

### Letters to the Editor.

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#### The 'Forbidden' Line of Mercury at $\lambda 2270$ in Absorption.

IN a letter to NATURE of May 28, p. 778, I stated that the line  $\lambda 2270$  which is 'forbidden' by the selection principle for inner quantum numbers, could not be observed in absorption. I have now repeated the attempt with a more powerful instrument, which has been placed at my disposal by a grant from the Council of the Royal Society. A definite positive result has been obtained, the line showing up clearly in absorption by a column of mercury vapour 45 cm. long boiling at a pressure of 95 cm. It is well seen on several different negatives. The range of conditions for observing it is very limited. Too much vapour blots out the continuous background; too little fails to show the line absorption.

The observation seems of considerable theoretical interest, as showing that direct transition from the normal to the metastable excited state of the mercury atom can sometimes occur, even though very rarely. The resonance line of mercury,  $\lambda 2537$ , would, I believe, show up in comparable intensity with the same column of mercury vapour at the atmospheric temperature; thus at about one millionth of the density used for  $\lambda 2270$ .

RAYLEIGH.

Terling Place, Chelmsford,  
Aug. 10.

#### Prof. Labbé's Copepod 'Allomorphs.'

PROF. LABBÉ, in the succession of papers in which he has expounded his theory of allelogensis, claims to have established as a fact that, in the salines of Croisic and in the aquaria of his laboratory, an evolution of Copepoda has been observed, leading through a series of eight stages from *Canthocamptus* to *Cyclops*. According to his theory, the eggs laid by one species of Harpacticid may produce 'allomorphs' which, according to accepted standards of classification, would be considered to represent distinct genera or even families. If such were indeed the case, and it were possible in seven years to observe the transformation of *Canthocamptus* into *Cyclops*, it would indeed be necessary for systematists to abandon their task in despair.

Those who are not specially conversant with the detailed systematics of the Copepoda will appreciate the position more clearly if it were stated in terms of a more familiar group. One may, I think, quite fairly say that it would be much less surprising if the egg of a sparrow were to produce a robin, and the robin's egg a swallow, than if the eggs of a *Canthocamptus* gave rise to *Wolterstorffia* and those of the latter to Copepods having the characters ascribed to *Ferroniera*. Such revolutionary results are obviously unacceptable, and should not even be considered, unless supported by the most scrupulously exact descriptions and experimental evidence. Prof. Labbé in his most recent paper (*Arch. Zool. Exp. et Gen.*, 66, pp. 135-290; 1927) states (p. 246), "Nous avons maintenant une sériation complète de stades qui donne la preuve de l'arbre généalogique. C'est cette preuve qu'apporte le présent travail," so that

we may suppose that he has now offered all the evidence which he is prepared to give. I have already (NATURE, Sept. 4, 1926) given some reasons why such evidence as he has previously offered is insufficient, and it is only necessary to consider that which is now brought forward.

First, with regard to the descriptions of the 'species' with which Prof. Labbé deals. A detailed discussion of each of these would be a long and tedious business and, for reasons which I shall give, it is scarcely practicable or necessary, but one or two cases must suffice.

Prof. Labbé does not appear to have availed himself of the most indispensable systematic work on Copepods, Prof. Sars's "Crustacea of Norway." Had he done so he would scarcely have redescribed *Metis ignea* Philippi under the new name of *Parametis sanguinea*. That the two are identical there can be no doubt, and a comparison of his figures with those of Sars will give some gauge of the accuracy of Labbé's figures in general.

The original parent form of the experimental cultures from which were derived in "huit étapes successives" *Wolterstorffia croisicensis*, *Ferroniera mirabilis*, *F. cyclopoides*, *Regis servus*, *Herouardia paradoxus*, *Cyclops phaleroides* and *C. serrulatoides*, is called *Canthocamptus salinus*. This is the species named in previous papers *C. minutus* O. F. M., but Labbé has recognised that the original identification was unfounded (p. 209). A short description and some figures of this parent form are given which require comment. In the first place, the 1st antenna of the female is stated to be of seven "très courtes" joints, but it is figured (Fig. 41) as of seven unusually long joints, the fifth of which bears an æsthete. It may safely be said that this is not the antenna of a *Canthocamptus*, and indeed I am not aware of any genus or family of Copepod to which it could possibly be referred. Secondly, the first leg of the male and female are shown entirely unlike, and in neither case with an inner seta on the 2nd joint of the exopod. I do not know of any Harpacticid showing such an extraordinary sexual difference, and, if the figures are correct, it is quite clear that the animal is not a *Canthocamptus*, and that two species have been confounded in one description. What these may have been it is impossible to suggest. There is reason also to suppose that the same error, namely, that of giving figures and drawing up descriptions from different species and uniting them under one name, has given rise to others of the remarkable forms here dealt with. It would not be difficult to produce some very striking new creatures by such combinations!

If Fig. 103 of the 2nd antenna of *Ferroniera cyclopoides* male be compared with Fig. 117 of the same appendage of *Rhynchoceras rota*, a strong suspicion is aroused that in this case the same male has been attached to both species. The appendage as figured is so extraordinary that it could scarcely be identical in two species of different genera. The sexual difference in this appendage is, so far as I know, a new discovery by Prof. Labbé.

It is possible, in some cases, to suggest or to affirm the identity of some of these new species with others already known, but it is not worth while discussing them all in detail:

(1) The genus *Portierella* is very extraordinary, and the two species of it in some respects so unlike that they cannot possibly be congeneric if the figures are correct. It seems probable that there is a mixture here too, and that some species of *Tisbe* forms part of it.

(2) *Rhynchoceras rota* may, in part, be *Tachidius*

*brevicornis*. *R. elongatus* is certainly *Eulerpina acutifrons*, but the "1st gnathopod" does not seem to belong to it.

