arrangement can not be distinguished microscopically from the forms which were found to be capable of transmitting the disease. So far attempts to stabilize this coccoid form of *B. alvei* have been unsuccessful, its separation by replating having resulted either in a return to the original rod type or a failure to grow on the medium employed. These difficulties in connection with the stabilization of new forms have been already emphasized by Löhnis and Smith.[3] These authors have shown the possibility of stabilizing coccoid cells from *Azotobacter,* while Cunningham and Jenkins[4] have obtained a coccus from cultures of *B. amylobacter* (A. M. et. Bredemann). That a similar stabilization of a coccoid form of *B. alvei* is feasible, is reasonable to presume, and its accomplishment would lend support to the hypothesis regarding the identity of *B. alvei* with the organism responsible for the infection in foul-brood, and furnish interesting light regarding the relationship of developmental phases of pathogenic bacteria to virulence. With all work concerned with etiology and pleomorphism, however, too much emphasis can not be laid upon the necessity for repeated confirmation of results. The writer would have preferred to withhold even this preliminary communication which is given reluctantly in view of the outlined circumstances.

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## CONCERNING MAN'S ANTIQUITY AT FREDERICK, OKLAHOMA

AMONG the several recent reports of evidences of Pleistocene man in America, the case of Frederick, Oklahoma, must be received with caution. In 1926 the owner of a fossil-bearing gravel pit at this place unearthed several artifacts. The site was first examined and reported by J. D. Figgins, H. J. Cook and O. P. Hay,[1] and later by C. N. Gould, C. E. Decker and the writer.

The gravel pit has been sunk into a stratum of gravel and sand which caps a ridge a half mile wide and several miles in length. The stratum is from ten to twenty-five feet deep and lies on beds of Permian age. The ridge is the highest point for some miles around, the red beds falling away to the Red River. All observers are agreed that the gravel bed is of Pleistocene age; the presumption being that it was deposited in a valley bottom, subsequent erosion of the surrounding areas having left it in its present high position.

The artifacts are two arrowheads or blades and five possible metates or mealing slabs. According to Mr. Holloman, the owner, one blade was from the very bottom of the gravel, he having picked it up from loose material at the foot of the pit face as it was torn down by workmen. The second was at a somewhat higher level, four to eight feet. Mr. Holloman stated that he scratched this artifact from the face of the pit with his fingers. The slabs, identified as metates by Mr. Figgins, were taken from a level of a foot or two above the blades. All observers are agreed on the honesty of Mr. Holloman's representations.

Figgins, Cook and Hay concluded that the human artifacts are original constituents of this Pleistocene gravel bed as it was first laid down. Before this verdict becomes generally accepted, I should like to broach several problems.

No scientific man has seen the gravels in the immediate vicinity of the spot where the finds were made. These were all found within a short distance of one another, near the center of the gravel pit which now extends over several acres. We do not know the original position of the surface at this point with respect to the artifacts. The deposit is considerably eroded. There is the possibility that these artifacts lay on the surface of a depression, were subsequently covered by wash, and have only a specious claim to the antiquity of the near-by fossils.

As against this possibility is the fact that I was told that no artifacts have been found on the surface in the vicinity. On the other hand, it is quite possible that they may yet be found. The ridge presents the only body of suitable material for flaking blades for miles around and at the same time affords a wide view of the surrounding country.

The artifacts themselves are equivocal. The blades are clearly artifacts, resembling modern Indian forms, but the metates are questionable. It is difficult to decide from Mr. Figgins' illustrations whether the slabs were fabricated, and I have not seen the originals. They may be water-worn boulders. While at the time Mr. Figgins wrote that no other stones of a similar nature had been found, our party saw several slabs, clearly water-worn, which suggests the possibility that some of them had been selected as metates because of their close resemblance to such forms.

[3] Löhnis, F., and Smith, N. R. "Studies upon the lifecycles of the bacteria—Part II: Life history of Azotobacter." *Jour. Agr. Res.* 23, 401–432. 1923.

[4] Cunningham, A., and Jenkins, H. "Studies on *Bacillus amylobacter* A. M. et Bredemann." *Jour. Agr. Sci.* 17, 109–117. 1927.

[1] J. D. Figgins, "The Antiquity of Man in America" (Natural History, 27, 1927, 229–239). Harold J. Cook, "New Geological and Paleontological Evidence bearing on the Antiquity of Mankind in America" (*loc. cit.*, 240–247). Oliver P. Hay, "Early Man in America" (*Science News-Letter,* 12, 1927, 215–216).

More important than these conjectures, which are at best only possibilities, is the incongruity of the find with all we know of man's cultural history.

