Letters to the Editor.

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The Cause of Rickets.

THE scientific interest of the work that has been done and is reviewed in the leading article in NATURE of July 29, p. 137, is that it shows that ultra-violet light, acting on the skin, produces by a photo-chemical reaction a definite substance which circulates in the blood. This substance is able to replace vitamin-A in the food; whether wholly or only partially is not yet certain, although it should not be difficult to decide the question. If the former, it appears that light actually causes the formation of the vitamin, as suggested by Prof. Harden, or at all events some compound closely similar to it. Dr. Rollier finds in his sunlight treatment of tuberculosis that cod-liver oil is quite unnecessary; but, of course, his patients get vitamin-A in butter and so on. In rickets, vitamin-A can apparently be reduced to a very small amount if there is plenty of sunlight, but it is uncertain whether the vitamin can completely replace sunlight.

Looking at the evidence as a whole, it seems to me that the six or seven causes enumerated in Dr. Findlay's article may really be reduced to two and perhaps ultimately to one. These two are deficiency of sunlight and of vitamin-A. Taking the remaining suggested causes in the order mentioned, it is obvious that bone cannot be made without its constituents calcium and phosphate, and, as the article points out, this is not a matter of great practical importance, especially if a proper quantity of milk is included in the diet. As to the avoiding of cereals in favour of meat, it seems that the question here is really one of the rate of growth. There is no doubt that the more rapid the growth, the more vitamin-A is needed, probably because it is stored to some extent in the new tissues, especially if these consist of much fat. Prof. Mellanby's experiments showed clearly that the addition of carbohydrate to the diet of his puppies necessitated more vitamin because the growth was so much more rapid than on meat diet alone. It is of interest that Dr. Rollier's experience with tuberculous cases is at variance with Dr. Findlay's with rickets. Rollier finds that much meat is injurious, and that oatmeal is one of the best foods. At the same time, he deprecates over-feeding.

The next cause, rapid growth, has been dealt with above.

I am inclined to think that the factors included in bodily confinement and lack of exercise actually mean lack of sunlight. I understand that at Johns Hopkins Hospital it was found that the two factors mentioned were immaterial if exposure to ultra-violet light was given. It is very doubtful whether massage and electrical treatment have much effect. It is remarkable that the effects ascribed to these are obtained by Rollier in cases which of necessity have to lie quiet, such as tuberculous vertebræ, by the action of sunlight alone. The firmness and "tone," even growth, of the muscles is very obvious.

I doubt whether much advance is likely to be made by obscure references to increase of general metabolism as an explanation of the action of ultra-violet light. The dogmatic statement that animal protein is of especial value rests on no good evidence. Apart from vitamin-A, diet does not seem of great import-

ance, and even this vitamin may be reduced to a very small quantity in presence of adequate sunlight.

Should not "vitamin-B" in the seventh line from the bottom of the first column on p. 138 read "vitamin-A"? And also in the sixth line from the top of the second column?

I would conclude that we can reduce the effective factors in the prevention of rickets to vitamin-A and sunlight. It may be found to sunlight alone.

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I AM much obliged to Sir William Bayliss for pointing out my inexcusable mistake of writing "B" for "A" vitamin. One of the advantages of solving the rickets question may be that we shall be able to use a more definitive and memorable nomenclature. "Anti-rachitic" factor is at present plainly inadmissible; it would be pleasant to call it "Hopkins' stuff," were it not obvious that the identity of the substances which promote growth and have a preventive influence on rickets is still an open question.

On the general question it seems to me that Sir William Bayliss is too ready to accept as a demonstrated fact that ultra-violet light, acting on the skin, produces by a photo-chemical reaction a substance which is equivalent to or identical with the factor in cod-liver oil which influences growth and rickets. It is a very tempting hypothesis because it brings into line a number of apparently discrepant observations. But it neglects a great mass of clinical experience which relates the occurrence of obvious rickets to the total intake of food and to the influence of exercise and massage. This experience may not be capable of the precise formulation one would like, it may not be of any very high order of observational or experimental accuracy, but it has, I think, none the less to be taken into account. It is known too, though here again the data are not beyond criticism, that light increases the rate of general metabolism in experimental animals.

