was sketched in a detailed Report communicated by me to the Geological Society on April 25, 1888. My friend Prof. Lapworth has no scientific comrade who has more frankly and practically acknowledged his great geological achievements than I have done.

ARCH. GEIKIE.

January 23, 1893.

The Identity of Energy.

I AM glad to see that in the introduction to his severely-difficult memoir, published in the *Philosophical Transactions* for 1892, "On the Forces, Stresses, and Fluxes of Energy in the Electromagnetic Field" (p. 427), Mr. Oliver Heaviside notices and criticizes some ideas of mine, published in the *Philosophical Magazine* for June 1885 and other places, concerning energy.

The statements I then made, and to which I still rigidly hold, are (1) that energy has identity like matter, and not merely conservation; (2) that whenever energy is transferred from one body to another, it is also transformed from potential to kinetic, or *vice versa*.

The basis of the first assertion is the fact that energy is always passed on continuously through space, *i.e.* that its transfer occurs along a definite path, instead of merely appearing in one place and disappearing in another.

The law of conservation would be satisfied by disappearance and equal reappearance; the law of identity requires a continuous act of transfer. The latter is true for matter, and I assert that by thinking of a number of instances, it will be perceived true for energy. In all mechanical instances, as of belts and shafting, the transfer of energy is obvious; it was not so obvious in electromagnetic actions, between dynamo and motor for instance, until Prof. Poynting clearly demonstrated that it was in accordance with Maxwell's principles.

Mr. Heaviside objects that we are not able to assert it for gravitational energy. Well, that depends on what view we take of gravitation; but I submit that until something more is certainly known about it, the safest plan is not to assert, but to assume, that in this case also what is known in every other case likewise occurs, and to trace the consequences of the hypothesis in the hope that it may lead to some conclusion verifiable or falsifiable by experiment. The reason I attach importance to this doctrine of the identity or continuity of transfer of energy is because it greatly simplifies the fundamental mechanical laws, and emphasizes without risk of vagueness the denial of action at a distance.

If action at a distance (no matter how minute) can ever occur, then indeed the continuous transfer of energy breaks down. But observe that there is no necessity for the transfer to occur at a finite velocity in order to avoid action at a distance, *i.e.* action without a medium. By the thrust of an incompressible pole, energy is transferred from butt to tip, just as really as if the compressed and recoiling layers could be demonstrated and its velocity measured. So likewise the pull of gravitation may be (and *pro tem.* I believe is) transmitted by an incompressible (or nearly incompressible) ether, so that the force is felt instantaneously (or nearly instantaneously) at all distances where matter exists; but that by no means militates against a genuine act of transfer. The conservation of matter makes experiments on gravitation difficult; if we could suddenly create or destroy a piece of matter there might be some remote chance of determining the rate at which its gravitative influence was felt. Especially if by alternately generating and destroying it we could set up a series of waves of perhaps measurable length.

And although this is as yet impossible, many known facts lead us to conclude that if gravitation has any velocity at all short of infinite, it is at least immensely greater than the speed of light. And seeing that the one phenomenon is concerned with the transverse (electric) elasticity of ether, and the other with its longitudinal elasticity, there is nothing surprising in that.

By all means, however, as Mr. Heaviside urges, let gravitation be included in general etherial equations whenever possible; and it may perhaps be wise to assume some unknown finite rate of propagation and trace its consequences with the object of verifying or disproving them.

So far as I understand, however, this is not unlike what Helmholtz did, by his generalization of Maxwell's electromagnetic theory; with the result that the course of experiment so far has been to justify the simple Maxwellian theory, and to make the longitudinal ether thrust velocity practically infinite.

And now for the second assertion, that whenever energy is transferred from one body to another, it is also transformed, and *vice versa*. This is to me not an opinion, but a demonstrated theorem (as has been shown in the paper referred to); but it must be understood in what sense I consistently use the word body in this connection. I do not necessarily mean a visible lump of matter. The molecules of a lump are to be regarded as a different "body" to the whole mass; and again, the ether everywhere embathing them is another distinct "body."

But so long as a piece of matter is merely moving through space with all the energy it may happen to contain, I do not consider that a transfer at all. There is a transfer of energy in one sense, *viz.* that of locomotion, but there is no transfer from one body to another except when work is done at their point of contact, and energy gained by one and lost by the other, being transferred across their common boundary surface. In all such cases of "activity" the energy transferred is necessarily in the first instance transformed; though by means of another transfer it may very speedily be transformed back again; and so speedily sometimes is the re-transformation effected that the intermediate condition has a tendency to get overlooked. In wave-motion a transfer and transformation occurs during every quarter period.

Mr. Heaviside seems to think that the mere convection of energy should be included as one kind of transfer; but surely that is scarcely convenient? So long as energy retains its form and adherence to one body, so long there is no true activity; no work is being done—the energy is simply stored. It may be stored in a bent spring, or in a flying bullet, or in a revolving fly-wheel. It is impossible to have kinetic energy at all without convection, and a distinction must be drawn between the mere existence of energy and the active and useful flux or transfer of the same.

