RECENT LITERATURE

Edited by Bertram G. Murray, Jr.

MIGRATION, ORIENTATION, AND HOMING

(See also 29)

1. **Variation in direction and spread of directions of bird migration in northern Switzerland.** (Zeitliche und räumliche Unterschiede in der Richtung und Richtungsstreuung des Vogelzuges im Schweizerischen Mittelland.) B. Bruderer. 1975. *Ornithol. Beob.*, **72**: 169-179. (In German with English summary).—For 64 nights in spring and 38 nights in autumn an average of 150 migrating birds were tracked each night with an X-band (3 cm wavelength) tracking radar. Measurements of winds aloft were made with the radar two or three times each night. Although most birds came from the large northeastern land masses, some movements from the N and NW were recorded. The major axis of migration was the same throughout the seasons, but there was a tendency for larger movements to the N and NW in late spring. The mountain ridges and the associated winds had a strong influence on the prevailing direction of migration, toward 60° in spring and toward 240° and 250° in autumn. The author found that the birds tended to fly on nights when and at altitudes where winds were most favorable. Winds from the right of the main axis of migration induced heavier migration to the north and northwest, whereas winds from the left of the main axis of migration were usually compensated. Small numbers of birds flew downwind even when the wind direction was not blowing toward their goal direction.

Deviations from the normal flight directions, especially reversed migrations, occurred mainly at lower altitudes, and these movements were associated with unfavorable weather conditions and opposing winds in relation to the principal direction of migration. On nights with undisturbed weather the mean direction of the migration shifted clockwise a few degrees during the night. Bruderer's study is well done, and his findings suggest that the influence of winds on the direction of bird migration is still an intriguing problem.—Sidney A. Gauthreaux, Jr.

2. **Spring migration in the Skylark (*Alauda arvensis*) in Denmark. Influence of environmental factors on the flocksize and correlation between flocksize and migratory direction.** J. Rabøl and H. Noer. 1973. *Vogelwarte*, **27**: 50-65.—Skylarks seldom migrated in mixed species flocks. However, the conspecific flocks showed much fusion and splitting. Several environmental factors appear to influence both flock size and direction. The overall distribution of flocksize closely fits a negative binomial equation, possibly caused by a social attraction between individuals of the species up to a certain point. The daily mean flocksize increased under increased cloudiness, with decreased visibility, and increased snow cover. In December and January increased snow cover promoted migratory movements to the west and southwest. After January snow cover stopped migration and produced large flock build-up in the fields. Reverse (westward) migration was more pronounced with winds from the west than with easterly winds. Rabøl attributes this to a "headwind migration tendency." Because he was dealing with the lower, visible migrants, an alternate explanation could be that the birds were flying lower into the headwind to regain land and were thus more visible to the observers. If "headwind migration" were an important factor, one would expect it to be more prevalent with forward migration than reverse migration. Rabøl also found that the amount of reverse migration and the dispersion of the migratory directions decrease as flocksize increases. This was attributed to an averaging of the individual headings of the members of the flock. He produced a model, based on the headings of single migrants, to predict the expected headings of paired migrants. The results of the model closely fit the actual headings of paired migrants.—Robert C. Beason.

3. Homing experiments on swifts *Apus apus* (L.) deprived of olfactory perception. V. Fiaschi, A. Farina, and P. Ioalé. 1974. *Monit. Zool. Ital.*, **8:** 235-244.—Tests of the olfaction hypothesis of avian navigation have been conducted with homing pigeons (see *Bird-Banding* **47:** 369-370, 371-372, 1976, reviews no. 3 and 5). This paper represents an attempt to test the hypothesis using a different species. All birds were captured on the nest. Prior to release all birds underwent sectioning of the right olfactory nerve and the opposite nostril was plugged with wax in experimental birds while the ipsilateral nostril was plugged in controls. In this way swifts of both groups were subjected to the same operation and the same respiratory disturbance. The release of the experimental (24) and control (22) birds took place over a period of nearly two months under clear skies at sites ranging from 47 to 66 km from the birds' nests. The distribution of vanishing bearings with respect to home did not differ from a uniform one in either experimentals or in control birds. Furthermore no significant difference was noted in vanishing times for the two groups. The rates of return to the nest, however, did differ: only 3 (12%) of the experimentals returned (all without their nostril plugs), whereas two thirds (68%) of the control swifts returned (all with nostril plugs) within one week following release.—Frank R. Moore.

4. Effects of a "permanent" clock shift on the orientation of young homing pigeons. W. Wiltschko, R. Wiltschko, and W. T. Keeton. 1976. *Behav. Ecol. and Sociobiology,* **1:** 229-243.—This important paper describes some ingenious experiments designed to dissect the relationships between the internal clock, sun compass, and geographic directions. Similar experiments were performed in the springs of 1974 and 1975. Up to about 24 days of age, nestling pigeons were cared for by their parents. One young was then removed from each nest and placed in a light-tight room in which artificial lights came on 6 hr after real sunrise and went off 6 hr after real sunset. The other young bird from each nest was placed in an identical room with a light cycle in phase with the natural day. Both groups were given access to a joint aviary each day during the overlapping portions of their photoperiods. Later in development, both groups were allowed to exercise outdoors and were given a series of flock-toss training releases beginning 1.6 km from the loft and extending to 35 km from each of the cardinal directions. Thus the birds were given ample opportunity to see the sun, but a noon sun viewed simultaneously by both groups would have been a "morning sun" for the experimental birds and a normal noon sun for the controls. A series of homing tests was then performed, with experimental and control birds released alternately.

A brief summary of the results of the homing trials is as follows:

(1) In seven releases under sun to distances of 74 km the experimental birds showed no differences from the controls in vanishing bearings, vanishing intervals, or homing performance.

(2) The clocks of the experimental birds were then "normalized" by placing them in the room with the controls (i.e., subjecting them to a 6-hr-fast clock shift). When released on their first homing trial, these normalized pigeons gave vanishing bearings shifted about 90° counterclockwise from the controls as predicted.

(3) On second and subsequent releases, this directional shift disappeared and the experimentals again became indistinguishable from the controls.

(4) Following exposure to the normal photoperiod for varying periods of time from 4 weeks to one year, the experimental birds were again subjected to a 6-hr-slow phase shift and released with controls. The results were equivocal with only the birds housed under normal conditions for a full year showing an obvious shift in bearing of the predicted direction and magnitude.

The authors conclude that the calibration of the sun compass in young pigeons is learned on the basis of sun-photoperiod relations during early experience. The results under (2) above indicated that the compass can be recalibrated (at least when young) given ample exposure to a new regime. The sun compass of the experimental birds in (1) was apparently coupled to their shifted clock as indicated by the shift in bearings when first released under the normal photoperiod (2). The recalibration of the phase-shifted birds to normal time occurred remarkably rapidly. The results in (4) suggested that the birds had ceased to rely heavily on their sun compass and were using primarily some other compass system. The underlying reference system by which the sun compass is calibrated is unknown, but the authors suggest the magnetic compass as one possibility.—Kenneth P. Able.

5. Bar magnets mask the effect of normal magnetic disturbances on pigeon orientation. T. S. Larkin and W. T. Keeton. 1976. *J. Comp. Physiol.*, **110A**: 227-231.—In an earlier paper (*J. Comp. Physiol.*, **95A**: 95-103, 1974; see *Bird-Banding*, **47**: 78, 1976, review no. 7) Keeton et al. reported that natural magnetic storm intensity was correlated with a counterclockwise deflection in the vanishing bearings of homing pigeons. To strengthen the case for a direct causal relationship between magnetic events and the bird's behavior, experiments were performed to see if bar magnets attached to the pigeons would eliminate the directional deflection. Seventeen birds that showed the highest correlations between bearings and K-values in the previous experiments were selected and divided into two groups such that each group had nearly the same range of correlations. Twenty releases were made at Weedsport, N.Y. (73.5 km N of home loft), under sunny skies. The two groups were alternately equipped with magnets or brass bars. For each individual, a mean vanishing bearing for each treatment was computed. For each release, the deviation of each bird's vanishing bearing from its mean bearing for that treatment was calculated, and these were averaged over the treatment. These "deviation means" were significantly correlated with the intensity of magnetic disturbances (K_{22} values as used previously) when the birds wore brass bars; no correlation existed among the magnet birds. Thus the results provide a nice confirmation of the earlier report. Because the effect occurs under sun when the pigeons are presumably using their sun compass and because it involves a directional shift rather than an increase in scatter, the authors wonder whether the effect they see is not somehow involved with the map component of navigation.—Kenneth P. Able.

