

## Reviews of Books.

*Vegetable Teratology: an Account of the Principal Deviations from the usual Construction of Plants.* By MAXWELL T. MASTERS, M.D., F.L.S. London: Published for the Ray Society by Robert Hardwicke. 1869.

THE Ray Society has from time to time published many valuable works on general Natural History, and the work before us, which is that last issued, is by no means inferior to many of its predecessors. It is not now a question with us as to how far the Ray Society is carrying out its legitimate aims in publishing works on any other subject than British Natural History; that is a question which will ere long be very distinctly raised. But admitting that the publication of general treatises is justifiable on the part of the Society, we think that it was well to issue a work on so important a subject as that of Vegetable Teratology.

The study of the monstrosities of plants is a highly fruitful one, and at the present time, when so much is being thought of Mr. Darwin's opinions in connection with the variation of plants under domestication, the scientific study of teratology ought to furnish us with the clue to many mysteries. For such a task as that the Ray Society took in hand, no fitter candidate than Dr. Masters could have been found, and we can only regret that, in discharging that task, he has adopted a plan which, however useful it may be as supplying a record of valuable facts, leaves the science of teratology pretty nearly where it was before. What we mean is this, that in the elaborate and painstaking account which Dr. Masters has given us of the present knowledge of the phenomena of teratology, we find very little of that philosophical investigation of the processes of evolution which we should have expected from one so eminent in his profession as the author.

Following tolerably closely the plan laid down by the late M. Moquin-Tandon in his well-known treatise, Dr. Masters gives us a "piled-up" history of the multitudinous monstrosities which plants display throughout the whole of the group Phanerogamia. He supplies a great number of separate sections and classes, which possibly may be found convenient for reference, but which, in our opinion, are most unnecessary, and which are horribly perplexing to the student. We cannot help expressing our opinion very strongly on this point, for we feel that had Dr. Masters followed a simpler scheme of classification he would have done vastly more to popularize his subject than he has effected in the present volume. We must also object to the author's classification, on the ground that it is, so far as we can judge, extremely empirical, and without regard, in many instances, to the physiological laws of development. Indeed, all through the work we notice the tendency of the writer to ignore the study of those phenomena of development which can only be made out with the microscope, and which throw so much light on the relation between the axial and foliar parts of plants.

From this view of the subject, then, we are disposed to complain that Dr. Masters's book is not what it ought to have been. But then we must not forget that the title which Dr. Masters has given his work exonerates him from the charge of negligence, as it states that the work is, after all, only "An Account of the Principal Deviations," &c. As such, it

honestly fulfils its promise. It is a most exhaustive account, well written, easy of reference, and amply illustrated by Mr. E. M. Williams, and, much as it may be lacking in its history of teratological development, it must for the present be regarded as the highest authority on its subject.

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## Correspondence.

It is distinctly to be borne in mind that we do not, by inserting letters, convey any opinion favourable to their contents. We open our columns to all, without leaning to any; and thus supply a channel for the publication of opinions of all shades.

No notice whatever will be taken of anonymous communications.

We cannot undertake to return rejected communications.

### THE ORIGIN OF MORAL INTUITIONS.

SIR,—I do not think your correspondent "S. L." is justified in speaking so contemptuously of Mr. Hutton's original and well-reasoned article "A Questionable Parentage for Morals;" and I am inclined to think that Mr. Herbert Spencer himself would not claim for his views on this subject that demonstrable certainty which "S. L." seems to think attaches to them. The question really depends upon the more fundamental one, of whether man's entire mental and moral nature is the product and out-

come of that material organization whose laws of growth and development Mr. Spencer has so well elucidated. If mind with all its powers is simply a function of organized matter, then Mr. Spencer's theory of the origin of morals is the only one which can be held by a student of science. If, however, there is anything in man more than his physical organization, then it becomes a subject of strict scientific and philosophical inquiry to determine from a study of the phenomena of his mind in various stages of growth and under various conditions, what is the mental substratum required to account for the development of the faculties we actually find in him. Mr. Spencer maintains that an appreciation of utilities is all that is required to develop the moral sense; Mr. Hutton argues that this is insufficient, and that the moral sense itself, the appreciation of right and wrong, with a mental impulse towards the first and away from the second, is an essential part of the mental substratum of our nature.

