250 feet thick, with all the other regular beds below it in position.

Now the key to this problem—the origin of Meteor Butte—seems to me to rest, not in a mythical meteor, but in the presence near the surface of this Kaibab limestone.

It is well known that every rock formation possesses certain peculiarities—certain individual characteristics which we might call "personal" features. One exhibits cross-bedding; another has a tendency to chon-coidal fracture and presents arches and natural bridges; still another yields towers, pinnacles, and so on. This is all too well known to require more than mention.

The peculiarity or "personal" quality of the Kaibab formation is that it has a sponge-like character. On the Kaibab Plateau, whence comes the name of this limestone, there are no brooks or streams flowing on the surface. Instead there are circular drainage basins without apparent inlet or outlet. These basins are of varying size and they are numerous. Their diameter and depth range from an almost imperceptible slope from circumference to center, to several hundred feet in diameter and a hundred feet or more in depth. Some hold water; some do not, most in fact do not.

These sinkholes appear to be the individual feature of the Kaibab limestone. On my first visit there, many years ago, they struck my attention immediately as being something unusual. Dutton was there about the same time and noted the sinkholes as something new.

The explanation seems simple. It is merely a broad downward drainage through porous rock. The Kaibab, of wide extent, is devoid of surface streams, even of the smallest rivulets, yet there is a considerable rainfall, while snow is deep, owing to an altitude of 8,500 feet. The water goes off, of course, but it goes *down* all over the plateau forming these sinkholes.

Doubtless this feature occurs elsewhere but not so prominently as on the Kaibab Plateau. The dissolved rock and other débris is carried down and deposited below where the water reappears as it does in Havasu Canyon and along the breaks of the north wall of the Grand Canyon in the Kaibab Division.

These sinkholes of the Kaibab, some of them at least one fifth as large as Meteor Butte, being in the same limestone that forms the upper structure of Meteor Butte would seem to offer a perfectly reasonable explanation of the origin of Meteor Butte.

That is to say: *Meteor Butte is entirely the work of erosion* and no meteor has had anything to do with its formation. The interior cliffs of the circumference appear, from the photographs, to be cliffs of erosion, for they have every characteristic. The exterior slopes of the circumference appear to be slopes of erosion, for they have every characteristic. The down inside drainage undoubtedly is into the near-by Canyon Diablo.

Where there are local tiltings and dislocations as they occur in the circumference they are doubtless due to washing out of softer portions or some other well-understood freak of erosion.

Meteor Butte, then, seems to be merely the reverse of a solitary mesa which preserves itself by a hard roof against erosion. The Meteor Sink had a soft spot where its hat ought to have been.

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DIASTROPHISM AND DISCOURTESY

IN youthful days the writer regarded geologists as supermen, devotees of the most inclusive and inspiring of all the sciences. But membership in the guild, for lo, these many years, reveals that we are ordinary mortals, with the common frailties of human-kind.

In his address to Section E, American Association for the Advancement of Science, as reported in this journal of January 17, page 53, Dr. Frank Leverett stepped out of the path of his address to make an ungenerous and unjust personal reflection, as follows:

> In this connection attention is directed to an erroneous map prepared by a leading American glacialist, in which isobases of postglacial uplift are made to correspond to an estimated thickness of the ice-sheet in the region east of the Mississippi River, thus disregarding the results previously published of observations showing that there is no such correspondence. No progress can be made where office speculation is substituted for or given more weight than field studies.

A perusal of my description and discussion of the "erroneous map" (Bulletin of the Geological Society of America, 29, 1918, 201–205) will convince the reader that the implied discredit in "office speculation" is not justified. It would appear as if the speaker had merely looked at the map and neglected the accompanying explanation. Evidently he ignores it, and regards "previously published observations" as final and sacred.

The following excerpt from my paper (page 203) is only part of the tentative and suggestive matter which was, and is, wholly justified, as will be shown later.

> The map shows that the postglacial land uplift of northeastern America is fairly proportionate to the area and thickness of the latest ice-sheet, and it appears legitimate to suggest similar relation in the Mississippi

and Great Lakes region. The southward curve given to the lower-value isobases may be excessive, but they suggest an uplift for which evidence should be sought.

It should be understood, therefore, that the isobases as extended in the Mississippi Basin are intended to be only suggestive of the southerly limit of the Pleistocene land uplift, . . .[1]

The personal element may be dismissed, but the involved geologic problem merits brief discussion. The isobases of postglacial continental uplift in my map were based on careful field study, extended in both area and time; with included data from Canadian explorers. That the area of diastrophic movement, postglacial land uplift, closely coincided with the glaciated area, and that the amount of uplift, where clearly determined, was in close agreement with the supposed thickness of the ice-sheet was fact from observation and was not theory.

