hair, but having in other respects the characteristics of natives attacked by leprosy. Making inquiries from one of the principal native revenue officials at the place, it was ascertained that there was a family living hardly a mile away, of which more than one of the members had been born, and continued, white all their lives. That this did not result from their being lepers, and that none of their neighbours were in the least afraid of them, though opinion was not quite clear as to the whiteness not being disease.

Losing no time, it did not take long to reach the hut in which this family of albinos were to be found. They are of the Hindu blacksmith caste. The father and mother are stated to be of the ordinary blackness of natives of India, but were not seen on this occasion. A son, aged twenty-two, was there working at his trade, with the white colour, features, and light flaxen hair of a European, the only difference being a coarseness of the texture of the skin, and a slightly vacant expression. There was, beside him, an apparently elder brother, quite dark, and a native Hindu in every respect. It was said that albinos had occasionally appeared in the family, one of the uncles, for instance, having been white.

On being questioned as to whether there was any difference between the albinos and ordinary natives, it was at once said that the former could not stand being in the sun, which reddened and inflamed the skin, upon which the remark fell from the writer that it would be worth while to transport such individuals to a cold climate, where they would he exposed to no inconvenience. And so it would, because there can be no doubt that one of these white Hindus, early taken, and educated in a European climate, would from palpable observation of the specimen now described be absolutely indistinguishable as a native of India.

Evidently some cause has interfered with the production of pigment in the cells of the skin, with the effect of rendering the albinos highly sensitive, and more so than a European, to the invisible heat rays of the spectrum, which are so injurious to the constitution in India.

The contrast between the faces of the brothers was peculiarly striking, for there was sufficient resemblance, in the lower part of the face especially, to show there was a distinct relationship —that of the one who was dark wore the ordinary mild composure; but the other, by the mere change of colour, had completely and inadvertently thrown off the Oriental mask; and it would be almost impossible to convey to any one, not seeing it exemplified, how vast a change could be made by so simple an alteration, displaying the way the real individuality of race is lurking in an extraordinary manner beneath a tropical blackness.

India, February 24 A. T. FRASER

Far-sightedness

THOUGH I have already published a note on the subject in a Dutch paper (*Tijdschrift van het Aardrijkskundig Genootschap*, February, 1885), perhaps you will kindly allow the following lines to have a place in NATURE, because those who are occupied in the trigonometrical survey of British India may take an interest in the matter, and be able to give more particulars about it.

In a paper on Mr. Whymper's travels in Greenland, which appeared in *Ausland*, t. xii., 1884, I found in a foot-note the following remark:—"The reader might be astonished on hearing that I [Mr. Whymper] could see a mountain at such a great distance (about 100 English miles); but I may add that the day before I saw two other mountains 40 and 150 English miles distant; with one exception this was the greatest distance at which I have ever been able to make out objects."

Since I have not found any other reports in which it is expressly stated that objects were seen at a greater distance, I presume I may allege my own experience. While occupied with the trigonometrical survey of Western Java I sometimes had an opportunity of seeing objects at a very great distance, though, under the circumstances I was in, I had no time to look for them on purpose.

The greatest distance at which the angular points of triangles of the first order were from each other was about 105 kilometres; no difficulty ever arose from the distance, and no difference was made whether signals or heliostats (square mirrors of about 3 inches side) were observed.

When on Gng Karang (Bantam) I made out Keizerspick (Sumatra) at a distance of more than 110 English miles, though not quite easily, the top just peeping out from the slopes of Sebesic; if there had been a signal on Keizerspick at that time I think I could have observed it.

The greatest distance at which I remember ever to have seen an object was noted during my stay on Gng Tjikoraij (Preanger Regentsch), when I made out Gng Merapi (Java) most distinctly at a distance of about 180 English miles[1] and I suppose that Gng Lawu was also visible (225 English miles distant), but I could not quite distinguish it from the group of mountains of which it is one. It is, of course, from high summits that objects are seen at the greatest distances, and objects which are more elevated at a greater distance than such as are close to the ground.

I think it would be interesting to gather experiences referring to the subject made in different climates and under different circumstances. EMIL METZGER

Stuttgart, March 23

Krakatoa

SUPPOSING that the underground noises heard at Caïman-Brac on Sunday, August 26, 1883, were not only synchronous with, but actually the same as, those caused by the great eruption in the Straits of Sunda, it does not seem to follow that the sound-waves were propagated through the whole diameter of the earth. On the contrary, the question is at once raised, at what depth below the surface did the disturbances occur which found such destructive vent at Krakatoa? And if only the time-record east and west were accurate and satisfactory, there would seem to be some datum supplied for approximately estimating this depth. The centre of disturbance may have receded from and become inaudible at the Caïmans in proportion as, on the 27th, it found final vent at Krakatoa. HENRY CECIL

