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WORLD-LIFE

World-Life; or, Comparative Geology. By Alex. Winchell, Professor of Geology in the University of Michigan. Pp. 642. (Chicago: Griggs, 1883.)

AT the present day cyclopædic knowledge has become very rare, and a scientific man is generally like a miner intent on his own special shaft, and too often careless or ignorant of the general plan of the whole mine of science. The work of the collator and summariser is thus continually rising in importance, and care, patience, and judgment are now more requisite than ever before. Although these scientific "consolidation acts" can hardly fail to be open to criticism, yet every man of science must receive them with gratitude, for they afford him a general view of his science, and furnish him with a useful repertory of reference.

In this work Prof. Winchell's field is very wide, when he undertakes to collate astronomy, cosmogony, and geology, in the widest acceptation of these terms. So many subjects does this book touch on that it will only be possible within the limits of an article to give a general view of its scope. The author's reading has been extensive, and we are glad to observe that copious references are provided. He expounds with care, although perhaps sometimes too diffusely, the views of many writers, and thus brings to a focus a great mass of literature, and his own speculations are generally interesting, although not always above criticism.

As already indicated, this work is intended to give a general survey of stellar and planetary systems, to note the marks of evolutionary processes revealed by the telescope, to discuss various cosmogonic theories, to examine the probable life-histories of nebulae, suns, planets, and satellites, and to consider the influences under which the surfaces of planets are modelled and transformed.

Modern cosmogony is properly a department of physics and dynamics; but when states of matter irreproducible in the laboratory, and the mechanics of systems too complex for rigorous mathematical treatment, are dealt with, moderation in the general reasoning employed has not always been duly observed. No one can doubt that speculation is of the highest scientific importance, but it is also equally certain that in work of this kind a descending scale may be formed, beginning with speculations founded on rigorous mechanical principles and ending with wild and lunatic fancies. Every writer on such topics must, I suppose, sometimes question himself with misgiving as to where in such a list his name would stand. Mr. Winchell appears to treat all speculations with judgment, although one is sometimes tempted to think the exposition over-elaborate and the consideration too patient.

The first part or book is entitled "World-Stuff," and begins with a good account of meteors and meteoric dust. The author thinks that, according to Mr. Aitken's theory of the formation of fog, the highest clouds in our atmosphere reveal the presence there of a very fine dust, probably of cosmic origin. The sunset-glows of last winter appeared

to illuminate clouds at an unusual altitude: may not these clouds have owed their existence to the very dust which caused the glow?

The zodiacal light is then described, and is attributed to swarms of meteorites circulating round the sun, and the visibility of the light on both horizons simultaneously is taken as showing that the orbits of some of them are greater than that of the earth. The author also suggests the probability that swarms of meteorites circulate about the planets as satellites.

Comets, whose association with meteorites is now generally accepted, are described. Later (p. 77) the author writes:—

"The phenomena of the tail, especially in the vicinity of aphelion, are such as would result if we could conceive the nucleus of the comet surrounded by an aura extending on all sides as far as the remotest limits of the tail, and could recognise the tail as merely a *luminous shadow* cast by the nucleus in intercepting certain radiant energy proceeding from the sun. . . . The tail would be, therefore, not a material form moving with the comet, but something perpetually renewed, while the older and more distant emanations disappear from visibility."

That theory which divides the tails of comets into three classes, according to the gas of which they are formed, is not given.¹

The nebulae are then passed in review, and are well illustrated by drawings. They are classified as amorphous, spiral, spiro-annular, annular, and planetary, and the class is taken as giving an indication of the stage of evolution.

In the case of a spiral nebula, such as that in Canes Venatici (Fig. 8, *op. cit.*), it seems difficult to believe that we view the whole. And we suggest that the great mass of the gas is non-luminous, the luminosity being an evidence of condensation along lines of low velocity, according to a well-known hydrodynamical law. From this point of view the visible nebula may be regarded as a luminous diagram of its own stream-lines.

In the second chapter the author enters on the generation of heat in nebular masses. The discussion appears unsatisfactory, and as it is a matter of primary importance, I propose to make some criticisms thereon. The usage of mechanical and thermic terms is loose, so that it is somewhat difficult to determine the author's meaning.

