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.*).

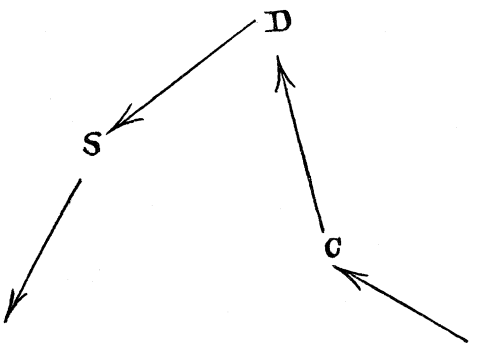
II. That, granting this disparity of age between closely related faunas, all evidence as to the uniformity of physical conditions over the surface of the earth during the same geological period (i.e., the periods of the geological scale), as would appear to be indicated by the similarity of the fossil 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.

tion, as indicated by the arrows, would set in from *D* to *S*, one from *C* to *D*, one from *S* to some possibly South American Cambrian locality, and one, bringing a Permian or some later-day fauna, from an unknown locality towards *C*. Were this order of migration to continue here, or at other portions of the earth's surface, in this or in a similarly consecutive manner, the results obtained would be in perfect consonance with the facts presented by geology. But is there any reason whatever for the continuance of this order of migration? Surely no facts that have as yet been brought to light argue in favor of a continued migration in one direction. Why, then, it might justly be asked, could not just as well a migration take place from *S* to *D*, and impose with it a Silurian fauna upon a Devonian? What would there be to hinder



a migration from *S* to *C*, placing the American Silurian fauna upon the carboniferous of Africa? Why, as I have asked, has it just so happened that a fauna characteristic of a given period has *invariably* succeeded one which, when the two are in superposition all over the world (as far as we are aware), indicates precedence in creation or origination, and *never* one that can be shown to be of a later birth? Surely these peculiar circumstances cannot be accounted for on the doctrine of a fortuitous migration. And it certainly cannot be claimed that through a process of transmutation or development, depending upon the evolutionary forces, a fauna with a Silurian facies will, in the course of a possible migration toward a carboniferous locality, have assumed a carboniferous or Permian character.

The facts of geology and paleontology are, it appears to me, decidedly antagonistic to any such broad contemporaneity or non-contemporaneity as has been assumed by Professor Huxley; and their careful consideration will probably cause geologists to demur to the statement that "all competent authorities will probably assent to the proposition that physical geology does not enable us in any way to reply to this question: Were the British cretaceous rocks deposited at the same time as those of India, or are they a million of years younger or a million of years older?"

ANGELO HEILPRIN.

Academy of natural sciences,
Philadelphia, Dec. 8.

THOMSON AND TAIT'S NATURAL PHILOSOPHY.[1] — II.

BEFORE proceeding to an account of the rest of the work, we shall add a few more words of explanation upon the harmonic solutions of the differential equation (6), expressed in polar co-ordinates. On attempting to integrate this equation, it is found that there is an infinite number of particular solutions, as was before stated must necessarily be the fact; and each of these solutions is the product of three factors. One factor is an arbitrary constant; another factor is the radius vector raised to any integral power, positive or negative; and the remaining factor is a function of the angular co-ordinates, dependent for its form upon the exponent of that power of the radius vector by which it is multiplied. It is this last factor, or coefficient, which gives the name of 'spherical harmonics' to the solution: indeed, these functions of the angular co-ordinates are themselves surface-harmonics.

If we restrict ourselves, as is usually done, to real integral powers of the radius vector r, positive and negative, then, from the well-known principle that a general solution is obtained by taking the sum of particular solutions, we should have the most general possible solution by taking the sum of a series of particular solutions, such as have just been described, in which the powers of r have all integral values between $+\infty$ and $-\infty$. But since it is found, upon computing the functions of the angular co-ordinates which constitute their coefficients, that the coefficients of r^i and $r^{-(i+1)}$ are identical, it will be more convenient to write the general solution in the form —

$$V = a_0 f_0(\theta,\phi) + (a_1 r + b_1 r^{-2}) f_1(\theta,\phi) + (a_2 r^2 + b_2 r^{-3}) f_2(\theta,\phi) + \ldots + (a_i r^i + b_i r^{-(i+1)}) f_i(\theta,\phi) + \ldots \quad (8)$$

In applying this to any given case, either all the arbitrary constants a vanish, or all the constants b; thus giving rise to the two general forms of solution before mentioned, in which there is a series of terms, either in ascending integral powers of r, or of descending integral powers of r.

A value of V consisting of several terms is a compound spherical harmonic of the degree (positive or negative) of its numerically highest power of r. A value of V consisting of a single term is a simple harmonic.

Returning, now, to the consideration of chapter vii. p. 98, entitled 'Statics of solids and fluids,' the subject of rigid solids is disposed of in the course of thirty pages, nearly half of which is occupied with inextensible strings in the form of catenaries of various kinds.

The authors hasten on to the more intricate matter of elastic solids. As is well known to students of this subject. the general problem

[1] Concluded from No. 36.