6. Migrating birds respond to Project Seafarer's electromagnetic field. R. P. Larkin and P. J. Sutherland. 1977. *Science*, **195**: 777-779.—Southern has previously reported an effect of the low frequency (72 to 80 Hz) a-c magnetic field generated by the Project Sanguine (= Seafarer) antenna on the orientation of gull chicks. Larkin and Sutherland tracked lowflying (80 to 300 m) nocturnal migrants as they flew over the above-ground antenna with a 3-cm tracking radar. The two 22.6-km antennas form a cross oriented approximately N-S and E-W. Current varying from 75 to 300 amp at 1800 v could be passed through one or both of the antennas. Energizing or de-energizing the antennas involved a linear ramping of current from either 300 amp or 150 amp to 75 amp. The duration of the ramping was 100 to 110 sec from 300 amp, 45 to 52 sec from 150 amp. The drop from 75 amp to 0 amp was instantaneous. At a distance of 100 to 400 m, the antenna was calculated to produce a magnetic field intensity less than 1 percent of the earth's. Tracks of 469 birds obtained between 21 April and 10 May 1975 were analyzed with respect to five conditions: (1) N-S antenna on; (2) E-W antenna on; (3) both antennas on; (4) both antennas off; (5) one or both antennas changing (off-on or on-off). No effect on overall migration direction or flight speed was found. However, tracks determined by a blind procedure to have X-Y or altitude nonlinearities were significantly more frequent when the antenna was on or changing (off—4% of 55 nonlinear tracks; on—12%; changing—28%). Some of the flight changes were transient, others persisted for the duration of the track. For several reasons, it was not possible to associate the nonlinearities more precisely with presumed stimuli from the antenna. Other external conditions (cloud cover, wind condition, K-values) did not appear to influence the response, although nonlinear tracks were more frequent on some nights than others.

These data appear to provide us with another subtle effect of magnetism on bird orientation. Perhaps the accumulation of enough effects will shed some light on what is really going on, but we still seem to be well in the dark. These data imply a sensitivity to magnetic fields on the order of that reported by Southern, Keeton, and Moore with respect to magnetic storms. As Larkin and Sutherland state, these studies imply a heretofore unsuspected sensitivity in birds' detection of magnetic fields. This is true if the birds are in fact responding to magnetic phenomena rather than some correlate of the magnetic field changes, a possibility that cannot be excluded in this case. Finally, the effect described here does not imply that birds use magnetism for orientation. When they occur (a minority of cases) the nonlinearities appear to be minor perturbations of flight course (not disorientation) and are often of very short duration.—Kenneth P. Able.

7. **New method for measuring the flight altitude of birds.** T. Gustafson, B. Lindkvist, and K. Kristiansson. 1973. *Nature,* **244:** 112-113.—A new kind of altimeter carried by the birds was used to measure the flight altitudes of homing pigeons and swifts. The altimeter is based on the fact that the range of alpha particles from a radioactive source in air is inversely proportional to the density of the air which decreases in a regular way with changes in altitude. The apparatus weighs about 1.0 g and consists of a lucite tube 50 x 8 x 5 mm^3 with a ^{210}Po source at one end of the tube. The detectors are sheets of cellulose acetate placed in two 45° slits in the other end of the tube, making it possible to measure the maximum range and range distribution of α-particles. The error amounted to about 100 or 200 m in flight altitude in the homing pigeon experiments. On the basis of five experiments with homing pigeons the authors found an individual variation in flying behavior and no indication that temperature determined the flight height. The altitudes recorded were from 300 m to 1,700 m above sea level.

The work with swifts involved a lighter (0.5 g) and shorter (20 mm) altimeter. Four swifts flew from 1,400 m to 2,000 m, five from 2,000 m to 3,000 m, and seven from 3,000 m to 3,600 m. The extended results and discussions of this work will appear at a later date.—Sidney A. Gauthreaux, Jr.

POPULATION DYNAMICS

8. **The mortality factor in the Willow Ptarmigan population of Bolshezemelsk Tundra.** (Koeffitsient smertnosti v populyatsii beloi kuropatki Bolshezemelskoi Tundry.) R. Voronin. 1976. *Ekologiya,* **1976** (5): 95-98. (In Russian.)—During the non-lemming years of 1971 to 1975 predator pressure on *Lagopus lagopus* was high; as indicated by an annual bag of over one million ptarmigans, it was not too high to be compensated for by the annual increment in this arctic territory. There was some amount of undetermined loss through disease; some struck telephone wires during annual passage from tundra to forest and back. During lemmingless years foxes, weasels, Snowy Owls, and Ravens foraged for the wounded under the wires along highways, as did the local human population. Over all, the annual yearling loss was estimated as high as 90 to 95%, in addition to 60 to 86% of the adult stock. Yet it was indicated that fantastic breeding productivity enabled the Willow Ptarmigan population to achieve recovery in a short time. A sharp decline is followed by an abrupt rise in nesting density and fertility. A decline of raptor pressure during years of lemming abundance aided and abetted the ptarmigan situation. This was particularly notable during the years of this survey.—Leon Kelso.

NESTING AND REPRODUCTION

9. **Bald Eagle nesting attempts in southern Ontario in 1974.** F. M. Weekes. 1975. *Can. Field-Nat.,* **89:** 438-444.—In 1974, six pairs of Bald Eagles (*Haliaeetus leucocephalus*) nested in southwestern Ontario, and three pairs successfully reared one eaglet each. The 50% success in 1974 contrasts with an average 20% success in each of the five preceding years. In 1970, the province of Ontario banned DDT. Additionally there has been an increase in local concern for the Bald Eagle's welfare. Both factors may have contributed to the increased success of nesting attempts by the Bald Eagle, although, as pointed out by the author, one successful year hardly establishes a trend.—Edward H. Burtt, Jr.

10. **Gray Jay nesting in the Mer Bleue Bog, Ottawa, Ontario.** H. Ouellet, S. J. O'Donnell, and R. A. Foxall. 1976. *Can. Field-Nat.,* **90:** 5-10.—A pair of Gray Jays (*Perisoreus canadensis*) fledged one nestling in the first recorded nesting attempt in the lowlands of the St. Lawrence valley. The nest, built in a black spruce in a relic boreal bog, was approximately 175 km south of La Veren-

drye Park in Quebec, the closest locality where Gray Jays are known to breed. The female was banded and photographed, the juvenile banded, and the nest collected after the family had left. The article is marvelously illustrated, and there can be no doubt as to the authenticity of the record despite the absence of a specimen.—Edward H. Burtt, Jr.

BEHAVIOR

(See also 4, 5, 40, 41, 42, 43)

11. Surplus killing and caching by American Kestrels (*Falco sparverius*). 1976. G. L. Nunn, D. Klem, Jr., T. Kimmel, and T. Merriman. *Anim. Behav.*, **24**: 759-763.—Is predation motivated by hunger alone? No, American Kestrels (*Falco sparverius*), whether captive or wild, killed more mice or rats than they consumed within an hour of killing. The first prey animal was partially consumed before it was cached, whereas all subsequent prey were killed and immediately cached. One wild female killed 20 mice in 105 minutes, utterly dispelling the notion that surplus killing is pathological behavior seen only in caged animals.

Is surplus killing adaptive? Yes, cached mice and rats were subsequently eaten by both caged and wild kestrels. Furthermore foxes (*Vulpes vulpes*) are known to return for cached eggs months after caching the eggs (N. Tinbergen, "The Animal in its World," Harvard Univ. Press. 1972). During severe weather when hunting is miserable, cached prey can be recovered and eaten, thereby aiding survival under adverse conditions.—Edward H. Burtt, Jr.

12. Sensitivity to odours in the embryo of the domestic fowl. B. E. Tolhurst and M. A. Vince. 1976. *Anim. Behav.*, **24**: 772-779.—On the day before hatching embryos of the domestic chicken increase the heart rate and the rate of beak clapping in response to three odorants: dichloroethane, formic acid, and cineole. Blocking the nostrils with wax abolished the response, and reopening the nostrils reestablished the response. Is there any reason to suppose that changes in the rate of beak clapping or heart beat are responses to olfactory stimulation? That is the nagging problem acknowledged by the authors, but nevertheless unresolved. Whereas the study indicates that embryos may be sensitive to odors, there is no evidence that embryos can distinguish between different odors.—Edward H. Burtt, Jr.

13. The behavior of domestic chicks during exposure to two stimuli. D. F. Chantrey. 1976. *Anim. Behav.*, **24**: 780-785.—Chicks exposed to two stimuli in rapid alternation (classification together) learn the discrimination more slowly than chicks exposed to two-stimuli that alternate at half-hour intervals (classification apart). Chantrey (*J. Comp. Physiol. Psychol.*, **81**: 256-261, 1972; *J. Comp. Physiol. Psychol.*, **87**: 517-525, 1974) has suggested that the process of classifying together is important when the chick is learning that various views of the mother are different views of one individual and not views of different individuals. Different views of the mother are temporally grouped and therefore are classified together, whereas views of diverse objects tend to occur at long intervals and hence are classified apart.

