be one of degree rather than of principle. For, even if its application for the full time required for that stage of evolution, in the case of so large a nebula as the original solar one, were considered doubtful, there is no objection apparently on that head to be added in the case of smaller nebulae, or comets (the smallest class of nebulae); in which latter case there is some support to the theory afforded by experiment and observation.

In regard to the question of "elasticity," the only resource, in my view, is to abandon this idea, in the ordinary sense of that term (which conveys the idea of retention of form), and suppose that there may be complete disintegration by the collisions at times,¹ welding or fusion together at others; so that the mean degree of aggregation remains constant so long as the translatory motion remains constant. This idea of the existence of a number of possible mean states of aggregation of matter between the extremes of complete integration into one mass, and complete disintegration into molecules (the states being dependent on the rate of translatory motion), was thrown out by me in NATURE (vol. xix. p. 461);² also, further, in the *Philosophical Magazine*, August 1879, p. 153.

According to this view, it is implied that "cohesion," as a central force, can play the same part as "chemical action" under translatory motion, and produce fluctuations about a mean state of aggregation—just as in a compound gas, for example, even at normal temperature, the small lumps of matter which move as wholes in the motion of translation are disintegrated now and then, and integrated elsewhere, the mean state (only) of the gas remaining unchanged.

The above, with the exception of the last two paragraphs, was written before the appearance of Prof. G. H. Darwin's reply (March 14, p. 460), to some criticism I ventured to offer (March 7, p. 436) on certain points of his theory in my first letter. I would make a few remarks in addition here.

I am not quite able to agree with the view of M. E. Minary, expressed in a paper brought before the French Academy on February 18, of which an abstract appeared in NATURE of February 28, p. 432, and is referred to in Prof. Darwin's letter. The chief passage, as given in the abstract, is as follows:—

"The gases being perfectly elastic bodies, and in the upper atmospheric regions in an extremely rarefied state, heat cannot be produced by the shock of bodies endowed with great velocity and impinging on perfectly elastic molecules capable of receiving the motion and acquiring the velocity of these bodies: in this case the movement is communicated, not dissipated or transformed into heat."

This view is opposed, I venture to think, to the deduction we may draw from such an experiment as that with the "fire syringe," where the air in a cylinder is inflamed by suddenly compressing a piston. What is the flame here observed through the tube of glass due to? It is due, of course, to the vibrations of the molecules of air, which break the ether up into waves, and so affect the eye. From the fact that the air molecules are "perfectly elastic," it becomes impossible for a moving body to impinge against them violently without throwing the molecules into energetic vibration, which is the physical basis of "radiant heat." The same must occur on a greater scale, as it appears, when the air is compressed by a flying meteorite, although I accept M. Cornu's suggestion (quoted in the abstract, p. 432) that the luminosity observed may partly be of electric origin.

In Joule's "Scientific Papers," vol. ii., experiments in association with Sir William Thomson are described, of whirling thermometers through the air (attached to a lathe). The experiments, which were numerous, gave 163.7 as the velocity in feet per second; on the average, equivalent to a rise of temperature of 1° C. (vol. ii. p. 316). Whirling a thermo-electric junction attached to a reflecting galvanometer was tried with consistently the same result as the thermometer (p. 310). These experiments were made partly³ with the view to test the theory

¹ A special property of iron, which may have importance here.

² It would seem even curious to my mind, if there were no intermediate state between these two extremes of complete integration and complete disintegration, by a varying rate of translatory motion (or energy). It may be observed that the radiant energy set free so abundantly at a collision is not lost, but radiated in great part to another region in the nebula (and there absorbed).

³ Also see vol. i. pp. 399, 536. The temperature was found to be independent of the form and size of the thermo-electric function, and was assumed as evidently independent of the density of the air. No doubt the more air there is the more there is to heat. But it nevertheless seems plain, I think, that the temperature of a meteorite must rise higher in dense air than in excessively rarefied air. How is this to be explained? Each individual molecule of air in striking the moving meteorite, is thrown into violent vibration, and this (temperature) is independent of the number of air molecules evidently.

of the heating of meteorites. The temperature was found to be as the square of the velocity. The law of Clausius, that the translatory motion and the vibratory motion (which latter motion alone affects the eye and senses as radiant heat) of the molecules of an ordinary gas are proportional to each other, has—as Prof. Darwin allows—been experimentally verified through¹ a considerable range of temperature. To my mind it appears obvious that these two forms of motion (translatory and vibratory) must be interconvertible and mutually sustain each other. When the gas, for example, is exposed to the pulsations of ether waves (radiant heat), this vibratory motion is first taken up by the molecules, but part of it is converted into translatory motion, as proved by the rise of pressure. If, on the other hand, the translatory motion of the molecules of gas be augmented, part of this is instantly, as we know, converted into vibratory motion, the source of radiant heat. S. TOLVER PRESTON.

Paris, March.

