substantial evidence of so improbable an occurrence as the breeding of the passenger pigeon in arid southwestern Arizona before they will be willing to accept these observations as a part of the history of a now practically extinct species. If specimens of the birds in question had been obtained and identified by competent authority, it would doubtless have saved burdening the literature of the wild pigeon with another questionable record, and one that may prove extremely difficult to eliminate.

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ON THE TRANSFERENCE OF NAMES IN ZOOLOGY

As the preparation of an official list of *nomina conservanda* is now under consideration by the International Commission on Zoological Nomenclature it may not be out of place to call attention to a point that seems to me of prime importance in this connection, although it has received little notice from recent writers on nomenclatorial reform.

It is simply this—while the rejection and replacement of familiar names for well-known animals is, of course, an inconvenience to zoologists, it is a trivial matter in comparison with the grave possibility of confusion that arises when the names are used in an altered sense. In the former case we merely multiply synonyms, and, unfortunately, they are so numerous already that a few more hardly matter; in the latter case there is a real and serious danger of ambiguity. Thus, at present, a writer who mentions *Trichechus* may be referring either to the Walrus or the Manatee, *Simia* may mean either the Orang or the Chimpanzee, *Cynocephalus* may be either a "flying Lemur" or a Baboon, and so on through all the great groups of the animal kingdom till we come to *Holothuria* which may refer either to a sea-cucumber or to a Portuguese man-of-war. Cases like these seem to me to be on an entirely different plane, as regards practical importance, from those in which an old name is simply rejected; even if the shore-crab is to be called *Carcinides* for the future we have only the additional burden of remembering that it was once called *Carcinus*.

A striking (if somewhat exceptional) instance of the pitfalls that are in preparation for future students is found in the section on Crustacea in Bronn's Thierreich (Bd. V., Abth. ii.). On p. 1056 there is an allusion to "*Astacus*" and on the following page to "*Astacus* (= Homarus)." In the bound volume (unless the part-wrappers have been kept in place) there is nothing to show that a change of authorship intervened between these two pages and that, while the second "*Astacus*" refers to the lobster, the first indicates the crayfish.

If the International Commission could be persuaded to consider first those names that are threatened with *transference,* before proceeding to deal with those that are merely in danger of *replacement,* they would, I believe, secure the support and cooperation of many zoologists who have doubts as to the practicability of the schemes lately put forward.

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SCIENTIFIC BOOKS

African Mimetic Butterflies. By H. ELTRINGHAM. Oxford, Clarendon Press. 1910.

The remarkable resemblances often observed between insects of different genera, families or even orders, have long attracted the attention of naturalists. In some, probably many, cases the explanation may be found in parallel variation, or similar conditions of life. Such explanations do not go far into the heart of the matter, but they are satisfying to those who like to give a "reason" for everything. Bates, who was so familiar with the insect-fauna of the Amazons, hit upon a more special "reason" for resemblances observed by him. This was, in short, that certain species which were edible simulated others which were distasteful and so gained protection. The subject was taken up by Wallace and other naturalists, and soon a large body of evidence was available, especially in relation to butterflies. It was proved to be a fact that certain

forms were disliked by birds and other natural enemies; it was shown that young birds did not instinctively reject these insects, but that after having tried and found them nauseous, they avoided them subsequently. It was then not difficult to see that if an edible species came to sufficiently resemble an inedible one it would be often taken for it and so escape. At this point it was observed that sometimes there were two or more butterflies very much alike, but all inedible. Fritz Müller pointed out that there would be gain in this, since the experience obtained in tasting one might suffice to cause a bird to reject all subsequently; whereas if they were all alike, each would separately have to pay its tribute to inexperience. Thus there were recognized two sorts of "mimicry," called the *Batesian* and *Müllerian,* respectively.

Examples of these phenomena have especially been observed in the tropics, where substantially the same conditions have existed for long ages, and living things have had time to develop some very nice adjustments and interrelations. In the volume just issued, Mr. Eltringham has taken up the mimetic butterflies of Africa, and has covered the ground so well that any reader may gain a good knowledge of the main facts without access to a collection of African specimens. There are given no less than 176 excellent colored figures illustrating the different species, varieties and sexes, while in the text each one is discussed at some length. There are also sufficient bibliographical references. In addition to the matter indicated by the title, there is a good general discussion of the whole subject of mimicry, and a summary of the evidence relating to natural enemies. Thus Mr. Eltringham's book may well serve as a guide to those taking up the subject and will be found useful in biological departments of universities, where "mimicry" is discussed along with other biological theories.