The paper is exceedingly difficult to read, and the tables inadequately explained. I sincerely hope that the conclusions follow from the data. Too few data are presented for the reader to verify the author's conclusions.—Edward H. Burtt, Jr.

14. Physiological maturation and reproductive behaviour of female doves (*Streptopelia risoria*) held under long and short photoperiods. N. R. Liley. 1976. *Can. J. Zool.*, **54**: 343-354.—Female doves were held under long (16 h per day) or short (8 h per day) photoperiods and exposed to actively courting males for 4.5 h per day for 15 days. Seven out of eight females on long photoperiods laid eggs, against only one of nine females on short photoperiods. Female readiness to breed was best indicated by soliciting and nest-building behaviors, and these activities were correlated with the growth of oviduct and follicles. Copulation and related activities were not thus correlated. The results suggest that female responsiveness to male courtship is enhanced by long photoperiods, but with courtship activities partly dependent on ovarian hormones and partly on other unidentified factors.—Raymond J. O'Connor.

15. Visual detection of cryptic prey by Blue Jays (*Cyanocitta cristata*).. A. T. Pietrewicz and A. C. Kamil. 1977. *Science,* **195:** 580-582.—The procedure used here was to show pictures of a typical prey species of *Catocala* to Blue Jays. These moths rest by day on tree bark, which shows a strong resemblance to the markings of the particular species of moth. The jays were trained "to respond differentially to the presence and absence of moths in projected images." The test slides showed bark with moths on appropriate or inappropriate bark and head up or head down. In general the cryptic condition, either head up or head down, yielded the fewest correct choices.

There were two points that seem obscure and on which Dr. Kamil kindly elaborated in a letter to the reviewer. The Blue Jays were obtained a few days out of the nest and then held in captivity, those used being 2 to 6 years old. The birds were fed ad libitum, but for experimental purposes were starved until they had dropped to 80% of their full-fed weight, called in the article "free feeding weight."—Charles H. Blake.

16. Hostile encounters among spruce-woods warblers (Dendroica: Parulidae). D. H. Morse. 1976. *Anim. Behav.,* **24:** 767-771.—Although the birds are not interspecifically territorial, an interspecific dominance hierarchy exists among spruce-woods warblers. Whether based on the number of attacks vs. the number of times attacked or on the number of times the species sang at an intruder vs. the number of times the species fled when sung to, Magnolia Warblers (*Dendroica magnolia*) dominate Black-throated Green Warblers (*D. virens*); both dominate Yellow-rumped Warblers (*D. coronata*); and all dominate Blackburnian Warblers (*D. fusca*). No correlation exists between the frequency of interactions and the dominance rank of the species, nor is size correlated with interspecific dominance. Reversals are frequent and may depend on the distance of the attacker to its nest.

Temporal fluctuations in both interspecific and intraspecific interactions of these four species are discussed. Agonistic behavior of males and females is compared, but the interspecific dominance hierarchy is by far the most intriguing result of the study. Two notes of caution: the data tables require thoughtful reading, and the phrase "(columns/rows)" in Tables VI and VII should read "(rows/columns)."—Edward H. Burtt, Jr.

ECOLOGY

(See also 8, 46)

17. Coexistence, coevolution and convergent evolution in seabird communities: a comment. J. Bédard. 1976. *Ecology,* **57:** 177-184.—Occasionally a paper appears that stimulates intense controversy and provokes immediate reaction. Written responses to published papers usually find themselves relegated to a "letters to the editor" section or some analagous forum buried ignominiously within the final pages of a journal. Here, however, we have a lengthy commentary, amounting to a well documented blistering retort, published as a major paper in a major journal. The target of Bédard's criticism is the 1973 paper by Martin Cody referred to in his title (*Ecology,* **54:** 31-44, 1973). In the first (and last?) round of what has all the earmarks of a classic Gaussian conflict, Bédard dissects Cody's arguments point by point, uncovering fallacy after fallacy, presenting counterevidence, and ultimately eliminating every shred of life from Cody's once cogent model. Bédard emerges triumphant, the new champion of alcid biologists.

What are the issues that prompted such a response? In studying colonies of breeding alcids off Washington state and Iceland, Cody drew the following conclusions: (1) alcid species, having basically similar diets and breeding seasons, are able to coexist because they partition feeding space by distance from the breeding colony and (2) the resulting patterns of distribution of feeding birds, along with differential predation pressures within the colony, interact to mold diverse species-specific life history strategies. Bédard considers Cody's spatial argument to be "ethereal at best" and presents Cody's own data in a way that weakens his (Cody's) own case. He also emphasizes that the small sample sizes used by Cody provide insufficient data from which to generalize. Bédard cites evidence from his own extensive studies of alcids which contradict many of the conclusions drawn by Cody. Finally, Bédard criticizes Cody's rationale in comparing two very different types of colonies in the Pacific and Atlantic. Space precludes a detailed discussion here of all the differences of interpretation by these investigators.

Is there a clear victor here? After reading both papers, I conclude that, although Bédard's views on alcid ecology may not be wholly acceptable to all investigators in the field, he has dealt Cody a convincingly devastating blow. The significance of these papers, however, transcends the facts about alcid biology. It lays bare a more fundamental dichotomy, that between the meticulous empiricist (Bédard) on the one hand and the more theoretically inclined modeller (Cody) on the other. Ideally both approaches should achieve a synergistically healthy balance. But either approach taken to its extreme can approach triviality. That is, the empiricist should collect data that have potential bearing on biologically interesting and useful concepts (although some will argue that random, undirected data-gathering is never futile); and the modeller should analyze adequate amounts of data objectively without being biased by the constraints of tantalizing hypotheses. Cody appears to have fallen victim to the latter trap. At the very best, he has not effectively substantiated his contentions with enough hard data. A real danger here, as Bédard alludes to, is that all too often the appealing, rather than the accurate, becomes perpetuated in the literature. Perusal of these two papers in quick succession should be time well spent for those interested in avian community ecology.—Marshall A. Howe.

18. Aggregation, migration and population mechanics. L. R. Taylor and R. A. J. Taylor. 1977. *Nature,* **265:** 415-421.—Migration in this paper is used in a wider sense than that familiar to most ornithologists. It refers to mobility, and as the authors point out in their introductory comments, migration is a very necessary component in the regulation of populations in addition to the commonly accepted Malthusian regulators of disease, predation, and resource limitation. They mention that until recently migration has often been regarded as an idiosyncrasy to be included with unaccountable mortality in life tables based on census information from a restricted area, but migration is not synonymous with mortality. As a result little is really known about the role of migration in the dynamics, and hence in the fundamental concept, of population.

The authors argue that all spatial dispositions can be regarded as resulting from the balance between repulsion and attraction behaviors. The balance between these two conflicting behavioral tendencies operating on each individual determines its movements and hence the resulting spatial patterns of the population at any instant in time. It is the response of this balance to changing internal and external environmental conditions that constitutes the dynamic element in populations. From this basis the authors bridge the gap between models and data with a functional mechanism for the distributive processes in a population that leads to the diffusion rates and spatial dispositions observed in nature.—Sidney A. Gauthreaux, Jr.

19. Distribution and ecology of the Marsh Sandpiper in central Siberia. (Rasprostranenie i ekologiya porucheinika, *Tringa stagnatilis,* v srednei Sibiri.) V. Tolchin. 1976. *Biol. Nauki,* **1976**(5): 42-48. (In Russian).—This paper reports the expansion of the species' range, especially the eastward spread around Lake Baikal. In spring the birds arrive in streams of 1,000 or more or in small groups of 5 to 15 in early May, usually at twilight. Eighty nests were found. Both sexes had brood patches and incubated. They boldly swooped at humans and drove dogs from territories by persistent annoyance. Nests were 80 to 150 m apart. Incubation lasted 21 to 22 days. Young at hatching weighed 9.8 to 10.0 g. Aquatic insect larvae, chiefly Chironomidae, comprised 60 to 78% of the food at all seasons, and plant matter formed a trace. The birds stood abdomen-deep in water and picked the food from the surface to the mud floor.—Leon Kelso.

WILDLIFE MANAGEMENT AND ECONOMIC ORNITHOLOGY

(See also 44)

20. Guidelines for the protection and management of colonially nesting waterbirds. P. A. Buckley and F. G. Buckley. 1976. North Atlantic Regional Office, National Park Service, 150 Causeway Street, Boston, Mass. 02114. 54p, available from the authors.—Colonial waterbirds seem only recently to have come to the attention of park management and fish and wildlife workers. The present paper is a manual for those who find themselves in charge of managing a tern, gull, heron or other waterbird colony on federal, state, or local lands. It explains why such colonies should be protected and how, what possible problems exist and how to deal with them. As such it is an invaluable beginning for a sourcebook on managing these species.

