surface is more nearly black than white, which seems to render the existence of snow fields upon it less probable, unless they are covered with volcanic dust, as the end of a glacier usually is with rock *débris*.

But even if we take Mr. Reade's view, it is still conceivable that steam may have been the explosive agent in the moon's volcanoes, while her internal temperature was very high, and that the resulting water may have been subsequently absorbed after the body became cool, because the water would occupy less space within the interstices, which this theory of imbibition postulates, than the equivalent vapour did, when the temperature was high. The case of the earth would not be a parallel one, because it has not yet cooled.

Although not myself a selenologist, I have a suspicion that very little is known about the constitution of the moon; and that it is not even certain that its enormous craters are all of them really volcanic. It has been admitted by Prof. Darwin, in discussing the subject with Mr. Nolan, that on his view of the genesis of the moon it must have originally existed as a "flock of meteorites." These falling in during the later stages of the building up of its mass would have produced pits on a viscous surface, much like some of the craters.

At any rate it seems unsafe to rely upon arguments respecting the condition of the earth's interior, of which we know little, drawn from that of the moon's body, of which we know less.

Harlton, Cambridge.

O. FISHER.

Exact Thermometry.

THE interesting experiments of Dr. Sydney Young, recorded in NATURE of December 19 (p. 152), seem to leave no doubt that the main part of the permanent ascent of the zero-point of a mercurial thermometer, after prolonged heating to a high temperature, is not due to compression of the bulb—rendered more plastic by the high temperature—by the external atmospheric pressure. Researches on the effects of stress on the physical properties of matter have convinced me that the molecules, not only of glass, but of all solids which have been heated to a temperature at all near their melting-point, are, immediately after cooling, in a state of constraint, and that this state can be more or less abolished by repeatedly heating the solid to a temperature not exceeding a certain limit, and then allowing it to cool again (it is not only the heating but the cooling also that is efficacious). It appears that the shifting backwards and forwards of the molecules, produced by this treatment, enables them to settle more readily into positions in which the elasticity is greatest and the potential energy is least.

This "accommodation" of the molecules, as Prof. G. Wiedemann and others have called it, is, as one might suppose, attended with alterations of the dimensions and other physical properties of solids, and is not confined to the release of molecular strain set up by thermal stress, but is extended to the strain set up by any stress whatever. As years roll on, the time of vibration of a metal pendulum gradually alters (and so, no doubt, do the lengths of our standard measures), the bulb of a thermometer diminishes in volume, a steel magnet parts with more or less of its magnetism, a coil of German-silver wire gains in electrical conductivity, &c. The changes in all these cases would probably be far less than they actually are if the temperature throughout the whole time could be maintained constant; but this last is not the case—heating and cooling goes on more or less every day. We may assist the effect of time by artificially increasing the range of temperature, but it would appear that we must not exceed a certain limit of temperature, which limit depends partly upon the nature of the substance and partly upon the stresses that are acting upon it at the time. Thus, the internal friction of a torsionally oscillating iron wire which has been previously well annealed may be enormously diminished by repeatedly raising the temperature to 100° C., keeping it there for several hours, and then allowing it to fall again. The amount of diminution of internal friction depends upon the nature of the wire, and on the load which there is at the end of it (if the load exceeds a certain amount, the friction is increased instead of diminished). In attempting to "accommodate" the molecules in this manner the heating must, at any rate in some cases, be prolonged for several hours, and the substance should then be allowed to remain cold for a still longer period.

I have not had much experience with glass, but I think it prob-

able that the settling down of the zero-point of an ordinary thermometer into its ultimate position could be very materially facilitated by the heating and cooling process mentioned above.

HERBERT TOMLINSON.

36 Burghley Road, Highgate Road,
December 23, 1889.

Self-luminous Clouds.

WITHOUT venturing to call in question the occasional occurrence of self-luminous clouds, I may be permitted to relate an observation which seems to reveal a possible source of error in the records of such phenomena.

On June 14, 1887, about 10.45 p.m., I witnessed an appearance over the north-north-west horizon which struck me as very remarkable. Amidst the strong glow of twilight a few fragments of cirrus cloud shone with a pure white light having so much the character of phosphorescence that it was difficult to believe the objects were not self-luminous. Looking out again an hour later, I found no trace of bright clouds, but in their place were small bands of cirrus showing dark and grey against the feeble twilight that remained. I could not but conclude that the clouds in both instances were the same or similar, lit up by the direct rays of the sun at the time of the first observation, and having lost his rays at the time of the second observation. Had they been self-luminous they should have become brighter instead of darker as the twilight faded.

It has been suggested to me that the bright clouds seen at 10.45 p.m. may have owed their brightness, not to the sun's rays falling on them at the time, but to a temporary phosphorescence, the result of exposure to the sun's rays in the day-time, and that this temporary quality had died out in the interval between the two observations.

I think this explanation is unnecessary for the following reasons. In the first place, it is certain that if a cirrus cloud were present in the atmosphere at a sufficient height to catch the sun's rays at 10.45 p.m. of a midsummer day, it would appear as a bright object amidst the surrounding gloom. And, secondly, there can be nothing incredible in the presence of a cirrus cloud at that height, when the persistence of twilight proves the presence of atmospheric particles of some kind at a greater elevation still.

GEORGE F. BURDER.

Clifton, December 19, 1889.

Duchayla's Proof.

I HAVE read with much interest the new proof given by Mr. W. E. Johnson of "the parallelogram of forces," in NATURE of December 19 (p. 153), and regard it as deserving a place among the best proofs that have been given.

I think, however, that, in his criticism of Duchayla's proof, Mr. Johnson runs to excess, when he says, "To base the fundamental principle of the equilibrium of a *particle* upon the transmissibility of force, and thus to introduce the conception of a *rigid body*, is certainly the reverse of logical procedure."

Duchayla's proof only requires us to suppose the transmission of force by *strings*. A particle is unthinkable. In presenting to a learner the conception of three equilibrating forces acting on a particle, we cannot do better than represent the forces by pulls in strings, and the particle itself by the knot where the three strings are tied together. All the steps of Duchayla's demonstration that the resultant force is directed along the diagonal of the parallelogram can be presented in tangible form with the aid of strings. I do not think this is an illogical or unnatural procedure.

J. D. EVERETT.

Belfast, December 23, 1889.

The Satellite of Algol.

THE results of Vogel's photographs as to the satellite of Algol are of great interest to your astronomical readers. The observations made at Greenwich tended to the same result, but were unfortunately intermitted before anything approaching certainty was arrived at.

Regarding it as certain that the variations of Algol are due to the interposition of a satellite, the question of the slight change