REVIEWS

EDITED BY KENNETH C. PARKES

Pennsylvania birds. An annotated list.—Earl L. Poole. 1964. Narberth, Pennsylvania, Livingston Publishing Co., for the Delaware Valley Ornithological Club. Pp. i–x, 1–94; line drawings, maps. $9\frac{1}{4} \times 6\frac{1}{8}$ in. $4.00.—This is, in effect, an abridged check-list of birds of the Keystone State, arranged in A.O.U. order, and briefly annotated. The Introduction opens with a historical review of Pennsylvania ornithology, from colonial times down to the present. Both Wilson and Audubon began their work in the Philadelphia region, and the founding of the Academy of Natural Sciences in 1812 presently made the city the ornithological center, so to speak, of the country. The roster of names of active workers and writers in this field during the Nineteenth Century includes nearly all American ornithologists of note. In 1890, with the formation of the Delaware Valley Ornithological Club, there began an intensive study of the bird life of the region, which was later expanded by individual members to include eastern Pennsylvania and the state at large. Dr. Poole, who became connected with the Reading Public Museum in 1926, early formed the plan of preparing the "first comprehensive work on the birds of the entire state of Pennsylvania since Warren's 1890 work." To this end he has covered the state in person and by proxy, and has studied the increasing literature, utilizing thus the work of local observers. The manuscript of his report already runs over 2,500 pages, of which the present book is only a preliminary abstract and résumé.

The physiography of Pennsylvania is briefly sketched, and illustrated by an endpaper map. The author recognizes the central Allegheny Mountain Section, bordered on the west by the Appalachian Plateau, and on the east by the Ridge and Valley Section, with a Piedmont Province in the southeast. These distinctions have an important bearing on the distribution of bird life in the state—a subject which our author treats at some length and with good effect, and which is of special interest to this reviewer. Three life zones (shown on another endpaper map), represented respectively by the Canadian, the Alleghanian, and the Carolinian faunas, enter the state, but the original picture has been confused by the amelioration of the climate in recent years, by deforestation, and by other human agency. The whole life zone situation is in a state of flux and transition. Some species have yielded to the changing conditions, while some others have not.

Twenty-one species of birds are listed as of the Canadian fauna, which occupies the Allegheny Plateau and the Allegheny Mountain Section. So many characteristic species of this fauna are missing, however, that the assemblage is christened "sub-Canadian." Such an omission was indeed recognized by this reviewer (*Birds of western Pennsylvania*, 1940), but not by name. Because of destruction of their habitat requirements, some of the species on the list are believed to have abandoned their quondam breeding stations, and the southward ranges of the species are far from uniform. The Alleghenian Fauna, here as elsewhere, is a transition zone, and is more or less indefinite as to collocation and composition; it is assigned to the northwestern part of the state, and to the Ridge and Valley Section, also to a considerable area in eastern Pennsylvania. The Carolinian Fauna is restricted to the southwestern and southeastern counties, but some of its component species are expanding their ranges markedly.

The body of the book comprises the list of species recorded from the state and

duly authenticated. Six to eight species are treated on each double-column page, usually enlivened by one or two excellent line-cuts by the author, adapted from earlier publications. Dr. Poole adopts a commendable middle course in dealing with records out of the ordinary. "The basis for full inclusion of a species has been the evidence of at least one specimen collected in this State, a recognizable photograph, or evidence that the species has been identified independently by three or more competent observers who were previously familiar with the species in life." Thus listed are 361 species, but of these I count but 189 which are known to breed; the balance consists of winter visitants, transients, and strays. The number of the last is astonishing. Most of the records of such come from the southeastern counties, and are of wanderers from the Atlantic coast, but there are some records of western birds from the western counties.

For each species the time of occurrence is first indicated, often (in the case of migrants) with the usual and extreme dates of appearance. Thus, the Green Heron is listed as occurring from mid-April to late September, with March 10 and December 20 as extreme dates. Resident species are marked "all months," as also are certain others whose occurrence in winter is more or less casual. There is perhaps enough breadth in such a general presentation to cover the usual divergencies due to latitude and altitude.

