grow out of certain characteristics of some of the poisons used. For example, hydrocarbons such as DDT, Toxaphene, and Dieldrin may be cumulative in their toxic effects. Thus, single exposures may not cause mortality, but repeated applications, in the same year, or in successive years, may attain lethal concentrations in game birds, and presumably in others. If not killed, physical damage to vital organs or processes may result. Wildlife populations may thus be affected by such insecticides in three ways —outright killing, delayed killing, or a decline in the reproductive rate of the population.

A precise evaluation of wildlife losses, direct and indirect, from control poisons is presently impossible, despite the certainty of attrition and the availability of a literature encompassing several thousand titles. The inability of even an informed analyst, to say nothing of this layman writer, to make such an evaluation is due to nothing more than the lack of results from controlled experimentation; for as stated, most wildlife-poison studies have been of an ". . . after the horse is stolen . . ." variety.

This discussion has been generalized purposely in an effort to be fair to both sides—both legitimate in primary objectives—of an intricate and controversial problem. But in a field as vast as that of insecticidal and herbicidal poisons, one as relatively new and unstudied, and one particularly lacking in the results of objective and controlled investigation, no empirical generalization can be made. The reviewer can only conclude: *There is probably no more needed or opportune field for wildlife research.*

As an obvious recommendation, therefore, the writer urges intensified investigational programs on the part of institutions and governmental agencies, and by wildlife agencies, to the end that factual information ample for sound, renewable, natural-resource management is made available. Such knowledge is not now at hand. The need for it will almost certainly become more acute before the conflict of interest inherent between wildlife values and even legitimate plant and animal control is resolved. Other control programs, involving wide-scale use of new, highly toxic, and inadequately tested poisons, may prove tragic indeed, if enacted.—LEE E. YEAGER.

LETTER TO THE EDITOR

The review by Irby Davis of Eugene Eisenmann's "The Species of Middle American Birds" (1955. *Wilson Bull.*, 67:317–318) demands answering comment for a number of reasons. It would be unfortunate if this extremely valuable book did not reach the readership it deserves merely because it does not agree with the personal views of the reviewer in one minor aspect, that of vernacular names.

Indeed, the overwhelming concern in this review is this matter of the selection of common names, and the very first sentence reads, "The main purpose of this little book is to provide a suggested list of English or common names for the benefit of persons visiting Mexico or any of the Central American countries." May I point out a few errors in this sentence? First, the basic purpose of this book is not to provide a list of common names; its purpose and great value is that it provides, *for the first time, a complete and up-to-date check list of the species of birds recorded in Middle America.* In doing so, Eisenmann has done a great service not only to *visitors* to this region, but to all students of the ornithology of the area. It is a complete species list as correct in its scientific nomenclature as is currently possible (with copious footnotes explaining alternate points of view), with a summary of the range of each species, including many unpublished or previously unorganized data; it includes an excellent regional bibliography. The same first sentence seems to imply that the emphasis is on Mexico, which is not true, nor is the book a handy guide for the

traveller. It is not a guide at all. I find the rest of the review, while perhaps not as misleading as its opening, no less defensible.

For example, the reviewer makes this statement that should not go unchallenged: "It seems that we will never know just what constitutes a species." But among modern ornithologists, the species has a well-defined meaning. What we do not yet have in all cases is the factual knowledge that enables us to determine with finality where to put our species boundaries. The more knowledge we have, the more clear and stable will our species boundaries become. The species concept, however, should not be at issue here, nor should the reviewer question (he calls it futile) the goal of a single appropriate name for each species. The A.O.U. Check-list Committee has been striving towards this goal for species north of the Mexican border. Should another principle be applied south of it?

Perhaps the most surprising statement in the review is that "Species are lumped or split so frequently by taxonomists that the amateur field student should not be expected to change his common name for a bird, which he has long known, just because there has been a new technical grouping suggested, and his bird has now perhaps been made a race of some South American species that he has never heard of before." The inferences here are that taxonomists switch birds back and forth like an aimless, endless game of volleyball, that there is no progress towards the ultimate truth in taxonomy, and finally that there is something about the home grounds of the reviewer that makes a race found in Mexico more important in a pan-American species than races found elsewhere.