The alternative hypothesis suggested in the article seems to have the advantage of bringing all the more or less certain and uncertain data which are available into line. What is, of course, needed is a whole series of clinical experiments made with the control and precision of the observations carried out by Dr. Harriette Chick and her colleagues in Vienna. Experiments of this kind are laborious and difficult. Meanwhile the practical sanitarian can get to work with sunlight and cod-liver oil and abolish the disease before any one has found out what part of the spectrum is effective.

THE WRITER OF THE ARTICLE.

The Phenomena and Conditions of Sex-change in the Oyster (*O. edulis*) and *Crepidula*.

IN NATURE of December 15, 1921 (vol. 108, p. 500), I described an experiment from which a sexually mature male oyster was obtained of a maximum age of 23 weeks, from the River Blackwater. In this experiment a fair number of oysters born in 1921 were obtained on specially prepared shells kept isolated *in the sea* with the view of determining the conditions of sex at a known age at later intervals. With the aid of a Government grant from the Royal Society it has been possible to follow up the experiment this year with the following highly interesting results. The young oysters this year were found mostly to be sexually mature or had recently spawned. In one sample of 32 examined from shells on the south shore, River Blackwater, most of the individuals were males, but one large individual (28 × 31 mm.)

was found on July 3 to be carrying thousands of young oyster embryos. This same individual was taken to Plymouth and kept alive; on July 18 it was again examined and found to be practically ready to spawn as a male.

It is, therefore, clear that even in England, in such suitable circumstances as occurred in 1921, oysters may become mature as females in the first year of growth, and further that a one-year-old oyster which had spawned as a female in the summer following that in which it was born changed immediately after spawning into a male. An examination of about 300 young oysters from the oyster-beds, estimated as one-year-old oysters from comparison with the known one-year-olds, gave the same result as that obtained from the examination of the spat known definitely to have settled in 1921, namely, that all the smaller ones were males but that some of the larger ones were either females or had already spawned as females and were changing or had changed into males. From these results the conclusion is drawn that all oysters are born as males, but may or may not change into females at an age of one year. The proofs for this statement are not yet sufficient to establish it as a fact, and indeed actual proof could only be obtained in the most fortuitous circumstances. The kind of result required to amount to proof would be one which gave 1000 individuals all males out of 1000 individuals examined, but as oyster larvæ settle at different intervals over a period of several months, a heterogeneous population with regard to age—apart from other causes—results unless very special precautions are taken. Since, however, sex-change may occur very rapidly, a difference in age of a few months in young oysters is sufficient to give time for sex-change in a collection of rapidly growing oysters whose greatest age is not more than one year, hence the difficulty. In spite of the difficulties, however, the knowledge of the conditions of sex-change mentioned above will help towards designing an experiment to lead to a definite result.

An extremely interesting result follows the observation that an oyster may function as a female at an age of one year, namely, that Gerbe's work in 1876 (*Revue et Magasin de Zoologie pur et applique*, 3 serie, iv.) can be regarded as confirmed. Gerbe examined 435 one-year-old oysters and found 35 with spawn in the gill, 127 with eggs in the gonad, 189 with sperm in the gonad, and presumably 84 with the gonad undifferentiated. An additional observation supporting these was also obtained from the Blackwater experiment mentioned above. A few Portuguese oysters (*O. angulata*) settled on the shells at the same time as the native oysters. By good fortune one ripe male and one ripe female were obtained. An artificial fertilisation made from these two individuals gave a very good proportion of swimming oyster larvæ and quite as good as a control gave an adult Portuguese oysters.