The Molecular Formulæ of Aluminium Compounds.

In a letter to NATURE, December 27, 1888 (p. 198), I gave a tabulated statement of the numerous vapour-density determinations of halogen, and a few other compounds of aluminium and the allied metals, and pointed out what appear to me to be the legitimate conclusions to be derived from the experiments, regarding the molecular formulæ of these compounds.

Since then two interesting articles have appeared in NATURE (pp. 447 and 495), in which accounts are given of determinations of the vapour-densities of aluminium acetyl acetate, $\text{Al}(\text{C}_5\text{H}_7\text{O}_2)_3$, and aluminium methide, $[\text{Al}(\text{CH}_3)_3]_n$. The simple formula given for the first compound has been proved to be correct, at any rate for the conditions under which the experiments were made. The results given for aluminium methide are: calculated for $\text{Al}_2(\text{CH}_3)_6$, 4.98; calculated for $\text{Al}(\text{CH}_3)_3$, 2.49; observed (10° above the boiling-point under atmospheric pressure), 3.92.

What I ask permission to call attention to and to criticize is the conclusion drawn by the author of these articles from the experimental results. Speaking generally, the conclusion may be stated in this way: Molecules of the formula MR_3 do exist, therefore molecules with the double formula M_2R_6 do not. I confess that I am wholly unable to appreciate the force of the argument. Must we take the existence of the molecule NO_2 as a proof of the non-existence of N_2O_4 ?

That I have not stated the argument unfairly may be shown by quotations from the article (p. 495). Speaking of aluminium acetyl acetate the author says: "It is supremely satisfactory that in this case the density, at a temperature only 45° above the boiling-point, was found to actually correspond precisely with that required by the triad formula, precluding again the possibility of the existence of molecules of the type Al_2R_6 ." And previously, after giving the results for aluminium methide, notwithstanding the fact that the observed density 3.92, obtained by Quincke, corresponds rather more closely to the higher than the lower formula, the author remarks: "Hence it can no longer be doubted that molecules of the double formula are incapable of existence." The italics in both cases are mine. The conclusion I should draw from all the experiments with aluminium compounds is this: The experiments of Deville and Troost, Friedel and Crafts, and Louise and Roux, prove conclusively that molecules of the higher formula Al_2R_6 are capable of existence; the results obtained by Nilson and Pettersson, and by Buckton and Odling, point also to the existence of molecules of the lower formula, but further proof was certainly needed, and this has now been afforded by the valuable experiments of M. Alphonse Combes with aluminium acetyl acetate.

But in dense air, no doubt, the heat accumulates much faster than it can be radiated away, and so the temperature of the meteorite attains a final maximum, which is greater the denser the air is. The temperature may probably be appreciably constant within certain limits of variation of density; but it appears obvious that in excessively rare air the temperature developed must be less, owing to rapid dissipation in space by radiation.

¹ The ratio of Maxwell referred to in my last letter, and which Prof. Darwin remarks in a footnote (p. 460) was stated inaccurately by me—was not, I may explain, the one quoted by him from Maxwell's "Theory of Heat." It is contained in Maxwell's paper "On the Dynamical Evidence of the Molecular Constitution of Bodies" (NATURE, vol. xi. pp. 337 and 374). After alluding to the law of Clausius, Maxwell remarks:—"In 1860 I investigated the ratio of the two parts of the energy on the hypothesis that the molecules are elastic bodies of invariable form. I found, to my great surprise, that, whatever the shape of the molecules, provided they are not perfectly smooth and spherical, the ratio of the two parts of the energy must be always the same, the two parts being in fact equal" (p. 375).

In conclusion, it appears to me that the close relationship of the metals aluminium, gallium, indium, iron, and chromium, makes it probable that molecules of both formulæ M_3R_3 and MR_3 may be capable of existence in the case of all these metals, even though the conditions necessary for the stability of the higher molecules have not yet been attained for indium and chromium chlorides. That the conditions have been attained in the case of the chlorides of aluminium, gallium, and iron, appears to me to admit of no doubt.

SYDNEY YOUNG.

University College, Bristol, March 23.

Luminous Night-Clouds.

IN accordance with my anticipations (see *Astr. Nachr.*, 2885), luminous night-clouds have now been seen at the southern point of South America. Herr Stubenrauch, meteorological observer at Punta-Arenas, writes to me that he twice saw the phenomenon in December 1888. According to the same observer, it was noticed several years ago by a naval officer in the Beagle Channel, rather to the south of Punta-Arenas. The description given by Herr Stubenrauch leaves no room for doubt that the phenomenon is identical with that observed in Europe.

Sternwarte, Berlin, March 28.

O. JESSE.

Zodiacal Light Observations.

COULD any of your readers furnish me with a list of observations of zodiacal light, or refer me to any record where such a list can be found? I applied to the Meteorological Office some years ago, and found that no such list was to be obtained. All I require is the precise time of the display, the place of observation, and any other mathematical observations concerning the angle of inclination, &c., which may be relied on.

