appeared very faint. This I attributed to its proximity to the bright star.

I shall continue the search for it. The moon will leave in a few days and I then hope to be able to see the comet again.

E. E. BARNARD.

May 15, 1881.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE:"

I should have attempted a reply to the restrictions of Mr. Dopp before this time if I had not had my hands too full of other work, but lest any might think I have nothing to say if an answer of some kind did not shortly appear, I will ask the favor of a little space, and first I entirely disclaim the pretension of undertaking to reconstruct Physical Science which Mr. Dopp seems to impute to me, and whatever was put forward as new was only hypothetical, and perhaps I was not guarded enough in specifying it as such. Yet there is more that may be said for some of the statements made than appears in those papers, which were very brief and did not pretend to give references. But now if I shall deal with the subject of internal and external energy which is attacked in the last part of Mr. Dopp's paper, it will save saying very much about the first part.

Mr. Dopp quotes from Maxwell's works on Heat, and says they disprove and invalidate all my calculations. But it will probably be allowed to hear Maxwell in 1875 against Maxwell of 1872:

"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 be the shape of the molecules, provided they are not perfectly smooth and spherical, the ratio of two parts of the energy must be always the same, *the two parts being in fact equal.*" He also says a few lines beyond when speaking of the researches of Boltzmann, he "makes the whole energy of motion twice the energy of translation." See Nature, volume 11, p. 375. This language justifies my work and my calculations are not invalidated. What is to be understood by $E' - E = \varepsilon$, is their difference and not a ratio, and the expression in the paper is wrong, but there is nothing in the paper that depends for its correctness upon any mathematical expression in it, whether it is right or wrong, and cannot be raised against it. That is to say, there is nothing in the first paper that is a deduction from any mathematical work given.

As to my definition of ether as not matter, again Maxwell is quoted against me, and I will therefore again quote Maxwell in my favor. "According to Thomson, though the primitive fluid is the only true matter, yet *that which we call matter is not the primitive fluid itself but a mode of motion of that primitive fluid.*" See Art. *Atom* Enc. Brit., 9th Ed. The italics are mine, but if it does not plainly make a distinction between ether and what we call matter, then I don't understand it. But I claim more, that to call ether the primitive matter is to call two different things by the same name, and my first paper was a protest against that. Newton's law of Universal Gravitation states that "every particle of matter in the Universe attracts every other particle of matter," and until it is discovered that ether possesses this property of attraction, I hold that the name *matter* should not be applied to it. If, however, any one thinks it to be a proper use of words, I shall not quarrel with him, only when he talks to me of matter I shall need to ask whether he means gravitative matter or non-gravitating matter. As for the objection that I use the term density applied to ether and am therefore to be held to what is implied in the word; any one who undertakes to express a new conception must either employ words that have some fixed meaning or else coin some new word which in its turn must be defined with old words. So while the term density conveys my meaning in a tolerable way, I do not wish to have it imply that density in ether and density in matter are identical. In the same article on Atoms, Maxwell says concerning the vortex-ring theory: "We have to explain the inertia of what is only a mode of motion," and this is in strict accordance with all I have written about it.

We do know that the motions of atoms set up corresponding motions in the ether, and it is not difficult to perceive how it may happen, though the particular mechanical conditions may not all be known. *Assuming that the conditions are mechanical,* then the analogy of the vibrating tuning fork is not so far fetched as it might be. I do not see the necessity for my being held to atoms combining in only one plane. It is as easy to see that three or four or more could all unite at the same place so as to form a radial structure or a triangular one when one of the two represented in the diagram should swing round 120°, which, so far as I can see, would not imperil its stability at all, and it then would be in position for another similar atom to unite with each, and so on almost any kind of a geometrical solid made. But I did not intend to assert at all that in this hypothesis there was anything more than an idea. I am not ignorant of the molecular form of ordinary matter, but my assumption was that the molecular form was due to its vibratory energy, and, consequently, I was mostly treating of atoms, and the statement was made that at or near absolute zero the chemical affinity was *nil,* and hence dissociation. This is plainly the case if chemism is due to heat vibrations, but it is corroborated by mathematical calculations. In a paper read before the American Academy, in February last, by Mr. D. E. N. Hodges, of Harvard College, but which has not yet been published, the same conclusion is deduced from thermo-dynamic considerations, namely, that at absolute zero "there can be no cohesion of molecules, and probably the same for atoms; it is the temperature of dissociation." Mr. Dopp quotes from Professor Tait what he knew about the phenomena of vortex-rings, but since Mr. Dopp's paper was written he has probably heard of some more phenomena of vortex-rings. See "SCIENCE," April 16th.

