keyed to Ridgway's color standards. These seem strange bedfellows. I cannot help wondering if more synthesis and succinctness would not have been advisable; to state clearly, even in synoptic form, in places where such would be useful, and then where appropriate, as for general behavior and habitats, use Skead's smooth-flowing narrative prose.

The following illustrates the arrangement and coverage. Each family has an introductory section, varying from seventy-eight pages for the sunbirds to two pages for the creepers, followed by species accounts with such headings as: Local Names (English and others), Distribution in South Africa, Field Characters, Habits, Habitats, Food, Voice, Song, Call Notes, Breeding Season, Courtship, Territory, Nest Site, Description of Nest, Nest Building, Clutch Size, Egg Color, Egg Size, Incubation Period, Nesting Period, Nest Sanitation, Post Nestling Period, Breeding Success, Parasitism, Sundry, and Taxonomy (with synonymies, description and subspecies with diagnosis where appropriate). There is also a bibliography, a gazetteer, and an index.

There are ten very attractive color plates showing male, and occasionally female, and an egg of each species; 12 monochrome plates showing such things as habitats and nests; and 41 line drawings illustrating such things as tongues, bills, feeding habits, poses, and displays. There are also maps of the ranges of each species. In a pocket inside the back cover is a phonograph record of sunbird and white-eye voices.

As a practical point, it is interesting to project the completion of this series if it is to cover all the 813 species listed for South Africa. The first volume, covering the 17 species of canaries and buntings, was published in 1960, this second volume, covering 26 species, in 1967.—AUSTIN L. RAND.

LETTERS TO THE EDITOR

SHARP-SHINNED HAWK MIGRATION IN THE NORTHEASTERN UNITED STATES

Sir:

Mueller and Berger's (Wilson Bull., 79:50–63, 1967.) criticism of my (Wilson Bull., 76:257–264, 1964.) interpretation of Sharp-shinned Hawk migration in the northeastern United States requires comment. However, neither my time nor your space will permit a full answer.

Our interpretations differ concerning the role that wind plays in causing concentrations at points along the Atlantic coast. A part of all hypotheses proposed by Trowbridge (1895, 1902), Stone (1922, 1937), Allen and Peterson (1936), and now, Mueller and Berger (1967) is that wind drifts hawks off course and that northwesterly winds drift hawks from inland to the coastal region, where they continue southward until they are concentrated by the large bays. My hypothesis is that the wind's direction differentially affects the hawks' migratory behavior, northwesterly winds being most effective in causing the passing hawks to divert, that is, to drop to a lower altitude and to fly along the shorelines, while a varying proportion of hawks continues across the bays.

(1) Mueller and Berger contend that a correlation exists between hawk concentrations and northwesterly winds. Such a correlation cannot distinguish between the two hypotheses because each hypothesis states that a northwest wind is most effective in causing concentrations; the one, by drifting hawks, the other, by diverting hawks.

(2) Mueller and Berger attempt to show that there are too many hawks at Cape May to be accounted for by any hypothesis that does not include wind-drift as a component. Their analysis is faulty. First, Mueller and Berger assume that hawks are

uniformly dispersed across the broad front. This is not necessarily so; hawks may become concentrated for reasons other than wind-drift. Perhaps the hawks from New England and the Maritime Provinces (east of the NE-SW line in my Figure 3), once reaching the southern New England coast, continue their migration southward in the coastal region and over Cape May. Perhaps hawks reaching the Delaware Bay shore several miles west of the Cape May peninsula divert eastward and continue southward on the peninsula rather than cross the widest part of the bay. Second, Mueller and Berger assume that only one mile of a 2,500-mile-long front is intercepted at Cape May Point. Actually, Cape May Point is the small end of a funnel that is at least 10 miles wide at its mouth (the north end of the peninsula). Thus, the hawks entering the peninsula may be concentrated at least 10 times when they reach the point. Third, Mueller and Berger assume that each hawk is counted only once. Most counted hawks at Cape May are heading northward. If these northward-flying hawks are remaining on the peninsula until wind conditions permit their crossing the bay, as thought by Stone (1937), then it is possible that some hawks are counted more than once. These possibilities are not mutually exclusive.

I agree with Mueller and Berger that the observations at Cape May are of concentrations, but I do not agree that concentrations are proof of wind-drift.

Mueller and Berger point out the difficulties involved in obtaining evidence for drift. Perhaps we can never get unquestionable evidence without expensive instrumentation. Until unquestionable evidence is obtained, I think it is wise to recognize the inadequacies of the data and the tentativeness of our speculations.

I am grateful to James T. Tanner and Robert C. Frohling for permitting me to examine unpublished data and to Stephen T. Emlen for reading an earlier draft of this letter.—BERTRAM G. MURRAY, JR., *Section of Neurobiology and Behavior, Division of Biological Sciences, Cornell University, Ithaca, New York. (Present address: Department of Natural Science, University College, Michigan State University, East Lansing, Michigan 48823).*

Sir:

We remain in disagreement with Dr. Murray. If there remains a reader interested in this controversy, we should be happy to correspond with him.—HELMUT C. MUELLER, *Department of Zoology, The University of North Carolina, Chapel Hill, North Carolina,* AND DANIEL D. BERGER, *Cedar Grove Ornithological Station, Route 1, Cedar Grove,* Wisconsin.

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