ing their properties. The investigation is still in progress at the University of Wisconsin. So far a ton of the material has been produced.

C. J. Borgmeyer,
Corresponding Secretary.

THE CLEMSON COLLEGE SCIENCE CLUB.

The sixtieth regular meeting of the club was held on Friday evening, January 19. The following program was given:

Dr. R. N. Brackett: 'The Contact Process of Making Sulphuric Acid.'

Professor F. T. Dargan: 'Modifications in Laboratory Apparatus.'

Dr. L. A. Klein: 'New Developments in the Prophylaxis and Treatment of Tuberculosis.'

Professor S. B. Earle: 'The Internal Combustion Engine with Especial Reference to the Diesel Engine.'

Fred H. H. Calhoun,
Secretary.

DISCUSSION AND CORRESPONDENCE.

THE PHYSIOGRAPHY OF THE ADIRONDACKS.

To the Editor of Science: An article by Professor Kemp in the March number of the *Popular Science Monthly* with the above title treats a subject on which I have been desirous of getting fuller information for some years past, namely, the origin of the mountain and valley forms in the Adirondacks; but there is a certain phase of the subject which still, to my reading, remains in doubt, namely, the age of the faults by which the mountain sides —or valley sides—are determined. The question arises whether the faults may not be relatively ancient rather than 'of no great geological antiquity,' and whether the valley-side scarps which now indicate the course of the faults may not be, not 'obviously the result of faulting,' but the result of differential erosion.

It is well known that the scarps which follow fault lines are of two kinds. Of one kind are those scarps which are the direct result of faulting or displacement, modified more or less by later erosion on the scarp face. Such a scarp is found along the western base of the Wasatch Mountains in Utah; and I believe that a similar fault scarp marks the base of the Rocky Mountains a few miles south of Colorado Springs. In both these cases, the displacement seems to have been progressive and to have continued through so long a period of time that the upper part of the fault face has been much dissected and worn back by erosion; the true fault scarp is seen only along the mountain base, where the face of the most recent uplift is comparatively little changed. It is evident that, as time passes, such fault scarps will be more and more worn back, and that in time they will be topographically obliterated. Topographically obliterated faults are common in the Appalachians where the faulting is of remote date.

Of another kind are those scarps which truly follow fault lines, yet which are directly the result of erosion rather than of faulting. For example, the Hurricane Ledge or escarpment in the Arizona plateaus north of the Colorado canyon. This scarp was originally described as wholly the direct effect of faulting; but later study has given good reason for believing that it is the effect of differential erosion; that the original effect of the displacement was obliterated in a past cycle of erosion, and that in the present cycle the scarp has been brought to light again by the removal of the weaker strata on the west of the displacement, while the more resistant strata remain in strong relief on the eastern side. Similarly in the Triassic formation of Connecticut, numerous scarps in the trap ridges are here known to follow fault lines; yet the faults are demonstrably so old that their original topographic effects were completely obliterated in a past cycle of erosion, and the fault scarps as now seen result from the revival of erosion following a general uplift of the worn-down region and the consequent removal of the weaker rocks on one side of the fault line, leaving the resistant trap sheets on the other side in strong relief. (This statement does not apply to the western faces of the trap ridges, which are merely retreating escarpments entirely due to erosion; but to the oblique escarpments, where the trap ridges are cut off by the faults.) In the same region there are a few narrow graben-like troughs, enclosed by steep walls; they are not due to recent faulting, but to the removal by modern erosion of the zone of shattered

rock that follows a comparatively ancient fault. In all these cases, it is true enough that erosion has been guided by a fault; but it is certainly desirable to distinguish between scarps of that kind and scarps directly produced by faulting.

It is not easy to determine, on reading Professor Kemp's article, whether the scarps that he describes are of one kind or of the other. Certain supposable conditions, not excluded by anything in his article, would allow an ancient date for the faulting, and would leave the freshness of the scarps to relatively modern erosion, especially to glacial erosion. It is of course possible that the full knowledge which Professor Kemp possesses of the rock structures in the Adirondacks may enable him to exclude these supposable conditions.

There is one feature of the Adirondacks which seems to me to favor the idea of a remote date for at least those faults by which the graben-like valleys are determined. This may be made clear by the following alternative considerations. If the graben-like valleys were the result of faulting of so modern a date that their scarps are still steep as a direct result of displacement, it is evident that the date of such faults must be of later date than the general erosion of the region. In this case there should not be expected any particular relation between the course of the older valleys and the course of the new graben. On the other hand, if the faults are of ancient date, the general valleys of the region might have been eroded along them, and might have gained a wide-open expression appropriate to their advanced age; while the graben-like valleys might then be due to more modern erosion —especially to glacial erosion—along the zones of fault-shattered rock. In this case there should be a close association between the direction of the general valleys and the course of the graben. As far as I know the region from its maps the latter relation seems to prevail.

W. M. DAVIS.

HARVARD UNIVERSITY.

TO THE EDITOR OF SCIENCE: Professor Davis has kindly submitted his letter to me before publication and I gladly embrace the opportunity to add a word regarding the points raised. I assume that the relief is admitted to be primarily due to faulting and that the question of faults as against joints or other influences I need not take up. While faults in the central region of crystalline rocks are less easy to demonstrate than in the outer and contrasted sediments, yet in the areas of the latter they are frequent and they can be followed in instances into the mountains themselves.

Generally speaking, the region of the Colorado River can not with justice be compared with the Adirondacks, because in the former we have canyons alone; whereas in the Adirondacks we have both canyons and broad, open valleys of equal depth and believed with reason to be older topographic forms. Rivers, even if following faults, could under the circumstances scarcely yield canyons, since they would be tapped off by the older valleys long before they could accomplish much; but two opposing faults with a dropped strip or a 'Grabensenkung' could. If we had one scarp produced because a stream worked down a dip, on a hard stratum and against a soft one, erosion without faulting could be urged, but, in these old crystallines, we usually have no contrasts in hardness, which would lead to sapping, and in the valleys we very often have precipitous fronts on each side.

The production of the scarps by other processes than faulting chiefly centers around the work of the ice sheet. The question essentially boils down, as soon as we know the country, to the decision whether or not the ice alone was able to pluck away the fronts of single escarpments and to carry off the intervening rock between two so as to leave the 'Grabensenkung.'

Evidences of an old plateau or peneplain are found in certain flat-topped mountains. Dr. I. H. Ogilvie has remarked a striking one on Treadway Mountain in the Paradox Lake quadrangle (Bulletin New York State Museum, No. 96, p. 468) and another appears in Coot Hill in the northeastern portion of the town of Crown Point, Ticonderoga quadrangle. We have imagined these and others like them to be remnants of

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.'

J. F. KEMP.

COLUMBIA UNIVERSITY.

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.

THOS. L. CASEY.

ST. LOUIS, MO.