Unfortunately in three areas, the authors' interpretations are seriously open to questioning. First, they divide the monitoring of breeding populations into four categories: surveying (finding the colonies), censusing (counting the birds), monitoring (watching the colonies through the breeding season), and evaluating (determining "the productivity and health of the colony"). The authors suggest that surveying and monitoring be done first if resources are limited. I fail to understand how these can provide much useful information in the absence of census data on the size of the colony. Monitoring a colony requires a significant and regular investment in time and resources to get other than non-trivial results.

Second the authors strongly advocate the use of helicopters for surveying and censusing. It would be very unfortunate if this method became popular at the expense of more plodding but thorough ground methods. Aerial work provides few data on the health of a colony or even on whether the species are actually nesting and not just loafing. Until there is rigorous evidence of the superiority of one method over another, premature enthusiasm for one particular method should be avoided. Additionally we have no information on the effect of helicopter disturbance on colonies. The immediate effect of low-flying helicopters at several tern colonies at which I have worked on Long Island is comparable to the disturbance caused by low-level supersonic jets and Peregrine Falcons. The effects of "judiciously flushing" colonies with helicopters should be more carefully investigated, especially for tree-nesting species, before such methods become too popular.

Finally and most seriously, the Buckleys advocate stricter control of scientific work and, especially, banding at colonies. They suggest, without presenting any evidence, that "scientists can impact (sic) their subjects severely." They further allude to damage alleged to be done by "recreational" banders, claiming that they ". . . have seen the damage they do, often not evident until several hours, days or years after they leave." I certainly wish the Buckleys would publish their evidence to substantiate such claims. I do not deny that scientists and banders could severely damage colonies, but most of the individuals that I know have a common sense understanding of what is detrimental and have tried to minimize trauma caused by their activities. These two groups have consistently sought increased protection of waterbirds. Banders have provided almost all the available information on population trends. The Buckleys apparently rank scientists and banders as some of the more serious sources of disturbance, comparable to vandals, sonic booms, dogs, and poaching! A more profitable approach might be to recognize that scientists and banders can often be invaluable to management, both through their knowledge of individual species' requirements and as the sources of surveying, censusing, and monitoring data.—David C. Duffy.

CONSERVATION AND ENVIRONMENTAL QUALITY

(See also 9, 27, 44)

21. Organochlorine residue levels in herons and raptors in the Transvaal. D. B. Peakall and A. C. Kemp. 1976. *Ostrich,* **47:** 139-141.—"Despite the restrictions on the use of DDT in North America and many European countries in recent years the global usage of DDT is probably still increasing." Alas, our struggle to save our environment from continuing pollution with chlorinated hydrocarbons has not yet been won, as some of us had complacently come to believe.—Paul A. Stewart.

22. Sailing on reservoirs and its effect on water birds. L. A. Batten. 1977. *Biol. Conserv.*, **11:** 49-58.—In the heavy and increasing use of all fresh water areas for recreation, water birds are continually being deprived of more suitable habitat. This paper suggests that increasing attention be given to expanding multi-purpose goals to include provision of conditions favorable to water birds. Toward this end, lakes should be constructed so they contain marshy areas not accessible to boats, and these marshy areas should be screened from outside interference with plantings of shore and island vegetation. Hopefully some of this same brand of thinking can be introduced to affect the construction of farm ponds in the southeastern United States. Here it is illegal to construct a pond with emergent vegetation such as is required by Wood Duck broods because of the possible hazard from mosquitoes. Maybe the mosquito problem can be solved by increased knowledge of fish management.—Paul A. Stewart.

PARASITES AND DISEASES

23. Use of sentinel ducks in epizootiological studies of anatid blood protozoa. C. M. Herman and G. F. Bennett. 1976. *Can. J. Zool.*, **54:** 1038-1043.—White Pekin ducks (*Anas platyrhynchos*) and domestic geese (*Anser anser*) were held in cages on New Brunswick marshes as sentinels for haematozoan infection. Infection of the ducks by *Leucocytozoon simondi* occurred over six weeks from early to mid-June, and by *Haematoproteus nettionis* over a similar period from mid-July. Infection rates were low and not especially lethal.

There are minor inconsistencies between the accounts under "Materials and methods" and under "Results" as to how many geese were actually studied, and these make it problematical to assess the significance of species differences in infections.—Raymond J. O'Connor.

PHYSIOLOGY

(See also 14)

24. Terrestrial locomotion in penguins: it costs more to waddle. B. Pinshow, M. A. Fedak, and K. Schmidt-Nielsen. 1977. *Science*, **195:** 592-594.—Three species of penguins, Emperor, Adelie, and White-flippered, were tested standing and walking a treadmill at various speeds. The birds were trained to wear light masks, which permitted the exhaled air to be captured. The results were compared with mammals and with "other birds," and the penguins, in relation to their body weight, expended more energy. Dr. Pinshow writes me that the righthand scale of Figure 2 should be corrected. It is to be approximately multiplied by 10, and 20.08 matches unity on the lefthand scale.

The short legs and body sway are less effective for walking than long legs and a steady body. However, the penguins can improve the situation by tobogganning.—Charles H. Blake.

25. The mineral profile of plumage in captive lesser snow geese. J. P. Kelsall and W. J. Pannekoek. 1976. *Can. J. Zool.*, **54:** 301-305.—Primary feathers one half to two thirds grown and feathers fully formed were analyzed for changes in chemical composition in the course of growth. Changes in the content of Na, Ca, K, Fe, Mn, Mg, and Si were detected but no differences in Cu and Zn were found. Primaries 1 and 10 differed in content of all elements. Sex differences in Mg and Zn content were also demonstrated.

Current interest in wildfowl feather chemistry centers on hopes of identifying the origins of the birds through correlations between flight feather composition and the chemistry of the molting grounds. The results presented in this paper point to a serious limitation to this technique.—Raymond J. O'Connor.

26. The effects of estradiol and progesterone on the growth and differentiation of the quail oviduct. C. L. Boog and C. V. Finnegan. 1976. *Can. J. Zool.*, **54**: 324-335.—Daily injections of 0.2 mg estradiol in 5- to 10-day-old Japanese Quail induced the formation of tubular gland cells in the oviduct after 5 days, and epithelial differentiation after 10 days. Estradiol also enhanced oviduct size, weight, and protein, RNA and DNA contents, and induced the synthesis of the proteins ovalbumin and lysozyme. Replacement of estradiol by progesterone over the last 5 days of injections slightly enhanced tissue differentiation and RNA production, and depressed DNA production, when expressed relative to the estradiol treatment. Progesterone was also specifically an inducer of avidin production. Withdrawal from hormone treatments caused a loss of cells and of tissue constituents, greater in those birds previously on progesterone than in birds on estrogen only.

An additional treatment group—viz., estradiol for 10 days followed by hormone-free carrier injection for 5 days—would have simplified the interpretation of the withdrawal results, by eliminating three of the six explanations the authors were forced to consider.—Raymond J. O'Connor.

27. Altered yolk structure and reduced hatchability of eggs from birds fed single doses of petroleum oils. C. R. Grau, T. Randybush, J. Dobbs, and J. Wather. 1977. *Science*, **195**: 779-781.—Laying Japanese quail were given bunker C oil in a single dose, 200 mg. The eggs laid on the following few days had yolks altered in structure and in staining properties, and the hatchability was greatly decreased. These changes reached a maximum in eggs laid two days after administration, but eggs returned to normal laid four or more days after. Somewhat similar results were noted with chickens and Canada Geese, although their hatchability was not tested. Other types of petroleum oil were also tried with similar results. Larger doses halted egg production in quail for a longer period.—Charles H. Blake.

28. A laboratory simulation of predator-induced incubation interruption using ring-billed gull eggs. R. A. Hunter, H. A. Ross, and A. J. S. Ball. 1976. *Can. J. Zool.*, **54**: 628-633.—Predators appearing at *Larus* colonies at night can interrupt incubation and lead to exposure of the eggs to low temperatures for up to several hours. Hunter and his colleagues investigated the effects of such disturbances by artificial incubation of *Larus delawarensis* eggs. Incubation was interrupted by 4 hours at 10° C on one, two, or three consecutive days, commencing on days 7, 13, or 19. Embryonic mortality increased from 7 percent in the day 7 interruption group to 11 percent with day 13 interruption, and rose further, to 18 percent, in the late interruption sample. Mean incubation time increased in interrupted eggs by 0.5 days, with no clear trend with respect to duration of interruption. Within each period (4, 8, 12 hr) the variance of incubation times rose systematically as the timing of interruption was delayed. The authors identify this loss of hatching synchrony as of potential importance in increasing chick mortality after hatching, since chick age heterogeneity may disrupt the territorial stability of the colony.

