

visual process which was based upon *general* properties of the nervous substance would be open to the same objection. This is true, and it applies to Müller's own explanation of the phenomenon in question with peculiar force. But the conclusion to be drawn is not that one visual theory is sure to be just as good as another, but rather that that theory which posits a chemical process which is *not* exactly like what goes on everywhere else in the body has by so much the advantage over another theory. The idea of a photo-chemical substance which is unstable after a partial dissociation, which it is as far as possible from being a remote idea to the chemist, is *just as far* removed from our conception of other physiological processes as it *must be*, in a well-devised theory, in order to account for anything so extremely distinctive as is the visual after-image.

But even though it had been necessary to go very far afield for the conception of a semi-suicidal chemical substance, this could not have been counted, by any one who had given a moment's consideration to the subject, as a point of superiority on the part of Hering's theory over mine; for his assumed photo-chemical substance is 'suicidal' from the start. If blue is the color of assimilation, then after we have looked at a blue surface for a few moments there has been piled up in the retina, according to Hering, a large amount of the blue-yellow substance, and it is the going to pieces of this immediately afterwards which is the cause of the after-image; this assumed process is not in itself an objection to the theory, but it is 'suicidal' to the last degree.

Professor Müller's recent papers in the *Zeitschrift für Psychologie* are a monument of learning and acumen, as I have already said in the pages of *The Psychological Review*. How far they are from substituting for the original theory of Hering a theory which can lay any claim whatever to being considered an adequate account of the phenomena of color-vision I am about to show in connection with a general discussion of color theories. Meantime I rejoice in the fact that Professor Titchener has renewed his study of the subject of color. It is to be hoped that this will lead him to remodel the brief statements regarding color which are found

in his book on Psychology; what he says there (while it is not incomprehensible to one who has the clue to his secret meaning) must seem contradictory and confusing in the extreme to the ordinary reader, and certainly constitutes a serious blemish in a book which is otherwise not simply a good text-book, but a valuable contribution to the science of psychology.

C. LADD FRANKLIN.

BALTIMORE.

A PRECISE CRITERION OF SPECIES.

TO THE EDITOR OF SCIENCE: I thank you for the suggestions contained in your kind discussion in SCIENCE, No. 178, of Mr. Blankinship's and my paper on a 'Precise Criterion of Species.' Our paper was concerned with a method which, if applied, will constitute a small, but, we think, important, step toward giving greater precision to the defining of particular species and to the distinguishing of varieties from species. To my mind the only important objection urged so far, an objection which was anticipated, is that it is impracticable to use in systematic work so great precision as our method calls for; it takes too much time and too large a number of individuals. *A priori* argumentation cannot dispose of this formidable objection; only the demonstrated advantage of the method in practice can avail against it. I should like to urge anthropologists, mammalogists, ornithologists, ichthyologists, malacologists and others who have already gone some way in the direction of applying statistics to species to put the method to practical test. Mr. Blankinship and I are doing so. I should be very glad to assist those who meet with difficulties in the application of the method, as, for example, in the measurement of color and complex forms. The ingenious naturalist will find, however, as anthropologists have found, few, if any, specific differentiae which are not measurable.

C. B. DAVENPORT.

ELECTRICAL ANÆSTHESIA.

TO THE EDITOR OF SCIENCE: While making some experiments on the sensations derived from sinusoidal currents I noticed (April 12, 1898) that anæsthesia of the tissues resulted

from currents of high frequency, the condition lasting for some time after the removal of the electrodes. While in this condition the finger could be pricked with a pin without any resulting sensation except that of dull contact. Sensitiveness to cold was also removed. The investigation has been continued and has shown the possibility of employing a sinusoidal current of high frequency as an anæsthetic. Full details as to the requisite frequency and intensity will be published later.

E. W. SCRIPTURE.

YALE UNIVERSITY,
NEW HAVEN, CONN., May 25th.

SCIENTIFIC LITERATURE.

The Sun's Place in Nature. By SIR NORMAN LOCKYER. London and New York, The Macmillan Company. 1897. Pp. 360. Price, 12 shillings.

The character of this latest work of its eminent author might, perhaps, be misunderstood from its title. It discusses the Sun's place in the order of evolution of the stars, and not in relation to the solar system. It is, therefore, to be classed as a book on stellar astronomy, and is to a considerable extent based upon a course of lectures recently delivered by the author at the School of Mines in London. The nature of the work may best be shown by quoting in full the conclusion: "I am not aware that any more crucial test than the foregoing can be applied to the rival schemes of stellar classification, and, as I hold that the result of its application is entirely in favor of the one which assumes the existence of some stellar bodies which are increasing their temperature while others are reducing it, the Sun's place in Nature must be regarded as near that occupied by Arcturus and Capella, and very far separated from that occupied by α Cygni, γ Cygni, and α Tauri. Nor is this all, the origin of the Sun in a nebula not exclusively gaseous, but only containing gases among its constituents, is greatly strengthened by the extended study of the classification problem which has occupied the last few chapters. Along all lines, then, the fundamental requirements of the Meteoritic Hypothesis have been strengthened by the later work."

The book may be regarded, then, as a sequel

to 'The Meteoritic Hypothesis,' which appeared in 1890, and is intended to reply to the criticisms of the earlier work, as well as to present the author's view of the bearing on that hypothesis of the relevant discoveries in the intervening years. It is, therefore, not a book for the instruction of the general reader, unless he has a taste for argumentative reasoning, adduced in behalf of a theory which has not met with general acceptance. It is written in Sir Norman's easy style, and may readily carry the reader who is not critical along to the conclusions of the author.

An interesting account is given of the discovery of terrestrial helium and the investigations of its spectrum from various minerals, in which the researches of the author were early and important. Some seventy minerals were examined in his laboratory at South Kensington, and the D_3 line of helium was detected in the spectrum of sixteen of them.

It is an essential feature of the Meteoritic Hypothesis that nebulae are meteoritic in their nature, and that they pass into the stage of 'stars' as the meteoric 'swarms' become more condensed. Accordingly considerable space is devoted to the chemistry and nature of the nebula and their relation to stars. Professor Lockyer has himself obtained very successful photographs of the Orion nebula, and he gives a list of 54 lines on a plate taken in 1890. If there is any connection between nebulae and meteorites it would certainly be expected to reveal itself in some resemblance of their spectra. As a matter of fact, however, aside from hydrogen and helium, which are abundantly represented by lines, the only elements which Professor Lockyer identifies are calcium (three lines), iron, carbon and magnesium (one line each). Now Keeler's measures have demonstrated that the chief nebular line does not coincide with the magnesium line, and still less do the remaining lines agree in wave-length with the lines of the elements mentioned. Thus it appears that there is an entire absence of spectral similarity between nebulae and meteorites, except that both contain the universally prevalent hydrogen.

The references made to the work of Dr. Huggins, especially in connection with the