3. Secondary group (mesozoic).
   Triasssic system, *violet.*
   Jurassic " *blue* (lias, *dark blue*).
   Cretaceous " *green.*
4. Tertiary group (cenozoic), *yellow*, using lighter shades as the beds become more recent.
5. Quaternary deposits. Decision referred to the committee of the map of Europe.
6. Resolutions of detail relative to shades, reserves, etchings, and letter notations.

III. Rules concerning the nomenclature of species.

1. The nomenclature adopted is that in which each animal and plant is designated by a generic name and a specific name.

2. Each one of these names is composed of a single Latin or Latinized word, written according to the rules of Latin orthography.

3. Each species may present a certain number of modifications, related to each other in time or in space, and designated respectively under the name of *mutations* or of *varieties.* The modifications whose origin is doubtful are simply called *forms.* The modifications will be indicated, when requisite, by a third term, preceded, according to the case, by the words *variety, mutation,* or *form,* or the corresponding abbreviations.

4. The specific name should always be precisely designated by the indication of the name of the author who established it. This author's name is to be placed in parentheses when the primitive generic name is not preserved; and in this case it is useful to add the name of the author who changed the generic name. The same disposition is applicable to varieties elevated to the rank of species.

5. The name attributed to each genus and to each species is that under which it has been primarily designated, provided the characters of the genus and the species have been published and clearly defined.

Priority will not be carried beyond Linné's Systema naturae, 12th edition, 1766.

6. In future, for specific names, priority will be irrevocably acquired only when the species shall have been not only described, but figured.

LETTERS TO THE EDITOR.

A powerful direct vision spectroscope.

AT a journal meeting in which Professor Rowland and the students of physics take part, an article came up for discussion which needs correction. In *Comptes rendus*, April 9, 1883, Ch. V. Zenger, in a note entitled '*Spectroscope à vision direct très puissant,*' claims a dispersive power equal to that of thirteen sulphide-of-carbon prisms of 60° angle for a spectroscope composed of a parallelopiped of two prisms, — one of quartz, and the other of a mixture of ethyl cinnamate and benzine, — combined with a third prism of crown glass of angle of refraction 27° 13′. He gives as the angles the three rays make with the perpendicular to the last prism after they have passed through, —

A	−90° 0′
D	−55° 15′
H	+42° 55′

It will be easily seen that *H* should be negative in place of positive; which will make the dispersion between *A* and *H* 47° 5′, in place of 132° 55′ which the writer gives. H. R. GOODNOW.

Johns Hopkins university.

Connecticut minerals.

The towns of Middletown, Portland, Haddam, and Chatham, in this state, have long been famed as a region remarkable for the number of minerals occurring in the veins of coarse granite. Within the last few days two minerals have been discovered in these veins, which, so far as I am aware, have not previously been reported.

Torbernite has been found at Andrus' Quarry, near the boundary between Portland and Glastenbury, associated with autunite, the occurrence of which has been previously reported.

Rhodonite has been found at the White Rocks in Middletown. WM. NORTH RICE.

Wesleyan university, Middletown, Conn.
June 9, 1883.

Book reviews.

I wish to quarrel a little with the critic of Gage's 'Elements of physics' in your issue of June 8, p. 517, for not keeping the following promise, found in the 'Prospectus of SCIENCE for 1883:' "To promote one of its chief objects, and as a distinctive feature of the journal, SCIENCE will give its hearty support to those who are endeavoring to introduce the study of the natural and physical sciences into public and private schools, by drawing attention in every possible way to the high importance of this measure, as well as by giving illustrated articles, plainly worded, prepared by skilful hands, to guide the efforts of the teachers." He has failed to keep this promise by failing to give such information about the book he reviews as "those who are endeavoring to introduce the study of physical science into public and private schools" would like to have. Many teachers cannot afford to buy every text-book they see advertised, and therefore must needs trust to reviews to tell them enough of a book to enable them to decide whether it is worth purchasing. In regard to a work on physics, they wish some such questions as the following answered: —

1. What is the plan of the book? Does the author expect the pupils to do experimental work, or that the teacher only will perform experiments? 2. If the author wrote with the view of having experiments performed by the pupils, how well has he succeeded in executing his plan? Has he succeeded in giving such experiments as will be of real service in laying the foundation of scientific work, and as can be performed in the short time that teachers in high schools and academies have for such work? Could pupils manage the experiments without the aid of a teacher? 3. Does the author give any directions in regard to preparing apparatus? If so, are these directions sufficiently exact and minute to enable an inexperienced person to follow them without trouble?

All of these questions a teacher would like to find answered in the review of a new book on physics. All the information he would get on these points from the review of Gage's book is found in this sentence: "The book is of merit as giving many experiments with apparatus of easy make." The reviewer said more than this, of course; but this one sentence is all to answer such questions as I have asked above. He was probably right in what he did say, which makes it the more to be regretted that he did not go farther. My quarrel with him is, that he did not say enough; that he did not say as much as your readers had a right to expect, — certainly not enough for those readers who had not seen the book, and wished to know whether it was worth buying. This suggests a question. Are reviews written for the benefit of those that have made the acquaintance of a book, or for those that have not? For myself, I can answer that I care most for the reviews of those books that I have not seen. In conclusion, I wish to say that Mr. Gage is a stranger to me, and I have never had any sort of communication with him. Whatever one might say in his behalf, my remarks were not made for his benefit, but to point out what I believe to be one of the first duties of the reviewer of a scientific book to his readers. S. T. M.