(3) *Regis servus* is *Wolterstorffia confluens* Schmeil. It is possible that *R. racovitzai* may be *W. blanchardi*.

(4) *Herouardia paradoxus* certainly includes *Hali-cyclops magniceps*, but some of the figures cannot be correct. For example, in no known Copepod is there an exopod on joint 3 of the 2nd antenna, and leg 1 (Fig. 176) has some quite unique characters. Labbé lays great stress on the presence of a spiny operculum in his new species, and, if it actually existed, it is, of course, impossible that he could have been dealing with *H. magniceps*. On the other hand, he figures it (Fig. 174) on the ventral side and attached to the fourth abdominal somite—a quite impossible situation.

(5) *Mesquieria caerulea* is *Acartia latisetosa* Kric.

It appears, then, that reliance cannot be placed on the accuracy of Labbé's descriptions and figures. Further, if he has in some cases confounded two or more species in one description, it seems that the whole edifice of theory which he has built on his facts must crumble, for the succession of forms on which it is based disappears.

With regard to the reliability of the experimental methods, it does not seem necessary to say much since (p. 211) Labbé himself admits that no attempt has been made to prevent contamination of the cultures by the introduction of extraneous nauplii. He disposes of this difficulty simply by saying that his interpretation is more probable than that species should always have been introduced in the same order, and by the statement that the allomorphs always appeared in his cultures long before they were "generated" in the salines. On the other hand, we are not told anything about the number of cultures in which this order of succession was observed, or indeed anything whatever about these observations, so that it is impossible to weigh the probabilities. It is very necessary to know more about them. For example, these Harpacticids are minute creatures creeping about in mud and vegetation, and in any culture in which they would be able to thrive it would be most difficult to remove and examine the whole population. They can seldom be recognised except under high powers of the microscope, and without examining the whole, or at least a large part, of the population of an aquarium, it would be rash indeed to say that all the individuals belonged to one species. A very small aquarium stocked as Labbé's seem to have been stocked might readily contain half a dozen species, and it might involve a lengthy examination before all of them were discovered. The Harpacticids provide peculiarly bad material for an investigation of this kind.

It is most remarkable that the salines of Croisic should contain only an assemblage of new species and genera and lack so many that are characteristic of such places. For example, no species of *Amphiascus* is mentioned; no *Tachidius*; no *Mesochra* and no *Stenhelia*. Labbé's identification of *Nitocra hibernica* is obviously wrong, and there can be little doubt that other species of this genus actually occur. As I have pointed out above, some of these genera were probably actually present and have been described under other names.

Almost every page and paragraph of this paper provokes criticism, but it seems scarcely worth while to pursue the subject further or to deal with Labbé's views on the systematics and comparative morphology of the Copepoda. They need not be taken seriously. The only question which concerns zool-

ogists is whether or not species at Croisic are giving rise by allelogenesis to new species or genera. If this paper contains all the proof which Labbé is prepared to offer, one can say with complete certainty that there is no substantial evidence that such is the case.

ROBERT GURNEY.

Ingham, Norwich, Aug. 3.

#### Ectoplasmic Matter.

A PROTEST should surely be made against the statement of the reviewer on page 111 of NATURE for July 23 that "various kinds of . . . ectoplasmic formation are facts of experience." The number of persons, among those competent to form an opinion, who are of this belief, must be a very small minority, and the supposed existence of ectoplasm is no more proved than that of any other psychic phenomenon.

One of the proofs of the existence of ectoplasm relied upon by Dr. Geley in the book to which the review refers are wax masks of spirit hands. As has recently been shown by Sir Arthur Keith and others, these can easily be counterfeited, wax being a substance that readily becomes plastic and capable of fraudulent manipulation at quite low temperatures.

I have, therefore, elsewhere recently made the suggestion that these masks would be more conclusive if made, say, in cast-iron or some other metal which is rigid and nonplastic at ordinary temperatures; but I fear that ectoplasm would frizzle just as easily as the living hands of the mediums or of their confederates, which, I am convinced, are the real agents involved.

A. A. CAMPBELL SWINTON.

THE complete sentence in my review was: "It must now be admitted that the various kinds of lucidity and of ectoplasmic formation are facts of experience as actual, though as sporadic, as hypnotism, insanity, or physical deformity." Mr. Campbell Swinton's protest is interesting, because it seems to imply that all facts of experience must be scientific facts and, inversely, that all scientific facts are common facts of experience. The gist of the review, as well as my previous communications on psychic phenomena (Oct. 23, p. 588, and Nov. 13, 1926, p. 693), is to the effect that no 'proof,' in the strictly scientific sense, has been obtained of any supersensible phenomenon. Many 'facts of experience' cannot be explained as yet by exact science, which requires a formula so that the experience may be repeated or prevented at will. Again, much of the phenomena of scientific laboratories are not general facts of experience and are accepted credulously and without understanding by the lay majority. Such common facts of experience, known to the majority as disease, deformity, dreams, and insanity, are admitted to be actual, but they do not, therefore, come under exact science, since the laws underlying these states of matter have not been clearly, that is scientifically, defined. Science has advanced and will continue to advance by discovering the laws underlying all facts of experience, thus bringing the latter under self-conscious control.

Uncommon facts of experience, known only to the minority, are not readily admitted by the majority, for the very good reason that experience is an individual matter. To 'believe' in the reality of another's experience one must have had an analogous experience unless one understands the laws behind or is an undeveloped, credulous person. This is a beneficent law of individual development, and a protection against superstition and charlatanism. On the other hand, we cannot believe that all those who have had experiences unknown to ourselves are fools or knaves.