Thus Gerbe's results—although nearly 50 years old—may be accepted and taken into account definitely in discussions on sex in the European oyster. There is nothing in his results at variance with the observations described above. The view one naturally took of Gerbe's results—prior to the writer's observations given above—was that European oysters at birth developed in approximately equal numbers into males and females, and in view of sex-change afterwards taking place from female¹ to male and almost certainly also from male to female, the sex-phenomena in this species appeared to be unique. The rapidity of sex-change in oysters must now,

¹ More than 100 cases of authenticated change from female to male have now been accumulated.

however, be taken into consideration, and the writer's view of the sex-changes in the young oyster found to be carrying embryos this year may be recapitulated. That oyster settled some time in 1921 after June 9, it is predicated that this oyster became sexually mature as a male and spawned as a male in the summer of 1921 (see NATURE, December 15, 1921). On July 3 this year this same individual had spawned as a female and was carrying thousands of young, and on July 18 this same individual was again sexually mature and practically ready to spawn as a male. Thus this oyster has already had two and probably three experiences of sex alternating from male (?) to female and from female back to male within about one year. It is hoped to follow further sex-changes in this particular oyster, which is still alive, for its last condition of sex, namely as a male, was determined by tapping the gonad through a boring in the shell. It has been found that if the boring and tapping operations be carefully performed an oyster can easily and quickly recover and cement over the boring on the internal face of the shell.

The definite information obtained from this one oyster is corroborated by sex-conditions in other young oysters taken from the grounds whose age could be determined as one-year-old oysters with practical but not with absolute certainty. As indicated above, however, the conditions in 1921, namely the unusually long warm summer, were highly abnormal, and it is not to be expected that one-year-old female oysters will often occur in British waters.

The rapidity of the sex-change in oysters is paralleled by a similar observation on sex-change in the American slipper-limpet (*Crepidula fornicata*) in the same series of experiments in 1921–22. *Crepidula*, moreover, has undoubtedly a similar span of life to that of the European oyster. Sex-change from male to female occurred in *isolated* slipper-limpets (up to 26.5 mm. long) in the same period as that observed in the oyster, but unfortunately none of the sex-changed limpets were actually in spawn at the time of examination although the gonad in several cases was full of either ripe or fairly large ova, and the penis had been reduced to a mere discoloured trace. Individuals of a similar age settled on other slipper-limpets had the normal beautifully-shaped and well-developed healthy black penis of the young male. (See Orton, Proc. Roy. Soc., vol. 81, B, 1909, p. 469.)

The conditions under which sex-change occurs in the oyster and the slipper-limpet are of much general interest. In the oyster development of both the male and female sex-elements can occur in the winter and spring period. We also know that male-elements can begin and attain full development in the summer and autumn, but as yet we have no definite information about the development of the female sex-elements in the summer and autumn. Experiments have been started to obtain information on the latter problem, but until that information is obtained one is not in a position to discuss the possible causes of sex-change in the oyster. It would appear, however, that the factor for sex-causation is within the control of the organism and not in external conditions, but it will be more profitable to await further information on sex-phenomena before discussing the question fully. In *Crepidula* there is no doubt that sex-change occurs in young forms when the young males cannot function as males; on the other hand, if young males settle down on females, they undoubtedly remain males and function as such for a variable time, which may be as long as five or six years or as short as one year, but again the underlying factor appears to be that of opportunity to function as a male. The

development of the male sexual elements in *Crepidula* is certainly independent of season, and so also apparently is that of the female sexual elements, since *Crepidula* breeds almost continuously from about March to December.

The males of *Crepidula* can certainly recognise females by some particular sense as yet unknown, as will be seen from the following facts. In examining a large number of chains of *Crepidula* (see Orton, 1909, *loc. cit.*), small to medium *Crepidula* were often found isolated and settled on the left-hand side of the females. Now this is the wrong side to permit of copulation, but in spite of the fact that copulation could not be effected these individuals were found to have an unusually fat and extensible penis capable of stretching probably twice as far as usual. In the experimental observations described above it was found that isolated *Crepidula*—certainly not older than those settled in the wrong place—had their penis absorbed. The conclusion is obviously reached that the males on the females knew that the latter were there and tried their best to reach them, whilst the totally isolated ones have resigned themselves unreservedly to a complete sex-change. It is only since proof has been obtained of rapid sex-change following complete isolation that a satisfactory explanation could be given for the phenomena of the misplaced males, but the explanation given above has for a long time been suspected of being the correct one.

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Wegener's Displacement Theory.

I QUITE agree with Mr. Lake's remarks (July 15, p. 77) as to the unsuitability of the tracing-paper method of investigating the merits of Wegener's hypothesis. All who wish to pursue the subject will do well to adopt his suggestions as to the practical method.

