common. That M. Vinot should commence his book with references to the Deluge, the destruction of Sodom and Gomorrah, and the giving of the law on Sinai, seems perfectly natural. But most readers will note with some surprise that the last chapter of Dr. Hoernes's book is one entitled "Die Sintfluth." We cannot but regard it as a remarkable testimony to the profound influence of that striking and suggestive book of Dr. Suess, "Das Antlitz der Erde," that this chapter should have been added by Dr. Hoernes to his systematic treatise on Earthquakes. It is scarcely necessary to point out that the flood to which the Austrian geologist devotes the final chapter of his treatise is the deluge, not of Sir Henry Howorth, but of Noah and Hasîs-Adra, and that the connexion between this final chapter and the preceding ones is of the very slenderest character. But the legends of our own childhood and of the childhood of our race have a fascination for us, which neither the brilliant French essayist nor the painstaking German professor seem to have been able to resist.

OUR BOOK SHELF.

The Points of the Horse. By M. Horace Hayes, F.R.C.V.S. (London: W. Thacker and Co., 1893.)

IT is certainly curious that although the English nation justly prides itself on its knowledge of horse flesh, and its success in producing the various equine breeds, it should possess no work dealing in an exact and scientific manner with the conformation of the animal that it has done so much to improve. That certain shapes are indicative of great speed, whilst others point to strength rather than speed, has, of course, always been insisted upon in a general way, but it has been left to Captain Hayes to imitate the example of several French authors, and deal with the subject in a scientific spirit. A soldier, a certificated veterinarian, a traveller, and a successful rider, the author is well qualified to treat of all that pertains to the subject before us. The work represents a painstaking endeavour to discover and explain the various principles which govern the make and shape of the horse.

Starting with a study of animals like the Indian black buck and cheetah, which possess terrific speed, he compares them with others such as the buffalo and rhinoceros, which are examples of great strength, a comparison which leads to the conclusion that animals of great strength are distinguished by a long body and short legs; those of great speed by a short body and long legs. This is an exemplification of Marey's law that muscles of speed are long and slender, and those of strength short and thick. Whether it was necessary to stray so far from home to find examples of this fact may be doubted. The thoroughbred racehorse on the one hand, and the massive carthorse on the other, are surely sufficiently contrasted types of speed and strength, whilst between the two extremes are numerous examples exhibiting the union of these two attributes in various degrees, the hunter, for example, uniting considerable strength with moderate speed.

The defects as well as many of the beauties of conformation are admirably depicted in a series of photographs, such defects as turned-in and turned-out toes, sickle-shaped hocks, and upright pasterns, being particularly good. The photographic plates, of which there are over seventy, certainly constitute an important feature in the work, embracing, in addition to the above, portraits of many celebrated racers, notably "Ormonde" and "St. Simon," as well as horses and ponies of various breeds found in different parts of the globe. A chapter is devoted to an examination of these photographs, the leading features and points of the animals represented being analysed and commented upon. It would be unfair in this connection to omit favourable mention of the 200 excellent drawings by the late J. H. Oswald Brown, which serve throughout the work to illustrate the letterpress.

Author, artist, and publisher have successfully united in producing a first-rate work, which may be cordially recommended to all lovers—and their name is legion—of the horse.

W. F. G.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Quaternions and Vector Analysis.

IN a paper by Prof. C. G. Knott on "Recent Innovations in Vector Theory," of which an abstract has been given in NATURE (vol. xlvii. pp. 590-593; see also a minor abstract on p. 287), the doctrine that the quaternion affords the only sufficient and proper basis for vector analysis is maintained by arguments based so largely on the faults and deficiencies which the author has found in my pamphlet, "Elements of Vector Analysis," as to give to such faults an importance which they would not otherwise possess, and to make some reply from me necessary, if I would not discredit the cause of non-quaternionic vector analysis. Especially is this true in view of the warm commendation and endorsement of the paper, by Prof. Tait, which appeared in NATURE somewhat earlier (p. 225)