Mr. Heaviside further seems to consider circuitual fluxes of energy as strange and useless phenomena. But I see no reason in this at all. The circulation of matter—for instance in the inner circle of the Metropolitan railway—is, I suppose, considered useful. The circulation of commodities is the essence of commerce. So does the circulation of energy constitute the activity of the material universe. It is the act of transfer that is beneficial (or the reverse); what becomes of a conservative quantity is a minor matter. It must go somewhere, and may very well, after a series of transfers, ultimately return to its starting point. [Parenthetically I should like to preach here against what I hold to be the pernicious doctrine of (at least amateur) political economists, that because money locally spent is not destroyed, but remains in the community, it does not much matter how much transferring power is permitted or granted to one individual,—as if the money itself were the useful commodity, and not the power of determining its direction of transfer or non-transfer. The control of every transfer should be jealously watched, for that is the greedily-desired power.]

So long as circuitual convection of energy goes on *without* transfer—as, for instance, in the rim of a non-working fly-wheel—so long the energy is merely stored; but directly a belt is fitted on with different tensions in its two halves, a portion of the energy is tangentially tapped off, and transfer and activity begin. The kinetic energy of the wheel is converted into strain or stress energy of the belt, which then by simple locomotion passes it on to something else. I perceive, however, that there is a slight difficulty about this simple case of locomotive conveyance of stress energy by a really inelastic substance; but only because the details of any infinitely rapid process are difficult to follow. I perceive moreover that in many cases it is not worth while to attend to the alternate compressions and motions which constitute a longitudinal pulse, and that the idea of simple locomotion may be conveniently introduced to cover the case of a stressed body moving; but the convenience is I think only attained by shutting our eyes to the essential processes which in all actual matter must be occurring.

I trust that Mr. Heaviside may find time to notice this letter, and attack anything he disagrees with, in order that the whole matter may become thoroughly clear.

OLIVER LODGE.

A Proposed Handbook of the British Marine Fauna.

I AM obliged to Prof. Thompson for his criticism of my scheme, although only one of the points he raises is new to me—as I think it will be to most zoologists—*viz.* that "there are no nematophores on the stem" in *Antennularia*. I thought A.

ramosa had nematophores on the stem, and I think so still. Some of his other remarks are so very obvious as to have scarcely required mention, at any rate to biological readers; a few, however, are just such debatable points as I was anxious to have opinions upon from as many naturalists as possible, and I am glad to know Prof. Thompson's. I am glad to say a number of biologists have written to me, since the scheme appeared in NATURE, expressing general approval, and criticising various points of detail, and some of them kindly making offers of assistance in special groups—and without that kind of assistance from specialists I need scarcely say it would be impossible to carry out the work satisfactorily. The proposal was first brought before the Biological Society of Liverpool on November 11, and it was only after some weeks of intermittent discussion with some of my friends in that Society (such as Dr. Hanitsch, Mr. Isaac Thompson, and Mr. A. O. Walker) who are specialists in certain groups of marine invertebrata, and after correspondence with Canon Norman and other biologists, that I sent the scheme to NATURE, with the view of getting further opinions. Consequently some of the debatable matters alluded to by Prof. Thompson (limits of British area, introduction of certain non-British forms, specific nomenclature, how to treat records of size and distribution, best terms to use for zones of depth, and, I may add, for relative abundance) have already been considerably discussed. The other points raised by Prof. Thompson in connection with *Antennularia* only require a few words. I said *A. ramosa* was usually branched. Prof. Thompson says it "may sometimes" be unbranched. The difference between these statements is slight. As to dimensions, a zoophyte which grows to 12, or occasionally to 24, inches in height, will, of course, be also frequently found of smaller sizes; and it might be the best plan to give the extreme range, say, 1 to 24 inches. What I gave was the fair average size of most of the specimens dredged or seen in collections, which I still consider to be 6 to 9 inches.

The rest of Prof. Thompson's contention is practically that there are great difficulties in the way of drawing up such a book of the known British marine invertebrate animals, and that if it is ever done it will be more or less incomplete, because Canon Norman and others (I hope including both Prof. Thompson and myself) will continue to find new British animals. That is perfectly true—in fact obvious—but the same objection applies more or less to every work on systematic zoology that has ever been published; and I do not consider that because our British Pycnogonids, and some other small groups, are still very imperfectly known, that is any sufficient reason for delaying indefinitely an attempt to deal with the rest of the invertebrata. On the contrary my opinion is rather that an approximation is better than nothing, and that every group, or every family, reduced to "Handbook" form with specific diagnoses and figures must be a distinct gain. I hope Prof. Thompson will not think that I am trying to dispute all his criticisms, or that I am ungrateful for the trouble he has taken. I have no doubt that he could correct me in many details, and give me great assistance in records, &c., of zoophytes, pycnogonids, and other groups, and I hope he will do so. W. A. HERDMAN.