Several points deserve comment. Although mean incubation times did indeed increase by 0.5 days on interruption, this figure fails to take into account the 0.33 day (on average) suspension of incubation attributable to the duration of the interruption itself. Secondly, the measure of hatching synchrony used is the sample variance of incubation time: since this changed from 0.737 for controls to 2.773 in the worst affected experimental group, the actual standard deviations changed from 0.86 to 1.66 days. I am a bit skeptical as to the likelihood of such a small between-nest-difference in chick ages disrupting the colony, and thus chick survival, to the extent envisaged by Hunter et al. Finally, I note that embryonic mortality was 6.7 percent for the laboratory control group but only 2.4 percent for a group of controls left untouched in the field. The numbers of deaths involved are very small, but the difference between the two raises questions about the effects of the eggs' transfer to the laboratory on their subsequent performance on the tests.—Raymond J. O'Connor.

29. An attempt to confirm magnetic sensitivity in the pigeon, *Columba livia*. J. P. Beaugrand. 1976. *J. Comp. Physiol.*, **110A**: 343-355.—This paper describes another well-controlled experiment to attempt to document magnetic sensitivity in a bird. As with all other recent attempts, it failed to do so. Restrained pigeons were subjected to earth-intensity fields resultant from a field produced by Helmholtz coils and the local field in the room. The resultant stimulus fields had declinations of −48.4° and −66.2° with respect to natural magnetic north, and inclinations of 22.3° and −13.5°, respectively. The pigeons were presented with the magnetic stimuli alone and coupled with electric shock; heart rate was used as an assay of reactivity. There was no indication that the pigeons detected the change in the magnetic field produced by activating the coils. These results support those of Kreithen and Keeton (*J. Comp. Physiol.*, **91A**: 355-362, 1974; see *Bird-Banding*, **47**: 170, review no. 16). As with those experiments, a stationary bird was asked to respond to a short-duration magnetic stimulus presented over a small number of trials (in this case, conditioning was attempted only on two consecutive days). It is becoming clearer that birds may need to move about in order to respond to magnetic stimuli, although the Wiltschko experiments suggest that the movement can be quite restricted and need not involve flight.—Kenneth P. Able.

30. Melatonin—Effects on the circadian locomotor rhythm of sparrows. F. W. Turek, J. P. McMillan, and M. Menaker. 1976. *Science*, **194**: 1441-1443.—Melatonin (an indoleamine) is produced by the pineal gland. After at least three weeks of constant darkness, House Sparrows (*Passer domesticus*) were implanted intraperitoneally with slow release capsules of melatonin and held in darkness. Two apparently opposite effects were observed: (a) some birds showed both a reduction in the length of the active period and in the space from one active period to the next and (b) continuous activity, the latter generally produced by larger doses. These results resemble the effects of constant dim light or constant bright light, respectively, on House Sparrows.—C. H. Blake.

MORPHOLOGY AND ANATOMY

31. Pre-1941 eggshell characteristics of some birds. D. R. Osborne and R. Winters. 1977. *Ohio J. Sci.*, **77**: 10-23.—Osborne and Winters present measurements of eggs from the 328 clutches of 253 primarily North American species in the Lloyd collection at Miami University, Oxford, Ohio. Two strengths of the paper are the lucid discussion of measurements and calculations useful in zoological work and the table of values from the eggs. The table presents a ready source of data on egg parameters of the species in the Lloyd collection. The authors compare their data with comparable measures from several other sources. Certain inconsistencies in their table of data, however, will necessitate correction before the data can be widely used. They fail to separate Boat-tailed (*Quiscalus major*) from Great-tailed grackles (*Q. mexicanus*) and lump data on clutches from both species. The scientific names (and data?) in the table were very poorly proofread. Once an adequate list of corrigenda is prepared, however, the data will be available for the diverse uses they merit.—Paul B. Hamel.

32. Morphologies of cells from 1-day chick embryos. A. R. Gingle and A. Robertson. 1977. *Science*, **196**: 59-60.—Disaggregated cells are of three types: biconcave, like mammalian erythrocytes; knobbed; and epithelioid with microvilli.—Charles H. Blake.

PLUMAGES AND MOLTS

(See 25)

ZOOGEOGRAPHY AND DISTRIBUTION

(See also 10, 19, 45, 46)

33. Birds of the tundra biome at Cape Churchill and La Perouse Bay. F. Cooke, R. K. Ross, R. K. Schmidt, and A. J. Pakulak. 1975. *Can. Field-Nat.*, **89**: 413-422.—In a superbly written and organized account Cooke et al.

extend our knowledge of bird life near Churchill, Manitoba (Jehl and Smith. "Birds of the Churchill Region, Manitoba." Manitoba Museum of Man and Nature, Winnipeg, Special Publication 1, 87 p. 1970) to the rarely-visited, tundra biome east of Churchill. The status of most species in the Cape Churchill - La Pérouse area is similar to their status near Churchill. Differences appear to result from the utter lack of trees in the Cape Churchill - La Pérouse region. The most noteworthy differences are the first nesting in Manitoba of Ross' Goose (*Anser rossii*), the first nesting in the Churchill area of Mallards (*Anas platyrhynchos*), the first record of the Boreal Owl (*Aegolius funereus*) in the Churchill area, and the first summer record of the Gyrfalcon (*Falco rusticolus*). —Edward H. Burtt, Jr.

34. Records of the Carolina Parakeet in Ohio. D. McKinley. 1977. *Ohio J. Sci.*, **77**: 3-9.—In a careful analysis of published reports and early travelers' accounts, McKinley discovered that many records, including one of Audubon, were unfounded. The only report of the species breeding in Ohio could not be verified. The paper suffers in that the careful historical analysis and weeding out of unfounded observations is not followed by a table, map, or textual summary of bona fide records, which presents the author's best estimate of the past occurrence of *Conuropis carolinensis* in Ohio.—Paul B. Hamel.

SYSTEMATICS AND PALEONTOLOGY

35. A model for the evolution of perching birds. J. A. Feduccia. 1977. *Syst. Zool.*, **26**: 19-31.—In this plenary lecture presented before the A. O. U. meeting in Winnipeg in 1975, Feduccia summarizes and synthesizes extensive researches in two areas: his own work on the bony stapes and his and others' work on paleontology of Tertiary birds. From this evidence he proposes a new hypothesis for the phylogeny of the several orders of perching birds. Feduccia very cogently postulates a sequence of ancestral-derived states for the stapes and constructs his phylogeny based on this feature with reference to earlier work. Major proposed revisions are a redefinition of the Coraciiformes into several orders and the separation of the suboscine Tyranni and Eurylaimi from the rest of Passeriformes into a new order Tyranniformes. This passerine redefinition is the central feature of his hypothesis. From this base he uses the known fossil record to develop a thought-provoking discussion of the paleobiogeography of the perching birds. This interesting paper is "must reading" for avian systematists and evolutionary biologists of all stripes. Feduccia's analyses will stimulate considerable work on evolutionary relationships of oscines and suboscines, on the competitive relationships of oscine and suboscine forms in South and Central America, on past distribution of suboscines in North America, and other questions. Unfortunately, the paper was inadequately proofread, resulting in one statement (p. 22) that in Piciformes, digit III is reversed instead of digit IV!—Paul B. Hamel.

36. Pleistocene avifaunas and the overkill hypothesis. D. K. Grayson. 1977. *Science*, **195**: 691.—The main conclusion is that the genera of birds show about the same proportion of Pleistocene extinction as do larger mammals in North America. Granting that the avian evidence rests on a few La Brea pits, most of the birds that have become extinct were not dependent in any way on large mammals for their food supply.

The reviewer concurs with the author in the general conclusions but, in addition, points out that at the time of Columbus's discovery there were probably less than 20 million natives in the whole of North America north of Mexico. This may or may not have represented the maximum number up to that time, and yet such large mammals as could withstand the climatic changes of the Pleistocene persisted, until the white man, with his superior equipment and the will essentially to exterminate large mammals, came along. It seems to be fairly widely agreed that primitive peoples did not kill for sport and that they regarded themselves as so much a part of nature that they perhaps did not even kill predators.—Charles H. Blake.

37. Teeth in *Ichthyornis* (Class: Aves). L. D. Martin and J. D. Stewart. 1977. *Science,* **195:** 1331-1332.—The teeth of *I. dispar* are similar to those of the hesperornithids. It is suggested that in both forms the young have the teeth in an open groove and that discrete sockets develop with age.—Charles H. Blake.

38. The specific distinction of Naumann's Thrush. (O vidovoi samostoyatelnosti drozdov Naumanna.) V. Yakhontov. 1976. *Z. Zhurn.,* **45**(8): 1263-1265. (In Russian with English summary).—Differences in behavior, adult plumage color, and nesting biotopes lead to the belief that the dark and rufescent forms of "Naumann's Thrush" are two distinct species: *Turdus euonomus* Temm. and *T. naumanni* Temm., notwithstanding an extensive overlap of their ranges. The former is more northern, the latter more southern in Siberia. There are no marked differences in calls or in egg coloration.—Leon Kelso.

