

we find pieces of that sandstone close enough to have been stained by nickel-iron vapors, and liquefied by the friction of the mass itself, but still clearly recognizable as products of the Coconino sandstone. Had there been any appreciable amount of meteoric stone involved in the impact it would seem impossible that evidence of it should not have been found.

I have elsewhere<sup>1</sup> summed up the reasons for believing the impacting mass to have been a compact cluster of millions of small, rounded individuals, rather than a single mass of iron (or stone) or a single large mass accompanied by a few satellites. This conception of the body coincides with the accepted belief as to the nature of comets. One of the reasons for this conception is that most, if not all, of the Canyon Diablo irons, when in their original condition (*i.e.*, when not acted upon by terrestrial erosion) are of a rounded or oval outline. This is explained by the long-continued attrition between individuals of the clusters, attrition that may have been very slow, but that had millions of years in which to accomplish its results. If, then, the cluster had contained at its inception any appreciable number of stony individuals, they would have been subjected to exactly the same process as that which takes place in a ball mill; that is, they would have been chipped and shattered to total disintegration long before the iron members had been worn away. The dust they became would have been blown away from the comet by the pressure of light if the comet had ever come near the sun; if not, it would at least have been filtered out of the swarm at the first touch of the earth's atmosphere.

Professor Fairchild mentions the pitting of the typical Canyon Diablo irons, ascribing it (and I think rightly) to the removal of some enclosing matrix from around the unoxidizable iron. But this matrix he believes, from no evidence that I know of, to have been stone. There is strong evidence, on the other hand, that the matrix was not stone but the oxidizable variety of nickel-iron, for a good many of the pittings are partially filled with iron oxides, firm in texture and adhering closely to the iron. Also, as I have mentioned above, some of the oxidizable but metallic individuals preserved in the rock flour show unoxidizable nuclei. Here is clear evidence that the matrix which originally enclosed the Canyon Diablo irons was chlorine-bearing iron. There is no evidence to indicate it to have been stone.

The rounded shape of the original irons (for all the fragments found preserved in the rock flour were rounded) also argues against their having been inclusions in a large mass of stone. Many stony meteorites exhibiting iron inclusions are known, but those inclusions show no evidence of rounding, being on the

contrary of irregular, angular shapes, filling spaces between crystals or chondrules of the enclosing matrix, or ramifying through the rock as irregular veinlets. Why should a large hypothetical siderolite exhibit such a totally different structure from the known small ones?

One stony meteorite was found at the crater, or rather at a distance of a mile or so from the rim. This is mentioned in my father's paper<sup>2</sup> of 1909, and part of it is now in the Meteor Crater collection in the Guyot Museum at Princeton. It was distinctly an individual piece, hardly to be thought of as a chip from a larger mass, and had markedly rounded outlines. As is pointed out in my father's paper, there is strong reason for believing that this was a separate and later fall than the Meteor Crater swarm.

Interesting as Professor Fairchild's conception of the Barringer meteorite is, he has presented no new evidence in support of that conception, and his conclusions from the old evidence do not warrant, to my mind, a change from the more accepted picture of the comet. My father's visualization of the celestial intruder, as a cluster of small rounded iron meteorites, containing in all probability no stony members or parts, still has all the evidence in its favor. But, though we differ from Professor Fairchild in this particular conclusion, I am deeply sensible of his long and helpful interest in the question, and of his frequent and sturdy assistance in the problems connected with it.

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#### CONCERNING THE RATE OF FORMATION OF STALACTITES

DURING the past summer I visited old Fort Pickens, on the west end of Santa Rosa Island, opposite Pensacola, Florida. In prowling around one of the dismantled structures, I came upon a room the ceiling of which held a number of stalactites. Considerable stalagmitic material also covered the floor. This unusual occurrence of deposits aroused my curiosity, as I thought they might throw some light on the rate of deposition of certain cave deposits.

The room where the stalactites were found was made of brick, laid in lime mortar. Both the walls and the roof were four or five feet thick. The roof was somewhat overgrown with vegetation growing from loose earthy material covering the brick. There were ample openings in the walls for a free circulation of air, yet not situated so as to allow violent winds to strike the interior. The conditions seemed quite similar to those of a limestone cave, as far as the formation of stalactites was concerned.

