

## REVIEW

NEW CONCEPTS IN FLOWERING-PLANT TAXONOMY. By J. Heslop-Harrison.  
London : Heinemann. 1953. Pp. 135. 5s.

The beginnings of genetic ecology by Turesson and of chromosome systematics by Tischler are now 30 years old. Since these beginnings new ideas and new methods have been growing up over the whole field of plant and animal classification. They have shown that the static assumptions of Linnaeus and of the modern museum are in conflict with the evolutionary assumptions made by genetics. In the nineteenth century this conflict was concealed in abstractions. It is now open and obvious.

Dr Heslop-Harrison in this little book dissects and describes this conflict. He does so without timidity and without evasion. His table (pp. 198-99) showing the opposition of classical and experimental systematics is a proper climax to his argument. It stands also in pointed antithesis to the opinion given by Dr W. B. Turrill in his Foreword that the practice of plant classification has been "adjusted" to the coming of the theory of evolution. Of course it has not been adjusted; it is not being adjusted; and its practitioners are well organised to resist the danger of having to adjust it. It is to its meeting this resistance that Dr Harrison's book owes its chief value.

Dr Harrison's sympathies are, no doubt, in favour of the experimenters and evolutionists: otherwise he could not be so unkind to the namers and describers. They, however, should know that he has not been too kind to the evolutionists. He has dispensed with the modern evidence of cultivated plants. Yet that evidence has done as much for the understanding of natural variation in our hands as it did in Darwin's hands. He has also denied himself the use of the facts and ideas derived from chromosome studies during the last 25 years. *Erophila* appears but without the polyploidy of Winge; *Taraxacum* appears but without the mutant chromosome types of Sørensen and Gudjonsson. He has failed to note the development of the new sciences of chromosome ecology and chromosome geography. The works of Manton, Löve, Barber, Fernandes, Janaki Ammal, Stebbins, Stern and Skalinska, in this field pass unrecorded.

Can it be that Dr Harrison is tempering the cold wind of experiment to the shorn lamb of systematics? It seems not. It seems rather that he has been content to study the new aspects of his subject at second hand. In his arrangement of the book he puts systematics first, breeding second, and the chromosomes last. Can it be that he looks at his problems in this order and sees the chromosomes as the end of the story of variation and not as the beginning? Probably. For on this view many things are explained. It is natural that he should say that "without a reasonably full fossil record phylogeny cannot be positively determined". He merely means that the body of inference from the chromosomes is negative or secondary or fallacious. He has not noticed that they can preserve a record of phylogeny of its own kind more certain than any fossil record, a principle which has been known for twenty years or more.

It is also natural that Dr Harrison should ascribe mutation in apomictic

strains of plants to a mysterious process of "autosegregation" which should "theoretically" not occur. If he had meant that the name should not occur we might have agreed. But the thing and the theory, the crossing over without effective meiosis, the system of subsexual reproduction, these have been known for twenty years or more.

Again it is natural that Dr Harrison should forget that inbreeding does not always produce a homozygote: it sometimes produces *Enothera*. In producing *Enothera* it has produced the most thoroughly investigated of all problems in the formation of plant species. Which we have likewise known for twenty years or more.

Both in the text and in his glossary Dr Harrison defines his terms. In these definitions we encounter even deeper difficulties. To him adaptation (like mutation to some geneticists) is not a process so much as a "character" (p. 123). Now what he means by a character is not clear. Sometimes he may mean a genetically determined difference; but sometimes (p. 48) he certainly means a physiologically determined difference. A biotype, on the other hand, is a group of "individuals which are genotypically all essentially the same" (p. 123). This would be right enough if we were told what was essential. But when he come to individuals themselves he changes his system. He does not define them by genotype at all. They are merely plants with "physiological independence" (p. 13). Different potato plants of a clone are therefore different individuals and potato "strains", by which he means clonal varieties, "exist in populations of thousands or millions of individuals" (p. 31).

Does Dr Harrison misunderstand the foundations of the subject? Or is he merely confused in presenting them? We cannot be sure. But we may venture an opinion. It is that if Dr Harrison had cleared his book of the biotypes and karyotypes, topoelines and cytodemes, autosegregations and agamospermies and if he had left a space for the fundamental notions of what constituted an individual, a hybrid, and a strain, he would have served his purpose better.

We must, nevertheless, be grateful to Dr Harrison. What he writes, although most of it might have been written some years ago, in fact was not written by any systematist at that time. He has gone half way to meet genetics. It is only unfortunate that half way is not enough.

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