by the state. Once more however we admit that the scope and limits of his work have imposed upon him almost insuperable difficulties.

Introductory Lessons in Quantitative Analysis. By John Mills and Barker North. (London: Chapman and Hall, 1889.)

THIS book of eighty-five pages is the first part of a larger work by the same authors, which will shortly be published. It is designed mainly for the use of "students in evening classes who have but little time to spare in acquiring such knowledge," and also to be of service for the Science and Art Department examination, as well as those of London University. The descriptions contained in the three chapters constituting the book, and which treat of preliminary operations, gravimetric analysis, and volumetric analysis, respectively, are meagre in the extreme, and lack many details essential to a primer. Slips and loose statements are numerous. For example, the student is led to infer that the ash of *any* of Schleicher and Schüll's filter-papers is negligible. Lead is estimated by means of "bichromate of potash," which is formulated as K_2CrO_4 . On p. 62 the authors assert that "Normal solutions of univalent substances like iodine, silver nitrate, sodium chloride, &c., contain *their molecular weight* in grams in one litre." Whatever be the meaning attached to this, it is in no way confirmed by what follows on p. 63—namely, that "The *atomic* weight of iodine being 126.5, a normal solution would contain *this* number of grams in one litre."

The general scheme of work set out in the lessons is satisfactory, and if carefully elaborated might be useful. In its present condition, however, the effect of the book on the beginner cannot be other than confusing.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Note on a Probable Nervous Affection Observed in an Insect.

WHILST walking in the garden one bright September morning, my attention was called to a moth fluttering in a peculiar manner on the ground; it kept going round and round in a circle, running with its feet on the stones, its wings meanwhile being in rapid motion.

I captured the insect, which proved to be a quite fresh specimen of a male *Orgyia antiqua* (vapourer moth), of which there were many in the garden.

I replaced the insect without injury on the path, and watched it more closely.

The movements of the wings were irregular, convulsive, and very rapid in character; the feet and body were also in rapid movement, resulting in a circular motion of the whole insect from right to left—that is, in the same direction as the movements of the hands of a watch.

I again captured the insect, thinking that perhaps one of its antennæ might have been injured; but on careful examination with a hand lens, I could detect no lesion nor the presence of any parasite which might account for the condition.

I again placed the insect on the path, when it immediately began to rotate as before. It seemed unable to keep still, though evidently trying to do so.

Occasionally it would wedge itself in between two or more small stones, with its head downwards, and the under surface of its body upwards, its wings resting on the stones below; in this position it appeared to obtain some relief, as the movements were less continuous, though every breath of wind caused a

convulsive twitching of the wings and body. On one occasion a leaf fell upon the insect whilst wedged in, causing a very violent convulsion of the whole insect, by which it was jerked quite out of its retreat, when the gyrating movements at once began again.

I tried stroking the antennæ with the point of a pencil, but this had no effect, nor could I obtain cessation of movement by stroking the body or the wings; on the contrary, when the insect was wedged in each touch caused a convulsion, varying with the intensity of the stimulus applied.

These movements continued without interruption for fully forty minutes, the insect gyrating in a space about a foot square. At the end of that time I placed it upon a piece of smooth paper, when the movements became more rapid and the gyrations less ample, it completing a turn in much less time than on the stones, owing, no doubt, to there being no projections on the paper to cause the insect to deviate.

I then placed it in a shallow cardboard box in the full sunlight, but protected from the wind. In this way the convulsive movements were less intense and less frequent; the insect, however, was often jerked over on to its back, then, after a struggle or two, would right itself, and begin to go round. When, however, it managed to press the top of its head against the side of the box, so that its antennæ were pressed between the head and the side of the box, all movement ceased till some external stimulus again set it in motion.

At the end of one hour the insect seemed quite exhausted, a strong stimulation being required to develop one convulsion.

On examination I found that it had worn away, in its movements, all its legs with the exception of the left hind leg, which was apparently pretty intact, and had broken both its wings on the right side, so that the greater part of them hung useless over its body.

After a few more violent convulsions, the upper wing of the right side was broken off, and the insect now began to revolve from left to right, owing, I suppose, to the movements of the left leg; the others being reduced to mere stumps would have little power of propelling the insect.

About twenty minutes later, during a convulsion, the right hind wing was broken off.

Shortly afterwards I noticed that the convulsive movements of the antennæ, which had been slight up to that time, were much increased; indeed, they were moving so rapidly as to have the appearance of two small black wings.

One hour and fifty-five minutes after I first noticed the insect all convulsions had ceased; no stimulus could excite any; the moth was dead.

Conclusion.—The insect, suffering from no apparent injury, and being attacked by no internal or external parasite, was, I believe, suffering from some nervous lesion. I was unfortunately unable to examine the insect microscopically to ascertain if the nervous centres exhibited any pathological characters.

E. W. CARLIER.

Does the Bulk of Ocean Water increase?

THE idea was, I think, suggested by myself, and has been referred to with approval by Mr. Jukes-Browne, that much of the water on the surface of the globe was originally occluded in the molten interior, and has been emitted by volcanic action in the course of ages. Mr. Mellard Reade argues against this, that the moon is covered with volcanic craters, and yet has no water on its surface, and that if the accumulation of surface water has followed volcanic action on the earth, it ought likewise to have done so on the moon. He concludes:—"At all events, it seems a reasonable question to ask why oceans should be supplied with water from the perspiring pores of mother earth, while her offspring, the moon, is so dry as to have absorbed into herself all evidence of any aqueous envelope that may have formerly existed."

It is a singular coincidence that one possible answer to this objection is suggested by a notice in the "Astronomical Column" of the same number of NATURE which contains Mr. Reade's letter. Therein Prof. Thury attributes apparent changes in the aspect of a lunar crater to the melting of snow or ice around it. Neither is he the only selenologist who thinks that those crater-rings consist more or less of frozen water. If they do so, then there *is* water on the moon, although in a solid state. On the other hand, Proctor, in his work on the moon, says that her

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