The question is concerning the generation of heat in a contracting nebular mass, and on p. 86, § 9, he concludes:—

"It is true, then, that contraction develops heat, and that its development delays final refrigeration; that is, the progress toward final refrigeration is not as rapid as the amount of radiated heat implies. But it is not true that contraction (from cooling) can have developed the whole amount of heat at any time existing in the mass, or can even maintain a body at a constant temperature."

From this conclusion I venture to dissent, and in order to show my grounds I will give a paraphrase of the author's argument, as far as I am able to grasp it.

Let there be two similar planetary spheres with layers of equal density similarly arranged, and let the linear dimensions of the smaller (or say configuration β) be $1/n$ th of those of the larger (or say configuration α); or,

¹ This was sketched by Prof. Hall in his late lecture at Montreal, but I have unfortunately forgotten the originator's name.

in other words, let a and a/n be any corresponding radii of α and β .

Let the mass, however, contained within radius a of α be equal to that within radius a/n of β ; so that β might be formed from α by simple contraction; and suppose both systems to be in hydrostatic equilibrium. Then it is easy to show that if ρ be the density at any point of α , the corresponding density of β is $n^3\rho$; and if p be the pressure at the same point of α , the corresponding pressure of β is n^4p ; and lastly, the modulus of elasticity being $\rho dp/d\rho$ at any point of α , the corresponding elasticity of β is $n^4\rho dp/d\rho$.¹

Now if we suppose the mass to have contracted from a state of infinite dispersion to the configurations α or β , there must in each case be a certain exhaustion of potential energy of mutual attraction of matter, developing heat in the mass. Then it may be shown that if h is the exhaustion of energy of the matter within a radius a in passing from infinite dispersion to configuration α , the exhaustion of energy of the matter within a radius a/n in passing from infinite dispersion to configuration β is nh .² The same is also true of any stratum in course of its contraction. If we take a succession of configurations with radii infinity, 1 , $\frac{1}{2}$, $\frac{1}{3}$, &c., in harmonic progression, a constant amount of heat will be generated in passing from any one configuration to the next.

Now let us suppose that in course of contraction neither convection, conduction, nor radiation takes place; then if the temperature in the condition of infinite dispersion be zero, and if the specific heat be constant, the temperature of any stratum a of α being θ , that of stratum a/n of β will be $n\theta$. In this case $\rho\theta$, being density multiplied by absolute temperature, becomes, in passing from α to β , $n^4\rho\theta$. If, therefore, the modulus of elasticity varies as density multiplied by temperature, we have the elasticity in β n^4 times that of α . But we have already seen that $\rho dp/d\rho$ is augmented in passage from α to β by the factor n^4 . Hence the hypotheses as to arrangement of strata, specific heat, and law of elasticity are such as to insure equilibrium in every configuration, if it holds in any. This law of elasticity is that of the *isothermal* contraction of a so-called perfect gas.

Now Mr. Winchell's argument appears to me to be that, when there is loss of heat by radiation, there is necessarily deficiency of temperature to make up the elasticity, and thus equilibrium is impossible unless we look for heat from other causes. He does not seem to notice, however, that it will be far nearer the truth (if any such physical hypotheses can be said to be near thereto) to take the elasticity from the adiabatic contraction of the perfect gas, which we know to vary as $\rho^\gamma\theta$, where $\gamma = 1.408$. With this law the argument breaks down. In any case the constancy of specific heat, the similarity of successive configurations, and the law of elasticity of "perfect" gases are untenable. In order, however, to do justice to the author I must point out that he attributes later the supply of heat to "conglomeration," which differs I presume from

"contraction" in the supposed absence of hydrostatic equilibrium in successive stages, and in the irregularity of the masses concerned.

The paragraph in this chapter on nebular rotation appears to clothe the matter in an unnecessary mystery. Surely we may admit that the existence of a nebular mass with an absolute zero of resultant moment of momentum is highly improbable; and if the expanded nebula has finite resultant moment of momentum, then *must* the agglomerated nebula rotate. Even with zero momentum the nebula might perhaps divide into two portions with equal and opposite momenta.

We next come to paragraphs on nebular annulation and the "spheration" of rings. The intractability of these problems to mathematical treatment renders the discussion highly speculative, but the author seems to treat his subject with discretion.

