

burden after burden, in the form of new notation, is added apparently for the sole purpose of exercising the faculty of memory." He would vastly prefer, it would appear, to write with Hamilton  $m\phi^{-1}$ , "when  $m$  represents what the unit volume becomes under the influence of the linear operator." But this notation is only apparently compact, since the  $m$  requires explanation. Moreover, if a strain were given in what Hamilton calls the standard trinomial form, to write out the formula for the operator on surfaces in that standard form by the use of the expression  $m\phi^{-1}$  would require, it seems to me, ten (if not fifty) times the effort of memory and of ingenuity, which would be required for the same purpose with the use of  $\frac{1}{2}\phi \times \phi$ .

I may here remark that Prof. Tait's letter of endorsement of Prof. Knott's paper affords a striking illustration of the convenience and flexibility of a notation entirely analogous to  $\phi \times \phi$ , viz.  $\phi : \phi$ . He gives the form  $S\nabla\nabla_1 S\sigma\sigma_1$  to illustrate the advantage of quaternionic notations in point of brevity. If I understand his notation, this is what I should write  $\nabla\sigma : \nabla\sigma$ . (I take for granted that the suffixes indicate that  $\nabla$  applies as differential operator to  $\sigma$ , and  $\nabla_1$  to  $\sigma_1$ ,  $\sigma$  and  $\sigma_1$  being really identical in meaning, as also  $\nabla$  and  $\nabla_1$ .) It will be observed that in my notation one dot unites in multiplication the two  $\nabla$ 's, and the other the two  $\sigma$ 's, and that I am able to leave each  $\nabla$  where it naturally belongs as differential operator. The quaternionist cannot do this, because the  $\nabla$  and  $\sigma$  cannot be left together without uniting to form a quaternion, which is not at all wanted. Moreover, I can write  $\phi$  for  $\nabla\sigma$ , and  $\phi : \phi$  for  $\nabla\sigma : \nabla\sigma$ . The quaternionist also uses a  $\phi$ , which is practically identical with my  $\phi$  (viz. the operator which expresses the relation between  $d\sigma$  and  $d\rho$ ), but I do not see how Prof. Knott, who I suppose dislikes  $\phi : \phi$  as much as  $\phi \times \phi$ , would express  $S\nabla\nabla_1 S\sigma\sigma_1$  in terms of this  $\phi$ .

It is characteristic of Prof. Knott's view of the subject, that in translating into quaternionic from a dyadic, or operator, as he calls it, he adds in each case an operand. In many cases it would be difficult to make the translation without this. But it is often a distinct advantage to be able to give the operator without the operand. For example, in translating into quaternionic my dyadic or operator  $\phi \times \rho$ , he adds an operand, and exclaims, "The old thing!" Certainly, when this expression is applied to an operand, there is no advantage (and no disadvantage) in my notation as compared with the quaternionic. But if the quaternionist wished to express what I would write in the form  $(\phi \times \rho)^{-1}$ , or  $|\phi \times \rho|$ , or  $(\phi \times \rho)_s$ , or  $(\phi \times \rho)_x$ , he would, I think, find the operand very much in the way.

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### On Secular Variations of our Rainfall.

IN studying the rainfall of this country, it is instructive, I think, to compare a number of curves for different places, and a long series of years, all smoothed by means of five year averages. In the case of places not too far apart, one may then recognise a common type amid some diversity of detail. But it is not easy to trace such "family likeness" between e.g., curves for the west of Scotland and the east of England.

The east of England curves seem to conform to the general law affirmed by Brückner for the greater part of the globe, viz. cold and wet periods alternating with warm and dry ones at intervals of about 35 years; so that, taking recent years, there was, in most places, a rainy period between 1841 and 1855, and again between 1871 and 1885, while a dry period occurred between 1856 and 1870.

In the accompanying diagram are shown two east of England curves, one for East Anglia, giving mainly the rainfall for Dickleburgh, in Norfolk, continued for about 17 years by that of Norwich (according to *British Rainfall*), the other for Boston (from the same work). These curves, it will be noted, dip down from a relative maximum in the early years, 1843 and 1847, and rise again to maxima in 1877 and 1881.

Some rainfall statistics for Oviedo were recently given in the *Meteorologische Zeitschrift* (Feb., 1892, p. 71). This is, it may be well to state, a university town in the north of Spain, capital of the province of Asturias, and about 20 miles from the coast of the Bay of Biscay. Now, the smoothed curve of this place, from 1853, has a form distinctly opposite to those just considered (as the diagram shows<sup>1</sup>). It rises to a maximum in 1864, goes

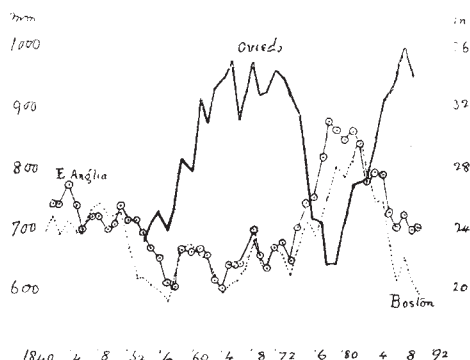
down to a minimum in 1877, after which it rises again, reaching, perhaps, another maximum in 1887.