Following the chronology for each species its regional and seasonal status is concisely stated. Two lines suffice for the Downy Woodpecker, and only four or five for most of the common and generally distributed species. But for many others further comment and explanation are given in support. Specific records, chosen as the most significant, are cited to fortify the accepted status. In the case of accidental or rare visitants all the known records are briefly cited, and will be fully documented in the author's final report.

Of special interest is the series of maps showing the local ranges of 32 species, chosen for their significance as life zone indicators in Pennsylvania. Nesting records are designated by dots; "presumed" or "implied" nesting by open circles. Casual inspection of those in the latter category shows that some should be marked by dots instead, since there are sets of eggs in Carnegie Museum from these very localities. The Mourning Warbler is included among those species which have retreated from their breeding grounds in the Canadian Life Zone during the last half century; as a matter of fact, the Mourning is one of the few warblers able to survive deforestation, since it is a bird of the bushy undergrowth rather than the deep woods.

The volume closes with an abridged bibliography. This reviewer finds little to criticize and much to commend in this little volume. Within this brief compass the author has succeeded in presenting an abstract of the salient facts concerning the occurrence, migration, and distribution of the Pennsylvania avifauna. It is to be hoped that the publication of the author's projected larger volume on the birds of Pennsylvania will not be too long delayed.—W. E. Clyde Todd.

Biosystematics of sibling species of flycatchers in the Empidonax hammondii-oberholseri-wrightii complex.—Ned K. Johnson. 1963. Univ. California Publs. Zoöl., **66**(2): i–iv, 79–238; 24 tables, 28 figs., 8 pls. $3.50.—At what Parkes (*Wilson Bull.*, 76: 194, 1964) calls the "forest level," or superficially, this is an impressive paper. It abounds in long tables, scatter-diagrams, maps, and illustrations of vocalizations and tail-feathers; over 3,300 specimens were examined from 24 collections; 20 nests were found; molt in museum skins was carefully analyzed; recorded songs

were played back (almost always to the same species already on the tape); many birds of known vocalizations were collected; the bibliography lists over 100 titles.

At the "twig level," the paper is well done. There are occasional minor lapses in detail; figures in the tables and in the scatter-diagrams of the same individuals do not always agree exactly, and some of the references (Löhrl, p. 196; Marshall) are confusing. None of this detracts seriously from the clarity of this well-organized paper, which, as usual in this series, is all but free from typographical errors. The name *griseus* is inadvertently used for *wrightii* on p. 214. Occasional passages are hard to understand: (p. 137) "in *hammondii* the majority of first-year birds and probably most of the adults returning in the spring have completely ossified skulls" immediately after having noted that 11 out of 15 on their breeding grounds in California, mostly "first-year birds," had skull windows. Nor is it clear to me what "single-layered but ossified frontal bone areas" are (p. 139).

The substance of the paper (the "tree level") is uneven. The sections on molt and on apparent interspecific territoriality of *E. oberholseri* and *E. wrightii* (closely correlated, of course, with differences in ecology) appear to be new and worthwhile contributions to knowledge. Most of Johnson's taxonomic and distributional conclusions agree with current opinion as reflected in the 1957 A.O.U. Check-List, although a Gray Flycatcher (*E. wrightii*) is recorded from Chihuahua in June, south of the known breeding range. As is usual in this series of publications, keys are lacking. The section on "Nesting Biology" takes no account of the literature, but reports only Johnson's own findings. There are no descriptions of eggs or of natal plumages, nothing on weights, development or behavior of young, nor are there data on how long young stay in the nest or remain dependent on the parents.

