

How Thought Presents Itself among the Phenomena of Nature

IN your paper of the 5th you give a short abstract of a recent lecture at the Royal Institution by Mr. G. Johnstone Stoney, on the question "How Thought presents itself among the Phenomena of Nature." In this abstract I observe an assertion which is quite new to me, and, I must add, quite unintelligible. It occurs in the first paragraph. The assertion seems to be that there is an absolute distinction between molar and molecular motion, inasmuch as that, in the case of molecular motion there is no authority for the conviction that there must be some "thing" to be moved. The conception of motion involves the conception of matter as a necessary or inseparable concomitant—although the abstract idea of motion may, in a sense, be separately entertained. Is there any difference in this respect between molar and molecular motion? A molecule is a group of atoms, and an atom is only conceivable as an ultimate particle of matter. I hope that some further explanation may be given upon this point, which is one of the highest interest and importance, both as a matter of physical and of metaphysical speculation.

Inverary, March 8

ARGYLL.

The Compound Vision and Morphology of the Eye in Insects

MR. SYDNEY HICKSON, in your issue of February 12 (p. 341), makes certain statements concerning my paper in the *Transactions* of the Linnean Society on this subject. I will not follow Mr. Hickson through his entire article, as I conceive it is sufficiently refuted by my paper itself. He says: "It would be tedious to bring evidence of this kind to confirm a theory which is already fully established." I would ask Mr. Hickson if anyone can explain the vision of the compound eye intelligibly on the received theory? I would also remind your readers that Prof. Huxley, writing of the crayfish in 1880, accepted the view with extreme caution; he said, "The exact mode of connection of the nerve fibres with the visual rods is not certainly made out;" that Claparède never accepted it, and Max Schultze admitted that there were grave physical difficulties in the way of its acceptance.

Mr. Hickson is very anxious, apparently, to deny me what I never claimed—*i.e.* the discovery of a layer of definite structure beneath the basilar membrane. What I do claim is the discovery of the nature of its elements. I deny, in my paper, that the optic nerve passes through these structures, and I deny that these consist of a fine reticulum of nerve-fibres. These are questions of fact and observation, not of theory or deduction. If I am wrong, I am wrong. But the way to test my work is by working out the eye as I have worked it out. I have spent nearly ten years in this work, and I do not expect to have my views generally accepted for another ten years.

The absence of pigment and retinal purple is a secondary question. I do not know, nor does any one know, whether there be retinal purple or not in this layer. I admit that pigment is absent in the retina (my retina) of some insects and crustaceans, and I have recorded the fact. I am not yet convinced that we can say vision is impossible without it. Albinos have vision undoubtedly in the absence of retinal pigment. He would be a bold man who asserted that vision could not be effected without pigment in the retinal region. The colourless collodion film of the photographer is affected; why not retinal rods? Here, again, it is a question of fact, not theory.

The presence of pigment proves nothing with regard to the function of the great rods, any more than it shows that the iris of a vertebrate is sensitive to light.

The absence of my retinal layer in *Periplaneta* and *Nepa* is imaginary on the part of my critic, for I have examined it carefully in both, and I figure the elements from the former. I maintain that the same structures exist in all the crustacea, although they are short and more difficult to demonstrate.

Again, in the morphological question my views are not fairly stated by Mr. Hickson. I admit his facts, but deny his deductions. The hypodermis forms the dioptric structures, as the epidermis of the vertebrate forms the lens; my contention is that the retina in the insect, like the same structure in the Vertebrata, is developed as an outgrowth from the nervous system.

BENJAMIN THOMPSON LOWNE

65, Cambridge Gardens, Notting Hill, W., February 23

I do not wish to undertake a lengthy controversy with Mr. Lowne on the question of the retina of insects, but I cannot refrain from making a few remarks on the letter you publish above.

I am afraid Mr. Lowne has misunderstood my criticism when he asks me "If any one can explain the vision of the compound eye intelligibly on the received theory?" My criticism was not meant for any theory of pure optics, but for the theory that the retinulae are not the true nerve-end cells.

Mr. Lowne's statement that albinos are devoid of retinal pigment is not strictly accurate, for Kühne pointed out, and any one can see for himself, that all albino rabbits and other vertebrates possess a true retina purple. Moreover, the rods of Cephalopods and of Pecten, which seem to be devoid of pigment in spirit specimens, possess, as Hensen has pointed out, a true retina purple. In fact, I know of no exception to the rule I laid down—namely, that optic nerve-end cells are pigmented, and I should be glad if any of your readers could point out any exceptions to it.

Mr. Lowne's reiterated statement that the optic nerve fibrils do not end in the retinulae is, as I said, contrary to my own observations. I have submitted my preparations to several eminent naturalists, who agree with me in my account of their distribution. I shall be happy to submit them to any others who may feel interested in this matter.

The other statements in my notice which Mr. Lowne controverts I will not refer to again here, as they will be fully explained and illustrated in my forthcoming paper in the *Quarterly Journal of Microscopical Science*, the proof-sheets of which I have now in hand.

