change of position in space of the centre of gravity of the atom, and the other the change of form of the atom itself; the first of these is known as free path motion, and the second as heat. The evidence for this may be briefly given.

First—It is certain that a heated body loses its heat by radiation, that is, it imparts its motion to the ether which transmits it in every direction as undulations having certain wave lengths and amplitudes. Second—It is certain that the energy of such undulations depends upon the amplitude of such undulations, and if the amplitude of the undulation was measured by the free path of the atom, then the radiant energy of the atom would vary as its free path, or in other words the rarer a gas is the greater its radiant energy. Now when the spectrum of a gas, say hydrogen, is examined, it is seen to be composed of lines having definite wave lengths, and wave length is dependent solely upon the rate of vibration. If this rate depended upon the number of impacts per second of the atoms or molecules of a gas, then these atoms would need to be always at exactly the same distance apart and the velocity of free path motion invariable, which conditions are physically impossible among free atoms, otherwise the spectrum we should obtain would be a continuous spectrum such as solid incandescent bodies give. But the spectrum of hydrogen for a given temperature is the same whether the gas be at ordinary pressure or very rare. This necessitates the conclusion that the heated atom which is thus radiating energy is vibrating quite independent of its position in space or of its free path motion, and the energy embodied in such vibratory motion is often spoken of as *internal energy*. When a swiftly moving bullet strikes a target, both bullet and target are heated and oftentimes a flash of light may be seen at the instant of impact. The free path motion has been changed into atomic vibrations, which at the first instant had a period capable of giving the sensation of light, but if the bullet be picked up at once it may not be uncomfortably hot. Now imagine two atoms in space urged by gravitation towards each other until they strike each other; each will be set vibrating, that is they will both be heated by impact, and until they were thus made to vibrate they would have no temperature at all; their energy would be represented by their free path motion; the greater their distance apart, when they began to approach, the greater would be their velocity at impact, and the period of vibration of each after impact would depend upon the character of the atoms themselves. One might have such a period as to give out undulations that might affect the eyes and we would say it was luminous while the other one might not, luminosity being dependent upon the rate of vibration, not upon the energy of vibration or the amplitude.

There are many phenomena, that are familiar enough, which show that luminosity does not depend upon high temperature. The decaying stump that shines at night, has a temperature not appreciably higher than surrounding objects; the swift moving molecules in a Crookes tube, that spend their energy upon the walls of the tube, cause the latter to glow, and the molecules themselves shine as they move in their long, free paths, but the tube is not uncomfortably hot, much less *very hot*. It is true that by increasing the energy of the moving atoms, the tube may be made red hot, but the point here is, that this is not essential for luminosity.

If then, in the process of universe building, we start with dissociated atoms, without any temperature,—at absolute zero, and let gravitation alone act among them, the first motions will be free path motions, and there will be no such thing as heat until atomic impact has begun; the energy that was at first represented solely by gravitation will now be partly changed into heat and radiation proper will begin, and the actual loss of energy to the involved atom will be greater than what would be due solely to gravitative approach; there might be luminousness with very little temperature, and one might speak of it as "fire mist," and as "glowing vapor," and yet not threaten the "Law of Interaction of Forces." Neither does the Nebula Theory fall, if originally matter was not hot, but cold.

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A. E. DOLBEAR.

DISCREPANCIES IN RECENT SCIENCE.

To the Editor of "SCIENCE:"

In his communication to your excellent journal (Vol. II., p. 142), Mr. Larkin has very correctly stated the discrepancy which is contained in the designation "fire-mist," as applied to the initiatory stage of nebular cosmogeny, the "Chaos" of Laplace—*sit venia verbo!* If the Nebular Hypothesis is a true representation of the history of our solar system (or all solar and other systems, for that matter) then, certainly, *heat* could have been present only after motion, and very lively motion at that, had been going on for quite a number of—well, let us say, billions of years, or pretty nearly that.

