Letters to the Editor.

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Interspecific Sterility.

THE implications of modern genetics have been so little considered by biologists in this country that the criticism of my address by Dr. Cunningham (NATURE, June 17), though in purpose destructive, is not unwelcome. Of the points he raises one chiefly calls for reply. I directed once more the attention of naturalists to the fact that we still await the production of an indubitably sterile hybrid from completely fertile parents which have arisen under critical observation from a single common origin. So far as our knowledge goes, all the domesticated races—for example, of dogs, of pigeons, of fowls among animals; and of cabbages, of peas, of *Primula sinensis*, and many more among plants—when intercrossed among themselves never produce this sterility in their mongrels, though the races are often distinct enough to pass for species. But if we begin crossing natural species, even those which on our reckoning must be very closely allied, we constantly find either that they will not interbreed, or that, if they can be crossed, the result is more or less sterile. Dr. Cunningham takes exception to my speaking of this interspecific sterility as the chief attribute of species, but he will not dispute that it is a chief attribute of species.

The races of fowls might, as he holds, on account of their enormous divergences, be without impropriety compared to natural species. They may also, as he thinks, all descend from *Gallus bankiva* (though I find that difficult to believe); but inasmuch as they do not show interspecific sterility they do not help us to understand how that peculiar property of species arose in evolution. In contemporary variation we witness the origin of many classes of differences, but not this; yet by hypothesis it must again and again have arisen in the course of evolution of species from a common ancestry. The difficulty is no new one; but I emphasised it because naturalists should take it more seriously than they have done hitherto. Especially now that a great deal of experimental breeding is in progress, watch should be kept for such an occurrence. I by no means declare that the event cannot happen, but, so far as I know, it has not been witnessed yet.

Dr. Cunningham tries to fill the gap by adducing two instances. The first is that of *Oenothera gigas*. Now I had not forgotten the tetraploids, which so often do not breed freely with diploids, but the applicability of that example is exceedingly doubtful. Interspecific sterility or incompatibility may well be a consequence of nuclear diversity, though we can scarcely regard an unresolved pair of twins, such as the tetraploid must be, as a specifically distinct organism.

His second illustration, if authentic, would be more nearly what is wanted. He says that "two mutants of *Drosophila* in Morgan's experiments are almost completely sterile with one another." The allusion is probably to a paper of Metz and Bridges (Proc. Nat. Acad. Sci., 1917, iii. p. 673), in which they claimed to have found two mutants of *D. virilis* which gave partially sterile hybrids when intercrossed. Dr. Cunningham is not perhaps aware that this claim

was afterwards withdrawn (Metz, *ibid.*, 1920, vi. p. 421), inasmuch as one of the mutants was found to have been partially sterile. Metz and Bridges did adduce another example in *D. ampelophila*, but for a variety of reasons that, even if substantiated, would scarcely be to the point. As a matter of fact, however, in so far as opportunity of repeating the cross has occurred, complete fertility resulted. I know of no other example to which Dr. Cunningham can be referring.

Mr. Crowther (NATURE, June 17, p. 777) mistakes my meaning. It is, as he says, not difficult to "imagine" interspecific sterility produced by a gradual (or sudden) modification. That sterility may quite reasonably be supposed to be due to the inability of certain chromosomes to conjugate, and Mr. Crowther's simile of the sword and the scabbard may serve to depict the sort of thing we might expect to happen. But the difficulty is that we have never seen it happen to swords and scabbards which we know to have belonged originally to each other. On the contrary, they seem always to fit each other, whatever diversities they may have acquired.

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Geology and the Nebular Theory.

I DO not deserve the reproaches of Prof. Coleman (NATURE, June 17, p. 775). My molten earth did not, in point of fact, owe its thermal energy to primitive condensation, but to accumulated radioactive heat, as the concluding part of my lecture might have shown. I am committed neither to the nebular theory nor to the planetesimal theory.

Had I adopted the wider definition of the Archæan favoured by Prof. Coleman, I could not, of course, have described the Archæan sediments as scanty. I referred to an Archæan limited to the Keewatin, and to the Laurentian outpourings of granitic materials. The Keewatin is generally described as mainly volcanic in origin. The definition of Archæan and Algonkian favoured by Van Hise and Leith (Bulletin 360 of the United States Geol. Survey) would bear me out.

While many geologists would agree with Prof. Coleman as to his estimates of Archæan sediments, many, I think, will disagree with him in his contention that there was nothing exceptional in the thermal conditions attending the Laurentian revolution. Prof. Coleman's most interesting discovery of an ice age in Huronian times has, I submit, nothing to do with the matter. On the other hand, I think geologists in their interpretation of the Archæan should keep in mind the possibility (or probability) that the phenomena observed are due to paroxysmal thermal developments traceable to deep-seated radioactive substances: and that these developments, which appear to have been world-wide in extension, may have been sufficiently intense to have closed a biological era. So that, in fact, we have in the Archæan the almost obliterated record of a prior geological age.

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I HAVE read with interest Prof. Coleman's timely reminder, in NATURE of June 17, p. 775, of the essentially intrusive relations of the Archæan and of the frequently made deduction that the oldest visible rocks of the earth's surface are sedimentary. Of course this deduction is perfectly sound, provided the age of an intrusive rock is taken, as has been the