Bregner, Bournemouth, March 30

The Recent Aurora

THE "Sunk" lightship is in electrical communication with the Essex coast, being connected thereto by a telegraphic cable 8·984 nautical miles in length, laid from Walton-on-the-Naze in an easterly direction. The electrical condition of this cable is ascertained daily at 10 a.m., by means of tests applied at the shore ends. Until the 15th inst. these tests were very regular and satisfactory, but on the morning of that day it was found to be impossible to obtain any satisfactory results, owing to electrical disturbances produced in the cable by some external influence. The electrician on board reported that the weather was very fine and summer-like, sea perfectly smooth, with variable light airs, and he could in no away account for the effects the electrician was observing on shore. Between 9 and 10 p.m. those on board the lightship observed in the northern sky a very brilliant aurora, from which at intervals two very bright columns extended upwards to the zenith, and there apparently joined.

I send you these particulars as they may be worth recording in connection with the aurora seen at Christiania on the same evening, and described by Mr. Sophus Tromholt in his letter to NATURE, published on the 26th inst. (p. 479). There can be no doubt but that the aurora seen at Christiania was identical with that noticed by the men on the lightship off Walton-on-the-Naze, and, although it was not visible until the evening, it was evidently affecting the electrical condition of the earth on the morning of that day, and was the direct cause of the electrical disturbance in the cable. Since that date the tests have been as satisfactory and regular as before. WILLOUGHBY SMITH

March 30

THE COSMOGONIC THEORY OF M. FAYE[2]

M. FAYE has expounded his theoretical views on cosmogony in the several publications named above, and in his book he has also treated of the historical development of cosmogonic theories. We shall in the present article confine our attention to that which is original in his speculation; and we recommend the

[1] In the junction of triangulations of Spain and Algiers the greatest side is about 270 kilometres.

[2] "Comptes Rendus," 1880, vol. xc. pp. 637 and 1246.
"Sur l'Origine du Monde." Pp. 257. (Paris: Gauthier-Villars, 1880.)
"Annuaire pour l'an 1885, Bureau des Longitudes." Pp. 757-804. (Gauthier-Villars)

reader to refer to the essay in the "Annuaire" of the Bureau des Longitudes, 1885, for this portion of his work. M. Faye's writing is always easy and finished, and this essay has been intended for the general scientific reader. Had the original speculation been condensed for insertion in a purely technical journal it would have occupied but a few pages.

The earlier portion of the essay we may dismiss by saying that it gives a lucid exposition of the state of our knowledge of stellar systems, as derived from the spectroscope and the telescope, interpreted by aid of the principle of conservation of energy. In the following description of M. Faye's theory, we do not follow his words, but we believe that we give a fair interpretation of his meaning.

The best general idea of the line of speculation adopted may be given by saying that it is a theory of evolution from meteorites, instead of from the nebulous matter which gives its name to Laplace's theory.

In its primitive condition the Universe consisted of matter widely scattered in chaotic disorder. Currents were then generated in the midst of this chaos under the influence of mutual gravitation; and in consequence of these intestinal movements rags or shreds of matter became detached, and moved with rapid linear and slow gyratory motion.

It is not claimed that the existence of these currents can be explained, but the spectroscope affords evidence of a sorting process, for some nebulæ consist of a single gaseous element, whilst the stars with continuous spectra consist of a great diversity of elements.

The various modes are sketched in which one of these shreds may proceed to agglomerate and evolve itself, but we shall not follow M. Faye in the application of his theory to the formation of nebulæ, double-stars, and star-clusters.

The solar system is taken to have originated from a shred which aggregated into a spheroidal shape, and consisted, at the epoch when we begin to watch it, largely or principally of separate meteorites. The spheroidal aggregate possessed a considerable amount of rotation (moment of momentum), about an axis approximately identical with the axis of the sun's rotation.

It is at first supposed that the spheroidal aggregate consists of matter pretty nearly equally distributed throughout its volume, and later a nucleus is formed. If r be the distance of any point from the centre, the force is central, and follows the law $ar + \frac{b}{r^2}$, where, in the beginning of the evolutionary process, b is very small, and later a becomes small.

Initially, then, when the force is simply as the distance from the centre, each meteorite moves in an ellipse about the centre, and the periodic time of all of them is the same, whatever their eccentricity of orbit. Those meteorites whose orbits are decidedly eccentric, cross the orbits of many others, and have much less chance of escaping collision than those whose orbits are nearly circular. In consequence of collisions, a central nucleus is soon formed, and then many meteorites with very eccentric orbits begin to strike against it, and to be absorbed into it. As the nucleus increases the a in our formula for the force diminishes, and the b increases; but orbits which are circular still retain that form, notwithstanding the progressive change in the law of force.

