the other (pendent) longer, thinner, straight, and appressed closely downwards to the stem; the leaves on the branches being closely imbricated all round. The stem bears leaves very different in form and structure from those of the branches.

Now *Sphagnum acutifolium* is a most variable moss; the list of recognized species in Europe alone numbering about thirty.

Among these are several distinct and well-marked forms, such as the following: In one the branch leaves, instead of being straight and closely imbricated as described above, are bent back in the middle and spread almost at right-angles from the branch — the *forma squarrosa*. In a second the branches, instead of being straight or nearly so, are hooked or contorted — the *falcate* variety. In a third, the *forma compacta*, the whole plant takes a short, compact habit, the stems being much shortened and closely tufted, the fascicles of branches close together, and the branches themselves short and stunted, with the leaves closely set. In a fourth the differentiation between the stem and branch leaves almost or quite disappears, the former acquiring the form and structure of the latter, the *forma homophylla*, and so on with two or three more distinct varieties.

Now, if we turn to the other species of the genus, we find that of those found in Europe and North America there is hardly one which does not include one or more of these six or seven distinct varieties which we find in *S. acutifolium.* Thus of nineteen European species (all but two of which are natives of North America) sixteen, and perhaps eighteen, have varieties belonging to the *forma compacta*, fourteen at least, and perhaps four others, have the squarrose variety, and so on to a greater or less degree with the other forms. At least two of these forms are found under every one of the species, and in more than one species all the forms are found.

Here we have a clear case of analogous variations. It cannot be supposed that they are instances of reversion to a common ancestral form, for, apart from other considerations, the variation in some of the forms is in a directly opposite direction to that which it takes in others. The delicate, elongated forms of the *tenellæ* and the dense, compact forms of the *compactæ* can hardly both be reversions to a common ancestral type!

So far we have exactly the same thing that we see in many races of domesticated species, such as Darwin has pointed out, for instance, in the races of the domestic pigeon; but we do not often see it carried out on such a wide and instructive scale.

But what is of especial interest in the case of the Sphagnaceæ is that, when we go further and consider the characters that distinguish the different species from one another, we find that the very points which we have seen mark off the above varieties (and render them, as a rule, more distinct than the other varieties of the species) are in several cases those which are most characteristic in separating from one another the species themselves. Thus *S. squarrosum* is specially marked by the spreading leaves; *S. rigidum* has for its most obvious features the very characteristics by which the *compacta* forms above described are distinguished; *S. subsecundum* in most of its forms is marked by its falcate or contorted branches; while a group of species, classed by Lindberg as HOMOPHYLLA, are characterized by that similarity of stem and branch leaves which I have described above as the feature of the corresponding variety; and so on with the other forms. Here we have exactly fulfilled the supposition of Darwin quoted above, "that a variety of one species would resemble in certain characters another and a distinct species," and fulfilled, too, on a scale which, at any rate, precludes the possibility of its being due to fortuitous coincidence.

On any theory of creation that did not presuppose a common ancestry for these species of Sphagnum, it might indeed be possible to account for the analogy between the varieties of different species by assuming the variations to be the direct results of the environment (a more than doubtful assumption, moreover); but the more we lay this cause under contribution to account for the varietal forms, the harder it is to believe that precisely the same variations in the species, only carried out to a higher degree of permanency, are due to entirely different and quite unconnected causes.

The above facts appear to me to form a peculiarly interesting support to Darwin's argument from analogous variation. In the first place, the possibility of reversion is, as I have pointed out, eliminated, and reversion and analogous variation, which are quite distinct principles, are too often indistinguishable in their results for us to be quite certain that we have a genuine example of the latter. In the next place, as Darwin points out, analogous variations are liable to be eliminated as not being necessarily serviceable; that they are not eliminated in the Sphagna is, I believe, partly due to the peculiar conditions under which these plants usually grow, but this opens too wide a field to enter upon here. In addition to these reasons, we have here an illustration drawn from species and varieties in a state of nature; examples of analogous variations have usually to be drawn from domesticated forms, where their value is limited by their necessarily applying to races and varieties only, and not to distinct species.

I append a table (taken from Jensen's paper quoted above), which shows at a glance the distribution of these varietal forms among the European species of Sphagnum. A † indicates the existence of the variety heading the column under the species opposite to which it is placed; a ? means that the existence of such a form is probable, but is insufficiently attested or not clearly enough marked to be entered as certain. It must be remembered that there is always a possibility of gaps being filled up by future research, but the table is, I think, as it stands, sufficiently striking.