FOOD AND FEEDING

(See also 11, 19)

39. Trophic relations of the Rook in steppe biomes. (Troficheskie svyazi gracha (*Corvus frugilegus* L.) v stepnykh ekosistemakh.) M. Voistvenskii, A. Petrushenko, and V. Boyarchuk. 1976. *Vestnik Zool.,* **1976**(6): 9-17. (In Russian.)—Conducted from 1972 to 1974, this study concentrated on the most common corvid species, and evidently the most economically significant species in the Ukrainian steppe areas, particularly the agricultural. About 180 food contents of "stomachs" of adults and juveniles afforded a diet list of 270 invertebrate food items, the majority insects, of 270 species of over 190 genera. As usual in food habits lists the length and variety of items could have been extended indefinitely according to the number of contents of "examples" available for examination. Plant contents were 31% by occurrence but comparatively trivial in volume. Ninety species and forms of beetles were taken.—Leon Kelso.

40. Some observations on the feeding activity of the Corvidae. B. Dubrowska. 1976. *Folia Biol.,* **24**(4): 367-372. (In English with Russian and Polish summaries.)—This study of circadian activity of birds, which presumably is determined by endogenous and exogenous factors, was undertaken to observe foraging of some corvids in relation to light intensity. The subjects were 2 Rooks (*Corvus frugilegus*), 2 Magpies (*Pica pica*), and 1 Common Jay (*Garrulus garrulus*). They were tested at light intensities of 0 to 30 lux. The results obtained indicated definite adaptability relative to approach of darkness. The decline of activity was not sharp but gradually declined with light intensity. "The possibility of shifting the limits of feeding activity toward low intensities of light shows that the inborn rhythm of corvine birds is flexible and that under certain circumstances it may be changed."—Leon Kelso.

SONG AND VOCALIZATIONS

41. Duetting song as territorial display in birds. (Duettieren als Revier-Anzeige bei Vogeln.) U. Seibt and W. Wickler. 1977. *Z. Tierpsychol.,* **43:** 180-187. (In German with English summary.)—The acoustic intensity of duet-song in several species of African birds was measured either in the wild or in captivity, as a function of the distance from the singing pair. Playback shows that a level of about 35 dB is required to elicit responsiveness; the duet-song falls to this level about 30 to 50 m from the singing pair. Because the duet is performed from song perches in the territory, it elicits approach and counter-singing from other pairs, the females participate in territorial defense, and because there are no other territorial songs, the duetting song is believed to function in territorial defense. Postures made by the duetting pair are interpreted as appeasement displays (male and female sit adjacent when duetting), and low-intensity components of the duetting song are probably signals between the mates.—Jack P. Hailman.

42. Vocal mimicry and the evolution of bird song. F. N. Robinson. 1975. *Emu,* **75:** 23-27.—Several important characteristics of mimicry and mimics are discussed. The author emphasizes that whereas some species mimic only during subsong (e.g., Western Magpie, *Gymnorhina dorsalis*) other species mimic during subsong and during full song (e.g., Superb Lyrebird, *Menura novaehollandiae*). Recorded sequences of imitations by Superb Lyrebirds, with notes unique to the lyrebird deleted, are recognized as Superb Lyrebirds by other Superb Lyrebirds and birds that follow the Superb Lyrebird to feed on grubs uncovered by the lyrebird's scratching. Recognition may be based on tonal quality or the fact that imitations follow one another in rapid sequence unlike the intermittent songs of the models. Australian mimics including the lyrebirds prefer models with loud harsh calls that have an energy distribution similar to the vocalizations of the mimic. Superb and Albert lyrebirds (*M. alberti*) mimic species that are normally quiet during the winter breeding cycle of the lyrebirds. Furthermore, both species of lyrebird and some bowerbirds mimic potential predators. Robinson suggests that such mimicry helps protect the mimics. The bowerbird sings near its stationary bower, and the lyrebirds fly poorly. Hence both mimics may benefit from Batesian, vocal mimicry.

The author's conclusion that mimicry is a displacement activity appears unlikely. The more ethologists learn about communication, the less use they make of the concept of displacement activity (Hailman, "Optical Signals: Animal Communication and Light," Indiana University Press, in press). So-called displacement activities are found to be integral parts of the species' communicatory repertoire and to have specific meanings. Such activities are not irrelevant as originally suggested. Surely mimicry is not the irrelevant activity Robinson implies when he labels it a displacement activity.—Edward H. Burtt, Jr.

43. Song of the finch *Lagonosticta senegala*; interspecific mimicry by its brood-parasite *Vidua chalybeata* and the role of song in the host's social context. G. A. Sullivan. 1976. *Anim. Behav.*, **24:** 880-889.—The African Village Indigobird (*Vidua chalybeata*) mimics the alarm calls, contact calls, begging calls, and songs of its brood-host, the Senegal Firefinch (*Lagonosticta senegala*). Nonetheless Senegal Firefinches are unresponsive to the indigobird's imitations. The author concludes that firefinches fail to respond to the indigobird's mimicry.

However, different populations of firefinches have recognizably different dialects, and indigobirds imitate the dialect of the surrounding population of firefinches. The indigobirds and firefinches used in this study come from nonoverlapping populations. The author cites evidence that firefinches respond to songs in their own and foreign dialects, but is that the problem? Firefinches can distinguish between a song in their own dialect and one in a foreign dialect (Payne, *Ornithol. Monogr.*, No. 11. 1973). Furthermore, firefinches reply to songs in foreign dialects and songs in their own dialect, but the present study deals with imitations of firefinch songs. Are the firefinches unresponsive to the indigobird's imitations because they fail to recognize the imitations as firefinch song, as the author suggests, or could firefinches recognize the imitations as imitations of a foreign population of firefinches? Indigobirds parasitize the same population of firefinches whose song dialect they mimic, hence an indigobird is no threat so long as it imitates a foreign dialect. The firefinch need not respond to an imitation if the imitation is of a foreign dialect. Such an interpretation is possible, hence the results fail to eliminate the possibility of interspecific communication. —Edward H. Burtt, Jr.

BOOKS AND MONOGRAPHS

44. Ecologie de la Zone de l'Aeroport International de Montreal—Les Oiseaux et le peril aviaire. Raymond McNeil, Normand David, and Pierre Mousseau. 1976. Les Presses de l'Université de Montréal, Montréal, Québec, Canada. 255 p. $10.00 (paper, in French).—In 1969 the government of Canada decided to construct a new international airport about 50 km to the northwest of Montreal at Mirabel. The project would potentially affect some 36,000 hectares, and several government agencies were deeply concerned about the social and ecological impact of such a project. In an effort to assess the impact of the proposed project the National Research Council, the Ministry of Transportation, and the new airport authority asked the University of Montreal to assemble a group of ecologists to make an environmental assessment of the site and formulate an impact statement. The group under the direction of Professor Dansereau contained members from the Center for Ecological Research of the University of Montreal and the Center for Research in Environmental Sciences of the University of Quebec in Montreal. An ecosystem approach was emphasized, and several study sections were organized along the lines of trophic levels: (1) minerotrophic—geology, geomorphology, drainage, and soils; (2) phytotrophic—flora and vegetation; (3) zootrophic—fauna and animal activities; (4) zootrophic—ornithology and potential bird hazards to aviation; (5) the industrial and urban impact, pollution, and planning; (6) demography, migration, perception of the environment, and social psychology. The volume being reviewed, "Les Oiseaux et le péril aviaire," covers the findings of the fourth study section listed above and is one of the 10 volumes that have been published on the ecological studies of the proposed airport site.

The information on potential bird hazards at the new airport was gathered during 1971 and 1972 and emphasizes, for all times of the year, the relative abundance of each species of bird encountered in the region of Mirabel, the nature of the habitats that attract groups of birds, the behavior of birds that would enhance a collision, and finally, the feeding regimes and daily activity patterns of those species judged to be the most dangerous in terms of aviation safety. For each of the bird species judged to threaten the safety of aviation, the reader will find a qualitative evaluation of the danger, the time of the year when this danger is present, as well as the behavioral and ecological factors responsible for the species being present in the region. The volume contains 16 tables, 29 figures, and 34 photographs. The reproduction of the photographs showing flocks of birds is very poor, and too many of these appear in the text. Of the 185 species of birds found at the airport site, 37 were judged to be of particular concern, and the text contains histograms of seasonal abundance for each of these species from May 1971 to October 1972.

