a fearfully intricate-looking table with the heading "(I.) Analysis by Triple Formula and Genealogy. (A) Analysis into Words." To the meaning of "triple formula" we find no clue but such as may be contained in the statement of the preface that "Analysis resolves itself into Limitation, Limiting Capacity, and Modification to denote Limiting Capacity." The table, which fills three pages, should, the author says, be learnt by heart. Further on there are two other similar tables, headed "(B) Analysis into Sentences and Clauses," and "(II.) Parsing." The "analysis into words" is exemplified by a number of specimens, in which the words forming the sentence are arranged in a diagram resembling a genealogical tree. The "analysis into sentences and clauses" is also performed by means of diagrams, but of a different kind. We must confess that the first impression we received on turning over the pages was one of utter bewilderment. After a while, however, we began to see that the author had a meaning, and for the most part a reasonable meaning, though his mode of presenting his ideas is not felicitous. Mr. Tickell seems to be one of those persons who have a talent and a passion for methodical classification, and an exaggerated estimate of its importance. We have no doubt that he has found the preparation of this little book a valuable help to the attaining of clear ideas on the structure of English sentences, but we should greatly pity any schoolboy who was condemned to learn his grammar from such a manual. On the other hand, we think it is possible that teachers may find in the book a good deal of useful suggestion.

The Chemistry of Plant and Animal Life. By Prof. H. Snyder, Professor of Agricultural Chemistry, University of Minnesota. Pp. xvii+406. (New York: The Macmillan Company; London: Macmillan and Co., Ltd., 1903.) Price 6s. net.

THIS is a text-book of a type which is not very satisfactory from an educational point of view, as being written to supply the needs of a special class of students. It aims at supplying the chemistry, both pure and applied, which will be required by a class of mature students taking a college course in agricultural science. No preliminary knowledge of chemistry is assumed, and the preface indicates that the work is to be completed in a session of six months with one class-room or laboratory exercise each day. In consequence, the contents range from instructions for bending glass tubes to an account of such debatable matters as the relation of gliadin and glutenin to the quality of wheat flour. It would be hardly fair to call the treatment superficial, but clearly the object of the book is not so much to educate the student as to supply him with a certain amount of information about the scientific side of practical matters, and particularly to put him into a position to follow the current investigations of the experiment stations. Of course, the task is an impossible one; no ordinary student can acquire a real knowledge of pure and agricultural chemistry in six months, but given the special conditions, the book represents Prof. Snyder's abundant experience of trying to make the most use of the limited opportunity. The first 150 pages are devoted to ordinary inorganic chemistry, then about 100 pages deal with the proximate constituents of plants, followed by a section on the general course of development in the plant. Then come discussions of the composition of individual crops, and about eighty pages on digestion and the nutrition of animals. In the earlier part of the book a number of experiments are suggested, with a useful series of questions designed to make the student think out the object of each experiment.

NO. 1797, VOL. 69]

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.]

A New Mineral from Ceylon.

IN the beginning of February I bought from Mr. Holland 5 cwt. of the mineral described by Prof. Dunstan in last week's NATURE (p. 510). It crystallises in cubes, and the density is substantially that found by him. Mr. Tyrer, of the Stirling Chemical Works, Stratford, was so kind as to promise to work it up for me, and the process is still being carried on.

I had hoped to have positive and definite results to communicate before describing its constituents, but the publication by Prof. Dunstan of an analysis, and his statement that he is still engaged in its investigation, makes it necessary to write this letter.

The mineral, when heated alone, gives off 3.5 cubic centimetres of helium per gram; fused with hydrogen potassium sulphate, the amount is increased to 9.5 cubic centimetres. From this source I have already stored about 12 cubic feet of pure helium extracted in Mr. Tyrer's works.

