RECENT LITERATURE

BANDING AND LONGEVITY

(**See also 19, 20, 50**)

1. "The sparrow that lived for 19 years" (chapter no. 2): Being an extraordinary account of how one digit changed a Brown-headed Cowbird in Maryland in '63 into a 19-year-old White-throated Sparrow. E. B. Baysinger. 1969. *Audubon Bull.* (Ill.), no. **150**: 29.—The chief of the Bird-Banding Laboratory at Laurel, Md. reports that the mis-reading of the first digit on the bands of the two birds concerned changed a *Molothrus ater* banded in 1963 into a *Zonotrichia albicollis* banded 19 years ago. This corrects the report reviewed in *Bird-Banding,* **40**(2): 148, 1969, review no. 4.—Margaret M. Nice.

[Earl Baysinger was also kind enough to call this error to our attention directly so that it could be corrected before secondary citations assured its place in the literature. He has re-emphasized the willingness of the Bird-Banding Laboratory to check out unusual records before publication, as a substantial percentage of the most unusual ones turn out to represent some simple error. The Recent Literature section attempts to call attention to unusual records, but has no way of checking these. To guard against perpetuating errors, corrections from readers are solicited.—Ed. and Rev. Ed.]

2. Some aspects of banding. S. G. Lane. 1968. *Emu,* **67** (3-4): 231-233.—This is essentially a plea for banding of "resident" species that are often overlooked by the bander. One "permanent resident," the Eastern Silvereye (*Zosterops lateralis*) was recovered 995 miles away. A chain of such examples makes the case.—Jack P. Hailman.

3. Lack of association among duck broodmates during migration and wintering. R. K. Martinson and A. S. Hawkins. 1968. *Auk,* **85** (4): 684-686.—The titular conclusion is based on two or more recoveries from nestmates in the case of 10 broods (three species). The suggestion is that they thus do not migrate in family units, and that getting back to the breeding grounds is an individual matter not dependent upon guiding by the hen as has sometimes been suggested. This is a valuable use of banding data.—Jack P. Hailman.

4. The activities at Ottenby Bird Station 1963 and 1964. Report No. 48. (Verksamheten vid Ottenby fågelstation 1963 och 1964.) Ingemar Alnås. 1969. *Vår Fågelvärld,* **28**: 9-17. (In Swedish with English summary.)—The support of industries and private persons made the erection of new buildings possible to facilitate the work. The station was active 5 April-20 November 1963 and 22 March-7 November 1964. A total of 29,391 birds were banded with 541 recoveries. Special attention was paid to molts and ectoparasites. Of the recoveries, one Swift (*Apus apus*) was retaken at the age of 17 years and a Dunlin (*Calidris alpina*) was at least 14.5 years old. A foreign recovery, a Robin (*Erithacus rubecula*), had traveled 1,100 km. in nine days.—Louise d K. Lawrence.

5. Banding activities at Falsterbo Bird Station 1965-1967. Report No. 39. (Ringmärkningsverksamheten vid Falsterbo fågelstation 1965-1967.) Gunnar Roos. 1969. *Vår Fågelvärld,* **28**: 18-44. (In Swedish with English summary.)—This is a very detailed review containing 16 tables, one appendix of foreign recoveries and five maps of birds banded at Falsterbo and recovered elsewhere. The year 1966 showed the highest record for Sweden with 22,663 birds banded. Some of these were taken during April when record cold temperatures and recurring snowfalls brought on a reverse migration of hitherto unrecorded intensity and banding had to be temporarily inhibited when it became obvious that such birds as the Redwing (*Turdus musicus*), the Song Thrush (*Turdus philomelus*) and the Hedge Sparrow (*Prunella modularis*), were due to suffer mass death.—Louise de K. Lawrence.

6. **Bird-banding at Powdermill, 1968.** R. C. Leberman and M. H. Clench. 1969. *Powdermill Nat. Reserve Res. Rept.*, No. **23,** 17 unnumbered tables, charts and references, mimeo.—We cite this report primarily to draw attention to this banding station and nature reserve of the Carnegie Museum of Pittsburgh, where banding operations occur on the average of 204 days per year, and more than 10,000 birds were banded in 1968. I recently saw a claim that Point Reyes Bird Observatory in California is the only full-time banding station in America. Powdermill certainly seems to be pushing toward full time, and I wonder if there are not others (comments from readers on this point invited). The report contains the usual analyses, but in admirable detail. In addition, adult-to-immature ratios for more than 7000 birds of 15 species are given for the fall migration period. Adults occupy from 10.2% (Black-capped Chickadee, *Parus atricapillus*) to 55.6% (American Goldfinch, *Spinus tristis*). There is a bibliography of publications arising from studies at the Reserve.—Jack P. Hailman.

7. **Bird banding in California.** E. A. Stoner. 1969. *Calif. Fish and Game,* **55** (1): 4-11.—An introduction to banding in a state that has seen 1.2 million individuals ringed since 1931. Table 2 lists banding stations in the state where more than 500 individuals were banded in 1966; Thomas Harper of Fairfield caught my eye: he banded 1400 birds of *two* species. There is no indication of what they were, but he must have learned a great deal about them. Table 3 lists the 24 species of which more than 500 individuals were banded in 1966. I was surprized that waterfowl do not comprize even a third of the list, although ducks occupy the first two places. Sparrows, pigeons and warblers seem to be favorites. Appendix A lists selected out of state recoveries, and Appendix B some longevities and movements discovered through banding. Three passerines were recovered in their tenth year after banding: House Finch (*Carpodacus mexicanus*), Brown Towhee (*Pipilo fuscus*) and Golden-crowned Sparrow (*Zonotrichia atricapilla*). Some things that can be discovered through banding are mentioned. This is the sort of brief summary that might interest persons in becoming serious banders.—Jack P. Hailman.

8. **Skull ossification in the White-throated Sparrow.** W. R. Mellencamp. 1969. *EBBA News,* **32,** (2): 109-111.—Of interest to banders who want to determine the age of *Zonotrichia albicollis.*—Jack P. Hailman.

MIGRATION, ORIENTATION AND HOMING

(See also 3, 59, 62, 104, 106)

9. **Orientation by pigeons: is the sun necessary?** W. T. Keeton. 1969. *Science,* **165**(3896): 922-928.—Keeton and his colleagues discovered, "more or less by chance", that the pigeons they were using for homing experiments at Ithaca, New York, consistently oriented in homeward directions when released under overcast skies, in defiance of generally accepted theory. As a critical test, he shifted the internal clocks of a group of pigeons by six hours by means of an artificial light regime, and released them, alternately with unshifted birds, from a point from which they had already returned once. The results were striking: under overcast skies, both clock-shifted and unshifted birds oriented homewards and returned home promptly; when the sun was visible, the unshifted birds flew homewards, but the clock-shifted birds were deviated by 90°, adopting the direction expected if they used the sun as a compass. The same result was obtained at two different distances and with birds clock-shifted both forwards and backwards. These results suggest that pigeons can home without sight of the sun, but that when the sun is visible they use it as a compass, relying on it in preference to the clues which they use under overcast (whatever these may be). Keeton regards the observations as in complete disagreement with Matthews's sun-arc hypothesis, and as requiring a major reformulation of Kramer's map-and-compass concept. The latter conclusion seems overstated: the behavior of the birds released under sun is only comprehensible in terms of the map-and-compass. It would be more reasonable to say that the results indicate that the map-and-compass is only one of two (or more) homing mechanisms.

The main limitation of the experiment is that releases were made in only one direction (east) from the home loft. Keeton rather casually denies that his birds ever show the directional biases, which Matthews terms "nonsense orientation" and claims have bedevilled homing experiments (see *Bird-Banding*, **40**(2): 176, 1969, review no. **83**). However, real evidence is needed, including releases from the west. As a devastating conclusion, Keeton reports that statistical analysis of the data used by Matthews to demonstrate random orientation under overcast actually shows, in several cases, significantly non-random grouping in the homeward direction.—I. C. T. Nisbet.

10. Bird migration and natural selection. D. Lack. 1968. *Oikos*, **19**(1): 1-9.—This essay ranges over a number of aspects of bird migration and seeks to show how each can be understood as a product of natural selection. It lacks critical discussion (which is understandable as it is the text of a lecture): for most observations only one explanation is given, and the reader is often left wondering whether another explanation might be equally plausible. However, the paper includes one extremely important new idea. "While actual proof is not yet available, I think it likely on theoretical grounds that the numbers of a migrating species are determined in a density-dependent way by the availability of food on the wintering grounds." The theoretical grounds for this generalization are not made explicit, and its corollaries—that ecological factors operating on the breeding ground have no influence, that different species must segregate into different sub-habitats in winter to avoid competition, and that northern migrants must be excluded from the tropical forests of Africa because the "niches are full"—seem extremely implausible. Nevertheless, like all Lack's generalizations, it is very stimulating and immediately prompts a search for counter-evidence—which is very hard to find.—I. C. T. Nisbet.

11. Drift migration: a correction. D. Lack. 1969. *Ibis*, **111**(2): 253-255. Lack originally reported that his radar observations showed that migrants in England do not compensate for drift (see *Bird-Banding*, **35**(2): 209-210, 1964, review no. 20), but subsequently several other radar ornithologists have reported complete or partial compensation. Lack has now reanalyzed his original data and reports evidence for substantial (but not always complete) compensation for drift in several different groups of birds. He accepts that the phenomenon of "selective departures" (see *Bird-Banding*, **38**(3): 182, 1967) was responsible for part of the night-to-night variation in mean tracks which he observed, and can now cite no case of complete non-compensation.—I. C. T. Nisbet.

12. "Headwind migration". N. H. Christensen and J. R. Jacobsen. 1969. *Dansk Orn. Forenings Tidsskr.*, **62**(2): 153-159.—The authors severely criticize the concept put forward by Rabøl (see *Bird-Banding*, **36**(4): 273-274, 1965, review no. 5; **39**(2): 135, 1968, review no. 7) of a tendency for diurnal migrants to fly against the wind, in addition to other directional tendencies. They point out that those birds which fly against the wind tend to be more conspicuous because they fly lower. In their succinct formulation, the dispute is whether "birds flying low fly against the wind" or "birds flying against the wind fly low".

Rabøl makes an involved semantic defense (pp. 160-165). As reviewer, I believe that better evidence for "headwind migration" is provided by the observed tendency for birds to *turn into the wind* when they reach a leading-line. However, this does not prove that the tendency exists in the absence of topographical sign-stimuli. Christensen and Jacobsen are right to point out the importance of altitude, and they demonstrate defects in Rabøl's ethological model.—I. C. T. Nisbet.

13. Bird migration: influence of physiological state upon celestial orientation. S. T. Emlen. 1969. *Science*, **165**(3894): 716-718.—By manipulating day-lengths, Emlen brought a group of Indigo Buntings *Passerina cyanea* into the physiological condition appropriate to autumn migration three months early, in May. When tested in company with normal birds in a planetarium under a sky appropriate to May, the shifted birds oriented south, whereas the normal birds oriented north. This beautifully neat experiment indicates that the physiological state is more important than the star pattern in determining the seasonal orientation.—I. C. T. Nisbet.

14. Reversed migration as the cause of westward vagrancy by four Phylloscopus warblers. J. Rabøl. 1969. *British Birds,* **62**(3): 89-92.—This paper analyzes the records in Great Britain and Ireland of two pairs of eastern vagrants—one pair from Asia and one pair from eastern Europe. In each case the records of the species with the more northern breeding range are grouped significantly further north within the British Isles. Rabøl assumes that western populations of these species migrate east at the outset of their autumn migrations, and therefore suggests that the vagrants had oriented rather strictly opposite to this normal direction. This is a possible explanation, but there is really no evidence for due eastward orientation in any of the species, and in fact the vagrants of at least one species must have travelled distinctly south of west. The pattern of occurrences must mean something, but it is far from clear that it indicates reversed migration.—I. C. T. Nisbet.

15. Orientation of autumn migrating Garden Warblers (*Sylvia borin*) after displacement from Western Denmark (Blåvand) to Eastern Sweden (Ottenby). A preliminary experiment. J. Rabøl. 1969. *Dansk Orn. Forenings Tidsskr.,* **63**(2): 93-104.—Norwegian Garden Warblers caught on passage in western Denmark and tested in Emlen-cages oriented, on average, to the SSE: this is probably the standard direction for the population. When a group of juveniles was transported 530 km to the east and tested immediately, they oriented SW'S. The two lines, when projected, intersect in central Europe, somewhere north of the Alps. Similar results were obtained with at least two other species.

This is an important experiment. It not only suggests that juvenile migrants can correct promptly for lateral displacement (see *Bird-Banding,* **40**(1): 55, 1969, review no. 13); it suggests that they may do so by heading towards an intermediate point on the migratory route. Rabøl believes that Norwegian Garden Warblers follow a zig-zag route through western Europe: if so, this experiment would suggest that they may achieve it by navigating to a series of "staging areas".—I. C. T. Nisbet.

16. Migration of Little Gull (*Larus minutus*) in the North Atlantic Region. B. Bruun. 1969. *Dansk Orn. Forenings Tiddskr.,* **62**(2): 126-136.—A review of banding recoveries and published records of Little Gulls in northern Europe suggests that the autumn migration takes place in two phases. The main departure is in July and August, by inland routes across Europe. A smaller number lingers in the Baltic Sea until October-November, when they apparently move westwards, resulting in a peak in the British Isles. Bruun argues that the latter birds must continue west to (unknown) wintering grounds in the North Atlantic, in order to account for a peak in records in eastern North America in November.

This argument seems entirely circular: in the absence of actual observations of Little Gulls "well offshore" in the North Atlantic, the North American records provide the only evidence for wintering there; the supposed movement offshore is the only evidence for transatlantic crossings. In fact, most records of Little Gulls in North America have been in association with Bonaparte's Gulls, *Larus philadelphia,* and the largest numbers (overlooked by Bruun) have been in the Great Lakes, where the peak is in September (J. Burger and R. Brownstein, *Kingbird,* **18**(4): 187-194, 1969). It seems more plausible that a small population of Little Gulls migrates regularly up and down North America each year with the Bonaparte's Gulls.—I. C. T. Nisbet.

17. Radar characteristics of birds in flight. T. G. Konrad, J. J. Hicks and E. B. Dobson. 1968. *Science,* **159**: 272-280.—Although there is no direct and simple way to distinguish the "angels" (birds) from other objects on the radar screen, there are lots of complicated, computer-assisted ways of doing it.—Jack P. Hailman.

POPULATION DYNAMICS

(**See also 24, 25, 46, 50, 51, 52**)

18. The Black Brant population on Spitzbergen and Franz-Josef Land. (Navaerende bestand av ringgjess (*Branta bernicla hrota* (Müll.)) på Svalbard og Frans Josefs Land.) Magnar Norderhaug. 1968. *Sterna*, **8**(2): 73-80. (In Norwegian.)—The population of both areas winters in England and Denmark, and numbers 2,500 to 3,000 per census of 1966. Since censuses in the 1950's found about 4,000, the population appears in a decline. Protection measures are discussed.—Leon Kelso.

19. What do Keas die of? J. R. Jackson. 1969. *Notornis*, **16** (1): 33-44.—Starvation. At least that is what the author concludes from a ten-year banding study. The Kea (*Nestor notibilis*) is an endemic New Zealand parrot whose name comes from the Maori natives. Causes of mortality in endemic species are especially important to know for conservation efforts. The known deaths of 74 Keas banded in a particular area are due largely to man (44 or nearly 60%), through shooting, trapping, drowning in water tanks, electrocution or running over with autos. Banding results show heavy mortality in young birds in the fall, presumably due to starvation. Table 12 lists illnesses and diseases found in birds at the time of banding, and there is also a list of parasites collected. Injuries found in banded Keas are given in table 13, and some notes on predation end the paper. The evidence for the conclusion that "starvation is the most important cause of death" is not apparent.—Jack P. Hailman.

NESTING AND REPRODUCTION

(**See also 28, 30, 36, 37, 44, 48, 64, 65, 66, 67, 68, 102**)

20. On the ecology of the Great Black-headed Gull in the Kustanai region. (K ekologii chernogolovogo khokhotuna, *Larus ichthyaëtus* Pall., kustanaiskoi oblasti.) Yu. A. Samorodov and V. F. Ryabov. 1969. *Vestnik moskovskogo universiteta, biol. soil sci. div.*, **24**(1): 32-37. (In Russian.)—Rivaling the Great Black-backed (*L. marinus*) for title of largest of the gulls, with its satiny black head contrasting with elegant dove-gray tints of the upper parts and regarded by Russian authors as "decidedly the most beautiful", this species is peculiar to saline inland seas and lakes of southwestern Russia and adjacent lands. It surpasses its rival in gregariousness, breeding in colonies of several to over a thousand pairs, tolerating a numerous peripheral population of Herring Gulls (*L. argentatus*), interspersed with nests of whatever other waterfowl that particular locality affords, and even a few raptors and passerines.