The literature is sometimes handled in a loose and rather partisan manner. Certain "approved" workers (Grinnell, Hoffmann, Moore, Swarth) always turn out to be right; most others are apt to be wrong. For example, five molting empidonaces have been reported in the literature as *E. hammondii* obtained away from the usual molting area, the breeding grounds. One of these, a casual from Nevada, is not mentioned by Johnson, although he lists van Rossem's 1936 paper, in which the specimen was reported, in his bibliography. This is the only one of the five that is actually *hammondii*. A second "*hammondii*," reported by Swarth, turns up in Johnson's paper (p. 129) as a mistake by Bent (who had merely copied Swarth). The other three, reported by Moore, are not mentioned by Johnson here. Later (*Proc. XIII Intern. Orn. Congr.*, p. 880, 1963) Johnson does cite Moore, but in such a way as to imply that Moore *corrected* the mistakes of others on this point! To me, Johnson's criticisms of D. E. Davis (p. 175), Dickey and van Rossem (pp. 125–126), Mengel (p. 104), Peterson (p. 162), Stein (pp. 181–182), Walkinshaw and Henry (p. 175), and Woodbury and Russell (p. 157) all seem exaggerated or unwarranted.

Coverage of the literature is spotty, and analysis imperfect. This results in a few false claims of originality. For example, Johnson states (p. 149) that previously "there were no explicit records in the literature of localities where *oberholseri* and *wrightii* were found in close proximity." But the well known *Birds of California* by William Leon Dawson, whose interesting work Johnson never cites, says: "And now come two young scientists of most reputable standing who assert that they have found *wrightii* [= *oberholseri*], *hammondi*, and *griseus* [= *wrightii*] breeding together at the higher levels (say 10,000 or 11,000 feet) of the White Mountains." Similarly Johnson's discussion of the formal separation of *oberholseri* and *wrightii* (p. 86) overlooks the first actual publication, i.e., Salvin and Godman's separation of *E.*

canescens, a name not included in Johnson's "Synopsis of type specimens" (p. 90), although it was once current in California.

On p. 214 Johnson states: "The single example of close relationship between two species . . . that has been *brought to light by the present study* [italics mine] is between *hammondii* and *minimus*." The similarity of these two species had been repeatedly stressed by Coues (as quoted by Johnson himself, pp. 85, 87), and Ridgway mentioned that the two had been confused at times (*U. S. Natl. Mus., Bull.* 50 (pt. 4): 564–567, 1907). Bendire wrote that *hammondii* "appears to be the western representative of *Empidonax minimus*" (*Life histories of North American birds*, vol. 2, 1895; p. 315); and Dawson wrote: "*Hammondi* is the western analogue of *minimus*" (*Birds of California*, 1923; p. 887). Thus this relationship is hardly a new discovery in 1963! Most of the references cited above are listed in Johnson's bibliography. The reviewer is unaware that these several empidonaces had "received varying taxonomic treatment" (p. 214); thus no one will be surprised that they are not "geographic races," a straw man set up on p. 79.

The elaborate tables and statistics would be more meaningful if they were broken down by specific populations, were based on correctly sexed skins, and adequately covered the colonies of atypical Gray Flycatchers in northwestern Arizona (see Phillips, *Anales Inst. Biol. Univ. Méx.*, 30: 360, 1960). The species discussed in this paper do *not* present uniform measurements wherever they nest, but vary geographically. Johnson's tables, incidentally, do not include the measurements of the type specimens given on p. 90. I cannot discover the "several criteria not previously available" that Johnson promises us on p. 84.

Of his statistics, Johnson writes (p. 89):

> A point to be emphasized is that the various parameters [*sic*] of the species to be presented here were obtained from samples of 'typical' specimens . . . of obvious identity according to general coloration and wing formula. The collections of the Museum of Vertebrate Zoology . . . was [*sic*] utilized for this purpose. Most of these specimens . . . had been labeled in years past with the scientific name of the species, without revealing the identity of the systematist who had made the species determination. It was from this group of specimens that the samples for measurement were selected. Other specimens, often those of disputed identity, bore the initials of the worker who had identified the specimen. None of these initialed specimens was used in the samples, for many of these skins were not easily identified by general coloration and wing formula. . . . My basic assumption was that by the use of a series of specimens of undoubted identity, selected chiefly on the basis of features other than size . . . the degree of intraspecific variability in critical measurements and ratios could be better understood.

