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-