From 1960-1967 a colony at Lake Zharkol, northern Khazakhstan, on an island of 3 ha area was observed. Here 96, later 79 pairs, occupied a 400 m^2 area; 2 nests per 10 m^2 average; greatest density in center of colony, 35 nests in one 10m^2 plot, 25 to 40 cm apart. A peripheral population of Herring Gulls, 125 pairs, tolerated 63 pairs of Common Gull, *Larus canus*. Herring Gull nests were 2-4 m away from those of host species. In 1966 Great Black-headed Gull nests were sparsely lined, in earth depressions; in 1967, with cold early spring, nests elevated above soil, with ample vegetation lining. Females did most ot the nest-building, incubation & care of young. Of 79 nests, 66 had 3 eggs; 8, 2 eggs; and 5, 1 egg. Parents alternated at incubation 'several' times a day, females 4-7, males 1-4 hrs. per sitting. Marsh Harriers, Herring Gulls and Carrion Crows inflicted some nest losses. From 219 eggs, 104 young survived.

In activity the species proved partially nocturnal as well as diurnal; the situation portrayed was one of continuous round-the-clock activity, the noise of chattering and quarreling endless. Activity of adults was highest in morning and evening, at 4 to 8 and 15 to 19 hrs, with as many as 60 adults departing the colony and about as many returning, hourly. Young were fed 1-5 times per hr. Adults seemed never to sleep, and well-oriented at night, brought food unerringly to their own nest. At any given moment 30-40% of the adults would be away foraging.

While it is a fish-eating species as indicated by the specific name, analysis of 270 pellets collected in June found small mammals to comprise the main food, 78.4% by occurrence, with fish 57.8%, and insects, mostly beetles, 18.5%. Apparently nothing has been recorded on their mode of capturing rodents, but despite their ponderous and apparently slow flight they capture insects in the air, and are reported to forage as far as 50 to 60 km from the nest. As compared ethologically with the local Herring Gull race (*L. a. cachinnans*) in another account (I. A. Dolgushin, *Birds of Kazakhstan, Ptitsy Kazakhstana,* **2**: 298-308, 1962.) while chicks of the latter swim voluntarily at age of 2-3 days or as soon as convenient, and scatter when in danger, those of *L. ichthyaëtus* will not take to water unless forced before 10 days of age, and when alarmed huddle in dense flocks which can be driven into traps for banding.—Leon Kelso.

21. Polygamy in Hen Harrier. (Polygamie bij de Blaue Kiekendief (*Circus cyaneus*). C. Van de Kraan and N. J. Van Strien. 1969. *Limosa,* **42**(1-2): 34-35. (In Dutch, Summary in English)—On the island of Ameland one male Hen Harrier, distinguished by a missing flight feather, apparently had seven mates during one season. He brought food to at least five of the females and 18 young were raised successfully. A sixth female was not served after courtship, yet she raised two chicks alone. A seventh female deserted her nest and the island after laying four eggs.

Neither Witherby *et al.* (1938-41) nor Niethamer (1937-42) mention polygamy in this species. So far as I know the champion polygamist among birds is still the Corn Bunting (*Emberiza calandra*), one male of which had as many as seven mates at one time; his activities were chiefly confined to watching his many mates, while they did practically all the work of feeding the chicks, although at times helped in this task by a number of full grown young birds (Col. and Mrs. B. H. Ryves, *Brit. Birds,* **28**: 2-26, 154-165.)

This remarkable case of the polygamous Hen Harrier is a preliminary report on a research program directed since 1959 by Prof. Dr. K. H. Voous.—Margaret M. Nice.

22. Gay colored rarities. (Bunte Raritäten. Europäische, australische und africanische Bienenfresser brüten in der Voliere.) L. Koenig. 1968. *Vogel-Kosmos,* **12** (Dec.)—In this paper Frau Koenig describes the nesting of two foreign species in the aviaries of the Institut für Verhaltensforschung der Oesterreichischen Akademie der Wissenschaften Wilhelmimenenberg, Vienna.

The most charming of all five species of Meriopidae they have raised proved to be the Rainbow-bird, *Merops ornatus,* from Australia. At seven in the morning the group of these bee-eaters leave their sleeping burrows and fly onto a perch from which they greet all their familiar friends with a special call and in turn are greeted by them with a lift of the hand and an imitation of the call! This species started to court at the age of 7 months, and at 16 months dug their nesting hole. The male zealously fed his mate both before and during incubation, in which he, too, took part. The eggs hatched in 23 days; both parents fed the chicks for 28-30 days and for four weeks afterwards.

A pair of Red-throated Bee-eaters (*Melittophagus bulocki*), from East Africa, hatched their eggs in 20 days; the young left the nest at four weeks and became independent at about 50 days.

Remarkable photographs show a parent *ornatus* feeding a naked child and in another we see the five members of the African family perched tightly together on a rainy day in the aviary.—Margaret M. Nice.

23. Nest protection of Eiderducks and Shovelers by means of faeces. C. Swennen. 1968. *Ardea,* **56** (3-4): 248-258.—*Somateria mollissima* and *Anas clypeata* soil their eggs with foul-smelling faeces while fleeing (as do a few other species of *Anas* listed in table 1, although no birds studied other than ducks seem to do this). Food was given to Ferrets (*Putorius furo*) and Norway Rats (*Rattus norvegicus*) in a choice situation: food with and without eider faeces added. When the faeces were from breeding birds, neither nest predator would touch the soiled food to any extent (table II), but when from nonbreeding eiders, there was no difference in food choice (table III). Unfortunately, the basic food in the first experiment was "food pellets" while in the second was shellfish and fish. Also, in the first experiment portions of the entire faeces were used, while in the second only the liquid component was put on the food.

Similar experiments were conducted with Shoveler faeces (breeding season, apparently) with similar results. Ferrets also did not discriminate unsoiled foods from those soiled with faeces from three species of "non-ducks." Certainly the results point toward an anti-predator effect of the duck faeces, but the confounding of variables leaves the difference between breeding and non-breeding faeces open to some doubt.—Jack P. Hailman.

24. The breeding biology of the Red-wing (*Turdus iliacus L.*). H. Tyrvainen. 1969. *Ann. Zool. Fenn.*, **6** (1): 1-46.—This is a long, complete and detailed study, which includes material on habitat, nest site, nest building, egg laying broods and clutch size, incubation, nestlings, fledglings and other subjects. There is much quantitative data on nearly every major point. Figure 35 shows that there is a loose negative correlation between the size of the clutch and the feedings young near the end of the nestling stage. A more sensitive measure, weight of nestlings, shows that those in a clutch of five (the modal clutch for the species) are the best fed (per individual). The data are consistent with David Lack's theory that food is the limiting ultimate factor that controls clutch size. This is a fine study.—Jack P. Hailman.

25. An experimental study of clutch size of the American Coot. L. H. Fredrickson. 1969. *Auk*, **86** (3): 541-550.—When we ask "why does this species lay n eggs?" we may in fact be asking two different sorts of questions, as David Lack pointed out long ago. We may be asking what the immediate environmental stimuli and internal physiological mechanisms are that determine how many eggs are to be laid by a given female, or we may be asking why natural selection limits the average clutch of the species to those eggs dictated by the mechanism. The point of the second question is that clutches as large as possible would seem to be favored if the physiological mechanism for determining clutch size is heritable and if larger clutches lead to more offspring than smaller clutches. In the arguments that have raged over this second question since Lack's considerations of it beginning in the late 1940's, the question of the heritability of clutch size has never (in my knowledge) been investigated. If clutch size is heritable, the argument goes, then the average clutch size must be held by natural selection to that number because (for some reason) larger clutches actually produce fewer viable individuals for the next generation. It is not a question of the percent success of the eggs laid, but, rather of the absolute number of individuals resulting from a given clutch.

Lack's theory is basically that large clutches are disadvantageous because the available food is spread too thinly among all the hungry mouths, so that on the average few young survive to breeding age. Lack's theory is supported by some evidence (largely in temperate passerines), not supported by others (seabirds and tropical species).

This paper begins with Lack's hypothesis, but the second paragraph drops that red flag of caution to the reader when the author states "Not all workers agree with Lack's theory on food as the *mechanism* for controlling clutch size" (emphasis mine). Indeed, Fredrickson confuses physiological mechanism with ultimate selective advantage in the control of clutch size. The data show simply that when eggs are added to *Fulica americana* nests, the eggs tend all to hatch, even if the clutch size is elevated from a norm around eight to something like 20 eggs. However, data on survival of the brood "were difficult to obtain," so that no strong conclusions about selective forces controlling clutch size can be drawn. One conclusion is made: "that coots have a greater drive to incubate large clutches than small or control clutches" (because the percentage of hatch is slightly larger in the former). Rereading the paper several times I cannot see what the result implies about incubation *drive*. Like the introduction, the discussion seems to jump repeatedly between discussing physiological mechanisms and selective forces. The only thing I can see that these data show are that the selective forces working against larger clutches in the coot do not seem to have much to do with the size of the clutch that the parent bird can successfully incubate. And the statement that can be made about mechanisms is that an experimentally reduced clutch sometimes leads to more laying so that the clutch size may be proximally controlled in part through feedback from the number of eggs present. We need clearer thinking on these problems if we are ever to get clear answers to the two separate questions involved.—Jack P. Hailman.

26. Egg-shape and breeding-success in the Guillemot *Uria a. aalge.* (Eiform und Bruterfolg bei Trottellummen *Uria aalge aalge* Pont.) B. Tschanz, P. Ingold and H. Lengacher. 1969. *Ornithol. Beobach.*, **66** (2): 25-42.—Whereas a round ball will roll and bounce downhill, a cone-shaped object will roll in an arc. The Guillemot's egg is more cone-shaped than that of the Razorbill (*Alca torda*) and other alcids, and therefore tends to roll in a tighter arc when accidentally displaced. This is also true of plaster models made of the eggs of these species. Therefore, few Guillemot eggs are disasterously pushed over the sides of the nesting cliffs.

This nice little study combined simple experiments with observations of the breeding colonies. Some other factors that affect the fate of displaced eggs are: their content (liquid vs. solid), center of gravity, speed of rolling, acceleration due to the incline on which the egg is rolling, and obstacles in the way. Exposed nests suffer higher egg loss, as do nests subject to disturbance. The convincing data are graphed simply as percentages without statistical analysis, but the actual data are shown in tiny numbers on the graphs so that statistical analyses could be performed by the reader for closer scruity of the phenomena. I tried one such check as an example.

The authors make a point that losses due to disturbance and exposure do not combine additively, but multiplicatively (p. 38). This is the result to be expected if the two factors are acting independently of one another. I tried to check this statistically. The sample sizes in the four combinations (little disturbance, little exposure; great disturbance, little exposure; etc.) are different, but all so close to 100 trials that the calculated percentages may be used. The result is a 2 x 2 contingency table that shows independence (for instance, when disturbance and exposure are great, the predicted eggloss is 52% from the overall data under the assumption of independence; the actual figure is 50%). Therefore, although it is unclear under what sort of alternative hypothesis one would expect the effects to be "additive", in fact the effects cannot be shown to be other than independent of one another, so that the authors' intuitive assessment of the results holds statistically. I think all of their conclusions would if similarly checked. We need more of this kind of relevant experimentation combined with direct observation.—Jack P. Hailman.

27. Retardation as a factor in the synchronization of hatching. M. A. Vince. 1968. *Anim. Behav.*, **16:** 332-335.—In previous papers (cited in the bibliography), Margaret Vince has shown that in quail clutches synchronization of hatching occurs because the embryoes "communicate" through vibrations passed via the substrate on which the eggs lie. In this paper she continues to explore this amazing phenomenon by seeing whether the effect tends to slow down the development of the more advanced eggs, as well as accelerate the less advanced ones. Both effects are found in the Japanese Quail (*Coturnix coturnix japonica*) but the interpretation is less clear for the Bobwhite (*Colinus virginianus*) and more experimentation will be required to settle the question. These are fascinating studies.—Jack P. Hailman.

BEHAVIOR

(See also **20, 23, 26, 27, 40, 63, 65, 66, 67, 68, 77, 88, 92, 96, 98, 100, 102, 103, 106**)

28. The organisation, control and development of parental feeding in the Oystercatcher (*Haematopus ostralequs*). M. Norton-Griffiths. 1969. *Behaviour*, **34** (1-2): 55-115.—This long study is important not only as a contribution to the biology of a fascinating species, but as an attempt to account for behavior in somewhat the same way in which programmers deal with control in computer systems. There are four chapters.

Chapter 1 is a general description of how the parents feed the young, and it would have been helpful to know how many young European Oystercathers normally have. The parent obtains food, and either brings it back by flying or running, or else waits for the young to approach. When waiting it rarely vocalizes (only 19 times of more than 1000 observations), but when flying or running usually does (278 of 288 times observed). Table 1b shows in percentages that the "weep" call is associated with flying, the "chuck" call with running, but the absolute numbers cannot be deduced since running and flying are lumped in table 1a (p. 58) in the total of 278 calls. On p. 60 the text refers to 52 observations of "weep" and 215 of "chuck", but these sum to 267, not 278 plus 19 as would be expected from the table. In fact, there are several frustrating parts of the data expressed in percentages where the reader cannot reconstruct the actual sample. Because the calls are given in other situations before the young are hatched, the author concludes "these calls should therefore not be considered feeding calls," but the argument does not prove that their use in feeding is non-functional as it appears to imply. Feeding of the young is apparently normal feeding arrested before swallowing, so that the parent stands with food in its beak or drops the food under the beak. An argument that purports to demonstrate that only swallowing is inhibited is introduced by way of new data in the discussion to chapter 1 (fig. 1, p. 64). The graph shows that number of "presentation" acts is correlated with the time elapsed between the first presentation and when the chicks eat. One reader's reaction to the graph is that a linear correlation is not the best fit, and that as time elapses presentation acts become less frequent. But either way, it is difficult to see how these data show that "inhibition is acting only on swallowing and not on the pre-ingestion motor patterns themselves." Inhibition here apparently means diminution of frequency, and no data whatever are given to describe what the parent does in normal feeding, if there are no chicks and the adult does not eat what it has caught. The analysis here seems vague.

Chapter 2 is on the seasonal development of parental feeding. Behavior in the egg and the response of brooding parents suggest a possible learning of the parental "chuck" call by the chick prior to hatching. These are valuable data on the further development of interactions between parent and young. Parents bring food increasingly closer to the nest as hatching draws near, and later seem to initiate feeding of the newly-hatched chicks, which only occasionally peck at the bill or food before they have first been fed. Then they quickly learn the situation and respond readily on subsequent feedings. Citing my work on gulls, the author draws a sharp distinction, in that in larids the chicks seem to initiate the first feeding. But this varies, as I have stated, and may depend upon whether the parent has previously bred. Unfortunately, the breeding history of the Oystercatchers in this study could not be known.

Chapter 3 is on the organization and control of parental feeding, and is really the heart of the study. Basically, the birds feed as the tide comes in or goes out, whether feeding for themselves or for their young. The text makes a point of noting a pause in feeding at dead low water; although less than half the pauses (18/37) actually bridge dead low (fig. 10), there certainly is an unexplained pause within a few hours of this time. If the young take the food fairly soon after it is offered, the parent fetches more. A "waiting-time threshold" model is erected to account for this: the parent begins timing as soon as it presents the food. If the chick eats within a certain threshold time, the parent brings more; if the chick tries to eat, but fails within the time limits, it is allowed to eat that bit, but the parent does not fetch more; and if the chick does not react within the time, the parent eats the food and makes no more trips back. Field data are consistent with predictions from this model, and the threshold time decreases from about half a minute (when the young are a week old) to less than 5 sec by the third week after hatching. Some exceptions to the scheme (no quantitative data) are when the parent must break up the food, the young beg, or the young give a distress call. Young increasingly take the initiative in feeding situations, as the waiting time shortens, until the entire system breaks down as the young are weaned and forage on their own.

A "model" of the entire feeding system is presented in fig. 16 and dealt with at length in the text. Although there is not space to describe the model in detail, the interested reader will require some help with the figure, since no less than five arrowheads indicating the direction of control sequencing have been omitted. The figure is a flowchart such as used by computer programmers in setting up the sequence of logical operations to be performed by a computer (principally computations and decisions, or comparisons). In such a diagram, only one arrow is supposed to come into a component box; fig. 16 violates this restriction and therefore the unlabelled arrows from "compute T" and "compute L" to "is T greater than or equal to WTT" may cause confusion. The arrows should go from the last-named to the first two listed, and the "bottom" arrow should be labelled "true" (if I understand the system correctly). The other missing arrowheads are fairly self-explanatory. The model contains many hypothetical variables (OS, OST, D, L, K, etc.), none of which have been measured in the study and some of which seem inherently unmeasurable.

