is mainly one of adventure; nevertheless, much of scientific interest is weaved into it, and the boy who reads it without skipping the closely-printed portions will obtain some useful knowledge of the natural history of Borneo.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Human and Comparative Anatomy at Oxford.

I AM sorry that Prof. Burdon Sanderson should state that a "misstatement" occurs in the article written by me in your issue of October 26, and still more so that whilst making such a charge he altogether omits to cite the "misstatement" which he sets out to correct.

My knowledge both of the University of Oxford and of the teaching of anatomy is, as a mere result of individual history, far more intimate than is that of my colleague, Prof. Sanderson, a fact which may account for some difference in our opinions on these topics.

When in 1885 the Convocation of the University was asked to sanction the payment of a small salary for a limited period to a lecturer in Human Anatomy—out of University funds already taxed to an inconvenient extent—there existed four ancient foundations in Oxford assigned to the support of teachers of "Anatomy," viz.: the Linacre Professorship of Human and Comparative Anatomy, the Tomlinsian Lectureship of Anatomy, the Aldrichian Professorship of Anatomy, and the Lee's Readership in Anatomy. The small Tomlinsian and Aldrichian endowments had been united in 1803 by Parliamentary authority, and the salary arising from them was in 1858 assigned to the payment of a demonstrator or demonstrators nominated by the Linacre Professor. There were thus, as teachers of anatomy in the University, the Linacre Professor and his Aldrichian demonstrator, and the Lee's Reader. Whatever may have been the conceptions of the ancient founders as to the nature of that study which they designated "Anatomia," or the intentions of University Commissioners who gave the title of "Professorship of Human and Comparative Anatomy" to Linacre's revived readership in medicine, or by whatever conditions the result may have been determined, it is certain that in 1885 the holders of the Linacre, Aldrichian, and Lee's teacherships were not giving that "technical training in anthropotomy," that "topographical knowledge of the human body," which forms a part of medical professional education, and is administered in every hospital-medical school throughout the country. What these Oxford teachers were doing was to teach anatomy in its broad academical sense. They taught the anatomy of man and of animals as a branch of biological science, and I venture to assert that they taught the anatomy of man and of vertebrate animals with as ample material illustration, and in as complete and detailed a way, as is desirable for the training of University students intending to pursue the study of any one of the great branches of biology as a science. What they did not do was to furnish the professional medical student with that special acquaintance with the arrangements of tendons, nerves, and blood-vessels in each little tract of the human body which might be the seat of a surgical operation or a medical exploration.

Both the teaching and the acquirement of these details is tedious and uninteresting, although necessary for the surgeon. The subject-matter is called "human anatomy," or the "anatomy of man"—as distinguished from "anatomy" in its wider sense, and more especially as distinguished from comparative anatomy. Human anatomy is not a branch of science; it is topographical information. In order to render it less uninteresting than it would be (and under some teachers is) when strictly taught, it is customary for the teacher of human anatomy to introduce scraps of the various branches of science which bear upon the significance of animal structure into his text-books and lectures. He thus imports a little physiology and mechanics, a little comparative anatomy or animal morphology and embryology into his teaching. The real significance, however, of the facts learnt by the medical student in his course of "human

anatomy" is in their application to surgical and medical practice, and it is in referring to these applications that the teacher of human anatomy finds the legitimate and most successful haven for his dead weight of detail.

Such being the somewhat repellent character of the pursuit of human anatomy considered apart from the science of morphology or comparative anatomy, it is not surprising that the Oxford teachers of "anatomy" in 1885 had all devoted themselves to the latter study, and left the technical topographical human anatomy uncared for.

But unattractive and uninteresting as it is, this human anatomy has to be "gone through" by the medical student. Oxford had been roused to a sense of her obligations to medical study by a movement, in which I took a somewhat prominent part, and accordingly when in 1885 it was represented to Convocation that although the University had three teacherships of anatomy and splendid collections illustrating the structure of animals (including man), yet there was no provision for carrying on the instruction in anthropotomy necessary for technical students of medicine, that body generously and deliberately consented to provide a new teachership for the specific purpose of filling this gap in the machinery of the Oxford Medical School.

I should be very sorry to see any tendency to frustrate the excellent purpose of Convocation, and I feel confident that the renewal of the periodic grant made by that body to pay the salary of a lecturer in human anatomy must depend on the lectureship being strictly restrained within its original boundaries. We do not want at Oxford a fifth teachership of anatomy added to the four which have somehow slipped away from "anthropotomy" into pleasanter and more philosophic regions. The University ought not to be asked for more money with the object of effecting once more such a transformation. But this will certainly be the case if vague theories about "the academical teaching of human anatomy" are allowed to pass without protest.

Oxford, November 4.

