the paper in *Naumannia*, and comparing it with Professor Newton's paper above quoted, we cannot help seeing that there is a decided issue of fact between them, especially as to the eggs of *Accentor modularis*.

The conclusion which Dr. Baldamus draws from the facts stated by him is that Nature, by means of such arrangements, has ensured and facilitated the preservation of a species otherwise much exposed to danger, and that she has attained this object by investing every hen Cuckoo with the faculty of laying eggs coloured exactly like the eggs of the bird of whose nest she prefers to make use, according to the locality. Now if this were really the case, and it were really true that this colouring of the eggs were essential for the preservation of the species, would it not be just one of those laws of Nature which we should expect to find universal, or so nearly so that there would be but very few exceptions? But according to Dr. Baldamus himself the exceptions are numerous, and Professor Newton would make them still more numerous, and would no doubt be quite right in doing so. How, then, do the eggs in the exceptional cases prosper? Does the Hedge-Sparrow or the Redstart throw the egg of the Cuckoo out of its nest because it does not resemble its own? or do the birds to whose tender mercies the Cuckoo, according to Dr. Baldamus himself, is occasionally obliged to entrust its eggs when it cannot find a fitting nest in which to place them, do so? This does not appear to be at all

^{*} Where I have quoted from this paper, I have quoted from the translation by the Rev. A. C. Smith, published in the *Zoologist* for 1868, which professes to be an accurate translation, and there seems to be no possible reason to doubt its being so.