

has claimed wheat as native to Egypt. The distribution of wild Emmer, *T. dicoccoides*, is fairly well established; it ranges from Mount Hermon in Syria to the mountains of Moab. A single plant was found by Strauss in the Zagros mountains between Bagdad and Kermanshah. It would seem, therefore, that wheat and the practice of cultivating it must have been introduced from Asia. May not the cultivation of barley have been similarly introduced, for wild barley has a wider distribution in Asia than in Africa?

I have not yet seen Prof. Breasted's new book, but the passage quoted by Elliot Smith is rather surprising. The tablets he refers to are evidently the Nippur tablets and the Weld-Blundell prism. When publishing the latter, Langdon deduced from it that the date of the first dynasty of Ur was about 4000 B.C., though at the last moment in the preface he reduced this by fifty-six years. If from the same source Prof. Breasted deduces that the date of this dynasty is about 2900 B.C., it is clear that these tablets do not determine the maximum age of the earliest written documents with precision. The difference of more than a thousand years is not negligible.

It is quite likely that the second layer at Susa is contemporary with the first dynasty of Ur. The lowest layer, which contained the painted pottery, was succeeded by another layer containing a different type of ware, and this again by a sterile layer, 1 to 2 metres in thickness, before the layer known as Susa II. was deposited. How long it would take for such a thickness of soil to accumulate on an unoccupied hillock it is impossible to compute, but it is clear that the people of Susa I., who were cultivators of grain, lived very long before the first dynasty of Ur.

One sentence in Elliot Smith's letter has somewhat surprised me: "We know that people lived in Egypt at this time, many centuries before the metal copper was known." It is usually conceded that copper objects occur, rarely it is true, in the very earliest predynastic graves; moreover, Brunton has told us that in graves of the Badarian culture, which are earlier still, he found beads made of narrow copper ribbon and a stout copper pin or borer.

I should like to explain that the reason why the facts adduced by Elliot Smith relating to the early use of barley were not mentioned in recent discussions at meetings of the British Association and at the Royal Anthropological Institute was not that they were unknown to many of those present, but that both discussions were confined to the early cultivation of wheat in Egypt, and the question of barley did not arise. I trust, however, that these few lines, giving items of information apparently not very well known, may help to remove some of the widespread misunderstanding of which Elliot Smith quite rightly complains. I hope to deal with the question more fully at the Royal Anthropological Institute next week.

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Biological Fact and Theory.

ON returning from a holiday, my attention has been directed to an attack upon my book "The Physiology of the Continuity of Life," which appeared in NATURE on Dec. 25.

Fortunately an author is not expected to defend himself from criticism, just or unjust, but there is one point in the article which requires attention. The reviewer quotes the statement, "The F_2 shows all gradations from bar-eye to normal eye," and calls upon me to withdraw it. The words occurred as one of the following sentences:

"There are many examples of failure to show the

clear-cut differentiation expected in the F_2 generation. Morgan cites the case of the cross between the normal and bar-eyed *Drosophila*. The F_1 generation is intermediate. The F_2 generation shows all gradations from bar-eye to normal eye. Here the segregation does not seem to be complete."

Morgan's words in his "Physical Basis of Heredity," page 31, are: "A mutant eye shape of *Drosophila*, called 'bar' (Fig. 7, a), has an intermediate hybrid type (Fig. 7, b). The F_2 group may be represented (Fig. 8) in the following scheme:

"Fig. 8.—Relation of bar-eye to normal eye, as shown by the F_2 classes.

In this case the hybrid, intermediate type overlaps the bar type, so that in F_2 these two latter types give a nearly continuous class. At the other end of the F_2 series the round-eyed normal (or wild) type can be distinguished without difficulty from either of the other classes."

I think that my interpretation is justified.

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Jan. 7.

PROF. PATON'S letter is another instance of what I complained of in my review—failure to grasp the fundamentals of genetics before proceeding to attack and criticise its conclusions.

What are the facts about bar-eye as revealed by Prof. Paton's quotation from Morgan? They are: (1) That bar is a partial dominant; (2) that the variability of the heterozygote is greater than that of either homozygote; (3) that the heterozygote's eye-type overlaps that of the dominant; (4) but that it does *not* overlap that of the recessive (wild-type or round eye).

Three further important points not quoted by Prof. Paton are as follows: (5) In F_2 the frequency curve for facet-number (eye-size) is definitely trimodal. There is a discontinuity between the round-eye mode and the rest; but the "nearly continuous class" comprising bar and heterozygote types shows two well-defined modes. (6) The overlap only occurs in the females. This, however, does not mean that the males show segregation, the females not, but is of course due to the fact that no heterozygous males can exist, since Bar is sex-linked (the frequency curve for males alone is therefore bimodal, while that for females alone remains trimodal). (7) We can therefore, as regards overlap, consider only females. But if *any* female from the "nearly continuous class" be taken and crossed with a wild-type (round-eyed) male, one of two results will invariably be seen in the female offspring: either (a) they will all be of heterozygous type, with a unimodal frequency curve; or (b) they will be half round-eyed and half of heterozygous type, *with no overlap*. Further (c), if enough individuals be tested, the ratio of those giving result (a) to those giving result (b) will approximate 1:2.

This can only be explained if the female parents are either of constitution BB or Bb, and that therefore segregation did occur in the germ cells of F_1 , as well as in their own germ cells.

Even apart from these additional points, however, the quotation from Morgan definitely shows that segregation exists. For F_1 was all of the heterozygous type, not overlapping with the recessive round eye; and yet in F_2 this recessive wild-type eye was recovered pure, and without overlap!

It should, however, be strongly emphasised that this question of phenotypic overlap has of itself nothing to do with the question of segregation; and this is the graver error into which Prof. Paton appears to have fallen. Segregation—I also quote from