Group.	Species.	Forma homophylla.	Forma compacta.	Forma tenella.	Forma falcata.	Forma squarrosula.	Forma immersa.
Sphagna cuspidata.	Sphagnum laxifolium, C. M.		?		†	?	†
	" intermedium, Hoffm.		†	†		?	†
	" riparium, Angstr.					†	†
	" lindbergii, Schimp.		?			†	†
	" wulfii, Girg.		†			†	?
	" acutifolium, Ehrb.	†	†	†	†	†	†
	" strictum, Lindb.		†			†	?
	" fimbriatum, Wils.		†			†	?
	" teres, Angstr.		†			†	?
	" squarrosum, Pers.		†			†	†
S. subsecunda.	" subsecundum, Nees.	†	†	†	†	†	†
	" caricinum, Spruce.	†	†	†	†	?	†
	" tenellum, Ehrb.	†	†		†		†
S. compacta.	" compactum, D. C.		†			†	†
	" molle, Sull.		†			†	
	" angströmii, C. Hartm.		†			?	?
S. palustris.	" cymbifolium, Ehrb.	†	†			†	†
	" papillosum, Lindb.		†			†	
	" austini, Sull.		†			†	?

THE CLOSE OF THE ICE AGE IN NORTH AMERICA.

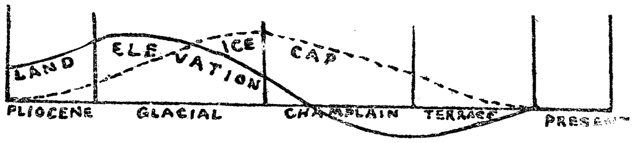
BY R. W. MCFARLAND, LL.D., LATE PRESIDENT OF MIAMI UNIVERSITY.

THIS is a question of interest to scientific men in general, and to geologists and glacialists in particular.

In Professor Wright's "Ice Age in North America," p. 448, in speaking of Croll's table of the eccentricity of the earth's orbit, he says: "According to this table the modern period most favorable to the production of a glacial epoch began about 240,000 years ago, and ended 70,000 years ago." Again, on p. 450, we have this: "If, therefore, the glacial period should prove to have ended only 10,000 years ago, instead of 70,000, the Darwinian would be relieved of no small embarrassment."

A genuine scientist, of course, has no preconceived theory to sustain — whether of Darwin, or of Archbishop Ussher — he seeks only to know the truth, whatever may be the consequences. Perhaps the points mentioned in this paper further along have not had sufficient attention hitherto. "Come, let us reason together."

The first extract above sets the close of the "Ice Age" entirely too far back. One of the objects of this paper is to make good this assertion. From the facts set forth below, it is reasonable to conclude that even on Croll's theory alone the close was not over 40,000 years ago, and possibly not over 35,000. If the views of Professor LeConte, given in his paper of January, 1891, have the weight and influence which their importance demands, it seems to me that there need no longer be any contest between gacialists who reject the astronomical theory, by reason of the remoteness of the time, and those who refer the ice age to terrestrial causes alone. Professor LeConte's theory is so clearly and tersely set forth that it is best to quote it entire, as given by Professor Wright on pp. 618–9, including the figure used in illustration:—



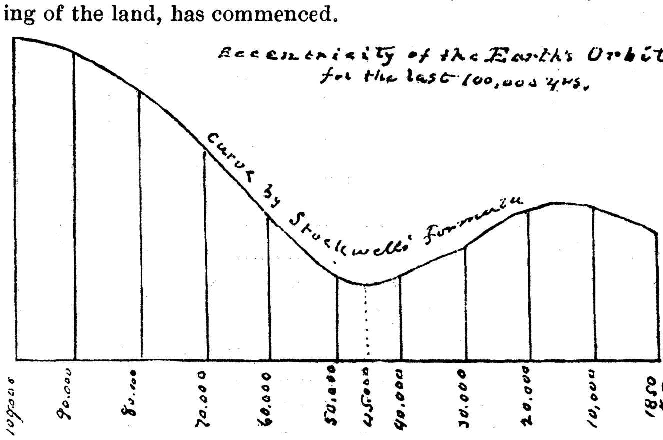
"1. That the continental elevation which commenced in the Pliocene culminated in the early Quaternary, and was, at least, one of the causes of the cold, and therefore of the ice accumulation.

"2. That the increasing load of ice was the main cause of the subsidence below the present level.

"3. That the removal of the ice-load by melting was the cause of the re-elevation to the present condition.

"4. That all these effects lagged far behind their causes.