Because a tilting uplift is not registered in the glacial lake beaches in the southern ends of the Michigan and Erie basins, the assertion is made that there was an entire absence of uplift in the territory on south. But such assertion, with disdain for different opinion, is not proof. Lack of positive evidence is often inconclusive.

Along the border of the glaciated territory wherever water-planes existed to record land warping there was postglacial uplift to the extreme reach of the ice-sheet. This applies to all the area east of New Jersey.

With an exception to be noted below there were no extensive bodies of water over Illinois, Indiana and Ohio to register differential uplift. But with a thickness of several thousand feet of ice of the earlier ice invasions pressing down on that territory it seems highly improbable that it was not affected like lands under even thinner ice in the eastward areas.

Following the slow removal of the weight of ice, from south to northward, the rise of the land surface evidently by a wave-like uplift, progressing northward over the United States. The land rise about the periphery of the depressed area was earlier in time than northward toward the center of the ice load. Hence it should be expected that the southern ice-covered area in the Mississippi Basin would be upraised before the area of the Great Lakes. And such rise was either previous to or during the life of the glacial lakes Chicago, Maumee and Whittlesey. For that southern district the rise was "postglacial," but in what was yet "glacial time" for the northern lands.

[1] This map was republished in SCIENCE, Vol. 47, 1918, page 166. It carries one serious error. Newfoundland is represented as an area of uplift distinct from that of North America. The contributed information on which that mapping was based was later found erroneous.

However, we do have a bit of positive evidence of postglacial land warping in the southern area. In 1914 Professor George D. Hubbard described the tilted shorelines of an extinct lake in Alaska and Wayne counties, in north-central Ohio.[2]

This "Craighton Lake" was eighteen miles long, and the differential uplift of the shorelines was determined as about four feet per mile. This study has been adversely criticized because it was unexpected and contrary to theory.

In the same address (page 53), after assuming the absence of uplift south of the Michigan and Erie basins, although granting a thickness of thousands of feet of ice, Leverett makes the following statement:

> These studies and studies in other basins occupied by glacial lakes have shown that the uplift extends only a short distance beyond the Precambrian lands into the lands covered by Paleozoic formations. There appears to be a closer correspondence with the border of the Precambrian lands than with the amount of ice weighting. It appears that the ice weight was insufficient to cause such a depression in the stable areas covered with sedimentary Paleozoic formations as it was able to produce in the highly eroded Precambrian areas.

Every text-book of geology will show the main Precambrian area lying north of the Great Lakes and the Ontario-St. Lawrence valley. Southward, the Precambrian patches are the core of the Adirondacks, the Hudson Highlands and a portion of New England. But all the Great Lakes area and all the continent east of the Hudson Valley has participated in the postglacial uplift. From north of Lake Ontario to New York City and from north of the St. Lawrence to the coast of Nova Scotia is not "a short distance" into the sedimentary formations.

The central area of postglacial uplift is not in the midst of the great Precambrian mass, but lies near the south border of that mass, southeast of James Bay, between that bay and Quebec City.

This center of the dome-shaped land uplift is presumably the area of the greatest depression under the ice-weighting. And evidently the latter was in consequence of the greater thickness of the ice. The central area of the postglacial uplift quite certainly locates the center of the Quebec (Laboradorian) ice cap, its feeding ground or "alimentation area." And such location was not affected by the nature and age of the underlying rocks, but was determined by the snow supply, which in turn was dependent on altitude and the atmospheric circulation.

The postglacial land uplift everywhere, as far as it has been clearly measured, appears to have direct relation to the thickness and weight of the ice load.

[2] *Amer. Jour. Science,* 37, 1914, 444–450.

It has no apparent relation to the character of the underlying rocks, nor to the land relief or the gross topography. And why should it? The cause of the up and down (diastrophic) movement of the land surface can be only slightly due to the elastic compression and expansion of the rocks. It is regarded as chiefly due to yielding and rock-flowage in the deep-down zone of plasticity. And this is very far below the base of any sedimentary rock.