This basic assumption is patently fallacious. The easy specimens, which any anonymous ornithologist can identify, are obviously *not* the ones that show the limits of variability. What Johnson calls " 'typical' specimens" are not typical of their species and sex, but merely conform well with previous imperfect keys. Thus Johnson's refusal of expert help results in artificial skewing of his figures and statistics, particularly of female *E. hammondii* (the species and sex most commonly misidentified). His females (table 1 and figures 1 to 3) are all skewed toward a more pointed wing, showing less sexual dimorphism, than actually occurs in random samples. In the latter, the difference between primary 5 and primary 10 exceeds 1.5 mm in over 20 per cent and 1 mm in 43 per cent of those I have measured, whereas Johnson gives the average difference as 0.25 to 0.44 mm, with hardly any females even *reaching* 1.5 mm.

Johnson's tables and figures are admittedly (p. 95) flawed by the use of a number of mis-sexed specimens. "An effort was made to use specimens taken only by compe-

tent collectors" (p. 113), but the scatter diagrams indicate that, in all three principal species, some 20 to 30 per cent of young birds were mis-sexed. Accuracy in sexing seems better for older birds of most species, but Johnson's figures for even the older females of *E. minimus* are inexplicable without reassembling his series. Females sexed by really competent collectors and measured by L. L. Snyder, K. C. Parkes, and me in our respective museums, have wings measuring (chord) regularly up to 62 mm but very rarely exceeding that figure (the largest seen by us measures 63 mm). The average is, of course, far below 62. The tail usually ranges from 50 to 54.5 mm. Yet Johnson's *averages* for adult and first-year females, respectively, are: primary 8 (normally the longest), 63.32 and 61.15; tail, 55.28 and 53.40! Obviously his specimens are mostly males, or, if really females, must be of some other species (*hammondii* or *traillii*?). Thus one cannot accept his findings of greatly reduced sexual dimorphism in *minimus* and overlap of measurements of females of *minimus* and *hammondii* (pp. 109–110, table 5, and figs. 6 and 10). The statement (p. 106) that "occasional individuals of *minimus* have deeply emarginate tails" points to his possible inclusion of some *hammondii* in his series of "*minimus*." In any case, science would be better served by the collection and careful sexing of new material than by statistical treatment of past errors.

Unfortunately, there are more serious failings in this paper than the incomplete coverage of the literature, exaggerated criticism of "disapproved" authors, unwarranted claims of originality, and erroneous statistics. There are evidences of circular reasoning: (a) To determine the age of individuals of *E. hammondii*, Johnson relies chiefly on the shapes of the tips of the rectrices and the amount of wear they show. The accuracy of his age determinations is supported in that (p. 138) "no adults with the extremely pointed rectrix tips characteristic of a majority of the first-year birds have been noted, nor have any first-year birds been found with blunt-tipped rectrices." As a matter of fact, it should be noted that, of the two birds "with adult rectrices" specified by Johnson (p. 137) that I have re-examined, one (Monson collection no. 57) is an immature, as evidenced by narrower primary coverts (a character overlooked by Johnson) and narrower central rectrices (about 6.5 mm wide), which have become "blunt-tipped" through wear by March. Johnson's apparent assumption that all birds of similar age have identical rectrices, identically worn, is unwarranted, especially for small birds. (b) "A few juveniles and immatures that could not be placed with certainty in either *oberholseri* or *wrightii* by color or bill length were finally identified according to primary 10-tail ratio [Johnson does not summarize his data on this ratio except in six different scatter diagrams, fig. 5, p. 108]. It is significant that no overlap was found between the comparable juveniles and immatures of *oberholseri* and *wrightii* even after the inclusion of specimens otherwise of dubious identity" (p. 109). Thus (p. 212) "nonoverlapping morphologic characters . . . enable the specific determination of all specimens previously reported as hybrids." See, however, my discussion of American Museum of Natural History specimen no. 56451, below.

Inaccuracies abound. On p. 88 Johnson quotes my discovery of the very rounded wing of *oberholseri* relative to that of the Gray Flycatcher, with the outer primary usually shorter than the fourth. He adds: "Ridgway, 1907: 547, had all but stated this in his key." Actually, Ridgway *did not* mention the wing formula, simply placing *oberholseri* without comment in the general category "tenth primary shorter than fifth (sometimes shorter than fourth)" *along with* the Gray Flycatcher and several other forms.