The second main division of the work bears the title of "Planetology." An elaborate survey of the solar system is given, with a consideration of the arguments for and against the nebular hypothesis. The fact that the inner satellite of Mars revolves in a period shorter than that of the rotation of its planet is regarded as a great difficulty in the acceptance of Laplace's theory. Our author, whilst suggesting as an explanation a diminution of the primitive period through the influence of a resisting medium, refers favourably to the theory that solar tidal friction has retarded the planet's rotation whilst leaving the period of the satellite unaltered. I have myself regarded the fact of which we speak as a very striking confirmation of the importance of tidal friction in planetary evolution.

Faye's modification of the nebular hypothesis, in which the planetary annuli are supposed to arise in the interior of the nebula, is criticised by Mr. Winchell with some success. An account is also given of Spiller's theory. That author rejects the annuli entirely, and supposes the planets to arise by a combination of tidal action with centrifugal force. The formation of the planet is supposed to take place after the central mass has reached the condition of igneous fluidity.

"It is manifest that a separated planetary mass must produce a tidal swell of some magnitude upon the fluid central mass. . . . At some perihelion of the planet therefore—concurring perhaps with a conjunction of planets—the centrifugal tendency of the equatorial portion of the central fluid mass would exceed gravitation, and the tidal swell would be lifted bodily from connection with the central mass. . . ."¹

Neptune generated Uranus, Uranus Saturn, and so on.

Now I venture to say that Spiller could not have made any numerical estimate of the efficiency of a planet's tidal action on the sun, or he could not have proposed this fantastic theory.² It would therefore hardly have seemed to me worth while to have referred to this passage had not Mr. Winchell stated that this theory might be regarded as a prototype of one of my own.

I had suggested that when the earth, then without a satellite, was rotating in four or five hours, the free period of oscillation of the fluid planet would be almost the same

¹ The reader acquainted with Laplace's theory of the earth's figure will have no difficulty in proving this, or even a simple acquaintance with hydrostatic principles will suffice.

² The exhaustion of a homogeneous sphere of mass M and radius a is $\frac{3}{8}\pi M^2/a$, where μ is the attractive constant. Hence for a heterogeneous sphere we have $\frac{1}{2}\pi^2\mu \int_0^a \rho^2 a^2 da$. If ρ becomes $n^3\rho$ and a becomes a/n , obviously the exhaustion becomes n times as great as before.

¹ P. 213, *op. cit.*

² For such an estimate see a paper "On the Tidal Friction of a Planet attended by several Satellites, &c." (*Phil. Trans.* Part 2, 1881). On p. 515 it is shown that, supposing the coefficient of viscosity in the sun to be the same as that in the earth, then the increase of earth's orbital moment of momentum due to earth's tides in the sun is $1/113000$ th part of that due to sun's tides on the earth. See also Table III, p. 526.

as the period of the solar semi-diurnal tide, and that the solar tide might undergo such kinetic augmentation as to rupture the planet. A piece torn off might form the moon. The suggestion was only thrown out tentatively, and it might perhaps have been better had it been suppressed. The whole essence of the suggestion lies, however, in the approximate identity of the free and forced periods of oscillation, and this reasoning has no place in Spiller's theory.

In considering the history of a cooling planet, the author is opposed to Sir William Thomson, and concludes that the surface would harden into a crust. It seems to me that the time is hardly ripe for a very confident opinion on the point.

A large place is given in this book to the influence of tides in the evolution of a planet. A description is given of the tidal retardation of planetary rotation and the recession of the satellite; and the chapter is in fact principally a *résumé* of my own papers. The author is at one with me in rejecting Prof. Ball's view, that an enormous exaggeration of marine tides can have taken place within geological history. He is inclined to adopt the view that the trends have been imparted to our great continents by means of the wrinkling consequent on tidal friction in a primitively viscous mass; but he hardly notes, as I pointed out, that if this be so we have to accept a continuous adjustment of the general ellipticity of the earth to a figure of equilibrium, without obliteration of the wrinkles. The suggestion is thus perhaps placed in almost too favourable a light.

On p. 282 Mr. Winchell speaks as though solar tidal friction is adequate to cause a sensible lengthening of the year, so that in earlier ages it was sensibly shorter. It is impossible to admit the correctness of this view, as I have elsewhere shown.¹

In a section on orogenic forces we have, amongst much other interesting matter, an account of M. Favre's experiment, in which a layer of clay is placed on a tense elastic membrane, which is then allowed to contract: an illustration of many of the facts of mountain geology is thus furnished.