As to the paper on Atoms as forms of Energy the *idea* is not mine, but Thomson's, and whether or not the method therein shown of computing atomic weights is mathematical jugglery, as Mr. Dopp calls it, all I have to say is, I did not stake anything upon it. I thought if matter is a form of energy, the fact should appear in atomic weights, and so I made the calculations and published them, and if anyone thinks they signify nothing, why I will not quarrel with him. After so long a paper finding fault with anything I had written, it was something of a pleasure to read that he thinks my theory can be made "a fair working hypothesis to explain adhesion, cohesion, and even crystallization,—surface tension of liquids and capillary attraction, and possibly those of osmosis, dialysis and occlusion."

This is not an unworthy stock of phenomena to explain, and if what I advanced can not be made to do all I proposed to have it do, I might be content if it explained in a fair way any one of the above phenomena.

A. E. DOLBEAR.

COLLEGE HILL, Mass., *May* 10th, 1881.

To the Editor of SCIENCE:—

As two of your correspondents, Mr. A. E. Dolbear and Mr. George W. Rachel, have adversely criticized certain points in my article in the April 9 number of "SCIENCE, and as I still consider my position as stable, I must request a limited space to reply to these gentlemen.

The main difficulty seems to be that I have gone

counter to certain authors whom they are disposed to consider as authorities. But, in my view of the case, Science has no authority, except the authority of facts, and theoretical views are always fair food for criticism. Mr. Dolbear quotes from Clausius to the effect that "all heat existing in a body is appreciable by the touch or the thermometer; the heat which disappears * * * * exists no longer as heat, but has been converted into work." Heat is undoubtedly appreciable, but not necessarily measurable, by the touch or the thermometer. As the heat capacity of any substance increases its temperature effect for equal volumes of inflowing heat diminishes, so that the thermometer fails to indicate the exact quantity of heat which a substance receives in passing through a fixed range of temperature. It is customary in late authors to speak of this apparently lost heat as converted into work, or, in other cases, to speak of it as changed from actual into potential energy. This is, undoubtedly, a very convenient way of getting around the difficulty; but, with all due deference to the distinguished writers who advance this hypothesis, I venture to question if it is a strictly scientific way. To come plump up against a difficulty in your path, to explain this difficulty by a nicely sounding word which explains nothing, and then to go swimmingly on, enables one to get over a great deal of ground in a short time; but it is very apt to leave stumbling blocks for those who come after.

I should certainly like to see a precise definition of the word "work" in this connection. Heat produces a certain effect. That effect is called work. But the important question remains, what has become of the heat? It was a motion. Has it ceased to be a motion? If so, then motion can cease to exist. Yet I hardly think any scientist will admit such a possibility. But if it has not ceased to be motion, where is it? Is the word "work" advanced as a name for some new mode of motion? Whether it is or not, however, it fails to explain what has become of the heat. We meet with a like difficulty in the theory of the conversion of actual into potential energy. Actual energy we can readily comprehend; it is the energy of the motion of masses. But what is potential energy? It is a possibility of mass motion. A body rests upon the earth. It cannot possibly descend further. It has no potential energy. A body is suspended in the air. It may possibly descend further. It has potential energy. Potential energy then, is possibility of motion. Actual motion has been converted into possible motion. If this amounts to more than the explaining of a difficulty by a meaningless phrase, I should certainly be glad to have some one scientifically explain the explanation. I must quote from my former article: "Motion is motion and cannot possibly be or become anything else." Actual motive energy cannot cease to exist, and be replaced by an abstract possibility of motion, called potential energy.

In regard to Mr. Rachel's remarks on my views respecting variation in heat capacity, he must permit me to correct his quotation. He quotes me as saying: "Temperature and heat are very different things." I find my expression to be: "Temperature and *absolute* heat are very different things." There is a considerable difference of meaning between these two expressions, which it would have been well for him to give me credit for. The main difficulty in the minds of both my critics seems to be a somewhat confused idea as to what constitutes heat. Mr. Dolbear claims that the free vibration of molecules is not heat. In this he certainly disagrees with most authors. Mr. Rachel states that "latent heat is not heat." He intimates that it is work, but will he be kind enough to explain scientifically just what work means in this connection? He says further, "Water does not contain more heat than ice at 32°; it contains * * * more motion, but not motion of the heat kind." Of what kind then? "Nor is it true that as density diminishes the heat capacity increases." The heat has disappeared as heat, "but it nevertheless exists in the gas as a greater range of mobility."

