demonstrates the truth of the suggestion, made many years ago by Prof. Tait before any spectroscopic facts were available, that the nebulae are masses of meteorites rendered hot by collisions.

Surely human knowledge is all the richer for this indication of the connection between the nebulae, hitherto the most mysterious bodies in the skies, and the "stones that fall from heaven."

Celestial evolution.

But this is, after all, only a stepping-stone, important though it be. It leads us to a vast generalisation. If the nebulae are thus composed, they are bound to condense to centres however vast their initial proportions, however irregular the first distribution of the cosmic clouds which compose them; each pair of meteorites in collision puts us in mental possession of what the final stage must be. We begin with a feeble absorption of metallic vapours round each meteorite in collision; the space between the meteorites is filled with the permanent gases driven out further afield and having no power to condense. Hence dark metallic and bright gas lines. As time goes on, the former must predominate, for the whole swarm of meteorites will then form a gaseous sphere with a strongly heated centre, the light of which will be absorbed by the exterior vapour.

The temperature-order of the group of stars with bright lines as well as dark ones in their spectra, has been traced, and typical stars indicating the chemical changes have been as carefully studied as those in which absorption phenomena are visible alone, so that now there are no breaks in the line connecting the nebulae with the stars on the verge of extinction.

Here we are brought to another tremendous outcome, that of the evolution of all cosmical bodies from meteorites, the various stages recorded by the spectra being brought about by the various conditions which follow from the conditions.

These are shortly that at first collisions produce luminosity among the colliding particles of the swarm, and the permanent gases are given off and fill the interspaces. As condensation goes on, the temperature at the centre of condensation always increasing, all the meteorites in time are driven into a state of gas. The meteoritic bombardment practically now ceases for lack of material, and the future history of the mass of gas is that of a cooling body, the violent motions in the atmosphere while condensation was going on now being replaced by a relative calm.

The absorption phenomena in stellar spectra are not identical at the same mean temperature on the ascending and descending sides of the curve, on account of the tremendous difference in the physical conditions.

In a condensing swarm, the centre of which is undergoing meteoritic bombardment from all sides, there cannot be the equivalent of the solar chromosphere; the whole mass is made up of heterogeneous vapour at different temperatures, and moving with different velocities in different regions.

In a condensed swarm, of which we can take the sun as a type, all action produced from without has practically ceased; we get relatively a quiet atmosphere and an orderly assortment of the vapours from top to bottom, disturbed only by the fall of condensed metallic vapours. But still, on the view that the differences in the spectra of the heavenly bodies chiefly represent differences in degree of condensation and temperature, there can be, *au fond*, no great chemical difference between bodies of increasing and bodies of decreasing temperature. Hence, we find at equal mean temperatures on opposite sides of the temperature curve, this chemical similarity of the absorbing vapours proved by many points of resemblance in the spectra, especially the identical behaviour of the enhanced metallic and cleveite lines.

Celestial dissociation.

The time you were good enough to put at my disposal is now exhausted, but I cannot conclude without stating that I have not yet exhausted all the conceptions of a high order to which Fraunhofer's apparently useless observation has led us.

The work which to my mind has demonstrated the evolution of the cosmos as we know it from swarms of meteorites, has also suggested a chemical evolution equally majestic in its simplicity.

A quarter of a century ago I pointed out that all the facts then available suggested the hypothesis that in the atmospheres of the sun and stars various degrees of "celestial dissociation" were at work, a "dissociation" which prevented the coming together of the finest particles of matter which at the temperature of the earth and at all artificial temperatures yet attained here compose the metals, the metalloids and compounds.

On this hypothesis the so-called atoms of the chemist represent not the origins of things, but only early stages of the evolutionary process.

