Race de nain dans les Pyrénées.

Par suite de l'évidence que j'ai obtenue à propos de l'existence d'une race de nain en Espagne, j'ai écris à Mr Mc Pherson, notre consul à Barcelone et j'ai ci inclus sa réponse. Il y a depuis longtemps des rumeurs sur la survivance d'une race de nain préhistorique ou une race existant dans une partie de l'Espagne mais des demandes soigneuses faites à Madrid ont manqué d'ajouter aucune information définie sur ce sujet. L'été dernier en lisant un ancien numéro du Kosmos (Paris 1887), j'ai trouvé un paragraphe ~~grief~~ bref concernant une petite race ayant été trouvée dans la province de Gerona, Espagne qui avait faiblement les yeux Mongolian les figures jaunes ~~plates~~ larges carrées haut de 1m10 à 1m15 et les cheveux rouge. Un monsieur Australien m'a dit récemment avoir vu sur la place du marché ~~à~~ Salamanca quelques paysan d'un taille au dessous de la moyenne avec des figures larges et les cheveux crépus couleur d'acajou. Vous verrez que ces rapports s'accordent et que ces nains et ceux de l'Afrique sont précisément semblables.

J'ai obtenu une quantité d'information d'une vielle femme espagnole qui appartient à une famille de nain de race croisée ou qui ont une touffe

de laine
~~d'étoffe~~ rouge et sont aussi petites que les petits garcons ordinaires. Mais ces touffes d'étoffe sont particullièrement caracteristiques des races de nains de partout. J'écrirai plus longuement des informations parmi les nains de race croisée; mais ils sont d'un interêt très secondaire maintenant que nous pouvons trouver la race pure des nains avec des recherches faciles. Il est heureux qu'ils oivent dans la vallée de Ribas et le col de Tosas à une distance d'une demi journée de Toulouse Quelques explorateurs ou touristes dans le sud de la France peuvent peut être se sentir inclinés a faire une visite a ces petits peuples. Si l'ont agissait ainsi et si l'ont avait des résultats satisfaisant Je serais heureux de recevoir une ligne a ce sujet adressée à Pall Mall 28. Halburton.

Consulat anglais Barcelone 10 Decembre 1892

Cher monsieur. Depuis que j'ai reçu votre lettre du 18 Novembre et son contenu je me suis efforcé d'assurer cette verité qu'il y a a l'exposition ces pigmies ou nains qui existent dans la Vallée de Ribas. Des conversations que j'ai eues avec des personnes differentes qui ont visité cette contrée il m'est apparu certain qu'une race d'hommes d'environ un mètre à un mètre 2 de hauteur d'un teint noirâtre (cuivre coloré) cheveux noir et laineux et plats nez large, vit dans cet arrondissement particullièrement dans le « Collado de Losas. Ils sont actifs et sont généralement employés comme bergers. Il est aussi certain qu'ils ne sont pas très intelligents et qu'ils paraissent comprendre et se faire comprendre avec difficulté. Ce serait un voyage facile d'aller à cet endroit de cette ville. Je n'ai pas eu peu de difficulté à trouver qu'une telle race vit là car plusieurs personnes avec qui j'ai parlé sur ce sujet étaient évidemment embrouillées et m'ont embrouillé, à coté de cela ~~évidemment~~ parmi les races de pigmées, il y a dans ces environs quelques « crétins » qui me furent indiqués comme s'ils étaient nains. Je suis maintenant certain qu'il y a des crétins et des pigmées dans la vallée de Ribas. Il est établi que les nains ont disparu rapidement et que plus tard plusieurs sont mort de la petite vérole. Ces hommes vous ~~parleront de~~ disent que ceux qu'on voyait à Salamanca sont natifs de Batuecas ou plutot de Los Hurdes. Ces hommes étaient découvert au seizième siècle et ils étaient alors et même encore maintenant dans un état presque complet de sauvagerie. Le reste de cette race est négligé et il ne parait pas qu'ils sont nains. E G H. Votre très dévoué

Wm Mc Pherson.

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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 versâ*.

