questions on this point, and we would be obliged if any of our correspondents would furnish a few facts. We understand that some makers make use of a very soft glass, the surface of which becomes defaced in a short time: a dealer calls this "*spongy glass.*" We would like to know where the respective makers purchase glass for objectives, and its composition.

ASTRONOMY.

COMET (*b*) 1881.

Prof. Barnard, of Nashville, Tenn., announces the discovery of a comet on the morning of May 12, 1881, in R. A. $22^h\ 59^m\ 18^s$, dec. $+ 14° 24' 30''$. An observation on the following day gave R. A. $22^h\ 58^m\ 52^s$, dec. $+ 14° 36' 0''$, thus indicating a motion 24^s in R. A., and $11''$ in Dec. The comet is reported as very faint.

ON THE USE OF THE ELECTRIC TELEGRAPH DURING TOTAL SOLAR ECLIPSES.*

If we suppose a single observer to be prepared for the observation of all the total solar eclipses of a century, we shall find that the entire amount of time during which he may contemplate the totally eclipsed sun will not differ much from an hour. We may be sure then of the expediency of any scheme whereby the rare moments of these eclipses may be utilized to their utmost extent. If such scheme is devised, two important results are likely to follow.

(1.) Economy of the sum-total of energy in any particular line of solar research.

(2.) A consequent enlargement of the means of research in other directions.

The general conception of the scheme proposed in this paper may be very briefly stated: Suppose a station to the east and a station to the west on the line of any total eclipse, as widely separated as practicable, and equipped for similar observations of discovery during the progress of the eclipse; the method proposes the electro-telegraphic transmission of important observations made at the western station to observers at the eastern station, with due speed for their verification or rejection when the lunar shadow reaches the latter station.

For illustration, consider the next total eclipse,—that of 1882, May 16. In detail, the particular advantages in connection with this eclipse seem to be about these:

(1.) The path of totality is almost exclusively on land. Central eclipse begins in West Africa; the line of totality passes to the north-east, crossing Upper Egypt and the Nile at El-Akhmym; thence over the Red Sea, crossing the Tigris a few miles to the south of Bagdad; then passing a little to the south of Teheran, it traverses Central Asia, and leaves the Asiatic Continent somewhat to the north of Shanghai.

(2) Though not generally through the inhabited regions of the globe, the path of totality lies through several inhabited regions which are widely separate, viz: Egypt near the Nile, Central Persia and Eastern China.

(3.) These regions are inter-connected by telegraphic cables and land-lines.

Now, we will suppose that an important observation of discovery is made at El-Akhmym,—an observation of an intra-mercurial planet for example. Between 40 and 45 minutes of absolute time elapse before totality comes on at Teheran. During this interval the observer at El-Akhmym will have an abundance of time for transcribing the apparent magnitude and the precise position of the new body, and transmitting the same to his fellow-observer at Teheran several minutes before the lunar shadow reaches him. The latter observer will then have leisure to proceed with the setting of his circles, the verification of their readings, and the pointing of his instrument to the precise part of the heavens indicated. He may then be able to see the suspected object before the eclipse becomes total. He may also decide upon a neighboring star for comparison with the planet, and thus obtain a very accurate determination of its position. The observer at Teheran should also be prepared for an independent search for the suspected planet, in the event of receiving a negative message from the observer at El-Akhmym.

The observation at El-Akhmym should also be transmitted to Shanghai, (reached by the shadow more than two hours after totality at Teheran), for independent verification at that point. We might thus observe the result of nearly three hours' motion of the planet,—which we might reasonably expect to give important data in regard to its orbit about the sun. Of course, the result of observation at Teheran would also be transmitted to the observer at Shanghai.

It was my intention primarily to have considered the total eclipse of 1882 merely as an illustration of the method proposed. Further investigation, however, seems to show that it is at least one of the two most favorable eclipses during the present decade, if not during the present century.

WASHINGTON, *May* 18, 1881. W. C. W.

*Abstract of a paper by D. P. Todd, M. A., presented before the American Academy of Arts and Sciences, Jan. 12, 1881.

COMET (*b*), 1881, BARNARD.

To the Editor of "SCIENCE:"

On the morning of the 12th, while sweeping the eastern sky in search of comets, at about three o'clock, an object entered the field of my telescope which I strongly suspected was a comet, as I did not know of any nebula near its place. I at once secured its position relative to *α Pegasi*, it being in the field with that star. Its position at seven minutes past three o'clock was:

$$\text{R. A. } 22^h\ 59^m\ 18^s$$
$$\text{Decl. } + 14°\ 24'\ 29''$$

The object was watched at intervals until about four o'clock, when daylight prevented further observation. During this time no motion was detected. Wishing to confirm the discovery by a second observation, before announcing it, I waited until the following morning, when upon turning my telescope to the point where the object was seen, I found it had disappeared.

No doubt now remained in my mind of its cometary character. I began a search to re-discover it. After sweeping for some time in the immediate neighborhood, I found it again as day-light was whitening the sky. It was very close north following *α Pegasi*. The object was then only visible when the bright star was obscured by a part of the ring suspended in my eye-piece. It followed the star by six seconds and was therefore in R. A. $22^h\ 58^m\ 52^s$. I estimated the difference of declination between comet and star, and found it to be in north declination $14° 36'$. No doubt now remaining that it was a comet I telegraphed its position to Professor Swift, Director of the Warner Observatory at Rochester. On the morning of the 14th I again began a search as soon as the object had risen above the horizon, but it could not be found. At first I attributed my not finding it to its low altitude and the bright moonlight. The search was continued until daylight, and I was deeply mortified at not finding any trace of the object. In the morning telegrams from Rochester and Boston announced failure to find it at those places.

A short search this morning, when the sky had cleared, at about day-light, resulted no better than yesterday morning. The object on the 12th was slightly smaller than Swift's last comet, which I had been observing on the 11th, and was probably a little brighter. On the 13th it 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 $E' - E = \epsilon$, 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