On p. 91 Johnson writes: "It is also apparent from figure 1 that the character proposed by Phillips to separate *hammondii* and *oberholseri* by measuring the difference

between the 5th primary and the 10th . . . breaks down because the species overlap in this regard." Turning to this figure 1 (p. 94), we find that it plots *not* primary 5 but primary 4 in *oberholseri*, with an indication that primary 5 is about 3.4 mm longer; overlap is shown (if this figure be accepted as constant) between only the most pointed-winged males of one species and the most rounded-winged females of the other! Sex for sex, *no* overlap is shown on p. 94, *contra* p. 91.

Johnson contends strongly (p. 88) that "use of the wing formulae and wing-tail ratios . . . is not adequate," even when specimens are segregated by sex. "*The specimens must also be placed in their proper age categories* [italics Johnson's], because, as is demonstrated beyond, wing formulae and wing-tail ratios are intraspecifically different between adults and the category of juveniles, immatures, and first-year birds. Failure to determine age has led Phillips to misidentify a number of specimens [which are then specified]." This alleged difference would indeed be an important new discovery, but the reader will search the text and tables in vain for the promised "demonstration." Finally reaching p. 225, among the plates, he learns that the tail of adult *hammondii* is more "immature"-looking than those of immatures of the other two species! Now he sees why "age determination" (pp. 136–140) was discussed *by species*. Johnson's italicized claim on p. 88 is patently spurious, since age can only be determined *within* the species in these flycatchers; one cannot ascertain age first and *then* sort out specimens as to species.

Johnson presents no data on any wing formulae aside from the above-mentioned figure 1 and data on primary 9 minus primary 5 in the same two species (tables 1 and 2, fig. 3). No analysis of any wing formula at all is presented for the other species (average shapes are plotted, fig. 2). Thus Johnson's claim of inadequacy of this character is unsupported. These formulae and ratios are indeed important, and can certainly not be discredited without having been tested! As to the alleged differences between adults and young, I have previously remarked important differences in both size and color in *Empidonax* (*Auk*, 65: 513, 1948; *Anales Inst. Biol. Univ. Méx.*, 30: 362, 1960). In these flycatchers *all* juvenal flight feathers are slightly shorter than those of adults; the formulae and ratios therefore differ to a minor degree only. The differences do not compare in magnitude to those between the sexes, as Johnson himself admits elsewhere (p. 95, lines 15–18).

As to the few "misidentifications" listed by Johnson: Moore Coll. 50293 is, as he correctly states, *E. minimus*, although my error arose not from a faulty age determination but from the fact that the specimen is patently mis-sexed. The "typical immature *wrightii*" I "misidentified" as a probable hybrid *wrightii* × *oberholseri* (AMNH 56451) has been independently measured by two other ornithologists, William George and Wesley E. Lanyon, to whom I am indebted. The specimen is now before me, and I can see no reason to alter my original determination, written on its label in 1946: "bill and crown as in *wrightii* [= *griseus*]; wing formula intermediate; wing-tail ratio (and general color, to some extent) of *oberholseri*. Probably a hybrid." It does seem to be an immature, and quite likely mis-sexed as a female. Chord measurements, averaging those of George and Lanyon for each wing, are: primary 10, 63.4 mm; 9, 70.5; 8, 71.3; 7, 70.7; 6, 70.0; 5, 66.7; and 4, 60.9 (my measurement is 62.6). Tail measurements are: longest rectrix (next to outermost on right side), 67.0; next longest rectrices, 64.75. As Johnson's fig. 5 shows, no. 56451 is *not* a "typical immature *wrightii*" structurally. All but two of Johnson's non-adult males of *wrightii* have tails shorter than 64 mm, and these two have the 10th primary 66 to 66.5 mm. As a matter of fact, the only specimens plotted by Johnson in the vicinity of 56451 are of *E. affinis pulverius*, which of course 56451 is not. If we include the longest rectrix

and plot 56451 as immature, it falls well outside the limits of *both* species, as we might anticipate in a hybrid. Excluding this rectrix, 56451 falls near the *theoretical* limit of male *wrightii* provided one of Johnson's odd specimens is really a male. As an adult, it is *oberholseri* unless the long rectrix is excluded, whereupon it falls right between the two species.