The author takes a mild poke at my superficially similar diagram of interactions between parent and young gulls in feeding ("but unfortunately quantitative and qualitative details are lacking."). Since this is the only other attempt (so far as I am aware) to make this sort of representation of this kind of behavior, a few words of comparison are in order. First, the gull model is a cybernetic diagram, in which arrows represent variables (that is, it models the flow of information, not sequential control). Every box in the gull diagram is completely and totally explicit in accompanying tables giving the qualitative details (despite the quoted statement above), whereas only about half the boxes in the Oystercatcher diagram are even accessible to data collection. Finally, the simple descriptive diagram for gulls brings out a number of new points at the start of the study which are subjects of subsequent experimentation, whereas the Oystercatcher model is presented at the end of the paper and dropped.

The final chapter is a discussion of evolution of the system, and is largely speculative. This paper is important for its attempt to bring modern analytic techniques to bear upon behavior, and there are good observations in it throughout. The systems model, however, is labored and then not even used, in contrast to the smaller model of the waiting time situation which is explicit and reasonably well tested. The paper should be read by all persons who want to experience the thrust that is moving the study of bird behavior from the realm of qualitative natural history into the realm of quantitative science.—Jack P. Hailman.

29. Messages of vertebrate communication. W. J. Smith. 1969. *Science*, **165**: 145-150.—John Smith is one of the contemporary leaders in the analysis of animal communication, particularly of birds. This paper is "more of a progress report than a confident statement" of the generalities he has gleaned from studies of tyranid flycatchers and other species, particularly with regard to the "messages" of communication. By "messages of a display" is meant "the information available to an individual as a result of having received just the display; all other sources of information are considered contextual." In a previous publication (see review **74**, *Bird-Banding*, **40**: 73, 1969) the author distinguished the signal (physical form of the display), from the circumstances that appear to produce the display (message), from the total information available to the recipient (meaning). The slightly revised definition of message here appears more careful and operational, and deserves consideration.

Smith believes that there is a limited set of messages that may be more or less similar in all species with any reasonable degree of social complexity, and he provides a provisional list of 12, as follows. (1) Identification (a species, or one sex, or an age class or even a particular individual can be identified by a sound given, for example); (2) probability; (3) general set; (4) locomotion; (5) attack; (6) escape; (7) nonagonistic subset; (8) association; (9) bond-limited subset; (10) play; (11) copulation and (12) frustration. All these seem fairly straightforward except probability, general set and nonagonistic subset. The first may signal the probability of the communicator behaving in a certain way; for instance, some birds have various calls that signal various probabilities that they will attack. I fail to see how this message differs from one such as (5) in which the probability of attack is near 1.0; surely all displays have a message with some probability value attached, even identification messages. When I hear "Bob-white!" around my house it is sometimes a Mockingbird calling.

The general set refers to displays given when the animal may be doing any one of a number of things. As Smith admits, further study may show such displays to have a more distinct message. One attractive idea he suggests is that such displays signal a change from one behavior pattern to another, regardless of what the behavior patterns are. Lastly, the nonagonistic subset is difficult to comprehend; apparently these messages are given when the animal is not about to attack or flee, but it is hard to see how this differs from the general set.

Smith then asks why there are so few messages. The total number of signals seems also to be limited, to between 15 and 45 or so. The upper limit on the number of displays is not clear, nor are the factors that set it. "One plausible explanation is that displays must be sufficiently distinct from one another to be recognizable by the recipient." Whatever the reason, the actual functions served by the displays are much greater in number than the displays themselves, and this is because the context provides so much additional information. Thus, messages may be combined to yield a larger number of displays than there are messages, but the really great amount of information is conveyed by the messages in their contexts. Smith argues that probably the number of messages is limited by the number of displays, so that evolution has focused on the kind of messages that can be combined readily with different contexts to allow a maximum transmission of information.

Finally the author discusses some situations that should lead to messages not in his list, primarily more specific messages due to the absence of contextual information. Of the myriad of voices indulging in metacommunication at present, Smith's is one of the clearest. We await his next progress report.—Jack P. Hailman.

30. Visual isolation in gulls. N. G. Smith. 1967, *Sci. Amer.*, **217** (4): 94-102.—A readable, semitechnical account of Neal Smith's exciting studies of the large *Larus* gulls in the vicinity of Baffin Island. The full account is in his *A. O. U. Ornithol. Monog.*, **4**, 1966. Briefly recounted, the story goes like this. Four forms have overlapping ranges: Kumlien's Gull (*L. glaucoides*), Thayer's Gull (*L. thayeri*), Herring Gull (*L. argentatus*) and Glaucous Gull (*L. hyperboreus*). All look similar, with white bodies, darker mantles, white heads and a yellow beak with a red subterminal spot on the lower mandible. Basically, the question is, are all four forms good species, and if so how do they avoid hybridization?

There are some physical differences in the forms. The Herring Gull has a darkish gray mantle with black wing tips; Thayer's has light gray tips. Glaucous and Kumlien's have lighter mantles, the former with white wing tips, the latter with very light gray. They also range in size, and they differ in the color of the iris and the fleshy ring that surrounds the eye. The eye-pattern appears to be the primary visual isolating mechanism keeping these species from interbreeding, although other factors also contribute.

The effect of the eye ring color on the sexes and at different times in the reproductive cycle was tested by capturing gulls and painting a different eye ring. An unmated female chooses the correct color-contrast of the male with which she will mate, but eye color of the female does not affect the unmated male. Later, the situation is just reversed, where changing the eye color of the mated male has no effect, but changing the color of the female causes disruption of the pair bond.

This is a very nice story that deserves a wide audience.—Jack P. Hailman

31. Varieties of social stimulation in the feeding behaviour of domestic chicks. C. W. Tolman. 1968. *Behaviour*, **30**: 275-286.—This is another in a series of fascinating experiments by Tolman exploring the problem of how siblings bring about social facilitation of feeding in domestic chicks. The present study shows that merely having physical contact with a companion probably has but a small effect. If the companion is active (nonfeeding), however, then social facilitation is much stronger. If the companion is feeding, the facilitation is greatest. These results are striking similar to those on chicks of the Laughing Gull, *Larus atricilla* (Hailman, *Behav. Suppl.*, **15**, 1967, Chapter V), and since the two species have such different ecological situations one thus suspects that social facilitation in feeding of young birds may be quite widespread.

Tolman's previous studies have pinpointed some stimuli that enhance social feeding, such as the tapping sound of a beak striking the surface on which food lies. Many of his previous studies are cited in the bibliography, and these are briefly summarized at the opening of this paper.—Jack P. Hailman.

32. Illumination and Wood Duck roosting flights. D. Hein and A. O. Haugen. 1966. *Wilson Bull.*, **78** (3): 301-308.—Or, "how to write a paper without revealing your data." The major lesson is to calculate repression lines and graph them without showing any data points. Then duplicate the graphed material in tables with lots of numbers. But be careful not to show standard deviations or some other measure of variation that might reveal something about variability in your data. Oh yes: then draw conclusions.—Jack P. Hailman.

33. **Spread-wing postures in Pelecaniformes, Ciconiiformes, and Falconiformes.** G. A. Clark, Jr. 1969. *Auk.* **86** (1): 136-139.—The cormorant-like postures occur in other birds and may have been independently evolved several times. The proposed function of "drying the wing feathers" is yet unproved (and Clark offers two other hypotheses for which there is no evidence whatsoever).—Jack P. Hailman.

34. **Provoked release of mobbing -- a hunting technique of *Micrastur* falcons.** N. G. Smith. 1969. *Ibis,* **111**(2): 241-243.—Here is another fascinating observation on adaptations of birds from the pen of Neal Griffith Smith. A few species of uncommon, noisy hawks attract potential prey to come mob them, and then pick off an obvious individual. Hawks with easily located calls call less frequently, since the strategy is to require the prey to get close enough to make visual contact. The detailed observations are worth reading.—Jack P. Hailman.

35. **Courtship and copulatory behaviour of the New Zealand Grey Duck.** M. Williams. 1969. *Notornis,* **16** (1): 23-32.—Konrad Lorenz sparked interest in the comparative study of duck display behavior that persists now a quarter century after his early studies. Paul Johnsgard summarized our knowledge of the subject in a slightly overtitled but magnificent volume (*Handbook of Waterfowl Behavior,* 1965). Despite the fact that we probably have a more complete index of display behavior in waterfowl, especially the ducks, than in any comparable group in the animal kingdom, Williams has here added descriptions and photographs of a previously little known species, *Anas superciliosa.* Basically, the displays are similar to those of the Mallard (*A. platyrhynchos*).—Jack P. Hailman.

36. **Territorial relationships of Blue-winged Warblers, Golden-winged Warblers and their hybrids.** M. S. Ficken and R. W. Ficken. 1968. *Wilson Bull.,* **80** (4): 442-451.—The great ethologist Niko Tinbergen has remarked that the division between experimental and observational biology is a false one, since Mother Nature will provide controlled experiments for the patient observer. Here is an example. The Blue-winged Warbler (*Vermivora pinus*) will hybridize with the Golden-winged Warbler (*V. chrysoptera*) and produce hybrids that have been given distinct names. The Brewster-type hybrid (*V. leucobronchialis*) looks something like a Blue-winged, but is less yellow; the Lawrence-type hybrid (*V. lawrencei*) looks something like a Golden-winged, but is more yellow. There are actually varying degrees of intermediacy. The hybrids sing songs that resemble either one of the parent species' songs, but song-type is not correlated (at least strictly) with plumage. Herein lies the experiment.

Birds that both look and sing alike exclude one another from their territories, regardless of what types they are. Birds that both look and sing differently have overlapping territories. Birds that look different but sing alike also have overlapping territories, but birds that look alike and sing differently exclude one another! Therefore, plumage (probably face pattern) determines recognition for territorial purposes. An interesting experiment.—Jack P. Hailman.

37. **Reproductive behavior of Hairy Woodpeckers. III. Agonistic behavior in relation to courtship and territory.** L. Kilham. 1969. *Wilson Bull.,* **81**(2): 169-183.—The interesting aspect of the latest report of this careful observer in the field is that the territories and pair relationships in *Dendrocopos villosus* are hightly variable among individuals.—Jack P. Hailman.

ECOLOGY

(See also 20, 25, 90)

38. On the methods of resource division in grassland bird communities. M. L. Cody. 1968. *Amer. Nat.*, **102**(2): 107-147.—This paper is in the mold of Robert MacArthur's work, in that it is concerned with factors regulating bird species' diversity and how to predict this item. By utilizing only two factors, vegetation height and its standard deviation, Cody finds he is able to predict the number of species of birds in grasslands, the differences in their feeding ecology, and the relative habitat separation of these species in the community. Predictions of South American grassland communities based upon studies of North American communities verified this hypothesis. For the habitats selected, these predictions held regardless of any existing grazing or irrigation regimes, suggesting that these areas contain a full quota of species optimally adapted to the present environment. Specifically, in each habitat no exceptionally large differences were noted in habitat utilization among the group of species present, and thus no large gap exists for exploitation.

Bill size and shape is invoked as a measure of differences in food preferences. Undoubtedly this is usually the case, though if used by itself this measure could occasionally lead to dangerous generalizations. Several behavioral measures also are presented. Among those considered, rates of foraging or intervals between foraging need not act as absolute separating factors between species. Though Cody states that measurements of feeding behavior are very consistent, not only from the same individual but from different individuals of the same species, difficulties sometimes arise in making such assumptions. Marked differences may appear in these factors in individuals of many species during the course of a season. Hence, where the measurements mentioned in this paragraph may be perfectly valid for the situations reported upon in this paper, one should use caution before using them as universally valid statements.

Food-specialization and food-overlap values are obtained from the bill measurements and behavioral observations.

At different grass heights space appears to be divided by different methods. Birds can use different methods of searching and pursuing in short grass and can work at different heights in tall grass. In medium heights such vertical division is not possible, but the habitat is divisible horizontally by differences in habitat selection. Apparently medium heights are too short to permit vertical division and too tall to permit a variety of different types of feeding behavior (such as by running rapidly or pursuing flying insects).

This paper represents an important extension of our knowledge and understanding of the factors important in determining whether an environment can be utilized successfully by a given species of animal.—Douglass H. Morse.

39. On the winter ecology of tits in Cisbaikal. (Ob ekologii gaichek v Predbaikale.) G. B. Zonov. 1969. *Izvestiya sibirskogo otdeleniya Akad. Nauk, SSSR, ser. biol.*, **1969**(5): 61-64. (In Russian.)—The two species concerned, Marsh Tit (*Parus palustris*) and Willow Tit (*P. montanus*), are so much alike as to be often confused in the field, but the former is more attached to riverside forests, while the latter is prevalent in mixed conifer-deciduous forests on southern slopes of elevations where it is 2-3 times the more numerous. The Marsh Tit is much more vegetarian than the other, as shown not only by stomach analyses, but also in having a decidedly more capacious digestive tract (a feature correlated with more vegetarian feeding), and in being able to subsist in cages on an almost exclusively vegetable diet. In winter the two species may forage in a common flock by day, but separate at night to roost in holes they dig in the snow.—Leon Kelso.

40. Some aspects of the ecology of migrant shorebirds. II. Aggression. H. F. Recher and J. A. Recher. 1969. *Wilson Bull.*, **81** (2): 140-154.—The important thing concluded is that intraspecific aggression is more frequent than interspecific aggression, a conclusion that does not necessarily follow from the data. For instance, "of 926 aggressive interactions scored involving Semipalmated Sandpipers [*Calidris pusillus*] in situations where other species were present, only 4.3 per cent were interspecific." The simple facts needed to get from that observation to the conclusion are not given, namely, the numbers of Semipalmateds and non-semipalmateds present. I am prepared to believe that intraspecific aggression also was of a higher "intensity" (according to a scored set of values) than interspecific aggression, but a statistical test establishing the reliability of the difference would have been desirable.—Jack P. Hailman.

WILDLIFE MANAGEMENT AND ECONOMIC ORNITHOLOGY

41. **Tetraonids as geographic resources.** (Geografiya resursov teterevinykh ptits.) O. N. Shubnikova. 1969. *Izvestiya Akad. Nauk, SSSR, seriya geograficheskaya,* **1969**(2): 55-59. (In Russian.)—From a joint session of the Russian Geographic Institute and the Moscow Naturalists' Society come the following resolutions on tetraonid conservation: (1) Intensifying and expanding long-term research on the effects of human social change in various ecological situations; (2) More work on banding and other methods of marking; (3) A critical reexamination of census methods employed; (4) Expansion of program of mapping upland game, particularly of population shifts and drumming localities; (5) Promotion of research on tetraonid food by biochemical analysis of items, especially those more preferred; (6) The adoption of research on artificial propagation of tetraonids in the Darwin Reserve and in the USSR General Genetics Institute.—Leon Kelso.

42. **A comparison of some broadcasting equipments and recorded distress calls for scaring birds.** J. C. Bremond, P. Gramet, T. Brough, and E. N. Wright. 1968. *J. Appl. Ecol.,* **5** (3): 521-529.—As a result of concern over birds roosting on airfields, considerable interest has developed in the possibilities of using recorded distress and alarm calls to repel them. This study is the result of a cooperative venture of English and French workers. The authors used playbacks of both local gulls and ones from France upon English birds. Playbacks of local French gulls were also run in France. When responding strongly, gulls usually approach the sound and fly overhead for some time before dispersing and completely deserting the area. Depending upon the strength of response, tests were divided into good and poor categories.

The English birds responded as strongly to the playback of the French birds as to playbacks of their own vocalizations. However, the recordings from France contained a wider variety of notes than the English playbacks; hence, the authors feel that the variety may have enhanced the response. Herring (*Larus argentatus*), Black-headed (*L. ridibundus*), and Common (*L. canus*) gulls all participated in flocks tested, and it is difficult to evaluate quantitatively the data presented, for no mention was made of the proportions of these species in the different experiments.

Tests were run under several conditions, with both low-fidelity and high-fidelity equipment. Under some conditions the response to high-fidelity equipment was superior to that of low-fidelity equipment. This was especially true under windy conditions, though in dense vegetation little difference was noted between the two types.

One further group of tests was run in France upon flocks of corvids, usually Rooks (*Corvus frugilegus*) and Jackdaws (*C. monedula*). These compared their responses to distress calls of Jays (*Garrulus glandarius*) recorded under different technical conditions. The recording made under poor technical conditions elicited a stronger response than the one made under better conditions. The authors again suggest that the basis for this difference is that a wider variety of calls was presented on the initial tape. From this result they conclude that a mediocre recording of a biologically effective call has a higher effective value than a good recording of a less effective call.

Unfortunately the authors never present the information to verify conclusively whether their major points are tenable; that is, that the recordings of French gulls played to English birds and the old recording of Jay vocalizations were more variable than the alternate selection. Neither do they present the critical information to indicate whether part of the variability of response is the result of differences of species composition in the test flocks. This study presents several interesting conclusions, but much additional work is necessary to verify them.—Douglass H. Morse.

