DISCUSSION

AMERICAN SCIENTIFIC ORGANIZATIONS CALL FOR STABILITY OF RULES OF ZOOLOGICAL NOMENCLATURE

STABILITY of the rules of zoological nomenclature is becoming increasingly important. The action of the International Zoological Congress, at Padua, Italy, in 1930, in adopting the Horn resolution,[1] stimulated interest in this subject, and led to a meeting in Washington, D. C., of members of the nomenclature committees of several local, national and international American societies interested in zoology.

At this meeting on May 8, 1931, the following resolution was approved:

> We, the undersigned, members of the committees on nomenclature of the various scientific societies listed below, view with alarm the action taken on the Horn Resolution at the International Zoological Congress held in Padua, Italy, in 1930, and consider that this action establishes a precedent which seriously jeopardizes the stability of zoological nomenclature. The adoption of the Horn Resolution by the Congress was contrary to the 1901 agreement, which provided that proposals regarding the international rules of zoological nomenclature would not be submitted to the Congress without the unanimous recommendation of the International Commission on Zoological Nomenclature. We believe that the passage of the Horn Resolution was unparliamentary, contrary to the methods of procedure approved by the International Commission on Zoological Nomenclature, and, consequently, invalid. We, therefore, reaffirm our adherence to the International Rules of Zoological Nomenclature as constituted under the 1901 agreement.

The foregoing resolution has been formally adopted by the following ten organizations:

American Society of Zoologists,
Section F, American Association for the Advancement of Science,
American Society of Mammalogists,
American Society of Parasitologists,
American Society of Ichthyologists and Herpetologists,
American Malacological Union,
Biological Society of Washington,
Entomological Society of Washington,
Helminthological Society of Washington,
Geological Society of Washington.

This matter is here brought to the attention of American zoologists to acquaint them with the action taken. If this position be endorsed by additional organizations (academies, faculties, societies, etc.), it is requested that copies of their resolutions of endorsement be sent to the undersigned.

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[1] *Cf.* Stiles, SCIENCE, n. s., lxxiii, 1892, pp. 351–352, April 3, 1931.

BACTERIA IN PENNSYLVANIA ANTHRACITE

SEVERAL times during the last few years notes have appeared in scientific journals and newspapers calling attention to the discovery by Dr. C. B. Lipman of living bacteria in Pennsylvania anthracite. According to Lipman[1] these bacteria are "descendants directly from cells which have lain dormant there from the time of the coal's formation, which, according to one method of the geologist's reckoning, would be fifteen million years, and according to another method, from one to two hundred million years."

In accordance with commonly accepted theories, anthracite was formed from coals of lower rank through the action of dynamic metamorphism or by contact with igneous rocks. A microscopical examination of Pennsylvania anthracite[2] shows that the most resistant plant substances, such as waxes, gums, and resins, have been changed to anthracite. Furthermore, the original argillaceous sediments associated with the vegetation have been changed to shale and slate-like rocks containing micas and other metamorphic minerals. It seems unreasonable, therefore, to expect a relatively perishable substance like the protoplasm of bacteria or the spores thereof to be exempt from the changes which have taken place in their vastly more resistant associates.

Mr. M. A. Farrell and the writer made a careful study of Pennsylvania anthracite taken from the same bed and same mine from which Dr. Lipman secured his samples and concluded that this anthracite contains no bacteria other than common living forms which have found ingress through fracture cracks and coal laminae communicating with surface water and air.[3]

The samples which we examined were collected by ourselves from the Primrose vein in the Otto colliery of the Philadelphia and Reading Coal and Iron Company at Pottsville, Pennsylvania. This location was selected because it was the same one from which Lipman's samples were secured. The fact that we collected our own samples and studied the condition of the coal in place, together with the structural nature of each sample, may have some bearing on the fact that our findings differ from those of Lipman. Apparently Lipman was not aware that his samples came from the side of a pitching syncline outcropping at the surface, and that the outcrop was badly breached, giving free access to surface waters. Lip-

[1] C. B. Lipman, "Living Microorganisms in Ancient Rocks," *Jour. of Bacteriology*, xxii, No. 3.

[2] H. G. Turner, "Constitution and Nature of Pennsylvania Anthracite with Comparison to Bituminous Coal," *Trans. A. I. M. E.*, February, 1930.