University College, Liverpool, January 20.

PROF. D'ARCY THOMPSON'S letter raises a question which is, I think, well worthy of Prof. Herdman's consideration. That a handbook of our marine fauna is needed cannot for a moment be doubted, and the only matter that calls for discussion is one of scope and method, of ways and means. Prior to the appearance of Prof. Herdman's circular and article I had intended, if possible, to bring this very matter before the British Association at its next meeting, believing that a select Committee of the Association would best be able to further the interests of marine zoology in this respect. But, as the matter now stands, I leave any such action very willingly to Prof. Herdman's initiative.

Put broadly (although I well know that such a work in Prof. Herdman's hands would by no means have the character of a mere compilation), the question at issue is whether the handbook should be mainly a compilation from existing material, or should express the work of various specialists and be based upon a series of special investigations. For myself I agree with Prof. Thompson, and for the same reasons, that the adoption of the latter alternative would be likely to meet our needs most fully and satisfactorily. It would ensure, as far as possible, the equal treatment of the various groups, and would thus give to

the book (which is important) a more permanent and authoritative value than could be attained by a book depending upon the personal labours of one zoologist. I feel confident that, should Prof. Herdman admit the force of this consideration and be willing to edit a handbook in which the diagnoses were drawn up for the various groups by specialists or specially-chosen investigators, he would find no difficulty whatever in meeting with willing co-operation.

But I hardly see the point of extending the scope of the work to the extent which Prof. Thompson would seem to desire. We need a handbook for use around the coasts of our own islands. To include the fauna of the whole North Atlantic would needlessly add to the size of the work, delay the time of its appearance, and even in the end be incomplete; while it is doubtful whether the advantages would at all outweigh these defects.

W. GARSTANG

Marine Biological Association, Plymouth, January 20.

Fossil Plants as Tests of Climate.

MR. J. STARKIE GARDNER, in his interesting review of Mr. Seward's valuable essay, makes a statement which I fancy may be misinterpreted at page 268 of NATURE, where he speaks of the fragmentary character of the Arctic tertiary plants, and the inexperience of the collectors. He doubtless is referring to the remains of certain supposed "palms and cycads in the Greenland Eocene," but those who have not followed this branch of Arctic research would hardly gather from the review that Prof. Heer has determined a magnificent flora of more than 350 species from these northern tertiaries, and that he at once pointed out the absence of tropical and subtropical forms, and the fact that large leaves are not only perfectly preserved up to their edges, but that upright trees associated with their fruits and seeds prove them to have grown on the spot. "Thus of *Sequoia Langsdorffi*," he writes, "we see not only the twigs covered with leaves, but also cones and seeds, and even a male catkin."¹

In April 1875 I endeavoured to give an abstract of all that was then known of Arctic geology, in a series of articles that appeared in your columns (NATURE, vol. xi. pp. 447, 467, 492, and 508), and added some general conclusions of my own, which are further accentuated in the joint communications of Colonel Feilden and myself to the Geological Society in 1878, and in the "Geology Appendix" to Sir George Nares' "Voyage to the Polar Sea," in which expedition Colonel Feilden played a most valuable part. I have ever since carefully followed the progress of Arctic research, and am now of opinion that looking to the identity of a large number of species (often extending to the varieties of the same) occurring in the Silurian, Carboniferous, Lias, Oolite, Cretaceous, and Tertiary strata of the Arctic regions, with those occurring in similar strata in Europe and other parts of the world, they point to a common temperature over these areas and probably over the whole world, from Silurian to early Cretaceous times, and that this was the case does not appear to me to be affected by the question as to whether or not these deposits were homotaxeous.

In late Cretaceous times commenced *horizontal* variation of cold, or what we now term "climate," though previously *vertical* variation had evidently been present, for the later investigations of Messrs. Blanford appear to place beyond doubt the existence of glaciers in geological times, as was suggested in 1855 by my lamented chief, Sir Andrew Ramsay; but I equally fail to see that the slightest evidence has been anywhere adduced to support the theory of "recurrence of ice-ages," originated by my talented colleague the late Dr. Croll, and now supported with a "modification" by Sir Robert Ball.

The facts, whether we look to the history of plant life, or animal life, or the character of the rocks themselves, appear to me to be all the other way, as they disclose nothing resembling the refrigeration that, gradually increasing in the Tertiary epoch, culminated in the Glacial episode, which choked up the North and Irish Seas with an ice-sheet since man has been an occupant of our islands.

CHAS. E. DE RANCE.

H.M. Geological Survey, Alderley Edge, Manchester.

Racial Dwarfs in the Pyrenees.

IN consequence of evidence that I had obtained as to the existence of a dwarf race in Spain, I wrote to Mr. McPherson,

¹ "On the Miocene Flora of North Greenland," by Prof. Oswald Heer. Translated by R. H. Scott, F.R.S., Brit. Assoc., 1897, pp. 53.