CONSERVATION AND ENVIRONMENTAL QUALITY

(See also 18, 19, 41, 93)

43. Pesticide causing rapid extinction of the Brown Pelican. D. Perlman, 1969. *Can. Field Nat.*, **83**(2): 173-174.—All the fish off the coast of California by now are contaminated with DDT; this situation makes the egg shells of the fish-eating birds so fragile that they cannot endure more than a few days of incubation (see review no. **46** of Hickey and Anderson, *Bird-Banding*, **40**: 62, 1969). Repeated visits of observers during 1969 to the nesting colonies of *Pelecanus occidentallis californicus* from Anacapa to those on the Mexican islands of Baja California all gave the same grim picture of *no young whatsoever*. This situation poses a grave threat to marine (and raptorial) birds throughout the world.—Margaret M. Nice.

✓ **44. DDT residues and declining reproduction in the Bermuda Petrel.** C. F. Wurster, Jr. and D. B. Wingate. 1968. *Science*, **159**: 979-891.—1978 is the year that *Pterodroma cahow* will become extinct. The authors' estimate is based on the mean annual rate of decline of 3.25 percent over the last ten years. During March 1967 five unhatched eggs and dead chicks were tested for DDT and its metabolites (decay products, including DDE and DDD): all samples were contaminated. The residues averaged 6.44 parts-per-million, and ranged from a low 3.61 ppm of an addled egg to 11.02 ppm of another egg (a chick brain had only 0.57 ppm). Levels of the biocide in other birds high in the food web are reviewed, along with their recent reproductive failures. Then the literature on DDT in food chains is cited, along with the known effects of DDT in vertebrates on aggressive behavior, nervousness, weak egg shells, egg-eating of parent birds, enzyme-induction, indirect hormonal destruction and interference with calcium metabolism. Since DDT is not used on the Bermuda islands where the Cahow nests, one can only conclude that the biocide is widely dispersed in the world's seas. Ten years ago Wingate proudly showed me the species he had hoped to "revive from the dead" by a clever baffle preventing tropicbirds from utilizing the nesting holes of the petrel. He had thought that he had to beat only the tropicbird; now he must take on the entire biocide industry to achieve his ambition and the odds against him are incalculable.—Jack P. Hailman.

✓ **45. DDT residues and Bermuda Petrels.** L. A. McLean. 1968. *Science*, **162** (3839): 397.—Commenting on a paper by Wurster and Wingate (*Science*, **159**: 979, 1968; see review **44**), McLean charges that their comparisons with DDT residues in other bird species do not substantiate their alarming claims concerning the Bermuda Petrel. McClean argues that Stickle's 1966 report on the Bald Eagle (*Haliaeetus leucocephalus*) shows that few eagles die of pesticide poisoning, despite 10.6 parts per million pesticide residues found in the eggs. McClean also cites evidence purporting to show that the reproductive potential of the Herring Gull (*Larus argentatus*) is not declining, since the numbers of Herring Gulls are increasing, despite 120-227 ppm residue in their eggs. Therefore, McClean concludes, the finding of 6.44 ppm residues in Bermuda Petrel eggs is not a matter of concern. (Wurster and Wingate reply in the next paper reviewed.)—Jack P. Hailman.

✓ **46. DDT residues and Bermuda Petrels.** C. F. Wurster, Jr. and D. B. Wingate. 1968. *Science*, **161** (3839): 397.—Commenting on McLean's criticisms (review **45** immediately above), Wurster and Wingate note first that Stickle was talking about direct death from an overdose of pesticide, and at the time of her paper the effects of pesticides, through hepatic enzymes, on steroid hormones were unknown. (More on this later.) Second, one must distinguish between the reproductive potential of polluted Herring Gulls and the increase in total numbers of Herring Gulls. The total population is indeed increasing, due to man's pollution by dumping massive amounts of garbage and thus insuring a superabundant food supply, but the individual reproductive potential of residue-striken gulls is nevertheless declining.

To return to the enzyme problem, Wurster and Wingate cite recent studies showing that rats fed as little as 1 ppm DDT in the diet developed hepatic enzyme induction. The enzymes destroy not only the pesticides but also the steroid hormones; this, in turn, lowers reproductive vigor, and also interfers with calcium metabolism, so that birds may lay eggs with very thin shells that are easily broken.

McClean had argued (previous review) that 6.44 ppm were not dangerous to Bermuda Petrels because this "is less than the amount permitted in foods." Yet 1 ppm induces hepatic enzymes in rats. As Wurster and Wingate state, "we wish that environmental scientists could share MeLean's complacence . . ." about *both* petrels and people.—Jack P. Hailman.

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47. **Dieldrin residues in the Gallinules *Porphyrula martinica L.*, and *Gallinula chloropus L.* and its effect on clutch size and hatchability.** M. K. Causey, F. L. Bonner, and J. B. Graves. 1968. *Bull. Environm. Contam. and Toxicol.*, **5**: 274-283.—Comparison of level of dieldrin and other insecticides in eggs of birds in treated rice fields (56 nests) and control areas (17 nests), using gas and thin layer chromatography methods, finds in eggs of *G. chloropus* 65.1 mg. of dieldrin per kilogram; in *P. martinica*, 9.37 mg. per kg. No significant difference in clutch size or hatchability was found.—Leon Kelso.

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48. **Dieldrin and DDT: Effects on Sparrow Hawk eggshells and reproduction.** R. D. Porter and S. N. Wiemeyer. 1969. *Science*, **165** (3889): 199-200.—At the Patuxent Wildlife Research Center in Laurel, Maryland, sophisticated experiments have been carried out on a group of *Falco sparverius*, starting in 1964 with wild-caught fledglings. They were divided into three groups of eight pairs each: one as untreated controls, one as lightly dosed with the hydrocarbons, one as heavily dosed. Results are tabulated for 1967 and 1968 for the original birds, as well as for a group of 24 pairs of the first generation, all treated as their parents had been. Reproductive success was high in all control groups—"equal to that of a wild population," but low to very low in the experimental groups.

"Eggshells of dosed birds of the parental generation in 1968 were thinner by 8 to 10 per cent on the average than those of controls of the parental group; eggshells of the first-generation dosed birds were thinner by 15 to 17 percent on the average than those of first-generation controls." "The crucial factor responsible for reproductive failure of dosed birds was disappearance of eggs through time of hatching and may have included the disappearance of some newly hatched young." This is an admirably conceived and executed piece of research.

Incidentally I was mistaken in my review (No. 23 in the April 1969 *Bird-Banding*) in suggesting that Frau Koehler's success in raising *F. sparverius* in captivity from 1961 through 1968 was the first published record of the successful breeding of a falcon in captivity. Mr. W. O. Mattox of Canaan, N.H. courteously informs me that in Hens Weller's book *Der wilde Falk ist mein Gesell*, published by Neudam in 1962, the author tells of this feat with a Peregrine Falcon in the early 1940s. I understand that recently several North American falconers have induced successful breeding with both Peregrine and Prairie Falcons (*F. peregrinus, F. mexicanus*) as well as with a few "hawks."

For instance, Jimmy White of Cypress, California, reports of his pair of Harris Hawks (*Parabuteo unicinctus*): eggs laid April 24 and 28; hatched May 28 and 31; the young left the nest July 24. "He also mentions the diet of rats, chicks, and chicken necks." (*Raptor Research News*, **3**(2): 19. Apr. 1969—Margaret M. Nice.

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49. **Photochemical decomposition of DDT by a free-radical mechanism.** A. R. Mosier, W. D. Guenzi and L. L. Miller. 1969. *Science*, **164**: 1083-1985.—The reason that DDT and similar compounds are such effective biocides lies in their resistance to degradation. Here the authors irradiated DDT as a thin solid and in solution of hexane and were able to effect chemical decomposition using ultraviolet radiation (2537Å). Products included DDD, DDE and DDC=O. Cautious optimism leads to the hope that man might somehow devise a chemical means of "depolluting" his environment, but reviewers are supposed to think the unthinkable. Suppose we find a cheap, effective method for breaking down and dissolving away DDT and its cousins; is not the likely course of events that the method will be applied to our own food, but not generally to the environment ("too expensive")?—Jack P. Hailman.

50. An endangered Osprey population: estimates of mortality and production. C. J. Henny and H. M. Wight. 1969. *Auk.* **86:** 188-198.—Banding analysis from New York and New Jersey from 1926 to 1961 show an *annual* rate of decline of about 10-15%. (The ordinate in figure 1 is labelled "semilog", but of course it is logarithmic and the whole graph is semilogarithmic, lest any confusion arise.)—Jack P. Hailman.

51. The 1966 status of 24 nest sites of the Bald Eagle (*Haliaeetus leucocephalus*) in east-central Florida. J. C. Howell. 1968. *Auk,* **85** (4): 680-681.—The table shows these percentages of sites occupied at about five-year intervals since 1935: 46, 54, 67, 33, 21, 25. The habitat is being steadily destroyed.—Jack P. Hailman.

52. Nesting performance and pesticide residues in Alaskan and Yukon Peregrines in 1967. J. H. Enderson, D. G. Roseneau and L. G. Swartz. 1968. *Auk,* **85** (4): 683-684.—Fledging success was about normal in the *Falco peregrinus* observed. Tissue samples were taken from four trapped females, which had 130, 717, 754 and 2435 ppm (fat basis) of pesticide residue. The lowest is as low or lower than any found in the north, the highest much higher than any previously found there. No cause for rejoicing.—Jack P. Hailman.

PARASITES AND DISEASES

53. Synchronized life cycles in the Orange-crowned Warbler and its mallophagan parasites. M. S. Foster. 1969. *Ecology,* **50**(2): 315-323.—Studies were made upon Mallophaga (avian biting lice) of four races of Orange-crowned Warbler (*Vermivora celata*) in order to determine the relationship in timing of the cycles of host and parasite. They were based upon examinations of over 2,000 museum specimens collected from a wide range of localities during each month of the year.

Three species of mallophagans were found in the study. A close coordination exists between the breeding cycle of the warbler and its parasites (*Ricinus picturatus* and *Menacanthus* sp.), the parasite's cycle being determined by the presence of intact nits (eggs). A less precise relationship appeared between the host and *Philopterus* sp., a parasite that apparently normally does not feed on blood; however, data for this conclusion are rather limited. Two of the three species of mallophagans were identified only to genus as a result of the poor taxonomic condition of the groups, but Foster established to her satisfaction that all individuals of a genus in question belonged to a single species. She also found it possible to identify nits to species. One might initially question whether Mallophaga could lay eggs on Orange-crowned Warblers, even though they might not be regular parasites. However, species of Mallophaga are quite closely confined to specific hosts, and seldom do individuals of other species become successfully established, perhaps in part due to competitive exclusion. A close relation even exists between avian taxonomy and that of their mallophagan associates.

As pointed out by other workers, there appears to be a direct relationship between the breeding cycle of mallophagan parasites and the titer of reproductive hormones of their host. Foster found that the breeding season was the longest in Mallophaga parasitizing the race of warblers having the longest breeding season (*celata*). Also, slight increases in reproduction of *Ricinus* in the fall may be associated with the slight increase in reproductive hormone titer reported in several species of birds. Breeding of bloodsucking species is almost nonexistent during the refractory period of the host—also a time of molt and consequently one in which many nits might be lost. The fact that *Philopterus* appears not to be a regular blood feeder and that its reproductive cycle appears not to be as closely tied to its host as the other species is consistent with the hormone hypothesis. Nits were not found in juvenal birds, though adult Mallophaga were; in view of the fact that some activity of the reproductive hormones occurs in these birds, one might question the reason for their absence, since limited breeding of *Ricinus* occurs in the fall on adult hosts.

A fairly extensive citation section accompanies this paper (considering its length) and would serve as an excellent introduction to the literature of this fascinating group of parasites.—Douglass H. Morse.

54. Salmonellosis in wild birds. J. W. Macdonald and L. W. Cornelius. 1959. *Brit. Birds,* **62** (1): 28-30.—Persons who maintain feeding stations may not like the implications of this study. It reports 23 outbreaks of the virus *Salmonella typhimurium* in three months time in the south of England. Several strains (principally U218, U239, U19 and U165) were involved. Apparently House Sparrows (*Passer domesticus*) and Greenfinches (*C. chloris*) carry the infection. As long as the birds are not subjected to stress, little harm results. But when artificial feeding attracts large numbers of birds, disaster results with hundreds of deaths. "Offering smaller quantities of food except during short spells of very hard weather and changing the part of the garden where the food is put down would prevent the ground becoming heavily contaminated with droppings. The use of wire baskets or seed-hoppers instead of indiscriminate scattering of the food on the ground would also reduce the risk of picking up infection."—Jack P. Hailman.

55. A survey on the incidence of salmonellae in wild birds. G. P. Faddoul, G. W. Fellows and J. Baird. 1966. *Avian Diseases,* **10** (1): 89-94.—The discovery of the widespread incidence of salmonella infection in wild birds in Europe (review no. **48** in *Bird-Banding,* **40:** 161, 1969 and immediately preceding review) is apparently paralleled in the New World, since the first 100 wild bird consignments to the diagnostic laboratory revealed 12 birds from Mass. and R. I. having the infection. All were *S. typhimurium* except for one case of *S. derby,* and most came from Brown-headed Cowbirds (*Molothrus ater*), although *Passer domesticus, Larus argentatus* and *Zonotrichia albicollis* were also infected. As is previous reports, incidence of disease is high in winter when birds are crowded together at feeding areas.—Jack P. Hailman.

PHYSIOLOGY AND PSYCHOLOGY

(See also 13, 33, 77, 106, 107)

56. Winter fat deposition and overnight survival of Yellow Buntings (*Emberiza citrinella L.*). P. R. Evans. 1969. *J. Anim. Ecol.,* **38**(2): 415-423.—In view of recent statements that inadequate information existed to determine the exact role of temperature in regulation of fat deposition, Evans conducted a study on Yellow Buntings (Yellow Hammer, in the B. O. U. Checklist) designed to investigate the problem. From 10 to 20 birds were captured at a roost near Oxford, England, during each of nine nights from early November to early March. Analyses were made of the weights of undigested food in gullet and gizzard, fat, and lean dry material (largely muscle, protein, and skeleton). Lean dry weight was chosen as a measure of body size. This measure appears to be a more accurate one for Yellow Buntings than more conventional measures such as wing length (though not in Bullfinches, *Pyrrhula pyrrhula*). Wing length was discarded because of the apparent abrasion of the longest primaries during the period of the winter and the variations in length associated with age within a given sex. Data were also taken on water content and feather weight.

Total energy reserves were calculated as fat stores and food in crop and gullet. The difference between these reserves and calculated required energy is highest in January. One would expect that in most years crises would occur with maximum frequency at that time. Days are still very short and weather maximally severe (the 20-year average for each of the three sampling days in January was lower than the average for any other sampling date). Both the energy stores in fat and crop and gizzard increased early in the season more rapidly than did the energy requirement, and they declined more rapidly after January than the energy requirement. However, the mean value of reserve energy did not at any time approach the requirements, and the line of 2 standard deviations only crossed it barely at one time (6 November). Hence, the period when the greatest energy reserve exists is the one of greatest probable environmental demands.

Though a high correlation exists between mean fat content and long-term average temperatures, a significant correlation was not found between mean fat content and the mean temperature of the day of capture or the day or night before. Thus Evans concludes that temperature does not *directly* influence fat deposition, a situation that contrasts with that of the Bullfinch. Neither did a significant direct correlation exist between day length and fat content. Hence he concludes that temperature acts as an ultimate factor. Long-term average temperatures show a slightly higher correlation with fat reserves than minimum temperatures. A close correlation was noted between mean long-term temperatures and the day length 30 days earlier, accounted for by the lag occurring in heat loss by the earth. Hence, day length could be the proximal factor acting in regulating fat deposition.

These data were compiled only for the winter of 1966-67, but do indicate that at least for that year energy requirements were considerably less than the average energy reserves estimated. Since this population of buntings lives largely on spilled grain, it is questionable whether a food shortage often exists and whether the 1966-67 data would differ markedly from other years. Hence, food availability probably did not influence the extent of fat deposition in this study.—Jack P. Hailman.

57. Tolerance of cold and Bergmann's rule. S. C. Kendeigh. 1969. *Auk*, **86:** 13-25.—The report contains a large tabular literature review on metabolism of birds, which leads to several conclusions: nonpasserines are affected by cold more than passerines and small birds more than large ones. Northern species are less affected by cold than southern ones of similar size, have greater feather insulation and higher metabolic rates. Therefore, Bergmann's rule (individuals of a single species get larger as one goes toward the poles) has a possible physiological explanation, but the advantage must outweigh the disadvantage of having to maintain a greater body size in the colder regions.—Jack P. Hailman.

58. Comparative adaptations of the Alaskan redpolls to the arctic environment. W. S. Brooks. 1968. *Wilson Bull.*, **80** (3): 253-280.—The highlight conclusions of many experiments on the Hoary Redpoll (*Acanthis hornemanni exilipes*) and the Common Redpoll (*A. f. flammea*) are these: they can survive temperatures colder than any passerine thus far reported; they have a high rate of energy intake, due in part to a special storage pouch (esophageal diverticulum), which is filled just prior to darkness; they continue activity at low light intensities; they select high calorie foods (e.g., birch seeds) over others; they can increase digestion efficiency at low temperatures; and their plumage may provide better insulation against cold than that of non-arctic birds.—Jack P. Hailman.