For some time I have been engaged on the subject, and, though I must plead guilty to the use of the tracing-paper method in the first instance—and there is this to be said in its defence, that we are attacking the master with his own weapon—results certainly warrant Mr. Lake's mild censure that for the truth of Wegener's theory to be accepted we must also believe in a great degree of plasticity for the earth's crust.

If the American coast be superimposed on that of Africa, the parts that coincide (according to Wegener, with a divergence of never more than 100 kilometres) are confined to that represented on the African coast by the distance from Kamerun to a point slightly north of the mouth of the Orange River. There is a divergence along the coast of Cape Colony, and an angle of approximately 15° between the superimposed coast of South America (N.E. coast of Brazil, etc.) and the African coast along the Gold Coast, Ivory Coast, Liberia, etc. These divergences may be easily accounted for by comparatively recent denudation, or fracture.

Assuming the truth of fracture—after Wegener—along the line Kamerun to Orange River, the Zwarte Bergen of Cape Colony certainly do fall into place exactly with the Permian cordillera of the Pampas. But this added coincidence merely leads us into greater difficulty. For to make the superimposed American coast coincide with the African coast in this manner, we have to swing the American continent through an angle of 45° from its present position.

This leaves us with Newfoundland in the position approximately 45° W., 32° N.—in the Atlantic Ocean.

The Hercynian Appalachians—another of Wegener's "test" ranges—appear in a position in the Atlantic north of Cayenne, stretching in a general N.N.E. direction (along the line 52° W., 8° N.— 47° W., 20° N.). They are in the right direction for joining up with the British Hercynian range, but are separated therefrom by a distance of ocean above 2000 miles.

To lessen this distance, and bring it within a reasonable distance of the British Hercynian range for joining-up purposes, we cannot allow any bending of the American continent. Any alteration in the relative positions of North and South America throws the direction of the Appalachians out absolutely and entirely. The only way the joining-up can be done for both the Zwarte Bergen-Buenos Ayres range and the Hercynian range on both sides of the Atlantic is either (1) a great movement of the Eurasian continent south-west, or (2) a movement of the African continent south to a distance of about 500 miles from its present position, and at the same time a rotation about an axis somewhere in the neighbourhood of Suez (for example) of not less than 50° .

In other words, since the fracture, either the Eurasian continent has been rotated in a general S.E. direction (clockwise) or the African and Indian masses in a N.E. direction (counter-clockwise), or both these motions have taken place, from a centre somewhere in the Suez-Madeira Islands line.

Are the Himalayas, the Carpathians, the Alpine system, the Atlas Mountains, the result of the clashing together of the African-Indian, European-Asiatic continents by these movements? As Prof. Sollas has reminded me, the first word on Wegener's theory lies with the astronomers and physicists. To them I leave the task of finding a force which has acted in two parallel directions west on the North and South American continents, making their advance west without rotation relative to each other and overcoming the resistance at the expense of the Andes Cordillera and its continuation in North America, and has at the same time driven the Eurasian and Asiatic continents south-east and the African-Indian continent north-east (relative to the Americas) with such determination that the great folding of the Himalayas-Alps line resulted—and waited until Tertiary times to do most of it.

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The Elliptic Logarithmic Spiral.

I AM much obliged to Mr. Wright for his correction (*NATURE*, July 8, p. 40). I had made a search in English and Continental books on curves and concluded that this spiral had been overlooked as a curve. But it appears to have been recognised in connexion with the spherical pendulum. Prof. Lamb in his "Dynamics," p. 288, as I now find, refers to the curve as "a kind of elliptic spiral," and Dr. Besant describes it as "an ellipse gradually shrinking in size."

I take, however, a little umbrage in having given the curve a name, especially as it seems to be of importance in damped elastic systems with one degree of freedom, and in fact it may be called a characteristic. Thus in the elastic system without friction, the force displacement diagram is a straight line; with fluid friction varying as the velocity, we have the elliptic logarithmic spiral; and with solid friction, a series of parallelograms. The dissipation per cycle, its rate during the cycle, as well as what may be termed the timbre of the motion, are in this view brought out very clearly.

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