adapted for running instead of swimming; and he thus explains the loss of the exopodites of the cormopods, the strengthening of the endopodites, the shortening of the abdomen, the loss of power in the pleopods, the flatness of the body and abdomen, the thickening of the integument, and the loss of eyestalks and of the antennary scale. The respiratory function of the pleopods he attributes to the loss of the carapace, and the thickening of the integument.

The general conclusions of this highly suggestive and interesting paper may be summarized as follows.

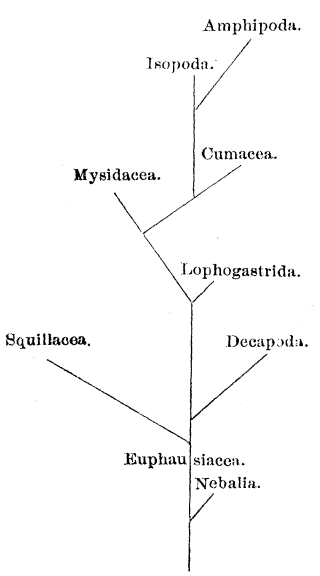
The Malacostraca are descended from the phyllopods, among which Nebalia is their nearest relative.

The Euphausiacea are the most primitive Malacostraca. The decapods originated from the Euphausiacea, although the most primitive decapods, the Natantia, are now widely separated from this ancestral form. The Squillacea stand by themselves, their nearest, although distant, allies being the Euphausiacea. They show in certain points a more primitive condition than any other Malacostraca; although, as a whole, they are highly modified.

The Mysidacea are also derived from the Euphausiacea; although they are so different from them that they must be placed in a distinct order, and the group Schizopoda must be abandoned. The Mysidacea have no close relationship to the decapods.

The Cumacea arise from the Mysidacea, and the amphipods and isopods from a form between the Mysidacea and the Cumacea. The amphipods and isopods are not a primitive group distantly related to the Podophthalmata, but they are the most highly specialized of the Malacostraca.

He gives the following as his phylogenetic classification of the Crustacea: —



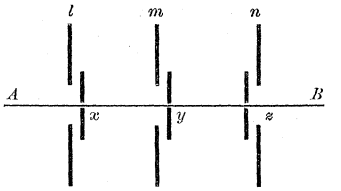
W. K. BROOKS.

LETTERS TO THE EDITOR.

Radiant heat.

MR. FITZGERALD has favored me with a paper [1] in which he takes exception to my views respecting radiant heat,[2] wherein he says, —

"Suppose that two regions, A and B, be separated by three parallel screens, l, m, and n, having apertures in them, x, y, z, capable of being opened and closed from the centre, so as to make every thing perfectly symmetrical round the line AB, perpendicular to the screens. Now, if x be opened for a very short time, a certain quantity of radiant energy will escape out of A into the region between l and m; and if y be opened when this heat reaches m, it can certainly be let on into the region mn; and if z be similarly opened when it reaches it, this radiant heat will get into B. While z was open, however, some heat left B; but, as Dr. Eddy observes, y may be closed so as not to let this even get through the screen m, and it can be all returned into B by reflection through z or some other aperture. So far I entirely agree with Dr. Eddy, and so far it seems as if the result had been to transfer heat from A to B without B's losing any heat by having it transferred to A. As I warned Dr. Eddy when I heard his paper, there are, however, other bodies and regions to be considered besides A and B. There are more than two bodies considered: there is the region of the screens. Consider what happens when the heat that escaped out of B into the mn region tries to get back into B. Some door must be opened to let it pass; and, while it is passing in, an at least equal amount of heat will be passing out of B into the mn region, so that you can never really get the heat that has once left B back into B again. This is true, whether you adopt doors over fixed apertures, such as I have supposed, or moving apertures, such as Dr. Eddy proposed. What really takes place is this: a certain quantity of heat escapes out of A and reaches B; and a not less quantity of heat leaves B, and is kept entangled in the region of the screens, and it is only possible to let the heat pass from A to B by means of this third region. Hence this only really comes to the same thing as letting A radiate some of its heat into the screen region, while B is kept closely shut up. Now, be it observed that Dr. Eddy practically postulates that this screen region is at least colder than A — in fact, he assumes it to be perfectly cold, i.e. to contain no radiant heat except what is admitted from A and B, so that it is by no means contrary to the theory of exchanges that A might cool by radiating into this region."



Now, Mr. Fitzgerald has stated only two of the three things which occur while the door z is open. He omits to state, that in my process a certain amount of heat which has come from A also passes through the door z every time it is opened, into the region B; and this is all which is proposed to be accomplished by the process which is at all unusual or peculiar. Thus the fact remains, that although a definite amount of heat from B remains entangled in the region mn, which is not increased with the lapse of time, there is a continued passage of heat through this region into B, that being the very object sought to be accomplished by my process. It is not easily seen how the arrangement of screens and apertures proposed by Mr. Fitzgerald could be so manipulated as to bring the heat coming from A into a position such

[1] On Dr. Eddy's hypothesis that radiant heat is an exception to the second law of thermodynamics. By George F. Fitzgerald, M.A., F.T.C.D., *Sc. proc. roy. Dubl. soc.*, iv. pt. i.

[2] *Sc. proc. Ohio mech. inst.*, July, 1882.

that it would be in readiness to pass into B at the same time as the heat which originally came from B is returned to B, though my arrangement of moving screens readily accomplished this, as was admitted by Prof. J. Willard Gibbs in his criticism of my paper.[1]

H. T. Eddy, Ph.D.

Area of a plane triangle.