59. Bioenergetics of the flying bird. (Bioenergetica letyashchei ptitsy.) V. R. Dolnik. 1969. *Z. obshchei biologii,* **30**(3): 273-291. (In Russian, English summary.) This is a summation of all energy studies available to the author pertinent to bird flight, including many European and American titles (42 titles in all). There are sections discussing the principles expounded by Brody and others, and new data from his own experiments. Points particularly emphasized are, that "in flight energy expenditure not involved in flight equals standard metabolic energy"; that "during flight not more than 15% of heat is expended via respiratory evaporation," so water loss is slight; that "energy of prolonged flight may not exceed 16″ standard metabolism values and usually approaches that, being lower in distant migrants and in aerial feeders; that available flight power increases with body weight at a ratio of 3 to 4, but power *required* increases at a 7 to 6 ratio. This inequity limits the size birds could attain evolutionarily and yet be able to fly, and also limits the weight of fat they can carry during migration.

In this connection may be noted: Power requirements for horizontal flight in the Pigeon, *Columba livia* by C. J. Pennycuick, *J. Exptl. Biol.*, **49:** 527-555, 1968; and, a wind-tunnel study of gliding flight in the Pigeon by C. J. P., *ibid.*, 509-526. These studies arrive at conclusions in line with the above, but note that the bird's performance is very poor by engineering standards. This is reminiscent of those engineering principles whereby the bumblebee should not be able to fly at all.

Of some significance in the long run might be the findings in a companion article: On photoreceptive and photokinetic activity of insect wings (*O fotoretseptornom i fotokineticheskom deistvii krylev nasekomykh*) by N. K. Alekseev, *Z. obshchei biologii,* **30**(3): 292-302, 1969. Here is developed the concept that not only the eyes but the wings, particularly those with parallel venation, receive and utilize radiant energy, and that this fact is involved in the lure that ultraviolet light traps exert on certain insects. It is emphasized that night-flying insects have wings with predominantly parallel venation more responsive to weaker radiant energy, and by day they retire to shaded retreats; day-flying insects have predominantly reticulate wing venation and are accordingly adapted to strong light. Bird feathers have an abundance of fine paralleliform structure and should thus be receptive of molecular kinetic and radiant energy, and this might be the missing factor in bioenergetic studies to date.—Leon Kelso.

60. Ecological evidence of adaptive forms of avian behavior in response to stationary food stimuli. (Ekologicheskie obosnovaniya adaptivnykh form povedeniya ptits pri reshenii zadachi na nepodvizhnyi pishchevoi razdrazhitel.) A. P. Krapivnyi and V. V. Turchaninov. 1969. *Z. Zhurn.*, **48**(5): 717-721. (In Russian, English summary.)—In 8 bird species (Caracara, *Polyborus* sp.; Egyptian Vulture, *Neophron percnopterus*; Honey Buzzard, *Pernis apivorus*; White Stork, *Ciconia ciconia*; [European] Siskin, *Carduelis spinus*; Greenfinch, *Chloris chloris*; Rook, *Corvus frugilegus*; and Parakeet, *Melopsittacus undulatus*), having different ecolo-morphological characteristics, the rate of development of conditioned reflexes in feeding from test tubes was recorded. Time required varied according to species, the number of errors likewise varying. By a described method, rates of learning and forgetting of reflexes were calculated. The authors were much intrigued by the similarity of the patterns of errors, learning and forgetting in Siskins and Parakeets, regarding this as due to the similarity of their original ecological niches in Europe and Australia.—Leon Kelso.

61. Receptors sensitive to carbon dioxide in lungs of chicken. D. F. Peterson and M. R. Fedde. 1968. *Science*, **162:** 1499-1501.—The control of breathing rate in mammals depends not primarily upon the oxygen concentration in the blood, but upon the carbon dioxide waste produced by respiration. In mammals, the CO^2 level is monitored by a receptor in the carotid body, but birds are more sensitive to CO^2 levels and respond faster than do mammals. Mammals have receptors in the lung, but they appear to be stretch receptors not sensitive to CO^2. No stretch receptors have been found in the chicken lung, but the receptors there are CO^2-sensitive, as demonstrated by the experiments reported.—Jack P. Hailman.

62. Pineal function: the biological clock in the sparrow? S. Gaston and M. Menaker. 1968. *Science*, **160** (3832): 1125-1127.—The results of this study are amazingly clearcut. House Sparrows (*Passer domesticus*) maintain a circadian activity rhythm in captivity that persists under constant dark conditions. If the pineal organ is removed, the rhythm is abolished under dark conditions, but can be established and entrained by a light-dark cycle. Sham-operated birds maintain the constant-dark rhythm.

This is an extremely competent study of a confusing phenomenon. This report establishes the pineal organ as a critical part of the endogenous timing mechanism underlying activity rhythms in the House Sparrow. I had the privilege of hearing Susanne Gaston present this work before the 1968 International Congress of Photobiology, and there she additionally talked about transplanting pineal organs from one bird to another. If those subsequent experiments went as well as these, we have a treat in store in her next publication.—Jack P. Hailman.

63. Following during imprinting as a function of auditory stimulus intensity. G. J. Fischer and S. C. Gilman. 1969. *Develop. Psychol.*, **1** (3): 216-218.—The results **appear** to be straightforward: when domestic chicks are exposed to various sound intensities of tapping, they follow most when the tapping is about 5 to 25 decibels above noise level. The authors conclude that this is an example of Schneirla's theory that high stimulus intensities elicit withdrawal from the stimulus source, low intensities approach toward it.

However, the chicks do not show active withdrawal from the stimulus source at any intensity. Furthermore, the peak of following does not come at the lowest intensity, as demanded by the "theory." (Clearly the chicks hear the stimulus even when it is below the noise level, since following to stimuli below noise level is greater than following when there is no stimulus and just noise.)

But even if the results are viewed without the complications of an inapplicable "theory" there is some doubt about their meaning. Stimulus intensities were measured 6 inches from a loudspeaker-containing model that moved circularly around an apparatus, and "following" was defined as when the chick was within 1 foot of the model. Thus, a chick consistently following 13 inches behind the model is scored as a non-responder. Further, the signal and noise levels at the chick's ear cannot be specified. It could be, for instance, that the chick positions itself at such a distance from the model that the sound reaching its ear is of a particular intensity. Testing for an intensity preference in imprinting is much more difficult than it first appears, and clearly more experimentation will be needed.—Jack P. Hailman.

64. The Phalarope. E. O. Höhn. 1969. *Sci. Amer.*, **220** (6): 104-109, 111. —The physiologist Otto Höhn has been studying the hormones and behavior of phalaropes for a number of years, and it is useful to have this popular overview of his studies on these most interesting birds in which the male and female have reversed roles in almost all phases of reproduction (except, of course, egg-laying). —Jack P. Hailman.

65. Physiological conditions for the stimulation of prolactin by external stimuli in the male ring dove. M. Friedman and D. S. Lehrman. 1968. *Anim. Behav.*, **16**: 233-237.—Our understanding of the interactions between hormones and behavior is probably more complete for *Streptopelia risoria* than any other bird, and perhaps any animal at all. This fact is due largely to the early work of Riddle and the contemporary work of D. S. Lehrman's laboratory. In this contribution, males were removed from their mates and separated from them by a glass plate at various times in the reproductive cycle. They were given a set of eggs to incubate for a test, and the engorgement of their crops (indicating readiness to feed young) was measured on the 13th day after laying of the second (last) egg. Basically, the longer these males are with their females before being separated by the glass, the better the incubation tendency and crop development at the usual hatching date. Incubation and crop development reach normal levels only if the male participates in incubation for a couple of days before being separated. The authors conclude that prolactin secretion (indicated by crop development) is maintained by the visual contact, but it is hard to see how this conclusion is reached in the absence of the obvious controls. That is, why were there not males that were separated at various times, but then *not* allowed to watch the female during the interval between incubation test and autopsy? The authors may be correct in asserting that visual stimulation is necessary, but they have not shown this to be true.—Jack P. Hailman.

66. Hormonal initiation of parental behavior in inexperienced Ring Doves. D. F. Lott and S. Comerford. 1968. *Z. Tierpsychol.*, **25** (1): 71-75.—This is the latest contribution to a fascinating problem, one that is exemplary of how hormones interact with behavior. In 1935 the great endocrinologist O. Riddle claimed that the hormone he discovered, prolactin, directly activated the mechanisms leading to the feeding of the squabs of *Streptopelia risoria*. Twenty years later a study by D. H. Lehrman challenged Riddle's belief, since inexperienced doves injected with prolactin did not feed foster squabs, while experienced birds did. Further, when the crop was anesthetized, injected experienced birds did not feed either. Lehrman's theory was that prolactin causes crop engorgement, which leads to production of the crop "milk" of sluffed-off cells, but experience with squabs was necessary to develop the feeding response, since the squabs created a tactile irritation that produced an emetic effect, and hence the first feeding.

A decade passed before Hanson's experiments toppled Lehrman's theory. Inexperienced Ring Doves feed young prior to any crop engorgement, thus ruling out the emetic effect (and other workers found similar feeding in another species).

The authors of this paper used inexperienced male Ring Doves and injected with various combinations of hormones. Progesterone alone established brooding behavior, and the addition of prolactin caused them to feed. However, those injected only with prolactin neither brooded nor fed. (Birds with progesterone alone were observed to feed briefly on occasion.) Apparently, then, Ring Doves do not have to learn to feed their young *per se*. Prolactin produces feeding if progesterone is already present. Experienced birds may not require the externally supplied progesterone, but have enough present as the result of their experience in order to "prime" for the prolactin.

This series of studies seems to me to be a perfect example of the scientific method at work, with the cycle of data-hypothesis-rejection with new data-new hypothesis continually applied. That the fall of Lehrman's theory comes from two papers produced in his own laboratory rounds out the tribute nicely.—Jack P. Hailman.

67. **The annual cycle of nest building and agonistic behavior in captive *Quelea quelea* with reference to endocrine factors.** P. A. Butterfield and J. H. Crook. 1968. *Anim. Behav.*, **16**: 308-317.—The species is a ploceine weaver from Africa that apparently has no standard English common name. Nestbuilding, agonistic encounters and plumage were noted throughout a year in a group of six males. Nestbuilding did not correlate with the other two variables, but they were positively correlated with one another. Nestbuilding does correlate with other seasonal records of testis size. Thus nestbuilding may be under testosterone control while aggression may be under gonadotroptin control (from the pituitary). Valuable data are also given on individual birds as well as the group means, a habit that should be encouraged among investigators. The most stable positions in the dominance hierarchy were the bottom and top birds, which, among other things, shows that the finding by Gilbert about leadership rank in fallow deer (Gilbert and Hailman, *Nature*, **209**: 1041-1042, 1966) is not general: that is, that uncertainty in rank is correlated negatively and monotonically with mean rank. If we are to understand social organization organization we need more studies on the variation in structure over time, not just a set of static means. This is a nice paper, with the results thoroughly analyzed.—Jack P. Hailman.

68. **Effects of testosterone propionate and luteinizing hormone on agonistic and nest building behavior of *Quelea quelea*.** J. H. Crook and P. A. Butterfield. 1968. *Anim. Behav.*, **16**: 370-384.—This paper, a followup to the previous one (review **67**) should dispel some myths. Injecting testosterone in low ranking males did not increase their chances of winning an aggressive encounter, but injection of LH did. Testosterone did evoke more nestbuilding. However, neither the dominance hierarchy based on individual distance nor a "hierarchy" based on squabbles over nesting material correlated with amount of nest building, although testosterone-injected birds showed more interest in nesting material.—Jack P. Hailman.

69. **Color preference shift in hungry and thirsty pigeons.** J. Delius. 1968. *Psychonomic Sci.*, **13**: 273-274.—The major finding is that when hungry adult pigeons peck at yellow-green, green and green-blue stimuli; when thirsty, at green-blue and blue. The conclusion drawn is that a shift in the kind of motivation causes a change in color perception, and several alternative and ingenious suggestions as to what this change may be are offered.

The experiment demands closer scrutiny. The stimuli are not specified (other than by apparent hue to the human observer). The title suggests that the behavior is a spontaneous preference, but it is actually more complicated than that. The birds were first pre-trained to peck at keys, with grain as a reward. Then, they were either deprived of food or water and tested in an eight-key apparatus without rewards in order to determine the "color preference." The first question is: were the birds really acting under different *kinds* of motivations in the two situations? A bird may get no nutrient from water, but it does get (metabolic) water from its food. Does 18 hours of water-deprivation create the same thirst as 18 hours of food-deprivation? If not, then the difference in motivation might be one of strength rather than kind.

The second question is whether we are really dealing with a "preference" in the usual sense of an unconditioned selectivity. Since the birds were rewarded for pecking at yellowish seeds before the critical experiments, is there not a possibility that a generalization from yellow seed to yellow-green stimulus cards took place? Indeed, the thirsty pigeons show a secondary response peak at yellow-green. The third question is whether or not we are dealing with "color", since the physical aspects of the stimuli are not reported at all.

It would have been helpful to have an entire second experiment in which the pretraining used water as a reward instead of food. Additionally, experiments at various levels of food and water deprivation could act as controls for shifts due merely to the level of motivation. Lastly, the experiments should utilize some control over the physical stimuli. As it stands, the reader cannot at all be sure that the experiment deals with color, or preference, or hunger and thirst.—Jack P. Hailman.

70. The ontogeny of a pecking preference in domestic chicks. R. Dawkins. 1968. *Z, Tierpsychol.*, **25** (2): 170-186.—The preference alluded to in the title is a preference for solid over flat shapes. Chicks shown a hemisphere and a circle peck more at the hemisphere, and increase the percentage of pecks at the hemisphere over the first three days of life. However, in chicks with experience pecking at the stimuli, the total number of responses decreases, even though the percentage of responses to the hemisphere increases. (Data on the total responses not given.) It is also stated that chicks without previous experience *increase* the total number of responses with age, while also increasing the percentage of pecks to the hemisphere (again, data on total responses not given).

A test was then made to see if waning of responsiveness is stimulus-specific, by rearing some chicks with two hemispheres and some with two circles for a day, and then giving them the choice test. The chicks reared on circles greatly preferred the hemisphere (23 to 0, of 63 chicks tested). However, the chicks reared on hemispheres split their responses 5-5 (only 10 of 36 doing any pecking at all). The author concludes that waning is stimulus-specific, but this conclusion seems precarious. First of all, a control group (reared with neither shape) had only 12 chicks as opposed to three times that many for each experimental group. Table 1 states that nine of these controls pecked, but the preference figures given are 7-to-1 for the hemisphere. Given that 7-to-1 is correct, we may then ask if the experimental groups differ from this figure significantly (the author does not use his control data, but only directly compares the two experimental groups in the reported statistics). The 23-to-0 ratio certainly does not differ from 7-to-1 reliably, and a Fisher exact test that I calculated shows that the 5-to-5 ratio is nowhere nearly significant, even with a onesided test. In essence, neither group can be proven to differ reliably from the control, even though the tendency does seem slightly in favor of a stimulus-specific waning.

A further test was run to see if waning of responsiveness was generalized from the rearing environment. Chicks were reared either in flat boxes, as before, or in boxes lined with corrugated cardboard, so that they could not peck at flat surfaces during rearing. Eight such experiments were run, with sample sizes not reported; the experiments differed in ways "which are not important" (and not otherwise specified). In all eight experiments, both groups chose the hemisphere over the circle in tests. Only the briefest outline of the data is presented in table 2, which depends on the number of chicks that directed their *first* peck at a particular stimulus (nothing whatever is said about the overall percentages to the stimuli—the data relied upon in the previous experiments). The flat-reared birds do seem to show a slightly lower preference for the hemisphere, but no statistical tests are mentioned, only a conclusion: that the waning—which the author believes to have established in the previous experiment—generalizes. The reader simply cannot evaluate this second conclusion.

Next the author reared a group of chicks in the dark and then tested them on the second day after hatching. No control group was run (because of lack of chicks, it is stated), but this is extremely unfortunate in view of the author's earlier stress on the great batch-to-batch variation in responsiveness of these White-leghorns. The results show a preference for the hemisphere (12 chicks to zero out of a total not reported). The author concludes that visual experience is not necessary for the development of the preference, and although I am prepared to believe the conclusion on faith, it certainly does not follow from an experiment that lacks the critical control.

Next, to see if the preference for the three-dimensional object was dependent upon binocular vision, experimental chicks were treated by covering one eye or the other. Unfortunately, the text does not tell us whether the chicks were "blindfolded" during rearing and testing, or during testing only. In view of the previous conclusions by the author that visual experience has a considerable effect, the rearing conditions in this experiment are absolutely critical to its interpretation. The groups showed no difference (10-to-2 in favor of the hemisphere in both groups, out of 28 experimentals and 20 controls). The author's conclusion is that "binocular vision is probably of little if any importance in this discrimination." However, if the experimentals were *reared* (not just tested) monocularly, then they might have learned to use other depth clues, even though they would ordinarily depend upon binocular stereoscopy.