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 versâ*. 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 circuital 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 circuital 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 debateable 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 debateable 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., 1867, pp. 53.

our Consul at Barcelona, and enclose his reply. There have long been rumours of survivals of a dwarf or a prehistoric race existing in parts of Spain, but careful inquiries at Madrid failed to supply any definite information on the subject. Last summer on reading over an old number of *Kosmos* (Paris, 1887), I found a brief paragraph referring to a pigmy race having been found in the province of Gerona, Spain, who had slightly Mongolian eyes, yellow, broad, square faces, height from 1 m. 10 to 1 m. 15, and red hair.

An Austrian gentleman recently told me he had seen, in the market-place at Salamanca, some very under-sized peasants, with broad faces and mahogany-coloured woolly hair.

You will see that these accounts all agree substantially, and that these dwarfs and those of Africa are precisely similar.

I have got a deal of information from an old Spanish woman who belongs to a half-breed nano family, and who says that there are in such families frequently nanos (or "enanos") who have red tufts of wool, and are as small as ordinary small boys. But these tufts of wool are peculiarly characteristic of dwarf races nearly everywhere.

I shall write more fully as to my inquiries among half-breed nanos; but they are of very secondary interest now that we can find pure racial nanos within easy reach.

It is most fortunate that they live in the Valley of Ribas and the Col de Tosas, within a little more than a half-day's journey from Toulouse. Some health-seekers or tourists in the South of France may perhaps feel inclined to pay a visit to these little people.

Should the suggestion be acted on, and prove satisfactory, a line to myself on the subject, addressed to 28, Pall Mall, would be highly valued. R. G. HALIBURTON.

Tangier, January 9.

[COPY.]

"*British Consulate, Barcelona, December* 10, 1892.

"DEAR SIR,—Since I received your letter of November 18 and its enclosures I have endeavoured to ascertain what truth there is in the statement that pigmies, or 'enanos' (not 'nanos') exist in the Valley of Ribas. From conversations I had with various individuals who have visited that district it appears certain that a race of men, of about from one metre to one metre and twenty centimetres high, of a darkish complexion (copper-coloured), dark hair and woolly, and flat, broad nose, live in that district, particularly in the 'Collado de Tosas.' They are active, and are generally employed as shepherds. It is also asserted that they are not very intelligent, and that they appear to understand and to make themselves understood with difficulty. It would be an easy journey to go to that place from this town. I had no little difficulty in finding out that such a race lived in that place, for many of the persons with whom I have spoken on the subject were evidently confused and confused me, as besides these, evidently racial pigmies, there are in that neighbourhood many 'cretins,' which were at times described to me as if these were the 'enanos' I spoke about. I am now certain that there are cretins and pigmies in the Valley of Ribas. It is stated that the 'enanos' are rapidly disappearing, and that latterly many have died of smallpox. The men you speak of, who were seen at Salamanca, are, I should say, natives of the Batuecas, or rather of Los Hurdes. These men were discovered in the sixteenth century, and they were then and are even now, in an almost absolute state of savagery." [The remainder as to this race is omitted, as it does not appear that they are nanos.—R. G. H.]

"Yours very truly,
(Signed) "WM. MCPHERSON.

"R. G. Haliburton, Esq."

British Earthworms.

I WRITE to suggest—in connection with the recent letters in NATURE upon this subject—that some one give a thoroughly trustworthy list of British earthworms, with the memoirs in which the species were originally described, and the chief characteristics of each. Dr. Benham would be doing very useful and acceptable work if he were to accomplish this. From what I understand everybody has been making mistakes, and the whole matter is in the utmost confusion. It is very necessary that such a classification should exist, if only for the benefit of those who are working on the earthworm more from a comparative anatomist's than from a specialist's point of view.

FRANK J. COLE.

Zoological Department, Edinburgh, January 12.

DANTE'S "QUÆSTIO DE AQUA ET TERRA."

"Quæstio Aurea ac perutilis edita per *Dantem Alagherium*, poetam florentinum clarissimum, de natura duorum elementorum Aquæ et Terræ disserentem."

"Lo, the past is hurled
In twain: up thrust, out staggering on the world,
Subsiding into shape, a darkness rears
Its outline, kindles at the core, appears
Verona."—R. BROWNING, "Sordello," Book i.