A special chapter is devoted to objections to the theory of mimicry, but those wishing to see the strongest adverse arguments should consult Professor R. C. Punnett's paper on mimicry in Ceylon butterflies, recently (September, 1910) published in *Spolia Zeylanica,* Vol. VII. Professor Punnett spent two months in Ceylon investigating some well-known cases, and came to the conclusion (or fortified a conclusion previously reached?) that the phenomena should be explained in quite another manner. Professor Punnett, like Mr. Eltringham, gives us admirable colored figures, and his discussion is most interesting. Some of the points brought forward are the following: (1) In Ceylon birds seem not to be serious enemies of butterflies. The chief enemies are apparently lizards and Asilid flies, and these appear to lack discrimination. (Experiments with a lizard were made.) (2) In various specified cases, the "model" and "mimic" do not occupy the same area to any extent, or the "model" is scarce when the "mimic" is common, a condition irreconcilable with the Batesian theory. (3) The resemblance is often imperfect, and when the flight of the insect is different it seems unlikely that they should be confused.

Against evidence of this sort may be placed the abundant data of Wallace, Marshall, Trimen and others, who have spent long years in the tropics, instead of a short two months. It may be reasonably urged, however, that if in only a few cases it can be demonstrated that "mimicry" has a meaning quite different from that assigned by Bates and Müller, serious suspicion is thrown on the whole theory or group of theories. Professor Punnett, long associated with Professor Bateson, is of course well known as an ardent Mendelian, and it is not a surprise to find at the end of his paper a Mendelian interpretation of mimicry. As he states, breeding experiments are urgently required, but judging from the classic experiments of Doncaster on *Abraxas,* he formulates an hypothesis to account for the polymorphism of mimetic *Papilio* in Ceylon. We can not take the space here to copy his tables, but the results he gets appear to coincide with the facts. Incidentally he cites the case of *Colias edusa,* in which the pale *helice* form of female, crossed with a normal male (necessarily so, as *helice* occurs only in the female) gave females of

both *edusa* and *helice* types. He remarks that the fact that *edusa* ♀ can come from *helice* appears to disagree with his hypothetical scheme, but he adds that the typical female differs from the male, and suggests that there *may* be or have been a possible "*a* type" of female resembling the male (such a type is well known in the *Papilio* under discussion). It is interesting to be able to state that the hypothetical "*a* type" of female *C. edusa* is actually known, and may be found mentioned in *Entomologist,* 1889, p. 26.

I can not help thinking it probable that whether or not the precise Mendelian hypothesis offered by Professor Punnett is justified by subsequent research, the facts will be found to be very much as he has postulated. I do not think, however, that the theory of mimicry is thereby contradicted. According to the old view that all organisms are everywhere varying (aside from non-inherited environmental effects), and that natural selection is necessarily in continuous operation to keep them constant or to modify them as needed, it must be confessed that some of the observed facts are hard to interpret. According to the newer view that "original variations" happen at relatively rare intervals, and that a stable type once produced may continue indefinitely if not discriminated against, the matter assumes a very different aspect. Consider the great antiquity of insect genera, as shown by fossil remains; consider the kaleidoscopic changes in insect-type producing innumerable species often without material advance in the general type; with all this time and change there must have been produced many pairs of more or less unrelated species resembling one another. When this resemblance has been advantageous it has been preserved, while other forms have died out; and hence to-day the proportion of such cases is vastly greater than we could expect from chance coincidence. It is not necessary that everywhere and at all times mimicry should be functional; the evidence seems to show that it generally is, and that is sufficient. Indeed, if a type has been preserved because of its ability to "throw" mimetic forms, it is likely enough to continue to do this, even in places where this is unnecessary.

Those who are confronted by the vast array of insect species rarely think of the unseen gaps in the ranks. These may perhaps be best appreciated by considering the fauna of the Hawaiian Islands, as elucidated by Sharp, Perkins and Walsingham. Here we have large genera with multitudes of allied species, no doubt the result of the immigration in ancient times of single types of a few groups. Comparatively free from the stress of competition these Hawaiian groups have, as it were, nearly their full membership; on continental areas only remnants usually remain.

