own brains, I would seem to be justified in resenting this peculiar argumentation.

I might, in view of this unjust criticism, retort that perhaps it is altogether a better way to rely on an occasional authority, a good number of whom are towering up high above the sea of opinions as trustworthy beacons of light, than to steer along without looking up to them as guides now and then, and perhaps be wrecked on some unknown shore or unsuspected reef. The tendency to scoff at authorities because they are authorities, is just as pernicious as that to put faith in them for this same reason only.

As to my somewhat confused idea of heat, of which Mr. Morris takes the liberty to speak, I confess that I have supposed he understood the difference between radiant and conducted heat,* and he also was aware what was understood by universal consent with the expression "work." I should not have undertaken this discussion on physical subjects had I not been convinced that the terms to be used were agreed upon. However, Mr. Morris seems to be in a fair way to come down to the very last questions about the nature of motion and matter.

As to "*latent heat*," if Mr. Morris, Sir Wm. Thompson,* and many others persist in calling heat that which is not heat, they are at liberty do so; yet they are wrong.

This I have conclusively shown, and Mr. Morris has not even tried to argue on it. Nor has he thought necessary to argue in regard to my remarks on his erroneous conception of the action of gravity. He only reiterates his assertion that the energy with which a body weighing a million pounds would fall on a body weighing one pound is the same. In order to prove this he says "we must add," and add and add, and then *one* will develop just as much energy as a *million!*

It seems futile to argue longer on a proposition that is in direct conflict with Newton's first law. If Mr. Morris has no room for the latter in his Universe, I must respectfully decline to enter it, preferring to stay outside of it in company with Sir Isaac and various others equally sound and reliable.

If Mr. Morris says motion is motion and cannot possibly become anything else, he is certainly right; but he forgets that there are certain *forces* for which we have as yet not been able to *prove* conclusively that they *are* motions. Of course, Mr. Morris has told us how he conceives of this relation between gravity and molecular motion, so called. (And there is cohesion and magnetism yet to account for.) But his explanations are wide away from the mark, which lies in an entirely different direction.

The combined action of all the radiant energy emanating from an infinite number of celestial bodies is transmitted in every direction through the Universe, and by oscillations, vibrations, and undulations of the attenuated matter (*not ether*—there is no ether!) which fills the interstellar spaces. In striking the surface of the various orbs, great and small, it exerts a uniform pressure, gravity.

Respectfully,
GEO. W. RACHEL, M. D.

NEW YORK, *May* 30, 1881.

THE "*Astronomische Nachrichten*."—It is announced that after the termination of the current volume, by authority of the Prussian Government, a new arrangement for the management of this journal will take effect. It will be edited by Prof. A. Krueger, the director of the Observatory at Kiel, in co-operation with the president of the "Astronomische Gesellschaft," of which association it will become a recognized organ.

* SCIENCE, Vol. I. p. 245. L. 24 fr. below.

* * Admits that t is not heat, but favors the expression for convenience.

*To the Editor of* SCIENCE:

I can scarcely permit such curious statements as made by Prof. A. E. Dolbear, to pass unnoticed. In "SCIENCE" No. 43, he says:—"The decaying stump that shines by night, has a temperature not appreciably higher than surrounding objects." Can it be possible that he compares the state of matter in ancient wood, with the inconceivably rare gas whence Neptune was formed? Several cubic miles of it only weighed a grain, as has been proven by Helmholtz. It was in dissociation, no two atoms touched, therefore we assert with reason that it was absolutely cold and dark. The atoms in the stump had been in intimate association; indeed their organization was once so complex as to have been endowed with that most mysterious of all entities—LIFE!

When decaying, it was surrendering the force whose work organized it, and its faint luminosity was a portion thereof. The light was a result of preceding work, but in interstellar space, where atoms were yards apart, no previous work had been performed, and no force evolved whether heat, light, or any other save gravity and the slowest radial motion possible.

EDGAR L. LARKIN.

NEW WINDSOR OBS., Ill., *June* 13, 1881.

## REPLY TO DR. J. J. MASON'S LETTER.

The writer of the review referred to, states that notwithstanding the construction which Dr. J. J. Mason now desires to see placed upon his words, the most careful reader would fail to draw any other conclusion from Dr. Mason's article, than that it was written in support of the theory that large cells are motor, and that sensory cells are small. It is true as Dr. Mason states that the sentence just preceding the one quoted in his letter refers specifically to the spinal cord of the turtle. But it is none the less true that the whole paragraph polemizes against a statement of Stieda's that the observations "have great weight against the conclusion that only the large nerve cells are connected with motor fibres," as not representing the ordinary view. In the earlier part of his article, Dr. Mason indeed goes so far as to question the statements of our best cerebral antomists that certain very large cells are connected with the auditory, *i. e.* a sensory nerve, and this in obedience to the same theoretical bias which is manifested a few lines further on in this wise. "I would suggest, however, to those who may feel disposed to regard these cells (large cells of auditory nucleus and oblongata) as connected with the sense of hearing, that such a view involves giving to this apparatus in its central portion, a structure almost identical with one universally admitted to be motor, like, for example, that concerned in raising the lower jaw; whereas in the central structures for vision and olfaction *the cells are all very small.*" (Italics are own.) What other than the size of the cells and their nuclei does Dr. Mason refer to when he speaks of a "structure universally admitted to be motor?" Especially when it is borne in mind that immediately after he claim that all sensory cells are very small. In view of all this Dr. Mason's statement that no such claims as the one imputed to him by the reviewer had ever been made by him "in any form by hint, inference or otherwise," must have been penned in strange forgetfulness of what he has laid down in his published article. The reviewer can only interpret the remonstrance as an abandonment by Dr. Mason of his previous position. Every statement in the quoted paragraphs is simply erroneous, and to bring Dr. Mason face to face with facts that he has questioned, the reviewer refers to Dr. Mason's statement that the cells connected with vision "are *very* small," and the reliable findings of Professor Packard, who happened to state that in the locust these cells are very large in relation to the other cells of the nervous system.

R. C. S