This oppositeness in the variation of rainfall appears to merit attention. How is it to be explained?

One of the most interesting meteorological facts brought to light in recent years is, that the depressions which come over from the west do not take, as it were, a random course, but tend to follow, with more or less frequency, certain well-defined paths. The course of several of these paths has been indicated by Van Bebber, who has made a special study of the subject. Some of the paths are known to shift in the course of the year, having a different direction in midsummer from what they have in midwinter. And there can be little doubt, though the matter is still obscure, that the paths shift in successive years. The paths numbered IV and V by Van Bebber, are said to have shifted in the years 1879 to 1884.5 from a more maritime to a more Continental position, and Lang connects with this an observed variation in the rate of travel of thunderstorms in South Germany (see *Met. Zeits.*, Nov., 1891, p. [68], of *Literaturber.*). Such shifting is very probably accompanied with variations of rainfall. Hellmann supposes this to be the reason why in Spain a year that is wet in the north-west is generally dry in the south-east, and *vice versa*. We might, perhaps, roughly compare such variations to the case of a man watering a lawn with a garden hose, and directing the jet of spray now on one side, now on the other.

I do not know whether any suggestion of this nature is applicable to the case before us, or whether some other and better explanation may be forthcoming.

Oviedo is not, apparently, included in Brückner's data for estimating Spanish rainfall; and it is to be noted that he



regards the north of Spain as conforming to his thirty-five years law, while southern Spain is reckoned exceptional.

Brückner has two classes of exceptions: the "permanent," in which the curves are opposite to the normal (Ireland and the Atlantic islands being examples), and the "temporary," in which there is conformity to the rule, for a time; then, during some lustra, there come irregular variations. To this latter class are relegated south and middle Spain, Mediterranean France, West England, and Scotland. If Brückner's view regarding the north of Spain is correct, how comes it that the Oviedo curve has the character indicated, which is apparently that of the permanent exceptions?

In discussions on the subject of sunspot influence on weather one sometimes hears the opposite character of weather in different regions urged as a difficulty in the way of accepting such influence. Thus, in connection with a paper read by Mr. Scott to the Royal United Service Institution last year (*Journal*, May, p. 510) I find him remarking: "It is not possible to say whether or not the mere fact of our having very wet or dry weather is due to the sunspots, when our neighbours not very far off are having exactly the contrary. . . . Last summer everybody was abusing the weather because of its wetness. I myself was then living in the Black Forest, and we had four days' rain in eight weeks. Which of these conditions depended on the sunspots? Was it my fine weather or was it the rain here?"

With all deference to an excellent authority, and without offering an opinion upon the particular cases cited, it seems to me not impossible that the influence of the solar cycle might be manifested in an opposite succession of effects in different

<sup>1</sup> The vertical scales, right and left, are not to be taken as equivalent.

regions. Suppose, *e.g.* that in some region the rainfall in a long series of years varied, not as in the cases above considered, but in a certain regular correspondence with the sunspot curve; and in another region (perhaps further south) in opposite correspondence; also that these variations were traced to the shifting of a depression path. The opposite correspondence would obviously not be a good reason for denying sunspot influence, but rather corroborative evidence of such influence. Again, it will be admitted, I think, as conceivable that we might find certain great anticyclonic systems to vary in position or extent with the sunspot variations. Suppose, then, an anticyclone which lay over a region (*a*) at the time of minimum sunspots, were moved in a given direction, say northwards, so that it came to cover a region (*b*) at the maximum of sunspots and that it returned to *a* by the next minimum. In that case a place, *e.g.*, in the south part of region *a*, would have high barometer at minimum sunspots, while a place in the north part of region *b* would have low barometer. And at the maximum of sunspots, on the other hand, the two places would again have opposite conditions of pressure (to each other and to the first). These are some out of many aspects of the matter which seem to me to render doubtful the affirmation that if the solar cycle influences weather, it cannot produce an opposite succession of effects in different (even neighbouring) regions.

To revert, for a moment, to the shifting of depression-paths, might it not, in some cases, account for certain changes observed in the relative proportion of different wind directions? Suppose *e.g.* that, by the shifting of a path a little southwards, a place which has been for some years in its southern border comes to lie in the northern border, might it not thus come to have more easterly wind and less westerly? A. B. M.

#### The Non-Inheritance of Acquired Characters.

DR. WALLACE, in a letter which appeared in *NATURE* on July 20, asks for the opinion of naturalists as to the interpretation of certain facts bearing upon the question of the "Non-inheritance of Acquired Characters," and as I have given much thought to the subject I venture to offer my opinion.

In two papers published in *Natural Science*, vol. i. (1892), I set forth at some length a theory of heredity which has hitherto, so far as I am aware, met with no public criticism, and which I believe sets the question at rest, not by establishing the views of either of the rival schools associated with the names of Weismann and Lamarck respectively, but by showing that another interpretation is possible, and one which while fundamentally opposed to both of these makes it possible that there may be some truth hidden in the almost meaningless statements of the Weismannians and of the Lamarckians alike.

