First, let the fact be stated, that during the total phase of the eclipse which lasted but 162 seconds, two experienced observers, with telescopes in every way well adapted for the work, state with positiveness that each saw two objects not down on any star chart, and, that they were not there when the sun had sufficiently withdrawn to allow the locality to be re-observed. On the other hand, three observers who searched west of the sun, one in a cloudy sky, and two of the others poorly equipped, and, devoting but a few seconds to the search, saw nothing, not even θ Cancri, a star of the fifth magnitude, near where one of Watson's and both of my objects were seen. The weakness of this negative testimony will be apparent from a few extracts from their reports.

Mr. Wheeler (telescope 5 inch, power 100) says, he observed the second and third contacts (beginning and end of totality), also the Corona on both sides of the sun, saw with the naked eye Venus, Mercury and Regulus, observed carefully the several prominences, etc., and then says, "An unsatisfactory attempt was made to sweep for Vulcan. The time given to it was limited, as I was expected to observe all the contacts, and time was consumed in recording the second, and again in bringing the telescope into position for observing the third contact." Now when it is considered that he undoubtedly occupied several seconds in looking at the grand sight with the naked eye, and, that the power used was altogether too high, and of course, the field very small, the time devoted to the search for Vulcan could have been but a few seconds. Is it therefore surprising that Mr. Wheeler saw nothing of the objects seen by me? Only those familiar with the use of telescopes know how perplexingly difficult it is to bring a well-known object in the field of a telescope, using a power of 100.

Mr. Bowman (telescope 3 inch, power 30) says he searched *north* and west of the sun (my objects, also Watson's, were southwest), and that some time was lost (during totality) in exchanging the diagonal tube for the straight one, swept to the westward 5° or 6° in the declination of the sun, and then returning, shifted the declination just far enough *north* to clear the Corona and swept to the westward again, then returned to the R. A. of the sun and shifted to the proper declination just in time to observe the third contact. When it is considered how much precious time was lost in observing and recording in his note-book the time of second contact, changing tubes, and probably observing the eclipse for several seconds with his naked eye, which he could hardly refrain from doing, is it at all wonderful that Mr. Bowman saw nothing of my objects or Watson's either?

Prof. Todd (telescope 4 inch, power 20) says, "I searched 15° each side of the sun, but the sky was *cloudy*, so much so that I was unable to see Delta Cancri," (a 4th mag. star). He does not say how much time he spent searching west of the sun. It certainly could have been but a moment, and, in the region where my objects were, but a few seconds. He, too, observed the second contact, also the Corona, saw Mercury, Venus, Mars, and Procyon. Again I ask is it at all surprising that Prof. Todd saw nothing of the objects seen by me?

Prof. Pritchett (telescope 3½ inch, power 90) says he first observed the grand scene with a naked eye, then swept along the ecliptic several degrees *each* side of the sun, observed all the phenomena of the eclipse, the second contact, Corona, the prominences, and the question arises how many seconds he searched with a very small field west of the sun for the "Ghost of Vulcan," as he facetiously calls it. Still again I ask is it at all wonderful that Prof. Pritchett saw nothing of the objects seen by me? Would it not, in fact, have been very surprising had he seen them at all?

Your correspondent has given in his diagram the outlines of the regions swept over by the above observers, saying: "The place of one of Watson's stars was covered by Wheeler, Bowman and Pritchett, and the place of Swift's two stars was examined by Bowman and Wheeler, and that one of the stars appears in the corner of Pritchett's sweep." Now all this is calculated to convey a wrong impression, for it is not likely that either of them knew within from 1° to 3° the exact boundaries of their hastily-made sweeps; neither do I pretend to be exact about the location of the stars I saw, although I made three estimates of their deviation and distance from the sun, by sighting along the outside of the telescope tube.

They are wrongly placed in the diagram. They were nearer where Theta is, and probably somewhat west of it, which would place it outside of the sweeps of all the observers. I should strongly suspect that one of them was θ, were it not that Watson, who says he saw that star, says nothing about another equally bright some 7′ from it, both ranging with the sun's centre.

Neither in his published statements, or letters to me, does he allude to this vital point. It was as impossible for him to have seen one and not the other, as for one to see Epsilon 4 Lyræ, without, at the same time, seeing Epsilon 5.

Again, he says, as far as relative position is concerned, my objects resemble closely d^2 Cancri, and B. A. C. 2810, on the *east* side of the sun. I hope he does not mean to be understood as inferring that it was on the east, instead of the west, of the sun I was searching.

Finally, he says, the existence of an intra-mercurial planet is not yet admitted by the majority of astronomers. This may be true, but I hope their opinion is based on stronger evidence than that adduced by "W. C. W."

LEWIS SWIFT.

ROCHESTER, N. Y., *April* 11, 1881.

CORRESPONDENCE.

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

DISCREPANCIES IN RECENT SCIENCE.

To the Editor of SCIENCE:—

The article on "Discrepancies in Recent Science" in a late number of this journal demands some attention, not because the Nebula Theory is seriously threatened by it, but because it properly calls attentions to some physical inferences that have been drawn from other phenomena and applied to the Nebula Theory, especially in the domain of heat. It is assumed by the writers quoted in that article, that *luminousness implies high temperature* and also that the rarity of the gaseous material of the nebula is the immediate result of the high temperature of the constituent atoms. Neither of these assumptions is correct. The trouble comes chiefly from the writer's failure to make the proper distinction between *energy* and *heat*, and I apprehend, also, in the failure to see clearly what the nature of heat is. Most of the books treat of this in a very loose way, and most of the statements on the subject by Mr. Charles Morris are wrong. How far wrong may be seen by comparing his statements with the following quotation from "The Mechanical Theory of Heat," by Clausius, Chap. 1st, Sec. X, p. 24: "*All heat existing in a body is appreciable by the touch and by the thermometer; the heat which disappears under the above changes of condition (fusion and vaporization) exist no longer as heat, but has been converted into work, and the heat which makes its appearance under the opposite changes (solidification and condensation) does not come from any concealed source, but is newly produced by work done on the body.*" We have all along been familiar with the conception of *heat as a mode of motion*, but not with the character of the motion except as "a brisk agitation of the molecules" or "a rapid vibration of the atoms;" but there are two kinds of vibratory motions possible to atoms, one of the character of pendulous motion or a 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.

TUFTS COLLEGE, MASS.

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