It was at first believed that the mineral was rich in uranium, but different specimens contain only from 8 to 12 per cent. of that element, agreeing in this respect with the analyses published by Prof. Dunstan. Next, the other main constituent was believed to be zirconium, but the high density of the mineral rendered this improbable. An analyst of high standing, whose daily business it is to analyse minerals of this kind, returned 82 per cent. of zirconia as a constituent; the percentage of thorium was trifling—under 1 per cent. The mineral contains practically no thorium; this has been repeatedly confirmed in my laboratory. Nor does it contain any appreciable amount of cerium, lanthanum and didymium. The oxalate is almost completely soluble in excess of ammonium oxalate—a reaction which excludes thorium and the cerium group, but which points to zirconium. The equivalent of the elements of the oxalate group, which I at first took for zirconium, excludes the presence of any large quantity of zirconium, although that element is undoubtedly present. Fractionation shows that the oxalate precipitate (the portion soluble in ammonium oxalate) gives equivalents between 25.0 (the most insoluble portion of the double sulphate) and 44.7 (the most soluble portion); by far the major part of the element has the last mentioned equivalent. The separation of this portion is now being carried out with large quantities of material; several hundredweights are being worked up.

Assuming that the element is a tetrad, which is probable from its behaviour, it undoubtedly possesses an equivalent approaching the highest number (44.7), and for this there is a gap in the periodic table between cerium and thorium; one at least of the elements present (supposing that there is more than one present) will probably have an atomic weight of about 177, preceding tantalum (182.5) in the horizontal row of the periodic table.

I am at present engaged in mapping the spectrum of this new body or bodies.

As for the radio-activity, the mineral was bought in the hope that it would have a high content of radium. There is a trace of radium present, due, no doubt, to the spontaneous change of the uranium which the mineral contains. But the radio-activity due to this source is certainly not 5 per cent. of the total.

The period of decay of the emanation appears to point to the presence of a radio-active element closely resembling thorium X. The half value is 50 or 51 seconds, and while this is not quite the time for the decay of thorium emanation, it very nearly approaches it; at present the balance of evidence appears to point to the presence of an element closely resembling thorium, but not identical with it. The total radio-activity, moreover, is much greater than can be accounted for by the supposition that the one consists of pure thorium. Within the limits of a letter I am obliged to omit many more characteristics of this curious ore which have been ascertained, but I hope soon to be able to publish

more definite results; as it is, I regret to have been obliged to tell an imperfect story.

I should like to conclude by acknowledging the great assistance given me in this work by Mr. Tyrer and by my students, Messrs. Gimmingham and Le Rossignol.

University College, April 4.

WILLIAM RAMSAY.

The Blondlot or *n*-Rays.

In this laboratory we have obtained uniformly negative results in experiments on the Blondlot rays. Our experiments were made with the help of seven observers, including five doctors, one student, and one laboratory attendant. Calcium sulphide screens rendered fluorescent in a separate room by burning magnesium were employed. They were brought into an absolutely dark room in which the observers had been kept for some time. Two forms of screens were used:—

(1) Flat screens on which a circular area on a slip of glass is covered by calcium sulphide.

(2) The later form in which a circular area at the back of a hemispherical lens is covered by calcium sulphide. The screens were made by Mr. Leslie Miller.

The screens were either held by the hands of the observers or were clipped on stands.

The observers were told first to look steadily at the screens and report any variation in brightness, calling out "bright," "dimmer," "dim," "brighter," &c., as the appearances seemed to change. Even with the screens on the slips of glass the observers after a few moments were able to call out the changes, *although there was no attempt at muscular contraction*. With the lens form of screen the changes in brightness were very marked.

We next attempted to find whether muscular contraction behind the screen caused an increase of brightness. Of course, where the observer sees a change in brightness without muscular contraction it is easy to be misled on this point. We made the observer continue to call out the degree of brightness, and we contracted the muscles of the arm behind the screens sometimes after he had called out "bright" and sometimes after he had called out "dim." In the great majority of cases the effect we looked for did not follow. In the few cases in which it occurred we naturally attributed the results to the changes in brightness which can be observed without any muscular contraction.

We next told our observers to look, as it were, into the distance beyond the bright spot, and to report on the brightness of the screens. When the accommodation of the eyes for near vision was relaxed they reported without exception that the brightness of the screens was constant, and that muscular contraction made no difference.

When observers were then asked to touch the backs of the screens, thus warming them, they reported an increase of brightness.