The remaining set of experiments tested the effects of "lighting from above" in the recognition of solid objects over flat ones. In the first experiment, photographs of the hemisphere and circle were presented, with the hemisphere lighted from above in half the trials; in the other half the photos were inverted, so that the hemisphere appeared to be lighted from below. The chicks pecked more at the hemisphere photo in the ordinary orientation (14 groups to 1 group, out of 32 groups of 6 simultaneously tested chicks), but divided their responses when the hemisphere photo was inverted (8 groups to 8 out of 32). These were light-reared groups, so that how this preference comes about must wait for completion of this series of experiments. Next, the real hemisphere was added to the photographs (in correct orientation) and given in the three possible pairs to three different groups of chicks. Choices were not the same in the three groups. All preferred the hemisphere photo to the circle photo, as in the previous experiment; two of the three preferred the real hemisphere to the circle photo and one out of three preferred the real hemisphere to the photo. In experiment C, 15 groups of chicks preferred the photo and only 7 the real hemisphere. Lastly, dark-reared chicks were tested on the photo of the circle and the correctly oriented photo of the hemisphere. Ten delivered the first peck to the hemisphere photo, nine to the circle photo, and a total of 37 chicks was used. Tests were "discontinued" if the experimenter thought the chick would not peck (no given cutoff time). Chicks of 1, 2, and 3 days were tested, but the results are not broken down by age. There was no simultaneous control with light-reared chicks, and the first experiment in this series is not a good control because of batch-to-batch variation, differences in stimuli presented, differences in age, and differences in the way the choice between stimuli is reported. It would seem that nothing whatever can be concluded from this experiment, even in conjunction with the others.

In the last experiment, chicks were reared with light coming up through the floor, rather than from above as usual. The 2-3 day-old chicks pecked more at the correctly-oriented hemisphere photograph than at the inverted one. This experiment has no obvious flaws, although it would be interesting to see what effect long-term rearing in this kind of an environment would have.

All the conclusions of this study seem reasonable enough on an intuitive basis, but only the last experiment seems to be a trustworthy one. It is a great shame that so many clever experimental ideas were so poorly executed that the conclusions are left in serious doubt. Dawkins is obviously a bright worker, but he needs to gain further experience in running, analyzing and reporting experiments. But then, experimental ability is not "inborn . . . in the sense of Lorenz's dichotomy between phylogenetic and ontogenetic sources of adaptation to particular environmental factors."—Jack P. Hailman.

71. Adaptation and negative aftereffect to lateral optical displacement in newly hatched chicks. P. J. Rissi. 1968. *Science,* **160** (3826): 430-432. —Goggles displace the image 8.5 degrees, and chicks increasingly compensated. After four days the strike was off by about 3.6 degrees, after eight days by only 1.5 degrees. Then the prisms in the goggles were replaced by clear glass and the chicks overcompensated, missing the target to the opposite direction by about the same displacement (1-2 degrees). A nice, clean experiment.—Jack P. Hailman.

72. Optomotor responses to monocular stimulation: relation to visual system organization. E. S. Tauber and A. Atkin. 1968. *Science,* **160** (3834): 1365-1367.—Every bander knows that if you hold a bird by its body with its head free, and then rotate the body, the bird holds its head fixed in space—or, more exactly, it turns its head in the opposite direction in order to compensate continually for the visual displacement you cause by rotating the bird. This phenomenon can be dealt with experimentally, by placing the bird (or other animal) inside a cylinder with vertical stripes. Rotation of the cylinder makes the animal turn its head (or eyes) in order to keep a constant visual input, until a maximum head-turning posture is reached, at which point the head (or eyes) snaps back, the animal looks straight ahead, and then immediately begins turning with the stripes again.

It turns out that if one blindfolds one eye of the animal that this optomotor response may or may not occur. In some species it occurs whichever direction the stripes move, whereas in others it occurs only when the stripes move from the uncovered eye toward the covered one. The question is, then, what does this difference tell us about the organization of visual systems?

One theory was that animals showing the unidirectional type of monocular optomotor response have optic fibers that cross completely to the other side of the brain, whereas animals showing the bidirectional monocular response have the optic tract splitting and going to both sides of the brain. (Just why one would propose this cannot be told from the report—it appears to be one of those unfounded generalizations based on three laboratory species of animals.) The authors propose an alternative theory, and then attempt to test it with four species of mammals, 19 reptiles and 17 birds.

The new theory basically is this. Animals that have retinal foveas (deep pits in which receptor cells are tightly packed for high visual acuity) focus the image of objects on such foveal areas, and track the object while they themselves are moving. Therefore, foveate species must not only have to move the eyes or head in order to compensate for their own body movements, but also for the movement of the object they are tracking. Afoveate species presumably do not track prey objects, so only need to compensate visually for their own movements. When they move forward in the usual manner, the environment appears to move from their nose toward their side—which is equivalent to the stripes moving from the covered eye toward the uncovered one in the monocular experiment. Therefore, having no need to track moving objects (and no fovea to do it with), afoveate animals must ignore nasal-to-lateral movements of the environment. (The logic needs to be extended beyond that in the paper. If a moving afoveate animal would respond to nasal-to-lateral movement, it would have such input in both eyes, these could cancel; if the animal winked, got something in one eye, or was diseased or wounded in one eye, then it would continually turn its head in the direction of the good eye.) The prediction, then, is that foveate animals will show bidirectional monocular optomotor responses (with head or eye or both), whereas afoceate animals will be unidirectional.

The data, mostly from animals at the Bronx Zoo, reject several possible correlations between the type of response and other variables: reptiles, which apparently all have complete crossing of the optic tracts, show both uni- and bidirectional responses; and, the type of response is not correlated with whether the eye alone is moved, the head alone, or both. But whether the results add any weight to the authors' own theory is another question.

The results for mammals and reptiles are set out in detail in table 1, but the birds are listed by common name only (2 exceptions with Latin names) in a footnote. Put simply, all mammals tested are unidirectional, all birds bidirectional and the reptiles are mixed. True, all bidirectional reptiles are foveate and all unidirectional ones are afoveate; but note that all foveate reptiles have cone retinas and all afoveate reptiles have purely rod retinas. Therefore, we are faced with a double confounding of variables: first, mechanisms may differ in the different vertebrate classes, and second, the response may correlate more with the type of receptor dominating the retina rather than the presence or absence of a fovea.

For a critical test then, we must find a nonreptile with a cone retina and no fovea, or with a rod retina and a fovea. Only two animals from this study fit the bill: the prairie dog (*Cynomys ludovicianus*) has an afoveate cone retina and shows the unidirectional response, whereas the similarly equipped woodchuck (*Marmota monax*) refused to show a response to either direction viewed monocularly (though it did show the usual binocular response). So the whole question boils down to four individuals of one species, plus some citations from the literature that are of questionable value for direct comparison because slightly different methods were used. The literature citations are these: the pigeon and chicken show unidirectional responses and supposedly differ from all other birds by being afoveate, yet having cones.

The theory is ingeneous, and possibly correct. However, it is worrisome that the possession of rods or cones, and the possession of fovea or not, was determined by relying on old classic reviews such as Walls's *Vertebrate Eye and its Adaptive Radiation* (which the authors cite as a 1963 publication, whereas that edition is merely a reprint of the 1942 classic, which is not only riddled with errors, but cites precious little of the literature from which it was compiled). In other words, I think the entire study is based on shaky information from the review literature, concerning the structure of various species' retinas. The way to proceed would be to find modern histological studies that show the receptor type and the presence or absence of a fovea; from such reliable data, pick afoveate cone species and foveate rod species (if any exist), and then carefully test these animals. Without these critical tests, the authors' hypothesis remains merely an intriguing suggestion.—Jack P. Hailman.

73. Visual field dependency in pigeons. D. R. Thomas and J. Lyons. 1968. *Anim. Behav.*, **16**: 213-218.—If pigeons are trained to peck at a key with a vertical line on it, and then the line is shown in various orientations, they peck most at the vertical and then increasingly less as the stimulus departs from verticality (the phenomenon of stimulus generalization). The question is how do they judge verticality: by the angle with the floor or the angle with gravity? In other words, what do they really learn in the training? To find out, pigeons were trained as usual and then the whole cage was tipped; they responded to the stimulus that was perpendicular to the floor, not the one normal to gravity. This result shows that the choice learned was that of the stimulus perpendicular to the floor, but does not demonstrate whether it was learned as a straight-forward visual angle discrimination or learned as an angle with gravity that was subsequently "converted" to an angle with the floor (or other aspects of the visual environment, such as parallel sides of the chamber). An experiment is briefly described in the discussion section to test this, yet occupies half of the summary. Birds were trained in the dark except for the light from the stimulus key and from the food hopper, and then tested in the tilted apparatus, where they apparently performed the same as the others. This seems to show that the discrimination is learned (or can be learned) as an orientation to gravity, and then utilized as a visual angle with the floor. But the lights in the apparatus leave some doubt about the real absence of visual cues during training (and certainly the birds can get some information about the angle of the stimulus with the floor through tactile information from the legs and feet). It is interesting to note that human beings also depend largely on visual clues for perception of the vertical.—Jack P. Hailman

74. A test to determine if Bobwhite Quail hunt crickets by sound. J. J. Whitesell and T. J. Walker. 1969. *Auk*, **86** (2): 348-349.—When Starlings (*Sturnus vulgaris*) caused the crash of an Electra aircraft at Logan Field in Boston, it was suggested that the birds might have been attracted to the engine noise, which resembled songs of crickets. However, tests revealed no striking similarities in the songs, nor did Starlings locate crickets by sound in some tests. Bobwhites (*Colinus virginianus*) are known to eat crickets, so further tests were run to see if they could hunt by *Gryllus* sounds. They did not.—Jack P. Hailman.

75. The brain of birds. L. J. Sttner and K. A. Matyniak. 1968. *Sci. Amer.*, **218** (6): 64-68, 73-76.—Some persons do not consider the term "bird-brain" to be perjorative. (Wouldn't the Navy be happy to build a navigational system in submarines that was only the size of a homing pigeon's brain?) It seems to me that a number of factors have conspired to degrade our assessment of the avian intellectual capacities. First, an Aristotelian view of the "natural scale of animals" always places *Homo sapiens* at the pinnacle, looking down on his mammalian relatives (both smart and dumb), with the birds next and so on. The view incorporates nicely two misconceptions: first, that merely because there is a general trend in evolution toward greater intelligence, the phylogeny of a group such as the vertebrates is also a unidirectional path of increasing intelligence; and, second, that mammals are somehow "higher" than birds, despite the fact that both sprang independently from reptilian ancestors. (The evolutionary tree is not a straight pine branching only at the top, but, rather, a sturdy oak branching everywhere as it grows.)

The second problem is that birds have small brains. They must, in order to fly without undue weight. Furthermore, birds don't do things we consider intelligent; which really means they don't use their hands (which happen to be all covered with feathers). As if that were not enough, it turns out that the brain tissue in birds is mainly made up of kinds of striatum, rather than the cortex we champion as the seat of our own intelligence.

Well, the upshot of the story is that psychologists are finding that birds do right well in conditioning apparatus, despite their deficient cortical material, and, as the subtitular material asserts "recent work suggests that they may simply use other parts of the brain to effect intelligent behavior." Now, even if you arrive at this conclusion through deduction, you might like to see some photographs of real bird-brains in cross-section.—Jack P. Hailman.

76. Effects of age on taste discrimination in the Bobwhite Quail. L. D. Brindley and S. Prior. 1968. *Anim. Behav.*, **16:** 304-307.—None.—Jack P. Hailman.

MORPHOLOGY AND ANATOMY

(See also 26, 75, 107)

77. On the optic retinal structure of gulls and terns. (O stroenii setchatki glaza chaek i krachek.) K. V. Avilova. 1969. *Vestnik Moskovskogo Univ., Biol.-Poch. ser.*, **24**(2): 121-123. (In Russian.)—This is a microhistological study of the optic retina in the Black-headed Gull, *Larus ridibundus*, White-winged Black Tern, *Chlidonias leucopterus*, Little Tern, *Sterna albifrons*, and (for comparison with a non-larid species) Bee-eater, *Merops apiaster*. Comparative measurements of a few features of retinal structure are recorded to find possible correlations with ecological niches occupied and feeding habits. For the 4 species respectively the thickness of the outer retinal layer was, 9.0, 7.2, 3.6, and 3.6 micrometers (formerly microns); the respective length of the outer segments of the photoreceptors was 30.6, 34.2, 27.9, and 18.0 um. The higher measurements of the first 2 species are regarded as a correlation to a more shaded habitat and more crepuscular feeding activity. Remarkable is the density of photoreceptive cells, in the Black-headed Gull for example, 119, 000 per mm square in the center of the retina, 44,000 per mm^2 on the periphery.—Leon Kelso.

78. The egg tooth of some charadriiform birds. J. R. Jehl. 1968. *Wilson Bull.*, **80** (3): 328-330.—Notes on some representatives from the Charadriisae, Scolopacidae, Recurvirostridae, Phalaropodidae, Stercorariidae, and Laridae.—Jack P. Hailman.

79. Appendicular myology of passerine birds. A. J. Berger. 1969. *Wilson Bull.*, **81** (2): 220-223.—To quote: "This summary of our current knowledge of the differences found among representatives of passerine families is presented in order to facilitate the work of students and in order to emphasize how little still is known about the appendicular myology of passerine birds."—Jack P. Hailman.

PLUMAGES AND MOLTS

80. The molt of the Ruff in Schleswig-Holstein. (Die Mauser des Kamplaufers, *Philomachus pugnax*, in Schleswig-Holstein.) D. Drenckhahn. 1968. *Corax*, **2**(3): 130-160. (In German.)—Males have three plumages: winter, prenuptial, and nuptial; females, two. Only the summer molt is complete. Prenuptial and nuptial plumages are of different origin. Molt into the nuptial begins mid to late March, depending on temperature, ending in early May. Development of gonads to breeding state occurs simultaneously. Summer molt starts mid June; during molt of remiges flight ability may be lost briefly. Juvenals molt contour feathers, in September. Year-old Ruff are sexually mature, and are usually distinguishable from adults only by greenish tint of the legs. There is a bibliography of 17 titles. (From Russian *Referativny Zhurnal*, no. **5** (birds): 51, 1969, in absence of original.)—Leon Kelso.

ZOOGEOGRAPHY AND DISTRIBUTION

(See also 14, 90, 100)

81. Speciation in Amazonian forest birds. Jürgen Haffer. 1969. *Science*, **165** (3889): 131-137.—The lowlands of the Amazonian region currently possess the richest forest fauna in the world. Haffer contends that the extraordinary diversity of birds is largely the result of rapid speciation in the Pleistocene, caused by frequent environmental changes and resultant vegetational modifications. Alternating humid and dry periods resulted in radical changes in the distribution of forest and grassland vegetation, with forests repeatedly expanding and contracting during this period. Certain periodically isolated areas apparently always were forested and thus served as refugia and possible sources of differentiation. With the small size of the isolated populations and relatively amenable climates (in relation to nontropical areas) the possibility of rapid differentiation of populations existed, though with the small population sizes, he grants that extinction was probably high as well. He accepts Moreau's statements that under favorable situations speciation in birds, particularly small species, may be completed in 20,000-30,000 years, or even less. If such figures are reasonable, the fluctuations of the Quaternary may have been great enough to allow for the presently observed diversity.

Thus Haffer does not subscribe to the idea that rivers have been the major factor in avian speciation in the Amazon region, as some have suggested. He feels that in the vast majority of cases the rivers have merely modified or occasionally limited the dispersal of forest birds after their speciation in the isolated refugia, rather than serving as the major isolating factors. He believes instead that the frequently-observed correspondence of the range of many forms to banks of large rivers probably is the result of inhibition by competition or swamping of the occasional colonists that manage to get across such a water-course. Most species appear to have crossed sizeable rivers in the course of extending their ranges, provided an ecologically incompatible relative did not exist on the opposite bank.

To locate the forest refugia, present inequalities of present rainfall in the area were utilized, as were the current distribution patterns of birds, particularly members of superspecies. Currently, definite patterns of rainfall exist, with three large areas of over 2500 mm annual rainfall; the upper Amazon, Madeira-upper Tapajós, and southern Guiana-mouth of the Amazon. Small isolated areas of high rainfall also appear in the foothills of the Andes. Haffer's interpretation depends upon these current areas of high rainfall also being the moist areas during dry spells. Since the major orographic factors resulting in the present rainfall patterns were present during most of the Pleistocene, he feels that it is acceptable to hypothesize that the main forest refugia of the dry periods coincided with the present areas of high rainfall. Other isolated forested areas of small size are hypothesized to have existed along major water courses, on isolated mountains, and in certain parts of the lowlands. The pattern of distribution currently observed appears to depend quite heavily upon these small refugia, areas difficult to pinpoint or verify with confidence. Without assuming their existence it would be difficult to account for the observed conditions, since the larger refugia seemingly would have been overburdened, and extinctions would probably have been even higher than Haffer acknowledges.

