

Letters to the Editor

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Early Man in Java

THE paragraph on early man in Java, in NATURE of June 11 (p. 863), concerning the Ngandong skull, induces me to make the following remarks.

Mr. W. F. F. Oppenoorth, who described the skull, says in the Summary, p. 63 of his paper: ¹

"Just in those features, where the Ngandong skull deviates from the Neanderthal type—especially in the shape of the occipital bone—it approaches a skull, found some ten years ago in South Africa, now known in literature as the Rhodesian skull; the torus supra-orbitalis, however, of the Ngandong skull is not so heavy and a little more bent.

"The back of the Ngandong skull also bears resemblance to that of the Australian race, so perhaps we may see in it a much more primitive prototype of that race than was the Wadjak man of Dubois.

"I think it justifiable to separate the Ngandong skull and call it *Homo (Javanthropus) soloensis*, n. subg., n. sp."

I may here direct attention to the well-known fact that in the slope of the nuchal plane of the occipital bone, there is a wide range of variation in Australian skulls, and there is also great individual difference, in this respect (among other things), between the two skulls of Wadjak man,² from one of which only I described the strikingly primitive upper and lower jaw, the brain case and the other bones of the face being in a fragmentary and incomplete condition. This skull much more approaches the Ngandong skull, in the shape of the occiput and in other respects.

A careful study of Oppenoorth's paper, which is called a 'preliminary' report, leaves little doubt, however, in my mind, that Ngandong man and Wadjak man are one identical type. The new skull, in my opinion, bears out the proto-Australian character of the Wadjak type, and also tends, I believe, to designate Rhodesian man as the prototype of the species *Homo sapiens*.

EUG. DUBOIS.

Haarlem,
June 11.

¹ *Homo (Javanthropus) soloensis*, een Pleistocene Mensch van Java (Voorloopige mededeeling). Wetenschappelijke Mededeelingen No. 20 van den Dienst van den Mijnbouw in Ned.-Indië, pp. 49-63, six plates. Batavia, Landsdrukkerij, 1932.

² Described in *Proc. Kon. Akad. Wetenschappen*, Amsterdam, vol. 23, pp. 1013-1051, two plates; 1921.

The Inheritance of Acquired Characters

PROF. MACBRIDE'S criticism ¹ of my lecture on the inheritance of acquired characters falls into three parts. He claims that I have misinterpreted Lamarck, that certain experiments demonstrate the transmission to the offspring of characters acquired as the result of a change of environment, and finally that this latter principle can be inferred from other facts, apart from the results of any experiment.

Lamarck, as I pointed out, used different language on different occasions. I think that my second citation from him shows that, sometimes at least, he expressed the views which I attributed to him.

I am glad that Prof. MacBride does not deny that Dürken employed selection. But, he writes, "selection as an effective cause of anything is a superstition that dies hard". Selection is quite an effective cause of change of the proportions in which various types occur in a mixed population. And such a change was all that Dürken obtained. Some of his original population gave green pupæ. He selected these, and got a larger proportion in later generations. The facts cited by Prof. MacBride in his letter as to the behaviour of certain larvæ in orange light are irrelevant to the issue, namely, whether this treatment, apart from selection, increases the proportion of green pupæ in a given environment, in successive generations.

I must apologise, in this connexion, for writing *Pieris rapæ* for *P. napi*. I must also apologise to readers of NATURE for quoting Prof. MacBride's account of Metalnikoff's experiment rather than the original. I have read three accounts by Metalnikoff of his experiments, and from none of them can I discover whether he bred from those caterpillars which had been immunised both with living and dead bacteria, as appeared from Prof. MacBride's lecture, or only from the latter class, as he states in his letter. The interpretation of his results depends on which alternative was true.

Prof. MacBride dismisses a hypothesis which he attributes to me as "fantastic", on the ground that *Salix rubra* is a rare hybrid. Actually it is of such economic importance that three different strains of it are grown for basket-making, and common enough to occur in sixty-nine of Druce's British vice-counties. On the other hand, the original host species *S. andersoniana* is an economically unimportant plant occurring in only thirty-four vice-counties. The hypothesis that some of the ancestors of flies colonising the latter had lived on the former does not appear particularly fantastic. Of the "larval memory hypothesis" we read that "it assumes that the instinct of the mother to seek a certain plant and the capacity of the larva to live on this plant are inseparable". After discussing the larval memory hypothesis I actually wrote, "The other factor in successful colonisation is the ability of the larva to eat and digest its food". The assumption (which is clearly false) was therefore Prof. MacBride's and not mine.

Against the eminent authorities cited in favour of Lamarckism, I will only quote one, namely, the Royal Society's motto, "Nullius in verba". I am awaiting such experimental confirmation of their views as would be furnished by a successful repetition of McDougall's experiment.

To call the alternative theory that of "Chaos and Chance" does not, of course, disprove it. The kinetic theory of gases proves that chaos and chance can form the basis of very exact and fully verifiable predictions. Unfortunately for the kinetic theory of species, conditions in a natural population are not chaotic. Mutation does not occur in all directions. From a knowledge of the types of mutation common in one species we can predict those common in another. And mating is not in general at random. If a species were a real chaos, the theory of evolution would be nearly as simple as the kinetic theory. So I fear that Prof. MacBride's phrase may have led readers who are acquainted with that theory to suppose that the case for neo-Darwinism is even stronger than is actually the case.

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¹ NATURE, 129, 900, June 18, 1932.