"This lagging of effects behind their causes is seen in all cases where effects are cumulative. For example, the sun's heating power is greatest at midday, but the temperature of the earth and air is greatest two or three hours later; the summer solstice is in June, but the hottest month is July; and in some cases the lagging is much greater. The cause of sea-breezes,— i.e., the heating power of the sun, — culminates at midday, but the effects in producing air-currents culminate late in the afternoon and continue far into the night, long after the reverse cause, i.e., the more rapid cooling of the land, has commenced.



"Now, in the case under consideration, it is probable that the lagging would be enormous in consequence of the reluctant yielding of the crust and the capacity of ice to produce the conditions of its own accumulation. Although the elevation produced the cold, and therefore the ice accumulation, yet the latter culminated long after the former had ceased, and even after a contrary movement had commenced."

So far LeConte.

The close of the glacial epoch as above given — 70,000 years — is wholly arbitrary, and is evidently very far from the truth, as will be shown. At that time the eccentricity of the earth's orbit was nearly twice as great as it is now, and the consequent excess of the sun's time on one side of the equator above that on the other side (depending on the longitude of the perihelion) was about fourteen days. It had decreased from thirty-five, when the difference was greatest. But this difference of fourteen days would work in the direction of great difference of climate between the hemispheres, and would so continue to work as long as there was any difference at all. And not only so, but the effect would continue and accumulate according to the universal law of nature in the cases above cited, long after the smallest eccentricity had been reached. And that smallest eccentricity occurred less than 45,000 years ago, whether the computation be made by the formula of LeVerrier or by that of Stockwell.

The last sentence of the extract from LeConte is significant: "Yet the latter culminated long after the former had ceased and even after a contrary movement had commenced." In this latitude the usual temperature for the first week in December is not very different from that of the first week in March. Yet the sun in the first case is more than twenty-two degrees south of the equator, and at the latter date is scarcely five degrees. In like manner, and in accordance with the law above named, suppose the intense cold resulting from the wide glaciation of the northern parts of this continent, to have continued long after the eccentricity had reached its minimum, then it is not only possible, but even probable, that the close of the ice age was not more than 35,000 years ago, even if 30,000 would not be a more accurate designation. In which event, the objection to the astronomical theory, by reason of the long time elapsed since the days of high eccentricity, is wholly removed. And Professor Wright himself, although long favoring the short period of 10,000 years, has lately seen cause to doubt whether this is not too small. In a letter to the New York *Nation*, under date of Sept. 15, 1892, in view of his recent investigation of the old northern outlet of the great lakes into the Ottawa River, he says the facts "will . . . considerably lengthen our computation."

This throwing back of the close of the ice age by the glacialist, and the preceding shortening of the period in accordance with a universal law of nature, may both serve to strengthen the hypothesis of LeConte, and commend it to all interested in these questions, as the explanation which best accounts for the admitted facts.

CURRENT NOTES ON CHEMISTRY.—I.

[*Edited by Charles Platt, Ph.D., F.C.S.*]

Properties of Diamonds.

THE experiments of M. Moissan in the production of artificial diamonds and other precious stones, his remarkable results in the reduction of the most refractory oxides and his whole line of work at high temperatures, are well known to readers of the scientific magazines. Some of the more recent investigations have been of the properties of the diamond when heated in oxygen, hydrogen, chlorine, etc. When the temperature is raised slowly the combustion is correspondingly slow and without production of light, being recognized solely by the action of the gas evolved on baryta solution. At 40°–50° above the point at which this slow combustion takes place the combustion becomes more rapid, producing a visible flame. Yellowish-brown carbonado burned with a flame at 690°; black carbonado with a flame at 710°–720°; transparent Brazilian diamond without a flame at 710°–720°; transparent crystallized Brazilian diamond without a flame at 760°–770°; cut diamond from the Cape without a flame at 780°–790°; Brazilian bort and Cape bort without a flame at 790°, and with a flame at 840°; very hard bort without a flame at 800°, and with a flame at 875°. As a rule, the harder the diamond the higher its point of ignition.

When heated to 1200° in hydrogen the Cape diamond loses nothing in weight, but becomes lighter in color and less transparent; dry chlorine and dry hydrogen fluoride have no action at 1100°–1200°. Sulphur attacks diamonds at 1000°, but with carbonado carbon bisulphide is readily produced at 900°. Sodium vapor has no action at 600°. Iron at its melting point attacks the diamond with the production of graphite on cooling; melted platinum also combines readily. Fused potassium hydrogen sulphate and alkali sulphates, potassium chlorate and nitrate are all without action on the diamond, but, according to Damour, attack