As soon as motion, *i. e.*, aggregation (and rotation) had begun, then, by the impact of the more distant portions of matter on those nearer the centre of the solar nucleus, heat was produced equivalent to the motion thus arrested. The primordial "Chaos," therefore, was cold and dark, if it ever did exist at all.

Mr. Larkin, consequently, is correct: There *is* a discrepancy!

Not so, Mr. Morris, whose objection is stated, *in nuce*, by himself (Vol. II., No. 41) in these words:

"Temperature and heat are very different things."

"It is one thing to contain heat and another thing to be in what we call a heated state."

To prove this he mentions the generally accepted facts "that a mass of water at 32° contains far more heat than an equal mass of ice at the same temperature; and a mass of water gas, (steam?) at 212° contains far more heat than an equal mass of water at that temperature."

The foregoing facts illustrate the phenomenon of "latent heat" or heat not appreciable by the thermometer. But *latent heat is not heat!* It is a misnomer that should have been eradicated from scientific nomenclature long ago. The heat which melts a pound of ice is employed in *performing* a certain amount of *work* by *overcoming* the *cohesion* of the solid ice. Its subsequent liquid state is the result of this work of heat. This heat has disappeared, is no more heat; exactly as the muscular force of the locksmith's arm disappears (is latent) at night, because by eight hours of filing he has overcome the cohesion of a quantity of iron. We can not look for the work and the force spent on it at the same time.

The greater mobility of the liquid and the diminished cohesion are the equivalent of the heat that has "become latent," *i. e.*, disappeared, absolutely, utterly and entirely, as heat. In changing water back again into ice, from the liquid into the solid state, the same amount of heat must be liberated, withdrawn, or allowed to escape, as was necessary to melt it.

Water, therefore, does not contain more *heat* than ice at 32° F.; it contains more mobility, energy, potentiality—in short, more *motion*, but not motion of the heat kind.

The same relations exist between water and steam at 212° F. Here the peculiar property of the gaseous condition allows us to appreciate the nature of the difference between water and steam much more precisely than that between water and ice. "Latent heat" is here simply *expansion*, and as expansion is the work of heat it is not heat. This we can prove by confining steam or any gas in a vessel with a movable wall. If the gas just fills the receptacle and we now apply heat, a thermometer will show a rise of temperature in the interior of the vessel.

As soon as the heat reaches a certain point, so that the

expansive force of the gas equals the resistance by the weight or friction of the movable wall, the latter will move and our thermometer, indicating the temperature in the interior of the vessel, will record *a fall of temperature*. Heat has "become latent," has disappeared, because it has done work, has moved the wall.

It is, therefore, not in accordance with the facts, if Mr. Morris states that a thing may be in a heated state and yet not contain heat. Nor is it true that "as density diminishes the heat capacity increases." The true state of things is evident from the examples given. Since diminution of density is effected by heat, is the work of heat, the gas thus expanded does not contain more, but less heat. The attenuation, the change of its state of cohesion, is the action of a certain amount of heat, and this heat has "become latent," has disappeared as heat, but it nevertheless exists in the expanded gas as a greater range of mobility, as diminished density. On condensation this heat again reappears, *i. e.* the range of mobility of the gas is diminished, and the motion, potentiality, energy or whatever name Mr. Morris should prefer to apply to this *causa efficiens*, is transformed into heat. This is, by the way, the very process that is supposed to have been going on in the Laplace "Chaos," and, therefore, proves again the discrepancy between the principle of the conservation of energy and the assumption that the primordial nebula was a "fire-mist," as Mr. Larkin has correctly explained.

As to Mr. Morris' conception of the action of gravitation, it seems still more erroneous. This gentleman says: "The earth must fall towards the body with the same energy that the body displays in falling towards the earth." Now, the two fundamental laws of gravitation, as first discovered by Newton are: Attraction acts in direct proportion to mass and in indirect proportion to the square of the distance.