At the present time we have tens of thousands of facts which were not available twenty-five years ago. All these go to the support of the hypothesis, and among them I must indicate the results obtained at the last eclipse, dealing with the atmosphere of the sun in relation to that of the various stars of higher temperature to which I called your attention. In this way we can easily explain the enhanced lines of iron existing practically alone in Alpha Cygni. I have yet to learn any other explanation.

I have nothing to take back either from what I then said or what I have said since on this subject, and although the view is not yet accepted, I am glad to know that many other lines of work which are now being prosecuted tend to favour it.

I have no hesitation in expressing my conviction that in a not distant future the inorganic evolution to which we have been finally led by following up Fraunhofer's useless experiment, will take its natural place side by side with that organic evolution the demonstration of which has been one of the glories of the nineteenth century.

And finally now comes the moral of my address. If I have helped to show that observations having no immediate practical bearing may yet help on the thought of mankind, and that this is a thing worth the doing, let me express a hope that such work shall find no small place in the future University of Birmingham.

DIFFUSION IN RELATION TO WORK.

IN this month's *Philosophical Magazine* Mr. A. Griffiths has an interesting paper on diffusion convection, in which he suggests an indirect method of measuring rates of diffusion of liquids, and concludes with the following deduction from the fact that diffusion sometimes produces convection currents and sometimes does not:—"Does not this indicate that the heat produced on mixing a solution with water depends on how the mixing takes place? Is the matter connected with a sort of surface-tension existing in the spaces between a strong and a weak solution?"

Mr. Griffiths does not seem to have observed that his investigation applies quite well to gases as to liquids, and that his indirect method of measuring rates of diffusion is applicable to gases. In the case of gases there can be sensible surface-tension, and, as the theory of diffusion in gases is quite simple, there is no serious difficulty in seeing how there is a difference between different ways of mixing them.

It is generally known that two different gases may be mixed by irreversible, or by, at least, partially reversible, processes. They may be allowed to diffuse freely into one another, or may be separated by a porous partition. In the latter case a considerable difference of pressure may be produced between different parts of the space containing them, and this difference of pressure can be used to do work. The final condition in this case is, of course, cooler than if the gases did no external work. In the same way a solution diffusing into water may do so without doing external work, or it may do so by a reversible process, through a semi-permeable diaphragm, producing considerable differences of pressure, which may be used to do work. The final condition in this latter case would, of course, be cooler than in the former case of inter-diffusion without doing external work. Now whenever convection currents are produced, these are to some extent reversible. We might put vanes into the liquid to be moved by the currents and to do work outside the liquid, and by reversing this we would reverse the convection currents. Hence any method of mixing in which convection currents are produced, which do work or produce heat outside the liquid, will necessarily produce less heat in the liquid than a method of mixing in which there are either no convection currents, or these produce heat by viscous flow inside the liquid.

That we can, at pleasure, either use the diffusion of two gases into one another to do external work or not, is really not different from the case of a single gas expanding into a larger volume. We may do work by this expansion and cool the gas, or we may allow the gas, as in Joule's experiments, to expand into a larger volume without doing external work, and in this case there is only a very small change of temperature.

In these cases it is a question of change of entropy in the system, which can either be effected by an irreversible process in which no work is done, or by a variety of other processes, more or less reversible, in which the more reversible they are the more work can be done. In the case of producing convection currents, or, in general, of diffusion of a heavy fluid upwards into a lighter one, the amount of heat produced would not be exactly the same as if gravity were not acting: the centre of gravity of the system is raised by diffusion. Now in Mr. Griffiths's case, and in the case of diffusion currents generally, this raising of the centre of gravity takes place throughout part of the space considered by diffusion, and the centre of gravity is continually falling down again in the convection currents. Hence the work that can be done by the convection currents is part of the work that was done by diffusion against gravity. In the case of diffusion without convection currents, we might use the whole of this work done against gravity, by which the centre of gravity of the system has been raised, to do external work. If, for example, the containing vessel were supported at its centre of gravity, in the unmixed condition, the centre of gravity would, after diffusion, be above the point of support, and the vessel and its contents might be arranged to turn round the support doing work during the fall of the centre of gravity to its original level. Another way of utilising the rise is to allow the fluid to flow into another broader vessel until its centre of gravity has returned to the original level. The thing to be specially observed is, that the amount by which the centre of gravity is raised depends entirely upon the shape of the vessel. If it be tall, the centre of gravity will be raised a great deal; while if it be low, the centre of gravity will be only slightly raised. By causing diffusion to take place in a tall thin vessel, the final temperature will be lower than in a broad low one, not on account of any superficial tensions, but on account of the work done against gravity. In Mr. Griffiths's methods diffusion is continually taking place along tall thin vessels, and convection currents lowering the centre of gravity again by flow into broad ones. GEO. FRAS. FITZGERALD.