In the following chapter the author follows the various lines of argument by which limits are placed on the age of a planet, and by a subsequent geological discussion endeavours to derive a time scale; but I feel incompetent to judge of the worth of the conclusion. We may regret to find the revival in this place of Prof. Haughton's argument, viz. that the absence of a measurable nutation of 306 days proves the enormous antiquity of the elevation of Europe and Asia. The argument is, I think, worthless, as I believe that Prof. Haughton now admits.²

The principal topics dealt with in the rest of the book are the geology of the moon, the physical condition and habitability of other planets, and the final effects of tidal friction.

The fourth main division of the book is historical, and contains a review of the evolution of cosmogonic theories, with an exposition of the speculations of Kepler, Descartes,

Leibnitz, Swedenborg, Kant, Lambert, William Herschel, and Laplace.

From the account which has now been given of this work it must be evident that Mr. Winchell set before himself a task of portentous magnitude, and that he has performed it conscientiously. The criticisms which have been made should not impair the conviction that the student of this group of subjects will find his work of great value.

G. H. DARWIN

LETTERS TO THE EDITOR

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[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

The Pentacrinoid Stage of *Antedon rosaceus*

IN compliance with Prof. Herdman's request, I have to state that my experience—acquired during seven years of consecutive dredging in Lamlash Bay (1855-61)—is in entire accordance with his own. Although the most active period of reproduction in *Antedon rosaceus* is undoubtedly (as stated by Sir Wyville Thomson) the early part of the summer, so that the Pentacrinoids which spring from the ova then matured and fertilised are ready to drop off their stems in the succeeding autumn, yet I never failed to obtain *Pentacrinoids* in all stages, as well as *Antedons* still "in fruit," throughout the months of August and September. In fact, the whole of my study of this type—which, as regards the skeleton, is fully recorded in my memoir in the *Philosophical Transactions* for 1865, and of which, as regards the soft parts, a general account is given in the *Proceedings* of the Royal Society for 1876, was carried out during those months; my official duties keeping me in London until after the first week in August.

I may take this opportunity of directing the attention of those interested in Crinoidal structure (1) to a communication I have recently made to the Royal Society (*Proceedings*, May 29) on the Nervous System of the Crinoids; (2) to a paper by Prof. A. Milnes Marshall in the *Quarterly Journal of Microscopical Science* for July last; and (3) to a paper by Dr. Carl Jickeli of Jena, in the *Zool. Anzeiger*, 7 Jahrgang, No. 170.—The doctrine I propounded on this subject nearly twenty years ago (that the quinquelocular organ contained in the centro-dorsal basin of *Antedon* is a nerve-centre, and that the radial cords issuing from it, which traverse the calcareous segments of the arms and pinules, and give off branches to the successive pairs of muscles, are nerve-trunks), though supported by the experimental evidence which I published in 1876, and by the careful microscopic investigations of my son, Dr. P. Herbert Carpenter, has not been accepted by Zoologists generally; being for the most part either ignored altogether, or pooh-poohed as "evidently" fallacious, because inconsistent with homological theory. When I made my recent communication (1) to the Royal Society, summing up the very remarkable confirmatory evidence afforded by my son's inquiries, and referring (as Prof. Marshall had kindly enabled me to do) to the then unpublished results of his experiments (2), which entirely tallied with my own, Prof. Huxley, while admitting the strength of my case, remarked that the position I assign to the nervous system of the *Crinoidea* is as anomalous (in relation to that of Echinoderms generally) as it would be for a Vertebrate animal to have its spinal cord lying along its ventral surface. In reply, I asked, "What more proof can you ask for, of the nervous function of the quinquelocular organ and radial cords?" The only additional evidence that Prof. Huxley could suggest, was the result of electric stimulation. Before my paper was published in the *Proceedings*, I learnt (3) that this experiment had been actually tried four years ago by Dr. Jickeli, whose results entirely confirmed my doctrine.

It is to be hoped, therefore, that those who have so confidently and persistently clung to a homology, which is in direct contradiction to the most complete and conclusive proof that experiment can afford—supported as this is by the large body of

¹ *Phil. Trans.* Part 2, 1881, p. 524: "From this it follows that, if the whole of the momentum of Jupiter and his satellites were destroyed by solar tidal friction, the mean distance of Jupiter from the sun would only be increased by 1/25000th (misprinted 1/2500th) part. The effect of the destruction of the internal momentum of any other system would be very much less."

² See *Proc. R.S.* February 19, 1878, No. 186, p. 1, "On Prof. Haughton's Estimate of Geological Time."