Present distributions of some birds supply indirect evidence supporting the conclusions from the rainfall patterns. The areas of high rainfall are characterized by certain species of localized range, which apparently never extended their range far beyond their area of origin. Further, a number of superspecies have similar distributions, suggesting that all responded similarly to existing conditions.—Douglass H. Morse.

SYSTEMATICS

(See also 53, 96, 100, 109)

82. The subspecies concept. (Witherby memorial lecture.) A. Landsborough Thomson. 1969. *Bird Study*, **16** (1): 1-13.—A preeminent author and editor of ornithological texts honors a likewise eminent predecessor by dedicating to him this discussion of a long-time controversial topic in systematics. Under the following headings: Impact on ornithology, The essential importance of the species, Interspecific hybridization, Speciation, Subspeciation, Rate of subspeciation, Inadequacy of subspecies as a taxonomic unit, Polymorphism, Field application of the concept, Subspecific categories, Phusilogical races, and Conclusion, the author, admitting he is not primarily a systematist himself reairs most of the dilemmas that have plagued taxonomists for generations. His own final words are: "To sum up in a sentence: the general thesis of this lecture is that the subspecies has lost much of its former supposed validity as a basic taxonomic unit; but that, with advances in knowledge, subspecific categories of all kinds have acquired increasing significance as growing points of evolution."

If, since this reviewer spent a considerable proportion of his academic life on systematics, and has his name involved in the nomenclature of both birds and plants, he may indulge in remarks pertinent not just to this paper but to numerous others of recent years in the same vein (for the taxonomic dilemma exists on both sides of the "iron curtain"), it should be noted first that an hour or a dollar spent on the problems of taxonomy is as much of an expenditure as if spent on something else. One would never suspect this from the demands for greater perfection and specialization implied in some of the published discussions and critiques. And these demands impel some introspection for the basic truth: Is systematics (taxonomy) a science or an art form? The same question has been entertained about the practice of medicine. And as some cultural form is it not rather a nordic "ism" best comprehended and handled by nordics? Since there is so much resentment against having persons' names attached to nature's forms and varieties, against endless changes ruled by priority, and such nomenclatural atrocities as *Albizzia julibrissin* and *Soja max,* this reviewer has suggested and even published examples of the usage of numbers or formulae instead of spelled-out specific or varietal names. It is self-evident that no overwhelming acceptance or approval resulted.—Leon Kelso.

83. Taxonomic aspects of avian hybridization. L. L. Short. 1969. *Auk,* **86:** 84-105.—Lester Short, whose analyses of hybridization phenomena in birds are many and excellent, here provides an overview of some of the problems and concepts of the phenomenon. Hybridization is defined as the interbreeding of populations that differ morphologically (and hence, presumably, genetically), regardless of the current taxonomic status of such populations. Hybrids are those animals that are products of sympatric and allopatric hybridization (including secondary intergradation), but "intergrades" are products of primary intergradation. Short sets up six criteria for information that should be available to assess the taxonomic implications of hybrids discovered, but these are so broad that only major studies will reveal the necessary data. (Short is correct, however, in that the data are required.)

The paper then reviews various situations. Rare hybridization, especially between species of different genera, helps establish relationships that might not otherwise have been expected. Within a genus, rare and unique hybrids reveal little. Short then moves to a discussion of zones of overlap and hybridization—a general term for an area of "secondary intergradation occupied by numerous hybrids and both parental forms as well." The "hybrid zone" is more restricted—an area "where only hybrids occur." Since recombinants of hybrids can resemble the parents, Short proposes a rule-of-thumb requiring at least 5% of the birds present to be of parental types, to insure that true parental species are present. "Other than the indirect evidence for partial isolating mechanisms provided by the existence of a zone of overlap and hybridization, there appears to be no logical basis for deciding the taxonomic status of the interacting forms." The forms participating in such a zone have aspects of both species (morphological distinctiveness and partial isolating mechanisms) and intraspecies populations (interfertility). Following Amadon (*Syst. Zool.*, **15:** 245-249, 1966) such forms are called "semispecies" and should be treated taxonomically as species. (The semispecies is one form of "allospecies", the other being basically allopatric. All the allospecies of a group are collectively called a superspecies.) The stability of zones of overlap and hybridization is then discussed.

Next hybrid zones *sensu stricto* are taken up, with the conclusion that "forms connected by a hybrid zone are considered conspecific because of lack or ineffectiveness of reproductive isolating mechanisms." The rest of the paper deals with knotty problems of hybrid swarms, combinations of situations and circular overlap of populations, about which no generalizations are possible. A convenient table (p. 90) summarizes Short's suggestions on how to treat various hybridizing forms.

This seems a useful paper full of common sense, although I suspect it may provoke disagreement in some ornithological corners, particularly dusty ones.—Jack P. Hailman.

84. A hybrid Grasshopper Sparrow x Savannah Sparrow. R. W. Dickerman. 1968. *Auk,* **85** (2): 312-315.—This "intergeneric" hybrid, *Passerculus sandwichensis* x *Ammondramus savannarum*, was a juvenile female.—Jack P. Hailman.

85. A hybrid Lesser Scaup x Ring-necked Duck. B. W. Anderson and R. L. Timken. 1969. *Auk,* **86** (3): 556-557.—Citations of previous *Aythya affinis* x *A. collaris* hybrids are given.—Jack P. Hailman.

86. A hybrid Eastern Bluebird x Mountain Bluebird. J. Lane. 1968. *Auk,* **85** (4): 684.—The *Sialia sialis* x *S. currucoides* was a male who mated with females of *both* species. The female *currucoides* hatched six of seven eggs; the female *sialis* laid five that proved to be infertile. But when the latter immediately renested with a male of her own species, they successfully reared a brood of five. These are the kinds of things we want to know about hybrids before they become museum skins. This is the first substantiated record of an Eastern x Mountain cross.—Jack P. Hailman.

87. The relationships of sparrows in the genera *Ammodramus, Passerherbulus,* and *Ammospiza* with a description of a hybrid LeConte's x Sharp-tailed Sparrow. B. G. Murray, Jr. 1968. *Auk,* **85** (4): 586-593.—The group is a mess. The author's opinions are that the LeConte's and Sharp-tailed Sparrows (*Passerherbulus caudacutus* and *Ammospiza caudacuta*) are closely related, and the Seaside Sparrow (*A. maritima*) is somewhere near them. Priorities suggest that the name of the LeConte's should thus become *A. leconteii.* The Henslow's Sparrow (*P. henslowii*) is closest to the Grasshopper Sparrow (*Ammodramus savannarum*) and should be placed in the latter's genus. The only evidence outside of the hybrid and two sonograms is classical plumage analysis. We need a lot more information.—Jack P. Hailman.

88. The courtship display of the Asiatic Spruce Grouse. (Tokovanie aziatskoi dikushii (*Falcipennis falcipennis*).) R. L. Potapov. 1969. *Zool. Zhurn.,* **48**(6): 864-870. (In Russian, English summary.)—There is a detailed description of the courtship performance including elaborate comparison with the latest descriptions of the American forms of Spruce Grouse, which they regard as congeneric (L. S. Stepanyan, *Ornitologiya,* **5:** 368-371, 1969). Features particularly noted are a strutting pose, spreading and folding the tail, accompanied by a "rumbling" sound, two successive leaps with wing-clapping and clicking, like that of the Rock Capercaillie, *Tetrao parvirostris.* It is concluded that the courtship behavior of the Spruce Grouse group comprises all of the main elements of tetraonid display, some in a rudimentary state, this indicating direct affinity to a common tetraonid ancestor.—Leon Kelso.

89. A comparative study of the egg white proteins of some species of the avian Order Gruiformes. H. T. Hendrickson. 1969. *Ibis,* **111** (1): 80-91.—In these families traditional taxonomic characters have failed to bring workers into agreement about relationships. The biochemical method suggests that a monophyletic group is represented by the Eurypygidae, Heliornithidae, Rallidae, Turhicidae and Psocphildae. The Aramidae and Gruidae are close, and the Rhynochetidae and Cariamidae may be related; the Otididae seems quite separate. Analysis based on one (in two cases, two) genera per family, except the Rallidae from which a large sample was available.—Jack P. Hailman.

EVOLUTION AND GENETICS

(See also 30, 81, 83, 91, 109)

90. Subspecies and sympatry in Darwin's finches. David Lack. 1969. *Evolution*, **23**(2): 252-263.—Darwin's finches (Geospizinae) are one of the most frequently invoked textbook examples of evolution. As Lack says, "the factors involved in their miniature adaptive radiation seem to be the same as those involved in the larger world".

Two problems are considered here: the factors influencing differences between islands in (1) degree of subspecific endemism, and (2) the number of resident species, viewed particularly in the light of (a) isolation and (b) ecological differences in the vegetation and in other geospizines. In order to accomplish these goals Lack finds it necessary to revise the classification of the group and assess the distribution of the taxa on the different islands .

He reviews and evaluates the work and major conclusions of previous students of geospizines, including Snodgrass and Heller, Swarth, Bowman, Hamilton and Rubinoff, as well as Lack. Disagreement exists among members of this group on a number of taxonomic, ecological, and evolutionary points. Lack considers the degree of subspecific endemism to be related both to isolation and to ecological differences of the areas in concern. He feels that the numbers of species on different islands have largely been the result of ecological differences of the different islands. Though Hamilton and Rubinoff found a correlation between the size of the finch fauna and the distance between islands, the frequency with which stragglers have been found on islands upon which they are not known to breed suggests that problems of actual dispersal are not the main factor restricting diversity of the fauna.

The data suggest that on small outer islands where a single species may occupy essentially the space utilized by two species on larger islands, evolutionary pressures upon the bill size have moved in the opposite direction from that of ancestral forms. Hence in several cases on an island where one species is present, its bill will be medium-sized, while neighboring islands containing two species, one of which may well be the former species' ancestors, will contain one form with a large bill and one with a small one. There is evidence in some cases that on islands where large and small-billed species exist, they have evolved that way at least partially in response to competitive pressure from each other. Causal observations suggest that the sole species on small islands exploit a wider range of food items than any single species living in the presence of other species of finches. —Douglass H. Morse.

FOOD AND FEEDING

(See also 20, 28, 31, 34, 39, 74, 90)

91. Ecological chemistry and the palatability spectrum. L. P. Brower, W. N. Ryerson, L. L. Coppinger and S. C. Glazier. 1968. *Science*, **161**: 1349-1351.—The bird-predators versus the butterfly-prey is the classical example of mimicry relations among prey, where a "bad-tasting" prey species may be mimicked by a species that is palatable. Despite the fact that analyses of both Batesian and Müllerian mimicry depend ultimately upon defining palatability, little experimental work has been done on the subject. In this case, Monarch Butterflies (*Danaus plexippus*) were reared on milkweed species containing variable amounts of cardia glycosides; the insects were then ground up and force-fed to Blue Jays (*Cyanocitta cristata*). The emetic effect on the jays was scored. Insects reared on a plant having no glycosides failed to cause an emetic effect; plants having glycosides could be placed on a spectrum of types according to their effective dosages. A fine step in Brower's ongoing studies of predator-prey relations and mimicry.—Jack P. Hailman.

92. The recognition and treatment of venomous and non-venomous insects by small bee-eaters. C. H. Fry. 1969. *Ibis*, **111** (1): 23-29.—Bee-eaters (*Merops*) definitely have a problem to solve. They solve it by beating worker bees (*Apis*) against a perch and then rubbing them; the technique improves with experience. Drones are not immediately recognized as non-venomous, but the rubbing technique is performed with less vigor. Non-hymenoptera including near-mimics of bees are only beaten, not rubbed: a bee, or not a bee, that is the question.—Jack P. Hailman.

93. The relationship of Purple Martins to mosquito control. H. W. Kale II. 1968. *Auk,* **85** (4): 654-661.—None. *Progne subis* feeds where mosquitos are not, despite claims of sellers of martin houses that this beautiful bird "eats 2,000 mosquitoes per day." There are good reasons to protect this bird, but mosquito control is not one of them. A good debunking.—Jack P. Hailman.

94. Spruce seeds as food for the Wood Pigeon and the Hawfinch. (Granfrö som föda för ringduva (*Columba palumbus*) och stenknäck (*Coccothraustes coccothraustes*).) Conny Askemo. 1969. *Vår Fågelvärld,* **28:** 7-8. (In Swedish with English summary.)—When the spruce cones in western Sweden opened in the spring 1968 to shed an unusually good crop of seeds, Wood Pigeons were observed picking the seeds from the ground and eating them. The species is apt to concentrate on an abundant food supply to the exclusion of other foods. In this case they also appeared in flocks instead of in territory-holding pairs as they usually do during the breeding season. Among other birds, also a pair of Hawfinches with fledged young fed on the seeds.—Louise de K. Lawrence.

95. New data on the distribution and biology of the Thick-tailed Jerboa. (Novye dannye rasprostranenii i biologii priaralskogo tolstokhvostogo tushkanchika, *Pygerethmus platyurus.*) V. B. Silverstov. 1969. *Byull. moskovskogo obshch. isp. prirody, otdel. biol.,* **74**(3): 118-133. (In Russian, English summary.)—This mammalogical article attracts ornithological attention in that owl pellet analyses are its basis, particularly for considerable extension of the range of the jerboa. In 3,682 pellets of the desert variety of Little Owl, *Athene noctua bactriana,* were remains of 4,188 small mammals, chiefly rodents; in 947 pellets of the Eagle Owl, *Bubo bubo,* there were 2,350.—Leon Kelso.

96. Prising by honeycreepers. (Zirkeln bei Zuckervogel-Arten (Coerebidae).) W. Winkel. 1968. *Z. Tierpsychol.,* **25** (5): 533-536. (In German with English summary.)—Prising is the forcible opening of the mandibles, given by several honeycreeper species while feeding and preening. As it has evolved independently several times, the behavior is not reliable as a taxonomic character.—Jack P. Hailman.

97. An experimental comparison of Screech Owl predation on resident and transient white-footed mice (*Peromyscus leucopus*). L. H. Metzgar. 1967. *J. Mammal.,* **48** (3): 387-391.—A gray phase *Otus asio* was placed into a room with two mice, one of which had from 2.5 to 10.5 days familiarity with the room ("resident") and the other of which had none ("transient"). The owl took 2/20 of the residents and 11/20 of the transients. From the ornithologist's viewpoint, then, owls should hunt mice that are in unfamiliar territory. The mammalogist's viewpoint expressed by the author is that the effect is due to three possible bases: (1) the resident may be more alert (because it is not exploring); (2) the resident may know the hiding places; and (3) the transient, by exploring, may be more active, and hence attract the owl's attention. Is it not also possible that the resident drives the transient from the best shelter?—Jack P. Hailman.

98. Tongue-flicking by a feeding Snowy Egret. P. A. Buckley and F. G. Buckley. 1968. *Auk,* **85**(4): 678.—*Leucophoyx thula* apparently either disturbs fish or lures them. Another amazing adaptation in the feeding of birds.—Jack P. Hailman.

SONG AND VOCALIZATIONS

(See also 29, 42)

99. A vocal cline in Palearctic birds: pitch differences of the vocalizations of different latitudes. (Un cline vocal chex les oiseaux palearctiques: variation tonale des vocalizations, sous differentes latitudes.) C. Chappuis. 1969. *Alauda,* **37**(1): 59-71. (In French with English Summary).—Nearly 30 species of all kinds of birds (falcon, owls, woodpecker, tits, wren, creeper, wagtail, thrushes, warblers, kinglets, finches, etc.) have a higher-pitched voice in the north. This fact is documented by graphs of vocalizations that present the same information as the more familiar amplitude sections from sonograph machines (i.e., intensity of sound at each frequency). Most of the comparisons are between individuals in Morocco and France, but some come from Spain and Holland. As if the graphs were not convincing enough, a 33-1/3 rpm plastic record accompanies the article and presents the latitudinal comparisons for 13 of the species. Presumably such a consistent difference across taxa represents a basic adaptation of some sort.

No species shows a lower-pitched voice in the north, but 14 species showed no differences in the sample. Although this list of 14 includes some congeners of the birds showing the effect (e.g., *Sylvia* and *Emberiza*), the great proportion of the 14 are—ironically enough for this journal—larks, including *Alauda* itself. A fascinating discovery, paralleling in some respects Ficken's discovery a few years ago that warblers living higher in trees sing with higher pitch.—Jack P. Hailman and Elizabeth D. Hailman.

