upon Neuroptera and Diptera. Among insects, the Lepidoptera were studied with especial care, the type selected being *Thyridopteryx ephemeraiformis*. The careful account of this embryology, together with the numerous excellent figures, entirely substantiate the author's claim that the study of this type, "if it has brought to light nothing new, has, in the opinion of the writer at least, settled some important points connected with the embryology of this group of insects." In the account of segmentation and the formation of the blastoderm, the author completely confirms Bobretzky's descriptions. The development of *Thyridopteryx* occupies twelve quarto pages: for the details the original must be consulted. The account of the embryology of Orthoptera, represented by *Mantis* and the grasshopper, and of the embryology of spiders, is also very complete.

At the end of the paper many interesting and suggestive conclusions are appended. Among these it is significant that a writer who has done so much work upon the early stages of Limulus should unhesitatingly regard this latter form as an Arachnid. The Trilobites he considers as "possibly the ancestral form of Limulus."

Only a short account of this excellent paper has been given here. All those interested in embryology, and the light shed by it upon morphological science, will, of course, make a careful study of this work. E. B. P.

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.*]

The Satellite of Procyon.

IT is well-known that Procyon, like Sirius, does not travel through space in a straight line, its motion undergoing changes similar to those which would result from the disturbing action of a large satellite. This explanation was advanced by Bessel in 1844, and though the satellite has not yet been *seen*, its existence has been placed beyond reasonable doubt by Dr. Auwers's investigations on the subject.

Does it not seem probable that this interesting body may be revealed by the aid of photography? It is now possible to photograph stars and nebulæ which are beyond the visual range of the most powerful telescopes; and if the companion of Procyon, like that of Sirius, be self-luminous, there would seem to be a good prospect of obtaining its image on the sensitive plate.

As the companion is doubtless in pretty close proximity to its brilliant primary, it will be necessary, for photographic purposes, to intercept the image of the latter by means of a suitable screen. Since the *direction* of the satellite at any time can be found from Dr. Auwers's elements,[1] there would be no uncertainty as to the position in which this eclipsing disk (or wire) ought to be placed, though its proper adjustment would be a somewhat delicate operation. Should the satellite be photographed, its position will become known from its configuration with respect to other stars recorded on the negative.

If a very large telescope were employed, the images of both components, as distinct and separate dots, might be obtained on the plate. For Procyon, a very short exposure would be requisite, and this could be secured by the use of a movable stop or screen, similar to that devised by Prof. Pritchard, and used for parallax work at the Oxford University Observatory.

It is scarcely necessary to dilate upon the interest which would attach to a photograph showing Procyon's companion. As the parallax of Procyon has been satisfactorily determined by Dr. Elkin and others—being 0″·266 according to the Yale College observations—we could ascertain the *actual* as well as the *relative* masses of the two components. And the brightness, or more strictly speaking the photographic magnitude, of the satellite might also be determined with some precision.

[1] At present the position-angle of the satellite is *about* 233°. Its *distance* probably amounts to but a few seconds of arc, and may be within 2″.

It may not be too late to obtain such results during the present season, but exposures of four or five hours, under good conditions, will not be practicable before next winter.

J. M. BARR.

St. Catharine's, Ontario, Canada, March 4.

"Les Tremblements de Terre."

THE issue of NATURE for February 7 (p. 337) contains a review of the little work on earthquakes published for me by Messrs. J. B. Baillière. The anonymous author of the article makes several criticisms on my book to which I desire to reply.

Your critic thinks it a grave fault not to have entered into a detailed description of the seismographs and seismometers at present in use. He reproaches me in particular for having but just mentioned Prof. Ewing's duplex pendulum seismograph; for having omitted to speak of the same inventor's horizontal pendulum seismograph; and especially for seeming to ignore the experiments made with these instruments by Prof. Ewing in Japan. I confess that I had not been struck by the excellence of the instruments in question, and that it was not through an oversight that I omitted to describe Prof. Ewing's observations in Japan, while I quoted in detail those of his *confrères*, Messrs. Milne and Gray, in the same country.

Your critic defends with some acerbity a certain class of seismographs, and wrongly accuses me of failing to appreciate the principles on which their construction is based. The objections which he makes to my treatment of M. Cavalleri's pendulums of unequal length are entirely refuted from a theoretical point of view by the learned note due to M. Poincaré, which is inserted on p. 46 of my book. I need not insist further on this point.

Your critic thinks I have not done sufficient justice to the work of the Italian *savants*: he forgets the limits necessarily imposed on a book destined especially to give to the general public an idea of the present state of an important question.

The writer of the article regards the seismographs of to-day as perfectly sufficient for all scientific needs. I am far from being the only person engaged in the study of earthquakes who does not share this opinion. Finally, he describes, and not very clearly, the experiments which I made with M. Michel Lévy to measure the rate of propagation of disturbances through the soil, and the registering apparatus designed for this purpose. According to the writer, these experiments constitute the only advances we have made in the study of earthquakes. If he had rendered justice to our work on the subject, we should have been content, and I should have raised no objections to his article. But your critic reproaches us with having given results which are masked by inevitable causes of error. A more careful study of the book, and especially of the extracts from our original memoir, published in the *Comptes rendus* for 1885 and 1886, would have preserved him from so inexact an assertion. In fact, he unjustifiably mixes up the *preliminary* experiments, made at Le Creusot by means of an apparatus exactly similar to that used by Mallet and Abbott,[1] with what really constitutes the basis of our work—I mean the determinations made with the aid of photographic registration and explosives. What your critic calls the "personal equation" of the instrument is here nearly negligible[2]; and the merit of our method lies precisely in this point, which distinguishes us from our predecessors.

We would beg the readers of NATURE to verify for themselves the truth of our statement. This verification will enable them to judge of the value of the article laid before them.

F. FOUQUE.

Finding Factors.

IT may add interest to Mr. Busk's ingenious method of distinguishing between prime and composite numbers to state the algebraic basis on which it rests.

Let N be any number, and n^2 the next higher square number, and let $N = n^2 - r_0 = (n+1)^2 - r_1 = (n+2)^2 - r_2 =$ &c. r_1, r_2 are formed successively from r_0 by the successive additions of $2n+1$, $2n+3$, . . . the increments being in arithmetic progression, so that $r_m = r_0 + 2mn + m^2$. As soon as r_m becomes a square, N is expressed as the difference of two squares, and its factors are found.

[1] See p. 219 *et seq.*

[2] The only subsisting cause of error is that due to the inertia of the mercury, which we have estimated and introduced into all our calculations. (see p. 246).