

smaller pieces, evidently broken from this, and somewhat worn. This rock presents all of the characteristics and all of the grades of the nodules found in the marl conglomerate,—the same shells, same large amount of sand, and the same appearance. The character of the rock changes gradually here. Between Warsaw and Kenansville it is richest, yielding forty to fifty per cent phosphate, while both east and west it grows more sandy. Between Sampson on the west and Jones on the east we find all the grades of rock which were found in a single place in the conglomerate beds of the lower country. We conclude, therefore, that this conglomerate was formed from extensive breaking up and mingling of beds similar to those seen at the present time in Sampson, Duplin, and Jones counties, and not from stray coprolites, as has been supposed.

Whether this field will yield any phosphate of more than local value depends upon conditions yet to be determined.

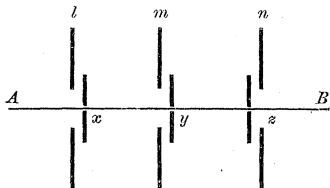
CHAS. W. DABNEY, Jun.

N. C. experiment-station, Jan. 2.

Radiant heat.

While it appears that Mr. Fitzgerald's criticism upon Dr. Eddy's hypothesis is conclusive, yet the latter makes a statement in your issue of Dec. 21 which is misleading, since it implies that the device will produce the desired result. Dr. Eddy says,—

"Thus the fact remains, that, although a definite amount of heat from *B* remains entangled in the region *m n*, which is not increased with the lapse of time, there is a continued passage of heat through this region into *B*, that being the very object sought to be accomplished by my process."



Now, the fact is, there cannot be 'a continued passage of heat through this region into *B*,' without permitting the passage of heat from *B* to *A*, by any of the processes described. Granting that heat is entrapped in the space *m n*, it will escape into the space *l m* whenever the door *y* is opened for the passage of heat from *A* into the space *m n*; and the heat so entrapped in the space *l m* will pass on to *A* whenever *x* is opened to admit heat from *A*. This is so plain, that it is only necessary to call attention to the fact, to have it admitted. If the only object sought, as stated in the above extract, was to permit the passage of heat from *A* to *B*, it could be secured at once without any device between *A* and *B*. As originally stated, the object was to transfer more heat from *A*, the colder body, to *B*, the hotter one, than was passed in the opposite direction. The writer has shown in another place¹ that Dr. Eddy's system of moving screens fails to accomplish this result.

DE VOLSON WOOD.

Limits of tertiary in Alabama.

The announcement in *Science* (ii. 777) of Professor Johnson's extension of the border-line of the tertiary in Alabama to a position ten miles north of

Allenton, and six north of Camden, recalls similar observations made by Alexander Winchell in 1853, and published in *Proc. Amer. assoc. adv. sc.* for 1856, pp. 88, 89. These sub-Claiborne beds he designated 'buff sand,' and the overlying ledge of calcareous grit was traced by him "eight and a half miles north of Allenton, which" was "twenty-five miles farther north than the tertiary beds had been hitherto recognized in this part of the state." The undescribed fossils collected were left with Professor Tuomey, who pronounced them eocene, and held them for description till his death in 1857. A few years later the vicissitudes of war involved the destruction of the Tuscaloosa cabinet by fire. Mr. Winchell's observations were communicated orally in December, 1853, to Professor Tuomey, who noted them down on a manuscript map, from which was compiled the map published in 1858 in Tuomey's (posthumous) second report, edited by Mallet. This places the boundary of the eocene a mile north of Allenton, which, as shown above, is not so far north as Winchell traced the formation. There is, however, nothing in the text of the report on which any change in the older map of this region could be based. Professor Tuomey's observations had been directed to other parts of the state; and Mr. Thornton, his assistant, reports tracing this line through Monroe county, while the map shows it located nine or ten miles north of that county, and, if fully conformed to information in Professor Tuomey's possession, would have shown it seventeen and a half miles north. These statements are only important on the principle of *sum cuique*.

A. W.

Italics for scientific names.

The scientific name of every described plant and animal consists of two or more words: namely, that of the genus, used as a substantive; and the specific name, which follows, and is an adjective adjunct. A species may have a dozen or a hundred common or vulgar names, in half as many languages; but there is only one name in the dead, unchanging, scientific nomenclature. It seems to me that the importance of scientific names, over all others, makes them deserving of a more emphatic type than that of the general text. In the ordinary print—as that of this page of *Science*—any scientific name should be given in italics. Take, for example, the American larch, tamarack, or hackmatack. This tree of our swamps may have many local names, but it has only one in science the whole world over. The emphasis of this fact is largely lost if it is written without an underscore, or printed thus, *Larix Americana*. It would be only a short step farther to have it *larix americana*.

It does not follow that names of groups need to be italicized. Thus we can have the order *Liliaceae*, which contains the genus *Lilium* with its Canada lily (*Lilium Canadense*), the golden-banded lily of Japan (*L. auratum*), and *L. candidum*, or the common white lily. *Quercus*, *Pinus*, *Prunus*, *Ranunculus*, and the thousands of other genera of plants and animals, when used alone, may be set in the common type of the page, and stand thus,—*quercus*, *pinus*, *prunus*, and *ranunculus*; but I do not like it. Many of the generic names are derived from proper names, as *Linnaea*, *Magnolia*, *Tournefortia*, *Begonia*, etc.; and these certainly should begin with capitals. When, however, the name of any genus is the common name of all the plants in that genus, it is reasonable to use it without a capital, when employed in a general way. We may say of a plant, it is a fine *begonia*, or a stately *magnolia*, or a delicate *linnaea*, and the absence of

¹ *American engineer*, Chicago, 1883, Jan. 12, Feb. 9, 23, and April 6; also *Journ. Frankl. inst.*, May, 1883, 347.

capital letters is well enough, even though the names have been derived from proper names; but, if we say it is a choice specimen of *Begonia Rex*, the case is different. The word 'begonia' now becomes a part of the scientific name of a species of plant. In the same manner the stately magnolia may be *Magnolia glauca* or *M. grandiflora*.