<sup>2</sup> "Meteor Crater," by D. M. Barringer, read before the National Academy of Sciences, November 16, 1909.

<sup>1</sup> *Scientific American*, July, August, September, 1927.

The largest of the stalactites were about the size of a lead pencil and about 10 inches long. All were quite fragile.

As to the time required for these depositions no definite statement can be made. The fort was in use during the Civil War, and it is likely that the roof remained in fair condition for thirty years longer. The impression received was that the rate of deposition had been much greater than is commonly thought to be the case in the growth of limestone cave deposits. It is thought that the stalactites had not very recently been disturbed, as the floor deposits were fairly commensurate with the amount of material still hanging to the ceiling. The rate of deposition may have been an inch a year. And the entire deposit came from the meager supply of limy material contained in the mortar of the brick roof.

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#### THE LANGUAGE OF CLERGYMEN

I HAVE read the article entitled, "The Language of Scientists," by the Reverend George W. Lay, with a great deal of pleasure. Some of the mispronunciations to which he calls attention are really delightful. Certainly every scientist should be meticulous in the use of scientific terminology. But I wonder if it is not equally important for theological scientists to be somewhat careful of the structure of sentences. In Mr. Lay's amusing castigation of his fellow members of the Association for the Advancement of Science, I see this amazing statement: "An example of ignorance or carelessness appeared in an important paper by an eminent scientist that was published in SCIENCE." I want to congratulate the publisher who undertook so stupendous a task as that. We have all heard of books that are published, but this is the first time that I, for one, have ever heard of publishing an eminent scientist. Later in his article, Mr. Lay writes: "These words are practically always derived from the Latin or the Greek . . ." Does he mean that they are *usually* so derived? Still later, the supercritical (or is it hypercritical) Mr. Lay gives us this charming bit of English: "Attention has been called recently to two examples of unscientific confusion in the meaning of words." Perhaps Mr. Lay would be good enough to tell us what *scientific* confusion would be like. One more delightful bit of English meets us near the end of his article. He writes: "Scientists can not even trust each other." Are there, then, but two scientists who are thus antagonistic? Perhaps all scientists distrust *one another*. I have no doubt that Mr. Lay is quite correct in all his pronunciations, but a good rhetoric would

tell him that there is as great a danger in misplaced phrases and misused words as in misplaced accents. If we are to carry culture into the laboratory, by all means let us expand the meaning of the word "culture" to include correct sentence structure.

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"THE Language of Scientists" was certainly worth publishing. However, it suggests to me two questions. Mr. Lay speaks of a "co-ed graduate student." Are all participants in coeducation female?

He states later that one micromicron is a thousand times greater than another. Is it possible that he meant "a thousand times as great as"? Or, if you will, "999 times greater than"?

EDWARD S. ALLEN

#### BABYLONIAN MATHEMATICS

IN SCIENCE for December 12, 1930, page 601, Professor G. A. Miller writes: "The Babylonian mathematics is of special interest in view of the fact that our division of the circle into 360 parts called degrees, and our division of the degree and the hour into 60 parts called minutes and of the minute into 60 parts called seconds can be traced back thereto." May I suggest that nothing would be of greater interest to readers of SCIENCE than a presentation of references to *sources* where these various statements may be checked? Cantor makes no such claim, nor does he, in his references to Babylonian geometry, give adequate references to sources to check even the statement he does make: "for a certainty we have the division of a circle into 6 parts, then into 360 degrees." Heath reproduces no such statement. Tropfke in the third edition (1930) of Volume 1 of his history does not furnish proof of Professor Miller's claims. In 1928 Thureau-Dangin argued merely that the division of a circle into 360 parts was natural, but that further sexagesimal division was unnatural. During the past year I have given in SCIENCE<sup>1</sup> some references suggesting the difficulty, in the present state of our knowledge, of arriving at any definite conclusion in this regard.

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#### AN ENGINEER IN AUTHORITY

MOST scientific men were delighted when for the first time since George Washington an engineer be-

<sup>1</sup> SCIENCE, 71, 117-118, January 31, 1930; 71, 342, March 28, 1930. Many more detailed references are given in my "Bibliography of Egyptian and Babylonian Mathematics" in Chace's edition of the Rhind Mathematical Papyrus, 1927 and 1929.