thing about calculus, at least enough to follow the developments in such works as Müller's *Grundlegung* or Weinstein's *Physikalische Maassbestimmungen*. The more physics he knows the better.

OUR CRIPPLED WEATHER SERVICE.

BY JAMES P. HALL, BROOKLYN, N.Y.

A recent order of the new Secretary of Agriculture stops all the scientific research which, until this month, was being conducted by the United States Weather Bureau, and limits the functions of the experts in the Central Office to mere forecasting. Quite apart from all personal and political considerations, this is a lamentable event on many accounts.

It appears to be necessary, even in this enlightened age, to prove afresh that "pure science" is a prerequisite to most of our material progress. We are still under the necessity of making out that Columbus, who conceived that other lands lay to the westward of the great Atlantic, who visited one potentate after another to secure aid for his schemes, who haunted the courts and camps of Ferdinand and Isabella year after year, and who backed up his case with only the calculations of "pure science," really served Spain in particular, and civilization in general, quite as well as the "practical" men who handled the ropes and sails of the three caravels. We must elaborately demonstrate, all over again, to some of our fellow-countrymen that the unknown inventor of the mariner's compass and those other "pure scientists" who make charts showing the deviation of the needle, have conferred as great benefits on mankind as the pilot who uses that quivering bit of steel in bringing his ship safely across the seas. We must be prepared to face a question whether the captain of a New England fishing smack who thumbs his almanac to find out at what hour the tide rises or falls on a given day is not, after all, the superior (as an agent in civilization) to those learned astronomers and mathematicians who compute the tables for that little pamphlet. We must not be surprised if sane, intelligent, even eminent men, tell us that all the amazing development in steel production which we have witnessed in Europe and America in the last quarter of a century would have come just as soon—perhaps sooner—if Henry Bessemer had not carefully evolved his wonderful process from chemical theories and laboratory tests, nor ought it to startle us if some one insists that the sweating laborer in a rail mill, who grasps with tongs the fiery snake which emerges from the rollers and drags it away to have its ends sawed off, does more toward the building of a safe and lasting road than the expert who sits at a table and figures out the precise cross-section of rail that will give the greatest resistance to all the complex strains to which those bars must be subjected in service, even though these calculations extend over years and are based on long-extended and carefully designed tests. We cannot count on the universal acceptance of our opinion—if it happens to be our opinion—that Roebling, in computing the exact size and number of the wires to hold up a bridge over East River, and in drafting all the plans for that wonderful structure, was at all comparable in usefulness with the truckman who now drives a two-horse team across it every day. If we positively assert that the projectors of the great railway systems beyond the Mississippi have done more than the men who drove spikes with sledge-hammers to open up that region to settlement and to provide outlets for the enormous grain and pork product which has resulted, we know not how soon nor how flatly we shall be contradicted. We may meekly hint that the physician who prescribes does as much to cure us as the drug clerk who compounds the prescription; that the arithmetic maker is as much of a public benefactor as the corner groceryman who foots up the total cost of ten pounds of sugar and two pounds of coffee; that Edison, who perfected the incandescent lamp after long years of experiment with no end of substances for his filament, did as much to give us an electric light as the man who tacks up cloth-covered wire in our offices and screws pear-shaped globes into wall-fixtures; that Graham Bell was quite as instrumental in enabling us to converse over a wire with people a dozen miles away as the patient girl who answers our ring and sticks a little brass plug in a hole for us; and that we owe as much to the long array of designers, from Watts to Buchanan, who have brought the locomotive engine up to its present perfection, as the engineer on the "limited" express for the marvellous speed we make in going to Chicago; but we must not mistake for conviction the tolerance with which these utterances are received.

And so in meteorology. There are minds so constituted that they regard the observer as the equal or superior of the inventor of the barometer and thermometer; the "practical" man who jots down figures on a map and then draws "isobars," "isotherms" and wind signs on it as more useful than the pure scientist who, without touching pencil to paper, studies the movements of high and low pressure areas across the country, and the man who guesses what changes will occur during the next twenty-four hours, in the shape, size, position, intensity and other features of the cyclonic and anti-cyclonic systems, are doing better work than one who discovers and formulates the laws that govern those changes, and thus renders forecasting possible. What makes this the more amazing is the insufficiency of our present rules for weather predictions. The principles involved are not yet fully established. The most successful experts in this line realize that they are working under only a provisional code that must be greatly modified and supplemented. There is not a science so young and undeveloped as meteorology; there is not a bureau in the national government whose maxims and procedure are not better established, nor, when one considers the immense and varied interests—railway, shipping, agricultural, commercial and individual—which are affected by the weather, is there any branch of the service which affects so many people, and affects them so directly, as this, unless we except the postal business? Not to strain every nerve to improve the quality and character of the work by fuller inquiry into fundamental theories is folly, if not crime. Such a policy of neglect involves direct waste, as ignorance always does. Our expenditure, year after year, would not thus be made to the best possible advantage. On the other hand, to use one per cent ($10,000), out of the $1,000,000 appropriated for the bureau, in expert work, would be a measure of true economy by gradually revealing how best to use the rest. That has been true of the bureau from the start; and it has never been a wiser course than it would be now. Any manager of a creamery, sawmill, cotton factory, iron foundry or railroad who deliberately threw away such a chance as this for improving what everyone recognized as the inadequate facilities of his business, at a trifling cost, would be set down by "practical" men as strangely blind or culpably reckless.

ANALOGOUS VARIATIONS IN SPHAGNACEÆ (PEAT-MOSSES).

BY H. N. DIXON, F.L.S., NORTHAMPTON, ENGLAND.

IN the "Origin of Species" (6th ed , p. 126) there is the following passage, under the heading of "Analogous Variations:" "As all the species of the same genus are supposed to be descended from a common progenitor, it ought to be expected that they would occasionally vary in an analogous manner, so that the varieties of two or more species would resemble each other, or that a variety of one species would resemble in certain characters another and distinct species,—this other species being, according to our view, only a well-marked and permanent variety."

A clear example of this is of considerable value in the support it gives to the theory of descent; but, as Darwin goes on to show, there are several reasons why such examples are not common.

A very striking illustration is, however, to be seen among the peat-mosses, or species of Sphagnum, and, as I do not know that anyone has drawn attention to the facts from this point of view, I think it may be of interest to present them briefly. Many of the facts quoted below are taken from a paper by C. Jensen (translated in the *Revue Bryologique*, 1887, p. 33, by F. Gravet), entitled "Les Variations Analogues dans les Sphagnaceés."

Sphagnum acutifolium may be taken as a typical species of the genus; in its most characteristic form it is a plant with tall, slender stems, bearing at intervals fascicles of simple branches of two kinds, the one (divergent) stouter and more or less horizontal, 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