The charge which most requires a reply is expressed most distinctly in the minor abstract, viz. "that in the development of his dyadic notation, Prof. Gibbs, being forced to bring the quaternion in, logically condemned his own position." This was incomprehensible to me until I received the original paper, where I found the charge specified as follows: "Although Gibbs gets over a good deal of ground without the explicit recognition of the complete product, which is the difference of his 'skew' and 'direct' products, yet even he recognises in plain language the versorial character of a vector, brings in the quaternion whose vector is the difference of a linear vector function and its conjugate, and does not hesitate to use the accursed thing itself in certain line, surface, and volume integrals" (Proc. R.S.E., Session 1892-3, p. 236). These three specifications I shall consider in their inverse order, premising, however, that the *epitheta ornantia* are entirely my critic's.

The last charge is due entirely to an inadvertence. The integrals referred to are those given at the close of the major abstract in NATURE (p. 593). My critic, in his original paper, states quite correctly that, according to my definitions and notations, they should represent dyadics. He multiplies them into a vector, introducing the vector under the integral sign, as is perfectly proper, provided, of course, that the vector is constant. But failing to observe this restriction, evidently through inadvertence, and finding that the resulting equations (thus interpreted) would not be true, he concludes that I must have meant something else by the original equations. Now, these equations will hold if interpreted in the quaternionic sense, as is, indeed, a necessary consequence of their holding in the dyadic sense, although the converse would not be true. My critic was thus led, in consequence of the inadvertence mentioned, to suppose that I had departed from my ordinary usage and my express definitions, and had intended the products in these integrals to be taken in the quaternionic sense. This is the sole ground for the last charge.

The second charge evidently relates to the notations Φ_s and $\Phi_\times$ (see NATURE, vol. xlvii. p. 592). It is perfectly true that I have used a scalar and a vector connected with the linear vector operator, which, if combined, would form a quaternion. I have not thus combined them. Perhaps Prof. Knott will say that since I use both of them it matters little whether I combine them or not. If so I heartily agree with him.

The first charge is a little vague. I certainly admit that vectors may be used in connection with and to represent rotations. I have no objection to calling them in such cases *versorial*. In that sense Lagrange and Poinsot, for example, used versorial vectors. But what has this to do with quaternions? Certainly Lagrange and Poinsot were not quaternionists.

The passage in the major abstract in NATURE which most distinctly charges me with the use of the quaternion is that in which a certain expression which I use is said to represent the quaternion operator $q(\ \ )q^{-1}$ (vol. xlvii. p. 592). It would be more accurate to say that my expression and the quaternionic expression represent the same operator. Does it follow that I have used a quaternion? Not at all. A quaternionic expression may represent a number. Does everyone who uses any expression for that number use quaternions? A quaternionic expression may represent a vector. Does everyone who uses any expression for that vector use quaternions? A quaternionic expression may represent a linear vector operator. If I use an expression for that linear vector operator do I therefore use quaternions? My critic is so anxious to prove that I use quaternions that he uses arguments which would prove that quaternions were in common use before Hamilton was born.

So much for the alleged use of the quaternion in my pamphlet. Let us now consider the faults and deficiencies which have been found therein and attributed to the want of the quaternion. The most serious criticism in this respect relates to certain integrating operators, which Prof. Tait unites with Prof. Knott in ridiculing. As definitions are wearisome, I will illustrate the use of the terms and notations which I have used by quoting a sentence addressed to the British Association a few years ago. The speaker was Lord Kelvin.

"Helmholtz first solved the problem—Given the spin in any case of liquid motion, to find the motion. His solution consists in finding the potentials of three ideal distributions of gravitational matter having densities respectively equal to $1/\pi$ of the rectangular components of the given spin; and, regarding for a moment these potentials as rectangular components of velocity in a case of liquid motion, taking the spin in this motion as the velocity in the required motion" (NATURE, vol. xxxviii. p. 569).