E. RAY LANKESTER.

P.S. It is strange that Prof. Sanderson (himself an Edinburgh man) speaks in his letter of the University professor of *Human Anatomy* in Edinburgh. There is no such professorship in Edinburgh. Sir William Turner is professor of *Anatomy* in Edinburgh, as is the eminent comparative anatomist Gegenbaur in Heidelberg. The separation of human anatomy from comparative anatomy has not been carried out in these Universities as it has been deliberately in Oxford. Anthropotomy is taught in the former by demonstrators and assistants acting under the direction of the professor of anatomy. That was the intention of the late University Commissioners with regard to Oxford when they constituted the Linacre Professorship of Human and Comparative Anatomy. Some persons, however, thought it best (I am not now discussing the merits of the arrangement) that the example of Edinburgh, Heidelberg, and other European Universities, should be departed from in Oxford, and that the functions of the Edinburgh Professor of Anatomy and his staff should in Oxford be divided between the Linacre Professor of Comparative Anatomy and an independent lecturer in Human Anatomy.

The Monros, Goodsir, Allen Thomson, and other distinguished Scotch teachers were, like Gegenbaur, Kölliker, and others in German Universities, professors of "Anatomy," not exclusively of "Human Anatomy."

The Oxford plan of relieving the titular Professor of Human and Comparative Anatomy of an important but technical branch of his teaching, by the appointment of a lecturer *ad hoc*, has not a precise parallel in other Universities. Moreover, in Germany the subject of microscopic anatomy or histology is very usually undertaken by the Professor of Anatomy (e.g. Kölliker, Waldeyer, His), although in Oxford the Professor of Physiology is by statute called upon to give instruction in histology. It would probably not meet with unanimous approval were the present Oxford lectureship in Human Anatomy—in imitation of "academical teaching" elsewhere—to be diverted wholly or partly to the subject of histology.—E. R. L.

"Geology in Nubibus." An Appeal to Dr. Wallace and others.

In his timely and important letter to you, Dr. Wallace congratulated us all on having got rid of a *real* glacial nightmare by sweeping away the tropical glaciation which has been favoured

by some high authorities, including himself, Mr. Darwin, and Mr. James Geikie. While we may all share in this congratulation, it must be remembered what it involves.

It has been the fashion with an extreme and aggressive school of glacialists to postulate an excavating tendency in ice to which the formation of lake basins and valleys-without-outlets in mountain districts has been attributed. They will not allow that rock basins are due to any other cause than "omnipotent ice." They scoff at explorers of the mechanics of ice in Alpine countries, like Prof. Bonney and Mr. E. Hill. They jeer at those who have devoted much patience to unravelling the mysteries of Plutonic action, like Prof. Judd and others, who attribute a large number of lakes to dislocations and to foldings of the sub-jacent rocks. It is no use, in arguing with them, to refer to mechanical difficulties like those involved in conveying thrust of more than a certain amount through a substance like ice, which is known to crush under a moderate pressure, nor to produce any number of mechanical arguments against the capacity of ice to erode lake basins such as those in question; nor is it any use appealing to the stupendous geological difficulties against their conclusions which have been accumulated by quite a number of skilled geologists at home and abroad. All these efforts are futile, for we are told that the ice to which appeals must be made is quite a different thing to any ice we can experiment upon or examine, and that it must not therefore be measured by the ordinary laws that govern ice such as we know it, and this appeal to transcendental ice is considered to be orthodox science in the nineteenth century, an age when induction is supposed to have become a supreme law to us all, and when *a priori* postulates are generally discarded from the realm of physical research. Let this pass, however, and let us test the question in another way. Let us test it, in fact, by this very case of Brazil.

There has never been a glacial period in, nor are there traces of glacial action in the highlands of, Brazil, we are told by Dr. Wallace. Granted. How then can Dr. Wallace, and those who agree with him in this matter, explain the existence on the plateau of Bahia of perhaps the largest and most remarkable collection of rock basins in the world, rock basins existing, too, in close juxtaposition with most perfect examples of giants' cauldrons on the largest scale. This is assuredly a dilemma for the transcendental school of geologists.

Let me quote from Mr. Allen's graphic descriptions of these rock basins. Speaking of the plateau of Bahia, he says: "Over this whole region there is an almost entire absence of loose materials on the surface . . . slight knolls and shallow basins alternate which rarely differ more than 20 or 30 feet in elevation. In the rainy season many of these basins become filled with water, forming shallow lagoas varying in area from less than one to more than 50 acres, from most of which the water evaporates in the dry season . . . So numerous were these lagoas for more than 50 miles that it seemed natural to speak of this region in my notes as the "Lake Plain." Almost everywhere the elevations are evenly rounded, indicating that the rocky crust has been exposed to rain and probably long continued abrasion. But the absence of abraded materials seemed most remarkable; very rarely were even loose boulders observed, though a few such were repeatedly noticed. At frequent intervals there were irregular holes in the rocks, usually nearly filled with water, to which the inhabitants give the name of 'caldeiraos.' These caldeiraos are of frequent occurrence. . . . Nearly all of the considerable number examined proved to be genuine pot-holes, and some of them were of great size. The largest one I measured was elliptical in outline, 18 feet long, 9 or 10 in width, and 27 deep, with smoothly worn sides. . . . These pot-holes often occur out on the plain, far away from any high land, and they are sometimes found excavated on the summits of slight bulgings in the plain, or even on the top of a hill."

