LETTERS TO THE EDITOR.

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The Direct Cynthesis of Optically Active Proteid-like Substances

IN a recent communication to the Royal Society (*Proceedings*, No. 364, lx. 337-349), of which an abstract has appeared in NATURE, Dr. Pickering describes experiments he has made—in extension of those carried out by Grimaux several years ago—on the synthesis of certain proteid-like substances: the substances were obtained by heating amido-acids, such as par- and meta-midobenzoic acid and tyrosine, with amides, such as alloxan, biuret and xanthine, in presence of a dehydrating agent, or even by heating the amido-acid alone with the dehydrating agent. The substances so produced are said to be all soluble in warm water, forming opalescent *lævorotatory* solutions, the values given for α_D varying between -38 and -52; but it is not clear what is meant by this, as the symbol α_D is commonly used to denote the observed rotatory power, and is meaningless unless the strength of solution, &c., be stated, from which the specific rotatory power can be deduced.

Dr. Pickering does not appear to be aware that if his statements are correct, he has made a discovery of a startling character, altogether remarkable in the light of our present knowledge. In all cases hitherto studied—not excluding nitrogen compounds (*e.g.* artificial conine)—as Pasteur foresaw would doubtless be the case, optically active substances are never directly produced; the synthetic product is always inactive. For example, when tartaric acid is synthesised, a mixture is obtained consisting of mesotartaric acid—the internally compensated modification—and racemic acid, this latter being resolvable into equal quantities of the two equally but oppositely active tartaric acids.

It is therefore desirable that Dr. Pickering should state exactly what is the evidence on which he relies as proving that the substances he has obtained are possessed of optical activity. Having reason to think that nitrogen may manifest peculiarities hitherto unsuspected, I await such information with impatience.

Dr. Pickering speaks of having obtained several of the substances in translucent yellowish plates. What are we to understand from this? It would be interesting if we knew whether the substances are crystalline.

Grimaux, who has discussed coagulation phenomena in a thoroughly scientific manner, has pointed out that the proteids do not differ as colloids in any essential manner from mineral and other colloids such as Graham investigated; and it is perhaps, therefore, fair to question whether the production of intravascular coagulation, on which Dr. Pickering lays stress, is so significant a property as he supposes as indicating affinity with true proteids. The substances he has obtained cannot well, from the chemical point of view, bear any real structural relationship to natural proteid substances.

HENRY E. ARMSTRONG.

Carbon in Bright-Line Stars.

DR. HUGGINS should verify his references; it has taken me some considerable time to find the article he erroneously states to be contained in vol. xlviii. of NATURE.

That article was an attempt to summarise a good deal of work I had communicated to the Royal Society, with all necessary details.

To avoid the necessity of giving these details in the article, I distinctly stated that "in the Bakerian lecture for 1888 I gave a complete discussion of the spectra of bright-line stars," and referred to the "bright fluting of carbon which extends from 468 to 474."

The details were thus stated in my communications to the Royal Society:—"The bright band, with its maximum at 468, is the bright carbon fluting commencing at 474, and extending towards the blue with its maximum at 468, as photographed at Kensington" (*Roy. Soc. Proc.*, vol. xliv. p. 37, March 1888).

"It is necessary to state that the maximum luminosity of the blue band, under some conditions, is at about 468. As I have so often had occasion to refer to this, I here reproduce one of the many photographs of the spectra of carbon compounds which show it" (*Roy. Soc. Proc.*, vol. xlv. p. 169, November 1888).

In a paper communicated to the Royal Society, on November 9, 1889, in which this blue band of carbon is very frequently referred to, both in connection with comets and bright-line stars, its position is throughout defined by the figures 468-474, whether the brightest part was at 468 or 474. I had previously shown that the maximum might be at either wave-length in the spectra of different comets, and my earlier papers had sufficiently stated that in the case of bright-line stars the modified band, with the maximum at 468, was in question. Thus in comparing the spectra of comets and bright-line stars, "468-474" was used as a short title for the blue band, whether in flame, comet, or star, and this applied also to the new observations which were recorded at the same time, showing the coincidence of the star band with the spirit-flame band. With the instrument employed, the whole group in the flame spectrum appears as little more than a broad line; but that the previously noted shift of the maximum to 468 was simply regarded as ancient history, is shown by sketches in the Solar Physics Observatory note-books, which I shall be glad to show Dr. Huggins, if he cares to see them.

I certainly see no reason to withdraw my assistants' observations of the blue band, but in the article which has given rise to this discussion (NATURE, January 28, 1897) I regarded them as superseded, as most of Dr. Huggins' observations have been, by recent observations, made with much greater optical means. I am not aware that it is customary to formally withdraw *observations* which have simply been superseded with the help of improved instruments.

I retained the observations of the green fluting, however, for reasons sufficiently stated in my article.

Dr. Huggins apparently objects to my statement that Prof. Campbell does not discuss the origins of the lines and bands which he has measured, but it will be seen by his quotation from Prof. Campbell's paper that my statement is amply justified. Prof. Campbell makes only a general reference to the question of origins, and has only compared with "well-known artificial spectra." It is not quite clear what is meant by "well-known" spectra, but presumably it is the published tables of lines seen in the arc and spark spectra of the more familiar substances which are meant; these lines, however, would not be the only ones to be expected under the exceptional conditions which exist in a bright-line star. The experimental work on the blue band of carbon is only one indication of the necessity for observing terrestrial spectra under special conditions for such an investigation. It is certainly impossible that the resources of a comparatively young institution like the Lick Observatory can be sufficient to cope with this inquiry into origins.

With reference to Prof. Vogel's observations of the varying position of the maximum of the carbon band, I can only repeat the statement of my regret that I had forgotten them when my paper of 1888 was written. I have nowhere stated that this observer ascribed the blue band in the bright-line stars to carbon, but it is certainly strange he has not done so, since a similar band at about the same wave-length in the spectrum of comets was experimentally demonstrated by him to be probably due to carbon.

It would appear that if Dr. Huggins had done me the honour of reading my communications to the Royal Society, his letter would not have been written. J. NORMAN LOCKYER.

February 9.

Origin of the Cultivated Cineraria.

THE discussion in these pages, rather more than a year and a half ago, upon the origin of the "Cultivated Cineraria," by Mr. W. T. Thiselton-Dyer, Mr. W. Bateson and others, was productive of very considerable interest. It raised in my mind the idea of producing some living evidence on the question; but the unfortunate position is, that certain kinds required for the purpose are not in cultivation. I venture to appeal, therefore, to the readers of NATURE, who were interested in the discussion, and who may visit or live in the Canaries or Madeira, to be so good as to send me any seed they may be able to obtain. It is desirable to have seeds of all the herbaceous species of *Senecio*, without exception. *S. Tussilaginis* I consider important, whatever the facts of origin may have been, and it is necessary to have a new stock of *S. cruentus*. I have already used the material at command, and have made a variety of crosses among four distinct types. The set first in flower was exhibited, on account of its showy features, at a recent meeting of the Royal