THE EXPECTED METEORIC SHOWER.

THE imminent return of the Leonids once more attracts us to prepare for their observation and discuss their phenomena. The circumstances this year will be much more favourable, all round, than they were in 1897, but our prospects of witnessing a really brilliant return appear to be somewhat slender. No doubt, on the morning of November 15, meteors will appear in sufficient abundance to gratify moderate expectation, but the conditions scarcely warrant the influence that we are to have a grand display. We must wait until 1899 or 1900 to see the shower at its best. In 1832 it is true Dawes saw many astonishingly fine meteors; and well he might, for the parent comet of the Leonids was very near that section of the orbit which the earth intersected in the year named. In 1865 we passed through a region of the stream some way in advance of the comet, for the latter arrived at its descending node about two months after the earth had crossed the point. There was nothing deserving the title of a great meteoric shower on that occasion. But there was certainly an unusual number of fine shooting-stars, the majority of the objects observed being as bright as, or brighter than, stars of the first magnitude. At Greenwich it was estimated that more than 1000 meteors must have been visible on the morning of November 13. Mr. Knott, observing at Cuckfield in Sussex, estimated the number as more than one per minute for two observers. According to some other accounts the richness of the display far exceeded this, for a captain of a British ship, near the West Indies, wrote to say that the heavens were in a blaze with shooting-stars from 8 p.m. on November 12 to 5 next morning. But accounts of the latter description are often exaggerated, and it is always unsafe to draw any definite conclusions from them.

At the approaching return the earth crosses the meteoric orbit still further in front of the comet than it did in 1865. In fact the comet will have five or six months' journey to run at its highest rate of speed before it reaches its descending node. This is not allowing for any perturbations which the comet has experienced since 1866, and there is no doubt that some serious disturbances have been introduced, particularly, by Saturn and Jupiter.

It seems that in July 1895, the comet approached to within 45 millions of miles of Saturn, and though the former has not passed so near as this to Jupiter, both planets have exercised a very appreciable influence both on the comet and its associated meteoric stream. Dr. Berberich gives these conclusions in an important paper published in *Ast. Nach.*, 3526, and states as a result of his investigation that the meteor shower will appear 21 hours late in 1898 and 26 hours behind time in 1899. The comet of Tempel (1866 I.) is not, according to Dr. Berberich, likely to be observed at the ensuing return to perihelion, as it will present itself under unfavourable conditions. Dr. Berberich's results are interesting as showing the necessity for expecting the meteors on the mornings of the 15th and 16th, rather than on earlier dates. His conclusions seem strengthened by the fact that last year a pretty strong shower of Leonids was witnessed just before sunrise on the morning of the 15th, whereas very few were seen on the previous morning.

Under all the circumstances a very rich shower can hardly be expected. Our historical records do not warrant the assumption that the section of the orbit in the van of the comet is thickly strewn with meteoric particles. In the comet's wake, for an enormous distance, the material appears to be densely distributed. This was sufficiently attested by the succession of three brilliant displays of 1866, 1867 and 1868.

Meteoric and cometary phenomena are, however, somewhat unstable in character, and certainly variable