[3] M. A. Farrell and H. G. Turner, "Bacteria in Anthracite Coal," *Jour. of Bacteriology*, xxiii, No. 2, February, 1932.

man states that his samples were collected from a place in the mine where there is no evidence whatever of percolating water in or near the point at which the samples were obtained. There may not have been percolating water present when his samples were taken, but that does not mean that there never could have been any there. In his paper Lipman gives 1,800 feet as the depth at which his samples were secured, whereas a check made by myself of the company's maps showed a vertical depth of less than 500 feet beneath the surface of the ground at this place.

Lipman, by making a study of permeability of anthracite, wisely attempts to offset any mistakes that could be made through failure to examine coal in place or in the laboratory. His permeability study, however, is not at all convincing. He selects two pieces of coal, one of which he treats as he did previous pieces, except that the period of heating in a hot-air oven is much longer than that applied to other pieces of coal in which he found bacteria. This particular piece of coal gave negative results. It is difficult to see why it produced negative results, for there was no reason to suspect that this piece of anthracite did not contain the original ancient bacteria. Of course the longer period of heating may have destroyed them, but this could hardly be the case, for Lipman says in another part of his paper that "it seems as if the longer periods of heating cause the organisms to grow more effectively." Regardless of results, I fail to see the value of this test except in confirming my belief that there are no ancient bacteria in anthracite.

The second piece of coal selected for the permeability tests was treated in the same manner as the first up to the point where it was submerged in a suspension of a pure culture of the coccus derived from the coal previously found to contain bacteria. From this point on the procedure was different. In this case "a few colonies all told, perhaps not more than eight or ten, were found," and Lipman concludes from this that "if the coccus in which the coal sample was submerged had penetrated to any extent at all into the coal each culture made from the crushed sample would have shown heavy growth." The fact that he found some bacteria shows, in my opinion, that either they penetrated the coal or represent original bacteria. If they represent original bacteria some similar growth should have been found in the previous piece which actually gave negative results. One must conclude, therefore, that the control piece either was not treated properly or that the few bacteria found actually penetrated the coal.

Entirely aside from the results obtained in these permeability tests it should be noted that the bacteria in which the coal was suspended were in the vegetative form, which presented much larger particle sizes than the "visible or invisible spores" which, according to Lipman, these organisms are capable of producing. Why can not an invisible spore penetrate an invisible crack or pore? Lipman's statement that "particles as big as a coccus are too large to penetrate the coal, either through crevices or microscopic pores," is also meaningless because he fails to give the dimensions of the cocci, pores or crevices in question.

It is difficult to see how reliable conclusions regarding the presence of ancient bacteria in anthracite can be reached without a thorough study of the history, structure and texture of the coal both in the field and in the laboratory. Long before the shaft is sunk the coal has been subjected to possible contamination through circulating ground-waters. As the shaft is sunk, impure water and air advance with it. Practically every mine is equipped with pumps to keep the water low enough to permit working, and even then flooding is not uncommon. The possibility of securing an uncontaminated piece of coal or rock from a mine is so remote that the whole problem resolves itself into a study of permeability. The student of coal petrography realizes that one piece of coal may be impervious, while another piece from the same bed may contain fractures or laminae which could easily be penetrated by large or small bacteria. A permeability study, then, would be of little value without a knowledge of the texture and structure of the sample used.

The reported finding of bacteria millions of years old is news and as such can do no harm. But, when it begins to appear as a fact, and is used to overthrow well-founded theories on the origin of coal, it is time to ask ourselves whether or not it is true. My object in writing this discussion is to check wild theories and speculations which are being advanced by other writers on the assumption that Lipman really found bacteria of great antiquity in anthracite.

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THE JURASSIC IN OKLAHOMA

WHILE doing field work in the preparation of a paper on the Pleistocene mammals of Oklahoma, the writer visited the valley of the Cimarron River in Cimarron County, Oklahoma, where he learned that some "big bones" had been uncovered along highway 64, just east of Kenton.

The "big bones" proved to be part of a dinosaur since identified as *Brontosaurus*.

The discovery of this specimen is significant in two respects. It is the first distinctly Jurassic dinosaur

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