

LETTERS TO THE EDITOR.

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Sir Edward Fry on Natural Selection.

I ASK leave to make a few observations on Mr. Galton's letter under the above heading which appeared in your issue of February 12.

In my papers on the age of the inhabited world and the pace of organic change in the *Monthly Magazine* for last December and January, I had a passage on the difficulty which appeared to me to exist in conceiving mimetism to have been produced by the gradual accumulation of minute points of likeness. On this Mr. Galton observes that "two objects that are somewhat alike will be occasionally mistaken for one another when the conditions under which they are viewed are unfavourable to distinction." If by "somewhat alike" Mr. Galton means have some point of likeness, however minute, then the proposition would refute my objection; but it would, as I think, be manifestly untrue. If, on the other hand, by "somewhat alike" be meant a considerable likeness, then the proposition is manifestly true, but leaves unanswered the difficulty on which I have dwelt, viz. the difficulty of seeing how natural selection could have helped the organism to convert minute points of likeness in the midst of unlikeness into such a preponderance of likeness as to produce deception.

Mr. Galton has illustrated his point by the fact that "i" may often be mistaken by the beholder for "l," "k," or "h." But here he starts with an obvious and considerable likeness, and the question is, how could that degree of likeness be reached by natural selection?

Let us take two sheets of paper, the one a *tabula rasa*, the other covered with a thousand dots arranged so as to produce a highly complicated pattern. Then let dots appear successively, but sporadically, on the white paper in places where there are dots on the other paper, until, in the end, the two papers are indistinguishable. It seems to me to be obvious that for a long while no eye would mistake the one paper for the other; but that, as the process goes forward, a point will be reached where an occasional mistake will occur under conditions unfavourable to distinction. Now I agree that it is conceivable that from this point forward natural selection may operate, but as to the whole interspace between the first minute change that deceives no one to the point of first deception, it appears to me plain that natural selection cannot operate at all, and that the theory of the accumulation of minute variations, therefore, fails to account for the facts of mimetism in insects and other organisms.

If the two suggestions of sudden and great variation on the one hand, and of the slow accumulation of small variations on the other be considered as the possible explanation of the facts of mimetism, I cannot but think that the latter will be found far more probable than the former; and therefore, whilst willingly admitting the great weight to be attributed to the opinion of Mr. Galton on the subject, I remain unconvinced.

But suppose that on this point I am wrong and Mr. Galton is right, does he not judge my argument with undue severity when he treats it as "so faulty as to seriously compromise the value of the memoir as a whole"? My observations on mimetism are not the basis of my argument, which is a collection of facts which appear to show the existence of sudden and heritable variations. They are a part, and a separate part only, of an argument that the accumulation of minute variations will not account for some known facts attributed to it. The inculpatory paragraph may be struck out of my paper, and all the rest will stand unaffected. Even if this error, if error it be, has compromised not a single passage only but the whole of my paper, I am glad to find that Mr. Galton is in sympathy with its general purport, and I thank him for the courteous language which accompanies his condemnation of my lapse.

Failand, February 23.

EDW. FRY.

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The Assumed Radio-activity of Ordinary Materials.

WITH reference to Mr. Strutt's recent article and Prof. J. J. Thomson's letter on this subject, may I venture modestly to urge that it may be well to consider whether the condition set up in air to which attention is directed be not the outcome of the occurrence of a minute amount of chemical change of an ordinary character—whether it be not a sort of Russell effect on an infinitely minute scale, detected by an infinitely delicate test? That oxidative change is in continual progress, I imagine, is the belief of everyone who has paid the slightest attention to the subject; and that leaf surfaces—if not waterfalls—are the certain seat of such changes may be regarded as unquestionable. Those of us who require something more than an attitude of papal infallibility in proof of a scientific proposition would like to see the old love honourably retired before the new one is accepted in society.

HENRY E. ARMSTRONG.

The Dissociation Theory of Electrolysis.

IN a recently published "Text-book of Electrochemistry," by Svante Arrhenius, and translated by Dr. McCrae, I find on p. 114 of the translation the following statements:—

"Even when working with polarisable electrodes . . . the smallest fall of potential is sufficient to cause a current in the liquid. This fact was proved by Buff with currents so small that it was only after months that a cubic centimetre of explosive mixture was obtained. According to this the very smallest force is sufficient to split the molecules of the Groththus chain . . . Faraday's view is therefore incorrect. The radicles of the salt molecule cannot be held together by a force of finite value."

The ideas of current and electromotive force are here rather mixed, but obviously the passage refers to a very necessary part of the foundation of the dissociation theory of electrolysis, viz. that a minute E.M.F. can evolve in the free state the ions of an electrolyte the heat of combination of which is considerable.

On referring to Buff's papers (*Lieb. Ann.*, lxxxv. p. 1 and xciv. p. 1), I find no mention of an evolution in the manner described of any explosive mixture whatever; taking this to mean free oxygen and free hydrogen evolved simultaneously by an E.M.F. less than that of one Daniell's cell. Indeed, towards the end of his second paper, Buff incidentally states that a single cell produces merely a polarisation which almost stops the current.

Surely in the attempt to found a theory in opposition to that of Faraday some modicum of care should be taken to verify the sources of information.

In "Outlines of Electrochemistry," by Prof. Harry C. Jones (1901), we find at p. 15 the same kind of statement, that the dissociation theory accounts for, and is founded on, experimental evidence, showing that "a very weak current" can decompose water and set free its constituents simultaneously. Here also the word "current" is used, though "E.M.F." is apparently meant.

No reference is given, but the statement occurs in a discussion of the well-known Clausius theory. In his description of this theory (*Pogg. Ann.*, ci. p. 338), Clausius certainly does not mention, and apparently did not believe, that any such phenomenon could occur.

It would be interesting to know if anyone has ever observed it.

At all events, the acceptance of the theory in question is certainly not encouraged by an encounter with such serious errors in the description of experiments put forward as its foundations.

J. BROWN.

Analysis of the "Red Rain" of February 22.

SOME of your readers will probably be interested to know something of the nature of the muddy rain which fell here on Sunday, February 22. A sample of the downfall, caught in an open field between 10 a.m. and 12 noon, was brought to me to examine, and particulars of the partial analysis of the suspended matter which the water contained are subjoined. The large percentage of organic matter seems to me to be the most remarkable point in the analysis, and I regret not having had time to make a separate investigation of this. A rapid examination of the physical proper-