Thus I think that the newer work on heredity, read aright, only strengthens the theory of mimicry, by relieving it of a load it was ill-fitted to carry. I do not see any other plausible way of accounting for the facts, unless it is by supposing that similar environments give rise to similar modifications of the germplasm. This idea loses support when we remember the cases (*e. g.,* in butterflies and bees) in which the same superficial appearance is due to entirely different structures.

It may still be debated whether natural selection has had much to do with the production of mimetic forms, in the sense of bringing about the *accumulation* of favorable variations. For my own part, I can not doubt that this cumulative effect of selection is real, and is a necessary cause of the more striking and complex instances of mimetic resemblance. The rarity of original variations, while great enough to relieve selection from the necessity of acting continuously on all characters, is doubtless not so great as to prevent it from bringing about many striking cumulative results, in the manner postulated by Darwinians.

I have wandered too far away from Mr. Eltingham's book, but I must return to it to mention his remarkable experiments with the larva of a moth, *Odontoptera bidentata.* Larvæ fed on ivy were offered to a lizard, and found extremely distasteful. They were, although of cryptic coloration, nearly always rejected by the reptile. Several larvæ were then transferred to apple, and after feeding on this

plant for a few days were again offered to the lizard, which ate them readily. Thus it is shown that a mere change of food plant may be of great importance in relation to destruction by natural enemies; furthermore, that some distasteful larvæ do not possess "warning" coloration, and again, that these cryptically colored larvæ were not recognized, after a few days, as objectionable. It would be interesting to repeat the experiment, having, if possible, ornamented the larvæ in some way so that they would be more easily recognized.

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Sewage Disposal. By Leonard P. Kinnicutt, Director of the Department of Chemistry, Worcester Polytechnic Institute; C.-E. A. Winslow, Assistant Professor of Biology and Biologist in charge of the Sewage Experiment Station of the Massachusetts Institute of Technology, and R. Winthrop Pratt, Chief Engineer of the Ohio State Board of Health, late director of Sanitary Engineering of the Republic of Cuba. New York, John Wiley & Sons. Price, $3.

This octavo book of 421 pages consists of a well-blended recital of American and European, especially English, experiences which have established the principal features now recognized in the science and art of sewage disposal. Almost without exception it is free of views that are either radical or so old-fashioned as to be regarded as superseded.

The joint authorship of this book has much to commend it and it will be noted that it includes in Professor Kinnicutt one of the foremost sanitary chemists in America, and one who has been fortunate enough to make numerous inspection trips to sanitary works in Europe, during the past thirty years. Professor Winslow, formerly of the Institute of Technology, in Boston, now of the College of the City of New York, has had unusual opportunities of studying the biology of this subject, particularly in connection with extensive experiments made at Boston. The practical side, from an engineering standpoint, has occupied the attention of Mr. Pratt for many years, first in Massachusetts and later in Ohio, with a valuable experience in Cuba.

The chemical and biological aspects of the book are more comprehensive and detailed than those of an engineering nature. Probably this is wise in a book of this size on a subject of such a wide scope as this one and which is undergoing such rapid changes in some of the more important aspects of engineering practise. Numerous references are given to details of results obtained from the findings of the Royal Commission on Sewage Disposal of Great Britain, as well as the results of tests and practical operations in America and abroad, especially in England. References are rather meager as to German investigations and experiences. To some extent the same is true of the results of current practise in the design and operation of disposal works in the United States other than in Massachusetts and Ohio.

After an interestingly stated introduction as to the sanitary demand for sewerage and sewage disposal, the book is divided into thirteen chapters, of which brief mention may be made to advantage as follows:

Chapter 1, pp. 1–20, deals with the composition of sewage in the terms of the analyst. Chapter 2, pp. 21–44, outlines the disposal of sewage by dilution. Chapter 3 gives many details as to the screening and straining of sewage, pp. 45–67.

The preliminary treatment of sewage by sedimentation, chemical precipitation and septic process occupies Chapters 4, 5 and 6, pp. 68–166. These chapters are unusually well-written, although they do not bring fully up to date very recent developments with the so-called "Imhoff" tanks, which have shown themselves to be a marked step in advance during the past year or two in practical operations in western Germany.

The expensive, bothersome and frequently unsuccessfully solved question of the disposal of sewage sludge is well outlined on pages 167 to 192.

Chapters 8 to 11, inclusive, on pages 193–274, contain a well-balanced statement of ex-