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-

cone Structure," mentioned in March Number of GEOL. MAG., offered a short explanation, that seems satisfactory to myself, and others, that have seen my specimens,—regarding the radiated and inverted structure of the cones, sometimes seen in nodular masses; this radiation, as stated in paper, pp. 26, 27, being due to secondary causes that had acted on the cone stratum after the cone structure itself had been developed.

I point out in my paper, in the first place, that the cones, in any continuous level stratum, were evidently formed, through the upward escape of gases from below, whilst the stratum itself was being deposited the cones, invariably having their apices directed downwards to the lower part of the bed. In those cone strata where there has been an after-tendency in the sediments to aggregate into nodular masses, these nodules, in their contraction from larger into smaller dimensions, during their solidification, often show clear evidence of the gradual pulling of the cones, from their former erect position, all over the surface of the nodules; they radiating, outwards, from the centre to the circumference, there also being evidence of much crushing and distortion of the cone structure all along their outer edges. I further point out that "where the contraction would be greatest, as in the more argillaceous nodules, there may have been a bending, and in some instances a complete inversion of the cones around the edges of the nodules."

Since my paper was printed, in 1886, I have obtained many other illustrative specimens from our Scottish coal-field. These clearly show that the radiation and inversion of the cones, in nodular masses, was due to after secondary causes, the cone structure being first, the formation of the nodules being second, and the amount of radiation, and inversion of the cones, affords a measure of evidence as to the amount of contraction that has taken place amongst these nodules previous to complete solidification.

HUNTERIAN MUSEUM, UNIVERSITY, GLASGOW.
May 9th, 1892. JOHN YOUNG.

DISSIPATION OF ENERGY AS A GEOLOGICAL FACTOR.

SIR,—Many readers of the GEOLOGICAL MAGAZINE will, perhaps, be glad to have their attention drawn to to the following passage, which concludes an article by Lord Kelvin (P.R.S.), on "Dissipation of Energy," in the "Fortnightly Review" for March, 1892:—

"The whole store of energy now in the sun, whether of actual heat, corresponding to the sun's high temperature, or of potential energy (as of a not run-down weight of clockwork)—potential energy of gravitation depending on the extent of future shrinkage which the sun is destined to experience, is essentially finite; and there is much less of it now than there was three hundred thousand years ago. Similar considerations of action on a vastly smaller scale are, of course, applicable to terrestrial plutonic energy, and thoroughly dispose of the *terrestrial 'perpetual motion,'* by which Lyell and other followers of Hutton, on as sound principles as those of the humblest mechanical perpetual-motionist, tried to find that