Lexington, Va., June 13.

[The limited space at our command will not allow of extended analyses of the many text-books of science which are continually appearing. A short notice either of their general merit or demerit is all we can give. In the case of Gage's 'Elements of physics,' the reviewer used the book as a text to preach against the common custom of teachers in using the atomic theory in their explanations as if we knew definitely that atoms exist.]

Solar constant.

Prof. C. A. Young has kindly called my attention to an unintentional oversight in my article entitled 'Solar constant' (SCIENCE, p. 542). In the general equation sent me by him, t represents 'degrees of heat,' not 'quantity of heat;' and m represents 'time,' not 'unit of time.' H. A. HAZEN.

A zoo-philological problem.

On the New-England coast, where Mya arenaria is abundant, and known as the 'clam,' an annelid which is common in the same localities is called the 'he-clam,' and is believed by many fishermen to be the male of the mollusk.

In Norway, Mya arenaria is abundant in the fiords of the north. It has no economic uses; but its associate, an annelid, the 'pür' (said to be Arenicola piscatorum), is an important bait, and gives its name to the Mya which is called the 'pürschaal.'

Why should the common annelid and the common mollusk be thus associated in popular nomenclature in remote regions? It is interesting to observe that the form possessing commercial value in each instance gives its name to the one which is in lower esteem. G. BROWN GOODE.

The sun's radiation and geological climate.

In my objecting (SCIENCE, p. 395) to the assumption that the dissipation of solar energy from loss of heat diminishes the supply of sun-heat received by the earth, I said, that, so far as there has been any change in the supply, it has been in the direction of an increase, and hence cannot explain the undoubted decrease in the temperature of the earth's atmosphere. I think Professor Le Conte's criticism (SCIENCE, p. 543), taken in its entirety, corroborates my position. He shows that the quantity of heat incident normally on a unit of surface in a unit of time varies as the area of a great circle of the sun × heat-emitting power of each physical point of the sun: hence the quantity emitted would not increase, unless the heat-emitting power increased faster than the square of the temperature. He adds that "some physicists (Rossetti) make the latter proportional to the square of the absolute temperature, while others (Stephan) make it as high as the fourth power." If Rossetti is right, there has been no decrease in the amount of solar heat received; while, if Stephan is right, there has been a very great increase: for, on the assumption that the temperature is inversely as the radius, as stated in Professor Newcomb's article (Popular astronomy, p. 508), the heat-emitting power, if the solar radius is reduced to one-half, will be increased four times, and will just compensate for the great circle being reduced four times in area. If the emissive power increases, as Stephan claims, then a doubled temperature will increase it sixteen times, and, the area being diminished only to one-fourth, the earth will receive quadruple the heat.

It is true that the heat-emitting power of any (solid) body varies according to the area of its surface, providing all the other conditions are unchanged. In case of solids and liquids, very little change can be made in their density by any force that we can apply, — so little, indeed, that no appreciable effect can be produced; but gases are easily affected, and there is no difficulty in conceiving them reduced many times in bulk. Now, suppose two spheres, e.g., of hydrogen, of equal masses and of the same temperature, but one having twice the radius of the other. They will radiate equal amounts in equal times, as I shall try to show. I assume that the radiation goes on only from points of matter, — the atoms of the hydrogen. Conceive each sphere made up of a vast number of concentric layers, each one molecule thick. The number of layers will be the same, and the number of molecules in each will also be the same: consequently the heat-emission of the outside layer will be the same in both spheres. What would be true of the first layer would be true of all, unless the outer one intercepts some of the rays. So far as the outer layer is gaseous and elementary (it is very doubtful whether any chemical compounds can exist in the intense heat of the sun), it is a vacuum to radiant heat; for Professor Tyndall, in 'Heat considered as a mode of motion,' has shown (p. 362) this in reference to oxygen, hydrogen, nitrogen, and air, and, in general (see rest of the lecture), that elementary gases or vapors produce little or no effect upon the radiant heat that passes through them. It must be remembered, too, that the source of heat employed in his experiments was icy-cold in comparison with the sun, and that the penetrating power of heat-rays increases as the temperature of their source rises. It is therefore probable that the heat from the lower layers passes through the upper ones, so far as they are gaseous, with little or no loss, and hence that in gaseous bodies the heat-emitting power for any given temperature is proportional, not to the surface, but to the mass or density.

But suppose that diffused through the upper layers were molecules that were capable of stopping every ray that impinged upon them. Neither the absolute number nor the size of these bodies would be affected by shortening the radius, but only the space between them. If the radius were reduced to one-half, the apertures would be reduced in area to one-fourth, while the radiating molecules within any given distance would be increased eightfold: in other words, the chances of not passing out into space would be increased only four times, while the number of shots would be increased eight times; so that, in this case, the heat-emissive power would be actually increased by the condensation. If to this be added an increase of the same power from the rise of temperature (either as the square or the fourth power, *Rosetti* or *Stephan*), there can, I think, be no doubt that any change which has occurred in the earth's temperature from the sun's losing energy has not been in the direction of growing cooler.

As a corollary of the above, I add, the radiant or heat-emitting power of a sphere of gas appears to be a function of mass and temperature, and not of surface and temperature.