SYDNEY J. HICKSON

Anatomical Department, Museum, Oxford, February 25

Civilisation and Eyesight

IN connection with Lord Rayleigh's letter in *NATURE*, p. 340, on the above subject, I venture to hope that the following may be of interest:—

In the "Expression of the Emotions" the late Mr. Darwin quotes some observations—if I recollect correctly—by Gratiolet tending to show that, under the influence of *fear*, the pupils of animals' eyes dilate. Observations extending over some years have convinced me that fear is undoubtedly capable of thus causing dilation of the pupils (see Dr. Hack Tuke, "Influence of the Mind on the Body"); and in general literature, such as travels, novels, &c., I have met with many instances in which the eyes of both men and animals under this condition have been so described by the writers.

Is dilation of the pupil under the influence of fear to be explained on the assumption that the increased aperture of the eye enables a more effective scrutiny of the object causing terror, and has thus been of service in the struggle for existence?

An answer to this question is not easy to give, for, although dilation of the pupil under the influence of fear may have originally been of direct service to an animal, yet this condition may in time have come to be associated with other emotions in which it is not so easy to trace any such direct benefit.

Observations upon the subject are by no means easy (varying light, for instance, varies the aperture of the eye), but in the course of my observations I became much inclined to believe that other strong mental emotions besides fear (*e.g.*, joy or pleasure) may be capable of giving rise to dilated pupils.

Charlotte Brontë, in "*Jane Eyre*," is one of the only writers who associates a dilated pupil with other emotions than fear. Here is the sentence:—"Pain, shame, ire, impatience, disgust, detestation, seemed momentarily to hold a quivering conflict in the large pupil dilating under his ebony brow."

It is to be feared that the experimental investigation of eyesight with artificially contracted or dilated pupils is scarcely practicable, for drugs, such as atropine or eserine, act not only on the pupil, but also on the power of accommodation for distance.

J. W. CLARK

Liverpool, February 21

P.S.—I see Dr. M. Foster, in his "*Text-Book of Physiology*," mentions the dilation or contraction of the pupil which attends the adjustment of the eye for distant or near objects respectively, and also its dilation "as an effect of emotions." It thus seems highly probable that strong and very different mental emotions may give rise to dilated pupil. Dr. Herdman has suggested to me, as an explanation of this, that an intense

excitation of one brain centre may possibly act in the same way as a direct inhibitory impulse by partially paralysing an adjacent centre.

The Forms of Leaves

THERE are several points in Sir John Lubbock's lecture (NATURE, February 26, p. 398) which seem to invite some little criticism. That "the size of the leaf . . . is regulated mainly with reference to the thickness of the stem" seems somewhat self-evident, as a large leaf must have a large stem to carry it, as, e.g., may be seen by comparing the slender shoot of a Dodard with a cabbage-stalk; but he adds: "The size once determined exercises much influence on the form." This is a *deduction* which seems to require *verification*. Sir John gives the area of a beech-leaf as about 3 square inches, but the form remains the same whatever the size. Size rather depends on vigorous growth, as in the following instances: *Populus alba* leaves on a vigorous basal shoot were $6\frac{1}{2} \times 3\frac{1}{2}$ inches, the diameter of the shoot being $\frac{1}{2}$ inch; on the upper branches of the same tree many leaves were only $1\frac{1}{2}$ to $2\frac{1}{2}$ inches long, the diameter of the shoot being also $\frac{1}{2}$ inch. Similarly growing oak leaves of the same shape were 6×3 inches and $2 \times \frac{3}{4}$ inches respectively. An *Aucuba japonica* bore rounded leaves on a basal shoot $4 \times 3\frac{1}{4}$ inches, but those on the stem were 4×1 inch. In this case, as in other plants with (normally) dimorphic leaves, as ivy, it is difficult to see what connection there is between size and form. Indeed leaves of every degree of superficial area can be found amongst the lobed ones on the climbing stem of ivy, and the entire ones of the flowering branch. Sir John adds that "the form of the inner edge [of the beech] . . . decides that of the outer one." He does not seem to have verified this deduction. The two edges are symmetrical in this leaf, but they are not so in the elm and lime. How will the inner edge explain the cause of their obliquity? If, however, the *buds* of the lime be examined, a more probable cause (as it seems to me) will be discovered in the conditions of development. He describes the *Eucalyptus*, when young, as having "horizontal leaves, which in older ones are replaced by scimitar-shaped phyllodes." Bentham and Hooker say of *Eucalyptus*: "Folia in arbore juniore saepe opposita, in adulto plerumque alterna," but makes no mention of phyllodes. Speaking of evergreen leaves, he says: "Glossy leaves have a tendency to throw [snow] off, and thus escape, hence evergreen leaves are very generally smooth and glossy." This sentence appears to imply that such leaves are glossy in anticipation of snow! a deduction which certainly requires verification. Again: "Evergreen leaves often have special protection . . . by thorns and spines. Of this the holly is a familiar illustration; and it was pointed out that in old plants above the range of browsing quadrupeds, the leaves tend to lose their spines and become unarmed." The inference the reader draws from this is that when the holly grows out of reach of browsing animals it has no necessity to produce prickly leaves, and so changes them accordingly, thereby implying that unarmed leaves were in some way preferable. This is another instance of deductive reasoning which requires verification, for it seems to be attributing to the holly a very unexpected process of ratiocination! But it is not at all usual for hollies to do this. I have several from six to nearly twenty feet high, and not one has borne an unarmed leaf. Though my cows do not touch a holly hedge, yet one young bush lately planted has taken their fancy, and they have bitten it all to pieces. On the other hand one bush (in the garden), a variety with unarmed foliage, occasionally throws out a branch with prickly leaves, though the cows are not admitted where it grows.