The statement of Mr. Morris, therefore, is absolutely false.

Nor is this all. The possibility that gravity can act lies in the space given for the fall of a body acted upon. If, therefore, a body should fall, it must be raised first to allow it space in which to fall. If by some force 100 pounds are raised to the height of one foot, this body, if unsupported, will by its fall develop the very same force as was employed to raise it previously, viz.: 100 foot-pounds. In striking on some resistance, say the surface of the earth, it will develop an amount of heat, equivalent to its mechanical force 100 foot-pounds = 100 calorics or (small) heat units. Mr. Morris ignores the element of space, in so far as it must always have been previously furnished. He says:

"The motion that exists in a falling body was not created for the purpose. It existed in the falling body in some other form and has simply been transformed, not created."

But nobody ever contended that it was created. The possibility of its fall was given to the body by some elevating force, and it is the very same force which, having existed as potentiality to fall, as long as the body was supported, is transformed into motion, into falling, as soon as the support is withdrawn. And this fall is therefore not due to "internal forces" but to gravity and the space furnished by previous raising.

But is the discrepancy laid bare by Mr. Larkin the only one existing in regard to the Nebular Hypothesis? Nay, is this hypothesis really the true history of the world, and of our planetary system in particular? Is it as free from uncertainties and as little at variance with observed facts, as an hypothesis, which has found such ready credence and such universal approval, should be?

I believe it has no claim to such confidence as has been bestowed upon it, and since I have gone so far, I might just as well state a few reasons for my conviction, which I am glad to say is shared, or rather also held, by one of our first astronomers. This I shall do further on, but at present would beg to call the attention of your readers to two other discrepancies which exist between the principle of the conservation of energy and the Nebular Hypothesis—assuming, for argument's sake, the truth of the latter.

They may be best stated by two questions, to wit:

1. What existed before "Chaos," and how was "Chaos" brought about?

2. How long and by what influence did "Chaos" remain at rest, and what cause acted on it to force it into formative action?

I have never been able to get a satisfactory answer to these questions, nor have I been successful in answering them myself. They seem to convey the impression that a most glaring discrepancy with "conservation of energy" exists in this matter, of which every reader may become convinced on mature reflection.

For, in the light of the principle of the conservation of energy, motionless matter is an impossibility, as it is totally inconceivable to our understanding in every aspect. Again: Why should attraction—if we suppose that it was dormant while the primordial nebula was forming—suddenly begin to act? What force, what cause, instigated this action at a particular moment?

It has been said, and truly, I think, that it is a very cheap and easy matter to write upon the Nebular Hypothesis. But is it also an easy thing to write *against* this hypothesis? From the fact that it is rarely done, it would appear that it is not; and yet there are dozens of facts and arguments to be brought against it. Of these I will only name the more prominent ones, without going into detail:

The eccentricity of all planetary and lunar orbits.

The various deviations of the planes of all the planetary and lunar orbits from each other.

The retrograde motion of the moons of Uranus and of that of Neptune.

The composition of the rings of Saturn.

The greater velocity of revolution of the innermost portions of the rings than that of Saturn's surface.

The greater velocity of revolution of the inner moon of Mars than that of the latter's surface.

The immense number of comets and meteorites; their great eccentricities; the considerable number that have retrograde motion; the absence of any planetary nebula, so-called, which would allow of being interpreted as an initiatory stage of the formation of a solar system.

The multiple stars, etc.

The elaboration of the various facts above stated and several others* would far exceed the proper limits of this communication—which have, indeed, been rather overstepped already. I may be allowed, however, to add what Prof. Asaph Hall, to whom the paper described in the foot-note was submitted, wrote in reply:

"To me the Nebular Hypothesis is a very doubtful thing. The facts you mention are against it. Possibly its supporters may fudge it so that it will last a little longer, but it is always unsafe to rest on a theory based largely on our ignorance."