First with respect to the metates; among living peoples these are in use only by those who are cereal-raisers or who are in contact with them. For example, in North America grinding slabs are used only by the corn-raising Southwestern Indians and their seed-gathering neighbors of the Basin-Plateau region. Indeed the use of the metate may not be of remote date even in this area, and all Americanists are agreed that cereal-raising is not one of the original constituents of Indian culture. In the Old World also cultivation is a Neolithic art, that is, of geologically Recent provenience.

The blades are likewise of European Neolithic type, or at best of Solutrean technique (from the middle or close to the fourth glaciation).

Yet Dr. Hay has it that this deposit is of early Pleistocene age. The fossil animals include "a primitive elephant, a mastodon, two species of camels, two species of ground-sloths, a glyptodon and three or four species of horses, one very large, one pony-like. . . . As to the animals, I hold that they are characteristic of the first interglacial stage (the Aftonian) of the Pleistocene."

If Dr. Hay is right, and I have no reason to doubt his identification, we are confronted by an unusual situation. Artifacts which would be identified by an archeologist as Recent (or terminal Pleistocene) are held to be of the same age as an early Pleistocene fauna. This incongruity seems not to have occurred to Dr. Hay.

Is there any warrant to support this from what we know of the course of human events elsewhere? I think not. The earliest definitely human remains from the Old World (*H. heidelbergensis*) date from the second interglacial or the first. The Frederick deposit may antedate this. The earliest human artifacts (Chellean or Pre-Chellean) date from the middle or close of the third interglacial. These are quite roughly made in contrast to the well chipped Frederick blades. The zoological evidence conforms. Most authorities are agreed on man's anthropoid ancestry. The anthropoids are Old World forms; there are no known anthropoid prototypes of man in America.[2]

It seems to me that the onus of proof rests with those who hold that Neolithic implements are congruous with an Aftonian age. I am doubtful that the mass of cultural and zoological evidence to the contrary now available will be set at naught.

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[2] The case of *Hesperopithecus*, a single tooth of Tertiary age from Nebraska, seems disposed of by W. K. Gregory's recent determination of it as pertaining to an extinct peccary (SCIENCE, n.s., 66, 579–581).

## "EXIT THE TENTAMEN, BUT . . ." WHAT?

UNDER the above caption, minus the last word, my esteemed friend, Mr. Wm. T. M. Forbes, in the issue of SCIENCE for October 28, 1927, undertook to reply to an article from my pen published in the same journal, July 1, 1927, entitled "Exit Hübner's Tentamen." Mr. Forbes addresses numerous questions to me. At one point he says: "What would Dr. Holland do about it?" To all of his numerous queries I shall give appropriate answers elsewhere in a journal more strictly devoted to the technical nomenclature of entomology, and shall in that article show how greatly Mr. Forbes, and others, who hold with him, have misunderstood the writings of Hübner, and his tentative system of classification. There is only one point upon which I wish to touch in this brief paper.

Mr. Forbes at the end of his paper says: "In bringing in the *Verzeichniss*, Dr. Holland does not mention that ten years had intervened and that in the meantime Hübner had used all the Tentamen names of butterflies as generic (as the first names of binomials) also many of the moths. This fact completely invalidates his argument."

Passing by the implication that I was making an "argument" in a matter which in my judgment is not open to argument, and was simply stating obvious truths, this allegation of Mr. Forbes awoke my utter astonishment. I am familiar with every page and line which Hübner gave to the world. Mr. Forbes's statement seemed to me most amazing. Accordingly I wrote to him inquiring upon what he based his sweeping statement that from 1806 to 1816 Hübner had used "binomials" in his nomenclature of the butterflies. Mr. Forbes has kindly replied to my inquiry and informs me that he based his assertion upon the legends of the plates in Vol. I of the *Sammlung exotischer Schmetterlinge*. Mr. Forbes's answer still more amazes me. Any one, who takes the trouble to look at these plates from a corner of one eye, can instantly see that the legends are all *trinomial*, and not *binomial*, as Mr. Forbes says. Mr. Forbes is under an illusion. *Three* is *not* equal to *two*, as *twice four* is not equal to *five*. Hübner in the legends of these plates was consistently true to the "System" he had adopted. On these plates he gives 1, the name of the *Stirps;* 2, the name of the *familia;* 3, the name of the *Gattung* (species). Not once does he employ a generic name, either in his sense, or ours. Mr. Forbes is wholly in error.

As Mr. Forbes's premise is false, and contrary to facts, his conclusion is equally false. His "argument" involves the logical error of *petitio principii*. It is