"Fleshy leaves were principally found in hot and dry countries, where this peculiarity [*sic*] had the advantage of offering a smaller surface, and therefore exposing the plant less to the loss of water by evaporation." Surely the usual explanation, that it is the thick cuticle which prevents rapid exhalation is a better reason than Sir John's deduction from the small size of the leaves? Speaking of aquatic plants, he says that the submerged "cut up" leaves of such plants presents a greater extent of surface; and adds that "such leaves would be unable to support even their own weight, much less to resist any force, such as that of the wind." I should be glad to know if he has verified the first statement by actual measurements; for an *à priori* assumption leads one to fancy that a complete leaf would have a greater surface than one represented by its ribs

and veins only. With regard to the second and third statements a "natural experiment" completely refutes his deduction, for I know a place where a small pond dried up last summer, and a large portion of the ground was covered with a dense velvet-like carpet, composed of the erect filiform branchlets of the "cut-up" leaves of *Ranunculus aquatilis*, which had become modified by their new medium, and perfectly adapted to enjoy an aerial existence.

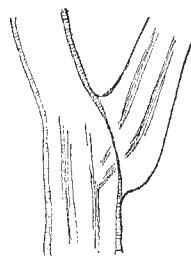
In offering these few criticisms for Sir John Lubbock's consideration, I would venture to remark that he seems to have followed too closely in the deductive methods of another writer on leaves, and which called forth the following remark from Prof. Lankester:—[He] "gives us hypotheses, suppositions with insufficient evidence, and deductions from the generalisation of Evolution, but he is relatively deficient in 'verification'" (NATURE, vol. xxviii. p. 171).

GEORGE HENSLOW

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The Fall of Autumnal Foliage

MR. FRASER alludes to "the unpursued inquiry into the cause of leaves falling in autumn" (NATURE, February 26, p. 388), and I do not find it mentioned in Sach's "Text Book"; but Dr. Masters, in Hensley's "Elementary Course of Botany," fourth edition, p. 515, speaks of "a layer of thin-walled cells being formed across the petiole," but does not say whence this layer is derived. Duchartre, however, gives a pretty full account of opinions up to 1877 ("El. de Bot.," deux. éd. p. 443), which he reduces to two, viz. Schacht's, who attributes it to a growth of periderm, and that of Mohls, who recognises a special layer which he calls *couche séparatrice*, considering the peridermic layer as being often, but not always formed. Subsequently, M. Ledgegauck



examined different plants and corroborated Schacht in regarding the periderm as the *cause prédisposante*, and cold to be the *cause efficiente*, which contracts "le tissu de la base du pétiole, spongieux, aéré, élastique à un degré beaucoup plus considérable que celui du coussinet." From my own observations on the horse-chestnut, ash, &c., it appears to be in these clearly a continuation of periderm produced by the phellogen of the branch, which invades the base of the petiole, till it meets in the middle, cutting right through the fibro-vascular bundles of the petiole. As this suberous layer dies, the leaf necessarily falls off. But as long as a leaf is in vigorous health it would seem to resist this invasion, and last longer, as do evergreens. I inclose a figure I possess of a slide showing the process in the horse-chestnut.

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GEORGE HENSLOW

Forest-Trees in Orkney

IN NATURE of February 26 (p. 388) Mr. A. T. Fraser says that "a peculiarity of Caithness and the Orkney and Shetland Islands is that no forest-trees can be got to grow," and he proceeds to explain this by the preponderance of polarised light. As far, at least, as Orkney is concerned, I am prepared to rebut this calumny. It is true that forest-trees are not the striking feature of the Islands, but they do occur. At Binscarth, between Kirkwall and Stromness, there are willow, ash, sycamore, and Scotch fir. They require to be protected—from the wind, I presume, and not from the light—by hedges of bour-tree (elder). In the street at Kirkwall itself there is a fair-sized sycamore.

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JAMES CURRIE

YOUR Indian correspondent, Mr. A. T. Fraser, can hardly be acquainted with the primitive jungles of Southern India, or he would have observed that there, at one and the same time, the aspect of all the four seasons is displayed in the vegetation.