In the terms and notations of my pamphlet the problem and solution may be thus expressed:

Given the curl in any case of liquid motion—to find the motion.

The required velocity is $1/4\pi$ of the curl of the potential of the given curl.

Or, more briefly—The required velocity is $\frac{1}{4\pi}$ of the Laplacian of the given curl.

Or in purely analytical form—Required ω in terms of $\nabla \times \omega$, when $\nabla . \omega = 0$.

Solution—

$$\omega = 1/4\pi \nabla \times \text{Pot}\, \nabla \times \omega = 1/4\pi\, \text{Lap}\, \nabla \times \omega.$$

(The Laplacian expresses the result of an operation like that by which magnetic force is calculated from electric currents distributed in space. This corresponds to the second form in which Helmholtz expressed his result.)

To show the incredible rashness of my critics, I will remark that these equations are among those of which it is said in the original paper (Proc. R.S.E., Session 1892–93, p. 225), "Gibbs gives a good many equations—theorems I suppose they ape at being." I may add that others of the equations thus characterised are associated with names not less distinguished than that of Helmholtz. But that to which I wish especially to call attention is that the terms and notations in question express exactly the notions which physicists want to use.

But we are told (NATURE, vol. xlvii. p. 287) that these integrating operators (Pot, Lap) are best expressed as inverse functions of ∇. To see how utterly inadequate the Nabla would have been to express the idea, we have only to imagine the exclamation points which the members of the British Association would have looked at each other if the distinguished speaker had said:

Helmholtz first solved the problem—Given the Nabla of the velocity in any case of liquid motion, to find the velocity. His solution was that the velocity was the Nabla of the inverse square of Nabla of the Nabla of the velocity. Or, that the velocity was the inverse Nabla of the Nabla of the velocity.

Or, if the problem and solution had been written thus: Required ω in terms of $\nabla\omega$ when $S\nabla\omega = 0$.

Solution: $\omega = \nabla\nabla^{-2}\nabla\omega = \nabla^{-1}\nabla\omega.$

My critic has himself given more than one example of unfitness of the inverse Nabla for the exact expression of thought. For example, when he says that I have taken "eight distinct steps to prove two equations, which are special cases of

$$\nabla^{-2}\nabla^2 u = u,"$$

I do not quite know what he means. If he means that I have taken eight steps to prove Poisson's Equation (which certainly is not expressed by the equation cited, although it may perhaps be associated with it in some minds), I will only say that my proof is not very long, especially as I have aimed at greater rigour than is usually thought necessary. I cannot, however, compare my demonstration with that of quaternionic writers, as I have not been able (doubtless on account of insufficient search) to find any such.

To show how little foundation there is for the charge that the deficiencies of my system require to be pieced out by these integral operators, I need only say that if I wished to economise operators I might give up New, Lap, and Max, writing for them ∇ Pot, $\nabla \times$ Pot, and $\nabla .$ Pot, and if I wished further to economise in what costs so little, I could give up the potential also by using the notation $(\nabla . \nabla)^{-1}$ or ∇^{-2}. That is, I could have used this notation without greater sacrifice of precision than quaternionic writers seem to be willing to make. I much prefer, however, to avoid these inverse operators as essentially indefinite.

Nevertheless—although my critic has greatly obscured the subject by ridiculing operators, which I beg leave to maintain are not worthy of ridicule, and by thoughtlessly asserting that it was necessary for me to use them, whereas they are only necessary for me in the sense in which something of the kind is necessary for the quaternionist also, if he would use a notation irreproachable on the score of exactness—I desire to be perfectly candid. I do not wish to deny that the relations connected with these notations appear a little more simple in the quaternionic form. I had, indeed, this subject principally in mind when I said two years ago in NATURE (vol. xliii. p. 512): "There are a few formulæ in which there is a trifling gain in compactness in the use of the quaternion." Let us see exactly how much this advantage amounts to.