In the *Mathematical magazine* (Erie, Penn.) for April, Mr. James Main publishes, as a matter of curiosity, a collection of ninety-four expressions for the area of a plane triangle. In *Mathesis* (Gand, Belgium) for June this list is republished; and in the August number of the same journal the subject is taken up again by M. Ed. Lucas, who extends the collection, and classifies into five groups. In the first group are eleven '*unique*' expressions for the area, i.e., expressions that do not admit of other similar expressions by permuting the letters; in the second group are nine expressions, each admitting of two other similar expressions by permuting the letters; in the third group are eleven expressions, each admitting of three other similar expressions; in the fourth group are seven expressions, each admitting of five similar expressions; and, last, the fifth group consists of a single formula, admitting of eleven similar expressions. Thus we have a hundred and thirty-six expressions for the area of a plane triangle in terms of the sides, angles, perpendiculars, semiperimeter, and radii of the circumscribed, inscribed, and escribed circles. M. Neuberg adds also three other unclassified formulae, with the statement that many other such may be found. The total number of expressions for the area of a plane triangle, in this collection, is therefore a hundred and thirty-nine, making it, perchance, the most complete collection that has been published. M. B.

The Dora coal-field, Virginia.

In the November number of *The Virginias* is contained a review of the report on the mineral resources of the United States, recently published by the U.S. geological survey, which contains the following: — "In Mr. Charles A. Ashburner's report on anthracite coal, p. 32, is this statement concerning the Dora coal-field: 'Of one of the reported anthracite localities in Virginia, that in Augusta county, recent tests with the diamond-drill would seem to prove the presence of anthracite,'" etc. In explanation of the above, I would like to state, that, by referring to the report reviewed, on p. 24 will be found a footnote as follows: "Mr. Ashburner's contribution and statistics end here." I only stand responsible for a *portion* of the statistics relating to the anthracite region in Pennsylvania (pp. 7 to 24 inclusive). I desire to make this explanation public from the fact that I do not wish to be held accountable for *questionable data* relating to a coal-field of a very uncertain character, and which I have never examined.

Charles A. Ashburner,
Geologist in charge Penn. anthracite survey.
Philadelphia, Penn.

Synchronism of geological formations.

In Science of Dec. 7 your correspondent, Mr. Nugent, takes issue with me as to my conclusions bearing upon the relative ages of geological formations, and contends that the geological and paleontological researches of the last twenty-one years (i.e., during the period that has elapsed since the publication of Professor Huxley's address referred to in my communication before the Philadelphia academy of natural sciences) have only tended 'to maintain the logical basis' on which the distinguished English naturalist rested. As the subject is a very important one, and one that has not, it appears to me, received its full measure of attention or discussion, I trust that you will permit me a little space for fuller explanation, even at the risk of repeating what has already been said in your valuable columns.

Professor Huxley, in his anniversary address delivered before the London geological society in 1862 (*Quart. journ.*, xviii. p. xlvi), maintains substantially, —

I. That formations exhibiting the same faunal facies may belong to two or more very distinct periods of the geological scale as now recognized; and, conversely, formations whose faunal elements are quite distinct may be absolutely contemporaneous: e.g., "For any thing that geology or paleontology is able to show to the contrary, a Devonian fauna and flora in the British Islands may have been contemporaneous with Silurian life in North America, and with a carboniferous fauna and flora in Africa" (*loc. cit.*).

II. That, granting this disparity of age between closely related faunas, all evidence as to the uniformity of physical conditions over the surface of the earth during the same geological period (i.e., the periods of the geological scale), as would appear to be indicated by the similarity of the fossil remains belonging to that period, falls to the ground. "Geographical provinces and zones may have been as distinctly marked in the paleozoic epoch as at present; and those seemingly sudden appearances of new genera and species which we ascribe to new creations may be simple results of migration."

Now, without wishing to enter into the minutiae of the question, I believe a little reflection will clearly show, that if, as it is contended, several distinct faunas (i.e., faunas characteristic of distinct geological epochs, and separated in age from each other by possibly millions of years) may have existed contemporaneously, "evidences of inversion," to quote my own words, "in the order of deposit, ought to be common; or, at any rate, they ought to be indicated somewhere, since it can scarcely be conceived that animals everywhere would have observed the same order of direction in their migrations." Given the possible equivalency in age, as hypothetically claimed, of the Silurian fauna of North America with the Devonian of the British Isles and the carboniferous of Africa, or any similar arrangement, why has it never happened, it may be asked, that when migration, necessitated by alterations in the physical conditions of the environs, commenced, a fauna with an earlier life-facies has been imposed upon a later one, as the Devonian of Great Britain upon the carboniferous of Africa, or the American Silurian upon the Devonian of Britain? Or, for that matter, the American Silurian may have just as well been made to succeed the African carboniferous. Reference to the annexed diagram, where D represents a Devonian area, say, in Europe, S a Silurian one in America, and C a carboniferous one in Africa, — all contemporaneous, — will render this point more intelligible.

Now, on the proposition here stated, reasoning from our present knowledge of the antiquity of faunas, and accepting the doctrine of migration, as maintained by Professor Huxley and others, to account for the possible contemporaneity of distinct faunas, it may be assumed that S (or America) will receive its Devonian fauna from D; D (Europe), its carboniferous from C; and C (Africa), a later fauna from some locality not here indicated. In other words, a migra-

[1] Science, i. 160.