At the same time that the nucleus is being formed, a series of flat and nearly circular rings arise around it, those near to the nucleus attaining a definite shape sooner than the remote ones. It is not adequately explained why the matter should be sifted, and should arrange itself in rings at definite intervals around the nucleus; still less is any light thrown on the law of Titius concerning the distances of the planets from the sun. Nor do we see why the rings should first be formed nearest to the nucleus. We must, however, here follow M. Faye and accept these conclusions.

If there be only a small nucleus (b small), each ring revolves with very small relative motion of its parts; whilst if the nucleus be large (a small), each meteorite in a ring revolves after Kepler's laws, and the bodies in the external margin have a slower angular velocity than those in the internal margin. As the nucleus gradually increases there will be a transition from one mode of motion to the other.

Now let us follow the first ring:—Slight differences of angular velocity, mutual attractions between the parts of the ring and collisions gradually cause the aggregation of all the matter in the ring around some centre in its line. When the nucleus is small the ring moved as a rigid whole, and the linear velocity of the outer meteorites was greater than that of the inner ones, therefore when the planetary aggregate is formed it will be found rotating with direct motion about an axis nearly perpendicular to the plane of its orbit.

Whilst the first ring is agglomerating into a planet, a second ring is being formed outside of it, and this in its turn agglomerates; but the tendency to direct rotation is weaker than in the first planet, because the increase of the solar nucleus by absorption of meteorites has prevented so large an excess of linear velocity of the outer meteorites over that of the inner ones as in the first case.

The process continues and the planets are successively formed, until we come to an epoch when the nucleus has increased so far that on agglomeration the tendency to direct rotation vanishes—the constituent ring, in fact, revolved irrotationally.

Still further we come to planets in which the meteorites move nearly according to Kepler's laws, and here the resulting planet has a markedly retrograde rotation. Each planetary agglomeration in its turn forms a miniature solar system, and generates satellites by the same process as that in which the planets were formed.

We have now sketched this theory in its main outlines, and must refer the reader to the original sources for further details.

Neither in the historic part nor in his cosmogonic speculations does M. Faye make reference to the possible effects of tides in the evolution of the solar system, perhaps thinking that a theory founded on that influence is not even worthy of mention. It is, however, a factor which cannot be left out of account. Tidal friction is a *vera causa*, and the possible effects on our evolution have been submitted to a rigorous quantitative examination.[1] As it is the only cosmogonic influence which has hitherto been so treated, the results to which it points are at least as worthy of attention as those of other vaguer influences.

The hypotheses that tidal friction has had free play in the past leads to a remarkable quantitative coordination of the several elements of the earth's rotation, and of the moon's orbital motion, and points to the genesis of the moon close to the present surface of the earth. No phenomenon in the heavens could have been devised more perfectly apt to confirm the truth of the hypothesis than the rapid orbital motion of the inner satellite of Mars. Near to the sun solar tidal friction would be much more powerful than at a distance, and thus the rotation necessary for the manufacture of satellites would be destroyed in the vicinity of the sun; a light is thus thrown on the cause of the observed distribution of satellites in the system.[2]

It has, however, been decisively shown that tidal friction cannot have played the leading part either in the evolution of the whole solar system or of the remoter

[1] We refer to a series of papers by the present writer on this subject in the *Phil. Trans. Roy. Soc.* from 1878 to 1882.

[2] This theoretical effect of tidal friction has not been commented on by any writer. Further numerical details and discussion will be found in *Phil. Trans.*, Part ii., 1881, p. 531.

planetary systems, and whilst the field is thus left open to the nebular hypothesis or other rival theories, it is submitted that tidal friction has a bearing on those theories which cannot be neglected.

A numerical comparison of the distribution of moment of momentum amongst the several planetary sub-systems shows that the terrestrial system differs considerably from all the others, but it would hardly be logical to postulate an absolutely independent mechanism in this case, and it is not very easy to reconcile the genesis of the moon close to the earth with the formation of a ring in the midst of a planetary agglomeration of meteorites. Let us now summarise the advantages and disadvantages of M. Faye's scheme.

The conception of the growth of planetary bodies by the aggregation of meteorites is a good one, and perhaps seems more probable than the hypothesis that the whole solar system was gaseous, and that the influence of hydrostatic pressure was felt throughout. The internal annulation of the meteorites is left unexplained, and this compares very unfavourably with Laplace's system, where the annulation is the very thing explained. The difference of orbital motion of the inner and outer meteorites of a ring, the development of that difference as time progresses, and the consequence of direct and retrograde rotation at different distances from the sun is an excellent idea. But it is necessary to this idea that the inner planets should have been formed the first, and we are met directly by the fact that the single surviving ring, that of Saturn, is nearer to the planet than are the satellites. It is, of course, possible that special causes have preserved this ring, but we should be driven to the startling conclusion that Saturn's ring is the oldest feature of his system.

