the Cretaceous peneplain—Coot Hill, for example, itself consisting of hard granite-gneiss, fronts Lake Champlain with an almost precipitous escarpment 1,200 or 1,400 feet above the flat Ordovician, Beekmantown limestone, which abuts sharply against its base. There is surely a great fault between the two and the question arises is the relief due in large part to a fault-scarp not yet obliterated or did the ice-sheet take away 1,200–1,400 feet of Chazy, Trenton and Utica strata which must otherwise have stretched eastward from it? Or again, did some pre-glacial river aid in the work? My own disposition is to place confidence in faulting of date since the Cretaceous and not yet obliterated.

I have never been able to establish post-glacial faulting either by dislocated drift or broken glacial striæ, although both possibilities have been kept in mind.

As Professor Davis states, the graben-like valleys do run usually with the general structural trend, but there are occasional ones which strike across this direction. The valley of the Cascade lakes, shown in Fig. 7 of my paper, is such an one (*Popular Science Monthly*, March, 1906). The sides are quite steep and lofty and the lakes are almost at the crest of a divide. A similar cross-canyon lies just north. The little cross-valleys mentioned on p. 201 have similar relations.

On p. 203, the fifth line from the bottom, 'Needles' should read 'Noses.' The old New York name for this uplift is the 'Noses.'

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VARIATION VERSUS MUTATION.

The suggestion of weakness in the mutation theory of evolution given by Dr. Merriam in a recent number of this journal, from evidence afforded by living faunas as affected by geographic environment, contrasts greatly with apparent evidence in favor of the mutation theory advanced by Professor White in the preceding volume. Both are able expositions, but from very different standpoints, the one from living evidence, the other from such information and rational inferences as we have been able to derive from paleontology. The facts in both instances are well known, for no biologist could for a moment deny that geographic isolation plays an important part in modifying and frequently originating species, though to a wonderfully variable extent, some forms remaining constant throughout very extended ranges, while others are much more plastic, giving rise to species or subspecies in almost every large mountain valley. The change wrought in many winged Coleoptera when established on oceanic islands is frequently alluded to, the wings becoming aborted and other modifications supervening which eventually give rise to what must be considered distinct species.

At the same time we must admit that species are succeeded by other species in successive strata of a geological formation, with such abruptness and frequently with such marked divergencies, as to preclude the idea that the modifications could be brought about by simple changes of climate. It would appear that something else has affected the stability of species to give rise to these observed facts, and, as the development of species by mutation has proved to be at least possible, this seems at present to be the most plausible hypothesis in many instances. A so-called sport is much more difficult to comprehend than any modification brought about by visible alteration of environment, and is probably caused by some let-up in the multitude of environmental conditions that hedge about a species in nature and cause it to maintain its constancy. There is no reason to assume that if this sudden change in the surrounding conditions should be maintained the sport might become a firmly established species, and this mode of evolution seems, from paleontological evidence, to have become much more universal at certain epochs of the earth's history than at others.

In other words, there is probably much truth in both the hypotheses that have been advanced to account for evolution, and it seems to me that Dr. Merriam condemns the mutation theory much too sweepingly—there may be a good deal in it.

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