

## LETTERS TO THE EDITOR.

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## Archebiosis and Heterogenesis.

THE columns of the daily papers have during the last two weeks contained many references to the question of the origin of life. One of the most recent utterances has been that of Lord Kelvin, who has roundly declared himself an unbeliever in the natural origin of living matter either in the present or in the past. We must suppose, therefore, that in reference to this question he is content to believe in miracles.

Prof. Ray Lankester and Dr. Chalmers Mitchell, however, proclaim themselves, as followers of Huxley, believers in evolution generally, and in the natural origin of living matter in the past. They, like many others, refuse to believe that it takes place at the present time, because undoubted proof of its occurrence cannot be produced by laboratory experiments. The uniformity of natural phenomena would certainly lead us to believe, as Sir Oliver Lodge has intimated, that if such a process occurred in the past, it should have been continually occurring ever since—so long as there is no evidence to show cause for a break in the great law of Continuity. Certainly no such evidence has ever been produced, and if the origin of living matter takes place by the generation in suitable fluids of the minutest particles gradually appearing from the region of the invisible, such a process may be occurring everywhere in nature's laboratories, though altogether beyond the ken of man.

My point may be illustrated thus. Bacteriologists all over Europe and elsewhere have been working for the last thirty years by strict laboratory methods, and notwithstanding all that they have made out and the good that has thereby accrued to suffering humanity, they have apparently never yet seen the development from Zoogloea aggregates of Fungus-germs, of flagellate Monads, or of Amœbæ. If, however, they would only examine what goes on in nature's laboratory when a mixed bacterial scum forms on suitable fluids, they would have no difficulty in satisfying themselves as to the reality of these processes. I described such processes in your columns in 1870, more fully in the *Proceedings* of the Royal Society in 1872, and finally in my "Studies in Heterogenesis" (pp. 65-84, pls. vi. and vii., Figs. 53-71). Even during the last week I have again obtained photomicrographs demonstrating the origin of flagellate Monads from Zoogloea aggregates forming in a bacterial scum, and if you will admit an illustrated communication on this subject to your columns, proving by such a test case my position as to the reality of heterogenesis, I shall be happy to present it, and to show that something beyond the recognised strict laboratory methods of the day is needed if we are to fathom some of nature's deepest secrets.

The councils of the Royal and Linnean Societies are guided in the acceptance of papers by referees who are wedded, on biological questions, to laboratory methods. It is useless for me, therefore, again to attempt to submit such a communication to them. Their referees (probably not having worked at such subjects themselves) would not advise the acceptance of the paper, and my communication might simply be consigned to their archives. The Royal Society "for the Promotion of Natural Knowledge" on two occasions would not even allow me to submit my views to the consideration of, and discussion by, its fellows. In these circumstances, Sir, I appeal to you, in the interests of science, to allow me to send you an illustrated paper proving, so far as such proof can go, the heterogenetic origin of flagellate Monads and of Fungus-germs.

H. CHARLTON BASTIAN.

Manchester Square, October 31.

[In reply to Dr. Bastian's appeal we will print his communication, and also any important replies from competent workers on the subject which may be sent to us.—ED.]

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## Average Number of Kinsfolk in each Degree.

I was glad to read the first paragraph of the reply by Prof. G. H. Bryan to my letter, in which he acknowledges his mistake, but I cannot allow the second paragraph to pass without protest, in which he says "the discrepancy can be accounted for more simply still" in a way he describes. I do not wholly understand his present view, but only enough of it to be assured that it is vitiated by some fundamental misconception. In these circumstances it is best to re-state my original argument in different words. We agree to start on the assumptions that boys and girls are on the average equally numerous, and that all other conditions are to be ignored. Then, if an individual be taken out of a family of  $2d$  children,  $2d-1$  children will be left, of whom  $d-\frac{1}{2}$  will, on the average of many experiences, be girls and  $d-\frac{1}{2}$  will be boys. The sex of the individual who was taken out in the first instance is quite unimportant; the result will be the same whether that individual be a boy or a girl.

Prof. G. H. Bryan thinks, if I understand him rightly, that the sex of the individual in question is of importance.

Some persecuting demon must have again caused my pen to write and my eye to overlook an absurdly erroneous figure in my last letter. The faulty passage runs "... is 80 ( $=2\frac{1}{2} \times 16$ , as it should be)"; the 16 ought to be replaced by 32. It is intended to be quoted from the right hand column of line (5) in the table which accompanies that letter.

FRANCIS GALTON.