It is not easy to explain the phenomena we have described. We believe that there is difficulty in accommodating for the fluorescent circle, and that there is a wavering movement of the ciliary muscles, and probably also a wavering in the size of the pupils. Yet it is asserted that we can focus a point of light in a dark room, and it is difficult to see why the fluorescent screen cannot also be kept steadily in focus when it consists of a flat glass slip with fluorescent circle. In the case of the later, and presumably more successful form of apparatus, the difficulty is easily understood. In that form the fluorescent rays proceed from the back of a hemispherical lens, that is, from a point within the posterior principal focus, and they are widely divergent, and thus strain the accommodation of all but near-sighted people. The fact that in every instance we found that the light becomes steady after relaxation of the accommodation is very striking.

But the phenomena observed by us do not go any distance towards explaining the results described in M. Blondlot's papers. How is it that he and many of his compatriots see increase of brightness under conditions in which we see none? Is the explanation to be found in the paper by Heinrich, "Die Aufmerksamkeit und die Funktion der Sinnesorgane" (*Zeitschr. für Psychologie u. Physiol. d. Sinnesorg.*, vols. ix and xi.), in conjunction with our observations? Heinrich found after many careful experi-

ments that the pupil dilates when attention is directed to an object situated in the field of indirect vision, and that it dilates still more, during a short mental effort, such as a calculation. He found also that on directing attention to an object in the field of indirect vision the ciliary muscle relaxes, thus diminishing the curvature of the crystalline lens, and that during mental calculation this change is very marked, causing a curvature even less than that required for vision of a remote object. He found also that under the same conditions the axes of vision tend to become parallel or even divergent.

Can it be that the mental condition of some observers in a state of expectancy reacts on the intrinsic muscles of their eyes, and thus they see what they think they should see?

We have also experimented with the rays from a Nernst lamp, but without result.

JOHN G. MCKENDRICK.

WALTER COLQUHOUN.

Physiological Laboratory, The University, Glasgow,
March 29.

Learned Societies.

IN NATURE of March 10, Mr. Basset directs attention to the fact that referees frequently know less about the subject-matter of the papers than the author, and that their reports frequently contain errors from their not understanding the papers.

Had Mr. Basset held a brief for the opposite camp to that which he claims to represent, he could hardly have adduced more powerful arguments in favour of the referee system.

If a paper is of any value, the author must *ipso facto* know more about the subject-matter than anyone else. If he does not he is not the proper man to write the paper. But it is just because authors so frequently send up papers in a form in which other people cannot understand them that referees are necessary.

At present few people have time to wade through pages and pages of discursive and ill explained writings on the off chance that they may ultimately light on an interesting result. On the other hand, it is desirable that workers in one branch of science should have some insight into the general character of the investigations which are being pursued by specialists in other directions. Now I have before me a number of mathematical papers which contain no indication whatever of what the authors are driving at. They begin by putting down certain formulæ which the reader is assumed to know, and when they have twisted these formulæ about a bit they stop short abruptly without any obvious rhyme or reason. There are, of course, specialists who understand and appreciate these papers, but to the man who has specialised in applied mathematics or in a different branch of pure mathematics, the whole thing as presented appears meaningless. On the other hand, I have read with interest many well expounded papers dealing with such subjects as physiology, palæobotany, or psychology of the general character of which it is easy for anyone to form an estimate, even without previous university training. The functions of a referee should be to see that the arguments in a paper are clearly put forward, and that the main conclusions are prominently stated at the beginning or end in such a way that a general survey of the ground covered can be formed by the reader before the methods are examined in detail.

I believe that a useful purpose would be served if one of the referees of a paper were in each case selected on account of his *ignorance* of the subject-matter with which the paper dealt. Transactions would then be more readable and more widely read than they are at present.

It frequently happens, moreover, that results are buried in out-of-the-way corners of lengthy papers where they get overlooked, and that when someone has published similar results in a more accessible journal an undignified priority controversy is the result, whereas the original discoverer has only himself to blame for failing to present his subject in a readable form.

Learned societies are frequently penny wise and pound foolish in issuing their transactions *uncut*, consequently those who wish to study the contents have to waste much of their time in jaggings and often tearing the pages with a