To go into the question at all fully would be quite out of place here; I will therefore only adduce one group of facts which seem to me inexplicable on the utilitarian hypothesis. The utilitarian sanction for truthfulness is by no means very powerful or universal. Few laws enforce it. No very severe reprobation follows untruthfulness. In all ages and countries falsehood has been held permissible in love, and laudable in war; while, even to the present day, it is held venial by the majority of mankind in trade and commerce. A certain amount of untruthfulness is a necessary part of politeness in the east and west alike, while even severe moralists have held a lie justifiable to elude an enemy or prevent a crime. Such being the difficulties with which this virtue has had to struggle, with so many exceptions to its practice, with so many instances in which it brought ruin or death to its too ardent devotees, how can we believe that considerations of utility could ever invest it with the mysterious sanctity of the highest virtue,—could ever induce men to value truth for its own sake and practice it regardless of consequences?

Yet it is a fact that such a mystical sense of wrong does attach to untruthfulness, not only among the higher classes of civilized people, but among whole tribes of utter savages. Sir Walter Elliott tells us (in his paper "On the Characteristics of the Population of Central and Southern India," published in the *Journal of the Ethnological Society of London*, vol. i., p. 107) that the Kurubars and Santals, barbarous hill-tribes of Central India, are noted for veracity. It is a common saying that "a Kurubar always speaks the truth;" and Major Jervis says, "the Santals are the most truthful men I ever met with." As a remarkable instance of this quality the following fact is given. A number of prisoners, taken during the insurrection, were allowed to go free on parole, to work at a certain spot for wages. After some time cholera attacked them and they were obliged to leave, but every man of them returned and gave up his earnings to the guard. Two hundred savages with money in their girdles walked thirty miles back to prison rather than break their word! My own experience among savages has furnished me with similar, although less severely tested, instances, and we cannot avoid asking, how is it that in these few cases "experiences of utility" have left such an overwhelming impression, while in so many others they have left none? The experiences of savages men as regards the utility of truth must, in the long run, be pretty nearly equal. How is it that in some cases the result is a sanctity which overrides all considerations of personal advantage, while in others there is hardly a rudiment of such a feeling?

The intuitional theory explains this by the supposition that there is a feeling—a sense of right and wrong—in our nature antecedent to the independent of experiences of utility. When

line play is allowed to the relations between man and man, this feeling attaches itself to those acts of universal utility or self-sacrifice which are the products of our affections and sympathies, and which we term moral, while it may be, and often is, perverted to give the same sanction to acts of narrow and conventional utility which are really immoral,—as when the Hindoo will tell a lie but will sooner starve than eat unclean food, and looks upon the marriage of adult females as gross immorality.

The strength of the moral feeling will depend upon individual or racial constitution,—the acts to which its sanctions are applied will depend upon how far the simple feelings and affections of our nature have been modified by custom, by law, or by religion.

The question to be considered is, first, whether such an intense and mystical feeling of right and wrong (so intense as to overcome all ideas of personal advantage or utility) could have been developed out of accumulated ancestral experiences of utility; and, in the second place, whether feelings so developed by one set of utilities, could be transferred to acts of which the utility was partial, imaginary, or altogether absent.

Although myself an enthusiastic admirer of Mr. Spencer's writings, and a follower of his philosophy, I am decidedly of opinion that there is a limit to the sphere which that philosophy embraces, and that the limit is to be found in the doctrine of the origin of morals.

I remain, &c.,

ALFRED R. WALLACE.

FORAMINIFERA IN MINERAL VEINS.