"TO all and each who shall see this document, *Dante Alighieri* of Florence, the least amongst true philosophers, wishes health in Him who is the Beginning of truth and the Light.

"Be it known unto ye all that whilst I was at Mantua there arose a certain question, the which after having been many times dilated upon rather for vain show than for Truth's sake, still remained undecided. Wherefore I, since from boyhood I have been nurtured continually in love of Truth, could not bear to leave the question undiscussed; but I thought fit to show the truth concerning it and to dissolve the arguments adduced to the contrary, both for love of Truth and hatred of Falsehood. And lest the malice of many who are wont to fabricate envious lies against the absent should behind my back alter what was well said, I have moreover thought fit to leave written down on paper what I proved, and to set forth the form of the whole disputation."

These are the words with which Dante commences this "golden and most useful" inquiry concerning the nature of the two elements, earth and water. The treatise is little known in comparison with the other writings of the poet;[1] but although rejected by Ugo Foscolo and others as "impostura indegna d'esame," its genuineness and importance are now almost universally admitted; and without yielding unreservedly to the enthusiastic opinion of an Italian geologist (Stoppani) that there are more truths relating to cosmology to be found prognosticated, affirmed, and even demonstrated in these few pages of the supreme poet than in all the writings of the middle ages taken together, we may nevertheless acknowledge it to be a work of the greatest interest and importance, and by no means unworthy of the singer of the "Divina Commedia."

It seems to be the last work of the poet's life, written at that period which he himself describes in his sonnet to Giovanni Quirino:—

"Lo Re, che merta i suoi servi a ristoro
Con abbondanza, e vince ogni misura,
Mi fa lasciare la fiera rancura,
E drizzar gli occhi al sommo consistoro
E qui pensando al glorïoso coro
De' cittadin della cittade pura,
Laudando il Creatore, io creatura
Di più laudarlo sempre m'innamoro."
—Sonetto xliv. ed. Fraticelli.[2]

Dante was at this time the guest of Guido Novello di Polenta at Ravenna. About the commencement of the

[1] It is, I believe, the only one of Dante's writings that has not yet been translated into English.

[2] "The King by whose rich grace His servants be
With plenty beyond measure set to dwell,
Ordains that I my bitter wrath dispel
And lift mine eyes to the great consistory;
Till, noting how in glorious quires agree
The citizens of that fair citadel,
To the Creator I, His creature, swell
Their song, and all their love possesses me."
—Rossetti's translation in "Dante and his Circle."

year 1320, he seems to have gone for some unknown reason to Mantua, and there to have entered upon this discussion, which he then completed at Verona. The disputation took place at this latter city on January 20, 1320, as Dante himself tells us, in the church of St. Helena (where in recent years the metropolitan chapter have put up a monument in commemoration of the event). All the clergy of Verona were present, except some few who, in the words of Rossetti—

> "Grudged ghostly greeting to the man
> By whom, though not of ghostly guild,
> With Heaven and Hell men's hearts were fill'd."
> —"Dante at Verona."

From a passage which occurs in the course of the treatise, one might almost think that ladies also were present, but let not the reader therefore conclude that the assemblage which listened to Dante's eloquence in that little Veronese temple resembled so many modern philanthropical and other associations in being chiefly composed of ladies and clergymen, for doubtless Can Grande della Scala himself was present to do honour to his former guest, and his poetic fame, which we know to have already spread far and wide, would certainly have brought together as many as the church could hold.

The question to be solved is whether, on any place on the earth's surface, *water* is higher than the *earth*. This question, Dante tells us, was generally answered in the affirmative, and he gives us the five chief reasonings adduced in support of it, of which perhaps the most striking is this one:—

"If the earth were not lower than the water, the earth would be entirely without waters, at least in the uncovered part, and so there would be no fountains, nor rivers, nor lakes. So water must be higher than the earth. For water naturally flows downwards, and the sea is the source of all waters, and if the sea were not higher than the earth, the water would not flow to the earth, since in every natural motion the source of the water must be higher."