As shown above, the next-to-outermost rectrix on the right side is distinctly longer than the rest of the tail. It is also somewhat more worn (in early November), indicating that it was not merely displaced in collecting the bird. Furthermore, it seems slightly wider than the other tail feathers. Slight abnormalities in the bill or tail are sometimes signs of genetic disturbance in true hybrids between full species of birds. A specimen in my collection (ARP original no. 3331), which I have identified as a *hammondii* × *oberholseri* hybrid, also has an abnormal, broad rectrix.

As to color: compared with Ridgway, *Color standards and color nomenclature*, 1912, the back of 56451 is browner than Buffy Olive (near Light Brownish Olive in hue, but much paler); the chest is near Naphthalene Yellow; the anterior wing-bar is near Light Grayish Olive, and the posterior (except at its proximal end) somewhat paler, toward Olive Buff. These colors, although occasionally approached by adult *wrightii*, are certainly not those of a "typical immature *wrightii*"; AMNH 56451 does not even agree with the occasional olive variants of that species. It is difficult to understand how Johnson rationalized his identification despite (1) my clear comments already on the label; (2) his own stress on the primary 10-tail ratio in separating these species; and (3) his claim (p. 109) that "no overlap was found between the comparable juveniles and immatures of *oberholseri* and *wrightii*" in this ratio. It is possible that Johnson's difficulty stems, at least in part, from his decision "at the outset of my research" that these species "rarely if ever hybridize" (p. 79).

In summary, Johnson's paper represents a great deal of hard work. It is unfortunate that he did not simply gather facts impartially and present them clearly and accurately, letting them speak for themselves.—ALLAN R. PHILLIPS.

A preliminary study of avian blood groups with special reference to the Passeriformes.—Robert A. Norris. 1963. Tall Timbers Research Sta., Bull. no. 4, 71 pp.—The period since "sputnik" has seen a greatly increased amount of public funds become available for research in ornithology. Considerable sums have gone into efforts that have often delved into intriguing research channels opened through recently developed or adapted techniques. Such projects must be supported, although to date discouragingly few significant results are yet apparent.

In the present study, Dr. Norris tested bird blood samples by mixing small amounts with commercially available human blood group antisera and then observing the resulting degree of clumping of red cells. He believed that as the red cells of some or all individuals of a species sample are clumped by certain human antisera, these agents define blood group systems that seem not distantly related to those of humans and other primates. He suggested that results of these tests may aid in solving problems in population immunogenetics, immunoethology, ecogeographical variation, and biosystematic and evolutionary relationships. In all, 750 blood samples of 138 species were tested.

The study must be judged on two levels, that of the applicability of the technique used, and that of its ornithological utility. Dr. C. H. Templis, Immunologist in the School of Public Health, University of California, Berkeley, kindly provided the following comments on the serological technique used by Norris:

As the title indicates, it is a preliminary study and the data tell us nothing more than how widespread ABO-like antigens are among birds . . . . The author states on page 1 that one of his objectives is to describe and compare in quantitative fashion and in the summary to give quantitative rating to his reactions; however, the "slide agglutination" test is a qualitative test.

On page 43 he uses the terms "antigen rich" and "antigen poor." He is referring to the ABO-like substances, but to an inexperienced reader these terms could be misleading. Both groups could be antigen rich or poor, depending upon which blood group or type antigens are being measured.

I should mention that A and B substances are commonly found in both plants and animals. Therefore, it is questionable how much significance can be attached to these data on the basis of taxonomy, evolution, etc.

His literature review indicates excellent knowledge of the related work done in the last 30 years.

To evaluate a new technique of analysis, we must compare its results with our accumulated knowledge. If the new ideas are *amply* documented by facts, and can be reconciled *to some degree* with previous knowledge, the technique is probably valuable. However, if the new data conflict sharply with the existing literature, then we may delve further into their relevance. If the new results are applicable to systematics, one would expect to find some uniformity within a given taxon. In the present study, what can be the significance of the random distribution of completely negative results or "very low" level of positive reactions to the human antisera by over 25 per cent of the species tested? Because of the limited blood available, not all tests could be run on every sample, so only minimal data are presented for some species. The results are presented in paragraph form by species, making comparisons difficult.