There is nothing which the most rigid quaternionist need object to in the notation for the potential, or indeed for the Newtonian. These represent respectively the operations by which the potential or the force of gravitation is calculated from the density of matter. A quaternionist would, however, apply the operator *New* not only to a scalar, as I have done, but to a vector also. The vector part of New ω (construed in the quaternionic sense) would be exactly what I have represented by Lap ω, and the scalar part, taken negatively, would be exactly what I have represented by Max ω. The quaternionist has here a slight economy in notations, which is of less importance, since all the operators—New, Lap, Max—may be expressed without ambiguity in terms of the potential, which is therefore the only one necessary for the exact expression of thought.

But what are the formulæ which it is necessary for one to remember who uses my notations? Evidently only those which contain the operator *Pot*. For all the others are derived from these by the simple substitutions

$$\text{New} = \nabla\ \text{Pot},$$
$$\text{Lap} = \nabla \times \text{Pot},$$
$$\text{Max} = \nabla .\ \text{Pot}.$$

Whether one is quaternionist or not, one must remember Poisson's Equation, which I write

$$\nabla . \nabla\ \text{Pot}\ \omega = -4\pi\omega,$$

and in quaternionic might be written

$$\nabla^2\ \text{Pot}\ \omega = 4\pi\omega.$$

If ω is a vector, in using my equations one has also to remember the general formulæ,

$$\nabla . \nabla\omega = \nabla\nabla . \omega - \nabla \times \nabla \times \omega$$

which as applied to the present case may be united with the preceding in the three-membered equation,

$$\nabla . \nabla\ \text{Pot}\ \omega = \nabla\nabla .\ \text{Pot}\ \omega - \nabla \times \nabla \times\ \text{Pot}\ \omega = -4\pi\omega.$$

This single equation is absolutely all that there is to burden the memory of the student, except that the symbols of differentiation (∇, $\nabla \times$, $\nabla .$) may be placed indifferently before or after the symbol for the potential, and that if we choose we may substitute as above *New* for ∇ Pot, &c. Of course this gives a good many equations, which on account of the importance of the subject (as they might almost be said to give the mathematics of the electro-magnetic field) I have written out more in detail than might seem necessary. I have also called the attention of the student to many things, which perhaps he might be left to himself to see. Prof. Knott says that the quaternionist obtains similar equations by the simplest transformations. He has failed to observe that the same is true in my *Vector Analysis*, when once I have proved Poisson's Equation. Perhaps he takes his model of brevity from Prof. Tait, who simplifies the subject, I believe, in his treatise on Quaternions, by taking this theorem for granted.

Nevertheless, since I am forced so often to disagree with Prof. Knott, I am glad to agree with him when I can. He says in his original paper (p. 226), "No finer argument in favour of the real quaternion vector analysis can be found than in the tangle and the jangle of sections 91 to 104 in the 'Elements of Vector Analysis.'" Now I am quite ready to plead guilty to the tangle. The sections mentioned, as is sufficiently evident to the reader, were written at two different times, sections 102–104 being an addition after a couple of years. The matter of these latter sections is not found in its natural place, and the result is well enough characterised as a *tangle*. It certainly does credit to the conscientious study which Prof. Knott has given to my pamphlet, that he has discovered that there is a violent dislocation of ideas just at this point. For such a fault of composition I have no sufficient excuse to offer, but I must protest against its being made the ground of any broad conclusions in regard to the fundamental importance of the quaternion.

Prof. Knott next proceeds to criticise—or, at least, to ridicule—my treatment of the linear vector function, with respect to which we read in the abstract:—"As developed in the pamphlet, the theory of the dyadic goes over much the same ground as is traversed in the last chapter of Kelland and Tait's 'Introduction to Quaternions.' With the exception of a few of those lexicon products, for which Prof. Gibbs has such an affection, there is nothing of real value added to our knowledge of the linear vector function." It would not, I think, be difficult to show some inaccuracy in my critic's characterisation of the real content of this part of my pamphlet. But as algebra is a formal science, and as the whole discussion is concerning the best form of representing certain kinds of relations, the important question would seem to be whether there is anything of *formal* value in my treatment of the linear vector function.