W. DONISTHORPE.

32 Pembroke Villas, W., March 30.

Vapour, or Meteoritic Particle.

ABOUT 6.30 p.m. on Friday last, while out in the country, I observed a large meteor falling slowly and almost vertically in the north-north-east. After it had disappeared, half-way between the zenith and the horizon, I noticed, in the strong twilight, a faint phosphorescent trail, which did not disappear, but changed to a straw-colour, like a streak of vapour illuminated by the setting sun. Gradually the shaft expanded in the centre and contracted at the ends, until it assumed the shape of a balloon. Continuing to change, it flattened out horizontally like a football, and apparently about half the size; and it looked a little brighter as it became more condensed. This process occupied quite half an hour, and the object remained like a nebulous patch lit up by the sun's rays, until that orb was well below the horizon, finally disappearing about 7.15 p.m. That part of the sky was perfectly clear—there were no clouds except the usual dense bank on the horizon.

F. B.

Rugby, March 27.

The Satellite of Procyon.

WITH reference to Mr. Barr's letter on p. 510, I would mention that Mr. Burnham has lately observed Procyon with the 36-inch refractor at the Lick Observatory, and finds no trace of any close companion with powers up to 3300.

H. SADLER.

March 29.

RECENT RESEARCHES ON THE RARE EARTHS AS INTERPRETED BY THE SPECTROSCOPE.¹

IF I name the spectroscope as the most important scientific invention of the latter half of this century, I shall not fear to be accused of exaggeration. Photography has rendered vast services in recording astronomical and biological phenomena, and it even supplies us with indirect means of studying ray vibrations to which the human retina does not respond. The electro-acoustic devices of

¹ Abstract of Address delivered on Thursday, March 28, at the annual general meeting of the Chemical Society, by the President, Mr. W. Crookes, F.R.S.

Edison and his co-workers permit almost magical communication between human beings. Ruhmkorff's coil and the Geissler tube have rendered notable service in physical investigations; and the electric lamp promises to aid in exploring the internal parts of living animals as well as in studying the organic forms of the deep sea. But in the spectroscope we possess a power that enables us to peer into the very heart of Nature. In the extent of its grasp and the varied character of its applicability it surpasses the telescope, and at least rivals the microscope. It enables the astronomer to defy immeasurable distance, and to study the physical condition and the chemical composition of the sun and the stars as if they were within touch, and even to ascertain the direction of their movements.

Without attempting to discuss the import of the results thus gained—which would lead us too far—I may point out that they overthrow a dogma concerning the classification of the sciences. It has been said that the simpler and more general sciences lend both doctrines and methods to the more complex and less general sciences, and that the latter give nothing in return. But we now see chemistry endowing astronomy with an original and fruitful method of research.

Turning to the very opposite extremity of the scientific hierarchy, we find that to the biologist the spectroscope is of value in studying the relations of animal and vegetable fluids, and even of certain tissues. But this wonderful instrument is clearly destined to play its chief part in what is called terrestrial chemistry—the field where it has won the most signal triumphs.

It must be remarked, despite this vast range of applicability—a range sweeping through the whole universe and embracing all the four elements of antiquity—and despite the astonishing results already achieved and the prospect of greater revelations to come, that the spectroscope is still inadequately appreciated by professed men of science, and in consequence is to a great extent ignored by the “educated and intelligent public.” In urging its more thorough recognition, I do not advocate the formation of Spectroscopic Societies for the fragmentary study of everything that can be observed with a spectroscope. But I recommend researching chemists to appeal to this instrument wherever requisite and possible.

An elaborate spectroscopic study of the basic constituents of rare minerals from different localities would be of great value, and I would suggest that on all possible occasions meteorites should be submitted to careful spectroscopic analysis.

I do not propose to discuss all the splendid achievements of the spectroscope in chemistry; nor its applications in ordinary analysis, qualitative and quantitative; nor the conduct of technical operations, such as the Bessemer process. I confine myself to the light thrown by the spectroscope upon the nature and the relations of our elements, real or supposed.

Though systematically employed by few experimentalists, the spectroscope has already led to the discovery of several hitherto unknown elements. In the early days of spectrum analysis, attention was mainly concentrated on the flame spectra: that is, the bodies in question were vaporized and rendered luminous by the action of a flame, such as that of the Bunsen burner or of the oxyhydrogen jet. This procedure in the hands of Bunsen and Kirchhoff gave us caesium and rubidium; afterwards, in my own hands, thallium; and in those of Reich and Richter, indium.

Then followed the production and examination of spark spectra. The spark produced by means of the induction coil, especially when its energy is reinforced by the intercalation of a Leyden jar, volatilizes and renders luminous minute portions of matter, solid, liquid, or gaseous, which may then be examined by the spectroscope. In this