We here get his definition of "work." It is "motion, but not motion of the heat kind;" it is "a greater range of mobility." Motion, then has not ceased to exist, and we have been splitting hairs about nothing. It is molecular motion, but not the special mode of motion which he calls heat. Yet it would be well to bear in mind that scientists are somewhat indefinite in their ideas as to just what mode of motion does constitute heat. In one case they speak of radiant waves as heat, in another as local molecular vibrations as heat; in a third, of the free motions of gas particles as heat, and in a fourth, of motive influences which cease to affect the thermometer as heat, for what else is meant by absolute heat? The *authorities* certainly consider that heat continues to exist as heat in the case of increased heat capacity, when they assert that specific heat varies with variation in the temperature of substances. Thus it seems that all motive influences of which we become aware in matter, outside of gravity, electricity, magnetism, light, chemism, and mass motion, are grouped together as heat, their varying conditions being simply pointed out by qualifying adjectives. The phrase, "Latent Heat," has by no means gone out of use. Sir William Thomson, in the last edition of the Encyclopedia Britannica, considers it necessary to still retain it. In fact there are various modes of motion, some centrifugal, others centripetal in their effects, so closely related to ordinary heat, that it has proved more convenient to consider them as special heat conditions than to devise separate names for them.

Mr. Rachel is still more decisive in regard to another portion of my article. He says, "Mr. Morris's conception of the action of gravity is 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 discovered by Newton, are attraction acts in direct proportion to mass and in indirect proportion to square of distance. The statement of Mr. Morris is therefore absolutely false."

Perhaps so, yet I hardly think that Newton himself would have so absolutely denied my proposition. Let us suppose the falling body to be increased until it equals the earth in weight. What would follow then—would not gravity cause them to approach each other with equal energy? Their attractive pulls upon each other would be equal, and therefore the effects of these pulls must be equal.

If, however, the falling body be greatly decreased in weight, this may seem to some to change the elements of the problem. Yet it can readily be shown that difference in weight makes no difference whatever in the result. We must not look upon the earth as fixed and the falling body alone as movable. They are both freely floating masses, each capable of yielding to any exterior impulse. The size or weight has nothing to do with the question. If an atom and the earth be side by side, and be attracted by a distant mass with the same vigor, they must move with equal energy towards it. Yet an energy which would give the atom excessive speed would produce an inappreciable effect upon the earth.

Suppose, for the sake of illustration, that the falling body weighs one pound and the earth one million pounds. Then the falling body will attract each pound of the earth's mass with a vigor dependent on its distance, and be attracted by it with equal vigor. To reach the whole attraction of the falling body we must add together this million of separate attractions. But, in like manner, to get the whole attraction of the earth we must add together its million of separate attractions. The body exerts a separate attraction upon each pound of the

earth's mass. Each pound of the earth's mass reacts with an equal vigor of attraction upon the body. We must add all these separate attractions together to get the whole sum of attraction in either case, and these whole sums are necessarily equal. The body, therefore, attracts the earth with as much vigor as the earth attracts the body, and necessarily, therefore, they must approach each other with equal energy. Of course not with equal speed. Under the above supposition their respective weights were as a million to one, and a million pounds falling one inch would be equivalant to one pound falling a million inches. Their acceleration in speed must likewise, in both earth and body, obey the law of gravitative acceleration.

It is well, therefore, to bear strictly in mind, that in gravitation, as in every other form of force, action and reaction are always equal and opposite.*

CHARLES MORRIS.
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PRIMEVAL ROTATION AND COSMICAL RINGS.

II.

To the Editor of "SCIENCE":—

Prof. A. Winchell, recounting the history of formation of the solar system from a sphere of incandescent gas, says: "The cooling and contraction of this vapor inaugurated a rotation." [1]

Matter is governed by law, hence the ball of gas must have obeyed laws governing gases. Men have detected several laws of nature, while doubtless there are others eluding research. The globe of gas was dominated by known or unknown laws; if by unknown, no scheme of planetary evolution can be outlined; by known, hypotheses are tested by their application. There exists a doctrine, the Nebular Hypothesis, and we take it for granted that its advocates conceive the gaseous sphere to have been wrought by known laws.

But no law of nature yet discovered is able to cause a sphere of gas to rotate.

Contracting by cooling did not begin rotation, for, by dynamic laws, the mass was not hot, but cold. If hot, contracting would not cause rotary motion, but would give rise to two motions, centripetal and peripheral, both radial instead of circular. The heaviest atoms, gravitating towards the centre, would displace the lightest towards the circumference.