I would ask, in all seriousness, whether, if phenomena like these had been described from the Alps or from Nova Scotia, they would not assuredly have been pointed to by extreme glacialists as the unerring footprints of great ice-sheets, and yet Dr. Wallace, who is a champion of the school, repudiates the former glaciation of Brazil altogether.

What is to be said in regard to this dilemma then? It is quite clear that either the facts must be disputed (and who is to dispute them?), or else the champions of ice at all hazards must concede that rock basins and giants' cauldrons can be made by other agencies than ice. If so, they can be made as well in one

place as in another. If they could be made by other causes on the plateau of Bahia, why not in the highlands of Tasmania?

I am bound to say I was taken aback by Dr. Wallace's comments on a letter from one of your correspondents, which appeared in *NATURE* a short time ago. That gentleman professed to make an exploration of certain parts of Tasmania with another experienced geologist. They were both champions of the glacial theory. They both went prepared to find traces of glacial action there, and certainly in our latitudes no evidence seems more easily discriminated, and they came back convinced that in the districts where the rock basins of Tasmania abound, there are no traces of glacial action to be seen. They could find none. Mr. Johnstone, who has written an elaborate and detailed geological memoir on the island, and who has explored it in many directions, could find none either, save on the western flanks and in the valleys of the Tasmanian Alps in the western part of the island, where it has been long known that traces of former local glaciers exist. There is absolute unanimity among the native geologists that nothing in the shape of ice-sheets existed there, and there is no ice-spoor in the central districts where the great Tasmanian lakes occur. Dr. Wallace's answer to all this was certainly unexpected. He has not himself visited the island, and yet he disputed not only the inferences but the facts and the observations. Why should the voice of Esau be listened to and approved in Brazil, and that of Jacob be repudiated in Tasmania? Mr. Johnstone and the other observers in Tasmania are assuredly to be trusted in such an issue quite as much as Prof. Branner. I cannot see on what ground the discrimination is made, except the desperate inconvenience of postulating a glacial nightmare in the tropics.

Assuredly the whole difficulty lies in championing a theory of the origin of lakes, unknown in geology until introduced by Ramsay, whose extravagance at times may be measured by some of his phrases addressed to the British Association when he presided over the geological section. From all sides there comes a revolt against this theory, which is based on no empirical evidence, and is at issue with the mechanical properties of ice so far as we know them, and with the observations of practised observers of the first rank. I am bound to say that those geologists who habitually make appeals to forces in Nature, and to properties of matter which are purely hypothetical and unwarranted by experience, are leading us back to times when Aristotle and deductive reasoning dominated European thought, and when Bacon had not yet taught us better things.

My attention has been called to an oversight in my previous letter. Among those who many years ago did good work in dissipating the particular glacial monster that was generated in the valley of the Amazons, was my old friend Dr. Woodward, whose papers on the subject in the volume of the *Ann. and Mag. of Nat. Hist.* for 1871, pp. 59 and 101, I had overlooked.

HENRY H. HOWORTH.

30 Collingham Place, Cromwell Road, October 27.

Correlation of Solar and Magnetic Phenomena.

I WAS glad to see (*NATURE*, vol. xlix. p. 2) in the notice of Miss Clerke's "Popular History of Astronomy," that attention was drawn (1) to the correspondence in time between a certain luminous outburst seen on the sun on September 1, 1859, by Carrington and Hodgson, and a disturbance of the magnets at the Kew Observatory; and (2) to the statement of the late Mr. Whipple that the magnetic movement was really a small one, and that in his opinion the observed correspondence was a mere accidental coincidence. Those who have read Carrington's original account (Monthly Notices of the Royal Astronomical Society, vol. xx. p. 13) will remember that at the time he himself did not lean towards hastily connecting the phenomena, remarking that "one swallow does not make a summer." But authors of text-books on astronomy, who may be only to a partial extent observers, are too apt to state the matter in such a way as to give an impression that we have here an undoubted instance of direct connection, instead of a case of apparent connection, to be taken merely for what it is worth, seeing that the occurrence has remained to the present time without corroboration. I should like to take the opportunity to support, in the fullest manner, the opinion of Mr. Whipple, which acquaintance with the Greenwich magnetic registers tells me to be a true one. The magnetic movement in question, as recorded at Greenwich, was similarly small.

But the erroneous impression lives long. May I therefore be