Now, Prof. Knott distinctly characterises in half a dozen words the difference in the spirit and method of my treatment of this subject from that which is traditional among quaternionists, when he says of what I have called dyadics—"these are not quantities, but operators" (NATURE, vol. xlvii. p. 592) I do not think that I applied the word quantity to the dyadics, but Prof. Knott recognised that I treated them as quantities—not, of course, as the quantities of arithmetic, or of ordinary algebra, but as quantities in the broader sense, in which, for example, quaternions are called quantities. The fact that they may be operators does not prevent this. Just as in grammar verbs may be taken as substantives, viz. in the infinitive mood, so in algebra operators—especially such as are capable of quantitative variation—may be regarded as quantities when they are made the subject of algebraic comparison or operation. Now I would not say that it is necessary to treat every kind of operator as quantity, but I certainly think that one so important as the linear vector operator, and one which lends itself so well to such broader treatment, is worthy of it. Of course, when vectors are treated by the methods of ordinary algebra, linear vector operators will naturally be treated by the same methods, but in an algebra formed for the sake of expressing the relations between vectors, and in which vectors are treated as multiple quantities, it would seem an incongruity not to apply the methods of multiple algebra also to the linear vector operator.

The dyadic is practically the linear vector operator regarded as quantity. More exactly it is the multiple quantity of the ninth order which affords various operators according to the way in which it is applied. I will not venture to say what ought to be included in a treatise on quaternions, in which, of course, a good many subjects would have claims prior to the linear vector operator; but for the purposes of my pamphlet, in which the linear vector operator is one of the most important topics, I cannot but regard a treatment like that in Hamilton's "Lectures," or "Elements," as wholly inadequate *on the formal side*. To show what I mean, I have only to compare Hamilton's treatment of the quaternion and of the linear vector operator with respect to notations. Since quaternions have been identified with matrices, while the linear vector operator evidently belongs to that class of multiple quantities, it seems unreasonable to refuse to the one those notations which we grant to the other. Thus, if the quaternionist has e_q, $\log q$, $\sin q$, $\cos q$, why should not the vector analyst have $e\Phi$, $\log \Phi$, $\sin \Phi$, $\cos \Phi$, where Φ represents a linear vector operator? I suppose the latter are at least as useful to the physicist. I mention these notations first, because here the analogy is most evident. But there are other cases far more important, because more elementary, in which the analogy is not so near the surface, and therefore the difference in Hamilton's treatment of the two kinds of multiple quantity not so evident. We have, for example, the tensor of the quaternion, which has the important property represented by the equation—$\mathrm{T}(qr) = \mathrm{T}q\mathrm{T}r$.

There is a scalar quantity related to the linear vector operator, which I have represented by the notation $|\Phi|$ and called the *determinant* of Φ. It is in fact the determinant of the matrix by which Φ may be represented, just as the square of the tensor of q (sometimes called the *norm* of q) is the determinant of the matrix by which q may be represented. It may also be defined as the product of the latent roots of Φ, just as the square of the tensor of q might be defined as the product of the latent roots of q. Again, it has the property represented by the equation

$$|\Phi \cdot \Psi| = |\Phi||\Psi|$$

which corresponds exactly with the preceding equation with both sides squared.

There is another scalar quantity connected with the quaternion and represented by the notation $\mathrm{S}q$. It has the important property expressed by the equation,

$$\mathrm{S}(qrs) = \mathrm{S}(rsq) = \mathrm{S}(sqr),$$

and so for products of any number of quaternions, in which the cyclic order remains unchanged. In the theory of the linear vector operator there is an important quantity which I have represented by the notation Φ_s, and which has the property represented by the equation

$$(\Phi \cdot \Psi \cdot \Omega)_{\mathrm{S}} = (\Psi \cdot \Omega \cdot \Phi)_{\mathrm{S}} = (\Omega \cdot \Phi \cdot \Psi)_{\mathrm{S}}$$

where the number of the factors is as before immaterial. Φ_s may be defined as the sum of the latent roots of Φ, just as $2\mathrm{S}q$ may be defined as the sum of the latent roots of q.