SIR.—In a report on "Mineral Veins and their Organic Contents in Carboniferous Limestones," at the recent meeting of the British Association at Exeter, I gave a list of about 185 species I had found in the lead veins of the south-west and the north of England.

It is interesting to observe that when a key is once obtained to open up a particular line of investigation how soon our knowledge in that direction may be extended. Thus the discovery of a solitary seed of a minute coniferous plant, the *Flemingites gracilis* (Carr), in one of the Yorkshire veins, enabled me soon to detect its presence in the Staffordshire coal-field, and since then to find that whole beds in the Somersetshire area are almost composed of it.

In like manner our knowledge of another class is being extended. In my report I alluded to the discovery in the mineral veins of a series of Foraminifera, which I had placed in the hands of my friend Mr. H. B. Brady, and which were described at greater length by him. Amongst these were some nearly spherical bodies more or less drawn out at their two poles, as though they had formed portions of a moniliform test. In one example two or three of these are attached end to end, but the connecting portion was so attenuated that the mere contraction of the cement used in mounting them was enough to sever it. It was suggested by Mr. Brady that these were joints of a large *Lituola*, and he has provisionally named it *L. gigantea*, but the number of specimens obtained from the Alston veins was too limited to allow of a definite or detailed description.

Those interested in this portion of my report will be glad to learn that Mr. Brady has since received, from Sir Walter Trevelyan, a portion of a slab of carboniferous limestone, almost entirely composed of the organisms in question, which is important as evidence of the age of the microzoa from certain of the mineral veins named in my paper.

Whether the fossil really belongs to the genus *Lituola*, or may not be yet another phase of that wonderfully polymorphic type *Involutina* which occurs with it, is a matter upon which my friend is at present engaged.

As in the case of the *Flemingites gracilis* of the coal-measures, so we have here a bed of mountain limestone, of considerable extent and thickness, almost entirely formed of these minute organisms. The subject is, I think, sufficiently remarkable to merit more than a passing notice, and I hope we shall soon have from Mr. Brady a detailed account of the investigations in which he is engaged.

I remain, &c.,

CHARLES MOORE.

ON THE ASSERTED OCCURRENCE OF PYGOPTERUS IN THE NEWSHAM COAL-SHALE.

SIR.—As it has been stated in SCIENTIFIC OPINION on more than one occasion, and specially in a communication in your last issue, that the genus *Pygopterus* occurs in the black shale of Newsam, would you allow me to state, for the benefit of your readers, that this genus has not yet been observed in this district by any competent and experienced palaeontologist?

And further, I may be permitted to remark that the statement of your correspondent, that "the evidence adduced in favour of the tipped teeth being in the jaws of Palaeoniscus is not satisfactory," is contrary to fact. It has been established by Messrs. Hancock and Atthey, in the *Annals of Natural History*, that these enamelled-tipped teeth have been not only found in jaws attached to entire specimens of undoubted Palaeonisci obtained from Newsam, but also that these teeth so attached are identical with those frequently occurring in the same locality, scattered in the matrix. All this has been demonstrated with the greatest care and certainty, and yet your correspondent ventures to ignore these observations in the most unjustifiable and reckless manner.

I remain, &c.,

RICHARD HOWSE.

17, Saville-row, Newcastle-upon-Tyne, Sept. 10.

MR. ATHEY v. MR. BARKAS.

SIR.—I am as little disposed as is Mr. Henslow to enter into controversy respecting the merits or demerits of either Mr. Atthey or Mr. Barkas, and had the writers of the paper which was read before the members of the Tyneside Naturalists' Field Club (see p. 197, ante) been equally indisposed to criticize disparagingly, and, as I think, unfairly, opinions that had been published respecting a fossil in my possession, no word of criticism or censure would have flowed from my pen.

It is impossible, without occupying more of your space than you would be disposed to appropriate to the explanation, to give the leading transactions which preceded the present controversy, and, at the risk of having my position misunderstood, I refrain from doing so.