The report contains nine chapters. The first gives a general introduction to the study, and the second covers the geography and general characteristics of the site for the new airport. The methods used for inventorying bird populations are treated in chapter three, and chapter four examines the limits of the zones of bird and agricultural control in terms of land use practices. Chapters five through eight are the major chapters in the volume and cover the species of birds particularly dangerous to aviation in the area, both agricultural and nonagricultural land use practices and their effect on bird populations, and the special measures that can be used to control the populations of birds in the airport environment. The last chapter briefly outlines the need for continued observations and studies once the airport is functional. The bibliography is adequate and contains 95 references, but most of the references after 1974 refer to papers specifically related to the project and published by the University of Montreal. The volume will undoubtedly serve the purpose for which it was written—a bird hazard impact statement for a proposed major airport—but the information will be of little general applicability.—Sidney A. Gauthreaux, Jr.

45. Alabama Birds. Thomas A. Imhof. 1976. University, Alabama, Univ. Alabama Press. 445 p., illus. $22.50. (Second Edition).—Fourteen years have passed since the publication of the first edition of "Alabama Birds." The first edition greatly stimulated field work in the state, and the second edition clearly documents the results of the increased activity. Thirty-six species have been added to the state list for a total of 378 species. The second edition further benefits from knowledge of the state's breeding birds gathered after the start of the Breeding Bird Survey in Alabama in 1966. Prior to the first edition 93 Christmas Counts had been published from the state during a period of 52 years, but from 1962 to 1974, 74 counts have appeared.

It is immediately evident that the text has been thoroughly revised and reorganized. The size of the type is smaller and the paper is thinner. The old edition was 591 pages in length, and the new edition is only 445 pages but contains considerable additional information. The glossary near the beginning of the book has been retained but has been revised and shortened. Terms like accidental, casual, distribution, immature, soporific, and hymenopterous have been eliminated from this section, but "soft parts" and "species" have been added. Unfortunately certain terms are not included, e.g., sonogram, call, song.

All the plates have been grouped and placed in the middle of the book. There are no new paintings, but four plates of 24 small color photographs are new and appear immediately after the old plates. Some of these photographs document records, but others are of questionable value and probably could have been eliminated. Many new black-and-white photographs and state distribution maps are scattered throughout the species accounts. Several new graphs show population trends over several years and the relative abundance of individual species in a particular group (e.g., finches) in the Birmingham area based on several years of counts. The bibliography is completely reorganized and grouped into the following sections: general, regional works, recordings, periodicals, and Alabama references. The section on Alabama references is further subdivided into historical, counts and lists, season reports, state lists, distributional data, banding and migration, miscellaneous bird biology, miscellaneous Alabama information, and papers on specific birds. Although this format is quite useful for those looking for information on a particular topic, it is hard to use if one is looking for a particular reference and knows the author. The end papers of the book contain a check-list of Alabama birds and can be used for keeping a personal state list.

Imhof has done a tremendous job in writing the second edition. All too often the second editions of state bird books are changed minimally, but this is certainly not the case with "Alabama Birds." It is more a new book than a typical second edition and is well worth purchasing.—Sidney A. Gauthreaux, Jr.

46. Island Biology, Illustrated by the Land Birds of Jamaica. David Lack. 1976. Berkeley and Los Angeles, Univ. Calif. Press. 445 p. $20.00—"To do science," wrote Robert MacArthur (1972), "is to search for repeated patterns, not simply to accumulate facts." David Lack was one of the best and most influential pattern searchers in ornithology. In 1947 he published a superbly concise, readable, and informative book on Darwin's Finches, which made evolutionary sense of the ecological and systematic patterns he perceived among those birds (Lack, 1947). He then switched his attention to matters of population regulation and only returned to the theme of island birds in 1969. In the meantime, MacArthur and Wilson (1963, 1967) had published their celebrated equilibrium model of island biogeography. It incorporated some of Lack's thinking. But Lack himself found it deficient and said so (Lack, 1969). He followed this up with a year's field work (1970-1971) in the West Indies, chiefly on Jamaica, while on sabbatical leave. The book under review is the product of that visit. It is a treatise on island bird biology with particular reference to Jamaica (Lack, 1976), and it is largely an elaboration and documentation of his 1969 views.

According to the MacArthur-Wilson equilibrium model, the number of species on an island is determined by the opposing processes of immigration and extinction. Their magnitude is influenced by the area and isolation of the island. Stochastic elements intrude in the determination of which particular species occur on an island. Lack objects that the model does not apply to birds because, like fern spores, spiders and some seeds, they are highly mobile and can get anywhere. Numerous observations of exotic birds reaching islands support this view. How, then, can the relatively small number of bird species on islands be explained? If dispersal is not the problem, settlement on the islands must be. Lack suggests that "ecological impoverishment" of islands is responsible, meaning that there are relatively few animal and plant species for the birds to exploit. Furthermore, the first species to arrive appropriate vacant niches and become generalists; they competitively exclude the proper occupants that arrive later, the phenomenon being most pronounced on remote islands. They do not pass through the taxon cycle outlined by Wilson (1961), Ricklefs (1970), and Ricklefs and Cox (1972).

This, essentially deterministic, theory is explained in chapter 1. It serves to structure the copious observations on West Indian birds that follow. The remaining chapters in the first half of the book are devoted to showing how various aspects of bird distribution in the West Indies support the theory; the equilibrium model is practically ignored. The chapters discuss the origin and present stability of the Jamaican bird fauna, the habitats occupied by the land bird species (most are habitat-generalists), competition for food, geographical displacement (replacement), the features of two interesting groups (hummingbirds and warblers) and the similarities with other islands, mountain-tops and mainland (Honduras). It turns out that Jamaica was not ideally suited to this study because it is not very remote, few waifs have been recorded, and only two of the 66 breeding species of land birds show evidence of niche expansion. Nevertheless, the general picture emerging is one of frequent dispersal to islands and frequent competitive exclusion. The second half of the book presents useful systematic and ecological notes on every species breeding on Jamaica. The species are well illustrated by Kay Kepler, and the notes should be a great help to the ornithologists who are stimulated by the book to solve the many unanswered questions about Jamaican bird ecology.

There are two conceptual weaknesses in Lack's theory. The first is that dispersal is equated with colonization potential. It is one thing for a warbler to visit Jamaica in the winter, it is another thing altogether for it to remain in the summer and breed. It is one thing for a few birds to reach an island occasionally, it is another thing for them to stay long enough, in proximity to each other, to pair, mate and reproduce, and thereby start a breeding population. So the supply of birds to islands may not be as great as Lack supposes, nor may their responses to immediate circumstances be as flexible as he believes. Lack is alone among ornithologists in promoting the idea that difficulties of dispersal are not important in determining low bird species number on remote islands (Abbott, 1973; Salomonsen, 1976). Distributional evidence will not settle the issue by itself, but it often points strongly against Lack's view. For example, Salomonsen (1976), using data given by Volsøe (1955), points out that there are seven species of swallows in northwest Africa but none on the Canaries, and he suggests that their absence on the Canaries cannot be explained by ecological factors.

The second weakness is the superficial treatment given to the question of ecological impoverishment. Bird species impoverishment is attributed to ecological impoverishment. It is only on the penultimate page of the first part of the book that Lack attempts to account for the hitherto unexplained ecological impoverishment: "This . . . changes the problem to a question of why should remote islands have a reduced ecological diversity in their plants, especially if difficulties of dispersal over the sea are not the cause. One factor, I suggest, is the more uniform oceanic climate, and another may be that remote islands are almost invariably small." There is no further discussion and the ideas are left hanging. The first factor rests on the doubtful assumption that uniform climates promote *low* plant species diversity; I would guess the opposite would be the case. The second factor comes very close to accepting a major tenent of the equilibrium model, as does a statement on page 195 that the degree of isolation exerts its influences on birds only through its effect on other organisms on which birds depend. I take this to mean that the equilibrium model could apply to other organisms, in Lack's view, but birds are a special case.

So the theories are not very different after all. Lack was confused about the difference, as is evident from the Preface and from two remarks in the text. He suggests that the stability of the Jamaican avifauna argues against the equilibrium model because the model implies high turnover rates. But the implication is not necessarily correct, and low turnover does not invalidate the model. Elsewhere he suggests that the essence of the argument of "other bird biologists" is that there is plenty of ecological room for other species, similar to those present, if only they could get there. He believes this to be mistaken and that all the niches are filled. But the equilibrium model allows for the addition of new species, although only by the replacement (i.e., extinction) of existing ones. The difference is that Lack believes island bird communities are absolutely resistant to invasion by virtue of absolute competitive superiority of the residents. He also believes invasion attempts, and competitive exclusion, to be very frequent events. So the differences between the theories are matters of degree and emphasis, not matters of kind. The combination of over-emphasis of colonization potential and inability to provide an adequate explanation of ecological impoverishment makes Lack's theory a poor competitor with the equilibrium model. The model has neither of these defects but has the decided advantage of generality. However, it is not without its own shortcomings (e.g., Abbott and Grant, 1976; Schoener, 1976).