## Misuse of Words and Phrases.

IN the preface to my book on "Cubic and Quartic Curves" I have stated my views on the matters referred to in the last paragraph of T. B. S.'s letter. I am a strong advocate of the use and, if necessary, the invention of words of classical origin to express new ideas, and I consider the phrase *self-cutting* inelegant.

My objection to the phrase non-singular cubic or quartic curve is that no such curves exist, since Plücker has shown that all algebraic curves, except proper conics, possess a determinate number of singularities. Thus anautotomic quartics possess 52 simple singularities, viz. 28 double and 24 stationary tangents. It is also possible for such curves to possess compound singularities, formed by the union of one double and two stationary tangents.

With regard to the use of *an*, the rule is that before a word beginning with a vowel *an* is to be used instead of *a* for the sake of euphony, but when a word beginning with a vowel is pronounced as if it commenced with a consonant, *a* must be used instead of *an*. The phrases *such an one*, *a uniform rod*, *a wonderful sunset*, *a yew tree*, are all equally incorrect.

A. B. BASSET.

November 4.

## The Coming Shower of Leonids.

THE pretty abundant shower of Leonids witnessed last year encourages the hope that a fairly rich return may be observed this year. There will be no moonlight to interfere with the brilliancy of the display should it occur, and the most probable time of its apparition will be before sunrise on November 15.

In 1903 the maximum occurred between 5 and 6 a.m. on November 16, and, allowing for leap year, the ensuing maximum should take place on November 15 at about noon. The shower seems likely to be observed to the best advantage at American stations, as in 1901, but it should be carefully watched everywhere, and with a special view to ascertain the hour of greatest abundance.

It is to be hoped that some further attempts will be made to determine the place of the radiant by photography. We have already a sufficient number of eye observations of the position, and the work of ordinary observers will be better directed to counting the number of meteors visible at regular intervals during the night, and registering the most brilliant objects. The meteors from other showers should also be noted, and especially any conspicuous Taurids that may appear. The latter by their slow long flights and yellow trains are readily to be distinguished from the swiftly moving Leonids with their green streaks.

W. F. DENNING.

OWING to the large numbers of shooting stars visible on the night of November 15, 1903, the expectation of witness-

ing a meteoric spectacle on perhaps a more extensive scale will probably be revived on the near approach of the Leonid epoch of 1904. Reasons have already been given for supposing that last year's display was connected by the nineteen years' period with a very similar phenomenon observed on November 13, 1865, the interval between the two events representing two complete revolutions of the meteoric cycle. The present epoch, therefore, which is thus associated with the historic meteor shower of November 14, 1866, will be liable to reproduce its brilliant prototype, though only to a limited extent.

The anticipated shower, however, if it takes place, will not occur on the night of November 14, as it might naturally have been expected to do, owing to 1904 being a leap year. The meteor-swarm, according to calculations made by the present writer, has undergone considerable retardation since 1903, and as a result of this perturbation the Leonid meteor shower becomes due in 1904 on the night of November 15. It is on the latter night, therefore, that the maximum will take place, whether it culminate in a shower or not. There will occur, however, on November 14, 15h., an interesting miniature meteor display. The shower on the night of November 15, though not so intense, will be more extensive than that of 1866, as maxima fall due at 9h., 12h. to 15h., and 17h. 30m. G.M.T.

JOHN R. HENRY.

### The Definition of Entropy.

FROM time to time controversies have appeared in various journals regarding that most difficult of all physical conceptions—entropy. I have purposely avoided passing any opinions as to the merits of the views of different writers, as I have considered the question far too large a one to be dealt with satisfactorily by destructive criticism directed towards particular points. I have, however, now found a definition of entropy which certainly appears to meet most of the objections to the conventional treatment. That definition may be stated somewhat as follows:—

Let the available energy of any system at any instant relative to a refrigerator of temperature  $T_0$  be defined by the condition that it is the maximum amount of energy that could be obtained from the system at that instant by reversible thermodynamic engines working between the system and the refrigerator  $T_0$ , the remaining portion of the energy being, of course, called non-available energy. Then in any change of the system the increase of entropy is the quantity obtained by dividing the increase of non-available energy by the temperature  $T_0$  of the refrigerator.

I hope to publish a detailed treatment shortly, but in the meantime I would mention that this definition overcomes all the difficulties inherent in the conventional treatment of at least the more ordinary irreversible phenomena, such as friction, impact, gas rushing into a vacuum.

