

may recur slightly before or after the beginning of a second round; in either case there is a false suggestion as to the rotation. The railings would make discontinuous vision of the spokes of the motor wheels, and a spoke might be seen upright in one gap but at slightly different angles at other gaps. I do not feel that the solution of the problem lies in this direction.

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Natural Selection and Plant Evolution.

MANY readers of NATURE must have been browsing with delight in the goodly volume on "Darwin and Modern Science" which Prof. Seward, of Cambridge, has taken such admirable pains to collect. Of all its many chapters few are more significant than that on the palaeontological plant record by Dr. D. H. Scott, because there, perhaps for the first time, the evidence of the fossils with regard to the influence of natural selection has been fairly tackled by competent hands.

Dr. D. H. Scott does not attempt to maintain that the record to-day is nearly so imperfect as it was when Darwin wrote his famous chapter thereon, fifty years ago. Dr. Scott's namesake and collaborator from Princeton speaks even of the record as, in some parts, "crowded with embarrassing wealth of material"; and yet what about evidence of natural selection? The present writer ventured to say (*Contemp. Rev.*, July, 1902, p. 83):—"Modern palaeobotanists furnish us with next to no evidence at all of the work of Natural Selection in evolving new species." Prof. Seward vehemently challenged my statements next month; yet, seven years later, Dr. Scott feels constrained to tell us:—"As regards direct evidence for the derivation of one species from another there has probably been little advance since Darwin wrote."

To put it more plainly, Dr. Scott is forced to admit that he can adduce absolutely no satisfactory evidence at all. All he does is to affirm his own firm conviction (as it is Prof. Seward's too) that natural selection must have been the chief agent; and he instances two cases where he thinks the possible inference extremely plausible, viz. (1) the case of the pollen tube, quite absent in the Palaeozoic seed-plants, found very short and imperfect in the living cycads and ginkgos, and fully developed in the angiosperms, but fossil proof of linking forms there seems none; (2) the embryo in the angiospermous seeds, whilst Palaeozoic seeds contain none. It may, as he says, be "impossible" to some "to resist the conclusion" that the nursing of the embryo by the seed was a process of adaptation. But, at any rate, there is no fossil proof thereof; and yet, as Dr. Scott will scarcely deny, there surely ought to have been some hint and trace thereof, the record being so comparatively rich and full as it is. In the case of the Tertiary mammals the action of natural selection can be very clearly demonstrated in numberless cases. If natural selection was the factor in plant evolution too, why is the record so obstinately silent?

Dr. Scott, like Prof. Seward, takes refuge in the thought that our plant record, for many purposes, begins far too late. "An immense proportion of the evolutionary history lies behind the lowest fossiliferous rocks." My chief object in writing this letter is to ask, Is there any valid proof of this in regard to *land plants*, the matter specially in hand? Their record begins, actually, in the Upper Silurian, and though it is very, very meagre and imperfect, the traces are too widespread to be denied. To deny the existence of known Upper Silurian plants is rankest scepticism, though Dr. Scott avoids all reference to them whatsoever. Why, even so very cautious an investigator as Mr. Robert Kidston tells us of "a plant showing woody structure," a plant so high as that, in the Lanarkshire Ludlow beds (Summary of Progress of Geol. Survey for 1897, p. 74). The most important Upper Silurian plant-remains are probably those from the Tanne Greywacke of the Harz, a fairly numerous and well-developed series, of age a good deal lower than Wenlock. Drs. Scott and Seward (*Encyclop. Brit.*, Supplement) wish to pronounce all these fern-like and other plants Devonian, because of their facies; but Sir Archibald Geikie (*Text-book of Geology*, ii., p. 976) tells us that these Tanne

plants are found a long way below shales with graptolites, which surely is proof enough of Silurian age.

We have, then, fair evidence of land plants in the Upper Silurian. Our very first air-breather or land animal, a cockroach, comes from the top of the Lower Silurian; and the fossil record of the whole Silurian is rich, varied, widespread, without gap. Yet it yields no hint of conditions favourable to land life below the top of the Lower Silurian. Is it, then, scientific to postulate dogmatically land plants earlier than the Silurian, simply because a theory requires it? Dr. Scott admits quite freely that the known facts go the other way.

He tells us not only of the opposition of the mighty like Nägeli, he also tells us that, as regards the succession of species, there are no greater living authorities than Grand'Eury and Zeiller, and that, in their opinion, "the evidence from continuous deposits favours a somewhat sudden change from one specific form to another." This is most certainly true of the palaeontological record as a whole. The evidence is overwhelming here, if only our men of science would be brave enough to forget their theories for a little while. Why insist on exalting the *a priori* methods of the schoolmen on the fair field of modern science? Why insist on refusing all evidence that does not suit? Why? Surely it is not, and cannot be, to enjoy the pleasure of barring out all design from the world in which we dwell.

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Musical Sands.

I CANNOT call to mind the occasion upon which Dr. Irving suggested that grains of hyaline quartz might produce the notes from musical sands, but, as a matter of fact, the grains do not "ring," or vibrate individually as sonorous bodies, and there is no apparent resonance or sensible continuance of the note after the plunger is withdrawn. I do not think any particular variety of silica is essential, because coral-sand is often musical, and my artificial musical sands are made up almost entirely of silicate of iron.

I have already shown that the natural sorting action of winds and waves is a requisite condition for the formation of musical patches on sea-beaches.

Mr. M. S. Gray's letter in NATURE of July 29 giving interesting particulars of his visit to the musical sand-hill near Copiapo, in Chile, confirms the statements made by the inhabitants to Darwin in 1835. In his "Journal during the Voyage of H.M.S. *Beagle*" Darwin referred to this hill of sand as "El Bramador"—"the roarer or bellowing," but he did not personally visit the spot.

The extraordinary sensations experienced by Mr. Gray were probably similar to those which have been described, by the various writers who, from time to time, visited Rig-i-Rawan and Jebel Nakous, both of which were referred to at length in my paper on musical sands in 1888. Particulars of the artificial production of notes from certain sands were also given by me in NATURE of August 6, 1891.

CECIL CARUS-WILSON.

A Question of Percentages.

IF a student obtains 37 out of 50 in one paper, 50 or full marks in a second, and 71 out of 100 in another, what is his percentage on the three taken together? If we add the marks as they stand we get 158 out of 200, or 79 per cent. If, on the other hand, we double the marks on the first two papers, we have 74 per cent., 100 per cent., and 71 per cent. If we add these we get 245 out of 300, or 81½ per cent. Will any of your mathematical readers kindly tell me which of these two different results is the true percentage for the three papers taken together? The answer may be very obvious; I can see that the two results must be different, but I cannot see which is the more correct method to use.

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60 Milton Park, Highgate, N., July 24.