The actual distribution of satellites in the solar system is at variance with M. Faye's theory, for, according to him, the internal planets were generated from rings whose motion was such as would give greater moment of momentum to the planetary agglomeration than would the external ones. The number of satellites manufactured should be greater the greater the amount of rotation in the primitive agglomeration of meteorites, and thus the nearer planets should be richer in satellites than the remote ones.

The celebrated experiment of Plateau, in which a drop of oil rotating in alcohol and water is made to parody Laplace's solar system, is worthy of attention, and it tells against Faye and in favour of Laplace. It is of course to be admitted that surface-tension does not duly represent gravity.

On the whole, then, we must hold the opinion that there are great difficulties in the acceptance of M. Faye's theory, notwithstanding its excellences. The time does not appear yet ripe for definite judgment on this very complex subject, but science is undoubtedly the gainer by such suggestive theories. Whilst a false statement of fact always proves a serious detriment, the enunciation of false or partially true theories is always the incentive to, or initiation of, the discovery of truth.

G. H. DARWIN

SIR WILLIAM THOMSON ON MOLECULAR DYNAMICS [1]

II.

IN the present article Sir William Thomson's spring and shell molecule will be described and its theory sketched, in so far as this has been investigated with the view of getting over some of the difficulties which surround the wave theory of light. In Helmholtz's memoir on anomalous dispersion, a sketch of such a theory was published. But this new molecule differs from that of Helmholtz in several points, chiefly in the fact that absorption is not accounted for by any viscous action in the molecule dissipating the energy of vibration into low grade heat. Most readers who have ever visited the natural philosophy lecture-room in Glasgow University will recognise a very old friend in this new molecule, where they have seen it vibrating, I suppose, any time since the University occupied its present site. In appearance the molecule has been changed, but its theory as taught to the students there is identical. For a description of this molecule let us refer to page 10 of the lectures :—

"Imagine for a moment that we make a rude mechanical model. Let this be an infinitely rigid spherical shell; let there be another absolutely rigid shell inside of that, and so on, as many as you please. Naturally we might think of something more continuous than that, but I only wish to call attention to a crude mechanical explanation possibly of the effects of dispersion. Suppose we had luminiferous ether outside, and that this hollow space is of very small diameter in comparison with the wave-length. Let zig-zag springs connect the outer rigid boundary with boundary number two. I use a zig-zag, not a spiral, spring which has the helical properties which we are not ready for yet, such properties as sugar and quartz have in disturbing the luminiferous vibrations. Suppose we have shells two and three also connected by a sufficient number of spiral springs, and so on; and let there be a solid inclosed in the centre with spring connections between it and the shell outside of it. If there is only one of these interior shells, you will have one definite period of vibration. Suppose you take away everything except that one interior shell; displace that shell and let it vibrate. The period of its vibration is perfectly definite. If you have an immense number of such shells with moveable molecules inside of them, distributed through some portion of the luminiferous ether, you will put it into a condition in which the velocity of propagation of the wave will be different from what it is in the homogeneous luminiferous ether. You have what is called for, viz. a definite period; and the relation between the period of vibration in the light considered and the period of the free vibration of the shell will be fundamental in respect to the attempt of a mechanism of that kind to represent the phenomena of dispersion.

"If you take away everything except the one shell, you will have almost exactly, I think, the view of Helmholtz's paper—a crude model as it were of what Helmholtz makes his paper on anomalous dispersion. Helmholtz, besides that, supposes a certain degree or coefficient of viscous resistance against the vibration of the inner shell, relatively to the outer one. Helmholtz does not reduce it to a gross mechanical form like this, but merely assumes particles connected with the luminiferous ether and assumes a viscous motion to operate against the motion of the particles."

In the lectures the action of such a molecule when subjected to forced vibrations was illustrated by a model of ingenious construction, which among the irreverent passed by the name of the "wiggler." A steel wire was hung vertically, and five or six lathes 2 feet long and 2 inches wide were attached in a horizontal position to the wire, each one having three pins fixed in it for this purpose. These lathes were loaded at their ends, the weight on each lathe being less than that on the one above it. The lowest lathe was attached to a pendulum arrangement which impressed forced vibrations upon the system, the period being adjustable. The theory of such a system is the same as that of the molecule described above.

But in working out the theory a third type of vibrator was used, the identical one which vibrates in the lecture room at Glasgow. This is a series of weights attached to each other by vertical springs which can be stretched. The highest is the heaviest, and the others are arranged in the order of weight.

[1] Continued from p. 463.