As may be imagined, I was very agreeably surprised by this card and asked Prof. Hall's permission to make use of it in case occasion should offer. This permission the excellent gentleman first declined to give; his reasons for so doing were stated in the following language:

"Such questions as the Nebular Hypothesis will only be decided by the slow growth of observation and knowledge and not by the opinion of this man or that. I have but little faith in it. . . . ."

After some further expostulation on my part, this permission was kindly given in a letter, written December 9, 1879. Not, however, until now have I made any use of

* This I have done in a paper published in the November and December issues of the *Gaea* of the 1878, edited by Dr. Hermann Klein, at Cologne, Germany.

it, except to refer to it in a general way.* I am glad that the discussion arising from Mr. Larkin's letter has furnished such opportunity, and avail myself of it to give it to "SCIENCE" for publication. GEO. W. RACHEL.

NEW YORK, *April* 11, 1881.

MICROSCOPICAL NOTES.

Recent investigations respecting the pathological relations of diphtheria, and the discovery of a micrococcal organism in the false membrane, have made it almost certain that the morbid poison which gives rise to the disease is a parasitic organism. M. Talamon now states that he has succeeded in finding this organism in eight cases. In the condition of complete development they presented a characteristic mycelium and spores. The former are tubes with partitions from two to five thousandths of a millimetre in length. These under favorable circumstances, elongate and bifurcate, the bifurcations being characteristic in consequence of their incurved branches, like the sides of a lyre. In other conditions the mycelia do not become elongated, although they multiply so rapidly as to cover the surface of the cultivated liquid; they remain short and assume irregular forms, and give rise to numerous straight rods. The spores are of two kinds, round or oval, which may be termed the spores of germination, and rectangular spores or conidia. The latter characterize the species. They form small rectangles of various sizes, their length being sometimes fifteen thousandths of a millimetre. They may be isolated or united in festoons or zigzag chains. At first homogeneous they soon become filled with small round granules, highly refracting, and of the size of ordinary micrococci. The round or oval spores are those which by their elongation constitute the mycelium. They appear as clear points, from three to five thousandths of a millimetre in diameter, in the middle of a mass of granular material.

Animals and birds inoculated with these organisms all died developing the characteristic false membrane.

These facts are very important, and open up an entirely new field of investigation, and M. Talamon already hints that he has a clue as to the source from which the organism is derived in the case of human infection. We trust that those of our subscribers who possess microscopes will follow up the researches of M. Talamon, which promise results of the highest value to science and to humanity.

NOTES.

THE bicarbonate of soda prepared by the Solvay process contains from two to three per cent. ammonia, and is therefore not suitable for pharmaceutical use, and for certain technological purposes.

A NOVEL FERRIC HYDRATE.—C. Graebe has received from the Baden Aniline and Soda works a reddish crystalline substance deposited in cast-iron vessels in which potash has been melted. It has the same composition as hematite and gœthite, but its specific gravity is only 2.93.

CHLORINATED DERIVATIVES OF CARBAZOL.—On treating carbazol suspended in acetic acid with chlorine, the liquid turns blue, yellowish, greenish, and, lastly, red. If the reaction is then interrupted the product is trichlorocarbazol in white needles, melting at 185°. If the process is continued for ten or twelve hours, hexachloro-carbazol is produced, fusible at 225°.—W. KNECHT.

PRESENCE OF ALCOHOL IN THE SOIL, THE WATER AND THE ATMOSPHERE.—A. Muntz has previously shown that it is possible to detect exceedingly slight traces of alcohol by converting it into iodoform. On concentrating the alcohol in a small volume of water by means of fractionated distillation, and using the microscope to verify the presence of iodoform, he was able to recognize with ease 1-300,000th of alcohol mixed with water. He has since improved the process so as to detect quantities even smaller than 1-1,000,000. During the last four years he has applied this method to river, spring, and sea-water, as well as to rain and snow. The results obtained leave no doubt of the presence of a neutral body, more volatile than water, and yielding iodoform. He thinks alcohol is the hydro-carburetted body present in the atmosphere, indicated by the researches of Boussingault and De Saussure. Soils rich in organic matter yield alcohol in such proportions that its essential properties may easily be verified.

