

centre, producing a spherulite, which, but for the occurrence of intermediate stages, might be supposed to have originated entirely by divergent growth. The authors give details of the appearances presented by intermediate stages of growth.

The prevailing type of spherulite, both in Lipari and Vulcano, shows in section a dusky fibrous central area, which may possess concentric as well as radial structure, surrounded by an irregular brown cloudy zone of various width. The authors' studies lead them to the conclusion that this type owes its characters to the dual mode of growth, and therefore to the original presence of vesicles in the rock. Commonly the process of infilling does not go so far as this; on the ends of the felspar fibres plates of tridymite are deposited, and this seems to close the growth. It is clear that the lithophysal structure of the Lipari obsidians was formed during the cooling of the mass, and not by subsequent amygdaloidal infilling of vesicles.

The authors discuss the effect of confined vapours on such rocks as those forming the subject of the paper, noting that these vapours may be kept at a high temperature for a considerable time, each vesicle thus becoming a sphere of hydrothermal action; so that if the surrounding glass remains at a temperature little below its fusion-point, crystallization will be promoted in it, and at the same time the action of the vapour in the vesicle will produce reactions on its walls.

An Appendix, by Prof. Cole, treats of the lithophyses and hollow spherulites of altered rocks. While admitting the presence of true lithophyses in many of the Welsh lavas, he is not prepared to abandon a former suggestion that the interspaces between successive coats of the Conway lithophyses result from alteration of a formerly solid mass. In the lavas of Esgair-felen and near the Wrekin he has no doubt as to the production of "hollow spherulites" by ordinary processes of decay. The typical Continental pyromerides are truly spherulitic, as is much of the Wrekin lava. In the latter case and that of the rocks of Bouley Bay it will be difficult to distinguish between infilled primary and secondary cavities.

---

#### CORRESPONDENCE.

---

##### "THE RECENT ELEVATION OF THE HIMALAYAS."

SIR,—The nummulitic limestones occurring in the Hundes Valley show that in Eocene times the ground where the Himalayas now stand was covered by the sea. This is, I believe, admitted by Dr. Blanford. In the Manual of Indian Geology for which he and Mr. Medlicott are responsible he goes further, and expressly says that "*at the close of the Miocene epoch no such mountain barrier as exists at present separated the Indian peninsula from Central Asia*" (op. cit. p. 585). The opinion of Dr. Blanford in 1878 therefore was that the Himalayas are of *post-Miocene* origin.

The superficial beds which contain the mammalian remains we have been disputing about lie unconformably upon certain speckled

sandstones which Griesbach classes as of "Miocene and possibly of later age." Mr. Lydekker, after discussing the remains, says of these mammalian beds, "the beds in question are probably of *Pleistocene age*, and almost certainly not older than Upper Pliocene" (Records, Geol. Surv. India, vol. xiv. p. 181).

General Richard Strachey, who examined them with great care, says of them in his article "Himalaya" in the last edition of the *Encyclopædia Britannica*, "there is no room for doubt that these deposits have been raised from a comparatively low level to their existing great elevation of upwards of 15,000 feet *since they were laid out*." Mr. Griesbach allows that these beds are everywhere raised up on end as is also the case along the southern margin of the lower hills which are skirted by the Siwaliks (Mems. Geol. Survey of India, xxiii. p. 34).

These facts seem to me to justify my contention that the Himalayas have been largely uplifted since Pliocene times. Mr. Blanford was himself once of the same opinion. When the Manual already quoted was published, namely, in 1878, his view was that the Himalayan elevation took place after the deposition of the Siwalik beds, that is, that it was post-Pliocene. I understand him to say that he has since changed his mind because Mr. Lydekker has shown the Rhinoceros remains found at Hundes to belong to an extinct genus, and he refers me to the Catalogue of Fossil Mammalia in the British Museum, vol. iii. p. 158, where he says I shall find that the Tibetan Rhinoceros was closely allied to a species belonging to *Aceratherium*, a small hornless (extinct) group with well-developed incisors in both jaws, and Dr. Blanford accordingly says very positively that the Rhinoceros in question cannot have been the *R. antiquitatis* as I had conjectured, and implies that the beds in which the remains occur must belong to an older horizon.