Till "heredity" is defined, and till we know exactly what we mean by "inheritance of characters" (be they "acquired" or "blastogenic"), it is useless to argue as to whether characters are "inherited" or not.

Is the word "heredity" an abstract noun, the name of a quality, a sort of magnified "family-likeness," or is it not? Those who write of heredity are too prone to speak of "heredity" as if it were a force or combination of forces producing an effect; as an "inherent tendency," to resemble parents or other ancestors which it is perhaps not unfair to compare to the "inherent tendency" of a watch to tell the time or of a weathercock to point to the south-west. There are those who even speak of it as being "latent" for a time and then, owing to some unknown cause, "springing into activity" anew and giving rise to what we call "atavism." Even "atavism" is not infrequently spoken of, as if it were of the nature of a force or combination of forces, comparable to a "latent tendency," which after lying "dormant" or "latent" for a time in a weathercock, suddenly springs into new activity and causes it to point as of old to the south-west.

It appears to me that if we once grasp the idea that "heredity" is the name of a quality, a particular kind of "likeness" or "similarity," and nothing else, we shall be saved from much useless discussion of propositions which are intrinsically almost, if not quite, meaningless.

*Artemia salina* is the collective name given to a large number of individuals which have certain characters in common. It would hardly seem to be necessary to suggest the probability that this possession of many characters in common is due to the action of Natural Selection; that each new individual possesses the characters in question solely by virtue of the fact

that Natural Selection has led to the production of individuals possessing the power to produce, under given constant conditions, eggs, which by virtue of their constitution will develop under given conditions into adults possessing the characters which natural selection has under those conditions rendered nearly constant.

It has been found that this same constitution does not necessarily lead to the same series of developmental changes under other conditions, and that in strong brine the eggs develop into animals which, though capable of living and multiplying under those conditions, differ in form from the ancestral *A. salina*. This new form has no more right to rank as a species than has a "worker" bee whose adult form differs from that of its parent merely on account of certain conditions to which it is exposed during development.

It appears to me to be absurd to ask whether the "acquired characters" of the so-called *Artemia Milhausenii* are inheritable or not. Experiment has shown that the constitution of the species *A. salina* has so little changed that it still has the power to produce eggs which under one set of conditions develop into *A. salina* and under another set of conditions into *A. Milhausenii*. The average constitution of the species has not varied: it still produces ova which will develop into either *A. salina* or *A. Milhausenii*, according to the conditions to which it is exposed. If we look upon the species as a whole, it is not too much to say that it exhibits no acquired characters. If bred in strong brine the individuals of many generations are alike, having been moulded by like influences, intrinsic as well as extrinsic. If the extrinsic influences change, new individuals differ from the old ones, simply because the constitution of the individuals as well as that of the species is such that under the new conditions the developmental changes occurring differ from those which would have occurred under the old conditions.

Whether this is true of all species and under all conditions consistent with life and multiplication, or is not true of some, is a matter for experiment, and can never be decided by argument. The experiment has been made by nature, and also by man in the case of *Amblystoma*, and with a result in exact conformity with the result in the case of *Artemia*.

The experiment has also been made with white mice in Freiburg, and it has been conclusively shown that under constant conditions the characters of successive generations are constant. One element of the environment in one series of cases was Prof. Weismann armed with tools for amputation of the tails of the young mice, plus a determination to amputate those tails. So long as this remained a constant factor in the environment, so long and no longer did the taillessness of the adult mice remain a constant character of the species.

The Texan species of *Sturnia*, so long as the exclusive supply of *Juglans regia* is a constant factor in the environment, may or may not have a constant group of characters. That is a matter for experiment; but innumerable experiments, called collectively "domestication," have shown that whatever effect changes of certain details of the environment—such as food—may have, the suspension of natural selection will in the long run lead to inconstancy of all those characters which are relieved from its restraining influence.

If anything has ever been rendered certain in biology by prolonged experiment and observation, it is the fact that specific characters are maintained constant by selection and by that alone. Long continued selection—natural or artificial—may produce a seeming constancy of characters (which we call "heredity"!), but in the long run this constancy will vanish when the particular selection which has induced it has been suspended for a sufficiently long period.

The discussion upon the "Inheritance of Acquired Characters," though it has led to many valuable results, has been throughout little more than a quibble, for in the whole discussion, so far as I am aware, the meaning of the word "inheritance" has never been defined. Most of the disputants appear to use the term as the name for the action of a force or combination of forces, which some have called "heredity"—a force or influence—either simple or complex—of which it is perfectly safe to deny the existence. There is no such thing as heredity—heredity is only a quality, a likeness or similarity, and nothing more. That likeness of characters is simply and solely due to the likeness of the influences which have produced the like characters, and pre-eminent amongst those influences is natural selection, though every factor of the environment has also had its part to play. So long as those like influences—