Family	Coloured normal	White normal	Coloured hyperglycæmic	White hyperglycæmic	Probability
B	6, 5·5	5, 5·5	—	—	0·4512
E	—	17, 17·5	—	18, 17·5	0·2641
F	8, 7·75	8, 7·75	7, 7·75	8, 7·75	0·02159
G	6, 5·75	6, 5·75	6, 5·75	5, 5·75	0·03281
H	26, 26·44	9, 8·81	9, 8·81	3, 2·94	0·005179
J	18, 18	6, 6	6, 6	2, 2	0·009292
K	16, 16·9	6, 5·6	6, 5·6	2, 1·9	0·03472

results. The last column gives the probability of obtaining so good a result. As an example of the calculation, the following observations would have given as good a fit or a better one in the case of family K : 16, 6, 6, 2 ; 17, 6, 6, 1 ; 17, 6, 5, 2 ; 17, 5, 6, 2. Since the expectations are $\frac{9}{16}$, $\frac{3}{16}$, $\frac{3}{16}$ and $\frac{1}{16}$ of the total of 30, the respective probabilities are :

$$\frac{30!\,3^{44}}{16!\,6!\,6!\,2!\,16^{30}}, \quad \frac{30!\,3^{46}}{17!\,6!\,6!\,1!\,16^{30}}, \quad \text{and} \quad \frac{30!\,3^{45}}{17!\,6!\,5!\,2!\,16^{30}}$$

in the last two cases. The sum of these is 0·03472.

In all cases except the last, the fit was as good as possible. We are therefore justified in multiplying the probabilities together, and find that, for the first six families, the probability of obtaining the best possible fit is $4·06 \times 10^{-9}$. We cannot make an estimate of the joint probability of so good a fit for all seven families without using a criterion such as χ^2, which is not wholly suitable in such cases. But it is clear that Cammidge and Howard's results cannot reasonably be explained on a basis of random sampling.

Other workers on mouse genetics, even when their totals agreed very well with expectation, have always observed a considerable amount of divergence in individual families, which was sometimes greater than that to be expected on a basis of random sampling, and never much less. The results obtained by Cammidge and Howard[2] in the case of congenital hypoglycæmia are not quite so striking, but they too are difficult to explain on the basis of sampling theory. Both sets of data would be fully explicable had the authors selected for publication out of a much larger number those data for which the agreement between theory and observation was closest. We therefore consider that, as in the case of Moewus' results, discussed by Philip and Haldane[3], those of Cammidge and Howard should not be accepted until they have been confirmed by other workers.

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         April 10.

[1] Cammidge, P. J., and Howard, H. A. H., "Hyperglycæmia as a Recessive Character in Mice", *J. Genet.*, **16**, 387–392 (1926).
[2] Cammidge, P. J., and Howard, H. A. H., "The Hereditary Transmission of Hypoglycæmia in Mice", *Proc. Roy. Soc. Med.*, **23**, 1341–1343 (1930).
[3] Philip, U., and Haldane, J. B. S., "Relative Sexuality in Unicellular Algæ", NATURE, **143**, 334 (1939).

True and False Teleology

THERE has recently been a considerable revival of interest, largely due to the work of Darlington[1], in the teleology of different systems of reproduction. The logical status of teleological arguments is very different in this connexion from that in other spheres, since the 'purpose' which is brought forward is the fulfilment of the conditions for rapid evolutionary advance under the influence of natural selection. That is to say, a genetic system which achieves its 'purpose' provides in so doing the mechanism for its survival. The considerations which have led to the rejection of teleological arguments in other connexions therefore do not apply ; though one might still question whether the teleological phraseology is the most convincing in which the arguments can be framed.

There is, however, a danger that the teleological method of argument will be carried over, by association, into regions in which it cannot be sustained. This seems to have occurred, to some extent, in the valuable article by Mather[2] in which he discusses the evolutionary significance of the formation of two different sexes in the diploid phase. He is not content to point out that the separation of the sexes is a mechanism for encouraging cross-breeding, but he contrasts this statement with some sentences, taken from a recent work of mine[3], on the developmental mechanisms involved, from which he deduces that "the sexes are separated supposedly in order to ensure that the gametes are differentiated". Such a view, he states later, must be rejected.

But such teleological statements should never arise in a discussion of developmental mechanisms. It is not sufficient to recognize that the development of two distinct sexes may be an evolutionary advantage ; we have still to find out how it is done, and the "developmental-genetical idea" cannot be "dismissed". At the same time, this does not invalidate the arguments which Mather brings forward as to the evolutionary consequences of such a differentiation ; in fact, he will find a statement of his main point, that the evolutionary advantage of having two distinct sexes is that it ensures cross-breeding, in the sentence immediately preceding the ones he chooses to quote. But if the new teleology is to be received with the respect which is its due, it is of the greatest importance that it should not stray outside its own legitimate fields.

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      April 1.

[1] Darlington, C. D., "The Evolution of Genetic Systems". (Cambridge: University Press, 1939.)
[2] Mather, K., NATURE, **145**, 484 (1940).
[3] Waddington, C. H., "An Introduction to Modern Genetics". (London: Allen & Unwin, 1939.)

DR. WADDINGTON's criticisms seem to be two. In the first place, I am taken to task for the unwarrantable use of teleological expressions, particularly in the specific case of my paraphrase of his own discussion of sex separation. Inasmuch, however, as the discussion was originally Waddington's and not mine, I can scarcely be called to account for its nature, whether teleological or otherwise. In any event, the point is trivial, as I feel confident that Darlington and Waddington would agree with me in regarding adaptation as the outcome of selection and in denying that it was purposeful, whether the discussion concerned genetical or morphological questions.

Secondly, I am criticized for wishing to "dismiss" the "developmental-genetical idea". This I have no desire to do in general as, clearly, developmental studies can contribute much to our understanding of genetics. But I do disagree with the specific idea,