It may be, as the critic asserts, that some field students want separate names for every subspecies they think they can distinguish in the field. But the purported identification of subspecies is now deplored by all the experts, and there will be no subspecific vernaculars in the forthcoming A.O.U. Check-list. The book in question, furthermore, is a *species* list, and nothing more, although in itself it represents thousands of hours of scholarly work. A demand for names for all the thousands of subspecies seems contradictory from a critic who has just complained about the instability of the (far more stable) *species*. If subspecific vernaculars are ever wanted, Eisenmann has prepared the ground with truly pan-American specific names, which can be converted, with a single modifier, into subspecific names that indicate true relationships, a benefit heretofore generally lacking.

The bulk of the review in question dwells on Eisenmann's *choice* of vernaculars. The review gives the unwarranted impression that the names selected would be strange to current workers in Mexican ornithology. In fact, the names are those found in Blake's well-known "Birds of Mexico" (1953), the only manual and guide in its field, and a work to which every serious student must constantly refer. These names were also adopted in Paynter's "Ornithogeography of the Yucatan Peninsula" (1955), and agree with about 90 per cent of those in Edwards' "Finding Birds in Mexico." Most of the Mexican bird names had previously been used in Sutton's "Mexican Birds" (1951), and were largely drawn from the earlier monographs of Ridgway and Hellmayr. But Mexico was not the only country included in this list, and in choosing the most appropriate name for polytypic species ranging through several thousand miles, access to broad collections is required (which Eisenmann had and the reviewer did not have) and the occasional discarding of a name "long known" in Mexico may have been the only sensible solution.

The field of vernaculars is apparently charged with emotion. I will not comment at length on the reviewer's vague disparagement of the choices, beyond saying that the names particularly mentioned as unwarranted novelties introduced by Eisenmann were

oddly enough not novelties at all. "Slaty-breasted Tinamou" dates back to Sutton's book and was adopted in the subsequent Mexican works mentioned. The same applies to "Spot-breasted Woodcreeper." The use of "Woodcreeper" as the general name for the Dendrocolaptidae goes back at least to 1929, when Frank M. Chapman adopted it in his well-known book "My Tropical Air Castle." This usage has been followed in the Mexican works mentioned and in many other papers on neotropical birds. The name "Woodhewer" translates the technical designation, but creates a misleading impression of destructiveness, while "Woodcreeper" well suggests the behavior and appearance of these birds.

The reviewer is obviously a traditionalist who would hew to the older names, and prefers perfunctory translations of the technical names, no matter how erroneous, misleading, or inappropriate. This attitude is inconsistent with his complaint that technical names show little stability. It is the belief of this writer, on the other hand, that, if anything, Eisenmann was over-cautious on the side of traditionalism. Surely, where Middle American common names are concerned, nothing is "long known." The great days of ornithology in this region lie ahead; for every student of today or reference of the past, surely a thousand will yet come. If changes are needed, better now than later. Eisenmann's list supplies a basis for pan-American uniformity of usage, a great step forward. It probably will never satisfy everyone in every detail, but it is a major contribution, and surely even its severest critic will find constant use for it, for years to come.—ROBERT S. ARBIB, JR.

A notice concerning decisions on the names for certain birds recently adopted by the International Commission on Zoological Nomenclature has been provided by the Secretary to the Commission, Francis Hemming.

NOTICE is given that the International Commission on Zoological Nomenclature has recently taken a series of important decisions on the names for certain birds regarding which applications to the Commission were published in October 1952 in Part 1/3 of volume 9 of the *Bulletin of Zoological Nomenclature*. Among the decisions so taken the following are of interest to American ornithologists:

(1) suppression of the generic name *Colymbus* Linnaeus, 1758, and acceptance of the generic name *Gavia* Forster, 1788, for the divers (loons) and of *Podiceps* Latham, 1787, for the grebes (*Opinion* 401);

(4) suppression for nomenclatorial purposes of the names by Linnaeus published in 1776 in the "Catalogue of Birds, Beasts, . . . in Edwards' Natural History" (*Opinion* 412);

(5) validation of the name *Columba migratoria* Linnaeus, 1766, for the Passenger Pigeon (*Direction* 18);

(6) validation of the generic names *Bubo* Dumeril, 1806, *Coturnix* Bonnaterre, 1790, *Egretta* Forster, 1817, and *Oriolus* Linnaeus, 1766, by the suppression of older homonyms (*Direction* 21);

(7) acceptance of *Gallinago* Brisson, 1760, and rejection of *Capella* Frenzel, 1801, as the generic name for the Snipe (*Direction* 39).

The foregoing *Opinions* and *Directions* are now in the press and will be published at an early date. All inquiries should be addressed to the Publications Officer, International Trust for Zoological Nomenclature (address: 41 Queen's Gate, London, S.W. 7, England).