There are a few statements in the letter of your correspondent to which, with your permission, I shall briefly refer. He states that Mr. Atthey's collection is finer than mine, that it contains specimens unknown to me, and that for several years it contained specimens of *Climaxodus* similar to that I described in the *Geological Magazine*, November, 1868.

Not having seen Mr. Atthey's collection for 18 months, I cannot of course say what it contains, but in all probability, it does contain specimens unknown to me, as mine probably contains specimens unknown to him. Mr. Atthey's collection is unquestionably finer than mine, it having been the result of many years' labours, while mine is the product of about as many months. So far as *Climaxodus* is concerned, as I was not permitted to know the contents of Mr. Atthey's cabinet, and as *Climaxodus* as a fossil of our local coal-measures had never been described, I felt myself perfectly justified in describing it when and where I thought proper.

I have no wish whatever to do either Mr. Atthey or any one else an injustice, and I believe it is quite true that, in addition to being a collector, he has a very excellent practical knowledge of the fossils of the coal-measures, but he is in reality the collector and preparer of the fossils of which Mr. Hancock is the describer.

I may be permitted to state that when Mr. Henslow saw my cabinet it was without classification, in fact, I was receiving from the collieries weekly, from 10 to 20 boxes of shale containing fossils, and my entire leisure time was occupied in splitting up the shale, extracting the fossils and sending off duplicates as presents to various parts of the kingdom, with a view to reducing my stock preparatory to classification.

My cabinet is now more perfectly arranged than it was when seen by Mr. Henslow in November last, it contains specimens which I have reason to believe are unique, and it is open to the inspection of Mr. Henslow or any other palaeontologist who may do me the honour to call and inspect it.

I remain, &c.,

Newcastle-on-Tyne, Sept. 3.

T. P. BARKAS.

METEOROLOGY OF AUGUST, 1869.

SIR.—I beg to send you some particulars of the meteorology of the last month, deduced from observations conducted by myself in this inland neighbourhood.

The circumstances of atmospherical temperature and moisture have reference to a stratum of air 4 ft. from the ground.

The "radiating" thermometer rests on a horizontal board one foot above the grass, and is freely exposed to the sky.

The top of the rain-gauge is one foot from the ground.

The barometer is 335 ft. above sea-level, and its readings are corrected for temperature.

GENERAL RESULTS.

Mean maximum temperature of the air ...	68.2°
Mean minimum temperature of the air ...	49.2°
Mean daily range of temperature of the air	19.0°
Greatest range in 24 hours of temperature of the air, on the 31st .....	28.1°
Least range in 24 hours of temperature of the air, on the 29th .....	3.7°
Highest temperature of the air, on the 28th	80.5°
Lowest temperature of the air, on the 31st	31.8°
Total range of temperature of the air during the month .....	49.7°
Lowest reading of the "radiating" thermometer, on the 31st .....	23.0°
Mean temperature of the air .....	59.6°
Mean temperature of the dew-point .....	52.6°
Mean humidity of the air .....	78°
Mean weight of water in a cubic foot of air .....	4.5 gra.
Highest 9 a.m. reading of the dew-point, on the 27th .....	65.0°
Lowest 9 a.m. reading of the dew-point, on the 30th .....	42.0°
Mean height of the barometer .....	29.840 in.
Highest observed reading of the barometer, on the 31st .....	30.090 in.
Lowest observed reading of the barometer, on the 9th .....	29.310 in.
Observed range of the barometer .....	0.780 in.
Rainfall .....	2.300 in.
Greatest fall of rain in 24 hours, on the 1st .....	0.380 in.
Number of days on which rain fell .....	12
all of which were in the former half of the month.	

The temperature of the air rose above 70° on nine days; above 75° on four days; and up to or above 80° on three days.

On the other hand, it fell below 45° on four nights, and once below the freezing point of water. There was a sharp frost on the morning of the 31st; when the temperature of exposed vegeta-