For instance, the discussion of the author's largest sample (87 bloods) from the relatively uniform family Parulidae, presents a stochastic array of data from which one would be hard pressed to extract any semblance of order that would contribute to our knowledge of parulid systematics.

Norris discussed the relationship of the genera *Junco* and *Zonotrichia* as an example illustrating the value of his technique. He believed that earlier conclusions reached with "too heavy reliance on one characteristic may have been premature or erroneous." Those conclusions, based in part on the considerable number of hybrids, suggested that *Junco* and *Zonotrichia* were very closely related (see also Short and Simon, *Condor*, 67: 438–442, 1965). Norris by contrast considered the "antigenic-fabric provided by ABO-like and other blood groups" to indicate these two are "miles apart"! The ability, and tendency, of two species to produce numerous hybrids in the field seems to the reviewer to be a more valid indication of close relationships than the reaction of bird antigens to human antibodies. (In another context, Norris cited a hybrid Golden-crowned Sparrow × White-crowned Sparrow as indicating close relationships between these two species.)

Norris must be credited with presenting to ornithologists a well documented discussion of the possible importance of serology to systematics and related problems. If the grave difficulties of obtaining highly specific blood group and type antisera are mastered, we can look forward to significant contributions in this field. From the results of the present study, however, further comparisons of avian and non-avian systems do not appear to be worth the effort.—ROBERT W. DICKERMAN.

Other Books Received

From time to time, we shall list here books of which *The Auk* has received a review copy, but for which a full-length review, for one reason or another, does not appear warranted. Each will be described in a few sentences, either by the Editor (R.M.M.) or the Review Editor (K.C.P.).

Adventure lit their star.—Kenneth Allsop. 1964. New York, Crown Publishers. Pp. 222, illus. by Antony Smith. 8½ × 5¾ in. $3.95.—A novelization of the establishment of the Little Ringed Plover as a breeding bird in England, far superior to most in this genre, with a minimum of anthropomorphism. First published in England in 1949. The drawings are inexcusably crude.—K.C.P.

Familiar garden birds of America.—Henry Hill Collins, Jr., and Ned R. Boyajian. 1965. New York, Harper and Row. Pp. x + 309, illus. by John C. Yrizarry (color) and Nina Williams (half-tone). 9½ × 6⅝ in. $7.95.—Easily the most superfluous bird book in years, with accounts of only about 75 species (including such "familiar" birds as the Spotted Dove and Green Jay). It is hard to imagine an appropriate audience. The color plates are, in general, attractive and the drawings adequate.—K.C.P.

Thoreau on birds.—Helen Cruickshank, ed. 1964. New York, McGraw-Hill. Pp. xii + 331, illus. 10 × 7 in. $7.95.—Extracts from Thoreau's writings, arranged and well annotated by the editor, who has contributed much more than most compilers of such books. Illustrations are samples (in black and white) of plates in reference books used by Thoreau.—K.C.P.

The bird watcher's America.—Olin Sewall Pettingill, Jr., ed. 1965. New York, McGraw-Hill. Pp. 441, illus. by John Henry Dick. 8½ × 5¾ in. $7.50.—Descriptions of "the best areas for birds in the United States and Canada" ranging in size from a small sanctuary (Hawk Mt.) to an entire state (Colorado), written by 44 authors. Style, naturally, varies, as does explicitness of directions. Brief biographies of authors by the editor. More literary, and in some ways more up-to-date, but less "practical" than Pettingill's "Guides to bird-finding."—K.C.P.

Bird studies at old Cape May.—Witmer Stone. New York, Dover Publications. Two vols., paper, pp. xliv + 941, illus. 8½ × 5⅜ in. $2.75 per vol.—An unrevised offset reprint of the 1937 edition, long out of print and selling at a premium. New material includes an introduction by Roger Tory Peterson, a list of additional species recorded in Cape May County, New Jersey, since 1937, and a biographical note on Witmer Stone.—K.C.P.