The analogy of these notations may be further illustrated by comparing the equations

$$\mathrm{T}(e_q) = e^{\mathrm{S}q}$$

and

$$|e\Phi| = e\Phi_{\mathrm{S}}.$$

I do not see why it is not as reasonable for the vector analyst to have notations like $|\Phi|$ and Φ_{S} as for the quaternionist to have the notations $\mathrm{T}q$ and $\mathrm{S}q$.

This is of course an *argumentum ad quaternionisten*. I do not pretend that it gives the reason why I used these notations, for the identification of the quaternion with a matrix was, I think, unknown to me when I wrote my pamphlet. The real justification of the notations $|\Phi|$ and Φ_{S} is that they express functions of the linear vector operator *quâ* quantity, which physicists and others have continually occasion to use. And this justification applies to other notations which may not have their analogues in quaternions. Thus I have used $\Phi_\times$ to express a vector so important in the theory of the linear vector operator, that it can hardly be neglected in any treatment of the subject. It is described, for example, in treatises as different as Thomson and Tait's *Natural Philosophy* and Kelland and Tait's *Quaternions*. In the former treatise the components of the vector are, of course, given in terms of the elements of the linear vector operator, which is in accordance with the method of the treatise. In the latter treatise the vector is expressed by

$$\mathrm{V}\alpha\alpha' + \mathrm{V}\beta\beta' + \mathrm{V}\gamma\gamma'.$$

As this supposes the linear vector operator to be given not by a single letter, but by several vectors, it must be regarded as entirely inadequate by any one who wishes to treat the subject in the spirit of multiple algebra, *i.e.* to use a single letter to represent the linear vector operator.

But my critic does not like the notations $|\Phi|$, Φ_{S}, $\Phi_\times$. His ridicule, indeed, reaches high-water mark in the paragraphs in which he mentions them. Concerning another notation, $\Phi^{\times}_{\times}\Phi$ (defined in NATURE, vol. xliii. p. 513), he exclaims, "Thus 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 $\frac{1}{2}\Phi {}^{\times}_{\times} \Phi$.

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 $\Phi {}^{\times}_{\times} \Phi$, 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 ∇ applies as differential operator to σ, and ∇_1 to σ_1, σ and σ_1 being really identical in meaning, as also ∇ and ∇_1.) It will be observed that in my notation one dot unites in multiplication the two ∇'s, and the other the two σ's, and that I am able to leave each ∇ where it naturally belongs as differential operator. The quaternionist cannot do this, because the ∇ and σ cannot be left together without uniting to form a quaternion, which is not at all wanted. Moreover, I can write Φ for $\nabla\sigma$, and $\Phi : \Phi$ for $\nabla\sigma : \nabla\sigma$. The quaternionist also uses a ϕ, which is practically identical with my Φ (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 $\Phi {}^{\times}_{\times} \Phi$, would express $S\nabla\nabla_1 \; S\sigma\sigma_1$ in terms of this ϕ.

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 $(\Phi \times \rho)_\times$, he would, I think, find the operand very much in the way.

J. WILLARD GIBBS.

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[1]). 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.

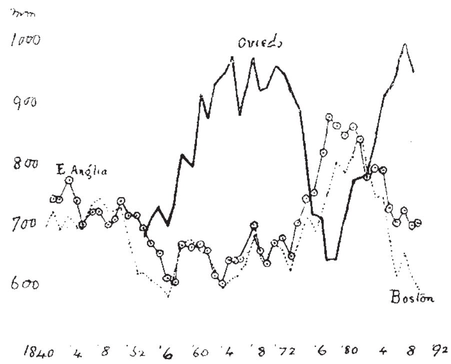
[1] The vertical scales, right and left, are not to be taken as equivalent.

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 versâ*. 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