NEW SYNTHESIS OF DESOXY-BENZOIN AND CRYSENE.—MM. Graebe and Bungener have obtained desoxy-benzoin by causing the chloride of phenyl-acetic acid to react upon benzol in presence of aluminium chloride. By the same reaction, naphthalin being substituted for benzol, they produce benzyl-naphthyl-keton. They then reduce with hydriodic acid and phosphorus at 150° to 160°, and pass the vapors of the carbide thus obtained through a red-hot tube, when 4 atoms H are removed and chrysene remains.

CERTAIN PHENOMENA OF OPTICS AND OF VISION.—M. Trève mentions the fact that the flame of a lamp appears brighter, and that a vertical shaft, a post, or mast is seen more distinctly through a vertical than through a horizontal slit, whilst a house, a landscape, or the disk of the sun or moon is perceived more clearly through a horizontal slit. He finds similar differences in photographs according as the light passes from the object to the plate through a vertical or a horizontal slit, and ascribes the results to the action of diffused light.

COPAL varnish for mounting objects for the microscope has been suggested by Mr. Julien Derby of the Quekett Club, who states that Mr. Van Heurck, of Antwerp, who first used it, has met with much success in mounting diatoms with that medium. This varnish is used about the consistency of oil and should be of that brand known as "pale copal." It has about the same refractive index as balsam, and is free from bubbles. Drop the copal over the object and slightly heat over a spirit lamp. In some cases a cover can be dispensed with, as it soon takes the consistency of amber, and is hard enough to sustain wiping and brushing with a soft brush with impunity.

WIDENING OF THE RAYS OF HYDROGEN.—The nebulous expansion of the spectral rays of hydrogen, noticed on increasing the pressure of this gas in a Geissler tube, is still ascribed to the influence of the pressure, though Dr. Shuster, Secchi, and others have shown that it is not possible to alter the pressure of a gas without at the same time affecting the resistance of the medium, and in consequence the temperature of the spark which traverses it. C. Fievez has undertaken to examine separately the influence of the different agents, temperature, pressure, direction of the current, etc., which have been suggested as contributing to produce this phenomenon. He finds that the widening of the hydrogen rays is correlative to the rise of temperature. We may affirm that the temperature of a celestial body is higher than that of another if its hydrogen rays are broader.

RECIPROCAL DISPLACEMENTS OF THE HYDRACIDS.—The action of the hydracids upon the salts formed by the halogens is in general the inverse of that of the elements themselves. Thus hydriodic acid expels hydrochloric acid from the metalic chlorides and hydrobromic acid from the bromides, whilst hydrobromic acid also liberates hydrochloric acid from the chlorides. The chlorides in general are decomposed by hydrobromic acid, and this decomposition preponderates according to the thermic value of the principal action. But the bromides may also be decomposed, though less readily, by hydrochloric acid. This inverse action previously pointed out by M. Hautefeuille in the salts of silver at a red heat, and by the author in the moist way, has lately been observed anew by M. Potilizine, but it is in no way contrary to thermo-chemical principles. It results from the existence of secondary compounds, partially dissociated, which intervene with their peculiar heat of formation. The theory of these reciprocal actions and equilibria is always the same. In every case we have to do with a principal re-action, foreseen by the thermic theory, and a perturbation equally foreseen by the same theory, of which it is a necessary confirmation.—M. BERTHELOT.

*"SCIENCE," Vol. I, p. 246, foot-note to the paper on Friedrich Mohr's Life and Works; *Scientific American Supplement* No. 266, p. 4,241, in a paper on "The Actual Figure of the Earth."