I confess, Sir, that his statement surprised me greatly. In the first place I knew that the remains from Hundes comprised no teeth and no skull, and I could not understand on what possible grounds Mr. Lydekker, whose patience and caution are so conspicuous, could have come to such a very definite conclusion about the specific character of the Rhinoceros referred to. As a matter of fact he does nothing of the kind. He expressly says of the remains "*they are not specifically determinable*" (loc. cit.).

In the face of this specific statement, I can neither understand how Dr. Blanford should have attributed to Mr. Lydekker a conclusion for which there seems to be no foundation, and secondly, how he can, because of a mere phantasm, justify to himself and us completely changing his own views on a most critical matter. I am bound to say, apart from everything else, there is a very strong *a priori* improbability that the Rhinoceros remains from Hundes belong to an extinct genus. Mr. Lydekker distinctly says "all the remains found at Hundes, with the doubtful exception of *Hippotherium*, belong to *living* genera," and he treats them as Pleistocene or Pliocene. The genus *Aceratherium* belongs to an older horizon altogether. So far as we know it was a tropical or

semitropical genus, and there is no warrant for attributing to it, as Dr. Blanford does, a capacity for living under conditions like those of the Hundes plateau. The only species of Rhinoceros known to me which were the companions of the Horse, etc., etc., elsewhere, were the *R. antiquitatis* and the *R. Merckii*, to one of which I believe the remains probably belonged.

My view in regard to the impossibility of supposing that any species of Rhinoceros could live where the remains are found in Tibet is shared by better authorities than myself. Strachey expressly says that "their existence in the present condition of the Tibetan plateau would be quite impossible," while Dr. Falconer, *facile princeps* as an authority on the Pachydermata recent and fossil, says, "Henry Colebrooke, the first who, along with Colonel Crawford, measured the heights of the Dwalagiri, procured from the plateau of Chauthan in the Himalayas, at a height of 17,000 feet above the sea-level, fossil bones, which were brought down and exported as charms into India, to which the natives attributed a supernatural origin, and called them 'lightning or thunder bones.' At the present time, during eight months in the year, the climate differs in no important respect from that of the Arctic circle, and in the whole of the district there is not a single tree or shrub that grows larger than a little willow about nine inches high. The grasses which grow there are limited in number, and the fodder in the shape of Dicotyledonous plants is equally scarce. Yet, notwithstanding this scantiness of vegetation, large fossils were found of the Rhinoceros, the Horse, the Buffalo, the Antelope, and of several carnivorous animals; the group of fossil faunas as a whole involving the condition that, at no very remote period of time, a plateau in the Himalayan Mountains, now at an elevation exceeding three miles above the level of the sea, where we get the climate of the Arctic regions, had then such a climate as enabled the Rhinoceros and several subtropical forms to exist. . . . The only rational solution which science can suggest is that within a comparatively modern period, a period closely trenching upon the time when man made his appearance upon the face of the earth, the Himalayas have been thrown up by an increment closely approaching 8,000 or 10,000" (Proc. Roy. Geol. Soc. vol. viii. pp. 41 and 42). I commend this passage to Dr. Blanford.

I had written a detailed criticism of his rejoinder, in which I traversed every point he has made, but I do not think it right to unduly load your pages with an ephemeral polemic, and I have merely therefore selected one issue as a sample, and I venture to think it shows that the position I have supported is unassailable.

HENRY H. HOWORTH.

#### CONE-IN-CONE STRUCTURE.

SIR,—In reference to the statement of Mr. Alfred Harker, F.G.S., regarding radiation, and inversion of the cone structure, in nodular masses, in May Number of *GEOL. MAG.*, I hope you will kindly allow me to state, that I have in my printed paper, on "Cone-in-