Science does not use emphatic type for the scientific names of genera or species, and doubtless for good reasons. I should like to learn what views the editor and other authorities in scientific nomenclature hold on the above subject. BYRON D. HALSTED.

New York, Dec. 31, 1883.

[We do not agree with our correspondent in his estimate of the value of the scientific names of plants and animals. They are a simple convenience, and have no higher value; and the use of italics for their proper mission—that of emphasis, or as catch-words—is lost if the page bristles with italics having other meaning.]

The skidor in the United States.

In *Science*, No. 44, mention is made, in Norden-skiöld's account of the Greenland inland ice, of the 'skidor,' or Norwegian snow-shoe. It may be interesting to your readers to know that it is the snow-shoe most commonly used in Colorado. It is much preferred to the Canadian or web snow-shoe, and in the mountains in winter is often the only means of getting about from place to place—as from the mines on the mountains to the towns, and from one small mining town to another—when there is not enough travel to keep a road open through the deep snow. I know of one case in which a daily mail is carried twenty-five miles on snow-shoes; two men having the route, each making a single trip in a day, but going in opposite directions. The motion can hardly be called 'running,' as it is in the footnote on p. 737, as the shoes are not lifted from the surface of the snow at all, but slid forward at each step, the foot being raised slightly at the heel as in commencing a step in ordinary walking. The shoes that I have seen are from six to eight feet long, and about four inches wide. A pole about seven feet long is used as a guide and support, especially in sliding down hill, when a tremendous pace is often attained on a long slope. E. R. WARREN.

Colorado Springs, Jan. 1.

Standard thermometers.

In your editorial in this week's *Science* you quote the report of the chief signal-officer of the army, implying that a sensible difference exists between the theoretical standard thermometer adopted by this observatory and that of the International committee of weights and measures, and that the signal-service of the army has adopted a new standard thermometer more nearly agreeing with the latter.

I should be very greatly obliged to the chief signal-officer if he will anticipate the regular course of publication of the scientific work of his office, and give to the scientific public the results, at least, of the work from which it is concluded that the signal-service of the army has reached a nearer approximation to the standard thermometer of the International committee.

I have no doubt that there is a small difference between the standard air thermometer and the particular mercurial standard adopted by this observatory as its practical representative, at points distant from the freezing and boiling points; but, as our own stand-

ard has never been compared with any air standard in the possession of the signal-service of the army, I shall be quite interested to see the work by which it is concluded that there exists a sensible difference between the two. LEONARD WALDO.

Dec. 31, 1883.

Romalea microptera.

In 1879, in Alabama, I had many opportunities for observing the habits of the 'lubber grasshopper;' and, if my memory serves me, my observation showed that the hissing referred to by Capt. Shufeldt (*Science*, ii., 813) is due in large part to the forcible expulsion of air from the thoracic spiracles. It was always noticed on the occasions referred to by him, but at no other time. W. T.

Synchronism of geological formations.

I cannot agree with Professor Heilprin in the line of argument adopted in his letter to *Science* of Dec. 21, based, as it mainly is, on the assumed non-occurrence of 'evidence of inversion.' Professor Heilprin asks, "Why has it just so happened that a fauna characteristic of a given period has *invariably* succeeded one which, when the two are in superposition all over the world (so far as we are aware), indicates precedence in creation or origination, and *never* one that can be shown to be of a later birth?"

In reply I would say, that some years previous to Professor Huxley's address on this subject, Barande, in his 'Système Silurien de la Bohême,' had shown such evidences of inversion to exist in the Silurian formation of Bohemia; and though many geologists and paleontologists disagreed with Barande at that time, as to his theory of 'colonies' by which to account for the facts, yet none could dispute the facts cited by him. If we now turn to the old red sandstone of Scotland, we find still further evidences of inversion of like kind; for, while the crustacean genus *Pterygotus*, common to both the upper Silurian and lower old red sandstone, has been recently found also high up in the middle series of this formation, the carboniferous limestone shells, *Productus giganteus*, *P. punctatus*, *Spirifer lineatus*, and others, have been found in the old red sandstone far below the fish genera *Pterichthys* and *Holoptychius*, so characteristic of the upper old red division. Though there appears to be no reason why such instances of inversion should not have occurred over and over again, one can readily understand why, through the imperfection of the geological record, and the comparatively small fraction of the earth's surface which has been systematically examined, their occurrence is almost unknown.

With reference to the doctrine of migration, I judge, that, from Professor Heilprin's argument, we look at the matter from two different stand-points. He apparently takes no account of the generally accepted view of biologists, that, while organic development has been closely similar in all parts of the world, the rate at which it proceeded has varied within the widest limits, even in adjacent regions. I cannot help looking on the various formations as the records of that development; and, judging of the past distribution of life on the earth from what we at present see before us, I am forced to believe that identity of organic contents in widely separated strata, instead of being evidence of chronological contemporaneity, is exactly the reverse.

Instead of encroaching further on your valuable space, I would refer to Prof. A. Geikie, who, in the current issue of the *Encyclopaedia Britannica* (9th