Stability of avifaunas in the face of frequent dispersal requires considerable competitive exclusion, in Lack's view, and most of the book is concerned with evidence for this process. Here are displayed all the strengths and weaknesses evident in Lack's previous writings on population regulation (see Chitty, 1967).

Whenever he looked at closely related species he saw ecological differences among them and, even though similarities were more striking than differences in some groups (e.g., warblers), concluded that the species were ecologically segregated. With echoes of his book on ecological isolation in birds (Lack, 1971), Lack writes simply that ecological segregation can *only* be the result of competitive exclusion (p. 183) (my italics). At this point, if not before, a credibility gap will open between writer and most readers. From here on it is easy to "explain" most features of bird distribution in terms of competitive exclusion. For example, "competitive exclusion must be in operation, for a further 12 parulid species have occurred on Jamaica as transients, and they presumably do not stay for the winter because excluded by those that do so." A whole chapter on Geographical Displacement argues that the occurrence of different congeners alone on neighboring islands is the result of competitive exclusion. When observations do not fit the pattern, procrustean mental acts are performed to bring the erring facts into line, as shown by this comment on two geographically-replacing *Coereba* species that have very *different* diets: "It is therefore hard to imagine that the two species could be sufficiently similar in ecology in the West Indies to exclude each other geographically, yet their respective ranges *hardly permit any other explanation*" (p. 337) (my italics).

When competitive exclusion fails as an acceptable explanation, ecological deficiencies are invoked. Take the case of the 18 species of warblers that spend the winter in Jamaica, then leave to breed in North America. They surely cannot be excluded by the two resident species. So they must leave because their niches disappear. Similarly, birds that breed occasionally in Ireland have not established permanent populations there because of ecological deficiencies. In other words, one way or another, certain birds do not establish themselves because there is no ecological room. That might be acceptable, if not very informative, for species that are at least known to arrive. But, it is much less satisfactory when applied to instances of missing species. For example, the presence of one species in a genus on an island (but two or more on the mainland) is interpreted as "there is evidently room for only one of them on any one island" (p. 102). Again, "To a casual observer, the island forests will not look so 'full' [as mainland forests], but this, I suggest, is deceptive" (p. 166).

The trouble with this approach of buttressing a theory with observations and ad hoc explanations is that every observation has to be shown to be consistent with the theory, or else dismissed as unexplained. Thus, the Coal Tit may not have been able to invade the Canaries, but "just as likely" the Blue Tit has excluded it. The Blue Chaffinch may not have reached the western Canaries from the central ones, but "it seems far more likely" that the generalist has excluded it. For West Indian birds "It is absurd to suppose that they [some flycatcher species] could not also have reached Cuba or Hispaniola from Jamaica. Presumably, they are unable to survive on these neighbouring islands, but why this might be so is not known, and they are not replaced by similar-looking species which might there fill their respective niches" (p. 306).

There are many other observations which simply cannot be explained. These include how *Cyanophaia bicolor* fits into Martinique and Dominica communities, since it breaks the rule of three hummingbird species per mountainous island, why *Glaucis hirsuta* supposedly excludes *Eulampis jugularis* only on Grenada and has not spread to other islands; why the greater isolation of the Greater Antilles is associated with greater differences in species composition of adjacent islands; why some Jamaican endemics have no ecological equivalents elsewhere (the same applies to some Hispaniolan and Cuban endemics), and so on.

Actually these unexplained observations are potential invalidations of his theory that competitive exclusion rules supreme. I find this lack of objectivity the most distressing aspect of the book. Why did he adhere so strictly to the competition viewpoint? Partly I think for historical reasons, partly because it was forced upon him by the constructs of his theory, and partly because he feared the theory would collapse if exceptions were found. In another context he makes the revealing remark, "For, once one admits one small exception, others immediately appear" (p. 156). The most he admits is that wintering parulid warblers are a "partial exception" to the exclusion of specialists by generalists.

I think one important lesson to be learned from Lack's writing in general is that pattern, like beauty, is to some extent in the eye of the beholder. Further, it is not enough to seek patterns and offer interpretations of them. It is necessary to test those interpretations by whatever means are possible and to discard the poor ones.

It is not going to be easy to test Lack's theory because a careful reading will show that it is heavily qualified and phrased in language that renders it nearly unfalsifiable. The best test I can devise is to create an island midway between an existing island and the nearest mainland. When the new island has acquired a flora it can act as a stepping-stone to the original island. The equilibrium model would predict a higher equilibrium bird fauna on the original island as a consequence of its diminished isolation. Lack's theory would predict no change. But, remembering that Lack's theory applies strictly only to birds, and in *undisturbed* habitat, could we ever hope to observe the required results of such an experiment? An alternative manipulation is to create a new island out of a piece of mainland. The equilibrium model predicts a fall in the number of species with time. Barro Colorado island was in fact created this way in 1910-1914. Thirteen species of sedentary forest birds became extinct in the period 1923 to 1970 and were not replaced by new species (Willis, 1974). This is consistent with the equilibrium model. Lack's theory would predict no change and hence be falsified by the absence of replacements. But escapes from falsification are possible. Human disturbance of the forest could be claimed as an explanation of the disrupted bird fauna (but see Willis, 1974). Ecological impoverishment could also be invoked; some insect and plant species may have become extinct. These remarks illustrate the difficulty of devising a rigorous test of Lack's theory.

But Lack himself was not a tester of ideas so much as an explainer of observations and hence a generator of ideas. He asks specialists of other groups of organisms in the West Indies to *confirm* his views. His approach to biogeographical and ecological problems was essentially qualitative and inductive, what is often described as intuitive. It was non-statistical and hence poorly equipped to deal with randomness and coincidence. He counted rather than measured, he was a synthesizer rather than an analyzer. He was an adherent of what Ernest Williams (1969) has described as the "close" views of communities, as opposed to the "distant" view practiced by many biogeographers now. But there is an even closer view, the view of the population ecologist. In my opinion, this view is needed to comprehend fully the phenomena addressed in this book, and the book does a service by indirectly drawing attention to it. For a book with a well developed competitive exclusion theme it is remarkable, if not unusual, that almost nothing is said about competition as a population process. This is because it was not measured, nor was it conceived in any detail. Future generations will surely wonder that we discuss competition so much yet know it so little and measure it even less. When food is the object of competition, quantitative estimates of food consumption in relation to food availabliity are required, at the least, for the process to be studied directly.

David Lack's field work strategy was to be a generalist, sacrificing some depth for breadth. But, counter to the process of ecological generalization of birds on islands, specialists among students of birds replace generalists. I suggest the future research on island bird biology lies largely in the hands of those with the distant view and those with the very close view. That is a testable prediction! Simply counting how many birds occur in different parts of an island has limited utility now, except when undertaken to test a priori hypotheses.

I have been critical of Lack's views and methods at some length because the subject matter is important and has many ramifications in community ecology. This discipline is still in its youth; some would say infancy. Lack's previously expressed views on the importance of competition have been adopted uncritically by some community ecologists. This book will reinforce their beliefs, no doubt, but to others it should serve as a warning that competition-based theories still have a fragile foundation. Of course, Lack may be right in his interpretations (Abbott et al., 1977) despite taking questionable paths to them. Our task now should be to see whether they stand the test of prediction. It should not be to see how many more observations can be explained by competitive exclusion.

There are several strengths in this book that tend to offset the weaknesses. The book is written clearly and interestingly. It is refreshingly free of jargon, although two novelties are introduced: Bond's line is offered for the faunal boundary between Grenada and Tobago, and "collection" is the name coined for a loose aggregation of birds that do not behave as a cohesive flock. Lack has a stimulating habit of raising questions and pointing out areas of ignorance. For example, he provides a useful list of 16 questions directed to specialists of other groups. He makes the first comprehensive attempt to compare the bird fauna of a West Indian island with the fauna on a segment of the mainland (Honduras). As demonstrated in previous books, he builds enticing bridges from the particular to the general, for example in the comparison of percent endemism of land birds in the West Indies and on other archipelagoes around the world. Finally, he deserves great credit for providing a wealth of information on West Indian birds in 48 Tables, 29 Appendices, and 58 Figures. These data should provide an excellent preparation for future work. For the community ecologist, I suggest starting with the hummingbirds.

Regrettably, David Lack died in 1973, before he had completed this book. The task of bringing it into print has been performed admirably by his wife and by Dr. James Monk. It is ironic and sad that he died within six months of Robert MacArthur, whom he had inspired, but whose style was so different. No two ornithologists have influenced ecological thought more in the last two decades. G. Evelyn Hutchinson (1975) concluded an essay in appreciation of Robert MacArthur with the words "Robert MacArthur knew his warblers." With equal force it can be said that David Lack knew his birds.

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—P. R. Grant.