If we adopt the principle of degradation of energy as the fundamental second law of thermodynamics (as I suggested in the Boltzmann *Festschrift*), Clausius's statement that the entropy of the universe tends to a maximum now follows at once. So, too, do his inequalities. For every irreversible transformation in the interior of a system produces loss of available energy, and therefore (since it does not affect the total energy) increase of non-available energy, and therefore increase of entropy. We may say that entropy can be generated, but never destroyed. It follows that the total increase of entropy in the system is greater than the quantity of entropy entering from without. This is Clausius's inequality for an irreversible non-cyclic process. If the process is cyclic the total gain of entropy is zero, and therefore the entropy generated in the system must be exported during the cycle. This is Clausius's inequality for a cyclic process.

The introduction of the refrigerator presents no real difficulty. If non-available energy, instead of being given to the refrigerator  $T_0$ , is worked down reversibly to a refrigerator at a lower temperature  $T_1$ , its amount will be decreased in the ratio  $T_1 : T_0$ .

G. H. BRYAN.

### The Direction of the Spiral in the Petals of *Selenipedium*.

In *Selenipedium grande*, *S. longifolium*, and *S. conchiferum*, the twisted petals are so arranged that the direction of the spiral is right-handed on each side.

They are not heteronymous, i.e. the right petal with a left twist and the left petal with a right twist, as in all

antelopes' horns, nor are they arranged homonymously, as in most sheep's horns,<sup>1</sup> but the twisted petals have the same direction on each side, and in the cases above mentioned the right-handed spiral is always present. In trying to find a cause for the direction, I expected it to appear that before and during the unfolding of the flower the petals were twisted when lying together, and thus took the bias, which continued during growth. If two strips of paper be laid together and twisted into a pipe-lighter, each, when separated, would exhibit the same spiral twist.

Examination of the still-folded flower proves that this simple explanation is not the true one, and, at least in *S. grande*, the petals are straight when they show at first (two inches or more in length), and become afterwards spirally twisted during growth and elongation.

The necessary bias to determine the direction of the spiral evidently acts after the unfolding of the flower, and is a slight force acting continuously during growth, such as would be made by the circulation if there were a difference in the circulation of the sap in the two edges of each petal.

This difference would act alike in each, and would make each petal twist in the same way; but, of course, this is a mere conjectural suggestion.

GEORGE WHERRY.

Cambridge, October 30.

### Thinking Cats.

THE story of the cat that saved the cook, in your last issue, is certainly remarkable, but surely it is not unusual for cats to find out how to direct attention when they want to get into or out of a house, or for them to conceal their kittens in curious places.

Two instances of the former occur to me among many. A cat in my father's house used to rattle the letter-plate at the front door (it was in a window near the door) whenever it was shut out, and another, in my own house, would come to any lighted window, even on the top storey, and tap at the glass if it was shut out at night. In the same house a cat hid its kittens, after one family had been destroyed, under the boards of a lead flat, so that, as they grew, it could not get them out, and directed our attention to them by running backwards and forwards. They were released by taking up the boards.

From cats to birds seems a natural transition. I have a curious instance, at this moment, of a pair of robins mistaking their own importance. Last spring they built, and reared their family, in a hole in the wall of an old country mansion, which was being rebuilt under my supervision. The wall was inside the house, in the great hall, and the female sat on her nest, looking out at the workmen, amid all the noise and disturbance of building. They disappeared in the summer, but now that the house is finished and occupied, the pair have returned, and flit about the same hall and the adjoining drawing-room, evidently under the impression that the house was built for them.

R. LANGTON COLE.

### Change in the Colour of Moss Agate.

A FRIEND of mine possesses a penholder the handle of which is made of moss agate. Originally the colour of the handle was bluish throughout, but recently the upper part of the handle has become very much lighter in colour and much more transparent.

I thought perhaps some of your readers could tell me whether it is usual for moss agates to undergo changes of this kind after having been cut and polished, and, if it is usual, to what agent or agents the change is ascribed.

W. A. WHITTON.

County School, Bridgend, November 7.

### The Origin of Life.

MR. HOOKHAM ingeniously argues that experiments to evolve living out of non-living matter are inconclusive and must probably always fail because the sterilising agent used, which is commonly heat, "eliminates not only life, but its potentiality at one stroke."

Most of us believe that the earth was at one time an incandescent globe. Neither life nor the potentiality of life could have existed in such circumstances. How would Mr. Hookham, on the theory of evolution, explain their first introduction?

GEOLOGIST.

<sup>1</sup> NATURE, December 12, 1901; *Lancet*, January 1, 1898.