Another is this:—"Water seems chiefly to follow the motion of the moon, as is evident in the flow and ebb of the sea, and therefore since the moon's orbit is eccentric, it seems reasonable that water in its sphere should be eccentric too; and another argument shows that this cannot be unless it be also higher than the earth."

Such be their arguments, but sense and reason alike are against them, and Dante proceeds to explain how he will treat the question. First, he will prove that it is impossible that water in any part of its circumference be higher than this emergent or uncovered earth on which we dwell. Secondly, he will prove that this emergent earth is everywhere higher than the surface of the sea. Thirdly, he will urge arguments against his own demonstrations, and then demolish these objections. Fourthly, the final and efficient cause of the elevation and emergence of the earth will be shown. Fifthly, he will demolish the five chief arguments of the other side which he has already stated.

I. It is impossible that water in any part of its circumference be higher than the earth.

There are only two ways whereby water can thus be higher than the earth: either the water must be *eccentric*, or, if it be *concentric* with the earth, it must be *gibbous* in some part. By water being *eccentric*, Dante means the centre of its natural sphere to be out of and different from the centre of the earth; by being *gibbous*, Dante means some part of its sphere to be raised up so as to form a protuberance or hump, just as he considers the earth on which we live to be a protuberance or gibbosity of the spherical surface of the earth.

He now shows by means of diagrams that neither of these things are possible, but first makes these two statements—(1) Water naturally flows downwards; (2) Water is by nature a labile body and has not a boundary of its own, but takes the boundary of the thing in which it is contained.[1]

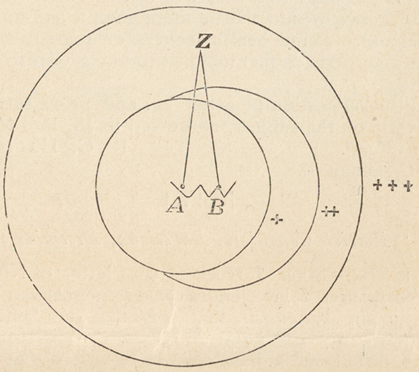
We may compare with this a modern definition of a fluid:—

"A perfect fluid is a body whose form can be changed to any extent, provided its volume remain constant, by the application of a stress, however small, if we allow it sufficient time."—Garnett, "Treatise on Heat."

In the first place, *water cannot be eccentric.*

For if it were so, then three impossibilities would follow—(1) Water would naturally flow both upwards and downwards; (2) water would not be moved downwards by the same line as the earth; (3) an equivocation would arise in speaking of the *gravity* of water and of earth; all which things are seen to be not only false but impossible.

The demonstration *ab absurdo* follows thus:—Let the heavens be the circumference on which are placed three crosses; water the circumference on which are two; earth the circumference on which is one cross.



Let the centre of heaven and earth be at point A, the centre of water at point B. Thus A, being the centre of the universe, is the lowest spot of all, and everything which has in the world a position alien from A must be higher. Now if there be any water at A and the way be open to it, it will naturally flow to its own centre, B, since it is the property of every heavy body to move to the centre of its own sphere. But the motion from A to B is a motion upwards; therefore water will flow *upwards*, which is impossible.

Again, let there be at Z a lump of earth and some water, and let there be nothing to hinder. Then, since it is the property of every heavy body to move to the centre of its own sphere or circumference, the *earth* will move in a straight line to A, and the *water* in a straight line to B, and this, from the figure, must needs be along different lines. This, says Dante, is not only impossible, but would make Aristotle laugh if he were to hear it.

The third impossibility follows thus:—*Gravity* and *levity* are "passions" of simple bodies which are moved with linear motion, and *light* bodies tend upwards and *heavy* tend downwards, by "heavy" and "light" being meant that which has the power of being moved. If now water moved to B and earth to A, since these are simple bodies and heavy, they will be moved down to different centres. If this were so, the word *gravity* would have an *absolute* signification with respect to earth and *relative* with respect to water. This is what the argument amounts to, and so there would be an equivocation of meaning in the word "gravity."

Therefore, *ab absurdo*, water in its natural circumference is not eccentric or out of the centre common to the circumference of the earth.

In the second place, *water cannot be gibbous.*

[1] "Aqua est labile corpus naturaliter, et non terminabile termino proprio."—§ xi.