100. The influence of the environment on the evolution of bird calls: commentary on the paper of C. Chappuis. (L'influence du milieu sur l'evolution des chants d'oiseaux: commentaires a la note de C. Chappuis.) A. Brosset. 1969. *Alauda,* **37**(1): 72-76. (In French.)— Basically, the species that show the north-south difference in vocalizations (review **99**) also show evident morphological differences in plumage (or measurements, or behavior). Birds showing no vocal differences show little or no morphological differences. Furthermore, as would be expected, the first group tends to be distinguished at the subspecific level, the last group not. These comparisons do not explain, of course, why the northern individuals sing higher-pitched (never lower) songs than their southern counterparts. They merely suggest (to us) that restricted gene flow reflected by the morphological differences helps to prevent the cline in voices from being swamped out.—Jack P. Hailman and Elizabeth D. Hailman.

101. Modulation in birds sounds. R. C. Stein. 1968. *Auk,* **85**: 229-243.—Birds can use both amplitude and frequency modulation, but the former does not seem to occur clearly in the absence of the latter. The "coupled oscillator" hypothesis of oscine vocalizations is discussed, and possible mechanisms of song production spelled out.—Jack P. Hailman.

PHOTOGRAPHY

102. Observations on the Crested Bowerbird *Amblyornis macgregoriae.* S. Diczbalis. 1968. *Misc. Rep. Yamashina Inst. Orn.,* **5** (3): 199-203, plus plates 17-22. (In English with Japanese summary.)— Notes on bower building, behavior of the male at the bower before, during and after the presence of the female, and some remarks and remarkable photographs of this rare species.—Jack P. Hailman.

103. Tool-using bird, the Egyptian Vulture. J. van Lawick-Goodall. 1969. *Natl. Geog.,* **133** (5): 630-641.—Jane van Lawick (nee Godall), familiar to readers of *National Geographic* and to the ethological community for her studies of chimpanzee behavior (including tool-using), reported tool-using of *Neophron percopterus* in a communication to *Nature.* The interest in this popular article is the magnificent photographs by her husband Baron Hugo van Lawick of the vultures breaking open ostrich eggs by throwing stones down upon them. A good picture is still worth 10^3 words.—Jack P. Hailman.

MISCELLANY

104. Migratory animals as dispersal agents of cultural materials. R. F. Heizer. 1968. *Science*, **161:** 914-915.—Animals shot but not killed may carry the offending weapon (e.g., arrow) elsewhere, giving rise to the false belief that two cultures had direct contact when the two kinds of weapons are later found together by anthropologists. Example: two Honey Buzzards (*Pernis aplvorus*) shot in Finland in 1894 and 1900 had arrows made by the Pangwe tribe of southwestern Cameroon. Many other examples given.—Jack P. Hailman.

105. Do we need special critics? M. A. Williamson. 1969. *Research/Development*, **20** (7): 32, 34.—Sometimes a suggestion is so novel that it cries for a wider audience than it was prepared for. In this editorial-like article an engineer suggests that science needs outside critics, such as we are accustomed to in fine arts, drama, music and other walks of life. He argues that professional criticism (editorial review of manuscripts plus post-publication reviews such as this section of *Bird-Banding*) serves only half the job that needs to be done: there should be a bridge between science and the common man, and it should be a critical bridge. How many great scientists are known as widely in society as are the great composers and performers, Williamson asks? And should not the poor performance in science be panned as widely as poor performances in drama and music? His last argument is that if science does not get this kind of feedback, then it will communicate poorly with society, eventually leading society to impose unreasonable restrictions upon something it cannot judge or understand.

Williamson closes with this: "We might do well to ponder what George Bernard Shaw wrote years ago. He said something like this: The reasonable man tries to adapt himself to the world. The unreasonable man tries to make the world over to accommodate him. Therefore, all progress depends on unreasonable men." I like the quote, but I am dubious about Williamson's thesis since it proposes no mechanism for the responsible criticism. For years, John Lear has been doing apparently what Williamson wants in the *Saturday Review*, but when Lear touches on areas I know something about I always have the strong impression he badly misunderstands both the basic science, and therefore its possible effects on society. The problem of communication is certainly real, but the answer is by no means apparent.—Jack P. Hailman.

BOOKS, MONOGRAPHS AND RECORDINGS

(See also 99)

106. ***Bionic Problems.*** (*Voprosy Bioniki.*) M. G. Haaze-Rapoport, editor. 1967. Published for the science council on complex problems of cybernetics, "Nauka" Publishing House, Moscow. 596 pp. (In Russian.) (About $7.00 U. S.)—This is a sequel to *Bionics* (*Bionika*) reviewed in *Bird-Banding*, **39**(2): 145, 1968. Following a foreword by Berg and Sotskov (Status and trends in the development of bionics), there are 7 sections comprised of 97 articles. (1) Receptors, analyzers, and recognition of objects (22 articles); (2) Neuron structure and stability (11 articles); (3) Memory (7 articles); (4) Psychological aspects of bionics (7 articles); (5) Bionic aspects of control and design (model simulation) (24 articles, of especial interest being Presman's Electromagnetic fields and regulative processes in biology, finding evidence of various effects); (6) Orientation and navigation (14 articles, largely ornithological: Naumov and Simkin, Modes of animal communication and their design; Airapetyants and Konstantinov, Research on neural mechanisms of echolocation in bats; Ilyichev, Avian acoustic location; Malyukina, Chemoreception and schooling in fishes; Manteufel and Jacobi, Present problems of bionic research on migratory animal orientation; Protasov, *et al.*, Gravitational waves in fish orientation; Podubnyi *et al.*, Research on sturgeon migration by biotelemetric methods; Bodryagin *et al.*, Spatial location of food sources in honeybees; and Dolnik and Shumakov, Proof of navigational ability in birds.

In the last article, in experiments with Scarlet Grosbeak (*Carpodacus erythrina*), Barred Warbler (*Sylvia nisoria*), Chaffinch (*Fringilla coelebs*), and Starling (*Sturnus vulgaris*), there was found no maintenance of standard direction relative to original territory, which negates both simple orientation by nonastronomic factors *and* celestial orientation by "clocks" set on local time. In shifts to different geographical latitudes there was increased restlessness but not according to regular migratory or navigational direction. There was instead a contraclockwise shift of zugunruhe direction with increase of longitudinal distance from home. Apparently the first two named species could perceive differences in longitude, and possibly in latitude, and make corresponding corrections in their course.

Section 6 also includes: Kistyakovskii and Smogorzhevskii, Primordial distance orientation in some passerines; Sakayan, On Homing pigeon orientation; Blagosklonov and Rodionov, Individual orientation ability of the Pied Flycatcher during the nesting season; Shumakov, Experimental determination of magnetic orientation capacity in birds; Eldarov, Experimental investigation of short distance orientation; and section (7) Biomechanics (12 articles); this group includes Kokshaiskii, on Reynolds' number diapasons in biological flight mechanisms; Vyazovich, The "helicopteran" flight phase of the Mallard; and Stegmann, On the automation of avian wing action (notes that in *Archaeopteryx,* as indicated by skeletal fragments, the operative effect of air resistance on the elbow and wrist joints of the wing had not yet been developed; that it evolved later with perfection of flight and ability to travel greater distances).—Leon Kelso.

107. ***The Biology of Avian Embryo Development.*** (*Biologiya embrionalnogo razvitiya ptits.*) V. V. Rolnik. 1968. "Nauka" Publishing House, Leningrad. 424 pp., 58 figures, 54 tables, bibliography of 1066 titles. 2 roubles, 59 kopecks (about $6.00 U. S.). (In Russian.)—The studies of Miss Rolnik on avian eggs and embryology began about 30 years ago with a well-instrumented investigation of egg temperatures of the Rhea during incubation (*Voprosy ekologii biotsenologii,* **5**(6): 236, 1939.). This is a handbook which can go into a moderately-sized coat pocket, summarizing recent, particularly Slavic, progress in avian embryology. Somewhat smaller than Romanoff's *The Avian Embryo* (1960) by about half in bibliography titles, it is decidedly less expensive. It is the first in the USSR to attempt a synthesis of comparative and experimental embryology. There are three sections comprising 22 chapters. The first section reviews egg structure and embryo development from the start of incubation. Features of spermo- and oogenesis, formation of yolk and egg cell, with development of the egg envelopes follow; then, fertilization, morpho-physiological and chemical changes during fertilization; observations of variations in development of germ cells by time of egg deposition. The author comments significantly on the relative insensitivity of germ cells to external conditions after egg laying, and the effects of artificial feeding, and various environmental factors on egg production. The concluding chapter (5) of the first division dwells on germ cell survival from egg deposition to start of incubation, with an attempt to correlate the involved interruption of embryo growth with the evolutionary transition to development on land.

The second section of the book presents the morphology and physiology of embryo development during incubation. Described very briefly are features of early embryogenesis, mainly of domestic fowl, from formation of germ layers and gastrulation to anlage of the primitive gut, with a review of temporary organs: yolk sac, amnion, serosa, and allantois. There is detailed the development of organ systems: circulatory, respiratory, nervous, excretory and digestive, reproductive, muscular and skeletal systems, with a review of periodicities in growth. Rolnik believes it expedient to divide avian embryo development into five periods rather than the three or four recognized by other authors. On growth and differentiation the author has interesting opinions on the regulative mechanisms enabling the organism to adapt to a broad scope of external factors.

The third section is devoted to environmental factors providing normal course of development. Of major interest are chapters reviewing effects of temperature and humidity on avian development. The chapters on the influence of ventilation, and other means of manipulating conditions, such as humidity, egg position and egg rotation are of rather practical or management value. Some topics on avian embryology are abbreviated or omitted: *e.g.,* variations in early development, and the evolution of temporary structures, such as the yolk sac.

Being a specialist in embryophysiology Rolnik stresses morphological aspects of development according to published accounts. In saying that the yolk sac in the early 3 weeks of chick postembryonal development is of apocrine secretion, later converting to a lymphoid organ, the author does not give the source of this opinion on this still controversial topic. Such faults as the reviewer would note are editorial, or pertinent to the morphological element of the monograph. Those topics on avian embryo physiology are elaborated thoroughly and at a high scientific level.

Miss Rolnik's book should undoubtedly serve as a useful text and manual for biologists in general, ornithologists, and specialists in poultry biology. While this reviewer has this text at hand, the above is a condensation of his translation of that by I. M. Zusman (*Zool. Zhurn.*, **48**(5): 771-772, 1969) to have the benefit of that reviewer's greater familiarity with this field.—Leon Kelso.

108. ***The Audubon Book of True Nature Stories.*** John K. Terres, editor. 1968. Thomas Y. Crowell Co., New York. x + 294pp. $6.95.—This collection of articles, some by well-known and others by unknown authors, all first appeared in various numbers of *Audubon Magazine*. Selected and edited by John Terres, the articles were published in book form illustrated by Walter Ferguson's black and white drawings in 1958. This unrevised new edition makes available to a younger generation this interesting light reading about many birds and other wild creatures.—Elizabeth S. Austin.

109. ***Species and Speciation.*** (*Vid i vidoobrarovanie.*) K. M. Zavadskii. 1968. "Nauka" publishing house, Leningrad. 404 pp. bibliography of 816 titles. (About $5.00 U. S.) (In Russian.)—A valuable discussion and compendium of speciation literature, this book is an invaluable contribution to systematics and research on one of the principal and critical dilemmas of modern biology.

The species doctrine or concept is the pivotal point at which intersect the most diverse divisions of biology: of systematics and nomenclature, geography and ecology, physiology, cytology and genetics, paleontology, evolutionary theory, and many others. Moreover the species concept is obligatorily applicable to various forms of life, whether plants, animals, microorganisms, viruses and even primordial problematic structures standing at the boundary between the living and the dead. It can be said, furthermore, that there is no phase of biology which can be researched on a sound basis and yet not touch on the species problem. After the passage of two and a half centuries research on the species continues with the usual controversy, each line of biology taking its own approach.

Following a foreword, there are here two sections and 10 chapters. *Section* 1, history of species science, presents in chapter 1, evolution of the species concept, with modern estimates of the numbers of species that now exist, and which may have since the beginning of time, with discussion of asymmetrical geographic dispersion of major taxons. Ch. 2, The species to the mid-19th century, presents stages of species recognition development: (1) ancient times to 18th century, (2) Ray to Linnaeus in 18th, about 70 years, (3) Linnaeus to Darwin, about 100 yrs., (4) second half of 19th century, about 40 yrs., (5) early 3rd. of 20th century, with founding of new criteria, and (6) modern, since 1930's, with development of populational concept, treatment of species as one of the fundamental phenomena of organic life. Stability and distinctness of species, the subjectivity of species recognition, and sundry denials of species reality are discussed, with practical standards in the pre-Darwinian period, concept definitions, of the monolithic and polytypic species. Ch. 3, reviewing the latter 19th century, discusses Darwin's contributions, general status of the species problem, speciation beliefs, the role of hybridization, and the mutation hypothesis. Ch. 4, reviewing the early 20th century, remarks on the morpho-geographic method in systematics, with the views of Korzhinskii and Komarov on polytypic species standards; the crisis in species science, the genetic approach, reflecting again on subjectivity of judgments, seasonal races, experimental proof of mutation hypothesis, paleontologists' contributions, the migratory hypothesis, and again the disputed reality of the species. Ch. 5, recent history, elaborates on experimental studies, ecological trends, and microsystematics, genetics and natural populations, intraspecific relationships and caryosystematics and experimental hybridization. The work of Vavilov, centers of origin and diversity, the Linnean species (is it a unit or a system?), concepts in botany, the amplitude of wheat species concepts, resistance to practical species standards, species in paleontology, monotypic standards for "small" species, erroneous concepts of species and speciation, denial of species reality (again) and pseudodarwinism, received attention.

Section 2, review of species theory, continues with Ch. 6, criteria and common features of species: the author taking up the species and systematics, species theoretics (eidology), morphological, biochemical, geographic and other criteris, universality and inequities, basic features, some modern definitions, the species as an organization type, with the biological species defined. Ch. 7, the species as one of the fundamental and principal forms or organized life, discusses main forms and their subdivisions, the Linnean hierarchic series, the term "organization level", primordial and fundamental forms of living organization, the species as a supraindividual system, possibility of species' existence indefinitely, as a formation capable of independent evolutionary development; is there a species stage in evolution?; species' integrity, redefinition of concept, degrees of reality, adaptations, and determination of species. Ch. 7, species' structure, takes up concept definitions, a critique of diversity concepts, intraspecific unit systems, differentiation analysis methods, unit systems in plants, differrentiation concepts in zoology; allopatric groups, general features, ecological races (ecotypes), geographic races (subspecies), reality of subspecies and ecotypes, local populations: definition, types, size and limits, sympatric units; concepts, intrapopulational groups, genetic polymorphism, ecoelements, and biotypes. Ch. 9, intraspecific relationships, opens with their concepts and definitions, and continues with fundamental and derived individual integration; inequities existing, experiments on plants, abundance as a specific adaptation, population density regulation, optimal densities, overpopulation and its consequences; and specific correlations. Ch. 10, speciation, concludes with: speciation and morphogenesis, microevolution, speciation variability, types of speciation, types: syngenesis and segregogenesis, allopatric speciation; geographic speciation, sub- and semispecies as speciation stages, ecological speciation, sympatric speciation: occurrence, examples; hybridogenic speciation and the "new species form" concept, recreation of natural forms evidently of hybrid origin, experimental synthesis of "new species", neoformogen and microaccumulative speciation, isolation and speciation; classification as form, spacial, temporary, and sexual isolation, speciation and progressive evolution; inequities of species, arogenic species, and evolutionary improvement of vitality of individuals and species. Useful topical and author indices conclude a book which one must grant is a most remarkable *tour de force* on its topic.

The book is ornithological in that as evidence on points eminent ornithologists and their research are quoted. Furthermore, as almost everyone has attended school and has his own decided opinions on the general cause of education, so almost every person of whatever contact with biology has his own decided opinions on systematics and nomenclature. Whatever the value of Linnaeus and what he started, his work has had the universal power to stimulate everyone to constitute himself a "Siegfried", a "knight in shining armor" defending the system, or a "great white father" defending Linnaeus. One botanical systematist claims that he has felt it expedient to get away quickly from sessions of taxonomic discussion lest mayhem be wreaked on him. A photographer acquaintance of the reviewer has given up discussing colors with anyone because: "No two people see colors alike." Perhaps the same condition underlies discord in systematics.

The text reviewed above returns willingly or not to the subjective or personal taste or reaction aspect of taxonomic judgments. That fact and the welter of discussions now appearing in biological literature of both the Eastern and Western worlds recall those old oft-quoted words from "The Rubiyat": *"And heard great argument therein and about: but evermore, Came out by the same door wherein I went."*—Leon Kelso.

REQUEST FOR INFORMATION — SCARLET TANAGERS

Kenneth W. Prescott (Director, New Jersey State Museum, Cultural Center, Trenton, N. J., 08625) is attempting to summarize migratory data on the Scarlet Tanager (*Piranga olivacea*) as supplied to him by the Bird-Banding Laboratory of the Fish and Wildlife Service. He would deeply appreciate additional information (and permission to use) which banders might be able to supply him in relation to (1) recoveries, (2) returns, (3) repeats, (4) individual age, or (5) other relevant data.