Another interesting fact is that the isobases, or lines of equal uplift, pay no apparent respect to the great topographic features, as the deep and wide embayment of the St. Lawrence and the masses of the Adirondacks and White Mountains. And, again, why should they? These great features were produced far back in Tertiary time, and isostatic equilibrium had been long established, for both the nature of the rocks and the surface relief, before the Glacial Period. The ice caps were freshly imposed loads, with independent effect.

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THE CLASSIFICATION OF PYTHIUM

THE writer has read with some interest a note in a recent number of SCIENCE by C. P. Sideris entitled "The Proper Taxonomic Classification of Certain Pythiacious Organisms,"[1] as he has been investigating for some years those members of *Pythium* which possess filamentous sporangia, placed by Butler[2] in the subgenus *Aphragmium,* and has had an opportunity to examine minutely most of the newer species and nearly all the older ones.

It should be borne in mind that the genus *Pythium* was founded in 1858 by Pringsheim[3] on what must be regarded as a form possessing entirely filamentous sporangia (the term "sporangium" will be used here in its older sense without entering into the grounds for the distinction of pro- or pre-sporangium used by some more recent writers). Two years later de Bary[4] published his description of *P. proliferum,* a form in which the sporangium consisted of a spherical portion and a more or less elongated beak, the former structure being delimited from the rest of the hypha by a cross wall.

With the describing of *P. debaryanum* by Hesse,[5] in 1874, and de Bary's subsequent work on this species, *Pythium* became of peculiar interest to the pathologist because of the destructiveness of this species to the seedlings of various plants of economic importance. Since then, the pathological literature has contained many references to various types of diseases ascribed to *P. debaryanum,* and through it the genus has been, one might say, widely advertised.

In recent years it has become increasingly apparent that, aside from the two sporangial types heretofore described, there is a third one. Briefly, this consists of a basal portion of more or less compacted, swollen, digitate elements, separated as a whole by cross walls from the concomitant hyphae, and a filamentous evacuation tube through which the protoplasm of the two portions is discharged into a vesicle in the usual manner. This type is represented by such forms as *P. complens* Fischer, *P. aphanidermatum* (Eds.) Fitz. and others.

Fischer[6] in 1892, divided the species of *Pythium* then known into three subgenera. In *Aphragmium* he placed those forms which possess filamentous sporangia not differing from the vegetative hyphae and not cut off from these structures by cross walls. In *Nematosporangium,* he placed those forms with filamentous sporangia which did not differ from the vegetative hyphae, but were separated from them by septa. The species possessing subspherical to spherical sporangia he put in the subgenus *Sphaerosporangium.*

In 1897, Schröter[7] raised *Nematosporangium* to generic rank with two subgenera, *Aphragmium* and *Eunematosporangium.* Butler[8] proposed to retain the two subgenera of Fischer, but merged *Nematosporangium* with *Aphragmium.*

To any one who has studied the non-sexual reproduction of any of these filamentous types, it is apparent that cross walls must be laid down somewhere in the mycelium which will limit the flow of protoplasm, otherwise the whole content of the mycelium would be discharged at one time into the vesicle. In the hundreds of examples of such reproductive activity observed by the writer among various species which possess entirely filamentous sporangia, delimiting cross walls have always been observed.

These preliminary considerations lead to Mr. Sideris's suggested treatment of the genus.

Aside from the fact that he does not separate the entirely filamentous sporangial forms, such as *P. dictyosporum* Racib., *P. afertile* Kanouse and Hum-

[1] C. P. Sideris, "The Proper Taxonomic Classification of Certain Pythiacious Organisms," SCIENCE, 71: 323–324, March 21, 1930.

[2] E. J. Butler, "An Account of the Genus Pythium and Some Chytridiaceae," Mem. Dept. Agr. India, Bot. Series 1: 5, 162 pp., illus., 1907.

[3] N. Pringsheim, "Beitrage zur Morphologie und Systematik der Algen II," *Pringsheim's Jahrb. für wiss. Botanik,* 1: 284–306, 1858.

[4] A. de Bary, "Einige neue Saprolegnieen," *Pringsheim's Jahrb. für wiss. Botanik,* 2: 169–192, 1860.

[5] R. Hesse, "Pythium debaryanum, ein endophytischer Schmarotzer, etc.," Inaugr. Diss. Halle, 1874.

[6] A. Fischer, "Phycomycetes," in Rabenhorst's "Kryptogamenflora von Deutschland, etc.," 4: 391–410, 1892.

[7] J. Schröter, "Fungi," in Engler and Prantl's, "Die natürlichen Pflanzenfamilien," I[1]: 104, 1897.

[8] E. J. Butler, *loc. cit.*