Repulsion did not exist; this force can only obtain in matter *not* dissociated. Repulsion causes dissociation and vanishes, gravity reasserting dominion. Hence, repulsion is more ancient than that gravity which caused the mass to develop a solar system; else the first state of matter was in dissociation.

These things are unknowable; therefore, with adherents of the hypothesis, we dismiss repulsion, leaving the mass subject to no known energy but gravity. If repulsion did act it could not cause rotation. Gravity could never cause the ball to turn; it would bring every atom to a rest. The whole mass would arrange itself in concentric strata, whose distance from the centre would depend on specific gravity. Calm would ensue unless pressure was sufficient to force atoms within range of chemism. Chemical reaction would have no power to start axial revolution. It would evolve heat, repulsion and temporary expansion, which, waning, would leave the mass smaller through combination, no sign of rotary motion having appeared. The mass extended half way to α Centauri, it being equal in mass to the sun. Helmholtz has shown that if the matter in the solar system expanded to Neptune, "it would require several cubic miles to weigh a single grain." [2] But the same matter filled a sphere whose radii were half the distance of the stars in length. Estimation of its tenuity indicates that a space as large as the moon only contained a grain. Yet it was "intensely heated." [3] It is not known how many atoms make a grain; counting them by the million, they were yards apart, in frigid voids—hot! Obeying gravity, they descended with slowest conceivable motion; at no point in their fall displaying tendency to move in arcs of circles at right angles to their radial movement, which they must do to begin rotation in the cosmic sphere. In the present state of knowledge, judging from laws at work in the Universe, it can be safely asserted that the ball had no rotary motion. Ignoring these considerations, we will assume with Winchell that it was in revolution.

"The cooling and contraction of this vapor inaugurated a rotation which was inevitably accelerated to such an extent that a peripheral ring was detached which became a planet. The same process continued and other rings were detached which became other planets in due succession. Similarly, the planetary masses detached rings which became their satellites." [4]

Conceiving the mass to have cut loose from 61 Cygni and other cosmic masses; admitting cooling, contraction and acceleration, then the sphere would be unable to cast off by any law of nature hitherto discovered, the least particle, to say nothing of a massive ring. The ball had dwindled to the orbit of Neptune, acquiring such volocity as to no longer remain intact, so it cast off equatorial matter enough to form that planet.

The rate of motion on the equator was only 3.36 miles a second; and a vacuum as made by Crookes is as a solid compared with the density of the ring; yet Neptune's mass is nearly 102 sextillions of tons [5]. The material, being exterior, was of the lowest specific gravity of any in the mass; thence its volume was enormous; so great as *not* to be peripheral. The word periphery alludes to the surface, and Winchell says the ring was peripheral. It was not,—it was formed of gas torn up from a depth of hundreds of millions of miles, in order to secure substance sufficient to form Neptune. If not,—the mass was piled above the level of the equator, an impossibility, as gravity would bring it down. As soon as force raised a line of atoms above the equatorial level, around the ball, the next line of atoms below would ascend, then the next, and so on. The poles would depress causing the mass to assume lenticular form. This would retard rotation, allowing central attraction to regain control, bringing the mass to a sphere as in the beginning. This oscillation must take place so long as the mass remained a gas. Should it become fluid, then the alternations would be between a sphere and spheroid, and the mutation would obtain until solidification sets in. No atom at any period had power to overcome gravity, the stability of the mass being assured by inhering laws. The mass of the solar system, the mass of any planet, the direction and velocity of the planet's original motion, determine what orbit it shall traverse.

The orbit of Neptune is determined; it makes regular revolutions, hence the centre of the assumed ring that formed it, when abandoned coincided with the present track of the planet's centre. Therefore the ring was not detached when the mass was lenticular, for its edge then extended far beyond where Neptune now revolves; if it had been the planet would now describe our orbit much *farther* from the sun.

The mass reached the present path of Neptune when spherical, and that world was thrown off where it now makes circuit, the mass being a sphere when it parted with its first ring.

*In my previous article, above referred to, there is a typographical error, which slightly confuses the meaning. On page 167, line 49, the phrase "this force is increasing," should read, "this force is unceasing."

[1] Geology of the Stars, p. 260.
[2] Youman's Correl. and Con. Forces, p. 231.
[3] Geology of the Stars, p. 279.
[4] Geology of the Stars, p. 279.
[5] Chambers' Astronomy, p. 898.